

Discussion Papers

Why Are Welfare Caseloads Falling?

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Assessing
the New
Federalism

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Program to Assess
Changing Social
Policies*

Assessing the New Federalism

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Abstract

Between 1994 and 1999, welfare caseloads fell by half. Research on aggregate state data shows the economy to have played an important role, with a 1 percentage-point drop in unemployment leading to a 5 percent reduction in reciprocity. Many studies find that work-oriented welfare reforms were also influential, although if caseloads are allowed to adjust sluggishly to new conditions—the natural dynamic when a stock of welfare families adjusts incrementally to the flow of entries and exits—policy effects go away. Caseload declines have not been traced convincingly to individual reform measures such as time limits or work sanctions.

Executive Summary

Between 1994 and 1999, welfare caseloads—initially Aid to Families with Dependent Children (AFDC), then Transitional Assistance to Needy Families (TANF)—fell by half. The conventional wisdom is that both falling unemployment and new work-oriented policies played major roles in this drop in dependence for low-income families. A number of economists and other researchers have lately sought to confirm this impression through scientific study of caseload levels and change, state to state and year to year. This paper summarizes the results of that effort—which remains ongoing—drawing from published reports of the President’s Council of Economic Advisers and a set of academic papers.

Authors for all nine of the studies examined agree that the strong economy played a role in the caseload decline of the 1990s, reducing the number of AFDC and TANF recipients roughly 5 percent for every 1 percentage-point reduction in the nation's unemployment rate. There is less agreement on the influence of changes in welfare policy, including reforms initiated by states in the mid-1990s (known as the “waiver” reforms) and federal welfare reform legislated in the Personal Responsibility and Work Opportunities Reconciliation Act of 1996. Most analyses show policy making a difference, albeit a smaller one than economic change.

However, three of the nine studies conclude that the association between policy reform and caseload change in the 1990s is misleading. They attribute caseload decline to two nonpolicy factors: lagged responses to economic growth and generally sluggish adjustment of welfare rolls to *any* change in external circumstances. This conclusion follows from the fact that, starting at least by the mid-1980s, sluggish caseload adjustment is evident throughout the observational period. Once the resulting “persistence effects” of past caseload levels are factored into the data through 1996, policy reforms in certain states and years no longer associate with lower caseloads. (These three studies do not look at the TANF years following 1996.)

The remaining studies—six of the nine reviewed—conclude that welfare reform did have an important downward influence on caseloads during the mid-1990s, effects that grew following the federal reforms of 1996. There, researchers did not posit sluggish caseload adjustment as a general phenomenon (although they did provide for delayed reactions to changes in economic conditions). At least one author justified this exclusion by suggesting that generalized “persistence effects” mask more important and fundamental factors at work in moving welfare caseloads up and down.

If persistence is not a factor, work-oriented policy reforms explain 15 to 35 percent of the drop in welfare dependence in recent years, with the economic expansion accounting for another 25 to 50 percent. (Twenty-five to 60 percent remains unexplained or relates to shifts in other factors such as demographics.) The analysts who do not see policy playing a part in falling rolls attribute an even larger share of the decline to the economy, up to 80

percent. None of these analysts has been able, in convincing fashion, to tie caseload declines to individual welfare reform components such as benefit time limits or increased work sanctions; apparently, available data and methods focused on state-level variations in results cannot reliably decompose the general effect of adopting *some* work-focused welfare reform into the effects of different individual policies. The other reform components typically addressed are family caps, enhanced earnings disregards, and tightened work exemptions.

Which camp is right on the larger question of the overall role of welfare reform? What are policymakers and others to make of the conflicting evidence in this realm as attention turns toward the decision to reauthorize federal welfare reform in 2002? These are difficult questions. They should not be settled by simple “majority rule” among the published analyses—in part because other very new studies (as well as a few more peripheral studies) have not been taken into account. The case must be decided on the respective merits of the two different research approaches.

At present, two factors tip the balance toward caution in interpreting recent declines in welfare caseloads as the effect of policy reforms. First, recent work on the dynamics of caseload adjustment—that is, how families cycle on and off the rolls in response to economic change, and how those changes cumulate to determine the size of the caseload year to year—provides a basis for *expecting* persistence and sluggish adjustment to influence caseload size independently of other factors. This suggests that analyses leaving out persistence effects miss something important, and that their conclusions on the role of policy may be off the mark as a result.

Also, even the researchers who found a persistent association between policy and dependency caution that this does not imply *causation*. There could be other reasons why welfare reform has been followed consistently by drops in dependency (e.g., states experiencing the greatest *expansion* of welfare dependence in the early 1990’s recession might have been most eager to adopt policy reforms under the waiver system and—for the same reason—most likely to outstrip other states in the magnitude of caseload *decline* once the economy turned, even without experiencing any effects from the policy reforms).

The bottom line? At present, the models that incorporate sluggish caseload adjustment provide the most sophisticated assessment of the causes of reduced welfare dependency in the 1990s. This does not mean, however, that we should conclude that the waiver reforms, and their successor federal reforms, had no effect. Rather, what we have learned is that—constrained by available research methods and data, and facing a highly complex world—the research community is unable to show convincingly that work-oriented welfare reforms contributed to the caseload decline of the 1990s. Much more research is needed, starting with extension of analyses incorporating sluggish adjustment to the TANF era and increased attention to outcomes more central to the long-run success of welfare reform than simply getting families off benefits—outcomes such as parental employment, family poverty, and child well-being. These and other key extensions to the investigation identified here deserve priority.

For policymakers considering the future of welfare reform, the clearest lesson from this review is that the best evidence—like the best policy—changes over time and needs to be revisited regularly.

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Why Are Welfare Caseloads Falling?

One of the hottest topics in welfare policy today is the large decline in the number of individuals and families receiving cash assistance under the Aid to Families with Dependent Children (AFDC) and its successor, the Temporary Assistance for Needy Families (TANF) programs since the mid-1990's. Cash assistance caseloads have been cut in half between 1994 (the programs' high) and 1999. The key question is whether the drop was caused by the numerous welfare reform policies passed in the 1990's or by the strong U.S. economy during in the same time period. Some welfare reformers use caseload decline as an indicator of success for new policies with a strong emphasis on work and independence. Others argue that the strongest economic growth period of the 20th century provided welfare recipients better employment opportunities to leave cash assistance. Demographic shifts also may have played a role (e.g., declining teen birth rates) as well as other policies (e.g., changes in the minimum wage and the Earned Income Credit). The reality may be that all these forces have worked together.

This debate has led many economists to study the causes of caseload declines in the 1990's. Spurred by a 1997 report by the Council of Economic Advisors (CEA) that attributed about equal shares of the decline to economic growth and welfare reform, researchers have focused on how welfare policies and strong economic conditions relate to welfare outcomes. This paper reviews the most often-quoted set of papers in this literature, focusing on changes in cash assistance caseloads. The papers reviewed form a coherent set of studies that are, in many ways, similar in approach but that build on one

another through communication among the authors in an effort to resolve their differences. The studies rely on differences in caseloads, welfare policies, and economic conditions across states and over time to explain the effects of policy and the economy on caseloads. All papers examine the period of state-initiated waiver reforms in the mid-1990s, and some extend into the first two years of federal welfare reform following the Personal Responsibility and Work Opportunities Reconciliation Act (PRWORA) of 1996.

Background and Overview

Prior to passage of PRWORA in August 1996, many states received waivers from federal regulations that allowed them to experiment with new AFDC policies.¹ These reforms experimented with many of the concepts incorporated in PRWORA including stronger work incentives, work requirements, tougher sanctions for those who did not follow the rules, “family caps” that prevented increases in benefits when a child was conceived and born while a family was on welfare, and benefit time limits. Although states were eligible for waivers beginning in the 1960s, most waiver reforms were approved in the early to mid-1990s when states began to take a more active role in reforming welfare and the Clinton Administration readily approved states’ experimentation. Subsequent changes stipulated for the Temporary Assistance to Needy Families (TANF) program, PRWORA’s successor program to AFDC, moved states further in this direction beginning in late 1996.² The research studies reviewed use this variation in policy across states and over time to consider whether the reforms

¹ See U.S. Department of Health and Human Services (1997) for a summary.

² See Gallagher et al. (1998) for a summary of early TANF reforms in each state.

corresponded with—or, in fact, caused—reductions in cash assistance caseloads. The studies also examine whether states with larger drops in unemployment rates or other signs of economic vitality had larger declines in caseloads than states with less robust economies.

This literature reveals that it is very difficult to untangle the causes of cash assistance caseload declines. Available data on welfare participation are either fairly crude (annual aggregates by state) or limited in sample size at the state level (national surveys), and the potential causes of participation are many. Not all of the expected determinants of caseload size can be measured well and some arbitrary variation in caseload measures is inevitable. Even when many causal factors are considered at once, none of the several studies reviewed here comes close to explaining *all* of the unprecedented caseload decline in the 1990s.

Most disturbing is the “mixed bag” of evidence produced regarding policy's influence on caseload size. Although there is considerable evidence that states with welfare reform waivers had larger declines in caseloads than states without waivers, the role of waiver policies in *causing* the declines remains uncertain. The same is true for policy changes under PRWORA, where the simple association between caseload size and reform measures is even stronger. Although available research techniques show these associations (after controlling for other factors such as the economy), they cannot establish clear cause-and-effect relationships. Nor, when caseloads respond slowly to new conditions, can these techniques fully and unambiguously separate the “carry-over” effects of earlier economic and policy changes from the influence of new, emerging policies. Alternative scenarios in which policy reforms did not cause caseload declines

cannot be ruled out in all cases, and in some instances seem the most consistent with the data. Inherent difficulties plague the analysis: Relationships are complex, policy choices can be driven by caseload levels rather than the reverse, caseloads may respond sluggishly to new influences, one waiver provision may offset another, and different subsets of welfare recipients may react differently to the same policy reform. Not surprisingly under such circumstances, different studies have arrived at substantially different conclusions regarding the influence of individual policy reforms.

In contrast, the theory and evidence on the role of economic factors is fairly clear. Researchers agree that strong economic growth contributes to caseload decline in a causal sense, and there is general agreement on the magnitude of this effect over the long run. Economic growth does not produce immediate fruit, however; most of the reduction in caseloads occurs one to two years after unemployment drops.

Finally, as noted previously, much of the change in cash assistance caseloads during the 1990s remains unexplained in every analysis, even factoring in influences beyond economic growth and welfare policy such as demographic change and cross-state differences in benefit levels. Though not surprising from a statistical standpoint, the remaining “unexplained variation” creates new areas of uncertainty. What else might have affected welfare dependence over the interval examined? Might the influence of the measured factors—including welfare policy and economic growth—itsself have been underestimated, due perhaps to the limitations of the specific variables used? It is also possible that the economy and policy interact; for example, enhanced work incentives may reduce caseloads in strong economies but increase them when jobs are tight. Little

has been done to examine these relationships. As a result of all these complexities, none of the literature on caseload decline can be relied on absolutely.

The paper proceeds as follows. It first introduces the set of papers to be reviewed, pointing out their common features and sequencing. It then summarizes their findings with regard to three questions: 1) Have welfare reform policies made a difference? 2) How much did the economy matter? and 3) How much do we really know about the causes of caseload decline? Conclusions and a discussion of remaining issues appear in a closing section.

Recent Studies of Welfare Caseload Decline

The literature on the role of policy reforms and the economy in the drop in welfare caseloads has grown rapidly over the last four years. We will focus here on the set of papers spurred by a Technical Report by the CEA in 1997 on the causes of AFDC caseload changes through 1996. Listed in table 1, these include CEA reports from 1997 and 1999; Ziliak, Figlio, Davis and Connolly (2000; originally published in 1997); Blank (1997); Figlio and Ziliak (1999); Bartik and Eberts (1999); Wallace and Blank (1999); Moffitt (1999); and Schoeni and Blank (2000). Several other more recent or less central entries in this literature that are not reviewed in detail here are identified in table 2.

