Determinants of the Food Stamp Program Caseload

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Abstract

The Food Stamp Program (FSP) is intended to help low-income households afford a nutritionally adequate diet. Welfare (cash assistance) and FSP policies have changed significantly since the 1990s. This report examines 1990-2004 data to consider how the policy changes and the changing economic climate have affected the FSP caseload over time. Results show that the FSP caseload shifted sharply from nearly half receiving cash benefits from Temporary Assistance for Needy Families (TANF) to less than a fifth. The share of the FSP caseload not receiving cash benefits from either TANF or Supplemental Security Income (nonpublic assistance, or NPA) increased sharply. The NPA caseload rose when the economy was weak and was sensitive to reporting requirements for the FSP (for example, how often participants must be recertified as eligible). The decline in the share of the FSP caseload that receives TANF is not well-explained by the changes in the economy or program policies. The Food Stamp Program was renamed the Supplemental Nutrition Assistance Program (SNAP) in October 2008.

This study was conducted by RAND under a cooperative research contract with USDA's Economic Research Service (ERS) Food and Nutrition Assistance Research Program (FANRP): contract number 43-3AEM-5-80090 (ERS project representative: Michele Ver Ploeg). The views expressed are those of the authors and not necessarily those of ERS or USDA.

PREFACE

This document constitutes the final report of the USDA-RAND Cooperative Agreement 43-3AEM-5-80090 “Determinants of the Food Stamp Caseload”. This work was conducted with the RAND Center for the Study of Social Welfare Policy within RAND’s Labor and Population Program.

This document should be of interest to those trying to understand the effect of the Food Stamp Program policy, welfare policy, and the economy on the Food Stamp Program caseload, and more broadly on the causes of the wide swings in the caseload—overall and its components.

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SUMMARY

The Food Stamp Program (FSP) is intended to help low-income households afford a nutritionally-adequate diet[1]. Since the 1990s, the FSP caseload has varied widely—rising sharply in the early 1990s, dropping sharply in the last 1990s, and then rising again throughout the 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 impact of these three factors separately on three mutually exclusive and exhaustive groups: (i) FSP households receiving cash benefit from TANF (formerly AFDC), but not SSI; (ii) FSP households receiving cash benefits from SSI, and perhaps TANF; (iii) FSP households receiving cash benefits from neither AFDC/TANF, nor SSI-—whom we refer to as NPA/Non-Public Assistance. Over our period, 1990 to 2004, the FSP caseload shifted sharply from nearly half AFDC/TANF to less than a fifth. The share of the FSP caseload receiving SSI increased some. The share of NPA FSP caseload increased sharply.

We find that the explanatory power of our models is concentrated almost exclusively in the NPA part of the caseload. In that increasingly important subset of the caseload, the economy and FSP policy have the expected effects and the effects are large. Specifically, the NPA caseload moves counter-cyclically (i.e., it rises when the economy is weak) and the NPA caseload is quite sensitive to the burden of participating in the FSP—as proxied by short certification periods and Simplified Reporting. We find no evidence for effects of EBT adoption or outreach expenditures. These qualitative results are robust to multiple variations in the specification. Specifically, estimating the models excluding Hispanics—as a rough proxy for the effects of immigration and PRWORA's immigration related provisions—does not substantially shift the results. Simulations imply that our models explain much of the

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[1] On October 1, 2008, the Food Stamp Program changed its name to the Supplementary Nutrition Assistance Program (SNAP). The new name reflects the program's mission to not only provide food assistance, but also to increase nutrition to improve the health and well being of low-income people. However, since the report uses data from prior to the name change, the term "Food Stamp Program" is used throughout the report.

movement in the NPA caseload, but do not explain well the movement in the other two caseload components.

ACKNOWLEDGMENTS

This work was conducted under the USDA-RAND Cooperative Agreement 43-3AEM-5-80090 “Determinants of the Food Stamp Caseload”. Margaret Andrews and Shelly Ver Ploeg of USDA-ERS (the project officers for this project), and Dr. David Smallwood (Deputy Director of Deputy Director for Food Assistance Research in the Food and Rural Economics at the Economic Research Service of USDA) have been unusually supportive and helpful.

At USDA, Bob Dalrymple arranged access to the FSPQC data and Jenny Genser provided insights into State policies. Other staff at USDA-FNS providing guidance included John Bedwell, Nadine Nichols, and Dan Woodhead.

Staff at Mathematica Policy Research were uniformly helpful. They supplied early years of FSPQC data that were not available on the web site. They supplied additional variables that were not included in the public distribution file. Finally, they cheerfully and thoroughly answered questions about the data. This study would not have been possible without their assistance.

We have benefited from our interaction with a team doing similar analyses at the Urban Institute as part of the USDA-Urban Institute study “How are State Food Stamp and TANF Policy Choices Affecting Food Stamp Participation.” They include Caroline Ratcliffe, Signe-Mary McKernan, and Kenneth Finegold.

This work is a direct continuation of earlier analyses supported by a USDA Small Grants administered by the Institute for Research on Poverty, University of Wisconsin, Madison to Caroline Danielson at the University of California. That earlier support is gratefully acknowledged.

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ACRONYMS

Symbol	Definition
AFDC	Aid to Families with Dependent Children
ASEC	(Current Population Survey) Annual Social and Economic Supplement
CPS	Current Population Survey
EBT	Electronic Benefits Transfer
EITC	Earned Income Tax Credit
ERS	Economic Research Service
FGLS	Feasible Generalized Least Squares
FFY	Federal Fiscal Year *yyyy* (October 1, *yyyy*-September 1, *yyyy*)
FNS	Food and Nutrition Service
FSP	Food Stamp Program
FSPQC	Food Stamp Program Quality Control
GA	General Assistance
GLS	Generalized Least Squares
GNLLS	Generalized Non-Linear Least Squares
GMM	Generalized Method of Moments
NLGLS	Non-Linear Generalized Least Squares
NLLS	Non-Linear Least Squares
NPA	Non-Public Assistance (Food Stamps)
PA	Public Assistance (Food Stamps)
PRWORA	Personal Responsibility and Work Opportunities Reconciliation Act of 1996
ORA	USDA, Office of Research and Analysis
QC	Quality Control
SIPP	Survey of Income and Program Participation

SSI	Supplemental Security Income
TANF	Temporary Assistance to Needy Families
TFP	Thrifty Food Plan
USDA	United States Department of Agriculture
WNLS	Weighted Non-Linear Least Squares
WLS	Weighted Least Squares

I. INTRODUCTION

The last decade and a half have been a period of major change for the Food Stamp Program (FSP). After sharp increases in the early-1990s, the overall FSP caseload fell by about a third in the late-1990s and then rose by a third in the early-2000s. Furthermore, these wide swings in the overall caseload mask strikingly different patterns in the components of the caseload. According to this classification of the caseload, the decline in the caseload occurred almost exclusively among households receiving food stamp benefits along with “welfare” (AFDC/Aid to Families with Dependent Children” and then TANF/Temporary Assistance to Needy Families). The increase in the caseload occurred almost exclusively among households receiving food stamp benefits, but not currently receiving welfare (see Figure 1.1). It seems likely that many of them had recently received welfare and continued to receive Food Stamps even after leaving welfare (for a job or due to time limits).

What might have caused these changes in the overall FSP caseload and in its components? Theory and previous research leads us to look to both the economy and policies, and, indeed, the path of the total FSP caseload closely follows the path of the aggregate economy (e.g., the unemployment rate). Furthermore, the sharp decline in the welfare component of the FSP caseload is coincident with major welfare reform (e.g., PRWORA/Personal Responsibility and Work Reconciliation Act of 1996). Finally, the sharp increase in the non-welfare component of the FSP caseload follows a series of reforms to the FSP intended to lower the burden of participation and expanded eligibility.

OUR APPROACH

Each of these explanations of caseload change is plausible. It is also possible that other, unmeasured, factors played a role. The relative importance of these measured factors and their ability to explain the path of the aggregate FSP caseload and its components is less clear. Evaluating the role of these measured factors and estimating the likely effect of extending or repealing welfare or FSP reforms requires an econometric model. This paper reports the results of our efforts to estimate such an econometric model.

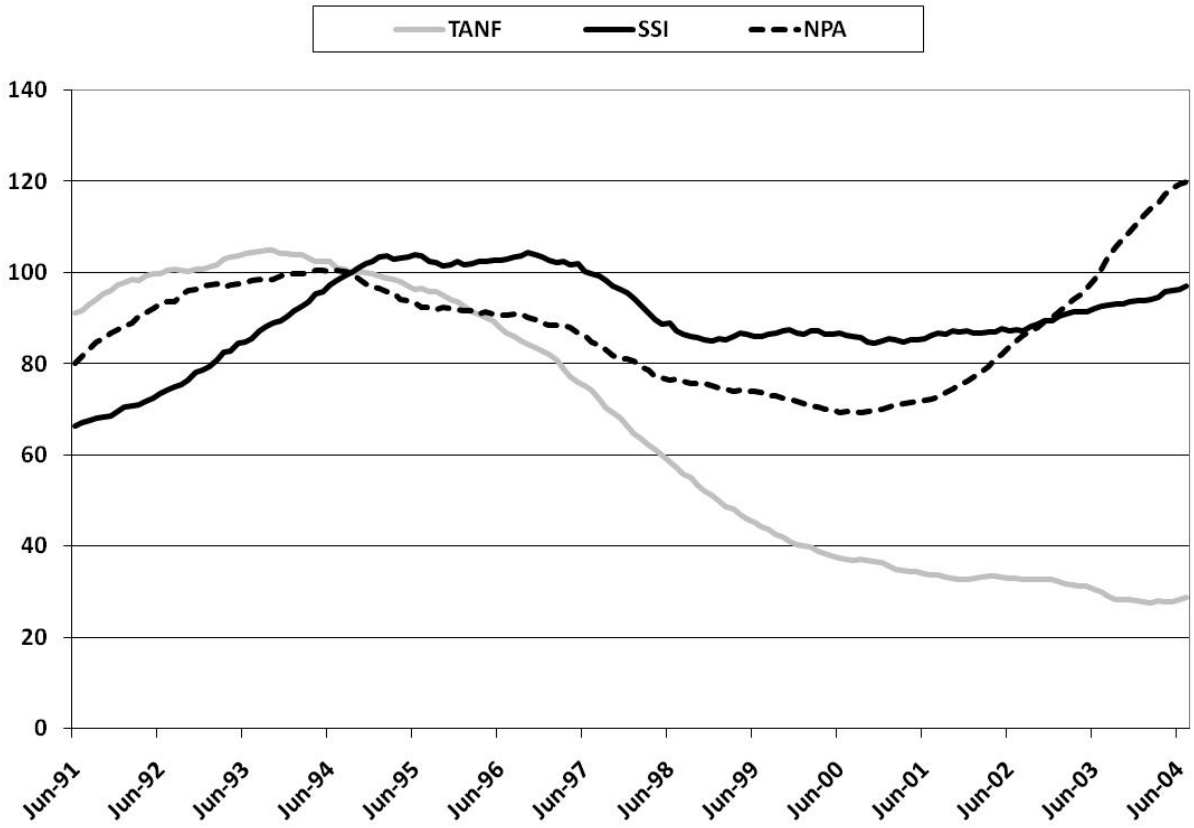

Figure 1.1-Normalized FSP Coverage Rate, by Cash Assistance Receipt Sub-Group (FFY1994=100)

Source: Tabulations from RAND FSPQC Analysis File

Such an econometric model begins with data describing the FSP caseload itself—the total number of cases and their distribution across cash assistance-receipt sub-groups. Specifically, we estimate separate models for those receiving welfare (AFDC/TANF, but not SSI), SSI (possibly including TANF), and everyone else—who we refer to as the NPA/Non-Public Assistance caseload. We compile those data from individual-level data collected as part of the Food Stamp Program Quality Control (FSPQC) system.

An econometric model also requires careful coding of the economic and policy forcing variables and how they vary across States and through time. We code FSP policy based on USDA summaries, Rosenbaum (2003), and consultation with individual States about their policies. Similarly, we code welfare policy based on our earlier review of published summaries and consultation with individual States (Danielson and Klerman, 2008). With respect to the economy, we consider not only the current unemployment rate, but also total

employment and wages per worker. Throughout, we carefully explore the dynamics of the effect of these forcing variables on the FSP caseload. A naive theoretical approach might posit that only current values of the forcing variables affect the current caseload. A richer theoretical approach and our earlier empirical work suggests that a more complex specification of the forcing variables including not only current values of the forcing values, but also carefully-specified lagged values will significantly improve the fit of the model and the plausibility of the implied policy responses.

Finally, an econometric model needs to carefully control for spurious effects due to unmeasured factors affecting the FSP caseload. We do so in several ways. First, we analyze, not national data, but State-level data. Doing so allows us to apply difference-of-differences (DoD) methods. In practice, such methods involve including fixed effects (i.e., dummy variables) for each State and time period (e.g., year). Including such dummy variables controls for time invariant variation across the States and national period effects. Even after these controls, there is considerable auto-correlation in the residuals; i.e., if the model over-predicts in this period, it is extremely likely to over-predict in the next period. Failure to control for such auto-correlation leads to overestimation of the precision of the estimates. Our approach controls for such autocorrelation and general forms of heteroscedasticity.

We find that the explanatory power of our models is concentrated almost exclusively in the NPA part of the caseload. In that increasingly important subset of the caseload, the economy and FSP policy have the expected effects and the effects are large. Specifically, the NPA caseload moves counter-cyclically (i.e., it rises when the economy is weak) and the NPA caseload is quite sensitive to the burden of participating in the FSP—as proxied by short certification periods and Simplified Reporting. We find no evidence for effects of EBT adoption or outreach expenditures. These qualitative results are robust to multiple variations in the specification. Specifically, estimating the models excluding Hispanics—as a rough proxy for the effects of immigration and PRWORA's immigration related provisions—does not substantially shift the results. Simulations imply that our models explain much of the movement in the NPA caseload, but only poorly explain the movement in the other two caseload components.

PLAN OF PAPER

The balance of this document proceeds in six chapters. The next chapter provides an overview of the FSP, its place in the broader American "safety net" for the nation's poor, and our data on FSP caseloads. The third chapter describes our "forcing variables"—FSP policy, welfare policy, and the economy. The fourth chapter describes our econometric methods. The fifth chapter presents the results, for our three sub-groups and for a wide range of alternative specifications including dropping Hispanics as a rough test for the effects of immigration and welfare reform's immigration related changes to the FSP. The final chapter summarizes our findings, discusses their implications for policy, and outlines directions for future research. Four appendices discuss in more detail the FSPQC data and the "forcing variables", and present additional regression results.

NOTES ON LANGUAGE

We conclude this introduction with some notes on language. We use the terms "participants" and "recipients" interchangeably to refer to an individual who is certified for and receives food stamps. Note that there can be non-participants/non-recipients in a household that receives food stamps (e.g., undocumented immigrants, ineligible noncitizens, adults in sanction for failure to participate in welfare or FSP welfare-to-work activities). In general, food needs of such non-participants are not considered when computing the food stamp benefit.

Also, we use the term "coverage rate" to refer to participants *per member of the population*, whether or not they are (appear to be) eligible for the FSP. We reserve the term "participation rate" for the ratio of participants to (apparently) eligible members of the population (sometimes referred to as a "take-up rate"). We note that this usage differs from the usage in some USDA documents (see, for example, Kornfeld, 2002, p. 4-4; Kabbani and Wilde, 2003).

II. THE FOOD STAMP PROGRAM

This chapter describes our dependent variables, the FSP caseload and related outcomes. The chapter begins with an overview of the FSP itself—its history and goals and its place in the nation's social safety net. It then discusses our FSPQC data and how we use those data to create rates. Finally, it presents recent trends in the—aggregate and disaggregated—caseload. We deliberately defer our presentation of the specific policy options allowed under federal law, State choices from those options, and the literature on the effect of those State choices until the next chapter's discussion of "forcing variables".

PURPOSE AND HISTORY

The FSP is one of the key programs in the federal safety net for low-income families and the largest of the domestic food and nutrition assistance programs administered by USDA/FNS. By statute, the program's purpose is "to permit low-income households to obtain a more nutritious diet by increasing their purchasing power" (Food Stamp Act of 1977, as amended, P.L. 95-113). The program "serves as the first line of defense against hunger", enabling "low-income families to buy nutritious food" (the language is from http://www.fns.usda.gov/fsp/).

The program advances these goals by providing cash equivalents for the purchase of food to an average of 22 million individuals per month during the period covered by our data. Since the period covered by our data, the Food Stamp Program caseload has continued to increase sharply. For a family of four in FFY 2008, the maximum FSP benefit was $542 per month. Originally, the program provided food "stamps"—i.e., coupons; thus, the program's name. Through the 1990s and into the 2000s, States gradually converted from paper "stamps" to Electronic Benefit Transfers (EBT)—an ATM like card (see discussion in the next chapter and the table of State-by-State adoption dates in Appendix B). Today, essentially all benefits are provided through EBT.

The roots of the FSP date back to a series of pilot programs begun in 1961 and then the Food Stamp Act of 1964 leading to a program in 22 States.[2] The amendments of 1971 (PL 91-671) made the program national with uniform eligibility standards and benefits. The current funding structure of the program dates to the Food Stamp Act of 1977.[3] Specifically, the federal government pays all benefits and splits the cost of administration with the States.

Most FSP features are common across the States. However, as we discuss in detail in the next chapter, this is less true today than it was in the mid-1990s. Alongside devolution of authority in designing other programs (e.g., welfare/AFDC/TANF; see Table 2.1 above), since the mid-1990s, there has been increasing State discretion in the operation of the FSP.

ELIGIBILITY

FSP eligibility and benefit computation rules are quite complex.[4] We discuss the aspects that have varied over time and across States in more detail in the next chapter.

In brief, most families are eligible for benefits if they have gross incomes less than 130 percent of the monthly poverty line ($2,238 for a family of four in FFY 2008), net countable income less than 100 percent of the poverty line for households with no elderly or disabled members ($1,721 for a family of four in FFY 2008), and less than $2,000 in countable assets ($3,000 for households with elderly or disabled members). The treatment of automobiles relative to this asset standard varies across time and States.

Households receiving benefits or services from welfare (AFDC/TANF), cash income from Supplemental Security Income (SSI), or in some places cash income from General Assistance (GA) are categorically eligible (i.e., automatically, irrespective of other program eligibility requirements, and without filling

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[2] On the earlier history of the FSP, see Hoynes and Schanzenbach (2007).

[3] See http://www.fns.usda.gov/fsp/rules/Legislation/history.htm for a more detailed history of the program.

[4] This discussion draws heavily on the overview in Poikolainen (2005). Cunnyngham (2005) also explores this issue, including a careful analysis of changes in national FSP policy over time.

out another application), although any cash income from these sources is counted against their FSP benefit[5].

In addition to these income and asset rules, families must meet certain non-financial eligibility standards to participate in the FSP. Only citizens and legal residents are eligible to participate in the FSP. Also, since the passage of PRWORA in 1996 some recent immigrants have been barred from participation, although several laws passed subsequently narrowed the grounds for exclusion.[6] Also since 1996, certain nondisabled, nonelderly, adults living in households without children are subject to work participation requirements or time limits on benefits.

After they are initially certified to receive benefits, FSP participants must file regular reports to verify that they are still eligible for the program. In addition, they must be recertified periodically (usually, but not always, in person). As we discuss in the next chapter, there is considerable variation through time and across States in the details of these requirements. Estimating the effect of those reporting rules on participation and the recent increase in the caseload is a major goal of this study.

BENEFIT COMPUTATION

The maximum benefit to which a household is entitled is based on the cost of the Thrifty Food Plan (TFP). The TFP is based on an "economical and nutritious diet", adjusted for household size and composition and is $542 per month for a family of four in FFY 2008.

A household's benefit is computed on the basis of net monthly income, which is equal to gross income less a standard deduction, and where applicable, deductions for earned income (20 percent of earnings), dependent

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[5] Prior to 2001, only households receiving cash AFDC/TANF, SSI, or GA benefits were categorically eligible. However, regulations published in November 2000 required States to confer categorical eligibility on families receiving benefits or services at least 50 percent funded by TANF, and gave them the option of conferring categorical eligibility on families receiving benefits or services less than 50 percent funded by TANF.

[6] For a summary of the current exclusions, see http://www.fns.usda.gov/fsp/Rules/Memo/02/POLIMGRT.HTM. We note that the largest immigrant-receiving states typically put in place replacement food assistance programs for legal immigrants made ineligible for the federal program (Zimmerman and Tumlin, 1999). However, such participants are not included in the federal FSPQC data that we analyze.

care expenses, medical expenses for elderly or disabled participants, child support payments, and excess shelter costs exceeding 50 percent of income). A household's monthly benefit is computed by subtracting 30 percent of the net income from the maximum benefit. This formula implies that benefits are reduced by 30 cents for every additional dollar of net unearned income and slightly less for earned income.

FSPQC DATA

This document reports analyses of data generated as part of the FSP Quality Control (FSPQC) process. Because the federal government pays all benefits, States lack strong financial incentives to properly review applications. Therefore, to ensure payment accuracy, federal statute requires States to participate in the carefully designed and implemented Food Stamp Program Quality Control review program (hereafter, FSPQC[7]).

