Explaining the Recent Decline in Welfare Caseloads
Is the Council of Economic Advisors Right?
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In early May 1997 the Council of Economic Advisers (CEA) published a technical report on sources of variation across states in use of Aid to Families with Dependent Children during the first four years of the Clinton Administration. The Technical Report: Explaining the Decline in Welfare Receipt, 1993-1996 is the outcome of a longer-term Council study of AFDC caseload trends. The research on which it was based was cited by President Clinton in March, and the principal conclusion "that over 40 percent of the decline [in the aggregate national AFDC caseload from 1993-1996] can be attributed to economic growth and that almost one-third is related to waivers, particularly those that sanction recipients who do not comply with work requirements," (p.2) was reported widely in one form or another in the media.

Much care was shown in some of the report's discussion. However, we think the analysis is flawed and are concerned about the policy implications that may be drawn from CEA's conclusions. This paper explains the basis for this judgment and concern. There is a growing professional interest in the problem of modeling aggregate welfare use (Blank 1997; Ziliak et al., 1997); we include as an appendix some discussion of issues raised by the CEA report that are relevant to this literature.

CEA's Method

We begin with an abbreviated review of the method CEA used in its report. The object of the Council study was to analyze the effect of economic and policy factors on variations in public assistance use (or share of the population receiving AFDC) across states from fiscal year 1976 through 1996. To this end, the logarithm of each state's AFDC recipiency rate was regressed on the logarithm of the state's maximum AFDC benefit, the current and previous year's unemployment rate, and a series of indicator (dummy) variables defined by the presence or absence of federally granted waivers for AFDC policy innovation. The recipiency rate is the ratio of AFDC recipients to the state's population. The preferred models include state fixed effects, year fixed effects, and state-specific linear time trends. The model producing the quoted conclusion is reproduced in Table 1.

The CEA's method for dividing responsibility for the 1993-96 decline in welfare receipt among unemployment, waiver, and other influences is as follows. First, the share attributable to change in unemployment was calculated by for each state predicting, using the estimated coefficients, the utilization in 1996 had unemployment changed but all other explanatory variables (including the waivers indicators) stayed constant at their 1993 values. This unemployment-effects-only change in the utilization rate was then divided by the actual change in the utilization rate to produce an estimate of the share (expressed in percent) of the change attributable to reductions in unemployment. This computation was performed state by state and then averaged to produce a national estimate, using state population as weights.

The exercise was then repeated for the waivers indicators. In this case all other variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Estimated Coefficient (standard error in parentheses)</th>
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<tbody>
<tr>
<td>Logarithm of maximum current AFDC benefit for family of three</td>
<td>8.61 (4.83)</td>
</tr>
<tr>
<td>Unemployment rate</td>
<td>-0.77 (0.42)</td>
</tr>
<tr>
<td>Lagged (one year) unemployment rate</td>
<td>4.79 (0.41)</td>
</tr>
<tr>
<td>Any welfare waiver applicable statewide approved</td>
<td>2.26 (2.38)</td>
</tr>
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including the measures of the unemployment rate, were held constant at their 1993 levels, so that the predicted change was the result only of growth in the number of approved waivers as reflected in the waivers indicators. The result in this case is interpreted as the percent of the actual change in rates of welfare use that is the result of waiver approval. The difference between 100 and the sum of the percentages attributable, respectively, to the unemployment rate and to the waivers is the residual the CEA assigns to "other unidentified factors." 4

CEA’s Results

The CEA confines attention to waivers approved for statewide application during the Bush and Clinton administrations, because waivers granted prior to this, notably during the Reagan administration, "affected a very small share of a state's caseload or were superseded by national legislative changes" (p. 4). The Council’s principal inferences from the regression are as follows:

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<thead>
<tr>
<th></th>
<th>Coefficient</th>
<th>Standard Error</th>
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<tr>
<td>Statewide waiver approved permitting tougher JOBS sanctions</td>
<td>-6.96</td>
<td>(3.11)</td>
</tr>
<tr>
<td>State has received or will receive during coming fiscal year approval for any statewide welfare waiver</td>
<td>-6.28</td>
<td>(2.21)</td>
</tr>
<tr>
<td>State has received or will receive during coming fiscal year approval for tougher JOBS sanctions waiver</td>
<td>-1.50</td>
<td>(2.60)</td>
</tr>
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</table>

Source: Council of Economic Advisors 1997. According to the source, the combined cross-section (all states), time-series (annual averages, 1976-1996) regression includes state and year fixed effects and linear, state-specific time trends.