The 1997 CEA Report uses annual state-level administrative data on the number of AFDC recipients divided by the total population of the state to measure the share of the total population receiving AFDC. Welfare reform policy is represented by the year a statewide waiver reform policy was approved, and economic conditions are measured using state unemployment rates. The model also incorporates other factors that might

Table 1
Caseload Decline Studies

(in order of initial release)

<u>Author(s)</u>	<u>Title</u>	<u>Year</u>
Council of Economic Advisors	Technical Report: Explaining the Decline in Welfare Receipt, 1993 - 1996	1997
James P. Ziliak David N. Figlio Elizabeth E. Davis Laura S. Connolly	Accounting for the Decline in AFDC Caseloads: Welfare Reform or the Economy?	2000
Rebecca M. Blank	What Causes Public Assistance Caseloads to Grow?	1997
David N. Figlio James P. Ziliak	Welfare Reform, the Business Cycle, and the Decline in AFDC Caseloads	1999
Timothy J. Bartik Randall W. Eberts	Examining the Effect of Industry Trends and Structure on Welfare Caseloads	1999
Geoffrey Wallace Rebecca M. Blank	What Goes Up Must Come Down? Explaining Recent Changes in Public Assistance Caseloads	1999
Robert A. Moffitt	The Effect of Pre-PRWORA Waivers on AFDC Caseloads and Female Earnings, Income and Labor Force Behavior	1999
Council of Economic Advisors	Technical Report: The Effects of Welfare Policy and the Economic Expansion on Welfare Caseloads: An Update	1999
Robert Schoeni Rebecca Blank	What Has Welfare Reform Accomplished? Impacts on Welfare Participation, Employment, Income, Poverty, and Family Structure	2000

Table 2

Additional Studies Not Reviewed

(by year of publication)

<u>Author(s)</u>	<u>Title</u>	<u>Year</u>
David J. Fein	Understanding Recent Declines in State AFDC Caseloads: An Analysis of Cross-State Variation	1996
David C. Stapleton Gina Livermore Adam Tucker	Determinants of AFDC Caseload Growth	1997
Phillip B. Levine Diane Whitmore	The Impact of Welfare Reform on the AFDC Caseload	1998
Robert E. Rector Sarah Youssef	The Determinants of Welfare Caseload Decline	1999
Richard Bavier	An Early Look at the Effects of Welfare Reform	1999
Peter R. Mueser Julie L. Hotchkiss Christopher T. King Phillip S. Rokicki David W. Stevens	The Welfare Caseload, Economic Growth, and Welfare to Work Policies: An Analysis of Five Urban Areas	2000
Mark Henry Willis Lewis Lynn Reinschmiedt	Reducing Food Stamp and Welfare Caseloads in the South: Are Rural Areas Less Likely to Succeed Than Urban Centers?	2000
James P. Ziliak David N. Figlio	Geographic Differences in AFDC and Food Stamp Caseloads in the Welfare Reform Era	2000
Jeffery Grogger	Time Limits and Welfare Use	2000
Dan A. Black Terra G. McKinnish Seth G. Sanders	Are We Understating the Impact of Economic Conditions on Welfare Roles?	2000
Thomas MaCurdy David Mancuso Margaret O'Brien-Strain	How Much Does California's Welfare Policy Explain the Slower Decline of Its Caseload?	2000
Rebecca M. Blank	What Causes Public Assistance Caseloads to Grow?	2001

affect caseloads such as state AFDC benefit levels, past years' unemployment rates (called "lag" factors), and "lead indicators" of coming waiver reforms in the year before they are approved (to capture possible anticipatory effects of the reforms should recipients become aware of proposed changes and accordingly adjust their behavior in advance).³ The report concludes that 44 percent of caseload decline nationally between 1993 and 1996 was due to the economy, 31 percent to state welfare policy changes, and 25 percent to "other" factors—both those included in the model and those not anticipated.⁴

Papers by Ziliak et al. (2000; originally published in 1997) and Blank (1997) emerged in response to the 1997 CEA report and extended the literature by using new data and techniques that add to our ability to understand caseload decline. These authors replicated the CEA results and then showed differences in results coming from the new techniques. Ziliak et al. used monthly caseload data instead of annual data in an attempt to measure and distinguish between short-term and long-term effects. This allowed them to include a very rich set of lagged measures to determine if the effects of either the economy or welfare policies take time to have an impact on caseloads. Blank added variables to the model to try to explain state demographic and political factors that might affect caseload levels—such as the percentage of families headed by a single mother and the percentage of the population with a high school degree. She also looked at more

³ Additional controls deal with factors not explicitly included in the model. These include permanent distinctions among states caused by fixed factors that predate the reforms (called "state fixed effects"—e.g., demographic, historical, and political differences); shifts in national welfare participation patterns year to year (called "time fixed effects" or "year effects"), and state-specific trend lines for prereform caseload changes that depart from national norms.

⁴ The CEA report also resulted in a journal article by Levine and Whitmore (1998). This and other less prominent papers are included in the supplemental bibliography of studies not reviewed here.

detailed measures of AFDC caseloads, breaking the total caseload into basic and unemployed parent cases.⁵ The Blank results tended to confirm the CEA's conclusions, while Ziliak et al. reached sharply different conclusions, attributing almost all of the caseload decline during the waiver period to the influence of a strong economy and finding essentially no evidence that policy reforms made any difference.

Figlio and Ziliak (1999) and Wallace and Blank (1999) tried to reconcile these two sets of results by focusing on their methodological and data differences. A series of possible explanations were tried and rejected, moving step by step from one analytic approach to the other. Ultimately, both sets of authors traced the conflicting results to differences in how the original papers modeled caseload adjustments over time in a dynamic framework. In addition, Figlio and Ziliak simulated the effects of a recession on caseload size and showed that waiver reforms exert more downward influence on caseload size in a strong economy than a weak one. For their part, Wallace and Blank extended Blank's analysis of demographic and political influences as complements to policy and economic effects and, like Blank, examined changes in food stamp caseloads as well as cash welfare receipt. They also begin to explore post-PRWORA data for the first time.

Bartik and Eberts (1999) and Moffitt (1999) enriched the literature in other ways. Bartik and Eberts examined a range of economic measures thought likely to better represent employment opportunities for welfare recipients than the overall unemployment rate in a state. Moffitt (1999) used household interview data instead of state

⁵ AFDC Basic cases make up the majority of AFDC caseloads. AFDC-UP cases are those for two parent families facing difficult employment transitions in the work force. Blank (1997) argues that policy and

administrative records to measure welfare participation at the state level. By starting with micro data, Moffitt was able to interact age and education with other components of the model to see if policies or economic conditions affect certain subsets of the population more than others. He also considered how labor market outcomes closely related to welfare use—specifically, employment and earnings—were affected by policy and the economy during the waiver period and how the influence of the business cycle on caseloads changed over the two decades leading up to PRWORA.

Following this spate of papers, the Council of Economic Advisors (1999) produced an updated report on caseload change that addressed many of the concerns raised by others about the 1997 report, including an important critique by Martini and Wiseman (1997). The update added data through 1998 to measure not just the effects of waiver reforms but also the early effects of national welfare reform under PRWORA. It found continued benefits from reform into the TANF era, supporting a similar pattern in the exploratory findings of Wallace and Blank. Other changes from the earlier Council report included dating the effects of waiver reforms from the start of policy implementation rather than approval, adding the 1996 increase in the minimum wage as an explanatory factor, and exploring more subtle variations in states' work exemption policies (by age of youngest child) and work sanctions (full vs. partial benefit removal).

The most recent paper reviewed, by Schoeni and Blank (2000), also examined the early post-PRWORA period through 1998, this time using household survey data aggregated to the state level, rather than state administrative records. Paralleling Moffitt, these authors looked at how a person's level of education interacts with economic and

economic conditions are likely to have different impacts on these two components of the total AFDC

policy influences to determine welfare participation. They also revisited outcomes with a strong influence on welfare dependence such as employment, earnings, and—for the first time—looked at family structure and income as additional channels through which welfare reform might exert an influence.

Several other studies from the literature, both emerging analyses and ancillary assessments of waiver effects, are listed in table 2 above but not reviewed here.⁶

Have Welfare Reform Policies Made a Difference?

Policy makers want to understand whether welfare reform policies have had an impact on AFDC and TANF caseloads separate from the economic boom. There are several policy-relevant issues to be considered. The most obvious is whether welfare reforms had a significant impact on caseloads and whether the magnitude of that impact was large or small—information of great importance to the upcoming Congressional debates regarding the reauthorization of TANF past 2002. The timing of impacts is also of interest—whether individuals respond immediately to policy changes or whether there is a lag. Similarly, it is important to know whether individuals respond to the announcement of a reform or to the implementation of the new policies. Moving away from the all-encompassing term “welfare reform”, policymakers would like to know which specific policies make a difference and which do not. For example, policies that

caseload.

⁶ For example, Blank (2001)—an update of Blank (1977)—was received too late to be included fully in this review. Certain elements of the paper are noted, however.

limit the number of years benefits can be received (time limits) are likely to have quite a different impact on caseload size than policies that hold benefits constant when a mother has a child while on welfare (family caps). Finally, there are the questions of whether reforms affect some types of welfare recipients more than others or work better in a strong economy than a weak one.

Summary of Findings. Collectively, the reviewed studies address all of these questions within the limits of available data and analytic techniques. They first consider whether reforms affected caseloads at all, controlling for the existence of business cycles and other factors to the extent possible. At this level no attempt is made to understand the role of specific policy provisions such as a family cap or an enhanced earnings disregard, a subject to which we will return later. Instead, the focus is on whether a state adopted *any* statewide waiver reform during the pre-PRWORA period and—in more recent analyses—what happened after states implemented their TANF programs.

Even at this general level, results are not consistent or entirely satisfying. The majority of studies found that states adopting reforms had significantly sharper caseload declines than those without reforms, after accounting for other potential influences such as the economy and welfare benefit levels (see table 3). This effect results in caseloads 5 to 18 percent smaller during the waiver period than what they would have been without reforms, and 19 to 35 percent smaller following implementation of TANF. Six of the eight reviewed studies report the impact of “any welfare reform” showed this pattern, including three that looked at data past 1996 (through 1998) and hence indicate continued

Table 3**The Effect of "Any Reform" on AFDC/TANF Caseload Size***

Study	Time Period Analyzed	Predicted Percentage Change in Caseload
CEA 97	1976-96	-5.17 ^a
Ziliak et al. 00	1987-96	na
Blank 97 (AFDC-Basic only)	1977-95	-10.7 ^a
Figlio & Ziliak 99	1976-96	.505
Bartik & Eberts 99	1984-96	.66
Wallace & Blank 99	1980-96	waivers - 7.2 ^a
	1980-98	waivers -13.8 ^a
	"	TANF - 34.7 ^a
Moffitt 99	1977-95	-5.751 ^a
	1977-95 (CPS)	-15.047 ^a
	1981-95	-1.653
	1987-95	3.744 ^a
CEA 99	1976-98	waivers -9.40 ^a
		TANF -18.84 ^a
Schoeni & Blank 00	1976-98	waivers -10 ^a
		(approx.) TANF -22 ^a (approx.)

*Estimates are from authors' preferred specifications. Policies include statewide or nearly statewide waivers and (for analyses extending beyond 1996) statewide TANF policies.

^a Statistically significant at at least the 10% level.

caseload reductions attributable to TANF.⁷ The other two papers in this group—by Figlio and Ziliak (1999) and Bartik and Eberts (1999)—showed essentially no effect of the waiver reforms. They did not address the TANF period.

Why this difference on such a crucial point? Given the general difficulties of caseload analysis discussed earlier, it is not surprising that some explanatory factor—policy or otherwise—should have a range of measured influences across studies. To understand the specific pattern of results seen here, we must concentrate on the estimated effects of the waiver reforms—for which the main discrepancy arises—and return to the TANF results later. Many hypotheses have been advanced and tested for why the waiver results differ among the papers, including differences in how waiver policies are measured (fairly uniformly⁸), when the influence of waivers is presumed to have begun

⁷ Wallace and Blank, who supply the 35 percent figure for the TANF period, warn that it probably overstates TANF's influence by attributing all otherwise unexplained changes in caseloads (after accounting for various economic, demographic, and political factors included in the model) after December 1996—when their TANF indicator turns from 0 to 1—to the reforms. Though not mentioned by the authors, this figure is also an *underestimate* relative to estimates from the other two studies that look at TANF, CEA 1999 and Schoeni and Blank. Unlike those authors, Wallace and Blank allow the waiver provisions to have a separate influence even once TANF is added in January 1997; they do not "turn off" the waiver indicator variable as did other authors at that point. As a result, Wallace and Blank's finding of a 35 percent decline in caseloads due to TANF is the *additional* decline due to TANF, over and above the effects of any waiver policies that remained in place into the TANF era (as many did). This way of structuring the results contrasts with the papers that "turned off" the waiver indicator when TANF arrived, generating measures of the combined effect of TANF and any remaining waiver provisions relative to standard AFDC rules. Fortunately, the Wallace and Blank results can be translated into the same type of comprehensive measures. To do so, we first adjust Wallace and Blank's estimate of the effects of waivers alone—a 14-percent reduction—downward to 9 percent to reflect the fact that waivers' post-TANF influence must be averaged across all states, not just the two-thirds of the states that had waivers when TANF arrived. This gives a hypothetical "with waivers but without TANF" caseload projection for the TANF period equal to 91 percent of what the AFDC caseload would have been without any reforms. An additional 35 percent reduction due to TANF brings the overall caseload to 59 percent of what it would have been absent all reform, implying a 41 percent total reduction due to the two sets of reforms combined. This result far exceeds the corresponding finding produced by CEA '99 (a 9 percent reduction) and implied by Schoeni and Blank and is certainly an exaggeration of the true effect of waivers plus TANF.

