Breaking the Cycle?

The Intergenerational Effects of Head Start*

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Abstract

Despite substantial evidence that resources and outcomes are transmitted across generations, there has been limited inquiry into the extent to which anti-poverty programs actually disrupt this cycle. We explore how Head Start effects transfer across generations. We leverage sibling comparisons and the geographic rollout of the federally funded preschool program to estimate the effect of Head Start exposure among mothers on their children's long-term outcomes. We find evidence of intergenerational transfer of Head Start effects in the form of increased educational attainment and reduced teen pregnancy and criminal engagement in the second generation.

Keywords: early childhood, intergenerational, Head Start, long-term

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1 Introduction

The effects of poverty are pernicious and persistent across generations. Those born to parents in the lowest quintile of the income distribution are twice as likely to end up there as children born to middle-income parents. Similarly, the intergenerational correlations in income, education levels, female headship, receipt of government assistance, and risky behavior are quite high.[1] Family, school, and neighborhood contexts collectively shape children's trajectories and generate correspondence between their parents' outcomes and their own. These linkages are particularly acute for minorities, potentially contributing to the early emergence and persistence of achievement gaps by race/ethnicity.[2]

Societal investments in education may disrupt the transmission of poverty across generations by increasing educational attainment and labor market attachment and decreasing engagement in risky behavior. Early childhood in particular is a critical developmental period and an opportunity for especially effective intervention. Indeed, multiple studies indicate that interventions in the preschool and early school years can have substantial effects on schooling attainment, labor market success, and other measures of health and well-being into adulthood.[3] And yet, we know almost nothing about whether these benefits carry over to the next generation. In other words, do these needs-targeted early childhood programs truly break the cycle of poverty?

We answer this question in the context of the Head Start program, providing the only evidence of which we are aware of the intergenerational effects of an early childhood intervention in the United States. The Head Start program, funded and administered through the U.S. Department

[1] A variety of recent estimates suggest intergenerational correlations in income of 0.3 to 0.6 (Black and Devereux 2011, Chetty, Hendren, Kline, Saez and Turner 2014b, Mazumder 2005, Solon 1999), in education levels of 0.4 to 0.5 (Hertz, Jayasundera, Piraino, Selcuk, Smith and Verashchagina 2008), in female headship of 0.2 (Page 2004), and in welfare use of 0.3 (Page 2004). Similarly, (Duncan, Kalil, Mayer, Tepper and Payne 2005) reviews and provides a variety of evidence indicating positive intergenerational correlations in early pregnancy, drug use, and other measures of delinquent or risky behavior.

[2] While there is some dispute about the magnitude of these gaps, there is consistent evidence that cognitive test-score gaps by race and ethnicity exist at formal school entry and remain throughout the schooling years (Fryer and Levitt 2004, Fryer and Levitt 2006, Murnane, Willett, Bub and McCartney 2006), and while race/ethnicity achievement gaps have narrowed in recent decades, gaps by socioeconomic status are pronounced (Reardon and Portilla 2016).

[3] Long-term evidence from the Abecedarian Project, Perry Preschool Project, Head Start, and the Project STAR class- size reduction intervention all suggest large positive effects on participants (Campbell et al. 2014, Chetty et al. 2011; Deming 2009, Dynarski et al. 2013, Garces et al. 2002, Heckman et al. 2013, Schweinhart et al. 2005).

of Health and Human Services, has been an integral part of the conversation about early childhood intervention for the 50 years of its existence. Easily the largest early childhood education program in the United States, annual Head Start enrollment has grown from 400,000 during the early years of the program to nearly a million participants today. Quasi-experimental evidence indicates that participation in the Head Start program yields important long-term benefits, particularly for early cohorts of program participants (Carneiro and Ginja 2014, Deming 2009, Garces, Thomas and Currie 2002, Ludwig and Miller 2007).[4]

To explore intergenerational spillovers, we leverage two research designs to test whether the effects of Head Start transfer from parents to children. The first approach builds on the previous literature with a family fixed effects design, comparing siblings in the first generation who differ in their Head Start participation in terms of their children's—the second generation's—outcomes. Our second, and preferred, approach capitalizes on differential exposure to Head Start induced by variation in the early rollout of the program. In both approaches, we are interested in effects on the second generation's long-term outcomes including educational attainment, teen pregnancy, and criminal engagement.

We find evidence of intergenerational spillovers using both strategies. While our family fixed effects approach is somewhat underpowered, we find suggestive evidence of reductions in teenage parenthood and criminal behavior and increases in educational attainment among the children of Head Start participants. These results are supported by significant effects of Head Start availability on the same measures. We find a significant impact of Head Start availability, between 0.25 and 0.45 standard deviations, on a summary index of long-term outcomes for the second generation. The legitimacy of the geographic rollout strategy is bolstered by estimates that demonstrate a relationship between our measure of within-county Head Start availability and both self-reported participation and state-level participation rates derived from administrative Head Start enrollment data. Furthermore, Head Start availability does not appear to affect children of individuals unlikely

[4]Estimates of the effect of Head Start on more recent cohorts of participants is less clear. Results of the National Head Start Impact Study, the first large-scale, randomized controlled study of the program showed initial impacts on cognitive and non-cognitive skills for Head Start participants, but these effects faded almost entirely by the first and third grades (Puma, Bell, Cook, Heid, Lopez and et al. 2005, Puma, Bell, Cook, Heid and et al. 2010, Puma, Bell, Cook, Heid, Broene, Jenkins, Mashburn and Downer 2012). However, quasi-experimental evidence indicates meaningful effects on long-term outcomes despite the fade out of test-score impacts (Deming 2009).

to have been eligible for Head Start.

Our findings indicate that societal investments in early childhood education can disrupt the intergenerational transmission of the effects of poverty. Indeed, when comparing children of mothers more or less likely to have grown up in poverty, our estimates suggest that Head Start closes most of the gap in a summary index of long-term outcomes for the second generation.

2 A Path Out of Poverty

There is substantial evidence of the path dependency of socioeconomic status. At each stage from school readiness through adulthood, children from the highest family income quintile are dramatically more likely to obtain benchmarks of lifetime economic success than those from the lowest quintile (Sawhill, Winship and Grannis 2012). While children growing up in disadvantage fall behind at each stage, successful navigation of each stage from early childhood to adolescence translates into increased likelihood of attaining a middle class existence (Sawhill et al. 2012). These effects carry over to the next generation, resulting in high intergenerational correlations in income, educational attainment, and risky behavior.

Evidence on the lack of intergenerational mobility in the United States, coupled with growing income inequality, has led to considerable interest in understanding why the resources, behaviors, and outcomes of parents are so strongly related to those of their children, and furthermore, whether interventions that improve these behaviors and outcomes might carry over to the affected individuals' children (Auten, Gee and Turner 2013, Chetty et al. 2014b, Corak 2013, Lee and Solon 2009). The existing evidence on the collective importance of childhood contexts—families, schools, and neighborhoods—in determining longer-term outcomes suggests that social interventions or programs that intervene in these contexts may be influential (Chetty, Hendren, Kline and Saez 2014a).

