Is the Social Safety Net a Long-Term Investment? Large-Scale Evidence from the Food Stamps Program

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May 2022

Abstract:
We use novel, large-scale data on 17.5 million Americans to study how a policy-driven increase in economic resources affects children's long-term outcomes. Using the 2000 Census and 2001-2013 American Community Survey linked to the Social Security Administration's NUMIDENT, we leverage the county-level roll-out of the Food Stamps program between 1961 and 1975. We find that children with access to greater economic resources before age five have better outcomes as adults. The treatment-on-the-treated effects show a 6 percent of a standard deviation improvement in human capital, 3 percent of a standard deviation increase in economic self-sufficiency, 8 percent of a standard deviation increase in the quality of neighborhood of residence, a 1.1-year increase in life expectancy, and a 0.5 percentage-point decrease in likelihood of being incarcerated. These estimates suggest that Food Stamps' transfer of resources to families is a highly cost-effective investment in young children, yielding a marginal value of public funds of approximately 62.

Acknowledgments:
The opinions and conclusions expressed herein are solely those of the authors and should not be construed as representing the opinions or policy of any agency of the U.S. Census Bureau, the National Institutes of Health (NIH), the National Science Foundation (NSF), or the Laura and John Arnold Foundation (LJAF). All results have been reviewed to ensure that no confidential information is disclosed. Data collection for the War on Poverty project was generously supported by the NIH (R03-HD066145). Data linkage and analyses for this project was supported by the LJAF (PI Bailey), the NSF (PIs Rossin-Slater and Walker, Award No. 1459940 and PI Rossin-Slater CAREER Award No. 1752203), and the Institute for Industrial Relations (PI Hoynes). We gratefully acknowledge the use of the services and facilities of the Population Studies Center at the University of Michigan (P2CHD041028) and the California Center for Population Research at UCLA (P2CHD041022)., the Michigan Research Data Center (NSF ITR-0427889), and its training program (Binder and Timpe were supported by the NIA T32AG000221 as University of Michigan Population Studies Center Trainees). Evan Taylor and Bryan Stuart provided exceptional assistance in translating string names in the SSA's NUMIDENT file into GNIS codes. We also thank Ariel Binder, Chris Campos, Dorian Carloni, Raheem Chaudhry, Grant Graziani, John Iselin, Kate Moulton, Krista Ruffini, Bryan Stuart, Matt Tarduno, and Brenden Timpe for excellent research assistance and Clint Carter for the many hours spent helping us disclose these results. A pre-analysis plan for this project can be found at https://osf.io/t6vsz.

I. Introduction

Social safety net programs are designed to help low-income individuals meet their food, housing, and healthcare needs. Dating to President Lyndon B. Johnson's War on Poverty, the objective of these programs is "not only to relieve the symptom of poverty, but to cure it and, above all, to prevent it" (Johnson 1965).

In 1964, the War on Poverty substantially expanded the Food Stamps program into a core component of the U.S. safety net. Now called the Supplemental Nutrition Assistance Program (SNAP), this program provides poor individuals with vouchers to purchase food at grocery stores. As it expanded to all areas of the U.S. under the War on Poverty, the program achieved its first objective of relieving the *symptoms* of poverty. It raised food spending among participating families by 21 percent (Hoynes and Schanzenbach 2009) and improved infant health (Almond et al. 2011).

Today, SNAP remains the second largest anti-poverty program for children and the most important program for reducing deep child poverty (National Academy of Sciences 2019). In 2018, SNAP raised 3.1 million people out of poverty at a cost of $65 billion dollars.[1] Food Stamps has also supported millions of families affected by the COVID-19 pandemic, in which adults have lost jobs and children have lost access to food delivered through school breakfast and lunch programs. However, the extent to which SNAP can achieve its second objective of *preventing* poverty in the future has been difficult to study, largely due to data limitations.

This paper provides novel evidence on this question by quantifying the lasting effects of

[1]By comparison, spending on the Earned Income Tax Credit was $65 billion (https://www.irs.gov/pub/irs-soi/18in25ic.xls) and spending on Temporary Assistance for Needy Families is much lower at $29 billion (https://www.acf.hhs.gov/ofa/resource/tanf-financial-data-fy-2018), both for 2018. The poverty reduction figure comes from Fox (2019) and is measured for 2018.

childhood access to Food Stamps in the 1960s and 1970s on multiple measures of adult economic productivity and well-being today. Linking the 2000 Census long form (a 1-in-6 sample of all U.S. households), the 2001 to 2013 American Community Surveys (ACS), and the Social Security Administration's (SSA) NUMIDENT, which contains information on county and date of birth, allows us to calculate the likely access of more than 17 million American adults today to the Food Stamps program *in childhood* decades earlier. These data also allow us to observe a wider range of outcomes than the previous literature, including educational attainment, labor market productivity, poverty status, participation in public programs, incarceration, physical and cognitive disabilities, mobility from one's county of birth, the quality of one's adult neighborhood of residence, and mortality.

Our empirical strategy, described in a pre-analysis plan to minimize concerns about multiple hypothesis testing and specification search, builds upon the validated approach of Hoynes and Schanzenbach (2009), Almond et al. (2011), and Hoynes et al. (2016), who exploit the county-by-county rollout of Food Stamps in the 1960s and 1970s.[2] We estimate event-study, linear-spline, and difference-in-difference models that rely on variation in the availability of the Food Stamps program across birth counties and birth cohorts. To limit concerns about the endogeneity of the program's implementation, all specifications follow prior studies and control for birth-county fixed effects and 1960 county characteristics interacted with linear trends. Moreover, our larger samples allow us to include individual birth-state by birth-year fixed effects, which account for the rich set of policy changes at the state level during the 1960s, as well as survey year fixed effects, which control for dramatic changes in the U.S. economy from 2000 to 2014.

[2] Prior studies have documented that the initial Food Stamps rollout is largely uncorrelated with other observable county economic and demographic characteristics (Hoynes and Schanzenbach 2009, Almond et al. 2011, Hoynes et al. 2016), and we confirm this finding for the counties and years in our analysis sample.

Our results show that more exposure to Food Stamps *in utero* and in early childhood leads to better adult outcomes. Using pre-specified indices that combine our many outcomes into four domains (Kling et al. 2007), we document that access to Food Stamps during the entirety of time between one's estimated month of conception and age five leads to a 0.009 standard-deviation increase in a composite index of adult well-being. This aggregate improvement is driven by increases in human capital (0.010 of a standard deviation), economic self-sufficiency (0.004 of a standard deviation), and neighborhood quality (0.012 of a standard deviation). Examination of the individual components of our indices demonstrates improvements along many margins, including an increased probability of attending college, higher adult labor income, a lower likelihood of living in poverty, a lower likelihood of receiving public benefits, a higher likelihood of home ownership, and improvements in multiple socio-economic characteristics of one's neighborhood of residence in adulthood. Finally, we find that full exposure to the Food Stamps program before age five is associated with a 0.2 year increase in life expectancy and a 0.08 percentage point reduction in the likelihood of being incarcerated.

These reported magnitudes correspond to intention-to-treat (ITT) estimates because our analysis sample includes individuals who never used the program. Scaling these ITT estimates by Food Stamp participation rates of about 16 percent among children ages five and younger at the time the program rolled out (Appendix Figure 2A) implies average treatment effects on the treated (TOT) of 6 percent of a standard deviation increase in human capital, 3 percent of a standard deviation increase in economic self-sufficiency, 8 percent of a standard deviation increase in neighborhood quality, a 1.2 year increase in life expectancy, and a 0.5 percentage point decrease in likelihood of being incarcerated.

We examine heterogeneity in our estimates by individual race and sex. Overall, the improvements in adult human capital are driven by white males and females, while the impacts on other outcomes extend to nonwhite individuals. That said, most of the sub-group estimates are not statistically significantly different from one another, perhaps reflecting the substantially smaller sample sizes of nonwhite individuals in our data.

When we examine impacts of exposure to Food Stamps at older ages beyond five, we observe no effects for most outcomes, with one important exception. Specifically, we find that, conditional on exposure at younger ages, exposure to Food Stamps at ages six to 18 leads to a 2.4 percentage point reduction in the likelihood of incarceration for non-white males. Taken as a whole, our results suggest that—for most outcomes and sub-groups—greater resources for mothers during pregnancy and in their children's first five years of life are more impactful in terms of shaping adult human capital, health, and productivity than resources provided as children get older.[3] The larger impact of exposure in early childhood relative to later childhood is consistent with evidence from other public programs, in which younger recipients tend to derive higher value relative to older beneficiaries (see Chetty et al 2016 for Moving to Opportunity, and Hendren and Sprung-Keyser 2020 for a broad set of programs). However, this pattern does not hold for all programs—for example, Bastian and Michelmore (2018) find larger effects of exposure to the Earned Income Tax Credit in later childhood in their analysis of impacts on completed education, which they interpret as reflecting an importance of cash on hand for higher education. This explanation may also be relevant for explaining the larger impact of exposure at older ages that we

[3] It is also the case that Food Stamp participation rates are somewhat lower among children ages 6-18 than children ages 5 years or younger (Appendix Figure 2A). However, scaling the insignificant (and often opposite-signed) coefficients on exposure at ages 6-18 by the relevant participation rates yields economically small effect magnitudes. Additionally, analysis of PSID data (Appendix Figure 2B) shows that there are no discontinuous changes in the length of time individuals spend on Food Stamps between those who first use the program at age five versus age six, suggesting that the difference between exposure below and above age five is not driven by a difference in the duration of benefit receipt.

find on non-incarceration among nonwhite males. The concentrated effects in Food Stamps' effects in early life for the other outcomes and sub-groups are consistent with nutrition being an important mechanism (Barker 1990, Hoynes and Schanzenbach 2009).

Our data—which have information on individuals' counties of birth and counties of residence in adulthood—further allow us to study migration. We show that Food Stamps availability in early childhood increases the likelihood that an individual moves away from their county of birth. Further, we find that individuals are more likely to move to counties with a higher number of four-year colleges, suggesting that childhood access to Food Stamps allows individuals to move to places with better opportunities. That said, although the impacts of Food Stamps on adult outcomes appear to operate in part through this geographic mobility channel, we also find long-term benefits for individuals who stay in their counties of birth until adulthood.[4] More generally, our results on mobility imply that analyses that use location in adulthood to assign childhood exposure are biased by endogenous migration choices.

Our analysis of a comprehensive set of adult outcomes has important implications for valuing Food Stamps as a long-term public investment. For instance, the fact that childhood exposure increases adult labor income and reduces adult poverty implies that the social safety net for families with young children may, in part, pay for itself by increasing taxes and, therefore, government revenue in the long term. For a more formal assessment, we follow the framework proposed by Hendren (2016) and Hendren and Sprung-Keyser (2020) to calculate the Marginal Value of Public Funds (MVPF). The MVPF is the ratio of the benefit of the policy to its recipients (i.e., childhood Food Stamps beneficiaries) to the net cost to the government. We calculate that

[4] In fact, we find that the impacts of Food Stamps are larger for individuals who are residents in their counties of birth in adulthood than for those who move away. This difference may reflect higher rates of measurement error for movers than for stayers or sub-group heterogeneity, as movers are positively selected.

the MVPF of childhood Food Stamps is approximately 62. The high value is consistent with Hendren and Sprung-Keyser (2020)'s finding that programs targeting children tend to generate larger MVPFs than programs for adults and exceeds MVPFs estimated for highly regarded early childhood education interventions, such as the Perry Preschool and the Carolina Abecedarian Program.

Our paper builds on three recent studies that have analyzed the effect of Food Stamps on long-run outcomes. Hoynes et al. (2016) broke new ground in documenting some long-term benefits of Food Stamps using the Panel Study of Income Dynamics (PSID). They find suggestive evidence that greater exposure to Food Stamps before age five leads to a reduction in adult metabolic syndrome conditions and improvements in some measures of economic self-sufficiency for women. However, the strength of these conclusions is limited by small sample sizes and high attrition rates in the PSID, and their data do not capture other dimensions of adult well-being, such as life expectancy, incarceration, and the quality of one's neighborhood of residence. More recently, Bitler and Figinski (2019) use data from the Social Security Administration's Continuous Work History Sample, which contains information on earnings for one percent of U.S.-born individuals. They find that exposure to Food Stamps before age five has no impact on social security disability receipt, increases adult earnings for women, and has insignificant effects on adult earnings for men. Lastly, Barr and Smith (2022) use data from North Carolina and find that childhood exposure to Food Stamps reduces the likelihood of a criminal conviction in young adulthood. Our large-scale linked data allow us to study an unprecedented number of adult outcomes, explore migration as a potential mechanism, and use our estimates to provide a comprehensive evaluation of the efficacy of the Food Stamps program via the MVPF framework.

We also contribute to a body of research that documents that safety net programs including near cash (Food Stamps, the Earned Income Tax Credit, Aid to Families with Dependent Children) and in-kind transfers (Special Supplemental Nutrition Program for Women, Infants, and Children, Medicaid) improve infant health (see, e.g.: Currie and Cole 1993, Currie and Gruber 1996a, Currie and Gruber 1996b, Bitler and Currie 2005, Almond et al. 2011, Hoynes et al. 2011, Rossin-Slater 2013, Hoynes et al. 2016). These findings are relevant in light of the expansive literature on the importance of the early life environment for individual well-being throughout the life cycle (see reviews by Almond and Currie 2011a, Almond and Currie 2011b, Almond, Currie and Duque 2018). While early work on this topic has tended to use variation from large adverse shocks to early childhood conditions, studies linking childhood access to safety net programs with long-term outcomes have only recently begun to emerge (see Hoynes and Schanzenbach 2018 and Page 2021 for reviews). Studies show that childhood access to cash welfare (Aizer et al. 2016), the Earned Income Tax Credit (Bastian and Michelmore 2018), cash payments for tribal citizens (Akee et al 2010), Medicaid (Brown et al. 2020, Miller and Wherry 2019, Cohodes et al. 2016, Goodman-Bacon 2021), and community health centers (Bailey and Goodman-Bacon 2014) lead to improvements in human capital and/or health in adulthood. Page (2021), in her review of these studies, shows that an increase in $1000 in childhood leads to less than 1 percent increase in earnings in adulthood and a 0.01-0.02 increase in completed years of education. Based on a comparison from Page (2021)'s review, our estimated impacts of Food Stamps are similar in magnitude to the estimates for tribal payments and the EITC.

Lastly, our work is related to the literature on the long-term effects of early childhood income (for some overviews, see, e.g. Duncan and Brooks-Gunn 1997, Solon 1999, Duncan et al. 2010, Black et al. 2011, National Academies of Sciences, 2019). This work faces similar data constraints as the literature on safety net programs, along with the substantial challenge of separating the causal effects of income from other factors associated with disadvantage. Several studies have made important strides in overcoming this challenge by exploiting variation in aggregate economic conditions, finding positive relationships between economic activity during childhood and education, income, and health in later life (Van den Berg et al. 2006, Cutler et al. 2007, Banerjee et al. 2010, Løken et al. 2012, Cutler et al. 2016, Rao 2016). A related set of studies examines the relationship between parental job loss and children's long-run outcomes (Page et al. 2007, Bratberg et al. 2008, Oreopoulos et al. 2008, Coelli 2011, Hilger 2016, Stuart 2018). Complementing studies on the long-term effects of economic conditions, our results show that increasing children's income through public policy is also strongly predictive of a broad range of measures of long-term well-being.

II. The Food Stamp Program and the Food Stamp Rollout

A. The Food Stamps Program

Food Stamps (or SNAP) is a means-tested program designed to supplement low-income families' food budgets. It is a "voucher" program in that it can be used to purchase most foods at grocery stores.[5] The benefits are structured to fill the gap between the resources a family has available to purchase food and the resources required to purchase an inexpensive food plan. Eligibility requires that families have incomes below 130 percent of the federal poverty line. The program has few other eligibility requirements and thus extends benefits to nearly all income-eligible applicants.[6] Maximum benefits vary with family size (and are adjusted for changes in food prices from year to year), and the benefit is phased out at a 30 percent rate with increases in income

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[5] Food Stamps can be used to purchase all food items available in grocery stores except hot, ready to eat foods.
[6] In addition to the income test, Food Stamps also has an asset test, currently set at $2,250 (or $3,500 for the elderly and disabled). There are also limits on access relating to immigrant status and income eligible recipients who are not aged, disabled or with children face time limits in the program.

(after deductions). This is a federal program, administered in the U.S. Department of Agriculture, and benefits are equal across different regions of the U.S. (except Alaska and Hawaii). Benefits are paid monthly; in 2019 recipients received an average of $258 per household per month or $4 per person per day. An extensive literature documents that the Food Stamps program reduces food insecurity (see reviews by Hoynes and Schanzenbach 2016 and Bitler and Siefoddini (forthcoming)).

B. The Food Stamp Rollout

The Food Stamps program began as President Kennedy's first Executive Order, issued on February 2, 1961, which led to the launch of pilot Food Stamps programs in eight counties.[7] These counties were quite poor and included counties in Appalachia, Native American reservations, and Wayne County in Michigan (containing the city of Detroit). The pilot counties expanded to a total of 43 counties through 1962 and 1963.

The pilot programs were significantly expanded under President Johnson's War on Poverty with the passage of the Food Stamp Act of 1964 (FSA), which gave local areas the authority to start up the Food Stamp Programs in their county. Local officials had to apply for the program, and Congress appropriated funding to these applications. In the first year, $75 million was appropriated; $100 million for year 2; and $200 million in year 3. Following the FSA, the rollout across counties increased (Appendix Figure 1). The 1973 Amendments to FSA, passed on August 10, 1973, required that the program be expanded to the entire U.S. by July 1, 1974. By mid-1973 almost 90 percent of the U.S. population lived in counties that had a Food Stamps program. Figure 1 displays a county map of the U.S. indicating the date of county Food Stamps initiation, with darker shaded counties representing later program introduction. The map shows substantial *within-*

[7] For a compact history of the Food Stamp program see https://www.fns.usda.gov/snap/short-history-snap.

state variation in the timing of Food Stamps' implementation, which our analysis exploits.

C. *Expected Effects of Childhood Access on Long-Run Outcomes*

How might having access to Food Stamps in early childhood lead to differences in adult outcomes? Food Stamps increases household resources by providing a voucher to purchase food if the family is income eligible.[8] Standard consumer theory predicts that inframarginal participants (those who receive benefits in an amount less than they would otherwise spend on food) respond to Food Stamps benefits like ordinary income (Hoynes and Schanzenbach 2009). This suggests that the launch of Food Stamps would lead to increases in spending on food and other goods. The available evidence, from the contemporary Food Stamps program, shows that the vast majority of Food Stamps recipients spend more on food than their Food Stamps benefit amount, implying most would be inframarginal (Hoynes, McGranahan and Schanzenbach 2015). Some studies find that households respond to Food Stamps like ordinary cash income (Schanzenbach 2007, Hoynes and Schanzenbach 2009, Beatty and Tuttle 2020, Bruich 2014), while other studies find that Food Stamps yields more spending on food than ordinary income (Hastings and Shapiro 2018). Either way, one potential channel for long run impacts is an increase in the quantity or quality of food available in the household during early childhood.

An extensive body of evidence, beginning with Barker (1990), establishes that better early life nutrition leads to improvements in adult health. This implies that the availability of Food Stamps, *in utero* and in early childhood in particular, could lead to increases in adult health. Moreover, greater health and nutrition in early life may make subsequent investments in child development more productive (Cunha and Heckman, 2007; Heckman and Masterov, 2007;

[8] This is net of any efficiency loss due to any induced reduction in labor supply due to the benefit and phase-out rate (Hoynes and Schanzenbach 2012, East 2018).

Heckman and Mosso, 2014), compounding more for children who are younger when they are first exposed. More generally, many aspects of the early life environment have been found to be important for individual well-being throughout the life cycle (Almond and Currie 2011a, Almond and Currie 2011b, Almond, Currie and Duque 2018).

To what extent does the research on the long run effects of the social safety net line up with these predictions? First, there is consistent evidence that social safety net investments during childhood lead to improved adult human capital and economic outcomes as well as health. Aizer et al. (2016) examine an early 20th century cash welfare program and find that additional income in childhood leads to greater educational attainment, income, body weight, and life expectancy. Increased family resources during childhood through the Earned Income Tax Credit have been shown to increase children's cognitive outcomes (Dahl and Lochner 2012, 2017, Chetty et al. 2011) as well as educational attainment and employment in young adulthood (Bastian and Michelmore 2018). While perhaps less mechanistically connected to the increase in resources from these near cash programs, related work shows that public investments through Head Start preschools[9] and Medicaid[10] also lead to improvements in adult human capital and health. The evidence on the relative importance of *early childhood* exposure is more mixed. Hoynes et al. (2016) show that the beneficial effects of Food Stamps on adult metabolic health derive from exposure prior to age five. Aizer et al. (2016) provide suggestive evidence that the positive effects

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[9] Using a county-birth-cohort research design and the same restricted dataset as this paper, Bailey et al. (2019) show that Head Start programs that began in the 1960s had long-term effects on children's educational attainment as well as economic self-sufficiency, poverty status, and public assistance receipt as adults. Barr and Gibbs (2018) show that these effects persisted across generations. Work using the PSID and NLSY based on sibling comparisons also shows that test-scores and outcomes in early adulthood appear to have improved (Garces et al. 2002, Deming 2009).

[10] Access to Medicaid *in utero* and in childhood leads to improvements in educational attainment (Brown et al. 2020, Miller and Wherry 2019, Cohodes et al. 2016), earnings (Brown et al. 2020), mortality (Goodman-Bacon 2021, Wherry and Meyer 2015, Brown et al. 2020), and the health of the next generation (East et al. 2017). While the mechanisms for the long run effects of health insurance may be different from Food Stamps (or other cash and near cash assistance), the research consistently points to positive impacts of these investments in early childhood.

of cash welfare may be larger for children exposed at younger ages. Bastian and Michelmore (2018), however, find that larger EITC payments during the teen years, rather than early childhood, drive the increases in educational attainment and earnings in young adulthood.

Another mechanism for long-run effects of Food Stamps is a reduction in stress. Recent work shows that lower socioeconomic status may be causally related to stress hormones (e.g. cortisol) and that additional resources may attenuate this relationship (Aizer et al. 2016b, Evans and Garthwaite 2014, Fernald and Gunnar 2009). In turn, Black et al. (2016) and Persson and Rossin-Slater (2018) document that in utero exposure to maternal stress has adverse impacts on children's short- and long-term outcomes.

In light of this evidence, we expect childhood exposure to Food Stamps to improve adult outcomes with possibly larger impacts for exposure in early childhood. To illustrate these effects, Figure 2 plots a hypothetical relationship between adult well-being and the age an individual was when Food Stamps was introduced in their county of birth. Movement along the x-axis from right to left represents earlier (and longer) exposure to Food Stamps. We present three hypothetical cases. The red dashed line illustrates the case of a "dose-response" relationship between Food Stamps and adult well-being, whereby each year of exposure (moving left on the x-axis) leads to a fixed increase in the adult outcome. The line is downward sloping representing improved outcomes with an additional year of exposure. The blue solid line in Figure 2 illustrates the case of a non-linear relationship, whereby an additional year of exposure in early childhood (before age five) leads to larger improvements in adult well-being than an additional year of exposure in later childhood. The blue dotted line illustrates the case where additional years of exposure in later childhood—beyond age five—lead to no improvement in adult outcomes.

Note that all three cases in Figure 2 show a trend break between cohorts aged -1 to -5 when the Food Stamps program began and cohorts who were aged 0 to 5. In fact, the line segment from -1 to -5 is flat in all three cases—a pattern we refer to as the "pre-trend" due to its location on the left side of our graphs. These hypothesized cases thus predict that the effects for children who were conceived in years *after* the Food Stamps program was implemented are the same, regardless of if they were conceived 1, 2, 3, 4, or 5 years after the program started.

