

## **Why Did the Welfare Caseload Decline?**

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## **Abstract**

A substantial literature has considered the effects of welfare reform policies on the aggregate caseload but has been less successful in disaggregating the effects of specific policies. Using monthly caseload data from October 1989 through June 2003, we estimate a flexible model for the dynamic response of the welfare caseload to the economy and to the three major welfare reform policies: Financial incentives to combine work and welfare, sanctions for noncompliance with work-related requirements, and time limits on the receipt of cash assistance. Our results are consistent with the predictions of economic theory and indicate the importance of carefully specifying the intensity and dynamics of policy changes and of including a rich set of measures of the economy. Simulations of the implied effect of each of our preferred model's components on the national welfare caseload indicate that sanction and time limits policies played a role, as did the economy, although an undifferentiated reform bundle had a larger impact.

## *I. INTRODUCTION*

Welfare reform—the Aid to Families with Dependent Children (AFDC) waivers of the early 1990s, culminating in the federal Personal Responsibility and Work Opportunities Reconciliation Act of 1996 (PRWORA)—produced major changes in government aid to poor children and their families. Three policies in particular—financial incentives to combine work and welfare, sanctions for noncompliance with work activities, time limits on the receipt of aid—form what can plausibly be called the centerpiece of this period of reform. One measure, although certainly not the only one, of the success of these reforms is the resulting decline in the welfare caseload.

A substantial literature exists that seeks to estimate the effects of welfare reform policies on the welfare caseload (CEA, 1997; CEA, 1999; Grogger, 2003; Grogger, 2004; Hofferth, Stanhope, and Harris, 2002; Levine and Whitmore, 1998; MaCurdy, Mancuso, and O’Brien-Strain, 2000; Martini and Wiseman, 1997; Mead, 2000; Moffitt, 1999; Schoeni and Blank, 2000; Wallace and Blank, 1999; Ziliak et al., 2000). However, persuasively disentangling the effects of individual policy changes from reform as a bundle and from the impact of the economy on the caseload has posed a challenge (Blank, 2002).

Using monthly administrative data covering a number of years before and after the major welfare policy and economic changes of the mid-1990s, we estimate difference-of-difference models of the determinants of the aggregate welfare caseload. Our empirical specification incorporates flexible specifications of the timing of the effects of the forcing variables—welfare policy and the economy—on the aggregate caseload and of the intensity of the three major policies. The resulting parameter estimates are consistent with hypothesized effects of financial incentives, sanctions, time limits, and the economy, and the estimated effects are large. Furthermore, these specification choices matter. Hypothesis tests decisively reject the simpler specifications. More important, compared to our full model, models that measure the impact of

policies with indicator variables imply the policies have much smaller effects, and models that include only the unemployment rate imply the macro-economy has much smaller effects.

Nevertheless, our simulations suggest only a moderate role for any specific reforms in explaining the large observed drop in the welfare caseload. We attribute about a quarter of the caseload decline to time limits and sanctions, about a fifth to the economy, and about a third to a residual policy bundle; the remainder of the decline is absorbed by unexplained time effects.

The balance of this paper proceeds as follows. The next section reviews the earlier literature on the three major welfare reform policies: financial incentives to combine work and welfare, work-related sanctions, and time limits. We discuss the likely timing of the effects of imposing these specific policies on the caseload. The third section describes our data, while the fourth section describes our empirical specification, informed by, and intended to capture, lagged effects. The fifth section provides our results, including simulations of the implied impulse response functions. The sixth presents simulations that trace out the implied effect of each policy and the economy on the observed path of the caseload. The paper concludes with a discussion of the broader implications of these results.

## ***II. DETERMINANTS OF THE WELFARE CASELOAD***

The theoretical and empirical literature on the effect of welfare policy on the welfare caseload is large and well reviewed elsewhere. (See Moffitt, 1992; Blank, 2002; Grogger, Karoly, and Klerman, 2002; Moffitt, 2002.) Here, we consider three major policy determinants of the welfare caseload in the approximate order they were introduced into the policy arena: financial incentives to combine work and welfare, work-related sanctions, and time limits on aid receipt.

- *Financial Incentives to Combine Work and Welfare:* To encourage work among welfare recipients and to raise their household resources, many states either increased the amount of earnings not counted when calculating the cash assistance benefit or reduced the rate at

which the cash assistance benefit falls as earnings grow, or both. Since such policies have the effect of making individuals with more earnings still eligible for welfare, they are expected to raise the welfare caseload, at least in the short run. Some proponents of these programs hoped that the increased contact with the labor market would reduce the welfare caseload in the long run. Both experimental and econometric studies yield mixed evidence of a positive caseload effect (Becerra et al., 1998; Bos et al., 1999; CEA, 1999; Hendra and Michalopoulos, 1999; Michalopoulos et al., 2000; Miller et al., 2000; Moffitt, 1999; Ziliak et al., 2000). To differentiate between states that did adopt significantly increased financial incentives and those that did not, we code a state as having a financial incentive if a recipient remains eligible for cash assistance while working full-time at the state's minimum wage (Committee on Ways and Means, 1996; Committee on Ways and Means, 2000).

- *Work-Related Sanctions:* At least since the time they implemented their PRWORA-mandated TANF programs, all states require work or participation in activities leading to work. Failure to comply with these requirements is penalized with a “sanction”: the loss of either the adult portion of the benefit or the entire benefit. These sanctions are likely to lower the welfare caseload, having both mechanical effects (those who are sanctioned are dropped from welfare) and behavioral effects (those who realize they will be required to participate in an activity will find jobs and leave welfare). Most econometric studies find that immediate, full-family sanctions have negative effects on the welfare caseload of that range in size from -15 to -40 percent (CEA, 1999; MaCurdy et al., 2001; Mead, 2000; Rector and Youssef, 1999). We distinguish among states that adopted immediate, full-family sanctions and those that adopt gradual, full-family sanctions (Crouse, 1999; Pavetti et al., 2003). In our coding, states that imposed adult-only sanctions in the TANF period remain in the AFDC comparison group.

- *Time Limits on Aid Receipt:* Under waivers, several states adopted life-time limits on welfare receipt. PRWORA strongly encouraged states to do so. If recipients are forward-looking, we would expect a decline in the caseload before any family could have reached the time limit; this anticipatory response would occur because some recipients bank their months in anticipation of future need (Grogger and Michalopoulos, 1999; Klerman, 2004). If recipients are short-sighted (or myopic), we would expect a sharp decrease in the month (or perhaps the first few months) that time limits begin to bind. While no waiver-era experiment tested a time limit alone, quasi-experimental analyses provide evidence of an anticipatory response (Grogger, 2004; Grogger and Michalopoulos, 1999). Evidence from simulations suggest the potential for a large myopic effect if recipients are not forward-looking. However, tabulations of the size of the caseload that reached the time limit indicate either that recipients are not purely myopic or that states are not dropping time-limited families from the rolls as frequently as their policies announced they would (Bloom, Farrell and Fink, 2002; Danielson, 2001; Duncan, Harris, and Boisjoly, 2000). Our specification distinguishes between (i) no time limits, (ii) adult-only time limits—where payments to the children continue after the time limit is reached, (iii) long, full-family time limits—where long is 48 or more months, usually 60 months; and (iv) short, full-family time limits—where short is 36 months or less.

Table II.1 gives summary counts of states adopting each policy under waivers and under TANF, according to our coding. (See Appendix A for a discussion of the coding of state policies.) Figure II.1, which graphically depicts the timing and types of policies that states adopted, demonstrates that while states often adopted policy bundles, there is still substantial variation in the policies that states enacted.

<Figure II.1 about here>

<Table II.1 about here>

We also consider other determinants of the welfare caseload: the payment level, the minimum wage, and the state of the economy:

- *Payment Level*: Higher payment levels should raise the welfare caseload, since some are made newly eligible and others who previously did not participate because they found the net benefit of welfare to be too low are drawn in. Previous estimates of the impact of changing the benefit level are large: CEA (1999) and Blank (2001) estimate elasticities ranging from 0.15 to 0.57. We proxy for the payment level with the welfare benefit for a family of three with no other income.
- *Minimum Wage*: A higher minimum wage could have either a negative or a positive effect on the caseload. A higher minimum wage may induce businesses to substitute away from labor, thus increasing the caseload; at the same time, the minimum wage jobs that remain are better paying, thus reducing caseload. In net, the effect on welfare caseloads is ambiguous. Previous research has found a negative effect of the minimum wage on the welfare caseload: CEA (1999) estimates an elasticity of between -0.25 and -0.52, while Grogger (2004) also finds a negative impact of the minimum wage on welfare use among single mothers with children over nine. We use the state-level minimum wage and, following CEA (1999), compute the real monthly earnings of an individual working at the minimum wage for 30 hours a week.
- *Economy*: Welfare is a substitute for work. Thus, we expect the welfare caseload to be counter-cyclical. The literature clearly finds an effect of the economy, but estimates of the magnitude of the effect vary widely (CEA 1999; Figlio and Ziliak, 1999; Klerman and Haider 2003; Moffitt, 1999; Ziliak et al., 2000). Following Hoynes (2000) and Haider, Klerman, and Roth (2003), we include the following measures of the economy: the unemployment rate, total per-capita employment, and real total per-worker wages.

Given the coincidence of welfare reform and the robust economic expansion of the 1990s, controlling for these other policies and the economy is crucial if we want to accurately estimate

the effects of the policies of interest. Furthermore, these estimates are likely to be of interest in their own right.

### ***III. DATA***

Ideally, we would test the implications of our time limits model using national, monthly, individual-level panel data on welfare receipt for a large sample. (See Klerman and Haider, forthcoming, for a discussion of the data issues and of individual-level modeling of welfare dynamics.) Unfortunately, the available panel data (e.g., the Survey of Income and Program Participation, SIPP, the National Longitudinal Survey of Youth, NLS-Y, and the Panel Study of Income Dynamics, PSID) have relatively small samples and only limited information (often subject to considerable recall bias and, particularly important for our strategy of using monthly data, seam bias) on the history of welfare receipt. Cross-sectional data (e.g., the Current Population Survey, CPS) have larger, but still relatively small, samples of welfare recipients, but almost no information about the history of welfare receipt (as would be required to construct proxies for reaching time limits); such data also appear to be subject to considerable (and worsening) under-reporting of welfare receipt.

Instead, like Wallace and Blank (1999) and Ziliak et al. (2000), we analyze aggregate, monthly state-level counts of welfare cases readily available from the Administration for Children and Families (ACF) with approximately a six-month lag. We use data from the start of federal fiscal year 1990 (October 1989) through June 2003. This results in 165 months of observations on 50 states and the District of Columbia, for a total of 8,415 state-month observations. These monthly data are attractive given our interest in testing for the presence of a sizable drop in the caseload when the leading edge of recipients reaches the time limit. As we discuss in detail in Appendix A, we adjust the data in 27 states to correct for anomalies in

reporting to the federal government. Following Moffitt (1999), we convert total cases to reciprocity rates by dividing by the at-risk population—women age 15 to 54—in each state.<sup>1</sup>

#### ***IV. EMPIRICAL ESTIMATION STRATEGY***

We begin with a conventional difference-of-differences specification, then augment it with a flexible functional form for the policies that are our focus and for the economy. In particular, we estimate the following equation for the log caseload:

$$\text{Log}[C_{st}] = \mathbf{a} + X\mathbf{b} + \mathbf{d}_s + \mathbf{d}_s * y + \mathbf{g} + \mathbf{t} + \mathbf{e}_{st}$$

where  $X$  represents the forcing variables (e.g, policies and the economy) and  $\beta$  the corresponding regression coefficients,  $d$  is a vector of state dummies,  $y$  is a linear year trend (interacted with the vector of state indicators),  $\gamma$  a vector of month dummies, and  $t$  a national year spline, with knot points at the start of each federal fiscal year. The subscript  $t$  indexes time measured in months and the subscript  $s$  denotes states. See Appendix A for a discussion of the exact specification and data sources.

