

# WELFARE RESEARCH AND EXPERIMENTATION

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HEARINGS  
BEFORE THE  
SUBCOMMITTEE ON PUBLIC ASSISTANCE  
OF THE  
COMMITTEE ON FINANCE  
UNITED STATES SENATE  
NINETY-FIFTH CONGRESS  
SECOND SESSION

NOVEMBER 15, 16, AND 17, 1978



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# WELFARE RESEARCH AND EXPERIMENTATION

WEDNESDAY, NOVEMBER 15, 1978

U.S. SENATE,  
SUBCOMMITTEE ON PUBLIC ASSISTANCE  
OF THE COMMITTEE ON FINANCE,  
Washington, D.C.

The subcommittee met, pursuant to notice, at 11:45 a.m., in room 2228, Dirksen Senate Office Building, Hon. Daniel P. Moynihan (chairman of the subcommittee) presiding.

Present: Senator Moynihan.

[The press release announcing these hearings follows:]

[Press release]

## FINANCE SUBCOMMITTEE ON PUBLIC ASSISTANCE ANNOUNCES HEARINGS ON WELFARE RESEARCH AND EXPERIMENTATION

The Honorable Daniel Patrick Moynihan (D. N.Y.), Chairman of the Subcommittee on Public Assistance of the Senate Finance Committee, announced today that the Subcommittee plans to hold public hearings concerning various welfare experiments and research projects which have been undertaken over the past several years. The first day of hearings will be Wednesday, November 15, 1978 beginning at 11:00 A.M. in Room 2228 Dirksen Senate Office Building. Hearings will continue on Thursday, November 16 at 10:00 A.M. and on Friday, November 17, also at 10:00 A.M.

Senator Moynihan observed that: "The last ten years have seen the development of a large body of research related to welfare programs. Included in this research have been a variety of experimental projects involving alternative approaches to income maintenance and to employment policy. There have also been numerous studies of the welfare population and evaluations of existing programs. The Subcommittee expects these hearings to assist it in bringing a number of major research findings together. As we resume the arduous process of considering structural revisions of our welfare system, we need to develop a more complete understanding of what is and is not known in this area. What are the strengths and weaknesses and potentials for improvement in our existing system? What may be the gains—and what are the dangers—of alternative approaches that might be considered? These are questions to which the substantial research investment of the past decade should be able to provide some informed answers."

Senator Moynihan announced that the following witnesses are scheduled to testify at the hearings:

*Wednesday, November 15, 1978, 11 a.m.*

Dr. Jodie Allen, Special Assistant to the Secretary of Labor for Welfare Reform.

Dr. Irwin Garfinkel, Director, Institute for Research on Poverty, University of Wisconsin.

Dr. Robert Spiegelman, Director, Center for the Study of Welfare Policy, SRI International.

*Wednesday, November 15, 1978, 2:30 p.m.*

Dr. John Cogan, Department of Economics, Stanford University.

Dr. Finis Welch, Department of Economics, University of California at Los Angeles.

Dr. Samuel Z. Klausner, Center for Research on the Acts of Man, Philadelphia, Pennsylvania.

*Thursday, November 16, 1978, 10 a.m.*

The Honorable Donna E. Shalala, Assistant Secretary of Housing and Urban Development for Policy Development and Research.

Dr. Michael Wiseman, Institute of Business and Economic Research, University of California.

Mr. Norman Angus, Deputy Director for Public Entitlements, Utah Department of Social Services.

*Thursday, November 16, 1978, 2:30 p.m.*

Dr. Myles Maxfield, Jr., and Dr. David Edson, Mathematica Policy Research, Inc.

Dr. David W. Lyon, the Rand Corporation.

*Friday, November 17, 1978, 10 a.m.*

The Honorable Henry Aaron, Assistant Secretary of Health, Education, and Welfare for Planning and Evaluation and Michael Barth, Deputy Assistant Secretary for Income Security Policy.

Dr. Robert G. Williams, Mathematica Policy Research, Inc.

Dr. Bradley Schiller, Department of Economics, American University.

*Written Testimony.*—Senator Moynihan stated that the Subcommittee would be pleased to receive written testimony from these persons or organizations who wish to submit statements for the record. Statements submitted for inclusion in the record should be typewritten not more than 25 double-spaced pages in length and mailed with five (5) copies by November 30, 1978, to Michael Stern, Staff Director, Committee on Finance, Room 2227 Dirksen Senate Office Building, Washington, D.C. 20510.

Senator MOYNIHAN. Good morning. I would like to apologize to our witnesses and to our guests for the delay in the convening of these hearings. The computer which manages the Eastern Airlines flights went down and we stayed on the ground, and we could not help that.

I have a statement about these hearings which I would like to introduce into the record and not delay the proceedings any further.

[The prepared statement of Hon. Daniel P. Moynihan follows:]

#### PREPARED STATEMENT OF SENATOR DANIEL PATRICK MOYNIHAN

This morning, the Subcommittee on Public Assistance begins three days of hearings to review the results of recent research and experimentation in the field of public welfare and to consider the likely consequences of proposed welfare reforms. We are holding these hearings now, when no specific legislation is pending, so that our examination can be thorough and open-minded. Before beginning another round in the welfare reform debate, it is imperative that we understand what this research has produced. Having committed ourselves to an experimental mode, we are plainly obligated to pay attention to the results of our experiments even when, as it appears, some of the major findings—first presented to this Subcommittee last spring—raise questions about the fundamental premise of the type of welfare reform proposal that has dominated our thinking for the past decade.

The idea of a large addition to the income maintenance provisions of the Social Security Act goes back to the early 1960's, when Senator Neuberger proposed a family allowance. As Assistant Secretary of Labor for Policy Planning and Research, I prepared a paper on family allowances that constituted perhaps the first serious examination of this idea within the executive branch.

Subsequently, there was a general movement of opinion toward the negative income tax approach, an idea that attracted a wide range of academic endorsement. The Johnson Administration gave serious consideration to proposing a national income maintenance plan of this sort. It is fair to say that this was, in a certain sense, to have been the culminating enactment of the Great Society. But when the Johnson Administration ran into difficulties that are well known at this time, rather than making a legislative proposal the President appointed

a Commission on Income Maintenance Programs—generally known as the Heineman Commission—to study the matter. In the meantime, the Department of Health, Education, and Welfare, having judged—correctly, as it seems—that no major national program would be adopted in the near future, decided to go ahead and test the idea through experiments. The first of these began in New Jersey in late 1968. Further experiments were conducted in rural North Carolina and Iowa, in Gary, Indiana, in Seattle, Washington, and in Denver, Colorado. Approximately \$112 million in all has thus far been committed to these endeavors. In the history of social policy there has never been such an extraordinary use of the experimental mode.

History meanwhile got itself jumbled somewhat, because following President Johnson's decision not to propose a national income maintenance plan, President Nixon decided to do so. He announced the Family Assistance Plan in August, 1969, and this proposal dominated five years of public policy debate in the early seventies. Then in 1977 President Carter proposed a variation on the same theme: the Program for Better Jobs and Income.

The principal aim of all these proposals has been to replace AFDC, Food Stamps, and similar programs with a single, national income maintenance plan. This objective was endorsed by the Heineman Commission, which reported in late 1969, and it has dominated most thinking about welfare "reform". Through this means, we thought it would be possible to assure a decent level of assistance to the needy everywhere in the United States, while avoiding the incentives for dependency and family-splitting that were thought to characterize the existing welfare system. At least that was the nearly unanimous view of experts inside and outside the national government.

In truth, little solid evidence could be mustered to support this view. As I had written in 1967, "there are not 5 cents worth of research findings" to sustain the argument that AFDC leads to family breakup. Almost nothing was known about why welfare rolls were growing, whether existing programs were fostering dependency or encouraging the poor to migrate from one part of the country to another, or what the consequences of replacing welfare with a national income maintenance plan might be. This lack of knowledge became painfully apparent in the debate over the Family Assistance Plan and was in no small measure responsible for the political stalemate over welfare reform that has ensued, particularly as the dollar costs of such proposals have soared.

Even as Congress was deliberating, efforts were undertaken to generate the information that we lacked. The most notable were the aforementioned income maintenance experiments, carefully controlled and monitored tests of how low-income families would react to programs similar to those under consideration. In addition, the Federal government sponsored a number of studies on the existing welfare system intended to gain more information about how it works, what its effects are, and what would be the probable results of various alterations in it. All told, we have spent upward of \$150 million on welfare research during the last decade.

What have we learned? What are the implications of this research for future public policy? This round of hearings is addressed to those questions.

It does not seem likely that the answers will be comforting to those of us who had hoped to replace existing programs with some form of national income maintenance or negative income tax program. The evidence presented to us last spring suggested that, far from strengthening family ties, such a reform might further weaken them. Moreover, instead of encouraging work and self-sufficiency, the kinds of plans tested appeared to produce substantial reductions in work effort and corresponding increases in dependency on public subsidy. Ten years ago, we expected quite different outcomes from these tests. We must now be prepared to entertain the possibility that we were wrong.

These findings will be examined in detail during these hearings. Perhaps their policy implications are not as serious as they seem; perhaps other research offers useful insights into ways of dealing constructively with the problems of dependency and family stability. In any event, we trust that everyone interested in welfare reform will give the most serious consideration to what is known and can be learned about these issues. To act in disregard of knowledge is as dangerous as to act in ignorance of it, and is surely less justifiable.

Senator MOYNIHAN. It is the special honor and privilege and pleasure, in particular to this lone chairman, to have as our opening witness one of the persons who has pioneered in this research, who

has brought to it a standard of candor and openness and clarity which anyone who has ever sought to work with research data of this kind will envy and admire. Not for nothing has she an international reputation in the world of income maintenance.

I am, as I say, honored to welcome Dr. Jodie Allen who is special assistant to the Secretary of Labor for welfare reform, to open these hearings.

Dr. Allen, good morning.

**STATEMENT OF JODIE ALLEN, SPECIAL ASSISTANT TO THE  
SECRETARY OF LABOR FOR WELFARE REFORM**

Ms. ALLEN. Thank you, Mr. Chairman. I am pleased to be here today to survey some of the results of recent social science research relating to existing public assistance reforms and to the current program. You have already covered, for this audience, some of the more salient findings, that I will discuss, but I will probably go ahead and repeat them.

Senator MOYNIHAN. Please take as much time as you want.

Ms. ALLEN. Fine, thank you very much.

I will, however, be focusing, in my testimony, on those aspects of welfare reform which relate primarily to the responsibilities of the Department of Labor in welfare reform and those of course, are the responsibility to minimize the need for transfer programs by promoting the development and maintenance of a healthy labor market.

I have a longer, prepared statement which I would like to submit for the record which includes citations for the various findings which I will discuss today.

The ultimate purpose of income maintenance programs, of course, is to assure at least a minimal adequate level of subsistence for all Americans. We have to keep that basic goal in mind.

For those families with potentially employable members, three factors are of primary relevance in determining the need for income assistance: First, is there an adequate demand for the services of the family's potential breadwinner? Does the labor demand exist?

Second, is the family willing to provide the services of these potential workers to the market, and in what quantity? In other words, how is the labor supply?

And third, how effective are employment and training programs in filling gaps on both the demand and supply side, in the first instance by direct augmentation of demand and in the second, by up-grading of skills and potential learning. In other words, how good a job is the Labor Department doing?

Before turning to each of these questions, it is obviously relevant to ask whether, indeed, there are a sufficient number of low-income families with employable members to make their treatment in welfare programs a significant issue. Some have argued that since most persons on welfare are either aged, disabled or young children, employment programs have little relevance for welfare policy.

Well, I think there are several obvious things wrong with this argument. The first, of course, is that the aged and disabled are no longer, if they ever were, really at the heart of the debate over welfare reform. We have an SSI program; there may be reason to improve it, but that is not what all of the argument is about.



Senator MOYNIHAN. May I interrupt you to point out that people who think we never get anywhere might well usefully remind themselves that the SSI program emerged from the debate over the family assistance plan. It was a part of that plan, a title which was passed.

Ms. ALLEN. And a very important one, one which we think has done a lot of good.

So, having set the aged and disabled aside, there is the next question of what about the children? Well, the obvious thing about them is that they do not live alone. We are not talking about driving children off into factories or mines. That is not the issue.

The issue is that these children, like most children, live with parents and relatives and if those parents and relatives can become self-supporting then they can take the children off welfare.

So what do we know about the adults in low-income families? That is what it really comes down to.

Numerous cross-sectional and longitudinal surveys show that the great majority of low-income families with children have at least one worker—in 1975, about 65 percent. The more recent data we have shows that of families below one-and-a-half times the poverty line, at least 75 percent of them, in the most recent years, have earners.

Even female heads of families on welfare have a high probability of being employed at some time over 5-year period—almost 70 percent, in one study by Rainwater and Rein.

Given this apparent economic potential among many low-income families, the prime concern of the Department of Labor in the area of welfare reform is the encouragement of maintenance of work effort by the low-income population and the development of employment and training opportunities to sustain that work effort.

I think it is useful to think of the problem of employment among the low-income population as having a demand and supply side aspect. On the demand side, people must have jobs at which to work. One can have all the work incentives in the world, and if there are no jobs, they will do no good.

On the supply side, they must be willing and able to do the work. If you have a lot of jobs but if the incentives are such that no one will take them, that will do no good either.

Both sides of the problem are important, and there are crucial ways in which the two are linked. The traditional diagnosis of employment problems among the low-income population is in terms of inadequate skills, education, work experience and the set of incentives built into the transfer system that reduce the rewards from work.

This view, while raising important problems, is incomplete because of the importance of labor demand and the availability of jobs.

On the demand side, the most efficient generator of job opportunities is obviously a strong and growing economy. Many, although not all, problems of employing the low-income population are greatly reduced by overall increases in economic activity.

A high-pressure economy causes employees to seek, train, and upgrade workers who, during more slack periods, might be considered structurally unemployed. This, in turn, improves the long-term stock of human skills available to the economy. And there are other beneficial side effects, as well. The recent study which the Labor Department sponsored by Sommers and Clark has found the important result that

not only do labor force participation rates increase during good times, as we have long known, but individuals drawn into the labor force stay there.

The availability of jobs created by strong aggregate demand creates a long-term commitment to work on the part of those drawn into the labor force.

Senator MOYNIHAN. It is the case, is it not, that the proportion of the population in the work force has risen in recent years after being stable for much of the century? It was about 56 percent, was it not, for some 60 years, and it has now gone up?

Ms. ALLEN. Yes, you are right. It is now at record highs and it was stable, or relatively stable.

Senator MOYNIHAN. What is it now; about 59 percent?

Ms. ALLEN. Oh, yes, close to 60.

Senator MOYNIHAN. Close to 60.

This is a point that I would like to bring up—

Ms. ALLEN. We have the number here.

Senator MOYNIHAN. Thank God for the Bureau of Labor Statistics. You have not introduced your colleagues, incidentally.

Ms. ALLEN. Accompanying me are Joseph Corbett and Raymond Uhalde of my staff. We have so many statistics we cannot find them.

The employment rate is at a record high—not just the labor force participation rate, but the number of people actually in jobs.

Senator MOYNIHAN. Why do you not find those numbers and submit them for the record; all right?

Ms. ALLEN. We will do that.

[The following was subsequently supplied for the record:]

The "labor force participation rate" (that is, the percentage of the population 16 years and over who were either working or seeking work) was 63.6 percent in November, an all time high. The "employment-population ratio" (that is, the percentage of the population 16 years and over who were working) also hit a record high in November, surpassing 59 percent for the first time.

Senator MOYNIHAN. This series, as I recall, goes back to around 1895 or 1910?

Ms. ALLEN. I would think so.

Senator MOYNIHAN. It is one of our oldest data series, and it is a point that we are going to hear an awful lot about, you know, "People won't work." We hear a lot about that. And there are people who will not work. We all know some of them, and we are all related to some of them, I guess.

But it is a much more universal fact that the number of people working and seeking work in this country as a proportion of the population is the highest it has ever been, higher than back in those good days in 1910 when America was better, or whatever it is that it was in 1910. And God knows when life was harder, and income was harder to get, and wages were lower, still the proportion of the people working or seeking work was lower than it is today.

Ms. ALLEN. I think that is a terribly important point. It is one that is frequently overlooked. We tend to look at the unemployment rate, forgetting that the economy is responding very well to this terrific pressure that has been put on it to create a very large number of new jobs because of the very significant increases in the size of the labor force. The economy has responded extremely well to this challenge.

We tend to always look at the negative side of things.

Senator MOYNIHAN. Right.

Ms. ALLEN. That is the best news that we can have, because the negative side of the findings which I just presented is that unemployment carries a large and enduring cost, not just in the short term, but in the foregone development of human skills and of work commitment.

The strong demand for labor also obviously influences the efficacy of training programs designed to increase the supply of labor skills among the low-income population. Consideration of training programs brings us naturally in turn to a consideration of labor supply effects, since the purpose of such programs is to upgrade labor skills and, in turn, to increase both earnings and desired hours of work.

In a recent evaluation in November 1976, the National Council on Employment Policy concluded that the weight of the evidence over the last 15 years is that employment and training programs have had positive impact and that there is more proof of effectiveness for such programs than for any other major social welfare activity.

Senator MOYNIHAN. Tell us about that. The National Council on Employment Policy, what is that?

Ms. ALLEN. It is an independent group of well-known researchers. It is not a Government agency.

Senator MOYNIHAN. But it is Labor Department oriented?

Ms. ALLEN. Oh, yes, sir.

Senator MOYNIHAN. So it starts out that the Labor Department does things better than any other department? They have one of those over at HEW, too.

Ms. ALLEN. They did pull together a variety of different studies. It is a long compendium, you know. It points out that one researcher says this and one says that and again none of the studies are definitive in themselves, but when you do look at them and you realize that the training programs are usually judged against a very harsh standard, the results are quite encouraging. For instance, they usually require training and employment programs in these analyses to justify their own costs simply in terms of increased earnings for the participants without any account taken of possible savings in unemployment compensation, welfare, and other social programs.

Generally, we have judged our training programs against pretty rigorous standards, and they seem to hold up pretty well without claiming that they are infinitely successful.

Senator MOYNIHAN. I think we made more claims before they got started than we ought to have done, but the Manpower Development Act was passed in 1962 and you now have about 15 years' experience and you are satisfied with what you have got?

Ms. ALLEN. We are generally satisfied.

Senator MOYNIHAN. I am.

Ms. ALLEN. Certainly you can see, particularly when you get down to the local level, an awful lot of ways to improve training programs. Some training programs are spectacularly successful and we can begin to see the ingredients of those successes. Usually it is because they are much better related to the private sector. They work with the employers in the area. You can just tell the things that are going to make it work better. And we would like to disseminate that knowledge

more, improve administration, make a more uniform product at the local level.

But I think that we kind of know how to do that and what it takes now is a commitment to better administration.

Senator MOYNIHAN. Yes.

Ms. ALLEN. Transfer programs, such as welfare, exert an opposing influence on the labor supply. The effects of transfer programs on the supply of labor are of concern for both macroeconomic reasons and microeconomic reasons. At the macro level, transfer programs can affect the overall economy by worsening the relationship between unemployment and inflation. One way this can happen is that by raising the returns of not working relative to working, the programs may influence people to delay taking jobs for a variety of reasons, such as trying to find a higher paying job, or even going fishing.

These delays may lengthen the duration of job vacancies, raise the rate of wage increase, or force employers to use less efficient workers. In any case, the result can be less productivity, more inflation, and ultimately even additional reductions in employment if public policymakers try to restrain inflation by inducing an economic slowdown.

The more traditional concern about program effects, however, focuses on the individual supply of labor; that is, his work effort. What heightens our concerns is the realization which the chairman observed, that relatively small program effects on work effort can translate into ineffective program outcomes. That is, we can spend a lot of money on program benefits and get very little in the way of income improvement among the beneficiaries.

For example, if increases in benefit levels and benefit reduction rates were to cause only a 10 to 11 percent reduction in family hours worked, it could easily turn out that the Government would spend millions of dollars on benefit payments and have little effect on families' incomes. Instead, most of the expenditures—perhaps as much as 55 percent under some plans—would offset reductions in earnings.

It was out of such concern that OEO and later HEW funded several large negative income tax experiments to try to quantify the amount of work reductions caused by alternative benefit levels and tax rates. The idea, with which you are very familiar, was to select a sample of families and, on a random basis, assign some families to a financial payments plan and other families to a control group.

Now, families in the control group might be receiving welfare payments, and it is important to remember that all of the results reported are incremental to the current welfare system which has its own disincentive effects.

Then, by observing differences in work effort between families eligible for payments and control families, the precise incremental impact of the payments scheme could be determined.

Unfortunately, we found that we had a lot to learn about how to experiment before we could get down to finding out what the experiments showed us. The highly publicized New Jersey experiment produced only tentative results because of simultaneous changes in the New Jersey welfare system, insufficient sample size and sample design problems.

Other technical problems beset the Gary and rural experiments. For example, there were only 105 families receiving payments in the

rural experiments and these were split among farmers, nonfarmers, among two different ethnic groups and two very different States and five different benefit plans. Now, obviously it is very hard to draw conclusions from a small sample split up in that fashion. Questions were also raised as to whether the results observed in a 3-year experiment could be generalized to a permanent program.

The Seattle-Denver income maintenance program avoided many of the pitfalls of the earlier experiment—it is always easier to do something a second time. Its sample size was larger, 25 percent larger than the combined sample of all the other experiments. Operators exercised strict control over the interaction of the experiment with existing programs and included an additional sample of families eligible for payments over a 5-year period.

This experiment provides the best evidence to date on the effects of the negative income tax on work effort. Bob Spiegleman from Stanford Research will be here this morning discussing these results and I will just mention a few of the basic findings.

In comparisons between families eligible for payments and control families where account is taken of such important variables as pre-program work effort, race and normal income, Stanford Research and HEW analysts found the negative income tax plans produced the following reductions in work effort: Compared to control families, work effort reductions in families eligible for 3-year payment plans as compared to families in the control group averaged 7.6 percent among husbands, 16.7 percent among wives, 13 percent among females heading families.

Among other family members aged 16 to 21 who do not head families of their own, the reduction in work effort was about 43 percent for males and 33 percent for females—and I might note that these last reductions were not offset by increases in school attendance.

The 5-year reductions were particularly higher. Presumably such results are more representative in some ways of the effect of the long-term program.

Senator MOYNIHAN. Why do you not go right through those on page 11?

Ms. ALLEN. You know I have a different version than you do. Page 11?

Senator MOYNIHAN. I am sorry.

Ms. ALLEN. I have a shorter version.

Senator MOYNIHAN. The average work reductions were usually substantially larger when comparing families eligible for 5-year payment plans.

Ms. ALLEN. Right.

Senator MOYNIHAN. Why is that? Some group was told you are in this for 3 years, and some were told you are in this for 5 years?

Ms. ALLEN. Right, and the point—we were trying to get at this question of duration bias, which was probably the strongest criticism leveled against the New Jersey findings.

Senator MOYNIHAN. Which was what? Say it out: Duration bias.

Ms. ALLEN. That when you observe the results of a short-term program, when you come in and you tell somebody, here is the guaranteed income, but it only lasts 3 years—

Senator MOYNIHAN. I am going to give it to you; you are going to have it for the next week.

Ms. ALLEN. Right.

Senator MOYNIHAN. As against the rest of your life?

Ms. ALLEN. Obviously, it is a big difference.

Senator MOYNIHAN. Yes.

Ms. ALLEN. And as we expected, we found that families on the 5-year program did reduce their work effort more. We found an 11.4-percent reduction for husbands, 25.3 percent for wives, 15 percent for female heads of household and for other family members who are nonheads, young nonheads, we observed a 43-percent work effort reduction for the males and 60 percent for the females.

But there are some important things about this. The results are not symmetrical. You get a bigger—and these were all predicted by Charles Metcalf, who was then at the University of Wisconsin, who wrote the definitive paper on duration bias. He predicted that the effect of a short experiment would be to understate the impact of the benefit level and to overstate the effect of the tax rate.

This is for two reasons. First, on the income side, obviously the expected value of a permanent income increase is larger than the expected value of a short-term income increase. On the other hand, when you put a high tax rate on earnings, you make it very expensive to work right now.

If a person has a sort of view of life in which they think, well, you know, out of the next 30 years I will probably work 26, they might decide to concentrate their leisure now during the program when it costs them less, and so you might have an overreading of the tax effect.

Senator MOYNIHAN. I see.

Ms. ALLEN. And it turned out that Metcalf was right, although I must stress that unfortunately—and here is where we made another mistake—our 5-year sample was only about a quarter as big as the 3-year sample which I, in retrospect—and I am responsible for that decision—I think was a mistake, and it means that it is hard to get statistically significant results among benefit levels and tax rates in the 5-year sample.

So we do not have such strong results for the 5-year families taken alone. It is for that reason that all of the results that we have used in the cost estimating are an average of the 3- and 5-year families, and they are overweighted by the 3-year families.

Now, what does that mean? When we make estimates for different level programs, it does mean that for a generous program we are understating the average reduction somewhat, but it is important to note that for a relatively modest program, one with a low benefit level, perhaps 65 percent of the poverty line with a high tax rate, or something, the results flip around. In that case, you find that the 5-year results are actually somewhat lower, or at least not very different, from the 3-year results, simply because the tax-rate effect is reduced.

Senator MOYNIHAN. If you do not mind my saying so, that is pretty heavy stuff.

Ms. ALLEN. I am sorry.

Senator MOYNIHAN. I mean, a 60 percent drop, a 43 percent drop—

Ms. ALLEN. We were particularly concerned about—these results of the nonheads are new; Bob Spiegleman will be talking about them. We

have not seen them before. Again, we ought to study them some more.

They are somewhat upsetting because we find that when young male nonheads marry—these are young people who are in the age range of 16 to 21—their work effort reductions remain very high. On the 5-year program, they are 55 percent, and this is disturbing because these are young people who are undertaking new family responsibilities and it is possible that we have had a deleterious effect on their long-term labor force attachment.

Now, we cannot conclude that. We do not see them for long enough, but it does give us some thought.

Senator MOYNIHAN. Do you want to spell that out again? You're a young male nonhead; normally speaking you are living with your own family.

Ms. ALLEN. Yes; all of these young people were initially living with their own parents or other relatives.

Senator MOYNIHAN. And there is a 55-percent reduction in their hours worked as against the control?

Ms. ALLEN. The ones who marry, who become family heads themselves, had a 55-percent reduction if they were on the 5-year program and a 28-percent if they were on a 3-year program.

Senator MOYNIHAN. There is a wonderful line of Charles Lamb which goes, "If ever I marry a wife, I will marry a landlord's daughter/sit at the bar all day and drink cold brandy and water." How many of these people married landlord's daughters?

Ms. ALLEN. We could probably determine that.

Again, you have got to realize that it was a relatively short-term program and you just do not know the long-term effect. We did follow these families up few some time after the experiment. It will be very interesting to see whether these kinds were simply taking a vacation, essentially, and after the experiment ended they went back in the labor force just as you would have expected them to. So it is very important to remember that. The duration bias works in both ways.

The numbers that I have given here—

Senator MOYNIHAN. May I ask about your two versions? Is one the version for the committee and the other the real story you are telling the Secretary of Labor?

Ms. ALLEN. No, Mr. Chairman, in no way. I thought when we got done writing this thing and I looked at it and found that it was—

Senator MOYNIHAN. "Oh, my God, don't tell them that," you said.

Ms. ALLEN [continuing]. Thirty-one pages, I thought you probably did not want to sit here through the whole thing and so I went through and took a meat ax to it and cut and pasted a shorter version, but there are no substantive differences between the versions.

The numbers that we just talked about, of course, it is important to remember summarize what actually happened on the experiment. They are not extrapolations to any particular reform proposal. To interpret their relevance for policy purposes, we have to take account of a wide variety of factors which influence the results in various directions.

On the one hand, the average generosity of the payment plans in SIME DIME was generally higher than plans proposed in welfare reform debates. It did cover the range of policy relevance, but some

of the plans tested had guaranteed 125 percent, and even 140 percent, above the poverty line. Now, that is considerably higher than what we have been talking about.

Senator MOYNIHAN. Right.

Ms. ALLEN. At least for a basic Federal program.

On the other hand, the experiment operated in only two cities and in these cities the average wages and income were relatively high. We do not know whether if we went into poorer areas we would have a larger effect.

Senator MOYNIHAN. So that, then, could tend to counteract the impact of higher benefit levels, if they would not be proportionate.

Ms. ALLEN. Exactly. So we do not know exactly how to adjust for all of that, but we tried to.

There were also various differences in program administration and payment calculations—

Senator MOYNIHAN. May I just interrupt myself? I said, in other words, that would offset the impact of the benefit levels being high. I should say, in other words, commonsense tells you that we do not know a damn thing about it, do we? We are constantly finding out that what everybody knows turns out not to be so.

Ms. ALLEN. These results are all very consistent with what theories predicted and, indeed, are very close to some labor supply results that were produced in the late sixties using nonexperimental data.

Senator MOYNIHAN. I see. That Mr. Metcalf was interesting.

Ms. ALLEN. Yes. That was very gratifying to see it work so well. I think Chuck was surprised himself.

But we do have to look at these important differences which make it impossible to directly carry these results over to the current welfare program and to many alternatives.

Some of the things that I might mention is that there are differences in income accounting procedures. Income was measured over a longer period than is usually the case in a welfare program. This might tend to diminish the effects on the one hand since the payments did not go up as quickly when earnings declined; but on the other hand, there was no work requirement in any of these programs. People did not have to go down to the welfare office, they did not have to register for work or training. Some of them were offered a training opportunity—they usually had to pay for a part of the cost—but that was purely voluntary.

Senator MOYNIHAN. No work requirement.

Ms. ALLEN. No work requirement.

It is also important to remember that the actual effects of any proposed welfare reform will depend upon the particular parameters chosen. Other things equal, the higher the basic benefit and the higher the benefit reduction rate, the greater will be the work effort reduction among the population covered.

However, part or all of the total effect of raising the benefit reduction rate may be offset, at least in the short run, by the corresponding reduction in the size of the eligible population. That is, if you hold the guarantee constant and raise the benefit reduction rates, you will make fewer people eligible—at least initially—and those people will reduce their work effort more, but since there will be fewer of them, the total



work effort reduction may be less than in a program which has a lower benefit reduction rate.

This is a very interesting finding.

Senator MOYNIHAN. You mean a marginal rate of 80 percent will discharge an awful lot of people who stay somehow within that possibility but the number of people covered is very much lower than with a marginal rate of 20 percent?

Ms. ALLEN. Exactly.

Now, do you want to be a little bit cautious about that, though, because the long-term effect may not be quite that favorable. When we use our models and look at these effects, we look only at people who have income below some rather low level in any given year and there are relatively few of them, thank Heaven. We then conclude that those are the only people that were affected, and maybe they will not work, maybe not at all, but there are so few of them that we may not care a whole lot.

The only problem is that, as we know from numerous studies that are very well summarized in a study by David Lyons, there is a great deal of normal fluctuation in income among the low-income population, so that the number of people who might dip down below the eligibility limits for this program over time will be much greater than the number of people we initially made eligible. As a result over time more people may become affected by the disincentive associated with a high tax rate and the effects might cumulate.

Senator MOYNIHAN. Is that a stable finding, that income fluctuates more in these lower ranges than in middle and higher ranges?

Ms. ALLEN. My own studies of income fluctuation always suggested that, but mine were mostly based on the data that we had from the experiments themselves. You have very little data on intrayear income flows—you have pretty good year to year data from the longitudinal surveys like the Michigan longitudinal survey and they do show a lot of fluctuation.

The Seattle-Denver experiments and the other experiments are our only source of information on intrayear income flows and I did find a lot of fluctuations in the studies I did on monthly reporting and income accounting. I found that at the very lowest income ranges fluctuations increased somewhat with income—if you do not have any income, then it obviously cannot fluctuate. Income fluctuations increased somewhat as income increases over some range and then they decline as income increases.

You face this tradeoff between high tax rates and high eligibility limits and we do not really know what will happen over time. We might have a tendency to stockpile additional people not working overtime, but our results do not extend to that sort of finding.

Now, while the SIME-DIME results are important there are, as I said, quite a lot of remaining uncertainties. Chief among these, perhaps, is the question of how different the effects would be under programs that look more like our current welfare system.

To examine this issue, we have to look at studies of current program experience, although such studies, of course, do not have the precision of controlled experiments.

For the current welfare population of female heads of families, the estimates derived from several studies which we cite are gen-

erally consistent with SIME-DIME findings—that is, female heads of families are sensitive to benefit levels and tax rates. Unfortunately, we have little evidence of the impact of food stamps and the unemployed fathers component of the AFDC program on the work behavior of two-parent families. In fact, it is not even clear why so few low-income two-parent families participate in the two programs.

We do cite a couple of studies that came out in this regard which suggest that in the case of UF, program restrictions, such as the limit on work per month to 100 hours, the prior work experience rule and strict work requirements, are the cause of the low participation in the program.

Senator MOYNIHAN. What is the AFDCU participation?

Ms. ALLEN. No one really knows. Oh, probably about 50 percent, I would say. That is what a recent simulation estimate for 1975 suggests.

Senator MOYNIHAN. About half.

Ms. ALLEN. Some estimates are as low as 15 percent, but you have a lot of measurement problems in such studies.

One thing, it is easy to overmeasure eligibility. We do not have really good data on income for low-income people. As you know, there is a big hidden economy. People do not—well, people have no particular reason to hide income from the census worker and we are usually using census data. Nonetheless, if they are engaged in extra-legal or quasi-legal activities, they are not likely to report the income from them in great detail to the census worker.

So we may be overstating eligibility.

There are a couple of studies on this subject. Neither one of them even pretends to be definitive. They both suggest the same thing, though, that one real reason is that the AFDC-UF program is run in a very strict fashion with a lot of pressure on participants. While the WIN program does not begin to serve the number of potential people it could help because of restricted resources, it does focus its resources on the UF family, and they are the ones who get first priority in training, first priority in placement, and all of those administrative factors tend to keep turnover on the UF program quite high and participation low.

Michael Weisman, who is going to be up here testifying tomorrow has some interesting findings. He has been looking at California and he notes that even though the program in California, the UF program, is nominally just as generous as it was before, the fact is that they have really been tightening the administrative screws and, as a result, they have been knocking the program levels down.

This, of course, is the next point that I make in my testimony. That it is important to remember—and there are several studies on this point by Weisman, Lidman, Reidel, and Holmer—that cost-conscious program administrators can often exert a far more effective influence on the actual level of case openings and closings, and hence on the size of the caseload, than can all of these larger social and economic forces that we spend a great deal of time worrying about, and this, again presents us with an agonizing choice. We would like to simplify, streamline, and humanize welfare administration and we would certainly like to reduce fiscal burdens on hard-pressed localities.

But, as we change ease of access to welfare, we can change participation levels. And we can also change participation levels by changing the financial incentives, not just for the recipients, but for the program administrator. That is to say, if you have a local government administering a program and its stake in the cost of that program is not very high, you may find that there is more lenient administration of the program than otherwise and this can affect caseload, for better or worse.

One other measure designed to increase employment among welfare recipients deserves mention, and we have actually talked about it a bit, and that is the work requirement. This, of course, is currently a part of WIN and the food stamps program.

The work requirement requires that the participant accept suitable jobs or training opportunities, if available, or suffer a reduction in benefits. We really have only one study of this and I wish we had more, and a more definite one, but the effectiveness of this measure has been recently examined by researchers at Brandeis University. It was a field study—they did go out and compare experiences in several sites—but it was not a real experiment in that they did not have a control group. They were not able to take a certain number of people off the work requirement and compare their work effort with those facing ONC. Instead, they just compared experience among areas, and I think that weakens the study.

Senator MOYNIHAN. Frederick Mosteller has a remark he loves that "We didn't have any controls because it is only an experiment."

Ms. ALLEN. Well, yes. That was true of this study.

The Brandeis researchers conclude that while the work requirement can be used to maintain pressure for job search on those who do not work, there is little evidence that, under existing work tests, such pressure results in the actual return of registrants to work.

However, the author suggested that the effectiveness of a work requirement could be strengthened by incorporating a more structured and better monitored job search component and by adding a large-scale public employment component so that sufficient jobs will be available for all those subject to the work requirement.

Senator MOYNIHAN. Are you saying that WIN, as it now operates, has no substantial work incentive, as found by this group?

Ms. ALLEN. No, I am not saying that. I think that WIN does help a lot of people find employment. But as far as making somebody work who does not want to work, I do not think we have any evidence that it does that.

Fortunately, there are so many people on the welfare rolls who want employment assistance that it is easy to do a lot of good without worrying a whole lot about whether you are forcing somebody to work who does not really want to.

Senatory MOYNIHAN. You have more applicants than you—

Ms. ALLEN. Absolutely. Many more, we always do.

There is another important interaction between transfer programs and labor supply and demand, which must concern us. This is the relationship of these factors to the changing structure of families and the implications of such changes, in turn, for employment and transfer policy.

The most obvious phenomenon of note is the rise of the female-headed family, with which you are more than familiar. Between 1968 and 1975, the number of women heading families with children increased by a very startling 60 percent. Labor force participation, as you know, among women is lower than among men and while participation among women generally has been rising steadily, this has not been the case among women heading low-income families.

Whether this trend continues depends, in part, upon the structure of welfare programs themselves, but perhaps and even greater, in part, on the number and types of jobs available to families and young workers in the next several years.

Several studies have explored the relationship between employment of family breadwinners and family dissolution, and this, of course, is the problem which you, Mr. Chairman, first called to the public's attention over a decade ago.

There are many studies that focus on this issue. The studies suffer from common problems of unobserved variables, uncontrolled factors, or confounding influences, but all of the studies finds a direct relationship between the employment stability of husbands and family stability.

One of the studies, which I do not think we cite in the prepared testimony but which is of particular interest, is the study of Sawhill, Peabody, and others which observes that the fact of unemployment is a far greater determinant of family stability than absolute differences in income. For instance, they find that for black families, the effect of prolonged unemployment of the head has a greater effect on splitting than even a \$10,000 absolute difference in income.

So unemployment appears to be a very important factor in determining family stability.

Senator MOYNIHAN. Who did you say this was? Sawhill—

Ms. ALLEN. Sawhill, Isabel Sawhill; Caldwell; Peabody and company. We have the citation somewhere.

Senator MOYNIHAN. Send us those and we will put them in the record.

Ms. ALLEN. Yes. We have a bibliography that we will put in the record which cites this study.

[The following was subsequently supplied for the record:]

#### RELATIONSHIP OF UNEMPLOYMENT TO MARITAL STABILITY

Saul Hoffman and John Holmes. "Husbands, Wives, and Divorce." In *Five Thousand American Families: Patterns of Economic Progress*. (Vol. IV). Institute for Social Research, University of Michigan, 1976.

Using the Panel Study of Income Dynamics, hours worked was found to have an important curvilinear effect on marital dissolution over a seven year period. Husbands working 48 hours per week were found to have the lowest split rate with significant increases in splitting occurring for those working more than 60 or less than 40 hours a week. Also, husbands recently experiencing unemployment or high job turnover were more likely to experience a marital dissolution.

Isabel Sawhill, Gerald Peabody, Carol Jones, and Steven Caldwell. *Income Transfers and Family Structure*. The Urban Institute, 1975.

Among poor or near poor families experiencing serious unemployment problems during the first five years of the Panel Study of Income Dynamics, their probability of separation over the next four years rose by more than 16 percentage points if the husband experienced serious unemployment problems. A typical low income white family's probability of dissolution rises 7.6 percent

to 24 percent. The typical low income black family's probability of dissolution rises from 12 percent to 30 percent.

Andrew Cherlin. "Employment, Income, and Family Life: The Case of Marital Dissolution." The Johns Hopkins University, 1978.

Using the sample of mature women (age 30-44) from the National Longitudinal Surveys of Mature Women (Parnes), it was found that a greater number of weeks worked by the husband in 1968 significantly reduced the probability of marital dissolution between 1967 and 1971, indicating that the employment stability of the husband is an important determinant of marital dissolution.

Marjorie Honig. "AFDC Income, Recipient Rates and Family Dissolutions." *Journal of Human Resources* 9 (1974): 303-322. \* \* \* "A Reply." *Journal of Human Resources* 11 (1976): 250-260.

Using 1960 and 1970 census data for SMSAs, higher unemployment rate were found to increase the female headship rates of whites in 1970 and of both rates in 1960.

Joseph Minarik and Robert Goldfarb. "A Comment." *Journal of Human Resources* 11 (1976): 243-249.

Using 1970 data on states, results similar to these noted by Honig were obtained. The unemployment rate had a positive coefficient, but was not statistically significant.

Heather Ross and Isabel Sawhill. *Time of Transition. The Growth of Families Headed by Women*. The Urban Institute, 1975.

Using 1970 Census Employment Survey data on 41 cities, it was found that doubling the median weeks of male unemployment raised white headship rates by 55 percent. There was no effect for black women.

Kristen Moore, Steven Caldwell, Sandra Hofferth, and Linda Waite. *The Consequences of Early Childbearing: An Analysis of Selected Parental Outcomes Using Results for the National Longitudinal Survey of Young Women*. The Urban Institute, 1977.

Among young women in the National Longitudinal Survey, it was noted that living in an area with high unemployment caused statistically significant increases in the marital split rate.

Saul Hoffman and John Holmes. "Husbands, Wives, and Divorce." In *Five thousand American Families: Patterns of Economic Progress* (Vol. IV). Institute for Social Research, University of Michigan, 1976.

Controlling for important demographic and economic variables, the husbands hourly wage rate was negatively associated with a lower probability of dissolution over a seven year period. These results were obtained from the Michigan Panel Study of Income Dynamics.

Isabel Sawhill, Gerald Peabody, Carol Jones, and Steven Caldwell. *Income Transfers and Family Structure*. The Urban Institute, 1975.

During the first four years of the Panel Study of Income Dynamics, women's earnings and the probability of marital dissolution were found to be positively associated. For each \$1,000 increase of earnings by the wife, the percent of couples that separate increased by one percentage point.

Andrew Cherlin. "Employment, Income, and Family Life: The Case of Marital Dissolution." The Johns Hopkins University, 1978.

Among women in the mature sample (age 30-44) of the National Longitudinal Surveys (Parnes), the greater the ratio of the wife's to the husband's actual or expected wage in 1967, the greater the probability of dissolution by 1971.

Heather Ross and Isabel Sawhill. *Time of Transition: The Growth of Families Headed by Women*. The Urban Institute, 1975.

Among families in 1970 Census Employment Survey data for 41 cities, a ten percent rise in the median income of intact families lowered rates of female headship by seven percent.

Margorie Honig. "AFDC Income, Recipient Rates, and Family Dissolutions." *Journal of Human Resources* 9 (1974): 303-322 \* \* \* "A Reply." *Journal of Human Resources* (1976): 250-260.

Using 1960 and 1970 census data for SMSA's, rates of female headship among whites and blacks in 1960 and 1970 were found to be negatively associated with male earnings and positively associated with female earnings.

Joseph Minarik and Robert Goldfarb. "A Comment." *Journal of Human Resources* 11 (1976): 243-249.

Using 1970 data on states, results similar to these noted by Honig were obtained.

Ms. ALLEN. So as you observed, unemployment incurs dependency costs that go well beyond the immediate. Presumably, employment and training programs which upgrade the skills of and/or increase demand for family breadwinners will exert a stabilizing influence on families. While we have no experimental evidence of this effect, we do have suggestive evidence from the frequently observed strong, positive effect of increased incomes on family stability and the fact that the higher is the unemployment rate of the husband, the higher is the probability of family instability.

Senator MOYNIHAN. I might interrupt you here to say that we have got some recent data which were put together for us by the Congressional Research Service. When I first worked on this subject in the Labor Department in the early 1960's, what startled me and led me to think that we were going to have a very sharp rise in dependency was that there had been a very close correlation between male unemployment rates and the AFDC case openings.

The correlation was about 0.91 over about a 14-year period. And then in the late fifties that correlation began to get weak and in the early sixties it disappeared altogether and that struck me as saying that something had gone wrong and we were going to have a lot of that trouble.

I can now tell you, however, that by the 1970's, about a decade later, the relationship seems to have picked up again. We went through a turbulent decade, and now are back where we had been; that is, unemployment does seem to have a direct correlation with the number of AFDC cases. I found that interesting.

Ms. ALLEN. It is very interesting. I think that the underlying relationship was always there, it is just that we had this huge growth in welfare participation during that period induced by administrative changes and by greater levels of knowledge and changes in benefits which obscured it.

Senator MOYNIHAN. And don't forget that cohort of people just crashing into every institution we have, whether they are colleges or maternity wards, or what. It was the biggest cohort that size in the history of the country. It will probably never happen again.

The population 14 to 24, the cohort of which I speak—those people who cause all the trouble in the world and who do all the things that are interesting—between 1890 and 1960, that cohort grew by 10.5 million; then in the 1960's, it grew by 11.3 million.

In 70 years, it grew by 10.5 million, and in 10 years it grew by 11.3 million. This decade, it grows by 800,000; in the eighties, it will decline.

That is what actually happened to college campuses, for example, despite all those explanations that it had something to do with the personality of the dean or the weather in Berkeley or foreign policy—it was size.

Ms. ALLEN. They certainly did mess up all our lovely curves, didn't they?

Senator MOYNIHAN. They certainly did.

Ms. ALLEN. Things are getting back to normal. It is very reassuring.

We were talking about the expected stabilizing effects of improving employment and training opportunities on family stability. Now, as you observed, extending welfare programs per se to intact families

does not appear to offset the destabilizing effect of unemployment among family heads.

Studies by Weisman and by Lidman find very high rates of family splitting among families on the AFDC unemployed fathers program. Of course, you cannot extrapolate these findings directly to a general population because of the fact that these family heads are unemployed and have, at least at the time they are on the program, very low incomes. Other things equal, they are more likely to split and indeed they do.

But it seems likely that the fact of being on welfare, replacing earnings by welfare, cannot offset the destabilizing effect of unemployment per se.

We also have the controlled experimental data from the SIME-DIME program to which you referred and these indicate that providing cash assistance to two-parent families in the manner tested in the experiment increases, rather than reduces, family splitting on average. Of course, you have to apply a lot of caution in extrapolating these results.

Senator MOYNIHAN. Now here is where I have to ask you a question, and it is a hard one. You say we have to use caution in extending these later findings. Well, yes, of course we have to use caution. But certainly at this point, unless someone can help us in how to handle experimental modes, have our experiments not told us you have to have a lot of caution going ahead with the President's program?

Ms. ALLEN. I think there are very important differences. The first thing to remember is that we have a big welfare program right now, and it is hard to imagine that large effects would occur if you change it marginally, particularly in States such as your own where you already have a pretty generous program: You have an unemployed fathers program; you have a home relief program—

Senator MOYNIHAN. Are you describing the President's proposal as marginal? Watch yourself.

Ms. ALLEN. Well, really—

Senator MOYNIHAN. Do you know the life expectancy of Assistant Secretaries?

Ms. ALLEN. If you really look at the cash assistance side of PBJI and compared it to what was currently operating, it was no sweeping revolution. It would only have been in the 10 or 11 low-payment States of the South that it would have actually increased the basic benefits, and we were very, very cautious in how we extended cash assistance to the two-parent families, for exactly these reasons.

I mean, we had these results 2 or 3 years ago and we looked at them. They were preliminary at that time; there were lots of questions. But we knew they were going to come out and obviously we were very concerned about them.

Now, there is no question—I mean, I think you can argue about technique on the experiment and this and that but I do not think you are going to shake those findings as they apply to that experiment very much, as Bob Spiegelman will tell you. That is what happened; it is strong. The findings are robust with respect to the experiment.

But the experiment was quite different in very many important ways from what we are talking about doing in cash assistance even under the more ambitious PBJI program and as you suggested we

are likely to come up with more incremental proposals in this next go-around.

And you have to feel that differences in degree certainly do become differences in kind. It would be remarkable if we made very modest changes in the current welfare system, and left the way we administer it mostly alone if all of a sudden 10 million men went running away from their families.

On the other hand, there is no question that you can imagine programs that would have a large effect, at least initially.

You expect in an experiment—or, indeed, in the real world—that when a program first comes in it will tend to have a higher initial impact than it might have in the long term because there may be a pent-up stock of discontented husbands and wives who say: “Whoopie now we can get away from each other.” After 2 or 3 years this potential for splitting will diminish, and that, indeed, has occurred on the experiment. Now, you cannot exactly extrapolate—

Senator MOYNIHAN. Has it begun to taper off?

Ms. ALLEN. It has begun to taper off. On the other hand, you cannot conclude too much from the experiments for the reason that you are stuck with a panel, a fixed sample in the experiment, and in the real world you would have a replenished stock of available people to split. In the sample you get what you might call “panel fatigue.” You can only do so much splitting in a 3-year time period, particularly if you have already split once.

Senator MOYNIHAN. Panel fatigue? That is almost as good as “sibling overlap.”

Ms. ALLEN. So you cannot conclude exactly from the experiment that you would have as much diminution in a permanent program, although you would have some.

Senator MOYNIHAN. But Dr. Allen, as a friend of this committee, you will grant that there is a level of explanation that is required of the advocates of these programs higher now than before the experimental findings have come forward? Right?

Say “Right,” Dr. Allen.

Ms. ALLEN. Right.

Senator MOYNIHAN. It is true.

Ms. ALLEN. Of course, I reserve the right to edit that for the record.

Senator MOYNIHAN. It is true. I mean, do you know what I am going to have to explain to this Senate? I will say, well, yes, there is a 55-percent drop, but think about it this way.

I mean, many people have already stopped thinking when they have heard that.

It is a lot more complicated than before we—I told you never to go and find out about this stuff.

Ms. ALLEN. I know, darn it. Well, we certainly do not want to make that mistake again.

Senator MOYNIHAN. You will not make that mistake again, right? This is the last time anybody will experiment with a bold new program.

Ms. ALLEN. Well, there is another important thing that I have just got to point out and that is that we have put a lot of emphasis in the administration's reform program and a lot of dollars into creating jobs rather than putting people on cash assistance.

Senator MOYNIHAN. That is true, and this is the response to these findings.



Ms. ALLEN. Exactly, and that was an extremely important component—

Senator MOYNIHAN. I frankly wish that these findings had been made a little more public. We would have digested them by now.

Ms. ALLEN. I think we are in the process of digesting them now, though. I think that, for instance, these hearings are very, very helpful in that regard so we can talk about the findings, and I think we have to talk very frankly, too, about the trade-offs. You know, you could totally eliminate both the work incentive effects and the effects on family stability of welfare by closing down the welfare office.

The obvious problem with this solution is that you would have untoward side effects in terms of starvation, illegal income, and causing a great deal of human misery. So we have trade-offs in all of these things. There is nothing that we do that does not incur some side costs, but I really think that you can minimize those costs if you know about them ahead of time and weigh them against the alternatives.

I also think that we should not exaggerate the expected effects of any likely welfare reform on the basis of these experiments, which were quite generous compared to anything that we are talking about in terms of average payments. They were also very liberal in the way they were administered.

On the other hand, I do not think it is a convincing argument that the observed levels of splitting in the experiment were exaggerated because the experimenters were not able to verify that the men were still there in the families and we would have known they were there in the real world. I mean, there is certainly a certain amount of disguising of the presence of men under both the AFDC program and probably under this one, but a diligent effort was made to find these men, more diligent than under the average AFDC program.

On the other hand, it was relatively easy for a family to claim a change in status. They did not have to go down and wait in a long queue in the welfare office; they did not have to submit to an examination by a social worker. It was clear what would happen if the husband left—that once his earnings were gone the benefit level would immediately go up in the next check a month later. And that reduction of uncertainty is, I think, a very important factor.

I would not be willing to conclude, however, that that responsiveness is not something that you want to have in a welfare program. I do think we want to streamline administration. There are offsetting concerns.

Senator MOYNIHAN. You can devise a whole range and direction of social policy that is not at all agreeable. You cannot tell people their rights, threaten them, keep their benefits at the absolute minimum, and if you want to put a high value on things like family stability, the direction of these findings—

Ms. ALLEN. I guess we have come to the point where we have to discuss how successful employment and training programs have been in alleviating the problems of insufficiency in both labor supply and demand among low-income populations and what we might expect from expansions or innovations from such programs.

In the prepared testimony that we have submitted for the record, we provide some findings from the WIN program, from a 3-year experimental project in Vermont which provided jobs, training, and supportive services for low-income families; from the welfare demonstration projects which were initiated under the Emergency Employ-

ment Act in the early 1970's; from the title VI CETA expansion and from several other projects.

I am not going to go through all of those findings.

Now, none of these efforts was, or is, of sufficient scale relative to the demand for such services, but all of the programs have shown positive results for participants and have generally had successful program performance—that is to say that people have performed useful services, their skills have been upgraded and their postprogram experience was positive.

I will mention one recent well-controlled effort of this sort which is the supported work program. Supported work is a special job creation program for persons who traditionally have had great difficulty in finding or keeping employment. One of the target groups of supported work is women on AFDC and in particular that subset of women who have had long-term dependence on welfare and repeatedly demonstrated lack of successful employment. So, it was aimed at sort of the hard-core, the hardest-to-employ people.

Results from a preliminary sample of AFDC enrollees, including a rigorously constructed control group—they did have a control group—indicate that during the first 9 months of participation women in the experimental group earned \$3,150 more, worked 1,051 hours more, received \$1,147 less in welfare incomes, and had total incomes \$2,007 higher than those in the control group.

We think this is very encouraging. We do not yet have the long-term results, but this is much better than we expected from this group of women who were picked, purposefully, to be the hardest ones to help.

Clearly, there are still a lot of unanswered questions, and as both you and Senator Long have suggested, we think it is obviously sensible to begin any major social initiative with carefully controlled field testing. Recently, the Department of Labor provided—

Senator MOYNIHAN. Would you say that again?

Ms. ALLEN. We think—or I think—it is a good idea. Maybe I should make that clear. I think it is a good idea to try things out in the field before you initiate them, particularly in the case of employment and training programs where a lot of problems are administrative. As we discussed before, I think that we have a pretty solid research base which indicates that if you do these things right, if you run your employment and training problems well, they can be very successful. But we also know if you do not run them right, they will not have those effects.

So I think that in any program which depends so heavily for its successful impact on the way that it is administered, it obviously makes sense to test it out in the field, try out different things, and phase it in gradually over time.

Senator MOYNIHAN. You know that I get awfully nervous when I hear someone say, "Run this program right," because the 1960's were filled with wonderful tales about the Harvard Ph. D. who went off to teach children in preschool for the 2 years he had to go until his draft eligibility expired and he did marvelous things with them, you know, and then, of course, he turned 26 and that was the end of that marvelous experiment, but it proved that you could just do anything if

you had a teaching profession made up fully of Harvard Ph. D.'s instead of a teaching profession drawn from—well, you know where they come from.

Herman Wouk had a wonderful line in "The Caine Mutiny" in which he described the Navy in the Second World War—a Navy in which I served, and I agree with him—as a system devised by geniuses to be run by morons. Anybody can gear up an absolutely crackerjack program but who is going to run it in 15 years time in a place you never heard of in a State you barely ever heard of? Who is going to be running it?

Ms. ALLEN. I agree with you. I think we have a good answer on that. I was not going to talk about the welfare pilot projects, the employment opportunity pilot projects, because we do not yet have funding for them. We do hope we will. We have planning grant money, and we have started the planning grant phase. The way that we designed the program was meant to address exactly that problem of replicability.

We deliberately did not decide to run small experiments here and there run by Ph. D.'s out of a consulting firm or a university. The commitment of the Department was to run the tests on a credible scale under regular administrative apparatuses at the local level.

Now, this confined us in certain ways, although it greatly increased the potential power of the findings. It meant that you have to go into a locality and you have to run the pilot as if it were a real program. This also means you have to make it available to all of the eligible people and that gets quite expensive. Of course, it also has the tremendous advantage of allowing you to observe labor market effects in a way which you never could if you had a little, tiny sample, and I think that that is a very important issue. I do not think it would be worthwhile to go out in the field with some small-scale gilded experiment.

So we felt that we needed to have at least 15 sites and we tried to pick our sites to make them not only representative geographically and demographically but also representative of the kind of administrative arrangements you get at the local level which vary greatly.

We picked prime sponsors that we thought were good ones, but with typical types of problems. So I think that if we can do this, it will be useful.

We do not know yet whether we will have the money to fund the experiments—I personally hope that we will, but of course there are other budget choices that have to be made.

I would mention one last interesting finding of a somewhat different sort because it falls in the category of the Metcalf finding, and that is the findings of economist George Akerloff. Arguing from a theoretical perspective in the American Economic Review, Akerloff presents an important rationale for combining employment and training programs with income assistance programs.

By distinguishing between employable and nonemployable persons, Akerloff finds employment and training programs allow higher benefit levels for those who cannot work and lower rates of marginal taxation for those who can work, and this balance increases the social effectiveness of such programs.

Mostly I throw this in because I found it amusing to see a well-known academician working his way through a very complicated set of equations—

Senator MOYNIHAN. And then he came out—I was going to say, I could have written that article. I know that.

Ms. ALLEN. Well, I thought he had a statistically significant, theoretically elegant confirmation of commonsense.

Now, where does all this experience lead us? I do not think—and I think you will agree—it means that we should weaken our effort to help the poor. A decent society comes to the aid of its unfortunate members. What the results do tell us is that we must take care to provide help in the most effective way.

We have learned the unsurprising lesson that the poor, like other people, act in a way which is consistent with their perceptions of their self-interest. In some cases, like the rest of us, they must misjudge them, but we can count on the fact that, sooner or later, social program beneficiaries will respond to the incentives, good or bad, which policy planners build in social programs. There are two ways we can respond to this finding.

We can try to distract the poor from pursuing their self-interests by confusing program designs or conflicting rhetoric. More sensibly and successfully, I believe, we can design our social programs to make them provide the kinds of incentives which encourage low-income people to move toward achievement of social and economic goals which, by and large, they share with the rest of society.

To me, this means combining work incentives and work opportunities with cash assistance and other income-related programs. Although the notion of a welfare trap is often overstated, duration on welfare is increasing.

Senator MOYNIHAN. It is increasing?

Ms. ALLEN. Oh, yes. We have the 1973 and 1975 AFDC surveys—well, it is hard to sort these things out. We could get into a discussion of turnover rates. The duration is increasing, although partly that is because the caseload growth has stopped, so some of that effect is statistical.

Senator MOYNIHAN. Let us have that for the record, won't you, when you get it?

Ms. ALLEN. We will put it in the record.

[The following was subsequently supplied for the record:]

The data shown in Table 1 tend to support the observation that duration on welfare is increasing. Between 1971 and 1975, the proportion of the caseload continuously on welfare for more than 3 years increased from 31 to 45 percent. The proportion on for more than 5 years increased from 18 to 26 percent. Part of the difference between the latest years and the earlier years may be attributable to the fact that in 1969 and 1971 the welfare rolls were still growing rapidly, a factor which, in itself, will reduce the average duration of those on the rolls at any point in time. However, the trend to longer durations has persisted during the more recent years of relatively level caseloads.

While Lyons and others point to considerable evidence of caseload turnover, there is no evidence that such turnover is increasing. Wiseman and Rydell also note that much of the observed case "opening" and "closing" are simply administrative churning including reclassification of cases from one category to another, minor adjustments in grants and notification problems.

TABLE 1.—AFDC FAMILIES BY LENGTH OF TIME SINCE MOST RECENT OPENING, 1969-75

	[Percent of caseload]			
	Up to 1 yr	1 to 3 yr	Over 3 yr	Over 5 yr
1969.....	32.2	31.2	36.6	22.8
1971.....	35.2	33.0	31.4	17.7
1973.....	30.2	34.5	34.7	17.8
1975.....	27.8	27.4	44.7	25.6

Source: 1969, 1971, 1973, and 1975 AFDC Surveys, U.S. Department of Health, Education, and Welfare. The 1975 numbers are from table 1 of the publication.

Ms. ALLEN. It is important that we turn this trend around first, by utilizing a combined work subsidy approach for the many poor two-parent families not on welfare and, second, by offering new work and training opportunities for the many poor already on welfare.

The administration's approach in its welfare reform proposal of last year tried to achieve exactly such a balance. I think it is essential that, as we consider alternatives to that approach, we recognize first that any welfare reform program, whether labeled "comprehensive" or "incremental" will influence the behavior of its intended recipients in one way or another and, second, that the direction and magnitude of these responses can be highly affected by the seeming minutiae of program design.

Details with respect to benefit levels, benefit reduction rates, benefit duration, program administration, and method of financing can also exert large influence on program costs, caseloads, and ultimate outcome.

We cannot eliminate all unintended consequences, but we can attempt to foresee them and in so doing, I believe—and I think that you believe—that we can develop a balanced program of reform which will maximize long-term improvements in the well-being and self-esteem of the poorest members of our society.

Thank you.

Senator MOYNIHAN. Well, thank you, Dr. Allen. That is extraordinary opening testimony.

Let me note that you skipped over, for purposes of compression, that Haverman and Bishop have done studies of the effect of the job tax credit and they seem to suggest that combining work opportunities with those particular kinds of advantages are more effective than sort of general stimulus methods—well, certainly they will when the economy is going the way that the economy is going right now.

I have a couple of questions. First of all, Richard Nathan at Brookings has said about welfare reform that food stamps were the reform. It happened in such a way that no one noticed it; it happened because an alliance developed in the Congress. People on the Agriculture Committees noticed that there were fewer farmers every year and if they were going to get anybody to vote for their programs to keep the price of food up, they had to find some allies in the cities, and they did—the people who wanted food stamps—and it happened like that.

Similarly, unnoticed, with no academic approval or literary sanction such as fires people to write profound articles about it, this committee, the Committee on Finance, commenced the single most dra-

matic change of income maintenance arrangements in this country since social security in the form of the earned income tax credit.

In the bill that the President signed last week—secretly, as though people would not find out about this benefit—I think about a quarter of American families with children will receive benefits from the earned income tax credit. It is somewhere between 20 and 25 percent. A big number.

Ms. ALLEN. It would be about that; yes.

Senator MOYNIHAN. About that, yes. A big number, so big that the administration may even want to conceal it. They do not want people to know it. They will feel that it is an extravagant, liberal administration after all, or they might find out that members of the Finance Committee are not monsters of reaction.

It is the closest thing we have to family allowances and it is extraordinary. Without anybody noticing it or approving of it, because it was not thought up you-know-where, it has come into existence. It was thought up here, which guarantees its disdain by academics.

A quarter of American families with incomes now receive this and we have boosted it very carefully in this last bill.

So we already have in place a universal system, food stamps and the earned income tax credit. I just make that point.

What I guess I have to ask you is, from what we know from these experiments, do you feel that the Congress would be warranted in going ahead with a further universal program at a very large cost, as we know, costs which always are underestimated—at least typically always have been—without some more experiment?

Ms. ALLEN. I do not think the Congress should enact a program like the SIME-DIME program.

Senator MOYNIHAN. About which we know something. You think we should enact a program about which we do not know anything, therefore we will not be to blame. That is a very important principle.

Ms. ALLEN. Even if we buy those findings from generous programs which have characteristics that we might not even carry over and extrapolate them to the sort of program that we are thinking about, we find that the predicted consequences are not mind boggling, that you might very well—I think that there are reasons to think that the experiments, particularly the marital stability findings, are high relative to what you would find even if extrapolated to more modest programs, and if you do extrapolate them to the kind of program we are talking about, they get to a level where they would certainly be acceptable if they could be detected at all.

I think it is important to remember that what was observed are marginal influences. Obviously there are very strong trends going on in society right now with respect to family splitting. Probably that will taper off, but at the moment it is very fashionable to split.

Senator MOYNIHAN. Right.

Ms. ALLEN. So you are talking about marginal influences. I think that—

Senator MOYNIHAN. A 60-percent increase over the control group where there is a lot to begin with—is that marginal, ma'am? That is not what I call marginal. You would not call that marginal. If you found a 60-percent increase in work effort, income and happiness in one of your job-training programs, you would declare a breakthrough.

**Ms. ALLEN.** Well, that is true, but it tapers off over the 3 years. Bob Spiegelman will talk about that. There is certainly some initial acceleration effect. I think that if you look at the expected results with regards to a program on the scale that the administration is talking about last year, particularly a program with a strong jobs component, I think that you would find that the predicted results were much, much smaller.

So I certainly do not think that one can use these results as an excuse not to do sensible, clearly conservative types of things with regard to changing the program.

There are so many things that we know we can do without the possibility of large-scale—

**Senator MOYNIHAN.** Well, let me just ask you. One of the things that was discouraging last year is that the President's program got nowhere. It was not voted on.

Do you have a feeling about why that was so? I mean, in terms of what you are going to do this year?

**Ms. ALLEN.** I think it was too big, but it was also so new. I think we made a lot of progress last year.

In the first place, we saw the emergence of a considerable consensus. We had several serious proposals on the table all of which looked—you know, they were smaller than PBJI, but they all had that balancing of components, jobs and training, modest incremental-type cash assistance reforms. There really was a lot of consensus building of that sort.

Also, as you observed, we achieved a lot last year and maybe because not a whole lot of attention was paid.

**Senator MOYNIHAN.** Last year?

**Ms. ALLEN.** The earned income tax credit. It was expanded, and in a very sensible fashion.

We also passed the targeted tax credit. Senate Finance got it through.

**Senator MOYNIHAN.** Yes, I hope we have got—you have a tremendous opportunity out there, just tremendous. The wages of welfare recipients are practically free to employers with the tax credit—the combined 50-percent tax credit plus your normal deduction, and you know, there are good workers to be got for nothing, and the worker gets paid and the employer does not have to put up the money.

**Ms. ALLEN.** We expect that that will be a very helpful first step in our direction.

The third important thing that happened was that we had a new CETA reauthorization which included a title II, heavily focused on the structurally unemployed and that is a big downpayment in terms of where we want to go in employment and training programs for low-income heads of family. There already is some language in that title about priority for welfare recipients. It is not as large an authorization as we need, it is not as closely targeted, but it is a big step in the right direction and it establishes important principles with regard to holding the line on wage rates, targeting, and interlinking with private sector opportunities.

We have the authorization for the new private sector initiative programs. We made a lot of progress last year down the path we hope to go and I think that a lot of consensus began to emerge toward the

end of last session. As Stu Eizenstat told you, we are currently developing incremental reform options. We hope in the next month to be up talking to you and to the other Senate and House leaders about what kinds of options make sense.

We have not yet taken our options to the President and obviously his choices will be heavily influenced by the decisions that are made on budget priorities, but we will be up with a package and we know generally what types of elements will be in that package.

Senator MOYNIHAN. Let me say that we are very much in your debt. We are extraordinarily happy that you opened these hearings the way that you did and the way the Department of Labor has always been in these things—coming forward in a very open and helpful way.

This subcommittee has a responsibility to the Senate that we have to discharge. You are an economist and a distinguished academician and you will follow me when I say that nuances that are very important to you will not seem very important to people who have other things on their mind and these sort of raw, rough findings come crashing in upon the sensibility, on the general perceptions of the Congress, and it is going to make it a harder program to sell now.

To say we do not want a program like SIME-DIME, no, we do not want that. Well, tell me how your program is not like SIME-DIME.

That is what we are going to have to go through, is it not? All right, we will.

I do agree, very much, that in an odd way we have been transforming this whole field incrementally and I think we have begun to see the emergence of a comprehensive incremental program.

You know, last year we increased the family allowance for a quarter of the low-income population. We provided huge incentives for hiring people with particular drifts of difficulties getting into the labor market. We provided all kinds of food stamps—for example, you read in the paper this morning that food costs are going up, but food stamp recipients have indexed benefits, and things like that.

So, au revoir. I thank you very much, Dr. Allen. I thank your associates; you have been most generous. We will see you in January.

Ms. ALLEN. Thank you very much, Mr. Chairman.

[The prepared statement of Dr. Allen follows:]

STATEMENT OF JODIE T. ALLEN, SPECIAL ASSISTANT TO THE SECRETARY OF LABOR,  
U.S. DEPARTMENT OF LABOR

Mr. Chairman and Members of the Subcommittee: I am pleased to appear before you today to survey some of the results of recent social science research relating to existing public assistance programs and potential welfare reforms.

My testimony today will focus on those aspects of welfare reform which relate to the responsibility of the Department of Labor to minimize the need for transfer programs by promoting the development and maintenance of a healthy labor market.

The ultimate purpose of income maintenance programs is to assure at least a minimally adequate level of subsistence for all Americans. For those families with potentially employable members, three factors are of primary relevance in determining the need for income assistance: (1) Is there an adequate demand for the services of the family's potential breadwinner? (2) Is the family willing to provide the services of these potential workers to the market and in what quantity? (3) How effective are employment and training programs in filling gaps on both the demand and supply side by direct augmentation of demand in the first instance, and by upgrading the skills and potential earnings in the latter?



THE RELEVANCE OF EMPLOYMENT TO THE PROBLEM OF LOW-INCOME FAMILIES

Before turning to each of these questions it is obviously relevant to ask whether there are indeed a sufficient number of low-income families with employable members to make their treatment in welfare programs a significant issue. Some have argued that since most persons on welfare are either aged, disabled or young children, employment programs have little relevance for welfare policy. There are several obvious things wrong with this argument. The first is that the aged and disabled are not really part of the current debate (except insofar as the rapidly growing ranks of the disabled include an increasing number of "discouraged workers" whose inability to find steady work translates eventually into physical and mental disorders.) As for the children, they do not live alone but, in most cases, with parents and relatives. And if these adults were able to become self-supporting, their children would be removed from dependency; longitudinal surveys, many of them summarized by David Lyon in a useful compendium, show that the great majority of low-income families with children have at least one worker (65 percent in 1975), that even female heads of families on welfare have a high probability of being employed at some time over a five-year period (69 percent in one study by Rainwater and Rehn).

Given this apparent potential for economic independence among low-income families, a prime concern of the Department of Labor in the area of welfare reform is the encouragement and maintenance of work effort by the low-income population, and the development of employment and training opportunities to sustain that work effort. It is useful to think of the problem of employment among the low-income population as having demand and supply side aspects. On the demand side, people must have jobs at which to work; on the supply side, they must be capable of doing the work. Both sides of the problem are important, and there are crucial ways in which the demand and supply sides are linked.

LABOR DEMAND EFFECTS

The traditional diagnosis of employment problems among the low-income population is in terms of inadequate skills, education, work experience and the set of incentives built into the transfer system that reduce the rewards from work. This view, while raising important problems, is incomplete because of the importance of demand and the availability of jobs.

On the demand side, the most efficient generator of job opportunities is a strong and growing economy. Many, although not all, of the problems of employing the low-income population are greatly reduced by increases in overall economic activity. A high performance economy causes employers to seek, train and upgrade workers who, during more slack periods, might be considered structurally unemployed. Two studies presented at recent sessions of the Brookings Panel on Economic Activity (1973 and 1977) find evidence that during periods of vigorous overall economic activity, not only are many more jobs created, but workers are upgraded from bad jobs to good jobs, thus improving the long-term stock of human skills available to the economy. Strong demand for labor induces improvements in supply in other ways as well. A recent study by Summers and Clark has found the important result that not only do participation rates increase during good times, as has long been known, but individuals drawn into the labor force stay there. The availability of jobs created by strong aggregate demand creates a long-term commitment to work on the part of those drawn into the labor force.

The negative side of these findings is that unemployment carries a large and enduring cost in the foregone development of human skills and work commitment.

A strong demand for labor also influences the efficacy of programs designed to increase the supply of labor skills among the low-income population. Many policies have been implemented in the last 15 years to improve the employment experience of low-income persons by augmenting their work skills and education. A simple but often ignored point is that the effectiveness of even the best administered training programs depends crucially on having jobs available for the trainees. A growing demand for labor also minimizes the likelihood that a program graduate simply takes a job away from another potential employee.

## LABOR SUPPLY EFFECTS

Consideration of training programs brings us naturally to a consideration of labor supply effects since the purpose of such programs is to upgrade labor skills and in turn increase both earnings and labor supply.

In a recent evaluation (November 1976), the National Council on Employment Policy concluded that the weight of the evidence over the last 15 years is that employment and training programs have had positive impacts and there is more proof of effectiveness for such programs than for any other major social welfare activity.

Transfer programs, such as welfare, exert an opposing influence on labor supply. The effects of transfer programs on the supply of labor are of concern for both macroeconomic and microeconomic reasons.

Transfer programs can affect the overall economy by worsening the relationship between unemployment and inflation. One mechanism is that, by raising the returns for not working relative to working, the programs may influence people to delay taking jobs in order to try to find a higher paying job or for other reasons. The delays may lengthen the duration of vacancies, raise the rate of wage increase, or force employers to use less efficient workers. In any case, the result can be less productivity, more inflation, and, ultimately, additional reductions in employment if policymakers attempt to restrain the inflation by inducing an economic slowdown. Documenting these effects is difficult, but some researchers such as Hammermesh, Marston, Feldstein and others claim to have found convincing evidence that for a given level of aggregate demand, the unemployment rate is higher partly as a result of increased welfare and unemployment insurance benefits.

The more traditional concern about program effects focuses on the individual's supply of labor, i.e., the work effort. What heightens this concern is the realization that relatively small program effects on work effort can translate into ineffective program outcomes. For example, if increases in benefit levels and benefit reduction rates to cause only a 10-11 percent reduction in hours worked by poor families, it could easily turn out that the Government might spend billions of dollars on benefit payments and have little effect on the families' incomes. Instead, most of the expenditures would offset reductions in earnings.

It was out of such concerns that OEO, and later HEW, funded several large negative income tax (NIT) experiments to try to quantify the amount of work reduction caused by alternative benefit levels and tax rates. The idea was to select a sample of families and, on a random basis, assign some families to a financial payment plan and other families to a control group. Then, by observing differences in work effort between families eligible for payments and control families, the precise impact of the payment scheme could be determined. Unfortunately, a learning curve had to operate in this new field. The highly publicized New Jersey experiment produced only tentative results because of simultaneous changes in the New Jersey welfare system, insufficient sample size and sample design problems. Other technical problems beset the Gary and rural experiments. For example, the rural experiment included only 105 families receiving payments and these were divided among farmers and nonfarmers in two different States enrolled in 5 different benefit plans. Questions were also raised as to whether the results observed in a three-year experiment could be generalized to a permanent program.

The Seattle-Denver income maintenance experiment (SIME-DIME) avoided many of the pitfalls of the earlier experiments. Its sample size was large—25 percent larger than the combined sample of all the other experiments. Operators exercised strict control over the interaction of the experiment with existing programs and included an additional sample of families eligible for payments over a five-year period. This experiment provides the best evidence to date on the effects of a negative income tax on work effort. While analyses of the rich data from the experiment will continue for several years, the effects on work have already been studied extensively. I will try to summarize the basic results.

In comparisons between families eligible for payments and control families, where account is taken of such differences as preenrollment hours, race, and normal income, the Stanford Research Institute and HEW analysts found the negative income tax plans produced the following reductions in hours of work:

Compared to the control group families, work effort reductions in families eligible for three-year payment plans averaged 7.6 percent among husbands, 16.7 percent among wives, 13.0 percent among females heading families. Among other family members aged 16-21 who do not head families of their own the

reduction in work effort was about 43 percent for males and 33 percent for females.

The average work reductions were usually substantially larger when comparing families eligible for 5-year payment plans with families in the control group; in families eligible for 5-year payments, husbands worked 11.4 percent less than control group husbands, wives worked 25.3 percent less than control group wives, and female heads worked 15.0 percent less than control group female heads; among other family members who are not family heads, the males worked 43 percent less than controls while females worked 60 percent less than controls. However, because of offsetting effects these higher average coefficients do not necessarily translate into higher costs for programs with low benefit levels.

One disturbing consequence of note is that when male nonheads become family heads through marriage, their labor supply reductions remain very high (a 55 percent reduction for those on the 5-year program and 28 percent for those on the 3-year program). As Richard West of SRI observes, "these estimated responses are so large that they imply that an NIT plan would have profound effects on the labor supply of the male heads of young families, even without considering the definite possibility of an even more pronounced response to a permanent program \* \* \*. The reduction in work effort by male nonheads who become husbands is clearly important. These males are reducing their work effort just at the time when they are undertaking family responsibilities. Not only is their response important in the current period, but the reduction in work effort may also have long-term effects on their labor supply behavior."

Although these numbers summarize what actually happened during the experiment, interpreting their relevance for policy purposes forces us to take account of a wide variety of factors which influence the results in various directions. On the one hand, the average generosity of the payment plans in SIME-DIME was generally much higher than plans usually proposed in welfare reform debates (although results cover the range of policy relevance). On the other hand, the experiment operated in only two cities, whose average wages and incomes are relatively high.

There were also various differences in program administration and payment calculations which must be accounted for. Families were required to report their income monthly whereas in most present AFDC programs income reporting requirements are far less frequent. Payments depended on income over the preceding 12 months rather than only the current month so that, with any given basic benefit level, actual payments were usually lower than in a comparable level AFDC program. On the other hand, beneficiaries were not subject to a work requirement and although they were interviewed periodically in their homes they did not have to report to a welfare office.

Keeping these complications in mind, an even more important line of analysis relates to how these effects change when programs with different basic benefit levels and benefit-reduction rates are considered. Because of the relatively large size of the combined SIME-DIME sample, SRI researchers have been able to divide work effort reductions into the components associated with the gains in income produced by the basic benefit and the component associated with the reduction in the workers net wage caused by the benefit reduction rate, or "marginal tax rate."

Using these calculations, efforts have been made to simulate the impact of alternative negative income tax plans on a national population. For example, an NIT plan offering an income guarantee of 75 percent of the poverty line and imposing a 50 percent tax rate would have caused participating families to reduce their work effort by about 10 percent. Raising the tax rate to 70 percent would produce a 16 percent reduction in hours worked among participating families.

While these results are interesting, many uncertainties remain. Chief among them, perhaps, is the question of how different the effects would be under programs more like our current welfare system. To examine this issue, we must look at studies of program experience, although such studies do not have the precision of controlled experiments. For the current welfare population of female heads of families, the estimates are generally consistent with SIME-DIME findings. Female heads of families are sensitive to benefit levels and tax rates. Studies by Garfinkel and Orr, by Williams, and by Levy all provide evidence that financial incentives influence the amount worked by mothers heading families.

Unfortunately, we have little evidence about the impact of food stamps and AFDC-UF on the work behavior of two-parent families. In fact, it is not even

clear why so few low-income, two-parent families participate in the two programs. In the case of AFDC-UF, many believe that program restrictions, such as the limit on work per month to 100 hours, the prior work experience rule, and strict work requirements are the cause of the apparently low participation. Other explanations include the possibility that eligibility for the program is overestimated as the result of income and asset undercounts in the survey data upon which they are based. However, it may be that many intact low-income families simply do not resort to welfare until their situation is desperate.

This last observation points up the importance of administrative factors and financial incentives for program administrators in determining welfare costs and caseloads. Wiseman, Lidman, Rydell and others observe that cost-conscious program administrators can often exert far more effective influence on the level of case openings and closings than can larger social and economic forces. Simplifying, streamlining and humanizing welfare administration and reducing fiscal burdens on hardpressed local governments are surely important goals in themselves; but, it is important to remember that such changes can have substantial effect on program participation in either direction.

One other measure designed to increase employment among welfare recipients deserves mention, and that is the work requirement, currently a part of the WIN and food stamp programs, which requires that participants accept suitable jobs or training opportunities if available or suffer a reduction in benefits. The effectiveness of this measure has recently been examined by researchers at Brandeis University. They conclude that while the work requirement can be used to maintain pressure on those who do not work, there is little evidence that under existing work tests such pressure results in the return of registrants to work. However, the authors suggest that the effectiveness of a work requirement could be strengthened by incorporating a more structured and better monitored job search component, and by adding a large-scale public employment component, so that sufficient jobs will be available for all those subject to the work requirement.

There is another important interaction between transfer programs and labor supply and demand which must concern us. This is the relationship of these factors to the changing structure of families and the implication of such changes in turn for transfer and employment policies. The most obvious phenomenon of note is the rise of the female headed family. Between 1968 and 1975 the number of women heading families with children increased by 60 percent. Labor force participation among women is lower than among men and while participation among women generally has been rising steadily, this has not been true among women heading low-income families. Whether this trend, with its obvious upward impact on welfare costs and caseloads, continues will depend in part on the structure of welfare programs themselves and in part on the number and types of jobs available to families and young workers in the next several years.

Several studies, such as those by Sawhill, Peabody and others, by Cherlin, and by Hoffman and Holmes have explored the relationship between employment of family breadwinners and family dissolution, a problem which you, Mr. Chairman, first brought to public attention over a decade ago. While these studies suffer from common problems of unobserved variables, uncontrolled factors and/or confounding influences, all of the studies find a direct relationship between the employment stability of husbands and family stability. Unemployment thus incurs dependency costs that go well beyond the immediate.

Presumably employment and training programs which upgrade the skills of and/or increase demand for family breadwinners exert a stabilizing influence on families. However, we have no direct evidence of this effect other than the frequently observed strong positive effect of increased incomes on family stability. Extending welfare programs to intact families does not appear to produce a stabilizing effect. Studies by Wiseman and Lidman find very high rates of family splitting among families on the AFDC-Unemployed Father program. Controlled experimental data from the SIME-DIME program indicate that providing cash assistance to two-parent families in the programs tested increased rather than reduced family splitting. Caution should be used in extending these latter findings to the current welfare system or proposed amendments thereto.

#### EXPERIENCE WITH EMPLOYMENT AND TRAINING PROGRAMS

This brings us to the question of how successful employment and training programs have been in alleviating the problem of insufficiencies in both labor supply and demand among low-income populations and what we might expect from expansions or innovations in such programs.

The first obvious source of information on promoting employment among welfare recipients is the WIN program. WIN was primarily a training program in its initial years of operation. Evaluations conducted during that period of the impact of WIN training on participants' employment experience were quite problematic. Taken in their best light, the results suggest that there may have been some modest gains as a result of WIN training, but the gain may not have been lasting.

The WIN program as modified in 1971 has received much more rigorous evaluation. The most comprehensive evaluation of recent WIN programs, involving three waves of interviews with a panel of WIN participants and a comparison group has yielded a number of important findings. The results indicate that WIN participants earn, on average, from \$330 to \$470 per year more than comparable nonparticipants.

Although it is difficult to attribute the gains to any particular service (since most participants received combinations of services), the least effective services were directly placement and education while institutional training, on-the-job training (OJT) and public service employment (PSE) were most effective (in cost-benefit terms and in raising participants' earnings.).

A major shortcoming of the evaluation is that the data collected do not extend beyond the point when most participants are still in subsidized OJT and PSE placements. Therefore it is not known whether the employment gains due to these subsidized placements result in later improved unsubsidized employment. Further, data are being collected to fill in this gap in our knowledge.

As noted earlier, several research and evaluation studies have cited the overriding importance of the labor market in determining the success of employment and training program participants. In other words, more than the characteristics of the participants themselves or the quality of services they have received, the availability or unavailability of jobs determines whether a participant will become employed and leave welfare. In view of this, planners have looked more and more at the feasibility of creating public jobs on a large scale to fill the job gap in the unsubsidized sector, both public and private.

In the early 1970's, a three-year experimental project was conducted in Vermont using subsidized public service employment and high levels of support services for welfare recipients and other low-income families as a means of helping them to permanent unsubsidized employment. Analysis of the follow-up data showed a relatively high post-subsidy placement rate, with very little subsequent attrition. Overall, for the entire group of clients, there was a substantial reduction in the total government outlay in welfare payments after they had gone through the program. An independent evaluation of the project concluded that public service employment is an important tool to be used in manpower programming for the low-income unemployed, including welfare recipients.

Also in the early 1970's, a series of welfare demonstration projects were carried out under the Emergency Employment Act with the aim of significantly reducing welfare caseloads through the creation of a large number of public service jobs for that population.

The projects provided additional funding to 12 jurisdictions in four States; in all, there were 7,000 participants in over 5,000 jobs created in about 400 agencies. In brief, an independent evaluation concluded that the projects were readily able to create a sufficient number of real jobs which met real needs; that participants' income were raised while in the projects and to some extent after their subsidized employment; that welfare recipients' performances on the jobs were comparable to that of regular workers; and that, overall, public service employment for welfare recipients is a feasible alternative to income maintenance. Sample data on participants indicated that the post-program welfare participation rate had dropped by 64 percent from the pre-program rate.

Another major job-creation demonstration project, called the Minnesota Work Equity Program, has recently been implemented in St. Paul and a number of counties in central and southern Minnesota. The project will determine if employment and training institutions in both urban and rural areas can create meaningful jobs near the minimum wage on a scale large enough to assure jobs for all welfare families with children. We hope to serve up to 6,000 persons from AFDC, General Assistance, and the food stamp rolls over a period of about two years. The scale of job creation in this project, in terms of saturation of the labor market, would exceed previous efforts. The project will also offer its clients the full range of manpower and employability development services, including day care, with special emphasis on thorough and continuous job search and placement in nonsubsidized jobs.

Under another contract, the Urban Institute and the American Institute for Research completed a research project entitled "The Feasibility of Large-Scale Countercyclical Public Job Creation. The objective of this project was to carry out a systematic identification of useful public work activities and provide a careful analysis of the feasibility of their large-scale expansion to provide jobs for large numbers of unemployed persons, including welfare recipients. The study identified 233 potential job-creation activities in 21 different program areas as likely candidates for large-scale expansion. The study also yielded information on the scale at which these activities might be performed on a national basis, costs involved at various levels of effort, proportions of capital and labor costs, the mix of skills required, the total employment-generating effect, and possible institutional or legal barriers and resource constraints. It was estimated that between 1.2 and 1.8 million low-skill jobs could be created.

Most encouraging are the results from the Supported Work program. Supported Work is a special job-creation program for persons who traditionally have had great difficulty finding or keeping employment. One of the target groups of Supported Work is women on AFDC, and in particular that subset of women without children under six who have had long-term dependence on welfare and demonstrated lack of successful employment. Results from a preliminary sample of AFDC enrollees and a rigorously constructed control group indicate that during the first nine months of participation, women in the experimental group earned \$3,150 more, worked 1,051 hours more, received \$1,147 less welfare income, and had total incomes \$2,007 higher than those in the control group.

Only 32 percent of the control group members were employed at any point during the nine months covered by the data; and had average earnings of only \$523. This indicated that Supported Work had not "creamed off" only job ready applicants, but had been successful in creating jobs for persons who do not succeed in the regular job market.

Encouraging information can also be drawn from observing the operation of title VI of the CETA program. Even though title VI is designed to serve countercyclical objectives, and is therefore not focused on the welfare population, substantial numbers of public assistance recipients did participate in the 1977-78 build-up to 725,000 jobs. In a recent study by WIN and CETA staff, a survey was conducted of 11 representative prime sponsors in the spring of 1978, six with above average AFDC participation. The study concludes that the one factor that can consistently explain a high degree of AFDC participation in title VI jobs is a consciously articulated policy decision by local officials to target employment opportunities on the most severely economically disadvantaged.

With respect to the possible effectiveness of attempts to encourage private sector employment of the structurally unemployed, Haveman and Bishop argue that selective employment subsidies, such as the WIN and the newly enacted targeted tax credits, are more potentially effective and less inflationary methods for stimulating increased employment of disadvantaged workers than general stimulus measures. They find supporting evidence in the experience with the 1978 New Jobs Tax Credit while recognizing deficiencies in the design of that measure.

One last interesting finding of a somewhat different sort is that of economist George Akerlof. Arguing from a theoretical perspective, Akerlof presents an important rationale for combining employment and training programs with income assistance programs. By distinguishing between employable and non-employable persons, employment and training programs allow higher benefit levels for those who cannot work and lower rates of marginal taxation for those who can work, thus increasing the social effectiveness of such programs.

#### CONCLUSIONS

Where does all this experience lead us? It does not mean that we should weaken our effort to help the poor. A decent society comes to the aid of its unfortunate members. What the results do tell us is that we must take care to provide help in the most effective way. We have learned the unsurprising lesson that the poor, like other people, act in a way which is consistent with their perceptions of their self-interest. In some cases, like the rest of us, they may misjudge that interest. But, we can count on the fact that sooner or later social program beneficiaries will respond to the incentives, good or bad which policy planners build into social programs. There are two ways we can respond

to this finding. We can try to distract the poor from pursuing their self-interest by confusing program designs or conflicting rhetoric. More sensibly and successfully, I believe, we can design our social programs to make them provide the kind of incentives which encourage low-income people to move towards achievement of the social and economic goals which, by and large, they share with the rest of society.

To me, this means combining work incentives and work opportunities with cash assistance and other income-related programs. Although the notion of a welfare trap is often overstated, duration on welfare is increasing. It is important that we turn this trend around first by utilizing a combined work-subsidy approach for the many poor two-parent families not on welfare, and second by offering new work and training opportunities to the many poor already on welfare.

The Administration's approach in its welfare reform proposal of last year tried to achieve such a balance. I think it is essential that as we consider alternatives to that approach, we recognize that any welfare reform program whether labeled "comprehensive" or "incremental" will influence the behavior of its intended recipients in one way or another and further, that the direction and magnitude of these responses can be highly affected by the seeming minutiae of program details with respect to benefit structure, program administration and method of financing. We cannot eliminate all unintended consequences, but we can attempt to foresee them. In so doing, I believe that we can develop a balanced program of reform which will maximize long-term improvement in the well-being and self-esteem of the poorest members of our society.

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QUESTIONS SUBMITTED BY SENATOR MOYNIHAN TO MS. ALLEN AND HER RESPONSE TO THEM

*Question 1.* Would it not be expected that AFDC recipients in Supported Work projects would work more hours and earn more money than a control group, since, by definition, the former are in an employment project?

*Answer.* It is certainly true that one would expect higher earnings among those participating in an employment project than those who do not. However, what is so encouraging about the Supported Work experience with women on AFDC is the extent of voluntary and successful participation in an employment project by a group who have a secure alternative income from public assistance. Earnings cause reductions in welfare benefits for this group, and therefore the participation and successful stable employment experience in Supported Work is evidence that if jobs are made available, welfare recipients will and can work. It is correct that the evidence presented so far does not indicate whether the work experience provision by Supported Work has enduring benefits in terms of improved unsubsidized employment. However, it is not necessarily true that if there were



no substantial gains in post-program earnings the program would be considered a failure. While in Supported Work participants produce outputs whose value can offset some or all of the costs involved in creating the jobs while, at the same time, welfare savings are produced and recipient incomes improved.

**Question 1a.** Is it not true that the preliminary results of the Supported Work project showed the AFDC recipients were more difficult to place in jobs and earned lower wages than other groups?

**Answer.** Because Supported Work represents the first significant job opportunity for many of the AFDC participants, they have tended to stay in the program significantly longer than have the other target groups. The rates of voluntary departure and of firings among the AFDC group are much lower than the other target groups. Therefore during the early stages of the project, fewer AFDC participants have been leaving to find alternative jobs. However, of the number of people who eventually depart from the program the proportion of the AFDC group that are placed in jobs compares favorably with the proportion placed from the other groups. The Manpower Demonstration Research Corporation has published the following figures:

Percentage of departures that were placements to permanent employment: AFDC, 29.2 percent; ex-offenders, 26.3 percent; ex-addicts, 19.7 percent; and youth, 24.4 percent.

Program operators have reported that the AFDC participants are less likely to find jobs by themselves and are more reliant on program placement services. The above figures indicate, however, that with placement assistance the AFDC group can be placed in jobs at rates similar to other target groups.

The figures on job placement wage rates do indicate, as you have noted, a significantly lower wage for placements among the AFDC participants, \$3.02 for AFDC as compared to \$3.99, \$3.87, and \$3.05 for ex-offenders, ex-addicts, and youth respectively. This difference may reflect some difference in previously accumulated work skills among the different groups, but undoubtedly it also reflects the continuing discrimination which females face in the labor market. However, a comparison of the placement wage rates among the different target groups is not the only relevant comparison. Because Supported Work is intended to improve employability, the crucial comparison is between the wage rate obtained in post-program experience by each target group *as compared to* the wage obtained by their respective control group. While Supported Work and hence job creation may not be able, by itself, to end discrimination in the labor market, it may still be able to improve individual employability. The judgment on Supported Work's relative effectiveness in raising wages of the different target groups must wait for future data.

**Question 2.** The newspaper last week reported "widespread abuse" of the CETA public service jobs program and the retention by DOL of a "200 member investigation staff" to uncover fraud and abuse. Does this give grounds for confidence in the integrity of a large new public employment program as part of welfare reform?

**Answer.** There is no evidence that the CETA program is subject to "widespread abuse." Compared to its size and the number of separate entities involved in the administration of the program, the numbers of reported cases of abuse is very small. There are over 30,000 organizations involved in carrying out some aspect of the program. In addition there are several million persons participating in the program. To some extent the recent news stories about program abuse simply reflect a recognition that CETA is now a newsworthy governmental program. Not only in the recent past were there no stories about CETA program abuse, there were few if any news stories about CETA pro or con. The expansion of the program since 1977 has changed all that.

It is of course impossible to run a program of this magnitude without some level of error. However as compared to other major Federal programs of equal size, the level of abuse reported in CETA is still small. The AFDC, Medicaid and Food Stamp programs have long had higher levels of reported abuse by both providers and recipients.

Despite this relatively good record, the Department is committed to improving program management and reducing misuse of funds to a minimum. The announcement of an expansion and consolidation of investigative staff is a reflection of this commitment rather than an acknowledgement of serious program abuse. The emphasis of this new effort will be on prevention—with the goal of identifying and correcting weaknesses in the system before they lead to problems. We believe that the expansion of the investigative staff along with the establishment

of the Congressionally mandated Office of Management Assistance for CETA will insure a high level of program integrity.

In addition, as noted in response to Question 4, we are planning a major program of field testing to insure that adequate management systems are developed prior to national implementation of welfare reform-related employment program.

**Question 3.** Because of the current earnings disregard—the \$30 plus  $\frac{1}{2}$  provision—we recently calculated that it would take a full-time job paying \$6.00 per hour before a typical welfare mother in New York City would be earning enough to leave welfare.

Are such generous disregards necessary?

Wouldn't welfare recipients take jobs that provided only a little more than what they could obtain from welfare? Are you concerned that we may have created a welfare system that makes it so difficult to escape dependency?

**Answer.** To the extent that our current welfare system encourages dependency the fault would not seem to be with overly generous "earnings disregards." The \$30 and  $\frac{1}{2}$  provision imposes an effective benefit reduction rate of 67 percent on earnings in excess of \$30 a month. The cumulation of the Food Stamp benefit reduction rate raises this to about 75 percent. In other words a recipient would net only 25 cents for each dollar earned, ignoring work-related expenses. Numerous studies—including the income maintenance experiments, cross-sectional analysis and comparisons of work effort among the low income population in states with differing AFDC programs—demonstrate that benefit reduction rates this high, particularly in conjunction with relatively generous welfare associated benefits, can result in substantial disincentives to work particularly among persons with relatively low wage rates.

It is true, however, that overly generous work-related expense reimbursements can have the effect of reducing effective benefit reduction rates and hence extending welfare eligibility to some recipients with relatively high earnings. For this reason the introduction of controls on work-related expense deductions would be desirable.

The real source of the problem is, of course, that as the result of humanitarian concerns, many states now provide welfare recipients with a package of benefits including cash assistance, Food Stamps, housing assistance, Medicaid and other special needs benefits which far exceed the value of their potential earnings. By the same token many welfare recipients have higher standards of living as the result of welfare-related benefits than many non-welfare poor and near-poor families can afford on their own. It is unreasonable to assume that welfare recipients will be willing to relinquish these benefits for a lower net income from work particularly since working may be inconvenient, uncertain in duration and expensive in itself.

There is no easy solution to this problem. Four courses of action suggest themselves: (1) Improving the benefits available to non-welfare families. Medical benefits are probably the most important item needing equalization since few Americans at any income level and hardly any low-wage workers have access to medical coverage approaching that provided by Medicaid in the more generous states. However, since the average cost of Medicaid coverage is extremely high in such states it is unlikely that comparable coverage could be extended generally; (2) Restricting the cost of welfare-related benefits—again medical benefits are the prime target. The introduction of cost controls and incentives for efficient utilization by both providers and users are essential to reduce the cost of Medicaid coverage to a level relatively comparable to that which might be made available to low-income working families; (3) Resisting further efforts to associate eligibility for other benefit programs with welfare eligibility; (4) Providing work experience and training to welfare recipients to raise their potential wages and hence reduce the cost to them of leaving welfare; and (5) Reorient the welfare system so that the primary source of assistance for the employable poor comes from employment rather than welfare.

**Question 4.** What would you think of the desirability and feasibility of conducting another round of experiments intended to test the effects of a combined program of income maintenance and jobs such as the Administration proposed last year? Or do you think that present data and analyses speak clearly to this prospect? While I have no difficulty accepting the proposition that the Administration's plan differed in significant respects from those tested in the income maintenance experiments, I am not yet aware of any solid evidence as to the effects of those differences and wonder if you think we should consider finding out before embarking on a major overhaul?

Answer. We are satisfied that the income maintenance experiments and other studies provide us with sufficiently reliable evidence with respect to the likely national demand for employment and training assistance among the low income population, the effect of alternative welfare program designs (i.e., benefit levels, reduction rates and other features) on that demand and the likely impact of such programs on welfare participation and on the incomes of poor families. Experience with various employment and training programs which have served comparable populations in whole or in part—including the stimulus buildup of Title VI of CETA, WIN, Supported Work and Community Work and Training Programs, has also convinced us that, with sufficient planning and emphasis, such programs can be efficiently and effectively administered. However, we believe that a well-planned program of pilot projects testing the joint work-welfare concept are essential to determine the appropriate type and mix of job and training placements, measure recipient impacts, work out administrative arrangements with other involved agencies and the private sector and develop program models suitable for implementation in areas with varying demographic, institutional and economic characteristics. We are currently planning to begin implementation of such projects this year in order to provide useful findings in time for regular program buildup beginning in 1981.

Question 5. On page 30 of your prepared testimony, you observe that "Although the notion of a welfare trap is often overstated, duration on welfare is increasing." Would you please cite the data that support this conclusion.

Answer. The statement that "duration on welfare is increasing" is based upon the data shown in Table 1. Between 1971 and 1975, the proportion of the caseload continuously on welfare for more than 3 years increased from 31 to 45 percent. The proportion on for more than 5 years increased from 18 to 26 percent. Part of the difference between the latest years and the earlier years may be attributable to the fact that in 1969 and 1971 the welfare rolls were still growing rapidly, a factor which, in itself, will reduce the average duration of those on the rolls at any point in time. However, the trend to longer durations has persisted during the more recent years of relatively level caseloads.

While Lyons and others point to considerable evidence of caseload turnover, there is no evidence that such turnover is increasing. Wiseman and Rydell also note that much of the observed case "opening" and "closing" are simply administrative churning including reclassification of cases from one category to another, minor adjustments in grants and notification problems.

TABLE 1.—AFDC FAMILIES BY LENGTH OF TIME SINCE MOST RECENT OPENING, 1969-75  
(Percent of caseload)

	Up to 1 yr	1 to 3 yr	Over 3 yr	Over 5 yr
1969.....	32.2	31.2	36.6	22.8
1971.....	35.2	33.0	31.4	17.7
1973.....	30.2	34.5	34.7	17.8
1975.....	27.8	27.4	44.7	25.6

Source: 1969, 1971, 1973, and 1975 AFDC Surveys, U.S. Department of Health, Education, and Welfare. The 1975 numbers are from table 1 of the publication.

Question 6. We heard testimony that suggests that a certain amount of earned income "disappeared" when participants in the income maintenance experiments were required to report it. Can you sustain, refute or otherwise shed light on this phenomenon?

Answer. We are not familiar with the exact testimony to which you refer. There are however a couple of studies related to the experiments which bear on this subject.

1. Halsey and others<sup>1</sup> compared information on preexperimental income of AFDC recipients subsequently enrolled in the experiment with the income which they had been reporting to the AFDC office. He found that only about 60 percent of earned income was actually reported to welfare authorities and that 25 percent of those who report no earnings to AFDC do, in fact, have some. These findings do not, however, pertain to the experiment itself.

<sup>1</sup> Harlan Halsey, Mordecai Kurz and Robert Spiegelman. *The Reporting of Income to Welfare: A Study in the Accuracy of Income Reporting*, Stanford Research Institute, Research Memorandum 42, August 1977.

2. Comparisons of income reported to the experiment to information on income available from tax authorities and other public agencies indicated that income reporting to the experiment was generally reliable compared to information available from those sources, and was far better than income reporting to AFDC.<sup>2</sup> The difference is attributed to the more rigorous and frequent income reporting requirements imposed in the experiments as compared to most AFDC programs.

3. A more recent and as yet unpublished study by Halsey<sup>3</sup> finds that, within the experiment the amount of earnings underreporting is *positively* related to the level of the benefit reduction rate. This effect is expected since high benefit reduction rates raise the cost (in terms of lost benefits) to families of reporting income accurately.

Senator MOYNIHAN. And now, although the committee has run over a bit on time, there are two persons who are still to testify this morning.

We have the distinct honor and pleasure to have testifying before us Dr. Irwin Garfinkel who is the Director for the Institute for Research on Poverty of the University of Wisconsin.

I remember the discussions at which we decided to establish that institution, Dr. Garfinkel and I recall the observation that whether the war on poverty was doing anything for the poor, it would help the professoriate greatly, and I think both things have been the case.

In particular, one of the real achievements, I think, of OEO was to establish your center and it has been a source of high-level, highly-competent work from the beginning and we welcome you.

If you have testimony, go right ahead, just as you wish, Doctor.

Mr. GARFINKEL. Thank you. I have a written statement that I would like to submit in full and I will try and summarize it.

Senator MOYNIHAN. Fine. We will put that into the record as if read.

#### STATEMENT OF IRWIN GARFINKEL, DIRECTOR, INSTITUTE FOR RESEARCH ON POVERTY, UNIVERSITY OF WISCONSIN

Mr. GARFINKEL. Thank you for the opportunity to testify on what we have learned from the income maintenance and jobs experiments and the implications thereof for welfare reform. I have a written testimony which I would like to submit for the record.

We have learned a great deal from social experiments. Indeed we lead the world in social science experimentation. Academics from other countries whom I have met admire our pioneering efforts in this area. I hope you were only kidding when you said that you would hesitate to do any more experimentation. I would agree with Jodie Allen that it is very appropriate to do experimentation before we launch our new programs.

Senator MOYNIHAN. I assure you, Dr. Garfinkel, that I was only kidding. Your appropriation is safe.

Mr. GARFINKEL. Today I want to confine my remarks to only a few of the many things we have learned from the experiments about the effects of income maintenance and jobs programs on work effort and marital stability. Then I will talk briefly about the policy implications of these findings.

<sup>2</sup> Harlan Halsey, Bina Muraka and Robert C. Spiegelman, *The Seattle and Denver Validation Study*, Stanford Research Institute, June 1976.

<sup>3</sup> Memorandum on "Effect of Program Marginal Tax Rate on Income Reporting" from H. Halsey to R. Spiegelman, February 9, 1978, Stanford Research, Inc.

As you know, the income maintenance experiments were designed primarily to find out how such programs affected the work behavior of low-income people. What did we learn?

We learned that simultaneously increasing the unearned income and reducing the rewards for work of low-income people will induce them to work less. We also learned that how much less low-income people work depends upon how much we increase their income and how much we decrease their net wage.

To economists, this was no surprise. Indeed, conventional economic theory predicted it—I might say that common sense suggests the same things as well.

We learned that how much less low-income people work depends upon what kind of person they are, that is, upon what group they belong to and what is therefore expected of them by society.

The work reductions of married women and female heads are much larger, in percentage terms, than those of married men. Again, this is not surprising. Married men are expected to work whereas for married women and female heads, it is equally legitimate to take care of children and do housework.

Finally we learned something about magnitude of these effects. For example, on the basis of results in the Seattle-Denver experiments, we can now say that on average husbands participating in a negative income tax program with a poverty line guarantee and 50-percent tax rate will work about 7 percent less than they would in the absence of a program. Similarly, if the tax rate is increased to 70 percent, the reduction in work will increase to about 12 percent. Comparable percentages for wives are 22 percent and 28 percent.

Although the experiments were not designed to measure the effects of income maintenance programs on marital stability, we now find, to our great surprise, that the experimental negative income tax programs seem to increase marital splits. I say "seem" only because I remain skeptical of, and puzzled by, these findings. I have not found any fatal flaw in them, although I must admit that I have tried a bit. Most experts believed, prior to the experiments, that aiding two-parent families would decrease marital splits. They believed that aiding only one-parent families increased splits because it forced the man to abandon the wife and child in order for the wife and the child to become eligible for aid. If intact families were also eligible for aid, so the argument went, the man would not have to split.

Assuming that we accept the results of the experiments as being correct, why were we experts so wrong? We do not know yet for sure. My friend and colleague at the Institute, John Bishop, has speculated that providing welfare aid to intact families undermines the role of the male head. On the other hand, analysts at the Stanford Research Institute have speculated that income from the experiment is more attractive to wives considering divorce and separation than equivalent amounts of income from welfare. You, yourself, earlier suggested that perhaps it is simply the greater knowledge of the availability of benefits that was automatically provided by virtue of participating in the program that led—

Senator MOYNIHAN. Could I also, Dr. Garfinkel, say that when you ask why were the experts wrong, I was a quasi-expert once and in a

paper I wrote in 1967 on this subject which Gilbert Steiner retrieved for something he wrote recently, I said what was then the case—and is the case now. In 1967, I said there is not 5 cents worth of evidence that the AFDC program leads to marital breakup.

I did not say it did not; I just said there was no evidence that it did.

Mr. GARFINKEL. There is some evidence that it does, but it is not, in my judgment, terribly good evidence and there is some other evidence that it does not.

Senator MOYNIHAN. In 1967 when we began seriously talking about these things—one of the reasons I wanted to have these hearings is that I have tried to keep what we say in some reasonable compliance with our evidence—I said very explicitly that there was not 5 cents worth of evidence that there is this marital effect.

But try to tell that to a Presidential speechwriter.

Mr. GARFINKEL. I remember a speech in 1969 which claimed—

Senator MOYNIHAN. We have actually—and I would like to put in the record—a very fine study of this matter done for us by the Congressional Research Service which begins—this particular doctrine begins at the National Conference of Social Work in 1912 with Mary Richmond. But in any event, the profession has been rather sloppy about this. There was no need ever to go around letting this be said. There was no need not to have challenged it—I am not speaking to you, but it has been clear for a very long while that this is not a research finding but it has been allowed nevertheless to stand unchallenged.

Go ahead, Doctor.

[The document referred to follows:]

[From the Library of Congress]

#### UNEMPLOYMENT AND WELFARE CASELOADS

(By P. Royal Shipp, Senior Specialist in Social Welfare (Income Maintenance); Vee Burke, Analyst, Social Legislation, with the assistance of Carmen D. Solomon, Reference Assistant, Education and Public Welfare Division)

##### SUMMARY

Studies and data series confirm a positive link between the unemployment rate and welfare caseloads. However, measurements of the size of the relationship differ sharply. Unemployment has more impact on the programs of AFDC for Unemployed Fathers and General Assistance than on regular AFDC because many welfare mothers have weak ties to the labor market. It appears that the divergence of AFDC case openings from the unemployment rate in 1963-69 was an aberration.

##### I. Introduction

The relationship between unemployment and welfare caseloads and expenditures is difficult to unravel. A number of studies, some still underway, have attempted to answer this question either directly or as part of a broader inquiry into the reasons for change in welfare caseloads.

Some welfare programs appear to be sensitive to changes in unemployment, namely, Aid to Families with Dependent Children for Unemployed Fathers (AFDC-UF), General Assistance, and the portion of the food stamp program comprised of persons who do not also receive cash public aid.<sup>1</sup> Since needy two-parent families are eligible for AFDC-UF and General Assistance in some States, as are some other employable persons; and since virtually all persons

<sup>1</sup> This paper does not deal with food stamp caseloads. However, they reached an annual average peak of 18.6 million persons in FY 1976 (October 1, 1975-September 30, 1976), reflecting the high male unemployment rate (for those at least 20 years old) of 6.3 percent in July 1975-June 1976. The U.S. Treasury pays the full cost of bonus food stamps, plus 50% of State local administrative costs.

with low earnings and assets are eligible for food stamps, it is reasonable to expect caseloads of these three programs to respond to changes in employment conditions. In the total welfare picture, however, these programs are overshadowed by the "regular" AFDC program, which is for needy children without an able-bodied father.

For the regular AFDC program, the nation's largest cash welfare operation, the evidence concerning caseloads and unemployment is less clear. Table I shows that regular AFDC constitutes 95 percent of total AFDC recipients and 88 percent of combined AFDC-General Assistance enrollment. State funds in FY 1977 paid 37% of the cost of the AFDC program; local funds (11 States only), 9 percent; and Federal funds, the remaining 54 percent. Benefits to regular AFDC cases totaled \$10.1 billion in 1977, compared to \$0.6 billion for AFDC-UF families. The dominant size of regular AFDC makes it the most significant program to consider in judging the impact of caseload changes on State budgets. Table 2 shows the growth of regular AFDC recipient and family numbers and the changes in AFDC-UF enrollment from 1960-1977.

Table 1.—Welfare recipients, December 1977

	<i>thousands</i>
AFDC-Regular .....	10, 198
AFDC-UF .....	565
General Assistance .....	819
<b>Total .....</b>	<b>11, 580</b>

SOURCE: Department of Health, Education, and Welfare.

TABLE 2.—I. NUMBER OF AFDC AND AFDC-UF RECIPIENTS AND CASES AS OF JUNE OF EACH YEAR, 1960 TO PRESENT

Month, year	AFDC cases (families)	AFDC recipients	AFDC-UF cases	AFDC-UF recipients	AFDC-UF States <sup>1</sup>
June 1960.....	794, 400	3, 015, 000	( <sup>2</sup> )	( <sup>2</sup> )	( <sup>2</sup> )
June 1961.....	878, 300	3, 369, 000	23, 418	112, 252	6
June 1962.....	944, 000	3, 658, 000	49, 260	227, 637	15
June 1963.....	962, 600	3, 850, 000	48, 110	270, 207	15
June 1964.....	1, 014, 100	4, 126, 000	61, 286	354, 950	18
June 1965.....	1, 062, 800	4, 306, 000	59, 020	344, 741	18
June 1966.....	1, 090, 700	4, 472, 000	52, 399	309, 433	21
June 1967.....	1, 208, 000	4, 977, 000	63, 900	368, 000	22
June 1968.....	1, 383, 000	5, 609, 000	62, 600	348, 000	21
June 1969.....	1, 661, 000	6, 577, 000	64, 700	353, 000	25
June 1970.....	2, 158, 000	8, 292, 000	99, 100	519, 000	23
June 1971.....	2, 747, 000	10, 224, 000	158, 000	794, 000	26
June 1972.....	3, 039, 000	10, 917, 000	127, 200	595, 700	24
June 1973.....	3, 135, 000	10, 907, 000	104, 400	485, 500	24
June 1974.....	3, 187, 000	10, 756, 000	88, 300	402, 800	26
June 1975.....	3, 478, 000	11, 300, 000	111, 900	494, 100	25
June 1976.....	3, 554, 000	11, 236, 000	146, 500	642, 500	28
June 1977.....	3, 571, 000	11, 029, 000	145, 100	645, 500	28

<sup>1</sup> "State" includes District of Columbia and Guam.

<sup>2</sup> Program established in 1961.

Sources: U.S. Department of Health, Education, and Welfare. "Statistics on Public Assistance," Department of Health, Education, and Welfare, 1967-76. "Social Security Bulletin," October 1960-66.

The bulk of the evidence about unemployment rates and *regular* AFDC, indicates that changes in economic conditions *do* have an impact on the size of this program. It is a smaller change, may occur with substantial lags, and is difficult to separate from other influences on caseload size.

The basic reason for the more obscure and weaker relationship of unemployment rates to regular AFDC is that a smaller proportion of its family heads are regular members of the work force. Table 3 shows that since 1967 the percentage of AFDC mothers who worked in a survey month has increased from 13.7 percent to 16.1 percent in 1975. Although difficult to estimate, it is likely that double this proportion, or more, work at *some time during the year*, but their work often is sporadic. The 1975 AFDC Study reported that 53.5 percent of AFDC mothers were known to have a usual occupation; only 7.3 percent were found never to have been employed, and it was unknown whether another 3.9 percent ever had worked.

TABLE 3

	Percent of AFDC mothers—	
	Working during month	Who worked some-time during year <sup>1</sup>
1967.....	13.7	(3)
1969.....	14.5	(3)
1971.....	13.9	26.6
1973.....	16.1	26.6
1975.....	16.1	26.2

<sup>1</sup> These data were obtained by adding the percentage of mothers who left their last job within a 1-yr period to the percentage of mothers currently employed (in survey month—full time, part time).

<sup>2</sup> Data unknown.

<sup>3</sup> 29.6 percent of mothers in May 1969 either worked or had left a job sometime in the previous 17 mo.

Note: The Bureau of the Census indicates that 59 percent of families receiving AFDC had some earnings in 1975.

Source: "Aid to Families with Dependent Children (AFDC) Study," U.S. Department of Health, Education, and Welfare, SSA, Office of Research and Statistics, 1967-75.

Table 4 shows that the proportion of female family heads and subfamily heads with children under 18 years old who worked rose by 1.4 percent in 1971-1976, from 63.1 to 64 percent. However, among *poor* mothers, the proportion who worked declined by 8 percent, from 41.3 to 38.0 percent. Among *nonpoor* mothers, the proportion who worked rose by 5.9 percent, from 78.4 to 83 percent.

## II. Kinds of studies

One problem with basing policy decisions on studies of the relationship between unemployment and welfare is that the various studies have reached different conclusions. Generally, they have been consistent in reporting a correlation, but their measurements of it differ greatly. There are two main reasons why data from such studies sometimes are difficult to reconcile. These are:

TABLE 4.—PERCENT OF FEMALE FAMILY HEADS AND SUBFAMILY HEADS WITH CHILDREN UNDER 18 YRS WHO WORKED IN THE YRS 1971-76

	Poor	Nonpoor	Total
1971.....	41.3	78.4	63.1
1972.....	42.1	78.5	63.1
1973.....	43.6	80.3	65.0
1974.....	43.7	79.9	65.0
1975.....	39.0	82.4	64.1
1976.....	38.0	83.0	64.0

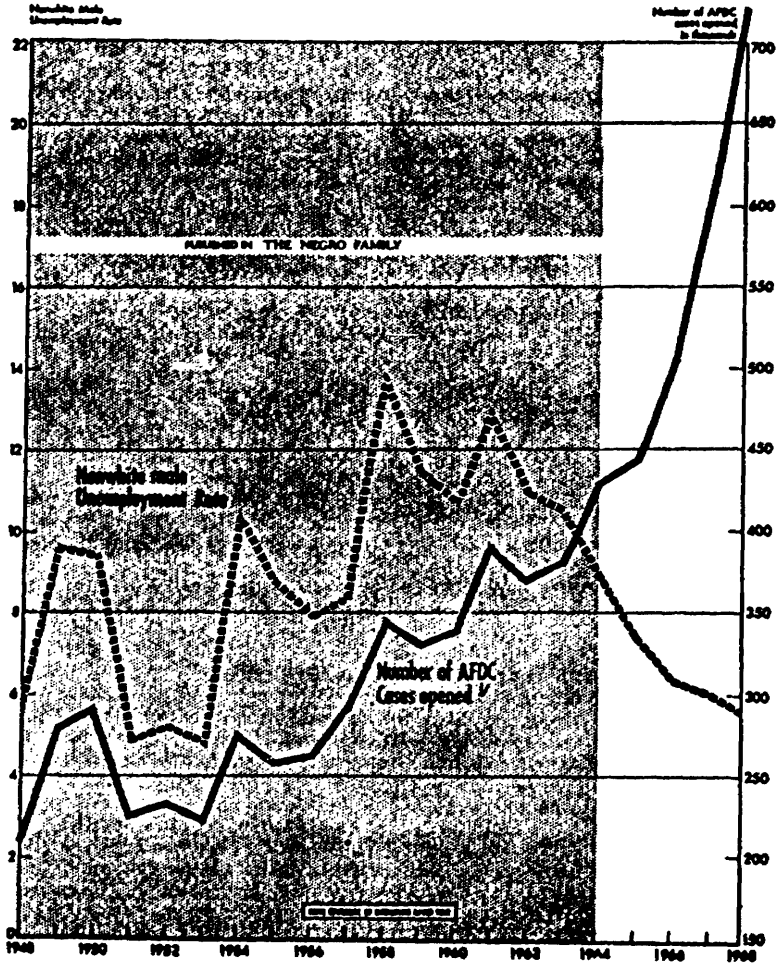
Source: Characteristics of the Population Below the Poverty Level, Current Population Reports (annual), 1969-76. U.S. Department of Commerce, Bureau of the Census.

1. The relationships are complex and the methodologies used different. The studies have used sophisticated econometric and microsimulation modeling in order to isolate unemployment from other factors affecting caseload change. Differences in the models includes: (a) great variability in the extent of aggregation of data (some have used national data, others aggregate State data, and others micro data), (b) different data bases, (c) nonuniform specification of explanatory variables, and (d) lack of uniformity of the underlying theory of caseload dynamics. As a result, the estimates derived from the studies differ from each other. With one notable exception, they have consistently found a rise in unemployment to correlate with growth in AFDC case numbers, but there are wide variations in size and timing of effect.

The exception was a 1965 study by Daniel Patrick Moynihan, then an Assistant Secretary of Labor and Director of the Office of Policy Planning and Research. This study, entitled "The Negro Family: The Case for National Action," reported that after 1962, even though unemployment rates fell, new AFDC case openings climbed sharply. The Moynihan report plotted nonwhite male unemployment rates against the number of regular (non-UF) AFDC cases opened. Chart 1 reprints this graph, as it was updated in 1969, showing a continued divergence of the nonwhite male unemployment rate and the number of non-UF AFDC cases openings. However, after 1969 the old positive relationship between unemployment and case openings was restored (see Chart 5, p. 17, prepared by CRS).



**CHART 1**  
**CASES OPENED UNDER AFDC**  
**COMPARED WITH UNEMPLOYMENT**  
**RATE FOR NONWHITE MALES**



By flow of funds, some cases may appear which otherwise would not appear in full of conditions in another state before the program is completed.

Reprinted from *The Relationship of Employment to Welfare Dependency*.  
 Urban Affairs Council, March, 1969.

2. The studies had different purposes, covered different periods, and were set up to answer different questions. In general, the studies can be grouped into three general classifications, those designed to provide:

a. *Greater understanding of the dynamics of caseload growth or expansion.*—This question is one that has been asked at least over the past 15 years. Until the early 1960's, growth in the caseload was not considered to be much of a problem—largely because relatively little growth had occurred. For example, in the first five years of the 1960's, the AFDC caseload grew about 25 percent (see Table 2, p. 3). In the 5 years from 1968 to 1973 the caseload more than doubled, a growth rate of 127 percent. The caseload explosion during the late 1960's and early 1970's aroused concern, especially since it persisted in the face of falling unemployment, defying usual relationships. It appeared that the historical correlation between welfare caseload and unemployment had collapsed, as the Moy-nihan graph (Chart 1) indicated.

b. *The impact of the 1974 recession on the poor.*—Studies done at or financed by HEW attempted to predict and to estimate the performance of income transfer programs during the 1974 recession to determine whether they softened the impact of the recession on the poor. One major question was the impact of unemployment on income transfer programs.<sup>2</sup>

c. *Predictions of caseload and cost size for planning and budget purposes.*—The Federal budget must be printed 1½ years before the end of the budget year and, thus, programs must be estimated even further in advance.

In recent years HEW has been criticized for both over- and underpredictions of future budget costs. Several of the studies done or underway have been addressed to this problem.

The last two sections of this paper present, first, findings of various studies, and second, analysis, comments, and conclusions.

### III. Findings of studies

A. *AFDC-UF and General Assistance.*—Since these programs, at least in some places, are open to unemployed males barred from the regular AFDC program, their enrollment would be expected to respond most clearly to changes in unemployment rates. Chart 2 shows that this is the case. For months between January 1970 and December 1977, the chart presents:

Male unemployment rate (for those 20 years and over).

General Assistance cases.

AFDC-UF cases.

Non-UF AFDC cases.

(The male unemployment rate in Chart 2 is the proportion of the civilian labor force that consists of unemployed males aged at least 20. This differs from the insured unemployment rate, which is the percentage of the population qualified for unemployment insurance who are unemployed. Unemployment rates in this paper relate to the total civilian labor force and are not insured unemployment rates.)

The chart shows that annual peaks of male unemployment rates were reached in February 1971, 1973, 1974, and 1977; in January, 1972 and 1976; and in March, 1975. In all of these years AFDC-UF peaked in March. However, AFDC-UF case numbers did not rise proportionately as much as the unemployment rate in this period. The male unemployment rate in February 1977 was up 38 percent from its January 1972 level, but AFDC-UF cases were up 17 percent, totalling 165,665. Moreover, in March 1975, when unemployment rose to 7.8 percent, highest monthly rate in this period, the number of AFDC-UF cases was sixth lowest for March in this 7-year period, numbering 120,193.<sup>3</sup>

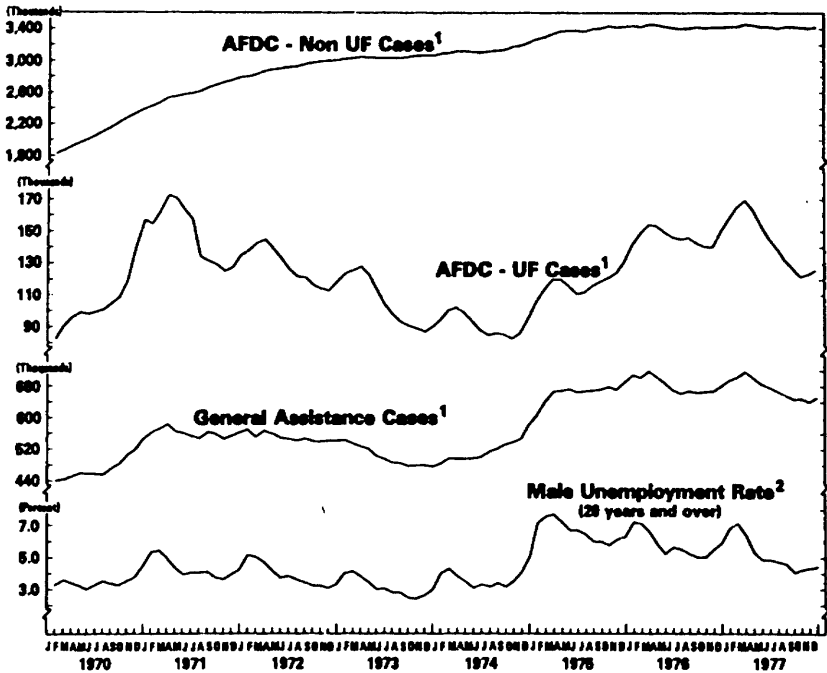
Chart 2 shows a general tendency for General Assistance cases to mirror the male unemployment rate even though State General Assistance programs vary greatly in scope, purpose, and eligibility rules. Similarly, the national AFDC-UF case trend line reflects the male unemployment rate even though the program is unavailable in 24 States. The 28 jurisdictions that offer AFDC-UF, including D.C. and Guam, account for 72 percent of the total AFDC caseload.

<sup>2</sup> Barth, Michael C., and others. "The Cyclical Behavior of Income Transfer Programs: A Case Study of the Current Recession." Technical Analysis Program No. 7, Office of Income Security Policy, Office of the Assistant Secretary for Planning and Evaluation, Department of Health, Education, and Welfare, October 1975.

<sup>3</sup> Between March 1975 and February 1977, a Supreme Court decision and new law increased the pool of families eligible for AFDC-UF. In June 1975, the Court held that States must give needy unemployed fathers an option to choose between unemployment insurance and AFDC-UF, and on October 20, 1976, P.L. 94-566 was enacted, requiring AFDC-UF States to permit unemployed fathers to receive both forms of aid. Previously, HEW had ruled that fathers eligible for unemployment insurance could not receive AFDC-UF.

Chart 2

## Number of Welfare Cases Compared to Unemployment Rate — United States



SOURCE: <sup>1</sup>Public Assistance Statistics, U.S. Department of Health, Education and Welfare, Jan. 1970 - Dec. 1977. <sup>2</sup>U.S. Bureau of Labor Statistics, Employment Earnings - U.S. 1970-1977.

For non-UF AFDC cases, however, Chart 2 shows a steady but slight rise, irrespective of changes in the male unemployment rate, until 1976, when a virtual plateau was reached. As will be seen later, the apparent relative insensitivity of total regular AFDC caseloads to male unemployment rates contrasts with close relationship, shown on chart 5, between male unemployment rates and AFDC case openings in the 1970's. Data from the 1975 AFDC Study indicate that a rising portion of AFDC families are remaining for longer times on the welfare roles (see table 7, p. 34).

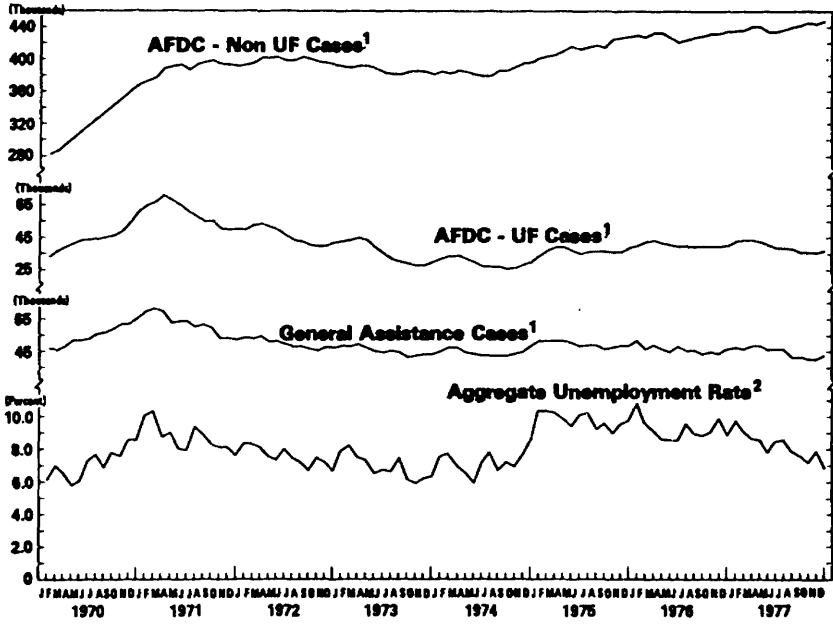
Charts 3 and 4 examine the relationship of unemployment in the two States with largest AFDC rolls, California and New York, to the same welfare programs, General Assistance AFDC and AFDC-UH. However, because State male unemployment rates were not available on a monthly basis, each State's aggregate unemployment rates was substituted. The aggregate unemployment rate in California seems loosely related to General Assistance (GA) and AFDC-UF case numbers; and in New York to General Assistance numbers. In February 1978, California accounted for 13 percent of non-AFDC-UF families and 35 percent of AFDC-UF families;<sup>4</sup> New York for 10 percent and 11 percent respectively.

The econometric studies that have estimated various causes of caseload change support the general relationships seen in Chart 2. These studies indicate that AFDC-UF and General Assistance exhibit a closer tie to unemployment rates than does regular AFDC.

<sup>4</sup> In addition, California aided approximately 48,565 needy families of unemployed fathers with its own funds because the fathers failed to meet Federal eligibility rules concerning prior attachment to the labor force..

Chart 3

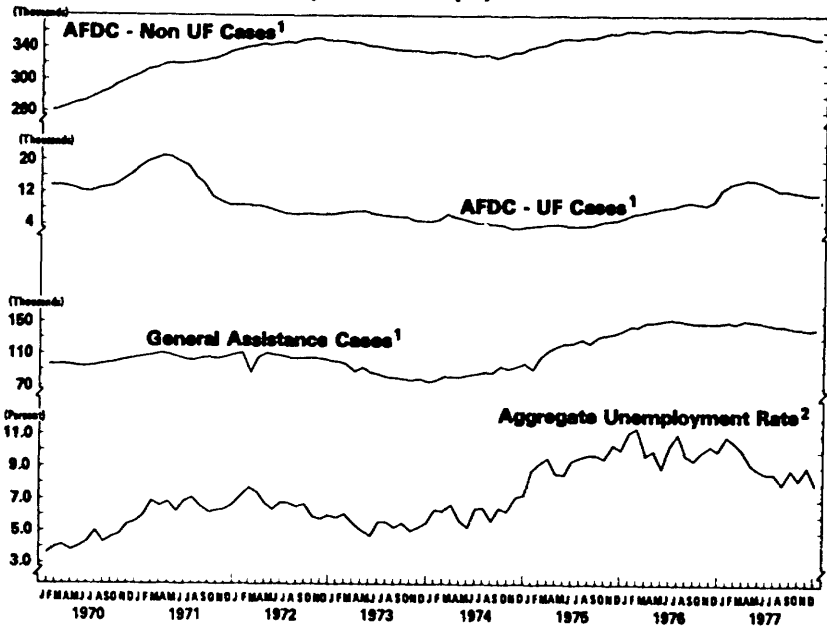
Number of Welfare Cases Compared to Unemployment Rate - California



SOURCE <sup>1</sup> Public Assistance Statistics, U.S. Department of Health, Education and Welfare, Jan. 1970 - Dec. 1977.  
<sup>2</sup> Labor Sec. of Economics Division, Library of Congress.

Chart 4

Number of Welfare Cases Compared to Unemployment Rate - New York



SOURCE <sup>1</sup> Public Assistance Statistics, U.S. Department of Health, Education and Welfare, Jan. 1970 - Dec. 1977.  
<sup>2</sup> Labor Sec. of Economics Division, Library of Congress.

In the 1975 HEW study on the cyclical behavior of welfare programs,<sup>5</sup> analysts developed estimates of enrollment in various programs for 1975 and 1976 under alternate assumptions about economic conditions. The analyst who examined AFDC-UF estimated that by the fourth quarter of 1976, if the unemployment rate rose 50 percent above the 1974 pre-recession level of 5 percent, AFDC-UF case numbers would rise to 756,000 cases,<sup>6</sup> up 112 percent from what they would have been in the absence of a recession. Actually, in the final quarter of 1976, the unemployment rate averaged 7.9 percent, up 58 percent from the pre-recession level, and AFDC-UF cases numbered 633,000, up 78 percent from the number predicted in the absence of a recession. For 1976 as a whole, the unemployment rate was 7.7 percent, below the rate of 8.1 percent assumed in the study.

In the same study estimates of enrollment in the General Assistance program were developed. It was estimated that a one percentage point increase in the unemployment rate (as from 6 to 7 percent, a gain of 17 percent in the rate itself) results in a 14 percent rise in the General Assistance caseload.<sup>7</sup> The study estimated that by the fourth quarter of 1975, if the unemployment rate averaged 8.9 percent, General Assistance enrollment would climb to 1.4 million. Actual average monthly enrollment that quarter turned out to be 965,000 persons, but the employment rate was 0.4 percentage points lower than assumed.

Studies by Professor Barry Bluestone and Associates from Boston College, working under contract for HEW, have confirmed the strong correlation between unemployment and AFDC-UF caseloads. Their basic technique was to develop explanatory equations that provide a "good fit" to the actual data for a certain period in the past, and then change selected independent variables, such as the unemployment rate, to determine the impact on the caseload of the simulated situation. In the case of the AFDC-UF program in some States, including upstate New York, this procedure found a very strong effect. "A full employment economy would have left upstate New York AFDC-UF caseload with fewer than 900 families, only one-third of the actual number enrolled during 1974," said the study.<sup>8</sup>

**B. Regular AFDC (non-UF cases).**—Studies of the relationship between unemployment and the regular AFDC program are less conclusive than those dealing with AFDC-UF, and these studies have found it more difficult to isolate the impact of unemployment from other factors affecting caseload. However, they have generally confirmed that a link exists.

The relationship between unemployment and AFDC caseload is visible in Charts 5, 6, 7, and 8, all of which measure AFDC case *openings*. Chart 5 shows that national AFDC case openings have moved with the male unemployment rate since 1969, after a reversal of this pattern in 1963-1969. Similarly, Chart 6 shows that the U.S. quarterly AFDC case openings also have reflected monthly aggregate unemployment rates in the period 1973-1977; and charts 7 and 8 provide the California and New York picture.

Chart 5 is based on annual data, 1964-1977. It compares the total number of AFDC cases opened to the yearly unemployment rates for nonwhites and for whites. It represents an updated and revised edition of the original chart first published in the 1965 Moynihan report on the Negro family. It appears from Chart 5 that the puzzling phenomenon in 1963-1969 of simultaneous rapid AFDC caseload growth and declining unemployment rates was a temporary aberration. It is possible that the reversal of usual relationships in the mid-1960's was due to unidentified other factors that overwhelmed the impact of declining unemployment. These were years of active recruitment of AFDC enrollees, when antipoverty lawyers and social welfare workers worked to expand participation. They were years when AFDC became more valuable in economic terms, especially after the 1965 passage of Medicaid, which is automatically provided to AFDC recipients, and less stigmatizing.

<sup>5</sup> Barth, Michael, and others. *The Cyclical Behavior of Income Transfer Programs: A Case Study of the Current Recession*. Technical Analysis Paper No. 7. Office on Income Security. Office of the Assistant Secretary for Planning and Evaluation, U.S. Dept. HEW, October 1975. 174 pp.

<sup>6</sup> Hough, David. *The Impact of the Recession on Income Transfer Programs: AFDC-Unemployed Fathers*. In work cited in previous footnote. Pp. 43-45.

The estimate assumed a specified rate of inflation, which was slightly exceeded, and a certain growth in the Gross National Product.

<sup>7</sup> Mills, Gregory. *The Impact of the Recession on Income Transfer Programs: General Assistance*. In Barth work cited in previous footnote. P. 64.

<sup>8</sup> Bluestone, Barry, et al. *Recent State AFDC Caseload Dynamics and the Duplicators for Welfare Reform Forecasting*. Mimeographed. April 7, 1977: 4.

