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What Goes Up Must Come Down?
Explaining Recent Changes in Public Assistance Caseloads

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Over the past decade, public assistance caseloads have increased rapidly to a historical high point and then decreased with even greater speed to their lowest level in decades. Several recent papers have focused on the rise in Aid to Families with Dependent Children (AFDC) caseloads in the early 1990s and the turn around in the mid 1990s. This research indicates that both macroeconomic factors and program factors appear to be important for these changes. A key question is whether these recent declines are permanent and how much they might turn around in a more sluggish economy. This paper is focused on the relationship between recent caseload changes and the overall economy, comparing estimates from a wide variety of models using both annual and monthly data. By using monthly data, which is available through late 1998, we also present several rough estimates of the impact of welfare reform after 1996.

The Food Stamp program has also experienced major program changes, although it has remained relatively unchanged for single mothers and their children who once participated in AFDC. This paper also provides a detailed comparative analysis of AFDC/TANF caseload changes with food stamp caseload changes.

The welfare reform legislation of 1996 has been cited as a primary reason why AFDC/TANF caseloads began a steep decline in the mid 1990s. This legislation, which states were required by law to implement by July 1997, abolished the AFDC program and replaced it with the TANF block grant, giving states much greater discretion over the design of cash assistance and related work programs for low-income families. The extent of change in state public assistance programs following this legislation has been enormous. Virtually all states have implemented major changes in the way in which they determine eligibility, require and support work effort, and organize their public assistance offices.[1]

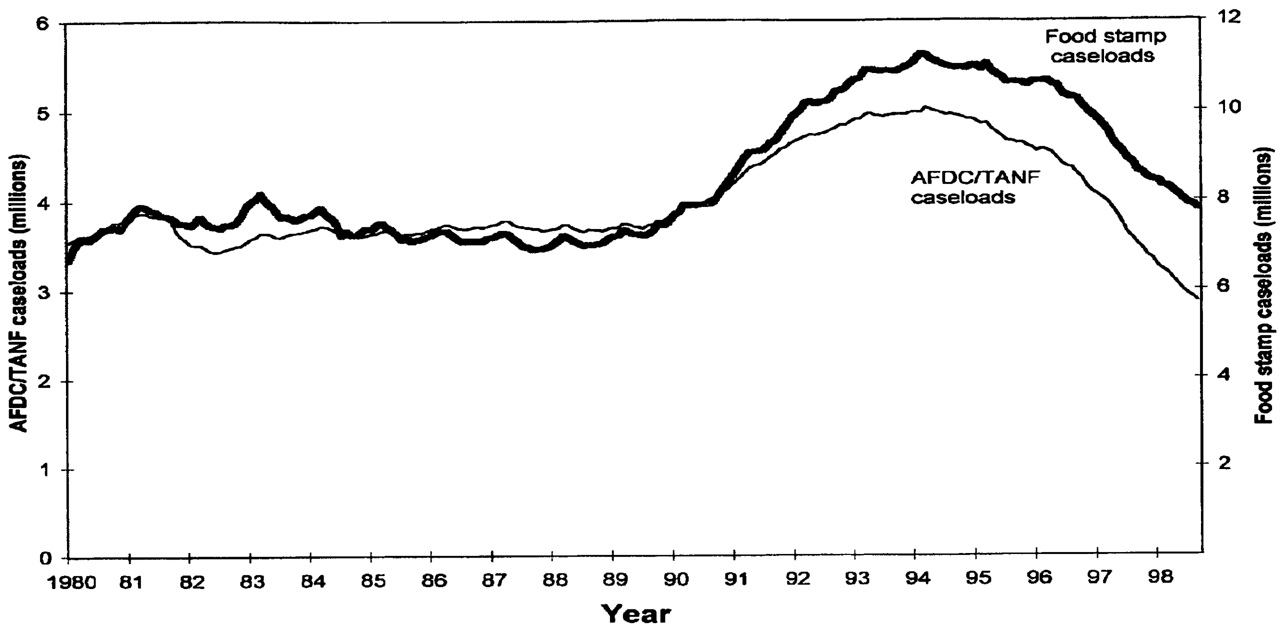
These program changes occurred in a very strong labor market. At the end of 1998, the unemployment rate was at a 30-year low. Among workers who lacked a high school diploma, unemployment was near 7 percent, after being in the double digits for years. This has led many observers to suggest that program reform may be less important to the decline in caseloads than many states are claiming. The widespread availability of jobs should have produced a steep decline in caseloads even in the absence of program changes. The question of how much caseload decline can be explained by economic factors is particularly important in forecasting future caseload changes. If the decline is largely due to tight labor markets, it may be more reversible in a future economic downturn than if the decline is due to tightened eligibility rules or greater "diversion" activities (keeping people off public assistance by providing one-time assistance or requiring participation in job search activities).

Figure 1 shows AFDC/TANF and food stamp caseloads from 1980 through 1998. Note that food stamp caseloads are consistently about twice as high as AFDC/TANF caseloads.[2] This reflects the broader eligibility rules in the Food Stamp program. The unusual trends in the past decade are clearly apparent in this figure. AFDC caseloads, which were largely flat from the mid 1970s through 1990, rose by 27 percent between 1990 and 1994, but between 1994 and mid 1998 they fell by 40 percent. In June of 1998, they were at their lowest level since 1972.

Food stamp caseloads follow a remarkably similar trend. They decline slightly faster than AFDC caseloads in the mid 1990s, in part because the legislation that abolished AFDC also cut access to food stamps among a number of immigrant groups and limited their availability to families without children. But they have continued to decline even after the implementation of these changes.

The rapidity of these changes is almost unprecedented. Indeed, the caseload increases of the early 1990s were one reason behind growing support for welfare reform. In turn, the caseload declines that have

Figure 1 AFDC/TANF and Food Stamp Caseloads



occurred since the enactment of welfare reform have been unexpectedly large in the opinion of most observers. Many of the new state-designed programs funded under the TANF block grant involve time limits and extensive sanctioning policies, as well as efforts at diversion, all of which might be expected to cause caseload declines. These program changes, however, lead to an important caveat: estimates that use historical evidence on the AFDC program to predict future changes in TANF-funded programs are probably unreliable. This is particularly true with regard to macroeconomic effects. As more persons reach time limits or are sanctioned, or as state dollars are more limited, people may be less able to return to the rolls in an economic downturn. The long-term effect of current program changes on the responsiveness of caseloads to a future economic slowdown is hard to foresee with any certainty, although this is a key policy concern.

EXISTING RESEARCH ON CASELOAD CHANGE

Several recent studies have investigated the determinants underlying the caseload changes in AFDC. A host of early studies focus on state-specific data or on data from only a few years.[3] More recent studies have used panel data on caseloads across many years. This includes work by the Council of Economic Advisers (1997), Blank (1997b), Ziliak et al. (1998), and Levine and Whitmore (1998). Table 1 provides a brief comparison of this research. Except for the Blank study, these papers have focused almost entirely on the effect of economic variables and of program-related variables on caseloads. The Blank study went further in trying to utilize a host of demographic and political variables, although these appear to make little difference in her results on the impact of the economy or of programs. Blank's was also the only study to differentiate between the AFDC-Basic program for single parents and the AFDC-UP (unemployed parents) program, a much more limited program for married-couple families.[4]

One topic of concern in these studies is to understand the effect of AFDC "waivers," which allowed states to run experimental welfare programs prior to the 1996 welfare reform legislation. These waivers differed greatly across states, but typically included some combination

Table 1 Major Research on Caseload Change

Study	Data	Dependent variable	Included variables	Results on key variables
Blank (1997b)	Annual state panel 1977–95	ln(AFDC caseloads female pop. ages 15–44) (Also separates this into AFDC-Basic and AFDC-UP caseloads)	Economic (including unemployment) Program (including waivers and AFDC benefits) Demographic Political State effects Year effects	Estimated share of caseload change due to economic factors: • 23% in 1990–94 • 51% in 1994–95
	Monthly state panel 1977–96	VAR model using ln(AFDC caseloads) and unemployment rates as co-determined. (State fixed effects also included.)		One-point rise in unemployment leads to • 3.5% in AFDC-Basic •20% rise in AFDC-UP over an 18-month period
Council of Economic Advisers (1997)	Annual state panel 1976–96	ln(AFDC caseloads)/total population	Unemployment rates Program (waivers and AFDC benefits) State effects Year effects State year trends	Estimated caseload change due to economic factors: • 24–31% in 1989–93 • 31–45% in 1993–96 4.1% estimated change in AFDC caseloads due to 1-pt. increase in unemployment. –5 2% change due to waivers.