Our approach differs from the previous literature in the manner in which we specify the forcing variables,  $X$ . Conventional practice in the literature on the effects of welfare policy on the caseload, and in the difference-in-difference literature more generally, is to code a policy as having its full effect on the day the policy becomes “effective” (CEA, 1999; Moffitt, 1999; Schoeni and Blank, 2000; Wallace and Blank, 1999; Ziliak, Gundersen, and Figlio, 2000). Several considerations suggest that lagged reactions to policy changes should, in fact, be expected:

1. Policies have intrinsic implementation schedules. For example, welfare time limits do not affect recipients immediately; in the time period we examine, the shortest time limit on aid was 18 months, and the modal policy was 60 months. Similarly, sanctions for

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<sup>1</sup> We note that a better measure of caseloads for our purposes would be the cases that contain an adult; child-only cases are not subject to welfare reform policies. These caseloads are not available for the entire period of our data.

noncompliance with work activities cannot have effects on the caseload until a recipient is assigned to an activity, fails to complete the activity, and any due process procedures have been followed.

2. Many policies require a bureaucracy for full implementation. Putting that bureaucracy into place is rarely instantaneous (see Klerman et al., 2000, for the case of California's CalWORKs program, where implementation was measured in years).
3. In general, the target population's behavioral response to policy changes depends on the spread of knowledge about the policy and confidence that it will be applied.
4. Klerman and Haider (forthcoming) and Haider and Klerman (forthcoming) show that if a policy affects the flows onto and off of welfare, the effect on the caseload stock will be a distributed lag, with the lag lasting several years.

This last point is applicable to the economy as well (Klerman and Haider, forthcoming; Haider, Klerman, and Roth, 2003). For the case of the economy, the previous literature has sometimes included lagged values and those lagged values often substantially increase the estimated total effect of the economy. (See CEA, 1999; Klerman and Haider, forthcoming; Ziliak et al., 2000.)

For financial incentives and sanctions, as well as for a bundled measurement of other reforms, we include an indicator variable that takes on the value of 1 in every month after implementation, a linear effect in months since implementation, and a spline beginning at 24 months after implementation.

The argument for more lagged responses to policy changes is somewhat more subtle for time limit policies. Figure V.1 depicts our parameterization for a 60-month time limit. The anticipatory effect of time limits "turns on" in the month that a state implements its time limit; thereafter, the time limit counts up by percentage points to the 0<sup>th</sup> exhaustion month, and then up by months to the end of the data. This specification of time relative to reaching the limit despite varying time limit lengths is analogous to the specification in Meyer (1990) for the effects of unemployment insurance. The schematic illustrates the hypothesized effects of time limits for a

recipient: one-time drops in the caseload in the months that time limits were implemented and first became binding, a steady, and possibly accelerating, decline in the months before any recipient exhausted her time limit, and a continued, and possibly sharper, decline after the 0<sup>th</sup> month. We allow for discontinuous effects of implementation and of first reaching the time limit, as well as incorporate a set of splines before the 0<sup>th</sup> month to allow for an accelerating anticipatory decline and a spline after to accommodate myopic impacts.

The specification also includes flexible generalized controls for the economy. Following Haider, Klerman, and Roth (2003), we enter current, lagged, and interacted values of the unemployment rate, total per-worker earnings, and total per-capita employment.

Estimation proceeds using STATA's GEE (generalized estimating equations) routine with three adjustments for the non-i.i.d. nature of the data. First, even given the fixed effects and covariates, the data appear to be auto-correlated. This is not surprising given the monthly frequency of our data; however, our estimates imply a moderate level of autocorrelation even in the corresponding annual data. The mean, annualized empirical first autocorrelation is 0.80 across all states. We correct for correlation between the observations on a state with an AR(12) process using a common autocorrelation parameter for the 51 panels.<sup>2</sup>

Second, the standard errors are computed using the Stata "robust" option to control for arbitrary heteroscedasticity. Third, we estimate the model unweighted, computing standard errors that are robust to arbitrary heteroscedasticity. This approach implies that we treat each of the 50 states and the District of Columbia as independent experiments conditional on the fixed effects included in the model. The parameter estimates represent policy effects had the "average" national policy been implemented.

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<sup>2</sup>Wallace and Blank (1999) and Ziliak et al. (2000) take first differences to eliminate non-stationarity in caseloads measured at monthly intervals. We argue that the data do not appear to warrant this approach. A regression of the residuals from an OLS regression on their lagged values results in a point estimate of 0.95. The test that this estimate of rho is significantly different from one has a t-statistic of 13.28.

## V. *RESULTS*

We present results of our preferred model in column 1 of Table V.1. The specification is complex, and interpretation of these parameters is not straightforward. Figures V.1 and C.1-C.4 plot the implied impulse-response functions and their standard errors for time limit policies, work-related sanctions, financial incentives, AFDC waivers and TANF, and the economy, respectively.

<Table V.1 about here>

<Figure V.1 about here>

### *Financial Incentives and Work-Related Sanctions*

. The parameter estimates for the effects of sanctions for noncompliance with work requirements and financial incentives to combine work and welfare have the expected signs and relative relationships (see Table V.2). Strong financial incentives (i.e., those for which recipients working full-time at the state minimum wage retain eligibility for cash benefits) generate a caseload effect that grows at 0.64 percent per month, reaching 15 percent after two years. Thereafter, there is no statistically significant additional effect. By six years after implementation, the implied effect is still large; however, it is not precisely estimated.

<Table V.2 about here>

Sanction policies cut the caseload, and the effect grows with the severity of the sanction. We estimate a common implementation effect. The point estimate, which is not significantly different from zero, implies that little of the total decline is at initial implementation. Taking the adult-only sanction policy as our baseline, the caseload effect at two years after implementation is 0.5 percent in gradual full-family sanction states and -21.8 percent in immediate full-family sanction states. The point estimate for the impact of gradual sanctions grows to -14 percent at six years after implementation, although it is not significantly different from zero. In immediate sanction states, there is no statistically significant growth in the effect after two years; however, the effect at six years continues to be large and significant.

*Time Limits*

The parameter estimates provide statistically significant evidence of drops in the caseload both before and after recipients reached full-family time limits (summarized in Table V.3). Consistent with forward-looking behavior, the estimates imply a total anticipatory effect of 22 percent. We tested specifications that allowed both the implementation and hit effects to vary by time limit length and found substantial difference in the point estimates, but we were unable to reject the hypothesis of common effects.

<Table V.3 about here>

We used monthly data specifically to be able to detect a one-time drop in the caseload implied by myopia or forward-looking behavior with a high discount rate (a low variance of wage offers or high preference for receipt at younger ages) in the month that the leading edge of the caseload reaches the time limit. Our parameterization allows for such a drop. The parameter estimates imply a significant, but substantively small, drop in the caseload in the month that time limits were first binding. The bureaucratic effort required to implement time limits suggests looking over a slightly longer period. Over the 12 months after the time limits hit, there is an additional drop of 8 percent in states that implemented a short, full-family time limit. There is no additional drop in states that implemented long, full-family time limits, and the point estimates are also small. This result is consistent with the interpretation that states that implemented the TANF-standard time limit were reluctant to drop families that reached it. This result is also consistent with the interpretation that 60 months was a long enough period to enable families to fully anticipate the time limit.

Thereafter, there is no significant additional drop in states that implemented full-family time limits, but there are few such months in our sample. For example, only two states with long, full-family time limits are beyond 24 months after the time limit.

Effects are smaller (in net, approximately zero) for adult-only time limit states. There are only nine states in the adult-only category, so these estimates are noisy: They imply a significant decline in the caseload initially, followed by a significant increase 12 months after the first

families could have reached the time limit. The estimates imply a significant 14 percent anticipatory drop, but then a 0.33 percent increase each month after time limits hit, augmented by an additional 0.22 percent increase after 12 months. This implies that at about 30 months after time limits hit, the total effect of time limits is zero in adult-only states.

We also consider generic reform dummies for “any waiver” and “TANF implementation” for policies not included in our specification (see Table V.2).<sup>3</sup> In our model, waiver and TANF estimates represent “all else,” including childcare subsidies and miscellaneous policies like family caps and child immunization requirements, policies towards noncitizens, and the general mood of reform. Our estimates suggest that, above and beyond the policies we model directly, waivers had no effect on caseloads, while TANF implementation *apart from* the implementation of time limits, financial incentives, and work-related sanctions had a large negative impact on caseloads during the first two years after implementation. Table V.2 and Figure B.3 show that we estimate a cumulative TANF effect of -18 percent at two years after implementation; the decline continues at a reduced pace thereafter, reaching -39 percent by six years after implementation.

We include bundled policy effects in our main models to avoid spuriously attributing their dynamics to time limit, sanction, and financial incentive policies. Nevertheless, it is possible that the model that includes a general effect of the imposition of waivers and of TANF is over-specified in a model that already distinguishes major reform policies: time limits, benefit reduction rates, and sanctions for noncompliance with work requirements. Table B.1 presents the results of a model that excludes these general (i.e., TANF and waiver) policy effects. While the policy parameter estimates typically become more negative, qualitatively the results generally remain the same.

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<sup>3</sup>These include changes to sanctions or financial incentives under waivers or TANF that did not meet our criteria for inclusion (for instance, increased financial incentives that still left a recipient working full-time at minimum wage ineligible for cash assistance).

*Effect of the Economy*

The economy shifted widely over this period, experiencing first a long and robust expansion followed by a moderate and lingering recession. The expansion is roughly simultaneous with the period in which we would expect anticipatory effects of time limits, as well as initial impacts of work-related sanctions; the recession is roughly simultaneous with the period in which we would expect mechanical effects. Clearly, a careful specification for the economy is crucial for proper estimates of time limits. Furthermore, the relative roles of welfare policy and the economy as determinates of the caseload is a subject of substantive interest in its own right.

Following Klerman and Haider (forthcoming) and Haider, Klerman, and Roth (2003), we include fully interacted current and lagged values of several measures of the economy: the unemployment rate, total employment, retail employment, and total earnings. The parameter estimates for the economy in Table V.1 are consistent with the implications of Klerman and Haider (forthcoming) that including both lags and interactions of the lags of the measures of the economy are needed to capture the dynamics of caseload change. In addition, they are consistent with the finding in other research that the unemployment rate does not fully capture the effects of the economy on the caseload (Haider, Klerman, and Roth, 2003; Hoynes, 2000). Figure B.4 plots the impulse response functions for a common one standard deviation year-to-year change.<sup>4</sup> Table V.4 shows the cumulative effect at one year after this change, and at two years, the point at which, in our parameterization, the change has been fully felt in the caseload. Thus normalized, the unemployment rate, which is the conventional measure of the economy used in the literature, has about half the total impact of the other two proxies for the economy.

<Table V.4 about here>

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<sup>4</sup>We set the measures of the economy on the same scale by first calculating the equivalent of a one percentage point change in the underlying unemployment rate (i.e., from which state-specific and seasonal effects have been removed) in standard deviations for all the measures of the economy, and by then simulating how an improvement of this size phases in, starting at the mean of each measure.

*Maximum Benefit and Minimum Wage*

The maximum benefit has the expected sign. The coefficient implies that for every 10 percent increase in the real monthly welfare benefit, the caseload increases by 3.4 percent. This estimated elasticity is large, but it is within the range estimated by CEA (1999) and Blank (2001). In our preferred model, the point estimate falls short of the conventional significance level, although when we estimate the standard errors less conservatively (column 3 of Table V.1), the maximum benefit coefficient is significantly different from zero. The minimum wage had a negative but insignificant effect on the welfare caseload. Other recent research has found a negative and significant impact of the minimum wage (CEA 1999; Grogger, 2004).

