The Effect of Early Childhood Education on Adult Criminality: Evidence from the 1960s through 1990s

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Abstract

We investigate the impact of early childhood education on adult criminal behavior, leveraging changes in policies that occurred over a number of decades. Using variation across birth cohorts generated by the rollout of Head Start (for those born in the 1960s and 1970s) and Smart Start (for those born in the 1980s and 1990s), along with administrative crime data that include the birth county of all individuals convicted of a crime in North Carolina, we find that improvements to early childhood education led to large (20 percent) reductions in the likelihood of a serious criminal conviction in adulthood. These reductions were concentrated in high poverty counties. While the benefits generated by each program in the form of crime reduction account for a large portion of the costs of the education provided, we find substantial relative gains from the targeting of funds to high poverty areas and to areas without existing access to subsidized care.

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1 Introduction

Are criminals made or born? This fundamental question not only has important implications for our understanding of criminality, but also is central to any efforts aimed at reducing the large costs that crime imposes on society ($2 trillion annually).[1] Policies that primarily address these costs through the justice system implicitly assume that the development of criminals cannot be prevented in a cost-effective way; instead they focus on incapacitating and rehabilitating those who have already become criminals. However, relatively little is known about the factors that influence an individual's likelihood of becoming a criminal in the first place. Furthermore, the concentration of crime among a small number of perpetrators (less than 6 % of the population commit the majority of crime) provides an opportunity for policy interventions to have outsized effects if they can prevent the development of criminals.[2] In fact, some estimates suggest that preventing the development of a single career criminal could result in as many as 600 fewer victims of crime each year.[3]

One policy intervention that may influence later criminal behavior is early childhood education. Understanding its relationship to criminality is important because recent attempts to improve the quality and accessibility of early childhood education have been driven in large part by policymakers' belief that those interventions will have large impacts later in life. Recent attention has focused on early childhood as a critical developmental period, yet the limited evidence on the effect of early childhood education on later criminal behavior is mixed and inconclusive. The most compelling evidence comes from a single evaluation of a small-scale high-intensity intervention, Perry Preschool, where effects on crime account for

[1]For context, $2 trillion dollars is 17% of annual GDP. (United States. Senate Committee on the Judiciary. Hearing on The Costs of Crime. September 19, 2006 (statement of Jens Ludwig))

[2]Farrington (2006) generates this statistic by tracking the criminal behavior of a set of boys in London. Given the higher propensity to commit crime among males, 6% is likely a substantial overestimate of the share of the population that commits the majority of crime.

[3]Across major crime categories, estimates suggest that a relatively small proportion of individuals (consistently less than 10%) account for the majority of crime. These "career criminals" commit hundreds of crimes each year (authors' calculations from Chaiken and Chaiken (1982)).

40-65% of the estimated benefits of the program (Heckman et al., 2010). However, a randomized evaluation of a similar program, the Abecedarian Project, indicates no effect of the program on crime (Campbell et al., 2012). Furthermore, while these studies provide rigorous evidence driven by random assignment, both rely on small samples with substantial attrition to support their conclusions.[4] Evidence on the effects of Head Start on criminal behavior is also mixed and relies on small samples, self-reported crime data, and sibling comparison approaches that may underestimate the effects of community-level program availability in the presence of spillovers (Deming, 2009; Garces et al., 2002).

We contribute to this literature by using administrative crime data to examine the effects of two large-scale early childhood education programs operating in two different time periods. This approach substantially improves external validity over prior estimates of long-run crime effects because it leverages a much broader treated population. It also enables a direct comparison of adult crime effects of two different interventions implemented in very different contexts. For both interventions, we find that adult crime reductions are concentrated in high poverty counties. This result has important implications for the debate between universal and more targeted early education policies. Additionally, by capitalizing on the later program's rollout in areas with and without existing access to the earlier program, we provide evidence of diminishing marginal returns to early education funding.

We also add to the limited evidence on the effectiveness of *modern* early childhood education programs by demonstrating that these effects extend past the schooling years (for example, Fitzpatrick (2008, 2010); Ladd et al. (2014) show short-term effects on test scores and maternal labor force participation). Positive externalities, such as reductions in adult criminal behavior, are particularly important for thinking about the social return to investments in early childhood education programs: unlike the labor market return to improvements in

[4] At adult follow up, the Perry experiment had 123 members and the Abecedarian experiment had 101, both somewhat reduced from initial samples.

human capital, these benefits accrue largely to those who are not directly affected by the programs and thus may not be considered in parents' participation decisions. Because of the large social costs of criminal behavior, even modest reductions may warrant large subsidies to early childhood education. Indeed, we calculate that the discounted social benefits from these programs generated by the later crime reduction account for a substantial portion of their costs, even without considering their many other potential benefits.

Effects on criminal behavior are not a direct implication of effects on test scores. There is no evidence that a program that raises test scores in childhood will produce reductions in adult criminal behavior through this cognitive channel. Furthermore, there is a variety of evidence that non-cognitive skills are much more closely related to impulsive and criminal behavior than cognitive skills (i.e., test scores), and that each skill type can be influenced independently (Heckman et al. 2006; Hill et al. 2011; Heckman et al 2010; Jackson 2018).[5] Indeed, Deming (2009), finds large effects of Head Start participation on measures of cognitive ability (0.13 SD at ages 7-10), but no effect on criminal behavior. Heckman et al. (2013) decompose the effects of Perry Preschool on eventual criminal behavior and show that they operate entirely through a non-cognitive channel (externalizing behavior). The different roles of cognitive and non-cognitive skills in determining adult outcomes suggests that the limited evidence on the test score effects of modern early childhood education programs may not be informative in predicting these programs' impacts on criminal behavior.