• The economy matters. After two years (recall the lag effect), a 1 percentage point drop in the jobless rate reduces the share of the state's population receiving AFDC by 4 percent.

• Waivers affect the caseload, at least when they authorize more stringent JOBS sanctions. 5 A state receiving federal approval for a statewide welfare reform that includes latitude for stricter JOBS sanctions is estimated to experience a decline of 4.7 percent in the incidence of public assistance receipt.

• Caseloads fall when waivers are in prospect. Perhaps the most striking result is the presence of what the Council terms a "threat effect": state caseload growth in any year is negatively affected by the fact that the state will have a statewide waiver approved in the year to come. The prospect of a waiver approval next year lowers this year's incidence of public assistance receipt by over 6 percent.

• Both the economy and waivers count. When executed using the regression reported in Table 1, the simulation method outlined above leads to the conclusion quoted at the beginning of this paper. Of the decline in the aggregate national AFDC caseload between 1993 and 1996, 44.1 percent is attributed to the change in unemployment, 30.9 percent to welfare waiver approval, and the residual is assigned to other factors (CEA 1997, Table 3).

Problems with These Results

We discuss four points of particular importance here; in the appendix we identify other problems and opportunities for future research. The major issues below concern (1) the specification of waivers, (2) the interpretation of sanctions, (3) the theory behind the demand for waivers, and (4) the role of waiver anticipation.

The Specification of Waivers

A key theme of the CEA’s report is that what happened was a result of federal policy. However, during the period examined the Clinton administration did not attempt to direct the character of welfare reform at the state level. Rather, the policy was largely permissive, and the content of welfare innovation was left up to the states (Wiseman 1996a). The federal role was limited to approving state applications for waivers of requirements of the Social Security Act that restricted discretion in AFDC, Food Stamps, and Medicaid program operation. The (perhaps inadvertent) econometric reflection of this policy emphasis is that in the CEA report, the effects of waivers are associated exclusively with the date of waiver approval (the "award" that is the manifestation of federal policy), not the date of program implementation (the beginning of tangible consequence for AFDC as experienced by applicants and recipients).

This creates a significant specification problem, since in many cases the lag between waiver approval and program implementation is substantial. It was not part of administration policy to monitor closely the actual implementation of the innovations the waivers authorized; indeed, the capacity of the agency responsible for waivers oversight was substantially reduced through budget cutbacks. As a result, complete and reliable data on the time pattern of implementation have yet to be assembled. Examples of problems can be obtained, however, from research in progress. 6 Here are four:

• Delaware is reported by the CEA to have been approved on May 8, 1995 for a major statewide waiver that includes enhanced JOBS sanctions. In fact, while the program was intended to be statewide, implementation began on a small scale, affecting only a small portion of the caseload. The process will not be completed until September 1998.

• Texas is reported to have been granted a major statewide waiver on March 22, 1996. The program was not scheduled for implementation until fall 1996, with its showpiece-time limits on assistance-deferred for implementation until 1997.
• **Virginia** is reported to have received a waiver on July 1, 1995. However, implementation of the reform is gradual; by July 1997 only about half of the state's caseload was affected.

• **West Virginia** is counted as receiving a major statewide waiver on July 31, 1995, but the program authorized was not implemented until February 1996, and affects only about half of the two-parent (AFDC-UP) caseload, a minority of AFDC cases in the state.

Thus, if the indicator variables used by the CEA are intended to identify the point at which innovations make a concrete difference for welfare operation, or even the entire period over which the authorized programs are in effect, the variables appear to contain errors. There appear to be problems of commission as well as omission. By restricting the analysis to waivers issued after 1991, the CEA dismisses significant innovations in states such as Alabama, California, Washington, and Wisconsin that were approved and implemented prior to this cutoff (see Wiseman 1993). Some of these initiatives—notably Wisconsin's—were statewide in impact. It is not true that these efforts affected only "a very small share" of the caseloads in the states involved, as is claimed in the report. Moreover, the fact that some of the innovations pioneered by these states were subsequently "superseded by national legislative changes" (p. 4) does not justify ignoring the effects of such initiatives when and where originally implemented. After all, many of the innovations counted in the analysis have been superseded by the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA). It does mean that it is inappropriate to associate the beginning of Wisconsin's statewide initiatives with approval of the "Work Not Welfare" initiative in June 1994. The first statewide experiments with changes in welfare work incentives, employment program requirements, and eligibility standards for two-parent families were approved and initiated much earlier (Wiseman 1996a). The point is that if the effects of state innovations on caseloads are to be studied, we should include all innovations, not just those implemented during the turndown from what was an historically unprecedented cyclical increase.

