Consumption Reponses to In-Kind Transfers: Evidence from the Introduction of the Food Stamp Program

Hilary W. Hoynes
University of California, Davis and NBER
hwhoynes@ucdavis.edu

and

Diane Whitmore Schanzenbach
Harris School of Public Policy
University of Chicago
schanzenbach@uchicago.edu

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Abstract

Economists have strong theoretical predictions about how in-kind transfer programs – such as providing vouchers for food – impact consumption. Despite the prominence of the theory, there is little empirical work documenting actual responses to in-kind transfers, and most work that does exist finds results that fail to support the canonical theoretical model. In this work, we leverage previously underutilized variation in the date of the county-level original implementation of the Food Stamp Program in the 1960s and early 1970s. Using the Panel Study of Income Dynamics, we employ difference-in-difference methods to estimate the impact of program availability on food spending. Consistent with theoretical predictions, we find that the introduction of food stamps leads to a decrease in out of pocket food spending and an increase in overall food expenditures. The results are precisely estimated for total food spending, with less precision in estimating the impacts on out of pocket food costs. We also use this experiment to test whether households are inframarginal. We find that, consistent with theory, households respond similarly to a dollar in cash income and a dollar in food stamps. Further, we find that households that are predicted to be "constrained"—desiring lower food expenditures than expected food stamps—indeed experience larger impacts in food consumption than unconstrained households.

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I. Introduction

Providing assistance to the poor through in-kind transfers such as vouchers for food and housing garners more political support than providing assistance in cash. Supporters of such policies believe that providing voucher payments for certain goods (like groceries) will cause recipients to purchase more of the goods being subsidized, and that recipients will not be able to use public assistance to buy other, less socially desirable goods (like alcohol or cigarettes). According to canonical economic theory, though, providing a transfer in-kind should lead to the same outcome as a similar sized cash transfer as long as program participants are inframarginal. As a result, depending on consumer preferences the provision of in-kind transfers (relative to cash) may have little to no impact on purchases of the actual goods being subsidized.

Despite strong theoretical predictions about consumer behavior, little empirical evidence has been brought to bear on the impacts of providing in-kind transfers on consumer purchases.[1] The Food Stamp Program (FSP) is one of the largest transfer programs for the low income population. In 2004, for example, the program cost $27 billion and served 24 million persons. This compares to $25 billion for Temporary Assistance for Needy Families and $33 billion for the Earned Income Tax Credit. It has been very difficult for researchers to isolate the causal impact of the FSP on food spending, nutritional intake, labor supply and other outcomes. Because the program is national, there is not variation in program parameters (such as stark differences in state benefit levels or eligibility) that are typically exploited by researchers to measure program impacts. In the absence of programmatic variation, most researchers have studied the impact of the FSP by comparing food stamp recipients with eligible non-recipients. Since we would expect participants and non-participants to differ in important – and potentially unobservable – ways (Currie 2006), researchers

[1] There is a literature that explores the impact of cash transfer programs (Engen and Gruber 2001; Gruber 1997, 2000; Hubbard et al. 1995; and Kantor and Fishback 1996) and in-kind transfer programs (Gruber and Yelowitz 1999) on consumption and wealth. This literature is particularly concerned with the insurance element in public programs and in estimating the impact on precautionary savings and consumption smoothing. More recently, Meyer and Sullivan (2004) examine the impact of welfare and tax reform on consumption, making the argument that consumption is an important measure of family well-being that has been largely ignored in the evaluation of transfer programs (Meyer and Sullivan 2003).

have employed a variety of methods to control for selection into the program (see Fraker 1990, for a comprehensive review of the literature on food stamps and consumption).

In general, the literature conflicts with the canonical economic model and finds that food stamps increase food purchases among program participants by a much larger amount than would be predicted by the canonical economic model. These studies have found that the marginal propensity to consume food out of food stamp income is about 4 times higher than it is out of cash income (Fraker 1990). As a result, food stamp benefits worth $100 are thought to cause about a $17-$47 increase in food spending while a cash transfer of $100 is associated with closer to a $5-$10 increase in food spending. But, as mentioned above, these results have been based on studies that rely on strong and untested assumptions.

To measure the impact of the food stamp program in this project, we utilize an underexploited source of variation: the original introduction of the program across counties.[2] There is tremendous variation in the timing of FSP introduction across counties in the United States—the earliest county programs were established in 1961 and the last county programs were established in 1975. The FSP started as eight county-level pilot programs and later expanded to 43 counties. This led to passage of the Food Stamp Act of 1964 which gave local areas the authority to start up a FSP in their county. This led to a steady increase in FSP adoption over the next ten years. Finally, the 1973 Amendments to the Food Stamp Act mandated that all counties offer FSP by 1975.

Our approach has the appeal of relying on non-marginal changes in incentives faced by consumers. This "program introduction" research design has been taken in recent analyses of other social programs such as Head Start (Ludwig and Miller 2007), Medicare (Finkelstein and McKnight 2008), and Title I (Cascio et al. 2006). It is also part of a larger literature examining impacts of the Great Society and Civil Rights era (for example see Almond, Chay and Greenstone 2006).

We use data from the Panel Study of Income Dynamics (PSID) from 1968-1978 to examine

[2] Almond, Hoynes and Schanzenbach (2008) and Currie and Moretti (2006) use food stamp program introduction to examine the impact of the program on birth outcomes.

the impact of the FSP on food consumption.[3] Specifically, we look at expenditures on food spent at home, meals out and total food spending. We test theoretical predictions that food stamps leads to a decrease in out of pocket spending on food and an increase in total food spending. Further, we examine whether consumers are inframarginal—that is a dollar in cash income and a dollar in food stamp benefits generate equal impacts on food spending.

We employ a difference-in-difference model where the treatment is at the county level, with controls for county and year fixed effects and state linear time trends. In this model, identification requires that there are no contemporaneous county level trends that are correlated with food stamp introduction and family economic outcomes. We also estimate a triple difference model that uses variation across subgroups with varying propensities of being affected by food stamps.

We control for possible confounders in two ways. First, we examine the determinants of the FSP introduction and find that earlier food stamp program introduction occurs in counties that are more populous, urban, black, low income, and with a smaller fraction of land used in agriculture. While these county characteristics are statistically significant determinants of county FSP implementation, we find that they explain little of the overall variation in food stamp implementation. Nonetheless, we include linear trends interacted with these pre-treatment variables to control for possible (observable, parametric) trends across counties. Second, FSP introduction took place during a period of great expansion in programs for the poor in the United States. To control for the possible coincident expansion of other programs such as AFDC, Medicaid, Medicare, and social security, we include county-by-year controls for federal spending on other social programs. We show that the results are very robust to adding these controls, suggesting that our identification is "clean." Finally, we estimate event study models that further support the validity of the research design.

Overall, our results indicate that people behave as the theory predicts. We find that the introduction of FSP leads to a decrease in out of pocket food spending and an increase in overall food

[3] Because the PSID begins in 1968, we can only take advantage of the counties that adopt food stamp programs after that time.

expenditures. The expected effect on the propensity to eat meals out at restaurants is theoretically indeterminate, and we find mixed and statistically insignificant results. The results are quite precisely estimated for total food spending, with less precision in estimating the impacts on out of pocket food costs and meals out. Further, we find evidence that the marginal propensity to consume food out of food stamp income is close to the marginal propensity to consume out of cash income. In addition, those predicted to be constrained (at the kink in the food/nonfood budget set) experience larger increases in food spending with the introduction of food stamps. The results are robust to many sensitivity tests including adding more fixed effects, examining subgroups of the sample, and placebo tests on groups not likely to use food stamps.

The remainder of the paper proceeds as follows. Section II presents a history of the food stamp program. Section III discusses the expected effects of the program and Section IV reviews the existing literature. Section V describes the data and Section VI presents the methodology. Section VII presents our results and Section VIII concludes.

II. Introduction of Food Stamp Program

The origins of the modern Food Stamp Program began in 1961 with President Kennedy's first executive order establishing eight county-level pilot programs.[4] The pilot programs were later expanded to 43 counties in 1962 and 1963. The success with these pilot programs led to the Food Stamp Act of 1964 (FSA). The FSA gave local areas the authority to start up a FSP in their county. As with the current FSP, the program was federally funded and benefit levels did not vary across areas. In the period following the passage of the FSA, there was a steady stream of counties initiating food stamp programs. Support for requiring food stamp programs grew due to a national spotlight on hunger (Berry 1984). This interest culminated in passage of 1973 Amendments to the Food Stamp Act, which mandated that all counties offer FSP by 1975.

At the time the FSP was introduced and expanded, hunger and nutritional deficiencies were

[4] This section is based on Berry (1984) and MacDonald (1977).

not uncommon among Americans. For example, a survey of low income families in Texas, Louisiana, Kentucky, and West Virginia in 1968-1970 found that 15 percent of whites and 37 percent of blacks had low hemoglobin levels (Eisinger 1998). There were also relatively high rates of deficiencies in vitamin C, riboflavin and protein. A 1968 CBS documentary "Hunger in America" raised national awareness of the problem and possibly influenced the policy debate on the FSP (Berry 1984).

It is important to understand the political context in which the FSP was introduced in the United States. Prior to the modern day FSP, some counties provided food aid through the commodity distribution program (CDP). The main goal of the CDP was to support farm prices and farm income by removing surplus commodities from the market. It was seen, however, as inadequate to promote the nutritional well-being of low income persons and the Citizens' Board of Inquiry into Hunger and Malnutrition in the United States declared that "the commodity distribution program is a failure" in its 1968 report *Hunger, U.S.A.* (Citizen's Board of Inquiry 1968). Of the 1,000 poorest counties, one-third offered no food assistance of any kind in 1967. Furthermore, of counties that did offer the program, the commodities rarely reached a majority of the poor population because of obstacles such as distribution centers that were difficult to reach, the limited range of products and infrequent timing of the distribution of goods. Consequently, debate about moving from the CDP to the FSP pitted powerful agricultural interests against advocates for the poor (MacDonald 1977, Berry 1984). In fact, as described in Berry (1984), passage of the 1964 Food Stamp Act was achieved through classic legislative logrolling. The farm interest coalition (Southern Democrats, Republicans) wanted to pass an important cotton-wheat subsidy bill while advocates for the poor (Northern Democrats) wanted to pass the FSA. Neither had majorities, yet they combined forces, supported each others bills, and both bills passed.

This political history is important because it illustrates that there was significant heterogeneity across the country in support for the FSP. Remember that the 1964 Act allowed for

counties to *voluntarily* set up food stamp programs. The above discussion suggests that counties with strong support for farming interests may adopt FSP later in the period while those with strong support for the low income population may adopt FSP earlier in the period. Consequently, the food stamp program introduction may not be completely exogenous. We return to this below.