Despite the clear importance of this question, the data requirements necessary to answer it convincingly have resulted in limited applications. While we are unaware of any study to focus on the intergenerational effects of an anti-poverty program, several have estimated the intergenerational effects of increases in educational attainment in adolescence and beyond.[5] Increases in college access or attainment have resulted in improved birth outcomes and reduced grade retention in the next generation (Currie and Moretti 2003, Maurin and McNally 2008, Page 2009). The evidence at the middle and high-school levels is more mixed, with positive intergenerational effects of additional schooling generated by compulsory schooling changes in the U.S. and Great Britain in the 1960s and 70s and no effect in Norway (Black, Devereux and Salvanes 2005, Oreopoulos, Page and Stevens 2006, Chevalier 2007). Taken together, the evidence for the intergenerational effects of education is promising. However, we know little about the intergenerational the effects of early childhood programs, despite the large estimated effects of early childhood programs on adult outcomes.[6]

Indeed, there is a substantial body of empirical evidence demonstrating that early childhood programs can generate improvements in participants' life chances over the long-term, and evidence that early skills are important predictors of subsequent academic attainment and labor market success (Chetty, Friedman, Hilger, Saez, Schanzenbach and Yagan 2011, Duncan, Dowsett, Claessens, Magnuson, Huston, Klebanov, Pagani, Feinstein, Engel, Brooks-Gunn, Sexton and Duckworth 2007, Dynarski, Hyman and Schanzenbach 2013). Specifically, evidence from the Abecedarian Project, Perry Preschool Project, Head Start, and the Project STAR class-size reduction intervention collectively suggests that interventions in the preschool and early school years can have substantial effects on schooling attainment, labor market success, and other measures of well-being into adulthood (Chetty et al. 2011, Deming 2009, Schweinhart, Montie, Xiang, Barnett, Belfield and Nores 2005). Recent evidence documents improvements in life chances that include better health, reductions in behavior problems, and higher rates of college-going (Campbell, Conti, Heckman, Moon, Pinto, Pungello and Pan 2014, Carneiro and Ginja 2014, Dynarski et al. 2013).

We contribute to this important conversation by providing some of the first evidence—and the first evidence in a U.S. context, to our knowledge—on whether the effects of early childhood

[5]There are also a handful of papers that examine the intergenerational effects of negative shocks. For example, (Page, Oreopoulos and Stevens 2008) focuses on the intergenerational effects of job loss in Canada.

[6]Rossin-Slater and Wust (2017) explore the intergenerational impact of preschool and a nurse home visiting program in infancy. They find educational attainment effects in the first generation that persist, and are similarly-sized, in the second generation.

programs transfer across generations. The answer to this question has important implications for policies aimed at reducing poverty or socioeconomic gaps in educational attainment, risky behaviors, and relatedly, labor market success. Most importantly, if these types of policies have large spillover effects on the next generation it suggests a concerted effort for a single generation of impoverished youth might break the cycle of poverty and reduce the need to provide similar services to future generations.

2.1 The Evidence on Head Start

The policy discussion often seizes on the Head Start literature as most relevant for current policy considerations around large-scale, publicly provided preschool interventions. The Head Start program was an early component of President Lyndon B. Johnson's War on Poverty, commencing as a summer program in 1965, serving 560,000 children (Vinovskis 2005). It was then quickly expanded to a year-round program. While Head Start today is characterized as an early childhood education program, the initial emphasis was on a variety of "preschool"-related services and supports, including nutrition, vaccinations and health care, dental services, and social development (Vinovskis 2005).

While not thought of in its present-day form as high-quality as the much-touted Perry Preschool and Abecedarian Projects—in part because of it's somewhat lower per child funding levels—Head Start started as a comprehensive attempt to aid poor children. In the early years of the program, the mission of Head Start was characterized as "providing the children of the poor with an equal opportunity to develop their full potential" (of Child Development 1970). To this end, centers provided medical and dental care, nutritional services, parent involvement activities, employment and training to the disadvantaged, linkages to social services, and mobilization of community resources in addition to providing programming to foster children's social-emotional and cognitive development.[7]

The Head Start program served a decidedly disadvantaged population in the early years of the program. The median family income was less than half that of all families in the U.S. and approxi-

[7]Depending on the year, 40 to 50 percent of families indicated that their child's Head Start medical examination uncovered a problem, and of those found to have an issue, 65 to 70 percent received treatment.

mately 50 percent of early full-year program participants were black children (of Child Development 1968). In the early years of the program, between nine and 17 percent of families reported having no running water inside the home. Only five percent of mothers reported some postsecondary schooling or more, with approximately 25 percent indicating that they graduated from high school, and 65 to 70 percent of mothers with less than a high school education. Approximately 25 percent lived in female-headed households and between 65 and 70 percent of participating children's mothers were unemployed (of Child Development 1968).

From the earliest years of Head Start implementation and evaluation, there has been considerable controversy about the program's effectiveness culminating in recent debate about the first national, experimental study of Head Start. While the Head Start Impact Study (HSIS) found initial positive effects on cognitive skill for participants in the mid-2000s, there were no persistent effects at first and third grade follow-ups (Puma et al. 2005, Puma et al. 2010, Puma et al. 2012). Re-analyses of the HSIS data suggest a more nuanced picture (Montialoux 2016). These analyses revealed that there is considerable variation in impact by center (Walters 2015), that effects are most pronounced among children who would otherwise be in parental or relative care (Kline and Walters 2016), and that Hispanic children and children with low skills at program entry experience the greatest benefit (Bitler, Hoynes and Domina 2014).

In contrast to the HSIS debate, prior quasi-experimental studies suggest Head Start has had important long-term effects for cohorts of children who participated from the late 1960s through the 1980s. Leveraging sibling comparisons and discontinuities in grant-writing assistance and program eligibility, studies have documented increased educational attainment, better health, higher earnings, and less involvement in risky behaviors (Carneiro and Ginja 2014, Deming 2009, Garces et al. 2002, Ludwig and Miller 2007), even in the presence of short-term test-score fadeout (Deming 2009). A recent follow-up to Deming's study looks at even longer-term outcomes and finds persistence of effects later into adulthood, including impact on participants' later-life parenting practices (Bauer and Schanzenbach 2016).

To build on the existing evidence of Head Start effectiveness, we capitalize on plausibly random exposure to the early rollout of the Head Start program across siblings and over geography and birth cohorts (Figure 1). The latter strategy, leveraging the introduction of Head Start across counties and over time to explore the impact of program availability and exposure, has been employed in looking at the impact of other War on Poverty and poverty reduction programs, including the Food Stamp Program (Almond, Hoynes and Schanzenbach 2011), the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) (Hoynes, Page and Stevens 2011), and Community Health Centers (Bailey and Goodman-Bacon 2015).[8]

3 Data

To explore Head Start's intergenerational effects we rely on rich, longitudinal survey data that connect mothers and their children. The National Longitudinal Survey of Youth–1979 Cohort (NLSY79) is a nationally representative sample of adolescents who were 14 to 22 years old when they were first surveyed in 1979. The survey follows 12,686 young men and women, with annual interviews through 1994 and biennial interviews continuing since then. In addition to rich data on labor market participation and transitions, the data provide extensive information on education and training, health, mobility, and family formation. The data facilitates analysis on a large, representative sample of American men and women born in the 1950s and 1960s, and living in the United States in 1979. The timing is particularly advantageous for the purposes of this study as individuals born in the late 1950s and 1960s are differentially exposed to Head Start via its introduction and rollout in the mid to late 1960s.