⁸ Blank (2000) suggests that the unusual specification of waiver policies as a one-time effect in the Figlio and Ziliak formulation may be one reason waivers are found to have little effect. However, when waivers are allowed to have an added effect the year after implementation and in any subsequent year, both Ziliak et al. (table 5, column 5) and Figlio and Ziliak (footnote 10) find that waivers do even less to reduce caseloads as time goes on.

(at approval, at or after implementation, or even preapproval), and how long it took for economic changes to have their full effect on caseload size. Table 4 summarizes all nine studies in these respects, also highlighting the different data sources used (administrative or survey, annual or monthly) and the years examined leading up to the waiver period (starting from as early as 1976 or as late as 1987). Based on research in the papers themselves, none of these factors accounts for the difference in results.⁹ Nor do the different secondary influences on caseload size considered by the different authors, such as nonwaiver policies or measures of the political, labor market, and demographic context (see list in table 5).

What does affect the results is the assumption made about *the pace of caseload adjustment* to these different influences. As shown in the last column of table 4, all six papers that show waivers to have had a downward influence on caseloads assume that caseloads react quickly to new external and policy circumstances.¹⁰ In contrast, the papers reaching a different conclusion, that the waiver reforms did not affect caseload

⁹ While not capable of explaining the differences in results *across* the studies shown in Table 3, Moffitt did find that the length of time prior to the waiver era covered by the analysis influences the estimated waiver effect. Moffitt's main results, drawn from data going back as far as 1977, show waivers to have significantly reduced caseloads, similar to CEA '97, Blank (1997), and Wallace and Blank. This pattern is reversed when the historical reference period is shortened. Specifically, Moffitt finds that the waiver effect becomes insignificant when analysis is limited to just the year 1981 and beyond, and turns significantly *positive* when studying an even shorter period beginning in 1987. Others have used a shortened reference period in deriving their *main* results, including at least one paper in the set that finds no significant influence from waiver adoption (Bartik and Eberts). But there are also counterexamples to the suggestion that more limited historical data explain the insignificant findings in Table 3. For example, Figlio and Ziliak also obtain insignificant results without restricting their historical time frame to later years (they analyze data from 1976 through 1996) while Bartik and Eberts show that even foreshortened data (1984 through 1996) can produce significant, negative waiver effects when analyzed using the CEA '97 or Blank (1997) methodologies. Ziliak et al. (1997) make the same point using even more recent data, 1987 to 1996. The question of the *preferred* reference period for the analysis, raised by Moffitt, has been left unresolved in the literature. It does reappear, however, in the discussion of the caseload and the economy, below.

¹⁰ More precisely, they estimate impacts as if new circumstances had an immediate, fixed influence rather than a changing or lagged effect.

size, assume that adjustments are sluggish—that preexisting caseload levels “hang on” over time, making today’s caseload higher or lower than observable circumstances would dictate simply because last month’s (or last year’s) caseload was high or low. This persistence effect, called “state-dependence” by researchers (since it implies that today’s outcomes depend on the “state of affairs” at a previous time), is something researchers often look for when studying changes over time in economic aggregates such as the unemployment rate or the money supply. Figlio and Ziliak and Bartik and Eberts (as well as Ziliak et al. 2000) allow for the possibility of sluggish change when analyzing welfare caseloads prior to and during the period of waiver reforms, while other papers do not. That this should make such a difference—the difference between concluding that waiver policies reduced caseloads to an important degree and concluding that they did not¹¹—requires some explanation. We seek that explanation in this paper, building on but moving beyond the literature itself while recognizing the difficulties inherent to any clear-cut interpretation of the data.

Interpreting Differences in Results. Since 1994, the point at which large numbers of waiver reforms began to be adopted, the tendency has been for each year's

¹¹Three papers demonstrate the importance of dynamic assumptions in determining the size of the estimated waiver effect. Figlio and Ziliak compare results from their “first differences” model—in which they seek to explain year-to-year *changes* in caseload size—with results on caseload *levels* taken from models patterned after CEA ‘97 (pp. 31–32). The former show insignificant waiver effects while the latter essentially replicates the “waivers matter” conclusions of the original CEA report. Ziliak et al. achieve the same contrast by adding the previous year’s caseload to the basic CEA model for caseload levels to create what is known as a “lagged dependent variable” model. They then confirm the lack of any important waiver influence in a “first-differences” analysis (see columns 1 through 4 of their table 7). Wallace and Blank (1997)—picking up on a feature of the original Ziliak et al. analysis—achieve this same contrast by forcing caseload size to follow a smooth curve from one year to the next along a “cubic” time trend path, which again eliminates the apparent influence of waiver policies on caseload size produced by simpler models (p. 83).

Table 4
Studies and Methods

Study	Time Period(s) Analyzed	Type (and Source) of Caseload Data	Economic Indicator Used	Economy's Influence Lasts:	Policies Examined	Characteristics of Preferred Model		
						Adjustment to Policies Begins	Pace of Implementation	Caseload Adjusts
CEA 97	1976-96	Annual (state administrative records)	Unemployment Rate	2 Years	Any Waiver Individual Reforms	Pre-Approval	Immediate	Quickly
Ziliak et al.	1987-96	Monthly (state administrative records) Annual (state administrative records)	Unemployment Rate Employment per Capita	12 Months 2 Years	Individual Reforms	At Approval	Gradual	Sluggishly
Blank 97	1977-95	Annual (state administrative records—AFDC-Basic cases only)	Unemployment Rate	3 Years	Any Waiver Individual Reforms	At Approval	Immediate	Quickly
Figlio & Ziliak	1976-96	Annual (state administrative records)	Unemployment Rate	5 Years	Any Waiver	At Approval	Immediate	Sluggishly
Bartik & Eberts 99	1984-96	Annual (state administrative records)	Unemployment Rate Employment Growth Rate Share of Workers with High School Diploma Share of Workers on AFDC Above/Below Average Wage Rates	3 Years	Any Waiver	At Approval	Immediate	Sluggishly
Wallace & Blank 99	1980-96 1980-98	Annual (state administrative records) Monthly (state administrative records)	Unemployment Rate	3 Years 24 Months	Any Waiver Any Waiver, TANF	At Approval Pre-Approval	Immediate	Quickly
Moffitt	1977-95 1981-95 1987-95	Annual (state administrative records) Annual (Current Population Survey)	Unemployment Rate	2 Years	Any Waiver Individual Reforms	At Approval	Immediate	Quickly
CEA 99	1976-98	Annual (state administrative records)	Unemployment Rate	3 Years	Any Waiver Individual Reforms TANF	At Implementation	Immediate	Quickly
Schoeni & Blank	1976-98	Annual (Current Population Survey)	Unemployment Rate Employment Growth Rate	3 Years	Any Waiver TANF	At Implementation	Immediate	Quickly

Table 5

Factors Other Than Welfare Reform Policies Used to Predict Caseload Size

Other Factors Influencing Caseload Size in Preferred Specification	CEA 97	Ziliak et al. 00	Blank 97	Figlio & Ziliak 99	Bartik & Eberts 99	Wallace & Blank 99	Moffitt 99	CEA 99	Schoeni & Blank 00
Policy Variables									
Maximum AFDC benefit	*		*		*	*	*	*	*
AFDC-UP indicator			*			*			
Average family Medicaid expenditures			*						
Political Variables									
Political party - governor			*			*			
Political party - statehouse			*			*			
Labor Market Variables									
Median hourly wage rate			*			*			
20th-percentile hourly wage rate			*			*	*		
Minimum wage rate								*	
Demographic Variables									
% black			*			*			*
% Hispanic									*
% female head			*			*			
% nonmarital births						*			
% immigrants (lagged)			*			*			
Age			*			*	*		*
Education			*			*	*		*

AFDC/TANF caseload to be low by historical standards, and lower than the year before. The many nonwaiver determinants of caseload size examined in the literature—change in economic circumstances, demographic shifts, nonwaiver policy changes—cannot fully explain the relatively low and declining nature of welfare participation during this time, even when economic influences are allowed to “cascade” through the caseload for several years using lagged economic variables. Some of the remaining decline is general to the nation, or consistently more rapid (or less rapid) in individual states, elements taken into account in most of the analyses reviewed. The remaining decline—specific to individual states (or sets of states), out of line with basic trends, and not explained by other factors in the model—is where conflicting results arise. All of the studies reviewed here seek to interpret this “residual” decline, and the resulting unexpectedly low caseloads in certain states during the mid 1990s, as consequences of the gradual arrival of waiver reforms between 1993 and 1996. This explanation makes sense when no further factors are considered.

However, three of the papers—Ziliak et al., Figlio and Ziliak, and Bartik and Eberts—go further. While still allowing the arrival of the waiver reforms to account for the remaining decline, these authors also consider sheer persistence in caseload levels as an alternative explanation. They posit that both influences—policy changes and persistence—were at play and run an analysis that determines how powerful each must have been to account for the observed pattern to the fullest extent possible. They find that a more complete explanation of the many aspects of caseload levels and movements during the mid 1990s is possible when the sheer persistence of past caseload levels is allowed to exert a strong influence on current caseload size. Once this additional factor

is taken into account, the falling caseloads of the mid 1990s are no longer systematically associated with the arrival of the waiver reforms as in other formulations. When confronted with both possibilities, the data accord better with the “universal persistence” hypothesis than with an independent caseload-reducing role of the waiver reforms. The immediate implication is that if the waiver reforms do not correlate with caseload changes once persistence and other factors are taken into account, they almost certainly did not *cause* the caseload declines of the mid 1990s.¹²

The story of sluggish caseload adjustment suggests that some event or series of events at the end of the 1980s or during the early 1990s jostled the caseload downward in a way not captured by other measured factors. Subsequent caseload declines—or at least persistently lower caseload levels than expected given observable circumstances—followed because of the sluggishness with which the caseload responds to any specific stimulus. Persistence of past levels plays a role, not just changes in current circumstances.

To assess this conclusion, one must delve deeply into what “sluggish adjustment” and “persistence” mean and where they come from. Rebecca Blank argues that persistence masks more fundamental mechanisms at work in the economy and in the influence of policy.¹³ In her view—offered also by Wallace and Blank in response to Ziliak et al.—we should seek an explanation of caseload levels based on real economic

¹² The added factor of persistence is modeled either by including the previous year's (or month's) caseload as an explanatory factor in a regression model or by analyzing year-to-year changes in caseloads in a “first differences” framework that assumes the current caseload mirrors last year's caseload except due to changes in other measured factors. Ziliak et al. apply both of these techniques at once, running “first differences” regressions using an equation that includes the prior period's caseload level. Figlio and Ziliak—and Bartik and Eberts—apply the two techniques separately, confirming that they give equivalent (i.e., insignificant) results regarding waiver impacts.

¹³ Personal correspondence via e-mail, October 31, 2000.

and policy variables, not circumstantial patterns in the data over time. While the data may match up well with the formal representations of “persistence” in mathematical models (i.e., lagged dependent variables, first-difference equations), the factors that *cause* persistence to be present hold the greater interest.

Blank seeks those factors in the period just prior to the waiver reforms. AFDC caseloads *rose* to an unusual degree during the late 1980s and early 1990s for reasons little understood in the literature. In a paper extending her earlier results,¹⁴ Blank (2001) shows that growth in two-parent and child-only cases is a large part of the explanation. But her analysis focuses on the growth in caseloads that took place around the country, which, while certainly important, does not speak to the persistence effects found by other researchers once state and national trends have been removed from the data. Even so, Blank's analysis may have something to tell us about persistence—or at least about future research on the subject.

Summarizing her findings in a review paper, Blank (2000) notes that simple models without persistence terms adequately explain variations in the “core” AFDC program—the portion remaining after removing two-parent and child-only cases—during the late 1980s and early 1990s. If persistence did not play a role over this period, the evidence of persistence in the longer time frame analyzed by Ziliak and others, 1987 (or 1976) to 1996, might not have been so strong.¹⁵ Under such a scenario—that is, once one

¹⁴ The earlier results referenced here appear in Blank (1997).