**VI. IMPLIED EFFECTS ON THE TOTAL CASELOAD**

The point estimates help us to understand the effect of specific policies, holding all else constant. Figure VI.1 displays the results of simulations to understand the implications of these parameter estimates for the path of the aggregate caseload from February 1992 forward (the peak of the national unemployment rate; see Klerman and Haider, forthcoming, who argue for using the turning points in the forcing variables as the baseline rather than the turning point in the caseload). Individual simulations consider the path of the aggregate caseload, successively holding each of the factors at their (real) base levels but allowing all other forcing variables (including the fixed effects for time) to continue to evolve.<sup>5</sup> Because the caseload declined dramatically during this period and because about a quarter of the total caseload decline over this period had already occurred *before* states implemented their TANF policies, the percentage point impact of each of the policies on the caseload decline in the simulations is substantially smaller than the corresponding cumulative effects implied by the parameter estimates, which implicitly hold all else equal.

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<sup>5</sup> We use the smearing estimate developed in Duan, 1983 to retransform the predictions in the log scale to the level scale. We further adjust each simulation by the difference between the actual and predicted actual caseload in each month.

<Figure VI.1 about here>

The results suggest that policies explain about a quarter of the caseload decline, waivers and TANF over a third, and the economy about a fifth—in fact, had the economy remained at its February 1992 level, the simulations imply that the caseload would have increased by an additional quarter of a million families before beginning to decline. The first row of Table VI.1 shows the percentage-point difference between the actual and simulated caseload decline from the height of the national caseload (March 1994) to the last month of our data (June 2003). Holding the economy fixed at its February 1992 level results in a decline that is smaller by 9.9 percentage points. If financial incentives had not been implemented, the national caseload would have declined slightly more than it actually did; the effects in states that enacted weak financial incentives nearly cancelled out the impact of implementing strong financial incentives. In addition, California, with nearly a quarter of the nation's caseload, implemented its financial incentives in 1992, so that by March 1994, in our parameterization the bulk of the impact of this policy was fully reflected in the state's caseload. Both work-related sanctions and time limits had moderate negative impacts on the caseload: Each contributed about 6 percentage points to the actual decline. Finally, TANF implementation had a large negative impact of about 20 percentage points, or a nearly a third of the actual decline.

<Table VI.1 about here>

The second row of Table VI.1 provides the same information for a somewhat shorter time period: March 1994 to June 2001 (i.e., until the approximate end of the caseload decline). It is evident that the caseload decline over this period was nearly identical to the decline over the longer time period. However, the contribution of the economy to the decline in this time period was more than 50 percent higher, while the contribution of TANF and of individual policies were markedly lower. The latter result is not surprising, since the policies continued to depress the caseload at least through six years after implementation in the modal state, which was approximately October 2002. The explanation for the sizable change in the amount of the decline explained by the economy is somewhat more subtle. The low point of the unemployment

rate occurred in late 2000; thus, in June 2001 the lagged effects of the economic boom were still being felt in the caseload. By June 2003, the simulated caseload is moving back in the direction of the actual caseload, because both the contemporaneous and lagged measures of the economy were moving back in the direction of their February 1992 levels.

Succeeding rows of the table report simulated declines for alternate specifications of the model. If we had not included lagged and interacted values of the four measures of the economy, we would also have attributed much less of the decline to the economy. Similarly, if we had used a less rich specification of the economy, measuring it only with the unemployment rate, we would have underestimated the effect of the economy and overestimated the effect of TANF. If, as in much of the previous literature, we had included only indicator variables to denote that a policy was in effect, excluding splines, we would have estimated the effect of policies at close to zero. These results are striking; they indicate that the previous literature that has parameterized welfare reform policies with indicator variables and the economy with the unemployment rate and/or with current measure(s) of the economy has underestimated the impact of policy changes on the caseload. Finally, if we had used the unadjusted ACF data, the overall story would have remained similar, but individual effects would have shifted somewhat.

## ***VII. CONCLUSION***

This paper has used monthly administrative data to estimate difference-of-difference models of the welfare caseload. Our specification of policies and the economy is rich—we incorporate multiple levels of policies, multiple proxies for the economy, and allow for lagged and interacted effects. The resulting estimates of the effects of policy and the economy are substantively large.

Despite the plausibility, magnitude, and statistical significance of these results, our simulations imply that the measured policies explain only about a quarter of the 58 percent decline in the national caseload between the peak of the national caseload (March 1994) and the

end of our data (June 2003). The set of variables measuring the residual impact of TANF is markedly more important. This last result could imply that the included proxies capture only some of the true policy variation. Finally, we conclude that the robust economy explains about a fifth of the caseload decline.

These results are sensitive to modeling decisions. In particular, the implied impulse response functions for each of the policies and for the economy are nontrivial. In general, a richer specification of the lag structure for a factor yields a larger estimate of the effect of that factor, while a more conventional parameterization of the factors would lead us to conclude that the factor had essentially no impact on the caseload. Some of the literature to date has experimented with lags of measures of the economy and with lagged policies, and our estimates strongly imply that simply parameterizing policy changes with indicator variables fails to capture the impact of policies on the caseload stock over time.

Finally, we note that compared with the earlier literature, these results simply benefit from more time. Caseload reports are noisy, and the impulse response functions for policies are nontrivial. Together, the presence of these hurdles suggest the difficulty of “instant policy analysis” in the years immediately following a policy change, which are unfortunately also the years when interest is typically highest.

## ***APPENDIX A. DATA***

This appendix describes our data. We begin with a discussion of the welfare caseload data and our adjustments to the raw data published by ACF (available at [www.acf.dhhs.gov](http://www.acf.dhhs.gov)). We then discuss the coding of the key independent variables: time limits, sanctions, financial incentives, and the economy. Finally, we discuss the specification and coding of other control variables for other welfare policies.

### **Dependent Variable: Monthly State Welfare Caseload**

The TANF caseload data come from monthly reports that all states make to the Administration for Children and Families (ACF) and that ACF publishes with about a six-month lag and provides upon request. We divide these caseloads by the female population age 15–54 in each state to obtain a measure of per-capita caseloads. State-level population data come from the US Bureau of the Census at [www.census.gov](http://www.census.gov), and monthly estimates are derived by interpolating linearly between yearly estimates and projections.

Table A.1 reports our data sources for cash assistance caseloads. There are three reasons why the ACF data do not accurately record welfare caseloads in the TANF period: the TANF Emergency Data Reporting Period, the creation of Separate State Programs (SSPs), and the implementation of Tribal TANF programs. To construct a consistent time series, we both obtained complete state data where possible and imputed caseloads where state-level information was incomplete.

During the TANF Emergency Data Reporting Period, which lasted from approximately federal fiscal years 1997 through 1999, not only were states not required separately to report the number of child and adult recipients, they also had considerable discretion in how they defined their caseload, such that the concepts often differ substantially from those reported under AFDC or under the final TANF reporting rules. This leeway noticeably affected the reporting of the caseload in a number of states. Because the intercept shift in Texas was sizeable, because it occurred during the period in which TANF policies were being implemented, and because Texas

was not able to not provide comparable data for this period, we adjust the caseload in this state for this period by constraining the caseload change in the month beginning and ending the TANF Emergency Data Reporting Period to be the average caseload change in the three months before and the three months after the period, respectively.

Under PRWORA, states have the option of creating SSPs using state funds to provide non-TANF cash assistance to families for whom they do not wish the federal TANF rules to apply; two parent families are the most common category of case that have been moved to an SSP. While these cases are not subject to the federal time limit, they are typically subject to state time limits. States typically do report these cases to the federal government; however, ACF does not routinely release these data. Beginning in July 1997, 27 states—including all the states with substantial two-parent caseloads—moved two-parent cases to SSPs. In the fall of 2001, the percentage of cash assistance cases that were in a SSP ranged from 0.7 percent in Georgia to 30.3 percent in Hawaii (Bloom, Farrell, and Fink, 2002). We handle these “missing” cases in two ways. First, where possible, we have obtained data from the state on the entire case assistance caseload—sometimes at monthly intervals for the entire period in question, and sometimes at more widely-spaced intervals or for part of the period. Second, where this information could not be provided, we shift the monthly caseload reported to ACF up by the percentage of the total caseload that the SSP caseload represented in the month before the cases were moved to the SSP. That is, we make the assumption that the proportion of the caseload that is in an SSP is roughly constant from the last month creation date of the SSP forward.<sup>6</sup>

It is important to note that states can also move time-limited cases that have reached 60 federally countable months to SSPs to extend their months on aid; however, they can also move them to a segregated state program or use their 20 percent federal extension allowance to continue to provide cash assistance to these cases. If states use either of the latter two strategies, the time-limited cases will continue to be counted as part of the TANF caseload. For instance, in

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<sup>6</sup>States can also segregate state funds within their TANF programs; cases in such segregated programs are also not subject to federal time limits (though they are typically subject to state time limits). Since these cases are reported as part of the TANF caseload, no adjustment for these types of programs is necessary.

the summer of 2003, officials in Michigan indicated in a personal communication that they were using the 20 percent extension allowance and did not expect to exceed it in the foreseeable future. The only state to date that has systematically shifted families reaching the time limit to an SSP is New York, in which 14.6 percent of the caseload seemingly left aid in the first month that time limits were binding according to the TANF caseload data; in reality, most if not all were moved to a state-funded voucher program. Table A.1 shows that we obtained state-level information for both TANF and SSP cases in New York.

<Table A.1 about here>

Finally, in the PRWORA period, 37 tribes or tribal coalitions formed their own Tribal TANF programs in 15 states. When these programs went into effect, members of tribes covered by a program were no longer reported to ACF as part of a state's cash assistance caseload. The estimated number of families covered by these programs ranged across the states from 10 to 8,937. These totals represent the total caseload that Tribal TANF programs expected to serve, but are based on 1994 estimates, reflecting the formula used to calculate Tribal TANF program federal block grants. These estimates imply that tribal TANF programs absorbed just over 0.02 percent of California's caseload, but 40 percent of Wyoming's caseload. We shift up caseloads in states in which Tribal TANF programs were initiated by the estimated percentage of the caseload moved to a Tribal TANF program from the date of implementation of the program forward. See OFA (2003) and Hillabrant et al. (2003) for more detail on Tribal TANF programs. While it is the case that PRWORA exempts adults living on reservations with unemployment rates greater than 50 percent rates from time limits, it is not the case that tribal members uniformly receive this exemption. (See, for example, Winston, 2002.)

### **Policies and the Economy**

Table A.2 summarizes our coding of the policy choices that states made and the fraction of the national caseload affected by each set of policy choices. Below, we describe the sources of this coding.

<Table A.2 about here>

*Time Limits:* We code time limit policies using the characterization of the policies given in Bloom, Farrell and Fink (2002). This characterization includes implementation month, length, and to whom the policy applies. We use states' time limits policies, not the federal policy, because they override the federal policy. Florida and Texas have time limits that vary in length depending on characteristics of the case; we use the longest of the possible time limits in both cases.

Michigan, New York, and Vermont, do not impose time limits on their caseloads. New York has moved many time-limited cases to their "Safety Net" program, which is an SSP. Michigan and Vermont have likewise pledged to continue all cases. Through the end of our data, Michigan used federally-allowed extensions exclusively to continue aid to time-limited cases. Because Vermont had a waiver in place until July 2001, cases there will not reach the federal time limit until 2006.

*Sanction Policies:* We take implementation dates of sanction policies from ASPE and severity of sanctions from a recent Mathematica Policy Research review of sanction policies (Crouse, 1999; Pavetti et al., 2003).<sup>7</sup> In states that implemented a new policy during the waiver period and that did not change policies under TANF, policies are assumed to have been implemented in the first month the waiver was in force, and to remain in force thereafter. The coding reflects the fact that some states did alter their waiver sanction policies when they implemented TANF. We define a sanction policy change to be imposing either a gradual or immediate full-family sanction for noncompliance with work activities. We assume that states imposing adult-only sanctions under waivers or TANF did not significantly change from the AFDC/JOBS period.

