Accounting for the Decline in AFDC Caseloads

Welfare Reform or the Economy?

James P. Ziliak
David N. Figlio
Elizabeth E. Davis
Laura S. Connolly

ABSTRACT

We use state-level monthly panel data to assess the relative contributions of the macroeconomy and welfare reform in accounting for the 1993–96 decline in Aid to Families with Dependent Children (AFDC) caseloads. Our results suggest that the decline in per capita AFDC caseloads is attributable largely to the economic conditions in states and not to waivers from federal welfare policies. Nationwide, we attribute 66 percent of the decline to the macroeconomy. However, we do find substantial heterogeneity in the impact and timing of alternative waivers on AFDC caseloads. States with waivers impacting parental responsibilities experienced greater caseload declines than states with waivers that made work more attractive. Overall, our model predicts that had it not been for the influence of economic factors, welfare reform would not have led to any decrease in aggregate caseloads.

James P Ziliak is an associate professor of economics at the University of Oregon; David N. Figlio is an assistant professor of economics at the University of Florida; Elizabeth E Davis is an assistant professor of applied economics at the University of Minnesota; Laura S. Connolly is an assistant professor of economics at Oregon State University. *The authors are grateful to Ebelyn Mills at the Office of Family Assistance, U S Department of Health and Human Services, for providing us with the caseload data. In addition, we wish to thank two anonymous referees, David Card, Sheldon Danziger, Phil Levine, Marianne Page, Joe Stone, Steve Ziliak, and seminar participants at Bowling Green State University, Michigan State University, the University of Michigan, the 1997 Association for Public Policy and Management Meetings, and the 1998 North American Winter Econometric Society Meetings for helpful comments and discussion. The authors take responsibility for all remaining errors. The data used in this article can be obtained beginning March 2001 through February 2004 from James P. Ziliak, Department of Economics, 1285 University of Oregon, Eugene, OR 97403-1285.*
[Submitted April 1998; accepted January 2000]

I. Introduction

The dramatic recent decline in the number of families receiving Aid to Families with Dependent Children (AFDC) has captured substantial attention among policy makers and the popular press (DeParle 1997; Harris and Haveman 1997). Nationwide, AFDC caseloads decreased by about 18 percent between January 1993 and September 1996, while some states, such as Wisconsin, Indiana, and Oregon, had declines of 40 percent or more during that period. Two factors are frequently suggested as possible causes: state-level experiments with welfare reform and strong economic conditions. Accounting for the relative importance of state level welfare reforms and the economy on AFDC caseloads will establish a baseline for assessing the 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 and economic variables. We conclude that although welfare reform has had a modest effect on caseloads in some states, a much larger fraction of the decline is a result of the strength of the economy.

Welfare-reform demonstration projects began in several states, notably, Wisconsin, in the late 1980s and early 1990s. Most of these demonstrations were small in scale, covering only a few select counties, and having limited impact on welfare caseloads (Wiseman 1996). Applications for statewide waivers from federal welfare program rules began in earnest with the 1993 inauguration of the Clinton Administration, such that by 1996, 43 states had received approval for welfare waivers from the Department of Health and Human Services (HHS). The waivers varied in scope, ranging from sanctions for failing to work or participate in a training program to a time limit on benefit receipt. Indeed, given the breadth of these state-specific waivers, many states' welfare programs are substantially unchanged after implementation of the new federal welfare reform (Blank 1997a).

Weighing the relative merits of economic stimulus versus changes in program generosity and access as mechanisms to reduce welfare caseloads is crucial to evaluating welfare reform. 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 aggregate models that can forecast changes in the number of families receiving AFDC 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.

Perhaps surprisingly, few attempts have been made to examine the link between welfare waivers and caseloads. Two projects using annual state-level panel data and static models were conducted concurrently with this study (U.S. Council of Economic Advisers 1997; Blank 1997b). Using their preferred estimates, the CEA concludes that the economy accounts for 44 percent of the decline in AFDC caseloads from 1993 to 1996, while welfare waivers account for 31 percent of the decline.

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)

Blank's primary focus is on the unexpected run-up in AFDC caseloads from 1990 to 1993; however, she also finds that both the macroeconomy and welfare waivers have had a significant effect on caseload declines.

In this paper, we use monthly state-level data, permitting us to estimate a model of per capita AFDC caseloads with a rich dynamic specification. A key advantage of monthly over annual frequency is that it alleviates potential aggregation bias in annual data resulting from the fact that AFDC eligibility is determined on a monthly basis. We use per capita employment and the unemployment rate to measure economic activity, while we categorize welfare waivers into four components: work requirements, time limits, making work more attractive, and parental responsibility. Our dynamic model allows for considerable path-dependence in caseload changes from month to month, and also describes the ways in which employment (or unemployment) changes over time affect subsequent caseload changes. We also explore the impact of lags in the response of caseloads to welfare waiver approval as it may take many months or years for states to implement new programs.