¹⁵ Recall that Ziliak et al., Figlio and Ziliak, and Bartik and Eberts made both patterns—unexpectedly high state-specific growth in the rolls during the late 1980s and early 1990s, and unexpectedly steep state-specific declines in the mid-1990s—part of the same phenomenon, general sluggishness in caseload adjustment (in either direction, at any time). Indeed, many of their analyses—those using still longer-term historical data—combine these two periods with evidence of potential caseload sluggishness during the preceding decade (1976–1986).

removes a substantial portion of the basis for believing in persistence generally—the surprising downward movement of caseloads in the mid-1990s might, in fact, have been best explained by the arrival of the waiver reforms even in models allowing for persistence as a competing explanation. New analyses would need to be run to test this possibility, analyses that take into account the growth in two-parent and child-only cases at the start of the 1990s, the arrival of waiver reforms mid-decade, and the potential for generalized persistence effects over the entire 1987 (or 1976) to 1996 period. Until these analyses are done, we can only speculate.

Other recent research has provided theoretic *support* for the persistence theory. Rather than assuming—as did other authors—that the economy directly affects caseload size, Klerman and Haider (2000) posit a world in which economic conditions influence the *rate* of welfare entry among families not currently on the rolls and the *rate* of welfare exit among families already receiving benefits. These “transition rates” depend on current economic conditions in a stable fashion, since in an unchanging economy job offers come in to a pool of potential workers (here, welfare recipients) gradually and at a steady pace (or, in a weak economy, jobs dry up gradually at a steady pace). Overall caseload size at any point in time reflects entry and exit over many prior years. Hence, unlike current flows, current caseload size depends on both current and *prior* economic conditions.

Based on this model—they see as consistent with the microeconomic foundations of welfare participation decisions—Klerman and Haider derive an expression for caseload size that depends not on prior economic conditions but on *current* economic conditions and last year's caseload size. (The latter captures fully the influence of all

preceding years' economic conditions.) This is precisely the Ziliak et al. and Bartik and Eberts formulation for persistence in caseload size—or, equivalently, for sluggishness of caseload adjustment.¹⁶ Thus, Klerman and Haider show how the dynamics of caseload change in and of themselves can create “persistence effects,” with no external factor or deeper meaning needed beyond the normal ebb and flow of jobs in the economy.

Welfare policy variables and other noneconomic factors can be added to the model without changing its fundamental character:¹⁷

Choosing between Competing Explanations. Does this mean that the persistence models have arrived at the correct answer, and that the apparent effects of the waiver reforms in other studies are an illusion? Yes and no. Yes, if one believes it possible that some underlying force keeps caseloads low when they have been low and high when they have been high out of proportion to what measured influences say should

¹⁶ Figlio and Ziliak subtract last year's caseload from the left-hand side of their regression equation rather than including it on the right-hand side, yielding the "first differences" version of persistence. The result is much the same. In drawing the comparison to the Klerman-Haider results, it is important to note some features not present in the other articles, including a dependent variable specified in levels not logs and appropriate interactions between the lagged caseload variable and current economic conditions. Allowance for "duration dependence"—welfare exit rates that change (usually, drop) as families spend more and more time on the rolls—also complicates the Klerman-Haider model in ways that other authors do not take into account. But the basic point still holds: a one-period lagged value of the dependent variable belongs in the model when economic conditions act on entry and exit rates rather than caseload size directly.

¹⁷ The question arises as to whether welfare policies, like the economy, matter most through their effects on entry and exit rates or by having a more direct, immediate effect on caseload size itself. Klerman and Haider do not address this point. There are reasons to think the "flow" model of caseload determination—sometimes called a "stock and flow" model—makes less sense for policy changes. As noted in the text, economic conditions are thought to affect the caseload gradually, through the ebb and flow of job openings (or closings) in the private sector, a flow that dictates at a point in time who can leave welfare or—in a recession—who loses their jobs and must go onto welfare. Welfare policy changes do not have such an obvious flow component. To the extent that program rules matter, they make welfare either more or less attractive to a family at a given point in time. Families do not have to wait for other families to move off the rolls—or for new welfare "slots" to open up incrementally—to respond to this added attraction or deficit, as they do when looking for jobs in the economy. As soon as a policy change is implemented and understood, its full effect on caseloads can, in principle, be felt. In particular, the arrival of a new waiver policy is capable of making all the difference it will ever make in a hurry, without waiting for new flows on and off the rolls to ripple through the caseload as must happen in responding to economic change.

happen. Besides the natural dynamics of caseload flow, other mechanisms of this sort are possible. The most likely candidate is force of habit in human behavior. Families have inertia that at least in some cases keeps them on the rolls in year T simply because they were on in year T minus 1, despite other indicators that might suggest exit, while the reverse is true for families not on the rolls. Thus, the caseloads may have been surprisingly low in the aftermath of waiver reforms not because the waivers themselves exerted a downward influence but because previous forces had moved more than the usual number of families off the rolls, before the arrival of the waiver reforms, and—once sparked—the experience of living without welfare became habit-forming year-to-year.

Closely akin is the theory of barriers to entry and exit in the welfare system—hurdles that must be surmounted to qualify for benefits initially and that then discourage exit.¹⁸ These hurdles include the familiar paperwork burdens families face in establishing their assistance cases in the welfare office and the need to meet behavioral requirements in order to qualify for benefits, such as looking for a job or immunizing one's children. Not only do these hurdles retard entry onto the rolls for those not currently receiving support, they provide an incentive to stay on the rolls once the “transactions costs” of entry have been met—even when circumstances improve to the point that exit would otherwise take place. Future prospects remain uncertain, and no one wants to run the gauntlet of the application process again should they fail to attain permanent self-sufficiency. Both sets of factors—habitual behavior and the “stickiness” of the welfare

¹⁸ The author is grateful to Alan Weil for this suggestion, a point also made by Klerman and Haider (2000).

system—may have led families that left AFDC in the early 1990s to stay off benefits during the inevitable ups and downs of their personal lives during the mid-1990s. Waiver policy changes, if they played a role at all, were secondary to these factors under this theory.

Alternatively, habitual behavior, institutional barriers, and the gradual responses of the caseload “stock” to the flow of entries and exits may not be real. Instead, a whole different set of factors may have been at work that none of the authors to date has analyzed, factors that can explain the unexpectedly high caseloads of the late 1980s and early 1990s without recourse to persistence effects and, thus, leave the waiver reforms as the best possible explanation for later declines. There are many candidates for the role of missing factors, although some must be ruled out because they would have had a fairly uniform influence across the country. This includes such things as a shift in social attitudes toward “getting what one can for oneself” that may have taken place during the 1980s, as part of what is often called the “me decade.” While this could lead to greater utilization of government income transfers among the eligible population, there is no reason to suppose it was more at play in some states than others.

Prewaiver policy changes also may have caused caseloads to grow over that period, changes not included in anyone's model. Some of these could have been state-specific and out of line with overall state trends and, thus, account for the remaining variation behind persistence, such as changes in state minimum wage laws. Or—as Blank (2000) points out—certain national policy changes such as a hike in the federal minimum wage may have exerted a greater influence on labor markets and caseload

levels in some states (e.g., those with low general wages) than in others.¹⁹ Others seem likely to have had more uniform consequences, such as the expansion of the federal Earned Income Tax Credit. The job market may also have changed during the 1980s and early 1990s to make it harder for low-skilled parents to stay off welfare by working, but in a way not captured by the broad economic indicators typically included in the analyses (e.g., the general unemployment rate). However, such changes again would have had to have occurred differently by state—and out of keeping with past state-specific trends—to account for the unexplained patterns underlying the persistence results.

A Bottom-Line Assessment and Extension to TANF. At this point, no one can really say which of these many explanations is the right one. Three conclusions regarding the role of waivers in reducing AFDC caseloads during the mid-1990s are possible, however:

1. The conflicting results seen are not simply statistical artifacts, but reflect real uncertainties about how welfare families behave, how welfare caseloads adjust, and how policies and labor markets change over time, particularly prior to the period of waiver reforms.
2. The persistence of welfare dependence (and non-dependence) over time due to habitual behavior, welfare rules, or stock and flow patterns of case entry and exit

¹⁹ CEA (1999) included the federal minimum wage in its analysis and found that it did not substantively affect the estimated impact of welfare policy reform (for either the AFDC or TANF period). We do not know whether this variable would have any effect on measures of persistence, perhaps supplanting the persistence explanation altogether, were it added to a model that includes a persistence term.

provide the most complete explanation to date of caseload ups and downs during the 1980s and 1990s. Though by no means a certainty, these interpretations—if correct—leave little possibility that the waiver reforms had an influence on caseload reductions in the 1990s.

3. The same caseload patterns in the 1980s and early 1990s could be due to unmeasured changes in policy, labor market conditions, or other factors that exerted different effects in different states but were not captured in the analysis.

Besides looking for alternative explanations of caseload growth in the years prior to waivers, a number of other analyses could help to confirm or deny the persistence interpretation. Persistence models need to be extended to outcomes other than caseload size, particularly outcomes on which reforms might exert some influence but for which habit-forming behaviors on the part of welfare recipients and sluggish stock and flow progressions are less likely. Moffitt, and Schoeni and Blank, look at certain noncaseload outcomes, including earnings and income, in models without persistence. For these indicators—or for outcomes even less likely to be influenced by habitual behavior or sluggish adjustment, such as childbirth or training program participation—one would not expect the influence of policy to decline as much when persistence effects are added to the model through lagged dependent variables or other devices. If policy's influence remains, it may mean that the persistence models used by Ziliak et al., and others were not picking up habitual behavior or stock and flow adjustments at all—even for caseload change, where in theory they could be powerful. Such a result would shift the balance of

evidence toward studies that—in rejecting persistence effects—have already shown waivers to be associated with caseload declines.

It is also critically important to do more research on caseloads in the later 1990s, once TANF came on board. Here, again, a crucial gap concerns models that allow for sluggish caseload adjustment. All the evidence on TANF reviewed here comes from models that omit potential persistence effects. Those studies show that TANF exerted a more powerful downward influence on caseloads in 1997 and 1998 than did state waiver reforms that preceded it (see table 3, above). Based on several comparable on persistence models (those of the CEA, Moffitt, and Blank and her various coauthors), estimated declines in caseloads due to waivers range from 5 to 18 percent, while those induced by TANF reach as high as 19 to 35 percent. Even if both sets of estimates are exaggerated by some margin (due, possibly, to the omission of persistence effects), the 2:1 ratio and the large absolute differential may be real.

Should this prove the case with additional analyses, the size of the contrast between waiver and TANF effects would carry major policy implications regarding TANF's origins and future. Those who see PRWORA as a way to strengthen and make more universal the message—common to many state waiver reforms—that welfare is no longer a way of life would see in this result confirmation that “nationalization” of welfare reform has accomplished an important goal. Alternatively, to onlookers fearful that TANF's time limit provisions and other features will push more families into perilous economic circumstances (beyond those already pushed by the waiver reforms), such a finding would add urgency to the need for remedial action as the number of ex-recipient families in economic peril rises. Depending on which view prevails, TANF's reforms

will be either extended or undone in the upcoming Congressional debate on reauthorization of PRWORA for 2002. The rhetoric may be more muted if further research shows TANF not to have surpassed waivers to such a degree in its caseload reduction effects.

Further Considerations. Even those authors who report a significant relationship between implementation of waiver reforms or TANF and caseload declines caution against interpreting this correlation as “cause and effect.” In addition to the issues already discussed, their caution arises from recognition that a variety of other forces accompanied adoption of a waiver in certain states—forces that themselves may have affected caseloads without entering the analysis, leaving all “credit” for the reductions to the waivers. These spurious correlations are possible because states adopting waiver reforms often differed from other states on factors such as the strength of a state's pre-PRWORA education and training system or the resources available for work-support services such as transportation and child care. Some of these differences will have been removed in the analysis—especially those that do not change over time—though some will remain.²⁰ It is possible that remaining differences would have led states adopting waivers to experience larger declines in caseloads than other states anyway, even in the absence of reform, because their education and training systems were being revamped more aggressively at that time or because they had a larger pool of new employment support resources to draw on.

²⁰ Although the models control for state fixed effects—that is, they incorporate a single term for each state to attempt to explain differences across states—this technique only controls for those factors in a state that remain fixed over time. The time-fixed effects included in most models only account for those factors that change over time equally in all states.

