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Accounting for the Decline in AFDC Caseloads:
Welfare Reform or Economic Growth?

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Abstract

Nationwide, AFDC caseloads have decreased by about 18 percent since March 1994, while some states, such as Wisconsin, Indiana, and Oregon, have seen declines of 40 percent or more. Two factors are frequently suggested as possible causes: state-level experiments with welfare reform and strong economic growth. In this paper, we use state-level monthly panel data from 1987 to 1996 to assess the importance of each of these factors by estimating a model of AFDC caseloads as a dynamic function of time-dependent state welfare reform variables (welfare waivers) and economic variables such as per capita employment. Our results from the dynamic model suggest that the decline in per capita AFDC caseloads is attributable largely to the economic growth of states and not to waivers from federal welfare policies. In the 26 states experiencing at least a 20 percent decline in per capita AFDC caseloads between 1993 and 1996, we attribute 78 percent of the decline to business-cycle factors and 6 percent to welfare waivers.

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1. INTRODUCTION

The dramatic recent decline in the number of families receiving Aid to Families with Dependent Children (AFDC) has captured substantial attention in the popular press (e.g., DeParle, 1997; Harris and Havemann, 1997; Milbank and Georges, 1997). Nationwide, AFDC caseloads have decreased by about 18 percent since March of 1994, while some states, such as Wisconsin, Indiana, and Oregon, have seen declines of 40 percent or more. Two factors are frequently suggested as possible causes: state-level experiments with welfare reform and strong economic growth. Accounting for the relative importance of welfare reform and economic growth on AFDC caseloads may foreshadow the potential impact of the recent federal changes in welfare policy resulting from the 1996 passage of the Personal Responsibility and Work Opportunity Reconciliation Act. In this paper, we use state-level monthly panel data to assess the importance of each of these factors by estimating a model of AFDC caseloads as a dynamic function of time-dependent state welfare reform variables (welfare waivers) and economic variables such as per capita employment. Using a variety of empirical specifications, we conclude that although welfare reform has had a modest effect on caseloads, a much larger fraction of the decline is a result of the strength of the economy.

Although it is too soon to expect the new federal restrictions to have visible consequences, many states have experimented for several years with programs that entail similar restrictions under waivers from federal rules governing the AFDC program. For example, Wisconsin received a waiver in November 1993 for its Work Not Welfare demonstration project that imposed both work requirements and time limits on the receipt of AFDC benefits in the affected counties. Meanwhile, the first major waiver for Oregon was approved in July 1992, which expanded work requirements and limited exemptions from the JOBS Program. Subsequent federal waivers have allowed Oregon to further strengthen work requirements and have added time limits and subsidized private sector jobs as well as expanded the availability of child care subsidies, placement assistance, and health insurance to former recipients. Indeed, given the breadth of the state-specific waivers, many states' welfare programs will remain substantially unchanged after implementation of the new federal welfare reform (Blank, 1997a).

Weighing the relative merits of stringent reforms as opposed to economic stimulus as mechanisms to reduce welfare dependency is crucial to evaluating welfare reform. Perhaps surprisingly, few attempts have been made to examine the link between welfare waivers and caseloads. Recently, the U.S. Council of Economic Advisers (1997) employed annual state-level panel data for 1976–1996 to model per capita AFDC recipiency rates as a function of current and one-period lagged unemployment rates and a variety of welfare-waiver dummy variables, while also controlling for AFDC maximum benefit guarantees and unobserved state fixed and time-varying effects. In its preferred estimates, the CEA concludes that economic growth accounts for 44 percent of the decline in AFDC caseloads from 1993 to 1996, while welfare waivers account for 31 percent of the decline. Blank (1997b) also uses annual state-level panel data on AFDC caseloads for 1979–1995. While Blank's primary focus is on explaining the unexpected run-up in AFDC caseloads from 1990 to 1993, she also finds that both the macroeconomy and welfare waivers have had a significant effect on caseload declines. However, Blank also presents evidence that waivers are proxying for "a whole set of changes that occurred in states where waivers were implemented" (p.19). Although Blank's model and data do not permit her to separate these effects, she concludes that waivers are not the primary determinant of changes in caseload levels.

A few previous papers have considered the impact of economic stimuli on caseload levels without examining the concurrent effects of welfare reform.[1] The purpose of most of these studies has been to develop models that can accurately forecast changes in the number of families receiving AFDC

[1]See Congressional Budget Office (1993) for a complete list of these studies. See, also, the related work in Friedlander and Burtless (1995), Gueron and Pauly (1991), and Moffitt (1996).

over time. Not surprisingly given their purpose, they tend to use time series data and focus on a single state, and in some cases on a single city (New York). Most of these models find that as labor market opportunities improve, the aggregate caseload declines.

Hoynes (1996), using microlevel data on individual case duration, finds strong evidence that labor market conditions affect welfare spell duration. While Hoynes's results are compelling, her data do not permit her to explore the potential effects of the current round of welfare reform. To our knowledge, no microlevel data on welfare spell duration exist that are current enough to evaluate the effects of the recent welfare waivers and economic growth, which are of central interest in this study. However, Hoynes's work underscores the importance of controlling for both observed and unobserved cross-sectional heterogeneity.

Unlike the aforementioned studies by the CEA (1997) and Blank (1997b), we use monthly state-level data on per capita AFDC caseloads from 1987 to 1996. We show that the use of annual caseloads masks important short-run dynamics in caseload levels that can more adequately be captured with monthly data. Moreover, because many state welfare waivers were not granted until as recently as 1996, the use of annual data will miss the dynamic adjustment to the reforms. The monthly frequency also alleviates potential bias arising from two sources: the aggregation bias in the annual data resulting from the fact that AFDC eligibility is determined monthly (Hoynes 1996), and, most important, the difficulty of controlling for the lag between the date of approval and the implementation of the waiver provisions with annual data.

We use the monthly unemployment rate and per capita employment interchangeably to measure economic growth, while we categorize welfare waivers into four components: work requirements, time limits, making work more attractive, and parental responsibility. We estimate both static and dynamic models that control for a cubic trend in caseloads along with state-specific fixed and time-varying effects, in addition to economic growth and welfare waivers. The inclusion of time-varying, state-specific effects controls for other variables that are likely to affect caseloads, including changes in both policy and demographic variables. Our dynamic model allows for considerable path-dependence in caseload changes from month to month and also describes the ways in which employment changes over time affect subsequent caseload changes.[2] We are also able to explore the lags between welfare waiver approval and the actual *implementation* of the waivers in ways that previous studies have not been able to do because of their use of static models and annual data.

Our results from the dynamic model suggest that the decline in per capita AFDC caseloads is attributable largely to the economic growth of states and not to waivers from federal welfare policies. For example, in Wisconsin, the state with the largest caseload reduction from 1993 to 1996, we attribute only 11 percent of the decline to welfare waivers but 53 percent to business-cycle factors. In the 26 states experiencing at least a 20 percent decline in per capita AFDC caseloads, we attribute 78 percent of the decline to business-cycle factors and 6 percent to welfare waivers.

2. DATA AND ESTIMATION ISSUES

The data used in the empirical analysis come from four sources. First, we collect state-specific monthly AFDC caseload data for the 1987–1996 federal fiscal years (October 1986 to September 1996) from *Quarterly Public Assistance Statistics*, published by the Office of Family Assistance of the U.S. Department of Health and Human Services (HHS).[3] Using AFDC caseloads as the dependent variable is preferable to the number of AFDC recipients for two reasons. First, number of recipients confounds the number of households receiving AFDC with the within-household fertility behavior. In addition, the

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[2]Using a vector autoregression with monthly data, Blank (1997b) also contends that the monthly employment-caseload relationship follows a highly dynamic process. This portion of her analysis does not consider the effects of welfare reform.

[3]Our preference would have been to use county-level caseloads, so that we could capture intrastate variation in economic fluctuations. However, to our knowledge these data do not exist in a centralized format for years after 1992.

statistic that has received the most press and political attention lately is the number of cases, rather than the number of persons per case.

We download state-specific monthly employment levels and unemployment rates from the U.S. Bureau of Labor Statistics *Most Requested Series* Web page.[4] It is not obvious which measure is better at capturing the effect of the business cycle on caseloads. However, Hoynes (1996) argues that unemployment rates confound movements in labor supply and demand while employment better captures demand conditions. This, of course, rests on the assumption that workers are off of their notional labor supply curves, making employment demand-determined. For robustness purposes, we estimate models using each measure interchangeably.

To adjust caseloads and employment levels to reflect differing state populations, we download annual population figures from the U.S. Census Bureau's *State Population Estimates* Web page. Although caseloads and employment are observed at monthly frequencies, versus annual frequencies for population, we deflate caseloads and employment by year-specific population under the assumption that it is cross-sectional differences rather than time-series differences in population that are likely to affect caseloads and employment.[5]

We rely primarily on information from the HHS Web site for the approval dates and types of welfare waivers in each state (HHS, 1996a). HHS classifies the state-specific waivers into five categories: (1) those that require work, (2) those that impose time limits on benefits, (3) those that provide work incentives (i.e., that "make work pay"), (4) those that are related to child support enforcement, and (5) those that encourage parental responsibility (e.g., requirements that children in AFDC families regularly attend school and get health checkups, or a so-called "family cap," which does

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[4]It is possible that monthly employment and caseload data are more noisy than annual data. However, we believe that the benefits of being able to avoid the aggregation bias in annual caseloads, given that AFDC eligibility is determined monthly, and of being able to capture the lag between waiver approval and implementation, given how recently most states's waivers have been approved, outweigh the possible noise in the high-frequency data.