Chart 5

The Relationship Between Unemployment and the Number of AFDC Cases Opened

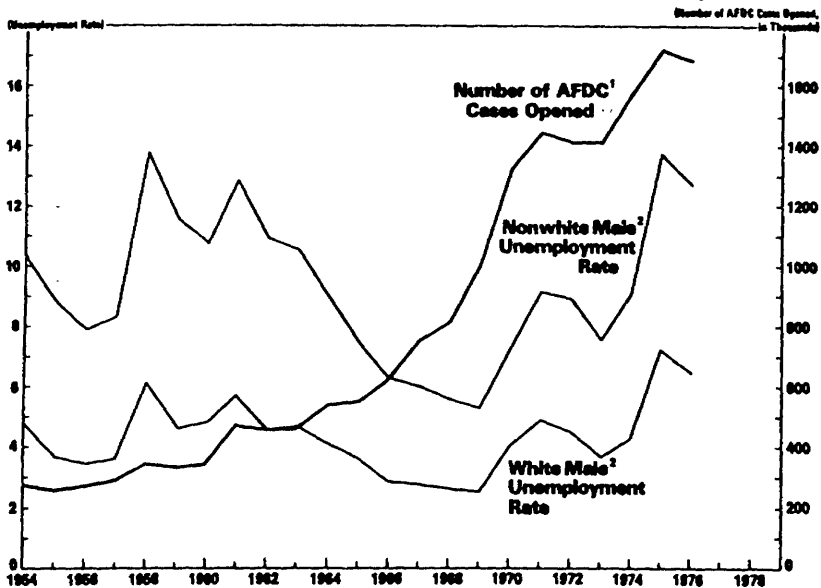


Chart 6

Number of AFDC Cases Opened Compared with Unemployment Rate -- United States.

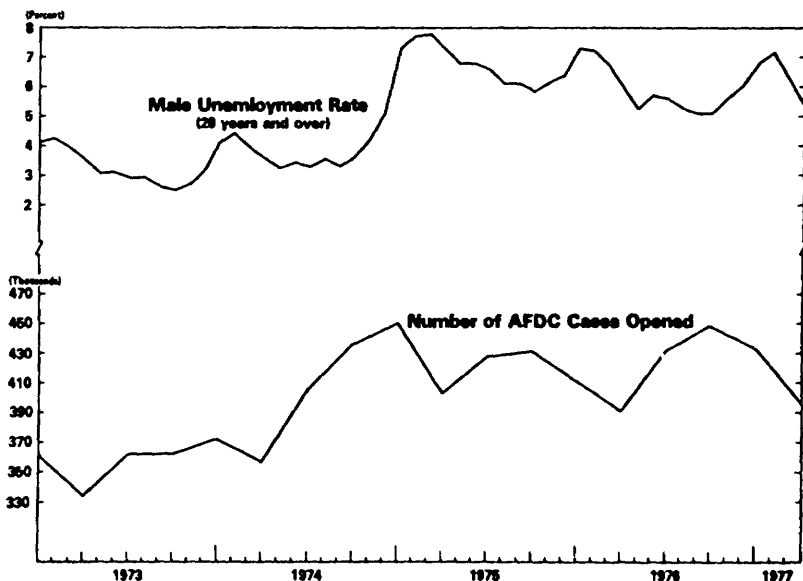
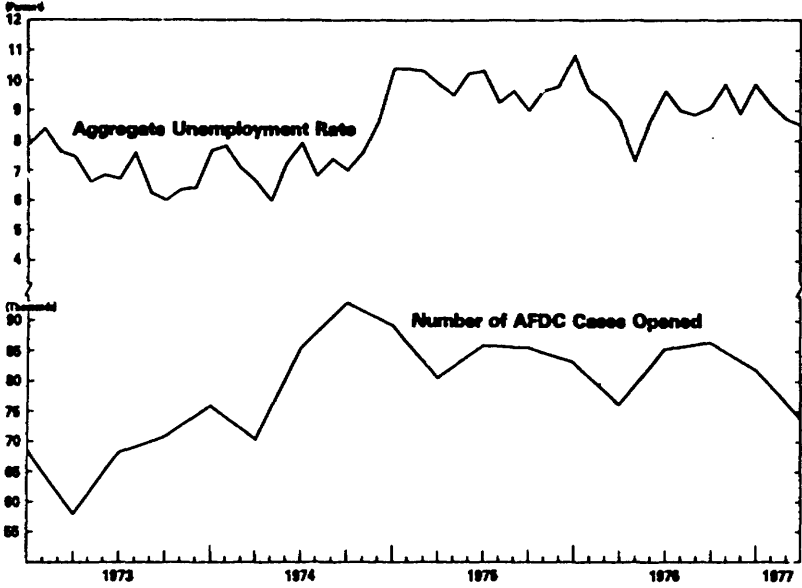


Chart 7

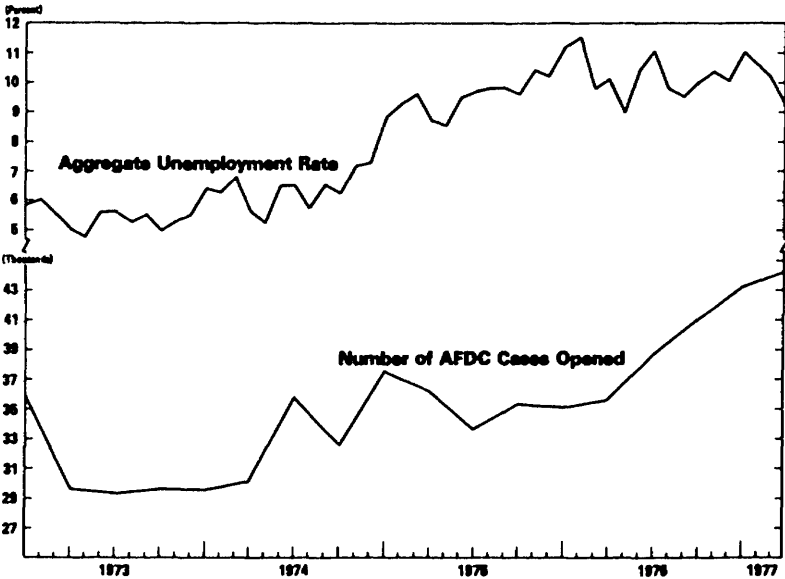
**Number of AFDC Cases Opened Compared with Unemployment Rate – California**



SOURCE: Applications and Cost Dispositions for Public Assistance, U.S. Department of Health, Education and Welfare, Office of Research and Statistics, Quarterly 1973-1977.

Chart 8

**Number of AFDC Cases Opened Compared with Unemployment Rate – New York**



SOURCE: Applications and Cost Dispositions for Public Assistance, U.S. Department of Health, Education and Welfare, Office of Research and Statistics, Quarterly 1973-1977.

Chart 6 shows that national quarterly AFDC case openings have moved with monthly male unemployment rates in the period covered, 1973-1977. It appears from this chart that a lag of about two quarters often occurs before case openings change direction. Charts 7 and 8 provide similar data for California and New York, but show State aggregate unemployment rates, rather than male unemployment rates.

The evidence pointing to a consistent link between unemployment and AFDC case openings does not indicate that unemployment is the most significant variable in explaining caseload changes, nor that its effect is large relative to impacts of other factors. However, researchers have identified a consistent and statistically strong relationship in this area. A brief discussion of some of the studies follows:

1. Studies by Professor Barry Bluestone and Associates at Boston College. This group of researchers has conducted work in recent years on the determinants of welfare caseload change. A basic hypothesis of this group is that there is no national AFDC program. They judge the State-by-State differences to be so great that aggregate trends distort and obscure the true picture. Thus, they have developed models for study of individual State programs and caseloads.

Their studies have found trends in employment conditions to be important determinants of AFDC caseloads in a number of States, even though reported labor force participating rates among U.S. poor female heads of households are low. For example, the study concluded that if employment conditions (measured by the unemployment rate and other employment indices) had remained at their most favorable levels of the July 1962-December 1974 period, the caseload at the end of the period would have been reduced in North Carolina by 46 percent and in Washington by 27 percent from their actual levels.<sup>9</sup>

The studies showed, as would have been expected, that in States with a relatively high proportion of AFDC employed parents, changes in the unemployment rate had a much greater impact on AFDC caseloads. Even in New York City where relatively few AFDC household heads report employment,<sup>10</sup> employment conditions affect case closings, and, thus, the overall size of caseload.

2. Peter S. Albin, City University of New York and Bruno Stein, New York University, found a relationship between the rate of unemployment and AFDC caseloads from time-series data for 1959-1971. They concluded, "The impact of the unemployment effect is, as we expected, strongly positive and significant, with the impact beginning after a quarter's delay and with conditions four years back having an effect on demand [for welfare]."<sup>11</sup>

3. In 1974 and 1975, HEW sponsored research on the impact of unemployment, inflation, and transfer payments on the distribution of income, particularly to the poor. One paper in this series, by Martin Holmer of HEW's Office of the Assistant Secretary for Planning and Evaluation, found a "weak effect" of the relationship between the state of the economy and AFDC. A summary of his study states that "in the short run, changes in unemployment conditions alter AFDC payments by small amounts, and, even in the long run, after changes in openings, closings, and benefit levels have worked their way through the system, a 10 percent rise in the unemployment rate \* \* \* raises AFDC caseloads by only 2.7 percent."<sup>12</sup>

<sup>9</sup> The 1975 AFDC Recipient Characteristics Study by HEW (Part 1) found that 10.8 percent of South Carolina AFDC mothers were working in the survey month and 3.15 percent were awaiting recall from layoff. Data for Washington were not reported because of small sample size.

<sup>10</sup> Multiple Welfare Benefits in New York City. A study based on data gathered by the New York City-Rand Institute (which was dissolved in October 1975) reported that 7.9 percent of New York City AFDC families had earnings in 1974. Prepared under a grant from the Dept. of HEW R-2002-HEW, August 1975: viii.

The 1975 AFDC study found that 11.1 percent of New York State mothers worked in May 1975, and one percent were awaiting recall from layoff.

<sup>11</sup> Albin, Peter S. and Bruno Stein. *The Impact of Unemployment on Welfare Expenditures, Industrial and Labor Relations Review*, vol. 31, No. 1 (ct. 1977): 41.

<sup>12</sup> Gramlich, Edward M. *Are the Poor Falling Behind? The Impact of Unemployment, Inflation, and Transfer Payments on the Distribution of Income*. Summary of a Policy Research Project sponsored by the U.S. Dept. of HEW, March 1977: 32.



4. In a report done in 1974 for the Congressional Research Service, Mathematica, Inc. reported that by using the Transfer Income Simulation Model (TRIM), a sophisticated micro-simulation model, it was determined that a one percent increase in unemployment (as from 4 to 5 percent, a 25 percent increase in the rate itself) produced only a 1.5 percent increase in the annual AFDC caseloads (persons receiving benefits at any time during the year). Mathematica said a percentage point increase in the unemployment rate produced a 5 percent increase in the AFDC-UP component of the caseload.<sup>13</sup>

5. Under contract to the State of California, Rand Corporation published in January 1976 a survey and evaluation of welfare caseload estimating techniques. Their review of work completed at that time concluded: "Over ten studies on our list reported statistically significant relationships among caseload and one or another economic variable. There is too much strong evidence and too much agreement among these studies to doubt that real progress in this regard is within close reach."<sup>14</sup>

*C. AFDC family dependency ratios.*—Chart 9 shows that the proportion of families with children under 18 enrolled in AFDC climbed 70 percent from 1970 to 1977. In March 1970, 1 in 15 families received AFDC benefits; by March 1977, the proportion was more than 1 in 9 (11.53%). The rise was uninterrupted. However, the rate of gain slowed to a near-halt from March 1973–March 1974, paralleling a relatively low plateau in the male unemployment rate between the same two months.

<sup>13</sup> Mathematica, Inc. Policy Studies Group. *Estimating Costs and Caseloads for Federal Transfer Programs*. November 18, 1974: 1952-3.

<sup>14</sup> Abrahamse, Allan, et al. *Welfare Caseload Estimating Techniques: Report No. R-1916-CDOBP* prepared for the California Department of Benefit Payments, January 1976: 49.

Chart 9

## AFDC Family Dependency Ratios Compared with Unemployment Rates



SOURCE: Current Population Reports - Characteristics of the Low-Income Population, 1970-1977. U.S. Department of Commerce, Bureau of the Census.  
 Employment and Earnings, U.S. Department of Labor statistics, 1970-1977.

Plotted as a proportion of male-headed families with children under 18, the AFDC-UF March 1970-1975 caseloads showed slight rises and declines that dimly reflect the male unemployment rate. In this period AFDC-UF families ranged from 0.4 to 0.7 percent of all male-headed families with children.

Table 5 shows that the proportion of U.S. children enrolled in AFDC has risen about one-third since 1970. The proportion peaked in 1975 at 12.1 percent and since has declined slightly.

TABLE 5.—PROPORTION OF U.S. CHILDREN ENROLLED IN AFDC

	AFDC children <sup>1</sup>	AFDC children as a percent of all children under 18 <sup>2</sup>
1970.....	6,204,000	8.9
1971.....	7,389,677	10.6
1972.....	7,896,165	11.4
1973.....	7,837,561	11.9
1974.....	2,731,295	11.5
1975.....	8,038,055	12.1
1976.....	7,768,781	11.9
1977.....	7,688,578	<sup>3</sup> 11.8

<sup>1</sup> As of July. (However, U.S. child population for 1970 is as of April.)

<sup>2</sup> The May 1975 AFDC study found 2.4 percent of the children to be students aged 18 or over. They totaled 192,727.

<sup>3</sup> Estimate.

#### IV. Analysis, comments, and conclusions

The existence of a positive relationship between unemployment and AFDC caseloads has been demonstrated, both by simple charts and by studies using sophisticated analytical tools. The direction and the statistical validity of the relationship is consistent, but there is considerable uncertainty about the size of the impact.

This uncertainty about size of an effect is obvious from the varying conclusions of the published studies. Their differences cast doubt on the present capacity to forecast future caseloads on the basis of unemployment assumptions. An example of the pitfalls of prediction is an HEW estimate published in October 1975 of the peak number of AFDC recipients expected during 1976. On the basis of specified economic assumptions, including an annual average unemployment rate of 8.0 percent, the study predicted that enrollment would climb to 14.8 million persons in the second quarter of 1976 and decline to 14.5 million by the last quarter.<sup>14</sup> The actual number of recipients, with an annual unemployment rate two-tenths of a percentage point below the assumed rate, turned out to be 11.3 million in the second quarter and 11.2 million in the final quarter, a short fall of more than 3 million persons.

Because of the complex nature of the separate State AFDC programs and the general lack of understanding of their impact on human behavior, it is difficult to forecast precisely caseload changes under alternative economic assumptions. The problem is compound by the interplay of many other factors, some more influential than the unemployment rate, that profoundly impinge on AFDC enrollment. Some of these other factors were especially forceful in expanding AFDC caseloads in the mid- and late 1960's.

Phenomena that have caused increases in AFDC enrollment can be grouped into two classes: (1) growth of the eligible pool, which itself is directly affected by economic conditions, and (2) increase in the AFDC-participation rate of eligibles.

These are briefly discussed below:

##### 1. Growth of the eligible pool.

a. *Increase in female-headed families with children.*—From 1969 to 1976 the number of female-headed families and subfamilies with their own children under 18 rose from 3,374,000 to 5,403,000, a rise of 60 percent. (See table 6.) Since eligibility for regular AFDC basically is confined to this group, its size is directly related to AFDC caseloads. In the same period the number of AFDC families

<sup>14</sup> Holmer, Martin. *The Impact of the Recession on Income Transfer Programs: Aid to Families with Dependent Children.* In work cited earlier (*Cyclical Behavior of Income Transfer Programs: A Case Study of the Current Recession*), p. 40.

climbed from 1,621,000 to 3,590,000, an increase of 122 percent. States determine eligibility for AFDC, and it is not related to the official poverty index of the Census Bureau. Welfare payments, however, appear to have filled a greater part of the poverty income gap of fatherless families in 1976 than in 1969. As shown in Table 6, 42.2 percent of the female-headed families and sub-families with children under 18 had money income, including cash welfare, that was below the poverty cutoff in 1976, compared with 44.5 percent in 1969.

TABLE 6.—FEMALE-HEADED FAMILIES AND SUBFAMILIES WITH OWN CHILDREN UNDER 18 YR<sup>1</sup>

	With income <sup>2</sup> below poverty level		
	Total	Number (thousands)	Percent of total
1969.....	3,374	1,500	44.5
1970.....	3,837	1,680	43.8
1971.....	4,140	1,711	41.3
1972.....	4,373	1,848	42.3
1973.....	4,684	1,953	41.7
1974.....	5,110	2,107	41.2
1975.....	5,212	2,194	42.1
1976.....	5,403	2,279	42.2

<sup>1</sup> Numbers as of March in subsequent year.

<sup>2</sup> Including cash welfare.

Source: Characteristics of the Population Below the Poverty Level, Current Population Reports, Annual Reports, 1969-76. U.S. Department of Commerce, Bureau of the Census.

The extent to which the incidence of fatherless families itself is affected by economic conditions is under study. There is a significant body of research in progress to examine the impact of unemployment on family splitting. The hypothesis is that loss of a job causes stress and pressure on marriages, resulting at times in family breakup, which creates a newly eligible fatherless family. It also is thought that poor economic conditions sometimes may deter marriage of a couple with a child. Needy children of non-married parents are eligible in all States for AFDC.

b. *Liberalization of eligibility by courts and Congress.*—Since 1968 the U.S. Supreme Court made several rulings that have expanded eligibility for AFDC,<sup>10</sup> including these:

1968—*King v. Smith*, 392 U.S. 309: Court ruled that AFDC could not be denied to needy children because the mother cohabited with a man, nor could Alabama compel child support from such men, whom the State treated as "substitute fathers."

1969—*Shapiro v. Thompson*, 394, U.S. 618: Court held invalid State rules requiring residence within a State for at least one year as a condition of AFDC eligibility.

1970—*Goldberg v. Kelly*, 397 U.S. 254: Court held that States must give an AFDC recipient prior notice or opportunity to respond before halting benefits.

1970—*Rosada v. Wyman*, 397, U.S. 397; Court ruled invalid changes in New York's standard of need computation formula that resulted in decreased benefits and lower eligibility cutoff.

1970—*Lewis v. Martin*, 397 U.S. 552: Court ruled invalid California's presumption of support for needy children from a man with whom the mother lived.

1971—*Graham v. Richardson*, 403 U.S. 365: Court held invalid State laws that conditioned welfare eligibility upon U.S. citizenship or imposed a durational residence requirement on aliens.

1971—*Townsend v. Svanek*, 404 U.S. 282: Court ruled invalid Illinois' limitation of AFDC benefits for students aged 18-20 to those attending high school or voca-

<sup>10</sup> In this period some Supreme Court rulings had the effect of restraining AFDC eligibility. For example, in 1970 (*Dandridge v. Williams*, 397 U.S. 471) the Court upheld Maryland's maximum grant provisions, which set a ceiling on monthly benefits per family. In 1971 (*Wyman v. James*, 400 U.S. 309) the Court upheld New York's mandatory home visit rule for welfare eligibility. In 1972 (*Jefferson v. Hackney*, 406 U.S. 535) the Court upheld Texas' system of percentage of need grants that provided AFDC families with a lower fixed percentage than that provided to the needy aged. In 1977 (*Batterson v. Francis*, 97 S. Ct. 2399) the Court upheld a Maryland rule denying AFDC-UF to needy children of workers unemployed because of participation in a strike.

tional school, holding that eligibility must be offered to all students, including those in college, if to any.

1972—*Carlson v. Remillard*, 406 U.S. 598; Court held invalid a California law denying AFDC benefits if the reason for parental absence was military service.

1974—*Shea v. Vialpando*, 416 U.S. 251; Court held invalid a Colorado rule that placed an absolute ceiling on the amount of work related expense that must be considered in determining an AFDC family's need, holding that all reasonable expenses must be taken into account.

1975—*Van Larc v. Hurley*, 421 U.S. 337; Court held invalid New York's "lodger" regulations which reduced an AFDC family's benefits solely because a nonlegally responsible person resided in the home.

1975—*Philbrook v. Glodgett*, 421 U.S. 707; Court held invalid Vermont's denial of AFDC-UF to an unemployed father on grounds of mere eligibility for unemployment insurance, ruling that unemployed fathers must receive an option to enroll in whichever program was more generous.

In the last decade Congress also has liberalized eligibility for AFDC. In 1967, it enacted a work incentive provision (mandatory disregard of the first \$30 earned monthly plus one-third of the rest.)<sup>17</sup> This provision, combined with the earlier requirement that work-related expenses be disregarded, has resulted in requiring all States to continue to pay AFDC supplements to working mothers even when their net earnings exceed the State-established standard of need. Under current law, a working AFDC mother remains eligible for a declining AFDC payment until gross earnings equal \$30 monthly, plus 150 percent of the State's payment standard, plus 150 percent of work-related expenses. The result can be very high ceilings on earnings eligible for AFDC supplementation. For example, if one assumes work expenses equal to 15 percent of gross earnings, a New York City AFDC mother of four remains eligible for AFDC until gross earnings equal \$11,520 per year. With each extra dollar of work expense, such as taxes, the cutoff rises by \$1.50. At 1977 Congressional hearings, Dr. Blanche Bernstein, then deputy commissioner of the New York State Department of Social Services, said it was theoretically possible for a New York City mother of three to remain eligible for AFDC, and its automatic fringe benefit, Medicaid, until annual earnings reached \$29,000.<sup>18</sup>

In another action that expanded potential eligibility, Congress passed P.L. 94-506 (enacted October 20, 1976) to permit unemployed fathers of needy children to receive both AFDC-UF and unemployment insurance. HEW regulations to implement this law were issued in March of 1977.

Program changes that expanded or eased eligibility, whether resulting from litigation, law, or regulation, are considered a major factor in the history of AFDC's growth. Holmer estimates that as much as 80 percent of the increase in caseload between 1952 and 1972 can be attributed to the greater ease of enrolling in AFDC. " \* \* \* Changes in AFDC laws and regulations during the 1960's, as well as shifts in the every day administration of the program, made it less difficult for nonrecipients to become recipients and easier for people who were already on the program to maintain their recipient status," he concluded.<sup>19</sup>

c. *Increase in benefit levels.*—During the 1960's benefit levels in AFDC were liberalized in most States. For example, the AFDC maximum payment of the median State for a four-person family of four in July 1977 was \$314. This was up 73 percent from the corresponding benefit paid 10 years earlier, \$181. Analysis of caseload growth over the period has shown that the increase in benefit levels does not itself explain caseload growth. However, benefit increases have the effect of increasing the number eligible for benefits in most States (if they have earnings or other private income), and this expansion of the eligible population, together with the increasing likelihood that those eligible will enroll, is a large factor in swelling AFDC enrollment.

## 2. Increase in the AFDC-participation rate by those eligible.

<sup>17</sup> In 1967, in an effort to curb the expansion of AFDC rolls, Congress voted to freeze the proportion of illegitimate children in a State eligible for Federal matching funds. However, Congress later voted to postpone and then to repeal this provision. Thus, it never took effect.

<sup>18</sup> Details of this example, provided later for the hearing record, showed that \$13,548 of earnings were not counted as income because they represented work expenses. This amounted to \$1,129 monthly (Federal income tax, \$555; State taxes \$177; social security, \$142; lunch and car fare, \$49; and child care, \$215). U.S. Congress. House. Hearings before the Subcommittee on Intergovernmental Relations and Human Resources of the Committee on Government Operations. Administration of the AFDC Program. July-October 1977: 297.

<sup>19</sup> Holmer. Work cited earlier, p. 33.

A study by Barbara Boland of the Urban Institute found that while the eligible pool increased by 24 percent between 1967 and 1970, the caseload doubled. She estimated that aggregate participation increased from 56 to 78 percent in those years (with participation for the non-UF portion of the program climbing from 63 to 91 percent). She concluded that the high level of participation for female-headed families "suggests that in the future any substantial growth in caseloads for this group will have to come from the creation of new eligibles."<sup>20</sup> Her work helps explain the slowdown in net caseload growth since 1971.

a. *Administrative liberalization.*—Part of the reason for a rising enrollment rate of persons eligible for AFDC is the greater accessibility to the program. The work of Holmer and that of Bluestone identify this as perhaps the single most important factor responsible for the caseload explosion in the late 1960's. In this period anti-poverty workers and many social workers actively recruited AFDC enrollment. However, in recent years administrative tightening has resulted from the quality control drive to reduce erroneous AFDC payments and from the operation of the new Child Support program (Title IV-D of the Social Security Act).<sup>21</sup>

b. *Attitudes toward welfare.*—In the late 1960's AFDC membership became more valuable, especially after passage of Medicaid, and more acceptable. The philosophy of welfare as a right rather than as a privilege blossomed.

c. *Longer tenure on welfare.*—In recent years AFDC families have increased their average stay in the program. This can be seen as a variety of increased participation. From 1971 to 1973 the median stay rose by 72 percent, to 31 months. See Table 7.

#### SUMMARY

The non-UF AFDC program is not so strongly affected by economic conditions, particularly the unemployment rate, as are AFDC-UF and General Assistance, since the ties to the labor market of many AFDC mothers are tenuous. The studies confirm this, but point, nevertheless, to a visible link between regular AFDC and unemployment.

TABLE 7

Year	Median stay on AFDC (months)	Since most recent case opening enrolled in AFDC (percent of families)	
		18 mo or less	More than 4 yr
1975	31	36.3	26.4
1973	24	40.8	17.2
1971	18	47.5	17.7

Source: AFDC biennial studies made by Health, Education, and Welfare.

MR. GARFINKEL. At the moment, we do not have the evidence to distinguish between the possible explanations for why we are observing these increases in marital splits. But one thing I believe we have already learned from these marital stability results is that how you administer something can make a big difference. This may not appear to be terribly surprising to most, but because economists specialize in measuring the effects of variations in incomes and prices, we have not paid sufficient attention to the effects of variation in administrative factors.

<sup>20</sup> Boland, Barbara. Participation in the Aid to Families with Dependent Children Program (AFDC). In *The Family, Poverty, and Welfare Programs: Factors Influencing Family Instability*. Studies in Public Welfare Paper No. 12 (Part 1). Subcommittee on Fiscal Policy, Joint Economic Committee, 1973: 139.

<sup>21</sup> In spring 1973, AFDC launched a drive to reduce States' AFDC payments to ineligible and their over- and underpayments to eligibles. Payment error rates were cut almost in half between April-Sept. 1973 and Jan.-June 1977. Also, since the Child Support program was begun in late 1975, more than \$1 billion has been collected by States from absent fathers of AFDC children. Officials of this program say it may be deterring some ineligible from applying and that it is achieving more vigorous enforcement of eligibility rules.

Finally I must also note that we do not know yet what effects the splits induced by the experiment have had on the former spouses and their children. I think it is terribly important to find out, and if I may modestly make the suggestion to the Congress that that would be something worth funding.

Senator MOYNIHAN. I would like to make a point here for the record which I do not expect anybody to pay attention to but I want to make it anyway. I was much involved with the founding of the poverty program and the community action program and I wrote a book in 1967 called "Maximum Feasible Misunderstanding" which was very, very unpleasantly reviewed by the present Secretary of HEW, if I recall, in which I said about those community action programs that there really was some question about the ethics of experimenting in those ways, that social scientists had to ask themselves what medical doctors had for a long time asked themselves, and that is what are the ethics of experiments with human beings? What are the ethics of experimenting with communities?

There is a saying around the criminal courts, the lawyer always goes home, and you fellows at the Institute for Research on Poverty say, "Well, another disaster. Let's find out what that one is all about." And you would be surprised what you can learn.

Finally, I must note that we do not yet note what effect the splits *induced* by the experiment have had—my italics, induced—on the former spouses and their children. Note that.

We march into those communities and break up those families. That is a big thing to happen to a 3-year-old kid, you know.

Mr. GARFINKEL. That is correct. It may be very harmful. Ergo, I think it follows that it is much better to do it on a small scale than to blunder into doing it to the whole Nation and given that we already did it on a small scale, I would simply suggest that it might make sense to follow them up.

Senator MOYNIHAN. I agree with you.

Would you take my point that the social sciences have not been as careful as they might with the things they do to people?

Mr. GARFINKEL. Absolutely. I agree with that, and I think—

Senator MOYNIHAN. They are trying to do good. Doctors are trying to do good, too, but doctors have worked out a much more careful regimen of what you can do and what you cannot do and what needs peer review. If it is the case that we moved in and played God with a lot of these people and if, to use your word, we induced the breakup of their families, there is something to explain.

Go ahead, sir. I am sorry.

Mr. GARFINKEL. I just wanted to add that I think social science experimentation is quite young and that I think social sciences are now taking quite seriously the ethics—

Senator MOYNIHAN. That is why I wrote my book in 1967, to point out that we should have paid more attention—well, leave it there.

Mr. GARFINKEL. From the supported work experiment, we learned that if you offer minimally decent jobs—and I stress the word minimal—to AFDC mothers, ex-addicts, ex-convicts, and unskilled teenagers, that they will work substantially more than if you do not offer them such jobs. Whether this finding is surprising or not is difficult to say. It depends upon who hears it.

Most of the people I deal with do not believe that these results are very surprising because they were raised, as I was raised, on Keynesian economics and believe deep down in at least one part of their heads and hearts that there is such a thing as involuntary unemployment.

One of the latest developments in economic theory—which is really not a new development; it goes back 200 years and we are rediscovering it—is that there is no such thing as involuntary unemployment. According to these new theorists, unemployment is just job search and the problem lies with the unemployed themselves and they are just looking for too good a job.

Senator MOYNIHAN. Tell me, that is a new literature that I do not know. Who do you associate that with? What name?

Mr. GARFINKEL. Ultimately I would say Milton Friedman, although he himself has not developed that, it comes out—

Senator MOYNIHAN. I have not read anything by Milton Friedman on that.

Mr. GARFINKEL. I said he himself has not worked in that literature, but it is his students.

Senator MOYNIHAN. Do you have some names?

Mr. GARFINKEL. I will get you the names.

Senator MOYNIHAN. Would you, for the record? I would appreciate it. Maybe you could send us some of their papers.

Mr. GARFINKEL. I can send you some of the literature, as well.

Senator MOYNIHAN. The idea is that the market does not clear because of conditions established on the supply side.

Mr. GARFINKEL. Martin Feldstein is the name.

Is it true that the supported work jobs are too good? They are dead-end jobs, lasting at most 12 months in some cities and 18 months in others; they pay less than \$3 an hour. But these jobs must obviously have been more attractive than the job opportunities available to controls.

In one sense, then, the new job search theorists must be correct. The unemployed are looking for too good a job—in this sense, good being relative to what is available to them at the moment.

Senator MOYNIHAN. Can I just interrupt to say that my associate, Mr. Lenkowsky, gives me a note that these are the microeconomists that he and I were talking about the other day and he suggests Martin Feldstein, Michael Boskin, and Finis Welch as some of the names. Dr. Welch will testify.

Mr. GARFINKEL. Dr. Feldstein—the others have not worked as much in this particular area, but Dr. Feldstein—

Senator MOYNIHAN. All right.

Mr. GARFINKEL. In other words, in some sense the claim of the new job search theorists is true by definition in the following sense, that supported work reduces unemployment and increases labor force participation because it offers more job opportunities to participants than would otherwise be available to them.

But the question remains, are these jobs too good for participants? That is, do they deserve less? To answer that question requires a value judgment.

Suppose for the moment that we agree that adults deserve to make at least \$3 per hour. Then it follows that there is something the matter with the economy for failing to generate a sufficient number of such



jobs. We should put our social scientists to work to figure out how to fix up the economy.

Well, suppose, on the other hand that you say, well, \$3 is too high—some people would say \$2.50, that everyone ought to make at least that. Others might say \$2. Some, but not many people, would say no wage is too low. My guess is that most Americans would say that everyone should earn at least the minimum wage, if not higher. The point is, though, that so long as you set some minimum, the previous conclusion will hold—namely, there is something the matter with the economy that needs fixing.

Those who use search theory to argue otherwise are, in effect, implicitly making the ethical judgment—and it is an ethical judgment—that no wage is too low.

This last point leads very nicely into my discussion about the policy implication of the findings from the experiments for income maintenance for welfare reform. What are the implications? The simple answer is that there are no direct policy implications. The facts do not speak for themselves. If I have heard that once, I have heard it 10 times, from Gunnar Myrdal, whom I had the privilege to meet when he visited the Institute last year. I think he is absolutely correct on that. Policy choices depend upon what we value, as well as on the way the world works.

Let me summarize briefly what the experiments tell us about how the world works and then lay out the value choices we must confront. The income maintenance experiments tell us if we improve the lot of the poor by increasing their unearned income and decreasing the rewards for work, they will work less, while the supported work experiment tells us that if we improve the lot of the poor by increasing the attractiveness of work, they will work more. Does it follow that we should improve the lot of the poor only through increasing the attractiveness of jobs? Not necessarily. That depends upon how much value we put upon the work effort of the poor, and upon the relative costs to society of improving the lot of the poor through alternative strategies.

On the one hand, work is one of the most fundamental and important activities of human beings. Work is essential for the well-being of both the individual and society. On the other hand, the progress of civilization may be measured in large part by the extent to which we have been able to free ourselves from the necessity of work. My values are such that if the poor work a little less, I conclude that is a tragedy neither for them nor for us. For example, married women in poor households currently work somewhat more than married women in middle- and upper-income households. If as a result of an income maintenance program, more poor women stay home to raise their children rather than work, there is no ground for alarm. Similarly, if poor men remain unemployed a bit longer to look for a better job, we needn't hit the panic button. But if, as a consequence of the unavailability of decent jobs and the availability of welfare, we were to induce the poor to work substantially less than the rest of us, that would, in my judgment, be tragic for us all.

What are the relative costs of improving the lot of the poor through jobs rather than cash or in-kind transfers? There is no simple answer. There are a large number of possible cash and in-kind transfer pro-

grams, and an equally large number of ways to improve the labor market opportunities of low wage workers. How effective any particular program is will depend upon what other programs exist, whether the objective is to provide short-term or long-term aid, and many other factors. Providing cash for short-term unemployment, for example, is certain to be less costly than providing jobs or work relief. The key, it seems to me, is to retain balance. Relying too heavily on either transfers or labor market strategies is likely to be inefficient. Let me add, however, that nothing will do more in the long run to reduce dependency and promote equality and efficiency than to achieve full employment.

Besides improving pay and working conditions through full employment, there are two additional ways to increasing the relative attractiveness of work to the poor. First, the nonwork sources of income available to the poor, such as food stamps, can be reduced. My values are such that I would not recommend decreasing the attractiveness of existing options open to the poor. In general, they are poor precisely because they have no good options. What is the justification for taking away what little they have? That it will be cheaper for us in the room? Even if that were so, it is a poor excuse. They need the benefits a lot more than we need to pay less taxes.

Second, the tax rates in programs designed to aid the poor can be reduced, thereby increasing the net rewards for working. The best way to do this is to reduce the role and scope of welfare programs by increasing the proportion of aid to the poor which is provided through universal programs which benefit rich and poor alike. Welfare programs are designed to aid only the poor and by their very nature, therefore, tax them more heavily than everyone else is taxed in our society.

Consider, for example, the work effort issue. On the other hand, work is one of the most fundamental and important activities of human beings. On the other hand, the progress of civilization may be measured, in large part, by the extent to which we have been able to free ourselves from the necessity of work.

My values are such that if the poor work a little less, I conclude that is a tragedy neither for them nor for us. But if they are discouraged from working at all by the combination of the availability of welfare and the unavailability of decent jobs that would, in my judgment, be tragic for us all.

Now the experiments show that if we so desire, we can decrease the work effort of the poor in one of two ways. Either we can reduce their alternative sources of income or we can increase the job opportunities available to them. That is to say, we can either decrease the attractiveness of not working or increase the attractiveness of working.

Again, values come in and are critical. My values are such that I would not recommend decreasing the attractiveness of existing options open to the poor. In general, they are poor precisely because they have no good options. What is the justification for taking away what little they have? That it will be cheaper for us in this room?

Even if that were so, it is a poor excuse. They need the benefits a lot more than we need to pay less taxes. There are economists who believe that it might be better for society to pay unskilled workers for no work than to create jobs for them, and in the paper I have an explana-

tion for why I believe that to be absurd, but I think I will just skip that.

Nothing will do more in the long run to reduce dependency and promote equality and efficiency than to achieve full employment.

In addition to seeking full employment, I would seek to reduce the role and scope of welfare programs. The most important lesson I have learned about welfare is that we now have too much of it. Not that we are helping the poor too much; quite the contrary. We do not do enough, either for their sake or for ours, to provide better opportunities and resources, but welfare is far from the best way of aiding the poor.

The first and most fundamental thing that is wrong with welfare programs is that they are designed to help only poor people. By virtue of helping only poor people, they tax them. For reducing benefits as income increases is perfectly equivalent to taxing income. We measure the tax burden each of us pays by comparing our gross with our take-home pay. By that measure, the tax burden on the poor is very high. For each dollar an AFDC mother earns in excess of \$30 a month, her AFDC benefits are reduced by 66 cents. Her food stamp benefits are also reduced and if she lives in public housing, her rent will increase. What's more, nearly all welfare reform proposals would increase rather than decrease tax rates on the poor. Under many proposals total benefit reduction rates of 70 percent to 90 percent would be common.

There are other serious problems with welfare programs. They place the poor in politically vulnerable separate programs, stigmatize and rob them of their dignity, create incentives for the poor to cheat more than the rest of us, and exacerbate class tensions. In response to questions, I would be happy to elaborate on any of these other indictments of welfare.

I would like to take the opportunity—

Senator MOYNIHAN. I would like to ask you what your evidence is that the poor cheat more than anybody else?

Mr. GARFINKEL. I didn't say the poor cheat more, I said that the program creates incentives for them to cheat.

Senator MOYNIHAN. How do you know that? Have you studied it?

Mr. GARFINKEL. I think it follows theoretically, and I think we have—

Senator MOYNIHAN. It does not follow theoretically. I want to know if you have data on this, or if you do not.

Mr. GARFINKEL. Can I make the argument?

Senator MOYNIHAN. Yes.

Mr. GARFINKEL. Whenever you place tax rates on people you create an incentive for them to avoid reporting income; just as you create an incentive for them to work less, you also create an incentive to avoid reporting income. That is true of rich people as well as poor people.

We have loopholes in the positive income tax system and we have evidence that people march through them—indeed, that is why we placed them there, so that people could avoid their taxes legally. But we also know that there is a certain amount of illegal avoidance as well, and I am talking about rich people or middle income people now, and not poor people.

There is no reason to believe that poor people are any different. The only difference is that they face higher tax rates, not lower tax rates, and so, therefore, the incentive is greater. It is a very perverse world. We create incredible incentives for them not to work, incredible incentives for them to cheat, and then if we find somebody working a little less or hiding a little income, we say, gee, if only they would work more and did not cheat, they would not be poor. But it is the environment that we created for them.

Senator MOYNIHAN. All right, sir. Now you have four specific programs.

Mr. GARFINKEL. I would like to elaborate briefly on the stigma issue, if I might, and relate it to the family splitting issue. If my colleague, John Bishop, is right in tracing the negative effect of experimental negative income tax programs on marital stability to undermining the male role, we must ask if the male role is undermined because the payments are welfare-type payments, that is, payments just for the losers in the economic game, or simply because the money is not attributable to his effort, past and present.

I strongly suspect the former. I know you were a previous advocate of children's allowances, and I heard you say today you still are. Well, imagine if we had a universal children's allowance program—

Senator MOYNIHAN. No, you did not hear me say I still am. You heard me say that the earned income tax credit was rather the equivalent of a children's allowance. I try to be careful in what I say.

I am an advocate of the President's program, whatever it turns out to be.

Mr. GARFINKEL. If we had a universal children's allowance program for rich and poor alike, I do not believe that it would promote more marital splits than a program for which only children of split families were eligible. It might promote more splits than no program at all by decreasing the unattractiveness of being a single parent for women. But in my judgment, this is an acceptable price one must pay for greater equality between men and women and increased well-being for single parents and their children. That is to say, adverse effects are not ipso facto grounds for not doing something, because not doing something may have even larger adverse effects. We may have to be willing to live with effects we would otherwise prefer not to have happened. That is true in the work effort area and the same thing would be true in the marital stability area.

But, I think this is a case where we need not undermine the absolute status of an unemployed male head by forcing him to apply for welfare because there are other, better, ways to provide aid: An expanded unemployment insurance program is one; full employment is another; and now an expanded earned-income tax credit is a third.

I would like to now suggest four specific income maintenance reform measures that would substitute universal benefits for welfare benefits and thereby simultaneously increase the incomes of the poor and the relative attractiveness of work.

First: I would convert the benefits of the food stamp program into a refundable \$600 per person tax credit in the Federal income tax and administer the program through the Internal Revenue Service and the tax withholding system. The President has already proposed cashing out food stamps and this would carry that proposal a step further and

would fully integrate the program with the income tax. This is a case where I believe the nature of the administration of the program makes an incredible difference, and if you would like, I will respond to questions on that.

Let me just add one other thing. I think earlier you said you predicted that we would not get a guaranteed income if we did not get the family assistance program, but later you said but we did get food stamps. It seems to me that indeed food stamps is a guaranteed income; it only happens to be one in funny money. But it has exactly the structure of the negative income tax and it is a noncategorical program. In a lot of ways it is worse than most welfare programs because it stigmatizes people much more because it publicly identifies participants every time they spend them. But in other ways, it is better than most welfare reform programs because it has a low tax rate and that makes it easy and cheap to convert it to a refundable tax credit. It will not cost an awful lot more than we are now spending. It will cost some, but not an awful lot.

Second: We should restructure the social security program so that all beneficiaries get a benefit at least equal to the poverty line. There are some people who advocate that we eliminate these aspects of social security which favor the poor and aid the poor aged in a separate welfare program like the supplementary security income program. I believe that is a serious error. It would isolate the poor aged in a way that is simply not necessary, and not efficient. Economists who claim it is efficient are misusing the term.

Third: We should enact a universal social child support program. I think I will not take the time now to describe it. It is described in my written testimony.

Fourth: We should adopt a comprehensive national health insurance program of the kind favored by Senator Kennedy. Medicaid is, in some ways, one of our worst welfare programs. It not only separates the poor from the rest of us, but it also lifts some of the poor above people who are just a little bit higher than them in the socioeconomic ladder. That kind of social leapfrogging is very divisive and eventually a corrosive element in the social structure. The medical care industry does not function efficiently in the free market. All Americans pay for this inefficiency, and we all want insurance coverage. Here is a case where we can help the poor in the process of helping ourselves.

All of these programs would reduce tax rates on the poor and integrate them into the mainstream of society. Some—though not all—of them will cost more money initially to those of us at the top of the economic pyramid. But I am convinced that those of us who have already benefited so much from our wonderfully productive economy will find that this is a very good investment. It's also an investment we can take pride in. Thank you.

Senator MOYNIHAN. I do thank you for this, and the committee thanks you.

Let me ask you a question one of the committee staff asked if I would put to you. Do you have any idea, or could you get us an idea if you do not now have one, what your four proposals would cost on a fully implemented calendar year?

Mr. GARFINKEL. Well, the social child support program I believe would not cost the general taxpayer much—it is conceivable that the

general taxpayer might even save some money because I would propose that a good deal of it be financed out of a tax on absent spouses and that tax would be a way of collecting child support from all absent spouses.

Senator MOYNIHAN. A tax on absent spouses?

Mr. GARFINKEL. Yes, sir.

Senator MOYNIHAN. That is a new idea.

Mr. GARFINKEL. It is a new idea. Perhaps I should say something more about it. It is a good idea.

Senator MOYNIHAN. I am sure it is if it comes from your distinguished center. It is a tax?

Mr. GARFINKEL. Yes.

Senator MOYNIHAN. The opposite of a bride tax, or something like that?

Mr. GARFINKEL. The basic idea of the social child support program is to, on the one hand, say to all guardians of children that they will automatically be entitled to the payment from the State, which will not depend upon their income, so it is not a welfare program. It is more like our social insurance program.

Now, most of those people happen to be women. About 95 percent of the children of splits go with the women, so I will speak as though it is women, but the program will not be a sexist one.

The program would be financed, in part, by a tax on the absent spouse, and it could be a proportional tax. For example, you could say for the first child you must pay 10 percent of your income in child support and whoever was caring for the kid would get either the 10 percent that the absent spouse was paying, or some minimum payment, whichever was higher.

For example, if the minimum payment were \$1,600 and someone had a taxable income of \$20,000—the caretaker of the child would get \$2,000 rather than the \$1,600. In that case, the general taxpayer is not paying anything more. It is entirely financed by the tax on the absent spouse.

Now take the case where you have someone earning only \$5,000 and you have to pay out \$1,600. The difference, the \$1,100 would have to come out of general revenues. However, we are already spending a good deal of that in aid to families with dependent children.

Senator MOYNIHAN. You are already spending a good deal of that.

Let me just ask you two questions. First—it is not a question, so much as it is just a point. I think you would find that hearings we have had and testimony that we have had suggest that because of the various disregards the real tax rates faced by welfare recipients are, in fact, often quite nominal and often lower than middle income taxpayers.

You can, in New York City, continue to receive welfare with an income of something like \$26,000 a year, as Dr. Blanche Bernstein has testified.

On the ethical issue of whether wages are too high or too low, a colleague here offers this comment: The ethical issue is not whether the wage is too high or too low, at least one could put it that way. The ethical issue is whether employment at a lower wage is preferable to unemployment at a higher one.

The effects of unemployment on the family structure, and so forth, seem to me to get closer to what we really are legitimately entitled to call ethical issues.

But, no matter. How much would this program cost?

MR. GARFINKEL. I do not have the exact costs.

SENATOR MOYNIHAN. Send us an estimate, will you not? We would like to have it in the record. Obviously the cost of the food stamp provision is a displacement of costs—I mean, you might just offset other costs, and we would very much like to hear it.

[The prepared statement and letter of Mr. Garfinkel follow:]

#### TESTIMONY OF DR. IRWIN GARFINKEL

Thank you for the opportunity to testify on what we have learned from the income maintenance and jobs experiments and the implications thereof for welfare reform.

We have learned a great deal from social experiments. Indeed, we lead the world in social science experimentation. Academics from other countries whom I have met admire our pioneering efforts in this area. Today, I will confine my remarks to only a few of the many things we have learned from the experiments about the effects of income maintenance and jobs programs on work effort and marital stability. Then I will talk about the policy implications of these findings.

I believe I am qualified to speak on this issue. I helped to analyze the results of the first income maintenance experiment and I am currently part of the research team that is evaluating the supported work experiment. Moreover, I have published widely in journals, and just recently in a book, on the labor supply effects of income maintenance programs.<sup>1</sup> Finally, for over a decade I have studied the labor supply question within the broader context of a general evaluation of alternative income maintenance programs, and I am now in the midst of writing a book about the policy implications of my technical research.

The income maintenance experiments were designed primarily to find out how such programs affected the work behavior of low-income people. What did we learn?

We learned that simultaneously increasing unearned incomes and reducing rewards for work will induce low-income people to work less. We also learned that how much less low-income people work depends upon how much we increase their incomes and decrease their net wages. To economists this was no surprise. Conventional economic theory predicted it.

We learned that how much less people with low incomes will work depends upon who they are, that is, upon what groups they belong to and what is therefore expected of them by society. The work reductions of married women and female heads are much larger, in percentage terms, than those of married men. Again, this is not surprising. Married men are expected to work. Raising children and doing other work in the home is at least an equally legitimate role as market work for married women and female heads.

Finally, we learned something about the magnitude of these effects. For example, on the basis of results from the Seattle-Denver Experiment, as reported in the Winter 1978 issue of the *Journal of Human Resources*,<sup>2</sup> we can now say that on average husbands participating in a negative income tax program with a poverty line guarantee and a 50 percent tax rate will work about 7 percent less than they would in the absence of a program. Similarly, if the tax rate is increased to 70 percent, the reduction in work will increase to 12 percent. The comparable percentages for wives are 22 and 28 percent.

We have also learned that our best estimates of the effects of income maintenance programs on work could easily be wrong. For example, on the one hand, the estimates from the experiments are too low because the experiments, which are temporary, increase long-run incomes of the poor by a smaller amount than would a real, permanent program. On the other hand, the estimates are too high because a temporary program reduces the reward for work only temporarily, and thus puts not working—leisure—temporarily on sale. And when something is on sale, people buy more of it during the sale. That our best estimates could easily be wrong suggests only that modesty and open-mindedness are in order. Policy decisions cannot await certainty.

<sup>1</sup>Masters, S. and Garfinkel, I. 1977. *Estimating the labor supply effects of income maintenance alternatives*. New York: Academic Press.

<sup>2</sup>Keeley, M. C., Robins, P. K., Spiegelman, R. G. and West, R. W. 1978. The labor supply effects and costs of alternative negative income tax programs. *Journal of Human Resources*, XIII (1), 3-36.

Although the experiments were not designed to measure the effects of income maintenance programs on marital stability, we now find, to our great surprise, that the experimental negative income tax programs seem to increase marital splits. I say "seem" only because I remain skeptical of and puzzled by these findings. The fact that the most generous plans induce the fewest splits and the least generous plans, which are most comparable to aid available to the control population for the experiment through AFDC and food stamps, induce the most splits is particularly puzzling. And, as yet we do not understand why. Still, I have not found any serious flaw in the experimental results, although I have looked for some.

Before the experiments most experts believed that aiding two-parent families would decrease marital splits. They believed that aiding only one-parent families increased splits because the man had to leave for the woman and children to become eligible for aid. If intact families were also eligible for aid, the man would not have to split. This kind of reasoning played an important role in the extension of the Aid to Dependent Children program in 1961 to unemployed parents and was a staple argument for a negative income tax program to aid the working poor all during the 1960s and early 1970s.

Assuming that we accept the results of the experiments as being correct, we must ask, "Why were we experts wrong?" We don't know yet for sure. My friend and colleague at the Institute for Research on Poverty, John Bishop, has speculated that providing welfare aid to intact families undermines the role of the male head. On the other hand, analysts at Stanford Research Institute have speculated that income from the experiment is more attractive to wives considering divorce than equivalent amounts from welfare would be. We cannot yet distinguish between these and other possible explanations. One thing I believe we have learned already from these marital stability results is that how you administer something can make a big difference. This may not appear surprising. But because economists specialize in measuring the effects of variations in incomes and prices, we have not paid sufficient attention to the effects of variations in administrative practices. Finally, I must also note that we do not know yet what effects the splits induced by the experiment have had on the former spouses and their children. This is an area where Congress should fund some follow-up studies.

From the Supported Work Experiment, we learned that if you offer minimally decent jobs—and I stress the word *minimally*—to AFDC mothers, ex-addicts, ex-convicts and unskilled, in-trouble, or trouble-prone teenagers, they will work substantially more than if you do not offer them such jobs. Whether this finding is surprising or not is hard to say. Most of the people at the Labor Department, the principal funder of the experiment, the Manpower Development Research Corporation, which is the organization that oversees the experiment, and the Institute for Research on Poverty and Mathematica Policy Research, which are the organizations evaluating the experiment, do not believe these results are very surprising. For all of them were raised on Keynesian economics; they believe, in at least one part of their heads or hearts, that there is such a thing as involuntary unemployment. Yet one of the latest developments in economic theory suggests that there is no such thing as involuntary unemployment. Unemployment, so goes the argument, is really just job search. The problem lies with the unemployed themselves. They are looking for too good a job.

Are the supported work jobs too good? They are dead-end jobs, lasting at most 12 months in some cities and 18 months in others. They pay less than \$3 per hour. But these jobs must obviously have been more attractive than the job opportunities available to the control population in the experiment. In one sense, then, the new job search theorists must be correct—the unemployed are looking for jobs that are too good relative to what is available to them at the moment. This is true by definition. Supported work reduces unemployment and increases labor force participation because it offers better job opportunities to participants than would otherwise be available to them. But are these jobs too good for the participants? Do they deserve less? To answer that question requires a value judgment.

Let us, for the moment, agree that all adults deserve to make at least \$3 per hour. Then it follows that there is something the matter with an economy that fails to generate a sufficient number of such jobs. Clearly, we should put our social scientists to work to figure out remedies.

But what if we agree that \$3 per hour is too high. Some will say \$2.50 is better, others \$2, and some, but not many, will say that no wage is too low. My guess is that most Americans would say that everyone should earn at least the minimum wage, if not higher. So long as you set some minimum, the previous conclusion



will hold—there is something the matter with the economy that needs fixing. Those who use search theory to argue otherwise are, in effect, implicitly making the ethical judgment that no wage is too low.

This last point leads very nicely into my discussion about the policy implications of these findings for welfare reform. For the first point to make is that there are no direct policy implications. Facts do not speak for themselves. Policy choices depend upon what we value as well as upon the way the world works.

Let me summarize briefly what the experiments tell us about how the world works and then lay out the value choices we must confront. The income maintenance experiments tell us if we improve the lot of the poor by increasing their unearned income and decreasing the rewards for work, they will work less, while the Supported Work Experiment tells us that if we improve the lot of the poor by increasing the attractiveness of work, they will work more. Does it follow that we should improve the lot of the poor only through increasing the attractiveness of jobs? Not necessarily. That depends upon how much value we put upon the work effort of the poor, and upon the relative costs to society of improving the lot of the poor through alternative strategies.

On the one hand, work is one of the most fundamental and important activities of human beings, essential for the well-being of both the individual and society. On the other hand, the progress of civilization may be measured in large part by the extent to which we have been able to free ourselves from the necessity of work. My values are such that if the poor work a little less, I conclude that is a tragedy neither for them nor for us. For example, married women in poor households currently work somewhat more than married women in middle- and upper-income households.<sup>3</sup> If, as a result of an income maintenance program, more poor women stay home to raise their children rather than work, there is no ground for alarm. Similarly, if poor men remain unemployed a bit longer to look for a better job, we needn't hit the panic button. But if, as a consequence of the unavailability of decent jobs and the availability of welfare, we were to induce the poor to work substantially less than the rest of us, that would, in my judgment, be tragic for us all.

What are the relative costs of improving the lot of the poor through jobs rather than cash or in-kind transfers? There is no simple answer. There are a large number of possible cash and in-kind transfer programs, and an equally large number of ways to improve the labor market opportunities of low-wage workers. How effective any particular program is will depend upon what other programs exist, whether the objective is to provide short-term or long-term aid, and many other factors. Providing cash for short-term unemployment, for example, is certain to be less costly than providing jobs or work relief. The key, it seems to me, is to retain balance. Relying too heavily on either transfers or labor market strategies is likely to be inefficient.

In a very simple economy, a jobs strategy would always dominate. Yet the one study we have of the cost-effectiveness of a special jobs program for those very difficult to employ suggests that the cost of employing these people is \$3000 to \$4000 more than the value of what they produce.<sup>4</sup> What that suggests to me is that to the greatest extent possible, we should try to place people in private and regular government jobs rather than special employment programs. To do that we must achieve full employment. Nothing will do more, in the long run, to reduce dependency and promote equality and efficiency than to achieve full employment. Finally, there is quite a bit of evidence to indicate that unemployment increases marital instability and full employment reduces it.<sup>5</sup>

I do not claim that I have the answer to the question, "How do we achieve full employment without runaway inflation?" Heretofore, this has not been the area of my expertise. But I am now trying to learn fast. I suspect that wage bill subsidies and other programs which subsidize low-wage labor, such as the new jobs tax credit, are likely to play key roles in such an endeavor. Moreover, I believe it is unwise for us to rule out consideration of wage and price controls. Some say wage and price controls are unacceptable. But is a 6 percent unemployment rate—40 percent for black teenagers—acceptable? If neither unemployment nor inflation are acceptable, then perhaps wage and price controls for some period of time may have to be acceptable.

<sup>3</sup> Garfinkel, I. and Haveman, R. 1977. *Earnings capacity, poverty, and inequality*. New York: Academic Press.

<sup>4</sup> Haveman, R. 1978. Dutch social employment programs. In J. L. Palmer (Ed.), *Creating jobs: Public employment programs and wage subsidies*. Washington, D.C.: The Brookings Institution.

<sup>5</sup> Bishop, J. *Jobs, cash transfers, and marital instability: A review of the evidence*. Institute for Research on Poverty, SR 19.

Besides improving pay and working conditions through full employment, there are two additional ways of increasing the relative attractiveness of work to the poor. First, the nonwork sources of income available to the poor, such as food stamps, can be reduced. My values are such that I would not recommend decreasing the attractiveness of existing options open to the poor. In general, they are poor precisely because they have no good options. What is the justification for taking away the few that they have? That it will be cheaper for us at these Hearings? Even if that were so, it is a poor excuse. They need the benefits a lot more than we need to pay less taxes.

Second, the tax rates in programs designed to aid the poor can be reduced, thereby increasing the net rewards for working. The best way to do this is to reduce the role and scope of welfare programs by increasing the proportion of aid to the poor that is provided through universal programs which benefit rich and poor alike. Welfare programs are designed to aid only the poor and by their very nature, therefore, tax them more heavily than everyone else is taxed in our society. For to reduce benefits as income increases is perfectly equivalent to taxing income. We measure the tax burden each of us pays by comparing our gross with our take-home pay. By that measure, the tax burden on the poor is very high. For example, for each dollar an AFDC mother earns in excess of \$30 per month, after deducting work-related expenses, her AFDC benefits are reduced by 66¢. The best estimate of the AFDC tax rate after work-related expenses are taken into account is 37 percent.<sup>6</sup> But food stamps are also reduced by about 30¢ for each dollar earned. Further, if the family lives in public housing, the rent will be raised as their income increases. And, for those who can earn enough to actually get off AFDC, the risk of losing Medicaid coverage acts like a very high tax on earnings. We don't know how to translate the risk of losing benefits into an exact tax rate, but most people in the field are convinced that it is an important deterrent to work. Finally, nearly all welfare reform proposals would increase tax rates in the AFDC program by reducing the scope of work-related reductions. Tax rates of 70 to 90 percent would be common.

The poor start out at birth with less chance of success than the nonpoor; they get less education and thus earn much less than the rest of us. A 50 percent benefit reduction rate reduces the returns for a job paying \$2.50 per hour to \$1.25. What is the justification for reducing, by even as much as 50 percent, the already small degree to which a poor person can improve himself, when we do not equally reduce the percentage by which much wealthier Americans can improve themselves? I see none. Placing the highest tax rates on the poor violates our commitment to equality of opportunity.

Another serious problem with welfare programs like Medicaid is that in many states they provide better medical care to the poor than is available to the lower middle class. This kind of social leapfrogging is unjust. The poor need more help than the near-poor and lower-middle class—inequalities should be narrowed. But positions should not be reversed. Leapfrogging is also perverse, for it creates powerful incentives for pauperization.

There are other serious problems with welfare programs. They place the poor in separate, politically vulnerable programs, stigmatize and rob them of their dignity, create incentives for the poor to cheat more than the rest of us, and exacerbate class tensions. In response to questions, I would be happy to elaborate on any of these other indictments of welfare.

I will elaborate briefly on the stigma issue, and relate it to the increase in family splitting that has been observed. If my colleague, John Bishop, is right in tracing the negative effect of experimental NIT programs on marital stability to an undermining of the male role, we must ask if the male role is undermined because the payments are welfare-type payments—that is, just for the losers in the economic game—or simply because the money isn't attributable to the husband's own efforts, past as well as present. I strongly suspect the former. If we had a universal children's allowance program for rich and poor alike, I do not believe that it would promote more marital splits than a program for which only children in split families were eligible. It might promote more splits than no programs at all by decreasing the unattractiveness of being a single parent to women. But in my judgment, this is an acceptable price for greater equality between men and women, and for increased well-being for single parents and their children—the overwhelming majority of whom would be single parents no matter what the economic incentives.

<sup>6</sup> Hutchens, R. M. 1978. Changes in AFDC tax rates, 1967-1971. *Journal of Human Resources*, XIII (1), 60-74.

But we need not undermine the absolute status of the unemployed male head by forcing him to apply for welfare. There are other, better ways to provide aid. An expanded unemployment insurance program is one. Full employment is another. An expanded earned income tax credit is a third.

Let me suggest four specific income maintenance reform measures that would substitute universal benefits for welfare benefits, and thereby simultaneously increase the incomes of the poor, and the relative attractiveness of work.

First, I would convert the benefits of the food stamp program into a refundable, \$600-per-person tax credit in the federal income tax, and administer the program through the Internal Revenue Service and the tax withholding system. The food stamp program is better than most welfare programs in that it has a lower tax rate—about 30 percent on earnings. That makes it relatively cheap to convert it into a universal program. It is also a low-guarantee program which makes it ideal for a basic, noncategorical income floor. Universal negative income tax advocates should realize that at long last they've won. Food stamps are a universal negative income tax. Their only problem is that the benefits are paid in funny money. Because of this, food stamps are worse than a cash welfare program. While shame discourages some eligibles from participating in *any* program designed to aid only the poor, a cash program submits participants to only one such potentially humiliating experience—when they apply. The food stamp program publicly identifies participants every time they purchase stamps and every time they use them. The food stamp program must also print, distribute, and redeem food stamps as well as determine eligibility and benefit entitlement. The administrative costs of the stamp distribution and redemption would be completely absent from the program I propose.

Most beneficiaries of the refundable tax credit program would receive their benefits automatically during the year in the form of lower withholding. Some will receive negative withholding benefits. Their paychecks will be supplemented by the benefit to which they were entitled. Certain groups who did not work, including the aged and disabled and some single parents, would be able to file an application for and receive full benefits during the year. All people would be entitled to receive whatever benefits they did not claim during the year when they file their income tax returns. All would be required to report on their income tax returns any benefits received during the year. The Wisconsin Homestead tax credit and the federal earned income tax credit have given us some experience with this kind of program. It can be done efficiently. The last British Conservative Government—I repeat, Conservative Government—proposed just such a refundable tax credit in 1972.

Cashing out food stamps will reduce administrative costs, the possibilities of fraud, and, most important, the extent to which welfare beneficiaries are singled out from the rest of the population and treated differently. Research indicates that very few families will buy less food as a result. A few families, of course, will buy less food. But the food stamp program is not an efficient vehicle to help the children who live in such families. We have many other institutions in society that help us to identify the few families who spend their money so unwisely that they harm their children. It is neither efficient nor wise to try to help those few through food stamps rather than cash, at the expense of the dignity and self-reliance of the many.

Second, we should restructure the social security program so that all beneficiaries get a benefit at least equal to the poverty line. The envelopes and checks are already addressed. We only have to change the amount. Doing so would eliminate the need for a large welfare program for the aged.

Third, we should enact a universal, social, child support program. The program would work as follows. All single adults caring for a child would be eligible for a child support payment from the state. The payment would not depend upon the income of the parent living with and caring for the child. Like our social security programs, rich and poor alike would participate in the same program.

The program would be financed principally by an income tax on absent spouses. Every adult would be required to file an income tax. All adults who had children not living with them would be required to pay an income surtax, proportional to their taxable income, for each absent child. For example, the tax could be 10 percent for the first child and 4 additional percentage points for each additional child. With the exception of those who abandon very large families, the poor would not pay much for the support of their absent children, but they would pay something. They would pay as much in proportion to their ability to afford it as the rich. The parent caring for the child would be entitled to a child care benefit

from the government which would equal either some minimum payment or the amount of tax paid by the absent spouse—whichever was larger. The difference between the minimum payment and the tax paid by the absent spouse would be financed from general revenues.

All women and all men who care alone for children would thereby be guaranteed child support, and all absent spouses would thereby pay child support.

Just compare the tax side of this proposal to the current system. We now hound men one at a time through the courts to get them to pay child support. The Federal Government proudly proclaims that in many states the benefit-cost ratio of its child support enforcement plan is greater than one. If the benefit-cost ratio is equal to 1, that says that it costs the state \$1 to raise \$1 of revenue! That is the definition of a very inefficient tax! Even if the benefit-cost ratio were 10 to 1, it would be an inefficient tax. How many taxes require 10¢ to raise \$1 of revenue?

Fourth, we should adopt a comprehensive National Health Insurance program of the kind favored by Senator Kennedy. Medicaid is one of our worst welfare programs. It separates the poor from the rest of us and lifts some of them above the nonwelfare poor. The medical care industry does not function efficiently in the free market. All Americans pay for this inefficiency, and we all want insurance coverage. Here is a case where we can help the poor in the process of helping ourselves.

All of these programs would reduce tax rates on the poor and integrate them into the mainstream of society. Some—though not all—of them will cost more money initially to those of us at the top of the economic pyramid. But I am convinced that those of us who have already benefited so much from our wonderfully productive economy will find that this is a very good investment. It's also an investment we can take pride in. Thank you.

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INSTITUTE FOR RESEARCH ON POVERTY,  
THE UNIVERSITY OF WISCONSIN,  
Madison, Wis., December 15, 1978.

Senator DANIEL PATRICK MOYNIHAN,  
*Dirksen Senate Building,*  
*Washington, D.C.*

DEAR SENATOR MOYNIHAN: Thank you for your very complimentary letter of November 22. I'm not sure I deserve it. I had very little time to prepare my testimony and was not pleased with several parts of it. Mike Stern indicated to me that some revisions would be okay if the changes were not wholesale. I tried to make a few changes as possible. I hope you have no objections to using the revised draft.

Let me take this time to respond more fully to four questions you asked me at the hearings and to one question you asked another witness.

First, with regard to tax rates in welfare programs, it is true as one of your staff members pointed out that studies have indicated that actual tax rates in the AFDC program are lower than 66 percent. Moreover, in a few cases, women with quite high incomes receive AFDC benefits. But it is not true that AFDC mothers in general face lower tax rates than those subject to the positive income tax. Most of the path-breaking work on actual tax rates in AFDC was done at the Institute by Heffernan, Lurie, and Hutchens. So I am quite familiar with their work. The best and most recent study is by Hutchens in the Winter 1978 issue *Journal of Human Resources*. He estimates the AFDC tax rate to be about 37 percent. This is already much higher than the tax rate faced by the overwhelming majority of Americans in the positive income tax. Moreover, it takes no account of the food stamp tax rate or the effects of public housing and Medicaid. Finally, nearly all welfare reform proposals would raise tax rates by tightening up on work related expense deductions. I am sorry that my written testimony was simplified to the point of being misleading on this point. But the welfare poor do face the highest tax rates in our society and this would be exacerbated by most welfare reform proposals.

Second, you asked me about the costs of the programs I proposed. By spring I hope to have cost and distributional estimates of converting the food stamps program to a \$600 per capita refundable credit in the income tax and restructuring the social security program to include poverty level minimum benefits. It will take longer to develop cost estimates of the social child support program because of the lack of good matched data on families and absent spouses.

Estimates of the budget and real economic costs of comprehensive national health insurance programs are already available at HEW.

Third, you asked me about the ethics of experimenting with people in such a way that we induced family break-ups. All social and economic policy making in a democracy is in some sense an experiment. It may be that AFDC-UP also breaks up families though when we enacted the program in 1961 we thought the opposite. Two key questions are how closely we monitor the effects of social and economic policy and how many people are effected by the policy. Genuine experiments entail the closest possible monitoring of effects and effect a much smaller number of people than full scale implementation of new programs. If the policy is a good one, the cost of experimentation is that the benefits of the program are confined to too few people. But if there are negative unexpected side effects of the policy (e.g. marital splitting), then experimentation is clearly to be preferred to full scale implementation. It seems to me that this is the real ethical dilemma of social science experimentation.

Fourth, you asked if the ethical issue isn't whether employment at a lower wage is preferable to unemployment at a higher wage? I would respond that contrary to what many economists assert, we can raise the minimum wage without increasing unemployment—if we are willing to subsidize wage rates through a wage bill subsidy.

Finally, I hope you will not mind if I take the liberty of answering a question you put to another witness. According to news accounts that I read, you asked Bob Speigelman why the minority groups had larger responses and he replied that he had no explanation. I'd like to offer a simple common sense explanation that is consistent with economic theory, my empirical findings on labor supply and my testimony before your committee. To wit: any individual black or hispanic on average will have worse labor market opportunities than whites—even holding income constant—because of labor market discrimination. Consequently if you give blacks and hispanics the same amount of unearned income and reduce their wage rates by the same percentage as whites you will create a bigger shift in the relative attractiveness of not working for blacks and hispanics than for whites. No wonder then that the Seattle-Denver experiment resulted in larger labor supply reductions for minority group members. Same thing for wives. The experiment changed the relative attractiveness of work for them more than for husbands too and their percentage reductions in labor supply are also greater. In short, there is nothing mysterious about these findings.

Sincerely,

IRWIN GARFINKEL, *Director.*

Senator MOYNIHAN. It is 20 minutes to 2. You have all been patient. Our guests today—who is here from HEW?

I knew that this time, Dr. Aaron, would have some people here to take notes, and you are very welcome.

We will be back at 2:30 when we will resume with Mr. Spiegelman.

[Whereupon, at 1:40, the hearing in the above-entitled matter recessed, to reconvene at 2:30 p.m. this same day.]

#### AFTERNOON SESSION

Senator MOYNIHAN. The committee will come to order and we will begin this afternoon where we ought to have left off this morning. We are honored, once again, to have before us Dr. Robert Spiegelman who is the director of the Center for the Study of Welfare Policy of the SRI International.

Dr. Spiegelman, I see that you have colleagues with you. I wonder if you would introduce them?

Mr. SPIEGELMAN. Yes, I will. Thank you.

On my right is Philip Robins and on my left, Lyle Groeneveld, both of Stanford Research Institute.

Senator MOYNIHAN. Mr. Robins and Mr. Groeneveld, we welcome you to this committee. We welcome you again, sir.

Mr. SPIEGELMAN. Thank you, sir.

I have introduced our larger testimony into the record, which I hope you will accept.

Senator MOYNIHAN. We will be happy to have that printed as if read.

**STATEMENT OF ROBERT SPIEGELMAN, DIRECTOR, CENTER FOR THE STUDY OF WELFARE POLICY, STANFORD RESEARCH INSTITUTE INTERNATIONAL**

Mr. SPIEGELMAN. Since you have been reading about the Seattle-Denver experiments for the past several months, I thought it might please you to know that these experiments were conducted by real people, not computers or ghosts, so we are here before you for the first time. Although we were before the committee before, we did not have the opportunity to speak to you. We now have that opportunity.

We are part of a large team, in fact, of economists and sociologists who have been working on the SIME-DIME for the better part of a decade. We started designing this back in 1969.

These experiments were designed by this group and we have been responsible for the operations and the evaluation of these experiments since their beginning. Some of this work was through subcontract with Mathematica and to the community colleges of Denver and Seattle.

Of particular interest, I know, is the experimental impacts on work effort and on marital stability and these I will address most completely. One should be aware that we have actually produced some 55 reports over the last 3 years in various aspects of the experiment, including impacts on consumption, impacts on migration, and various other things. We will not talk to those today.

I will repeat somewhat my earlier testimony because the experiment is so complex that it is hard to start anyplace in the middle and make sense out of the results.

You are aware that the Seattle-Denver experiments tested 11 different programs. The lowest of them was at 90 percent of the poverty line; the highest of them at 140 percent of the poverty line. These compare with the programs you are now considering that run 60 percent to 80 percent of the poverty line.

The results of our investigations, the impact of an NIT on work effort indicated that the size of the effect depended upon both the level of support and the tax rate of the program. However, none of the programs tested caused massive work reductions of male heads of families—we will talk about females later.

Using a microsimulation technique, we have extrapolated these experimental findings to the Nation as a whole and we estimated that a program with a 50-percent tax rate and a guarantee level that is 75 percent of the poverty line would produce 6-percent reduction in hours of work for husbands and a 23-percent reduction for wives in those families receiving benefits.

The total reduction in the hours of work for the estimated 7.5 million husband-wife families who would receive benefits under the program would be less than 10 percent. The 3 million single-parent families who would receive benefits would reduce their hours of work by 7 percent.

The estimated cost of this program was about \$10 million in 1974 dollars—we have not updated those and work-effort reduction accounted for about one-fourth of the cost—that is, the earnings replacement accounts for about one-fourth of the total cost of an \$8 billion program at 75 percent of the poverty line with a 50-percent tax reduction.

Senator MOYNIHAN. That is not far off the President's proposal.

Mr. SPIEGELMAN. That is right. We picked this one to talk about because it is the closest of the one we simulated to the President's program. I have heard words like "earnings replacement is 50 percent of the cost of the program." Well, it is true that earnings replacement in some programs was that, but in these that are much closer to the President, we are talking about more like 25 percent earnings replacement.

Our earlier analysis yields several important conclusions. First, and perhaps most important, both the labor supply response and the costs of the nationwide NIT program vary widely with relatively small changes in the program guarantee level or the program tax rate.

A program with a guarantee at 100 percent at the poverty level and a tax rate of 50 percent would cost \$40 billion more now in 1978 dollars than the existing welfare system, whereas a program with a guarantee of 50 percent of the poverty level and a tax rate of 70 percent, which is the least generous one we ever tested, would cost \$5 billion less than the existing welfare system.

State supplementation would add to these costs, of course.

Furthermore, adjustments of hours of work in response to these programs range between 25 percent and 55 percent for the programs tested.

The high tax rate program reduces hours of work for program beneficiaries more than a low tax rate program but leads to lower aggregate program costs. This reduction occurs because high tax rates lower the break-even level and therefore decrease the number of families receiving benefits. Using only a cash strategy, there does not appear to be any way out of the dilemma that a low guarantee, low tax rate program has a small work disincentive effect but may provide inadequate benefits for persons without earnings, while a high guarantee, high tax rate program may provide adequate benefits for persons with no earnings but also has a substantial work disincentive effect.

Our microsimulations also provided estimates of the effect on the current welfare population. An NIT program with support at 75 percent of the poverty level and 50 percent taxed, made 69 percent of the existing welfare population worse off. Even if the guarantee is raised to 100 percent of the poverty level, 24 percent of the welfare recipients would be worse off.

This implies the need for substantial State supplementation in order to achieve a hold-harmless condition.

The reduction in work effort manifests itself not mainly, however, as quitting jobs, but really as a slowdown of entry into jobs by those previously unemployed.

Let me emphasize this again. In comparing the controls and the experimentals, what we see is really not so much the experimentals reduce their hours of work under the experiment, but rather they increase their hours of work in many cases far less than the controls. So it is really an entry into work that is being reduced, not so much the worker quitting his job.

Now, for women especially, the implications of this becomes unclear. If the labor market is unable to absorb all of those who would want to work, then the effects of an NIT in the future may translate at least partly into less unemployment rather than fewer hours of work. So a lot depends on the course of the total economy.

Moreover, our results do not show the added benefits that derive from a wife or female head spending more time in the home with her children. We did not address that question, of the benefits from the female in her family.

Our recent work is focused on identifying differences in responses among racial and ethnic groups, between the two sites, Seattle and Denver, and over the duration of the experiment.

Our recent efforts indicate that, while our general results still hold, there are some differences in response. Nonwhite groups appear to respond more strongly than whites. The responses appear to be somewhat greater in Denver than in Seattle, and the response appeared to be somewhat greater for the 5-year program than for the 3-year program.

We have no explanation for the racial-ethnic differences at this time.

The site differences may reflect the higher unemployment rate in Seattle and thus the greater unwillingness of people to leave their jobs in a short experiment in which they knew there would be difficulty in getting it back in a period of high unemployment.

A criticism of experimental results is that they are of short duration, and this short duration biases the measurement of effects. Ms. Allen talked to you about that some this morning.

Well, SIME-DIME did largely avoid this problem by having families on programs of different durations. We had both a 3-year program and a 5-year program.

In general, we do find that the 5-year program people responded more—in other words, reduced their work efforts more than the 3-year program people. However, these differences are only sometimes statistically significant, so we do not have great confidence yet in this 3- to 5-year difference in effects.

And, furthermore, they are relevant to support programs at the high levels that we have tested. Ms. Allen quoted the work of Metcalf who has demonstrated that the short duration program has most effect on high income programs, so that when we reduce the income of a program, it would be less likely that the 3- and the 5-year difference would be important.

So in a program like the administration's, it is not to be expected that this kind of effect would appear in that level of a program.

To date, our investigations have concentrated on the heads of families. We just started investigating the effects of the experiment on the work efforts of youths who were secondary members of experimental families at the time of enrollment.

In a recent study of such youths aged 16 to 21 at the time of enrollment, we found evidence of substantial reduction in work effort. The male youths reduced their work effort on the average 23 percent, the female youth, 18 percent.

We have not yet found any evidence that this lower work effort has taken the form of increased time in school. Moreover, the males—

Senator MOYNIHAN. Have you not found it, or have you not gotten around to looking for it?



Mr. SPIEGELMAN. We have been looking at it and we just do not find it. We are not convinced we have done everything we can with the data to indicate this, but our first look at this in our first report is that we cannot find any evidence of increased schooling taking the place of the decreased work.

If that holds up, then that would be the conclusion. It is not there.

Moreover, among males, the reduction of hours of work is greatest among those who marry and become heads of families. The reduction of work effort in youths primarily takes a form of lower entry into the labor force rather than absolute deductions in hours of work, since both controls and experimentals increased hours of work over the period. This is the same kind of result we found with the heads. It is just more striking in the youths because so many youths increase their hours of work during their 16- to 21-year period.

The large proportionate reduction in work effort by male nonheads may have serious longrun consequences. We are now looking into this. We do not really know how to evaluate this yet.

The second major area of our work has been the measurement of the impacts on marital stability, and we are quite aware that our findings have caused a stir in political circles. The findings of the experimental NIT program is that increased marital dissolution ran counter to the expectations of most welfare reform proponents.

Our explanation is that the increase in marital dissolution involves three factors: There is an income effect, an independence effect, and a welfare discount effect.

The income effect reduces dissolution rates by making couples financially better off. The independence effect increases dissolution rates by making the alternative to marriage more attractive financially. The welfare discount effect refers to nonpecuniary differences between current welfare programs and our experimental programs that cause the effect of a given benefit from the current system to be less than the effect of benefits from the experimental NIT program.

This discounting may be the result of the stigma that is attached to the existing welfare system; to the level of information about benefits, which appears to be greater in the NIT program; or to the ease of obtaining benefits which is also greater, or is at least perceived to be greater by a woman still in the marriage state than is true in the existing AFDC system.

Our recent work on marital stability has concentrated on refining estimates, extending the time period of analysis, and testing the sensitivity of results to alternative assumptions and methods of analysis. Our new results show a consistent pattern of experimental effects for whites, for blacks, and for Chicanos, although the Chicano results are not statistically significant.

The effects on separation rates over the 3 years are smaller than they were over 2 years of the experiment, but this finding is not surprising, because the sample does not have the normal replenishment characteristics of the population at large.

Senator MOYNIHAN. That is the sample exhaustion effect?

Mr. SPIEGELMAN. That is panel fatigue.

Senator MOYNIHAN. Panel fatigue.

Mr. SPIEGELMAN. They are getting tired.

I would like to reflect for a moment on the national implications of these marital stability findings. What we have demonstrated is that

the experimental NIT program increased dissolution rates relative to the control group. The difference between the effects observed on the experimental program and a national NIT program could be substantial, however.

We cannot state with certainty whether a national program would have larger or smaller effects than we have observed, but our findings certainly suggest that the effects on marital stability will be large.

Our research so far has been limited to the effects in the making and breaking of marriages. It has not said anything about the effects on the personal well-being or the quality of family life. We will try to address some of these questions over the next 2 years, however.

In conclusion, we found no basis for refuting the preliminary results of the experiment which had been reported to the committee last May, either with regard to effects on work effort or marital status. These results emphasized the economic and social costs of a universal income maintenance program.

These do not speak to the benefits in terms of a more equitable distribution of income or lower administration costs or improved well-being of children in low-income families, which also will result from such a program. These, too, must be considered.

I would like to make one comment on a statement that you made earlier, if I may, this morning. I am not quoting you correctly, but it implied something of the sort that information seems to kill programs. It was a fairly startling kind of observation.

I would hope it is not just a sad commentary on the policymaking process. I believe that information should be looked at as a benefit. It is only if we regard programs as sort of a take-it-or-leave-it kind of thing does information get in the way.

Rather, if we look at program development as an evolutionary process, then information must be regarded as a positive benefit in making that process converge on an optimal solution for the system as a whole.

I would rather think we should be having more information than less. I think you really agree with that.

Senator MOYNIHAN. I very much agree with it. Political scientists a few years back talked about unanticipated consequences and generally speaking, if you knew all the consequences of the things you were going to do you either would not do them or you would have a different coalition that would agree to have them done.

The fundamental question is how do you get together and get the majority behind an issue, and if you know the real effects, well, they are perhaps going to be different from the anticipated ones. You might have a different majority. You might have no majority, or you might have unanimity. The range is considerable.

Mr. SPIEGELMAN. I think we are also learning something from our experience from manpower programs, that one can get, by not having the right preinformation, by not properly designing programs, one can get more discouragement than is really warranted from the failures of programs.

Senator MOYNIHAN. There is a great deal of social policy which is launched with a relatively naive set of expectations about how malleable behavior is. It is not very malleable and if you do not know that, and finding it out disappoints you, well, welcome to the company of

grownup people and how do you live in the world that you were born in?

But, sir, I still have to ask you, first of all, you say in your testimony which is superb—and I also want to thank Mr. Robins and Mr. Groeneveld for this superb paper—you have a statement here that “Our findings have been circulating publicly for 4 years and they have been put under a lot of pressure”—as they should.

Our previous witness mentioned that he had put the marital dissolution findings under a lot of pressure and could not find anything the matter with it and had to give it up. But earlier in the day a journalist asked whether the program, the SIME-DIME experiments, were like the proposals that President Carter proposed last year and as I recall I said well, they were of that family, yes.

Would you say that? I am not trying to suggest any response to you, but to my saying they are in that family of exercises, what would you say?

Mr. SPIEGELMAN. Yes; well, certainly the upper tier cash program of the program for better jobs and income is identical in structure, almost identical in structure with these. The lower tier program creates certainly far less benefits for the two-parent family, so there would be less of an income effect. I would say that would work in the direction of more divorce than less divorce. In other words, if you were going to compare our program with the Carter program—

Senator MOYNIHAN. You mean the lower tier program would produce more.

Mr. SPIEGELMAN. Right.

The other side of the coin, as everyone has mentioned, the “jobs aspect” of this does throw a different category of influences and their impact, I do not think we are prepared to talk to. I just do not know. They could be plus or they could be minus. There are many ways in which they could go both ways.

I do not know what their net effect would be.

Senator MOYNIHAN. I am sort of sorry about these hearings. I am certainly sorry about your findings. I wish they had been that everything we thought we should do was what we should do and that would be it.

But that is what intellectual work is about. It comes up with things you do not want to hear once in awhile. But we need to know them; we need to know.

If you were asked to devise a national income maintenance and job provision proposal for the next Congress—

Mr. SPIEGELMAN. Asked to do what? I am sorry?

Senator MOYNIHAN. To devise a program, a better jobs and income program, or call it what you will—the SRI international program—would you be concerned about your findings?

Mr. SPIEGELMAN. Oh, I definitely would be concerned about them. I would want to take into account as much as would be possible to take into account in devising a program, I think I might be prepared to live with some of this, but—

Senator MOYNIHAN. Could I ask, have you testified before any other committees of Congress, other than ours, on this matter?

Mr. SPIEGELMAN. No.

Senator MOYNIHAN. They didn't ask you on the House side?

Mr. SPIEGELMAN. If I remember correctly, the House side hearings were very early and our results were really just being formulated.

Senator MOYNIHAN. Those hearings went on for the better part of a year. You say this has been in circulation for about 4 years.

Mr. SPIEGELMAN. Well, some results have. We have been coming out with—

Senator MOYNIHAN. You have been turning out work.

Mr. SPIEGELMAN. Yes, this has been going on for a long time.

Senator MOYNIHAN. I have to say I think there has been an avoidance of this information.

Mr. SPIEGELMAN. I would characterize it differently, especially this marital information which is causing most of the stir. As you have pointed out yourself, it was contrary to what we expected to find. They were based on very complex modeling.

I think it took a great deal of look and search before we were prepared to stand up and testify that these were reasonable findings or before HEW was prepared to—

Senator MOYNIHAN. Let me be clear. When was the first time at which you would say you were pretty well prepared to testify? The GAO will not be called in to check your testimony.

Mr. SPIEGELMAN. Let's see. It appears that the first time that we really published a report that we had results that we were prepared to go with at all was in December of 1976. That was an 18-month study. That was our first real findings in this area.

We had some preliminary look at this that went back even 2 years before that, but that is—

Senator MOYNIHAN. December of 1976?

Mr. SPIEGELMAN. Yes; I would say that was the date.

Senator MOYNIHAN. Well, you know, the House hearings only began a year ago, in November of 1977.

Mr. SPIEGELMAN. Well, we were not asked to appear.

Senator MOYNIHAN. Yes; but HEW began testifying on this program. The Department of Health, Education, and Welfare had your mature results when the present group of persons came to office; did they not? I mean, they came and they were sworn in in January 1977.

Yes; they did. And we did not hear a word about it, not a word.

Mr. SPIEGELMAN. I am sort of sympathetic to them not having rushed forward, because they spent a long time analyzing—we had a conference just last February in which—it was called together by HEW—sort of eminences from around the country, including Dr. Garfinkel and others, to really review it. They had been reviewing our results.

Senator MOYNIHAN. They most certainly ought not to have rushed in and said, "stop everything, we have new, solid, final, definitive information," but they might have given us a hint that there might be some important things coming along. What do you think we put up the \$28 million for?

I think: we are getting—

Mr. SPIEGELMAN. It is a matter of timing. You have to judge.

Senator MOYNIHAN. I will. I do have to judge, and I will. I mean, we are all human, I think.

What is your advice to us?

Mr. SPIEGELMAN. That is a very heavy load, sir.

Senator MOYNIHAN. Do not feel that you have to give any. You can stand right on your scholarly rights and say, give me an experiment. I will conduct it and tell you what I found out.

Mr. SPIEGELMAN. I think there are certain implications that are coming out. The marriage ones are the most difficult for me to figure out what to do with, in a sense, for it depends a lot on what the causes of those marriage dissolutions are.

For instance, if it is really the independence of the wife that is causing this divorce, then that is going to be very hard to get around in any kind of program I can think of. I mean, one could devise one, but I think one would not like it—like you cannot get welfare benefits for 2 months after getting a divorce. Nobody wants that kind of thing.

If, on the other hand, the results are more traditional in that it is the husband essentially leaving or being forced out in some way, then I think that is much easier to deal with in the program. The jobs type program, a wage type supplementation, these kind of things I think will deal with that side of it much more easily.

So it is going to depend on a little more work to know the cause from the marriage side.

Senator MOYNIHAN. Would you like to see some more experiments along that line take place before we make a big decision?

Mr. SPIEGELMAN. Well, I would say if the marriage issue raises its head as being a decisive factor in the decision, then I think our experiments could have been designed better to get at marriage results.

Our assignment problems, our whole selection of families, our whole selection process, was aimed at the labor supply results.

Senator MOYNIHAN. Well, the labor supply results are not spectacular. There are people who think that decline is very considerable.

Mr. SPIEGELMAN. Yes, and I think things like the stronger work requirements or better job opportunities can. I think, perhaps take care of some of the labor supply effects—or we just buy a lower labor supply.

Senator MOYNIHAN. I will offer my own judgment. Dr. Garfinkel spoke of our having “induced” these family breakups in a great number of these experimental cases, and you heard me say, and I was very serious in that book I wrote 11 years ago, in saying that the social sciences had to give some attention to the ethics of experimenting with communities in the way that biomedical scientists have taken great pains to say, what is it you can and cannot do with human beings—and, for that matter, with animals.

What are the limits of experiments with communities? You write it all up and say, “Gee, it was very interesting. Everything went to hell.” What about those people you leave behind?

By which I do not mean—if we had, on a scale of events that somebody reduces their work effort 17 percent, or whatever, well, so what? There are certain events to which you say, well, it is only money, and maybe it is a good thing—and, you know, money is a relatively impersonal thing.

But when a child loses a father, that is a large event in that child’s life. That is when you really are at the center of the human experience and must ask whether the large risks here are really warranted?

I would like to get John Rawls down here to tell me. Do they have a Department at SRI on that?

Mr. SPIEGELMAN. On what?

Senator MOYNIHAN. On ethical responsibilities?

Mr. SPIEGELMAN. Well, yes. Of course, they do have a committee which reviews this.

Senator MOYNIHAN. How much of a risk can you take? What is the permissible level of risk? Doctors do that all the time when they make interventions, and epidemiologists do that.

Mr. SPIEGELMAN. Your point is well-raised. I would like some time to think about it.

Senator MOYNIHAN. Well, listen, would you?

Mr. SPIEGELMAN. I would be happy to try to respond to you.

Senator MOYNIHAN. It is a serious question and you should not have a quick answer.

There are more dramatic things all the time and they enter the realm of public policy and we are not prepared for them. How much money is a human life worth?

Mr. SPIEGELMAN. I suppose the only defense of our kind of experimentation is that when one says one induced divorce, one might be overstating the case. We provided an alternative to the marriage that was a totally free choice on the part of the mother.

Senator MOYNIHAN. When I say, how much is a life worth, remember that we do decide how many kidney dialysis machines to provide. We put a price on it, so to speak. This is not a choice that public policy faced 25 years ago. There were no kidney dialysis machines. Nobody in the Bureau of the Budget had to say, "Well, I am going to let 100,000 people die this year because it would cost too much money to save their lives."

Science and technology induce awful events and choices, and you might say that, well, we did not induce anything, but what if you knew a certain arrangement had a statistical probability of producing an outcome different from another outcome? Well, you have a certain responsibility for that difference, do you not, if you pursue it? You do, and it is not easy. It is not easy.

Would you think about that for us? We respect so much the work that you have done. We respect the integrity and the care with which you have brought it forward. I would hope you would not feel that you have, in any way, inhibited the policy formation process. I mean, we are going to grow up and we are going to mature the hard way. The bringing of systematic inquiry to bear on social issues is not an easy thing. There is no guarantee of pleasant and simple answers, but if you make a commitment to an experimental mode it seems to me—I am not enjoying this hearing one damn bit, but if you make a commitment to an experimental mode, something larger is at stake when you begin to have to deal with the results than just with a specific program.

I mean if we find results that are unwelcome and then reject the mode, then what are we?

Mr. SPIEGELMAN. We have created somewhat of a Catch-22 then because if you do not like the results from the experiment in the marital sense, I think, as Dr. Garfinkel said, you are certainly better off having had them on the experimental level than on the program level.

Senator MOYNIHAN. That is exactly the result, but now we face an honest and genuine problem, worthy of mature people; can we devise a national program in the same family of arrangements that will not have this effect?

Mr. SPIEGELMAN. That is the test. If you do not like the results, that is hopefully what information has provided the ground rules to go in that direction.

Senator MOYNIHAN. But you would certainly think you could pursue another 5 years of experiments and learn something more about job patterns and marital problems of the kind you have suggested.

Well, sir, I will not keep asking you questions which you are very properly reluctant to speculate about, but I would hope that you might send us your thoughts on that, and Mr. Robins and Mr. Groeneveld.\*

I want to repeat what I hope you will take back to Stanford, the word that this committee is very much in your debt and that of your colleagues and that you have had 10 hard years' work and you have produced dramatic information that is as important as anything I have seen in my lifetime to the formulation of judgments about large social programs, and we appreciate it and hope you have some sense of achievement.

Thank you.

[The prepared statement of Mr. Spiegelman follows:]

**ADDITIONAL EVIDENCE ON THE WORK EFFORT AND MARITAL STABILITY EFFECTS OF THE SEATTLE AND DENVER INCOME MAINTENANCE EXPERIMENTS**

(By Robert G. Spiegelman, Lyle P. Groeneveld, and Phillip Robins)

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**WORK EFFORT RESPONSE**

*Summary and implications of previous testimony*

In our previous testimony to the Subcommittee on Public Assistance (May 1, 1978) regarding the work effort effects of the Seattle and Denver Income Maintenance Experiments (SIME/DIME), we emphasized the relationship between the size of the response and the program guarantee level and tax rate. Identifying guarantee and tax effects is critical for evaluating alternative welfare reform proposals because each program parameter has a different impact on hours of work and has different implications for the adequacy of the program and the effect of the program on equalizing the distribution of income.

Economic theory predicts that the combination of a guaranteed annual income and a reduction in the net return from work will reduce hours of work. The guarantee reduces hours of work because it provides a source of income that enables families to maintain a given level of consumption without having to work as many hours. The tax rate reduces hours of work because it lowers the economic return to working additional hours.

The statistical methodology used to estimate the experimental effects in SIME/DIME enabled us to extrapolate the results to the national population using the technique of microsimulation. The microsimulation model predicts work effort response, program costs, and caseloads for a given NIT program. A response function estimated using experimental data and eligibility regulations for a particular program are applied to a representative national data base containing disaggregated information about individuals and families. Each eligible person's response depends on the particular NIT program being considered and on a variety of socioeconomic characteristics of the person and his or her family. To generalize the Seattle-Denver results, a version of the Micro Analysis of Transfer to Households (MATH) model was used. This model was developed jointly by SRI International and Mathematical Policy Research to assess the effects of alternative nationwide income maintenance programs. Another version of the model was used by the Department of Health, Education, and Welfare to provide estimates of the work effort responses and costs of the Carter administration's recent welfare reform proposal, the Program for Better Jobs and Income.

\*An additional communication was subsequently submitted to the committee and appears on page 424.

Tables 1 and 2 summarize some of the simulation results presented in our earlier testimony. These results have several important implications for welfare reform. First, and perhaps most important, both the labor supply responses and costs of a nationwide NIT program vary widely with relatively small changes in the program guarantee level or the program tax rate. The most generous program simulated (with a guarantee of 100 percent of the poverty level and a tax rate of 50 percent) would cost \$40.5 billion more (in 1978 dollar) than the existing welfare system, whereas, the least generous program (with a guarantee of 75 percent of the poverty level and a tax rate of 70 percent) would cost \$3.0 billion more than the existing welfare system. (State supplementation would add substantially to these cost estimates). Furthermore, adjustments of hours of work in response to these programs account for a substantial fraction of their costs, ranging from 23 percent to 55 percent of total costs.

TABLE 1.—AVERAGE CHANGES IN HOURS OF WORK PER YEAR IN RESPONSE TO A UNIVERSAL NIT PROGRAM  
(Percent per year)

Guarantee level	Tax rate			
	50 percent		70 percent	
	Participants	U.S. population	Participants	U.S. population
75 percent of poverty level:				
Husbands.....	-5.9	-1.0	-11.2	-0.5
Wives.....	-22.8	-2.4	-32.5	-6
Female heads.....	-6.7	-2.4	-9.3	-1.2
100 percent of poverty level:				
Husbands.....	-6.2	-2.4	-10.1	-1.2
Wives.....	-22.7	-6.3	-32.0	-2.3
Female heads.....	-12.0	-7.1	-14.9	-5.3

TABLE 2.—COSTS OF A UNIVERSAL NIT PROGRAM IN ADDITION TO EXISTING WELFARE COSTS  
(Amounts in billions of dollars in 1978; percent due to work effort response in parentheses)

Guarantee level	Tax rate			
	50 percent		70 percent	
	Amount	Percent	Amount	Percent
75 percent of poverty level.....	\$10.8	(30)	\$3	(55)
100 percent of poverty level.....	40.5	(23)	17	(28)

Second, although higher tax rates substantially reduce hours of work for program participants, they lead to smaller aggregate reductions in hours of work and smaller program costs than lower tax rates. Thus, it is possible adequate minimum benefits (i.e. a high guarantee level) can only be ensured feasible only if the tax rate of the program is high. But under such a program, participants may become dependent on welfare because of the very low economic return from working.

Because programs with higher guarantee levels and higher tax rates discourage work, a fundamental dilemma arises in designing an optimal program. Should a program with a low guarantee level and a low tax rate be preferred to a program with a high guarantee level and a high tax rate when both programs cost the same? The low guarantee, low tax rate program has less of a work disincentive effect, but may provide inadequate benefits for persons without earnings. The high guarantee, high tax rate program provides adequate benefits for persons with no earnings, but also discourages work effort more for the program participants. Unfortunately, because of the relatively large work effort responses to the programs simulated, there appears to be no easy solution to the tradeoff between benefit adequacy and the incentive to work.

#### Further evidence

Since our previous testimony, we have completed several additional studies regarding work effort responses to SIME/DIME. These studies focus on the time pattern of response and variation in the response by race-ethnic group (blacks, whites, Chicanos), site (Seattle and Denver), and duration of the experimental program (3-year and 5-year). In addition, we have evidence on the work effort responses of another major demographic group, young nonheads, and have explored further the source of the work effort response of family heads.



*Variation in work effort response over time*

To study variation in work effort response over time, we use data from the first two and one-half years of the experiments. Data beyond that period are not used because the 3-year program would be drawing to a close and a winding down effect would be observed for a large portion of the sample. Subsequent analysis has shown that two and one-half years of data are sufficient to estimate long run responses to an NIT program.

Table 3 shows how the work effort response varies over time and provides an estimate of the long run response to the programs tested in SIME/DIME. As this table indicates, the long run responses of husbands and wives are fairly close to their responses in the second year. For female heads, the long run response is substantially larger than the response in the second year. This suggests that our earlier estimates, which were based on the second year of data, are probably accurate for husbands and wives, but understate the long response of female heads. Since this group comprises a small part of the U.S. population, the nationwide estimates were provided in our earlier testimony are likely to be unaffected.

TABLE 3.—ESTIMATED EXPERIMENTAL EFFECTS FOR HEADS OF FAMILIES BY RACE ETHNICITY, SITE, AND EXPERIMENTAL DURATION

[Effects measured in hours of work per year]

	Experimental effect				Mean hours of work of control group in 2d yr
	1st yr	2d yr	1st half of 3d yr	Estimated long-run effect	
<b>HUSBANDS</b>					
Total.....	-77	-167	-169	-159	1,844
Race ethnicity:					
Black.....	-65	-148	-193	-230	1,744
White.....	-71	-152	-118	-96	1,869
Chicano.....	-113	-238	-268	-204	1,963
Significantly different?.....	No	No	No	Yes	
Site:					
Seattle.....	-34	-155	-130	-100	1,677
Denver.....	-111	-176	-200	-201	1,989
Significantly different?.....	No	No	No	Yes	
Length of program:					
3 yr.....	-59	-149	-144	-133	
5 yr.....	-114	-204	-221	-215	
Significantly different?.....	No	No	No	Yes	
<b>WIVES</b>					
Total.....	-53	-123	-119	-108	719
Race ethnicity:					
Black.....	-89	-135	-119	-116	932
White.....	-21	-102	-110	-89	633
Chicano.....	-74	-154	-144	-140	542
Significantly different?.....	No	No	No	No	
Site:					
Seattle.....	-39	-117	-94	-115	70
Denver.....	-65	-127	-139	-103	733
Significantly different?.....	No	No	No	No	
Length of program:					
3 yr.....	-32	-108	-102	-84	
5 yr.....	-96	-153	-154	-156	
Significantly different?.....	No	No	No	No	
<b>SINGLE FEMALE HEADS</b>					
Total.....	-40	-134	-202	-201	1,059
Race ethnicity:					
Black.....	-93	-192	-236	-217	1,102
White.....	-24	-43	-153	-150	1,027
Chicano.....	+78	-122	-217	-273	1,008
Significantly different?.....	No	No	No	No	
Site:					
Seattle.....	-104	-171	-218	-210	917
Denver.....	+17	-101	-188	-195	1,193
Significantly different?.....	No	No	No	No	
Length of program:					
3 yr.....	-35	-128	-204	-191	
5 yr.....	-41	-149	-197	-227	
Significantly different?.....	No	No	No	No	

Source: Philip K. Robins and Richard W. West, "A Longitudinal Analysis of the Labor Supply Response to a Negative Income Tax Program: Evidence from the Seattle and Denver Income Maintenance Experiments", Research Memorandum, Center for the Study of Welfare Policy, SRI International, (draft) August 1978.

### *Variation in work effort response by race-ethnic group*

Table 1 also reports separate estimates of the response by the three major race-ethnic groups, blacks, whites, and Chicanos. Breaking down the sample into race-ethnic groups decreases the precision with which we can estimate experimental effects. In our earlier work, we were unable to detect significant differences in the response of black and white family heads (the differences are small in the second year). In our latest work, we are again unable to detect a significant difference except for husbands where the estimated long run response of blacks and Chicanos is more than double the response of whites. While only suggestive (the model used to generate the long run response is currently undergoing revision), these results imply that we may have overstated the nationwide responses of husbands in our earlier testimony because blacks and Chicanos comprise a smaller portion of the U.S. population than they do of the experimental population. We are unable to provide a convincing explanation for why the estimated response of whites may be so much lower.

### *Site differences*

When the experiment began, the unemployment rate in Seattle was much larger than the unemployment rate in Denver. Average annual hours of work was about 22 percent less for husbands and 27 percent less for female heads (there was no significant difference in hours of work for wives). Over time, this difference narrowed somewhat for the control group. Since work effort response is measured in terms of actual (as opposed to desired) hours of work, it would not be surprising to observe a smaller response in Seattle. Given bleak employment prospects in Seattle, individuals may be less willing to give up a job already held and more willing to accept a job offered.

Table 1 shows how response differs in the two sites. Only in the case of husbands is the response significantly lower in Seattle. If we discount the Seattle results as being unrepresentative, then the responses reported in our earlier testimony are probably an underestimate of the nationwide response to an NIT program for economic conditions similar to those prevailing at the time of the experiments.

### *Effects by length of program*

SIME/DIME is testing programs of varying length. Approximately two-thirds of the experimental sample was enrolled for 3 years while the remainder was enrolled for 5 years. In addition, a small sample of families in Denver was enrolled in a 20-year program for 2 years. We have not yet analyzed the response of 20-year families.

The temporary nature of the experiment may create problems in extrapolating the results to a permanent program. In a seminal paper written by Charles Metcalf ("Making Inferences from Controlled Income Maintenance Experiments," *American Economic Review*, June 1973), a model is developed that predicts that the work effort response to a temporary experiment will be different from the work effort to a permanent program. Metcalf's model implies that in a temporary experiment, the guarantee effect is smaller and the tax effect is larger than in a permanent program. Thus, the work effort response to a permanent program may be either larger or smaller than the work effort effects of a temporary experiment. The ambiguity arises because the two effects go in the opposite direction.

SIME/DIME is the only experiment that is testing programs of varying length. Hence, it is the only experiment that can provide evidence on the relationship between work effort response and length of program.

Table 1 presents estimates of the work effort responses of family heads enrolled in the 3- and 5-year programs. As this table indicates, the responses of family heads enrolled in the 5-year program tend to exceed the responses of family heads enrolled in the 3-year program. In the case of the long-run response of husbands, this difference is statistically significant.

The responses reported in Table 1 combine the effects of the guarantee level and the tax rate. In a preliminary attempt to estimate these effects separately for 3- and 5-year families, we find that the estimated guarantee effect is larger for 5-year families and that the estimated tax effect is larger for 3-year families. Both of these results are consistent with the predictions of the theoretical model of Metcalf. They are not very precise in a statistical sense, however.

These findings have important implications for work effort responses to cash transfer programs. Because of the relatively generous nature of the SIME/DIME programs (the average guarantee for a family of four in 1978 dollars is \$7,250), the work effort responses of 5-year families are somewhat larger than the work effort responses of 3-year families. However, when the same income and tax effects are used to predict the effects of a much less generous program, such as The Program for Better Jobs and Income (where the guarantee for a family of four is \$4,200), the predicted responses of 3- and 5-year families are very similar. (Such an analysis has been performed by the staff of the Department of Health, Education, and Welfare.) In general, the SIME/DIME findings with regard to program length suggest that our earlier estimates (which combine the effects of the 3- and 5-year families) would tend to understate the work effort responses to a high guarantee, low tax rate program and overstate the work effort responses to a low guarantee, high tax rate program. The magnitude of the biases is not likely to be very large, however, for most feasible types of welfare programs.

#### *Implications of our latest findings*

The Seattle and Denver Income Maintenance Experiments were designed so that separate analyses could be performed by race-ethnic group, site, and length of program. Unfortunately, the sample sizes within each of these categories are too small to provide conclusive evidence regarding differential response. In our previous testimony, we presented results that aggregated the response over these categories. Further analysis has revealed that this aggregation may not be valid for husbands. However, some of the biases resulting from the aggregation would lead us to adjust our previous estimates upward while others would lead us to adjust them downward. On balance, the likely net effect of all these adjustments would probably leave the previous estimates unchanged.

#### *Source of the work effort response*

We have just seen that the experiment causes a reduction in annual hours of work for every demographic group. There are several ways this reduction can come about. First, an individual can reduce hours of work on a given job. Second, an individual can quit a job. Third, an individual can refuse a job offer during a period of unemployment, or can remain out of the labor force for a longer period of time. The source of the reduction in annual hours of work has important implications for the design of a national program. For example, if the reduction is due mainly to longer periods of unemployment, the cost of the unemployment compensation program may be affected.

We have performed some analysis on the source of the reduction in work effort. The results are summarized in Table 4. As this table indicates, the rate of leaving employment is virtually unaffected by the experiment while the rate of entering employment is significantly reduced for all three family heads. The effects are largest for women, a group that has been experiencing a general upward trend in labor force participation in the past decade. Our findings suggest that this upward trend may be significantly reduced under a universal NIT program. Our findings also suggest that persons with firm attachment to the labor force are not likely to leave their jobs in response to such a program.

#### *Evidence on the work effort response of young nonheads*

All our previous analysis of work effort response has been confined to heads of families. However, there are good reasons for believing that the work effort of nonheads, particularly youth, may be affected by an NIT program. Recently, we completed a study of the effects of the experiment on the work effort of young nonheads. The sample analyzed in this study consisted of persons between the ages of 16 and 21 at the time of enrollment and were either the children, stepchildren, or grandchildren of the heads of their families. The results of the study are summarized in Table 5. They indicate that young nonheads reduce work effort by a substantial amount in response to the experiment. Male nonheads reduce work effort by 23 percent and female nonheads reduce work effort by 18 percent. Further analysis (not reported in Table 5) has revealed that the reduction in work effort does not take the form of increased time spent in school.

TABLE 4.—ESTIMATED EXPERIMENTAL EFFECTS ON RATES OF LEAVING AND ENTERING EMPLOYMENT

[Percent of control group]

	Leaving employment	Entering employment
Husbands.....	2	1 -17
Wives.....	-5	1 -28
Female heads.....	-4	1 -37

1 Indicates significantly different from control group.

TABLE 5.—ESTIMATED EXPERIMENTAL EFFECTS FOR YOUNG NONHEADS

[Effects measured in hours of work per year]

	Effect	Percentage effect
<b>Males:</b>		
All.....	1 -241	-23
Remain nonheads (63 percent).....	1 -384	-40
Marry and become heads (12 percent).....	1 -540	-32
Leave household (25 percent).....	+113	+12
<b>Females:</b>		
All.....	1 -145	-18
Remain nonheads (59 percent).....	1 -353	-43
Marry and become heads (29 percent).....	1 -111	-18
Leave household (11 percent).....	+99	+10

1 Indicates statistical significance at the 10 percent level or lower.

Source: Richard W. West, "The Effects of the Seattle and Denver Income Maintenance Experiments on the Labor Supply of Young Nonheads", Research Memorandum, Center for the Study of Welfare Policy, SRI International, (draft) June 1978.

Persons between the ages of 16 and 21 are very mobile. During the course of the experiment, some of these nonheads marry and leave the household. About 12 percent of the males and 29 percent of the females married during the first 3 years of SIME/DIME and formed their own household. Others leave the household as single individuals. About 25 percent of the males and 11 percent of the females left home during the first 3 years of the experiment.

Nonheads that marry and leave the household get to take their original family's treatment with them, although an adjustment is made in the guarantee level for the change in family size. Nonheads that leave the household as single individuals get a guarantee of \$1,000 and a tax rate equal to the tax rate of their original family. Thus, nonheads who leave the household receive a more generous treatment if they marry.

Because of this differential treatment with regard to program benefits, we would expect to observe a different work effort response for these three types of individuals. This conjecture is borne out by the results presented in Table 5. Male nonheads who marry and leave the household reduce work effort by about 32 percent while those remaining in the original household reduce work effort by 40 percent. Male nonheads who leave the household as single individuals do not significantly change their work effort, apparently because of the low guarantee. Female nonheads who marry reduce work effort by 18 percent while those remaining in the household reduce work effort by 43 percent. As in the case of males, there is no discernible impact on work for female nonheads that leave the household as single individuals.

The estimated work effort responses for nonheads are large and should not be ignored when considering the possible effects of national NIT plans. One important implication of our results is that the responses to a national plan could not be reduced by refusing eligibility to single individuals since the estimated response is concentrated in the other groups, which could not be excluded from eligibility.

The response by male and female nonheads who remain nonheads, while large in percentage terms, may not be of great importance since these nonheads are generally secondary earners in their families. However, a reduction in work effort during this portion of their life cycle may have continuing effects by reducing their labor market experience, and thus, possibly reducing their ability

to obtain good jobs in the future. Such a long-term effect is purely speculative; there is little empirical evidence to support this hypothesis. The reduction in work effort by male nonheads who become husbands is clearly important. These males reduce their work effort just at the time when they are undertaking family responsibilities. Not only is their response important in the current period, but the reduction in work effort may also have long-term effects on their labor supply behavior.

#### MARITAL STABILITY RESULTS

##### *Experimental effects on the making and breaking of marriages*

In our earlier testimony we reviewed the reasons for expecting an NIT to affect the rates at which marriages are formed and disrupted. We suggested that any program providing financial assistance to families would have two kinds of effects. Providing additional income for low-income couples could reduce the pressures toward dissolution that arise from inadequate income. We have referred to such marriage stabilizing effects as income effects. Since an NIT would also alter the level of resources available outside of marriage and thereby alter the dependence of the members on marriage, an NIT could also have destabilizing effects. We refer to these as independence effects.

We pointed out the need to consider the nonpecuniary differences between the current welfare system and an NIT program such as those tested in SIME/DIME. If participation in the current system is degrading, families receiving payments would not experience either the full income or independence effects. This suggests that the effects of welfare on marital dissolution are discounted relative to the effects of an NIT. Other nonpecuniary differences between the current welfare system and an NIT also imply a welfare discount. These include differences in transaction costs and in the information available to potential recipients. Thus, the effects of any alternative form of income assistance on marital stability do not depend only on the generosity of the program, but also on differences in administration that might alter stigma and transaction costs and increase information about benefits.

The effects of an NIT on the formation of marriages are as important as the effects on marital dissolution. An NIT program which reduced the rate at which marriages are formed would have similar consequences to a program that increased the rate of marital disruption. In both cases the result would be an increase in the proportion of single-parent families. We expected that an NIT could have the same kind of opposing effects on marriage that we have discussed for dissolution, that the NIT would not only change the level of resources available to a single family head, but would also change the resources available upon marriage.

##### *Experimental findings*

In earlier testimony before this committee we reported that the NIT programs tested in SIME/DIME destabilized marriages. The magnitude of the destabilizing effect varied with the level of support. In Table 6, we report our findings by support level. The effects are expressed as percentage increases in the dissolution rate. These estimated effects are for the first 3 years of the experiment, and are similar to, but somewhat smaller than, the 2-year findings reported in Table 8 of our earlier testimony. The pattern of effects observed in the 2-year estimates holds in the 3-year estimates.

The lowest support level holds particular interest since it differs little in financial terms from the existing level of support available from AFDC<sup>1</sup> and Food Stamps. If there is no welfare discount, this program should have no income or independence effect. But the dissolution rate for families on this treatment greatly exceeds that of the control groups—by 43 percent for blacks, 63 percent for whites, and by 37 percent for Chicanos. The plan with the highest guarantee level, 140 percent of the poverty level, has the smallest impact for each race-ethnic group. Thus, our findings suggest that for the high support level there are large independence effects due to the discounting of welfare that are offset by the income effects of more generous programs.

To explain these experimental effects we formulated a model of the income and independence effects of the experimental NIT that included a welfare discount. Our model and the evidence supporting it are discussed at length else-

<sup>1</sup> Both Washington and Colorado had AFDC-U programs throughout the experimental period.

where (Hannan, Tuma, and Groeneveld 1977, 1978; Tuma, Hannan, and Groeneveld 1979). The model assumes that the income and independence effects depend on both the levels and on changes in the levels of resources available to married couples and female-headed families. Our earlier testimony illustrated how the effect of an NIT on marital dissolution varied with the support level and tax rate of the program and with the level of resources available to the couple.

Our analysis of the experimental effects on marriage revealed no significant difference in the rates of marital formation between the control and experimental groups. This finding of no effect needs to be considered in light of the anticipated "dowry" effect of the experiment. The dowry effect occurs because under the experimental rules of operation new spouses were eligible to receive benefits. Thus, a single family head on an experimental treatment would be a more attractive spouse than a potential spouse not eligible for a financial treatment. It was expected that this might artificially increase the rate of marital formation. It is possible, then, that our finding of no effect on marriage rates is a result of a true decrease in the rate offset by an increase caused by the dowry effect.

TABLE 6.—PERCENTAGE CHANGE IN MARITAL DISSOLUTION RATE BY LEVEL OF INCOME GUARANTEE<sup>1</sup>  
[In percent]

Guarantee level	Race-ethnic group		
	Black	White	Chicano
90 percent of poverty level.....	± 43	± 63	37
125 percent of poverty level.....	± 73	± 40	6
140 percent of poverty level.....	15	18	-31
Number of cases.....	1, 123	1, 561	646

<sup>1</sup> The effects reported here are estimated over the 1st 36 mo of the experiment. They are the percentage increases for couples enrolled for 5 yr when differences in family income and husband and wife characteristics are controlled. The percentage increases for couples enrolled for 3 yr are approximately 20 percent less than those for couples with 5-yr treatments. All couples who are married at any time during the experiment are included in these estimates.

<sup>2</sup> Significant at the 0.10 level.

<sup>3</sup> Significant at the 0.01 level.

#### *The robustness of the marital stability findings*

We wanted to be very sure that the results were not an artifact of some of the procedures we used or the result of some feature of the experimental design. We have taken several steps to establish the robustness of our findings.

We first determined that similar results would be obtained using a different analytic method. The results reported in Table 6 are estimated using a statistical procedure that, while well suited to the kind of data being analyzed, is unfamiliar to most social scientists. To reassure our colleagues and ourselves that the findings were not simply the result of an unfamiliar analytical method, we also analyzed the data using a more widely known technique, linear probability models. The findings of the linear probability analysis agree with those reported here; the NIT increased dissolution rates, the 90 percent poverty level had a large increase relative to the controls, and the 140 percent of poverty level guarantee had the least impact. The results of our linear probability analysis are reported in Hannan, Tuma, and Groeneveld (1976).

Since the assignment of the sample to the experimental treatments was stratified by income, it has been suggested that our findings might reflect the relationship between the experimental treatments and family income and not an experimental effect. We have examined the relationship between the experimental effect and the normal income levels in some detail and have found that the general pattern of effects reported in Table 6 holds even within income levels; the effect for the low support level is greater than the effect for the high support level.

Another issue that has caused us to evaluate the robustness of our findings is attrition. If the rate of marital status change among people who drop out of the experiment is different from the rate for those who remain, and if the rate at which people drop out is different for the controls than for the experimentals, then our analysis of the remaining sample may give misleading results. While the attrition rate is low, approximately 5 percent per year, we wanted to assure

ourselves that our findings were not caused by attrition. We have dealt with the problem of attrition in two ways. First, we chose an analytical method that utilized partial observations of people who drop out. Second, we studied the sensitivity of our results to attrition. Even when we assume that the dissolution rate for controls who drop out is several times higher than the rate among controls who remain, we obtain the same pattern of results (Hannan, Tuma, and Groeneveld, 1976).

A fourth factor to consider in evaluating the robustness of our findings is fraud. Because the payments to a single woman and her children will be larger if her employed husband is absent, families on experimental plans may report false dissolutions to gain increased payments. We find this an unlikely explanation of our findings. First, when a dissolution occurs our interviewers attempt to locate the husband and continue to interview him as well as interviewing the wife regularly. Thus, the couple would have to maintain the fictional dissolution over a series of interviews throughout the course of the experiment. Second, no simple fraud hypothesis is consistent with our findings. Couples on the high support levels should have more incentive toward fraud than low support couples, yet we find the low support effects larger. Also, the fraud hypothesis should apply to remarriage as well, yet we find no difference in remarriage rates between the controls and experimentals. Third, we have found no evidence of fraudulent dissolutions being restored during the postexperimental period when we are still interviewing the families, but they are receiving no payments. Thus, while some fraudulent dissolutions may have been reported, we conclude that fraud could not account for either the pattern of our findings or the magnitude of the effects.

In summary, we know of no evidence to cause us to doubt that the rate of marital dissolution in SIME/DIME was substantially higher among the experimental couples who were eligible for the NIT payments than it was among couples in the control group. Our confidence is further increased by the detailed scrutiny our work has received. At every stage our work has been reviewed by competent social scientists in the academic community and in the Federal Government. Our findings have been circulating publicly for 4 years. We have presented our findings at several professional meetings and published our findings in a leading journal. In addition, HEW convened an expert panel of statisticians, economists, sociologists, and demographers to review our work. That panel met last February and while alternative explanations of our findings were offered, there was general agreement that the experiment had increased marital dissolution rates.

#### *Implications of the marital stability findings for welfare reform*

The income maintenance experiments have taught us a great deal about the potential effects of welfare reform on marital stability. We have learned, for example, that the effects would depend not only on the generosity of the program but also on nonpecuniary aspects of the program. Our research, as well as that of other investigators, has called into question the belief that removing the categorical restrictions in the current welfare system would lower the dissolution rate among low-income families. Our findings for the low-support treatment suggest, if anything, that the contrary might be the case.

Our discussion of the independence effects calls attention to the importance of considering the change in the level of resources outside of marriage as well as the change in the resources available to a couple. Any welfare program will have independence as well as income effects, and the net effect of a program will depend upon the relative strength of those effects.

Consider, for a moment, the potential effects of a program which combines an NIT with a work requirement and a guaranteed jobs program. The cash assistance component of the program would have income and independence effects similar to those of any NIT. The provision of a guaranteed jobs program would also have both income and independence effects through the changes in income that job recipients would receive. When the primary earner takes a guaranteed job, the family income will be increased and the marital dissolution rate decreased. The independence effect arises because the wife (the more dependent partner) becomes eligible for a guaranteed job if she becomes single. The provision of this job may increase the income available to her outside marriage. The same argument can be made for the income and independence effects of income changes resulting from the improved employability of job recipients. Thus, the income changes generated by a guaranteed jobs program are expected to have both positive and negative effects on marital dissolution. The net effect depends upon the relative strength of the effects.

Consider another proposal for welfare reform, wage supplements. Under a wage supplement program, the government subsidizes the hourly wage rate of low-income workers, in effect, putting a floor on the wage rates of eligible persons. The effect of such a program would be to decrease the propensity to dissolution among low-income couples by increasing family income (income effect). But, unless such a program categorically excluded single family heads, it would also have independence effects since it would raise wage rates and earnings outside of marriage as well.

We conclude with a warning to guard against the impulse to generalize our results uncritically to a national NIT program. The differences between our experimental programs and a national program would, undoubtedly, influence the effects of a national program on marital stability. The impact of a national program might be greater or less than the impact of the experimental programs. However, our findings do indicate that a national program is likely to have effects on marital stability and that those effects may be large.

*Summary of the marital stability findings of SIME/DIME*

In closing, we want to summarize briefly our discussion of the marital stability findings of SIME/DIME. We have demonstrated that the overall result of the experiment was to increase the rate of marital dissolution in the experimental groups relative to the controls. This finding persists in the face of numerous attempts to explain it as an artifact of the experimental design or of our methodology. In further analysis, we have demonstrated that the pattern of experimental results we have observed can be accounted for by opposing income and independence effects. We concluded with a discussion of the relevance of our findings for evaluating programs that differ from our experimental NIT. In particular, we pointed out the potential independence effects inherent in a guaranteed jobs program and in a wage subsidy program.

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Senator MOYNIHAN. Now we are going to continue with the analysis of the particular data of these experiments. We have another Stanford professor, Dr. John Cogan, and Dr. Finis Welch who is of the department of economics at the University of California at Los Angeles.

Both of our panel members are eminent members of their profession—a dismal profession, but one for which we have not found an alternative. Maybe that is why all of these things go—if we had only gotten more social psychologists or something in this experiment, perhaps the findings would have been more cheerful.

We welcome you. Dr. Cogan, you are listed first, so why do you not begin?



**STATEMENT OF JOHN COGAN, DEPARTMENT OF ECONOMICS,  
STANFORD UNIVERSITY**

Mr. COGAN. Thank you for the invitation to testify. I take this opportunity to describe what I have learned about labor supply responses to negative income taxation. My knowledge of these responses stems largely from the results of a reanalysis of the New Jersey-Pennsylvania negative income tax experiment which I recently completed.

In this study, I estimated—

Senator MOYNIHAN. May I just ask, what prompted you to do that? Mathematica has put out all of those endless volumes. Did you just become curious?

Mr. COGAN. Basically that is it, sir. I was curious. It was the first large-scale social experiment conducted in the United States. It has had quite an important impact, I think, on the profession, and I have seen very little reanalysis of the data generated by the experiment by individuals who were not connected with the experiment's operation—

Senator MOYNIHAN. Well, good. I do not want to put it in crass terms, but you are paid to be curious.

Mr. COGAN. Yes, sir.

In this study, I estimated labor supply responses among white male heads of household and found work reductions much larger than those found in earlier analyses of the same data.

I will begin my testimony by summarizing the findings from my analysis and contrast these findings with those of earlier analyses of the same data and more recent analyses of the Seattle-Denver experiment.

I then discuss the potential importance of my findings to public policy. But before beginning my testimony, I should add that there are many problems in the experiment. Some of these problems were designed into the experiment; others were the result of unforeseen events. These problems include the truncation of the sample on the basis of income; the fact that families were not randomly assigned to the experimental programs and to the control groups; the coexistence of welfare programs for intact households; the particular accounting system used to determine NIT payments; and finally the fact that the experiment was of limited duration.

The existence of these problems is widely recognized, but the quantitative importance of their effects on the estimates of labor supply responses are not.

I have addressed some of these problems in my analysis. Although I will not discuss these problems in direct testimony, I will be glad to answer any questions relating to them and how they affected my analysis.

The central finding of my analysis is a large, statistically significant labor supply withdrawal among white male heads of household. Although the estimated withdrawal differs in each of the 3 years of the

experiments, it is on the order of a 5- to 7-hour-per-week reduction in work effort among those who received cash assistance from the experimental negative income tax programs. This estimate is especially striking because male heads constitute the demographic group of the population least likely to respond to the work disincentives of an NIT program.

The estimates reported in my study are in marked contrast to those obtained in the previous analysis which are estimated work reductions of between 2 and 3 hours per week for the same demographic group.

My method of analysis differed in two important respects from earlier analysis of the same data. First, families that participated in the negative income tax programs were distinguished from families that did not. Only families that received NIT benefits would be expected to respond to the work disincentives of the program. By making this distinction, I was able to estimate not only the work reductions by the program participants but also the parameters underlying the families' decision of whether or not to participate in the program.

Earlier analyses ignored this distinction and estimated the hours of work response by comparing the hours of work among families eligible to receive benefits with the hours of work among noneligibles.

Second, the effects of State welfare programs for intact households were explicitly controlled for. In one earlier study (Garfunkel, 1974) the effects of these programs were controlled for, but it reports no estimates for white male heads of household.

The reasons for distinguishing between NIT program participants and nonparticipants are fundamental to analysis of experimental data generated not only by the New Jersey-Pennsylvania experiment but also by all other social experiments in negative income taxation.

Eligible families who participated in the program have the work disincentive effects of the tax rate and the income guarantee embodied in their observed hours of work. Eligible families who do not participate implicitly reject the tax rate and the income guarantee of the program, and do not have the work disincentive effects of the program embodied in observed hours of work.

In the third year of the New Jersey-Pennsylvania experiment, at most only 60 percent of white families who remained in the experimental group actually received NIT payments. By the 11th quarter of the experiment, only 48 percent were receiving NIT payments.

Thus, only around one-half of the white experimental families would be expected to respond to the work disincentives of the NIT programs. With such data, estimates of the labor supply response to the work disincentives can only be obtained by distinguishing program participants from nonparticipants.

At a more general level, any social experiment in negative income taxation which the family's receipt of cash assistance is subject to the family's choice, potentially can indicate the probability that families will choose to participate in the program as well as their hours of work response conditional on participation.

These two pieces of information are necessary inputs to the formulation of public policy. Two alternative negative income tax programs that differ in their tax rates may induce the same expected labor supply response. Yet, because the program with the higher tax rate produces a larger labor supply withdrawal and covers a smaller segment

of the population, the consequences of the two programs for income redistribution may be quite different.

The primary goal of negative income taxation is income redistribution. Unless participants and nonparticipants are distinguished, the income redistribution consequences of NIT programs cannot be extracted from experimental data.

It is important to control for welfare because attempts to estimate labor supply responses to NIT programs without accounting for it necessarily confound the work disincentives of welfare and NIT programs.

For example, if all the controls and none of the experimentals receive welfare benefits, a comparison of the mean hours worked between the two groups would be an estimate of work disincentives of NIT relative to welfare.

On the other hand, if none of the controls receive welfare benefits but all of the experimentals did, a comparison of the mean hours worked would not indicate the supply response of negative income taxation. Rather, it would be an estimate of the work disincentives of the welfare program.

The incidence of welfare in the experiment is not trivial. Among whites, over 27 percent of the controls and 13 percent of the experimentals received welfare benefits. With such a high incidence of welfare participation, welfare program effects must be controlled for if estimates from analysis of experimental data are to have a precise interpretation.

The differential between my estimate of the hours of work reduction and those earlier analyses of the New Jersey experiment is primarily due to different methods of analysis, but the different methods are crucially important to comparing results.

By distinguishing between NIT program participants and nonparticipants, I obtained an estimate of a labor supply response among NIT program participants. Earlier analyses do not.

By controlling for the State welfare programs, I obtained an estimate of the labor supply response in the absence of a welfare alternative. Earlier analyses that ignore the effects of welfare necessarily estimate responses that confound the work disincentives of an NIT program with those of the State welfare program.

Recent analysis of the labor supply response in the Seattle-Denver experiment (Robins 1978) tends to support the conclusion of a relatively large work disincentive to the experimental NIT programs. In his recently completed, but as yet unpublished, paper, Robins also distinguishes between NIT program participants and nonparticipants, and estimates average hours of work reduction of between 5 and 7 hours a week among male heads who participated in the Seattle-Denver experimental program.

Senator MOYNIHAN. May I interrupt? That is precisely your estimate, is it not?

Mr. COGAN. Yes. It is precisely the range of my estimate.

This estimate is considerably higher than estimates reported in earlier analysis of the Seattle-Denver experiment. This earlier analysis ignored the distinction between program participants and nonparticipants, and estimated small work reductions similar to those esti-

mated in previous analyses of the New Jersey-Pennsylvania experiments.

What are the implications of the large labor supply response to public policy? The large labor supply response to the work disincentives created by an NIT program has important implications for the cost of negative income taxation. To provide some indication of its importance, I will use the negative income tax component of President Carter's program for better jobs and income as an example.

Under the President's program, every family of four whose annual income is less than \$8,400 per year would have received cash assistance from the Federal Government. A family of four with one worker earning \$7,000 per year receives \$700 per year in cash assistance, assuming no labor supply response to the program.

If the worker reduces his or her hours of work by 2 per week in response to the work disincentives of the program, the cash assistance would have risen to \$882 per year. If, however, the worker responded by reducing hours of work according to the lower bound estimate in my study, 5 hours per week, the cash assistance to this family would have risen to \$1,155 per year, a 31-percent increase in the program's transfer cost.

For a similar family of four where the one worker earns \$8,000 per year in the absence of the program, a 2-hour per week response would have resulted in the family receiving a cash subsidy of about \$408 per year. A 5-hour per week response would have resulted in the family's receiving \$720 per year, 66 percent higher.

Finally, a family wherein the worker earns the minimum wage would have received \$1,688 per year, assuming a 2-hour per week response, but \$1,895 per year if his response were 5 hours per week.

As these examples indicate, the transfer costs of an NIT program are highly sensitive to the labor supply reduction that such a program would induce. The relatively large work disincentives found in my analysis of the New Jersey-Pennsylvania experiment and those recently found by Robins and his analysis of the Seattle-Denver experiments, is just that the transfer costs of an economywide NIT program would be larger than current projections based on earlier estimated responses indicate.

Senator MOYNIHAN. I don't believe it. Do you mean to say that Harold Watts got down and measured the effects of a negative income tax upon the behavior of people who were not receiving negative income tax payments?

Mr. COGAN. That is precisely correct. I have here a book edited by Watts and Rees, entitled "The New Jersey Income-Maintenance Experiment, Volume II, Labor Supply Responses." The book summarizes the findings of the Institute for Research on Poverty's analysis of the labor supply responses in the experiment for the research community. The book is over 400 pages long. Nowhere in this book are the number of families that actually participated in the experimental NIT programs reported.

Senator MOYNIHAN. I don't believe it.

Mr. COGAN. It's true, Senator.

Senator MOYNIHAN. That borders on malpractice. I went through those New Jersey results and the whole thing seemed muted to me,

and there was no focus and I expected something a little more assertive about the findings, but I mean, my Lord.

A family could be in the experimental group and choose to participate or choose not to participate, is that it?

Mr. COGAN. To some extent, sir, yes. Imagine a family whose income in the absence of the program exceeded the program's break-even level. A person in that family would reduce its income, get on the program, and receive benefits.

So in that sense, yes; the family could choose whether to participate or not.

Senator MOYNIHAN. You published this in your paper "Negative Income Tax and Labor Supply," and you also had a nice little note in "Challenge."

What has been the response?

Mr. COGAN. It depends on who I talk to. The response I have received from the poverty institute at the University of Wisconsin and Harold Watts, who I believe is now at Columbia, has been quite negative. The response I received from my colleagues at RAND has been positive.

Senator MOYNIHAN. That's about right. I recognize that fact.

Mr. Robins of SRI has come up with the same range that you have, so we have at least these two findings which are—

Mr. COGAN. They are similar, but let's remember that the experiments were quite different. They are similar, though.

Senator MOYNIHAN. There may be some general relationship here; there may not be. But at least we have two findings that are in the same range, and they are considerable—you know, 7 hours is a day's work.

Mr. COGAN. It is substantial.

Senator MOYNIHAN. I do not like that one bit.

I am going to ask if there is somebody from the Department of Labor present?

All right. Stand up, won't you, and introduce yourself, if you would?

Ms. HUNTER. I am Ms. Hunter.

Senator MOYNIHAN. Ms. Hunter, you heard Dr. Cogan's testimony. I wish you would see that Dr. Allen has a copy, and I would like to hear from the Department of Labor what its comment is on this disparity. It reels the mind.

Your research is HEW financed, I believe?

Mr. COGAN. Yes, sir.

Senator MOYNIHAN. Let's get Labor to find out what Cogan did and see if they cannot explain this to us; all right? And let's have them do it by the end of the week; all right? If you can do it, you can do it. Let's hear what they have to say by Friday.

[The following was subsequently supplied for the record:]

COMMENTS ON TESTIMONY OF JOHN COGAN, PREPARED BY THE U.S. DEPARTMENT OF LABOR

Cogan has identified two legitimate criticisms of the analysis of New Jersey/Pennsylvania income maintenance experiment as presented in the HEW final report of the findings of that experiment.<sup>1</sup> The problems he identifies arise from two

<sup>1</sup>"Summary Report of the New Jersey Graduated Work Incentive Experiment," U.S. Department of Health, Education, and Welfare, December 1973.

sources: (1) some families enrolled in the experimental plans had incomes too high to qualify them for actual payments and thus may not have been affected by the payments offered; (2) many families in the "control" or comparison group (as well as a substantial number of experimental families) were actually receiving welfare payments under the Unemployed-Father component of AFDC (operative in both New Jersey and Pennsylvania), and thus may have been subject to even greater disincentive effects than the families receiving payments under the experiment. These two facts make it difficult to estimate the true response to the negative income tax among those actually receiving payments.

With respect to the first problem, it is the case that the results for the New Jersey experiment presented by HEW in their summary report did not distinguish between responses of people with incomes below the eligibility limit for the particular plan in which they were enrolled, and hence who actually received payments, and those with somewhat higher incomes. Instead the report presents the aggregate work effort response for a sample of low-income families (families with incomes less than 150 percent of the poverty line) some of whom were not income eligible for payments. While this is not an uninteresting statistic in analyzing aggregate program impacts on the population presumably at risk, it is clearly at least of equal interest to know the magnitude of work effort reductions among those who actually responded to the incentives provided by the experimental programs.

Obviously once an income maintenance program is operating it is the people who are actually receiving payments who are most likely to respond to the work incentives it provides. Nonetheless it is important to note that the people who actually receive payments may or may not have been eligible for such payments at the program's start. Not only will income fluctuations caused by non-experimental influences make some people newly eligible, but the program's incentives may themselves create new eligibles. That is, some people not initially eligible may choose to adjust their work effort (or other family circumstances) in order to become eligible. For this reason to capture full program effects it is necessary to consider the behavior of people who are somewhat above the income eligibility cut-off as well as those who are actually receiving payments although it is important to distinguish the two groups to the extent possible.

The problem is, however, not as easily solved as it may seem. Numerous researchers including Watts [1974] and Hall [1975] have attacked it. (A list of relevant studies is attached.)

The basic problem is that the existence of an income cut-off for eligibility for an income maintenance program insures that the only people who are eligible for benefits are those who (holding family size constant) have low wages, low hours of work or both. Necessarily this will produce a concentration of people receiving payments who have low hours of work relative to the broader population, whether or not they are responding to the program's incentives.

To find out how much of these low hours is due to other factors and how much to the program's incentives, it is necessary to compare the group receiving payments to a control group which is comparable in all respects, including hours of work and wages, except for the differences induced by the experiment. This is not easy to do since once the experiment is underway we can no longer distinguish experimentally induced changes in hours of work among the experimental group from those produced by other factors in both the experimental and control group.

