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

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Abstract:
We use novel large-scale data on 43 million Americans from the 2000 Census and the 2001 to 2013 American Communities Survey linked to the Social Security Administration's NUMIDENT to study how a policy-driven increase in economic resources for families affects children's long-term outcomes. Using variation from 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 experienced an increase of 6 percent of a standard deviation in their adult human capital, 3 percent of a standard deviation in their adult economic self-sufficiency, 8 percent of a standard deviation in the quality of their adult neighborhoods, 0.4 percentage point increase in longevity, and a 0.5 percentage point decrease in likelihood of being incarcerated.

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I. Introduction

Social safety net programs are designed to help the poorest members of society meet their food, housing, and healthcare needs. The modern American safety net was greatly expanded under President Lyndon B. Johnson's War on Poverty, which aimed “not only to relieve the symptom of poverty, but to cure it and, above all, to prevent it” (Johnson's January 1964 State of the Union Address, 1965).

An important pillar of this prevention strategy was the Food Stamps program, which provides poor individuals with vouchers to purchase food at grocery stores. Introduced in the 1960s, this program raised food spending among participating families by 21 percent (Hoynes and Schanzenbach 2009), increased infant birth weight, and reduced infant mortality (Almond et al. 2011). This program remains especially relevant today. Now called the Supplemental Nutrition Assistance Program (SNAP), today’s Food Stamps program raised 3.9 million children out of poverty with approximately $65 billion dollars in spending in fiscal year 2018.[1] Combined with other safety net programs like the Earned Income Tax Credit, Food Stamps helped lower the child poverty rate in the U.S. from 28 percent in 1967 to 16 in 2016 (National Academies of Sciences 2019).

These reductions in child poverty may translate into lasting effects on human capital, health, economic self-sufficiency, and overall well-being—benefits that are by necessity omitted in short-term, contemporaneous evaluations of Food Stamps. Hoynes et al. (2016) break new ground in documenting some long-term benefits of Food Stamps using the Panel Study of Income

[1]By comparison, spending on the Earned Income Tax Credit was $67 billion (https://www.irs.gov/pub/irs-soi/16in25ic.xls) and spending on Temporary Assistance for Needy Families is much lower at $28.3 billion (https://www.acf.hhs.gov/ofa/resource/tanf-financial-data-fy-2016), both for 2016. The poverty reduction figure comes from Fox (2018) and is measured for 2017.

Dynamics (PSID). They find that greater exposure to Food Stamps in childhood, particularly before age 5, leads to a reduction in *adult* metabolic syndrome conditions (including obesity, high blood pressure, diabetes, heart disease) 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. Bitler and Figinski (2018) use data from the Social Security Administration's Continuous Work History Sample, which contains information on earnings for one percent of US-born individuals. They find that exposure to Food Stamps before age 5 increases adult earnings for women but has insignificant effects for men. However, these data contain only a few outcomes, and they are unable to provide a more comprehensive analysis of the program's impact on population well-being. Consequently, the efficacy of the program in preventing poverty and partially "paying for itself" remains an open question.

This paper provides new, more comprehensive evidence regarding the long-term impacts of childhood exposure to Food Stamps on individuals' adult economic productivity and well-being—evidence that is essential for understanding the value of American preventative anti-poverty policy. Filling a crucial gap in this literature, the combined 2000 Census (a 1-in-6 sample of all U.S. households), 2001 to 2013 American Community Surveys (ACS), and Social Security Administration's (SSA) NUMIDENT allow us to calculate the likely exposure *as children* of more than 17 million American adults to the Food Stamps program. This large-scale linked survey and administrative data set also contains a wide range of new outcomes, including educational attainment, labor market productivity, poverty status, adult participation in safety net programs, incarceration, physical and cognitive disabilities, mortality, mobility from one's county of birth, and the quality of one's neighborhood of residence in adulthood.

Our empirical strategy, which we 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 previous work and control for birth-county fixed effects and 1960 county characteristics interacted with linear trends. Moreover, our larger samples allow us to include state-of-birth by *individual* birth-year fixed effects as well as survey year fixed effects in our preferred specifications, which account for the rich set of policy changes at the state level during the 1960s and dramatic changes in the U.S. economy from 2000 to 2014.

Our results show that greater access to Food Stamps *in utero* and in early childhood is associated with large improvements in a broad set of measures of adulthood well-being. Using pre-specified indices (Kling et al. 2007), we document that full exposure to Food Stamps (access for the entirety of time between one's estimated month of conception and age five) leads to a 0.009 standard-deviation increase in the composite index of adult human capital and well-being. This aggregate improvement is driven by 0.010, 0.004, and 0.012 standard-deviation increases in the human capital, economic self-sufficiency, and neighborhood quality indices, respectively. Full exposure to the Food Stamps program also increases the likelihood of survival to 2012 by 0.07 percentage points and reduces the likelihood of being incarcerated by 0.08 percentage points. These are intention-to-treat (ITT) estimates for an entire birth cohort, including children who never

[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.

used the program. Scaling these ITT estimates by approximate Food Stamp participation rates of about 16 percent among children aged five and younger at the time the program rolled out implies that the program's effects on participant children are more than six times larger (this is the approximate average treatment effect on the treated, or TOT; see Appendix Figure 2). We find no significant effects of exposure to Food Stamps at ages 6 to 18 once we control for exposure in early childhood, suggesting that greater resources for mothers while the child is *in utero* and greater resources in the household in the first five years of life are especially critical for shaping individuals' adult human capital, health, and productivity.[3]

Our analysis of this unprecedented range of adult outcomes has important implications for valuing Food Stamps as a long-term, public sector investment. For instance, when analyzing the individual components of the economic self-sufficiency index, we find that childhood exposure to Food Stamps reduces the likelihood that individuals receive income from public programs in adulthood. This implies that the social safety net for families with young children may, in part, "pay for itself" by reducing reliance on government support in the long-term.

Further, our ability to observe individuals' places of birth together with their residences in adulthood allows us to provide novel evidence on the long-term effects of Food Stamps on geographic mobility and resulting neighborhood quality. We show that Food Stamps availability in early childhood increases the likelihood that individuals move away from their counties of birth, own their own home, and reside in a single-family home. This is consistent with the idea that Food Stamps in early life allows individuals to move to "better" neighborhoods, as measured by a range of characteristics at the Census tract and county levels. Although the impacts of Food Stamps on

[3] It is also the case that Food Stamp participation rates are lower among children aged 6-18 than children aged 5 years or younger (see Appendix Figure 2). 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.

adult outcomes appear, at least in part, to operate through mobility, we also show long-term benefits for individuals who stay in their counties of birth until adulthood.[4]

Our results on the long-term impacts of early life access to Food Stamps contribute to two strands of prior literature. First, past research 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. 2015).

Second, and more broadly, a large literature documents 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 U.S. safety net programs with long-term outcomes have only recently begun to emerge (see Hoynes and Schanzenbach 2018 for a review). Studies show that childhood access to cash welfare (Aizer et al. 2016), the Earned Income Tax Credit (Bastian and Michelmore 2018), and Medicaid (Brown et al. 2015, Miller and Wherry 2018, Cohodes et al. 2016, Goodman-Bacon 2016) lead to improvements in human capital and health in adulthood.

Our work is also 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). However, this work faces similar data

[4] In fact, we find that the impacts of Food Stamps are larger for individuals who are resident 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 subgroup heterogeneity, as movers are positively selected.

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 recent 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, Akee et al. 2010). 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.

The paper proceeds as follows. Section II discusses the history of the Food Stamps program, its rollout, and how greater access to Food Stamps in childhood may lead to improvements in adult outcomes. Section III describes our data sources and presents summary statistics from our restricted Census-ACS-SSA sample, and Section IV discusses our empirical methods and identifying assumptions. We present our results in Section V, a discussion of magnitudes in Section VI, and offer some conclusions in Section VII.

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 (after deductions). This is a federal program, run out of the US Department of Agriculture, and benefits are the same across different regions of the US (except Alaska and Hawaii). Benefits are paid monthly; in 2018 recipients received an average of $252 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, 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 Program in their county. Counties had to apply for the program, and Congress appropriated funding at the amount of $75 million in year 1; $100 million for year 2; and

[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, FS also has an asset test, currently set at $2,250 (or $3,500 for the elderly and disabled).
[7] For a compact history of the Food Stamp program see https://www.fns.usda.gov/snap/short-history-snap.

$200 million in year 3. Following the FSA, the rollout across counties steadily increased (Appendix Figure 1). The 1973 Amendments to FSA, passed on August 10, 1973, required that the program be expanded to every county by July 1, 1974. By mid-1973 almost 90 percent of the US population lived in counties that had Food Stamps programs in place. Figure 1 displays a county map of the US indicating the date of county Food Stamps initiation, with darker shaded counties representing later program introduction. The map shows substantial *within-state* variation in the timing of implementation of the Food Stamps program 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). The evidence is mixed with some studies finding that households respond to Food Stamps like ordinary cash income (Schanzenbach 2007, Hoynes and Schanzenbach 2009, Beatty and Tuttle 2012, Bruich 2014), while other studies find that Food Stamps yields more spending on food than ordinary income (Hastings and Shapiro 2018). One channel for long run impacts could be the increases in the

[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 2015).

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 lead 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.

Additionally, the emerging literature on the long run effects of the social safety net shows that investments during childhood lead to improved adult human capital and economic outcomes as well as health. Aizer et al. (2016) examine the 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), 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.

Access to economic resources through Food Stamps may also reduce stress, which is an additional pathway for improving long-term outcomes. Recent work shows that lower socioeconomic status may be causally related to stress hormones (e.g. cortisol) and that additional

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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. 1996, Deming 2011).

[10] Additionally, studies show that access to Medicaid *in utero* and in childhood leads to improvements in educational attainment (Brown et al. 2015, Miller and Wherry 2018, Cohodes et al. 2016), earnings (Brown et al. 2015), mortality (Goodman-Bacon 2016, Wherry and Meyer 2015, Brown et al. 2015), 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.

resources may attenuate this relationship (Aizer et al. 2015, Evans and Garthwaite 2014, Fernald and Gunnar 2009, Hausofer et al. 2012). 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 Food Stamps to lead to improved human capital and economic outcomes by increasing resources during childhood. Moreover, the timing of childhood exposure to these programs should influence the strength of the long run impacts. Hoynes et al. (2016) shows that the beneficial effects of Food Stamps on adult metabolic health derive from exposure prior to age 5. Aizer et al. (2016) provide suggestive evidence that the positive effects 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, lead to increases in educational attainment and earnings in young adulthood. More generally, the broader literature documents that early life environment is particularly important for individual well-being throughout the life cycle (Almond and Currie 2011a, Almond and Currie 2011b, Almond, Currie and Duque 2018).

Given this evidence, we may expect the effects of Food Stamps to manifest non-linearly based on the age at which a cohort was first exposed. Figure 2 illustrates this expected relationship. A cohort that was *in utero* when the program was introduced in their county of birth would be expected to benefit directly in each year of life. Moreover, if greater health and nutrition in one's earlier life makes subsequent investments into child development more productive (Cunha and Heckman, 2007; Heckman and Masterov, 2007; Heckman and Mosso, 2014), then this relationship should compound more for children who are younger when they are first exposed. More generally, exposure to greater nutrition and resources at ages under five may be particularly consequential as this is a crucial period of child development. The length of exposure and these complementarities fall as children are first exposed to the Food Stamps program at older ages, which motivates the downward sloping shape in Figure 2.