[5]Our results are qualitatively the same when we do not deflate caseloads by state population.

not allow benefits to increase when another child is born in a family receiving assistance). We adopt the HHS classification with one modification—we combine waivers related to child support enforcement with those encouraging parental responsibility. The waiver variables take on values of zero until the month that the waiver is approved by HHS (not the month in which it was requested), at which time the waiver is set equal to one. Each of the waivers, with the exception of making work more attractive, is expected to reduce welfare caseloads.[6] Making work more attractive could actually lead to increased caseloads because these waivers include provisions such as increasing the amount of income AFDC recipients can earn without losing benefits. We also identify the states that implemented waivers statewide separately from those that had experimental programs affecting only small portions of the state. The effects of this distinction are addressed later in this paper. The Appendix contains details on the coding of the waivers, along with the various approval dates.

Preliminary Data Investigation

We begin with an exploratory look at trends for a select group of states in per capita caseloads and per capita employment before and after enactment of welfare reform. The four types of waivers are highly correlated, so throughout this section we focus attention on states that received approval for work-requirement waivers and compare them with states that did not receive such a waiver. Figure 1 plots state per capita welfare caseload in September 1996 (the last period in our sample) against state per capita welfare caseload in September 1992. The filled dots represent states that enacted a work-requirement waiver during this period, and the empty circles represent states that did not enact such a waiver. Based on a simple comparison of means, caseload changes across the two types of states do not appear much

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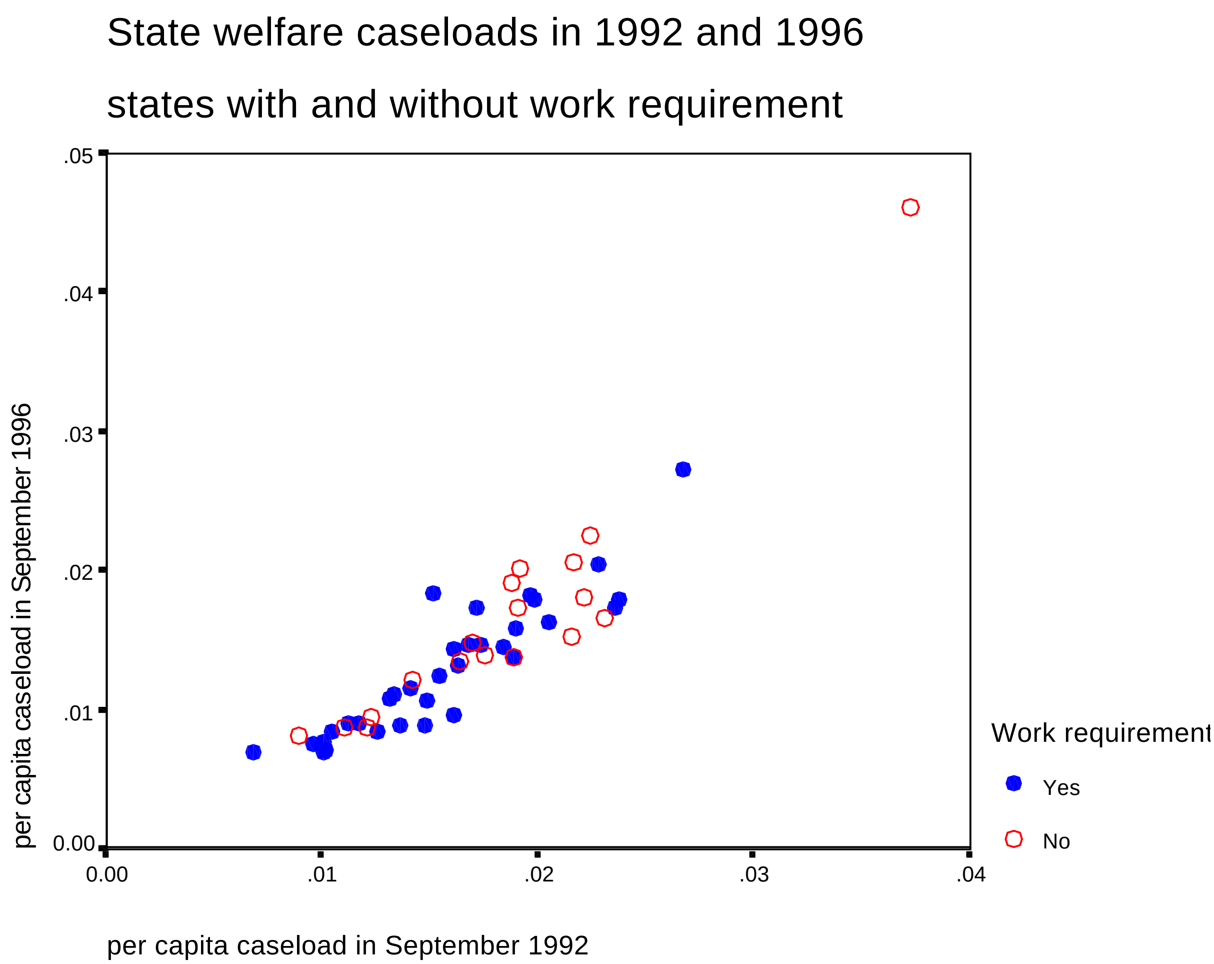
[6]Moffitt (1996) has argued that certain types of work-requirement waivers could actually increase caseloads by enticing some people to apply for AFDC in order to take advantage of the job training and job placement assistance. While this is certainly a possibility, it is unlikely that this would result in a long-term increase. In addition, other components of the work-requirement waivers, such as sanctions for failure to participate, could offset this effect.

Figure 1


State welfare caseloads in 1992 and 1996
states with and without work requirement
.05
.04
.03
.02
.01
0.00
per capita caseload in September 1996
0.00
.01
.02
.03
.04
per capita caseload in September 1992
Work requirement
Yes
No

different. The mean decrease in caseloads in nonwaiver states was 12.4 percent, while the mean waiver-state caseload decrease was 18 percent, but the difference is not statistically significant at conventional levels. Nonetheless, there is some evidence of a relationship between caseload changes and welfare waivers. Seven of the eight states in which caseloads fell the most are waiver states, but just four of the nine states with caseload gains are waiver states.

We next consider the possibility that welfare waiver states have experienced more rapid economic growth, on average, than have nonwaiver states. As a preliminary exploration of this possibility, we plot in Figure 2 the per capita employment in September 1996 against per capita employment in September 1992 for waiver states and nonwaiver states. Again, the difference is not statistically significant; employment per capita in waiver states grew on average by 3.5 percent from 1992 to 1996, compared to a 3.2 percent growth in nonwaiver states.

The prior discussion suggests that only weak relationships exist between welfare waiver imposition and recent changes in caseloads or employment. We do, however, observe large differences in caseload reductions across the states and among each type of state.

For instance, caseload changes in waiver states range from a decrease of over 40 percent to an increase of 22 percent over the 4-year period. Figure 3 suggests that a substantial determinant of caseload reduction among all states, regardless of waiver status, is economic growth. Among both waiver and nonwaiver states, the states whose economies grew the most also tended to have the largest caseload reduction (this relationship is significant at about the 1 percent level, and is significant at the 8 percent level when the two outlier states are eliminated). While the five waiver states with the smallest employment growth saw caseloads decline by an average of 6 percent, the five waiver states with the largest employment growth averaged a 22 percent decline in caseloads over the period in question. Therefore, it appears that caseload reductions are a function more of employment growth than simply of welfare reform.

