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LONG-RUN EFFECTS OF FOOD ASSISTANCE: EVIDENCE FROM THE FOOD STAMP PROGRAM AND ADMINISTRATIVE DATA

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ABSTRACT

Previous work using mostly self-reports shows large, positive effects of early-life exposure to Food Stamps on self-sufficiency, health, and well-being-lasting well into adulthood. We combine this same adoption timing with administrative data on earnings, employment, and use of disability benefits. Women born in counties with Food Stamps available in early life had 3 percent higher earnings at age 32. Effects were larger in counties with another in-kind food program in place before Food Stamps. Food Stamps relied on the other program's preexisting administrative eligibility determination. Our results establish links between pre-existing administrative infrastructure and the later-life impacts of Food Stamps.

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1 Introduction

Adverse conditions have both immediate and long-run effects on individuals and families. Experiencing adverse conditions or negative shocks during critical times for child development, typically thought of as lasting from conception through age 5, can have particularly detrimental, long-lasting effects. A large literature in economics, sociology, public health, and epidemiology finds that negative early-life shocks result in worse 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 these adverse conditions, the federal government has established various means-tested social assistance programs and social insurance programs, which we collectively call the social safety net. These are meant to improve the lives of those who experience adverse economic conditions. Much of the literature on the effects of these programs has focused on their immediate short-run impacts on health, education, and recipients' labor-market outcomes. However, analyzing the effects of early-childhood exposure to government programs on outcomes across the life course is crucial for accurately assessing the costs and, particularly, the benefits associated with these programs. This is especially important when considering effects on human-capital accumulation, health, and program participation; many of which may not materialize until adulthood. Moreover, because the social safety net, in many cases, explicitly provides benefits to children; these programs can be viewed as investments in human and health 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 deciding how to allocate funding (e.g., the marginal value of public funds measure suggested by Hendren and Sprung-Keyser, 2020).

We provide new evidence using administrative data on the effect of exposure to the Food Stamp Program (FSP) in early childhood. Specifically, we examine outcomes according to the share of time from conception through age 5 that the FSP was in place in children's county of birth. We use this plausibly-exogenous variation created by the initial implementation of the FSP to identify the effects of exposure to food stamps in early childhood on adult employment, earnings, and use of the Social Security Disability Program. The FSP was implemented in different counties at different times due to idiosyncratic reasons, creating cross-sectional and time-series variations in individual's exposure to food stamps. Prior research has shown the variation created by the initial implementation of the FSP is 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 FSP exposure on infant health (Almond, Hoynes, and Schanzenbach, 2011) and labor supply (Hoynes and Schanzenbach, 2012).

We contribute in two important ways to the existing literature on the long-run effects of the FSP and the broader social-safety net by providing new estimates on the effects of food stamp rollout. First, we use large, unique administrative panel data on earnings and SSDI. By contrast, most of the existing evidence relies on self-reported data (exceptions include subsets of studies about effects on mortality/moving (Bailey et al., 2024) and about effects on incarceration (Barr and Smith, 2023)). This use of administrative data is an important addition to the literature due to well-documented issues with misreporting of earnings (Bollinger et al., 2019) and program participation (Meyer, Mok, and Sullivan, 2015).

Second, we examine a previously unstudied aspect of FSP implementation, considering whether or not counties had a pre-FSP way of assessing eligibility and enrolling people in a similar program. To do this, we examine an understudied aspect of FSP implementation: In most counties, the FSP replaced an already existing in-kind food distribution program—the Commodity Distribution Program (CDP). Where CDP was in place, counties already had infrastructure in place to determine eligibility for the CDP which generally had harmonized eligibility rules with those of the FSP as CDP shrank and the FSP grew over time. Event studies show that the FSP was able to ramp up enrollment almost immediately in the places where CDP was located before Food Stamps, documenting the importance of having infrastructure in place to determine eligibility for new programs as well as the value of local experience in determining eligibility when ramping-up a new program.

Our large, unique panel data allow us to match the Food Stamp adoption status of an individual's county of birth to the same individuals, using place of birth from their applications for a Social Security Number (documented in the administrative NUMIDENT file). Social Security Numbers for people in our data are issued after the NUMIDENT is filed and the resulting Social Security Number is what the Social Security Administration (SSA) uses to track earnings and eligibility for SSA programs and this same SSN number is also used to identify people in tax data. We can thus link the administrative data on the same people's entire earnings history and use of the Social Security Disability Insurance (SSDI) program to individual's county of births' timing of Food Stamp adoption. Thus, our data include labor market and social insurance participation data for approximately 1 million individuals. With this large data set, we can both detect effects that would likely be missed in smaller, survey data sets and additionally use well-measured outcomes that do not suffer from reporting errors, which are extensive in self-reported earnings data. We are also able to use balanced panels of adults from ages 25–38 to look at the effects of FSP exposure in early childhood or

in utero on employment and earnings, unlike other studies that rely on outcomes measured with point-in-time survey data which pool people of different ages. Our study thus builds on the work of Hoynes, Schanzenbach, and Almond (2016), who use self-reported PSID data to establish that early exposure to the FSP led to improvements in health for adults and in economic self-sufficiency for women; and the work of Bailey et al., (2024) who precisely estimate effects on related outcomes using large-scale Census data.

We find that exposure to food stamps in an individual's county of birth from conception through age 5 increases earnings for women at age 32 by approximately 2.7 percent or $694 (in real $2015). However, this is an intent-to-treat effect as not all children in a county with the FSP in place participate in the Food Stamp Program. Thus, we use other data on participation to back out a treatment effect on the treated, under some assumptions. Data from the 1979 Current Population Survey (CPS) suggest that nearly 1 in 5 children under age 6 were in households where someone received food stamps. Under some assumptions, this participation of about 20 percent among families with children aged 0 to 5 could be used to approximate the "first stage" estimate of the effect of rollout on participation. If we assume that the only effect of FSP rollout was to increase take-up of food stamps, and participation during the in-utero period was the same as participation for these zero-to-five-year-olds, then the relevant CPS number for girls suggests a first stage effect of rollout of increasing take-up by 17.5 percent. Then, the associated implied treatment effect on the treated would be about 15.6 percent or $3,966 (dividing our reduced form above by the "first stage" of rollout of 17.5 percent for girls). This is an economically meaningful effect but also plausible given the size of the program's benefits, as we discuss further below. These reduced-form findings 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. Point estimates for the effects on the percentiles of earnings within birth cohort are also positive and of a similar magnitude.

When we consider the role of administrative infrastructure by stratifying the sample by whether CDP was in place pre-Food Stamp adoption, we find something surprising. While one might expect the effects of implementing the FSP would be larger where there was no other food program before Food Stamps because another program would also improve outcomes, we find the opposite. We find slightly larger effects of Food Stamp Program rollout in counties that had the CDP before the implementation of the 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). This is because the FSP as we show below was able to retain caseloads in the program while providing vouchers which bought higher quality food of various types. Because the CDP existed in about 90 percent of counties prior to the adoption of the FSP, for most counties the relevant pre-FSP counterfactual is one where there is a possibly low value to participants (in-kind) commodity program rather than no food assistance program. Regardless, the presence of the CDP in one's county meant that county officials were running a program before adopting the FSP and also were able to determine eligibility for the new FSP right when they implemented the FSP. We interpret this as strong evidence that effective public administration of the determination of eligibility for programs (Ko and Moffitt, 2024) coupled with not overwhelming administrative burden in signing up for the FSP for individuals (Herd and Moynihan, 2019) and high value of benefits for those getting them were important for understanding the 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; and 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. While we can't explicitly model the role of parental investments, we do model the effects of rollout according to the age of first exposure and find strong evidence with different data corroborating the evidence from Hoynes, Schanzenbach, and Almond (2016) and Bailey et al. (2024) that in-utero and early childhood exposure to the FSP improves adult self-sufficiency.

3 The Rollout of The Food Stamp Program: Background

In 2020, the U.S. marked the 55th anniversary of the War on Poverty. During the Johnson and subsequent Nixon Administrations, 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 new incarnation of the FSP, which President Kennedy re-started as a pilot program in 1961 (it had existed previously in a very different form). At that time, food stamps were vouchers for a specified dollar amount of unprepared foods purchased at participating stores. In the early days of the program which we study, 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).[2] This is different from the current FSP, where people get benefits equivalent to the bonus part of the coupon. Thus, the marginal propensity to consume food out of Food Stamp Program benefits was likely higher than now, because families had to both incur a cost before receiving the benefits and could only use the "purchase requirement" stamps on the same unprepared foods as they could use the bonus coupons to purchase.

The FSP 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 FSP was first being implemented, there was an additional requirement that counties wanting to adopt the FSP had to end their involvement with the Commodity Distribution Program (CDP), which was an in-kind program that provided families with commodities. Thus, the CDP was the predecessor to the FSP, and it operated in about 90 percent of counties pre-Food Stamp adoption. This meant that most counties had a process in place to assess eligibility rules pre-Food Stamp Program adoption as the eligibility rules were mostly harmonized across the two programs. We conclude that this lead to the rapid effects of FSP roll-out on participation as documented by Hoynes and Schanzenbach (2009) and also shown below. Starting in 1979, the FSP was changed to eliminate the purchase requirement and further standardize the program, and thus we end our analysis sample then.

Today, the FSP, 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, SNAP served nearly 1 in 7 Americans. Under current requirements, SNAP recipients need to have a gross income under 130 percent of the poverty guideline and a net income under the poverty guideline to participate.

Neoclassical economics suggests that without the purchase requirement, the Food Stamp Program—for families who would consume at least as much food as the bonus coupon—so-

[2] The purchase requirement was removed as of January 1, 1979. Our estimates include births from 1955-1980, although we also show that estimates are slightly larger if we restrict ourselves to ending our sample with births in 1974, all of whom were in utero or up to age 5 before the end of 1979. We discuss this more below.

called inframarginal consumers—the effect of food stamps should be similar to the effects of cash. And in the absence of the purchase requirement, those that would spend less than the bonus coupon would be constrained to spend more on food than they would choose in the absence of the program. However, in this period, the presence of the purchase requirement constrained families in that they also had to use the purchase requirement "coupons" to buy unprepared food. Thus, with this initial form of the FSP, those who would ideally spend less than the full combined amount of the bonus vouchers plus the purchase requirement—so-called extramarginal consumers—the FSP could induce them to spend considerably more on food than would a cash grant of the size of the bonus coupon. The fact that there was a purchase requirement also suggests that adoption of the FSP 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 and only for food.[3] This might constrain families to spend more on food than they otherwise would, and might also have important effects on children's health and well-being that translate into the positive effects on early adult earnings we document below.