Watts attempted in his initial analysis of the experiment [Watts, 1974] to do this by including the full set of experimentals and controls, whether or not the former were receiving payments, thus eliminating the effect of non-experimentally induced changes in hours. He attempted to account for the likely low response of experimentals who were considerably above the income cut-off by allowing them to respond differently from those with lower incomes. His analysis yielded inconclusive results partly because of small sample sizes and was not used by HEW in its summary report on the experiment.

Cogan attempts to deal with this problem (as well as the welfare participation problem) by comparing the hours of work of the following two groups: (1) individuals who receive experimental payments in a particular month and (2) all control families except those receiving AFDC payments.<sup>2</sup> He interprets this difference in hours of work as the response induced by the experimental program. This comparison is inappropriate for the following reasons. Because of the way in which the NIT formula works, the only NIT-eligible families who received NIT payments were those who had comparatively low earnings and hours of

<sup>2</sup> He also excludes experimental families receiving AFDC from the comparison.

work; families with higher earnings did not receive payments and hence would not have been included in Cogan's first comparison group. Conversely, members of the control group who did not receive welfare benefits tended to have higher-than-the-average earnings and hours of work. Thus, Cogan's estimate of the NIT effect is based on the difference between the experimental families with the least amount of work effort and the control families with the greatest amount of work effort. This comparison certainly exaggerates the effects of the NIT program.

A more satisfactory treatment is that employed by the Stanford Research Institute [Keeley, Robins, Spiegelman and West, May 1977, Research Memorandum No. 38]. For the purpose of cross-experimental comparisons SRI has also estimated an average response model comparable to that upon which the initial New Jersey estimates were based. However, the model—criticized by Cogan—which was used to estimate the costs and work effort reductions associated with specific plans, differentiated between those above and below the income eligibility cut-off. To do this SRI computes the actual change in each payment-eligible family's financial condition based upon the family's preexperimental income level. This avoids the problem of attributing random changes in income to experimentally induced responses. One possible problem with SRI's approach is that preexperimental income may not be representative of the families' normal income in the absence of the experiment. Some of the families designated as income eligible on the basis of preexperimental income may only have been transiently poor and would have increased their income in subsequent time periods in any case. This may lead to some underestimate of responses.

Account should also be taken of those downward flows into the eligible population which result from those reductions in hours of work induced by the experimental programs which cause experimental families initially above the income cutoff to fall below it. SRI has estimated such responses but the SIME/DIME sample with incomes above the income cut-off limits is small and it is difficult to generalize these effects to a national population. As a result these last effects have not been included in either the HEW or Congressional Budget Office welfare reform cost estimates. However what evidence there is of the magnitude of such effects suggests it would be small in comparison to those observed for families initially below the income cut-off levels.

With respect to the inclusion of welfare families in the comparison of experimentals and controls, whether or not this is a problem depends upon what it is you want to know. The New Jersey findings (and the comparable SIME/DIME average experimental/control responses reported) indicate the difference in work effort which could be expected if an income maintenance program, similar on average to those tested, were introduced instead of the welfare systems in the jurisdictions currently operative. This is obviously relevant in assessing the impacts of likely reforms in States and jurisdictions with comparable existing welfare programs.

Cogan attempts to answer a different, though still interesting question. What would be the effect on work effort of introducing the experimental plans tested into a world in which there was currently no welfare alternative? This is of interest in assessing both the effects of current programs and of introducing new programs into areas where benefits are low or coverage very restrictive.

As indicated above Cogan's analysis of this latter question is flawed by the fact that his comparison groups are not really comparable. However it is interesting to note that, if one accepts Cogan's methodology, his results yield the conclusion that the existing AFDC-UF programs in New Jersey and Pennsylvania reduce hours of work among those receiving benefits by  $6\frac{1}{2}$  hours a week more than did the experimental plans tested in New Jersey.

The initial researchers associated with the New Jersey experiment attempted to estimate the extent of the welfare effect on the estimated labor supply responses. Garfinkel (1974) estimated that ignoring the existence of welfare caused some understatement of the labor supply response but he concluded that the effect was small.

The most satisfactory treatment of the welfare participation problem to date is that produced by the joint efforts of Stanford Research and Mathematica Policy Research. They estimate labor supply response functions which are sensitive to the changes in each individual's wage rate and income induced by the experiment relative to their preexperimental status. These estimates are then incorporated into a microsimulation model which provides an accurate national representation of the current income, work effort and welfare status of the potentially

eligible population. The net effect on work effort and incomes (compared to the status quo) of any reform proposal can then be computed and the results analyzed in terms of the differential impacts in jurisdictions with different initial welfare programs and among individuals with different initial incomes, work experience, or family situations. A model of this type was employed by both HEW and CBO in analyzing the potential impact of the welfare reforms proposed by the Administration and others last year. Cogan's methodology cannot be used for this purpose.

#### CONCLUSION

The issues which Cogan raises are not new but they are important in assessing the meaning of experimental findings for welfare reform. The most important lesson to be drawn from this analysis is that in presenting results, researchers should be extremely careful to describe the exact nature of the populations for which the conclusions are being (or not being) drawn, the comparisons upon which they are based and the meaning of these comparisons, as well as other alternatives, for assessing policy impacts.

#### *The New Jersey-Pennsylvania experiment*

Harold Watts, et al., "The Labor-Supply Response of Husbands," *The Journal of Human Resources*, Vol. IX, No. 2, Spring 1974. (See especially pp. 86 ff.)

Harold Watts and Albert Rees, "The New Jersey Income-Maintenance Experiment, Vol. II, Labor Supply Responses," Academic Press, New York, N.Y., 1977. (See especially pp. 86ff.)

Robert E. Hall, "Effects of the Experimental Negative Income Tax on Labor Supply," in Pechman and Timpane, eds., "Work Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment," Brookings, Washington, D.C., 1975. (See especially pp. 117-119, 133-144.)

#### *The Gary experiment*

Robert Moffitt, "The Effect of Taxes and Transfers on Work Effort," MPR Working Paper, Princeton, N.J., May 1978.

Gary Burtless and Jerry Hausman, "The Effect of Taxation on Labor Supply: Evaluating the Gary Negative Income Tax Experiment," *Journal of Political Economy*, December 1978. (Also available as MIT Dept. of Economics Working Paper #211, Cambridge, Mass., November 1977.)

#### *The Seattle-Denver experiment*

Michael Keeley, et al., "The Labor Supply Effects and Costs of Alternative Negative Income Tax Programs: Evidence from the Seattle and Denver Income Maintenance Experiments, Part I—The Labor Supply Response Function," Research Memorandum #38, Menlo Park, Ca., May 1977. (See especially pp. 3-18. Forthcoming in *The American Economic Review*.)

Phillip Robins and Richard West, "Participation in the Seattle and Denver Income Maintenance Experiments and Its Effects on Labor Supply," Research Memorandum #53, SRI International, Menlo Park, Ca., March 1978.

Senator MOYNIHAN. Sir, you state that "because of budgetary limitations, I was unable to extend my analysis to women and other racial groups in the experiment." Do you mean to say that the faucet was turned off in Washington?

Mr. COGAN. No, sir. I received a contract from HEW to analyze the data generated by the New Jersey experiment. When I began my analysis, I had fully intended to examine the response among all demographic groups and women also.

When I found out that relatively few of the families were, in fact, participants, I had to get additional data from the Poverty Institute. The data I needed to distinguish between participants and nonparticipants was not provided in the analysis file which was distributed to the research community. I had to get that separately, and that set me back a good 2 months; and as a result I was not—

Senator MOYNIHAN. I hope you are pursuing this line of inquiry?

Mr. COGAN. No; I am not. My contract has ended and I have turned to other things.



Senator MOYNIHAN. You have turned to other things. Well, perhaps you are right, but you certainly turned up something that I want to find out more about. I do not like it one bit.

At the time, I did not much like the way that that final report of the New Jersey experiment came out. It made me uneasy and you have not made me any the less.

Well, sir, we will now turn to your associate here, Professor Welch who has written in Palmer and Peckman's "Welfare in Rural Areas" on the burdens of the farmer.

Mr. WELCH. Yes, sir.

Senator MOYNIHAN. I read it with the greatest possible interest and understood just about as far as the middle of the first paragraph.

Mr. WELCH. You were ahead of me.

**STATEMENT OF FINIS WELCH, DEPARTMENT OF ECONOMICS, UNIVERSITY OF CALIFORNIA AT LOS ANGELES**

Mr. WELCH. Thank you for giving me the chance to talk about labor supply response estimates in the NIT experiments. I have not participated in either the design or analysis units of any of these experiments and, as an outsider, I am reasonably familiar only with the Iowa-North Carolina rule experiment which I reviewed for Brookings.

This review involved only farmer responses and I am much more familiar with the farm than with the nonfarm part of that experiment.

As director of RAND's labor and population program, I also secured funding for examination of the New Jersey experiment by John Cogan. John and I often discussed his work and the parallels between the rural and New Jersey experiments, so I knew something about New Jersey as well.

I am not familiar with the Gary experiment and have only limited information of the Seattle-Denver one. I do, however, have the general impression that data generated by the experiment raised very difficult estimation problems that may be as hard to reconcile as problems associated with analysis of nonexperimental data.

I do not enjoy my position as a destructive critic, because I have the uneasy feeling that had I been involved in designing one of the experiments, I would not have done better. Yet, from the vantage of hindsight, it is clear that many problems were built in and that their resolution is not easy and may be impossible. The clearest, but by no means only example, is the fact the rural experiment used cash basis rather than accrual accounting.

The IRS permits farmers to use cash accounting which, over a career is much like interest-free loans and raises no other important analytical issues. But for a 3-year experiment for farmers who, with crop storage can time receipts, cash accounting can be devastating.

Consider, for example, a North Carolina tobacco or an Iowa corn farmer in the third year of an experiment that imposes a 70-percent tax on his net of expense cash income. The experiment simply withdraws 70 percent of his receipts realized in the third year but subsidizes 70 percent of the third year expense.

Since sales after the experiment are not taxed, why sell in the third year? Why not run up expenses and hold the crop until the experiment terminates?

The same question arises through the first and second years except that storage would be greater. Would it be surprising to find that work effort on farms increased by an experiment? They were, in fact.

If so, would this be a basis for forecasting responses for a permanent negative income tax scheme? Today, I briefly examine three issues or features of the experiment which have confounded estimates of labor supply responses. They include problems of interpretation resulting from erroneous and underreporting of income on which benefit payments are based; ambiguities arising from experiments, internal accounting rules and problems associated with ways analytical populations are selected.

Payments based on self-reported income. The IRS has well-specified incentives and penalties for accurate reporting, coupled with procedures for verification. One would suppose that a full Federal welfare program would too. And, if experimental responses are to shed light on real world outcomes, the procedures that would obtain in a full program should hold to the experiments as well.

Analysts of experimental responses are based estimates on income calculations from field interviews and a variety of other sources which presumably give fairly accurate measures, yet benefit payments are based on self-reported income. If program participants consistently underreport, then the effective tax is less than the program tax and labor supply responds only to the effective tax.

For example, Bill HARRAR estimated that in the rural experiment, only about 61 percent of farm income was reported in contrast to 91 percent of wage income for household heads. If so, the observed results that off-farm work declined while farm work increased should not be surprising.

It is very important that self-reported income be compared to the analyst income measures to determine: One, whether tax experiment occurred, and two, if it did, what it was. This kind of analysis is conspicuously missing from all studies I have seen.

The experiments occur in the real world with all the complexities of evasion and enforcement that we would expect, yet the analysis proceeds as though the experiments were textbook pure. Holding the analysts measure of income constant I found, for example, in the rural experiment that the increase in stated tax from 30 percent, the program's low, to 70 percent, the high, mysteriously reduced taxable income by \$2,400 annually. But from the perspective of estimating responses, I found a much more disappointing result that income of which payment calculations were based was very poorly correlated with the incomes on which response estimates were used.

Using program characteristics and the analyst income measures I could not explain as much as one-third of the variation in benefits. While there is evidence of consistent tendencies to underreport income, there was so much noise in the reported income that I suggested the rural experiment could not arguably be construed as reflecting a systematic tax experiment.

Accounting procedures for computing benefits. To my knowledge, each of the negative income tax experiments uses an accounting scheme in which benefit payments are based on current income or a moving average usually involving 3 months plus an accumulated surplus reflect-

ing past income above the break-even level or below zero for the self-employed.

Income also includes an imputation for owner-used durables like housing and farmers' farmland and building. Incidentally, the imputation for assets other than housing involves double counting, since for income-producing assets, the income is counted as it accrues and is counted again in the imputation.

The surplus carryover feature of the accounting scheme makes a lot of sense for a program in place, but for an experiment, it raises real problems.

In textbook examples, for example, someone whose earned income exceeds the break-even level simply is off the program, and his labor supply behavior should be no different from that of a similar control. But the accounting scheme's carryover implies that if someone's current income plus past carryovers exceed the break-even level, his current income is either: Not taxed by the experiment, which would be true if he receives no payment in the ensuing year; or he is taxed at the full program rate if he receives any payment in the following year.

In either case, he receives no current period payment and only the future reveals his current period tax. The analysis of responses to an uncertain future is not trivial.

I mention that the carryover accounting feature makes sense for a sustained program. This is true because after initial startup problems, carryover simulates accrual accounting with some aspects of interest-free loans. This, in itself, is an oversimplification because depleted carryovers are erased after 12 months, but even so, the startup problems are important for short-duration experiments.

In my review of the rural experiment, I gave an example of two families with average monthly income of \$300 on an NIT program with the \$200 guarantee and a 50-percent tax. The only difference was that family A lost \$300 for the first 6 months of each year—by the way, these losses are just nominal; they are incurring expenses before the crops are produced—and for the next 6 months of each year earned a net income of \$900.

Family B had exactly the opposite income stream, earning \$900 net for 6 months and then losing \$300 for each of 6 months. This would happen if the accounting period happened to split, the period between accumulating the crop and selling it.

For these families, annual payments—which should be \$600 per year—in the 3-year experiment would be: Family A, year 1, \$1,800; year 2, \$1,000; year 3, \$600. Family B, year 1, 0; year 2, \$450; year 3, \$600.

If the streams cannot be manipulated, family A would have a windfall of \$1,600 and family B a loss of \$750, and these windfalls might affect labor supply.

Farmers, of course, can manipulate their income streams and for the 105 experimental husband-wife farm families, I estimated that only two lost money by the sequencing option; 37 were unaffected; 66 made money. Farmers obviously played games with the system, and these games would have to be unraveled to identify labor responses in a long-duration program.

Sample selection and estimation of effects: Both the New Jersey and the rural experiments restricted participant families—experimentals and controls—to those whose income was not more than  $1\frac{1}{2}$  times the poverty line in the year preceding the experiment. My understanding is that the Seattle-Denver experiment used a similar criterion with a higher cutoff.

This kind of restriction, which seems reasonable, has important consequences. By now we are pretty much aware of the extremely dynamic character of the low-income population where transitory income fluctuations shift families above and then below the poverty line almost at random.

One consequence of restricting analytical populations to those with low income in 1 year is that independently of experimental effects, their income should rise in subsequent years. This is, of course, a case of the well-known phenomenon of regression toward the mean.

We see this in the New Jersey data where in the control population, proportions not working fell, and hours per week among those working increased over the 3 years of the experiment. This is the control population; no experimental effects.

And although we would not have anticipated transitory hours and wage variations, the data suggests that this would have been more important in transitory variation and workingtime. The price-deflated average hourly wage of male heads of households in the New Jersey experiment's control population rose by more than one-half in 3 years.

Most expect experimental design to solve problems of controlling for confounding factors, not create them, yet the income cutoffs increases reliance on control groups in trying to eliminate effects of spurious change. More importantly, it restricts attention to only one-half the poverty cycle.

The estimates may tell us something for families who are partly transitorily in poverty, about the way negative income tax effects emergence from poverty. By design, they can tell us less about these programs, pull into earnings levels below the poverty line, and about the first half of the poverty cycle, if indeed there is one.

The income rule for choosing which families are to be analyzed helps assure that over the duration of, say, a 3-year experiment, a fraction of the experimental population actually falling below the break-even level receiving benefits will decline.

For example, in the farm experiment, about one-half of the experimental families received payments in the first year, while only one-fifth received payments in the third year. Similarly in New Jersey, two-thirds received payments in the 3d quarter and only half received payments in the 11th quarter.

As such, it becomes increasingly important to distinguish between those experimentals who receive payments and those who do not. This may be a trivial point, but not until John Cogan's study was the distinction made.

In closing, I would like to summarize a few of my earlier points. One, social experiments are real-world exercises; problems of under-reporting of income exist, and I do not understand how they can be ignored, yet their incorporation into response estimates is not trivial and, to my knowledge, has not been attempted.

Two, the experiments create special problems that either require more sophisticated treatment of data than we have seen in most of the earlier studies, or that actually preclude estimation. The distinction between experimentals receiving payments and those not is a case in point.

Eligibility for payments depends on hours worked, and hours worked depends on the criteria for eligibility. Distinguishing between these opposing directions of effect is hard work, the realization of which is uncertainty.

Another example involves the carryover feature of the accounting rule. Someone not receiving payments cannot know his tax rate without perfect foresight, which may be too much to expect, even from farmers.

Finally, the income truncation leads to only one-half of the poverty cycle and increases dependence on control groups. It would be nice if the negative income tax experiments produced reliable estimates, but I, for one, wonder whether most of the information created has not been restricted to learning how to conduct social experiments and how to analyze data with their own peculiar problems.

Clearly, recent refinements are important, and the most recent estimates should be more reliable. They suggest larger responses than have been presumed from the initial analyses, which may be right, but those seeking reasons for doubts should be assured that there are many.

Senator MOYNIHAN. Very nice, sir.

Help me on the composition of the panel. This is Iowa and North Carolina?

Mr. WELCH. Yes.

Senator MOYNIHAN. What is the experimental panel? How was it drawn?

Mr. WELCH. Oh, I wish I knew. A poverty line is defined on a family-by-family basis as far as family income, and a family is selected as a potential control or experimental if its base period, which I think is the full year preceding the start of the experiment, base-period family income was below  $1\frac{1}{2}$  times the poverty level.

Senator MOYNIHAN. One and one-half times.

And so there is a period where, at any given moment once the experiment commences, some portion of that experimental group will be about that 1.5 ratio and therefore not getting any benefits, or whatever the top ratio for benefits is.

Mr. WELCH. Yes.

Senator MOYNIHAN. And those people that Cogan channeled out and Watts counted in New Jersey, for example.

Mr. WELCH. That's right.

Senator MOYNIHAN. Backward reels the mind. How could you suppose there would be a work disincentive effect for payments which you are not receiving?

Mr. COGAN. That is why the estimates are so low.

Mr. WELCH. The justification ordinarily given for not distinguishing is the simultaneity problem that I mentioned at the end, that if a family—suppose someone who otherwise would be above the break-even point and not receiving payments were to lose his job. His income

would fall, he would receive payments, and then in calculating effects it would appear as though the program had induced that response.

To try to avoid that kind of spurious calculation—now, what Cogan did in his Rand report—and I understand Robins (SRI) has some recent studies—is perform tests of simultaneity to see is that an important problem and John concludes not.

Senator MOYNIHAN. I am not familiar with the initial findings of the rural experiment. What did they say was learned about work?

Mr. WELCH. Well, they did not distinguish between participants and nonparticipants in the experiment. I would really hate to—

Senator MOYNIHAN. They did not say anything clear enough to—

Mr. WELCH. Well, no, they said things clearly enough—they were funny enough, as well.

One analyst observed that reported farm output fell. This is the way you lower your tax rate, and that hours reported worked on farms increased, and concluded that the efficiency of farming must have gone down.

Senator MOYNIHAN. Oh, gosh. I see.

Mr. WELCH. Another decided that maybe farmers were misreporting hours, that they wanted to create an aura of trying hard and in fact output had gone down, so he computed hours as being proportional to output as revealed from farm budget studies—you know, it takes 7 hours to grow an acre of corn—and concluded that hours, on that basis, had gone down and then analyzed that response as though it was, in fact, the hours response of the experiment.

In the summary report, the only data reported were simply average hours worked in the 3 months preceding, and then over the 3 or 4 years of the program between those families regardless of whether they received payments or not in the experiment.

By the way, the nonparticipants problem is much more important than the rural experiment. You would expect it just from these truncation points. We know farm income varies more transitorily than any other kind of income, or certainly than wage income. Only about 20 percent of the farm families were receiving payments in the third year and—

Senator MOYNIHAN. I see. Your point is that we learned from this about how to do this if we were going to do it right.

Mr. WELCH. I think so. We might. There are some very, very tough problems.

What really concerns me, in fact, is that we have identified a subset of problems that have raised very important problems that I wonder if there are not so many problems out there that—I am not certain at this point that it really is a convergent process that, you know, if we were to do another one, would another crop of problems pop up?

I talked to Robins at the break. He feels that SRI probably could conduct an experiment. One problem that neither John nor I mentioned that just makes things very, very difficult, and from an early statistical point of view made a lot of sense, was the way people were assigned to programs, because HEW gives a budget to conduct the experiment and the budget has so much transfers you want to get as many observations as you can, so you tend to find people programed so that they are hovering just below the break-even point.

As a result, we talk about the 11 programs in one experiment, and 5 or 6 programs in another experiment, but the characteristics of families in these programs are different, the preassignment characteristics. They also differ systematically from the control.

So it is not as though we can make simple control-experimental comparisons because they are not the same people, and that creates its own special statistical problem. We like to think that regression analyses, or the kinds of techniques that these people are using, that they can, in fact, control, but the reason for doing the experiment is to try to design out some of these statistical colinear control techniques to allow orthogonal designs so you can see what is happening without having to go through this fancy statistical calculation and, in fact, we are forced right back into it, as I pointed out with this truncation point, it is even more extreme reliance on this control population.

Senator MOYNIHAN. Right.

Well, I am going to have to read all of "Welfare in Rural Areas," obviously. I was kind of hoping I would be spared that.

Gentlemen, we thank you very much. This has been a great help to this committee. You have told us again some things we did not want to hear, but we are glad we did. We know more now than we did when we started.

Mr. WELCH. Could I say one additional thing on the marital response?

I am not familiar with this literature, but it would seem to me quite logical to expect a very different startup effect, either for an experiment or for a full-fledged welfare program than a continued effect, and whether we get married, for better or for worse, most of us expect better and if worse occurs and we fall in a poverty program or a poverty situation and then a welfare alternative is offered that adds independence, it would seem to me to be quite reasonable to expect marital dissolution for people who are unhappy under any circumstances.

Senator MOYNIHAN. It may not just be that panel fatigue effect. There is a first-time effect?

Mr. WELCH. I do not think so. I think it is a startup effect. In fact, I asked Larry Orr who is sitting beside me when I heard this result and he said by the third year of Seattle-Denver they could not find a very significant differential, that most of it was front-end loaded and it seems to me that startup problems, whether it would exist in the real world or only an experiment, would be very different from a persistent problem.

Senator MOYNIHAN. It also seems to me it would be useful to know a little bit more about that, don't you think?

Mr. WELCH. Oh, sure.

Senator MOYNIHAN. I mean, you are dealing with something pretty large in someone's life.

Mr. WELCH. Yes.

Senator MOYNIHAN. I thank you gentlemen very much and the committee thanks you.

Now, finally this afternoon we are going to have the pleasure to hear from Dr. Samuel Klausner who is the director of the Center for Research on the Acts of Man. Is that at the University of Pennsylvania?

Mr. KLAUSNER. It is an independent, nonprofit center and it is located at the university.

Senator MOYNIHAN. In that environment.

Mr. KLAUSNER. Yes.

Senator MOYNIHAN. Well, we welcome you, sir. I see you have a colleague with you.

Mr. CRAWFORD. I am Dr. Crawford.

Senator MOYNIHAN. Dr. Crawford, good afternoon, sir. It is good to have you here.

Sir, we have, of course, read some of your recent work and we are very happy to have you here to talk about our problems.

Mr. KLAUSNER. Thank you.

#### STATEMENT OF SAMUEL Z. KLAUSNER, CENTER FOR RESEARCH ON THE ACTS OF MAN, PHILADELPHIA, PA.

Mr. KLAUSNER. With the chairman's permission, I would like to preface my comments with a quotation which is 180 years old from Thomas Malthus.

Senator MOYNIHAN. From Thomas Malthus?

Mr. KLAUSNER. From Thomas Malthus. It is in the first edition of his population essay in 1798. Malthus says:

To remedy the frequent stresses of the common people, the poor laws of England have been instituted, but it is to be feared that though they may have alleviated a little the intensity of individual misfortune, they have spread the general evil over a much larger surface.

It is a subject often started in conversation that, notwithstanding the immense sum that is annually collected for the poor in England, there is still so much distress among them. Some think the money must be embezzled, others that the church wardens and overseers consume the greater part of it in dinners. All agree that somehow or other it must be ill-managed.

But a man who sees a little below the surface of things would be very much astonished if the facts were otherwise than it is observed to be.

Senator MOYNIHAN. It is a quotation with which I am familiar. I often have wondered what those dinners were like.

Mr. KLAUSNER. I would like to begin with a general orientation statement. In a way, I begin with this broad statement as, perhaps an ode upon reading Michael Harrington's "The Other America" and on the tendency to see the welfare population as of a single kind. Sometimes we create programs to deal with a particular population and then infer the existence of a community when we have created a community.

I begin by noting the diversity in the welfare population.

Three types of impoverished people may be distinguished, according to the social organizational source of their misfortune. One, the social outcast for whom economic exclusion is but one of an array of forms of social participation not easily available to them. They are socially banished, placed outside the regular social stratification system of society.

Second are unemployed proletarians. These are workers who are ordinarily employed but, because of economic conditions, regional shifts in industries or technological change, are displaced in the labor force. Their impoverishment is traceable to the mechanism of the social system of production.



Three, abandoned dependents. Typically one or a few members of a family engage in economic activity and distribute their income to children, the elderly and the disabled who are bound to them. These dependents become visible to society when the family transfer system is interrupted.

Different policy approaches are needed for each category, consistent with the organizational processes for reducing that problem. The outcasts, largely black, are not at the bottom of the stratification system but are outside it. The lowest stratum of the stratification system would have a recognized role in the system whereas the labor of these individuals seems sometimes to be considered socially useless.

A good part of the AFDC population, the largest category among WIN clients—and ours is a study of the work incentive program—consists of outcasts whose situation is complicated by their being abandoned dependents at the same time. Outcasts, and often the impoverished in general, are thought of as a homogeneous group, but they are not a group. They may include migrant workers and ragged women carrying newspapers and sleeping outdoors as well as the matrifocal family studied in Camden.

A 10-year study of the population served by the work incentive program in Camden, N.J., commissioned by the U.S. Department of Labor, has been carried out by the Center for Research on the Acts of Man. The study uncovers some errors in our ideas about that population. These include the notion that they consist of an unemployed proletariat.

True, many of the welfare clients are society's outcasts. Detachment from the economy is associated with exclusion or withdrawal from the dominant political and religious organizations of the society as well.

We have some analyses which deal, for example, with the question of black religion. The religion of a group is an index of that group's integral to the larger black society religion in America is not a part of white Christian's, but has the characteristic of a religion on its own, and so defines a community relatively culturally separate.

It does not, however, follow that many of the values and the ways of life of the white community do not hold for the black community.

The second error is that welfare reduces family stability by encouraging men to abandon families for the sake of an economic advantage. In reality, family instability in this welfare population has the same causes as instability in middle-class families. We have a rather extensive analysis of the basis upon which families broke down.

Welfare payments, however, by providing the women with a measure of economic independence may, as does alimony, retard the reformation of families once they have broken.

Third, that the female-headed household is matriarchal. Actually, these households belong to a patriarchal kinship system. As individual households, they are female headed but not matriarchal. The inability of a mother to exert authority may be one source of disorder in them.

The term "matriarchality" appropriately applies to the norms of the kinship groups or the community. One can have households within a matriarchal or a patriarchal community which do not have a matriarch or a patriarch present. Then there is a need for adaptation.

Our finding is that this community is basically committed to a patriarchal norm but misses the patriarch. That is where the problem is.

Senator MOYNIHAN. Why this is a hard thing for people to understand I do not know, but France in 1920 was not any the less a patriarchal society because a quarter of the households had female heads because males had been killed at Verdun.

Mr. KLAUSNER. Precisely.

Following are some other findings on the WIN program:

Husbandless mothers in the study may be divided on the basis of their commitment to either "traditional" or "modern" lifestyles. These lifestyles are determinants of WIN program participation, work behavior, and attitudes toward the family.

The WIN selection process is one of negotiation between clients and the agencies. By selecting for training the most competent and those already on their way to modernizing, the WIN agency selects for its own success. The WIN program is a rallying point for modernizing women. Over time, modernizing and traditionalist women in the welfare population become polarized.

Within the text of the report, we provide definitions of the terms "modernized" and "traditional."

WIN-trained mothers become modernized, active, and socially mobile, more than do low-income working mothers who have not gone through the program.

On the question of women in the labor force we find that higher levels of education do not translate directly into higher wages for these women. Our study adds nothing new on that topic.

The higher earnings of the better educated women result from working more hours rather than higher wages.

Educational attainment contributes more to economic independence when education is followed by labor force participation. This also has been found in other studies.

Vocational training alone does not have as much influence on later job success or earnings as does graduation from high school with a more general academic education. The import of that particular empirical finding has reference to job training. We cannot think simply of skill training. There is a wider socialization requirement for them to fit into the work situation, and it turns out—seemingly paradoxically—that a general education diploma basically a liberal arts education, is more likely to get them the job. They pick up the skills subsequently.

Females with an "authentic feminine gender of identity," rather than those identifying with males, are more likely to enter the labor force. These are working women in the traditional sense of the rural women working in the field.

A dimension of our study was psychological. We did objective testing of gender identification. A woman who is more accepting of her own feminine identity makes a stronger contribution to the work force.

Senator MOYNIHAN. That is a little outside the range of this committee, but we will let that stand.

Mr. KLAUSNER. The sex typing of work roles is less of a barrier to women entering the labor force than is the sex typing of household roles. The transition to modern work roles for women begins with a change in family roles.

The work status of a woman's friends and relatives has a more immediate influence on her own work participation than to her attitudes toward work and welfare. We looked at friendship and kinship networks and the characteristics of people in the network. The underlying notion of that finding is—to use the jargon of the social scientists—that the social structural situation is more important than the cultural attitude in determining what they do.

On men in the labor force: The greater the authority of their fathers in their childhood households, the more likely are the sons to advance socioeconomically. However, the greater the father's authority, the more likely are the daughters to be on welfare.

That has to do with traditionalism. In homes that were highly traditional and patriarchal, the advance of sons is promoted, while the women are encouraged to pursue the traditional woman's role in the household.

A small family of origin is more important than stability of that family in generating working income for a son among both whites and young blacks.

On families: Among men, the delay of marriage, and particularly delay of fatherhood, are conducive to economic success. Among women in this population, adolescence and common law marriages, nearly always fertile, endure longer than marriages they may form in later adulthood.

This is not a common finding.

Senator MOYNIHAN. That does seem rather counterintuitive, does it not?

Mr. KLAUSNER. That is counterintuitive. I am not prepared to generalize this to other populations, but the population of women who are fertile at age 16 and 17, apparently were more strongly committed to the traditional female role, to the wife and mother role. They tended to do more enduring in those relationships than those who deferred their fertility and marriage.

Women who enter their first serious relationship at age 18 as compared with those beginning later, have a stronger commitment to bearing and raising children. That is similar to what is found in demographic research. Eventually they will bear and rear large families.

Women who marry early report more marital conflicts in general, and particularly, more fighting over adultery, more sexual problems, more problems with drugs and alcohol.

The termination of adolescents' relations is more likely to be accompanied by quarreling and violence than is the termination of relations initiated later on. The early relations are indeed, although more enduring, more violent and passionate.

Absence of a father depresses the children's level of occupational achievement because it deprives them of financial and personal relational resource, not because it implies cultural deprivation.

Black households in this population are more patriarchal in all areas of family decisionmaking than are white households. Patriarchal authority in the family is associated with concentration on family life at the expense of participation by both mates in political and other forms of community life.

Matriarchal family organization emerges as the mate's attention is drawn to the wider arena of community life. We found a positive correlation between matriarchal authority and income.

**Senator MOYNIHAN.** Could I ask you, do you mean matriarchal or matrifocal?

**Mr. KLAUSNER.** In this case, it is matriarchal.

What we have here is the——

**Senator MOYNIHAN.** How do you get to be matriarchal in a society as patriarchal as ours?

**Mr. KLAUSNER.** You do not in the absolute sense. One could speak here of relative matriarchy and patriarchy.

We had a series of measures of family authority based on questions about who makes the decisions in the household.

We had a subsample of mated pairs. We asked them who decides in your house when to have sex relations, when to have children, which church to join or where the child should go to church, how to make a consumer purchase or a large purchase or a small purchase, who to have over to the house for dinner.

We measured whether or not they argued that these decisions in their households were always made by the male or the female. If both male and female agreed that the man always makes a particular decision, we called that household patriarchal. We then ran that measure against measures of social mobility and income.

We find the higher you go on the socioeconomic scale, the greater the amount of authority that is allocated to the women.

**Senator MOYNIHAN.** What do you do when the husband makes all the important decisions and they agreed the wife would make all the small decisions but no big decisions ever come along?

**Mr. KLAUSNER.** I think we want to make a distinction between the concern of that commonsense humorous story that you are referring to and the distinction between actual and delegated authority.

In a patriarchal household, even though the woman is making decisions within the household, the woman is doing it on the basis of authority delegated to her by the man.

The greater the significance of the family for the male, the more the tendency toward patriarchal authority. That is to say, in traditional patriarchal households, males are more involved in household affairs.

A poverty program targeted on the impoverished part of a population is inherently limited in what it may accomplish since the condition of that particular population results from processes in other parts of the society. Within the population itself, however, the study recommends varying the nature of services to meet the needs of the several types of clientele it serves. Central to the program should be an attempt to redevelop "community" by working with existing familial, religious, healing, educational, and political institutions.

The significance of this is in our concern about the notion of targeted programs. We feel the idea of targeting is an inappropriate generalization from areas in which instrumental thinking is more effective.

When one is dealing with technology, one focuses on a particular part of a technological system. Social systems are organic. You cannot easily treat one focal part. It is not more efficient to home in on one part of it without looking at the general impact.

Following are some of the recommendations of the report.

For the WIN program:

1. Voluntary deferment from WIN and similar work training programs should be granted nearly automatically in the case of husbandless mothers with four or more children.

The significance of this is that the current legislation provides for release from and based on the age of the children.

Senator MOYNIHAN. Not the number?

Mr. KLAUSNER. Not the number. The number is an indicator. It is not the underlying process. It is an indicator of traditionalism. This cultural evaluation is more important for job training than age of children.

2. Jobs should be available to "modernizers" and with Government subsidy or in public service when necessary.

3. Work training efforts of WIN should concentrate on the "temporary traditionalists" in transition to modernization. That is a matter of simple program efficiency.

Senator MOYNIHAN. If you had a big country to run, could you say that is how I spot an adjusted traditionalist when I see one?

Mr. KLAUSNER. The classifications are based on measures within the research instrument. And if you are asking if I could devise a criterion by which a counselor or social worker could make a reasonable classification, I think we could do that; yes.

4. Family service programs are needed for "adjusted traditionalists." Basically, we are suggesting the program not try to change the whole population. To modernize them, adjusted traditionalists, as we call them, getting into culture change. There are not enough dollars in the taxpayers' pockets to accomplish that. It is a more efficient use of the dollar if it is directed to those groups who are already seeking change.

Rehabilitation programs should be developed within the WIN structure and extended to cover all "incompetent" welfare mothers. Actually, we had measures of both mental and intellectual competence within the study. We found 20 percent of the population was not competent and should have been in rehab programs. That is what we are suggesting here.

Senator MOYNIHAN. Rehab?

Mr. KLAUSNER. Rehabilitation programs. That is, they were not competent to—

Senator MOYNIHAN. Had they been competent—rehabilitate them and they would be again. Rehabilitation suggests restoring an original condition, or am I wrong in that?

Mr. KLAUSNER. I am thinking in terms of the way the Government has used the term "rehabilitation program" which has been to work with people who are in some way disabled, beginning with the orthopedically disabled and those who are disabled because of mental conditions, and helping them adjust as best they can to social conditions.

Recommendation for AFDC populations:

1. Children above 14 in supported families should be scheduled for work training and job placement during summers and/or after they terminate regular schooling.

2. Support cottage industry. The argument against cottage industry has been advanced on the economic grounds that it is not competitive with the factory. Obviously but that is beside the point.

The important point is the self-development and independence of the person in the household. And even if it is necessary to subsidize that kind of artisan activity, we think that is worth while.

Senator MOYNIHAN. The sheltered workshops for the physically disabled?

Mr. KLAUSNER. I am thinking also of cottage industries for traditional women who are not disabled.

3. Day care should be provided to mothers who want to work even if their occupational status is not high enough to cover the costs. The rationale for day care must be that the program is beneficial to children and that the mother's employment can contribute to her own and her children's personal growth.

We make that statement because much of the analysis of the day care problem balances the "worthwhileness" of day care against the earnings of the mother. It strikes us that the important consideration for day care is the benefit the children may derive.

4. It is not sufficient to increase the skill level of the unemployed recipients. Wider socialization policies should be pursued in order to increase the attractiveness of these recipients to employers.

5. Generally, from the perspective of the economy, adolescent marriage and childbearing should be discouraged. But, where serious relationships are formed, their stability should be safeguarded. I think we need a more sensitive program in that area.

6. Employment and welfare policy must be developed in tandem with a national energy policy, and the appropriate organization mechanisms established for doing this.

This comment is one from a series based on studies we did relating to energy, transportation and poverty. Social welfare analysis generally takes place separately from the analysis of physical environment and resources. These facets of the poverty problem are also separated in governmental programming.

To work with the community is to deal with the physical habitat of that community as well as with social relationships.

7. Transportation systems serving low-income areas should receive a special subsidy to enable them to maintain appropriately frequent service with reasonable fares, connecting the population with places of employment.

For the long range: Select a limited sector of the outcast population as a focus of efforts to develop "community"—multifaceted participation by households in economic political, religious, healing, educational, and familial activities. Such a community may be developed, for instance, around an industry as a focus of employment and training efforts. By concentrating on a single industrial sector, a set of linked occupations, which link their incumbents and aid them in forming a common work culture, could be developed.

That is a bit difficult in terms of social policy. We would be selecting people in terms of their belonging to certain networks so that we could build up networks or communities of working people. But we do think it is worth thinking about some of the ramifications.

Senator MOYNIHAN. This is fascinating. Are you an anthropologist?

Mr. KLAUSNER. Sociologist.

Senator MOYNIHAN. It is a great pleasure to have someone besides those remorselessly accurate economists always pointing out the inac-

curacies of their colleagues' work, and to hear someone who is more in touch with people.

I have a couple of questions I want to ask you. We have heard a good deal about the family-splitting incentives here and there. What are the consequences of this? Is it generally speaking a good thing or a bad thing? You make the point that you don't see the welfare system per se leading to family breakup. Family breakup occurs for the same reason in this group as any other group. Welfare is the form of income you can get in the absence of a family income.

Have you studied anything about the condition of the children particularly when remarriage is not quite so common? There is a lot of serial monogamy among middle-class people. It is not quite the same in this lower group, is it?

Mr. KLAUSNER. Our feeling is that the family breakups, the family instability, is certainly not a salutary phenomenon for the society, that the family is one of the fundamental building blocks of communities. It is the one locus where many of the other social institutions come together around the socialization and development of children.

However, as you have noted, in our work we don't find that the breaking up of families is due specifically to the welfare programs. This is a general social process.

Senator MOYNIHAN. But if you had the option in some way or other to discourage the breaking of families, you would do so?

Mr. KLAUSNER. I would do so.

Senator MOYNIHAN. There is a lot of pretty irresponsible talk—sort of fading away now—but in the late 1960's, we began to hear all this was good, being rid of bourgeois hangups.

Mr. KLAUSNER. I am not for hedonism as a major social value nor do I think an individual's conception of what his happiness is at a moment should be allowed to be a major determining factor. I may be a bit traditionalist in that position.

Senator MOYNIHAN. You suggest we ought to discourage marriage and childbearing in adolescents. You are up against a powerful movement in the declining age of menarche. As you know, menarche dropped 4 years in a century. How would you discourage it? This seems to be something no one knows anything about.

Mr. KLAUSNER. You are asking me about—

Senator MOYNIHAN. Adolescent marriage and child bearing.

Mr. KLAUSNER. I think that adolescent child bearing raises real problems for the integration of the individuals into the economy and that is why we become so concerned about it.

Having said that, I don't consider it to be an abnormal phenomenon in the history of families. Our findings are that the adolescent liaison, the adolescent fertility that occurs in this population is basically occurring as a natural normal development of family.

For example, our findings are that adolescent girls who come from a two-parent home are more likely to get pregnant than those from a one-parent home. That is against the accepted wisdom of the system. It is often maintained that the girls who get pregnant are from broken and disordered homes. But that is not the case in the population we studied.

Senator MOYNIHAN. What is your hypothesis?

Mr. KLAUSNER. That the adolescent marriage is a normal continuation of normal family life and that the girls are just traditionalist

girls not attracted to the work world or the educational system and see their fulfillment in motherhood and wifehood and they get on with that as early as they can. So that I don't see that as an abnormalcy. I do see it as a disturbance in our current industrial culture because it disables them from appropriate economic participation later. That is where the problem arises.

Senator MOYNIHAN. Let me ask you one last thing and I see it is after 4:30. The majority leader asked me to call him, and I want to do that.

One of the things that attracted interest in the first reports of the Camden study, Dr. Klausner, was your seeming openness to providing traditional sorts of household-oriented work to the traditional household-oriented person. This Congress went through hell in the 1960's and during the 1970's over the suggestion that welfare dependent women ought to work at servile jobs, or seemingly servile jobs. And there was a political agenda on both sides of this argument and a great deal of bad manners and bad faith and no information. But we are a little calmer now. The women's movement has had the effect of making it much more respectable to suggest that women should work. It doesn't seem to be a punitive act.

But the question is, what is inappropriate? Do I take it that you feel from the basis of your research that there is a class of welfare recipients for whom the most natural and maybe even proper kind of work is the work that is associated with an extension of household work, what one does in parenting and that includes serving and ironing and washing dishes and looking after children? Do we read you correctly?

Mr. KLAUSNER. The distinction between modernizing and tradition is sometimes confused as that between working and not working. But there are modernizing and traditional occupations. Women who opt for the traditional family-focused way of life, are drawn toward occupations which are extensions of that role. They should be enabled to fulfill themselves within those types of occupations, there is a wide gamut of such occupations. Depending on their educational level they would go from domestic labor through teaching and nursing and other nurant types of activities. And I don't see anything wrong with these occupations being included in the programs.

Now, what does concern us is that sometimes domestic labor, is an unprotected laboring class. We have to attend to the conditions of work and work insurance for those who give their lives in the domestic field. But domestic labor per se seems to be quite appropriate for traditionally oriented people.

Senator MOYNIHAN. There are kinds of jobs which a certain kind of person prefers. It is an extension of that kind of life they have chosen to live. They don't actively want a different kind of life.

We might close with a line of Bernard Shaw's from "Maxims for Revolutionaries," if you remember. In "Man and Superman," it said simply: "Do not do unto others as they should do unto you. They might not have the same tastes."

And with that I think we will close today's hearings.

Thank you, Dr. Klausner, and Dr. Crawford for accompanying your colleague. It has been most helpful. It has been very refreshing and important to us to hear from you.



Mr. KLAUSNER. Thank you for the opportunity.  
 [The following was subsequently supplied for the record:]

CENTER FOR RESEARCH ON THE ACTS OF MAN,  
 Philadelphia, Pa., December 19, 1978.

Senator DANIEL I'ATRICK MOYNIHAN,  
 Committee on Finance, U.S. Senate,  
 Washington, D.C.

DEAR SENATOR MOYNIHAN: Thank you for yours of November 24 with the additional queries about our WIN research. Dr. Crawford and I have reviewed our data in the light of the points you raise and have arrived at the following observations.

Your first question concerns the relation between welfare, migration and marital stability. Our population was quite residentially stable—some two fifths of the WIN women were born in the Camden area and lived there all their lives. Of those not born in the Camden area, 95 percent arrived when they were 20 or younger. A few migrated following marital dissolution. More typically, though, the single parent families originated in Camden. Migration in search of welfare benefits was nearly nil.

Your second question refers to the influence of welfare on remarriage. We suspect the welfare retards remarriage, having an effect similar to alimony. Our data are not sufficient, however, for a definitive statement on this. The unmarried women, perhaps compensating for their disadvantage in marital status, develop larger kinship and friendship networks, both inside and outside of the households than do the married women. Those dependent on welfare are, therefore, not necessarily socially isolated. In fact, among them, marital dissolution has led to reliance and welfare and it has also led to the sharing of resources with extended kin and friends.

Your third question refers to the intergenerational effects of welfare. One could examine an array of influences on children of growing up in a welfare household. We concentrated, in our analysis, on the intergenerational transmission of welfare dependency. A slight relationship of this sort appears but we relate it to a larger phenomenon, the intergenerational transmission of social status. Though our society is, relative to other societies, open and mobile, we have both a benign cycle of affluence, and a vicious cycle of poverty. Surprisingly, this vicious cycle has not produced a larger caste of welfare dependents. Such a social event may await us in the future. Few of our sampled welfare dependents had parents who depended significantly on welfare.

Your fourth question refers to ways of discouraging adolescent marriage and childbearing. We suggest discouraging these events out of concern with their influence on economic mobility—not out of a sense that they are inherently dangerous to family life or to children. Further, the effort to discourage early marriage and childbearing should be consistent with the remainder of our original recommendation—that such marriages, when they occur, not be penalized. The “discouraging” may assume some common forms. We might improve or extend adolescent education about complications caused by early pregnancy—though most teenagers are aware of these complications. We might continue programs of family planning education as they are emerging through the schools as well as through other organizations. Our main observation, though, is that the best means of “discouraging” would be indirect ones. Early marriage and family formation becomes more attractive when educational and occupational career paths are blocked. Whatever social programs promote “modernizing,” or entry of women into modernizing occupations, or increasing their social mobility or their aspirations for social mobility, will, indirectly, defer marriage and childbearing.

This brings us back to job and job socialization programs designed to extend opportunities for women.

We hope these additional comments are helpful to the important effort in which you are engaged through the Senate Committee on Finance. It was an honor to be asked to testify.

Sincerely,

SAMUEL Z. KLAUSNER.

Senator MOYNIHAN. Thank you, ladies and gentlemen.

[Whereupon, at 4:40 p.m., the hearing was recessed to reconvene the following day.]

# WELFARE RESEARCH AND EXPERIMENTATION

THURSDAY, NOVEMBER 16, 1978

U.S. SENATE,  
SUBCOMMITTEE ON PUBLIC ASSISTANCE  
OF THE COMMITTEE ON FINANCE,  
*Washington, D.C.*

The subcommittee met, pursuant to recess, at 10:10 a.m. in room 2228, Dirksen Senate Office Building, Hon. Daniel P. Moynihan (chairman of the subcommittee) presiding.

Present: Senator Moynihan.

Senator MOYNIHAN. A very pleasant good morning to you all. We have a very interesting set of witnesses for the subject that we will be taking up this morning.

I would like first, just for the record, to correct a statement that appeared in the New York Times this morning which I believe might get us off into directions and hard feelings that nobody wants to have.

Dr. Jodie Allen, who opened our testimony yesterday, and who was brilliant and informative and open as always, is quoted in the Times as saying with respect to "my assertions" that "we did not propose a program of the type he is criticizing."

I spoke with Dr. Allen this morning and she did not say anything like that, and it is obviously a misunderstanding somewhere along the line, as I have been at great pains not to be criticizing anything in these hearings. I have said from the beginning that this was a major area of social policy in which we committed ourselves to an experimental mode.

We went two-track, you see. On the one hand, we have had four Presidents in a row advocating it; two (four different parties) have specifically proposed it to the Congress. Two others have been well-disposed to dealing with the matter. And, simultaneously, we have been carrying out experiments in New Jersey and North Carolina and Iowa and then the major Seattle-Denver one, and work has been done by people of the very highest quality who have spent a decade of their lives analyzing this work.

And I have felt that it is the responsibility, certainly, of this subcommittee to learn what has been learned. We are going to have spent \$150 million looking into the nature and dynamics of the various income support programs and we are going to find out what have been the results before we go into the next legislative session.

The results are clearly a disappointment to us and to many researchers in the sense that we would have hoped for a more resounding confirmation of the fact that there are large social benefits to be derived. But, all right. They came out as they came out, and I said to a succession of witnesses yesterday that they had done honorable work and

they had done their duty and they were supporting their results and that they should not, in any way, feel that they are somehow responsible for the fact that the data turned out as the data did. To the contrary, that is their integrity.

I would hope we are not getting into an atmosphere with the administration in which our presuming to ask what has been the results of these data is taken to be a hostile act. That is pretty low-grade stuff and it is not, in any way, what Ms. Allen intended.

I would like to make clear that she regrets that as much as you would expect us to do, and we will now put that aside.

Our subject today is what to do about New York City, and no one knows better than Donna Shalala. In the course of your testimony you may say anything you might wish, but I do hope you will find opportunity to comment upon the lead story in the Times this morning which is saying, very simply, that New York officials feel drastic steps are necessary to resolve the budget gap, as you and I have talked about many times before.

Dr. Shalala is, as is well known, the Assistant Secretary of Housing and Urban Development for Policy Development and Research.

Would you introduce your colleagues?

Ms. SHALALA. Dr. Raymond Struyk, a housing economist who is the Deputy Assistant Secretary for Research.

Senator MOYNIHAN. Dr. Struyk, we welcome you.

I will make a claim which is irrelevant, but not wrong, which is that the first Assistant Secretaryship for Policy Development Research in this area was started in Labor in 1962 and I was the first incumbent and then a well-known process of diffusion began and eventually it got to HUD where it has attained truly distinguished heights.

In the process, a kind of joint Department of HEW and Department of Labor exercise took place, and the proposal for an income guarantee came forward in the late 1960's or early 1970's. There was much talk about an income strategy as against a services strategy in these matters.

The Department of HUD got very envious—that is perhaps the wrong word—got very interested in this and said, we ought to have an income strategy too and they commenced the housing allowance and got through their committees what in a curious way could not then be gotten through the other committees.

And so now we have had this activity in place for some time now and it occurred to us that with so distinguished an incumbent—you have given a paper recently on the matter—we should hear something from you of the results of the housing allowance, and we welcome you to this committee.

Ms. SHALALA. Thank you, Senator Moynihan.

I thought I might summarize my statement, unless you would prefer that I read it.

Senator MOYNIHAN. We will put the statement into the record as if read, and then you may go ahead as you see fit.

**STATEMENT OF HON. DONNA E. SHALALA, ASSISTANT SECRETARY OF HOUSING AND URBAN DEVELOPMENT FOR POLICY DEVELOPMENT AND RESEARCH, ACCOMPANIED BY RAYMOND STRUYK, DEPUTY ASSISTANT SECRETARY FOR RESEARCH**

Ms. SHALALA. Just beginning with a reminder that the Department searched, beginning in the early 1970's, for some alternative housing policies which resulted in the design of this experimental housing allowance program, and it was focusing on ways to improve the housing consumption of poor people that led the Department, during that period, to search for a genuine alternative to the kind of new construction programs and other kinds of subsidized programs that housing policy had traditionally been about in this country.

We sought information about the behavior of low-income families in this case, when they were actually given cash payments for housing, and EHAP itself is a set of related experiments, each of which focuses on different aspects of a cash payment program.

One of the three focuses is directly on the behavior of low-income families, and we call that the demand experiment, and in Pittsburgh and in Phoenix we looked at how families acted in different kinds and different levels of assistance payments.

What is particularly important about the demand experiment, we believe, is that we not only looked at what people did about their housing after they received the cash payment, but we simultaneously monitored a comparable group who did not get the extra money at all.

Second, in addition to—

Senator MOYNIHAN. May I interrupt to make a remark that came to me yesterday. Fred Mosteller at Harvard used to say we didn't have any controls because it was only an experiment. You had controls; good.

Ms. SHALALA. Second, in addition to testing out family behavior, we wanted to see how housing markets would respond to a full-scale program, particularly a full-scale program where it was possible to mount a full-scale program, and this we were able to do.

The supply experiment was put into two contrasting metropolitan areas, and Dr. Struyk can discuss the soft and the tighter markets of Green Bay and South Bend. Those entire metropolitan areas were covered.

The major issue in the supply experiment was, of course, inflation—a critical one for today. The question we wanted answered was, would people get better housing for their additional dollars, or if we gave all eligible recipients the money directly would prices simply go up and they would get the same quality of housing? That is, what were the inflationary effects of a housing assistance program.

Finally, we wanted to know about the administration of the program. When it was run by local public agencies—we weren't really trying to find out if public housing authorities were better administrators than State welfare agencies. Instead, we really wanted to look

at important administrative questions, such as: Do different approaches to housing inspection influence the quality of housing a family chooses? Does the way in which an agency verifies a family's income make any difference at all?

Before I summarize our findings to date, let me simply repeat how the EHAP program works. An eligible family selects a place to live from the existing housing in their community and if the unit meets the housing requirements that have been established for the program, the family then receives a housing allowance in cash.

Now, Senator, if I might, let me summarize the first results from EHAP and then Dr. Strivyk and I would be happy to answer your questions. Some of them were surprising to us, and perhaps they should not have been, but they certainly were.

The first results from EHAP suggest the following: First, that cash assistance with no strings attached will not work to improve the housing conditions of low-income families who are living in inadequate housing. Remember, I am talking about cash assistance here, not about housing allowances yet.

Second, that housing allowances, on the other hand, are more than just an income transfer. They do seem to induce some increased housing consumption in the short run and, given the normally high mobility rates, longer run they will induce some increased housing consumption when families finally make a move.

But housing allowance incentives do practically nothing to speed up the moving process. That is, if you give people the money, it is not the housing allowance itself that will change their previous documented moving patterns. That has very important implications for integration, for those people who thought if you gave people some money earmarked for housing that people would go running out and then able to move into neighborhoods other than those that they were already in.

Third, housing allowances, we believe, are not a substitute for new construction or substantial rehabilitation, and this is because low participation rates resulted in very small increases in demand, and allowances do not provide the necessary incentives for such important objectives as neighborhood improvement or additions to the housing stock.

Of course, for the Carter administration, that is an extremely important finding, because as you will see, as we begin to answer some questions, the number of people who are actually eligible, the number of actual participants, does not constitute a large enough group to generate a large enough demand that provides enough resources to generate new construction or substantial rehab.

So particularly in a very tight market, for example, where this would—housing allowances could not be seen as a substitute for what the Department is now doing in the new construction area. It simply would not generate enough housing.

Finally, housing allowances contribute modestly to the maintenance of existing housing stock. They seem to encourage small repairs and thus, in the long term, they can serve to deter a process, a costly process, of deterioration, which ultimately may require a large dollar investment.

There you have it briefly, Senator: some very preliminary findings. We will firm them up by next year. We have, in fact, reorganized this

effort so that we intend to get the results out to all of you almost 2 years, in some cases, ahead of what we had expected. But those are the major findings, and for those of you who had expected the housing allowance program to substitute completely for what the Government is now doing, we simply have not had those findings.

Senator MOYNIHAN. You are finding that everything relates to everything and it is all a bit complicated, but let me ask you a little larger question: Do you feel that you know more about your subject today than when you started?

Ms. SHALALA. Absolutely. We have the best housing market data that we have ever had from the supply experiment, for example. We have never been able before to answer with any kind of precision what would happen if you actually gave people the money and let them go out and find their own housing. There is a big gap in our knowledge in housing policy about those kinds of questions, and this was essentially a study to answer those kinds of questions, rather than doing what the Government has traditionally done, and that is subsidies for public housing and the construction-oriented kind of approach. This was a test of an incomes transfer policy.

I do not know whether Ray wants to add anything to that.

Mr. STRUYK. I would like to add a couple of things, Senator. One is that, as you know, the country has never had a housing program that was available on an entitlement basis. In fact, in Green Bay and South Bend, that is where our programs were operated. So, for the first time, we have some firm idea of what the participation rate would be under these kinds of conditions, and they have turned out to be relatively quite modest.

Among renters, about half of the eligible renters actually end up in the program. Among homeowners—and homeowners are allowed to participate in both Green Bay and South Bend—the participation is about 30 percent.

Senator MOYNIHAN. This was rather striking to us, the participation rate being as low as it was—50 percent for renters, 30 percent for homeowners. What do you make of that?

Ms. SHALALA. Well, there are a variety of explanations when you disaggregate that data. There were different participation rates for different groups.

The elderly participated in lower percentages than did some other groups, and we know from other income transfer studies that there are problems of getting the elderly in some welfare-type programs.

Senator MOYNIHAN. So you have differences by age?

Ms. SHALALA. So that there are—

Senator MOYNIHAN. The cohorts there make a difference?

Ms. SHALALA. In addition to that, the poorest people had low participation because of difficulty in meeting housing requirements. This finding is similar to the results of other studies that have been done.

In addition to that, you will remember that if people were living in housing that was inadequate by our definition, to get the cash payments, they might have had to move out of their unit or even the neighborhood they were in, and some people simply chose not to do that. So that there are multiple reasons.

Is there anything that you would like to add, Ray?

**Mr. STRUYK.** Beyond that, the type of income seems to make some difference. We get high participation rates, particularly among those who are on welfare, those who are consistently on welfare. When we started these programs in Green Bay and South Bend, we were at a downpoint in the business cycle.

**Senator MOYNIHAN.** When was that?

**Mr. STRUYK.** They started payments in 1974 and 1975.

We had a number of young husband-wife households who came in the program at that time, but as soon as the economy went up, they were no longer eligible. They became reemployed and so on.

So what has happened is that we have a—

**Senator MOYNIHAN.** We heard yesterday from Dr. Allen a wonderful term about that phenomenon with respect to marriage formation and dissolution. We heard of "panel fatigue."

**Mr. STRUYK.** We have had a lot of churning in the program, a lot of turnover. About a third of those who were initially in it have left, for example.

In addition, while there is 50 percent participation at any point in time, it takes some time for people to get in the program since they have to find a unit, and they have 90 days or so to do that. So there is a float. So the actual number involved at any point in time is higher than that, so that when you consider all of that together, the participation rates are not so surprisingly low, although we simply did not know it would work out before.

I ought to mention that this really has a fundamental effect on the thinking of the Department about the total size of the programs, and so on, that we may have.

**Senator MOYNIHAN.** In what direction?

**Mr. STRUYK.** If the original idea was that you were going to have 80 or 90 percent of households under this kind of a program participating, it meant that if you went to the Office of Management and Budget or the Congress, your budget estimates would be much higher than they would be under these estimates, and it has affected it that way.

**Senator MOYNIHAN.** Are you thinking of expanding the program, making it permanent, making it a kind of entitlement? Would you like to do that if you could, if we gave you the money?

**Ms. SHALALA.** No. The Department now has a program that was designed, in part, out of some of the experience with EHAP which is the existing housing subsidy program, the section 8 existing program, which has some of the characteristics of the experimental housing allowance program. What we are really doing is combining our evaluation of that program with this one and improving that program which, like housing allowances provides subsidies to people who rent existing units in the private market.

At this moment, that program in some ways serves as the existing subsidy portion of the Department's overall housing strategy.

The Secretary will review these findings next year when we firm them up but, at this moment—

**Senator MOYNIHAN.** You are not finished at this point, but do you see a time when this will come to an end as an experiment?

**Ms. SHALALA.** Yes. The administrative experiment is ended; the demand experiment is, for all practical purposes, ended; the supply experiment, we are going to keep monitoring it for a couple of more

years because we are concerned about the inflation findings and we have got to make sure that our early finding about no inflationary effect is not simply a short-term one.

But we will, leaving aside the final report on the supply experiment, we will use internal findings from the supply experiment and final reports on the administrative agency experiment, the demand experiment next year.

Senator MOYNIHAN. We have commenced a mode of innovation in social policy which is basically experimental. Nothing quite like it has ever happened before.

One of the problems is to translate the results for policymakers. There is no member of the Cabinet, with the possible exception of the Secretary of the Defense, who could, in the normal circumstances, be expected to be able to deal with the statistical methodology involved. That is not unusual.

But it is new that they should be expected to do this, that one should come along and say here, I have this. Here it is. It is the answer.

And so the translator becomes a very important person. President Eisenhower hired a science adviser because it was very clear that there were decisions to be made in which he and his Cabinet had to deal with with science and he was only getting one kind of science advice from the Pentagon so he got himself an adviser of his own and then he got two science advisers. I don't know whether that helped him or not, but at least he knew there was more than one view of the matter.

What have you learned that has surprised you so far? What comes under the heading of counterintuitive?

Let me go back. The setting of this hearing is the finding of the guaranteed income results which have been making very clear that they are not programs that President Nixon proposed or President Carter proposed, but they are in the same family of ideas.

They produced, in the work area, higher rates of work reduction than had been anticipated and we heard some troubling testimony that the original reports greatly underestimated it. At least we have one set of scholars that say the work reduction was twice what the original reports said.

And the rates of marital dissolution were also rather startling. They ranged up to 60 percent higher for the experimental group than the control group. Well, that comes under the heading of counterintuitive.

What did you find that was counterintuitive that told you people do not act the way housing professionals say that they act, or housing committees in Congress have been saying they act?

Ms. SHALALA. Probably three findings, Senator. We expected the participation rates to be higher. That was the first finding that surprised us.

Second, we were surprised, and are still a little surprised, that housing allowances were not inflationary. Most housing economists during the period that this was designed expected to find some inflationary effects of housing allowances.

Third, the consumption patterns, which I think Ray Struyk knows a little bit about more than I do, were surprising to housing economists.

Mr. STRUYK. We have already talked a little bit about participation. Would you like us to elaborate a bit on the inflation one and on the consumption patterns?



Senator MOYNIHAN. No; I would like to ask you about participation, because your participation rates are about the same as food stamps, I think.

Ms. SHALALA. That is right.

Senator MOYNIHAN. Is there a ratio here that we are running into that we should know about?

Mr. STRUYK. We certainly would not suggest there is a general rule. The kinds of reasons that are given—we asked nonparticipants a whole series, or battery, of questions as to why they did not participate and we have looked carefully at participation patterns in terms of initial degree of satisfaction and dissatisfaction with neighborhood and dwelling and that kind of thing; this is a dimension that is different, clearly, in a housing program than it would be under the food stamps.

There may be some overlap in the hesitancy of possibly older households, for example, to be involved—the redtape, the fear of crime, and so on—but we would not have any information which would allow us to make that comparison in a tight way.

Senator MOYNIHAN. Fine.

Did you find any influence on family stability?

Ms. SHALALA. That was not one of the questions that we analyzed.

Senator MOYNIHAN. You did not pick up any data at all?

Ms. SHALALA. Remember that our concern here was to improve people's housing. We are not analyzing it as simply an income transfer program but we wanted to see what effect it would have to really improve their housing and the finding, therefore, that there was some improvement in their housing is important to us but it—

Senator MOYNIHAN. There is the opposite side of that finding and that is that people who were given money did not use it to improve their housing at all.

Ms. SHALALA. Right. Without a housing earmark—

Senator MOYNIHAN. Unless you had to use it for housing—

Ms. SHALALA. That is exactly right, which suggests that if one of your policy goals is to improve the housing of poor people that simply giving cash payments alone is not going to do that.

Senator MOYNIHAN. Now, let me not press you, Donna Shalala, but does this suggest that you do not need a Department of Housing and Urban Development?

Ms. SHALALA. No; it suggests that we do, Senator.

Senator MOYNIHAN. Does it suggest that we do because if we do not make these people do it, they will not do it on their own?

Ms. SHALALA. No; it suggests that this country continues to have, as its goal, decent, safe and sanitary housing for all people and the Federal Government continues to have as its goal improvement in the quality of housing of poor people and simply giving people the money will not do that, and therefore the Department of Housing and Urban Development—

Senator MOYNIHAN. Boy, have I got you roused up. [General laughter.]

Ms. SHALALA [continuing]. Which has a variety of programs, is—

Senator MOYNIHAN. What would you say if we had a goal that people could do what they damned well pleased and just had enough money to do it?

Ms. SHALALA. Well, none of these—

Senator MOYNIHAN. I have you there, don't I?

I am kidding you just a little bit, because you remember, and there are not many people around who do, that housing was the first social program of the National Government. It emerged in the 1930's when it was thought to have two formative influences on behavior.

It was observed that people who lived in nice houses had high incomes, sent their children to college, paid taxes, voted regularly, and infrequently went to prison, and it was concluded that these people were different in all respects except that they all had nice houses, and that it was nice houses that made them have high incomes, send their children to college, vote regularly, and infrequently go to prison.

And, as you know, it turned out not to be so. If housing has any influence on behavior, it is residual and not very well understood. It is something people consume and value, yes, but it is just something that they will consume and value, and there are other things that they will consume and value, too.

Its social rehabilitative role was much overestimated by people two generations ago.

Ms. SHALALA. As an independent product, Senator, but I think that what social science research has told us since then is that in combination with decent education, with decent incomes, that housing along with those other things does, indeed, have a usefulness and a reason of the National Government to be involved.

Senator MOYNIHAN. What? What research has told us that?

Ms. SHALALA. Well, I would assume, without getting pinned down on specific research projects, that the National Governments, concern about intervening in the education for poor people, in jobs for poor people, in housing for poor people, reflect some body of information that leads us to believe that—

Senator MOYNIHAN. Donna Shalala, that is not so.

Mr. STRUYK. Excuse me. Could I pursue, just for a minute, your statement earlier—

Senator MOYNIHAN. Sure.

Mr. STRUYK. Could I pursue for a minute your statement, a part of your broader statement, about the value that the poor place on housing? As you well know, the fraction of income that the poor devote to housing is a very large share of their budget.

Senator MOYNIHAN. And so is the fraction that they devote to food.

Mr. STRUYK. No. They do devote a large share on food, but we are talking about 40 percent of income—

Senator MOYNIHAN. That does not mean that they value food more than other people value food.

Mr. STRUYK. What we think we are observing in part here, Senator, is that the people have placed some minimum in their own minds, defined some minimum socially acceptable bundle of housing services that they feel they need to consume that is to some degree safe and sanitary and so on, and that is, in part, why we find a number of poor people living in units that meet our inspections at the beginning. And that when you give them additional money, then, they substitute away from housing to a fairly large degree, even in the face of earmarking, because they have already—because of this value they place on it, already devoted really disproportionate resources toward housing.

You know the budget shares show the standard budget for low income which the BLS calculates shows people would be spending 25 to 26 percent of their budget, and obviously the poor spend much more, of a lower income.

Senator MOYNIHAN. I am not trying to get rid of the Department of Housing and Urban Development; I like the Department of Housing and Urban Development. But I make the point, gently, that the research basis on which most of our programs are based is practically nonexistent.

Supposing we had your counterpart in the Department of HEW over here, and I said, could you please tell me what is the research basis for the educational programs of the Federal Government, what would he say?

Ms. SHALALA. I would not answer for Henry Aaron.

Senator MOYNIHAN. He would say, oh, we have this and we have that, and he would send you in a mountain of things, and it would not amount to a thing. They have no basis and they have no research results. They had none to start with, and what they have been trying to find, they have not been able to come up with. At least, then results are very peripheral and marginal.

In all truth, I think it is fair to say that housing goes on as a major expenditure of this Government because it is there, and the notion that housing constitutes a major social intervention does not have much basis in fact. It is a good thing. It is like Mosteller and I said when we put out that long book about the Coleman findings; when people said that the data shows that schools do not matter, we said that you would have to talk to someone who never went to a school, and then you would find that schools do matter.

And anybody who thinks housing does not matter should talk to someone who, for the last 3 years, has been sleeping in the park, and they would find that there is a difference between having a roof over your head and not having a roof over your head.

Ms. SHALALA. Sir, let me simply agree with you in part, that we do not have a lot of empirical evidence on whether housing changes people's behavior, but the real question may be whether it improves people's lives, and it is on that basis that—

Senator MOYNIHAN. You made a very quick slide between changing behavior and improving lives. I will tell you what it does do. I will tell you something that I know. People's behavior changes housing, right?

Deponent sayeth not. Of course it does.

Ms. SHALALA. Sure it does.

Senator MOYNIHAN. We thank you very much for this, and I think it is a very nice case of a Department carrying out a rather small-range experiment. How much have you spent on all of this?

Ms. SHALALA. Senator, this experiment, when it is completed, will cost \$200 million.

Senator MOYNIHAN. \$200 million. A medium-sized experiment, learning something and doing no harm and being very open about it and refreshingly informative. We thank you very much, and we thank you, Mr. Struyk.

[The prepared statement of Ms. Shalala follows:]

STATEMENT OF DONNA E. SHALALA, ASSISTANT SECRETARY FOR POLICY DEVELOPMENT AND RESEARCH, U.S. DEPARTMENT OF HOUSING AND URBAN DEVELOPMENT

The Experimental Housing Allowance Program (EHAP) about which I shall be speaking today has incurred a sizeable investment of dollars by the Government, and has produced extraordinarily detailed data about the behavior of lower-income families in our country. EHAP is the largest social experiment ever undertaken by the Federal Government. What we are learning we believe is not only useful to our discussions of national housing policy, but also to other social policies, such as income maintenance.

The experiments were meant to produce useful information for policymakers and legislators. It is with the concern that we share our findings with those who are charged with developing more effective public assistance policies, that I come before you today to report some preliminary findings.

EHAP was designed to provide information about the actions of low-income families when they are given cash payments for housing. The program is a set of three related experiments, each of which focuses on different aspects of a cash payment program.

One of the three experiments focuses directly on the behavior of low-income families. We have called it the "Demand Experiment." It was conducted in two large metropolitan areas—Pittsburgh, Pennsylvania, and Phoenix, Arizona.

In these two places we examined how families acted in response to different kinds and different levels of assistance payments. About 1,250 households were enrolled in each of the two locations.

What is particularly important about the Demand Experiment is that we not only looked at what people did about their housing after receiving a cash payment, but we also monitored a comparable group of about 550 families at each location who didn't get the extra money. Therefore, we can compare what families do when they get extra dollars for housing with what they would do without the extra money.

In addition to testing family behavior, we were also very interested in seeing how housing markets would respond if a full-scale allowance program were implemented by the government. Thus, we designed what has been called a "Supply Experiment." We chose two contrasting metropolitan areas—Green Bay, Wisconsin, and South Bend, Indiana, in which to run the full-scale programs.

The major issue we focused on in the Supply Experiment was inflation. We asked: would people get better housing for their additional dollars or would prices just go up for the same quality of housing?

To serve the entire eligible population, we included homeowners as well as renters in this experiment. Currently, around 9,000 households are receiving allowance payments in both locations.

Finally, we wanted to know about the administration of such a program when run by local public agencies rather than by research organizations. So we included a third experiment called "The Administrative Agency Experiment."

The purpose of this experiment was to see what kinds of administrative problems arose when different techniques were chosen to implement the program. We chose eight different agencies in eight varied metropolitan and rural areas across the country.<sup>1</sup> Each served from 400 to 900 eligible renter families.

Contrary to what many people think, the purpose of this experiment was not to see whether a local housing authority can run such a program better than a state welfare agency or vice versa. It was, instead, to look at important administrative questions such as: Do different approaches to housing inspection influence the quality of housing families choose? Does the way in which an agency verifies a family's income make any difference?

In summary, then, we designed our research to cover what were believed to be the critical policy issues relating to housing allowances.

Let me turn now to how EHAP actually works. Under the program, eligible families select a place to live from the existing housing stock in their community. If the unit meets the housing requirements established for the program, the family then receives a housing allowance—in cash.

Immediately, a number of questions arise:

Who participates in such a program?

<sup>1</sup> Springfield, Mass.; Durham, N.C.; Jacksonville, Fla.; Peoria, Ill.; Tulsa, Okla.; San Bernardino, Calif.; Salem, Oregon; and four rural counties in North Dakota.

How do the participants use their allowance?

Does the quality of their housing improve?

Where do they move to, if they relocate to find decent housing?

Do housing allowances promote integration?

What happens to the price of housing when people in the community have a housing allowance?

When the experiments began, many in the housing field assumed that just about all low-income families not subsidized by the Government were living in very bad housing. In fact this is not the case.

It appears that *about half* the low-income renters and homeowners are living in units that meet a minimum definition of "decent, safe, and sanitary."

Now to be sure, the story is more complicated than that simple number implies.

For example, *low-income homeowners* are likely to live in better units—that is: more decent, safe, and sanitary units—than renters in the same income group. That's as you might expect. But among the low-income, both owners *and* renters who live in the better housing units are spending an inordinate portion of their incomes to live in those accommodations.

The average poverty household, in fact, spends 45% of its total disposable income on decent housing. I needn't tell you how much they have left to spend on food and clothing.

It is also important to recognize that the *other half* of the low-income population is living in housing which is *not* acceptable. Some of it requires only modest repairs to make it acceptable. But some of it needs extensive repairs and rehabilitation. And some is not really amenable to rehabilitation—in rural areas for example.

We also found that significantly more minority than non-minority families are badly housed. In part that's because their incomes are lower. But even when we held income constant, minorities were significantly worse off than their non-minority counterparts.

When EHAP began it was expected that a very large percentage of eligible families would accept the money offered and use it to improve their housing conditions. We were familiar with the long lists of families waiting to get into public housing.

In fact, participation was much lower than we expected. In one of our experiments, where we individually solicited the participation of a representative sample of the income-eligible renters, about 80% agreed to take the first step and enroll. This means they were willing to have their incomes checked to be sure they were eligible and their housing inspected to see if it was acceptable.

Among these 80%, those who already lived in adequate housing simply accepted a check from the government *and reduced their rent burdens*.

But, there were also families who were not living in acceptable housing. To get the allowance, you'll recall, they had to improve their housing. And only about a third of them did so.

Much to our surprise, many of these families didn't even search for something better.

Thus, the answer to the question, "Who participates?" is not "almost all"—but "*only about half*."

Similar results were obtained in another of the EHAP experiments. In this case, we were testing housing market responses, and so we opened the door of the allowance program to all income-eligible homeowners and renters in two SMSAs.

After nearly three years, and extensive outreach efforts, about 60% of the eligible renters and around a third of the eligible homeowners enrolled. But of these, for various reasons some families dropped out.

In the end, a little less than half of these eligibles met the necessary housing adequacy requirements. Which means that a little less than half of the eligible population chose to receive cash payments that would reduce their housing expenses.

In summary, participation was much lower than had been anticipated. In addition, those who chose to participate came more frequently from the better-housed than from the worse-housed members of the eligible population.

Much to our amazement, some families even turned down cash payments that would not only have provided them with more adequate housing but that would also have freed up their income so more of it could have been spent for other purposes.

In the longer run, we found that some of the families who didn't move at first, moved later on and increased their housing consumption (improved their hous-

ing). Even so, we expected the cash incentives to have produced more immediate change than actually occurred.

All of these results are understandable when we consider what we have learned in other studies about the importance of neighborhood attachment, and the desire of families to be close to friends and relatives.

The results also contradict sharply the assumption that, given the opportunity, all low-income families will flock to the suburbs. No such pattern occurred. This finding also has important implications for our racial integration goal. It suggests that housing allowances by themselves will not increase racial integration.

Nor did families use their housing assistance to buy luxury units. Some people believe they might because there was no ceiling on the rents they could pay, although the subsidy only covered the difference between 25% of their income and the Fair Market Rent. As a matter of actual fact, the low-income people in EHAP behaved quite conservatively. If they accepted the allowance, they used it to minimize their housing expenditures. And they were very reluctant to move away from their neighborhoods.

We have some other findings which have yet to be fully analyzed. For example, at one of our sites, where enrollment is open to all of the income-eligible population, at the end of two years, 87% of the eligible black renters took the first step and enrolled in the program. A very much smaller percentage of eligible white renters enrolled. At the same time, and in the same location, only 16% of the eligible elderly couples who were homeowners enrolled.

It was of interest to us to discover that the vast majority of our housing program eligibles were not deriving most of their income from welfare payments. For example in St. Joseph County, Indiana--one of our experimental sites--5 out of 6 eligible families derived the major portion of their income from wages and salaries or from pensions and social security.

When it came to enrolling in the program, however, essentially *all* of the AFDC eligibles in St. Joseph County enrolled while less than 20% of those whose major source of income was earned actually enrolled. An almost identical pattern prevailed in Brown County, Wisconsin--another of our experimental sites.

It appears as if the elderly and the working poor are much more reluctant to seek government housing assistance than those already receiving most of their support from welfare payments.

So when we speak of overall enrollment rates, we must note that there are major differences in how sub-groups in the population behave. We will be exploring these patterns further as we continue to analyze the data which EHAP has produced.

We have also learned that when families are given dollars which have no strings attached, very few of those dollars will go to purchase housing over and beyond what would have been purchased anyhow. On the other hand, if housing requirements are attached to those dollars, EHAP results show that families spend three to four times more of their allowances on housing than they would without such requirements.

Before EHAP results became available, it was widely believed that a greatly accelerated demand for better housing, brought about by additional dollars in the hands of low-income consumers, would lead to rent inflation not accompanied by improvements in housing quality. In fact, apart from the normal inflation which occurred during the same time period, rent inflation did not take place.

The explanation for this finding can be found in some of the results I have already discussed:

Roughly 20% of the total U.S. population is eligible for EHAP's housing assistance.

Of these, only about half, or around 10%, will actually participate in the program.

Of this 10%, about half already live in acceptable housing.

This leaves about 5% of the total eligible population who are going to make any change in their housing circumstances, and some of these families will repair their existing units rather than move to standard housing. Thus, the demand for housing is increased very, very little. The increase is certainly not enough to produce rent inflation.

By the same token, the increase is much too small to induce the construction of new units. Therefore, while rent inflation has not occurred as a result of EHAP, neither has any new construction or major rehabilitation. This is a particularly critical problem in tight markets.

Another area of research in the EHAP program has to do with the administration of housing allowances by local agencies.

We found, for example, that local agencies had a really hard time following a consistent set of policies in applying the program's standards for decent, safe, and sanitary housing.

In this context, we've also learned how challenging it is to apply a reasonable set of housing quality standards.

For example, sometimes, when elderly persons, or other families attached to their neighborhoods, simply refused to move, some agencies overlooked what they felt were flaws in the housing unit in order to assist people, who they believed needed assistance, to remain in the location they preferred. As a consequence, we found that more than a few families were permitted to occupy units which were doubtfully classified as "decent, safe, and sanitary."

Therefore, any program which utilizes the existing housing stock needs careful monitoring to ensure a reasonable level of compliance with housing quality standards.

In summary, the first results from EHAP suggest the following:

Cash assistance with no strings attached will not work to improve the housing conditions of those low-income families who are living in inadequate housing.

Housing allowances, on the other hand, are more than just an income transfer. They *do* induce some increased housing consumption in the short run. And given our normally high mobility rates, in the longer run, they will induce some increased housing consumption when families finally make a move. But allowance incentives do practically nothing to speed up the moving process.

Housing allowances are not a substitute for new construction or substantial rehabilitation. Because low participation rates result in very small increases in demand, allowances do not provide the necessary incentives for such important objectives as neighborhood improvement or additions to the housing stock.

Finally, housing allowances contribute modestly to the maintenance of the existing housing stock. They seem to encourage small repairs and thus, in the longer term, they may serve to deter a process of costly deterioration, which ultimately requires large dollar investments.

The early results I have just briefly noted barely scratch the surface of detailed information which will be available from EHAP. We will continue to report the findings from this program, and hope that you will feel free to ask us for whatever information from our experiments you believe will be useful to your own deliberations.

Thank you very much.

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#### TESTIMONY GIVEN ON HUD HOUSING ALLOWANCE EXPERIMENT

Research indicating that "cash assistance with *no* strings attached will not work to improve the housing conditions of those low-income families who are living in inadequate housing" was presented to a Congressional Committee today.

Donna E. Shalala, Housing and Urban Development Assistant Secretary for Policy Development and Research, testified on the Experimental Housing Allowance Program (EHAP) before the Subcommittee on Public Assistance of the Senate Committee on Finance.

"EHAP is the largest social experiment ever undertaken by the Federal government. The experiments were meant to produce useful information for policy-makers and legislators. What we are learning we believe is not only useful to our discussions of national housing policy, but also to other social policies, such as income maintenance," Ms. Shalala said.

EHAP, designed to provide information about the actions of low-income families when they are given cash payments tied to housing, consisted of three related experiments in twelve different areas of the country.

"Participation was much lower than had been anticipated," Assistant Secretary Shalala said, "and those who chose to participate came more frequently from the better-housed than from the worse-housed members of the eligible population."

In fact, Ms. Shalala pointed out, "a little less than half of those eligible initially met the necessary housing adequacy requirements."

Several of the preliminary findings counter popularly held beliefs. For example, the widely held assumption that just about all low income families not subsidized by the government live in very bad housing proved not to be the case. Findings indicate about half occupy units meeting a minimum definition of

decent, safe, and sanitary. However, the average poverty household spends 45 percent of its total disposable income on decent housing.

"It was of interest to us to discover that the vast majority of our housing program eligibles were not deriving most of their income from welfare payments. When it came to enrolling in the program, however," Ms. Shalala reported, "essentially all of the AFDC eligibles in one experimental site enrolled while less than 20 percent of those whose major source of income was earned actually enrolled."

"It appears," she continued, as if the elderly and the working poor are much more reluctant to seek government housing assistance than those already receiving most of their support from welfare payments."

The rent inflation, without housing quality improvements, expected to accompany a greatly accelerated demand for better housing, brought about by additional dollars in the hands of low-income consumers, did not take place either.

Further, forecasts that, given the opportunity, all low-income families would flock to the suburbs, did not materialize.

The first EHAP results indicate: housing allowances are more than just an income transfer in that they do cause some increase in housing consumption, even in the short run and greater increases in the long run housing allowances lack the necessary incentives for neighborhood improvement or additions to the housing stock; housing allowances contribute modestly to the maintenance of the existing housing stock.

Following is a description of the three types of experiments under EHAP.

#### DEMAND EXPERIMENT

A test which looked at what people did about their housing after receiving a cash payment for housing; what a similar group did with cash not tied to housing; and what a comparable group of families did without any cash assistance. It was conducted in two large metropolitan areas—Pittsburgh, Pa., and Phoenix, Ariz.

#### SUPPLY EXPERIMENT

A test to determine how housing markets would respond if a full-scale housing allowance program were implemented by the Government. The major focus was inflation. Would people get better housing for their additional dollars or would prices just go up for the same quality of housing? Currently, some 9,000 households are receiving allowance payments in Green Bay, Wisc., and South Bend, Ind.

#### ADMINISTRATIVE AGENCY EXPERIMENT

A test to determine what kinds of administrative problems would develop when different techniques were chosen to implement an allowance program. Eight different agencies in varied metropolitan areas—Springfield, Mass.; Durham, N.C.; Jacksonville, Fla.; Peoria, Ill.; Tulsa, Okla.; San Bernardino, Calif.; Salem, Ore.; and four rural counties in North Dakota—were chosen, each serving from 400 to 900 eligible renter families.

A SUMMARY REPORT OF CURRENT FINDINGS FROM THE EXPERIMENTAL HOUSING ALLOWANCE PROGRAM—OFFICE OF POLICY DEVELOPMENT AND RESEARCH, U.S. DEPARTMENT OF HOUSING AND URBAN DEVELOPMENT, APRIL 1978

#### FOREWORD

This report is part of an effort by the Office of Policy Development and Research to disseminate the results of its research activities. Although the final analyses of the Experimental Housing Allowance Program (EHAP) have yet to be completed, this report is made available so that those interested in housing policy issues may be kept current on what is being learned from our experience of EHAP have also been used in designing and implementing the research.

The EHAP experiments provide empirical evidence on how housing markets and low-income households respond to various forms and levels of assistance in general and housing allowances in particular. These findings by themselves help us to answer fundamental questions about low-income household behavior and housing markets that have been debated for decades. The results and the



Section 8 Housing Assistance Payments Program. Additionally, in the months ahead the results of the initial evaluation of the Section 8 Program and of EHAP will be used jointly to address a series of pressing policy issues in the Department.

DONNA E. SHALALA,  
*Assistant Secretary,*  
*Office of Policy Development and Research.*

#### PREFACE

Section 504 of the Housing and Urban Development Act of 1970 as amended by Section 804 of the 1974 Act authorized the Department of Housing and Urban Development to establish an experimental program to test the concept of housing allowances.

Pursuant to the reporting requirements of the 1970 and 1974 acts, the Department submitted the first annual report of the Experimental Housing Allowance Program (EHAP) in May 1973, a second annual report in June 1974, and a 1976 report to Congress in February 1976. In addition, a report on initial impressions and findings from EHAP was provided the Congress in April 1975.

The present report is beyond the specific reporting requirements of the 1970 and 1974 acts but is made available so that Congress and others may be kept current with what is being learned from the experiments.

#### ACKNOWLEDGEMENTS

The Experimental Housing Allowance Program (EHAP), authorized by the Congress has been conducted under the direction of HUD within the Office of Assistant Secretary for Policy Development and Research. The HUD staff responsible for it include Garland E. Allen, Robert G. Causin, Terrence L. Connelly, Evelyn Glatt, Howard M. Hammerman, Jill Khadduri, David C. Kirkman and Harold D. Williams. Most of the work has been accomplished through contracts with Abt Associates, the Urban Institute, the Rand Corporation, and eight state or local government agencies. This report was typed by Mary Anthony Trujillo, edited by Ruth Limmer, and written by Jerry J. Fitts, Director, Division of Housing Research.

#### REVIEWERS

The following people reviewed earlier drafts of this report and gave us the benefit of their informed advice.

Marc Bendick, Jr., Project Manager, EHAP, Urban Institute.

Anthony Downs, Senior Fellow, Brookings Institution.

John F. Kain, Chairman, Department of City and Regional Planning, Harvard University.

Stephen D. Kennedy, Project Director, Housing Allowance Demand Experiment, Abt Associates, Inc.

Ira S. Lowry, Manager, Design and Analysis Group, Housing Allowance Supply Experiment, Rand Corporation.

Charles E. Nelson, Program Director, Housing Allowance Supply Experiment, Rand Corporation.

Walter R. Stellwagen, Principal Reviewer, Housing Allowance Demand Experiment, Abt Associates, Inc.

Raymond J. Struyk, Deputy Assistant Secretary for Research and Demonstration, Department of Housing and Urban Development.

James E. Wallace, Director of Design and Analysis, Housing Allowance Demand Experiment, Abt Associates, Inc.

Harold M. Watts, Director, Center for Social Sciences and Professor of Economics, Columbia University.

Louis Winnick, Deputy Vice President, Ford Foundation.

#### I. EXECUTIVE SUMMARY

##### A. Introduction

The body of this report (Section III) summarizes what has been learned to date from the Experimental Housing Allowance Program (EHAP). It is intended to serve an audience that has an interest in the research findings as they relate to issues of national housing policy. Thus, the report is limited to a synopsis of major findings; there is little discussion of research methodologies or analysis to

support the findings. (These are available from the references noted throughout the report.) Because analyses of individual experiments have yet to be completed, the findings in this report are not final statements.

#### *Concept of allowances*<sup>1</sup>

The core of the housing allowance is the provision of direct cash assistance to lower-income households to enable them to obtain adequate housing. Under such a program, a household selects housing of its own choice and receives assistance payments if the unit meets the housing requirements established for the program.

#### *Policy questions*

The purpose of the EHAP was to provide answers to such questions as:

Who participates in housing allowance programs?

Does a housing allowance program cause participants to change the location of their housing?

How do participating households use their allowance payments?

Does the quality of housing improve for participating households?

Are there significant market responses to a housing allowance program?

For example, what happens to the price of housing?

#### *Research design*<sup>2</sup>

The program was designed to answer the policy questions through three separate but related experimental elements, each designed to focus on a principal cluster of issues:

*The demand experiments (Pittsburgh and Phoenix).*—These experiments primarily examine how households respond to various types and levels of assistance payments. Approximately 1,250 renter households were enrolled in each of the two sites. Some of these households were offered assistance earmarked for housing. Others were offered unconstrained assistance payments (no housing requirements had to be satisfied). In addition, for purposes of comparison with assisted households, approximately 550 similar but unassisted households at each site were also monitored.

*The supply experiments (Brown County, Wisconsin, and St. Joseph County, Indiana).*<sup>3</sup>—These experiments analyze how housing markets respond to the housing demand created by a full-scale housing allowance program. They provide enrollment open to the entire eligible population, including both renters and homeowners. The design has placed particular emphasis upon measuring changes in price and quality of housing and related services brought about by the program.

*The administrative agency experiments (eight sites, see Figure 1).*—These experiments provide information on different administrative methods for conducting a housing allowance program. From 400 to 900 renter households have participated in the program at each of eight sites. Agency operations are analyzed in order to assess the impact of alternative approaches to the several administrative functions involved in operating an allowance program.

#### *Status of experiments*<sup>4</sup>

*Demand experiments.*—The three-year experimental phase of offering assistance to households ended in 1977. This report is based on analyses of data from the first year. The remaining analyses are scheduled for completion by late 1978.

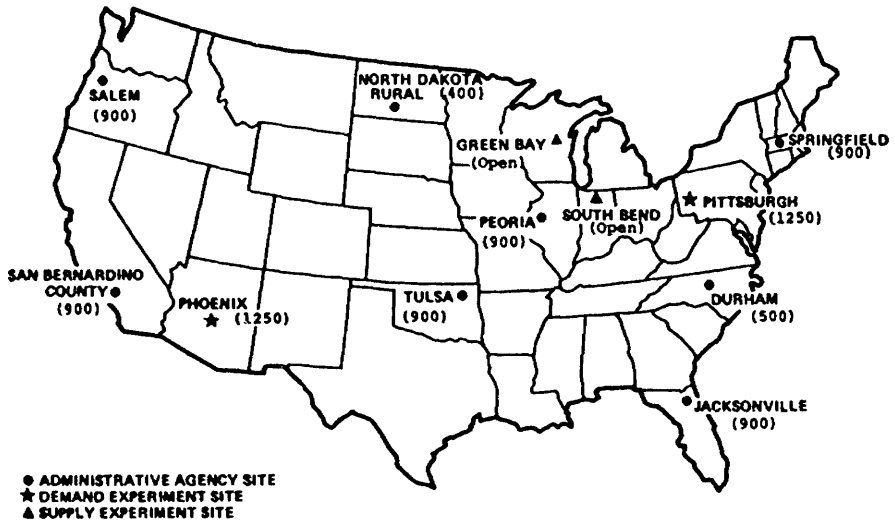
<sup>1</sup> See Appendix I for a more complete discussion.

<sup>2</sup> See Appendix I for a more complete discussion.

<sup>3</sup> The central cities of these counties are Green Bay and South Bend, respectively.

<sup>4</sup> For a more detailed summary, see Appendix II.

## EXPERIMENTAL SITES - EXPERIMENTAL HOUSING ALLOWANCE PROGRAM



Note: The numbers under each site show planned number of recipient households.

FIGURE I

*Supply experiments.*—The sites in which these experiments are operating have annual contribution contracts with HUD that will provide payments to households for ten years. The program has been operating for about three years in each site. Research data are to be collected over the first five years of operation. This report draws upon analyses using data from the first two years.

*Administrative agency experiments.*—These experiments, which operated in each site for about three years, were completed in 1976. Reports were completed in early 1977.

### Terminology

Although we have tried to eliminate technical jargon, it is necessary to establish the meaning of certain terms used throughout the report.

*Enrollee*—a household that has (1) applied for admission to the program; (2) provided such information as income, assets, rent, and household size; (3) agreed to abide by the program rules; and (4) been certified as being income-eligible.

*Recipient*—a household which receives the cash assistance provided by one of the plans offered in the experiments.

*Housing assistance*—assistance earmarked for housing. To receive such assistance, an enrollee must meet the housing requirements established by the program.<sup>5</sup> While several different types of housing assistance plans are tested in the experiments, in this report the cash assistance offered is the difference between the cost of standard housing (including utilities) and a percentage of the household's income (usually 25 percent). The cost of standard housing is established by the program<sup>6</sup> and varies by number of bedrooms according to family size and composition.

*Unconstrained assistance*—one of the experimental plans which offers cash assistance not earmarked for housing. In this plan an enrollee automatically becomes a recipient. The amount of the assistance is determined in the same way as it is in the housing assistance plan.

*Control households*—similar to households participating in both the housing and in unconstrained assistance plans but not receiving an assistance payment.

*Income eligible*—a household with a monthly income<sup>7</sup> less than four times the monthly cost of standard housing established by the program.

<sup>5</sup> Housing standards are described in Appendix III.

<sup>6</sup> The techniques used are described in Appendix I.

<sup>7</sup> Income definitions are defined in Appendix I.

### *B. Overview of Findings*

The concept of housing assistance has been debated for decades. Prior to EHAP, many of the questions raised about housing allowances could only be addressed by unproven, often conflicting theories. For example, no one could say with any degree of certainty who would participate, or how many households would move, or where they would move to, or if any of the payments would be used for housing, or if landlords would reap most of the benefits by simply increasing rents. There simply were no data relating directly to these questions.

As the debate continues, the EHAP results provide some facts to replace theories. This should help focus the debate for policy makers who will judge the usefulness of allowances as a tool of national housing policy.

#### *Participation*

Of the total eligible population, it appears that less than half of the renter and less than a third of the homeowner households become recipients in a housing allowance program. In contrast to assistance earmarked for housing, about 90 percent of the eligible households offered unconstrained assistance become recipients.

Roughly one-half of the households that become recipients already live in housing which meets the program standards. The other recipients either move to standard housing or stay in their units and upgrade them to the housing standards.

Some evidence suggests that households living in lower quality housing become recipients less frequently than others. The elderly also appear less likely to become recipients than younger households, other things being equal.

#### *Mobility*

A significant portion (usually over 25 percent) of low-income renter households move each year. However, offers of neither housing assistance nor unconstrained assistance seem to induce mobility rates or locational choices that are much different from those made by similar households not offered assistance. But, of those households that do move, those offered housing assistance move to units meeting the housing quality requirements more often than would occur otherwise. Yet on the other hand, it was particularly surprising to find that approximately one-half to a third of the enrollees that would receive assistance if they were to move to housing meeting the quality requirements did not even bother to search for another unit.

#### *Use of payments*

Although housing assistance payments to renters do induce increased expenditures for housing, the payments serve primarily to reduce out-of-pocket expenses for housing, in most cases from over 40 percent of a household's income to about 25 percent.<sup>4</sup> After subtracting out the change in expenditures that would have occurred without the program, about 29 percent of the assistance is used for increased housing expenditures (before adjusting for normal changes it was 48 percent). Households that moved made greater changes; these averaged about 40 percent of the assistance (83 percent before adjustments for normal changes).

In comparison, households receiving unconstrained assistance used only about 10 percent of their assistance (adjusted for normal changes) for increased housing expenditures, whether or not they moved.

#### *Improvements in Housing*

In this report, expenditures for housing are used as a proxy for housing quality because the analyses of housing quality by other means has yet to be completed. As might be expected, modest increases in expenditure were made by those households which, at the time of enrollment, already lived in units that met the housing requirements. Those whose units did not meet housing requirements until after enrollment increased their expenditures for housing by about 37 percent. Some of this increase would have occurred even without housing allowance payments because of general inflation and the rent adjustments that often accompany a change of units. After adjusting for this normal change, we estimate that housing assistance might induce a 19 percent increase in expenditures for housing.

<sup>4</sup> The 25 percent of income is based on income not including the assistance payment.

### *Housing market effects*

The evidence to date indicates that the increased demand for acceptable housing as a result of housing assistance payments has had no effect on rents or home prices. Nor has the program induced the construction of any new housing. While no price effects caused by the program have been detected, modest improvements in the existing housing stock are made when units enter the program and they continue to be made between the annual inspections.

### *C. Assumptions Versus Facts*

What these and other findings from EHAP mean to our understanding of housing programs and the housing problems of low-income households will be developed more fully in future reports. But until then, it is obvious that the empirical evidence summarized in this report already narrows the range of uncertainty on many critical issues and underscores the importance of predicating housing policy on facts rather than assumptions and untested theory.

*Assumption.*—Given the long list of households waiting to enter limited enrollment housing programs, many analysts expected very high participation rates. *Fact:* Not until we tried to reach eligible households (as in the Demand Experiments) or when, for the first time in U.S. history, we ran a test of a housing program open to all eligible households did we discover that about one-half (renters) to two-thirds (homeowners) of the eligible households would not participate as recipients.

*Assumption.*—Before these experiments were conducted, some observers assumed that a reformed and expanded welfare system could largely replace specific housing programs by providing low-income households with resources to purchase adequate housing. *In fact,* the experimental results indicate that although nearly 90 percent of eligible households will accept unconstrained assistance, only about 10 percent of that assistance will be used for housing. On the other hand, only about 50 percent of eligible households will receive assistance where it is earmarked for housing, but the amount used by them for housing is three to four times greater than when the assistance is not so earmarked.

The EHAP findings also highlights the costs low-income families face when they make changes in their housing. Specifically, housing assistance recipients who moved increased the amount of housing they purchased by an amount equal to 83 percent of their assistance payments. Yet, because we have the EHAP data, we know that the increase induced by the earmarked assistance amounted to only 40 percent of the payment.

*Assumption.*—Other observers expected that most eligible households would have to move to meet the housing quality standards and that a majority would do so given the incentive of cash assistance. *The facts* from the experiments indicate that about half of the recipients already live in units that meet the housing standards. Further, the incentive of assistance does not appear to induce households to move any more often than they would without the assistance. These findings carry with them important implications for all housing policies aimed at changing the housing conditions of low-income households.

*Assumption.*—A theory widely accepted before the experiments was that rents would be inflated through the increased demand for acceptable housing. *Fact:* The Supply Experiments indicate that rents are not inflated by the program. The findings from the other experiments help explain why. Let us develop this a little more fully.

About 20 percent of the total population is eligible for assistance, but less than half of the eligibles, or less than 10 percent of the total population, actually participate as recipients. Of this 10 percent about one-half already live in housing that meets the housing standards. This leaves less than 5 percent of the total population who make any significant change in their housing. And many of these make only small changes since they stay in their present housing by upgrading it to the housing standards. Thus the additional demand on the market is small. It comes from less than 5 percent of the total household population using about 40 percent of the assistance received (roughly \$40 per month) for additional housing.

In summary, it appears that the choices made by eligible households dominate the results of the experiments, and many of their choices are not much altered by offers of assistance. For example, of those households who would receive assistance if they moved to units meeting the housing requirements, about a half to one-third did not even bother to search for another unit. Even when the assist-

ance levels are doubled, the choices made by households do not change dramatically. Further, while the results of the experiments do vary with type of housing markets, it appears that the patterns of household responses are largely independent of those differences.

## II. EXPERIMENTAL APPROACH

Before examining the findings, it is essential to understand the approach used in the experiments. Since the scope of this report is focused on issues of households and market responses to a housing assistance program, only the Demand and Supply experiments are discussed. However, subsequent reports will present finding from combined analyses of comparable data from these two experiments as well as from the Administrative Agency Experiments.

The potential participants in the Demand Experiments were selected at random from a sample that was representative of the entire eligible renter population in and around the metropolitan area of each site (Pittsburgh and Phoenix). These potential participants were then randomly assigned to one of several experimental plans. This report considers two general types of assistance plans: housing and unconstrained.

Households in the housing assistance plan were offered payments large enough to bridge the gap between the cost of modest, existing housing and a reasonable fraction of their income (usually 25 percent). The cost of standard housing was established by the program and varied by number of bedrooms according to family size and composition. Households offered the housing assistance could become enrollees by agreeing to provide information on their income, assets, rent, and household characteristics. Households became recipients of the assistance only when the housing requirements established by the program were satisfied.

In the unconstrained assistance plan, eligible households were offered the payments described above but they did not have to meet any housing requirements. If they decided to enroll, they immediately became recipients of assistance.

For purposes of comparison, control households were also selected from the sample of eligibles. Although not offered any assistance, they received a \$10 monthly payment for providing monthly information and allowing their housing to be inspected.

The Demand Experiments thus established an empirical basis for assessing the responses to the different types of assistance of renter households representative of the eligible population. These responses can additionally be compared to the behavior of households that are similar, except that they are offered no assistance (control households).

The Supply Experiments were designed primarily to test market responses to a full scale program. Enrollment is open to all eligible renters and homeowners. Information about the program was spread by TV, radio, newspaper, and direct mail advertising as well as by more conventional brochures, public announcements, and speeches to community groups. By the end of the second year, surveys indicate that nearly all eligibles had heard of the program and thus had an opportunity to apply for assistance.

## III. MAJOR FINDINGS

The synopsis of current findings from the Experimental Housing Allowance Program, presented in this section, are subject to change primarily because short term effects (1 to 2 years) may differ from longer term ones. This report describes the more significant results and the experimental evidence upon which they are predicated. The discussions are intentionally brief; more comprehensive discussions of the results, research methodologies, and technical analyses can be found in the references noted throughout.

The findings under discussion are limited to five heavily debated policy questions.

Who participates in housing allowance programs?

Does a housing allowance program cause participants to change the location of their housing?

How do participating households use their payments?

Does the quality of housing improve for participating households?

Are there significant market responses to a housing allowance program? For example, what happens to the price of housing?

At this time the answers to the first question will rely on data from the Demand Experiment, supplemented by data from the Supply Experiment. The second,

third and fourth questions rely on the Demand Experiment. Data from the Supply Experiment address the market issues of the last question. Future analysis will combine comparable data across all the experimental sites.

#### A. Participation

To understand how an assistance program operates, we must deal with the issue of participation. Not until we clarify the issues of participation can we answer such questions as: What portion of the eligible population is served? What groups benefit? What are the costs of a program? How are the program funds distributed?

Participation rates of renter households are presented in Table I. Offers of housing assistance payments, averaging about \$70 a month, were made to approximately 1000 eligible households in the Demand Experiment sites of Pittsburgh and Phoenix. Of these eligible households in Pittsburgh, 82 percent became enrollees, and in the course of one year, 41 percent became recipients by satisfying the housing requirements. In Phoenix 86 percent became enrollees and 44 percent recipients. It is possible that the recipient rate could increase during the second year.

TABLE I.—ELIGIBLE RENTER HOUSEHOLD PARTICIPATION RATES

	Percent of eligible households	
	Enrollees	Recipients
Pittsburgh <sup>1</sup> .....	82	41
Phoenix <sup>1</sup> .....	86	44
Brown County, Wis. <sup>2</sup> .....	62	51
St. Joseph County, Ind. <sup>2</sup> .....	60	39

<sup>1</sup> Demand experiment draft report on participation using 1 yr data, Abt Associates, Inc., May 1977.

<sup>2</sup> Supply experiment, Housing Allowance Office management information report for Sept. 30, 1977. Excludes singles under 62 years old. Eligibility estimates are for 1974 in Brown County and 1975 in St. Joseph County.

After about three years of extensive efforts to inform the eligible population about the opportunity to participate in the Supply Experiment's housing assistance program, 51 percent of the eligible renter households have become recipients in Brown County, Wisconsin, and 39 percent in St. Joseph County, Indiana. For the first two years these rates were changing rapidly; now there are some indications that these rates are stabilizing. Additional analyses are necessary before a more certain statement can be made. We believe the enrollment rates in the Demand sites are higher than in the Supply sites because in the Demand Experiment the households were individually contacted and invited to enroll.

Homeowner participation rates are also available from the Supply Experiment. Again after about three years of program operation, 31 percent of the eligible population have enrolled in Brown County and 29 percent are recipients. In St. Joseph County 32 percent have enrolled and 28 percent are recipients.

Compare these percentages above with the households offered unconstrained assistance: in Phoenix, 92 percent of the households become recipients, in Pittsburgh 84 percent. The earmarking of assistance for housing drastically reduces the number of recipients.

#### Factors affecting participation of renters\*

Two primary characteristics distinguish recipients from enrollees who do not attain recipient status: the quality of their housing at the time of enrollment and their propensity to move. Roughly half of the recipients were living in units which met the housing requirements before they enrolled. Most of the other recipients (about one third) moved to housing that met the requirements. And about 1 out of 8 recipients upgraded the housing they occupied to the standards. It appears that households that live in lower quality housing become recipients less frequently than others.

\* The primary source of these findings is the *Fourth Annual Report of the Demand Experiment*, Abt Associates, Inc., December 1977.

With other household characteristics being the same, the probability of becoming a recipient increases with higher household income. This comes about primarily because households with higher incomes are more likely to live in units which already meet the housing requirements.

There are variations in the rates at which minorities become recipients. In some localities minorities enroll at rates higher than other groups; in other localities minorities enroll at lower rates. In general it appears that once enrolled they are less likely to become recipients, primarily because their pre-enrollment housing was less likely to meet the requirements.

Older households appear less likely to become recipients than younger households, other things being equal. In some cases this difference is traced to their pre-enrollment housing, which is less likely to meet requirements, and in others their apparent reluctance to move. Given these two factors, older households are less likely to become recipients once enrolled.

When payment levels are increased for a given income level and household size, the participation rates increase. This is as might be expected, but large increases in the payment level produce only modest increases in participation. For example, when payment levels were approximately doubled, recipient participation rates increased about 17 percentage points.

### B. Mobility

Obviously, many factors ultimately determine whether or not a household moves. But, the process of moving has two basic steps: first searching for another housing unit, then moving. Data on these two steps are presented in Table 2.

About 50 percent of the households in the control, housing, and unconstrained assistance groups in Pittsburgh searched for another housing unit. Such variations as exist between the three groups are small. In Phoenix about 62 percent of the households in each group searched, with nearly no variation between groups.

Again with some variation between the groups, in Pittsburgh about one fourth of the households actually moved; with nearly no variation in Phoenix, a little under 50 percent moved.

Table 3 separates the housing assistance group into those who met housing requirements at enrollment and those who did not. Of those who did, 49 percent in Pittsburgh and 52 percent in Phoenix searched for another housing unit. Since they were receiving payments—that is to say, since their present housing met the standards required—it would appear their search for another unit was not necessarily induced by the assistance program. Households which would receive payments if they met the housing quality requirements searched at slightly higher rates: 53 percent in Pittsburgh and 66 percent in Phoenix. Thus some enrollees may have been induced to search by the promise of payments. However, whatever inducements the payments provided were largely offset by a smaller percentage of the searchers who actually moved. The moving rate differences between those who met and those who did not meet the housing requirements at enrollment were not large: 28 versus 24 percent in Pittsburgh, and 42 versus 50 percent in Phoenix. A statistically controlled comparison yields no significant difference on the basis of these first year data.

TABLE 2.—1ST-YR SEARCH AND MOVING RATES

Type of household	Percentage searching	Percentage moving
<b>Pittsburgh:</b>		
Control.....	50	23
Housing assistance.....	52	25
Unconstrained assistance.....	45	29
<b>Phoenix:</b>		
Control.....	63	47
Housing assistance.....	62	48
Unconstrained assistance.....	62	47

Source: Demand experiment report, "Locational Choice, Part 1, Search and Mobility," P-A-131, Abt Associates, Inc., August 1977.



TABLE 3.—1ST-YR SEARCH AND MOVING RATES—HOUSING ASSISTANCE HOUSEHOLDS

Household type	Percentage searching	Percentage moving
Pittsburgh:		
Met housing requirements at enrollment	49	28
Did not meet housing requirements at enrollment	53	24
Phoenix:		
Met housing requirements at enrollment	52	42
Did not meet housing requirements at enrollment	66	50

Source: Demand experiment report, "Locational Choice, Part 1, Search and Mobility," P-A-132, Abt Associates, Inc., August 1977.

It would seem that households with the most incentive to move are those that would receive payments if they moved to housing meeting the housing requirements. Yet in Pittsburgh only 24 percent of those with the most incentive actually did move. That percentage can only be understood in relation to the control group: 23 percent of them moved too. The same comparison in Phoenix shows 50 percent versus 47 percent. In both sites the incentive of payments did not cause households to move significantly more often than control households.

There is another surprising finding in Table 3. Of those who would receive payments if they moved, 47 percent in Pittsburgh, and 34 percent in Phoenix, did not even search for another place to live.

So far we have found that housing assistance and control households move at about the same rates. But how do the neighborhoods they move to compare? Again, their behavior appears similar.<sup>10</sup> In both cases they moved to neighborhoods with reduced concentrations of low-income households and which rank more favorably in subjective assessment of less crime, less litter, more public services, better access to public transportation, etc. And when they moved, their choice of neighborhoods (according to racial mixture or whether inner city or suburb) was similar.

#### C. USE OF PAYMENTS

To answer the question of how assistance payments are used, we depend on data obtained from the control, unconstrained, and housing assisted household groups of the Demand Experiment. The findings are expressed in terms of increases in expenditures used for housing induced by the housing assistance payments. Since housing expenditures tend to increase over time, with or without assistance, data from the control households are crucial to these findings. By using them to "adjust" the expenditure increases of the assisted households, we can determine what changes of expenditure are the result of the program itself.

Using rents as the measure of change, we can say that housing assistance payments do cause renter households to spend more for housing. But the payments in the first year primarily served to reduce out-of-pocket expenses for housing, in most cases from over 40 percent of a household's income to about 25 percent.

Households that already met the housing requirements at enrollment generally showed a smaller program-induced increase in expenditure than households that did not meet the requirements until after enrollment. For both types of households combined, the program-induced increases averaged about 29 percent of the housing assistance payments. For the households receiving unconstrained assistance, the program-induced increases were only 10 percent of the assistance payment, a third of that of the housing assistance households. Figure 6 illustrates these changes for the Pittsburgh and Phoenix households.

To the extent that all households eventually move, households that moved during their first year in the experiment are particularly interesting because they may foreshadow the eventual response of other households. As we see in Figure 7, households who moved generally spent about 40 percent of their housing assistance payments on program-induced housing expenditures. Unconstrained households used only 10 percent of their assistance payments for program-induced increases.

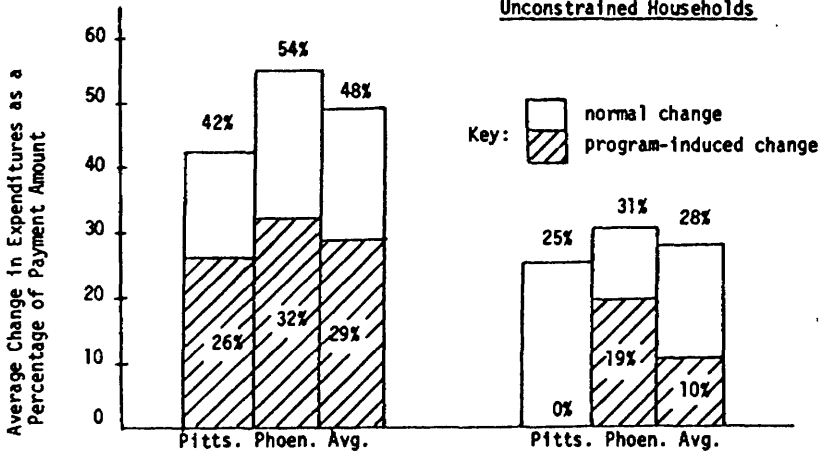
<sup>10</sup> See Demand Experiment Report, *Locational Choice, Part 2, Neighborhood Change*, Abt Associates, Inc., August 1977.

FIGURE 6

CHANGE IN HOUSING EXPENDITURES AS A  
PERCENTAGE OF PAYMENT

Housing Assistance Households

Unconstrained Households

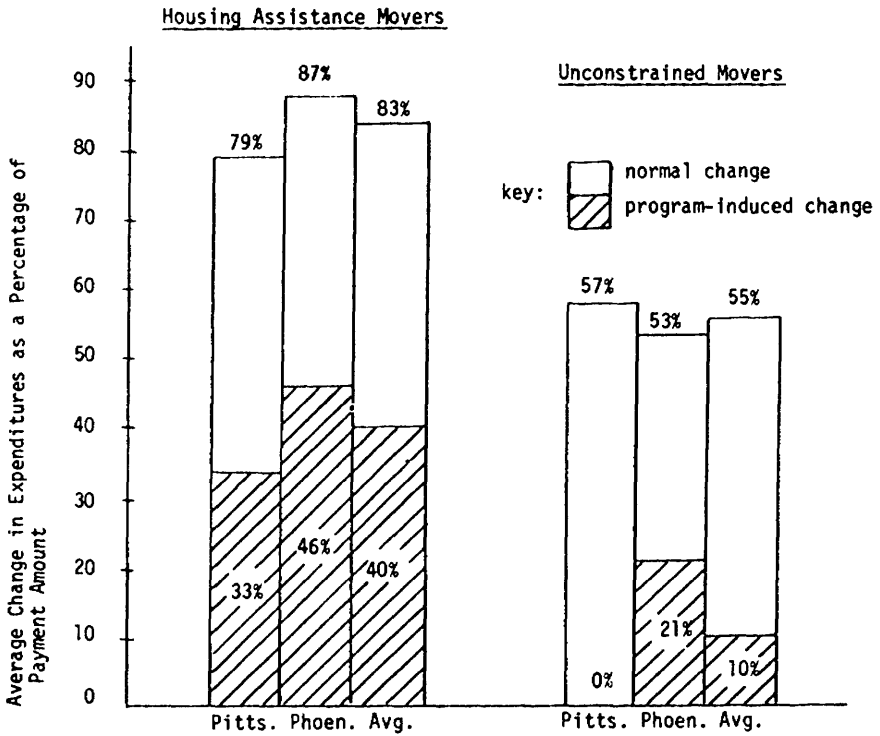


Number of Households:	238	210	69	46
Average Monthly Payment:	\$50	\$80	\$50	\$77

Source: Report, Preliminary Findings from the Housing Allowance Demand Experiment, Abt Associates, Inc., March 1978

**FIGURE 7**

**CHANGE IN HOUSING EXPENDITURE AS A PERCENTAGE  
OF PAYMENT FOR MOVERS**



Number of Households:	74	121	19	25
Average Monthly Payment:	\$51	\$92	\$58	\$83

Source: Report, Preliminary Findings from The Housing Allowance Demand Experiment, Abt Associates, Inc., March 1978

## D. IMPROVEMENTS IN HOUSING

To determine the improvements in housing experienced by households, we require some measure of housing quality. A house or apartment is a complex bundle of attributes, including those of the unit itself, its neighborhood, and the quality of public and private services. An approach has been developed to measuring housing quality,<sup>11</sup> but we have yet to complete our analyses of data from the experiments employing this measure.

Therefore, in this report rent expenditure is used as proxy for housing quality. The findings are based on the Demand Experiments' data from the first year. Experiences of the control households are used to adjust the data for those changes that would have occurred without the program.

As might be expected, modest increases in rent expenditures were made by those households which, at the time of enrollment, already lived in units that met the housing requirements. Those whose units did not meet housing requirements until after enrollment increased their rental expenditures by about 37 percent. Some of this increase would have occurred even without the program because of general inflation and the rent adjustments that often accompany a change of units. After adjusting for this normal change, we estimate that the program induces a 19 percent increase in rent expenditure. These are the combined results from the two sites. Figure 8 displays the data for each.

## E. HOUSING MARKET EFFECTS

The Supply Experiments, which were primarily designed to address the issues of market responses, involve a ten-year-long program open to all eligible renters and homeowner households in each of two metropolitan areas, chosen for strong contrasts in their housing markets, Brown County, Wisconsin (whose central city is Green Bay), and St. Joseph County, Indiana (whose central city is South Bend). The sites were selected from among all metropolitan areas whose populations in 1970 were under 250,000, the size limit reflecting resource constraints. In the one case, Brown County is a "tight" housing market undivided by racial segregation; in the other case, St. Joseph County is a "loose" housing market with a segregated minority population.

As of the end of September 1977 the program had been operating for about 39 months in Brown County and 30 months in St. Joseph County. There were 3,148 households receiving payments in Brown County (about 7% of all households) and 4,913 in St. Joseph County (about 8% of all households). Payments averaged about \$900 per year for each household.

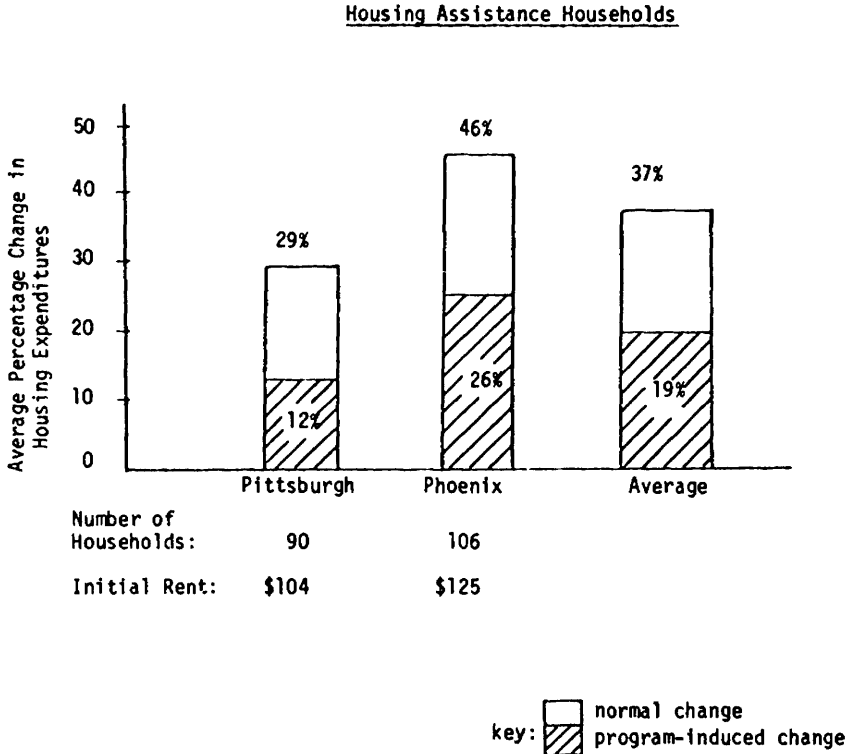
The response of a market to the increased demand for housing created by an allowance program might show up in the form of higher prices or increased housing production, or both. The evidence gathered from both sites indicates that as of now, the additional demand for acceptable housing has had virtually no effect on either rents or home prices. Nor is there evidence that the program has been responsible for any new construction.

In fact, in both sites rents have increased somewhat less than the national or regional averages. Moreover, virtually the entire increase is attributable to higher fuel and utility bills.

<sup>11</sup> Demand Experiment Draft Report, *Hedonic Indices as a Measure of Housing Quality*, Abt Associates Inc., December 1977.

FIGURE 8

PERCENTAGE CHANGE IN HOUSING EXPENDITURES FOR HOUSEHOLDS  
THAT MEET REQUIREMENTS AFTER ENROLLMENT



Source: Report, Preliminary Findings from The Housing Allowance Demand Experiment, Abt Associates, Inc., March 1978

The Supply Experiment's finding that the increased demand for housing created by assistance payments has had no effect on prices is consistent with the other EHAP results. As described in Section I, the additional demand on the housing market is small. It comes from less than 5 percent of the total household population using about 40 percent of the assistance received (roughly \$40 per month) for additional housing.

Although the program has not caused any price or quantity effects in the housing markets, it has been directly responsible for modest improvements to the recipient's home, whether rented or owned. Through September 1977, over 2,406 units in Brown County and 4,000 units in St. Joseph County were repaired by or at the request of enrollees seeking to qualify for payments. Another 900 and 1,200 units, respectively, were repaired following annual reinspections of the

recipient's housing. Because many of the defects—especially the health and tenants and their friends provided most of the labor, cash outlays were usually modest.

#### IV. VALIDITY OF RESULTS

The findings in this report are not final. Analyses of the individual experiments as well as analyses of comparable data across experiments have yet to be completed.

Further, we wish to note that some observers<sup>12 13</sup> have asserted that EHAP will not provide evidence conclusive enough to answer the principal research questions. They make that judgment because the sites selected for the experiments were, they thought, too few in number and lack the characteristics typical of urban areas. Therefore, they conclude that the findings will not permit a projection of the results to a national program.

The planners of EHAP considered such issues very early in the design of the experiments. Their conscious decisions about the choice and number of sites were made within a framework requiring judgments between program costs and the desire for reasonable results.

Let us examine the framework a little more fully. On the one hand, the number of sites could have been doubled. Had that been done, the costs of the program would have increased by about the same rate. Instead of about \$180 million, the costs would have risen to about \$360 million. Even the addition of one Supply Experiment site with a population of a million could have doubled the cost of the experiment. But even doubling the number of sites would not remove the charge that EHAP lacks a large enough number of sites to make statistically rigorous inferences above all parts of the nation. Given the nature of social science research, if a statistically rigorous set of sites could be defined, it would cost many billions of dollars to conduct the experiments in them. What EHAP does instead is to provide empirical facts from which reasonable projects can be made. That is considerably more useful than doing with EHAP entirely and going back to relying on theory.

There is also some criticism of the choice of housing markets; it has been said<sup>12 13</sup> that they are not statistically representative. In the process of selecting sites, we concluded that it was unrealistic, if not impossible, to obtain sites that would rigorously represent all urbanized areas of the country. Thus, sites were picked that offered contrasting characteristics (as discussed earlier for the Supply Experiment). This approach assures that the effects of these characteristics are captured in the results of the experiments. Although contrast was a criterion, the sites selected are not atypical. They are generally "representative" of the 248 urbanized areas of the country as measured by such characteristics as vacancy rates, racial composition, cost of housing, growth rates, age of housing stock, mobility rates, quality of housing stock, etc.<sup>14</sup>

Although the results of the experiments do vary with types of housing markets, it appears that they are determined more by the pattern of household behavior than by differences in the housing markets themselves. In other words, the normal choices that households make appear to dominate the results of the experiments. This in itself is a major finding and suggests that the lack of statistically representative sites is not likely to prohibit application to the vast majority of cities.

In its simplest form the issue is a choice between empirical evidence with some uncertainty and theory with nothing but uncertainty. Prior to EHAP, the questions raised about housing allowances could only be addressed by unproven, often conflicting theories. For example, no one could say with *any* degree of certainty who could participate, or how many households would move, or where they would move to, or if any of the assistance would be used for housing, or if landlords would reap most of the benefits by increasing rents. There simply were no data.

As the debates on the concept of housing allowances continue, decade after decade, we now have facts to answer the questions—empirical facts. The facts come from EHAP.

<sup>12</sup> Report to the Congress, *An Assessment of the Department of Housing and Urban Development's Experimental Housing Allowance Program*, by the Comptroller General of the United States, March 8, 1978.

<sup>13</sup> Report to the Congress, *Observations on Housing Allowances and the Experimental Housing Allowance Program*, by the Comptroller General of the United States, March 28, 1974.

<sup>14</sup> Report, *Generalizing from the Experimental Housing Allowance Program: An Assessment of Site Characteristics*, The Urban Institute, February 1978.

## APPENDIX I

## BACKGROUND AND DESCRIPTION OF THE EXPERIMENTAL HOUSING ALLOWANCE PROGRAM

## BACKGROUND

Housing allowances or "rent certificates" are not new concepts. They have played a role in discussions of housing policies and programs since the debates prior to the passage of the Housing Act of 1937. The Taft Subcommittee hearings on postwar housing policy in 1944 and the long discussions leading to adoption of the Housing Act of 1949 all involved position papers and testimony for and against rent certificates. In 1953, the President's Advisory Committee on Government Housing Policies and Programs also discussed the concept at some length. The Committee concluded that rent certificates would be degrading to recipients, that they would not "add to the housing supply," that they would deter participation by private enterprise, that proper administration of the program would be organizationally complex and that there would be no feasible way to limit the scale of such a program.

A shift in housing policy in the direction of housing allowances came in the Housing and Urban Development Act of 1964 when two new housing programs came into existence. The first was the rent supplement program which limited its subsidies to newly constructed or substantially rehabilitated housing but established the principle of income-related subsidies to residents of *privately* owned housing units. The amount of these subsidies varied according to household need.

Rent supplements offered recipients flexibility not permitted by conventional public housing. Households could occupy their housing units at market rents and would continue to receive assistance until their income increased to the point where they were no longer eligible. But the payments were made to the *owners* of eligible housing developments; households benefited only when they resided in such developments.

The second program added in 1965 was the Section 23 leased housing program—a program much closer in design to a housing allowance. It enabled local housing authorities to lease modest but adequate privately owned dwellings and then to sublease them to low-income households. The government paid the difference between the full cost of leasing the private unit and the amount (determined by a formula) the family could afford. The Section 23 leasing program had the advantage of using existing housing units scattered throughout a range of neighborhoods.

In the Section 23 programs, recipients were not necessarily tightly clustered geographically. The local housing authority almost always located and selected the housing, and negotiated rents and lease provisions with the landlord. A household did not receive its subsidy directly and could not automatically transfer the subsidy when it decided to move a new housing unit. Furthermore, under the Section 23 program, a family could only receive a subsidy in a local jurisdiction which approved the use of the program.

In 1967 and 1968, the President's Committee on Urban Housing, generally known as the Kaiser Committee, devoted extensive attention in its report to the housing allowance approach. The Committee did not propose immediate adoption of housing allowances, but it did recommend prompt initiation of an experiment to test allowances.

*Initial research on housing allowances.*—In 1969 and 1970 preliminary estimates of the costs of a national program were made. These estimates indicated that the subsidy cost per household through an allowance approach would be significantly lower than the average subsidy cost per unit under other federal housing programs. An analysis dealing with the rent response that would be brought about by an allowance program pointed to the need for more extensive modeling and analysis of market effects and implied the need for a more rigorous direct test of the housing allowance concept. Analysis during this period suggested that in the long run the response to a housing allowance would involve a substantial increase in the quantity of housing (75, 76, 77).

*Kansas City and Wilmington demonstrations.*—At the same time, the Kaiser Committee recommendation was translated into action under HUD's Model Cities Program. The local Model Cities agencies of two cities—Kansas City, Missouri, and Wilmington, Delaware—began demonstration programs in late 1970 designed to use housing allowances as a means of providing decent housing. Both demon-

strations were evaluated and gave some insights into the effects of housing allowances (81).

*Conceptual design of an experimental program.*—Upon passage of the 1970 Housing Act, an experimental program focusing on key policy questions about housing allowances was considered, and a detailed conceptual design was developed to systematically test the effects of different forms of a housing allowance on household behavior (79). This thinking evolved into what is now called the Demand Experiment.

In late 1971, a conceptual design to the market effects of an allowance program—the Supply Experiment—was begun. (78) Extensive efforts were also made to develop a model of urban housing markets which could predict the outcomes of housing allowances and alternative public policies. (82) Finally, in order to gain realistic experience about the administration of an allowance program by various governmental agencies, what is now called the Administrative Agency Experiment was considered.

By the spring of 1972, three separate but interrelated experiments had been planned. The combined effort was called the Experimental Housing Allowance Program (EHAP).

#### PROGRAM DESIGN FOR EHAP

Having made the decision to conduct three distinct experiments linked together by a common program design, the actual design elements for housing allowances in each of the experiments had to be chosen. Two considerations were central in designing the experimental allowance programs: (1) the need for an *integrated* design that would allow consistent policy analysis using data from all three experiments, and (2) legal restrictions on the use of federal funds under which EHAP would be operating. Of particular relevance here was the decision that program operating funds for the Administrative Agency and Supply Experiments would come from the Section 23 leased housing program.

Table I-1 gives a breakdown of key design elements in each of the three experiments. To facilitate the comparison, the table uses the "design center" of the Demand Experiment—in which payments and program requirement, are most like the program being employed at Supply and Administrative Agency Experiment sites. In the discussion below, however, we will also indicate other program elements being tested in the Demand Experiment.

Both the Demand and Supply Experiments were designed with the same *number of sites*—two. The Demand Experiment operated in Allegheny County (Pittsburgh), Pennsylvania, and Maricopa County (Phoenix), Arizona. The Supply Experiment is operating in Brown County (Green Bay), Wisconsin, and St. Joseph County (South Bend), Indiana. The Administrative Agency Experiment, however, involved a total of eight sites.<sup>1</sup>

*The administrative mechanism* used by each experiment also differs. In the Demand Experiment, a research organization—Abt Associates, Incorporated—operated the program. In the Supply Experiment, a non-profit Housing Allowance Office, established and controlled by the research contractor, the Rand Corporation, is employed. Because the purpose of the Administrative Agency Experiment was to assess various approaches to the administration of a housing allowance, eight public agencies were chosen to operate the program in the selected sites: the Housing Authority of Salem, Oregon; the Department of Community Affairs, Commonwealth of Massachusetts; Illinois Department of Local Government Affairs, Office of Housing and Buildings; the San Bernardino County Board of Supervisors; the Social Services Board of North Dakota; the Jacksonville Department of Housing and Urban Development; the Durham County Department of Social Services; and the Tulsa, Oklahoma, Housing Authority.

*The scale of the program* was set to meet the particular research needs of each experiment. In the Demand Experiment, the number of households under all of the 17 plans being tested was set at about 1,250 in each site. In the Administrative Agency Experiment, the number of households was designed to vary from 400 to 900 at each of the eight sites. The Supply Experiment, because it was designed to test the market response to a full-scale program, is open to all eligible households.

<sup>1</sup> In most of the EHAP sites, the precise program area served includes both the central city and surrounding suburban jurisdictions. At some sites, portions of rural areas are included.



TABLE I-1.—KEY PROGRAM DESIGN ELEMENTS IN THE EXPERIMENTAL HOUSING ALLOWANCE PROGRAM

Design elements	Demand experiment	Supply experiment	Administrative agency experiment
Number of sites	2	2	8
Administrative mechanism	Abt Associates, Inc. site office staff.	Housing Allowance Office established by Rand Corporation.	8 public agencies: 2 each of types.
Scale of program	1,250 households at each site.	Open enrollment	400-900 households at each site.
Payment formula	Center of design: Housing gap ( $P=C^*-bY$ ) Other variations tested.	Housing gap ( $P=C^*-bY$ )	Housing gap ( $P=C^*-bY$ ).
Definition of household unit	Households of 2 or more related individuals; elderly, disabled or handicapped single persons.	Households of 2 or more related individuals; single persons.	Households of 2 or more related individuals; elderly, disabled or handicapped single persons.
Tenure eligibility	Renters	Homeowners and renters	Renters.
Technique for estimating rent for adequate housing ( $C^*$ )	Center of design: Panel of experts (percent variations of this estimate also tested).	Rent survey and panel of experts.	Panel of experts.
Household contribution rate ( $b$ )	Center of design: $b=0.25$ ; other variations tested.	$b=0.25$	$b=0.25$ .
Income definition	Gross income minus Federal, State, and Social Security taxes; less \$300 annually per earner for work-related expenses; and other specific deductions.	Gross income minus \$300 exemption per dependent and each secondary wage earner; 5 percent standard deduction (10 percent for elderly); and other specific deductions.	Gross income minus \$300 exemption per dependent and each secondary wage earner; 5 percent standard deduction (10 percent for elderly); and other specific deductions.
Rent definition	Either gross rent or contract rent plus formula-based allowance for utilities which are paid by household.	Either gross rent or contract rent plus formula-based allowance for utilities which are paid by household.	Either gross rent or contract rent plus formula-based allowance for utilities which are paid by household.
Housing requirements (form of earmarking)	Center of design: Minimum standards; other variations tested.	Minimum standards	Minimum standards.
Nonmonetary assistance.	Housing information and equal opportunity support.	Housing information and equal opportunity support.	Housing information and equal opportunity support (varied by agency).

In designing EHAP, two general methods were identified for establishing a *payment formula* for determining the amount of a housing allowance to be paid to a particular household. One method is called the "housing gap formula" and the other is the "percentage of rent" formula.

The housing gap formula bases the amount of an allowance to be paid on the size and income of the household, and on local housing market conditions. The formula is calculated so that the household is offered an allowance equal to the difference between the market rent for an adequate rental unit of the appropriate size and a percentage of the household's program-defined income.

The allowance payment for a housing gap formula is calculated as follows:

$$P = C^* - bY,$$

where:  $P$  = Allowance payment.

$C^*$  = Estimate of market rent of adequate housing.

$b$  = The rate at which the allowance is reduced as income increases (usually 25% in EHAP).

$Y$  = Program-defined income.

The percentage of rent formula calculates the allowance amount as a fraction of the rent paid by an eligible household. An upper limit on rent against which the formula would apply may be specified. More complicated versions of this formula might change the fraction of the rent paid by household size, by income, and by the amount spent on rent.

A simple percentage of rent payment works this way:

$$P = aR \text{ for } R < \frac{C^*}{a}, \quad P = C^* \text{ for } R > \frac{C^*}{a},$$

where:  $P$  = Allowance payment;

$R$  = Rent paid by household;

$a$  = Percentage of rent paid by government;

$C^*$  = Maximum payment allowed.

As indicated in Table I-1, the payment formula used in the Supply and Administrative Agency Experiments is the housing gap formula; in the Demand Experiment, the housing gap formula is also used for a variety of treatments, including the center of the design. In addition, several variations of a simple percentage of rent formula are being tested in the Demand Experiment.

Having considered the formula by which payments were to be calculated, decisions were required on several key definitions and parameter values. First, the *household unit definition* established which households were eligible for the program. In EHAP, essentially the same definition was used in all three experiments. Households were eligible which were composed of two or more related individuals; in addition, households composed of single persons were eligible if the individual was over 62 years of age, disabled, or handicapped. This is essentially the definition of household used in the Section 23 leased housing program. In 1977, the Supply Experiment began enrolling non-elderly singles who were not disabled or handicapped.

Eligibility was also restricted by *tenure* in the case of the Demand and Administrative Agency Experiments. Only renters were eligible in those two experiments. Both renters and homeowners could apply for allowances in the two Supply Experiment sites.

