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Abstract

Over the last decade, a body of work studying the rollout of the safety net in the U.S.—with a focus in particular on programs which were created or greatly expanded during the War on Poverty—has given us evidence about short-run and long-run positive effects of these programs. In particular, papers focused on the rollout of the Food Stamp Program compare outcomes in counties where the program was implemented earlier versus later, controlling for national shocks and time-invariant differences across locations (e.g., Hoynes, Schanzenbach, and Almond, 2016). We take advantage of this same variation in the timing of food stamp adoption and combine it with rich administrative data on earnings, employment, and involvement with the Social Security Disability Insurance program (SSDI). Thus, our key independent variable is the share of time an adult was exposed to the Food Stamp Program from conception through age 5. Our dependent variables are administrative measures of earnings and involvement with the SSDI system. Restricting ourselves to natives born between 1955 and 1980 where we can link their place of birth to the data on the rollout of food stamps, we have a sample of nearly 1 million individuals. We model outcomes as a function of dummy variables for year of birth, county of birth, for being white, and where relevant, for age, and stratify by gender. For women, living in a county where food stamps were available for the entire time from conception through age 5 leads to an increase in total earnings of around 3% at age 32 but has no effect on employment. Effects for men are more varied. There is no impact on use of the SSDI system for either gender across the range of years we have data. These findings suggest important positive long-run effects of the Food Stamp Program.

1 Introduction

Exposure to adverse conditions has been shown to have both immediate and long-run effects. Experiencing adverse conditions or negative shocks during critical times for child development, typically thought of lasting from conception through age five, can have particularly long-lasting effects. In fact, a large literature in economics, sociology, public health, and epidemiology finds that negative early life shocks result in lower health, education, and labor market outcomes (see reviews in Almond and Currie, 2011; Currie and Almond, 2011; Almond, Currie, and Duque, 2018; and Currie and Rossin-Slater, 2015).

To alleviate adverse conditions, the federal government has created numerous means-tested social assistance programs as well as social insurance programs, collectively referred to as the social safety net, with the goal of improving the lives of those who experience adverse economic conditions. Much of the literature surrounding the effects of these programs has focused on the immediate short-run impact of these programs on health, education, and recipient's labor-market outcomes. However, understanding the effects of exposure, particularly in early childhood, to these government programs designed to alleviate adverse conditions is crucial for assessing the costs and benefits associated with these programs. This is particularly important when considering effects on human-capital accumulation and program participation which may not materialize until adulthood. Moreover, because the social safety net, in many cases, explicitly provide benefits to children, these programs can be viewed as investments in human capital. Assessing the potential returns on investment of these programs in terms of later-life labor market outcomes and receipt of government benefits has important implications for federal policymakers. We provide new evidence on the effect of exposure to the Food Stamp Program (FSP) in early childhood on earnings and program participation as an adult. Specifically, we examine the number of months from conception (nine months prior to birth) through age five that an individual's county of birth had the FSP in place.

We use the plausibly exogenous variation created by the initial implementation of the FSP to identify the effect of exposure to food stamps in early childhood on later life earnings and involvement with the Social Security Disability Program. The FSP was implemented in different counties at different times, creating cross-sectional and time-series variation in individual exposure to food stamps. Prior research has shown the variation created by the initial implementation of the FSP to be plausibly exogenous to county characteristics (Hoynes and Schanzenbach, 2009, 2012; Almond, Hoynes, and Schanzenbach, 2011). Additional work has used this rollout variation to examine the effect of exposure to the FSP on infant health (Almond, Hoynes, and Schanzenbach, 2011) and labor supply (Hoynes and Schanzenbach,

2012).

We contribute to the literature on the long-run effects of the social safety net by providing new estimates using a large, unique sample of administrative data and documenting the differential effects of the FSP in counties that had a predecessor program to the FSP known as the Commodity Distribution Program (CDP). Our large, unique sample allows us to match individual's place of birth from the NUMIDENT file to administrative data on their entire earnings history and interactions with the Social Security Disability Insurance (SSDI) program. Thus, our data include labor market and social insurance participation data for approximately 1 million individuals. With this large data set, we are able to detect effects that would likely be missed in smaller survey data sets, and have outcomes which are well-measured and do not suffer from reporting errors, which has been documented to be considerable in self-reported data on earnings (Bollinger, Hirsch, Hokayem, and Ziliak, forthcoming) and SSDI (Meyer, Mok, and Sullivan, 2015).

Our study builds on the work of Hoynes, Schanzenbach, and Almond (2016), who established that early exposure to the FSP led to improvements in health for adults and in economic self-sufficiency for women. Hoynes, Schanzenbach, and Almond's (2016) analysis, however, relies on a relatively small sample from the Panel Study of Income Dynamics (PSID). Their PSID data include self reports of earnings and program participation. In using our large, unique administrative data, we are able to overcome these issues. In addition to providing new estimates on the long-run effects of the FSP on earnings, employment, and social insurance program participation, we document and account for the widespread prevalence of the Commodity Distribution Program before the FSP was implemented. Because the CDP existed in about 90 percent of counties prior to the adoption of the FSP, for most counties the relevant pre-Food Stamp Program counterfactual is one where there is a possibly inefficient commodity program rather than no food assistance program. Regardless, the presence of the CDP in one's county meant that county officials were already determining CDP eligibility for families not receiving AFDC or old age/disability assistance. Since CDP eligibility rules were usually harmonized with food stamp rules, this meant counties could more easily make the transition to determining eligibility for the new Food Stamp Program when they implemented it.

We find that exposure to food stamps in your county of birth from conception through increases earnings for women at age 32 by approximately three percent or around $700 (in real 2015 $). Data from the 1979 Current Population Survey suggest nearly 1 in 5 children under age 6 were in households where someone was getting food stamps. If participation in food stamps were the only outcome affected by food stamp rollout, then this would suggest a treatment effect on the treated of around 1/6. These results are robust to adding

many controls, including state of birth or county of birth linear time trends, population in the county before the FSP, and county spending on the safety net at birth or from birth through age 18. They also hold when we look at earnings from ages 32 to 34, and point estimates for the effects on the percentiles of earnings within birth cohort are also positive and of a similar magnitude. While one might expect the effects of implementing the FSP would be larger when there was no other food program than when the CDP was present, we find the opposite. We find slightly larger effects in counties that had a CDP before the implementation of a FSP. We attribute this to the presence of infrastructure for getting the FSP eligibility determination process "up and running" based on the preexisting CDP along with anecdotal evidence about the low quality and inconsistency of the food provided by the CDP (e.g., U.S. Senate Select Committee on Nutrition and Human Needs, 1971). Effects on men's earnings are more varied and often insignificant. In addition, we find no effects on employment or involvement with SSDI for either gender. Our findings are consistent with the contemporaneous work of Bailey, Hoynes, Rossin-Slater, and Walker (2019), who use administrative data on place of birth linked to data from the decennial Census and the American Community Survey. Taken together, these findings suggest important positive long-run effects of the FSP.

2 Conceptual Framework

Our study tests key implications of models of the technology of skill formation. Both the persistence of early-life and childhood exposure to positive or negative experiences and their interactions with human capital investments can be explained by models of the technology of skill formation (Cunha and Heckman, 2007; Heckman and Mosso, 2014; hereafter HM). The technology of skill formation framework emphasizes that human capital formation is dynamic across the life course, meaning that impacts on future skills and investments depend on earlier skills and investments. In this framework, the notion that "skill begets skill" interacts with family and environmental influences and investments to determine the formation of skills across the life course.

HM develop this framework in more detail and derive important predictions which inform our analysis and explain why early exposure to food stamps might impact long-run outcomes. First, skills are self-productive, which means that the level of future skills depends on the level of past skills (Cunha and Heckman, 2007; HM). Embedded in this concept is also the idea that skills in one domain (i.e., cognition) may also influence future skills in other domains (i.e., non-cognitive or socio-emotional skills). This self-productive notion implies that a positive or negative shock at an influential stage of human capital formation will not only

affect a child's skills in that period, but will also influence subsequent accumulation of skills. Second, HM argue that although the relationship between prior and current skills is positive, it is less and less positive the higher the level of prior skills. This feature, called diminishing self-productivity, provides support for making substantial compensatory investments in early life among the high-risk population. Third, current skills and investments interact to produce subsequent skills in different ways across the life cycle. At early ages, investments and endowments can be substitutes (or weak complements), while in later periods, skills and investments complement each other (referred as static complementarity), and this later complementarity increases over the life-course. This implies that investments can be compensatory at early ages and increase early and later skills for disadvantaged children, which in turn will enhance the productivity of future investments in their development. These two concepts support the argument that early-life skills, possibly affected by in-utero exposure to positive interventions, may interact with subsequent investments to influence future skills.