Beginning in 1986, a separate, related survey of all children born to NLSY79 female respondents has been collected, the NLSY79 Children and Young Adults Surveys (CNLSY). In addition to all the mother's information from the NLSY79, the child survey includes direct information for each child collected from either the mother or child depending on age. The survey gathers data on children's schooling and training, labor market experiences, health, and engagement in risky behaviors. The CNLSY allows us to explore intergenerational effects of the mother's Head Start exposure.

[8]Two new papers use the early introduction of Head Start over geography and time to explore impact on first-generation outcomes; preliminary evidence in both cases suggests sizable, long-term impact for participants (Johnson and Jackson 2017, Thompson 2017), particularly when coupled with subsequent schooling investment (Johnson and Jackson 2017).

In addition to the NLSY surveys, we use county-by-year data from the Community Action Programs (CAP) and Federal Outlays System (FOS) files available from the National Archives and Records Administration for Head Start availability in fiscal years 1966–1968 (see Data Appendix for details). We aggregate data on Head Start grant recipients to the county level, and code a birth cohort and county pair as "exposed" to Head Start if it received per four-year old Head Start funding above the tenth percentile ($22 per four-year old in the county).[9] We do not otherwise leverage data on appropriated dollar amount because of concerns about the accuracy of the recorded funding amounts in the early years of the Head Start program as well as the endogeneity of funding levels.

The outcomes of interest are important, longer-term measures of the potential impact of Head Start on the next generation. We consider the second generation's long-term outcomes both because these outcomes are most important in assessing whether the intergenerational transmission of program effects disrupts the cycle of poverty and because there are myriad ways in which a mother's Head Start access may affect her children's outcomes. These pathways are likely cumulative across childhood. From changes in parenting practices and greater likelihood of enrolling one's child in early childhood programming to heightened expectations and spillovers from a mother's own increased human capital and income, we would expect the channels of impact on the second generation to accumulate over the childhood years. There are two positive outcomes: high school completion (including GED receipt) and college going, or attending college for any period of time. Because of findings in prior literature, we also consider two negative outcomes with important implications for children and teens' life changes: teen pregnancy and interaction with the criminal justice system (as measured by any arrests, convictions, or probations). These outcomes are important in capturing the second generation's private returns, but also have implications for measuring the broader societal benefits of the program.

The top panel of Table 1 contains summary statistics for these outcomes. The first column provides these measures for the full sample used for our family FE strategy. The second column provides analogous measures for the children of sisters who vary in their participation in Head

[9] In other words, we classify county birth cohorts with very low funding levels as unexposed. Our results are qualitatively similar using $0 as the cutoff.

Start. As seen from the statistics, the children of sisters who vary in their participation in Head Start are negatively selected relative to the population. They have higher rates of teen parenthood (27% vs. 19%) and interaction with the criminal justice system (33% vs. 29%), and lower rates of high school completion (78% vs. 82%) and college going (50% vs. 58%).

Columns (3) and (4) contain similar measures for the samples underlying our geographic roll-out strategy. Because of low levels of maternal educational attainment among early Head Start participants, we focus our analyses on NLSY79 mothers whose mother did not finish high school (column (2)).[10] We refer to this as our high impact sample because close to 70 percent of participants' mothers had less than a high-school degree, implying participation rates of close to 60 percent in counties with Head Start availability.[11] The summary statistics for children in this group are broadly similar to the sister variation sample. In column (3), we restrict the sample to NLSY79 mothers whose mother completed at most a high school degree. We call this our low impact sample as we estimate participation rates of only 30 percent. As might be expected, summary statistics for children in this group have somewhat better outcomes.

We present analogous statistics for maternal outcomes (i.e., NLSY79 respondents) in the bottom panel of Table 1. While rates of teen parenthood are substantially higher and rates of college going are substantially lower, the pattern of statistics across samples is similar.[12]

4 Estimation and Empirical Results

To account for individual selection into Head Start, we use two approaches to obtain estimates of program impact on the subsequent generation. The first approach leverages variation within families in Head Start participation of female participants in the NLSY79. The second, and preferred, approach relies on variation within counties over time in the availability of the Head Start program. This variation is generated by the rollout of the Head Start program during the mid-1960s. We use both approaches to explore how a mother's exposure to the Head Start program affects the adult

[10]Roughly 65 to 70 percent of the mothers of early participants reported less than a high school education, approximately 25 percent indicated that they graduated from high school, and only five percent of mothers reported some postsecondary schooling or more.

[11]See Data Appendix for details of these calculations.

[12]Similar measures of interaction with the criminal justice system are unavailable for this sample.

outcomes of her children.

4.1 Family Fixed Effects Approach

We begin with a family fixed effects model where we compare the outcomes of children of sisters who differ in their Head Start participation. This model is very similar to that used previously to explore effects of Head Start on the participants (Deming 2009, Garces et al. 2002, Currie and Thomas 1995). The basic specification is:

$$y_{ij} = \alpha + \beta X_i + \theta HeadStart_i + \gamma_j + \varepsilon_{ij} \tag{1}$$

where y_{ij} is an adult outcome for a child; X_{it} includes controls for the child's sex and age as well as the mother's birth year and birth order; and γ_j are family fixed effects. $HeadStart_i$ captures participation in Head Start as a binary variable, turning on for the affected sister within families. Robust standard errors are clustered on the mother's 1979 household. We are interested in identifying θ, the effect of Head Start participation on the adult outcomes of the participants' children. The key identifying assumption is that conditional on family fixed effects, cohort fixed effects, and birth order controls, Head Start participation is uncorrelated with other factors that would lead a woman?s children to have better or worse outcomes. While this is a strong assumption, we believe it is more likely to hold in our setting given the large changes within counties during this time period in the availability of Head Start.

Our baseline results in Table 2 indicate a significant (0.26 sd) improvement in the summary index of adult outcomes of a mother's children when the mother reports having participated in Head Start as a child. While the estimates for the component outcomes are not statistically different from zero, the magnitude and direction of all of the coefficients suggest improvements in outcomes, with a 3 percentage point reduction in the share of children who become parents as teenagers, a 9 percentage point reduction in criminal behavior, and 7 percentage point increases in the share of individuals who graduate from high-school and attend college.