Figure 1 summarizes the overall pattern of FSP introduction. In particular, the figure plots the percent of counties offering FSP, where the counties are weighted by their 1970 population. Note this is not the food stamp caseload, but represents the percent of the national population that lived in an area offering a FSP. The figure shows that there was a long ramp up period between 1964 and 1975, leading to the eventual universal coverage of the FSP. For example in 1968 (when the PSID begins) about half of the population lived in counties with FSP and by 1972 this rose to over 80 percent. It is this ramp up period that forms the basis of our research design.[5]

It is important to understand the CDP program in order to interpret the magnitude of the FSP effects. For example, if all food stamp recipients had previously received an equal amount of commodities, we would not expect to find any impact of the FSP on consumption. On the other hand, if counties adopting the FSP did not previously have access to the CDP or if the FSP provided a "larger" or "better" set of consumption choices, then the estimated coefficients would pick up the effect of the introduction of the program.

The evidence shows that the FSP indeed represents an important "treatment" over and above the CDP. First, as shown in Figure 2, the FSP caseload quickly overtakes the CDP caseload. In fact, the CDP was far from universally available prior to the FSP. For example, in 1967 35 percent of counties (not population-weighted) offered neither FSP nor CDP, 38 percent of counties offered only a CDP, 21 percent offered only a FSP, and 6 percent of counties offered both a CDP and FSP. Second, the CDP provided a very narrow set of commodities—the most frequently available commodities were flour, cornmeal, rice, dried milk, peanut butter and rolled wheat (Citizens' Board

[5] County FSP implementation dates are reported in USDA annual reports on county food stamp caseloads (U.S. Department of Agriculture, various years).

of Inquiry 1968). In contrast, the food stamp benefits can be used to purchase all food items (except hot foods for immediate consumption, alcoholic beverages, and vitamins). Further, the commodities were distributed infrequently. Finally, analyses of food intake in counties converting from CDP to FSP found that in its allowing participants to purchase a wide variety of food including fresh meat and vegetables, the FSP represented an important increase in the quality and quantity of food in comparison to the CDP (U.S. Congressional Budget Office 1977, Currie and Moretti 2006).[6]

To get more insight into the geographic variation in the ramp-up to a universal FSP, Figure 3 shows the timing of food stamp introduction by county. In the figure, the shading of the counties is assigned by county FSP start up date—with darker shading denoting a later start up date. This shows a great deal of variation in FSP introduction within and across states. Our basic identification strategy uses this county level variation in food stamp "treatment."

To further explore the degree of *within state* variation in FSP start dates, Figure 4 presents FSP coverage rates for selected states for 1961-1975.[7] This figure, as in Figure 1, plots the percent of the population (in this case in the state) that lives in a county offering food stamps. In some states, such as Massachusetts and Florida, there was little or no within state variation in food stamp start dates. Other states such as California and North Carolina the ramp-up was gradual within state. In most states, the county level food stamp introduction took place in a narrower period than for the country as a whole.

As discussed above, the 1964 FSA allowed counties to start a FSP—but it was voluntary. Therefore, the validity of our research design requires exogeneity of the county FSP start dates. The discussion above suggests that northern, urban counties with large poor populations were more likely to adopt food stamp programs earlier while southern, rural counties with strong agricultural interests adopted food stamps later. This systematic variation in food stamp adoption could lead to spurious

[6] Theoretically, counties were not supposed to have both FSP and CDP in place at the same time, but in practice some places did offer both. We have not been able to find a consistent time series for county participation in the CDP. Consequently, we are unable to use this information in our empirical analysis.
[7] Similar figures for the full panel of states are available online as Appendix Figures 1a and 1b.

estimates of the program impact if those same county characteristics are associated with differential trends in the outcome variables.

To explore this we compiled characteristics of counties in 1960, on the eve of the first food stamp pilot programs. We use these "pre" characteristics to predict the date that the county adopted a food stamp program. The dependent variable is the month and year of the county's food stamp start date—expressed as an index equal to 1 beginning in January 1961. The independent variables include the percent of the 1960 population that lives in an urban area, is black, is less than 5, is 65 or over, has income less than $3,000 (1959$), the percent of land in the county that is farmland, and log of the county population (constructed from the 1960 Census of Population and Census of Agriculture). We include the population to capture the fact that large counties might find the application process less costly relative to the benefits of application. All regressions are weighted by the 1960 county population.[8]

The results, presented in Table 1, include models with and without state fixed effects and with and without the early pilot counties (which were clearly nonrandom). We find that counties that are more populous, urban, black, and low income implement the FSP earlier. Further, we find that those with a larger share of the population that is very young or old implement earlier and counties where more of the land is used in farming implement later. These results are consistent with the political history of the program. We also find that the impacts of county characteristics are smaller (in absolute value) in counties in the South.

While these regression results show statistically significant impacts of the county characteristics on the timing of food stamp implementation, the quantitative importance of these predictors is small and most of the variation remains unexplained.[9] This is consistent with the

[8] In this analysis—and in the subsequent analyses of the PSID—we drop observations from Alaska due to inconsistencies in county definitions across samples and over time. Here we also drop the (few) counties where the percent of land used in farming was greater than 100 percent and we drop very small counties (with population less than 1,000) because of missing data.

[9] To illustrate this, online Appendix Figure 2 provides scatter plots of county characteristics (x-axis) against the county FSP implementation date (y-axis). These figures show that the magnitude of the association between the

characterization of funding limits controlling the movement of counties off the waiting list to start up their FSP: "The program was quite in demand, as congressmen wanted to reap the good will and publicity that accompanied the opening of a new project. At this time there was always a long waiting list of counties that wanted to join the program. Only funding controlled the growth of the program as it expanded" (Berry 1984, p. 36-37).

We view the weakness of the fit of the model as a strength when it comes to our identification approach—in that much of the variation in the implementation of FSP appears to be idiosyncratic. We should note that this only speaks to the lack of predictive power of observables; unobservable may still be a concern. Nonetheless, in order to control for possible differences in trends across counties that is spuriously correlated with the county treatment effect, we include interactions of these 1960 pre-treatment county characteristics with time trends in all of our models (as in Acemoglu et al. 2004).[10] The results are little impacted by the inclusion of these trends.

This period of FSP introduction took place as part of the much larger federal "war on poverty." For example, this period included the introduction of Medicaid, Medicare, Head Start and the Supplemental Nutrition Program for Women, Infants and Children (WIC). Further, AFDC, Social Security and disability income programs expanded. If these programs are mainly varying at the state level then our controls for state linear time trends or state-year fixed effects should absorb these program impacts. However, to control for the possible coincident expansion of other programs, we also include annual measures of county per capita transfer payments for cash income support, medical care, and retirement and disability programs.[11]

III. Expected Effects of Food Stamp Introduction

The current FSP provides a benefit to eligible families which is the difference between the

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county characteristics and the food stamp start date is weak and there is an enormous amount of variation that is not explained by the characteristics.

[10]Another approach might be to use these estimates to form propensity scores for matching counties. However, the weak fit of the model renders this less appealing.

[11] We have no documentation that any of these programs had the county roll out feature that provides the basis for our identification of the FSP. With respect to other nutrition programs, the National School Lunch Program had long been established, having started in 1946, and WIC was implemented more universally in 1972.

cost of a family-size adjusted "thrifty food plan" (e.g. the *guarantee* in transfer program parlance) and the amount a family can afford to spend on food. In this scenario, as understood in the canonical Southworth (1945) model and illustrated in Figure 5, the original budget line reflects the tradeoff between food and all other goods, and is shifted out horizontally by the amount of food stamps received (labeled here as B_F). The basic prediction of this transfer is that overall spending on food and other goods will increase as shown by the illustrated optimal points A_0* and A_1*. Out of pocket food expenses are expected to decrease (here the change is F_2-F_0). Consequently, the increase in total food consumption, shown here as F_1-F_0, is less than the increase in food stamps B_F. It is possible that a household with high demand for non-food relative to food might be constrained by the in-kind nature of food stamps (relative to a cash transfer) and would locate at the kink, as in point B_1^*. For these families, food stamps would lead to a larger increase in food consumption than an equivalent transfer in cash.[12]

Prior to 1979 (and during the time period studied here), families had to make an up-front cash payment to receive the food stamp benefits. This feature, called the "purchase requirement," did not change the magnitude of the benefits a family received.[13] Figure 6 shows the budget constraint in this case, where the amount of the purchase requirement is *P*. Note that the sloped part of the budget constraint is still shifted outward by the food stamp benefit (again B_F) but the top is censored and the attainable budget set is smaller. That is, a participant can no longer choose any consumption bundles that would have them spending more than their total income (*Y*) minus *P*. As a result there may be more people consuming at the kink in the budget constraint under the purchase requirement program than under the current program. This suggests that in our analysis the impact of food stamps on food spending may be somewhat larger than it would be under current (no purchase requirement) program

[12] Implicit in Figure 5 is the assumption that relative prices of food to nonfood are unchanged with the FSP. While it seems possible that the FSP could have led to increases in food prices (through increases in demand among the low income population) we have no data to test this hypothesis.

[13] That is, if the family was deemed able to afford to spend $60 on food, but the cost of the thrifty food plan was $80, the family could purchase $80 in food stamps for the cash price of $60. Under today's program, a similar family would receive simply receive $20 in food stamps and would not have to outlay any cash.

or under a cash transfer scheme.[14]

The focus of the paper is testing these predictions for consumption. The PSID provides several measures of food expenditures—cash outlays for food at home, cash outlays for food away from home, and food bought with food stamps.[15] Clearly food bought with food stamps should increase after the introduction of the program and cash outlays for food at home should decrease. The prediction for spending on meals away from home is ambiguous given the positive income effect (due to the income transfer) and negative substitution effect (due to the reduction in the price of food at home). Given this low income population and high subsidy to food at home, we expect that the FSP will lead to a decrease in meals out. Finally, we expect an increase in total food consumed from all sources—deriving from higher total cash plus food stamp income under food stamps and the distortion of consumption toward food for those consuming at or near the kink point. Unfortunately, the PSID does not provide the data necessary to test the prediction that FSP leads to an increase in nonfood expenditures.[16]

IV. Literature Review

A large literature, mostly using data from more than 20 years ago, focuses on whether the FSP leads to larger increases in food spending than a similar sized cash transfer. The observational studies (summarized in Fraker 1990 and Levedahl 1995) typically estimate the marginal propensity to consume food using the following linear specification (or semi-log or double-log specification):

$$(1) \qquad fspend_i = \beta_0 + \beta_1 cash_i + \beta_2 fstamp_i + Z_i\gamma + \varepsilon_i$$

where $fspend_i$ is expenditure on food for household i, $cash_i$ and $fstamp_i$ are income in cash and from food stamps, respectively, Z_i is a vector of covariates such as household size and age/gender

[14] Beginning in 1971, participants could purchase their food stamp benefits in increments of 0.25, 0.50, 0.75, 1.00 of their full food stamp allotment (MacDonald 1977). They could make these purchases every 2 weeks. This "variable purchase option" allows participating families to obtain a wider range of potential consumption bundles and will in theory reduce the number of families at the kink.

