**Why Did the Food Stamp Caseload Decline (and Rise)?
Effects of Policies and the Economy**

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Abstract

The Food Stamp Program (FSP) is intended to help low-income households afford a nutritionally adequate diet. Since 1990, the FSP caseload has varied widely—rising sharply in the early 1990s, dropping sharply in the late 1990s, and then rising again in the early 2000s. Welfare and food stamp policy changes, as well as the changing economic climate, are plausible candidates for explaining the path of the caseload over time. We estimate the effect of these three factors on the total caseload and on two of its components: persons in households combining cash assistance with food stamps, and persons in households where some or all are not receiving cash assistance. We find that welfare reform and the improving economy explain all of the FSP caseload decline during the late 1990s, and that policies aimed at increasing access to the FSP and the weakening economy explain about half of the FSP caseload increase in the early 2000s. Results analyzing the disaggregated caseloads are not as clear-cut, apparently because of measurement issues during the period when Temporary Assistance for Needy Families programs were implemented.

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I.   INTRODUCTION

The social safety net for parents with children in the United States has four main components: cash assistance, commonly known as welfare (and formally as Aid to Families with Dependent Children, AFDC, succeeded by Temporary Assistance for Needy Families, TANF), the Food Stamp Program (FSP), Medicaid, and the Earned Income Tax Credit (EITC). Major changes to cash assistance policies by granting states federal waivers from AFDC program requirements in the early 1990s and the passage of the federal Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in 1996, which created the TANF program, were intended to encourage work and increase the rates of welfare exits, while retaining and even strengthening other components of the social safety net—the EITC, Medicaid, and, most important for this paper, the FSP—for parents with children who complied with TANF program requirements but who were unable to achieve self-sufficiency through their own earnings (Haskins, 1999). However, a sharp FSP caseload decline during the same time period as the welfare reforms of the mid-1990s led to concern that the changes to states' cash assistance programs embodied in their TANF plans were also disconnecting eligible recipients from other means-tested programs (Government Accountability Office (GAO), 1999; U.S. Department of Agriculture, 2001; Wilde et al., 2000; Zedlewski and Brauner, 1999).

Analyses of those leaving cash assistance in the mid-1990s and late 1990s suggested that most recent welfare leavers remained eligible for food stamps, but that a substantial majority of those who were eligible did not continue to receive food stamps (GAO, 1999; Zedlewski and Brauner, 1999; Zedlewski and Gruber, 2001). Other analyses suggested that barriers to participation (e.g., the opportunity cost of complying with administrative program requirements) rose as more FSP participants were employed (McKernan and Ratcliffe, 2003). In response to such concerns, legislation, including, most important, the Farm Security and Rural Investment Act of 2002, gave states opportunities to implement policies aimed

at easing the burden of certifying and recertifying for food stamp benefits on families who were leaving cash assistance or who were combining cash assistance with earnings. There is some suggestive evidence that reducing the burden of program participation may have resulted in larger FSP caseloads (Zedlewski, 2004).

This report presents results of a project to understand the effects of FSP policies, cash assistance policies, and the economy on the total FSP caseload and on two components of it: the portion combining cash assistance and food stamps and the portion receiving only food stamps. Specifically, we consider three questions. First, to what extent did welfare policies aimed at moving cash assistance recipients into employment and off of cash assistance convert combined welfare and food stamp recipients into food-stamps-only recipients? Second, to what extent did policy changes in the mid-1990s and later aimed at decreasing barriers to participation in the FSP, especially among those with earnings, have the intended effect? Third, how large an effect did the economy have on the FSP caseload and the two components we consider?

Building on our earlier work on the determinants of the welfare caseload (Danielson and Klerman, 2004), we address these three questions using difference-of-difference methods and semi-annual state-level administrative data for federal fiscal year (FFY) 1989 to FFY 2004 (October 1988– September 2004). The difference-of-differences approach provides strong controls for spurious national trends and persistent state differences, but it prevents us from estimating the effect of national changes. Furthermore, consistent with the aggregate nature of the data we analyze, our approach is explicitly "reduced form"; that is, we analyze the effect of the FSP and welfare reforms and of the economy on the total caseload and on two components: (i) members of households in which everyone is combining food stamps and cash assistance, and (ii) members of households in which at least some members are receiving only food stamps. (We define these terms and our measures carefully below.) Thus, we model neither individual-level participation nor derivative measures of individual-level participation, such as entry and exit; we also do not model eligibility and take-up conditional on eligibility.

We find some effect of FSP and "bundled" welfare policy changes on the total FSP caseload and large effects of the economy on both the total and disaggregated caseloads. We do not find policy effects on the disaggregated measures of the caseload. We attribute our inability to detect policy effects in the disaggregated caseloads both to limitations in the definition of FSP caseloads that states are required to report and to changes in the way some states defined their caseloads combining cash assistance and food stamps as they implemented their TANF programs. We anticipate that ongoing analyses using individual-level administrative data from the Food Stamp Program Quality Control (FSPQC) files will address these issues.

The discussion of this paper proceeds as follows. The next section motivates the empirical analysis with a discussion of the expected effects of policy changes on the FSP caseload and its components. The third section describes the policies we measure, the data we use to describe them, and the results of earlier analyses. The fourth section describes our empirical specification, informed by, and intended to capture, lagged effects of policies and the economy. Sections V and VI report our parameter estimates and simulations of the implied effects of FSP policy, welfare policy, and the economy on the FSP caseload. Section VII concludes.

II.   BACKGROUND AND THEORETICAL FRAMEWORK

The FSP is intended to help low-income households afford a nutritionally adequate diet. The Food and Nutrition Service (FNS) of the U.S. Department of Agriculture (USDA) pays the entire cost of the food stamp benefit and shares the administrative costs of administering the benefit with the states. Families that have a gross income less than 130 percent of the poverty line and that meet other income, asset, and work requirements are eligible for the program.

From a broad social safety net perspective, the FSP caseload can be viewed as having two components. One component receives both cash assistance and food stamps (known as "PA"/Public Assistance); in most cases, receipt of cash assistance confers categorical eligibility for food stamps. The other component receives food stamps but not cash assistance (known as "NPA"/Non-Public Assistance).

While the two caseloads are distinct at a point in time, longitudinally they are not. Households transition between combining cash assistance and food stamps, receiving food stamps only, and not receiving any food stamps.

The size of the total FSP caseload has varied widely over the last 15 years (see Figure 1). As shown on the top half of the figure, the number of FSP participants rose sharply in the early 1990s, peaking in 1994 at 28 million. The caseload dropped sharply thereafter until the early 2000s. Since 2002, the FSP caseload has again risen sharply. However, this overall pattern hides a divergence between the PA and NPA components of the caseload, shown on the bottom half of Figure 1. (We discuss the precise definitions of the PA and NPA caseloads that states report to FNS semi-annually in Section III, below. Throughout, we use caseloads after our adjustments.) PA and NPA caseloads rose similarly through 1994 (44 and 51 percent, respectively). The total FSP caseload declined by 39 percent from January 1994 to July 2000. During the same period, both the PA and the NPA caseloads dropped, but the drop was much larger in the PA than in the NPA caseload (-52 percent versus -24 percent, respectively). Since 2000, the divergence between the two components has been even more dramatic: The PA caseload has declined slightly (-3 percent), while the NPA caseload has *increased* beyond its 1994 peak by 77 percent. Our goal is to understand the determinants of the path of the total FSP caseload and of its two components.

What would we expect to determine the size of the FSP caseload and the path it follows over time? Our approach is motivated by the standard economic theory of program participation (e.g., Keane and Moffitt, 1998; Grogger, Karoly, and Klerman, 2002). According to this theory, families jointly choose a bundle of work and program participation to maximize their utility (consumption of goods and leisure, net of any compliance burden and stigma from program participation), subject to program rules and available labor market options. In general, we would expect that as program rules become more onerous (e.g., work requirements, shorter certification periods), participation in a program becomes less attractive and enrollment falls. Conversely, as program rules become less onerous (e.g., electronic benefit