(continued)

Table 1 (continued)

Study	Data	Dependent variable	Included variables	Results on key variables
Levine & Whitmore (1998)	Same as CEA study	Same as CEA study	Same as CEA study, with more detailed data on waivers	Economic effects of same size as CEA study. Waiver states have almost twice the caseload reduction, but no difference in unemployment rates.
Ziliak, Figlio, Davis, & Connolly (1998)	Annual state panel 1987–96	Same as CEA study	Unemployment rates Waiver data State effects Year effects	4.1% estimated change in AFDC caseloads due to 1-pt. increase in unemployment. –9.1% change due to waivers.
	Monthly state panel 1987–96	Same as CEA study	Same as above	No separate estimates of economic effects alone. In 26 states with the largest caseload reduction, 78% of change due to economic *and* seasonal factors, 1993–96.

of time limits, expanded work requirements, expanded earnings disregards, strengthened sanctions, and family caps (limiting AFDC benefits to women who have additional children while receiving AFDC).[5]

Most of these waivers were approved in 1994 or later; following the 1996 legislation, many states with major waivers developed TANF-funded programs that were similar to their waiver programs. States without waivers often looked to the states with waivers for ideas about how to restructure their programs. This suggests that estimates of the impact of these waivers on caseload changes might provide a minimal estimate of the expected effect of welfare reform. We return to this issue below.

As Table 1 demonstrates, the research using annual panel data by state and year produces reasonably consistent results. A 1-point increase in the unemployment rate appears to produce about a 4 percent increase in the AFDC caseload share, while the implementation of a waiver program produced a 5–10 percent decline in caseload share. The economic cycle variables appear to explain about one quarter to one-third of the total caseload change in the early 1990s. In contrast, Ziliak et al. utilized monthly data on caseloads and unemployment rates and found far larger effects for the macroeconomy and smaller effects for program variables. It is difficult to make direct comparisons between their monthly data results and the other studies; this is one reason we estimate both annual and monthly models below. The difference appears to be primarily due to the difference in specifications and in how the results are reported.[6]

The general conclusion from this research is that a host of variables appeared to influence caseload changes through the mid 1990s. While the macroeconomy was important, there is evidence that program changes also had a substantial impact, particularly in states which implemented early waivers. The size and the interpretation of the effect of waiver implementation remain controversial. The increase in caseloads in the early 1990s is only poorly explained in these equations; neither the macroeconomy nor program changes justify the increase that occurred.

WHY FOOD STAMP CASELOADS ARE ALSO INTERESTING

So far as we know, there has been no equivalent analysis of food stamp caseloads to date. This is perhaps surprising, since food stamps have historically been as large a program as AFDC in terms of total expenditures. From a budget perspective, large changes in food stamp caseloads are nearly as costly as large changes in AFDC caseloads, although the states do not bear the cost of food stamps.[7]

Food stamps have historically served a broader population than AFDC and have been available to all low-income persons regardless of family composition, including the elderly as well as younger childless individuals and couples. Hence, a substantial share of the food stamp population does not receive AFDC.[8] On the other hand, most AFDC recipients are food stamp recipients. This share of the food stamp caseload should move in very similar ways to the AFDC caseload,[9] but other food stamp recipients may be less affected by program changes directed at the single-parent AFDC population and may respond differently to changes in the economic environment as well. Some of these other food stamp recipients are elderly and may be quite unresponsive to macroeconomic fluctuations, while others are single individuals or childless couples who might respond more strongly to changes in economic opportunities than single mothers.

The Food Stamp program also experienced major program changes in the mid 1990s. A one-time reduction in food stamp eligibility occurred as a result of the 1996 legislation, when most immigrants were removed from the rolls.[10] In addition, for many areas of the country, childless individuals or families were time-limited in their receipt of food stamps. For most single-parent families—the group that historically received both AFDC and food stamps—little changed in the Food Stamp program, but food stamp receipt has historically been closely linked to AFDC receipt, and the major changes in AFDC/TANF programs will affect food stamp eligibility. For instance, if welfare-to-work programs move most people into intermittent or very-low-wage jobs, they are likely to retain their food stamp eligibility. On the other hand, if these programs move families out of poverty, then food stamp caseloads will fall with TANF caseloads.

As Figure 1 illustrated, food stamp caseloads and AFDC/TANF caseloads have moved in similar ways. This is somewhat surprising. One might have expected the economic expansion that started in 1991 would have affected food stamp caseloads sooner than AFDC. Similarly, because food stamp eligibility was not as affected by waivers and state reform efforts in the mid 1990s, one might not have expected ongoing food stamp caseload declines once food stamp eligibility changes were implemented in 1997.

The close historical correlation between food stamp and AFDC caseload changes suggests that the behavioral and program changes driving AFDC recipiency also affect food stamp recipiency. In this case, the biggest uncertainty in the future evolution of the food stamp program may be whether the transformation from AFDC to TANF will fundamentally change who does or doesn't use food stamps. Historically, AFDC recipients were categorically eligible for food stamps and most states had combined eligibility determination procedures. With the implementation of time limits and restricted eligibility in many state TANF-funded programs, the number of food stamp eligibles who also receive TANF is likely to fall. Whether or not this expected reduction in the TANF-eligible food stamp population results in a fall in food stamp caseloads depends on whether individuals know about their continuing food stamp eligibility.[11] Whether food stamp receipt will remain as closely linked to TANF receipt as it was to AFDC receipt remains to be seen.

ESTIMATES OF THE DETERMINANTS OF BOTH AFDC/TANF AND FOOD STAMP CASELOADS

In this section, we provide comparative estimates of the determinants of both AFDC and food stamp caseloads based on annual panel data. Table 2 presents a set of regression estimates of the determinants of AFDC total caseloads based on three different specifications (columns 1 to 3) and compares these with caseload determinants for several food stamp caseload specifications (columns 4 to 6). Annual panel data on caseloads by state and year are used from 1980 through 1996.

Table 2 Estimates of the Determinants of Public Assistance Caseloads[a]

	Col. 1 Total AFDC cases[b]	2 Total AFDC cases[c]	3 Total AFDC cases[d]	4 Food stamp cases[e]	5 Residual food stamp cases[f]	6 Food stamp cases[g]
Unemployment rate	–0.004	0.015**	0.008*	0.015**	0.015	0.007*
	(0.006)	(0.006)	(0 005)	(0.005)	(0.009)	(0.004)
Unemployment rate$_{-1}$	0.021**	0.022**	0.020**	0.019**	0.021*	0.007
	(0.009)	(0.007)	(0.005)	(0.006)	(0.012)	(0.005)
Unemployment rate$_{-2}$	0.047**	0.023**	0.019**	0.033**	0.049**	0.021**
	(0.006)	(0.005)	(0.004)	(0.005)	(0.009)	(0.004)
ln(median wage)	–	–0 644**	–0.297*	–0.557**	–0.550**	–0.229**
		(0.137)	(0.134)	(0.121)	(0.234)	(0.100)
ln(20th wage percentile)	–	–0.115	–0.130	–0.103	–0.247	–0.044
		(0.106)	(0.095)	(0.093)	(0.180)	(0.076)
Percent black	–	0.291	3.034*	1.704**	2.377*	1.555**
		(0.871)	(1.496)	(0.765)	(1.482)	(0.624)
Percent single female heads	–	–0 495	–0.302	1.254**	3.697**	1.505**
		(0.526)	(0.428)	(0.462)	(0.894)	(0.377)
Percent nonmarital births	–	1.392**	1.025**	1.084**	0.870*	0.375*
		(0.255)	(0.295)	(0.224)	(0 435)	(0.187)
Years of education	–	–0.140**	0.078*	–0.117**	0.063	–0.046
		(0.040)	(0.045)	(0.035)	(0.068)	(0.029)
Percent elderly	–	–1.204**	0.565	–0.866*	–0.084	–0.253
		(0.459)	(0.424)	(0.403)	(0 781)	(0.330)

Percent immigrants$_{-1}$ (×100)	–	–0 017 (0 025)	0.015 (0.019)	0.019 (0.022)	0.094* (0.042)	0.027 (0.018)
Percent immigrants$_{-2}$ (×100)	–	0.020 (0.027)	–0.024 (0.021)	0.027 (0.023)	–0.025 (0.045)	0.017 (0.019)
Party of Governor (1=Republican)	–	–0.050** (0.008)	–0.042** (0.007)	–0.064** (0.007)	–0.089** (0.014)	–0 039** (0.006)
Both state Senate & House Democratic	–	–0.004 (0.013)	–0.014 (0.011)	–0.004 (0.011)	–0.017 (0.022)	–0.002 (0.009)
Both state Senate & House Republican	–	–0.042** (0.017)	–0 010 (0.014)	–0.031* (0.015)	–0.003 (0 028)	–0.010 (0.012)
AFDC-UP program (1=yes)	–	0.163** (0.015)	0.128** (0.016)	0.068** (0.013)	–0.022 (0.025)	–0 015 (0 011)
ln(maximum AFDC benefit level)	0.542** (0.071)	0.532** (0.061)	0.203** (0.062)	–0.068 (0.054)	–0.413** (0.104)	–0.339** (0.046)
Any major waiver	–0.104** (0.023)	–0.072** (0.020)	–0.040** (0.018)	–0.032* (0.017)	0 061* (0.033)	0.005 (0.014)
AFDC caseloads	–	–	–	–	–	0.509** (0.026)
State effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
State time trends	No	No	Yes	No	No	No
Number of obs.	850	850	850	850	850	850

(continued)

Table 2 (continued)

[a] Dependent variable is ln(caseloads/total population). All regressions based on data for 49 states and D.C. from 1980–96. (Data on food stamps in Vermont are not available for this time period. For consistency we drop this state in the AFDC estimates as well.) Standard errors in parentheses. ** indicates significance at the 1 percent level; * at the 5 percent level.