[15] Food bought with food stamps is the value of food bought less the price paid for the stamps. So it is the "benefit" or "bonus value" from participating in food stamps.

[16] Depending on the year, the PSID sometimes includes expenditures on rent, utilities, tobacco and alcohol. Unfortunately, these data are not consistently available in the early years of the PSID.

makeup, and ε_i is a disturbance term. Here the primary impact of food stamps is measured as the increased consumption out of food stamps compared to cash income, as measured by the differences in estimated coefficients by income type in equation (1).

This literature suffers from many of the standard shortcomings of observational studies conducted in the 1970s and 1980s. Importantly, food stamp participation is taken as exogenous and the estimates are identified by comparing food stamp recipients and "similar" non recipients. (Again, the program is a national one so there is no scope to use variation in eligibility and benefits across states.) Standard models of program participation (Currie 2006, Moffitt 1983) show that program participation is a choice variable and—in this case—positively correlated with tastes for food consumption. Critically, then, these naïve comparison between participants and non-participants will *overstate* the impact of the program.[17]

This upward bias seems evident in the literature. Fraker (1990), in his summary of the literature, reports that the estimates of the marginal propensity to consume (MPC) food out of food stamps are two to ten times higher than the estimated MPC food out of cash income. The median study in Fraker's literature review reports a marginal propensity to consume food out of food stamp income that is 3.8 times as large as that from cash income.[18] These findings are often interpreted as evidence that food stamps increase food spending by more than an equivalent cash-transfer system. Our research design takes this early literature to task and identifies the effects by variation in the timing of food stamp introduction across areas rather than the comparison of recipients and nonrecipients.

Because the previous literature soundly supports the idea that MPC out of food stamps is larger than it is out of cash, the USDA conducted a handful of randomized experiments in the early

[17] Other studies compare food stamp recipients to higher-income families who are not eligible for food stamps. These studies rely strongly on the functional form imposed on income, which may fail to adequately account for the Engel curve which predicts that spending on food accounts for a larger share of total spending at very low levels of income. If the nonlinearities in food spending implied by the Engel curve coincide with income from food stamps, then the estimated coefficient on food stamp income could be biased (Whitmore 2002).

[18] The MPC out of cash is estimated to be 0.03-0.17 (with most estimates between 0.05 and 0.10), and the MPC out of food stamps is estimated to be 0.17-0.47.

1990s in which the treatment group received its food stamp benefits in cash. The results of these experiments indicate that spending on food was about 5 percent higher among the group that received benefits paid in stamps (Ohls et al. 1992, Fraker et al. 1992). Schanzenbach (2007) finds that the mean treatment effect is a combination of no difference in food spending among inframarginal recipients, and a substantial shift in consumption toward food for stamp recipients who are constrained. Note, though, that the experimental literature asks a slightly different question than we do in this paper. The experimental literature measures the impact on spending of replacing food stamps with cash while holding total income constant. In this paper, we measure the impact on spending of the introduction of the FSP, which significantly increased total income for recipients.

V. Data

The PSID is a panel data set that began in 1968 with a sample of about 5,000 households, and subsequently all members (and descendants) of these original survey families are re-interviewed annually. The original 1968 sample consists of two subsamples: a nationally representative subsample of 3,000 households and a subsample of 1,900 households selected from an existing sample of low income and minority populations. To adjust for this nonrandom composition, the PSID includes weights designed to eliminate biases attributable to the oversampling of low income groups and to attrition. All results use the weights provided by the PSID.

Beyond the labor market and demographic variables that are the focus of the PSID, the survey includes annual food expenditures for food consumed at home, away from home, and food purchased with food stamps (the value of food purchased less the purchase requirement). These data have been used by many researchers examining impacts of social programs on consumption (for example see Blundell and Pistaferri 2003, Gruber 1997, 2000, and Hubbard et al. 1995). The public-use release of the PSID includes state identifiers. Through special arrangement, we have obtained county identifiers for each family in each year.

We use data from interview years 1968 to 1978. We stop the sample in 1978 so that our

entire analysis period is before the end of the purchase requirement (which occurred in 1979). Our sample excludes 1973 because the food consumption variables were not included in that survey and 1968 because of inconsistencies in the definition of the food variables in that survey.[19] We also exclude observations from Alaska (because of difficulties in matching counties with FSP provision areas) and we trim the sample of a few outliers (observations where the ratio of food spending to income exceeds 0.85, where total annual food expenditures were less than $100 in 2005 dollars or where annual family income was less than $500 in 2005 dollars). A test of sensitivity to these trimmed observations is included in online Appendix Table 3.

There is some ambiguity in what time frame the food variables correspond to. The survey is taken in spring and families are asked about "typical food consumption."[20] The PSID then annualizes this measure and applies it to the prior calendar year. Nonetheless we assume, as other researchers have, that the food spending variables apply to this year (Blundell and Pistaferi 2003, Gruber 1997, 2000, Hubbard et al. 1995, and Zeldes 1989).

Unlike virtually all other U.S. public assistance programs there is no categorical eligibility for the food stamp program. That is, eligibility depends on income and asset tests but it is not targeted on particular demographic groups, such as single parents with children. Table 2 presents food stamp participation rates by education, family type and race based on the 1976-78 PSID (chosen to be after all counties have adopted the program and before the elimination of the purchase requirement). These tabulations show that while food stamp participation is highest among single parent families with children, the participation is widespread across many demographic groups. For example, among families where the head has less 12 years of education, 46 percent of single parent

[19] In 1968, "food assistance" (rather than food stamp benefits) is provided that includes commodity distribution program, food stamp program, and other in-kind benefits. Further, the cost of meals away from home is defined more broadly than in later years and the amounts are bracketed.

[20] Specifically, spending on food at home and food out were first asked about "weekly" (1968-69), then for "last year" (1970-76), then finally settled into "annual spending" starting in 1976. Between 1968 and 1974 respondents were asked to report food stamp receipt "last year," but it is thought that most respondents answered about their current status. As a result, starting in 1975 the question was changed to inquire about food stamp receipt "last month."

families with children, 14 percent of married couples with children, 14 percent of single nonelderly persons with no children, and 10 percent of single elderly participate in food stamps. The rates are uniformly higher among black families, with 56 percent of single nonelderly parent families with children (where the head has less than 12 years of education) participating in food stamps.[21]

We start by measuring impacts of FSP introduction on the sample of all nonelderly families (e.g. head is less than 65) then restrict the subsamples of the data limited to groups with a higher probability of being affected by food stamps (female headed households, nonelderly families and singles headed by a person with 12 or fewer years of education).[22]

Our final samples include 39,623 (family-year) observations for all nonelderly households, 30,905 observations for nonelderly low education households, and 6,002 observations for female headed households. Online Appendix Table 1 presents some basic descriptive statistics for these samples. All dollar amounts are in 2005 dollars and real variables are constructed using separate CPIs for food at home and food away from home. For the nonelderly low education sample, annual food spending (in 2005 dollars) averages $7,914 overall and $7,280 among food stamp recipient families. Food spending is, on average, 19.6 percent of income overall and a much higher 36 percent among food stamp recipients. Among food stamp recipients, food stamp benefits average 33 percent of total food spending suggesting that the typical family is not constrained. In fact, further tabulations show that only 5 percent of food stamp recipients are observed to be constrained (have total food consumption $\leq$ food stamp benefits).

Using county identifiers, we merge the PSID with the FSP policy variables (U.S. Department of Agriculture, various years), 1960 county characteristics (City and County Data Book), and annual per capita county transfers (U.S. Bureau of Economic Analysis, Regional Economic Information System—REIS). The REIS captures all federal transfers to countries which we use to construct three

[21] The participation rates are very similar when tabulated on the larger sample sizes of the Current Population Survey in 1980 (the first year in which food stamp information is available).
[22] We limit the analysis to the nonelderly because of low take up rates among the elderly (Currie 2003, Haider et al. 2003). The results are qualitatively unchanged when we include the elderly.

per capita county transfer variables: *cash public assistance benefits* (Aid to Families with Dependent Children AFDC, Supplemental Security Income SSI, and General Assistance), *medical spending* (Medicare and Military health care), and *cash retirement and disability payments*.

VI. Methodology

We estimate a difference in difference model based on the PSID spanning the period during which the FSP is introduced. In particular, we estimate the following model:

$$y_{ict} = \alpha + \delta FSP_{ct} + X_{it}\beta + \gamma_1 Z_{c60} * t + \gamma_2 TP_{ct} + \eta_c + \delta_t + \lambda_s * t + \varepsilon_{ict} \quad (2)$$

where y_{ict} is the outcome variable (such as the log of total food spending) for family i living in county c in year t. FSP_{ct} is an indicator variable equal to 1 if county c in year t has a FSP program, X_{it} are family characteristics, Z_{c60} are 1960 county characteristics[23], TP_{ct} are the REIS per capita county transfer income variables, η_c are county fixed effects, δ_t are year effects and $\lambda_s * t$ are state-specific linear time trends. We also show results with state-year fixed effects. All estimates are weighted using the PSID family weight and the standard errors are clustered on county (Bertrand et al. 2004).

The controls X include education, race, urban location, state unemployment rate, and in some specifications log of family cash income. As suggested by Currie (2003), X also includes a full set of fixed effects for the number of children and number of adults in the family to control nonparametrically for the differences in food needs across families.

As described above, the food variables in the PSID measure expenditures as of the interview which is fielded in spring of each year. Thus t in (2) above refers to the interview year. Given this timing, we set the treatment variable FSP_{ct} to 1 if county c has a FSP program in place by January of year t.[24]

VII. Results for Expenditures on Food

[23] The variables in Z include the log of the population, the percent of land in farming and the percent of population black, urban, age less than five, age greater than 65 and with income less than $3,000.

[24] There is some evidence (Berry 1984) that it took some time to ramp up the new county FSP programs. We have explored the sensitivity to lagging the treatment effects and while the specific estimates change somewhat, the results are qualitatively similar.