### The Interpretation of Sanctions

At first glance, Table 1 seems to provide strong evidence that "harsher sanctions" requested "to motivate unwilling participants" (p. 22) have a significant effect on the caseload. In fact, however, there is widespread agreement that penalties for noncompliance authorized by the Family Support Act were inadequate and cumbersome (Wiseman 1993, argues for experimental investigation of alternatives). What we question is interpretation: is it really possible that the *tripling* of waiver effect associated in the CEA regression with inclusion of sanctions is the result of tough penalties? Since this indicator is the only measure of waiver activity with specific content that proved statistically significant, the policy implications are clear: to reduce welfare caseloads, the most effective policy is to go after the "unwilling participants."

We raise three cautionary notes. First, a state's request for a sanctions waiver may signal more than a desire to persecute. The usefulness of sanctions as an instrument of policy depends on the preexistence of a substantial level of administrative activism. To sanction a recipient for missing meetings or failing to conduct job searches requires that meetings be scheduled, job searches be planned, and attendance or activity registered. While it is true that in some instances sanctions are applied capriciously, in our experience it is primarily the agencies with something to offer recipients that feel justified in sanctioning those not participating. It is therefore possible that a state which seeks further sanctioning authority is already operating a more comprehensive and more tightly managed JOBS program than a state that has not sought such authority. Sanctions may have an effect on caseload, but they likely signal that a state is already operating a serious JOBS program.

Second, part of the statistical significance of the sanctions indicator may derive from relative measurement precision. Unlike the other policy changes studied by the CEA, an alteration in JOBS sanctions can be implemented immediately, and some of the effects of this change realized at the outset. Indeed, if our hypothesis is true that mainly "activist" welfare programs seek such waivers, agencies may already be well equipped to communicate changes in policy once authorized and may even have lists of "target" recalcitrants. If such policies influence behavior, effects should closely follow waiver approval. In contrast, the effects of other waiver-authorized policy changes may take time to unfold. The introduction of time limits, for example, has immediate caseload or case opening consequences only if participants respond by husbanding their time allotments.

Third, the CEA appears to equate the use of sanctions with the incidence of their application. The incidence of sanctions is an outcome, not an input. It is possible that the frequency with which states actually apply sanctions is an *inverse* indicator of the consistency and rigor of the obligations that states enforce upon recipients. If threats are believed, they may never have to be carried out.

### The Demand for Waivers

As the CEA acknowledges, waiver approvals are not wholly exogenous gifts from 1600 Pennsylvania Avenue. Rather, waiver awards are a response to waiver applications, and waiver applications are products of the complicated politics of welfare policy at the state level. It is possible that the correlation between waiver approval and caseload decline uncovered in the Council's regression is the result of some third factor linked to both. The CEA suggests that waiver applications might be interpreted as a response to exceptional caseload growth, and that, if this were the case, the waivers might lead a "reversion to the mean" process that would have occurred in any event. However, the Council reports that waiver states were not necessarily those with the greatest caseload runup prior to 1993. 10

We believe that state innovation works the other way—that is not a response to caseload increase, but to caseload decline. When states experience unanticipated caseload reduction, resources (both time and money)
are released for experimentation. Budgeting, both for benefits and for JOBS, is done prospectively. Having fewer people on welfare than expected means that, at least until the budgeting process catches up, more resources will be available per case. Moreover, the natural inclination of welfare bureaucracies to link caseload decline to welfare-to-work efforts stimulates more of the same and supports the case for sustaining agency budgets.

This bureaucratic dynamic supporting welfare activism is reinforced by politics. Thanks to Governor Thompson of Wisconsin, Governor Engler of Michigan, and others, governors nationwide learned in the early 1990s that there were political benefits to be reaped from active (and media visible) efforts at moving people from welfare to work. By 1996 states found it embarrassing not to have a waiver, and caseload decline made welfare reforms and the minor fiscal risks they sometimes involved feasible.