Our investigation exploits the staggered introduction of the Head Start and Smart Start programs in North Carolina. The Head Start program, funded and administered through the U.S. Department of Health and Human Services, has been an integral part of early childhood education nationwide since the late 1960s. It is the largest early childhood education program

[5] Jackson (2018) shows that teachers can have effects on test scores and behaviors, and that these value-added measures are only weakly correlated (r=0.22). This suggests that many teachers who raise test scores do not improve behaviors. Along similar lines, he shows that about 75 percent of the variation in his behavior index is unrelated to test scores.

in the United States, with an annual enrollment that has grown from 400,000 during its early years to nearly a million participants today. Head Start was designed to focus on the "whole child" by providing a number of wrap-around services alongside education (Ludwig and Miller, 2007). The Smart Start program was implemented in North Carolina in 1993 to address concerns about the preparation of children for school. The program helps parents pay for child care, improves the quality of early care and education programs, provides tools that help parents support their children, and ensures that children have access to preventative health care. The multi-faceted nature of Smart Start has many similarities with the early years of the Head Start program; in particular, both program implementations allowed considerable flexibility with respect to how funding was spent. Unlike Head Start, individuals do not enroll directly in the Smart Start program. Instead, the program operates by funding a variety of programs at the community level, and critically, these funds are not explicitly targeted towards poor children the way Head Start funds are.

To estimate the effect of early childhood education on adult crime, we leverage within-county variation in exposure and funding levels generated by the rollout of each program, along with individual-level administrative data for the universe of convicted criminals in North Carolina between 1972 and 2018. These administrative data are particularly well suited to our estimation strategy because they include each criminal's county of birth. Thus we are able to link early childhood policy exposure to later criminal outcomes and can overcome a variety of measurement and endogeneity difficulties that likely inhibited earlier research.[6] We combine these data with birth counts to construct birth-county by birth-cohort conviction rates, which we then link with information on early childhood education funding and availability in each county and year.

[6]Most administrative crime datasets do not contain county of birth. This would likely require researchers interested in the early childhood environment to make assumptions about the relationship between the location of arrest and earlier residence, thus introducing measurement error which would bias the effect estimates toward zero, as well as potentially introducing bias due to endogenous migration.

Our estimates suggest that early childhood education reduces adult criminality. In particular, we find that both programs are responsible for reducing the conviction rate by approximately 20% in high poverty areas. Head Start availability reduces by 1.3 percentage points the likelihood of a serious conviction by age 35, but only in high-poverty counties.[7] Smart Start, which operated decades after Head Start and often in different counties, generates reductions in later criminal behavior on the same order of magnitude, reducing the likelihood of a serious criminal conviction by 0.7 percentage points (13 percent) overall by age 24. These effects are larger in high-poverty counties (1 percentage point, or 21 percent) and are larger for blacks than whites, perhaps as a result of differential pre-existing access to high-quality care. The reductions generated by Smart Start are also larger in counties without access to Head Start; this suggests that the effectiveness of additional early education funding may diminish as funding increases. This result further suggests the importance of targeting funding towards areas with the greatest need.

The nature of the introduction of both programs suggests that program availability and funding was likely not related to an individual's propensity for criminal behavior.[8] Consistent with this, there is little relationship between baseline county characteristics and exposure for either program. Our estimates also are robust to a variety of standard checks that allow for differential trends in criminality across counties and control for potential confounders.[9] The legitimacy of the identification strategy is further bolstered by event study estimates that show no significant program "impact" in the years prior to its introduction in a given county

[7]This is perhaps not surprising given the focus of the Head Start program on poor children and the resulting concentration of funding among high-poverty counties. Head Start funding per capita is between three and four times larger in high- versus low-poverty counties (Appendix Figure A2).

[8]For example, Smart Start's pilot partnerships were chosen so as to be representative of North Carolina's diversity and geography.

[9]In the case of Head Start, for example, the estimates are robust to the inclusion of time-varying county-level controls for the availability of other War on Poverty Programs as well as birth county trends. Furthermore, Head Start availability is unrelated to other policy changes shown to affect crime (e.g., removal of lead from gasoline, changes to compulsory schooling law ages in North Carolina, or the legalization of abortion), which occurred at the state level and generally affected different cohorts of individuals.

but a reduction in criminality in the years afterward. While our data only cover crimes committed in North Carolina, we find no evidence of differential out-of-state migration as a result of Head Start availability or Smart Start funding levels in early childhood.[10] Given the modest level of migration out of one's state of birth (and the lack of evidence of any differential out-migration), we view our estimates as providing a lower bound for the overall effect on criminal behavior.

Finally, we show that the discounted benefits generated by early childhood education's later crime reduction account for a substantial portion of the costs of the education provided, and are likely more than the costs of Smart Start. This is especially noteworthy because later crime reduction was not the stated objective of either program, and these benefits likely accrue in large part to those not directly affected by the programs. Taken together, our results support recent state efforts to expand and improve early childhood education, but further point to large potential gains from targeting these efforts toward areas with the greatest need.