The existence of the CDP in most counties implies that the right counterfactual for effects of Food Stamp Program rollout is having a possibly inefficient in food delivery but competent in determining eligibility in-kind commodity program rather than no food assistance program. The presence of the CDP before the FSP has two key implications. First, counties already had in place a way to assess the eligibility of families for the CDP pre FSP adoption, and this process could be used to assess eligibility for the FSP when it came in. This meant that important infrastructure needed to get the FSP up and running was already in place. Second, the CDP, like other in-kind aid programs, provided inconsistent and anecdotally recorded low-quality food, as documented in Senate hearings (e.g., U.S. Senate Select Committee on Nutrition and Human Needs, 1971), which suggests it might not have had much of an impact on child outcomes despite being able to enroll people.

We build on existing literature that documents the positive effects of implementation of food stamps during early childhood on adults' outcomes. A series of papers have studied the rollout of the FSP. Hoynes and Schanzenbach (2009) document the plausibly quasi-random nature of the rollout of the FSP in 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

[3] Beatty, Bitler, and Van der Werf (2024) find an increase of around 5 percent in employment in food stores associated with the rollout of food stamps, suggesting that sales went up with rollout, perhaps in part due to the purchase requirement's rules about spending the full amount expended to get the bonus coupons on food.

and the idiosyncratic pattern across time and place in which counties adopted food stamps and when.

Hoynes and Schanzenbach (2009) also find that contemporaneous 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 FSP also had positive effects on infant health and rules out significant pre-trends in birth outcomes before FSP adoption. Hoynes and Schanzenbach (2012) document that the program led to a relatively small decline in contemporaneous labor supply as would be expected given estimates of income effects from the literature and the low benefit-reduction rate in the program. Hoynes, Schanzenbach, and Almond (2016) establish that early exposure to the FSP 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 et al., (2024) show how long-run outcomes measurable in the decennial Census and American Community Survey 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 individual's county of birth led to improvements in human capital, self-sufficiency, the quality of their neighborhoods, increased longevity, and declines in the probability of being incarcerated in adulthood. Barr and Smith (2023) look at effects of early exposure on criminal behavior and find that early exposure to food stamps led to declines in criminal behavior, and other work looks at other outcomes like voter registration and voting (Avenancio-León, Howard, and Mullins, 2024).

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.[4] We also can avoid comparisons which include adults of various ages such as those of Bailey et al., (2024) which include point in time estimates of earnings and other outcomes for those of different ages and exposures but do not have a balanced panel in age.

[4] Meyer, Mok, and Sullivan (2015) document an increasing problem with under-reporting of many safety net programs and incorrect imputation of missing data over time. Bollinger et al., (2019) and Bollinger and Hirsch (2013) document issues with self-reported earnings.

4 Data

4.1 Rollout data and exogeneity of implementation of the Food Stamp Program

Our panel of exposure to the FSP 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 for which we thank them (Hoynes and Schanzenbach, 2009, 2012; Almond, Hoynes, and Schanzenbach, 2011; Hoynes, Schanzenbach, and Almond, 2016).

We merge these rollout data with data from the Bureau of Economic Analysis' (BEA's) Regional Economic Information System (REIS) to control for other federal government spending on social welfare programs as well as the business cycle as follows. First, we only control for social programs where spending is actually measured at the county level according to the REIS documentation, rather than being allocated to counties from a higher level of geography. Second, we control for programs that are not closely linked to food stamps. Thus, for example, we exclude from our controls spending on the Aid to Families with Dependent Children Program, for which, during the time period, participation conferred eligibility for food stamps or simplified it. This means that among social programs, we only control for spending by Medicare or public funding of military health medical care, and for federal spending on Social Security Income. Second, we adjust for the business cycle by controlling for average real per capita income and unemployment compensation, as there are not sources of unemployment rates at the county by year level during this period. (These data are compiled from the Hoynes, Schanzenbach, and Almond's (2016) replication, online US Bureau of Economic Analysis Regional Economic Information System 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 these other measures of safety net spending and the business cycle during the year of birth. We augment these controls with data on the predecessor to food stamps, the Commodity Distribution Program (CDP). Our information on the presence of the Commodity Distribution Program (CDP) in counties at the time they adopt the FSP comes from the National Archives as well as from administrative data on Aid to Families with Dependent Children participants from caseworkers that ask about the form of food assistance programs in place. (We discuss CDP further below.)

We then create consistent county measures across time. For the majority of counties, this is the same concept as the county today. But for some locations, county boundaries changed considerably. For the vast bulk of these locations, we created "super counties," which are the smallest geographic 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.[5] These contextual data are merged to individual-level panel data on earnings and the 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 Summary 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 FSP. We note that we are in part 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 in several of these other papers showing the share of counties that had adopted the FSP from 1960 through 1975 by year. This shows that there is considerable variation over time in when counties adopted the FSP. Figure 2 replicates a figure showing the extensive cross-sectional variation across counties in the timing of their adoption of the FSP. Based on the data presented in this figure, within most states, there is considerable variation in when the FSP 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 for state and time factors in various ways, which should help account for the effect of any statewide programs or legal decisions (e.g., state adoption of Medicaid and ramping up of Medicaid over time).

Figure 3 shows event-study pictures of effects of food stamp implementation on per capita participation in the FSP in each county by time since adoption of the FSP. Period -1 is omitted, and it is clear from this graph that food stamps ramped up quickly from 0% participation (0 participants per 100 persons in the county) pre-implementation to almost 6% participation (6 participants per 100 persons in the county) after adoption. This is a remarkable rapid ramp up, due largely as we discuss further below to the existence of the Commodity Distribution Program before implementation.

[5] We exclude Virginia, Alaska, and Hawaii from our panel. For Virginia, this is due to challenges with creating consistent county measures with appropriate controls for the many cities that are not part of counties; for Alaska, boroughs change geography, and a number of controls are unavailable; and for Hawaii, this is due to a lack of relevant control variables.

Of course recent work about heterogenous effects across place and time in average effects in standard two-way fixed effects estimates and event studies (e.g., Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021), Callaway and Sant'Anna (2021)) raise concerns about bias in two-way fixed effects estimates if such heterogeneity exists. Thus, in Figure 4, we explore the variation in the treatment effects of adoption of food stamps on participation per capita in the program according to each treatment-control comparison of the differences in differences estimate by timing cohort. The x-axis for Figure 4 shows the weights from each adoption-timing-period's difference in difference comparison of adopting places compared to places that don't change their adoption status at the same time and the y-axis contains the point estimate for each adoption-timing-period comparison (including earlier-adoption places against not-yet-adopted places as well as comparisons of later-adopting places versus early adopters with no changes). Each such estimate is a point on this graph, and the dashed horizontal line shows the overall TWFE estimate of 0.057. This chart shows remarkable consistent average estimates of the effects of adoption at each value of the weights, although there is more variation in places with smaller weights and more consistency among the places with larger weights. Table 1 reports an alternative to our TWFE estimates of effects of adoption on caseloads per capita using de Chaisemartin and D'Haultfœuille (2020)'s approach, comparing those adopting FS to those who didn't adopt in the same two-year period, of 0.053. Taken together, we interpret these estimates as evidence that this form of heterogeneity in treatment effects across place and over adoption timing is not problematic in our setting.

4.2 Earnings, labor supply, and disability program involvement data

Next, we discuss the long-term outcome data we use which span young (minimum of age 25) to middle adulthood (maximum of age 38). Our data are drawn from administrative earnings and Social Security Disability program participation for a one-percent extract of all Social Security numbers (SSNs) ever issued, commonly referred to as the Continuous Work History Sample (CWHS). SSNs are included in the CWHS based on the last four digits of the SSN, which are not tied to geography (see Smith (1989) for more detail). Once an SSN is selected for inclusion in the sample, the SSN remains in the sample throughout that person's lifetime, and thus the data track individual's wage and salary earnings as reported for tax purposes by employers, with exceptions noted below. Data for the selected individuals are drawn from several Social Security Administration files. A key variable in this analysis, the place of birth, comes from the NUMIDENT file. The NUMIDENT file contains the date of birth,

race, sex, and date of death (if applicable) as well as the place of birth for all individuals included in the CWHS.[6] While in the overall CWHS-NUMIDENT file, a non-trivial share (about 3 percent) have no information about place of birth; in our ultimate sample of individuals born from 1955 to 1980, the share with no place of birth is well under 1 percent. The individuals with no recorded place of birth are dropped from the sample as we cannot identify whether they were born in a place with the FSP in place. A larger share of observations (about one-third) are dropped because they are born abroad or in a U.S. territory and present us with the same difficulty in establishing treatment status; leaving an eventual sample of nearly 1 million individuals.

Earnings data in the CWHS are drawn from the Master Earnings File (MEF). The CWHS includes Federal Insurance Contributions Act (FICA)-covered measures of 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 IRS Wage and Tax Statement Form W-2 for each year from 1978 to the present ("W-2 earnings"). Each of these has advantages and disadvantages.[7] Thus, to fully exploit the

[6] 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 practice, in our analysis, we select one entry per individual in the NUMIDENT, choosing the most recent entry with a non-missing place of birth.