Simulations of our model predict that 66 percent of the 1993 to 1996 decline in AFDC caseloads is attributable to economic factors, while, in the absence of other influences, welfare reform would not have led to any decrease in aggregate caseloads. We also find that the wide variety of reform provisions across states accounts for substantial heterogeneity in the impact of welfare reform on AFDC caseloads. Our model predicts that states with work incentive waivers but no responsibility waivers would have seen a caseload decline of 18.3 percent in the absence of other factors as opposed to the 13 percent that actually occurred in those states. In contrast, in states with responsibility waivers but no work incentive waivers, the reforms account for 11 percent (about three percentage points) of the 27 percent decline.

II. Data and Empirical Specification

Because AFDC eligibility decisions are made on a month-by-month basis, we use monthly caseload data for the combined AFDC-Basic and AFDC-UP programs.[2] The data, which cover the 1987–96 federal fiscal years for all 50 states plus the District of Columbia, are obtained from Quarterly Public Assistance Statistics, published by the Office of Family Assistance of the U.S. Department of Health and Human Services (HHS). We use the AFDC caseload as the dependent variable rather than the number of AFDC recipients for three reasons. First, the number of recipients confounds the number of households receiving AFDC with the within-household fertility behavior. In addition, the number of cases better represents the

2. The AFDC-UP program represents only 5 percent of the total AFDC caseload Figlio and Ziliak (1999) show that the total caseload provides an adequate representation of aggregate movements in the program. Our caseload measure does include "child-only" cases such as children in foster homes or children of immigrants whose parents do not qualify. This is a growing segment of the AFDC caseload that is likely to be less cyclical than the other segments of the program. Indeed, Blank (1997b) estimates that the share of child-only cases grew from about 13 to 20 percent from the mid 1980s to mid 1990s However, her estimates of the impact of the macroeconomy and welfare reform on per capita caseloads were unaffected by netting out child-only cases.

behavioral response to changes in economic conditions and welfare reform because it is the adult who makes the decision about whether or not to participate in AFDC. In most cases, there is only one adult per AFDC household, while there may be several children, so the caseload correlates most closely with the number of decision makers. Lastly, there appears to be more political interest in understanding the factors that affect the number of cases than in those that determine the number of recipients and, in fact, most welfare reform waivers are designed to affect the caseload rather than the number of recipients.

To reflect differing state populations, we deflate caseloads by the segment of the population most at risk of entering AFDC—female population between the ages of 15 and 44.[3] Because caseloads are observed at monthly frequencies, we start with the annual population figures from the U.S. Census Bureau's webpage (U.S. Census Bureau 1997) and impute the monthly state population by assuming a constant monthly arrival rate. Finally, we use the log of per capita caseloads to capture the possible nonlinear response of caseloads to explanatory factors.

We estimate our models using two alternative measures of economic activity; state-specific monthly unemployment rates and the log of the ratio of employment to female population (age 15–44) in each state.[4] These data are obtained from the U.S. Bureau of Labor Statistics *Most Requested Series* webpage (U.S. Bureau of Labor Statistics 1997). Although we could have used alternative economic indicators, we feel that these adequately capture the labor market conditions that are crucial to the AFDC participation decision of less-skilled individuals.

The welfare reform variables are dummy variables that equal zero prior to HHS approval of the state's request for a waiver from federal AFDC program requirements and one beginning on the date of approval. We rely primarily on information from HHS (1996a,b) for approval dates and types of waivers in each state, but also supplement it with information from Wiseman (1993a,b) and the Center for Law and Social Policy (1992). 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 (namely, that "make work pay"), (4) those that are related to child support enforcement, and (5) those that encourage parental responsibility (for example, requirements that children in AFDC families regularly attend school and get health checkups, or a so-called "family cap," which does not allow benefits to increase when another child is born in a family receiving assistance). We adopt the HHS classification with two minor modifications; we group together waivers related to child support enforcement and those encouraging parental responsibility, and we include time limits that result in a work requirement rather than a termination of benefits under the work requirement category (for example, requiring one to participate in job search or community service after 12 months of receiving benefits). The time limit category includes only those waivers that require a loss of benefits after a fixed number of years or months. Each of the waiver types, with the exception of

3. Our estimates are not sensitive to use of the full adult population, nor are they sensitive to the use of monthly state population as a covariate rather than using it to deflate the caseload

4. We deflate total employment by population to account for the significant cross-state differences in the potential size of the employment pool, while we take the natural log of the ratio in order to transform the coefficient into an easily interpretable elasticity

making work more attractive, is expected to reduce welfare caseloads.[5] 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. However, this positive effect may be offset by some of the work incentives like medical-assistance extensions which may have a negative effect on caseloads. While some states received waivers that covered only part of the state's caseload, we focus only on statewide waivers.[6] The Appendix contains a table of waiver approval dates, while a complete description of waivers is available in Ziliak et al. (1997).