Another feature of Figure 2 is that we do not expect individuals who were born after the Food Stamps program began (cohorts age -1 or younger on the x-axis) to experience larger effects than those born in the year it began. The rationale for this is that children born 1, 2, or ten years before the Food Stamps program should have access to the program their *entire* childhood. Our empirical analysis tests for both the declining effects of the Food Stamps program by age at its introduction as well as the flattening of this relationship for children born in the years after the program began.[11]

III. Data

Our primary source of data combines information on individual outcomes in adulthood 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 source is the 2000 Census Long Form (1 in 6 sample) and 2001-2013 ACS files linked to the SSA NUMIDENT file. The Census and ACS data contain a large number of individual outcomes, which we describe further below. The NUMIDENT file 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

[11] Almond et al. (2011) show that Food Stamp programs ramp up quickly with adoption of a new program. Bitler and Figinski (2019) show that in the 10 percent of counties that did not have a Commodity Distribution Program at some point prior to implementation of the Food Stamp Program, ramp up was slower, taking perhaps 5 years to reach the eligible population. (Those 90 percent with a Commodity Distribution Program experienced quick ramp up.)

large share of the US population. In particular, the Census covers 16.7 percent of the US population. After accounting for overlap in the samples, the ACS brings the total coverage to roughly 25 percent of the US 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). In practice, this algorithm delivers a crosswalk between the NUMIDENT place of birth string variable and county FIPS codes, with over 90 percent of individuals matched to their counties of birth.[12]

Our primary analysis sample includes individuals who were born in the U.S. between 1950 and 1980 in order for our sample to span cohorts born before, during, and after the Food Stamps program rolled out. We limit the sample to those 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. We also choose a sample of individuals with valid PIKs that allow linkage to the NUMIDENT file and who have a valid string for place of birth that can be matched to a county FIPS code. The Online Appendix provides more detail on our data and analysis sample.

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,

[12] Details on the matching algorithm is documented in a technical memorandum to the Census and stored with Research Data Center files for the 1284 project. Additionally, see the Online Appendix to Isen et al. (2017).
[13] For two outcome variables – physical disability and survival to 2012 – we widen the age range to 25-64.

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 orient the outcomes that are observed in all of the Census/ACS survey years in the same direction (such that a higher value represents a "better" outcome) and 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.

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

1. <u>Productivity and Human Capital Index</u> (years of schooling; high school or GED completed; any college received; college or more completed; professional degree obtained; professional occupation);
2. <u>Economic Self-Sufficiency Index</u> (in labor force; worked last year; weeks worked last year; usual hours worked per week; labor income; other income not from public sources; income-to-poverty ratio; not in poverty; reverse coded income from welfare, supplemental security, and other government sources);
3. <u>Neighborhood Quality Index</u> (value of home; gross rent; home or apartment ownership; residence with single and not multiple families; income-to-poverty ratio in census tract of residence; reverse-coded teen pregnancy in tract; reverse-coded share of single-headship in census tract, reverse-coded share of poor children in tract; share of home ownership in census tract of residence; median house price in census tract of residence; median gross rent in census tract of residence; and 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).[14]

Additionally, we separately consider two more outcomes:

5. Not Incarcerated (indicator for not being incarcerated, which we can infer based on information on residence in group quarters in the 2006-2013 ACS data).
6. Survival to Year 2012 (i.e., individual does not have a date of death in the 2011 NUMIDENT). This outcome is based on the full population of social security card applicants in the NUMIDENT and is not limited to our Census/ACS samples.[15]

Appendix Table 1 presents means of the normalized indices and the elements of each of the indices, for the full sample and for the race by gender subgroups.

Data on Food Stamps Rollout: Dates of Food Stamps introduction are available at the 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 hand entered using USDA annual reports on Food Stamps monthly caseloads by county and are available for years 1961-1979.

Data on County-level Controls: 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

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

[15] 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, expressed as a share of the number of births in that cell.

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 rollout 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 on the data sources and construction of variables.

IV. Empirical Methods

We exploit the county-by-birth year (or birth year and 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 x birth-county x survey-year cells, separately by gender and race (white versus non-white).[16] In some models we collapse by birth-month x birth-year 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

[16] Nonwhite includes all individuals with a non-missing race variable who do not report being white.

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 at which Food Stamps was first introduced ($a = b - FS_c$), and event-time coefficients range from 5 years before birth to age 17, with age 10 as the omitted category.[17] We control for fixed effects for the birth county, θ_c, and a full set of fixed effects for birth-state x birth-year, $\delta_{s(c)b}$, and survey year, ψ_t.[18] 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. 2015), we control for county-level controls from the 1960 Census, each interacted with a linear birth cohort trend, $Z_{c60}b$. In robustness checks, we also control for birth-county x birth-year and birth-cohort-varying controls, X_{cb}. The event-study 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.

Importantly, the years prior to conception (event-time < -1) provide our "pre-birth pre-trend" test per Figure 2. The event-study model allows for non-parametric estimation of the time path of effects of 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 exposure age -1), *in utero* through age 5 (-1 to +5), middle childhood (ages 6-11), and older childhood (ages 12-17). The linear spline model takes the form:

$$Y_{cbt} = \theta_c + \delta_{s(c)b} + \psi_t + X_{cb}\beta + Z_{c60}b\eta + \omega_1 1[b - FS_c < -1] * (b - FS_c) +$$

[17] Given birth cohorts 1950-1980 and Food Stamps rollout is for 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.
[18] We have also estimated a model that adds a quadratic polynomial in age at survey year. The results are very similar and available upon request.

$$\omega_2 1[-1 \leq b - FS_c < 6] * (b - FS_c) + \omega_3 1[6 \leq b - FS_c < 11] * (b - FS_c) +$$
$$\omega_4 1[11 \leq b - FS_c] * (b - FS_c) + \epsilon_{cbt} \quad (2)$$

for each cohort born in county *c* in state *s(c)*, and year *b*, and observed in survey year *t*. $b - FS_c$ is the age at Food Stamps introduction, which we interact linearly with four separate indicators for the exposure groups described above. This spline approach allows us to test for pre-trends ($\omega_1 = 0$, the pre-birth pre-trend test), and examine whether the marginal effect of one more year of exposure is larger in the *in utero* and early years (ω_2) that are considered to be critical in the early childhood literature (Almond and Currie 2011).

Lastly, we estimate a standard difference-in-difference (DD) model, using a cumulative measure of exposure in early childhood as in Hoynes et al. (2016). Specifically, we calculate the share of *months* each cohort is exposed to Food Stamps (based on the month and year the program began in each county) between the (approximate) month of conception and age five, $ShareFS_{cb}^{IU-5}$, and use it as the primary explanatory variable in the following equation:[19]

$$Y_{cbt} = \theta_c + \delta_{s(c)b} + \psi_t + X_{cb}\beta + Z_{c60}b\eta + \kappa ShareFS_{cb}^{IU-5} + \upsilon_{cbt} \quad (3)$$

for each cohort born in county *c* in state *s(c)*, and year *b*, and observed in survey year *t.*

Regardless of the model, it is important to remember that the 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, we never observe a birth cohort first exposed at age 2 but without exposure in later childhood. Instead our estimates reflect the effect of additional Food Stamps exposure earlier in childhood, conditional on also having access to it

[19] In this model we use the data collapsed at the birth-year x birth-month x birth-county x survey year. Conception is approximated as 9 months prior to birth. Whenever we have data at the birth-year x birth-month level, we control for fixed effects for month and year of birth.

later in childhood. Furthermore, in our setting, exposure earlier in life also means exposure for more years.

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 commodity distribution program (CDP). CDP was first and 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 CDP, so they had to drop 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) found 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.[20] Yet they also found that the county characteristics explain very little of the variation in adoption dates, a fact that is consistent with the characterization of Congressional appropriate limits controlling the movement of counties off the waiting list (Berry 1984).

The existence of a commodity distribution program (CDP) in some counties prior the Food Stamps program predicts a more rapid expansion in the Food Stamps program following county

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

adoption (Bitler and Figinski 2018). Because we do not have data on 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 vouchers for food through the CDP would lead our analysis to understate the effects of providing Food Stamps relative to 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 little cause for concern. The timing of the Food Stamp rollout is not correlated with the launch of the other War on Poverty programs, which allows our identification strategy to isolate the effect of Food Stamps.

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. Second, we test the sensitivity of our estimates to adding county-by-year controls. Third, our preferred model includes a full set of birth-state-by-birth-year fixed effects, which likely absorbs some of the potential non-randomness of Food Stamps introduction and means that we only rely on within-state variation in program rollout. Finally, the pre-birth pre-trend test in the event-study and linear-spline models provides an evaluation of differential trends in outcomes.

Table 1 presents estimates from the linear exposure model (3), using data collapsed to the birth-year x birth-month x birth-county level. Each row presents three sets of estimates of the coefficient on $ShareFS_{cb}^{IU-5}$ from the model using the listed county characteristic as the dependent variable. All models include birth-county and birth-state x birth-year fixed effects, as well as 1960 county characteristics interacted with a birth cohort trend. Column (1) uses a linear birth cohort trend, column (2) uses a quadratic in birth cohort, while column (3) uses a cubic in birth cohort. Out of 14 coefficients, 4 are statistically significant at the 5-percent level. Consistent with earlier work, we find greater Food Stamps exposure is associated with larger populations (Hoynes and Schanzenbach 2009) and 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. 2019). We also find no relationship between Food Stamps exposure and county income, county employment or county adult or infant mortality. We do find a statistically significant association between Food Stamps exposure and per capita spending on other transfer programs (social security, health, cash welfare). These estimates, however, are relatively small and are negative; implying that as Food Stamps exposure increases, there is *less* other transfer spending in the county. This relationship implies we should expect a downward bias in our estimates. We do not find that these conclusions change as we change the polynomial order of the trend interacted with 1960 county characteristics (columns 2 and 3). As we indicate in the table, none of these variables is available for all birth cohorts in our sample (1950-1980). Accordingly, we do not include these controls in our main estimates (but do include them in a robustness table) finding that they do not change our qualitative conclusions.

V. Results

A. 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, where the series with solid circles presents 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 series with squares is from a model that includes all of those variables and adds fixed effects for birth-state x birth-year. The estimates are scaled in standard-deviation units, which is represented on the y-axis. Because we do not observe program participation, these are intent-to-treat (ITT) estimates using all individuals in our sample—not just those participating in the program.

Recall that movement along the x-axis from right to left represents earlier (and longer) exposure to Food Stamps. The estimates suggest that additional years of access to Food Stamps in the early childhood (prior to age 5) lead to larger increases in the composite index than exposure at later years. In addition, there is no evidence of a differential pre-trend—the event-study is flat prior to conception (before exposure age -1), suggesting no differential trends in the composite index for cohorts who are exposed to the program for the duration of their childhood.

However, even with our large samples, the event-study coefficients are not precisely estimated. Panel B of Figure 3 repeats the most saturated event study model (with birth-state x birth-year fixed effects) and adds the fitted spline function from model (2) including the same sets of fixed effects. 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. This figure suggests that the spline provides a good representation of the estimates in the event-study and highlights that the additional assumptions yield more precise estimates.

The estimates from the spline model show that an additional year of exposure in early life (*in utero* to age 5) leads to a statistically significant 0.002 standard-deviation increase in the composite index and smaller and insignificant effects of additional years at older ages (splines for ages 6-11 and ages 12-17). Additionally, the pre-trend pre-birth spline (the spline between ages of exposure -5 to -2) is small and statistically insignificant.

Table 2 presents the results from the exposure model (3), where we show estimates with (column 3) and without (column 2) birth-state x 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 in early life are statistically significant and qualitatively similar in models with and without the state-by-year fixed effects. Thus, the rest of the paper presents results from the most saturated model that includes birth-state x birth-year fixed effects.

The coefficient in column (3) of Table 2 implies that moving from no access to Food Stamps to full access from conception through age 5 leads to a 0.009 standard-deviation increase in the adult composite index. To translate this ITT estimate into an average treatment-on-the-treated (TOT) effect, we require information on Food Stamp participation rates. In Appendix Figure 2, 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 where Food Stamps is available nationwide. The figure shows that Food Stamps participation declines with age, and that approximately 16 percent of all children aged 0 to 5 (and 13 percent for children 6-17) used the program during this time period.[21] Thus, we can divide our estimates by 0.16 (or, multiply them by 6.25) to obtain the implied TOT effects of full exposure from conception to age 5. Doing so

[21] 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); there food stamp participation for children five or less is 18 percent. We combine three years of data from the PSID to increase precision.

yields a TOT effect of 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.[22]

We next examine each of our four indices separately (human capital, economic self-sufficiency, neighborhood quality and physical disability) as well as survival to 2012 and non-incarceration. Figure 4 presents the four spline estimates and their confidence intervals for these outcomes. Figure 5 presents the event-study graphs along with the fitted spline models for these outcomes. To facilitate comparisons across outcomes (and for the splines, across different ages of exposure), we have used the same scale across all of the index outcomes. (The graphs for the survival and non-incarceration outcomes are on different scales, since those impacts are estimated as percentage point effects rather than standard-deviation effects.)