[7] Earnings data in the CWHS are drawn from the Master Earnings File (MEF). The CWHS contains Federal Insurance Contributions Act (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. First, the FICA-covered earnings are censored at the taxable maximum. This censoring is particularly problematic in years prior to 1978 although that is before our sample starts. 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 6 percent of the U.S. workforce works in non-covered employment (SSA, 2015). As recently as 1976, however, 10 percent of the labor force worked in non-covered employment (GPO, 1997), but this share was lower for our time period. While using W-2 earnings would circumvent some these issues, the W-2 earnings data are not without their own limitations. Unlike the FICA and Medicare earnings data in the CWHS data, no adjustments are made for delinquent (slow) postings or correction (adjusting errors) postings after the close of the initial posting year. 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 given our sample. 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

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 they are available and when they are 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 was removed (and Medicare earnings corresponded with earnings). In addition, to the extent the individual has self-employment income, we also account for that in our measure. We convert the earnings measures into real $2015 earnings, using the CPI-U. We also use the earnings data to construct a measure of labor supply for each person in each year as follows: Our labor supply measure is set to one during calendar years in which each person has non-zero total earnings and zero otherwise.

Finally, the CWHS data on SSDI benefit entitlement are obtained from the Master Beneficiary Record (MBR). The MBR contains entitlement, and termination dates for Social Security Disability Insurance (SSDI) entitlements as well as benefit amounts, payment status, type of benefit 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 adjudicated by the Social Security Administration) and have a sufficient period with no meaningful work activity. We note that there is an important advantage to using these administrative data on social insurance as well as administrative earnings, given issues with misreporting in survey data on participation in programs and earnings, which may lead to bias (e.g., Meyer, Mok, and Sullivan, 2015, for transfers; and Bollinger and Hirsch, 2013, and Bollinger et al., 2019, 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. Around 90 percent 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 in the following paragraph, we also wanted to focus on ages where we think people have completed schooling and are somewhat far along in their work life and years where we have a balanced panel of ages in our panel data set. Appendix Figure 6 shows average real earnings for persons in our panel at various ages from 25-34. Note that, in Appendix Figure 6, means for ages after around age 34 do not have all the birth cohorts contributing to the mean earnings. Nevertheless, it is also clear that there is no large slope

examine individual earnings is age 25. Moreover, our main results are for earnings in calendar year 1987 or later, well after when previous analyses indicated there was a problem with the W-2 earnings data.

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, both flattening out near 50 (an age which is out of our sample period, not shown here).[8]

These limitations on cohorts and ages were balanced to address 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 FSP, three years of the potentially misleading FICA earnings data would be used (see footnote 7). 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 FSP 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 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 37, excluding those who die prior to age 37.

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 FSP, estimating the following regression:

$$y_{ijbm} = \beta_0 + \beta_1 * FSP_{jbm} + \boldsymbol{X'_i} * \boldsymbol{\gamma_1} + \alpha_j + \delta_b + \boldsymbol{Z'_{jb}} * \boldsymbol{\gamma_2} + \theta_j(b) + \varepsilon_{ijbm}$$

where y_{ijbm} is the outcome (real earnings, any employment, SSDI use) of child i who is born in county j, in year b, and in month m. FSP_{jbm} measures the share of the time from conception through 60 months that the FSP was in effect in the child's county of birth j. $\boldsymbol{X_i}$ includes demographic characteristics of a child such as being white (as opposed to non-white) status from the NUMIDENT file.[9] The terms α_j and δ_b denote county of birth and year of birth fixed

[8] 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.

[9] While it may be possible to disaggregate the race data further, using the white/non-white classification is consistent with previous recent research using the CWHS. For example, see Song and Manchester (2007). Even

effects. They capture any time-invariant unobserved differences across counties and unobserved shocks common to all children born in the same year. (Age fixed effects are not typically included as the main results are all estimated when the children are all the same age, although they are added when relevant.)The term $\boldsymbol{Z_{jb}}$ is a vector of county controls for the business cycle and other aspects of the safety net (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). $\theta_j(b)$ controls for county-level or state-level linear time trends, which might be correlated with child development. 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) and Bailey et al., (2024). 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). Our main earnings models include adults aged 32 born in years of birth 1955-1980 (and thus each adult appears once), but we also explore pooling various ages. Our measures of any earnings or use of SSDI include all the ages which are available in our panel.[10]

5.1 No effects on employment

First, we consider the extensive margin. In our data, we model employment as having positive earnings in any calendar year that the individuals are in the data. Table 2 shows exposure to food stamps had no effect on the probability that either men or women had positive earnings in any calendar year. 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 0.5 percentage points while the baseline mean share of years with non-zero earnings is 0.77. Effects for men are similarly small in magnitude and precisely estimated.

when researchers disaggregate the race data further, only the following categories are used: white, black, and other (see Duggan, Gillingham, and Greenlees (2008)).

[10] Any earnings and any SSDI involvement are measured across the entire time people are in the CWHS to capture the broadest measure of these outcomes. We also find no effect on the extensive margin of labor supply during the year people turned 32.

5.2 Women's real earnings are higher when the Food Stamp Program is implemented, effects for men are mixed

Table 3 shows our first set of results about earnings at age 32 for exposure to food stamps for women and Table 4 shows the same for men. The first column reports results for our real total earnings measure, column 2 shows results for real W-2 earnings, and column 3 for real FICA earnings. Table 3 shows that being exposed to food stamps from conception through age five leads to earnings being between $477 and $727 higher at age 32 in real $2015 for women. These estimates are all significant at the 10 percent level, and two of the three are significant at the 5 percent level. The second panel of the table contains both the mean outcome and mean exposure to the FSP. These translate to women's earnings at age 32 being from 2 to 3 percent higher if a child was exposed to food stamps the entire time from conception through age 5.

Table 4 shows that the effects are insignificant for men, and much 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 5 shows an event-study-like figure for total earnings for women. (It is not an actual event study because our key independent variable is the share of time from conception to age 5 the individual was exposed to the FSP). Unlike usual event studies, here the *x*-axis denotes the age at which the FSP rolled out in the child's county, with numbers to the left meaning it was rolled out earlier in a child's life and those to the right meaning it was rolled out later in the child's life. 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 two-year bins for age at first exposure to the FSP. As usual in this literature, we leave out the fixed effect corresponding to one pre-treatment two-year period—here ages 6 to 7—and include county fixed effects.

First, note that Figure 5 shows no evidence of pre-trends (here pre-trends would mean children older than age six and seven at rollout would have significant effects on participation but instead they have small and statistically insignificant effects of exposure). Second, the effects are zero at age 10 to 11, where previous studies have set the event study effects of food stamps to zero. Next, note that there is an increase in earnings after food stamp implementation for children exposed any time 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. This is consistent with each year of exposure in early childhood having cumulative and proportional effects.

Table 5 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 through 6 are smaller. Columns 3 through 6 control for population in the county of birth in 1960, and for some columns, this reduces the sample size slightly due to the inability to measure population for some additional 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 and UI spending per capita as well as for per-capita real income. This further reduces the sample as the REIS data are not available for the full 1955 to 1980 period.[11] 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 FSP has significant long-run positive effects on earnings for women.

5.3 Magnitudes of earnings effects for women are plausible

These earnings effects are non-trivial in magnitude and economically meaningful, as noted above, with our preferred outcome (total earnings) showing effects of food stamp rollout on earnings of around 3 percent. 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 FSP participation rates of 6 percent. But this is overall participation. One might expect participation rates to go up by more for some groups, such as families with children.

We show that participation is higher among families with children using the March Current Population Survey (CPS). We use the earliest CPS Annual Social and Economic Supplement survey with food stamp information and summarize household participation in the FSP and household benefit levels in the 1979 calendar year. These numbers are presented in Table 6. We find that 18.3 percent of boys aged 0 to 5 and 17.5 percent of girls aged 0 to 5 were in a household with someone on the FSP.[12] Interestingly, there is little variation in this participation rate across ages (or by age and gender). The average nominal

[11] For the estimates using the REIS data, we must also leave out cohorts born before 1960, as these variables are unavailable then.

[12] We cannot look at in-utero exposure in the CPS because we don't know which women are pregnant.

benefit the family received was around $200 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 percent of children aged 0 to 5 were in a household on the FSP, and the average household benefit was $814. Note that if 18 percent 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 percent increase in earnings compared to 17.5 percent participation rate in food stamps for girls under 5/exposed in utero suggests a meaningful treatment on the treated effect if one treats 17.5 percent as the relevant "first stage" for the effect of FSP implementation 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. The US Department of Agriculture's 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 the 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. One comparison is Brown et al., (2020) who find evidence that the expanded Medicaid program of the late 1980s and early 1990s had substantial effects. They find each additional year of Medicaid coverage on average was associated with an increase in cumulative earnings of $1,784 by age 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 margin and found that there is no impact of Food Stamp Program 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.

5.3 Effects vary across the earnings distribution for women

We next turn to estimates of the probability that real earnings are above various multiples of $5,000. Figure 6 focuses on effects for real total earnings being above $5000, $10,000, and so on, 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 percent 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.

Appendix Figure 2 shows analogous estimates for the probability men's total real earnings are above the same cut-points at age 32. We note that the point estimates for men are uniformly smaller and insignificant (but always positive) at the ages where effects for women were large and significant. This is true even if we look at one of the same cut-points—$35,000—where the on the probability that women's earnings are greater than that amount are significantly different from 0.

5.4 Effects for women are robust to using more years of earnings, other ages, percentiles, other controls

To date, all of our estimates are calculated for a single year of age. In the literature on lifecycle earnings, analysts worry about effects estimated for a single year of age. In Appendix Table 1, we instead average earnings across ages 32 to 34. The effects for women are very similar to those reported for age 32, $783 compared to $694 for the same specification in column 1 of Table 3. The point estimate for men is larger here in Appendix Table 1 than in Table 4 column 1 but are still insignificant. The similarity between our main results and these additional results for earnings at other ages 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 between 1955 and 1974, and again, the effects for women aged 32 to 34 are $829, very similar to our main finding of $694. Appendix Table 3 looks at effects for the same cohorts born between 1955 and 1974 at ages 36 to 38. Here the point estimate for women is still positive, but it is smaller and insignificant at $240. Next, in Figure 7, we show the effects of FSP rollout across each age in our panel, from ages 25 to 39. We find effects are extremely small in magnitude and negative for 25- to 27-year-olds, turn positive for 28-year-old women, and then become statistically significant at the 5% level for women aged 32 and 33, peaking at age 33. They then decline to near zero by age 37 and are again small and negative for ages 38 and 39.