Figure 1
National Food Stamp Caseload[a]
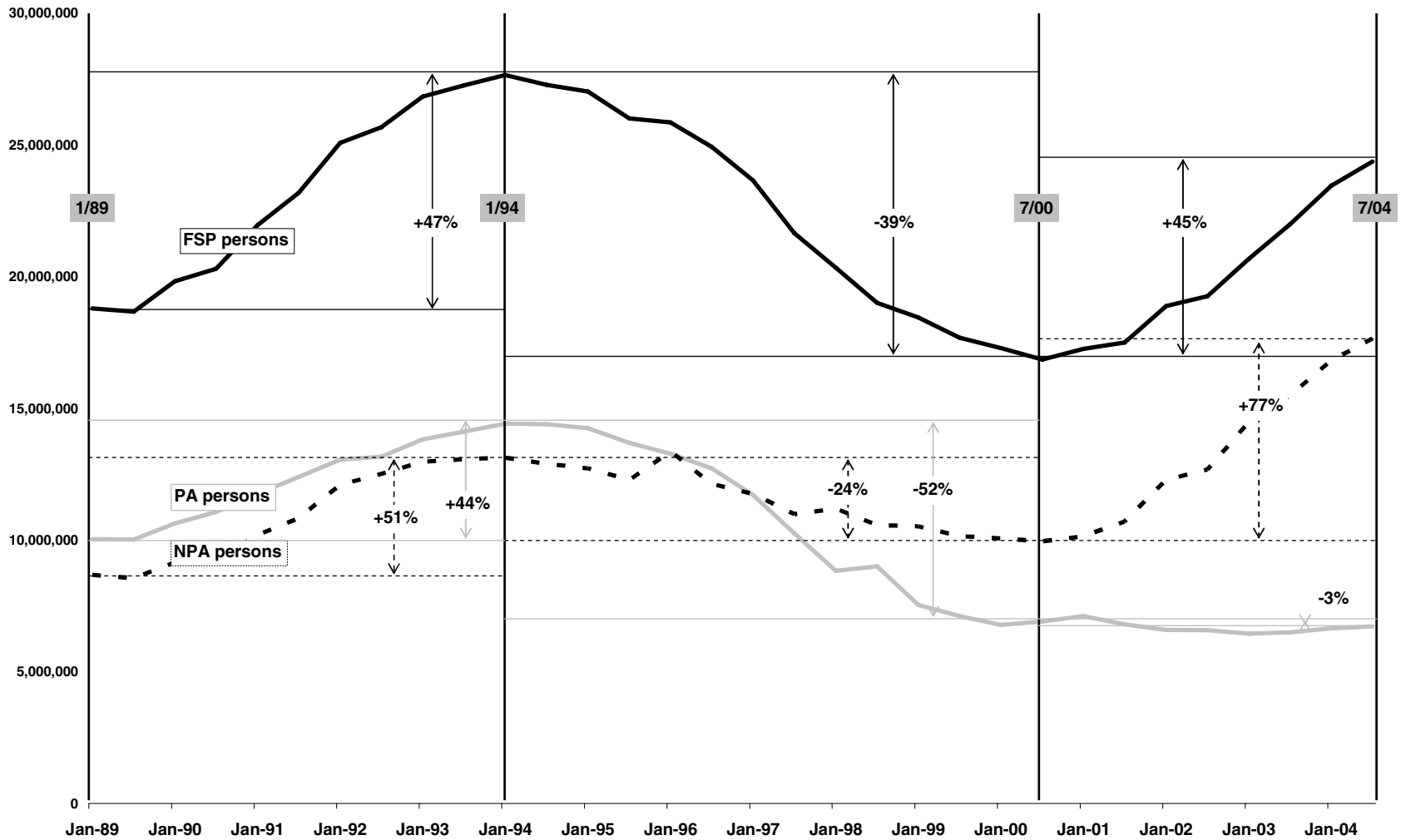
transfer, EBT, transitional benefits), we would expect participation in a program to become more attractive and enrollment to rise.

In addition, we would expect cross-program effects. As cash assistance becomes less attractive, we would expect the PA welfare-eligible caseload to fall and the NPA welfare-eligible caseload to rise, the net effect being a decline in the total FSP caseload. The net result is a reduction in the total caseload because welfare policies may have motivated recipients to enter the labor force, and some of these may have become income-ineligible for food stamps. In addition, those who leave cash assistance and remain eligible for food stamps (i.e., potential members of the NPA caseload) have a different cost-benefit calculus. Once families leave welfare, the stigma and paperwork requirements of the FSP remain nearly unchanged, but the net benefit is no longer welfare plus the food stamp benefit, but only the food stamp benefit. That off-welfare food stamp benefit is higher than if the family was receiving welfare, but much smaller than the combined welfare and food stamp benefit. Together, these two considerations imply that most changes in welfare programs that drive down the welfare caseload (and, thus, the PA welfare-eligible caseload) would be expected to also drive down the total FSP caseload, but by less than the decline in the welfare caseload (because the NPA welfare-eligible caseload would rise somewhat and because parts of the FSP caseload—those without children—are unaffected by welfare policies).

Finally, we would expect that a better economy makes work, more hours of work, and higher wages easier to find and more attractive and thus raises potential earnings. In some cases, the improved labor market conditions will lead to earnings so high as to make the family ineligible for food stamps. In other cases, earnings will remain low enough that the family remains eligible for food stamps. Note, however, that as earnings rise, families that remain eligible receive a smaller welfare payment and a smaller food stamp benefit. Given the smaller welfare payment and food stamp benefit, eligible families would be expected to be more likely to decide that the costs of participation (time to fill out the forms, loss of privacy, effort to satisfy program requirements) outweigh the benefits for which they remain eligible. Thus, we would expect welfare and FSP participation to fall as the economy improves.

Given this theoretical framework, our empirical work focuses on three factors: FSP policies, TANF policies, and the economy. We describe these policies in the next section as part of our description of our data.

III.   DATA

We begin by discussing the dependent variable: the FSP caseload. We then describe the independent variables: FSP policy, welfare policy, and the economy. As we discuss these independent variables, we also describe the underlying policies and the previous literature.

Dependent Variable

Our goal is to model the determinants of the FSP caseload and its components. Total FSP caseloads are also the data that Currie and Grogger (2001), Gleason et al. (2001), Figlio, Gundersen, and Ziliak (2000), Wallace and Blank (1999), and Wilde et al. (2000) have used. Our discussion of the determinants of the FSP caseload suggests that it would be useful to disaggregate that caseload into those receiving welfare (who would be directly affected by changes in AFDC/TANF policy) and those not receiving welfare. Figure 1 suggests that there is a substantial divergence in time paths to be explained.

Official tabulations of the FSP caseload disaggregate it into two categories—PA/"Public Assistance" and NPA/"Non-Public Assistance." Specifically, states file two reports documenting the size of their FSP caseloads to FNS: On a monthly basis, states file the FNS-388 form reporting total persons, households, and food stamp issuance. In addition, in January and July of each year, states file the FNS-388A form reporting disaggregated PA and NPA persons and households. We construct our dependent variables from the FNS-388A: total per capita FSP persons, PA persons, and NPA persons. Specifically, we use state-level semi-annual administrative data counts of total FSP, PA, and NPA per persons for January 1989 through July 2004, which represent FFYs 1989–2004 (since states file their 388A reports in January and July), which we obtained from FNS. In some cases, the FNS-388 caseload count differs from the total implied by summing the NPA and PA caseloads reported in the FNS-388A. In those instances,

we adjust the NPA and PA caseloads to match the FNS-388 total by computing the fraction of each

caseload in the FNS-388A total and multiplying that fraction by the total reported in the FNS-388.[1]

These are the available aggregate data.[2] For our purposes, this disaggregation is useful, but not

ideal. There are three issues. The first concerns the definition of PA: Those receiving some form of cash

assistance along with food stamps. The major welfare policy changes concerned the AFDC/TANF

program. Changes to other forms of federal cash assistance—Supplemental Security Income (SSI) and

local General Assistance (GA)—were smaller. Given the recent changes in policy, we would prefer to

distinguish AFDC/TANF PA cases from non-AFDC/TANF PA cases. Likewise, the NPA caseload

includes those whose family structure alone renders them ineligible for AFDC/TANF. Nevertheless,

households with children do make up the majority of the FSP caseload. The largest segment of the FSP

caseload is single-parent families with children (in 1994, 50 percent; see Table 1). Two-parent families

with children made up an additional 28 percent of the FSP caseload in 1994 and child-only cases made up

2 percent (Kornfeld, 2002).

The second issue with the available data concerns the treatment of mixed households (i.e.,

households in which some, but not all, members receive public assistance). For its tabulations, FNS

defines the portion of the caseload that is combining cash assistance and food stamps to be households in

which *every* member is receiving some form of cash assistance in addition to food stamps. This definition

is not ideal for our purposes: Members of a family receiving TANF and food stamps who are embedded

in a larger household with members receiving only food stamps are classified as NPA persons. Thus, their

transition from combining cash assistance and food stamps to food stamps alone is not captured in these

data. The issue is particularly salient because households among several important subpopulations of FSP

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[1]There are 121 cases in which the difference between the FNS-388 and FNS-388A totals was greater than 1 percent. In 60 of these cases, the difference was between 1 and 5 percent of the total caseload.

[2]The Food Stamp Program Quality Control file includes complementary individual-level information on individuals in food stamp households. We are currently exploring similar issues with those data.

recipients contain individuals not receiving TANF. These include households with immigrants, households in which children are cared for by nonrelatives (e.g., grandparents), and households with an adult subject to a welfare sanction. Thus, these households are classified as NPA and transitions of household members from combining cash assistance and food stamps to receiving only food stamps are not captured in the 388A data.

The third issue with the available data concerns data quality. During the transition from AFDC to TANF (i.e., from late 1996 forward), states began to make decisions about how to report cases that received noncash TANF-funded assistance and in some cases apparently had difficulty tracking the PA/NPA split as they changed data systems to implement TANF. As a result, there is considerable variation in the PA/NPA disaggregation during this crucial period. This noise appears to undermine our ability to estimate effects on the PA and NPA caseloads.

We model the semi-annual FSP caseload (both total and disaggregated) from January 1989 through the latest available data (July 2004). Rather than model the number of persons, we model the rate: total/PA/NPA FSP persons per capita. Doing so standardizes the measure of caseloads across states with very different populations. To compute per capita caseloads, we use annual estimates of the total population in each state published by the U.S. Bureau of the Census; we linearly interpolate between these estimates to obtain monthly population estimates.