Figure 2

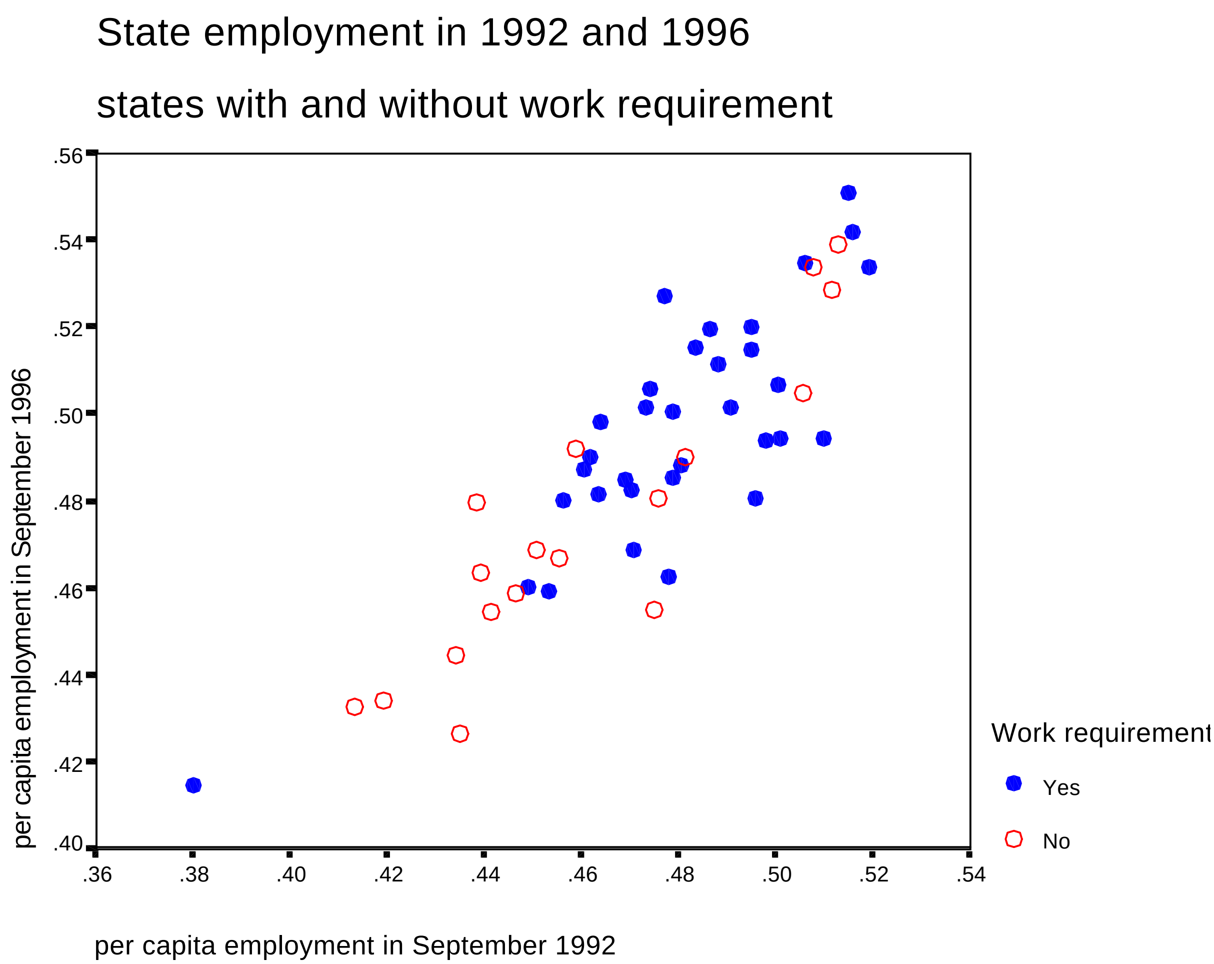
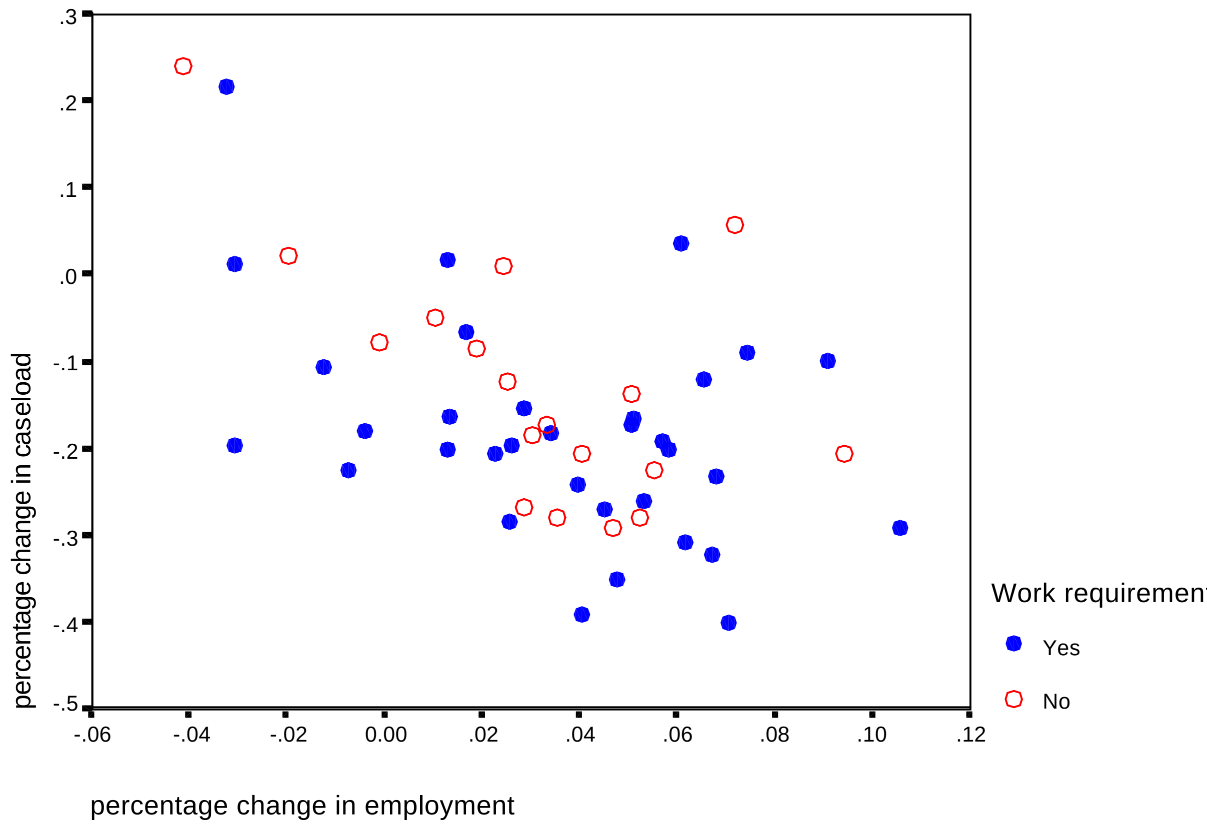

State employment in 1992 and 1996
states with and without work requirement
per capita employment in September 1996
.56
.54
.52
.50
.48
.46
.44
.42
.40
.36
.38
.40
.42
.44
.46
.48
.50
.52
.54
per capita employment in September 1992
Work requirement
Yes
No

Figure 3

Percentage change in caseloads and employment

in states with and without work requirements



Estimation Issues

To move beyond simple graphical correlations between economic growth, welfare waivers, and AFDC caseloads, we estimate an econometric model to control for other possible determinants of caseloads. To this end, we permit national economic and political trends to have an effect on AFDC caseloads by specifying the trend as a cubic polynomial in time t (t = 1,..,120). This allows enough nonlinearity in the trend to capture the fall (1987–1990), rise (1990–1993), and subsequent fall (1993–1996) in aggregate caseloads over the sample period.[7] In addition, we control for both time-invariant and time-varying state-specific effects; this captures not only fixed unobserved state-specific propensities to take-up welfare but also slow-moving state-specific trends in demographics such as fertility rates, marital status, and migration that are not available in monthly intervals.[8] Finally, because our monthly measures of caseloads and economic growth are not seasonally adjusted, we append month-of-year dummies to the empirical model to capture seasonal fluctuations in caseloads and employment.[9]

The static model of AFDC caseloads for each state i (i = 1,..,51) in month t is thus

$$C_{it} = \alpha E_{it} + W_{it}\beta + \gamma_1 t + \gamma_2 t^2 + \gamma_3 t^3 + \delta_i + \delta_i t + \eta_j + \varepsilon_{it}, \qquad \textbf{(1)}$$

where C_{it} is the natural log of per capita AFDC caseloads, E_{it} is the measure of economic growth (natural log of employment per capita or the unemployment rate), W_{it} is the vector of welfare waivers, t is a trend,

[7]Preliminary models included 119 month dummy variables, the coefficients of which when plotted against time suggest a cubic trend. We use the cubic trend rather than the month dummies to save on the number of parameters to estimate. The early results were not sensitive to use of the trend as opposed to month dummies.

[8]Failing to control for state-specific demographics, such as fertility rates, that likely affect welfare caseloads may lead to an omitted variable bias if they are not captured by either the state fixed effects or state-specific trends. The sensitivity section below suggests that omitting these variables does not affect our estimates of either the business cycle or welfare waivers on caseloads.

[9]We were initially concerned that a common set of month dummies for all states would not adequately capture seasonal fluctuations in employment and caseloads. Hence, we tried a number of specifications in which each state had its own unique set of month dummies. We found that these specifications yielded almost identical results to those with a common set of month effects, so to conserve on the number of parameters to estimate, we chose to use the more parsimonious specification.

δ_i is the time-invariant state-specific effect, $\delta_i t$ is the time-varying state-specific effect, η_j (j = 2,...,12) is the month-of-year dummy variable, and ε_{it} is a random error that permits conditional heteroskedasticity in caseloads.

Since we are using data at the relatively high frequency of monthly intervals, nonstationarity in AFDC caseloads is likely to be a problem. Indeed, preliminary estimates from a dynamic fixed-effect regression model (not shown here) produced a coefficient on the lagged dependent variable of 0.98 (s.e.=0.005).[10] Consequently, we first-difference the regression model in equation (1) to make caseloads difference stationary, yielding the first-difference estimating equation

$$\Delta \boldsymbol{C}_{it} = \alpha \Delta \boldsymbol{E}_{it} + \Delta \boldsymbol{W}_{it} \beta + \tilde{\gamma}_1 + \tilde{\gamma}_2 \boldsymbol{t} + \tilde{\gamma}_3 \boldsymbol{t}^2 + \delta_i + \Delta\eta_j + \Delta\varepsilon_{it}, \tag{2}$$

where $\tilde{\gamma}_1 = \gamma_1 - \gamma_2 + \gamma_3, \tilde{\gamma}_2 = 2\gamma_2 - 3\gamma_3,$ and $\tilde{\gamma}_3 = 3\gamma_3$. Notice that in equation (2) the time-invariant state effects drop out of the model; however, the effect of state-specific trends is still captured by δ_i, which we parameterize with 50 state (including the District of Columbia) dummy variables (Alabama is omitted for identification). The coefficients on the welfare waivers (β) now capture the short-run impact of welfare reform and thus are likely to understate the actual effects of the waivers on caseloads. In the next section, we suggest a method to capture the long-run impact of waivers (and, for that matter, of business cycles as well) in a dynamic model of caseloads.