2 Evidence on the Origins of Criminal Behavior

Research on the developmental factors that influence the likelihood that an individual will become a criminal is limited, with many studies focusing solely on adolescence. A number of evaluations of the Moving to Opportunity project provide mixed evidence on the effect of neighborhood environment on criminal behavior, while studies of assignment to foster care do suggest that family environment plays an important role in affecting both contemporaneous

[10] Across a variety of approaches and subsamples, our estimates indicate a small and non-significant relationship between childhood Head Start availability and the likelihood of living in one's state of birth. Assuming similar patterns of criminality among North Carolina leavers and stayers, our upper bound estimate of additional migration can explain at most 5% of our estimated effect. Additional analyses support Ladd et al.'s (2014) conclusion that Smart Start funding did not result in differential migration out of North Carolina. We discuss migration concerns further in Section 5.4.

and later criminal behavior (Sanbonmatsu et al., 2011; Doyle, 2007, 2008).[11] Several studies have focused on the relationship between secondary education and crime, suggesting that additional years of schooling, increases in school quality, and changes in the composition of school peers can affect the likelihood of criminal behavior several years later (Lochner and Moretti, 2004; Deming, 2011). Because these adolescent treatments occur at an age when individuals typically first decide to engage in crime, they may directly affect the costs or benefits of crime (e.g. through direct exposure to crime or criminal peers) rather than influencing an individual's development.[12]

Research on earlier periods of development is somewhat less common, yielding mixed evidence of effects. Emerging evidence suggests an important role for early childhood health and nutrition. Evaluations of the Nurse-Family Partnership Program and the Food Stamp program suggest significant effects of early health interventions on adolescent or adult criminal behavior (Olds et al., 1998, 2007; Barr and Smith, 2018).

Still fewer studies examine the role of early childhood education, and the evaluations of somewhat resource-intensive programs provide mixed evidence. Heckman et al. (2010) suggests that HighScope Perry preschool participation led to large reductions in criminal behavior, but the Campbell et al. (2002) evaluation of the Abecedarian program indicates limited effects of the program on crime. Furthermore, while these studies provide rigorous evidence driven by random assignment, both rely on small sample sizes from single sites to support their conclusions.[13] Even if one accepts these effects as given, it is unclear

[11] While early evaluations of the program found mixed evidence of effects on involvement with the criminal justice system at different ages (Katz et al., 2001; Kling et al., 2005; Ludwig and Kling, 2007), Sanbonmatsu et al. (2011) indicates no clear pattern of significant effects on arrests or delinquent behavior. Any effects that exist appear to be a result of current neighborhood conditions rather than the neighborhood that one grew up in. Doyle (2008) finds that those on the margin of placement are two to three times more likely to enter the criminal justice system as adults if they are placed in foster care.

[12] Deming (2011) suggests peer effects as one explanation for the effect of school quality on criminal behavior. Bayer et al. (2009) estimate criminal peer effects more directly, showing that juvenile offenders assigned to the same facility affect each other's subsequent criminal behavior.

[13] Recent evidence that adjusts for multiple hypothesis testing suggests that neither program had statistically significant effects on adult crime for boys or girls at the 5 percent level and that there may not have

whether these types of programs would continue to be effective on a larger scale with a less disadvantaged sample.

2.1 The Head Start Program

Easily the largest early childhood education program in the United States, Head Start began as a summer program in 1965. It quickly expanded to a year-round program in the following year. Head Start's mission was to "[provide] the children of the poor with an equal opportunity to develop their full potential" (Office of Child Development, 1970). To that end, it was designed to focus on the "whole child" by providing a number of wrap-around services alongside education (Ludwig and Miller, 2007). These additional services included: providing nutritious meals and snacks; access to social workers; mental health and dental treatment; immunizations; and health screenings.

Head Start served a decidedly disadvantaged population in the early years of the program; the median family income of children enrolled in Head Start was less than half that of all families in the U.S. (Office of Child Development 1968). Quasi-experimental evidence focused on this period suggests that Head Start has had important long-term effects for the cohorts of children who participated. Leveraging sibling comparisons and discontinuities in grant-writing assistance and program eligibility, a number of studies have documented increased educational attainment, better health, and higher earnings (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).[14] More recent evidence indicates that the positive effects of the program persist into later ages (Thompson, 2017), interact positively with school funding levels (Johnson and Jackson, 2017), and even spill over into the next generation (Barr and Gibbs, 2018). Johnson and Jackson (2017), an innovative attempt to

been statistically significant benefits for boys participating in either program (Anderson, 2008).

[14] Gibbs et al. 2011 provide a more comprehensive review of the Head Start literature.

test empirically for the presence of dynamic complementarities, suggests large effects of Head Start spending on the likelihood of ever being incarcerated, with these effects substantially magnified by the level of K-12 spending.

Two other studies include criminal behavior in their investigations of the long-run effects of Head Start, but yield conflicting evidence. Using the NLSY79, Garces et al. (2002) find that Head Start participation reduces later criminality, but only among black participants. Using the CNLSY, Deming (2009) finds no effect of Head Start participation on criminal behavior. While effects on crime are not the focus of either paper, these estimates should be interpreted cautiously given the well known issues with underreporting in self-reported measures of criminal behavior (Hindelang et al., 1981). Moreover, both of these studies use family fixed- effects designs, so we might worry that the choice to send one child to Head Start (while not sending a sibling) may be related to characteristics of the child or to the parents' circumstances at the time, potentially biasing the estimates. Finally, this design may underestimate the overall impact of Head Start on adult crime if there are substantial spillovers between siblings or between peers more generally.

To better capture the overall community-level effects, we focus on the plausibly exogenous variation in Head Start access by county and year (see Figure 1), and use unique administrative crime data with the offender's county of birth for everyone convicted of a crime in North Carolina in the last four decades.[15] Because the Head Start program rolled out quickly, and grant funds were distributed directly to local grantees, these programs became available in different counties at different times. There was substantial variation in the year of adoption among counties with similar baseline characteristics, generating plausibly exogenous variation in Head Start access (Barr and Gibbs, 2017; Thompson, 2017).