Under the FSPQC, State auditors draw a stratified random sample of all FSP cases[8], where the strata are States or sub-State regions. The exact number of cases varies with the State's total caseload and other factors, but is usually between 25 and 200 per month. We refer to the records generated as part of that audit as the Food Stamp Program Quality Control data, or simply as the FSPQC data. In recent years, the resulting national file includes about 50,000 records. The unit of audit is a household. The total number of individuals in an annual file is in excess of 100,000. Mathematica Policy Research (MPR), under contract to the USDA, prepares a version of these files for public release.

Our analysis file begins with the files distributed by MPR. We augment them with a variable on reporting period available in FFY2003 and FFY2004 that

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[7] For more detail on FSPQC, see GAO (2001) and Rosenbaum (2000). The latter article cites Section 16(c) of the Food Stamp Act (7 U.S.C § 2025(c)), which provides the statutory basis for the QC system. The food stamp regulations at 7 C.F.R. §§ 273.2(d)(2), 275.1 - 275.4, 275.10 - 275.14, and 275.21 and the Food Stamp Program Quality Control Review Handbook (FNS Handbook 310) provide much more detailed information on QC procedures. For a discussion of the strengths and weaknesses of the food stamp QC system and related penalties and incentives see Affholter and Kramer (1987).

[8] Certain households are excluded from the FSPQC sample, notably those receiving disaster benefits. Also, ineligible households were dropped from the edited QC data file starting in FFY 2003.

MPR also supplied to us directly, but is not on the standard public use distribution file. We consider only the 50 States and the District of Columbia. We thus drop Guam and the U.S. Virgin Islands. We also drop all sampled households and individuals who are ex post deemed ineligible for the FSP program.

Federal regulations precisely specify the contents of the FSPQC data and its collection methods (USDA, 2003), provide the detailed instructions for the reviewers, and reproduce the (then current) data collection form (reproduced in Appendix A of this document). The intention is that the FSPQC auditor will collect sufficient information to verify that the benefit was properly computed. As such, the FSPQC data include detailed demographic, labor market, assets, and other FSP eligibility information. The exact information collected has varied over time. Appendix A provides a review of year-to-year changes.

Individual-level public use FSPQC files are available back to 1980. They include most of the information from the FSPQC review. Information that would reveal the identity of individuals is suppressed. The file also includes the calendar month to which the review referred and weights to control for the sampling probabilities and to align the microdata with the administrative totals for participant households in each State. Poikolainen and Ewell (2005) provide documentation for the 2004 data file.[9] Other reports in this series provide documentation for the data files for other years. For FFY 1996 through the FFY 2004, the files are on the MPR web site (http://host4.MPR-mpr.com/fns/fnsqcdata/download.htm). For earlier years, MPR provided files directly to RAND.[10]

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[9] It should be noted that the actual file does not align exactly with this documentation. Specifically, the file contains many variables not described in the Code Book. In particular, the file appears to include the date the case was first opened (as used in analysis of FSP program dynamics), but that variable is not described in the Code Book.

[10] Apparently no reviews were conducted in the District of Columbia in March, July, August, and September of 1995. Our analysis file fills in the missing information by linear interpolation from adjacent months in which reviews were conducted.

The data also have some true zeros. In some States, in some months, there simply were no reviewed cases in some of our three sub-groups. In those cases, we set the counts to zero. This, however, is not appropriate for the conditional rates (e.g., the error rate). These missing data are especially problematic since we specify those variables as twelve month trailing

While the primary objective of the QC system is to assess the accuracy of eligibility determinations and benefit calculations, researchers also use it as a source of detailed information on FSP participant characteristics. From these data, MPR produces an annual report on the characteristics of FSP participants (e.g., Wolkwitz and Leftin, 2008) and a report on FSP participation rates (e.g., Wolkwitz, 2008). In addition, these data have been used for other microsimulation efforts (Rosso and Fowler, 2000; Brinkley and Ewell, 2000) and for econometric analyses (e.g., Gleason, et al, 2001; Kabbani and Wilde, 2003; Kornfeld, 2002).

FSP COVERAGE RATES

The FSPQC data provide information on the number of people participating in the FSP. However, our concept of interest is the coverage rate—which we define as participants per member of the population. Note that this "FSP Coverage Rate" differs from the "FSP Participation Rate" computed annually by MPR (e.g., Wolkwitz, 2008). In this report, we define the Participation Rate as the ratio of participants to *eligible* individuals. Our analysis considers the FSP coverage rate, not the "FSP Participation Rate". We defer to Chapter 4 and our motivation of our methods, our discussion of the FSP Participation Rate, issues with computing it, and why we do not use it.

Computing coverage rates raises several issues. First, we need to define participant groups of interest. Here and in the econometric analyses that follow, we consider three groups defined by participation in cash benefit programs: (i) FSP households receiving cash benefit from TANF (formerly AFDC), but not SSI; (ii) FSP households receiving cash benefits from SSI, and perhaps TANF; (iii) FSP households receiving cash benefits from neither AFDC/TANF, nor SSI.

This grouping corresponds to our understanding of likely policy effects on the FSP caseload. As we discuss in the next chapter, much of the late-1990s caseload decline appears to be closely related to the corresponding sharp shrinkage in the welfare caseload. To understand the importance of

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averages. Thus, one missing month forces us to drop twelve months of data. To address this problem, we interpolate such information between months in which there were observed cases and then take the average over months in which we actually compute an outcome and months for which we interpolate an outcome.

changes in the welfare program for the FSP caseload, we disaggregate by cash assistance receipt status.[11]

Second, to compute a rate, we need to assign an appropriate denominator. Consistent with our interest in "coverage rates", not "participation rates"; we define the denominators based on simple population counts; i.e., independent of cash assistance receipt status or income level within age-gender specific groups.

Third, we need a source for these population statistics. We extract the population statistics from the "Bridged Population Estimates" of the National Center for Health Statistics (NCHS) (DHHS, 2003; Ingram et al., 2003; see also see http://www.cdc.gov/nchs/data/series/sr_02/sr02_135.pdf). We use these NCHS estimates rather than the more conventional Census estimates because they provide a data series which is consistent across the 1990s and early 2000s. The 2000 Census enumerated many more people than would have been expected based on population projections from the 1990 Census. These increases were particularly notable for Hispanics. The official Census series cross-over from a 1990 Census base to a 2000 Census base in 2001. The cross-over leads to a discontinuity in population totals. As much as possible, we want a consistent series; and, ideally, one based on the more recent 2000 Census. The NCHS population estimates address this issue.

BROAD CASELOAD TRENDS

Having described the FSP, the source of the data on cases, and the source of the data on the overall population, we turn to caseload trends. The size of the total FSP caseload has varied widely over the last fifteen years (see Figure 2.1). The number of FSP participants rose sharply in the early 1990s, peaking in 1994 at 28 million. The caseload dropped sharply thereafter to

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[11] We note that States report biannual Public Assistance (PA) and Non-Public Assistance (NPA) cases and persons to the USDA. These data are useful, but not ideal, for pursuing the analysis of this report because they do not disaggregate by type of public assistance received, and because non-public assistance households *and* persons are classified as such if even one household member is receiving food stamps in the absence of public assistance (see Danielson and Klerman, 2006).

under 17 million in July 2000. Since then, the FSP caseload has risen sharply to over 29 million in August 2008.[12]

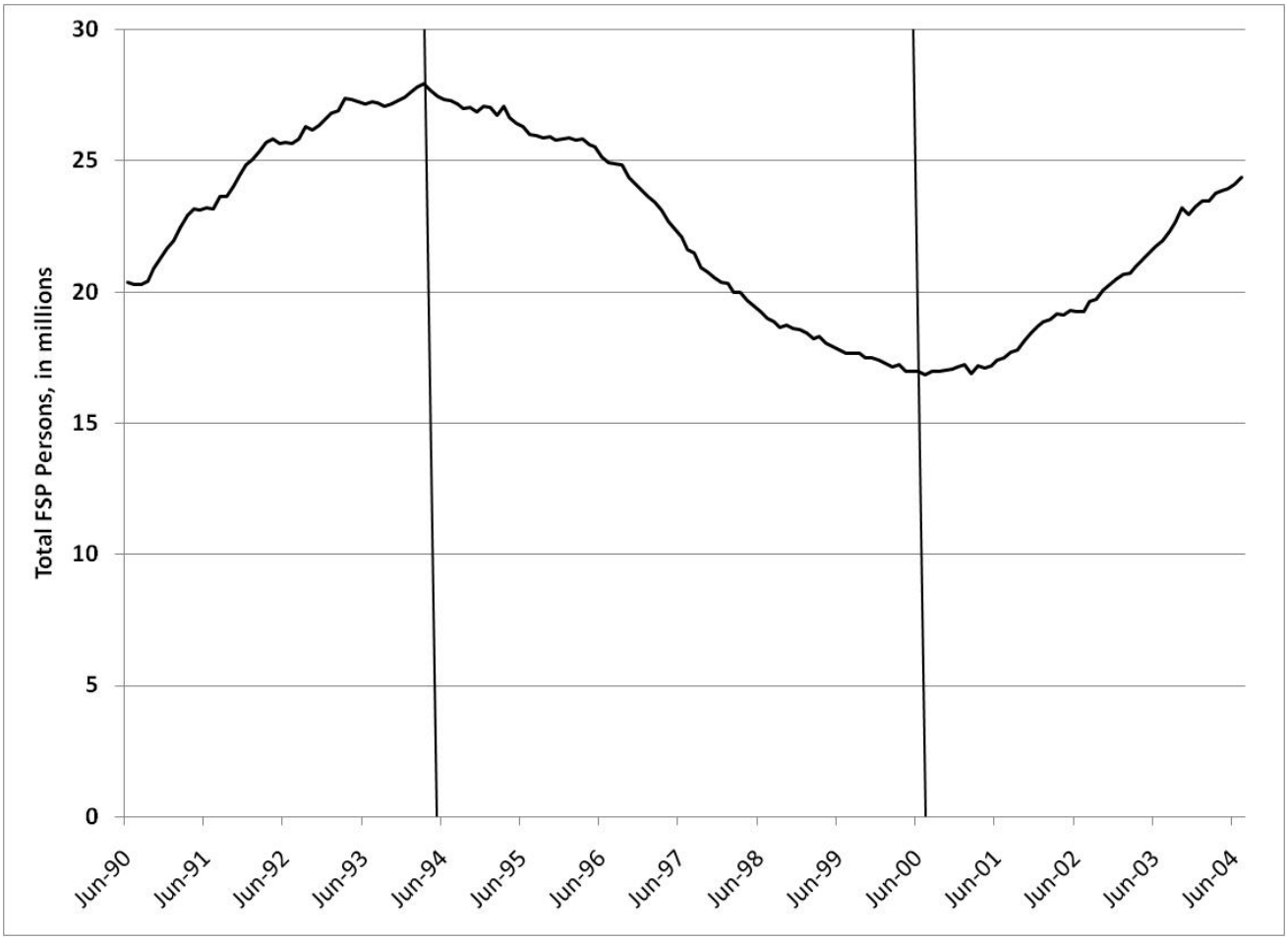

Figure 2.1—National FSP Caseload

Source: FNS-388 administrative counts.

This variation in overall case count over our study period hides widely diverging coverage rate trends. It is these diverging trends that motivate the disaggregated analysis below. Figure 2.2 plots those trends; specifically for our three groups it plots coverage rates relative to the FSP caseload in FFY1994 (approximate the peak of the total FSP caseload).

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[12] FSP participation reached an all-time high in November 2005, reaching nearly 30 million. However, an estimated 4 million received disaster benefits in the wakes of Hurricanes Katrina, Rita, and Wilma.

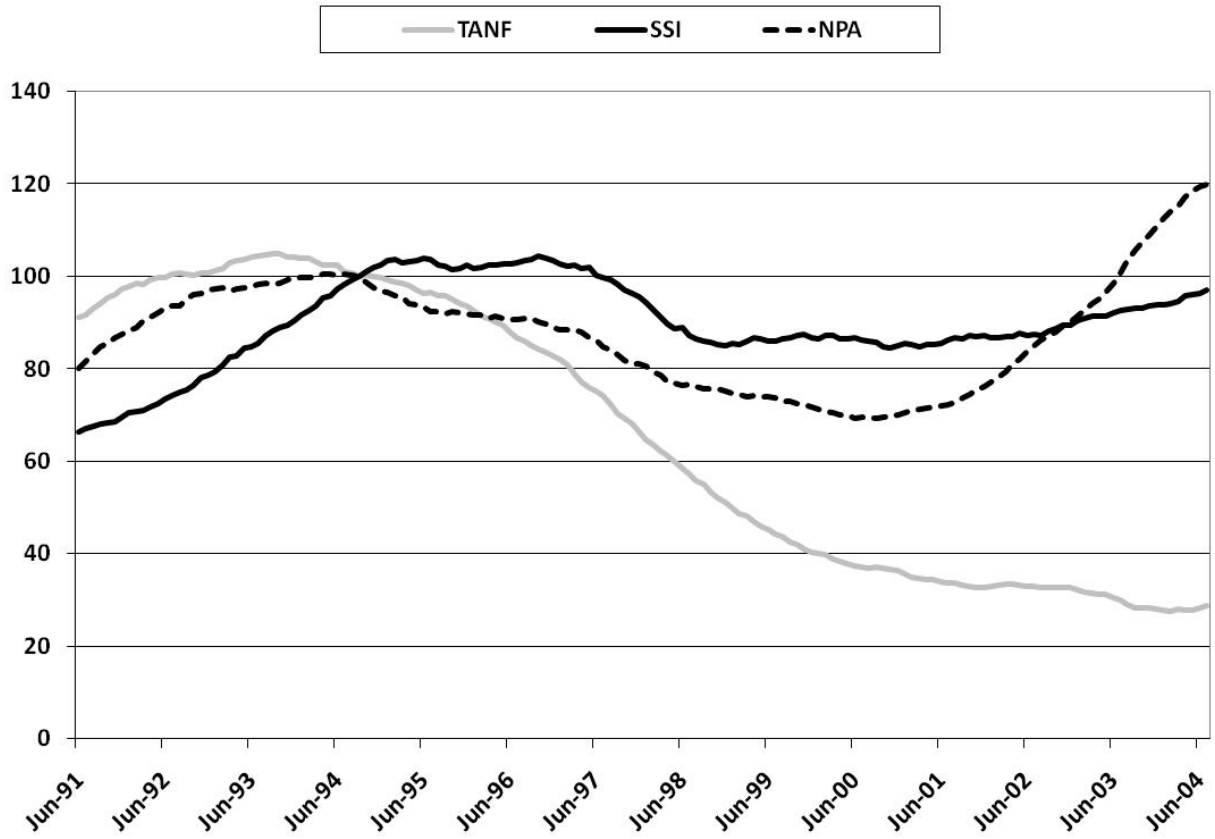

Figure 2.2—Normalized FSP Coverage Rate, by Cash Assistance Receipt Sub-Group (FFY1994=100)

Note: Rates are 12-month trailing averages of the computed coverage rate defined in the text, then divided by the FFY 1994 rate and multiplied by 100.
Source: Tabulations from RAND FSPQC Analysis File

Between 1994 and 2001—i.e., peak to trough of the total caseload, cases combining Food Stamps and participation in welfare dropped by over 60 percent. In contrast, cases combining Food Stamps and SSI dropped by less than 20 percent, and the Food Stamps-only caseload dropped by 30 percent.

Since 2001, the relative patterns look very different (see Figure 2.3). Cases combining Food Stamps and welfare continue to drop—another 10 percent from the 1994 level, about 15 percent from the 2001 level. Cases combining Food Stamps and SSI rose back to the 1994 level, which is nearly a 15 percent increase from the 2001 level. Finally, cases with Food Stamps but neither welfare nor SSI explode, 20 percent from their 1994 level and over 60 percent from their 2001 level.

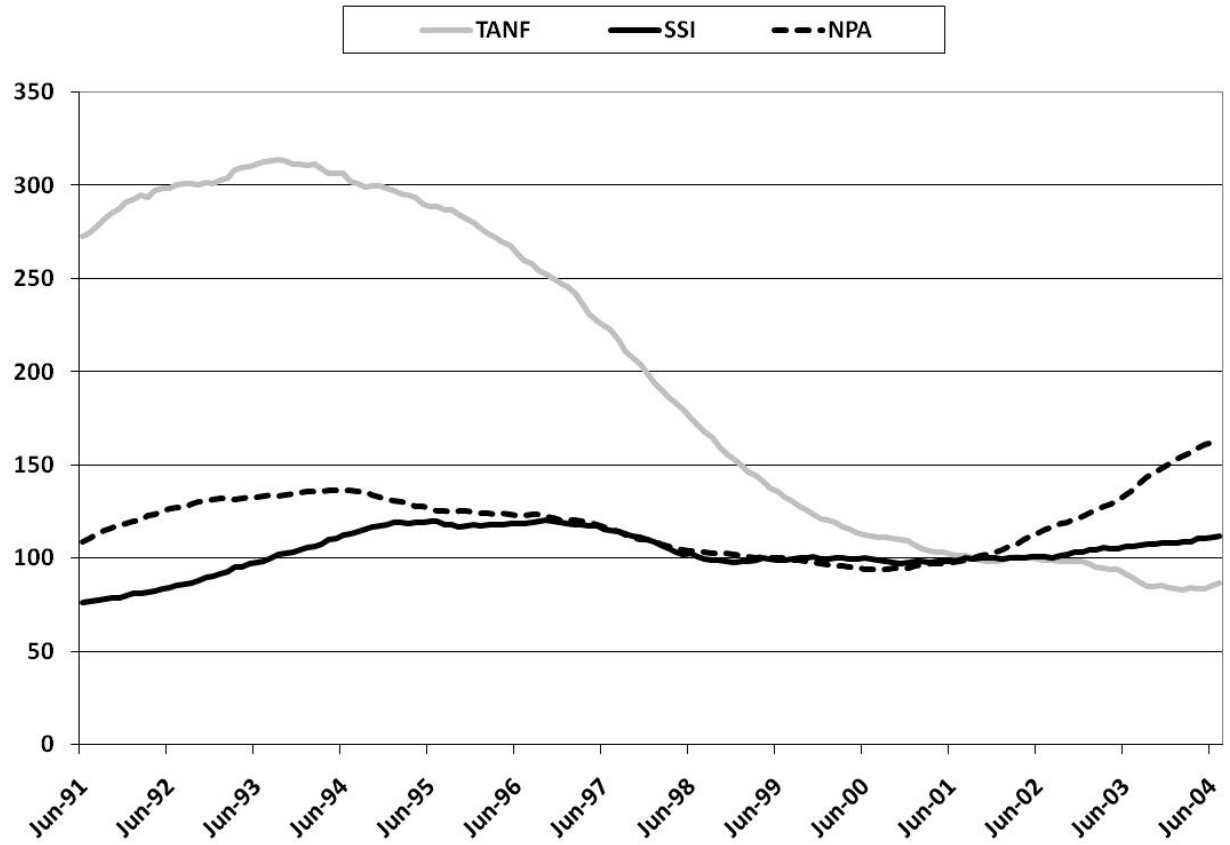

Figure 2.3—Normalized FSP Coverage Rate, by Cash Assistance Receipt Sub-Group (FFY2001=100)

Note: Rates are 12-month trailing averages of the computed coverage rate defined in the text, then divided by the FFY 2001 rate and multiplied by 100.
Source: Tabulations from RAND FSPQC Analysis File

The net result of these diverging participation trends has been a radical shift in the composition of the FSP caseload (See Figure 2.4). Through its peak, the FSP caseload was about half welfare recipients. In 2004, it is about one-sixth welfare recipients (48 percent to 17 percent[13]). In contrast, the share of combining SSI and food stamps has increased by half (from 13 percent to 21 percent) and the share with no cash assistance has increased by nearly two-thirds (from 38 percent to 62 percent).

[13] The proportion of FSP households receiving welfare has continued to fall, reaching only 12 percent by 2007.