Independent Variables

Our study considers the effect of specific FSP policy changes, the bundled effect of welfare reform (waivers from the AFDC program and the implementation of TANF), and the impact of the business cycle on the FSP caseload. Here we describe in detail those determinants of the FSP caseload, the previous literature about their effects, and how we measure them in our econometric model. We also briefly note national policy changes.

Anticipating our econometric approach (difference-of-differences) that exploits variation across states in the timing of changes in the determinants of the FSP caseload, our discussion focuses on

determinants for which there is cross-state variation. Given our econometric methods, simultaneous national changes in determinants cannot be separately estimated. Instead, the effects of such simultaneous national changes in determinants are absorbed into our models' national time dummies and into the TANF implementation variable. (See the discussion below.)

FSP Policies

Unlike the cash assistance (AFDC/TANF) program, food stamp benefit levels and most FSP policies do not vary by state (except in Alaska and Hawaii, where benefit levels are higher to compensate for the much higher cost of living). Our empirical analysis focuses on five FSP policies whose implementation (at all and when) varied across the states. Here, we discuss those policies and the findings of the previous literature with respect to their effects. We discuss our coding of state policies and dates of implementation in Appendix A. In brief, we combine published sources (starting with the USDA, 2004, and USDA, 2005b—the "State Options" reports published by FNS) with other available published sources on dates of implementation, using phone calls to the states to clarify ambiguities (usually regarding exact dates of implementation).

Payment Errors. FNS penalizes states for over- and underpayments. According to the GAO, in FFY 2000, states overpaid food stamp recipients about $976 million and underpaid them about $360 million, with errors making up about 9 percent of total payments. About half the errors were determined to have occurred because food stamp workers made mistakes; the other half were determined to have occurred because recipients deliberately or inadvertently provided inaccurate information (GAO, 2001). States are penalized equally for over- and underpayments and the two types of errors are positively correlated. Nevertheless, we enter measures of over- and underpayments separately in our models, drawing on the official error rates reported by the Office of Analysis, Nutrition, and Evaluation (USDA, 2005a). Doing so allows us to refine the hypothesis (Kornfeld, 2002) that error rates are a proxy for barriers to participation. We hypothesize that the direction of the errors is an indication of the "leanings" of the state's administration, where overpayments are a proxy for a state's interest in making food stamps

more widely accessible, and underpayments are a proxy for the opposite. We note that the 2002 Farm Bill lessened penalties for errors by penalizing only those states with combined error rates above the national average for two years in a row. Some of the previous literature finds no effect of error rates (Ziliak, Gundersen, and Figlio, 2003), while some finds that higher combined error rates are associated with higher caseloads among some populations, but not others (Kornfeld, 2002).

Electronic Benefit Transfer (EBT). PRWORA required states to convert from paper food "stamps" to EBT cards. Doing so decreases stigma and thus would be expected to increase the total FSP caseload and both of its components, all else equal. Twelve states have put only FSP (and in two cases, also Women, Infants, Children, WIC) benefits on the EBT card, while all other states have put TANF and sometimes other cash assistance on the card. The implementation of EBT might shift NPA welfare-eligible cases to PA cases if EBT cards lowered the stigma of cash assistance participation as well. State implementation of EBT occurred between April 1993 (Maryland) and June 2004 (California). Evidence of an impact of the introduction of EBT is mixed. Several analyses find no effect (McKernan and Ratcliffe, 2003; Ziliak, Gundersen, and Figlio, 2003), while several find a positive effect (Currie and Grogger, 2001; Kabbani and Wilde, 2003; Kornfeld, 2002).

Simplified Reporting. Once a household is deemed eligible for food stamps, it is certified for a period of months, the length of the certification period depending on characteristics of the household and state policy. Once that period ends, the household must be recertified (i.e., current income and family structure must be documented to prove continued eligibility for food stamps). Even between certifications, the household is required to report changes in circumstances that may affect eligibility or the benefit level. States may choose to require households to report changes within 10 days of occurrence ("incident reporting") or at specified intervals ("periodic reporting"). The latter reduces the burden on households with frequent changes in income. The simplified system further reduces the burden of

periodic reporting by requiring households to report changes that happen during a certification period only when their income rises above 130 percent of the federal poverty line.[3]

We use a conservative measure of implementation of simplified reporting. States that chose reporting intervals of at least three months (the modal policy specifies six months) and that included at least households with earned income (and often other households as well) in the simplified system are coded as having implemented simplified reporting. By the end of FY 2003, 45 states had adopted simplified reporting as we measure it (Rosenbaum, 2003; National Association for Program Information and Performance Measurement, 2004; USDA, 2004). Implementation occurred between September 2000 (Louisiana) and September 2004 (Missouri). The previous literature does not examine the impact of simplified reporting on the size of FSP caseloads; however, previous analyses do generally find that states that impose shorter recertification periods have smaller FSP caseloads (Kabbani and Wilde, 2003; Kornfeld, 2002; McKernan and Ratcliffe, 2003).

Transitional Benefits. States can provide transitional benefits when a family's TANF case closes. During this period, the family is not required to submit any documentation to prove continued eligibility for food stamps, and the food stamp benefit remains as it was before the case closed, adjusted for the loss of the TANF benefit (unless the household reapplies to have its benefit adjusted or the state opts to use information from another program in which the household participates to calculate the benefit). Before FFY 2003, only New York had adopted three months of transitional benefits. The 2003 Farm Bill lengthened the transitional period to up to five months, and by the end of FFY 2004, 13 states had adopted transitional benefits (Rosenbaum, 2003; USDA, 2004). We would expect transitional benefits to increase the total FSP caseload by increasing the NPA portion. The previous literature does not examine the impact of transitional benefits on the size of FSP caseloads.

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[3]The 2002 Farm Bill allowed states to expand the households subject to simplified reporting beyond those with earned income (GAO, 2004).

ABAWDs. PRWORA placed a time limit on benefits for able-bodied adults without dependents (ABAWDs) who do not meet work requirements; however, states could apply for waivers from this time limit based on regional economic conditions. In practice, states also had other leeway in defining their ABAWD policies. Based on a survey conducted in 2000, Czajka et al. (2001) construct an index to measure what proportion of ABAWDs in a state are subject to the ABAWD time limit. They summarize the severity of six policies using an index that ranges from 1 to 8. The higher the score, the more ABAWDs are not subject to FSP time limits. We use this index to measure states' application of the ABAWD rule. See Appendix A for details. Previous research has used only what proportion of a state's population was waived from the ABAWD requirement (Wilde et al., 2000; Ziliak, Gundersen, and Figlio, 2003). Measuring states' ABAWD policies as Czajka and his colleagues did is an improvement because a number of policies determine how many ABAWDs are subject to time limits. However, it is not ideal, because the index takes on only one value for the entire period and "turns on" when a state implemented its TANF program. Given available resources, this project was unable to construct a time-varying measure. We would expect higher ABAWD policy scores to result in higher caseloads, although ABAWDs make up only a small fraction of the FSP caseload—Table 1 indicates that adults living separately made up 11 percent of the FSP caseload in 1994—so we expect any policy effect to be small. Ziliak, Gundersen, and Figlio (2003) find that states in which a higher fraction of ABAWDs are subject to work requirements and time limits have sharply lower food stamp caseloads.

Welfare Policies

Because much of the FSP caseload is composed of households that also receive, or are eligible to receive, welfare (AFDC/TANF), welfare policies are likely to have an impact on the FSP caseload. The period covered by our data (1989–2004) was a period of major change in federal and state welfare policy. At the beginning of the period, most state welfare program policies were strongly constrained by federal regulations. Furthermore, those welfare programs had a benefit structure that strongly discouraged combining work with welfare, had weak work requirements, and no time limits (Grogger, Karoly, and

Table 1
Food Stamp Participants by Type of Household, 1994

Single adults with children	50%
Multiple adults with children	28
Adults living separately	11
Children only	2
Elderly living separately	6
Elderly with adults or children	3

Source: Kornfeld (2002).

Klerman, 2002). This changed when the U.S. Department of Health and Human Services (DHSS) granted waivers to states through 1996 and culminated in all states implementing TANF programs between 1996 and 1998. The transition to TANF substantially altered how states operate their cash assistance programs. PRWORA, the legislation that launched the TANF program, devolved responsibility for program design to the states and strongly encouraged them to reduce their caseloads. Time limits on benefits, strong sanctions for noncompliance with program requirements, and increased financial incentives to combine work and welfare characterized most state TANF programs.

In addition, PRWORA included policy changes that affected eligibility for food stamps. Many immigrants initially lost eligibility for food stamps; in addition, states were permitted to disqualify from the FSP those sanctioned for noncompliance with TANF program requirements and were required to disqualify those who did not cooperate with child support enforcement efforts. Finally, as described in a GAO report, in the absence of final regulations about the changes to the FSP contained in PRWORA, some states disproportionately raised the burden of FSP participation by, for example, not allowing households to apply for food stamps during their first visit to the welfare office.[4] Such requirements were prohibited by the FNS final rule (published January 2001; states were to implement by August 2001).