A key issue for model specification is the potential endogeneity of the welfare waivers. States with high AFDC caseloads may be more likely to request federal welfare waivers, resulting in an identification problem, since the direction of causality might go in both directions. This potential endogeneity is highlighted by the surge in welfare-waiver requests occurring after the general 1990–1993 increase in caseloads. Fortunately, we believe that endogeneity is not a problem in our model. Even if

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[10]The dynamic fixed-effect model here is not likely to suffer from the so-called Nickell (1981) bias, that is, the bias that arises from the correlation between the lagged dependent variable and the model's error term. Nickell shows that the bias goes to zero as T gets large, and because T = 120, the bias is expected to be ignorable.

rising caseloads are the impetus for waiver requests, a lag may occur before such requests are made. Therefore, waiver requests are not likely to be correlated with contemporaneous caseloads. In our case, there is even less reason to be concerned because we define the existence of waivers on the basis of the month that the waiver was *granted* by the federal government, not the month that the waiver was *requested*. The time between request date and grant date ranges from as short as 2 weeks in the case of the District of Columbia to over 2 years in the case of California, with the average length of delay being around 1 year. Consequently, our measures of welfare waivers can be considered to be predetermined.

Despite the foregoing argument, we attempt to verify that waivers are not endogenous. Specifically, we calculate the 1-year change in per capita caseloads in the year immediately preceding waiver approval for each state that imposed a work requirement waiver and compare this with the 1-year change in per capita caseloads during the same period *for states that did not yet have such a waiver approved*.[11] We observe that the pre-waiver change in per capita caseloads is less than 0.4 percent different from that in the states that had not yet imposed a welfare waiver. Moreover, this difference is not statistically significant at any reasonable threshold.[12] Thus, we are convinced that our results are not likely to be sensitive to the potential endogeneity of the welfare waivers.[13]

[11]We perform the analogous exercise with each of the other types of waivers and find virtually identical results.

[12]Similarly, the CEA (1997) finds that “waiver states did not experience a larger-than-average increase in their welfare rolls between 1989 and 1993” (p.9).

[13]In a further attempt to address the endogeneity of welfare waivers, we searched for variables that could be used as instruments. The difficulty here is in the paucity of variables available on a monthly basis that might be appropriate. We considered two annually observed variables, the time-adjusted “real” Americans for Democratic Action (ADA) voting score for the degree of liberalness of the state’s Congressional representatives (constructed by Groseclose, Levitt, and Snyder (1996) and generously provided by Tim Groseclose) and the percentage of the state’s Congressional representatives who are members of the Republican party. We estimated two instrumental-variables regression models, one in which the two instruments are used along with the time-limit and work-pays waivers, and one in which all four waivers are included. The latter model is identified by using the two instruments along with interactions of the two instruments with employment per capita. Not surprisingly, these parameter estimates were both distorted and highly inefficient because of the inconsistent frequency of observations between regressors and instruments.

Unlike the studies by the CEA (1997) and Blank (1997b), our model does not include welfare benefit levels as a regressor both because benefits vary only at an annual rate, as opposed to our monthly data, and because of the lack of suitable instruments that could deal with the possible simultaneity with caseloads. It is sensible to think that while benefit levels might explain welfare caseloads, the size of the caseload might also affect the benefit level. Indeed, the simultaneity between welfare benefits and recipiency has been shown by Shroder (1995) and Gramlich and Laren (1984), among others. But we also have reason to believe that our omission of benefit levels does not affect our results in this paper. While the CEA study shows that annual deviations from state trends in benefits and welfare recipiency are significantly correlated, we find that annual changes in welfare benefits are almost completely uncorrelated with *monthly* changes in welfare caseloads (the correlation is less than -0.01).

3. RESULTS

Table 1 presents ordinary least squares (OLS) estimates for four different specifications of equation (2). The specifications differ in their treatment of the welfare waivers, and each model is estimated twice, first with the log of employment per capita and then with the unemployment rate as the measure of economic growth.

Full-State Waivers

Specification (1) contains the results for the static first-difference model where the only welfare waivers modeled are those that apply statewide. The table reveals conflicting impacts of economic growth on welfare caseloads. Employment per capita has no statistical effect on welfare caseloads, while the unemployment rate has a significantly positive effect. It is not clear a priori why the unemployment rate is a contemporaneously important determinant of AFDC caseloads but per capita employment is not. One possibility is that movements in per capita employment capture populations that are less likely to

TABLE 1

First-Difference Estimates of Business Cycles and State Welfare Waivers on State-Specific Monthly AFDC Caseloads†

	(1)		(2)		(3)		(4)	
Employment/cap	2.925		2.943		2.904		2.938	
	(3.425)		(3.424)		(3.426)		(3.425)	
Unemployment rate		0.180***		0.179***		0.179***		0.179***
		(0.049)		(0.049)		(0.049)		(0.049)
Work required	-0.326	-0.328	-0.268	-0.276	-0.310	-0.319	-0.312	-0.314
	(0.240)	(0.238)	(0.245)	(0.243)	(0.259)	(0.257)	(0.243)	(0.243)
Time limit	-0.513**	-0.520**	-0.122	-0.127	-0.437*	-0.449*	-0.427	-0.439
	(0.253)	(0.253)	(0.236)	(0.235)	(0.259)	(0.259)	(0.263)	(0.264)
Work pays	0.408*	0.394*	0.140	0.149	0.334	0.331	0.300	0.294
	(0.225)	(0.232)	(0.246)	(0.246)	(0.259)	(0.259)	(0.259)	(0.260)
Responsibility	0.098	0.129	-0.132	-0.102	0.017	0.051	0.118	0.149
	(0.224)	(0.227)	(0.218)	(0.219)	(0.225)	(0.227)	(0.239)	(0.243)
Partial-state work					-0.163	-0.181	1.076	1.039
requirement					(0.255)	(0.249)	(1.381)	(1.347)
Partial-state time limit					0.593	0.619	1.884	1.819
					(0.416)	(0.406)	(1.862)	(1.816)
Partial-state work pays					-0.165	-0.120	-2.150	-2.019
					(0.286)	(0.285)	(1.433)	(1.425)
Partial-state responsibility					-0.498	-0.495	-0.126	-0.088
					(0.322)	(0.326)	(0.973)	(0.953)
Adjusted R^2	0.226	0.228	0.226	0.227	0.226	0.227	0.226	0.227

† All coefficients are multiplied by 100. Heteroskedasticity-robust standard errors are in parentheses. Each regression controls for a constant, a quadratic trend, 11 month-of-year dummies, and 50 state dummies. The number of observations used in estimation equals 6,069 (N=51; T=119). Specification (1) only permits full-state waivers; specification (2) makes no distinction between full- and partial-state waivers; specification (3) distinguishes full-state from partial-state waivers, giving equal weight to each partial-state waiver; specification (4) gives unequal weight to partial-state waivers depending on the fraction of the state's AFDC population covered by the waiver.

* = significant at the 10 percent level; ** = significant at the 5 percent level; *** = significant at the 1 percent level

participate in welfare programs, while unemployment rates may better capture labor-market opportunities of the poor. Another possibility, which is discussed further below, is that this model does not capture the dynamics of the labor market.

Three of the four full-state welfare waivers have the expected qualitative impact on AFDC caseloads, and two of the four waivers—time limits and making work pay—having a statistically significant effect. Interestingly, though, these two policies have the effect of canceling each other out. States with welfare time limits, but no other waiver, can expect about a 0.5 percent reduction in welfare caseloads in the month the waiver is granted. On the contrary, states with a work-pays waiver, but no other waiver, can expect a 0.4 percent *increase* in caseloads. Hence, states with both provisions, but not work or responsibility requirements, are likely to see little change in AFDC caseloads. Combining all four full-state welfare waivers, jointly significant at the 8 percent level using a Wald test, yields about a 0.34 percent short-run reduction in AFDC caseloads.

<u>Full- and Partial-State Waivers</u>

In specifications (2)–(4) in Table 1, we extend our analysis to include the impact of waivers that affected only a portion of caseloads in a state. In specification (2), we make no distinction between full- and partial-state waivers and note that the cumulative impact of all four welfare waivers is comparable to specification (1); that is, states with all four waivers can expect about a 0.39 percent short-run reduction in caseloads. However, none of the waiver coefficients are statistically significant, suggesting that a more appropriate approach is one that permits the full-state and partial-state waivers to have independent influences on caseloads.

Specifications (3) and (4) report results from models that separately identify full- and partial-state waivers, where specification (3) gives equal weight to each partial-state waiver and (4) gives unequal weight to partial-state waivers depending on the fraction of the state's AFDC population covered by each waiver. It is clear from model (3) that the statistically zero results for time limits and work pays in model (2) are due to the offsetting positive and negative effects of the corresponding partial-state time-limit and work-pays waivers. The results from the unequal weight model in specification (4) are less clear as the partial-state waivers yield untenable coefficients, possibly due to the fact that our weighting scheme is time-invariant whereas welfare populations are likely changing over time in these demonstration counties. However, regardless of whether the partial-state waivers are given equal or unequal weight, they do not significantly affect per capita AFDC caseloads; hence, in the remainder of the analysis we restrict attention to full-state waivers alone.

Dynamic Determination of Welfare Caseloads

Although the impact of economic growth on AFDC caseloads is robust across the four specifications in Table 1, a remaining puzzle is the inconsistent impact of the business cycle on caseloads depending on whether the cycle is measured by employment per capita or the unemployment rate. In this section we explore the possibility that this inconsistency might arise from misspecified business-cycle dynamics.