Looking across these graphs, several findings emerge. First, Figure 4 shows that none of the pre-trend effects are statistically different from zero (dashed lines with circles), and their magnitudes are the smallest among all of the splines for most outcomes. This is encouraging evidence in support of our research design. Second, for almost all outcomes, the coefficient on the spline in early childhood (solid lines with triangles) is the largest in magnitude and statistically significant, consistent with prior evidence about the importance of the early life period. Across the outcomes, the impact of additional years of exposure beginning in middle and older childhood does not translate into statistically significant improvements in long-run outcomes.

The estimates for the exposure model (3) for these outcomes are presented in Table 3. The magnitudes show that an increase from no access to full exposure from conception through age 5

[22] The estimate of on the linear spline covering early life (ω_2) is 0.0017, which multiplied by the 5.75 years of exposure (conception to age 5) implies a 0.01 standard-deviation increase in the composite index ITT or 0.06 TOT. One can see a similar magnitude by reading off the coefficients in the event-study.

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 no statistically significant effects on physical disability, possibly reflecting the relatively young ages of our sample as well as restricted data availability (this outcome is only available before 2008). For survival to 2012, full exposure leads to a 0.07 percentage point increase. We also find a 0.08 percentage point increase in the likelihood of being *not* incarcerated. One can multiply these ITT estimates by 6.25 (recall that the Food Stamps participation rate was about 16 percent for children aged 5 years and younger) to obtain approximate TOT impacts. The resulting magnitudes are large. For example, given that 96 percent of the sample survives to year 2012, the effect on the likelihood of survival expressed as a share of the non-surviving yields a 11 percent TOT impact: (0.0007/0.04) x 6.25=0.109. As a share of survival it is a 0.5 percent TOT impact.

Appendix Figure 3 provides estimates of effects for each of the elements of the four indices based on the exposure model, which we summarize here. In order to facilitate comparisons across outcomes, each outcome is standardized (z-scored) so the estimates reflect standard-deviation impacts. The human capital estimates show increases in education up through college graduation (and not beyond). Economic self-sufficiency estimates show small and statistically insignificant effects on extensive and intensive margins of labor supply, with positive and statistically significant impacts on log earnings, the log family income to poverty ratio, and the likelihood of having income higher than the poverty line. We also find that more exposure to Food Stamps in early life leads to a large reduction in the likelihood of having no income from public assistance in adulthood. These findings imply that by reducing the likelihood of reliance on government support in adulthood, the social safety net serves as a long-term investment that may at least in part “pay for itself”.

Interestingly, the components of the neighborhood quality index show the most consistent positive and statistically significant impacts. We document that greater childhood exposure to Food Stamps leads to a large increase in the likelihood of home ownership, residence in a single-family home, and overall improvement in the quality of the neighborhoods in which individuals live as adults. Specifically, at the Census tract level, we observe increases in mean income as well as reductions in child poverty and teen pregnancy rates and the share of single-headed households. We also find that early childhood exposure to Food Stamps is associated with an increase in the adult county's measure of absolute upward mobility (Chetty et al., 2014). The unstandardized estimates for each of the sub-index components are provided in the first column of Appendix Table 2. Full exposure to Food Stamps between conception and age 5 leads to a 0.2 percentage point increase (or 1.3 percentage-point TOT) in having a high school degree (or GED) compared to the mean of 93 percent (Appendix Table 1). Full exposure between conception and age 5 leads to a 0.4 percentage point increase (or 2.5 percentage-point TOT) on not being in poverty compared to the mean of 90 percent.

B. Heterogeneity in Estimates

Table 4 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. The results in the first row show that the ITT effects on human capital are largest for white males (0.010 standard deviation) with slightly smaller effects for white females (0.008 standard-deviation) and statistically insignificant effects for nonwhite men and women. These findings may be surprising, given that all else equal, we would expect larger ITT effects for nonwhites due to their lower average incomes and higher rates of eligibility for Food Stamps.

There are several important considerations when interpreting these effects. First, the lack of findings for nonwhites may reflect differences in sample sizes. 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. On the other hand, it is possible that the lack of access to high quality schools for blacks during this time period prevented them from reaping the benefits of exposure to Food Stamps before reaching school age. Consistent with this idea, Jackson and Johnson (2018) document the importance of "dynamic complementarities" between investments in early childhood (Head Start in their case) and school quality at older ages.

Appendix Figures 4 through 6 further explore the differences by race and gender by presenting event-study graphs for a few outcomes separately by subgroup. Appendix Figure 4 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 is much more constant throughout childhood, rather than concentrated in early life as we saw in the estimates for the full sample. Appendix Figure 5 demonstrates that access to Food Stamps leads to an increase in the probability of not being incarcerated, and only for nonwhite men. As with survival, the long-run benefits of Food Stamps for nonwhite males are consistent through childhood rather than being concentrated in early life. The implied 0.1 percentage point ITT impact for nonwhite men per year of exposure during early life is very large compared to the baseline mean incarcerated of 14 percent (Appendix Table 1). Appendix Figure 6 shows that the impacts of Food Stamps on neighborhood quality is the most consistent across the four race-gender subgroups. Appendix Figure 7 takes all of the estimates across the four race-by-gender subgroups and plots the pre-birth pre-trend spline estimates – of the 28 estimates plotted only 2 are statistically different from zero (neighborhood quality for nonwhite men and disability index for white women). The figure also makes clear that we have less precision when estimating effects for nonwhites, who, as noted previously, represent less than 15 percent of the overall sample.

Finally, we explore mobility as a potential mechanism in Table 5. In the first column, we use our entire sample and estimate the exposure model (3) using as the dependent variable the share not living in one's county of birth at the time of observation in our outcome data.[23] We find that full exposure to Food Stamps from conception to age 5 significantly increases the likelihood of moving away from one's county of birth by xx percentage points (xx percent relative to the sample mean of 70 percent).[24] This result suggests that the effect of Food Stamps on neighborhood quality (and, potentially, the other outcomes) at least in part operates through individuals being able to move to better places. In the rest of Table 5, we examine 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 (labeled "Movers"). Overall, comparing across the human capital, economic self-sufficiency and neighborhood quality indices (we drop the physical disability index from the main paper for space reasons, as the results are consistently statistically insignificant), the estimates of exposure to Food Stamps are larger for stayers compared to movers. The smaller estimates for movers are consistent with attenuation bias due to measurement error in Food Stamps exposure,[25] but it could also reflect subgroup heterogeneity; the means of the dependent variables and the fact that Food Stamps increases the likelihood of being a mover in the first place suggest that the movers are positively

[23] We observe residential location in the NUMIDENT file (capturing residence at birth) and in the Census/ACS (capturing residence at survey). We assign stayer/mover status using those two points in time.
[24] These estimates are not yet disclosed; and will be in the next version of the paper.
[25] Recall that we assign FS exposure using county of birth and we do not have any data on when the individual moved.

selected.

C. Robustness

In Table 6, we examine the sensitivity of our results to adding county time-varying controls, using the full sample composite index and the exposure model. 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 those measuring the War on Poverty programs, REIS transfer spending, and the natural log of county population. We limit the sample to the observations with non-missing variables for all of these controls. Adding the control for log population reduces the magnitude of the impact of Food Stamps exposure slightly, and adding further controls leaves the estimate virtually unchanged.

As we discuss above, once Food Stamps is in place it is never eliminated; therefore, if a person has access at age 5, he/she also has access throughout the rest of childhood (conditional on remaining in one's county of birth during childhood). Our main exposure model captures the share of time between conception and age 5 that Food Stamps is in place and does not account for exposure throughout the rest of childhood. In Table 7, 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.

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 but growing. However, few estimates provide ready comparisons to our outcomes. Hoynes et al. (2016) find that exposure between *in utero* and age 5 leads to a 0.7 standard deviation improvement in an index of metabolic syndrome and an insignificant effect on economic self-sufficiency (TOT). Bitler and Figinski (2018) find an effect of exposure from in utero to age 5 leads to a 15 percent (TOT) increase in earnings at age 32 for women and insignificant effects for men. We find a 7 percent (TOT) increase in labor income for the full sample of men and women (Appendix Table 2, 0.0114/0.16). Like Bitler and Figinski (2018) we find no significant effects on employment (Appendix Table 2).

Other comparisons come from studies on the long-run impacts of Head Start participation and Medicaid. Deming (2009) uses the PSID and NLSY and a sibling fixed effects model, and he finds that a TOT impact of participating in Head Start is a 0.23 standard-deviation increase in his 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. (2018) use the same Census/ACS/NUMIDENT data that we use 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).

Brown et al. (2015) use the variation in expansions of child eligibility for Medicaid across states and years and find that greater child Medicaid coverage leads to a reduction in adult mortality. 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 5 exposure model) and adjusting for take-up, their estimates suggest an 8 percent in mortality for women and a 13 percent reduction for men TOT. This compares to our 11 percent TOT estimate for the full sample.

VII. Conclusion

Children constitute nearly one third of all poor individuals in the United States, making them important beneficiaries of the social safety net system.[26] A recent report from the National Academies of Sciences documents that since the inception of Johnson's War on Poverty 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, it is not sufficient to evaluate the success (or failure) of safety net programs solely based on changes to the poverty rate. At their inception, these programs aspired to increase opportunities and give beneficiaries a 'hand up, not a handout'. Today, this framing typically applies to programs explicitly targeting the early childhood period—such as preschool and nurse home visiting interventions—which generate upfront costs but can be evaluated as an *investment* into adult human capital, health, and economic well-being. This framing underscores the point that the value of the investment may not materialize for many years. Interestingly, the broader social safety net system is not usually viewed in this way. Yet 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 to society, the social safety net system may partially "pay for itself".

In this paper, we use data on 43 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 US social safety net. We combine data from the 2000 Census and the 2001-2013 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,

[26] 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

economic self-sufficiency, neighborhood quality, disability, incarceration, and longevity.

Our results show that full access to Food Stamps in one's county of birth between the estimated month 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 0.010, 0.004, and 0.012 standard-deviation increases in the human capital, economic self-sufficiency, and neighborhood quality indices, respectively. We also document a 0.07 percentage point increase in the likelihood of survival to 2012, and a 0.08 percentage point rise in the likelihood of not 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. Our findings have important implications for current debates about the social safety net. The Food Stamps program (currently renamed as the Supplemental Nutrition Assistance 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 that is available to nearly all income eligible families, whereas other programs limit eligibility to particular subgroups determined by age, disability status, or household structure.[27] Food Stamps also plays an important countercyclical role by automatically increasing benefits as need increases (Bitler and Hoynes 2016); in the peak of the Great Recession nearly one in every seven individuals received Food Stamps benefits. 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

[27]Able-bodied adults 18-49 without dependents can only receive SNAP for 3 months in 3 years if they don't meet work requirements.

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 individuals who benefitted from it when it was initially rolled out. As more time passes and additional data linkages become available, these 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 the IPUMS, this top-coding is done at the state-year, 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).

Weighting the data: Our models are estimated on data collapsed to the birth-county/birth-year/survey-year level or at the birth-county/birth-year level. 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.

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

Cells: All models are estimated on data collapses to cells. For the event study (1) and spline (2) models, cells are defined as birth-year x birth-county x survey year. For the exposure model (3) the cells are birth-year x birth-month x birth-county x survey year. We collapse separately for the four gender x race subgroups. We don't have a cell for each combination. Some counties are dropped due to no match with county Food Stamps start dates (few). Some cells have no births in that month or year (small counties, particularly for nonwhite cells).

Creating Indices: We ignore missing values when aggregating to indices so indices will have the same number of observations (important for disclosure). When turning to specifications using sub-indices of outcomes you can see the differences in observations.

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 following (cite) using the institutionalized indicator. This data is only available for the 2006-2013 ACS.

II. COUNTY CONTROL VARIABLES

In robustness checks, we use county time varying variables. 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. Aggregate 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 (2011) use data from the Bureau of Economic Analysis Regional Economic Information System (REIS) to construct three per capita county transfer variables: cash public assistance benefits (AFDC, Supplemental Security Income, and General Assistance), medical spending (Medicare, Medicaid, and military health care), and cash retirement and disability payments (Old-Age Survivors Insurance, Disability Insurance, and other). 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 linear interpolate within counties to fill in the gaps (1960, 1961, 1963, and 1964).