The three parameters in the housing gap formula— $C^*$ ,  $b$ , and  $Y$ , also require operational meaning in order to establish the precise payment levels to go to eligible households.  $C^*$  estimation techniques vary slightly across the three EHAP experiments. The cost of adequate housing is estimated by the number of bedrooms, using the "panel of experts" approach in the Demand and Administrative Agency Experiments. Under this method, "modest neighborhoods" are selected and local realtors, government housing officials, and others with expert knowledge of the local housing market are asked their estimates of market rents given the number of bedrooms in standard housing in each neighborhood. Their responses were used to determine distributions of rent levels. HUD then selected a  $C^*$  value for each housing unit according to the number of bedrooms on the basis of the distributions. Finally, households of different sizes are assumed to require housing units with different numbers of bedrooms.

For the Supply Experiment, a rent survey was conducted as part of an initial screening survey of the local housing market in both sites. It was used as a principal source of information in the determination of  $C^*$ . In an effort to check the consistency of the rent survey approach with the  $C^*$  estimated elsewhere, the "panel of experts" technique was used at the first Supply site, Brown County, Wisconsin. The results of the two approaches were broadly consistent.

In the Demand Experiment, some allowance plans involved testing the use of higher and lower levels of  $C^*$  than the ones estimated by the estimation technique discussed above.

With respect to establishing  $b$ , the "household contribution rate," analyses were carried out on rent-income ratios, based on: (1) 1960 and 1970 Census data for households in the income range judged able to consume adequate housing without subsidy (approximately \$6,000-\$9,000), (2) an adjustment of rent-income ratios based on gross Census income to a roughly equivalent ratio based on the net income definition of EHAP, (3) and an evaluation of the potential cost of national programs at different values of  $b$ . Based on this work,  $b$  was set at 0.25 for all household sizes in the Administrative Agency Experiment and the "design center" of the Demand Experiment. The use of  $b=0.25$  is also a design element in the Supply Experiment. Higher and lower values of  $b=0.15$  and 0.35 were tested within the Demand Experiment.

The housing gap formula also required an *income* definition. This definition varied across experiments, chiefly as a result of legal restrictions which are tied to the way the Administrative Agency and Supply Experiments were funded. The definition in the Demand Experiment is free of such restrictions and basically involves deducting federal and state income taxes and Social security taxes from gross income, as well as subtracting \$300 per year for work-related expenses of full-time earners within the household. Child-care expenses, extraordinary medical expenses, alimony, and support payments are also deducted.

The definition of income used in the Administrative Agency Experiment and Supply Experiment differs from the Demand Experiment mostly in terms of deductions. This income definition was essentially imposed on these two experiments because of the reliance on Section 23 program funds. The definition used in these two experiments included an exemption of \$300 for each dependent as well as a \$300 exemption for each secondary wage earner. In addition, there was

a 5 percent standard deduction (10 percent for elderly households). Deductions for child-care, extraordinary medical expenses, and alimony were also provided.

The *rent definition* is important because, first, estimating the cost of adequate housing requires agreement on what constitutes rent, and second, since in all three experiments the allowance payment is not permitted to exceed rent, there must be a standard definition used to calculate what rent is.

Across the three experiments in EHAP, rent is defined in a very similar fashion as gross rent, which equals the contract rent plus an additional formula-based allowance for extra costs of utilities paid by the recipients.

A housing allowance is different from unrestricted cash assistance because of the housing-related requirements attached to the receipt of the subsidy. That is to say, a housing allowance is earmarked for housing. There are two methods of earmarking—by minimum standards and by minimum rents.

*Minimum standards.*—When minimum standards earmarking is applied to a household, that household receives an allowance payment only if it rents a housing unit which meets minimum housing standards. Such standards may be based on locally defined codes or on national codes. The requirements can be enforced either through certification by the allowance recipient or the landlord, through inspection by an authorized agency, or through reliance upon the findings of an effective housing code enforcement program.

*Minimum rent.*—Under minimum rent earmarking, a household receives a payment only if it spent at least a specified minimum amount for housing. This approach assumes that there is a close correspondence between rent and housing quality.

Both the Supply and Administrative Agency Experiments employed the minimum standards requirement. Minimum standards are also being tested at the center of the design in the Demand Experiment. In addition, minimum rent earmarking is being tested in other treatments in the Demand Experiment. (See Appendix II for Standard I used in the Demand Experiment.)

It is not clear that monetary assistance alone will assure that a large number of households obtain decent housing at a reasonable cost to the government. For many households, income may be the only obstacle to the attainment of decent housing; however, past experience indicates that for some households money is not enough. The major types of *non-monetary* assistance provided are:

*Housing market information* is given to households to aid them in house assessment and selection in terms of structural adequacy, maintenance, financial soundness, and landlord-tenant relations.

*Equal opportunity information and legal assistance* are made available to households in order to assist them in combating discrimination in the housing market.

The manner in which these and other services were provided and the effect of various services on participant outcomes is being analyzed in EHAP. The AAE in particular was designed for this kind of analysis.

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## APPENDIX II

### SUMMARY OF EHAP OPERATIONS

#### CURRENT STATUS OF EHAP OPERATIONS

As of January 1978, over 23,000 households had received at least one housing allowance payment since enrollment in EHAP began in March 1973. Enrollment is still in process only in the Supply Experiment; the Administrative Agency and Demand Experiments have both finished the experimental phase in which data were gathered on participating households. About 8600 families were receiving housing allowances in January 1978.

*Administrative agency experiment (AAE).*—A seven-month enrollment period was used at each of the AAE sites; initial enrollment was completed at the last site in May 1974. Only in Jacksonville was the number of participants significantly lower than anticipated. The enrollment period was reopened there to determine whether changes in agency operations could achieve different results. Through its second enrollment period, completed in July 1975, the agency was able to obtain the number of participants to reach its target.

The AAE was designed to provide two years of allowance payments to families in its experimental phase. The families receiving housing allowances in the experiment received an additional commitment from HUD of assistance under other

subsidized housing programs, primarily Section 23 leased housing. This commitment is for three years after the experimental phase ends and is conditional on family eligibility for these programs. The eight states and local agencies involved in the experiment have completed transition of their responsibilities to local agencies which will administer programs for the recipient families during the three-year follow-on period.

Over all, about 6,400 households have participated in the AAE. At the time the experiment was fully operating—before transition began—the average annual adjusted income of participating households was slightly under \$3,000, and their average monthly housing allowance payment was about \$80. (See Table II-1).

*The demand experiment.*—Enrollment in the Demand Experiment lasted for a ten-month period, beginning in April 1973 and concluding in February 1974. This experiment provided three years of experimental payments. Families who wished to continue on other HUD assistance programs after the experimental period was over were helped in doing so if eligible.

Table II-2 shows that there was almost 2500 households initially enrolled, and about half were receiving allowance payments after two years. The average payment was \$69 monthly.

*The supply experiment.*—In the Supply Experiment, open enrollment of households began in June 1974 in Brown County and in April 1975 in St. Joseph County. The enrollment period is scheduled to continue over the five year-period of the program. Eligible families may participate throughout the ten-year commitment HUD has made to each of the communities. This longer period was necessary in the Supply Experiment to see whether housing suppliers would make capital improvements and other long-term investments.

The status of operations of the Supply Experiment is shown in Table II-3. As of January 1978, over 8600 households were receiving housing allowances. Slightly more than half were homeowners. The average annual income of recipient renters was lower than that of recipient homeowners in both sites; their monthly allowance payments, in turn, were higher.

TABLE II-1.—STATUS OF THE ADMINISTRATIVE AGENCY EXPERIMENT AFTER THE FIRST YEAR OF OPERATION

Site	Operating time period	Recipient households after 1st year of operation <sup>1</sup>		
		Number	Average adjusted income <sup>2</sup>	Average monthly payment
Salem, Oreg.	March 1973 to January 1976	870	\$2, 800	\$84
Springfield, Mass.	April 1973 to February 1976	861	3, 000	89
Peoria, Ill.	April 1973 to February 1976	835	3, 700	85
San Bernardino, Calif.	March 1973 to March 1976	776	2, 900	84
Bismarck, N. Dak.	July 1973 to April 1976	367	3, 000	72
Jacksonville, Fla.	April 1973 to July 1977: <sup>3</sup>			
	1st enrollment	300	2, 000	86
	2nd enrollment	541	3, 200	74
Durham, N.C.	July 1973 to April 1976	483	2, 400	74
Tulsa, Okla.	August 1973 to May 1976	825	2, 700	72

<sup>1</sup> This represents steady-state operations—when the experiment was fully operating and before households were phased into other housing programs.

<sup>2</sup> Gross annual income minus deductions for dependents, medical expenses, etc.

<sup>3</sup> The operating period in Jacksonville is longer than at other locations because enrollment was reopened.

TABLE II-2.—STATUS OF THE DEMAND EXPERIMENT AS OF 2 YEARS AFTER ENROLLMENT

Site	Operating time period	Enrollments	Recipient households		
			Number <sup>1</sup>	Average adjusted income <sup>2</sup>	Average monthly payment
Pittsburgh, Pa.	April 1973 to February 1977	1, 211	736	\$5, 000	\$61
Phoenix, Ariz.	May 1973 to February 1977	1, 255	569	5, 100	78

<sup>1</sup> In addition, there were 39 households in Pittsburgh and 62 households in Phoenix who were on a temporary inactive status as of 2 years after enrollment. There were also 183 enrolled households in Pittsburgh and 150 households in Phoenix who were not meeting requirements which would enable them to receive payments; and 96 in Pittsburgh and 178 in Phoenix living in their homes or in subsidized housing and hence ineligible for allowance payments.

<sup>2</sup> Gross annual income minus Federal and State income taxes, social security taxes, an allowance for work-related expenses, medical expenses, etc.

TABLE II-3.—STATUS OF THE SUPPLY EXPERIMENT AS OF JANUARY 1978

Housing tenure by site and operating time period <sup>1</sup>	Recipient households		
	Number	Average adjusted income <sup>2</sup>	Average monthly payment
<b>Brown County, Wis.—June 1974 to June 1984:</b>			
Renters.....	2, 100	\$4, 600	\$75
Homeowners.....	1, 200	5, 300	64
Total.....	3, 300	4, 900	71
<b>St. Joseph County, Ind.—December 1974 to December 1984:</b>			
Renters.....	2, 100	3, 400	91
Homeowners.....	3, 200	4, 600	63
Total.....	4, 300	4, 100	74

<sup>1</sup> The time period shown includes the 5-yr experimental period and a 5-yr additional commitment of allowance payments to eligible participating families.

<sup>2</sup> Gross annual income minus deductions for dependents, medical expenses, etc.

### APPENDIX III

#### HOUSING STANDARDS

The following program standards were developed for analytical use across the EHAP experiments. The standards outlined below were used to qualify dwelling units as meeting program standards in the Demand Experiment. The eight agencies in the Administrative Agency experiments were allowed to define their own minimum housing standards to qualify units; however, independent evaluations using the program standards were performed on a sample of dwelling units at each site. The Supply Experiment used similar, but somewhat different, standards to qualify dwelling units to meet the minimum requirements set for the project.

1. *Complete plumbing.*—Private toilet facilities, a shower or tub with hot and cold running water, and a washbasin with hot and cold running water will be present and in working condition.

2. *Complete kitchen facilities.*—A cooking stove or range, refrigerator, and kitchen sink with hot and cold running water will be present and in working condition.

3. *Living room, bathroom, kitchen presence.*—A living room, bathroom, and kitchen will be present. (This represents the dwelling unit "core", which corresponds to an efficiency unit.)

4. *Light fixtures.*—A ceiling or wall-type fixture will be present and working in the bathroom and kitchen.

5. *Electrical.*—At least one electric outlet will be present and operable in the living room and kitchen. A working wall switch, pull-chain-light switch or additional electrical outlet will be present in the living room.

6. *Heating equipment.*—Units with no heating equipment; with unvented room heaters which burn gas, oil, or kerosene; or which are heated mainly with portable electric room heaters will be unacceptable.

7. *Adequate exits.*—There will be at least two exits from the dwelling unit leading to safe and open space at ground level. Exceptions will be allowed on a case-by-case basis when it appears that fire safety is met despite lack of a second exit.

8. *Room structure.*—Ceiling structure or wall structure for all rooms must not be in conditions requiring replacement (such as severe bulging or leaning).

9. *Room surface.*—Ceiling surface or wall surface for all rooms must not be in condition requiring replacement (such as loose surface material, containing large holes, or severely damaged).

10. *Ceiling height.*—For living room, bathroom, and kitchen the ceiling must be 7 feet (or higher) in at least one-half of the room area.

11. *Floor structure.*—Floor structure for all rooms must not be in condition requiring replacement (such as severe bucking or noticeable movement under walking stress).

12. *Floor surface.*—Floor surface for all rooms must not be in condition requiring replacement (such as large holes or missing parts).

13. *Roof structure.*—The roof structure must be firm.

14. *Exterior walls.*—The exterior wall structure or exterior wall surface must not need replacement. (For structure this would include such conditions as severe leaning, buckling or sagging and surface conditions such as excessive cracks or holes.)

15. *Light-ventilation.*—The unit will have a 10 percent ratio of window area/floor area and at least one openable window in the living room, bathroom, and kitchen or the equivalent in the case of properly vented kitchens and/or bathrooms.

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DEPARTMENT OF HOUSING AND URBAN DEVELOPMENT,  
Washington, D.C., December 12, 1978.

HON. DANIEL PATRICK MOYNIHAN,  
U.S. Senate,  
Washington, D.C.

DEAR SENATOR MOYNIHAN: This is in response to your letters of November 22 and 24, 1978 relating to the recent hearings of the Subcommittee on Public Assistance.

I appreciated the opportunity to appear before the Subcommittee and look forward to the final publication of the hearings. I am enclosing a copy of "A Summary Report of Current Findings from the Experimental Housing Allowance Program" which I believe should be included in the record. This publication provides the most recent published data on the housing allowance experiments and elaborates on the summary points presented in my testimony.

Following, for the record, are responses to the three questions you raised in your most recent letter:

*Question 1.* In the experiment you recounted, were housing allowances made available only to heads of households? If so, did you observe any impact of this program on family formation and dissolution?

Answer. Housing allowance payments were made available only to heads of households. As I mentioned in my testimony, only one of the three experiments (the Demand Experiment) had a control group which could permit comparisons between those who did and those who did not receive payments, much in the same manner as has been done in the Income Maintenance Experiments. The subject of family formation and dissolution was not a policy or research question our experiments were designed to answer. Thus, while the Demand Experiment data base does contain data on this subject, among many others, because of budgetary constraints we have no plans for analysis of these particular data. Further, I believe the Income Maintenance Experiments are a better source of data for this issue since they were designed to address this question.

However, we do expect to make the entire data base available for public use as soon as it is feasible to do so (I estimate that it is more than a year off), and at such time it will be possible for any interested person to analyze the data related to the family formation-dissolution question.

**Question 2.** Given what you say about "Importance of neighborhood attachment" and kindred forces that affect a family's choice of housing, how would you weigh what might be termed the structural or physical aspects of housing quality? These latter were the main criteria of "success" in your experiment, as I understand it, but are they necessarily the main determinants of behavior?

**Answer.** In part, you are asking about the importance that families attach to the physical aspects of their housing. Our findings suggest that the majority of families attach a great deal of importance to the physical aspects of their housing. As I said in my testimony, around half of low income families were living in physically acceptable housing before the experiment began, and to do so, incurred rent burdens which, on average, were around 45% for the poverty group. Thus, many families obviously sacrificed other necessities in order to live in decent housing. Our survey responses show that families in bad housing were very dissatisfied but some apparently found it difficult to leave the support system that neighborhoods provide through friends, and relatives, and other institutions. Some families clearly felt defeated before they began, and said they doubted they could find anything even as good as what they had, however poor that was.

I think it is impossible to answer whether the quality of one's housing is a "main determinant(s) of behavior." The determinants of human behavior are complex, as you know. The choices people make, and the reasons for these, are equally complex. However, the long-standing national policy to remedy unsafe and unsanitary housing conditions, declared by the Congress in the Housing Act of 1937, is a minimum standard which our society imposes irrespective of the relative importance different families attach to such conditions. Time and again in the hearings you stated that we must be concerned with the welfare of children. I feel that this minimum standard of housing addresses itself to just such a concern.

**Question 3.** Your finding that only fifty percent of eligible families participated in the housing allowance program is remarkably similar to participation rates in Food Stamps. Do you think that the two programs are comparable and that, if Food Stamps were replaced by an unrestricted cash supplement more needy people would claim benefits for which they are eligible?

**Answer.** It is true that aggregate participation rates in housing allowances and Food Stamps are similar. This could be simply a coincidence, or it could be a function of the restrictions that are inherent in both programs. We do not have the information to determine why the two participation rates are similar, at least in the aggregate. (They could, for example, be different for different subgroups of the populations. We just don't know.)

In EHAP, it is clear that there were higher participation rates among those who had no constraints with regard to the quality of housing they could occupy. I don't know whether this finding is directly transferable to the question of cashing out the Food Stamps program for the reasons given above.

I hope the above responses will be useful to you. If there is any further information I can provide, please let me know.

Sincerely yours,

DONNA E. SHALALA.

Senator MOYNIHAN. Now, Mr. Michael Wiseman of the Institute of Business and Economic Research of the University of California. Mr. Wiseman, good morning.

Mr. WISEMAN, you are an economist, as I understand it?

Mr. WISEMAN. Yes.

Senator MOYNIHAN. And you have been studying the dynamics of welfare dependency in Alameda County?

Mr. WISEMAN. Yes; that is correct.

Senator MOYNIHAN. We are very happy to have you here, and we look forward to your testimony.

Mr. WISEMAN. Senator, I plan to simply summarize the main points of the document that has been distributed. I would like to enter that into the record.

Senator MOYNIHAN. We will be happy to do that.

**STATEMENT OF MICHAEL WISEMAN, DEPARTMENT OF ECONOMICS  
AND INSTITUTE OF BUSINESS AND ECONOMIC RESEARCH, UNI-  
VERSITY OF CALIFORNIA**

**Mr. WISEMAN.** Let me begin by saying I am a little embarrassed. I operate on a budget of somewhat less than \$200,000, so I am not quite in the same league as those who just preceded me.

**Senator MOYNIHAN.** But you are just starting out, Dr. Wiseman.

**Mr. WISEMAN.** In a nutshell, as you mentioned, I have been working for several years on the dynamics of movements onto and from public assistance in Alameda County, Calif. Alameda County is the county across the bay from San Francisco.

**Senator MOYNIHAN.** Is that not Oakland?

**Mr. WISEMAN.** Yes, and the largest city is Oakland. About 60 percent of the recipients that I am going to be talking about reside in Oakland.

I have testified once before to the House committee last year, on welfare, and many of the results of my experiments or my studies were reported in that testimony. What I would like to concentrate on today are the results or implications of my work for family fragmentation in public assistance programs as now operated in California.

To begin with a quick nutshell summary, my research indicates that welfare turnover is substantial, especially in AFDC-U, the unemployed parents segment of AFDC, and a significant amount of this turnover is associated with family breakups. Furthermore, I am going to argue, or show, today that my research indicates that the probability that these separations will occur appears to be related to factors over which policymakers have some control.

**Senator MOYNIHAN.** Have some control?

**Mr. WISEMAN.** That's right.

**Senator MOYNIHAN.** The number of null hypotheses in this business is numbing.

**Mr. WISEMAN.** I think it is best to stop for just a moment before getting to those conclusions to understand the nature, or the way, in which my research is conducted, because it differs in a number of dimensions from the experimental results that have been discussed earlier in these hearings.

The data on which my conclusions are based are collected largely by sampling AFDC case files in Alameda County. My technique is basically to go to case files chosen at random on the basis of payments records and to compile from the information there and from caseworkers a demographic profile of the case as of a point in time. Then the status of that case at quarterly intervals following what we call the sample month is reexamined and changes in family status, including moving from welfare, separations and the like, are recorded.

**Senator MOYNIHAN.** Do you do that by field work or do you do it just by picking up from the records and saying—

**Mr. WISEMAN.** A combination of those two approaches.

We have been doing this for some time. The sample that I will be describing today goes back to 1967 and covers the period 1967 to 1972 and the year 1975. We have in progress a sample for 1978 that is not completed. The samples cover about 4,000 households.

The period is covered by this sample is interesting for a number of reasons. One way to stimulate interest in it is to look at the two graphs that appear on pages 4 and 5 of my statement. These show the caseload in Alameda County first in the family group program, the segment of AFDC that is made up largely of single-parent families, and then the caseload in AFDCU—what is called UF on the east coast—the two-parent segment of the AFDC program.

The period 1967 to 1972 spans the introduction of the so-called 30 $\frac{1}{3}$ , disregards policy, the work-incentive policy, the various changes of the hours rule for AFDCU and in 1971, the so-called Reagan welfare reforms in California. This period also saw two major recessions—1969–70 and 1974–75.

Looking at those diagrams, before we even get to fragmentation, the natural question is to ask what happened? Why did the caseload grow so rapidly in the late 1960's and then stop growing in the case of FG? Why in the case of U did it grow so rapidly and then fall equally rapidly?

I have discussed the reasons, or my guess for what happened to the caseload, in a number of papers that are cited in my testimony. Basically, my story is the same that Barbara Boland tells, that in Alameda County the caseload explosion stopped in 1970 when virtually every poor single-parent family headed by a woman had come on to public assistance. Fortuitously, that occurred at about the time that our Governor introduced the California welfare reform program and took much of the credit for the reduction in the rate of growth, which really seems to be demographically based.

In the AFDCU case, it is a little bit more difficult to understand what brought the turnaround. It seems to be related to (1) the elimination of a large—in Alameda County—number of General Motors workers who were on strike in the late 1970's, (2) a tightening of the unemployment hours rule used for defining AFDCU eligibility in 1971, and (3) tightening of the assets test for AFDCU eligibility both by legislation in 1971 and by inflation over the period following 1971.

So the data we will be looking at reflect caseload developments in the context of both changing AFDC policy and the changing economic environment. It turns out that that economic environment has some impact on fragmentation, as we will see in a moment.

I have summarized, if you are interested, on table 1 on page 6 of my statement, some rough statistics on characteristics of the caseload. In the case of mothers numbers they seem to suggest that three things have been happening. One is that over the period 1967 to 1969—

Senator MOYNIHAN. Would you help me? FG? This is a Californianism.

Mr. WISEMAN. Right. FG is the "family group" segment. That would be what would be typically called in New York just ADC.

Senator MOYNIHAN. AFDC.

Mr. WISEMAN. OK. AFDC.

Senator MOYNIHAN. ADC was the Aid to Dependent Children. The mother was made eligible around 1949, and there was no hyphen. It was the AFDC-U that came in in 1961 and that is what you have.

Mr. WISEMAN. And that is the second part of this.

Senator MOYNIHAN. I love bureaucracies. If it is at all possible, add a hyphen and some unexplained initials.

Mr. WISEMAN. Right. The FG is, by far, the larger program. My diagrams have different—

Senator MOYNIHAN. Yes; you go from 1967 to 1975 there.

Mr. WISEMAN. That's right.

Senator MOYNIHAN. And the first number is the portion and then you have the standard error.

Mr. WISEMAN. The interpretation, I think, can be readily seen. Going down the FG—

Senator MOYNIHAN. Age under 30 is that column.

Mr. WISEMAN. We can start there, if you like.

Senator MOYNIHAN. You tell me what it is. Don't let me tell you

Mr. WISEMAN. By and large, the caseload is getting younger. The average age of women in AFDC is now considerably less than it was in 1967. That really means that, as we know, the new families coming on to public assistance are young women with young children. They tended to be less likely to be black than is the case with the typical recipient before the caseload explosion, because the caseload explosion in California was really, in large part, the movement onto welfare of white families.

Since 1972, the number of white families has declined somewhat—the proportion of AFDC families which are white—has declined, and their place has been taken up by Chicano families. That does not show on this diagram.

Briefly, the other point to make is that the average education of mothers in at least the—or in both programs, has been going up so that, in 1975, more than half have at least a high school education or better.

In the AFDCU program, the—

Senator MOYNIHAN. Is that notation "greater than or equal"?

Mr. WISEMAN. That is greater than or equal, so that would be high school or some college.

I put those data in just to give you an impression of the additional dimensions of some of the demographic developments.

Senator MOYNIHAN. Do you think it is fair to say we had a national experience of this soaring caseloads in the late 1960's and that the subsequent leveling off simply meant that you were at the point where all who were eligible had come into the program?

Mr. WISEMAN. That is apparently true for the FG, or what we call in California the FG component of the program. The status of the eligible's exhaustion hypothesis with the ADFCU program is more ambiguous and that, presumably, is something that the people from RAND are going to talk about later.

Now, the background aside, we can now turn to issues related to turnover, and I think these are particularly interesting. Traditionally, the orientation of AFDC policy has been toward turnover; that is, the rate at which families leave dependence. Many existing welfare policies are oriented toward raising it.

In the past few years, much research has been devoted to determining just how great turnover is and whether policies like the 30½ disregard that I mentioned earlier is to lower it.

One measure of turnover is the ratio of the number of family units receiving assistance during any month, at any time during the calendar year, to, say, the largest monthly caseload.

In other words, if we want to measure turnover, we look at how many families receive at any time and compare that to what a typical caseload size is.

Alameda County, like most counties and States, also, does not maintain data in a form which permits direct calculation in the form of a turnover ratio. However, the sample that we have conducted allows estimation of that ratio, and I have put some of those numbers in table 2 of my statement, to give the impression of the amount of turnover that appears within an operating welfare system.

The results in table 2 suggest that there has been little change in public assistance turnover over the past 10 years despite all of these changes in policy and what have you. All things considered, I think that the turnover in AFDC-FG is quite small. There is a question here about what one's conception of small or large would be in reaction to these statistics, but what they say is, in terms of, for example, 1975, that the total number of families on public assistance in the FG program was only 18 percent larger than the largest monthly caseload, so that means there was not very much moving in and out of FG.

But, on the other hand, in the U program the number of cases that are opened during the year is about 62 percent larger than the largest monthly caseload. These numbers confirm what we anticipated before their collection, and that is that two-parent families are likely to close their welfare cases more rapidly than one-parent families.

Senator MOYNIHAN. The AFDC-U program is for unemployed people and people who are not dependent, they are simply unemployed. It is the equivalent of unemployment compensation.

Mr. WISEMAN. Yes and no, and I will show you the no in just a moment, but let me put it another way. The figures that I have showed in table 2 indicate that the average duration of an FG case in Alameda County is a little over 5 years, and the average duration of a U case is a little under 1½ years.

Senator MOYNIHAN. Five years. That strikes me as a long duration.

Mr. WISEMAN. Yes; it is.

Those averages mask wide variations in turnover rates across different types of families. With these average duration of case figures, one also has to be careful about, because it depends upon which type of family you are looking at. More often, average duration of case figures are cited for new families coming on public assistance, and very often those are families or mothers who are young, well educated, and have only a single child; and such women tend to have the highest probability of leaving public assistance.

So the average expected duration for a family of that type may be considerably less than 5 years. The figures that I was citing was for the average, given the characteristics of all persons on FG, all families, many of which have a much lower probability of leaving.

Now, the AFDCU closings which give rise to that high turnover number mentioned in table 2 do not all arise from the exodus of families from welfare. A substantial amount occurs because of family fragmentation. This effect is illustrated by comparing the sum of estimated total family units ever receiving assistance in the FG program, plus the sum of family units receiving assistance in the AFDCU program, to the sum of families receiving assistance in either program without differentiation. For this purpose, I am defining family in terms of the mother-child combination as sort of the nucleus from the administrative standpoint of the FG program.

I have tried to do those calculations to show what that looks like in table 3. Table 3 is a little hard to understand. Let me go across the bottom line to explain it.

For 1975, table 3 says that in Alameda County, about 28,201—I say “about” there; I am meaning to imply our estimated number—families received assistance in the FG program. During the same year, 3,166 family units received assistance in the U program.

And if we counted in our estimate of the number of units that received assistance of any type, regardless of program, that estimate is 30,629.

Now, summing the FG and U numbers gives us a number that is 738 larger than the 30,629 estimate of families that received assistance of any type. Now, what that means is that there were 738 families who received assistance in both programs, so that the way to think of it now is to divide that number by the total number of families in U at all. When you do that, you come out with an estimate that about 23 percent of all of the families receiving—we can think of this in terms of mothers; it is probably easiest that way—23 percent of the mothers who receive assistance, or were in units who received assistance through AFDCU at any time during 1975 also received assistance at some other time during the year in AFDC-FG and in almost all cases, that is a case in which a family started out, or was on assistance at U and then went into the FG program.

Now, there is another way to look at it and that is just to look at cases chosen at random from the AFDCU caseload and see what happens to them over 12 months and to calculate the percentage that make that kind of shift, and those proportions are cited in table 4 and the numbers are consistent with the figure that we just came up with, although the basis is slightly different.

Something about under a fifth or more of U families observed at any point in time on the caseload will have, within 13 months, shifted to FG.

Two-thirds of those transfers are due to separations in which the father leaves the home. Now, transfers from AFDCU to AFDC-FG can occur for other reasons, but the predominant one is through disability. The father, for example, is certified as an alcoholic and the family is reclassified as AFDC-FG from the U program.

But, again, in our study, two-thirds of the families who made the shift, the shift occurs as a result of family separation.

Now, what brings about these separations? This is where we get to the major conclusions that I want to emphasize today, and the point

at which there seems to be some policy impact, although it is modest. To get at that question, Jerry Silverman, one of my associates, and I, have estimated a number of models of the determinants of family separation, and we have set the models up such that the dependent variable is does the family fragment, or come apart, and the independent variables are a series of variables reflecting the family's characteristics and the general—the father's work history and general economic conditions in the country.

Those variables in our models are summarized in table 6 and that may be useful to look at, at least after this discussion is completed.

I want to tell you how we measured county economic conditions, because I think that is particularly important in light of the results. There is no good county unemployment variable. The current household survey really produces reliable area statistics only on an SMSA basis and then only on an annual basis and we were concerned about month to month fluctuations in economic conditions so we measured the jobs situation in Alameda County by the ratio of jobs filled by the working age population. And I think that that variation in the jobs filled in the county over time probably gives a better indicator of the general labor market conditions to which recipients in Alameda County are exposed than some sort of SMSA unemployment rate which includes places like Marin County and what have you.

The results. Perhaps the best way to get at the results is to look at them quantitatively in terms of the effects on probability of separations of various changes. We found that several factors influenced the likelihood of separation. Some of them were obvious and probably would be passed over fairly quickly.

As fathers get older, the likelihood that they will leave their families falls. That is no surprise. If we observe a family in which the mother is pregnant, the likelihood that a father would leave is much lower than would otherwise be the case.

Now, from the standpoint of the fact that both the general philosophy of the program for better jobs and income and the concerns of this group, perhaps of more importance is the fact that we found both an effect on fragmentation of duration of joblessness—that seems to make sense—and also an effect on fragmentation of variation of economic conditions. That is to say what we can observe, in Alameda County, a deference in the likelihood that fathers will leave, all other things equal, that seems to be dependent upon the general economic or job outlook in the labor market to which these recipients have access.

In table 7, these results are reproduced and are summarized in some numerical calculations. For our sample as a whole, over a 3-month interval, the probability of fragmentation for AFDC families is, on average, about 5 percent. Now, that is for a 3-month interval. That means over a year it accumulates—

Senator MOYNIHAN. Hold it. Say that again.

Mr. WISEMAN. I am on table 7.

Senator MOYNIHAN. You are on table 7.

Mr. WISEMAN. Recall, Senator, that this is over a 3-month interval. The earlier statistics we were looking at were probabilities over a year. The model that we are estimating here is sufficiently audacious to try



to predict the likelihood that a father will leave over a quarter, over 3 months.

The average for families of this group was about 5 percent of fathers in U families would leave within 3 months.

Senator MOYNIHAN. Help me make the distinction. Probability can be translated roughly into percent, can it not? It does not guarantee it, but in a universe, probability is a percent?

Mr. WISEMAN. Right. That is true.

Senator MOYNIHAN. And in a sample, it is just probability.

Mr. WISEMAN. That is true.

Senator MOYNIHAN. So that this record will be clear, if you are talking about the universe of possibilities, probability equals percent. If you are talking about a sample, well, then probability is probable percent.

Mr. WISEMAN. Probability is the percentage of the sample and it is our estimate of the universe.

Senator MOYNIHAN. Yes. It just gets down to this is likely but not expected at this point, because no sample is quite that good.

Mr. WISEMAN. But recall this is a large sample and we observed many families over repeated quarterly intervals and so the—although I have not printed the standard errors of these estimates, they are relatively small.

Senator MOYNIHAN. Can you help me in terms of how do I go from the probability of a 3-month period to the probability of a 12-month period?

Mr. WISEMAN. Well, roughly—which is not unreliable in this case—is just to multiply it by four.

The reason why it is rough is for one, that it compounds, but the second thing is that in this population, and we show it below, the duration of unemployment raises the probability that the family will fall apart. So you do not just run the same experiment each quarter. If the family makes it through one quarter and the father is still jobless, the probability that they will fall apart during the next quarter rises a small amount because he has been jobless a longer period.

Senator MOYNIHAN. So this is a population that has about a fifth of its unions dissolved in the course of a year?

Mr. WISEMAN. That is right.

Senator MOYNIHAN. And this population is already on welfare, on AFDCU.

Mr. WISEMAN. On AFDCU, so it is not representative of a random selection of all poor families.

Senator MOYNIHAN. You could have various time controls, but you do not have any at this point. You are just talking about these people.

Mr. WISEMAN. One of the points that I am getting to, and about to end up with, is that fragmentation is, of course, a concern of the type of populations that were sampled as the basis of the Seattle-Denver income maintenance experiment and other such experiments. But we should not lose sight of the fact that fragmentation is a very frequent event within the populations in the existing system.

Senator MOYNIHAN. That being so, that increase of 60 percent is an important increase. If it were 2 in 1,000, a 50-percent increase could

make an impressive headline, but it would not be a notable event. But if it is 200 in 1,000—

Mr. WISEMAN. But that is not a fair comparison because these people are already at an income maintenance—the increase that you are citing was an increase over the control group, I believe, which was not receiving public assistance.

Senator MOYNIHAN. I make the point that the increase was on a large base, not on a small one. I mean, it just did not happen to just a few people out of a 1,000. It was not that kind of a thing.

Mr. WISEMAN. I understand.

All right. Items 1 and 2 on that table 7 show what our results suggest will be the case in Alameda County, as a result of two hypothetical events. One is a reduction in the employment ratio, this measurement of the job market condition, by about the amount that occurred in 1974–75. The result of that is to raise this probability of fragmentation by about 16 percent for over a quarter.

So the change of labor market condition of the type that appeared between June of 1974 and March of 1975 in Alameda County raised the fragmentation likelihood among families by a factor of about 16 percent.

Now, the other thing that we found is that the duration of time jobless—this is something that I found not surprising, but I was interested that the data confirm it—also raised the likelihood of fragmentation, and I mentioned that before, but the effect is relatively small. If we increase our—looking at the models, they suggest that in comparing, all other things equal, two families, one in which the father had been unemployed for 23 months and the other in which the father had been unemployed for only 11 months, a year difference, the change in the probability of separation would be from 0.151 to about 0.154, which is a very modest change but, nonetheless, was statistically significant. We know that statistical significance is not the same as importance.

Let me summarize and then we can open this up for questions.

I have tried to concentrate in this statement on the portion of our results which relate to family instability because that seems to be an important concern of these hearings. That was not the central focus of our work.

It is probably important not to exaggerate the significance of these results. Both the strengths and the weaknesses of our research are related to its nonexperimental character. The fact that the Alameda County study is nonexperimental is attractive, I think, because, after all, we are dealing with a real welfare system which serves all eligible applicants and is run by the kind of people who ultimately do the job, regardless of the system selected.

The weakness is that we cannot be sure that in our analysis all things are really held constant, that our employment effects do not really reflect something else, and so forth. Nevertheless, I think that three things are important and worth emphasizing.

The first is that the Alameda County welfare study helps put welfare turnover in perspective. On the FG side, my figures indicate that the

common conception of welfare as a reflection of permanently dependent female-headed families is not particularly misleading.

I am concerned that the small children who came onto public assistance with the caseload expansion of 1969-70 are now reaching the age of candidacy for welfare dependence themselves and little attention is being paid to the critical period of transition now and which way they will go.

The little work that we have done on the current welfare status of women who were 15 to 16 years old, and on welfare in 1972—that is, children and families on welfare—shows that at least half were on public assistance as of their 21st birthdays.

Senator MOYNIHAN. Oh, really? We had quite different testimony from Dr. Klausner yesterday in a sociological study of Camden, N.J. But that is a small sample, you say?

Mr. WISEMAN. That's right.

Senator MOYNIHAN. But a large ratio?

Mr. WISEMAN. That is right. We might come back to that in a moment.

The one thing that seems to make a difference—and here I am citing results from my earlier testimony—in AFDC-FG is jobs. Recipients who work are much more likely to leave dependence than those who do not, thus the corollary of the common conception that welfare recipients do not necessarily have to stay dependent is not—the common conception, that is the conception that welfare is made up largely of a large group of permanently dependent families, the corollary of that seems to be that there is nothing we can do, and that is not necessarily true. There may be things that policymakers can do to enhance turnover and those things seem to be related to jobs.

Turnover in the AFDCU program is substantial. In our report last year to the Department of Labor, we showed that training services do increase the speed at which AFDCU families leave dependence. The problem for policy seems to be to design policies that will accelerate the departure of families which otherwise remain dependent while not interfering with the movement out of dependence of that large group which will leave of their own accord.

The second point is that—we have been over this—is the rate of family fragmentation among AFDCU recipients is high and appears to be in part related to external circumstances. It is not clear how the variation in labor market conditions captured by an employment variable affects family stability, but if the effect does operate through the parents' perception of their chances economically, presumably these perceptions can also be affected by appropriate training and, where necessary, jobs provisions programs.

I will be the first to admit the evidence here is weak, but to my knowledge, the Alameda County sample is the only data source which allows considerations of the effects of variations on labor market conditions over time on family fragmentation. The fact that this effect has surfaced is important.

The final impression that I wish to emphasize is that all of our research reveals ways in which policy can affect welfare outcome. In our research on turnover, we have shown that employment-related services affect the likelihood that recipients will take jobs.

Our research on separation shows that such jobs will reduce the likelihood of family fragmentation both by reducing the duration of

joblessness—and that effect, you will recall, is small—and perhaps by changing the employment outlook for recipients.

Likewise, as I reported in my testimony last year, our results indicate that the hours rule reduces the rate at which AFDCI fathers take jobs by so expanding unemployment. This suggests that the hours rule itself has a fragmentation effect.

The fragmentation effects we have observed are small numerically, but the size may reflect the crudeness of our measures. We wish that we were able now to produce more refined measures. We expect to be able to do so in the future as our work continues.

Senator MOYNIHAN. Sir, that is superb testimony.

It is too elegantly put together to come in here and have some number and say how much they mean and how much they don't mean and suggest where you are going. I think it is just a very important question. It certainly says jobs, jobs, jobs, doesn't it? But what are we to make of the strong suggestion of intergenerational dependency?

Mr. WISEMAN. I want to be cautious about that. We have only about 60-some-odd women.

Senator MOYNIHAN. You were cautious. I am saying it is the strongest suggestion we have had on this, it is something we will talk about. You are going to pursue it, you are staying with these?

Mr. WISEMAN. I think the important thing is we just haven't seen, that intergenerational effect has not hit us yet because the typical new woman coming on in 1969-70 had an infant or children three or four. Those children are really not yet at risk in an intergenerational transfer and that problem will be the problem of the eighties.

Senator MOYNIHAN. Yes; when the demography is working for us.

I wanted to ask you, have you picked up any influence of income encouragement provisions, of which the most notable is the \$30½ disregard?

Mr. WISEMAN. As I mentioned in the beginning, our sample covers public assistance in Alameda County before the introduction of \$30½ and through its various versions in California. It has been calculated a number of different ways and each change actually changed the work incentive involved.

The nature of our experiment has not been to look at work incentives in the way in which it is done, like the negative tax experiments. There the question is to look at variations in supply, in terms of hours work or hours worked of secondary members of the family, or what have you. Our approach to labor force participation has to rather look at transitions, transitions such as the one from not working at all to working, and transitions such as the one from working on public assistance to leaving public assistance altogether.

So when I talk about the effect of these innovations on labor supply I will be talking about those transitions and not in the variational sense that the labor supply has been discussed, was discussed yesterday.

Now, that said, in our results we have not been able to find any impact of, first, the introduction of the \$30½, its liberalization in

1970 and then its tightening, the tightening that was introduced by Governor Reagan in 1971, on the likelihood that recipients of public assistance who are not working would take jobs, would begin to work, participate in the labor force.

We found an impact of the introduction of the \$30½ disregards on the likelihood that they would leave public assistance and that impact was negative. The reason largely seems to be that the \$30½, the work incentive provision raises the break-even point, income break-even point, and that meant that some transitions that formerly had caused people to leave dependence no longer brought about that particular—

Senator MOYNIHAN. That really shouldn't surprise us. Blanche Bernstein has testified that in New York you can have earnings up to \$28,000 and still be on welfare under certain circumstances. The logic of the \$30½ provision, as I understand it, was a dynamic argument about labor force participation. It gets you into the labor market.

Mr. WISEMAN. Gets your feet wet. Once your feet are wet you are willing to dive in. We have not been able to detect any effects of that nature.

Senator MOYNIHAN. Listen, the Internal Revenue Code is filled with equivalents of \$30½ for people who have owned mines or have herds of Black Angus cattle, and so forth, and they maximize, that is all. It is positive economic behavior. People get as much income as they can for as little effort as they can.

Mr. WISEMAN. In our experiment the control is the period before July 1978, in which \$30½ was introduced. That period as we know from our studies, was not a period in which there were no work incentives. There was work, at least there were a great deal of incentives in the form of allowances for work expenses and what have you, and benefit computation, but nonetheless, during that period, at least in Alameda County, the break-even point was considerably lower in terms of earnings for working recipients and that fact did not seem to inhibit labor participation.

Senator MOYNIHAN. Let me ask you one last question, just to ask your judgment. This goes beyond your research. I don't know if you are familiar with the testimony we had yesterday from the Stanford Research Institute team that was analyzing the Seattle-Denver experiments and others who have reanalyzed effects in the New Jersey experiments. The findings were—I don't want to characterize them as more than surprising—larger work reductions than originally reported, large work reductions in some groups, considerable by any judgment, and very large reported increases in marital dissolution.

Do these come as much of a surprise to you as they perhaps have come to the policy research community here in Washington?

Mr. WISEMAN. Let me limit myself to the marital dissolution results first. I was surprised by the magnitude. I was surprised first in my own work by the magnitude of intra-AFDC flows. I began that work thinking that AFDCU served a distinct population from the—

Senator MOYNIHAN. And it turned out to flow?

Mr. WISEMAN. It is not the case. It is not the case. So I suppose that I was prepared for the fragmentation results by the Alameda County data. While I was amazed by the size of the Seattle-Denver effects, I

don't find them intuitively implausible now given the ethnic differences that are surfacing in the data, which also seem to be intuitively plausible.

Now, on the labor supply estimate, I think there are two aspects. One is the fact that it is as large as it is, and the second concerns the proper interpretation of the sequence of discovery with the first estimates which tended to be smaller than the subsequent reanalysis of the data suggested.

I found the final version of the labor supply reduction to be larger than my prior beliefs would have suggested was the case. I feel like the sequence of discovery really reflects the sequence of econometric development or at least the development of our understanding of appropriate technique for dealing with these situations in which labor supply involved not only variation in hours but also changes in state—whether you are “supplying” or not.

I was at the University of Wisconsin during the sixties—that is where I did my graduate work—at the same time the experimental design for the negative income tax experiment was being cooked up. At that time, we had no technique at all. The whole statistical problem of analyzing these data and especially formulating a project design that would be as cheap as possible but give us the results so we could analyze was just all new ground. It was breaking new ground and I think that the sequence of discovery really reflects a mistake made early in the design. The problems of discontinuous change like not working force versus working, are not the kind we traditionally dealt with.

Senator MOYNIHAN. It didn't surprise you, the testimony yesterday, that a reanalysis of Harold Watts' initial data show that the New Jersey experiment led to a much larger work disincentive than he originally thought. This is the technique developing. And if that is so there is nothing the matter with that. As a matter of fact, one of our witnesses said, “You think we have found anything out about a guaranteed income?” He said, “No; we haven't found anything about a guaranteed income, but we have learned quite a few things about how to find out things.”

Well, all right, that is OK. Apparently the problems of income reporting in the rural experiments were huge, but that is how things evolve, and nobody in a quasi-scientific discipline such as your own should hesitate to say we learned something from having done it not quite right the first time. As long as you do learn, you progress.

In the main findings are—

Mr. WISEMAN. Alarming.

Senator MOYNIHAN. Alarming is your word, sir?

Mr. WISEMAN. The findings show that the notion that you can accomplish all the objectives of welfare reform with the kind of incentives that can be built into the tax structure itself is just naive, that things don't work that way.

Senator MOYNIHAN. Yes. I will use your words, they are alarming, and what I want to do in these hearings is to get it all out so we won't become alarmed in the sense of panicky and start concealing and getting unhappy with one another and saying why did you find that,

which would not be the first time messengers have been stoned. No messenger has been stoned so far. A few administration witnesses have been beaten about the head and shoulders. That is their job and their role in life.

We thank you very much, Doctor. You couldn't be more helpful to us, and we will look forward to your continuing your work. You are not yet in the \$200 million class, but, that is because you are only an associate professor.

[The prepared statement of Dr. Wiseman follows:]

**FAMILY SEPARATION RATES IN THE AID TO FAMILIES WITH DEPENDENT CHILDREN PROGRAM**

(By Michael Wiseman, Associate Professor of Economics, University of California, Berkeley)

Mr. Chairman and members of the committee, thank you for the opportunity to summarize results of my research on caseload developments in the Aid to Families with Dependent Children Program in Alameda County, California. I shall limit this report to a summary of what our research shows about movements by families onto and from welfare in the county, about the frequency of separations among families dependent upon AFDC, and about the determinants of these separations. In a nutshell, our work indicates that turnover is substantial, especially in the AFDC-U (unemployed parent) segment of the program. A significant amount of this turnover is associated with family breakups, and the probability that separations will occur appears to be related to factors over which policymakers have some control.

Before getting into these matters, I am anxious to acknowledge that all of the research results to be cited in this statement are from work supported by the Employment and Training Administration. The family fragmentation results were derived in joint work with Gerald Silverman, a Ph.D. candidate at Berkeley's School of Social Welfare who is currently working in the Office of the Assistant Secretary for Planning and Evaluation at the U.S. Department of Health, Education, and Welfare. The computations are the work of Karl Iorio, an economist on the staff of our research project, and this manuscript was prepared by Diana Demeter. These results are preliminary; when our final report is prepared, copies will be provided to this subcommittee. A more comprehensive summary of my results was presented last September at the House of Representatives hearing on welfare reform.<sup>1</sup>

**THE DATA**

The numbers that follow are more readily understood if the sampling technique employed for their collection is outlined. I have long been concerned that too little is known about the experience of families receiving assistance in existing public assistance programs. To improve our understanding, I and my associates in the Welfare and Employment Studies Project at Berkeley have for several years collected data on families receiving public assistance in Alameda County, California. Alameda County is the urban county across the bay from San Francisco. Its largest city is Oakland; about sixty percent of the families in the caseload reside there.

These data are collected largely by sampling case files. Our sampling technique involves observing family characteristics at a point in time—a "sample month"—and then collecting detailed information on what happens to the family for the subsequent year. Less complete data on experience before and after the sample year are also collected. At this time, we are collecting data for a group of families selected from the caseloads for each month in 1978; we have sample data for the years 1967-72 and 1975 cleaned and ready for analysis. Results for the years 1967-72 were reported to the Department of Labor in 1977.<sup>2</sup> The total sample through 1975 includes 3,908 units.

<sup>1</sup> See Wiseman (1977).

<sup>2</sup> See Levy, Vickery, and Wiseman (1977).

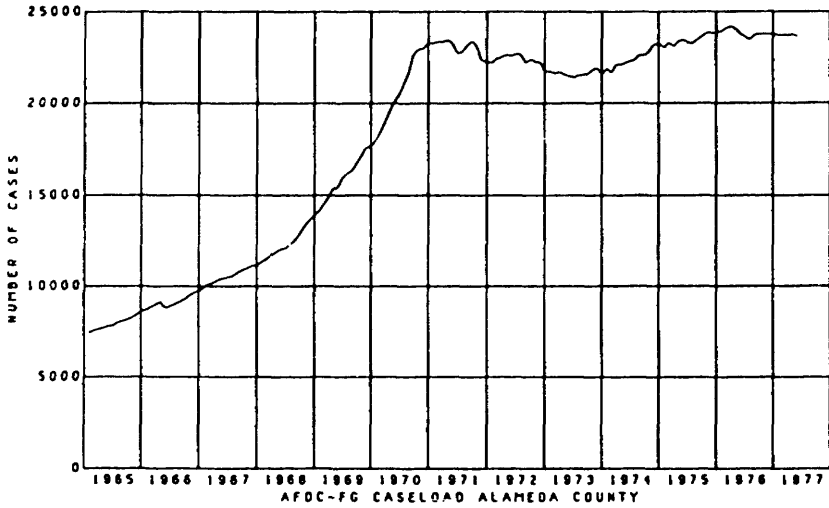


FIGURE 1

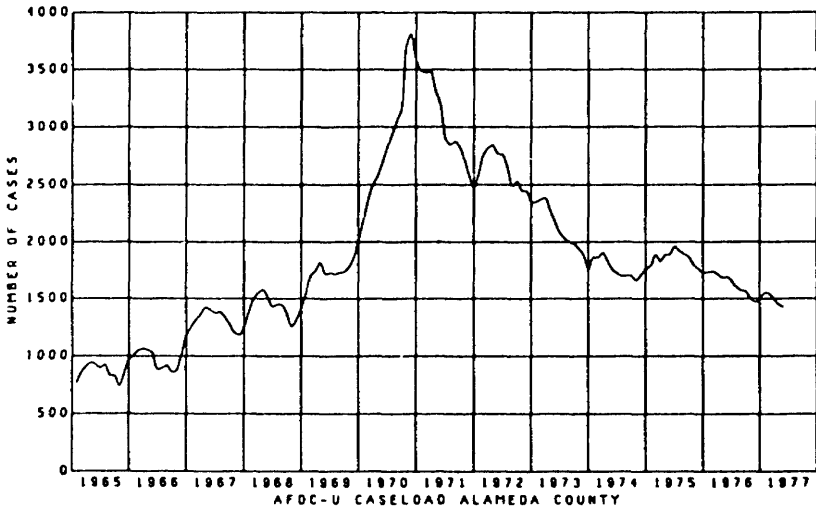


FIGURE 2

The period spanned by our sample is an interesting one for analysis. The trend in the caseload over this period is most readily summarized by two graphs: figure 1 shows the development of the AFDC-FG caseload up through the middle of last year, and figure 2 provides the same information for AFDC-U. The FG program covers assistance to single-parent families and two-parent families in which one adult is disabled. As can be seen from the figure, the FG caseload grew very rapidly through about 1971 and has changed little since that time.

I have discussed the reasons for the end of the welfare explosion in Alameda County elsewhere.<sup>8</sup> The best guess is that the FG caseload stopped growing in

<sup>8</sup> See Rence and Wiseman (1977).



1971 because there were virtually no more eligible families to come on. The AFDC-U program serves two-parent families which qualify for assistance because of one parent's joblessness. The AFDC-U caseload is much smaller than the FG caseload and has declined substantially since 1970-71. The reasons for the turnaround in AFDC-U are more difficult to discern than are the reasons for the FG decline. The caseload grew very rapidly in late 1970 because of a strike; the decline since then is most likely attributable to tighter eligibility restrictions.

Table 1 provides information on the intracaseload demographic developments associated with the last ten years of welfare experience in Alameda County. The data show a slightly different pattern for the two programs. In AFDC-FG, the caseload explosion was associated with a substantial increase in the proportion of white families on assistance. Since 1972 the proportion of whites has declined, while the number of black and Chicano families (not shown in the table) has increased. The average age of mothers in FG cases has declined, while the education level attained by the typical mother has increased. AFDC-U mothers are far more likely now to be white or Chicano and to be over thirty than was the case in the late sixties. The decline in the AFDC-U caseload since 1972 has been associated with an increase in the proportion of older families and (possibly) a decline in average levels of educational attainment.

TABLE 1.—MOTHER'S CHARACTERISTICS, ALAMEDA COUNTY AID TO FAMILIES WITH DEPENDENT CHILDREN PROGRAM, 1967-75

[Proportion of all mothers receiving assistance through the indicated program. Number in parentheses is the standard error of the estimate]

Program	Year	Age, less than 30	Black	Education, more than 12 yr
AFDC-FG.....	1967	0.48 (.029)	0.60 (.028)	0.33 (.027)
AFDC-FG.....	1968	.50 (.028)	.59 (.027)	.38 (.027)
AFDC-FG.....	1970	.51 (.027)	.51 (.027)	.46 (.027)
AFDC-FG.....	1972	.56 (.027)	.53 (.027)	.48 (.027)
AFDC-FG.....	1975	.59 (.025)	.55 (.026)	.53 (.027)
AFDC-U.....	1967	.57 (.031)	.44 (.031)	.38 (.032)
AFDC-U.....	1968	.65 (.029)	.46 (.030)	.36 (.030)
AFDC-U.....	1970	.67 (.027)	.36 (.028)	.49 (.030)
AFDC-U.....	1972	.73 (.025)	.37 (.028)	.53 (.029)
AFDC-U.....	1975	.64 (.030)	.35 (.029)	.49 (.034)

Source: Calculations by author from unpublished welfare case data described in text.

While the data in table 1 are for mothers in the program, the demographic trends for fathers are similar. The AFDC-U program apparently serves a more "hard-core" poverty population now than was the case in 1970; this story is supported by data on job experience and skills which we do not have time to examine.

#### TURNOVER

One measure of caseload turnover is the ratio of the number of family units receiving assistance during any month of the calendar year to the largest monthly caseload. This number ranges between one and twelve; a turnover ratio of one would indicate that no new families ever came onto welfare and none ever left. A ratio of twelve would indicate that every family receiving assistance during the year did so during only one month. Like most counties, Alameda County does not maintain data in a form which permits direct calculation of the turnover ratio. However, our sample permits estimation of the turnover ratio and our estimates are listed in table 2.

TABLE 2.—RATIO, ESTIMATED NUMBER OF FAMILY UNITS ON ASSISTANCE AT ANY TIME DURING YEAR TO LARGEST MONTHLY CASELOAD

[Numbers in parentheses are estimated standard errors]

Year	Program		U
	FG		
1967.....	1.22	(0.042)	1.63 (0.080)
1968.....	1.15	(.045)	1.70 (.088)
1970.....	1.13	(.039)	1.55 (.073)
1972.....	1.19	(.033)	1.49 (.064)
1975.....	1.18	(.029)	1.62 (.076)

Source: Calculations by author from unpublished welfare case data described in text.

The results in table 2 suggest that there has been little change in public assistance turnover during the past ten years. All things considered, the turnover on AFDC-FG is quite small: the total number of cases open during a year is only about eighteen percent greater than the largest monthly caseload. In the U program, the number of cases ever open during the year is about sixty-two percent greater than the largest monthly caseload. These numbers confirm what we anticipated before their collection: two-parent families are likely to close their welfare cases more rapidly than are one-parent families. Put another way, these figures indicate that the average duration of an FG case is a little over five years and the average duration of a U case is a little over a year and one-half. These averages mask wide variation in turnover rates across different types of families.

## FAMILY DISSOLUTION

The AFDC-U closings which give rise to the high turnover identified in table 2 do not all arise from the exodus of families from welfare. A substantial amount occurs because of family fragmentation. This effect is illustrated, albeit somewhat obliquely, by comparing the sum of estimated total family units receiving assistance in the AFDC-FG and AFDC-U programs taken separately for each calendar year with the estimated total number of units receiving assistance in any part of the AFDC program. For this purpose, we have defined a family unit as the mother-child(ren) combination which is the nucleus of the AFDC family. These numbers appear in table 3.

Table 3 is difficult to understand; it is best interpreted by looking specifically at the numbers for a particular year. Our estimates show that in 1975 28,201 family units received AFDC-FG assistance at one time or another and 3,166 families received help through AFDC-U. The sum of these two estimates is 31,367. However, our estimate is that only 30,629 families actually were on the rolls in any sort of AFDC program during the year. The difference, 738, is our estimate of the number of units which appeared at one time during the year in the U program and at another in the FG program. This is *twenty-three percent* of all units ever receiving aid through the U program.

TABLE 3.—ESTIMATED NUMBER OF FAMILY UNITS RECEIVING ASSISTANCE AT ANY TIME DURING YEAR, ALAMEDA COUNTY, 1967-75

[Numbers in parentheses are estimated standard errors]

Year	Families receiving assistance			Difference	Ratio: Difference/ in U
	in FG	in U	in program		
1967.....	13,643 (464)	2,314 (114)	15,293 (434)	664	0.29
1968.....	15,996 (620)	2,678 (139)	17,954 (611)	720	.27
1970.....	26,399 (915)	5,907 (277)	31,105 (919)	1,201	.20
1972.....	27,050 (758)	4,234 (181)	30,487 (759)	797	.19
1975.....	28,201 (700)	3,166 (148)	30,629 (671)	738	.23

Source: Calculations by author from unpublished welfare case data described in text.

Another way of getting at this statistic is simply to count the number of AFDC-U families which change programs (i.e., shift from U to FG) within twelve months of the sample date at which the family first fell into our random sample. These proportions are reported in table 4. Although the time-reference is slightly different (since, for example, a portion of the 1975 sample was derived from the December caseload, and one year from that sample date ranges through 1976), the message is the same: choose a family at random from the AFDC-U caseload in Alameda County and the odds are at least one in five that the unit will have transferred from the U to the FG caseload within twelve months. Two-thirds of these transfers are due to separations in which the father leaves the home.<sup>4</sup> The remaining cases shift because the father develops a disability (alcoholism, for example) or the mother leaves.

TABLE 4.—PROPORTION OF AFDC-U CASES WHICH SHIFT TO AFDC-FG WITHIN 12 MO OF POINT OBSERVATION  
(Numbers in parentheses are the standard errors of the estimates)

Year	Proportion
1967.....	0.24 (0.027)
1968.....	.27 (.027)
1970.....	.17 (.022)
1972.....	.24 (.024)
1975.....	.20 (.025)

Source: Calculations by author from unpublished welfare data described in text.

I conclude that separation rates are very high within the two-parent welfare systems we now have. It seems appropriate now to ask whether these separations are related to anything potentially influenced by policy. The answer is a qualified "yes."

#### THE DETERMINANTS OF FRAGMENTATION

In an attempt to sort out some of the factors influencing separation rates in this sample, Gerald Silverman and I have estimated parameters of "conditional logit" models of the separation process. The dependent variable in these models is the probability that a family on AFDC-U will split up over a three-month interval.<sup>5</sup> The independent variables describe the family's state at the starting point, the beginning of the three-month observation period. For this purpose, we combined where possible up to four three-month observations for each family in our sample. What we are looking for is systematic effects of family characteristics, economic conditions, and policy variables on fragmentation probabilities.

The functional form for this model is somewhat complex. If we identify for any observation  $i$  the list of independent variables as a vector  $X_i$ , then the probability of separation is related to the variables  $X_i$  by the function.

$$P(\text{separation}) = \frac{1}{1 + e^{-X\beta}}$$

where  $e$  is the base of the natural logarithms and  $\beta$  is the vector of coefficients. This functional form has the appropriate property of always producing probabilities lying between 0 and 1. Using the data we have collected, the  $\beta$  vector can be estimated by so-called maximum likelihood techniques. The estimation results appear in table 5.<sup>6</sup>

<sup>4</sup> Calculated from sample data for the years 1967 to 1972.

<sup>5</sup> We do not include movements from the U to the FG program which result from disability.

<sup>6</sup> The data for the logit are taken only from the 1967-72 samples. At the time this data file was constructed, the 1975 data were not ready for use.

TABLE 5.—AFDC-U FRAGMENTATION MODELS

(Numbers in parentheses are t-statistics)

Dependent variable: The probability that an AFDC-U father present at the month the case is observed will leave the family over the following 3 mo. For model 3, the sample is constrained to include only black families.

Independent variables	Means (full sample)	Models		
		1	2	3
EMPI.....	0.3733	<sup>1</sup> -16.16 (-2.273)	<sup>1</sup> -17.00 (-2.381)	-17.70 (-1.647)
Child.....	2.988	.0141 (.2829)	.0238 (.4794)	-.0315 (-.4231)
AGEF.....	390.3	<sup>2</sup> -.0049 (-4.398)	<sup>2</sup> -.0048 (-4.312)	<sup>2</sup> -.0048 (-2.650)
DURA1.....	98.18	.0083 (.5792)	.0126 (.8766)	.0039 (.1757)
DURA2.....	76.44	-.0120 (-.8086)	-.0163 (-5.103)	-.0071 (-.3103)
NVWRK.....	.1338	<sup>1</sup> -.5093 (-1.959)	<sup>1</sup> -.1362 (-2.021)	<sup>1</sup> -.5497 (-1.450)
NOJOB.....	23.08	.0048 (.2.156)	.0052 (.2.215)	.0066 (.2.049)
PTTIME.....	.1792	-.2882 (-1.287)	-.3670 (-1.650)	.0140 (.0447)
PREG.....	.1126	<sup>2</sup> -.6555 (-2.577)	<sup>1</sup> -.6371 (-2.506)	<sup>1</sup> -.8595 (-2.077)
AFDCU3.....	.2617	.0956 (.5479)	.0132 (.0767)	.0809 (-.2955)
Black.....	.4998	.1622 (1.023)	.1571 (.9937)	.....
OTHRACE.....	.1795	.0844 (.3965)	.0301 (.1381)	.....
Stress.....	.4234	<sup>2</sup> .5244 (3.530)	.....	<sup>1</sup> .4597 (2.037)
Constant.....	1.0	4.824 (1.813)	5.277 (1.975)	5.743 (1.440)
Number of observations.....		3,304	3,304	1,387
Log likelihood ratio.....		-745.3	-751.6	-329.7
Log likelihood ratio, coefficients constrained to zero.....		-2,290.0	-2,290.0	-961.4

<sup>1</sup> Significant at 0.05.<sup>2</sup> Significant at 0.01.

There are fourteen independent variables and three models in the table. Models 1 and 2 differ only by inclusion (in model 2) of a variable, "STRESS," which will be discussed below. Model 3 is model 1 estimated only for blacks. All the variables are defined in table 6.

TABLE 6.—Variable definitions (*All variables are defined as of the beginning of the observation period*)

Variable:

EMP1.....	Ratio of employment in jobs in the Alameda County private sector to residents age 16-64.
CHILD.....	Number of children in the household under the age of 18.
AGEF.....	Age of the father in months.
DURA1.....	Duration of marriage or cohabitation in months.
DURA2.....	Duration of marriage in months in excess of 24. If DURA1 is less than 25, DURA2 equals zero.
NVWRK.....	Equals 1 if the father has never held a job; equals 0 otherwise.
NOJOB.....	If the father has held a job, this is the time since his last job ended. If the father has never held a job, this is set equal to DURA1.
PTTIME.....	Equals 1 if the father was working at least part-time. Equals 0 if not.
PREG.....	Equals 1 if the mother was pregnant.
AFDCU3.....	Equals .33 if the observation period begins on August 1971; equals .67 if the observation period begins on September 1971; equals 1 if the observation begins after September 1971; equals 0 otherwise. Graduation of this variable is explained in Vickery, Levy and Wiseman (1977) Appendix II.
BLACK.....	Equals 1 if the mother is black; equals 0 otherwise.
OTHRACE.....	Equals 1 if the mother is not black or white (primarily Spanish surname); equals 0 otherwise.
STRESS.....	Equals 1 if the case record indicates any one of a number of problems which might reasonably suggest marital tension at the point of initial observation.

What do these results show? I shall first present a qualitative discussion and then turn to a few numerical examples.

EMP1, the first variable, is the ratio of employment in the county at the time the observation begins to county working-age population. These results indicate that even if the father's personal status does not change, an improvement in general economic conditions within the county reduces the likelihood that he will leave the household.

CHILD is the number of children in the household. We detected no significant effect of this variable on the likelihood of separation.

AGEF is the age (in months) of the father at the beginning point of the observation. The results indicate that the older the father is, the less the likelihood that he will leave.

DURA1 and DURA2 are variables which identify the length of time the couple has been together. We found no significant duration-of-marriage effect on the likelihood that an AFDC-U family would separate.

NVWRK, NOJOB, and PTTIME are all important indicators of the father's work history. PTTIME identifies those families in which the father is working part-time, but not so much as to lose AFDC-U eligibility. Our results indicate that such part-time work may reduce the likelihood of family separation, but the detected effect is not statistically significant. NOJOB identifies the time, in months, since the father last held a market job given that he had worked some time in the past. Our results indicate that, all other things equal, the longer the time the father goes jobless, the greater the likelihood that the family will fall apart. Average duration of unemployment within our sample is twenty-three months.

About thirteen percent of the fathers in our sample had no work history. For these fathers, NOJOB was coded zero and NVWRK was set at 1. The logit results indicate that fathers without work history were significantly less likely to leave the household than those who had some. While the reasons for this are not wholly clear, it may be that cases in which there is no work history have other unique factors which lower the likelihood of separation. It is possible, for example, that such people are enrolled in school or other training programs.<sup>7</sup>

The remainder of the variables are not immediately relevant to the argument developed here and can be quickly summarized. The PREG coefficient indicates that if the mother is pregnant at the time the family is sampled the father is less likely to leave. The AFDCU3 coefficient indicates that the likelihood of family separation is not significantly different following the introduction of major welfare reforms in 1971 than was the case before. We find no racial differentials in the likelihood of family fragmentation in our sample. By and large, the "black only" model, model 3, has coefficients that are similar to those in the all-sample model.

The dummy variable STRESS indicates whether the county welfare department had evidence at the point the case was initially observed that serious problems existed in the relationship between the man and the woman in the family. The significant coefficient estimated for this variable shows that such information was related to the likelihood of separation.

Table 7 illustrates the results quantitatively by showing the effect of various changes on the likelihood of family fragmentation. Setting all the variables at their mean values produces a probability of fragmentation of about .05. Since this is the probability over one quarter, the probability that a fragmentation would have occurred after one year is approximately .20 and is therefore comparable to the estimates derived from the gross separation statistics. The mean value of the employment ratio for this period was .37. During the 1969-70 recession, the employment ratio fell about .01. A change of this magnitude would increase the fragmentation rate by about sixteen percent. This effect is interesting, especially given that the employment rate identifies only the conditions of the labor market environment and nothing objective about the family itself. Apparently, an increase in the probability of job finding, all other things equal, makes men in Alameda County less likely to leave their families.

<sup>7</sup> In California, a work history is not required for AFDC-U eligibility. If such cases are not eligible for federal assistance, the state and counties cover 100 percent of the costs. The model as estimated assumes that the cumulative effect on likelihood of separation of time spent jobless is the same for persons who have no regular work experience as it is for those who have. We have tested this restriction extensively, and our conclusion is that there is nothing in our data which would suggest that it is inappropriate. These matters will be discussed in detail in our report to the Department of Labor.

While statistically significant, the direct effect of time spent jobless on the propensity for the family to come apart is modest. As the table indicates, a year spent without work raises the likelihood of separation by less than ten percent. One is tempted to say that these results suggest that the "hope" effect of a high employment ratio in the county outweighs the "experience" effect of long periods of joblessness on fragmentation.

Table 7. Effects of variable changes on estimated probabilities of family separation over a three-month interval

Probability of fragmentation, all variables at means.....	0.061
Change in probability from:	
(1) Reduction in county employment ratio by .01 (the impact of the 1974-75 recession).....	+ .008
(2) Reduction in father's time without job from 23 to 11 months....	-.003

Source: Calculations using estimation results from table 5.

#### SUMMARY

I have concentrated in this statement on the portion of our results which relates to family instability. This emphasis was chosen because of current interest in the topic, not because it has been the central or even a major focus of our work. It is important not to exaggerate the significance of these results. Both the strengths and the weaknesses of our research are related to its nonexperimental character. The fact that the Alameda County welfare study is nonexperimental is attractive because, after all, we are dealing with a real welfare system which serves all eligible applicants and is run by the kind of people who will ultimately do the job, regardless of the system selected. The weakness is that we can't be sure that in our analysis things are really held constant, that our employment effects don't really represent something else, and so forth. Nevertheless, I think the following results are important.

(1) The Alameda County welfare study helps put welfare turnover in perspective. On the AFDC-FG side, my figures indicate that the common conception of welfare as a collection of permanently dependent, female-headed families is not particularly misleading. I am concerned that the small children who came on to public assistance with the caseload expansion in 1968-70 are now reaching the age of candidacy for welfare dependence themselves, and little attention is being paid to the critical period of transition now and which way they go. The little work we have done on the current welfare status of women who were fifteen to sixteen years old and on welfare in 1972 shows that at least half are on public assistance at their twenty-first birthdays. The one thing that makes a difference in AFDC-FG is jobs; recipients who work are much more likely to leave dependence than are those who do not. Thus the corollary of the "common conception"—that welfare recipients necessarily have to stay dependent—is not necessarily true. There may be many things which policymakers can do to enhance turnover.

Turnover in the AFDC-U program is substantial. In our report last year to the Department of Labor, we showed that training services do increase the speed at which AFDC-U families leave dependence. The problem for policy seems to be to design policies that will accelerate the departure of families which otherwise would remain dependent while not interfering with the movement out of dependence of that large group which will leave of their own accord.

(2) The rate of family fragmentation among AFDC-U recipients is high and appears in part to be related to external circumstances. It is not clear how the variation in labor market conditions captured by our employment variable affects family stability. But if the effect does operate through the parents' perception of their chances economically, presumably these perceptions can also be affected by appropriate training and, where necessary, job-provision programs. I would be the first to admit that the evidence here is weak. But to my knowledge, the Alameda County sample is the only data source which allows consideration of the effects of variation in labor market conditions over time on family fragmentation. The fact that this effect has surfaced is important.

(3) The final impression that I wish to emphasize is that all of our research reveals ways in which policy can affect welfare outcomes. In our research on turnover, we have shown that employment-related services affect the likelihood that recipients will take jobs. Our research on separation shows that such jobs will reduce the likelihood of family fragmentation both by reducing duration of joblessness, and perhaps, by changing the employment "outlook" for recipients.

The effects we have observed are small numerically, but the size may reflect the crudeness of our measures. We wish that we were now able to produce more refined measures. We expect to do so in the future.

## REFERENCES

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- Wiseman, Michael. 1977. Statement before the U.S. Congress, House Committee on Agriculture. Joint Hearings Before the Welfare Reform Subcommittee . . . Committee on Education and Labor, Committee on Ways and Means. "Administration's Welfare Reform Proposal" 96th Congress, 1st Session on H.R. 9030 Part 2 (Members of Congress and Public Witnesses. Sept. 29, 30; Oct. 11, 12, 1977) : 1121-1127.

Senator MOYNIHAN. Now, again, from the West we are going to hear from Mr. Norman Angus, deputy director of public entitlements, Utah Department of Social Services. Mr. Angus, we are very happy to see you here, and one of the things we were interested in is your experience. It would not, I hope, preempt your own testimony if I just observed that one of the important things about Utah is that it is the one State that has felt that welfare recipients ought to work for their benefits, and so you are an experiment. Well, Utah has always been an experiment.

I remember seeing that wonderful welfare center in Salt Lake City, which goes back to the early settlement by the Mormons there. You have always had a very active welfare program, in the private sector, the church, but it involves people working who haven't worked in the market, and it is not for me to say you extended that principle to the State, but it is for you to tell us what you have done.

We are very happy to have you here.

#### STATEMENT OF NORMAN ANGUS, DEPUTY DIRECTOR FOR PUBLIC ENTITLEMENTS, UTAH DEPARTMENT OF SOCIAL SERVICES

Mr. ANGUS. We are happy to be here, Senator.

Unlike the previous two testimonies this morning, we will be talking about an actual program that has been operational for some 4 years in the State, and we believe that it is not an experimental kind of program but one which has proved its worth.

Our work experience and training program, which we call WEAT in the State of Utah, has as indicated been in effect since 1974, and as I understand it, it is the only approved program which requires as a condition of eligibility recipients to work for their AFDC grant, and the program has been reviewed constantly since its implementation in 1974 by various agencies of the Federal Government, and currently the Office of Family Assistance is again out in Utah reviewing the progress and activity of our WEAT program.

We have received inquiries from almost every State in the Union and many counties and even some foreign countries relative to how we operate the WEAT program and, as I understand it, there are a number of States at this point in time who have pending before their re-

gional offices work programs similar to Utah's WEAT program waiting for approval.

It is also my understanding that the Office of Family Assistance has formally denied Colorado's plan, which is similar to Utah's. I don't know how much longer Utah is going to be able to continue with their WEAT program.

In 1974, let me give you a little background where we are coming from, then I will go into how the program works—

Senator MOYNIHAN. Please do.

Mr. ANGUS [continuing]. And discuss it.

Senator MOYNIHAN. The Colorado has been turned down?

Mr. ANGUS. Colorado has formally been turned down.

Senator MOYNIHAN. Is there anybody here from HEW? Why has Colorado been turned down?

VOICE. I can't answer that.

Senator MOYNIHAN. Perhaps you would find that out and let us know for the record. We would appreciate that.

VOICE. Yes, sir.

[The following was subsequently supplied for the record:]

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE,  
OFFICE OF THE SECRETARY,  
Washington, D.C., November 22, 1978.

HON. DANIEL P. MOYNIHAN,  
Chairman, Subcommittee on Public Assistance,  
U.S. Senate, Washington, D.C.

DEAR MR. CHAIRMAN: This is in response to your request, made to an HEW staff member at the Subcommittee's November 16 hearings, for information on a proposed amendment by the State of Colorado to its AFDC State Plan.

It is our understanding that Norman Angus, Deputy Director for Public Entitlements, Utah Department of Social Services, brought to your attention that HEW recently denied an application from Colorado to amend their AFDC State Plan. Their application requested permission to implement a mandatory work program as part of their AFDC program.

Enclosed is a copy of the letter from Don I. Wortman, then Acting Commissioner, Social Security Administration, to Armando Atencio, Executive Director of the Colorado Department of Social Services, which explains the Department's decision, based upon Federal law and regulations.

As the last paragraph in Don Wortman's letter indicates, we have been working with Colorado on a WIN program component that would meet Colorado's needs and be consistent with AFDC and WIN requirements.

I hope this answers your question. If we can be of further assistance, please let us know.

Sincerely,

HENRY AARON,  
Assistant Secretary  
for Planning and Evaluation.

Enclosure.

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE,  
SOCIAL SECURITY ADMINISTRATION,  
Baltimore, Md., October 8, 1978.

MR. ARMANDO ATENCIO,  
Executive Director, Department of Social Services,  
Denver, Colo.

DEAR MR. ATENCIO: Mr. Sandy Crank, Regional Commissioner of Region VIII, has forwarded your State Plan Amendment Submittals 78-2 and 78-4 with the recommendation for disapproval.

The State's submittals propose to implement a mandatory work program which is in addition to the Work Incentive Program (WIN). Under the regulations in 45 CFR 233.140, Federal matching is not available in expenditures made in the form of payments for work except as provided in the Work Incentive Program authorized by Part C of Title IV of the Social Security Act. Matching



would therefore not be available for payments made under the proposed amendment.

In addition, a State may impose collateral eligibility conditions that would result in denial of aid only if they are consistent with the provisions of the Social Security Act. Section 402(a)(10) of the Act provides that eligible individuals must receive aid to which they are entitled. Since aid means money payments to specified individuals based on need, denying AFDC to an otherwise needy and eligible individual solely because that person refuses to perform uncompensated work is contrary to the purposes and provisions of the Act.

For the above reasons the State's submittals do not meet the conditions of Federal law and regulations.

After discussion and consultation at the Department level, as required by 45 CFR 201.3(c), it is our decision that the recommendation of the Regional Commissioner is correct and should be adopted. Accordingly, Colorado State Plan Amendment Submittals 78-2 and 78-4 are disapproved.

It is my understanding that we have reached agreement on the principal elements of a WIN program component which would satisfy Colorado's needs and be consistent with Title IV-A and WIN requirements. We believe that this agreement represents an approvable project under WIN and we are prepared to work closely with the State and WIN on that project.

Sincerely yours,

DON I. WORTMAN,  
*Acting Commissioner of Social Security.*

Senator MOYNIHAN, Sir.

Mr. ANGUS. In 1974, the State legislature, and I suppose this is probably an extension like you say, of the work ethic that prevails in the State of Utah, required in their legislative intent statement that all adult males and females with children of school age must, as a condition of eligibility, accept employment or job training or take part in a community work project, and failure to do either one of these would disqualify them from assistance. So it is from this beginning that we proceed to implement our WEAT program.

It took us 2 years to get the program plan approved by HEW and, as a sidelight, we are currently putting together a suit against HEW to claim FFP for that 2-year period that it took them to approve our plan.

Senator MOYNIHAN. Good for you. [Laughter.]

I don't know whether they were right or wrong to let you wait. After 2 years, sue them.

Mr. ANGUS. We shall.

Senator MOYNIHAN. They can answer those questions in a month. If they can't, they should expect to be sued. They have no reason to let you wait 2 years to try to run your own affairs. Good for you. Make it personal damages.

Mr. ANGUS. I don't know whether we want to go so far as personal damages.

Senator MOYNIHAN. Go as far as personal damages if you think it matters a damn to HEW that the Treasury might have to send you a check.

Mr. ANGUS. We would be happy if we could collect our Federal financial participation.

The basic premise beyond Utah's WEAT program is that all individuals should have the opportunity of providing for their own subsistence and as I indicated, I am sure the work ethic in Utah is as high as it is anywhere in the Nation and there is a great deal of support from the public as well as from many of the welfare recipients and many of the welfare rights groups are for the WEAT program.

WEAT is operated in cooperation with the work incentive program. It works closely together with WIN. And in order to establish our WEAT program we had to develop a series of sponsors throughout the State, and this has been done and recipients then are required to work as a condition of their eligibility for 96 hours per month on a work site.

Now, it was set up initially that the recipient would work 3 days and then have 2 days available for job search or for training or whatever other kind of things they needed to do to prepare themselves for full-time employment, in addition to the training activity and the work activity that went on in the WEAT project.

But, essentially they are required to work 96 hours per month, and they can do that in any combination that they want as long as they work 96 hours per month. Failure to perform the required number of hours on a WEAT project will result in an individual being removed from the grant, and in the case of an unemployed father, the grant may in fact be entirely closed if they fail to continue to perform.

The process that we follow is as follows:

At the time of the application the recipient is screened for WIN status as required by CFR 224.20 to determine that that individual is required to be registered for WIN or whether they are exempt from WIN. The WEAT program only considers those individuals who are in fact mandatory WIN referrals. We will accept into the WEAT program any volunteers, but persons who are exempt from the WIN program are not required to participate in the WEAT program.

Senator MOYNIHAN. Yes.

Mr. ANGUS. Okay.

As you are aware, in the WIN program there are various components to which an individual may be assigned, either to training or to on-the-job training or various other elements in the WIN program, and if he falls into any one of those WIN components he is likewise exempt from the WEAT program. So really, the only persons that are involved in WEAT are those individuals who have gone clear through the WIN program and wind up in what WIN calls the unassigned pool. So they are those individuals who are least likely to be able to be employed and have not at any point in the WIN process been picked off and put into some kind of WIN component.

So we are really talking about the hard core, unemployed individuals.

Senator MOYNIHAN. Yes.

Mr. ANGUS. When a person is actually placed in the unassigned pool, our district assistance payment office is notified that they have been placed there and they are called into our district assistance payment office and set up for a WEAT interview.

In that interview we do two or three things. One is we determine whether or not the individual is or is not employable. Now, there are a lot of folks who don't have either the physical or the mental stability to be able to work and so those people, even when they wind up in the unassigned pool are also waived from participation in the WEAT project.

If in fact an individual is deemed to be employable, then they are required to participate in a WEAT project. We try to assign them to a WEAT project which closely resembles or relates to any skill

they may have, or a WEAT project which would in effect develop the skills that they seem to be lacking.

Let me go back for 1 minute. As an incentive to participate in the program, we provide each WEAT participant with a \$25 what we call WEAT allowance, which supposedly takes care of lunches and travel expenses and this kind of thing. This can be increased if the individual has expenses which are greater than that.

Now, we have tried to locate our WEAT sites such that they are scattered geographically and we can respond in most instances to a need for a WEAT program assignment.

If the recipient fails to perform their 96 hours of work and the recipient is an unemployed father the process happens as follows:

We get a monthly time report from each one of these WEAT sponsors. If they fail to perform then our first sanction is that the individual, the unemployed father, is removed from the grant, so the grant would be reduced by one person. If that unemployed father continues to fail to participate then we would deregister him from WIN, and because of deregistration from WIN, the entire case would be ineligible.

Senator MOYNIHAN. How often has that happened?

Mr. ANGUS. Fairly frequently.

Senator MOYNIHAN. Very frequently?

Mr. ANGUS. Yes. We have one 6-month report in the back. That will tell you what happens in the process.

Senator MOYNIHAN. OK.

Mr. ANGUS. A regular AFDC recipient who is not an unemployed father, the sanction only applies to the adult in the household, and they are removed from the grant and the grant would continue then for the remainder of the household. In all cases we provide the required 10-day notice of any adverse action.

There are approximately 300 sponsors located throughout the State and these sponsors are located either in governmental entities or in nonprofit organizations. We don't go into the for profit kind of organizations and supply them as WEAT sponsors. We have some 600 different job sites and these 600 different job sites cover a multitude of different kinds of tasks.

Just to go through a few of them. We have job sites in accounting, carpentry, clerical community aid, computer aid, custodial detention center aid, filing clerk, fish hatchery helper, grounds keeper, et cetera, et cetera.

Senator MOYNIHAN. Journalist?

Mr. ANGUS. Yes, on and on.

Senator MOYNIHAN. That you find is a very low-skilled occupation with minimum requirements?

Mr. ANGUS. There are a lot of minimum requirement kinds of things. There are still some rather sophisticated kinds of job skill requirements. Accounting is. We have a widespread, as I indicate, we try to assign individuals the jobs where their skills can be utilized and where they can receive their best help.

Senator MOYNIHAN. Yes.

Mr. ANGUS. We place three basic requirements on sponsors. One is that they operate a job site and they are willing to take persons whom

we refer to them. We tried to establish a wide variety of projects so that we can accommodate a wide variety of individuals and they have to be willing to work with the individual to try to enhance their job skills in addition to just the normal kind of work situation.

As these sponsor agreements are signed, they continue in force until either one of us wants to terminate. If a sponsor wants to get out we let them out from under the responsibility.

Let me talk for 1 minute just about cost and benefits of the program.

Senator MOYNIHAN. Very impressive, this figure.

Mr. ANGUS. The WEAT program is run almost entirely as a by-product of WIN. We don't have a separate organization in the State that deals with WEAT. We have one individual at the State level who coordinates and participates in that program. So it has been difficult for us to estimate what the actual cost is.

HEW performed an audit of our WEAT program in mid-1977. They estimated if they took the time that was spent on WEAT and allocated the cost based on the time study, approximately \$380,000 a year was expended in our WEAT project. And, also it is difficult to determine the benefits but they estimated in their same audit there was approximately \$1,500,000 worth of benefits which accrued to the State because of either case reductions or people accepting employment through that process or—

Senator MOYNIHAN. Is that why they were against it?

Mr. ANGUS. It is difficult to say.

Senator MOYNIHAN. You could put them out of business. There is only one person here from HEW. I can torment. It was HEW's audit?

Mr. ANGUS. Yes, it was a HEW audit that did this. We have not personally in the State, gone through the calculation to determine that, so we feel that it is not only a benefit in terms of helping people to getting a better feel about getting a grant but also we think there are some numerical kinds of benefits.

If you turn back to attachment 5 on the last page, I have included there an AFDC WEAT activity report for the period January through June of 1978. During that period of time you will note that we had about 3,000 persons who were screened for WEAT assignments.

Only 14 we were not able to supply a project for. We waived as they came in for the WEAT interview 608 of them, and the second series of explanations there indicate why they were waived, those 608 were waived, and you will notice that the majority of them, 458, they were waived because of a coal miner strike we had going on during that first period, if you will remember.

Senator MOYNIHAN. No sir, I don't see that.

Mr. ANGUS. In the second—

Senator MOYNIHAN. Coal miners?

Mr. ANGUS. Coal miner strike. Four hundred and fifty-eight that were waived from the assignment. In other words, we did not make all those coal miners who were on strike go out and participate in the WEAT project, basically.

Senator MOYNIHAN. You let them collect to the degree that they had ADFC-U?

Mr. ANGUS. AFDCU eligibility. The reason being they are basically from a small community in Carbon County in the eastern part of the State. We just did not have any kind of project that could take care of that kind of workload. So that is the reason.

Senator MOYNIHAN. And you didn't need any of those people. As soon as they got the strike over, there was no problem with their work behavior?

Mr. ANGUS. Nothing like that. You will note there also that there were 321 that were employed over 20 hours per week. If an individual is employed more than half time we don't require them to work on a WEAT project.

Senator MOYNIHAN. This is absorbing. I have to make one comment, which is familiar to anybody who shows up from a State like Utah before the subcommittee, as you have.

I take it your AFDC case load is about 13,000?

Mr. ANGUS. Around 13,000.

Senator MOYNIHAN. Well, in my State we have over 1 million.

Mr. ANGUS. Yes, I understand that.

Senator MOYNIHAN. I don't understand it, but I am aware of it, and you know, I guess, there are just facts resulting from the weight of these different experiences. You in your capacity could know everybody in Utah on welfare.

Mr. ANGUS. That is not true.

Senator MOYNIHAN. Well—

Mr. ANGUS. In rural areas that is true because basically most of the workers would know everyone in the community. In the Salt Lake City-Ogden-Provo area that is not so, and we deal with many people that we don't know.

Senator MOYNIHAN. Did you hear what you have just said? You said that you deal with many people you don't know. Every so often somebody comes in and you say "I am sorry, we haven't met before." I see. "Sit down, won't you."

As against urban chaos in New York, with 1 million people, but you are a neighborly State and you help each other and you know each other and it works.

Senator Long has been interested in the kind of program you have here and he has proposed systems that would allow States to do what you are doing, but he provides that the requirement be waived if the individual has good cause for declining a work assignment.

Do you have some rule of thumb principle of that sort? What sort of people do decline to work in Utah? If someone says I don't want to do that, that is not fair work for me, or not good work for me—

Mr. ANGUS. We try to the greatest extent possible to make our job assignments relate to the individual. However, if we were to come down to the nitty-gritty of an individual saying I really don't want to plant this garden in Memory Grove or wherever it may be, and he has no physical or medical or mental reason for being excluded from the WEAT project, we would in fact say that you participate or we will terminate your grant, and we do.

Senator MOYNIHAN. How often do you? I see 295 people were removed from the grant for failure to perform.

Mr. ANGUS. That is right, it happens frequently.

Senator MOYNIHAN. That is not a small number.

Mr. ANGUS. It happens frequently.

Senator MOYNIHAN. Why? And that comes under the heading of people who just said I don't like that?

Mr. ANGUS. I just don't want to work.

Senator MOYNIHAN. And you find there are such people?

Mr. ANGUS. We provide a hearing process, where when a case is closed, for that reason, that a person can request a hearing and we can determine whether or not there is some reason, valid reason for them failing to work on that WEAT project. But there are a sizable number who would just as soon have their case closed.

Senator MOYNIHAN. What happens to them?

Mr. ANGUS. We don't know.

Senator MOYNIHAN. Do you find anybody—

Mr. ANGUS. What happens to them, in many cases we find—

Senator MOYNIHAN. They go to California?

Mr. ANGUS. Could be. We find that they ultimately decide, well, they are not going to weaken on their stance so they come back and say, OK, I will work on the WEAT program.

Senator MOYNIHAN. That might be people who, if that is the case, said "I will think it over?"

Mr. ANGUS. Yes.

Senator MOYNIHAN. There are people who will not work and there are people who will not stop working.

Mr. ANGUS. That is true.

Senator MOYNIHAN. And they are on the tails of a distribution, and most people are just in between like us, who work, and are unhappy if we don't work at all, and commence at a certain point to get unhappy if you have to work too much. Everybody knows when they put in a good day's work. Beyond that it gets painful, and there is no secret about that.

You get those people who drive everybody else crazy by putting in their 20 hour day. Washington is filled with people who are driven to work 17-18 hours a day. They are a public menace, much more of a menace than the people who won't work at all. I never found the latter to do much harm.

I don't mean to seem to be difficult with HEW, but I think that the bureaucracy is really not open to this kind of effort. I am not surprised it took them 2 years, and I am glad you are suing them, because if there is anything that anybody who has the minimum knowledge of Utah knows it is that Utah is not a place where strangers starve. One of the deep religious and political traditions of the State is the reception of people arriving penniless through migration and being put to work, and these people do well. They don't become a dependent class.

It is a place where there is an ethic of community provision and a religious base as well as a political base for it. If you are going to live in the Rocky Mountains and the desert, you have to work. There are no breadfruit trees out there. But you do well and I wish the Washington bureaucracy were not so fearful of it because they bring to it a level of association that isn't appropriate to your part of the country

at all, and one of the things that they bring is a certain kind of mythology of sweatshops in the Lower East Side of Manhattan in 1905, which were also true, but that is not what goes on in Provo and Salt Lake City and we welcome this testimony. It confirms a judgment of people on this committee that the United States is big enough to let the States be different, one from the other, as they pursue their own judgments about how they like to make these arrangements, and particularly a State such as yours, sir, which has handled this problem, has not got a serious welfare problem, in part because of its serious welfare tradition. You help people and you get them to work and they get off welfare and they help other people, and it is something to be proud of. We thank you for this case history, as it were, which will be very valuable to use in the Congress to come.

Mr. ANGUS. Thank you.

[The material submitted by Mr. Angus follows:]

#### UTAH WORK EXPERIENCE AND TRAINING PROGRAM

##### BACKGROUND

In 1974, the Utah State appropriations act required that all employable welfare recipients, primarily adult males and females with children of school age must, as a condition of eligibility, accept employment or job training or take part in a community work force project. Refusal to accept employment, participate in job training or a community work force project would mean loss of assistance. Only those recipients who are determined as being unemployable because of health reasons or other extenuating circumstances are exempted.

This legislative intent became the foundation of Utah's Work Experience and Training Program (WEAT) which was submitted to the Department of Health, Education and Welfare (HEW) in 1974 and formally approved on May 14, 1976, some two years after the original submission of plan material. Utah's WEAT program is not formally incorporated into its Title IV-A preprint which is included at Attachment E. The detailed-programmatic and procedural policies for WEAT are contained in Utah's Assistance-Payments Administration Manual Volume II, 236 (Attachment 2).

The basic premise of Utah's WEAT program is that all individuals should have the opportunity of providing for their own subsistence. The work ethic in Utah is probably as high as anywhere in the nation and consequently there is a great deal of support for the WEAT program from the general public, the legislature and in most instances from the recipients and welfare rights groups.

##### PROGRAM PROCEDURES

The WEAT program is operated in coordination with the Work Incentive (WIN) program and supports that program in assisting individuals to become employable. The basic structure of the WEAT program is as follows.

The Department has established sponsors who must be either government entities or non-profit organizations willing to serve as work sites for recipients. The recipient is required to work a minimum of 96 hours at a work site in order to be entitled to their public assistance grant. Failure to perform the required number of hours on a WEAT project will result in an individual being removed from the grant and ultimately the entire case may be closed if they continue to fail to perform.

The process is as follows: At the time of application the recipient is screened for WIN status as required by CFR 224.20 to determine whether or not the individual is exempt from WIN. If an individual is found to be exempt from WIN he/she is automatically exempted from WEAT, therefore, the WEAT program considers only mandatory or voluntary WIN referrals. If the individual is determined to be a mandatory WIN referral, the individual is sent to the WIN Team for registration and appraisal and the WIN program develops an employ-

ment plan and places the individual in a suitable WIN component. Should the individual not be assigned to any of the WIN components he/she is then placed in the WIN unassigned pool and becomes a candidate for WEAT assignment. Upon being notified by the WIN team that a recipient has been placed in the WIN unassigned pool the District Assistance Payments Administration (APA) Office must determine whether or not the individual is eligible for a WEAT assignment. The individual is brought to the District APA office for an interview to determine whether or not he/she is physically and emotionally able to work and to identify relevant job skills. A decision is made at this point and the recipient may be declared exempt and no further contact is made with the WEAT program. If the individual is declared employable, an extensive effort is made to evaluate the individual's past and present job skills and an attempt made to assign the individual to a WEAT project which could most effectively utilize and enhance the recipient's skills. The Department does not make referral to specific job sites but to the sponsor who assigns the individual to a specific job which is as closely aligned to his/her skills and needs as possible. The sponsor then keeps track of the recipient's time and reports to the local District APA Office his/her monthly time which in turn is taken into consideration in determining eligibility.

As an incentive to participation in the WEAT program, a \$25 allowance is made to cover the cost of transportation, meals, and etc. This allowance may be increased above \$25 provided the recipient can prove greater related expenses.

If a recipient performs less than the required 96 hours per month, the District APA offices evaluate the reason for lack of performance and may invoke sanctions as follows:

If the recipient is an unemployed father he will be removed from the grant and the WEAT allowance will be discontinued. He is immediately notified that he must contact the District APA office and discuss his failure to perform. If he fails to contact the APA office, he is deregistered from WIN and the entire case is closed.

If regular AFDC recipients fail to perform the required 96 hours per month, the WEAT allowance is discontinued and the household size is reduced by one person.

A recipient who has had a sanction can, by working for two weeks on a WEAT project have their full grant immediately reinstated.

As required in 45 CR 205.10 the individuals against whom a sanction is taken are given the 10-day notice prior to action and has the opportunity to appeal the decision.

#### WEAT SPONSORS

There are approximately 300 sponsors located in 600 job sites through the State who have signed contracts with the Department of Social Services (see Attachment 3). The Department has established three basic objectives in developing project sponsors for the WEAT program.

1. To establish work projects in the geographical areas in which clients live so transportation would not be a hardship.

2. To establish a wide variety of work projects so the client's work assignment could match, as nearly as possible, whatever skills they possessed.

3. To increase their skills or develop new ones to enhance their chance of employment.

Once the sponsor agreements have been signed they are ongoing until either party decides to terminate. Attachment 4 describes in part the types of WEAT jobs that are available throughout the State.

#### COST AND BENEFITS

The Utah WEAT program is run almost as a by-product of the WIN program, hence it is difficult to determine the actual costs associated with the program. HEW auditors conducted an audit of the WEAT program in mid-1977 and estimated that the costs allocable to the WEAT program approximated \$380,000 per year. They also determined case closures and grant reductions attributable to the WEAT program amounted to approximately \$1,525,000. A cost benefit ratio of 4:1 Attachment 5 reflects the WEAT activities for the six-month period ended June 31, 1978.



During this period 3,052 persons were screened for WEAT placement; 1,608 or 52 percent were waived from assignment; and an additional 202 or 7 percent had their cases closed prior to actual assignment. The remaining 1,242 or 41 percent were assigned and worked on WEAT projects. This represents about 10 percent of our AFDC caseload.

During this same time period, 1,829 persons left the WEAT program, 169 or 18 percent obtained full-time employment, 169 or 13 percent obtained part-time employment, 222 or 17 percent had their case closed because they failed to work the required hours; 282 or 21 percent entered back into a WIN component, and the remaining 36 percent were closed or exempted from further WEAT participation.

#### RELATIONSHIP TO WIN

Effective October 1, 1978, the WEAT program has been assimilated into the WIN program as one of the WIN components. This will serve to provide closer coordination with the ongoing WIN process.

The State of Utah's experience with the WEAT program has been most positive. Many welfare recipients do not have the very basic skills needed to begin to provide for themselves. While many of the work project jobs may be of menial nature, a great deal can be gained by many recipients in learning how to accept the responsibility of showing up for a job, following instructions, and seeing a job well done.

One of the minor problems encountered in the WEAT program is that individuals become too comfortable with the process and want to stay on a work project and continue to draw a welfare grant. In addition to performing a valuable service for recipients, the work projects have extensively benefited the community. The State Department of Transportation estimates that work projects saved the State \$85,000 each year. School districts, cities, counties and towns report similar experiences with work projects. We in Utah believe that the WEAT program is the first step for many recipients to becoming self-supportive. It works!!

IV-A STATE OF UTAH

Additional Eligibility Conditions Permissible under 45 CFR 233.10(a)(1)(ii) (B) and (b)(1)

1. State employment and employability conditions (not related to WIN (IV-C) or to Non-support (IV-D) cases)

[ ] No provisions

[x] Employable members of the AFDC assistance unit must:

[x] Not refuse employment or training under these specified conditions: AFDC - Work Experience and Training

[x] Maintain current registration with the State employment service.

[x] Show that they are actively seeking employment.

[ ] Other \_\_\_\_\_

The conditions checked above are applicable to:

[x] Parent. [ ] Other caretaker relative, under the following safeguards: \_\_\_\_\_

[x] Child age 16 or older, under the following provisions: AFDC - Work Experience and Training

[ ] Other \_\_\_\_\_

(continued)

Approval Date SEP 29 1976 Effective Date JUL 1 1975

SEE WINN XXXX  
dated 12-22-76.

Attachment 2.2D  
Page 2

IV-A STATE OF UTAH

-----  
Failure to comply with the conditions checked above results in --  
Removal of person from grant.

Criteria for determination of employability and "good cause" in this context are contained in State instructional material transmitted to the Regional Office.

- 2. State provisions applicable to incapacitated parent (not related to WIN, IV-C, cases)
  - No provision
  - Yes, the following provisions apply:

Failure to comply with the provisions reported above results in:

(continued)

-----  
Approval Date SEP 29 1976 Effective Date NOV 1 1976

236 AFDC and Work Experience and Training (WEAT)236.1 WIN Assignees to WEAT

1. All mandatory WIN registrants who are in Unassigned Recipient Status shall be assigned to a WEAT program.
2. A person who is exempt from WIN only because he lives too far away from a project to participate effectively shall be assigned to a WEAT program if one is available.
3. A WIN registrant placed in the "Suspense to Training" component shall be assigned to WEAT if the assignment is requested on Form MA5-97, WIN Status Change Notice.

Procedures and requirements for this assignment are found in Section 236.4.

236.2 Required Performance

1. An individual assigned to WEAT shall be required to perform ninety-six hours per month, averaging twenty-four hours (three days) per week. The performance period shall be from the 16th of one month through the 15th of the next month.
2. Each week, sixteen hours (two days) is available to the project assignee for job search activities utilizing the employment service, as well as training activities for good work habits, completing the job applications, and other activities designed to enhance employability and self-support capabilities.
3. Each person assigned shall be given a Form WP-1, Work Experience and Training Assignment.

236.3 WEAT Assignees due to WIN Unassigned Recipient Status or WIN Exempt due to Distance

These persons assigned to WEAT shall be given a Form 742, To All AFDC WEAT Clients, at the time the assignment is made.

236.31 Unsatisfactory Performance of an AFDC-U Father

If an AFDC-U father placed in unassigned recipient status is assigned to WEAT and fails to perform satisfactorily, according to the WP-2, WEAT Time Sheet, use the following procedures:

1. Remove the father from the financial/medical case effective the end of the month, discontinue the WEAT allowance, and reduce the household grant to the amount for the remaining household members.
2. Send him a hand-issued 228 notice that a contact with APA, acceptance of a new WEAT assignment, and two weeks of satisfactory performance are required before he can be added back into the grant.

The notice must also state that if he fails to contact the APA office by the first of the next month, or accepts a WEAT assignment and again fails to perform satisfactorily, he will be deregistered from WIN and the entire case will be closed as the household will be ineligible.

3. If he fails to contact the APA office in the specified time, APA will immediately notify WIN and request an MA5-97 deregistration. Upon receipt of the deregistration, close the case.
4. If a new WEAT assignment has been made, the APA worker will contact WIN after the AFDC-U father has had two weeks to perform on the project and will apprise WIN of the status of the case.
  - a. If the recipient failed to perform satisfactorily, unless excused, WIN will immediately deregister the AFDC-U father and submit the MA5-97 to APA. Close the case effective the end of the month.
  - b. If the recipient performed satisfactorily, he shall be added back into the financial/medical case effective the date of removal. Issue a supplemental check for the amount deducted due to his removal.

236.32 Unsatisfactory Performance of Other Assignees

When the District office receives Form WP-2, Work Experience and Training Time Sheet, after the 15th of each month, those persons who performed less than the full ninety-six hours per month without acceptable excuse shall be removed from the financial/medical case. Discontinue the WEAT allowance and reduce the household grant to the amount for the remaining household members. Send a 228 notice.

1. If the assignee is a medically needy child, application may be made for medical assistance only.
2. If a person who performed unsatisfactorily states he will perform satisfactorily and does so for two weeks, the grant may be increased to the full standard grant, and a supplemental check may be issued.

236.4 WEAT Assignees in WIN Suspense to Training

When a WIN MA5-97 is received indicating that a WEAT assignment is to be made or to continue, the assignment shall be for the full ninety-six hour performance requirement by using Form WP-1.

1. The WIN agency has full responsibility to monitor the performance, allow any exemptions, agree to change assignments, or apply any sanctions for non-performance.

It is also responsible to notify the recipient that the WEAT assignment is a WIN training program.

2. No APA WEAT allowance will be given as WIN will pay TRE and incentive payments while the recipient performs on the project.
3. When the District office receives Form WP-2, WEAT Time Sheet, after the 15th of each month, the hours reported by the foreman on these assignees shall be posted on the provided operational report and immediately submitted to the WIN office.

236.5 Excused Time and WEAT Exemptions

Form 568, Work Experience and Training Exemption, shall be completed for every person excused or exempt from WEAT assignment, except refugees.

- a. An individual is exempt if WIN has assigned him to training or employment.
- b. An individual may be excused if he provides the District Office with a statement from a physician or a licensed/certified psychologist verifying illness or incapacity.
  - 1) If the illness or incapacity appears to be of a serious or long-term nature, the District Office may explain to the individual that he may be eligible for Supplemental Security Income benefits.
- c. If an individual is referred by the Department of Job Services for a job interview, the individual may be excused for the time necessary to complete the interview.
  - 1) No excuse will be given if the job interview occurs during the two working days of a week the person is not assigned on the WEAT Project.
  - 2) If the job interview occurs during the three-day period that the person is assigned to perform on the project, the person shall be excused for the interview and shall be credited for performance on the project for the full excused period.
- d. An individual is exempt who is working more than twenty hours per week.
- e. The mother of school-age children is exempt during school vacations of more than one week.
- f. An individual is exempt if there is no WEAT project available.
- g. An individual is exempt from assignment to WEAT if age 60 or older.
- h. The District Director in conjunction with the WEAT sponsor may determine an individual is exempt through unusual or extenuating circumstances which prevent his performance on the project.

WORK EXPERIENCE AND TRAINING ASSIGNMENT

Case Number \_\_\_\_\_

Category \_\_\_\_\_

(1) TO: W.E.A.T. Foreman:

This is to certify that \_\_\_\_\_ is  
eligible for assignment to Work Experience and Training for  
\_\_\_ hours each month.

Date \_\_\_\_\_

\_\_\_\_\_  
(District Director or Authorized Representative)

APA District \_\_\_\_\_

-----  
(2) TO: Recipient:

To complete this assignment, present this statement to  
\_\_\_\_\_ at  
(Foreman of Project)

\_\_\_\_\_ on  
(Address)

\_\_\_\_\_ at \_\_\_\_\_ who will assign you to  
(Date) (Time)  
and supervise your work.

Final Action:

- (1) \_\_\_ To recipient for delivery to Project Foreman.
- (2) \_\_\_ Copy retained in case folder. (Replace with new one)
- (3) \_\_\_ Advance copy mailed to Project Foreman.
- (4) \_\_\_ To Division of Family Services (AFDC only)
- (5) \_\_\_ To Sponsor of Work Experience and Training Project.



INSTRUCTIONS FOR FORM WP-1

**PURPOSE:** This form shall be used by the District APA offices to inform an applicant/recipient and the WEAT foreman of an assignment to a Work Experience and Training Project.

**PREPARATION:** Prepare an original and one copy as follows:

Section (1) To WEAT Foreman: Self-explanatory.

Section (2) To Recipient:

Enter name of project foreman and the project name on the first line.

Enter exact address of project, including any room number assignment.

Enter date and time the client is to report to the foreman and project assigned.

If the person assigned has a medical limitation indicated by a physician that should be considered in determining project assignment and performance, include this information on the form.

**DISTRIBUTION:** As indicated on bottom of form.

**RETENTION:** Discard after completion of assignment.



**INSTRUCTIONS FOR FORM WP-2**

**PURPOSE:** The W.E.A.T. Foreman shall use the form WP-2 to report the hours worked to the District APA office.

The District APA office shall use the information reported on the WP-2 to determine hours performed and continued eligibility.

**DISTRIBUTION:** The W.E.A.T. Foreman shall complete an original and one copy of the WP-2. The recipient should sign the form indicating his agreement with the hours reported. The original shall be forwarded to the District APA office and the copy retained for the W.E.A.T. files.

**PREPARATION:** Self-explanatory.

**RETENTION:** One year.

WORK EXPERIENCE AND TRAINING TERMINATION

Date \_\_\_\_\_

To: \_\_\_\_\_  
\_\_\_\_\_

This is to notify you that:

Name \_\_\_\_\_ Case Number \_\_\_\_\_

Address \_\_\_\_\_ has terminated his Work  
Experience and Training Assignment because of: \_\_\_\_\_

\_\_\_\_\_

It is understood that the above person is not eligible for a future assignment  
unless he is again certified by the Office of Assistance Payments.

Signed \_\_\_\_\_

Office \_\_\_\_\_

INSTRUCTIONS FOR FORM WP-3

USE: This form is used by either the W.E.A.T. supervisor or the APA worker to terminate a W.E.A.T. assignment.

DISTRIBUTION: Two copies are prepared. The original goes to or remains with the project supervisor and the duplicate is retained in the case record.

GENERAL INSTRUCTIONS: Form WP-3, Work Experience and Training Termination, is prepared by the sponsoring agency and submitted to the District APA office, whenever a person who has been certified for W.E.A.T. has terminated.

In those instances where the District APA office has knowledge of the termination prior to the sponsoring agency it may use the same form to notify the sponsoring agency that the person previously certified will not be available for W.E.A.T.

PREPARATION: In the spaces provided, enter date of preparation, name and address of person to whom notice is directed, name, case number, and address of recipient, and reason for terminating W.E.A.T. Form WP-3 should be signed by the APA worker or project supervisor, whoever originates it.

RETENTION: Destroy after three years.

Utah-DSS-APA

Form Number WP-4  
1-1-76

## WORK EXPERIENCE AND TRAINING AGREEMENT

The \_\_\_\_\_, a government or private, non-profit unit, hereafter referred to as the SPONSOR, hereby enters into an agreement with the Department of Social Services, Office of Assistance Payments Administration, hereafter referred to as APA to conduct a work experience and training project.

## APA AGREES:

1. To refer, if available, the agreed upon number of employable trainees to the Sponsor for training and work experience furnished by the Sponsor.
2. To furnish time sheets (Form WP-2) upon which the Sponsor will enter the trainee's daily work performance record.
3. To pay trainees.

## SPONSOR AGREES:

1. To provide a productive work project wherein APA trainees can learn vocational skills and gain work experience.
2. Not to replace regular employees with APA trainees.
3. To furnish all equipment and materials necessary to insure the continuing accomplishment of the project and training objectives.
4. To accept only that number of APA trainees who can be utilized productively.
5. To assure competent, intensive supervision and training.
6. To prepare and forward monthly (no later than the \_\_\_\_\_ and \_\_\_\_\_) to the local APA office time sheets (Form WP-2) for each trainee certifying the days and actual hours of gainful trainee time spent in the activity.
7. To grant any trainee assigned to a project the time off necessary to report to prospective employers for employment interviews.
8. To pay all insurance, required by State or Federal law, for the protection of all trainees while engaged in project activity.

APA trainees participating in project activity are not employees of the Sponsor or Department, but are recipients of Public Assistance and are not compensated at an hourly rate for the work done.

This agreement entered into this \_\_\_\_\_ day of \_\_\_\_\_, 19\_\_\_\_.  
SPONSOR:

BY: \_\_\_\_\_ Title: \_\_\_\_\_

Department of Social Services  
ASSISTANCE PAYMENTS ADMINISTRATION      District: \_\_\_\_\_

BY: \_\_\_\_\_ Title: \_\_\_\_\_

Utah-DSS-APA

Form WP-4  
1-1-76

## INSTRUCTIONS FOR WP-4

**PURPOSE:** The revised WP-4 will be used to complete new Work Experience and Training Project Agreements to replace the Work Project Agreements.

**DISTRIBUTION:** The State Work Experience and Training Coordinator will complete an original and two copies of each agreement. The original will be filed at the State Office with one copy maintained at the District APA office and the other copy at the Sponsor.

**PREPARATION:** Only the State Work Experience and Training coordinator or his designee will complete the WP-4.

**RETENTION:** Upon completed use.

WIN STATUS CHANGE NOTICE										1. SOCIAL SECURITY NO.	
TC		CC 2. LOCAL OFFICE NO.		3. NAME (Last, First, Middle Initial)							
1. Dates: 2. Add 87		12-16 16		16		17		32			
4. CASE NAME										5. CASE NUMBER	
9. APPLICABLE: 1. Services from both agencies required 2. SNJ services only required 3. ES services only required 4. No services required 5. Other (Specify)										7. DATE STATUS CHANGED (M, Day, Yr)	
11. ENTERED EMPLOYMENT										CODE	
10. RECIPENT STATUS										12. TAX STATUS	
Code ENTER LEAVE STATUS										13. DAYS	
1. Working Register Status										14. 30 DAYS	
2. Suspense to Employment										15. 60 DAYS	
3. Intensive Support Services/ Orientation										16. 90 DAYS	
4. Federal Use										17. 180 DAYS	
5. Institutional Training										18. 365 DAYS	
6. Work Experience										19. NONE	
7. WIN/QJT Subsidized											
8. WIN/PSE											
9. Suspense to Training											
10. Part Time Employment											
11. Other WIN Non-Component Activity											
12. Unassigned Recipient											
13. SUPPORTIVE SERVICES PROVIDED FOR CERTIFICATION											
14. ADJUDICATION AND SUPPORTING ACTIVITIES											
15. DEMONSTRATION											
16. TAX CREDIT AUTHORIZED											
17. 30-DAY FOLLOW-THROUGH											
18. STATE USE											
19. COMMENTS											
20. WIN STAFF INFORMATION											



Utah-DSS-APA

Form MA5-97  
8-78INSTRUCTIONS FOR FORM MA5-97

PURPOSE: Form MA5-97 is used by WIN staff at the Department of Employment Services to report the WIN status of AFDC recipients to APA.

The APA office shall be alert to a status change notice and take action as appropriate.

RETENTION: Three years.

B-78-30-APA

WORK EXPERIENCE & TRAINING EXEMPTION

District \_\_\_\_\_

CASE NAME \_\_\_\_\_ CASE NUMBER \_\_\_\_\_

NAME OF AFDC or GA ASSIGNEE: -----

- 1. WIN Component \_\_\_\_\_
- 2. Incapacity or Illness \_\_\_\_\_ Duration: \_\_\_\_\_
- 3. Referred for Employment \_\_\_\_\_ Date \_\_\_\_\_
- 4. Employed (excess of 20 hrs/week) \_\_\_\_\_
- 5. Referred to WIN Training/Employment \_\_\_\_\_ Date: \_\_\_\_\_
- 6. No Project Available \_\_\_\_\_
- 7. No Project within reasonable distance \_\_\_\_\_
- 8. Referred to Job Interview \_\_\_\_\_ Date: \_\_\_\_\_
- 9. Mother of School-aged Children on Vacation \_\_\_\_\_ Dates: \_\_\_\_\_
- 10. Other as designated by Director \_\_\_\_\_

Date(s): \_\_\_\_\_

\_\_\_\_\_  
SIGNED

\_\_\_\_\_  
DATE

Filed in Case Record:

Date: \_\_\_\_\_

\_\_\_\_\_  
Signature of Worker ID No.

## INSTRUCTIONS FOR FORM 568

**PURPOSE:** The District APA Office shall use form 568 to document AFDC and GA Work Experience & Training project exemptions.

**DISTRIBUTION:** The District APA Office shall complete one copy of Form 568 to be placed in the case record.

**PREPARATION:** Self-explanatory. For exemption reasons explain. For example; if item 6 is the reason, give an explanation.

**RETENTION:** One year.

Utah-DSS-APA

Form 742  
8-78TO: All AFDC WEAT Clients  
FROM: District APA Office

1. All mandatory WIN registrants in the Unassigned Recipient status and those recipients exempt from WIN due to distance may be placed in the Work Experience and Training Program. (Full cooperation with WIN is expected to continue.)
2. The period assigned will be 96 hours a month. The performance period is from the 16th of a month through the 15th of the following month.
  - A. Each week the person assigned to a WEAT Project shall perform an average of three days (24 hrs) on that project.
  - B. Each week two days (16 hours) is expected to be used for job search and training activities through the employment office and the WIN counselors.
3. The mandatory WIN registrant may be excused from the WEAT project, although continued cooperation with WIN is expected, when:
  - A. WIN assigns him/her to a job training program or requires a WIN interview;
  - B. He/she provides the Assistance Payments Office with a doctor's statement verifying that incapacity or illness prevents performing on the WEAT project; or
  - C. She is the mother of school-age children who are on school vacations of more than one week.
4. An additional allowance of \$25.00 will be added to the grant each month to cover transportation, snacks, and special clothing necessary to perform on the project.
5. On the 15th of each month, the WEAT project foreman will submit a time sheet to the Assistance Payments Office. Satisfactory performance is a full 96 hours per month unless excused by one of the reasons listed in item 3.
6. If a child or mother does not perform satisfactorily, they will be removed from the household financial and medical assistance case effective the following month. In order to be added back onto the grant and medical case, the person removed must contact the Assistance Payments Office and accept a new WEAT assignment or verify the reason for non-performance. If a child removed has medical needs, a new application may be made.
7. If an unemployed father does not perform satisfactorily, the following actions will be taken:
  - A. He will be removed from the financial/medical case at the end of the month and the household will receive a reduced financial grant the following month.
  - B. At the end of this following month, the total case will be closed as he will be de-registered from WIN and the entire household will be ineligible for AFDC-Unemployed Father's assistance.
8. You have the right to appeal your WEAT assignment or any other decision with which you disagree. (Contact the local District APA Office for Hearing Rights information.)

Client's Signature \_\_\_\_\_ Date \_\_\_\_\_

Instructions for Form 742

- PURPOSE: Form 742 is used to inform an AFDC WEAT assignee of the individual's rights and responsibilities according to WEAT regulations.
- PREPARATION: For information only. The client shall sign an original with one copy to indicate understanding.
- DISTRIBUTION: The original shall be filed in the case record. The copy shall be given to appropriate clients with a completed Form WP-1.
- RETENTION: Three years.

WP-4

BY DISTRICT

USHER T. WEST

## WP-4 DISTRICT 1

Alcoholic Rehabilitation Center  
Army Opportunities  
APA District Office - Brigham City  
APA District Office - Logan  
Bear River Community Action Agency  
Box Elder County Government (Court House)  
Box Elder School District  
Brigham City Corporation  
Cache County Corporation  
Division of Corrections  
DFS Office - District 1  
Logan City  
Logan School District  
Pioneer Memorial Nursing Home  
Rich County School District  
Smithfield City  
State Department of Highways - District I Engineer  
Sunshine Terrance Nursing Home  
Tremonton City  
Utah National Guard - Brigham City  
Utah National Guard - Garland  
Utah National Guard - Logan  
Utah National Guard - Smithfield  
Utah State Parks and Recreation Division  
Utah Wildlife Resources Division

## WP-4 DISTRICT 2A

Alcoholism Recovery Center  
Assistance Payments District 2A  
Browning Armory  
Division of Rehab. Services  
Eccles Community Art Center  
Family Services District 2A  
Golden Hours Center  
Internal Revenue Ogden Service Center  
Morgan County Commission  
Morgan School District  
Ogden City Corporation  
Ogden City Schools  
Ogden Rescue Mission  
Office of Recovery Services  
Roy, City of  
Salvation Army  
State Department of Highways - District I Engineer  
Utah School for Deaf and Blind  
Utah State Parks and Recreation Division  
Utah Welfare Right Organization  
Utah Wildlife Resources Division  
Weber Council of Spanish Speaking Organizations  
Weber County Corporation  
Weber County School District  
Weber County Sheltered Workshop  
Weber Mental Health Center  
Weber State College  
Young Women's Christian Association



## WP-4 DISTRICT 2B

Adult Day Care Supervision and Treatment Center  
Alcoholic Rehab. Center  
American Civil Liberties Union Utah Chapter  
Army Aviation Support Facility  
Assistance Payments  
APA Office - Tooele  
Baptist Concern Center  
Central City Neighborhood Council  
Community Action  
Community Services Council  
Cross Roads Urban Center  
Department of Employment Security  
Department of Finance (Central Stores)  
Division of Corrections  
Division of Corrections - Probation Office  
Family Services  
First Step House  
General Services  
Goshute Tribal Council  
Governor's Mansion  
Grantsville City Corporation  
The Haven  
House of Hope  
Housing Authority of the County of Salt Lake  
Indian ARC  
Indian Walk-IN  
Interagency Information Resource Center  
Jobs for Progress, Inc.  
Jordan School District  
Magna Community Council  
Midvale City  
Murray-Jordan-Tooele Mental Health Center  
Murray National Guard  
Murray School District  
NAACP  
Nettie Gregory Community Center  
National Guard Armory  
Office of Recovery Services  
People's Freeway  
Pioneer Craft House  
Project Reality  
Redwood Community Council  
Riverton Town  
Rocky Mountain Navajo Housing Development Corporation  
Salt Lake City Corporation  
Salt Lake City Library

WP-4 DISTRICT 2B  
Page 2

Salt Lake City School District  
Salt Lake County  
Salt Lake County Cottonwood Sanitary Sewage District  
Salt Lake County Library System  
Salt Lake County Tenant Organization  
Salvation Army  
Sandy City  
Second District Juvenile Court  
Secretary Of State  
Society of Good Neighbors  
South Jordan City  
South Salt Lake City  
Special Forces Armory  
State Law Library  
State Department of Highways - District II Engineer  
St. Mary's Home  
Tooele City Corporation  
Tooele County  
Tooele School District  
University of Utah Personnel Administration  
Utah Air National Guard  
Utah Alcoholic Foundation  
Utah Coalition of Senior Citizens  
Utah Legal Services  
Utah Native American Consortium  
Utah National Guard Armory - Army Aviation Support Facility  
Utah National Guard Camp Williams  
Utah National Guard Tooele  
Utah State Chapter National Multiple Sclerosis Society  
Utah State Fairgrounds  
Utah State Parks and Recreation Division  
Utah State Tax Commission  
Utah State Visually Handicapped Services  
Utah Technical College at Salt Lake City  
Utah Welfare Rights Organization  
Utah Wildlife Resources Division  
VA Hospital  
Veterans in Community Service, Incorporated  
West Jordan City  
Westminster College  
Westside Community Council  
Westside Council  
Work Activities Center for Handicapped Adults

WP-4 DISTRICT 2C

Bountiful City  
Centerville City Corporation  
Clearfield City  
Davis County Alcoholism Program  
Davis County Community Action Program  
Davis County Courthouse  
Davis County Housing Authority  
Davis School District  
Layton City  
North Salt Lake City  
Utah National Guard - Layton

## WP-4 DISTRICT 3

Alcohol Recovery Center  
Alpine House  
Alpine School District  
American Fork City Corporation  
APA Office District 3  
Big Brothers and Sisters of Utah County  
Division of Corrections  
Division of Family Services - #3  
Division of Rehab. Services  
Goshen City  
Kamas City Corporation  
Lehi City Corporation  
Midway Town Corporation  
Nebo School Board  
North Summit School District  
Office of Recovery Services  
Orem City Corporation  
Park City Municipal Corporation  
Park City Recreation Department  
Park City School District  
Payson City  
Pleasant Grove City  
Provo School District  
Santaquin City  
Spanish Fork City  
Springville City  
State Department of Highways - District VI Engineer  
Timpanogos Community Mental Health  
Utah County  
Utah County Action  
Utah County Council on Drug Abuse Rehabilitation  
Utah County Department of Animal Regulation  
Utah National Guard - American Fork  
Utah National Guard - Lehi  
Utah National Guard - Provo  
Utah National Guard - Spanish Fork  
Utah National Guard - Springville  
Utah State Hospital  
Utah State Parks and Recreation Division  
Utah State Training School  
Utah Technical College at Provo  
Utah Welfare Rights Organization  
Utah Wildlife Resources Division  
Wasatch County

## WP-4 DISTRICT 4

Circleville City  
Delta City  
Division of Corrections  
Ephraim City  
Fairview Museum Corporation  
Fillmore City  
Juab School District  
Kanosh City  
Manti City Corporation  
Millard County  
Millard School District  
Moroni City  
Mount Pleasant City Corporation  
Nephi City  
North Sanpete School District  
Office of Recovery Services  
Salina City  
Sanpete County  
Sevier County  
Sevier School District  
South Sanpete School District  
State Department of Highways - District III Engineer  
Tintic School District  
U. S. Forest Service, Manti-LaSal National Forest  
Utah National Guard - Fillmore  
Utah National Guard - Manti  
Utah National Guard - Mt. Pleasant  
Utah National Guard - Nephi  
Utah National Guard - Richfield  
Utah State Parks and Recreation Division  
Utah Wildlife Resources Division  
Wayne School District  
West Millard Hospital

## WP-4 DISTRICT 5

Alcoholic Rehabilitation Center  
Beaver City  
Beaver County  
Beaver School District  
Cedar City Corporation  
Division of Corrections  
Dixie State Park  
Garfield School District  
Iron County School District  
Kanab City  
Kane School District  
Panguitch City  
Parowan City  
St. George City  
State Department of Highways - District V Engineer  
Utah National Guard - Beaver  
Utah National Guard - Cedar City  
Utah National Guard - St. George  
Utah State Parks and Recreation Division  
Utah Wildlife Resources Division  
Washington City  
Washington County School District

## WP-4 DISTRICT 6

Divison of Corrections  
Duchesne School District  
Family Services District 6  
State Department of Highways - District VI Engineer  
Utah National Guard - Vernal  
Utah State Parks and Recreation Division  
Utah Wildlife Resources Division  
Uintah Basin A.O.G.  
Uintah Basin A.R.C.  
Uintah School District  
Uintah Basin Social Services  
Ute Tribal Council

## WP-4 DISTRICT 7A

Alcohol Rehabilitation Center  
Carbon County  
Carbon Hospital  
Carbon School District  
College of Eastern Utah  
Community Action Program  
Division of Corrections  
East Carbon City  
Elmo Town  
Emery Town  
Emery County  
Emery County Nursing Home  
Emery School District  
Ferron City  
Grand School District  
Green River City  
Helper City Corporation  
Huntington City  
Moab City Corporation  
Office of Recovery Services  
Price City Corporation  
Scofield Town  
State Department of Highways - District IV Engineer  
Sunnyside City  
Utah National Guard - Price  
Utah State Parks and Recreation Division



## WP-4 DISTRICT 7B

Aneth Boarding School  
✓ Blanding City  
Division of Corrections  
Mexican Hat Elementary School  
Montezuma Creek Elementary School  
Monticello City  
Monument Valley Hospital  
ONEO (Utah) Headstart Program  
Red Mesa School  
San Juan County  
San Juan School District  
State Department of Highways - District IV Engineer  
Teecnospos Boarding School  
Utah State Parks and Recreation Division

TABLE 2  
Sampling of Jobs assigned to WEAT candidates

ACCOUNTING	PAINTER
ANIMAL CONTROL HELPER	PLUMBER'S HELPER
CARPENTRY	POLICE DISPATCHER
CLERICAL	PRINTER
COMMUNITY WORK	PROBATION COUNSELLOR
COMPUTER AIDE	RANGER'S AIDE
CUSTODIAL	ROAD MAINTENANCE
DETENTION CENTER AIDE	ROOFER
FILING CLERK	SANITATION WORKER
FISH HATCHERY HELPER	SECRETARY
GROUNDS KEEPER	SENIOR CITIZEN ATTENDANT
JANITOR	SUPPLY CLERK
JOURNALIST	TEACHER'S AIDE
KITCHEN AIDE	TRUCK DRIVER
LAUNDRY WORKER	TYPIST
LIBRARIAN AIDE	UPHOLSTERER
GENERAL MAINTENANCE	WAREHOUSEMAN
MECHANIC HELPER	WELDER
MENTAL HEALTH AIDE	YOUTH COUNSELLING
NURSES AIDE	

## AFDC WEAT ACTIVITY

ATTACHMENT 5

January 1, 1978 to June 30, 1978

TABLE I

Total Screened For WEAT Assignment	Total WEAT Assignment Waived	Total Closed Prior to WEAT Assignment	Total Assigned to WEAT
3,052	1,608	202	1,242

TABLE II

## Reasons WEAT Assignment Waived

Medical Reasons of	Short Duration	Employment over 20 Hours A Week	No Suitable WEAT Project	Became Exempt from WIN	Entered a WIN Component	School Age Child Summer Vacation	Voluntary WIN Registrants	Coal Miners on Strike	Other Reasons	Total WEAT Assignments Waived
	134	321	14	76	132	135	243	458	95	1,608

TABLE III

## Reasons Closed Prior to WEAT Assignment

Already Working Full-Time	Client's Request Which Informed of WEAT	Failure to Respond to Request	Preferred Step-Child Assistance Only	Separations	Receiving Unemployment Compensation in Excess of Grant	Other Reasons	Total Closed Prior to WEAT
46	26	27	3	7	9	84	202

TABLE IV

## Reasons Clients Left WEAT program

Removed From Grant - Failure to Perform	Added Back to Grant	Not Removed from Grant	Closed - Hired by Sponsor	Closed - Obtained Full-Time Employment	Employment Grant Reduced	Entered WIN Component	Became Exempt from WIN	School Age Children-Summer Vacation	Closed - Other Reasons	Total Leaving WEAT
295	73	222	8	161	169	282	111	175	201	1,329

Senator MOYNIHAN. Is Dr. Maxfield here? Dr. Maxfield, you are very generous to be able to be here. We are running a little ahead of time and I would like not to have an afternoon session, if we can, and you are here, and I am here, and we would very much appreciate the chance to hear from you.

Dr. Maxfield, of course, is associated with Mathematica Policy Research, and that is the organization which did the original analysis of the first income guarantee experiment, that in New Jersey.

**STATEMENT OF MYLES MAXFIELD, JR., AND DAVID EDSON, ECONOMISTS, MATHEMATICA POLICY RESEARCH, INC.**

Mr. MAXFIELD. Yes sir, thank you, Mr. Chairman.

I have accompanying me today Mr. David Edson, who participated with me in the investigation that I will report on.

Senator MOYNIHAN. Mr. Edson, you are very welcome.

Mr. MAXFIELD. We are grateful, Mr. Chairman, for the opportunity to report to you the results of our investigation into the fiscal impact of welfare reform on States. The research was, as you mentioned, conducted at Mathematica Policy Research under contract with the Department of Health, Education and Welfare.

However, today we are, of course, not speaking on behalf of either organization. What I would like to do first is describe to you the investigation that we performed, then very briefly to describe to you how it was done, and then to offer some speculation on the applicability of our results to the program for better jobs and income and for the welfare reform proposal of Senators Baker, Bellmon, Ribicoff, and Danforth.

We addressed three questions. One was how many Federal dollars would be expended in each State under a welfare reform program.

Second, how much fiscal relief would each State experience under such a program. And, last, if the additional Federal expenditures were to be financed by a Federal income tax surcharge, what would be the change in the net income of residents in each State? This last question views the residents of each State as a group which pays tax dollars into the income transfer system and which receives welfare benefits from the system. We sought to estimate this net amount of income flowing from and to the residents of each State.

Senator MOYNIHAN. Just to make sure we get this in the record, this is profoundly important. You are assuming, for purposes of the model, that there is a tax surcharge. Whatever the cost is we raise the extra money, then we distribute it.

This really isn't very different from saying that the existing revenue system brought in "x" amount of money, then it was distributed. It would come from about the same places the surcharge would bring it from, would it not?

Mr. MAXFIELD. That is hard to say. Different methods of generating revenue are distributed differently geographically by State.

Senator MOYNIHAN. I guess my question would be you assume the revenue that would be collected through the surcharge would be collected in the same proportions that the existing revenue system collects?

Mr. MAXFIELD. That is correct. I will speak in somewhat more detail to this aspect.

Senator MOYNIHAN. Fine, because you have dynamite here, as you probably know.

Mr. MAXFIELD. We attempted to answer these three questions for a particular welfare reform program, which is simpler in several aspects than many of those which have been proposed. Initially we tested a reform which replaces the aid to families with dependent children, the food stamps, and the supplemental security income programs, with a negative income tax for all families whose income is below a specified level. The negative income tax was specified by a uniform guaranteed minimum income equal to a percentage of the Federal poverty level such that it varied by family size but it did not vary State to State.

The second parameter was the rate by which the payment was reduced as earned income increased, which we called the benefit reduction rate. We made the assumption in our study that all families which are eligible for this negative income tax would participate in it.

The final feature of the tested welfare reform was that States were assumed to supplement the uniform Federal basic grant in order to guarantee that each family currently receiving welfare benefits would receive a payment under the reform program that is either the same or higher than that which the family is currently receiving.

Senator MOYNIHAN. How much higher?

Mr. MAXFIELD. In other words, we held families harmless.

Senator MOYNIHAN. So hold harmless, not higher?

Mr. MAXFIELD. We did not guarantee that any family's payment would be higher.

Senator MOYNIHAN. That is right.

Again, your basic grant was to be—

Mr. MAXFIELD. I am not sure I can recall for a family of four. It was the 1975 Department of Commerce poverty level.

Senator MOYNIHAN. What proportion of that, what was your proportion of that?

Mr. MAXFIELD. We tested three different percentages of that number.

Senator MOYNIHAN. Right.

Mr. MAXFIELD. Fifty percent, seventy-five percent, and one hundred percent.

Senator MOYNIHAN. Right.

Mr. MAXFIELD. Most of my remarks today will be about that middle guarantee.

Senator MOYNIHAN. About the 75 percent?

Mr. MAXFIELD. Yes, sir.

Senator MOYNIHAN. That is close to the administration proposal, which was 65 percent.

Mr. MAXFIELD. Yes, sir, that is my understanding.

Senator MOYNIHAN. And your benefit reduction rate?

Mr. MAXFIELD. Was 50 percent.

Senator MOYNIHAN. Thank you. We are talking about what I might call the Presidential program. This is the kind of program Presidents have been proposing for some time.

Mr. MAXFIELD. There are, however, a number of differences between our welfare reform and the President's proposal, which I will comment on in just a second. That is not to say that the results are not applicable in some ways.

So the States are required to supplement this Federal basic grant so as to hold families harmless. In summary, the welfare reform that we tested consisted of replacing the current AFDC matching formula, the SSI systems of State supplementation and SSI Federal basic grant, and Federal food stamp program with a uniform Federal negative income tax with State supplementation for holding families harmless.

The tool that we used in the investigation was a computer simulation of the reform of the existing welfare system. The categorical eligibility rules and the payment formulas of AFDC, SSI, food stamps, the hypothesis of negative income tax, plus the rules and regulations for the Federal personal income tax were reproduced in a computer program and applied to each person and family in a data base that represented the national population in the calendar year of 1975. The results for States and regions are the sums of the results for the individual families in the data base which reside within the States and regions.

The results indicate that the answer to our first question concerning the State distribution of Federal welfare funds is as one would expect. This intermediate negative income tax that was referred to, one which results in an increase in Federal expenditures of approximately 20 percent over those expenditures in 1975 for the replaced programs, would result in increased levels of Federal funding going to all but very few States.

Senator MOYNIHAN. Are these the amounts that you are using to illustrate on table 10, where you have an increased expenditure of \$3.9 billion?

Mr. MAXFIELD. Yes; that is correct.

So under such a negative income tax, almost all States, but not all States, receive an increase in Federal funds. Those States which do not are those which have relatively high levels of AFDC income guarantees. Those States with the lowest AFDC guarantees receive the biggest increment to—

Senator MOYNIHAN. I see here that it would cost the State of New York \$237 million by your calculations. Our neighbor, New Jersey, \$22 million. Am I reading right?

Mr. EDSON. What he was describing just now is the new Federal benefits which are—

Senator MOYNIHAN. Net transfer?

Mr. EDSON. New welfare dollars going to the States. The point he was making is that a State like New York in our table receives roughly \$94 million but the increment going to a State like North Carolina is larger. He is just saying of the new benefits, a larger portion is going to the States that have the lowest recipient benefit level.

What you are talking about is the net transfer which takes into account the surcharge scheme that we were talking about earlier. When you say the State of New York loses money it is because the additional taxes paid to finance the new welfare expenditures are higher in New York than many other States, and in fact, it exceeds what the State of New York gets as welfare payments, so their net transfer of Federal dollars drops.

Senator MOYNIHAN. Yes, I follow you. Go ahead. I didn't want to interrupt.

Mr. MAXFIELD. The estimates of the State fiscal relief and of the change in the net disposable income going to State residents were, however, not exactly as we anticipated. The same negative tax, this intermediate level negative income tax which increases total Federal expenditures by about 20 percent—

Senator MOYNIHAN. Could you help me there? You have cashed out food stamps, have you?

Mr. MAXFIELD. Yes, sir.

Senator MOYNIHAN. And then you have the existing welfare program, AFDC-U and SSI, plus food stamps?

Mr. MAXFIELD. That is correct.

Senator MOYNIHAN. Then you have to add another \$3.9 billion to get to this 75-50 percent pattern?

Mr. MAXFIELD. Yes, sir.

Senator MOYNIHAN. That is surprisingly small. But in the administration program, much money goes to jobs, in terms of the amounts of money involved, income transfers alone aren't that high. Here we are saying \$3.9 billions is not high but \$20 billion is where that program came out last year.