In Table 2 we offer a preliminary look at business-cycle dynamics by replacing equation (1) with

$$C_{it} = \alpha_1 E_{it-1} + \alpha_2 (E_{it} - E_{it-1}) + W_{it}\beta + \gamma_1 t + \gamma_2 t^2 + \gamma_3 t^3 + \delta_i + \delta_i t + \eta_j + \varepsilon_{it}, \qquad \textbf{(3)}$$

where α_1 is the long-run effect of the business cycle on caseloads and α_2 is the short-run effect. As before, we estimate equation (3) in first differences. The two models in Table 2 are now more harmonious in that a clear long-run effect of the business cycle is identified in both the employment per capita and unemployment rate specifications; however, only the unemployment rate reveals a short-run effect. In the long-run, a 1 percent increase in monthly employment per capita leads to a 0.186 percent reduction in monthly AFDC caseloads; in contrast, a 1 percentage point decrease in the monthly unemployment rate leads to a 0.6 percent reduction in monthly caseloads. Evaluated at the mean of 5.82 percent, a 1 percentage point decrease in the unemployment rate is a 17 percent reduction in the

TABLE 2

Estimates of Short-Run and Long-Run Business Cycles and Full-State Welfare Waivers on State-Specific Monthly AFDC Caseloads

Employment/Cap	5.027 (3.406)	
Employment/Cap (t-1)	-18.612*** (4.019)	
Unemployment rate		0.178*** (0.049)
Unemployment rate (t-1)		0.604*** (0.073)
Work requirement	-0.332 (0.223)	-0.344 (0.226)
Time limit	-0.483* (0.249)	-0.566** (0.248)
Work pays	0.450* (0.233)	0.434* (0.229)
Responsibility	0.088 (0.221)	0.161 (0.221)
Adjusted R^2	0.241	0.239

† All coefficients are multiplied by 100. Heteroskedasticity-robust standard errors are in parentheses. Each regression controls for a constant, a quadratic trend, 11 month-of-year dummies, and 50 state dummies. The number of observations used in estimation equals 6,069 (N=51; T=119).
* = significant at the 10 percent level; ** = significant at the 5 percent level; *** = significant at the 1 percent level.

unemployment rate, indicating that a 1 percent decrease in the unemployment rate leads to a 0.102 percent reduction in monthly AFDC caseloads. This suggests that employment per capita has a larger long-run effect on caseloads than does the unemployment rate. The impacts of full-state welfare waivers, meanwhile, are largely unchanged from specification (1) in Table 1; namely, the effects of time-limit and work-pays waivers are largely offsetting, and the cumulative effect of all four waivers is for a 0.3 percent reduction in AFDC caseloads.

The dynamic model in Table 2 is limiting in several respects. First, it forces the long-run business-cycle effect into a single coefficient when in fact there may be a more interesting pattern of dynamics across several periods. Second, the model ignores the possibility that even after controlling for heterogeneity in the form of state-specific fixed and time-varying effects, previous AFDC caseloads may have a direct impact on future caseloads, i.e., caseloads may sluggishly adjust to changing economic and political conditions. Finally, the models thus far have ignored any dynamic impact of welfare waivers. In most cases, a lag occurs between the point at which a welfare waiver is approved and the time at which it is actually implemented. Since this lag has not been incorporated in our previous models, the estimated effect of the welfare waivers presented so far is probably biased downward.

We address these problems in the model presented in Table 3. This table contains the OLS results from a more fully dynamic model of welfare caseloads, which captures state-dependence of caseloads by including lagged values of the dependent variable. It also captures the short-run and long-run business-cycle effects by including lags of employment per capita, and the implementation lag in the waivers by appending variables indicating the number of months since the waiver has been approved.[14] The lag lengths on caseloads and employment per capita were determined by iteratively estimating the

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[14]Thanks to David Card for suggesting this idea. Including months since approval allows the effect of waivers to grow continually over time, which is unrealistic. However, we do not wish to arbitrarily impose a limit on the length of the implementation lag, so we do not restrict this variable. Since most of the waivers have been in effect for a relatively short time, we do not believe that this poses a problem. However, to the extent that we do not cap the time horizon for phase-in when we should, we are likely to overstate the effects of the welfare waivers.

TABLE 3
Dynamic Business-Cycle and Welfare-Waiver Effects on State-Specific Monthly AFDC Caseloads

Caseloads (t-1)	-12.953*** (2.767)	Employment/Cap (t-7)	-0.255 (2.457)
Caseloads (t-2)	9.109*** (2.231)	Employment/Cap (t-8)	6.181** (2.699)
Caseloads (t-3)	12.869*** (2.008)	Employment/Cap (t-9)	6.139** (2.841)
Caseloads (t-4)	-1.053 (1.852)	Employment/Cap (t-10)	-0.527 (2.637)
Caseloads (t-5)	2.100 (1.834)	Employment/Cap (t-11)	-6.862*** (2.599)
Caseloads (t-6)	3.262* (1.853)	Work required	-0.475** (0.203)
Employment/Cap	1.049*** (0.159)	Time limit	-0.431* (0.226)
Employment/Cap (t-1)	-20.613*** (2.885)	Work pays	0.524** (0.209)
Employment/Cap (t-2)	-24.469*** (3.075)	Responsibility	0.148 (0.204)
Employment/Cap (t-3)	-8.892*** (2.778)	Work required: months since implemented	-0.010** (0.004)
Employment/Cap (t-4)	-0.629 (2.465)	Time limit: months since implemented	-0.001 (0.006)
Employment/Cap (t-5)	-9.555*** (2.846)	Work pays: months since implemented	0.020*** (0.005)
Employment/Cap (t-6)	-7.847*** (2.623)	Responsibility: months since implemented	-0.010** (0.004)

† All coefficients are multiplied by 100. Heteroskedasticity-robust standard errors are in parentheses. Each regression controls for a constant, a quadratic trend, 11 month-of-year dummies, and 50 state dummies. The number of observations used in estimation equals 5,508 (N=51; T=108).
* = significant at the 10 percent level; ** = significant at the 5 percent level; *** = significant at the 1 percent level

model with additional lags until the two most recently added consecutive lags were statistically insignificant. While we estimated this model using both the unemployment rate and employment per capita, we focus on the latter because it did not appear to have any effect in the first model estimated. In this way, we are giving as much credit as possible to the welfare waivers.[15]

It is clear from Table 3 that monthly state-specific caseloads show a rich dynamic structure, with caseloads having six lags and employment per capita having eleven lags. Moreover, the welfare waivers have both short-run and long-run effects on AFDC caseloads, and each has the expected qualitative impact. Three of the four waivers have statistically significant short-run effects and implementation lags. Because it is difficult to disentangle the independent influence of each regressor on per capita caseloads, we next summarize the relative impacts of economic growth and welfare reform with model simulations of Table 3 for specific states.

Dynamic Simulations

The dynamic model described above is sufficiently complex that it is difficult to envision the effects of the business cycle or welfare reforms by direct observation of the parameter estimates. For instance, short-run changes in caseloads associated with welfare reforms or employment growth also independently feed into future levels of welfare caseloads, suggesting a complicated dynamic structure to the caseload series. However, for ease of interpretation, we can use the parameter estimates derived from the dynamic model described above to simulate the effects of business cycles and welfare reforms over time. To carry out these simulations, we use actual past caseload (derived using the model after January 1988) and per capita employment data for each state from January 1988 to the present as starting values.

[15]The model presented in Table 2 shows that the two economic variables are comparable in their long-run effects of the cycle. Although the results are not shown here, estimates of models using the unemployment rate yield results comparable to those presented in Table 3.

We then use the parameter estimates to "implement" welfare waivers in the actual months in which they were approved.

We first use actual employment series from Oregon and Wisconsin to simulate the effects of each type of welfare reform (versus no reform) over time for a reform implemented in January 1993. We select Oregon and Wisconsin because these are the two states with the largest reductions in caseloads per capita since January 1993—43 percent and 48 percent reductions, respectively. These simulations are shown in Figures 4 and 5, in which the y-axis variable is the logarithm of per capita caseloads. Our model suggests that in the absence of welfare reform, the cyclical and seasonal fluctuations in employment that occurred since January 1993 in Oregon and Wisconsin would have led respectively to 23 percent and 25 percent caseload reductions by September 1996. Work-requirement and responsibility waivers would each be associated with about an 11 percent reduction in caseloads, and time-limit waivers would be associated with about a 2 percent caseload reduction by September 1996. Work-pays waivers, however, are associated with about a 22 percent *increase* in welfare caseload by September 1996. Therefore, our model suggests substantial reductions over time in welfare caseloads if work-requirement, responsibility, or (less so) time-limit waivers are introduced, but these effects are offset (perhaps completely—or more) if work-pays waivers are also implemented. However, our results suggest that a substantial fraction of caseload reduction in Oregon and Wisconsin would have occurred even without welfare reform.