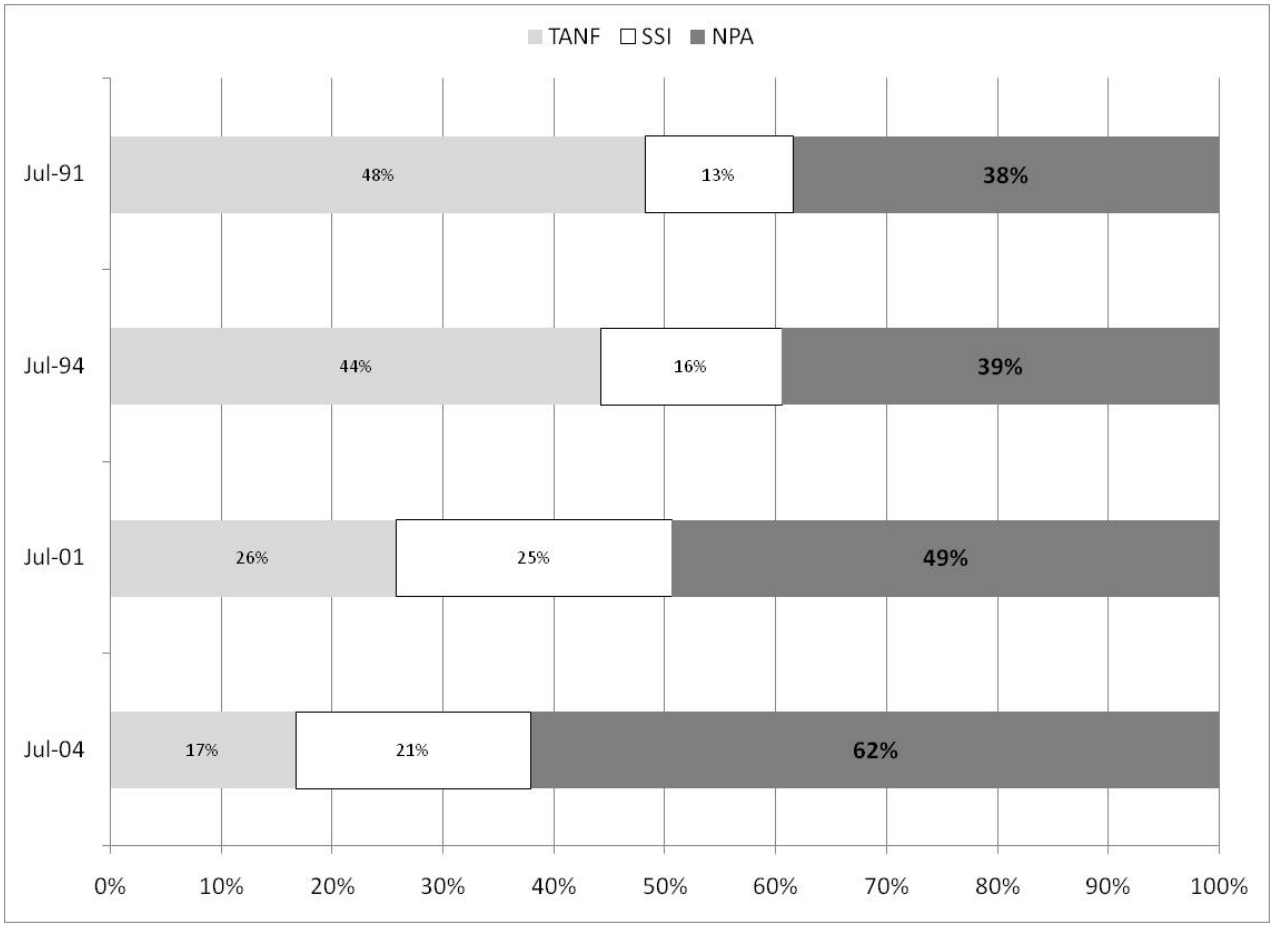

Figure 2.4—FSP Caseload Shares by Cash Assistance Sub-Group

Source: Tabulations from RAND FSPQC Analysis File.

Clearly, there is plenty to explain. Simple inspection of the preceding three figures, however, suggests several conjectures about the causes of the caseload variation:

- *FSP Policy*: A series of policy changes in the early 2000s lowered the compliance burden of participating in the FSP and expanded eligibility, especially for those not categorically eligible for the program.
- *Welfare Policy*: Federal welfare reform passed in 1996; State efforts under waivers preceded federal welfare reform; and State efforts to implement the provisions of federal welfare reform lasted for several years after 1996. Moving welfare recipients off the welfare caseload

meant, initially at least, increasing the burden of FSP participation for these leavers.

- *Case Conversion*: Many welfare leavers may have "converted" their combined welfare/Food Stamp cases to non-welfare Food Stamp cases. Offsetting increases in the FSP benefit, along with policies aimed at reducing the burden of participation among those not categorically eligible for the FSP, may have partially offset the increased burden of participation.
- *The Economy*: The path of the aggregate caseload follows the unemployment rate. The economy boomed in the late 1990s, followed by a recession in the early 2000s.

In the next chapter we provide a simple theoretical structure for these conjectures, we then describe the policy context and the specific policies (including the path of the economy), and how we measure them.

III. FORCING VARIABLES

In this chapter, we describe our "forcing variables"; i.e., the variables that might potentially shift the FSP caseload. Our discussion begins with a simple theoretical economic model of program participation. This simple theoretical economic model motivates the three classes of forcing variables that we consider in our empirical work: States' formal FSP policies and procedures, States' welfare policies, and the economy. Following our development of the theory, we describe the specific policies, discuss the expected effect, review the previous literature, and detail our methodological approach.

We organize our review of the previous literature by class of forcing variable. Table 3.1 provides a summary of the principal earlier studies of the determinants of the FSP caseload. Appendix B provides detail on our coding of State policies and dates of changes in those policies.

A SIMPLE THEORY OF FSP PARTICIPATION

What determines 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). Families choose a bundle of work and program participation to maximize their utility (consumption of goods, leisure, compliance burden, and stigma from program participation), subject to program rules and available labor market options.

Clearly anything that affects eligibility is likely to affect participation. In addition, this perspective suggests that not everyone who is eligible for the FSP and other transfer programs will actually participate (see GAO, 2004, for a discussion similar to the one presented below). Participation requires considerable effort. Indeed, studies suggest that to submit an initial application often requires several visits to the welfare office and about six hours of the applicant's time (GAO, 1999, 2004; Bartlett and Burstein, 2004). Similarly, recertifications are reported to take several hours and an in-person visit during regular working hours to a welfare office (GAO, 1999; USDA, 1999).

Table 3.1
Overview of Principal Earlier Studies

Authors	Participation Data (Period)	Notes
Wallace and Blank (1999)	CPS (1980-1996)	DV: Total; non-AFDC
Gleason, et al (2001)	FSPQC (1992-1999)	Linear time trends but no time fixed effects/some models. Other variables: Medicaid Policies.
Kornfeld (2002)	FSPQC (1987-1999)	State trends: Some models Models estimated for subgroups.
Ziliak, Gundersen, and Figlio (2003)	Agg. FSP (1980-1999)	Dynamics: Four lags of dependent variable See also Figlio, Gundersen, and Ziliak (2000).
Kabbani and Wilde (2003)	FSPQC (1990-2000)	DV: Also caseload w/ and w/o earnings, error rate Dynamics: Some models State trends: Some models
Currie and Grogger (2001)	March CPS (1980-1998)	State trends: Some models Models estimated for subgroups
Danielson and Klerman (2006)	Aggregate FSP data (1990-2004)	FE: State trends
Hanratty (2006)	SIPP (1996-2003)	DV: Income eligible, legal residents, with children (gross income <130% FPL; net income <100% FPL), stratifying 1-parent/2-parent
Ratcliffe, McKernan, and Feingold (2008)	SIPP (1996-2003)	DV: Low income (<170% FPL); plus households with children; single female headed households with children; 2-adults with children; ABAWD (18-50, not disabled, no children)
Klerman, and Danielson (2008; this paper)	FSPQC (1990-2004)	DV: Total, non-Hispanics State trends: Some models

Key: DV-dependent variable; FE-fixed effects; FPL-federal poverty line
Note: Unless indicated in “Notes” column all studies use the caseload (usually with some normalization for population) as the DV/Dependent variable, and all use econometric methods including fixed effects for State and year, but no dynamic terms (i.e., lagged dependent variables).

Data: FSPQC/Food Stamp Program Quality Control data, Agg. FSP/USDA Aggregate FSP Caseload Data (FNS 388 and/or FNS 388A), CPS/March Current Population Survey, SIPP/Survey of Income and Program Participation.

Not surprisingly, therefore, several studies have found that up to 40 percent of the eligible population does not participate because of the perceived burden of applying. Bartlett and Burstein (2004) report that among eligible FSP non-participants, 25 percent report that the "they would have to answer questions that were too personal", 40 percent report that "the application process required too much paperwork", 22 percent report that it would require "too much time away from work", 15 percent report that it would require "too much time away from child care or elder care responsibilities", and 13 percent report that "it was too difficult to get to the food stamp office".[14]

Given this burden, the smaller the expected FSP benefit, the less likely it will be that an eligible family will participate. McConnell, Ponza, and Cohen (1999) find that more than a third of those eligible report that they do not participate because they believe they are eligible for only a small benefit amount. Bartlett and Burstein (2004) report that 37 percent of eligible non-participants report making such a calculus. Emphasizing the link between AFDC/TANF participation and FSP participation, Bartlett and Burstein report that over two-thirds of this group reported that applying for food stamp benefits was not worth the effort because they believed they were not eligible for cash under TANF.

In addition, in the United States, participation in welfare programs, including the FSP, carries two types of "stigma". First, there is external stigma. People who participate in welfare programs (including the FSP) may be looked down upon by others. The more obvious it is to others that someone participates in a welfare program, the higher the stigma. Consistent with this perspective, several studies have found that a quarter of eligible non-participants who had received food stamps in the past reported some form of

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[14] For earlier studies with similar results, see Ponza et al., 1999; McConnell and Ponza, 1999; Bartlett, et al., 1992; see also Zedlewski and Brauner, 1999.

disrespectful treatment while using food stamps in a store (GAO, 2004; Bartlett and Burstein, 2004).[15]

Second, there is internal stigma. People prefer not to participate in welfare programs, even if no one else knows. Bartlett and Burstein (2004) term this "a desire for *personal independence*" (emphasis in the original). This is one interpretation of comments that people do not participate in the FSP because they can "get by without it"; i.e., the small benefit is not worth the "cost" in stigma (combined with the compliance burden). According to Bartlett and Burstein (2004), this group includes 89 percent of eligible non-participants. In addition, they report that 64 percent of eligible non-participants "do not like to rely on government assistance".

Some have claimed that both types of stigma increased with the welfare reform of the mid-1990s. On the other hand, the adoption of EBT in the early 1990s may have lowered stigma. While Food Stamps were quite distinctive, EBT cards look similar to a credit or debit card. We note that Bartlett and Burstein (2004) find no evidence of an increase in external stigma from 1996 to the early 2000s.

Finally, an improving economy will also lower the caseload by improving labor market opportunities. As earned income rises, the FSP benefit falls. Some people's income will rise so as to make them ineligible. Others, now eligible for only a smaller benefit and considering the burden and stigma of application, will decide not to apply or recertify. Consistent with this perspective, nearly all previous studies have found that the FSP caseload is extremely sensitive to the economy (see Table 3.4, below). Also consistent with this behavioral perspective, Martini (1992) finds that participation increases with the size of the benefit a household would receive.

Beyond this conventional economic model of program participation, there is some evidence that FSP participation is depressed by misunderstand of eligibility. It appears that some welfare leavers, legal immigrants, and U.S. born children of illegal immigrants do not realize that they are eligible (Bartlett, 2004). Given variation over time in exactly which immigrants are eligible for the FSP, this confusion is not surprising.

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[15] For older evidence that is consistent with this more recent evidence, see McConnell and Nixon, 1996; Ponza et al, 1999; see also the focus group evidence in Ponza and McConnell, 1996.

Given this simple theoretical perspective, we turn to a discussion of the specific forcing variables we include in our regression specifications, how we measure them, and the previous literature on their effects on the FSP caseload.

FOOD STAMP POLICIES AND PROCEDURES

The FSP benefit is not "free money". Participation requires considerable time, effort, and expense (e.g., bus fare) from recipients. Our theoretical model suggests that as program "policies" and "procedures" make participation less onerous (i.e., lower costs of participation or less stigma; e.g., simplified reporting, EBT), participation in a program becomes more attractive and enrollment would be expected to rise; conversely, as program rules become more onerous (e.g., shorter recertification intervals, work requirements for ABAWDs), participation in a program becomes less attractive and enrollment would be expected to fall.

Unlike the cash assistance program (AFDC/TANF), food stamp benefit levels and (most) eligibility requirements have been uniform across the continental United States since 1971 (in Alaska and Hawaii benefit levels are higher to account for a higher cost of living in those two states). Thus, using our difference-of-differences (DoD) econometric methods, we cannot estimate the effects of food stamp benefit levels (or other national policies). However, in the last decade FSP authorizing legislation and USDA regulations have granted States discretion over a widening set of FSP policies.

Table 3.2 summarizes the treatment of FSP policy in the principal earlier studies. The table also summarizes the significance pattern of the results. Unless otherwise noted, each statistically significant result is in the direction predicted by our simple theoretical framework. See Table 3.1 for the period and caseload data source in each of these studies.

Reporting Requirements: Once a household is deemed eligible for food stamps, it has several ongoing compliance requirements. Many of the details of those requirements are now State options. Furthermore, changes in regulation and statute have shifted the options open to the States over the 1990s and early 2000s.

- *Recertification:* Once a household is deemed eligible for food stamps, it is certified for a specific number of months, with the length of the certification period depending on characteristics of

the household and on State policy. Once this period ends, the household must be recertified. At recertification, a representative of the household meets with the FSP caseworker and provides documentation of income, household composition, residence, vehicles (if not excluded), assets, and changes in child support obligations. While this recertification was once almost always done in person, more recently, States have the option of allowing it to occur by mail. A mail recertification option considerably lowers the compliance burden.

Table 3.2
Food Stamp Variables in Principal Earlier Studies

Authors	FSP Vars
Wallace and Blank (1999)	None
Gleason, et al (2001)	Recert***
Kornfeld (2002)	ER***, EBT**, Recert***
Ziliak, Gundersen, and Figlio (2003)	ER, EBT, ABAWD Waiver**
Kabbani and Wilde (2003)	Frequent Recert***, EBT**, Outreach Expenditures**, Monthly Reporting***
Currie and Grogger (2001)	EBT (* for married no kids and rural), Recert (*** for single heads and ** for rural)
Danielson and Klerman (2006)	EBT*** (implementation dummy), ER (overpayments, current/lag)***, ABAWD-W, Simp (slope**), Trans (two-year spline**)
Hanratty (2006)	Recert*** (for earners), Simp*** (1-parent only),
Ratcliffe, McKernan, and Finegold (2008)	Recert***, Simp

Abbreviations: Recert/Recertification Interval, Rep/Reporting interval, ER/FSP Error Rate, EBT/EBT Implementation, Simp/Simplified Reporting, Trans/Transitional Benefits.
Significance levels are coded conventionally: "*" Statistically significant at the 10 percent level, "**" at the 5 percent level, and "***" at the 1 percent level.

- Periodic Reporting: For most of the 1980s, FSP participants were required to report monthly on their income and family situation. Beginning in 1988, States were allowed (and encouraged) to shift to quarterly reporting; i.e., a household had to file a report once every three months, rather than every month.
- In addition, November 2000 regulations allowed States to shift to simplified reporting for cases with earned income. The Farm Security and Rural Investment Act of 2002 (the "2002 Farm Bill") extended that option to almost all households.16 Under simplified reporting, recertification periods can be six months in length and changes during the certification period need not be reported unless they raise the household's income above 130 percent of the federal poverty line. By the end of FFY 2003, 42 States had adopted simplified reporting (CBPP, 2003; NAPIPM, 2004; USDA, 2004).
- Finally, States can opt to reduce the reporting burden for households not included in simplified reporting. For example, only if a household member changes his or her job does the household need to report a change (so-called "status reporting"). Or States can define changes to income that are necessary to report more broadly—e.g., $100 per month rather than $10 per month.

State choices about reporting and certification intervals have important implications for their error rates and those implications have shifted considerably over the last decade. See Dean and Rosenbaum (2002) for a discussion. In brief, through about 2000, shorter reporting and recertification periods led to lower error rates through two pathways.

- Prior to 2000, QC reviewers were to compare benefits paid against actual earnings. FSP participants were supposed to report changes in earnings, but in qualitative interviews, caseworkers state that

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16 The 2002 Farm Bill changes can be found at Section 4109 of the Farm Bill amending Section 6(c)(1) of the Food Stamp Act and codified at 7 U.S.C. 2015(c)(1). They were effective October 1 2002.

Trippe et al. (2004) States: "Missouri and Louisiana were two of the earliest States to adopt the option, implementing it for earners in May and August 2001, respectively, and expanding it to nonearners under the 2002 Farm Bill. Ohio implemented simplified reporting for earners in July 2002, and Arizona first implemented it for both earners and nonearners in January 2003."

changes are rarely reported. Thus, changes in earnings that should have been reported were often not reported, resulting in an FSPQC error. Required (monthly or quarterly) reporting and certification assured that the information the State had was more current and therefore more likely to correspond to what the QC reviewer would find—leading to fewer QC errors. Consistent with this pathway, USDA encouraged States with high error rates to shorten their reporting intervals in order to lower their error rates.

- By raising the burden of participation, States could induce some of the more error prone cases (in particular, cases with earnings) to leave the FSP caseload.

This changed in the early-2000s. Changes to statutes and regulations lowered the probability of a penalty. States are now penalized if their combined error rates are above the national average for two years in a row, rather than the one-year period used earlier (CBPP, 2005). Furthermore, beyond lowering the compliance burden for FSP participants, under simplified reporting the relevant information was the information as of the earlier report. There was thus no reason to collect information more frequently. Thus, adopting simplified reporting lowered error rates, lowered recipients' compliance burden, and—because there were fewer reports—lowered the administrative burden on the State welfare departments.

Not surprisingly, States have moved quickly to adopt simplified reporting. Figure 3.1 plots the (weighted) fraction of the FSPQC sample, in each month, in a state that has adopted short recertification periods, a standard proxy for these changes in the administrative burden of participation. Short recertification periods increase the burden of participation and would be expected to lower the caseload. Figure 3.1 also includes the aggregate caseload. The inverse relation of the two time series is striking. The timing is almost exactly coincident with the initial rise, the late-1990s drop, and the early-2000 rise in the caseload.

The existing literature suggests strong effects of the compliance burden on the FSP caseload. Currie and Grogger (2001) find that long recertification intervals increase the FSP caseload. Kornfeld (2002) finds that frequent recertification periods (defined as the proportion of the caseload whose reporting period is one to three months) lower the FSP caseload. Kabbani and Wilde (2003) find strong evidence that shorter recertification periods cut the

FSP caseload and some evidence that monthly reporting requirements have a similar effect. Hanratty (2006) finds that three month recertification periods for earners reduce the probability of participation among both single and two-parent families that meet income tests. Finally, Ratcliffe, McKernan, and Finegold (2008) find that longer recertification periods increase the probability of participation across all types of low-income households they examine

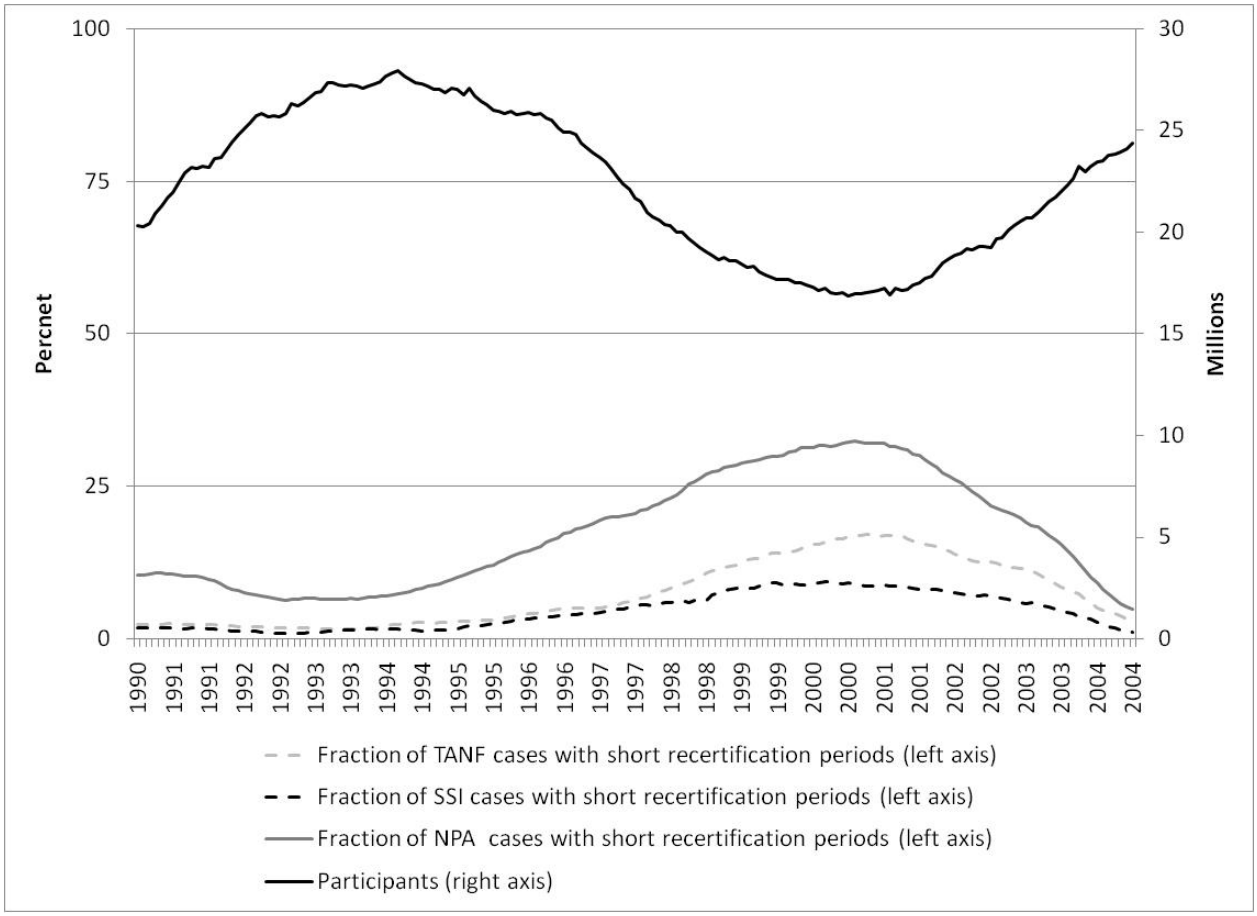

Figure 3.1—Short Recertification Periods and the FSP Caseload

SOURCE: Tabulations from RAND FSPQC Analysis File.