3 The Rollout of The Food Stamp Program: Background

In 2015, the U.S. marked the 50th anniversary of the War on Poverty. During the Johnson administration and subsequent Nixon administration, a host of safety-net programs were either introduced or expanded to help eradicate poverty, increase income at the bottom of the income distribution, prevent hunger, improve health, and nurture human capital. One of the first programs to be rolled out was the Food Stamp Program, which was started as a pilot program by President Kennedy in 1961. Until they were replaced by electronic benefit cards, Food stamps were vouchers for a specified amount of unprepared foods purchased at participating stores. In the early days of the program, families had to spend a certain amount on food (the purchase requirement) to obtain the food stamps and then obtained vouchers which were for the combination of the amount of food they had paid for and an additional amount that represented the benefit (the bonus coupon). The Food Stamp Program was implemented at the county level. In 1964, the program was made permanent, and it was made mandatory for all counties in 1973, with the goal of having all counties operate a Food Stamp Program by July 1974.

When the Food Stamp Programs was first being implemented, there was an additional requirement that counties that wanted to adopt the Food Stamp Program had to end their involvement with the Commodity Distribution Program (CDP), which provided families with commodities. The CDP was the predecessor to the Food Stamp Program in about

90% of counties. Beatty, Bitler, Figinski, Goodman-Bacon, and Hines (2018) study the transition from the CDP to the Food Stamp Program. They hypothesize that the fact that most counties had a process in place to assess eligibility rules which were mostly harmonized across the two programs led to the quick rollout of the Food Stamp Program documented by Hoynes and Schanzenbach (2009) and also shown below. Starting in 1978, the Food Stamp Program was changed to eliminate the purchase requirement and further standardize the program.

Today, the Food Stamp Program, renamed in 2008 as the Supplemental Nutrition Assistance Program (SNAP), is one of the largest and, in fact, the only universal safety-net programs in the U.S. During the Great Recession, Food stamps served nearly 1 in 7 Americans. Under current requirements, Food stamp recipients need to have gross income under 130% of poverty and net income under the poverty guideline in order to participate.

Neoclassical economics suggests that for families who would consume at least as much food as the purchase requirement plus the bonus coupons allows them to buy, so-called inframarginal consumers, the effect of Food Stamps should be similar to the effects of cash. For those who would ideally spend less than the full combined amount of the bonus vouchers plus the purchase requirement, so-called extramarginal consumers, the program could induce them to spend more on food than would a similar cash grant. The fact that there was a purchase requirement also suggests that adoption of the Food Stamp Program could lead families to change how they acquire food, as they would have a non-trivial amount of coupons from the purchase requirement that could be redeemed only at stores.[1]

The existence of the CDP in most counties implies that the right counterfactual for effects of food stamp rollout is having a possibly inefficient commodity program rather than no food assistance program. The presence of the CDP before the FSP has two implications. First, counties already had in place a way to assess the eligibility of families for the CDP which could be used to assess eligibility for the FSP when it came in. This meant that important infrastructure for getting the FSP up and running was already in place. Second, the CDP, like some other in-kind aid programs, provided inconsistent and anecdotally, low quality food, as documented in Senate hearings (e.g., U.S. Senate Select Committee on Nutrition and Human Needs, 1971).

We build on an already existing literature that documents the positive effects of implementation of food stamps. Hoynes, Schanzenbach, and Almond, in a series of papers, have studied the rollout of the Food Stamp Program. Hoynes and Schanzenbach (2009) document the plausibly quasi-random nature of the rollout of the Food Stamp Program in

[1] Beatty, Bitler, and Van der Werf (2019) find an increase of around 5% in employment in food stores associated with the rollout of food stamps, suggesting that sales went up with rollout.

counties across time. They show that it is hard to predict the timing of adoption with 1960 Census characteristics of counties. Further evidence of plausible exogeneity comes from the initial block grant nature of the funding of the program and the idiosyncratic pattern across time and place in which counties adopted food stamps when.

These authors have also documented the effects of exposure to the Food Stamp Program. Hoynes and Schanzenbach (2009) find that exposure to Food Stamps led to less out of pocket food spending and an increase in the amount of food consumed at home. Additional evidence provided by Almond, Hoynes, and Schanzenbach (2011) shows that implementation of the Food Stamp program also had positive effects on infant health, and rules out significant pre-trends in birth outcomes before Food Stamp adoption. However, Hoynes and Schanzenbach (2012) document that the program led to a relatively small decline in contemporaneous labor supply as would be expected given income effects and the low benefit-reduction rate. Hoynes, Schanzenbach, and Almond (2016) establishes that early exposure to the Food Stamp Program led to reductions in metabolic syndrome for men and women and improvements in economic self-sufficiency for women later in life. In complementary work to ours, Bailey, Hoynes, Rossin-Slater and Walker (2019) show how long-run outcomes measurable in the decennial Census and ACS data are affected by early-life and in-utero exposure to food stamps in one's county of birth. They find food stamp availability in one's county of birth leads to improvements in human capital, self sufficiency, the quality of their neighborhood, longevity, and declines in the probability of being incarcerated in adulthood. Barr and Smith (2019) look at effects of early exposure on criminal behavior and find that early exposure to food stamps led to declines in criminal behavior.

We build on this existing literature with our large panel of administrative data. With this large sample, we are able to detect effects that would likely be missed in small survey data sets, and have outcomes which are well-measured and do not suffer from reporting error.[2]

4 Data

4.1 Rollout Data and Exogeneity of the Food Stamp Program

Our panel of exposure to the Food Stamp Program during childhood is a county-level panel data set of program exposure for children born between 1955 through 1980. We start with program rollout data for food stamps from a series of replication files provided by other authors.

[2]Meyer, Mok, and Sullivan (2015) document an increasing problem with under-reporting of many safety-net programs and incorrect imputation of missing data. Bollinger et al. (forthcoming) and Bollinger and Hirsch (2013) document issues with self-reported earnings.

We merge these rollout data to data from the Regional Economic Information System to control for spending by Medicare or public funding of military health medical care, average real per capita income, unemployment compensation, and federal spending on SSI. (These data are compiled from the Hoynes, Schanzenbach, and Almond (2016) replication, on-line REIS files, and other sources.) We average exposure to these variables from ages 0 to 18 if the cohort has data for all of these years or for selected years if the cohort is earlier in the period, and is missing some of the REIS data which are only annual after 1969. Alternatively, we control for spending during the year of birth. We augment these data with data on the predecessor to food stamps, the Commodity Distribution Program. Our information on the presence of the Commodity Distribution Program in counties at the time they adopt the Food Stamp Program come from the National Archives as well as administrative data on AFDC participants from caseworkers that asks about the food assistance programs in place.

We then create consistent county measures across time. For the vast bulk of counties this is the same concept as the county today. But for some locations (notoriously, Virginia), county boundaries changed considerably. For those locations, we created "super counties" which are the smallest combination of counties (and sometimes cities) that are consistent across time. For the rest of the paper, when we refer to county, we mean super county.[3]

These contextual data are merged to individual-level panel data on earnings and presence of any earnings in the calendar year (our measure of employment) from the Social Security Administration's Master Earnings File, data on SSDI from the Master Beneficiary file, and data on place of birth, from the Social Security Administration's NUMIDENT data file.

Our first empirical task is to explore the exogeneity of the Food Stamp Program. We note that we are relying on existing work by Hoynes and Schanzenbach (2009, 2012); Almond, Hoynes, and Schanzenbach (2011); and Hoynes, Schanzenbach, and Almond (2016). In Figure 1, we reproduce with our data a figure of theirs showing the share of counties that had adopted the Food Stamp Program from 1960 through 1975 by year. There is considerable time-series variation in adoption of the Food Stamp Program. Figure 2 repeats a figure of theirs showing the extensive cross-sectional variation across counties in adoption of the Food Stamp Program. Based on the data presented in this figure, within most states, there is considerable variation in when the Food Stamp Program was adopted across counties. Further, there are early- and late-adopting counties across the entire U.S. This variation within states allows us to control in some specifications for state by year fixed effects which should account for the effect of any statewide programs or legal decisions (e.g., state adoption of Medicaid).

[3] We exclude Alaska and Hawaii from our panel due to challenges with creating consistent county identifiers.

Figure 3 shows participation in the CDP and Food Stamp Program across time nationally through 1968. This shows that the CDP was not insubstantial in the early days of the Food Stamp Program, with it being larger than Food Stamps through 1968.