*Financial Incentives:* We take implementation dates of changes to financial incentives to combine work and welfare from an ASPE tabulation of the dates of waiver and TANF changes to financial incentive policies (Crouse, 1999) and code a state as implementing a generous financial incentive if, according to the 1996 and 2000 US House of Representatives Committee on Ways

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<sup>7</sup>The exception is Arizona, which implemented a redesign of its policies in August 1997. According to Mills et al. (2001), full-family sanctions were first imposed in Arizona in November 1997.

and Means Green Books, the new policy made a single adult with two children working full-time at the state minimum wage eligible for cash assistance (Committee on Ways and Means, 1996; Committee on Ways and Means, 2000). While reductions in the real value of benefits or of the minimum wage may have induced some crossover in states close to the cut-off level in other years, we did not see a strong substantive argument in favor of counting such crossovers as policy changes.

*AFDC Waivers:* We code states as having implemented a waiver policy if the state adopted a policy that did not fit into the three described above. These include family cap and work exemption policies.

Table A.3 provides dates and severity for time limits, work-related sanctions, financial incentives to combine work and welfare, and AFDC waivers not otherwise counted.

<Table A.3 about here>

*Maximum Benefit and Minimum Wage:* We include the log of the average of the previous year's maximum benefit level for a family of three divided by 100, as well as the log of the average of the previous year's state-level minimum wage working 30 hours per week, 50 weeks per year (divided by 100). Both are taken from the data used in CEA (1999) and were updated using information from Urban Institute's Welfare Rules Database, the State Policy Documentation Project, the fifth TANF Report to Congress and information provided by the U.S. Department of Labor (CBPP/CLASP, 1999; OFA, 2003; Urban, 2001a).

*The Economy:* Following previous research that indicates that richer measures of the economy increase the share of observed change from the economy, we use three measures of the economy: monthly state-level unemployment rates (multiplied by 100) from the Bureau of Labor Statistics' series A; monthly state-level total covered employment as recorded by the Quarterly Census of Employment and Wages (QCEW) Program that the BLS conducts in cooperation with the states; and quarterly state-level total earnings per worker, as recorded by the QCEW. QCEW and CES data for the second quarter of 2003 are still preliminary. We divide total employment by the total estimated population in a state and month and multiply them by 100.

Following Klerman and Haider (2003), we model the effect of the economy using current and lagged values, as well as interactions of these values. The current and lagged measures are averages of the previous year's values and the average of the values one year previous.

*Other Controls:* The specification includes a national year spline with knots in October of each year, 51 state fixed effects, 12 month indicators, and state-specific linear year trends. To capture cases moved into Tribal TANF programs, we include a modified indicator variable measuring the estimated fraction of the caseload shifted to a Tribal TANF program from the month that each Tribal TANF program was implemented forward. Table A.1 lists the earliest date that a Tribal TANF program was implemented in each state, and the Division of Tribal TANF Management within ACF provides the estimated size of Tribal TANF programs (see [www.acf.dhhs.gov/programs/dts/](http://www.acf.dhhs.gov/programs/dts/)).

**Appendix B. Additional Empirical results**

<Table B.1 here.>

<Figures B.1-B.4 here.>

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**Table II.1. State policy choices**

	AFDC Waivers		TANF	
	No. of states	% of nat'l caseload (1996)	No. of states	% of nat'l caseload (1996)
<b>Financial incentives</b>				
Not eligible for cash assistance if working full-time	49	78.5%	34	49.3%
Eligible at full-time, minimum wage work	2	21.5%	17	50.7%
<b>TANF work-related sanctions</b>				
Adult-only	32	61.1%	16	46.2%
Gradual full-family	9	19.2%	17	22.8%
Immediate full-family	10	19.7%	18	31.0%
<b>Time limits</b>				
None	46	95.1%	3	13.8%
Adult-only	2	2.6%	9	33.8%
Long full-family	0	0.0%	23	28.7%
Short full-family	3	2.3%	16	23.7%

**Table V.1. Empirical results**

	<i>Point estimates</i>	<i>Conventional standard errors</i>	<i>Robust standard errors</i>
<b>Measures of the economy</b>			
Unemployment rate, (average over past year)	0.050	(0.0099)	(0.012)
Unemployment rate, 1 year lag	0.039	(0.0095)	(0.012)
Unemployment rate, interaction	-0.0050	(0.0014)	(0.0018)
Total employment, (average over past year)	-0.026	(0.0050)	(0.0074)
Total employment, 1 year lag	-0.022	(0.0042)	(0.0074)
Total employment, interaction	0.00028	(0.000057)	(0.00011)
Total earnings per worker, (average over past year)	-0.00054	(0.00019)	(0.00021)
Total earnings per worker, 1 year lag	-0.00038	(0.00019)	(0.00021)
Total earnings per worker, interaction	0.00000011	(0.000000058)	(0.000000073)
Log of the min. wage working 30 hours per week, past year's average	-0.16	(0.13)	(0.11)
<b>Time limit policies</b>			
Full-family TL implemented	-0.22	(0.084)	(0.048)
Slope from full-family time limit implementation forward (% scale)	-0.0021	(0.00089)	(0.00048)
Add'l effect in shorter TL states	0.00060	(0.00051)	(0.00078)
Spline from 67% before	-0.0015	(0.00070)	(0.00060)
Add'l effect in shorter TL states	0.0016	(0.00078)	(0.00070)
Spline from 33% before	0.0029	(0.0014)	(0.0012)
Add'l effect in shorter TL states	-0.0032	(0.0016)	(0.0014)
Add'l effect of first reaching the full-family time limit	-0.0080	(0.0029)	(0.0038)
Slope after full-family time limit first reached (months scale)	0.00056	(0.0017)	(0.0021)
Add'l effect in shorter TL states	-0.0068	(0.0030)	(0.0027)
Spline at 12 months after first reached (months scale)	0.0031	(0.0021)	(0.0027)
Add'l effect in shorter TL states	0.0032	(0.0027)	(0.0036)
Adult-only time limit implemented	-0.14	(0.066)	(0.076)
Slope from adult-only time limit implementation forward (% scale)	-0.0015	(0.00068)	(0.00077)
Spline from 67% before	-0.00031	(0.00042)	(0.00071)
Spline from 33% before	0.00061	(0.00076)	(0.0014)
Add'l effect of first reaching the adult-only time limit	-0.012	(0.0062)	(0.0078)
Slope after full-family time limit first reached (months scale)	0.0033	(0.0018)	(0.0030)
Spline from 12 months after	0.0023	(0.0023)	(0.0036)
<b>Financial incentives (ability to combine work and welfare)</b>			
AFDC waiver/TANF financial incentives: implementation	-0.0038	(0.014)	(0.0066)
Financial incentives: slope	0.0064	(0.0022)	(0.0017)
Financial incentives: spline at 24 months out	-0.0068	(0.0022)	(0.0022)
<b>Work-related sanctions</b>			
AFDC waiver/TANF work-related sanctions: implementation	-0.011	(0.0088)	(0.0048)
Gradual full-family work-related sanctions: slope	0.00058	(0.00073)	(0.00063)
Add'l effect of immediate full-family sanctions: slope	-0.0036	(0.0020)	(0.0015)
Gradual full-family work-related sanctions: spline at 24 months out	-0.0092	(0.0031)	(0.0018)
Add'l effect of imm. full-family sanctions: spline at 24 mths out	0.011	(0.0036)	(0.0025)
<b>Waivers and TANF</b>			
AFDC waivers: implementation	-0.0060	(0.0094)	(0.0039)
AFDC waivers: slope	0.00062	(0.00040)	(0.00032)
AFDC waivers: spline at 24 months out	-0.0011	(0.00082)	(0.00076)
TANF: implementation	-0.010	(0.020)	(0.0058)
TANF: slope	-0.0071	(0.0026)	(0.0013)
TANF: spline at 24 months out	0.0036	(0.0027)	(0.0017)
Log of the maximum benefit for a family of three, past year's average	0.34	(0.18)	(0.089)
Observations	8,415		

Standard errors in parentheses (column 1: robust standard errors).

Note: all models include state fixed effects, a common national year spline, state-specific linear year trends, common seasonal indicators, and an intercept shift for the implementation of tribal TANF programs. Models are estimated on 165 months of observations in 50 states and the District of Columbia.

**Table V.2. Cumulative Effects of Sanctions, Financial Incentives, AFDC Waivers, and TANF**

<i>Policy</i>	<i>24 months after implementation</i>	<i>72 months after implementation</i>
Gradual full-family sanctions	0.0047 (0.018)	-0.14 (0.078)
Immediate full-family sanctions	-0.22 (0.078)	-0.27 (0.11)
Strong financial incentives	0.15 (0.061)	0.12 (0.11)
AFDC waivers (residual)	0.0083 (0.012)	-. <sup>8</sup>
TANF (residual)	-0.18 (0.059)	-0.39 (0.12)

**Table V.3. Cumulative Effects of Full-Family Time Limits**

	<i>48 or 60 month, full-family time limit</i>	<i>18-36 month, full-family time limit</i>	<i>Adult-only time limit</i>
Implementation	-0.0052 (0.012)	-0.074 (0.043)	0.0077 (0.008)
67% before	-0.085 (0.029)	-0.11 (0.050)	-0.040 (0.022)
33% before	-0.19 (0.058)	-0.16 (0.066)	-0.10 (0.046)
Total anticipation	-0.22 (0.084)	-0.22 (0.084)	-0.14 (0.066)
Hit	-0.22 (0.084)	-0.22 (0.084)	-0.15 (0.066)
1 year after	-0.21 (0.097)	-0.30 (0.098)	-.11 (0.079)

**Table V.4. Cumulative Effects of Economic Improvements**

<i>Measure of the economy</i>	<i>Change one year after standardized improvement</i>	<i>Change two years after standardized improvement</i>
Unemployment rate	-0.027	-0.037
Total per capita employment	-0.028	-0.074
Total wages per worker	-0.030	-0.070

<sup>8</sup> Waivers were generally in place for 3 years or less. Only one state had a waiver that was in place for longer than 4 years.

**Table VI.1. Simulations**

Simulation (percentage point difference from actual)							
Time period	Actual change	Model	Economy	Financial Incentives	Work-related Sanctions	Time Limits	TANF
Mar/94-Jun/03	-57.6%	Preferred	9.9	-2.2	5.8	6.9	19.8
Mar/94-Jun/01	-57.8%	Preferred	15.2	-2.3	4.5	7.4	13.9
Mar/94-Jun/03		Measures of the economy: current values only	6.4	-2.3	6.3	6.5	19.0
		Measures of the economy: unemployment rate only	2.4	-2.6	5.9	7.0	19.8
		Measures of policies: indicator variables only	11.6	0.0	0.5	1.0	0.5
		Dependent variable: ACF- reported caseloads	10.3	-0.5	5.0	8.1	19.0

**Table A.1. Caseload data sources**

State	ACF	Sources		Separate State Programs (GAO, 2002)	Tribal TANF programs (OFA, 2003)	Estimated fraction of caseload NOT in ACF data in largest month of divergence <sup>9</sup>
		State	Imputed			
Alabama	Oct/89-Jun/03	-	Oct/97-Jun/03	Oct/97	-	0.8%
Alaska	Oct/89-Jun/03	-	-	-	Oct/98	38.6%
Arizona	Oct/89-Jun/03	-	-	-	Nov/97	16.0%
Arkansas	Oct/89-Jun/03	-	-	-	-	-
California	Oct/89-Feb/98	Oct/99-Jun/03	-	Oct/99	Mar/98	11.9%
Colorado	Oct/89-Jun/03	-	-	-	-	-
Connecticut	Oct/89-Sep/98	-	Oct/98-Jun/03	Oct/98	-	3.5%
Delaware	Oct/89-Sep/98	-	Oct/98-Jun/03	Oct/98	-	3.6%
District of Columbia	Oct/89-Feb/99	-	Mar/99-Jun/03	Mar/99	-	1.3%
Florida	Oct/89-Sep/96	Oct/96-Jun/03	-	Oct/96	-	4.3%
Georgia	Oct/89-Dec/96	-	Jan/97-Jun/03	Jan/97	-	0.4%
Hawaii	Oct/89-Jun/97	-	Jul/97-Jun/03	Jul/97	-	23.1%
Idaho	Oct/89-Jun/03	-	-	-	Jan/99	29.4%
Illinois	Oct/89-Jun/01	-	Jul/01-Jun/03	Jul/03	-	1.4%
Indiana	Oct/89-Sep/96	Oct/96-Sep/00	Oct/00-Jun/03	Oct/96	-	4.8%
Iowa	Oct/89-Jun/03	-	-	-	-	-
Kansas	Oct/89-Jun/03	-	-	-	-	-
Kentucky	Oct/89-Jun/03	-	-	-	-	-
Louisiana	Oct/89-Jun/03	-	-	-	-	-
Maine	Oct/89-Aug/97	Sep/97-Jun/03	-	Sep/97	-	27.3%
Maryland	Oct/89-Dec/96	-	Jan/97-Jun/03	Jan/97, Jan/99	-	4.3%
Massachusetts	Oct/89-Jun/03	-	-	-	-	-
Michigan	Oct/89-Jun/03	-	-	-	-	-
Minnesota	Oct/89-Jun/03	-	-	-	Jan/99	0.3%
Mississippi	Oct/89-Jun/03	-	-	-	-	-
Missouri	Oct/89-Sep/99	Apr/03-Jun/03	Oct/99-Mar/03	Oct/99, Oct/01	-	11.5%
Montana	Oct/89-Nov/01	Dec/01-Jun/03	-	Dec/01	Jan/99	9.9%

<sup>9</sup> Estimate includes both estimated fraction of cases moved to Tribal TANF programs and to Separate State Programs.