Estimates of the effect of welfare policies on the FSP caseload decline range from essentially no effect (Wallace and Blank, 1999; Wilde et al., 2000) to about a quarter of the decline (Currie and Grogger, 2001; Gleason et al., 2001). Little of the previous research on the FSP caseload has attempted to disentangle the effects of individual welfare reform policies. Only Kornfeld (2002) and Gleason et al. (2001) measure separately the effects of work-related sanction policies, welfare time limits, and financial incentives on the FSP caseload. Using the FSPQC data and measures of individual policies updated from the specification in Council of Economic Advisors (1999), Kornfeld finds that time limits and sanctions

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[4]GAO (1999) describes practices of four states and notes that FNS regional offices had not conducted timely reviews of state practices in 10 other states.

reduced the FSP caseload of single adults with children, while financial incentives increased this segment of the caseload by a small amount. Also using administrative FSPQC data, Gleason et al. (2001) find a small reduction due to sanction policies.

In our models, we include measures of when (and whether) a state had a waiver to AFDC program rules in place and when states implemented their TANF programs. We also tested models that included proxies for specific welfare policies. We do not report them in this paper because of lack of significance. Beyond restrictions in immigrant eligibility, PRWORA also made several nationwide changes in FSP eligibility and benefits that made program participation less attractive to the caseloads of interest (Gundersen, LeBlanc, and Kuhn, 1999). These changes are also captured in the year effects and in the TANF variables.

Finally, we include the maximum AFDC/TANF benefit for a family of three.

Economic and Other Policy Variables

The period we examine opened with a recession, was followed by a long and robust expansion, and closed with a mild recession (see Figure 2, plotting the unemployment rate and total FSP persons). Given the coincidence of welfare reform, FSP policy changes, and the robust economic expansion of the 1990s, controlling for the economy is crucial to estimate accurately the effects of the policies of interest.

Previous research proxies for the economy using the unemployment rate and finds that FSP caseloads are strongly countercyclical (Hanson and Gundersen, 2002). However, the magnitude of the estimated effect varies widely—from about a fifth (Currie and Grogger, 2001) to close to half (Figlio, Gundersen, and Ziliak, 2000; Gleason et al., 2001). This wide variation in the estimated effects of the economy is also found in the literature on the welfare caseload (Haider, Klerman, and Roth, 2003).

We note that previous research on the welfare caseload that includes other measures of the economy in addition to the conventional one—the contemporaneous unemployment rate—estimates a larger total impact of the economy (Haider, Klerman, and Roth, 2003; Hoynes, 2000; Danielson and Klerman, 2004). Following that literature, we include not only the state-level unemployment rate, but also

Figure 2
National Unemployment Rate and Total Food Stamp Caseload
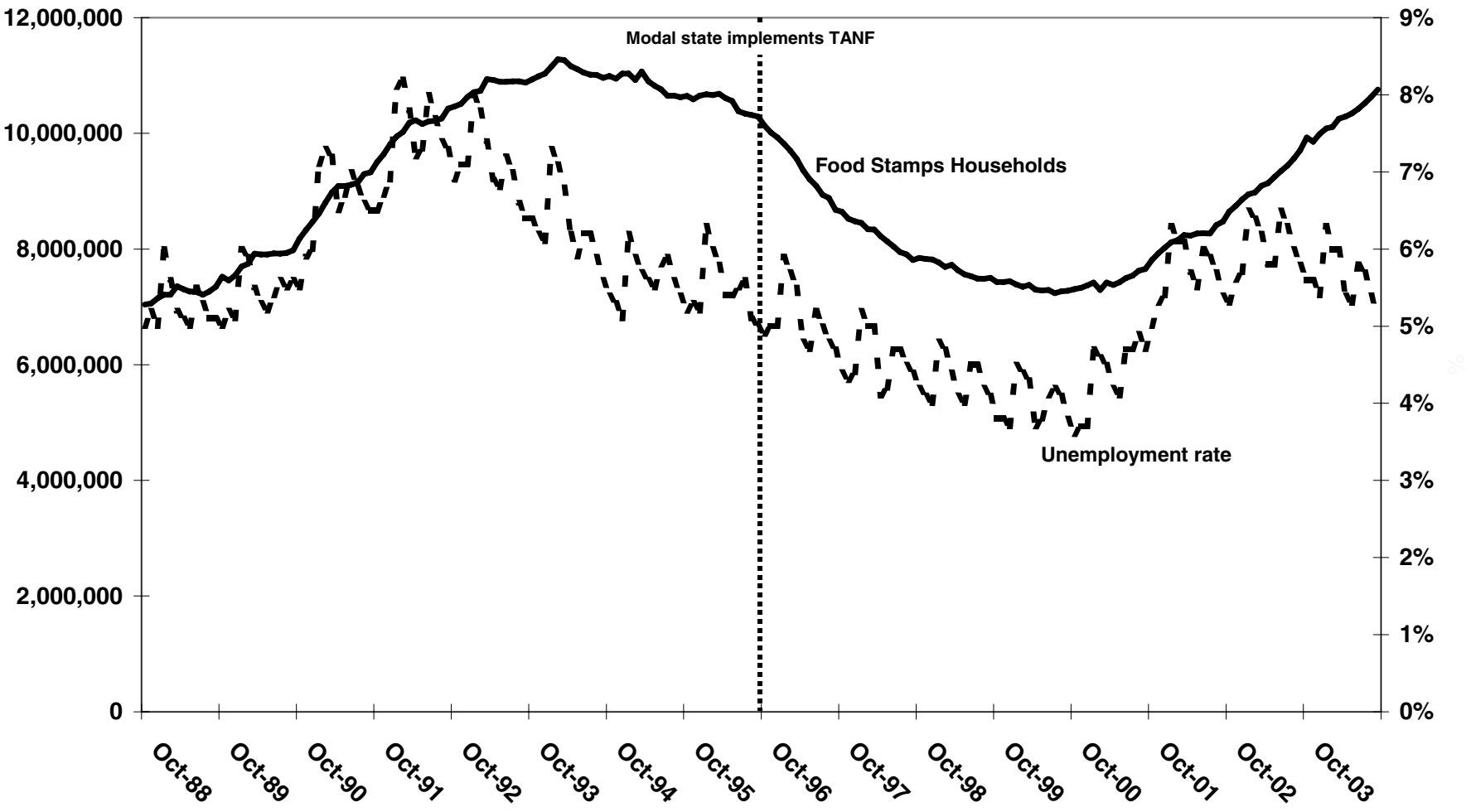
Source: USDA, Food and Nutrition Service, FNS-388; Bureau of Labor Statistics, Labor Force Statistics.

total per capita employment, total per worker wages, and the minimum wage. We include both contemporaneous and lagged measures of these variables. See Appendix A for details.

There were also major expansions in the federal EITC during this period. Those changes substantially increased incentives to find employment among low-income families in the 1990s. Previous research finds large effects of the EITC on employment and the welfare caseload (Grogger, 2004; Meyer and Rosenbaum, 2001). Unfortunately (as noted earlier), our methods do not allow us to estimate the effect of nationwide policy changes. There were some state EITCs during this period. They are much smaller and we do not consider them here.

IV.   ESTIMATION STRATEGY

Consistent with the aggregate nature of our data and like the previous research, we take a reduced-form approach; that is, we analyze the effect of the FSP and welfare reforms themselves and of the economy on the total caseload and on the components of interest. We do not model at the individual level, entry and exit, or eligibility and take-up conditional on eligibility.

Our approach begins with the conventional difference-of-differences specification:

$$R_{st} = \alpha + X\beta + \delta_s + \delta_s * y + \gamma + \tau + \varepsilon_{st}$$

where R is the natural log of per capita persons receiving food stamps (total, PA, and NPA), X represents the forcing variables (e.g., FSP policies, welfare policies, and the economy) and β the corresponding regression coefficients, δ is a vector of state dummies, y is a linear fiscal year trend (interacted with the vector of state indicators), γ a month dummy (for July), and τ a national year spline, with (discontinuous) knot points at the start of each federal fiscal year (in place of the more standard national time indicators). This approach is roughly equivalent to annual time dummies with annual data (i.e., it includes the same number of parameters). The subscript t indexes time measured in semi-annual intervals and the subscript s denotes states.

Our approach differs from the previous literature in the manner in which we specify the forcing

variables (i.e., the policies and the economy), *X*. The conventional approach in such aggregate models is

to model a policy as either not in place or in place. Implicitly, the effect of the policy is assumed to be a

step function: no effect yesterday; full effect today.

For several reasons, this conventional specification seems highly implausible:

1. Many policies require a bureaucracy for full implementation. Putting that bureaucracy into place is rarely instantaneous. For example, to enable ABAWDs to fulfill their work requirements, a program to provide jobs must be put in place.

2. In general, the target population's behavioral response to policy changes depends on the spread of knowledge about the policy and confidence that it will be applied. For example, for the stigma of program participation to be reduced, potentially eligible populations must come to know about the replacement of paper coupons with the ATM-like EBT card.

3. Klerman and Haider (2004) and Haider and Klerman (2005) show that if a policy affects the flows onto and off of a program, the effect on the caseload stock will be a distributed lag, with the lag lasting several years.

Given this critique of the conventional approach, we follow Danielson and Klerman (2004) in

specifying a flexible functional form for the effect of the policies on the FSP caseload. Our specification

includes three parts. First, we allow for the conventional one-time effect at implementation. Second, we

allow for a linear effect over the first two years. Finally, we allow for a different linear effect in the third

and following years. Both on a priori grounds and given past experience with this specification (see

Danielson and Klerman, 2004), we expect no immediate effect, an intermediate effect (i.e., the first and

second years) that varies with the particular program, and no additional effect in the long term (in the

third and following years).