The very forces that drove caseload decline in the first place also may have led to adoption of policy reforms at a particular time in a particular state. This would be the case, for example, if—as suggested by the original CEA report—unusually large increases in AFDC rolls in the prewaiver period both prompted reform measures out of concern for the problem and triggered a natural readjustment of caseloads to more typical levels independently of policy.²¹ Other examples are offered in the literature in which some force external to the waivers themselves, such as efficient welfare program administration, both stimulates reform and makes caseload decline inevitable, with neither of these changes causing the other. Under these conditions, omission of the true causal mechanism from the analysis leads to false attribution of caseload decline to the reforms.

Lagged and Lead Effects. Considerable attention has also been given to measuring whether welfare recipients respond to the *anticipation* of new policy changes, in addition to reacting to new policies once legislated or implemented. To test this hypothesis, some researchers have put two sets of waiver variables in their models. One version is the standard set of variables indicating the year in which waiver provisions were approved or began to be implemented (and all subsequent years). Another set demarks the year *prior* to waiver approval or implementation, to capture “lead” or anticipation effects. Three of these papers (CEA 1997, Blank (1997), CEA 1999) find a statistically significant caseload reduction the year before waiver approval or (for CEA 1999) implementation. This effect is maintained or increases in later years. The fourth

²¹ In fact, CEA (1997) reports that the waiver states did *not* experience greater-than-average increases in welfare participation between 1989 and 1993. But the principle still holds—exogenous factors may have influenced both waiver policy choices and caseload size independently of policy.

author to look at this question (Moffitt) finds no significant waiver effect in any period, although coefficients are negative on both lead and contemporaneous indicators of waiver approval, consistent with the significant reduction found when using just a single (contemporaneous) waiver indicator.

If waivers are posited to have a consistent influence in all years—that is, if just a single indicator variable is used—CEA 1999 finds that the effect of waivers is somewhat larger when estimated beginning in the year of waiver *approval* than when estimated beginning in the year of waiver implementation.²² This also suggests that the influence of reform begins *prior* to implementation. In looking separately at the PRWORA reforms, CEA 1999 again finds significant caseload reductions in the year prior to implementation. As with waivers, these reductions grow larger once reform implementation begins.

These results seem to make the case that individuals respond in anticipation of waiver policies that have not yet been implemented, or perhaps not even approved. An advance reaction could occur, for example, if some welfare recipients leave the rolls in anticipation of tougher work requirements or time limits about to be enacted. Alternatively, anticipatory effects could stem from changes in how welfare program administrators interact with recipients (and potential applicants) once they realize that more work-oriented policy changes are on their way. There are a number of reasons to question these theories, however. For instance, the findings on policy lead effects cited above come from models that do not allow for sluggishness of caseload reactions to new conditions. It is possible that apparent anticipatory influences of various reforms are the vestige of some prior downward “shock” to caseloads that is still working its way through

²² This information appears in footnote 9 on page 14 of CEA '99.

the system in the year prior to waiver reform. Ziliak et al. test this theory by examining lead effects in the presence of persistence and find that it does not hold up: lead effects remain significant and negative. Somewhat surprisingly, they are largely canceled out once waivers are actually approved, leaving little effect in the year of approval.²³ It is hard to understand why models incorporating persistence would show significant reductions in caseloads the year *prior* to waiver approval but not subsequently—in other words, why initial progress toward smaller caseloads engendered by the prospect of reform would *go away* once the reforms are actually adopted. This odd pattern casts doubt on both the notion of anticipatory effects and—at some level—on persistence models more generally.

These are not the only reasons that researchers caution against placing too much faith in measured anticipation effects. Moffitt (1999) warns that states applying for waivers may be systematically different from other states, with initial caseload decline *precipitating* later waiver adoption. Unless welfare recipients or welfare administrators are reacting to anticipated policy changes a year or more ahead of waiver implementation, it is not the reforms themselves that cause the decline but some outside factor influencing both. One possible mechanism is offered by Martini and Wiseman (1997) in their critique of the original CEA report: states whose caseloads have just declined to an unanticipated degree (or increased less than expected) may have slack resources and, thus, have more ability to design and press forward with waiver initiatives.

²³ Ziliak et al. also confirm the CEA 1997 results on “lead” effects when they use the CEA model (column 2 of their table 7). The more important findings are those from the model where “persistence effects” also play a role through first-differencing (column 4 of their table 7); these are the results cited in the text.

Further empirical evidence of this possibility is found in the models of caseload persistence discussed earlier. As already noted, when the previous year's caseload is added to the model to allow for sluggish caseload adjustment as conditions change, Ziliak et al. find that the measured negative effects of waivers disappear. Something else is picking up that downward influence, something associated with the arrival of waivers (and, hence, capable of “claiming” the same downward influence previously ascribed to waivers). This can only be the single new factor in the model—the previous year's caseload, which has a negative coefficient. Lower prior caseloads are acting in place of waiver adoption to explain the unusually low current caseload. For this to happen, prior caseloads must be negatively correlated with waivers—that is, states with unexpectedly low caseloads in one year were more likely to adopt reforms in the next. The “lead” variable for waiver adoption, which begins tracking waiver effects in the year prior to approval, perhaps incorrectly interprets this lower initial caseload level as an anticipatory effect of the reforms. In fact, its true cause may lie elsewhere, possibly in unmeasured events that *preceded both years* of interest.

Another question of importance concerns the lag needed for policy reforms to exert their full influence following approval or the start of implementation. Three papers model this factor (which is different than general persistence²⁴): Ziliak et al. and Moffitt look at lags following waiver approval, while CEA 1999 examines lags following the start of implementation. In monthly data, Ziliak et al. find that waiver effects grow over time following approval, although in annual data they do not. This suggests that

²⁴ General persistence is just that—general, and hence present every time some external factor acts on caseloads to move them up or down. Lagged responses to policy changes arise from a specific kind of

implementation of policy changes and the influence of those changes on caseloads—including their contribution to overcoming the inertia of persistence effects—evolve fairly quickly following approval. Moffitt, however, finds growing effects of policy from year to year.²⁵ Starting from the beginning of implementation rather than approval, CEA 1999 obtains mixed results regarding the evolution of waiver effects from year to year.²⁶ Additional analyses are needed to resolve this question.

Estimating Effects of Individual State Policies. Researchers have also explored caseload decline from the standpoint of the individual reform measures that might have played a role in overall waiver impacts, measures such as time limits, family caps, and work sanctions. Unfortunately, the results from these analyses are even less consistent than the findings on waiver effects overall. Very few studies agree on the impacts of individual policies; table 6 summarizes their disparate findings.

The policies examined include three reforms designed to increase work effort and, either directly or indirectly, reduce caseloads:

external “trigger”—a new policy—and do not show up in the data immediately after another kind of impetus such as a shift in teen childbearing or a major rise in the unemployment rate.

²⁵ This information appears in footnote 12 on page 114 of Moffitt.

²⁶ This information appears in footnote 17 on page 22 of CEA 1999.

Table 6

The Effect of Individual Welfare Reform Provisions on AFDC/TANF

Study	Time Period Analyzed	Predicted Percentage Change in Caseload Due to:				
		Added Time Limit	Added Family Cap	Increased Work Sanctions	Increased Earnings Disregard	Narrower Work Exemptions
CEA 97	1976-96	Benefits -6.37 ^a Work 2.86	-.49	-9.69 ^a	.11	2.64
Ziliak et al. 00	1987-96	Benefits -.566 ^a	.161	Combined with narrower work exemptions	.434 ^a	-.344
Blank 97 (AFDC-Basic only)	1977-95	Benefits 4.5 Work -5.9	-17.9 ^a	11.1 ^a	-3.6	-10.4 ^a
Figlio & Ziliak 99	1976-96	na	na	na	na	na
Bartik & Eberts 99	1984-96	na	na	na	na	na
Wallace & Blank 99	1980-96	na	na	na	na	na
	1980-98	na	na	na	na	na
Moffitt 99	1977-95	Benefits -6.79 Work -9.211 ^a	-10.58 ^a	-2.043	-4.569	5.733
	"	na	-	-	-	-
	1977-95 (CPS)	na	na	na	na	na
	1981-95	na	na	na	na	na
	1987-95	na	na	na	na	na
CEA 99	1976-98	Benefits or work -3.75	6.71 ^a	Partial -9.71 ^a Partial→full -18.14 ^a Full -39.36 ^a	+ ^a (\$)	Exempt if have . . . child < 4 yr. 12.37 ^a child < 6 mo. 11.56 no child exemptions 4.86
Schoeni & Blank 00	1976-98	na	na	na	na	na

* Estimates are from authors' preferred specifications. Policies include statewide or nearly statewide waivers and (for analyses extending beyond 1996) statewide TANF policies.

(\$) Earnings disregard modeled as a continuous variable → coefficient not comparable to other studies; effect is positive and significant.

^a Statistically significant at at least the 10% level.

- Narrower Work Exemptions — increases in the proportion of the caseload required to participate in work activities.
- Increased Work Sanctions — reduction or elimination of benefits for families that do not comply with work requirements.
- Time Limit Added — limits on the number of months or years a family can (a) receive benefits or (b) remain on the rolls without meeting work requirements.

Most of the coefficients on these policies are indeed negative, though only time limits on benefits and increased work sanctions were found by the majority of authors to have significantly reduced caseloads. Together with these expected results, some unexpectedly significant findings emerged. Blank (1997) attributed *higher* caseloads to increased work sanctions, while CEA 1999 associated narrower work exemptions with the same result.

Another work incentive provision is expected to increase caseloads by allowing recipients with higher earnings to stay on the rolls:

- Increased Earnings Disregard — increases in the amount of benefits a family is allowed to keep when it has earnings.
- Two of five studies produced this result to a statistically significant degree, and two others show the expected positive sign.

The final policy measure in the table may not affect caseload size at all:

- Family Caps Added — elimination of benefit increases for children conceived and born while a family is on welfare.

It is possible, however, that family caps reduced caseloads by encouraging work, if they made public assistance a less attractive option for low-income parents in expanding families—or if they reduced fertility and, hence, left fewer parents struggling to work while raising multiple children. There is no offsetting theory of family caps *increasing* welfare participation. Yet, the data show significant relationships in both directions.

Two questions arise from these results. First, can we conclude from this evidence that one or more specific reform measure contributed to the caseload decline of the mid-1990s? If not, why did the analyses fail to produce any clear conclusions?

As in the analysis of “any welfare reform,” what one concludes about individual policies depends on how one deals with the possibility of persistence effects caused by sluggish response to new conditions. Models that assume that welfare participation behaviors tend to persist as circumstances change incorporate persistence effects and tell a simple story with regard to the individual reform provisions: Benefit time limits significantly reduced caseloads in the waiver era, while larger earnings disregards increased them (Ziliak et al.). In part, this story is “clean” because to date only one persistence model has been applied to data on individual reform measures. But it is also true that this model produces no perverse, statistically significant effects, unlike two of the four nonpersistence models (Blank (1997), CEA 1999). Clearly, more research in this area is needed.

If one rejects the hypothesis of persistence, the three nonpersistence models estimated over the waiver period (CEA 1997; Blank 1997; Moffitt) also tell a fairly consistent, sensible story in terms of statistically significant findings. Work requirements, sanctions, time limits, and family caps all reduce caseloads in at least one of these papers, with only the sanction result offset by a counterexample (in Blank 1997). Of course, if these papers exaggerate overall waivers impacts because persistence does exist, it is likely that at least some of the significant effects they find for individual policies are illusory. Extending the same analysis model into the TANF era, CEA 1999 obtains a very different set of significant results—and a set with as many perverse effects (on exemptions and family caps) as expected effects (on sanctions and disregards). Surprisingly, both of these perverse findings indicate caseload *growth* because of a particular policy, even though the CEA 1999 model overall shows a decline.

On net, the analysis of individual reform measures tells us little on which we can depend. The general lack of statistical significance—and the conflicting directions of influence found among significant variables—is not that surprising, given the difficulties of caseload modeling. Other factors impinge here for the first time. First, it is very difficult to disentangle the effects of one policy from another in any assessment of welfare reform, including highly structured social experiments. In almost any welfare reform package, certain policies are closely related both in how they are implemented and how they affect (or are expected to affect) recipient behavior. Because they move in lockstep, separating their influences is—in the extreme case—statistically impossible and in practice very difficult.