We begin with estimates for the full nonelderly sample and the two subsamples at higher risk to be impacted by the introduction of FSP (households with non-elderly heads with 12 or fewer years of education, and female headed households). We consider four outcome variables: whether the household reported using any food stamps (i.e. whether there was a program), the log of cash (non food stamp) food expenditure at home, a dummy for any meals out, and the log of total (including food stamp) food expenditures. The meals out variable is equal to one if a household reports spending any money on meals out in a typical week. About seventy percent of the low educated, non-elderly sample and just over one-half of single mother families report any spending on meals out (online Appendix Table 1). Total food expenditures includes money spent on food at home, food out, and also includes food purchased with food stamps and the value of any other free meals or food that the household received.

These difference-in-difference results for the three samples are presented in Table 3. The estimates in Panel A show that the introduction of the food stamp program leads to increases in food stamp receipt (as expected).[25] The results in Panels B through D show that FSP introduction leads to increases in total food spending, decreases in propensity to eat out, and mixed results for cash food expenditures. To scale up the results to be per food stamp family (effect of the treatment on the treated) the numbers in italics divide the parameter estimates by the sample mean food stamp participation (Table 2). For female headed households (column 3), FSP introduction leads to a large statistically significant increase in total food expenditures, while the results for all non-elderly low-educated heads are smaller and less precisely estimated. We find no statistically significant results for meals out or cash food expenditure at home in these samples.

Table 4 presents results from placebo tests that have specifications identical to those in Table 3, but on subsamples that are unlikely to be impacted by the FSP. Column (1) presents estimates for high income (more than $50,000 in 2005 dollars) families, column (2) restricts the

[25] We can interpret the coefficient as the effective participation rate. Note that these implied participation rates are somewhat lower than those implied in Table 2 and may be due to a program ramp up period taking place.

sample to high income *married couples with children*, and column (3) restricts the sample further to *white* high income married couples with children. The results, as expected, show no significant impacts of the FSP on the receipt of food stamp benefits or on food expenditures for any of these groups. In fact, here the point estimates on our most robustly estimated outcome—total food expenditures—are small, mostly statistically insignificant and negative compared to the consistently positive estimates for the likely impacted groups.

Event study results

Our identification strategy hinges on the assumption that there are no underlying trends in the counties that are correlated with FSP introduction. This proposition can be evaluated directly in an event study analysis. Specifically, we estimate the following model on the female headed households sample used in column (3) of Table 3:

$$(3) \qquad y_{ict} = \alpha + \sum_{j=-3}^{3} \pi_i 1(\tau_{ct} = j) + X_{it}\beta + \gamma_1 Z_{c60} * t + \gamma_2 TP_{ct} + \eta_c + \delta_t + \lambda_s * t + \varepsilon_{ict}$$

where τ_{ct} denotes the event year, defined so that $\tau = 0$ for consumption that occurs in the same year as the FSP began operation in that county, $\tau = 1$ for consumption one year after the FSP began operation, and so on. In the full sample, τ is set to -3 for all event-years less than or equal to -3, and to +3 for all event-years greater than or equal to 3. For $\tau \leq -1$, food consumption was unaffected by a local food stamp program (i.e. spending was measured before the program started). The coefficients are measured relative to the omitted coefficient ($\tau = -1$).

Our baseline event study model is presented in Figure 7a, and the controls are identical to Tables 3 and 4 and include demographics, unrestricted fixed effects for county and year, county per capita transfers, state-specific linear time trends, and 1960 county characteristics interacted with linear time. This figure uses all available data (6,002 observations) to estimate the time effects and therefore represents an "unbalanced panel" – that is, the composition of the sample varies over event time because not all counties are observed in the PSID for three years before and after FSP is

implemented. Although there appears to be some pre-trending going on in the periods directly prior to the introduction of FSP, afterwards there is a sharp break in the trend and food expenditures are rather flat thereafter.

In order to eliminate possible compositional effects, in Figure 7b we use the standard practice of restricting the sample to a *balanced panel of counties*—specifically we include observations only if their county of residence is observed in the data for three years (or more) pre and post FSP introduction. Because of the timing of the PSID sample, this means that we restrict the sample to counties that adopt FSP after 1971. These results show strong evidence supporting the exogeneity of FSP introduction. First, there pre-trend is very flat—showing no systematic differences in county trends prior to food stamp adoption. Second, food spending increases sharply when the program was introduced. We view this as strong evidence for the validity of our identification strategy, as any confounding factor would have to very closely mimic the timing of county FSP introduction to result in a pattern like this. We also include in Figure 7b the results of the event study that excludes the county controls (REIS transfers *TP* and pre-treatment county characteristics *CB60*t*). The results are nearly identical when we exclude the county trends as controls, providing further evidence of the exogeneity of the treatment.

Triple Difference results

In choosing our preferred sample for this analysis, we face a tradeoff between sample size (using the larger sample of low educated nonelderly families but with overall lower participation rates) and targeting (using the smaller sample female heads of household with higher participation rates). Here, we refine these earlier results by using the nonelderly low educated sample (30,905 observations) but using a triple-difference specification that accounts for different probabilities of being affected by food stamps. In particular, we estimate the following model:

$$(4) \qquad y_{ict} = \alpha + \varphi FSP_{ct} + \delta FSP_{ct} P_g + X_{it}\beta + \gamma_1 Z_{c60} * t + \gamma_2 TP_{ct} + \theta_g + \eta_c + \delta_t + \lambda_s * t + \varepsilon_{ict}$$

To capture the varying risks of being treated we multiply the FSP treatment dummy by a group level food stamp participation rate (as in Bleakley 2007). The group food stamp participation rate Pg comes from Table 2 and is defined for 16 groups using education (<12, 12), race (white, nonwhite), marital status (married, not married) and presence of children (yes, no).[26] In addition to the controls used in Tables 3 and 4, we also include fixed effects for each group θ_g and (although not shown in (4)) interactions of Pg with demographics, CB60*t, *TP*, and year fixed effects. In this triple difference model, the maintained assumption is that there are no differential trends for high participation versus low participation groups within early versus late implementing counties.

The results are presented in Table 5. In column (1), the specification is analogous to that presented in Tables 3 and 4 (includes demographics, the 1960 county characteristics interacted with linear time, state linear time trends, county-level per capita transfer spending, and year and county fixed effects). In column (2) we add controls for interactions between the group participation rates and the other covariates. Note, now that the treatment effect is the interaction of FSP and the group participation rate Pg, the parameter estimates represent impacts for families that take up the program (and no post-estimation scaling needs to be done it already represents the effect of the treatment on the treated).

The first panel finds—as predicted by the theory—a small, negative impact of food stamps on cash (non food stamp) food expenditures at home. The second panel shows a small increase in the propensity to eat meals out (the theoretical prediction here is ambiguous). Both of these findings are imprecisely measured, however, and not statistically significant. In the third panel, the results show that the FSP is associated with a statistically significant and robust 19 (column 2) to 21 (column 1) percent increase in total (including food stamp) food expenditures. Overall, the results in Table 5 are consistent with the theoretical predictions but not always statistically significantly so. The parameter estimates vary little across specifications. Finally, note that across the specifications and outcome

[26] We explored using group level poverty rates rather than FSP participation rates and found very similar results.

variables, the coefficient on the main effect for FSP introduction is close to zero and never statistically significant. This is expected and encouraging, because it reflects the impact of FSP introduction on a group with zero probability of being affected by food stamps (and thus shows the same pattern as the placebo tests in Table 4).

Interestingly, the magnitude of the results for total food expenditure from this triple difference specification is quite similar to—but much more precisely estimated than—the double difference results in Table 3. For example, the results in the first two columns of Table 5 show that FSP introduction leads to a precisely estimated 19 to 21 percent increase in total food expenditures for nonelderly low education sample compared to very imprecisely estimated 16 percent increase for the difference-in-difference estimates for the same sample in Table 3 (column 2).

Implications for the Marginal Propensity to Consume

The results in Tables 3 and 5 show that food consumption responds as predicted by theory. An additional test of the theory is that—if households are mostly inframarginal (not constrained)—then an additional dollar of cash income and an additional dollar in food stamp benefits should lead to the same increase in food expenditures. To explore this hypothesis, we add the log of family cash income to specifications (1) and (2) in Table 5; the results are presented in columns (3) and (4). Our main models do not include a control for income because we do not have an experimentally identified estimate of income.[27] Nonetheless, it is important to point out that our estimates of the impact of FSP are little changed by the inclusion of income (columns 3 and 4).

We use these results to calculate the marginal propensity to consume (MPC) food out of cash income and food stamps. Table 6 presents the estimates and 95 percent confidence intervals of the MPC out of both types of income, and the ratio of the MPC from food stamps to the MPC from cash income.[28] The top panel displays the parameter estimates, while the bottom panel calculates the estimated marginal propensity to consume food. We present results from the low education

[27] We explored various instruments for family income including the BEA per capita transfer variables but these instruments were quite weak with low predictive power.

[28] The MPCs are evaluated at the mean values of food spending and income levels among food stamp recipients.

nonelderly sample (using the triple difference results in Table 5) and the results for female heads of household (using the difference-in-difference results from Table 3).

The results for the nonelderly low education sample indicate an MPC for food out of food stamp income equal to 0.16 and an MPC for food out of cash income of 0.09. The results are consistent with the theoretical predictions—the MPC food out of food stamps is quite close to the MPC food out of cash income, suggesting that most families are inframarginal. The ratio of the point estimates of the MPCs for this sample is estimated to be 1.9. The standard errors imply that we cannot reject that the ratio is equal to one (as implied by theory) and we can reject that the ratio is four or more. This is in stark contrast to the existing literature that finds the MPC food out of food stamps to be as much as ten times larger than the MPC food out of cash income. The results for female-headed families indicate a higher MPC out of food stamps equal to 0.30 and a MPC out of cash income of 0.10, which suggests that a higher proportion of these families are "constrained." This is consistent with the Engel curve relationship, which implies that as the sample becomes more disadvantaged, the MPC food out of cash income increases. The ratio of estimated MPCs is 3.0 among these families, but the 95 percent confidence interval both includes 1.0 and excludes ratios over 5.7 that have been found commonly in the prior literature.