Mr. MAXFIELD. With the jobs component, as I understand it.

Senator MOYNIHAN. Right.

Mr. MAXFIELD. So this same negative income tax supplemented in the manner that I described results in 43 States plus the District of Columbia being required to increase their welfare expenditures. Seven States, principally those with relatively high AFDC-U guarantees receive some fiscal relief. The fiscal burden of many States appears to be caused by the following sequence of events:

In order to extend welfare eligibility to many families which are now categorically ineligible for AFDC, while maintaining a reasonable total program cost, the benefit formula under the reform plan cannot be very much more generous than the AFDC benefit formula is. For many families it is less generous than the AFDC formula is. Thus, even though the current AFDC and SST State supplements are discontinued in our tested reform program, the State is required to supplement the Federal basic negative income tax grant of so many families that its welfare expenditures actually increase.

Senator MOYNIHAN. You will have to go over that with me again. You have discontinued the supplements?

Mr. MAXFIELD. What we have done is replace programs which now require State supplementation.

Senator MOYNIHAN. Yes.

Mr. MAXFIELD. So the existing State supplementation to AFDC and the existing State supplementation to SSI are discontinued.

Senator MOYNIHAN. But then there are States with more people eligible?

Mr. MAXFIELD. That is true.

Senator MOYNIHAN. In New York there aren't more units but the whole harmless provision requires that the State supplementation continue?

Mr. MAXFIELD. Right.

Senator MOYNIHAN. Got you.

Mr. MAXFIELD. To answer the final of our three questions on the implications for the net change in disposable income of State residents

we conducted a similar computer simulation of a Federal personal income tax surcharge just sufficient to generate additional Federal revenues equal to the additional Federal expenditures required by our tested program. We assumed that all the State expenditures under the reform program would be financed by residents within the States, so these had no impact upon the distribution from State to State of the net income of residents.

The pattern of these results is different, and in many ways opposite, to that of our fiscal relief results. Those States which currently have relatively high AFDC income guarantees and receive fiscal relief also contain residents which pay a disproportionately large amount of personal income tax. The result of this is that, in general, those States which experience fiscal relief contain residents which pay more into the system in terms of tax dollars than they get out of the system in terms of transfer payments.

This concludes the results of our investigation and I do have some comments about the applicability of these results to other proposals.

Now, these are speculations, these are not hard facts.

Senator MOYNIHAN. Fine, go right ahead and speculate. No reason you should be different from anybody else.

Mr. MAXFIELD. Thank you.

Senator MOYNIHAN. People who do this kind of hard work are entitled to a little speculation at the end of the day.

Mr. MAXFIELD. There are at least three major sort of difficulties for drawing implications from our study to those specific proposals which have been discussed recently. First, is that the tested reform did not contain any public service jobs other than those that were provided by the CETA and WIN programs in 1975.

Second, the tested program assumed a very specific type of State supplementation which may or may not differ from any given proposed program.

And, lastly, we tested a reform which replaced all three current welfare programs, AFDC, food stamps, and SSI, with a negative income tax, and some of the proposed programs do not reform all of these programs.

In spite of these difficulties, I think our results do shed some light on both the program for better jobs and income and the program proposed by Senators Baker, Bellmon, Ribicoff, and Danforth.

Senator MOYNIHAN. Could I ask you about the program proposed by Senators Baker, Bellmon, Ribicoff and Danforth. Would it be roughly comparable to table 10 also in your view?

[Discussion off the record with staff.]

Senator MOYNIHAN. Do you feel you have in this report, which I haven't been able to do more than just glance at, a table which would give us the changes in actual State disposable income resulting from the Baker-Bellmon plan?

Mr. MAXFIELD. No, I do not.

Senator MOYNIHAN. The same technique that got you this could get you that?

Mr. MAXFIELD. Yes, sir.

Senator MOYNIHAN. What do we have to pay you to get that? You could be an enormous resource to us, I will tell you. How long would it take you? What is your—Math is your model, isn't it?



Mr. MAXFIELD. That is correct.

Senator MOYNIHAN. Would you find out how long it would take and how much it would take to estimate Senator Ribicoff's, Senator Bellmon's, and Senator Baker's proposal and we will try to get you the money for it.

Mr. MAXFIELD. I will certainly find that out.

Senator MOYNIHAN. It would be helpful to us and we would appreciate it. Mathematica has a superb reputation. I don't know whether this kind of capacity is paralyzing social progress or facilitating, probably somewhere in between, but at least we know we have levels of information we never had before.

Mr. MAXFIELD. I would be happy to find out.

Just to conclude my remarks. I think that the program for better jobs and income is, as you have mentioned, similar to the reform that we have tested. The program does call for holding families harmless, which is often referred to as grandfathering recipient families, for a temporary transition period and that States would be involved in supplementing so as to hold these families harmless.

The program also extends eligibility to many families not currently eligible for AFDC and the payments levels, as you noted, are roughly comparable to those that we tested.

This suggests that in the absence of an explicit cap on State expenditures, under this supplementation plan, that many States may experience additional fiscal burden during this transition period for the program for better jobs and income.

The program did specify a cap on State expenditures to prevent this from happening, and our results indicate that the expenditures of many States may be constrained by that cap.

Senator MOYNIHAN. But not in those States which are grandfathering?

Mr. MAXFIELD. During the transition period I think those States which are grandfathering, those that are likely to be spending a lot, are those that are likely to hit up against this cap.

Senator MOYNIHAN. That is a new idea. We will have to find out more about that from HEW.

Sir, there is no avoiding the extraordinary difficulties proportionate to distribution of funds here. The State of New York, which has twice the tax burden per capita of the State of Texas, would pay an extra \$273 million in taxes, whilst the State of Texas would receive a net transfer of \$193 million.

Mr. MAXFIELD. That is what our result indicates.

Senator MOYNIHAN. That is what your result indicates you are darn right, and thank God you did it, sir.

Could I ask you, how did you become interested in the subject? Did you bid to do this project or was the project put out for bids?

Mr. MAXFIELD. As I understand, the project came about as a piece of the ongoing research that Mathematica was doing on the negative income tax experiments.

Senator MOYNIHAN. Which you have been involved in now for a decade?

Mr. MAXFIELD. Yes, sir.

Senator MOYNIHAN. This particular report got prepared in May of this year. Have you been asked to testify before any other committees?

Mr. MAXFIELD. No.

Senator MOYNIHAN. Are you aware that HEW has brought this information to the Congress? You don't have to answer that question. I will answer that question. No.

I want to thank you very much, it is superb work and, Mr. Edson, I thank you, sir. We very much hope that we could hear from you about what it would take to model the Baker-Bellmon plan. Then there will be others. We may keep you busy all next spring.

I congratulate you and would like to take this opportunity, if I can, to express the admiration of this Senator—I can't speak beyond that, obviously—for the work that Mathematica has done over the years. It is a new kind of enterprise and it is an extraordinarily effective and admirable one. I would like to put that on the record for what little it might be worth.

Mr. MAXFIELD. Thank you very much.  
[The above statement in full follows:]

STATEMENT OF MYLES MAXFIELD, JR., AND DAVID EDSON, ECONOMISTS,  
MATHEMATICA POLICY RESEARCH, INC.

Mr. Chairman and members of the committee, we are grateful for the opportunity to report to you the results of our investigation into the fiscal impacts of welfare reform on states. The research was conducted at Mathematica Policy Research under contract with the Department of Health, Education, and Welfare. We are today, however, not speaking on behalf of either organization. We would first like to describe what our investigations revealed, then to explain how the research was done, and lastly to comment upon the implications of the study for the welfare reform proposal of Senators Baker, Bellmon, Ribicoff and Danforth and for the Program for Better Jobs and Income.

We addressed three questions: How many federal dollars would be expended in each state under a welfare reform program? How much fiscal relief would each state experience under such a program? Lastly, if the additional federal expenditures were financed by an income tax surcharge, what is the net change in the income of the residents of each state. This last question views the residents of each state as a group which pays tax dollars into the income-transfer system and which receives welfare payments from the transfer system. We sought to estimate the net amount of the flows from and to the residents of each state.

We attempted to answer these three questions for a reform of the current welfare system which is simpler than many that have been recently proposed, namely one which replaces the Aid to Families with Dependent Children, Food Stamps and Supplemental Security Income programs with a negative income tax for which all families whose income is below a specified level would be eligible. The negative income tax was specified by a guaranteed minimum income equal to a percentage of the federal poverty level of income, which does not vary from state to state, and by the rate at which the payment is reduced with increased levels of earnings, which we call the benefit-reduction rate. We made the assumption that all eligible families would participate in the negative income tax. The final feature of the negative income tax was that states were assumed to supplement the federal payment in order to guarantee that each family currently receiving welfare benefits would receive a payment under the reform program that is either the same or higher than their current payment. In summary, the tested welfare reform consisted of replacing the current AFDC matching formula, the SSI system of state supplementation of the federal basic grant, and the federal Food Stamps program with a federal negative income tax with state supplementation to hold current recipient families harmless.

The tool used in this investigation was a computer simulation of this reform of the existing welfare system. The categorical eligibility rules and payment formulas of AFDC, SSI, Food Stamps, the hypothesized negative income tax, plus the rules and regulations for the federal personal income tax program were reproduced in a computer program and applied to each person or family in a data base that represented the national population in the calendar year 1975. The results for states and regions are the sums of the results for the individual families in the data base which reside within the states and regions.

The results indicate that the answer to the first question concerning the state distribution of federal welfare funds is as one would expect. A negative income tax which results in a 20 percent increase of total federal welfare expenditures increases the level of federal funding in all but a very few states which have relatively high AFDC income guarantees. States with the lowest AFDC income guarantees receive the biggest increment in federal funds.

The estimates of state fiscal relief and of the change in the net disposable income of state residents are not as we anticipated. The same negative income tax which increases total federal funding by 20 percent and which is supplemented by states to hold families harmless results in forty-three states and the District of Columbia being required to increase their welfare expenditures. Seven states, principally those with relatively high AFDC income guarantees, receive fiscal relief. The increased fiscal burden of many states appears to be caused by the following sequence of events: In order to extend welfare eligibility to many families which are now categorically ineligible for AFDC without increasing the level of federal expenditures by more than 20 percent requires that the federal negative income tax payment formula be less generous than the payment formulas of existing programs for many families. Thus even though the current AFDC and SSI state supplements are discontinued under the reform plan, the state is required to supplement the federal negative income tax payment of so many families that its welfare expenditures increase.

To answer the final question on the implications for the net change of the disposable income of state residents we conducted a computer simulation of a federal personal income tax surcharge sufficient to generate additional federal revenues equal to the additional federal expenditures required by the tested welfare reform plan. We assumed that all state expenditures were financed by state residents and thus had no net impact on the state distribution of personal disposable income. The pattern of these results are different from the pattern of the fiscal relief results. Those states which currently have relatively high AFDC income guarantees and receive fiscal relief also contain residents which pay a disproportionately large share of personal income taxes. The result is that, in general, those states which experience fiscal relief contain residents which as a group pay more into the income tax-income transfer system than they get out of the system in the form of negative income tax payments.

There are three major difficulties for drawing implications from these results for the variety of recently discussed welfare reform proposals. The first is that the tested reform did not contain any direct federal provision of jobs beyond those that were provided within the CETA and WIN programs in 1975. Secondly, the tested reform assumed a specific type of state supplementation of federal payments which differed from the type of state supplementation specified in the reform proposals. Third, some of the proposed programs did not call for the reform of all three current programs, AFDC, Food Stamps, and SSI. Because of this incomparability, direct answers to the three questions for proposed reform programs would require additional research.

In spite of these difficulties, the results of our investigations shed some light on the impacts of reform proposals such as the Program for Better Jobs and Income and that of Senators Baker, Bellmon, Ribicoff, and Danforth. The Program for Better Jobs and Income called for holding recipient families harmless, often referred to as "grandfathering" recipient families, for a temporary transition period of time. The program also extended eligibility to many families not currently eligible for AFDC and had payment levels which were no more generous for many families than the tested reform. This suggests that, in the absence of an explicit cap on state welfare expenditures, many states may experience additional fiscal burden during the transition period of the Program for Better Jobs and Income. The program did specify a cap on state expenditures and the results of this investigation suggest that the expenditures of many states may be constrained by that cap.

The results are less applicable to the Baker-Bellmon-Ribicoff-Danforth proposal because, although the proposal extends eligibility to many families previously ineligible for AFDC, the proposal specifies a benefit formula and a federal-state matching formula which is similar to that of AFDC. These features combined with the absence of holding current recipients harmless may result in more states experiencing fiscal relief than estimated for the tested negative income tax.

[A short recess was taken.]

Senator MOYNIHAN. Ladies and gentlemen, through the very thoughtful cooperation of Mr. David Lyon we have a panel, Mr. David Lyon and Dr. James Hosek of the Rand Corp., and they have been kind enough to come this morning, even though they were scheduled to appear this afternoon. We are happy to have you here, and once again we are finding people who have done important work in this field. We welcome two distinguished researchers, and I think that, let's see, Dr. Lyon, you are listed first, so why don't you proceed?

**STATEMENT OF DR. DAVID W. LYON, THE RAND CORP.**

Mr. LYON. Thank you, Senator. My comments this afternoon are a summary of the findings presented in "Materials Related to Welfare Research and Experimentation."

Senator MOYNIHAN. Which is what we call our "Green Book."

Mr. LYON. The Green Book. The first article in there is being summarized with this testimony.

Welfare dependency is popularly thought to be a chronic condition; that once a family moves onto the rolls, it stays there indefinitely. Part of the work my colleagues and I carried out for New York City was designed to test the assumption that the AFDC program creates a welfare class. In fact, our research tells quite a different story. Of all the cases opening onto the rolls in New York City at any given point in time, say today, 3 years from now only one-third of them will still be there without having left; 25 percent of the cases will leave the rolls after a stay of 1 year or less, and the remaining cases will move in and out of dependency for different lengths of time depending upon changing family circumstances, fortunes in the job market, or the health status of the case members. I will describe each one of these dimensions in this testimony.

Welfare dependency cannot be equated with "a welfare class." There are almost as many cases who stay for 1 year or less, never to return, as there are cases that stay for 3 years or more. While these findings describe New York City experiences, I have surveyed studies of the welfare decision in other jurisdictions, and they reach similar conclusions.

The research I am summarizing here was done over a period of 5 years by staff at New York City—the Rand Institute in the city of New York, using computer data files on welfare cases in New York City. Most of these findings pertain to case samples of between 40,000 and 60,000 cases. We are not talking about small samples.

Senator MOYNIHAN. You weren't here to hear the testimony from the State of Utah when we went into some detail about the very interesting work project they have there in a State, which has a total AFDC caseload of 12,000 persons.

Mr. LYON. A common test of the welfare decision as a rational, economic choice, is to compare potential benefit levels to wage levels from available jobs in the local market. After controlling for other factors, such as local administrative policies, I believe Professor Wiseman discussed this morning—

Senator MOYNIHAN. Yes.

Mr. LYON [continuing]. The studies generally support an alternative income hypothesis. As benefit levels, or the benefit/wage ratio,

rise welfare participation increases, and employment rates among welfare families increase. In other words, the welfare decision is made by comparing public assistance benefits to prevailing wages.

I want to emphasize here that this is as we would expect. We find that most empirically tested models of the welfare decision are consistent with economic theory.

Senator MOYNIHAN. That is one of the few theories that still seems to work, as price goes up demand goes down and vice versa.

Mr. LYON. Exactly. The benefit level is highly conditioned by family size—more children, higher monthly payment—so, it is no surprise that AFDC cases with extended dependency in New York City averaged over four persons in size, while short duration cases averaged less than three persons. Not only are child care arrangements more difficult, but the wage a mother of three or four children must receive to attract her to the job market must be higher than for a mother with fewer children. Large families eligible for welfare tend to participate more frequently than smaller families.

While benefit levels strongly affect the welfare decision, the fate of local economy has a much weaker linkage to the movement on and off welfare, and therefore, the size of the caseload. Aggregate unemployment rates or levels are generally insignificant variables when explaining changes in the regular AFDC caseload. This is the female-headed case.

On the other hand, the direct link between the fate of the local economy and the welfare decision seems to be strongest for general assistance and the AFDC-UF caseload. That is entirely consistent with the testimony that you received from Professor Wiseman this morning.

Senator MOYNIHAN. Right.

Mr. LYON. I have surveyed a number of studies that have found the same thing. For example, using our analysis of case openings and closings for New York City found that the home relief and AFDC-UF caseload significantly related to levels of unemployment, but that was not the case where we analyzed the regular AFDC caseload. These findings are consistent with our expectations.

Senator MOYNIHAN. I did a study in 1963 which showed that from 1947 to 1958, there was a strong correlation, about 0.91 between male unemployment rates and new AFDC cases. Then it started going blooey and disappeared.

Mr. LYON. Yes, that relationship is no longer to be found as significant as it used to be.

Eligibility for the UF program is conditional upon recent employment experience, and general assistance supports unemployed families or individuals who are temporarily unable to find or hold a job. However, a mother with dependent children covered by the AFDC program may find herself without support from a spouse quite independently of changes in the economy. That seems reasonable.

Let's turn to what we found out about the relationship between welfare and medicaid, something I think that has not been addressed so far today.

We analyzed the question of how the combined availability of AFDC, food stamps, social services, and medicaid impacts upon the welfare decision. After looking at the welfare population on a case-by-

case basis, we found that the AFDC grant by itself is only 55 percent of the total cash and noncash value of goods and services received by a typical four person welfare family. The average four person family in New York City received a total of approximately \$6,000 in cash and noncash services.

Senator MOYNIHAN. In 1975?

Mr. LYON. In 1974. In other words, however you may wish to treat the dollar value of noncash program support like medicaid, the AFDC grant is really only half of the picture when it comes to looking at the effectiveness of our existing welfare system.

Senator MOYNIHAN. Yes.

Mr. LYON. More importantly, from the point of view of the welfare decision, we found that short-term cases and cases that go on and off the rolls intermittently had much higher levels of medical paid health care than cases on the rolls continuously for 3 years or more. A good deal of the movement onto the rolls seems to be caused by demands for health care not covered by private insurance plans. Welfare may mask a large number of families more in need of health insurance than income maintenance.

It is clear that welfare dependency is strongly linked to the short-run demand for health care, and that there should be attempts to identify where the two systems overlap and where they could operate independently. These conclusions are supported by many studies identifying health problems as one of the most important reasons families turn to welfare.

Typically, for example, a welfare applicant is asked did you turn to welfare because you lost your husband, lost a job, or because of a health problem, and health problems consistently rank as either first or second as a reason for case openings in New York City.

Senator MOYNIHAN. In New York City, though, is it not the case that about half the persons who go on AFDC are young mothers with children who have not formed a household?

Mr. LYON. I think that is right.

Senator MOYNIHAN. So for more mature persons health becomes a problem?

Mr. LYON. I am just talking about an opening of an AFDC regular case, a mother with children. There are a number of reasons for those health problems. It could be that she is in need of care while having another child, or there could be health problems for the children she already has, or she may be having health problems herself and find herself unemployable and in need of short-term care.

Senator MOYNIHAN. Yes.

Mr. LYON. All of those are valid reasons for a case opening.

Now, I turn to welfare and the migration decision. Most studies of the welfare decision and migration have not found a strong linkage between State benefit levels for welfare and the decision to migrate. In our New York City work, we found that new arrivals in the city are less likely to go on welfare than those who have been in the city for several years. Rather than being a primary motivation for moving, welfare participation is more likely to happen after a family has been in a city awhile.

In other words, this finding suggests that families move to New York City for job opportunities. If they cannot find a job there is a

good income-support system to help them maintain their residency. This is a different finding than earlier views of welfare as a major reason for the migration decision.

Senator MOYNIHAN. The earlier ones never had any data to support them. Anybody who knew anything knew that those people came to New York for the same reason people have always come to New York. It is a good place to live.

Mr. LYON. Exactly.

Senator MOYNIHAN. A better place to live than where they were living.

Mr. LYON. Now, I turn to the effect of administrative rules and regulations on the welfare decision. The administrative discretion of local welfare agencies (the setting and interpretation of eligibility rules and regulations) has been identified as another major factor influencing the welfare decision. Each State welfare department has an allowance schedule and benefit tax rate that is set by legislation, but there are a series of rules and regulations (deductibles, disregards, set-asides, work tests, certification and recertification procedures, employment referral requirements, and employability definitions) that provide a significant degree of administrative discretion.

Changes in these regulations and the stringency with which they are enforced have a major impact on the welfare decision in the short run. The number of case openings, case closings, and transfers are affected and, as a consequence, the overall size of the caseload.

An example from New York City demonstrates how a significant portion of short run caseload dynamics is administratively induced. Our analysis of the 1973 eligibility control program in New York City, a program which was implemented nationwide, showed that a major decrease in the AFDC caseload from the middle of 1972 to early 1974 was attributable to largely a photo identification card and mail recertification program. Because of these two programs, the caseload declined by some 20,000 cases, or 6 percent over that period of time—the first major decline in New York City's caseload since the peak in the late 1960's.

Mailed requests to report to welfare centers for the photographs resulted in many case closures because some families didn't receive the forms, some didn't know what to do with the forms, and some simply because they knew they were ineligible to receive assistance. Many cases were closed off the rolls, and in aggregate, the caseload declined because there was a higher monthly average of case closings than had ever been recorded in the city.

However, by mid to late 1974 case reopenings averaged nearly three times what they had been before the eligibility control programs were launched. By early 1975, the caseload was back to the 1972 level.

From this analysis we draw two conclusions:

I should note here that the analysis was carried out by tracking actual cases to see what happened after they closed, and their determining whether it reopened and how long it had been off the rolls.

First, many eligible cases had been closed in error, and they were reopening after a hiatus of 1 or 2 months.

Second, administrative intervention generated inequities for individuals who had to leave welfare and then return because their case was closed in error.

These conclusions add to the view that the administration of the AFDC program is inherently complicated and at times it does generate short-term case dynamics that are confounding to analysts and to policymakers alike. When we sort out those short-term phenomena and look at the behavior of the caseload in the long run, you find it quite consistent with the basic principles of rational economic behavior.

Our evidence supports the view that over the long run the welfare decision is made in ways that are consistent with broad policy variables like the level of benefits.

In spite of the many inefficiencies and inequities contained in our current public assistance system, most families use welfare as intended—a temporary income support during periods of unemployment or loss of normal income support. The majority of recipients use public assistance for income support in much the same way other temporary support programs like unemployment insurance and workmen's compensation are used.

Modifications to relieve inequities in the current system—such as mandatory provision of AFDC-UF or liberalization of its eligibility rules—is likely to produce responses by eligible families that are consistent with the behavior we found by participants in existing programs.

Senator MOYNIHAN. Would you spell that out? What do you mean responses that are consistent? You mean there would be more people on welfare?

Mr. LYON. Responses are consistent in the sense higher benefit levels will induce higher participation rates, that the caseload will be affected by the administrative rules and regulations unique to each State, and that, of course, the multiple benefit programs available to these families will affect their decision to go on the rolls, depending on the level of benefits of the program.

Senator MOYNIHAN. We thank you for some very marvelous and sane testimony and, as you say, the one rule that seems to hold is as benefits go up so does the demand, in that kind of market system.

You used to work at the New York City Rand Institute in New York, or do you still?

Mr. LYON. I used to work with the institute in New York, I now work with the Rand Corporation in Santa Monica, Calif.

Senator MOYNIHAN. You would have known my dear friend Dr. Gifford at that time?

Mr. LYON. Very well.

Senator MOYNIHAN. Now, what would you advise us in New York? We find that we have no welfare class, that we have nothing the matter with people making rational decisions. They do make rational decisions, and they are very simple ones, that the more benefit there is for being on welfare, the more people will be on welfare.

The key variable is the amount of benefits, and this does in fact fit Feldstein's microeconomics and some of the other people's discussion of voluntary unemployment. There are plenty of jobs at a nickel an hour and very few jobs at a \$1,000 an hour, and in between people make their choices.

As you know, New York City is going bankrupt again, it is really heading for disaster. I quote today's Times: "New York aides fear drastic steps are necessary to solve budget gap. In spite of 3 years of



cuts, officials think that the situation is as perilous as ever with a solution in doubt."

Rand was brought to New York to think of solutions. We are in terrible shape. I have been saying this for quite awhile. It is not a new phenomenon, it has been very clear for a long time. We have a million people on welfare, Utah has 12,000, and our benefit levels are probably twice those of Utah. What should we do?

Mr. LYON. Well, my response is professional advice, and is not based on any—

Senator MOYNIHAN. We asked everybody who has told us what they know to tell us what they feel.

Mr. LYON. It is quite clear that New York City taxpayers cannot continue to support the share of welfare costs that they have supported in the past. Welfare is not the only income transfer program that New York City provides. Not only does it—

Senator MOYNIHAN. Medicaid, for example?

Mr. LYON. Medicaid is one of them, but we can move out of the income transfer and social service categories to include things like free tuition at the city's public university, and include a number of other subsidized public programs in the city—hospitals, for example—that drain the local tax base to subsidize families of all income levels.

Senator MOYNIHAN. Of all income levels?

Mr. LYON. In the light of the question you asked, "what should we do about welfare" in the narrow sense, there has to be a strong effort at the Federal level to absorb a greater share of welfare costs in New York City.

Senator MOYNIHAN. Thank you.

Did you see the Mathematica tables. Just before you came in, Dr. Maxfield was representing the work they have done on the overall transfer effect of what is, in effect, the President's program. Don't hold me to these numbers. But the net transfer out of New York, is about \$368 million. To Texas, \$122 million. The taxes are already twice in New York what they are in Texas. This would make it possible for them to become three times, and we would really have a distinction that is rarely enjoyed.

I was wrong, it is \$274 million that would be transferred out of New York and \$122 million into Texas. Just about 2 to 1.

Anyway, I ask you as an analyst, a respected economist, would it be a correct interpretation of your testimony and your research that New York City has many, many, many more persons on welfare than any other city, because its benefits are much higher than any other city?

Mr. LYON. I wish I could recall the exact participation rates for New York City as against the rest of the country, but if I recall the range of those numbers correctly, I believe New York City has a percentage of its population on welfare that is not greatly out of line with other large States, in fact, lower than some other jurisdictions.

Senator MOYNIHAN. It is lower than about three other cities.

Mr. LYON. In light of that data, I would say that the benefit levels alone in New York City do not account for these extraordinarily large numbers of people on welfare. You are dealing with a city with 8 mil-

lion people and you are talking about a percentage which is quite comparable with other jurisdictions of a very large base.

Senator MOYNIHAN. That presents some problems.

Now, we have some of the highest benefit levels in the country. Why don't we have the highest participation rates?

Mr. LYON. Well, as I have indicated, it is not benefit levels alone which control—

Senator MOYNIHAN. Benefit level primarily?

Mr. LYON. Benefit levels in combination with the administrative stringency which has been adopted as a policy by the city. The city's ability to respond to the demand for welfare is important. It may not be able to keep up with all of the people who want public assistance, people for whom the eligibility procedure is a rather difficult task, or for those who do not have all of the proper documentation to establish their eligibility.

Senator MOYNIHAN. As you know, the relative benefits have declined in the city because we cannot afford to increase them in the present crisis. Yet the numbers have not really gone down. I think your hypothesis would predict they would be going down somewhat by now.

Mr. LYON. There is certainly a cost of living differential.

Senator MOYNIHAN. And no increase. There has been about a 29 percent decrease in real value in 4 years, it is quite sharp.

Mr. LYON. Most models of welfare caseload levels or participation rates use benefits in real terms (corrected for inflation) and not in nominal dollars. So New York could have one of the highest nominal benefit levels, but not the highest participation rate.

Senator MOYNIHAN. Because it is not in fact the highest real value?

Mr. LYON. Yes, sir. The caseload would not necessarily decline if real benefits declined by 29 percent. The real value of wages is also declining. If the real value of benefits remained constant we might very well have seen a sizable growth in the caseload of over the 4-year period you mentioned.

[The following was subsequently supplied for the record:]

RAND,

*Santa Monica, Calif., December 11, 1978.*

Hon. DANIEL P. MOYNIHAN,  
U.S. Senate,  
Washington, D.C.

DEAR SENATOR: I have prepared the following responses to the questions listed in your 24 November letter.

*Question 1.* MOYNIHAN. If I understand your testimony, you suggest that the size or amount of welfare benefits is a key variable in the decision to go onto, and then remain on, public assistance, at least up to the point where available employment alternatives appear more financially rewarding. Does this not imply a form of voluntary unemployment on the part of some welfare recipients?

*Answer.* Any government income transfer program creates work disincentives or, as you say, voluntary unemployment. Work disincentives are not only present in the AFDC, food stamp and medical programs, but also in our unemployment insurance and workmen's compensation programs. The design challenge of all transfer programs is to meet the income support objectives for eligible families and individuals and, at the same time, to provide a means for minimizing the work disincentive effects. This design dilemma is as common to unemployment

insurance as it is to AFDC or a guaranteed income program. Most existing programs minimize work disincentives by periodically recertifying eligibility and by mandating job referrals.

**Question 2. MOYNIHAN.** You emphasize the relationship between employment and welfare, which you suggest is relatively fluid and different in various areas. But other witnesses have spoken of a pronounced growth in chronic dependency, a "settling-in" of recipients on welfare and perhaps also an intergenerational transfer of dependency. Do you see a large—or growing—sub-group of welfare recipients who are effectively outside the labor market?

**Answer.** As stated in my testimony, approximately one-third of cases opening on the New York City welfare caseload on any given day will be continuously dependent for three years or more. You might think of these families as "effectively outside of the labor market." Two-thirds of the opening cases have short stays or move in and out of dependency intermittently. These families have breadwinners who move in and out of employment in the local job market, but for various reasons (limited job opportunities, poor health, job instability, unstable marriages) their income is not continuously high enough to support them independently of public programs. However, I would not characterize these families as "effectively outside of the labor market."

Even in instances of long-term dependency (cases staying on continuously for three years or more), seven percent of New York City's AFDC cases, and 16 percent of H.R. cases had employment income. So it would be incorrect to say a "large" group of welfare recipients are outside of the labor market.

Our research in welfare dependency in New York City did not address the question of whether the proportion of welfare families without an attachment to the labor market is growing in size.

**Question 3. MOYNIHAN.** There seems to be a contradiction between one of the points you make in your testimony and one of your conclusions. If local employment conditions have only a slight effect on the decision to go on welfare, how do you conclude that most families use welfare as intended, i.e. as a form of unemployment-related income maintenance?

**Answer.** There are two parts to the answer: First, the size of the caseload for Home Relief and AFDC-Unemployed Father programs was found to be positively related to cyclical changes in the City's economy. We found no statistically significant relationship between the AFDC-regular caseload and short-term shifts in the economy. We expected this finding because a mother with dependent children may find herself without support from a spouse or besieged with health problems independently of changes in the economy. The HR and AFDC-UF programs, on the other hand, are directly tied to unemployment status.

Second, some family heads may not respond to short-run changes in the City's economy simply because their reason for unemployment is not tied to business cycle—for example, child care or health problems. Nevertheless, there is considerable evidence that many, if not most, families move into the job market as soon as their reasons for using welfare have passed.

I hope you find these answers responsive to your questions. Please don't hesitate to contact me again if you have any further questions.

Sincerely,

DAVID W. LYON,  
*Deputy Vice President,*

Senator MOYNIHAN. I think that is a nice opinion and thank you very much. Dr. Hosek, you are going to talk about the decision to go on AFDC. We welcome you.

#### STATEMENT OF JAMES B. HOSEK, THE RAND CORP.

Mr. HOSEK. Thank you for the opportunity to report on research into family participation in the AFDC-UF program, which I shall refer to as UF for short. My testimony will deal with three issues: What determines family participation in UF, why is the UF participation rate so low, and what are the consequences of extending the UF program to States that do not currently offer it.

The UF program is designed to aid husband and wife families, a group that is allegedly inequitably served under the existing income maintenance system. In 1975, 26 States plus the District of Columbia had active UF programs, with a total monthly caseload of about 117,000 families. It is argued that by mandating the UF program for all States and relaxing federally set eligibility requirements, the program would be a means of improving the income support system for "working poor" families.

Researchers have known for several years that the rate of family participation in UF is low, especially as compared with participation in AFDC-FG (which serves single parent families). The UF participation rate has been estimated at 15 to 30 percent, meaning that only 15 to 30 families out of every 100 presumably eligible families actually participate in UF in a given month. In contrast, the rate for FG was estimated at 71 percent in 1970.

Senator MOYNIHAN. And may have gone up a bit?

Mr. HOSEK. Yes. In fact, I think it may be over 80 percent now.

Senator MOYNIHAN. May be over 80?

Mr. HOSEK. Yes, sir.

With such a low participation rate in UF there is concern whether UF could be an effective component of welfare reform. This concern has provided an impetus to study the determinants of family participation in UF.

Several studies of participation have been based on aggregate data. Probably as a result of the crudeness of the data, these studies had difficulty in obtaining sensible effects on crucial variables. In particular, the studies raised uncertainty as to the importance of the benefit level and the family's employment opportunities.

In comparison to these studies, my research has utilized family level data. A similar picture emerges for blacks and whites alike: A family is more likely to participate in UF if its alternative economic opportunities are less attractive relative to the benefits they would receive on UF. Thus, a family's chance of participating is greater the higher potential benefits on UF, the lower the husband's wage rate and education, the lower the wife's age and education, and the lower the benefits the husband would receive under unemployment insurance should he choose that program.

The wage rate and education of the husband may be interpreted as measures of his opportunities in the labor market. Similarly, the earnings potential of a wife is known to depend positively on her age and education, so again the latter variables may be viewed as a measure of her labor market opportunities.

Finally, unemployment insurance can be thought of as an alternative form of income support, and the results suggest that families treat UF as a substitute for unemployment insurance.

In related work, I have also found that the chance of a family being on AFDC-UF rises as the unemployment rate rises.

A direct policy implication of these findings is that an increase in UF benefits will lead to an increase in UF caseload. A 1-percent increase in benefits, I estimate, will cause an approximate 1-percent increase in caseload.

There is considerable speculation as to whether the low rate of participation in UF can be attributed to factors concerning family eligibility and program operation. With respect to these variables I have recently undertaken a cross State analysis of UF caseloads. Controlling for the variables mentioned above, the results are that none of the administrative variables which were tried has any effect on State caseload.

Senator MOYNIHAN. Yes.

Mr. HOSEK. The variables are: The limit on liquid and semiliquid assets a family must not exceed in order to be eligible for UF, the State's error rate in handling its entire AFDC caseload, and the amount per AFDC case spent in training caseworkers. By my interpretation, these findings do not prove that administrative variables are inconsequential, but only that available data may be a crude counterpart to the theoretically relevant concepts. Indeed, as David Lyon has testified, changes in administrative variables do appear to have at least a shortrun effect on caseload.

Senator MOYNIHAN. Or the variables that now exist?

Mr. HOSEK. Correct. In fact, there is a side comment. Previous work, I think by Michael Wiseman, has indicated again the shortrun effects on caseload of changing administrative variables.

Senator MOYNIHAN. Yes.

Mr. HOSEK. Two other administrative variables have drawn attention. One is the requirement that a father, in order to be eligible for UF, must have worked in 6 of the 13 calendar quarters prior to application for support; the other is that a father on UF, if he works, must work no more than 100 hours per month. These requirements are set by the Federal Government and do not vary across States, so it is impossible to obtain statistical estimates of the effect of changing them on a family's decision to participate in UF.

Still, my viewpoint is that relaxing or eliminating these requirements would cause only a modest increase in participation. This is because most husbands have a strong attachment to the labor force and probably satisfy the prior work rule; and because if the 100-hour rule were eliminated, the family would soon come up against the limit on earned income, above which UF benefits are zero.

This conjecture, coupled with my statistical results, implies that changes in eligibility requirements and administrative practice do not seem likely to cause major increases in the rate of participation in UF. This leads me back to the earlier result that families are more likely to participate in UF the less attractive their alternative economic opportunities.

In other words, the participation rate may be low because many families can and do choose to take advantage of market opportunities. Some evidence of this is found in the following table included in my written statement. The table is based on families living in States that have UF programs.

Three facts are especially noteworthy: (1) The table shows that for a wage rate under \$4 an hour, for instance, the rate of participation of these families, that is, the percent of these families predicted to be on AFDC-UF averages perhaps 1½ percent.

[The following was subsequently supplied for the record:]

Wage rate of husband <sup>1</sup>	Total number of families in UF States <sup>2</sup>	Percent of total families on AFDC-UF (predicted) <sup>3</sup>	Earned income <sup>4</sup>	Nonearned income I <sup>5</sup>	Nonearned income II <sup>6</sup>
Less than \$2.....	282, 019	2. 12	\$8, 045	\$377	\$1, 053
\$2 to \$2.99.....	706, 298	1. 36	9, 834	267	906
\$3 to \$3.99.....	1, 482, 334	.95	11, 294	364	884
\$4 to \$4.99.....	2, 492, 568	.85	12, 706	396	960
\$5 to \$5.99.....	3, 090, 566	.60	15, 139	353	1, 039
\$6 to \$6.99.....	2, 849, 686	.49	17, 395	254	1, 192
\$7 to \$7.99.....	2, 037, 348	.36	20, 042	191	1, 148
\$8 to \$8.99.....	1, 357, 436	.29	22, 861	131	1, 326
Over \$9.....	2, 735, 667	.21	30, 058	109	1, 787

<sup>1</sup> Annual earnings divided by (annual weeks worked times usual hours per week).

<sup>2</sup> Weighted count of families that are married, spouse present with children aged less than 21 and with a male head, aged less than 65, able to work, and not in the military or an institution.

<sup>3</sup> Predicted percent of these families on AFDC-UF, based on the author's statistical analysis.

<sup>4</sup> Family earned income.

<sup>5</sup> AFDC, public welfare, and unemployment insurance compensation.

<sup>6</sup> Other nonearned income (interest, dividends, property income, pension income, veterans' payments, etc.).

Source: Survey of Income and Education. Survey reference year is 1975.

Senator MOYNIHAN. Can I say that is 1½ times the predicted amount?

Mr. HOSEK. No, the prediction is that of all of these families, 1½ percent of families having wages below \$4 would be on. Let me emphasize here that this prediction regarding the percent on UF isn't based on a prior calculation determining the families eligibility to be on UF. It is an unconditional percentage relative to all families falling in the population defined.

Senator MOYNIHAN. Yes.

Mr. HOSEK. Thus the percentage of these families predicted to be on UF is low even in the lowest wage group, (2) Families with low-wage husbands are characterized by multiple earners—note that a man earning \$2 per hour would have annual gross earnings of roughly \$4,000 if employed full time, whereas family earnings were over \$8,000 in 1975 in this wage group—

Senator MOYNIHAN. That is nice work.

Mr. HOSEK. (3) Although it is not apparent from the table, many of the husbands receive veterans' payments, which help account for the sizable level of nonearned income II.

As a rough gage to the income adequacy of these families with respect to welfare, the maximum potential sum of UF benefits and food stamp bonuses for a family of four averages \$425 per month—or \$5,100 per year—across the UF States. By this simple guide market opportunities would appear to dominate UF, on average, in each wage class. As a consequence, there may be little reason to worry that UF participation rates are "too low".

On the other hand, this average calculation does not imply income support is adequate for all these low-wage families.

I now turn to the issue of mandating the UF program for all States. By my predictions, and assuming that the eligibility requirements of the UF program are not changed, extension of the UF program to all

States would result in 24 more States being on the program, and an additional monthly caseload of 30,000 cases. This amounts to a 26-percent increase in the national monthly caseload. The maximum increase in AFDC-UF benefits would be \$8.2 million per month, or approximately \$100 million a year.

The maximum increase in food stamp bonuses would be \$3.7 million per month, and as to the maximum increase in medicaid payments, I do not have an estimate.

The increase in caseload is small because many non-UF States are not heavily populated and, assuming they adopt their current AFDC benefit schedule, they would offer low benefits under UF. In fact, caseloads predicted for nine of the added States are below 200 cases.

Senator MOYNIHAN. There is Utah again.

Mr. HOSEK. Yes.

Given the small predicted caseloads as well as the existence of setup and administrative costs associated with a UF program, it may be understandable why these States have currently chosen not to have a UF program.

To sum up, I believe this line of research has been successful in identifying a set of factors that appear to be important to determining family participation in UF. The research also suggests, although less solidly that certain administrative variables may not be important determinants of participation.

Further, given the similarities between the UF program and possible negative income tax programs, the information on the determinants of family participation in UF may carry over, at least qualitatively, to the issue of participation in these alternative programs.

Finally, it appears that without changing the basic identity of the UF program, extension of the program to the rest of the country would not cause much increase in caseload or cost.

Thank you.

Senator MOYNIHAN. We thank you, sir.

Just as Dr Lyon gave us some concise and important information, so did you. It is a mystery which I won't ask you to comment on, that if AFDC-UF were to be made mandatory it would have this modest effect, but it has assumed symbolic proportions in the minds of persons now interested in welfare reform. Quite a number of people now insist it has to be done, and that States have to be made to do it even though the States don't want to. We had some testimony concerning the legislation that Senators Long and Cranston and I introduced last summer which would move toward the general direction of the Federal Government assuming a larger proportion of the costs and let States make more of their own choices. Someone was testifying on behalf of the Council of State Legislatures, saying this was altogether unacceptable, this would be an impossible change, that we had to do this and we had to do that, we needed a program thus and so, and always describing things we have in New York City and they don't have in her State, and I said to her, well, I thought they were good things to have, we had them, we paid for them ourselves, you don't in your State.

Let me ask you this, I said: "It there any possibility, can you imagine any Senator from your State voting for such a bill as you desire here in the Senate?" "Oh no," she replied, "none of our Senators would ever vote for that bill."

So here we are, once again. It is our primary political role in New York State to persuade other people to do things that they don't want to do by offering ourselves to pay for them and expect somehow gratitude in return. That is the really great logical flaw.

You know the New York story about the fellow standing on the street corner and a complete stranger walks up and knocks him down, and the man looks up and says, "Why did you do that? I have never tried to help you."

When I think of the legislative history of the last 50 years in the Congress, I am surprised as a New Yorker that I can make my way from one end of this building to another without being tripped over or harassed in some way.

We have had a very good morning of testimony, we have learned a lot about what is known and a lot is known. I would like to emphasize that. In fact, I would like to say that this all began at Rand. I don't know when it was you made your decision to go into domestic social issues, but it was a powerful one, back in the early sixties.

Mr. LYON. 1968.

Senator MOYNIHAN. Well, it has had real consequences. You have brought truly powerful techniques to bear and we know more. Not everything we know has encouraged us, but would it be enough to leave you with that 19th century American saying that it is not ignorance that hurts so much as knowing all those things that ain't so.

And with that, we will close this morning's hearing.

Mr. LYON. Thank you.

Mr. HOSEK. Thank you.

[The prepared statement of Mr. Hosek follows:]

THE FAMILY'S DECISION TO PARTICIPATE IN THE AFDC-UNEMPLOYED FATHERS PROGRAM

(By James R. Hosek)<sup>1</sup>

Mr. Chairman and Members of the Committee: Thank you for the opportunity to report on research into family participation in the AFDC-UF program, which I shall refer to as UF for short. My testimony will deal with three issues: what determines participation in UF, why is the UF participation rate so low, and what are the consequences of extending the UF program to states that do not currently offer it.

The UF program is designed to aid husband-and-wife families, a group that is allegedly inequitably served under the existing income maintenance system. In 1975 26 states plus the District of Columbia had active UF programs, with a total monthly caseload of about 117,000 families. It is *argued* that by mandating the UF program to all states and relaxing federally-stipulated eligibility requirements, the program would be a means of improving the income support system for "working poor" families.

FAMILY PARTICIPATION IN UF

Researchers have known for several years that the rate of family participation in UF is low, especially as compared with participation in AFDC-FG (which serves single parent families). The UF participation rate has been estimated at 15 to 30 percent,<sup>2</sup> meaning that only 15 to 30 families out of every 100 presumably eligible families actually participate in UF in a given month. In contrast, the

<sup>1</sup> Economics Department, The Rand Corporation. The views expressed in this paper are the author's own and are not necessarily shared by The Rand Corporation or its research sponsors. The author would like to thank Richard Buddin for his expert research assistance, and also Philip Armstrong and David Lyon for their contributions to this research.

<sup>2</sup> Boland (1973), Hollenbeck (1975), Lidman (1975), and Rein (1972).



rate for FG was estimated at 71 percent in 1970.<sup>3</sup> With such a low participation rate in UF there is concern whether UF could be an effective component of welfare reform. This concern has provided an impetus to study the determinants of family participation in UF.

Several studies of participation have been based on aggregate data.<sup>4</sup> Probably as a result of the crudeness of the data, these studies had difficulty in obtaining sensible effects on crucial variables. In particular, the studies raised uncertainty as to the importance of the benefit level and the family's employment opportunities.

In comparison to these studies, my research has utilized family-level data, and the results strongly support the view that certain factors determine whether a family will participate in UF. A similar picture emerges for blacks and whites alike: A family is more likely to participate in UF if its alternative economic opportunities are less attractive relative to the benefits they would receive on AFDC-UF. Thus, a family's chance of participating is greater the higher potential benefits on UF, the lower the husband's wage rate and education, the lower the wife's age and education, and the lower the benefits the husband would receive under unemployment insurance. The wage rate and education of the husband may be interpreted as measures of his opportunities in the labor market. Similarly, the earnings potential of a wife is known to depend positively on her age and education, so again the latter variables may be viewed as a measure of her labor market opportunities. Finally, unemployment insurance can be thought of as an alternative form of income support, and the results suggest that families treat UF as a substitute for unemployment insurance. *In related work I have also found that the chance of a family being on AFDC-UF rises as the unemployment rate rises.*

A direct policy implication of these findings is that an increase in UF benefits will lead to an increase in UF caseload. A one percent increase in benefits, I estimate, will cause an approximate one percent increase in caseload.

#### THE EFFECT OF ADMINISTRATIVE VARIABLES

There is considerable speculation as to whether the low rate of participation in UF can be attributed to factors concerning family eligibility and program operation. With respect to these variables, I have recently undertaken a cross-state analysis of UF caseloads. Controlling for the variables mentioned above, the results are that none of the administrative variables which were tried has any effect on state caseload. The variables are: the limit on liquid and semiliquid assets a family must not exceed in order to be eligible for UF, the state's error rate in handling its entire AFDC caseload, and the amount per AFDC case spent in training caseworkers. By my interpretation, these findings do not prove that administrative variables are inconsequential, but only that available data may be a crude counterpart to the theoretically relevant concepts. Indeed, as Dave Lyon has mentioned, changes in administrative variables do appear to have at least a short run effect on caseload.

Two other administrative variables have drawn attention. One is the requirement that a father, in order to be eligible for UF, must have worked in six of the 13 calendar quarters prior to application for support; the other is that a father on UF must work no more than 100 hours per month. These requirements are set by the federal government and do not vary across states, so it is impossible to obtain statistical estimates of the effect of changing them on a family's decision to participate in UF. Still, my viewpoint is that relaxing or eliminating these requirements would cause only a modest increase in participation. This is because most husbands have a strong attachment to the labor force and probably satisfy the prior work rule; and because even if the 100 hour rule were eliminated, the family would soon come up against the limit on earned income, above which UF benefits are zero.

This conjecture, coupled with my statistical results, implies that changes in eligibility requirements and administrative practice do not seem likely to cause major increases in the rate of participation in UF. This leads me back to the earlier result that families are more likely to participate in UF the less attractive their alternative economic opportunities. In other words, the participation rate

<sup>3</sup> Boland (1973).

<sup>4</sup> See Sumrall (1976) for further description; Wiseman (1976) is an exception and employs microdata.

may be low because many families can and do choose to take advantage of market opportunities. Some evidence of this is found in the following table based on families living in states that have UF programs.

Wage rate of husband <sup>1</sup>	Total number of families in UF States <sup>2</sup>	Percent of total families on AFDC-UF (predicted) <sup>3</sup>	Earned income <sup>4</sup>	Nonearned income I <sup>5</sup>	Nonearned income II <sup>6</sup>
Less than \$2.....	282, 019	2. 12	\$8, 045	\$377	\$1, 053
\$2 to \$2.99.....	708, 298	1. 36	9, 834	267	906
\$3 to \$3.99.....	1, 482, 334	. 95	11, 294	364	884
\$4 to \$4.99.....	2, 492, 568	. 85	12, 706	396	960
\$5 to \$5.99.....	3, 090, 556	. 60	15, 139	353	1, 039
\$6 to \$6.99.....	2, 849, 686	. 49	17, 395	254	1, 192
\$7 to \$7.99.....	2, 037, 348	. 36	20, 042	191	1, 148
\$8 to \$8.99.....	1, 357, 436	. 29	22, 861	131	1, 326
Over \$9.....	2, 735, 667	. 21	30, 058	109	1, 787

<sup>1</sup> Annual earnings divided by (annual weeks worked times usual hours per week).

<sup>2</sup> Weighted count of families that are married, spouse present with children aged less than 21 and with a male head aged less than 65, able to work, and not in the military or an institution.

<sup>3</sup> Predicted percent of these families on AFDC-UF, based on the author's statistical analysis.

<sup>4</sup> Family earned income.

<sup>5</sup> AFDC, public welfare, and unemployment insurance compensation.

<sup>6</sup> Other nonearned income (interest, dividends, property income, pension income, veterans' payments, etc.).

Source: Survey of Income and Education. Survey reference year is 1975.

Three facts are especially noteworthy: (1) The percentage of these families predicted to be on UF is low even in the lowest wage group; (2) Families with low-wage husbands are characterized by multiple earners—note that a man earning \$4.50/hr would have annual gross earnings of roughly \$9,000 if employed full time, whereas family earnings averaged \$12,700 in 1975 in the \$4.00-4.99 wage group; (3) Although it is not apparent from the table, many of the husbands receive veterans' payments, which help account for the sizable level of Nonearned Income II.

As a rough gauge to the income adequacy of these families with respect to welfare, the maximum potential sum of UF benefits and Food Stamp bonuses for a family of four averages \$4.25 per month—or \$5,100 per year—across UF states. By this simple guide market opportunities would appear to dominate UF, on average, in each wage class. As a consequence, there may be little reason to worry that UF participation rates are "too low." On the other hand, this "on average" calculation does not imply that income support is adequate for all these low wage families.

#### EXTENSION OF THE UF PROGRAM TO ALL STATES

Extending the UF program to all states, but not changing eligibility requirements, leads to the following overall predictions (based on 1975 data):

States added.....	24
Monthly caseload added.....	30,000
Percent increase in monthly caseload.....	26%
Maximum increase in AFDC-UF benefits <sup>a</sup> .....	\$8.2 million/month
Maximum Increase in Food Stamp bonuses <sup>a</sup> .....	\$3.7 million/month
Maximum Increase in Medicaid Payments.....	No estimate

The increase in caseload is small because many non-UF states are not heavily populated and, assuming they adopt their current AFDC benefit schedule, they would offer low benefits under UF. In fact, caseloads predicated for 9 of the added states are below 200 cases. Given the small predicted caseloads as well as the existence of set-up and administrative costs associated with a UF program, it may be understandable why these states have currently chosen not to have a UF program.

<sup>a</sup> AFDC benefits are calculated assuming the family has no countable income.

<sup>a</sup> Food Stamp benefits assume 100 percent participation in the Food Stamp program among AFDC-UF families, with each family claiming a \$70/month shelter allowance and receiving the maximum allotment given family size and AFDC benefits. No subtraction is made for Food Stamps that might already be received by the family in the absence of the AFDC-UF program.

## CONCLUSION

First, the research has been successful in identifying a set of factors that appear to be important in determining family participation in UF. The research also suggests, although less solidly, that certain administrative variables may not be important determinants of participation. Also, given the similarities between the UF program and possible negative income tax programs, the information on the determinants of family participation in UF may carry over, at least qualitatively, to the issue of participation in these alternative programs.

Second, it appears that without changing the basic identity of the UF program, extension of the program to the rest of the country would not cause much increase in caseload or cost. Thank you.

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[Whereupon, at 1:25 p.m., the subcommittee was recessed to reconvene the following day.]

# WELFARE RESEARCH AND EXPERIMENTATION

FRIDAY, NOVEMBER 17, 1978

U.S. SENATE,  
SUBCOMMITTEE ON PUBLIC ASSISTANCE  
OF THE COMMITTEE ON FINANCE,  
*Washington, D.C.*

The subcommittee met, pursuant to recess, at 10:10 a.m. in room 2228, Dirksen Senate Office Building, Hon. Daniel P. Moynihan presiding. Present: Senator Moynihan.

Senator MOYNIHAN. I would like to say a very pleasant good morning to our audience, our guests, and our witnesses. This will be the third and concluding day of our hearings and it is perhaps not inappropriate to use the occasion to stress my own appreciation for the way the press has reported these hearings, which are an effort by this subcommittee to bring us up to date on a quite extraordinary development of the last 15 years or so in the United States, the beginning of an experimental mode with respect to social policy.

This is something without precedent, at least on the scale in which it has been carried out.

Learning to use this kind of information is a challenge. The information is new, and learning to use it is something that a social and political system has to have practice with. I was a little concerned at the beginning of the hearings that there would be rather ominous implications as to why we were holding them. The answer is that we are hearing the results for the same reason that the experiments were carried out.

As Dr. Shalala explained yesterday, HUD has spent \$180 million in providing various kinds of financial subsidies to families to see what they do with it. And the same impulse that suggests it would be a good idea to spend \$180 million suggests it would also be a good idea to find out what happened and to have the Congress find out.

Not all of these findings are as predicted. If you were in a situation in the world where anything you expected to happen always did happen whenever you carried out an inquiry then you could feel fairly comfortable not carrying out any inquiry. It is because sometimes we find out things that are what social scientists call counterintuitive, that is experiments are worth conducting.

And, as many scientists will tell you, everything is counterintuitive until you understand it. That is plain and simple.

There was a remark made by Hannah Arendt once which is not irrelevant here. She referred to the totalitarian elites of the 1920's and 1930's in Europe whose tactical superiority she said, lay in the ability to translate every statement of fact into a question of motive. I hope we will not see any more of that than is minimally necessary in a political capital having to do with a very difficult subject.

I contrast the calm of these hearings with the turbulence on the subject over the previous decade. That may be a sign of the passing of political vitality.

Our first witness today—we have three scholars and we will end up, of course, with our distinguished Assistant Secretary of HEW—our first scholar is Dr. Robert G. Williams of Mathematica, the organization that first analyzed the New Jersey income experiments.

Dr. Williams?

Good morning, sir. We welcome you to the committee.

You have two statements, I see, a long one and a short one. One is a summary?

Mr. WILLIAMS. The one was supposed to be a summary. I hope it is not too long for that purpose.

Senator MOYNIHAN. Please proceed exactly as you like. Would you like to put your full prepared statement in the record?

Mr. WILLIAMS. I would, please.

Senator MOYNIHAN. We will do that as if read, and you proceed, please, exactly as you wish, sir.

Mr. WILLIAMS. Thank you. I do have a brief, oral summary of that statement.

#### **STATEMENT OF ROBERT G. WILLIAMS, MATHEMATICA POLICY RESEARCH, INC.**

Mr. WILLIAMS. Mr. Chairman and members of the committee, I appreciate this opportunity to appear before you today. I will be discussing with you the work my firm has been doing to test new accounting periods and reporting requirements in the AFDC program. We believe that the results obtained thus far from this work have significant potential for improving the administration of existing public assistance programs as well as any new programs that might be considered by Congress in its welfare reform deliberations.

Since 1975, Mathematica Policy Research has been involved in the design, implementation, and analysis, of the Colorado monthly reporting demonstration. Considerable impetus for the initial funding of this project came from simulation studies and practical experience based on the use of new accounting periods and reporting requirements in federally funded income maintenance programs. The monthly reporting system that was developed for this project has three elements.

The first element is a monthly reporting requirement. Recipients are required to return a simple but comprehensive form each month as a requirement for continued eligibility. This contrasts with a much less frequent reporting schedule of every 6 months for AFDC cases in the traditional system.

The second element is a monthly retrospective accounting period. Each month's eligibility review and grant determination is based on actual circumstances of the recipient in the month prior to payment. This contrasts with a prospective accounting principle in the traditional AFDC system in which eligibility and grant amounts are based on estimates of recipients' needs in the month of payment.

The third element is an automated support system. The monthly reporting system automates most eligibility determination and grant computation processes. This level of automation contrasts with a heavy

reliance on manual processing for such functions currently in most jurisdictions.

As initially implemented, the Colorado monthly reporting demonstration had two major components which serve as the basis for the research findings.

The first major component was a controlled experiment to test the impact of the monthly reporting system on recipients and its effects on transfer payments. This experiment was conducted by randomly selecting 10 percent of the Denver County caseload to receive payments under the monthly reporting system and comparing their benefit payments with those made to a statistically comparable control group.

The second major component was an administrative pretest to determine the impact of the monthly reporting system on administrative costs and agency functioning. The pretest was conducted by implementing the monthly reporting system for the entire caseload in Boulder County which consisted of about 1,200 cases.

I would like to summarize for you certain key findings concerning the effects of monthly reporting and retrospective accounting in the AFDC program. These findings are presented in more detail in my prepared statement. The findings are based on 1 year of operations for both the experiment and pretest.

The findings are organized into three major categories. The first category is the impact of the monthly reporting system on transfer payments both in AFDC and in other related programs. Overall, there were payment savings during the first year which averaged 6 percent for monthly reporting system cases. These savings appear to result almost entirely from more timely discontinuance of AFDC cases by the monthly reporting system.

A proportionate level of savings was likely to have accrued for AFDC cases receiving medicaid, since eligibility for the two programs is directly linked. Similarly, an indirect link between AFDC and food stamp eligibility means that there were probably significant savings in the food stamp program as well.

In addition, the monthly reporting system identified and acted upon three times as many changes in recipients circumstances for experimental cases as did the traditional administrative system for control cases. This implies that the traditional AFDC administrative system simply missed many changes that should have affected recipient eligibility and grant amount.

The second category of findings is the impact of the filing requirement and accounting period on recipients. Recipients demonstrated the capability to comply with filing requirements quite effectively. About 90 percent of recipients who filed did so within 8 days of the time the monthly status report was mailed to them. Only 10 percent of the monthly status reports filed by recipients required a followup contact due to omissions, errors, or major changes.

There is no evidence that any otherwise eligible recipient was forced off the program by the filing requirements. However, temporary problems appear to have been caused for some recipients by the transition to new payment dates when the monthly reporting system was initially implemented. The potential variability in payment

dates from month to month also created budgeting difficulties for some recipients.

The third major category of findings is the effect of the month reporting system on administrative costs and functioning. There was an increase of between 2 to 4 percent in administrative costs per case resulting from implementation of the monthly reporting system.

AFDC eligibility workers responded favorably to implementation of the monthly reporting system. They cited increased administrative control and improved communication with recipients as the primary benefits.

Under the monthly reporting system, corrective payment actions by the agency diminished substantially. Retroactive adjustments declined by 60 percent. Check cancellations declined by 75 percent, as did recovery initiations.

The results obtained to date from the Colorado monthly reporting demonstration are very encouraging. They offer considerable evidence that implementation of a monthly reporting requirement and a retrospective accounting period, in conjunction with an automated support system, would represent a major improvement in the administration of the AFDC program. The underlying reason for the effectiveness of this system is that it gives adequate recognition to the high rate of change in household composition and income within the recipient caseload. A striking statistic that emerged from the research is the finding that almost 30 percent of the AFDC caseload on monthly reporting experienced a change in circumstances significant enough to affect either their eligibility or amount of payment during each month.

Senator MOYNIHAN. That is indeed a striking fact.

Mr. WILLIAMS. We think so.

This finding provides convincing evidence that the financial needs of AFDC caseloads undergo frequent fluctuations. It also suggests that monthly reporting and rapid processing of payments are necessary if administration of the program is to be accurate and responsive.

It should be apparent that the benefits obtained in the AFDC program from the monthly reporting system are relevant not only to AFDC. Rather, the findings extend by implication to other programs intended to serve low-income households. The potential for improving the administration of the food stamp program utilizing this type of system is particularly intriguing since there is substantial similarity and large overlap between the recipient populations of that program and AFDC.

Moreover, should this committee report a proposal for a new, national income maintenance program, the experience gained with the monthly reporting system in Colorado could serve as a valuable course of practical experience in specifying reporting requirements and accounting periods. From the findings that have been obtained to date from the Colorado monthly reporting demonstration, it appears that the features that have been tested can provide a mechanism for making public assistance payments more accurate and responsive than payments made under the administrative formulas of our present programs.

Senator MOYNIHAN. Well, sir, that is an extraordinarily compact and precise statement. Let's get some sense of the amounts that you are talking about here.

If you are referring to an overall first-year payment savings of an average of 6 percent in a sample that is, you know, a respectable sample that Mathematica would draw, you are talking about \$600 million or \$700 million a year if this turned out to be a valid national estimate, are you not?

Mr. WILLIAMS. That would be correct in terms of the AFDC program if the findings from the Denver experiment are representative of what would be found in other jurisdictions.

Senator MOYNIHAN. Right. And if it went over to food stamps, then you could be talking about as much as \$1 billion in savings.

Mr. WILLIAMS. Potentially that would be correct.

If I could, I would like to stress that, from a statistical point of view, one of the limitations of this type of experiment is that it was done at a single site, so that although the findings are representative within that site—that is to say, within Denver—they are not, strictly speaking, applicable to other jurisdictions.

At that point one would have to rely on one's professional judgment, I would say, of similarities.

Senator MOYNIHAN. Or take another bore. Try it in Bangor, Maine, and try it in Lowesville, Pa.

The point I would like to make is that we are still near the onset of an experimental mode in social policy—although this is not, strictly speaking, a social policy so much as it is management—and there is much to be learned here. We are still learning the techniques of learning. Some of the scholars who were here the other day were saying well, what have we learned from the income guarantee experiments? We are learning how to run one.

I have had occasion to read the testimony prepared by Secretary Aaron in which he refers to some of the things that have been learned about accounting procedures and possible savings. Clearly, this is something that has been successfully tried in Denver. It is within the range of possibility, I assume, for any State government to do this, if it wishes, and it is probably within the range of HEW to ordain it and the Congress to legislate it, is it not?

Perhaps that is not fair to ask you. You are an economist and not a politician, but what is your understanding?

Mr. WILLIAMS. First I would like to point out that HEW has recently proposed regulations to permit States to implement this type of system on an optional basis.

Senator MOYNIHAN. Why should it be optional if it is something that has the potential of enormous savings?

Well, that is not fair to ask you.

Mr. WILLIAMS. I would be happy to offer my judgment on that issue.

Senator MOYNIHAN. Please do.

Mr. WILLIAMS. When we were talking about changing existing systems as opposed to building requirements into a new program where we are starting fresh, then I think, based on our experience in doing this in one State so far, which is all that has been done anywhere, that we would counsel patience and judicious use of the carrot, rather than some kind of immediate mandating of this type of program.

Senator MOYNIHAN. Well, the savings are shared, so there is an incentive to it.



Mr. WILLIAMS, That is right.

The reason that we would counsel patience is that it is hard for me to overemphasize the amount of thoroughgoing change that is required in existing agency practice and procedures and structure in order to accommodate this type of system. If it is to be done right and if the maximum benefits are to be gained and if the transition is to be orderly, it is something that takes considerable time and considerable effort on the part of everyone involved in this type of implementation.

Senator MOYNIHAN. Right, but, in theory, we do not operate our welfare programs for the convenience of the welfare agencies. In practice we do, as anybody who studies bureaucratic behavior knows, but that is another matter.

What you have learned is obviously something that has a cost to recipients. They have to report what happens to them, and as they do, they report when they come into extra income and in consequence they lose some welfare income, but still they do it. You do not find any great resistance to it?

Mr. WILLIAMS. No. There has periodically been some opposition from a selected number of recipient advocate groups; basically around transition periods. When we first went in with the experimental sample in Denver County and now that we are expanding to 100 percent of the caseload in Denver County, we have found some opposition. I believe it has been based primarily on two things—one is a reaction to the reporting requirement, even though I think objectively it is not an onerous one; and the second is the fear that this type of system will be used as a harassment mechanism.

Now, we believe there are substantial advantages to recipients. They do have to report monthly; that is true. But the advantages they get in exchange for this are very substantial.

This finding that we are picking up more than three times as many changes in grant means—

Senator MOYNIHAN. The change goes down as well as up.

Mr. WILLIAMS. That is correct, and in fact, we are picking up proportionately more increases in grants than we are decreases.

Now, for a variety of reasons that would take me too long to explain, that does not interfere with the savings, but the fact is that there are a lot of recipients getting underpaid out there under the existing system.

Senator MOYNIHAN. Even though you overall have a 6-percent savings, in terms of the experience of individuals, more individuals are going to get increases than decreases.

Mr. WILLIAMS. That is correct and the problem from the standpoint of the recipient groups is that this is something that is relatively transparent to the given recipient at a given point in time, whereas the reporting requirement is not; it is something that is quite visible.

Now there is another advantage which relates to the first. This system provides a mechanism with certainty of reporting that requires the agency to act upon a recipient report. It also gives information at the other end. It provides information to the recipient about what the recipient reported and how that was used to compute the grant.

It really gives the recipient much more control, input, and information at the other end than they have ever had before.

Senator MOYNIHAN. They have some idea of how this turns out the way it does.

Mr. WILLIAMS. Yes.

Senator MOYNIHAN. It strikes me as being as close to Pareto-optimality as anything that we have had proposed in these hearings so far.

Dr. Williams, we thank you very much and let me take this occasion, as I have done before, to express the admiration of this subcommittee for the work which Mathematica has done. You have developed a capacity here that has large consequences for American social policy and we admire it. We do not admire it so extensively that we are not aware that we pay for it, but we are getting good value for whatever it is we pay.

Thank you very much.

Mr. WILLIAMS. Thank you. We very much appreciate your comments.

[The prepared statement of Mr. Williams follows:]

PREPARED STATEMENT OF ROBERT G. WILLIAMS, VICE PRESIDENT,  
MATHEMATICA POLICY RESEARCH, INC.<sup>1</sup>

INTRODUCTION

Mr. Chairman and Members of the Committee, I appreciate this opportunity to appear before you today to discuss the work my firm has been doing to assist in designing and testing improvements in the administration of public assistance programs. For the past three years, Mathematica Policy Research has been involved in the design, implementation, and analysis of the Colorado Monthly Reporting Demonstration. This project was a field test in the AFDC program of a monthly reporting requirement, a monthly retrospective accounting period, and an automated support system. The project had two components: an experiment in Denver County to test the effects of the Monthly Reporting System on transfer payments, and a pretest in Boulder County to determine the system's impact on administrative functioning and costs.

The Colorado Monthly Reporting Demonstration had its origins in the federally sponsored income maintenance experiments. In each of these experiments, participating low-income families were required to submit monthly reports of income and family composition as a basis for determining on a retrospective basis their continued eligibility and amount of benefits. Administrative experience from these experiments indicated that monthly reporting and retrospective accounting could be administered in a practical manner and that recipients could comply with the filing requirements of such a system. Additional data from the experiments demonstrated that income and household composition patterns in low-income households were considerably more variable than had been previously thought, raising the possibility that the less frequent reporting and prospective accounting systems generally used in public assistance programs might not be accounting for many changes in household circumstances. The potential benefits of using monthly reporting and retrospective accounting in the regular AFDC program prompted HEW to sponsor a simulation study conducted by Jodie Allen, then of the Urban Institute. Using data from the New Jersey, Seattle, and Denver income maintenance experiments, this study projected significant aggregate reductions in assistance payments if monthly reporting and retrospective accounting were used in the AFDC program.

Based on these findings, the Department of Health, Education, and Welfare and the Colorado Department of Social Services initiated the Colorado Monthly Reporting Demonstration in July 1975 to test whether monthly reporting and retrospective accounting, in conjunction with an automated support system, could increase the accuracy and responsiveness of benefit payments. The project was also intended to assess the administrative feasibility of such a system and determine the impact on overall administrative costs.

<sup>1</sup> Opinions expressed are those of the author and do not necessarily represent the views of sponsoring agencies.

Results from the first year of the project indicate that it has been very effective in attaining its original goals. The monthly reporting system appears to have improved greatly the accuracy of benefit payments to AFDC households. Findings from the experiment in Denver County indicate an overall savings in aggregate benefit payments of more than six percent in the first year. The responsiveness of the AFDC program to changes in recipient needs also seems to be considerably increased. The Monthly Reporting System has detected and acted upon three times as many changes in household circumstances as the traditional system. These improvements have been achieved, along with other administrative benefits, with only a small increase in administrative costs, in the range of two to four percent.

These results offer considerable encouragement that monthly reporting and retrospective accounting can significantly improve the administration of such programs as AFDC and Food Stamps, as well as any programs for low income households that might be enacted by Congress as a result of its welfare reform deliberations. In the rest of my statement, I will describe the nature of the Colorado project in more detail, provide additional information on research results that are currently available, and describe the ongoing effort to expand the range of useful information provided by the research and develop administration support systems for implementation of monthly reporting and retrospective accounting on a border scale.

#### DESCRIPTION OF THE COLORADO MONTHLY REPORTING SYSTEM

In describing the characteristics of the prototype system that was developed for the project, I would like to contrast its features with those of the administrative system traditionally used in determining eligibility and computing benefits for the AFDC program. Although the Committee is aware that there is considerable variation in the manner that the states and territories administer AFDC, there is enough similarity in broad administrative principles for us to refer with reasonable accuracy to a "traditional AFDC system." Comparison of the Monthly Reporting System in two Colorado counties with the traditional AFDC system used in the rest of Colorado is the source of information concerning the impact of monthly reporting on recipients, levels of transfer payments, and administrative patterns and costs.

The Monthly Reporting System developed in Colorado consists of three basic elements: a monthly reporting requirement, a monthly retrospective accounting period, and an automated support system. Recipients are required to return a simple but comprehensive form to the agency each month as a requirement for continued eligibility. On this form, called a Monthly Status Report (MSR), recipients are required to report their income, household composition, and other relevant eligibility factors such as school attendance status of children over the age of sixteen. This monthly reporting requirement contrasts with the much less frequent formal reporting schedule used in the traditional AFDC system. The traditional system requires the completion of comparable forms only every six months for recipients in the regular segment of the AFDC program and only every three months for the smaller number of recipients in the unemployed parent segment of AFDC. Under the traditional administrative system, recipients are instructed to report changes in circumstances that take place within the intervals between formal reports. However, this informal reporting requirement is often difficult for recipients to interpret and for agencies to enforce. Moreover, it is often hard for recipients to penetrate institutional barriers to report a change and to be sure that agency staff will take the proper action.

According to the monthly retrospective accounting principle, each month's grant is based on actual circumstances of the recipient in the month prior to payment. In the case of the Colorado Monthly Reporting Demonstration, recipients in the calendar month reporting cycle file a Monthly Status Report detailing their actual circumstances for a given month by the fifth of the following month. The data on the Report, as verified and confirmed by the agency, serve as the basis for their next grant payment. In the traditional AFDC administrative system, however, grants are based on the agency's estimate of a recipient's "current" need, which is referred to as a prospective accounting principle. Thus, recipients' payments are based on their needs for the month in which the payment is made. Administrative processing lags require that this payment actually be estimated well in advance. Moreover, the length of time between formal reports means that the agency must estimate individual recipient needs for more than six months in advance for most AFDC cases. These estimates, which serve

as the basis for payment during that period unless subsequently altered by a recipient-initiated report of change, are computed according to a complex set of rules for projecting, averaging and predicting.

In the Colorado Monthly Reporting Demonstration, the Monthly Status Reports filed by recipients provide the input for an automated support system for the project. This support system performs many clerical and bookkeeping functions which traditionally consumed the time of eligibility workers. Upon receipt by the agency, data from the Monthly Status Reports are entered directly into a computer processing system. The system edits the Monthly Status Reports for completeness and consistency and issues reports of edit problems to eligibility workers. The system also redetermines eligibility; computes grants; produces reduction and discontinuance notices to recipients; generates checks, check registers and recipient Grant Explanations; produces case status reports for Technicians and their supervisors; and provides a management summary report of caseload status at the end of the processing cycle. This level of automation contrasts sharply with the level attained in the traditional AFDC systems. Although the states and counties vary in the level of automation of administrative functions, the traditional system is characterized by a heavy reliance on manual processing for such functions as eligibility redetermination, grant computation, and transfers of information between forms.

The most important point to be made in describing this difference in automation is that the traditional AFDC systems cannot reach the levels of automation possible under the Monthly Reporting System because of inherent limitations caused by traditional use of a prospective accounting period and less frequent reporting periods. Since under the traditional system eligibility determination and grant computation require projecting future needs based on a combination of actual past data and recipients' estimates of future needs, human discretion is required to carry out these functions. The data are too imprecise and the rules too ambiguous to permit eligibility determination and grant computation from raw data supplied by the recipient. In the Monthly Reporting System, however, actual data provided on the Monthly Status Reports is used directly by the computer to make these determinations.

#### RESEARCH FINDINGS

The research findings from this project are based on two separate components. The first component consisted of an experiment within Denver County intended primarily to provide statistically valid data on the effects of the Monthly Reporting System on transfer payments. The experiment was conducted by randomly selecting approximately ten percent of the Denver County caseload for participation in the prototype monthly reporting system. Payments made to this group were then compared with payments made to a comparably selected group of control cases, whose payments were administered under normal processes. At the time the experiment began, the Denver County AFDC caseload consisted of slightly more than 12,000 cases, so that the experimental and control groups each consisted of about 1,200 cases. As the experiment progressed, new cases were also randomly sampled to replace cases that went off assistance.

The second component of the project consisted of an administrative pretest in Boulder County in which the entire caseload of approximately 1,200 cases was transferred to the Monthly Reporting System. The administrative pretest provided data for an assessment of the effects of the Monthly Reporting System on administrative costs and agency structure.

Data are available from the first twelve months of operation for both the Denver experiment and Boulder pretest components of the project. The findings that I will be discussing with you are drawn from the report on first year results which is available in draft form. These findings are organized into effects on program transfer payments, effects on recipients, and effects on program administration.

#### *Effects on program transfer payments*

Data from the first twelve months of operation in the Denver County experiment provide strong evidence that monthly reporting and retrospective accounting have resulted in a substantial reduction in aggregate benefit payments. Cases in the Monthly Reporting System received about six percent less aggregate payments than were made to control cases in the traditional system in the entire twelve month period, even though both groups were paid based on the same benefit standard. This reduction was caused largely by a decrease in length of stay on

assistance rather than by a reduction in the size of average payments to recipients receiving benefits. Apparently, the Monthly Reporting System identified cases as ineligible and discontinued them promptly, rather than continuing payments to recipients for as much as several months after they had become ineligible.

Research from the project thus far has sought to identify changes in benefits paid only under the AFDC program. However, it is probable that a reduction in the average period of AFDC eligibility has resulted in a corresponding reduction in the period of eligibility for other programs that are linked to AFDC. Since AFDC recipients automatically qualify for Medicaid, a corresponding decrease in aggregate benefit payments for AFDC monthly reporting cases should occur in that program. We intend to estimate such decreases more precisely in future research. The total value of savings created by the Monthly Reporting System could be substantially increased since Medicaid expenditures for AFDC recipients in Colorado during this period amounted to almost one-third of expenditures for AFDC financial assistance. Savings could also be expected for AFDC recipients participating in the Food Stamp program. These savings would probably not be proportionate to the decrease in length of eligibility span since recipients could remain eligible for Food Stamps even after their eligibility for AFDC terminated. Any conclusions that we might draw about the effects of AFDC monthly reporting on benefits paid under other programs are tentative pending further research. However, it is important to take these effects into account, since any reduction in the length of eligibility for AFDC would reduce benefits paid under related programs.

One of the most significant findings from this project is that the Monthly Reporting System processed three times as many grant changes for its caseload than the traditional system did for the control caseload. Even though the two groups of recipients were statistically comparable, operation of the Monthly Reporting System resulted in computation of grant changes for 18.7 percent of ongoing cases each month, whereas operation of the traditional system resulted in computation of grant changes for only 6.3 percent of cases each month. Although some part of this difference undoubtedly derives from the use of averaging and the exclusion of irregular income under the traditional system, the magnitude of the overall difference suggests that under the traditional AFDC system many changes in recipient circumstance are simply missed. Apparently at any given time under the traditional system, a disturbingly large proportion of AFDC recipients are receiving insufficient assistance to meet their needs and are therefore underpaid, while another large proportion are receiving too much assistance for their needs and are therefore overpaid. The magnitude of this difference is larger than we would have expected, and may help to explain why there tends to be an unacceptable level of error in the AFDC program.

### *Impact on Recipients*

There has been considerable concern over the potential impact of this project on recipients. The first concern is that recipients might be unable to meet the filing requirements placed upon them and that many legitimately in need of assistance would thereby be forced off the program. A second concern is that the retrospective nature of the system might prove to be unresponsive to recipients suffering sudden setbacks, especially applicants for assistance who frequently have emergency needs. In this section of my testimony, I will discuss these two issues along with two others that have arisen during the course of the project: the necessity to change payment dates when recipients are transferred to this program and the fluctuation in payment dates that occurs from month to month for some recipients.

The Monthly Reporting System used in this project placed a new procedural requirement upon recipients: they were required to file a mail-in postage-paid Monthly Status Report each month to retain eligibility and provide data for computation of the next payment. The Monthly Status Reports were mailed to recipients three working days before the end of the monthly reporting period. If these reports were received by the fifth of the following month, the recipient received payment on the first possible payment date which was the sixteenth. Subsequent filing deadlines of the twelfth and twentieth corresponded to second and third payment dates of the twenty-third and thirtieth, respectively. The forms were designed for comprehensiveness and ease of completion. Recipients were required only to respond to questions by circling "yes" or "no" or filling in the amounts of any income received. No arithmetical computation was required of recipients; they submitted only the raw data required for automated calcula-

tion. However, recipients were required to send in pay stubs as verification of earnings and provide suitable documentation for other income and changes in household composition.

Experience gained in both the Denver experiment and Boulder pretest clearly demonstrated the ability of AFDC families to submit the required monthly reporting forms promptly and to compete them with an acceptable level of proficiency. Generally, about ninety percent of families who filed did so by the first filing deadline, which fell on the fifth of the month—about eight or nine days after the forms were mailed to the recipients by the agency. Most of the remaining families who filed did so by the second deadline of the twelfth, with only one or two percent submitting forms only in time for the third deadline of the twentieth. The level of accuracy of completed forms is high. Almost half of the forms would be computer processed and used as the basis for a grant computation without any intervention by the eligibility worker. The remaining forms required review by the worker to make corrections or review significant changes in household composition or income. However, only about ten percent of all forms processed required a family contact and, of this ten percent, only about one-third required an office by the recipient. In a survey conducted by Mathematica Policy Research of recipients in both the Monthly Reporting and traditional systems, the forms used in the Monthly Reporting System were rated easy to understand by a much larger group of recipients than in the traditional system. Significantly fewer monthly reporting recipients said that they required outside help in completing them. There is evidence that recipients found the monthly reporting forms easier to complete partly because the forms themselves were simpler and partly because the frequency of completion enabled the recipients to become more practiced in filling them out.

Fewer than five percent of the families normally failed to return a Monthly Status Report by the final filing deadline in a given month and were therefore discontinued. These families appeared to place themselves in this category deliberately since they ignored three separate warnings of the consequences for non-filing. The first warning was prominently displayed on the Monthly Status Report. Second and third warnings, which were formal notices of discontinuance, were sent after non-receipt of the Monthly Status Report on the first two filing deadlines. In the survey of participants in the project, the overwhelming majority of recipients who had been discontinued for failure to file the Monthly Status Report cited as reasons either that they decided to leave welfare or that they thought they were no longer eligible for assistance. No cases have surfaced during the course of the project in which any otherwise eligible recipients were forced to leave the AFDC program because of the filing requirements, even though the Denver and Boulder Departments of Social Services have been quite sensitive to this potential problem. Moreover, when the project was initiated in Denver County, eligibility staff attempted to contact each recipient that had failed to file a Monthly Status Report when the project was implemented, but were unable to find any cases of recipients who were otherwise eligible but had failed to file. The rate of reapplication for AFDC has been no higher for monthly reporting cases than those recipients functioning under the traditional system and the number of hearings requested has been slightly lower under the Monthly Reporting System. Although further research is being conducted by the State of Colorado regarding this issue, it appears that the requirement for recipients to file a Monthly Status Report can be placed upon recipients without causing major problems of compliance provided that the forms are designed to be simple and easy to complete.

Contrary to initial fears, the retrospective accounting principle does not appear to have had a significant adverse effect on recipients. In this project, the effects of a retrospective accounting principle were mitigated by processing Monthly Status Reports rapidly, thereby minimizing the lag between the occurrence of changes in circumstances and receipt of a payment reflecting those changes. This aspect of the design has enabled more than eighty percent of recipients to receive their payment on the early payment date, the sixteenth of each month, only a half month after the end of the reporting period. Almost all of the remaining cases receive payment on the normal payment date, the twenty-third of each month, with only one to two percent receiving payment late, on the thirtieth. Another potential adverse effect from the retrospective accounting principle is mitigated by calculation of payment on a "current" need basis for new cases, which are those most likely to experience sudden drops in income or other types of emergencies. Although the "current" need computation for new applicants is

lower than the retrospectively based computation only for thirteen percent of the cases, existence of the alternative basis for providing benefits has avoided possible hardships for this minority of cases.

With the design features of rapid payments processing and alternative computation of benefits for new cases, there remain very few recipients who are adversely affected by the retrospective accounting principle. Cases that do encounter emergencies can be accommodated through use of General Assistance or emergency Food Stamps. In considering the impact of the retrospective accounting principle, it is important to understand that the ideal of the prospective system to meet "current" need is frequently not attained in practice. As discussed earlier in my testimony, it is apparent from the results of this project that the infrequent reporting requirement of the traditional system causes many changes in recipient circumstances to be undetected or ignored. The ability of the traditional system to meet current need is also severely compromised by long delays in processing grants, as well as the human inability to predict the future with accuracy. Consequently, it appears that the Monthly Reporting System with retrospective accounting does not appear to cause substantially more numerous critical recipient shortfalls in resources than the traditional system. Welfare agencies can cope with those shortfalls that do occur.

Although the rapid processing of Monthly Status Reports has contributed markedly to the overall responsiveness to recipient needs, the design of the payment cycle in the Monthly Reporting System did introduce two types of problems for recipients. The first was simply a transition problem. Recipients were paid soon after the first of the month under the traditional system and the transfer to the monthly reporting payment dates of the sixteenth, twenty-third, or thirtieth caused temporary problems for some recipients. Even though all recipients so transferred were given transition payments, some had difficulty adjusting their family budgeting to the new cycle and encountered problems in the payment of rent. These were resolved on an individual basis by the agencies. The second was a problem of variability of payment date. The use of alternative payment dates in the Monthly Reporting System caused additional anxiety about whether the form was completed correctly according to our recipient survey. Moreover, a somewhat higher proportion of recipients perceived their checks to be late under the Monthly Reporting System and reported the need to borrow money. Because of the possibility of receiving checks either earlier or later than the normal payment date, between a quarter (in Denver) and a third (in Boulder) of recipients experienced intervals between checks exceeding one month more than twenty percent of the time. Based on this pattern of payments, as well as recipient perceptions, the problems of transition and payment date variability bear further examination. It may well be possible to retain the responsiveness of the system that is permitted by rapid processing and variable payment date while easing the transition and alleviating recipient anxiety over the consequence of failing to file correctly.

#### *Impact of Program Administration.*

As I noted earlier, monthly reporting and monthly retrospective accounting, with an automated support system, were tested county-wide in Boulder County to determine the effects of the system on administrative costs and agency functioning. A major issue from the outset of the project was that of administrative costs. Many administrators feared that a virtual six-fold increase in written reports from recipients would result in a nearly proportionate increase in administrative costs. Data from the first twelve months of operation in Boulder County demonstrate that such a system can be operated for an increased administrative cost *per case* that exceeds the cost in the traditional system by only two to four percent.

A less prominent issue prior to implementation was the effect of the system on agency functioning. Experience in the administrative pretest has been that the system has been quite beneficial in this area. Eligibility workers report that the system has improved communications with recipients and helped them organize and control their caseloads better. In addition, the system has brought about a sharp reduction in the number of corrective payment actions that the agency has had to carry out.

A characteristic of the traditional AFDC system is a heavy reliance on manual processing. Although there are differences from state to state, a large amount of eligibility staff time is invariably spent performing clerical duties such as computation of grants and filling out forms, often copying substantial amounts of information from one form to another. In contrast, the Monthly Reporting System developed for Colorado takes full advantage of the potential for automation

inherent in a monthly reporting and retrospective accounting system. Recipient data are entered directly into the computer for machine editing. Once the data from the Monthly Status Report is complete and consistent, the computer performs the grant calculation and produces the necessary forms, notices, grant explanations, and checks (among other functions). Because so much of the clerical workload is automated under this system, the increased information flow from recipients can be handled by approximately the same level of eligibility staff as in the traditional system. There was some increase required in data entry staff, but this was nearly offset by a reduction in the amount of clerical support time required, particularly to prepare forms and check registers. As would be expected, there was some increase in the costs for computer time, postage, and printing. Taking all these factors into account, the net effect of the Monthly Reporting System on administrative costs after one year of operation was an increase of between two and four percent.

Eligibility workers participating in the experiment noted a number of administrative advantages gained from the Monthly Reporting System. One of the most significant of these was increased constructive contact between workers and recipients. The high level of automation in the system has freed workers from many routine clerical tasks and focused their efforts on obtaining information from recipients and processing the monthly report forms. Workers administering the Monthly Reporting System in Denver County estimated that their contact with recipients increased by as much as fifty percent relative to the traditional system. This permitted the workers to become more aware of recipient circumstances, further improving the accuracy of payments as well as increasing the likelihood that recipients will be referred to needed social services.

The Monthly Reporting System has also had the beneficial effect of clarifying the respective roles of eligibility workers and recipients. For the first time, recipients have unambiguous requirements with which they must comply, as well as a reliable channel for reporting changes in circumstances. In the Monthly Reporting System, recipients can even verify that information has been processed correctly. They receive computer-printed grant explanations with each check, a type of document that is almost unknown in the traditional system. Similarly, workers have the clear obligation to process the reported information, obtain supplemental data from the recipient if necessary, and ensure timely issuance of the grant.

Eligibility workers have expressed a higher degree of job satisfaction under the Monthly Reporting System. They find their jobs more demanding because of the absolute nature of deadlines and the need to learn certain new skills. However, even this challenge contributes to their feeling of greater satisfaction, as does the better organization of their daily responsibilities and, most of all, the greater sense of control over their caseloads.

Because of the small caseloads participating in the Monthly Reporting System in the two test counties, there have been no conclusive results provided on the effects of monthly reporting and retrospective accounting on Quality Control error rates. Although planned for the future, the evidence available to date cannot be considered statistically significant. Because of the increased frequency of reporting and improved precision of retrospective accounting, we expect that the system tested in Colorado has lowered absolute payment error rates by a considerable margin. However, it is important to note that the Quality Control process as utilized in the AFDC program does not—and cannot—constitute a measure of absolute program accuracy. Rather, it only attempts to measure errors in terms of the traditional administrative system, disregarding inaccurate payments that are caused by normal and legally allowable reporting and processing lags. In Colorado, for example, a recipient is allowed by statute thirty days to report a change in income. By federal rules, in addition, all state welfare agencies are allowed a month to act upon a change after it is reported. Thus, it would be possible for a recipient to begin working, report that information thirty days after the fact, and have the agency take an additional month to process that report without having an error according to Quality Control rules. Thus, an error would not be assessed against the agency even though as many as three "wrong" checks could have been issued in the absolute sense. Because of the limitations of the Quality Control measures of error rate, therefore, we believe that a better assessment of the improved accuracy of the Monthly Reporting System comes from a direct comparison of benefits paid, rather than a comparison between Quality Control error rates.



An important administrative effect of monthly reporting and retrospective accounting was the sizable reduction in corrective payment actions that took place for Monthly Reporting System cases in both Denver and Boulder Counties. Corrective payment actions represent adjustments to the amount of the grant after a payment has been generated. These include retroactive payments, which are increases to a past payment; cancellations, which void payments already issued; and initiated recoveries, which are actions by the agency to establish that an overpayment has been made and to start collection procedures if possible. Such corrective payment actions represent additional administrative costs to agencies as well as hardships to recipients. During the first year of operation, the Monthly Reporting System caused a sixty percent reduction in retroactive payments to recipients, a seventy-five percent reduction in check cancellations, and a seventy-five percent reduction in the number of recoveries that the agency initiated. Reductions of these magnitudes are directly beneficial to agencies and recipients. Moreover, they are indirectly important as measures of accuracy of payments. The substantial reduction in the number of payments needing correction is additional evidence of the improved accuracy of monthly reporting and retrospective accounting.

#### *Limitations of the Research*

The research results now available have important implications for national administrative policy relating to income maintenance programs, but their limitations should be recognized in relating the results to other jurisdictions. The first limitation of the transfer payments findings is the derivation of the results from a single site. Even though Denver County is typical in many ways, such as level of benefits, educational level of recipients, and Quality Control error rate, it would not be statistically valid to generalize the results of the monthly reporting experiment beyond Denver County. In terms of other jurisdictions, the results must be regarded as suggestive; it is likely that they would vary according to the characteristics of the recipients, the level of benefits, and the administrative practices of the agency. It seems reasonable to expect, for example, that the savings might be larger in jurisdictions with higher proportions of recipients that receive earnings, and that the savings might be smaller in jurisdictions with a higher staff/recipient ratio and/or more stringent documentation and reporting requirements than Denver County.

A second limitation is that the transfer payment results are based on one year of operation in Denver County. Within that twelve months, the pattern of variation was not stable enough to enable an accurate prediction to be made of a long-term trend. There was a general upward trend in the savings through the ninth month, at which time the Monthly Reporting System cases received 9.2 percent less in aggregate benefits than did the control cases. Thereafter, the savings declined somewhat to 6.8 percent in the twelfth month. We will shortly have results from the second year of operations that will help to predict more clearly the long-term effects of the Monthly Reporting System.

A third limitation relates to the administrative cost results. These results required a comparison of data obtained during two different years: the first year under the traditional system and the second year under monthly reporting. During this time period, there were significant policy and staffing changes unrelated to the Monthly Reporting System. There was also a marked drop in the Boulder County caseload which was probably attributable in part to monthly reporting and retrospective accounting. Moreover, the small size of the site meant that staffing adjustments had to be made in large increments in proportion to the total staff size. The magnitude of the staffing adjustments required by implementation of the Monthly Reporting System were therefore difficult to identify precisely. Finally, workload studies would have been desirable to pinpoint the nature and components of the staffing changes more precisely than these changes could be estimated in the absence of this methodology. Because of these factors, the administrative cost estimates should be viewed as being less definite than will ultimately be the case. Also, they would most likely be subject to considerable variation elsewhere based on the administrative costs and patterns of the jurisdiction in which a monthly reporting system is implemented.

#### FUTURE DIRECTION OF MONTHLY REPORTING RESEARCH AND SYSTEMS DEVELOPMENT

Based on the findings from the Denver experiment and Boulder pretest, the Department of Health, Education, and Welfare has with Congressional support embarked on efforts to expand upon the research for monthly reporting and retrospective accounting and to develop a more sophisticated support system.

The first effort has been a continuation and expansion of the Monthly Reporting System in Colorado. One doubt that was expressed about the original Monthly Reporting System was whether it could operate well in a large-volume situation. Some observers expressed concern that the number of forms that had to be processed and the complexity of the data processing system precluded workability of the design in a large agency. To test the validity of this concern, Colorado is expanding implementation of the prototype Monthly Reporting System from the original sample of ten percent of the cases to the entire caseload in Denver County. This will increase the workload from the original 1,200 cases to approximately 10,000 cases. Currently the system is being operated for slightly more than sixty percent of the caseload and is scheduled to be implemented for the remainder in February 1979. Thus far, several new features have been added to the system to adjust for the larger volume. However, there is no evidence that largeness *per se* presents an insuperable challenge to implementation of monthly reporting and retrospective accounting provided that they are implemented in conjunction with an effective automated support system.

The continued expansion of the demonstration project in Colorado has two additional benefits. First, research will be conducted on the effects of Denver County implementation on administrative costs, agency staffing patterns, and Quality Control error rates. Because of the larger size of this agency and the performance of workload studies, we anticipate more precise definition of the range of possible results from our administrative analysis than we were able to obtain in Boulder County. In addition, expansion of monthly reporting and retrospective accounting in Denver County, as well as their continued implementation in Boulder County, provides for considerable ongoing savings in transfer payments costs in those two jurisdictions pending later statewide implementation in Colorado.

The second effort is extension of monthly reporting research to other locations outside of Colorado. The Department of Health, Education, and Welfare has given planning grants to three other states and New York City so that they can ultimately replicate and expand upon research conducted in Denver and Boulder Counties. Expansion of the research to other jurisdictions should be useful in many respects. It will yield data on the variation in transfer payments and program administration effects in several settings with a range of recipient and agency characteristics. It is also our understanding that the Department of Health, Education, and Welfare plans a systematic variation of design features for a monthly reporting and retrospective accounting system. This will define a range of reasonable options from which other states might choose when they implement a Monthly Reporting System.

The third effort planned is development of a more sophisticated and flexible model system to support monthly reporting and retrospective accounting. Even though the prototype system that was designed for the Colorado project represented a major increase in the level of automated support for welfare administrative processes, it provided no support to programs other than AFDC. Thus, with the assistance of Mathematica Policy Research, Colorado and Vermont are designing a model system that will support the administration of monthly reporting and retrospective accounting in AFDC while providing for integration of reporting procedures and benefits disbursement across programs. This model system is also intended to take advantage of recent technological developments within the data processing field that will make possible increased administrative responsiveness while minimizing operating costs. Development work on this system is scheduled for completion during 1979, with implementation in both states to take place during 1980. In the development of this system, considerable effort is being made to maximize its potential for transferability to other jurisdictions.

#### CONCLUSION

The results of the Colorado Monthly Reporting Experiment offer considerable evidence that implementation of a monthly retrospective reporting system similar to that tested in Colorado would represent a major improvement in the administration of the AFDC program. Data from the first year indicate that aggregate benefit payments declined by six percent, primarily due to more timely discontinuance of cases when they become ineligible. The system also caused a substantial decrease in corrective payments actions; retroactive payments declined by sixty percent while cancellations and recoveries declined by seventy-five percent. Although the system apparently leads to an increase in administrative costs, such increase would be marginal; in Boulder County, the per case increase was

estimated only to be two to four percent, which would be more than offset by the expected decrease in caseload. In relating any increased administrative costs to decreased transfer payments, it is important to note as well that it takes a nine percent increase in AFDC administrative costs in Colorado to offset a one percent decrease in aggregate benefits, so that any additional costs of operating a monthly retrospective reporting system even on a per case basis would be offset by only a fraction of the reduction in benefit payments. Figures of this magnitude suggest that there would likely be a very high benefit/cost ratio in implementing this type of system elsewhere as well.

The monthly reporting and retrospective accounting system tested in Colorado has been very effective because it gives adequate recognition to the high rate of change in household composition and income within the recipient caseload. Implicit in the opposition to frequent reporting requirements for welfare programs is the persistent myth that receipt of AFDC is a long-term arrangement for most recipients. While conducting research for the monthly reporting experiment, we found that the expected length of a single episode on assistance for the average monthly reporting recipient amounted only to fifteen months for regular AFDC cases and seven months for AFDC-U recipients. Perhaps more striking, however, is the finding that almost thirty percent of the AFDC case load experience a change in circumstances significant enough to affect either their eligibility or the amount of payment during each month. Specifically, more than eighteen percent of the ongoing cases require a change in grant, while five percent of the cases are discontinued and another five percent are added to the caseload. This finding offers convincing evidence that the financial needs of AFDC caseloads undergo frequent fluctuation and suggests that monthly reporting and rapid processing of payments are necessary if administration of the program is to be accurate and responsive.

Although monthly reporting and retrospective accounting hold great promise for improving the administration of AFDC and other programs, it would be an error, in our judgment, to push these requirements too quickly upon the states, for our experience has been that they require considerable time and care to implement. The changes brought about by these two principles require far-reaching administrative adjustments in agencies where they are implemented: new forms must be prepared, a data processing system developed and installed, new equipment acquired, community agencies prepared, and staffs restructured and retrained. Usually new policies and procedures must also be developed and submitted for legal review. These types of changes require much preparation and significant management resources on the part of agencies. Even though the changes yield major benefits, the difficulties of implementation should not be overlooked in any future action by the federal government.

It should be apparent that benefits obtained in the AFDC program are relevant not only to AFDC, but extend by implication to the administration of other programs serving low-income households, particularly Food Stamps. The potential for improving the administration of the Food Stamp program utilizing this type of system is particularly intriguing since there is substantial similarity and large overlap between the recipient populations of that program and AFDC. It would make very good sense to experiment with the possibilities of introducing monthly reporting and retrospective accounting simultaneously in both the AFDC and Food Stamp programs. This could be expected to increase the accuracy and responsiveness of benefit allotments in Food Stamps as well as AFDC, and would also markedly simplify the joint administration of the two programs. With this type of system, the administration of the two programs could share a common form, comparable procedures, and compatible filing schedules. This would be a major advantage to those recipients who obtain benefits under both AFDC and Food Stamps since they would be faced with a single form and a single process for obtaining both types of benefits. It would also substantially reduce administration costs for disbursing benefits to joint recipients of the two programs. A common approach to administration of AFDC and Food Stamps is essential in the long run if unnecessary administrative costs and needless recipient burden are to be avoided.

It is my understanding that this Committee will take up the subject of welfare reform in the near future. Every welfare reform proposal developed during the past decade has incorporated requirements for frequent reporting and retrospective accounting. Should this Committee report a proposal for a national income maintenance program, experience gained from the Colorado Monthly Reporting Experiment and Pretest could serve as a valuable source of practical

experience in specifying reporting requirements and accounting periods. If a national income maintenance program is not considered to be desirable or feasible at this time, however, the results from the Colorado experiment and pretest offer considerable potential as an approach to improving the administration of public welfare. Because monthly reporting and retrospective accounting give adequate recognition to the fluid circumstances of low-income families, these features can provide a mechanism for making public assistance payments more accurate and responsive than payments made under the administrative formulas of our present programs.

Senator MOYNIHAN. And now Dr. Bradley Schiller will testify. Dr. Schiller is of the Department of Economics at the American University. Dr. Schiller, a pleasant good morning to you.

I have just had the chance to glance at the opening lines of your testimony. I see an old and distinguished friend has just come into the room and in the tradition of anonymity of journalists I will not mention the name, but I guess I might repeat for anybody who did not hear my earlier statement, the reference to that remark of Hannah Arendt that the totalitarian elites in Europe in the 1920's and 1930's preserved their tactical advantage by their success in converting every statement of fact into a question of motive.

So we welcome you here to present your facts and unless you wish to discourse on your motives, we will leave those to your confessor. Professor Schiller, go right ahead.

#### STATEMENT OF BRADLEY SCHILLER, DEPARTMENT OF ECONOMICS, AMERICAN UNIVERSITY

Mr. SCHILLER. Thank you, Senator.

I appreciate the opportunity to speak at this funeral for welfare reform. I first suspected that welfare reform was dead when HEW proposed a plan whose success depended on the creation of another million public jobs, an annual accounting period for welfare benefits, and our ability to make permanent distinctions between employable and unemployable poor people.

However, it was not until President Carter pledged to cut the Federal deficit \$30 billion while accepting higher unemployment rates that I knew that welfare reform was dead.

At times like this, it seems appropriate to reflect on the basic character of the deceased. Was welfare reform such a good idea that we hope it will be born again, or do we now recognize that the spirit of welfare reform was basically flawed and is not worthy of reincarnation?

For my part, I prefer to bury welfare reform rather than praise it, and I say this in full recognition of the fact that the reincarnation of welfare reform appeals to the professional and financial interests of most of the economists who have testified before this subcommittee, the bureaucratic interests of the DOL and HEW administrators who have attended this hearing, and perhaps the political interests of the subcommittee itself.

Despite these many vested interests, we must ask how the pursuit of welfare reform has benefited us and what it has cost us.

On the benefit side, the pursuit of welfare reform has taught many of us just how complex the realities of poverty and welfare are. Poor people do not fit into the neat little stereotypes we have created for them; neither do welfare recipients. Welfare and poverty are dynamic

conditions that are at least as much influenced by the state of the economy and the family as by the structure of our income transfer system.

As a consequence, small changes in that system do not substantially alter the dimensions of welfare or poverty. Senator Moynihan, you, yourself, recognized this fundamental point at least 13 years ago when, as an Assistant Secretary of Labor, you emphasized the relationship of family stability to changes in the national unemployment rate.

Another benefit of the quest for welfare reform has been an increasing awareness of the inherent conflicts in our social and economic goals. We simply cannot assure a decent standard of living for everyone and at the same time minimize program costs and work disincentives. A compromise must be struck.

I do not think as many people would agree with that conclusion 10 years ago as would agree now.

Finally, we must count among our benefits the increased humility of the academic research community. Like the political community, it is learning that promises must bear some closer relation to potential. One reason welfare reform lived so long is that academic researchers kept it alive with artificial support systems, mathematical models of our income maintenance system that bore little resemblance to welfare reality but provided a convenient basis for comparison.

Although these many benefits are substantial—

Senator MOYNIHAN. A footnote there, Professor Schiller. Like most people who try to get through books quickly, I tend to read footnotes. They are usually the most interesting things.

An "anonymous example"; that is intriguing. What is being covered up here?

Mr. SCHILLER. Nothing.

I will offer two examples of artificial support systems. One is the persistent insistence of the academic research community to evaluate existing and alternative income maintenance systems on the basis of nominal, rather than effective, marginal tax rates, despite the fact that there is available evidence that effective marginal tax rates are substantially lower than nominal ones.

The AFDC system as it pertains to females, the marginal tax rate, as you are aware, is 67 percent. Most of the research—

Senator MOYNIHAN. The nominal one?

Mr. SCHILLER. The nominal one. That is, the theory is that they lose 67 cents for every dollar they earn. In fact, the effective marginal tax rates are substantially lower than that, something less than 25 percent, a result that occurs largely because of the caseworker discretion in computing work expenses.

Senator MOYNIHAN. And then the static equilibrium analysis?

Mr. SCHILLER. The second point I would like to make there is that the theory of labor supply response to changes in work incentives is very clear and elementary. Basically it says that once a welfare recipient confronts a certain wage rate, they will try to find that level of work which optimizes the tradeoff between leisure and wages, so that they work as much as they desire at the going wage.

Senator MOYNIHAN. Right.

Mr. SCHILLER. That is simply not a realistic assessment of the situation that most welfare recipients find themselves in. They cannot find as much work as they want at the going wage.

So, in effect, the income maintenance experiments are comparing the change in work effort that occurs under the experiments to a presumed equilibrium that never existed for the people who were unable to find as much work as they wanted at the going wage in the first place.

Although these many benefits of our quest for welfare reform are substantial, there have been high costs as well. I will focus on only one: the effect of an ongoing welfare and employment and training program.

Preoccupation with the design of a major new transfer system has limited efforts to understand and improve the existing system. I regard this as a very high cost, largely because I believe that modest improvements of the existing system are not only politically more feasible but they also best serve our income transfer goals.

Even if one does not share this faith in what has been called incrementalism—

Senator MOYNIHAN. A dread accusation.

Mr. SCHILLER. It would still seem presumptuous to design a new welfare system before we determine what elements of the present system have worked well or poorly.

When we turn our attention to this kind of issue the questions we ask must be reformulated. From the modest perspective of incrementalism, the question to be addressed is not whether the existing system is effective or not but, instead, which elements of the existing system have worked best for which kinds of target groups under what circumstances?

This question is not very exciting, but may be extremely productive. Very little research has been done on this question, but some preliminary efforts have begun and I would like to summarize results from one such study.

Since you insist that I read the footnotes, let me also note that research by Lee Friedman and Mathematica on the supported work experiment; continuing work by Ketrion, Inc., of Philadelphia on the WIN program; and preliminary analyses by Westat, Inc., of CETA outcomes are examples of other work on the question of relative effectiveness.

I want to focus here, however, on only one—a longitudinal evaluation of the work incentive—WIN program—funded by the Office of Program Evaluation in the Employment and Training Administration.

As you are aware, Senator, the WIN program has provided nearly 2 million welfare recipients with some form of employment or training assistance during the last 10 years. In addition, the WIN program has experimented with nearly every mechanism yet suggested for moving people from welfare to work, including child care assistance, vocational training, medical examinations, subsidized private and public jobs, counseling and improved work incentive formulas.

All adult welfare recipients are required to participate in WIN unless specifically exempted. Hence, the WIN program provides an ideal basis for examining the potential of public policy to move people off of welfare and into employment.

The WIN evaluation that I participated in focused on the longitudinal experiences of 5,000 WIN participants and comparable non-participants in 78 cities. Data on family, income, welfare and employment status were collected from three waves of personal interviews, welfare case records and WIN program records.

The fourth wave of personal interviews is still underway with the recipients.

In these dimensions the WIN study was much larger in scope than any of the negative income tax experiments, although much less costly.

The WIN evaluation was not designed to measure either work disincentives or family stability. It is worth noting, however, that we did observe marginal tax rates much lower than nominal rates. For a woman, the effective marginal tax rate appears to be less than 25 percent and for men less than 50 percent. These rates contrast with nominal rates of 67 percent for both sexes.

The basic focus of the WIN study was on the impact of the various employment and training services provided. These included four basic types: job referrals and other placement services; educational services; vocational training; and subsidized employment, either on-the-job training in the private sector or public service employment in the public sector.

Our interest in this study was to determine which of these services worked best for which types of welfare recipients. Specifically, which employment services yield the greatest net gains in employment, earnings and financial independence? I will summarize our findings on earnings changes only; other outcomes follow similar patterns.

Overall, the impact of WIN program services is quite modest. The average net gain in annual earnings was on the order of \$300 to \$350 a year, much less than average program costs.<sup>1</sup>

Senator MOYNIHAN. Would you say that again, sir?

Mr. SCHILLER. The average gain in annual earnings of the individual recipient who participates in the WIN program is on the order of \$300 to \$350 a year. That is the net gain to the recipient, which is less than the associated cost of serving the average recipient.

Senator MOYNIHAN. Do you want to say that again?

Mr. SCHILLER. OK.

Senator MOYNIHAN. This is the first time this committee has heard this. We have been under the understanding that the WIN program would have a clear, net social—

Mr. SCHILLER. I am not speaking about—

Senator MOYNIHAN. In the sense that it costs something to do it and then it produces something as a result and that the earnings and the additions to earnings are greater than the costs. That is the elemental proposition that this committee has heard asserted without challenge.

Mr. SCHILLER. Let me clarify the statement, then. The net gain in earnings of the recipients refers to the earnings gained in the first year following the program's services. To the extent that those gains are maintained over a substantial amount of time, then it is possible that the net gains will equal, and possibly exceed modestly, the average program costs.

<sup>1</sup> Correspondence from Ernest G. Green, Assistant Secretary for Employment and Training, concerning the cost-effectiveness of the WIN program and Dr. Schiller's statement on average net gain may be found on page 278.

Senator MOYNIHAN. Oh, God, really? Where is the Labor Department? Is anyone here from the Labor Department?

One person. Would you introduce yourself, sir?

Mr. EASLEY. Easley, program staff.

Senator MOYNIHAN. Is this your understanding, Mr. Easley? I do not mean to ask you to say something, as you did not come here for that purpose. Do I take it you have not finally finished the work, and are in a fourth wave?

Mr. SCHILLER. Yes, the fourth wave. The results that I am reporting on refer to the status of the average participant approximately 1 year after program termination.

Senator MOYNIHAN. And this data is available in the Department?

Mr. SCHILLER. Yes.

If you would allow me, I would like to move the focus slightly from the average gains to something which I regard as much more upbeat; namely, the potential for improvements in the average performance of the program.

Senator MOYNIHAN. Please do.

Mr. SCHILLER. There are very large differences in the net benefits received by identifiable subgroups of WIN participants from specific services. This may be illustrated by comparing the net earnings gains of welfare recipients who had some employment in the 6 months prior to entering the WIN program to the net gains of those recipients who had no recent employment in the period just prior to entering the program.

The comparisons, summarized in table 1, which you have, are striking, and let me just review a couple of them. Again, we are looking at four basic program services: job search and placement assistance; educational services; training services; and subsidized employment. The point is that each WIN participant can take any or all of these services.

[The table referred to follows:]

TABLE 1.—NET CHANGE IN ANNUAL EARNINGS DUE TO SPECIFIC WIN SERVICES, BY EMPLOYMENT EXPERIENCE AND SEX

Type of service	Males—		Females—	
	With recent work experience	Without recent work experience	With recent work experience	Without recent work experience
Job search.....	-\$226	\$224	-\$143	† \$500
Education.....	-1, 043	-1, 102	-264	147
Training.....	116	† 1, 346	-118	† 840
Subsidized employment.....	† 1, 579	† 2, 736	† 1, 188	† 1, 710

† Significant at 0.01 level.

‡ Significant at 0.05 level.

Mr. SCHILLER. Most, in fact 60 percent of them, receive job search and placement help only. Education and training is received by a small group, training by a small group, and subsidized employment by approximately 20 percent of the participants.

Senator MOYNIHAN. You are going to have to help me here, sir. On page 5, I do not follow your last paragraph. I think there is an "only" missing, or a "without" missing.

Would you see if those sentences and see if they—



Mr. SCHILLER. Starting with "Overall"?

Senator MOYNIHAN. It says: "Females with recent job experience benefited substantially from"—your subject is the same in both sentences.

Mr. SCHILLER. Oh, I see. Let me start with the males, then.

Senator MOYNIHAN. No, go through those sentences for me, would you?

Mr. SCHILLER. Oh, yes, there is something missing.

Senator MOYNIHAN. There is a "without" missing.

Mr. SCHILLER. Yes, there is a "without."

Senator MOYNIHAN. You have the same subject for both sentences and you are contrasting it. Which is which here?

Mr. SCHILLER. The first one, on line 1, is without.

Senator MOYNIHAN. Females without recent job experience benefited substantially from job search and vocational training. Got you.

Mr. SCHILLER. OK. Let me review your testimony just for a minute here.

Let me look first at males. Again, contrasting those with recent work experience and those without. Again, when we speak of work experience, we are talking about whether or not these recipients had a job in the 6 months prior to entering the WIN program and we are asking what were the net increases in earnings by these two groups as a result of specific services provided under the WIN program.

Senator MOYNIHAN. Got you.

Mr. SCHILLER. OK.

Job search services provided to males apparently have no significant benefit in terms of earnings increases for either those with recent experience or those without, although—

Senator MOYNIHAN. Wait a minute, would you help me? Would you explain the negative? We are on table 1, right?

Mr. SCHILLER. That is right. We are on table 1 looking at the males. Males with recent job experience experience an increase in earnings that was \$226 less than increases in earnings experienced by those who did not participate in the program.

Senator MOYNIHAN. Your control?

Mr. SCHILLER. Yes, the control.

Senator MOYNIHAN. I have to repeat myself, because it is such a great pleasure to me always to invoke my dear friend Mosteller who used to say, "We didn't have any controls because it was only an experiment."

You have a control and there is a loss—OK.

Mr. SCHILLER. In other words, the male participants in the WIN program who received job search and placement assistance only did not do as well in the job market as comparable welfare recipients who did not participate in the program.

Senator MOYNIHAN. All right.

Mr. SCHILLER. On the other hand, there is marginal evidence that males without recent job experience might have benefited some, but the results, as we say, are statistically insignificant.

Senator MOYNIHAN. Wait. Only those with asterisks are statistically significant findings?

Mr. SCHILLER. That is correct.

Senator MOYNIHAN. OK.

Mr. SCHILLER. So turning, then, to those that are significant, we find that vocational training provides no significant benefit to males—

Senator MOYNIHAN. Would you go right across here? I see. So when you get to females with, you get a significance at the 0.01 level, which is a high, and it encourages you to think that that pattern may be true in males too?

Mr. SCHILLER. Right.

The job search and placement assistance which, again, is provided to 60 percent of the recipients appears to have substantial positive benefits for only the women recipients without recent work experience.

Senator MOYNIHAN. Might have it for the males.

Mr. SCHILLER. And might have it for the males.

Senator MOYNIHAN. You have a statistically significant finding. That does not mean that your finding is, in fact, real.

Mr. SCHILLER. That is right.

Senator MOYNIHAN. OK. Now, education.

Mr. SCHILLER. Education, first of all, was provided to only a small fraction of the welfare recipients participating in the WIN program. I believe the figure is 8 percent; it may be 6 percent.

In any case, we find that there are no substantial, or significant, increases in earnings attributable to the educational component of the WIN program.

The third type of service provided is vocational training, typically in an institutional setting, like the old MDTA program or the institutional training provided under CETA. In fact, many of the welfare recipients participating in the WIN program receive the vocational training services under the auspices of CETA which simply pays for it.

What we find here is that vocational training provided to welfare recipients who have had some, that is, any, recent job experience yields no net benefits. However, when that same training is provided to male or female recipients without recent job experiences, the benefits are not only statistically significant, but they are very substantial.

The gain for males is on the order of \$1,300 in the first year and for the females, \$800 in the first year.

Senator MOYNIHAN. Can I ask you, how many males are there in the WIN program?

Mr. SCHILLER. At the time that we did this survey, which was 1973 through 1975, that is when they participated in the program, 40 percent of the population was male, of the WIN participant population. The reason for that is there are certain priorities established for who was mandated to be served by program services.

Senator MOYNIHAN. So AFDC-U people came through it?

Mr. SCHILLER. That is correct. The AFDC-U people would come through as well, and the male recipients had the highest priority and therefore had first access to the limited resources available in the WIN program at the time.

Senator MOYNIHAN. Got you.

Mr. SCHILLER. The WIN program could not serve everyone at that time, so we found a much higher proportion of males in the program than would be characteristic of the AFDC population.

The fourth type of service, employment and training service, provided under WIN is subsidized employment which, as I indicated earlier, would be subsidized on-the-job training in the private sector or public service employment, PSE, of the type provided under CETA.

Again, both of these might, in fact, be under CETA auspices; that is, conducted by the prime sponsor.

Senator MOYNIHAN. Is that finding sort of tautological? You have subsidized wages—well, not tautological, but there is an increase in earnings because the public sector is providing it.

Mr. SCHILLER. There are two aspects to that, Senator. First of all, to the extent that the welfare recipients without recent job experience are, in fact, getting on-the-job training in an OJT private sector job or the public sector, then we presume that there is some continuing net gain in terms of higher skills afforded the recipient.

Senator MOYNIHAN. I have had to interrupt a number of witnesses these last few days to say where did that “presumably” come from?

Mr. SCHILLER. Let me add the second point then. Second, the numbers reported here do not distinguish between those recipients who are continuing to receive a subsidy and those who are off the subsidy. That is, their subsidized job has ended and they are now in strictly private employment, or in the public sector without a subsidy.

So that these benefits as reported here carry some subsidy but we do not know exactly how much. In fact, that was—

Senator MOYNIHAN. Some of these people are no longer in subsidized employment, but their earnings are, nonetheless, much higher?

Mr. SCHILLER. That is right.

Unfortunately, at the end of the third wave of interviews, we are unable to determine how many were completely finished with the subsidy or not, and that was largely the motivation behind a fourth wave of interviews which I understand is just coming to an end.

Senator MOYNIHAN. All right. Fascinating.

Mr. SCHILLER. So the numbers reported here should be regarded as order of magnitude and what we are really concerned with in this testimony are the differences in net earnings between those recipients who had recent job experience and those who did not. And so reading across the bottom line there, Senator, you see that the net increase in earnings received by WIN participants who participated in subsidized employment are striking in the first year. However, there continued to be substantial differences between the groups. Males with recent job experience have a net first year gain of approximately \$1,600; males without recent job experience have a net first year gain of \$2,700.

The same kind of difference is apparent in the females. Females with recent job experience have a net first year gain of \$1,200; females without recent job experience have a gain of \$1,700.

So again here we see that those AFDC recipients who did not have recent job experience benefited much more from the program than those who did.

Senator MOYNIHAN. Dr. Schiller, what is “recent”?

Mr. SCHILLER. Recent is any job in the 6-month period prior to entering the program.

Let me add that the superior gains of less employable recipients continue to stand out even when higher costs of serving the hardest to em-

ploy are considered. Those recipients without recent job experience do require more service, including child care, work orientation and longer education and training. However, these higher costs are more than compensated by their higher net gains.

Notice, again, that placement, education, and training services yield no net gains for WIN participants with recent job experience. They would have done just as well without WIN's help. Under these circumstances, such services cannot be cost-effective no matter how cheap they might be. By contrast, the substantial first year net earnings increases experienced by WIN recipients who were less job-ready easily cover attendant costs.

Our longitudinal WIN study yielded still more detailed findings on a variety of important programmatic and policy issues. Rather than provide more details here, however, I would prefer to summarize some salient implications of our research.

First, the WIN study demonstrated that there is substantial variety in the impact of specific welfare or employment and training services. Whatever might be said about the average impact at present, that average can be improved substantially by better design and targeting of program services. Such improvements may actually lower welfare costs.

Second, we do not know enough about the relative effectiveness of the various services provided to welfare recipients to draw any hard conclusions about which program configurations work best for which clients in what circumstances. More research on this issue is desperately needed.

My third and final point is that all evidence suggests that movements from welfare to employment are a recurring event for most welfare recipients and are influenced much more by national economic trends and family dynamics than by changes in the welfare system. Our expectations for welfare policy, whether in the shape of fundamental reform or mere incrementalism must be modest and tempered by macroeconomic reality.

Senator MOYNIHAN. Well, sir, that is extraordinary testimony. Let me make a couple of points here if I can.

First of all, we do seem to have some different information from the Department of Labor and will try to straighten it out. Mr. Galvin of the committee staff informs me that last year Secretary Green, who is a well-regarded friend of this committee, testified that the cost effectiveness of the WIN program was very pronounced. There was \$2 gained for every \$1 expended—is that right, Mr. Galvin?

Well, we will have to ask and we will see what can be learned.<sup>1</sup>

Now, I want to get back to your opening paragraph, and to assert with great vigor that these hearings do not constitute a funeral for welfare reform. If I could use an image from Margaret Mead, whom we lost this week, I would like to think that this was the coming of age of welfare reform.

We have information about this matter. We have the work of 12 years and more of exceedingly competent, careful people checking

<sup>1</sup> Correspondence from Ernest G. Green, Assistant Secretary for Employment and Training, concerning the cost-effectiveness of the WIN program and Dr. Schiller's statement on average net gain may be found on page 278.

each other's work, revising each other's work. We are learning about this thing.

I would like to make another point which is so important here, and I hope I will not give offense to anyone, but if I have to give mild offense, so be it. That is to say that the first object of welfare policy is the well-being of children.

It is not the protection of the State from the possibility that some adult may be loafing. It is the welfare of children that got me involved in this issue. I have grown gray in the service of it.

In 1962 I wrote for President Kennedy a measure on children's allowances. I was in the Department of Labor then and found, as I think you may know, that there had been for a long period an extraordinarily powerful correlation between the number of new AFDC cases and male unemployment rates. Then it broke apart, it collapsed, and I said something is going crazy here.

I can tell you that it has resumed. That correlation resumed in the mid-seventies.

One of the consequences was a huge increase in the number of dependent children. At least some of us foresaw this. There is no level at which you could say this is a methodology that will tell you what is going to happen. But we sensed that something was going to happen and it did happen and it is still there.

It was a huge increase in dependency and it was accompanied by vast amounts of dishonesty everywhere. When it was not dishonesty, it was worse. There were those people in HEW whose main interest was to deny that it was happening. I remember that my good friend, the current Secretary of HEW, who was then in the White House, got himself on the front page of the New York Times one day in 1967 by announcing to a startled world that only 5 percent of the recipients of welfare were able-bodied males.

Well, whoever said they were any more than 5 percent? I mean, the great majority of recipients of welfare are mothers with dependent children. We all know that.

And so if you proved that there were not any males loafing in the system somehow you proved there was no problem of welfare, which was the problem of dependent children. And the bureaucracy of HEW was appallingly—I am sorry to use the word—dishonest. By proving that there were few adult males on the system they somehow gave you the impression that there were not, in fact, millions of children. That is not easily explained, in the best of circumstances. We would hope more for our children than that.

And similarly there were those who would exaggerate the number of males involved. Again, it is a problem of dependent children and all of our effort is to make the adults in the welfare system work more, earn more, et cetera. That is good, that is important. Why is it important? Because it is important for the children.

The social interest of this committee, at least, has to do with those kids.

One of the things that nobody seems to want to talk about is the fact that for many of the people who go on welfare, employability is no issue. They are teenage females with children and they do not have any husbands.

I should not say they are teenagers, but they are young females who have dependent children. They are dependent people.

With respect to the WIN program and all, what you are suggesting, I think sir, is that there are cost-effective areas of it, there really are, where you should find them and put your bets there and you will get something while over here you are not getting much. That is management. That is good economics. That is being cost effective and it seems to me you have done good work.

Why did you say you suspected welfare reform was dead when "HEW proposed a plan whose success depended on the creation of another 1 million-plus public jobs, an annual accounting period for welfare benefits, and our ability to make permanent distinctions between employable and unemployable people. However, it was not until President Carter pledged to cut the Federal deficit to \$30 billion while accepting higher unemployment rates that I knew welfare reform was dead?"

Mr. SCHILLER. I am using welfare reform in the sense of a fundamental revision of our existing system which, in recent years, has been understood as an introduction of a negative income tax plan. I regarded it to be dying for those three reasons. First of all, as you are aware, the cost estimates associated with the administration's welfare plan are extraordinarily sensitive to the success of the jobs component of the program. Jodie Allen, I believe, testified on that very subject before you.

Senator MOYNIHAN. She did, yes.

Mr. SCHILLER. Second, I think the annual accounting period for welfare benefits is highly unrealistic. There is a lot of mobility in income and employment among welfare recipients. There are some people who receive benefits during the year who would not be defined as eligible on an annual basis. If somehow they were excluded and somehow could better manage their financial resources, then certainly the number of eligible people on welfare and the size of our benefit program would be reduced.

Senator MOYNIHAN. Would you say that again, please?

Mr. SCHILLER. The notion of an annual accounting period, or anything longer than 1 month, is motivated by the recognition that there are many people who receive welfare benefits at some time during the year who would not be defined as welfare eligible, or perhaps not even poor, based on their annual income, because the individual families experienced sharp fluctuations in their income.

Senator MOYNIHAN. I think Dr. Williams was testifying about this just earlier.

Mr. SCHILLER. I think the Mathematica experiment on the monthly accounting system is definitely a step in what I would consider the right direction of incrementalism—how can we improve the efficiency of the existing system.

Senator MOYNIHAN. We are thinking of having a comprehensive incremental program.

But you are saying that annual accounting is designed to keep the numbers down and that if you had quarterly accounting, then that number would scare everybody more than they apparently were scared over in HEW.

Mr. SCHILLER. Senator, I think your experiences would lead you to agree with me that it is ridiculous to presume that a young mother with a child can wait a substantial amount of time to buy her groceries

when she has no money to declare eligibility, just as it is ridiculous to assume that if somehow at the end of the year she receives more income and improves her lifestyle, she would be in a position to pay back HEW for the welfare benefits she got earlier in the year.

All I am suggesting is that poverty and welfare are very dynamic conditions dictated by concerns other than the income transfer system.

Senator MOYNIHAN. It is almost not a problem of poverty; it is dependency. There are people who are not in any useful term described as poor, but who are, on occasion, just utterly dependent. It is a different phenomenon and has a different dynamic, because you can get out of it like that, bang.

Mr. SCHILLER. If we look at the welfare and the poverty numbers in any year, we recognize that the majority of the people there do experience changes in their family situation and in their income and employment circumstances. The question is not how can we eliminate the last welfare recipient from the roles or reduce costs or improve the welfare of the dependent children in those families, but how, at the margin, can we speed the flow of welfare recipients into more stable families, more stable employment and income circumstances.

That is what I regard as the appropriate focus for what you might call welfare reform but which has not been understood as welfare reform for the last several years.

Senator MOYNIHAN. Why do you feel that the notion that success depended on the creation of another million-plus public jobs was an indication that this was not going forward? Just your fear, your assumption, that Congress would not do that?

Mr. SCHILLER. Yes. My opinion is that the Congress would not support that large an increase in expenditures on the welfare population at that point, which was several months ago, and certainly not at this point when the President is pledging to cut the fiscal 1980 deficit.

Senator MOYNIHAN. It is a fact that the overall package began to come out at about \$20 billion and it is also the fact, I am sorry to say, that in the House of Representatives, the Ways and Means Committee never even took the bill up. The special subcommittee, as you know, was put together to draft a bill and then it did not get to the full committee stage at all.

Mr. SCHILLER. I have here the administration's welfare reform proposal, an analysis done by the Congressional Budget Office, and the figures in there, I think, are the kinds of changes in total costs which would be incurred with different benefit levels or employment conditions. They show a tremendous range of estimates.

Senator MOYNIHAN. A tremendous range, and they also showed, as you know, that every time the program doubled in cost, the amount of fiscal relief was cut in half. I did not follow that. You have to be a mathematician to understand that.

But in my city of New York, just as an aspect of concern about children, the mother with three children has not had a 5-cent increase in her welfare payments since 1974, such that their true value had declined, when we last calculated them, by about 29 percent. We have cut those children's allowances by 30 percent.

By the end of this year, it will be 40 percent, and pretty soon we will not have much of a welfare problem.

Mr. SCHILLER. Does that figure incorporate changes in the food stamp allowance?

Senator MOYNIHAN. Food stamps are indexed, as you know, and I should tell you that rent is also indexed. But the actual amount of money for other things has been frozen because the city's situation is frozen, but that is another matter.

I would like to ask you one last question as I do not want to keep you any longer. Ought we to shift our priority in WIN? Senator Long, for example, has suggested that the WIN emphasis perhaps should move over to young mothers with one, or at most, two children, who are a more flexible group of people, as it were, and who might most benefit from employment opportunities and training. That seems to be sort of the thrust of some of your findings.

Mr. SCHILLER. Those would be included, I suppose, in the category that I would regard as the hardest to employ.

Senator MOYNIHAN. You seem to get the most—

Mr. SCHILLER. Although I would be most reluctant to target it on the young mother of one or two children. It seems to me that I take a more old-fashioned and longer term view; namely, that if we can provide more jobs for the males as well, then the number of female-headed families and associated dependency may not grow so much over time, but that is a long-range—

Senator MOYNIHAN. Well, that has been my uninformed judgment for the last 15 years or so, although you do know that young mothers with children are in the labor force at astonishing rates, who are not in any way dependent on welfare. I do not know whether this is a good thing or a bad thing—I do not know what good or bad is. That is obviously somebody's judgment and I do not know whether we should make a social judgment about whether it is good or bad.

Obviously, I could always throw a number at you, but if memory serves, almost half of all mothers with children under 6 years of age are now in the work force. Evidently they made the judgment that they want to be, or at least one has a sense that for a very large number it is an option. They can do it or they cannot.

Mr. SCHILLER. I think the same thing holds true within the welfare population. Work supported by the Labor Department done by the Brookings Institute demonstrated, I think convincingly, that mothers of households have at least as much commitment to the work ethic as other families.

Senator MOYNIHAN. Oh, that work-ethic business, oh, you know—

Mr. SCHILLER. That raises the question of providing the work and training services that facilitate movement into financial independence.

Senator MOYNIHAN. I remain doubtful that there is a problem of "work ethic" in this country when, after about 60 years with the proportion of the population in the work force frozen, almost, at 56 per cent, in the last 5 or 6 years we have seen it climb to over 60. More Americans are working and looking for work today than in 1900 when we know all Americans were good Americans.

Well, sir, we thank you. You have opened up some questions about the cost effectiveness of WIN which are legitimate questions and I think we will hear more from the Department of Labor about it. I would like to say that every time we ask the Department of Labor about things like that they are very forthcoming.



I would like to suggest that, while maybe there is an interest that has developed in studying these problems as you suggested, it is a legitimate interest and some of the studies which you are reporting are very helpful, certainly, to this subcommittee. We thank you very much.

Mr. SCHILLER. Thank you.

[The following was subsequently supplied for the record:]

HON. DANIEL PATRICK MOYNIHAN,  
Chairman, Subcommittee on Public Assistance, Committee on Finance, U.S.  
Senate, Washington, D.C.

DEAR MR. CHAIRMAN: On November 17, Dr. Bradley Schiller of the Department of Economics, American University, testified before your Subcommittee on an evaluation study of the WIN program.

At one point in his testimony, Dr. Schiller noted: "Overall the impact of specific program services is quite modest: the average net gain in annual earnings was on the order of \$300-350/year, much less than average program costs." The summary of the study prepared by Pacific Consultants, for which Dr. Schiller was research director and author, states, "*The economic benefits of WIN-II exceeded its costs.* The present value of net earnings gains to both males and females were greater than their associated employment and supportive-service costs. In this sense WIN-II was economically cost effective."

We must point out that the gain in annual earnings is only one of the benefits by which the effectiveness of WIN is measured.

One such measure is the extent of welfare (AFDC) grant reductions. In FY 1977 WIN reported welfare grant reductions of \$436,332,000. In addition we estimated food stamp bonus reductions of \$101,490,000 and medical reductions of \$114,615,000. These total \$652,437,000, a figure about 1.7 times the FY 1977 expenditures of \$376,510,000.

It was this data to which I testified earlier in the year and to which I believe your comment indicating that WIN benefits exceeded costs referred.

It should also be noted that the study to which Dr. Schiller referred assumed that no benefits were achieved if an unemployed person was assisted to find employment unless that new employment generated wages in excess of the previous year's earnings. The focus of WIN is and has been since the 1971 amendments the employment of WIN registrants in jobs meeting certain minimum standards regardless of whether income generated is greater than that of the previous year. We believe that the 1971 WIN amendments provide that the program be directed to employment of the maximum possible number of WIN registrants and note that each year since the amendments became effective the number of persons entering employment has increased, last year reaching 271,271.

We hope this information is helpful to you.

Sincerely,

ERNEST G. GREEN,  
Assistant Secretary for Employment and Training.

Senator MOYNIHAN. And now to conclude our hearings and the morning, we have the very special honor and pleasure of hearing from Dr. Henry Aaron who is the Assistant Secretary for Planning and Evaluation of the Department of Health, Education, and Welfare and in the new language of our age, the "flak catcher," as you are called.

I would like to make the point, in order that it be understood by our guests, that in ordinary circumstances we would have asked Dr. Aaron to testify first and to open these hearings, but he requested that he conclude them in order that he could hear the points raised and arguments made and comment upon them as he chose to do.

Mr. Secretary, we are very honored to have you here and I hope it is not the case, as one hears, that you are returning to Brookings and that this will be your last appearance before this committee?

Mr. AARON. Unless you invite me to testify again before December 1, it will be.

Senator MOYNIHAN. Well, we are not going to invite you before then. We will invite you afterward and you can come in here and say what the ridiculous job the Assistant Secretary for Planning and Evaluation is and why do they not get anybody in Government who can get things straight and clean up that mess at HEW.

It has been a distinguished 2 years. I can understand that you would be wanting to leave. I do not know if there are tenure arrangements at Brookings, but I never found that staying around here for more than 2 years was to anybody's advantage. You will not be far away, and that is a very pleasing thing for us.

You have an associate with you whom you would perhaps introduce?

Mr. AARON. Yes, Michael Barth who is the Deputy Assistant Secretary for income security policy in the Office of Planning and Evaluation who is accompanying me today.

Senator MOYNIHAN. Mr. Barth, you are very welcome to this committee, and Mr. Secretary, you may go ahead exactly as you wish.

Mr. AARON. Thank you very much. I have submitted a longer statement for the record. I would like to simply hit certain highlights from that statement.

Senator MOYNIHAN. We will include that statement as if read.

Mr. AARON. Thank you very much.

**STATEMENT OF HON. HENRY AARON, ASSISTANT SECRETARY OF HEALTH, EDUCATION, AND WELFARE FOR PLANNING AND EVALUATION, AND MICHAEL BARTH, DEPUTY ASSISTANT SECRETARY FOR PLANNING AND EVALUATION, INCOME SECURITY POLICY**

Mr. AARON. We welcome this opportunity to discuss with you the broad program of income maintenance experimentation that was begun by the Office of Economic Opportunity in the mid-1960's and was continued by the Office of Health, Education, and Welfare.

The Government has supported income maintenance research for over a decade. This support reflects a bipartisan commitment spanning four administrations to provide improved information on which to base decisions about changes in the welfare system.

The Seattle-Denver experiments taught us to propose a plan different from the one tested there. The Seattle-Denver plan reduced work effort. The administration's welfare reform plan and all of the major incremental alternatives advanced in the Congress combine jobs and cash assistance and increased work effort.

Work opportunities must be included in welfare reform. That is the lesson of Seattle and Denver.

The primary goal of the experiments was to obtain information that would make it possible to estimate the costs of various income maintenance alternatives, in particular to obtain information on whether, and by how much, recipients of income maintenance payments would reduce their work effort, a major determinant of the cost of any welfare program.

Controlled social experiments offered the greatest promise of generating the reliable data needed to estimate the cost of changes in welfare programs. I believe that promise has been fulfilled.

The findings on work effort, particularly those from the Seattle-Denver experiment, have led the administration to propose a welfare reform plan that unlike the simple negative income tax plan used in the experiment integrates jobs and reform of cash assistance.

Data from the last of the four income maintenance experiments, those in Seattle and Denver, have provided the empirical foundation for simulations by the administration, the Congressional Budget Office, and others of the costs of the administration's welfare reform plan and most of the alternative proposals.

The experiments have produced new accounting and budgeting procedures, mentioned by Mr. Williams, that hold out promise of saving hundreds of millions of dollars in existing welfare programs.

Senator MOYNIHAN. That was, of course, a rather dramatic example this morning, the testimony about Colorado, the Denver experiments.