Next, we follow the work of Chetty, Hendren, and various coauthors (e.g., Chetty et al., 2014) 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 0.34 percentile rank points, which is non-trivial, although the estimate is not significant at the 10% level.

We also explore robustness to removing the control for white, and to including state-by-year fixed effects. Appendix Table 5 shows our preferred estimates (column 2, $694) along with estimates leaving out the control for the woman being white (coefficient is $743 rather than our main estimate of $694). The last check is to include state-by-year fixed effects. This leads to an insignificant estimate, but is also quite demanding on the data, leaving estimates to be identified by differences in a state and birth year by month of adoption or no adoption.

Finally we explore whether effects are larger in counties with higher pre-FSP poverty levels. Appendix Table 6 shows there are no larger effects in counties with median income below the median.

5.5 We find no effects for men or women on involvement with the SSDI program

In Table 7, we focus on the effects of food stamp exposure on use of SSDI (benefit receipt). These are cumulative measures, 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. We find no significant evidence that FSP exposure increases the use of SSDI by either gender. The point estimates are very small in magnitude as well.

5.6 Mechanism for rapid ramp-up: Pre-existing CDP program infrastructure for eligibility determination

The event studies noticeably show an almost immediate increase in the Food Stamp Caseload when the program was put in place, undoubtedly contributing to the relatively large in magnitude effects on earnings. Next, we turn to a discussion of a possible explanation for this immediate ramp up of the FSP, the presence pre-FSP adoption of the Commodity Distribution Program (CDP). CDP was an in-kind program, delivering commodities to areas, where non-profits or local governments would take over the process of distributing foods while local governments monitored eligibility for CDP. Thus, CDP areas had experience in the logistics of implementing a program with quite similar eligibility rules where rules were harmonized. Figure 8 shows participation in the CDP and the FSP across time nationally through 1968. This shows that the CDP was not insubstantial in the early days of the FSP, with it being larger than the FSP through 1968. Interestingly, as the FSP spreads out over time, increasingly the eligibility rules for CDP and the FSP were harmonized (this ensured neighboring counties didn't face different eligibility for these two programs). Figure 9 shows

the change in the share of people participating in one of these two food programs as counties transition from CDP to the FSP. (The underlying data for this figure came from data at the National Archives showing the CDP caseloads right before the transition and FSP caseloads after, for counties transitioning through 1968 along with data on AFDC and use of CDP.) First, note that most counties experienced at most a small drop in participation with a large mass near 0, although there are some places that had larger declines. This large mass near 0 means the transition from CDP to the FSP did not lead to large declines in participation. We conclude this is due to the fact that the CDP counties already had some government entity determining eligibility, often using identical income thresholds. This likely made the transition to the FSP easier, and marks what Ko and Moffitt (2024) call "effective program agents."

Next, we explore the ramp up of food stamps. Figures 10 and 11 show event studies of the per-capita caseload in food stamps as a function of the year that a county adopted the FSP, by whether the county had the CDP program before it adopted the FSP (Figure 10, "ever CDP") or not (Figure 11, "never CDP"). Like most event studies, these estimates condition on county fixed effects and show the point estimates on dummies for adoption years in event time (dummies for being a specific number of years before or after adoption). Figure 10 shows the event study for the large majority of counties that had the CDP before adopting the FSP. (As noted above, we will call these the "ever-CDP" counties.) This figure shows that the program led almost immediately to an increase from no participation to a 6 percent rate of use of food stamps per capita. Figure 11, by contrast, shows that, for the 10 percent of counties that never had the CDP before the FSP, participation edged up slowly and didn't reach 6 percent until after 2 years. Thus, in Section 5.7 below, we present some results for the sample of ever-CDP counties given the clean event study there, which suggests that 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—used to get the treatment effect on the treated of the FSP 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 were totally ineffectual, it

would suggest the first stage for the FSP 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 the implementation of the FSP led to increases in earnings without any corresponding large increase in persons obtaining some form of food assistance

5.7 Effects for women are larger for counties with CDP in place before the FSP

Finally, Table 8 reports our preferred estimate of FSP exposure for the subset of counties where the CDP was present before food stamps (ever-CDP counties). The estimate for women is a slightly larger $735 compared with the $694 for the full sample, and that for men is 182. Thus, instead the effects of this change from CDP to the FSP being smaller because of the pre-existing program's benefits, if anything the point estimates show the effects in the ever-CDP places are larger than those in the never-CDP places, although we cannot reject that the effects are the same.

6 Why are the effects focused among women?

Our results raise one obvious additional question—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, although less so with the findings of Bailey et al., (2024).

Second, we can use the NUMIDENT data to explore a proxy for changes in marital status. Appendix Table 7 shows the effects of exposure to the FSP on the number of name changes filed for at the Social Security Administration. We find no significant effects for women or men. Thus, the effects are not driven by any change in the probability of marriage that was a result of exposure to the FSP.

Third, 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 already 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.

While one might be concerned that we are not looking in the right place in the life-cycle and might be missing effects for men. We did our best to rule this out with the analysis above in sections 5.3 when we considered effects across the distribution and section 5.4 where we looked at different age groups.

7 Conclusion

We have comprehensively shown that the rollout of the Food Stamp Program (FSP) across counties led to an economically meaningful and statistically significant increase in earnings for women of about 3 percent while a back-of-the-envelope calculation of the first stage of rollout on participation in the FSP of about a 17.5 percent increase in participation in the FSP. Together, these imply a treatment effect on the treated near 17 percent. 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 administrative earnings data. There is no impact on employment or Social Security Disability Insurance involvement for men or women. Most counties had another food assistance program—the Commodity Distribution Program (CDP)—in place before the FSP. We find that effects are larger in counties with CDP, which we attribute to the fact that these counties already had an infrastructure for evaluating eligibility for families in place before the FSP was implemented, combined with the fact that the CDP was likely inconsistently run and likely provided low-quality foods. These results are meaningful for informing discussions about the long-run benefits of safety-net programs.

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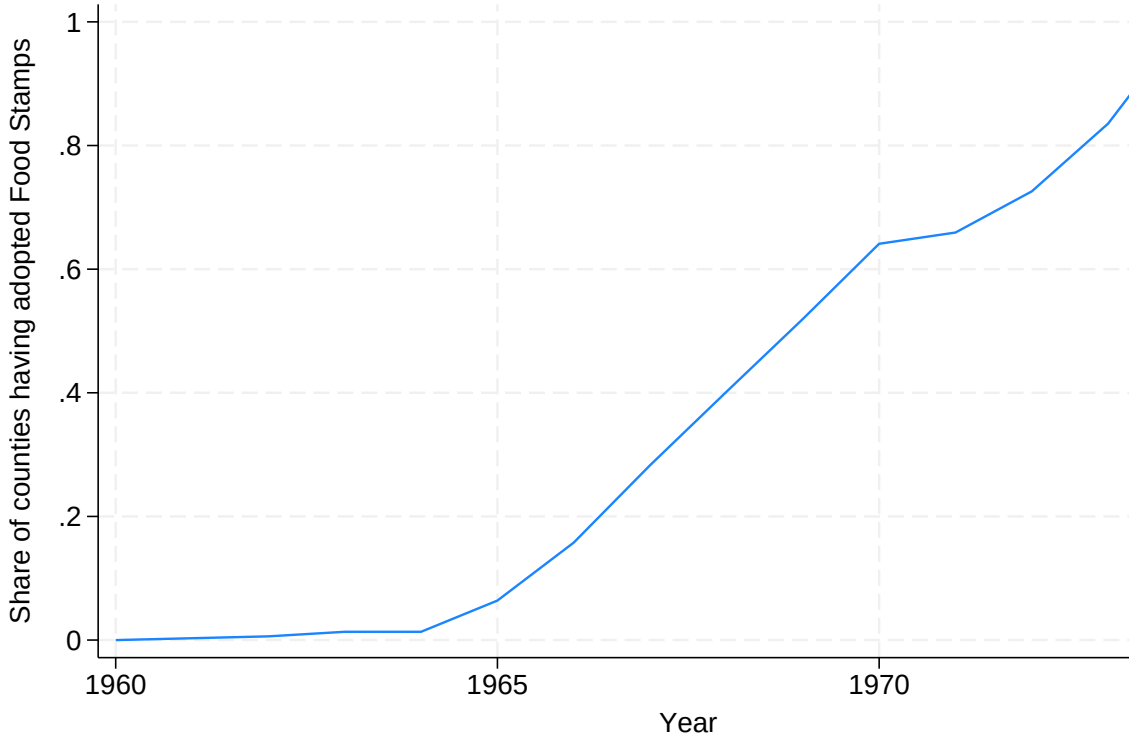
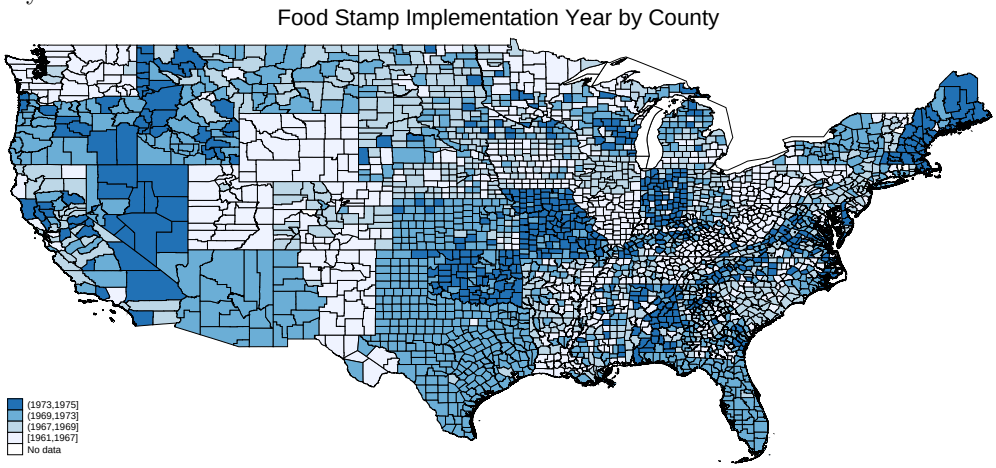
Figure 1: Share of counties adopting the Food Stamp Program