[b] Specification includes unemployment rate + program variables + state and year effects.

[c] Specification is that of Col. 1 + control variables (see pp. 62–63).

[d] Specification is that of Col. 2 + state specific time trends.

[e] Specification is that of Col. 2.

[f] Dependent variable is residual food stamp cases, which equals food stamp caseloads minus estimated AFDC recipients who also receive food stamps (see p. 64).

[g] Specification is Col. 4 + AFDC caseload control.

The dependent variable is the ln(share of caseloads), with caseloads in each state and year divided by total state population.

The models in Table 2 include fixed effects for each state and for each year. This allows each state to have a different constant level of caseloads and controls for any omitted variables in the specification that might be largely constant within states over time. This specification essentially allows the coefficients to be interpreted as the effect of changes in the independent variables over time within a state on a state's caseload. We discuss below the impact of also including state-specific time trends in these models.

The first column presents a specification similar to that used by the CEA (1997), which is quite sparse and includes only unemployment rates and a few program variables. The results are estimated over a longer time period and include 1996, for which data were not available when the CEA report was produced. The second and third columns use a much richer specification, originally utilized by Blank (1997b). The independent variables include four sets of variables.[12] First, a set of economic-related variables are included. The unemployment rate (current, lagged one year, and lagged two years) is probably the best state-specific measure of economic cyclicality. In addition, we have calculated the natural logarithms of median wages and of the 20th quintile of the wage distribution.[13]

Second, a set of state demographic variables are included, consisting of percent elderly, percent black, percent single-female headed families, percent non-marital births, average years of education, and percent immigrant. The immigrant share is defined as the number of newly admitted immigrants in a state divided by the state's total population. The immigrant share is lagged by one and two years to allow time for increases in immigration to affect public assistance caseloads. Third, a set of political variables are included, based on the political affiliation of the governor, whether both state legislative houses are Republican, and whether both state legislative houses are Democratic.

Finally, we include program-related variables. AFDC benefit levels measure the maximum cash support available to a four-person family in the state. A dummy variable for the presence of an AFDC-UP program is included where appropriate. We also include a variable for the share of the year in which a state has a major program waiver approved for implementation in the post-1991 period.[14] As noted above, these waivers allowed states to implement major variations to the AFDC program and were precursors to the TANF-funded programs that flourished after welfare reform.

The coefficients in columns 1 through 3 provide an indication of how AFDC caseloads respond to these variables, particularly the economic and program variables. Column 1 shows results for a model that includes only unemployment rates and program variables, i.e., those variables which often receive the most attention from policymakers. Although the estimating period and the specification are slightly different, the results are very similar to those reported by the CEA (1997). Unemployment has a strong relationship to caseloads, although much of this effect occurs only over time. Three years after a 1-point rise in the unemployment rate, caseloads will have risen by 6.4 percent. Program variables also have strong effects. States that raise their AFDC maximum benefits levels experience a rise in caseloads; states that implemented waivers experienced a decline in caseloads, all else equal.

As earlier researchers indicated, interpreting this waiver coefficient is difficult. These effects are almost surely more than just the direct program effects of the waivers; they probably also include "demonstration effects," whereby individuals who were actually unaffected by the waivers nonetheless changed their behavior because of the strong message states were trying to send that they were going to "get tough" on welfare recipients. Evidence of this is in Blank (1997b), who showed that states with waivers actually saw significant declines in their caseload in the year before the waiver was granted.

Even with state fixed effects included, one might worry that the model in column 1 excludes a large number of variables that might reasonably affect caseloads within a state over time. Column 2 provides a much richer specification. The most striking result in column 2 is that the impact of unemployment and of program variables is quite similar, even when a very rich set of other control variables is included in the model. Although the timing of the unemployment effect is somewhat different in column 2, a 1-point rise in unemployment results in a 6.0 percent rise in caseloads over a three-year period, very similar to column 1. The coefficients on AFDC benefit levels and waivers are virtually identical between the two columns; this is true even though the additional variables in column 2 are collectively quite important in explaining caseload changes over time.

Changes in median wages, in non-marital birth rates, in years of education, and in percent elderly are all important in determining state caseload changes. With regard to state political variables, Republican governors appear to negatively affect AFDC caseloads. There is an additional negative effect on AFDC caseloads if both state legislative branches are controlled by Republicans. The magnitude and significance of these political effects indicate that even prior to TANF, states could affect caseloads, probably through their organization of public assistance offices and the messages which case workers were instructed to deliver to clients. But these variables must be largely uncorrelated with the unemployment and program variables in column 1, since their inclusion has little affect on those coefficients.

Column 3 includes state-specific time trends, in addition to state and year effects. On the one hand, this controls more fully for omitted variables within states that might be trending up or down over time. On the other hand, the effect of the included variables that are validly correlated with caseload changes may be reduced by the inclusion of state-specific time trends if those variables also trend up or down gradually over time. Our own preference is for the specification in column 2, which does not include these state-specific time trends that we think may overcontrol for omitted variables, but the results in column 3 provide a comparison for those who prefer to include state-specific trends.

Column 3 indicates that including controls for state-specific time trends reduces the magnitude of most coefficients, as expected. Yet, almost all of the same variables are significant in columns 2 and 3, and the general conclusions about what drives caseload changes over time within states would be similar regardless of the specification. In column 3, a 1-percentage-point rise in unemployment results in a 4.7 percent increase in caseloads over a three-year period. We interpret column 3 as indicating that state-specific time trends do not change most of the larger conclusions about the determinants of caseloads.

Because we prefer the specification in column 2, we use that specification in analyzing the food stamp data in columns 4 through 6. We have repeated the food stamp regressions with state-specific time trends included, and the results are similar to those seen in comparing columns 2 and 3 (data not shown).

Column 4 estimates total food stamp caseloads using a specification identical to column 2. Ideally, given the very diverse populations on food stamps, one would like to look at single-parent food stamp recipients separately from other recipients, just as one might separate AFDC-Basic and AFDC-UP recipients. Unfortunately, there are no data available which provide regular information on food stamp receipt by family composition by state. Hence, we try two different approaches in order to separate the AFDC population from the rest of the food stamp recipients.

First, we attempt to net out the AFDC population from food stamp caseloads. In each year, we know nationally how many AFDC recipients also receive food stamps.[15] We take this share and multiply it by the number of AFDC recipients in each state and subtract this from the food stamp caseloads. This should leave us with a dependent variable that provides an estimate of non-AFDC food stamp recipients, which we refer to as "residual food stamp cases." This number is used as the dependent variable in column 5. Note that this dependent variable is measured with error; in general, measurement error in the dependent variable will not bias the estimates, but it will increase the standard errors.

Our second effort to net out the effect of AFDC is seen in column 6, where we include AFDC caseloads as a control variable in the regression for total food stamp caseloads. Since food stamp and AFDC recipiency are often jointly determined, there are some endogeneity problems with this approach. Thus, we prefer the estimates in column 5, but we provide the estimates in column 6 as a comparison.

Begin by comparing the determinants of food stamp caseloads in column 4 to the AFDC caseload estimates in columns 1 through 3. Food stamps are more responsive to the unemployment rate than AFDC. A 1-point rise in the unemployment rate will increase the food stamp caseload by 6.8 percent over a three-year period.[16] Like AFDC, food stamp caseloads are also responsive to median wage levels in the state. Food stamps also appear to be more responsive to demographic factors than AFDC. The percent black, the percent single female heads of household, and the percent of nonmarital births significantly increase food stamp caseloads, while years of education and the percent elderly decrease food stamp caseloads.

The political variables have very similar effects on both AFDC and food stamp caseloads. This is unexpected, since there are no avenues by which states can directly affect food stamp eligibility and payment rules through state legislation or regulations. However, to the extent that AFDC and food stamp recipiency are jointly determined, discouraging AFDC participation may also discourage food stamp participation. This historical evidence of a tight link between food stamp caseloads and variables that can only affect food stamps through AFDC receipt is consistent with more recent stories which suggest that current food stamp caseloads are being affected by women leaving the TANF program.