Next we explore the ramp of up Food Stamps. Figures 4 and 5 show event studies of the per-capita caseload in Food Stamps as a function of the year that a county adopted Food Stamps. Like most event studies, these condition on county fixed effects and create dummies for years in event time (dummies for being a set number of years before or after adoption). Figure 4 shows the event study for the large majority of counties which had the CDP before adopting Food Stamps. This figure shows that the program led almost immediately to a increase from no participation to a 6% rate of use of food stamps per capita. Figure 5, by contrast, shows that for the 10% of counties which never had the CDP before Food Stamps, participation edged up slowly. Thus, we present some results for the sample of ever-CDP counties given the clean event study there which suggests the simple difference-in-differences coefficient is likely to capture the effects of the program.

We note that the existence and prevalence of the CDP before Food Stamps is likely an issue of a different counterfactual for interpreting existing results about the effects of food stamps in the short and long run rather than an issue about exogeneity. To the extent the CDP was an effective program, the implicit first stage in any food program participation is smaller than the event study suggests for food stamps alone. This also suggests that the corresponding adjustment to get the treatment effect on the treated from the reduced form would be larger than if the CDP were ignored. There is also the possibility that the design of the programs means the switch could have different effects for different groups due to the purchase requirement. Higher-income families who switched from the CDP to Food Stamps would have had to start paying more for their benefits, while very low-income families would have not been affected much. Finally, if the CDP was totally ineffectual, it would suggest the first stage for the Food Stamp Program alone is the right one for inflating reduced-form effects. Anecdotal evidence suggests that the CDP provided low-quality foods (e.g., U.S. Senate Select Committee on Nutrition and Human Needs, 1971). Figure 6 shows that when the FSP was implemented in CDP counties, there was on average a small decline in the number of persons getting food assistance. This combined with the anecdotal evidence further supports the hypothesis that the CDP was relatively ineffective since implementation of the Food Stamp Program lead to increases in earnings without any corresponding large increase in persons obtaining some form of food assistance.

4.2 Earnings, Labor Supply, and Disability Program Participation Data

The Continuous Work History Sample (CWHS) contains administrative earnings and social insurance outcomes for a one-percent extract of all Social Security numbers ever issued. Social Security numbers are selected into the extract based on certain digits of the Social Security number. Once a SSN selected for inclusion in the extract is issued, the SSN remains in the extract for life. Data for the selected individuals are drawn from several SSA administrative files. A key variable in this analysis, the place of birth, comes from the NUMIDENT file. The NUMIDENT file contains data on the date of birth, race, gender, and date of death (if applicable) as well as the place of birth for all individuals included in the CWHS.[4] Unfortunately, about 543,000 individuals of the 4.9 million in the NUMIDENT file have no entry which indicates a place of birth. (In our ultimate sample of individuals born from 1955 to 1980, the share with no place of birth is very much smaller.) As a result, these individuals are removed from the sample. Table 1 reports the reductions in sample size from removing those without places of birth in the U.S. Only a small share of observations are omitted due to a missing place of birth (3,000 of 1,281,000 born from 1955–1980). Another 309,000 are lost because they are born aboard or in a U.S. territory, leaving an eventual sample of nearly 1 million individuals.

Earnings data in the CWHS are drawn from the Master Earnings File (MEF). The CWHS contains FICA-covered earnings for each year from 1951 to the present ("FICA earnings"), Medicare-covered earnings for each year from 1983 to the present ("Medicare earnings"), and total compensation drawn from Box 1 of Form W-2 for each year from 1978 to the present ("W-2 earnings"). While the FICA earnings data are available over the longest period and benefit from being administrative records, they are not without their drawbacks.[5] While using W-2 earnings would circumvent these issues, the W-2 earnings data are not without

[4]Typically, a NUMIDENT entry is created each time an individual completes a Social Security card application (Form SS-5). Individuals complete Form SS-5 for several reasons. As a result, there are an average of two entries per individual. In accordance with SSA practices, we limit the NUMIDENT file to one entry per individual by selecting the most recent entry with a non-missing place of birth.

[5]First, the FICA-covered earnings are censored at the taxable maximum. This censoring is particularly problematic in years prior to 1978. In each year between 1951 and 1978, at least 20 percent of individuals between the ages of 25 and 54 with non-zero earnings had earnings at or above the taxable maximum. However, the first year of earnings for our cohorts at age 25 is 1980 (for the cohort born in 1955). The second drawback of the FICA-covered earnings is that it is impossible to distinguish between a year with zero earnings and a year with zero covered earnings. Covered earnings refer to earnings received by individuals who work in industries where employers and employees are required to pay Social Security taxes. Under the original Social Security Act, only those who worked in commerce and industry, approximately 52 percent of the labor force, were covered (Olsen and Hudson, 2009). Over time, coverage was gradually expanded; today only approximately six percent of the U.S. workforce works in non-covered employment (SSA, 2015). As recently as 1976, however, ten percent of the labor force worked in non-covered employment (GPO, 1997).

their limitations. Unlike the FICA and Medicare earnings data in the CWHS data, no adjustments are made for delinquent or correction postings after the close of the initial posting year.[6]

As a result, to fully exploit the earnings data available to us, we create an amalgamation of the various earnings measures available in the CWHS data which we refer to as "total earnings." Specifically, we rely on FICA earnings when available and less than the annual FICA taxable maximum minus $10. Otherwise, we rely on W-2 earnings or Medicare earnings for years after 1993 when the cap on Medicare taxable earnings is removed. In addition, to the extent the individual has self-employment income, we account for that in our measure. We also construct a measure of labor supply for each person in each year that is one in calendar years in which that person has non-zero total earnings and zero otherwise.

Finally, the CWHS's data on benefit entitlement are obtained from the Master Beneficiary Record (MBR). The MBR contains application, entitlement, and termination dates for SSDI entitlements, as well as benefit amounts, payment status, type of benefit received, and the complete history of all benefits ever applied for and received. Applicants who have worked enough quarters (now equivalent to earning enough in at least 10 years) are eligible for SSDI if they then become unable to work (as judged by the Social Security Administration) and have a sufficient period with no meaningful work activity. We note that there is an advantage to using these administrative data on social insurance as well as administrative earnings, given issues with misreporting which may lead to bias (e.g., Meyer, Mok, and Sullivan, 2015, for transfers and Bollinger and Hirsch, 2013, and Bollinger et al., forthcoming for earnings).

We limit the CWHS sample to those individuals born between 1955 and 1980, who have a valid place of birth, and who were born in the U.S. Table 2 reports the statistics for matching place of birth by various approaches, showing 92% of our sample of those with a place of birth in a U.S. state are matched to a county of birth. (Additional details about the matching process are available upon request.)

We focus on earnings acquired during ages 25 to 34. In addition to data-driven reasons for these restrictions discussed immediately below, we also wanted to focus on ages where we think people have completed schooling and are fairly far along in their work life. Appendix Figure 1 shows average real earnings for persons in our panel at various ages. Note that in Appendix Figure 1, means for ages after age 34 do not have all the birth cohorts contributing

[6]While others have documented issues with the total compensation data from Box 1 of Form W-2, these issues are not particularly relevant to us. Specifically, Kopczuk, Saez, and Song (2010) point out that there are data issues related to the W-2 earnings data from 1978 to 1980. These issues are not relevant to our analysis because the earliest birth cohort in our data is those born in 1955 and the earliest age at which we examine individual earnings is age 25. As a result, the first year of W-2 earnings data from the CWHS we use is from 1980 and our main results are for earnings realized in calendar year 1982 or later, well after when previous analyses indicated there was a problem with the W-2 earnings data.

to the mean earnings. Nevertheless, it is also clear that there is no large slope change at those ages, and both men and women are facing an upward sloping earnings profile after about 30 for women and perhaps earlier for men.[7]

These limitations of cohorts and ages balance two concerns. First, only censored FICA earnings are available prior to 1978. If earnings beginning at age 18 were used to examine the long-run economic effects of the Food Stamp Program, three years of the potentially misleading FICA earnings data would be used. Examining earnings beginning at age 24 would result in no FICA earnings being used. If instead the sample were extended back in time to include those born since 1950, a minimum of four years of FICA earnings would need to be used. Thus, we chose to focus on cohorts born in 1955 and after, at ages 25 and above.

Second, the rollout of the Food Stamp Program extends from the early 1960s to the late 1970s. By examining birth cohorts from 1955 to 1980, we can focus on those cohorts most likely to exposed to the rollout of Food Stamps, while also including cohorts that are either never or always exposed to food stamps from conception to age 5. Our final sample includes approximately 882,000 individuals, and we have a balanced panel of these individuals from ages 25 to 34, excluding those who die prior to age 34.