There are a variety of state-level characteristics that are expected to affect the caseload, many of which are unobservable. We attempt to capture these by using state fixed effects and state trends, the latter of which capture slow-moving demographic changes such as fertility rates, divorce rates, and migration.[7] One important state-level variable that is observable and available monthly is the amount of public assistance available to a typical recipient. We measure this by using the maximum combined real AFDC/Food Stamp benefit for a family of three in each state (U.S. House of Representatives, selected years).

A. Empirical Specification

We begin our empirical analysis by specifying a static model that, in addition to the variables discussed in the previous section, includes a cubic time trend to allow for variation in AFDC caseloads resulting from national economic and political trends. This allows enough nonlinearity in the trend to capture the decline (1987–90), rise (1990–93), and subsequent decline (1993–96) in aggregate caseloads over the sample period.[8] In addition, we append month-of-year dummies to the empirical model to capture seasonal fluctuations in caseloads and employment because our monthly measures of these variables are not seasonally adjusted.[9] Because there is a lag between the date on which a waiver is approved and the date on which it is implemented, we include a variable, which we call the "implementation lag," that cap-

5. 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. Although 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.

6. Ziliak et al. (1997) show that the results are qualitatively similar when partial-state waivers are included in the analysis, but at the expense of efficiency as most partial-state waivers covered only a small fraction of cases.

7. Blank (1997b) argues against state trends in that they "overfit" the data. We believe that controls for state trends are important because of the possibility of slowly changing demographics within states. Sensitivity tests in Figlio and Ziliak (1999) indicate that excluding measured demographics do not affect the estimates of either the business cycle or welfare waivers on caseloads However, omitting state-specific trends implies that all unobserved trending differences are attributable to welfare reform, which does not seem plausible.

8. 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.

9. We also considered a model with state-specific seasonality effects as an alternative to our common-month effect specification. This yielded results comparable to those reported here

tures the length of time since approval.[10] We parameterize the implementation lag as a series of dummy variables for 1–6, 6–12, 12–24, and more than 24 months since waiver approval. In Ziliak et al. (1997) the implementation lag is specified as a linear trend; however, the dummy variable specification is more flexible in that it permits nonlinearities in the effect of time on welfare caseloads.

The static model of AFDC caseloads for each state i ($i = 1, \ldots, 51$) in month t($t = 1, \ldots, 120$) is

$$(1)\quad C_{it} = \mu + \alpha E_{it} + \lambda B_{it} + W_{it}\beta + I_{it}\theta + \gamma_1 t + \gamma_2 t^2 + \gamma_3 t^3 + \delta_{1i} + \delta_{2i} t + \eta_j + \varepsilon_{it},$$

where C_{it} is log per capita AFDC caseloads, E_{it} is the measure of economic activity, B_{it} is the real AFDC/Food Stamp benefit, W_{it} is the vector of welfare waivers, I_{it} is the vector of implementation lags, t is the trend, δ_{1i} is the time-invariant state-specific effect, $\delta_{2i}t$ is the state-specific trend, η_j($j = 2, \ldots, 12$) is the month-of-year dummy variable, and ε_{it} is a random error that permits conditional heteroskedasticity in caseloads.

Because 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 produced a coefficient on the lagged dependent variable of 0.98.[11] Consequently, we first-difference the regression model in Equation 1 to make caseloads difference stationary, yielding the first-difference estimating equation

$$(2)\quad \Delta C_{it} = \alpha \Delta E_{it} + \lambda \Delta B_{it} + \Delta W_{it}\beta + \Delta I_{it}\theta + \tilde{\gamma}_1 + \tilde{\gamma}_2 t + \tilde{\gamma}_3 t^2 + \delta_{2i} + \Delta\eta_j + \Delta\varepsilon_{it},$$

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 byδ_{2i}, which we parameterize with 50 state dummy variables.

The static model in Equation 2 is limiting in that it ignores the possibility that even after controlling for heterogeneity in the form of state-specific trends, previous AFDC caseloads may have a direct impact on future caseloads; namely, caseloads may sluggishly adjust to changing economic and political conditions. In addition, we expect lagged economic conditions to be important as well since welfare recipients are likely to be the last ones hired during an economic recovery and thus may not instantaneously move from welfare to work. The dynamic first-difference estimating equation is thus

$$(3)\quad \Delta C_{it} = \sum_{s=1}^{S} \rho_s \Delta C_{it-s} + \sum_{k=1}^{K} \alpha_k \Delta E_{it-k} + \lambda \Delta B_{it} + \Delta W_{it}\beta + \Delta I_{it}\theta + \tilde{\gamma}_1 + \tilde{\gamma}_2 t + \tilde{\gamma}_3 t^2 + \delta_{2i} + \Delta\eta_j + \Delta\varepsilon_{it},$$

10. Data on actual implementation dates are not available. However, even if they were, there would still be a lag between implementation and caseload response for some types of waivers (such as time limits). Omitting the implementation lag would likely cause the effect of welfare reform to be underestimated

