United States General Accounting Office

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Briefing Report to the Chairman, Committee on Finance, U.S. Senate

April 1992

UNEMPLOYED PARENTS

An Evaluation of the Effects of Welfare Benefits on Family Stability

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Program Evaluation and
Methodology Division

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April 29, 1992

The Honorable Lloyd Bentsen
Chairman, Committee on Finance
United States Senate

Dear Mr. Chairman:

This is the second report responding to your request for information on the Aid to Families with Dependent Children—Unemployed Parents (AFDC-UP) program. Our first report (Unemployed Parents: Initial Efforts to Expand State Assistance, GAO/PEMD-92-11, January 1992) presented descriptive data on how the states responded to the mandate in the Family Support Act of 1988 to expand the previously voluntary UP portion of the AFDC program to all states. On February 12, 1992, we briefed your staff on the results of our second review, which are summarized in this report. Addressing your interest in the effect of the AFDC-UP program on family stability, the report finds mixed evidence on whether the presence of the program affects AFDC-Basic caseloads.

This report does not provide conclusive support for either side of the debate over the possible consequences of the recent expansion of AFDC-UP. Proponents of the expansion of the program argued that the availability of this assistance would encourage stability among poor families. They reasoned that the availability of assistance for poor two-parent families would remove the incentive for families experiencing economic hardships to separate in order to receive benefits from the AFDC-Basic program (which are reserved primarily for single parents). In contrast, opponents of the expansion argued that the addition of AFDC-UP benefits could decrease family stability in the long term and increase the AFDC-Basic caseload by undermining the role of the parents in providing support for their children.

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Similarly, it has been argued, the elimination of a state's AFDC-UP program could result in either an increase or a decrease in family stability. The elimination of the program could drive poor two-parent families experiencing hardships to separate and qualify for AFDC-Basic benefits as mentioned above. (Most two-parent families able to qualify for AFDC-UP could, in the event of a dissolution, qualify for AFDC-Basic.) However, the elimination of the program might build parents' self-reliance and increase family stability in the long term, resulting in slower growth in AFDC-Basic. Under any of these scenarios, we would expect changes in caseload growth

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to be small because the AFDC-UP program generally serves less than 10 percent of the number of families served by the AFDC-Basic program.

This report uses state data on trends in AFDC-Basic caseloads during 1975-89 to explore these competing theories. During this period, some states started and others eliminated the AFDC-UP program. For 8 of these states, we developed regression models of the AFDC-Basic caseload that adjusted for a number of factors other than AFDC-UP that might affect the caseload. We then examined the effect of AFDC-UP on caseload growth in the context of the other factors included in our models. (For a detailed description of our methods, see appendix I.)

Our analyses provide a limited test of opposing theories about the relationship between the availability of AFDC-UP benefits and family stability. We used AFDC-Basic caseloads as a proxy for family stability; a direct test was precluded by the absence of regular data series on, for example, separations and divorces among families potentially eligible for AFDC. Where they were available, however, we also modeled variables that should be more sensitive to the hypothesized effect on family stability—for example, the number of new cases added and the number of cases approved specifically because a caretaker left the home and reduced support to the children.

The results of our analyses suggest that the presence of an AFDC-UP program either decreases or has no effect on growth in the number of families receiving AFDC-Basic. In 4 states—Colorado, Maine, Montana, and Oregon—the presence of an AFDC-UP program was associated with decreased growth in the AFDC-Basic caseload. While in Montana the association could be explained by a policy change that occurred near the time of the AFDC-UP intervention, in Oregon we could find no plausible alternative explanation for the decreased rate of growth in the Basic caseload during the AFDC-UP program. The policy change that occurred at or near the time of the AFDC-UP interventions in Colorado and Maine did not, however, provide a strong alternative to AFDC-UP as an explanation of the association found between UP and Basic caseloads. In the remaining 4 states—Missouri, South Carolina, Utah, and Washington—the presence of the AFDC-UP program was not associated with any change in the AFDC-Basic caseload. However, these results are also inconclusive because of the presence in each state of a coincident policy change. Notably, in none of the 8 states was there evidence that the AFDC-UP program was associated with higher AFDC-Basic caseloads or higher rates of growth. (See section 2 for the details of our results.) But whatever the evidence, it is important to remember that the findings from these 8 states cannot be generalized to the nation. (See section 1 for a discussion of generalizability.)

Do these findings of no change or lower rates of growth in the AFDC-Basic caseload translate into a finding that the AFDC-UP program increased family stability? The evidence that this occurred is strongest in Oregon; in the other states, other policy changes that occurred in the same time period may account for some part of the effects. In addition, the number of AFDC-Basic cases may grow at a slower rate when AFDC-UP benefits are available for reasons other than a change in rates of marital dissolution. For example, eligibility officials might use AFDC-UP instead of the AFDC-Basic benefits for two-parent families in which one of the parents is incapacitated. Finally, as in any regression analysis, unidentified variables that were omitted from the models but correlated with the change in AFDC-UP policy could alter our results. (Appendix I describes the types of variables that we used in developing our models.)

We requested and received comments on our draft report from officials of the U.S. Department of Health and Human Services (HHS). (See appendix II.) They identified two principal concerns. First, previous research has suggested that cash assistance destabilizes families. However, as detailed in a May 1988 GAO report entitled Welfare Reform: Projected Effects of Requiring AFDC for Unemployed Parents Nationwide, this research was not conclusive for families with children and tested the effects of an experimental program quite different from AFDC-UP. Second, HHS noted that the changes we found in AFDC-Basic caseloads cannot be causally linked to the AFDC-UP program because we used a nonexperimental research method. Although experimental research could be more conclusive than the quasi-experimental design we used, experimentation on the AFDC-UP program is an unlikely if not impossible evaluation strategy because it would require denying benefits to eligible families. In addition, we chose a modeling technique that allowed us to control for a variety of variables that might also affect AFDC-Basic caseloads and we explored alternative explanations with state officials.

As arranged with your office, we will be sending copies to the Secretary of Health and Human Services and to others upon request. If you have any questions or would like additional information, please call me at (202) 275-1854 or Robert York, Director of Program Evaluation in Human Services Areas, at (202) 275-5885. Other major contributors to this report are listed in appendix IV.

Sincerely yours,

Eleanor Chelimsky

Eleanor Chelimsky
Assistant Comptroller General

Contents

Letter		1
Section 1 Objectives, Scope, and Methodology		10
	Objectives	10
	Scope	11
	Methodology	11
Section 2 Does AFDC-UP Influence the Size or Growth Rate of AFDC-Basic Caseload?		15
	States in Which AFDC-UP Was Consistently Associated With Slower Growth in AFDC-Basic Caseload	17
	States in Which There Was No Consistent Evidence of an Association Between AFDC-UP and AFDC-Basic Caseload or Its Rate of Growth	24
Appendixes	Appendix I: Statistical Analysis of the Effect of AFDC-UP Policy on AFDC-Basic Caseload	32
	Appendix II: Comments From HHS	49
	Appendix III: Advisory Panel Members and Technical Consultants	51
	Appendix IV: Major Contributors to This Report	52
Bibliography		53
Related GAO Products		56
Tables	Table 1.1: Participation in AFDC-UP by Eight States, Fiscal Years 1974-89	12
	Table 2.1: Classification of States by Results of Analyses and Identification of Confounding Factors	15
	Table I.1: Generalized Least Squares Model of Colorado's Monthly AFDC-Basic Caseload, January 1978 to December 1987	39
	Table I.2: Generalized Least Squares Model of Maine's Monthly AFDC- Basic Caseload, October 1978 to December 1988	40
	Table I.3: Generalized Least Squares Model of Missouri's Monthly AFDC-Basic Caseload, January 1975 to September 1989	41

Table I.4: Generalized Least Squares Model of Montana's AFDC-Basic Monthly Caseload, July 1978 to June 1988 42
Table I.5: Ordinary Least Squares Model of Oregon's Monthly Openings in AFDC-Basic, June 1980 to December 1987 43
Table I.6: Generalized Least Squares Model of Oregon's Monthly Basic Caseload, June 1980 to December 1987 44
Table I.7: Generalized Least Squares Model of South Carolina's Monthly AFDC Approvals for Loss of Support, July 1979 to December 1987 45
Table I.8: Generalized Least Squares Model of South Carolina's Monthly Basic Caseload, July 1976 to December 1988 46
Table I.9: Generalized Least Squares Model of Utah's Monthly Basic Caseload, July 1976 to December 1987 47
Table I.10: Generalized Least Squares Model of Washington's Monthly Basic Caseload, January 1976 to December 1987 48

Figures

Figure 2.1: Actual and Modeled AFDC-Basic Openings in Oregon, June 1980 to December 1987 18
Figure 2.2: Actual and Modeled AFDC-Basic Caseload in Oregon, June 1980 to December 1987 19
Figure 2.3: Actual and Modeled AFDC-Basic Caseload in Colorado, January 1978 to December 1987 20
Figure 2.4: Actual and Modeled AFDC-Basic Caseload in Maine, October 1978 to December 1988 22
Figure 2.5: Actual and Modeled AFDC-Basic Caseload in Montana, July 1978 to June 1988 23
Figure 2.6: Actual and Modeled AFDC-Basic Caseload in Missouri, January 1975 to September 1989 25
Figure 2.7: Actual and Modeled Approvals for Loss of Support in South Carolina, July 1979 to December 1987 27
Figure 2.8: Actual and Modeled AFDC-Basic Caseload in South Carolina, July 1976 to December 1988 28
Figure 2.9: Actual and Modeled AFDC-Basic Caseload in Utah, July 1976 to December 1987 29
Figure 2.10: Actual and Modeled AFDC-Basic Caseload in Washington, January 1976 to December 1987 30

Abbreviations

AFDC	Aid to Families with Dependent Children
AFDC-UP	Aid to Families with Dependent Children – Unemployed Parents
ARIMA	Autoregressive integrated moving average
DEFRA	Deficit Reduction Act of 1984
GLS	Generalized least squares
HHS	U.S. Department of Health and Human Services
OBRA	Omnibus Budget Reconciliation Act of 1981
OLS	Ordinary least squares
UP	Unemployed Parents

Objectives, Scope, and Methodology

Objectives

When enacted in 1935, the Aid to Families with Dependent Children (AFDC) program did not provide cash benefits to families if both parents lived at home unless one of the parents was disabled. In 1961, under the Unemployed Parent (UP) segment of AFDC, states were first given the option to provide AFDC benefits to needy two-parent families in which the major earner was unemployed. Just over half of the 54 states and territories used that option before the program was extended to all states in 1990. The Family Support Act of 1988 expanded UP benefits to all states; however, it also allowed states that did not previously offer UP benefits to limit assistance to no fewer than 6 months in any 12-month period and allowed states to require the participation of one or both parents in an employment or training program.

The expansion of the UP program did not occur without substantial debate on both sides of the issue. Proponents of extending the UP program argued that if benefits were not available to two-parent families, those families would be more likely to separate in order to qualify for benefits available to single-parent families. The opponents cited findings from negative income tax experiments as evidence that broadening access to UP would be harmful to family stability.[1] While analyses of the negative income tax experiments in two of the research sites showed that the couples receiving a guaranteed income were more likely to separate than those who did not, these findings cannot be clearly generalized to UP families. Among other issues, the analyses included couples without children, for whom the effect of cash benefits was stronger than for couples with children.

We examined UP's effect on the stability of poor two-parent families by analyzing the association between changes in the UP program before the Family Support Act and subsequent changes in the number of cases in Basic, a program that primarily serves single-parent families. Between fiscal years 1961 and 1990, the period when UP was optional, 32 states used the option to provide AFDC cash benefits to the unemployed and partly employed. Several of these states suspended the program for at least a year after beginning to offer it. Taking advantage of the starts and stops of the UP program in 8 states prior to the Family Support Act, we investigated whether the addition or elimination of a UP program was associated with changes in a state's Basic caseload.

[1]For a detailed summary of this research, conducted between 1968 and 1978, and its relevance to the AFDC-UP program, see U.S. General Accounting Office, May 1988. See also Cain and Wissoker, 1990, and Hannan and Tuma, 1990.