C. County Characteristics and Local Economic Conditions

County income is the 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): Local Area Employment Indicators for 1969-1980. This data is also used to construct total transfers per capita for 1969-1980.

D. County Level Mortality Data

We use data from Almond et al. (2011) who create county by year measures of infant mortality for 1959-1980 based on 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, deaths per 1,000 comes from Bailey and Bacon-Goodman (2015).

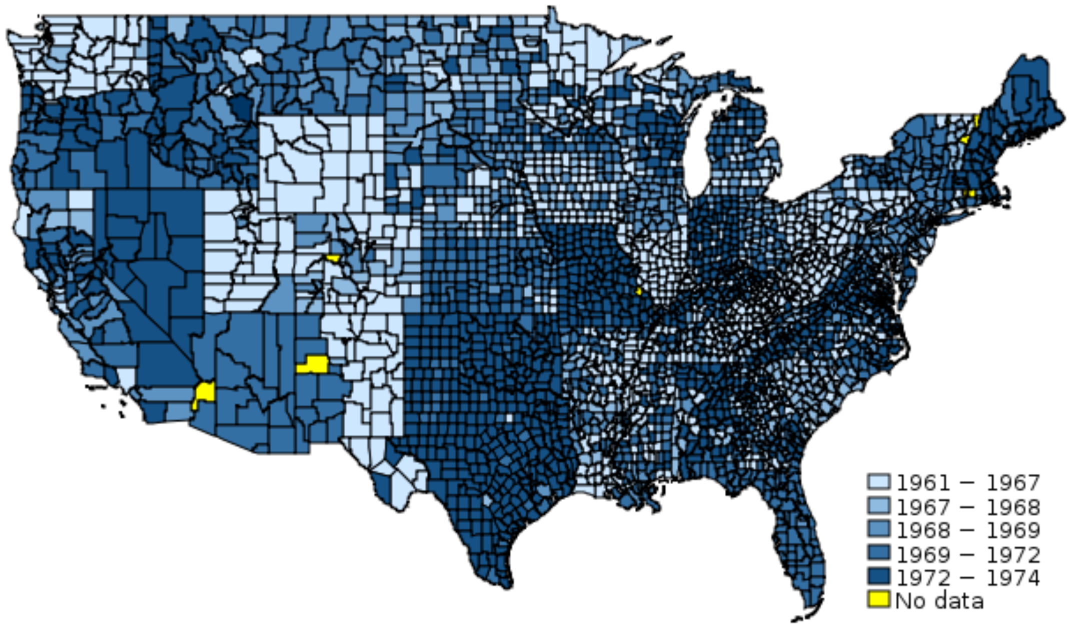
E. 1960 County Control Variables

We capture trends across counties over time, we control for 1960 County Characteristics interacted with linear 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

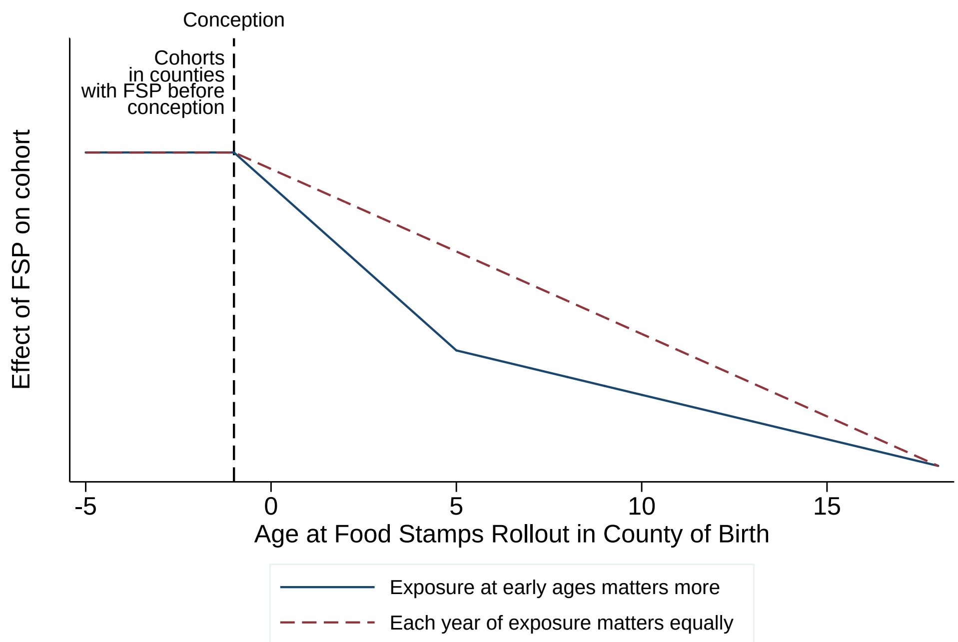
We estimated models where the dependent variable was the share missing of our outcome variables. There was no relationship between Food Stamp rollout and the incidence of missing variables.

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



Notes: Hoynes and Schanzenbach (2009) tabulations based on administrative data from the U.S. Department of Agriculture in various years.

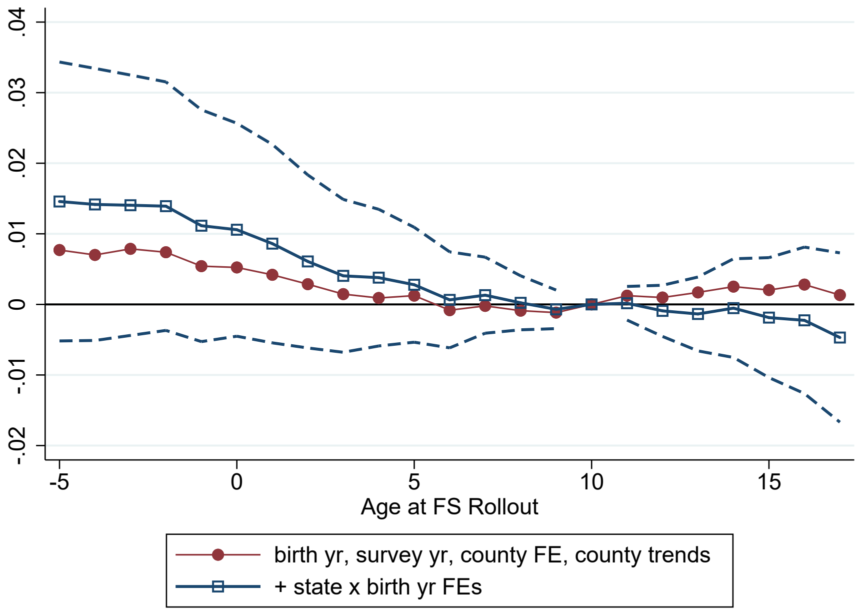
Figure 2: Expected ITT Effects of Food Stamps on Adult Well-Being by Age of the Cohort when the Program Began



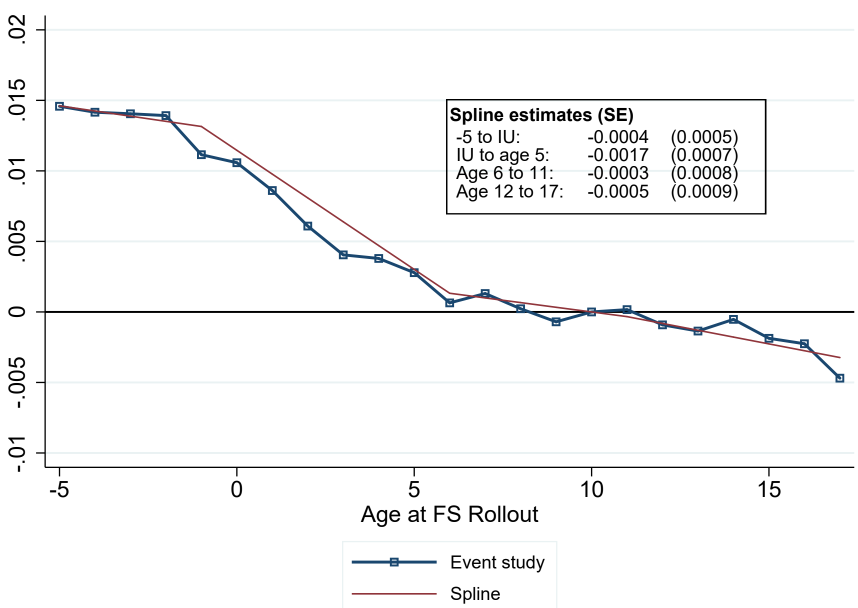
Notes: Figure illustrates the potential effects of Food Stamps by a cohort's age at the time the program started. Unlike other War on Poverty programs, the take up of Food Stamps was very rapid, so we do not model delayed take-up. The two series show different hypothetical effects: one series demonstrates how the estimates would appear if the effects on adult outcomes are the same for each year of additional exposure to the Food Stamps in childhood, which results in a linear pattern between ages 0 (in utero) and age 18. Alternatively, a second series show how the effect may be non-linear, with the effects of Food Stamps having a larger effect on adult outcomes for children with access in early childhood (before the age of 5).

Figure 3: Event-Study and Spline Estimates of the Estimated ITT Effects of Food Stamps Exposure by a Cohort's Age when the Program Launched

Panel A. Composite Index, Event-Study Estimates

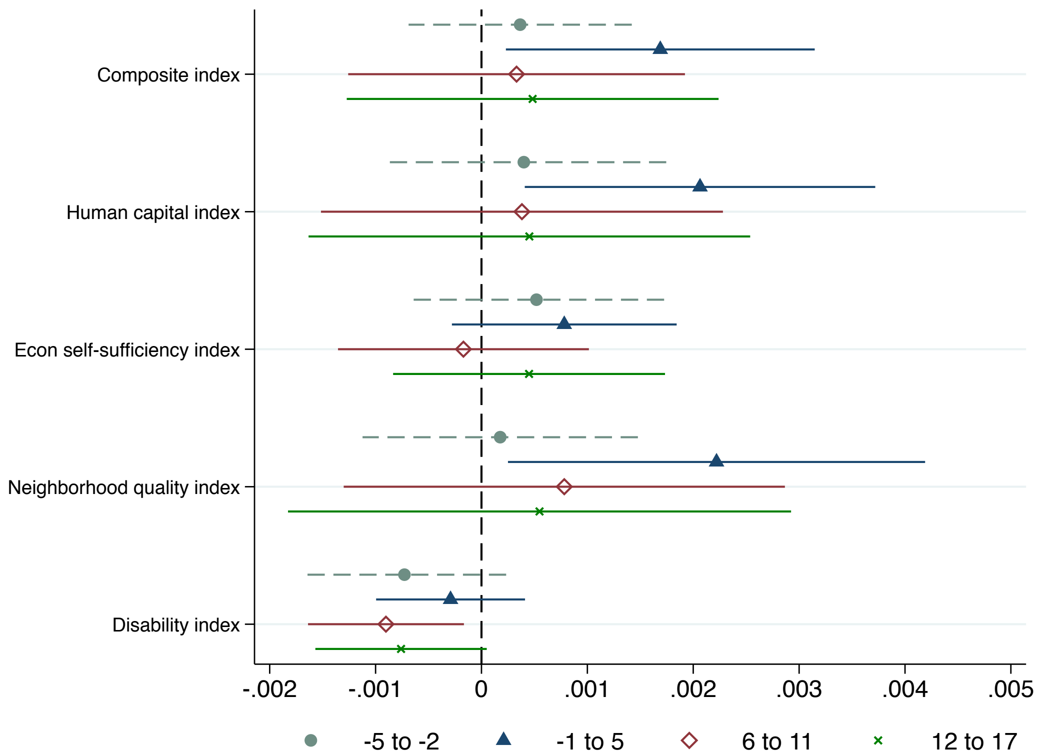


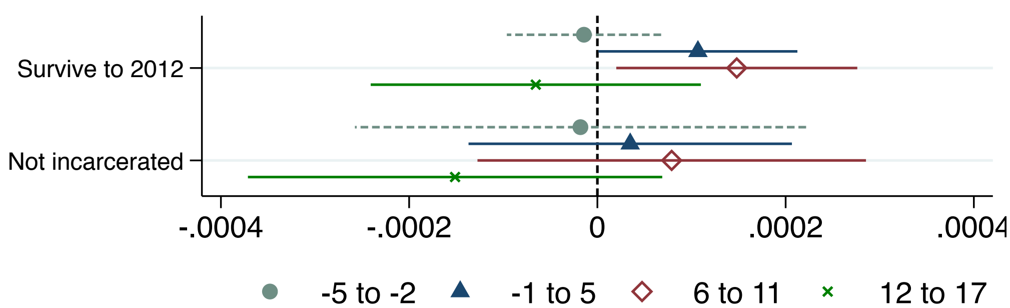
Panel B. Composite Index, Three-Part Spline



Notes: The panels plot event-study estimates for the composite standardized index of adult outcomes using the specifications in equations (1) and (2). Standard errors clustered at the birth-county level. Dashed lines show 95-percent, point-wise confidence intervals for each estimate in Panel A. Models in Panel B include fixed effects for birth-county, birth-year, survey year, and birth-state x birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. Data 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. Regressions estimated on data collapsed to cells defined by birth-county x birth-year x survey years and regressions are weighted using the number of observations per cell.