[15]This strategy captures peer effects within the same birth cohort, but may still underestimate the total effect if there are spillovers to older cohorts.

2.2 North Carolina's Smart Start Program

While the initial rollout of Head Start in the 1960s and 1970s provides a compelling source of variation, it is not clear whether these early estimates can be generalized to more recent periods. Indeed, the extreme poverty, hunger, and poor early childhood health that were prevalent during this period are uncommon in recent decades. To assess the more general potential for early childhood education to reduce criminality, we examine the impact of the Smart Start program in North Carolina.

The Smart Start program was created in 1993 to address concerns about the school-readiness of children in North Carolina. The program helps parents pay for child care, improves the quality of early care and education programs, provides tools that help parents support their children, and ensures that children have access to preventative health care. The multi-faceted nature of this program is similar in many ways to the early years of the Head Start program. But unlike as with Head Start, individuals do not enroll directly in the Smart Start program. Instead, the program operates by funding a variety of initiatives at the community level. And critically, the funds are not required to be targeted toward poor children.

Like Head Start, Smart Start was rolled out to different counties at different times, resulting in significant variation in funding across birth cohorts within similar counties (Figure 1). Ladd et al. (2014) use this variation in county-year funding to demonstrate sizable short-term effects of Smart Start funding on 3rd grade test scores (around 5 percent of a standard deviation for the average level of funding in a county at ages zero to five). However, we are not aware of any studies of the longer-term effects of Smart Start. We adopt the identification strategy of Ladd et al. (2014) to provide the first estimates of the effect of Smart Start on an adult outcome, criminal behavior. This outcome is associated with large (and mainly external) social costs and is largely influenced independently from cognitive skills (Heckman et al. 2006, Hill et al. 2011; Heckman et al 2010; Jackson 2018). Therefore, our estimates provide critical information on the social benefits of Smart Start that could not be inferred from earlier test score effects.

3 Data

Our primary data source is administrative conviction data from the state of North Carolina. We use these data, along with birth records, to calculate rates of conviction by county-year birth cohort. We use Head Start and Smart Start funding information to construct measures of program exposure at the same level. We link the measures of program exposure to the conviction rates at the county-year birth cohort level to estimate the effect of each early childhood education program on later adult crime.

3.1 North Carolina Data

We obtained data containing public information on all individuals convicted of a crime in North Carolina between 1972 and 2018 from the North Carolina Department of Public Safety. The administrative data contain information on the type of crime, including the statute of the offense and whether it was a felony, as well as the name, dates of birth, gender, and race of the perpetrator. An important advantage of the North Carolina data over other state criminal databases is the inclusion of county of birth for each individual. Combining information on criminals' years and counties of birth with birth counts obtained from the North Carolina Department of Health and Human Services allows us to construct conviction rates for birth cohorts of individuals born in North Carolina. For example, to generate the cohort conviction rate by age 35 for children born in county c in 1961, we divide the number of individuals born in county c in 1961 and convicted by age 35 by the total number of individuals born in county c in 1961.

For our Head Start analyses, we restrict the sample to individuals born between 1955 and 1968, allowing us to leverage the variation in Head Start availability that occurred up to and including 1972 (as Head Start availability is measured four years after birth). For our Smart Start analyses, we focus on individuals born between 1980 and 1994, which allows us to observe criminal convictions through age 24 for all cohorts in the sample (we observe convictions through 2018).[16]

Summary statistics are contained in Table 1. Slightly less than 5 percent of individuals born between 1955 and 1968 were convicted of a serious crime by age 35; likewise, slightly more than 5 percent of individuals born between 1980 and 1994 were convicted of a serious crime by age 24. Our definition of a serious crime is based on the FBI's Part I offenses.[17]

While the data contain the universe of individuals convicted of a crime in North Carolina during this time period and allow us to link these individuals to their counties of birth, they are limited in that they do not allow us to observe convictions for individuals who are born in North Carolina and then leave the state. While most likely criminals remain in their state of birth (and the likelihood of criminal behavior is lower for those who leave), this may be a concern for the interpretation of our estimates if Head Start availability or Smart Start spending affects out-of-state migration, especially if it leads individuals with a higher-propensity for criminal behavior to leave the state.[18] We return to this concern below in Section 5.3, providing evidence that program availability and funding do not appear to

[16]We find similar effects when we focus our estimates on convictions through older ages, which necessitates a reduction in our identifying variation (e.g., through age 25 (using cohorts through 1993), and through age 26 (using cohorts through 1992)). See Appendix Table A4. We focus on convictions by age 24 because it provides a balance between variation in Smart Start funding exposure across birth cohorts and our capacity to observe these cohorts at ages with the highest rates of criminal behavior.

[17]We largely follow the convention of FBI's Uniform Crime Reporting Statistics for Part I offenses. Violent crimes are defined as offenses containing the words "murder", "assault", or "robbery". Property crimes are defined as offenses containing the words "burglary" or "larceny".

[18]Roughly 70% of individuals born in North Carolina during this period reside there between the ages of 18 and 35. This share is even higher (roughly 80%) for those with the highest rates of criminal behavior (between ages 18 and 24, non-white, or with less than a high-school degree). Authors' calculations using public Census and ACS data.

influence migration rates.[19,20]

3.2 Head Start Availability Measure

We follow Barr and Gibb's (2017) definition of Head Start availability, which relies on county-year data from the Community Action Programs (CAP) and Federal Outlay System (FOS) files obtained from the National Archives and Records Administration (NARA).[21] We construct an availability indicator equal to one if a county had Head Start expenditures per four-year old above the tenth percentile.[22] We do not otherwise leverage data on appropriated dollar amounts due to concerns about the accuracy of the recorded funding amounts in the early years of the Head Start program as well as the potential endogeneity of funding levels.