We do not mean to imply that state demonstrations lacked substance or were motivated wholly by the quest for media exposure. What we argue is that a complete model of waiver effects calls for a theory of waiver application, which requires input from political scientists and economists. Our simple model leads to a prediction that welfare decline will lead rather than follow waivers, as was discovered by the Council.

**Waivers in Prospect**

In fact, the CEA's results are heavily dependent on the role played by the "lead" indicator of waiver approval (see the sixth variable in the regression reported in Table 1). If this variable and the similar variable for the JOBS sanctions indicator are removed, the estimate of the contribution of waivers to the 1993-1996 caseload decline falls by almost one-third, from 31 to 22 percent.

The Council identifies the lead indicator as a measure of waiver "threat" effects. This claim is not completely credible for three reasons. First, many of the waivers are not threatening: Why should a recipient household be threatened by the prospect of an expansion of the earned income disregard? Indeed, the empirical results show that the one clear "threat"—that is waivers permitting more stringent JOBS sanctions—does not have a significant lead effect beyond that associated with the prospect of any waiver. This is particularly surprising given the fact, discussed above, that the JOBS sanctions waivers are likely to be most quickly implemented.

Second, it is hard to believe that news of the prospect of waiver approval acts on welfare accessions and terminations twelve months in advance of the actual date of federal approval. At minimum, the appropriate reference point would seem to be the point of waiver application, and then only if the application is publicized, which is often not the case. If the threat of future policy is driving down the caseload, we do not understand why it is necessary for the waivers to be statewide in application, as the Council maintains. Surely even a small demonstration (Wisconsin's test of a two-year time limit occurred only in two small rural counties) can presage more general stringency for recipients elsewhere.

Third, if the Council believes in threat effects, every state without a statewide waiver by August 1995 should thereafter be coded with one in prospect for August 1996, for that was when the Personal Responsibility and Work Opportunity Reconciliation Act was signed. PRWORA mandated statewide implementation of a series of reforms similar in content to those that triggered indicator variables for states granted earlier waivers. If the award of waivers triggered caseload decline in advance, why should we not expect the same from federal legislation?

Of course, it is difficult to imagine potential and actual welfare recipients being influenced a year in advance by anticipation of PRWORA passage, especially when experts doubted as late as mid-summer 1996 that agreement between the White House and Congress could be achieved. What is easier to believe is that caseload decline creates elbow room for innovation, or at least the opportunity to score easy political points from the publicity surrounding waiver application. Our "theory" readily explains why the leading indicator for any statewide waiver has a statistically significant coefficient, while the leading indicator coefficient for the one real and specific threat, tougher JOBS sanctions, does not. What is being measured is fiscal latitude. The realization of any substantial waiver implies that in the preceding year the state used the fiscal leeway created by caseload decline to increase the ambition of its JOBS program. Specific waiver content is irrelevant to this linkage. On the other hand, content is relevant at point of implementation, and may explain why the JOBS sanction indicator "works" contemporaneously.

**The Consequences**

This discussion involves more than econometrics. In our opinion the Council report supports three policy conclusions that are politically attractive but are not sufficiently supported by the empirical evidence. Here are the messages:

- **The caseload is the target.** The aim of public assistance policy has been to alleviated need. Over the past decade commentators increasingly have claimed that the welfare system itself has contributed to the incidence and persistence of poverty. If this is so, reducing the numbers of families exposed to aid is in and of itself a desirable end. By terming receipt of public assistance "welfare dependence" (p. 1) and suggesting (p. 2) that the discovery of association of welfare waivers with caseload decline would justify "continued efforts along these lines," the Council implicitly endorses the objective of reducing caseload. We know of no evidence indicating that reduction in caseload, by itself, has unambiguously positive consequences for the well-being of families with children. Those contemplating "continued efforts along these lines" might wish to devote more attention to identifying the lines we are on.

- **Need has a soft margin.** If just the anticipation of waivers causes some recipients to leave welfare and
others to refrain from applying, then such families must have resources not recognized by the system as it operated before PRWORA. If granting states the right to impose "tougher" sanctions for failure to comply with requirements to prepare for and to seek work causes caseload to decline by over 7 percent, then the message is that a significant number of recipients are lazy. Taken together, the CEA results imply that at the beginning of 1992 over 12 percent of the caseload had the capacity to leave welfare if prodded by the prospect of statewide waiver-based activity or a tougher JOBS policy. Is this message justified? If justified in 1992, is it true for today?