Figure 4: Spline Estimates of the ITT Effects of Food Stamps for Cohorts of Different Ages when the Program Launched for Different Indices of Well-Being, Longevity, and Incarceration





Notes: The panels plot–for different indices—the estimates on the four linear splines in equations (2). The splines correspond to ω_1 (ages -5 to -2), ω_2 (ages -1 to 5), ω_3 (ages 6 to 11), and ω_4 (ages 12 to 17) where age is when Food Stamps launched in their county of birth. 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, which is why these outcomes appear on different scales. All models include fixed effects for birth-county, birth-year, survey year, and birth-state x birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. Data includes 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. Regressions estimated on data collapsed to cells defined by birth-county x birth-year x survey years and regressions are weighted using the number of observations per cell.

Figure 5: Event-Study and Spline Estimates of the ITT Effects of Food Stamps for Cohorts of Different Ages when the Program Launched for Different Indices of Well-Being, Longevity, and Incarceration

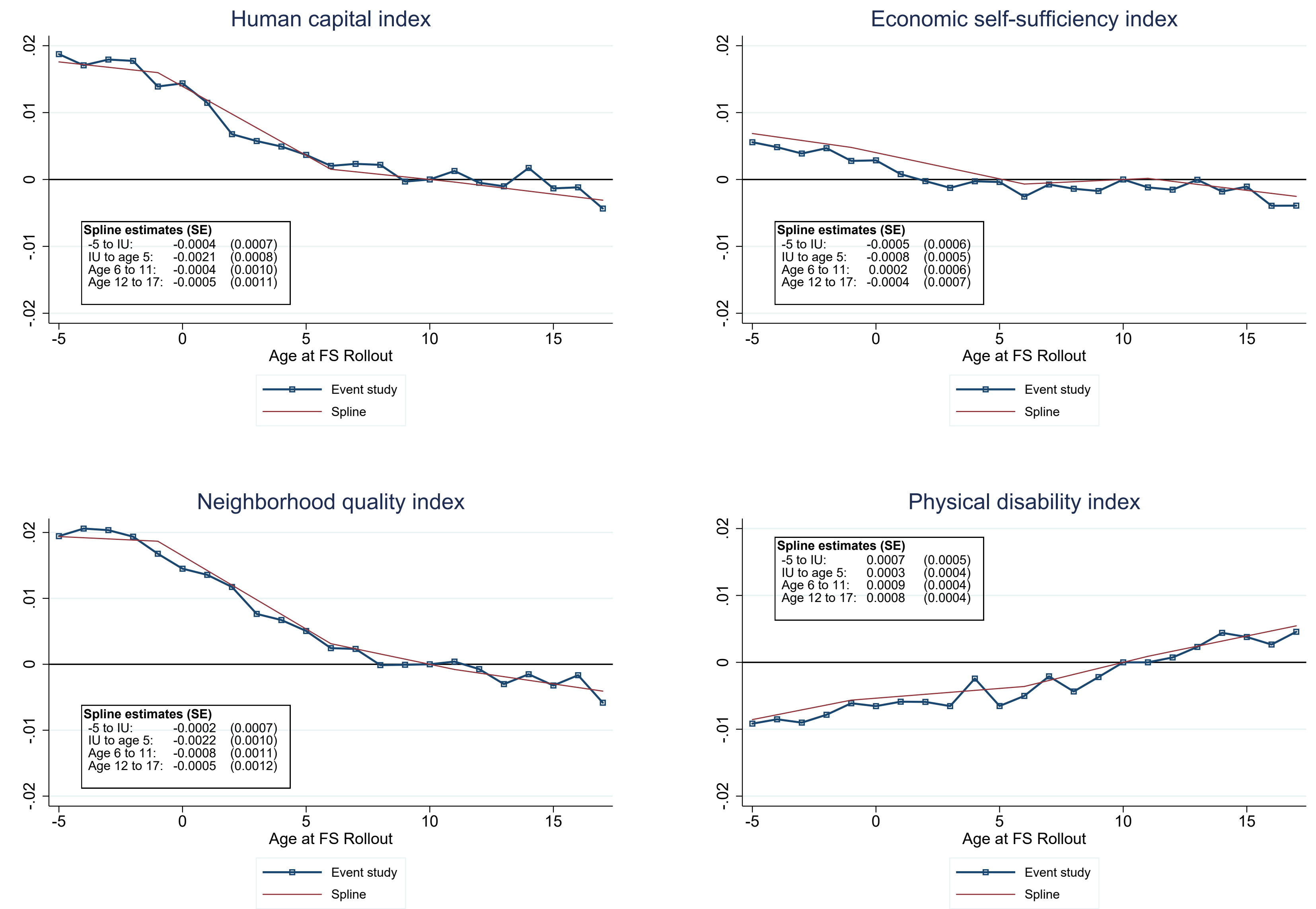
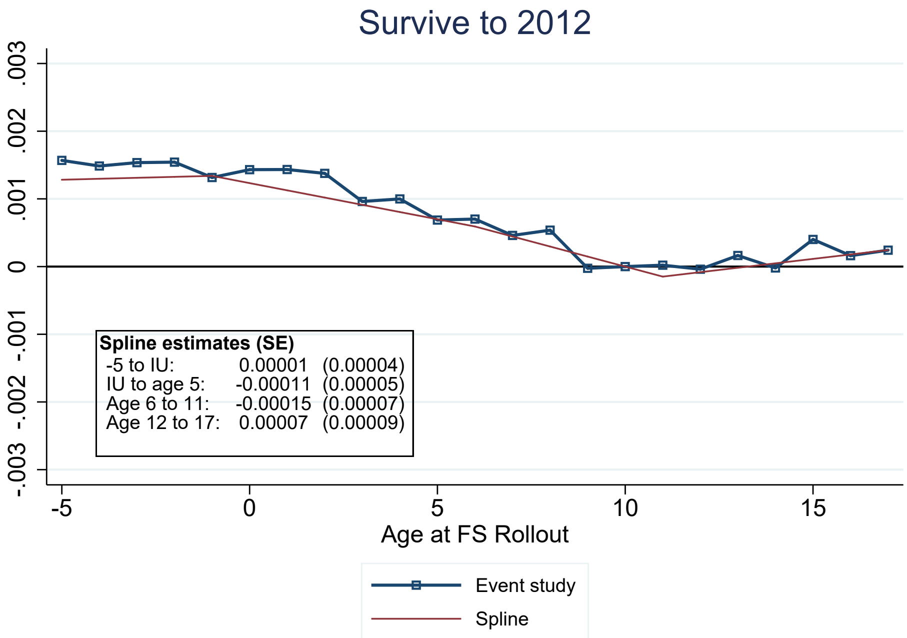
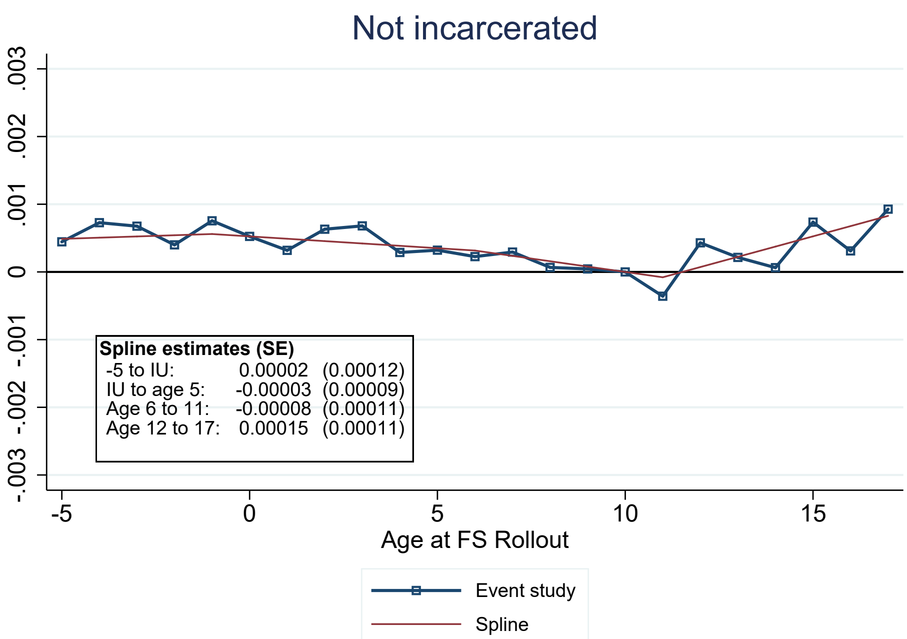


Figure 5: (continued)





Notes: The panels plot event-study and splines estimates—for various outcomes– using the specifications in equations (1) and (2). All models include fixed effects for birth-county, birth-year, survey year, and birth-state x birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. 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, which is why these outcomes appear on different scales. Data 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. Regressions estimated on data collapsed to cells defined by birth-county x birth-year x survey years and regressions are weighted using the number of observations per cell.

Table 1: Balance Tests: Estimated Effects of Food Stamps Exposure on County Characteristics and Other Programs

	(1)	(2)	(3)	Number of cells (1000s)	MDV	Range of years covered in the data
Other War on Poverty						
WIC	-0.0758	0.0227	0.0227	348	0.389	1970-1980
	(0.0526)	(0.0482)	(0.0482)			
Head Start	0.0196	-0.0188	-0.0188	722	0.500	1959-1980
	(0.0208)	(0.0188)	(0.0188)			
Community Health Center	-0.0254	-0.0864	-0.0863	722	0.063	1959-1980
	(0.0293)	(0.0242)	(0.0242)			
REIS Income Transfers Per Capita ($1000s)						
Retirement and DI benefits	-0.2089	-0.2122	-0.2121	725	1.003	1959-1980
	(0.0598)	(0.0608)	(0.0608)			
Military and military Medicaid	-0.0280	-0.0238	-0.0239	725	0.177	1959-1980
	(0.0052)	(0.0052)	(0.0052)			
Income maintenance (exc Food Stamps)	-0.0525	-0.0591	-0.0591	725	0.242	1959-1980
	(0.0190)	(0.0188)	(0.0188)			
Real total transfers	0.0282	0.0523	0.0523	382	2.266	1969-1980
	(0.0286)	(0.0285)	(0.0285)			
Other County						
Real personal income	-0.0710	-0.1565	-0.1565	382	19.960	1969-1980
	(0.1967)	(0.1955)	(0.1955)			
Log population	0.0499	0.0379	0.0379	722	12.340	1959-1980
	(0.0084)	(0.0084)	(0.0084)			
Log employment	-0.0006	-0.0049	-0.0049	382	11.710	1969-1980
	(0.0156)	(0.0160)	(0.0160)			
Mortality						
Adult mortality rate	-1.2420	0.9264	0.9275	722	866.700	1959-1980
	(3.1300)	(3.2310)	(3.2310)			
Infant mortality rate	0.0154	0.1454	0.1453	711	20.110	1959-1980
	(0.1805)	(0.1909)	(0.1909)			
Neonatal mortality rate	0.0902	0.2427	0.2425	711	14.620	1959-1980
	(0.1450)	(0.1550)	(0.1549)			
Post-neonatal mortality rate	-0.0748	-0.0973	-0.0973	711	5.495	1959-1980
	(0.0991)	(0.1023)	(0.1023)			
FE county, state x birth year	X	X	X			
Cty_{60} x linear cohort	X					
Cty_{60} x quadratic cohort		X				
Cty_{60} x cubic cohort			X			

Notes: Each row and column provide estimates for the exposure model in equation (3). The unit of analysis is at the county x year x 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 characteristic, including Other War on Poverty Programs, REIS Transfers and Income per capita, Mortality and other county characteristics. Each row and column presents a regression estimate. Regressions are weighted using the population in each cell. Column 1 includes county of birth and state by birth year fixed effects. Column 2 adds 1960 county characteristics interacted with a linear and quadratic in year of birth. Column 3 adds 1960 county characteristics interacted with a cubic in year of birth. Standard errors are clustered by county of birth and indicated in parentheses. The number of observations appears in column 4, and the mean of the dependent variable is presented in column 5. Some outcome variables are not available in all years, which is indicated in column 6. See 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 for the exposure model in equation (3). The unit of analysis is at the birth-county x birth-year x birth-month x survey-year level and the coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort would have been 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 x 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 Different Indices of Well-Being, Longevity, and 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 for the exposure model in equation (3). The unit of analysis is at the birth-county x birth-year x birth-month x survey-year level and the coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort would have been 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, survey year, and birth-state x 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 indicated 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 Figure 3 notes for more information on the sample and outcome.