For the case of the economy, the previous literature has sometimes included lagged values, and

those lagged values often substantially increase the estimated total effect of the economy.[5] In the FSP

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[5]See CEA (1999); Figlio, Gundersen, and Ziliak (2000); Klerman and Haider (2004); Ziliak et al. (2000). The previous literature has sometimes also included lagged values of the dependent variable (Ziliak et al., 2000; Ziliak, Gundersen, and Figlio, 2003).

literature, Schoeni (2001) presents evidence that the typical parameterization of economic variables likely underestimates the true proportion of the decline resulting from the economy. To capture lagged effects of the economy on caseload stocks, we include the moving average of the past year's value of each measure, a one-year lag of this value, and the interaction of the two.

Finally, we carefully consider the structure of the residual process. The residuals appear to be autocorrelated, even after the inclusion of year fixed effects. We estimate the regression parameters by Feasible Generalized Least Squares (FGLS) with an AR(2) autocorrelation structure in the semi-annual data (using Stata's Generalized Estimating Equation routine, xtgee).

Our unit of analysis is the state-period (two periods per year). State populations vary widely. Some conventional econometric analyses therefore suggest weighting by (the inverse of) state population. Dickens (1990) shows that this is appropriate only if there are few important unmeasured variables at the level of the unit of analysis. On a priori grounds, such unmeasured variables seem likely and Dickens' analyses suggest that weighting in these circumstances usually induces more error into the estimated standard errors (and in the parameters). Therefore, following Dickens, we do not weight the data. Instead, we equally weight each state-period observation. Nevertheless, a superpopulation perspective suggests that inasmuch as populations vary across states (which they clearly do), the residual variance will be smaller in larger states. To correct the standard errors for such heterogeneity—and any other heterogeneity—our preferred results apply a Huber-Eicker-White approach to correcting the standard errors for arbitrary heteroscedasticity.

V.   RESULTS

We present results of our preferred models in the first column of each panel on Table 2. All results include the AR(2) correction. Columns 2 and 3 report conventional and robust standard errors, respectively. Below, we report significance using the robust standard errors in column 2. However, consistent with Dickens (1990), the patterns of significance change very little when we remove the correction for arbitrary heteroscedasticity.

Table 2
Results from Preferred Models

	Log of Per Capita FSP Participants			Log of Per Capita FSP-Only (NPA) Participants			Log of Per Capita FSP + Cash Assistance (PA) Participants		
	(1)	(1a)	(1b)	(2)	(2a)	(2b)	(3)	(3a)	(3b)
Food Stamp Policies									
Overpayment errors: current (x100)	0.0075	(0.0013)*	(0.00099)*	0.027	(0.012)*	(0.0098)*	0.0022	(0.0028)	(0.0031)
Overpayment errors: 1 year lag	0.0034	(0.0017)*	(0.00099)*	-0.0063	(0.0093)	(0.0099)	0.011	(0.0065)	(0.0031)*
Underpayment errors: current (x100)	-0.0021	(0.0040)	(0.0020)	-0.0059	(0.019)	(0.021)	-0.012	(0.0082)	(0.0063)*
Underpayment errors: 1 year lag	-0.0017	(0.0027)	(0.0019)	-0.0057	(0.028)	(0.019)	-0.012	(0.0071)	(0.0058)*
ABAWD index	0.0023	(0.0028)	(0.0030)	0.0036	(0.0089)	(0.027)	-0.0099	(0.015)	(0.0082)
EBT: implementation	0.019	(0.0067)*	(0.0079)*	0.088	(0.062)	(0.087)	0.041	(0.027)	(0.026)
EBT: slope	0.0066	(0.0055)	(0.0034)	0.068	(0.080)	(0.033)*	0.0099	(0.013)	(0.012)
EBT: spline after 2 years	-0.0026	(0.0049)	(0.0035)	-0.0059	(0.020)	(0.034)	-0.036	(0.019)	(0.013)*
Transitional benefits: imp.	-0.026	(0.015)	(0.016)	-0.0074	(0.11)	(0.16)	0.017	(0.035)	(0.049)
Transitional benefits: slope	0.016	(0.010)	(0.0071)*	0.011	(0.040)	(0.071)	0.013	(0.021)	(0.024)
Transitional benefits: spline after 2 yrs	0.023	(0.010)*	(0.042)	-0.066	(0.060)	(0.40)	0.041	(0.017)*	(0.12)
Simplified reporting: imp.	-0.0036	(0.0096)	(0.0095)	-0.30	(0.24)	(0.10)*	0.012	(0.040)	(0.030)
Simplified reporting: slope	0.014	(0.0056)*	(0.0047)*	0.10	(0.091)	(0.045)*	0.0090	(0.031)	(0.016)
Simplified reporting: spline after 2 yrs	0.0027	(0.0087)	(0.010)	-0.16	(0.12)	(0.11)	0.041	(0.049)	(0.036)
Welfare Reform Bundles									
AFDC waivers: implementation	-0.0098	(0.0079)	(0.010)	0.0029	(0.019)	(0.11)	-0.027	(0.021)	(0.032)
AFDC waivers: slope	-0.0011	(0.0032)	(0.0034)	-0.012	(0.013)	(0.037)	0.011	(0.0053)*	(0.011)
AFDC waivers: spline after 2 years	0.0065	(0.0066)	(0.0076)	0.025	(0.025)	(0.082)	-0.033	(0.014)*	(0.024)
TANF: implementation	-0.034	(0.014)*	(0.015)*	-0.046	(0.050)	(0.14)	0.050	(0.073)	(0.040)
TANF: slope	-0.029	(0.0078)*	(0.0059)*	-0.033	(0.051)	(0.049)	-0.11	(0.019)*	(0.016)*
TANF: 2 year spline	0.020	(0.0063)*	(0.0060)*	-0.11	(0.071)	(0.053)*	0.095	(0.017)*	(0.017)*
Maximum AFDC/TANF benefit (log)	-0.085	(0.063)	(0.049)	0.25	(0.45)	(0.47)	0.11	(0.23)	(0.17)
Economy									
Unemployment rate (x100): current	0.057	(0.010)*	(0.0062)*	0.14	(0.047)*	(0.063)*	0.038	(0.022)	(0.022)
Unemployment rate: 1 yr lag	0.053	(0.010)*	(0.0066)*	0.17	(0.073)*	(0.068)*	0.031	(0.023)	(0.024)
Unemployment rate: interaction	-0.0057	(0.0012)*	(0.00086)*	-0.020	(0.0088)*	(0.0089)*	-0.0036	(0.0029)	(0.0032)
Tot. per capita empl (x100): current	-0.013	(0.01065)	(0.0037)*	-0.037	(0.030)	(0.037)	-0.022	(0.015)	(0.013)
Tot. per capita empl: 1 year lag	-0.0035	(0.0073)	(0.0035)	-0.0068	(0.024)	(0.034)	-0.022	(0.011)*	(0.012)
Tot. per capita empl: interaction	0.000016	(0.000081)	(0.000043)	0.000057	(0.00023)	(0.00040)	0.00020	(0.00012)	(0.00016)
Per worker wages: current	-0.0011	(0.00025)*	(0.00024)*	-0.0025	(0.0015)	(0.0021)	-0.0023	(0.00084)*	(0.00078)*
Per worker wages: 1 year lag	-0.00063	(0.00027)*	(0.00025)*	-0.0011	(0.00097)	(0.0022)	-0.0032	(0.00081)*	(0.00080)*
Per worker wages: interaction	0.00000026	(0.00000013)	(0.00000013)	0.00000076	(0.00000077)	(0.0000011)	0.0000015	(0.00000046)*	(0.00000042)*
Minimum wage	-0.16	(0.074)*	(0.068)*	-0.32	(0.28)	(0.67)	-0.19	(0.13)	(0.23)

Observations: 1,632

Note: All models incorporate state fixed effects, a national fiscal year spline, state-specific linear year trends, and month indicators and include an AR(2) correction. The first column of each panel reports parameter estimates, while the second and third columns report robust and conventional standard errors, respectively.
* Significant at 5%.

We report changes in the slope coefficients for the policy variables on a yearly basis. Because a one-unit change since implementation is equal to 6 months in our data, the yearly change is twice the slope parameter estimates. Splines are summed with the slope coefficients and multiplied by 2 in order to arrive at the annual estimated change due to a policy 2.5 years after implementation and later.

Overall, the pattern of results is as follows. In our model for the total FSP caseload, we find effects of both TANF and later FSP policy changes, and we find strong effects of the economy. In our models for the disaggregated FSP caseload, we do not generally find effects of specific FSP policy changes on the PA and NPA caseloads, although we find effects of the economy. These results are surprising.

We now discuss the results in detail. We return to the question of why the disaggregated results are so different from the aggregate results in the final section of the paper.