State	ACF	Sources		Separate State Programs (GAO, 2002)	Tribal TANF programs (OFA, 2003)	Estimated fraction of caseload NOT in ACF data in largest month of divergence <sup>9</sup>
		State	Imputed			
Nebraska	Oct/89-Sep/00	-	Oct/00-Jun/03	Oct/00	Apr/01	9.8%
Nevada	Oct/89-Sep/01	Oct/01-Jun/03	-	Oct/01	-	14.9%
New Hampshire	Oct/89-Jun/03	-	-	-	-	-
New Jersey	Oct/89-Sep/97	Oct/97-Jun/03	-	Oct/97	-	4.8%
New Mexico	Oct/89-Nov/00	-	Nov/00-Jun/03	Dec/00	Oct/00	20.4%
New York	Oct/89-Dec/00	Jan/01-Mar/03	Apr/03-Jun/03	Jan/01	-	20.3%
North Carolina	Oct/89-Jun/03	-	-	-	-	-
North Dakota	Oct/89-Jun/03	-	-	-	-	-
Ohio	Oct/89-Jun/03	-	-	-	-	-
Oklahoma	Oct/89-Jun/03	-	-	-	May/98	0.3%
Oregon	Oct/89-Jun/03	-	-	-	Jul/97	0.9%
Pennsylvania	Oct/89-Jun/03	-	-	-	-	-
Rhode Island	Oct/89-Sep/98	-	Oct/98-Jun/03	Oct/98, Oct/99, Jan/00	-	6.7%
South Carolina	Oct/89-Jun/03	-	-	-	-	-
South Dakota	Oct/89-Jun/03	-	-	-	Oct/97	3.6%
Tennessee	Oct/89-Sep/99	Oct/99-Jun/02	Jul/02-Jun/03	Oct/99	-	1.8%
Texas	Oct/89-Jun/97, Oct/99-Sep/01	Oct/01-Apr/03	Jul/97-Sep/99 <sup>10</sup> , May/03-Jun/03	Oct/01	-	14.1%
Utah	Oct/89-Dec/96	-	Jan/97-Jun/03	Jan/97	Oct/00	15.7%
Vermont	Oct/89-Aug/01	-	Sep/01-Jun/03	Sep/01	-	5.7%
Virginia	Oct/89-Sep/99	-	Oct/99-Jun/03	Oct/99	-	2.2%
Washington	Oct/89-Jun/03	-	-	-	Oct/98	1.6%
West Virginia	Oct/89-Jun/03	-	-	-	-	-
Wisconsin	Oct/89-Oct/97	Nov/97-Sep/02	Oct/02-Jun/03	Nov/97	Jul/97	51.1%
Wyoming	Oct/89-Dec/96	-	Jan/97-Jun/03	Jan/97	Jul/98	65.7%

<sup>10</sup> TANF Emergency Data Reporting Period

**Table A.2. Detailed Policy Coding**

State	Earliest AFDC waiver implemented (not otherwise coded)	TANF implemented	Time limit implemented	First possible month of exhausting time limit	Financial incentives waiver implemented	TANF financial incentives implemented	Work-related sanctions waiver implemented	TANF work-related sanctions implemented
Alabama		15-Nov-1996	01-Nov-1996	01-Nov-2001				15-Nov-1996
Alaska		01-Jul-1997	01-Jul-1997	01-Jul-2002		01-Jul-1997		
Arizona	01-Nov-1995	01-Oct-1996	01-Nov-1995	01-Nov-1997			01-Nov-1995	01-Oct-1996
Arkansas	01-Jul-1994	01-Jul-1997	01-Jul-1998	01-Jul-2000				
California	11-Sep-1995	01-Jan-1998	01-Jan-1998	01-Jan-2003	01-Dec-1992	01-Jan-1998		
Colorado		01-Jul-1997	01-Jul-1997	01-Jul-2002				01-Jul-1997
Connecticut	01-Jan-1996	01-Oct-1996	01-Feb-1996	01-Nov-1997	01-Jan-1996	01-Oct-1996	01-Jan-1996	01-Oct-1996
Delaware	01-Oct-1995	10-Mar-1997	01-Apr-1996	01-Apr-2000	01-Oct-1995			10-Mar-1997
Dist. of Columbia		01-Mar-1997	01-Mar-1997	01-Mar-2002				
Florida		01-Oct-1996	01-Oct-1996	01-Oct-1999				01-Oct-1996
Georgia	01-Jan-1994	01-Jan-1997	01-Jan-1997	01-Jan-2001			01-Jan-1994	01-Jan-1997
Hawaii	01-Feb-1997	01-Jul-1997	01-Dec-1996	01-Dec-2001	01-Feb-1997	01-Jul-1997		01-Jul-1997
Idaho		01-Jul-1997	01-Jul-1997	01-Jul-1999				01-Jul-1997
Illinois	23-Nov-1993	01-Jul-1997	01-Jul-1997	01-Jul-2002	01-Dec-1993	01-Jul-1997	01-Oct-1995	01-Jul-1997
Indiana	01-May-1995	01-Oct-1996	01-May-1995	01-May-1997			01-May-1995	
Iowa	01-Oct-1993	01-Jan-1997	01-Jan-1997	01-Jan-2002	01-Oct-1993	01-Jan-1997	01-Oct-1993	01-Jan-1997
Kansas		01-Oct-1996	01-Oct-1996	01-Oct-2001				01-Oct-1996
Kentucky		18-Oct-1996	01-Nov-1996	01-Nov-2001				18-Oct-1996
Louisiana		01-Jan-1997	01-Dec-1996	01-Dec-1998				01-Jan-1997
Maine		01-Nov-1996	01-Nov-1996	01-Nov-2001		01-Nov-1996		
Maryland	01-Mar-1996	09-Dec-1996	01-Jan-1997	01-Jan-2002	01-Oct-1996		01-Oct-1996	09-Dec-1996
Massachusetts	01-Nov-1995	30-Sep-1996	01-Dec-1996	01-Dec-1998	01-Nov-1995		01-Nov-1995	30-Sep-1996
Michigan	01-Oct-1992	30-Sep-1996			01-Oct-1992		01-Oct-1994	30-Sep-1996
Minnesota		01-Jul-1997	01-Jul-1997	01-Jul-2002		01-Jul-1997		
Mississippi	01-Oct-1995	01-Jul-1997	01-Oct-1996	01-Oct-2001				01-Jul-1997
Missouri	01-Jun-1995	01-Dec-1996	01-Jul-1997	01-Jul-2002			01-Jun-1995	
Montana	01-Feb-1996	01-Feb-1997	01-Feb-1997	01-Feb-2002				
Nebraska	01-Nov-1996	01-Dec-1996	01-Dec-1996	01-Dec-1998				01-Dec-1996

State	Earliest AFDC waiver implemented (not otherwise coded)	TANF implemented	Time limit implemented	First possible month of exhausting time limit	Financial incentives waiver implemented	TANF financial incentives implemented	Work-related sanctions waiver implemented	TANF work-related sanctions implemented
Nevada		03-Dec-1996	01-Jan-1998	01-Jan-2000				03-Dec-1996
New Hampshire		01-Oct-1996	01-Oct-1996	01-Oct-2001		01-Oct-1996		
New Jersey	01-Oct-1992	01-Jul-1997	01-Apr-1997	01-Apr-2002			01-Oct-1992	01-Jul-1997
New Mexico		01-Jul-1997	01-Jul-1997	01-Jul-2002				01-Jul-1997
New York		01-Nov-1997				01-Nov-1997		
North Carolina	01-Jul-1996	01-Jan-1997	01-Aug-1996	01-Aug-1998			01-Jul-1996	01-Jan-1997
North Dakota		01-Jul-1997	01-Jul-1997	01-Jul-2002				01-Jul-1997
Ohio	01-Jul-1996	01-Oct-1996	01-Oct-1997	01-Oct-2000	01-Jul-1996	01-Oct-1996	01-Jul-1996	01-Oct-1996
Oklahoma		01-Oct-1996	01-Oct-1996	01-Oct-2001				01-Oct-1996
Oregon	01-Feb-1993	01-Oct-1996	01-Jul-1996	01-Jul-1998			01-Jul-1995	01-Oct-1996
Pennsylvania		03-Mar-1997	01-Mar-1997	01-Mar-2002				03-Mar-1997
Rhode Island		01-May-1997	01-May-1997	01-May-2002		01-May-1997		
South Carolina		12-Oct-1996	01-Oct-1996	01-Oct-1998				12-Oct-1996
South Dakota	01-Jun-1994	01-Dec-1996	01-Dec-1996	01-Dec-2001			01-Jun-1994	01-Dec-1996
Tennessee	01-Sep-1996	01-Oct-1996	01-Oct-1996	01-Apr-1998	01-Sep-1996	01-Oct-1996	01-Sep-1996	01-Oct-1996
Texas	01-Jun-1996	05-Nov-1996	01-Jan-1997	01-Jan-2000			01-Jun-1996	
Utah	01-Jan-1993	01-Oct-1996	01-Jan-1997	01-Jan-2000		01-Oct-1996	01-Jul-1996	01-Oct-1996
Vermont	01-Jul-1994	20-Sep-1996			01-Jul-1994		01-Jul-1994	
Virginia	01-Jul-1995	01-Feb-1997	01-Oct-1997	01-Oct-1999		01-Feb-1997		01-Feb-1997
Washington		10-Jan-1997	01-Aug-1997	01-Aug-2002				
West Virginia		11-Jan-1997	01-Dec-1996	01-Dec-2001			01-Feb-1996	11-Jan-1997
Wisconsin	01-Jan-1996	01-Sep-1997	01-Oct-1996	01-Oct-2001	01-Jul-1994	01-Sep-1997	01-Jan-1996	
Wyoming		01-Jan-1997	01-Feb-1997	01-Feb-1999				01-Jan-1997

## Notes

1. Shaded time limit cells indicate the state adopted an adult-only time limit while unshaded cells indicate the state adopted a full-family time limit. Likewise, shaded sanction cells indicate the adoption of a gradual full-family sanction, while unshaded cells denote the adoption of an immediate full-family sanction policy.
2. Cells with a diagonal slash indicate states that adopted a waiver or TANF policy that did not meet our definition of a policy change.