Table 4: Estimated ITT Effects of Food Stamps Exposure between Conception and Age 5 on Different Outcomes, by Race and Sex

	(1)	(2)	(3)	(4)	(5)
	All	White males	White females	Nonwhite males	Nonwhite females
Human capital	0.0103	0.0102	0.0078	0.0044	-0.0007
	(0.0035)	(0.0036)	(0.0030)	(0.0067)	(0.0068)
Economic self-sufficiency	0.0043	0.0037	-0.0002	0.0063	0.0038
	(0.0016)	(0.0020)	(0.0027)	(0.0044)	(0.0049)
Neighborhood quality	0.0115	0.0048	0.0095	0.0019	-0.0042
	(0.0036)	(0.0024)	(0.0028)	(0.0050)	(0.0046)
Physical disability	0.0013	0.0001	0.0000	0.0083	-0.0035
	(0.0013)	(0.0018)	(0.0016)	(0.0036)	(0.0032)
Survive to 2012	0.0007	0.0006	0.0003	0.0008	0.0001
	(0.0003)	(0.0004)	(0.0002)	(0.0009)	(0.0006)
Not incarcerated	0.0008	0.0004	0.0001	-0.0001	0.0002
	(0.0004)	(0.0006)	(0.0002)	(0.0039)	(0.0011)
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	3000	3000	3000	2900	2900

Notes: See Table 3 notes.

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Human capital		Economic self-sufficiency		Neighborhood quality	
	Stayers	Movers	Stayers	Movers	Stayers	Movers
%IU - Age 5	0.0113	0.0064	0.0062	0.0031	0.0156	0.0085
	(0.0031)	(0.0021)	(0.0029)	(0.0019)	(0.0035)	(0.0031)
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	5,182,000	12,200,000	5,182,000	12,200,000	5,182,000	12,200,000
Number of cells	2,101,000	3,567,000	2,101,000	3,567,000	2,101,000	3,567,000
Number of counties	2700	3000	2700	3000	2700	3000
Mean DV	-0.115	0.068	-0.0228	0.0533	-0.152	0.0679
R^2	0.283	0.181	0.0662	0.0425	0.538	0.301

Notes: Stayers are individuals who –at the time of observation in the 2000-2013 Census/ACS—are observed in the same county as they were born. Movers are individuals who are not living in their birth county at the time of observation. See also Table 3 notes.

Table 6: 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)
%IU - Age 5	0.0087	0.0070	0.0074
	(0.0034)	(0.0033)	(0.0029)
FE county, year	X	X	X
Cty_{60} x linear cohort	X	X	X
State x year FE	X	X	X
County population		X	X
Other county controls			X
Number of observations	11,200,000	11,200,000	11,200,000
Number of cells	3,115,000	3,115,000	3,115,000
Number of counties	3000	3000	3000
R^2	0.213	0.213	0.213

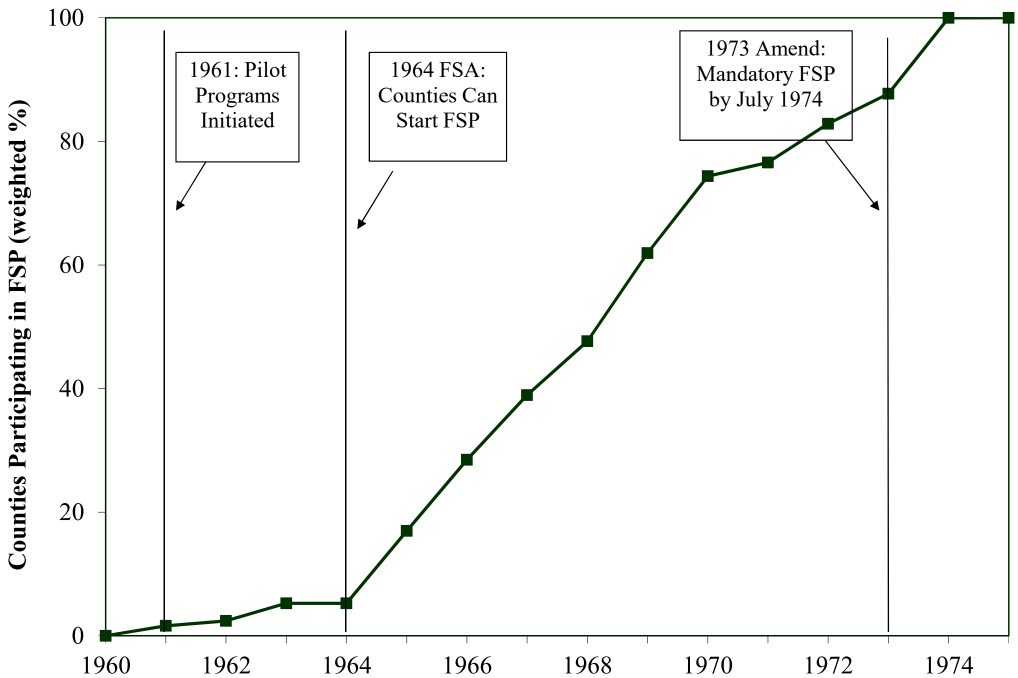
Notes: Each column provides estimates for the exposure model in equation (3). The unit of analysis is at the birth-county x birth-year x birth-month x survey-year level and the coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort would have been exposed 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, survey year, and birth-state x birth-year as well as 1960 county characteristics interacted with a linear trend in year of birth. Column 2 adds a control for the log(population) and Column 3 adds all other controls in Table 1 that are available for 1959-1980 (War on Poverty programs, REIS transfer spending, and mortality). Data limited to those born between 1959 and 1980. Standard errors are clustered by county of birth and indicated in parentheses. See also Table 3 notes.

Table 7: Estimated ITT Effects of Food Stamps Exposure in Early (Conception to Age 5) and Later Childhood (Ages 6 to 18) on Different Indices of Well-Being, Longevity, and 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)
%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

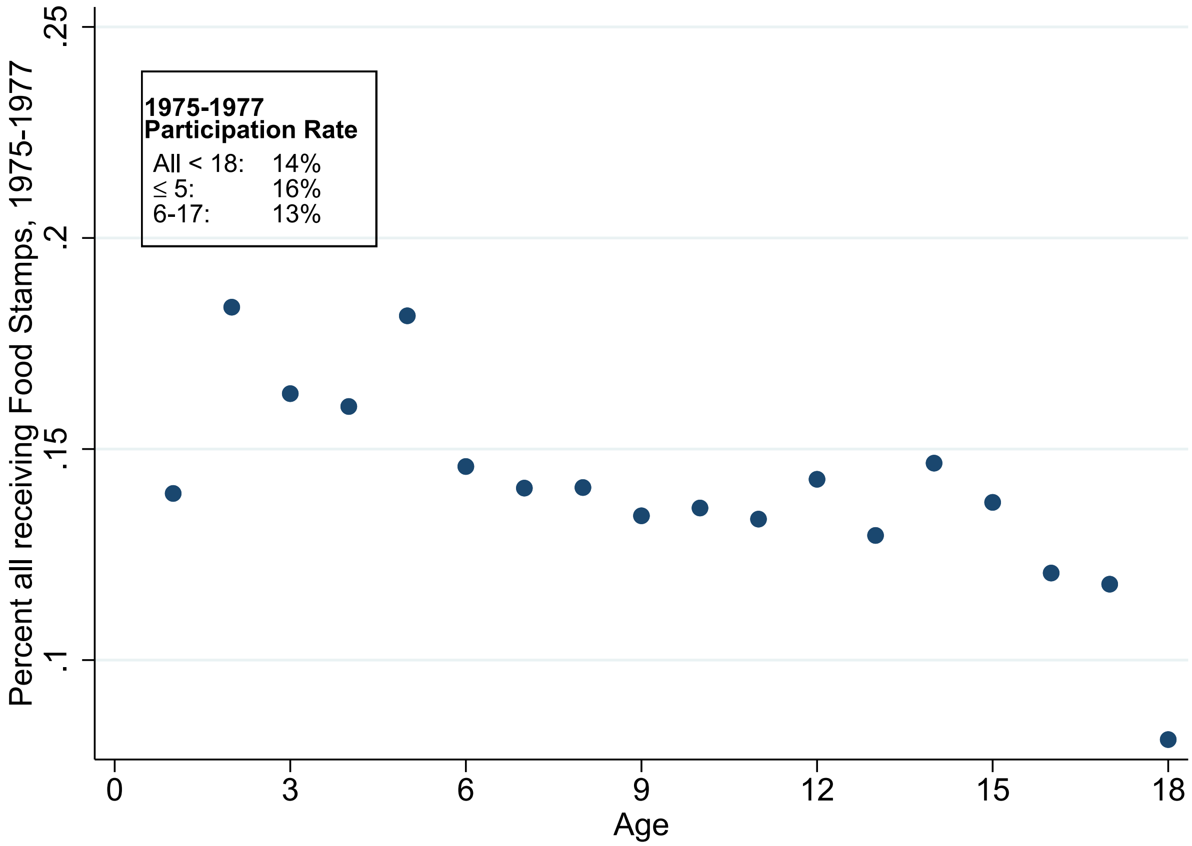
Notes: Each column provides estimates for the exposure model (equation 3) that includes two exposure variables – share of months between conception and age 5 and share of months between ages 6 and 18. See notes to Table 3 for more on model, sample, and data.