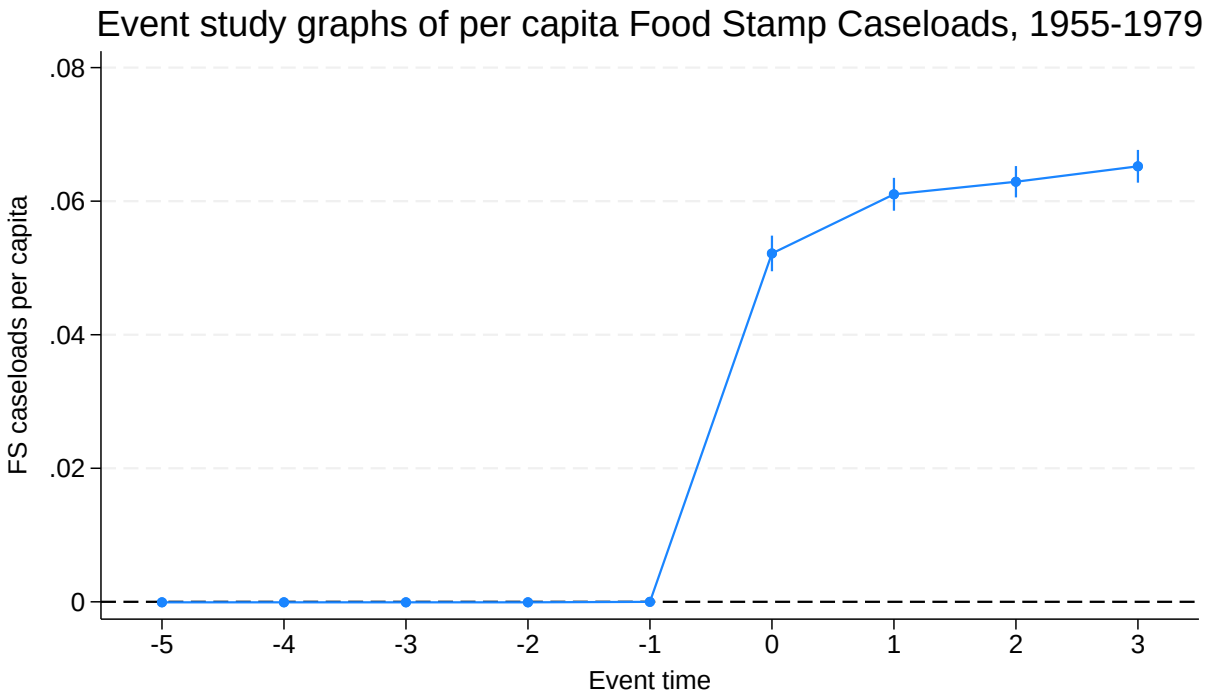
Notes: Figure shows share of counties adopting the Food Stamp Program (FSP) by year. Based on Hoynes and Schanzenbach, 2009.

Figure 2: Cross-sectional variation in adoption of the Food Stamp Program by county



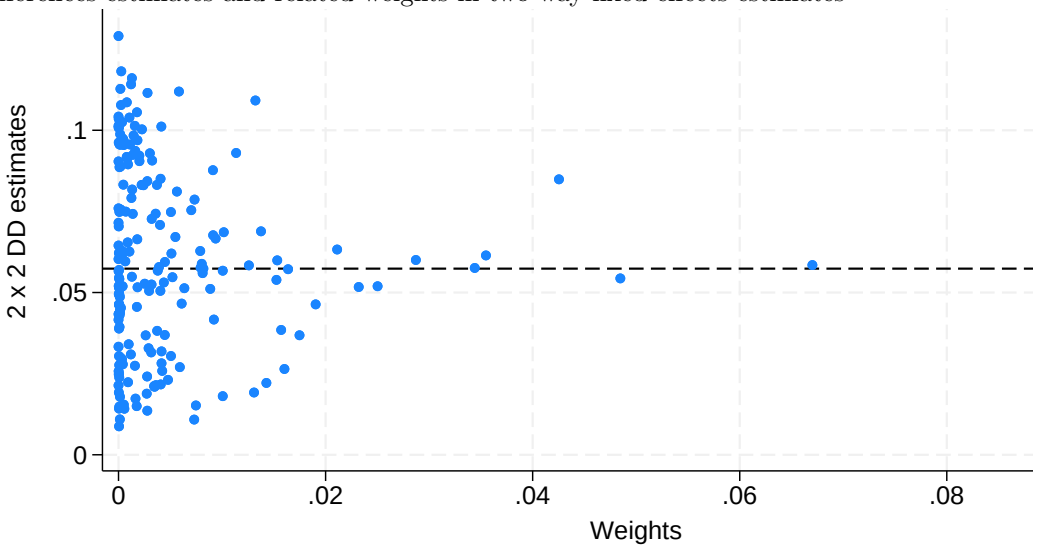
Notes: Figure shows timing of adoption of the Food Stamp Program (FSP) by county. Based on Hoynes and Schanzenbach, 2009.

Figure 3: Food Stamp Program caseloads per capita event study as a function of Food Stamp Program adoption



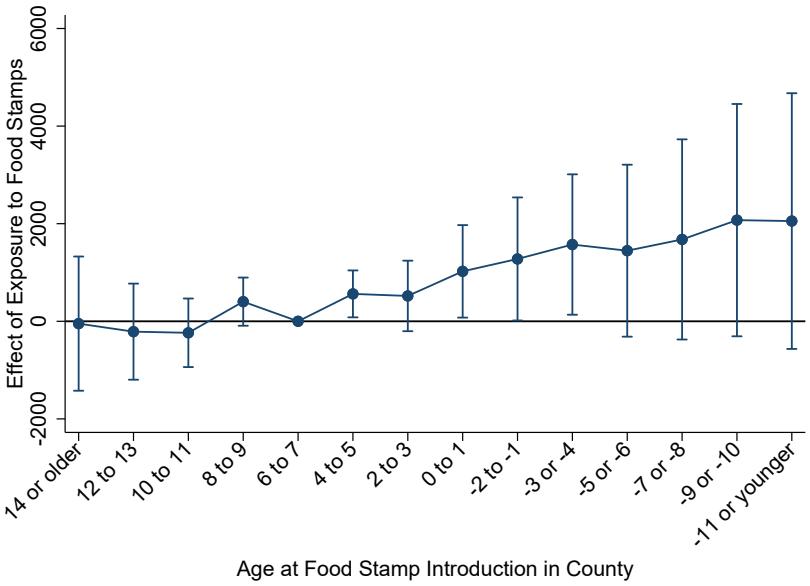
Notes: Figure shows participation per capita in each county in the Food Stamp Program (FSP) in an event study. Dummies included for county and coefficients shown for event time. Balanced panel of counties shown, additional controls for event time being -6 or earlier or 4 or later. Caseloads for 1955–1979.

Figure 4: Goodman-Bacon (2021) decomposition of two by two differences-in-differences estimates and related weights in two way fixed effects estimates



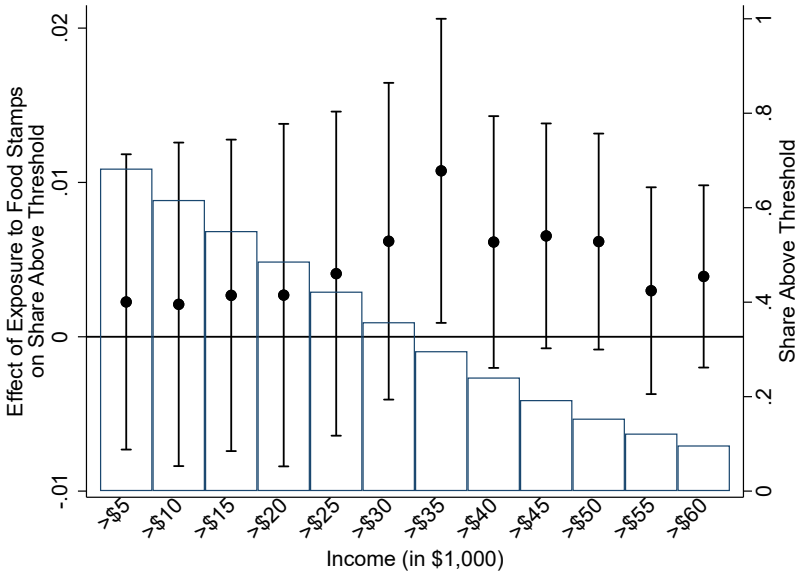
Notes: Figure shows Goodman-Bacon (2021) decomposition of two by two differences-in-differences estimates where treatments is the entry of the county into the Food Stamp Program (FSP). The x-axis reports the weight of teach timing group contribution to the overall two-way fixed effects estimate and the y-axis is the magnitdude of each timing groups' differences-in-differences comparison, using all the available controls (not yet treated, already treated). The overall two-way fixed effect estimate is 0.057, meaning adoption of foodstamps led to 5.7 percent of the county's residents participating in the program and this is reported as a horizontal line. Caseloads per capita for each county included for the period 1955–1979.

Figure 5: Effects of exposure to the Food Stamp Program on women's total earnings at age 32, cohorts born 1955–1980, with controls, event study, unbalanced panel



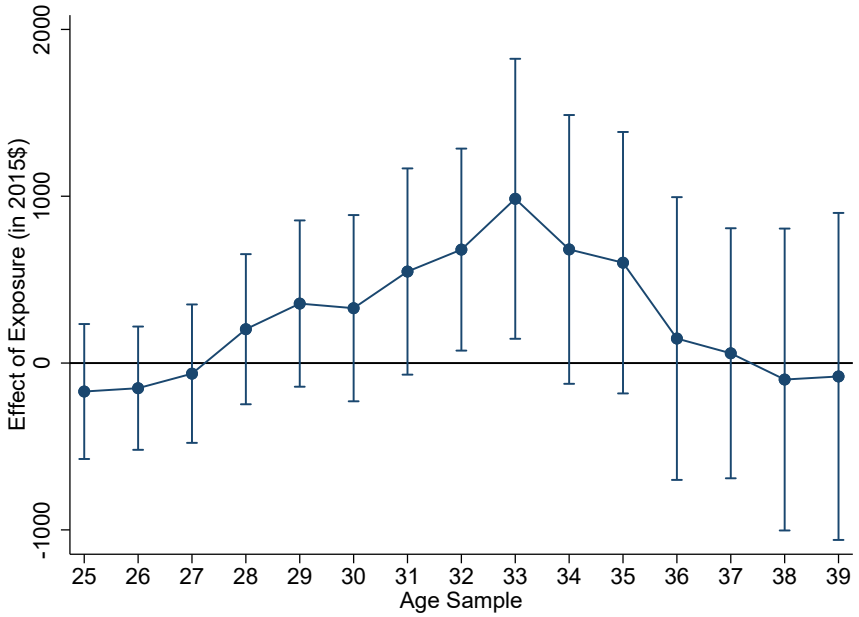
Notes: Figure shows income from the Continuous Work History Sample for women at age 32 by cohort as a function of the age at which the Food Stamp Program (FSP) was introduced in the county of birth. Dummies included for county and coefficients shown for age at introduction in two-year bins. Unbalanced panel of counties and years shown. Controls also included for being white. Omitted category first exposure is at ages six and seven.