As with all studies that leverage family fixed effect designs, there are questions about what generates variation within a family in program participation (or reported participation). For example, it is possible that the parents of NLSY79 participants placed greater focus on the education or care of one child versus another, perhaps as a result of changing financial security or perceived differences in aptitude. This would result in overestimates of the effect of Head Start. On the other hand, Head Start eligibility depended, and continues to depend, largely on family resources; at least 90 percent of Head Start participants served at each program site had to be from families below the federal poverty line. This constraint resulted in within family variation in eligibility that was correlated with family resources. In other words, when comparing a sibling who participated with one who did not, the one who participated is more likely to have grown up in poverty than the one who did not participate. We would expect this to bias our estimates of the effect of Head Start participation down. A second concern with our family fixed effects approach is the measurement of Head Start participation, which comes from retrospective responses of NLSY79 sample members in 1995, a minimum of 25 years after their potential participation in the program. Our preferred strategy addresses both of these concerns, using plausibly exogenous variation in Head Start availability.

4.2 Geographic Variation in Head Start Availability Over Time

We leverage the rollout of the Head Start program during the mid-1960s to identify the effect of program availability within a mother's county of birth on the adult outcomes of her children. As between 65 and 70 percent of the mothers of early Head Start participants did not complete high school (Barnow and Cain 1977, of Child Development 1968), we focus our analyses on this "high impact" population. Our basic specification is:

$$y_{ict} = \beta_0 + \beta_1 X_i + \beta_2 HSavail_{ct} + \gamma_c + \lambda_t + \varepsilon_{ict} \tag{2}$$

where y_{ict} is an adult outcome for a child; X_{it} includes controls for the child's sex and age as well as the mother's birth order and race; and γ_c and λ_t are county of birth and birth year fixed effects. $HSavail_i$ indicates whether Head Start was available for a mother in a particular birth cohort t and birth county c. $HSavail_{ct}$ is set to one for a mother when there is a non-trivial level of Head Start funding in that mother's birth county four or five years after her year of birth.[13]

Table A1 illustrates that, conditional on county of birth and birth year fixed effects, our measure of Head Start availability predicts self-reported Head Start participation and state-level participation rates derived from administrative Head Start enrollment data. The top panel presents estimates of the effect of Head Start availability on self-reported participation. When a program is available in a county four or five years after a mother's year of birth, the mother in our high impact sample is 10 percentage points more likely to report having participated in Head Start as a child. Given the self-reported and retrospective nature of the Head Start participation variable, we expect there is considerable misreporting; therefore, we do not interpret these estimates as a first stage.[14] Nevertheless, the positive relationship between program availability and self-reported participation supports our research design. The middle panel contains estimates using state-level variation in participation rates in 1966.[15] The estimates suggest close to a 30 percentage point increase in the likelihood of participation for individuals in our high impact sample. The table also contains implied participation rates, assuming that all participation occurred in counties with Head Start availability. These estimates provide an upper bound for the effect of Head Start availability of nearly 60 percentage points for our high impact sample, and 30 percentage points in our low impact sample.

Our baseline results are contained in Table 3. The first row contains estimates from our high impact sample, restricted to grandchildren with grandmothers with less than a high school degree. We observe a large positive effect (0.47 sd) of a mother's Head Start availability on our index of adult outcomes for her children. This effect is driven by significant reductions in teen parenthood (8 percentage points) and criminal behavior (15 percentage points) and increases in high school graduation (13 percentage points) and college enrollment (17 percentage points). The second row presents estimates from our low impact sample. As expected given their lower levels of Head Start

[13]Explicitly, $HSavail_{ct}$ is set to one when the level of Head Start funding within a county exceeds the 10th percentile of observed funding per four year old (roughly $22). The results are qualitatively similar using a cutoff of $0.

[14]Given that underreporting is almost necessarily positively correlated with program availability, we expect that the measurement error attenuates the relationship between Head Start availability and self-reported Head Start participation.

[15]Disaggregated state-level enrollment numbers are not available in other years during this time period. The Data Appendix contains the details of these calculations.

participation and disadvantage, the effects are smaller for individuals in this group: a 0.22 sd increase on our index of adult outcomes.

4.2.1 Threats to Internal Validity

To interpret these estimates as the causal effect of Head Start availability, it must be the case that the availability of a Head Start program is, conditional on county and year of birth fixed effects, unrelated to other factors that would affect the outcomes of children born to women who did and did not have the program available. For example, one concern would be that the type of woman who became a mother or the type of woman included in the sample (due to non-response or our sample restrictions) was affected by the availability of a Head Start program in early childhood. To check for this we can examine how Head Start availability predicts maternal background characteristics that are unlikely to have been affected by Head Start directly (columns (1)-(5) of Table A4). We do this exercise separately for the full sample and the two restricted samples we use for our rollout analyses. There is little relationship between maternal characteristics (race, maternal birth order, 1978 household poverty status) and Head Start availability. Similarly, there is no evidence that the education levels of the grandmother, which we use to focus our sample, are affected by Head Start availability. In column (6) and (7), we present analogous estimates focused on second generation characteristics unlikely to be affected by Head Start: the age and gender of the child. While there is no relationship with child age, Head Start availability is correlated with child gender in our high impact sample. Given the general balance of observables across samples and characteristics we are not particularly concerned by this difference, but as a further check we explore how our treatment estimates are affected by the inclusion of covariates. As expected from the balance of observable characteristics, our estimates are robust to the exclusion of covariates, supporting the argument that the availability of Head Start is conditionally exogenous.

A second concern is that of endogenous program adoption or pre-existing positive trends in outcomes in counties that adopted a Head Start program. The presence of meaningful pre-trends might reflect general improvements in early childhood conditions or programs that were correlated with Head Start availability. While the relatively tight window of analysis limits concerns related to pre-existing trends, we also probe the robustness of our estimates to the inclusion of differential trends by birth county (Appendix Table 5). Allowing differential birth cohort trends interacted with baseline (1960) county characteristics, or even county-specific trends, does little to change our point estimates. The estimates are similarly robust to the inclusion of more specific county-cohort controls for spending on War on Poverty programs and state by birth cohort fixed effects, which flexibly control for changes over time within states that could affect maternal outcomes.

We further address endogeneity concerns related to the availability of the Head Start program using a placebo exercise. In Table 6, we explore the effect of Head Start availability on the children of a group of individuals who are largely ineligible for the program. Specifically, we run our basic specification, restricting the sample to women whose mothers obtained at least a high school degree. Only a small fraction of women in this group were eligible for or participated in Head Start.[16] If something other than Head Start availability is driving our main results, we might expect to see similar affects show up for the children of women in this group. Table 6 illustrates that the point estimates for this group are small, frequently opposite-signed, and non-significantly different from zero across all outcomes.

4.2.2 Effect Size and Heterogeneity

As with much research estimating effects of early childhood programs, the effects are quite large (Deming 2009, Garces et al. 2002, Heckman, Pinto and Savelyev 2013, Johnson and Jackson 2017). Furthermore, where comparable, the effects on the second generation are as large (and sometimes larger) than effects on the first generation. While there are few benchmarks for comparison, this high intergenerational correlation in effects is consistent with some recent findings (Rossin-Slater and Wust 2016).[17]

Notably, other evidence on the long-term effects of similarly timed programs, including Head Start and other early childhood programs, has documented comparably large long-term impacts.