3.3 Smart Start Funding Exposure Measure

We follow Ladd et al.'s (2014) definition of Smart Start funding penetration, which relies on county-year data on Smart Start funding obtained from the Budget & Grants Compliance Officer at Smart Start. For each county-year, we compute the annual funding amount per child aged 0-5. We then sum this county-year funding penetration by cohort, generating the total amount of funding exposure a given cohort received from ages 0 to 5. Thus, our Smart

[19]Specifically, for Head Start, we explore the relationship between measures of childhood Head Start availability (at the state of birth by birth cohort level) and the likelihood of living in one's state of birth. Across a variety of approaches and subsamples our estimates indicate a small and non-significant relationship between childhood Head Start availability and the likelihood of living in one's state of birth. Similar analyses support Ladd et al.'s (2014) conclusion that Smart Start funding did not result in differential migration out of North Carolina. We address concerns about differential migration further in Section 5.4

[20]We may also be missing individuals with one-time nonviolent convictions (at any age) or one-time drug convictions (under age 22) that hired a lawyer and had the record expunged. However, these possible missing observations are a threat to identification only if they occur differentially based on early childhood educational status.

[21]See Barr and Gibb's (2017) Data Appendix for details.

[22]We use this threshold for consistency with Barr and Gibbs (2017), who find that using this threshold better predicts Head Start take-up, but neither the values of the availability indicator (in North Carolina) nor the main results are sensitive to moving this threshold.

Start funding measure tracks the cumulative exposure to Smart Start funding a given county birth-cohort received.

4 Estimation of Program Availability Effects

To estimate the effect of early childhood education on adult crime, we exploit within county variation in exposure generated by the roll out of each program. For Head Start, we leverage within county variation in the availability of Head Start generated by the initial roll-out of the program in the 1960s. For example, we utilize the fact that eligible four-year-olds in 55 out of North Carolina's 100 counties had access to Head Start in 1968 while no four-year-olds had access to Head Start prior to 1965 (panel (a) of Figure 1). For Smart Start, we leverage the funding expansion from zero counties in 1992 to all 100 counties by 2000, with substantial differences in per capita funding levels (panel (b) of Figure 1).

For both interventions, we estimate the following specification:

$$C_{ct} = \beta EC_{ct} + \gamma(X_c \times t) + \alpha_c + \sigma_t + \epsilon_{ct}, \tag{1}$$

where C_{ct} is the conviction rate for those born in county c in year t, EC_{ct} is a measure of county-year birth cohort exposure to the early childhood policy, and α_c and σ_t are birth county and birth year fixed effects. In robustness checks, we include $X_c \times t$, which are controls for baseline birth county characteristics interacted with a time trend, to account for any differential trends by county characteristics.[23] For example, this would account for differences in crime trends between more and less rural counties.[24] Standard errors are

[23]For Head Start the baseline characteristics are from 1960, whereas for Smart Start they are from 1980.

[24]Following Hoynes et al. (2016), the 1960 county characteristics include the percent of people living in families with less than $3,000 (1960 dollars), the percent living in urban areas, the percent black, the percent under 5 years old, the percent over 65 years old, the percent of land in farming, and the percent of employment in agriculture. Following Ladd et al. (2014), the 1980 county characteristics include the share of births to black mothers, the share of births to Hispanic mothers, the share of births to low education mothers, the share of the population using food stamps, the total number of births, the total population,

clustered at the county of birth level.

The key identifying assumption is that exposure to each early childhood education policy is, conditional on birth county and birth year fixed effects, unrelated to the propensity to be convicted of a serious crime for some reason other than the policy. For example, if Head Start was rolled out at the same time that other programs targeted at children were adopted we could be mistakenly attributing the effects of this other program to Head Start.

The exogeneity of the national-level Head Start roll out has been supported at length in related work, with multiple studies referencing the quick and haphazard nature of initial Head Start grant-making as well as demonstrating a limited association between baseline county observables and the presence and timing of Head Start adoption once a county's poverty level is accounted for (see, for example, Barr and Gibbs (2017); Johnson and Jackson (2017); and Thompson (2017)).[25] We further bolster the exogeneity of the Head Start rollout in North Carolina with an event study (Figure 2), discussed in detail in Section 5.1, that finds no "effects" on the likelihood of conviction in the years Head Start's initial adoption in a county and a sharp jump immediately following the program's introduction.

Just as in the case of Head Start, the nature of Smart Start's introduction suggests that program exposure was, conditional on birth county and birth year fixed effects, unrelated to an individual's propensity for criminal behavior. The pilot partnerships were chosen to be representative of North Carolina's diversity and geography, with one county selected from each congressional district. As noted by Ladd et al. (2014), experts used county-level data on poverty-rates, tax base, and number of young children in need, to rank North Carolina's 100 counties into one of four resource bands: high, medium high, medium low, and low.

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and the median family income.

[25] We present analogous analyses specific to North Carolina in Appendix Tables A13 and A14. We find no statistically significant relationship between county characteristics in 1960 and the timing of Head Start availability within North Carolina. Appendix Figure A8 presents this relationship graphically and similarly suggests little relationship between baseline county characteristics and the timing of introduction. Appendix Table A14 indicates that more populous counties were more likely to get the program at all during this time period.