Figure 6: Effects of exposure to the Food Stamp Program for women at age 32 on the probability of earning more than various threshold amounts, cohorts born 1955–1980



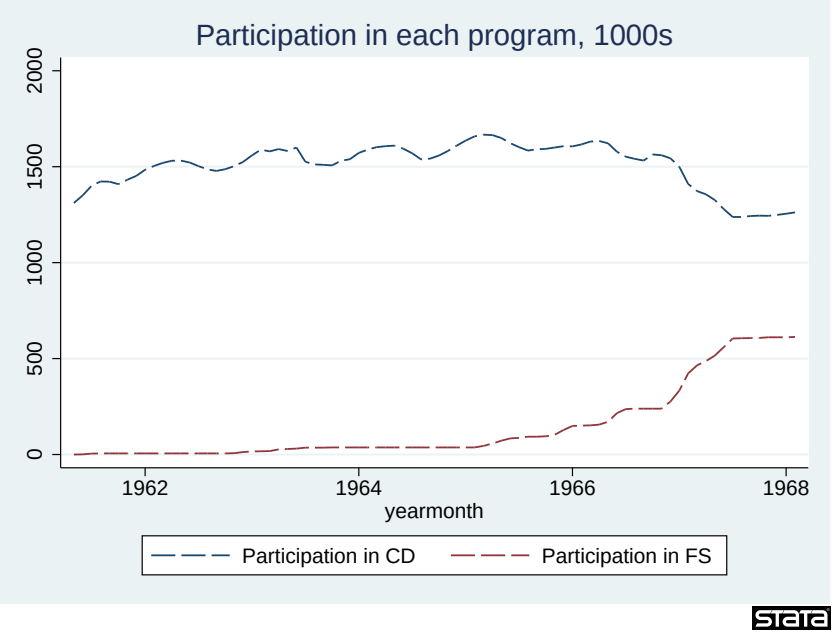
Notes: Each dot indicates the effect of exposure to the Food Stamp Program (FSP) from conception through age five on the probability of real total earnings at age 32 being above various thresholds (left axis). The capped bars indicate the 95% confidence interval for each estimate. The hollow histogram 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 422,000 (rounded to the nearest 1,000). Mean exposure to the Food Stamp Program (FSP) is 0.5102 of the time from conception to age five. Controls include fixed effects for being white, birth year, and county of birth.

Figure 7: Effects of exposure to the Food Stamp Program on women's real total earnings at various ages, cohorts born 1955–1980



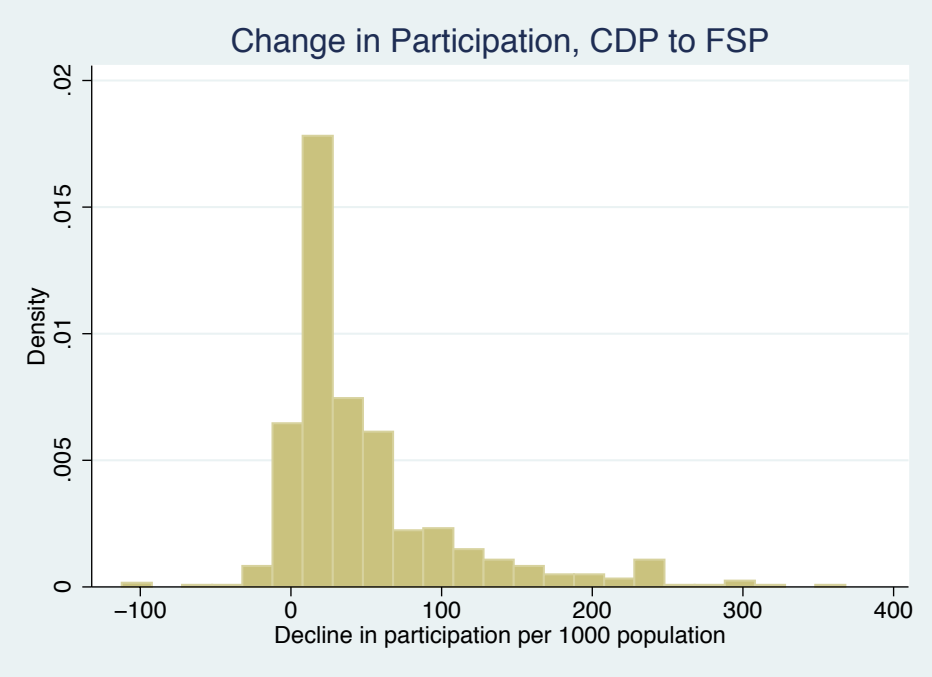
Notes: Figure shows the effect of exposure to the Food Stamp Program (FSP) for women at different ages. Balanced panel for those who are alive at all ages. Controls included for being white, birth year, and county of birth. Each point shows results from a separate regression.

Figure 8: Participation in the Food Stamp Program and Commodity Distribution Program through 1968



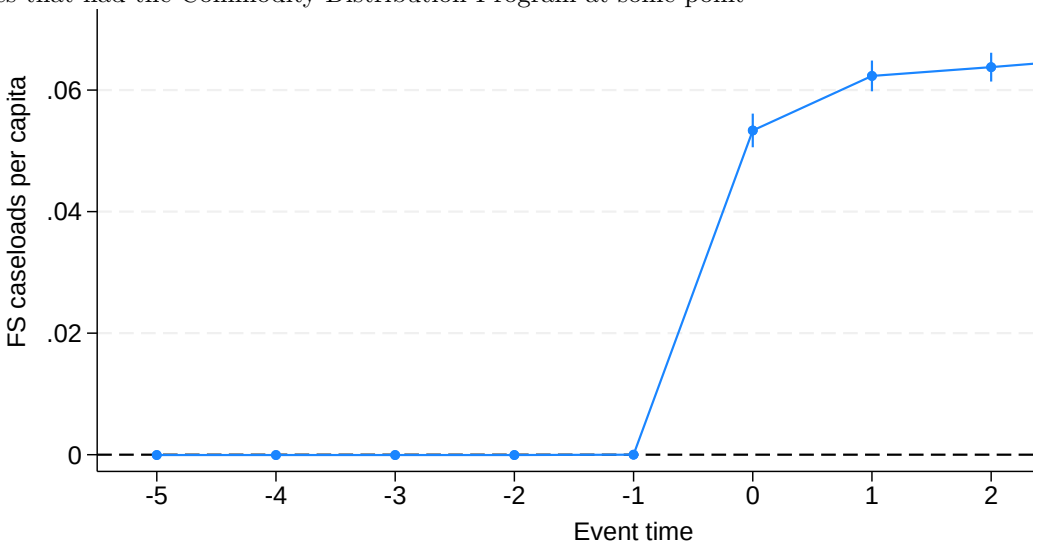
Notes: Figure shows participation in the Food Stamp Program (FSP) and Commodity Distribution Program (CDP) nationally by year through 1968. Based on counts from National Archives documents.

Figure 9: Decline in caseloads when counties switched from the Commodity Distribution Program to the Food Stamp Program, switches through 1968



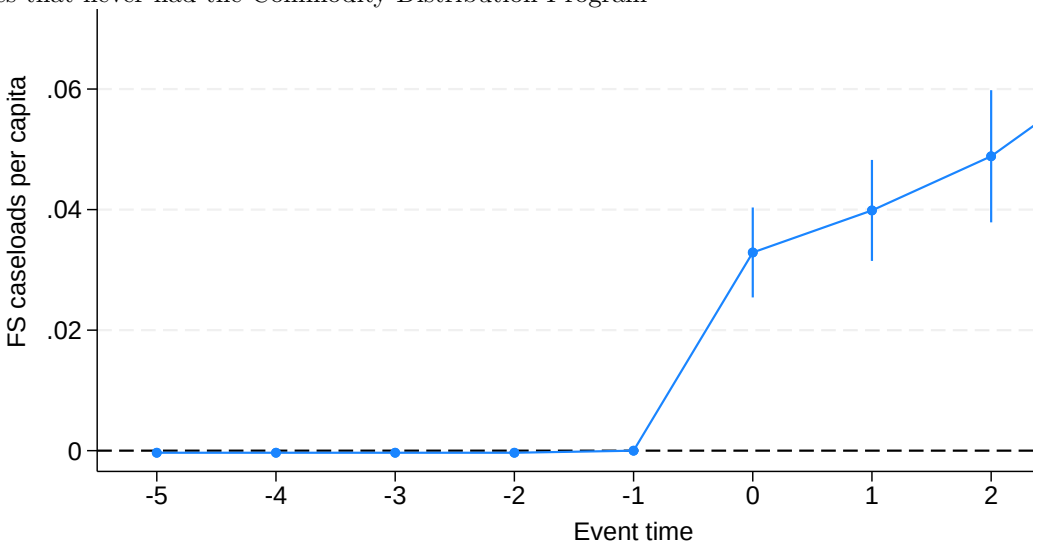
Notes: Figure shows the decline in caseloads when counties made the transition from the Commodity Distribution Program (CDP) to the Food Stamp Program (FSP) through 1968. Based on counts from National Archives documents.