[16] We estimate that participation rates in this group were at most 1/5 of those in our high impact sample. See the Data Appendix for additional details.

[17] Rossin-Slater and Wust (2016) show that the children of women with access to a government-approved preschool are 1.2 percentage points more likely to have more than a compulsory education by age 25. This effect is similar in magnitude to the effect in the first-generation (1.3 percentage points).

To put our results in the context of recent literature with similar outcome measures, our implied TOT effects on high school graduation (21-25 percentage points) for the second generation are similar to those estimated for participants in the Perry Preschool program (20 percentage points), those estimated previously for Head Start (20 percentage points for white participants), and about a third of the size of those estimated for Food Stamp usage in families with young children (74 percentage points).[18] While it is difficult to construct comparable measures for criminal behavior, our implied TOT effects on criminal behavior (26 and 18 percentage points) are larger than the effects on somewhat similar measures reported in evaluations of the Perry Preschool program (12 percentage points on arrest by age 27, 19 percentage points on 5 or more arrests by age 27) and Head Start (12 percentage points for blacks). However, given power limitations in all of these studies, the confidence intervals are overlapping. Furthermore, Perry Preschool enrolled a very particular type of student, extremely disadvantaged, black children in Ypsilanti, Michigan. While statistically indistinguishable, if we use our estimates for the children of black mothers, our implied TOT estimates are roughly half the size of the estimates from the Perry Preschool evaluations (Table 4).[19]

Of course it may not be reasonable to convert our estimates to TOT effects as there may be important spillover effects of program participation; indeed, it is not difficult to imagine that improving the trajectories of a large share of a group results in improvements for the group as a whole that are substantially larger than what we might expect to see if an individual was treated in isolation (as in the Perry Preschool experiment where only 58 children were offered a place in the program). To the extent that subsequent schooling quality or productivity increased because a share of each school-entry cohort was more prepared, these sorts of spillovers could occur.

These effects show up across subgroups (Table 4), with somewhat larger effects on crime and somewhat smaller effects on teen parenthood for male children, perhaps as a result of the higher rate of crime and lower rate of reported teen parenthood for this group.[20]

[18]Our estimates are also relatively similar to (Johnson and Jackson 2017) who find that Head Start availability leads to an 8 percentage point increase in the likelihood of finishing high-school for likely participants, quite similar to our first-generation estimates (Table 7).

[19]This is due to the higher participation rates for black individuals as well as the somewhat smaller point estimates.

[20]Table A3 provides analogous estimates for the family fixed effects design. The pattern of results is similar.

5 Discussion and Conclusion

Research and policy attention frequently focus on how to level the playing field for those born into poverty. We focus instead on whether such interventions truly break the cycle of bad outcomes. While there is increasing interest among researchers and policymakers in understanding the intergenerational spillovers of particular policies and interventions, there exist very few contexts in which these questions can be tested empirically; this is particularly true for early childhood interventions, an area of increasing focus and investment. The federal Head Start program provides a context in which data availability and the time horizon since first implementation facilitate such an empirical exploration, allowing us to contribute the first evidence in the United States context on the intergenerational transmission of early childhood intervention effects.

We find consistent evidence that Head Start participation and exposure in the earliest years of the program transferred across generations in the form of improved long-term outcomes for the second generation. The pattern of results suggests decreases in teen parenthood and criminal engagement and increases in educational attainment across empirical approaches, with particularly pronounced effects for male children and in the south. The effects are large in magnitude, but broadly consistent with the positive first-generation effect sizes found in evaluations of similar early childhood programs that provided an array of services to disadvantaged youth.[21] Furthermore, because of the large scale of Head Start, the program likely provided benefits beyond the direct effect on participants.

Indeed, the availability of Head Start, at least during the early years of the program, appears to have been quite successful at breaking the cycle of poor outcomes for disadvantaged families. Head Start access closes most of the gap in outcomes between individuals with more and less advantaged grandmothers (as proxied by education or household poverty status in 1978). These results imply that cost-benefit analyses of Head Start and similar early childhood interventions underestimate the benefits of such programs by ignoring the transmission of positive effects across generations. This has an important policy implication: if we are going to spend money on early childhood programs,

[21] Recall that the programs provide medical and dental care in addition to engagement of parents of the participants, and provided linkages to other social services in service of an extremely disadvantaged population of children and families.

particularly those targeted at the poor, we should invest now as the benefits will transfer across generations.

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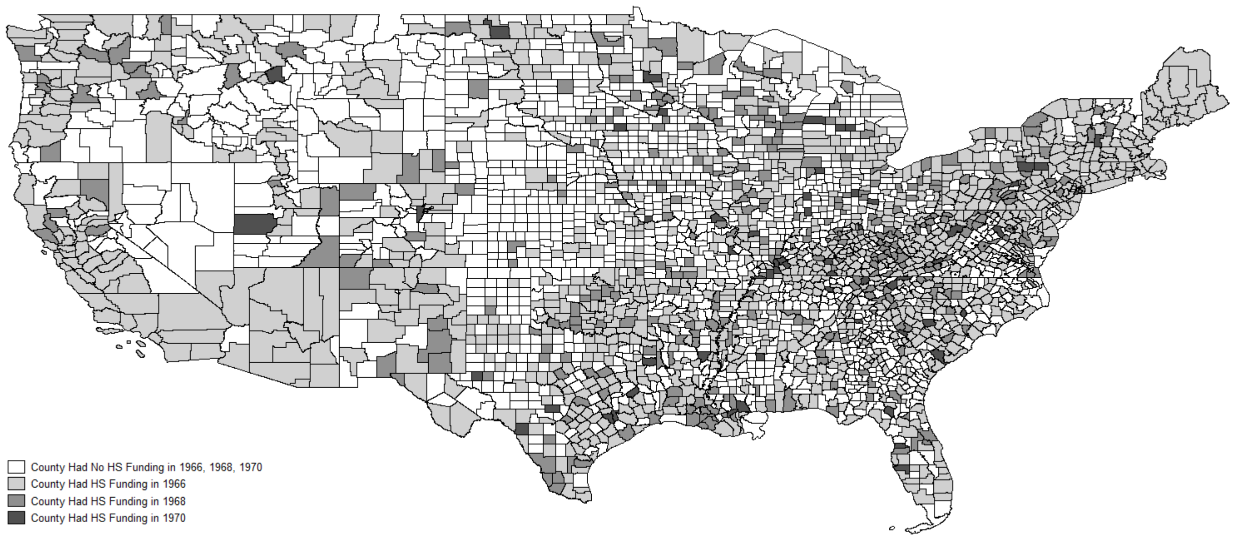
Figures

Figure 1: Early Geographic Expansions of Head Start



Source: National Archives and Records Administration.