Following the earlier literature, we code this concept using the fraction of cases (in a cash assistance sub-group) with short certification periods. We define "short" as three months or less. We compute this fraction directly from the information in our FSPQC data files. We would have liked also to include reporting requirements (e.g., monthly, quarterly). However, that

information is only available in the FSPQC data for 2003 and 2004 and sporadically in earlier years.

Electronic Benefit Transfer (EBT): Our theoretical discussion emphasized the non-monetary "costs" of participating in the FSP. Among those costs is the "stigma" induced by using food "stamps" and thereby revealing participation in a "welfare" program (broadly defined). EBT cards look like regular credit or debit cards. The transition to EBT should therefore have reduced that "stigma".

With PRWORA, States were required to convert from paper food "stamps" to EBT cards. Twelve States have put only FSP benefits (and in 2 cases, also WIC) on the EBT card. Other States have put TANF and sometimes other cash assistance on the card.[17]

Figure 3.2 plots EBT adoption and the FSP caseload. EBT adoption would be expected to raise the FSP caseload. The previous literature has found mixed results on the effects of EBT adoption. Currie and Grogger (2001), Kornfeld (2002), and Danielson and Klerman (2006) find a small positive effect on the caseload. Kabbani and Wilde (2003) and Ziliak, Gunderson, and Figlio (2003) find no effect. Ratcliffe, McKernan, and Finegold (2008) find that the introduction of EBT increases participation among two-adult households with children, but not other types of households.

We tabulate information on EBT adoption from FNS (available at http://www.fns.usda.gov/fsp/ebt/ebt_status_report.htm).

Nutrition Education and Outreach Initiatives: There is considerable evidence that some eligible families do not participate in the FSP because they do not realize that they are eligible. Bartlett and Burstein (2004) report that almost all eligible non-participants are aware of the FSP, and many have participated in the past. However, about half do not realize that they are probably now eligible and there is considerable confusion as to eligibility requirements for individuals not on AFDC/TANF.[18]

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[17] Four States implemented EBT Statewide before the passage of PRWORA. The earliest was Maryland in April 1993. The last State completed conversion in October 2003 (USDA, 2004).

[18] For older evidence that is consistent with this perspective that lack of knowledge is important, see Coe, 1983, GAP, 1988; Hollenbeck and Ohls, 1984; see also GAO, 1999. Daponte, Sanders, and Taylor (1999) provide random assignment evidence.

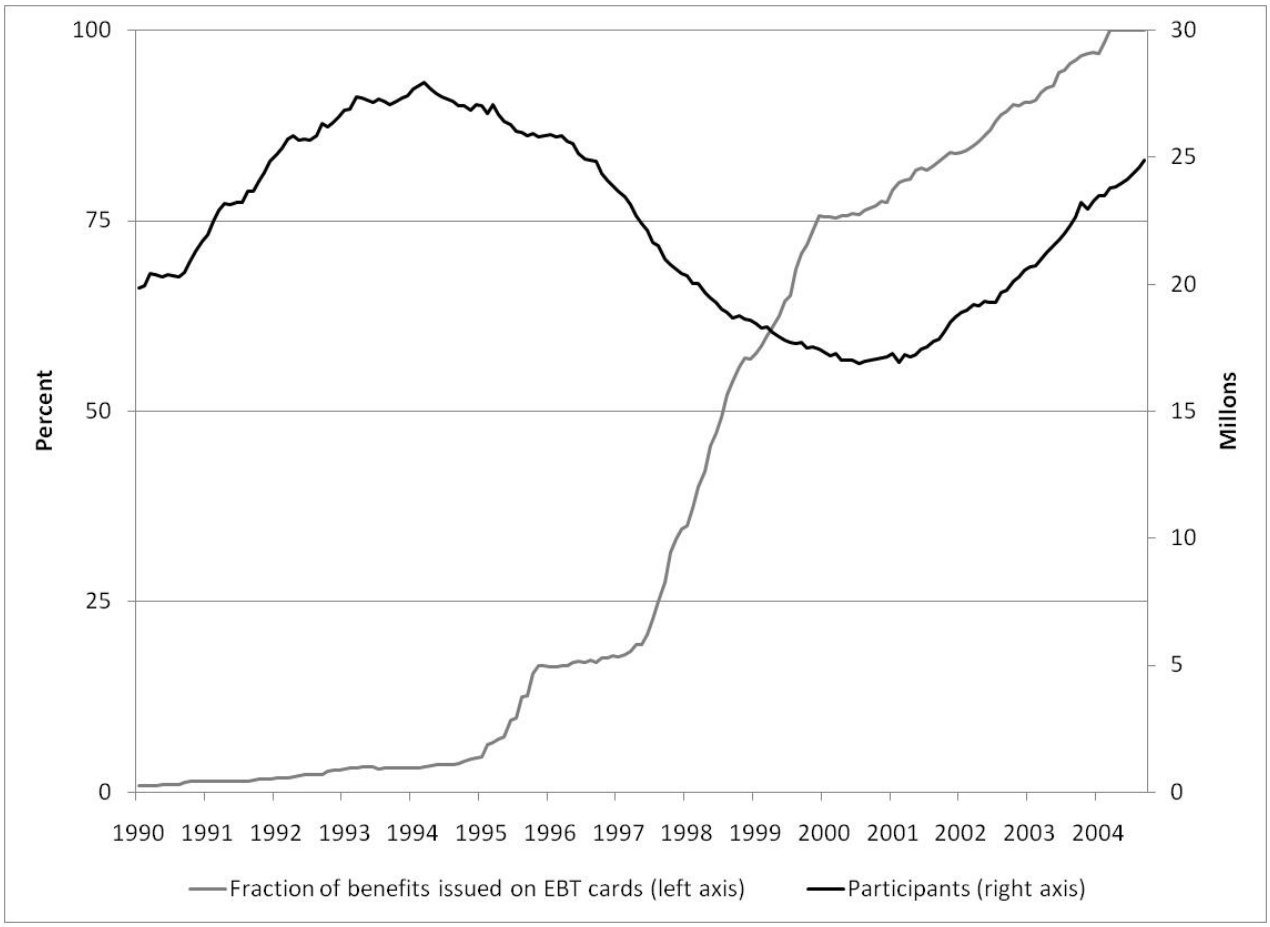

Figure 3.2—EBT Adoption and the FSP Caseload

SOURCE: Tabulations from RAND FSPQC Analysis File.

In response to the sharp decline in the FSP caseload in the late 1990s and reports of increased use of food banks and food pantries (GAO, 1999), USDA allocated funds for FSP outreach (GAO, 2004). We explored proxying for these efforts as expenditures per capita (as in Kabbani and Wilde, 2003 and Ratcliffe, McKernan, and Finegold (2008).

Other Policies: In Appendix B, we review several other policies for which there is inter-State variation, but which we do not include in our estimated models. Those policies include outreach and education efforts, Transitional Food Stamps, Asset Policies, and Immigrant Policies. In the appendix, we discuss this decision. In brief, there are few degrees of

freedom available and on a priori grounds we do not expect these policies to have effects as large as the included policies. In general, these decisions are supported by results from the previous literature (also surveyed in the appendix.

WELFARE POLICY

Because well over half of the FSP caseload (and much more in the early 1990s) is composed of households that also receive, or are potentially eligible to receive welfare (AFDC/TANF), welfare policies are likely to have an impact on the FSP caseload. The effect of welfare policies is complex. Welfare usually conveys categorical eligibility for FSP benefits. In addition, many people who leave welfare remain income-eligible for FSP benefits (see the discussion of Transitional Food Stamps in Appendix B and the citations there). Some leavers will remain in the non-welfare part of the FSP caseload. The fact that few of them appeared to do so was of considerable concern in the late 1990s (e.g., prompting an official Report to Congress; USDA, 2001). The role of welfare reform in causing the decline in the FSP caseload in the late 1990s is a focus of this study.

The period covered by our data (1990-2004) was a period of sweeping change in welfare policy. At the beginning of the period, State welfare programs were strongly constrained by federal regulations and included benefit structures that strongly discouraged combining work with welfare, weak work requirements, and no time limits (see Grogger, Karoly, and Klerman, 2002). This changed via waivers from US DHHS to States in the 1990s and culminated with all States' implementation of TANF programs between 1996 and 1998. The transition to TANF substantially altered States' operation of 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 non-compliance with program requirements, and increased financial incentives to combine work and welfare characterized most States' TANF programs.

Figure 3.3 plots broad welfare reform proxies and the FSP caseload. The welfare reform measures rise sharply in the mid-1990s. Actual implementation of these reforms (e.g., strengthened sanctions and time limits) continued over the next few years. Welfare reform made welfare program participation more

burdensome. It would thus be expected to lower the FSP caseload. These reforms thus might explain some of the sharp drop in the FSP caseload over that period.

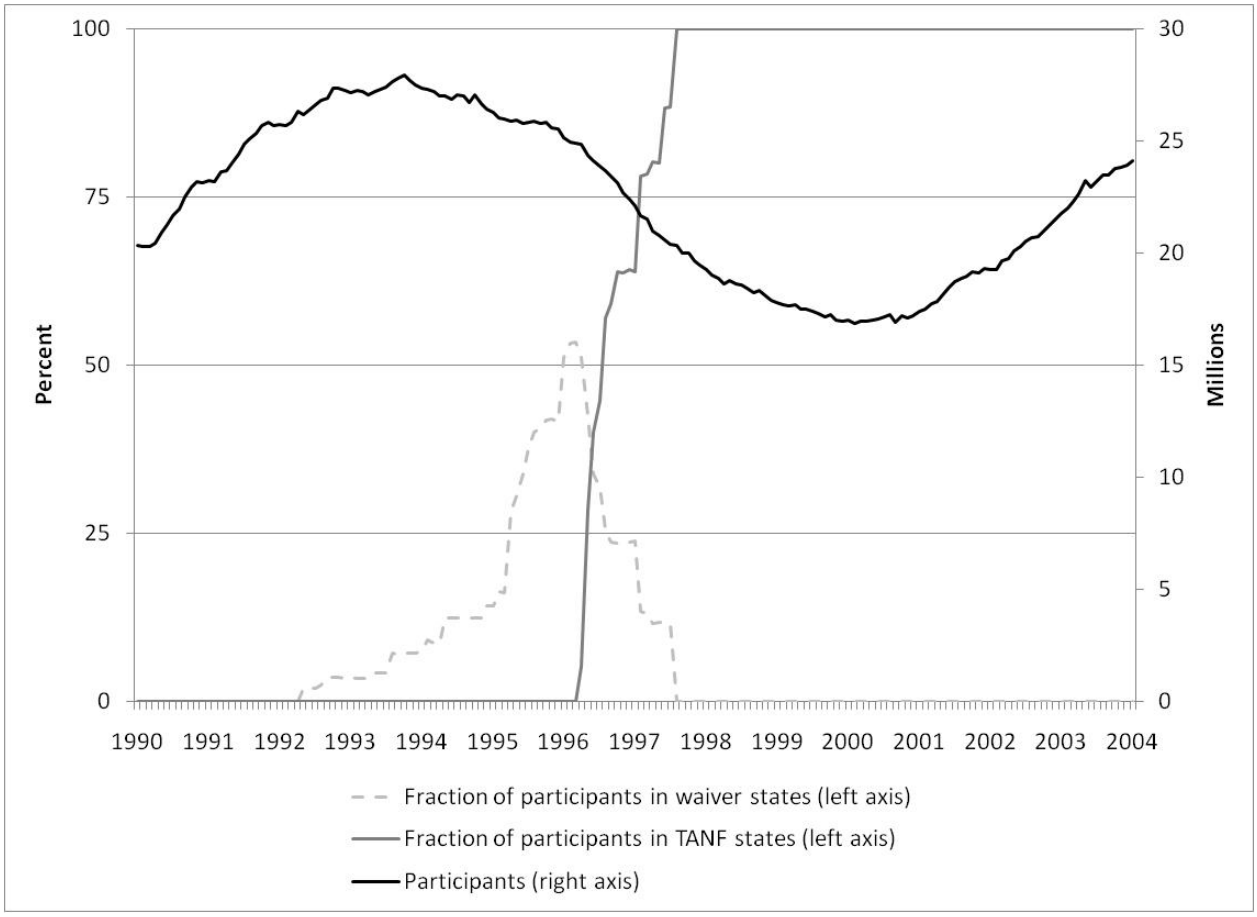

Figure 3.3—Welfare Reform and the FSP Caseload

SOURCE: Tabulations from RAND FSPQC Analysis File.

Table 3.3 summarizes the treatment of welfare policy in the principal earlier FSP studies. Grogger, Karoly, and Klerman (2002) summarize the treatment of welfare policy in the welfare reform literature.

In our empirical work, we considered two ways to specify welfare reform. Our primary approach includes high-level summaries of reform; specifically, the number of waivers and the time since implementing a State's new TANF program. We also considered specifications using specific state policies. In work with state welfare caseloads (Danielson and Klerman, 2008), we generated plausible estimates of the effects of individual policies. We explored

similar models here with FSPQC data, but the results were not precisely estimated.

Table 3.3
Welfare Variables in Principal Earlier Studies

Authors	Welfare Vars
Wallace and Blank (1999)	BL, AFDC-UP***, AFDC-W**, AFDC caseload***
Gleason, et al (2001)	Dis, Sanc, TL
Kornfeld (2002)	Dis*, TL, Sanc (between ns and ***), Family Cap***
Ziliak, Gundersen, and Figlio (2003)	BL, AFDC- W** (pre reform), TANF I, AFDC caseloads
Kabbani and Wilde (2003)	AFDC-W, TANF-I
Danielson and Klerman (2006)	AFDC-W (slope and spline), TANF-I (slope and spline)***, ln(max BL)
Hanratty (2006)	AFDC-W** (2-parent only), TANF-I* (1-parent only), BL** (2-parent only, “wrong sign”)
Ratcliffe, McKernan, and Finegold (2008)	BL, Dis, Sanction*, TL; Results for all households; more effects for sub-populations (defined on presence of children and number of adults)

Abbreviations: AFDC-W/AFDC Waiver, TANF-I/TANF Implementation, BL/Welfare Benefit Level, Sanc/Sanction, Dis/Earned Income Disregard, TL/Time Limit, AFDC-UP/AFDC Unemployed Parent.
Significance levels are coded conventionally: “*” Statistically significant at the 10 percent level, “**” at the 5 percent level, and “***” at the 1 percent level.

THE ECONOMY

There is broad consensus that the economy affects the FSP caseload (USDA, 1999). The correlation is unmistakable in a simple time series plot (e.g., Figure 3.4).

That the economy and the FSP caseload track is not surprising. We would expect a better economy to make work, more hours of work, and higher wages easier to find and more attractive and, thus to raise 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 to leave the family still eligible for some, but smaller, FSP benefit. Some of those families will decide that the costs (compliance, loss of privacy, stigma, work requirements) are not worth the smaller benefit. Thus, we would expect FSP participation to fall when the economy booms.

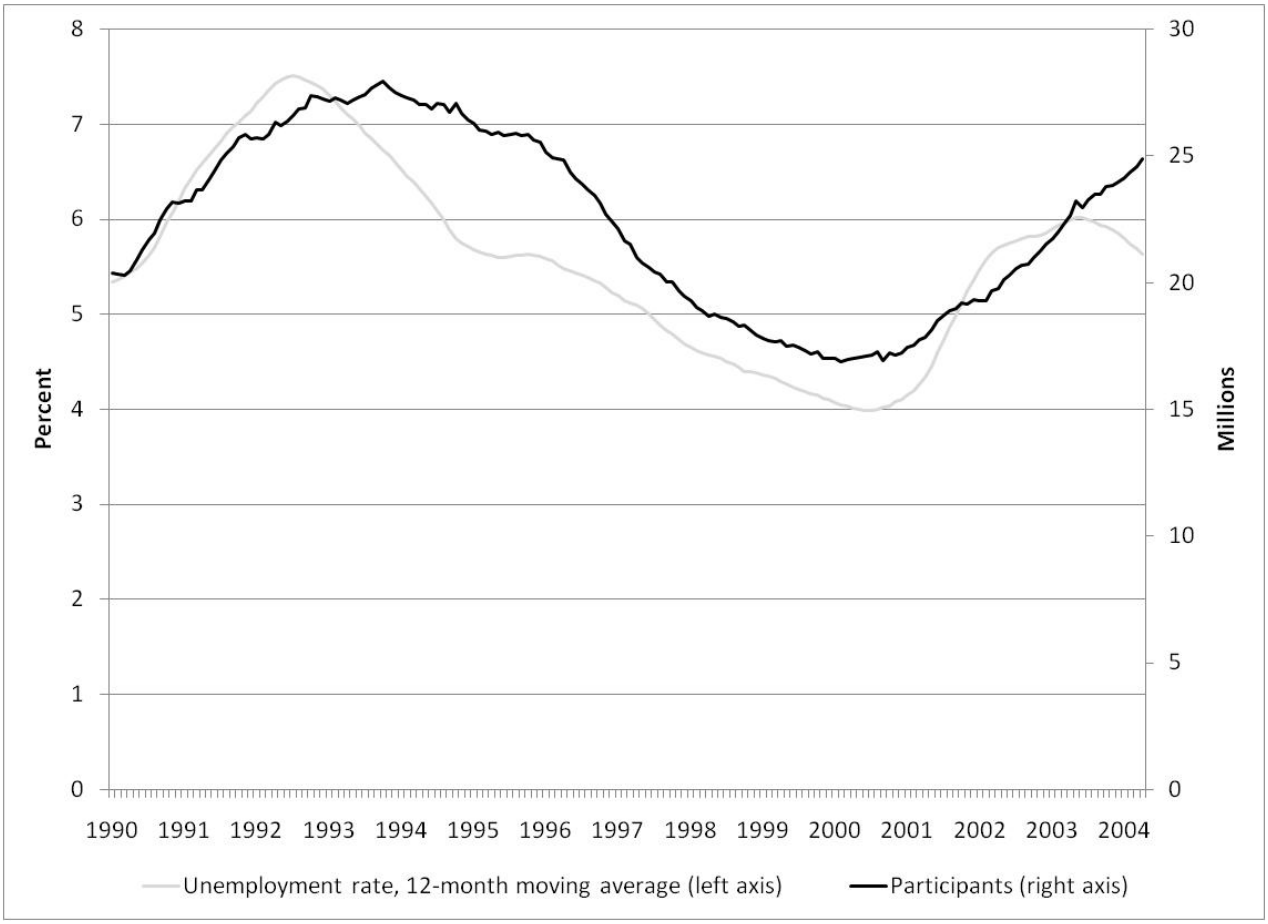

Figure 3.4-The Unemployment Rate and the FSP Caseload

SOURCE: Tabulations from RAND FSPQC Analysis File.

The period we examine opened with a recession, followed by a long and robust expansion, a mild recession, and finally an expansion (see Figure 3.4). Given the coincidence of welfare reform, FSP policy and procedure changes, and the robust economic expansion of the 1990s, controlling for policies and the

economy is crucial if we want to accurately estimate the effects of the policies of interest.

Table 3.4 summarizes the treatment of the economy and the pattern of significance in earlier studies. Previous research typically proxies for the economy using the unemployment rate. That literature finds that FSP caseloads are strongly counter-cyclical (Hanson and Gundersen, 2002). However, the fraction of the caseload change explained by the economy 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 (see Haider, Klerman, and Roth, 2003).

OTHER FORCING VARIABLES

As we discuss in the next chapter, we control directly for broad demographic shifts. Two other time-varying forcing variables are worthy of discussion: The minimum wage, and the Earned Income Tax Credit (EITC).

Minimum Wage: A higher minimum wage could have either a negative or a positive effect on the caseload. A higher minimum wage may induce businesses to substitute away from (unskilled) labor, thus decreasing employment and thereby the caseload; at the same time, the minimum wage jobs that remain are better paying, thus reducing caseload. In net, the effect on welfare caseloads is ambiguous. Previous research has found a negative effect of the minimum wage on the welfare caseload: CEA (1999) estimates an elasticity of between -0.25 and -0.52, while Grogger (2004) also finds a negative impact of the minimum wage on welfare use among single mothers with children over nine. We use the State-level minimum wage and, following CEA (1999), compute the real monthly earnings of an individual working at the minimum wage for 30 hours a week.

Earned Income Tax Credit (EITC): There were also major expansions in the federal Earned Income Tax Credit (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), the methods we use that enable us to estimate the effect of FSP and welfare policy changes on the caseload do not allow us to estimate the effect of nationwide policy changes. A minority of

States had a state earned income tax credit during this period. They are much smaller and we do not consider them here.

Table 3.4
Economy Variables in Principal Earlier Studies

Authors	Econ Vars
Wallace and Blank (1999)	UR (L2)***, ln(median wage)***, ln(20th quintile of wage distribution)
Gleason, et al (2001)	UR (L1)***, Mean Income (L1)***, PR (L1**), MMW (L1)***
Kornfeld (2002)	UR(L2)***, EGR, ln(wage), ln(wage 20th percentile)**
Ziliak, Gundersen, and Figlio (2003)	UR(L4)*** (t and t-4), EGR(L4), Ratio of 50th to 10th Income Percentile
Kabbani and Wilde (2003)	UR(L2)**, EGR
Currie and Grogger (2001)	UR
Danielson and Klerman (2006)	UR (L1)***, Per cap employment** (L1), Total per cap worker earnings*** (L1), ln(min wage earnings)
Hanratty (2006)	UR** (*** 1-parent), service wage** (2-parent only)
Ratcliffe, McKernan, and Finegold (2008)	None

Abbreviations: UR/Unemployment Rate, EGR/Employment Growth Rate, PR/Poverty Rate, MMW/Mean Manufacturing Wage. Lag structure indicated in parentheses: No parentheses/no lags, others Ln/n-lags.
Significance levels are coded conventionally: "*" Statistically significant at the 10 percent level, "**" at the 5 percent level, and "***" at the 1 percent level.