Table 4 presents the actual caseload reductions, the percentage of these reductions attributable (by our model) to welfare reform, and the percentage of these reductions that we attribute to cyclical and seasonal economic fluctuations for each of the 20 states with the largest caseload reductions since January 1993. We observe that in only three of these states (Wisconsin, Oregon, and Oklahoma) does welfare reform explain more than 5 percent of the state's caseload reduction, while invariably cyclical and seasonal economic fluctuations explain at least 50 percent of the reductions (and usually much

Figure 4

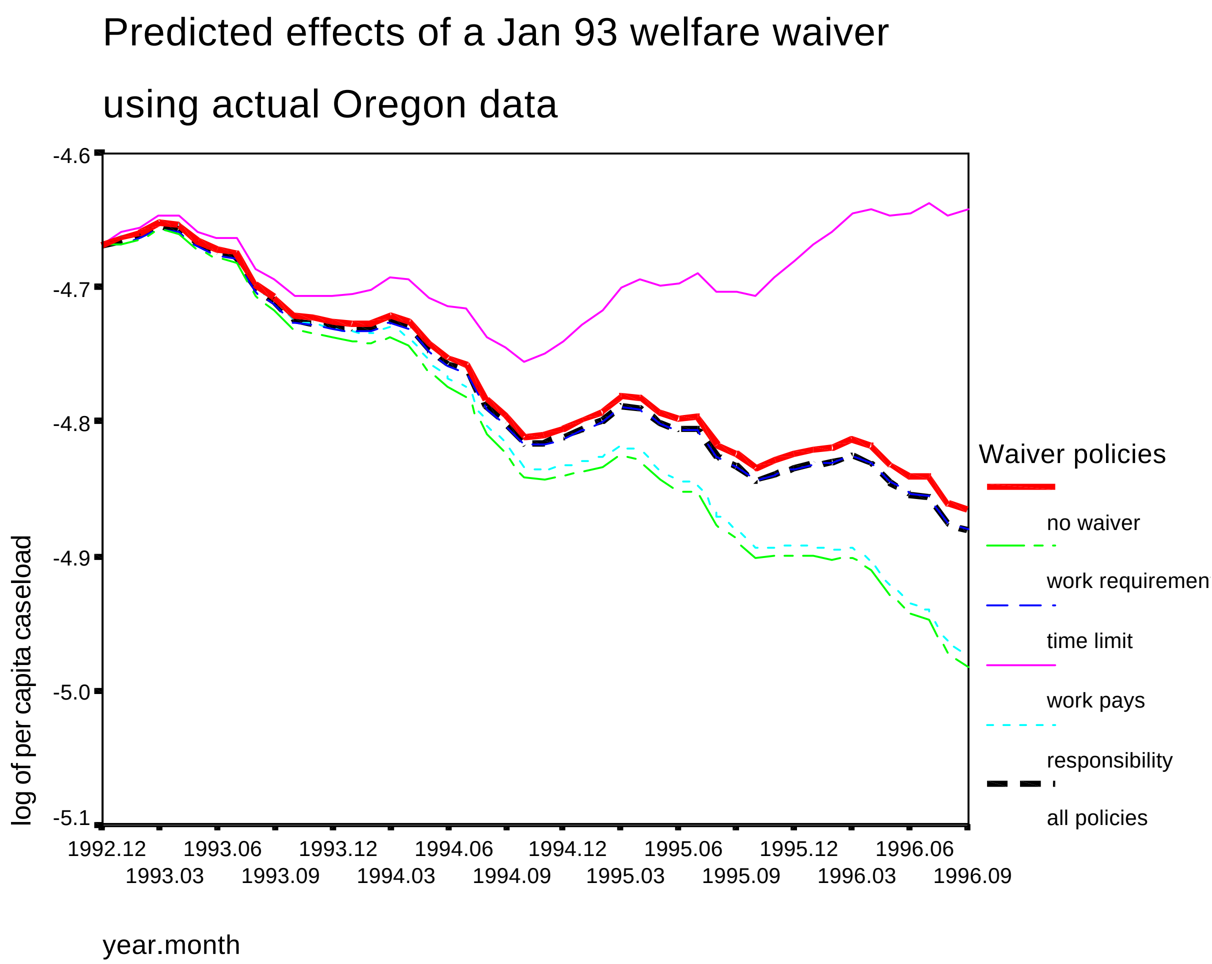
Predicted effects of a Jan 93 welfare waiver
using actual Oregon data
-4.6
-4.7
-4.8
-4.9
-5.0
-5.1
log of per capita caseload
1992.12
1993.03
1993.06
1993.09
1993.12
1994.03
1994.06
1994.09
1994.12
1995.03
1995.06
1995.09
1995.12
1996.03
1996.06
1996.09
year.month
Waiver policies
no waiver
work requiremen
time limit
work pays
responsibility
all policies

Figure 5

Predicted effects of a Jan 93 welfare waiver

using actual Wisconsin data

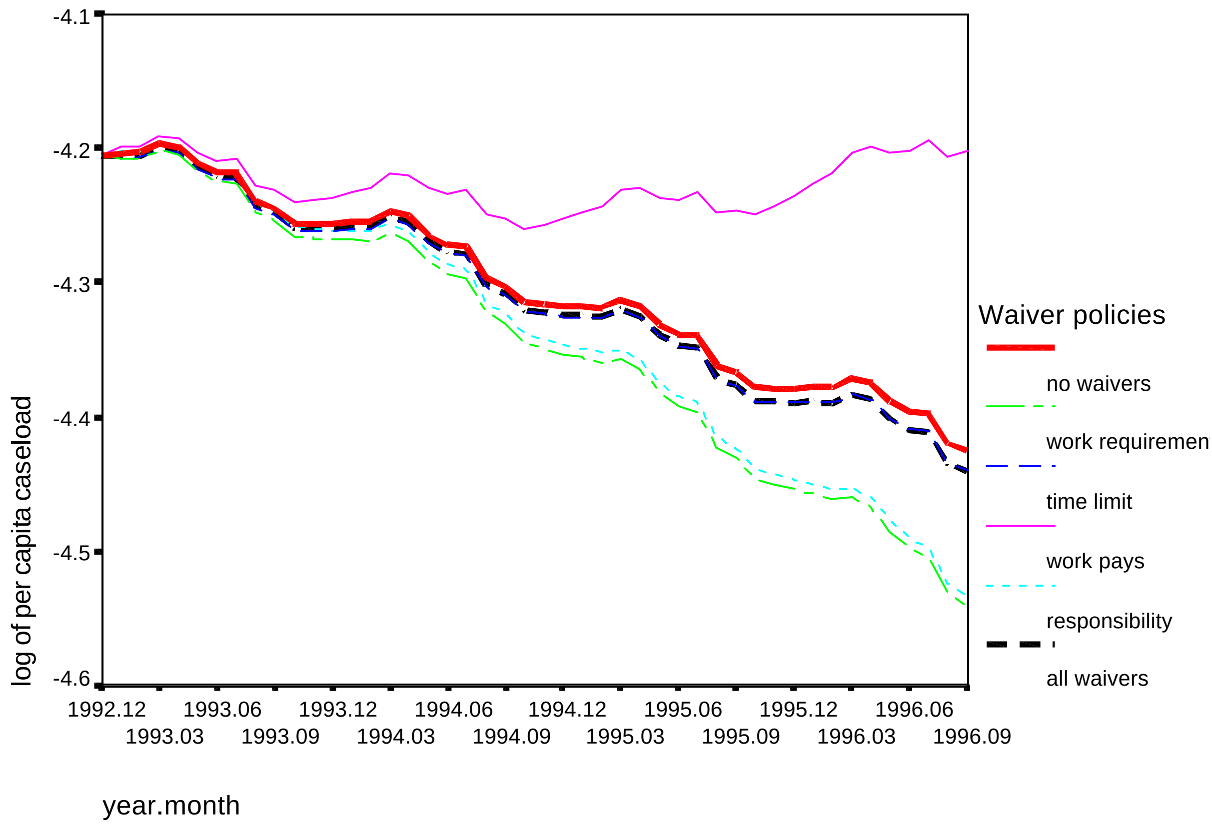

TABLE 4
Percentage of Caseload Reduction Explained by Welfare Reform and Economic Growth in the Twenty States with the Largest Caseload Reductions, 1993–1996†

State	Percent Change in Caseload, Jan 1993 to Sep 1996	Percent Explained by Welfare Reform	Percent Explained by Cyclical and Seasonal Fluctuations
Wisconsin	-48%	11%	53%
Oregon	-43	33	55
Wyoming	-43	0	55
Indiana	-36	1	71
Oklahoma	-36	12	61
North Dakota	-35	4	77
Utah	-35	-3	70
Louisiana	-33	2	67
Michigan	-32	-1	72
Massachusetts	-32	-1	72
Colorado	-31	0	78
Mississippi	-31	2	76
Florida	-30	0	75
Alabama	-26	0	92
Ohio	-25	-1	89
South Dakota	-25	-4	100
Kansas	-24	-1	82
North Carolina	-24	1	87
South Carolina	-23	1	91
Arizona	-22	-6	124

† A negative sign in the second column implies that the welfare reforms in the state are associated with an *increase* in welfare caseloads, while numbers in excess of 100 in the last column indicate that our model overpredicts caseloads in that particular state.

more). Therefore, it appears that with only a few exceptions, welfare reform *does not* explain a substantial fraction of the caseload reduction that had occurred up to September 1996.

What is special about Oregon, Wisconsin, and Oklahoma, the three states among this group where welfare reform does explain a substantial fraction of the reduction in caseloads? *(Note, however, that seasonal and cyclical economic fluctuations* ***still*** *explain a higher percentage of the caseload change in these states than does welfare reform!)* All three states received relatively early welfare waivers, giving the waivers sufficient time for the implementation effect to occur. But many other states obtained waivers at about the same time, so the timing of the waivers cannot be the sole answer. More important may be the *types* of waivers implemented. Wisconsin implemented statewide work-requirement and responsibility waivers, but no statewide work-pays waiver. Oregon had early work-requirement and responsibility waivers, and only 6 months before the sample ended did Oregon receive a work-pays waiver. However, at the same time, Oregon implemented a time-limit waiver, thereby mitigating the pro-caseload effect of the work-pays waiver.[16] Oklahoma implemented all four types of waivers but has one of the first responsibility waivers. In most other states, anti-caseload waivers were coupled with work-pays waivers, leading us to attribute to welfare reform only a small fraction of the state welfare caseload reductions observed recently, and in some cases we predict that welfare reform alone would actually have led to slightly increased welfare caseloads (note, for instance, the negative percentages explained in states such as Arizona and Utah). Therefore, we can conclude that in the vast majority of states, much more of the decline in caseloads can be attributed to economic growth than to welfare reform.[17]

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[16]Oregon's time-limit waiver is less restrictive than most, however. The state does not even start counting time on AFDC unless a recipient fails to participate in required work activities, so we probably modestly overstate the fraction of Oregon's welfare caseload reduction attributable to welfare waivers.