Second, it is unclear how some policies should be expected to affect participation. For example, family cap laws may discourage those anticipating large families from applying for welfare but may not have the same effect on families already on welfare. Likewise, increased earnings disregards will undoubtedly keep some people on welfare longer as their earnings begin to rise, but may later lead to caseload decline as recipients gain enough work experience to become self-sufficient even at a higher eligibility threshold. Third, some of the policies tested may not be binding in the time period studied. For example, it is possible that a state with a five-year time limit on benefits may not see large impacts from that provision until recipients approach the limit. Finally, as discussed earlier, none of these models control for all the other factors that may be changing in the state at the same time as policy. As a result, very little can be concluded about the impact of individual policies on caseload decline from this set of studies.²⁷

How Much Did the Economy Matter?

A good measure of the relationship between caseloads and economic conditions can provide valuable information to policymakers on its own. First, it allows for better predictions on caseload size under different economic conditions, which leads in turn to more accurate fiscal planning. This capability is particularly important in relation to the TANF “rainy day fund” provision (the portion of the federal block grant designated for state use if caseloads and costs rise during a future economic downturn). Second,

²⁷ Individual state waiver evaluations do a better job at this, having the advantage of randomized designs in many cases (e.g., matched sets of families, one subject to a particular policy provision and the other not), though even those studies often combine multiple reforms into a single “package” that makes separate reforms difficult to untangle. See Finegold and Scheuren (2000) for a recent discussion of this point, made previously by a number of other observers, most prominently Robinson Hollister.

understanding how business cycles affect the welfare and “near-welfare” population will enable policymakers to better evaluate future policies to spur employment and economic growth. In particular, understanding which types of economic growth reach the welfare and near-welfare populations would be valuable information for policy and tax reform debates on how to increase the economic well-being of individuals at various levels of the income distribution. More broadly, the relationship between economic health and the size of the welfare population gives an important indication of the degree to which “a rising tide lifts all boats” in a robust economy—and, conversely, how much of the most vulnerable population is stranded as the tide recedes. It is particularly important for policymakers and planners to understand lags between economic changes and their caseload repercussions. Finally, depending on one's optimism regarding the twenty-first century economy, knowing how economic success carries over to lessened dependence gives some idea of how likely the gains in family self-sufficiency seen during the last six years are to continue over the next six.

The results from this set of studies show the economy to be closely tied to caseload size, and a major contributor to recent declines in dependence. Most authors used annual state-level unemployment rates as their measure of economic health, with lower rates signifying a stronger economy. Some explored other measures in addition (e.g., Bartik and Eberts), while two papers did not look at economic effects at all or only over a very short follow-up period. As shown in table 7, all seven papers that explored long-run economic effects found that reductions in the unemployment rate lead to lower welfare caseloads. Findings were consistent as well in indicating that an important component of the effect is lagged one or more years beyond the actual change in

Table 7

The Effect of Changes in the Unemployment Rate on AFDC/TANF Caseload Size*

Study	Time Period	Predicted Percentage Change in Caseload with a Sustained 1 Percentage Point Reduction in the Unemployment Rate			
		Current year	1 year lag	2 year lag	3 year cumulative effect **
CEA 97	1976-96	.77 ^a	-4.97 ^a	na	-4.02 ^b (2 years)
Blank 97 (AFDC-Basic only)	1977-95	-.7	-1.4 ^a	-1.7 ^a	-3.8 ^b
Figlio & Ziliak 99	1976-96	na	na	na	-5.9 ^b (4 years)
Bartik & Eberts 99	1984-96	na	na	na	-6.49 ^a (4 years)
Wallace & Blank 99	1980-96	-1.5 ^a	-2.2 ^a	-2.3 ^a	-6.0 ^b
	1980-98	na	na	na	-4.0 ^a (2 years)
Moffitt 99	1977-95	-.031	-4.334 ^a	na	-4.365 ^b (2 years)
	1977-95 (CPS)	.512	-4.672 ^a	na	-4.160 ^b (2 years)
	1981-95	.745 ^a	-4.500 ^a	na	-3.755 ^b (2 years)
	1987-95	-.031	-2.220 ^a	na	-2.251 ^b (2 years)
CEA 99	1976-98	.36	-1.50 ^a	-4.27 ^a	-5.41 ^b

* Estimates are from authors' preferred specifications. Ziliak et al. report only very short-term (2 months) economic effects. Schoeni & Blank do not report economic effects.

** Assumes 1% decline in unemployment is sustained over 3 years (or other indicated period); sum of three preceding columns.

^a Statistically significant at at least the 10% level.

^b Test of statistical significance not run.

economic conditions. Moreover, the magnitude of effect over several years of improved economic conditions differs remarkably little from study to study. In all cases, a sustained 1 percentage point decrease in the unemployment rate produces a 4 to 6 percent decrease in the welfare rolls by the end of the second, third, or fourth year, with longer intervals producing larger effects.²⁸

How Effects Vary over Time. The different studies tell different stories in one respect, however: the speed of response to an improved economy. The immediate change in caseload in the current year ranges from a statistically significant decrease in welfare reciprocity of 1.5 percent to a statistically significant *increase* of 0.8 percent. A year later all analysts show a decrease, both for that year and cumulatively (i.e., summing the first two columns), although magnitudes still vary considerably for both the year-1 effect alone and the cumulative effect of two years of an improved economy. Ultimately, however, there is little disagreement on how strongly a sustained reduction in the unemployment contributes to caseload reductions over several years.

The differences in results that do appear have been attributed to a range of factors. The most extensively examined factor concerns the time frame covered by the analysis. As can be seen in table 7, researchers used somewhat different historical time periods in their models, some starting as early as 1976 and other as late as 1987, and ending variously with 1995, 1996, or 1998 data. While critical for understanding policy effects due to TANF, the end point of the analysis does not seem to make much difference to estimates of the economy's role in determining caseload size. The one fully comparable

²⁸ A temporary reduction in unemployment does not have such a large effect but still ripples through the system for two or more years, according to all three studies that looked separately at each of the first two years following a change (Blank (1997), Wallace and Blank, and CEA 1999).

finding incorporating post-TANF data for 1997–98—CEA 1999—falls in roughly the middle of the distribution of measures of the long-run impact unemployment on caseload size.²⁹ In analyses of pre-TANF data, omission or inclusion of 1996 has been shown to make almost no difference to the measured effect of unemployment on caseloads (Figlio and Ziliak, Moffitt). These findings are not surprising: Because steady declines in unemployment and the arrival of waivers went hand in hand in the mid-1990s, most of the measured influence of the economy on caseloads independently of policy reform is determined by data from the prewaiver period.

The relationship of welfare dependence to the state of the economy prior to 1993 was not entirely steady. Two full business cycles—growth, then recession, followed by growth, then recession—occurred between 1976 and 1992. Moffitt describes how the AFDC caseload moved in different ways relative to the economy at different points in this interval. From 1976 to 1979, and again after 1987, falling unemployment was accompanied by falling caseloads, as expected; similarly, caseloads rose at the beginning of the 1980–81 recession. This “normal” story was reversed—or at least nullified—from 1982 to 1987, however, as caseloads *fell* despite a deepening recession in the early 1980s and then rose or held constant over the first few years of the subsequent recovery. Even once the pattern returned to normal beginning in 1998, shifts were evident from earlier cycles; Moffitt points out that the recession of 1990–92 had an exceptionally strong upward effect on caseloads compared to earlier cycles.

²⁹ Wallace and Blank study the post-TANF period using monthly data, for which long-run results are not available and might not be comparable to annual findings from other studies. Schoeni and Blank do not report any findings on the influence of the economy.

How much does the selection of historical time periods matter to the findings? Moffitt shows that it makes some, but not a great deal of, difference which economic cycles enter the calculations. Omitting the unusual counter-cyclical movement of caseloads in the 1980s (and the preceding “normal” interval, 1977–1980) reduces his estimate of the cumulative influence of the economy by about half: rather than a 1 percentage point reduction in unemployment reducing caseloads by 4 percent, it reduces caseloads by around 2 percent. Oddly, though, the other two studies to omit portions of the early years—Bartik and Eberts (starting in 1984) and Wallace and Blank (starting in 1980)—produce the largest reported estimates of cumulative economic effects, 6 percent or more. Shorter-term findings across the three studies are also quite inconsistent: Wallace and Blank show significant current-year declines in the rolls as unemployment falls while Moffitt shows a significant increase (when analyzing 1981–95) or no change (when analyzing 1987–95). In also looking at economic responses by time period, CEA 1999 is at times consistent with Moffitt and times not.

These facts leave some question as to the appropriate time period for studying caseloads from an economic perspective. Moffitt provides two perspectives: that all business cycles should be included to incorporate as much historical information as possible, and that only the most current cycles are relevant, given the large structural changes that have occurred in the labor markets since the late 1970s that render the patterns in older business cycles obsolete. Ironically, this choice carries larger

implications for measures of the effect of welfare reform than for measures of the effect of the economy.³⁰

Other Sources of Variation. Other variations in economic results are due to the use of monthly versus annual data and the choice of time trends and lag structures in the models. Ziliak et al. and Figlio and Ziliak argue that aggregation of caseload data into annual measures leads to a loss of important variation found in monthly caseload changes. Almost all authors explore the implications of assuming longer or shorter lags between changes in economic conditions and caseload adjustments. Wallace and Blank find that differences also emerge when researchers choose one form of time trend over another in modeling secular changes in caseload size. Unfortunately, there is little agreement on the appropriate data or method to use in any of these cases. The good news, for now, is that findings on the long-run contribution of the economy to caseload change vary little with the range of options examined. The economy matters—and by a consistent amount—no matter how one looks at it.

As a final point on the influence of the economy, it is notable that all authors used state level unemployment rates as a primary—and usually the only—indicator of how the economy might affect the employment and self-sufficiency of potential welfare recipients. This may not be the most appropriate measure of the effects of the economy at the lower end of the income distribution. Some argue that unemployment rates for those labor sectors where welfare recipients and low-skilled workers are likely to find

³⁰ Recall from an earlier footnote that the negative effect of waivers on caseloads disappeared when Moffitt confined his analysis to 1981 and beyond and reversed itself—i.e., showed waivers to have *increased* caseloads—when analyzing only 1987 and beyond. CEA 1999, on the other hand, finds that waivers reduce caseloads regardless of how uniformly or distinctly past business cycles are handled.

jobs should be used instead of the general unemployment rate. In addition, state-level unemployment rates may not represent the situation facing low-skilled workers, who may be clustered in local communities with unemployment rates much different than the state average. Others suggest that alternative economic controls should be incorporated, such as measures for job growth and the skill mix for industries in a given area.

Among the reviewed studies, Bartik and Eberts make the strongest case for alternative measures of labor demand for welfare recipients. They alone explore several economic measures in depth; in addition to state unemployment rate, these include employment growth, share of jobs likely available to welfare recipients based on industry mix, share of jobs requiring no more than a high school education, and the overall wage level in the economy relative to the mix of jobs.³¹ All these variables are found to significantly influence caseloads, though not always in the expected direction (e.g., a shift in industrial mix toward industries that tend to employ welfare recipients is estimated to increase welfare rolls, perhaps because of heightened job instability). Collectively, they add to our ability to explain the surprisingly limited response of caseloads to a growing economy in the late 1980s—when the demand for less-skilled workers was declining in relative terms—but not the drop in caseloads in the 1990s.

Bartik and Eberts also find that growth in total employment may be as important in reducing caseloads as reductions in the unemployment rate *per se*. While the two normally go hand in hand, they need not in all cases. For example, unemployment might fall due to unusually high out-migration or labor force exit (e.g., retirement) without any

³¹ Ziliak et al. (1997) used employment per capita as an alternative economic indicator but limited the influence of economic factors to just 12 months. Subsequent papers finding large lagged effects of the economy over several years limit the relevance of those results, however.

net increase in the total number of jobs (or even a decline). Should this occur, Bartik and Eberts warn that welfare rolls are likely to respond quite differently—and, in this case, far less favorably—than has been true in the past.

Interactions between the Economy and Policy. Policy reforms may also exert a different influence under different economic conditions. All the findings on the role of waiver and TANF policies reviewed so far treat the two influences as separate; the adoption of new policies is assumed to have a uniform effect on the caseload under any economic conditions. If, instead, reforms push caseloads down more in a strong economy, the economic and policy trends of the 1990s fed off each other and—more ominously—the reductions achieved in welfare reciprocity may retreat more quickly in a future recession than otherwise expected. Not only would a weakening economy push up caseloads in its own right, some of the contribution of the waiver and TANF reforms to caseload reduction that we are seeing now would evaporate. Alternatively, reforms might be more effective in a weak economy by, say, slowing the influx of economically marginal families onto the rolls. If the interaction works in that direction, the contribution of welfare reform to caseload reduction may be even larger in the next recession than it has been to date.