It is worth pointing out that the difference between our results and the previous literature comes fully from our lower estimate of MPC food out of food stamp income, which is the result of our credible research design. The MPC food out of cash income is not identified by our new research design and instead is identified cross sectionally. We are confident, however, in the validity of this estimate for several reasons. First, the estimated MPC food out of income is unchanged when we add family fixed effects to the model (and therefore is identified by changes over time in income). Second, the MPC food out of income (or equivalently the income elasticity of food expenditures) is a widely studied and estimated parameter. Our estimates are squarely in the (quite tight) range of the estimates in a literature that use a wide range of data (cross sectional, time series) and econometric

methods. Reviews of this literature show estimates for the MPC food out of income ranging from 0.03 to 0.17 among studies focused on low income families (Fraker 1990, Ohls and Beebout 1993). Other estimates not limited to low income populations also fall into this range (Deaton and Muellbauer 1980; Souleles 1999, Blanciforti and Green 1983).

While close in magnitude (and certainly much closer than the prior literature suggested), our results show that the estimated MPC food out of in-kind transfers (at 0.16 for the full sample) is larger than the MPC food out of cash income at (0.09). Thus far, we have maintained that the only reason for a higher MPC food out of food stamps compared to the MPC food out of cash income is that households are constrained by the in-kind nature of the program (as illustrated in Figure 5). There are, however, other reasons why the MPCs may differ. The family member with control over food stamp benefits may be different from the person that controls earnings and other cash income. If the person with control over food stamps has greater preferences for food, then we may find that the MPC food out of food stamps is higher than the MPC food out of cash income. Alternatively, it is possible that the in-kind transfer sets a mental target for how much a family "should" spend on food, and as a result alters a family's preferences toward consuming at the budget kink point. Finally, families may perceive that food stamp benefits are a more permanent source of income compared to earnings.

Finally, we are cautious in interpreting these "marginal" calculations (e.g. MPC) for food stamp income because of the nonmarginal nature of the research design – i.e. the design compares spending when there was no program to spending after the program was introduced rather than a continuous measure of food stamp income.

Consumption among families predicted to be "constrained"

A further prediction of the theory is that families with preferences for low food consumption relative to their eligible food stamp allotment will be constrained (as in Figure 5). As a result, these families are then predicted to experience a larger increase food expenditures with food stamp

introduction than other families. We explore this by creating a dummy variable equal to one if a family is predicted to be constrained (PREDCONSTR) and by interacting PREDCONSTR with the main treatment variable. We create PREDCONSTR in two steps. First, we regress food expenditures on demographics (including a rich set of dummy variables capturing the number of persons in the household by age and gender), a cubic in income, and state and year fixed effects. We use this to predict food expenditures for each family. We then compare the predicted food spending to the food stamps the family is eligible for given their family income.[29]

The results, presented in Table 7, are consistent with theory but are estimated imprecisely. The introduction of food stamps leads to larger impacts on total food expenditures for those predicted to be constrained compared to those predicted to be inframarginal. Specifically, the coefficients on the interaction of PREDCONSTR and the treatment effect range between 0.06 and 0.07 depending on the specification, with the main treatment effect a statistically significant 0.15. Thus constrained families increase food consumption by about 22 percent (0.07 + 0.15) compared to 15 percent among those predicted to be inframarginal and 18 percent in the full sample.

Sensitivity Checks

In Table 8 we explore the sensitivity of the results to specification and functional form. We present the specification tests for the triple difference models in Table 5—using the sample of low educated nonelderly household. (The results are qualitatively the same for the female headed sample.) Column (1) restates the results from column (3) from Table 5. In column (2) we include state by year fixed effects (instead of state linear time trends). This makes little difference in the results. The other specifications in Table 8 experiment with the functional form—including food expenditures and cash income in levels (rather than logs), and including a quadratic in income. To compare the results across specifications, we report MPCs out of food stamp and cash income at the bottom of the table. The results are similar across specifications, although imposing linearity on the

[29] Predicted food stamp levels are based on the 1968 food stamp benefits schedule for a family of three, reproduced in Kotz (1969). This is the only schedule we could locate. Benefits were predicted for other family sizes based on the ratios of the 1968 poverty thresholds by family size.

relationship between cash income and food spending tends to reduce the MPC out of cash income.

The expansion of the FSP took place during a time of great change in the U.S. system of government support. We address this in the main results by controlling for the county transfer variables. Another, more direct approach, is to examine the impact of the FSP on PSID reported government transfer income. In particular, with the PSID we can measure income of the head and wife from AFDC, other welfare income (SSI, General Assistance), and Social Security. The results of that exercise, presented in online Appendix Table 2, show no significant impact of the FSP on other sources of income support.[30]

In sum, the results in this section show that the food stamp program is associated with increases in total food consumption and (less consistently) decreases in out-of-pocket food spending. The results are robust to including state linear time trends, state-year fixed effects, and do not appear to be the result of other program expansions during this time period.

VIII. Conclusion

In this paper we present evidence on the effect of the largest near-cash transfer program – food stamps – on the consumption of the poor. The overall program effects of the FSP have been difficult for researchers to isolate because there is little cross-state or over-time program variation to exploit. Here we use county variation in the adoption of the program from 1963-1975 to identify the impact of food stamps. Using the PSID, we find that the FSP significantly increases food expenditures. By introducing and developing this research design, the paper provides an important contribution to the literature on evaluating the behavioral impacts of transfer programs.

Perhaps more importantly, the paper tests the economics of in-kind transfers. Economic theory has strong predictions about how consumers will reallocate their spending in response to in-kind transfers. Despite the well-known theoretical predictions, there has been relatively little empirical work to test those predictions. Our results—while mixed in statistical significance—are

[30] The results are robust to many other specification checks. Online Appendix Table 3 shows that the results are little changed by adding back in the trimmed observations or dropping allocated observations. The results are also robust to adding family fixed effects.

uniformly consistent with theoretical predictions. First, the poor react to in-kind transfers by reducing their out-of-pocket spending on the targeted good (although these results are not statistically significant). Second, total consumption of the targeted good from all sources—cash outlays and in-kind transfers—increases. Third, providing food stamp benefits in voucher form leads to a minimal distortion of the consumption choice relative to what it would be if the benefit were provided in cash.

Our findings, then, contribute to the literature in two distinct ways. First, we provide important and new estimates of the impact of the food stamp program on consumption using a credible research design. Second, we test predictions of the theory of consumer choice and in particular the differential impacts of cash versus in kind transfers.

Even though there have been changes in the population of the U.S. and the parameters of the FSP since the period we are studying, these results are relevant for today's policy debates. To date, there have been no studies that we have found that provide credible evidence on the impact of the FSP on consumption and income. The FSP (recently re-named to be the "Supplemental Nutrition Assistance Program") is once again receiving considerable political attention, and it is crucial from a policy maker's perspective to be able to measure the benefits of the program not only on food spending, but also on other outcomes like income, child well-being, and health.

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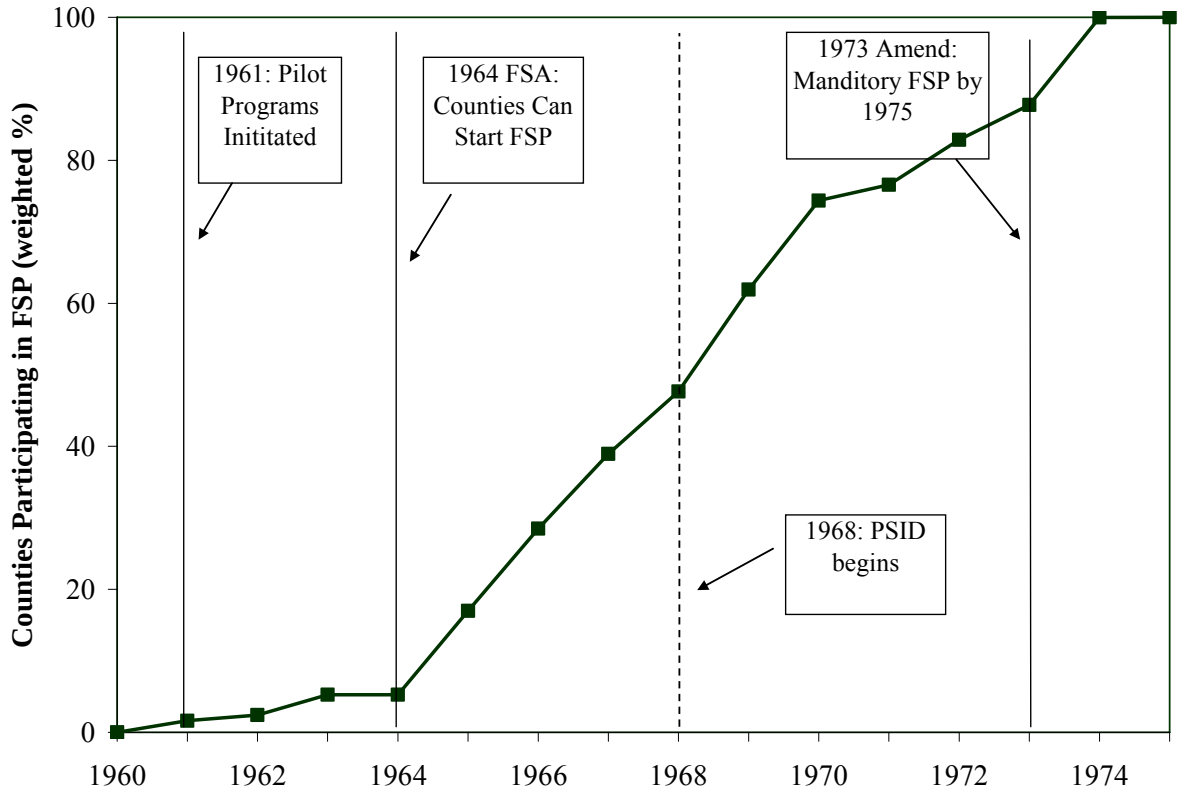
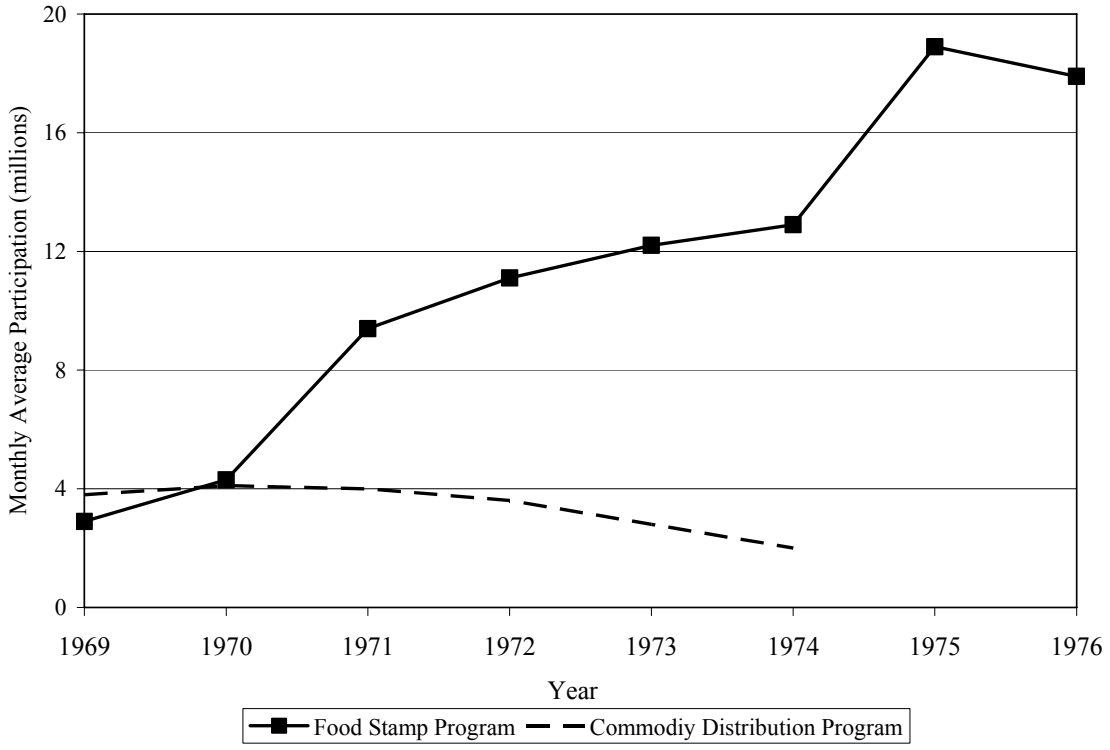
Figure 1: Cumulative Percent of Counties with Food Stamp Program, 1960-1975