5 Analytic Approach and Results

We use a natural-experiment event-study-like generalized difference-in-difference design that exploits temporal and geographic variation in exposure to the Food Stamp Program, estimating the following regression:

$$y_{ijbm} = \beta_0 + \text{FSP}_{ijbm} + X_i \cdot \gamma + \alpha_j + \alpha_b + \theta_j(b) + \epsilon_{ijbm},$$

where y_{ijbm} is the outcome of child i who is born in county j, in year b, and in month m. FSP_{ijbm} measures the share of the time from conception through 60 months that the Food Stamp Program was in effect in the child's county of birth. X_i includes demographic characteristics of a child such as age (when we use data for multiple ages), and white (as opposed to non-white) status from the NUMIDENT file.[8] The terms α_j and α_b denote county of birth and year of birth fixed effects. They capture any time-invariant unobserved differences across counties, and unobserved shocks common to all children born in the same

[7] In the appendix, we also explore using several years of earnings averaged and using alternative ages to address concerns that this is not the right point in the life cycle to look at effects. We also explore percentiles of earnings within birth cohort there.

[8] The current NUMIDENT file includes a host of options for race/ethnicity, but this detail is not available for the earlier period.

year. (Age or year fixed effects are not typically included as the main results are all estimated when the children are all the same age.) The term $\theta_j(b)$ control for county-level trends which might be correlated with child development. Alternative models include state-specific linear time trends. Lastly, ϵ_{ijbm} represents the random error term. To address potential spatial correlation by location as well as the fact that the key independent variable is only measured at the county level, we cluster standard errors at the county level. We present results separately for men and women, following Hoynes, Schanzenbach, and Almond (2016). Hoynes and Schanzenbach (2009) found that population was an important predictor of adoption of the FSP, so we include controls for the 1960 county population in most specifications (and the results are robust to instead controlling for the 1950 population). In some specifications, we also control for per capita spending on other welfare programs and unemployment insurance (UI) as well as real per capita income in the county for the child from ages 0 through 18 or at age 0.

5.1 No effects on employment

First, we consider the extensive margin. In our data, we model employment as having non-zero earnings in a given calendar year. Table 3 shows the effect of exposure to Food Stamps had no effect on the probability that men or women had positive earnings in the calendar year whenever they appear in our panel. The point estimates are small, with exposure leading to an increase of 0.0039 in the probability that women had earnings, and we can rule out effects larger than 1 percentage point while the baseline mean share of years with non-zero earnings is 0.78. Effects for men are similarly small in magnitude and precisely estimated.

Table 4 shows our first set of results at age 32 for exposure to Food Stamps for women and Table 5 for men. The first column reports results for total earnings, column 2 for W-2 earnings, and column 3 for FICA earnings. Table 4 shows that being exposed to food stamps from conception through age 5 leads to earnings being between $512 and $751 higher at age 32 in real 2015$ for women. These estimates are all significant at the 10% level, and two are significant at the 5% level. The second panel of the table contains both the mean outcome and mean exposure. These translate to earnings being from 2.2% to 3.1% higher if a child was exposed to food stamps the entire time from conception through age 5.

Table 5 shows that the effects are insignificant for men, and smaller in magnitude for our two preferred measures (total earnings and W-2 earnings), although the equality of effects between women and men cannot be rejected. This highlights what will be a common theme; effects for men are quite mixed.

Figure 7 shows an event-study-like figure for total earnings for women. Unlike usual event

studies, here the x-axis denotes the age at which food stamps rolled out in the child's county. Since the program never shuts off, and we are looking at the long-run effects at a single age (or set of ages), we cannot separate out the effects of age at first exposure from the effects of years of exposure. For power, we combine 2 year bins for age at first exposure to the Food Stamp Program. As usual, we leave out the fixed effect corresponding to one pre-treatment 2 year period—here ages 6 to 7, and include county fixed effects. First, note that Figure 7 shows no evidence of pre-trends (children older than age 6 and 7 at rollout have small and statistically insignificant effects of exposure) and the effects are zero at age 10 to 11, where the previous studies have set the event study effects of food stamps to zero. Second, note that there is an increase in earnings after food stamp implementation for children exposed anytime before age 6, and it seems to increase with years of exposure (children who are younger at first exposure have more years of exposure) and then flatten out once children have been exposed for the entire period from conception through age 5.

Table 6 shows the robustness of the previous findings for women at age 32 for earnings across specifications with different controls. Columns 1 and 2 are for the same sample as in Table 3. The samples in columns 3–6 are smaller. Columns 3–6 control for population in the county of birth in 1960, and this reduces the sample size slightly due to the inability to well measure population for places where cities and outlying areas were separate in 1960. In columns 4 and 5, we add controls for year of birth measures of per-capita real spending on Medicare and publicly funded medical care for military families, UI, and the federal SSI program as well as for per capita real income.[9] In column 6, we control for the REIS variables averaged across ages 0 to 18, and lose a few additional thousand observations. The coefficients are similar in magnitude and significance levels across columns when adding state of birth linear time trends, county of birth linear time trends, 1960 county population, or the other contextual controls. This table shows striking evidence that the Food Stamp Program has significant long-run positive effects on earnings for women.

5.2 Magnitude of earnings effects for women

These effects are non-trivial in magnitude, as noted above, with our preferred outcome (total earnings) showing effects of food stamp rollout on earnings of around 3%. One might ask is this magnitude believable? A first comparison would be to the participation effect, where implementation led to an increase in per-capita Food Stamp Program participation rates of 6%. But this is overall participation. One might expect participation rates to go up by more for some groups, such as families with children.

[9]For this, we must leave also out cohorts born before 1960 as these variables are unavailable then.

We show this using the March Current Population Survey (CPS). We use the first CPS ASEC year with food stamp information, and summarize household participation in the Food Stamp Program and household benefit levels in the 1979 calendar year. These numbers are presented in Table 7. We find that 18.3% of boys aged 0 to 5 and 17.5% of girls aged 0 to 5 were in a household with someone on the Food Stamp Program.[10] Interestingly, there is little variation in this participation rate across ages (or by age and gender). The average nominal benefit the family received was $202 per year unconditionally, and again there were no significant differences across age or gender. Among households in official poverty in the 1979 calendar year, 61% of children ages 0 to 5 were in a household on the Food Stamp Program, and the average household benefit was $814. Note that if 18% of children are on food stamps from conception through age 5, then this is an average duration of a little more than one year when the program is fully implemented. In light of this evidence, a 3% increase in earnings compared to nearly a 20% participation rate in food stamps suggests a meaningful treatment on the treated effect if one treats 20% as the relevant "first stage" for the effect of FSP implementation on on food stamp participation.

We have also done a back of the envelope calculation to see what the projected effect on food spending would be relative to the average at the time. Administrative data from fiscal year 1975 suggest nominal average benefits of $21.42 per month per person, or $257 per year. USDA Agricultural Research Service reports suggest average expenditures for food purchased at home in 1975 of $621 per person. Thus, average benefits from Food Stamps are approximately half of average annual food spending during this period. One could imagine that such a large increase in food spending could improve earnings in adulthood.

Finally, we compare our findings to estimates from other programs. By comparison, Brown et al. (forthcoming) find evidence that the expanded Medicaid program of the late 1980s and early 1990s had substantial effects. One year more of Medicaid coverage on average was associated with an increase in earnings of $656 from ages 19 to 28. Our findings are slightly smaller than theirs for around a one year increase in receipt of benefits.

Recall that we have also considered the extensive employment margin. We found that there is no impact of food stamp implementation on the probability that within a calendar year, the individual had positive administrative earnings. Given this, it is clear that higher mean impacts have to come from impacts higher up the distribution.

[10] We cannot look at in utero exposure in this analysis.

5.3 Effects across the earnings distribution

We next turn to estimates of the probability real earnings are above various multiples of $5000. Figure 8 focuses on effects for real total earnings being above $5000, $10,000, ... up to $60,000 (left scale) for women at age 32. The right scale is associated with a histogram of the probability that earnings are above the cut-points as the share of persons in each $5000 cell need not be constant. This shows (not surprisingly, given the no effect on earnings we showed above) that the effects are non-trivial and significant at some higher cut-points and they are never negative. In particular, they are statistically significant at the 5% level for being above $35,000 or $45,000 in real earnings. Note that about 25% of women have more than $35,000 in earnings.

Figure 9 shows analogous estimates for the probability men's total real earnings are above various cut-points at age 32. We note that the point estimates for men are uniformly smaller and insignificant (but always positive). This is true even if we look at one of the same cut-points—$35,000—where the effects on women's earnings are significantly different from 0.