It is possible, however, that the "pre-trend" is not completely flat for at least two reasons. First, a woman's health reflects her cumulative pre-conception nutrition and could, therefore, also reflect her pre-conception years of exposure to Food Stamps. If maternal nutrition pre-pregnancy affects her child's later-life outcomes, we anticipate a smaller downward trend for cohorts ages -1 to -5 at the time of implementation relative to cohorts ages 0 or older. Second, the Food Stamps program was implemented rapidly (Almond et al. 2011) but not overnight.[11] To the extent that awareness and take-up increased as the program matured, we could see continued growth in the treatment effects of the program the longer the program was in a given county (Bailey 2012, Bailey and Goodman-Bacon 2014, Bailey et al. 2021). This could also induce a slight pre-trend among cohorts aged -1 to -5. Ultimately, we test empirically for the "flatness" of the pre-trend. We view the fact that we find no statistically significant pre-trends for any of our main outcomes to be consistent with the validity of our empirical strategy.

III. Data and Primary Outcomes

Our primary source of data combines information on individual outcomes in adulthood

[11] Bitler and Figinski (2019) show that in the 10 percent of counties that did not have a Commodity Distribution Program (CDP) at some point prior to implementation of the Food Stamp Program, ramp up was slower, taking perhaps five years to reach the eligible population. The 90 percent with a CDP experienced quick ramp up which Bitler and Figinski attribute to a mature administrative process for eligibility determination and program implementation. We discuss the role of the Commodity Distribution Program in the history of FS in Section IV.

with information on their exact counties and dates of birth. We also use several sources of data on county-level economic conditions, social safety net programs, and other controls.

Individual-level outcome data: Our primary data sources are the 2000 Census Long Form (1-in-6 sample) and 2001-2013 ACS files, each linked to the SSA NUMIDENT file. In addition to the large number of individual outcomes, which we describe below, the NUMIDENT contains information on individuals' dates and places of birth, as well as the date of death for those who are deceased. The data sets are linked using a unique internal individual identifier at the Census Bureau called the Personal Identification Key (PIK). These data cover a large share of the U.S. population. In particular, the Census covers 16.7 percent of the U.S. population. After accounting for overlap in the samples, the ACS brings the total coverage to roughly 25 percent of the U.S. population; and the NUMIDENT file represents the full set of U.S. individuals applying for a Social Security card.

The NUMIDENT place-of-birth variable is a string variable detailing in most cases the city and state of birth. We have developed a matching algorithm to translate this string variable to the Census Bureau's database of places, counties, and minor civil divisions as well as the United States Geological Survey's Geographic Names Information System (GNIS) file, building on prior work by Isen et al. (2017) and Black et al. (2015). Summarized in Taylor et al. (2016), this algorithm delivers a crosswalk between the NUMIDENT place-of-birth string variable and county Federal Information Processing Standards (FIPS) codes, with over 90 percent of individuals matched to their counties of birth.[12]

Our primary sample includes individuals who were born in the U.S. between 1950 and

[12] Details on the matching algorithm are stored with Research Data Center files for the 1284 project and can be accessed by individuals who obtain access from the Census. Additionally, see the Online Appendix to Black et al. (2015) and Isen et al. (2017).

1980 in order to span cohorts born before, during, and after the Food Stamps program rolled out. We limit the sample to individuals ages 25 to 54 to capture completed education and labor-market outcomes in prime-age working years.[13] To minimize disclosure risk, we limit our sample to observations with non-allocated, non-missing values for all outcomes in our analysis.[14] We also limit the sample to individuals with valid PIKs (to enable linkage to the NUMIDENT file) and with a place of birth that can be matched to a county FIPS code (see the Online Appendix for more details).

Our resulting sample size consists of 17.5 million individuals. In some specifications, we test the robustness of our results to the inclusion of various county-level controls described below, and therefore limit our baseline sample to cohorts for which these control variables are available.

To mitigate concerns about multiple hypothesis testing, we follow our pre-analysis plan in analyzing four standardized outcome indices (Kling et al. 2007). We reverse-code each outcome in the Census/ACS data as needed such that a higher value represents a "better" outcome. We then calculate z-scores by subtracting the control group mean and dividing by the control group standard deviation, where we use the 1950-54 cohorts as the control group. In addition, when studying individual components of each index domain, we account for multiple hypothesis testing using the Romano-Wolf procedure to calculate p-values.[15]

We create a composite index of well-being by taking an unweighted average of the four indices and also analyze each of the following sub-indices individually:

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[13] For two outcome variables – physical disability and survival to 2012 – we widen the age range to 25-64.

[14] We allow for missing information on physical disability and incarceration for the survey years when these variables are not available.

[15] The Romano-Wolf correction controls for the domain error rate, which is the probability of rejecting at least one true null hypothesis among a domain of hypotheses under a test. We treat each set of outcomes in each sub-index (e.g., human capital) as a domain. See Romano and Wolf (2005a,b), Romano et al. (2010), Romano and Wolf (2016), and Clarke et al. (2020).

1. Productivity and Human Capital Index: years of schooling; high school or GED completed; any college received; college or more completed; professional degree obtained; professional occupation.
2. Economic Self-Sufficiency Index: in labor force; worked last year; weeks worked last year; usual hours worked per week; log labor income; indicator for positive labor income; log non-labor income (excluding public sources); indicator for positive non-labor income; log income-to-poverty ratio; not in poverty; reverse coded log of public source income (social security, supplemental security income (SSI), and other public assistance income); reverse coded indicator for positive public source income.
3. Neighborhood Quality Index: log of house value; log of gross rent; home ownership; residence with single and not multiple families; log of mean income-to-poverty ratio in census tract of residence; reverse-coded teen pregnancy rate in tract; reverse-coded share of single-headship in tract, reverse-coded child poverty rate in tract; mean home ownership in tract; log of median house value in tract; log of median gross rent in tract; county absolute mobility score using estimates from Chetty et al. (2014).
4. Physical Ability and Health Index: no work disability; no ambulatory difficulty; no cognitive difficulty; no independent living difficulty; no vision or hearing difficulty; no self-care difficulty.[16]

Additionally, we separately consider two more outcomes:

5. Not Incarcerated: indicator for not being incarcerated, which we can infer based on

[16] Physical ability and health measures are only available in years 2000-2007.

information on residence in group quarters in the 2006-2013 ACS data.

6. Survival to Year 2012: indicator for being alive in year 2012, which is constructed using information on date of death in the full NUMIDENT with valid place of birth strings, and is not limited to our Census/ACS samples.[17] We choose this outcome for our analyses because it is observed for a very large sample and provides a powerful way to analyze the long-term health benefits of the Food Stamps program. To supplement this outcome, we also construct a model-based based estimate of life expectancy following the code and methods from Chetty et al. (2016), who estimates mortality rates by age for race and sex subgroups and then sum them to arrive at a group life expectancy estimate. This estimate of life expectancy is easy to interpret, not sensitive to age at measurement, and useful for monetizing in our MVPF calculation.

The Online Appendix provides more details on construction of these variables and life expectancy in particular. Appendix Table 1 presents means of each of these measures as well as the indices for the full sample and for the race by sex subgroups.[18]

Data on Food Stamps Rollout: Dates of Food Stamps introduction are available at the

[17] The NUMIDENT sample is limited to those who applied for a Social Security Number, are born in the U.S. and whose county of birth string was successfully matched to the county FIPS codes. The variable "Survived to 2012" is the count of the individuals in a birth-year/birth-county cell that have no date of death on record through 2011 (the vintage of our NUMIDENT file), expressed as a share of the number of births in that cell.

[18] The share incarcerated in our sample is higher than other estimates. We weight our regressions using the number of observations in the cell (rather than the sum of the survey weights) to reflect the different sample sizes across our two data sources. Because we are combining the 2000 Decennial Census 1-in-6 sample and the annual ACS, using the sum of the weights would equate the importance across these two samples (since each ACS is representative of the entire U.S. population). Instead by using the sum of observations in the cell, we upweight the Decennial Census relative to the ACS reflecting its significantly larger samples. Practically, however, this has no impact on sample means or model estimates for the outcomes other than incarceration. However, because of ACS sampling, incarcerated (and all those in group quarters) have systematically lower survey weights compared to non-incarcerated in that sample; therefore our share incarcerated is higher than other sources (and higher than we get using survey weights). Our model results for incarceration are not changed qualitatively if we use survey weights. See the Online Data Appendix for more information.

county-by-year-by-month level from data originally collected by Hoynes and Schanzenbach (2009) and subsequently used in Almond et al. (2011) and Hoynes et al. (2016). These data were derived from USDA annual reports on Food Stamps monthly caseloads by county and are available for years 1961-1979.

Data on Potential County-level Confounders: In our main model, we use data on county-level characteristics from the 1960 Census of Population and Census of Agriculture including: the percent of the 1960 county population that lives in an urban area, is Black, is younger than 5, is older than 65, has income less than $3000 (in 1959 dollars), the percent of land in the county used for farming, and log county population. In some models, we also use data on time varying county controls. We use data from the BEA Regional Economic Information System (REIS) to measure county-level control variables on per capita transfers (originally collected by Almond et al. 2011) and population. The REIS data are available for 1959 and 1962, and then annually from 1965. Data from the National Center for Health Statistics (NCHS) are used to measure infant and adult mortality from 1959-1980. We also control for the roll-out of other War on Poverty programs including WIC, Head Start and Community Health Centers (Bailey 2012, Bailey and Duquette 2014, Bailey and Goodman-Bacon 2015, Bailey et al. 2019, Hoynes et al. 2011). The Online Appendix contains more details about the data sources and construction of variables.

IV. Empirical Methods for Identifying the Effects of the Food Stamps Program

We exploit the birth-county-by-birth-year (or birth-year-month) variation in Food Stamps availability in event-study, linear spline, and difference-in-difference models. For computational ease, we collapse our data into birth-year × birth-county × survey-year cells, separately by sex and race (White versus non-White).[19] In some models, we collapse the data by birth month, birth year, and birth county to capture more detailed information on exposure to the Food Stamps program in months since conception.

In order to characterize the effect dynamics by age, we use an event-study specification of the following form:

$$Y_{cbt} = \theta_c + \delta_{s(c)b} + \psi_t + X_{cb}\beta + Z_{c60}b\eta + \sum_{a=-5[a\neq 10]}^{a=17} \pi_a 1[b - FS_c = a] + \epsilon_{cbt} \qquad (1)$$

where an outcome, Y, is defined for a cohort born in county c in state $s(c)$, in birth year b, and observed in survey year t. FS_c is the year in which Food Stamps was first available in county c and event time is a, denoting the age of the individual when Food Stamps was first introduced ($a = b - FS_c$), and event-time coefficients range from five years before birth to age 17, with age 10 as the omitted category.[20] We control for fixed effects for the birth county, θ_c, and a full set of fixed effects for birth state by birth year, $\delta_{s(c)b}$, and survey year, ψ_t. Note that the inclusion of state-by-birth-year fixed effects was not possible in prior analyses due to small sample sizes. Our large-scale administrative data allows us to narrow the scope for bias arising from coincident state-level economic and policy changes (Bailey and Duquette 2014), which is a significant innovation in the literature on the long-run effects of Food Stamps (Hoynes et al. 2016). Per a pre-analysis plan and following the earlier studies using the Food Stamps roll-out (Hoynes and Schanzenbach 2009, Almond et al. 2011, Hoynes et al. 2016), we control for county-level covariates from the 1960 Census, each interacted with a linear birth-cohort trend, $Z_{c60}b$. In robustness checks, we also control for variables that vary at the birth-county × birth-year level, X_{cb}. The event-study

[19] Non-white includes all individuals with a non-missing race variable who do not report being white.
[20] Given birth cohorts 1950-1980 and Food Stamps rollout spans 1961 to 1975, the event-time model is balanced between ages -5 and +11. Therefore, we add binned end points for event time ≤ -6 and ≥ 18 but suppress them from the plots because they are compositionally imbalanced.

coefficients, π_a, capture the effect of access to Food Stamps beginning at age a (relative to the omitted age, 10) on outcome, Y_{cbt}. We cluster standard errors by county of birth and weight using the number of observations in the collapsed cell (Solon et al. 2015).[21]

The event-study model allows for non-parametric estimation of the effects of initial exposure to Food Stamps at different ages during childhood. Following Lafortune et al. (2018), we also estimate a more parsimonious spline model, that allows for different linear slopes for exposure to Food Stamps during different age ranges: pre-conception (prior to age –1), *in utero* through age five (–1 to +5), middle childhood (ages six to 10), and older childhood (ages 11 to17). The linear spline model takes the form:

$$\begin{aligned} Y_{cbt} = \theta_c + \delta_{s(c)b} + \psi_t + X_{cb}\beta + Z_{c60}b\eta + \omega_1 1[b - FS_c < -1] * (b - FS_c) + \\ \omega_2 1[-1 \leq b - FS_c < 6] * (b - FS_c) + \omega_3 1[6 \leq b - FS_c < 11] * (b - FS_c) + \\ \omega_4 1[11 \leq b - FS_c] * (b - FS_c) + \epsilon_{cbt} \end{aligned} \qquad (2)$$

for each cohort born in county c in state $s(c)$, and year b, and observed in survey year t. The segment, $b - FS_c$, is the age at Food Stamps introduction, which we interact linearly with four separate indicators for the exposure groups described above.

Importantly, as discussed above in relation to Figure 2, the years prior to conception (event-time < -1) provide a pre-trend test, since these cohorts are conceived after Food Stamps is introduced in their county and are fully exposed during their entire childhoods. If the effect of Food Stamps on adult outcomes is the same for cohorts born five, four, three, two, or one years after the program is introduced in their county, then the spline model should yield a slope

[21] Our sample includes outcomes captured across a range of ages (e.g. 25-54). In model extensions, we control for a quadratic in age at observation (Table 7), and our results are similar. In addition, we have estimated outcomes at standardized ages, including 30, 31, 32, 33, 34, and 35. We find that the coefficients vary across the ages somewhat and are similar when averaged to the main estimates in the paper. The standard errors are larger for these age regressions, consistent with having smaller sample sizes.

coefficient, ω_1, that is not statistically different from zero. Additionally, we expect that the spline coefficients on exposure after conception, $\omega_2, \omega_3,$ and ω_4, are negative in sign because as age at Food Stamps introduction increases (i.e., $b - FS_c$ is higher), cohorts have *less* exposure to the program. Thus, when discussing the spline estimates below, we often refer to the absolute values of these coefficients. With these estimates, we examine whether the marginal effect of one more year of exposure is larger in the *in utero* and early years than at older ages (i.e., whether $|\omega_2| > |\omega_i|, i = 3{,}4$).

Lastly, we estimate a difference-in-difference (DD) model, using cumulative exposure before age five as the "dose" of the program (Hoynes et al. 2016). We calculate the share of months each cohort is exposed to Food Stamps using the month and year the program began in each county and the (approximate) month of conception and age five, $ShareFS_{cb}^{IU-5}$.[22] We use this measure as the primary explanatory variable in the following equation:

$$Y_{cbmt} = \theta_c + \delta_{s(c)b} + \rho_{\mathrm{m}} + \psi_t + X_{cb}\beta + Z_{c60}b\eta + \kappa ShareFS_{cbm}^{IU-5} + \upsilon_{cbmt} \qquad (3)$$

for each cohort born in county *c* in state *s(c)*, year *b* and month *m*, and observed in survey year *t*. Note that we additionally control for birth-month fixed effects, ρ_{m}, in equation (3), since our data are collapsed to the birth-year × birth-month × birth-county × survey-year level for the estimation of this model.

Regardless of the model, Food Stamps introduction is permanent—once a county implements Food Stamps, it never eliminates it. This feature restricts the set of comparisons that we can make. For example, our data do not allow us to observe a birth cohort first exposed at age two but without exposure in later childhood—if children move after birth, we do not see this in the

[22] Conception is approximated as 9 months prior to the exact date of birth.

data.[23] Therefore, our estimates reflect the effect of additional Food Stamps exposure earlier in childhood, conditional on also having access to it later in childhood. Furthermore, in our setting, exposure earlier in life also means exposure for more years.

Evidence of Treatment Effect Heterogeneity. Another issue for the interpretation of our empirical design stems from the staggered implementation of Food Stamps across counties and absence of untreated counties. Several recent studies explore the implications of such staggered timing for the DD design, demonstrating that conventional DD estimates in this setting represent weighted averages of all possible two-by-two comparisons in the data and may introduce bias if treatment effects are heterogeneous (Athey and Imbens 2022; Goodman-Bacon 2021; de Chaisemartin and D'Haultfoeuille 2020; Borusyak et al. 2020), including when using a continuous treatment variable as in our equation (3) (Callaway et al. 2021). While the econometric literature has not settled upon a single solution to this problem, a common recommendation is to rely on an event study (rather than pooled differences-in-differences), which allows for the possibility of dynamic treatment effects and limits the set of units that can act as comparisons for the treatment group. Importantly, we find limited treatment effect heterogeneity when we compare effects across earlier versus later-adopting counties, which mitigates concerns that our estimates misrepresent the average treatment effects of the Food Stamps program.[24] In addition, limited scope for heterogeneity mitigates concerns raised in Sun and Abraham (2021) about the interpretability of leads and lags in event-study settings.[25]

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[23] Migration in early childhood could be endogenous. As discussed below, we use the PSID to explore this issue and find little evidence of Food Stamps directed migration.

[24] Specifically, we have examined treatment effect heterogeneity by including an interaction term in model (3) between our main treatment variable, $ShareFS_{cb}^{IU-5}$, and an indicator for a county being an "early adopter" (i.e., adopted Food Stamps before 1967, which is when only one third of our analysis sample population had Food Stamps at the time of their birth). Out of the 6 main outcomes analyzed, only one yielded a significant interaction term—the positive effect on the physical health and ability index appears to be stronger among cohorts born in early adopting counties.

[25] Because we do not have never treated units, we are limited in our ability to implement Sun and Abraham's (2021)

Identifying Assumptions and Balance Test. Our research design relies on the assumption that the timing of the Food Stamps roll-out across counties is uncorrelated with other county time-varying determinants of our long-term outcomes of interest. A central threat to identification relates to the potential endogeneity of the policy change, whereby the early adopting counties experience different cohort trends than later adopting counties.

What might be the source of endogenous county adoption of Food Stamps? First, prior to Food Stamps, some counties provided food aid through the CDP. The CDP was foremost an agricultural price support program, in which the surplus food was distributed to the poor. Counties were not permitted to operate both Food Stamps and a CDP, so they had to drop the CDP to implement Food Stamps. Thus, adopting Food Stamps led to a political economy conflict between agricultural interests who favored the commodity program and advocates for the poor who favored Food Stamps (MacDonald 1977; Berry 1984). Hoynes and Schanzenbach (2009) show that, consistent with the historical accounts, more populous counties and those with a greater fraction of the population that was urban, Black, or low income implemented Food Stamps earlier, while more agricultural counties adopted later.[26] Yet they also find that the county characteristics explain very little of the variation in adoption dates, a fact that is consistent with the characterization of Congressional appropriated limits controlling the movement of counties off the waiting list (Berry 1984).

Bitler and Figinski (2019) find that counties with a CDP prior to the Food Stamps adoption had a more rapid expansion in the Food Stamps program following county adoption, which they attribute to the presence of a developed administrative system. Because we do not have data on

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recommended "interaction-weighted" estimator.

[26] See Table 1 and Appendix Figure 2 in Hoynes and Schanzenbach (2009).

this unobserved source of heterogeneity, we are not able to test for this relationship directly. However, the fact that some counties already had some form of food aid program would lead our analysis to understate the effects of providing Food Stamps in a setting with no prior program.

Second, the Food Stamps introduction took place during a massive expansion of federal programs as part of Johnson's War on Poverty, and many of these programs were rolled out across counties. If Food Stamps programs expanded at the same time as other programs were being launched in a county, it would limit our ability to separate the effects of Food Stamps from these other programs. Bailey and Duquette (2014) and Bailey and Goodman-Bacon (2015) compiled information from the National Archives and Records Administration on changes in other county-level funding under the War on Poverty between 1965 and 1980. Using these data, Bailey and Goodman-Bacon (2015) and Bailey, Sun and Timpe (2019) show that the timing of the Food Stamp rollout is not correlated with the launch of CHCs or Head Start. In addition, Bailey and Duquette (2014) show that less than 3 percent of the cross-county variation in the availability of War on Poverty programs is explained by 1960 characteristics.

<u>Additional Validity Checks</u>. In addition, we assess the validity of the research design in four ways. First, we directly test whether our treatment variable is correlated with observable county time-varying characteristics, using the linear exposure model (3). Second, we test the sensitivity of our estimates to adding county-by-year controls, including the rollout of other War on Poverty programs. Third, our preferred specifications include a full set of birth-state-by-birth-year fixed effects, which means that we only rely on within-state variation in program rollout. Finally, the pre-trend test in the event-study and linear-spline models provides an evaluation of differential trends in outcomes for cohorts who were conceived in different years *after* the program was implemented. While there is some theoretical basis for such pre-trends (see section II.C), pre-

trends could also suggest issues with the internal validity of our empirical strategy. Thus, we do not view the pre-trend test as a definitive test of our identifying assumption, but rather as one piece of the evidence providing support for it.

Table 1 presents estimates from the linear exposure model (3) using data collapsed to the birth-year × birth-month × birth-county level. Each row presents estimates of the coefficient on $ShareFS_{cb}^{IU-5}$ from the model using the listed county characteristic as the dependent variable. Consistent with earlier work, we find greater Food Stamps exposure has no association with other War on Poverty programs including WIC, Head Start and Community Health Centers (Bailey and Goodman-Bacon 2015, Bailey et al. 2021) but is associated with larger populations (Hoynes and Schanzenbach 2009). We also find no relationship between Food Stamps exposure and county-level total per capita transfers, average income, employment, or adult or infant mortality rates. We do find a statistically significant *negative* association between Food Stamps exposure and per-capita spending on three categories of transfer programs (retirement and disability, health, cash public assistance). Put differently, these correlations imply that as Food Stamps exposure increases, there is *less* spending on other public transfers in the county. In principle, this could reflect the fact that lower spending on transfers reflects better economic conditions in a given county, which could bias our estimates. Because we find no significant relationship between Food Stamps exposure and county employment or income, we suspect that Food Stamps is reducing the use of these other transfer programs. Overall, the results show that four out of 14 coefficients are statistically significant at the 5-percent level. Because none of these variables is available for all birth cohorts in our sample (1950-1980), we do not include them as controls in our main estimates. Our robustness analysis shows that including them does not change our qualitative conclusions.

V. Results: How Food Stamps Exposure in Childhood Affects Adult Outcomes

A. Main Results in the Full Sample

We begin by presenting estimates for the composite index for the full sample. Panel A of Figure 3 presents the event-study estimates (equation 1), where the estimates (and the y-axis) are in standard-deviation units. The x-axis in Figure 3 denotes a cohort's age at the time of Food Stamps implementation, so that movement along the x-axis from right to left represents earlier (and longer) exposure to Food Stamps. Negative values on the x-axis represent cohorts that were conceived *after* Food Stamps has been implemented in their county—for example, the value "−5" is assigned to cohorts for whom Food Stamps was implemented five years before their conception, while the value "−1" is assigned to cohorts for whom Food Stamps was implemented one year before their conception.