11 The dynamic fixed-effect model is not likely to suffer from the so-called Nickell (1981) bias that arises from correlation between the lagged dependent variable and the error term because of our long time series.

where all variables and coefficients are defined as before in Equation 2. For added flexibility, the lag length, which is determined by the Schwarz criterion, is not restricted to be the same for lagged caseloads and the economic indicators.[12]

A final 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 due to fiscal stress. Alternatively, as Martini and Wiseman (1997) postulate, waiver requests may be the result of fiscal *surplus* as declining caseloads free up additional funds for welfare experiments. To examine whether endogeneity is a problem, we calculate the one-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 one-year change in per capita caseloads during the same period for states that did not yet have such a waiver approved.[13] The difference is less than 0.4 percent and is statistically insignificant, suggesting that endogeneity is unlikely to be problematic. To the extent that it does exist, however, the bias is most plausibly in favor of a negative link between welfare reforms and caseloads, implying an upward bias of our welfare reform estimates.

III. Results

Equations 2 and 3 are estimated with ordinary least squares (OLS), while inference is carried out with standard errors that are robust to conditional heteroskedasticity. We first present results for the static model, Equation 2, and then proceed to the dynamic model, Equation 3. We use the dynamic parameter estimates to predict the fraction of caseload decline attributable to welfare reform and to economic activity. All coefficients are multiplied by 100 for ease of presentation.

A. Static Models

Table 1 contains three specifications, each with two variants that differ based on whether the log employment per capita or the unemployment rate is the measure of economic activity. In Specification 1 we omit the waiver implementation lags and thus measure the impact of welfare reform based on the approval date alone. In this specification, the business cycle, whether measured by the log of employment per capita or the unemployment rate, has a significant contemporaneous impact on AFDC caseloads. For example, the elasticity of per capita caseloads with respect to per capita employment is about -0.1.

Three of the four welfare waivers have the expected qualitative impact on AFDC caseloads, and two—time limits and work incentives—have a statistically significant effect. Interestingly, though, these two policies partially cancel each other out. States with time limits on benefit receipt, but no other waiver, can expect about a 0.65 percent reduction in welfare caseloads. On the contrary, a work incentive waiver alone leads to an increase in caseloads of 0.3 percent. Hence, states with both provis-

12. The Schwarz criterion is an alternative to the adjusted R^2 as a variable selection method (Johnston and DiNardo 1997, p. 74). Its advantage lies in the fact that it penalizes the (efficiency) loss of degrees of freedom more heavily than the adjusted R^2.

13. We perform the analogous exercise with each of the other types of waivers and find similar results.

Table 1
Static Estimates of the Effect of the Business Cycle and Welfare-Reform Waivers on the Log of Per Capita AFDC Caseloads

Variable	(1)		(2)		(3)	
Log per capita employment	−9.923 (3.580)		−9.921 (3.579)		−9.919 (3.597)	
Unemployment rate		0.215 (0.051)		0.217 (0.051)		0.213 (0.051)
AFDC/Food Stamp benefit	−0.102 (0.427)	−0.093 (0.426)				
Work required	−0.358	−0.361	−0.356	−0.360	−0.471	−0.476
1–6 month lag	(0.236)	(0.241)	(0.236)	(0.241)	(0.267)	(0.271)
					0.078	0.080
6–12 month lag					(0.124)	(0.124)
					0.098	0.088
12–24 month lag					(0.127)	(0.125)
					0.156	0.153
> 24 month lag					(0.126)	(0.126)
					−0.325	−0.342
					(0.163)	(0.163)
Time limit	−0.649	−0.657	−0.648	−0.656	−0.792	−0.783
1–6 month lag	(0.242)	(0.246)	(0.242)	(0.246)	(0.308)	(0.311)
					0.244	0.228
6–12 month lag					(0.194)	(0 195)
					−0.316	−0.297
12–24 month lag					(0.256)	(0.255)
					0.235	0.248
> 24 month lag					(0.314)	(0.311)
					−0.181	−0.223
					(0.352)	(0.349)
Work pays	0.325	0.327	0.326	0.328	0.447	0.449
1–6 month lag	(0.198)	(0.200)	(0.198)	(0.200)	(0.231)	(0.233)
					−0.014	−0.015
6–12 month lag					(0.125)	(0.125)
					0.307	0.309
12–24 month lag					(0.122)	(0.123)
					0.172	0.169
> 24 month lag					(0.125)	(0.125)
					0.818	0.830
					(0.180)	(0.180)
Responsibility	0.128	0.146	0.127	0.146	0.215	0.228
	(0.223)	(0.224)	(0.224)	(0.225)	(0.242)	(0.242)
1–6 month lag					−0.146	−0.137
					(0.105)	(0.105)
6–12 month lag					−0.265	−0.257
					(0.113)	(0.113)
12–24 month lag					−0.183	−0.176
					(0.104)	(0.104)
> 24 month lag					−0.369	−0.363
					(0.159)	(0.159)

Notes: Heteroskedasticity robust standard errors in parentheses. The number of observations equals 6,069 (N = 51; T = 119). Each model controls for state-specific effects and month-of-year dummies Coefficients are multiplied by 100.

ions, but no work or responsibility requirements, are likely to see a small negative change in AFDC caseloads. Combining all four welfare waivers, jointly significant at the 8 percent level, yields about a 0.55 percent reduction in AFDC caseloads.