AFDC benefit levels are not highly correlated with food stamp caseloads, although the presence of an AFDC-UP program does cause higher food stamp caseloads, perhaps because it provides easy access to food stamps for the AFDC-UP population. The implementation of waivers appears to have a negative effect on food stamps, although smaller than their effect on AFDC. This result further suggests that food stamp participation is linked to AFDC utilization, since few of these waivers involved changes to food stamp rules, *per se*. Food stamp recipients may also experience some of the same demonstration effects as AFDC recipients, hearing the message about "getting tough" on welfare recipients without clearly distinguishing that it does not apply to the Food Stamp program.

Comparing columns 4 and 5, column 5 provides an (admittedly imprecise) measure of food stamp usage among non-AFDC recipients. We see somewhat stronger responsiveness to unemployment in column 5. A 1-point rise in unemployment results in an 8.5 percent rise in residual food stamp caseloads over three years. The coefficients on wages and on demographic effects are generally similar to those for total food stamp caseloads.

The determinants of residual food stamp usage show strong policy responsiveness. The presence of a major waiver increases the non-AFDC food stamp population, while the increases in the level of AFDC benefits decrease the non-AFDC food stamp population. The sign of the effect of a major waiver and AFDC benefit levels on residual food stamp caseloads is consistent with the hypothesis that the people who are pushed off AFDC because of waiver implementation or falling real benefit levels remain in low-wage employment, thus, retaining their food stamp eligibility. Hence, higher AFDC benefits result in fewer non-AFDC food stamp cases, and waivers (which reduce the AFDC caseload) result in more non-AFDC food stamp cases.

There are some difficulties in the interpretation of column 6, which includes AFDC caseloads as an independent variable explaining food stamp caseloads, because of the endogeneity between AFDC caseloads and food stamp caseloads. AFDC caseloads are highly correlated with food stamp caseloads, and once AFDC caseloads are included in the food stamp regression, other variables generally become much less significant.

In general, the results in Table 2 demonstrate that food stamp caseloads have been quite closely tied to AFDC caseloads, and AFDC program variables affect food stamp receipt. The determinants of food stamp caseloads appear quite similar to the determinants of AFDC caseloads, although food stamp caseloads are somewhat more cyclical and more affected by a range of demographic characteristics.

HOW WELL DO THESE ESTIMATES EXPLAIN BOTH THE RISE AND FALL OF CASELOADS?

The sharp rise and fall in caseloads in the 1990s raises the question of how well these estimates are explaining this pattern. At some level, it would be very surprising if they fully explained these changes; such dramatic changes in program utilization are rarely well explained by smoothly changing economic or demographic variables.

Figure 2 provides a sense of how (in)effectively the rise and fall in caseloads is explained by the control variables in Table 2. The figure shows the value of the year fixed effects from 1985 through 1996 (with 1985 normalized to 0) for AFDC and food stamp caseload shares (columns 2 and 4 in Table 2). These fixed effects measure the unexplained caseload level in that year (relative to 1985), after the effects of the included variables on caseloads are taken into account. If the regressions fully explain all the variation in the data, the year effects should be zero in all years. If, however, there is a rise or fall in the dependent variable over time which the included variables do not account for, then the year effects will rise or fall.

Figure 2 Year Fixed Effects from the Total AFDC and Food Stamp Share Regressions (1985 normalized to 0)

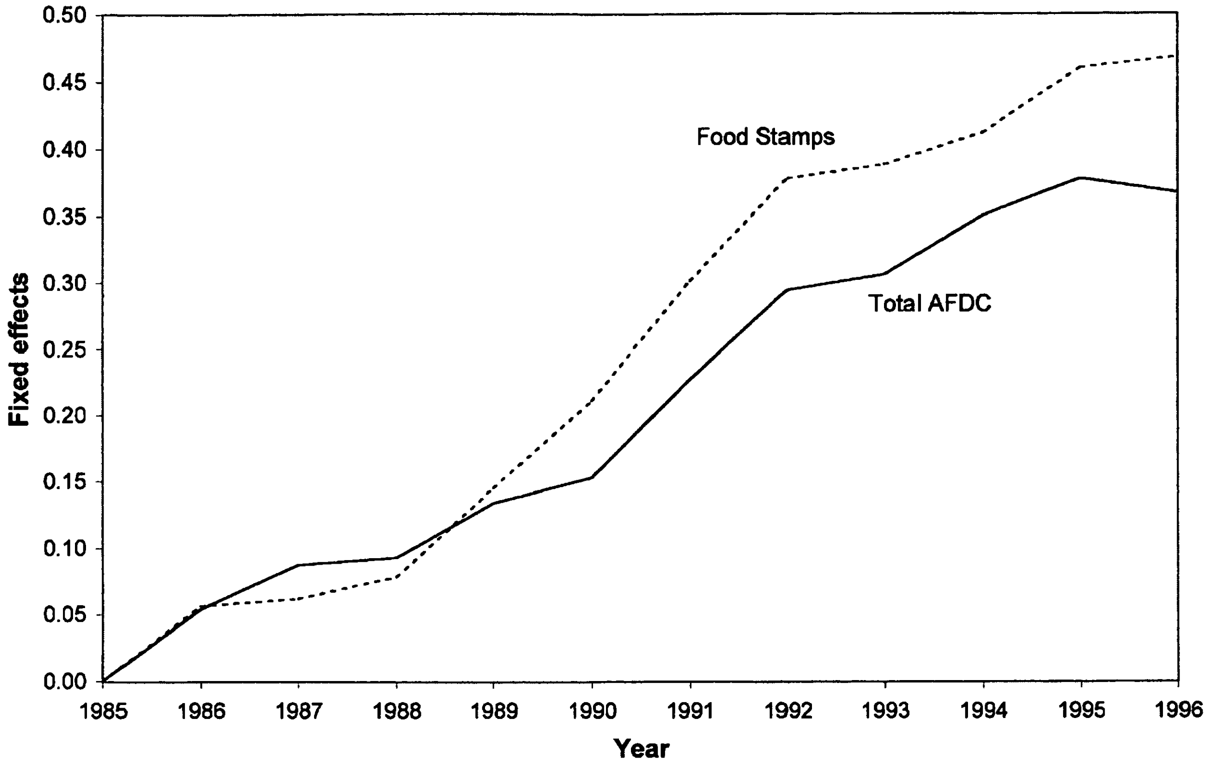


Figure 2 suggests that there was a significant unexplained increase in both AFDC and food stamp caseloads during much of the period from 1985 to 1996. Interestingly, as Blank (1997b) indicated, this unexplained increase starts around 1985, although the actual caseload data is flat from 1985 to 1990. This increase in the year fixed effects after 1985 suggests that the included variables predict that caseloads should have fallen between 1985 and 1990; instead they remained constant. From 1985 to 1989 unemployment fell, median wages rose, and AFDC benefits declined. All of these should have led to a decline in caseloads during these years, which did not occur. A mild recession in 1990 and 1991 was followed by an economic expansion and changes in the political and policy environment that (by 1994) should have produced much lower caseloads than were realized.[17]

Table 3 provides additional information on how well (or how poorly) these regressions predict actual caseload changes over these years. Columns 1 through 3 provide information on actual and predicted changes based on the annual panel data models for AFDC cases, food stamp cases, and residual food stamp cases. The table focuses on three periods:1990–94, when caseloads were rising rapidly; 1994–96, the period for which we have a complete set of control variables and when caseloads began to fall; and 1994–98, the entire recent period of caseload decline (for which we have only limited information on other variables.)

Between 1990 and 1994, the ln of AFDC-Total caseload share rose by 0.18 points (row 1, column 1), but our regression forecasts that it should have fallen by 0.02 points (row 2). In short, over this time period, the model has no predictive power at all; none of the caseload increase is explained. This does not mean that none of the variables have predictive power, however. Changes in unemployment alone would have predicted 50.5 percent of the actual caseload rise (row 3), but these changes were offset by strong predicted falls due to movements in demographic and program variables. Although only a few states implemented waivers during this time period, these waivers by themselves would have caused a 13 percent decline in caseloads (row 4).