Figure 3 plots two series. One with solid circles as markers plots estimates from a model that includes fixed effects for county, birth year, survey year, as well as 1960 county characteristics interacted with linear cohort trends. The other series with square markers is from a model that includes all of those variables but also adds fixed effects for birth-state × birth-year. The latter is our preferred specification, because it captures potentially confounding, time-varying state policies, such as the roll-out of Medicaid (Goodman-Bacon 2018, 2021), the Elementary and Secondary Education Act (Cascio et al. 2013), the Civil Rights Act (Donohue and Heckman 1991, Almond et al. 2007), and the Economic Opportunity Act (Bailey and Duquette 2014). Consistent with these confounders obscuring the effects of the Food Stamps program, including these birth-state × birth-year fixed effects tends to make the estimates larger. Because we do not observe program participation in our data, note that these are ITT estimates. We discuss the approximate TOT magnitudes below.

The estimates from our preferred specification in panel A suggest that additional years of access to Food Stamps in early childhood (between conception and age 5) lead to larger increases in the composite index. In contrast, there is little evidence that exposure to Food Stamps had effects for children who were aged 6 to 18 years when the program began. Further, there is no evidence of a differential pre-trend for children ages -5 to -1: the effect of Food Stamps exposure is very similar for this group as for children who were in utero (age at Food Stamps rollout=0) at the time the program started.

Even with large samples, the individual event-study coefficients are imprecise. Panel B of Figure 3 plots our preferred event-study specification (with birth-state × birth-year fixed effects) and adds the fitted spline function (equation 2). To match the event-study graph, we plot the spline relative to a value of zero for age 10. We also report the spline coefficient estimates and standard errors in the figure legend. This figure shows that the spline provides a good representation of the estimates in the event study, and highlights how this parsimonious, parametric, model yields more precise estimates. The estimates from the spline model show that one additional year of exposure in early life (*in utero* to age five) leads to a statistically significant 0.0017 standard-deviation (ITT) increase in the composite index and an insignificant, and order-of-magnitude smaller, effect of additional years of exposure at older ages (insignificant 0.0003 for ages six to 11 and insignificant 0.0005 for ages 12-17).[27] Additionally, the pre-trend estimate for cohorts exposed to the program before conception is small and statistically insignificant, meaning that we fail to reject that the pre-trend is different than zero.

Table 2 summarizes these results from the early life cumulative exposure model (equation

[27] As discussed in the description of model (2) above, we refer to the absolute values of the spline coefficients in order to interpret the effect of an additional year of exposure.

3), both with (column 3) and without (column 2) birth-state × birth-year fixed effects. For completeness, we also show results from models without the 1960 county characteristics interacted with linear cohort trends (column 1). The coefficients on Food Stamps exposure before age five are statistically significant in models with and without the birth-state × birth-year fixed effects. Further, looking across the three columns of Table 2, adding these controls tends to increase the magnitudes of the estimates because they account for the secular cohort trends toward worse outcomes in poor, urban areas over the 1960s and 1970s, which bias the analysis against finding a treatment effect in the raw data. Going forward, we use the most saturated model that includes birth-state × birth-year fixed effects as our main specification, which more effectively captures state-level, time-varying confounders.

The preferred model (column 3, Table 2) implies that moving from no access to Food Stamps to full access from conception through age five leads to a 0.009 standard-deviation increase in the adult composite index. To translate this ITT estimate into an average TOT effect, we construct estimates of Food Stamp participation rates. Detailed in Appendix Figure 2A, we use PSID data to plot the share of children living in households who report receiving Food Stamps, by the age of the child, averaging over survey years 1975-1978 to increase our precision. We choose these years as they are the first three calendar years in which Food Stamps is available nationwide. The figure shows that Food Stamps participation among all children averaged 14 percent in these years, breaking down to 16 percent for children ages zero to five and 13 percent for children ages six to seventeen.[28] Thus, we divide our exposure model estimates by 0.16, which suggests the TOT

[28] The PSID provides the earliest available survey estimates for calculating Food Stamp participation rates and we use the first years when Food Stamps is available in all counties. The Current Population Survey begins measuring the Food Stamp participation in 1980 (measuring Food Stamps in 1979); using that data Food Stamp participation for children zero to 5 is 18 percent.

effects of full exposure from conception to age five is around 0.06 standard-deviation units for the adult composite index outcome. The implied TOT from the spline model generates a similar effect, as do the implied TOT magnitudes from the event study.[29]

We next examine each of our four indices separately as well as survival to 2012 and non-incarceration. Figure 4 presents the event-study graphs along with the fitted spline models for each outcome, while Figure 5 presents the absolute value of the spline estimates and their 95% confidence intervals. To facilitate comparisons across outcomes, we use the same y-axis scaling across the four indices. The graphs for survival to 2012 and non-incarceration are on different scales just below, since those impacts are estimated in percentage-point rather than standard-deviation units.

Figure 4 provides striking evidence that early-life exposure to Food Stamps had large effects on adult human capital (panel A), economic self-sufficiency (panel B), and neighborhood quality (panel C), with insignificant and wrong signed impacts on physical ability (panel D). Several patterns emerge across these results. First, Figure 4 shows this visually, and Figure 5 tests this explicitly using the spline specification (plotted as dashed lines with circle markers), that cohorts conceived after the program began do not exhibit effects that are different from children in utero at the time the program began or trends statistically different from zero. This evidence supports the validity of our research design and suggests that a longer amount of Food Stamps exposure for women in pre-conception years is not predictive of their children's long-run outcomes, at least in our setting.

Second, across most outcomes, we see large and statistically significant impacts of

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[29] The absolute value of the estimate of the linear spline covering early life (ω_2) is 0.0017 (see textbox in Figure 3B), which multiplied by the 5.75 years of exposure (conception to age five) implies a 0.01 standard-deviation increase in the composite index ITT or 0.06 TOT. One can see a similar magnitude in the coefficients in the event study.

additional exposure to Food Stamps in early childhood (*in utero* to age five), while the impact of additional years of exposure beginning in middle and older childhood does not translate into statistically significant improvements in long-run outcomes. Figure 5 tests this explicitly and shows that the coefficients on the spline for additional exposure in early childhood are consistently larger and statistically significant (solid lines with triangle markers) than the spline for initial exposure at older ages (solid line with diamond and solid line with star).

Table 3 summarizes these estimates using the early-life cumulative exposure model (equation 3) for these six outcomes.[30] The magnitudes suggest that an increase from no access to full exposure (measured from conception through age five) leads to a 0.010 standard-deviation increase in human capital, an 0.004 standard-deviation increase in economic self-sufficiency, and a 0.012 standard-deviation increase in neighborhood quality. We find little evidence of an effect on physical disability, possibly reflecting the relatively young age of our adult sample as well as data availability (physical disability is only available before 2008). Full exposure to Food Stamps leads to a 0.07 percentage-point increase in the likelihood of surviving until 2012. We also find a 0.08 percentage-point increase in the likelihood of *not* being incarcerated. Dividing these ITT estimates by the Food Stamps participation rate of 16 percent (for children ages five years and younger) shows these results imply quantitatively large TOT effects on long run outcomes: a 6 percent of a standard-deviation increase in human capital, a 3 percent of a standard-deviation increase in economic self-sufficiency, an 8 of a percent standard-deviation increase in neighborhood quality, a 0.4 percentage-point increase in survival to 2012,[31] and a 0.5 percentage-

[30] Appendix Table 2 presents estimates for these six outcomes, where we sequentially add controls to check their robustness (as in Table 2).

[31] These survival probabilities can be transformed into a measure of life expectancy for our MVPF analysis. Appendix Table 3 presents the results from estimating model (3) using life expectancy as the outcome. Our preferred model (column 3) shows that an increase from no access to full exposure from conception through age five leads to a 0.2

point decrease in likelihood of incarceration.[32]

B. Unpacking the Individual Outcomes and Mechanisms

It is useful to "unpack" the standardized indices to gain insight into which individual outcomes are affected by Food Stamps exposure. Figure 6 provides estimates of effects for each of the elements in the four outcome indices based on the exposure model. We use the Romano-Wolf correction when calculating p-values in these analyses to account for multiple hypothesis testing. In order to facilitate comparisons across outcomes, each sub-index outcome is standardized (as a z-score) so the estimates reflect standard-deviation impacts. Additionally, each outcome is reverse-coded as needed such that a higher value reflects a "better" outcome.

This figure illustrates the comprehensive nature of the positive impacts of childhood exposure to Food Stamps on later life outcomes. An increase in Food Stamps in early childhood leads to increases in education through college graduation. Economic self-sufficiency estimates show small and statistically insignificant effects on extensive (in the labor force, worked last year) and intensive margins of labor supply (weeks worked, usual hours worked per week), but also positive and statistically significant impacts on log earnings, the log family income to poverty ratio, and the likelihood of not being in poverty according to the official federal poverty line. These findings imply that by increasing earnings and reducing the likelihood of poverty in adulthood, the social safety net serves as a long-term investment that may at least in part pay for itself. We also find that more exposure to Food Stamps in early life leads to a reduction in the likelihood of having any income from public sources in adulthood.

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year increase in life expectancy (ITT) or a 1.1 year increase (TOT).

[32] As detailed in the data section and the Online Data Appendix, the share incarcerated in our sample is higher than other estimates due to a lack of using survey weights in our main analysis. Our results are not qualitatively changed we weight with the sum of survey weights.

Food Stamps exposure also increased measurable dimensions of neighborhood quality. We document that greater childhood exposure to Food Stamps leads to a large increase in the likelihood of home ownership and an overall improvement in the quality of adult neighborhood of residence. Specifically, using Census tract statistics, we find that Food Stamps exposure during early childhood increases mean income as well as reduces child poverty, teen pregnancy rates, and single-headship of one's neighbors (in the census tract). We also find that early childhood exposure to Food Stamps is associated with an increase in the absolute upward mobility of one's county of residence in adulthood (Chetty et al. 2014). Overall, the improvements in individuals' economic circumstances in adulthood—both in terms of neighborhood quality and family income—are strongly suggestive of important intergenerational effects of Food Stamps. In other words, early childhood exposure to Food Stamps changes long-term outcomes for affected individuals *and* improves the conditions into which their children are born.

The magnitudes suggest important effects of resources on long-run outcomes. The unstandardized estimates for each of the sub-index components are provided in the first column of Table 4. The stars indicate significance using Romano-Wolf p-values; all p-values are reported in Appendix Table 4. Full exposure to Food Stamps between conception and age five leads to a 0.04 year increase in the number of years of schooling (0.25 years TOT), a 0.7 percentage point increase in educational attainment of some college or more (6 percent TOT), a 1.1 percent increase in earnings (7.1 percent TOT), a 0.4 percentage point reduction in likelihood of being poor (2.4 percentage points TOT), and a 0.6 percentage point increase in the likelihood of home ownership (3.7 percentage points TOT). The pervasiveness of these estimates suggest that there is unlikely to only be a single channel driving the long-term effects of Food Stamps. For example, a back-of-the-envelope calculation suggests that the increase in adult earnings is unlikely to be entirely explained by the increase in educational attainment.[33] Indeed, our findings on incarceration and mortality suggest that improvements in other determinants of long-term earnings (e.g., non-cognitive skills, health) also play a role. The improvement in metabolic health found by Hoynes et al. (2016) is consistent with this conjecture as well.

C. Heterogeneity in Estimates by Race, Sex, and Mobility

Table 5 presents the estimates from the exposure model for the four indices plus survival and non-incarceration, separately for white men, white women, nonwhite men, and nonwhite women. We use bolding of the estimates to denote which sub-group estimates for white women and non-white men and women are statistically different from those for white men. Qualitatively, the human capital effects are strongest for white men (0.01 standard deviations ITT) and white women (0.008 standard deviations ITT) and insignificant for nonwhites. However, these differences are not statistically significant. This pattern reflects both differences in sample sizes and sampling variation. Limiting the sample to nonwhites reduces the sample sizes to less than 15 percent of the overall sample and, unlike the PSID, the Census/ACS data have few family background characteristics to explain the considerable variation in outcomes. That said, the lack of access to high quality schools for Black individuals during this time period may have prevented them from reaping the full benefits of the Food Stamps program. Consistent with this idea, Johnson and Jackson (2019) document the importance of "dynamic complementarities" between investments in early childhood (Head Start in their case) and school quality at older ages.

We also find that the improvement in neighborhood quality is particularly large among

[33] The return to an additional year of schooling for the cohorts in our analysis is approximately 10 percent (Card 1999). Thus, our TOT estimate of 0.25 of a year increase in schooling due to early childhood Food Stamps exposure would imply that earnings should increase by 2.5 percent. We instead report a TOT estimate of a 7.1 percent increase in earnings, which is nearly three times larger.

white women, while non-white men experience an improvement in physical health as captured by the physical disability index. The survival and incarceration effects are not significantly different across the groups.

The last four columns of Table 4 report the results for the individual components of our four indices for the four race-sex sub-groups. We bold the estimates to indicate those that statistically significantly different from white males, and the stars indicate p-values from the Romano-Wolf correction to account for multiple hypothesis testing. Consistent with the results using the indices, we find that the positive impacts on human capital outcomes are concentrated among whites. For example, full exposure leads to a 0.7 percentage point (ITT) increase in the probability of some college or more for white men and 0.6 percentage points for white women with small and statistically significant effects for nonwhite men and women. White males also experience the largest increases in adult labor income, while non-white males show increases in employment and hours worked. Interestingly, we observe negative effects on several labor market outcomes among white females, although these are not statistically significant once we account for multiple hypothesis testing. At the same time, white females experience improvements in several margins of adult neighborhood quality, which may be consistent with these women being more likely to marry white men who are earning more as adults. All but one of the coefficients for these individual components are statistically insignificant for nonwhite females. Moreover, we cannot reject that the effects among nonwhite subgroups are statistically different from those white males for most of the outcomes.

Appendix Figures 3 through 5 further explore the differences by race and sex by presenting event-study graphs for outcomes separately by subgroup. Appendix Figure 3 shows that the gains in survival are concentrated among nonwhite men and women, with small and insignificant (and for white men opposite-signed) effects for whites. Additionally, the effect of Food Stamps exposure on survival for nonwhites extends more through childhood, rather than concentrating in early life as we see in other outcomes. Appendix Figure 4 demonstrates that access to Food Stamps leads to an increase in the probability of *non*-incarcerated but only for nonwhite men (with noisy and wrong signed results for white men and nonwhite women). As with survival, the long-run benefits of Food Stamps for nonwhite males, in reducing incarceration, are consistent through childhood rather than being concentrated in early life. The estimates are sizable—for every year of exposure during early life (in utero through age 5) incarceration declines by 0.1 percentage points or about 1 percent (ITT). Notably, the preventative effects of Food Stamps on incarceration for nonwhite males was not evident in the exposure model estimates (Table 5, column 6). As shown in Appendix Table 5, this is because the early life exposure model does not model exposure in later childhood. The model in Appendix Table 5 estimates the non-incarceration model for nonwhite men and includes exposure in utero to age five as well as exposure between ages 6-18. The estimates are positive for both and statistically significant for exposure at ages 6-18. (We present this extended model more comprehensively across our other outcomes below.) Finally, Appendix Figure 5 shows that the impacts of Food Stamps on neighborhood quality is consistent across the four race-sex subgroups.[34]

We explore mobility as a potential mechanism in Table 6. In the first column, we use our entire sample and estimate the exposure model (equation 3) using as the dependent variable the share moving from one's county of birth to a different county in adulthood in our outcome data.[35]

[34] Appendix Table 6 presents the spline estimates for the four indices, survival and incarceration for each of the four subgroups.

[35] We observe location of birth in the NUMIDENT file (capturing county of birth) and in the Census/ACS (capturing residence at the time of the Census or survey). We assign stayer/mover status using those two points in time.

We find that full exposure to Food Stamps significantly increases the likelihood of moving away from one's county of birth by 0.85 percentage points (5.3 percentage-point TOT, or 7.5 percent TOT relative to the sample mean of 71 percent).[36] This result suggests that the effect of Food Stamps on neighborhood quality (and, potentially, the other outcomes) at least in part operates through individuals relocating to better places. To explore this further, we compare individuals' birth and adult counties along two (time-invariant) margins: the number of four-year colleges and the degree of urbanicity. As shown in Appendix Table 7, we find that full exposure to Food Stamps is associated with a 0.2 percentage-point increase in the likelihood that one's adult county has a higher number of four-year colleges than one's birth county. Subgroup analyses indicate that this effect is seen for all race/sex sub-groups, except for non-white males. We do not find a statistically significant impact on the change in urbanicity between one's adult and birth county in the overall sample, but we do see a significant effect on this outcome for non-white females. These results suggest that geographic mobility to places with more colleges and more urban areas are potential mechanisms.

The rest of Table 6 examines the differences in impacts on our main outcomes between the subsample who remain in their county of birth (labeled "Stayers") and those whom we observe no longer living in their county of birth at the time of the survey in adulthood (labeled "Movers"). Overall, comparing across the human capital, economic self-sufficiency and neighborhood quality indices (we drop the physical disability index from this table for space reasons, as the results are statistically insignificant), the estimates of exposure to Food Stamps are larger for stayers compared to movers. The smaller estimates for movers are consistent with misclassification in childhood Food Stamps exposure, subgroup heterogeneity, or that geographic mobility is a mechanism for the treatment effect of Food Stamps (the means of the dependent variables are larger and Food Stamps increases the likelihood of being a mover).[37]

D. Robustness and Validity of the Design

Children fully exposed to Food Stamps (i.e., those who are conceived after the program was implemented in their county of birth) provide one potential test of the validity of our design. Appendix Figure 6 reports the absolute values of all of the spline estimates (and their 95-percent confidence intervals) of the "pre-trend" for seven outcomes (composite plus four sub-indices, survival, and non-incarceration) and four race-sex subgroups. The figure shows that of the 28 estimates, only two are statistically different from zero (neighborhood quality index for nonwhite males and disability index for white females), no more than expected by chance. This provides additional evidence supporting our empirical strategy. The figure also makes clear that we have less precision when estimating effects for nonwhites, who represent less than 15 percent of the overall sample.

We also examine the sensitivity of our results to adding addition controls, shown in Table 7. First, we consider the sensitivity to adding controls for age at survey, recognizing that we have a range of ages in our sample. However, because our baseline model includes birth-state × birth-

[37] Recall that we assign Food Stamps exposure using county of birth and we observe location only at birth and in adulthood in our outcome sample. Consequently, we do not have data on when the individual moved (if they did). If children move in early childhood, this could generate misclassification error in Food Stamp exposure. Alternatively, endogenous or "directed" migration could introduce bias into our exposure measure if motivated (and potentially more economically successful) individuals who are not exposed to Food Stamps are systematically more likely to move to counties with Food Stamps before age five (i.e., a negative correlation between Food Stamps exposure at birth and subsequent Food Stamps availability in one's destination county). We have explored this possibility using restricted longitudinal PSID data, which contains information on individuals' counties of birth and counties of residence during childhood for cohorts born in 1968 or later. Appendix Table 8 presents those estimates where we relate the incidence of moving by age five (columns 1, 3) and moving by age five to a county with Food Stamps (columns 2,4) to our Food Stamp exposure measure (share of time between conception and age five that Food Stamps was in place in your county of birth). We do not find evidence consistent with endogenous migration—if anything, Food Stamps exposure in one's county of birth is slightly positively correlated with the likelihood of moving to a county with Food Stamps during childhood.

year fixed effects and survey year fixed effects, and given the identity that survey year = birth year + age at survey, we are limited in our ability to control for age fixed effects. Instead, we add a control for a quadratic in age at observation, and our results are unchanged. Column 1 presents our base estimate of 0.0087 (repeated from Table 2, column 3) and column 2 shows that adding the quadratic in age reduces it to 0.0085. The rest of Table 7 shows the sensitivity to adding time-varying county-level controls. We include all of the variables in our balance table (Table 1) that are available for 1959-1980 (covering most of our full birth-cohort sample of 1950-1980), including the presence of War on Poverty programs, transfer spending, mortality rates, and the natural log of county population. We limit the sample to the observations with non-missing variables for all of these controls. In column 3, we estimate our baseline specification for this restricted sample and the coefficient on Food Stamps exposure (0.0087) is unchanged from that estimated in the full sample. Adding the control for log population (column 4) reduces the magnitude of the impact of Food Stamps exposure slightly, and adding further controls (column 5) leaves the estimate virtually unchanged.

Once Food Stamps is in place it is never eliminated. Therefore, exposure at younger ages implies exposure at older ages. Our main exposure model captures the share of time between conception and age five that Food Stamps is in place and does not account for exposure throughout the rest of childhood. In Table 8, we present estimates for the six main outcomes for the full sample adding a second exposure variable—the share of time between ages 6 and 18 that Food Stamps is in place. None of the estimates of the later child exposure is statistically significant, and the estimates on the early-life exposure variable remain of similar magnitude and statistical significance as in the main results.[38] Thus, with the important exception of the results for non-

[38] As seen in Appendix Figure 2A, Food Stamps participation rates among children ages 6-18 are slightly lower than

incarceration among nonwhite males discussed above, we find no evidence that Food Stamps exposure at ages beyond five years old has additional impacts on adult outcomes.

VI. Magnitudes and Relation to the Existing Literature

The literature on the long-run impacts of early-life exposure to the near-cash social safety net is small. However, a few studies provide some comparisons to our estimates. Hoynes et al. (2016) find that full exposure from conception to age five leads to a 0.7 standard deviation improvement in a "metabolic syndrome" index and a 0.4 standard deviation improvement in an economic self-sufficiency index (both are TOT estimates). However, only the estimate for the metabolic syndrome index is statistically significant at conventional levels. While we do not observe outcomes that we could group into a similar metabolic syndrome index, their economic self-sufficiency index includes measures spanning our human capital and economic self-sufficiency indices. We note that our estimated TOT effect sizes are substantially smaller but still statistically significant—we find a 0.03 standard deviation increase in economic self-sufficiency (TOT, 0.004/0.16) and a 0.06 standard deviation increase in human capital (TOT, 0.010/0.16). This finding underscores the importance of using a large enough sample to detect long-term impacts of early childhood programs with precision.

Bitler and Figinski (2019), who use large-scale administrative earnings data, find that full

those among children ages 5 years or less (13% vs. 16%). Yet even if we scale the (insignificant) coefficients on exposure at ages 6-18 by the age specific participation rates, we get economically small magnitudes. Additionally, in Appendix Figure 2B, we use PSID data to demonstrate that there are no discontinuous changes in the length of time individuals spend on Food Stamps between those who first use the program at ages younger than 5 versus ages older than 5. This suggests that the difference in effect sizes between exposure below and above age five is not driven by a difference in the duration of benefit receipt. Further, Appendix Figure 7 uses 1970 and 1980 Census data to show that there are no discontinuous jumps in migration rates between children under age five and over age five among children in disadvantaged families (as proxied by mothers having less than a high school degree). Thus, the difference in effects between exposure below and above age five is also not driven by differences in measurement error when assigning exposure based on a child's county of birth.

exposure to Food Stamps from conception to age five leads to small and statistically insignificant effect on earnings at 32 for men and a 15 percent (TOT) increase in earnings at age 32 for women. We find a 7 percent (TOT) increase in labor income for the full sample of males and females (Table 4, 0.0114/0.16). That being said, and in contrast to Bitler and Figinski (2019), we find that this effect is concentrated among white males.

Page (2021) presents a comprehensive quantitative review of the literature estimating the long run effects of safety net programs on children, including our study. Analyzing studies of the EITC (Bastian and Michelmore 2018), Negative Income Tax Experiments (Price and Song 2017), tribal cash payments (Akee et al 2010), and Food Stamps (Bitler and Figinski 2019, Hoynes et al 2016, and this study), Page finds that an increase in $1000 in childhood leads to just less than 1 percent increase in earnings in adulthood and an increase in completed years of education of 0.01-0.02. Using her reported range of results, our estimated impacts of Food Stamps are larger than the NIT and similar to the estimates for tribal payments and the EITC.