The other covariate in the model is the log real AFDC/Food Stamp benefit for a family of three. However, the coefficient on this variable is small and statistically insignificant. This result is not too surprising because to the extent that states change the nominal benefit it is typically on an annual basis such that all within-year variation in benefits is due to changes in the CPI. When we estimate the levels model in Equation 1, we find a positive and significant estimate of benefit generosity, which is consistent with the previous literature. Previous authors did not investigate the possibility of nonstationarity in caseloads, however, which gives rise to our difference specification. A further complication arises because the inclusion of benefit levels may cause an endogeneity problem; that is, the size of the caseload may affect the generosity of the state benefit package.[14] As Specification 2 shows, the estimates of the business cycle and welfare reform effects are unaffected by the omission of benefits.[15] Based on this and on our concerns over potential benefit endogeneity, we exclude benefits in the remainder of the analysis and estimate only the reduced-form models.

In Specification 3 we consider the impact of lags between waiver approval and waiver implementation. The work requirement waiver now has a significant negative contemporaneous effect (at the 0.10 level). In addition, there is a large negative implementation effect 24 months after approval of this type of waiver. This is to be expected because many state work requirements begin after 24 months of benefit receipt. Interestingly, much of the effect of time limits occurs without any lag. Because none of the time limits were binding during the period under study, this variable may reflect a new, "get tough" attitude in state welfare offices. It may also indicate that recipients left AFDC in order to save their eligibility in case of later emergencies.

As in Specification 1, the work incentive waivers in Specification 3 have a significant *positive* contemporaneous effect that partially offsets the impact of time limits. Work incentives also have a positive implementation effect six to 12 months after approval and again after 24 months. The substantive lag in the effect of work incentives on caseloads may be due to the fact current recipients extend their time on the rolls because their benefit is no longer taxed until they reach higher income and asset levels. Additionally, it takes time for those households previously ineligible for the program due to their income and asset levels to recognize that they qualify after the reforms.

It takes some time for responsibility waivers to affect caseloads, but after six months, these also have a negative effect, as expected. Given the offsetting effects, the static model predicts that a state with all four waivers can only expect a 0.3 percent decline in caseloads after 24 months of waiver approval.

B. Dynamic Models

In Table 2 we expand the analysis to allow caseloads to dynamically adjust to past caseload levels and economic conditions. Specification 1 uses the log of employment

14. The simultaneity between welfare benefits and recipiency has been shown by Case, Rosen, and Hines (1993), Figlio, Kolpin, and Reid (1999), Gramlich and Laren (1984), and Shroder (1995).

15. We also tried using two lags in maximum benefits. Again, neither lag was significant at conventional levels, nor were the business cycle or welfare reform estimates affected by their inclusion.

Table 2
Dynamic Estimates of the Effect of the Business Cycle and Welfare-Reform Waivers on the Log of Per Capita AFDC Caseloads

Variable	Log Employment per Capita	Unemployment Rate
Log caseload/pop (t−1)	−11.870	−12.690
	(2.741)	(2.700)
Log caseload/pop (t−2)	11.070	10.690
	(2.127)	(2.126)
Log caseload/pop (t−3)	13.360	13.060
	(1.993)	(1.957)
Cyclical indicator (t)	−12.073	0.266
	(3.525)	(0.049)
Cyclical indicator (t−1)	−18.932	0.483
	(3.028)	(0.049)
Cyclical indicator (t−2)	−16.703	0.377
	(2.791)	(0.051)
Cyclical indicator (t−3)	−6.629	0.221
	(2.571)	(0.050)
Cyclical indicator (t−4)	−3.514	0.199
	(2.636)	(0.050)
Cyclical indicator (t−5)	−7.556	0.148
	(2.723)	(0.051)
Cyclical indicator (t−6)	−7.861	
	(2.672)	
Work required	−0.471	−0.460
	(0.230)	(0.235)
1–6 month lag	0.106	0.126
	(0.119)	(0.118)
6–12 month lag	0.176	0.086
	(0.121)	(0.119)
12–24 month lag	0.264	0.251
	(0.120)	(0.118)
> 24 month lag	−0.185	−0.286
	(0.153)	(0.155)
Time limit	−0.690	−0.646
	(0.293)	(0.301)
1–6 month lag	0.212	0.043
	(0.195)	(0.190)
6–12 month lag	−0.415	−0.350
	(0.254)	(0.245)
12–24 month lag	−0.025	0.041
	(0.297)	(0.293)
>24 month lag	−0.041	−0.356
	(0.316)	(0.329)