Clearly, this is an important and complex question. Research on this topic is still fairly limited—it is confined to the waiver era, for example, and has not yet looked at how TANF interacts with the economy. Given all the other challenges in modeling caseload change, second-level interaction effects between the economy and policy may be particularly difficult to sort out. Four of the reviewed studies (plus one other not reviewed) have explored the possibility that the effects of welfare reform on dependency

vary with the economy. Of the five, only one—Figlio and Ziliak—finds waivers to have had different effects in the times and places where economic conditions were more favorable.³² As the only researchers to consider this question using national data and allowing for sluggish caseload adjustment, Figlio and Ziliak conclude that the mid-1990s waiver reforms did more to reduce caseloads in strong economies.

By contrast, CEA 1997 and one additional study not reviewed here (Levine and Whitmore, 1998) find no relationship between the economy and the size of waiver effects.³³ Nor do Bartik and Eberts, using a model similar to that of Figlio and Ziliak but data for just one state (Michigan). The inclusion or omission of sluggish caseload adjustment may again be a telling factor in this regard. No study to date has suggested that reforms work better in a weak economy, making it unlikely that recent policy changes will mitigate caseload growth in the next recession. Just the opposite is more likely, if models incorporating sluggish caseload adjustment are the most trustworthy. Clearly, more research in this area is needed, especially an examination of policy and economic interactions under TANF beginning in 1977.

What Do We Know about the Relative Importance of Policy versus the Economy?

Based on the results presented thus far, is it possible to draw strong conclusions about the comparative contributions of welfare policy and the economy to recent caseload

³² In their preferred model, Figlio and Ziliak find that the approval of a waiver reform had essentially no effect on caseload when unemployment was very low (around 2 percent) but *increased* the caseload by 6 percent when unemployment was high (around 8 percent). In another model, waivers reduced caseloads by 6 percent when unemployment was low but increased caseloads by 3 percent when unemployment was high.

³³ The CEA result appears in footnote 20, page 17, CEA 1997.

declines? Policymakers yearn for this type of information, and quotes about the relative influence of the two factors—in the mode of “who should get credit?”—have made it into the popular press. Yet, as we have seen, the story is not so simple and continues to be unclear in major respects.

Still, some things can be said with confidence. We can be certain that the strong economy played a role in the caseload declines of the mid- to late 1990s. We also know that its influence was substantial over a sustained period of economic growth, though some of that influence occurred with a lag. The strengthening economy may also have made it easier to achieve caseload reductions through policy reform, though this is much less certain. It is also clear that, in a strictly descriptive sense, states that adopted waiver reform policies in the mid-1990s had larger caseload declines than states that did not—and that implementation of the TANF reforms in 1997 and 1998 was accompanied by still further declines. Causality in this relationship is questionable, however, and some researchers find that even the correspondence in timing goes away once adjustment is made for changing economic conditions and the potential for sluggish caseload response.

Even if causality and sluggish adjustments were not issues, the results summarized thus far do not tell us which of the two influences—reform policies or economic growth—did the most to lower dependency in the 1990s. The answer to this question depends on more than just the responsiveness of the caseload to specific reform measures or to changes in the unemployment rate. To understand how we got from historically high to stunningly lower caseload levels in just five years, we must also consider *how many* states adopted reforms between 1993 and 1998 and *how much* the economy improved during that interval.

The question of “reform versus the economy” holds great interest to policymakers for several reasons. First, understanding the relative importance of reforms and the economy may provide some information on whether families are coming out ahead in the transition off welfare—a more likely result if a strong economy is leading to their independence than if policies such as stronger work sanctions and benefit time limits made the largest contributions. In addition, caseload declines attributed to welfare reform have a greater chance of permanency than those driven by the economy—assuming that their effects hold up in a weakening economy, an as-yet-untested proposition. The architects of welfare reform sought to permanently change the culture of welfare, moving from an income support philosophy to a strong work orientation, reducing *lifelong* reliance of public assistance. For that to have been accomplished, progress to date must hold up in less robust economic times—that is, it must be driven by policy changes more than economic growth..

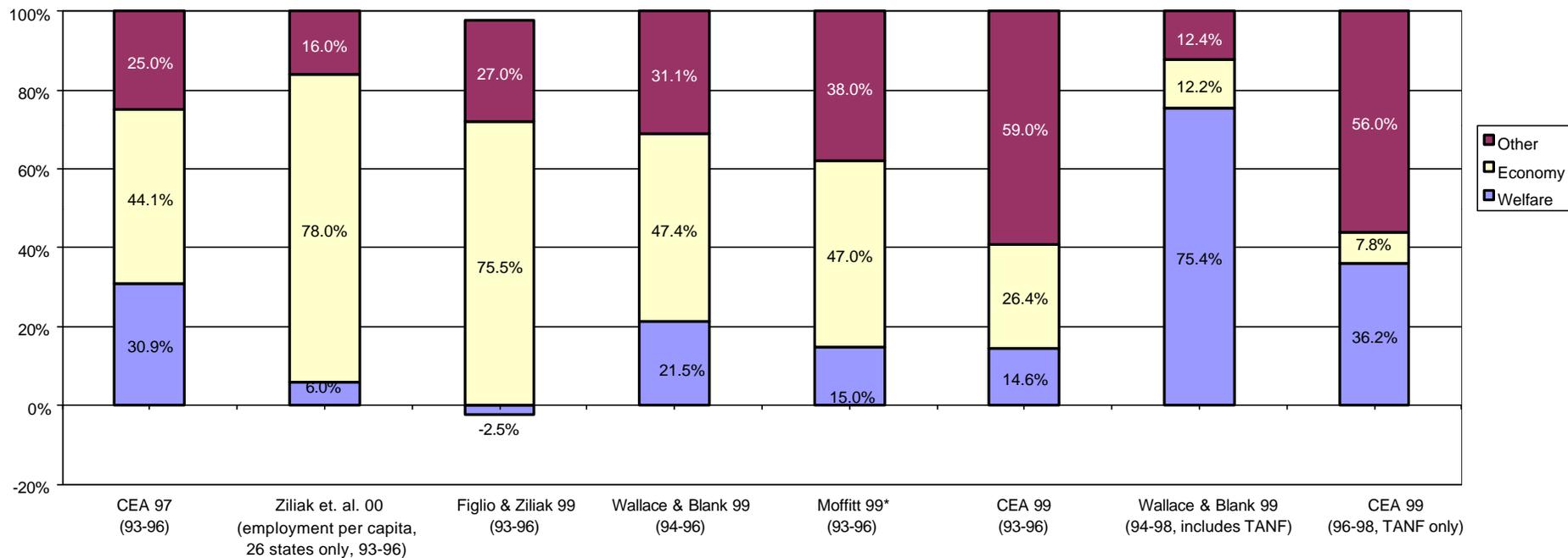
This crucial difference—the prospect of transient versus permanent change—has major implications for disadvantaged families, state and federal budgets, and for the nation's anti-poverty policy in general. If it is primarily the economy that moves families out of dependence and into work—and makes certain reform measures effective—future anti-poverty efforts should focus on macroeconomic growth and targeted jobs programs such as local enterprise development. Alternatively, if welfare program rules are the decisive factor, a continuation or even an extension of recent reforms may make the most sense as the way to insure continued reductions in dependency.

The distinction between policy and the economy also has implications in fiscal matters, affecting the allocation of funds between current and future budgets. For

example, a permanent drop in caseloads due to welfare reform implies a freeing-up of resources for other uses for many years. In contrast, any fiscal freedom gained because welfare rolls shrank in response to the economy could be temporary. Such a “windfall” should not automatically be made available to other short-term uses in either state or federal budgets. As with the federal “rainy day” funds provided under the TANF block grants, a more prudent course would be to save at least some of this “surplus” to fund caseload *increases* in the future—in other words, in the next recession. However, we would not expect to see this sort of budget reversal, or a need to save current windfalls for future use, if policy reforms caused most of the recent caseload decline. Were this known to be the case, policymakers could safely invest surplus funds in other uses (or in tax cuts).

Several of the studies reviewed address these issues by calculating the percentage of caseload decline between 1993 and 1996 (or 1998) attributable to the economy, to policy, and to other factors. While there is no assurance that future changes will mimic the patterns of the past—particularly with regard to policy changes that, unlike economic measures, have only recently begun to be tested and to be tested in just one direction (toward *greater* emphasis on work)—the findings of the last decade form the best basis for extrapolating the future. Figure 1 shows how the two factors compare for the studies that considered their relative contributions during the 1990s.

FIGURE 1
Relative Importance of Welfare Reforms, the Economy, and Other Factors
in Explaining AFDC/TANF Caseload Declines from 1993 to 1998



The reference point here is the breakdown of caseload decline into its economic and policy components provided by the first CEA study. CEA 1997 reports that from 1993 to 1996 the economy accounted for around 40 percent of all caseload reduction and welfare reform for another 30 percent, leaving the rest unexplained and, presumably, due to other causes. This relatively balanced decomposition shifts sharply, however, when allowance is made for the possibility of sluggish caseload adjustment to new external conditions. Using persistence models, Ziliak et al. and Figlio and Ziliak traced three-quarters of the caseload decline between 1993 and 1996 to the growing economy, finding that policy played a negligible role. This contrast dovetails with results obtained by Klerman and Haider (2000) for California based on their model of persistence as a natural property of an ever-changing caseload (see above).³⁴

Both approaches likely underestimate the influence of each of the factors—particularly that of the economy—by apportioning a component of each state's yearly caseload change to broad national shifts and a preexisting state-specific trend factor.³⁵ Such elements subsume some of the caseload movement actually caused by economic and (to a lesser extent) policy changes without subsequently “reinserting” that influence in calculating the contribution of specific factors as reported in Figure 1.³⁶

³⁴ Specifically, Klerman and Haider find that county unemployment rate explains 21 percent of California's caseload decline between 1994 to 1998 when persistence is left out of the model and 62 percent when modeled properly.

³⁵ The author is indebted to Robert Schoeni for making this point (personal correspondence via e-mail, October 23, 2000). Since economic measures such as the unemployment rate tend to move more continuously with time, their influence is more likely to be usurped by other time-trend and shift terms than is the influence of more discrete, one-time changes in policy.

³⁶ To be fair to the authors, there is no way to separate out the role of the economy or policy once it is encapsulated in the broader trend components, since it is only the portion of that influence that does *not* move distinctly from other underlying factors that gets “swallowed up” in the national and state trends to begin with. One cannot simply omit the collective trend components from the model without creating even larger problems as unmeasured nonpolicy, noneconomic factors with a trend component interfere with reliable measurement of all relationships in the data. These two problems are known as “multi-collinearity”

The other three papers that decomposed caseload decline over this interval³⁷ tend to agree with the CEA 1997 conclusions that put policy and the economy in a more equal balance—a not surprising result since, like CEA 1997, these other papers did not include “persistence effects” in their models. Two of these papers—Wallace and Blank, and Moffitt—attribute somewhat less than half of the decline in caseloads to falling unemployment, and 15 to 20 percent to the waiver reforms. CEA 1999 credits the economy with just a quarter of the decline during the waiver period, apportions another 15 percent to welfare reform, and leaves a whopping 60 percent unexplained.³⁸ The other five studies of the waiver era left 15 to 30 percent of the decline unexplained, an issue to which we shall return shortly.

Two papers move beyond the waiver era and seek explanations for the declines in caseloads that took place during the first two TANF years. These authors examine the trend in caseloads from 1994–98 (Wallace and Blank) and from 1996–98 (CEA 1999). The two papers differ strikingly in their conclusions. Wallace and Blank attribute far more of the drop in the rolls between 1994 and 1998 to policy reforms than any other analysis, fully 75 percent. CEA 1999, on the other hand, estimates policy to have made an important but more modest contribution in that time, explaining 36 percent of the

and "omitted variable bias," respectively, in the econometric literature and are present to some degree in almost any analysis of complex relationships.

³⁷ Neither Blank (1997) nor Bartik and Eberts provides a decompositional analysis in their examination of the waiver years. Nor do Schoeni and Blank when looking at the TANF era.

³⁸ The inability of CEA 1999 to attribute a major share of caseload decline to policy would seem to conflict with the large percentage by which waiver reforms reduced caseloads in that study (see table 3). Recall from table 4 that CEA 1999 began measuring effects at waiver *implementation* rather than at waiver approval as in all the other studies included in figure 1. Because implementation lagged approval, the large implementation effect estimated by CEA 1999 had less time to contribute to caseload decline between 1993 and 1996 than the smaller application effects estimated by other authors.

decline, and—like the same study's conclusions regarding the waiver period—leaves the majority of decline unexplained.