Waivers made the difference. Finally, the CEA report amounts to an endorsement of the position that, cyclical effects aside, federal law was the principal constraint operating on state policy. The administration’s policy of eliminating the constraints on discretion in AFDC program operation imposed by the Social Security Act allowed more families to get off welfare. The clear implication is that this federal policy did indeed play a significant role, and "continued efforts along these lines are likely to lead to additional reductions." The only "line" identified is the relaxation of federal oversight. From 1993-1996 political incentives existed for states to pursue waiver-based innovation, and the activist policy with which such innovations were associated most likely did affect the caseload. But recipients were protected then by entitlement, and the fiscal incentives created for states by matching grants encouraged spending. Entitlement is now gone, and fiscal incentives have changed. State responses need to be monitored carefully. The "race to the bottom" may still take place, and the CEA seems to have opened the door to the stairwell.

Conclusion

In sum, we believe that the Council’s claim to have explained the decline in welfare receipt from 1993 through 1996 is unjustified, as is its argument about the large role that welfare waivers played in reducing welfare caseloads. The CEA analysis is undermined by errors in variables, lack of attention to the circumstances that created states’ demand for waivers, and the dependence of a major share of the results on highly questionable anticipation effects. We caution policymakers against relying on these results to formulate future welfare policy at the national or state level.

Appendix: Notes on Time Series Analysis of AFDC Caseloads

Although at this writing the CEA has not done so, it is apparently planning to release the data used in Explaining the Decline. In addition to the reservations expressed in our paper, we think the following issues deserve more attention in future econometric investigations and other empirical work.

1. Dependent variable. The Council report uses as dependent variable the ratio of persons receiving AFDC to the entire population. Thus the denominator includes many persons neither currently nor potentially categorically eligible for AFDC. The mix of population differs across states and changes over time in ways that cannot be captured wholly by fixed state effects or fixed linear trends. Since virtually every case is associated with a mother, analysis might be better conducted by looking, say, at the ratio of AFDC cases to the population of adult women of less than retirement age. This is the procedure followed by Blank (1997).

2. Observation weighting. While not detailed in the report itself, it is our understanding that the state observations used in the CEA report for regression estimation were weighted by state population. Blank (1997) does this as well. It is not clear that weighting in this way is appropriate. If the states are laboratories for examination of waiver effects, each trial should be given equal weight. If theory suggests that effects will differ by state size, size should enter as a right-hand variable (or be captured by fixed state effects). Of course, at the simulation stage large states will account for a larger share of the predicted aggregate national caseload, since the (transformed) dependent variable is multiplied by population to produce estimated caseload.

3. Benefit deflation. The benefit variable used in the CEA regressions is deflated by the consumer price index. This is a misspecification, since AFDC recipients are all eligible for Medicaid, which provides in-kind health services. Therefore the benefit variable should be deflated by the CPI less medical expenses. This adjustment is significant because over the period 1976-1996 inflation in costs of health care was a substantial and variable component of increases in the CPI. Blank (1997) uses the deflator for Gross Domestic Product; Zilliak et al. (1997) avoid the problem (but possibly create others) by excluding dollar-denominated variables from their model.

4. Competing opportunities. The standard theory of demand for public assistance treats the attraction of welfare receipt partly as function of the return to available alternatives, most notably work. The CEA model includes a measure of the gains from welfare (the basic benefit variable) but no measure of the gains from work. Zilliak et al. (1997) do likewise. Available empirical research (Brady and Wiseman 1997) suggests that potential earnings from work vary over the interval covered by the CEA in a way that cannot be captured by fixed state effects and a fixed state time trend alone. Blank (1997) controls for wage trends using wage distribution data from the Outgoing Rotation Group (ORG) of the monthly Current Population Survey. However, she attempts no adjustment for variation in net earnings at given wages generated by changes in tax, unemployment insurance, and Social Security contributions. Such changes, in particular recent expansions of the Earned Income Tax Credit, significantly affect the gain from low-wage employment.