As required by the Smart Start legislation, the experts selected three counties within each resource band to receive initial funding. The program quickly expanded, reaching all counties by the 1998-99 school year. Consistent with the initial intent to provide balanced exposure to the program, there is little relationship between baseline county observables and the timing or magnitude of Smart Start funding (Appendix Figures A9 and A10). We provide additional evidence for the exogeneity of the Smart Start rollout in North Carolina with another event study (Figure 3), discussed in detail in Section 5.1, that finds no "effects" on the likelihood of conviction prior to the initial funding of Smart Start in a county and a phasing in of effects following initial funding that is consistent with the targeting of funds to a broader age range.[26]

While both programs experienced a similarly rapid rollout across the state, they differed substantially in how funding was distributed to participating counties. As Appendix Figure A1 shows, Head Start funding was concentrated in higher poverty counties, while Smart Start funding was distributed relatively evenly across counties. This difference reflects the different mandates of the two programs. As part of the War on Poverty, the Head Start program focused on children from families at or below the federal poverty line; indeed, at least 90 percent of Head Start participants at each site were required to be from families below the poverty line. Consistent with this, Head Start funding per four-year old is three to four times as high in high-poverty counties (Appendix Figure A2). By contrast, poverty was not an explicit focus of the Smart Start program. This yielded a more equal distribution of funds across high and low poverty counties. Nevertheless, differential effects of Smart Start by poverty status may still occur if families in poverty have fewer outside options for high-quality early childhood education. Accordingly, for both policies we conduct much of our analyses separately for high and low poverty counties, splitting counties at the median county poverty level for North Carolina in the baseline year for each program (i.e., 1960 for

[26]We discuss the expected and observed pattern of results further below.

Head Start and 1980 for Smart Start).[27]

5 Main Results

Our primary interest is in the coefficient β in Equation 1, which represents the effect of early childhood education availability or funding on adult crime. We find large effects on the likelihood of an adult criminal conviction for individuals born in high-poverty counties, with effect sizes around 20 percent (Table 2).[28]

Our main Head Start estimates indicate that program availability generated a 1.3 percentage point reduction in the likelihood of a criminal conviction by age 35 for high-poverty counties, but no measurable effect for low poverty counties (top row of Table 2).[29] [30]

Because Smart Start funding is not targeted at a particular age group, we follow Ladd et al. (2014) in estimating the effect of the average funding provided per 0 to 5 year old over the first five years of life. In the bottom row of Table 2, we report estimates per $1,000, which is roughly equal to the average funding exposure for cohorts born after Smart Start enters a county during our sample period.[31] Our main estimates suggest a 0.65 percentage point (13 percent) reduction in the likelihood of a serious criminal conviction by age 24

[27]It is worth noting, however, that poverty rates improved dramatically across the state between the 1960s and 70s and 1980s and 90s.

[28]For Smart Start, the effect size is for average funding exposure, which is slightly less than $1,000 for cohorts born after Smart Start enters a county (these cohorts are affected throughout ages 0 to 5).

[29]While our main estimates are identified off of the set of counties that ever received Head Start between 1965 and 1976 the results are similar when including the counties that did not receive Head Start during this period (Table A16). Appendix Figure A5 presents the corresponding event study, which similarly suggest a reduction in criminal behavior following Head Start availability.

[30]Figure A3 presents coefficient estimates for the same specification by poverty quintiles. The most dramatic effect occurs in counties in the highest poverty quintile. We have also estimated specifications that interact the continuous poverty rate with an indicator for Head Start availability (Appendix Table A1). Using this approach, we estimate that the reduction in crime rate due to Head Start availability is 0.2 percentage points larger for each 10 percentage point increase in the poverty rate. Consistent with our prior estimates, these estimates suggest that the effect of Head Start ranged from 0.0 percentage points in the county with the lowest poverty rate (23 percent) to roughly 1 percentage point in the county with the highest (74 percent).

[31]These cohorts are fully treated, unlike those born a 1 to 4 years before Smart Start entry, which experience partial treatment.

from an additional $1,000 in funding exposure (Table 2). Due to the timing of the Smart Start variation, we cannot present effects on conviction by age 35. As with Head Start, the estimated effects are larger in high-poverty counties, a 1 percentage point (23 percent) reduction in the likelihood of a serious criminal conviction.[32] While SS did not explicitly target children in poverty, this pattern of results is perhaps explained by the lower quality counterfactual care options available to low-income families or in high-poverty areas. This would conform with recent evidence that suggests larger short-term effects of early childhood programming for those who would otherwise have relied on parental or relative care (Kline and Walters, 2016).

For both programs, the estimated effects in high poverty counties appear to increase with age (similarly to the baseline conviction rate). Table A2 shows our Head Start estimates for conviction by age 24, 30, and 35, while Table A4 shows Smart Start results for conviction by age 24, 25, and 26 (older ages are not feasible given the timing of the Smart Start variation. While, the estimate of Head Start's effect on convictions by age 24 is not statistically significant, the percentage reduction in convictions remains similar to Smart Start for the same age. Furthermore, the corresponding event study (Appendix Figure A4) strongly suggests a reduction in the likelihood of conviction by age 24 following Head Start availability in a county.

While the rollout of the two programs occurred two to three decades apart, Head Start continued to operate in many North Carolina counties during the rollout of Smart Start. This provides an opportunity to investigate whether early childhood education funding yields increasing or diminishing returns. In Table 3, we examine whether Smart Start funding was more or less effective in the presence of Head Start. The estimates indicate that the

[32] As with Head Start, we have estimated specifications that interact the continuous poverty rate with our Smart Start funding measure (Appendix Table A3). Using this approach, we estimate that the reduction in crime rate due to a $1,000 of Smart Start funding is 0.4 percentage points larger for each 10 percentage point increase in the poverty rate.

effects of Smart Start are larger in counties that were not previously served by Head Start, pointing to the diminishing returns of early childhood education funding and underscoring the importance of targeting funds to areas with the greatest need.