Tables

Table 1: Descriptive Statistics of NLSY 79 Women and Their Children

Sample	(1) Family FE (Full Sample)	(2) Family FE (Sister Variation)	(3) Rollout (Grandmother <HS)	(4) Rollout (Grandmother ≤HS)
Second Generation (Child) Outcomes				
Teen Parent	0.19	0.27	0.22	0.19
Crime	0.29	0.33	0.31	0.30
High School	0.82	0.78	0.78	0.82
Some College	0.58	0.50	0.51	0.56
Index	0.00	-0.19	-0.12	-0.01
Observations	3533	473	1687	2732
First Generation (Mother) Outcomes				
Teen Mom	0.32	0.47	0.52	0.43
High School	0.86	0.83	0.81	0.84
Some College	0.47	0.43	0.29	0.37
High Grade	13.2	12.9	12.3	12.7
Income	46919	33963	34534	40123
Observations	2398	271	821	1398

Note: The bottom panel presents sample means for women in the NLSY 79. The top panel provides sample means for the children of these women, restricted to individuals over 20 in 2012. Eachcolumn provides sample means for a different sample, corresponding to our research design. Column (1) provides sample means for sisters in the NLSY 79 and their children. Columns (2) and (3) provide sample means for women in the NLSY 79 and their children, with the sample restricted based on the education level of the mother of the NLSY 79 participant (i.e., the grandmother of the children). Column (2) is restricted to participants in the NLSY 79 whose mothers dropped out of high school. Column (3) is restricted to to participants in the NLSY 79 whose mothers finished high school but received no additional education.

Table 2: Family FEs: Effect of Head Start Participation on Next Generation Outcomes

VARIABLES	(1) Teen Parent	(2) Crime	(3) High School	(4) Some College	(5) Index
Mother Head Start	-0.026	-0.101	0.092	0.073	0.263*
	(0.065)	(0.077)	(0.065)	(0.076)	(0.148)
Observations	3,580	3,580	3,579	3,533	3,533
Mean	0.278	0.329	0.773	0.495	-0.195

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on mother's 1979 household. The dependent variables are indicated by the column titles. Coefficient presented for binary variable indicating mother's self-reported participation in Head Start. In addition to family fixed effects, controls include child's birth order, sex, and age, and mother's birth order and age. Sample includes individuals over 20 in 2012. There is insufficient variation within the sibling comparisons to include mother or child's race/ethnicity.* (p<0.10) **(p<0.05), ***(p<0.01).

Table 3: Geographic Variation: Reduced Form Effect of Head Start in County

VARIABLES	(1) Teen Parent	(2) Crime	(3) High School	(4) Some College	(5) Index
Grandmother $<$ High School	-0.086***	-0.156***	0.127***	0.169***	0.466***
	(0.031)	(0.043)	(0.048)	(0.055)	(0.101)
Observations	1,709	1,709	1,709	1,687	1,687
Grandmother $\leq$ High School	-0.058**	-0.063*	0.064	0.070	0.218**
	(0.028)	(0.034)	(0.039)	(0.045)	(0.085)
Observations	2,770	2,770	2,769	2,732	2,732

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on mother's county of birth. The dependent variables are indicated by the column titles. Coefficient presented for binary variable indicating Head Start availability in mother's birth county 4 or 5 years after the year of mother's birth. In addition to year of birth and county of birth fixed effects, controls include childs gender, age, and age squared, as well as mother's birth order and race. Sample restricted to mothers whose mothers had less than a high-school degree. * (p<0.10) **(p<0.05), ***(p<0.01).

Table 4: Geographic Variation: Reduced Form Effect of Head Start in County (combined)

VARIABLES	(1) Teen Parent	(2) Crime	(3) High School	(4) Some College	(5) Index
Black & Grandmother $<$ High School	-0.114**	-0.086**	0.125	0.113	0.396**
	(0.053)	(0.041)	(0.079)	(0.080)	(0.153)
Black & Grandmother $\leq$ High School	-0.058	-0.011	0.089	0.050	0.185
	(0.048)	(0.042)	(0.063)	(0.078)	(0.137)
Male & Grandmother $<$ High School	-0.015	-0.271***	0.144*	0.147**	0.505***
	(0.045)	(0.066)	(0.078)	(0.073)	(0.129)
Male & Grandmother $\leq$ High School	-0.043	-0.139***	0.102*	0.082	0.321***
	(0.040)	(0.052)	(0.060)	(0.058)	(0.106)
South & Grandmother $<$ High School	-0.103**	-0.101**	0.155**	0.186**	0.491***
	(0.048)	(0.043)	(0.068)	(0.075)	(0.130)
South & Grandmother $\leq$ High School	-0.076*	-0.054	0.097*	0.092	0.286**
	(0.040)	(0.037)	(0.055)	(0.072)	(0.116)

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on mother's county of birth. Rows indicate subsample (South designation is based on mother's birth county). The dependent variables are indicated by the column titles. Coefficient presented for binary variable indicating Head Start availability in mother's birth county 4 or 5 years after the year of mother's birth. In addition to year of birth and county of birth fixed effects, controls include childs gender, age, and age squared, as well as mother's birth order and race. Sample restricted to mothers whose mothers had less than a high-school degree. * (p<0.10) **(p<0.05), ***(p<0.01).

Table 5: Geographic Variation: Reduced Form Effect of Head Start in County (robustness)

VARIABLES	(1) Index	(2) Index	(3) Index	(4) Index	(5) Index	(6) Index	(7) Index
Grandmother < High School	0.417***	0.466***	0.436***	0.488***	0.370**	0.292*	0.557***
	(0.111)	(0.101)	(0.105)	(0.105)	(0.144)	(0.167)	(0.144)
Observations	1,688	1,687	1,687	1,653	1,687	1,687	789
Grandmother ≤ High School	0.196**	0.218**	0.221***	0.234***	0.213**	0.243**	0.196*
	(0.087)	(0.085)	(0.082)	(0.086)	(0.108)	(0.109)	(0.114)
Observations	2,733	2,732	2,732	2,669	2,732	2,732	1,466
Covariates	N	Y	Y	Y	Y	Y	Y
Birth County Chars. (1960) x Trend	N	N	Y	N	N	N	N
WOP Measures	N	N	N	Y	N	N	N
Birth County Trends	N	N	N	N	Y	N	N
State by Year Fixed Effects	N	N	N	N	N	Y	N
Exclude South	N	N	N	N	N	N	Y

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on mother's county of birth. The dependent variables are indicated by the column titles. Coefficient presented for binary variable indicating Head Start availability in mother's birth county 4 or 5 years after the year of mother's birth. In addition to year of birth and county of birth fixed effects, controls include childs gender, age, and age squared, as well as mother's birth order and race. Column (2) contains base specification. Sample restricted to mothers whose mothers had less than a high-school degree. * (p<0.10) **(p<0.05), ***(p<0.01).

Table 6: Geographic Variation: Reduced Form Effect of Head Start in County (Falsification)

VARIABLES	(1) Teen Parent	(2) Crime	(3) High School	(4) Some College	(5) Index
Grandmother $\geq$ High School	-0.028	0.020	-0.015	0.001	-0.021
	(0.041)	(0.053)	(0.045)	(0.069)	(0.110)
Observations	1,355	1,355	1,354	1,338	1,338

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on mother's county of birth. Rows indicate subsample based on selection of mother's birth years. The dependent variables are indicated by the column titles. Coefficient presented for binary variable indicating Head Start availability in mother's birth county 4 or 5 years after the year of mother's birth. In addition to year of birth and county of birth fixed effects, controls include childs gender, age, and age squared, as well as mother's birth order and race. Sample restricted to mothers whose mothers had at least a high-school degree. * (p<0.10) **(p<0.05), ***(p<0.01).