IV. ECONOMETRIC METHODS

Having described our data for FSP participation and the forcing variables, we now describe our methods. We begin with a broad overview of the approaches used in the existing literature relative to the DoD econometric methods we use. We then describe our methods in detail.

SIMULATION OF FSP TAKE-UP

USDA's semi-official estimates of policy impact are derived from a simulation model maintained by MPR based on the March Current Population Survey.[19] That model is used to generate annual estimates of FSP coverage rates and the effect of policy changes (e.g., Cunnyngham, 2005). Rather than estimating participation directly (as we do below), the MPR simulation model takes a two-step approach. In the first step, MPR estimates the number of individuals eligible to participate in the FSP. In the second step, they estimate the participation rate, the fraction of those eligible who actually participate.

For two reasons, this approach is quite attractive.

1. As noted by Cunnyngham (2005), the participation rate is of intrinsic interest. It can plausibly be used as a performance measure as defined in the Government Performance and Results Act of 1993. Specifically, while the FSPQC error rate focuses on people who get food stamps who should not (or who get too much food stamp benefits), the participation rate focuses on the extent to which those who should/could get food stamps actually get them. Nevertheless, USDA does not use the participation rate as an official performance measure. The required data are not available in a sufficiently timely manner. USDA's Program Access Index—the ratio of participants to the population at or below 125 percent of the federal poverty line, as measured in the American Community Survey—as a performance measure.

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[19] On microsimulation models in general, see Citro and Hanushek (1991). On the MPR efforts see: http://www.MPR-mpr.com/welfare/math-2.asp.

2. As Cunnyngham (2005 and earlier years) shows, the methods are quite powerful. They allow the precise simulation of the effect of past and potential future policy changes—even policy changes that affect only a small part of the FSP population or that were adopted, simultaneously, nationwide.

Nevertheless, their simulations methods are, for several reasons, not perfect; and the econometric methods we apply below are complementary. The most notable problem with the simulation approach is that the data are not sufficient to support the approach. MPR's simulation model requires information on eligibles and participants. For each group, the model requires enough information to establish eligibility under current and alternative FSP policies.

Consider first information on participants. The FSPQC data (the same data we use below) provide relatively high quality information on actual participants. The main issue is that inasmuch as alternative policies would require new information on those currently receiving Food Stamps, the current FSPQC sample data collection instrument may not collect the right information. Thus, for example the pre-1996 FSPQC data does not include detailed information on time since entering the U.S. for noncitizens. This complicates the estimation of the likely or actual effect of the immigrant reforms.

Now consider the information on eligibles. Here the problem is much more severe. By definition, FSP administrative data files (e.g., the FSPQC data) do not have information on eligible non-participants. Since estimating participation rates requires counts of eligible individuals, MPR uses the March Current Population Survey (CPS), Annual Demographic File. The CPS and other surveys are known to under-count participants in means-tested programs, including FSP participants, and there is some evidence that the under-reporting is becoming more severe.[20] This is why MPR uses the CPS to generate information on eligibles, but not on participants.

The CPS data, however, have several problems.

1. The March CPS simply does not have all of the information required to impute eligibility. There is almost no information on assets.

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[20] See the survey in Hotz and Scholz, 2000. See also Primus, et al, 1999; Marquis and Moore, 1990; Bolinger and David, 1997; Wheaton and Gianarelli, 2000; Klerman, Ringel, and Roth, 2005; Bavier, 2000.

Information on citizenship and immigration is limited. Information on work and other activities is not sufficient to verify satisfaction of the ABAWD rules. The Census household does not exactly correspond to the Food Stamp Unit (and no direct information is available on what the Food Stamp Unit would be). The CPS does not have enough information to compute net income.

2. The March CPS uses an annual income concept. The FSP uses a monthly income concept.
3. The model must implicitly assume that the information provided is exactly correct. We know that FSP participation is under-reported (Resnick, et al, 2004). It seems likely that other information is sometimes inaccurate. For example, welfare/AFDC/TANF and Supplemental Security Income (SSI) provide categorical eligibility for the FSP, but they are themselves under-reported in the CPS by about 25 percent (Klerman, Ringel, and Roth, 2004).

MPR is well aware of these issues. Appendix C to their report (Cunnyngham, 2005, and earlier years) provides a careful discussion of their approach to each of these issues. They plausibly impute the missing information from other data sources (often the Survey of Income and Program Participation (SIPP); but also the Panel Study of Income Dynamics (PSID), and other administrative data) and plausible assumptions. Their work is careful and the approach is sensible. However, no one would deny that the resulting estimates of eligibles are imperfect.

Beyond these problems with the data, the simulation approach implicitly assumes that the household and individual characteristics leading to eligibility are not themselves affected by program policies and regulations. This seems implausible.[21] The basic premise of PRWORA was that when faced with different incentives, more individuals would choose to work and leave welfare.[22] That is also an implication of any economic model of program participation (e.g., Keane and Moffitt, 1998; Grogger, Karoly, and Klerman,

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[21] See Kornfeld, 2002, p. 5-3 for a similar argument.

[22] See USDA (2001, p. 31, fn. 5) “The changes in eligibility rules may explain even more of the fall in the number eligible if some of the increase in income and assts of these groups is a result of welfare reform changes encouraging work.”

2002). Such changes in behavior would change income and thus FSP eligibility.[23]

It follows that the simulation results do not usually incorporate the effects of any behavioral response to policy changes. In the language of tax modeling, they are therefore "static estimates" rather than "dynamic estimates" (e.g., Mankiw and Weinzierl, 2005; CBO, 2005). Ideally, any policy effect estimates would be dynamic; i.e., they would include changes in behavior that lead to changes in eligibility.

Finally, these simulation methods are not informative for policies that change the conditions of participation, but not eligibility. Thus, any effects of EBT (which might lower stigma) or reporting and recertification policy changes (which might lower burden) are not captured by the participation rate estimates. MPR's simulation methods provide no way to disentangle the effects of such changes on take-up.

The problems raised by the last two points are not that simulation methods are incompatible with behavioral responses. Given estimates of behavioral responses, a simulation model could incorporate such responses.[24] However, the current MPR models do not incorporate such responses. Part of the problem is probably the lack of appropriate estimates of such behavioral parameters (e.g., how much participation would increase for every hour the application and recertification burden were reduced).

SURVEY AND SIMPLE REGRESSION ESTIMATES

An alternative to this simulation-based approach is estimation of econometric models.[25] For a given observed policy change, compute the change in the number of participants—*while attempting to hold all else equal*. The last caveat is crucial and challenging. The conventional approach is to include a long list of proxies for individual characteristics. This approach

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[23] Econometric estimators that stratify on family structure are also open to this criticism. We note that the evidence that policy affects work and welfare participation is strong. The evidence that policy affects family structure (e.g., marital status, the presence of children) is much weaker. For a comprehensive review, see Grogger, Karoly, and Klerman, 2004).

[24] See for example MATH STEWARD (Jacobson, et al, 2001).

[25] Random assignment experiments would be another option. See Grogger, Karoly, and Klerman (2002) on the relation of econometric estimates and random assignment studies for welfare policy.

is potentially attractive for the analysis of individual level survey data (e.g., from the CPS or the SIPP).

However, this conventional approach is problematic from at least two perspectives. First, we know that program participation is seriously under-reported in survey data (e.g., SIPP or CPS; see Cunnyngham, 2005). If the dependent variable is not properly measured, it seems likely that the estimated effects will also be problematic. Formally, if under-reporting did not differ by individual characteristics, we would be less concerned about under-reporting (with appropriate functional forms, it would only affect the constant). However, by matching CPS data on Californians to California administrative data that records program participation, Klerman, Ringel, and Roth (2004) show that for welfare and Medicaid, under-reporting itself varies with covariates. The same is likely to be true for the FSP.

Second, while survey data are rich in individual characteristics, it is not clear that those characteristics are valid regressors. We would not want to condition on (i.e., include as a regressor) any characteristic that might itself be affected by the forcing variables. This line of argument suggests that neither family structure, nor family income, are valid regressors. Granting this point, the advantage of the additional information in the survey data nearly disappears.

MOTIVATING THE DOD ESTIMATOR

Instead, we follow the natural experiments literature (e.g., Meyer, 1995). Specifically, we regress aggregate coverage rates at the State-month level (stratified by demographic characteristics) directly on the policies themselves. We control for other factors using dummy variables for State and calendar year and month. Such methods are known as “difference-of-difference” (DoD) methods.

The name “difference-of-differences” derives from a simple pre-/post experiment/control design for a binary policy implemented only in the post-period and only in the experimental group, in the absence of random assignment (Cook and Campbell, 1979; Shadish, Cook, and Campbell, 2001). In that design, the DoD estimate is computed in three steps. First, one computes the change over time (“after” minus “before”) in the treatment group. That first difference is the naïve estimate of the impact of the policy. It eliminates any persistent difference in the treatment group. Second, one computes the

change over time in the control group. This second difference gives an estimate of the pure time effect, which is implicitly assumed to be common across the experimental and control groups. Finally, subtract the change over time in the control group from the change over time in the treatment group, eliminating any pure time effects. This last number is the DoD estimate. It controls both for persistent differences across units (treatment/control, or in our case, States) and pure time effects (that are common to the treatment and control groups).

Our approach generalizes this simple DoD estimator to a regression framework (Meyer, 1995). Rather than truly differencing, we include dummy variables for each State and time period (see below for the exact specification of the time effects). Such dummy variables can be shown to be the functional equivalent of differencing. These dummy variables control for any time invariant variation across States and any nationwide variation across time periods. Including dummy variables in a regression allows us to consider multiple, non-binary policies, adopted at various times, by various States. Furthermore, the regression specification allows the inclusion of other controls and more robust estimates of the appropriate standard errors.

OUR SPECIFICATION FOR THE MEAN

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:

(IV.1) $$\log\left[y_{g,s,t}\right] = \frac{M_{g,s,t}}{N_{g,s,t}} = \alpha_g + X_{g,s,t}\beta_g + \mu_{g,s} + f\left[t,\theta_g\right] + \varepsilon_{g,s,t}$$

The g subscript indicates cash assistance receipt sub-groups, the s subscript denotes States, and the t subscript indexes time measured in months. Every term has a g subscript to emphasize that our estimates totally stratify by sub-group; in practice, these are the three sub-groups defined and discussed in Chapter 2. The coverage rate is defined as the ratio of participants, M,

to the population, N. In our specifications, we specify the log of this coverage rate as our dependent variable. Note that we are not modeling the participation rate. We therefore do not try to restrict the population to some estimate of eligibles.

In the equation, X represents the K forcing variables (e.g., FSP policies, welfare policies, and the economy) and β the corresponding K regression coefficients, μ is a vector of 51 State dummies (for the 50 States, plus the District of Columbia), f[t,θ] is our specification for time effects (see below), and ε is a residual.

In some models, we also include State-specific linear time trends. The econometric model then becomes:

$$\text{(IV.2)} \qquad \log\left[y_{g,s,t}\right] = \frac{M_{g,s,t}}{N_{g,s,t}} = \alpha_g + X_{g,s,t}\beta_g + \mu_{g,s} + \delta_{g,s}t + f\left[t,\theta_g\right] + \varepsilon_{g,s,t}$$

We note that there is some controversy about their inclusion in the literature. Wallace and Blank (1999) have argued that the time series are too short to estimate such linear time trends. Thus, the effect of their inclusion is to soak up true policy effects.

Consistent with earlier work, we consider both contemporaneous forcing variables (i.e., the policies and the economy), X and their lags (e.g., Wallace and Blank, 1999; Klerman and Haider, 2004; Danielson and Klerman, 2006, 2008; Ratcliffe, McKernan, and Finegold, 2008). 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 implausible:

1. Many policies require a bureaucracy for full implementation. Putting that bureaucracy into place is rarely instantaneous. For example, programs to enable ABAWDs to fulfill their work requirements 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, in order for the stigma of program participation to be reduced, potentially eligible populations must come to know about the replacement of paper coupons with the EBT card. See Wemmerus,

and Gottleib (1999) who find that changes in the vehicle asset test continued to affect the caseload for 18 months.

3. Klerman and Haider (2004) and Haider and Klerman (2004) show that if a policy affects the flows onto and off welfare, 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 (2006, 2008) 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 three years. Finally, we allow for a different linear effect in the fourth and following years. Both on a priori grounds and given past experience with this specification, we expect no immediate effect, an intermediate effect (i.e., the first through third years) that varies with the particular program, and no additional effect in the long-term (in the fourth and following years).

For the case of the economy, the previous literature has sometimes included lagged values, and those lagged values often substantially increased the estimated total effect of the economy (See CEA, 1999; Figlio, Gundersen, and Ziliak, 2000; Klerman and Haider, forthcoming; Ziliak, Figlio, Davis, and Connolly, 2000; Kornfeld 2002). In connection with the FSP caseload in particular, see Schoeni's (2001) comments on Currie and Grogger (2001). He argues 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 for each measure the moving average of the current and previous eleven month's values and the one-year lag of this average.

Our motivation for the DoD estimator implies time dummies for every period (i.e., months). Here, we take a more parsimonious specification. We estimate a national linear trend within each calendar year (January to the following January; requiring the trends to overlap) and then a seasonal effect for each calendar month (taking January as the excluded month). Thus, we estimate not Yx12 parameters, but Y+1+11 parameters (where Y is the number of federal fiscal years in the data).

Finally, we do not include lagged dependent variables. We note that much of the FSP (and welfare) literature does include such variables (e.g., Figlio,

Gundersen, and Ziliak, 2000; Ziliak, Gundersen, and Figlio, 2001). Our rich specification of the effect of earlier changes in policies and the economy should capture the same effects intended to be captured by lagged dependent variables. Furthermore, we have argued elsewhere (Klerman and Haider, 2004) that such models are likely to be mis-specified; in addition econometric issues lead the estimates to be inconsistent. Finally, we note that policy estimates tend to be severely muted in models that include lagged dependent variables (see the direct comparisons in Wilde et al., 2000). It appears that the lagged dependent variables are soaking up much of the true policy effect.[26]

OUR SPECIFICATION FOR THE RESIDUALS

Our dependent variable is the coverage rate for a group in a State-year. We include fixed effects for State and time. There should thus be no components of variance at the State or year level. Nevertheless, for two reasons, it seems unlikely that the data are independently and identically distributed (i.i.d.).

First, time-series data usually have autocorrelated residuals. Inasmuch as there are unobserved factors that affect the coverage rate, those unobserved factors are likely to be autocorrelated. That autocorrelation in the unobserved factors will induce autocorrelation in the residuals. We addressed this problem by estimating AR(12) models

Second, State populations vary widely. Some conventional econometric analyses therefore suggest weighting by the inverse of the State population. Alternatively, the FSPQC sample sizes and coverage rates vary. Some conventional econometric analyses would therefore suggest weighting by the sampling variability for a binomial variable (pq/N) or the equivalent expression when the dependent variable is the log of a binomial variable (see Maddala, 1983).

For the second reason, Dickens (1990) argues that this will be appropriate only if there are not important unmeasured variables at the State-year level. On a priori grounds such unmeasured variables seem likely and

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[26] We note that Bell (2001) has a slightly different reading of this literature; with which we disagree.

Dickens' analyses suggest that weighting usually induces more error into the estimated standard errors (and in the parameters). Following Dickens we do not weight the data. Instead, we equally weigh each State-year observation.

Nevertheless, some component of variance due to FSPQC sample size and the coverage rate, and generic heteroscedasticity also seems likely. A super-population perspective suggests that, inasmuch as populations vary in size, the variance of the residual will vary inversely. 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.

This approach includes no formal controls for heterogeneity; e.g., variation in income or race/ethnicity across States and years. Given that we include State fixed effects and that any such drift in heterogeneity is likely to be small, the lack of formal controls for heterogeneity is unlikely to induce significant bias.

It is also possible that the possibility that the individual terms are heteroscedastic due to the varying State population, sampling rates, weighted sampling, and different numbers of reviews. The FSPQC sample sizes are chosen to assure a minimum level of precision in the resulting estimated error rates. It follows that the sampling variance of the estimate of the coverage rate is approximately constant across States and time. Thus, heteroscedasticity due to sampling variability is unlikely to have a substantial effect on the computed standard errors or the parameter estimates. We correct for any such heteroscedasticity by using robust (i.e., Huber-Eicker-White) standard errors (as calculated by Stata).

V. RESULTS

Having described our data, the forcing variables, and our econometric methods, in this chapter we present our results. The first section discusses our basic results. The second section considers results of some alternative specifications and their implications for the interpretation of our basic results. The third section considers the expanded sets of covariates. The fourth second considers models for FSP benefits paid. The fifth section develops the implications of our parameter estimates via a series of simulations. A final section presents some summary comments about the modeling results. We defer our summary of the substance until the next chapter.

BASE MODEL ESTIMATES

Table 5.1 presents the results for our basic specification, which we refer to below as the "Base Model". The dependent variable is the log of cases per capita. Thus, the parameter estimates can be interpreted as percentage changes in the caseload. We estimate the models totally stratified by TANF (and no SSI); SSI (possibly also TANF), and no TANF and no SSI (labeled "NPA" for non-public assistance". Results for these three sub-groups are reported in the three columns.

All estimates were generated using the Stata xtgee command. We report the parameter estimates, standard errors, and conventional indicators for statistical significance ("*" for 5 percent; "**" for 1 percent). The standard errors are computed using an AR(12) specification to control for autocorrelation in our monthly data. In addition, to make our standard errors robust to heteroscedasticity and other forms of serial correlation we use robust (i.e., Huber-Eicker-White) standard errors; and, following Bertrand, Duflo, and Mullianathan (2004), we also compute the standard errors clustering on state (both are implied by Stata's "robust" option in xtgee).

These results implicitly make two decisions about the estimation sample: (i) they exclude California and (ii) they include Hispanics. In the next section, we explain these choices and explore the sensitivity of our results to them.

Table 5.1
Results for Basic Specification

	TANF	SSI	NPA
unemp_0	0.034	-0.015	0.019
	(0.029)	(0.023)	(0.012)
l_temp_0	-1.689	-1.169	-1.764
	(1.300)	(0.916)	(0.471)**
l_Rwpw_0	0.141	-0.333	-0.345
	(0.423)	(0.273)	(0.190)
cert_short	-0.516	0.424	-0.197
	(0.407)	(0.325)	(0.047)**
SR_imp	-0.174	0.088	0.073
	(0.115)	(0.055)	(0.020)**
EBT_pct	-0.014	-0.006	0.027
	(0.053)	(0.043)	(0.017)
outreach_pc	0.001	-0.016	0.003
	(0.007)	(0.011)	(0.005)
tanf	-0.101	-0.176	-0.031
	(0.051)*	(0.050)**	(0.023)
waiv	-0.026	-0.005	-0.003
	(0.032)	(0.031)	(0.018)
l_Rmin_wage_0	-0.496	-0.269	0.134
	(0.390)	(0.311)	(0.168)

Note: Dependent variable is log cases per capita.
All models include dummy variables for state and calendar month, and fiscal year splines (not reported).
8450 observations (50 states x 169 months; excluding California).
Standard errors in parentheses; computed using AR(12) specification, clustering on state, and robust standard errors.
* significant at 5%; ** significant at 1%.

The first three rows give the effects of contemporary economic conditions: the unemployment rate, the log of total employment, and the log of real wages per worker, respectively. To control for seasonality, each of these variables is measured as the average over the current month and the previous 11 months. The next four rows give the effects of our FSP policies: short certification periods, simplified report, adoption of EBT, and outreach expenditures, respectively. The final three rows give proxies for welfare reform (a dummy for any AFDC waiver, a dummy for TANF implementation), and the log of the state minimum wage. The source and exact definitions of each of the variables are given in Appendix B.

Having described the basic structure of the tables, we turn to the substance. The results are striking and confirm the utility of

stratifying by cash receipt status. The only thing that significantly affects either the TANF caseload or the SSI caseload is TANF implementation. That effect is negative and large—about 10 percent for the FSP TANF caseload and 18 percent for the FSP SSI caseload.

Consistent with our earlier theoretical discussion, the expected effect of TANF implementation on TANF is clear. TANF clearly limited eligibility and increased burden. We expect to find and do find that TANF lowers the FSP TANF caseload.

The expected effect of TANF on SSI is less clear. On the one hand, TANF might be expected to depress the FSP SSI caseload because the PRWORA legislation that created TANF also limited eligibility for SSI (e.g., some immigrants, some with alcohol or drug addiction). On the other hand, PRWORA gave states an even stronger incentive to move cases from TANF to SSI. With PRWORA, TANF benefits went from a federal-state split to a block grant, while the federal government continued to pay all SSI benefits. This might have led us to expect an increase in the SSI caseload. These results suggest that the first effect dominated.