Scope

We used Basic caseload as an indicator of family stability for two reasons. First, most two-parent families able to qualify for UP would, in the event of a marital dissolution, qualify for Basic. Thus, it is logical to link the number of Basic cases to family instability among UP recipients and among poor, two-parent families who would have been eligible for UP had it been available. Second, as noted above, in the debate over the expansion of UP that preceded the passage of the Family Support Act, claims were made about the effect of the UP program on the likelihood that a poor, two-parent family would separate and receive the AFDC benefits available to single parents.

However, changes in the number of families receiving benefits under Basic occur not only as a consequence of changes in rates of marital dissolution but also in response to fluctuations in the economy, the number of unwed mothers, and the rates of family formation. In addition, the total caseload includes some long-term recipients whom we would not expect to be affected by the suspension or implementation of the UP program. The number of new Basic cases opened or approved should be more sensitive to changes in the UP program because it does not include such long-term Basic families. Unfortunately, only 2 of the 8 states provided this kind of information: Oregon reported the number of Basic cases opened each month, and South Carolina reported the total number of Basic cases approved each month and the numbers approved for specific reasons, such as approvals for loss of support or for a father's absence. The 6 other states either did not report these figures or did not maintain them in a way that permitted us to separate new UP openings from new Basic openings.

Methodology

Modeling Procedures

For the states listed in table 1.1, we modeled monthly Basic caseloads with generalized least squares regression procedures and tested the effects on the Basic caseloads of the implementations or suspensions of the UP program. Implementations and suspensions of the UP program may themselves reflect political or economic changes in a state that could also affect the Basic program, but the procedures we used can adjust for the importance of other factors, such as the unemployment rate or new policies. The 8 states included in the study were selected because they either began or stopped their UP programs after 1975. We excluded years before 1975 because we could not adjust for the presence of AFDC-Foster Care cases in the total case counts.[2] In addition, we excluded states that changed their UP policy for less than 2 years in order to provide time for any effects to develop.

Table 1.1: Participation in AFDC-UP by Eight States, Fiscal Years 1974-89[a]

State	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Colo.	x	x	x	x	x	x	x	x	x	x	x	x				
Maine												x	x	x	x	x
Mo.		x	x	x	x	x	x	x			x	x	x	x	x	x
Mont.	x		x	x	x	x	x	x	x				x	x	x	x
Oreg.	x	x	x	x	x	x								x	x	x
S.C.													x	x	x	x
Utah	x	x	x	x	x	x	x	x								
Wash.	x	x	x	x	x	x	x	x			x	x	x	x	x	x

[a] "X" indicates the state had a UP program in at least the first month of the federal fiscal year, which begins in October.

Source: Data and reports from HHS and reported by the Congressional Research Service in 87-969 EPW, "State Use of the Aid to Families with Dependent Children—Unemployed Parent (AFDC-UP) Program: An Overview."

In each state, we modeled a period for which we had data on the Basic caseload and the economic, policy, and demographic variables we used in the analysis. Once we had developed a satisfactory model from a selection of economic, policy, and demographic variables, we included program variables to assess the effect of the UP intervention (either suspension or implementation). (See appendix I for a description of the model development process.) These variables included a dummy variable for the UP intervention (1 during the program and 0 when the program was not in place) and a variable that was the product of the UP dummy and a trend variable. The interaction between the UP dummy and the trend variable measured the effect of the UP intervention on the rate of change in the

[2] Prior to 1975, monthly information on the number of Foster Care cases was not consistently available. In 1980, federal legislation mandated that states provide for foster care and adoption assistance under title IV-E of the Social Security Act. This mandate was effective October 1, 1982, although states were allowed to initiate such programs earlier. In October 1981, HHS stopped counting AFDC-Foster Care in its total AFDC caseload figures.

Basic caseload. In the discussion that follows, models developed with this strategy are referred to as dummy variable models.

Where possible, we also used a second strategy of developing models of the period preceding the UP intervention (either suspension or implementation) and predicting into the postintervention period. Using more than one strategy allowed us to examine the consistency of findings across different methods of analysis. We do not report the findings of predictive models for all states because we could not always develop technically acceptable models with the available data. (See appendix I for our model evaluation criteria.)

To the extent that the availability of UP benefits encourages stability among poor families, we would expect the presence of the UP program to be associated with a decreased rate of growth in Basic. Alternatively, if UP decreased stability among poor two-parent families, we would predict its presence to be associated with an increased rate of growth in Basic. In either case, we anticipated that any association between UP and the number of cases receiving benefits under Basic would be small because the UP program generally serves less than 10 percent of the number of families served by Basic.

Strengths and Limitations

Our methodology shares some of the limitations of previous research. For example, in their analysis of 1980 data, Schram and Wiseman (1988) found that children in states that provided UP were 2 percent more likely to be receiving Basic benefits than children in states that did not provide UP. However, as they note, this difference could be attributed to other influences, such as region and urbanization. (Most of the states that did not provide UP benefits in 1980 were in the southern and mountain regions.) Just as the results found by Schram and Wiseman (1988) could be attributed to factors other than the UP program, the associations between the UP program and the Basic caseload found in our analyses could be caused by other events occurring at the same time as the introduction or elimination of UP. In order to address this possibility, we sought information from state officials about such events and selected a modeling technique that allowed us to control for a variety of variables that could affect the Basic caseload. In addition, the 8 states vary in the timing and type of UP policy change, making consistent results across states less easily attributed to other events.

Also in common with previous research, our results are based on information that dates from before the Family Support Act. The effect of the availability of income assistance for two-parent families may be quite different to the extent that the Family Support Act has enhanced child support enforcement and employment and training services for AFDC families. However, the states that we included represent varied approaches to welfare and some have had elements of the Family Support Act in place for several years; thus, the current and historical environments are not entirely dissimilar. While our results may be applicable to the environment after the enactment of the Family Support Act, they cannot be generalized to states that were not included in our analyses.

Unlike previous research on the UP program, our review combined longitudinal data with regression techniques to test the association between the UP program and the rate at which the Basic caseload grows. Because the UP caseload is small in comparison to the Basic caseload, our analyses needed to have sufficient statistical power to identify any effect of UP's termination or adoption. We were able to identify models of the Basic caseload that met our criteria for each of the 8 states and, for the analyses of Oregon and South Carolina, we also analyzed potentially more sensitive data than the number of Basic cases. Nonetheless, it is possible that small, undetected effects existed in the states where we found no evidence that UP was associated with the Basic caseload.

We conducted our review in accordance with generally accepted government auditing standards between April and August 1991. The advisory panel of experts listed in appendix III reviewed the study design, including our model selection criteria. In addition, technical consultants reviewed both the study design and our procedures for building and evaluating the caseload models. The Administration for Children and Families at HHS reviewed and commented on a draft of this report.

Does AFDC-UP Influence the Size or Growth Rate of AFDC-Basic Caseload?

We found no evidence that the AFDC-UP program destabilizes two-parent families since the AFDC-Basic caseloads in the 8 states did not grow at a faster rate while UP was in place. Instead, the Basic caseloads in 4 of the 8 states we examined grew at a slower rate while the UP program was in place, thus providing some evidence that UP encourages family stability.

We found that the presence of a UP program was associated with lower rates of growth in the Basic caseload in 4 states. This is summarized in table 2.1. We observed no association or no consistent association between UP and the Basic caseload in the 4 other states. In all but 1 of the states, the interpretation of the results is complicated by the occurrence of other policy changes at or near the same time as the UP intervention. Notably, in no state was there consistent evidence that changes in the UP program were associated with higher Basic caseloads or higher rates of growth.

Table 2.1: Classification of States by Results of Analyses and Identification of Confounding Factors

Potentially confounding factors identified	UP consistently associated with lower Basic caseloads or lower rates of caseload growth[a]: Yes	No
Yes	Colorado	Missouri
	Maine	South Carolina
	Montana	Utah
		Washington
No	Oregon	

[a]In no state was AFDC-UP consistently associated with higher AFDC-Basic caseloads or a faster rate of caseload growth.

Table 2.1 also classifies the 8 states by whether we identified potentially confounding factors during our analyses. The major policy changes that complicate the interpretation of our findings are the Omnibus Budget Reconciliation Act of 1981 (OBRA) and the Deficit Reduction Act of 1984 (DEFRA). Among other changes, OBRA restricted eligibility to the AFDC program by limiting the combined gross income of all members of the assistance unit to 150 percent of the state's standard of need and including a portion of stepparents' earned income in determining eligibility. Policies incorporated in OBRA were implemented at the state level in late 1981 and caused a large drop in caseload in many states. If the UP program was suspended near the time when OBRA was implemented—as it was in Missouri, Montana, Utah, and Washington—any effects of the suspension of the UP program might be masked by the initial post-OBRA drop in caseload or overstated by the subsequent rapid increase in Basic caseloads found in some states. In contrast, DEFRA generally loosened eligibility standards by raising the total amount of income that an AFDC family could have to 185 percent of the state's need standard. DEFRA also included some policy changes that could have lowered caseloads, so the effect of DEFRA on caseloads is not as clear as that of OBRA tends to be. However, most models developed by other researchers suggest that DEFRA has been associated with modestly higher AFDC caseloads when other factors are held constant (see Angel, 1989; O'Neill, 1990; and Plotnick and Lidman, 1987).

The potentially confounding factors identified in 7 of the 8 states are not all equally plausible explanations for the association or lack of association we found between the UP program and rate of growth in the number of families receiving Basic benefits. For example, in Montana, we were unable to separate the probable effect of OBRA from the possible effect of UP because OBRA was implemented immediately before the UP program was suspended. In contrast, DEFRA is not a strong alternative to the UP program as an explanation for the changes in caseload growth rate that were detected in Colorado and Maine, although it occurred at or near the same time as the UP changes.

While our findings support the idea that UP either depresses or has no effect on the rate of growth of Basic, the relationship between UP and family stability is less clear. Increased family stability (or decreased incentive to separate) is just one of the possible explanations for the relationship we found between UP and Basic caseloads. As noted above, other policy changes occurring around the time of the UP change may account for some of the effect we found. In addition, the number of Basic cases may grow at a slower rate when UP benefits are available if eligibility workers use UP instead of the Basic category for two-parent families in which one of the parents is incapacitated.

We discuss our results in two groups: (1) states in which UP was consistently associated with lower rates of growth in the Basic caseload and (2) states in which UP was not associated with changes in the Basic caseload.

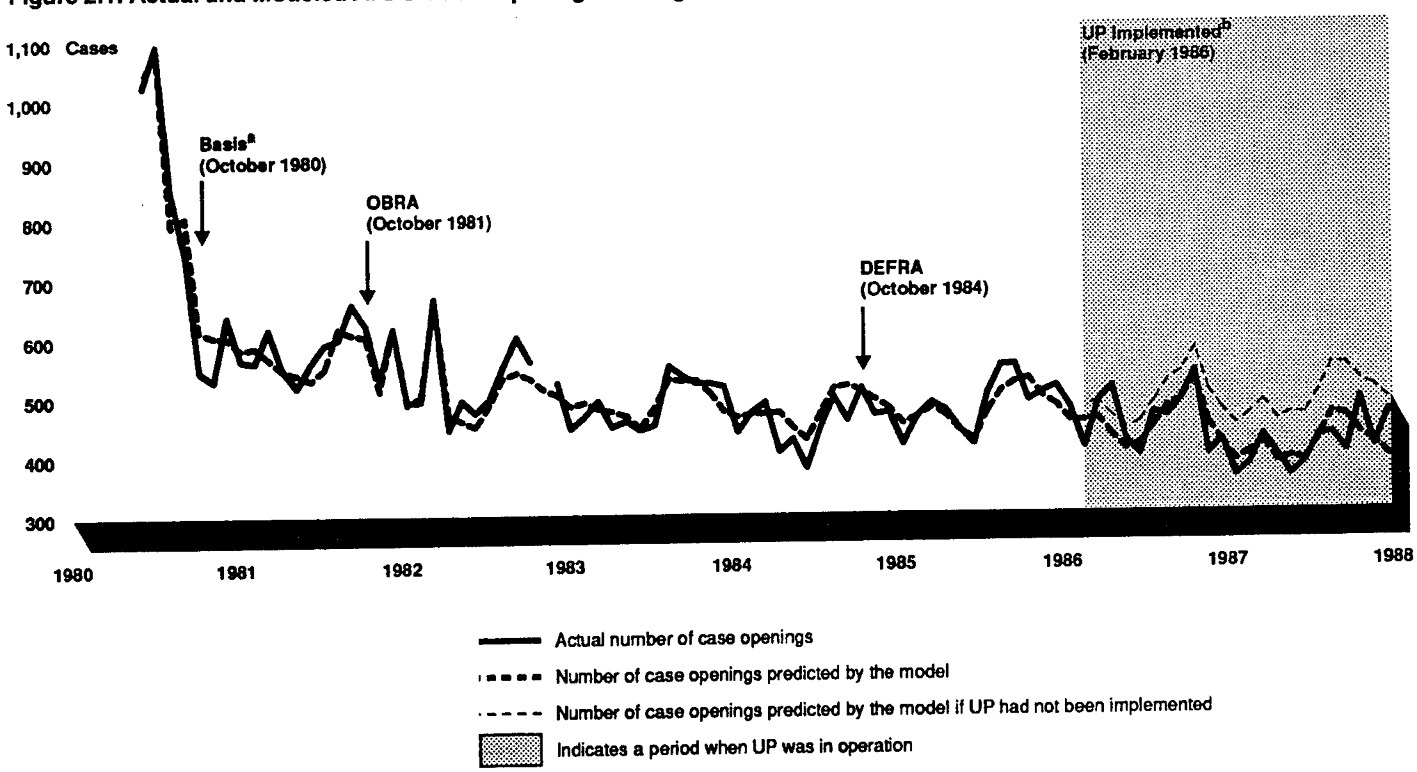
States in Which AFDC-UP Was Consistently Associated With Slower Growth In AFDC-Basic Caseload