Appendix Figure 1: Population Weighed Share of Counties With a Food Stamp Program, by Year



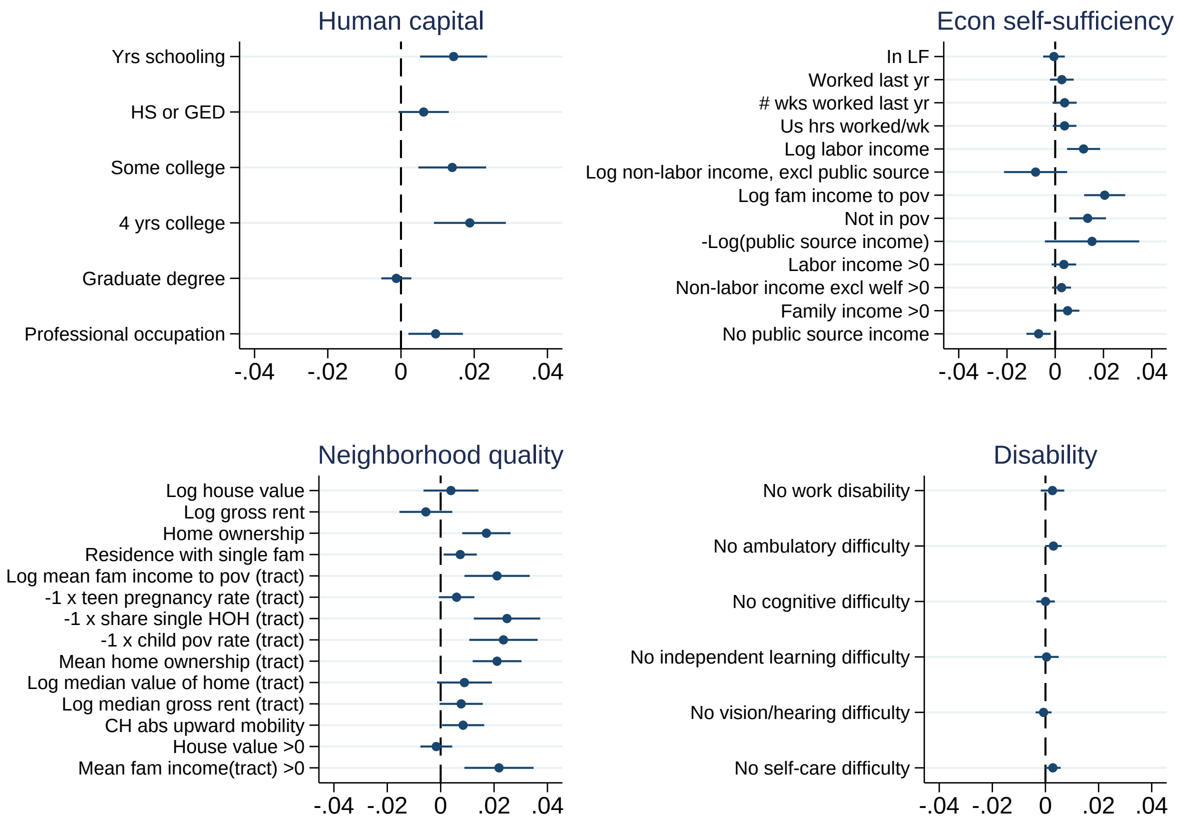
Notes: Hoynes and Schanzenbach (2009) tabulations based on administrative data from the U.S. Department of Agriculture in various years.

Appendix Figure 2: Child Participation Rates in Food Stamps, by Age



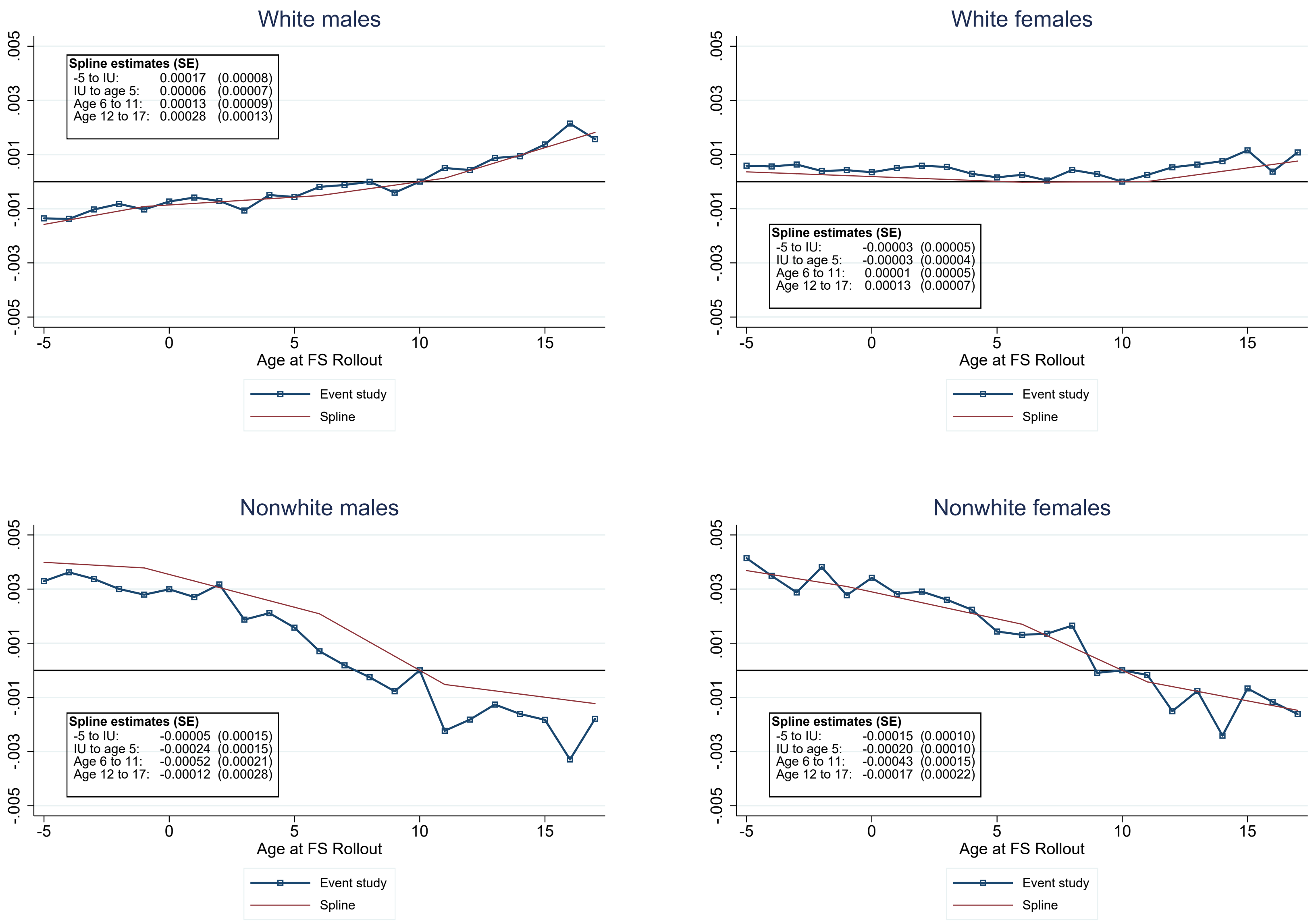
Source: Panel Study of Income Dynamics pooling calendar years 1975-1977. Participation is at the household level, but data is tabulated on a sample of children.

Appendix Figure 3: Exposure Model Estimates of the ITT Effects of Food Stamps for Standardized Sub-Index Components



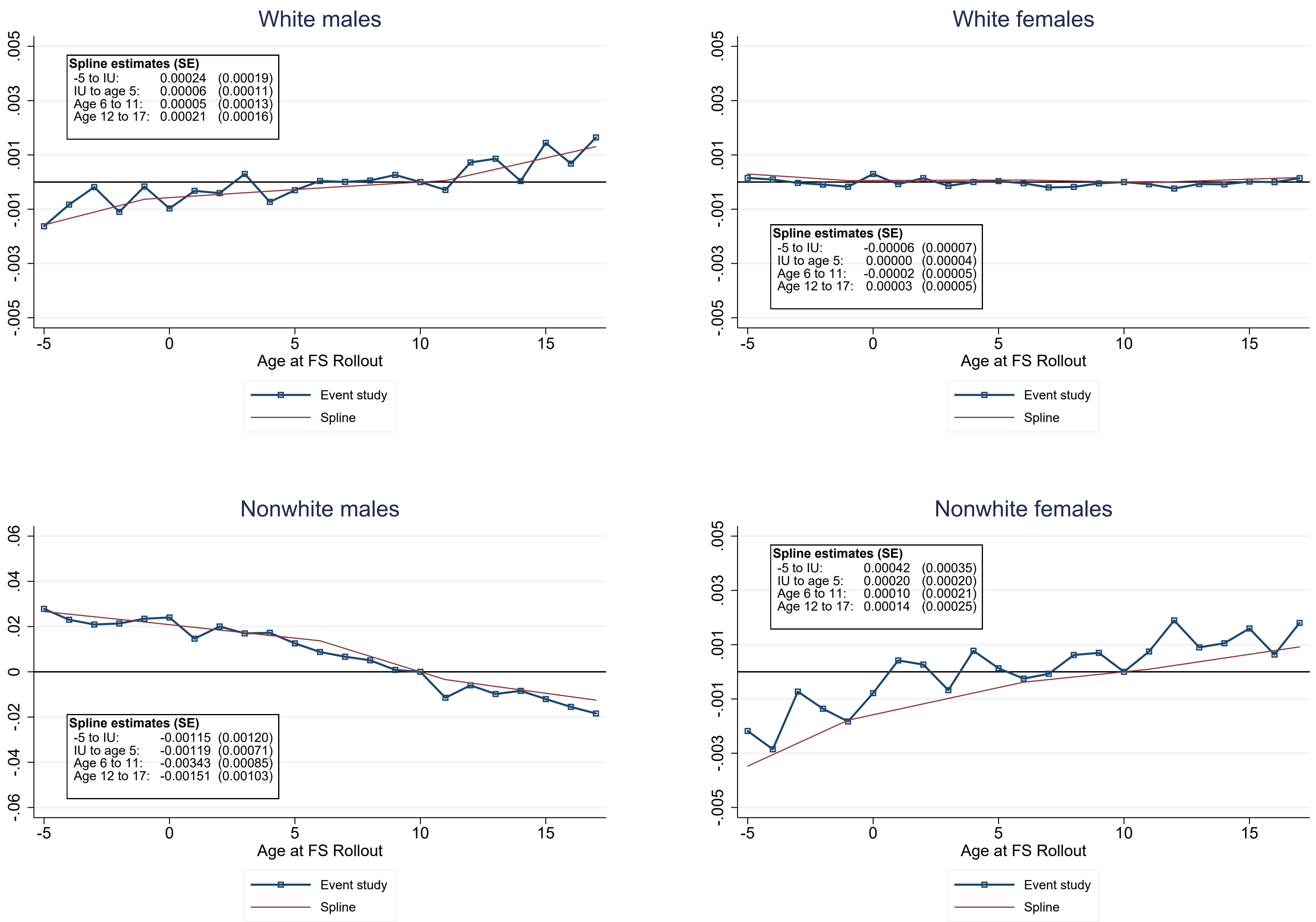
Notes: Each row in each figure provides estimates for the exposure model in equation (3). The unit of analysis is at the birth-county x birth-year x birth-month x survey-year level and the coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort would have been exposed to Food Stamps based on when the program began in the cohort's county of birth. Standard errors are clustered by county of birth and 95 percent confidence intervals are included. Each outcome is a sub-index where each is standardized in terms of standard deviations. Estimated models and samples are identical to Table 3.

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



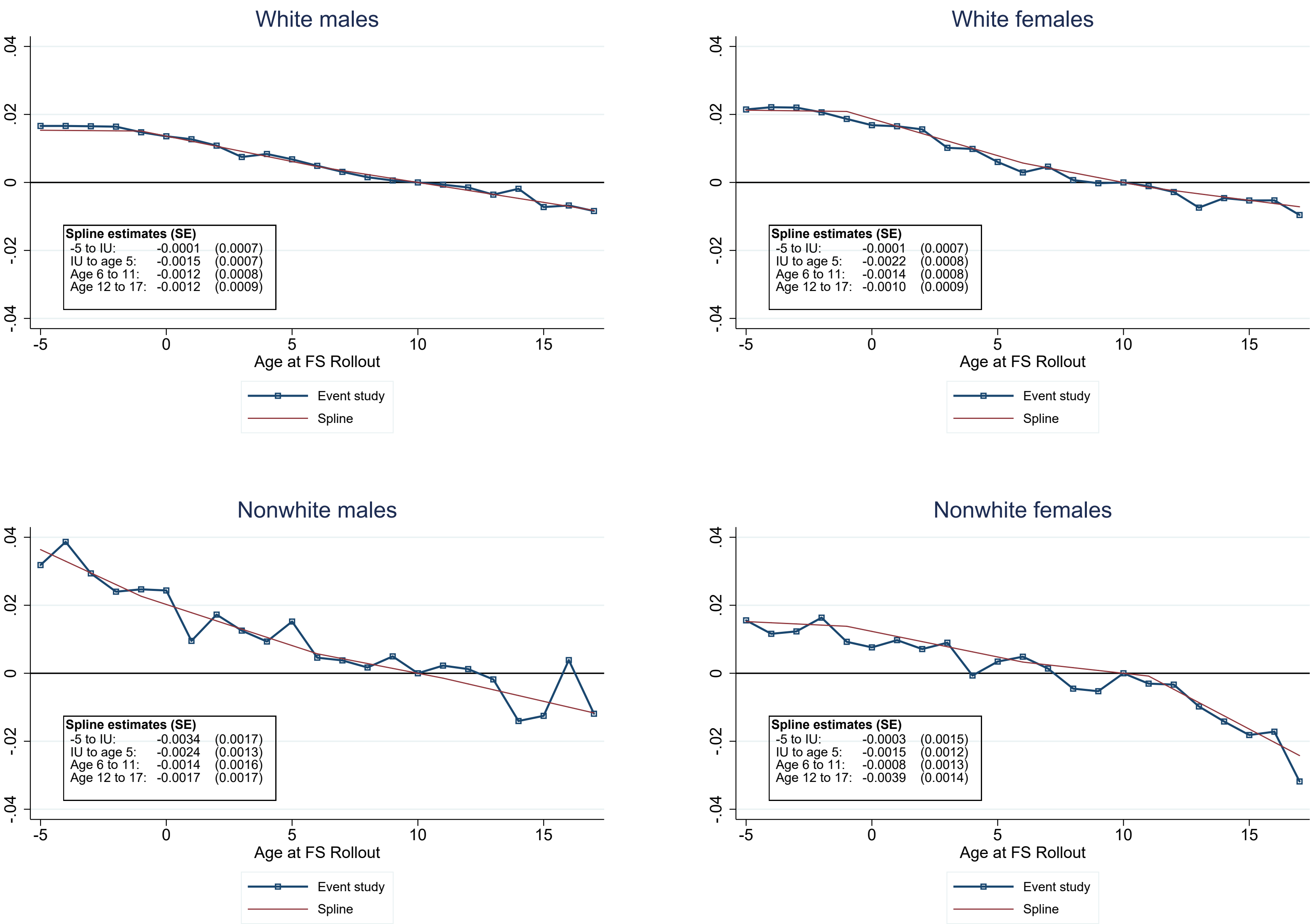
Notes: The panels plot event-study and spline estimates for survival to 2012 using the specifications in equations (1) and (2) separately by race and sex. All models include fixed effects for birth-county, birth-year, survey year, and birth-state x birth-year as well as 1960 county characteristics interacted 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 where we observe place of birth. 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 Incarceration by a Cohort's Age when the Program Launched, by Sex and Race