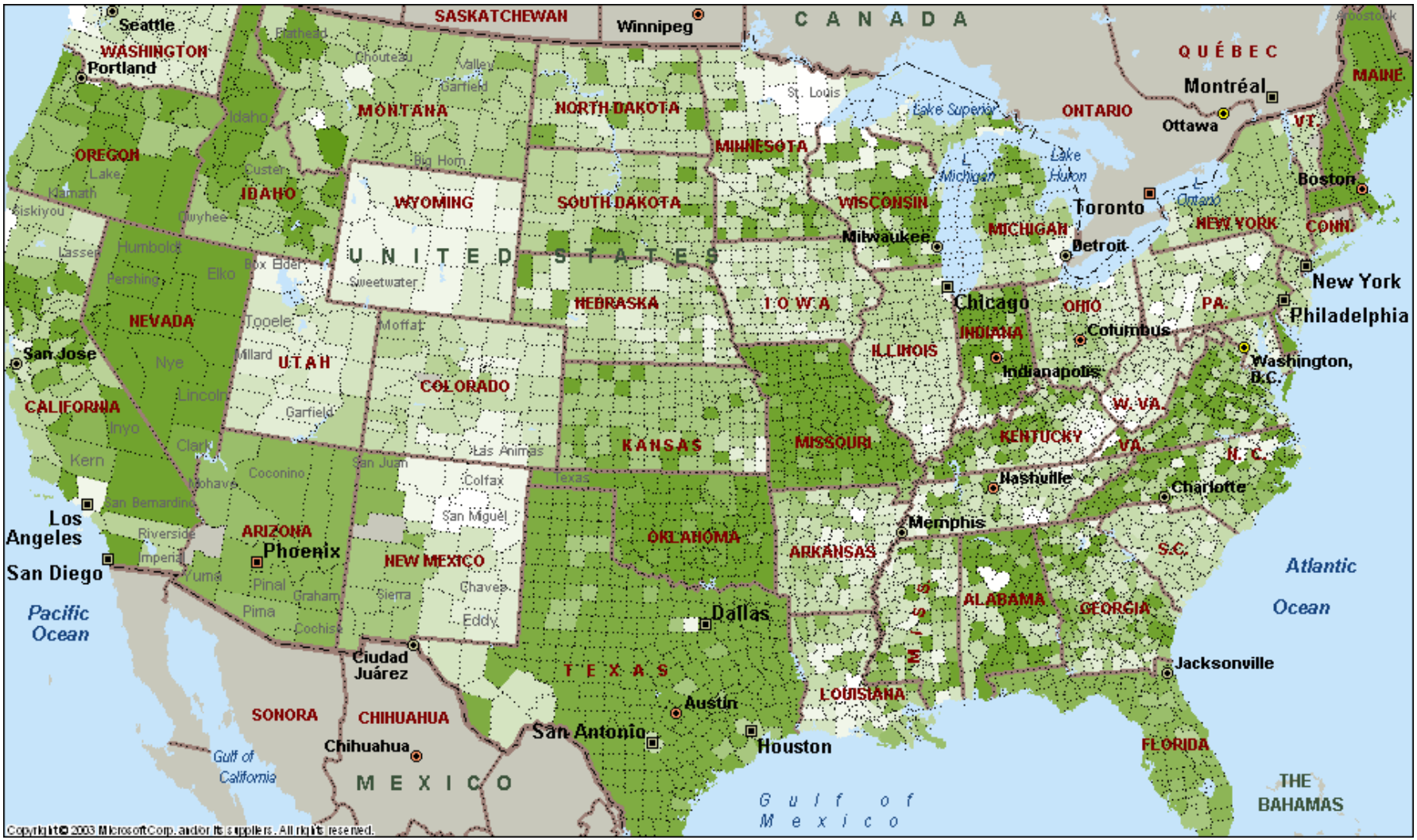
Source: Author's tabulations of county FSP start dates. Counties are weighted by their 1960 population.

Figure 2: Food Assistance Program Participation, 1968-1976



Source: Berry (1984), Table 3.

Figure 3: Food Stamp Program Start Date, By County (1961-1975)



Source: Authors' tabulations of food stamp administrative data (U.S. Department of Agriculture, various years).

Figure 4: Trends in Weighted Percent of Counties with FSP, Selected States

Source: Authors' tabulations of food stamp administrative data (U.S. Department of Agriculture, various years). Counties weighted by 1960 population.

Figure 5: Food Stamps and Food/Non-Food Consumption: No Purchase Requirement

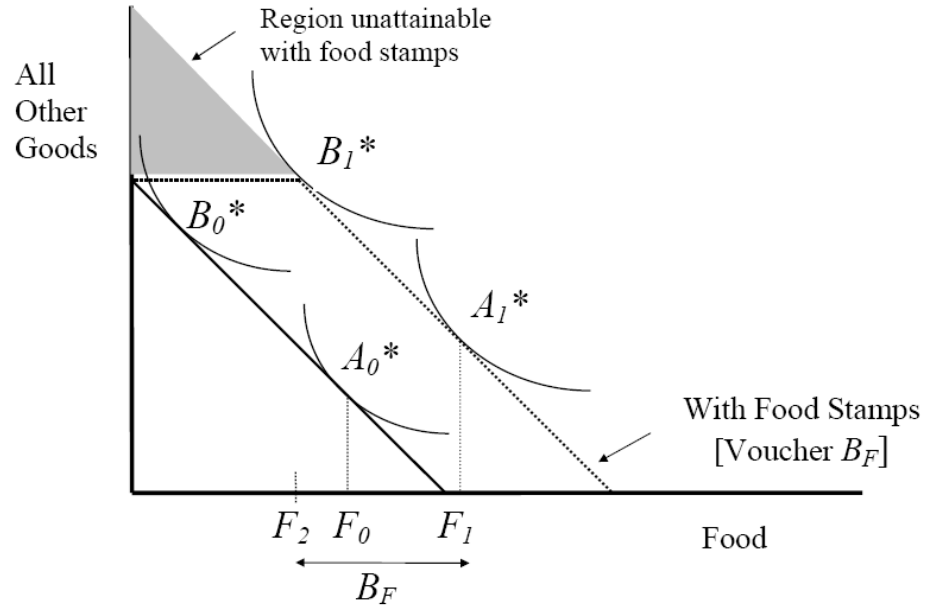


Figure 6: Food Stamps and Food/Nonfood Consumption: With Purchase Requirement

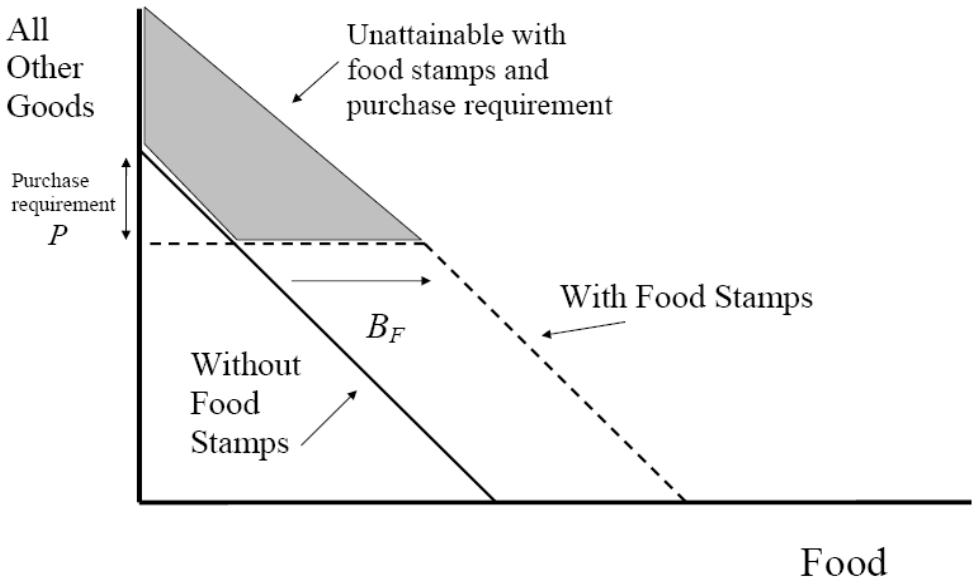


Figure 7a
Event Study Estimates of Impact of FSP on Total (including food stamp) Food Expenditures
Unbalanced Panel