Oregon

Our strongest evidence that the presence of the UP program reduces the rate of growth in the Basic caseload comes from Oregon, which reinstated UP in 1986 after it had been suspended for several years. Models of both new openings (figure 2.1) and Basic caseload (figure 2.2) yielded evidence that the reinstatement of the UP program was associated with decreased growth in new openings and in caseload. This evidence is strong because Oregon is the only state we examined in which the UP intervention is not confounded by other major policy changes. In addition, the evidence was consistent across dummy variable and predictive models. Figure 2.1 compares the actual number of openings to the number of openings predicted by our model if the UP program had not been reinstated in 1986.

Figure 2.2 shows the results from our analyses of the Basic caseload in Oregon. Consistent with the results of the analyses of the number of openings, these analyses indicate that UP had a dampening effect on the growth of the Basic caseload. Could the slower rate of growth in the Basic caseload be a result of improving economic conditions instead of the UP program? The unemployment rate was decreasing when the UP program was reinstated. However, the inclusion of economic control variables, such as retail employment, unemployment rate, and unemployment insurance claims, did not change the results of our Oregon analyses. Thus, changes in the economy are not a likely alternative explanation for the association between the UP program and the decreased rate of growth in the Basic caseload. In addition, an official from Oregon said that during the periods that Oregon suspended UP, two-parent families continued to receive medical assistance through a state program. Despite this source of support, she believed that when UP benefits were not available, needy two-parent families were added to Basic as two-parent families in which one parent was incapacitated and as single-parent families. Thus, our findings in Oregon are consistent with the idea that the availability of UP encourages family stability.

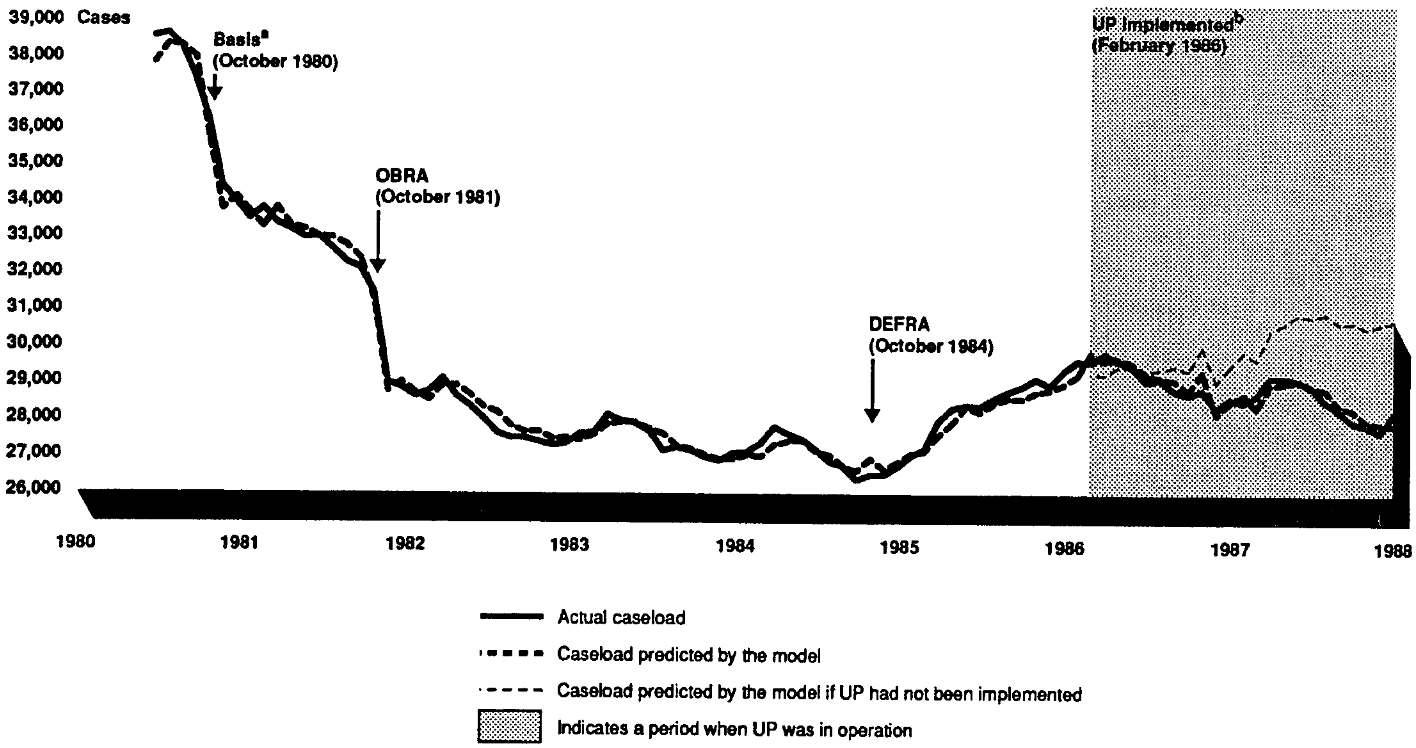
Figure 2.1: Actual and Modeled AFDC-Basic Openings in Oregon, June 1980 to December 1987



[a]"Basis" refers to a policy that changed the basis of the state's need standard to the minimum wage.

[b]Oregon also briefly suspended its UP program between August and October 1986, although October 1986 was the only month in which the UP caseload reported to HHS fell to zero. For visual simplicity, the "predicted if UP had not been implemented" line was drawn by treating this isolated month as if UP had been present. However, in the caseload models on which our conclusions are based, UP was considered absent during this suspension.

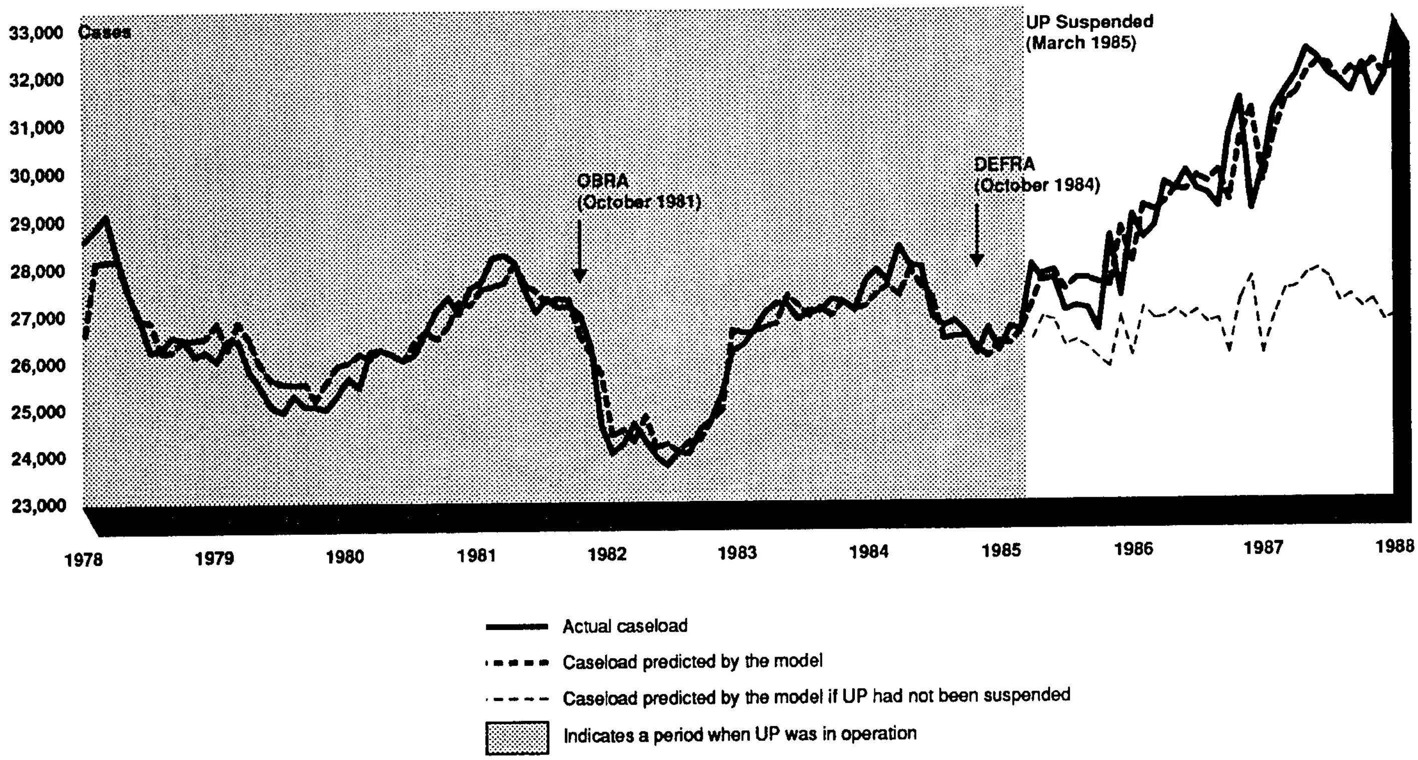
Figure 2.2: Actual and Modeled AFDC-Basic Caseload in Oregon, June 1980 to December 1987



[a]"Basis" refers to a policy that changed the basis of the state's need standard to the minimum wage.

[b]Oregon also briefly suspended its UP program between August and October 1986, although October 1986 was the only month in which the UP caseload reported to HHS fell to zero. For visual simplicity, the "predicted if UP had not been implemented" line was drawn by treating this isolated month as if UP had been present. However, in the caseload models on which our conclusions are based, UP was considered absent during this suspension.

Figure 2.3: Actual and Modeled AFDC-Basic Caseload in Colorado, January 1978 to December 1987



Colorado

Colorado's Basic caseload increased at a faster rate after UP was suspended, but this may be partly attributable to the implementation of DEFRA 5 months before the UP suspension. (See figure 2.3.) Each of these policies—DEFRA, which eased eligibility requirements for AFDC, and the suspension of the UP program—could result in increased Basic caseloads. However, some evidence exists that the increased rate of growth in Basic stems in greater part from the suspension of UP. First, the predictive model applied to the postsuspension period indicated that the actual Basic caseload is higher than would have been expected if the UP program had continued. This model accurately predicted the caseload after the implementation of DEFRA and before the suspension of UP. Furthermore, as a Colorado official indicated, the increased gross income limit mandated by DEFRA would have added only a small number of cases because Colorado's payment standard remained low.