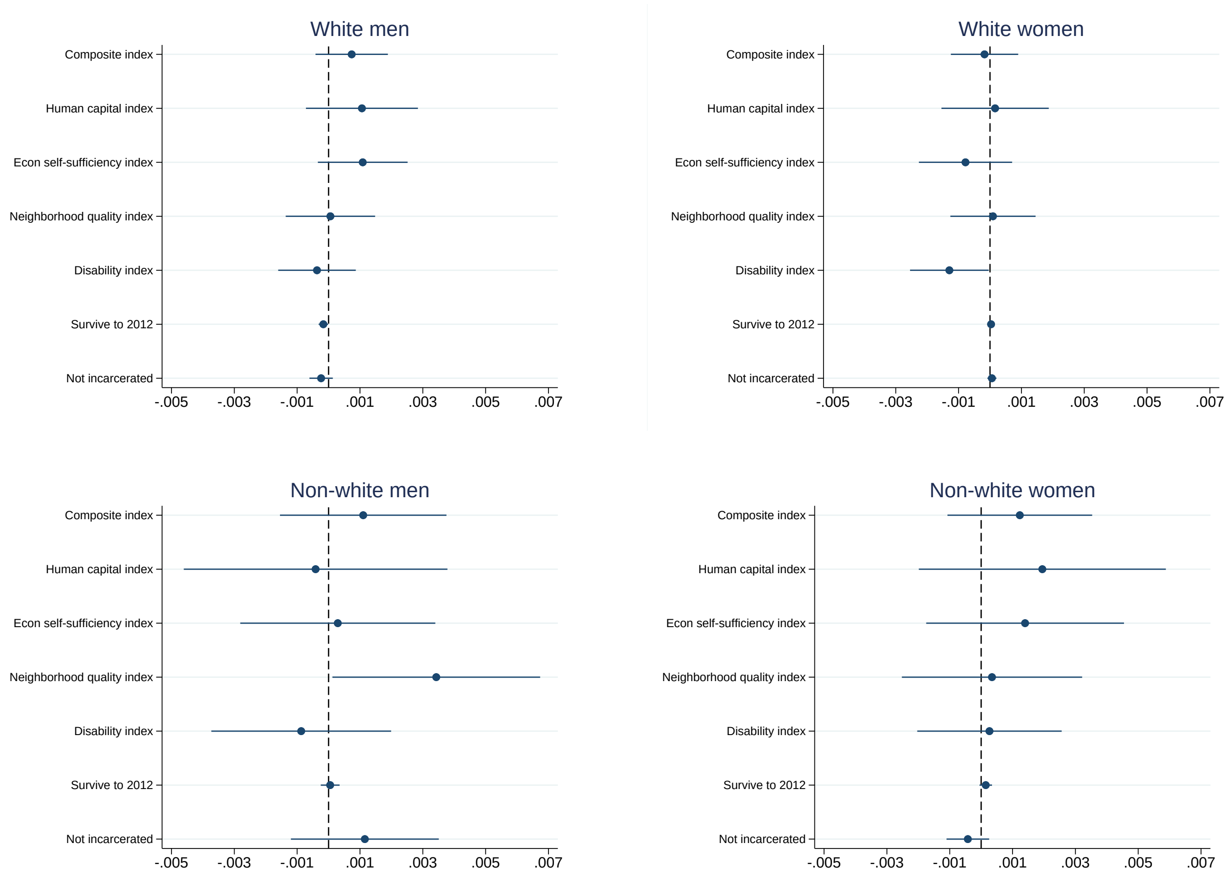
Notes: The panels plot event-study and spline estimates for survival to 2012 in the 2006-2013 ACS using the specifications in equations (1) and (2) separately by race and sex. All models include fixed effects for birth-county, birth-year, survey year, and birth-state x birth-year as well as 1960 county 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 6: Event–Study and Spline Estimates of the ITT Effects of Food Stamps Exposure on Neighborhood Quality by a Cohort's Age when the Program Launched, by Sex and Race



Notes: The panels plot event-study and spline estimates for 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, birth-year, survey year, and birth-state x 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 7: Spline Summary Estimates on the Pre-Trend of the ITT Effects of Food Stamps for Different Indices of Well-Being, Longevity, and Incarceration, by Race and Sex



The panels plot–for different indices and subgroups—the estimates on the pre-trend linear splines (ω_1 covering ages -5 to -2) in equations (2). 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 in percentage point units. 95-percent confidence intervals are provided. See Figure 5 for more on sample, specification and data.

Appendix Table 1: Mean of the Outcomes for Adult-Wellbeing, All and by Race and Gender

	All	White males	White females	Nonwhite males	Nonwhite females
Yrs schooling	13.760	13.750	13.910	13.140	13.370
HS or GED	0.930	0.928	0.949	0.866	0.886
Some college	0.665	0.648	0.700	0.561	0.635
College	0.328	0.332	0.350	0.227	0.256
Graduate	0.031	0.038	0.026	0.026	0.022
Professional occ	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 year	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
Labor income > 0	0.871	0.939	0.819	0.860	0.799
Log non-labor PI	7.355	7.221	7.393	7.641	7.753
Log SF income/pov	5.851	5.943	5.865	5.622	5.427
SF income > 0	0.975	0.982	0.974	0.962	0.954
Not in pov	0.903	0.934	0.904	0.849	0.782
Log PA income	-9.039	-9.177	-9.037	-9.001	-8.738
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 SF income/pov (tract)	5.891	5.920	5.926	5.689	5.656
Log mean SF income/pov (tract) > 0	0.941	0.944	0.944	0.919	0.916
-1 x teen pregnancy (tract)	-0.040	-0.037	-0.036	-0.059	-0.062
-1 x child pov rate (tract)	-0.436	-0.422	-0.420	-0.519	-0.531
Mean home ownership (tract)	0.742	0.758	0.762	0.641	0.632
Log median home value (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
No work disability	0.914	0.918	0.927	0.859	0.868
No ambulatory disability	0.950	0.956	0.952	0.933	0.924
No cognitive disability	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
Composite index	0.016	0.008	0.008	0.009	0.014
Human capital index	0.012	0.001	0.008	0.007	0.021
Economic self-sufficiency index	0.036	0.036	0.027	0.032	0.034
Neighborhood quality index	0.000	-0.013	-0.010	-0.014	-0.013
Physical disability index	0.024	0.017	0.027	0.008	0.021
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	3000	3000	3000	2900	2900

Notes: The table provides means of each of the outcome variables reported in the paper, for all and by race and gender. Subindex values are unnormalized; indices are normalized. For details on sample and data, see Table 3.

Appendix Table 2: Exposure Model Estimates of the ITT Effects of Food Stamps for Sub-Index Components, All and by Race and Gender

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 or GED	0.0016	0.0007	0.0005	0.0003	-0.0013
	(0.0009)	(0.0009)	(0.0009)	(0.0032)	(0.0025)
Some college	0.0067	0.0070	0.0058	0.0010	0.0006
	(0.0023)	(0.0025)	(0.0021)	(0.0049)	(0.0048)
College	0.0088	0.0091	0.0081	0.0030	0.0020
	(0.0023)	(0.0026)	(0.0021)	(0.0043)	(0.0037)
Graduate	-0.0002	0.0001	-0.0006	0.0013	-0.0009
	(0.0004)	(0.0006)	(0.0004)	(0.0015)	(0.0013)
Professional occ	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 year	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)
Labor income > 0	0.0013	0.0005	-0.0031	0.0058	0.0037
	(0.0009)	(0.0008)	(0.0019)	(0.0029)	(0.0035)
Log non-labor PI	-0.0176	-0.0088	-0.0120	0.0415	-0.0353
	(0.0145)	(0.0158)	(0.0194)	(0.0588)	(0.0303)
Log SF income/pov	0.0182	0.0126	0.0110	0.0019	0.0086
	(0.0039)	(0.0027)	(0.0035)	(0.0084)	(0.0083)
SF income > 0	0.0008	0.0000	0.0004	0.0026	-0.0008
	(0.0004)	(0.0005)	(0.0005)	(0.0015)	(0.0016)
Not in pov	0.0038	0.0006	0.0012	0.0040	0.0013
	(0.0011)	(0.0009)	(0.0010)	(0.0029)	(0.0032)
Log PA income	-0.0138	-0.0062	-0.0076	0.0026	0.0021
	(0.0090)	(0.0146)	(0.0151)	(0.0269)	(0.0221)

Appendix Table 2: (continued)

Title	All	White males	White females	Nonwhite males	Nonwhite females
Neighborhood quality					
Log house value	0.0034	0.0011	0.0035	0.0015	-0.0098
	(0.0047)	(0.0048)	(0.0040)	(0.0099)	(0.0085)
House value > 0	0.0000	0.0000	0.0000	-0.0001	-0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)
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 SF income/pov (tract)	0.0084	0.0036	0.0065	0.0013	0.0020
	(0.0025)	(0.0018)	(0.0019)	(0.0044)	(0.0044)
Log mean SF income/pov (tract) > 0	0.0008	0.0003	0.0003	0.0006	0.0004
	(0.0002)	(0.0002)	(0.0001)	(0.0004)	(0.0004)
-1 x teen pregnancy (tract)	0.0003	0.0001	0.0001	0.0004	-0.0002
	(0.0002)	(0.0002)	(0.0002)	(0.0006)	(0.0005)
-1 x child pov rate (tract)	0.0029	0.0011	0.0020	0.0003	-0.0003
	(0.0007)	(0.0004)	(0.0006)	(0.0012)	(0.0010)
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 home value (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)
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: Each column and row provides estimates for the exposure model in equation (3). The unit of analysis is at the birth-county x birth-year x birth-month x survey-year level and the coefficient is on the exposure variable: the share of months between conception and age 5 that a cohort would have been exposed to Food Stamps based on when the program began in the cohort's county of birth. Standard errors are clustered by county of birth and are in parentheses. Each outcome is an unnormalized sub-index. Estimated models and samples are identical to Table 3.

Appendix Table 3: Spline Estimates of the Estimated ITT Effects of Food Stamps Exposure on Different Indices of Well-being, by Race and Gender

	All	White males	White females	Nonwhite males	Nonwhite females
Panel A: Composite					
Pre-trend: -5 to IU	-0.0007	-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: 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 D: 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 E: 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: See notes for Table 3.