5.4 Robustness to using more years of earnings, other ages, percentiles

To date, all of our estimates are calculated for a single year of age. In the literature on life-cycle earnings, analysts worry about effects estimated for a single year of age. In Appendix Table 1, we instead average earnings across ages 32–34. The effects for women are very similar to those reported for age 32, $779 compared to $705 for the same specification in column 1 of Table 6. The point estimate for men is larger than in Table 5 column 1, but still insignificant. This addresses concerns about using a single year of age for our main specifications. In Appendix Table 2, we restrict the sample to slightly older birth cohorts with earnings measures at older ages, all born 1955–1974, and again the effects for women aged 32–34 are a very similar $862. Appendix Table 3 looks at effects for the same cohorts born 1955–1974 at ages 36–38. Here the point estimate for women is still positive, but it is smaller. Finally, we follow the work of Chetty, Hendren, and various coauthors and look at percentiles of the earnings distribution within birth cohorts. So, for each birth cohort, the dependent variable is the within-birth-cohort percentile of earnings. These are presented in Appendix Table 4. The point estimate for women is an increase of 0.53 percentile rank points, which is non-trivial, and the estimate is significant at the 10% level. Following the theme, we also find a similar but insignificant estimate for men.

5.5 No effects on involvement with the SSDI program

In Table 8, we focus on the effects of food stamp exposure on any SSDI involvement (benefit receipt or application denied). These are estimated as of the last year when individuals are in the panel, and thus the panel is unbalanced with earlier birth cohorts having more exposure. Here we see there is no significant evidence for more use or attempted use of SSDI by either gender. The point estimates are very small, as well.

5.6 Effects larger for counties with CDP in place before the FSP

Finally, Table 9 reports our preferred estimate of Food Stamp Program exposure for the subset of counties where the CDP was present before food stamps. The estimate is a slightly larger $764 compared to the $705 for the full sample for women. (In future work, we will more fully examine the effects of the FSP by whether the county ever had the CDP/never had the CDP.)

6 Why Are the Effects Focused among Women?

One obvious additional question arises, why are the effects focused among women? We have explored several possible explanations, but cannot definitively say. First, we note these findings are consistent with Hoynes, Schanzenbach, and Almond (2016) for self-sufficiency. Second, one explanation could be that girls were more disadvantaged (or that parents invested more in boys after birth). We have explored whether in the predecessor to the National Health and Nutrition Examination Survey baseline health for young male children is better than that for girls. Evidence on this is not conclusive, with girls looking less well off for some outcomes and more so for others. We also showed above that household participation in food stamps and food stamp nominal benefits in 1979 did not vary across gender and age for young children in 1979. We do plan to explore whether effects on take-up are larger for girls, using the PSID.

Finally, it has been suggested that we are not looking in the right place in the life-cycle, and might be missing effects for men. We ruled this out above.

7 Conclusion

We have comprehensively shown that rollout of the Food Stamp Program across counties led to a economically meaningful and statistically significant at the 5% level increase in earnings

for women of about 3%, compared to a back of the envelope calculation of the first stage of rollout on participation in the FSP of about an 18% increase in participation in the Food Stamp Program. Estimates for men are more varied, but we cannot reject that they are the same as those for women. Our estimates rely on large samples of highly accurate earnings data. There is no impact on employment or SSDI involvement for men or women. Most counties had another food assistance program—the Commodity Distribution Program—in place before the FSP. We find that effects are larger in those counties, which we attribute to the fact that these counties had an infrastructure for evaluating eligibility for families in place before the FSP was implemented combined with the fact that the CDP was likely inconsistent and likely provided low-quality foods. These results are meaningful for informing discussions about long-run benefits of safety-net programs.

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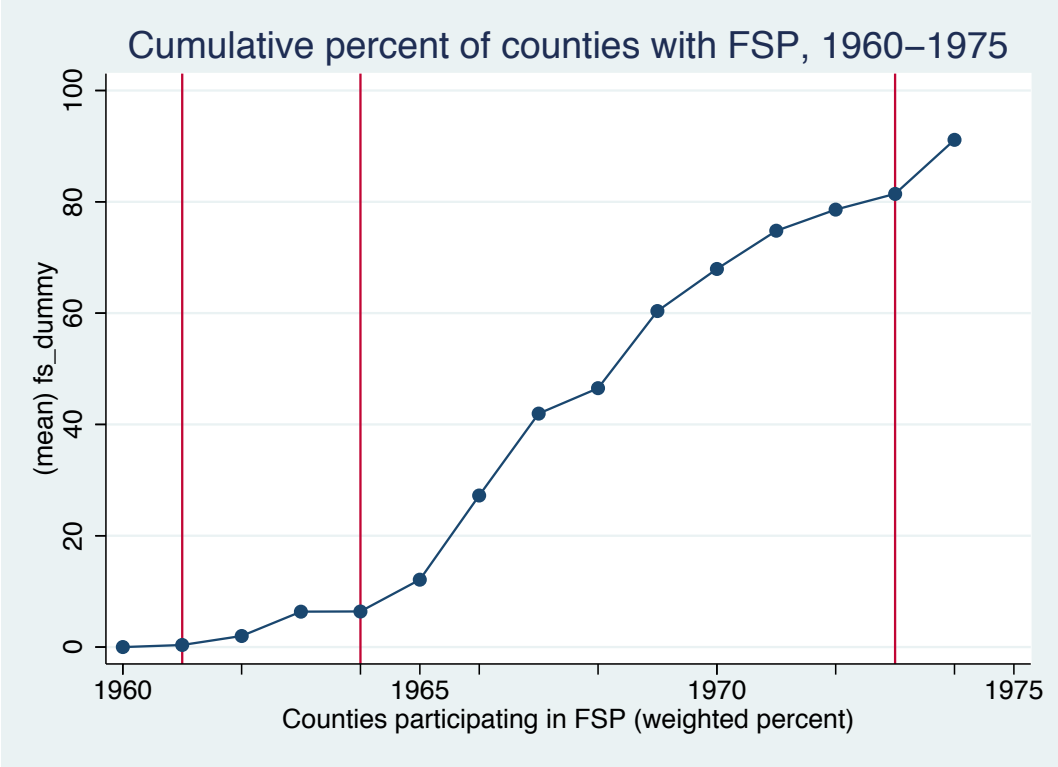
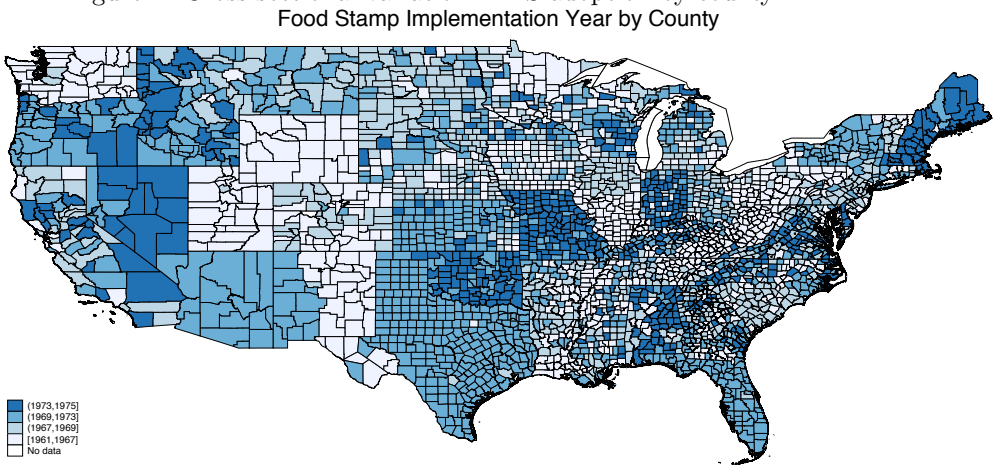
Figure 1: Share of counties adopting FS



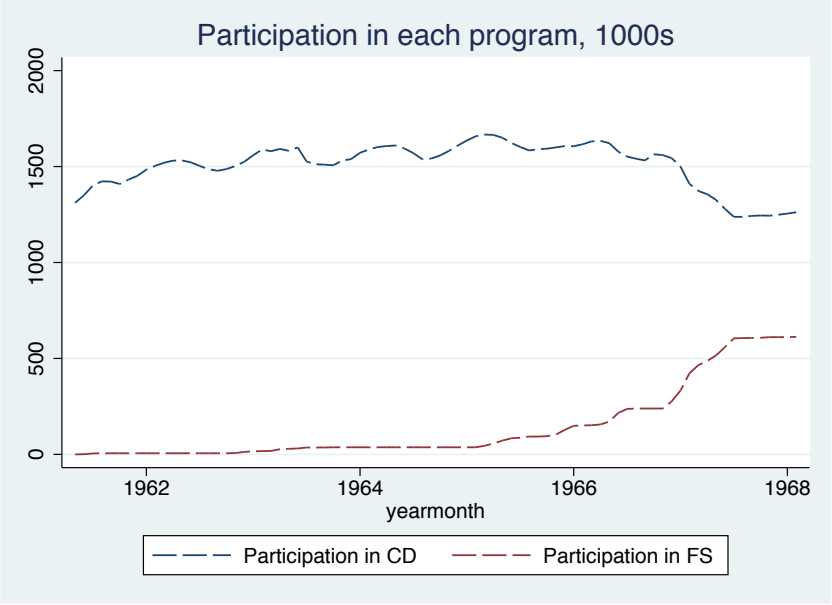
Notes: Figure shows share of counties adopting the Food Stamp Program by year. Based on Hoynes and Schanzenbach, 2009.