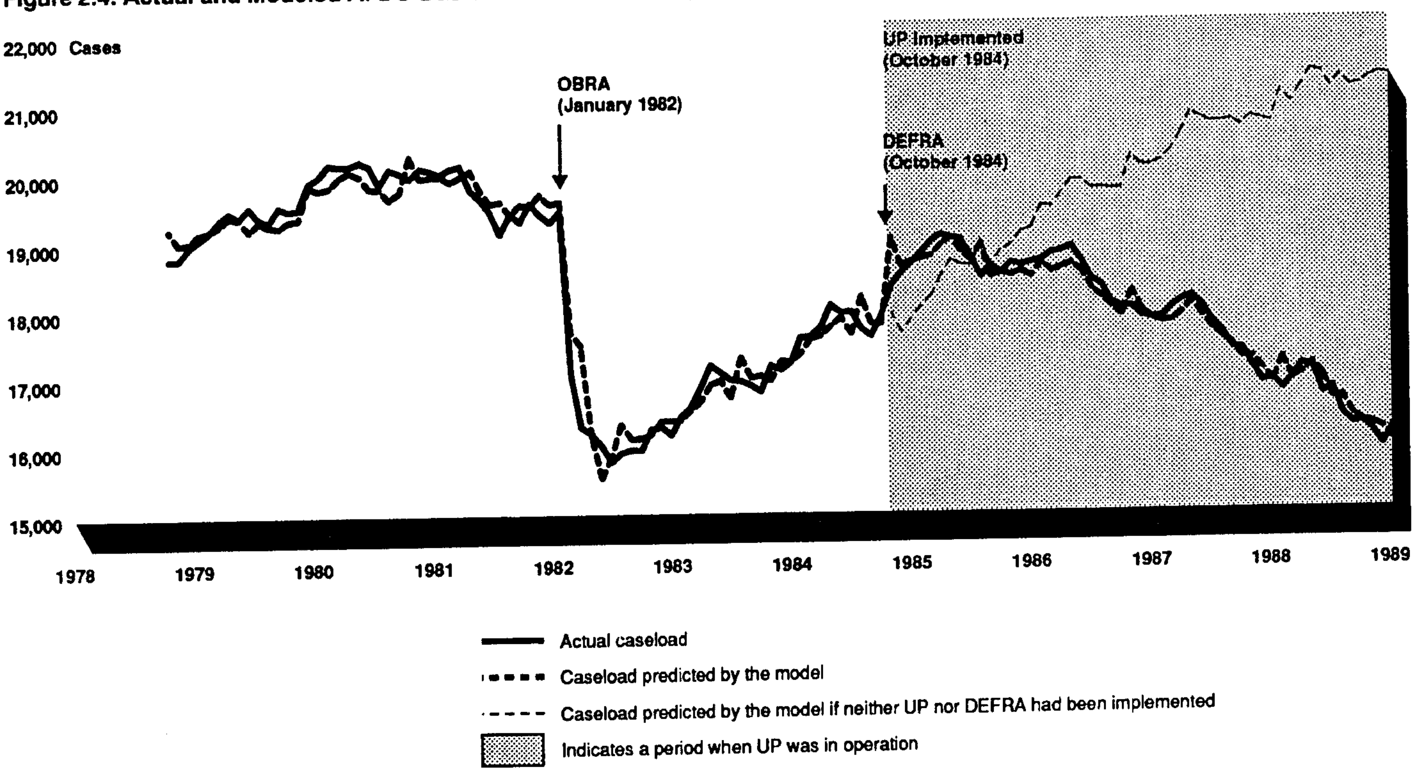
It has also been suggested that the incapacity segment of Basic may have been used with greater frequency after the UP suspension, causing increased growth in Basic that would not imply a decrease in family stability. However, there is evidence that this did not occur statewide. According to a state official, one county in Colorado responded to the suspension of UP by reclassifying UP cases as Basic cases in which one parent is incapacitated. If this type of reclassification accounted for most of the change in Basic, then the increase in the Basic caseload would not reflect a change in the stability of two-parent families. However, HHS statistics on the proportion of children eligible for AFDC in Colorado as a result of parental incapacity suggest that it did not vary greatly between periods before and after UP suspension and may even have declined slightly. This reduces the likelihood that the growth in Basic cases associated with the UP suspension is largely attributable to more frequent use of the incapacity category.

To summarize, our analyses for Colorado associate the period of UP suspension with evidence of reduced family stability in the form of increased rates of growth in Basic. Some of this increase in growth may be attributable to the effects of DEFRA or to increased use of the incapacity category within Basic. However, neither of these other factors provides a full or convincing alternative explanation for our finding.

Maine

In the years following Maine's adoption of both UP and DEFRA in October 1984, the growth rate for the state's Basic caseload declined sharply, observable as a decline in caseload level. (See figure 2.4.) Although DEFRA was implemented at the same time as UP, the change in the growth rate of the Basic caseload is more easily attributed to UP. DEFRA and UP cannot be separated statistically, so the models included a dummy variable measuring the combined effect of both changes. A model including both the dummy variable and a term for its interaction with caseload growth indicated that the rate of caseload growth decreased after October 1984, which is inconsistent with the expected effect of DEFRA's major provisions. Nonetheless, we cannot completely rule out DEFRA because it did include provisions regarding the counting of income from immediate family members living in the same household that could have exerted some downward pressure on the Basic caseload.

Figure 2.4: Actual and Modeled AFDC-Basic Caseload in Maine, October 1978 to December 1988



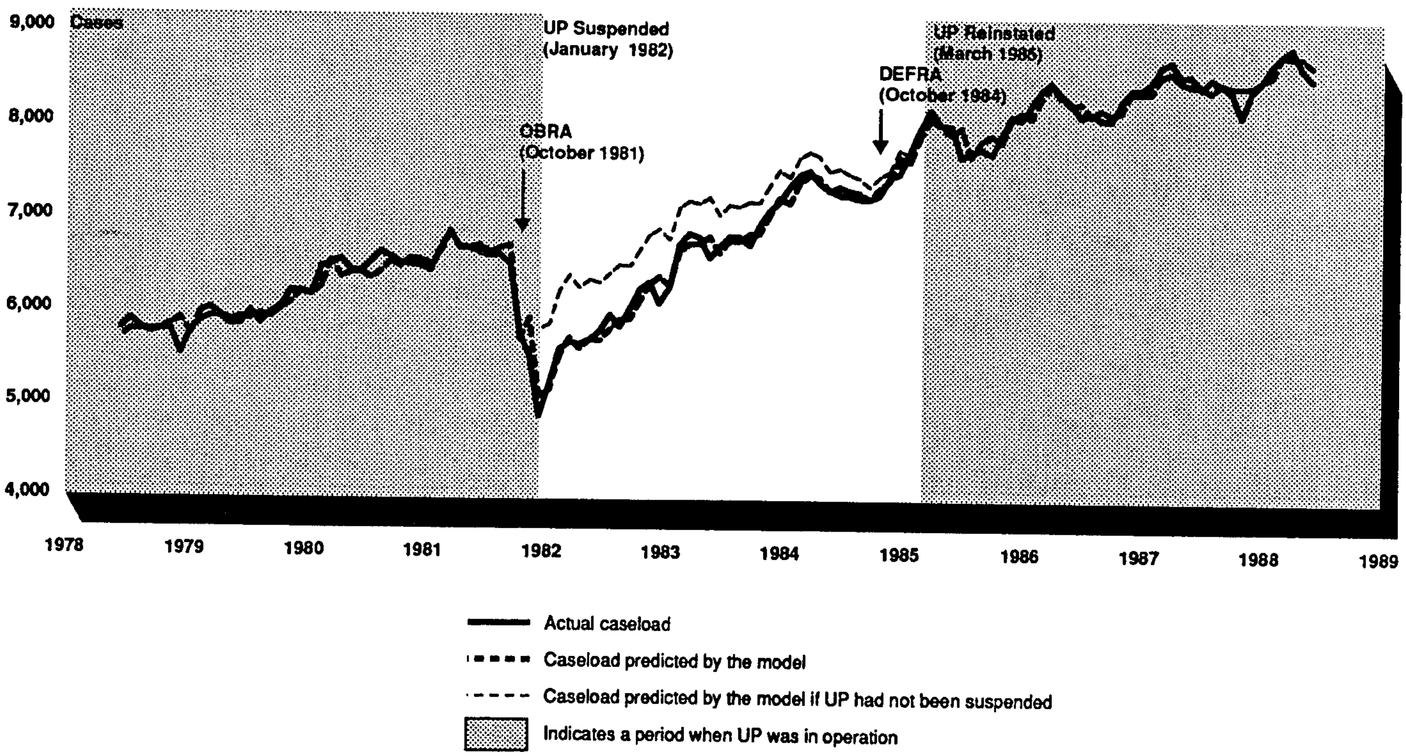
While the state unemployment rate indicates that Maine's economy improved in the period around the time of the UP implementation, our analyses did not support the change in the economy as a plausible explanation for the change in the AFDC-Basic caseload. Models of Maine's caseload that included the unemployment rate did not remove the association between the implementation of the UP program and the rate of Basic caseload growth.

Montana

Montana's Basic caseload increased at a faster rate during a 3-year UP suspension beginning in January 1982, but this may stem in whole or in part from the requalification of families whose benefits had been terminated by OBRA during the previous 4 months. (See figure 2.5.) A rapid resurgence in the caseload following the post-OBRA low was also observed in some other states. Montana fully suspended its UP program in March 1982. However, since the number of UP cases dropped to fewer than 50 in

January 1982 (from 442 cases in December 1981), our analyses considered the suspension period to have begun in January 1982, just as the Basic caseload reached its post-OBRA low point. Consequently, we cannot rule out a post-OBRA rebound as an alternative explanation for the increased rate of Basic caseload growth during the period of UP suspension.

Figure 2.5: Actual and Modeled AFDC-Basic Caseload in Montana, July 1978 to June 1988[a]



[a]Although figure 2.5 implies that the Basic caseload would have been larger if the UP program were not terminated, our analyses indicated that the rate of growth was slower during the period when the UP program was in place than while it was suspended.

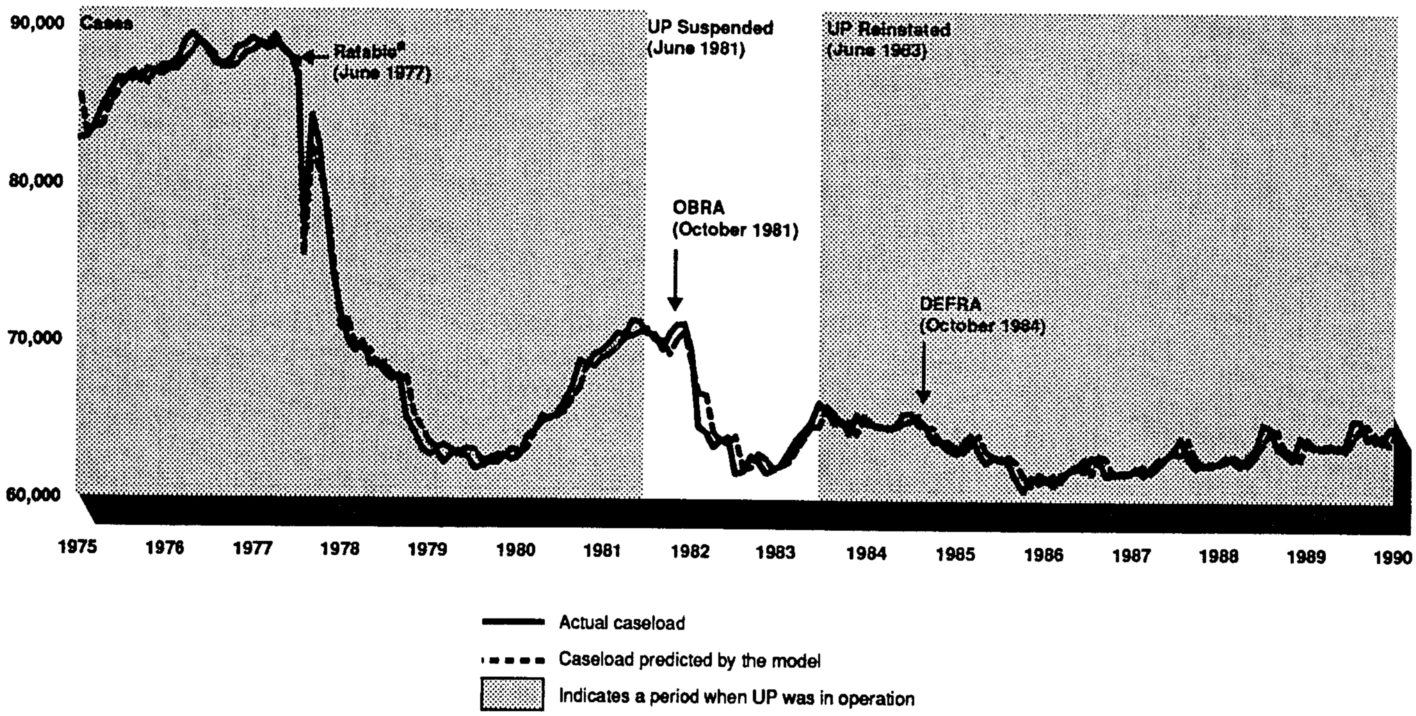
States in Which There Was No Consistent Evidence of an Association Between AFDC-UP And AFDC-Basic Caseload or Its Rate Of Growth

In Missouri, South Carolina, Utah, and Washington, the results of our analyses either showed no evidence that the UP program affected the Basic caseload or changed depending on which variables were included in the models. In all 4 of these states, the change in UP policy occurred at or near the same time as another major policy change. In Missouri, Utah, and Washington, the suspension of UP occurred less than a year before the implementation of OBRA. In South Carolina, the implementation of UP began in the same month that the state increased its need standard for the first time in 7 years. The figures accompanying the discussion of these analyses show only the actual caseload and our best model of the caseload. They do not compare the actual Basic caseload to what the model predicts the caseload would have been without the UP intervention, because in these cases we do not have reliable evidence of an association between UP and the number of Basic cases or the caseload's rate of growth.