Results for most of the economic variables have the expected, counter-cyclical, sign, but are not close to statistical significance (the estimate for unemployment for SSI has the “wrong sign”, but note that it is less than one standard deviation from zero). There is no evidence of any FPS policy effects. This is consistent with our conjecture that TANF and SSI participation is determined by the even more onerous rules for participation in those programs, rather than FSP rules.

In contrast, several of our regressors shift the NPA caseload. As expected, the non-cash assistance is counter-cyclical. Our specification includes three proxies for the economy. All three proxies suggest that the FSP caseload is counter-cyclical; i.e., the FSP caseload increases when the unemployment rate rises and when either total employment or real wages fall. Unlike previous studies, our results are significantly different from zero at conventional significance levels, only for the log of total employment ($p<0.01$). However, we note that the point estimates for the unemployment and real wages are also close to conventional statistical significance ($p=0.11$ and $p=0.07$).

Furthermore, the coefficients are substantively large. The variable is specified in logs, so the parameter is interpreted as an elasticity. Thus, a one percent decrease in total employment is estimated to increase the FSP NPA caseload by 1.8 percent. The coefficient for the unemployment rate is also large. A one percentage point increase in the unemployment rate is estimated to increase the FSP NPA caseload by nearly 2 percent. Finally, a one percent decrease in real wages is estimated to increase the FSP NPA caseload by about a third of a percentage point.

Interpreting the effects of these substantively related and statistically correlated is difficult. Below, we present results of simulation estimates that help us to understand the total effects and their ability to explain the observed path of the caseload.

Similarly, lowering the burden of participation in the FSP NPA increases the caseload. Specifically, a ten percent change in the fraction of the population subject to short certification periods (less than 3 months) pushes down the caseload by about 2 percent. Over time, the national fraction of the caseload subject to short-certification periods varies by about 30 percentage points, so this might explain about 6 percent of a caseload increase or decrease. In addition, a State's adopting Simplified Reporting pushes up the caseload by about 7 percent. There is no statistically significant effect of EBT adoption or outreach expenditures.

SAMPLE INCLUSION DECISIONS

As noted earlier, this Base Model specification makes two sample inclusion decisions. In this section, we explain those choices and explore the sensitivity of our estimates to them.

Our Base Model excludes California. Since 1974, California has "cashed out" Food Stamps for its SSI population; i.e., rather than getting Food Stamps according to the FSP formula, all California SSI recipients receive a fixed increment to their SSI payment, regardless of their other income. Over this period, that increment was approximately $10. This SSI FSP cash-out implies that we would expect the California SSI caseload to react differently to our forcing variables. In as much

as there is substitution between the SSI, TANF, and NPA caseloads, we might expect some effects on the other two categories.

Table 5.2 presents results both for our Base Model and for a model that includes California. As might be expected given that California is only one "State" among 51, the estimated coefficients barely change. Patterns of statistical significance are unchanged. Limited inspection of other results suggests that this qualitative finding is not limited to this particular model specification. We conclude that our results are not sensitive to the exclusion of California. Excluding California gives purer estimates. Therefore, in the balance of the paper, we report results excluding California.

Table 5.2
Sensitivity to the Inclusion of California

	TANF		SSI		NPA	
California	Excluded	Included	Excluded	Included	Excluded	Included
unemp_0	0.034	0.017	-0.015	-0.021	0.019	0.024
	(0.029)	(0.034)	(0.023)	(0.022)	(0.012)	(0.013)
l_temp_0	-1.689	-1.759	-1.169	0.247	-1.764	-1.714
	(1.300)	(1.314)	(0.916)	(1.532)	(0.471)**	(0.455)**
l_Rwpw_0	0.141	0.310	-0.333	-0.601	-0.345	-0.380
	(0.423)	(0.558)	(0.273)	(0.368)	(0.190)	(0.205)
cert_short	-0.516	-0.533	0.424	0.359	-0.197	-0.192
	(0.407)	(0.466)	(0.325)	(0.352)	(0.047)**	(0.047)**
SR_imp	-0.174	-0.186	0.088	0.091	0.073	0.075
	(0.115)	(0.110)	(0.055)	(0.046)	(0.020)**	(0.020)**
EBT_pct	-0.014	-0.029	-0.006	0.045	0.027	0.037
	(0.053)	(0.063)	(0.043)	(0.062)	(0.017)	(0.019)
outreach_pc	0.001	0.007	-0.016	-0.004	0.003	0.004
	(0.007)	(0.008)	(0.011)	(0.017)	(0.005)	(0.005)
tanf	-0.101	-0.097	-0.176	-0.205	-0.031	-0.028
	(0.051)*	(0.050)	(0.050)**	(0.054)**	(0.023)	(0.023)
waiv	-0.026	-0.030	-0.005	-0.034	-0.003	0.007
	(0.032)	(0.034)	(0.031)	(0.034)	(0.018)	(0.020)
l_Rmin_wage_0	-0.496	-0.491	-0.269	-0.621	0.134	-0.017
	(0.390)	(0.393)	(0.311)	(0.542)	(0.168)	(0.213)

Note: Dependent variable is log cases per capita.
All models include dummy variables for state and calendar month, and fiscal year splines (not reported).
Base Model: 8450 observations (50 states x 169 months; excluding California).
Results w/California: 8619 observations (51 states x 169 months).
Standard errors in parentheses; computed using AR(12) specification, clustering on state, and robust standard errors.
* significant at 5%; ** significant at 1%.

Our Base Model includes Hispanics. The 1996 welfare reform (PRWORA/Personal Responsibility and Work Opportunities Reconciliation

Act) substantially shrunk eligibility for the FSP among legal immigrants. Later legislation reversed some of those changes.

Our difference-of-differences approach cannot directly estimate the effects of those national changes in eligibility. We note, however, that the overwhelming fraction of affected legal immigrants are Hispanics. As a rough proxy for the effects of these immigrant provisions, we re-ran our Base Model excluding Hispanics both from the caseload and from the population counts.

Table 5.3 presents those results as well as our base case results. Again, the results are relatively robust. For FSP NPA cases, the effect of the unemployment rate increases slightly, so that it is now significant at p=0.05; the effect of total employment decreases slightly (in absolute value), with no effect on the level of statistical significance. The expected effects of excluding Hispanics are on the TANF coefficient. There is essentially no change in this coefficient in the FSP TANF and FSP SSI caseloads. There is a small decrease (in absolute value) in the FSP NPA caseload. This is the expected direction of effect, but the coefficient is far from statistical significance in either specification. We conclude that the effects we find are not being driven by PRWORA's immigrant provisions.

RICHER SPECIFICATION OF POLICIES AND THE ECONOMY

As we discussed in Chapter 3, our earlier theoretical and empirical work and other empirical work on the FSP caseload has found some lagged effects of policy and the economy. Earlier theoretical and empirical work suggests that the TANF and waiver dummy variables are poor summaries of the interstate and over-time variation in AFDC and TANF policy. A richer model would include proxies for the details of state AFDC and TANF policy.

Table 5.4 reports results for richer specification that includes lagged variables of each of the policy and economic variables (as well as our Base Model). Specifically, for each of our three sub-groups (TANF/SSI/NPA), we present four models: (A) our Base Model; (B) a model with lags of the economic variables; (C) a model with lags of the economic variables and the policy variables; and (D) a model with specific welfare policies and their lags.

Table 5.3
Sensitivity to the Exclusion of Hispanics

	TANF		SSI		NPA	
Hispanics	Included	Excluded	Included	Excluded	Included	Excluded
unemp_0	0.034	0.047	-0.015	-0.010	0.019	0.024
	(0.029)	(0.025)	(0.023)	(0.023)	(0.012)	(0.012)*
l_temp_0	-1.689	-1.630	-1.169	-1.269	-1.764	-1.715
	(1.300)	(1.417)	(0.916)	(0.935)	(0.471)**	(0.505)**
l_Rwpw_0	0.141	0.226	-0.333	-0.340	-0.345	-0.352
	(0.423)	(0.484)	(0.273)	(0.273)	(0.190)	(0.216)
cert_short	-0.516	-0.524	0.424	0.426	-0.197	-0.195
	(0.407)	(0.423)	(0.325)	(0.292)	(0.047)**	(0.050)**
SR_imp	-0.174	-0.176	0.088	0.063	0.073	0.071
	(0.115)	(0.118)	(0.055)	(0.054)	(0.020)**	(0.022)**
EBT_pct	-0.014	-0.019	-0.006	-0.016	0.027	0.027
	(0.053)	(0.055)	(0.043)	(0.043)	(0.017)	(0.017)
Outreach_pc	0.001	0.004	-0.016	-0.016	0.003	0.003
	(0.007)	(0.007)	(0.011)	(0.011)	(0.005)	(0.005)
Tanf	-0.101	-0.107	-0.176	-0.181	-0.031	-0.022
	(0.051)*	(0.053)*	(0.050)**	(0.047)**	(0.023)	(0.023)
Waiv	-0.026	-0.025	-0.005	-0.000	-0.003	0.000
	(0.032)	(0.033)	(0.031)	(0.028)	(0.018)	(0.019)
l_Rmin_wage_0	-0.496	-0.568	-0.269	-0.310	0.134	0.207
	(0.390)	(0.393)	(0.311)	(0.310)	(0.168)	(0.176)

Note: Dependent variable is log cases per capita.
All models include dummy variables for state and calendar month, and fiscal year splines (not reported).
Base Model: 8450 observations (50 states x 169 months; excluding California).
Standard errors in parentheses; computed using AR(12) specification, clustering on state, and robust standard errors.
* significant at 5%; ** significant at 1%.

Consider first the results with lags of the economic variables (i.e., Model B vs. Model A). Specifically, the contemporaneous effect is the average of the current month and the eleven previous months. The lagged effect is the effect of the previous twelve months. One can thus think about these results are “the last year” and “the year before that”. With one exception, our earlier finding of no economic effects on the FSP TANF and FSP SSI caseloads continues to hold. The exception is the lagged value of wages per worker in the SSI caseload. The point estimate is as expected counter-cyclical (i.e., negative; lower wages cause higher FSP caseloads) and statistically significant at p=0.05.

Turning now to the FSP NPA caseload where our Base Model did have statistically significant economic effects, we find a richer story. The unemployment rate whose contemporaneous effect is not quite significant in the Base Model has a strongly statistically significant lagged

effect. Furthermore, computing a “long run effect” as the sum of all of the terms, we see a sharp increase from 0.019 to 0.038 (=0.009+0.029). Thus, a two-year one percentage point increase in the unemployment rate is estimated to increase the NPA FSP caseload by 3.8 percent. Given that we observe swings in the unemployment rate of over 3 percent in our data, this is a potentially large effect.

Table 5.4
Sensitivity to the Inclusion of Lags of the Forcing Variables

	TANF				SSI				NPA			
Model	A	B	C	D	A	B	C	D	A	B	C	D
unemp_0	0.034	0.040	0.029	0.028	-0.015	-0.064	-0.064	-0.064	0.019	0.009	0.005	0.005
	(0.029)	(0.026)	(0.028)	(0.031)	(0.023)	(0.041)	(0.039)	(0.040)	(0.012)	(0.011)	(0.012)	(0.011)
unemp_1		-0.024	-0.027	-0.014		0.101	0.093	0.101		0.029	0.029	0.028
		(0.041)	(0.041)	(0.039)		(0.058)	(0.056)	(0.059)		(0.010)**	(0.010)**	(0.010)**
l_temp_0	-1.689	-1.988	-1.453	-1.612	-1.169	-1.012	-1.053	-0.923	-1.764	-1.542	-1.420	-1.189
	(1.300)	(1.301)	(1.236)	(1.342)	(0.916)	(0.950)	(0.923)	(0.979)	(0.471)**	(0.447)**	(0.415)**	(0.381)**
l_temp_1		0.019	0.088	0.194		0.814	0.796	1.134		0.121	0.176	0.189
		(1.207)	(1.184)	(1.055)		(1.453)	(1.414)	(1.498)		(0.294)	(0.295)	(0.278)
l_Rwpw_0	0.141	0.155	0.496	0.539	-0.333	-0.312	-0.105	-0.108	-0.345	-0.405	-0.392	-0.474
	(0.423)	(0.531)	(0.561)	(0.502)	(0.273)	(0.192)	(0.252)	(0.242)	(0.190)	(0.187)*	(0.200)	(0.207)*
l_Rwpw_1		0.000	0.359	0.364		-0.728	-0.513	-0.540		-0.276	-0.268	-0.365
		(0.469)	(0.506)	(0.511)		(0.303)*	(0.269)	(0.256)*		(0.189)	(0.209)	(0.218)
cert_short	-0.516	-0.515	-0.500	-0.505	0.424	0.449	0.434	0.492	-0.197	-0.194	-0.170	-0.152
	(0.407)	(0.412)	(0.400)	(0.382)	(0.325)	(0.328)	(0.311)	(0.273)	(0.047)**	(0.048)**	(0.047)**	(0.039)**
SR_imp	-0.174	-0.173	-0.199	-0.196	0.088	0.087	0.095	0.088	0.073	0.071	0.055	0.052
	(0.115)	(0.115)	(0.119)	(0.119)	(0.055)	(0.054)	(0.058)	(0.059)	(0.020)**	(0.019)**	(0.020)**	(0.019)**
SRslp			0.004	0.004			-0.001	-0.001			0.003	0.002
			(0.005)	(0.005)			(0.002)	(0.003)			(0.001)	(0.001)
SRslp3			-0.180	-0.190			0.022	0.035			-0.000	0.009
			(0.021)**	(0.036)**			(0.021)	(0.023)			(0.004)	(0.007)
EBT_pct	-0.014	-0.015	-0.005	-0.008	-0.006	0.002	0.026	0.022	0.027	0.030	0.030	0.025
	(0.053)	(0.054)	(0.051)	(0.046)	(0.043)	(0.042)	(0.044)	(0.046)	(0.017)	(0.016)	(0.018)	(0.019)
EBTslp			-0.005	-0.003			-0.002	-0.003			0.001	0.001
			(0.005)	(0.004)			(0.002)	(0.002)			(0.001)	(0.001)
EBTslp3			-0.009	-0.002			-0.002	-0.002			-0.000	-0.000
			(0.005)	(0.005)			(0.002)	(0.002)			(0.001)	(0.001)
outreach_pc	0.001	0.002	-0.001	-0.002	-0.016	-0.016	-0.017	-0.016	0.003	0.003	0.002	0.002
	(0.007)	(0.007)	(0.007)	(0.006)	(0.011)	(0.011)	(0.011)	(0.011)	(0.005)	(0.005)	(0.005)	(0.005)
tanf	-0.101	-0.099	-0.075	-0.033	-0.176	-0.176	-0.185	-0.182	-0.031	-0.033	-0.031	-0.027
	(0.051)*	(0.051)	(0.059)	(0.041)	(0.050)**	(0.049)**	(0.054)**	(0.069)**	(0.023)	(0.023)	(0.024)	(0.025)
tanfslp			-0.025	-0.017			-0.011	-0.011			-0.003	-0.002
			(0.004)**	(0.007)*			(0.004)*	(0.006)			(0.002)	(0.002)
tanfslp3			0.001	0.002			-0.004	-0.003			0.003	0.003
			(0.006)	(0.007)			(0.003)	(0.003)			(0.001)*	(0.002)
waiv	-0.026	-0.026	-0.028	-0.016	-0.005	-0.002	0.020	0.017	-0.003	-0.002	0.009	0.008
	(0.032)	(0.032)	(0.029)	(0.036)	(0.031)	(0.032)	(0.041)	(0.046)	(0.018)	(0.017)	(0.023)	(0.024)
waivslp			-0.001	-0.000			-0.004	-0.003			-0.001	-0.000
			(0.002)	(0.002)			(0.002)	(0.002)			(0.001)	(0.001)
l_Rmin_wage_0	-0.496	-0.522	-0.087	0.018	-0.269	-0.094	0.115	0.014	0.134	0.187	0.301	0.313
	(0.390)	(0.387)	(0.349)	(0.391)	(0.311)	(0.315)	(0.317)	(0.382)	(0.168)	(0.171)	(0.179)	(0.167)
div				-0.059				-0.048				0.032
				(0.064)				(0.050)				(0.027)
divslp				-0.003				0.004				0.002

	(0.005)	(0.003)	(0.001)
divslp3	-0.008	-0.001	-0.001
	(0.005)	(0.001)	(0.001)
brr_13p20w	0.005	0.022	0.019
	(0.053)	(0.068)	(0.021)
brr_13p20wslp	0.002	0.006	0.001
	(0.010)	(0.003)	(0.002)
brr_13p20wslp3	-0.003	-0.001	0.000
	(0.008)	(0.001)	(0.001)
sanc_adXff	0.059	0.096	0.025
	(0.046)	(0.094)	(0.025)
sanc_adXffslp	-0.003	-0.000	-0.000
	(0.004)	(0.004)	(0.002)
sanc_adXffslp3	-0.009	0.003	0.002
	(0.004)*	(0.002)	(0.001)*
sanc_ffXff	-0.212	0.015	-0.043
	(0.167)	(0.059)	(0.030)
sanc_ffXffslp	-0.021	0.001	-0.002
	(0.012)	(0.002)	(0.002)
sanc_ffXffslp3	0.009	0.001	0.001
	(0.006)	(0.002)	(0.001)
tl_ad	0.013	-0.009	-0.128
	(0.060)	(0.056)	(0.098)
tl_ad_slp	0.000	-0.000	-0.001
	(0.001)	(0.001)	(0.001)
tl_hitXad	0.031	-0.260	-0.280
	(0.161)	(0.173)	(0.095)**
tl_hitXad_slp	0.001	0.001	0.001
	(0.001)	(0.001)	(0.000)**
tl_ff	-0.054	-0.024	-0.005
	(0.029)	(0.044)	(0.020)
tl_ff_slp	-0.002	-0.001	0.000
	(0.002)	(0.001)	(0.000)
tl_hitXff	-0.255	-0.071	0.002
	(0.227)	(0.059)	(0.044)
tl_hitXff_slp	0.001	-0.000	0.000
	(0.001)	(0.000)	(0.000)
l_Rmaxben_0	0.175	-0.067	-0.220
	(0.551)	(0.249)	(0.125)

Note: Dependent variable is log cases per capita.
All models include dummy variables for state and calendar month, and fiscal year splines (not reported).
Base Model: 8450 observations (50 states x 169 months; excluding California).
Standard errors in parentheses; computed using AR(12) specification, clustering on state, and robust standard errors.
* significant at 5%; ** significant at 1%.

Shifts in the other economic variables are less dramatic. In our base specification, (the log of) total employment was the only economic variable that was different from zero at conventional statistical levels. In this enriched specification, its lag is not statistically significant. Furthermore, the long-run effect drops slightly from –1.764 to -1.421; i.e. a one percent decrease in employment increase the FSP NPA caseload by 1.4 percent.

With respect to wages per worker, in the Base Model it was borderline significant. With the addition of a lag, the main effect is significant at the 5 percent level. Furthermore, the long-run effect grows from –0.345 to –0.681; i.e., a one percent decline in wages increases the FSP NPA caseload by 0.7 percent.

Finally, we note that, each of these economic results is relatively stable as we enrich the specification of the policy variables (i.e., Model C and Model D).

To Model B that includes lagged economic effects, Model C adds two lags of some of the policy variables. Specifically, building on the approach of Danielson and Klerman (2008), the first lag of the policy variables is a linear change in the policy over the first three years since implementation; while the second lag is the linear change in the policy over the second three years since implementation. (See Appendix C for the exact specification.) Consider first the FSP policies and the FSP NPA caseload results where the Base Model found statistically significant effects of short certification periods and Simplified Reporting. There is no evidence of lagged effects for any of the variables. If anything, the long run effects shrink slightly. Turning to the FSP TANF and FSP SSI caseloads where previously there was no effect, we note that anomalous result of a large and statistically significant effect of Simplified Reporting at the second lag. This lagged effect is in addition to a large, but not statistically significant, contemporaneous effect. This lagged effect appears to be due to small samples. This term models the change in the effect of Simplified Reporting for implementations four to six years after implementation. We have very few observations of Simplified Reporting after it was in place for more than three years. We therefore believe that this result is spurious.

Turning now to the welfare variables, there are small changes with the inclusion of lags (Model C). For the FSP TANF caseload, the significant TANF

term is now the first lag, but the long-run effect is nearly unchanged. For the FSP SSI caseload, there is a small, but statistically significant effect at one lag, and the long-run effect grows slightly. For the FSP NPA caseload, there is a small, but statistically significant, effect at two lags. With respect to waivers, all of the effects continue to be statistically insignificant.

Finally, Model D includes proxies for individual TANF policies. The included policies are presence of a welfare diversion program ("div"), a generous benefit reduction rate ("brr_12p20w", coded as one if a family of three remains eligible for any benefit, after on welfare for more than a year, while working 30 hours per week at the state's 20th percentile wage), sanction policy (gradual sanction, progressing to a full family sanction—"sanc_adXff"—and immediate, full family sanction—"sanc_ffXff"), time limit policy ("tl": clock ticks in this month for adults only–"tl_ad"; adults could have reached the time limit in this month–"tl_hitXad"; and the corresponding variables for a full family time limit "tl_ff" and "tl_hitXff"), and the log of welfare benefit for a family of three ("l_Rmaxben"). These policy variables are coded as a dummy variable for the policy in place, a linear slope with time since implementation, and a linear spline beginning 3 years after implementation (with no suffix, a "_slp", and a "slp3" suffix, respectively).