It is also useful to make broader comparisons to the literature (beyond those that focus on cash or near-cash assistance), such as studies on the long-run impacts of Medicaid and Head Start. Brown et al. (2020) find that increases in Medicaid coverage in childhood leads to a reduction in mortality in young adulthood. Using their estimates for the linear effect of years of Medicaid eligibility multiplied by 5.75 years of access (equivalent to length of access for our *in utero* through age five exposure model) and adjusting for take-up, their estimates generate an 8 percent TOT reduction in mortality for women and a 13 percent TOT reduction for men. This compares to our 11 percent TOT estimate for food stamps the full sample. Deming (2009) finds that participating in Head Start leads to a 0.23 standard deviation increase (TOT) in a summary index of young adult outcomes that includes high school graduation, college attendance, idleness, crime, teen parenthood and health status. This is probably best compared to our Food Stamps TOT impact on human capital of 0.06 standard deviation. Bailey et al. (2019) use the Census/ACS/NUMIDENT data used here along with a county Head Start rollout design and find that a TOT effect of Head Start on human capital index of 0.10 standard deviation (slightly larger than our estimate).

VII. Comparing Costs and Benefits of Early Childhood Food Stamps Exposure

This final section uses the framework proposed by Hendren (2016) and used in Hendren and Sprung-Keyser (2020) to calculate the Food Stamps Program's marginal value of public funds (MVPF), or the ratio of benefits to the net government costs (i.e., fiscal externalities). Equivalently, the MVPF is the ratio of the beneficiaries' willingness to pay for the increase in expenditure out of their own income to the cost to the government of the policy per beneficiary.

In terms of benefits, how much would Food Stamps recipients be willing to pay for a dollar of program expenditures? Because the benefits must be used to purchase food, they may not be valued dollar for dollar by beneficiaries. In addition, we need to value how much children of eligible parents would be willing to pay out of their own income for an extra dollar of Food Stamps benefits transferred to their parents. We use our estimates of (i) the increases in labor income, (ii) the increases in life expectancy, and (iii) the reductions in incarceration rates in adulthood to quantify the long-run benefits to children. We then translate these benefits into willingness to pay (WTP) measures, while also calculating the implied fiscal externalities associated with these changes.

We make the following assumptions when evaluating WTP for the Food Stamps program for a household with children between conception and five. While some evidence suggests that Food Stamps receipt does not significantly alter purchase decisions in ways that would imply dollar for dollar valuation (Smeeding 1982), Whitmore (2002) suggests that individuals only value a dollar of SNAP payments at 80 cents. For children, we include their willingness to pay for their increase in after tax earnings and estimated increases in life expectancy. To estimate after tax earnings gains for children, we follow Hendren and Sprung-Keyser (2020) to estimate, first, the lifetime earnings of children exposed to Food Stamps during early childhood. They use the parental earnings estimates from Hoynes, Schanzenbach and Almond (2016), which they convert to present discounted lifetime parental income using the profile of lifetime earnings in the 2015 ACS, a 0.5 percent wage growth assumption, and estimates of the distribution of parental earnings from Chetty et al. (2018). They then apply an intergenerational elasticity to this number to recover a predicted present discounted value of child lifetime income as adults. Lastly, they take the TOT estimates on the labor-market returns from this study and apply an average tax rate of 12.9% to suggest that children would be willing to pay $0.45 for every $1 of Food Stamps spending due only to the gains in the labor income. Our preferred estimate of the increase in life expectancy implies a TOT increase of 1.2 life years for full exposure to Food Stamps between conception and age five (0.198/0.16, Appendix Table 3, column 3). We follow the standard approach of using the value of a statistical life (VSL) to convert changes in mortality rates into dollars. Our primary approach relies on the U.S. Environmental Protection Agency's (EPA) VSL estimate of $10.95 million (2018 USD).[39] Following Carleton et al. (2019), we calculate the value of lost life-years by dividing the U.S. EPA VSL by the remaining life expectancy of the median-aged American (47.2). This recovers an implied value per life-year of $232,000. In 2018, recipients received an average of $3,024 in Food Stamp benefits per household annually. Using an average family size

[39] This VSL is from the 2012 U.S. EPA Regulatory Impact Analysis (RIA) for the Clean Power Plan Final Rule, which provides a 2020 income-adjusted VSL in 2011 USD, which we convert to 2018 USD.

of 3.29 and computing the total benefit from conception to five years, an average child from a treated household would receive $4,595 in benefits. Thus, the implied WTP for the increase in life expectancy for children is estimated to be around $278,400 (a 1.2 year increase in life expectancy times $232,000 in value per life year) or $61 per dollar of SNAP spending for a child from conception to age five.

Fiscal externalities associated with the program include the potentially distortionary impact of Food Stamp provision on earnings and government revenue (Hoynes and Schanzenbach 2012), the program-generated long-run reductions in public assistance income and incarceration rates, and the increased tax revenues stemming from improvements in labor income of affected children. The distortionary impact of the program on adult earnings is a cost to the government in terms of foregone tax revenue, whereas the reductions in government payments on public assistance, incarceration, and increased tax revenue from children's labor-market gains offset some of these costs.

Following Hoynes and Schanzenbach (2012) and Hendren and Sprung-Keyser (2020), Food Stamp's introduction leads to a statistically insignificant decline in labor earnings of $219 among households headed by a nonelderly individual with a high school education or less, which, scaling by the participation rate of six percent, implies that Food Stamps enrollment leads to a $3,650 decline in annual labor earnings. Using a tax rate of 12.9 percent, this calculation implies a fiscal externality or cost of $471, or $0.16 for every $1 of Food Stamps benefits expenditures. In terms of our estimated reductions in incarceration rates, the current estimated costs of incarceration are $31,978 annually (in 2016 dollars).[40] According to the Bureau of Justice Statistics, the average

[40] See https://www.federalregister.gov/documents/2016/07/19/2016-17040/annual-determination-of-average-cost-of-incarceration (accessed on 3/11/2020).

length of time spent incarcerated is 2.6 years.[41] Our TOT estimates suggest that Food Stamps increased the fraction *not* incarcerated by 0.5 percentage points, and thus the total fiscal externality amounts to $416 ($31,978*2.6*0.005) or $0.09 per dollar of expenditure using the $4,595 in benefit expenditure from above. Lastly, additional fiscal externalities are associated with the increases in government tax revenue, because of the long-run labor-market impacts of the children. As mentioned above, the after-tax earnings benefits for children are $0.45 per dollar of expenditure using an average tax rate of 12.9 percent. This implies a revenue externality for the government of $0.07 per dollar of expenditure.[42] The net impact of these offsetting costs/benefits turns out to be a fiscal cost of $0 per dollar of Food Stamps expenditures (i.e., -0.16+0.09+0.07).

Based on these calculations, we arrive at an MVPF of 62.25.[43] Note that one could also amend these calculations to incorporate relative social welfare weights between parents and children, whereas here we treated them equally. Our Food Stamp MVPF is similar to or larger than the MVPFs estimated for child Medicaid expansions and highly regarded early childhood education interventions, such as the Perry Preschool and the Carolina Abecedarian Program (Hendren and Sprung-Keyser 2020). It is also higher than Hendren and Sprung-Keyser (2020)'s calculation of the MVPF associated with the Food Stamps program. There are two main differences between our estimates and Hendren and Sprung-Keyser (2020). The first is that we directly estimated improvements in life expectancy as opposed to backing them out from estimates of survival until the year 2012. Second, we use a VSL amount that is more consistent with recent federal regulatory impact analyses and is larger than the value used in Hendren and Sprung-Keyser

[41] See https://www.bjs.gov/index.cfm?ty=pbdetail&iid=6446 (accessed on 3/11/2020).
[42] This calculation comes from the difference in the pre-tax earnings gains per dollar of expenditure ($0.516), relative to the post-tax earnings gains per dollar of expenditure (0.45).
[43] This is calculated by summing the willingness to pay and dividing by the net cost to the government (i.e., (0.8+0.45+61)/(1+0) = 62.25).

(2020).

VIII. Conclusion

Children constitute nearly one third of all poor individuals in the United States, making them important beneficiaries of the social safety net system.[44] A recent report from the National Academies of Sciences documents that since the War on Poverty began in the 1960s, there has been substantial progress in reducing the child poverty rate from 28.4 percent in 1967 to 15.6 in 2016 (National Academies of Sciences 2019).

However, changes to the poverty rate provide an insufficient metric for evaluating the success (or failure) of safety net programs. At their inception, these programs aspired to prevent poverty, increase opportunities and give beneficiaries a “hand up, not a handout.” Today, policy makers often use this rationale to motivate spending on early childhood programs—such as preschool and nurse home visiting interventions—which generate upfront costs but can be viewed as *investments* into adult human capital, health, and economic well-being. That is, the value of these investments may not materialize for many years.

A similar logic suggests that understanding the potential *long-term* benefits of access to anti-poverty programs in early life is critical from a public finance perspective—if these programs improve adult economic well-being, thus generating both private returns and public benefits, the social safety net system may partially pay for itself.

In this paper, we use data on 17.5 million Americans to provide the most comprehensive analysis to-date of the long-term impacts of early childhood access to the Food Stamps program, a central pillar of the U.S. social safety net. We combine data from the 2000 Census and the 2001-

[44] See the U.S. Census Bureau for statistics about the age distribution of the poor: https://www.census.gov/data/tables/time-series/demo/income-poverty/historical-poverty-people.html

2013 ACS with data from the SSA NUMIDENT, and exploit the county-by-year variation in the initial rollout of Food Stamps over 1961 to 1974 to measure the impacts of exposure to the program at various ages during childhood on a wide range of adult outcomes, including human capital, economic self-sufficiency, neighborhood quality, disability, incarceration, and longevity.

Our results show that access to Food Stamps in one's county of birth in every month between the time of conception and age five has large consequences for adult well-being. Specifically, we find a 0.009 standard-deviation increase in a composite index of adult human capital and well-being, driven by a 0.010 standard-deviation increase in human capital, a 0.004 standard-deviation increase in economic self-sufficiency, and a 0.012 standard deviation increase in neighborhood quality. We also document a 0.07 percentage-point increase in the likelihood of survival to 2012 (or a 0.2 year increase in life expectancy) and a 0.08 percentage-point reduction in the likelihood of being incarcerated. Scaling these ITT impacts by the approximate 16 percent Food Stamps participation rates in early childhood implies large long-term benefits of Food Stamps for participating children. These estimates imply a MVPF of 62.25, suggesting the program is highly cost-effective.

Our findings have important implications for current debates about the social safety net. The Food Stamps program (or SNAP) is one of the largest U.S. cash or near-cash means-tested transfer programs and is the only safety net program available to nearly all income eligible families (other programs limit eligibility to particular subgroups determined by age, disability status, or household structure).[45] Food Stamps also plays an important countercyclical role by automatically increasing benefits as need increases (Bitler and Hoynes 2016), it played an important role in

[45]Able-bodied adults 18 to 49 without dependents can only receive SNAP for three months in three years if they do not meet work requirements.

protecting families from income loss in the Great Recession and is doing so during the COVID-19 pandemic. Credible and comprehensive estimates of the program's long-term impacts are essential for informing cost-benefit calculations that may influence budgetary decisions.

There are still many questions left open by this study. Importantly, we are unable to observe the precise mechanisms driving the impacts of early childhood exposure to Food Stamps on adult outcomes. Additionally, the fact that we find improvements in adult economic self-sufficiency and neighborhood quality suggests that there may be *intergenerational* impacts of the program on the children of the children who benefitted during the program's initial roll out. As more time passes and additional data linkages become available, investigating these even-longer-term benefits may be fruitful areas for future research.

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Online Appendix

I. DATA DETAILS CENSUS/ACS

Allocated Values: We treat as missing any variable that is allocated. An important exception to this rule arises for allocations of age, sex, relationship to household head, and marital status. Because the family interrelationship assignment relies on the location of individuals within a household, we follow IPUMS and use these variables to construct the family interrelationship variable. After these relationship variables are constructed, we treat these four variables as missing if they are allocated.

Top-coded Values: For each income measure, we follow IPUMS and designate as the top code the 99.5th percentile of the (weighted) income measure distribution. Following IPUMS, this top-coding is done at the state-year level, identifying those at the 99.5th percentile and above separately for each state and year. Any observation greater than or equal to the top code is replaced with the state-year mean among all observations above the top code. This top-coding is done on the sample after eliminating allocated income variables. Aggregate income measures (e.g., earned income: the sum of wage and business/farm income) are constructed after the top code adjustment. We follow the same procedure for gross rent, which is the sum of rents and the cost of electricity, water, gas, and fuel. In particular, we separately top code each component and then construct gross rent as the sum of the top-coded components. We also follow the same procedure for housing values in years 2000 and 2008-2013; in years 2001-2007, housing values are only reported in intervals, which eliminates the need for top-code adjustments.

Imputation of Categorical Variables: Only categorical values of certain variables appear in some years. For example, from 2008-onwards, weeks worked last year is reported in intervals: 1-13, 14-26, 27-39, 40-47, 48-49, and 50-52. Using data from 2000-2007, we calculate the average number of weeks worked for each interval, and use this imputed mean in our analysis. We use the same method to impute means for housing value (2001-2007), and education (for 2000-2007, education is binned for grades 1-4, 5-6, and 7-8).

Real Values: All monetary variables are expressed in in 2015 dollars, adjusting for inflation using the Consumer Price Index.

Unit of observation: For computational reasons, all models are estimated on data collapsed to cells using Census or ACS weights. For the event study (equation 1) and spline (equation 2) models, cells are defined as birth-year x birth-county x survey year. For the exposure model (equation 3) the cells are birth-year x birth-month x birth-county x survey year. We collapse separately for all sex and race categories combined as well as by four sex x race subgroups (male-female-white-nonwhite). Sometimes, we do not have a cell for each combination, because the distribution of race is not even across all counties or there are no births in a given county for a specific month-year-race combination. In addition, a handful of counties are dropped from the analysis if we do not have information on when Food Stamps started (these are indicated in yellow in Figure 1).

Weighting the Data: In our main estimates we weight by the number of observations in each cell. We have also explored alternative weighting using the sum of the person weights (the recommended census/ACS weights) in the cell, which yield similar estimates. In accordance with the Census policy of minimizing disclosures, we have only disclosed our preferred set of estimates.

Creating Indices: We ignore observations with missing values on *any outcome of interest* when aggregating to indices so indices will have the same number of observations in our sample for all outcomes. This is in accordance with Census policy to minimize implicit samples in disclosure.

Incarceration and Group Quarters: Incarceration is assigned using the group quarters variable. Group quarters are separated between the institutionalized and noninstitutionalized. We proxy for incarceration using the institutionalized indicator (National Research Council 2012, Ch. 2). This data is available for the 2006-2013 in the ACS. The group quarters question is included in the 2000 Census but this variable is unfortunately not available in the RDC.

Appendix Table 1 shows that the mean incarceration rate for our nonwhite male sample is 14 percent, whereas tabulations of the public use 5-year 2010 ACS yield estimates more along the lines of 6 percent. Our higher incarceration rate is due to two factors. First, and most importantly, while we use Census and ACS survey weights to construct cell means, we use the number of observations represented in each cell to weight the regression and to construct global means. That works well for most of our outcomes, but the nature of group quarter survey design yields person weights for incarcerated individuals that are lower than non-incarcerated individuals. For example, in the public use 2016 ACS, institutionalized men 25-54 have a person weight of 61 on average compared to 112 for non-institutionalized men of this age. As a result, when the number of observations is used as a weight for each cell, institutionalized individuals are upweighted relative to their incidence in the population. Second, we construct our sample to include only "full information" observations: in particular, we drop all observations that are missing or allocated for any of our outcome variables. As discussed in Section III we do this to minimize disclosure risk (e.g. to maintain one sample across the outcomes). However, we cannot impose this restriction on the institutionalized (group quarters) sample because they are enumerated in the Census but not subject to the full survey. Thus this also upweights the incarcerated sample. These two factors explain the higher than expected incarceration rate. These factors have no impact on other variables in our analysis. And the estimated models for incarceration are qualitatively similar if we incorporate survey weights (using the sum of the weights instead of the number of observations) and yields a mean that is more consistent with other sources.

II. COUNTY CONTROL VARIABLES

In robustness checks (Table 6), we examine the sensitivity to adding county-time varying variables to our models. In Table 1, we use a longer list of county variables for a balance test on our design. These variables are assigned at the county-by-year-of-birth level.

A. *Other War on Poverty Programs*

We use data from Bailey and Duquette (2014) and Bailey and Goodman-Bacon (2015) to account for the launch of other War on Poverty programs. They collected data on the OEO's community programs from the National Archives Community Action Program (NACAP) files as well as from some administrative sources.

For Head Start, they compared data with Ludwig and Miller (2007) and Barr and Gibbs (2018) on county-level Head Start program expenditures over 1965-1980 and also compared their figures against state-level administrative reports. The resulting database contains information on (1) the county where a program delivered services, which allows each federal grant to be linked to birth counties and (2) the date that each county received its first program services grant, which typically provides the year that programs began operating.

For Community Health Centers, they entered information from annual Public Health Service (PHS) Reports. This database contains information on (1) the county where CHCs delivered services, which allows each federal grant to be linked to county-level mortality rates; (2) the date that each county received its first CHC services grant (this excludes planning grants), which provides a consistent proxy for the year that each CHC began operating; and (3) information on CHC grants between 1978 and 1980 from the National Archives Federal Outlays (NAFO) files.

For WIC we use data from Hoynes, Page, and Stevens (2011) who collected data on the county-by-county rollout of the WIC program from several directories and congressional filings that provide lists of local agencies that provided WIC services. The rollout occurred between 1974 and 1980. This information is available for years 1974, 1975, 1978, 1979, and 1989.

For each of these programs, we construct an indicator variable capturing whether the county had a given War on Poverty program in place that year.

B. REIS County Transfer Spending

We use data from Hoynes and Schanzenbach (2009) and Almond et al. (2011) to control for other social safety net spending at the county level. Hoynes and Schanzenbach (2009) use data from the Bureau of Economic Analysis Regional Economic Information System (REIS) to construct four per capita county transfer variables: cash public assistance benefits (AFDC, Supplemental Security Income, and General Assistance), medical spending (Medicare, Medicaid, and military health care), cash retirement and disability payments (Old-Age Survivors Insurance, Disability Insurance, and other), and all transfers. The data are available digitally beginning in 1969. Almond et al. (2011) extended the REIS data to 1959 by hand-entering data from microfiche for 1959, 1962, and 1965 to 1968. We linearly interpolate within counties to fill in the gaps (1960, 1961, 1963, and 1964).

C. County Employment, Income, and Population

County income is real per capita county income and is available from the Bureau of Economic Analysis and County Business Patterns (Ody and Hubbard 2011, Bureau of the Census 2006) and available for 1969-1980. County employment comes from Bureau of Economic Analysis Local Area Employment Indicators for 1969-1980. County population is available from SEER from 1969-1980 and is interpolated between decennial censuses for years prior to 1969.

D. County Mortality Data

We use data from Almond et al. (2011) who create county-by-year measures of infant mortality for 1959-1980 using the Vital Statistics Detailed Cause of Death data. The data encompass the universe of death certificates (except in 1972, when they are a 50-percent sample); we use information on age of the decedent and the year and county of death. We then construct infant mortality (deaths in the first year), neonatal mortality rate (deaths in the first 28 days) and post-neonatal mortality (deaths in months 2-12) each expressed per 1,000 live births. Vital statistics data on births (per year and county) are used to construct the denominator for live births.

Adult mortality rates (deaths per 1,000) comes from Bailey and Bacon-Goodman (2015).

E. 1960 County Control Variables

To capture trends across counties over time, we control for 1960 County Characteristics interacted with linear trend in birth cohort. Following Hoynes and Schanzenbach (2009) we use the 1960 City and County Data Book, which compiles data from the 1960 Census of Population and Census of Agriculture, is used to measure economic, demographic, and agricultural variables for the counties' pretreatment (before Food Stamps is rolled out) period. In particular, we use the percentage of the 1960 population that lives in an urban area, is Black, is less than 5 years old, is 65 years or over, has income less than $3,000 (in 1959 dollars), the percentage of land in the county that is farmland, and log of the county population.

III. ADDITIONAL ROBUSTNESS

In addition to the robustness analyses discussed in the text, we have explored the sensitivity of our findings to other specifications. We examined whether the findings were robust to excluding observations with missing values on any outcome variable. We also estimated models where the dependent variable was the share of the cell missing as an outcome variables. There was no relationship between Food Stamp rollout and the incidence of missing values. We estimated models with different weighting procedures, including counties that could not be easily linked to GNIS FIPS codes, and using different birth-years in our sample. In accordance with Census guidelines to minimize implicit samples and disclosure burden, we have not disclosed these results from the RDC.

IV. LIFE EXPECTANCY ESTIMATES

In our main estimates, we use the social security NUMINDENT file to estimate the probability of surviving to 2012. For our cost-benefit analysis, it is valuable to extend this survival analysis to calculate measures of life expectancy. Here we describe that process, following methods in Chetty et al. (2016).

We estimate life expectancy conditional on reaching age 40 by first using Gompertz functions to estimate mortality rates by age for different subgroups of the population. We then sum over these mortality rates to arrive at group-specific life expectancy estimates. The steps below cover this process in more detail.

1. We first create a "group" variable (sex×birth-year×county-of-birth) and calculate raw mortality rates for each age by dividing the number of individuals in each group×age cell by the number of deaths at that age during our sample window (Decennial Census and ACS

yields a 2000-2013 sample window).

2. We then estimate a Gompertz function, which imposes that the mortality rate m is an exponential function of age a in the following expression $m(a) = e^{\alpha+\beta a}$. We use maximum likelihood to estimate these models, allowing for different mortality gradients (α and β) by sex, county, and birth year.

   We restrict this analysis to ages 30-63, as the oldest individuals in our sample to receive Food Stamps would have been 63 in 2013 (birth cohort 1950). We then predict mortality rates for ages 40-90 within each group.

3. For mortality rates at ages over 90, we use estimates from the NCHS and the SSA. The NCHS provides estimates of mortality rates by sex×race for those ages 90-100. For ages 101-111, we use estimates (by sex) from the SSA. We use year 2000 SSA mortality estimates, and averages of the NCHS mortality rates from 2001 to 2011. We append these mortality rates onto the age 40-90 mortality rates estimated in step 2.