Missouri

There was no evidence that the suspension of UP in Missouri changed the Basic caseload or its rate of growth, but the circumstances would have made such a change difficult to observe. As shown in figure 2.6, Missouri's suspension of its UP program lasted only 2 years—from July 1981 through June 1983. Both the brevity and the timing of the suspension, which occurred just a few months before OBRA, made it difficult to observe any effect of the presence or absence of the UP program. Furthermore, any incentive for poor two-parent families to try to become eligible for Basic benefits may have been diminished by the continuing eligibility of children in UP for Medicaid during the suspension. The predictive model applied to the postsuspension period accurately forecast the caseload in all the months before OBRA, indicating that the suspension did not have an immediate effect on the caseload. Dummy variable models that controlled for the effect of OBRA also found no evidence that the suspension of UP had an effect.

Figure 2.6: Actual and Modeled AFDC-Basic Caseload in Missouri, January 1975 to September 1989



[a]"Ratable" refers to the state's implementation of a different method for calculating benefit amounts.

South Carolina

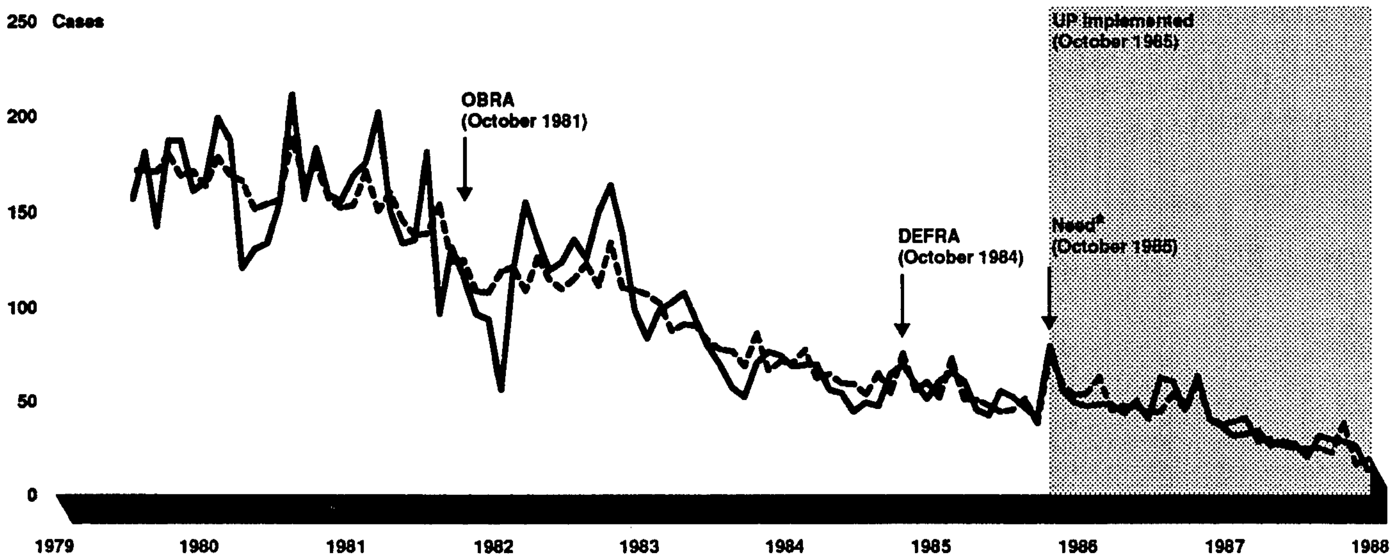
The results from South Carolina are mixed. Although analyses of the Basic caseload found that its rate of growth decreased after UP was implemented, analyses of a potentially more sensitive measure—new cases approved for loss of support—did not find any evidence of an effect. Two factors made it unlikely that we would be able to identify any effect of UP in South Carolina, which implemented the program for the first time in October 1985. First, South Carolina's UP caseload was quite small during the period we examined, averaging around 370 cases per month, or approximately 1 percent of the Basic caseload. Second, South Carolina raised the need standard for a three-person family from $187 to $369 (the first increase in 7 years) in the same month that it implemented UP. In theory, the changes in the need standard and the UP program might have opposite effects, with the start-up of the UP program tending to decrease the Basic caseload (or its rate of growth) and the more generous need standard tending to increase it (or its rate of growth). Statistically, the correlation between the need standard and the dummy variable for the implementation of UP made it difficult to determine what effect, if any, UP had on the Basic caseload.

We attempted to develop models of four series of data on South Carolina's Basic program: the total Basic caseload, the total number of cases approved, the number of cases approved because the family had lost support from a caretaker who had left the home, and the number of cases approved because the father was absent. We were unable to develop models of total approvals or approvals attributed to an absent father that met our statistical standards.

However, we did develop acceptable models for both approvals attributed to loss of support from a caretaker and Basic caseloads. (See figures 2.7 and 2.8.) Models of the former found no evidence that the implementation of UP had an effect on the number or rate of change in the number of approvals for loss of support. In contrast, the best model of the Basic caseload in South Carolina showed an association between UP and the growth rate of the Basic caseload, indicating that the caseload—already decreasing—decreased faster after UP was implemented. Caution should be used in interpreting this evidence of an effect for two reasons: (1) the analyses of the more sensitive measure—approvals for loss of support—did not find any association with the UP variables and (2) it was difficult to statistically separate the influence of need standard from the influence of UP.[1]

[1]The need standard variable behaves erratically in these models, reflecting the high correlation between it and the UP variables.

Figure 2.7: Actual and Modeled Approvals for Loss of Support in South Carolina, July 1979 to December 1987



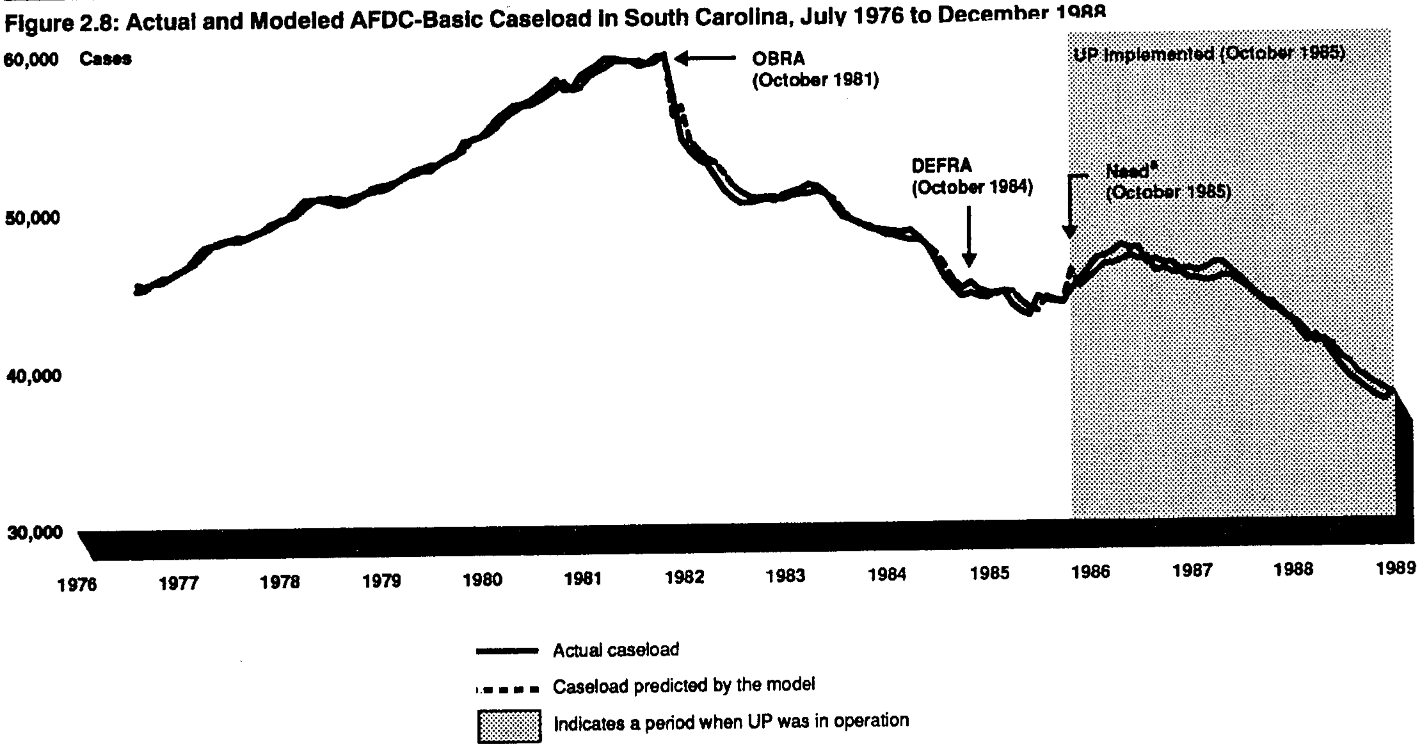
Actual number of cases approved because of "loss or reduction in support from caretaker as a result of leaving home and stopping or reducing support"

Model's prediction for the number of cases approved because of "loss or reduction in support from caretaker as a result of leaving home and stopping or reducing support"

Indicates a period when UP was in operation

[a]"Need" refers to a large increase in the state need standard used to determine AFDC eligibility.