Figure 10: Food Stamp Program caseloads per capita event study among counties that had the Commodity Distribution Program at some point



Notes: Figure shows participation per capita in each county in the Food Stamp Program (FSP) 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 -6 or earlier or 4 or later. Caseloads for 1955–1979. Sample is counties that ever had the Commodity Distribution Program (CDP).

Figure 11: Food Stamp Program caseloads per capita event study among counties that never had the Commodity Distribution Program



Notes: Figure shows participation per capita in each county in the Food Stamp Program (FSP) 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 -6 or earlier or 4 or later. Caseloads for 1955–1979. Sample is counties that never had the Commodity Distribution Program (CDP).

Table 1: de Chaisemartin and D'Haultefœille estimates, using non-changers one period before adoption and also placebo for effects of exposure to the Food Stamp Program for women at age 32, cohorts born 1955–1980, outcome is county participation in the Food Stamp Program per capita

	County FSP participation per capita
FSP adoption	0.053
Placebo for "effect" of adoption 1 year prior	0.000

Notes: The table shows de Chaisemartin and D'Haultefœille (2020) measures of the effects of food stamp adoption on the Food Stamp Program (FSP) caseload per capita. Each row represents a different regression. The first row presents results using counties that did not change adoption one period before as the control. The second row shows results for a placebo change from two years to one year before adoption.

Table 2: Effects of exposure to the Food Stamp Program, cohorts born 1955–1980, any employment

	(1)	(2)
	Women	Men
Fraction of months exposed to FSP	0.0039	0.0039
	(0.0030)	(0.0035)
Mean probability any employment	0.7682	0.8185
Mean exposure to FSP	0.4057	0.4073
Number of person observations	423,000	436,000
Number of person-year observations	11,648,000	11,828,000

Notes: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age five on the probability the individual ever had 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 the nearest 1000. Controls include fixed effects for being white, birth year, and county of birth.

Table 3: Effects of exposure to the Food Stamp Program 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 FSP	694**	727**	477*
	(306)	(305)	(268)
Mean outcome variable (in 2015$)	25,453	24,438	23,762
Mean exposure to FSP	0.5089	0.5089	0.5089
N	421,000	421,000	421,000

Notes: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age five 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 the nearest 1,000. Controls include fixed effects for being white, birth year, and county of birth.

Table 4: Effects of exposure to the Food Stamp Program 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 FSP	152	91	577
	(1,095)	(1,060)	(495)
Mean outcome variable (in 2015$)	41,304	38,959	36,868
Mean exposure to FSP	0.5083	0.5083	0.5083
Number of individuals	432,000	432,000	432,000

Notes: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age five 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 the nearest 1,000. Controls include fixed effects for being white, birth year, and county of birth.

Table 5: 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 FSP	694**	624**	623**	617**	769**	535*
	(306)	(298)	(298)	(309)	(341)	(307)
Mean real earnings	$25,453	$25,453	$25,453	$26,078	$26,118	$25,455
Mean exposure	0.5089	0.5089	0.5089	0.6153	0.6162	0.5090
Number of individuals	421,000	421,000	421,000	348,000	346,000	421,000
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: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age five 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 the nearest 1,000.

Table 6: Average household participation in the Food Stamp Program 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 FSP, 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 FSP benefits, all children						
Female child	171	208	213	217	215	214
Male child	189	209	180	203	197	212
Household got FSP, 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 FSP 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 (FSP) and average household benefits in nominal terms for children 5 and under by gender. Data are from the 1980 March Current Population Survey, weighted to be population representative. Reference period is 1979 calendar year. The top two panels are for all children, the bottom two panels are 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 7: Effects of exposure to Food Stamp Program, cohorts born 1955–1980, ever on SSDI, sample ever in panel

	(1)	(2)
	Women	Men
	Ever on SSDI	Ever on SSDI
Share months exposed to FSP	-0.0017	-0.0017
	(0.0027)	(0.0030)
Mean outcome variable	0.0949	0.1029
Mean exposure to FSP	0.5089	0.5079
Number of individuals	423,000	436,000

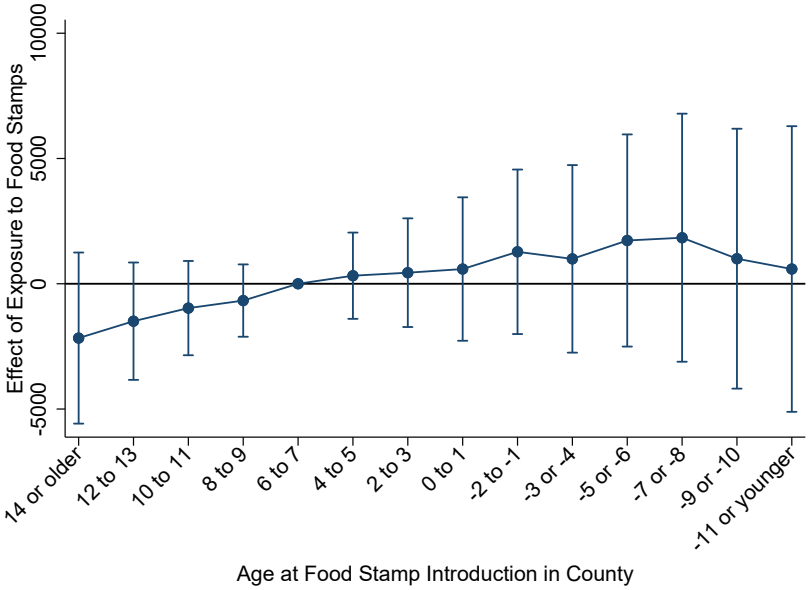
Notes: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age 5 on participation in Social Security Disability Insurance (SSDI) at any age for women and men born between 1955 and 1980. ***, **, and * denote significance at the 1, 5 and 10 percent level. SEs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

Table 8: Effects of exposure to Food Stamp Program at age 32 in counties that ever had the Commodity Distribution Program, cohorts born 1955–1980, total real earnings

	(1)	(2)
	Women	Men
Fraction of months exposed to FSP	735**	182
	(308)	(1,104)
Mean earnings	25,465	41,340
Mean exposure to FSP	0.5077	0.5072
Number of individuals	414,000	424,000

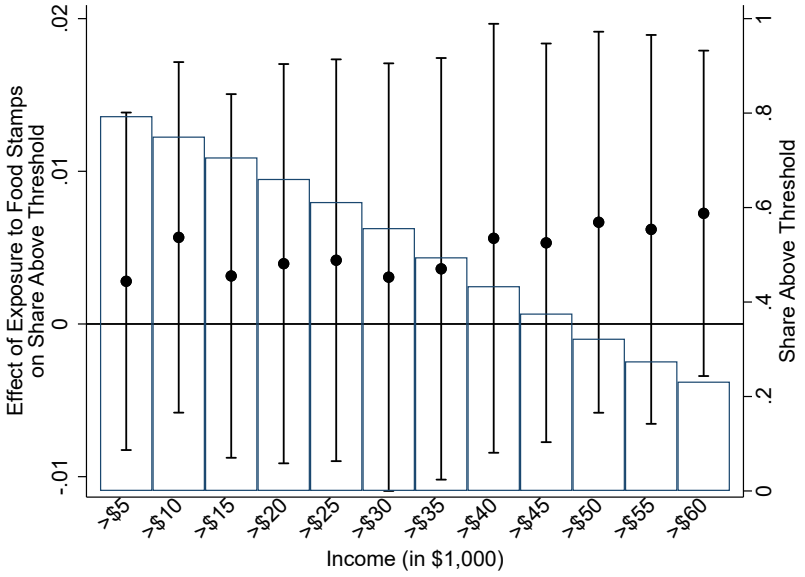
Notes: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age 5 in counties that ever had the Commodity Distribution Program (CDP) on real earnings at age 32 for women and men born between 1955 and 1980. ***, **, and * denote significance at the 1, 5 and 10 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 the nearest 1,000. Controls include fixed effects for being white, birth year, and county of birth.

Appendix Figure 1: Effects of exposure to the Food Stamp Program on men's total earnings at age 32, cohorts born 1955–1980, with controls, event study, unbalanced panel



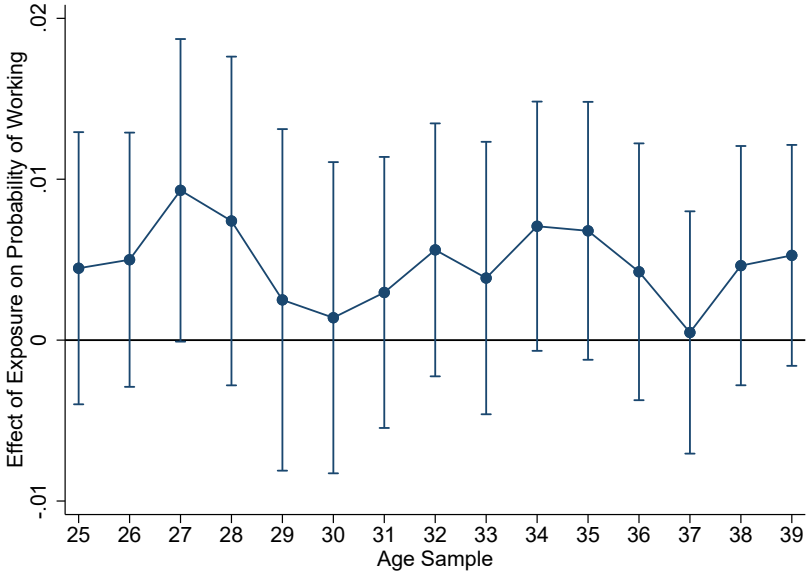
Notes: Figure shows income from the Continuous Work History Sample for men at age 32 by cohort as a function of the age at which the Food Stamp Program (FSP) was introduced in the county of birth. Dummies included for county and coefficients shown for age at introduction in 2 year bins. Unbalanced panel of counties and years shown. Controls included for being white. Omitted category first exposure is at ages 6 and 7.