5.1 Dynamics and Robustness

To understand the dynamics of how the program may have affected adult criminal outcomes and to test for pre-trends that may confound our baseline specification, we also present estimates from event study specifications. For example, for Head Start, we center counties around the first year that the program is available, and estimate the effect of leads and lags of program availability.[33]

Figure 2 indicates a flat trend in cohort conviction rates before Head Start rollout for both high and low poverty counties. This provides evidence that our difference-in-differences estimates are not capturing differential pre-existing trends in the years prior to county's rollout of Head Start. For cohorts exposed to Head Start, we see significant decreases in the conviction rate for the high poverty counties but continue to see no evidence of changes in the low poverty counties.[34] In the high poverty counties, the estimates of crime reduction appear to grow somewhat as the program persists in a county. In particular, the impact of Head Start availability in the first year of the program is substantially smaller than in subsequent

[33] We estimate the following specification separately for counties above and below the median poverty rate:

$$C_{ct} = \sum_{\tau=-6}^{7} \beta_\tau 1(t = T_c + \tau) + \alpha_c + \alpha_t + \gamma(X_{c,60} \times t) + \epsilon_{ct} \tag{2}$$

. We are primarily interested in the coefficients on the indicators, $1(t = T_c + \tau)$, each of which indicates how many years cohort t in county c is removed from the first cohort in county c exposed to Head Start, T_c. We use the first year in which the treatment measure exceeds the 10th percentile (i.e., \$10 per child per year) as the year of adoption within a county.

[34] This figure also addresses concerns that there were subsequent changes in a county that affected crime rates, such as changes to its criminal justice system, that are correlated with but not caused by the timing of a county's Head Start adoption. For such a correlation to produce our event study results, the policy change would have to precisely target only cohorts exposed to Head Start availability and have no effect on cohorts born just a couple years earlier.

years. This may be due to centers improving (or increasing the size of) their Head Start programs during the first years of operation or as a result of peer effects.[35] Funding does appear to increase somewhat during the early years of program operation, consistent with program growth (Appendix Figure A2).

The targeting of the Smart Start intervention to a broad age range does not lend itself to a standard event study because cohorts experience different intensities of treatment based on their age relative to the maximum targeted age (i.e., 5) when the funds become available in a county. This is illustrated in the top panel of Figure 3, which demonstrates that our treatment measure rises for five to six years following adoption. The bottom panel provides event study estimates of the effect of Smart Start availability on the likelihood of a criminal conviction. As with Head Start, our baseline dynamic estimates (bottom panel of Figure 3) indicate a flat trend in cohort conviction rates before Smart Start funding arrived in a county. This provides evidence that our estimates are not capturing differential pre-existing trends in the years prior to a county's adoption of Smart Start. Consistent with the phase in of our treatment measure, we observe a phase in of effects on the likelihood of a criminal conviction. The figure also shows that higher poverty counties enjoyed larger decreases in cohort conviction rates than low poverty counties, despite Smart Start funding being similar across county poverty levels.

The estimates of the effects of both programs are robust to the inclusion of pretreatment county characteristics interacted with time trends (see column (2) of Appendix Table A7). These controls limit concerns that that counties with different characteristics have differing trends in the likelihood of criminal behavior across cohorts that are correlated with the timing of program rollout. In the case of Smart Start, the results are also robust to the inclusion of time-varying controls at the county by birth cohort level, which addresses concerns about

[35] If peer effects are an important factor in criminal behavior, we would expect smaller effects of the program in the first year as compared to subsequent years when older peers would have also experienced the program.

changes in the composition or wealth of birth cohorts over time that could be driving the observed result (Appendix Tables A8-A9).Similarly, the Head Start estimates are robust to the inclusion of covariates indicating availability of other War on Poverty programs, such as Food Stamps, Medicaid, Community Health Centers, etc. (Appendix Table A10).[36]

While our baseline inference relies on standard errors clustered at the county of birth, we have also explored the robustness of our p-values to an even more conservative approach: randomization inference. Under this procedure, we randomly assign the rollout year or funding level of Head Start and Smart Start in each county and estimate our baseline specification. The distribution of these estimates over 1,000 iterations is contained in Appendix Figure A6 and A7. As can be seen in the figures, the estimates we observe in our baseline results are quite unlikely under random assignment. The implied two-tailed "p-values" we obtain from this randomization inference approach are similar to those obtained using our baseline approach with standard errors clustered on the county of birth.[37]

5.2 Heterogeneity of Effects

The most natural channel through which early childhood education may affect crime is by raising the return to work and thereby increasing the opportunity cost of participating in criminal behavior. This channel is supported by the observed increases in human capital demonstrated in recent evaluations of modern early childhood programs (for example, Fitz-

[36]We also test directly for relationships between these potential confounders and our measure of Head Start availability. Consistent with the limited effect of the War on Poverty controls on our estimates, we find no significant relationships between funding for various War on Poverty Programs and Head Start availability (Table A11). We also find no relationship between Head Start availability and measures of infant mortality. This suggests that the relationship between Head Start availability and later criminal behavior is not driven by broader improvements in infant health or medical treatment unrelated to Head Start.