Over- and Underpayment Errors

Consistent with an interpretation of the direction of payment errors as a proxy for state attitudes, we find that overpayment errors result in a larger caseload. A permanent one-percentage-point increase in a state's overpayment rate is associated with a 1.1 percent increase in the total FSP caseload. We also find evidence that overpayments increase the NPA caseload, with the point estimates implying that a permanent one-percentage-point increase in overpayments results in a 2.1 percent increase in the NPA caseload. Overpayment errors do not have a significant effect on PA caseloads. Underpayment errors do not have a significant effect on the total or disaggregated caseloads, although the sign on all the parameter estimates for underpayments is, as expected, negative. We can reject the hypothesis of equality in the impact of over- and underpayment errors. In the case of the PA caseload, the point estimates for underpayments are quite large, although imprecisely estimated—a permanent one-percentage-point increase in underpayment errors results in a 2.4 percent reduction in the PA caseload. This pattern of results suggests that the forces driving overpayment errors are, as we suspected, different from forces

driving underpayment errors: Overpayments are often associated with an increased caseload, while underpayments have no significant effect.

ABAWD Policies

We find no effect of ABAWD policies. This is not surprising; we would expect the effect to be difficult to detect because ABAWDS make up a small fraction of the FSP. It is also the case that our proxy for the severity of the policies is imperfect (it is not time-varying within the period of ABAWD implementation).

EBT, Transitional Benefits, and Simplified Reporting

Consistent with our a priori expectations that EBT lowers stigma, the point estimates imply a 1.9 percent increase in the total FSP caseload because of the implementation of EBT but, surprisingly, no significant effect thereafter. We also find a large, positive, but not significant effect of EBT on the NPA caseload (an 8.8 increase at implementation and a 13.6 percent increase each year the new policy was in effect) and an anomalous negative, but insignificant, effect (5.2 percent yearly decline) on the PA caseload in the third year and later after implementation. Although the effect of the introduction of EBT on the disaggregated caseloads is not significant, the larger effect on the NPA caseload is consistent with the notion that the stigma of benefit receipt is more salient for households receiving only food stamps.

We find no initial implementation effect of transitional benefits in the total caseload and a 7.8 percent increase yearly starting in the third year after implementation. We also see an anomalous (because transitional benefits are extended to those leaving TANF cash assistance) 10.8 percent yearly increase due to transitional benefits in the PA caseload starting in the third year after implementation. And although transitional benefits are provided to those leaving cash assistance, we see an 11.0 percent decline that is, however, insignificant in the NPA caseload.

We see a significant increase of 2.8 percent in the total FSP caseload in the first two years after the implementation of simplified reporting (the combined point estimates do not reach conventional levels

of significance in the third year and after implementation). Point estimates for the effect of simplified reporting are far from significant in the disaggregated caseloads, although the slope parameter estimates generally have the expected (positive) sign, and, also as expected, the effect is much larger in the NPA than the PA caseload.

AFDC Waivers and TANF

We would expect the primary effect of welfare policy changes to be on the PA caseload. To the extent that welfare policy changes (in particular, changes that restrict eligibility) simply move people from the PA caseload to the NPA caseload, we would expect to find an effect on the PA caseload, an offsetting effect on the NPA caseload, and no effect on the total caseload. Conversely, changes that broaden eligibility (e.g., changes to the benefit structure to encourage combining work and welfare) would be expected to increase the PA caseload and reduce the NPA caseload, while leaving the total caseload approximately unchanged. However, since our main specification does not measure welfare policy changes individually and the implementation of TANF also marked the start of policies that limited FSP eligibility for both PA and NPA caseloads, we interpret the estimates of TANF implementation to be a combination of welfare reform and FSP policy changes.

Consistent with the explanation that AFDC waivers broadened eligibility, we find a positive 2.2 percent yearly effect of waivers on the caseload combining cash assistance and food stamps in the first two years after implementation, no significant effect thereafter, and no significant effect on the NPA or total caseloads. Consistent with the former explanation (narrowed eligibility through program rule changes), we estimate a sharp 22.0 percent yearly decline from TANF in the first two years after implementation in the PA caseload, no significant decline thereafter, and no significant decline in the NPA caseload. As expected, we see a significant instantaneous effect of the implementation of TANF in neither the PA nor the NPA caseload. However, in the total FSP caseload there appears to have been an overall discouragement effect: a 3.4 percent instantaneous reduction attributable to states' implementation of their TANF programs and an additional 5.8 percent yearly decline for the first two years after

implementation that stops in the third year after implementation. This result is consistent with the findings of other econometric studies of the effect of TANF on the FSP caseload (Currie and Grogger, 2001; Gleason et al., 2001; Wallace and Blank, 1999; Wilde et al., 2000).

This pattern of significance across the total and disaggregated caseloads is consistent with the interpretation that the implementation of TANF reduced cash assistance receipt and that any increase in the NPA caseload was offset by decreases due to narrowed eligibility for the FSP. In our earlier analyses of the welfare caseload (Danielson and Klerman, 2004), we found the expected effects of individual welfare policies (benefit structure, welfare-to-work sanctions, time limits on benefit receipt). We estimated models including the same proxies for individual welfare policies on the total and disaggregated FSP caseloads, but found no consistent pattern of effects. We return to this surprising finding in the conclusion. Finally, we find no significant effect of the maximum AFDC/TANF benefit on the total or the disaggregated FSP caseloads.

Economy

Turning to the impact of the economy on the total caseload, we find, as expected, a positive relationship between the FSP caseload and the unemployment rate and a negative relationship between the FSP caseload and both total employment and per worker wages. However, among the three measures of the economy, the parameter estimates are individually significant in the case of the unemployment rate and per worker wages, but not for total per capita employment. We include total per capita employment in our preferred specification in order to remain consistent with the specification in Danielson and Klerman (2004). For all three measures of the economy, each set of parameters is jointly significantly different from zero.

In the case of the NPA caseload, the set of parameters for the unemployment rate are individually and jointly significant, but total employment and wages are not; for the PA caseload, the set of parameters measuring real per worker wages are significant individually and jointly, and the other measures, apart

from the lag of total per capita employment, are not. Possibly the noise in the disaggregated caseloads is obscuring the separate effects of the individual measures, which are themselves correlated.

These three economic proxies have different scales, so their magnitudes are hard to compare. To aid in interpretation, in Table 3 we simulate the impact of the equivalent of a one-percentage-point improvement in the unemployment rate (i.e., that per worker wages improved by $81 and that per capita employment increased by .02) for all measures of the economy.[6] The table shows the cumulative effect at one and two years after a common one standard deviation year-to-year improvement. Thus normalized, total per capita employment and real per worker wages have about the same or greater impact on the total and disaggregated caseloads as the unemployment rate, the conventional measure of the economy used in the literature (i.e., all three appear to be important economic variables).

We conjecture that a model including only the unemployment rate would show the NPA caseload to be more sensitive to the economy than the PA caseload—that the transition to a better job or more hours of work is easier than entering the labor market at all or that, alternatively, some of those on welfare are never expected to work (e.g., the disabled). The results in Table 4 are consistent with that conjecture. A permanent one-percentage-point improvement in the unemployment rate implies a 7.0 percent drop in total FSP participation, a 15.7 percent drop in the NPA caseload, and a 5.2 percent decline in the PA caseload, once the improvement has phased in completely (after two years).

An increase in the minimum wage implies a decrease in the FSP caseload; the implied elasticity is 0.16 and is significant, but not large.

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[6]We set the measures of the economy on the same scale by first calculating the standard deviation of the residuals from a regression of each measure on state and month dummies. We then simulate how an improvement equivalent to a one-percentage-point reduction in the unemployment rate phases in for each measure, starting at the population-weighted mean of the measure for the period.

Table 3
Cumulative Effects of Improvements in the Economy

Measure of the Economy	Change One Year after Standardized Improvement	Change Two Years after Standardized Improvement
Unemployment rate		
FSP persons	-2.5%	-5.2%
PA persons	-1.8	-3.2
NPA persons	-2.9	-10.8
Total per capita employment		
FSP persons	-2.9	-3.5
PA persons	-3.4	-6.7
NPA persons	-8.2	-9.2
Total real per worker wages		
FSP persons	-9.1	-11.7
PA persons	1.1	-7.1
NPA persons	-17.0	-14.0

Note: PA=receiving food stamps and cash assistance; NPA=receiving food stamps but not cash assistance