Figure 2.8: Actual and Modeled AFDC-Basic Caseload in South Carolina, July 1976 to December 1988

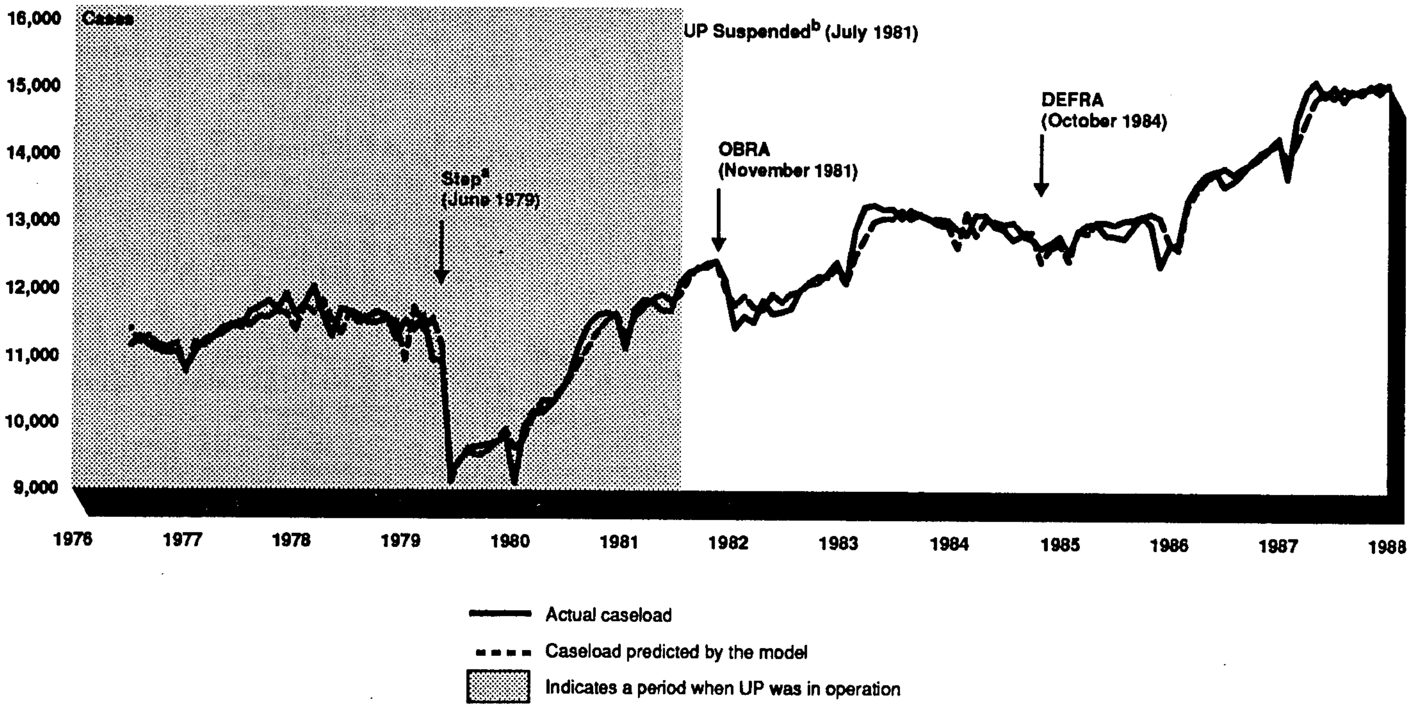


[a]"Need" refers to a large increase in the state need standard used to determine AFDC eligibility.

Utah

There was no evidence that the suspension of the UP program in Utah changed the level or growth rate of the Basic caseload. If the July 1981 suspension did affect the caseload, that effect was probably masked by the implementation of OBRA 4 months later. (See figure 2.9.) The best dummy variable models found no association between the UP program and the size or rate of growth of the Basic caseload. In January 1983, Utah implemented the Emergency Work Program, which provided short-term cash benefits and an intensive combination of work, education, training, and job search assistance to recipients. Because two-parent families were eligible for the Emergency Work Program, we also tested for effects of its implementation on the Basic caseload but did not find any.

Figure 2.9: Actual and Modeled AFDC-Basic Caseload in Utah, July 1976 to December 1987



[a]"Step" refers to the imposition of a policy that required consideration of stepparents' income in determining AFDC eligibility.

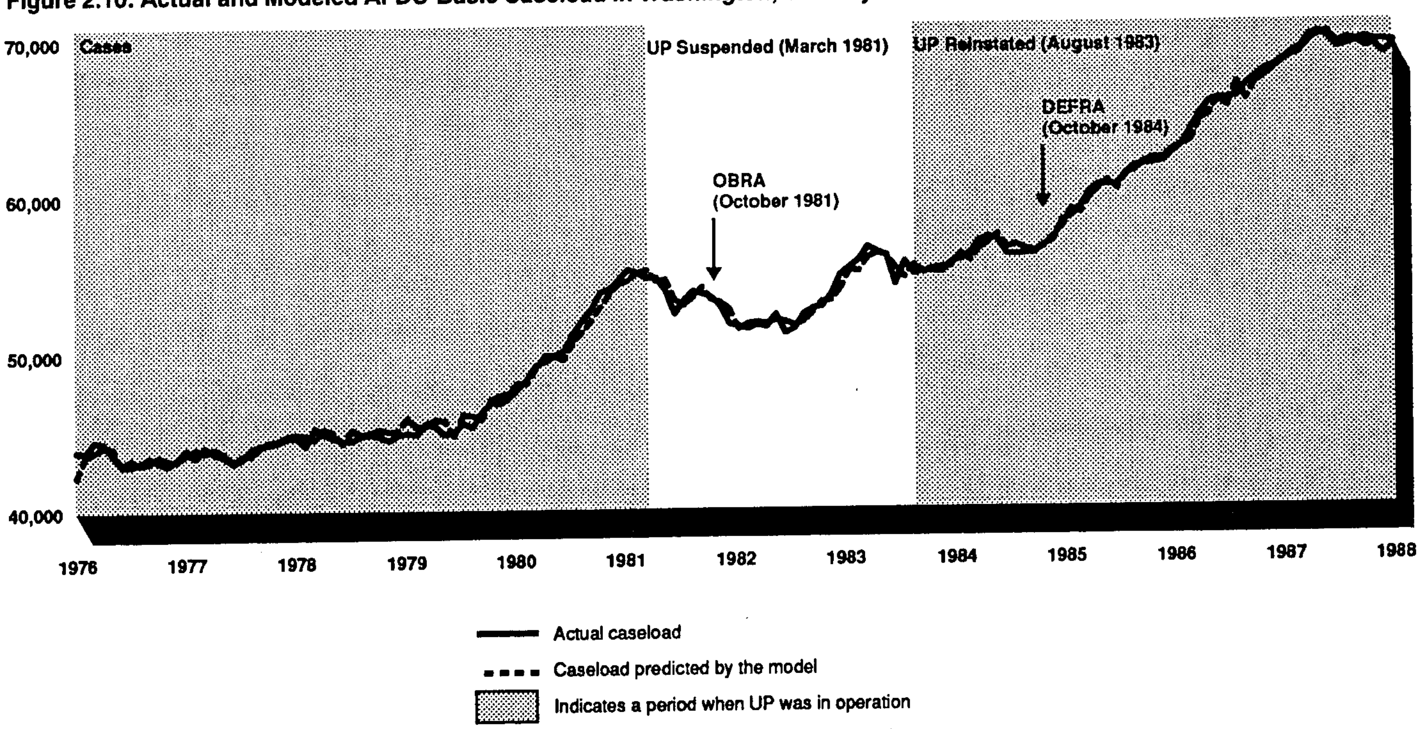
[b]In January 1983, 18 months after UP was suspended, Utah implemented the state-funded Emergency Work Program, which provides time-limited cash benefits and other services to two-parent families.

Although we did not find any association between the suspension of the UP program and changes in the Basic caseload, Utah followed 1,434 UP recipients who were terminated when UP was discontinued and found that 13.6 percent were receiving regular AFDC assistance as separated or divorced households 6 months after program termination (Janzen, Bartlome, and Cunningham, 1987). This is nearly double the 7.4 percent of the 1980-81 UP caseload who separated and received Basic benefits by the end of a similar 6-month period.

Washington

The analysis of Washington's Basic caseload showed no evidence that a 30-month suspension of UP was associated with changes in the Basic caseload. However, the implementation of OBRA occurred during the UP suspension and may have masked any change. (See figure 2.10.) Although the dummy variable for UP in Washington was associated with an increase in the caseload in one model, alternative models that were stronger in terms of the variability explained showed no evidence of an association between UP and the Basic caseload. This is consistent with the research of Plotnick and Lidman (1987) who found that a dummy variable for UP did not contribute to the predictive ability of their model of Washington's Basic caseload.

Figure 2.10: Actual and Modeled AFDC-Basic Caseload in Washington, January 1976 to December 1987



Like Utah, Washington followed UP families who lost their benefits when UP was terminated (see Nelson and Fiedler, 1984). While a number of the former UP cases received Basic benefits within a 17-month period, no data were available on the separation rates of UP recipients while the program was operating (regardless of whether the separation resulted in receiving Basic) or on the proportion of UP families who moved to Basic specifically because of marital separation while the UP program was operating. As a result, it is not possible to determine whether the marital dissolution rate in the group of former UP families is atypical of the population eligible for UP in that state.

Statistical Analysis of the Effect of AFDC-UP Policy on AFDC-Basic Caseload

We used an interrupted time series design to evaluate changes in each state's Basic caseload associated with the implementation or withdrawal of the UP program. Ideally, the intervention studied in an interrupted time series analysis should be a discrete event that occurs at a well-defined point in time and that can be observed as an immediate change in the outcome measure. In regression terms, the intervention is usually specified as a dummy variable that changes from 0 to 1 when the event occurs. For example, in our analysis of UP's effect on the Basic caseload, we included a UP dummy variable that was coded 1 when UP was in place but coded 0 when UP benefits were unavailable. Because the effect of UP policy on the Basic caseload was expected to be gradual, our analyses incorporated an additional variable to index the effect of UP policy on the rate of growth in the Basic caseload.[1]

Statistical Analysis

The statistical analysis of an interrupted time series is iterative: alternative models are identified and tested until one is found that is both statistically adequate and parsimonious. The details of model estimation are covered in most regression textbooks that address time series. (For further background on the regression techniques used in this report, see Jaccard, Turrisi, and Wan, 1990; Lewis-Beck, 1986; Makridakis, Wheelwright, and McGee, 1983; Ostrom, 1990; SAS Institute, 1988; Sincich, 1989.)

Regression Methods

We used the AUTOREG procedure in the SAS/ETS program library for data analysis based on the two-step Prais-Winsten estimator. We selected this generalized least squares (GLS) regression procedure over ordinary least squares (OLS) methods because the GLS procedure estimates models that incorporate a term to adjust for serially correlated prediction errors. Nonrandom, serially correlated prediction errors, a phenomenon common in regression analyses of time series, violate the assumptions of OLS

[1]The effect of UP on the rate of caseload growth was indicated by the coefficient for a variable coded as the product of the UP dummy variable and a time counter that was also incorporated in the model. When all other factors are held constant, the coefficient for the time counter can be interpreted as the monthly rate of increase in the caseload when UP is not present; the coefficient for the UP x time product variable is interpreted as the linear change in the rate of caseload growth associated with the presence of UP.

regression procedures for calculating standard errors and significance levels.[2] Thus, the GLS procedure is generally more appropriate for this type of data.

We chose GLS regression procedures over autoregressive integrated moving average (ARIMA) methods because we wanted to explore the importance of a variety of independent variables that would have necessitated quite complex ARIMA models. Mathematical forecasting methods like ARIMA generally have more difficulty predicting cyclical subpatterns and major turning points because they rely primarily on past observations of the dependent variable to inform model construction. In contrast, the GLS procedure permits the incorporation of various independent variables that help identify alternative explanations for changes in the caseload associated with UP.

Fit Statistics

For most models, we report the following statistics: OLS R-squared, Durbin-Watson d, GLS total R-squared, GLS regression R-squared, and root mean square error.

The OLS R-squared statistic measures the proportion of variance in the dependent variable (usually Basic caseload), which is accounted for by the variables in the ordinary least squares model. The value of R-squared can vary between 0 and 1. Generally, the higher its value, the more accurately the model estimates the data. However, R-squared may give a misleading impression of the accuracy of a model's predictions if the model's errors are serially correlated rather than random; the Durbin-Watson d statistic helps determine whether this is so.

The Durbin-Watson d statistic assesses whether the degree of first-order serial correlation among the residuals is high enough to seriously violate the assumptions of the ordinary least squares approach. The d statistic can vary between 0 and 4; the closer it comes to either extreme, the stronger the autocorrelation between residuals. In general, a d statistic close to 2 suggests that first-order serial correlation among residuals is negligible. In

[2]Although all the caseload models discussed in this report were estimated using GLS methods, the model of Oregon's case openings was estimated using OLS. Unlike the caseload, which generally depends heavily on its previous value, the number of new case openings in a particular month can be viewed as an independent observation. However, because serial correlation was found in our analyses of South Carolina's approvals for loss of support, we used GLS procedures to estimate those models.

the model of Oregon's openings, no GLS model is shown because the value for the Durbin-Watson d statistic did not indicate one was required.

The GLS model extends the OLS model by adding an "autoregressive" term to account for serial correlation among the OLS model's prediction errors. GLS total R-squared is a measure of how well the next value can be predicted using the structural part of the model and the past values of the residuals.

After adjusting for the autocorrelation, SAS/ETS generates a statistic showing how well the other variables in the model estimate the adjusted data. This statistic, which varies between 0 and 1, is referred to in this report as the GLS regression R-squared and by SAS/ETS as the "regression R-squared" or the "structural R-squared."

To supplement the R-squared statistics, we have reported root mean square error—the standard deviation of the residuals for the GLS regression model. In general, the more accurate the regression model, the smaller this value will be. However, it should be interpreted with reference to the average value of the dependent variable—a standard error of 400 is unacceptably large when estimating something that averages near 1,000, but it is quite small when estimating something that averages 60,000.