Figure 2: Cross-sectional variation in FS adoption by county



Notes: Figure shows timing of adoption of the Food Stamp Program by county. Based on Hoynes and Schanzenbach, 2009.

Figure 3: Participation in the FSP and CDP through 1968



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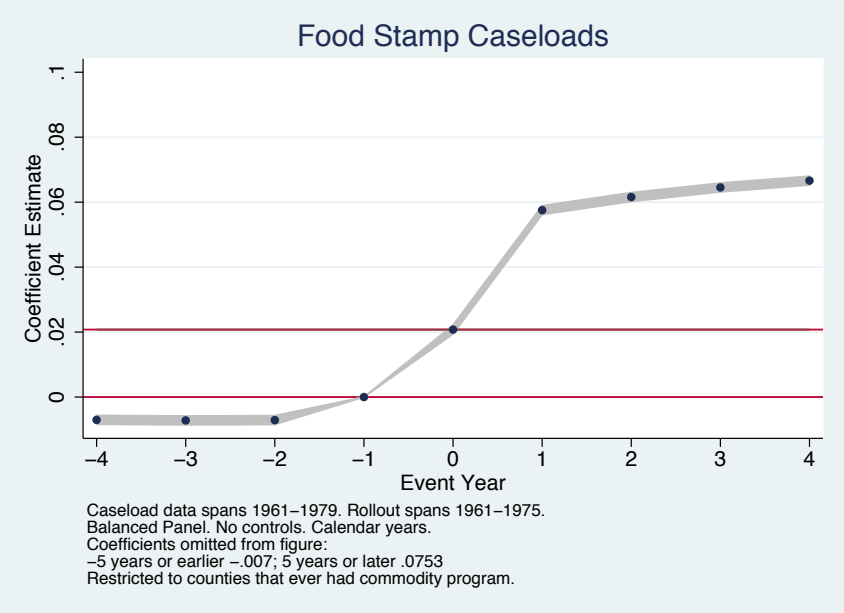
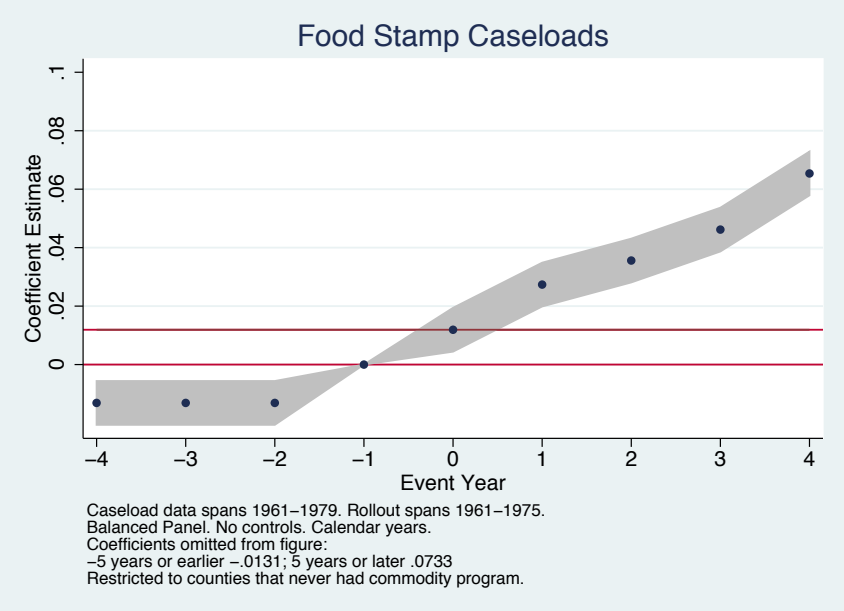
Notes: Figure shows participation in the Food Stamp and Commodity Distribution Program nationally by year through 1968. Based on counts from National Archives documents.

Figure 4: FSP caseload per capita event study, ever CDP



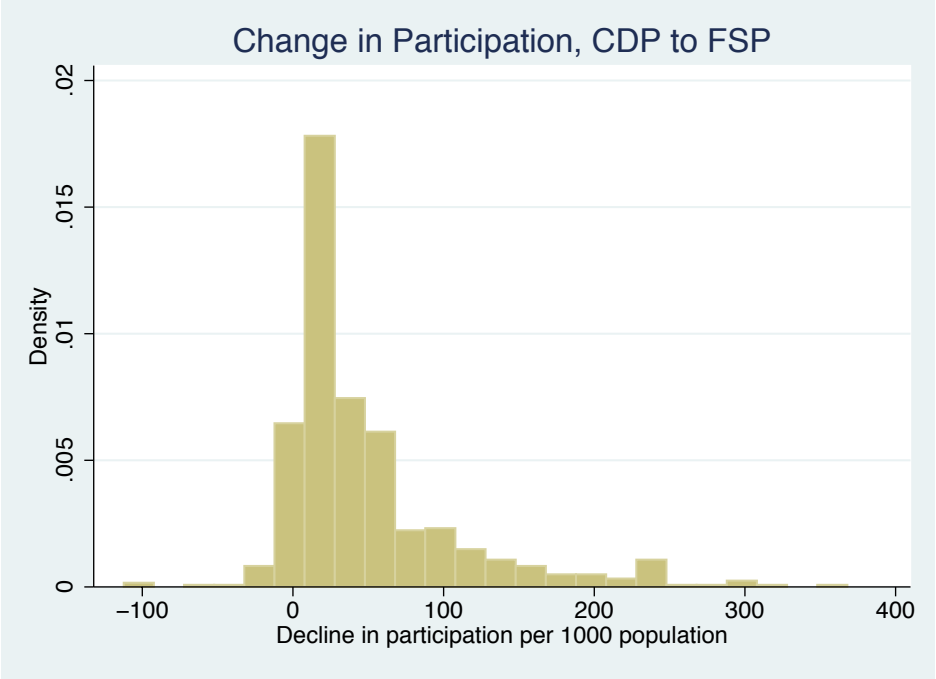
Notes: Figure shows participation in the Food Stamp Program in an event study. Dummies included for county and coefficients shown for event time. Balanced panel of counties shown, additional controls for event time being -5 or earlier or 5 or later. Caseloads for 1961–1979. Sample is counties that ever had the CDP program.

Figure 5: FSP caseload per capita event study, never CDP



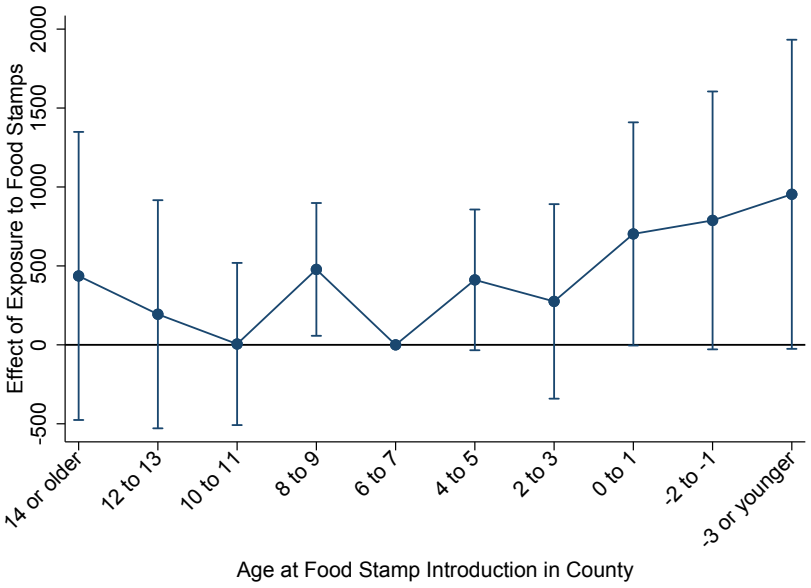
Notes: Figure shows participation in the Food Stamp Program in an event study. Dummies included for county and coefficients shown for event time. Balanced panel of counties shown, additional controls for event time being -5 or earlier or 5 or later. Caseloads for 1961–1979. Sample is counties that never had the CDP program.