In contrast, the food stamp caseload share prediction (column 2) for 1990 and 1994 at least moves in the same direction as the actual caseload, but far under-predicts the actual rise that occurs. The regression predicts a 0.10-point increase, when in reality a 0.30-point

Table 3 **Predicted versus Actual Changes in Public Assistance Caseloads, and Share Explained by Economic Factors**[a]

	Models from annual panel data			Models from monthly panel data			
				24 lags, no lagged dependent variable		12 lags, with lagged dependent variable	
	1	2	3	4	5	6	7
Years	AFDC-Total[b] caseload share	Food stamp caseload share	Residual food stamp caseload share	AFDC-Total caseload share	Food stamp caseload share	AFDC-Total caseload share	Food stamp caseload share
1990–94							
Actual change	0.175	0.300	0.397	0.252	0.362	0.252	0.362
Predicted change	–0.020	0.100	0.297	0.036	0.068	0.083	0.078
% of actual predicted by:							
Unemployment alone	50.5	34.3	34.9	19.8	20.3	36.5	22.4
Waivers alone	–13.1	–3.4	4.9	–5.3	–1.5	–3.8	–0.8
1994–96							
Actual change	–0.149	–0.084	–0.078	–0.113	–0.029	–0.113	–0 029
Predicted change	–0.165	–0.139	–0.109	–0.080	–0.083	–0.053	–0.044
% of actual predicted by							
Unemployment alone	47.4	95.9	136.1	39.2	226.1	20.9	122 1
Waivers alone	21.5	16.9	–34.7	31.3	57.8	25.8	26 8

(continued)

Table 3 (continued)

				Models from monthly panel data			
				24 lags, no lagged dependent variable		12 lags, with lagged dependent variable	
	Models from annual panel data						
1994–June 98							
Actual change	–0.621	–0.350	NA	–0.611	–0.269	–0.611	–0.269
Predicted change	NA	NA	NA	–0.535	–0.340	–0.386	–0.249
% of actual predicted by							
Unemployment alone	18.7	37.6	NA	12.2	41.3	8.0	26.7
Waivers and welfare reform dummy variable	NA	NA	NA	75.4	84.8	55.2	65.7

[a] The actual and predicted change in each column are based on the ln(caseload share of the total population).

[b] For 1994–98 rows, this column is AFDC/TANF caseload share.

increase occurs. This suggests that two-thirds of the food stamp caseload increases between these years is unexplained by the model. Changes in the unemployment rates alone would have predicted 34 percent of the increase that actually occurred. Changes in residual food stamp caseloads are better explained by this model (recall that they are more affected by demographic and economic factors). Only 25 percent (0.10/0.397) of the rise in residual food stamp caseloads is unexplained.

The model does not do quite so badly for the 1994–96 period when caseloads begin to decline. For both food stamps and AFDC, the regression predicts a larger decline than actually occurred. For AFDC, the regression predicts a 0.17-point decline in the caseload share; the actual decline was 0.15 points. Changes in the unemployment rate explain just under half of this decline. For food stamps, changes in unemployment explained 96 percent of the decline in total caseloads and predict a substantially larger decline for residual food stamps than actually occurred. The implementation of waivers in a growing number of states explain another 21 percent (17 percent) of the decline in AFDC (food stamps). This suggests that information on unemployment and waivers would explain 69 percent (112 percent) of the total decline in AFDC (food stamp) cases.

At the bottom of Table 3 we include information on TANF caseloads and explore the decline in caseloads through mid 1998. Because we lack information on many of the explanatory variables in the annual data model for 1998, we cannot predict an aggregate 1998 caseload number; however, we do have actual information on unemployment, which we can use to predict the share of caseload change due to unemployment alone.[18] The AFDC/TANF caseload share fell 0.62 points from 1994 through mid 1998, with particularly steep declines post-1996. The unemployment rate alone explained 19 percent of this decline, and 38 percent of the decline in food stamps. This suggests that the most recent and rapid decline in TANF and food stamp caseloads is only partially explained by economic factors.

The results in Table 3 indicate three things. First, changes in caseloads in the 1990s are only poorly explained by these regressions. None of the increase in AFDC and only a small share of the increase in food stamps in the early 1990s is predicted by these models. Second, if unemployment alone was used to predict caseload change, it would explain about half of the increase in caseloads in the early 1990s, but only about 20 percent of the decline in caseloads in the mid 1990s. The inability of economic factors to explain the dramatic fall in caseloads after 1994 suggests that other factors have influenced participation in TANF-funded programs in recent years. This is consistent with the argument that welfare reform has caused changes in behavior among potential welfare recipients (more leave early or never enter) or is limiting the rolls (keeping people off or removing current recipients) through tighter sanctioning and eligibility requirements. Third, food stamp changes are better predicted by these models than AFDC changes. This is particularly true for residual food stamp cases where participation in food stamps is not tied to the AFDC program. Like AFDC, however, the majority of the change in total food stamp caseloads is unexplained by economic factors through most of the recent time period.

CROSS-CHECKING THESE ESTIMATES WITH MONTHLY DATA

In this section, we use monthly data to examine the responsiveness of AFDC/TANF and food stamp caseloads to the monthly state unemployment rate, early implementation of waivers, and program changes associated with the 1996 welfare reform legislation. These estimates serve two purposes. First, they provide an important robustness check of our estimates using annual caseload data.[19] Secondly, use of the monthly data allows us to analyze the caseload data after 1996. A major drawback of utilizing the annual panel data is that many of the dependent variables are only available through 1996, although the caseload data is available through June 1998 for AFDC/TANF and for food stamps.

With monthly data, we are forced to use a much sparser specification: the only variable available monthly by state is the unemployment rate. This lack of data limits our ability to interpret the results. For instance, if states with more rapidly plummeting unemployment rates are also states that move faster and push harder on welfare reform, then we will pick up some program effects with the unemployment variable.

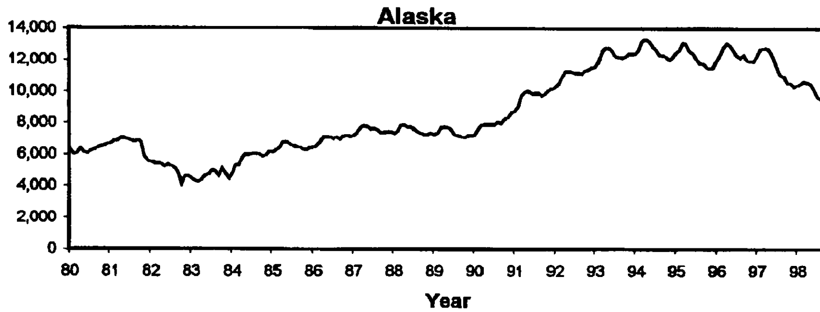
Hence, these regressions provide an alternative estimate of the extent to which employment changes are driving caseloads, but it is probably a somewhat less reliable estimate than we were able to derive in Table 2 with annual data. On the other hand, the addition of other variables to the model in Table 2 (compare columns 1 and 2) appeared to have only minor effects on the unemployment coefficients, and we take this as evidence that a sparser specification with monthly data may produce reasonably reliable results.

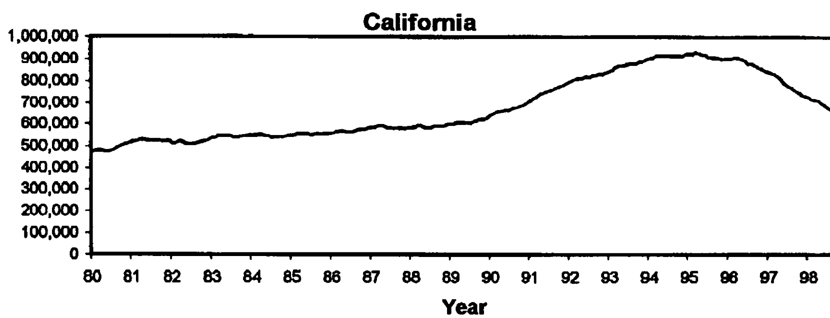
In addition to the unemployment rate, we include the waiver dummy variables described above, which "turn on" in states in the month when a waiver is approved for implementation,[20] and a dummy variable for welfare reform, which equals 1 in all months after December 1996. This latter variable will pick up any shift in the constant (in the models described below, this represents a shift in the rate of change in caseloads) after the passage of the welfare reform legislation in late 1996. The coefficient on this dummy variable will describe the average unexplained caseload change in states post-1996 after controlling for unemployment and a host of state and month fixed effects.

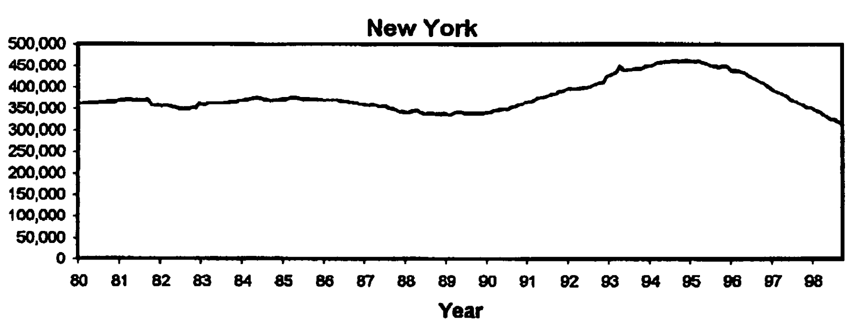
There are several difficulties in dealing with the monthly caseload data: the data is highly seasonal; seasonal patterns vary significantly across states; and the data has a strong trend. Because each state's data is very different in terms of seasonal patterns and trend, it is difficult to estimate traditional panel data models. What is needed to obtain accurate estimates from the monthly caseload data is an estimation procedure that accounts for the different patterns of seasonality and trending between states, while throwing away as little information as possible. Figure 3 shows the monthly caseload data from three states: Alaska, California, and New York. The diversity of the monthly caseload data in terms of trend and seasonal patterns is apparent. The data from Alaska is highly seasonal and exhibits a strong upward trend over the sample range; the data from California exhibits a strong upward trend but is not very seasonal; and the data from New York exhibits neither a strong seasonal pattern nor an upward trend.