3. States that implemented a policy after the first of the month are counted as having implemented the policy in that month if the date of implementation is in the first half of the month; otherwise, implementation occurs in the following month.
4. Bloom et al. (2002) indicate that Maine has a full-family time limit policy. Discussions with state officials indicated that it more closely resembles an adult-only policy, so we code it as the latter.
5. New Mexico has two TANF implementation dates: the implementation of the governor's plan in July 1997, which was enjoined by the state supreme court in the fall, and the implementation of the legislature's plan in July 1998.

**Table B.1. Empirical results: models excluding waiver and TANF effects**

	<i>Point estimates</i>	<i>Conventional standard errors</i>	<i>Robust standard errors</i>
<b>Measures of the economy</b>			
Unemployment rate, (average over past year)	0.051	(0.0097)	(0.012)
Unemployment rate, 1 year lag	0.039	(0.010)	(0.013)
Unemployment rate, interaction	-0.0048	(0.0014)	(0.0019)
Total employment, (average over past year)	-0.024	(0.0049)	(0.0075)
Total employment, 1 year lag	-0.020	(0.0039)	(0.0076)
Total employment, interaction	0.00025	(0.000056)	(0.00012)
Total earnings per worker, (average over past year)	-0.00060	(0.00018)	(0.00022)
Total earnings per worker, 1 year lag	-0.00045	(0.00018)	(0.00022)
Total earnings per worker, interaction	0.0000023	(0.00000059)	(0.00000075)
Log of min. wage working 30 hours per week, past year's average	-0.23	(0.13)	(0.11)
<b>Time limit policies</b>			
Full-family TL implemented	-0.32	(0.071)	(0.044)
Slope from full-family time limit implementation forward (% scale)	-0.0032	(0.00074)	(0.00045)
Add'l effect in shorter TL states	0.00057	(0.00049)	(0.00078)
Spline from 67% before	-0.0014	(0.00070)	(0.00061)
Add'l effect in shorter TL states	0.0016	(0.00079)	(0.00072)
Spline from 33% before	0.0029	(0.0014)	(0.0012)
Add'l effect in shorter TL states	-0.0031	(0.0016)	(0.0014)
Add'l effect of first reaching the full-family time limit	-0.0081	(0.0028)	(0.0038)
Slope after full-family time limit first reached (months scale)	-0.0021	(0.0015)	(0.0021)
Add'l effect in shorter TL states	-0.0064	(0.0029)	(0.0028)
Spline at 12 months after first reached (months scale)	0.0030	(0.0021)	(0.0027)
Add'l effect in shorter TL states	0.0038	(0.0028)	(0.0037)
Adult-only time limit implemented	-0.26	(0.055)	(0.075)
Slope from adult-only time limit implementation forward (% scale)	-0.0027	(0.00056)	(0.00076)
Spline from 67% before	-0.000097	(0.00056)	(0.00072)
Spline from 33% before	0.00017	(0.0010)	(0.0015)
Add'l effect of first reaching the adult-only time limit	-0.011	(0.0059)	(0.0079)
Slope after full-family time limit first reached (months scale)	0.00045	(0.0015)	(0.0030)
Spline from 12 months after	0.0030	(0.0025)	(0.0037)
<b>Financial incentives (ability to combine work and welfare)</b>			
AFDC waiver/TANF financial incentives: implementation	-0.0088	(0.0086)	(0.0061)
Financial incentives: slope	0.0042	(0.0023)	(0.0017)
Financial incentives: spline at 24 months out	-0.0047	(0.0021)	(0.0022)
<b>Work-related sanctions</b>			
AFDC waiver/TANF work-related sanctions: implementation	-0.016	(0.016)	(0.0042)
Gradual full-family work-related sanctions: slope	0.00073	(0.00082)	(0.00060)
Add'l effect of immediate full-family sanctions: slope	-0.012	(0.0031)	(0.0018)
Gradual full-family work-related sanctions: spline at 24 months out	-0.0042	(0.0019)	(0.0015)
Add'l effect of imm. full-family sanctions: spline at 24 mths out	0.014	(0.0037)	(0.0025)
Log of maximum benefit for a family of three, past year's average	0.33	(0.20)	(0.091)
Observations	8,415		

Standard errors in parentheses (robust standard errors in column 1).

Note: all models include state fixed effects, a national year spline, state-specific linear trends, common seasonal indicators, and an intercept shift for the implementation of tribal TANF programs. Models are estimated on 165 months of observations in 50 states and the District of Columbia.

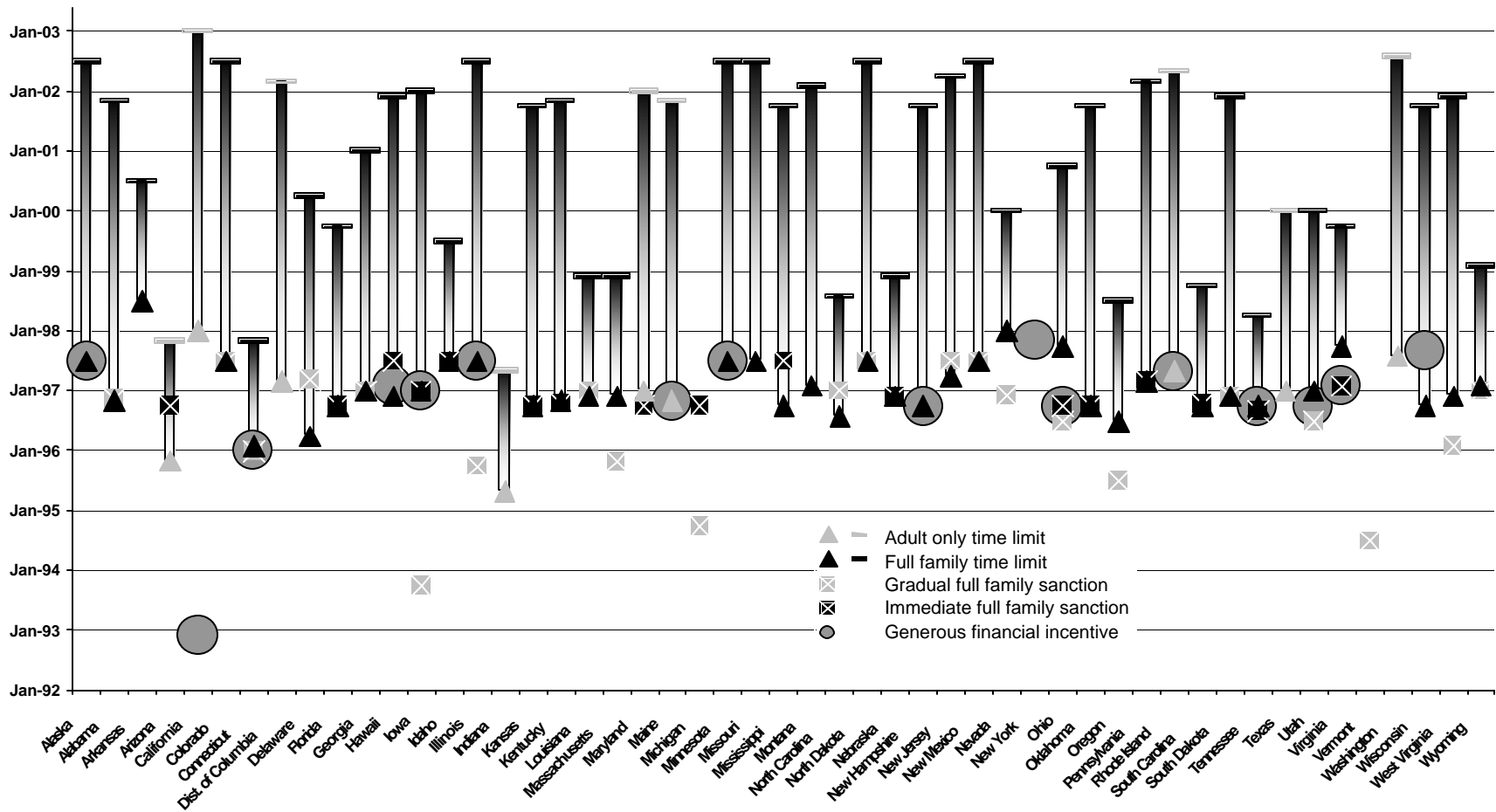


Figure II.1 Timing of State Reforms: Time Limits and Other Policies<sup>11</sup>

<sup>11</sup> The coding shown in this chart is consistent with the coding used in the analysis, which we provide in table form in Table A.3.

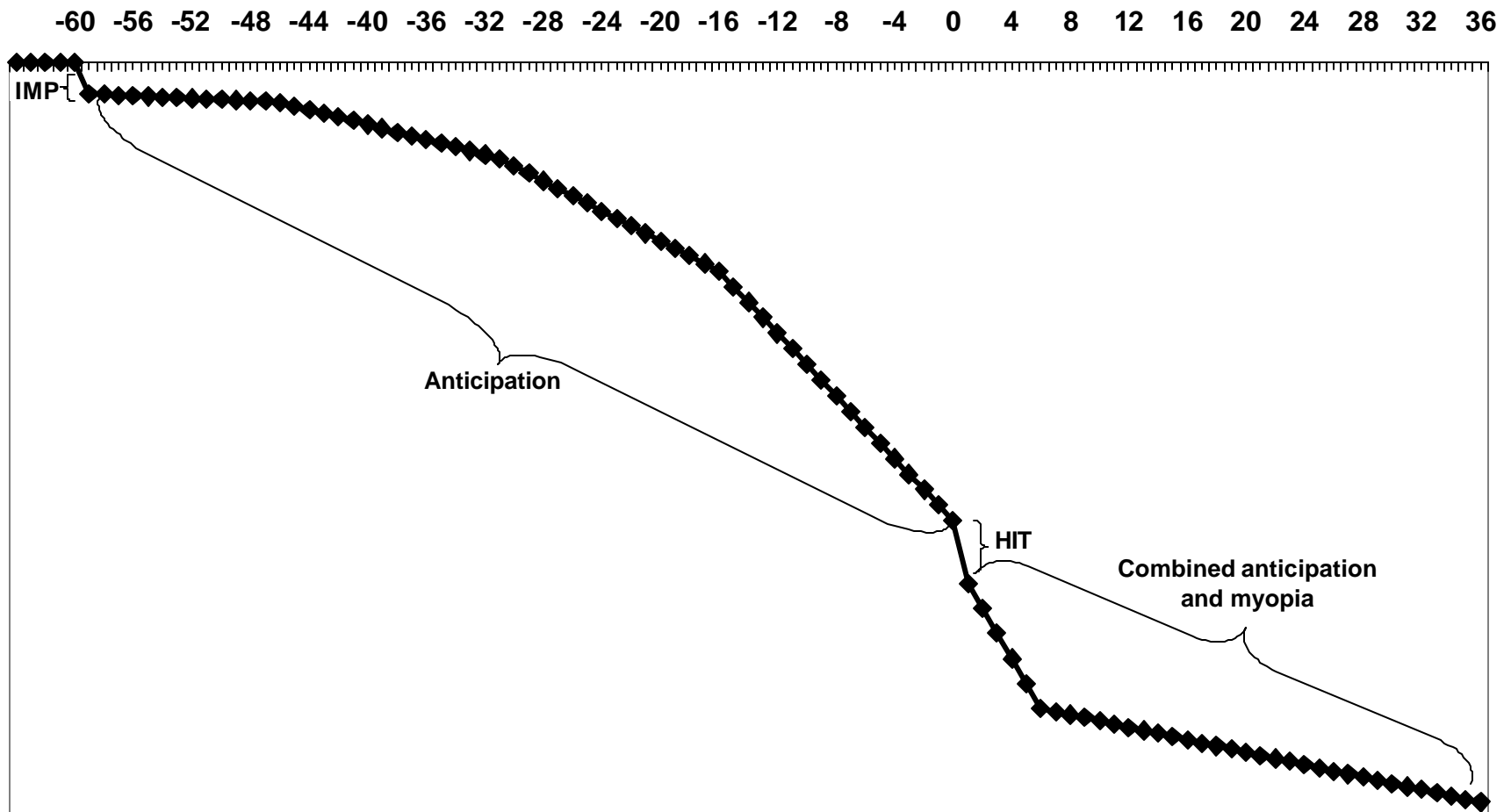


Figure IV.1. Schematic of Time Limits Policy Parameterization (60 month time limit)