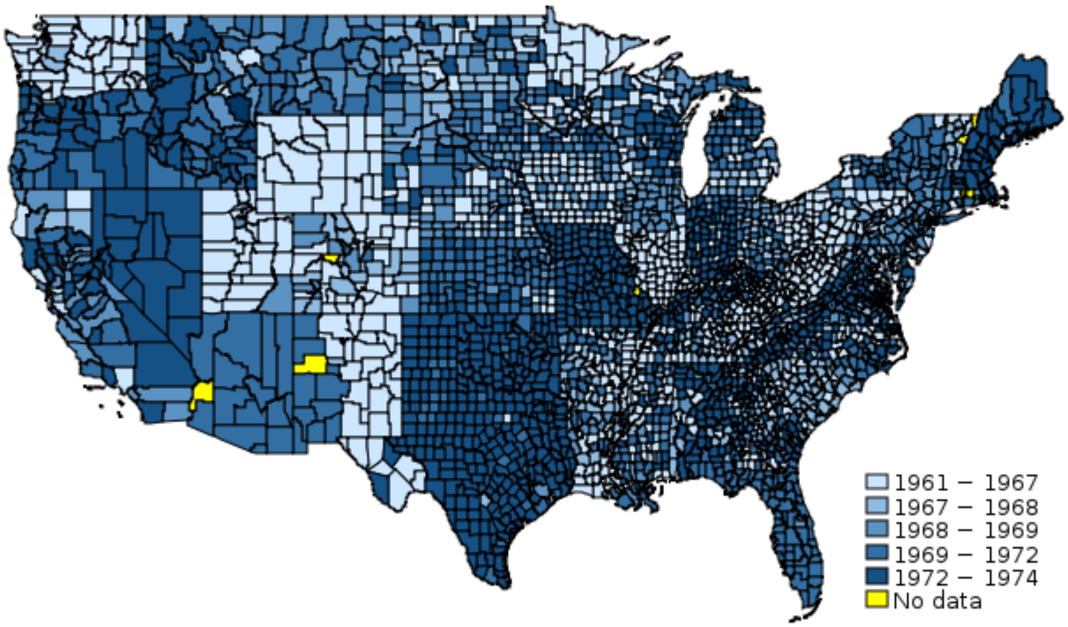
4. The Gompertz function together with the NCHS and SSA data give us mortality rates by age (m_a) for each group. We then calculate life expectancy as follows:

   a. Calculate $l_a = \prod_{a=40}^{a-1}(1 - m_a)$. This is the "survivorship" to age a.
   b. Calculate $L_a = \frac{l_a + l_{a+1}}{2}$. This is "midpoint survivorship;" the proportion of the population that makes it to the midpoint of age a.
   c. Calculate life expectancy $LE = \sum_{a=40}^{a=119}(L_a * m_a * age)$.

   We then merge these life-expectancy measures back onto the Census microdata by the group identifiers (sex×birthyear×county)

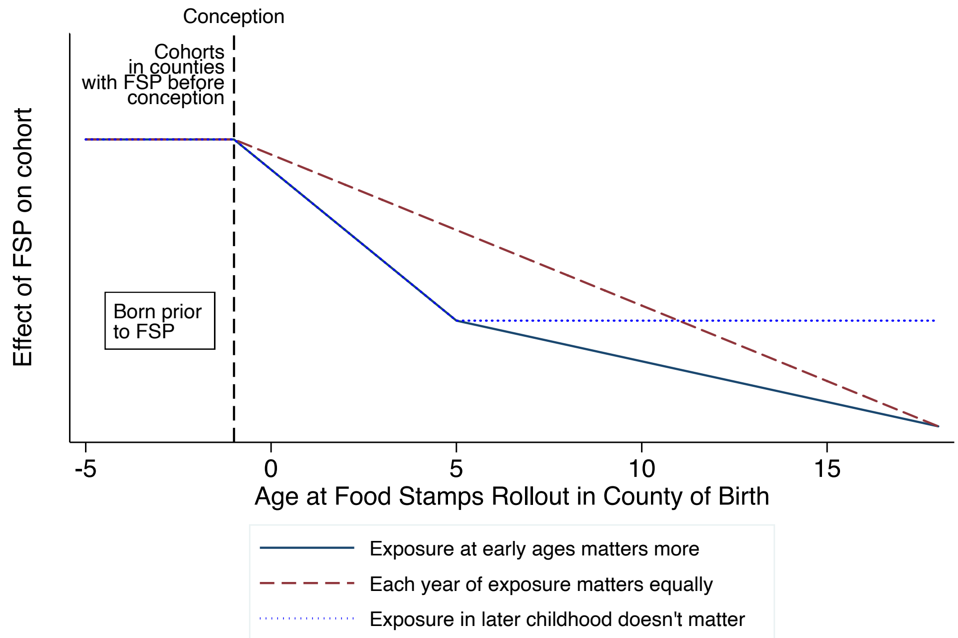
Appendix Table 3 presents results from using this measure of life expectancy as the dependent variable in our standard exposure specification from the text (model 3). Our preferred ITT estimates from Column (3) suggest that exposure to Food Stamps from conception to age 5 increases life expectancy by 0.198 years on average. The TOT analogue corresponds to an increase in life expectancy of 1.2 years (0.198/0.16).

Figure 1: The Geography of the Roll-Out of the Food Stamps Program, 1961-1975



Notes: This map depicts the year of Food Stamps implementation in each county, based on tabulations from administrative data from the U.S. Department of Agriculture in various years by Hoynes and Schanzenbach (2009). Darker blue color represents later implementation. Yellow denotes counties with no data.

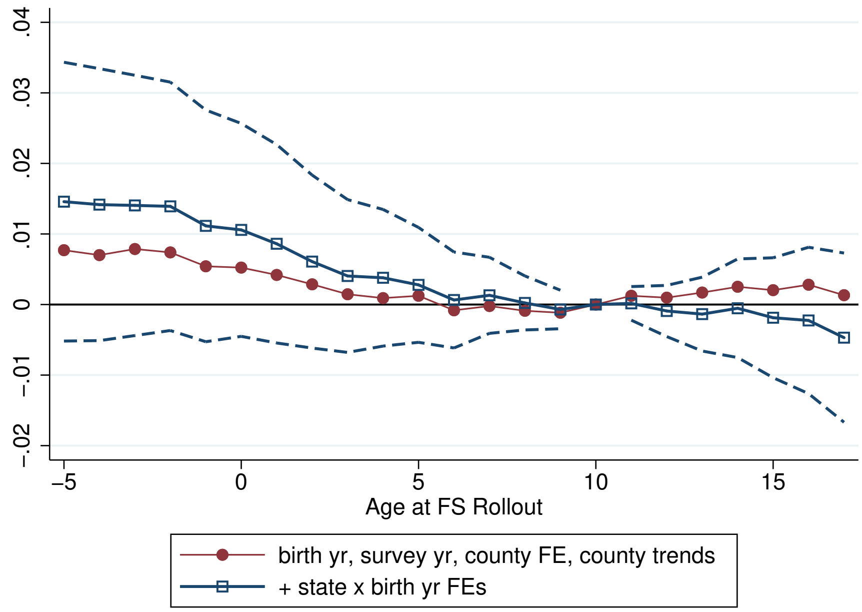
Figure 2: Hypothesized ITT Effects of Food Stamps on Adult Outcomes, by Age of Cohort when the Program Began



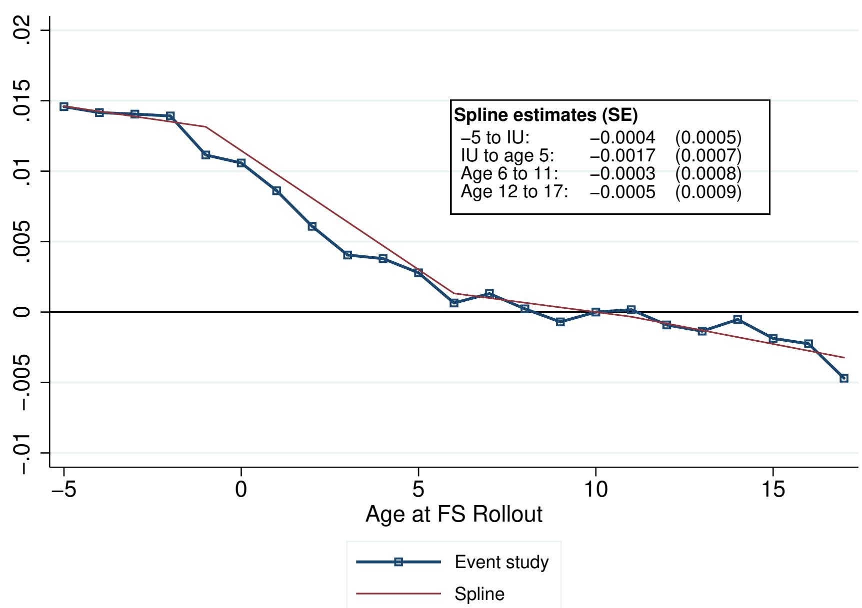
Notes: This figure illustrates the hypothetical effects of access to Food Stamps by a cohort's age at the time when the program started. The three series show different hypothetical effects. The red dashed line shows how the estimates would appear visually if the effects on adult outcomes were the same for each year of additional exposure to the Food Stamps program during all of childhood, i.e., a single line segment from age 0 (in utero) to age 18. The solid blue line shows the case with non-linear effects, such that exposure earlier in childhood (before age 5) matters more than exposure in later childhood. This results in there being a steeper slope from age 0 to 5, followed by a flatter slope at older ages. The blue dotted line shows the case in which exposure after age 6 does not affect outcomes at all. For all three cases, we show a flat "pre-trend"—that is, for cohorts conceived after Food Stamps is in effect, who are represented by ages -5 to -1 in the graph, there is no differential impact on adult well-being. See text for more discussion of the "pre-trend," and whether it makes sense to assume it should be flat.

Figure 3: ITT Event-Study and Spline Estimates of Effects of Food Stamps, by Age of Cohort when the Program Launched

Panel A. Composite Index, Event-Study Estimates



Panel B. Composite Index, Four-Part Spline



Notes: The two panels plot event-study and linear spline estimates (equations 1 and 2, respectively) using the composite standardized index as the outcome. The sample includes more than 17 million U.S. individuals born in the U.S. between 1950 and 1980 who are observed in the 2000 Census 1-in-6 sample and 2001 to 2013 ACS merged to the SSA's NUMIDENT file using PIKs. The regressions are estimated on data collapsed into birth-county×birth-year×survey-year cells, and weighted using the number of observations per cell. Standard errors clustered at the birth-county level. All regressions include birth-year, survey-year, and birth-county fixed effects, as well as 1960 county characteristics interacted with a linear trend. Panel A presents two sets of estimates, with and without birth-state×birth-year fixed effects, in blue squares and red circles, respectively. The blue dashed lines show the 95% confidence intervals from the model that includes birth-state×birth-year fixed effects. In Panel B, we overlay the event-study estimates (from our preferred specification with birth-state×birth-year fixed effects) with the linear spline estimates depicted using the solid red line. The estimates of the slopes of each spline segment are presented in

Figure 4: ITT Event-Study and Spline Estimates of Effects of Food Stamps, by Age of Cohort when the Program Launched

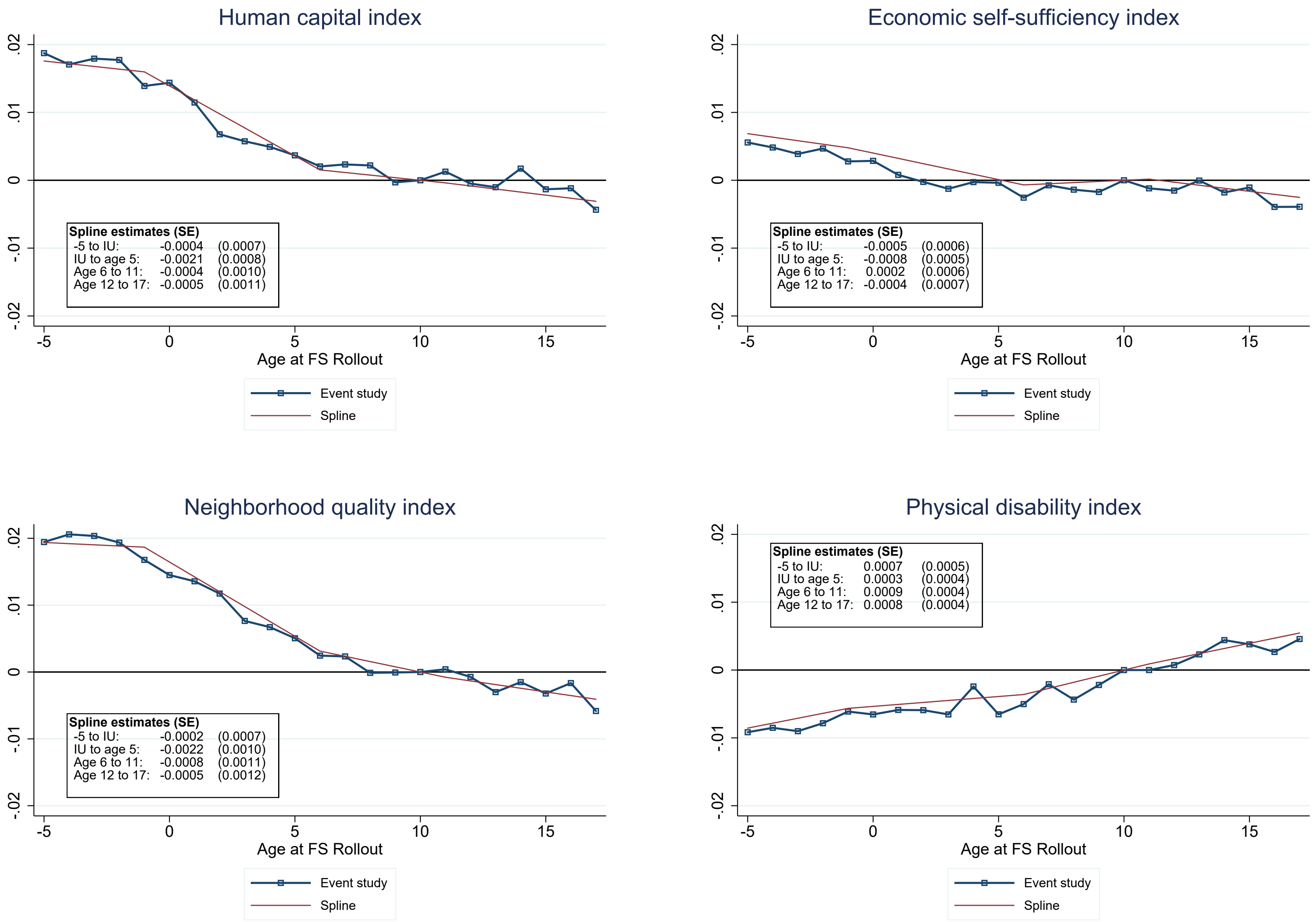
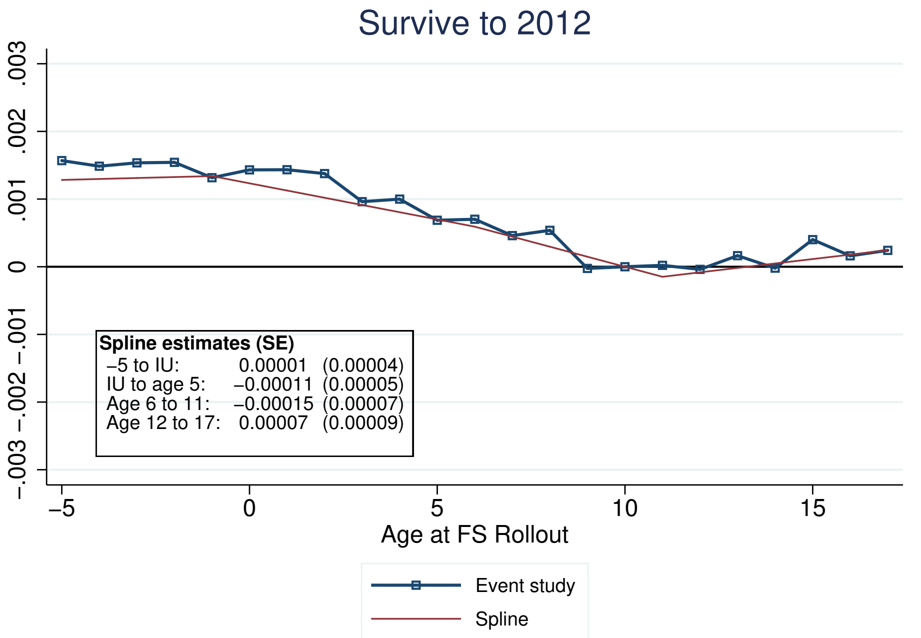
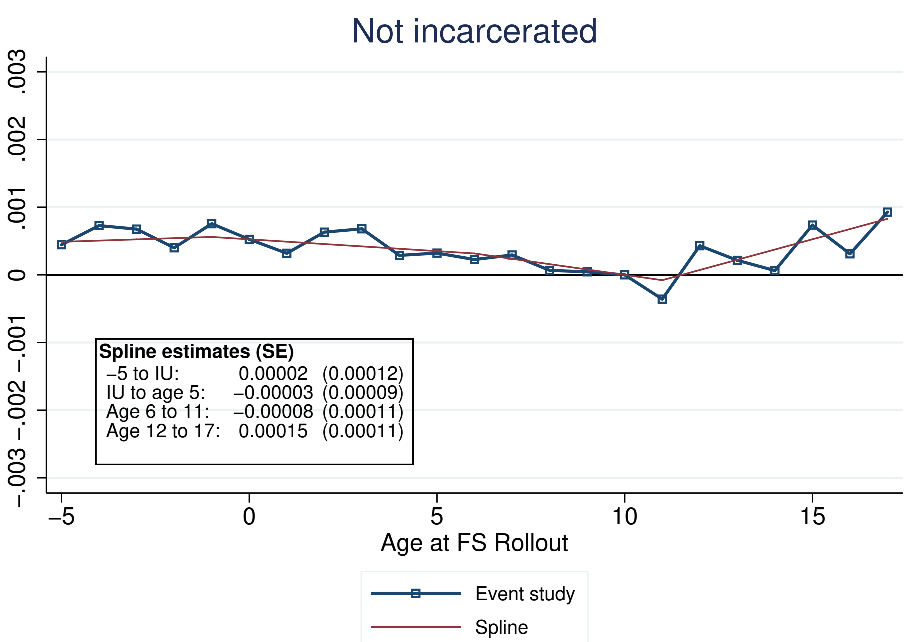


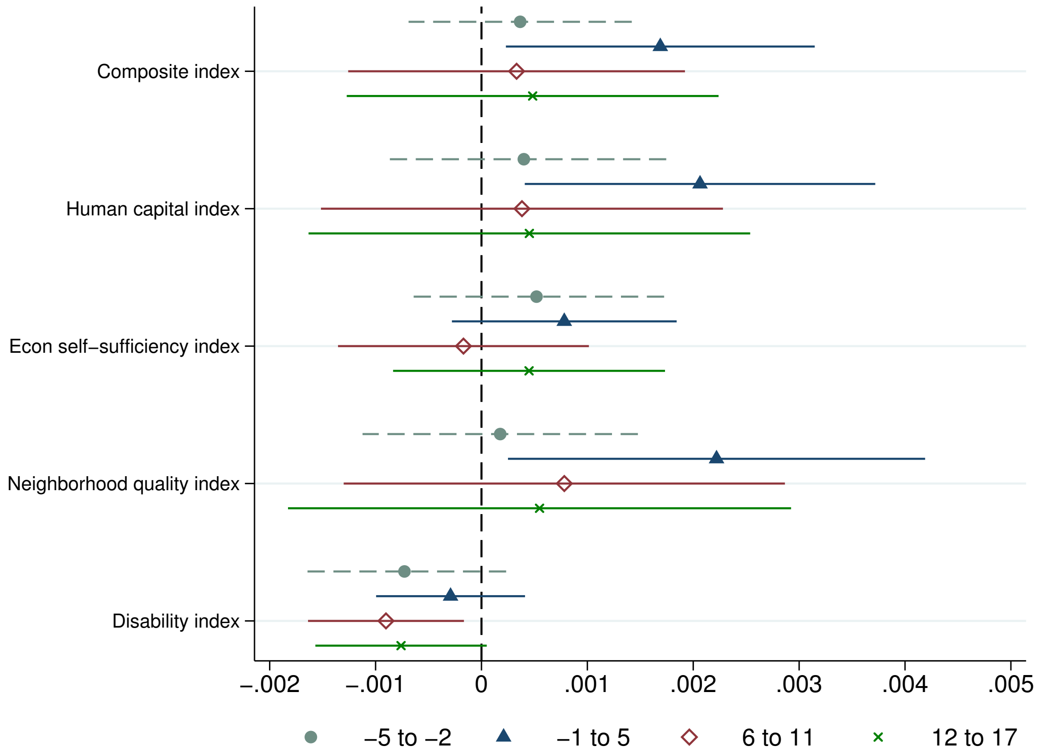
Figure 4: (continued)

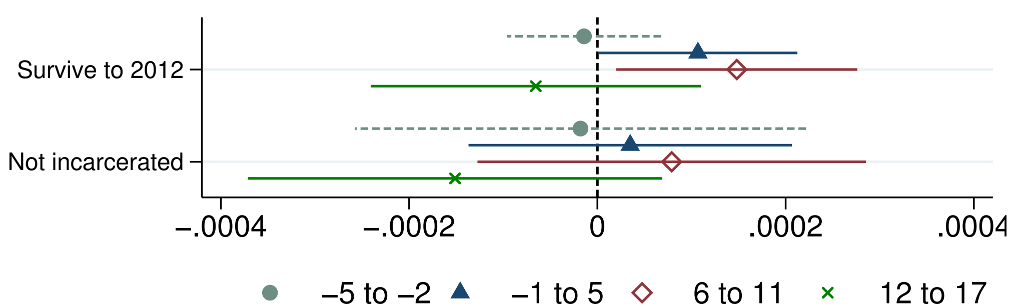




Notes: These graphs plot event-study and spline estimates using the specifications in equations (1) and (2), respectively, for each of our six main outcomes. The event-study estimates are shown using blue squares, while the spline estimates are shown using red lines. The sample includes more than 17 million U.S. individuals born in the U.S. between 1950 and 1980 who are observed in the 2000 Census 1-in-6 sample and 2001 to 2013 ACS merged to the SSA's NUMIDENT file using PIKs. The regressions are estimated on data collapsed into birth-county×birth-year×survey-year cells, and weighted using the number of observations per cell. Standard errors clustered at the birth-county level. See text for more information on the construction of the sample and the outcomes. Note that the indices are standardized in terms of standard deviations, but "Survive to 2012" and "Not incarcerated" are not. All models include fixed effects for birth-county, birth-state×birth-year, and survey year, as well as 1960 county characteristics interacted with a linear trend in year of birth.