Appendix Figure 2: 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



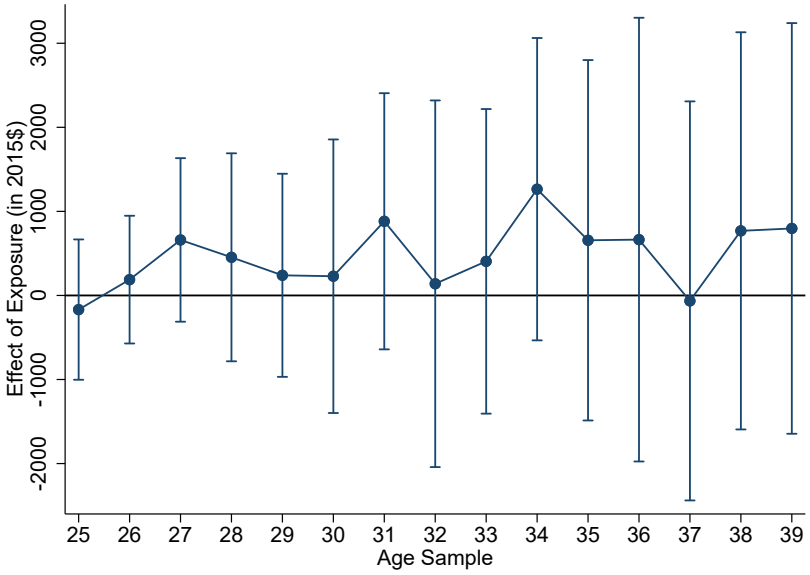
Notes: Each dot indicates the effect of exposure to Food Stamps from conception through age 5 on the probability of real total earnings at age 32 being above various thresholds (left axis). The capped bars indicate the 95% confidence interval for each estimate. The hollow histogram bars indicate the 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 433,000 (rounded to the nearest 1,000). Mean exposure to the Food Stamp Program is 0.5098. Controls include fixed effects for being white, birth year, and county of birth.

Appendix Figure 3: Effects of exposure to the Food Stamp Program on women's effect on probability of working at various ages, cohorts born 1955–1980



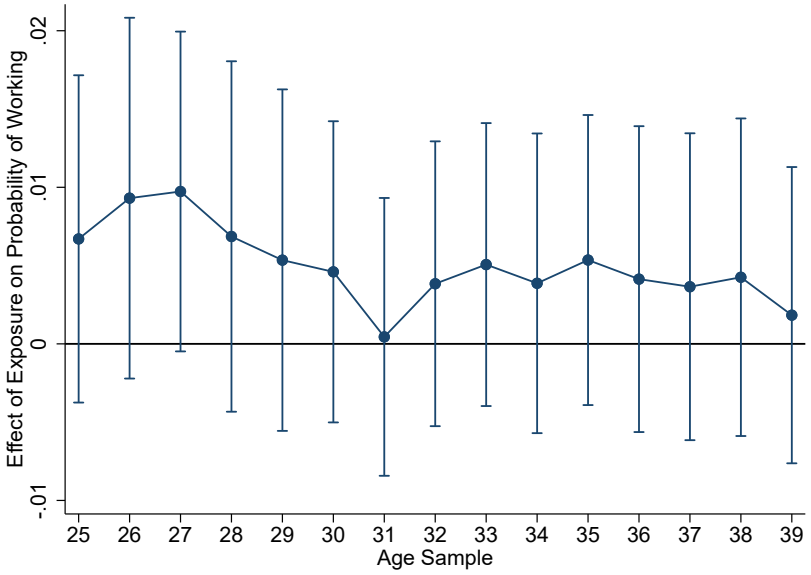
Notes: Figure shows the effect of exposure to the Food Stamp Program for women at different ages. Balanced panel for those who are alive at all ages. Controls included for being white, birth year, and county of birth.

Appendix Figure 4: Effects of exposure to the Food Stamp Program for men's real total earnings at various ages, cohorts born 1955–1980



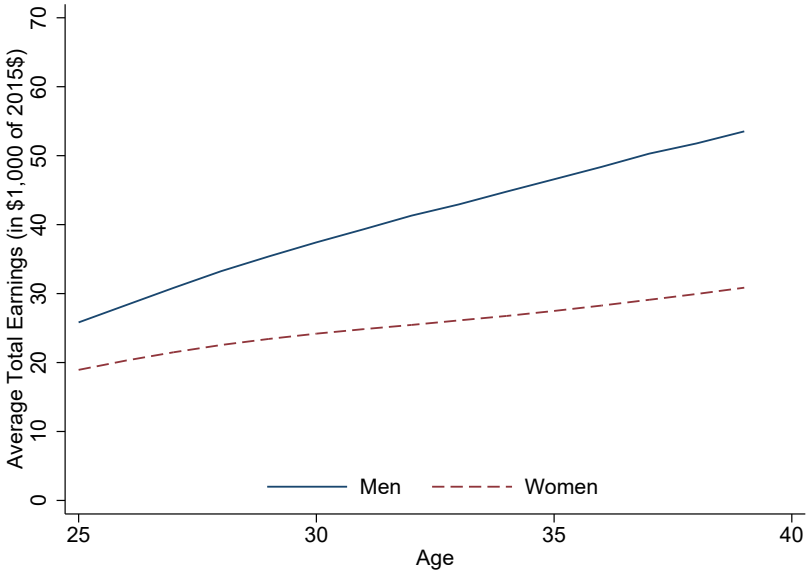
Notes: Figure shows the effect of exposure to the Food Stamp Program (FSP) for women at different ages. Balanced panel for those who are alive at all ages. Controls included for being white, birth year, and county of birth.

Appendix Figure 5: Effects of exposure to the Food Stamp Program for men's effect on probability of working at various ages, cohorts born 1955–1980



Notes: Figure shows the effect of exposure to the Food Stamp Program (FSP) for women at different ages. Balanced panel for those who are alive at all ages. Controls included for being white, birth year, and county of birth.

Appendix Figure 6: Real average earnings across the life cycle for men and women



Notes: All dollar amounts are measured in 2015 dollars and are adjusted using CPI-U. Individuals with a missing month of birth or born in counties with missing Food Stamp Program rollout information are excluded from the sample. Some of the youngest cohorts are missing values starting at ages 38.

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

	(1)	(2)
	Women	Men
Fraction of months exposed to FSP	783***	611
	(343)	(864)
Mean earnings	26,120	43,051
Mean exposure to FS	0.5090	0.5084
Number of individuals	420,000	430,000

Notes: The table shows effects of exposure to the Food Stamp Program (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 the nearest 1000. Controls include fixed effects for being 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 the Food Stamp Program for adults aged 32–34, cohorts born 1955–1974, average real total earnings

	(1)	(2)
	Women	Men
Fraction of months exposed to FSP	829**	-93
	(393)	(825)
Mean earnings	25,571	43,960
Mean exposure to FS	0.3767	0.3769
Number of individuals	329,000	335,000

Notes: The table shows effects of exposure to the Food Stamp Program (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 the nearest 1000. Controls include fixed effects for being 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 the Food Stamp Program for women aged 36–38, cohorts born 1955–1974, average real total earnings

	(1)	(2)
	Women	Men
Fraction of months exposed to FSP	240	-231
	(447)	(1,145)
Mean earnings	28,261	50,667
Mean exposure to FS	0.3767	0.3769
Number of individuals	329,000	335,000

Notes: The table shows effects of exposure to the Food Stamp Program (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 the nearest 1000. Controls include fixed effects for being 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 the Food Stamp Program at age 32, cohorts born 1955–1980, percentile rank of earnings by birth cohort

	(1)	(2)
	Women	Men
Fraction of months exposed to FSP	0.3404	0.3462
	(0.3091)	(0.4752)
Number of individuals	421,000	432,000

Notes: The table shows effects of exposure to the Food Stamp Program (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 the nearest 1000. Controls include fixed effects for being white, birth year, and county of birth.

Appendix Table 5: Effects of exposure to the Food Stamp Program on real total earnings for women at age 32, cohorts born 1955–1980, adding various controls

	(1) No control for being white	(2) Baseline/controls for being white	(3) Controls for being white & state-by-year FE
Fraction of months exposed to FSP	743**	694**	103
	(309)	(306)	(368)
Mean outcome variable (in 2015$)	25,453	25,453	25,453
Mean exposure to FSP	0.5089	0.5089	0.5089
Number of individuals	421,000	421,000	421,000
Controls for:			
COB FE	Y	Y	Y
White	N	Y	Y
YOB FE	Y	Y	Y
SOB-YOB FE	N	N	Y

Notes: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age five on real total earnings at age 32 for women born between 1955 and 1980, without the control for being white (column 1), with the control for being white (our baseline, column 2), and with this control adding state by year fixed effects (column 3). ***, **, 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 the nearest 1,000.

Appendix Table 6: Effects of exposure to the Food Stamp Program at age 32, cohorts born 1955–1980, effects varying by whether 1959 county median income is below or at or above median for all counties

	(1) Women	(2) Women	(3) Man	(4) Men
Fraction of months exposed to FSP	694**	961***	151	263
	(306)	(316)	(1,095)	(1,125)
County income below median		2,248*		-905
		(1,268)		(2,075)
Fraction of months exposed to		-1,816***		-599
FSP · county income below median		(293)		(567)
Mean outcome variable (in 2015$)	25,453	25,379	42,304	41,249
Mean exposure to FSP	0.5089	0.5082	0.5083	0.5072
Number of individuals	421,000	415,000	432,000	426,000

Notes: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age five 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 the nearest 1,000. "County income below 50%" denotes whether the individual's birth county had a median county income in 1959 that was below the median of all counties' median incomes in 1959.

Appendix Table 7: Effects of exposure to the Food Stamp Program on the number of name changes individuals filed for at the Social Security Administration, cohorts born 1955–1980, persons ever in panel

	(1)	(2)
	Women	Men
Fraction of months exposed to FSP	-0.0287	0.0125
	(0.0273)	(0.0350)
Mean name change	2.312	1.446
Mean exposure to FS	0.5089	0.5079
Number of individuals	423,000	436,000

Notes: The table shows the effect of exposure to the Food Stamp Program (FSP) from conception through age five on the number of SS-5 entries that could be associated with a name 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 the nearest 1000. Controls include fixed effects for being white, birth year, and county of birth.