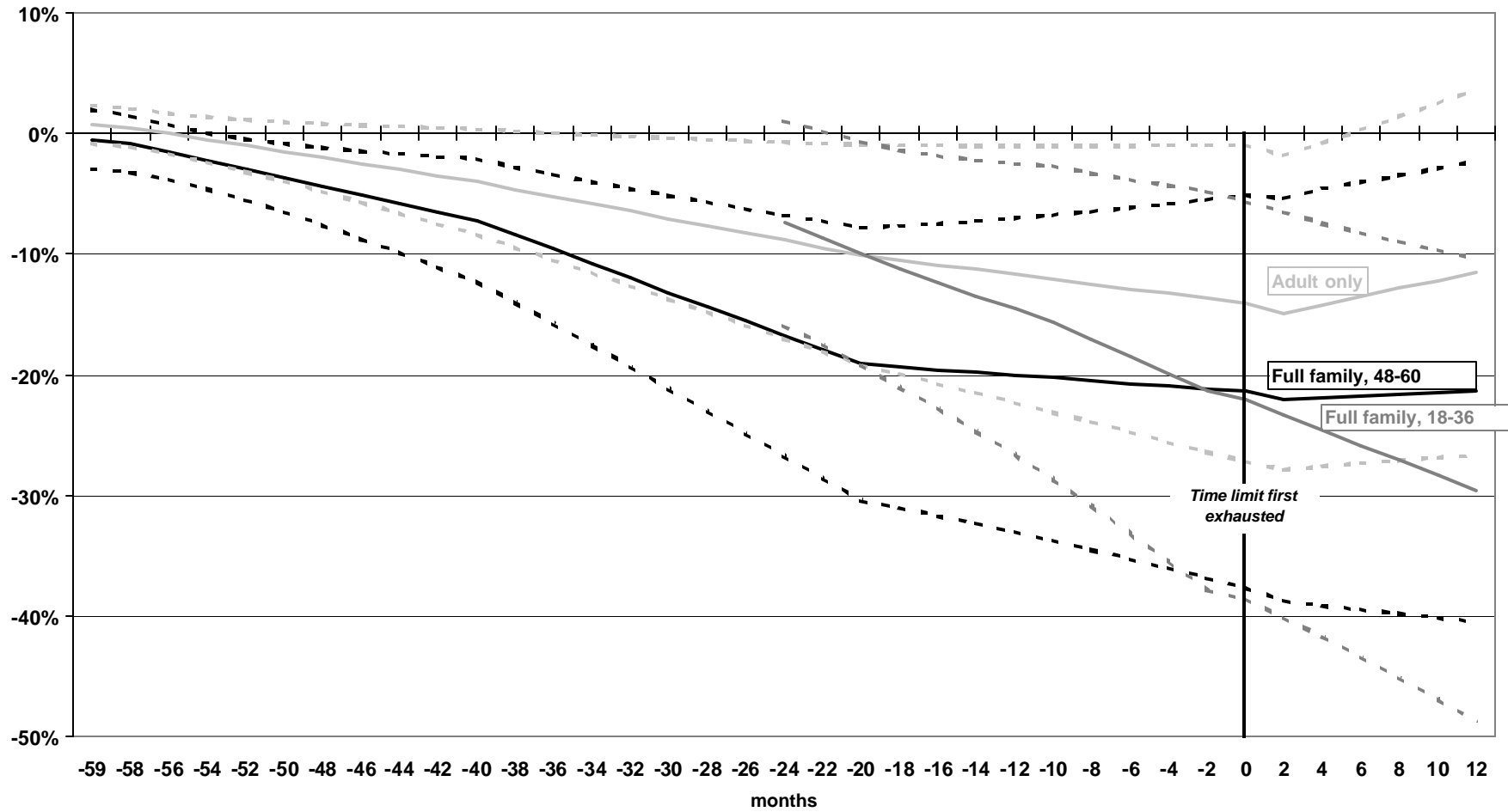


Figure V.1. Impulse Response Functions: Time Limits

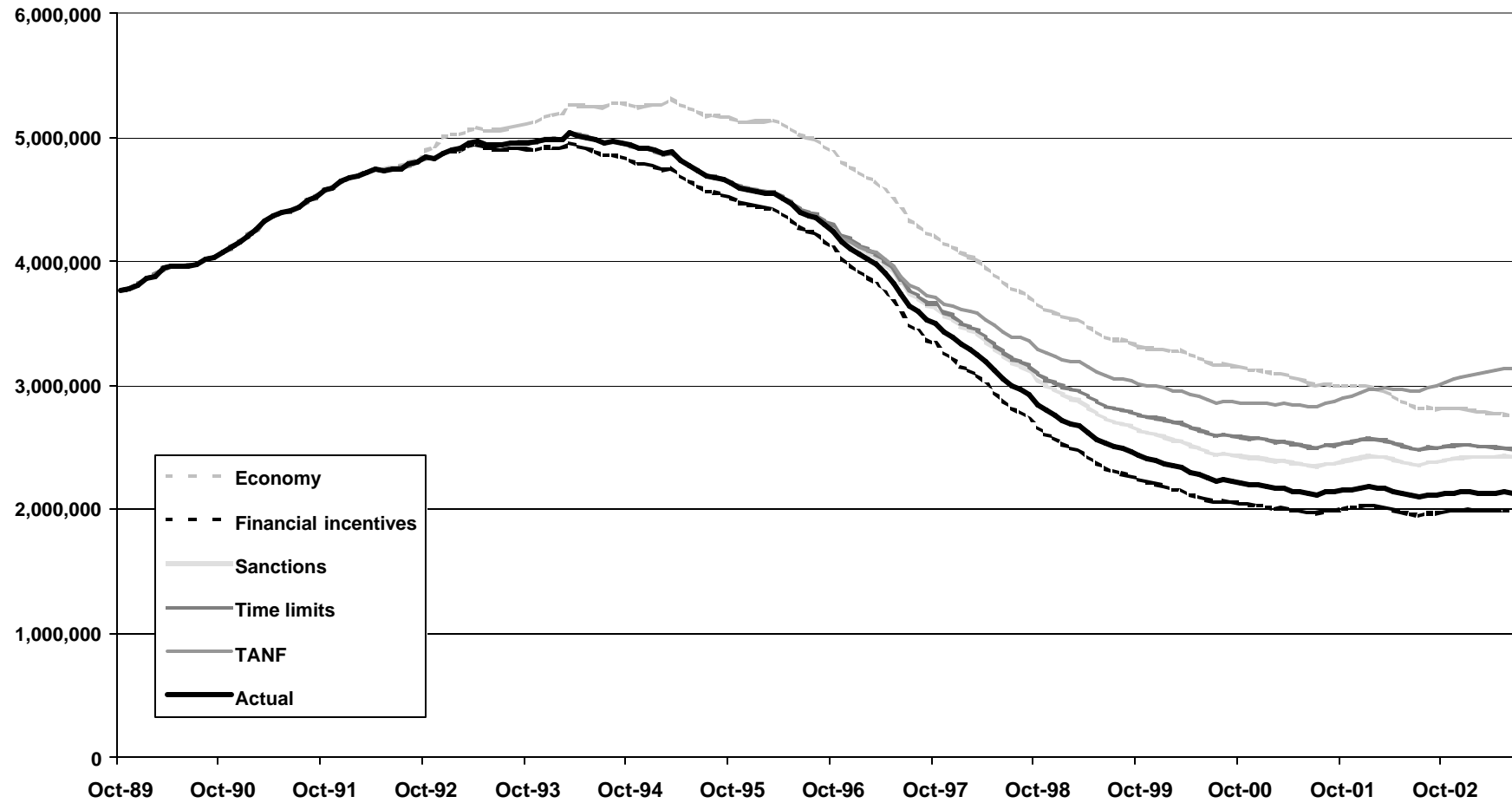


Figure VI.1. Simulated national caseload

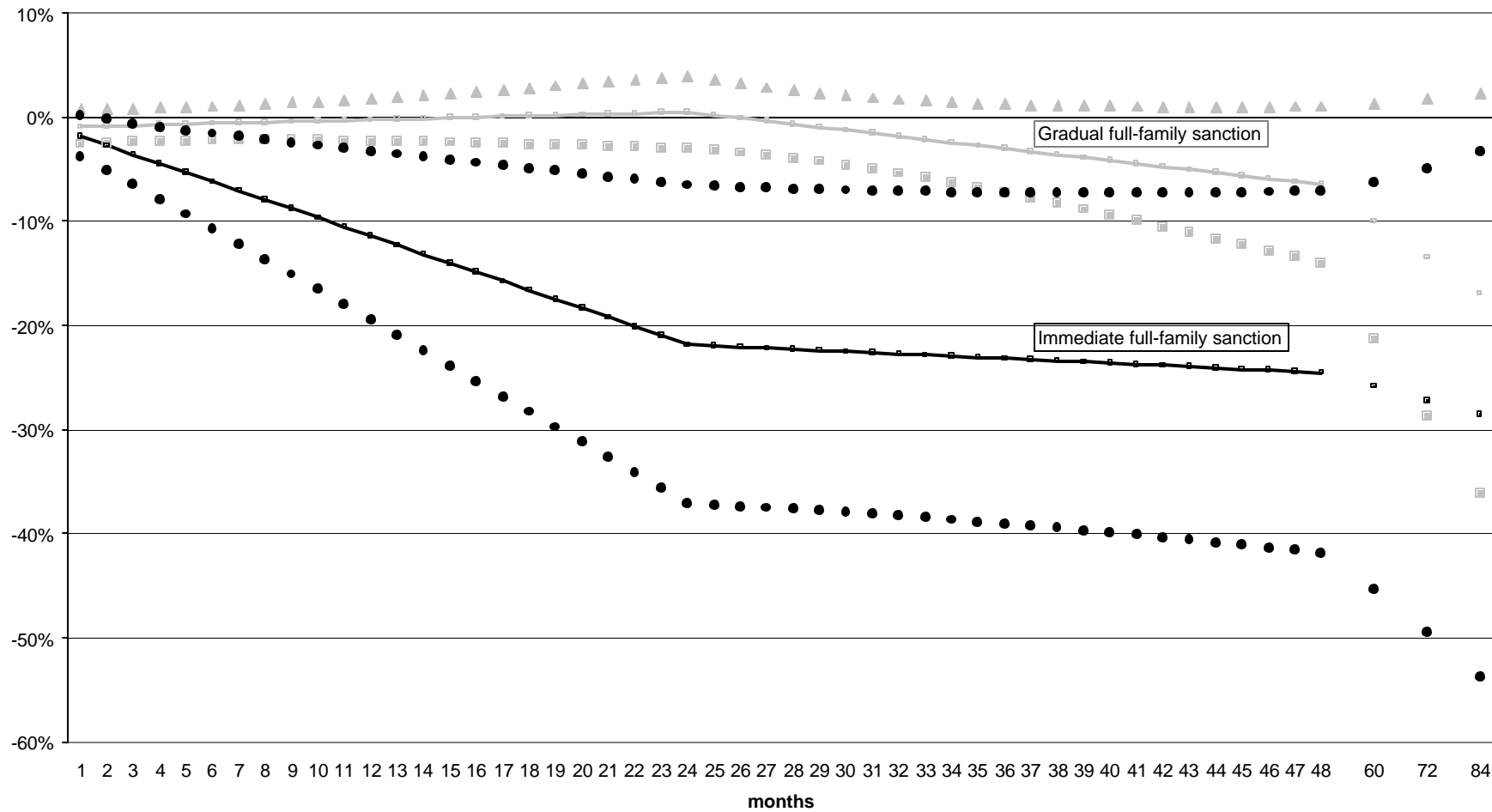


Figure B.1. Impulse Response Functions: Work-Related Sanctions