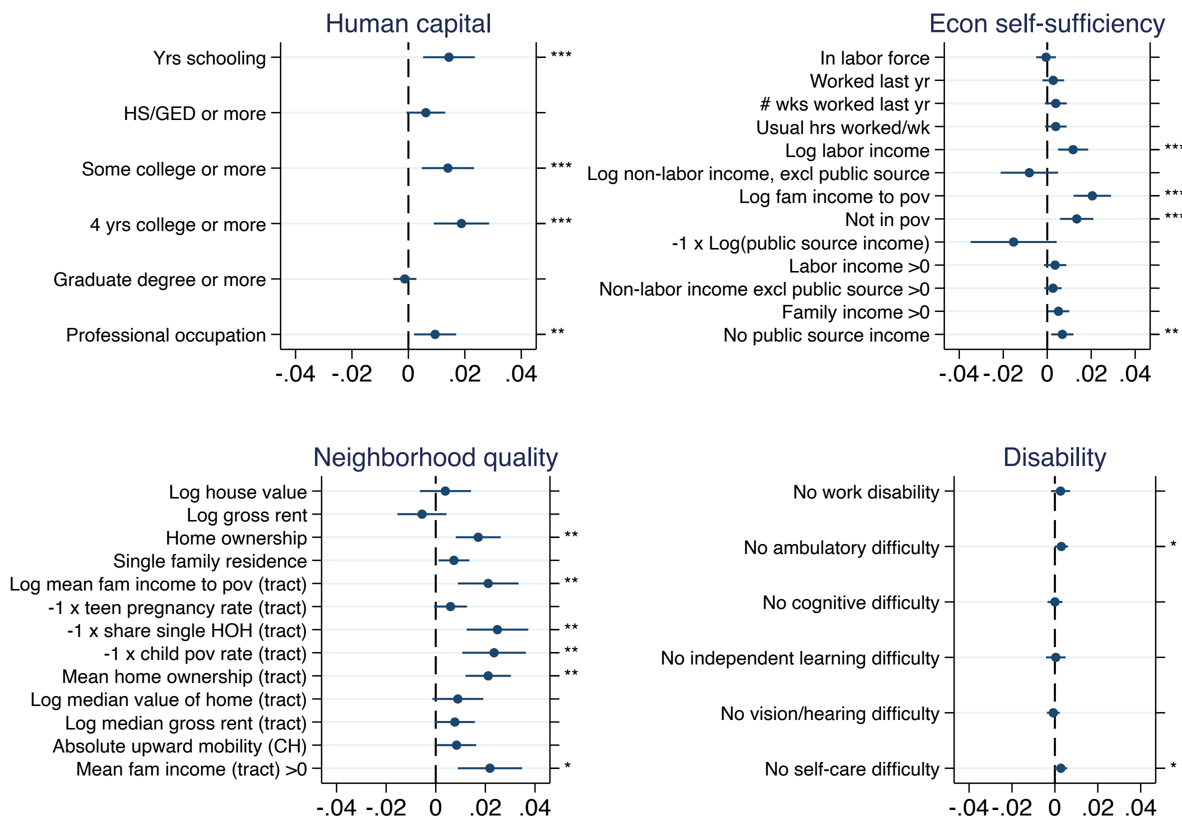
Figure 5: ITT Spline Estimates of Effects of Food Stamps, by Age of Cohort when the Program Launched





Notes: These figures plot the absolute values of the estimates (and their associated 95% confidence intervals) on the four linear spline segments in equation (2) for each outcome listed along the y-axes. In particular, we plot $|\omega_1|$ (ages -5 to -2), $|\omega_2|$ (ages -1 to 5), $|\omega_3|$ (ages 6 to 11), and $|\omega_4|$ (ages 12 to 17), where age is when Food Stamps launched in their county of birth. The sample includes more than 17 million U.S. individuals born in the U.S. between 1950 and 1980 who are observed in the 2000 Census 1-in-6 sample and 2001 to 2013 ACS merged to the SSA's NUMIDENT file using PIKs. The regressions are estimated on data collapsed into birth-county$\times$birth-year$\times$survey-year cells, and weighted using the number of observations per cell. Standard errors clustered at the birth-county level. See text for more information on the construction of the sample and the outcomes. Note that the indices are standardized in terms of standard deviations, but "Survive to 2012" and "Not incarcerated" are not, which is why these latter two outcomes appear on different scales. All models include fixed effects for birth-county, birth-state$\times$birth-year, and survey year, as well as 1960 county characteristics interacted with a linear trend in year of birth.

Figure 6: Exposure Model Estimates of the ITT Effects of Food Stamps for Standardized Sub-Index Components



Notes: These graphs provide coefficient estimates and 95% confidence intervals for different individual outcomes that are included as components of our four indices, based on the exposure model in equation (3). The outcomes are standardized in terms of standard deviation units to facilitate comparisons across them. The reported coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort is exposed to Food Stamps based on when the program began in the cohort's county of birth. While the 95% confidence intervals are based on models with standard errors clustered by the county of birth, we use star symbols to denote statistical significance based on the Romano-Wolf p-value adjustment for multiple hypothesis testing. The sample includes more than 17 million U.S. individuals born in the U.S. between 1950 and 1980 who are observed in the 2000 Census 1-in-6 sample and 2001 to 2013 ACS merged to the SSA's NUMIDENT file using PIKs. The regressions are estimated on data collapsed into birth-county×birth-year×birth-month×survey-year cells, and weighted using the number of observations per cell. See text for more information on the construction of the sample and the outcomes. All models include fixed effects for birth-county, birth-month, birth-state×birth-year, and survey year, as well as 1960 county characteristics interacted with a linear trend in year of birth. Significance levels: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$.

Table 1: Balance Test: Correlations Between Food Stamps Exposure, County Characteristics, and Other Programs

		Number of cells (1000s)	MDV	Range of years covered in the data
Other War on Poverty				
WIC	-0.0758	348	0.389	1970-1980
	(0.0526)			
Head Start	0.0196	722	0.500	1959-1980
	(0.0208)			
Community Health Center	-0.0254	722	0.063	1959-1980
	(0.0293)			
REIS Income Transfers Per Capita ($1000s)				
Real total transfers	0.0282	382	2.266	1969-1980
	(0.0286)			
Retirement and DI benefits	-0.2089	725	1.003	1959-1980
	(0.0598)			
Medicare and military health care	-0.0280	725	0.177	1959-1980
	(0.0052)			
Income maintenance (exc Food Stamps)	-0.0525	725	0.242	1959-1980
	(0.0190)			
Other County				
Real personal income	-0.0710	382	19.960	1969-1980
	(0.1967)			
Log population	0.0499	722	12.340	1959-1980
	(0.0084)			
Log employment	-0.0006	382	11.710	1969-1980
	(0.0156)			
Mortality				
Adult mortality rate	-1.2420	722	866.700	1959-1980
	(3.1300)			
Infant mortality rate	0.0154	711	20.110	1959-1980
	(0.1805)			
Neonatal mortality rate	0.0902	711	14.620	1959-1980
	(0.1450)			
Post-neonatal mortality rate	-0.0748	711	5.495	1959-1980
	(0.0991)			
FE for county, state x birth year	X			
Cty_{60} x linear cohort	X			

Notes: Each row provides estimates from the exposure model in equation (3). The unit of analysis is a county×year×month, and the coefficient is on the exposure variable: the share of time between conception and age 5 that Food Stamps is in place (for someone born in this county-year-month cell). We test for whether exposure predicts a given county time-varying characteristic, including the existence of other War on Poverty Programs, per-capita transfers from REIS data, mortality rates, population, personal income, and employment. Regressions are weighted using the population in each cell, and include county, month, and state×year fixed effects, as well as 1960 county characteristics interacted with a linear trend in year. Some outcome variables are not available in all years, which is why we have different numbers of observations across outcomes. The years for which each outcome is available are listed in the final column. The column titled "MDV" reports the mean of the dependent variable. See text and the Online Appendix for more information on data, samples, and sources.

Table 2: Estimated ITT Effects of Food Stamps Exposure between Conception and Age 5 on a Composite Index of Well-Being

	(1)	(2)	(3)
%IU - Age 5	0.0042	0.0075	0.0087
	(0.0025)	(0.0027)	(0.0025)
FE county, birth year, survey year	X	X	X
Cty_{60} x linear cohort		X	X
State X birth year FE			X
Number of observations	17,400,000	17,400,000	17,400,000
Number of cells	4,272,000	4,272,000	4,272,000
Number of counties	3000	3000	3000
R^2	0.229	0.231	0.232

Notes: Each column provides estimates from the exposure model in equation (3), using as the outcome the composite index of adult well-being. The data are collapsed into cells at the birth-county×birth-year×birth-month×survey-year level, and the reported coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort is exposed to Food Stamps based on when the program began in the cohort's county of birth. All columns include fixed effects for birth-county, birth-year, birth-month, and survey year. Column 2 adds 1960 county characteristics interacted with a linear trend in year of birth. Column 3 adds birth-state×birth-year fixed effects Standard errors are clustered by county of birth and indicated in parentheses. The number of observations, number of cells, and number of counties are rounded to the nearest 1,000 for disclosure purposes. See also Figure 3 notes for more information on the sample and outcome.

Table 3: Estimated ITT Effects of Food Stamps Exposure between Conception and Age 5 on Well-Being Indices, Survival, and Non-Incarceration

	Indices					
	Human capital	Economic self-sufficiency	Neighborhood quality	Physical disability	Survive to 2012	Not incarcerated
%IU - Age 5	0.0103	0.0043	0.0115	0.0013	0.0007	0.0008
	(0.0035)	(0.0016)	(0.0036)	(0.0013)	(0.0003)	(0.0004)
FE county, survey year	X	X	X	X	X	X
Cty_{60} x linear cohort	X	X	X	X	X	X
State x birth year FE	X	X	X	X	X	X
Number of observations	17,400,000	17,400,000	17,400,000	16,800,000	114,000,000	7,705,000
Number of cells	4,272,000	4,272,000	4,272,000	2,796,000	943,000	2,591,000
Number of counties	3000	3000	3000	3100	3000	3000
R^2	0.127	0.058	0.379	0.053	0.696	0.027

Notes: Each column provides estimates from the exposure model in equation (3). The data are collapsed into cells at the birth-county×birth-year×birth-month×survey-year level, and the reported coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort is exposed to Food Stamps based on when the program began in the cohort's county of birth. All columns include fixed effects for birth-county, birth-month, survey year, and birth-state×birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. Standard errors are clustered by county of birth and reported in parentheses. See text for more information on indices and outcomes. The indices are standardized in terms of standard deviations, but "Survive to 2012" and "Not incarcerated" are not. The number of observations, number of cells and number of counties are rounded to the nearest 1,000 for disclosure purposes. See also notes for Figures 4 and 5 for more information on the sample and outcomes.

Table 4: Exposure Model Estimates of the ITT Effects of Food Stamps for Sub-Index Components, Whole Sample and by Race and Sex

Title	All	White males	White females	Nonwhite males	Nonwhite females
Human capital					
Yrs schooling	0.0367***	0.0374***	0.0336***	-0.0028	-0.0045
	(0.0119)	(0.0126)	(0.0105)	(0.0251)	(0.0239)
HS/GED or more	0.0016	0.0007	0.0005	0.0003	-0.0013
	(0.0009)	(0.0009)	(0.0009)	(0.0032)	(0.0025)
Some college or more	0.0067***	0.0070***	0.0058**	0.0010	0.0006
	(0.0023)	(0.0025)	(0.0021)	(0.0049)	(0.0048)
4 yrs college or more	0.0088***	0.0091***	0.0081***	0.0030	**0.0020**
	(0.0023)	(0.0026)	(0.0021)	(0.0043)	(0.0037)
Graduate degree or more	-0.0002	0.0001	-0.0006	0.0013	-0.0009
	(0.0004)	(0.0006)	(0.0004)	(0.0015)	(0.0013)
Professional occupation	0.0046**	0.0047**	0.0027	0.0040	0.0007
	(0.0018)	(0.0022)	(0.0019)	(0.0039)	(0.0040)
Economic self sufficiency					
In labor force	-0.0002	0.0014	**-0.0049****	0.0026	0.0044
	(0.0009)	(0.0008)	(0.0019)	(0.0025)	(0.0036)
Worked last yr	0.0010	0.0005	**-0.0034**	**0.0053**	0.0038
	(0.0009)	(0.0008)	(0.0018)	(0.0028)	(0.0034)
# wks worked last yr	0.0754	0.0545	**-0.2111**	**0.4391****	0.2036
	(0.0493)	(0.0503)	(0.0981)	(0.1581)	(0.1742)
Usual hrs worked/wk	0.0704	0.1250*	**-0.1995**	0.2231	0.0367
	(0.0452)	(0.0527)	(0.0931)	(0.1491)	(0.1562)
Log labor income	0.0114***	0.0139***	**0.0030**	**-0.0013**	0.0095
	(0.0034)	(0.0035)	(0.0041)	(0.0083)	(0.0096)
Log non-labor income, excl public source	-0.0176	-0.0088	-0.0119	0.0415	-0.0352
	(0.0145)	(0.0158)	(0.0194)	(0.0588)	(0.0303)
Log fam income to pov	0.0182***	0.0126***	0.0110**	0.0019	0.0086
	(0.0039)	(0.0027)	(0.0035)	(0.0084)	(0.0083)
Not in pov	0.0038***	0.0006	0.0012	0.0040	0.0013
	(0.0011)	(0.0009)	(0.0010)	(0.0029)	(0.0032)
-1 $\times$ Log(public source income)	-0.0138	-0.0062	-0.0076	0.0026	0.0021
	(0.0090)	(0.0146)	(0.0151)	(0.0269)	(0.0221)
Labor income > 0	0.0013	0.0005	**-0.0031**	**0.0058**	0.0037
	(0.0009)	(0.0008)	(0.0019)	(0.0029)	(0.0035)
Family income > 0	0.0008*	0.0000	0.0004	**0.0026**	-0.0008
	(0.0004)	(0.0005)	(0.0005)	(0.0015)	(0.0016)

Table 4: (continued)

Title	All	White males	White females	Nonwhite males	Nonwhite females
<u>Neighborhood quality</u>					
Log house value	0.0034	0.0011	0.0035	0.0015	-0.0098
	(0.0047)	(0.0048)	(0.0040)	(0.0099)	(0.0085)
Log gross rent	-0.0030	-0.0034	0.0012	0.0007	-0.0050
	(0.0028)	(0.0038)	(0.0035)	(0.0062)	(0.0069)
Home ownership	0.0059**	0.0022	0.0040**	-0.0015	0.0005
	(0.0016)	(0.0014)	(0.0015)	(0.0043)	(0.0035)
Single family residence	0.0023	0.0009	0.0031**	0.0063	-0.0037
	(0.0010)	(0.0012)	(0.0012)	(0.0034)	(0.0028)
Log mean fam income to pov (tract)	0.0084**	0.0036	**0.0065*** **	0.0013	0.0020
	(0.0025)	(0.0018)	(0.0019)	(0.0044)	(0.0044)
-1 × teen pregnancy rate (tract)	0.0003	0.0001	0.0001	0.0004	-0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0006)	(0.0005)
-1 × share single HOH (tract)	0.0029**	0.0011*	**0.0020*** **	0.0003	-0.0003
	(0.0007)	(0.0004)	(0.0006)	(0.0012)	(0.0010)
-1 × child pov rate (tract)	0.0032**	0.0012	0.0017**	0.0008	0.0015
	(0.0009)	(0.0006)	(0.0006)	(0.0016)	(0.0015)
Mean home ownership (tract)	0.0037**	0.0015	**0.0028** **	0.0000	0.0008
	(0.0008)	(0.0007)	(0.0009)	(0.0021)	(0.0016)
Log median value of home (tract)	0.0052	0.0014	0.0046	-0.0014	-0.0028
	(0.0031)	(0.0026)	(0.0022)	(0.0046)	(0.0050)
Log median gross rent (tract)	0.0028	0.0024	0.0025	-0.0001	-0.0008
	(0.0015)	(0.0015)	(0.0016)	(0.0030)	(0.0034)
Absolute upward mobility (CH)	0.0354	0.0279	0.0160	-0.0055	**-0.0906*** **
	(0.0169)	(0.0153)	(0.0159)	(0.0298)	(0.0248)
Mean fam income (tract) > 0	0.0008*	0.0003	0.0003	0.0006	0.0004
	(0.0002)	(0.0002)	(0.0001)	(0.0004)	(0.0004)
FE county, survey year	X	X	X	X	X
$Cty_{60}\times$ linear cohort	X	X	X	X	X
State × birth year FE	X	X	X	X	X
Number of observations	17,400,000	7,423,000	7,817,000	1,028,000	1,310,000
Number of cells	4,272,000	2,684,000	2,781,000	561,000	668,000
Number of counties	3,000	3,000	3,000	2,900	2,900

Notes: Each estimate is from a separate regression model shown in equation (3) ("exposure model"). The data are collapsed into cells at the birth-county×birth-year×birth-month×survey-year×race×sex level, and the reported coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort is exposed to Food Stamps based on when the program began in the cohort's county of birth. Standard errors clustered by county of birth are reported in parentheses. The outcomes are individual components of our four indices. Estimated models and samples are identical to Table 3. We use the Romano-Wolf p-value adjustment method to account for multiple hypothesis testing and use star symbols to denote statistical significance (Significance levels: * $p < 0.1$ ** $p < 0.05$ *** $p < 0.01$). Additionally, we use bold font to indicate estimates for white females, non-white males, and non-white females that are statistically significantly different from those for white males at the 10% significance level or below.

Table 5: Estimated ITT Effects of Food Stamps Exposure between Conception and Age 5 on Well-Being Indices, Survival, and Non-Incarceration, by Race and Sex

	Indices					
	Human capital	Economic self-sufficiency	Neighborhood quality	Physical disability	Survive to 2012	Not incarcerated
White males						
%IU - Age 5	0.0102	0.0037	0.0048	-0.0001	0.0006	0.0004
	(0.0036)	(0.0020)	(0.0024)	(0.0018)	(0.0004)	(0.0006)
Number of observations	7,423,000	7,423,000	7,423,000	7,077,000	44,900,000	3,264,000
Number of cells	2,684,000	2,684,000	2,684,000	1,831,000	916,000	1,586,000
Number of counties	3,000	3,000	3,000	3,000	3,000	3,000
White females						
%IU - Age 5	0.0078	-0.0002	**0.0095**	0.0001	0.0003	0.0001
	(0.0030)	(0.0027)	(0.0028)	(0.0016)	(0.0002)	(0.0002)
Number of observations	7,817,000	7,817,000	7,817,000	7,340,000	43,000,000	3,411,000
Number of cells	2,781,000	2,781,000	2,781,000	1,878,000	913,000	1,629,000
Number of counties	3,000	3,000	3,000	3,000	3,000	3,000
Nonwhite males						
%IU - Age 5	0.0044	0.0063	0.0019	**0.0083**	0.0007	-0.0001
	(0.0067)	(0.0044)	(0.0050)	(0.0036)	(0.0009)	(0.0039)
Number of observations	951,000	951,000	951,000	1,028,000	12,900,000	494,000
Number of cells	561,000	561,000	561,000	466,000	622,000	338,000
Number of counties	2,900	2,900	2,900	2,900	3,000	2,700
Nonwhite females						
%IU - Age 5	-0.0007	0.0038	**-0.0042**	-0.0035	0.0001	0.0002
	(0.0068)	(0.0049)	(0.0046)	(0.0032)	(0.0006)	(0.0011)
Number of observations	1,204,000	1,204,000	1,204,000	1,310,000	13,000,000	536,000
Number of cells	668,000	668,000	668,000	546,000	627,000	360,000
Number of counties	2,900	2,900	2,900	2,900	3,000	2,700
FE county, survey year	X	X	X	X	X	X
Cty_{60} x linear cohort	X	X	X	X	X	X
State x birth year FE	X	X	X	X	X	X

Notes: We replicate the models shown in Table 3 separately for four sub-groups defined by race and sex: white males, white females, non-white males, and non-white females. We use bold font to indicate estimates for white females, non-white males, and non-white females that are statistically significantly different from those for white males at the 10% significance level or below.

Table 6: Estimated ITT Effects of Food Stamps Exposure between Conception and Age 5 on Well-Being Indices, by Mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Share of	Human capital		Economic self-sufficiency		Neighborhood quality	
	Movers	Stayers	Movers	Stayers	Movers	Stayers	Movers
%IU - Age 5	0.0085	0.0113	0.0064	0.0062	0.0031	0.0156	0.0085
	(0.0031)	(0.0031)	(0.0021)	(0.0029)	(0.0019)	(0.0035)	(0.0031)
FE county, survey year	X	X	X	X	X	X	X
Cty_{60} x linear cohort	X	X	X	X	X	X	X
State x birth year FE	X	X	X	X	X	X	X
Number of observations	17,400,000	5,182,000	12,200,000	5,182,000	12,200,000	5,182,000	12,200,000
Number of cells	4,272,000	2,101,000	3,567,000	2,101,000	3,567,000	2,101,000	3,567,000
Number of counties	3000	2700	3000	2700	3000	2700	3000
Mean DV	0.712	-0.115	0.068	-0.0228	0.0533	-0.152	0.0679
R^2	0.18	0.283	0.181	0.0662	0.0425	0.538	0.301

Notes: We use the same sample as in Table 3 to study the effects of Food Stamps exposure on the incidence of mobility and differences in effects between stayers and movers. The data are collapsed into cells at the birth-county×birth-year×birth-month×survey-year level, and the reported coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort is exposed to Food Stamps based on when the program began in the cohort's county of birth. Standard errors clustered by county of birth are reported in parentheses. In the first column, the outcome is the share of individuals in a cell who are observed in the 2000-2013 Census/ACS to be in a different county than the one in which they were born (i.e., the share of movers). Stayers are those who are observed to be in the same county as the one in which they were born. The subsequent columns present estimates from our main exposure model (equation 3) for our four outcome indices separately for these two sub-groups.

Table 7: Robustness of the Estimated ITT Effects of Food Stamps Exposure between Ages 0 and 5 on a Composite Index of Well-Being

	(1)	(2)	(3)	(4)	(5)
%IU - Age 5	0.0087	0.0085	0.0087	0.0070	0.0074
	(0.0025)	(0.0025)	(0.0034)	(0.0033)	(0.0029)
FE county, survey year	X	X	X	X	X
Cty_{60} x linear cohort	X	X	X	X	X
State x birth year FE	X	X	X	X	X
Age, age squared		X			
County population				X	X
Other county controls					X
Number of observations	17,400,000	17,400,000	11,200,000	11,200,000	11,200,000
Number of cells	4,272,000	4,272,000	3,115,000	3,115,000	3,115,000
Number of counties	3000	3,000	3000	3000	3000
R^2	0.232	0.237	0.213	0.213	0.213

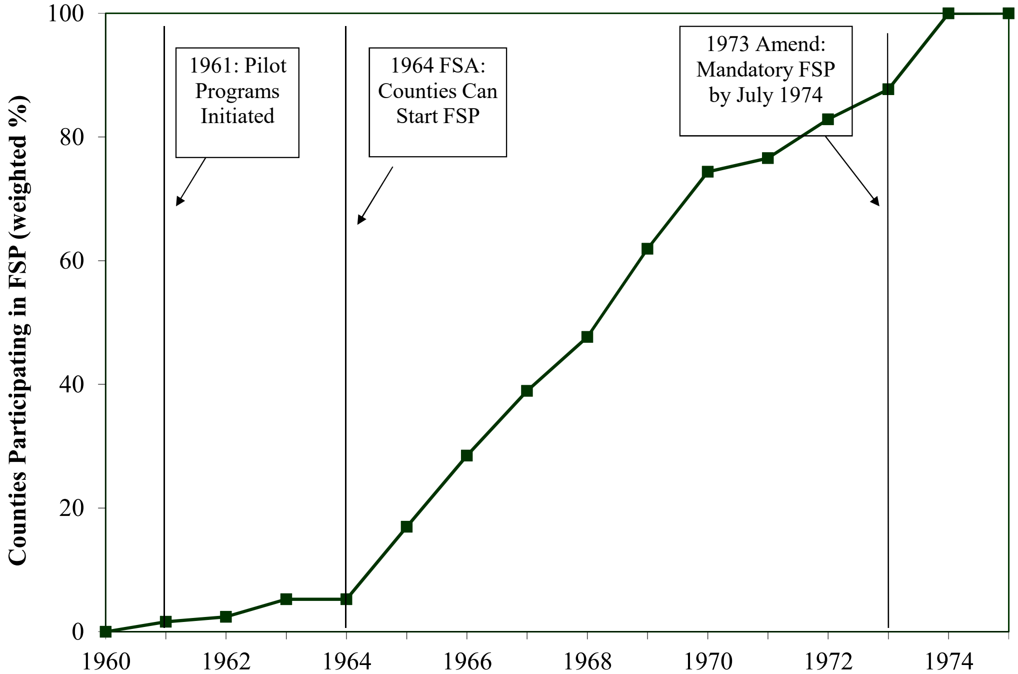
Notes: Each column provides estimates from the exposure model in equation (3) using the composite index of adult well-being as the outcome. The data are collapsed into cells at the birth-county×birth-year×birth-month×survey-year level, and the reported coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort is exposed to Food Stamps based on when the program began in the cohort's county of birth. All columns include fixed effects for birth-county, birth-month, survey year, and birth-state×birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. Standard errors are clustered by county of birth and reported in parentheses. Column 1 replicates our main estimate reported in Table 2, column 3. Column 2 adds a control for age and age squared. Column 3 replicates column 1, but only using the sample for whom we have county-level control variables. Column 4 adds a control for the natural log of county population, and column 5 adds all other county time-varying controls from Table 1 that are available for 1959–1980 (other War on Poverty programs, REIS transfer spending, and mortality).