Mr. AARON. Yes, sir, I think it is.

Senator MOYNIHAN. Those are the kinds of things you can learn.

Mr. AARON. It is an example of a byproduct, something that was not expected at the outset of the experiment.

Senator MOYNIHAN. Yes.

Mr. AARON. Turning to page 8 of my testimony, I would like to summarize some of the findings of the experiment.

The income maintenance experiments yielded roughly consistent results on the primary question of whether recipients of cash assistance would reduce work effort and, if so, by how much. All of the experiments provided guaranteed income maintenance payments but imposed no work requirements or provided no special job opportunities.

To summarize these results, male heads of two-parent families reduced hours of work between 1 and 11 percent, but few of them withdrew entirely from the labor force. Female family heads reduced hours by about 15 percent. Some did withdraw from the labor force, or did not enter at all.

Women in two-parent families who worked between a little over 3 and a little over 16 hours in a week, on the average, in the experiments, reduced the hours they worked between 0 and 31 percent.

Young adults aged 16 to 21 who did not head families reduced their work effort between 18 and 24 percent.

The evidence on income reporting which I summarized on page 10 of my testimony indicates that it is feasible to collect information from recipients on a monthly basis about actual income, program participation and family composition. The demonstration going on in Boulder and Denver adds some additional evidence.

Senator MOYNIHAN. We just heard that from Dr. Williams.

Mr. AARON. Yes, sir.

I would like to interject, at this point, that, as you know, we now have additional demonstrations going on in other parts of the country—

Senator MOYNIHAN. Did you say Boulder?

Mr. AARON. Yes, Boulder, Colo.

But in addition, the Congress added funds for us to carry out a series

of demonstrations in other parts of the country in order to examine how the system of monthly reporting functions in communities with different kinds of caseloads—some of which are older cities, some of which may or may not have computer capabilities readily available—and to test variations of the reporting scheme to determine the best way to proceed.

In answer to a question you posed to Mr. Williams, that is the reason we are proposing to make monthly reporting voluntary at the present time. We would like to see how it operates in different environments.

We concur roughly with Mr. Williams' estimates of the savings that can be achieved by monthly reporting.

On the question of family stability, which begins on page 11 of my testimony, each of the four experiments collected data on the stability of families in both the control and experimental groups. Families in the experiment are defined to include couples living together whether or not they are legally married.

There is a table in the testimony that makes it clear that the findings have been far from consistent. In some of the experiments, families in some ethnic or racial groups that received cash assistance were less stable than families in control group. Sometimes in the same experiment, for different benefit plans and for other ethnic or racial groups, families receiving cash assistance were more stable than other families.

Senator MOYNIHAN. I see signs of panel fatigue. Can you spot them there?

Mr. AARON. We have looked into the question of panel fatigue, Senator, because it is an experimental question that deserves attention. We find that if one adds to the sample, those couples who formed in the course of the experiment, which tends to offset panel fatigue, the same pattern continued at least in Seattle and Denver.

We have not examined that question of panel fatigue in the other site. But there is a possibility that panel fatigue does occur. I would not want to dismiss it completely.

Senator MOYNIHAN. Could you read this table so I would understand it?

"The ratio of experimental to control group of family instability rate." Now does that first New Jersey 3-year results mean that there were 9 percent greater instances of family dissolution?

Mr. AARON. It means that is the statistical estimate that was not statistically significant that merged from the analysis performed.

Senator MOYNIHAN. Where is that?

Mr. AARON. The asterisk—

Senator MOYNIHAN. You have to look at your asterisk, right.

And rural there was 2½ times—

Mr. AARON. For whites, but—

Senator MOYNIHAN. But not statistically significant?

Mr. AARON. That is correct.

Senator MOYNIHAN. In Seattle-Denver, you had—

Mr. AARON. On the rural experiment, Senator, I want to point out that there were very few families that split in either the control groups or the experimentals and we have tended not to attach much weight to those people.

Senator MOYNIHAN. So you can have a small base and see it double and it may look remarkable but the numbers are still small. I understand that in Seattle-Denver, however, the base was large.

Mr. AARON. Yes, sir.

Senator MOYNIHAN. And so, do I read there in Seattle-Denver that in the 2-year results, there was an 85 percent higher rate in the experimental group than the control group?

Mr. AARON. Yes, 85 percent for whites, 48 percent for blacks, 12 percent for Hispanics.

Senator MOYNIHAN. And then you have in the third year it drops off to less than for whites. Then the 3-year results are 44 percent, 45 percent, 46 percent. For third-year only you seem to have some fatigue or stabilization or whatever that is.

Mr. AARON. Well, this table presents the analysis, as I indicated, for a broad sample that includes couples who formed during the course of the experiment. When couples who formed during the experiment are not included in the data, the results are approximately the same in their general pattern.

I would be glad if you like, to submit data on those for the record.

Senator MOYNIHAN. No, no. You are the people in charge of the data. We trust you. We are just trying to find out the general range of these things.

But the statistically significant results all show a pronounced increase.

Mr. AARON. There are four statistically significant results, yes, that is correct.

Senator MOYNIHAN. All right, sir.

Mr. AARON. May I continue?

Senator MOYNIHAN. Please do.

Mr. AARON. You have just, in the course of these questions, gone over the material that appears at the bottom of page 13 of my testimony, namely, that for some groups, the effect continued and for some it did not.

Finishing up with the summary—

Senator MOYNIHAN. Well, you have to say you do not know that because you have not got a statistically significant finding. At least it does not look that way.

Mr. AARON. We do know that the coefficients are, in some cases, greater than 1 and in some cases less than 1. The only statistically significant findings are the ones that are greater than 1.

Now, we did look at some other questions, although they were not the major reason for the experiment, and the testimony summarizes some findings with respect to demand for social services, data on health and nutrition, housing and home ownership, school attendance and performance, consumption, attitudes toward AFDC and income maintenance and on manpower, training, and education.

I think it would be best if I turned to the interpretation and implications of the results and that is on page 16 of my testimony.

I would like to try to explain how the findings on work effort, the principal subject of the research, have been, and are being used extensively in policy planning, and I would like to discuss also the family stability findings yielded by one of the experiments, the Seattle-Denver experiments, a matter I know interests you, Senator.

First of all, with respect to work effort, the income maintenance experiments, together with a large body of nonexperimental research, have taught us a number of important lessons that bear directly on policy.

Evidence from the experiments persuaded the administration not to propose the pure cash assistance program examined in all the experiments, but rather to propose a carefully constructed package. Indeed, this program initiated a novel integration of jobs and cash assistance that had the following major elements: A restructured cash assistance program that distinguishes between those who are and those who are not expected to work; a limit on benefit reduction rates for families containing a member expected to work; greatly expanded employment and training opportunities for the low-income population; work requirements and an expanded earned income tax credit to aid low-income working families even further.

Because of its work requirements and direct job creation provisions, the President's program would have increased, not reduced, total hours worked and raised total earnings in the economy by an estimated \$5.4 billion. Significantly, the major alternative plans for the welfare reform program proposed in the last Congress, including that advanced by the New Coalition, all include similar features.

With respect to the question of family stability, the results of the experiment on family stability are puzzling, in contrast to those on work effort. The findings on work effort are consistent with a well-developed body of theory, do not differ significantly from nonexperimental analysis, are consistent across experiments, and contain relatively few anomalies.

On the other hand, the results on family stability do not grow out of developed theory. The specific findings of the Seattle-Denver experiment conflict, to some extent, with nonexperimental analysis; are not consistent with findings from the other experiments; and contain some puzzling results that I will come to in a minute.

I want to say, however, that given its greater sample size, the results from Seattle and Denver probably merit more attention than those from any of the other experiments. Nevertheless, the diversity of results across experiments and over time, in my judgment, precludes generalization.

The conditions in the experiments were quite unlike any system of income support and assistance that now exists or that Congress is likely to enact. The experiment had no work requirement, no program of child support enforcement, no caseworker contact or advice, no WIN registration, job search or placement activities.

When one points to these differences, however, it is not to criticize the research design which was planned principally to measure the effect of financial incentives to work. To measure those effects, other influences were minimized.

Senator MOYNIHAN. I assume that the research design was at least approved by HEW; you paid for it.

Mr. AARON. Yes, sir. I believe it was approved in the early 1970's.

With respect to the effects of income maintenance on family stability, we believe these and other differences between conditions in the real world and in the experiments tend to raise serious questions

about the relevance of the experimental findings to any welfare reform plan likely to be proposed. Thus, for example, benefits paid by the Seattle-Denver experiment differed from those in AFDC in one key respect: Fathers or mothers in the control group who left their partners and children were ineligible for AFDC benefits; but fathers or mothers in the experiment who left their children continued to be eligible for benefits which, in 1978 dollars, were approximately \$1,600.

I might add that a large fraction of the parents who left their partners, in fact, did retain their eligibility for benefits.

This difference made family splitting more attractive to families in the experiment than it would have been to families in the AFDC program.

The second point was that the administrators of the experiment scrupulously and, in my opinion, correctly, informed families about the terms of assistance, including the fact that each member of the experimental family would remain eligible for assistance if the family split. The Seattle-Denver experiment provided participants with complete information about benefits for which they were eligible and the alterations that would occur following changes in the family situation.

Such counseling is not characteristic of real-world welfare programs.

Third, not all family breakup was marital breakup. A family was defined as a couple, with or without children, that was married or living in a consensual union. Some fraction of the family breakups studied were, in fact, terminations of consensual unions. Moreover, families who split under the experiment were not subject to the child support enforcement program, an important feature of current welfare programs that may act as a deterrent to family instability.

In addition, there were a number of statistical features, some of which I have mentioned, as well as others I would like to point out at this time, that I find puzzling. First, as I pointed out, receipt of assistance did not significantly increase family instability among Hispanics. We were not able to explain why Hispanics behaved so differently from either blacks or whites.

During the third year, the differential in the—

Senator MOYNIHAN. You do not have a statistically significant finding on Hispanics, do you?

Mr. AARON. No, sir.

It is statistically significant, however, from the breakup rate of the other groups. That is the puzzling feature. It is not significantly different from zero.

During the third year, as we pointed out before, the differential in the breakup rate for white families disappeared, but it declined only slightly for black families. The same analytical model that was used to estimate the effects of cash assistance on family stability produced a result that I find extremely hard to rationalize. According to this model, the rate at which white and Hispanic families split increased 49 percent and 87 percent respectively when they were merely offered—merely offered—job counseling.

If, in fact, they took training, that effect largely vanished.

We do not have any plausible explanation for that particular result, but I want to point out that it was produced by the same model, the same equation, that yielded the estimates of the impact of cash assistance on family stability.

Finally, there is something that I know you have also heard about in the course of the past 2 days called attrition bias, that tends to reduce the statistical estimates of the impacts of the experiments on family stability, although I want to point out, not enough to make it go away.

The experiments offered recipients cash assistance alone. Starting with the President's program, present welfare reform proposals provide for reform of cash assistance and for direct job creation. The findings of two other nonexperimental major longitudinal surveys suggests that this difference is critical.

Studies based on the panel survey on income dynamics that was sponsored by HEW and run by the Michigan Survey Research Center and on the National Longitudinal Survey that is sponsored by the Department of Labor and run by Ohio State University, produced evidence to support the following statements.

First: That unemployment of a husband is associated with marital instability.

Second: If a family's economic situation worsened in the most recent year, the likelihood of marital breakup increases. Similarly, if its economic situation improved, the likelihood of marital breakup declines.

Third: When a husband's earnings are low relative to his expected earnings, the likelihood of marital breakup increases.

Finally, the possession of economic assets such as homes and savings is correlated with marital stability.

This evidence suggests that employment is an important influence on marital stability, partly because of its impact on earnings and partly because of the impact of work on attitudes. It tells us that the impacts of cash assistance experiments on family stability may not carry over to programs that create jobs while they reform cash.

Let me wind up by saying that my own conviction is unshaken that the best way to achieve equity in the welfare system, a goal that most people share, lies in the adoption of the general strategy proposed by the administration last year and adopted in the major incremental plans offered as alternatives. That strategy includes development of employment and training opportunities and equitable reform of the distribution of cash assistance.

Lessons we learned from the experiments about the impact of cash assistance on work effort have helped us to shape this strategy and will continue to influence welfare policy for years to come.

Thank you.

Senator MOYNIHAN. Well, I thank you, sir, and let me join you in that conclusion. I would like to be explicit on the point that, as you know, I introduced the President's bill last year and I did not introduce any other bills, although I worked with the new coalition at a certain point, because I wanted to maintain my adherence to the President's bill until the time came when the House announced it was not going to hold hearings and there was not any chance of its coming over.

I think that the budgetary situation—well, you know, we have problems, but we are going to see what you are going to bring forward this year, and whatever happens this year it is going to be very much—your mark is going to be on it, and that is going to be very reassuring to me.



I would like to ask you just a couple of questions—well, more than a few—and answer them or do not. You have 4 weeks left; you can just raise hell.

Mr. AARON. I cannot tell you how relaxed I am feeling.

Senator MOYNIHAN. On the question of work effort and the reduction of families, on page 8 you sort of estimate it between 1 and 11 percent, but on Wednesday, John Cogan of Stanford testified that he had re-analyzed that New Jersey data and in effect doubled the rates which Harold Watt reported first. He explained how the doubling came about, that Watt was reporting reductions in rates from people who did receive the negative tax and also those who did not.

So that is what that is all about. How would you have an effect or receiving a certain income if you are not receiving it? And Cogan gets an estimate for adult males of roughly 12.5 percent to 17.5 percent per week higher—I mean 5 to 7 hours. Is that not the way he put it, 5 to 7 hours? That is almost a workday.

And the analysts at Stanford Research Institute, SRI, said they got the same numbers. They are different from your numbers. I do not know whether you have caught up with them or whether you have gone past them, or what.

Mr. AARON. Well, first, Senator, I would like to mention that we are aware of Mr. Cogan's results. Indeed, we sponsored the research.

Senator MOYNIHAN. Yes; you are darned right you did. I want to make that very clear. All of these things that are coming up are things you paid to find out and they are unflinching and they are difficult and that is called the world.

Mr. AARON. The second point is that we did not use the New Jersey results at all in estimating the cost of welfare reform proposals, nor did any of the other agencies.

Senator MOYNIHAN. SRI seems to have the same rate out of Seattle-Denver.

Mr. AARON. There are two kinds of statistics that the experiments yield, and I hope I will not be getting too technical, but let me try and go into that a bit. One is the simple difference between the overall behavior of the control group and the experimental group. The other is called "structural estimates," or "parameters" in statisticians' terminology. They indicate the degree to which people respond to variations in the amount of basic benefits and the degree to which they respond to variations in benefit reduction rates.

The comments that Mr. Cogan made go to the simple differences between experimental families and controls. Such statistics from neither New Jersey nor Seattle-Denver are used in making direct cost estimates. We do not believe that his procedures are correct for adjusting or correcting the estimates of the structural model.

Senator MOYNIHAN. So you are now arguing methodology? In the profession, you are now having this argument?

Mr. AARON. I am pointing out that the way he presents the results—a comparison of one kind of group with another kind of group—is interesting, but it does not bear on the question of cost estimation.

Senator MOYNIHAN. Let me assure you that this subcommittee is entirely comfortable with the fact that statisticians argue. They argue all the time. The only thing that statisticians do do is argue, but you learn a lot that way.

Mr. AARON. Absolutely.

Senator MOYNIHAN. You go through this endless, seemingly pointless dreary thing, but in the end you learn things that way. I edited a book on correlation coefficients with Frederick Mosteller, so I ought to know something about arguing.

Mr. BARTH. The only thing I would add, Senator, is that I believe Mr. Cogan said that the cost estimates given out by HEW and the Congressional Budget Office may be off because of the alterations he suggested. I think that the work done by HEW and the Labor Department suggests that the specific kinds of estimates has produced for us by the Stanford Research Institute are not subject to bias.

Senator MOYNIHAN. I leave that to the professional judgment of the Department. We have no questions about it. In the end, you make decisions. Nothing is easy as you get into those things. We know that and that is why it is an honored profession. It is not easy.

But I do think, I must say, that both of them came out with this 5 hours to 7 hours decline which was not just coincidental. But I must say that Cogan's explanation of how he came out differently from Watts was a little bit discouraging—nothing to do with you people, but it seems to me that Professor Watts has something to explain.

Let me ask you, though, a couple of other questions. On Wednesday, Robert Spiegelman of SRI testified that the kind of program tested in Denver and Seattle corresponded—and these are his words—“almost exactly to the upper-tier benefit level of the better jobs and income program,” which was the President's program, and that the lower-tier benefit was set at a level which his research suggested would be especially prone to producing an independent effect and consequently increase family splitting.

That was kind of hard for us to hear. Did you get that word back?

Mr. AARON. Yes, we did, Senator.

I would only point out again the various ways in which the experimental environment and the level of benefits differed from that of existing welfare programs or, indeed, from any that the Administration is likely to propose or that Congress is likely to enact. The crucial differences lie in the existence of a child support enforcement program, and the presence of work requirements and job opportunities and training, none of which were present during the experiments. (For a more detailed discussion of both financial and administrative differences between the experiment and the Administration's Welfare Reform Proposal, see Assistant Secretary Aaron's response to Senator Moynihan's letter of November 24, 1978, also in this record.)

Senator MOYNIHAN. We have to face the fact that by the time the President's program last year got all the optionals and extras and special disc brakes and things like that that deal with these difficulties, it was costing \$20 billion. That is right, is it not?

Mr. AARON. That is the estimate of the Congressional Budget Office.

Senator MOYNIHAN. That makes a lot of claim on our attention.

Mr. AARON. They are a very reputable group.

Senator MOYNIHAN. Twenty billion bucks. We are not going to get \$20 billion. You know that. The President is going to send up a program which, at most, will not likely cost much more than \$6 billion. I have been informed of that. They are being very straightforward. They are not concealing the expectation it is going to be a tight budget. We have heard rumors about it all over town.

Mr. AARON. Word has gotten around to the Department of Health, Education, and Welfare.

Senator MOYNIHAN. Can you get a program that meets your concerns for that cost?

Mr. AARON. I think we can get a program that meets the basic principles that were contained in the administration's proposal last year, a national minimum benefit to increase the most egregiously low benefits; improved coverage for two-parent families; and the provision of job opportunities for most or all of those who would become eligible for cash assistance, together with some improvement, perhaps in the earned income tax credit, all of which were part of the proposal last year.

Senator MOYNIHAN. Is Mr. Barth writing, or telling you in that note, to include fiscal relief?

Mr. AARON. You have retained the great skill of a bureaucrat, which is to be able to read upsidedown. That is correct. He was writing fiscal relief. [General laughter.]

Senator MOYNIHAN. He will go far.

Mr. AARON. He is remaining in HEW.

Senator MOYNIHAN. I guess I want to ask just a few things more. Donna Shalala was here yesterday and they have spent more money on their experiments than you have on yours, and that is because no one knew about their experiments. They obviously went to their committee and said, look, those people got experiments, why do we not have experiments? All good, up-to-date committees, finance experiments.

She indicated the results were disappointing, but is it ever entirely disappointing to learn something? It should not be disappointing to learn something. You ought always say, listen, if an experiment fails, you have learned something. I mean, there is knowledge that comes out, albeit knowledge of a negative kind, that this is important.

I said yesterday that one of the things about all of this is that there was a 19th century American saying that goes, "It is not so much ignorance that hurts as knowing all those things that ain't so," and if so, we have found out some things in housing that ain't so. Well, she says they are going back to the drawing boards.

I guess I have to put this to you, sir. On page 27, you say to us that the evidence suggests that there is new evidence from the survey research center and the National Longitudinal Survey and so forth and I quote you: "It tells us that the impacts of cash assistance experiments on family stability may not carry over to programs that create jobs while they reform cash."

"May not" is a very conditional term.

Mr. AARON. Yes, it is. If I were dead sure, I would not have used it.

Senator MOYNIHAN. I know you would not. I know you are very careful about what you are saying.

We discussed this the other day, when one of the witnesses was talking about the circumstances of the families whose disruption has been induced—the word was "induced." I wrote a book about 10 years ago now, badly reviewed by the Secretary of HEW, I must say, on the community action programs of the Federal Government, in which I said that social scientists had to ask themselves about the ethics of experiments with communities in the way that medical researchers and pathologists have concern about the experiments with human beings.

There is such a thing as malpractice with experiments with communities. There is a saying around the criminal courts that the lawyer always goes home and it was an interesting defense, but it did not work. I mean, you know, to set about an enterprise that may have, or may not have, consequences are pretty large. It is a large responsibility.

If it turned out that we have a program that reduced work effort by 3.2 hours per week in white males, I think the world could go on and it would not be any great disaster. But breaking up families is a large event. It means setting up situations which have differences, and demonstrably related differences. That is doing something to a child.

It seems to me that children have got to be the focus of what we do. We do this for children. We started out as an Aid to Dependent Children, not a job program, not a training program. It is not a program in social mobility. It is designed to take better care of children.

We do have, do we not—I guess I would ask you to say this but maybe I should not. I do not want to ask you something that in any way seems to require you to say something more than you want, but if we do not get the whole of the program, should we take parts of it? Are we to the point that we think that these things are delicate enough in their relationships that we had better have it all, or you cannot predict what in the devil you might do?

Mr. AARON. I would not speculate, but I would like to point out a couple of reasons why I think even piecemeal advances are worth making. Benefits are extremely low, even for single-parent families, in some parts of the United States. There are hungry children precisely because such benefits are low and to the extent that—

Senator MOYNIHAN. Not many, and the amounts of money required is not that far. There are about 11 States where you would say they are just too low.

Mr. AARON. Well, it depends on what one's standard is. But there are States that continue to have very low benefits and, as you know, the participation rate in the food stamp program which is the only thing that enables us to keep that number as low as 11, is far from complete.

Senator MOYNIHAN. Far from complete.

Mr. AARON. Second, we treat two-parent families under the Unemployed Father program in a highly inequitable way. And that deserves attention.

Senator MOYNIHAN. You know we had some testimony about the degree to which that induces marital stability?

Mr. AARON. No, I did not know there was any testimony on that.

Senator MOYNIHAN. Yes, we did. There is no bad news we have not heard in the last 3 days.

Mr. AARON. Did you also have testimony to the effect that the one study done on the impact of the Unemployed Father Program indicates that it may have a small, positive impact on family stability among whites and essentially no effect with respect to blacks?

That was not an experimental study. It was a study done by a scholar using other data.

Senator MOYNIHAN. He did not—

Mr. AARON. I would be glad to supply that for the record, if you would like.

Senator MOYNIHAN. Oh, if you would. We did not pretend to be exhaustive in this.

[The following was subsequently supplied for the record:]

REVIEW OF THE LITERATURE ON THE EFFECTS  
OF  
INCOME SUPPLEMENTS ON MARITAL STABILITY

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Recent approaches to modeling the relationship between marital status and income decisions are reviewed in this paper.\* An overview of previous research findings on the association between income supplements and marital stability is also reviewed. For more extensive reviews of the association between other variables in this large and growing body of literature and marital stability, the reader is referred to Ross and Sawhill (1976), Hayes (1976), and Michael (1977), among others.

#### Modeling Marital Status and Income Decisions

Recent models of marital dissolution and formation decisions are characterized by several unifying themes. The first is that individuals are utility maximizers and that the marital state (and marriage) the person occupies provides the maximum attainable utility. Secondly, single and married persons are regarded as choosing to remain in their current marital state, over its alternative, on the basis of the net utility they expect to gain from comparisons across marital states. And finally, these choices are regarded as being made on the basis of a household production function. The time allocations, purchased goods and services, and in some treatments, non-pecuniary satisfactions of its members, are treated as inputs to the family's home production of commodities which cannot usually be purchased in the market--satisfaction, love, companionship, emotional security, and the like, which are referred to as utility. Each household is assumed to maximize its utility from these home produced commodities subject to the time and wealth constraints of its members. In this way, persons marrying are regarded as being better off and having chosen a mate that

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\*This paper is part of an analysis of the simultaneity between marital and labor decisions among SIME/DIME families.

makes them best off; dissolutions result when one or both of the spouses no longer regards his or her current household as the optimal unit of production (or some other spouse is preferred over the current mate).

The notion of people making marital status decisions on the basis of the net utility derived from comparisons across marital status is not new. Social-psychological literature, which regards marital relationships to be special cases of paired relationships, has long embraced this notion. Levinger [1965], however, is generally credited with providing the first integrative review of this literature with regard to marital dissolution. His initial efforts were subsequently extended by Cutright and Scanzoni [1973] who assert that the level of marital satisfaction relative to the constraints inhibiting marital dissolution is the determining factor in marital dissolution. Levinger argues similarly and sketches a picture of the process by which dissolution results:

...people stay in relationships because they are attracted to them and/or they are barred from leaving them, and that, consciously or not, people compare their current relationships with alternative ones. If internal attraction and barrier forces become distinctly weaker than those from a viable alternative, the consequence is breakup (Levinger, 1974:43).

Levinger cites the 1951 work of Lewian as the basis for his interpretation that "barriers or restraining forces" keep people intact. The hypothetical constructs of "attraction" and "alternative attractions", which refer to perceived or expected rewards--love, money, status, service, etc., and costs associated with membership in various relationships, are generally attributed to Thibaut and Kelley [1959] and Foa [1971].

It is only recently, however, that there have been explicit efforts to model utility comparisons across marital states. The earliest approaches to modeling marital change decisions were limited to what are termed "prediction studies", in which an effort was made to identify characteristics and experiences which affected the quality of interpersonal relationships. Psychologists since the 1930's, in particular, have concentrated on constructing "marital success indexes" from economic, attitudinal, and individual and family background variables [Terman, 1938; Burgess and Cottrell, 1939; Locke, 1951]. More sociological efforts have concentrated on studying dissimilarities among spouses with respect to their socioeconomic background, religion, education, and age, as predictors of "marital durability" [Kirkpatrick, 1937; Williams, 1938; Burgess and Waller, 1953; Thomas, 1964; Bowerman, 1964; Goode, 1971; Bumpass and Sweet, 1973].

Much of the subsequent empirical research on marital formation and dissolution [Ross and Sawhill, 1975; Sawhill et al., 1975; Orcutt et al., 1976; Hayes, 1976; Cherlin, 1976] has also concentrated on identifying the determinants of these events, but the focus has been directed at estimating the partial effect of a whole host of variables hypothesized to have an impact. The general approach taken is to relate the probability of a change in marital status  $P$  during period  $t$  to a large set of exogeneous variables,  $X_1, X_2, \dots, X_n$ , such as age, income, duration, place of residence, AFDC status, and the like:

$$P_t = \alpha_t + \sum \beta_{t1} X_i + \epsilon_t, \quad (1)$$

where  $\beta_{t1}$  is interpreted as the partial effect of variable  $X_i$  on the probability of a change in marital status. More recently, Hannan et al. [1976, 1977] report findings from the Seattle and Denver Income Maintenance experiments where  $P_t$  is transformed into a "continuous rate" of marital change:



$$r_1(t/t') = \frac{\frac{dP_1(t)}{dt}}{[1 - F_1(t)]} \quad (2)$$

where  $r_1(t)$  is the probability density of dissolution at time  $t$  given that a change in marital status has not occurred since  $t'$  (the beginning of the married or single state) and  $F_1(t)$  is the probability that the  $i^{\text{th}}$  person will experience a change in their marital status before time  $t$ .  $r_1(t/t')$  is regressed upon many of the same set of  $X$  variables included in other analyses of marital behavior.

Recent analysis of marital status decisions have endeavored to provide a more rigorous foundation for considering the effects of various predictors. The genesis of this body of work, as represented by the research of Keeley [1974], Hutchens [1976], and Wolf [1977], is Becker's "theory of marriage." Briefly stated, the household is regarded as a producing unit for commodities which (usually) cannot be otherwise purchased in the market place. Each person is viewed as making marital choices on the basis of a household production function, with the economic, social, and psychological characteristics of the two spouses as production inputs, and utility as its output. Thus, letting  $Z$  represent the combined outputs of the household, it is assumed that each household has a utility function  $Z$  as its only argument: \*

$$Z = f(x_1, \dots, x_m; t_1, \dots, t_k; E) \quad (3)$$

where  $x_i$  are goods and services;  $t_j$  the home inputs of individual members; and  $E$  represents certain "environmental variables." Utility is assumed to be an increasing function of  $Z$ , and is maximized subject to the household's budget constraint:

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\* This particular description of Becker's model draws upon that provided by Robert Hutchens.

$$\sum_{i=1}^m p_i x_i = \sum_{j=1}^k w_j (T - t_j) + V, \quad (4)$$

or:

$$\sum_{i=1}^m p_i x_i + \sum_{j=1}^k w_j t_j = \sum_{j=1}^k w_j T + V = S, \quad (5)$$

where  $w_j$  is the wage rate of the  $j^{\text{th}}$  household member;  $V$  is property income;  $T$  is the total time available to the household members for market and non-market work; and  $S$  is full income--the maximum attainable money income given  $w_j$ .

It is further assumed that persons marry because they expect to be better off in a two person production unit--that is they expect to increase their consumption of  $Z$ , and select a mate that makes them best off. The marital choice depends then on whether or not a single male or female family produces more output, net of the costs of production, than a family producing with inputs from two spouses. A male or female will prefer marriage if, and only if:

$$Z_{m0} \leq Z_{mf}^m, \text{ and } Z_{of} \leq Z_{mf}^f, \quad (6)$$

where:

- $Z_{m0}$  = the amount of  $Z$  produced by a single male household
- $Z_{of}$  = the amount of  $Z$  produced by a single female household
- $Z_{mf}$  = the combined output of a married household where  $Z_{mf}^f$  and  $Z_{mf}^m$  are the male and female shares of  $Z_{mf}$ , respectively.

A necessary condition for marriage to occur, however, is that the marginal gains from being married exceed the marginal costs:

$$Z_{mf} \geq Z_{m0} + Z_{of} \quad (7)$$

One source of such gains arises out the opportunity for low wage household members to substitute time they would otherwise spend in the market in home production, thus permitting the higher wage member to spend more time in the market. Becker argues that the main sources of the gains in marriage, however, lie in the desire of married households to raise their own children and the physical and emotional attraction between sexes:

The importance of own children and love implies that, even with constant returns to scale, M and F gain from marriage because  $t_m$  and  $t_f$  are not perfect substitutes for each other or for goods and services supplied by market firms or households. When substitution is imperfect, single persons cannot produce small-scale equivalents of the optimal combination of inputs achieved by married couples [Becker, 1974:818-819].

Becker extends his "theory of marriage" in a recent paper to more directly focus on the causes of divorce [Becker et al., 1978]. The theoretical model presented incorporates uncertainty about marital decision outcomes into the utility maximization and market framework previously developed. Persons are regarded as seeking to maximize their expected full wealth--the life time utility they expect to receive from home produced commodities discounted to their present value. Under conditions of certainty, various marital strategies produce known amounts of full wealth, and the person chooses the one that promises the greatest life time wealth. With uncertainty, however, dissolution occurs because of unexpected events. Since the gains from marriage are not usually known with certainty, the probability of divorce is viewed as a function of the expected gains from marriage and the distribution of a variable describing various unexpected outcomes. The greater the expected gains and the smaller the variance of the distribution of this variable describing unanticipated gains, the smaller the occurrence of divorce.

Other known formal marital status change models are very much in keeping with the theoretical framework developed by Becker. Hutchens (1976) introduces time elements and transition costs into a model which closely parallels that of Becker. A person will want to marry when:

$$\sum_{i=1}^N [(U_w^* - U_s^*) / (1 + d)^{i-1}] + U' > 0, \quad (8)$$

where:

- $U_w^*$  = is the maximum level of utility attainable by the person when married
- $U_s^*$  = is the maximum level of utility attainable by the person when separated (or single)
- $d$  = is the rate at which the individual discounts the future
- $N$  = is the number of periods the person expects to remain married (or alternatively separated or single)
- $U'$  = is the cost of transition from separated to married status (or alternatively from married to separated status)

Alternatively, a person will want to divorce when the discounted difference in utility between the two states and the transaction costs are less than zero.

Hutchens's "time notions" are subsequently discarded, as he develops his model along the lines of Becker's. Utility,  $Z$ , is related to household outputs associated with each state, which in turn, are functionally related to nonmarket time inputs, market goods, and environmental variables. Household utility is maximized subject to the time and budget constraints of its members. The utility gain from marriage for a female,  $\Delta U_f$ , for example, is then specified to depend upon her utility level while single,  $U_{of}$ , and the difference between the utility accruing to her in a married-couple household:  $U_{mf} - U_{of}$  (in other words her gains from marriage):

$$\Delta U_f = g(U_{of}, U_{mf}^f - U_{of}) \quad (9)$$

The utility gain from marriage for males is similarly specified, and  $\Delta U_f$  and  $\Delta U_m$  are expressed as functions of exogeneous economic--wages rates, unearned income, AFDC guarantee and tax rate, and non-economic variables-- education, sex ratio, age, religion, residence, race, and health:

$$\Delta U_f = f(X_1, \dots, X_n) + e \quad (10)$$

$$\Delta U_m = m(Y_1, \dots, Y_n) + v \quad (11)$$

The stochastic terms:  $e$  and  $v$ , reflect the fact that all the determinants of  $\Delta U_f$  and  $\Delta U_m$  are not observed. Thus the decision to split (S) or marry (M) is determined by:

$$S = g[f(X_1, \dots, X_n); m(Y_1, \dots, Y_n)] + v \quad (12)$$

$$M = h[f(X_1, \dots, X_n); m(Y_1, \dots, Y_n)] + w \quad (13)$$

and a split occurs when:

$$f(X_1, \dots, X_n) + e < 0, \text{ or } m(Y_1, \dots, Y_n) + v < 0 \quad (14)$$

Wolf [1978], in another analysis of marital split decisions among wives, also follows the Becker tradition. Triggering the split decision is the utility difference between the single and married states for the female in excess of her psychic loss due to a marital split. In line with the general approach taken by Hutchens, the nonmarket time of household members and household consumption in both the married and single states are specified as utility arguments. Labor supply and income in both the single and married states, replaces nonmarket time and consumption, respectively, in the utility difference expression:

$$U_w(L_{wS}^*, 0, Y_S^*) + Z_{wS} - \{U_w(L_{wI}^*, L_{hI}^*, Y_I^*) + Z_{wI}\} > 0 \quad (15a)$$

or:

$$U_w(L_{wS}^*, 0, Y_S^*) - U_w(L_{wI}^*, L_{hI}^*, Y_I^*) > (Z_{wI} - Z_{wS}), \quad (15b)$$

where:

- L = is leisure or nonworking time
- w,h = index the wife and husband
- Y = is total family income
- S,I = index the single and married state
- Z = index nonpecuniary or psychic components of well-being.

Wolf's model differs from the indirect utility function approach taken by Hutchens, however, in that the psychic loss due to a split is specified to contain only systematic, predetermined variable determinants; none of the exogenous budget line components: effective wages or non-employment income at zero earnings levels, are specified in the Z vector.

Wolf assumes for the purposes of his analysis that the optimal values of L and Y in single and married states are chosen in a conventional utility-optimization framework subject to the budget constraints within each state. However, in the operationalization of his model, Wolf does assume a certain degree of endogeneity of labor supply and income by differentiating between a "reduced form" and a "structural form." For the latter, Wolf estimates instrumental variables for income and labor supply and uses these instruments to estimate equation (15); for the former, Wolf inserts the equations for income and labor supply into equation (15) and estimates the equation containing all the "exogenous" variables. This model can be characterized as "recursive" or "simultaneous", but it is consistent and econometrically equivalent with either.

In his estimations, Wolf must choose a particular functional form for the utility function, and chooses an additive, linear form in  $L$  and  $Y$ . He also uses a sample of female-headed families to estimate instrumental values of income and labor supply in the single state, and assumes that these accurately measure the alternative opportunities for married women in the sample. In addition, all income and labor supply values are taken as of a prior point in time to the split decision. Thus the model is explicitly dynamic rather than static.

Marital Stability and Income

The primary focus of this analysis is the relationship between marital stability and income. Although not reviewed here, a considerable amount of evidence has been generated as to the effect and statistical significance of various other predictors of marital events. The array of micro-level variables presented in table 1 and 2 are representative of the many determinants which are considered important. The literature also posits several macro-level variables hypothesized to effect marital events. Various proxies for business cycle conditions and wars dominate this body of literature [Galbraith and Thomas, 1941; Jacobson, 1959; Glick et al., 1963]. For example, the divorce rate reached a precipitous high after World War II. The important and as yet unanswered question is whether macro variables have an impact on marital events independent of their micro-level implications. For example, does the national unemployment level have an effect on dissolution net of the cumulative micro effects on the marriages of those directly affected by unemployment? Are the aggregated relationships between unemployment and divorce simply summations of the individual (micro) impacts? Statistically significant, although quantitatively small, "period" effects on divorce have been estimated (Orcutt et al; 1976), but the estimation was unable to control for changes over time in the distribution of micro variables affecting divorce.

Notwithstanding the lack of theory underlying most of the available descriptive analyses, their cumulative force provides strong support for the notion that marital changes are not random processes. Quite the contrary, there is clear selectivity or a strong sorting process at work. This sorting is affected by attitudes, aspiration, and perceptions; by current and potential status; by ascribed demographic and family of origin characteristics, and by cer-



TABLE 1  
MICRO-LEVEL VARIABLES RELATED TO MARITAL FORMATION PROBABILITIES

Psychological Variables	Social, Cultural and Demographic Variables	Economic Variables
Adolescent social and emotional adjustment	Sex	Human Capital
Age at initially dating	Race	Education
Frequency of dating	Place of Residence	Mental Ability
Number of steady date partners	Region	Work Experience
Age at engagement	Urban/Rural	Wage Rate
Happiness and Satisfaction	Social Class	Hours worked
Family orientation	Parents' education	Assets
Expectations for marriage	Parents' occupations	AFDC Status
Adolescent Goals	Parents' income	Income
Educational aspirations	Religion	
Occupational aspirations	Ethnicity	
Influence of Others	Premarital Sexual Behavior	
Perception of others' expectations	Age of first intercourse	
Peer behavior	Premarital conception/birth	
	Age	
	Dependent Children	
	Duration single	

TABLE 2

MICRO-LEVEL VARIABLES RELATED TO  
MARITAL DISSOLUTION PROBABILITIES

Psychological Variables	Social, Cultural and Demographic Variables	Economic Variables
Social and Emotional Adjustment	Race	Human Capital
Dependence on the marriage for emotional fulfillment	Place of Residence	Education
Quality of interaction between spouses	Region	Disability
Happiness and Satisfaction	Urban/Rural	Wage Rate
Expectations for marriage	Social Class	Hours worked
Realism of Marital Expectations	Religion	Assets
Influence of Others	Ethnicity	Wife's Dependence on the Marriage for economic support
Involvement with relatives and other family members	Age	Income
Involvement with other community members	Duration Married	
	Number of Times Married	
	Dependent Children	

tain relevant experiences. Some of these variables are completely outside the control of the person involved (sex, race, characteristics of family of origin), while others are under their short-term (adolescent goals) or long-term control (education, income).

The available research has generated a number of findings with respect to the relationship between marital stability and income. Among the more prominent of these is the strong stabilizing effect of husband's and family income upon family dissolution. A strong negative "income effect" has been reported by Ross and Sawhill [1975], Sawhill et al. [1975], Hampton [1975] and Becker et al. [1975], among others. The findings with respect to the income of wives, however, indicates the opposite effect. Many of these same studies as well as others [Lannan et al., 1977] report a positive destabilizing effect on family stability of the wife's income. This has led to speculation that the financial independence of the wife results in an "independence effect." One possible explanation for this relationship is that a number of marital relationships are maintained strictly because of the wife's dependence upon the marriage for financial support, resulting in a sort of "coercive marital contract." The availability of an alternative source of income permits the wife to support herself and, in effect, to escape from the marriage.

The availability of AFDC permits women to support themselves independently of their husband's earnings and leave an otherwise unfulfilling marriage. This connection has led to speculation that welfare is a contributing factor in the dissolution of families, especially through its requisite of singleness as a usual condition for eligibility. Moreover, the financial incentives of welfare programs like AFDC are such that the family may actually be better off economically if they split up or at least pretend too.

Unequivocal evidence of a direct relationship between welfare and family stability is non-existent, however. Both positive (predicting dissolution), ambiguous, and negative results are reported in various studies using both cross-sectional and panel data. The cross-sectional effects noted by Honig [1973, 1974, 1976] and Ross and Sawhill [1975] are generally not supported by Cutright and Scanzoni [1973] and Minarik and Goldfarb's [1976] findings, which are also based on cross-sectional data. Both positive [Sawhill et al., 1975] and negative [Hoffman and Holmes, 1976] results have also been reported from the Michigan Panel Study of Income Dynamics (MPSID). Finally, marital stability results are available from each of the income maintenance experiments (New Jersey, Rural, Gary, and Seattle-Denver). Except for Gary, the measured rates of marital dissolution are larger in the experimental groups than in the control group. However, these associations (including Gary) are statistically insignificant for each of the experiments except Seattle-Denver.

Honig (1974) uncovers a positive and significant relationship between AFDC and female headship. She estimated her two equation model on 1960 Census data for 44 SMSAs by regressing: (1) the AFDC caseload and (2) the proportion of families headed by women with children less than 18 upon mean AFDC payments, female wage and unemployment rates, and male wage rates in the SMSA. She found that a 10% increase in average AFDC payments resulted in a 3% - 4% increase in the proportion of families headed by women. A slightly smaller effect was noted when replicated using 1970 data, although significant for nonwhites only.

Ross and Sawhill conduct a similar analysis using 1970 Census Employment Survey data from 41 cities but report finding that AFDC had no effect on

white family structure, but did have an effect on nonwhites resembling those reported by Honig. Durbin [1973] also provides estimates of the relationship between the proportion of female-headed households and ADC--the predecessor to AFDC, using 1960 Census health area data for New York City, but her results proved ambiguous. Her empirical model employs (among other independent variables) the rejection rate of ADC applicants in different welfare centers to indicate the likelihood of being accepted for ADC, which varied under alternative specifications of control variables. Significantly negative results were noted if nonwhites were included and positive, though insignificant effects occurred when the proportion of in-migrants was employed instead.

Counter balancing these results are Cutright and Scanzoni's [1973] failure to find a relationship between state AFDC benefit levels for 1950, 1960, and 1970 and the proportion of married women. They grouped states by the change in the percentage of husband-wife families with children. States with the largest benefit increases had a decline of 2.2%, while states with the greatest benefit losses showed a decline of 2.7%. They concluded that evidence does not support the view that AFDC program benefits affect the formation of white female-headed families. Minarik and Goldfarb arrived at a similar conclusion after failing to detect any AFDC effects on family structure. They estimate a model similar to Honig's with 1970 statewide data, except that race was not included. However, none of their measures of AFDC parameters--the maximum monthly family income permitted to be eligible for AFDC, a dummy variable indicating the presence of an AFDC-U program for the state, and a continuous variable indicating the administrative restrictiveness of eligibility rules in various states--were significant predictors of the proportion of female-headed families with children in the state.

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The methodological and sample differences between these studies preclude treating any one set of findings as being more definitive. For one, other factors associated with stocks of female-headed families are not statistically controlled for in many of these studies--first marriage and remarriage, migration, mortality, illegitimacy, and emancipation, which may result in certain biases. These findings are also based upon cross-sectional data which is not entirely appropriate for inferring causality from AFDC participation to family dissolution. It is quite possible that the direction of causality runs in the opposite direction. Limited evidence of this possibility is provided by Moffitt [1976] who notes a positive relationship between Gary AFDC benefit levels during 1952 to 1969 and the local unemployment level and the relative importance of low-wage jobs in the local economy. He suggests this may point to a responsiveness upon the part of local welfare administrators to variations in the level of need of potential welfare recipients. Finally, many of these studies suffer from problems of endogeneity.

The effect of welfare upon family stability has also been studied using longitudinal panel data, which is more appropriate for addressing the relationships being examined. These studies provide more precise estimates of the effect of AFDC on marital dissolution, i.e., the dependent variable is the probability that an intact family will split over some period of time, rather than having to indirectly infer a relationship through headship rates of female family heads. The existing findings are inconsistent, however, providing little comfort for either rejecting or accepting the hypothesis that welfare is a cause of family instability.

Cherlin [1977] estimated a multiple regression model of marital dissolution on the National Longitudinal Surveys of Mature Women to test for

the influence of welfare payments. His sample consisted of white non-farm women who were intact in 1967; the dependent variable was coded to indicate whether a dissolution had occurred by 1971. To test for possible welfare effects on the probability of dissolution, an average monthly regional AFDC payment was computed. The AFDC variable had no detectable effect, however, and also proved to be insignificant when interacted with the expected wages of the wife.

Sawhill et al.'s [1975] findings from the first five years of data from the Michigan Panel Study of Income Dynamics (MPSID) also revealed no relationship between the availability of higher welfare benefit levels and family stability. They regressed a four-year probability of dissolution (constructed for years 2 - 5 for families with heads under 54 years of age) upon the potential annualized state welfare payments (among other variables) a wife might receive if she were to become single. This measure was constructed from data obtained from year one of the file by multiplying the average AFDC income per recipient in the state of residence in 1968 by 12 and by the number of children plus the mother in the family. Separate models were estimated for the complete sample and various subsamples: white and nonwhite, white poor (including the near poor) and non-poor and nonwhite poor or near poor, but in no case did their AFDC term approach statistical significance. An unexpected negative sign was even indicated in regressions including nonwhites only. Wolf [1977:26] suggests that these results--the unexpected negative sign and insignificant coefficients, may be attributable to the measure of potential AFDC they employ--a conclusion that would also apply to Cherlin's findings. For one, it "does not distinguish between women who are likely to qualify for and receive AFDC payments after a marital dissolution and those who are not. The estimated

coefficients on the AFDC variable actually used may be a weighted average of a strong effect among women for whom AFDC matters, and a zero effect among women for whom AFDC is irrelevant."

Hoffman and Holmes [1976], on the other hand, found evidence of a positive relationship between high state welfare payments and marital stability. Using seven years of PSID data--the dependent variable is defined for years 3 - 7, they employ a simultaneous-equations model, which interacts the "demand" for the marital arrangement on the part of women with the willingness of men to "supply" the arrangement. To parameterize inter-state differences in AFDC benefits, states of residence were categorized by whether "high" or "low" AFDC benefits are provided, which is interacted with another dummy variable indicating a high or low 1968 family income to needs ratio. The sum of the absolute values for families in high payment AFDC states and low income-needs (expressed as deviations from the grand mean of marital dissolution) and low AFDC and low income-needs indicated a significantly negative net relationship between AFDC and family stability, given a low income-needs ratio. These are reduced form results, so they can be regarded as opposite to the findings reported by Sawhill et al. Notwithstanding, the indicated effects are somewhat surprising, since the samples employed in both studies are very similar with respect to sample size, period for which the family was intact and dissolution was measured, the mean of the dependent variable, and the proportions coded as having a low income-needs ratio and poor or near poor.

Evidence from the New Jersey and Pennsylvania income maintenance experiments have been reported in Sawhill et al. [1975] and Wolf [1976]. There have also been results reported for the North Carolina and Iowa Rural experiments [Middleton and Hass, 1977], the Seattle and Denver Income Maintenance



experiments [Hannan et al., 1978] and the Gary Income Maintenance Experiments [Wolf, 1977]. These studies differ both analytically and methodologically, making it difficult to draw conclusions across them, but the rate of marital dissolution for experimentals is consistently higher for controls, in each analysis.

The models estimated by Sawhill et al. and Wolf differ but provide, none-the-less, consistent findings of a higher rate of dissolution for experimentals than controls. It is quite possible, however, that the results from both studies are partially due to higher rates of attrition for families experiencing a dissolution and not the experiment itself. Sawhill et al. focus on the effect of income support payments on the probability of dissolution and include a variety of income and earnings variables in their single equation marital dissolution function. Alternative specifications of various sources of income: total income, total income less NIT payments, all other income, and a constant set of demographic variables: age and education of husband, number of children less than 18 in the family, race, and whether the family owned their own home, were regressed on the probability of a divorce over the three years of the experiment. The income variables were averaged over the quarterly periods the family remained intact. In every case, the various income sources were found to be inversely related and significantly so (in practically every instance) to the probability of a dissolution. A measure of the independence income a wife would receive in the event of a dissolution was also tested, but proved insignificant (although positive in sign). Thus, Sawhill et al. conclude that a negative income tax program would result in increased marital stability, even though the rate of dissolution was higher for experimentals than for controls.

Wolf's analysis of the New Jersey data is directed more at uncovering differences between experimentals and controls. Variables measuring experimental status, the guarantee, and the implicit tax rate were tested in a reduced form dissolution equation. The full simultaneous model includes equations for husband's hours of work, wife's hours of work, NIT benefits, and the probability of a marital dissolution. A positive, although statistically insignificant experimental effect was noted in two-thirds of the cases--eight distinct combinations of the guarantee and the implicit tax rate were tested on three separate subgroups.

Middleton and Hass [1977] also report finding a slightly higher adjusted mean rate of marital dissolution for experimentals than controls. However, this difference was not statistically significant. They strongly qualify their findings in light of their small sample and the relatively few dissolutions observed, but conclude that they could find no evidence, positive or negative, of a relationship between income maintenance and marital stability.

The most extensive analysis of the relationship between income maintenance and marital dissolution was conducted by Hannan et al. [1978], who find that income maintenance has a robust and significantly positive effect upon marital dissolution. Controlling for the assignment model and other theoretically important variables measured at enrollment--duration of marriage, ages and educational levels of the husband and wife, numbers and ages of children, and the like, a 61% and 58% percentage increase in the dissolution rate due to the NIT was noted for blacks and whites, respectively. The effect for Chicanos was essentially zero; a 4% reduction in dissolution was observed among experimental families.

Similar findings were noted when the actual guarantees were tested. The pattern of dissolution was lowest for families assigned to the most generous NIT treatments, however. Hannan et al. offer that this finding is due to the fact that the "income" and "independence" effects of the treatment are nonlinear and that different effects dominate over different ranges and combinations of the guarantee and tax rate: the income effect is a reverse S shape, which results in smaller effects the higher the family income. The independence effect, on the other hand, has an S shape. This results in higher rates of dissolution, the higher the level of income. It is also offered that part of the indicated effect is due to the fact that welfare income is discounted (due to stigma or lack of information) relative to negative income tax payments.

Finally, Wolf [1977] finds no differences, between the rates of dissolution among experimentals and controls in the Gary Income Maintenance Experiment. A significantly positive effect of predicted AFDC income on marital dissolution, controlling for predicted earnings and/or labor supply was noted, however. This effect was also noted in a replication of the model using the Michigan Panel of Income Dynamics. It was not possible to test for the relative effects of NIT, however, because measures of NIT payments were not available.

Senator MOYNIHAN. Who testified on that? Wiseman from California, Professor Wiseman, an economist at Berkeley.

Mr. AARON. A good friend.

Senator MOYNIHAN. You fellows know each other. He gave some very careful work. He had been working in Alameda County and had a very bearish—is that the word?—report. He had obviously done some very careful 5-year panel study.

Mr. AARON. He is a very careful analyst and he has taken a very close look at a very small body of data. I would point out that the behavior he observed in Alameda, in terms of the continuation of cases, differs from behavior in other places, in New York City for example. I gather you also had testimony about that from Mr. Lyon of the Rand Corporation.

Senator MOYNIHAN. Yes.

Mr. AARON. While agreeing with you fully that our primary objective is to deal with problems of need and want among children, I would come back to the point that it is important to offer opportunities to two-parent families for the sake of the children as well as the father. That is why I think it is important to focus attention on the work strategy at the same time we try to repair shortcomings in cash assistance.

Senator MOYNIHAN. Right.

I will press you just another moment. It is possible to describe—we got into this sort of silly incremental versus comprehensive debate—but you know, without doing anything at all to change the system, we could put in a national minimum. That is no change in the system and there is no great cost involved.

Mr. AARON. That is correct.

Senator MOYNIHAN. But if you had the opportunity just to put in the Seattle-Denver income maintenance arrangements without the work requirements, you would not do that?

Mr. AARON. I do not see that it is possible to put in the Seattle-Denver arrangements—

Senator MOYNIHAN. It is possible.

Mr. AARON [continuing]. Because of the very peculiar experimental environment within which they took place. Nobody is discussing that kind of a program at the present time.

Senator MOYNIHAN. But it is possible. We could pass a bill.

Mr. AARON. Of course. I overstated. I meant to suggest that it is unlikely that one would run a real program paying the same degree of attention to information that social scientists did. There are a host of differences between that and welfare administrators.

Senator MOYNIHAN. Then my meaning was not clear. Just a straight negative tax.

Mr. AARON. We are not proposing such a program.

Senator MOYNIHAN. I know you are not. I am saying you would not?

Mr. AARON. At this time, no sir.

Senator MOYNIHAN. It would be your judgment from what you have learned that, by itself, it would not be a good thing?

Mr. AARON. I would not want to say that. I would want to say that it is our judgment that a mixed strategy that was embodied in our proposal and in most of the incremental alternatives is clearly superior.

Senator MOYNIHAN. Well, God love you and we are sorry you are leaving.

Mr. AARON. Thank you.

Senator MOYNIHAN. You are not going far. I know the Secretary will have the benefit of your advice and that this committee will and we want to say the public never says its thanks very well, but this subcommittee of the Committee on Finance would like to say its thanks to Henry Aaron who has been an energetic and faithful, productive public servant in his 2 years in office and we look forward to reading your book about it.

[The prepared statement of Mr. Aaron follows:]

STATEMENT BY HENRY AARON, ASSISTANT SECRETARY FOR PLANNING AND EVALUATION, DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE

Mr. Chairman, I am Henry Aaron, Assistant Secretary for Planning and Evaluation. Accompanying me this morning is Michael Barth, Deputy Assistant Secretary for Income Security Policy. We welcome this opportunity to discuss with you the broad program of income maintenance experimentation begun by the Office of Economic Opportunity in the mid-1960s and continued by the Department of Health, Education, and Welfare.

The government has supported income maintenance research for over a decade. This support reflects a bipartisan commitment spanning four administrations to provide improved information on which to base decisions about changes in the welfare system.

The Seattle/Denver experiments taught us to propose a plan different from the one tested there. The Seattle/Denver plan reduced work effort. The Administration's welfare reform plan, and all major incremental alternatives advanced in the Congress, combined jobs and cash assistance and increased work effort. Work opportunities must be included in welfare reform—that is the lesson of Seattle/Denver.

The experiments were sophisticated applications of social science, and the results, in some instances, are complex. As I shall indicate, some of these results have already influenced policy. Others, we think, should influence future research.

In my testimony I shall sketch the major findings of the experiments. I shall indicate how the results on work effort affected the development of the Administration's welfare reform proposal and were used in preparing cost estimates of that and other plans. I shall indicate a number of puzzling features of the findings on family stability that have, I believe, raised more questions than they have answered. I shall also sketch some of the other results of the income maintenance experiments.

BACKGROUND OF THE INCOME MAINTENANCE EXPERIMENTS

The Office of Economic Opportunity initiated income maintenance experimentation in 1966. The primary goal of the experiments was to obtain information that would make it possible to estimate the cost of various income maintenance alternatives, in particular information on whether, and by how much, recipients of income maintenance payments would reduce their work effort—a major determinant of the cost of any welfare program. Controlled social experiments offered the greatest promise of generating the reliable data needed to estimate the cost of changes in welfare programs.

That promise has been fulfilled.

The findings on work effort, particularly those from the Seattle/Denver experiment led the Administration to propose a welfare reform plan that integrates jobs and reform of cash assistance, unlike the simple negative income tax used in the experiment.

Data from the last of the four income maintenance experiments—those in Seattle and Denver—have provided the empirical foundation for simulations by the Administration, the Congressional Budget Office, and others, of the costs of the Administration's welfare reform plan and most of the major alternative plans.

The experiments, like most large scale research, have produced new accounting and budgeting procedures that promise to save hundreds of millions of dollars in existing welfare programs.

The basic structure of each experiment was similar. Designed to measure changes in work effort associated with variations in the amount and conditions of income maintenance payments, the experiments tried to isolate the effects of income maintenance from other factors that might affect families' responses. The behavior of families who received the payments (referred to as "experimental families") was compared with the behavior of other families, similar in income, geographic location and racial/ethnic background who did not receive income maintenance payments (called "control families"). The differences in behavior patterns between these two groups represent the "response" of the families to the experimental payments.

In each experiment, a predetermined number of families who met the requirements for enrollment were randomly selected and assigned to the experimental or control groups. The experimental groups received income maintenance payments under one of several plans characterized by a different basic benefit level and benefit reduction rate. A basic benefit is the amount of assistance provided if a family has no outside resources. A benefit reduction rate is the rate by which benefits are reduced per dollar of family earnings.

Families in the control groups received no such payments, but remained eligible for welfare and Food Stamps.

Families eligible for payments under the experimental plans reported their income every month and were interviewed about other matters several times a year. Families in the control groups received a token payment in return for information supplied to researchers several times a year.

The problem of assembling representative control groups and the high cost of administering a national program influenced planners to decide on a "test bore" procedure, under which a series of experiments was run in representative parts of the United States rather than a single experiment using a large national sample. This procedure required development of several experiments that, taken together, would represent the variety of geographic, ethnic, and other sociological characteristics that exist in this country.

Each of the four experiments concentrated on one population segment. The New Jersey experiment was limited to two-parent families of the working poor living in industrial urban centers in New Jersey and Pennsylvania. The Rural Experiment concentrated on wage-earning and farming families living in geographically, culturally, and racially distinct rural areas in North Carolina and Iowa. The Gary Experiment focused on black urban families whose members included either one or two parents and at least one child. And the Seattle/Denver experiment, the largest and longest lasting of all, covered one- and two-parent black, white, and Hispanic-American families.

There already exists voluminous literature describing and summarizing the four income maintenance experiments.<sup>1</sup> The table below compares the four experiments in a variety of ways.

<sup>1</sup> This literature includes a DHEW summary report on each experiment (Summary Report: "New Jersey Graduated Work Incentive Experiment", DHEW, December 1973; Summary Report: "Rural Income Maintenance Experiment", DHEW, November 1976; "The Gary Income Maintenance Experiment: Summary of Initial Findings", Kenneth Kehrer, March 1977; and "The Seattle-Denver Income Maintenance Experiment: Mid-Experimental Labor Supply Results and a Generalization to the National Population", DHEW, February 1978.). Numerous unpublished reports are listed in the summary reports, and a considerable number of articles have been published in various professional journals. In addition, the Academic Press (New York, New York) has published a three-volume series on the New Jersey Income Maintenance Experiment (Volume I: "Operations, Surveys, and Administration", by David Kershaw and Jerilyn Fair, 1976; Volume II: "Labor Supply Responses", edited by Harold W. Watts and Albert Rees, 1977; and Volume III: "Expenditures, Health, and Social Behavior: and the Quality of the Evidence", edited by Harold W. Watts and Albert Rees, 1977). Technical papers on the Rural Income Maintenance Experiment are found in the Final Report of the Rural Income Maintenance Experiment, available at reproduction cost from the Institute for Research on Poverty, University of Wisconsin, Madison, Wisconsin. Technical papers on the Gary Income Maintenance Experiment are found in the Initial Findings Report, available from the Office of Income Security Policy Research, Office of the Assistant Secretary for Planning and Evaluation, Washington, D.C. Several volumes of papers that contain initial findings from the Seattle-Denver Income Maintenance Experiment are presently being edited for publication.

For critical evaluations of the New Jersey and Rural Experiments, see Joseph Pechman and Michael Timpane, ed. "Work Incentives and Income Guarantees: The New Jersey Negative Income Tax Experiment", Brookings, Washington, D.C., 1975, and John Palmer and Joseph Pechman, ed., "Welfare in Rural Areas: The North Carolina-Iowa Income Maintenance Experiment", Brookings, Washington, D.C., 1978.

## INCOME MAINTENANCE EXPERIMENTS

[Duration, location, sample composition, and treatments tested]

	New Jersey/ Pennsylvania	Rural	Gary	Seattle/Denver
Start/end, date of permits.	August 1968 to August 1972.	1969-73.....	January 1971 to August 1974.	January 1971 to present.
Sample size (families).	1,357.....	809.....	1,780.....	4,800.
Sites (percent of total sample).	Tranton, Paterson, N.J., (12 percent); Passaic, N.J., (36 percent); Jersey City, N.J., (29 percent); Scranton, Pa. (23 percent).	Rural Iowa (38 percent cent); rural North Carolina (62 percent).	Gary, Ind. (100 percent).	Seattle, Wash. (43 percent); Denver, Colo. (57 percent).
Racial composition of sample (percent).	Black (37 percent); white (32 percent); Hispanic-American (31 percent).	Black (35 percent); White (65 percent).	Black (100 percent).....	Black (43 percent); white (39 percent); Hispanic-American (18 percent).
Types of families enrolled (percent).	2-parent (head of household aged 18 to 58) (100 percent).	Prime aged (18 to 58) male-headed family (73 percent); prime aged (18 to 58) female-headed family (13 percent); family headed by male or female aged 59 or older (14 percent).	2-parent (41 percent); single-parent (59 percent) (head of household aged 18 to 58).	2-parent (61 percent); single-parent (39 percent) (head of household aged 18 to 58).

[In percent]

	New Jersey/ Pennsylvania		Rural		Gary		Seattle/Denver		Rate of decline <sup>1</sup> per 1,000 in earnings
	Basic benefit percent of poverty line	Tax or benefit reduction rate	Basic benefit percent of poverty line	Benefit reduction rate	Basic benefit percent of poverty line	Benefit reduction rate	Basic benefit percent of poverty line	Benefit reduction rate	
NIT plans tested...	50	30	50	50	90	70	90	80	0.25
	50	50	75	30	116	70	116	80	.25
	75	30	75	30	116	70	116	80	.25
	75	50	75	50	116	70	116	80	.25
	75	70	75	70	77	40	116	80	.25
	100	50			77	60	135	80	.25
	100	70	100	50	101	40	90	50	
	125	50			101	60	90	70	
							116	50	
							116	70	
							135	70	
							135	70	
Family eligibility for payments.	3 yr.....	3 yr.....	3 yr.....	3 yr.....	3 yr.....	3 yr.....	3 yr.....	3 yr.....	71 percent
							5 yr.....	25 percent	
							20 yr.....	4 percent	
Other experimental treatments offered.	None.....	None.....	(1) Day care subsidies offered to selected sample of NIT-eligible and control families; subsidy rates of 35, 60, 80, and 100 percent were offered to 43 percent of G-X families.				Manpower counseling; counseling/50 percent subsidy of educational expenditures; counseling/100 percent subsidy of educational expenditures.		
			(2) Social service "access workers," (referral officers).				These treatments were offered to 59 percent of NIT-eligible and control families enrolled in SIME/DIME.		

<sup>1</sup> Families assigned to a declining tax experimental plan faced average benefit reduction rates which declined as their level of earnings rose. In each of these plans the average tax declined 2.5 percent as earnings rose by an additional \$1,000 per year.

## THE FINDINGS OF THE EXPERIMENTS

*Effect of Income Maintenance on Work Effort*

The experiments yielded roughly consistent results on the primary question of whether recipients of cash assistance would reduce work effort and, if so, by how much. All of the experiments provided guaranteed income maintenance payments, but did not impose work requirements or provide special employment opportunities.

To summarize these results:

Male heads of two-parent families reduced hours of work between one and 11 percent; few of them withdrew entirely from the labor force;

Female family heads reduced hours by about 15 percent; some withdrew from the labor force or did not enter at all;

Women in two-parent families, who worked between 3.4 and 16.4 hours per week on the average, reduced the hours they worked between zero and 31 percent;

Young adults (aged 16 to 21) who did not head families reduced their work effort between 18 and 24 percent.

A simulation of the cost and numbers of recipients of the alternative plans, using the work-effort response data from Seattle/Denver, shows that it is the combination of basic benefit level and benefit reduction rate that determines program costs, the number of families eligible for payments, and the fraction of total cost that can be attributed to changes in work effort. That is, as work hours are reduced, income falls, in turn causing assistance payments to rise. For example, a hypothetical program whose basic benefit level was set at 75 percent of the poverty line and whose benefit reduction rate was set at 70 percent would assist about 5 million families and cost about \$3.1 billion in current dollars, more than current welfare programs. By contrast, a more generous program with a basic benefit equal to the poverty line and a benefit reduction rate of 50 percent would aid about 18 million families and cost about \$36 billion in current dollars.

However, the cost of decreases in work effort would be proportionately higher (though absolutely lower) in the less expensive program than in the more generous program. In the former, reduced work effort would account for 58 percent of the cost; in the latter, 17 percent. There are two reasons for this: First, the higher benefit reduction rate of the less generous program would cause a larger decline in work effort than the lower reduction rate would cause in the more generous program. Second, the less generous program would usually cover lower-income people who work less to begin with. A decrease in their work effort would be proportionately greater than a similar decrease in work effort of a group that initially worked more.

*Effects of Limited Duration of Experiments*

From the beginning, analysts suspected that the limited duration of the experiments would generate results different from those of a permanent program. Theoretical studies suggested that an experiment of limited duration would overestimate the impact on work effort of the benefit reduction rate, and would underestimate the impact of the basic benefit level.

Enrollment of families for varying lengths of time in the Seattle/Denver experiment permitted a test of this prediction. Evidence suggests that a short-run experiment is likely to understate the permanent effects on work effort of a relatively generous income maintenance program, but that this understatement probably diminishes and may be reversed as the program becomes less generous. Thus, the size and nature of the error produced by using three-year results, for example, would depend on the terms of the precise income maintenance plan being studied. In fact, simulations of the cash assistance component of the Administration's welfare reform plan, using both the three-year and five-year results from the Seattle/Denver experiment, differ negligibly.

*Income Reporting*

Experience with all the experiments confirms that it is feasible to collect information from recipients, on a monthly basis, about actual income, program participation, and family composition.

Results from the New Jersey experiment suggested that periodic retrospective reporting results in more accurate and more responsive payments than the system of prospective budgeting and infrequent reporting typically used in current welfare programs. As a result, we initiated a demonstration of these adminis-



trative practices in Boulder and Denver, Colorado. Results from these tests suggested that a combination of monthly reporting, retrospective budgeting, and automated data systems could significantly reduce erroneous payments. If implementation of these procedures in States not now using them yielded similar returns, national savings could range from \$280 million to \$560 million per year. A broader testing program of monthly reporting and retrospective budgeting is now under way. As a result of these findings, the Department recently issued a Notice of Proposed Rule Making that would explicitly authorize States to use retrospective budgeting in their regular AFDC systems. This proposed policy change is a prime demonstration of how research results can be used to improve the efficiency with which scarce tax dollars are distributed to needy people.

The income maintenance experiments had as their principal purpose the generation of data that could be used to measure the effects of income maintenance on work effort. While the experiments were not designed primarily to study such other questions as family stability, school performance, manpower development and training, child care, and geographic mobility, some interesting data on these issues were generated.

**RATIO OF EXPERIMENTAL TO CONTROL GROUP FAMILY INSTABILITY RATES, ACROSS EXPERIMENTS AND EXPERIMENTAL GROUPS**

[Sample size used in analysis is in parentheses]

	New Jersey, 3-yr <sup>1</sup> results	Rural, 3-yr results	Gary, 3-yr results	Seattle-Denver		
				2-yr results	3-yr results	3d yr only
Whites.....	1.09 (246)	2.49 (408)	NA	1.85 (1,480)	1.44 (1,561)	0.89 (1,655)
Blacks.....	1.24 (189)	1.58 (208)	0.78 (643)	1.48 (1,070)	1.45 (1,123)	1.44 (1,171)
Hispanics.....	2.22 (153)	NA	NA	1.12 (617)	1.06 (646)	1.09 (672)
Total.....		1.5 (616)				

<sup>1</sup> Family stability ratio for plan with basic benefit of 75 percent of poverty line and benefit reduction rate of 50 percent.

<sup>2</sup> Family instability rates too small for meaningful interpretation: over the life of experiment there were a total of only 32 family splits.

<sup>3</sup> Statistically significant.

### Family Stability

Each of the four experiments collected data on the stability of families in both the control and experimental groups. Families in the experiment are defined to include couples living together whether or not they are legally married. As the attached table makes clear, the findings have been far from consistent. In some experiments, families in some ethnic or racial groups that received cash assistance were less stable than families in control groups. Sometimes in the same experiments, for different benefit plans, and for other ethnic or racial groups, families receiving cash assistance were more stable than the others.

For example, the New Jersey experiment produced data that suggest decreases in stability among families receiving payments from the experiment, but none of the results is statistically significant.

So few families in the Rural experiment broke up that meaningful analysis is not possible.

The Gary experiment yielded data suggesting that family stability is greater among families who received cash assistance than among those that did not, but, again, the results are statistically insignificant.

During the first two years of the Seattle/Denver experiment, rates of family instability were about 60% higher among blacks and whites who received assistance than among those who did not. These results are statistically significant. But there was no significant effect on Hispanics, overall. However, during the third year of the experiment, the effect on white families declined, causing white families in the experimental group to be actually more stable than their counterparts in control groups, although by a statistically insignificant amount. I shall return to this issue at some length later in my testimony.

## OTHER FINDINGS

*Demand for social services.*—In the one experiment that measured demand for social services (Gary), families receiving payments reduced their use of social services by about nine percent and freed resources spent on them for other uses.

*Health and nutrition.*—Two of the experiments (Gary and Rural) provide some evidence about the impact of income maintenance on health. The birth weight of babies born to mothers receiving income maintenance in Gary was significantly higher than the weight of babies born to mothers in the control groups. Improvement of birth weight, a widely accepted index of infant health, was particularly noticeable among women whose babies would otherwise have been at higher-than-average risk.

North Carolina families who received income maintenance in the Rural experiment improved the quality of their nutrition. This improvement occurred in a population whose previous diet was highly deficient. A similar experimental effect was not found in Iowa, but pre-experimental nutrition was much better among rural families in that State.

*Housing and home ownership.*—Families receiving income maintenance in Gary were twice as likely to purchase their own homes during the experiment as families in control groups. An increased amount of homeownership was also noted in the two other experiments where this behavior was studied. The three urban experiments also revealed that families receiving the experimental payments were more likely than families in control groups to move out of public housing.

*School attendance and performance.*—Children, in grades 2-8 in families receiving income maintenance in the rural North Carolina experiment, improved school attendance, scholastic grades, classroom behavior, and their scores on standardized tests.

In all of the experiments, when their families received payments, teenage children appeared to reduce the hours they worked substantially. In several cases (N.J., Gary males) these reductions were more than offset by increases in time spent in school. In other instances, however, an increase in teenage school attendance did not appear to occur (Rural, Seattle/Denver). Female teenagers in Gary neither increased the amount of school attendance nor decreased their participation in the labor force.

*Consumption.*—Families in all experiments used increases in income, both from cash assistance and earnings, to purchase durable goods and necessities and to save.

*Attitudes toward AFDC and income maintenance.*—In the Gary experiment, former AFDC families reported more favorably about their experience with the experiment than about their experience with the operation of the AFDC program. Almost half thought that AFDC rules were too difficult to understand; only 16% said that about the Gary experiment. Three out of four believed that AFDC rules were not equitably enforced. A little more than one out of four reported that about the experiment.

*Munpower training and education.*—Family members in Seattle and Denver increased the time they spent in training and education programs when these activities were partly or fully subsidized, as they were for a group of participants. On the other hand, provision of counselling, without subsidies, produced no change in time spent on these programs.

Increases in education and training attributable to subsidies, however, produced no measurable increase in wage rates. For the first four years after the subsidy program was implemented, wage rates remained substantially equal for those who were eligible for subsidies and those who were not.

## INTERPRETATIONS AND IMPLICATIONS

In the remainder of my testimony, I shall explain how the findings on work effort—the principal subject of the research—have been and are being used extensively in policy planning. And I shall discuss the family stability findings yielded by one of the experiments.

*Work effort*

The income maintenance experiments, together with a large body of non-experimental research, have taught us a number of important lessons that bear directly on policy. The experiments confirmed what the public and economists have long suspected—that low-income people respond both to financial incentives to work and to financial incentives not to work. That is, if we increase the financial reward for work, people will be more willing to work and will have larger pre-tax earnings.

Increases in income maintenance payments for all low-income families with children would not cause large reductions in work effort among prime-age men or female family heads, nor would they cause much outright withdrawal from the labor force.

The large percentage reductions in secondary earners' work effort that can be expected, however, suggest that work requirements and job programs aimed solely at primary earners may not fully offset the anticipated overall reduction in family labor supply.

As reward for work declines, work effort declines. Prime-aged males in families receiving cash transfers reduce the hours they work between 8 and 12 percent when the benefit reduction rate is 50% and the basic benefit is between 50 and 100 percent of the poverty level. But when the benefit reduction rate is increased to 70%, hours worked fall by between 15 percent and 20 percent.

The responses to benefit reduction rates teach a simple lesson—keep benefit reduction rates as low as possible. Unfortunately, as you know, Senator, this lesson is difficult to apply because lowering benefit reduction rates increases costs and extends eligibility, thus increasing the number of people affected by reduced incentives to work.

Evidence from the experiments persuaded the Administration not to propose the pure cash assistance program examined in all the experiments, but rather to propose a carefully constructed package. Indeed, this program initiated a novel integration of jobs and cash assistance that had the following major elements:

A restructured cash assistance program that distinguishes between those who are and those who are not expected to work;

A limit on benefit reduction rates for families containing a member expected to work;

Greatly expanded employment and training opportunities for the low-income population;

Work requirements; and

An expanded earned income tax credit to aid low-income working families even further.

Because of its work requirements and direct job creation provisions, the President's program would have increased, not reduced, total hours worked and raised total earnings by an estimated \$5.4 billion. Significantly, the major alternative plans for the welfare program proposed in the last Congress, including that advanced by the New Coalition, all included similar features.

The methods used to analyze the work-effort data generated in the Seattle/Denver experiment embodied accepted theories of labor supply behavior, were roughly consistent across experiments, and were consistent with a variety of findings based on other surveys.

*Family stability*

The results of the experiment on family stability are puzzling. In contrast to those on work effort. The findings on work effort are consistent with a well-developed body of theory, do not differ significantly from nonexperimental analysis, are consistent across experiments, and contain relatively few anomalies. On the other hand, the results on family stability do not grow out of well-developed theory. The specific findings from the Seattle/Denver experiments conflict to some extent with nonexperimental analysis, are not fully consistent with findings from the other experiments, and contain puzzling results. Given its greater sample size, the results from Seattle/Denver probably merit more attention than any of the other experiments. Nonetheless, the diversity of results across experiments and over time precludes generalizations.

To explain the Seattle/Denver findings, the analysts at SRI developed the following rationale: Cash assistance on the terms offered in the experiment produces two effects.

First, assistance received by a family produces an "income effect," i.e., the availability of greater financial resources tends to stabilize two-parent families.

Second, an increase in potential resources (from cash assistance, earnings, or any other source) available to family members who leave the two-parent family setting (i.e., available to single-parent families) creates an "independence effect" which destabilizes families by increasing the financial independence of family members. Thus, increases in welfare or other forms of assistance will, so the explanation goes, decrease family stability.

By themselves, these two forces could not explain the results of the Seattle/Denver experiments. This is because the rate of family breakup increased more in plans with low benefits—benefits most like those paid under AFDC—than in plans with high benefits. A third element, which, in combination with the other two, is necessary for SRI's explanation of the findings, is the possibility that families do not value a dollar of income from welfare as highly as they value a dollar of income from any other source (including an income maintenance experiment) because of the stigma or administrative burdens associated with regular welfare programs. The existence of such a "welfare discount" is supported by the rather considerable differences in the administration of regular welfare programs and the more benign administration of the experiments, a point to which I shall return. If the "welfare discount" exists, it would not by itself affect family stability, but would simply explain why assistance from the experiment's low-support plans could be more valuable to people than the equivalent income available to control families from the conventional welfare system.

These three elements, if they in fact exist, are one possible explanation of the overall findings of the Seattle/Denver experiment. The reasoning would run as follows: families believe that in the event of separation, low-benefit experimental plans would (because of the "welfare discount") offer them greater benefits than those available from regular welfare programs. However, low-benefit plans do not offer sufficiently generous benefits to produce an offsetting, stabilizing "income effect." In this view, the high-benefit plan is less destabilizing than the low-benefit plan because its "income effect" is larger than its "independence effect."

Some analysts have considered a second explanation for the results, the "role-model" hypothesis. According to this explanation, receipt of assistance undermines the traditional role of the man as head of the family and breadwinner, and he ultimately departs from the family. This hypothesis is hard to accept because the rate of family instability was lower among families on high-benefit plans, which should undermine the male's role most completely, than it was among families on low-benefit plans. Furthermore, there is some increase in the rate of family breakup among families eligible to receive, but not in fact receiving, payment, and again, this effect is greater in the low-benefit plans.

#### *Further thoughts on the use of these findings*

The conditions in the experiments were quite unlike any system of income support and assistance that now exists or that Congress is likely to enact. The experiments had:

- No work requirement,
- No program of child support enforcement,
- No caseworker contact or advice,
- No WIN registration, job search or placement activities.

To recognize these differences, however, is not to criticize the research design. The experiments were designed principally to measure the effects of financial incentives to work. To measure those effects, other influences were minimized.

With respect to the effects of income maintenance on family stability, we believe that these and other differences between conditions in the real world and in the experiments tend to raise serious questions about the relevance of the experimental findings to any welfare reform plan likely to be proposed.<sup>2</sup> Thus:

Benefits paid by the Seattle-Denver experiment differ from those under AFDC in one key respect. Fathers (or mothers) in the control group who left

<sup>2</sup> We know of the research that the differences between real world conditions and those associated with the experiments would almost certainly *strengthen* the conclusion drawn from the experiments that income maintenance does not significantly reduce the work effort of primary earners.

their partners and children were ineligible for AFDC benefits, but fathers (or mothers) in the experiment continued to be eligible for benefits (\$1,600 in 1978 dollars). This difference made family splitting more attractive to families in the experiment than it would be to families in the AFDC program.

The administrators of the experiment scrupulously informed families about the terms of assistance, including the fact that each member of an experimental family would remain eligible for assistance if the family split. The Seattle-Denver experiment provided participants complete information about benefits for which they were eligible and the alterations that would occur following changes in the family situation. Such counselling is not characteristic of real-world welfare programs. In fact, the welfare discount, a necessary component of the theory developed by researchers at SRI to rationalize the results, is nothing more than a technical way of saying that getting assistance from an experiment is rather different from getting assistance from AFDC. For these reasons we believe it would be a mistake to apply, uncritically, the experiment's findings on family instability to programs in the real world.

Not all family breakup was marital breakup. A family was defined as a couple, with or without children, that was married or living in a consensual union. We have asked the research contractor, SRI International, to determine how many couples belonged to each category. But at present, we do not have this information, and it may prove hard to get, because the experiment did not ask for documentation of marital status as a condition of participation. Some fraction of the family breakups studied, were, in fact, terminations of consensual unions. In other words, we do not have any firm information at this time about the impact of the experiments on marital stability. Moreover, married families who split under the experiment were not subject to Child Support Enforcement, an important feature of current welfare programs that may act as a deterrent to family instability.

The experiment also generated a number of statistical anomalies that should cause us to proceed carefully in drawing inferences from the results.

In Denver, receipt of assistance did not significantly increase family instability among Hispanics. The percentage change in the dissolution rate of Hispanic families in the experiment was small (12 percent). We are unable to explain why Hispanics behaved so differently from either blacks or whites. The possible explanation of religious differences is undercut by the finding that in New Jersey, Puerto Rican families broke up somewhat more than either black or white families did. In neither case are the results statistically significant.

White and black experimental families separated more often than their counterparts in control groups during the first two years of the experiment. During the third year, the differential in the break-up rate for white families disappeared, but declined only slightly for black families.

The same analytical model that was used to estimate the effects of cash assistance on family stability produced a result that is hard to rationalize. According to this model, the rate at which white and Hispanic families split increased 49 percent and 87 percent respectively, when they were merely offered job counselling. Both increases are statistically significant. Job counselling and manpower subsidies were separate, independently varied features of the experimental plans tested in Seattle and Denver. If Hispanics were offered 100 percent subsidies for training in addition to counselling, the rate of family breakup among experimental families was lower than among controls by statistically insignificant amounts. (Fewer than 50 percent of the families offered job counselling or training subsidies actually used them.) We have found no explanation for these particular results, but they were produced by the same equation that yielded estimates of the impact of cash assistance on family stability.

Estimates of the impact of cash assistance on family stability from all of the experiments tend to be overstated because of a problem known as "attrition bias." The problem is quite simple. Families who received assistance had strong incentives to continue to provide information after a split, because they remained eligible for cash assistance. Control families did not have such incentives; if they split, they could drop out of the experiment without giving administrators a reason and without losing any experimental benefit. This effect is generally acknowledged. It cannot explain the entire difference in the rates at which experimental and control families broke up in the Seattle/Denver experiment, but taking it into account could reduce the differential between experimental and control families by as much as 40 percent.

The experiment offered recipients cash assistance alone. Starting with the President's program, present welfare reform proposals provide for reform of cash assistance and for direct job creation. The findings of two major longitudinal surveys suggest that this difference is critical. Studies based on the Panel Survey on Income Dynamics (sponsored by HEW and collected by the Survey Research Center) and the National Longitudinal Survey (sponsored by the Department of Labor and managed by Ohio State University) support the following statements.

Unemployment of a husband is associated with marital instability.

If a family's economic situation "worsened" in the most recent year, the likelihood of marital breakup increases; similarly, if its economic situation improved, the likelihood of marital breakup declines.

When a husband's earnings are low relative to his expected earnings, the likelihood of marital breakup increases.

The possession of economic assets such as homes and savings is correlated with marital stability.

This evidence suggests that employment is an important influence on marital stability, partly because of its impact on earnings and partly because of the impact of work on attitudes. It tells us that the impacts of cash assistance experiments on family stability may not carry over to programs that create jobs while they reform cash.

#### CONCLUSIONS

I remain convinced that the best way to achieve equity in the welfare system, a goal most people share, lies in the adoption of the general strategy proposed by the Administration last year, and adopted in the major incremental plans offered as alternatives. That strategy includes development of employment and training opportunities and equitable reform of the distribution of cash assistance. The lessons we learned from the experiments about the impact of cash assistance on work effort helped to shape this strategy and will continue to influence welfare policy for years to come.

Thank you.

[Thereupon at 12:10 p.m., the hearing in the above-entitled matter was adjourned.]

[By direction of the chairman the following communications were made a part of the record:]

## RECENT STATE AFDC CASELOAD DYNAMICS\* AND THE IMPLICATIONS FOR WELFARE REFORM AND FORECASTING

(By Barry Bluestone and James Sumrall, Kathleen Sestak, Richard Sheehan, Charles Piper, Mary Stevenson, Steven Venti, Social Welfare Regional Research Institute.)

### INTRODUCTION

The Social Welfare Regional Research Institute (SWRI) was established at Boston College in 1970 to conduct policy research on the relationship between welfare and the economy. The studies of the Institute have been concerned with (1) evolving issues in employment and welfare that concern the development of income maintenance policy (2) programs aimed at moving client populations toward self-support (3) the impact of employment opportunities on the effectiveness of welfare programs (4) the work experience patterns of client populations and (5) the interaction of welfare policies and the employability of recipients.

More recently, under contract to the Social and Rehabilitation Service of H.E.W., SWRI has been investigating the determinants of AFDC caseload and expenditure trends for the period 1959-1974 in each of five jurisdictions. These include New York City, Upstate New York, Georgia, North Carolina, and Washington.<sup>1</sup> By means of a simulation procedure we have been able to use statistical models for the AFDC programs in each of these states to estimate the impact of various economic, political, and administrative factors on caseload size and expenditure levels.

The use of a "systems" approach to modeling AFDC, along with careful measurement of exogenous variables, provides some new insights into public assistance trends and some important policy implications. The results from this research suggest the following broad conclusions:

1. The determinants of caseload growth vary widely between jurisdictions. This reflects basic structural differences in regional economies and substantial variance in program administration. Because of this it is accurate to conclude that there is no "national" AFDC program. Rather, each state has a categorical program named AFDC, but these often serve significantly different populations and respond to significantly different economic, political, and administrative factors.<sup>2</sup>

\*The work upon which this article is based was performed pursuant to Contract SES-500-75-0005 with the Social and Rehabilitation Service, Department of Health, Education, and Welfare.

<sup>1</sup>The State of Michigan was included in the original research plan. However, all of our efforts at generating a useful model for this State were frustrated by inconsistencies and gaps in the historical data series. Therefore, Michigan is not included in this overview of results.

<sup>2</sup>Data from the biennial AFDC Surveys indicate the demographic characteristics of AFDC vary substantially between states. In 1978, only 14.7 percent of the AFDC families in Mississippi lived in SMSA's while in New York and California the percentage was well over ninety. In Colorado only 11.5 percent of the caseload was black, while throughout the South more than three-fourths are non-white. In Michigan only 8.1 percent of AFDC mothers had not completed 8th grade while in most Southern states a third have not. In 1967 only a quarter of the AFDC families had been on relief for a year or less, while in Washington nearly half of all welfare families were short-term. As for work experience, only 7.3 percent of AFDC mothers were employed while receiving assistance in Ohio, while in Missouri a third were employed.

Program characteristics differ as well. In 1973, none of the families on AFDC in Wisconsin had "unmet financial needs" given administratively set standards. Conversely in Missouri and Alabama over 90 percent of all recipients were in families where benefit levels failed to cover estimated financial need. In Texas in 1971 only 2.3 percent of the assistance group was enrolled in WIN while in Washington nearly 40 percent were. In Alabama fewer than two out of five AFDC families were participating in the food stamp program in 1973, while in Ohio more than four out of five participated.

Furthermore there is strong evidence that administrative attitudes toward welfare dependency clearly differ. Characteristically less than a third of all AFDC mothers in Southern states are considered needed at home while in Northern states the proportion is normally greater than half.

For more detail on jurisdictional differences in the AFDC program, see the 1967-1978 AFDC Studies, op. cit.

2. While there is variance between states, it is true that in most jurisdictions the growth in AFDC benefits has had only a modest impact on caseload size. For instance, if cash benefits had never been increased beyond their 1962 levels, the 1974 AFDC caseloads in New York City and the State of Washington would still have been more than 80 percent of their actual recorded levels. Even in North Carolina, Georgia, and Upstate New York, the caseloads would have been no more than 24 percent smaller if cash benefits had remained at the levels which prevailed a decade and a half ago.

3. Cyclical and secular trends in employment conditions have been an important determinant of caseload size in a number of states even though the reported labor force participation rates among female-headed families are low. A full employment economy throughout the 1960's and 1970's could have been responsible for reducing North Carolina's caseload by as much as 46 percent while Washington's caseload would have been 27 percent lower.

The impact of employment conditions on AFDC-UF is even more pronounced. A full employment economy would have left the Upstate New York AFDC-UF caseload with fewer than 900 families, only one-third of the actual number enrolled during 1974. An effect of almost precisely the same magnitude was found in Washington.

4. Beside these economic variables, political and administrative factors were found to play a critical role in explaining welfare trends. The "\$30 and  $\frac{1}{2}$ " income disregards universally increased caseloads, with the effect strongly correlated with the work experience of the welfare population in individual states. "Simplified Eligibility" played a critical role in the Georgia welfare "explosion", but was found to be of negligible importance in New York City and Washington. Most of the caseload trend in New York City could not be explained by the usual economic variables; only after inclusion of the AFDC acceptance rate could the New York City trend be understood. More general political variables, including a specially constructed Congressional voting index, were useful explanatory variables in nearly all the modeled states.

#### *Implications for welfare reform and welfare administration*

Before examining some of the statistical results in greater detail, it is worthwhile to consider a number of the more important policy considerations that we believe are implied by this research. There are obviously many, but we have chosen to focus on just seven.

1. Perhaps the most important implication follows from the finding of wide diversity among the states in terms of the populations served by the present public assistance programs and the substantial jurisdictional differences in the impact of economic, political, and administrative factors on caseloads and expenditure trends. We think this finding strongly suggests that any uniform national welfare reform program can be expected inevitably to have very different effects in the various regions, states, and local jurisdictions of the nation.

The same change in welfare benefits may increase caseloads to a measurable extent in one state while having almost no impact in another. An increase in employment opportunity in one area may serve to drastically reduce welfare caseloads and expenditures, while in another area a similar improvement in economic conditions will have a much smaller effect.

More critical for policy purposes is the fact that a uniform change in federal rules and regulations governing welfare will likely have a non-uniform result. Simplified application procedures, for example, will have a negligible effect in jurisdictions where procedures are already reasonably open, but the same regulations can be expected to have a substantial caseload expansionary impact elsewhere. Changing the income disregard and benefit reduction rate schedule will have a more profound impact in jurisdictions where work and welfare are more commonly used together.

2. Since our results indicate that benefit levels, employment conditions, and political/administrative factors all affect caseload trends in one way or another, it is incumbent that any comprehensive welfare reform program be multi-faceted and address all three kinds of factors. As much or more attention must be spent on developing carefully drawn administrative guidelines as agreeing on equitable and reasonable benefit levels.

Moreover, central to reform efforts should be the consideration of job expansion through either the public or private sector. This involves careful consideration of proper income disregards and benefit reduction schedules, as well as new efforts at job training and placement.



A lack of regard for the complexity of the reform issue will almost surely lead to unwarranted expenditures or grossly inequitable treatment of low-income families.

3. On a more specific level, our findings imply that the provision of an adequate benefit level will not lead to a break-down in the work ethic or a massive shift from the labor market to the welfare system. To a large extent the growth in public assistance caseloads during the late 1960's and early 1970's was due to factors other than rising benefit levels. There is no reason to believe that future boosts in the benefit standard will trigger anything more than modest increases in the size of the national caseload.

4. One of the chief factors upon which future levels will most likely depend is the state of the economy and particularly the level of employment opportunity for lower-skilled workers. This is especially true if a family allowance type program is expanded to cover the working poor. As is, even those with the weakest ties to the labor market—female-headed families—appear to respond to changing employment conditions. The number of families receiving AFDC-UF was found to be highly sensitive to job opportunities. We expect the same thing would be true for the working poor.

What this implies is that a national full employment policy is a critical element in the containment of welfare costs and therefore must be considered like other policy instruments in dealing with issues of welfare reform. The net cost of job creation, for instance, should take into account the savings not only in unemployment compensation funds, but in welfare budgets as well. It is, therefore, altogether proper and indeed necessary to link the debate over welfare reform to the broader debate over domestic economic policy.

5. The same statistical results that indicate employment conditions strongly affect caseload and expenditure levels suggest that voluntary work programs will be sufficient to move a large number of families off welfare if a full employment economy can be generated. "Workfare" or other forms of administratively expensive forced work programs can be avoided if a sufficient number of jobs are available for present and potential welfare clients. Again this is particularly the case for intact (male-headed) families, the group that traditionally has been the focus of such "workfare" legislation.

6. Still another implication of our empirical results concerns administrative equity in the enforcement of bureaucratic regulations. The political decisions of state legislatures and individual state and local welfare offices, as measured by a series of proxy variables, clearly affects the relative availability of AFDC to families in different jurisdictions. Where national standards exist, or there is federal oversight, there is a strong indication of greater fairness in program administration.

"Simplified eligibility", for example, tended to equalize the rights of potential welfare recipients in the various states. This was clear from our empirical results. Similarly we expect that fair hearing regulations increased national equity, although inadequate data on fair hearings prohibited a statistical test of this hypothesis. In all, the simple doctrine of "fairness" seems to imply that stronger federal policies are needed to guide application and acceptance procedures as well as to regulate the termination process. These, in addition to "quality control" measures, will help to minimize inequities in the administration of the public assistance system.

7. Finally there is a policy implication for those responsible to preparing forecasts of future welfare expenditure demands on local, state, and federal budgets. This research strongly suggests that the determinants of public assistance trends vary sharply between jurisdictions. Therefore, initial forecasting should be done at the state (and possibly the local) rather than the national level. Only in this way can we begin to better understand the true underlying dynamics of welfare trends and do a better job of projecting budget needs.

Unfortunately in the past, some state welfare budget forecasts have been so misleading as to create a need for repeated eleventh hour supplementary appropriations. This inevitably has contributed to a political and social climate which increasingly views social welfare expenditures as a sinister plot against the average taxpayer. A better understanding of the causes of welfare growth and better forecasts may serve to clear the political air so that the issue of welfare reform can be discussed rationally and fairly.

#### *Estimating the determinants of AFDC caseload and expenditure trends*

The policy implications suggested in the previous section follow either directly or indirectly from the series of comprehensive AFDC models developed at the

Social Welfare Regional Research Institute since 1975. For each of the five selected jurisdictions, we constructed a multi-equation model which decomposes changes in the size of the caseload into its component parts. Instead of a single caseload equation, separate monthly time series regressions were estimated for: (1) Applications received; (2) Processing rate; (3) Acceptance rate; (4) Closing rate; (5) Expenditures/case. These individual equations were then recombined to yield estimates for the size of the caseload and the value of total expenditures.

The use of such a "components" methodology marks a departure not only from our own previous approach, but also from most prior research.<sup>3</sup> The advantage of the components model is that it allows the researcher a much finer representation of the actual caseload determination process. Consequently AFDC program dynamics can be measured with greater precision than ever before. The empirical results generated from these individual state models turn out to yield new insights about the underlying "causes" of AFDC trends.

In the course of this research, we have been able to test the relevance of three theories about caseload dynamics. One is the "alternative income" hypothesis which suggests that a large part of the growth in caseloads is a voluntary response to rising AFDC benefit levels relative to wage opportunities in the labor market. The "employment opportunity" theory, on the other hand, posits the importance of job availability per se as the key to understanding caseload dynamics. Finally, the "institutional" theory suggests that changes in demographic characteristics, political attitudes, and changes in welfare rules and regulations are the dominant causes of rising assistance rolls.

To test these three hypotheses, a voluminous data set was collected for each of the states in the model. Benefit information, including maximum cash allowances, food stamp bonus value, and an imputed actuarial value for in-kind medical services, was gathered to measure the total value of public assistance to the average-sized recipient family. Measures of potential female spendable earnings in relatively low-skill occupations were also developed. Combined with the benefit data these formed the benefit/wage ratios. Testing the employment opportunity hypothesis relied on information about aggregate unemployment rates, as well as employment levels in various "low-training" non-durable manufacturing industries and "high-turnover" retail trade and service sectors where many welfare recipients are often employed. Over fifty different institutional variables were developed, including those which were used to measure demographic trends, political attitudes, and specific AFDC regulations such as "simplified eligibility", "workfare", WIN, and the "\$30 and 1/2" earned income disregard.

The ultimate analysis of caseload and expenditure dynamics is accomplished through the use of a simulation procedure in which monthly caseload estimates are generated solely from regression equations (1) through (4) above, the exogenous data, and the initial (December 1958) values for cases and applications pending. By adjusting the exogenous data and simulating alternative scenarios, we are able to evaluate a variety of economic and policy changes which occurred over the sample period. For example, if we increase statutory or scheduled benefit levels by a certain amount and leave all other exogenous data at actual levels, the induced change in the caseload is an indirect measure of the overall impact of benefit levels on the caseload.

We have chosen to turn tests using hypothetical data "counterfactuals" and tests based completely on actual data "simulations". The differences between simulated and counterfactual levels of the caseload and total expenditures can be attributed to the variables that take on hypothetical values in the counterfactual.

In Table 1 we have compared the simulated with the actual caseloads and expenditures for the five jurisdictions for fiscal year 1974. The simulations are based on the multi-equation regression models developed for each of these jurisdictions and are created by reconstituting the caseload "identity".<sup>4</sup>

$$\text{Caseload}_t = \text{Caseload}_{t-1} + (\text{Applications}_t * \text{Processing Rate}_t * \text{Acceptance Rate}_t) - (\text{Closing Rate}_t * (\text{Caseload}_{t-1} + \text{Openings}_t))$$

Total expenditures are generated simply by multiplying the estimate of the caseload by the estimate for expenditures per case.

<sup>3</sup> One previous study that utilized this methodology is by Martin Holmer, "The Economic and Political Causes of the 'Welfare Crisis'" (Ph.D. Dissertation, Massachusetts Institute of Technology, 1975).

<sup>4</sup> See the Appendix to this paper for the construction of the caseload identity.

Total Expenditures = Expenditure/Case, \* Caseload.

These econometric models simulate the caseload accurately with errors as low as 1.5 and 1.2 percent in Washington and New York City in FY 1974. In the remaining states the error is slightly larger but not unreasonable.<sup>5</sup>

TABLE 1.—SIMULATED VERSUS ACTUAL CASELOAD AND EXPENDITURES, FISCAL YEAR 1974  
(In thousands)

	Average monthly caseload			Total annual expenditures		
	Actual	Simulated	Percent difference	Actual	Simulated	Percent difference
North Carolina .....	48,680	46,269	-5.0	\$75,522	\$72,708	-3.7
Georgia .....	105,258	109,277	3.8	129,426	134,376	3.8
Upstate New York .....	92,463	95,211	3.0	366,440	316,022	3.1
Washington .....	38,833	38,255	-1.5	106,395	105,329	-1.0
New York City .....	239,475	236,565	-1.2	886,958	879,431	-.8

Total expenditures follow the same pattern in each state and contain errors often even smaller than those found in the caseload estimates. These simulations yield a strong indication of the modeling accuracy provided by the components method.

The simulated values in Table 1 provide a benchmark against which the counterfactuals can be compared. In Tables 2 through 8, we present counterfactuals which illustrate some of our tests of the three welfare hypotheses mentioned earlier.

*The alternative income hypothesis*

To test the impact of the alternative income theory, we generated two counterfactuals. In one we froze the maximum allowable cash benefit at its July 1962 level as though state legislatures had never voted benefit increases. In the second scenario, we set cash benefits at levels 10 percent higher than actual for every month in the analysis period (1960-1974). All other factors in the model, including potential earnings levels, food stamp benefits and Medicaid, were allowed to change as they actually did. In the first case we originally expected to find much lower caseloads in accord with the alternative income theory; in the latter we expected modest caseload increases.

Somewhat surprisingly we found that the substantial cash benefit boosts during the 1960's and 1970's had much less to do with the caseload boom than predicted by the alternative income theory. Table 2 indicates that of the five jurisdictions in the analysis, the largest difference in the caseload due to the low cash benefit scenario is found in Upstate New York. If maximum allowable cash benefits had been kept at \$200 instead of rising to \$319 as they did, the caseload in Upstate New York would have been 2,311 or 24 percent smaller in FY 1974. This is substantial, but obviously still leaves a large portion of the caseload growth unexplained. Even more surprising, holding cash benefits at their lowest levels in Washington (\$164) and New York City (\$220) resulted in caseload declines of only 8.4 percent and 6.5 percent. The welfare "explosion" in these two areas must be explained almost totally by other factors.

TABLE 2.—LOW CASH BENEFITS (JULY 1962 LEVEL) VERSUS SIMULATED CASELOAD AND EXPENDITURE, FISCAL YEAR 1974  
(In thousands)

	Average monthly caseload			Total annual expenditures		
	Simulated	Counter-factual	Percent difference	Simulated	Counter-factual	Percent difference
Upstate New York .....	95,211	72,371	-24.0	\$316,022	\$194,851	-38.3
Georgia .....	109,277	90,293	-17.4	134,376	79,656	-40.7
North Carolina .....	46,269	38,265	-17.3	72,708	39,672	-45.4
Washington .....	38,255	35,049	-8.4	105,329	65,780	-37.6
New York City .....	236,565	221,091	-6.5	879,434	577,206	-34.4

<sup>5</sup> The larger error in the North Carolina model can partially be attributed to poor data and the need to rely on a quarterly model.

Table 3 presents the results for the +10 percent counterfactual. These hypotheticals are somewhat more realistic and can be compared across states whereas the previous example cannot. They provide for an equal proportional change in cash benefits unlike those in Table 2 which vary from state to state depending on how much cash benefits actually increased. The results are similar, but not identical, to those found in the lowest benefit scenario. In Georgia, the caseload increased by 12.8 percent to 123,267 in response to the 10 percent boost in benefits. Total expenditures rose by a substantial 25.7 percent to almost \$169 million on an annual basis. Again changes in benefits made little difference in New York City and Washington.

TABLE 3.—CASH BENEFITS 10 PERCENT HIGHER VERSUS SIMULATED CASELOAD AND EXPENDITURE, FISCAL YEAR 1974

(In thousands)

	Average monthly caseload			Total annual expenditures		
	Simulated	Counter-factual	Percent difference	Simulated	Counter-factual	Percent difference
Georgia.....	109,277	123,267	12.8	\$134,376	\$168,851	25.7
Upstate New York.....	95,211	101,132	6.2	316,022	353,280	11.8
North Carolina.....	46,269	48,156	4.1	72,708	83,514	14.9
New York City.....	236,565	241,970	3.4	879,434	1,004,826	14.3
Washington.....	38,255	39,153	2.4	105,329	117,830	11.9

A major difference between the two benefits scenarios is found in Upstate New York. The first scenario identifies the maximum increase in caseload which can be directly attributed to the growth in benefits, whereas the second scenario illustrates the marginal or incremental change associated with less extreme benefit fluctuations. The caseload responded significantly to holding benefits at the 1962 level, but when we artificially increased allowances by 10 percent, the caseload rose by only 6.2 percent. This suggests that the sizeable sixteen year growth in the caseload was due to substantial boosts in the benefit level itself and not caused by a super-sensitivity to each small increment in the cash amount. In Georgia, on the other hand, where the maximum cash benefit increased by only \$33 during the analysis period, the caseload is highly sensitive to each increment.

Further analysis of this information suggests that the sensitivity to benefit levels is related to the relative value of welfare benefits and potential wages in each state. In Georgia, benefits were very low relative to wages during the early part of the analysis period. A family could normally receive in total benefits less than half the value of what could be earned in the low-skilled labor market. Over time the value of benefits (including food stamps and Medicaid) rose faster than wages and consequently more families found themselves on the work-welfare margin. This led, according to our estimated model, to a considerably larger number of applicants and acceptances and fewer terminations. In New York and Washington benefits were always a higher proportion of potential wages. Increases in the benefit ratio apparently affected the voluntary decisions of families very little. This explains why only a tiny fraction of the growth in caseloads and expenditures is attributed to these factors. In North Carolina, the small counterfactual difference in Table 3 can be explained by the fact that actual benefits declined relative to wages during the analysis period. A hypothetical 10 percent boost in 1974 would therefore also have affected few family decisions because benefits were nowhere near the level required to compensate for lost wages. Only a large change such as that found in the lowest benefit counterfactual can affect the size of the caseload.

The different benefit histories during the period 1969-1974 can therefore tell us something about the growth in AFDC. But for each of these jurisdictions, other explanatory factors must be at least equally as powerful, if not more so, in explaining caseload and expenditure growth. Changing job opportunities appears to be one of these factors.

#### *The employment opportunity hypothesis*

Two tests of the employment opportunity hypothesis are summarized in Tables 4 and 5. In the "recession economy" scenario, the unemployment rate was held at its highest value in the period between July 1962 and December 1974. If either the non-durable manufacturing or retail trade and service employment index

declined from its initial July 1962 value, the index was allowed to take on its actual value in subsequent years. However, if either index actually rose over time, it was held constant at its initial value. In the "growth economy" scenario, the conditions were reversed: the unemployment rate was held constant at its lowest value; if an employment index rose it was allowed to assume its actual value; and if it fell it was assigned its initial July 1962 level.

TABLE 4.—RECESSION ECONOMY VERSUS SIMULATED CASELOAD AND EXPENDITURE, FISCAL YEAR 1974

(in thousands)

	Average monthly caseload			Total annual expenditures		
	Simulated	Counter-factual	Percent difference	Simulated	Counter-factual	Percent difference
Georgia.....	109,277	129,626	18.6	\$134,376	\$170,267	26.7
Upstate New York.....	95,211	106,441	11.8	316,022	353,265	11.8
New York City.....	236,565	272,846	15.3	879,434	1,013,646	15.3
Washington.....	38,255	45,607	19.2	105,329	127,109	20.7
North Carolina.....	46,269	59,771	29.2	72,708	139,362	91.7

TABLE 5.—GROWTH ECONOMY VERSUS SIMULATED CASELOAD AND EXPENDITURE, FISCAL YEAR 1974

(in thousands)

	Average monthly caseload			Total annual expenditures		
	Simulated	Counter-factual	Percent difference	Simulated	Counter-factual	Percent difference
Georgia.....	109,277	97,764	-10.5	\$134,376	\$120,221	-10.5
New York City.....	236,565	212,949	-10.0	879,434	789,007	-11.3
Upstate New York.....	95,211	80,309	-15.7	316,022	261,613	-17.2
Washington.....	38,255	27,803	-27.3	105,329	75,426	-28.3
North Carolina.....	46,269	25,242	-45.5	72,708	37,707	-48.1

In the "recession economy", caseloads and expenditures in all five areas grow substantially, but the rates of growth vary significantly from state to state. The caseload would have been nearly 30 percent greater in North Carolina under these "permanent recession" conditions while only 11.8 percent higher in Upstate New York.<sup>6</sup> These differences reflect an underlying variance in the economies of each region and possibly the extent to which AFDC recipients mix work and welfare.<sup>7</sup> North Carolina's caseload has been extremely sensitive to sharp declines in agriculture while Washington's AFDC rolls reflect the state business cycle caused by severe fluctuations in aerospace spending. New York City's AFDC applications are not particularly affected by employment conditions, but careful inspection of the individual equations in this model indicate that the number of closings responds to both aggregate unemployment rates and to employment levels in the non-durable manufacturing sector including the garment industry.

<sup>6</sup> One should be careful, however, in comparing differences across states in the recession and growth scenarios as they are based on different proportional changes in exogenous variables, i.e., the range in the unemployment rate in Georgia is between 2.6 and 4.9 percent (an 88 percent difference) while in Washington, the unemployment rate varies between 3.1 and 11.9 percent, a 284 percent difference.

<sup>7</sup> The uneven sensitivity between states that we found in these models is supported by evidence in the biennial AFDC characteristics studies. There we find significant variance between states in the trend of the caseload employed. In North Carolina the proportion jumps substantially between survey years, suggesting a possible strong economic relationship. On the other hand, in Georgia and New York, the percentage remains relatively constant, varying little with changing employment opportunity.

	Percent of AFDC mothers employed			
	1967	1969	1971	1973
Georgia.....	26.6	30.2	27.7	30.1
Washington.....	5.7	NA	8.2	NA
New York.....	6.2	7.9	8.6	8.5
North Carolina.....	18.5	NA	11.0	22.9

The growth economy scenario reported in Table 5 suggests a similar picture of uneven caseload and expenditure response to changes in economic conditions. Again North Carolina is most sensitive, with the counterfactual caseload level almost half the stimulated caseload of 46,269. Washington is also sensitive to economic growth with the caseload some 27 percent lower or only 27,803 in FY 1974 if full employment conditions had prevailed.

Given the nature of these counterfactuals, the most accurate portrayal can be found by comparing the difference between the recession and growth scenarios as shown in Table 6 below.

TABLE 6.—RECESSION ECONOMY VERSUS GROWTH ECONOMY CASELOAD AND EXPENDITURE, FISCAL YEAR 1974

(in thousands)

	Average monthly caseload			Total annual expenditures		
	Recession	Growth	Ratio	Recession	Growth	Ratio
New York City.....	272,845	212,949	1.28	\$1,013,546	\$789,007	1.28
Upstate New York.....	106,441	80,309	1.33	353,265	261,613	1.35
Georgia.....	129,626	97,764	1.33	170,267	120,221	1.42
Washington.....	45,607	27,803	1.64	127,109	75,496	1.69
North Carolina.....	59,771	25,242	2.37	46,454	12,569	3.69

Clearly North Carolina and Washington show the greatest response to economic conditions while the caseloads in Georgia, Upstate New York, and New York City indicate approximately the same sensitivity to shifting employment patterns. Expenditures follow a similar pattern, with the "recession-growth" ratios somewhat larger due to the fact that economic conditions affect the average level of benefits per case as well as the caseload level.<sup>a</sup> All of these counterfactuals suggest that the economic opportunity hypothesis is in fact one of the principle factors explaining caseload and expenditure patterns at least in North Carolina and Washington. Indeed, the availability of jobs significantly affects the size of the caseload in every state we investigated. This strongly implies a powerful link between the overall health of the economy and the size of welfare rolls.

#### *The institutional hypothesis*

Beyond the economic variables in these models, we tested a rich array of institutional factors. These varied from general measures of political attitudes to specific changes in actual legislation or program policy. Many of these are unique to each state and are not comparable. However, there are a few important ones which appear in all states and we present a few of these here.

One key program revision involved the "\$30 and 1/2" income disregards. This new policy was originally intended to reduce total welfare expenditures by providing a greater incentive to work. As Table 7 suggests, just the opposite occurred. In each state, the caseload, as well as total expenditures, would have been smaller if the "\$30 and 1/2" program had never been instituted.

TABLE 7.—NO \$30 AND 1/2 VERSUS SIMULATED CASELOAD AND EXPENDITURE, FISCAL YEAR 1974

(in thousands)

	Average monthly caseload			Total annual expenditures		
	Simulated	Counterfactual	Percent difference	Simulated	Counterfactual	Percent difference
Georgia.....	109,277	69,747	-36.2	\$134,376	\$85,763	-36.2
North Carolina.....	46,269	40,133	-13.3	72,706	63,069	-13.3
Upstate New York.....	95,211	85,709	-10.0	316,022	284,494	-10.0
Washington.....	38,255	34,487	-9.8	105,329	102,968	-2.2
New York City.....	236,565	220,924	-6.6	879,434	821,263	-6.6

<sup>a</sup> Note the much larger difference in the "recession/growth" ratio of total expenditures compared to the caseload ratio in North Carolina. Under the recession counterfactual for FY1974 the average benefit per case rises steeply to \$195/month from a simulated actual of \$130. This apparently occurs as many employed AFDC mothers lose their jobs and require maximum cash benefits rather than reduced cash allowances supplemental to earnings. In other state models, the same phenomenon occurs, but it is much less pronounced.

As is now well-known, the income disregards provided a strong incentive for the working poor to apply for welfare.<sup>9</sup> At the same time the earnings exemptions substantially reduced the probability that a family would leave the rolls due to income ineligibility. Our models indicate that the relative impact of the disregards is strongly correlated with the proportion of the caseload working in each state. For instance, in Georgia where almost a third of the caseload had working mothers in 1973, the absence of "\$30 and 1/2" would have reduced the caseload by over 36 percent to 69,747. In New York City where only 6.9 percent of the mothers were employed, "\$30 and 1/2" increased the caseload by only 6.6 percent.

Another policy which we chose to evaluate provides some information about the impact of "liberal" versus "conservative" program administration. In Table 8, we have summarized the counterfactual in which "simplified eligibility" was assumed not to have been initiated. In the absence of simplified eligibility an intake worker could much more effectively follow the letter of the law in interpreting eligibility criteria. After simplified eligibility, the social worker was required to take the client's word at face value if there was no documentary proof to the contrary. Thus, where a state had before been very strict in ascertaining eligibility, one might expect a fairly large increase in the size of the caseload. In a state which had been fairly flexible in interpreting eligibility we might expect only a small increase.

TABLE 8.—NO SIMPLIFIED ELIGIBILITY VERSUS SIMULATED CASELOAD AND EXPENDITURE, FISCAL YEAR 1974

(In thousands)

	Average monthly caseload			Total annual expenditures		
	Simulated	Counter-factual	Percent difference	Simulated	Counter-factual	Percent difference
Georgia.....	109,277	71,838	-34.3	\$134,376	\$88,334	-34.3
Upstate New York.....	95,211	90,512	-4.9	316,022	300,435	-4.9
New York City.....	236,565	234,930	-.7	879,434	873,352	-.7
Washington.....	38,255	38,013	-.6	105,329	104,576	-.6
North Carolina.....	46,269	NA	NA	72,708	NA	NA

#### Notes on the AFDC-UF models

In addition to the five separate models for the "regular" AFDC program, SWRRI also developed similar models for the AFDC-UF segments in Upstate New York and Washington. The counterfactual analysis of these two models indicates that changes in benefit levels have relatively little to do with UF trends while employment conditions are critical.

Keeping cash benefits at the 1962 level would have been responsible for reducing the 1974 Upstate New York UF caseload by only 9.7 percent (only 266 cases) while in Washington the caseload would still have been 77 percent of its actual level.

On the other hand, long-term full employment in Upstate New York would have reduced the caseload by 68 percent with the consequence that total UF expenditures would have fallen to only 30 percent of the actual 1974 level. In Washington the results are almost identical with the caseload down by 67 percent and expenditures down by a similar amount.

#### Empirical conclusions and recommendations for further research

As to a general conclusion reached from this research, we suggest the following: All three theories advanced in our work play some role in explaining AFDC benefits and caseload growth. But, one cannot point to any one of the hypotheses as being the principal cause of growth in all states. Benefit factors may be more important in one state, employment factors in another, and institutional factors in still a third. To try to understand the dynamics of the AFDC program without being aware of the basic structural differences between states invites serious errors in analysis and policy prescription.

<sup>9</sup> For example, see Gary Appel, "Effects of a Financial Incentive on AFDC Employment: Michigan's Experience between July 1969 and July 1970" (Minneapolis: Institute for Interdisciplinary Studies, 1972).

In Georgia we found the caseload has been particularly sensitive to incremental changes in the benefit structure—but the fact remains that benefits have been relatively constant in Georgia over the last 16 years, increasing on a per case basis by only one-third. Thus the growth of the AFDC program in Georgia (second only to Michigan on a recipients/total population basis) can be explained only partly by changes in the benefit structure. The income disregards, however, played an important role.

Payments per case more than doubled in Upstate New York while the recipient/total population ratio quadrupled. Yet the impact of a 10 percent increase in benefits, according to our components model, is a modest 6.2 percent increase in the caseload. Changes in employment opportunities in the Upstate Region have a more significant impact on the caseload, yet they too do not explain the full magnitude of the growth between 1959 and 1974. Institutional factors also explain only modest changes in the caseload over the period of analysis. Thus in Upstate New York we conclude that caseload growth was truly a product of all three factors.

The results in Washington and North Carolina point much more directly to fluctuating employment opportunity as the explanation of caseload trends. The caseload is not particularly sensitive to changes in the benefit structure, nor for that matter, to "\$30 and  $\frac{1}{3}$ ". Of the institutional factors evaluated for Washington, the one that has the largest impact is the WIN program—which is employment oriented. In the "recession economy" scenario, the caseload grows by almost 20 percent, while in the "growth economy" scenario, the caseload falls by more than one-quarter. Thus we conclude that in Washington, one may successfully reduce the AFDC caseload by providing more employment opportunities, but not by manipulating any of the other program parameters. The same can generally be said for North Carolina. In contrast to all of these models, New York City caseloads appear to be a function of institutional variables.

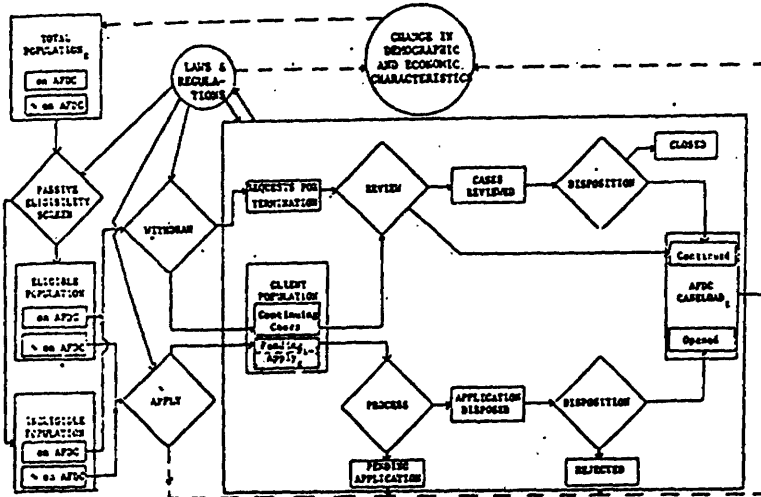
Our analysis, therefore, suggests that a careful state by state evaluation of the AFDC program is necessary. Policy manipulations aimed at reducing the welfare rolls by attacking only one or another of the characteristics of the program may work in some states, but not in others. Increasing benefits might increase program participation in Georgia, but it would do so only marginally in Washington. On the other hand, providing jobs for which AFDC women could qualify might serve to reduce caseloads in all of the states but will work particularly well in states similar to North Carolina and Washington. If one had a simulation model for each of the 50 states, one could conceivably evaluate some of the common sensitivities to benefits, employment, and institutional factors. This would provide a solid foundation on which to base policy changes. The need for further research in this area is now fairly clearly defined.



Appendix

The AFDC caseload and expenditure dynamics project uses a systems approach to econometric modeling to derive estimates for the determinants of public assistance trends. The underlying micro model traces the path by which each family in the general population moves through the "AFDC system" on route to becoming a continuing non-recipient, a new AFDC case, a newly closed case, or a continuing one. Each family is theoretically passed through a number of filters or screens which determine eligibility, the probability of applying for assistance, the probability of having their application processed in a given month, the probability of being accepted, and the probability of closing if already a welfare recipient. Figure 1 provides a schematic of this process generated from a careful review of the AFDC program in each state. A separate micro model was developed to provide a theoretical basis for how each AFDC family's cash allowance is determined.

FIGURE 1.



This schematic approach can then be translated into a simple set of mathematical identities which describe the AFDC system and how the caseload and total expenditures change over time.

$$(1) C_t \equiv C_{t-1} + O_t - CL_t$$

$$(2) O_t \equiv \alpha \beta A_t$$

$$(3) CL_t \equiv \gamma(C_{t-1} + O_t)$$

$$(4) C_t \equiv C_{t-1} + \alpha \beta A_t - \gamma(C_{t-1} + O_t) \\ \equiv (1-\gamma)(C_{t-1} + \alpha \beta A_t)$$

where: C = caseload in period t

O = case openings

CL = case closings

A = AFDC applications<sub>t</sub> + pending applications<sub>t-1</sub>

and where:  $\alpha$  = "Processing Rate" = Applications Processed<sub>t</sub> / (Applications<sub>t</sub> + Pending Applications<sub>t-1</sub>)

$\beta$  = "Acceptance Rate" = Applications Accepted<sub>t</sub> / Applications Processed<sub>t</sub>

$\gamma$  = "Closing Rate" = Case Closings<sub>t</sub> / (C<sub>t-1</sub> + O<sub>t</sub>)

According to this system, changes in the caseload are identified by four variables (A,  $\alpha$ ,  $\beta$ ,  $\gamma$ ). By estimating the determinants of each one, a complete model of AFDC caseload dynamics is constructed.

One further identity is needed to obtain total cash benefit expenditures.

$$(5) X_t \equiv \nabla C_t$$

where:  $\nabla$  = Expenditure/Case

A complete "components" model therefore has a minimum set of five equations\*: one for  $A_t$  and  $V_t$  plus one for each of the rates ( $\alpha$ ,  $\beta$ ,  $\gamma$ ). In the actual estimation of the model we generated OLS and GLS (rho-corrected) regressions for each of these exogenous variables using \*\* monthly time series data for the period January 1959 to December 1974. Once these regression estimates were generated the caseload identity was reconstructed through a simulation program which iteratively solves equations (4) and (5) based on initial values for  $C_{t-1}$  and the number of pending applications in  $t-1$ , the regression coefficients in each equation, and data on the exogenous variables. Counterfactuals can then be generated by simply changing the values for the exogenous data set. By comparing the actual simulation estimates for  $C_t$  and  $X_t$  with counterfactual estimates, we generate evidence for the sensitivity of state AFDC programs to such factors as benefit/wage ratios, employment availability, various program regulations, and a set of political characteristics. The variables in the model are based on a broad range of economic and social science theories including neoclassical labor supply hypotheses, segmented labor market theory, and institutional theory.

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\*In models with separate AFDC-R and AFDC-UF segments, it is necessary to generate two processing rates ( $\alpha_1$  and  $\alpha_2$ ) making a total of six equations. If case closings can be disaggregated into voluntary quits and terminations, still another equation could be added ( $\gamma_1$  and  $\gamma_2$ ).

\*\*Because of data limitations the North Carolina model was estimated from quarterly data.

MATHEMATICA POLICY RESEARCH, INC.,  
Princeton, N.J., November 29, 1978.

HON. DANIEL PATRICK MOYNIHAN,  
U.S. Senate,  
Washington, D.C.

DEAR SENATOR MOYNIHAN: Thank you for your invitation to present my views to the Subcommittee on Public Assistance on the income maintenance experiments.

In my statement, which I have sent to Michael Stern, I comment briefly on the findings on work effort and family stability and make a few observations on the generalizability of the findings. However, my statement focuses on the experiments' other findings on family well being, which, in my view, were given insufficient attention by witnesses during your public hearings.

My statement summarizes the findings to date on family consumption and saving; housing and migration; health, nutrition, and fertility; and school attendance and performance of children. In addition, I discuss the effects of other benefits provided by some of the experiments—day care and training subsidies and counseling—and describe some of the other findings that compare the experience in the experimental programs with the participants' experience with AFDC. These findings are not uniformly positive or consistent, and they include some negative as well as numerous beneficial effects. However, they should be taken into account, along with the findings on work efforts and family stability, in a balanced examination of the implications for welfare reform of the research on the income maintenance experiments.

Sincerely yours,

KENNETH C. KEHRER.

STATEMENT BY KENNETH C. KEHRER, VICE PRESIDENT, MATHEMATICA POLICY RESEARCH

I thank the Subcommittee on Public Assistance for this opportunity to present my views on the income maintenance experiments and on recent research in the area of welfare reform. I was on the staff of the Gary Income Maintenance Experiment from 1971 to 1975, and become project director during the later stages of the experiment.<sup>1</sup> In addition, I have directed research at Mathematica Policy Research on some aspects of the Seattle-Denver experiment.

I would like to comment briefly on the findings on work effort and family stability and make a few observations on the generalizability of the findings. However, my remarks will focus on the experiments' other findings on family well-being, findings which, in my view, were given insufficient attention by witnesses during your public hearings of November 15-17, 1978.

WORK EFFORT

Total welfare costs depend partly on the disincentive effect of welfare programs on work effort. Any income support program is expected to have some disincentive effect because providing income support payments takes away part of the reason to work. The challenge in designing a new income support system is to develop a program that provides adequate benefits with a minimum of work disincentive.

The primary objective of the income maintenance experiments was to obtain better measures of work disincentive effects. The testimony at these hearings has reviewed the findings on work effort in some detail. It should be emphasized that, because of the experiments, we have been able to substantially narrow the range of uncertainty about the work effort disincentives of alternative benefit reduction rates and support levels.

Research on nonexperimental data had produced such a wide range of estimates of the disincentive effect that the estimates were not very useful for public policy formulation (for a comprehensive review, see Cain and Watts, 1973). For example, predictions based on nonexperimental data of the reduction in hours worked in response to a negative income tax plan with a benefit reduction rate of 50 percent and a support level equal to the poverty line range

<sup>1</sup> I have appended to my remarks the executive summary of the Initial Findings Report on the Gary experiment. The final report will be sent to the Subcommittee when it is completed in a few months.

anywhere from virtually nothing to 60 percent for adult men and between 13 and 100 percent for adult women (Moffitt and Kehrer, 1977). As indicated in Dr. Aaron's testimony, the work effort responses to the experiments fall into a much more narrow range: a reduction of between 1 and 11 percent for adult males and between 0 and 31 percent for adult females. It should be emphasized that the experiments tested a variety of different plans and enrolled samples that differed somewhat in their composition. Nonetheless, the relatively narrow range of work responses among different population groups in different parts of the country gives us confidence in being able to predict the work effort responses to alternative welfare reform proposals.

#### FAMILY STABILITY

In contrast to the findings on work effort, the findings on family stability are inconsistent across the experiments, and raise questions about the long-held notion that extending welfare benefits to intact husband-wife families would help stabilize low-income families. No effects on family stability that met the conventional tests of statistical significance were observed in the New Jersey, Rural, or Gary experiments (Wolf, 1976; Middleton and Haas, 1976; Wolf, 1977), but the Seattle-Denver experiment found substantial increases in family break-up, particularly in the early years of the experiment, among both blacks and whites (Hannan, Tuma, and Groeneveld, 1978). In this area, the experiments appear to have raised more questions than they have answered.

Although more weight should probably be given to the Seattle-Denver findings than to each of the other experiments (the Seattle-Denver experiment had a much larger sample size and exhibited improvements in design and implementation over the earlier experiments), the disparity in findings across the experiments on family stability as compared with those on work effort underscore the need to examine the generalizability of those findings.

#### GENERALIZABILITY OF THE FINDINGS

In my view, there are two important problems in generalizing the findings from the experiments: (1) the nature of the sample design strategy of the experiments, and (2) the differences in the experimental designs across the experiments.

The experiments were not designed to be national probability samples with known generalizability to the nation. Instead they were "test bores" in a few selected sites, chosen to represent a variety of ethnic and geographic characteristics.<sup>3</sup> That is, the samples were chosen judgmentally, and hence some judgment is required in interpreting and generalizing the findings based on them.

Where the findings from all the experiments cluster in a similar range, as they appear to with work effort, we can be more confident in generalizing the findings. But where the findings differ across experiments, we must examine whether these differences arise from characteristics particular to each site before generalizing the findings. For example, family break-up is generally greater in the West and Northwest. Is this part of the explanation for the Seattle-Denver findings?

Another potential reason for the divergent findings is the differences in design across the experiments. One major difference is the variation in support levels and benefit reduction rates across experiments, with the less generous plans having been tested in the Rural and Gary experiments and the most generous plans in Seattle-Denver. However, these differences can be controlled for in conventional statistical analysis.

Another important, but often overlooked, difference in the design of the experiments is the detailed rules under which each experiment was administered. Each experiment attempted to develop a simple set of rules to determine eligibility, payments levels, other benefits, administrative remedies, etc. But these rules turned out to be more complex than initially anticipated as they were modified

<sup>3</sup> The test bore approach was adopted for a variety of reasons. A decade ago, when the experiments were conceived, there was little survey experience in interviewing low-income families; thus, a dispersed national sample appeared to present more risks than samples concentrated in a few areas. Similarly, the problems of administering payments and implementing an experimental payments design in a dispersed national sample were much greater than those presented in only a few sites. Finally, the need for representative control groups that faced the same labor markets as did the families eligible for experimental payments pushed the designers in the direction of a few concentrated sites.

to take into account existing program regulations, situations peculiar to each site, and administrative decisions made on a case-by-case basis.<sup>9</sup>

More important for considering the generalisability of the findings is that these rules were different across experiments in some important ways. For example, in the Seattle-Denver experiment, if the husband and wife split up, both individuals (and their dependents) were eligible for continued payments. In Gary, until mid-experiment, a husband who left his wife and children was not eligible for payments. The Gary rules were changed at mid-experiment to permit a spouse leaving the household to remain eligible, but the Gary families appear not to have understood the change in rules (Tidwell, Kaluzny, Bruml, and DuRoss, 1976). Moreover, in Seattle-Denver a husband who left was eligible for the same support level (\$1,000 a year), regardless of the generosity of the NIT plan his original family had been assigned. These differences in the rules of the two experiments may have been the cause, or part of the reason, for the inconsistencies in the findings on marital break-up between the two experiments. Similarly, the rules in Seattle-Denver provide more of an incentive to break up in the least generous plans, which is what the empirical research finds (Hannan, Tuma, and Groeneveld, 1978). Although these considerations may not have influenced the response to the experiment, they do suggest that differences in rules must be carefully examined when generalizing the findings from the experiments.

#### FAMILY WELL-BEING

The major reason for having public welfare programs is to provide support for those who cannot support themselves. Our concern includes both those who are unable to earn a living because of some permanent incapacity and those who are temporarily down on their luck. The issue that has been debated sharply for many years is, of course, the level of benefits to be provided by public welfare programs.

The income maintenance experiments provided payments that were generally substantially more generous than were AFDC benefits in each site. Thus they can provide some insights into the effects that higher benefit levels would have on families.

These hearings have produced considerable testimony on the effect of the experimental negative income tax programs on work effort. It is clear that some of the benefits of a more generous welfare program will be taken in the form of reduced work hours. Although these work disincentive effects have to be reckoned as part of the cost of increasing the adequacy of existing welfare programs, the experiments also provide evidence that the more adequate benefits resulted in a broad range of improvements in family well-being. In addition, the evidence suggests that there are considerable secondary effects on families—that families used the payments in ways that will reduce their dependence on income support in the long run. These findings are not always consistent across experiments, nor do they occur in every population group studied. (In some cases, particular outcomes were studied only in some of the experiments.) Nonetheless, taken together, the cumulative findings from the experiments suggest the broad benefits that this kind of welfare reform would bring to poor families and to the society at large.

I summarize below the findings to date on the effect of the experimental payments on family consumption and saving; housing and migration; health, nutrition, and fertility; and school attendance and performance of children. In addition, I discuss the effects of other benefits provided by some of the experiments—day care and training subsidies and counseling—and describe some of the other findings that compare the experience in the experimental programs with the participants' experience with AFDC.

#### FAMILY CONSUMPTION AND SAVING

Economic theory and consumption studies of nonexperimental data suggest that income support payments would be used to reduce debt and to acquire durable goods. Only the Rural experiment detected any increase in savings as a result of the experimental payments, but all four experiments observed some reduction in debt by some subgroup.

<sup>9</sup> The objective of establishing more simplified rules than those of the existing AFDC system was at least partially achieved. See the findings described below on participant understanding of the rules.

In Gary, among intact (husband-wife) families, the income support payments resulted in an increase in such financial assets as savings accounts. There is also some evidence that the experimental support payments enabled these families to shift their debt from high-interest lending institutions such as stores and credit cards to more traditional lending institutions such as banks, credit unions, and savings and loan associations. Husband-wife families also used the payments to purchase home appliances and additional food, clothing, and medicine, but did not use the money to purchase automobiles. The experimental income support payments did not have an effect on the assets or debt of female-headed families. These families, who were much poorer than the husband-wife families, used the payments to buy home appliances, furniture, and clothing. (An increase in clothing consumption was also observed in the Rural experiment.)

#### HOUSING AND MIGRATION

There is some evidence from all of the experiments that families tended to use the payments to move to better housing. Families that rented responded to the experimental payments by upgrading their rental situations. Families in New Jersey, Gary, Seattle, and Denver tended to move out of public housing into private rental housing, and other families in New Jersey and Gary that were already living in private housing moved to units with higher rents (Woodriddle, 1977; Avrin, 1978; Kaluzny, 1978a). Families eligible for experimental payments also were more likely to purchase a house than were families in the control groups in New Jersey, North Carolina, and Gary (Johnson, 1976a; Poirier, 1977; Woodriddle, 1977; Kaluzny, 1978a). Evidence that the payments were being used to finance a move to another area with the promise of better employment opportunity and a better life was found in North Carolina (Johnson, 1976b) and Seattle-Denver (Keeley, 1977), but not in New Jersey (Woodriddle, 1976) or in Gary (Kaluzny, 1978b).

#### HEALTH, NUTRITION, AND FERTILITY

There are some fragmentary findings in this area suggesting that payments were being used to improve family health and well-being. The New Jersey and Rural experiments investigated the effect of the payments on health status and on changes in utilization of health services (Kerachsky, 1977; Kerachsky, 1978; Lefcowitz and Elesh, 1977). There is some evidence of positive effects on the utilization of health services. In New Jersey, such effects are particularly discernible for adults who have no private health insurance or Medicaid. No such pattern is evident for adults in the Rural experiment; however, in that experiment the utilization of health services appears to increase among children. In neither experiment did the increased use of health services result in improvements in participants' health status during the experiments.

Among the three experiments in which psychological health was investigated, there was no apparent effect in the New Jersey or Rural experiments (Middleton, 1976; Middleton and Allen, 1977), but there is some evidence that the payments may have resulted in increased stress among families eligible for payments in Seattle and Denver (Thoits, 1978).

The Gary experiment examined the effect of the experiment on the birth weight of children born during the experiment (B. Kehrler and Wolin, 1977). Low birth weight, defined as 2,500 grams (5.5 pounds) or less, is associated with sharply elevated infant mortality rates and with higher rates of morbidity during infancy and later years. Consequently, the weight of an infant at birth is an important index of its health status. Income support payments might be expected to have an effect on birth weight through their influence on the mother's nutrition, prenatal care, and the possibility for reducing paid employment at strenuous jobs during the later months of pregnancy. Beneficial effects were found in Gary for children born to high risk mothers—women who smoke, teenagers, older women, and women with short intervals between pregnancies. The beneficial effects range as high as an additional pound for children born to the highest risk mothers. These gains in birth weight are an indication of the potential broad benefits of an improved income support system. HEW plans to conduct research to investigate whether a similar effect on birth weight occurred in Seattle and Denver.

The Rural experiment studied the effect of the payments on nutrition (O'Connor, Madden, and Prindle, 1976). Although no effect was found in Iowa, North Carolina families with an inadequate diet used the payments to improve the

quality of their nutrition. Interestingly, no nutritional effects were found among North Carolina families with as high a level of nutrition as found on the average in the Iowa sample. These results, together with the findings on birth weight, suggest that the experimental payments improved health and nutrition among those who were most in need.

#### SCHOOL ATTENDANCE AND PERFORMANCE OF CHILDREN

The evidence from the experiments suggests that there may be some beneficial effects on the schooling of children, although the evidence is inconsistent. All four experiments have examined the effects on drop-out behavior among teenagers. While no effects have been detected in the Rural and Seattle-Denver experiments (Middleton, Haas, and Haas, 1976; Hall and Weiss, 1977), male and female teenagers in New Jersey (Mallar, 1977) and male teenagers in Gary (McDonald and Stephenson, 1976) reduced their employment and stayed in school to a greater extent than did teenagers in the control group. The New Jersey experiment also found evidence of increased college attendance among young adults in families eligible for the experimental payments.