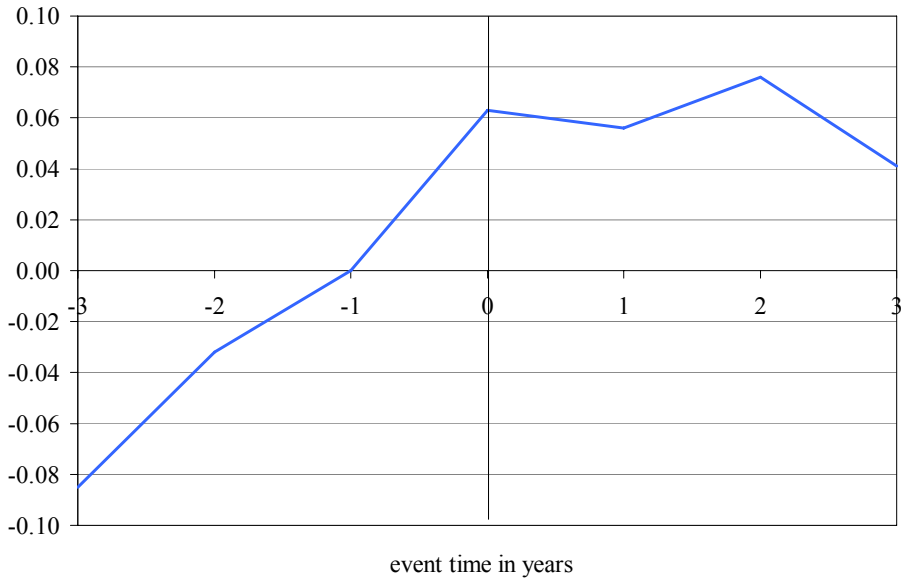
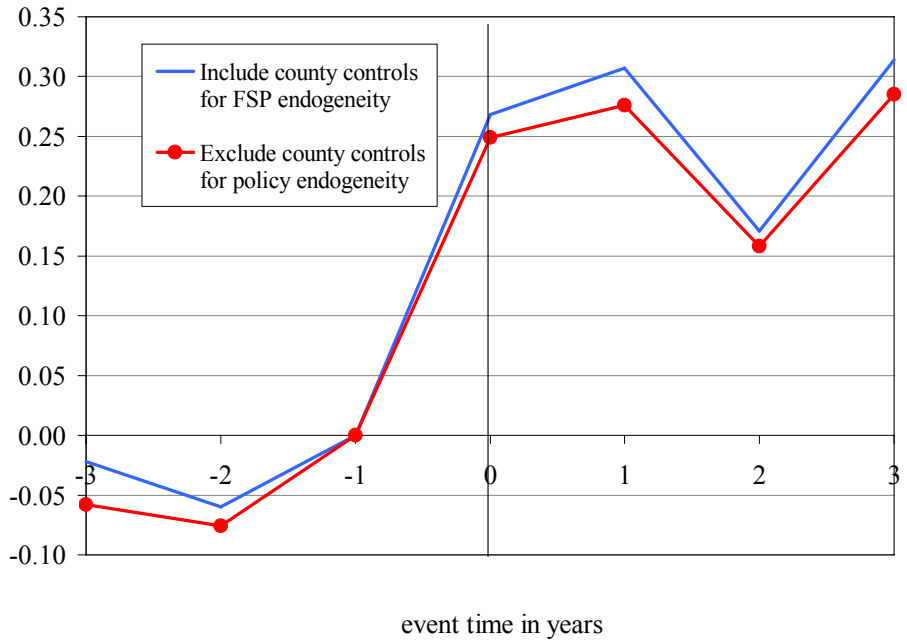


Figure 7b:
Event Study Estimates of Impact of FSP on Total (including food stamp) Food Expenditures
Balanced Panel



Notes: Each figure plots coefficients from an event-study analysis. Coefficients are defined as years relative to the year the Food Stamp Program is implemented in the county. Figure 7a includes the entire female headed household sample and Figure 7b is a balanced county sample, where observations in a county are included only if there are 3 years pre- and post- implementation data. The specification includes controls for demographics, unrestricted fixed effects for county and year, county per capita transfers, state-specific linear time trends, and 1960 county characteristics interacted with linear time.

Table 1: Determinants of County Level Food Stamp Program Start Date
Analysis Using the 1960 City and County Data Book

	All Counties		Limiting to post-pilot counties	
	(1)	(2)	(3)	(4)
Percent of land in farming	-0.025	0.124	0.114	0.136
	(0.830)	(0.028)***	(0.027)***	(0.033)***
Percent of pop with income<$3000	0.005	-0.544	-0.347	0.085
	(0.050)	(0.092)***	(0.088)***	(0.147)
Percent of pop urban	0.214	-0.068	-0.040	-0.001
	(4.36)**	(0.041)	(0.039)	(0.053)
Percent of pop black	-0.326	-0.208	-0.212	-0.474
	(4.36)**	(0.070)***	(0.067)***	(0.145)***
Percent of pop age < 5	-3.566	-2.329	-2.954	-3.557
	(4.92)**	(0.625)***	(0.593)***	(0.786)***
Percent of pop age > 65	-1.030	-0.982	-1.133	-3.048
	(2.49)*	(0.390)**	(0.371)***	(0.524)***
log population	-11.229	-9.139	-7.819	-7.335
	(13.44)**	(0.752)***	(0.718)***	(0.932)***
South * % of land in farming				-0.125
				(0.058)**
South * % pop with income<$3000				-0.603
				(0.188)***
South * % pop urban				-0.110
				(0.080)
South * % pop black				0.373
				(0.165)**
South * % pop age < 5				0.787
				(1.222)
South * % pop age > 65				3.467
				(0.754)***
South * log population				0.645
				(1.548)
State Fixed Effects		X	X	X
Number of Observations	2,957	2,957	2,939	2,939
R squared	0.14	0.56	0.55	0.56

Notes: The data are at the county level and the dependent variable is equal to the calendar month (normed to 1 in January 1961) that the county began offering the Food Stamp Program. The control variables come from the City and County Data Book for 1960. Alaska counties are dropped due to missing data on the food stamp program. Very small counties (with population less than 1,000) are dropped because of missing data on some control variables. A small number of counties are dropped because the variable *percent of land in farming* exceeds 100 percent. Estimates are weighted using the 1960 county population.

Table 2
Food Stamp Participation Rates by Demographic Group

		Education Group		
	All	Less than HS	High School Grad	More than HS
A. All Races				
All family types	0.08	0.14	0.06	0.02
Single with children	0.32	0.46	0.23	0.15
Married with children	0.07	0.14	0.06	0.01
Single, no children	0.07	0.14	0.05	0.03
Married, no children	0.02	0.04	0.01	0.01
Single, no children elderly	0.07	0.10	0.03	0.01
Married, no children elderly	0.03	0.05	0.00	0.00
B. White				
All family types	0.05	0.10	0.04	0.02
Single with children	0.22	0.38	0.14	0.07
Married with children	0.05	0.12	0.05	0.01
Single, no children	0.05	0.11	0.04	0.03
Married, no children	0.01	0.03	0.00	0.01
Single, no children elderly	0.05	0.07	0.02	0.01
Married, no children elderly	0.02	0.04	0.00	0.00
C. Nonwhite				
All family types	0.22	0.28	0.18	0.09
Single with children	0.51	0.56	0.44	0.43
Married with children	0.16	0.22	0.14	0.03
Single, no children	0.13	0.20	0.09	0.04
Married, no children	0.06	0.10	0.02	0.02
Single, no children elderly	0.24	0.25	0.11	0.00
Married, no children elderly	0.10	0.13	0.00	0.00

Notes: Weighted means of food stamp participation rates using families in the 1976-1978 Panel Study of Income Dynamics. These years were chosen because by 1976 all counties had implemented food stamp programs yet it was before the elimination of the purchase requirement in 1979.

Table 3
Impact of Food Stamp Introduction on Family Food Expenditures
Difference in Difference Models

	All nonelderly households (1)	Nonelderly, Head Educ<=12 (2)	Female Heads (3)
A. Any Food Stamps (0/1)			
County FSP Implemented	0.035	0.050	0.194
	(0.007)***	(0.009)***	(0.040)***
Number of Observations	39,243	30,905	6,002
R Squared	0.22	0.25	0.40
B. Log of Cash (non food stamp) Food Expenditures at Home			
County FSP Implemented	-0.006	-0.008	0.042
	(0.016)	(0.019)	(0.055)
	-0.081	*-0.078*	*0.116*
Number of Observations	39,243	30,541	5,788
R Squared	0.55	0.53	0.45
C. Any Meals Out (0/1)			
County FSP Implemented	-0.005	-0.003	-0.055
	(0.015)	(0.019)	(0.048)
	-0.068	*-0.029*	*-0.152*
Number of Observations	39,623	30,905	6,002
R Squared	0.26	0.25	0.38
D. Log of Total (including food stamp) Food Expenditures			
County FSP Implemented	0.007	0.016	0.102
	(0.013)	(0.016)	(0.042)**
	0.095	*0.157*	*0.282*
Number of Observations	39,623	30,905	6,002
R Squared	0.52	0.51	0.49
Demographics	X	X	X
1960 Cty Vars * Linear Time	X	X	X
Year and County Fixed Effects	X	X	X
Per Capita Cty Transfers	X	X	X
State x Linear Time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variable on a dummy variable equal to 1 if the county-year observation had a food stamp program in place by January of that year. The samples include 1969-72 and 1974-78, and exclude observations from Alaska and observations with unusual expenditure values (annual food expenditures less than $100, annual family income less than $500, or income share on food greater than 0.85). For details on this sample selection, see text. Estimation samples include all nonelderly families (column 1), nonelderly families with head education less than or equal to 12 (column 2), and female headed households (column 3). All outcome variables correspond to annual measures taken as of the interview (in spring of the interview year). Demographic controls include dummies for education, number of children, and number of adults, race, urban location and state unemployment rate. 1960 county variables include log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000 each interacted with a linear time trend. Per capita county transfer income include measures for public assistance (AFDC, General Assistance), medical care (Medicare, Medicaid, military), and retirement and disability benefits. Estimates are weighted using the PSID weight and clustered on county. Standard errors are in parentheses and ***, **, and * indicate that the estimates are significant at the 1%, 5% and 10% levels. The numbers in italics inflate the parameter estimate by the sample food stamp participation rate in 1978.