Table 8: Estimated ITT Effects of Food Stamps Exposure in Early (Conception to Age 5) and Later Childhood (Ages 6 to 18) on Well-Being Indices, Survival, and Non-Incarceration

	Indices					
	Human capital	Economic self-sufficiency	Neighborhood quality	Physical disability	Survive to 2012	Not incarcerated
%IU - Age 5	0.0092	0.0027	0.0123	-0.0015	0.0010	0.0008
	(0.0047)	(0.0023)	(0.0052)	(0.0016)	(0.0003)	(0.0006)
%Ages 6-18	-0.0033	-0.0049	0.0025	-0.0081	0.0012	0.0002
	(0.0112)	(0.0053)	(0.0122)	(0.0031)	(0.0008)	(0.0014)
FE county, survey year	X	X	X	X	X	X
Cty_{60} x linear cohort	X	X	X	X	X	X
State x birth year FE	X	X	X	X	X	X
Number of observations	17,400,000	17,400,000	17,400,000	16,800,000	114,000,000	7,705,000
Number of cells	4,272,000	4,272,000	4,272,000	2,796,000	943,000	2,591,000
Number of counties	3000	3000	3000	3100	3000	3000
R^2	0.127	0.058	0.379	0.053	0.696	0.027

Notes: Each column provides estimates from an augmented version of the exposure model (equation 3) that includes two exposure variables—(i) the share of months of Food Stamps exposure between conception and age 5 and (ii) the share of months of Food Stamps exposure between ages 6 and 18. Otherwise, the outcomes, sample, and models are identical to those shown in Table 3.

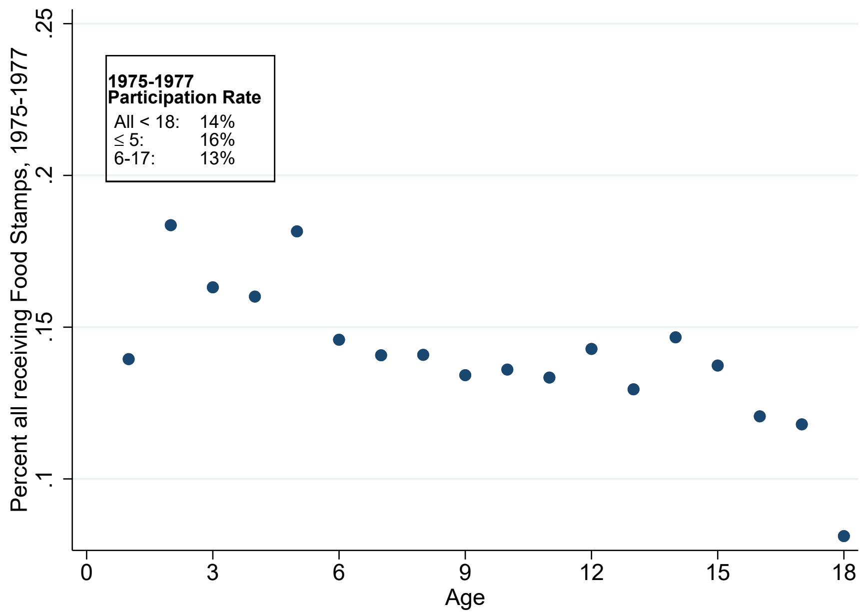
Appendix Figure 1: Population Weighted Share of Counties With a Food Stamps Program, by Year



Notes: This graph shows the population weighted share of counties that had a Food Stamps program by year, based on tabulations from administrative data from the U.S. Department of Agriculture from various years by Hoynes and Schanzenbach (2009).

Appendix Figure 2: Childhood Use of Food Stamps in the Panel Study of Income Dynamics

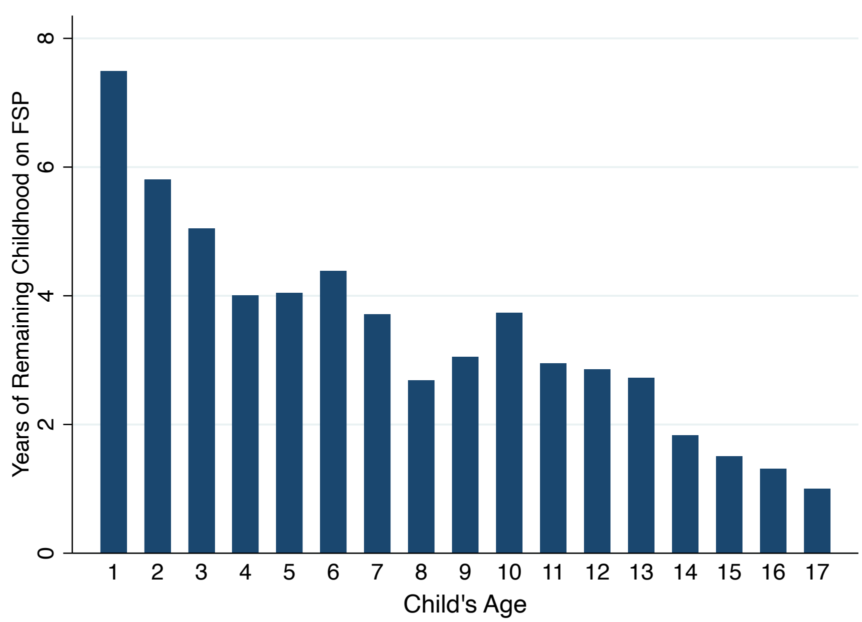
(A) Child Participation Rates in Food Stamps, by Age



Source: Panel Study of Income Dynamics, pooling data from years 1975 to 1977.

Note: We use this period because 1975, 1976, and 1977 are the first three years in which Food Stamps were universally available.

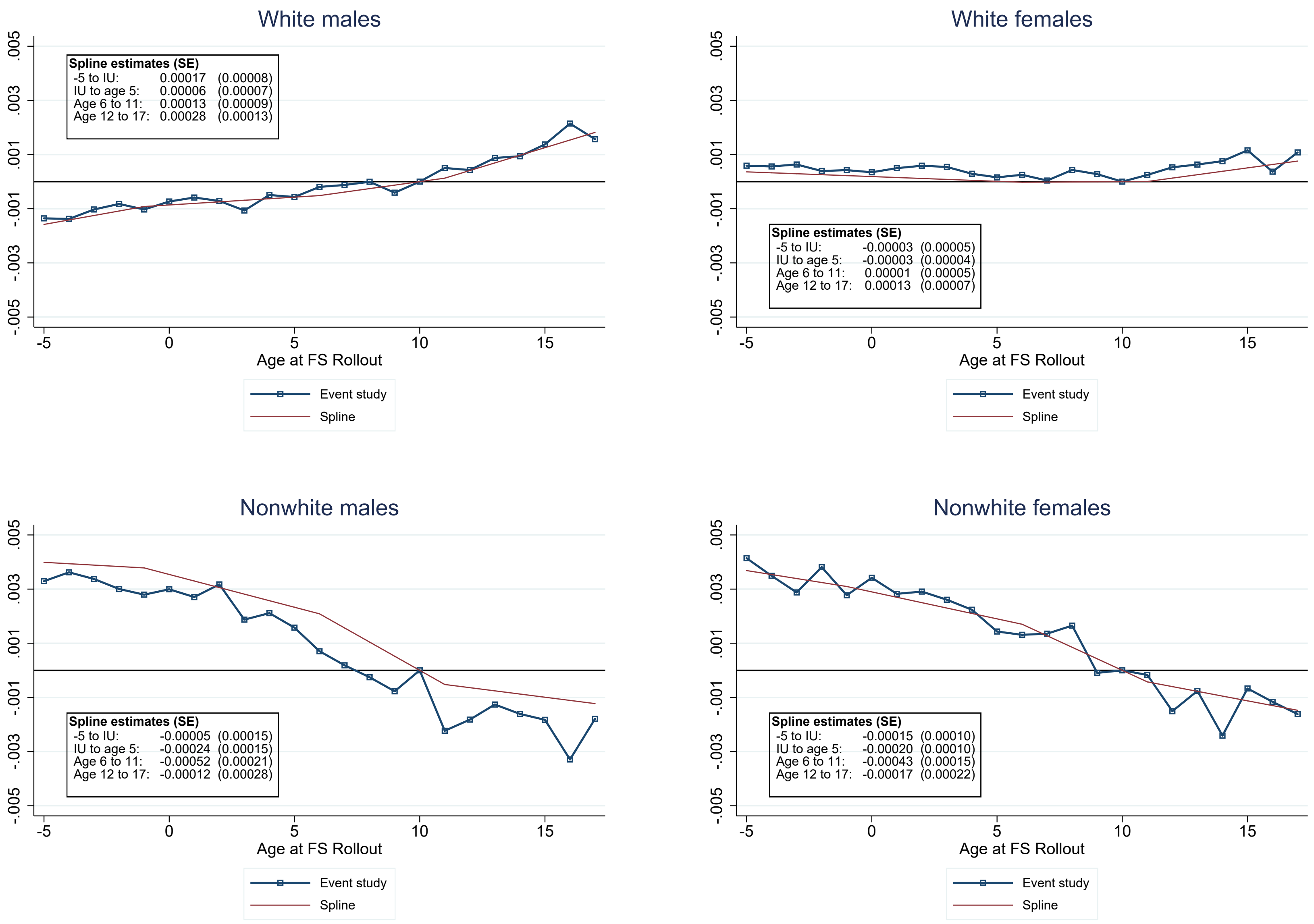
(B) Number of Years of Food Stamps Receipt, by Age at First Use



Source: Panel Study of Income Dynamics, 1972-1999

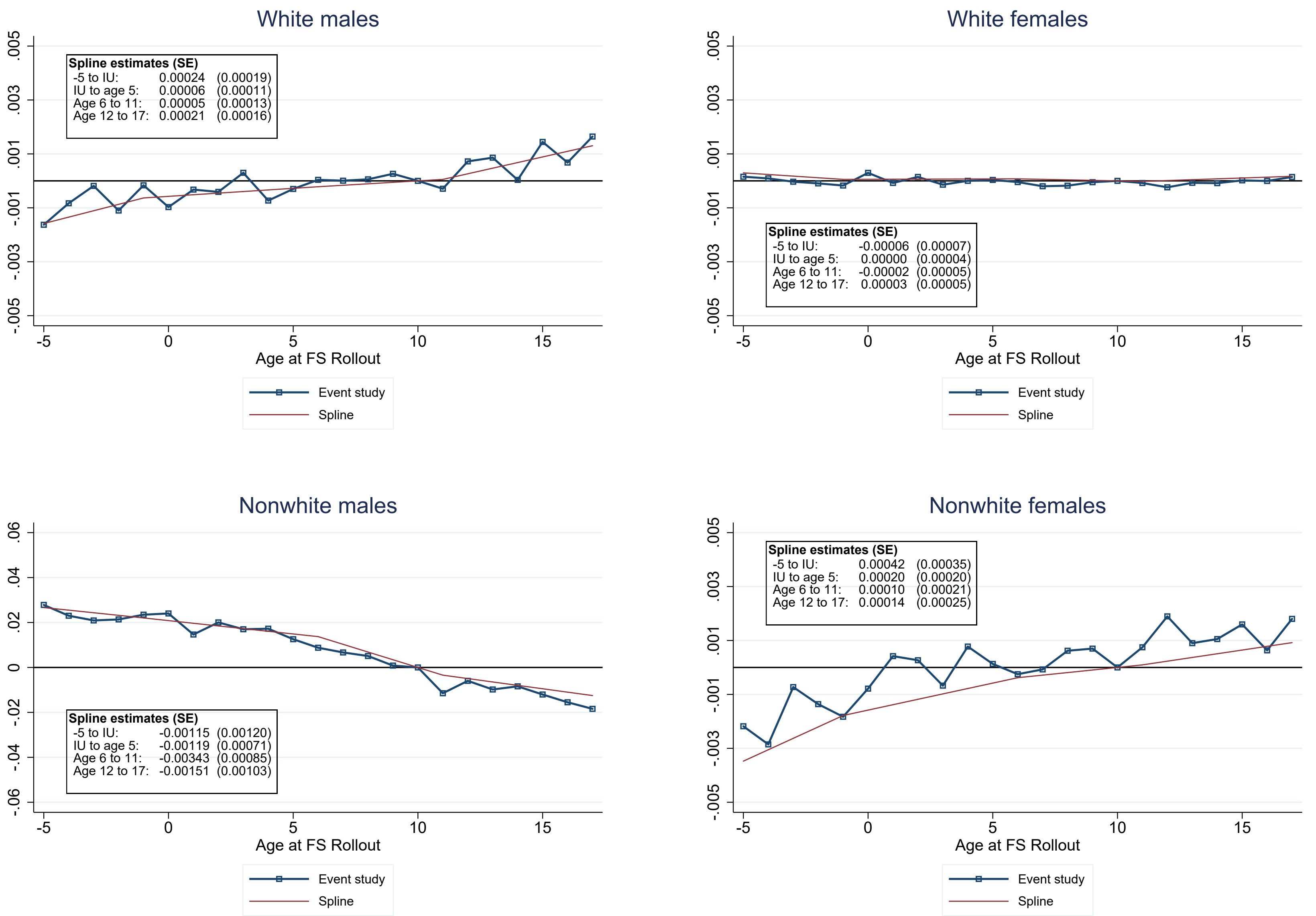
Note: We limit our sample to children who were first observed being on the Food Stamps Program between 1972 and 1975. We start this period in 1972 because the PSID begins in 1968, and we wanted to assure that there was no participation in the prior four years. We end this period in 1975 to target the rollout period. The results are similar when we change the sample to include children who were first observed on the Food Stamp Program between 1972 and 1981, although the results are slightly less noisy due to the larger sample size.

Appendix Figure 3: Event-Study and Spline Estimates of the ITT Effects of Food Stamps Exposure on Survival to 2012 by a Cohort's Age when the Program Launched, by Race and Sex



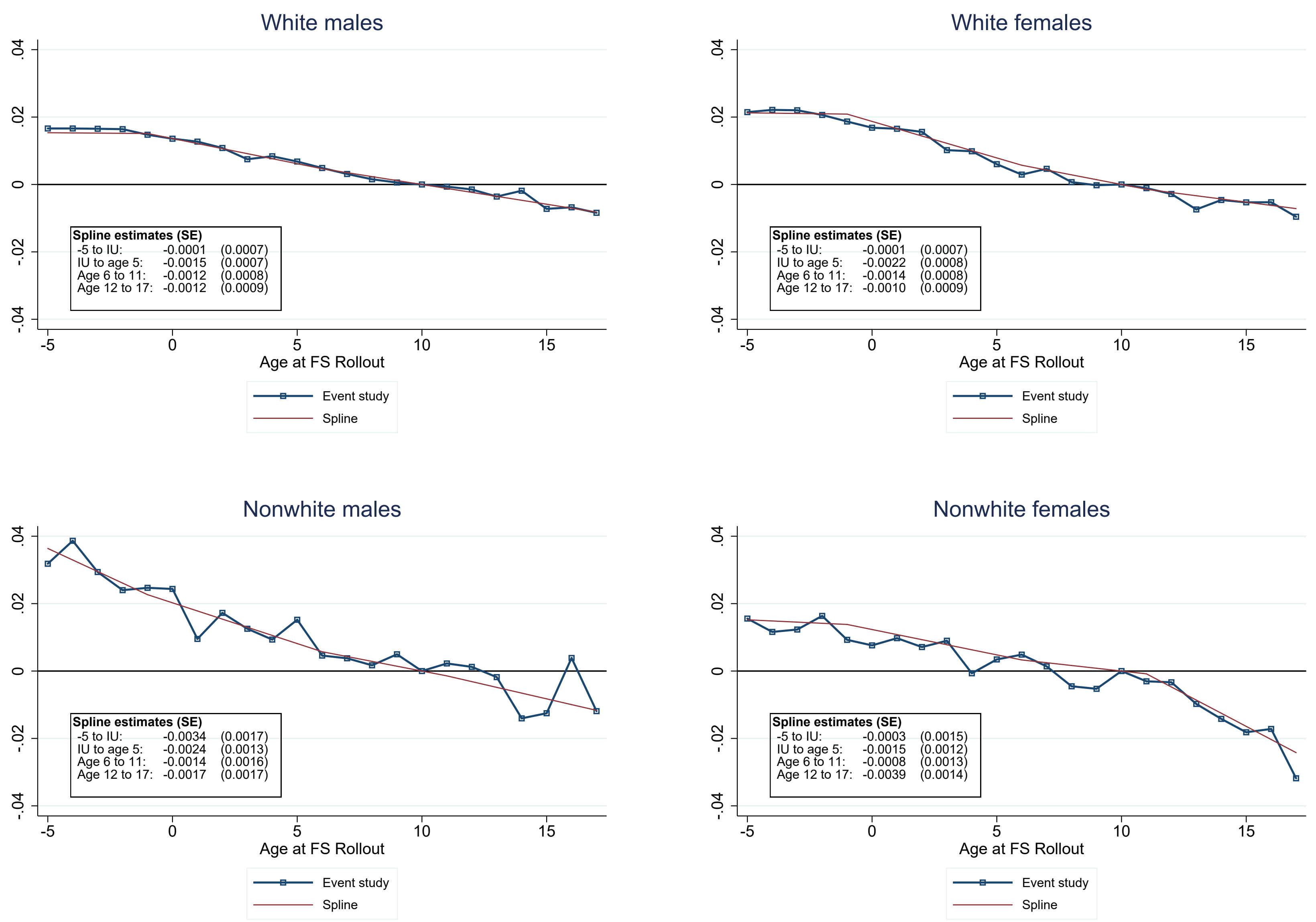
Notes: The panels plot event-study and spline estimates for survival to 2012 using the specifications in equations (1) and (2) separately by race and sex. All models include fixed effects for birth-county, survey year, and birth-state×birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. Survival to 2012 is expressed in percentage point units.The survival estimates are based on the 114 million U.S. individuals born in the U.S. between 1950 and 1980 for whom we observe place of birth. See Figure 5 for more on sample, specification and data.

Appendix Figure 4: Event-Study and Spline Estimates of the ITT Effects of Food Stamps Exposure on Non-Incarceration by a Cohort's Age when the Program Launched, by Race and Sex



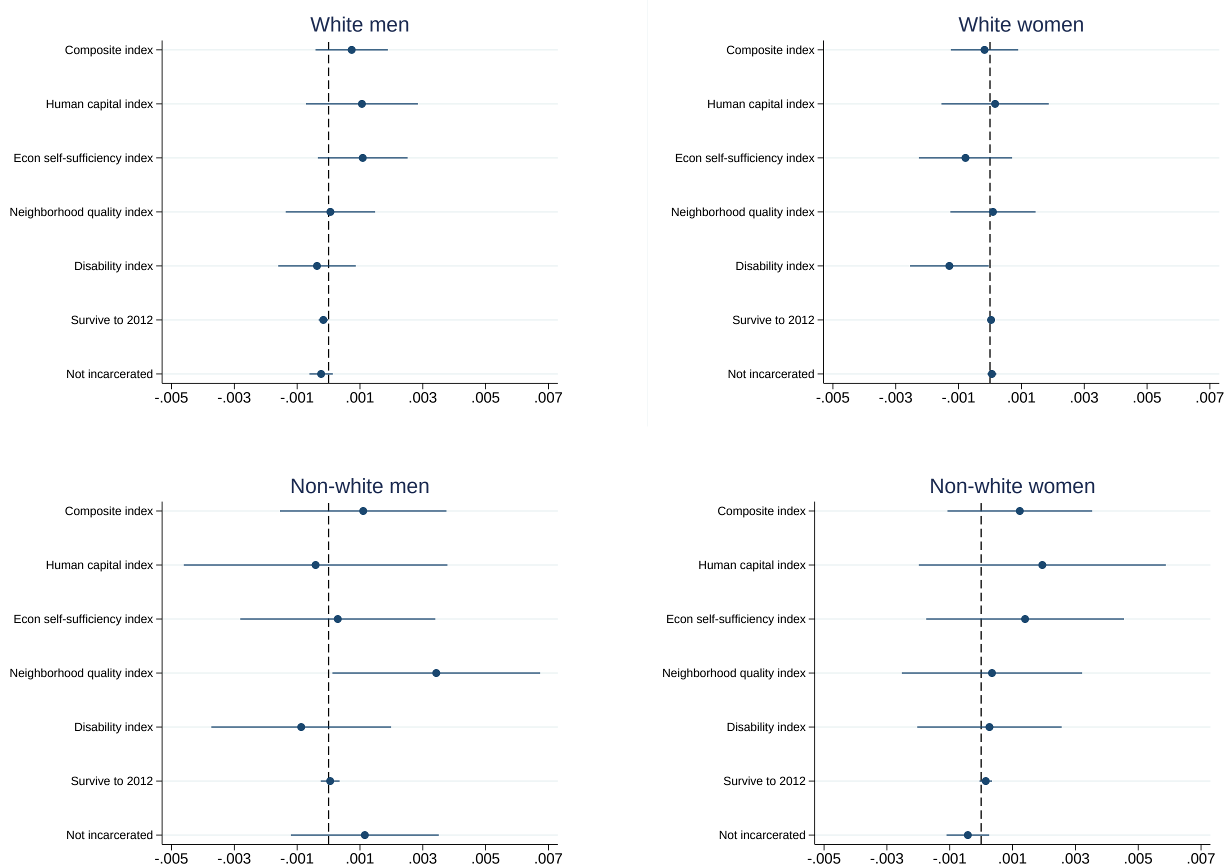
Notes: The panels plot event-study and spline estimates for non-incarceration in the 2006-2013 ACS using the specifications in equations (1) and (2) separately by race and sex. All models include fixed effects for birth-county, survey year, and birth-state×birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. Not incarcerated is expressed in percentage point units. See Figure 5 for more on sample, specification and data.

Appendix Figure 5: Event-Study and Spline Estimates of the ITT Effects of Food Stamps Exposure on the Neighborhood Quality Index by a Cohort's Age when the Program Launched, by Race and Sex



Notes: The panels plot event-study and spline estimates for the standardized index of neighborhood quality using the specifications in equations (1) and (2) separately by race and sex. All models include fixed effects for birth-county, survey year, and birth-state×birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. See Figure 5 for more on sample, specification, and data.