The effect on school performance of children, as measured by achievement test scores, academic grades, and attendance, was studied in three of the experiments (Maynard, 1977; Mannheim and Minchella, 1978; Murnane and Maynard, 1978). The findings are somewhat inconsistent, but they do indicate the existence of some beneficial effects. The most striking results are found in the North Carolina sample, where younger children (grades 2-8) in families eligible for payments showed improvements relative to the control group in all measures examined—school attendance, scholastic grades, classroom behavior, and scores on standardized tests. However, no discernible effects were found for the older children in North Carolina or for the (generally higher income and better performing) children in Iowa (Maynard, 1977). In Gary, the findings included both positive and a few negative effects on performance for various subgroups (Murnane and Maynard, 1978). However, as in the Rural experiment, significant performance gains were observed among younger children and children in the lowest income families. Preliminary examination of school performance in Seattle and Denver has detected no effect of the experimental payments (Mannheim and Minchella, 1978).

Thus, the findings from the experiments on the school performance of children indicate some beneficial effects on some subgroups in some sites. The lack of discernible effects on most subgroups, together with the few negative findings, indicates that these beneficial effects should be evaluated cautiously. On the other hand, given the difficulties of measuring performance gains and the likelihood that the full benefits of a home intervention strategy such as income maintenance cannot be captured in a short-run experiment, the beneficial effects observed in the experiments provide some evidence of the potential school performance gains from an improved income maintenance system.

Fertility is another area in which we cannot expect to learn very much from a short-run experiment about the long-run effects of increased income support payments. Findings from New Jersey and Seattle-Denver indicate only very small effects on fertility during the experiments (Cain, 1977; Keeley, 1978), but in Gary there is evidence of a reduction in fertility among unmarried women (Wolfin, 1978).

#### DAY CARE AND TRAINING SUBSIDIES AND COUNSELING

Both the Gary and Seattle-Denver experiments included subsidy and counseling programs in addition to the cash support payments. In Gary, selected families were eligible for child-care subsidies and for the services of "access workers," who provided referrals to social work agencies. In Seattle and Denver, child care was subsidized through the support payments; child-care expenses were deducted from income counted in the payments calculation. The Seattle-Denver experiment also provided manpower counseling to some families, manpower counseling and a 50-percent subsidy for education and training expenses to other families, and manpower counseling and 100-percent subsidies for education to still other families.

In Seattle and Denver, the child-care subsidies increased the use of extra-familial child-care services (particularly informal care) between 22 and 35 percent for families with a single head (which were eligible for dollar-for-dollar matching of child-care expenses) and between 14 and 18 percent for husband-



wife families that received lower subsidies (Kurz, Robins, and Spiegelman, 1975). In Gary, there was little utilization of the child-care programs, although female-headed families eligible for experimental income support payments and child-care subsidies were much more likely to use the child-care program than were other families (Behrens, 1978a). However, the child-care subsidy program in Gary did appear to have a strong positive effect on the work effort of wives (Behrens, 1978b).

The counseling programs had very little impact. The access worker services in Gary were utilized so little that the program was phased out prior to the end of the experiment. The manpower counseling services (in the absence of any subsidy) had no effect on participation in training and education programs in Seattle and Denver. However, when provided in conjunction with training subsidies, the manpower services resulted in increased time spent in education and training programs, suggesting that the subsidies were the critical element of the manpower program.

#### PARTICIPANTS' UNDERSTANDING AND EXPERIENCE

Participants in the income maintenance experiments had a better understanding of the experimental programs than they had of AFDC and preferred these programs to AFDC.

In several of the experiments, participants were asked a series of questions to ascertain the extent of their understanding of the experimental income support programs. Household heads were highly knowledgeable about the rules that governed a family's eligibility for continued participation in the experiment, but, as under AFDC, considerably less knowledgeable about how their benefits were calculated (Knudsen, et al., 1974; Harrar, 1976; Tidwell et al., 1976). In Gary, families were asked about their perceptions of the program itself. Over 60 percent described their participation as having been "very worthwhile" and 81 percent indicated that the program had helped them (Tidwell, 1977).

In Gary the participants were also asked to compare the experimental program with their experience in the AFDC program (Tidwell, 1977). The findings suggest that those families that had been AFDC recipients viewed the experimental program more favorably than they viewed AFDC. In matters of administration in particular, the AFDC program was judged to be inferior. For example, almost 48 percent of the families thought that AFDC rules were too difficult to understand, while only 16 percent held the same view about the Gary program. Similarly, 85 percent believed that AFDC rules were too intrusive; 48 percent thought so about the Gary program. Finally, almost 75 percent felt that AFDC rules were not enforced equitably, while only 28 percent felt this way about the Gary program.

#### IMPLICATIONS OF THE FINDINGS

The findings on family well-being that I have summarized here are not uniformly positive or consistent, and they include some negative as well as numerous beneficial effects. But, taken together, they are suggestive of the potential effects of a more adequate system of income support. I conclude that these findings indicate that welfare reform that results in the kinds of programs tested in the experiments would contribute to less welfare dependency in the long run, which would (at least partially) offset the disincentive effects on work effort. Many families would use the payments to increase their long-run well-being and their earnings capacity, increase their savings, reduce their debt, obtain additional education and training, and migrate to areas with better opportunities. There would be improvement in nutrition and in the health of children at birth. Dropout rates among teenagers would diminish, and perhaps there would be improvements in school performance. There would be a reduction in the use of social services agencies and less reliance on public housing.

The kinds of programs tested in the experiments were substantially more expensive than the programs now being debated in Congress, and the beneficial effects observed in the experiments might not emerge under less adequate programs. However, the findings I have described here should be taken into account, along with the findings on work effort and family stability, in a balanced examination of the implications for welfare reform of the research on the income maintenance experiments.

THE GARY INCOME MAINTENANCE EXPERIMENT: SUMMARY  
OF INITIAL FINDINGS

(By Kenneth C. Kehrer)

EXECUTIVE SUMMARY

The Gary Income Maintenance Experiment was one of a coordinated series of experiments supported by the U.S. Department of Health, Education, and Welfare and the Office of Economic Opportunity to test the work incentive effects and other consequences of alternative income support plans. The experiments were conducted with different population groups in different parts of the country. The income support plans tested in Gary were similar in structure to those of existing welfare and transfer programs, except that the benefit formulas were simplified and eligibility was universal, depending only on family income and family size, and the presence of a dependent child. Benefits were determined by the support level, that is, the basic benefit provided to a family with no other source of income, and an implicit tax or benefit reduction rate, that is, the rate at which the benefit is reduced as other sources of income increase. Some benefits were paid to all families with income below a breakeven level, with the largest benefits going to those families with the lowest incomes. Thus, under such a plan the size of the benefit decreases as family income rises, but total family income always increases as earnings from work increase.

Four different income support plans, combining two implicit tax rates and two support levels, were tested in Gary. The tax rates were 40 and 60 percent, and the support levels were equal to the poverty level and about three-fourths of the poverty level annual income for each family size. In 1972, for example, when the official poverty threshold for a four-person-nonfarm family was \$4,275, the two Gary support levels were \$4,300 and 3,300 for that family size. Benefit schedules were adjusted every six months to compensate for increases in the cost of living.

The income maintenance experiments were experiments in the sense that otherwise similar families were randomly assigned either to an experimental (payments-eligible) or control group. By comparing the behavior of the experimental and control families, it is possible to determine statistically the effects of the income support plans, because the only important difference between the two groups was the randomly assigned experimental status.

The experimental group families were eligible for the income support payments for three years. All participating families filed monthly reports of income and family composition changes, and were interviewed before the experiment, about three times a year during the experiment, and after the experiment. Selected families were eligible for child care subsidies at various subsidy rates, and for experimental information-referral services.

*Characteristics of the participating families*

Each of the experiments studied the responses of different population groups. The Gary experiment focused on black families in an urban environment. Eligibility was also limited to families with at least one child under age 18. Of the 1,799 families who enrolled (voluntarily), 57 percent were assigned-eligibility for experimental income support payments, while the remainder were control subjects. Almost 60 percent of the participating families were female-headed families (families without a male head of household present).

The families with a male head of household present (almost all of which were intact husband-wife families) usually had low incomes but generally were not extremely poor. The husbands were typically full-time workers who were able to earn enough to keep their families out of poverty—only 10 percent of these families had incomes below the poverty line. The wives, on the other hand, typically did not work outside the home—only 13 percent were employed at the start of the experiment. In the relatively few families where both the husband and the wife were employed, the wife's earnings usually raised family income so high that the family no longer qualified for the receipt of income support payments.

The husband-wife families studied in Gary would not be considered typical welfare families because of their attachment to the labor force and their income levels, and because public assistance payments were not generally avail-

able to husband-wife families in Indiana. But under the income support plans tested in Gary, many of these families were eligible to receive modest income supplements. The analysis of the Gary experiment can therefore provide insight into the consequences of extending an income supplement program to working, but low-income, families.

The families with female heads of households were generally much poorer than the husband-wife families studied. Over 80 percent were receiving welfare benefits from the Aid to Families with Dependent Children (AFDC) program immediately prior to the experiment. About three-fourths of the families that switched from AFDC to the experiment had incomes below the poverty line. The female heads on AFDC at enrollment were very dependent on welfare: 86 percent of their monthly income came from public transfers, with AFDC grants alone accounting for slightly more than half of their incomes. As with the wives studied, only 13 percent of the AFDC female heads were employed.

The female-headed families not on AFDC prior to the experiment were somewhat better off; only 38 percent had incomes below the poverty level. Approximately 60 percent of the income of the non-AFDC female-headed families came from earnings (40 percent of the female heads in these families were employed), while most of the rest of their income came from Food Stamps, Social Security, and other transfer programs.

The income support plans tested in Gary were considerably more generous than AFDC. Average experimental payments to female-headed families by the end of the second year of the experiment were \$258 a month, as compared to \$159 for AFDC payments. Thus, the Gary experiment can provide information about the effects of increasing the generosity of welfare payments to female-headed families and extending eligibility for income support to female heads who do not currently receive AFDC.

The research reported here is based on limited data from the first two years of the experiment. Because the analysis exploited only a small portion of the available data, these initial findings are still tentative. Once all of the data have been analyzed, the tentative conclusions summarized here may be revised. Nonetheless, several conclusions emerge from the data with strong statistical support.

#### INITIAL FINDINGS ON WORK EFFORT RESPONSE

The initial analysis focused on the work effort of household heads who were of working age and capable of working. The work effort response at the end of the first and second years of the experiment was estimated using multiple regression analysis, a statistical technique that took into account the effects of major differences among families likely to influence work effort (e.g., age and education of household heads, family size, and labor market conditions). The available data have been subjected to numerous alternative specifications to test the sensitivity of the work effort response. While these sensitivity tests do not exhaust all possibilities, the narrow range of the response estimates do provide support for the tentative conclusions summarized here.

The initial analysis detected little difference in the work effort response among alternative income support plans. That is, the various support levels and implicit tax rates tested in Gary did not appear to result in greatly varying levels of work effort. Thus, the initial findings summarized in the following pages compare individuals eligible for the experimental support payments—regardless of the specific plan—with individuals who were control subjects.

The initial findings indicate that the experiment had a modest disincentive effect on the work effort of household heads by the end of the second year (see Table 1). In intact families, husbands reduced their total hours worked by an average of 7 percent, and wives reduced their hours of work by 17 percent. These estimates are quite similar to those of the work effort response of husbands and wives in the New Jersey and Rural experiments. Female heads who switched from AFDC to the NIT reduced their hours of work by 5 percent. However, because both AFDC female heads and wives worked few hours prior to the experiment—about 6 hours a week on the average—their reductions in work effort had only a small impact on total family labor supply and earnings. Our findings indicated that female heads not on AFDC at enrollment actually increased their hours of work slightly relative to controls, but we have little confidence in this result.

TABLE 1.—SUMMARY OF INITIAL FINDINGS ON WORK EFFORT FROM THE GARY INCOME MAINTENANCE EXPERIMENT

[Hours worked at the end of the 2d year]

	Effect of the experiment on total hours worked per week	Mean hours worked by control group	Work effort response as a percentage of control group mean
Husbands.....	-2.5	36.0	-7
Wives.....	-1.0	5.7	-17
Female heads:			
On AFDC prior to the experiment.....	-.3	6.5	-5
Not on AFDC prior to the experiment.....	.3	14.7	+2

Note: These estimates of the work effort response were obtained using a statistical technique (regression analysis) that controlled for family composition, other family income, earnings of other family members, the individual's age, education, normal wage rate, and preexperiment work effort and AFDC status, and the unemployment rate and season at their time of the interview.

### *The work effort response of husband-wife families*

A major focus of public debate over welfare reform has been the potential disincentive effects of extending coverage to all husband-wife families, or of increasing existing welfare benefits for these families. Currently only about half of the states provide benefits (under AFDC-UF) to intact, husband-wife families where the husband is unemployed.<sup>1</sup> The income support plans tested in the income maintenance experiments generally provide higher payments than existing AFDC benefits, and cover more husband-wife families than AFDC.

The work effort response of intact families to the support plans tested in Gary was centered among the husbands, who reduced their total hours worked by 2.5 hours a week in response to the experiment. This disincentive was largely the result of the complete withdrawal of a few individuals from the labor force rather than of small reductions in work effort by most of the husbands. Those who were not well established in the labor market prior to the experiment were mostly likely to withdraw from the labor force.

The wives responded to the experiment by reducing their total hours worked by one hour a week. This decline in work effort consisted of both a disincentive effect on employment of a few wives and an across-the-board reduction in hours worked by those who continued to work. However, many of the wives who stopped working remained in the labor force since they continued to look for work. In general, the estimates of the work effort response of wives are statistically insignificant, partly due to the small number of working wives in our sample.

Earlier income maintenance experiments focusing on intact families were conducted in cities in New Jersey and Pennsylvania and rural areas of Iowa and North Carolina. These experiments tested income support plans with benefit levels set between 50 and 125 percent of the poverty line and tax rates of between 30 and 70 percent. However, most of the participating families were assigned to income support levels between 75 and 100 percent of the poverty level and tax (benefit reduction) rates around 50 percent, as in Gary. It is useful to compare the findings from these experiments which tested similar income support plans on different population groups.

For husbands, the estimated average experimental response of total hours worked from the completed experiments falls in the range between -1 and -7 percent (see Table 2). The average response of black husbands in Gary appears to have been about the same magnitude as the response of white husbands in cities in New Jersey and Pennsylvania. The Gary response was centered in a reduction in employment among a few husbands, while the response in the other experiments was characterized by a marginal reduction in hours worked by many husbands. One reason for this may be that, in the highly institutionalized labor market in Gary, husbands may not be able to make small adjustments to their work effort. The only way to reduce work effort may be to quit work altogether.

<sup>1</sup> However, eligibility for AFDC-UF benefits depends not only on income, but also on attachment to the labor force or on previous employment. All states provide benefits under AFDC to intact families where one of the parents is incapacitated.

TABLE 2.—COMPARISON OF FINDINGS ON WORK EFFORT FROM 3 INCOME MAINTENANCE EXPERIMENTS

[Percentage changes in hours worked]

	Husbands	Wives
New Jersey experiment.....	-6	-31
Rural experiment.....	-1	-27
Gary experiment.....	-7	-17

Note: These estimates are weighted averages of the response in hours worked of different population groups. Because there were some technical problems in estimating the response of black and Spanish-speaking groups, the estimates from the New Jersey experiment reported here are for whites only. Recent reanalysis of the New Jersey data for husbands provides evidence that the magnitude of the response for these groups is similar to the response of whites (see Kertchsky and Meltzer, 1976). More detailed response estimates are presented in table 12 on p. 64.

The results from the three experiments for wives indicate a large disincentive effect in percentage terms (the estimates range from -17 to -31 percent), although the response is more modest in absolute terms; the range of response for wives was between 1 and 5 hours a week. Black wives in Gary and in the New Jersey sites appear to have reduced their work effort less than the other wives studied in the experiments.

Thus, the evidence from quite different population groups suggests that the reduction in work effort by prime-age husbands in response to an income support plan with a support level of about 85 percent of the poverty level and a tax (benefit reduction) rate of around 50 percent would not be large. The work effort response of wives to such a plan would be larger in percentage terms, but would not be large in terms of actual hours. Further analysis of data from the Gary and Seattle-Denver experiments will examine the generalizability of these findings and the impact of alternative income support plans.

#### *The work effort response of female heads*

Switching female heads from AFDC to the experimental support plans resulted in only a modest reduction in work effort, 3 hours worked per week on the average (about 5 percent), despite the relative generosity of the experimental payments. The response consisted primarily of a few female heads who stopped working rather than an across-the-board reduction in hours worked. The modest disincentive effect—somewhat smaller than the negative experimental response of husbands in the sample—may reflect the work disincentive effects of the AFDC program, which enables mothers to reduce their work effort in order to care for their children. Switching from AFDC to a more adequate income support program may not lead to large reductions in work effort because many female heads may have already reduced their hours of work under AFDC. In any case, these estimates suggest that increasing the support levels of the AFDC program along the lines of the income support plans tested in Gary would result in a decline in the work effort of female heads by only a modest amount.

Female heads who were not on AFDC prior to the experiment appear to have increased their work effort slightly, by about 3 hours a week, in response to the experiment. However, the estimated responses for the group of female heads who were not on AFDC prior to the experiment are statistically insignificant and unstable over time. The sample size of this group is quite small and may not be large enough to permit us to estimate the effect of the experiment on their work effort with confidence.

#### OTHER INITIAL FINDINGS

While work effort was the central focus of the initial analysis, it was not the only response of interest. Studies were also conducted on experimental responses in four other areas: the effects of income maintenance on family consumption; the demand for housing; the demand for social services; and the choices teenagers make among school, work and leisure. In addition, other studies investigated the utilization of the experiment's subsidized child care and social services information-referral programs, and the degree of participants' understanding of the rules of the experiment and the mechanics of the income support plans.

The effect of the experiment on family consumption was investigated by comparing differences between the experimental and control groups in debt, monthly purchases, and the acquisition of durable goods between mid-experiment and

the period prior to enrollment. This preliminary analysis suggests that experimental families tended to use their additional income to increase their expenditures on clothing, medicine, and automobile repairs (but not to purchase automobiles), and to reduce their medical debt. In addition, families eligible for experimental payments spent 78 percent more on home production appliances and 64 percent more on furniture than control families. Initial examination of the housing consumption patterns of a subsample of families indicates that the experimental payments did not appear to induce families to move to different housing. On the other hand, among those families that did move during the experiment, public housing residents in the experimental group were about 50 percent more likely to move to private dwellings than similar control families, and experimental families were twice as likely to purchase homes. Thus, the experimental payments appear not to have influenced the decision of families to move but, among those families who would have moved anyway, the payments influenced their choice of residence.

Experimental families used social agencies less extensively than did the control families. Controlling for other factors, families eligible for experimental payments reduced their use of social service agencies about 13 percent. Thus, the evidence from the Gary experiment suggests that a universal income support program, available to more families and with higher benefits than AFDC in Indiana, may reduce the demand for social services to some extent.

The experimental payments appear to have had a positive effect on school attendance among male teenagers, who tended to reduce their labor force participation and continue their high school education. On the other hand, the experimental income support plans appear to have had no effect on high school continuation for female teenagers and no effect on college attendance by either sex. Of course, black female teenagers are already much more likely to finish high school than black males, so there exists less opportunity for a positive experimental response among females.

Child care subsidies were available to selected experimental participants at varying subsidy rates—100, 80, 60, and 35 percent. For most of these families, the availability of the subsidies was contingent on working (or engaging in a work-related activity). The number of families who used the child care program was much smaller than originally anticipated; less than 5 percent of eligible families used the program during the second year. The rate of utilization generally declined as the subsidy decreased, and utilization was higher among families with preschool children; for families with preschool children the rate of use in the 80 and 100 percent subsidy plans with a work requirement was 15 percent. These initial findings suggest that utilization of a child care subsidy program will depend on the rate of subsidy, and that utilization will be concentrated among families with preschool children.

The Gary experiment attempted to test the usefulness of "access workers" who provided information and referral services to a subgroup of the study sample. However, utilization of the access workers was much lower than expected and declined to an almost negligible level from the beginning of the program to the end of the first year of the study. About 25 percent of the eligible families contacted the access workers.

Household heads were highly knowledgeable about the rules that governed a family's eligibility for continued participation in the experiment. However, as had been anticipated, they were considerably less knowledgeable about the mechanics of the income support plans. These results are similar to findings from the New Jersey and Rural experiments.

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STATEMENT OF THE NATIONAL CONFERENCE OF CATHOLIC CHARITIES, PREPARED BY  
REV. EDWARD J. RYLE, DEAN, MARYWOOD COLLEGE GRADUATE SCHOOL OF SOCIAL  
WORK, SCRANTON, PA.

We welcome the opportunity to submit this testimony in relation to recent research in the field of public welfare and some of the implications of this research for welfare reform. The research carried on through the initiative and support of the federal government has generated a body of rich data and hypotheses which should not be neglected as we look ahead to the Ninety-sixth Congress and the challenge it will face to work for welfare reform.

Last year we testified before the Special Welfare Reform Subcommittee of the House as it considered H.R. 9030. We recognized that the bill was a major step toward structural reform of the welfare system, but found it lacking in a number of respects, including the adequacy of its benefit level, soundness of its job program, and a number of other more specific matters. On the whole, however, we did believe it represented a good basis for discussion and development of a reformed public assistance program.

The morning on which we testified happened to be the same morning that the Washington Post carried a story about the Denver-Seattle Income Maintenance Experiment and its report that guaranteed minimum income programs had a destabilizing effect on the family. We indicated in our oral testimony at the time that, at the level of basic values, this was disturbing news and noted that the results covered only the first two years of the experiment and were, necessarily, incomplete. At the same time we believed that providing poor people with an adequate income support program might well represent too important a value to be held hostage to the possibility that such reform might indirectly have a destabilizing effect on some families. At that time we pointed out the old phrase in logic that goes: *Post hoc ergo propter hoc, non valet illatio*. That means: Just because something happens after something else, it doesn't mean it happens because of something else. Given, however, our very limited knowledge at the time, we appreciate this opportunity to comment again on the matter, as well as to make an initial response to some of the major research findings presented to the Subcommittee on Public Assistance.

#### SOCIAL SCIENCE AND PUBLIC POLICY

Ten years ago the distinguished Chairman of this Subcommittee wrote that "It is the necessary condition of politics that action be based on insufficient knowledge." (*Maximum Feasible Misunderstanding*; 189). We fully agree. The political process and the social policy that it produces deal with the realm of the practical, the contingent, the to-be-created, the future, and the free actions of human beings. Almost by definition, therefore, will they be exposed to unintended as well as intended outcomes, to latent as well as manifest functions. Short of a society of robots, public social policy will necessarily be based on less than perfect or, in many cases, even reasonably adequate evidence, despite the best efforts of social scientists.

There is, moreover, a further reason for the inadequacy of knowledge in relation to political action for social policy development. This has to do with the value elements and choices that are part of social policy.

We are not recommending agnosticism here; but we do note that in a pluralistic society, in which a number of value systems are operative, a certain amount of disagreement and confusion about values is bound to make clear, discriminating judgments of value a very difficult business. And beyond this is the further matter of conflicts among quite elusive values which policy makers might seek to realize for our country. A good example is the apparent conflict between a guaranteed minimum income and family stability as indicated by the Seattle/Denver Income Maintenance Experiment.

One further reason for caution in assessing the role of knowledge in social policy development—and here we are thinking especially of the contributions of the social sciences—has to do with the role of theory. Theory can point to questions for empirical research and assist in the interpretation of such research. If research is concerned with getting "the facts," theory is concerned with the meaning of the facts. Both theory and facts or data are indispensable for the social sciences. We believe, however, that in the United States, social scientists have advanced much further in their ability to conduct research, whether survey, experimental or panel, than they have in theory-building. To the extent that this is true, it is an added reason for caution in use of the social sciences in social policy formulation. Moreover, it leads us to believe that there is still much truth in Senator Moynihan's judgment ten years ago that "the role of social science lies not in the formulation of social policy, but in the measurement of its results." (*Ibid.*, 193. *Italicized in text.*)

This is not to deny that there has been significant progress in social science research in the past ten years. The four major income maintenance experiments are proof that there has been. But even these studies do not give policy makers clear and unambiguous criteria or guidelines for policy development.

## THE INCOME MAINTENANCE EXPERIMENTS

The New Jersey, Rural, Gary and Seattle/Denver income maintenance experiments agree in finding that men reduce their work effort somewhat if a guaranteed income is available; women in two-parent families reduce their work effort more than men; and female family heads reduce their work effort by about 15 percent, withdraw from the workforce, or do not even enter it. Overall, these results were not surprising. Moreover, they provided research to support inclusion of some work requirement in the Administration's welfare reform proposal. We would note here that a full employment economy would probably do more than a bill such as H.R. 9050 to provide jobs for poor people and keep welfare costs down.

The most troubling finding of the income maintenance experiments is that a guaranteed minimum income seems to have a destabilizing effect on marriages. Such a finding was not expected, since a positive relationship between economic well-being and marital stability has been a widely accepted generalization in the social sciences. (Cf. Michael T. Hannan and Nancy Brandon Tuma, "Income and Marital Events: Evidence from an Income Maintenance Experiment," *American Journal of Sociology*, Vol. 82, 6: 1187-90 and Arthur J. Norton and Paul C. Glick, "Marital Stability: Past, Present and Future," *Journal of Social Issues*, Vol. 32, 1: 13-14).

Of the four experiments, the Seattle/Denver experiment was the only one that found a statistically significant difference in family breakup between the experimental and control groups, and this only for black and anglo families, not for Hispanics. Too, in the third year of the experiment, the dissolution rate for anglo families was lower, though not by a statistically significant amount, for families in the experimental group than in the control group. For all the experimental groups the dissolution rate was lower in the third year of the experiment than in the first two.

The Seattle/Denver research is valuable, an important contribution to our knowledge of the possible impact of a guaranteed minimum income on marital stability. Others have commented on some of the limitations of the Seattle/Denver experiment. (Cf., for example, "Statement by Henry Aaron, Assistant Secretary for Planning and Evaluation, DHEW, before the Subcommittee on Public Assistance of the Senate Finance Committee, Washington, D.C. November 17, 1978".) Among other matters Aaron pointed out that the research was not designed to monitor impact on family life, and indeed the findings do not differentiate between husband and wife dissolution and a common law situation. We would like to make a few observations about the policy implications of the experiment.

As we noted above, policy makers can face quite genuine conflicts in values. In the present case, there is an apparent conflict between developing a more adequate welfare program for poor people and the important value of family stability. Should efforts to reform and upgrade welfare programs be held up till there is solid evidence that they will not destabilize marital unions? We think they should not be for the following reasons:

(1) Efforts to provide adequate welfare payments, in a dignified manner, to needy people should not be delayed because of the tentative possibility that a slightly higher percentage of families might experience marital dissolution than would do so in the absence of more adequate assistance. As we indicated earlier providing poor people with an adequate, decent level of sustenance might well represent too important a value to be held hostage to the possibility that its provision might indirectly have a destabilizing effect on some families. Moreover, most research indicates the contrary.

(2) The assistance given through the income maintenance experiments was not linked with employment programs and provision of social services. A well constructed welfare reform plan will have a jobs component, since employment can have a stabilizing effect on marriages, as well as reduce welfare costs. Too, welfare reform that offers access to social services can assist couples having marital problems to seek help in resolving their problems.

(3) There is a danger inherent in holding back on welfare reform to avoid a destabilizing impact on the marital stability of poor families, the danger of using public assistance as a means of controlling the highly personal decisions of husbands and wives. Given the long history of using poor law in both England and the United States as a means of social control, we believe it important that welfare reform proposals carefully respect the dignity and right to self-determination of recipients of public welfare.



## CONCLUSION

We appreciate very much the opportunity to submit this testimony for the record. We recognize that the issues being studied by the Subcommittee are very difficult and complex. We hope that what we have presented is of some help to you, Mr. Chairman, and assure you of our desire to cooperate as best we can with you and the Members of the Subcommittee in the vitally important task of reviewing income adequacy and security for the poor and needy of our country.

## STATEMENT OF HAROLD W. WATTS

Let me first express my gratitude for the invitation to offer testimony to this Committee. I am writing as an economist with many years of experience in analyzing income maintenance policies, and as one who shared primary responsibility for the design and analysis of the New Jersey Negative Income Tax experiment. I hope that my remarks here will be of use to the Committee and the Subcommittee in its difficult task of understanding and interpreting evidence from the experiments in the formation of improved policies.

I will comment first on the testimony offered by Dr. John Cogan concerning his reanalysis of the New Jersey evidence. This testimony is misleading in its claim that new estimates show larger labor supply responses than other studies. The estimates cited answer an essentially different question, one which is in itself of secondary importance for policy purposes. The testimony cites a ". . . five to seven hour per week reduction in work effort among those who received cash assistance . . .", which is contrasted with a two-to-three hour reduction he has selected from a crude and preliminary segment of my own analysis of the response of married men.

He focuses his analysis on "participants" who are defined as male heads of families that are currently receiving benefits under one of the experimental programs. He measures the difference between such men (i.e., participants) and those (i.e., non-participants) who are heads of families who do not receive either these benefits or benefits from the welfare program in New Jersey or Pennsylvania. The earlier estimates prepared by me and others are aimed, in contrast, at the difference between those families offered coverage under a negative tax program and those who were not.

It may be useful here to digress briefly on Cogan's use of the term "participant" because the confusion in large part originates in his usage. There is a common usage among those who analyze public programs that distinguishes as participants those who carry out the steps of formal application and enrollment and finally receive the program's benefits, e.g., Food Stamps. The participants are, thus, a subgroup of the eligible population who meet all the requirements for benefits, whether or not they have applied for them. The ratio of participants to eligibles is usually termed the participation rate. Both "eligibles" and "participants" are defined as those who meet any income, wealth, or employment criterion that is a condition for eligibility. Because income and employment fluctuate for individual families, both groups contain a shifting cast of families.

The basic situation in the experiment is very different. Among those who agreed to take part in the experiment (I forego the term participate), some, the control group, agreed to be interviewed at regular intervals for a three-year period. The experimental group agreed, in addition, to report their earnings and other income on a monthly basis. They were also told that benefits would be paid according to a specific schedule if and when their income experience dropped below a certain level. Thus, the experimental families were enrolled once and for all, and whenever their reported economic and program status made them "eligible," they were automatically sent benefits, i.e., were automatic participants. So, in the experiment, there is a fixed and pre-selected group of enrolled families who may move into or out of participant status depending on their income experience.

Both experimental and control families exhibited substantial income fluctuation from month to month (fully confirming the instability found in other longitudinal studies) and, in consequence, the current participant group is constantly changing (as well as the size of benefit for individual participants). To use the term participant in this context is to impute deliberate intention to all income-change events which move a family into or out of payment status despite the evident similarity of such fluctuations in the control group. Because variation in earnings of family members is the most important source of these fluctua-

tions it is almost tautological that Cogan's participants will be working less than non-participants. They couldn't be participants of his definition otherwise.

The same regression analysis that produced the five to seven hours per week reduction for participants in the experiment programs yielded estimated reductions for participants in the public welfare programs that were typically twice that large. Yet these have not been mentioned in Cogan's testimony. I find no merit in either estimate as a measure of labor supply behavior occasioned by the different incentives or disincentives provided by the experimental or welfare programs. The reason is that both include a definitional component that would indicate a spurious labor supply "reduction" even if there were no proper response to those incentives. Even if all income variations of families were outside their control, Cogan's regressions would show a substantial response.

In estimating the differentials between those who received experimental benefits and those who received neither experimental benefits nor welfare (mostly AFDC-UP), Cogan seems to be saying that if no benefits were provided the former group they would work as much as the latter (the same would be true for the participants in welfare who showed much larger "reductions"). Even if that hypothesis were supported by his or others' analysis (and it emphatically is not), the issue is not whether all sources of transfers for low income families should be stopped. I have little doubt that a resolute "root hawg or die" policy would produce an increased level of labor supply among the survivors. But I seriously question whether a highly refined estimate of the amount of that increase is of great interest to this Committee. If it is of interest, then the experimental evidence is of very limited value for the simple reason that we did not (and could not legally) impose starvation options on a pre-selected sample of families. Yet, if they try to measure anything, Cogan's estimates try to measure such a magnitude. I can only say that any such estimate, based on this or any other existing income maintenance experiment and using any amount of econometric sophistication, must finally rely upon much raw supposition.

The estimates from my work, which Cogan contrasts with his own, can be interpreted as the incremental reduction in labor supply as a consequence of adding the experimental program to the options that already existed at the various experimental sites (including welfare). There is no pretense of estimating the "zero based" effect. Because of this difference and because of the tautological component of his estimates, there is no reason to be surprised that his numbers are larger than mine. They simply show a different contrast, and one that is incapable of causal interpretation or application to significant policy issues.

A fundamental flaw in Cogan's analysis lies in what he claims to be most distinctive about his approach. He states in his testimony (Page 2, paragraph 2, sentence 3): "Only families that receive NIT benefits would be expected to respond to the work disincentives of the program." From this, he defines participants and proceeds to measure their response. Yet would he or anyone also assert that only those who "participate" in the highest bracket of the federal income tax schedule can be affected by its (often claimed) disincentives? Many planned and unplanned events serve to change one's employment, income, and family status over any reasonable long period of time, and these events change the array of taxes and transfer programs in which one "participates." We must suppose that experience and information about all public programs can have an effect on a person's planned and realized pattern of work behavior, not just the ones that are activated at a particular moment or week. There simply is no theoretical or empirical basis for Cogan's assertion about ex ante response behavior.

Cogan's entire analysis ignores the very basis upon which the experiments were designed, and aims at estimating a single number of hours that measures some sort of average work reduction of all those who "participate" in one of several distinct N.I.T. variants. Useful measures of response cannot be so simply stated. It was assumed in the design that the response to "coverage" by a program which offered a schedule of benefits related to income would depend upon the earning capacity and other income sources of the covered families. Since the benefit schedules only modify the income pattern of those with low incomes, either persistently or intermittently, we reasoned that the response would become negligible at some point well above the break-even point and that the response would vary inversely with earning capacity (or normal pre-experimental income) below such a point. The objective was to estimate a complete response schedule for the range of initial income levels that were allowed in the experiment. (The

range permitted by OEO did not include all those who could reasonably be expected to display a non-negligible response). A given negative tax scheme should not be expected to affect those who earn \$100 a week by the same amount as those who earn \$200 or \$300. Equally we should not expect groups who earn \$150 a week by working 60 hours to respond in the same way as those who earn the same amount by working 35 hours. Cogan's analysis does not address this issue at all, despite the fact that in his analysis for "participants" there is a strong built-in relationship between the initial level of income and the reduction necessary to become a participant. While there may be only a small chance that someone as fortunate as me would decide to (or happen to) become a "participant," if I did it would require a much larger withdrawal than 5-7 hours! At the opposite end, someone who had not worked for years could hardly increase his withdrawal no matter how great the disincentive.

The estimates that were produced by myself and others from the New Jersey experiments proceeded beyond those crude estimates Cogan has mentioned to provide such schedules of response, and further examined the effect on labor force withdrawal, and unemployment, as well as on adjustments in hours worked. The Seattle-Denver experiments provided a much larger sample and have also been utilized to estimate well-specified and coherent response patterns. Those estimates have been used by H.E.W. analysts to project the response to various welfare reform proposals. Their estimates are, within the limits of precision that can be obtained by samples, generally consistent with comparable results from the New Jersey experiment. The labor supply reductions are not inconsequential, and the pattern shows a more marked response for those with lower earning capacity. But they remain quite small in aggregate terms and do not, to my mind, serve to dictate a negative over-all evaluation to the sort of policy alternatives that have been seriously advanced by our three most recent administrations.

My remaining comments are devoted to more general issues concerning the role of experimentation in the analysis of welfare alternatives. I believe the evidence from the experiments is extremely important for adding to our knowledge in specific ways, and that the potential is as yet only partly realized. But it is important to emphasize the inevitable limitation of this evidence as well.

Clearly no evidence or well established finding about how income subsidies affect families can displace the value judgments that are inherent in a choice of policy. Even if there were no uncertainty concerning the consequences of explicit policy alternatives for labor supply on family stability, those consequences will be acceptable, or even desirable for some and unacceptable for others. A container can be half full and half empty at the same time, and the most that can be expected of scientific inquiry is resolution of uncertainties about the consequences of specified alternatives. The assessment of those consequences and ultimate choice lies outside the range of scientific evidence.

But we are far from eliminating uncertainty about consequences and the experiments to date have focused on reducing the uncertainty about crucial but very incomplete parts of the question. If, for example, the labor supply response to alternative income subsidies were perfectly known for each category of worker (by age, sex, family status, wage rate, etc.), there is still the question of labor demand. Only if demand is perfectly elastic for each category of worker will that change in supply be reflected exactly in a change in equilibrium employment. Clearly we do not believe that the market can absorb unlimited increases or decreases in supply with no change in wage rates. If demand were perfectly inelastic, on the other hand, a reduced supply would be reflected entirely in equilibrating wage rate changes and no change in employment. In fact, neither extreme can be taken seriously, but the experiments do almost nothing to reduce our uncertainty about the actual outcome. For workers who are frequently affected by unemployment, the change in labor supply may be largely absorbed by reducing the difference between the amount the worker wants to work and the amount of work it is possible to find, with little or no change in the latter.

The experiments, important as they are, have not yielded exact knowledge about labor supply behavior either, and they should not be expected to. But they can reduce the range of our uncertainty and focus discussion more on real than on imaginary issues. They provide increments to the evidence available that cannot be drawn from other sources, and that is their primary contribution. But we are still a long way from fully digesting this incremental evidence, as the testimony to this subcommittee shows. It is possible for different investigators to offer very different estimates of what is claimed to be the same magnitude. It

is possible for many of these differences to be resolved on scientific grounds, of course, but the example illustrates the importance of caution and judgment in interpreting the first findings from any new and complex body of evidence. There should be an opportunity and encouragement for re-analysis by investigators using different theoretical and empirical approaches to the basic data, and the strengths and weaknesses of the various alternatives should be thoroughly explored. Only in this way can the robust findings from new evidence be identified and (provisionally) used to confirm or modify our understanding of something as complex as labor supply behavior.

There is a widespread and unfortunate belief that experiments such as the ones in New Jersey or Seattle-Denver can lead directly and immediately to summary judgments about alternative policies, or specifically about the policy which is identical to the experimental treatment. This is not the case as the comments above indicate. A well-designed experiment is both more and less powerful than that. It is less powerful in that it can only partially replicate the conditions of a full-scale national policy and cannot in any case resolve differences in values. It is more powerful in its potential for gaining evidence on specific issues such as labor supply behavior that is relevant for assessing the consequences of a much wider class of policies, including a continuation of the status quo or modest incremental changes.

But there are real and important differences between the experiments and possible policies. The experiments did not impose a work requirement. The seriously proposed policies have included such provisions. The experiments included no features to enforce or reinforce parental responsibility for children; real policies do (and should). The reports on the design of these experiments provide many more examples. Such qualifications do not render the experimental evidence useless, but they certainly complicate its interpretation and application to the choices that must be made by this committee and others who have the responsibility for actual policy.

The task of integrating and reconciling the new evidence with existing beliefs about issues such as labor supply is inevitably a long and difficult task for scientists, political leaders and the general public. Despite the difficulty, I remain convinced that the experiments have an important contribution to make toward finding more effective and equitable policies. Much of this potential is as yet unrealized, and resources are limited for carrying on secondary analyses. But hearings such as this one can be very valuable in emphasizing the importance of the researchable issues and encouraging efforts to help resolve them.

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#### PATTERNS OF WELFARE USE

(By Martin Rein and Lee Rainwater, Joint Center for Urban Studies of M.I.T. and Harvard)

#### INTRODUCTION

One of the recurrent issues in the debate about welfare policy is how to identify the people who are generally thought of as belonging to a "welfare class." Just how many people stay on the welfare rolls for particular amounts of time and what characteristics are common to those who remain on welfare for long periods of time have been central concerns of a long-term Joint Center research project funded by the U.S. Department of Health, Education, and Welfare.

This project, of which we are the directors, has taken a quite different tack from the only other attempt to measure the number of heavy welfare users. Based on a national biennial sample of welfare cases extracted from AFDC administrative records, that count estimates that about one-quarter of the cases sampled had entered the welfare system at least five years before. This figure, however, refers only to the recipients' most recent continuous use of welfare, and it counts cases, not families. It also says nothing about how dependent the recipients are on welfare, that is, whether or not they are living primarily on aid from the welfare system. This issue is particularly important because it addresses the question of whether there is a large group of citizens who do not fit with the American ideal that, except for occasional emergencies, everybody should be self sufficient.

The Joint Center's project, on the other hand, has counted the number of people on welfare, not cases, and has designed a way to estimate how much the recipients

rely on the welfare system as a means of support for their families.<sup>1</sup> These are the two issues addressed in this paper.

#### THE DATA

The data we chose for our work comes from the first seven years of the Longitudinal Panel Study of Income Dynamics (PSID) from the University of Michigan's Survey Research Center. This material provides information on sources and levels of income for a national cohort of 5,060 households surveyed annually since 1968. (Data for the eighth year, 1975, was just becoming available as the research for this analysis was drawing to a close.) More families toward the lower end of the income distribution were sampled so that a disproportionate number of welfare recipients were interviewed. While the total figures were weighted to represent national trends, the additional lower-income families and minority groups provided greater reliability in the results about welfare use than a simple random sample of this size.<sup>2</sup>

We selected as the basic unit of our analysis the individual woman aged 18 to 54 in 1968 and the families "around" her during the seven-year period. Initially, we had tried to use the family as our unit of analysis, but we found this impossible given the volatile nature of American society and, particularly, of our sample. When couples split up, data as it was gathered by the PSID became available on both the husband and his new family as well as on the wife and her new family. Then, as families reorganized themselves, which person to follow became unclear. There was no simple way to analyze the family unit in our sample except by limiting our analysis to those families whose heads had not changed. However, we were especially interested in those families that had changed their structure. Our way of resolving this dilemma was to make the women, not the men, the focus of our concern because entitlement to many welfare programs depends largely on their status. Thus we followed them and the families with whom they had lived over time.

Our extract from the PSID, then, contains records on the households of 3,088 women who remained in the sample throughout all seven years of the survey. One-quarter of these household records include women whose families received welfare at least once in seven years. These we included in our "ever-welfare" group.<sup>3</sup>

As usual in this kind of applied microeconomic work, there are limitations to the data which should be pointed out at the start. First, the elementary time unit which measures (for example) a "single welfare spell" or "experience" is the year. Within such a time frame it is impossible to ascertain whether the experience actually lasted one year or less or whether there were possibly several spells within the year. A second problem we found is the absence in the PSID of retrospective (1967 or before) information on the subjects' experiences with welfare. To analyze a particular cohort, we were forced to restrict our sample to those individuals who had been observed for one year before going on welfare.

A third problem arises from the fact that the PSID is based on a survey of families and not on administrative records. The result is an extensive under-reporting of welfare income especially, we think, among families which receive only a modest amount of welfare during the course of the year. The reason that the survey, which counts families on welfare, ends up with fewer welfare cases than one which contains administrative statistics is probably quite simple. Even though several members of one family may receive benefits from a number of programs, that family would still be counted in the survey as a single welfare case. Despite this difficulty, we preferred to rely on the PSID as a resource rather than the Current Population Survey of the U.S. Bureau of the Census because the PSID retrieves more of the welfare expenditures per family.<sup>4</sup> The

<sup>1</sup> The project's results were first published in abbreviated form in Challenge 20 (1977). "How Large is the Welfare Class?" by Martin Rein and Lee Rainwater. The detailed analyses were submitted earlier this year to HEW. Besides summaries of the material, the present Working Paper contains a summary by the staff at HEW and two appendices explaining the more technical aspects of their work.

<sup>2</sup> For the present study, "welfare" is an aggregate of all forms of public assistance, both federal (AFDC, AFDC-U) and state (General Assistance, etc.). Means-tested programs include public assistance and food stamps. Public transfers refer to cash benefits from workmen's compensation, unemployment insurance, and social security. Private transfers include income from relatives, alimony, and child support. Those people designated as of "minority status" are blacks and hispanics. All other groups are of "majority status."

<sup>3</sup> When the sample is weighted, the percent of the families of women in this age range (from the PSID sample) which ever received welfare becomes 11.9.

<sup>4</sup> Joseph Minarik, "New Evidence of the Poverty Count," Proceedings of the Social Statistics Section of the American Statistics Association (1975), pp. 54-59.

accuracy of reporting all income is also likely to be greater in the PSID survey because the family is not penalized for a truthful account of the level and sources of income, including welfare. On the other hand, these differences between the national statistics, which are based on month-by-month analyses of welfare cases, and the PSID longitudinal survey, which is based on the annual experience of families, mean that comparative studies of the data become quite difficult.

## DURATION

*Entering, leaving, and coming back on welfare*

Welfare attachment can be described in statistical terms by examining patterns of use, that is, the length of time and number of periods when welfare provides a source of family income.<sup>6</sup> PSID provides us with the only available nationwide estimate of annual welfare dynamics which is based on the experience of families that use welfare and not on a count of cases as they are administered. We derived patterns of continuous or intermittent use of welfare from the experience, as recorded by the PSID, of women aged 18 to 54 years old in 1968, a period when people are generally launching or have launched their careers, their jobs, and their families.

In order to establish the patterns of welfare use for the families of women in this age group, we constructed a seven-digit variable in which each digit represents one year. The first, or "millions," digit stands for the welfare experience of the woman's family in 1967. The second is for the family's experience in 1968, and so on. Each digit is coded "0" if no one in the family received welfare, "1" if the family received less than \$100 from welfare during the year, and "2" if it received more than \$100 from welfare during the year. Thus, a family with a value of 2220012 received more than \$100 of income from welfare in 1968, 1969, and 1970, went off welfare for two years in 1971 and 1972, and then came back on welfare for two years, receiving less than \$100 in benefits in 1973 and more than \$100 in 1974.

Characterizing the individual's family welfare experience in this way allowed us to capture the number and duration of welfare episodes over the seven years of PSID data. In describing these episodes, we avoided terms such as "chronic" and "recidivist" in favor of the more neutral term "cyclic." Table 1 represents information on the number of consecutive welfare years over the seven-year period during which the families of women aged 18 to 54 in 1968 annually received at least \$100. (Of those who went on and off welfare, 21 percent had two cycles [on-off and back on again] and 1.7 percent had three cycles. These figures do not show in Table 1.) The table shows that 4.8 percent of all 3,086 women in the PSID, aged 18 to 54 years in 1968, received welfare in 1967 and that this proportion first climbed to a high of 6.6 percent in 1972 and then declined slightly in 1973. During this period, an average of 5.5 percent of all women in our age group received welfare in a given year.

TABLE 1.—EVER-WELFARE WOMEN AGED 18 TO 54 IN 1968 WHOSE FAMILIES RECEIVED WELFARE BETWEEN 1967 AND 1973

Year	Number of years of continuous welfare use—							Total on welfare	Percent on welfare from total 3,086 in age group
	0	1	2	3	4	5	6		
1967.....	295							295	4.8
1968.....	90	207						297	4.9
1969.....	65	56	173					294	4.8
1970.....	110	48	36	160				354	5.8
1971.....	104	54	36	36	145			375	6.0
1972.....	80	60	37	24	31	133		365	6.6
1973.....	88	36	42	21	19	20	114	340	5.6
Average percent on welfare.....									5.5

Note: The diagonal cells include women using welfare more than the indicated number of years, because that figure includes both the women who started in welfare in 1967 as well as those who were on prior to 1967 (years for which we have no data).

<sup>6</sup> Ideally, we would like information on the use of welfare over a very long span of time. And, at some point, the intergenerational use of welfare should be examined. However, the analysis here is limited by our source material, the seven years of the PSID; and we know of no good data on intergenerational use.

TABLE 2.—PROBABILITY OF FAMILIES OF WOMEN AGED 18 TO 54 IN 1968 GOING OR STAYING ON WELFARE IN CALENDAR YEAR, AS A FUNCTION OF THE YEARS PREVIOUSLY ON WELFARE

Year welfare started	Going on—	Staying on for the nth year—					
		2	3	4	5	6	7
1967.....		0.702	0.836	0.926	0.906	0.917	0.857
1968.....	0.015	.622	.642	1.000	.861	.645	.....
1969.....	.011	.738	.750	.667	.792	.....	.....
1970.....	.019	.491	.685	.568	.....	.....	.....
1971.....	.018	.577	.700	.....	.....	.....	.....
1972.....	.014	.450	.....	.....	.....	.....	.....
1973.....	.015	.....	.....	.....	.....	.....	.....
Average for 1968-73.....	.015	.576	.695	.745	.827	.645	.....
Simplified model.....	.015	.600	.700	.750	.800	.800	.800
Number per 100,000 going and staying on welfare using simplified model....	1,500	900	630	473	378	302	242

Note: Estimated probability of going on welfare in the following year as a function of a number of years off welfare: 1 yr, 0.25; 2 yr, 0.20; 3 yr, 0.12; 4 to 5 yr, 0.07.

Once we had identified each welfare cycle for the women aged 18 to 54 in 1968 (Table 1), we computed the proportion of those on welfare in any given year who stayed on for an additional year (Table 2). Ideally, we would have preferred to begin with a universe of starters, that is, women who had never received welfare before. The best we could do with our data was to report the experience of women who had not received welfare the year before. We excluded those who were already on welfare in 1967 because we did not know how long their previous experience had been. Thereafter we calculated the proportion of women who stayed on another year to all women who went on welfare in each calendar year between 1967 and 1973. From the data we generated we then found for each cohort of women going on welfare each year the average number who had an additional year of welfare.

Some of our findings seemed to be anomalous. For example, in the 1968 and 1970 cohorts, the probability of staying on welfare for each year first rose and then declined sharply in the last year for which data is available. As Table 2 shows, the computations are based on very small samples. It is possible that the fluctuation in our probabilities could be attributed to errors made in sampling the small number of cases that were on welfare a long time.<sup>6</sup> When we examined the full (295) cohort of women who were on welfare in 1967, we found that the probability of their staying on welfare in the last year did not decline so precipitously. Since the proportions estimated for the 1967 group were higher than those for other years, we decided that it was best to average the six-year experience of women who were on welfare in 1967 and 1968 for the purpose of developing a simplified model. We preferred to base our conclusions on averages among the cohorts on the belief that they would be more reliable than the proportions for any single cohort.

By averaging across cohorts we in fact created another, synthetic, cohort which would allow us to testimate the proportions of women going on and off welfare during the historic period between 1968 and 1973. This estimate is reported in Table 2 in the row labeled "Average for 1968-1973." We rounded these ratios to create the simplified model on which we based our final conclusions. Thus, the simplified model indicates that those who have not been on welfare in the previous year have a 1.5-percent chance of going on welfare and another 60-percent chance of staying on for a second year. Those who stay on the second year have a 70-percent chance of being on a third. There is a 75-percent chance of continuing to a fourth year. Thereafter, the probability of staying on welfare for an additional year rises to 80 percent where it plateaus.

#### *Relying on welfare*

In this section we describe how we determined the proportions of women on welfare in a given year who will be forming different patterns of attachment to welfare over the following seven years. We should note that the patterns of future

<sup>6</sup> Alternatively, this finding could indicate that perhaps some administrative process is in operation which excludes individuals who have been on welfare for several years. If this is the case, then the sharp drop in figures would suggest an extremely interesting social process of systematic exclusion at play.

welfare use, which we are to be outlining, will differ quite sharply depending upon whether we focus on a cohort of starters, on ever-welfare individuals, or on those receiving welfare in a given year.

Our findings are presented in Table 3. We comment first on the cohort of starters (see the third column). To create this synthetic cohort, as before, we counted the people going on welfare over a seven-year period as well as those going off. For each group that was leaving, we then calculated the number of them who will be going back on in each subsequent year. Of those who reenter, or cycle, we computed how many will stay on without leaving again, but we omitted the small group of second cyclers. The expected distribution of the first cyclers' experience on welfare over time is based on estimates both of the probability of staying and the probability of reentering once they leave the welfare rolls. We found that these estimated probabilities, as reported in Table 2, are a function of the number of years a person has been off welfare. Thus, if a woman has been off welfare for only one year, she has a 25-percent probability of returning to the welfare rolls in the future. When a woman has been off welfare for two years, her probability of returning to welfare drops to 20 percent. As time goes on, these probabilities decline so that she has only a 7-percent probability of receiving welfare once again after a stretch of four to five years off welfare.

In the second column of Table 3, we present data on an actual cohort of women who went on welfare in 1968. These women represent 13 percent of the universe of ever-welfare women. Note that the findings from the synthetic and actual cohorts are similar.

TABLE 3.—VARIOUS ESTIMATES OF YEARS ON WELFARE

Years on welfare in a 7-yr period	Percentage of cohorts of women aged 18 to 54 in 1968			
	Ever-welfare women	Actual 1968 cohort	Average (synthetic) cohort of starters <sup>1</sup>	Those on welfare in 1967
1.....	33.0	22.2	18.3	17.4
2.....	15.4	17.8	18.4	10.7
3.....	13.7	18.9	14.6	8.2
4.....	6.1	5.6	11.1	5.7
5.....	7.1	13.3	10.9	6.2
6.....	8.3	22.2	10.9	11.1
7.....	16.3	-----	18.1	38.1
Total percent.....	100.9	100.0	100.3	99.4
Percentage of cyclers.....	.096	.322	-----	.235
Proportion of all women.....	.12	.015	.015	.048

<sup>1</sup> Expected distribution based on staying and reentry probabilities.

When we are dealing with a single cohort, of starters for example, we want to know something about the total welfare career of the individuals within that cohort over the course of their lives. In reality, individuals who start their careers on welfare at different points in time may have different patterns of use. But this question cannot be explored systematically because we cannot examine long-term welfare use patterns for individuals who enter welfare in the latter years of the survey. We have to assume that, if our synthetic cohort of starters and our actual cohort are similar, we should be able to conclude that the cohort of starters has fewer short-term and more long-term individuals than the cohort of ever-welfare women, and that about one-third of the starters cycle on-off, then on, welfare. Overall, in a seven-year period, there are slightly more on welfare during the first and second years than later. Thereafter, for the remaining years, almost equal proportions of individuals are on the rolls. The basic reason that the pattern is flat for the last five years is that the number of individuals coming back on welfare just about balances the number leaving.<sup>7</sup>

<sup>7</sup> Strikingly, those with majority status are more likely to cycle as compared to those with minority status (38 percent versus 20 percent). One possible implication of this finding is that people of majority status try to get off welfare and fail. They therefore find themselves cycling on and off. Minority groups, by contrast, may either be locked in, or resigned to being on welfare. They are, therefore, less likely to cycle.



When we examine the pattern of welfare use by the ever-welfare women, we find that ever-welfare women are more likely than our synthetic cohort to receive welfare only once in seven years because they can enter welfare in later years (see first column, Table 3). Almost half receive welfare for one or two years, but they tend to cycle on and off welfare less than those in the synthetic cohort. Only about one-tenth enter, exit, and reenter, as compared with one-third of the women in the cohort of starters.

This group's propensity toward short-term use is largely a function of the way in which we pursued the data. Instead of focusing on a complete welfare career over several years, as we did for the synthetic cohort, we focused on the experience of women during a particular stretch of time. In such a framework individuals can enter the analysis any time in the period, even in its last years. The consequence is that our calculations lead us to expect more short-term participants than there exist in reality. We can also assume that many participants had actually been on welfare for several years before 1967. In our cohort analysis (Table 2) we saw that the proportion of those staying on welfare is higher than those who are going on for the first time. If we include in our count those people who were already on welfare, we find that the distribution becomes biased in favor of individuals with either short- or long-term attachment—a bimodal split. About one-third of the ever-welfare individuals receive welfare for five, six, or seven years; one-third receive welfare for one year.<sup>8</sup>

When we project the pattern of use for women who had received welfare in a particular year (e.g., 1967, column 4, Table 3), we find a large cluster of women who have been on for long periods of time and a small group who stay on only a few years.<sup>9</sup>

Our analysis has revealed a good deal of recurring dependence. That is to say, people tend to return to welfare after leaving it for a period of time. We find that about 10 percent of those who received welfare at least once in the seven years cycle as compared with about 25 to 30 percent of the synthetic cohort of starters or of those on welfare in a given year. This is because short-term users are a smaller proportion of the ever-welfare population.

#### DEPENDENCE

##### *Sources of income among welfare recipients*

The question of the sources of income of welfare recipients has not received much attention, perhaps because national statistics on the financial circumstances of currently active AFDC cases (those active in a given month) suggest that a good two-thirds of their average income is derived from welfare. In addition, about three-quarters of the average administratively defined budgetary requirements of AFDC families comes from welfare income. In computing these budgetary requirements, income which is disregarded in computing needs is not included in the measure of the family's actual income.

Data from the Current Population Survey (CPS) of the Bureau of the Census provide a different picture of welfare dependence from the data presented by the national AFDC survey of cases. The CPS figures are based on annual rather than monthly income, actual rather than "countable" income, and families rather than administrative cases.<sup>10</sup> Table 4 shows sources of income of female-headed families with children in 1970, according to the CPS survey.<sup>11</sup> For those who receive welfare (means-tested benefits) it remains the chief source of income (about half the total), but it no longer overwhelms the income package as the monthly figures taken from administrative records of cases suggest. Earnings are the second most important component of the income of families headed by women; the ratio of earnings to welfare is more than 50 percent. These census data, therefore, make it clear that to grasp the pattern of welfare use, we need to pay attention to the degree of economic dependence as well as the number of years on welfare.

<sup>8</sup> Among ever-welfare individuals, those with minority status are more likely to cycle and to be long-term as compared with those with majority status.

<sup>9</sup> About one-quarter of this group cycles. There is virtually no difference in the extent of cycling by race, as we found in our synthetic cohort.

<sup>10</sup> Because welfare users presumably rely more heavily on welfare in the month they receive it, it would be useful to have the number of months that individuals received welfare. Unfortunately, these data are not available in PSID survey.

<sup>11</sup> One striking feature of this table is how similar the income package is for minority and majority recipients.

TABLE 4.—SOURCES OF 1970 INCOME OF FEMALE-HEADED FAMILIES WITH CHILDREN, IN WHICH WOMEN AGED 25 TO 54 IN 1968 LIVED

Sources of income	Percentages of income from different sources					
	Female-headed welfare families			All female-headed families		
	Majority	Minority	All	Majority	Minority	All
Earned income.....	30.2	27.4	29.0	58.0	55.0	57.2
Heads earnings.....	24.4	22.0	23.4	45.9	40.2	43.3
Other earnings.....	5.8	5.3	5.6	12.2	14.8	12.9
Assets.....	3.9	3.8	3.8	9.3	5.2	8.2
Work-related benefits.....	11.5	10.9	11.3	16.2	12.7	15.2
Means-tested benefits.....	51.1	56.7	53.5	12.2	25.3	15.9
Other income.....	3.2	1.2	2.4	4.2	1.8	3.6
<b>Total percent.....</b>	<b>99.9</b>	<b>100.0</b>	<b>100.0</b>	<b>99.9</b>	<b>100.0</b>	<b>100.1</b>
Number of female heads.....	914,942	744,317	1,659,295	2,678,976	1,305,110	3,944,086
Percent of all female heads with children on welfare.....	34.7	57.0	42.1			

Note: Information taken from Current Population Survey, March 1968. These data are from a special run provided by Joseph Minarik of the Brookings Institution. Some aspects of the original data were altered in the course of merging CPS and tax data. For example, corrections were made for underreporting of income.

#### *Patterns of welfare use and degree of dependence*

Data on sources of income for families on welfare in a given year do not provide a complete or balanced picture of the sources of income of low-income, ever-welfare families in general. Such a picture should also include sources of income for both the long-term families and the occasional welfare users as well as a count of how much income both groups derive from welfare over several years. To fill out the picture, Table 5 divides the 745 women in the PSID who ever used welfare during the seven-year period into five types, as indicated in the stubs at the left.

Our definition of long-term (continuous) attachment as four to seven years of welfare experience is, of course, arbitrary. Women with long-term welfare experience by this definition comprise about 38 percent of the universe of all women who received welfare at least once in seven years. In dividing them into two groups—those who had 50 percent or more of their seven-year income from welfare and those who had less than 50 percent from welfare—we found that 12 percent had received at least half their seven-year income from welfare and 26 percent received less than half. There were not enough women to permit us to create a category which would measure the degree of dependence in combination with the amount of cycling found among women with long-term welfare attachment.

Short-term attachment was defined as the receipt of welfare for two to three years. In all, 28 percent of ever-welfare women were short term by this definition. Of them, cyclers accounted for about 9 percent and those continuously on welfare for the two to three years about 19 percent. But since we can track only part of the life history of these women, we cannot assume that cyclers and long-term women represent sharply distinct categories. An examination of the life histories of these people suggests that many short-term continuous welfare recipients may become cyclers later on in their careers. Nevertheless, this distinction remains useful for studying how they put together their income packages. The last category, women with only one welfare experience in a seven-year period (once only), accounts for about 34 percent of all ever-welfare women.

Table 5 shows not only the distribution of these various welfare patterns but also the extent to which each type of user depended upon welfare over the entire seven years they were observed. For those who used welfare only once, it was an insignificant portion of their total seven-year income (though it may have been very important at the time). Likewise, for those who used welfare only two or three years, it did not make up a very large proportion of their total seven-year income. For the long-term (four- to seven-year) users, welfare obviously was more important. The group defined as having more than half of their income from welfare called upon the system for almost 70 percent of their income. Perhaps even more interesting is the observation that over twice as many of the long-term group (26 percent vs. 12 percent) received less than half their

income from welfare. It is also noteworthy that only about 20 percent of the total income over the seven years for the entire ever-welfare group came from welfare.

Obviously, the question, "How much do welfare families depend on welfare?" is answered very differently depending on whether it refers to the number of cases in a single month or to women using welfare at any time over a period of several years.

TABLE 5.—DISTRIBUTION AND SELECTED CHARACTERISTICS OF 5 EVER-WELFARE TYPES BETWEEN 1967 AND 1973 (N = 745)

Pattern of use	Percentage of each type	Total 7-yr income	Percentage of 7-yr income from welfare	Percentage welfare income for years on welfare
Long-term (4 to 7 yr):				
(1) More than 50 percent of 7-yr income from welfare.....	12.0	\$25,347	68.9	72.2
(2) Less than 50 percent of 7-yr income from welfare.....	26.3	38,278	28.8	37.1
Short term (2 to 3 yr):				
(3) Cyclers.....	9.2	39,566	11.9	31.1
(4) Continuous.....	18.7	37,657	10.4	35.2
(5) Once only.....	33.9	50,070	2.9	20.3
Average.....		40,850	20.2	

#### *Shares of welfare income by pattern of use*

Although the long-term dependent group, as defined in Table 5, is only 12 percent of all welfare users in the survey, another tabulation (not shown in the table) reveals that they received 35 percent of all welfare income during the seven-year period. The other long-term users (four to seven years on welfare but with less than half of their seven-year income from welfare), a group that is twice as large, used another 45 percent of all the welfare income during the period. The other three groups in Table 5 received the remaining 20 percent of all welfare income. From these figures we would draw the conclusion that although the long termers, and especially those who are very dependent, are only a small fraction of all welfare users, they do consume a very disproportionate share of all welfare income.

#### LONG-TERM DEPENDENCE—HOW BIG IS THE WELFARE CLASS?

The data generated above indicate the kind of information we used to estimate how many women aged 18 to 54 in 1968 (the first year of the PSID survey) belonged to what we would call the "welfare class." Becoming a member of that welfare class, as we have defined it, involves three different procedures in combination: going on welfare at all; staying on welfare for a period of years; and being heavily dependent on welfare during those years.<sup>12</sup>

During the period from 1968 to the present, the probability that women between ages 18 and 54 will go on welfare in a given year is about 1.5 percent (Table 2). That is, about 15 of every 1,000 women who were not on welfare in the previous year will go on. Therefore, out of the roughly 46 million women in this country between the ages of 18 and 54, about three-quarters of a million women go on welfare each year.

The PSID data, as we saw above, allow us to calculate the probability of women staying on welfare for a second, third, fourth year or longer (Table 2, Simplified Model). Women on welfare have a 60-percent chance of staying on for a second year and a 70-percent chance for a third. The probability plateaus at 80 percent after the fourth year. Using these probabilities, plus the probabilities of cycling off and on again, we can calculate how many women who start on welfare will remain for two to ten years.

<sup>12</sup> This latter is particularly important. The PSID data reports families, for example, who are on welfare for long periods but derive only a small portion of their total income from welfare. Because the welfare portion typically comes from payments on behalf of foster children in their care, to include these long-term recipients as part of the welfare class would not conform with the conceptions policymakers and the general public have of that class.

The data show that at any given time, about 5.5 percent of women in the 18- to 54-year-old age range are members of families which derive at least part of their income from welfare (from Table 1). The proportions of going on and off welfare suggest that the average woman who goes on welfare all spends about four out of ten years on welfare. Her four years is the sum of two consecutive years on welfare and two years cycling back onto welfare after having been off for one or more years.

The conception of a welfare class held by researchers, policymakers, and the general public requires being on welfare for longer than four years. The operational definition we use here is somewhat more strict. We assume that members of the welfare class have been on welfare for six or seven years (projected to nine or ten years) out of seven years (or ten years) and were more than 50 percent dependent upon welfare income during those years. Let us look at the estimates of the various stages of welfare participation for 18- to 54-year-old women as a way of arriving at an estimate of the size of this long-term dependent group.

We start out with the approximately 50 million women who are between the ages of 18 and 54 in any year. Over a ten-year period, 7 million (14 percent) of these women, together with their families, will have some welfare income in at least one of these years. The remaining 43 million (86 percent) will have had no welfare experience during the ten-year period. The welfare experience of those 7 million women will be as follows:

TABLE 6.—10-yr welfare experience of a synthetic cohort of women

Total women 18 to 54 yr of age in 1st year.....	50,000,000
On welfare at least once in 10 yr.....	7,000,000
On welfare for 4 or fewer years.....	3,500,000
On welfare more than 4 yr.....	3,500,000
On welfare 5 to 8 yr.....	2,730,000
On welfare 9 to 10 yrs.....	770,000
Less than 50-percent dependent on welfare.....	154,000
50 percent or more dependent on welfare.....	616,000

Thus, out of the total of 50 million women, the size of the welfare class will be 616,000. These are the women who go on welfare, stay on welfare for nine or ten years, and in those years derive 50 percent or more of their family income from welfare payments. They are 9 percent of the women who ever go on welfare.

In any one year, some 2.7 million women are likely to be on welfare. But, by our definition, we find that only slightly over 20 percent of them (540,000) are members of the welfare class. The other 80 percent (2,160,000) have short- or medium-term welfare careers.

Overall, one can conclude that the welfare class is a definite minority among welfare recipients. They represent less than 10 percent of those who ever go on welfare, and only a little over a fifth of those already on at any one time. Such welfare career patterns suggest that it might be useful to think of other categories of welfare recipients as the more typical.

#### INCOME PACKAGING BY PATTERN OF WELFARE USE

Once it was established that the "welfare class" is in fact only a small portion of the welfare population, we turned our attention to how that population as a whole put together enough income to see them through. We compared the sources of income of women in the different welfare types and examined the ways in which these sources differed in the years the women did and did not receive welfare. By way of comparison, we included an analysis of the sources of income of a group of comparably low-income women who had never used welfare.

Several themes emerge from the information presented in Table 7. First, and perhaps most striking, is the diversity and complexity of the income sources upon which women in ever-welfare families relied. Aside from the heavily dependent, long-term women, ever-welfare women in general counted on welfare for only about a third of family income during welfare years; those who received welfare only once used it for only a sixth of their income. Therefore, an additional 66 percent or more of family income must come from other sources. The earnings of the head of family account for approximately 30 percent of the

family's income; and, in addition, bits and pieces are put together from the earnings of wives and children, from alimony and child support, from food stamps, public and private transfers, and work-related benefits. Savings are not an important source of income, partly because eligibility is conditioned by a lack of assets and partly because families tend to deplete their savings before they turn to welfare.

TABLE 7.—FAMILY INCOME FROM DIFFERENT SOURCES FOR WOMEN AGED 18 TO 54 IN 1968

[Percentages]

Income sources	7-yr pattern of use					
	Low income, never welfare	Long term (4 to 7 yr)		Short term (2 to 3 yr)		
		More than 50 percent of 7-yr income from welfare	Less than 50 percent of 7-yr income from welfare	Cyclers	Continuous	Once only
<b>YEARS ON WELFARE</b>						
Work-related benefits.....	6.9	2.4	6.6	9.8	9.2	7.4
Means-tested benefits.....	.4	80.4	44.4	36.7	41.4	17.4
Welfare.....		72.2	37.1	31.1	35.2	13.9
Food stamps.....	.4	8.2	7.3	5.6	6.2	3.5
Alimony and child support.....	.7	2.5	2.9	3.1	3.5	.8
Earnings and assets.....	88.2	12.3	39.8	43.0	42.2	73.2
Head's earnings.....	69.3	6.2	23.5	32.8	29.2	47.4
Wife's earnings.....	9.2	.4	1.5	3.9	4.1	7.5
Other's earnings.....	7.2	5.7	14.4	6.2	8.9	17.6
Assets.....	2.5	-----	.4	1	-----	.7
Public and private transfers.....	3.0	2.5	6.4	7.4	3.6	1.9
From others.....	1.3	1.2	4.5	4.2	1.2	.7
From each Husband and wife.....	1.7	1.3	1.9	3.2	2.4	1.2
Total.....	99.2	100.1	100.1	100.0	99.9	99.9
<b>YEARS NOT ON WELFARE</b>						
Work-related benefits.....	6.9	12.0	6.7	15.1	8.6	10.2
Food stamps.....	.4	6.1	2.4	2.9	1.9	1.7
Alimony and child support.....	.7	2.4	1.1	3.1	2.4	1.7
Earnings and assets.....	88.2	76.4	83.2	68.8	82.0	85.7
Head's earnings.....	69.3	52.7	62.0	55.0	65.4	61.4
Wife's earnings.....	9.2	1.4	9.6	6.6	8.7	11.1
Other's earnings.....	7.2	22.3	11.3	7.0	6.6	11.6
Assets.....	2.5	-----	.3	.2	1.3	1.3
Public and private transfers.....	3.0	3.1	6.5	10.2	5.0	2.2
From others.....	1.3	.6	4.3	7.5	2.2	1.0
From husband and wife.....	1.7	2.5	2.2	2.7	2.8	1.2
Total.....	99.2	100.0	99.9	100.1	99.9	101.0

Within these diverse and complex income packages, work remained quite robust for all women except those who were heavily dependent on welfare. The ratio of the head's earnings to welfare for this latter group was only about 9 percent. For all of the other groups, it was above 60 percent. For cyclers and women with only one welfare experience, earnings outstripped the importance of welfare as a component in the family's income.

We find evidence for both serial and simultaneous mixes of income from work and welfare. Our main findings about the role of work can be summarized as follows:

One measure of labor force participation is whether the head reports that he or she has an occupation. In 1971, for example, 48 percent of the heads of welfare families with long-term welfare attachment reported that they had no occupation. Only 17 percent of those who had other welfare-use patterns reported no occupation. By contrast, less than 10 percent of never-welfare, low-income heads reported that they had no occupation.

While they were on welfare, heads of the families in all welfare categories except the continuous, long-term welfare families generally earned between 24 and 27 percent of their families' income. The latter earned marginal wages that accounted for only 6 percent of their families' income. However, these women were only 12 percent of the ever-welfare group.

When we add together the earnings of all members of the family (i.e., wives and children, as well as family heads), we find the reliance on earned income for all welfare categories was much higher.

During the years when women did not receive welfare, the contributions of the head's earnings to family income increased to about two-thirds for three of our welfare patterns, and to somewhat more than half for those who were cyclers and for those who were long-term recipients with less than 50 percent of income from welfare.

Heads' earnings accounted for 70 percent of the seven-year incomes of the families who never received welfare.

The mix of work and welfare has received substantial attention in the legislative arena and in the academic literature. On the other hand, the mix of welfare and transfers and of earnings and transfers for those not on welfare have been somewhat neglected. Over the seven-year period of time, including years on and years off welfare, women with short- and long-term welfare experiences received about one-quarter of their income from all forms of public and private transfers, and the ratio of welfare to total transfers was over two-thirds. (The figures in this paragraph are not derived from Table 7.) Transfers were much less important for the 12 percent who had substantial dependency upon welfare and for those who only received welfare once in seven years. Sixteen percent of the income of the first group came from public transfers and about 11 percent of the latter group. Their transfers-to-welfare ratio was substantially lower than for the other welfare groups (21.6 percent for those who were heavily dependent and 53.6 percent for those who received welfare only once).

The importance of the transfer world becomes even more evident when we look at the incomes of women in years not on welfare, particularly those who cycle. Public and private transfers (nonfactor income) account for more than 30 percent of family income for this group. This 30-percent figure is important for understanding the nature of cycling in that it means we must consider cycling not only as a pattern of moving in and out of the labor market but also as one of moving in and out of different transfer systems. As people enter and leave the welfare system, the combination of transfers they rely on changes. The ratio of transfers to earnings, when the people we studied were not on welfare, varied between 25 and 57 percent. It was highest for cyclers and for those who were most heavily dependent on welfare (57 and 45 percent respectively), but it was also quite important for the remaining welfare types (26 percent). For families in three of the five welfare groups, the proportion of transfers to other sources of family income increased significantly when they moved off welfare. However, for those women who used welfare for a short time and for those who used it longer but not heavily, the importance of transfers declined. This is largely because they were unlikely to receive food stamps when they went off welfare. This declining reliance upon the use of food stamps appeared to prevail for all welfare types, perhaps with the exception of those who received welfare only once; but, in turn, they offset their loss of food stamps by increasing their use of other transfer payments.

During the years when women were not on welfare, their income packages were surprisingly similar to those of low-income women who never received welfare during the seven-year period. In three of the five welfare types, the earnings of the household head accounted for at least two-thirds of the total family income. Never-welfare women received about 70 percent of their family income from the earnings of the head of the family. By contrast, those who cycled and those who had been heavily dependent when they were on welfare relied on the head's earnings for only about half the family income. The relative importance of the earnings from wives and from children in the family was also much the same for ever- and never-welfare women, with the exception of those with long-term and heavy welfare dependency.

We have paid a great deal of attention to women who are highly dependent upon welfare. Now we comment upon the income packages of women who received welfare only once during the seven-year period. Not only did these women earn about twice as much income as those who were on welfare for long terms and were heavily dependent upon it, but they also earned about 30 percent more income than those in the remaining three groups. This is a very heterogeneous

group, however. Almost half (46 percent) received public transfer for four to seven years. For them, welfare served as a port of entry to other forms of public benefits. Others, generally at the high end of the income scale, experienced some economic crises and looked to welfare as a kind of net to catch them. When they recover, they will not return to welfare or any other form of public transfer. This is, at least, the pattern they exhibited during the seven years for which we have data. Another group to be included are women who, along with their husbands, headed families in the low to middle portion of the income distribution and were caring for a child who was not their own.<sup>13</sup>

SUMMARY<sup>14</sup>*Long-term vs. short-term use of welfare*

One good source of information on the numbers of people who use welfare for various lengths of time is the biennial survey of a one-percent sample of AFDC cases. In this survey, the caseworkers are asked to report the number of months since each case in the sample was opened. Table 8 displays the answers to this question from the surveys of 1967, 1973, and 1975. The indications are that, since 1967, about 25 percent of the AFDC cases enrolled at a given time have been on for over five years.

Table 9 shows three different ways of looking at duration of welfare use, all based on the University of Michigan Research Center's PSID. The first column is comparable to data from the AFDC survey data in Table 8, except that it is based on the use of any kind of public assistance, not just AFDC (although AFDC is still the major contributor). The percentages in the first column of Table 9 are likewise quite similar to those in Table 8.

TABLE 8.—DURATION OF TIME ON ASSISTANCE: AFDC SURVEY

[Percent of administrative records]

Length of most recent stay on welfare	Percentage of cases on assistance in a given month		
	1967	1973	1975
Up to 1 yr.....	31.2	30.2	27.7
1.1 to 3 yr.....	29.2	34.5	27.3
3.1 to 5 yr.....	14.0	16.9	18.8
5.1 to 10 yr.....	17.3	12.8	19.3
10.1 yr and over.....	8.2	5.0	6.3
Unknown.....		.4	.6
Total.....	100.0	99.8	100.0

TABLE 9.—DURATION OF TIME ON ASSISTANCE: PSID

Length of time on welfare	Percentage of families of ever-welfare women aged 18 to 54 in 1968		
	(1) Most recent stay on assistance for those on in a given year	(2) Total time on assistance for those on in a given year	(3) Total time on assistance for those ever using assistance
Up to 1 yr.....	28.0	22.4	18.3
1.1 to 3 yr.....	28.6	26.9	33.0
3.1 to 5 yr.....	15.9	16.6	22.0
5.1 to 10 yr.....	18.9	23.4)	27.0
10.1 yr and over.....	8.5	10.6)	
Total.....	99.9	99.9	100.3

Source: See app. A.

<sup>13</sup> In 1973 about 10 percent of AFDC administrative cases had no adult beneficiaries. The child receives the benefit whether it is a foster child or a related child living with relatives.

<sup>14</sup> Prepared by the Staff of the U.S. Department of Health, Education, and Welfare.

A weakness of the AFDC survey figures and the figures in the first column of Table 9 is that both refer to the most recent continuous use of welfare. What the table cannot reflect are the previous episodes on welfare of some of the people whose most recent entry to welfare was in the last year or two. The calculations reflected in the second column of Table 9 attempt to adjust for this by taking account of time spent on welfare previous to the current episode, which comes from information on the numbers of people who cycle on, off, and on welfare. This column shows that 34 percent, rather than 27.4 percent, of those on welfare at any given time are likely to have had more than five years of welfare at some time in their past and present.

The percentages in the first two columns of Table 9 are based on the number of families on welfare in a given year. The third column uses the number who ever use welfare over a period of several years, here the average experience of those entering the welfare system at some time during the six years of the panel study. Once again, the probability of their staying on, entering, or leaving the welfare rolls is projected from their experience during the panel years. With the number of people who ever use welfare as the base for finding these percentages, the proportion of long-run welfare recipients drops from 34 percent (of those on the rolls in a given year) to about 27 percent (of those who ever use welfare). These two numbers represent two reasonable answers to the question: "How many of those on welfare are 'long term'?"

#### *Long-term use and heavy dependence*

Most talk about the "welfare class" (persons who stay on public assistance for long periods) takes for granted that those who use welfare for long periods of time depend upon it for most of their income. They are not just "long-term;" they are "long-term, dependent."

In fact, not all people who use welfare, even for a long time, get most of their income from welfare. Estimates of just how important public assistance is to women on welfare for any length of time up to seven years are provided by the seven-year PSID. To estimate the numbers of women who are "long-term, dependent," Table 10 combines the information on the duration of the welfare experience (from Table 9) with information on the degree of dependence on welfare. For the purposes of this paper, "dependence" has been defined as the use of assistance for more than half the total income during the years when any assistance is received. "Long-term" is defined, as in the previous discussion, as the use of assistance for five years or longer. According to these definitions, about 20 percent of women on assistance at any given time are "long-term, dependent;" about 15 percent of those who have ever used assistance can be characterized in this way.

The estimates on Table 10 of the size of the long-term, dependent group rest, of course, on particular definitions of "long-term" and "dependent." Two other estimates, based on different definitions, are given in the body of this paper. One shows that those who use welfare for at least four out of seven years and for over half of their seven-year total income make up 12 percent of the ever-welfare group. The other estimate is based on a projection of the women's welfare experience over 10 years: If long-term dependence is defined as the use of welfare for nine or ten years out of ten and half the income during those years from welfare, then the proportion of people who are long-term dependent is only about 9 percent.



TABLE 10.—DURATION AND DEPENDENCE ON WELFARE: PSID

Number of years on welfare	Duration, per- cent of those on welfare in a given year	Dependence, <sup>1</sup> percent col. 1 who use 50 percent of family income in years on welfare	"Long-term, dependent," percent
1	22.4	12.1	
2	15.5	29.9	
3	11.4	34.2	
4	9.0	43.4	
5	7.6	25.5	
6	6.5	41.0	} 20.2
7 and over	27.5	67.4	
Total	99.9		
	Percent of those who ever use welfare		
1	18.3	12.1	
2	18.4	29.9	
3	14.6	34.2	
4	11.1	43.4	
5	10.9	25.5	
6	10.9	41.0	} 15.3
7	16.1	67.4	
Total	100.3		

<sup>1</sup> See app. B.<sup>2</sup> Of those on assistance in a given year.<sup>3</sup> Of those who ever use assistance.

In sum, various estimates of the size of the long-term dependent group range between 10 and 20 percent of the welfare population depending on just how "long-term dependence" and "~~welfare~~ population" are defined.

## APPENDIX A

This Working Paper explains how the PSID was used to generate estimates of duration and degree of dependence on welfare. The third column of the paper's Table 3 is the same as the third column of Table 9 in the Summary. However, the first two columns in Table 9 are not taken directly from the author's tables and need some explanation of how they were derived. This is done in Table A.

Table A: Duration on Welfare for Women  
Found on Welfare in a Given Year

Number of Years on Assistance	Women on Continuously (X)	Women on Before (.2X)	Women not on Before (.8X)	Estimated Number of Years on the Previous Time (Percentages)						
				1	2	3	4	5	6	7
				18.3 ↓ 36.7 (of .2X)	18.4 ↙ 14.6	14.6 ↙ 11.1	11.1 ↙ 10.9	10.9 ↙ 10.9	10.9 ↙ 16.1	16.1
-1	750.0	150.0	600.0	600.0	55.1	21.9	16.7	16.4	16.4	24.2
1.1-2	450.0	90.0	360.0	415.1	33.0	13.1	10.0	9.8	9.8	14.5
2.1-3	315.0	63.0	252.0	306.9	23.1	9.2	7.0	6.9	6.9	10.1
3.1-4	235.0	47.0	188.0	240.9	17.2	6.9	5.2	5.1	5.1	7.6
4.1-5	189.0	37.8	151.2	204.0	13.9	5.5	4.2	4.1	4.1	6.1
5.1-6	151.0	30.2	120.8	174.8	11.1	4.4	3.4	3.3	3.3	4.9
6.1-7	120.9	24.2	96.7	159.4	8.9	3.5	2.7	2.6	2.6	3.9
7.1-8	96.8	19.4	77.4	121.4	7.1	2.8	2.2	2.1	2.1	3.1
8.1-9	77.4	15.5	61.9	95.2	5.7	2.3	1.7	1.7	1.7	2.5
9.1-10	61.9	12.4	49.5	75.7	4.6	1.8	1.4	1.4	1.4	2.0
10.1 +	227.1	45.4	181.7	282.5	16.7	6.6	5.0	4.9	4.9	7.3
	<u>2,674.1</u>		<u>79.7</u>							
				<u>2,675.9</u>						

The first column of Table A shows the numbers of persons found on welfare in a given year whose most recent stay was one year, two years, and so forth. It is based on the PSID estimate that 1.5 percent of the approximately 50 million 18- to 54-year-old women, or 750,000, enter welfare each year, and that their probabilities of staying on continuously in the future, as demonstrated in the text, are .60, .70, .75 and so forth. Thus, after the first year 450,000 women will remain for two years, and 315,000 for three, and so on down the column. As can be seen the number estimated to be still on welfare after entering the system ten years before is 61,900. The number of those still on from more than ten years ago is estimated to be 227,000, for a total number of women on in a given year of about 2,675,000. This is the number used as the base in percentaging the first column, percents which are used in the first column of Table 9.

However, as we have noted, years of most recent continuous stay is not the same as total duration of time on welfare. Because some recipients return for a second (or even third) stay, their previous stays on welfare, as well as the most recent stay, must be counted in estimating the total length of time they spent on welfare. The rest of Table A shows how these additional cycles were estimated to produce the second column of Table 9 in the Summary.

Crucial for these estimates is the fact that 20 percent of those coming on in a given year have been on before (Table 10). The process for obtaining this figure from the PSID is as follows: The total number of women entering the welfare system between 1968 and 1973 is 537 (out of a population of 3,086). Some 87 of these are people who were on welfare in an early year of the study, then dropped off to come on later for a second time. This means that the 537 entries are made by 450 entrants, 87 of whom come on twice (or had been on already in 1967, dropped off, and came on again in between 1968 and 1973).\*

The estimated proportion of those entering the welfare system in a given year who had been on in earlier years but not in the year immediately preceding is 19.3 percent (87/450). We round up to 20 percent for the reason that the proportion of reentrants would likely rise slightly if the number of years of observation were extended.

To obtain the total welfare careers for this 20 percent who are on welfare in any given year, their prior careers must be added to their most recent continuous stay. To estimate these numbers, the estimates from Table 3 of total welfare careers for a cohort of starters were used together with a small (probably too small) adjustment.

Table A shows how the calculations for this adjustment were made. The first row of the table shows that 750,000 entered welfare during the last year. Some 20 percent of these, or 150,000, are assumed to have had a prior stay on welfare while the remainder, 600,000, have not. The rest of the first row is a partition of the 150,000 with a prior welfare career into those who have previously been on welfare anywhere from one to seven years.

If we assume that the prior welfare careers of these 150,000 were distributed the same way the total welfare careers are distributed (Table A, top) we would say that 18.3 percent had previously been on for one year, 18.4 percent for two years, and so forth. However, these previous careers are clearly not equivalent to total careers for people who cycle. Therefore we need to reduce the estimates of prior careers by subtracting one year from their estimated total careers. This is not as much of a reduction as it should be, but doing any more would be very complicated. Our figures, as we have calculated them, at least will show the maximum amount by which the distributions of "most recent stay" and "total stay" on welfare could differ.

The column in Table A labeled "Diagonal Totals" shows the total welfare careers of those now on welfare. They were estimated in the following way: The first number, 600,000, is the 80 percent of the 750,000 new entrants in year 1 (this year) who have not had prior welfare careers and hence have been on for

\*We ignore the rare cases of third entries.

a total of just one year. The second number, 415,100, is the estimate of the number now on welfare whose total welfare career to date is two years. It consists first of that proportion of the 450,000 who came on for the first time two years ago and stayed on, namely 360,000. To them must be added those who just came on this year but who also had a previous welfare career of exactly one year. If we were using the authors' estimates of the proportions of persons with each total of welfare careers as our estimate of the prior careers of these persons, we would add 18.3 percent of the 150,000 from last year who had been on welfare, or 27,500. However, as mentioned, there is an obvious problem with using these estimates. The prior careers of those on welfare for a second time is obviously less than their total careers. Therefore, to find their prior welfare use we subtract one year from each person's total welfare use. The exception is that for those who the authors say have one total year, we say they also have one prior year (not 0).

So we will add to the 18.3 percent of 150,000 from last year who had been on before for one year, the 18.4 percent of the 150,000 from last year whom the authors estimate had total welfare careers of two years. We're saying their prior welfare career was no more than one year: 18.3 percent plus 18.4 percent equals 36.7 percent. Since 36.7 percent of 150,000 equals 55,100, this is the number of persons coming on this year who had prior welfare careers of one year. They are to be added to the 360,000 women who entered the system two years ago and stayed on to find how many women now on welfare have total welfare careers of two years: 360,000 plus 55,100 equals 415,000.

Other numbers in the column of Table A labeled "Diagonal Totals" are obtained in similar fashion. For example, the 306,900 who had total welfare careers of 2.1 to 3 years consist of the 252,000 fresh entrants three years ago who stayed on, plus the 33,000 and the 21,900 entrants of more recent years who had previous welfare experience.

As mentioned, the totals almost certainly overestimate the total length of stay of the 20 percent who are on for a second time, so that the overall distribution shows more long-termers than there actually are. The exercise is worth doing because it demonstrates that the distribution does not change a great deal from the distribution which does not try to take account of prior welfare use by those on for the second time.

#### APPENDIX B

Tables 5 and 7 show the degree of dependence on welfare for five different types of welfare recipients. However, they do not show exactly the kind of detail which was used in deriving the second column of Table 10 in the Summary. That detail is presented here in Table B. It shows degree of dependence on welfare during welfare years, according to number of years on welfare.

It is perhaps interesting to note here the somewhat different numbers which can be obtained as estimates of the size of the "long-term, dependent" group, depending on what sample one uses and how one defines "long-term" and "dependent." In Table 10, those who are classified as "long-term, dependent" are estimated to be about 15 percent of all ever-welfare women. In Table 5, the figure drops to 12 percent. The former number is based on the synthetic cohort of entrants to the welfare system, "long-term" is defined as being on welfare six or seven out of seven years, and "dependent" is defined as being over half the income during those years from welfare. The 12 percent is based on the ever-welfare women actually counted in the panel study, "long-term" is defined as at least four out of seven years, and "dependent" is defined as over half the seven-year income from welfare.

TABLE B.—WELFARE AS A PROPORTION OF FAMILY INCOME IN THE YEARS THAT WELFARE IS RECEIVED BY THE NUMBER OF YEARS ON WELFARE

Welfare as a proportion of family income for years on welfare	Years on welfare															
	1		2		3		4		5		6		7		All	
	Number <sup>1</sup>	Percent <sup>2</sup>	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent	Number	Percent
0-0.30.....	225	78.9	70	55.1	31	37.8	19	35.8	18	35.3	8	14.3	9	9.8	380	50.7
0.30-0.50.....	25	9.0	19	15.0	23	28.0	11	20.8	20	39.2	25	44.6	21	22.8	144	19.3
0.50-0.70.....	22	7.5	25	19.7	14	17.1	17	32.1	9	17.7	11	19.6	31	33.7	129	17.4
0.70 and over.....	12	4.6	13	10.2	14	17.1	6	11.3	4	7.8	12	21.4	31	33.7	92	12.5
<b>Total.....</b>	<b>284</b>	<b>100.0</b>	<b>127</b>	<b>100.0</b>	<b>82</b>	<b>100.0</b>	<b>53</b>	<b>100.0</b>	<b>51</b>	<b>100.0</b>	<b>56</b>	<b>100.0</b>	<b>92</b>	<b>100.0</b>	<b>745</b>	<b>100.0</b>
50 and over (percent).....		21.1		29.9		34.2		43.4		25.5		41.0		67.4		29.9

<sup>1</sup> Number of women.

<sup>2</sup> Percent of women.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY,  
DEPARTMENT OF ECONOMICS,  
Cambridge, Mass., November 28, 1978.

Senator DANIEL P. MOYNIHAN,  
Senate of the United States,  
Washington, D.C.

DEAR SENATOR MOYNIHAN: I am enclosing written testimony for the record regarding recent testimony before your committee on the labor supply effects of a Negative Income Tax. I have also forwarded a copy to Michael Stern, staff director of the committee on finance.

I have done considerable research on the labor supply effects of two of the three urban NIT experiments: New Jersey and Gary. As I attempt to explain in the testimony, two outstanding statistical problems occur in these experiments: income truncation in the design of the original samples and sample attrition during the experiments. In the papers I refer to, these statistical problems are treated. Thus Professor Welch in his testimony before your committee was remiss in claiming that the truncation problem has not been addressed. Overall, I disagree with Professor Welch that the experiments have not been valuable in giving us new information about the likely response to a negative income tax. In no existing survey had we observed such high tax rates for male family heads who had low incomes. Thus, the existence of the NIT experiments has allowed us to estimate the response to these high tax rates.

In the testimony, I concentrate on the labor supply reduction of these male family heads since my research has centered on this group. It is interesting to note that the labor supply response in all three urban NIT experiments for a comparable group has been between 6 percent and 8 percent where properly measured. Dr. Cogan, who testified before your committee, has made two fundamental errors in his analysis: one logical and one statistical. Thus, his finding of much higher responses is fundamentally in error. Whether a 7 percent labor supply reduction is too large to permit implementation of an NIT is problematical. However, the response is certainly less than numbers I have seen reported from your committee hearings.

Yours,

JERRY A. HAUSMAN,  
Associate Professor.

STATEMENT OF JERRY A. HAUSMAN FOR THE SUBCOMMITTEE ON PUBLIC ASSISTANCE  
OF THE SENATE FINANCE COMMITTEE

*Labor Supply Response of Males to a Negative Income Tax\**

I. STATISTICAL PROBLEMS IN ANALYZING THE EXPERIMENTS

Two important statistical problems occur in analyzing results from the NIT experiments. The first and more important problem is income truncation, sometimes called sample selection. The samples were not randomly selected from a target population; rather, persons with low incomes at the time of the initial survey were more heavily sampled. For instance, the New Jersey NIT experiment was limited to persons with incomes less than 1.5 times the poverty limit. Since persons who were temporarily below their average earnings are more highly represented than persons who were temporarily above their average earnings, we would expect average earnings to be rising over time as a "regression towards the mean." Professor Welch in his testimony correctly pointed out that the truncation problem could be important in interpreting the experimental results. However, he was remiss in claiming that the problems had not been accounted for. In three papers I and my coauthor, Professor David Wise of Harvard University, have analyzed this problem:

- (1. "The Evaluation of Results from Truncated Samples: The New Jersey Income Maintenance Experiment," *Annals of Economic and Social Measurement*, 1976.)
- (2. "Social Experimentation, Truncated Distributions, and Efficient Estimation," *Econometrica*, 1977.)
- (3. "Stratification in Endogenous Variables and Estimation," Kennedy School Discussion Paper, 1977.)

\*Associate Professor of Economics, Massachusetts Institute of Technology, Cambridge, Mass. 02139.

The research in these papers has indicated that sample truncation can pose a serious problem if not accounted for. However, it is not particularly difficult to take account of sample truncation using modern statistical techniques.

The second statistical problem that must be confronted (not mentioned by Professor Welch) is sample attrition. Over a period of years individuals drop out of the sample: in Gary nearly 25 percent attrition occurred among males. Similar or higher rates of attrition occurred in New Jersey and Seattle-Denver. Furthermore, control individuals have a higher probability of attrition than experimental individuals since the former group is receiving no benefits. In a paper Professor Wise and I demonstrate how this problem can be treated.

(4. "Attrition Bias in Experimental and Panel Data: The Gary Income Maintenance Experiment," Kennedy School Discussion Paper, 1977.)

The question then arises whether, given the statistical problems inherent in analyzing the data and the results to date, have the experiments been worthwhile in adding new information to our knowledge of the response to introduction of an NIT? In my opinion they have. While the advocates of conducting the experiments in the late 1960s did not foresee the statistical problems that would arise, the problems can be treated satisfactorily. Furthermore, especially for male heads of household, the experiments have generated data not available in any other survey. These data provide evidence on how poor males will respond to a very high tax rate. The experiments and proposed NIT plans have rates up to 70 percent, much above what the federal taxes impose for people in this income category. Before the experiments most economists would have agreed on the direction of the response: a reduction in hours worked. However, only as a result of the NIT experiments have we been able to estimate the magnitude of the response. And it is, of course, the magnitude which is the crucial parameter for cost estimates of an NIT.

## II. RESPONSE OF MALE FAMILY HEADS TO AN NIT

Since male family heads have only been treated in a limited manner by existing welfare programs, their labor supply response to an introduction of an NIT is crucial. Female heads of household already are covered; but would extended coverage to male heads of household lead to a large labor supply response? It is remarkable that for comparable groups of male household heads, all three urban NIT experiments have had a mean response of between 6 percent and 8 percent. Yet Dr. Cogan has claimed before your committee that his analysis indicates a much higher response, on the order of 16 percent to 20 percent. We need to understand why his results differ.

The main point to understand is that Cogan has asked a different question from the other analysts, and it is not the correct question for public policy purposes. Cogan claims to "have found evidence of work reductions much larger than those found in earlier analyses of the same data." But previous analysts asked the question what is the population response to an NIT where the term "population" is that population represented by the sample enrolled in the experiment. Cogan has asked the conditional question: If individuals are below the breakeven point and receiving benefits what is their work response? While this question is well posed it differs from the previous question. It is also usually not the question we are interested in for the public policy context of the cost of an NIT plan. We want to know how an NIT is likely to affect the whole population of poor people, not just those below a certain income level. Thus Cogan has made a logical error in comparing his results to earlier results and claiming to find a larger reduction. Second, Cogan has committed a fundamental statistical error because he has not taken into account his artificial income truncation. Statistical techniques known since at least 1955 indicate that his response measure is biased so that the cost calculations he gives are also likely to be biased.

My best estimates of the likely work reduction by males arises from a paper written with a graduate student of mine, Gary Burtless (now at HEW).

(5. "The Effect of Taxation on Labor Supply: Evaluating the Gary NIT Experiment," Journal of Political Economy, 1978.)

In this paper we permit different responses in the population to introduction of the rest of the population will have only a small work reduction, but a very large statistically significant work response to introduction of an NIT is found to exist, although its magnitude is not especially large. However, the average response varies from 12.9 percent reduction for low paid people under the most generous NIT plan down to only a .3 percent reduction for high paid people

under the least generous NIT plan. Even for the same wage and same NIT plan, individuals are found to react with great variance. For example about 20 percent of the population of those considered will have almost a work reduction, most of the rest of the population will have only a small work reduction, but a very small part of the population will have a very large work reduction. Averaging over the population leads to the 7.7 percent estimate.

I do not know how large a reduction is too large for the purposes of national policy. Certainly almost all economists would expect some reduction, and we have found a small but significant reduction. This economic distortion may well be worth absorbing when reform of the welfare system with its many more striking effects is accomplished as a result of the introduction of an NIT.

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### FAMILY IMPACT AND WELFARE FORM<sup>1,2</sup>

(By Martha Phillips, assistant minority counsel, Committee on Ways and Means)

The United States has never articulated an explicit family policy despite its intense preoccupation with symptoms of stress in the institution of family life. Public policies nevertheless have innumerable implicit impacts on families, influencing decisions to marry or separate, to have children or not, to expand or contract household membership, to provide materially and emotionally for family members.

Because family life is a nearly universal experience, policy makers and their constituents frequently believe "they know a good one when they see it." Consequently, policy has frequently been based more on blind instinct and common sense than on hard data and clear objectives in many family-related areas. Unfortunately, recognizing a "good" family and prescribing a rational, consistent and explicit family policy, even within the context of a single income transfer program such as the one providing Aid to Families with Dependent Children (AFDC), are two vastly different propositions.

Family impact analysis is particularly pertinent to the welfare program because welfare dependency is most often precipitated by changes in family status and because welfare program requirements often appear to influence the decisions recipients make about family structure and functions. In the following pages, the concept of family impact analysis will be discussed. Next, the development of the welfare program over a forty-year period will be related in terms of some of its major intended and unintended family impacts. Then, six specific family issues frequently raised in welfare reform debate will be examined: preventing family breakup, repairing broken families, illegitimacy, child bearing by welfare mothers, paternal financial support, and working mothers. An attempt will be made to explore what is known about the existing and potential impacts of the welfare program on each of these areas and to clarify the desirability and feasibility of achieving specific family policy outcomes. Finally, the role of family goals in various welfare reform options will be assessed. The conclusion reached, briefly stated, is that although policy makers should indeed be cognizant of the family impact of current and proposed welfare policies and take particular care that they not adversely affect recipient families, it may be impossible to do much more than this. Frequently advocated family-related goals may, upon closer examination, not be either desirable or feasible, while other more important concerns may claim a higher priority in the development of income maintenance programs—adequacy of economic support, incentives for self-sufficiency, and equitable distribution of benefits.

#### FAMILY IMPACT

Family impact analysis as a technique is still in its infancy. Analysts are still wrestling with trying to determine "what is a family?" and "what is an impact?" Although measuring the impact of public policies on families can involve countless layers and angles of examination, it may be useful to divide family characteristics into at least four areas of particular public policy significance:

<sup>1</sup> The opinions expressed in this paper are those of the author and do not necessarily represent the opinion of any Member of the Committee on Ways and Means.

<sup>2</sup> This paper was prepared for the Working Group on Conceptual and Moral Issues in Welfare Reform under the auspices of the Center for Philosophy and Public Policy of the University of Maryland. It will be included in forthcoming volume of working group papers.



*Structural elements:* decisions regarding marital status (marriage, divorce, separation, desertion, cohabitation); children (having one's own, adoption or foster care of someone else's children, abortion, sending one's children elsewhere), household composition (nuclear family, extended family, or "augmented" families which include nonrelatives) and reliance on and contact with kin living outside the household.

*Economic functions:* providing for basic material needs of family members.

*Nurturant, health and socializing functions:* encouraging and supporting children's physical, intellectual and emotional growth and development and providing all family members with psychological sustenance, opportunities for expressions of intimacy and socialization into the wider community.

*Coordinating and mediating functions:* orchestrating and scheduling family activities, linking family members with services and benefits and programs in the "outside world."<sup>1</sup>

The impact of government programs on these family characteristics eludes easy definition. To the extent that programs make decisions more or less costly or change non-financial incentives to undertake certain behaviors regarding family status or functions, they can be deemed to have a family impact.<sup>2</sup> Relative family impact can be measured against several standards: behavior under proposed changes versus behavior under the existing program, behavior under the existing program compared to behavior in the absence of any program, or behavior under proposed changes versus behavior in the absence of any program. The concept of impact does not require eliminating all costs or incentives associated with specific decision; a program has an impact if it makes participants or potential participants more or less favorably disposed to act in certain ways or if it increases or reduces costs of various behaviors. Any behavior, however, is certain to be the more or less rational result of many incentives, motivations, preceptions and pressures, acting in combination. Of course not all of the pressures or incentives affecting families are related to the program, and non-program motivations frequently prevail. Often, incentives go unheeded because program recipients have scant knowledge of the options available to them. Thus, changing what appears to be the dominant program element having an undesirable impact may not result in any or very much change in behavior of program participants.

Even defining what constitutes a favorable or unfavorable family impact is confused by the fact that by a particular policy or contemplated change may have uneven effects on various functional and structural aspects of family life. A few examples may make this clear. As will be discussed in succeeding pages, increasing families' ability to meet their economic support functions by increasing welfare benefits may also greatly increase the rate of marital dissolution, an outcome which, all else being equal, probably negatively impacts the nurturant functions. A strict welfare program work requirement may conflict with the ability of a welfare mother to fulfill the nurturant and coordinating roles although it may in the long run enhance her family's economic support capability.

Ideally, government programs should simultaneously facilitate maintenance of stable, well-functioning family structures. In reality, it is more frequently the case that a program addresses just one of the functional or structural areas relating to family life, and changes in the affected area then trigger responses in the family's handling of other roles or structural arrangements. Unpleasant as it is even to contemplate the trade-offs, the mere existence of government programs affecting any aspect of family life mean that value decisions must be made—either implicitly or explicitly. This would be true even in a fiscally-unfettered welfare reform climate. The imposition of stern budgetary constraints on federal welfare policy make consideration of family impacts all the more necessary.

In such an examination of the different potential impacts of welfare policies, it is helpful to remember that the primary function of the welfare program is to provide an economic safety net under eligible individuals and families sufficient to enable them to meet basic human needs—food, clothing and shelter. This economic function is the first priority of welfare. It is to be hoped that the economic function can be fulfilled in such a way as to minimize accompanying family impacts that are generally viewed as "negative," while building on

<sup>1</sup> Family Impact Seminar, *Interim Report* (Washington, D.C.: Institute for Educational Leadership, the George Washington University, April, 1978), pp. 36-44.

<sup>2</sup> Maurice MacDonald, Isabel V. Sawhill, *Welfare Policy and the Family* (Washington, D.C.: The Urban Institute, September 29, 1977), pp. 4-5.

those deemed to be "positive." But this may not always be the case, as in the example of the relationship between welfare benefits and marital dissolution. Such trade-offs, however, seldom have to be made on an either-or basis, and it will usually be possible to structure income maintenance in such a way as to encourage generally favorable outcomes for the preponderant majority of affected families.

The AFDC program has a number of elements that can be adjusted to achieve various family impacts: the level of welfare benefits in relation to the recipient's other income alternatives and the prevailing economic climate, the likelihood that individual meeting various criteria will receive benefits, the rate at which taxes and benefit reductions offset earned income, eligibility rules (i.e. must maintain a home for a dependent child), and definitions of assets and incomes. A number of meeting various criteria will receive benefits, the rate at which taxes and benefit reductions offset earned income, eligibility rules (i.e. must maintain a home for a dependent child), and definitions of assets and incomes. A number of family impacts have been hypothesized regarding these elements in the current AFDC program:

The availability of AFDC to single but not married mothers favors a decision to bear an illegitimate child rather than marry the child's father, particularly if he has doubtful breadwinning prospects.

Remarriage by an AFDC mother is discouraged, particularly if she is already receiving unreported assistance from the man in addition to AFDC. But, marriage between an unrelated man and woman already living together is encouraged to the extent that they are not required to forfeit AFDC benefits for the woman's child.

A decision by an unwed mother to move away from her parents' home and establish her own household is encouraged by the higher benefits she receives as the head of her own household than as her parents' dependent and by the extent to which these benefits offset the cost of achieving privacy.

A regular pattern of child support by absent fathers is discouraged by reducing AFDC benefits to the children by the amount of paternal payments, thereby eliminating any net positive benefit to the child.

Work registration requirements for adults when their children reach school age may be an incentive to bear or informally adopt another dependent child in order to maintain eligibility.

The program strongly encourages the birth of the first child, the presence of whom is required for AFDC eligibility.

The addition of subsequent children to the family is somewhat encouraged since benefits for these children reduce the cost of their upbringing, even though such costs are not entirely eliminated.

Smaller living units or living alone is encouraged rather than large family or nonrelative groups to the extent that the differential between the costs of maintaining single and multi-person households are narrowed.

Resource-sharing among related or unrelated individuals would, if reported, reduce benefits and is therefore discouraged.

Including non-poor individuals in the living unit is discouraged since their income and assets would reduce benefits or entirely eliminate eligibility for members of the unit.

Extended family arrangements for the support of children are recognized to several degrees of kin relationship and encourage informal adoption of children by relatives.

The availability of benefits at adequate levels encourage husbandless mothers to keep their children with them and stay home to care for them while they are young.

#### GROWTH OF FEDERAL WELFARE BENEFITS FOR FAMILIES

This long list of often anti-marriage and pro-natal incentives is the result of four decades of incremental decisions regarding federal welfare policy. The federal welfare program began in 1936 as a public assistance model based on the exchange of involuntary helplessness of public support.<sup>3</sup> Subsequently, attempts were made to impose prevailing moral standards and norms on welfare recipients and to curb the phenomenal increase in numbers of dependents.

The original program was established as almost an afterthought to the controversial old age assistance legislation. Its aim was to assist economically-distressed widows with young children keep their families together during the tough De-

<sup>3</sup> Gilbert Y. Steiner, *Social Insecurity* (Chicago, Rand McNally & Co., 1966), pp. 112-14.

pression days when destitute women had few alternatives and when fewer than half managed to keep their children with them.<sup>4</sup> It was obvious that any federal funds provided them, however meager, would have a beneficial impact. It was equally clear that their husbands' death, which caused their plight, was beyond their control and not of their choosing.

In these early unambiguous years, 1936 through the end of the Second World War, the Aid to Dependent Children (ADC) program enjoyed warm political support. Congress even increased federal matching in 1939 to encourage greater state participation. Benefits were paid on behalf of dependent children, but not for their mothers or other caretakers, and children at first almost always entered the rolls because of their fathers' death. In 1943, however, absence of the father for the first time superseded death of the father as a cause of ADC dependency.

By 1950, absence of the father as a cause of welfare dependency was so prevalent that the possibility of negative family impact first showed up on the legislative record. The Director of the Bureau of Public Assistance testified before the Senate Finance Committee that \* \* \* "requiring that a parent be absent from the home before his children can receive assistance places a kind of financial premium on a broken home and exerts an influence exactly opposed to the purpose of the whole aid-to-dependent children program; namely to keep families together."<sup>5</sup>

In 1956, Senator John Kennedy unsuccessfully offered an amendment to include needy children of the unemployed, even though the father remained at home, arguing that, "when the father becomes unemployed, the family cannot receive aid to dependent children unless the father deserts. The Federal law thus puts a premium on desertion. I believe this is immoral and unsound."<sup>6</sup>

Five years later as President, Kennedy repeated these arguments partly in response to extreme and prolonged poverty in Appalachia. HEW Secretary Ribicoff testified before the Ways and Means Committee that, "there is no reason why a hungry child of an unemployed father should not be fed as well as a child in other unfortunate circumstances . . . what we are trying to do with ADC is keep families together. Certainly we should not, as a society, so conduct our programs as to discourage a family grouping or to encourage a parent to leave the home in order for his children to receive aid in their basic needs."<sup>7</sup>

These arguments prevailed at last, and the program of aid to dependent children of unemployed two-parent families began in May, 1961, available at state option. In 1962, the title of the program was changed to Aid to Families with Dependent Children (AFDC), and the Unemployed Fathers (UF) program was extended to 1967.

By 1967, the Unemployed Fathers (UF) was drawn tightly enough to negate its pro-family aspects. Eligibility for unemployment compensation, even in inadequate amounts, precluded UF assistance. The requirement of recent and substantial workforce participation was required, often excluded young fathers and the hard-core unemployed and was presumably an attempt to separate the deserving from the undeserving. UF programs were established primarily in industrial states which had already assisted this population. Attempts in 1967 to make UF mandatory for all states failed, despite arguments that the lack of the program encouraged family breakup. Today, although some 30 states participate, more than three quarters of UF families live in just seven states. Job search, training and registration requirements have kept rolls at the 100,000-range nationwide even in times of high unemployment.

The concern over family-splitting incentives that led to the establishment of the UF program also contributed to the creation of the social services adjunct to welfare benefits in 1966. These services, designed to avoid and reduce welfare dependency, were intended to help meet the official ADC goals of maintaining and strengthening family life and helping parents and relatives of needy children attain maximum self-support and personal independence consistent with the maintenance of continuing parental care and protection.

Accompanying the concern about family stability evidenced in the UF and social services programs was a cross-current of requirements and practices intended to force welfare families into conformance with prevailing

<sup>4</sup> Mary Jo Bane, *"Here To Stay: American Families in the Twentieth Century"* (New York, Basic Books, Inc., 1976, p. 13.

<sup>5</sup> Margaret Malone, *"Aid to Families with Dependent Children—Development of Program and Current Issues"* (Washington, D.C.: Library of Congress Legislative Reference Service, FD290, Mar. 13, 1968), pp. 9-10.

<sup>6</sup> *Ibid.*, p. 18.

<sup>7</sup> *Ibid.*, p. 19.

moral standards and norms. In 1950, a legal requirement was added to the Social Security Act that law enforcement officials be notified of desertions so that attempts could be made to locate the father and obtain child support.

Administratively, the "man in the house" rule was used by 19 states and D.C. to enforce moral standards on ADC recipients, and not incidentally to limit caseloads, before it was outlawed by the Supreme Court in 1968. The existence of a "substitute father," whether or not he actually contributed to the support of the children of the woman with whom he cohabitated was sufficient to deny ADC to the woman and her children. Midnight raids to search for such substitute fathers further discouraged ADC growth.

States attempted to crack down on illegitimacy-generated welfare cases. Louisiana cut off benefits for some 20,000 families whose homes were "unsuitable" because the dependent children had been born out of wedlock. The federal response in 1961 was to deny federal matching grants to states not assisting "unsuitable" homes unless other provisions were made to care for the children from these homes. To assist the states in making such provisions, federal payments were authorized for children placed in foster homes pursuant to court determination that the child's natural home environment was contrary to his/her best interests.

By 1967, the surge in welfare rolls and the transformation of the welfare clientele had sharply altered the political climate surrounding the program from one of compassionate beneficence to one of indignant parsimony. The program had metamorphosed into an income transfer program benefitting primarily families deserted by their men and, in rising numbers, families where no husband had ever been present. The legislation enacted that year reflects these altered perceptions. States were required to furnish family planning services to adult welfare recipients and to establish procedures for determining the paternity of AFDC children, locating absent fathers and collecting child support payments. A "freeze" was placed on the proportion of federal funding for children receiving benefits because of a parent's absence from the home, and although the freeze soon thawed, its enactment indicated increased concern over welfare subsidization of growing rates of actual or apparent desertion. Work requirements for mothers were enacted as a further attempt to reduce levels of welfare dependency. Under the Work Incentive (WIN) program all recipients were required to register for jobs and/or training unless they were legally exempt for various reasons, one of which was being needed in the home by children under the age of six.

The juxtaposition of solicitude for family stability with steps to compel moral rectitude on the part of welfare recipients continues today. In 1975, for example, Congress wrote a new "runaway fathers" law to establish paternity and secure child support payments. In 1977, President Carter made "incentives . . . to keep families together" one of his goals for welfare reform.

These conflicting policy goals are unlikely to be resolved. In fact, examining some of the welfare program family goals most frequently enunciated in the last decade demonstrates that their desirability and feasibility may not be as readily apparent as generally imagined. The impact of AFDC on either causing "undesirable" behavior or obtaining "desirable" behavior may be very slight compared to the impact of competing non-financial incentives outside the program.

The structural goals most frequently advocated in welfare policy debates have been:

- Keeping family units (husbands and wives) from splitting up;
- The "repair" of "broken" families;
- Discouraging poor people from having children out of wedlock; and
- Discouraging poor people from having more children than they can support.

Two economic goals compete with these structural goals for priority as welfare reform objectives:

- Encouraging assumption by fathers for support of their children; and
- Encouraging poor mothers to support their own families.

Each of these will be discussed in the following pages.

*Preventing family breakup:* The availability of AFDC benefits to one-parent but seldom to two-parent families is usually presumed to encourage fathers to leave home in order to qualify their families for benefits.

There is evidence however to indicate that at least some welfare-precipitated desertion is a statistical rather than real phenomenon. This "statistical desertion" occurs when the father claims to have moved out of the house but remains in

the immediate vicinity, continues to act as a father to his children, and provides whatever financial support he can.<sup>8</sup> The family gets both AFDC benefits and the father's support, making them better off than they would be under either welfare or the husband's wages alone. For these families, the financial incentives even under a welfare program supporting two-parent families will still favor "statistical desertion" unless benefits for two-parent families exceed those for single-parent families to such an extent that they surpass amounts needed to insure that, allowing for economies of scale, relative equity based on need is preserved.

Permanent marital dissolution (as contrasted to "statistical desertion") has been assumed to be related to the impact of low incomes on increasing marital dissatisfaction through unfulfilled male breadwinner role expectations and the frustrations of trying to meet family needs with inadequate resources. It could therefore be expected that increasing the income available to two-parent, low-income families would reduce their rate of family breakup. This could be accomplished through increasing male wages, female wages or welfare benefits. However, moving in an opposite direction from this "income effect" is the so-called "female independence effect" which occurs when wives through their own earnings, welfare benefits or other sources have income alternatives to continued dependence on their husbands. To what extent the "independence effect" causes marital dissatisfaction which culminates in marital break-up or merely facilitates the termination of what has already become an unsatisfactory situation is not clear and must await further study. A better understanding of this effect is crucial to a responsible assessment of welfare program family impacts.

A number of studies examined the impact of earned income and welfare benefits under the existing AFDC program on marital status.<sup>9</sup> There was some evidence that higher AFDC benefits, especially in relation to male earnings, were accompanied by slightly higher rates of female-headed families, particularly for blacks. But these studies taken together lead to the conclusion that the effects of AFDC on family dissolution are small and that allow-intact families to receive benefits "probably cannot be viewed as a major policy lever on family organization."<sup>10</sup>

Against this empirical background predicting neutral or slightly positive effect on family stability from an income transfer program providing benefits regardless of marital status, the initial findings of the federal income maintenance experiments were unexpected. When these experiments provided benefits to two-parent families with employed fathers, marital dissolution rates were substantially greater in the experimental group than in the control group not receiving benefits. In New Jersey, Seattle and Denver, the families on the least generous support plans experienced the largest increases in marital breakup, doubling their dissolution rate, while those on high support levels maintained their previous dissolution rates or experienced slight reductions.<sup>11</sup>

Several factors other than the structure of benefit amounts and eligibility requirements have been suggested to explain the results of the income maintenance experiments on marital stability. Recipients probably had far better information than "ordinary" low income individuals about the operation of the program and available options. This program did not carry the same stigma as a welfare program. Participants' knowledge that the experiment was only a few years' duration may have forced "now or never" decisions to dissolve unsatisfactory marriages.<sup>12</sup> It is also hypothesized that dissolution rates may have

<sup>8</sup> U.S. Congress, Joint Economic Committee, "The Concept of Family in the Poor Black Community," by Carol B. Stack and Herbert Semmel, Joint Committee Print, Study Paper 12, Part II (Washington, D.C.: Government Printing Office, 1973), pp. 275-305.

<sup>9</sup> Marjorie Honig, "AFDC Income, Recipient Rates, and Family Dissolution," *Journal of Human Resources*, Vol. IX, No. 3, Summer 1974, pp. 303-322; Joseph J. Minarik and Robert S. Goldfarb, "AFDC Income, Recipient Rates and Family Dissolution: A Comment," and Marjorie Honig, "A Reply," *Journal of Human Resources*, Vol. XI, No. 2, Spring 1976, pp. 242-260; Heather J. Ross and Isabel V. Sawhill, "Time of Transition: The Growth of Families Headed by Women" (Washington, D.C.: The Urban Institute, 1975); Phillips Cutright and John Scanzoni, "Income Supplements and the American Family," Joint Economic Committee Study Paper 12, Part I, pp. 54-89; and Stephen K. Mayo, "The Household Composition Effects of Income Transfer Programs," *Public Policy* (Summer 1976).

<sup>10</sup> Ross and Sawhill, "Time of Transition," p. 124.

<sup>11</sup> U.S. Congress, House, Committee on Agriculture, Committee on Education and Labor, Committee on Ways and Means, "Administration's Welfare Reform Proposal, Hearings," before the Welfare Reform Subcommittee, House of Representatives, on H.R. 9030, 95th Congress, 1st sess., 1977, Part III, testimony of John Bishop, pp. 1207-21.

<sup>12</sup> Bishop suggests, to the contrary, that a permanent program might have higher dissolution rates than a temporary one since families promised five-year benefits split up more frequently than those promised three-year benefits, *ibid.*, pp. 1213-14.

peaked in the early stages of the experiment and that under a permanent program dissolution rates would be lower.<sup>13</sup> But to the extent that these factors do not discount the results of the income maintenance experiments, welfare reform along the lines of the Carter Administration proposal, i.e. relatively low benefit payments and eligibility for two-parent, low-income families, would result in *more* rather than *fewer* broken marriages.

One explanation suggested for the high dissolution rates in the experiments is that the female independence effect operated more strongly in favor of dissolution than did the income effect in maintaining marriages. This may mean that, regardless of desirability of structuring a welfare system that discourages dissolution, it may not be feasible to do so simply by altering benefit levels and eligibility requirements unless benefits for two-parent families were made so much higher than those for single-parent families that the income effect would dominate the independence factor. Doing this would not only violate equity principles but would be too expensive to be considered a realistic policy option in the current fiscal climate. The draconian alternative of paring down the benefits for single-parent families relative to two-parent families moves away from rather than toward the priority goal of meeting basic human needs.

The importance of husbands' wages in encouraging marital stability offers an additional policy option.<sup>14</sup> Bishop argues that the income maintenance experiments may have increased marital dissolution because either husbands reduced their work efforts when receiving benefits or their low wages comprised such a small portion of family income compared to benefits that their male role performance was below par in the eyes of both husband and wife. He suggests that constructing a system that provided hidden benefits through (male) workers' paychecks would enhance the male breadwinner role performance and have a positive income effect without creating a female independence effect. He outlines a private sector wage subsidy device for providing income maintenance to married family heads which, although quite complicated administratively, has some precedent in the earned income tax credit.<sup>15</sup> However the earned income credit is available regardless of the worker's sex. It increases the earning power of women as well as men and thereby enhances the independence effect. Confining such benefits to men would doubtless prove highly controversial and unconstitutional in an era of feminism and two-worker families. Limiting the benefit to the primary wage earner, as the Carter Administration proposed for guaranteed jobs, avoids some of these problems but is nonetheless controversial from the feminist perspective.

In light of these overwhelming problems in structuring an income maintenance system which does not increase marital dissolution, one must decide whether this goal is worthy of such a stupendous effort. As far as public policy is concerned, this is primarily a question of the well-being of children who would be affected by divorce and desertion.

Although divorce, desertion and marital stress occur with greater frequency at low income levels than high, marital instability is pervasive throughout our society and about one-third of first marriages occurring in the 1970's are expected to dissolve. Even though remarriages usually occur, it is now estimated that four out of every ten children born in the 1970's will spend a part of their childhood in a one-parent family, usually with their mother.<sup>16</sup> When the better off segments of our society so frequently fail to achieve marital bliss, it may seem ill advised to place too high a priority on regularized marital patterns among people eligible for welfare. Since it is difficult to specify the alternatives to divorce—which occasionally mean reconciliation and a happy household but often involve stress, anger, physical abuse and an atmosphere destructive to children's well-being and nurturance—even a successful pro-marriage policy may not always achieve the desired favorable results.

<sup>13</sup> Nancy Brandon Tuma, Michael T. Hannan, and Lyle P. Groeneveld, "Variation Over Time in the Impact of the Seattle and Denver Income Maintenance Experiments on the Making and Breaking of Marriages" (Menlo Park: Stanford Research Institute, Research Memorandum 43, February, 1977).

<sup>14</sup> Lee Rainwater, "Poverty, Living Standards, and Family Well-Being," Joint Economic Committee Study Paper No. 12, Part II, pp. 209-218; Marc Fried and Ellen Fitzgerald, "Family and Community Life in the Working Class," Joint Economic Committee Study Paper No. 12, Part II, pp. 332-338; MacDonald and Sawhill, "Welfare Policy," p. 26.

<sup>15</sup> U.S., Congress, "Welfare Reform Hearings," testimony of John Bishop, 1222-37, 1245-82.

<sup>16</sup> Kenneth Keniston and The Carnegie Council on Children, "All Our Children" (New York: Harcourt Brace Jovanovich, 1977), p. 4.

The strongest evidence in favor of a pro-marriage policy is that two-parent families generally have higher incomes available for rearing their children. The burdens of children from the lowest economic groups have been repeatedly chronicled—lower cognitive development, poor health and emotional and social damage.<sup>17</sup> These economic effects on children's well-being, however, theoretically can be largely remediated through income transfers regardless of their parents' marital status.

Therefore, on both counts—feasibility and desirability—the goal of achieving marital stability probably ought not to be viewed as the top priority welfare reform goal. *Repairing "broken" families*: Encouraging single mothers to marry or remarry is implicit in many welfare reform agenda. This controversial goal is usually left unarticulated, however. It is fine to urge married couples to stay married, but persuading unwed mothers, divorcees and widows to find husbands to support them has, as a public policy, a calculating edge that does not make for good political soapbox oratory. Publicly espousing this goal may release a furor. In 1977, a leaked executive branch memo advocated that "for families in which there are small children and only one parent . . . the incentives should be arranged so that individuals prefer the two-parent arrangement. The earnings at work should be sufficiently greater than the dole on welfare to encourage . . . women who are single parents to remarry."<sup>18</sup> The immediate protests by the Women's Bureau, the Women's Lobby, the National Organization of Women, the National Welfare Rights Organization and other groups indicated that they rejected marriage as a solution to the welfare dilemma.

The AFDC benefit structure does not coerce women into marriage nor, since Supreme Court decisions in 1968 and 1970, does it penalize them for either marriage or cohabitation so long as their new mate is not the natural father of their children. Several studies indicate that availability and/or receipt of AFDC tends to reduce the probability of marriage for female family heads by about two percent over a five-to-seven-year period. Other non-AFDC factors such as the age and attractiveness of the woman as a potential marriage partner were more important in predicting remarriage rates.<sup>19</sup>

Given the relatively neutral impact of AFDC on (re)marriage decisions, it was not expected that the provision of benefits to one- and two-parent families under the income maintenance experiment would have much effect either. However, in the Seattle and Denver experiments, there were distinct ethnic group responses. Chicanos delayed remarriages, responding to the independence effect. Blacks increased marriage rates responding to the "dowry" effect created by permitting them to continue receiving benefits after marriage. White women exhibited no clear effect in either direction.<sup>20</sup> Thus, even allowing remarried mothers to enjoy higher family incomes than "still-married" mothers was not sufficient incentive to encourage (re)marriage in all ethnic groups.

Even though the structure of the welfare program appears to have at most a tangential impact on mothers' proclivity to (re)marry, this goal has still not been discarded. Some 83 percent of children receiving AFDC currently do so because their fathers are absent from home, and (re)marriage of their mothers would indeed reduce this public burden. Marriage to a "good provider" may be the best economic ticket for their mothers as well, given the below-poverty-level AFDC benefits available in most states and the dim prospects most welfare mothers have of getting good-paying jobs.

The desirability of (re)marriage as a welfare goal can be legitimately questioned, however. The high incidence of female-headed families, currently comprising some 14 percent of all families, means that such family types are no longer deviant or unusual. The lack of a husband may not deprive a mother's children of the support or presence of an adult male since cohabitation is not penalized and since many "absent" fathers maintain contact with their children. However, if the mother of an illegitimate child marries the child's father, she would, under current law, lose her benefits and be worse off than before, especially if the father had been supporting and seeing his family "on the quiet" as is often the case.

<sup>17</sup> Advisory Committee on Child Development, *Assembly of Behavioral and Social Sciences*. National Research Council. "Toward a National Policy for Children and Families" (Washington, D.C.: National Academy of Sciences, 1976), pp. 38-39.

<sup>18</sup> U.S., Department of Labor, Office of the Assistant Secretary, "Memorandum for Secretary Marshall from Arnold Packer on Welfare Reform Briefings," Mar. 14, 1977 (Washington, D.C.).

<sup>19</sup> MacDonald and Sawhill, "Welfare Policy," p. 32.

<sup>20</sup> Tuma, Hannan and Groeneveld, "Variation Over Time of Income Experiments on Marriages," pp. 19-24.

The quality of the relationship between the stepfather and stepchildren is at least as important as the financial outcome. "Having a man in the house is not always an unmixed blessing for the children," one observer noted.<sup>21</sup> The mother herself must also consider the trade-offs between the increased emotional and financial benefits likely to be associated with (re)marriage and the extra demands on her time and energy for household production.

Thus neither the desirability nor the feasibility of getting welfare mothers to marry is clear cut. It may be theoretically possible, as has been suggested, to make welfare benefits so low that welfare mothers would have no choice but to marry, but such a benefit level would sharply conflict with the concept of a benefit level adequate to meet basic needs, particularly for those families where the mother was unable to find a mate. For this and feminist reasons, such a policy is politically impossible.

*Illegitimacy*: Illegitimacy is often presumed, not illogically, to be an undesirable side effect of the welfare system. In 1973, some 46 percent of AFDC families had at least one child born out of wedlock. Because many unwed parents cannot earn adequate incomes, some 60 percent end up receiving AFDC at some time while some 80 percent of the out-of-wedlock children not legitimized or adopted receive welfare. Welfare eligibility is conditioned on having a dependent child. AFDC reduces the cost of raising children. It offers women pregnant out-of-wedlock an economic alternative to marriage. Case studies and interviews of young women with illegitimate children suggest that for some girls, setting up their own household supported by welfare seems like an improvement in their lives, giving them adult independence, recognition and autonomy and an escape from unhappy homes. This option has a degree of economic utility as well for the girl who is a poor student, has dismal employment prospects and whose child has been fathered by a youth unable to provide economic support.

The preponderance of available empirical evidence<sup>22</sup> seems to suggest that these theoretical incentives associated with AFDC benefits and eligibility have slight impact on illegitimacy rates while other factors such as increased education, availability of contraception and abortion, and geographic region are the significant variables. This was particularly true of studies making gross comparisons of illegitimacy rates and state AFDC levels,<sup>23</sup> but studies which took into account other variables similarly found that AFDC levels were less significant than other factors.<sup>24</sup> Even an effort to trace the chain of events and decisions which culminate in an illegitimate birth failed to find any conclusive relationships between illegitimacy and AFDC programs.<sup>25</sup> AFDC factors were found to have nothing to do with whether a young woman became sexually active or became pregnant. As to the third and fourth steps—deciding whether to continue a pregnancy, and if proceeding, whether to marry and legitimize the birth—low AFDC benefits were linked to higher probabilities of abortion which reduced the rates of either legitimizing marriages or illegitimate births. High AFDC acceptance rates were, contrary to expectations, linked to a significantly lower proportion of out-of-wedlock births. Even the availability of UF, which would give couples a means of support if the husband were unemployed, more strongly influenced abortion rates than marriage probabilities. Despite such meager evidence linking AFDC with illegitimacy, the theoretical incentives are so strong that analysts "are unwilling to reach a final conclusion that no such effect exists, and it is tempting to believe that their reluctance is justified."<sup>26</sup> Many policy makers concur.

Perhaps it is the desirability of avoiding out-of-wedlock births, particularly to poor women, that explains the persistence in seeking data to demonstrate that a welfare program could be structured to reduce illegitimacy rates. Not only do children born out-of-wedlock have a far higher chance of ending up on welfare, but their families as well are disadvantaged by generally lower incomes, greater marital instability, lower educational attainment, and a larger eventual family size.

<sup>21</sup> Andrew Billingsley, "Black Family Structure: Myths and Realities," Joint Economic Committee Study Paper No. 12, Part II, p. 317.

<sup>22</sup> An exception is the finding of Barbara S. Janowitz that for nonwhite younger women larger welfare payments are associated with higher illegitimacy rates. "The Impact of AFDC on Illegitimate Birth Rates," *Journal of Marriage and the Family*, Aug. 1976, pp. 483-94.

<sup>23</sup> Phillips Cutright, "Illegitimacy and Income Supplements," Joint Economic Committee Study Paper No. 12, Part I, pp. 90-138.

<sup>24</sup> Alan Fechter and Stuart Greenfield, "Welfare and Illegitimacy: An Economic Model and Some Preliminary Results" (Washington, D.C.: The Urban Institute, August 1973).

<sup>25</sup> Kristin A. Moore and Steven B. Caldwell, "Out-of-Wedlock Pregnancy and Childbearing" (Washington, D.C.: The Urban Institute, September 1976), pp. 28-87.

<sup>26</sup> Ross and Sawhill, "Time of Transition," p. 109.



The children have higher rates of mortality and morbidity and have an extremely high probability of ending up on welfare. Out-of-wedlock conceptions that force early marriage are related to a high incidence of later economic problems and divorce.<sup>27</sup>

The percentage of out-of-wedlock births has increased primarily because the total number of births has declined, but the number of illegitimate births—nearly half a million in 1976 out of 3.1 million total births—is not trivial considering the consequences of illegitimacy. The nonwhite population has a disproportionate 60 percent of out-of-wedlock births even though nonwhite babies comprise only 19 percent of all births. In 1976, 45 percent of nonwhite births were out-of-wedlock. Studies of family life in the black community suggest that although out-of-wedlock babies are loved and welcomed, their births are not consciously planned for or desired. Kin networks from both mothers' and fathers' families help care for and support the children, and more than two-thirds of the fathers of AFDC children in one study recognized their offspring.<sup>28</sup> While these strengths in family life in poor, black communities help families cope with the adverse effects of illegitimacy, it is doubtful that out-of-wedlock births have any favorable consequences outweighing those of legitimacy.

Thus, although there is general agreement that finding a way to avoid illegitimate births is desirable, there is nothing to indicate that the welfare system, no matter how organized, will achieve this effect. In the 18 years since Louisiana cut off welfare benefits to homes rendered "unsuitable" by illegitimate births, it has gradually come to be realized that denying or curtailing benefits for this reason would make the outlook for illegitimate children and their mothers even bleaker than it is already, penalize the children for a status over which they had no control, and, possibly, punish the mother for not choosing abortion over illegitimacy. A more acceptable alternative might be the suggestion put forth in the 1977 hearings that "incentives for marriage prior to the conception of children and the subsequent assumption by fathers of their legal and economic responsibilities to their children" should be created.<sup>29</sup> One such incentive advocated was the provision of job training and opportunities for younger minority males who are at the family formation stage. This notion implies that couples expecting an out-of-wedlock birth usually consider marriage seriously. A study in New York City indicated, to the contrary, that for more than three-quarters of the sample's women pregnant out-of-wedlock, the relationship was either marked by so high a degree of tension as to justify separation or the choice was not theirs to make. However, the fact that in 55 percent of the cases, welfare benefits exceeded the man's income means that AFDC cannot be ruled out as a possible explanation.<sup>30</sup>

In summary, reducing illegitimacy is an objective worth striving for but one which is probably beyond the reach of the welfare system. Indeed, it will probably continue to be the function of public welfare to provide an economic safety net for many of those suffering the consequences of illegitimacy, a fact which makes punitive measures unacceptable and renders positive incentives the only route to be explored. There is little cause to expect, however, that any welfare-related incentives will affect illegitimacy rates nearly as much as other factors outside the welfare system.

*Discouraging childbearing:* Since women are on welfare because their children cannot otherwise be adequately supported, and since children from low-income families face many drawbacks and difficulties, many believe that further childbearing by welfare mothers should be discouraged. The pro-natal incentives of the AFDC program are widely believed to encourage women not only to have the first child required for benefit eligibility but to have subsequent children in order to increase the size of benefit checks. Although the average welfare family

<sup>27</sup> Moore and Caldwell, "Out-of-Wedlock Pregnancy," p. 159.

<sup>28</sup> Stack and Semmel "Concept of Family," p. 287.

<sup>29</sup> U.S. Congress, "Welfare Reform Hearings," testimony of Robert E. Mitchell, National Council on Family Relations, Vol. IV, p. 2574.

<sup>30</sup> Blanche Bernstein and William Meegan, "The Impact of Welfare on Family Stability" (New York: Center for New York City Affairs, New School for Social Research, 1975), as discussed in Moore and Caldwell, "Out-of-Wedlock Pregnancy," pp. 72-75. Barbara S. Janowitz found that while male unemployment rates were never found to have a significant impact on illegitimacy rates, male income had a consistently negative impact, suggesting that marriage is discouraged and illegitimacy encouraged when fathers lack adequate means of support. However, the holding of this effect even for older (aged 30-34) women who almost never married to legitimize a pregnancy led Janowitz to conclude that the negative impact of income on illegitimacy may stem more from a positive correlation of income and contraceptive knowledge than from income and illegitimacy.

has only two children, 36 percent have three or more and 10 percent have five or more, percentages more than double those of the general population.