Table 4
Impact of Food Stamp Introduction on Family Food Expenditures
Difference-in-Difference Estimates on Placebo groups

	All high income households (1)	High Income Married w/Children (2)	White High Income Married w/Children (3)
A. Any Food Stamps (0/1)			
County FSP Implemented	0.005	0.004	0.003
	(0.003)	(0.004)	(0.003)
Number of Observations	16,796	9,813	1,947
R Squared	0.10	0.12	0.28
B. Log of Cash (non food stamp) Food Expenditures at Home			
County FSP Implemented	0.006	-0.001	-0.036
	(0.015)	(0.019)	(0.038)
Number of Observations	16,786	9,807	1,947
R Squared	0.56	0.46	0.63
C. Any Meals Out (0/1)			
County FSP Implemented	-0.029	-0.009	-0.017
	(0.018)	(0.021)	(0.038)
Number of Observations	16,796	9,813	1,947
R Squared	0.20	0.24	0.28
D. Log of Total (including food stamp) Food Expenditures			
County FSP Implemented	-0.004	-0.021	-0.067
	(0.015)	(0.020)	(0.038)*
Number of Observations	16,796	9,813	1,947
R Squared	0.46	0.43	0.61
Demographics	X	X	X
1960 Cty Vars * Linear Time	X	X	X
Year and County Fixed Effects	X	X	X
Per Capita Cty Transfers	X	X	X
State x Linear Time	X	X	X

Notes: Each parameter is from a separate regression of the outcome variable on a dummy variable equal to 1 if the county-year observation had a food stamp program in place by January of that year. The samples include 1969-72 and 1974-78, and exclude observations from Alaska and observations with unusual expenditure values (annual food expenditures less than $100, annual family income less than $500, or income share on food greater than 0.85). For details on this sample selection, see text. Estimation samples include high income (>$50,000 in 2005 dollars) families (column 1), high income married couples with children (column 2), and high income white married couples with children (column 3). All outcome variables correspond to annual measures taken as of the interview (in spring of the interview year). Demographic controls include dummies for education, number of children, and number of adults, race, urban location and state unemployment rate. 1960 county variables include log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000 each interacted with a linear time trend. Per capita county transfer income include measures for public assistance (AFDC, General Assistance), medical care (Medicare, Medicaid, military), and retirement and disability benefits. Estimates are weighted using the PSID weight and clustered on county. Standard errors are in parentheses and ***, **, and * indicate that the estimates are significant at the 1%, 5% and 10% levels.

Table 5: Impact of Food Stamp Introduction on Family Food Expenditures
Triple Difference Estimates for Nonelderly Low Education Sample

	Nonelderly, Head Education <=12			
	(1)	(2)	(3)	(4)
A. Log of Cash (non food stamp) Food Expenditures at Home				
County FSP Implemented x Pg	-0.043	-0.073	-0.081	-0.114
	(0.105)	(0.108)	(0.101)	(0.115)
County FSP Implemented	0.000	0.005	0.009	0.013
	(0.022)	(0.022)	(0.021)	(0.022)
Number of Observations	30,541	30,541	30,541	30,541
R Squared	0.54	0.54	0.58	0.58
B. Any Meals Out (0/1)				
County FSP Implemented X Pg	0.101	0.109	0.079	0.084
	(0.101)	(0.099)	(0.101)	(0.099)
County FSP Implemented	-0.014	-0.014	-0.009	-0.008
	(0.022)	(0.022)	(0.022)	(0.021)
Number of Observations	30,905	30,905	30,905	30,905
R Squared	0.26	0.26	0.29	0.29
C. Log of Total (including food stamp) Food Expenditures				
County FSP Implemented X Pg	0.208	0.187	0.169	0.143
	(0.096)**	(0.101)*	(0.090)*	(0.097)
County FSP Implemented	-0.005	-0.003	0.004	0.007
	(0.020)	(0.020)	(0.019)	(0.020)
Number of Observations	30,905	30,905	30,905	30,905
R Squared	0.52	0.52	0.58	0.58
Demographics	X	X	X	X
1960 Cty Vars * Linear Time	X	X	X	X
Per Capita Cty Transfers	X	X	X	X
Log(Real Family Income)			X	X
Group Fixed Effects	X	X	X	X
Year Fixed Effects (main and x Pg)	X	X	X	X
County Fixed Effects	X	X	X	X
State x Linear Time	X	X	X	X
Pg x Other Covariates (except area fixed effects)		X		X

Notes: Each parameter is from a separate regression of the outcome variable on the Food Stamp implementation dummy multiplied by a group food stamp participation rate. The food stamp implementation dummy equals one if the county-year observation had a food stamp program in place by January of that year. The group food stamp participation rate is calculated for each education-race-marital status-presence of children cell using the 1976-1978 PSID. The sample includes 1969-72 and 1974-78 and excludes observations from Alaska and observations with unusual expenditure values (annual food expenditures less than $100, annual family income less than $500, or income share on food greater than 0.85). For details on this sample selection, see text. Estimation sample includes all households (singles and families) where the head is nonelderly and has a high school education or less. All outcome variables correspond to annual measures taken as of the interview (in spring of the interview year). Demographic controls include dummies for education, number of children, and number of adults, race, urban location and state unemployment rate. 1960 county variables include log of population, percent of land in farming, percent of population black, urban, age<5, age>65 and with income less than $3,000 each interacted with a linear time trend. Per capita county transfer income include measures for public assistance (AFDC, General Assistance), medical care (Medicare, Medicaid, military), and retirement and disability benefits. Estimates are weighted using the PSID weight and clustered on county. Standard errors are in parentheses and ***, **, and * indicate that the estimates are significant at the 1%, 5% and 10% levels.

Table 6
Estimated Marginal Propensities to Consume Food Out of Food Stamps and Cash Income

	Nonelderly, head<=12 yrs of ed		Female headed households	
	Table 5 Column (1)	Table 5 Column (3)	Table 3 Column (3)	
Parameter Estimates				
County FSP Implemented	0.208 (0.096)**	0.169 (0.090)*	0.102 (0.042)** *0.282*	0.095 (0.042)** *0.262*
Log of real family income	--	0.290 (0.009)***	--	0.289 (0.019)*** *0.798*
Estimated Marginal Propensity to Consume Food				
MPC_f out of Food Stamps	0.200 *[0.02, 0.38]*	0.163 *[-0.01, 0.33]*	0.318 *[0.05, 0.58]*	0.296 *[0.04, 0.55]*
MPC_f out of Cash Income	--	0.087 *[0.08, 0.09]*	--	0.098 *[0.08, 0.11]*
Ratio of MPCs	--	1.9 *[-0.11, 3.83]*	--	3.0 *[0.33, 5.71]*
Observations	30,905	30,905	6,002	6,002

Notes: Each column reports results from a separate regression of the log of total food expenditures on the FSP treatment, the log of real family income (for columns 2 and 4), demographics, county variables, state-linear time, and county and year fixed effects. The results in columns (1)-(2) are from the triple-difference estimates for nonelderly low education sample in Table 5 (the county FSP treatment is multiplied by Pg, the participation rate). The results in columns (3)-(4) are from the difference-in-difference estimates for female heads of household in Table 4 (numbers in italics scale up the coefficients to reflect the impact per food stamp participant family). See the notes to tables 3 & 5 for more details. Standard errors are in parentheses and ***, **, and * indicate that the estimates are significant at the 1%, 5% and 10% levels. Estimates are weighted using the PSID weight and clustered on county. The marginal propensities to consume food are evaluated at mean values for food expenditures and family income *among food stamp recipient families*. The figures in [ ] are 95 percent confidence intervals around the estimated MPC.

Table 7
Impact of Food Stamp Introduction on Family Food Expenditures, Nonelderly Low Education Sample
Interactions between Treatment and Predicted as Constrained

	Nonelderly, Head Education <=12		
	(1)	(2)	(2)
FSP Treatment	0.180	0.151	0.148
	(0.093)*	(0.077)*	(0.077)*
FSP Treatment x PREDCONSTR		0.059	0.073
		(0.059)	(0.058)
PREDCONSTR		0.005	0.008
		(0.014)	(0.024)
Cubic in income	yes	yes	yes
Observations	30,905	30,905	30,905
R-squared	0.57	0.58	0.58
Demographics	X	X	X
1960 Cty Vars * Linear Time	X	X	X
Per Capita Cty Transfers	X	X	X
Group Fixed Effects	X	X	X
Year Fixed Effects (main and x Pg)	X	X	X
Year Fixed Effects x constrained			X
County Fixed Effects	X	X	X
State x Linear Time	X	X	X

Notes: Each column represents estimates from a separate regression on the sample of nonelderly low educated households (see notes to Table 5 for details). Column 1 takes the specification in Table 5, column 1 and replaces log income with a cubic in income. Column 2 adds a main effect for predicted to be constrained (e.g. eligible food stamp benefit >= predicted food expenditures) and the interaction of predicted to be constrained with FSP treatment. Column 3 allows for year effects to differ between those predicted to be constrained and those predicted to be unconstrained. For details on predicting constrained status see text. Estimates are weighted using the PSID weight and clustered on county. Standard errors are in parentheses and ***, **, and * indicate that the estimates are significant at the 1%, 5% and 10% levels.

Table 8
Impact of Food Stamp Introduction on Family Food Expenditures
Sensitivity to Alternative Functional Form: All nonelderly singles and families with head education <= 12

	Main Estimates (log-log)			semi-log	linear	quadratic
Specification for Dep. Var.	log(total food)		log(total food)	total food exp	total food exp	total food exp
Specification for Income	--		log(income)	log(income)	income	quad income
County FSP Implemented X	0.208	0.238	0.169	1,512	1,513	1,499
Group participation rate	(0.096)**	(0.097)**	(0.090)*	(762)**	(767)**	(764)*
County FSP Implemented	-0.005	-0.024	0.004	-37	-56	-48
	(0.020)	(0.022)	(0)	(147)	(148)	(148)
log(income)	--	--	0.290	1,875		
			(0.009)***	(68)***		
income					0.038	0.051
					(0.002)***	(0.003)***
income squared (/$10,000)						-0.001
						(0.000)***
Number of Observations	30,905	30,677	30905	30905	30905	30905
R squared	0.52	0.52	0.58	0.56	0.57	0.56
Estimated MPC food stamps	0.200	0.228	0.163	0.199	0.199	0.197
Estimated MPC income	--	--	0.087	0.077	0.038	0.048
Demographics	X	X	X	X	X	X
1960 Cty Vars * Linear Time	X	X	X	X	X	X
Per Capita Cty Transfers	X	X	X	X	X	X
Group Fixed Effects	X	X	X	X	X	X
Year Fixed Effects (main and x Pg)	X	X	X	X	X	X
County Fixed Effects	X	X	X	X	X	X
State x Linear Time	X		X	X	X	X
State x Year fixed effects		X				

Notes: Each column presents estimates from a regression of total food expenditures on a dummy variable equal to 1 if the county-year observation had a food stamp program in place by January of that year interacted with the group participation rate. The models differ with respect to the functional form (logs or levels for dependent variable, logs or levels for income) and whether they include state-year fixed effects. All other control variables and sample selection correspond to specification (1) in Table 5. See the notes to that table for more details. Standard errors are in parentheses and ***, **, and * indicate that the estimates are significant at the 1%, 5% and 10% levels. Estimates are weighted using the PSID weight and clustered on county. The marginal propensities to consume food are evaluated at mean values for food expenditures and family income among food stamp recipient families.