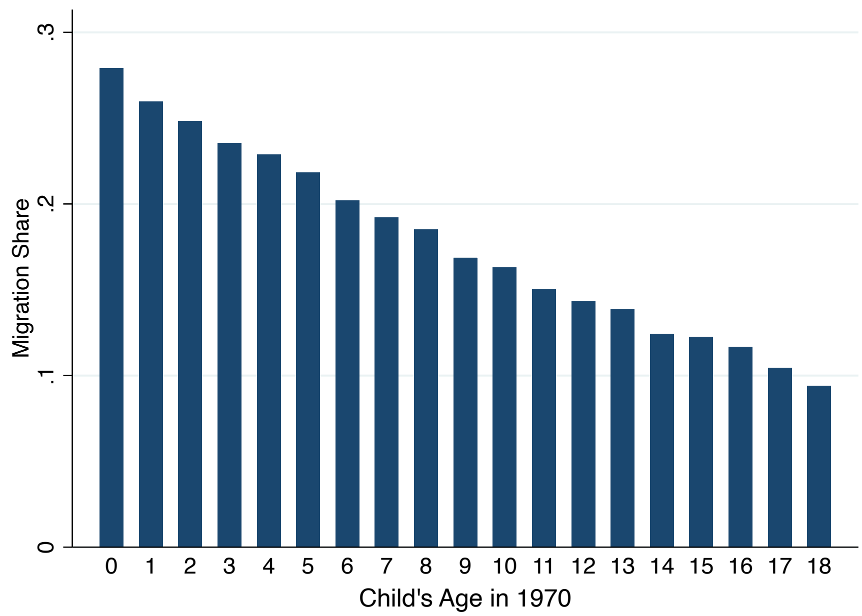
Appendix Figure 6: Spline Summary Estimates of the Pre-Trend of the ITT Effects of Food Stamps for Well-Being Indices, Survival, and Non-Incarceration, by Race and Sex



Notes: The panels plot the absolute values of the estimates on the pre-trend linear spline segments ($|\omega_1|$ covering ages -5 to -2) from equation (2) for our different outcomes and sub-groups, along with the 95% confidence intervals. The indices are standardized in terms of standard deviations, but "Survive to 2012" and "Not incarcerated" are in percentage point units. See Figure 5 for more on sample, specification, and data.

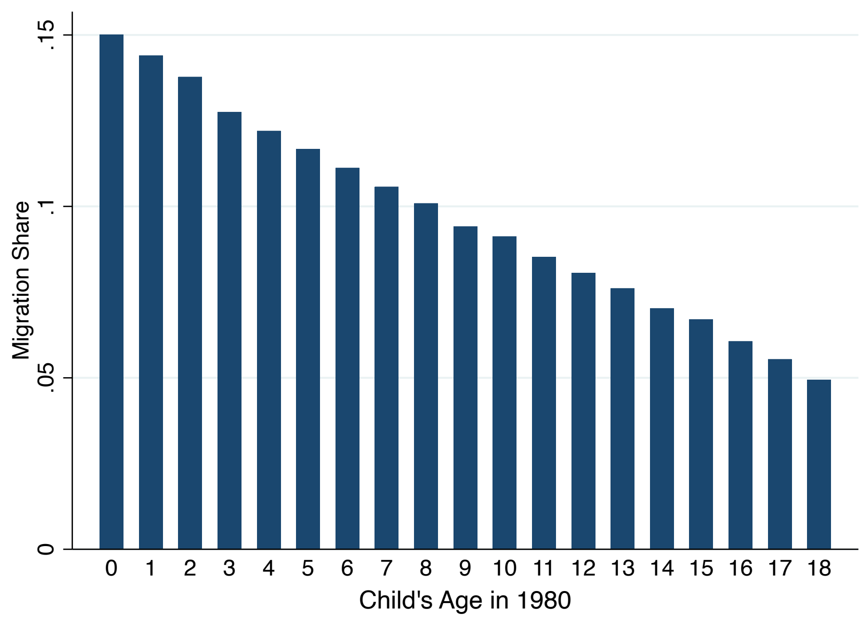
Appendix Figure 7: Five-Year Childhood Migration Rates

(A) Five-Year Childhood Migration Rates by Age of Child, 1970 Decennial Census



Source: 1970 Decennial Census.

(B) Five-Year Childhood Migration Rates by Age of Child, 1980 Decennial Census



Source: 1980 Decennial Census.

Notes: Using publicly available data from the 1970 and 1980 decennial census, the figure plots, by child age, the share of children who lived in a different county five years prior to the census year. We limit the sample to those with the child's mother (or the head of household, if no mother was present) had less than a high school degree. We use the migration of the mother (or head of household, if no mother was present) as a proxy for migration of the child. We present the results by the child's age in 1970 or 1980.

Appendix Table 1: Outcome Means, Whole Sample and by Race and Sex

Title	All	White males	White females	Nonwhite males	Nonwhite females
Yrs schooling	13.760	13.750	13.910	13.140	13.370
HS/GED or more	0.930	0.928	0.949	0.866	0.886
Some college or more	0.665	0.648	0.700	0.561	0.635
4 yrs college or more	0.328	0.332	0.350	0.227	0.256
Graduate degree or more	0.031	0.038	0.026	0.026	0.022
Professional occupation	0.372	0.363	0.405	0.260	0.325
In labor force	0.857	0.932	0.796	0.862	0.788
Worked last yr	0.876	0.942	0.826	0.868	0.808
# wks worked last yr	41.810	46.200	38.460	41.090	37.220
Usual hrs worked/wk	36.460	42.590	31.240	37.450	31.580
Log labor income	10.570	10.860	10.300	10.560	10.260
Log non-labor income, excl public source	7.355	7.221	7.393	7.641	7.753
Log fam income to pov	5.851	5.943	5.865	5.622	5.427
Not in pov	0.903	0.934	0.904	0.849	0.782
-1 x Log(public source income)	-9.039	-9.177	-9.037	-9.001	-8.738
Labor income > 0	0.871	0.939	0.819	0.860	0.799
Family income > 0	0.975	0.982	0.974	0.962	0.954
Log house value	12.090	12.100	12.120	11.890	11.860
Log gross rent	6.844	6.875	6.874	6.808	6.757
Home ownership	0.785	0.814	0.819	0.636	0.599
Single family residence	0.859	0.869	0.869	0.795	0.809
Log mean fam income to pov (tract)	5.891	5.920	5.926	5.689	5.656
-1 x teen pregnancy rate (tract)	-0.040	-0.037	-0.036	-0.059	-0.062
-1 x share single HOH (tract)	-0.436	-0.422	-0.420	-0.519	-0.531
-1 x child pov rate (tract)	-0.211	-0.197	-0.196	-0.303	-0.319
Mean home ownership (tract)	0.742	0.758	0.762	0.641	0.632
Log median value of home (tract)	11.990	12.010	12.010	11.840	11.800
Log median gross rent (tract)	6.823	6.823	6.826	6.796	6.775
Absolute upward mobility (CH)	42.250	42.620	42.580	40.420	40.080
Mean fam income (tract) > 0	0.941	0.944	0.944	0.919	0.916
No work disability	0.914	0.918	0.927	0.859	0.868
No ambulatory difficulty	0.950	0.956	0.952	0.933	0.924
No cognitive difficulty	0.968	0.971	0.969	0.957	0.955
No independent learning difficulty	0.963	0.970	0.968	0.934	0.927
No vision/hearing difficulty	0.981	0.979	0.985	0.973	0.979
No self-care difficulty	0.987	0.989	0.987	0.981	0.977
Not incarcerated	0.984	0.981	0.997	0.859	0.989
Survive to 2012	0.956	0.945	0.972	0.932	0.963
Number of observations	17,400,000	7,423,000	7,817,000	951,000	1,204,000
Number of cells	4,272,000	2,684,000	2,781,000	561,000	668,000
Number of counties	3,000	3,000	3,000	2,900	2,900

Notes: The table provides means of each of the outcome variables reported in the paper, for the whole sample, and by race and sex category. Sub-index outcomes are not normalized; indices are normalized. For details on sample and data, see Table 3.

Appendix Table 2: Sensitivity of the Exposure Model to Control Variables

	(1)	(2)	(3)
Panel A: Human Capital			
%IU - Age 5	0.0094	0.0140	0.0103
	(0.0037)	(0.0036)	(0.0035)
Number of observations	17,400,000	17,400,000	17,400,000
Number of cells	4,272,000	4,272,000	4,272,000
Number of counties	3,000	3,000	3,000
R^2	0.123	0.125	0.127
Panel B: Economic Self-Sufficiency			
%IU - Age 5	0.0017	0.0018	0.0043
	(0.0015)	(0.0014)	(0.0016)
Number of observations	17,400,000	17,400,000	17,400,000
Number of cells	4,272,000	4,272,000	4,272,000
Number of counties	3,000	3,000	3,000
R^2	0.057	0.057	0.058
Panel C: Neighborhood Quality			
%IU - Age 5	0.0014	0.0068	0.0115
	(0.0035)	(0.0041)	(0.0036)
Number of observations	17,400,000	17,400,000	17,400,000
Number of cells	4,272,000	4,272,000	4,272,000
Number of counties	3,000	3,000	3,000
R^2	0.375	0.378	0.379
Panel D: Physical Disability			
%IU - Age 5	-0.0015	-0.0001	0.0014
	(0.0021)	(0.0016)	(0.0013)
Number of observations	16,800,000	16,800,000	16,800,000
Number of cells	2,796,000	2,796,000	2,796,000
Number of counties	3,100	3,100	3,100
R^2	0.050	0.052	0.053
Panel E: Survive to 2012			
%IU - Age 5	0.0000	-0.0003	0.0007
	(0.0005)	(0.0005)	(0.0003)
Number of observations	114,000,000	114,000,000	114,000,000
Number of cells	943,000	943,000	943,000
Number of counties	3,000	3,000	3,000
R^2	0.684	0.692	0.696
Panel F: Not Incarcerated			
%IU - Age 5	0.0008	0.0007	0.0008
	(0.0004)	(0.0004)	(0.0004)
Number of observations	7,705,000	7,705,000	7,705,000
Number of cells	2,591,000	2,591,000	2,591,000
Number of counties	3,000	3,000	3,000
R^2	0.025	0.026	0.027
State X birth year FE	X	X	X
Cty_{60} x linear cohort		X	X
FE county, birth year, survey year			X

Notes: We report the same estimates as in Table 2, but for each of our 6 main outcomes (the four well-being indices, survival to 2012, and non-incarceration).

Appendix Table 3: Estimated ITT Effects of Food Stamps Exposure between Conception and Age 5 on Life Expectancy

	(1)	(2)	(3)
%Age 0 - Age 5	0.2320	0.1524	0.1977
	(0.0558)	(0.0449)	(0.0287)
FE county, birth year, survey year	X	X	X
Cty_{60} x linear cohort		X	X
State X birth year FE			X
Number of observations	17,300,000	17,300,000	17,300,000
Number of cells	1,322,000	1,322,000	1,322,000
Number of counties	3000	3000	3000
R^2	0.830	0.842	0.848

Notes: Each column provides estimates from the exposure model in equation (3), using as the outcome our estimate of life expectancy (see text and Online Appendix for more details on how we construct this measure based on our survival outcome). The data are collapsed into cells at the birth-county×birth-year×survey-year level, and the reported coefficient is on the exposure variable: the share of years between age 0 and age 5 that a cohort is exposed to Food Stamps based on when the program began in the cohort's county of birth. All columns include fixed effects for birth-county, birth-year, and survey year. Column 2 adds 1960 county characteristics interacted with a linear trend in year of birth. Column 3 adds birth-state×birth-year fixed effects. Standard errors are clustered by county of birth and indicated in parentheses. The number of observations, number of cells and number of counties are rounded to the nearest 1,000 for disclosure purposes.

Appendix Table 4: Romano-Wolf P-Values Associated With Estimates in Table 4

Title	All	White males	White females	Nonwhite males	Nonwhite females
Human capital					
Yrs schooling	0.0099	0.0099	0.0099	0.9901	0.9802
HS/GED or more	0.1188	0.5842	0.4950	0.9901	0.8515
Some college or more	0.0099	0.0099	0.0198	0.9901	0.9802
4 yrs college or more	0.0099	0.0099	0.0099	0.7822	0.8515
Graduate degree or more	0.4257	0.8614	0.2079	0.7822	0.8515
Professional occupation	0.0297	0.0297	0.2079	0.7327	0.9802
Economic self sufficiency					
In labor force	0.7723	0.1980	0.0495	0.7228	0.6436
Worked last yr	0.4653	0.9505	0.1584	0.2574	0.6931
# wks worked last yr	0.3465	0.6040	0.1485	0.0297	0.6931
Usual hrs worked/wk	0.3465	0.0594	0.1485	0.5347	0.9703
Log labor income	0.0099	0.0099	0.7723	1.0000	0.7426
Log non-labor income, excl public source	0.4653	0.9505	0.7723	0.9010	0.6931
Log fam income to pov	0.0099	0.0099	0.0198	1.0000	0.7426
Not in pov	0.0099	0.9010	0.6931	0.5941	0.9703
-1 $\times$ Log(public source income)	0.3465	0.9505	0.7723	1.0000	0.9703
Labor income > 0	0.3762	0.9406	0.3267	0.2277	0.7426
Family income > 0	0.0990	0.9505	0.7723	0.3960	0.9703
Neighborhood quality					
Log house value	0.5050	0.9109	0.6733	1.0000	0.7129
Log gross rent	0.4950	0.7624	0.6733	1.0000	0.8911
Home ownership	0.0396	0.3168	0.0396	0.9901	0.9901
Single family residence	0.1584	0.7624	0.0495	0.2079	0.5842
Log mean fam income to pov (tract)	0.0495	0.2178	0.0099	0.9901	0.9802
-1 $\times$ teen pregnancy rate (tract)	0.2574	0.9109	0.6733	0.9802	0.9901
-1 $\times$ share single HOH (tract)	0.0396	0.0594	0.0099	1.0000	0.9901
-1 $\times$ child pov rate (tract)	0.0396	0.1584	0.0297	0.9802	0.8218
Mean home ownership (tract)	0.0297	0.1287	0.0297	1.0000	0.9802
Log median value of home (tract)	0.2574	0.8416	0.1089	0.9901	0.9703
Log median gross rent (tract)	0.2475	0.3168	0.3168	1.0000	0.9901
Absolute upward mobility (CH)	0.1881	0.2574	0.6436	1.0000	0.0099
Mean fam income (tract) > 0	0.0594	0.2574	0.1089	0.6337	0.8515
FE county, survey year	X	X	X	X	X
$Cty_{60} \times$ linear cohort	X	X	X	X	X
State $\times$ birth year FE	X	X	X	X	X
Number of observations	17,400,000	7,423,000	7,817,000	1,028,000	1,310,000
Number of cells	4,272,000	2,684,000	2,781,000	561,000	668,000
Number of counties	3,000	3,000	3,000	2,900	2,900

Notes: This table reports the Romano-Wolf p-values associated with estimates reported in Table 4.

Appendix Table 5: Estimated ITT Effects of Food Stamps Exposure in Early (Conception to Age 5) and Later Childhood (Ages 6 to 18) on Non-Incarceration for Nonwhite Males

	Not Incarcerated
%IU - Age 5	0.0053
	(0.0046)
%Ages 6-18	0.0241
	(0.0087)
FE county, survey year	X
Cty_{60} x linear cohort	X
State x birth year FE	X
Number of observations	494,000
Number of cells	338,000
Number of counties	2700
R^2	0.067

Notes: This table provides results from estimating an augmented version of the exposure model (equation 3) that includes two exposure variables—(i) the share of months of Food Stamps exposure between conception and age 5 and (ii) the share of months of Food Stamps exposure between ages 6 and 18. The outcome is non-incarceration and the sample is limited to nonwhite males.

Appendix Table 6: Spline Estimates of the Estimated ITT Effects of Food Stamps Exposure on Well-Being Indices, by Race and Sex

	All	White males	White females	Nonwhite males	Nonwhite females
Panel A: Composite					
Pre-trend: -5 to IU	-0.0004	-0.0007	0.0002	-0.0011	-0.0012
	(0.0005)	(0.0006)	(0.0005)	(0.0014)	(0.0012)
IU to age 5	-0.0017	-0.0016	-0.0008	-0.0012	-0.0017
	(0.0007)	(0.0006)	(0.0006)	(0.0010)	(0.0010)
Age 6 to 11	-0.0003	-0.0008	0.0002	-0.0006	-0.0006
	(0.0008)	(0.0006)	(0.0007)	(0.0012)	(0.0012)
Age 12 to 17	-0.0005	-0.0011	-0.0001	0.0007	-0.0024
	(0.0009)	(0.0007)	(0.0008)	(0.0013)	(0.0011)
Panel B: Human captial					
Pre-trend: -5 to IU	-0.0004	-0.0011	-0.0002	0.0004	-0.0019
	(0.0007)	(0.0009)	(0.0009)	(0.0021)	(0.0020)
IU to age 5	-0.0021	-0.0023	-0.0016	-0.0005	-0.0019
	(0.0008)	(0.0008)	(0.0007)	(0.0017)	(0.0015)
Age 6 to 11	-0.0004	-0.0012	0.0002	0.0008	0.0004
	(0.0010)	(0.0009)	(0.0010)	(0.0021)	(0.0018)
Age 12 to 17	-0.0005	-0.0013	-0.0001	0.0021	-0.0019
	(0.0011)	(0.0011)	(0.0011)	(0.0021)	(0.0018)
Panel C: Economic Self-Sufficiency					
Pre-trend: -5 to IU	-0.001	-0.001	0.001	0.000	-0.001
	(0.0006)	(0.0007)	(0.0008)	(0.0016)	(0.0016)
IU to age 5	-0.0008	-0.0010	0.0014	-0.0008	-0.0016
	(0.0005)	(0.0006)	(0.0007)	(0.0009)	(0.0011)
Age 6 to 11	0.0002	0.0000	0.0019	-0.0011	-0.0015
	(0.0006)	(0.0006)	(0.0007)	(0.0010)	(0.0012)
Age 12 to 17	-0.0004	-0.0008	0.0006	0.0016	-0.0014
	(0.0007)	(0.0006)	(0.0008)	(0.0012)	(0.0012)
Panel D: Neighborhood quality					
Pre-trend: -5 to IU	-0.0002	-0.0001	-0.0001	-0.0034	-0.0003
	(0.0007)	(0.0007)	(0.0007)	(0.0017)	(0.0015)
IU to age 5	-0.0022	-0.0015	-0.0022	-0.0024	-0.0015
	(0.0010)	(0.0007)	(0.0008)	(0.0013)	(0.0012)
Age 6 to 11	-0.0008	-0.0012	-0.0014	-0.0014	-0.0008
	(0.0011)	(0.0008)	(0.0008)	(0.0016)	(0.0013)
Age 12 to 17	-0.0005	-0.0012	-0.0010	-0.0017	-0.0039
	(0.0012)	(0.0009)	(0.0009)	(0.0017)	(0.0014)
Panel E: Physical Disability					
Pre-trend: -5 to IU	0.0007	0.0004	0.0013	0.0009	-0.0003
	(0.0005)	(0.0006)	(0.0006)	(0.0015)	(0.0012)
IU to age 5	0.0003	0.0004	0.0005	-0.0009	0.0014
	(0.0004)	(0.0004)	(0.0004)	(0.0009)	(0.0009)
Age 6 to 11	0.0009	0.0008	0.0010	0.0008	0.0016
	(0.0004)	(0.0005)	(0.0004)	(0.0011)	(0.0010)
Age 12 to 17	0.0008	0.0005	0.0009	0.0007	0.0005
	(0.0004)	(0.0005)	(0.0005)	(0.0011)	(0.0011)
Panel F: Survive to 2012					
Pre-trend: -5 to IU	0.0000	0.0002	0.0000	-0.0001	-0.0001
	(0.0000)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
IU to age 5	-0.0001	0.0001	0.0000	-0.0002	-0.0002
	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0001)
Age 6 to 11	-0.0001	0.0001	0.0000	-0.0005	-0.0004
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)
Age 12 to 17	0.0001	0.0003	0.0001	-0.0001	-0.0002
	(0.0001)	(0.0001)	(0.0001)	(0.0003)	(0.0002)
Panel G: Not incarcerated					
Pre-trend: -5 to IU	0.0000	0.0002	-0.0001	-0.0012	0.0004
	(0.0001)	(0.0002)	(0.0001)	(0.0012)	(0.0003)
IU to age 5	0.0000	0.0001	0.0000	-0.0012	0.0002
	(0.0001)	(0.0001)	(0.0000)	(0.0007)	(0.0002)
Age 6 to 11	-0.0001	0.0001	0.0000	-0.0034	0.0001
	(0.0001)	(0.0001)	(0.0000)	(0.0009)	(0.0002)
Age 12 to 17	0.0002	0.0002	0.0000	-0.0015	0.0001
	(0.0001)	(0.0002)	(0.0000)	(0.0010)	(0.0002)

Notes: This table reports the spline estimates for each of our main outcomes, for the whole sample and separately by race and sex categories. See notes under Figure 4 for more details.

Appendix Table 7: Estimated ITT Effects of Food Stamps Exposure between Conception and Age 5 on Higher Percent Urban and Higher Number of Four Year Colleges in Adult County

	All	White males	White females	Nonwhite males	Nonwhite females
Panel A: Higher Percent Urban in Adult County					
%IU - Age 5	0.0017	0.0027	0.0022	-0.0008	0.0073
	(0.0016)	(0.0020)	(0.0019)	(0.0041)	(0.0047)
R^2	0.42	0.313	0.337	0.379	0.373
Panel B: Higher Number of Four-Year Colleges in Adult County					
%IU - Age 5	0.0020	0.0034	0.0030	-0.0011	0.0057
	(0.0014)	(0.0017)	(0.0017)	(0.0040)	(0.0039)
R^2	0.328	0.244	0.257	0.301	0.299
FE county, survey year	X	X	X	X	X
Cty_{60} x linear cohort	X	X	X	X	X
State x birth year FE	X	X	X	X	X
Number of observations	17,400,000	7,423,000	7,817,000	951,000	1,204,000
Number of cells	4,272,000	2,684,000	2,781,000	561,000	668,000
Number of counties	3,000	3,000	3,000	2,900	2,900

Notes: This table reports estimates of the exposure model (equation 3) using two outcomes, reported in the two panels. Specifically, using information on individuals' adult counties of residence and birth counties, we have merged in information on urbanicity (percent urban population from the 2010 National Historic Geographic Information System, or NHGIS) and the number of 4-year colleges using data from the Integrated Postsecondary Education Data System, which is published by the National Center for Education Statistics. We then created two outcomes: (i) an indicator equal to 1 for individuals for whom the adult county has a higher percent urban population than the birth county, and 0 otherwise, and (ii) an indicator equal to 1 for individuals for whom the adult county has a higher number of 4-year colleges than the birth county. Note that these indicators are set to 0 for individuals who do not move counties (i.e., their birth and adult county are the same). See notes under Table 3 for more details about the model and sample.

Appendix Table 8: The Effect of Childhood Food Stamps Exposure on Migration in PSID Data

	(1)	(2)	(3)	(4)
	1968-1970 Birth Sample		1965-1970 Birth Sample	
	Moved to another county by age 5	Moved to another county with FSP by age 5	Moved to another county by age 5	Moved to another county with FSP by age 5
Share of Age 0-5 with FSP	0.046	0.190	0.125	0.133
(Using County of Birth)	(0.837)	(0.379)	(0.060)	(0.022)
FE birth-county, survey year	X	X	X	X
Birth year FE	X	X	X	X
Number of observations	785	785	1648	1648
Mean DV	0.224	0.195	0.185	0.142
R^2	0.567	0.561	0.415	0.443

Source: Public and Restricted Panel Study of Income Dynamics.
Notes: Using the Panel Study of Income Dynamics, we explore the impact of geographic mobility on measurement error and directed migration. The restricted PSID allows us to observe the county and state of residence for most individuals in the PSID sample, starting in 1968. This means we can see the county of birth for those born in or after 1968. Our two samples include children born between 1965-1970 and 1968-1970. In selecting these two year of birth samples, we balance the need for sufficient sample size with the limitation of county of residence being observed starting in 1968. Additionally, because Food Stamps is available in all counties in 1975, there is no potential for endogenous or directed migration after that. Therefore we provide estimates for two samples, one that includes 785 children born in 1968-1970, with full information on residence from birth to age 5. The second expands to 1648 children born 1965-1970. In both cases we limit to births up to 1970 since our exposure by age 5 (1975) is the key variable.