The usual way of dealing with seasonality in aggregate monthly data is to include month fixed effects in the set of regressors. Because the seasonal patterns in the caseload data are not consistent across states, this approach is not ideal. With one set of monthly dummy variables each state's caseloads will be adjusted with respect to the average

Figure 3 AFDC/TANF Caseloads from Three States







state's seasonal pattern. This sort of adjustment is not a problem for states in which seasonal fluctuations are close to the average, but it is a problem for states in which the seasonal patterns are very different from the average. Take, for example, a state without a seasonal pattern in the caseload data (like New York in Figure 3). With one set of monthly dummy variables for all states, this state's caseloads will be adjusted up in months where the average state caseloads are low relative to the omitted month and will be adjusted down in months where the average state caseloads are high relative to the omitted month. The net effect of this seasonal adjustment for a state without a seasonal pattern is to add meaningless seasonal variability to the data.

An alternative approach to dealing with seasonality in the monthly caseload data is to estimate models with state-specific month effects. Dealing with the problem of seasonality in this manner requires estimating 612 (51×12) separate state-month effects, but it avoids the problem of incorrectly assigning the same month effects to all of the states despite their differing seasonal patterns. It should be noted that state fixed effects are a linear combination of state-month effects, thus the inclusion of state-specific month effects implies that the resulting estimator will be directly comparable to the specifications in Table 2.

There are several plausible approaches to dealing with the trend in the monthly caseload data. The simplest approach to dealing with the problem of strongly trending caseload data (i.e., nonstationarity) is to estimate a model with period fixed effects (i.e., a separate fixed effect for each month of data). This is the approach taken with the annual data in the Table 2 regressions. The potential drawback of this approach is that the combination of period fixed effects and state fixed effects is perfectly collinear with state-month effects if there are more than 12 years of data. This perfect collinearity means that if state and period fixed effects are included in the set of regressors, state-month effects cannot be. Since the inclusion of state-month effects is important for reasons described above, dealing with the problem of nonstationarity through the inclusion of period fixed effects is not ideal. Indeed, state-month effects provide a more flexible specification and constrain the data less than period fixed effects.

Another approach to this problem is to estimate models with state-specific time trends. Since much of the pattern in monthly state caseload data is composed of a strong upward trend and seasonal components, detrending the caseload data may throw out too much variability in the data. This is similar to our argument against including state-specific time trends in Table 2.

We adopt a third approach to address the problem of trending dependent variables, by estimating all models in first-difference form. While this is not a perfect way to deal with the problem of non-stationary monthly caseload data, it is probably the best choice given the constraint that the data from all of the states must be treated in a like manner. This approach does seem to produce stationary time series for most of the states and is probably better than the alternatives of detrending or estimating a model with period fixed effects.[21]

In order to assess the sensitivity of the results to choice of specification we estimate models with and without a lagged dependent variable and investigate the effects of different lag lengths within each model. Assume that ln caseloads in state i during period t are generated by the process

Eq. 1 $$\ln(c_{i,t}) = \sum_{j=0}^{q} \left[\beta_j u_{i,t-j} + \gamma_j w_{i,t-j} + \eta_j r_{i,t-j}\right] + \sum_{k=1}^{12} sm_{i,k} + \varepsilon_{i,t}$$

where

q = lag length
$u_{i,t}$ = the state monthly unemployment rate
$w_{i,t}$ = a binary variable indicating whether a state has a waiver in effect
$r_{i,t}$ = the welfare reform binary variable that equals 1 after January of 1997
$sm_{i,k}$ = the month effect associated with state *i* during month *k*
$\varepsilon_{i,t}$ = a random, mean zero error term.

Taking first differences and rearranging terms yields the following

Eq. 2 $$\ln(c_{i,t}) = \beta\Delta u_{i,t-q} + \gamma\Delta w_{i,t-q} + \eta r_{i,t-q} + \sum_{j=0}^{q}\Big[\beta_j(\Delta u_{i,t-j} - \Delta u_{i,t-q}) + \gamma_j(\Delta w_{i,t-j} - \Delta w_{i,t-q}) + \eta_j(\Delta r_{i,t-j} - \Delta r_{i,t-q})\Big] + \sum_{k=1}^{12} \Delta sm_{i,k} + \Delta\varepsilon_{i,t}$$

where $\beta = \sum_{j=0}^{q} \beta_j$, $\gamma = \sum_{j=0}^{q} \gamma_j$, and $\eta = \sum_{j=0}^{q} \eta_j$. Note that β, γ, and η represent the long-run effects of the unemployment rate, waivers, and the recent welfare reform legislation on caseloads. Also note that this model allows for exogenous caseload growth as long as $\sum_{k=1}^{12} \Delta sm_{i,k}$ is greater than zero. This offers some control for steady caseload growth due to changes in omitted variables such as demographic or political factors.

The first two columns of Table 4 present the estimates of the long-run effects of the unemployment rate, waiver implementation, and welfare reform on AFDC/TANF caseloads (part A) and food stamp caseloads (part B) with the lag length (q) set to 12 and 24 months, respectively.[22] The estimates of the long-run effect of unemployment from the monthly model with the lag length set to 24 months are remarkably similar to the estimates from the annual data. The estimates from the model with 24 monthly lags indicate that a one-point rise in the state unemployment rate will cause a 4 percent increase in AFDC/TANF caseloads. This estimate of the impact of the unemployment rate on AFDC caseloads is close to the 6 percent increase in caseloads associated with a one-point increase in the unemployment rate estimated using the annual data model.[23] For food stamp caseloads, the monthly model with 24 monthly lags and the annual data model both imply that the long-run effect of a one-point increase in the state unemployment rate is about a 6 percent increase in caseloads.

Table 4 Estimates of the Long-Run Determinants of AFDC/TANF and Food Stamp Caseloads by Model Specification

	No lagged dependent variable		With lagged dependent variable	
Long-run effect of	12 lags	24 lags	12 lags	24 lags
A. Dependent variable: ln(total AFDC/TANF caseloads)				
Employment ($\Sigma\beta_j$)	0.026**	0.040**	0.035	0.046
Waivers ($\Sigma\gamma_j$)	–0.079**	–0.138**	–0.107	–0.193
Welfare reform[a] ($\Sigma\eta_j$)	–0.277**	–0.347**	–0.362	–0.421
B. Dependent variable: ln(total food stamp caseloads)				
Employment ($\Sigma\beta_j$)	0.041**	0.061**	0.048	0.055
Waivers ($\Sigma\gamma_j$)	–0.025	–0.075**	–0.035	–0.117
Welfare reform[a] ($\Sigma\eta_j$)	–0.137**	–0.166**	–0.177	–0.199

[a] Dummy variable equal to 1 from first quarter 1997 onward.
** indicates significance at the 1 percent level.

The estimated long-run effect of waivers implied by the monthly data model with a lag length of 24 months is almost twice as high as the estimate generated with the annual data models. This result is not surprising considering that many of the waivers were not implemented until 1995 and 1996. If caseloads take time to adjust to the implementation of a waiver, the full effect of the waivers will not be realized until after 1996. Because the annual data only runs through 1996, it is doubtful that the specifications using annual data will pick up the full effect of the waiver.

The estimated effect of welfare reform (the post-96 dummy variable) is very large in this model, although interpreting this coefficient in any programmatic way is difficult. The model suggests that caseloads were 28 to 35 percent lower following the 1996 welfare reform legislation, all else equal. It is not possible to conclude anything about how much of this effect is due to program eligibility changes, behavior changes by clients and caseworkers, or other factors occurring at the same time. At best, this provides a maximal estimate of the impact of welfare reform on caseloads over this time period.