Table 4
Alternate Model

	Log of Per Capita FSP Participants			Log of Per Capita FSP-Only (NPA) Participants			Log of Per Capita FSP + Cash Assistance (PA) Participants		
	(1)	(1a)	(1b)	(2)	(2a)	(2b)	(3)	(3a)	(3b)
Food Stamp Policies									
Overpayment errors: current (x100)	0.0075	(0.0014)*	(0.00099)*	0.028	(0.012)*	(0.0098)*	0.0028	(0.0030)	(0.0031)
Overpayment errors: 1 year lag	0.0035	(0.0018)	(0.00099)*	-0.0058	(0.0094)	(0.0099)	0.012	(0.0065)	(0.0031)*
Underpayment errors: current (x100)	-0.0029	(0.0037)	(0.0020)	-0.0060	(0.019)	(0.021)	-0.012	(0.0079)	(0.0064)
Underpayment errors: 1 year lag	-0.0020	(0.0026)	(0.0019)	-0.0056	(0.028)	(0.019)	-0.011	(0.0071)	(0.0058)*
ABAWD index	0.0028	(0.0029)	(0.0030)	0.0043	(0.0092)	(0.027)	-0.0084	(0.015)	(0.0082)
EBT: implementation	0.019	(0.0066)*	(0.0078)*	0.091	(0.065)	(0.086)	0.047	(0.028)	(0.026)
EBT: slope	0.0054	(0.0060)	(0.0035)	0.065	(0.079)	(0.033)*	0.0072	(0.013)	(0.012)
EBT: spline after 2 years	-0.0014	(0.0047)	(0.0036)	-0.0031	(0.021)	(0.034)	-0.031	(0.018)	(0.013)*
Transitional benefits: imp.	-0.022	(0.014)	(0.016)	-0.0017	(0.10)	(0.16)	0.012	(0.035)	(0.049)
Transitional benefits: slope	0.021	(0.0084)*	(0.0072)*	0.016	(0.038)	(0.070)	0.017	(0.022)	(0.024)
Transitional benefits: spline after 2 years	0.014	(0.0083)	(0.043)	-0.084	(0.068)	(0.40)	0.066	(0.016)*	(0.12)
Simplified reporting: imp.	0.00094	(0.0094)	(0.0095)	-0.29	(0.23)	(0.10)*	0.011	(0.039)	(0.030)
Simplified reporting: slope	0.016	(0.0058)*	(0.0047)*	0.11	(0.088)	(0.045)*	0.021	(0.030)	(0.016)
Simplified reporting: spline after 2 years	0.0026	(0.0091)	(0.010)	-0.16	(0.11)	(0.11)	0.051	(0.058)	(0.036)
Welfare Policies									
AFDC waivers: implementation	-0.0091	(0.0079)	(0.010)	0.0014	(0.020)	(0.11)	-0.030	(0.021)	(0.032)
AFDC waivers: slope	-0.0011	(0.0034)	(0.0034)	-0.012	(0.013)	(0.037)	0.012	(0.0057)*	(0.011)
AFDC waivers: spline after 2 years	0.0065	(0.0074)	(0.0076)	0.024	(0.025)	(0.082)	-0.036	(0.014)*	(0.025)
TANF: implementation	-0.032	(0.014)*	(0.015)*	-0.040	(0.051)	(0.14)	0.043	(0.073)	(0.040)
TANF: slope	-0.037	(0.0083)*	(0.0059)*	-0.048	(0.049)	(0.046)	-0.12	(0.018)*	(0.015)*
TANF: spline after 2 years	0.021	(0.0066)*	(0.0061)*	-0.10	(0.072)	(0.053)	0.097	(0.018)*	(0.017)*
Maximum AFDC/TANF benefit (log)	-0.13	(0.078)	(0.049)*	0.17	(0.48)	(0.46)	0.0068	(0.23)	(0.17)
Economy									
Unemployment rate: current (x100)	0.073	(0.0087)*	(0.0058)*	0.18	(0.053)*	(0.058)*	0.051	(0.019)*	(0.020)*
Unemployment rate: 1 yr lag	0.071	(0.0096)*	(0.0061)*	0.21	(0.070)*	(0.061)*	0.054	(0.020)*	(0.022)*
Unemployment rate: interaction	-0.0073	(0.0013)*	(0.00085)*	-0.023	(0.0090)*	(0.0087)*	-0.0052	(0.0026)	(0.0031)
Minimum wage (log)	-0.14	(0.065)*	(0.067)*	-0.26	(0.29)	(0.66)	-0.060	(0.14)	(0.22)

Observations	1,632

Note: All models incorporate state fixed effects, a national fiscal year spline, state-specific linear year trends, and month indicators and include an AR(2) correction. The first column of each panel reports parameter estimates, while the second and third columns report robust and conventional standard errors, respectively.

* Significant at 5%.

VI.   SIMULATIONS

We motivated this study as an attempt to understand the path of the FSP caseload. It is, however, difficult to understand the implications of the raw regression coefficients for the overall path of the caseload. In this section, we therefore use the estimated regression coefficients to provide simulations of the caseload under actual and counterfactual paths for policy and the economy. Specifically, we run two policy simulations in which we trace the path of the caseload if states had not enacted the FSP and welfare policy changes that they did, and two economy simulations in which we hold the measures of the economy fixed at their worst (February 1992) and best (May 2000) levels over the period and observe how the caseload would have evolved if the economy had, first, never improved over the 1990s and, second, not worsened in the early 2000s, given our parameter estimates.[7] In all the simulations, we allow the policies (or the measures of the economy) not under examination and the state and time fixed effects to evolve as they actually did.[8] Thus, the difference between the observed and the simulated paths gives the effect of the policy/economic proxy held fixed in the simulation. Given the lack of significance of the policy results in our models using disaggregated caseload data, we restrict policy simulations to the total caseload, but report results of holding measures of the economy fixed for both the total and disaggregated caseloads.

Tables 5 and 6 report the results of policy and economy simulations, respectively. The left-hand panel of both tables compares the actual caseload decline from the highest to the lowest point of the total FSP caseload over the period (-39.0 percent from January 1994 to July 2000) to the simulated declines. Likewise, the right-hand panel of both tables compares the actual caseload increase from the lowest point

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[7]Note that we hold real per worker wages and total per capita employment, not wages and total employment, steady.

[8]We use the smearing estimate developed in Duan (1983) to retransform the predictions in the log scale to the level scale. We further adjust each simulation by the difference between the actual and our predictions of the actual caseload in each month.

Table 5
Simulations: Food Stamp and Welfare Policies

Caseload	Model	Caseload Decline Jan 94-Jul 00			Caseload Increase Jul 00-Jul 04		
			AFDC Waivers and TANF Not Implemented			EBT, Transitional Benefits, and Simplified Reporting Not Implemented	
		Actual	Simulated Decline	Percentage Points of Decline Explained by Simulation	Actual	Simulated Increase	Percentage Points of Increase Explained by Simulation
Total FSP persons	Preferred specification	-39.0%	-26.5%	12.5	+44.5%	+30.8%	13.7
Total FSP persons	Policy dummies only	-39.0%	-36.4%	2.6	+44.5%	+42.6%	1.9

Table 6
Simulations: Economy

| Caseload | Model | Caseload Decline Jan 94-Jul 00 | | | Caseload Change Jul 00-Jul 04 | | |
| | | | Economy Held at apex of the Unemployment Rate (Feb 92) | | | Economy Held at nadir of the Unemployment Rate (May 00) | |
Caseload	Model	Actual	Simulated Decline	Percentage Points of Decline Explained by Simulation	Actual	Simulated Change	Percentage Points of Change Explained by Simulation
Total FSP persons	Preferred specification	-39.0%	-13.4%	25.6	+44.5%	+33.8%	10.7
PA persons		-52.3%	-40.5%	11.8	-2.7%	-14.3%	11.6
NPA persons		-24.4%	+45.8%	70.2	+77.3%	+35.6%	41.7
Total FSP persons	Measures of economy: current only	-39.0%	-18.2%	20.8	+44.5%	+39.6%	4.9
PA persons		-52.1%	-48.6%	3.5	-2.8%	-8.0%	5.2
NPA persons		-24.2%	30.7%	54.9	+77.4%	66.2%	11.2
Total FSP persons	Measures of economy: unemployment rate only	-39.0%	-27.6%	11.4	+44.5%	+25.9%	18.6
PA persons		-52.3%	-46.3%	6.0	-2.7%	-12.7%	10.0
NPA persons		-24.4%	+10.2%	34.6	+77.3%	+24.1%	53.2
Total FSP persons	Measures of economy: current unemployment rate only	-39.0%	-31.7%	7.3	+44.5%	+34.7%	9.8
PA persons		-52.3%	-48.6%	3.7	-2.7%	-8.0%	5.3
NPA persons		-24.4%	-2.6%	21.8	+77.3%	49.5%	27.8

of the total caseload to the end of the data (44.5 percent from July 2000 to July 2004) to the simulated

increases. The top portion of each table reports the simulation results using our preferred model.

Succeeding sections of each table report results using simpler specifications of the policy and economic

variables, respectively.

The policy simulations indicate that if no state had implemented a waiver to its AFDC program

and no state had implemented TANF, the total FSP caseload decline from January 1994 to July 2000

would have been 12.5 percentage points less than the actual 39.0 percent decline. In other words, it would

have declined only two-thirds as much as it actually did: Welfare reform had a large negative impact on

the total FSP caseload. If, on the other hand, no state had implemented policies aimed at reducing the

stigma and burden of benefit receipt, the caseload would have increased 13.7 percentage points less than

the actual 44.5 percent increase from July 2000 through the end of our data in July 2004: These policies

improved access to the FSP.

Our regression models use a rich specification for the timing of a policy effect. The lower panel

of Table 5 compares simulation results obtained using our preferred estimates to ones derived from a

simpler specification—dummy variables that turn on when the policy was implemented—to measure the

effect of policy changes. Using this simpler specification reduces our estimates of their effect: Not

implementing TANF reduces the caseload decline in the 1990s by only 2.6 percentage points in this

specification, rather than the 12.5 percentage-point decline we find using our preferred specification.

Similarly, not implementing EBT, simplified reporting, and transitional benefits dampens the increase by

only 1.9 percentage points, rather than the 13.7 dampening obtained using our preferred specification.