Table 2 (*continued*)

Variable	Log Employment per Capita	Unemployment Rate
Work pays	0.412	0.432
	(0.220)	(0.233)
1–6 month lag	0.044	0.025
	(0.125)	(0.124)
6–12 month lag	0.304	0.324
	(0.119)	(0.119)
12–24 month lag	0.245	0.206
	(0.120)	(0.120)
> 24 month lag	0.739	0.802
	(0.175)	(0.175)
Responsibility	0.232	0.195
	(0.221)	(0.213)
1–6 month lag	−0.142	−0.095
	(0.106)	(0.105)
6–12 month lag	−0.228	−0.156
	(0.108)	(0.107)
12–24 month lag	−0.209	−0.146
	(0.098)	(0.099)
> 24 month lag	−0.366	−0.349
	(0.147)	(0.148)

Notes: Heteroskedasticity robust standard errors in parentheses. The number of observations equals 5,712 in log employment per capita specifications and 5,763 in unemployment rate specifications. Each model controls for state-specific effects and month-of-year dummies. Coefficients are multiplied by 100.

per capita as the cyclical indicator, while Specification 2 is based on the unemployment rate. Based on the Schwarz criterion, Specification 1 has six lags of employment per capita and Specification 2 uses five lags of the unemployment rate. Each model contains three lags of caseloads.[16]

Each of the coefficients on lagged caseloads are economically large and statistically significant at the 1 percent level. The lag structure suggests that overall there is positive autocorrelation in per capita caseloads (the sum of coefficients is about 0.13), but the small coefficient suggests that adjustment is relatively rapid. Moreover, caseloads sluggishly adjust to changes in economic conditions. For example, by summing the unemployment-rate coefficients we see that a one percentage point increase in the unemployment rate that lasts for five months results in a 1.7 percent increase in per capita

16. This lag structure is shorter than that employed in earlier versions of this paper. Previously, we used a "rule-of-thumb" approach to lag length; namely, lags were added until two consecutive lags were statistically insignificant. The consequence of the shorter lag is to downplay the role of the business cycle in explaining the caseload decline

caseloads in the short-run. Similarly, a 1 percent decrease in employment per capita lasting six months leads to a 0.7 percent increase in caseloads. The long and statistically significant lag structure in unemployment rates and employment per capita is consistent with the conjecture that welfare recipients are likely to be the last group hired during an expansion. Most of the waiver coefficients are similar to the static results in Table 1, though many of the estimates are more efficient in the dynamic model.

C. Simulations

The complexity of the dynamic model makes it 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 the macroeconomy also independently feed into future levels of welfare caseloads, suggesting a complicated dynamic structure to the caseload series. To better understand these effects, we can use the parameter estimates from the dynamic model to simulate the effects of business cycles and welfare reforms over time.[17] The simulated effects are constructed cumulatively by measuring the change in caseloads associated directly with welfare waivers or economic conditions in the appropriate period, as well as adding in the effects of past-period welfare waiver and macroeconomic effects.

In Table 3 we present the actual caseload reductions since January 1993, the percentage of these reductions our model attributes to welfare reform, and the percentage of these reductions that we attribute to cyclical and seasonal fluctuations. In the first five rows, we list the results for the top five caseload reducers. Welfare reform explains more than 5 percent of the state's caseload reduction in only three of these states (Wisconsin, Oregon, and North Dakota), while invariably cyclical and seasonal fluctuations explain around one-third of the decline.[18] The sixth row shows the average effect for these five states. Again, about one-third is due to economic activity, while 6 percent is due to welfare reform. Row 7 shows that for the country as a whole, our model predicts that the caseload decline nationwide would have been 9 percent greater (a decline of 19.6 percent instead of 18 percent) had it not been for welfare waivers. This result is driven by positive coefficients on the early work-requirement implementation lags, along with those on the work incentive waivers. In contrast, economic factors account for two-thirds of the decline nationwide. The model suggests 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 Wisconsin, Oregon, and North Dakota, the three states among this group where welfare reform does explain a sizeable fraction of the reduction in caseloads? 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 received statewide responsibility waivers, but no statewide work incentive waiver.

17 We use Specification 2 in Table 2 for the simulation. The simulation results are very similar for the dynamic model that uses employment per capita as the indicator of economic activity

18. As with all predictions, these simulations represent the *average* effect of a state with the same values of the welfare reform and economic variables as those listed here Of course, the actual effect will vary from the average, so these predictions should be interpreted with caution.

Table 3
Percentage of 1993 to 1996 Caseload Reduction Explained by Welfare Reform and Economic Activity

State	Percent Change in Caseload, Jan 1993 to Sep 1996	Percent of Reduction Explained by Welfare Reform	Percent Explained by Cyclical and Seasonal Fluctuations
Wisconsin	−48	16	22
Oregon	−43	11	32
Wyoming	−43	−22	27
Oklahoma	−36	−9	33
North Dakota	−36	6	35
Top five caseload reducers	−41	6	29
United States	−18	−9	66
States with work incentive but not responsibility waivers	−13	−41	98
States with both types of waivers	−16	−26	79
States with responsibility but not work incentive waivers	−27	11	40

Note: A negative sign in the second column implies that the welfare reforms in the state are associated with an *increase* in welfare caseloads.