Figure 6: Decline in caseloads when counties switched from CDP to Food Stamps, switches through 1968



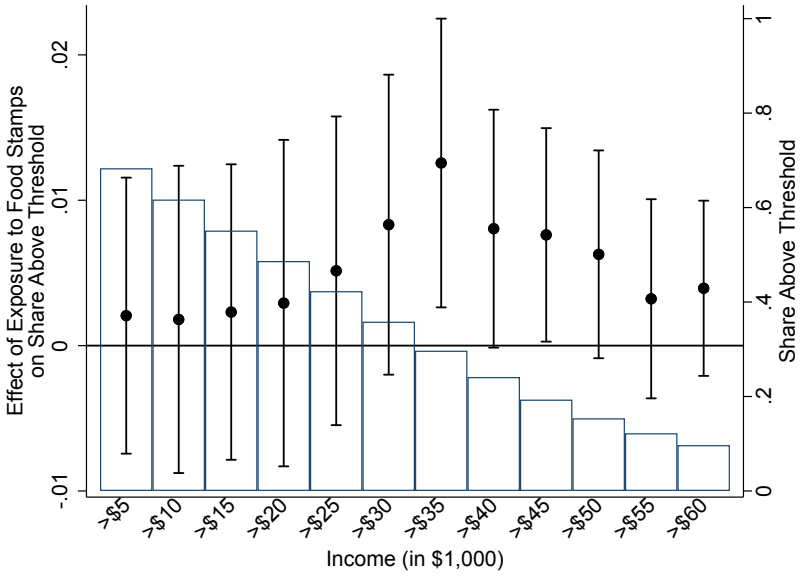
Notes: Figure shows the decline in caseloads when counties made the transition from the CDP program to the Food Stamp Program through 1968. Based on counts from National Archives documents.

Figure 7: Effects of exposure to the FSP on women's total compensation at age 32, cohorts born 1955–1980, with controls, event study, unbalanced panel



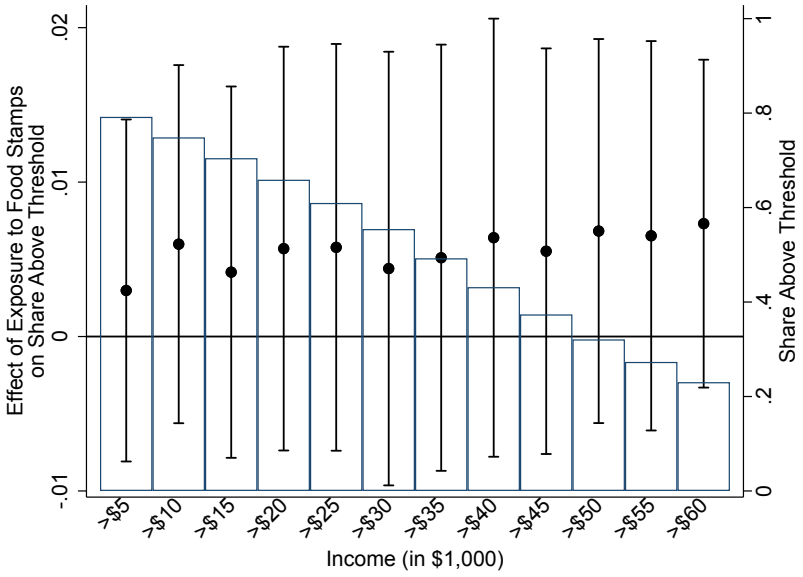
Notes: Figure shows income from the CWHS for women at age 32 by cohort as a function of the age at which Food Stamps was introduced in the county of birth. Dummies included for county and coefficients shown for age at introduction in 2 year bins. Unbalanced panel of counties and years shown. Controls included for being white. Omitted category first exposure is at ages 6 and 7.

Figure 8: Effects of exposure to Food Stamps for women at age 32 on the probability of earning more than a threshold amount, cohorts born 1955–1980



Notes: Each dot indicates the effect of exposure to Food Stamps from conception through age 5 on the probability of real total earnings at age 32 being above various thresholds (left axis). The capped bars indicate the 95% confidence interval for each estimate. The hollow histogram bars indicate the share of the population with earnings above the threshold (right axis). The sample includes women born between 1955 and 1980. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1000 and is 422,000. Mean exposure to the Food Stamp Program is 0.5102. Controls include fixed effects for white, birth year, and county of birth.

Figure 9: Effects of exposure to Food Stamps for men at age 32 on the probability of earning more than a threshold amount, cohorts born 1955–1980



Notes: Each dot indicates the effect of exposure to Food Stamps from conception through age 5 on the probability of real total earnings at age 32 being above various thresholds (left axis). The capped bars indicate the 95% confidence interval for each estimate. The hollow histogram bars indicate the share of the population with earnings above the threshold (right axis). The sample includes men born between 1955 and 1980. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1000 and is 433,000. Mean exposure to the Food Stamp Program is 0.5098. Controls include fixed effects for white, birth year, and county of birth.

Table 1: Changes in number of persons in data as impose restrictions that place of birth exists and person was born in the US

	(1)
	Born 1955 to 1980
Number of individuals	1,281,000
Missing POB	3,000
Foreign-born	286,000
Not born in a state	23,000
Remaining sample	970,000

Notes: Table shows how the sample size shrinks as we successively add restrictions that there be a place of birth reported on the application for a Social Security card and that the individual be born in a US state or the District of Columbia. The number of individuals are rounded to nearest 1,000 to make the detail add up to the total.

Table 2: Changes in number of persons in data as impose a unique match to place of birth, cohorts born 1955–1980

	(1)	(2)
	N	%
Straight match	643,000	66%
Other automatic match	205,000	21%
Manual match	43,000	4%
Non-unique match, resolved	92,000	%
Total matched	891,000	92%
Non-unique, unresolved	35,000	4%
Unmatched	44,000	4%
Total	970,000	100%

Notes: Table shows the share of starting sample lost due to inability to match places of birth to names of places from the Census. Other automatic match means name included an abbreviation (e.g.,"Frms" for "Farms", county labelled "Co") or length of string required to be ≤ 12.

Table 3: Effects of exposure to Food Stamps, cohorts born 1955–1980, any employment

	(1)	(2)
	Women	Men
Share of months conc. to 5 exposed to FSP	0.0039	0.0037
	(0.0029)	(0.0036)
Mean probability any employment	0.7756	0.8284
Mean exposure to FSP	0.3827	0.3840
Number of person observations	424,000	437,000
Number of person-year observations	9,663,000	9,548,000

Notes: The table shows the effect of exposure to the FSP from conception through age 5 on ever having positive total earnings within the calendar year at any age for women and men born between 1955 and 1980 from ages 25 to the time they are no longer in the panel. ***, **, and * denote significance at the 1, 5 and 10 percent level. SEs clustered at the county of birth level. The number of individuals is rounded to nearest 1000. Controls include fixed effects for white, birth year, and county of birth.

Table 4: Effects of exposure to Food Stamps for women at age 32, cohorts born 1955–1980, various measures of earnings

	(1)	(2)	(3)
	Total Earnings	W-2 Earnings	FICA Earnings
Fraction of Months Exposed to Food Stamps	705**	751**	512*
	(311)	(309)	(271)
Mean Outcome Variable (in 2015$)	25,495	24,479	23,803
Mean Exposure to Food Stamps	0.5102	0.5102	0.5102
N	422,000	422,000	422,000

Notes: Table shows the effect of exposure to the FSP from conception through age 5 on various measures of earnings at age 32 for women born between 1955 and 1980. ***, **, and * denote significance at the 1, 5 and 10 percent level. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1,000. Controls include fixed effects for white, birth year, and county of birth.

Table 5: Effects of exposure to Food Stamps for men at age 32, cohorts born 1955–1980, various measures of earnings

	(1)	(2)	(3)
	Total Earnings	W-2 Earnings	FICA Earnings
Fraction of Months Exposed to Food Stamps	175	135	607
	(1,183)	(1,067)	(497)
Mean Outcome Variable (in 2015$)	41,129	38,792	36,734
Mean Exposure to Food Stamps	0.5098	0.5098	0.5098
Number of individuals	433,000	433,000	433,000

Notes: Table shows the effect of exposure to the FSP from conception through age 5 on various measures of earnings at age 32 for men born between 1955 and 1980. ***, **, and * denote significance at the 1, 5 and 10 percent level. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1,000. Controls include fixed effects for white, birth year, and county of birth.