In the case of the economy, the simulations indicate that if the economy had never improved over

the course of the 1990s, the total FSP caseload would have declined only 13.4 percent instead of the

actual 39.0 percent decline from January 1994 to July 2000—a 25.6 percentage-point difference. In the

case of the PA caseload, the simulated caseload decline, while still sizable, is smaller: It would have

declined 11.8 percentage points less than the actual 52.3 percent decline. Finally, the simulations indicate

that the NPA caseload would have actually increased by 45.8 percent, not decreased by 24.4 percent, if the economy had not improved. Had the economy not deteriorated in the early 2000s, our simulations indicate that the total FSP caseload would have increased by 33.8 percent, not the actual 44.5 percent, that the PA caseload would have declined by 14.3 percent, not the actual 2.8 percent, and that the NPA caseload would have increased by 35.6 percent, not the actual 77.4 percent.

Turning to simulations that use less rich specifications of the economy, we see that using only the unemployment rate to measure the economy, or only current per worker wages, total per capita employment, and the unemployment rate, or only the current unemployment rate, generally reduces our estimate of the impact of the economy on the caseload. For example, if we include only current measures of the economy, the simulation on the total FSP caseload shows a decline of 18.2 percent, explaining 20.8 percentage points of the actual 39.0 percent decline rather than the 25.6 percentage points that our preferred model explains. If we include only the unemployment rate (current, lagged, and interacted), the simulated decline is 27.6 percent, explaining 11.4 percentage points of the actual decline. Finally, if we include only the current unemployment rate, the simulated decline is 31.7 percent in the total caseload, explaining only 7.3 percentage points of the actual decline. The same pattern holds true in the PA and NPA caseloads.

In sum, simulations using our preferred specification indicate that the improving economy combined with the implementation of TANF explains the entire FSP caseload decline in the mid-1990s, while the weakening economy and policies aimed at increasing access to the FSP explain half of the increase in the 2000s.

VII.   CONCLUSIONS

We find that welfare reform as a bundle, specific FSP policies, and the economy all played a role in the path of the FSP caseload in the 1990s and early 2000s. While the economy is twice as important as TANF implementation in explaining the drop in the total caseload in the 1990s, FSP policy changes aimed at increasing access to the program among eligible families are somewhat more important than the

economy in explaining the rise in the early 2000s. In addition, the economy played a large role in the path of the PA and NPA caseloads over the period.

We do not find clear-cut policy impacts in the PA and NPA caseloads. The promising evidence of reaction to FSP policies that we have found in the total caseload, coupled with evidence from discussions with state officials indicating that the definition of PA and NPA changed over time within some states as they were developing their TANF programs, leads us to surmise that measurement error is obscuring policy effects in the PA and NPA caseloads. This suggests that an effort to refine the measures of PA and NPA using a complementary data source will yield better results in the disaggregated caseloads. Such an effort is important because our theoretical framework strongly implies that policy effects should be different in these two segments of the caseload. Work that we have just begun promises to press ahead with these issues by analyzing individual-level FSPQC data.

Appendix A
Data

FOOD STAMP CASELOADS

We obtained states' reports of their total, PA, and NPA caseloads filed semi-annually as part of

the FNS-388A report from FNS for January 1989–July 2004. In cases where the FNS-388, which reports

monthly total FSP persons, households, and issuance, and FNS-388A differed, we adjusted PA and NPA

caseloads to sum to the FNS-388 total.

FOOD STAMP POLICIES

Table A.1 summarizes our coding of food stamp policy choices. Below, we describe the sources

of this coding.

Electronic Benefits Transfer. FNS reports the timing of statewide implementation of EBT

programs at http://www.fns.usda.gov/fsp/ebt/ebt_status_report.htm.

Simplified Reporting. While FNS publishes a yearly report documenting which states have

adopted the policy choices available to them (see USDA, 2004), there is no central source for information

about the implementation dates of simplified reporting. We have pieced together the implementation dates

used in this report from several sources: Rosenbaum (2003) and a 2004 survey by the National

Association for Program Information and Performance Measurement available on their Web site

(www.napipm.org). We verified this information and filled in gaps by examining states' on-line FSP

policy manuals and making calls to state officials.

Transitional Benefits. There is, likewise, no central source for implementation dates of

transitional benefits. We have pieced together the implementation dates used in this report from

Rosenbaum (2003) and by examining states' on-line FSP policy manuals and making calls to state

officials.

Table A.1
Detailed Food Stamp Policy Coding

State	Electronic Benefits Transfer Implemented Statewide	Simplified Reporting Implemented	Transitional Benefits Implemented
Alabama	01-nov-1997	01-aug-2004	
Alaska	01-jun-1998	01-jan-2004	
Arizona	01-aug-1999	01-jan-2004	01-jan-2003
Arkansas	01-apr-1998	01-nov-2003	
California	01-jun-2004	01-jul-2004	01-jan-2004
Colorado	01-feb-1998	01-nov-2003	01-dec-2002
Connecticut	01-oct-1997	01-jan-2003	
Delaware	01-sep-2003	01-mar-2002	
District of Columbia	01-oct-1998	01-may-2001	
Florida	01-oct-1998	01-aug-2003	
Georgia	01-nov-1998	01-oct-2002	
Hawaii	01-aug-1998		
Idaho	01-feb-1998	01-nov-2003	
Illinois	01-nov-1997	01-nov-2003	01-jan-2005
Indiana	01-mar-2002	01-jun-2003	
Iowa	01-oct-2003	01-jan-2004	
Kansas	01-mar-1997	01-oct-2002	
Kentucky	01-nov-1999	01-nov-2003	
Louisiana	01-dec-1997	01-sep-2000	
Maine	01-jun-2003	01-jun-2003	
Maryland	01-apr-1993	01-oct-2001	01-jan-2003
Massachusetts	01-oct-1997	01-oct-2002	01-dec-2002
Michigan	01-jul-2001	01-aug-2001	
Minnesota	01-oct-1998		
Mississippi	01-oct-2002	01-may-2005	
Missouri	01-may-1998	01-sep-2004	
Montana	01-jul-2002	01-apr-2002	
Nebraska	01-sep-2002	06-jun-2003	06-jun-2003
Nevada	01-jul-2002	01-oct-2004	
New Hampshire	01-jan-1999	01-jul-2005	01-jul-2005
New Jersey	01-jun-1999	02-jul-2001	
New Mexico	01-aug-1995	01-jan-2004	01-sep-2003
New York	01-feb-2001	01-oct-2002	01-dec-2001
North Carolina	01-jun-1999	01-jun-2003	01-jun-2003
North Dakota	01-mar-1997		
Ohio	01-oct-1999	01-nov-2003	
Oklahoma	01-jan-1998	01-mar-2002	
Oregon	01-may-1998	18-aug-2003	15-jun-2003
Pennsylvania	01-sep-1998	01-jun-2003	01-jun-2002
Rhode Island	01-oct-1998	01-dec-2004	
South Carolina	01-dec-1995	01-aug-2002	
South Dakota	01-mar-1997		
Tennessee	01-aug-1999	01-sep-2001	01-oct-2004
Texas	01-nov-1995	01-apr-2003	
Utah	01-apr-1996		
Vermont	01-oct-1998	01-nov-2002	
Virginia	01-jul-2002	01-jul-2003	01-jul-2003
Washington	01-nov-1999	01-oct-2002	
West Virginia	01-jun-2003	01-may-2001	
Wisconsin	01-oct-2000	01-jul-2003	01-mar-2004
Wyoming	01-jan-2000		

Over- and Underpayments. We use official, annual over- and underpayment error rates reported by the Office of Analysis, Nutrition, and Evaluation (OANE), which are calculated using the FSPQC data (USDA, 2005a).

ABAWD Policy. Czajka et al. (2001) measure six policies: (1) whether a state uses ABAWD waivers, and whether the proportion of the caseload included in the waiver is above or below the national median; (2) whether a state uses the 15 percent exemption and whether they exempt less than, or greater than, 10 percent; (3) whether the requirement for disability certification can be based on self-report, caseworker observation, or more stringent criteria are used; (4) how many adults in a household with a dependent child are exempted from the time limit and whether the maximum age for a dependent child is 17 or under; (5) whether the time limit starts when a recipient applies, or when the case is certified, or when the recipient receives his first full month of benefits; and (6) whether the state tracks the time limit using a rolling clock (continuous look back to the previous 36 months) for all or part of the caseload.

ECONOMY

Following previous research indicating that richer measures of the economy increase the share of observed change from the economy, we use three measures of the economy: (1) monthly state-level unemployment rates (multiplied by 100) from the Bureau of Labor Statistics' series A; (2) monthly state-level total covered employment as recorded by the Quarterly Census of Employment and Wages (QCEW) Program that the BLS conducts in cooperation with the states; and (3) quarterly state-level total wages, as recorded by the QCEW. We divide total employment by the total estimated population in a state and month and multiply them by 100 to obtain our measure of total per capita employment. We divide quarterly wages by total employment in a quarter to obtain our measure of per worker wages.

Following Klerman and Haider (2004), we model the effect of the economy using current and lagged values, as well as interactions of these values. The current and lagged measures are moving averages of the previous year's (i.e., of the twelve monthly) values and moving averages of the values one year previous.

MAXIMUM BENEFIT AND MINIMUM WAGE

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

OTHER CONTROLS

The specification includes a national year spline with knot points that we allow to be discontinuous in October of each year (the start of the federal fiscal year), 50 state fixed effects, 1 month indicator (for July), and state-specific linear fiscal year trends.

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