Inspection of these results shows no clear pattern. A priori, we would most expect to find an effect for the FSP TANF caseload and we did find such effect using DHHS-ACF TANF caseload data (Danielson and Klerman, 2008). Nevertheless, the only statistically significant effect is a small effect for the second lag of a gradual sanction. There are no statistically significant effects of the SSI caseload. Surprisingly, there are slightly more statistically significant effects for the FSP NPA caseload. Again, there is a small effect of the second lag of a gradual sanction. In addition, there is a moderate effect of reaching an adult only time limit. None of these effects are large and the pattern of effects does not seem to be easily interpretable. We conclude either that these data will not support disaggregated analyses of policies or that there are no such effects (above and beyond the FSP TANF and waiver variables). The former interpretation seems more plausible.

BENEFIT PAYMENTS PER CAPITA

Previous analyses have focused on the caseload. Survey reports of benefits received are notoriously imprecise. In contrast, our FSPQC data has high quality information on benefit amounts. Table 5.5 reports estimates for our Base Model, where instead of taking the dependent variable to be the log of the caseload per capita, we take the dependent variable to be the log of benefit payments per capita (labeled "Model E"). For comparison, we also include in the table results for our base model, with the log of the caseload per capita as the dependent variable.

Table 5.5
Log of Benefit Payments Per Capita (vs. Log of Caseload per Capita)

	TANF		SSI		NPA	
DV	Caseload	Benefit	Caseload	Benefit	Caseload	Benefit
unemp_0	0.034	0.018	-0.015	-0.042	0.019	0.006
	(0.029)	(0.045)	(0.023)	(0.040)	(0.012)	(0.017)
l_temp_0	-1.689	-2.033	-1.169	-1.917	-1.764	-2.259
	(1.300)	(1.753)	(0.916)	(1.563)	(0.471)**	(0.579)**
l_Rwpw_0	0.141	0.405	-0.333	-0.517	-0.345	-0.494
	(0.423)	(0.589)	(0.273)	(0.346)	(0.190)	(0.207)*
cert_short	-0.516	-0.768	0.424	0.979	-0.197	-0.184
	(0.407)	(0.627)	(0.325)	(0.546)	(0.047)**	(0.067)**
SR_imp	-0.174	-0.233	0.088	0.191	0.073	0.095
	(0.115)	(0.167)	(0.055)	(0.101)	(0.020)**	(0.028)**
EBT_pct	-0.014	-0.026	-0.006	-0.004	0.027	0.034
	(0.053)	(0.068)	(0.043)	(0.065)	(0.017)	(0.024)
outreach_pc	0.001	0.003	-0.016	-0.033	0.003	0.001
	(0.007)	(0.009)	(0.011)	(0.012)**	(0.005)	(0.009)
tanf	-0.101	-0.113	-0.176	-0.324	-0.031	-0.049
	(0.051)*	(0.064)	(0.050)**	(0.116)**	(0.023)	(0.046)
waiv	-0.026	-0.028	-0.005	-0.029	-0.003	-0.036
	(0.032)	(0.041)	(0.031)	(0.055)	(0.018)	(0.029)
l_Rmin_wage_0	-0.496	-0.257	-0.269	-0.229	0.134	0.288
	(0.390)	(0.544)	(0.311)	(0.507)	(0.168)	(0.281)

Note: Dependent variable is log cases per capita or log of benefit per capita.
All models include dummy variables for state and calendar month, and fiscal year splines (not reported).
Base Model: 8450 observations (50 states x 169 months; excluding California).
Standard errors in parentheses; computed using AR(12) specification, clustering on state, and robust standard errors.
* significant at 5%; ** significant at 1%.

In Table 5.5, the pattern of significance is similar between the caseload models and the benefit models. In the FSP TANF model for the caseload, the only significant term was for TANF implementation. In the benefit model, the point estimate increases slightly, but the standard error increases even more.

In net, the parameter is only borderline significant (p=0.08). In the SSI model for the caseload, again the only significant term was TANF implementation. In the model for the FSP benefit amount, that term continues to be significant (and nearly twice as large). In addition, there is an anomalous effect of outreach expenditures per capita; they lower the FSP SSI caseload. We have no explanation for this result.

Finally, we turn to the FSP NPA caseload results. In the model for the caseload, there were significant effect of total employment, short certification periods, and Simplified Reporting. That pattern of significance carries over to FSP benefit amount. The only change is that the magnitude of the unemployment effect declines (from borderline significant to clearly insignificant) and the magnitude of the total employment effect and the wages per worker also rise moderately.

We conclude that FSP benefit expenses move together with the caseload. This is not surprising. We had, however, expected that economic effects would be larger for FSP benefits than for the FSP caseload: Changes that make more people eligible would also be expected to decrease the earnings of those already eligible further increasing benefits. We find little evidence of any such effect.

SIMULATION RESULTS

The previous sections have presented results for a range of models for the components of the FSP caseload (and for FSP benefits paid). Our discussion has considered various ways to interpret the magnitude of the effects. Despite our efforts, it remains difficult to interpret the results—especially given the substantive and statistical inter-relation of the variables. In addition, our initial interest in these models was partially spurred by our desire to understand the wide swings in the FSP caseload over the last decade and a half. The parameter estimates reported in the previous sections are not directly informative about the causes of the swings in the caseload.

To address each of these concerns and thereby to improve our understanding of the implications of our parameter estimates, in this section, we report results of simulations based on our parameter estimates. Specifically, we summarize the effects of the included policies and the

economy on each of the three caseloads analyzed in this report by means a set of counterfactuals:

1. What would the FSP TANF, FSP SSI, and FSP NPA caseloads have been if no state had three-month certification periods for any case?
2. What would these caseloads have been if no state had spent funds on FSP outreach, or had implemented EBT and simplified reporting?
3. What would these caseloads have been if no AFDC waivers had been approved and the legislation creating the TANF program had never been passed?
4. And finally, what would these caseloads have been if the economy had never boomed in the mid- and late-1990s or stagnated in the early 2000s?

We simulate these counterfactuals using our Base Model (i.e., Table 5.1) and then for our model with lags of the economic variables (i.e., Table 5.4, Model B). Table 5.6 presents the results of this simulation exercise.

Interpreting the table requires considerable explanation. Consider first the top panel. It presents results from the Base Model for the period FFY94-FFY01 using the Base Model. This is the period of the sharp decrease in the caseload. Reading across the top row (labeled “Actual”), we see that the FSP TANF caseload declined 64 percent, the FSP SSI caseload declined 6 percent, and the FSP NPA caseload declined 20 percent.

Now consider the two columns labeled “NPA”. If we impose no change in the economy from February 1992 forward, our model suggests that the FSP NPA caseload would only have declined by 2 percentage points (vs. the 20 percentage points it actually declined). We interpret this simulation to imply that the robust economic expansion in the 1990s alone explain 18 percentage points (=20-2) of the decline. The right panel reports that these 18 percentage points are 88 percent (=18/20, before rounding) of the observed decline; i.e., the three economic variables together explain almost all of the decline in the FSP NPA caseload in this period.

Considering the other “Concepts”, imposing no Short Certifications during this period implies a decline 5 percentage points less than actual; which is 24 percent of the observed decline. Adoption of Simplified Reporting (combined with the other FSP policy changes—EBT and outreach expenditures) actually pushed the caseload up 2 percentage points. Finally, the adoption of TANF pushed the FSP NPA caseload down 2 percentage points.

Table 5.6
Log of Benefit Payments Per Capita (vs. Log of Caseload per Capita)

Period	Model	Concept	Simulated Caseload Change			Percent Explained		
			TANF	SSI	NPA	TANF	SSI	NPA
FFY94-FFY01	Base	Actual	-64%	-6%	-20%			
		Short Cert.	-61%	-9%	-15%	5%	-44%	24%
		Simp. Rep.	-63%	-6%	-22%	1%	1%	-11%
		TANF	-60%	12%	-18%	6%	289%	13%
		Economy	-56%	2%	-2%	13%	136%	88%
		Total				25%	383%	114%
	Econ Lags	Actual	-64%	-6%	-20%			
		Short Cert.	-61%	-9%	-15%	5%	-46%	23%
		Simp. Rep.	-63%	-7%	-23%	1%	-10%	-12%
		TANF	-60%	12%	-17%	6%	289%	13%
		Economy	-57%	21%	5%	10%	440%	123%
		Total				22%	673%	148%
FFY01-FFY04	Base	Actual	-7%	16%	68%			
		Short Cert.	-13%	19%	58%	-85%	-20%	14%
		Simp. Rep.	8%	8%	58%	222%	48%	15%
		TANF	-7%	16%	68%	0%	0%	0%
		Economy	-19%	13%	54%	-194%	18%	20%
		Total				-57%	46%	49%
	Econ Lags	Actual	-7%	16%	68%			
		Short Cert.	-13%	19%	58%	-94%	-21%	14%
		Simp. Rep.	8%	8%	58%	220%	48%	15%
		TANF	-7%	16%	68%	0%	0%	0%
		Economy	-18%	8%	51%	-168%	52%	25%
		Total				-42%	78%	53%

Note: Simulations using parameters from Base Model (Table 5.1=Table 5.4 Model A) and Econ Lags Model (Table 5.4 Model B).
"Simulated Decline" gives the implied decline in the caseload holding this "Concept" at its value at the start of the period.
"Percent Explained" is the percent of the total decline explained by this "Concept".
A "Concept" is a group of variables. "Simp. Rep." Includes the effect of Simplified Reporting, EBT, and outreach.
"Total" sums all of the estimated effects.
Effects of the economy are based on periods defined by the turning points in the unemployment rate (rather than the turning points in the caseload; specifically starting in 2/1992 and 10/2000).

The last row of this block sums the percentages explained. Thus, the regressors included in our model explain 114 percent of the observed decline; i.e., even ignoring the year fixed effects, our model slightly over-explains the decline. We consider this to be a success of the model.

The next panel presents results for the same period, using the richer model with lags of the economic variables. With more economic variables, the economy alone over-explains the decline (123 percent). As expected, the other effects are nearly unchanged. With the other variables, the explained variation is 148 percent of the observed variation (i.e., half again too large). We defer our discussion of this over-explanation until we discuss the simulation results for the models for FSP TANF caseloads over this same period.

The models for the FSP TANF caseload over this period are much less successful at explaining the decline. The observed decline is even larger—64 percent. However, TANF explains only 4 percentage points of that decline. The economy explains only 8 percentage points of that decline. All together we explain less than a quarter of the decline (22 percent). The models with lagged economic variables actually explain less of the decline. That we cannot explain the decline in the TANF caseload is a relatively standard result in the TANF literature. Observed variables clearly under-explain the TANF caseload decline. TANF appears to have involved some other changes that are not well proxied by our simple dummy for TANF implementation. Even in richer models that succeed in estimating plausible impacts of individual TANF policies (e.g., Danielson and Klerman, 2008), most of this FSP TANF caseload decline remains unexplained. Klerman and Haider (2004) appear to find larger effects of the economy for California data using hazard models. They explain the difference in terms of the lag structure induced by entry and exit. The results here are not consistent with that explanation.

Such unmeasured dimensions of welfare reform are a likely explanation for the over-explanation of the FSP NPA models. Those unmeasured dimensions of welfare reform probably pushed some people off of TANF, and onto the FSP NPA caseload. They would thus push up the FSP NPA caseload, eliminating (some of) the over-explanation of the FSP NPA caseload decline.

For SSI, we have the opposite problem. We massively over-explain the caseload decline (383%; nearly four times). The richer model with lags of the economic variables is even worse (673%). The explanation of this over-explanation appears to be that there is little to explain. The actual decline is only 6 percent. Thus, the moderate effects we do estimate swamp the actual increase. In addition, it seems plausible that some of the unmeasured TANF policies pushed up the SSI caseload. In particular, several states had active

policies to help TANF cases (where the state paid all of the costs) to qualify for SSI (where the federal government paid all of the costs).

Results for the later period are different. Over this period caseload trends diverged. The FSP TANF caseload declined by another 7 percent. The SSI caseload increased by 16 percent. Finally, the FSP NPA caseload increased by 68 percent.

Our model for the FSP NPA caseload explains about half of the observed increase (49% for the Base Model; 53% for the model with lags of the economic variables). The explanation appears to be nearly equal parts the decrease in short certification periods and the increase in Simplified Reporting, and a slightly larger contribution of the weakening economy (14%, 15% and 20% respectively, in the Base Model). We consider this to be a relatively successful estimation/simulation result.

Results for the FSP SSI caseload are similarly successful overall, but the individual results seem implausible. The Base Model explains 46 percent of the increase in the SSI caseload; while the model with lags of the economic variables explains 78 percent. The economic effects themselves seem plausible. However, the model attributes large effects to short certification periods and Simplified Reporting. We argued on a priori grounds that it seemed unlikely that these FSP policies would affect the SSI caseload. Furthermore, while substantively large, these corresponding regression coefficients were not statistically different from zero.

The FSP TANF caseload results appear to have similar issues. The model implies that the FSP TANF caseload would grow. It actually shrinks. Again, statistically insignificant regression coefficients are inducing large implied changes in the caseload. The economy is estimated to have pushed the caseload up another 11 percentage points. This seems plausible. Again, it appears that unmeasured features of TANF continued to push the TANF caseload down, even when the economy alone would have pushed it up.

DISCUSSION

This chapter has reported results from a Base Model for the disaggregated FSP caseload—TANF, SSI, and NPA, separately. It has also reported several sensitivity analyses for these models. Overall, we find that the basic pattern of results is robust across the various specifications, but the

patterns are very different across the components of the caseload. We defer discussion of the substantive findings to the next and final chapter.

VI. CONCLUSION

We have used FSPQC data to estimate the determinants of the FSP caseload and FSP benefits per capita for three mutually exclusive and exhaustive groups: (i) FSP households receiving cash benefit from TANF (formerly AFDC), but not SSI; (ii) FSP households receiving cash benefits from SSI, and perhaps TANF; (iii) FSP households receiving no cash benefits from either AFDC/TANF or SSI (NPA/Non-Public Assistance). Over our period, 1990 to 2004, the FSP caseload shifted sharply from nearly half AFDC/TANF to just over a sixth. The share of the FSP caseload receiving SSI increased somewhat. The share of NPA FSP caseload increased sharply.

We find that the explanatory power of our models is concentrated almost exclusively in the NPA part of the caseload. In that increasingly important subset of the caseload, the economy and FSP policy have the expected effects and the effects are large. Specifically, the NPA caseload moves counter-cyclically (i.e., it rises when the economy is weak) and the NPA caseload is quite sensitive to the burden of participating in the FSP—as proxied by short certification periods and Simplified Reporting. We find no evidence for effects of EBT adoption or outreach expenditures. These qualitative results are robust to multiple variations in the specification. Specifically, estimating the models excluding Hispanics—as a rough proxy for the effects of immigration and PRWORA's immigration related provisions—does not substantially shift the results. Simulations imply that our models explain much of the movement in the NPA caseload, but do not explain well the movement in the other two caseload components.

These results are qualitatively similar to estimates based on survey data from the Survey of Income and Program Participation (Hanratty, 2006, and Ratcliffe, McKernan, and Finegold, 2008). In particular, those estimates imply that shorter certification periods reduce participation among families that are likely eligible for the FSP. Ratcliffe, McKernan, and Finegold (2008) also find no effect of EBT and outreach spending on participation.

Our results differ in that we find that portions of the FSP caseload are higher in states that implemented simplified reporting. However, the effect we find is concentrated in the portion of the caseload not receiving SSI or TANF cash assistance. Thus it appears that our approach of focusing on

caseloads by type of program participation rather than by household composition may enable us to detect policy effects in a time period of rapid change in participation in means-tested assistance programs.

Our results may also differ simply because we use administrative and census data that more accurately track FSP caseloads and populations than do survey data. The FSPQC data also have larger samples of participants than does the SIPP. Against this accuracy we trade off the ability to target eligible households. The two approaches—one using survey data, the other using administrative data—are in this sense complementary.

APPENDIX A. FSPQC DATA

U.S. Department of Agriculture – Food and Nutrition Service

Quality Control Review Schedule

Form Approved OMB No. 0584-0299

PRIVACY ACT/PAPERWORK REDUCTION ACT. According to the Paperwork Reduction Act of 1995, no persons are required to respond to a collection of information unless it displays a valid OMB control number. The valid OMB control number for this information collection is 0584-0299. The time required to complete this collection is estimated to average 1.05 hours per response, including the time to review instructions, search existing data resources, gather the data needed, and complete and review the information collection. This report is required under provisions of 7 CFR 275.14. This information is needed for the review of State performance in determining recipient eligibility. This information is used to determine State compliance, and failure to report may result in a finding of non-compliance.

Section 1 – Review Summary

1. QC Review Number	2. Case Number	3. State	4. Local Agency	5. Sample Month & Year		6. Stratum

7. Disposition	8. Finding	9. FS Allotment Under Review	10. Error Amount	11. Case Classification

Section 2– Detailed Error Findings

	12. Element	13. Nature	14. Cause	15. Error Finding	16. Error Amount	17. Discovery	18. Verified	19. Occurrence a. Date	19. Occurrence b. Time Period
1									
2									
3									
4									
5									
6									
7									
8									

FNS-380-1 (10-01-2003) Previous editions obsolete.

1

Section 3 – Household Characteristics

20. Most Recent Cert. Action Month, Day, Year

21. Type of Action

22. Length of Cert. Period # of months

23. Allotment Adjustment

24. Amount of Allotment Adjustment

25. Number of Household Members

26. Receipt of Expedited Service

27. Authorized Representative Used at Application

28. Categorical Eligibility

29. Reporting Requirement

Resources:

30. Liquid

31. Property (excluding home)

32 a. Vehicle

32 b. Status 2nd Vehicle

33. Countable Vehicle Assets

34. Other Non-liquid

Income:

35. Gross

36. Net

Deductions:

37. Earned Income

38. Medical

39. Dependent Care

40. Child Support

41. Shelter

42. Homeless

Additional Information on Shelter Costs:

43. Rent/Mortgage

44. Use of SUA a. Usage b. Proration

45. Utilities (SUA or Actual)

FNS-380-1 (10-01-2003) Previous editions obsolete. 2

Section 4 – Information on Each Household Members

46. Person Number	47. FSP Participation	48. Relation Head of HH	49. Age	50. Sex	51. Race	52. Citizen Status	53. Edu. Level	54. Employment Status	54. Employment Hours	55. FSP Work Reg.	56. FSP E&T	57. ABAWD Status	58. Dependent Care Cost

You may record information on up to 16 individuals using additional pages.

FNS-380-1 (10-01-2003) Previous editions obsolete. 3

Section 5 – Income Identified by Household Member

59. Person Number	Source 1 60. Income Type	61. Amount	Source 2 62. Income Type	63. Amount	Source 3 64. Income Type	65. Amount	Source 4 66. Income Type	67. Amount

You may record income on up to 10 individuals by using additional pages.

Section 6 – Reserved Coding

68.	69.	70.	71.	72.	73.	74.	75.	76.

Section 7 – Optional For State Use

1																				
2																				
3																				
4																				

Complete documentation for the FSPQC Public Use Files for 1996-2004 is available on the MPR web site (http://host4.MPR-mpr.com/fns/fnsqcdata/download.htm; n.b., all years are federal fiscal years). It appears that the basic data collection instrument was stable from FFY1990 to early FFY 1998; specifically October 1989 through December 1997. Thereafter, the form has changed several times. PRWORA's block granting resulted in the end of joint the integrated (FSP/AFDC quality control system. With the end of that system, some earlier items were deleted, allowing room for new items. Furthermore, the major PRWORA (1996) changes to the FSP required the collection of new information. Therefore, several variables were added or changed in FFY 1999/April 1999 and in FFY2001/November 2000. These new variables included information on ABAWD status, citizenship status, and vehicle ownership.[27]

Then for FFY2003/October 2002 file, the form was revised again. Variables for which there was difficulty collection reliable data were dropped. In addition, the FSPQC stopped collecting information on ineligible households, since complete data was not available.

As far back as FFY1995, MPR reports problems with the coding of citizenship status (as well as deemed AFDC and wage income) that they addressed through a set of standardized recodes (beginning in FFY1996; the FFY1995 recodes were performed by hand). With the FFY1998 changes, MPR began to report that the ABAWD and citizenship variables appeared to be unreliable. As of the FFY2001 file, MPR deemed these two variables to be reliable.

MPR also notes that the Years of Education variables have very high missing rates (at least for FFY1998 when the problem was first noted, through FFY2001). In addition, with the FFY2003 file, MPR notes problems with the

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[27] Of lesser interest to this study there were also new items on shelter costs, allotment adjustments, homeless shelter deduction, energy assistance, and State diversion payments.

Race variable in some States (including New York in FFY2003 and Delaware in FFY2004).

APPENDIX B. SOURCES FOR FORCING VARIABLES

In this appendix, we provide a detailed discussion of our coding of the forcing variables. We begin with a formal description of changes to the FSP. We then discuss welfare policy, the economy, and the other forcing variables included in our models. Finally, we briefly discuss the reasons we excluded several other FSP policies from our preferred models.

INCLUDED FOOD STAMP POLICIES

Between 1996 and 2006, the FSP has been shaped by a series of authorizing statutes. The annual series “Characteristics of Food Stamp Households” published by the Office of Analysis, Nutrition, and Education within USDA includes a summary of statutory changes over the previous year; Cunnyngham (2005) collates these changes. Box B.1 reproduces those summaries.