One assumption of the distributed lag models estimated in Table 4 is that the adjustment period to shocks in unemployment or implementation of waivers is limited to the length of the lag in the model. It is useful to see how the estimated long-run effects change when this restriction is lifted. The restriction that the adjustment period is limited to the lag length can be lifted by allowing for lagged values of the dependent variable to enter into the right-hand side of eq. 1. Modifying eq. 1 to include lagged values of the dependent variable yields the following equation:

Eq. 3
$$\ln(c_{i,t}) = \sum_{j=1}^{q} \ln(c_{i,t-j}) + \sum_{j=q}^{q} \left[\beta_j u_{i,t-j} + \gamma_j w_{i,t-j} + \eta_l r_{i,t-j}\right] + \sum_{k=1}^{12} sm_{i,k} + \varepsilon_{i,t}$$

Taking first differences and rearranging terms,

Eq. 4
$$\Delta\ln\left(C_{i,t}\right) - \Delta\ln\left(C_{i,t-q}\right) = -\alpha\cdot\Delta\ln\left(C_{i,t-q}\right) + \beta\Delta u_{i,t-q} + \gamma\Delta w_{i,t-q} + \eta\Delta r_{i,t-q} + \sum_{j=0}^{q}\begin{bmatrix}\beta_j\left(\Delta u_{i,t-j} - \Delta u_{i,t-q}\right)\\ +\gamma_j\left(\Delta w_{i,t-j}\right) - \Delta w_{i,t-q}\\ +\eta_j\left(\Delta r_{i,t-j} - \Delta r_{i,t-q}\right)\end{bmatrix} + \sum_{k=1}^{12}\Delta sm_{i,k} + \Delta\varepsilon_{i,t}$$

where $\alpha = 1 - \sum_{j-1}^{q} \alpha_j$, $\beta = \sum_{j-0}^{q} \beta_j$, $\gamma = \sum_{j=0}^{q} \gamma_j$, and $\eta = \sum_{j=0}^{q} \eta_j$.

The long-run effects of the state unemployment rate, implementation of waivers, and the recent welfare reform legislation on caseloads are given by β/α, γ/α, and η/α.

Columns 3 and 4 of Table 4 present the estimated long-run effects of the unemployment rate, waivers, and welfare reform on AFDC/TANF and food stamp caseloads for lag lengths of 12 and 24 months in models with a lagged dependent variable. These estimates are quite similar to the estimates in columns 1 and 2, which are without the lagged dependent variable. The models that allow for lags of 12 months imply slightly lower estimates of long-run effects of the unemployment rate, waivers, and welfare reform than the models that allow lags of 24 months. While the estimated effects of the state unemployment rate from the model with 12 lags are lower than those estimated from the annual data, the estimated effect of the state unemployment rate from the model that allows for 24 lags are very close to the estimates from the annual data. As in columns 1 and 2, with an additional two and one-half years of data the estimated long run effects of waiver implementation are higher than those implied by the annual data.

Columns 4 though 7 in Table 3 present the predicted versus actual changes from the monthly data.[24] Columns 4 and 5 show the estimates for AFDC/TANF and for food stamps based on the model with 24 lags and no lagged dependent variable; columns 6 and 7 provide the same figures for the model with 12 lags and a lagged dependent variable. There is no good way to determine which of these models and what lag length to use. We show these two specifications to provide a range of estimates. The fact that both of these models produce relatively similar results suggests that the results are robust to these specification choices.

As with the annual data, the models which utilize the monthly data do not do a satisfactory job of predicting the changes in caseloads between 1990–94, 1994–96 and 1994–98. The models without lagged dependent variables account for 14 percent of the growth in log AFDC/TANF caseloads between 1990 and 1994 and about 19 percent of the growth in log food stamp caseloads over the same time period. The models which allow for lagged values of the dependent variables do a better job of accounting for the growth in both AFDC/TANF and food stamp caseloads between 1990 and 1994, explaining 33 percent and 22 percent of the increases in AFDC/TANF and food stamp cases, respectively. The fact that the monthly data models do a better job than the annual data models in predicting the caseload increases between 1990 and 1994 is due to the absence of a full set of demographic, economic, and program factors in the monthly specifications; as discussed above, many of these variables suggest caseloads should be declining over this period, not rising.[25] While the monthly models do a better job than the annual models of predicting the change in both AFDC/TANF and food stamp caseloads between 1990 and 1994, they imply a smaller percentage of the increase in caseloads can be attributed to changes in economic factors.[26]

Our greatest interest is in how the monthly models handle the caseload decline between 1994-98, a period over which we could not effectively make predictions from the annual models because many of the included variables were unavailable past 1996. The monthly models predict a high share of the fall in caseloads between 1994 and 1998. These "predictive" models include, however, the dummy variable for welfare reform post 1996. A better measure of the predictive power of the monthly models is the share of the caseload decline that would have been forecast by the changes in unemployment alone. For 1994–98, unemployment changes would have forecast between 8 and 12 percent of the AFDC/TANF caseload decline and between 27 and 41 percent of the food stamp decline. These figures are reasonably consistent with the prediction from the annual data.

The results in Table 3 for both monthly and annual panel data suggest that economic factors explain only a small share of the changes in AFDC/TANF and food stamp caseloads, (although the models do a somewhat better job of explaining food stamp caseload changes than AFDC/TANF caseload changes). This is true both for the rise in caseloads from 1990–94 and for the fall in caseloads from 1994–98.

The results presented in Table 4 show only a small subset of possible specifications available for obtaining estimates of the effect of employment and program changes on caseloads using monthly data. Because the models presented in Table 4 deal with the problems of seasonality and trending in what we think is the most reasonable way, they provide what is—in our opinion—the best approach to estimating these relationships. It is, however, important to note that alternative specifications provide different estimates of the impact of employment and program changes on AFDC/TANF and food stamp caseloads. In particular, Ziliak and Figlio (1999) come to very different conclusions about the relative impact of employment and waiver implementation on AFDC caseloads using monthly data and estimate much smaller effects of waivers.

Their preferred model for estimating monthly AFDC caseloads differs from the models estimated in this section in several ways. First, their specification does not contain any lags of the waiver variable. Instead, they include a contemporaneous waiver effect as well as a binary variable which, in a first-differenced model, is equal to 1 for all months between waiver approval and waiver implementation in their set of exogenous variables. Secondly, their model incorporates a slightly different lag structure.[27] Thirdly, they estimate their models with state fixed effects and month fixed effects, while we employ state-month effects. Fourth, they include a national quadratic trend to adjust for long-run changes in national factors such as the expansion of the Earned Income Tax Credit or shifting demographic and political factors. This section would be incomplete without a few comments about which of the differences between these models are responsible for the differences in results.

We do not have data on the time between waiver approval and waiver implementation, so we can not directly test the importance of including this variable. We can, however, perform an ad hoc analysis of this issue. If all states have the same time between waiver approval and waiver implementation, then the combination of a contemporaneous waiver effect along with this implementation lag variable is equivalent to restricting the lagged waiver coefficients in our specifications, where the waiver lag length is the number of months between waiver approval and waiver implementation. For example, suppose all states that approved a waiver for implementation waited 18 months before they actually implemented that waiver. Then, in a first-difference model of monthly caseloads, having a contemporaneous waiver effect, and a waiver implementation lag is the same as having a contemporaneous waiver effect and 18 lags of the waiver effect, with the coefficient on all 18 of the lags constrained to be the same. In the case where all states implement waivers at the same speed, we can test how important Ziliak and Figlio's restriction on the lagged waiver effects are in explaining the differences between their results and the results presented in this paper. It turns out the restrictions implied by their approach under the scenario where all states are the same do not make a measurable difference in assessing the long-run effects of the unemployment rate and waivers on AFDC caseloads.

How much of the difference between the results in Ziliak and Figlio is attributable to differing lag structures? The answer seems to be not very much. Estimating specifications equivalent to theirs but with our lag structure leads to surprisingly similar results. We do estimate slightly higher long-run effects of unemployment and waiver implementation, but the differences are small. Surprisingly, another difference that does not matter very much is the treatment of seasonality. Whether state fixed effects and month fixed effects or state-month effects are used makes very little difference in the estimated long-run effects of the unemployment rate and waiver implementation in the first-differenced caseload models.

The major factor in explaining the differences between Ziliak and Figlio's results and ours is that they include a quadratic time trend in their specifications, while we do not. When we include the quadratic time trend in our specifications, the long-run effect of unemployment remains virtually unchanged while the long-run effect of waiver implementation decreases by over half. This result is robust across all of the Table 4 specifications. What inferences are drawn about the magnitude of the effect of waiver implementation on caseloads hinges on whether you believe including a quadratic trend in models like the ones described in this section is appropriate.

We argue against including quadratic time trends in these specifications. For most states, the trend in the monthly caseload data is removed by first-differencing. After seasonally adjusting this differenced monthly caseload data, we believe that most (if not all) of the remaining variability in the data is meaningful and we should let it identify the parameters of interest. A simple look at Figure 1 will indicate why a quadratic term is highly significant, but this movement is exactly what we want to explain with the dependent variables. In our view including a quadratic time trend over adjusts the data and misestimates the actual effects of program changes over time.

CONCLUSION

This paper investigates the determinants of caseloads for both the AFDC and Food Stamp program, with particular attention to the role played by the macroeconomy. The results suggest that recent changes in caseloads appear to be due to a multitude of factors, many of them not readily measurable even with a very rich specification including economic, demographic, political, and policy-related variables. Although many of these factors are clearly correlated with caseload changes within states over time, they do not explain the recent trends well. The fact that the sharp increase in caseloads in the early 1990s is poorly explained by either our annual or monthly data models suggests that the on-going rapid drop in caseloads in the mid to late1990s is also likely to be largely unexplained by these models.