5. AFDC-Basic versus AFDC-UP. The CEA ignores the distinction between the basic (largely single parent)
AFDC caseload and two-parent families receiving assistance through the unemployed parent program. Up until 1988 only about half of all states offered AFDC to two-parent families. The Family Support Act mandated that all states adopt the program by 1991, although with provisions that new programs could include time limits. AFDC-UP caseload dynamics are generally much different from those for AFDC-Basic, and it would be desirable to model the two separately, as Blank (1997) does.

(6) Equilibrium and components of change. The CEA model assumes that adjustment of caseload to policy or economic shocks takes at most two years. This adjustment process operates both by altering the rate at which existing cases close and the rate at which new cases open. Many of the hypotheses proposed in the CEA report to explain the caseload effects of waivers may be expected to have different effects on rates of accession and rates of termination. While national data on components of caseload change are of questionable accuracy, some states have good information that might be profitably used to study waiver effects on the various components of caseload change.

(7) Intervention content. Finally, the classification system used by the CEA for waivers is derived from the large number of descriptive summaries of waiver proposals published over the past few years by various groups. More attention to development of theory might provide clues regarding how such innovations should best be characterized and aggregated for purposes of analysis of caseload dynamic effects. Traditionally, analysts have summarized the parameters of basic benefits systems in terms of "guarantee" and "benefit reduction rates." What is an equivalently parsimonious description of the new initiatives?

References


Notes


2. See, for example, DeParle (1997).

3. It is our understanding that the indicator variables were scaled on the basis of the precise timing of waiver approval. Thus, the indicator value for fiscal year 1994 for a waiver granted September 1 (the first day of the last month) is 1/12, or .083. The state observations are weighted by the state's population. These details are not presented in the CEA's technical report itself, but were obtained from conversations with CEA's staff.

4. This procedure assumes that waiver approval and unemployment rates are uncorrelated. The CEA reports testing for the presence of interaction effects, suggesting that waiver policies might possibly be more effective in states with lower unemployment rates (note 20, p. 17). The interaction effects were not statistically significant. However, such a test does not verify distributional independence. If waivers and unemployment rates are correlated, the CEA procedure will understate the size of the unexplained residual.

5. JOBS is the welfare-to-work education, training, work, and job search assistance program established by the Family Support Act of 1988 and terminated by the Personal Responsibility and Work Opportunity Reconciliation Act of 1996. To the extent resources permitted, adult AFDC recipients were required to participate unless exempted due to illness, employment, status as a caregiver, or certain other
considerations. Those eligible who refused to participate were subject to penalties involving reduction of welfare grants. See Ways and Means Committee 1996, pp. 402-428.

6. As part of the Assessing the New Federalism project the Urban Institute is building a general database on state welfare policy at present and over the recent past. The database includes information on the timing, scale, and content of state innovations and will be available on the World Wide Web in fall 1997. A similar information set, intended for use in conjunction with the Survey of Income and Program Participation and the newly inaugurated Survey of Program Dynamics, is under construction at the Institute for Research on Poverty at the University of Wisconsin-Madison.

7. The caseload expansion of the early 1990s is unprecedented only if one excludes the expansion of the late 1960s. The caseload expansion in 1960s was similar to recent experience in that growth seemed to be out of proportion to changes in economic conditions. In this earlier episode, however, it appears that what was occurring was a social transformation in which significant numbers of families eligible for benefits chose for the first time to apply for assistance after administrative and social barriers to their accession had diminished. When the pool of potential eligibles was exhausted, caseload growth stopped. The story for the most recent runup appears to be more complicated. As Blank (1997) points out, the big question for welfare policy is not really why caseloads fell after 1993, but why they had risen so much in the first place.

8. Note that all waivers counted are "statewide" and the full effect is measured by the sum of coefficients on the lead and contemporaneous indicators. Thus, introduction of a statewide waiver that includes more stringent JOBS sanctions reduces the caseload by 2.26-6.96-6.28-1.50 = -12.48 percent. Without the JOBS sanction, the full waiver effect is 2.26-6.28 = -4.02 percent, only one-third as great.


11. Note that the PRWORA effect will not be captured by year fixed effects. The PRWORA mandate altered the relative status of welfare recipients in states that had not previously applied for waivers to implement changes of the type mandated by the new legislation. Year fixed effects apply to all states.

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Other Publications by the Authors

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