Model Selection

For each state, model development began with the collection of data on a similar set of economic, policy, and demographic variables. Model development and selection were governed by general rules and specific criteria. The first step was to plot all variables in each state's data set by date and then examine their patterns of intercorrelation. Using these data sets, we attempted to develop two types of models: "dummy variable" models and "predictive" models. Below, we describe the approach we took in each case and the criteria we applied in selecting the models reported in this appendix.

Dummy Variable Models

We developed dummy variable models using data encompassing periods before and after a UP policy change. In developing models using this approach, we generally adjusted first for OBRA and other obvious policy changes (for example, the stepparent policy in Utah) and then included such basic variables as population and unemployment rate or unemployment insurance claims. From this point, model development proceeded with the aid of plots of model residuals and the patterns of correlation between model residuals and other variables in the data set.

Before accepting a model of this type for reestimation with the UP variables, we required that it meet two criteria: (1) the full model (including the autoregressive parameter that accounts for serial correlation) had a squared multiple correlation coefficient of .90 or higher and (2) all regression coefficients were significant and had the expected signs. OLS models were used only when the Durbin-Watson statistic showed no statistically significant first-order serial correlation among residuals.

From among the models that met these basic criteria, we chose a single best model by considering trade-offs in (1) the interpretability of the coefficients, (2) the percentage of variance explained by the model after accounting for autocorrelation, and (3) the value of the total R-squared. In general, if more than one model had high values for regression R-squared and total R-squared, we selected the one that included the most theoretically sensible set of variables or that had more interpretable coefficients.

Differences in the use of particular variables from state to state are attributable sometimes to variations in available data, sometimes to differences in the period of analysis and the policy changes it encompassed, but most often to interstate differences in the factors that showed sensible empirical relationships to the Basic caseload. For example, including the unemployment rate in our model of Oregon's Basic caseload resulted in an unemployment coefficient that was positive but not significantly different from zero. Similarly, models using various lags of Oregon's unemployment rate were rejected because these variables did not achieve significant coefficients or the net effect of their coefficients was negative. Consequently, our model selection criteria required that we exclude the unemployment rate from our model for Oregon. Similarly, although we used employment in retail trade in our model of Oregon's Basic openings, these data were not used in Maine, where retail trade and many other employment series showed either no relationship to the caseload or a relationship in an unexpected direction.

Predictive Models

To augment our basic approach, in a few states we also developed a second kind of model using data prior to the UP intervention. We validated such models by predicting the 12 months of data before the UP policy change. If a model issued predictions for this 12-month test period that were not significantly different from the actual values, we reestimated the model incorporating the test data and determined whether the model significantly over- or underpredicted the actual caseload after the UP policy change. Models of this nature were developed for the Basic caseload in Colorado, Missouri, and Oregon and for Basic openings in Oregon. In Maine, Montana, South Carolina, Utah, and Washington, predictive models are not reported because of either the timing of other policy changes or the failure to identify a model that met our criteria. In the four cases where technically acceptable predictive models were developed, their findings were consistent with those of the dummy variable models shown in tables I.1 through I.10. In the paragraphs below, we detail the development of a dummy variable model for the Basic caseload in Maine.

Example: Maine

In Maine and all other states, the monthly level of the Basic caseload was derived by using three series: the total AFDC caseload, the AFDC-UP caseload for the corresponding month, and the AFDC-Foster Care caseload. The Basic caseload was found by subtracting UP cases and Foster Care cases from the total caseload reported for the particular month. Foster Care cases were subtracted only prior to October 1981, since HHS did not include these cases in AFDC caseload totals after that date. Our calculations were based on data available in the Social Security Bulletin and data provided on microfilm by the Administration for Children and Families.

Before analysis, historical data were collected on a range of economic, policy, and demographic variables that might bear a meaningful relationship to welfare caseloads. Because there is no strong consensus on the set of factors that drive Basic caseloads, we considered a range of variables that had been used in one or more existing models of AFDC caseload.[3] Economic data for each state were obtained primarily from LABSTAT, an electronic data base maintained by the Bureau of Labor Statistics. Series available from LABSTAT included general unemployment as well as employment and wages in specific trades or industries. These data were supplemented by historical series on initial unemployment claims and unemployment insurance exhaustions, which were taken from the Social Security Bulletin.

[3]Existing models of state AFDC caseloads show striking differences in variable selection. For example, although Garasky (1990) incorporated the state need standard in a model of Massachusetts' Basic caseload, Barnow (1988) did not find that the state's maximum AFDC payment achieved a significant coefficient in his models of the New Jersey caseload. Other authors have used maximum benefit level or payment standard rather than state need standard (for example, O'Neill, 1990). Similarly, O'Neill (1990) uses no variable to index births to unwed mothers in her model of Massachusetts' caseload while other researchers have incorporated data on all births to teenagers (Garasky, 1990) or out-of-wedlock births summed over a varying period of years (Barnow, 1988; Plotnick and Lidman, 1987). Finally, although most models include some sort of employment indicator, researchers vary widely in its selection; the number of nonagricultural jobs (Plotnick and Lidman, 1987), unemployment insurance compensation claims (Garasky, 1990), rural manufacturing employment (Angel, 1989), and the state unemployment rate (Grossman, 1985; O'Neill, 1990) have been used by different analysts.

Next, we collected data on demographic caseload predictors, including state population, births, divorces, and births to unwed mothers. In most states, these data were not available on a monthly basis and the monthly values had to be estimated from annual totals. Finally, we obtained information on policy variables by interviewing state staff. Based on states' input, dummy variables were constructed to represent the implementation of OBRA, DEFRA, and state-initiated policy changes.[4] We also used these interviews with state officials to verify historical data on AFDC need standards and payment levels that were provided by the Congressional Research Service and the Administration for Children and Families.

Maine did not have a UP program until October 1984, when it adopted the program and operated it continuously through the end of our data collection period. DEFRA was implemented in the same month, so its effects on the caseload could not be statistically separated from the effects of UP. One policy variable was used to represent the combined effects of UP and DEFRA. However, since DEFRA was expected to liberalize access to Basic, an increase in the caseload or its rate of growth would be consistent with the anticipated effect of DEFRA, while a decrease in the caseload or its rate of growth would be more easily attributed to UP.

We used caseload data from October 1978 through December 1988 to develop a GLS regression model for Maine's Basic caseload. After several attempts, we arrived at a model that predicted Basic caseload based on OBRA, the real value of the payment standard 12 months ago, employment in Maine's lumber industry, the passage of time, and a proportion of the difference between the previous month's caseload and the model's prediction (the autoregressive term). Although we attempted to incorporate various lags of the state unemployment rate, this attempt neither yielded significant coefficients nor altered our ultimate results with regard to UP. The passage of time was incorporated in the model by a term to assess the caseload's general rate of monthly growth, which was coded 1 in the first month included in the analysis and increased by 1 for each succeeding month.

[4] OBRA was often implemented in stages or took effect gradually, so variants of the OBRA variable were used to account for this. For example, in Maine, OBRA was represented by two variables—a dummy variable coded 1 for all months following the implementation of OBRA in January 1982 and a dummy variable coded 1 for the first month of OBRA implementation in February 1982. The latter dummy variable helped correct a large misprediction of the February 1982 caseload that occurred when only the first OBRA variable was used. (The OBRA provisions were felt a few months later in Maine than in most other states, which implemented the changes in October 1981.)

To assess the effects of UP, we added two other terms to this list: (1) a dummy variable for the presence of the UP program (in Maine, this dummy also represented the implementation of DEFRA) and (2) a variable to assess the effect of UP and DEFRA on the caseload's growth rate, which was the product of the UP-DEFRA dummy and the variable representing the passage of time. When a policy is shown to affect the rate of caseload growth, its association with the level of the caseload is not very meaningful since the level of the caseload depends on the length of time the policy has been in place. Thus, we did not interpret the coefficient for the UP dummy variable when the variable indexing the effect on growth rate had a statistically significant coefficient.

The coefficient for the variable assessing the association between UP and DEFRA and the rate of Maine's caseload growth was statistically significant and negative, indicating that the rate of growth in the caseload dropped after the implementation of UP and DEFRA. As previously indicated, this result is more easily explained by the implementation of UP than by the implementation of DEFRA, which the director of Maine's AFDC program expected to have a small effect in the opposite direction.

Models of States' AFDC-Basic Caseloads

To assist readers in evaluating and building upon our findings, tables I.1 through I.10 present the regression models used to develop figures 2.1 through 2.10. In interpreting these models, the reader should be aware of the following caveats:

- The coefficients for particular variables could change substantially depending on the set of other variables included in the model, so the presence or absence of related variables should be carefully weighed before interpreting an individual coefficient.
- When a policy x time interaction term is included in a model, the coefficient of the corresponding policy dummy variable should not be interpreted in isolation.
- The models should not be used to forecast long-term effect. The effect of UP's presence probably levels off or decays over time so the coefficient for the UP x time variable may overstate the long-term effect of UP on the rate of growth in Basic.

Table I.1: Generalized Least Squares Model of Colorado's Monthly AFDC-Basic Caseload, January 1978 to December 1987[a]

Variable	Coefficient	Standard error
Constant	9,006.01**	2,939.5
Time (coded 1 in 1/78 and increasing by increments of 1 thereafter)	129.93**	21.8
OBRA (coded 0 through 10/81, 1 in 11/81, 2 in 12/81, 3 in 1/82, and 4 thereafter)	−715.19**	154.5
Need standard for a 3-person family, lagged 12 months	19.13**	5.6
Unemployment rate, lagged 12 months	269.14**	87.0
Initial claims for unemployment insurance (weekly average), lagged 4 months	.038*	.018
UP (coded 1 through 2/85 and 0 thereafter)	10,839.54**	2,558.8
UP x time	−133.38**	27.1
First-order autoregressive parameter	−.626**	.074

[a]N = 123.

Fit statistics:

OLS regression R-squared = .88
Durbin-Watson d for OLS regression = .66
GLS regression R-squared = .68
GLS total R-squared = .93
Root mean square error = 546.67

*.01 $< p \leq$ .05.
**$p \leq$ .01.

Table I.2: Generalized Least Squares Model of Maine's Monthly AFDC-Basic Caseload, October 1978 to December 1988[a]

Variable	Coefficient	Standard error
Constant	11,439.5**	1,203.0
Time (coded 1 in 10/78 and increasing by increments of 1 thereafter)	57.4**	6.3
OBRA 1 (coded 0 through 1/82, 1 in 2/82, 2 in 3/82, 3 in 4/82, and 4 thereafter)	-965.2**	57.1
OBRA 2 (coded 1 in 2/82 and 0 otherwise)	-915.1**	217.9
Real payment standard, lagged 12 months	23.5**	3.3
Employment in lumber industries (in thousands)	-95.0**	35.2
UP-DEFRA (coded 0 through 9/84 and 1 thereafter)	10,105.4**	644.0
UP-DEFRA x time	-123.9**	9.1
First-order autoregressive parameter	-.51**	.08

[a]N = 123.

Fit statistics:

OLS regression R-squared = .94
Durbin-Watson $\underline{d}$ for OLS regression = .95
GLS regression $\bar{R}$-squared = .87
GLS total R-squared = .97
Root mean square error = 242.8

*$.01 < p \leq .05$.
**$p \leq .01$.

Table I.3: Generalized Least Squares Model of Missouri's Monthly AFDC-Basic Caseload, January 1975 to September 1989[a]

Variable	Coefficient	Standard error
Constant	85,304.56**	1,144.2
Time (coded 1 in 1/75 and increasing by increments of 1 thereafter)	−19.12	13.9
Ratable reduction policy (coded 0 for all months through 6/77 and increasing to 6 by increments of 1 thereafter)	−2,795.90**	237.4
July 1977 (coded 1 in 7/77 and 0 otherwise to adjust for a large residual associated with the ratable reduction policy)	−7,799.89**	574.6
March (coded 1 in March and 0 otherwise)	307.17*	148.4
OBRA 1 (coded 0 through 10/81 and 1 thereafter)	−1,907.91*	768.9
OBRA 2 (coded 1 in 12/81 and 0 otherwise to adjust for a large residual in the period of OBRA implementation)	−2,157.97**	574.6
First-order autoregressive parameter	−.901**	.033

[a]N = 177.