Oregon had early work-requirement and responsibility waivers, and only six months before the sample ended did Oregon receive a work incentive waiver. However, at the same time, Oregon received a time-limit waiver, thereby mitigating the procaseload effect of the work-pays waiver. North Dakota, like Wisconsin, had no work incentive waivers but had an early work requirement. In most other states, anticaseload waivers were coupled with work incentive waivers, leading us to attribute to welfare reform only a small fraction of the state welfare caseload reductions observed recently.

To examine the effects of the different types of waivers, we perform the simulations for states with work incentive waivers but not responsibility waivers, those with both types, and those with responsibility but not work incentive waivers. As shown in Row 8 of Table 3, the caseload decline would have been 41 percent larger for states in the first group (18.3 percent instead of 13 percent) were it not for welfare reform, while in states with responsibility but no work incentive waivers welfare reform accounted for 11 percent of the overall 27 percent decline (row 10). We conclude that a much larger fraction of the decline in caseloads can be attributed to economic activity than to welfare reform.[19]

IV. 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 economic factors, and implementation lags in federal welfare waivers. Our results suggest that the decline in per capita AFDC caseloads in the period prior to federal welfare reform is attributable largely to robust economic activity and not to waivers from federal welfare policies. We attribute two-thirds of the national caseload decline to economic activity and predict that caseloads would have declined an additional 9 percent had it not been for 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 appears to matter: most states that experienced small changes in welfare caseloads attributable to welfare reform have work incentive 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 prior to federal welfare reform played only a modest role in the aggregate reduction of AFDC caseloads, we suspect that as states continue to implement the provisions stipulated in the 1996 Personal Responsibility and Work Opportunity Reconciliation Act, larger caseload reductions induced by welfare reform will be realized. 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.

19 These predictions differ substantially from those of the CEA (1997). We performed a number of tests to determine the cause of the large discrepancy. These tests have convinced us that it is a result of our use of monthly data and a dynamic model that includes lagged caseloads, economic indicators, and variables to account for the lag in waiver implementation as opposed to differences in coding of the waiver variables or other differences in model specification. Details are available in Figlio and Ziliak (1999).

Table A1
Federal Welfare Waiver Approval Dates in the States

State	Work Requirements	Time Limits	Work Incentives	Child Support and Responsibility
Alaska	no	no	no	no
Arizona	no	May 1995	May 1995	May 1995
Arkansas	no	no	no	Apr 1994
California	Sep 1995	no	Mar 1994	Mar 1994
Colorado	no	no	no	no
Connecticut	Dec 1995	Dec 1995	Aug 1994	Dec 1995
Delaware	May 1995	May 1995	May 1995	May 1995
D.C.	no	no	no	no
Florida	no	no	no	Jun 1996
Georgia	Nov 1993	no	Oct 1995	Nov 1992
Hawaii	Jun 1994	Aug 1996	Aug 1996	Aug 1996
Idaho	Aug 1996	no	no	Aug 1996
Illinois	Oct 1995	no	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	Jun 1996
Maryland	Aug 1996	no	Aug 1996	Jun 1992
Massachusetts	Aug 1995	no	Aug 1995	Aug 1995
Michigan	Aug 1992	no	Aug 1992	Aug 1992
Minnesota	no	no	Aug 1996	no
Mississippi	no	no	no	Sep 1995
Missouri	Apr 1995	no	Apr 1995	Apr 1995
Montana	Apr 1995	no	Apr 1995	Apr 1995
Nebraska	Feb 1995	Feb 1995	Feb 1995	Feb 1995
Nevada	no	no	no	no
New Hampshire	Jun 1996	no	Jun 1996	Jun 1996
New Jersey	Jul 1992	no	Jul 1992	Jul 1992
New Mexico	no	no	no	no
New York	no	no	no	no
North Carolina	Feb 1996	Feb 1996	Feb 1996	Feb 1996
North Dakota	Apr 1994	no	no	no
Ohio	Mar 1996	Mar 1996	Sep 1995	Sep 1995
Oklahoma	no	no	no	Jun 1994
Oregon	Jul 1992	Mar 1996	Mar 1996	Mar 1996
Pennsylvania	no	no	no	no
Rhode Island	no	no	no	no

Table A1 (*continued*)

State	Work Requirements	Time Limits	Work Incentives	Child Support and Responsibility
South Carolina	May 1996	May 1996	May 1996	May 1996
South Dakota	Mar 1994	no	Mar 1994	no
Tennessee	Jul 1996	Jul 1996	no	Jul 1996
Texas	Mar 1996	Mar 1996	Mar 1996	Jul 1995
Utah	Jul 1995	no	Jul 1995	Jul 1996
Vermont	Apr 1993	no	Apr 1993	Apr 1993
Virginia	Jul 1995	Jul 1995	Nov 1993	Jul 1995
Washington	no	no	Sep 1995	no
West Virginia	Jul 1995	no	Jul 1995	no
Wisconsin	no	no	no	Jun 1994
Wyoming	Sep 1993	no	Sep 1993	Sep 1993

Note: 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 caseload.