At best, the ongoing decline in unemployment rates can explain about 8 to 19 percent of the AFDC caseload declines since 1994 and about 28 to 44 percent of the food stamp caseload declines. Based on our best estimates from historical data, the expected effect of any future one-point increase in unemployment will be to increase TANF caseloads by 4 to 6 percent and food stamp caseloads by 6 to 7 percent. These estimates indicate that any future recession will surely raise caseloads, but is unlikely to bring them back to their mid 1990s level, all else equal.

This suggests that the recent caseload decline must be largely due to factors other than the strong economy. A minimal estimate of the affect of welfare reform is to forecast that welfare reform was the equivalent of implementing waivers in all states. Based on annual data, this approach indicates that welfare reform explains 8 percent (6 percent) of the caseload decline in AFDC/TANF (food stamp) caseloads from 1994–98. In reality, however, many states have implemented TANF programs that were quite different and more extensive than waivers (most notably, TANF programs typically affect a larger share of the recipient population than did many waiver programs.) A maximal effect of welfare reform from 1996–98 is the unexplained decline in caseloads, along with any ongoing effects of state waivers. Using this estimate from monthly data, welfare reform can explain up to 75 percent of the AFDC caseload decline and up to 85 percent of the food stamp caseload decline. Of course, these estimates ascribe all unexplained effects post-1996 to welfare reform and probably overestimate the effect.

The wide range between these minimum and maximum estimates indicates the need for further research to look more closely at behavioral changes in take-up, as well as state-specific changes in eligibility that might be driving these dramatic changes in caseloads. These results are certainly consistent with a story whereby potential welfare recipients are strongly influenced by a host of less-measurable factors (including their own sense of the "acceptability" of utilizing public assistance) when deciding whether or not to participate in public assistance.

The food stamp caseload has historically moved in very similar ways to the AFDC caseload. Given that many AFDC recipients also receive food stamps, the correlation in historical patterns of AFDC and foods stamp caseloads is not surprising. More surprising is the observation that food stamp caseloads appear to be influenced by political and program variables that should have no direct effects on the food stamp program but which do affect AFDC receipt. This tight historical correlation between food stamps and AFDC receipt raises major questions about the effect of current welfare reform on food stamp usage. It remains to be seen whether food stamp caseloads continue to fall along with TANF caseloads, or whether these two programs begin to diverge, as food stamp usage remains relatively high among low-wage working families even as many of these families leave TANF-funded services. Residual food stamps, those food stamps not received by AFDC/TANF-eligible households, are more cyclical than overall food stamps and their levels appear to be better explained by economic and demographic variables than are overall food stamp caseloads.

From a research perspective, we are just beginning to acquire the data necessary to begin to understand the impact of the recent welfare reform. Future work on caseload changes might involve more detailed coding of state-specific program changes, allowing us to identify the effect of specific program interventions on caseload changes. As more data becomes available, the inclusion of a richer set of control variables in the post-1996 period will allow us to better separate out the impact of welfare reform from the impact of other changing political and demographic factors. Finally, as data on household income, labor force behavior, and family composition become available for the post-1996 period, this can be used to identify behavioral changes and differentiate how much of the recent caseload decline is due to reductions in public assistance participation among eligibles as opposed to changes in eligibility.

Notes

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1. Many of these changes are still underway. The New Fiscal Federalism project of the Urban Institute indicates many of the state-specific changes on their Web site at <http.//newfederalism.urban.org>. Gais and Nathan (1999) provide a recent description of the nature of these state changes, while Blank (1997a) described these changes in a broader context.
2. Food stamp and TANF data are currently only available through June 1998. We use the average caseload in the first six months as the 1998 observation.
3. For instance, see Congressional Budget Office (1993) or Gabe (1992). Blank (1997b) included citations to a number of earlier studies.
4. Monthly cash benefits from AFDC were primarily available to single-parent families (known as the AFDC-Basic program), but a small number of two-parent families also received AFDC (known as the AFDC-UP program, "UP" for unemployed parents). Blank (1997b) demonstrated that the AFDC-UP program caseloads have a very different set of determinants than the AFDC-Basic program, and that the program is much more responsive to cyclical indicators. In addition, the changes in AFDC-UP caseloads over the 1990s are more readily explained by available data than are the changes in AFDC-Basic.
5. The CEA (1997) study put a great deal of effort into coding the point at which major state waivers were approved, with the assistance of those within the Department of Health and Human Services who approved the waivers. Blank (1997b) and Levine and Whitmore (1998) use this coding; Ziliak et al. (1998) used somewhat different coding.
6. Ziliak et al. reported the combined effect of the economic variables and their seasonal factors; it would be interesting to know the effects of the cyclical variables alone. The present paper presents a comparison of monthly versus annual data estimates and finds little difference in results.
7. In 1996, the average annual cost of food stamps was $1072 per person, while average annual cost of AFDC per person was $1865 (U.S. House of Representatives, 1998, Tables 7–11, 15–4, and 15–8). Both numbers include administrative costs as well as benefits paid. Historically, the Federal government has paid virtually all food stamp costs but split AFDC costs with the states through a matching

grant formula. Under TANF, the Federal payment share is substantial, but it is fixed by the block grant amount.

8. In 1996, an estimated 61 percent of food stamp households did not receive AFDC benefits.
9. Surprisingly, although virtually all AFDC recipients are eligible for food stamps, not all choose to receive them. Blank and Ruggles (1996) estimated that among single mothers eligible for both AFDC and food stamps only 54 percent received assistance from both programs; 11 percent reported receiving AFDC but not food stamps. The remainder did not participate in the AFDC program, despite their estimated eligibility.
10. Some of these changes were reversed in 1998.
11. Recent anecdotal stories suggest that, at least in some cases, when families end their TANF services, they are not being given information or encouragement to remain on food stamps.
12. Data sources and more detailed descriptions of these variables are available in Blank (1997b).
13. This data is based on the Outgoing Rotation Group data from the Current Population Survey, which provides a large enough sample to estimate annual numbers by state.
14. These waiver variables equal the share of the year they were in effect in the year in which they were approved and then equal 1 in all following years. In 1996, we turn "on" the waiver variable in September for all states, indicating the passage of the 1996 welfare reform act.
15. This is based on an annual calculation in the CPS. We actually calculate this number separately for New York, California, and the rest of the United States These two states have a large enough representation in the CPS to allow state-specific estimates.
16. The impact of unemployment on food stamp caseloads is even stronger if we use a sparser specification as in column 1.
17. Blank (1997b) also indicated that about 40 percent of the AFDC caseload increase between 1990 and 1994 is due to a rise in child-only cases, where children collect benefits but the adult caretaker is not eligible. She discussed this change at length. We do not focus on that issue here, largely because we want to compare aggregate AFDC and food stamp caseload trends.
18. In addition, we use projected population information for the total population. All other variables are maintained at the 1996 levels.
19. These estimates also provide further information on the claim in Ziliak et al. that the monthly panel data provides different answers than the annual panel data.
20. Once a waiver dummy variable is set to 1 within a state, it stays on for the remainder of the time period, even after the implementation of welfare reform. This allows states that received early waivers to show different caseload changes than states that did not and is consistent with the fact that the welfare reform legislation allowed states to continue their waiver programs.

21. The monthly data from some states is not characterized by a strong trend. Examples of states where ln(caseloads) looks to be stationary prior to differencing include Alabama, Illinois, Maine, Maryland, Massachusetts, Pennsylvania, South Carolina, South Dakota, and Wisconsin.
22. In the context of the distributed lag models in columns 1 and 2 of Table 4, the term "long run" refers to the length of the lag.
23. To see this, compare the coefficients on unemployment and waivers in Table 4 with the sum of the three unemployment rate coefficients in columns 2 and 4 of Table 2.
24. Note that the estimates in Table 3 for the monthly models are not entirely comparable to the estimates from the annual data models. This inconsistency is due to fact that all of the calculations for the annual data are computed in terms of the log caseload share while the calculations using the monthly data are computed in terms of log caseloads. The other major difference is that the regressions used to generate the figures for the annual data are weighted by the state total population, while the regressions used to generate the monthly data are not weighted.
25 The annual data models actually predict a decrease in AFDC/TANF caseload share between 1990–94, largely because of changes in demographic factors, political factors, and AFDC benefit levels.
26 In the context of the annual models, economic factors include unemployment rates, log median wages, and the log of the 20th percentile of wages, while in the monthly models the economic factors are the unemployment rates.
27. They estimate a autoregressive distributed lag model with three lags of the dependent variable and six lags of the unemployment rate.

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