[17]It should be noted that three states with meaningful caseload reductions that are not on this list also had welfare reforms that explain a substantial fraction (over 10 percent) of the caseload reduction. Arkansas's, Georgia's, and Maryland's welfare waivers each explain at least 15 percent of their caseload declines, according to our results. Arkansas and Maryland have relatively old responsibility policies. Georgia has relatively old responsibility and work-requirement policies, and only a recent work-pays policy.

Wisconsin's welfare reforms are arguably more stringent than other similarly categorized reforms. It is possible, therefore, that we are understating the effects of welfare reform in Wisconsin and overstating the effects in other states whose welfare reforms fall into the same categories as Wisconsin's. In an attempt to gauge the degree to which this is the case, we estimate our dynamic model with five sets of policy variables—the four types of welfare waivers mentioned above, plus separate Wisconsin-specific policies. Our Wisconsin-specific variables suggest that Wisconsin's welfare reforms may be associated with as much as a 16 percent caseload reduction to date (one-third of the total Wisconsin caseload reduction), larger than our previous estimate but still considerably lower than the estimated contribution of economic growth. The other policy effects are roughly similar to those presented above, suggesting that our previously mentioned results are not driven by Wisconsin.

4. DISCUSSION: RECONCILING WITH THE CEA

The CEA's results that welfare waivers account for a sizeable fraction of the decline in AFDC caseloads have been cited by President Clinton and a number of governors in policy speeches about the effectiveness of welfare reform. However, the results, and consequent policy implications, of our study differ markedly from those in the much-publicized CEA study (and to a lesser extent from Blank (1997b)). Specifically, the CEA attributes nearly a five-fold larger effect of welfare waivers on explaining the recent caseload decline than do we, and a business-cycle effect that is about 40 percent less. An obvious question arises about the source of this discrepancy; that is, is it due to our different coding of the waiver variables, our omission of measured demographics and lead effects, or our use of a dynamic model with monthly as opposed to annual data.[18] In this section, we provide evidence that the

[18]Differences in coding the states' welfare waivers arise from two sources: different sources of information and a slightly different classification of waiver types. The CEA excluded two states' waivers that were approved by HHS but never implemented by the states, based on information not found in the publicly available HHS document on waivers. In addition, we chose to follow the HHS classification by type of waiver, which is more inclusive than

latter is at the root of our difference with the CEA. We argue that the CEA's (and Blank's (1997b)) use of annual data masks both important short-run dynamics in AFDC caseloads and implementation lags in welfare waivers that are more adequately modeled with monthly data.

To begin, we attempt to replicate the results of the CEA (1997) and Blank (1997b) using our data and waiver coding by annualizing our monthly data to give us 10 years of data for each of the 50 states plus the District of Columbia. We follow the CEA and aggregate our four full-state waiver variables into a single variable, "any statewide waiver." The waiver variable now represents the fraction of a year that a waiver is in effect. Each model below measures the business cycle with the unemployment rate and controls both for state fixed effects and year dummies, yielding a specification that is comparable to the CEA's (1997) Table 1, column (1), and Blank's (1997b) Table 2, column (1).

In column (1) of Table 5, we report the least squares results of the base-case annualized model. The estimates of 4.07 for unemployment and -9.06 for the waiver are highly comparable to the CEA's estimates of 4.73 and -9.40, and with Blank's estimates of 4.40 and -10.60. Because we are able to replicate their findings using our data and coding methodology, this suggests that (1) the differences in our results are not due to differences in coding of the waivers and (2) the coefficients on our variables of interest, the business cycle and waiver variables, are not likely to suffer from an omitted variables bias. Although Blank controls for several measured demographics—such as the median wage, the fraction of single female household heads in the population, average education, and the AFDC benefit level—our results excluding those variables are indistinguishable from those that include them.

Another source of the difference between our results and the CEA's is their control for a potential lead effect in waivers, namely, the potential for "political rational expectations" signifying that welfare reform is on the horizon. In column (2) we report results of models with lead effects, with additional controls for lagged unemployment. The latter specification yields a significant lead effect in

that used in the CEA report. See the Appendix for further details.

TABLE 5
Annual Models of the Effect of the Business Cycle and Welfare Waivers on AFDC Caseloads†

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Unemployment rate	4.074***	1.359**	1.888***	0.662**	0.676**	0.643**	0.643*
	(0.505)	(0.679)	(0.334)	(0.338)	(0.340)	(0.330)	(0.336)
Lagged unemployment rate		3.286***	0.368				
		(0.669)	(0.307)				
Any statewide waiver	-9.058***	-2.046	1.493	0.770	0.576		
	(1.811)	(2.295)	(1.110)	(.1877)	(1.878)		
Lead statewide waiver		-7.008***	-2.446***	-1.882**	-1.522		
		(2.000)	(0.834)	(0.868)	(0.978)		
Lagged dependent variable			0.829***				
			(0.023)				
Implementation lag					0.439		
					(0.479)		
Work required						-4.440*	-4.007
						(2.676)	(2.574)
Time limit						0.884	0.499
						(3.642)	(3.730)
Work pays						4.584*	4.137
						(2.748)	(2.807)
Responsibility						-3.134**	-3.076
						(1.508)	(1.666)

(table continues)

TABLE 5, continued

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lead work required						0.506	0.114
						(1.461)	(1.774)
Lead time limit						0.073	0.016
						(1.519)	(1.553)
Lead work pays						-1.951	-1.702
						(1.281)	(1.522)
Lead responsibility						-1.452	-1.296
						(1.020)	(1.086)
Work required: years							-0.609
since implemented							(0.835)
Time limit: years							0.529
since implemented							(0.996)
Work pays: years							0.174
since implemented							(1.099)
Responsibility: years							0.342
since implemented							(0.628)

† Heteroskedasticity-robust standard errors in parentheses. The number of observations used in estimation equals 510 (N=51; T=10). Columns (1)–(3) are estimated in levels, while columns (4)–(7) are estimated in first differences. Each model controls for state-specific effects and year effects.

* = significant at the 10 percent level; ** = significant at the 5 percent level; *** = significant at the 1 percent level.

waivers, comparable in magnitude to the CEA results. This suggests that our dynamic monthly model (in Table 3) may not be picking up potential lead effects in waivers, so we test this by examining how the CEA's model stands up to some of our modeling choices.

In column (3) of Table 5, we augment the model in column (2) with a lagged dependent variable. We showed previously that significant state dependence exists in monthly caseloads, and thus may also exist in annual caseloads. The estimate of 0.83 with a t-statistic of 36 indicates that the CEA and Blank are not adequately capturing the model dynamics and that there may be a nonstationarity in annual caseload levels. While this is not a direct test for a unit root, our estimate of 0.83 is likely downward-biased. Nickell (1981) showed that when T is small (T=10, 16, and 20 in this model, Blank's, and the CEA's, respectively), the downward bias can be as large as 16 percent, suggesting that the true estimate may be closer to 0.95. Consequently, it is probably more appropriate to estimate the annual model in first differences to make caseloads difference stationary. In column (4) we report the results of a first-difference regression, where we have dropped lagged unemployment because the waiver variables were not significantly affected by their inclusion. It is clear that even in the first-difference model, significant waiver lead effects still exist.

However, missing from each of the first four models in Table 5, and in the CEA's and Blank's papers, is a control for the likely existence of significant lags between the time a waiver is approved and the time it is actually implemented. We construct an annualized analogue of our implementation lag, defined as the fraction of years since waiver approval, and append it to the model in column (4). The results are recorded in column (5), where we no longer find a significant lead effect, nor do we find a significant implementation effect, contrary to our monthly results in Table 3. We further examine the robustness of the lead effect by disaggregating the waiver variables into the four categories used previously: work requirements, time limits, work pays, and responsibility. In columns (6) and (7) we report the disaggregated waiver models without and with implementation lags and find that the only significant waiver effects are contemporaneous. Since our previous results with monthly data yield significant implementation effects, we conclude that the CEA's use of annual data not only misses important short-run dynamics in caseloads induced by business-cycle factors and state dependence, but also does not adequately capture the implementation lag in welfare waivers, resulting in an overstatement of the effects of welfare reform.

5. CONCLUSIONS

We account for the recent decline in AFDC caseloads using a dynamic model in which we permit a rich structure of lagged caseloads, lagged employment per capita, and implementation lags in federal welfare waivers. Our results suggest that the decline in per capita AFDC caseloads is attributable largely to the economic growth of states and not to waivers from federal welfare policies. Twenty-six states experienced declines in per capita AFDC caseloads of 20 percent or more over the 1993–1996 period. We attribute almost 80 percent of the decline in those states to economic growth and about 6 percent to the states' experiments with welfare reform under federal waivers. However, our results also suggest that two aspects of the welfare waivers may have substantial effects on AFDC caseloads. Specifically, the *form* of the welfare waiver matters: most states that experienced small changes in welfare caseloads attributable to welfare reform have work-pays provisions in their waivers, which our model predicts will increase AFDC caseloads. In addition, our results indicate that the effects of welfare waiver implementation are not immediate, but rather phase in over months or even years.