Table 7: Geographic Variation: Reduced Form Effect of Head Start in County (Mother)

VARIABLES	(1) Teen Parent	(2) High School	(3) Some College	(4) High Grade	(5) Perm Income (std)	(6) Number of Kids
Grandmother < High School	0.030	0.081	0.052	0.441*	0.162*	-0.136
	(0.072)	(0.053)	(0.055)	(0.228)	(0.089)	(0.133)
Grandmother ≤ High School	0.043	0.044	0.060	0.398**	0.050	0.082
	(0.048)	(0.037)	(0.047)	(0.191)	(0.068)	(0.108)

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on mother's county of birth. The dependent variables are indicated by the column titles. Coefficient presented for binary variable indicating Head Start availability in mother's birth county 4 or 5 years after the year of mother's birth. In addition to year of birth and county of birth fixed effects, controls include mother's birth order and race. * (p<0.10) **(p<0.05), ***(p<0.01).

Appendix A: Supplemental Tables

Table A1: Geographic Variation: Head Start Availability and Enrollment

	Grandmother < High School	Grandmother ≤ High School
NLSY 79 (self-reported)		
Head Start in County	0.100**	0.055
	(0.045)	(0.034)
Observations	805	1,374
Mean (Head Start Unavailable)	0.14	0.12
Mean (Head Start Available)	0.35	0.30
OEO (66 Enrollment Counts)		
Fraction Head Start in State	0.287*	0.149*
	(0.150)	(0.077)
Observations	49	49
Mean (Head Start Available)	0.58	0.30
National Enrollment Counts (66-69)		
Observations	4	4
Mean (Head Start Available)	0.56	0.29

Note: Each cell represents a separate OLS regression. The first panel contains estimates of the effect of Head Start availability on self-reported participation in the NLSY 79 (reported in 1995). The dependent variable is the mother's self-reported Head Start status. Coefficient presented for binary variable indicating Head Start availability in mother's birth county 4 or 5 years after the year of mother's birth. In addition to year of birth and county of birth fixed effects, controls include mother's birth order and race. Sample restricted to mothers whose mothers had less than a high-school degree or at most a high-school degree. The means are average self-reported Head Start participation levels in counties with and without Head Start availability. The second panel contains estimates from a regression of the state-level share of four-year olds participating in Head Start against the fraction of the state's four-year population with Head Start availibility. State level availability is determined by dividing the number of 4 year-olds born in counties with availbility in 1966 (determined using our county-level treatment variable and natality data) by the total number of 4 year-olds born in the state. State-level participation and availability counts are adjusted using national statistics on maternal education levels of participants and mothers of children born during this time period. The means are the fraction of children estimated to be enrolled in Head Start, assuming that only children born in counties with Head Start availability enrolled. The third panel contains similar means using the national level Head Start enrollment data for 1966-1969 combined with natality data. * (p<0.10) **(p<0.05), ***(p<0.01).

Table A2: Geographic Variation: Head Start Availability and Self-reported Participation (Falsification)

	Grandmother $\geq$ High School
HS in County	-0.005
	(0.053)
Observations	735
Mean	0.15

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on county of birth. Rows indicate subsample (South designation is based on mother's birth county). The dependent variable is the mother's self-reported Head Start status. Coefficient presented for binary variable indicating Head Start availability in mother's birth county 4 or 5 years after the year of mother's birth. In addition to year of birth and county of birth fixed effects, controls include mother's birth order and race. Sample restricted to mothers whose mothers had at least a high-school degree. * (p<0.10) **(p<0.05), ***(p<0.01).

Table A3: Family FEs: Effect of Head Start Participation on Next Generation Outcomes (heterogeneity)

VARIABLES	(1) Teen Parent	(2) Crime	(3) High School	(4) Some College	(5) Index
Black	-0.090	-0.094	0.087	0.055	0.305
	(0.081)	(0.108)	(0.087)	(0.106)	(0.209)
Male	-0.024	-0.220*	0.195*	0.203	0.543***
	(0.066)	(0.121)	(0.102)	(0.130)	(0.205)
South	0.030	-0.213	0.033	0.160	0.348
	(0.113)	(0.157)	(0.106)	(0.146)	(0.287)

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on mother's 1979 household. Rows indicate subsample (South designation is based on mother's birth county). The dependent variables are indicated by the column titles. Coefficient presented for binary variable indicating mother's self-reported participation in Head Start. In addition to family fixed effects, controls include child's birth order, sex, and age, and mother's birth order and age. There is insufficient variation within the sibling comparisons to include mother or child's race/ethnicity.* ($p<0.10$) **($p<0.05$), ***($p<0.01$).

Table A4: Geographic Variation: Head Start Availability and Self-reported Participation (heterogeneity)

VARIABLES	(1) First born	(2) Black	(3) HH in Poverty (78)	(4) Grandmother ≤ High School	(5) Grandmother < High School	(6) Child Age (12)	(7) Male Child
Full Sample	0.060	0.045	0.020	0.026	-0.001	0.036	-0.001
	(0.042)	(0.029)	(0.047)	(0.031)	(0.039)	(0.250)	(0.035)
Observations	1,676	1,685	1,595	1,585	1,585	3,272	3,273
Mean	0.424	0.288	0.356	0.863	0.514	24.38	0.448
Grandmother < HS	0.045	0.021	-0.015	N/A	N/A	0.096	0.116***
	(0.063)	(0.044)	(0.077)			(0.338)	(0.043)
Observations	824	826	792	826	826	1,709	1,710
Mean	0.424	0.288	0.356	0.863	0.514	24.38	0.448
Grandmother ≤ HS	0.067	0.045	0.022	N/A	-0.011	0.229	0.026
	(0.048)	(0.032)	(0.052)		(0.042)	(0.261)	(0.035)
Observations	1,407	1,412	1,347	1,412	1,412	2,770	2,771
Mean	0.424	0.288	0.356	0.863	0.514	24.38	0.448

Note: Each column represents a separate OLS regression with robust standard errors in parentheses, clustered on county of birth. Rows indicate subsample (South designation is based on mother's birth county). The dependent variable is the mother's self-reported Head Start status. Coefficient presented for binary variable indicating Head Start availability in mother's birth county 4 or 5 years after the year of mother's birth. In addition to year of birth and county of birth fixed effects, controls include mother's birth order and race. Sample restricted to mothers whose mothers had less than a high-school degree. * (p<0.10) **(p<0.05), ***(p<0.01).

Appendix B: Data Appendix

To generate measures of Head Start exposure in the late 1960s, we compile data from the National Archives and Records Administration (NARA) files on the Office of Economic Opportunity's Head Start grant funding (National Archives, n.d.). We employ two data sources, covering different spans of time, to construct county-level measures of Head Start activity.