Fit statistics:

OLS regression R-squared = .94
Durbin-Watson d for OLS regression = .17
GLS regression $\bar{R}$-squared = .74
GLS total R-squared = .99
Root mean square error = 773.39

*.01 < p ≤ .05.
**p ≤ .01.

Table I.4: Generalized Least Squares Model of Montana's AFDC-Basic Monthly Caseload, July 1978 to June 1988[a]

Variable	Coefficient	Standard error
Constant	−28,546.91**	4,964.6
Time (coded 1 in 7/78 and increasing by increments of 1 thereafter)	32.56**	5.6
OBRA (coded 0 before 12/81 and 1 in 12/81 and all following months)	−1,020.88**	112.8
Population (in thousands)	41.55**	6.5
Number of unemployment insurance claimants exhausting benefits	.27**	.04
DEFRA (coded 0 through 9/84 and 1 in 10/84 and all following months)	−2,792.56**	609.0
DEFRA x time	34.79**	7.6
UP (coded 0 from 1/82 through 3/85 and 1 otherwise)	1,475.40**	242.2
UP x time	−17.64**	3.6
First-order autoregressive parameter	−.47**	.08

[a]N = 120.

Fit statistics:

OLS regression R-squared = .98
Durbin-Watson d for OLS regression = 1.05
GLS regression R-squared = .96
GLS total R-squared = .99
Root mean square error = 117.96

*.01 < p ≤ .05.
**p ≤ .01.

Table I.5: Ordinary Least Squares Model of Oregon's Monthly Openings in AFDC-Basic, June 1980 to December 1987[a]

Variable	Coefficient	Standard error
Constant	1,039.72**	157.81
Time (coded 1 in 6/80 and increasing by increments of 1 thereafter)	-.11	.40
Births to unwed mothers, lagged 2 months	1.62**	.33
Employment in retail trade, lagged 6 months (in thousands)	-1.84*	.72
Summer (coded 1 during May, June, and July and 0 otherwise)	-53.36**	9.77
Policy linking need standard to minimum wage (coded 0 before 10/80 and 1 from 10/80 onward)	-505.43**	27.40
OBRA (coded 0 through 10/81 and 1 thereafter)	-76.70**	19.98
August and September 1980 (coded 1 in 8/80 and 9/80 and 0 otherwise)	-315.70**	35.47
December 1981 (coded 1 in 12/81 and 0 otherwise)	115.06**	37.54
March 1982 (coded 1 in 3/82 and 0 otherwise)	184.41**	36.66
UP (coded 0 from 6/80 through 1/86 and in 10/86 during a temporary suspension and 1 from 2/86 onward)	195.55*	93.32
UP x time	-3.22**	1.22

[a]N = 91.

Fit statistics:

OLS regression R-squared = .92
Durbin-Watson $\underline{d}$ for OLS regression = 1.79
Root mean square error = 34.21

*.01 < $p \leq$ .05.
**$p \leq$ .01.

Table I.6: Generalized Least Squares Model of Oregon's Monthly Basic Caseload, June 1980 to December 1987[a]

Variable	Coefficient	Standard error
Constant	35,950.95**	656.8
Time (coded 1 in 6/80 and increasing by increments of 1 thereafter)	-72.21**	10.7
Births to unwed mothers	8.97**	2.8
Policy linking need standard to the minimum wage (coded 0 before 10/80 and 1 from 10/80 onward)	-3,685.17**	333.1
OBRA (coded 0 through 9/81 and 1 thereafter)	-3,559.26**	329.7
DEFRA (coded 0 through 9/84 and 1 thereafter)	-8,255.55**	1,250.7
DEFRA x time	167.60**	22.4
October 1980 (coded 1 in 10/80 and 0 otherwise)	2,087.81**	288.7
October 1981 (coded 1 in 10/81 and 0 otherwise)	2,583.50**	288.4
UP (coded 0 from 6/80 through 1/86 and in 10/86 during a temporary suspension and 1 from 2/86 onward)	10,947.95**	2,072.3
UP x time	-150.87**	28.1
First-order autoregressive parameter	-.67**	.08

[a]N = 91.

Fit statistics:

OLS regression R-squared = .98
Durbin-Watson d for OLS regression = .64
GLS regression $\bar{R}$-squared = .94
GLS total R-squared = .99
Root mean square error = 283.19

*.01 <p ≤ .05.
**p ≤ .01.

Table I.7: Generalized Least Squares Model of South Carolina's Monthly AFDC Approvals for Loss of Support, July 1979 to December 1987[a]

Variable	Coefficient	Standard error
Constant	145.96**	9.40
Time (coded 1 in 7/79 and increasing by increments of 1 thereafter)	−1.50**	.21
Average weekly unemployment claims, lagged 1 month	.0004**	.0001
Need standard for a 3-person family	.12*	.05
OBRA (coded 0 through 9/81 and 1 thereafter)	−27.04**	9.96
October (coded 1 in October of each year)	17.08**	5.91
First-order autoregressive parameter	−.25**	.10

[a]N = 102.

Fit statistics:

OLS regression R-squared = .89
Durbin-Watson d for OLS regression = 1.50
GLS regression R̄-squared = .83
GLS total R-squared = .90
Root mean square error = 17.56

*.01 < p ≤ .05.
**p ≤ .01.

Table I.8: Generalized Least Squares Model of South Carolina's Monthly Basic Caseload, July 1976 to December 1988[a]

Variable	Coefficient	Standard error
Constant	42,743.01**	666.6
Time (coded 1 in 7/76 and increasing by increments of 1 thereafter)	208.03**	11.8
Need standard for a 3-person family	10.99**	2.7
OBRA (coded 0 through 10/81 and 1 thereafter)	28,033.97**	2,035.7
OBRA x time	−489.24**	29.1
DEFRA (coded 0 through 9/84 and 1 thereafter)	−10,632.06**	3,337.1
DEFRA x time	114.94**	33.9
First-order autoregressive parameter	−.77**	.05

[a]N = 150.

Fit statistics:

OLS regression R-squared = .96
Durbin-Watson d for OLS regression = .44
GLS regression $\bar{R}$-squared = .87
GLS total R-squared = .99
Root mean square error = 482.25

*.01 < p ≤ .05.
**p ≤ .01.

Table I.9: Generalized Least Squares Model of Utah's Monthly Basic Caseload, July 1976 to December 1987[a]

Variable	Coefficient	Standard error
Constant	10,901.43**	297.8
Time (coded 1 in 7/76 and increasing by increments of 1 thereafter)	10.45	8.8
Unemployment rate, lagged 6 months	79.20*	35.5
Step (a policy counting stepparents' income, coded 0 through 5/79 and 1 thereafter)	-4,946.62**	599.0
Step x time	81.61**	15.9
OBRA (coded 0 through 11/81 and 1 thereafter)	3,729.03**	936.9
OBRA x time	-64.19**	14.2
DEFRA (coded 0 through 9/84 and 1 thereafter)	-4,098.46**	1,188.1
DEFRA x time	37.17**	12.0
January (coded 1 in January of each year)	-346.53**	51.6
First-order autoregressive parameter	-.66**	.07

[a]N = 138.

Fit statistics:

OLS regression R-squared = .96
Durbin-Watson d for OLS regression = .68
GLS regression $\bar{R}$-squared = .86
GLS total R-squared = .98
Root mean square error = 203.52

*$.01 < p \leq .05$.
**$p \leq .01$.

Table I.10: Generalized Least Squares Model of Washington's Monthly Basic Caseload, January 1976 to December 1987[a]

Variable	Coefficient	Standard error
Constant	47,153.08**	1,882.4
Time (coded 1 in 1/76 and increasing by increments of 1 thereafter)	151.23**	13.6
Births to unwed mothers	6.05**	2.3
Employment in lumber (in thousands)	−136.99**	33.6
OBRA (coded 0 through 9/81, 1 in 10/81, 2 in 11/81, 3 in 12/81, and 4 thereafter)	−731.69**	209.8
DEFRA (coded 0 through 10/84 and 1 thereafter)	−18,209.00**	3,897.8
DEFRA x time	172.85**	35.2
June (coded 1 in June of each year and 0 otherwise)	−381.13**	103.2
First-order autoregressive parameter	−.87**	.04

[a]N = 144.

Fit statistics:

OLS regression R-squared = .98
Durbin-Watson d for OLS regression = .23
GLS regression R-squared = .87
GLS total R-squared = .99
Root mean square error = 453.04

*.01 < p ≤ .05.
**p ≤ .01.

Comments From HHS

DEPARTMENT OF HEALTH & HUMAN SERVICES

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February 6, 1992

FEB 1 '92

Ms. Eleanor Chelimsky
Assistant Comptroller General
United States General Accounting Office
Washington, D.C. 20548

Dear Ms. Chelimsky:

Thank you for the opportunity to review the GAO draft report, "Unemployed Parents: An Evaluation of the Effects of Welfare Benefits on Family Stability." The impact of cash assistance for two-parent families is an important and timely research topic. Your report finds mixed evidence on whether the presence of the AFDC Unemployed Parent (AFDC/UP) program encourages family stability by examining the impact of the presence of the UP program on growth in the number of families receiving AFDC-Basic.

I appreciate the difficulty of this task and the work performed by your staff; however, I have serious concerns about making any conclusions regarding the impact of the AFDC-UP program using nonexperimental research methodologies. The report's results are based on time series analysis, which is vulnerable to "specification errors," i.e., mistakes in specifying the appropriate theoretical structure of a model. The report itself acknowledges that this may be a problem: "As in any regression analysis, unidentified variables that were omitted from the models but correlated with the change in UP policy could alter our results." Other nonexperimental research (e.g, Schram and Wiseman, "Should Families Be Protected from AFDC-UP?," February 1988) using cross-sectional analysis, found that after adjusting for the effects of unemployment, welfare benefit levels, and other factors, States with AFDC-UP programs had significantly higher proportions of children living in single-parent AFDC families. In other words, their findings suggest that the UP program increases marital instability.

Because of the methodological problems associated with using nonexperimental designs for evaluating many kinds of welfare-related interventions, it is not surprising that the findings across studies are not consistent. In fact, nonexperimental research of training programs has shown such methods to be so unreliable, that Congress and the Administration have both insisted on experimental designs for the Job Training Partnership Act (JTPA) and the Job Opportunities and Basic Skills (JOBS) programs.

In addition, evidence from the Seattle and Denver Income Maintenance Experiments (SIME/DIME) suggests that marital decisions are not made primarily on economic bases. In SIME/DIME, intact families, as well as single-parent families, were provided benefits, and benefit levels were set so that an intact family was better off remaining together than splitting up. The expectation at the time was that a universal welfare system would promote marital stability, but the results suggest the opposite may have occurred. The availability of benefits to two-parent families did not generally reduce marital instability; the separation rates for intact families in the experimental group receiving benefits were as high or higher than those of comparable low-income families in the control groups (though there is still some debate among the academics over the magnitude of the effect).

Although SIME/DIME did not test the impact of the UP program itself, the findings are relevant to discussions of extending cash assistance to intact families and are in stark contrast to the conclusions reached in your report. Given the mixed findings in the literature, the only conclusion that I believe can be reached is that the impact of the AFDC-UP program on family stability is still an open question.

Finally, there are several other noteworthy caveats regarding the study. First, the States used in the analysis are not representative of the nation as a whole. Second, the UP programs in place prior to the Family Support Act typically did not have the work requirements that are soon to be implemented in the current program, nor did they have the time limit that 13 States have chosen to use for their programs. Third, the conditions - economic, demographic and others - present in the 1970s and 1980s may not be representative of conditions present in the 1990s. Fourth, the impact of a State decision to add or drop the UP program may not produce the same sort of change as a national requirement to adopt a program (which is likely to be perceived as a more permanent change).

Thank you again for the opportunity to review this report. If I can be of further assistance, please do not hesitate to call.

Sincerely,

Jo Anne B. Barnhart

Jo Anne B. Barnhart
Assistant Secretary
for Children and Families

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Related GAO Products

Unemployed Parents: Initial Efforts to Expand State Assistance (GAO/PEMD-92-11, Jan. 1992).

Welfare Reform: Projected Effects of Requiring AFDC for Unemployed Parents Nationwide (GAO/HRD-88-88BR, May 1988).

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