Despite these differences, both post-TANF studies attribute much more of the drop in dependence to policies than to the economy. Whereas the waiver-era analyses placed the economy above policy by a ratio of at least 1.8 to 1, these analyses showed policy outstripping the economy in explaining caseload decline by the late 1990s, by a ratio of at least 4 to 1. Wallace and Blank's 6-to-1 policy:economy ratio is particularly surprising since the period to be accounted for, 1994–98, overlaps the waiver interval in which they found policy to have barely half as much influence as the economy. The authors warn sharply that their approach exaggerates the role of policy in the TANF era by attributing *all* otherwise unexplained changes in caseloads after 1996 to TANF, a feature of the analysis that also accounts for the unusually low percentage of the 1994–98 decline left unexplained. The CEA 1999 results, which do not impose this assumption, provide a more reasonable indication of the apparently growing influence of policy following national reform—from explaining 15 percent of the waiver-era decline (1993–96) to explaining 36 percent of the early TANF results (1996–98). But even here, there is reason to be skeptical of the apparent influence of policy factors, given that in three separate studies the addition of persistence effects removes all measured influences of policy during the waiver era. So although it seems likely that the application of persistence models to the early TANF years would show policy to have had a greater influence than under waivers (i.e., to have some rather than no influence), this is not assured. While it will take more research to resolve this question, the odds are that—if

caseloads really do react sluggishly to external change for the reasons discussed earlier—TANF had a more limited role in the caseload reductions that continued into the late 1990s than the set of currently available results suggest.

Further concern about the reliability of current models stems from the fact that they all fail to explain a sizable proportion of caseload decline, both pre- and post-TANF. This is the portion attributed in figure 1 to “other factors.” This catch-all category includes from 16 to 59 percent of observed caseload changes, some of which certainly have economic or policy origins that do not show up once broad state and national trends enter the analysis (see above). It may also be that the specific economic and policy variables used by the various analysts are less than one-hundred-percent successful in reflecting the full influence that does exist independently of broader trends. In a nutshell, the mere existence of a large unexplained component calls into the question the measured magnitudes of what has been explained.

This large, unexplained component of caseload change has led many researchers to conclude that a wider set of variables needs to be considered when studying declines in dependence during the 1990s. Others (e.g., Mayer 2000) have pointed to the need for stronger *a priori* theorizing on how caseloads are determined, in order to accurately identify the causal factors missing from current analyses and the way in which each causal factor is posited to affect outcomes (e.g., its lag structure). The work of Klerman and Haider, cited earlier, goes some way toward this goal. Unfortunately, strong predictive theories are hard to formulate (from economic principles or any other starting point) for a process so complex and multidimensional; and so far there is no pattern in the

empirical results suggesting that models with a larger set of variables do a better job of explaining caseload decline.

Of the papers reviewed here, only Blank (1997) and Wallace and Blank tried to investigate why so much of the caseload fluctuation remains unexplained. In particular, they studied changes in the number of families eligible for welfare, changes in the number of eligible families who chose to receive welfare (“take-up rates”), and the recent increase in child-only cases (cases in which an eligible child lives with a noneligible adult).³⁹ They found that both eligibility and take-up rates increased in the early 1990s, and that a large percentage of reciprocity growth during that time was due to a 90 percent increase in the number of child-only cases. According to Blank (2001), eligibility fell rapidly between 1994 and 1996, accounting for 85 percent of overall caseload decline in that period. How policy and the economy contributed to eligibility and take-up as separate elements is not yet known.⁴⁰

Before closing the books on the currently available findings, it should be noted that the difficulties encountered by this set of studies when trying to explain overall welfare caseload movements are not new. Earlier studies using aggregate state data to explain caseload increases in the early 1990s found most of the increase unexplainable.⁴¹

³⁹ These are welfare cases in which the parent is not eligible for benefits for any of a variety of reasons: she or he is receiving Supplemental Security Income benefits, is an ineligible immigrant whose child was born in the U.S., has been sanctioned from the case, or is not living with the child.

⁴⁰ Blank (2001) notes that the data needed to calculate eligibility separately from participation must come from national household surveys, which are reliable at the state level for only a small minority of states (the 12 largest, in her assessment) and only since 1984. This leaves too few state-year observations with which to analyze complex relationships between the economy, policy, and welfare outcomes. Availability of data past 1996—Blank’s final year—will eventually solve this problem.

⁴¹ These studies included Gabe (1992) and the Congressional Budget Office (1993). Mayer (2000) summarizes this literature, and Stapleton, Livermore, and Tucker (1977) provide a more in-depth review and critique. As noted earlier, recent work by Blank (2001) advances this literature toward a more complete explanation of caseload increases during the early 1990s, just prior to the waiver period.

It is important to keep this in mind when considering the results of the more recent literature: while some of the declines are attributable to welfare or the economy, caseload fluctuations as a whole remain largely a mystery.

Summary, Extensions, and Implications

What can we learn from this set of studies? Recent welfare reforms that have focused on stronger work incentives, stricter work requirements, stronger penalties for lack of compliance, time-limited benefits, and family caps have been accompanied by strong caseload decline. We do not know how much of that decline was actually *caused* by the reforms, however, nor which individual policies made the most difference. We are more confident of the role of the economy in determining welfare rolls: all indications are that lower unemployment caused caseloads to decline between 1994 and 1998. Moreover, over the long run (three or four years), the extent of the economy's influence can be stated with some certainty: a sustained 1-percentage-point decrease in the unemployment rate leads to a 5 to 6 percent drop in the number of welfare-dependent families. Finally, we know that a large percentage of the caseload decline in the 1990s cannot be explained by *either* of these two factors, policy or the economy, at least not when using the methods and economic and policy variables included in the reviewed studies.

As a group, the studies highlight the difficulties of trying to disentangle policy from the economy in explaining changes in the size of the welfare population over time. They represent what one might think of as first-level analyses of the effects of a strong economy and new welfare policies on receipt of cash assistance. They are limited by the

use of aggregate state-level data in a cross-sectional, time-series framework and by the rather basic ways policies have been represented to date (as a simple “any waiver” indicator, or as indicator variables for specific policy provisions such as time limits or family caps).

Some of the most recent entries to the literature—not all of them reviewed here, in the interest of time—have begun to focus on more microeconomic analysis of individual family decisions to participate in cash assistance, examining a host of family and personal descriptors in addition to the usual aggregate policy and economic measures (Schoeni and Blank; Blank 2001). Other analysts have emphasized more refined measures of the economy and job availability for welfare recipients, and more subtle modeling of labor market dynamics (Bartik and Eberts; Black, McKinnish, and Sanders 2000). Geographically specialized studies have also come in vogue, including a look at specific metropolitan areas (Bartik and Eberts; Mueser et al. 2000) and a recent emphasis on caseload change in rural areas or specific regions of the country (Henry et al. 2000; Ziliak and Figlio 2000).

One area perhaps not receiving the attention it deserves is the development of more nuanced measures of policy and policy implementation. All the waiver-era studies encountered some difficulty interpreting often contradictory—and sometimes nonsensical—findings on individual reform measures. The problem has not been followed up in the published literature, but remains important. The future may hold more clarity in this regard as other analytic improvements are made and longer-term data become available. To resolve current mixed signals on individual policy effects, it is

critical to include possible sluggish caseload adjustment and persistence effects hypothesized by Ziliak et al. when analyzing change in the TANF era.

Analysts are also moving ahead in areas of related research.⁴² A large body of research looks beyond the question of what caused caseloads to decline to consider how families who left welfare—or who chose not to participate in the first place—have fared in an era of lessened dependence. Welfare “leaver” studies have been summarized elsewhere and continue to emerge;⁴³ federally initiated studies of state “diversion” efforts involving the redirection of TANF applicants or potential applicants are also underway.⁴⁴ In addition, some of the articles reviewed here (Moffitt; Schoeni and Blank), plus ongoing work by other scholars, are moving on to consider broader indicators of the progress of welfare reform beyond cash assistance receipt. Included in this group are analyses of the determinants of the decline in the food stamp rolls (Wallace and Blank; Figlio, Gundersen, and Ziliak, forthcoming; Henry et al. 2000; Zedlewski and Brauner, 1999) and of changes in the employment, earnings, and work hours of single mothers and high school dropouts.

One advantage of studies that look beyond caseloads is their ability to examine outcomes likely to be most directly affected by specific reform measures or sets of reform measures, instead of struggling to sort out the intersecting, often conflicting, influences of the many policy changes that can affect an amalgam indicator such as caseload size. For example, analyses of hours worked or duration of unemployment could produce clearer

⁴² See Moffitt and Ver Ploeg (1999) for a comprehensive description of the many welfare reform studies currently underway in the U.S.

⁴³ See Brauner and Loprest (1999).

⁴⁴ Moffitt and Ver Ploeg (1999), pages 71-72.

measures of the effects of policies such as an increase in the minimum hours of work required to receive family assistance or the imposition of a time limit on months one could receive food stamps without working.⁴⁵ An entirely different set of outcomes, specialized to their particular purpose, would be appropriate when studying reforms that seek to decrease nonmarital births, such as a family cap. In this instance, the critical outcome variables include the share of households headed by women and the out-of-wedlock birthrate.

The practice of extending the analysis to outcomes besides caseload has a final, and perhaps even more important, function: providing a broader social perspective on where welfare reform and the economy are taking America's low-income families. By looking at additional indicators of family well-being and labor market success, researchers can highlight that getting people off welfare is not the only—or even necessarily the most important—goal of welfare reform.

A final set of studies to shed light on the effects of welfare reform are those that use controlled experiments to examine how families or individuals respond to particular reform strategies. Many states began such studies during the waiver period as a condition for federal approval of their policy innovations, and some of these studies continue under TANF.⁴⁶ New randomized experiments are also being launched for evaluation purposes, largely by the federal government (e.g., the U. S. Department of Health and Human Services' Employment Retention and Advancement demonstration). Like their

⁴⁵ An excellent example of this type of analysis is that done by Schoeni and Blank, who use a long series of micro-economic data from the Current Population Survey to examine the effects of policy on welfare participation, employment, family structure, and poverty.

⁴⁶ For a recent discussion of ongoing waiver projects, see Rossi (2000).

predecessors, these studies cover a fairly narrow set of policy interventions but are broad-based in examining many different indicators of family well-being and labor market outcomes for the populations and policies studied. They also have the advantage of providing unambiguous inferences on causality; by assuring that the only systematic difference between randomly determined “treatment” and “control” groups is the application of some new policy to the treatment group, any significant difference in outcomes can be interpreted as the influence of reform (and not the confounding influence of participant or labor market differences that plague caseload analysis and other nonexperimental assessments of policy effects).

Clearly, there is much more to learn—and much more to be done—including continued efforts to synthesize the various strands of the literature into a single body of knowledge as more evidence emerges.⁴⁷ The contribution of state-level analyses of welfare caseloads has been considerable to date and will continue to be important. Already, these analyses have set a framework for other related research, making clear that the role of the economy must be taken into account in any assessment of welfare reform's effects. They also teach that the surest way to correctly remove economic influences to isolate policy is to provide for lag and lead effects and—potentially—to allow for persistence in behavior and the movement of caseloads over time. In addition, the work in hand illustrates the difficulties that lie ahead in attempting to attribute social outcomes to policy with any confidence and, in so doing, points to the value of applications where

⁴⁷ At least one synthesis project is currently underway, *A Research Synthesis of the Economic Effects of the Temporary Assistance for Needy Families Program*, sponsored by the Office of Planning, Research, and Evaluation at the Administration for Children and Families in the U. S. Department of Health and Human Services.

connections between individual reform measures and specific social outcomes can be most clearly drawn.

For now, the evidence of the caseload literature is not as clear as we would like it to be on whether the work-oriented reforms of the waiver era and early TANF years contributed to the decline in welfare rolls during the 1990s. Intuitively, given the extent and steadfastness of that decline, it is tempting to say they must have, and several defensible studies point in that direction. Equally defensible, at least for the moment, is the possibility that policy played very little role, and that much of the caseload decline of the 1990s was the result of sluggish adjustment to earlier changes. In the process of reaching this crossroads, a strongly cautionary tale emerges of how findings of strong policy influence—findings both plausible and oft-replicated—can be called into question by the arrival of alternative analytic methods and interpretations. Though unwelcome, this may be the strongest lesson for policymakers and their advisors as PRWORA's reauthorization approaches: When sifting through conflicting research results and assessing the conventional wisdom about PRWORA's effect on caseloads and other outcomes, the best evidence—like the best policy—changes over time and must be revisited regularly.

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