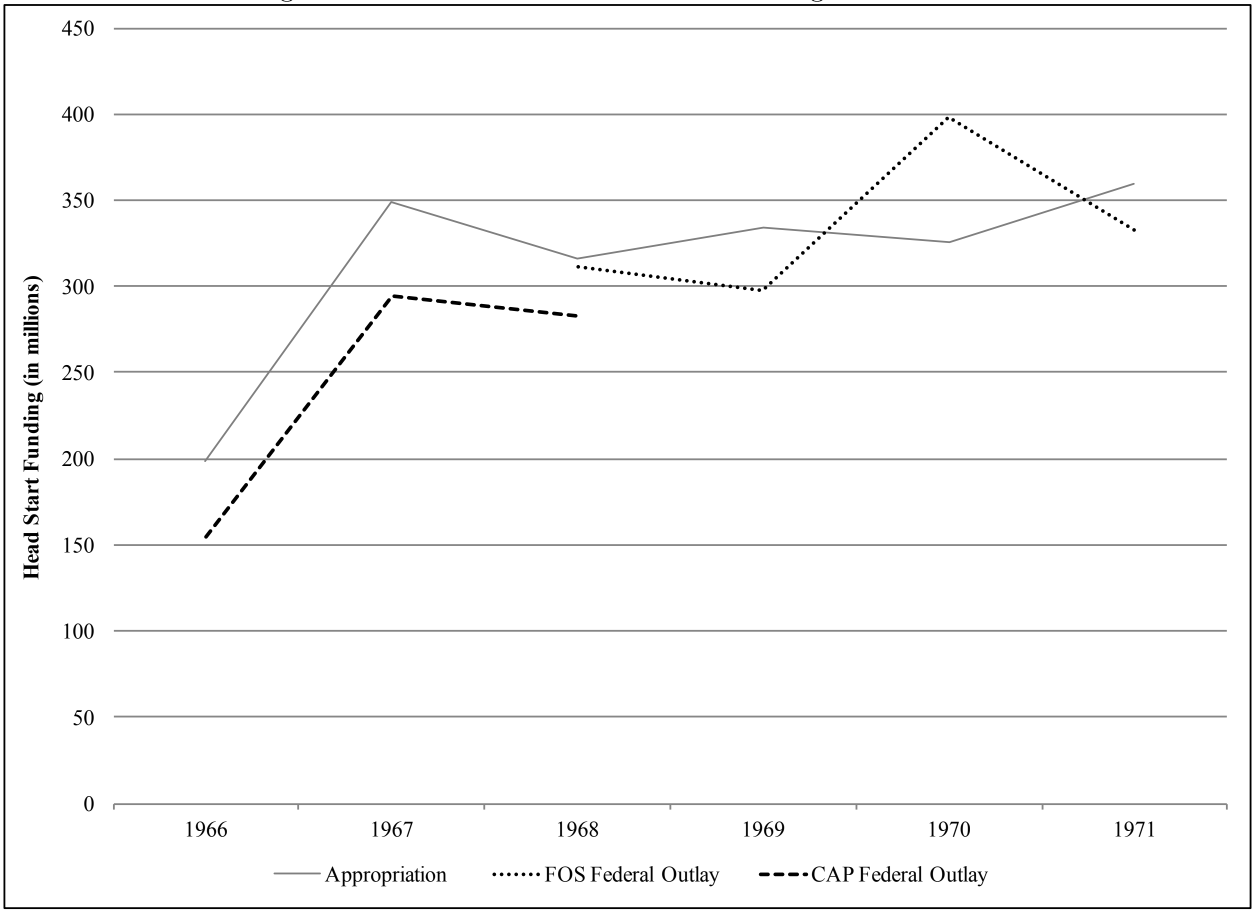
Community Action Program (CAP) Records, 1966–1968

The first data source is Records About Community Action Program (CAP) Grants and Grantees, NARA Record Group 381 (National Archives identifier 604417). These CAP files, created by the Community Services Administration, document the timeframe July 1964 through September 1981. The CAP records consist of two files, the Grantee Organization Master Files which provide information about the grantees receiving funds through CAP and the Funded Program Account Master Files which document specific grant actions. For the purposes of creating county-level funding amounts, we use the Funded Program Account Master Files which contain state and county identifiers.

We retain only records for which the grant action is Initial Application, Supplemental Funding, or Refunding at End of Program Year or for which the grant action field is blank. We exclude, therefore, grant actions Deobligation, Extension, Transfer In, and Transfer Out.

These files contain grant actions on a variety of CAP-funded poverty programs, including job training, housing services, health services, and community development. For this reason, we extract files with certain search terms in the project description field: Head Start, Headstart, child dev, preschool, pre-school, early childhood, HS child, and FYHS. We do not use the terms child care, daycare, or family care center though they appear in the project description field. These filters result in retaining only a subset of grant actions for each grantee number. Once we have the domain of Head Start programs, we aggregate federal funding to the county-by-FY level using state and county geographic codes. Notably, we drop nonnumeric characters from the funding amount field when they appear (always at the end of the field), assuming that these are placeholders for an input mask.

Figure B1: Crosswalk of Head Start Funding Data Sources



Source: National Archives Records Administration, Records of the Community Services Administration & U.S. Department of Health and Human Services, Administration for Children and Families, Office of Head Start.

Because Head Start data is missing from the electronic files for FY 1965 (also documented in (Bailey and Duquette 2014)'s data appendix), we construct CAP records for FY 1966, 1967, and 1968. After 1968, the search terms we employ largely drop out of the project description field and no longer appear after 1972. While we undercount total Head Start grant funding relative to published federal appropriations (Office of Head Start 2015), the pattern of funding levels and changes across these three years tracks well, as displayed in Figure B1.

Federal Outlays System (FOS) Files, 1968–1980

The second source is the Federal Outlays System (FOS) Files, also NARA Record Group 381 (National Archives identifier 599052). These records were collected from July 1967 through September 1980, also by the Community Services Administration. The files contain data on Federal Executive Branch outlays and include four files for each fiscal year: 1) a County/State Master File, 2) a City Master File, 3) a Geographic Table File, and 4) a Program Appropriations, Functions, and Agency Table File. The County/State Master Files for each year contain information on programs and outlays with state and county identifiers. We compile these records with the Program Appropriations, Functions, and Agency Table File across the years and again employ search terms to narrow to Head Start programs.

These files contain a variety of program types, so we search in the program title field for the following terms: Head Start, Headstart, child dev, early childhood, HS child, FYHS, and follow-thru program (OEO). Some terms are employed to align with Ludwig and Miller's (2007) file creation process. Terms related to preschool were excluded as they captured unrelated school-age programs. Records containing the following terms in the title field (appearing in conjunction with child dev or early childhood) were dropped: handicapped, handic, child abuse prev, and child welfare. In addition, 78 observations were deleted because they were missing state identifiers. Funding outlays were then aggregated to the county-by-FY level.