Table 6: Effects of exposure to Food Stamps for women at age 32, cohorts born 1955–1980, real total earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Months Exposed to Food Stamps	705**	645**	720**	648**	724**	519*
	(311)	(302)	(304)	(315)	(326)	(311)
Mean real earnings	$25,495	$25,495	$25,489	$26,104	$26,104	$25,489
Mean exposure	0.5102	0.5102	0.5093	0.6161	0.6161	0.5093
Number of individuals	422,000	422,000	418,000	346,000	346,000	418,000
Controls for:						
White	Y	Y	Y	Y	Y	Y
YOB FE	Y	Y	Y	Y	Y	Y
COB FE	Y	Y	Y	Y	Y	Y
SOB trend	N	Y	Y	Y	N	N
COB trend	N	N	N	N	Y	Y
REIS birth	N	N	N	Y	Y	N
REIS 0–18	N	N	N	N	N	Y
Pop., 1960	N	N	Y	Y	Y	Y

Notes: Table shows the effect of exposure to the FSP from conception through age 5 on real total earnings at age 32 for women born between 1955 and 1980. ***, **, and * denote significance at the 1, 5 and 10 percent level. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded for confidentiality. REIS controls include real income in the county, public spending on medical care from Medicare or medical spending for military families (set to 0 before 1966), spending on UI, and spending on the Federal portion of SSI. Sample in columns 3 and 6 is smaller due to inability to create population for some super counties where outlying areas and cities were combined. Sample in columns 4 and 5 only for those born in 1959–1980 and with super counties with matching populations and also smaller due to missing REIS data before 1959. The number of individuals is rounded to nearest 1,000.

Table 7: Average household participation in Food Stamps and value of Food Stamp Benefits for children 5 and younger for 1979, from March CPS, by age and gender

	(1)	(2)	(3)	(4)	(5)	(6)
	Age 0	Age 1	Age 2	Age 3	Age 4	Age 5
Household got FS, all children						
Female child	0.17	0.20	0.18	0.18	0.18	0.19
Male child	0.18	0.18	0.17	0.17	0.17	0.17
Nominal HH FS benefits, all children						
Female child	171	208	213	217	215	214
Male child	189	209	180	203	197	212
Household got FS, children in poverty, official poverty measure						
Female child	0.54	0.62	0.60	0.57	0.63	0.66
Male child	0.60	0.62	0.60	0.66	0.60	0.59
Nominal HH FS benefits, children in poverty, official poverty measure						
Female child	665	794	811	845	885	881
Male child	794	822	743	886	833	840

Notes: Tabulations of the probability of children's household participating in the Food Stamp program and average household benefits in nominal terms for children 5 and under by gender. Data from 1980 March Current Population Survey, weighted to be population representative. Reference period is 1979 calendar year. Top 2 panels for all children, bottom two panels for children in families in official poverty. Regressions of either HH participation or the value of benefits on dummies for being male of each age or female of each age show no significant differences in the participation or benefit levels across these groups.

Table 8: Effects of exposure to Food Stamps, cohorts born 1955–1980, SSDI involvement, ever in panel

	(1)	(2)
	Women	Men
	Ever on SSDI/rejected SSDI	Ever on SSDI/rejected SSDI
Share months conc.	-0.0028	0.0007
to age 5	(0.0032)	(0.0038)
Mean Outcome Variable	0.1146	0.1284
Mean Exposure	0.5095	0.5095
Number of individuals	424,000	437,000

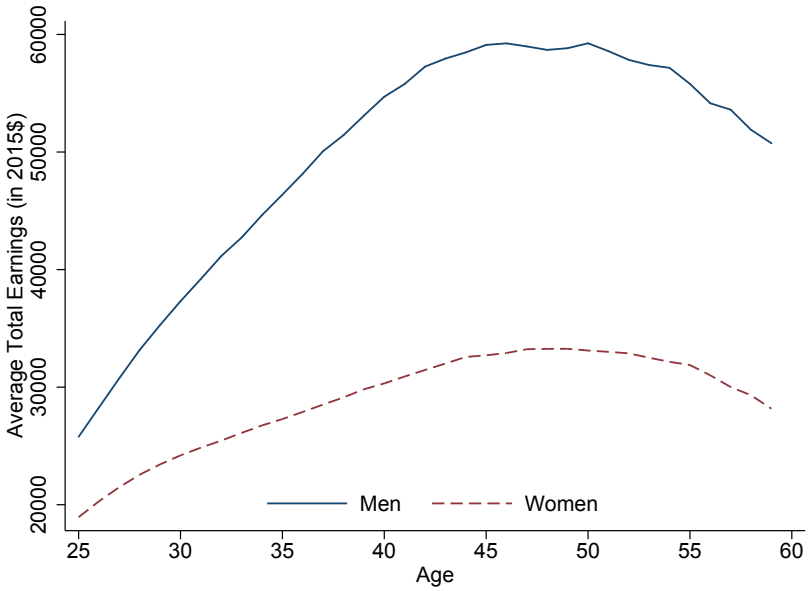
Notes: The table shows the effect of exposure to the FSP from conception through age 5 on participation in SSDI or being rejected from SSDI at any age for women and men born between 1955 and 1980. ***, **, and * denote significance at the 1, 5 and 10 percent level. SEs clustered at the county of birth level. The number of individuals is rounded to nearest 1000. Controls include fixed effects for white, birth year, and county of birth.

Table 9: Effects of exposure to Food Stamps at age 32 in counties ever having the CDP, cohorts born 1955–1980, total real earnings

	(1)	(2)
	Women	Men
Share of months conc. to 5 exposed to FSP	764**	113
	(316)	(1,119)
Mean earnings	25,495	41,197
Mean exposure to FSP	0.5085	0.5081
Number of individuals	408,000	418,000

Notes: Table shows the effect of exposure to the FSP from conception through age 5 in counties that ever had the CDP on real earnings at age 32 for women and men born between 1955 and 1980. ***, **, and * denote significance at the 1, 5 and 10 percent level. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1,000. Controls include fixed effects for white, birth year, and county of birth.

Appendix Figure 1: Real average earnings across the life cycle



Notes: All dollar amounts are measured in 2015 dollars and are adjusted using CPI-U. Individuals with missing month of birth or born in counties with missing Food Stamp rollout information are excluded from the sample..

Appendix Table 1: Effects of exposure to Food Stamps for adults aged 32–34, cohorts born 1955-1980, average real total earnings

	(1)	(2)
	Women	Men
Share of months conc. to 5 exposed to FSP	779***	655
	(343)	(842)
Mean earnings	26,160	42,865
Mean exposure to FS	0.5103	0.5100
Number of individuals	421,000	431,000

Notes: Table shows effects of exposure to the FSP from conception through age 5 on real total average earnings at ages 32–34 for men and women born between 1955 and 1980. ***,**, and * denote significance at the 1, 5, and 10 percent level. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1000. Controls include fixed effects for white, birth year, and county of birth. Only individuals alive from ages 32 to 40 are included in the sample.

Appendix Table 2: Effects of exposure to Food Stamps for adults aged 32–34, cohorts born 1955–1974, average real total earnings

	(1)	(2)
	Women	Men
Share of months conc. to 5 exposed to FSP	862**	82
	(394)	(806)
Mean earnings	25,607	43,736
Mean exposure to FS	0.38	0.38
Number of individuals	330,000	336,000

Notes: Table shows effects of exposure to the FSP from conception through age 5 on real total average earnings at ages 32–34 for men and women born between 1955 and 1974. ***,**, and * denote significance at the 1, 5, and 10 percent level. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1000. Controls include fixed effects for white, birth year, and county of birth. Only individuals alive from ages 32 to 40 are included in the sample.

Appendix Table 3: Effects of exposure to Food Stamps for women aged 36–38, cohorts born 1955–1974, average real total earnings

	(1)	(2)
	Women	Men
Share of months conc. to 5 exposed to FSP	223	-23
	(450)	(1,147)
Mean earnings	28,300	50,456
Mean exposure to FS	0.3781	0.3774
Number of individuals	330,000	336,000

Notes: Table shows effects of exposure to the FSP from conception through age 5 on real total average earnings at ages 36–38 for men and women born between 1955 and 1974. ***,**, and * denote significance at the 1, 5, and 10 percent level. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1000. Controls include fixed effects for white, birth year, and county of birth. Only individuals alive from ages 32 to 40 are included in the sample.

Appendix Table 4: Effects of exposure to Food Stamps at age 32, cohorts born 1955–1980, percentile rank of earnings by birth cohort

	(1)	(2)
	Women	Men
Share of months conc. to 5 exposed to FSP	0.5333*	0.5398
	(0.3064)	(0.5006)
Number of individuals	422,000	433,000

Notes: Table shows effects of exposure to the FSP from conception through age 5 on individual's percentile rank of earnings within birth cohort at age 32 for men and women born between 1955 and 1980. ***,**, and * denote significance at the 1, 5, and 10 percent level. SEs clustered at the county of birth level. Dollar amounts in real 2015 dollars and are adjusted using the CPI-U. The number of individuals is rounded to nearest 1000. Controls include fixed effects for white, birth year, and county of birth.