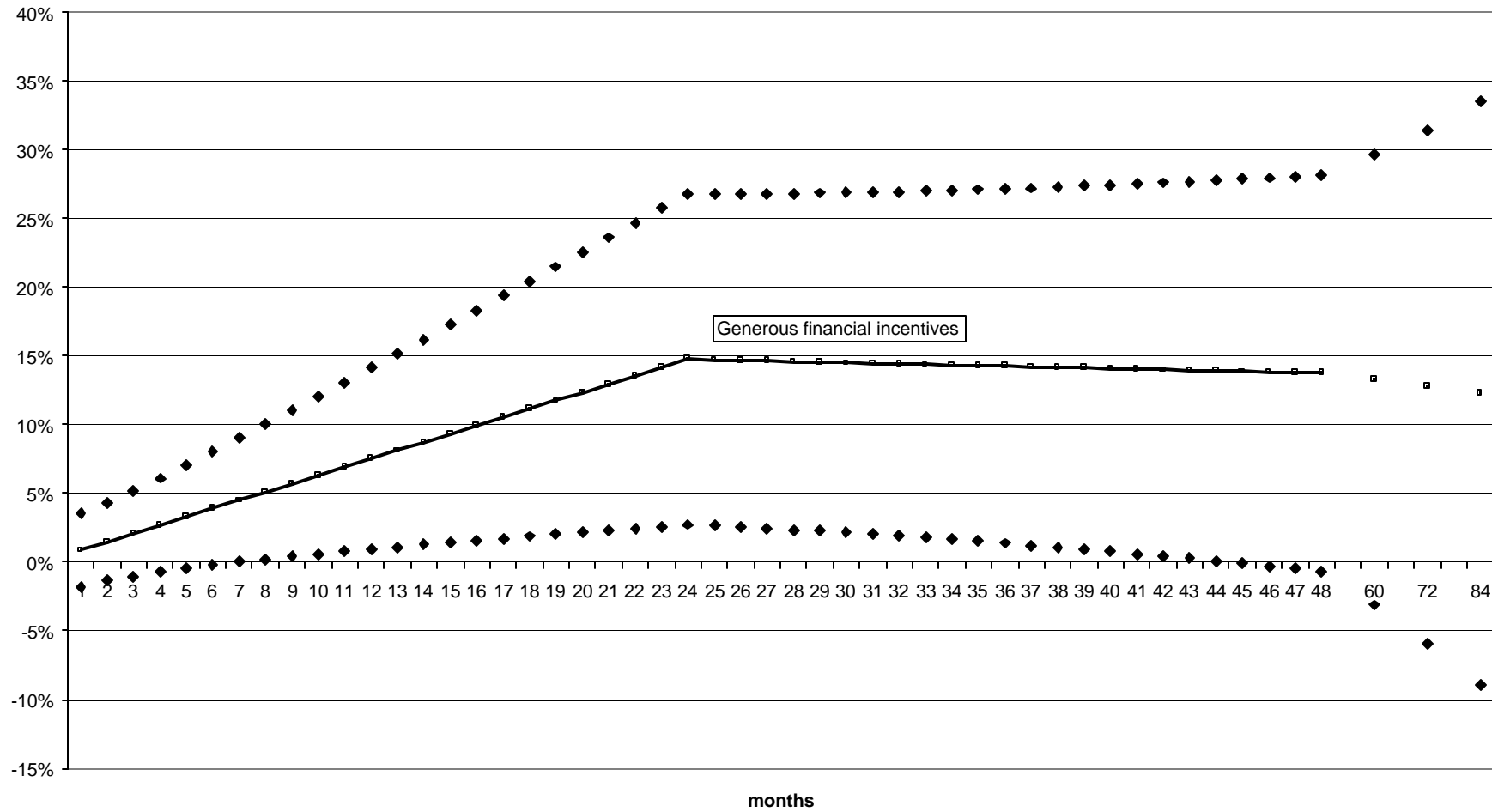
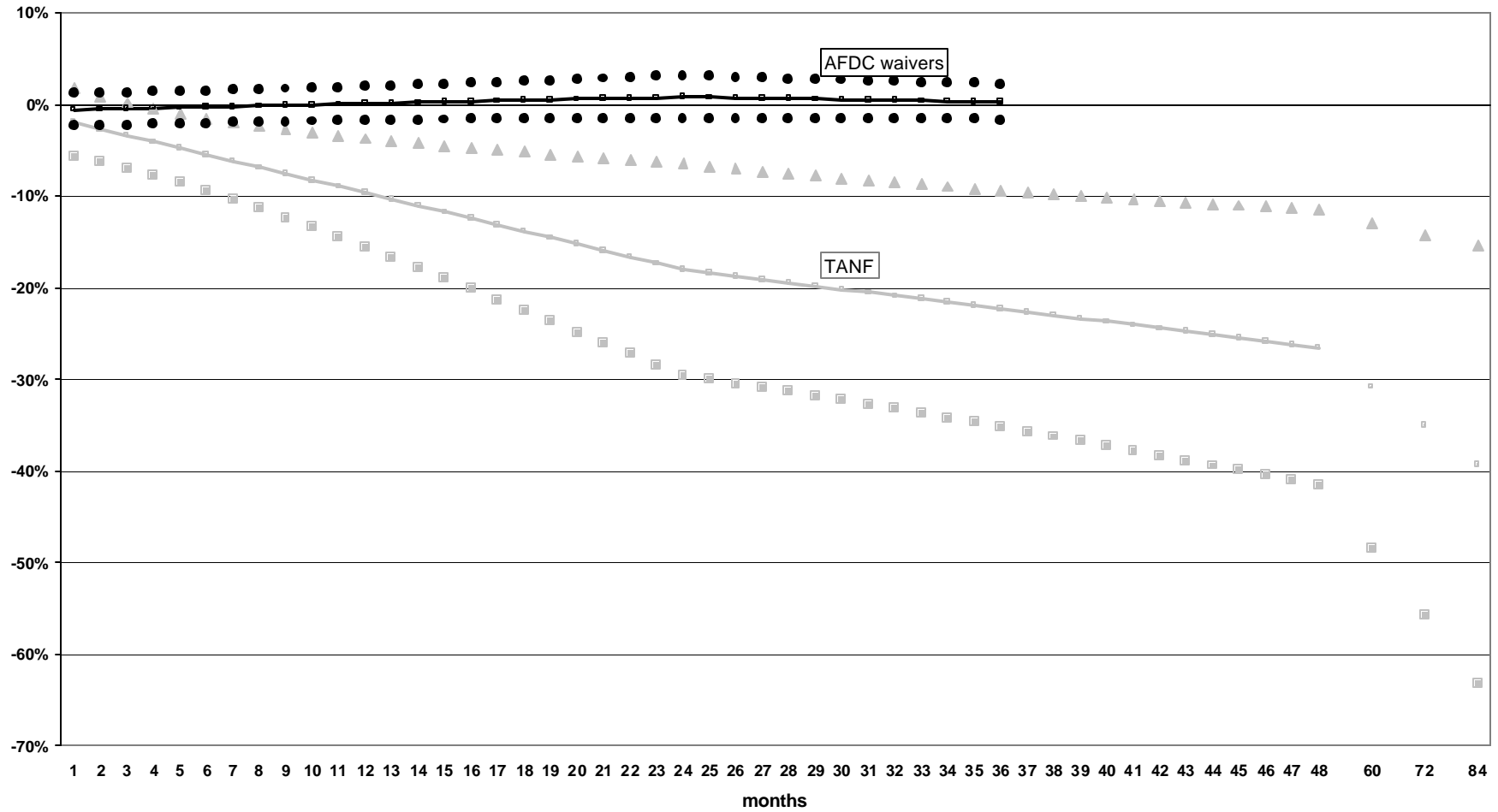


Figure B.2. Impulse Response Functions: Financial Incentives



**Figure B.3. Impulse Response Functions: Residual AFDC waivers and TANF**

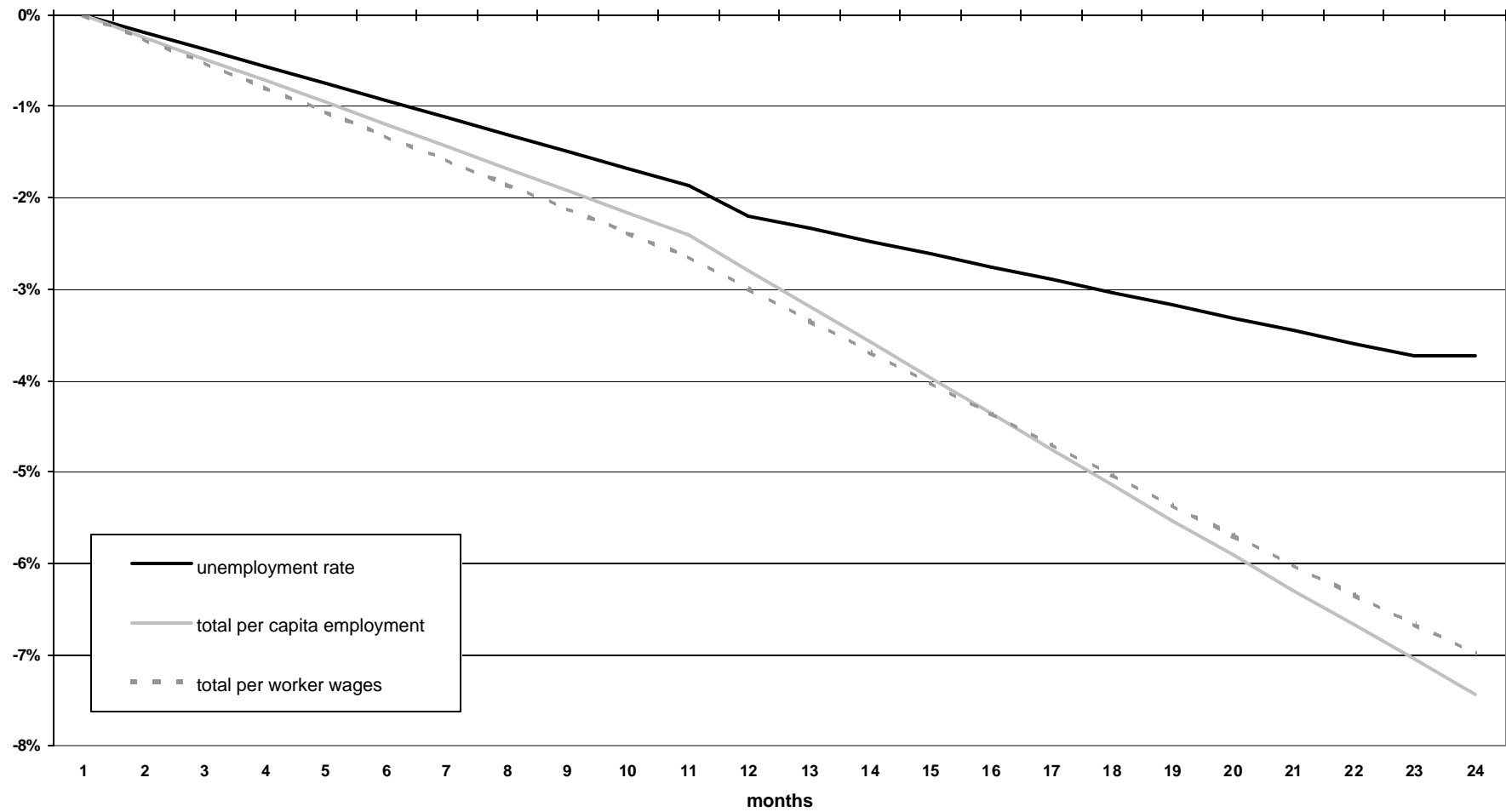


Figure B.4. Impulse Response Functions: Economy