Box B.1 **Statutory Changes**	
FFY1996	PRWORA was enacted on August 22, 1996 and made the following changes to the FSP: • States were required to implement EBT for the issuance of Food Stamp benefits by October 1, 2002. • The household definition was expanded to include married children and children who are parents, under age 22, who also live in the household. • Most permanent resident aliens are ineligible to participate in the FSP. • Most ABAWDs who are not working are only eligible for 3 months of benefits in a 36-month period. • The age at which a student's earnings begin to be included in the household's income was lowered from 22 years to 18 years. • The maximum monthly benefit was lowered from 103 percent of the TFP to 100 percent. • The fair market value limit for vehicles increased from $4,600 to $4,650. • The deduction for shelter costs was increased to $247 from $250, starting January 1, 1997
FFY1998	In 1997, Congress passed the Balanced Budget Act, which took effect in fiscal year 1998. This legislation allows States to exempt 15 percent of the unemployed, able-bodied, childless adults from the FSP time limits imposed by the Personal Responsibility and Work Opportunity Reconciliation Act of 1996. In addition, the Balanced Budget Act increased funds for the Food Stamp Employment and Training Program, while restricting how the funds could be used and making them available until expended. States were required to earmark 80 percent of their federal food stamp employment and training funds to provide approved work or training programs for childless, able-bodied 18- to 50-year-olds.

FFY1999	Effective November 1, 1998, the Agricultural Research, Extension and Education Reform Act of 1998 restored eligibility to some permanent resident aliens.
FFY2000	Effective July 1999, categorical eligibility was expanded to include households in which all members are authorized to receive benefits from means-tested programs funded over 50 percent by TANF and/or State Maintenance of Effort (MOE) funds. At a State's option, other programs that further the purposes of welfare reform may also be used to confer categorical eligibility. Thus, a family who leaves welfare to work but still receives transitional assistance, such as child support subsidies or transportation assistance, may still be considered categorically eligible for the FSP. States began implementing expanded definitions of categorical eligibility throughout Fiscal Year 2000.
FFY2001	New regulations implemented in January 2001 excluded from the asset test any vehicle with equity below $1,500, and exempted from the equity test one vehicle per adult in every household as well as any vehicles used by a teenager to drive to work or school. If there are no qualifying adults or teenagers in the household, one vehicle is still exempted from the equity test. For vehicles exempt from the equity test but not excluded entirely from the asset test, any fair market value exceeding $4,650 is counted toward the asset limit. For any remaining vehicles, the higher of either any fair market value in excess of $4,650 or any equity is counted.

FFY2001	In addition, the FY 2001 Agricultural Appropriations Act (enacted in September 2000 but not effective until July 1, 2001) allowed States to use TANF vehicle rules in place of food stamp rules if the TANF rules were more generous. Further, a broader interpretation of categorical eligibility rules was announced in July 1999 and implemented on November 21, 2000. This new interpretation requires States to confer categorical eligibility on families receiving or certified as eligible to receive benefits or services that are at least 50 percent funded by TANF or Maintenance of Effort (MOE) funds. (States have the option of conferring categorical eligibility on families receiving or certified to receive benefits or services that are less than 50 percent funded by TANF/MOE. They may also confer categorical eligibility on households where one member receives the benefit or service, but the State determines that the whole household benefits.) Categorically eligible households are exempt from the asset test and the net income test. They are subject to a 200 percent of poverty gross income test. These changes were designed to make it easier for low–income workers to keep a vehicle and still receive food stamps. The FY 2001 Agricultural Appropriations Act increased the maximum allowable shelter deduction on March 1, 2001. States were directed to apply the higher shelter deduction when certifying or recertifying households on or after March 1.
FFY2002	<none>
FFY2003	The 2002 Farm Security and Rural Investment Act restored eligibility for qualified noncitizens who are receiving disability payments, have lived in the United States for five years as a legal immigrant beginning on the date of entry, or are under 18. The legislation also raised the asset limit to $3,000 for households with disabled members, and adjusted the standard deduction to vary by household size and be indexed each year for inflation

FFY2004	The 2002 Farm Security and Rural Investment Act restored eligibility to certain qualified noncitizens. The eligibility of qualified noncitizens who are receiving disability payments or who have lived in the United States for 5 years as a legal immigrant beginning on the date of entry was restored in fiscal year 2003. The eligibility of noncitizen children was restored October 1, 2003.
FFY2005	<none>
FFY2008	The 2008 Farm Bill included renaming the program to SNAP, increasing the minimum deduction and adjusting for inflation, removing the cap on the dependent care deduction, increasing the standard deduction and adjusting for inflation, indexing the asset limits, and excluding retirement and educational savings.

Certification Periods: We compute the fraction of cases with certification periods of 3 months or less directly from the FSPQC dataset. This approach is the one taken in Kabbani and Wilde (2003) although we modify it by computing the fraction with short certification periods *within* each of our subgroups.

Program Outreach: FNS provided yearly total amounts States spent on outreach for FFY1990-2005. For each state, we divide the annual number by the population total, and allocate this per capita amount equally among the twelve months of the year. Twenty-two states had no outreach expenditures over the entire period. Among those with positive expenditures (in FFY2005, 21 states), the mean FFY2005 expenditure was $269,814, while the median expenditure was $46,950.

Electronic Benefits Transfer: PRWORA required that all states implement EBT by October 1, 2002. FNS reports the timing of statewide implementation of EBT programs at http://www.fns.usda.gov/fsp/ebt/ebt status report.htm. We compute the percent of FSP issuance made via EBT using forms FNS-46, “Issuance Reconciliation Report” and FNS-338, “State Issuance and Participation Estimates”. Statewide implementation of EBT occurred between April 1993 and June 2004.

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.

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 FFY2004, 45 states had adopted simplified reporting as we measure it (Rosenbaum, 2003; National Association for Program Information and Performance Measurement, 2004; USDA, 2004).[28] Implementation occurred between September 2000 and July 2005.

Table B.2 lists the month and year of adoption of EBT and simplified reporting. We assume if a policy is implemented in a month, it is in effect throughout the entire month.

Table B.2
Detailed Food Stamp Policy Coding

State	Electronic Benefits Transfer Implemented Statewide	Simplified Reporting Implemented
Alabama	Nov-97	Aug-04
Alaska	Jun-98	Jan-04
Arizona	Aug-99	Jan-04
Arkansas	Apr-98	Nov-03
California	Jun-04	Jul-04
Colorado	Feb-98	Nov-03

[28] We verified this information and filled in gaps by examining states' on-line FSP policy manuals and by making calls to state officials.

Connecticut	Oct-97	Jan-03
Delaware	Sep-03	Mar-02
District of Columbia	Oct-98	May-01
Florida	Oct-98	Aug-03
Georgia	Nov-98	Oct-02
Hawaii	Aug-98	
Idaho	Feb-98	Nov-03
Illinois	Nov-97	Nov-03
Indiana	Mar-02	Jun-03
Iowa	Oct-03	Jan-04
Kansas	Mar-97	Oct-02
Kentucky	Nov-99	Nov-03
Louisiana	Dec-97	Sep-00
Maine	Jun-03	Jun-03
Maryland	Apr-93	Oct-01
Massachusetts	Oct-97	Oct-02
Michigan	Jul-01	Aug-01
Minnesota	Oct-98	
Mississippi	Oct-02	May-05
Missouri	May-98	Sep-04
Montana	Jul-02	Apr-02
Nebraska	Sep-02	Jun-03
Nevada	Jul-02	Oct-04
New Hampshire	Jan-99	Jul-05
New Jersey	Jun-99	Jul-01
New Mexico	Aug-95	Jan-04
New York	Feb-01	Oct-02
North Carolina	Jun-99	Jun-03
North Dakota	Mar-97	
Ohio	Oct-99	Nov-03
Oklahoma	Jan-98	Mar-02
Oregon	May-98	Aug-03
Pennsylvania	Sep-98	Jun-03
Rhode Island	Oct-98	Dec-04
South Carolina	Dec-95	Aug-02
South Dakota	Mar-97	
Tennessee	Aug-99	Sep-01
Texas	Nov-95	Apr-03
Utah	Apr-96	
Vermont	Oct-98	Nov-02
Virginia	Jul-02	Jul-03
Washington	Nov-99	Oct-02
West Virginia	Jun-03	May-01
Wisconsin	Oct-00	Jul-03
Wyoming	Jan-00	

INCLUDED WELFARE POLICIES

Statutory changes summarized in Box B.1 implies that the implementation of TANF affected FSP caseloads both directly by changing eligibility and the attractiveness of the program and indirectly by beginning a process of unbundling welfare from food stamps receipt. We include two high-level welfare reform variables that capture the date of implementation of any statewide waiver to AFDC policies granted in the early 1990s and the date of

implementation of states' TANF programs. We draw the dates of implementation from Crouse (1999); this is also the coding used in CEA (1999).

INCLUDED PROXIES FOR THE ECONOMY

Following previous research that indicates that richer measures of the economy increase the share of observed change attributable to the economy, we use three measures of the economy: monthly State-level unemployment rates (multiplied by 100) from the Bureau of Labor Statistics' series A; monthly State-level total covered employment as recorded by the Quarterly Census of Employment and Wages (QCEW) Program that the BLS conducts in cooperation with the States; and quarterly State-level total earnings per worker, as recorded by the QCEW. We divide total employment by the total estimated population in a State and month and multiply by 100.

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

OTHER FORCING VARIABLES INCLUDED IN THE MAIN MODELS

Two other time-varying forcing variables are worthy of discussion: The minimum wage, and the Earned Income Tax Credit (EITC).

Minimum Wage: A higher minimum wage could have either a negative or a positive effect on the caseload. A higher minimum wage may induce businesses to substitute away from (unskilled) labor, thus increasing the caseload; at the same time, the minimum wage jobs that remain are better paying, thus reducing caseload. In net, the effect on welfare caseloads is ambiguous. Previous research has found a negative effect of the minimum wage on the welfare caseload: CEA (1999) estimates an elasticity of between -0.25 and -0.52, while Grogger (2004) also finds a negative impact of the minimum wage on welfare use among single mothers with children over nine. We use the State-level minimum wage and, following CEA (1999), compute the real monthly earnings of an individual working at the minimum wage for 30 hours a week.

Earned Income Tax Credit (EITC): There were also major expansions in the federal Earned Income Tax Credit (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), the methods we use that enable us to estimate the effect of FSP and welfare policy changes on the caseload do not allow us to estimate the effect of nationwide policy changes. A minority of States had an earned income tax credit during this period. They are much smaller than the federal credit, and we do not consider them here.

OTHER CONTROLS INCLUDED IN THE MAIN MODELS

The specification includes a national year spline with knot points in October of each year, fixed effects for each State, State-specific linear year trends (in some specifications), and indicators for each calendar month.

FOOD STAMP PROGRAM POLICIES NOT INCLUDED IN THE MAIN MODELS

As noted in Chapter 3, we explored several other FSP policies, but did not include them in our final models. We excluded them because our time series of cross-sections approach has only a limited number of degrees of freedom. On a priori grounds we concluded that the effect of these variables was likely to be small. Limited specification searches were consistent with that a priori expectation. Here we describe these other policies.

Payment Error Rates: FNS penalizes States for over- and underpayments. According to the GAO (2001), 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 of 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-payments and under-payments and the types of errors are positively correlated.

In a simple model, high overpayments would be expected to lead to higher caseloads (Kornfeld, 2002). Some people who should not be getting food stamps get them. Some people who should be getting smaller FSP benefits and might therefore choose not to remain in the program, are getting larger benefits and

therefore remain enrolled. Conversely, high underpayments might be expected to lead to lower caseloads.

The literature, plausibly, argues for a different pathway (Rosenbaum, 2000). Consider what happens if for exogenous reasons (e.g., poor supervision, large fraction of the caseload working) the combined error rate rises. Fearing federal penalties, States take actions to lower their error rates. Some of the actions are formal and captured in our earlier discussion of certification and reporting periods. Other measures are informal (i.e., there was no change in official policy that would be captured in the "State Options Report", but there was a de facto change in policy). Caseworkers feel the pressure and respond in ways that push down the caseload (e.g., shorter certification periods, more scrutiny of those with earned income).

The previous literature has found mixed results on the effects of error rates. Kornfeld (2002) finds an effect; Ziliak, Gundersen, and Figlio (2003) does not. Danielson and Klerman (2006) find that overpayments are associated with a higher overall caseload, but no relationship between underpayments and the caseload. Hanratty (2006) and Ratcliffe, McKernan, and Finegold (2008) and do not include payment errors in their models.

On a priori grounds, it seems likely that the reporting and recertification requirements are the crucial policy choice. Error rates themselves are not a direct cause of caseload levels.

Transitional Benefits: Clearly, leaving welfare is a change of circumstances; and, it is a change of circumstances of which the State welfare department is (in principle) aware. Furthermore, families sometimes leave because of a change in circumstances that would affect their FSP benefit (e.g., employment, marriage). Thus, leaving welfare would often trigger a requirement to reevaluate eligibility for the FSP (but see 7 C.F.R. 273.12(f)(3)). Given that compliance burden, we would expect FSP enrollment to drop sharply with welfare exit.

Because of the stigma of participation, we would expect a drop in FSP participation associated with welfare exits even if families remain nominally eligible. In as much as people leave welfare to escape demeaning contact with the welfare office and the stigma of welfare participation, we would expect those people to also exit the FSP at the same time. Indeed, studies of welfare leavers find sharp drops in FSP participation among leavers (Zedlewski and Brauner, 1999; Dion and Pavetti, 2000; Quint and Widom, 2001; Rafferty,

Zedlewski, and Gruber, 2001; Miller, Redcross, and Henrichson, 2002). Similarly, studies of welfare exit often find that the exiting family remained nominally eligible, though often for a relatively low benefit (GAO, 1999; Zedlewski and Brauner, 1999; Zedlewski and Gruber, 2001).

To address concerns about drops in FSP participation with welfare exit, 2001 USDA regulations allowed States to provide up to three months of Transitional Food Stamps. During this period, the food stamp benefit remains what it was before the case closed, adjusted for the loss of the TANF benefit (unless the household requests that its benefit adjusted, or the State opts to use information from another program in which the household participates to calculate the benefit). The 2002 Farm Bill extended the period during which States could offer such Transitional Food Stamps to five months.

Before FFY 2003, only New York had adopted transitional benefits (for the three months allowed under the pre-2002 regulations). With the 2002 Farm Bill, more States adopted Transitional Food Stamps. By the end of FY 2003, 13 States had adopted transitional benefits (CBPP, 2003; USDA, 2004).

We would expect transitional benefits to increase the total FSP caseload by increasing its non-welfare portion. However, welfare leavers as a fraction of the FSP caseload are small, especially given the short period of eligibility. It thus seems unlikely that Transitional Food Stamps will have a major effect on the aggregate FSP caseload. Furthermore, our data includes only a very short window in which transitional benefits might have been adopted, so our power to detect any effects would be low.

Asset Policies: In regulations in January 2001, new vehicle rules were implemented excluding vehicle equity below $1,500 and one vehicle per adult. Also, starting in 2001, states were allowed to adopt TANF vehicle asset rules if they were more generous than FSP rules (Table B.1). We do not consider asset policies here.

Immigrant Restrictions: The 1996 PRWORA legislation made many permanent residents (i.e., legal immigrants who were not citizens) ineligible for food stamps (Zimmerman and Tumlin, 1999). (Undocumented immigrants had never been eligible for the FSP.) Later legislation restored eligibility for some noncitizens (see Table B.3; USDA, 2000; Stavrianos, Cody, and Lewis, 1997) (see Table B.3).

Table B.3
Statutory Changes to FSP Treatment of Immigrants

Legislation	Effective Date	Provision
PRWORA (7/1996)	10/1996	Barred most legal immigrants (exempted groups included refugees (for 7 years), those with 40 quarters of work, and military personnel and their families).
Agricultural Research, Extension and Education Reform Act (AREERA) of 1998	11/1/1998	Restored eligibility for elderly, disabled, and children who were in the US August 22, 1996.
Farm Security and Rural Investment Act (Farm Security Act) of 2002	10/1/2002	Restored eligibility for qualified noncitizens receiving disability benefits.
	4/1/2003	Restored eligibility to qualified noncitizens who have lived legally in the US for over five years.
	10/1/2003	Restored eligibility for qualifying non-citizen children

Beyond the *de jure* effect on eligibility, there are claims that PRWORA's limitation on benefits to immigrants and other anti-immigrant moves during this period had an overall "chilling effect" even on unaffected immigrants (e.g., the citizen children of noncitizen immigrants; see Fix and Zimmerman, 1999; Fix and Passel, 1999; Borjas, 2001). Note that even without a "chilling effect", we might expect some drop in FSP cases, even among those in households with immigrants who remain eligible. Making some individuals in a household ineligible for food stamps lowers the FSP benefit. The administrative burden of applying for and remaining in the FSP, however, remains the same. Some households might decide that the administrative burden is not worth the now lower FSP benefit (see Kornfeld, 2002, for a similar argument).

Note that these were national program changes, so their effect cannot be estimated directly using our DoD methods. Many States instituted programs to restore "food stamps" to legal immigrants (see Zimmerman and Tumlin, 1999). Those were State programs. Participants in those programs do not appear in our federal FSPQC data. In contrast, they would probably appear in survey-data based analyses.

Finally, note that noncitizens make up a relatively small proportion of the caseload (9 percent in 1996, prior to enactment of PRWORA; and 6 percent in 2007). In some analyses in the body of this report, we proxy for the effect of these changes by dropping all Hispanics (relatively few blacks or whites are immigrants). When we do so, we drop Hispanics from both the numerator (the count of program participants) and the denominator (the estimate of the population).

Able Body Adults Without Dependents (ABAWDs): The 1996 PRWORA legislation required States to require able-bodied childless adults who do not have 20 hours per week in an approved activity—work or an approved training program—to receive FSP benefits for 3 months in 36. The original 1996 PRWORA legislation allowed States to waive the 3-month cutoff if no jobs were available. The 1997 Balanced Budget Act (effective October 1, 1997) gave States the additional authority to waive the three-month cutoff for an additional 15 percent of their ABAWD caseload. In practice, this appears to mean that States could extend the cutoff to five or even six months, if they so chose (see CBPP, December 7, 2000).

As Czajka et al. (2001) has shown (see also Savrianos, Cody, and Lewis, 1997; Genser, 1999), only a small fraction (1.3 percent) of the FSP caseload is potentially subject to ABAWD regulations and very few cases have been sanctioned. Thus, on a priori grounds it seems extremely unlikely that ABAWD regulations have affected caseloads even at the level of disaggregation considered here.

Other Policies: The State Options Reports (USDA, 2001, 2002, 2003, 2004) also provide information on State policies with respect to simplified definitions of income and resources, expanded categorical eligibility, simplified housing costs, simplified standard utility allowance, simplified determination of deductions, child support expense income exclusion, State option FSP for immigrants, simplified FSP for TANF households, wage supplements, education and training pledge States, employment and training sanction periods, comparable disqualification, child support-related disqualification, drug felony disqualification, and electronic application filing. On a priori grounds, we assume that these policies do not have large effects on the caseload.

WELFARE POLICIES NOT INCLUDED IN THE MAIN MODELS

Our main models only include high-level summaries of welfare policy: The maximum welfare benefit, any AFDC waiver, and time since TANF implementation. We also considered specific welfare policies. We also explored several other variables. Specifically, we considered the following detailed welfare policies.

Maximum Benefit: We computed this as the natural log of the average of the previous year's maximum benefit level for a family of three divided by 100. Maximum benefits are drawn from the dataset created for CEA (1999) and updated with information from Urban Institute's Welfare Rules Database, the State Policy Documentation Project, the fifth TANF Report to Congress and information provided by the U.S. Department of Labor (CBPP/CLASP, 1999; OFA, 2003; Urban, 2001a).

Specific TANF Policies: Table B.4 summarizes our coding of these TANF policies. We considered four major policy determinants of the welfare caseload: time limits on aid receipt, the rate at which the benefit declines as earnings increase ("financial incentives"), penalties for non-compliance with work-related aspects of welfare programs ("sanctions"), and programs aimed at assisting families facing a temporary need to avoid becoming part of the welfare caseload ("diversion"). We estimated a set of models including these detailed welfare policies. While largely of the right sign, they are imprecisely estimated.

Table B.4

State Welfare Reform Policies

	N. States
Financial incentives	
Earnings-eligible for TANF if single parent with two children working 30 hours a week at state 20th percentile wage	17
Not eligible (AFDC policy)	34
Work-related sanctions	
Grant eliminated at first instance of non-compliance with TANF requirements	12
Grant gradually eliminated if non-compliance is on-going	25
Grant reduced if adult is non-compliant (AFDC policy)[29]	15
Time limits	
No time limit (AFDC policy)[30]	5
Grant is reduced by adult's share if time limit reached	5
Grant is eliminated if adult reaches time limit	42
Diversion	
Families can apply for a short-term, lump-sum payment in lieu of TANF to meet emergency needs	28
No diversion program (AFDC policy)	23

Note: Row entries indicate the number of states that ever implemented each policy type, so the rows in the sanctions and time limit sections of the table may sum to more than 51.

Source: Urban Institute, Welfare Rules Database.

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[29] Note that three states altered the AFDC policy by reducing the family's grant by a fixed amount or percentage of the grant. We include these states in the partial sanction category because the incentives both policies present case heads are not substantially different.

[30] We count states that nominally have a time limit, but that offer substitute programs or time limit extensions to all adults who reach it, as having no time limit.

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