AFDC benefit levels for additional children reduce but do not eliminate the cost of these children to the family. Particularly as children grow older and more expensive, each additional child leaves the other members of the family unit relatively less well off economically than they would have been in his absence, particularly since total benefits for families in two-thirds of the states do not reach the amounts deemed sufficient to meet the basic needs. Each additional child further reduces the mother's chances of securing the employment that would enable her to escape welfare dependency since both the need for her household production and the difficulty of arranging child care are increased. Generally women on welfare indicate that they desire smaller families than do women not receiving welfare,<sup>21</sup> a recognition, perhaps, of these negative incentives to have more children.

The welfare program also has pronatal incentives as well. One possible positive incentive has to do with the disparity between how much people think is needed "just to get along" for families of various sizes and how much they actually receive in benefits. Rainwater found that by popular consensus a family of seven was seen as needing 46 percent more income than a family of two.<sup>22</sup> Nixon's welfare reform plan in 1971 would have given a seven-member family 250 percent more than a two-member family. Carter's 1977 proposal would have given a seven-member family \$6,000, double the \$3,000 for a two-member.<sup>23</sup> The current AFDC benefit structures in most states give between 100 and 250 percent more to eight-person families than to two-person families.<sup>24</sup> Despite the fact that the benefit amounts never exceed and often fall well below the basic needs standard, if, as Rainwater suggests, they are perceived by recipients to offer large families amounts sufficient to enable them to get along, they would constitute a pro-natal incentive. Several studies fail to show that the level of welfare benefits, or the level of income of the population in general has a clear relationship to fertility. However, there seems to be a positive relationship between the degree of certainty of receiving benefits and a woman's inclination to bear additional children<sup>25</sup> and between the family's income relative to their peers and the number of children they decide to have,<sup>26</sup> particularly as regards having four or more children.

The negative impact of additional children on the mother's flexibility and employability may not be as strongly perceived after there are already several children. After two or three children depending on their ages, the incremental impact on lifestyle and options is probably not very great and the positive values associated with having children may take over.

Overall, the possibility that existing or proposed welfare systems will have a pro-natal impact cannot be dismissed. But the possibilities of using welfare benefits to change this impact are nil. As in the case of illegitimate child bearing, non-welfare related incentives far outweigh the impact of welfare eligibility, benefits or availability. Reducing benefits per additional child to the level where they would be perceived as clearly inadequate (by placing limits of three or four persons in the filing unit, or not allowing filing unit sizes to increase after being entered on the rolls) would be neither politically acceptable nor in consonance with the concept of adequacy of benefits. The severe fiscal constraints on welfare preclude any notion of positive incentives such as "bonuses" for not becoming pregnant during the course of the year, and in any event, it is doubtful that such a bonus scheme would have much impact in the unlikely event that it found enough political acceptability to be adopted. Requiring even mothers with very young children to work might reduce an inclination to have another child, but again this might not be acceptable in terms of helping welfare families fulfill their nurturing functions. The possibility neutralizing pro-natal effects by allow-

<sup>21</sup> Pressner, Harriet and Linda Salsberg. "Public Assistance and Early Family Formation—Is There a Pronatalist Effect?" *Social Problems*, Vol. 23, No. 2 (December 1975) p. 227. cited in Moore and Caldwell, "Out-of-Wedlock Pregnancy," p. 77.

<sup>22</sup> Rainwater, "Poverty, Living Standards, and Family Well-Being," pp. 235-239.

<sup>23</sup> For a not-expected-to-work mother and her children.

<sup>24</sup> As of July, 1977.

<sup>25</sup> C. R. Winegardner, "The Fertility of AFDC Women: An Econometric Analysis," *Journal of Economics and Business*, Vol. 26, No. 3, Spring, 1974, pp. 159-166.

<sup>26</sup> Fred W. Reed, J. Richard Udry and Maxine Ruppert, "Relative Income and Fertility: The Analysis of Individuals' Fertility in a Biracial Sample," *Journal of Marriage and the Family*, November, 1975, pp. 799-804; Stephen J. Bahr, Bruce A. Chadwick and Joseph H. Stauss, "The Effect of Relative Economic Status on Fertility," *Journal of Marriage and the Family*, May, 1975, pp. 335-342.

ing childless, single and married people to receive benefits was proposed in Carter's 1977 plan but was quickly dropped by Congress as too expensive. The present controversiality of allowing government subsidization of abortions eliminates that option, at least for the foreseeable future. Perhaps the most effective, feasible approach is the one already being pursued—family planning services, which in 1973 were received by some 11 percent of all AFDC families.

*Paternal support:* Family economic functions are at the crux of the welfare program. One of the most frequently discussed welfare goals is reducing the necessity for public assistance for female-headed families by getting the absent fathers to support the children.

This problem was unforseen when AFDC began. Then widowhood accounted for most AFDC cases; today it accounts for only four percent. Marital instability is the leading cause of welfare dependency and illegitimacy is steadily gaining. Eventually most single mothers marry or remarry, but many become dependent on welfare before their children are grown.

Data reviewed and analyzed by Jones, Gordon and Sawhill indicated that only one-fourth of AFDC mothers have child support orders and another 6 percent have informal agreements. Amounts are in the range of \$7 to \$9 per week per child but even at this low level payment performance is poor. Only 45 percent of mothers with payment orders received regular payments (though not necessarily for the full amounts) according to one national survey; in the AFDC sample only 22 percent of the fathers were in full compliance and another 33 percent were in partial compliance. Roughly 40 percent of all divorced, separated and single women have never received financial assistance from the fathers of their children, and the 60 percent getting some support includes many who received irregular, partial or short term payments. In a given year, only about three percent of all eligible female-headed families receive enough child support or alimony along to put them over the poverty level.

The reasons for this abysmal child support situation involve short-comings of the legal system, the inability to establish paternity, the unwillingness of some women to have any connections with the fathers, and the fathers' job stability and health. Interestingly, the fathers' record in providing payments does not seem to be much affected by their total earnings or the ratio of support payments to those earnings.<sup>37</sup>

States have tried, albeit somewhat ineffectively, to enforce support orders through court action, garnishment and inter-state enforcement agreements. The federal government has vigorously pursued this objective with the so-called "runaway fathers" program enacted in 1975. Federal funds support state efforts to establish paternity, locate fathers and secure their payments to AFDC mothers and, on a voluntary basis, non-AFDC mothers. AFDC mothers must help establish paternity as a condition of receiving benefits. In 1977, some \$250 million was spent by the program to collect \$818 million, half of which was on behalf of AFDC recipients. HEW reported that in that year, 41,000 AFDC cases were closed or reduced in size because of child support collections. The Child Support Program has been acclaimed such a success that in July, 1978, HEW Secretary Califano announced "Project Responsibility" the purpose of which was to double AFDC child support collections in hopes of achieving a \$1 billion-per-year collection rate by the end of Fiscal 1979.

There are several reasons why this program has been favorably received, the most obvious being the generally-held belief that *both* parents have a duty and obligation to support their children to the best of their ability and to rely on public assistance only if absolutely necessary. Krause observes that the public welfare system could assume the support of children of shirking fathers when there were only a few of them. But with illegitimate children and female-headed households becoming the predominant situation in many cities, our institutions must find alternatives or they will break under the weight of excessive responsibilities. In short, we must return to enforcement of individual support obligations if we are not willing to pay for public support of a large percentage of our children.<sup>38</sup>

Over the long term, there is the additional possibility that as the inevitability of paternal support becomes generally perceived, it will cause people to avoid having more children than they think they can support. Knowing that he has

<sup>37</sup> Carol Adalre Jones, Nancy M. Gordon and Isabel V. Sawhill, "Child Support Payments in the United States" (Washington, D.C.: The Urban Institute, Oct. 1, 1976).

<sup>38</sup> Harry D. Krause, "Child Welfare, Parental Responsibility, and the State," Joint Economic Committee Study Paper No. 12, Part II, pp. 255-274.

inescapable financial obligations to the children of his first marriage may make a man limit the number of children he has in subsequent marriages.<sup>39</sup>

On a more personal level, third-party enforcement and collection may secure greater compliance than depending on the ability of estranged and hostile ex-spouses to achieve voluntary compliance. Third-party intermediaries tend to neutralize the emotional factors involved in both giving and receiving support assistance.

Not everyone agrees, however, that paternal support obligations ought to be energetically enforced. It is argued that AFDC children will not only fail to receive increased support from vigorous enforcement, but that they may end up with less total income than before.<sup>40</sup> Welfare benefits are reduced dollar for dollar by child support payments, leaving the family with the same total income. But in many low-income communities, fathers provide occasional help, gifts and emergency financial assistance which, not being reported, supplement rather than supplant the mother's welfare payment. Acknowledgement of paternity also opens the way for a child to avail himself of the frequently widespread kin network of mutual financial, emotional and in-kind assistance. Blaydon and Stack argue that stringent welfare sanctions against low-income black fathers who are not contributing regularly to the support of their children may cause some fathers to deny paternity and thereby deny their children participation in their kin network as well. It is also suggested that vigorous enforcement would drain resources from the father's current family, forcing this second family onto the AFDC rolls, or if in a non-UF state, into poverty without AFDC unless the father leaves the second family too.<sup>41</sup>

Requiring the mother's cooperation as a condition of receiving AFDC means forcing her to answer questions which many deem to be severe invasion of privacy, particularly given the nature of the questions that must be asked in order to establish paternity.

Blaydon and Stack suggest that child support enforcement "is better done in a uniform and equitable way, with incentives as well as requirements for compliance." They seem to be suggesting that father's support payments should not be entirely offset by welfare reductions but should leave the family somewhat better off.<sup>42</sup>

In summary, we appear to have already decided to obtain paternal support for as many children as possible. Where the father has income sufficient to support all of his children this is clearly a desirable goal. Where he has inadequate income, there can be no happy solution short of strategies that result in increasing his income so that he (and his spouse(s)) can keep two or more families above the poverty level. Purposely looking the other way so that fathers can supplement AFDC benefits outside the system produces inequitable results. Moving toward a universal child support system, although not without advocates, is a solution currently far removed from political or fiscal reality.

*Maternal support:* Whether or not welfare mothers should be required to work is the newest family impact issues to emerge in the welfare reform debate and is far from being resolved. It had long been an unchallenged goal of welfare to permit mothers deprived of male support to remain out of the work force in order to care for their children. But several current trends are forcing a re-thinking of prior assumptions. A majority of wives and mothers with children under age 18 are now in the work force, many of whose incomes keep their families above the poverty line; and the equity of providing government subsidization to other, non-working women's full-time homemaking and child-raising activities is being questioned. The feminist movement has urged economic self-sufficiency for women because so many fail consistently to receive adequate or any economic support from their men. The accelerated pace at which female-headed households are being formed is testing the limits of the welfare system. These factors are

<sup>39</sup> Wisconsin even went so far as to require a man with a legal obligation to support children from an earlier union to get court permission in order to remarry. Permission would be granted only if the man could prove that he had met all support obligations for previous children and that they were not likely to become public charges. The Wisconsin law was struck down by the Supreme Court on January 18, 1978 by an 8 to 1 vote as being violative of 14th Amendment equal protection guarantees.

<sup>40</sup> Colin C. Blaydon and Carol B. Stack, "Income Support Policies and the Family," *Daedalus*, Spring, 1977, pp. 147-81; Stack and Semmel, "Concept of Family," pp. 292-301.

<sup>41</sup> Blaydon and Stack, "Income Support Policies and the Family," pp. 153-56.

<sup>42</sup> *Ibid.*, p. 156.

magnified by the ever-present desire to save money by keeping the welfare caseload to manageable proportions.

From the family impact perspective, there is no clearcut guidance on this issue. The economic functions interact with the nurturant and coordinating functions, and weighing the balance probably must be an individual, subjective process.

For many welfare mothers, the financial imperatives leave little choice about this decision. One of every three female-headed families in 1976 had poverty-level incomes despite receipt of welfare benefits. Of the 800,000 families headed by women living solely on welfare, 94 percent were below poverty level. Over 60 percent of the almost 600,000 who had earnings to supplement welfare were also below poverty level.<sup>43</sup>

Advocates of requiring welfare mothers to work contend that in the long run the favorable family outcomes will outweigh any negative consequences. A young woman or teenager with several small children and no husband can look forward only to a life of being ground down by the welfare system, inadequate income and eventual unemployment years hence when her youngest children are grown if she does not find employment now. As one observer put it, "Few people seem to grasp how very isolated and harrassed many AFDC mothers feel year after year, how little they know about how to improve their circumstances, and how ill-prepared they are to support themselves eventually."<sup>44</sup> Feminists contend that considering the low remarriage rates for women with young children, welfare mothers would do better for themselves and their children in the long run by sacrificing the present time/income benefits of welfare in favor of gaining a foothold in the workforce and a start toward self-sufficiency.<sup>45</sup>

Those who favor the mothers' work requirement minimize the impact on children. They point out that the negative effects of poverty on children have been well-documented, and increasing family income through mothers' work might alleviate many of these problems. Half of the nation's children under age 18 have working mothers as do half of children under age six in female-headed families, without notably disastrous results. And having the mother at home is no insurance against problems, witness juvenile delinquency and emotional problems even in affluent, one-worker, suburban families.

What little reliable evidence there is on the impact of day care on children does not indicate any harmful effects. One study found no ill effects of "typical" as opposed to ideal daycare on low-income infants.<sup>46</sup> A critical review of current research similarly concluded that although available research is limited in both quantity and depth, with several important unresearched or poorly-researched issues, what empirical knowledge there is does not indicate any adverse consequences of good quality infant day care on maternal attachment, intellectual or social development.<sup>47</sup> This lukewarm approval is corroborated by the fact that hundreds of thousands of mothers at every income level either willingly or reluctantly place their children in day care every year.

The arguments against requiring welfare mothers to work tend to counter-balance those in favor of requiring work. As a simple matter of economics it is doubtful that the family will come out ahead financially, at least in the short run. Vickery contends that economists have, in general, overestimated the income of AFDC families with working mothers by ignoring the value of lost household production and by including reimbursement for working expenses as income. By her computations, if the family values the mother's services in the home other than child care at only \$0.85, the AFDC family is still better off with the mother staying at home rather than going into the work force at \$3 an hour. Given the educational level, training and work readiness of many welfare mothers, it is doubtful that they could initially command more than this \$3

<sup>43</sup> Heather L. Ross, "Poverty, Women and Children Last," *Economic Independence for Women*, ed. by Jane Roberts Chapman (1976, Sage Yearbooks, Vol. 1, 1976), pp. 137-44.

<sup>44</sup> Winifred Bell, "AFDC: Symptom and Potential," *Jubilee for Our Times: A Practical Program of Income Equality*, ed. by Alvin L. Schorr (New York: Columbia University Press, 1977), p. 261.

<sup>45</sup> Janet E. Harrell and Carl A. Ridley studied the relationships among employed mother's work satisfaction and the equality of mother-child interaction and concluded that they were positively related to one another, a finding which suggests that the time/income trade-offs may not be as onerous as assumed. "Substitute Child Care, Maternal Employment and the Quality of Mother-Child Interaction," *Journal of Marriage and the Family*, Aug. 1975, pp. 556-64.

<sup>46</sup> Barbara Radloff, "Average Day Care: Harmful or Beneficial?," *Carnegie Quarterly* (New York: Carnegie Corporation, Vol. XXV, No. 3, Summer 1977), p. 6-8.

<sup>47</sup> Louise Silverstein, "A Critical Review of Current Research on Infant Day Care" (New York: Columbia University School of Social Work, Nov. 1977).

an hour, much less the \$3.50 to \$5 an hour Vickery calculates is needed to put the welfare family above the poverty threshold.<sup>44</sup>

Opponents of the work requirement reject the tentative findings that day care is not harmful. They point out, correctly, that because "bad" day care has been hidden from officials and its existence steadfastly denied by its providers, the effects of "bad" care have never been systematically studied. Only a small percentage of children are served by organized centers while the rest disappear into a wide range of unregulated informal arrangements about which very little is known. It is sometimes suggested that the only way to be sure children are not being harmed is to provide indisputably good care—which often carries an extortionary and unrealistic price tag of \$3,000 to \$4,000 a year per child. Subsidizing such care for several children so their mother can earn \$3 an hour is obviously out of the question. Some child development experts contend that only mothers themselves can provide mothering adequate to prevent psychological damage.<sup>45</sup>

The importance of the mother and homemaker was emphasized by witnesses at the 1977 hearings of the Welfare Reform Subcommittee who argued that mothers of older as well as younger children ought to be relieved of work requirements if their children had special needs because of handicaps or a "rough" neighborhood.<sup>46</sup> It was pointed out that the work requirement for mothers of older children ignores the continuing need for the mothers' home production. Witnesses questioned the wisdom of requiring a woman to care for someone else's home and children for pay while receiving little credit for doing the same thing in her own home.<sup>47</sup> Another opponent observed that AFDC is an investment in children and as such is designed to keep mothers out of the labor force. She believes it is unrealistic to expect more poor mothers to take on a wage-earning role to support their children and simultaneously effectively discharge homemaking and mothering roles.<sup>48</sup>

Federal policy has flirted with this issue but has avoided a clear decision. The WIN program requires all AFDC recipients to register for work or training, but exempts people for several reasons, including being needed at home. The Administration's 1977 welfare reform proposal included a work requirement for mothers whose youngest child was age six or older and required mothers of school age children to work only part-time in order to be available when their children were out of school. Principal earners—mothers in single-parent families—were eligible for subsidized training or job opportunities. Both of these approaches compromise between those who want no work requirement at all and those who would prefer a more stringent requirement. It may, however, be as far as the government can go, given the prevailing controversy over the proper weight to place on the economic versus other family functions.

*Options:* Although the marriage, child-bearing and work goals discussed above are the central family-related objectives involved in the welfare program, they are not the only ones. Whether or not childless couples—who by many people's definition constitute a family<sup>49</sup>—or single childless individuals should be entitled to welfare benefits if they are in circumstances comparable to those that entitle families with children to benefits is of special concern because of its equity and cost implications. How to treat households that take in non-dependent relatives, how to treat two or more families living in a single household and the age at which dependent children in welfare families should be considered to be self-sufficient are other such issues which raise interesting questions of equity and family structure. These issues, however, are peripheral to the six major concerns discussed in preceding pages.

Even focusing just on these principal family policy concerns leads to confusion and controversy. Changing family behavior in the economic "mainstream" as

<sup>44</sup> This does not imply that Vickery opposes work for welfare mothers: she merely illustrates the dubious initial financial benefits of doing so. Clair Vickery, "Economics and the Single-Mother Family," *Public Welfare*, Winter 1978, pp. 18-21.

<sup>45</sup> Selma Fraiberg, "Every Child's Birthright: In Defense of Mothering" (New York: Basic Books, 1978).

<sup>46</sup> U.S. Congress, "Welfare Reform Hearings," testimony of Child Welfare League of America, Vol. IV, pp. 2252-57.

<sup>47</sup> U.S. Congress, "Welfare Reform Hearings," testimony of Robert E. Mitchell, National Council on Family Relations, Vol. IV, p. 2653.

<sup>48</sup> Winifred Bell, "AFDC: Symptom and Potential," p. 268.

well as the cost of obtaining behavioral changes via an economic support system make it questionable which family goals the welfare system should pursue, if any. If welfare policy were to put first priority on family goals, we would soon find that what little agreement was evident in previous debates would evaporate. A few examples demonstrate the difficulty in combining welfare and family policy objectives.

We may want to provide families with adequate resources to nurture their children, but we might well balk at doing so if it encourages poor people to have more children than they would have had otherwise.

We value stable marriages as the most appropriate combination of economic, emotional and nurturant circumstances for raising children, but question whether women should be economic hostages to marriages that they would abandon—for profound or frivolous reasons—if they could afford financially to do so.

We understand a special obligation to make sure that the children who constitute the next generation are assured an upbringing that will enable them to be responsible, contributing citizens, but we resent being forced to subsidize children of “irresponsible” parents, particularly when so many others are following the growing trend toward voluntary childlessness.

Abortion and forced marriage as alternatives to illegitimacy provoke near-violent controversy, but we are almost equally distressed at the possibility that welfare provides women who are pregnant out-of-wedlock with an alternative role as female heads of households and may thereby encourage them to favor this option.

We want parents to be economically responsible for their families but we cannot decide whether mothers as well as fathers should be required to or guaranteed work.

At the root of these conflicting values is the tension between the impact of welfare on the structural characteristics of families and its impact on the economic and nurturant functions of families. Income assistance programs are intended to bolster families' economic functions so they can better attend to their nurturant responsibilities. But virtually every form of economic intervention either facilitates or encourages a change in family structure, at least to the extent that the incentives implicit in welfare programs are realized in behavioral outcomes.

The absence of a welfare policy that is overtly designed to include incentives strong enough to result in a conventional structure for most welfare families, i.e. a working father, a homemaker mother and dependent children, is probably explained both by the fact that agreement on these structural objectives is only superficial and by the realization that the incentives would have to be very strong indeed to overcome the other forces that enter into the decisions individuals make about marriage, children and work. The “conventional” family type comprises only about 15 percent of all families today in the United States, and the “typical family of four” makes up only about 7 percent. With so many people belonging to families that do not conform to the traditional standard, there is little likelihood of requiring such conformance of welfare families any more than of non-welfare families. The plurality of styles and cultures in the United States further precludes imposing family structure requirements on welfare recipients (above and beyond the existing one that usually denies benefits to two-parent families). The strong incentives that would be necessary to achieve structural results would undoubtedly be controversial. For example, efforts to deny benefits to families with illegitimate children and to freeze the number of families with absent fathers eligible for federal-state matching were short-lived when it became evident that they had a punitive impact on the children involved. Pro-family incentives which offer extra benefits to families conforming to a legislated norm would be challenged as inequitable.

In the absence of agreement and/or willingness to pursue a “pro-family” policy (in terms of family structure), some have advocated that we try to keep the impact of welfare at least at neutral as possible. The idea would be to design a system that did not raise or lower the financial or nonfinancial costs of making decisions with regard to marriage, child-bearing or child support. However, efforts to devise such a system with neutral incentives have run headlong into the interaction between economic assistance and family structural changes.

Systems which give either direct or tax-related benefits to individuals regardless of marital status enhance the "independence effect" which enables and perhaps causes women to leave their husbands. Any such system which includes benefits scaled to family size reduces the cost of raising children and can be said to have a pronatal impact. The same is true for schemes which involve subsidizing families while children are dependent in return for a pay-back tax after the children are grown. The only policy option which is unquestionably neutral with regard to incentives affecting family structure is to have no welfare system at all. Only then would the financial and nonfinancial costs of making decisions with regard to marriage, children, household composition and child support not be affected.

A second neutrality possibility exists, however. This would be to try to design a system which has little or no impact on behavioral *outcomes* even though the law and regulations appear to contain incentives predisposed to one structural type or another. Trying to describe what such an operationally-neutral program might look like leads to the conclusion that the existing patchwork of benefits is as neutral in its outcome as any other approach. Some of this neutrality is achieved by keeping potential recipients in the dark about the availability of benefits and the certainty of receiving them. The lack of information results from different benefit structures and eligibility requirements from state to state and administrative and regulatory practices that are obscure, arcane and frequently changing. This tends to reduce incentive effects in any direction and target benefits, instead, on individuals who become eligible for welfare for reasons other than the fact that they were responding to welfare program incentives. The low level of benefits in many states—often far below the levels required for meeting even basic needs—helps to dampen the "independence effect" and the pro-natal incentives that would be implicit in adequate or generous benefit levels. The 1968 and 1970 Supreme Court decisions make welfare marriage-neutral for women. They can keep their present husbands and still receive benefits by merely signing affidavits that their husbands have left home, and they can take new husbands or boyfriends without losing benefits for their children. About the only prohibited option is letting the father publicly assume his role as head of the family. Possibly the sole area where the existing system may have an incentive effect is the subsidization of out-of-wedlock births which encourage pregnant women to choose this option over abortion or marriage, although conclusive evidence that this is so has yet to be found.

The very things that make the existing program neutral in its family structure impacts also make it a target for reformers' efforts—low benefits, varying treatment of different family types, lack of coverage of childless individuals and single people, sporadic and uneven availability of benefits, bureaucratic obstacles, and general lack of information to potential and actual recipients about how the system works. Any efforts to remedy these characteristics of the welfare system will impact on family-related behavior. These outcomes may be desirable or undesirable, depending on one's goals for family life. Probably the outcomes will be uneven, involving tradeoffs between benefit levels, availability and program information on the one hand and a slightly increased rate of marital dissolution and female-headed families on the other hand.

These and other potential tradeoffs ought not be made in the dark. Potential family impacts of any proposed changes should be thoroughly considered and carefully monitored. To the extent that the costs of decisions regarding family structure and nurturant characteristics are altered by changing the income support system, these changes ought to be weighed seriously.

No clear guidelines can be offered in advance for deciding between different options based on family structure and family function impacts. First, not only is it virtually impossible to reconcile the conflicting values regarding the importance of achieving specified family structures, but when these conflicts are ranged against economic variables, any remaining vestige of consensus disappears. We would prefer, for example, that the welfare system not leave poor people worse off than they would be in its absence. But weighing the degree of well-being on both the scales of family structure and economic considerations may produce different results. It is doubtful that the public policy process is capable of or



should attempt to make the choice between the two. Marital instability, for instance, is frequently perceived negatively, and families that break up are seen as less well off than those that stay together. However, providing welfare to enable people without other income to escape from abusive marriages which threaten the well-being of the adults and children involved generally receives sympathetic approval even though it means the dissolution of a marriage. But when welfare benefits also facilitate the light-hearted abandonment of a marriage on a frivolous impulse, stern public disapproval ensues, particularly if children are involved. Similar examples could be cited with regard to having children and meeting child support responsibilities.

Second, the public policy choices will seldom be "either-or" situations, but rather will be questions of more or less. Increasing benefit levels may also be accompanied by slightly higher divorce or childbirth rates. Stricter paternal-support enforcement may slightly increase the number of fathers denying paternity. Simply providing better information about how the system operates may affect the rates at which people make various decisions with regard to family structure. Most of the time, however, these family decisions will continue to belong to the private realm.

Welfare began as a limited, small program to help children of widows. But it has grown to the point where "universal" income maintenance benefits are being considered which could affect a rather large segment of our diverse, pluralistic population. The highest priority ought to be placed on achieving an income support that is fair, equitable or to succeed and would probably be incompatible in many instances with the income support goals. Welfare reform, therefore, should concentrate on its principal function of providing economic support to individuals. This is not to say that as a society we will not continue to value families for many important reasons—including their unique capacity to function in economic, nurturing and coordinating roles. The welfare system, therefore, should be vigilantly monitored for any changes that it might cause in family structure and functions. In the rare instances where existing or proposed policies are found to have unexpectedly large, negative consequences for significant numbers of recipient families, alternative policies will have to be sought. More frequently, it will be the case that existing or proposed welfare policies will have only a marginally negative impact on family structural stability, on the nurturing or coordinating functions. In such instances, it will probably be preferable as a matter of public policy to continue to give the highest welfare system priority to providing financial assistance in the most equitable and adequate manner feasible and to regulate family impact considerations to a position of secondary importance.

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SURVEY RESEARCH CENTER,  
INSTITUTE FOR SOCIAL RESEARCH,  
Ann Arbor, Mich., December 14, 1978.

Senator DANIEL PATRICK MOYNIHAN,  
*Committee on Finance,*  
*Senate Office Building, Washington, D.C.*

DEAR SENATOR MOYNIHAN: I have started several times to respond to your letter of November 14 inviting comments for your Subcommittee on Public Assistance, and in the process have misplaced the press release setting forth the procedure for formal submissions. Since what I have to say can be summarized briefly, let me simply put it down here. If you want to insert the rest of this letter into the record somewhere, you may.

We have already published and submitted evidence that the persistently poor are a substantially different subgroup from those who are temporarily poor in any one year. This means that formulas for distributing funds between states, counties or even individuals may be badly biased.

See Richard D. Coe, "Dependency and Poverty in the Short and Long Run," in *Five Thousand American Families, Volume VI*, Greg Duncan and James Morgan, editors, Institute for Social Research, University of Michigan, Ann Arbor, Michigan, 1978.

We are about to publish evidence that short-run changes in work hours in response to changes in wages are much greater than longer run changes. In other words, people may reduce their overtime or second jobs or even cut back on work when their wage rates rise faster than inflation, but within a few years may be back looking for more work again. Their aspirations rise. The evidence points to the notion that people have target income goals which motivate them, rather than some marginal substitution between leisure and money-work.

See James Morgan, "Hours of Work by Family Heads: Constraints, Marginal Choices, and Income Goals," in *Five Thousand American Families, Vol. VII*, forthcoming. For a totally different method, see L. F. Dunn, "An Empirical Indifference Function for Income and Leisure," *Review of Economics and Statistics* 60 (November 1978), 533-540.

This means that even expensive experiments may exaggerate the amount of negative work-response to increased income, because they focus on relatively short-run effects.

But beyond these two substantive points, there is a much more basic problem. Most of the research and discussion on welfare is based on some very strong assumptions: that more market work is better than less, that divorce is a bad thing. Surely there are some who work more hours than they would like or than we might think best. We need to ask not for programs and policies that push in one direction, but that are in some sense neutral, not distorting people's choices unduly. I realize this may be hard to define but we sometimes see clear evidence when we go wrong, as in the present income tax treatment of married couples. If a program reduces the average work hours of a group of people, it surely makes a difference whether the reduction results from somewhat less overtime and second jobs, whether it allows some time to look for a better job, or whether it leads to increases in unpaid work caring for children, starting a business, or working a farm.

Even marriage is not sacred. We want programs that encourage people to live together and share (or at least do not unduly discourage them), but divorce is not always worse than the situation that led to it.

Many present programs attempt to recapture for the government, in the name of economy, most of the economic advantages of living together. Economies of scale are assumed in setting support standards. They also ignore differences in work and leisure, hence discourage non-money work, including child care, as well as money work. I have suggested elsewhere that a set of standards for income maintenance which included economies of scale only in caring for children, had a target well-being standard that included non-money income and leisure time (taking account of money and non-money work) would be much less distorting of people's choices about money and non-money work, and about who lived with and shared with whom.

See James Morgan, *Individual Behavior, Economic Analysis and Public Policy* (The Vladimir Woytinsky Lecture), Department of Economics and Institute for Public Policy Studies, University of Michigan, Ann Arbor, Michigan, 1978.

Sincerely yours,

JAMES N. MORGAN,  
Research Scientist.

BRANDEIS UNIVERSITY,  
Waltham, Mass., November 29, 1978.

Senator Daniel Moynihan,  
U.S. Senate,  
Washington, D.C.

DEAR SENATOR MOYNIHAN: I find wholly unsupportable the assertion made before your subcommittee by Dr. Robert Spiegelman and his colleagues that "The NIT program destabilizes marriage." The assertion is contradicted by the data presented by Spiegelman himself in your volume of November 1978. Moreover, data on the Seattle-Denver experiment presented to the House Committee on Welfare Reform substantially are at variance with Spiegelman's data. Rather than respond to your invitation to submit my own written testimony, therefore, I write this letter requesting further exploration by your subcommittee of the relationship between NIT plans and marital dissolution.

Spiegelman's evidence suggests contradictory outcomes of the SIME/DIME experiment. On the one hand, his data suggest that NIT plans, compared to some unspecified mix of programs facing the control group, promote higher rates of marital dissolution; although the most generous NIT plans are less destructive than the others (see his Table 8 in your volume of November 1978). By contrast, other information offered by Spiegelman shows that some NIT plans—the more generous ones—actually promote marital stability (see his Figures 1 and 2).

Data submitted by Dr. John Bishop to the House Committee appear to be at variance with the first set of numbers noted above. Bishop's data from SIME/DIME, as well as from two other NIT experiments, suggest that generous NIT plans promote marital stability. Almost uniformly, lower dissolution rates are apparent for the most generously treated experimental groups as compared with the control groups.

The findings of Spiegelman and Bishop suggest to me a trade-off for policy-makers: they can choose between reducing marital dissolution rates by incurring high transfer costs or increasing marital dissolution by offering meager welfare payments. This trade-off strikes me as consistent with your view of the matter from the time you published your monograph on *The Negro Family*.

May I suggest that your subcommittee carefully scrutinize the results regarding marital stability from the several NIT experiments. Premature acceptance of the interpretations offered by the Spiegelman group would be unfortunate.

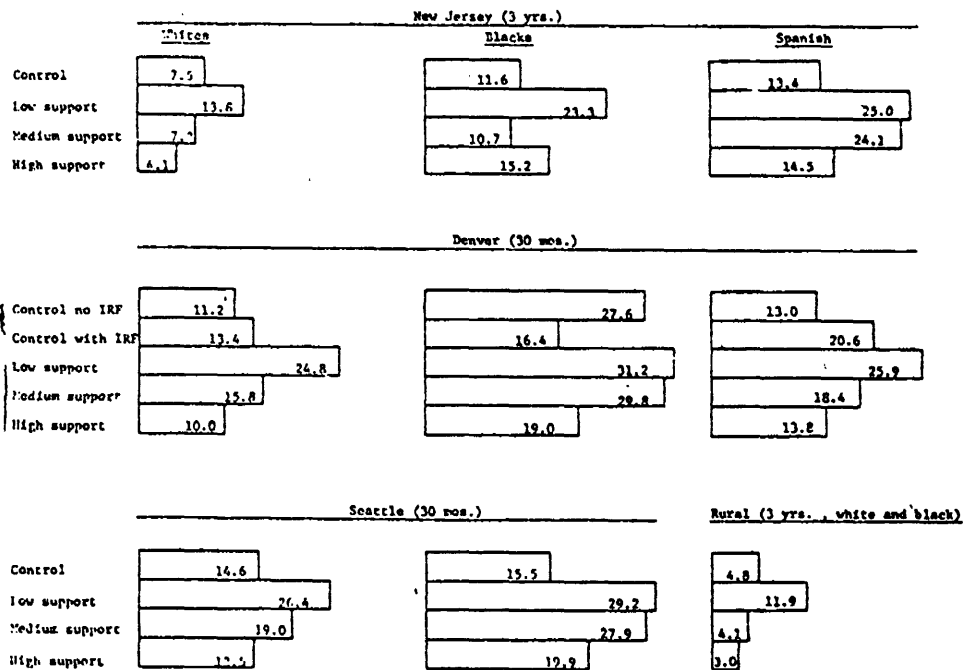
If I can be of further assistance to your subcommittee, I will be glad to discuss these matters at greater length.

Sincerely yours,

LEONARD J. HAUSMAN.

Source: John Bishop, "Jobs, Cash Transfers, and Marital Stability: A Review of the Evidence," written testimony submitted to House Welfare Reform Committee, October 14, 1977.

Figure 1. Marital Dissolution Rates in the Negative Income Tax Experiments



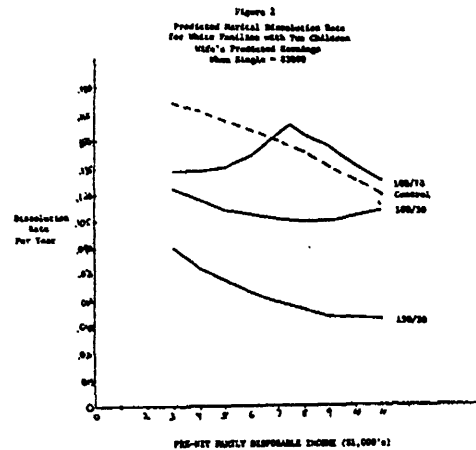
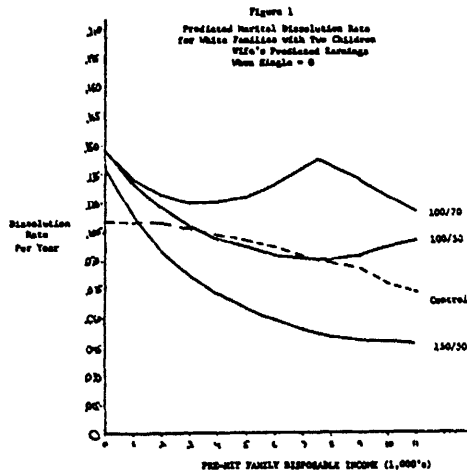
Note: Percent splitting is given at the top of the bar.

TABLE 8.—PERCENT CHANGE IN MARITAL DISSOLUTION RATE  
BY LEVEL OF INCOME GUARANTEE

Guarantee level	Race-ethnic group		
	Black	White	Chicano
90 percent of poverty line.....	<sup>1</sup> 67	<sup>2</sup> 96	60
125 percent of poverty line.....	<sup>2</sup> 93	<sup>1</sup> 55	-28
140 percent of poverty line.....	21	12	-35
Average of SIME/DIME program.....	<sup>1</sup> 61	<sup>1</sup> 58	-4
Number of cases.....	939	1,297	518

<sup>1</sup> Significant at the 0.05 level.  
<sup>2</sup> Significant at the 0.01 level.

Source: "Materials Related to  
Welfare Research and  
Experimentation" November 1978



STATEMENT OF WILLIAM J. GRINKER, PRESIDENT, MANPOWER DEMONSTRATION RESEARCH CORP.

For the past four years the Manpower Demonstration Research Corporation has been engaged in overseeing the operations and research of a large-scale demonstration designed to provide new insights into the relationship of work and welfare. That program is known as the National Supported Work Demonstration.

Supported work is a work experience program designed for persons who have severe employment disabilities: long-term AFDC recipients, poor youth who are high school dropouts with records of delinquency, ex-criminal offenders, and drug addicts. It is a program operated primarily by locally based nonprofit corporations that serve as a bridge employer; that is, these corporations serve individuals who cannot function effectively in a normal job situation. They offer the supported workers temporary jobs which can provide them with the type of experience and credentials necessary for entering the regular labor market and then succeeding in it. While supported work shares many features with other subsidized work efforts, such as public service employment and sheltered workshops, it is chiefly distinguished from other work experience programs by its high degree of structure and its reliance on three elements designed to make participants initially comfortable with the world of work and to accommodate their needs and attitudes toward work: per group support, graduated stress and close supervision. The first of these, peer support, is based on the theory that most participants in a new activity feel less anxious about their performance in the presence of people with similar disadvantages or fears, and that a significant proportion of what one needs to know about a job is learned through peer interaction. Graduated stress stems from the idea that getting and keeping a regular job is too difficult for certain people because they cannot meet the ordinary demands of the labor market. Through gradually increasing performance and productivity standards, it attempts to bridge the gap between what supported workers can do and what a job ordinarily requires. Supervision finally, represents the key link between the participant and the program, is chiefly responsible for the development of technical skills, for instilling positive work habits and attitudes, and for providing advice on work and personal problems.

Workers enter the program at salary levels pegged slightly above or at least at the minimum wage. Good performance is rewarded by small salary increases and bonuses, and prolonged inadequate performance results in firing. The jobs developed for them are not "make-work"; they are tailored to meet both the needs of the local market and the potential capabilities of the supported worker. And although the initial standards of job performance and attendance are somewhat less than they would be in a private sector job, as the worker gains experience and confidence on the job, these standards are gradually increased. The purpose of supported work is to instill in participants the habits and disciplines of work within a supportive structure so that he or she can ultimately develop into a *bona fide* worker and thence claim employment in a competitive society. Many participants are employed legitimately for the first time in their lives, and have their first real opportunity to develop two assets that are indispensable to getting and holding a job in the regular labor market: good work habits and a history of stable employment.

To find out the effects of the supported work program experience on this group of individuals, a rigorous research and evaluation design has been developed as a crucial and integral part of the national demonstration. There are four basic components of this research effort. First, we are testing a number of what are known as behavioral hypotheses, such as long-term earnings, recidivism, drug use, welfare dependency, and the like. To do this, for the first time in a national employment and training demonstration, a control group methodology is being used. Through a random selection process, supported work job applicants are assigned to either an experimental group which is offered employment, or to a control group which is not, although those in this group are willing to work and eligible in all other respects. Both groups are followed by statistical reporting and through periodic, confidential interviews for a period of up to three years. The total sample of people being followed through this method is about 6,500. The second component of the research is a benefit cost analysis. This is a fairly straightforward type of economic value analysis which will try to figure out what the program is worth to the taxpayer, to so-

ciety at large, and to the participants themselves. The third part of the evaluation is known as the process analysis, which is an innovative and experimental effort to try to figure out what features within the program seem to make a difference. For example, does the type of work, the type of supervisor, or the size of a supported work crew affect the performance of those enrolled? And the final part of the evaluation is what we call documentation, which is a look at some of the less quantifiable dimensions of the program, such as the effects of leadership and community factors, political structure, labor union involvement and the like, on program success.

For the past three and a half years we have been carrying out this nationwide supported work experiment in 14 local nonprofit corporations across the country. As of September of this year, approximately 9,000 people had participated in the program, and these sites now employ about 1,600 persons. Of the total number of people in supported work, a fourth had never worked before, and the average number of weeks worked in the year prior to enrollment was just seven. Average annual earnings were \$670. All members of the AFDC group, and a substantial percentage of the other three main groups were receiving welfare payments of some kind at the time of their enrollment. 86% of the combined ex-offender, ex-addict and youth groups had been arrested at least once, and 43% reported having used drugs regularly. 63% were Black, and only 30% of the entire population had finished high school. That this segment of the unemployed is unarguably the group for which supported work was designed is underscored by a comparison with the populations served by various CETA programs and by the WIN program, which show that the supported work population are far more disadvantaged as measured by length of continuous unemployment, educational level, minority status, prior criminal history and previous welfare dependency. Clearly a large portion of those working in the program form the nucleus of that somewhat difficult to define term: the underclass of American society. They are those who often considered alienated from the normal work ethic and those who are virtually impossible to place in employment.

We are still over a year away from the final evaluation of the program. However, from some early comparisons between participants and controls, we have drawn some preliminary conclusions regarding the population served, the early impact of the program upon those participants, and their attitude toward it and toward work in general.

One of the most important facts to emerge from the process of enrollment in the supported work program has been that large numbers of people who have widely been considered unemployable are, in fact, willing to take jobs when they are given the opportunity to do so. Furthermore, they are willing to accept jobs that pay the minimum wage or only slightly more, jobs which have no guarantee of skill training, jobs which offer little promise of advancement, and jobs which for the most part must be described as low-skilled and entry level. They are even willing to accept such employment knowing it will last no more than 12 or 18 months. The conclusion can only be drawn that there is no shortage of eligible and willing participants for a type of program like Supported Work.

The AFDC group in this demonstration is particularly one whose characteristics would not have promised great success. This is a group that, by definition, has been on welfare continuously over the past three years and have more children between the ages of six and eleven than do the average welfare recipients. Over 90% are from minority groups, and less than one third have completed high school. Their employment pattern offers little more hope; about 85% of the group have not worked at all in the last year, and about one third had never worked before. They have, therefore, little previous employment experience, particularly within the recent past, and limited skills with which to negotiate worthwhile employment. This group is, in fact, drawn from the "unassigned" pool of WIN registrants, for whom no other training or employment has been found or is planned. That they are indeed the most difficult to employ segment of the AFDC population can be seen by a comparison of the supported work AFDC women with the population generally served by WIN; it shows supported workers have a longer history of welfare dependency, less recent or a briefer work history, lower levels of education, a high proportion of Blacks, and a smaller proportion of young women. Realistically, then, their job prospects are poor indeed, but there are no lack of volunteers and at the present time, they form the largest segment of the enrolled demonstration population.

It can be argued that the legitimate alternatives to work for ex-offenders, ex-addicts and youth are few, and that their willingness to participate in supported work is therefore not so surprising. For the AFDC population, however, the alternative of welfare exists, so that in agreeing to participate in supported work, a woman at the least temporarily exchanges a certain, if not comfortable, source of income for an uncertain work future. This is even more difficult for them, for making the move from welfare to work entails additional adjustments in terms of child care, family life, and the possible loss of such benefits as food stamps, Medicaid, and for those living in subsidized housing, low rent. And although total earnings do increase through working, with the reductions in household welfare income, it has been roughly estimated that the net return to AFDC experimentals in the program is about \$1.85 an hour. The example of one supported worker in Hartford bears this out. Employed in a clerical-accounting job, she has income of \$160 a week from earnings and a small AFDC supplement for which she is still eligible because of her large family. This is compared to the \$400 a month she used to receive on welfare alone. However, the apparent economic gain is deceptive since her public housing rent has risen from \$60 to \$111 per month, and she now pays \$102 for food stamps which used to cost just \$60.

The early records for participation in the program show that AFDC supported workers do significantly better than their control counterparts for the first nine months after enrollment. Supported work enrollees reported working approximately 31 out of 36 weeks, while those in the control group only 4 out of 36 weeks. Average nine-months earnings for participants are almost \$3,700 compared to \$525 for controls; average welfare income was over \$1,100 lower during those nine months for participants than for controls, and about 10 percent of those enrolled went off welfare altogether. Somewhat surprisingly, less than 2 percent of those welfare mothers entering supported work, compared to 18 percent of the controls, said that making child care arrangements so they could work was a problem. Few from both groups reported using formal day care services, however. Most used baby sitters and some used other household members.

In the area of comparison between the AFDC supported workers and the other target groups within the demonstration, the findings continue to indicate a high interest in work. While in the program, the AFDC group had the highest work attendance rate of 90 percent, and the lowest percentage of firings, just 18 percent compared to the demonstration total figure of 38 percent. The AFDC women also stay in the program longer than any of the other target groups; their average time is 10 months. At this point of time, almost one third of them have gone on to permanent, unsubsidized employment, compared to about 20 percent of the other target populations. Only 11 percent have left the program because they didn't like the work experience and presumably preferred to remain on welfare.

AFDC women can be characterized in general as workers who are reliable and steady. They are, however, less likely to take the initiative in finding jobs for themselves. Only one fourth of those who gain employment find their own jobs, while half of the other target groups do so. This probably stems from a relatively low level of prior work experience, and their anxiety about entering the regular labor market. Supported work seems to provide them with a very important opportunity to acclimate themselves to the structure of the work setting, and a time in which to gain self-confidence.

The women's supported work jobs span a wide range of activities. In Chicago and New York, most hold clerical jobs; in several other cities they serve as nurses' assistants, kitchen helpers, daycare personnel or maintenance workers. In New York, most AFDC participants say they do not want blue collar or trade work, but many crews throughout the country are involved in work that is not usually done by females. In Massachusetts, for example, almost 27 percent of the AFDC women are engaged in construction-related activities, as are about 17 percent in Oakland. Overall, however, the majority of AFDC participants are engaged, during and after the program, in service-related activities. When they join the active labor force, their placement wage averages \$3.02 per hour, which is higher than their in-program income of \$2.74 an hour, but still lower than the average for all groups, \$3.59. This has been attributed to their shorter pre-program work experience and the general nationwide disparity between the male and female wage rates.

Supported work is financed by a number of sources. One source is the combined national government and private foundation funds which are granted to M.D.R.C. Grants from state and local governments form another. A third important source,



accounting for almost 20 percent of the total funds at present, is income which the sites generate from the production of useful goods and services. Income-producing work activities range from the manufacture of furniture in Hartford and Seattle, to the winterization of homes in Wisconsin and West Virginia to the maintenance of public and private buildings and facilities in Atlanta and New York. Finally, a number of the sites are now diverting income transfer benefits, which would otherwise have gone directly to the recipients, into a wage pool.

Diversion of such welfare benefits has developed into a funding source for supported work in nine of the national demonstration sites. Traditionally, our society has viewed such transfer payments solely as a way to provide income support to individuals and families who, for one reason or another, are unable to obtain full-time employment. It is altogether possible that income transfer payments can be used to help create employment opportunities through programs such as supported work on a more sustained basis.

The concept is quite simple: When persons receiving welfare are hired by a supported work program, they authorize the diversion of their income transfer payments to the program; those payments are then combined with other funds and used to pay wages for full-time supported work jobs. It is a concept which offers little risk and may produce substantial benefits. Recipients learn work habits and work skills while earning more money than they would have received directly from welfare. Recipients also have a better chance than they've ever had before to obtain permanent jobs and thus get off welfare completely. The income-maintenance system benefits because, with no additional financial outlay, it is investing in the production of meaningful goods and community services, and in job creation and job training which may lead significant numbers of long-term welfare recipients toward lives of self support.

AFDC benefits are now being diverted on a demonstration basis under the authority of section 1115 of the Social Security Act. As such, AFDC diversion requires site by site approval and is extremely complicated. There are a number of options available which could streamline this process and make it a more permanent fixture in an improved employment and income maintenance strategy. While this may require some changes in federal law or regulations, these changes are relatively minor and should probably be aimed only at the chronic AFDC recipient.

In conclusion, it seems clear, particularly from the examination of the AFDC supported workers' performance, that although many categorically determined "employable" individuals do not have the work skills or the work habits to make it in a competitive society, at least some of them do have the willingness, and often the determination, to work. Our experience with the supported work demonstration indicates that many such individuals can become competitive if just offered an opportunity to work in small groups composed of individuals from similar backgrounds and with similar disabilities; if offered low but gradually more stressful work assignments; and if rewarded more often than usual for successful job performance. This kind of work environment is not necessary for everyone on the welfare roles, but we believe it is appropriate for those who need some additional supports in order to compete effectively.

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THE URBAN INSTITUTE,  
Washington, D.C., November 29, 1978.

Senator DANIEL PATRICK MOYNIHAN,  
U.S. Senate,  
Committee on Finance,  
Washington, D.C.

DEAR SENATOR MOYNIHAN: Thank you for your invitation to submit testimony concerning the dynamics of welfare dependency.

We have completed several studies in which aspects of the current welfare system were evaluated. In one analysis, we explored whether aspects of the AFDC (Aid to Families with Dependent Children) program constitute an economic incentive to out-of-wedlock childbearing. In the second instance, we considered the impact of AFDC benefit levels and availability on marital dissolution and welfare receipt among respondents in the Panel Study of Income Dynamics (PSID).

In our study of out-of-wedlock fertility, we explored whether public policies affect the probability of premarital sexual activity among teenagers, the occur-

rence of premarital pregnancies and the outcome of premarital pregnancies among teenagers. In addition, we studied the association between state out-of-wedlock birth rates and public policies. In general, we did not uncover any patterns of evidence that provide statistical support for the notion that generous or easily available AFDC benefits represent an economic incentive for premarital pregnancy or childbearing. In one analysis, we found that women in states that cover unemployed fathers through AFDC are less likely to carry their pregnancy to an out-of-wedlock birth; however, the primary reason seemed to lie in association between coverage of fathers and more frequent abortion, rather than more frequent marriage.<sup>1</sup>

In the case of marital dissolution, a positive association between a state's AFDC benefit level and the probability of divorce or separation between 1972 and 1976 was noted among married couples in the PSID.<sup>2</sup> However, the magnitude of the effect is essentially trivial: a \$100 differential in the monthly AFDC benefit level across states was found to be associated with an increase in the probability of marital dissolution of 3 percent over the four-year time period. Furthermore, although multivariate controls were applied, the meaning of such an association is inevitably ambiguous. For example, "liberal" states may both have generous benefits and high divorce rates.

In a related analysis among female heads in the PSID survey, a \$100 increase in the monthly AFDC benefit was associated with a 4 percent increase in the probability that a female head would be receiving some form of welfare assistance; however, the association is not statistically significant.<sup>3</sup>

These studies did not address the possible impact of altering the current welfare system. However, the absence of large and statistically reliable welfare effects in these analyses—as in most other studies of which I am aware—suggests a conclusion that accords with common sense. People are unlikely to terminate a happy marriage or bear a child out-of-wedlock solely to qualify for government assistance. The economic incentive is not sufficient to prompt individuals to make such important decisions against their own best interests. Welfare may make it possible for an unmarried mother to support her child or enable a woman in an unhappy marriage to terminate the relationship; however, our work suggests that the net effects of any such tendencies are small.

In all of our analyses, other factors were found to be far more important determinants of people's behavior than the level or availability of welfare benefits. Affecting these other factors seems a preferable and more productive means of affecting welfare dependency. For example, in our study of out-of-wedlock fertility, teenage out-of-wedlock childbearing occurred more frequently among blacks where access to contraception was limited or restricted by age of consent laws and, among whites, where access to abortion was limited. Similarly, in our study of welfare receipt, we found that female household heads were more likely to receive welfare assistance if they had little work experience, low expected earnings, or large families, and if they did not receive alimony or child support. These findings suggest that implementation of equal employment opportunity, provision of family planning services, and enforcement of child support laws represent means of reducing the likelihood that marital dissolution will be associated with public dependency.

I am enclosing several relevant research articles for the use of your Committee. If I can be of any further assistance, please let me know.

Sincerely,

KRISTIN A. MOORE, PH. D.,

*Research Associate, Program of Research on Women and Family Policy.*

<sup>1</sup> Kristin A. Moore and Steven B. Caldwell, "The Effect of Government Policies on Out-of-Wedlock Sex and Pregnancy," *Family Planning Perspectives*, Vol. 9, No. 4, July/Aug. 1977.

<sup>2</sup> Kristin A. Moore, Linda J. Walte, Sandra L. Hoferth and Steven B. Caldwell, "The Consequences of Age at First Childbirth: Marriage, Separation and Divorce," *Urban Institute Working Paper* 1146-03, July 1978.

<sup>3</sup> Kristin A. Moore and Sandra L. Hoferth, "The Consequences of Age at First Childbirth: Female Headed Families and Welfare Reciprocity," *Urban Institute Working Paper* 1146-05, Aug. 1978.

THE CONSEQUENCES OF AGE AT FIRST CHILDBIRTH: MARRIAGE, SEPARATION AND DIVORCE

(By Kristin A. Moore, Linda J. Walte, Sandra L. Hofferth and Steven B. Caldwell)

THE URBAN INSTITUTE,  
Washington, D.C., July, 1978.

SUMMARY AND CONCLUSIONS

Although many teenage pregnancies and births, particularly among black teenagers, occur to young women who are unmarried, the vast majority of mothers have married by their early twenties. Indeed, a high proportion of marriages seem to be precipitated by pregnancy or birth. Not only is a current first birth highly associated with the probability of marriage, but a birth in the previous year is also related to a higher likelihood of entering marriage. Young women who have not married within two years of the birth of their first child, though, seem to experience a slightly lower probability of marriage.

The confirmation of a link between early pregnancy and early marriage confirms our everyday observations. A question of greater debate is whether the marriages formed under such circumstances are particularly unstable. The unique difficulties posed by the combination of early marriage plus parenthood are unlike those suffered by couples who simply marry young but postpone childbearing. In addition, couples who marry after or in response to a premarital pregnancy may face special difficulties. On the other hand, marriages between young people not yet done with their schooling and personal growth may be inherently unstable, while the presence of children may present many reasons to young parents to remain married. Several research strategies were employed with two national longitudinal data sets to approach these issues. The essential question is whether an early birth or an early marriage leads to a higher probability of marriage break-up.

The weight of the evidence that we have generated suggests that it is teenage marriage that is associated with a higher probability of marital dissolution. Regardless of the age of the mother at first childbirth and far more important than the timing of the birth relative to the marriage, the youthfulness of the couple, as measured by the wife's age, seems to be a critical determinant of divorce and separation.

In an initial analysis of young women interviewed between 1968 and 1972 in the National Longitudinal Survey of Young Women (NLS), the probability of having ever been separated or divorced by age 24 was found to be strongly affected by the woman's age when she contracted her first marriage. When age at marriage was controlled for statistically, age at first childbirth had no impact on the incidence of divorce or separation. A premarital first birth also had no effect on marital dissolution. A higher probability of having experienced marital break-up was noted among women with lower education, women living on the Pacific Coast, blacks, and young women from more recent birth cohorts.

Another analysis of the same NLS data explored the incidence of divorce over the survey years among young women who were married in the initial year. Again, age at first marriage was a critical predictor of marital dissolution over the period between 1968 and 1972, while age at first childbirth had no effect on marital stability. Also, black women, women with lower education, and women living on the Pacific Coast were more likely to terminate their marriages, as were women who themselves earned the majority of the family's income. Couples with relatively substantial assets were particularly likely to remain married. Net of these other factors, the husband's income, the presence of children under age three, the AFDC benefit level in the region of residence, marital duration, and the timing of the first birth relative to the marriage all had no impact on the probability of marriage break-up.

Because age at first marriage and age at first childbirth are so highly correlated ( $r=.71$ ), the NLS sample was broken down into sub-samples according to the woman's age at first marriage. Even among these groups of women all married at about the same age, the woman's age at first childbirth was not found to be related to the probability of divorce or separation. The model was found to explain marital dissolution considerably better among couples who wed when they were at least age 21, suggesting that more individualistic and idiosyncratic factors affect couples who marry at younger ages. Among couples married when the wife was 17 or younger, the only factors found to predict the marital stability were the number of years already married, race and parental socioeconomic

status. Whites were found to be less likely to experience separation or divorce, while wives from higher status families are more likely to end their marriages if they wed while teenagers. Among couples who married at closer to the usual age at marriage, having financial assets, the presence of a young child and being white all lessen the likelihood of divorce.

Because of the importance of race to the likelihood of divorce and separation in these analyses, separate regressions were conducted for NLS whites and for blacks. This analysis clearly indicates the importance of age at first marriage in predicting marital instability among whites. Whites who wed as teenagers experience a significantly higher probability of divorce and separation. However, when age at marriage is controlled, age at first childbirth has no positive impact on instability; if anything, white teenage parents have a lower probability of divorce, net of age at marriage. Among blacks, both early marriage and early parenthood predict to a slightly higher probability of marital instability, but neither effect is statistically significant.

The association between age at first childbirth, age at first marriage, and marital dissolution was then examined with a second national longitudinal survey, the Panel Study of Income Dynamics (PSID). Respondents in this survey were of all ages, and it was not possible to ascertain whether respondents had ever been divorced in the past. Consequently, the experience of divorce and separation over the years 1972 to 1976 was examined. Again teenage marriage but not teenage parenthood was associated with marriage break-up. However, many of the couples in this sample were sufficiently old to have experienced both divorce and re-marriage. Therefore, a smaller sub-sample was identified, composed of only those couples married for ten years or less in 1972, and the incidence of marital break-up among these couples was examined.

In this sample, too, couples who wed when the woman was a teenager experienced a considerably higher incidence of divorce and separation over the period from 1972 to 1976. Again, teenage childbirth was negatively associated with the probability of marital break-up, when age at marriage was controlled. Also, the timing of the first birth relative to marriage had no statistically significant association with marriage break-up, nor did the presence of young children, husband's income, husband's education, or the duration of marriage. Higher female wages and a higher unemployment rate in the local labor market, predict to a higher probability of marital dissolution, while better-educated wives tended to experience fewer break-ups.

In a final analysis, the year-by-year probability of marital dissolution was examined with each data set, and no evidence was found to suggest either that a current first birth or a past first birth serves to significantly increase the probability that a marriage will end.

In sum, none of the analyses conducted on these data sets indicate that teenage childbearing increases the risk of marital dissolution later in life. Moreover, women experiencing a first birth before or in the same year as marriage are not more likely to subsequently experience a marriage break-up. However, this does not mean that teenage childbearing is unrelated to the incidence of divorce and separation. As noted earlier, many marriages are entered during the teenage years under the press of an early pregnancy or birth. Certainly many of these young marriages would never have been formed or would not have been formed at the time if the pregnancy had not occurred. And our analyses strongly indicate that marriages entered during the teenage years are far less likely to succeed. Furthermore, divorce when it occurs may impose greater economic hardship on the family if the young mother has failed to complete her education and acquire work experience and if the young father has curtailed his education in order to support a family. Young parents may also fail to acquire the assets and education that seem to serve as a buffer against marital break-up. In addition, the hardship imposed by a divorce or separation probably tends to be greater when children are involved than when a childless couple splits up.

Our finding that teenage marriages appear to be particularly prone to end, whether the young couple have children or postpone the first birth, should certainly be evaluated using other research strategies and other data sets, particularly data sets that permit controls for whether the first birth was intended at the time it occurred and which permit analysis of whether a person has ever experienced a divorce or separation. However, the conclusion from these analyses is clear. The marriages of women who first wed during the teenage years are less viable than those of older brides. This relationship is not accounted for by the association between teenage marriage and teenage parenthood, by the relatively poor economic prospects faced by those who wed while very young.

or to differences in family background, social, or demographic characteristics associated with early marriage. Early marriage itself appears to be responsible. Given this finding, the current trend toward delayed marriage is a hopeful sign, one which may signal a decline in the frequency of divorce in the future.

#### FEMALE HEADED FAMILIES

The recent rise in the incidence of female headed families has concerned policy makers because nearly half of all families headed by a woman are in poverty. Although some of these families originate through the death of the husband, most are formed by divorce or separation or, to a lesser extent, by an out-of-wedlock birth. Since teenage births often precipitate early marriages, with their disproportionately high probability of break-up, or occur out-of-wedlock, the association between having a first birth as a teenager and later being a female head was explored among several samples of mothers. In the NLS, all women who had had a child by age 24 and all female heads with children at age 24 were studied. All PSID women with children under age 18 and all female heads with children less than 18 were also studied.

Teenage childbirth does not appear to be associated with subsequent female headship, either in cross tabulations or in multivariate analyses. However, the occurrence of a premarital birth does predict to later being a female head. A teenage marriage also predicts to later female headship, presumably because of the association between early marriage and marital break-up. Since pregnancy precipitates many early marriages and since teenage births occur disproportionately outside of marriage, early childbearing may be viewed as having an indirect effect.

Overall, women are less likely to be female heads if they have a young child, if they are white, attend church frequently, and, nonsignificantly, if they are Catholic. Women with relatively good earnings and work experience are more likely to be female heads, although it is not clear whether they become female heads in part because they are advantaged in the labor market or whether being a female head has resulted in greater experience and earnings. Labor market conditions were not found to have any effect over and above women's own earnings. Women in cities and on the Pacific Coast are somewhat more likely to be female heads, as are women with a physical limitation of some sort and women without a high school education. Young women in the NLS sample are more likely to be female heads, though there is no effect of age in the PSID sample.

#### WELFARE RECIPIENCY

Of greater concern than the incidence of female headed families is the poverty and welfare dependency of this family form. We find a strong association between receipt of welfare assistance and age at first birth overall; however, our analyses indicate that this association disappears when controls for education, family size, labor force participation, age at marriage and race are included.

Mothers whose first child was born outside of marriage are more likely to receive welfare; this association is particularly strong among younger women. Women who have never married are considerably more likely to be welfare recipients. Age at marriage, however, is not related to the probability of public assistance.

A number of factors other than age at first childbirth were found to influence welfare dependency, and several of them suggest indirect routes by which the occurrence of an early birth increases the odds of welfare receipt at a later age. For example, women whose first birth occurs during the teenage years tend to have larger families, and family size is a strong predictor of welfare recipiency. In addition, and early birth often disrupts the young women's schooling, and lower educational attainment increases the likelihood that a woman will later require public assistance. Women with relatively low earning ability and little work experience are also more likely to receive welfare, as are women who have some sort of physical limitation. Mothers who do not receive child support or alimony are considerably more likely to receive welfare, as are black women, and women who have been female heads for a relatively long time. In addition, women living in cities and on the Pacific Coast are slightly more likely to receive benefits. Finally, those women who live in states with relatively generous benefits in the Aid to Families with Dependent Children program have a little higher probability of being welfare recipients; but the association is not statistically significant among the sample of female heads.

In sum, early childbearing is not directly related to subsequent welfare dependency. However, a teenage birth can increase the probability of welfare receipt indirectly in numerous ways. To the extent that an early pregnancy precipitates teenage marriages which subsequently breakup, the birth contributes to the formation of a family with a high probability of welfare dependency. Similarly, a teenage out-of-wedlock birth creates a family form with a high probability of needing public assistance. Moreover, the low educational attainment and relatively large families of teenage mothers increase the likelihood of welfare receipt. For these reasons, teenage mothers tend to be disproportionately represented among the recipients of public assistance.

#### TRANSITION PROBABILITIES

Another approach was employed to examine the short run association between a birth and welfare receipt. A strong association was found. Among NLS women who are not receiving public assistance, a premarital first birth greatly increases the probability that a woman will go on welfare. Among women who already live in households that receive assistance, a premarital first birth reduces the probability that a young woman will go off welfare to virtually zero. Post marital first births exert only slight pressures on welfare entry and exit during the year of the birth. The impact of a first birth persists for several years, but in greatly reduced magnitude. Apparently, as the years go by the direct impact of a birth translates into an indirect impact that is transmitted instead through variables such as education, income, and family size.

#### THE EFFECT OF GOVERNMENT POLICIES ON OUT-OF-WEDLOCK SEX AND PREGNANCY

(By Kristin A. Moore and Steven B. Caldwell)

Of all children born out of wedlock, at least 60 percent end up on welfare. They represent over 30 percent of all children receiving Aid to Families with Dependent Children (AFDC), and the proportion is rising.<sup>1</sup> In fact, the proportion of all births occurring outside of marriage has been increasing steadily, from five percent in 1960 to 14 percent in 1975.<sup>2</sup> Concern over these trends has led to speculation that governmental policies may have encouraged them. It is frequently argued that the provision of welfare support for children born outside of marriage encourages women to become pregnant outside of marriage, or at least discourages marriage among women who do become pregnant while unwed.<sup>3</sup> The existence of AFDC is also sometimes alleged to encourage teenagers to become pregnant in order to form their own households and thus escape parental control and conflict.<sup>4</sup> In addition, it is suggested that the provision of contraceptive services and abortion encourages promiscuity and carelessness among unmarried people.<sup>5</sup> Such beliefs seem to constitute the basis for much of the opposition to government policies in the area of welfare support.

Several governmental programs are of particular relevance in this connection. AFDC provides welfare support for low-income families. The size of the monthly benefit and the ease of obtaining benefits vary from state to state. Do states with generous, easily available benefits encourage—or fall to discourage—childbearing among unmarried women? Some states refuse to provide AFDC if there is a father in the home—even if he is unemployed. Does absence of coverage for unemployed fathers reduce the likelihood of marriage and thus encourage out-of-wedlock childbearing? Does the availability of abortion in a state reduce the likelihood that premaritally pregnant women will carry pregnancies to out-of-wedlock births? Does abortion encourage contraceptive carelessness, thus increasing the rate of pregnancy? Does the availability of subsidized family planning services decrease the probability of conception among the unmarried, or does it rather enable or even encourage earlier initiation of sexual activity among unmarried females with no decline in the probability of conception? The research reported here represents an attempt to evaluate empirically whether government programs have consequences other than those officially intended.

#### DATA AND METHODOLOGY

The data are derived from a survey of a national probability sample of 1,479 black and 3,132 other (referred to as white henceforth) females aged 15-19 in 1971 living in households or college dormitories. The survey was conducted by John F. Kantner and Melvin Zelnik. A number of descriptive reports based on

Footnotes at end of article.

the data have already been published\* Respondents were questioned about their sexual and reproductive attitudes and histories, as well as their personal and family backgrounds. Only eight percent of the respondents had ever been married, but 28 percent reported having had sexual intercourse; nearly 14 percent reported having been pregnant at some time. Since women aged 15-19 bear more than half of all out-of-wedlock children—and probably even larger proportions of those who are unwanted and those who end up requiring welfare—this is an appropriate data base for examining the issues described above.

Three transitions are examined, each for a different eligible subpopulation:  
 first intercourse among virgins;  
 first pregnancy among the sexually experienced; and  
 pregnancy outcome (abortion, marriage or out-of-wedlock birth) among those who conceive premaritally.

The analysis is based on multiple regression with dichotomous dependent variables that are set equal to one if a transition takes place and zero if no transition takes place. For example, if first intercourse occurs during a particular year in the life of a female who is a virgin at the beginning of the year, the dependent variable equals one. If intercourse does not take place, the dependent variable equals zero. Similarly, if a first pregnancy occurs during a year to a young woman who is sexually experienced, the dependent variable equals one, and zero otherwise. Since we hypothesize that age and race each interact in major ways with other influences on sexual activity and pregnancy, separate regressions were performed by age (years 12-15\*\* and 16-18) and race for the first two transitions.

The number of young women who reported premarital pregnancies is, of course, considerably smaller than the total number in the initial sample. Of the 4,611 teenagers in the Kantner-Zelnik 1971 study, only 520 represent premarital conceptions. Because of the diminished sample size, analyses were conducted on the entire sample of premaritally pregnant teenagers, rather than on separate age and race groups. Three possible outcomes of a premarital conception were analyzed: marriage prior to birth; abortion; and live out-of-wedlock birth.† Clearly, these are not independent analyses. Their value lies in their ability to supplement one another.

Public policy variables were added to each respondent's computer record to represent governmental programs in her state of residence at about (or a little before) the time of the survey:

AFDC benefit level. AFDC (yearly amount paid in state to a family with four recipients, July 1971) divided by 1960 median family income in state

AFDC acceptance rate. AFDC application accepted in state in 1971, divided by AFDC applications in state in 1971

AFDC unemployed father program. Whether state AFDC program covered unemployed father, 1970:

0 = no program in respondent's state of residence

1 = program exists in respondent's state of residence

Family planning availability. Percent of need for subsidized family planning services met in state, 1960 (based on estimate of need in J. C. Dryfoos, "Women Who Need and Receive Family Planning Services: Estimates at Mid-Decade," Family Planning Perspectives, 7:172, 1975):

1 = 0-10% of need met

2 = >10-20% of need met

3 = > 20% of need met

Abortion availability. Availability in 1971 (a compound of abortion rates, ratios, laws and amount of time that passed since liberalizing legislation):

1 = liberal

2 = intermediate

3 = restrictive

#### FIRST INTERCOURSE

To test the hypothesis that liberal public policies encourage sexual activity, measures of the availability of legal abortion and of subsidized family planning

\* In 1976, Kantner and Zelnik fielded a second nationwide study of female adolescent sexuality, contraception and pregnancy; one report from this study (on sexuality and contraception) has been published. However, the results were published too late to be considered in this research. (See: M. Zelnik and J. F. Kantner, "Sexual and Contraceptive Experience . . ." 1977, reference 7.)

\*\*We assumed that all respondents were nonmarried virgins at age 11. Each cohort was then aged forward to the point at which they had initiated sexual activity and had experienced a pregnancy. (See: K. A. Moore and S. B. Caldwell, reference 1, p. 92.)

† Miscarriages were dropped from the sample after no patterns in the occurrence of this outcome were detected.

services in the respondent's state of residence were included in the analysis. Measures of the level of welfare benefits and of the rate of acceptance of welfare applications in the respondent's state of residence were also included to test the hypothesis that generous welfare policies encourage sexual activity by providing an income cushion in case of premarital pregnancy. As shown in the top deck of Table 1, no consistent pattern emerges indicating that public policies affect the initiation of sexual activity, although several weak associations can be found both in support of and against these hypotheses.

High AFDC benefits are associated with a higher probability of first intercourse among older white virgins; however, a high AFDC acceptance rate is related to a lower probability of first intercourse among older whites and among younger blacks. In addition to this inconsistency, subsequent analyses with these data (and with a related state-level data set) have produced no positive associations between welfare generosity and the probability of conception (see bottom deck of Table 1) or between welfare benefit levels and the probability of an out-of-wedlock birth (see Table 2, p. 166). This suggests that the single instance of a positive association between welfare generosity and early sexual experience may be an artifactual finding (especially since one would expect welfare benefits which are paid only for children to have more of an effect on pregnancy than on sexual activity).

TABLE 1.—SUMMARY OF RESULTS OF MULTIPLE REGRESSION ANALYSIS OF INDIVIDUAL DATA INDICATING PROBABILITY OF TRANSITION TO SEXUAL ACTIVITY AND TO PREGNANCY AMONG U.S. WOMEN AGED 15 TO 19, BY AGE AND RACE, CIRCA 1971

Variable	Direction of association, by race and age			
	Whites		Blacks	
	12 to 15	16 to 18	12 to 15	16 to 18
<b>Transition to sexual activity:</b>				
Older age.....	+	+	+	+
Higher education of father or father substitute.....	0	-	-	0
Higher education of mother or mother substitute.....	0	0	0	0
Recency of birth cohort.....	+	+	+	-
More frequent church attendance.....	0	0	0	0
Respondent Catholic.....	0	0	0	0
Nonintact family of origin.....	+	+	+	+
Respondent lives on Pacific coast.....	+	+	0	-
Farm background.....	-	0	0	0
Rural residence.....	-	-	-	-
Central city residence.....	+	+	+	+
High abortion availability.....	0	0	0	0
High AFDC benefits.....	0	+	0	0
High AFDC acceptance rate.....	0	-	-	0
High unmet family planning need.....	0	0	0	0
<b>Transition to pregnancy:</b>				
Older age.....	+	0	+	0
Higher education of father or father substitute.....	0	0	0	0
Higher education of mother or mother substitute.....	-	-	-	0
Recency of birth cohort.....	0	0	-	0
High importance of religion to respondent.....	0	+	0	0
Respondent Catholic.....	0	0	+	+
Nonintact family of origin.....	0	0	0	+
1 to 2 yr intercourse experience.....	+	+	+	0
Ever used contraception.....	0	0	0	0
Urban/rural residence.....	0	0	0	0
High abortion availability.....	0	0	0	0
High AFDC benefits.....	0	0	-	0
High AFDC acceptance rate.....	0	0	0	0
High unmet family planning need.....	0	0	0	+

Note: + equals positive association; - equals negative association; 0 equals no statistically significant association.

Source: K. A. Moore and S. B. Caldwell, reference 1, tables 37 and 39.

The top deck of Table 1 also shows that the availability of subsidized family planning services in 1969\* was not related to the initiation of sexual activity in any of these age-race groups. Nor was the availability of legal abortion found to be associated with a greater probability of sexual experience among any of the subgroups.

\* In 1969, relatively few unmarried teenagers were served by organized family planning clinics.



## OTHER FACTORS AND FIRST INTERCOURSE

Kantner and Zelnik have observed that more recent birth cohorts are experiencing first intercourse at earlier ages.† Such a change means that that much larger populations are at risk of pregnancy, abortion, forced marriage, out-of-wedlock childbearing and venereal disease.

No full measure of social class is available, but as Kantner and Zelnik found in their 1971 study,<sup>9</sup> we also found that, by and large, those teenagers with less-educated fathers began sexual activity at earlier ages. Later initiation of sexual experience was noted among whites, and among teens who attended church regularly, or who lived on a farm, or who came from intact families. Neither Catholic identification nor the educational level of the mother was found to be related to age at first intercourse.<sup>9</sup>

## PREGNANCY

The hypothesis that general AFDC benefits and high AFDC acceptance rates would be associated with a greater incidence of pregnancy was not supported by our research. As the bottom deck of Table 1 shows, no indication of such a relationship was found. In addition, no association was found between the availability of legal abortion and a higher probability of pregnancy, thus arguing against the contention that abortion encourages contraceptive carelessness.

On the other hand, the availability of subsidized family planning services was found to be negatively related to the occurrence of pregnancy among older black teens. The availability of family planning services was measured using data prepared for the U.S. Office of Economic Opportunity showing the percent of unmet need for family planning services in a state in 1969. Our analysis shows that there was a significant lower annual probability of pregnancy among older teenage black women living in states with the most subsidized family planning services. (This finding is not replicated among the other sub-groups.) Given the frequently lower income of blacks in the United States, it seems reasonable to assume that older black teenagers are somewhat overrepresented among users of subsidized family planning services and thus are more affected by their availability than whites. It is important to note that although family planning availability is *not* related to a greater likelihood of premarital sex, it does seem to be related to a *lower* incidence of conception, at least among older black teenagers.

## OTHER VARIABLES AFFECTING CONCEPTION

It has also been maintained that the education of the mother (or mother substitute) affects the probability of conception among teenagers, since young women with better educated mothers should be more knowledgeable about sex, reproduction and contraception. Our analysis confirms this argument; the likelihood of conception is lower for teenagers with better educated mothers, as is shown in the bottom deck of Table 1. This variable does not seem to be a proxy for social class, however, since the education of the father or father substitute was not found to be related to pregnancy. In addition, the probability of pregnancy is higher when there is no mother (or substitute) in the home, or when the respondent is not aware of the educational attainment of the woman designated as her caretaker. The probability of conception is also higher among blacks, among teenagers who did not live in intact families when they were aged 10-15, and among teens aged 15 or older.

†For example, 18.0 percent of 15-year-olds in 1976 compared with 10.9 percent in 1971 had initiated sexual activity; among 16-year-olds, 25.4 percent in 1976 had done so, compared with 21.2 percent in 1971.  
Footnotes at end of article.

TABLE 2.—SUMMARY OF RESULTS OF MULTIPLE REGRESSION ANALYSES OF INDIVIDUAL DATA INDICATING PROBABILITY OF ABORTION, MARRIAGE OR OUT-OF-WEDLOCK BIRTH AMONG PREMARITALLY PREGNANT U.S. WOMEN AGED 15 TO 19, CIRCA 1971

Variable	Direction of association, by pregnancy outcome <sup>1</sup>		
	Abortion	Marriage	O-W birth
Older age.....	0	0	0
College-educated father or father substitute.....	+	0	-
Pregnancy desired.....	-	+	-
Recency of pregnancy (calendar year).....	+	0	0
High importance of religion to respondent.....	-	0	0
Respondent Catholic.....	0	0	0
Respondent white.....	+	+	-
Nonintact family of origin.....	0	0	0
Urban/rural residence.....	0	0	0
High abortion availability.....	+	0	0
High AFDC benefits.....	-	0	0
High AFDC acceptance rate.....	0	0	-
AFDC unemployed father program.....	0	0	-

<sup>1</sup> All ever-pregnant respondents included in the same regression.

Note: + equals positive association; - equals negative association; 0 equals no statistically significant association.

Source: K. A. Moore and S. B. Caldwell, reference 1, table 41.

Older white teenagers who regard their religion as important to them, and black Catholics, are just slightly more likely to become pregnant. Not surprisingly, longer exposure to sexual intercourse is also positively associated with a higher annual probability of conception. Little impact from individual contraceptive use was documented, probably because of the lack of detail in the variable available for analysis and because of the sporadic and ineffective use of contraceptives among adolescents.‡

#### OUTCOME OF A PREMARITAL CONCEPTION

Once a premarital conception has occurred, decisions about continuing or terminating the pregnancy must be made. It seems reasonable to assume that policy variables would have the greatest impact at this time. What impact does governmental policy actually have on pregnancy outcome?

As Table 2 shows, in states having relatively generous AFDC benefit levels, the probability of abortion is significantly lower. The probability of marriage in order to legitimate a birth and the probability of having a child out of wedlock are both slightly (but not significantly) higher. On the other hand, states with high AFDC acceptance rates were found to have a significantly lower proportion of out-of-wedlock births. There is, then *no statistically significant evidence linking welfare availability with the probability of carrying an out-of-wedlock pregnancy to an out-of-wedlock birth.*

AFDC coverage of unemployed fathers seems to be a program with rather direct relevance to the probability of marriage among many couples faced with a premarital pregnancy. Young women residing in states with such a program do have a slightly (but not significantly) higher probability of marriage—but a considerably (though still not significantly) higher probability of undergoing abortion as well. Consequently, presence of an unemployed father program is associated with a significantly lower incidence of out-of-wedlock childbearing. One would expect more marriages in the presence of such a program because it allows young couples to marry and receive AFDC payments if the father is unemployed. The higher incidence of abortion was not predicted and may well be an artifact of a simultaneous occurrence of liberal abortion policies and AFDC coverage of unemployed fathers.

Table 2 also shows that wide availability of legal abortion has a strong, statistically significant impact on pregnancy outcome. Even in a 1971 data set, the impact of changes in abortion laws over time can be noted. When the outcomes

‡ Kantner and Zelnik found that in 1971, only 18 percent of sexually experienced, never-married teenage women reported that they always used contraception, and 17 percent never used it. In addition, those who reported ever use of a method relied heavily on withdrawal, the condom and douche. (See: J. F. Kantner and M. Zelnik, "Contraception and Pregnancy . . ." 1973, reference 6.)

of pregnancies occurring before 1970 are compared with those pregnancies occurring in 1970 and 1971, we find that the abortion outcome is twice as frequent in the later time period, and that there is about a six percent decline in the proportion of pregnancies that terminate in live births.\* There is also a slight (but nonsignificant) decline in the proportion of pregnant teenagers who marry, but this change seems to be outweighed by the impact of abortion, since there is a net reduction in the probability of a life out-of-wedlock birth. The importance of change over time in abortion laws is further emphasized by another variable added to the current data set.

Respondents were assigned a code for the kind of abortion law existing in their state of residence in 1971. An abortion outcome was much more frequently reported by females residing in those states in which abortion was legal and available, and in states where abortion was somewhat restricted but fairly available (if only by virtue of geographic propinquity to a state in which abortion was easily available). The impact of a high abortion rate is felt on both the marriage and the out-of-wedlock variables. Apparently, abortion availability slightly reduces the probability of a forced marriage, and has a significant effect in reducing the probability of an out-of-wedlock birth.

#### OTHER VARIABLES AFFECTING OUTCOME

Overall, among those teenagers who became premaritally pregnant, pregnancy outcome was most strongly affected by four factors. As noted, young women living in states with relatively liberal policies were significantly more likely to obtain abortions and, correspondingly, were less likely to bear a child out of wedlock or to marry to legitimize the pregnancy. As Table 2 shows, young women with college-educated fathers were also significantly more likely to obtain abortions. On the other hand, young women who desired their pregnancies were especially likely to marry. Black teenagers were far less likely to marry or obtain abortions, and thus were much more likely to carry their pregnancies to term outside of marriage.

Since this analysis was conducted on the entire sample of premaritally pregnant teenagers, a variable for respondent's race appears for the first time. The differences captured by this particular variable dwarf those captured by all other independent variables. Premaritally pregnant blacks are considerably less likely than whites to obtain an abortion,† and are also much less likely to marry before the birth. Thus, a markedly greater proportion of black teenagers end up delivering infants outside of marriage.

#### ANALYSIS OF STATE AGGREGATE DATA

To supplement the 1971 survey of 4,600 teenagers, an analysis of the variation in out-of-wedlock birthrates among states in 1974 was also conducted. One purpose was to see whether state programs and certain socioeconomic characteristics seemed to affect the overall out-of-wedlock birthrates of the *states* in the same way as they did out-of-wedlock childbearing among the *individuals* in the Kantner-Zelnik survey. The measurement of variables as they affect individuals was cruder, and many factors could not be measured at all in the state-level analysis.

\* The 1976 Kantner-Zelnik data should give us a better idea of the effect of abortion.

† These data refer to the period prior to the 1973 Supreme Court decisions on abortion, when legal abortion was much less accessible to young, poor and black women. Data on abortions at a later period, 1972-1974, show that the abortion rate among black teenagers (31.1) was 2.2 times higher than the rate for white teenagers (14.1). While the marital status of these teenagers was not provided, it can be assumed that a majority of them were unmarried. (See: C. Tietze, "Legal Abortions in the United States: Rates and Ratios by Race and Age, 1972-1974," *Family Planning Perspectives*, 9:12, 1977.)

TABLE 3.—SUMMARY OF RESULTS OF MULTIPLE REGRESSION ANALYSIS OF STATE-LEVEL DATA INDICATING PROBABILITY OF IMPACT ON OUT-OF-WEDLOCK BIRTHRATES AMONG U.S. WOMEN AGED 15 TO 44, BY AGE AND RACE, 1974

Variable	Direction of association, by race and age					
	Whites			Blacks		
	15 to 19	20 to 24	15 to 44	15 to 19	20 to 24	15 to 44
High AFDC benefits.....	0	0	0	-	0	0
High AFDC acceptance rate.....	0	0	0	0	0	0
High abortion availability.....	-	-	-	0	0	0
Family planning availability.....	0	0	0	-	0	0
Age of consent for contraception $\geq 18$ .....	0	NA	NA	+	NA	NA
Age of consent for abortion $\geq 18$ .....	0	NA	NA	0	NA	NA
AFDC unemployed father program.....	0	0	0	0	0	0
AFDC unborn child coverage.....	0	0	0	0	0	0
Medicaid abortion coverage.....	0	0	0	0	0	0
Median educational attainment.....	0	0	0	0	0	0
Percent of work force unemployed.....	0	0	0	-	0	0
Percent of females 15-34 employed.....	0	0	0	0	0	0
Female earnings.....	0	0	0	+	0	0
Female/male earnings ratio.....	0	0	0	0	0	0
Percent of State in SMSA's.....	0	0	0	0	0	0
Percent of State Catholic.....	0	0	0	0	0	+

Note: + equals positive association; - equals negative association; 0 equals no statistically significant association; NA equals not applicable.

Source: K. A. Moore and S. B. Caldwell, reference 1, table 43.

The results of this analysis as summarized in Table 3 strengthen the impression that public welfare policies do not act as economic incentives to childbearing outside of marriage. Neither AFDC benefit levels nor AFDC acceptance rates are associated with the out-of-wedlock birthrates of blacks or whites.

Abortion availability is negatively associated with white out-of-wedlock birthrates, but shows no statistically significant association with black rates. Since we know that nonwhites obtain nearly 30 percent of the abortions performed in the United States, it seems surprising that abortion availability is not related to black out-of-wedlock rates. This is probably due to the lack of race- and age-specific abortion data for states and to understatement of the unmarried population denominator in our data.\* We do find that existence of a state law limiting family planning services to women aged 18 or older is associated with significantly higher out-of-wedlock fertility among black teenagers. In addition, there is a negative association between the availability of subsidized family planning services and black teenage out-of-wedlock fertility, but no association with white rates. It seems probable that the availability of subsidized family planning services is most important to black teenagers, because of their frequently disadvantaged income position.

Overall, measures of attitudes, social controls, alternatives to childbearing and motivations for pregnancy and childbearing were not related to the out-of-wedlock birthrate at the state level. Decisions regarding sexuality and reproduction are intensely personal, however, and are better addressed at the individual level. One value of the state-level analysis is that it explores whether individual decisions add up to anything. That is, can an aggregate effect of contextual variables be identified? The variables of primary interest here are, of course, the public policy variables, and from our analysis, it does not appear that AFDC benefits encourage out-of-wedlock childbearing. In addition, subsidized family planning relates to lower black teenage out-of-wedlock fertility, while abortion availability predicts lower white out-of-wedlock fertility. It is reassuring that on these crucial questions, the two complementary approaches are in accord.

#### DISCUSSION AND CONCLUSIONS

Certainly the most important policy conclusion to be drawn from these analyses is that the level of AFDC benefits and the AFDC acceptance rate do not seem to serve as economic incentives to childbearing outside of marriage for either blacks

\* Estimates of the unmarried population were obtained by applying 1970 state marriage proportions to 1974 state estimates of population. Among whites, the proportion unmarried grew only slightly between 1970 and 1974, but among blacks it increased significantly. The effect, therefore, is to overestimate the out-of-wedlock birthrates for blacks.

or whites. In addition, the availability of contraception and abortion does not seem to encourage the individual to initiate sexual activity. However, the availability of subsidized family planning services does seem to lower pregnancy rates, especially among black teenagers; and the availability of abortion does seem to reduce substantially the incidence of out-of-wedlock childbearing among those who are premaritally pregnant, especially for whites.

Knowledge and information about conception and contraception seem to be important as well. The educational attainment of the mother (or mother substitute) is assumed to affect the amount of information a young woman has; it was found that females with relatively poorly educated mothers were more likely to become pregnant. Further, states which prohibited the provision of family planning services to teenagers had significantly higher out-of-wedlock birthrates among blacks aged 15-19 (almost 20 more births per 1,000 unmarried females of that age group). Even when services are available, however, use of contraception among the young, unmarried population frequently seems to be erratic and ineffective. Provision of better services and more information to those who want them seems to be an important policy goal.

It is also essential to recognize that although most unmarried people do not report that they desire pregnancy, some do. The latter are unlikely to seek abortions and are likely to marry before the birth. Information about the difficulties of early and single parenthood should also be made more available to this group, even though those who do not wish to become parents premaritally will be most motivated to take advantage of birth control information and services.

Personal and family life style also seem to be important explanatory factors. An intact family of origin and religious commitment seem to reduce the probability of sexual activity and pregnancy. Social and cultural factors are pertinent as well. For example, more recent birth cohorts, whites on the Pacific coast, and blacks are more likely to be sexually active, even after controlling for other factors.

It is crucial to acknowledge that most of the variance remains unexplained. In addition, the measurement of policy variables is crude. Ideally, such measurement should capture the type of government policy in force (which may differ from the policy on the books) in the respondent's state or local community at the time the respondent is making her decision about sex or pregnancy.

Other variables, not available in these data sets, also merit exploration. No really good retrospective measures of family income and social status were available, and it would be desirable to include some measures of these important factors. In addition, a number of personal attributes are probably very important influences. For examples, we still don't know the dynamics of the conception process among those not intending or desiring conception. What is the role of peer group pressure? How do sex-role attitudes affect the use of contraception and the desire for pregnancy? We also know little about the values, motivations and expectations of unmarried males. Certainly, the decision to be sexually active and to use contraception is made through an interactive process. How do unmarried males perceive their roles and responsibilities?

In many ways, our results are still descriptive. We know that the education of the father affects the likelihood that a young unmarried woman will become sexually experienced, but that among the sexually active, it is the education of the mother that affects the probability of pregnancy. Later, it is having a college-educated father that affects the likelihood that a premaritally pregnant female will obtain an abortion. The decisionmaking processes that underlie these associations are undoubtedly extremely complex. We can count offspring of unmarried people and estimate other important statistics, such as the proportion sexually active and the proportion having abortions, but we have little idea of what causes or explains the numbers. Even at this point, we do not have a handle on the decision-making process at the level of the individual person or couple.

The focus and the central task of this research has been to explore whether public welfare policies affect the occurrence of out-of-wedlock childbearing. Our data indicate that the answer to this question is that welfare benefits do not appear to provide an economic incentive that encourages the bearing of children outside of marriage. To answer the question of what really motivates or explains such childbearing requires a great deal of further, sophisticated and detailed analytic research.

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## EARLY CHILDBEARING AND EDUCATIONAL ATTAINMENT

(By Kristin A. Moore and Linda J. Waite)

Although fertility rates in the United States have declined dramatically in the past two decades among women aged 20 or older and have fallen slightly among those aged 15-19,\* the absolute number of births to teenagers has risen since the mid-1950s; at the present time, approximately one birth in five occurs to a young woman not yet 20. In 1975, 15-to-19-year-olds bore some 580,000 babies (compared to about 420,000 in 1950), and an additional 113,000 babies were born to girls 14 or younger.

As has been widely documented, early childbearing is associated with serious health consequences for both the mother and child.<sup>1</sup> In addition, women who bear their first child while still quite young themselves tend to complete fewer years of formal schooling than do those who delay entry into motherhood. One investigator found that in 1967, nearly six out of every 10 women who bore their first child at age 16 or younger has completed only eight or fewer years of schooling,<sup>2</sup> and a more recent report showed that young women who become pregnant and carry to term while still in high school are much more likely than their childless classmates to leave school without graduating.<sup>3</sup>

While there are reasons to expect that a birth during the teen years will often lead to termination of formal schooling—given such realities as lack of day care facilities, problems of financial support and absence of educational facilities geared to the needs of pregnant teenagers—several researchers have suggested that these young women would complete little formal schooling whether or not they bore children early.<sup>4</sup> They believe that the young women had low educational aspirations before they conceived and that the pregnancy merely provided an excuse to drop out of school. While this may be the case for some young mothers,

\* Only among girls aged 14 and under was there a sharp rise in fertility rates in the same period.

Footnotes at end of article.

there is evidence that a birth during the teens does indeed curtail education. Among one representative sample of New York City mothers of firstborns, the younger the age at first birth, the more likely the woman was to say that she had stopped going to school because of pregnancy. This reason was given by three out of four of those who had borne their first child at 15 or 16.<sup>2</sup> A study of teenage mothers in Baltimore found that the educational aspirations of the women in that sample did not appear to differ substantially from those of other teenagers,<sup>3</sup> but the adolescent mothers were much less likely than their childless classmates to realize their educational goals, suggesting that parenthood may have led to a change in plans for formal schooling.

This evidence, while suggestive, leaves a number of questions unanswered. How much of the lower educational attainment of teenage mothers is a result of family background, aspirations and such demographic characteristics as race, and how much is associated with age at first birth? If most or all of the negative consequences which have been attributed to untimely childbearing nevertheless occur even if the young women postpone childbearing, policies to avert these outcomes must focus on factors other than teen pregnancy and childbearing. If it is found that an early first birth influences educational attainment after the effects of family background, race and educational aspirations are removed, there are other issues to be addressed. One would want to know if the educational deficit of those who bear a child in their teen years is permanent—or if these young women catch up as they get older. Do they fall further behind those who delay parenthood? Is the effect of an early first birth the same for all young women?

In the research reported here, data on the education and childbearing experience of approximately 5,000 young women in their teens and early twenties gathered between 1968 and 1972 are examined to try to answer these questions.

We begin by building a model of the educational attainment of young women,<sup>†</sup> taking into consideration the following characteristics of an individual's family of origin, all of which have been found to have a sizable effect on the individual's attainment:<sup>7</sup> mother's and father's education, father's occupation, family income, size of the family of orientation, whether the family was intact,<sup>8</sup> and region of residence.<sup>9</sup> Previous work indicates that individuals from small, intact families living outside the South are relatively advantaged in the competition for education. Substantial inequalities between black and whites in the attainment of formal schooling have been documented, and the effects of virtually all determinants of educational attainment have been found to depend on race.<sup>10</sup> In addition, aptitude or ability, and encouragement from parents, teachers and peers, have been found to be important predictors of ultimate educational attainment.<sup>11</sup> Measures of these factors are included to the extent possible in our model of formal schooling.

Our hypotheses about the effect of adolescent childbearing on educational attainment include the following:

The younger the woman when she bears her first child, the fewer years of schooling she will complete. Furthermore, early childbearing is such an inhibitor of educational attainment that even after appropriate controls are included for factors of family background and motivation, the association between an early first birth and reduced educational attainment will remain.

The educational disadvantage suffered by young mothers will not decrease as they become older; those who bear children in their early teens will not be able to close the gap in attainment that separates them from young women who delay childbearing. Adolescent motherhood makes continued schooling more difficult and therefore less likely. It is expected that young mothers will fall further behind as their childless peers continue to progress in school.

The effects of adolescent childbearing will differ for blacks and whites. Since teenage parenthood is so much more common among blacks, it may carry less social stigma than among whites, and informal social mechanisms for coping with this event may be more highly evolved for blacks than for whites. Thus, after taking into account the other influences determining educational attainment, we hypothesize that an early first birth has fewer negative educational consequences for a young black woman than for a young white woman.

In numerous instances of early childbearing, persuasive if not definitive evidence can be marshalled to indicate that early childbearing actually causes the untimely termination of formal education—that the association is not merely correlational.

Footnotes at end of article.

## METHODOLOGY

The data used in the study are derived from a representative nationwide sample of 5,150 noninstitutionalized civilian women aged 14-24 at the time of the initial interview in 1968.\* Data were collected annually by personal interview; 4,625 respondents were retained through the 1972 wave of interviews.† Although these data are believed to be among the best available, attrition may have reduced the sample's original representativeness, and some caution in generalizing to the entire population is necessary.

These data are especially well-suited for a study of the consequences of early childbearing because they follow young women through the teenage and young adult years when family building often takes place. For a large proportion of the sample, data on marriage and childbearing are not retrospective but were gathered as events occurred. Because extensive information was obtained on the education and work experience, as well as on the social and economic background, of respondents, detailed comparisons between teenage mothers and young women who postponed their childbearing can be made. The data do not contain a childbearing history for each young woman, and therefore our measure of age at first birth was obtained from the household record. Thus, only children living with the woman were considered in calculating her age at the birth of her first child.‡ Own children cannot be distinguished from adopted children. We are measuring the age at which a woman takes on the duties and responsibilities of motherhood.

The young woman's educational attainment, in grades of formal schooling completed, is examined at ages 18, 21 and 24. Eighteen is the usual age of high school graduation; by age 24, virtually all women have completed their formal schooling. Age 21 provides a midpoint during the college years when marriage and childbearing are in active competition with school enrollment. Examination of the effect of age at first birth on educational attainment at 18, 21 and 24 allows us to determine whether early childbearers catch up overtime.§

The other variables in the analysis include the following: the young woman's race and age in 1968; the occupational status of the head of the household when the young woman was 14; her mother's and father's education; whether the family was intact; whether the young woman lived in the South as a teenager; an index of the availability of reading materials in her home; a measure of perceived help and encouragement from parents and teachers to continue education past high school; whether the young woman was enrolled in a college-preparatory curriculum in high school; and her parents' goal for her educational attainment. Age at first birth (AFB) is measured in a series of categories, grouped where necessary to provide a sufficient number of women for adequate analysis:

- Age at first birth less than 16 (AFB LT 16),
- Age at first birth 16-17 (AFB 16-17),
- Age at first birth 18 (AFB 18),
- Age at first birth 19-20 (AFB 19-20),
- Age at first birth 21-23 (AFB 21-23), and
- No children by 18, 21 or 24 (AFB > 18, 21, 24).

The effect of age at first birth on educational attainment is estimated by regressing years of schooling completed at a specified age on family background, attitudes and plans, as well as on the measures of age at first birth shown above. In this way it is possible to obtain estimates of the effect, after these other factors are controlled, of having had a birth at a particular age on education completed at the specified age.

\* The National Longitudinal Survey (NLS) of the Education and Labor Market Experiences of Young Women was designed by the Center for Human Resource Research at Ohio State University, supported by the Department of Labor, and conducted by the U.S. Census Bureau.

† Since the initial response rate was 94 percent, data on nearly 85 percent of the initially drawn sample are available for the present analysis.

‡ The measure of age at first birth used here does not include children who were given up for adoption shortly after birth, who were stillborn or who died in early childhood, or those who were sent to live outside the young woman's household.

§ It should be noted that the analyses of attainment at these three ages are not based on exactly the same young women, so some care in comparing them must be taken.



TABLE 1.—RESPONDENTS' MEAN EDUCATIONAL ATTAINMENT AT AGES 18, 21, AND 24, BY AGE AT 1ST BIRTH, RACE AND PARENTS' SES<sup>1</sup>

Age, race and SES	N	Years of education at 18	N	Years of education at 21	N	Years of education at 24
<b>All races:</b>						
<15.....	68	9.4	38	9.4	48	8.9
16-17.....	231	10.4	173	10.4	172	10.5
18.....			183	11.5	184	11.3
19-20.....			360	11.8	363	11.9
21-23.....					400	12.7
No children by 18, 21, 24.....	2,083	11.5	1,434	12.9	784	13.5
<b>All whites:</b>						
<15.....	30	9.4	22	9.4	32	8.9
16-17.....	164	10.4	126	10.4	135	10.5
18.....			156	11.6	161	11.3
19-20.....			306	11.8	322	11.9
21-23.....					365	12.8
No children by 18, 21, 24.....	1,889	11.5	1,316	13.0	722	13.7
<b>Low SES:</b>						
<15.....	12	8.6	9	8.7	10	8.8
16-17.....	43	9.4	43	9.4	59	10.0
18.....			39	10.9	43	10.8
19-20.....			71	11.1	80	11.1
21-23.....					67	11.4
No children by 18, 21, 24.....	234	11.0	156	11.4	82	11.5
<b>Medium high SES:</b>						
<15.....	16	10.2	9	10.2	14	9.0
16-17.....	102	10.8	70	11.0	62	11.1
18.....			99	11.8	97	11.8
19-20.....			200	12.1	208	12.2
21-23.....					264	13.2
No children by 18, 21, 24.....	1,539	11.7	1,072	13.3	581	14.1
<b>All blacks:</b>						
<15.....	38	9.4	15	9.4	16	9.0
16-17.....	67	10.6	47	10.4	37	10.1
18.....			27	11.1	24	10.9
19-20.....			55	11.8	42	12.0
21-23.....					35	12.1
No children by 18, 21, 24.....	193	11.0	118	12.2	62	12.0
<b>Low SES:</b>						
<15.....	19	9.2	7	8.8	8	8.5
16-17.....	30	10.6	21	10.3	22	10.1
18.....			14	10.8	13	10.3
19-20.....			26	11.5	20	11.5
21-23.....					14	11.3
No children by 18, 21, 24.....	85	10.8	49	11.6	30	11.6
<b>Medium high SES:</b>						
<15.....	8	10.4	3	(?)	3	(?)
16-17.....	17	10.8	12	11.0	6	11.8
18.....			8	11.6	8	11.7
19-20.....			21	12.2	14	12.7
21-23.....					14	12.9
No children by 18, 21, 24.....	71	11.4	50	13.1	23	13.3

<sup>1</sup> SES is the mean of 4 variables: occupation of head of household; mother's education; father's education; and presence of reading materials in the home of origin. Variables were standardized to have a mean of 10 and a standard deviation of 3.

<sup>2</sup> N < 5 or N = 0.

### Findings

The initial hypothesis predicts that the younger the woman when she bears her first child, the fewer years of schooling she will complete. Table 1 shows that this direct relationship holds true not only for women generally, but also for different racial and socioeconomic subgroups. It can also be observed that socioeconomic background (SES) strongly affects the number of years of schooling completed, while race has relatively little impact. Within socioeconomic groups, the association between age at first birth and educational attainment remains positive and appears without exception in every subgroup at every age. Every additional year that passes without the birth of a first child results in commensurate additional schooling; none of the groups of young women having children by either 18, 21 or 24 reaches the average level of schooling attained by those who postpone childbearing past those ages.\*

\* School enrollment data (not included here) show that mothers of any age are far less likely to be attending school than their childless peers.

The basic relationship between age at first birth and years spent in school makes intuitive sense and has been reported by other researchers.<sup>13</sup> The critical task is to assess the relative importance of this factor and other variables (such as the characteristics of the family of origin and motivational variables, in addition to race and parental socioeconomic status) that have been found to determine schooling. Earlier research primarily on males, identified parents' education, father's occupation, parental attitudes, small and intact family of origin, and availability of reading material in the home as having direct positive influences on educational attainment.<sup>13</sup> In general, the results shown in Table 2 support these findings. However, when age at first birth is included in the analysis, early childbearing is associated with significant educational losses *even after* the influence of all the other factors has been taken into account. These findings can be interpreted as follows. The table lists the unstandardized and standardized coefficients for the regression of educational attainment at ages 18, 21 and 24 on the variables listed in the column headed "independent variable." The unstandardized coefficients for age-at-first-birth categories can be interpreted as the effect of having had a birth at that age on years of school completed by ages 18, 21 or 24, compared with the effect of being childless at 18, 21 or 24. For example, at age 18, when most young women are just finishing high school or beginning college, girls who bore a child at 15 or younger suffer an educational decrement of one and one-half years, and those having a first birth at 16 or 17 experience a loss of two-thirds year, compared to those still childless at 18. The relative disadvantage of early childbearing is also seen in the analysis of educational attainment at 21 and 24. In fact, at each stage of attainment studied, age at first birth shows up as either the strongest, or among the strongest, of the influences considered. In this analysis, as in Table 1, race has no effect on educational attainment once family background, parental attitudes and ambition of the individual are taken into account. This is consistent with findings of other researchers.<sup>14</sup>

TABLE 2.—UNSTANDARDIZED AND STANDARDIZED PARTIAL COEFFICIENTS FOR REGRESSION OF RESPONDENTS' EDUCATIONAL ATTAINMENT AT AGES 18, 21 AND 24 ON RESPONDENTS' AGE AT FIRST BIRTH, FAMILY BACKGROUND, AND SOCIAL AND DEMOGRAPHIC TRAITS

Independent variable	Educational attainment					
	At 18 (N=1,593)		At 21 (N=1,386)		At 24 (N=1,016)	
	Unstandardized coefficients	Standardized coefficients	Unstandardized coefficients	Standardized coefficients	Unstandardized coefficients	Standardized coefficients
AFB-LT-16	†-1.465	†-0.195	†-2.130	†-0.146	†-2.824	†-0.182
AFB 16-17	†-.669	†-.158	†-1.312	†-.187	†-1.446	†-.171
AFB 18			†-.566	†-.082	†-.927	†-.113
AFB 19-20			°-.277	°-.054	†-.613	†-.099
AFB 21-23					-.244	-.041
AFB > 18, 21, 24						
Number of siblings	-.009	-.017	°-.042	°-.052	-.047	-.045
Occupation of head	-.002	-.040	.002	.029	-.000	-.009
Mother's education	.026	.059	*.041	*.063	*.047	*.060
Father's education	.015	.042	.025	.047	°.055	°.084
Intact family	†.268	†.065	.018	.003	.024	.003
Home culture index	†.218	†.149	†.275	†.126	†.320	†.129
Parents' educational goal	†.299	†.118	†.582	†.153	†.700	†.146
Parent-teacher help	.024	.049	†.111	†.155	†.140	†.156
High school curriculum	†.204	†.078	†1.045	†.268	†1.320	†.267
Age in 1968	-.038	-.042	.025	.019	°-.016	-.010
Race	-.019	-.005	-.052	-.009	-.009	-.001
South	†-.259	†-.095	.031	.008	.034	.007
R <sup>2</sup>		.271		.545		.569

†p < .001.

\*p < .01.

°p < .05.

Note: Ns in the tables vary because in models in which estimates are made for both races combined, observations on black women were weighted down to their true proportion in the population. In models estimated separately by race, the actual number of blacks in the sample was used.

Footnotes at end of article.

The size of the effect of an early first birth on years of schooling is striking. In every case, a hypothetical delay in age at first birth of one or two years would result in a substantial increase in ultimate attainment. As Table 2 shows, young women who had a first birth at 15 or younger completed about 1.4 fewer years of schooling by age 24 than their classmates who delayed motherhood until 16 or 17, and 1.0 fewer years than those who waited until 18 to bear their first child. Even among those who had a first birth at age 19 or later—after the usual age of graduation from high school—an older age at first birth means more years of schooling completed by age 24. An increase in age at entry into motherhood from 19 to 20 to between 21 and 23 is associated with a rise in educational attainment at 24 of almost half a year. Thus, the differences in years of schooling completed by young women who bore their first child at various ages are large and important.

Table 2 also shows the standardized coefficients for the regression of educational attainment at ages 18, 21 and 24 on the independent variables listed. These coefficients show the relative importance of each of the factors considered; the larger the coefficient, the more effect that factor has on years of schooling completed at the age being considered. Thus, in the analysis of attainment at age 18, by looking down the column of standardized coefficients, we can see that the best predictor of years of schooling completed at age 18 is a first birth at age 15 or younger or at ages 16-17. Examination of the coefficients in the analyses of attainment at 21 and 24 indicates that an early first birth is one of the most important predictors of attainment at those ages and that the earlier the age at first birth, the fewer years of schooling completed.

#### YOU NEVER CATCH UP

The data presented in Table 1 showing educational losses associated with early childbearing also support the second hypothesis, that the young mothers never make up these losses; on the contrary, they seem to fall further behind as their childless contemporaries continue their schooling. The regression models including the wider range of variables (see Table 2) also bear out this observation, showing that the educational disadvantage of young mothers increases with time. Indeed, women who become mothers at age 15 or younger complete 2.8 fewer years of school than their childless peers by the time they reach age 24; this is almost twice the educational deficit observed at age 18. Therefore, it appears that early childbearing poses more than a temporary setback for these young mothers. The educational losses associated with early childbearing persist over time and even increase at least through age 24.

#### FACTORS IN EDUCATIONAL ATTAINMENT

The factors that are important in the process of educational attainment differ substantially for those who bear a child in adolescence and those who delay motherhood. Our data (not shown) indicate that among those who postpone childbearing, motivation of the individual and encouragement or help from others are the most important factors affecting years of schooling completed at age 24; but these variables have much less effect among those who become mothers in their teens. For this group, the characteristics of the family of orientation are most important to eventual educational attainment.<sup>15</sup> An advantageous family background probably acts by providing motivation and support for those with normal educational careers uninterrupted by a birth. Early motherhood may change this process by making a young woman unable to act on her aspirations, plans or ability or on the encouragement she receives from others; by changing the likelihood that she will receive help or encouragement from her parents and teachers, or by lowering her parents' and her own expectations for her attainment.

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Footnotes at end of article.

TABLE 3.—UNSTANDARDIZED AND STANDARDIZED PARTIAL COEFFICIENTS FOR REGRESSION OF EDUCATIONAL ATTAINMENT AT AGE 24 ON RESPONDENTS' AGE AT FIRST BIRTH, FAMILY BACKGROUND AND SOCIAL AND DEMOGRAPHIC TRAITS, BY RACE

Independent variable	White (N=1,022)		Black (N=252)	
	Unstand- ardized coefficient	Standardized coefficient	Unstand- ardized coefficient	Standardized coefficient
AFB 15-16	†-3.110	†-0.178	-1.440	-0.149
AFB 16-17	†-1.530	†-.175	-.738	-.109
AFB 18	†-1.020	†-.126	-.028	-.003
AFB 19-20	†-.745	†-.123	.414	.064
AFB 21-23	†-.300	†-.052	.261	.038
AFB ≥24				
Number of siblings	*-.051	*-.047	-.038	-.043
Occupation of head	-.000	-.007	-.007	-.037
Mother's education	*.046	*.058	.051	.067
Father's education	*.058	*.088	.016	.023
Intact family	-.095	-.013	.493	.090
Home culture index	†.308	†.114	.434	.191
Parents' educational goal	†.643	†.137	*1.334	*.260
Parent-teacher help	†.132	†.152	*.255	*.230
High school curriculum	†.328	†.276	.938	.137
Age in 1968	-.036	-.022	.052	.088
South	.003	.001	.052	.010
R <sup>2</sup>		.571		.489

\*p < .05.  
 †p < .001.  
 ‡p < .01.

In discussion of Table 2, we noted the absence of an effect of race on educational attainment once the influences of age at first birth, family background and motivational factors are removed. The process by which educational attainment is determined appears to work differently for the two races, with blacks less able than whites to convert high parental status into attainment for themselves.<sup>16</sup> None of the studies of the process of educational attainment by race have examined racial differentials in the educational attainment of females. We have hypothesized that black females suffer less of an educational disadvantage from adolescent childbearing than do their white peers. Since teenage parenthood is much more common among blacks than among whites, social mechanisms for dealing with this occurrence may be better established among black families and in school systems with a high proportion of black students. Other evidence suggests that the presence of babies and young children interrupts the lives of black women less than those of white women. For example, black women are more accepting of employment of mothers of young children than are white women, and labor force participation rates reflect this difference.<sup>17</sup>

To test this reasoning, we estimated the regression model of education attainment at age 24 separately for whites and blacks. The results are shown in Table 3. To compare the effect of a first birth at a particular age on the attainment of black and white women, one needs only to examine the unstandardized coefficients for that age-at-first-birth category. As the reader will note, the hypothesis receives strong support. A first birth at age 15 or younger results in twice the educational decrement for young white women that it does for young black women—3.1 versus 1.4 years of schooling, respectively. At every age of first birth, the effect is smaller for blacks than for whites.\*

\* One could argue that the smaller negative impact on blacks of a birth during the early teens might be due simply to the lower average educational attainment of blacks than of whites in the United States. But a restriction in the range of education does not appear to be the explanation of this finding. While the average number of years of schooling completed at age 24 is somewhat lower for blacks than for whites (11.3 versus 12.6), the standard deviations are very similar (2.6 for blacks, 2.4 for whites).

Footnotes at end of article.

TABLE 4.—UNSTANDARDIZED AND STANDARDIZED PARTIAL COEFFICIENTS FOR REGRESSION OF EDUCATIONAL ATTAINMENT AT AGE 24 ON AGE AT FIRST BIRTH, FAMILY BACKGROUND, SOCIAL AND DEMOGRAPHIC TRAITS, BY WHETHER THE FIRST BIRTH OCCURRED IN THE SAME OR EARLIER YEAR THAN TERMINATION OF EDUCATION OR LATER THAN TERMINATION OF EDUCATION

Independent variable	AFB ≤ age at termination of formal education (N = 128)		AFB > age at termination of formal education (N = 875)	
	Unstandardized coefficient	Standardized coefficient	Unstandardized coefficient	Standardized coefficient
AFB-LT-16	†-4.220	†-0.617	†-3.004	-0.122
AFB 16-17	†-2.412	†-.489	†-1.756	-.165
AFB 18	†-1.906	†-.282	†-.831	-.088
AFB 19-20	†-1.418	†-.246	†-.515	-.083
AFB ≤ 21				
Number of siblings	.013	.013	°.051	°.049
Occupation of head	.006	.060	-.002	-.017
Mother's education	-.026	-.035	°.057	°.073
Father's education	.085	.144	†.058	†.088
Intact family	-.257	-.047	†.054	†.018
Home culture index	°.383	°.157	†.309	†.121
Parents' educational goal	.488	.105	†.648	†.136
Parent-teacher help	.078	.084	†.150	†.169
High school curriculum	.335	.070	†.389	†.282
Age in 1978	-.004	-.003	-.009	-.006
Race	-.272	-.045	-.064	-.008
South	-.312	-.065	.102	.020
R <sup>2</sup>		.667		.570

† $p < .001$ .

‡ $p < .01$ .

° $p < .05$ .

† Limited category.

\* $p < .05$ .

The results also indicate that for black but not for white women, no educational advantage is derived from delaying the onset of motherhood beyond 18. Black women who are childless at 24 do not complete significantly more years of formal schooling than classmates who bore their first child at ages 19-23. These findings may in part be due to the fact that substantially more white than black women were childless at 24 (42 percent versus 29 percent). Delay of a first birth past the early twenties is more frequently related to college attendance among whites than among blacks.

#### SCHOOL DROPOUT: CAUSE OR EFFECT

Does the occurrence of a pregnancy during the school years cause young women to leave school, or are those who have dropped out more likely to have a child while in their teens? We have hypothesized that in many instances, pregnancy and childbearing cause the termination of formal schooling, suggesting that more education would have been completed had this event not occurred. To test this reasoning, we divided the young women in the sample who had borne a child by age 24 into two groups: those who clearly had their first birth in the year before, or in the same year they left school; and those whose first child was born either in the year following the termination of their education or later. Since some young women who become pregnant while still in school may have dropped out six to eight months before the delivery, an unknown number of those who had a first birth in the year after they left school dropped out because of the impending birth. But for the young women who had a baby in the year they left school or in an earlier year, one can argue that the early childbirth occurred before the termination of education and quite probably caused it, at least in part.\*

As Table 4 shows, the young women who bear a child and then leave school ex-

\* A more conclusive test would have been to control for the grade attainment of the young women at age 15, before most pregnancies occurred. Unfortunately, these data are not available for this sample.

perience substantially greater educational deficits from a birth during the early teens than from a birth at older ages. Young women attending school who have a first birth at age 15 or younger complete four years less schooling than young women who first become mothers between ages 21 and 24.

The entire sample undoubtedly includes other women whose education was shortened by childbearing. For example, some women may have dropped out, intending to return but finding their plans disrupted by childbirth. It is important to remind the reader that pregnancies which end in abortion, miscarriage, adoption or foster care are not reported here. Therefore, although these pregnancies may have interrupted the educational careers of young women, we are unable to measure their effects. In addition, as noted above, young women who quit school early in pregnancy but do not report a birth until the interview in the following year are not included in the group for whom we are arguing that early childbearing causes the termination of formal schooling. This may mean that our estimates of the effects of an early first birth on educational attainment understate the true impact of this event.

#### CONCLUSIONS

These data show that early childbearing is strongly associated with a lower level of educational attainment, especially among young women attending school at the time of the birth of the first child, even when other factors known to affect educational attainment are taken into account. The negative impact of early childbearing on a woman's educational attainment is probably due to the difficulty and cost of arranging child care and running a household (if the woman heads her own household or is married), to the necessity of earning a living, and, not least, to the pressures she may encounter from family and friends to devote herself to child care.

There is no evidence that the young mother is ever able to catch up educationally with her childless peers. In fact, quite the opposite occurs; teenage mothers are unable to catch up and fall further behind their former classmates who have postponed parenthood.

Of the many family background factors, race is given special attention. It appears that while both black and white women are handicapped in their schooling by early pregnancy, the effect is much greater among white women. This may result from more accepting social attitudes and from the presence of social mechanisms in the black community for dealing with the more common phenomenon of teenage pregnancy. There is, however, no consistent effect for race when other factors (including age at first birth) are controlled for in the analysis.

Given the association between education and other measures of accomplishment, such as occupation and earnings, it seems likely that early childbearing greatly diminishes the overall attainment of women, suggesting potentially significant lifetime consequences that place young mothers at a permanent disadvantage. In view of other data indicating that adolescent pregnancy is typically unintended,<sup>13</sup> the importance of sex education, family planning, abortion and educational programs for adolescents should be clear.

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SOCIAL SERVICES,  
Salt Lake City, Utah, January 3, 1979.

HON. DANIEL PATRICK MOYNIHAN,  
U.S. Senator, U.S. Senate,  
Committee of Finance, Washington, D.C.

DEAR SENATOR MOYNIHAN: I apologize for my delay in responding to your letter of November 24, 1978. I have reviewed the transcript of the hearings and have indicated any changes which I feel should be made and have attached a copy of the data provided to the committee during the hearing.

I have also enclosed a chronology of the correspondence between the Regional Office and the Utah Department of Social Services concerning the submission and approval of plan material to the Denver HEW Regional Office.

Hopefully, this data will provide the information you need relative to the plan material as it was submitted and final draft of the deliberations which ensued between submission and approval.

Our currently approved plan material is not time-limited and, therefore, no termination date has been set. However, officials from the Office of Family Assistance have been in contact with Utah and we are, shall I say, "encouraged" to make appropriate modifications to our state plan material which will bring it into conformity with HEW's interpretation of the Social Security Act. Their prime concern seems to be in two areas:

1. The current Utah state plan requires all able welfare recipient to participate in the WEAT project as a condition of their eligibility for AFDC assistance. HEW contends that this is in direct contradiction with the Social Security Act.

2. The fact that Utah's WEAT program is not time-limited. That is, an individual can be assigned to a WEAT project for as long as they are on public assistance. HEW would require that the assignment be time-limited.

Utah wants to continue to maintain its WEAT program as it is currently implemented. We recognize that WEAT is not the solution for all states, however, it does work in the State of Utah.

It appears to us that one way to get around the problem which we have with HEW would be for Congress to amend the Social Security Act making work projects permissible under the Act. Such permissive language would allow states

the latitude of implementing or not implementing work projects based upon the needs of each particular state.

I appreciated the opportunity of appearing before your committee and would be pleased to provide you with an additional information you may request.

Sincerely,

NOBMAN G. ANOUS,  
Deputy Director, Public Entitlements.

Enclosures.

[Attachment A]

RECORD FOR RECONSIDERATION, DISALLOWANCE—UTAH WORK PROJECT \$539,461  
MA-UT7601

1. June 18, 1974.—Transmittal and Notice OPC-11 from Paul S. Rose, Executive Director, Utah State Department of Social Services to James R. Burress, SRS Regional Commissioner, Region VIII, re: Transmittal of State Plan Material—transmittal No. 74-38 (APA).

2. July 19, 1974.—Letter from Usher West, Works Project Coordinator, Utah Department of Social Services to Wayne Sage, SRS Acting Associate Regional Commissioner for Assistance Payments, re: Legislative intent regarding Public Assistance and Work Projects Agreement.

3. July 19, 1974.—Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal No. 74-58 (APA).

4. July 19, 1974.—Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal No. 74-58 (APA).

5. August 19.—Letter from D. D. Williams, Deputy Executive Director, Utah Department of Social Services to Francis T. Ishida, Acting Regional Commissioner, SRS, re: Utah Legislature Mandate to Establish Work Projects.

6. September 11, 1974. Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal 74-94 (APA).

7. September 16, 1974.—Letter from Roy Cooper, Director, Office of Assistance Payments Administration, Utah Department of Social Services to Wayne Sage, re: Travel Expense in Addition to the AFDC Grant.

8. September 27, 1974.—Letter from Paul S. Rose to Francis T. Ishida, re: Request for Extension of Approval of AFDC Work Project Program.

9. November 1, 1974.—Letter from James R. Burress to Paul S. Rose, re: 90 Day extension on Utah submittal 74-38.

10. November 8, 1974.—Letter from D. D. Williams to James R. Burress, re: Adm.—State Plan (AP)—Public Assistance—Work Projects.

11. November 8, 1974.—Memorandum from Milton Anderson, SRS State Grants Manager for Utah to C. Salazar, Jr., SRS Assistant Regional Commissioner for Management, re: Review of Utah OA-41's for the quarter ended 9-30-74.

12. January 1, 1975.—Memorandum from John A. Svahn, SRS Commissioner, Assistance Payments Administration to Wayne Sage, re: Utah—AFDC Work Project "Bulletin 74-28-APA" of May 28, 1974.

13. January 9, 1975.—Letter from James R. Burress to Paul S. Rose, re: Review of the Quarterly Expenditure Report for the quarter ended September 30, 1974, and the Quarterly Expenditure Estimate for the quarter beginning January 1, 1975.

14. January 24, 1975.—Letter from Paul S. Rose to James R. Burress, re: Plan Material—Request for Extension of 90 Day Period.

15. January 29, 1975.—Letter from Wayne B. Sage, Associate Regional Commissioner, Assistance Payments, SRS, to Paul S. Rose, re: OPC-11 Transmittal Number 74-94 (APA).

16. February 4, 1975.—Letter from Paul S. Rose to James R. Burress, re: Combining OPC-11, 75-5 and 74-83 with OPC-11, 74-38.

17. February 10, 1975. Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal No. 75-8 (APA).

18. February 13, 1975.—Memorandum from Milton Anderson to C. Salazar, Jr., re: Comments and recommendations on review of OA-41's for the period ended 12-31-74.

19. February 21, 1975.—Letter from James R. Burress to Paul S. Rose, re: Transmittal No. 74-76 (APA).



20. February 25, 1975.—Memorandum from James R. Burress to James S. Dwight, Jr., SRS Administrator, re: Disapproval of Utah Work Project Plan Material.
21. March 1, 1975.—Memorandum from Ray Myrick, SRS Assistant Regional Commissioner for Assistance and Services, Region VIII to Nicholas Norton, Acting Commissioner, Assistance Payments Administration, re: Questions Concerning Utah Work Project.
22. March 3, 1975.—Letter from James R. Burress to Paul S. Rose, re: Changes on OA-41, Quarterly Statement of Expenditures ending 12-31-74.
23. March 5, 1975.—Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal No. 75-10 (APA)
24. March 6, 1975.—Memorandum from George J. Ivans, Deputy Director, Regional WIN Unit SRS to Merwin S. Hans, Executive Director, National Coordination Committee Office of Work Incentive Programs, re: Status report of the Utah work projects for the first six months.
25. March 11, 1975.—Letter from James R. Burress to Paul S. Rose, re: Submittal of Plan Material. (Transmittal 74-38).
26. March 18, 1975.—Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal No. 75-11 APA.
27. April 14, 1975.—Letter from C. Salazar, Jr., to Paul S. Rose, re: Deferral of Items for Federal Financial Participation.
28. April 15, 1975.—Letter from Paul S. Rose to James R. Burress, re: Deferral of Expenditures.
29. April 16, 1975.—Memorandum from Milton Anderson to C. Salazar, Jr., re: Utah Deferred Payments.
30. April 21, 1975.—Letter from James R. Burress to Paul S. Rose, re: Deferred expenditures.
31. April 30, 1975.—Letter from Wayne B. Sage to Paul S. Rose, re: OPC-11 Transmittal No. 75-10 (APA).
32. May 9, 1975.—Memorandum from Milton Anderson to C. Salazar, Jr., re: Review of OA-41's for quarter ended 3-31-75.
33. June 20, 1975.—Memorandum from Merwin S. Hans to Robert J. Brown, Assistant Regional Director for Manpower U.S. Department of Labor, re: Staff Visit to Utah Work Project.
34. June 24, 1975.—Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal No. 75-24 (APA).
35. July 11, 1975.—Letter from James R. Burress to Paul S. Rose, re: Basis for Determining Deferred Costs.
36. August 20, 1975.—Memorandum from Milton Anderson to C. Salazar, Jr., re: Review of OA-41's—Utah, Fourth Quarter fiscal year 1975.
37. October 8, 1975.—Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal No. 75-49 (APA).
38. January 20, 1976.—Memorandum from Gary Peterson, Assistant Payment Specialist, SRS, to Wayne Sage, re: Utah—Trip January 4-5 and 14-16, 1976
39. January 20, 1976.—Letter from James R. Burress to Paul S. Rose, re: Deferral of a claim for the Works Project on AFDC OA-41.
40. March 29, 1976.—Memorandum from James R. Burress to W. Kent Dickson, Director, Division of State Grants Administration, SRS, re: RC's Decision on the OA-41 for the AFDC Program in Utah for the Quarter Ended December 31, 1975.
41. April 1, 1976.—Transmittal and Notice OPC-11 from Paul S. Rose to James R. Burress, re: Transmittal of State Plan Material—Transmittal No. 76-12 (APA).
42. April 5, 1976.—Letter from Ray Myrick, Jr. to Paul S. Rose, re: Acknowledgement of Receipt of State Plan Submittal PCO #76-12.
43. May 12, 1976.—Letter from Paul S. Rose to James R. Burress, re: Substitution of pages in OPC-11 No. 74-38 (APA).
44. May 14, 1976.—Letter from James R. Burress to Paul S. Rose, re: Substitution of certain pages in Transmittal 74-38.
45. May 14, 1976.—Memorandum from Ray Myrick, Jr. to James R. Burress, re: Utah—Transmittal Number 74-38.

46. May 17, 1976.—Letter from James R. Burress to Paul S. Rose, re: Disallowance of Work Project Expenditures.

47. May 18, 1976.—Letter from James R. Burress to Paul S. Rose, re: Clarification of letter date May 14, 1976, regarding approval of Utah Transmittal Number 74-38.

48. May 19, 1976.—Memorandum from James R. Burress to Don I. Wortman, Acting Administrator, SRS, re: Utah—Work Project.

49. June 11, 1976.—Letted from Paul S. Rose to Don I. Wortman, re: Reconsideration of May 17, 1976 disallowance of Work Project Expenditures.

50. June 25, 1976.—Letter from James R. Burress to Paul S. Rose, re: Utah AFDC Work Project transmittal No. 74-38 (APA).

51. June 29, 1976.—Memorandum from Mildred N. Hoadley, Director, Division of Income Maintenance Policy, SRS to Nicholas Norton, re: Utah—Work Experience and Training Programs.

52. July 12, 1976.—Memorandum from C. Salazar, Jr. to Randolph W. Lee, Director, SRS Division of Special Claims, re: Utah request for reconsideration dated June 11, 1976.

53. September 10, 1976.—Memorandum from Clarence M. Coster, SRS Associate Administrator for Management to James R. Burress, re: Acknowledgement of receipt of reconsideration request.

54. September 10, 1976.—Letter from Clarence M. Coster to Paul S. Rose, re: Acknowledgement of reconsideration request:

55. September 15, 1976.—Memorandum from James R. Burress to to Robert Fulton, SRS Administrator, re: Transmittal of record for reconsideration.

56. October 28, 1976.—Letter from Clarence M. Coster to Paul S. Rose, re: Transmittal of record for reconsideration to the State for comments.

57. November 29, 1976.—Letter from Paul S. Rose to Clarence M. Coster, re: Forwarding additional material for the record—Utah Work Project.

December 22, 1976.—Letter from Clarence M. Coster to Paul S. Rose, re: Addition of material to the record.

59. January 26, 1977.—Memorandum from Francis T. Ishida to Robert Fulton, re: Regional Commissioner's Analysis of the Issues, Utah Work Project Expenditures.

60. Unknown.—Letter from the Executive Director, Utah Department of Social Services to the Administrator, SRS providing comments on the regional Commissioner's Analysis.

61. March 4, 1977.—Letter from Dr. Anthony W. Mitchell, Executive Director Utah Department of Social Services to Mr. Francis Ishida, Acting Regional Commissioner Region VIII, requesting a conference.

62. April 25, 1977.—Letter from Clarence M. Coster to Dr. Anthony W. Mitchell acknowledging the request for a conference.

63. July 11, 1977.—Letter from Anthony W. Mitchell to James B. Cardwell, Commissioner of Social Security requesting that a conference be held in September or October.

JANUARY 12, 1979.

SENATOR DANIEL PATRICK MOYNIHAN,  
U.S. Senate, Committee on Finance,  
Washington, D.C.

DEAR SENATOR MOYNIHAN: In my testimony on November 15, 1978, you requested that I prepare a statement as to my views of the ethics of conducting a social experiment under conditions in which some persons in the experiment can suffer harm. Specifically, you were concerned with the ethics of conducting the income maintenance experiments, which appear to increase the rate of marital separation and thus cause some children to live in single-parent homes who otherwise would have lived in two-parent homes.

In discussing the ethics of conducting social experiments, there would seem to be three considerations: (1) Are the participants at risk of suffering harm? (2) Is there informed consent by the participants? and (3) Does society and/or the participants stand to gain from the results of the experiment? I will not attempt to deal generally with the ethics of social experiments, but rather to confine my observations to the specific concerns regarding the NIT experiments. Defining "harm" is not simple in a social experiment. In a medical experiment all may agree that an illness induced by the experimental treatment is harmful. Since the treatment in the NIT is essentially the provision of additional income, the treatment itself cannot be judged to be harmful. Rather, it is the participant's response to the treatment that may cause harm. But harm in whose terms? If

the individual knowingly chooses a response, then presumably he is better off making that response. For example, a response to the NIT treatment can be to reduce hours of work or to get a divorce. Since these responses are voluntary, harm is only a relevant consideration if the responses are not based on reasonable information provided the participants, and that participation in the experiment is not voluntary. In other words, is there "informed consent?"

In the NIT experiments, consent is obtained by requiring an enrollment agreement to be signed by the agents of the experiment and the family. This agreement explains the rights and obligations of the family members. The family may accept or reject membership. Furthermore, each family member has the ability to withdraw from the experiment at any time without penalty. Those families who were previously on welfare (AFDC), were guaranteed the ability to return to that program.

With regard to consent being informed, it is necessary that the individual has an ability to decide what is in his own best interest. As pointed out by Peter Brown, the two issues of information and risk of harm are highly related.<sup>1</sup> The higher the risks involved in the experiment, the greater the need that information about the experiment be imparted. Thus, in experiments in which the individual or family could suffer harm, the risks should be spelled out clearly and completely. This does not mean that it is necessary to inform the family of the overall purposes of the experiments. Thus, the fact that the income maintenance experiments are interested in measuring the effects on work effort does not have to be conveyed to the recipients. Conveying such information could change family behavior and nullify the experiments. What is necessary is that the family know how it will be directly affected by the experiment. The family should know what it is giving up in order to take part in the experiment (i.e., other welfare benefits) and how its behavior will determine the payments to the family. This creates no difficulty because complete information about how family behavior affects payments is essential not only for the ethical concerns but also for the success of the experiment. Furthermore, the income maintenance experiments have no "hidden" treatment as may exist, for example, in a birth control experiment in which some people are given placebos when they think they have been given birth control pills. This would be a violation of the informed consent provisions, and no such violation occurs in the income maintenance experiments.

The issue is complicated by the fact that the decision to participate in the experiment is that of the adult parent and not that of the child and it is potential harm to the child that is of concern here. This concern, however, is more a reflection on the parent-child relationship than on the relationship of the experiment to the child. It is well recognized that parents have a wide latitude within both the law and social custom regarding decisions that affect the child. A parent is not supposed to make decisions that are directly adverse to the child's well-being. As stated by Paul Ramsey, "The parent has a sacred obligation to protect the child's interest."<sup>2</sup> However, society does not require that a parent obtain the consent of the child to make decisions that affect the child's well-being. We cannot impose upon the participants in an experiment a set of consensual standards that differ from those imposed by society apart from the experiment. To do so would be an unnecessary infringement upon the rights of the participants. It would also compromise the experiment by introducing an artificial difference between the experimental situation and the real world. Parental decisions to divorce, remarry, quit work, or move to another town affect children both within and outside the experiment. The experiment does not (and should not) alter this parent-child relationship.

The last issue is benefits. These are twofold. There is potential benefit to both the individual and society as a whole from the conduct of the experiment. The main justification for conducting a social experiment is to gain information to improve the efficiency and effectiveness of social policy. There may be unanticipated side effects, however. If indeed as a result of the experiment harm is caused to participants, for example, an increase in the marital dissolution rate (if it is regarded as a harmful outcome), then it follows that the information in this case may be the most valuable of all. These unintended harms befall a very small number of individuals, whereas if the program had been harmed without the experiment, the number harmed may be far greater. Thus, societal

<sup>1</sup> See P. Brown, "Informed Consent in Social Experimentation: Some Cautionary Notes," in Rivlin, A.M. and P.M. Timpane, eds., *Ethical and Legal Issues of Social Experiments*, Brookings Institution, 1975.

<sup>2</sup> See Rivlin and Timpane, p. 42.

benefits from a social experiment are particularly high if that experiment uncovers a harmful effect of a policy that has a high probability of being implemented.

There should also be benefits to the individuals taking part in these experiments. One form of benefit may be the satisfaction gained from accepting risk of harm by an altruistic individual who wishes to aid society. In the case of income maintenance, however, no such altruism was required as the individual families could be expected to participate only if they received direct and immediate benefits. These benefits came in the form of increased cash incomes. One may argue that individuals could only perceive correctly their short-run benefits and may badly misjudge their long-term benefits which might not have been as positive. This is true, but irrelevant. Individuals make decisions for both short- and long-term gains. Long-term effects are always seen with much less certainty than short-term effects. If the individual was fully informed with regard to issues that influence longer term effects, such as the date the experiment would terminate, and the conditions that would be provided to him at the point of termination there is no special problem. Whether an individual sees the long-term effects of divorce or quitting a job is really not relevant to the informed consent doctrine for an experiment. This is part of a normal behavior process and the individual must evaluate the pros and cons of taking those actions as well as he can. Similarly, he must make such an evaluation when he decides to join the experiment.

We hold that the income maintenance experiments are on firm ethical grounds in that the members of the experiment are well-informed as to the benefits and costs of participation, that their voluntary consent is obtained and can be rescinded at any time, and that essentially participation is only made on the basis of an individual's benefit/cost calculation.

Sincerely,

ROBERT G. SPIEGELMAN,  
*Director, Center for the Study of Welfare Policy.*