References

Blank, Rebecca. 1997a. "Policy Watch: The 1996 Welfare Reform." *Journal of Economic Perspectives* 11(1):169–77.

———. 1997b. "What Causes Public Assistance Caseloads to Grow?" Mimeo. Northwestern University.

Case, Anne, Harvey Rosen, and James Hines. 1993. "Budget Spillovers and Fiscal Policy Interdependence: Evidence from the States." *Journal of Public Economics* 52(3):285–307.

Center for Law and Social Policy. 1992. "The Rush to Reform: 1992 State AFDC Legislative and Waiver Actions." November.

Congressional Budget Office. 1993. "Forecasting AFDC Caseloads, with an Emphasis on Economic Factors." Staff Memorandum. July.

DeParle, Jason. 1997. "A Sharp Decrease in Welfare Cases is Gathering Speed." *The New York Times*, February 7.

Figlio, David, Van Kolpin, and William Reid. 1999. "Do States Play Welfare Games?" *Journal of Urban Economics* 46(3):437–54.

Figlio, David, and James P. Ziliak. 1999. "Welfare Reform, the Business Cycle, and the Decline in AFDC Caseloads." In *Economic Conditions and Welfare Reform*, ed. Sheldon Danziger, 17–48. Kalamazoo, Mich: W.E. Upjohn Institute for Employment Research.

Friedlander, Daniel, and Gary Burtless. 1995. *Five Years After: the Long-Term Effects of Welfare-to-Work Programs*. New York: Russell Sage Foundation.

Gramlich, Edward, and Deborah Laren. 1984. "Migration and Income Redistribution Responsibilities." *Journal of Human Resources* 31(1):489–511.

Gueron, Judith, and Edward Pauly. 1991. *From Welfare to Work.* New York: Russell Sage.
Harris, John, and Judith Havemann. 1997. "Welfare Rolls Continue Sharp Decline." *The Washington Post,* August 13, p. A01.
Johnston, Jack, and John DiNardo. 1997. *Econometric Methods,* New York: McGraw-Hill.
Martini, Alberto, and Michael Wiseman. 1997. "Explaining the Recent Decline in Welfare Caseloads: Is the Council of Economic Advisers Right?" Mimeo. The Urban Institute.
Moffitt, Robert. 1996. "The Effect of Employment and Training Programs on Entry and Exit from the Welfare Caseload." *Journal of Policy Analysis and Management* 15(1): 32–50.
Nickell, Stephen. 1981. "Biases in Dynamic Models with Fixed Effects." *Econometrica* 49(3):1399–1416.
Shroder, Mark. 1995. "Games the States Don't Play: Welfare Benefits and the Theory of Fiscal Federalism." *Review of Economics and Statistics* 77(1):183–90.
U.S. Bureau of Labor Statistics. 1997. "Most Requested Series." URL: http://www.bls.gov/top20.html.
U.S. Census Bureau. 1997. "State Population Estimates." URL:http://www.census.gov/population/www/estimates/statepop.html.
U.S. Council of Economic Advisers. 1997. "Explaining the Decline in Welfare Receipt, 1993–1996." Technical Report. URL:http://www.whitehouse.gov/WH/EOP/CEA/Welfare/Technical_Report.html.
U.S. Department of Health and Human Services. *Quarterly Public Assistance Statistics.* Fiscal years 1987–96.
———. 1996a. "HHS Fact Sheet: State Welfare Demonstrations." Technical Bulletin. URL: http://www.acf.dhhs.gov/programs/opa/facts/stdemo1.htm.
———. 1996b. "ACF-Welfare Reform: Section 1115 Waiver Authority—October 1, 1996." Technical Bulletin.
U.S. House of Representatives, Committee on Ways and Means. *Green Book.* Selected years.
Wiseman, Michael. 1993a. "Welfare Reform in the States: The Bush Legacy." *Focus* 15(1):18–36.
———. 1993b. "The New State Initiatives." Institute for Research on Poverty Discussion Paper 1002–93. Madison: University of Wisconsin.
———. 1996. "State Strategies for Welfare Reform." *Journal of Policy Analysis and Management* 15(4):515–46.
Ziliak, James P., David N. Figlio, Elizabeth E. Davis, and Laura S. Connolly. 1997. "Accounting for the Decline in AFDC Caseloads: Welfare Reform or Economic Growth?" Institute for Research on Poverty Discussion Paper 1151–97. Madison: University of Wisconsin.