Hence, while our results strongly indicate that welfare reform has to date played only a modest role in the aggregate reduction of AFDC caseloads, we suspect that the recent round of waivers, as well as the 1996 Personal Responsibility and Work Opportunity Reconciliation Act, may lead to larger caseload reductions in the future. Nonetheless, when a recession next occurs, the slowdown in employment growth may more than offset the impacts of welfare reform, and caseloads are likely to rise again.

Appendix: State-Specific Welfare Waivers

Prior to passage of the new welfare law in August 1996, states were required to seek HHS approval to waive or change certain provisions of the federal laws governing the AFDC program. The types of waivers approved have varied over time, with waivers becoming increasingly large-scale and complex by the mid-1990s. Many states adopted partial-state waivers first, and later received approval for waivers covering the entire state. While early waivers tended to include relatively few provisions, some later waivers have represented complete overhauls of a state's welfare system (Boehnen and Corbett, 1996).

We rely primarily on information from the HHS Web site for the approval dates and types of waivers in each state (HHS, 1996a). In some cases, we supplement this information with other sources (HHS, 1996b; Wiseman, 1993a, 1993b; and CLASP, 1992). HHS classifies the state-specific welfare waivers into five categories: (1) waivers regarding work requirements, (2) waivers imposing time limits on assistance, (3) provisions "making work pay" (i.e., work incentives), (4) waivers regarding child support enforcement, and (5) waivers encouraging parental responsibility. We adopt the HHS classification with one modification; we combine waivers related to child support enforcement with those encouraging parental responsibility. Table A-1 shows our coding of the state-specific waivers approved by HHS between 1988 and 1996, based on the four categories.

Our coding of welfare waivers based on the HHS classifications differs slightly from the coding scheme used by the CEA (1997). That report separates the two types of time limits, those that require work after a set time period and those that impose an end to benefits after a time limit regardless of whether the parent is employed. In our coding based on HHS, most JOBS waivers (as coded in the CEA report) are counted as work-requirement waivers, and earnings-disregards changes are included in the work-incentives ("making work pay") category. We include more types of waivers, however, because the

CEA coding does not include most types of parental-responsibility waivers (other than a family cap) or work requirements other than a time limit and JOBS-related sanctions policies, nor does the CEA include extensions of transitional child care or medical benefits. Despite the fact that the CEA's coding is less inclusive than ours, as shown by our sensitivity analysis, differences in coding the waiver variables do not explain the difference in results.[19]

Table A-2 identifies the key provisions that may be included in each type of waiver (based on the HHS classification). Not all states include all these provisions in each waiver. There is some room for judgment in classifying the waivers, because the categories are not as clear-cut as they may appear and some categories overlap. Work-requirement waivers, for example, include provisions that sanction participants for not seeking employment but also include waivers that subsidize private sector employment or allow employers to contribute to special savings accounts for education and training. Time limits primarily include restrictions on the length of time a family may receive benefits but may also include a requirement that the parent find employment after a certain length of time on AFDC. Work incentives, or provisions for making work pay, vary from extensions of transitional medical or child care benefits to changes in earnings-disregards formulas to allow working recipients to keep a larger fraction of their benefits as they increase their earnings. The impact of waivers is likely to vary depending on the types and number of provisions adopted by each state.

[19]We included waivers in two states that received HHS approval but never implemented the reforms, according to information the CEA obtained that was not in the HHS Web site document. Excluding these two states does not qualitatively affect our results.

TABLE A-1
Federal Welfare Waivers in the States (Approval Date)[a]

State	Work Requirements	Time Limits	Work Incentives	Child Support and Responsibility
Alabama	no	no	no	no
Alaska	no	no	no	no
Arizona	May 1995*	May 1995	May 1995	May 1995
Arkansas	no	no	no	Apr 1994
California	Sep 1995	no	Mar 1994	Mar 1994
Colorado	no	Jan 1994*	Jan 1994*	Jan 1994*
Connecticut	Dec 1995	Aug 1994* Dec 1995	Aug 1994	Aug 1994* Dec 1995
Delaware	May 1995	May 1995	May 1995	May 1995
District of Columbia	no	no	no	Aug 1996[b]
Florida	Jan 1994*	Jan 1994*	Jan 1994*	Jan 1994* Jun 1996
Georgia	Nov 1993	Oct 1995*	Oct 1995	Nov 1992
Hawaii	June 1994	Aug 1996	Aug 1996	Aug 1996
Idaho	Aug 1996	no	no	Aug 1996
Illinois	Oct 1995	Oct 1995	Nov 1993	Oct 1995
Indiana	Dec 1994	Dec 1994	Dec 1994	Dec 1994
Iowa	Apr 1996	Aug 1993	Aug 1993	Apr 1996
Kansas	Aug 1996	no	Aug 1996	Aug 1996
Kentucky	no	no	no	no
Louisiana	no	Feb 1996	no	Feb 1996
Maine	Jun 1996	no	Jun 1996	June 1996
Maryland	Aug 1995* Aug 1996	Aug 1995* Aug 1996	Aug 1995* Aug 1996	Jun 1992
Massachusetts	Aug 1995	no	Aug 1995	Aug 1995
Michigan	Aug 1992	no	Aug 1992	Aug 1992
Minnesota	Apr 1994*	no	Apr 1994* Aug 1996	
Mississippi	Dec 1994*	no	Dec 1994*	Dec 1994* Sep 1995
Missouri	Apr 1995	Apr 1995	Apr 1995	Oct 1992* Apr 1995
Montana	Apr 1995	Apr 1995	Apr 1995	Apr 1995
Nebraska	Feb 1995	Feb 1995	Feb 1995	Feb 1995
Nevada	no	no	no	no
New Hampshire	Jun 1996	Jun 1996	Jun 1996	Jun 1996
New Jersey	Jul 1992	no	Jul 1992	Jul 1992
New Mexico	no	no	no	no
New York	no	no	Oct 1988* Oct 1994*	Oct 1988* Oct 1994*
North Carolina	Feb 1996	Feb 1996	Feb 1996	Feb 1996
North Dakota	Apr 1994	Sep 1995*	Sep 1995*	Sep 1995*

(table continues)

TABLE A-1, continued

State	Work Requirements	Time Limits	Work Incentives	Child Support and Responsibility
Ohio	Mar 1995* Mar 1996	Mar 1996	Mar 1995* Sep 1995	Mar 1995* Sep 1995
Oklahoma	Mar 1995*	Mar 1995*	Mar 1995*	Jan 1994
Oregon	Jul 1992	Mar 1996	Mar 1996	Mar 1996
Pennsylvania	no	no	Nov 1994*	Nov 1994*
Rhode Island	no	no	no	no
South Carolina	Jan 1995* May 1996	Jan 1995* May 1996	Jan 1995* May 1996	Jan 1995* May 1996
South Dakota	Mar 1994	Mar 1994	Mar 1994	no
Tennessee	Jul 1996	Jul 1996	Jul 1996*	Jul 1996
Texas	Mar 1996	Mar 1996	Mar 1996	Jul 1995
Utah	Oct 1992* Jul 1995	no	Oct 1992* Jul 1995	Oct 1992* Jul 1996
Vermont	Apr 1993	Apr 1993	Apr 1993	Apr 1993
Virginia	Jul 1995	Jul 1995	Nov 1993	Jul 1992* Jul 1995
Washington	no	Sep 1995	Sep 1995	no
West Virginia	Jul 1995	no	Jul 1995	no
Wisconsin	Nov 1993* Sep 1995	Nov 1993*	Apr 1992*	Apr 1992* Jun 1994
Wyoming	Sep 1993	no	Sep 1993	Sep 1993

[a]We exclude waivers that impacted only the AFDC-UP program, which in most cases lessened or eliminated requirements concerning the 100-hour rule or work history requirements for eligibility. These provisions affected only those families eligible for the unemployed parent program, a small fraction of the caseload, and were more likely to increase than decrease caseloads. In some cases, states have approval for later waivers, but these are not shown in the table if the new waiver expanded provisions within categories already covered.

[b]The District of Columbia withdrew its other waiver provisions in September 1996, prior to implementation.

*Denotes provisions implemented in only part of a state.

TABLE A-2
Examples of Waiver Provisions

Requiring work

- narrowing of criteria for exemptions from JOBS participation
- sanctions for failure to work or participate in a training program
- require community service work in exchange for benefits (“workfare”)
- expand job search requirements
- expand case management services
- wage subsidies in private sector jobs
- employers contribute to special accounts for education or training
- public/private partnerships
- workplace mentoring

Time-limited assistance

- time limit on receiving benefits
- requirement to work or participate in training after a specified time period
- develop/sign a self-sufficiency plan or agreement with goals and deadlines
- sanctions to enforce self-sufficiency agreements

Making work pay (work incentives)

- increase resource limits
- increase earned income disregards
- extend transitional child care and/or medical benefits
- one-time payment in lieu of AFDC
- disregard earnings of teens in the household

Encourage parental responsibility and child support enforcement

- expand child support enforcement programs
- increase child support pass-through
- minor parents required to live at home or in a supervised setting
- teen parents required to attend school
- children required to attend school, be immunized, get health check-ups
- no increase in benefits if another child is born (a “family cap”)

Source: “HHS Fact Sheet: State Welfare Demonstrations.” U.S. Department of Health and Human Services. August 22, 1996.

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