[37]Implied "p-values" presented are the two-tailed statistics calculated as the share of coefficient estimates obtained under random assignment of Head Start timing or Smart Start funding that are larger in absolute magnitude than the estimate produced using the true timing of assignment and funding. We preserve the patterns of availability and funding and randomly reassign to counties to better approximate the structure of rollout. This results in more conservative p-values than a strictly random assignment of timing or funding and better mimics our assumption of conditionally random assignment.

patrick (2008) or Ladd et al., (2014)), although the existing evidence suggests that improvements in test scores may not translate into crime reduction in the absence of improvements in non-cognitive skills. Early childhood education may also affect the financial or psychological benefits of criminal behavior or it may alter preferences in other ways, either by directly influencing child development or by influencing parenting, perhaps via effects on parental labor supply (Fitzpatrick, 2010).

Heterogeneity in effects across crime types may provide a hint at how these early childhood programs are affecting an individual's adult propensity to commit crime. For example, changes in the opportunity cost of crime may be more likely to affect property offenses, whereas effects on child development may be more likely to affect violent offenses. In Appendix Table A5, we explore whether the effects of early childhood education on serious convictions differ by crime type. The coefficients for violent crimes are not statistically distinguishable from those for property crimes for Head Start or Smart Start. As with Head Start, Smart Start has similar effects on property crimes and violent crimes. The point estimates indicate a 0.24 percentage point reduction in the likelihood of a property conviction and a 0.4 percentage point reduction in the likelihood of a violent conviction by age 24; as before, these effects are larger in high-poverty counties. The lack of clear differences in effects across crime types limits any insights into the channels through which early childhood education influences later criminal behavior.

Racial differences in the likelihood of program exposure, along with racial differences in the counterfactual options for early childhood education, suggest that the effects of Head Start and Smart Start may differ by race. In Table 4, we explore these heterogeneous effects by presenting estimates from Table 2 separately by race. We find similar effects of Head Start availability for whites and non-whites, though only the reductions in adult convictions for non-whites in high-poverty counties are different from zero at a 10 percent significance level.[38] The lack of significant differences across races is somewhat surprising given that black children were more likely to be eligible for and enroll in Head Start. In the case of Smart Start, we find that funding exposure had significantly larger effects for non-whites, and again, we find that these effects are concentrated in high-poverty counties. Smart Start reduced non-white convictions by 20 percent overall and 26 percent in high poverty counties (Table 4). This result is even more dramatic in counties without Head Start by 1980: in high poverty counties without prior Head Start availability, Smart Start reduced non-white convictions by 35 percent (Appendix Table A6). These results are consistent with a substantial racial gap in counterfactual early childhood education opportunities in high-poverty counties in North Carolina in the 1990s.

Ladd et al.'s (2014) estimates similarly suggest larger effects of Smart Start funding on the test scores of black children, although the implied differences are modest and the effects are entirely driven by differences in maternal education.[39] This lesser degree of heterogeneity in test scores is consistent with differential effects of early childhood education on the accumulation of cognitive versus non-cognitive skills. For example, evaluations of Perry Preschool, which enrolled disadvantaged black children, did not produce long run gains in IQ (particularly for males), but did produce persistent improvements in behavior.

5.3 Concerns about Migration out of North Carolina

One potential threat to the validity of our estimates relates to the data's coverage of convictions. While the data contain the universe of individuals convicted of a crime in North

[38] During this period in North Carolina, the black population comprised more than 95% percent of the non-white population (1970 Census).

[39] Ladd et al. (2014) find no evidence of differential effects of Smart Start funding on the test scores of black children once they condition on maternal education and its interaction with Smart Start funding. However, they also show that the effects on test scores are larger for mothers with less than a high-school degree. The lower average education levels among black mothers in North Carolina implies larger effects for black children when not separately conditioning on maternal education and its interaction with Smart Start funding levels. However, the implied difference in the effect of Smart Stat on test score outcomes is modest.

Carolina during this time period and allow us to link these individuals to their counties of birth, they are limited in that they do not allow us to observe convictions for individuals who are born in North Carolina and then leave the state. Fortunately, most individuals born in North Carolina remain there during adulthood; roughly 70% of individuals born in North Carolina during our sample periods reside there between the ages of 18 and 35. This share is even higher (roughly 80%) for those with the highest rates of criminal behavior (between ages 18 and 24, non-white, or with less than a high-school degree).

If Head Start availability or Smart Start funding has differential effects on migration out of the state, it will not affect our estimates of convictions in North Carolina, but it may limit our ability to interpret them as representing an overall reduction in criminal behavior. A specific concern is that Head Start availability or Smart Start funding did not reduce criminal behavior, but simply increased the likelihood of leaving the state, leading us to misattribute the estimated reduction in the likelihood of criminal convictions.

In Appendix Table A12, we explore the relationship between measures of childhood Head Start availability (at the state of birth by birth cohort level) and the likelihood of living in one's state of birth. Across a variety of approaches and subsamples our estimates indicate a small and non-significant relationship between childhood Head Start availability and the likelihood of living in one's state of birth. Assuming similar patterns of criminality among North Carolina leavers and stayers, our upper bound estimate of additional migration can explain at most 5% of our estimated effect.[40] Even this upper bound is likely an overestimate as the mean rate of criminal conviction for movers to North Carolina (i.e., the equivalent of state of birth leavers) is lower than the rate for those born in North Carolina in our data. We have conducted similar analyses at the county-year birth cohort level using restricted-use American Community Survey (ACS) data.[41] While these results are undergoing the

[40]Even assuming the largest estimated effect on migration, it would have to be the case that 65% of the marginal migrants were criminals to account for our estimates.

[41]These restricted data are available in the Federal Statistical Research Data Center (FSRDC)

disclosure process, they are consistent with the estimates in Appendix Table A12.

Smart Start funding similarly appears to have no effect on migration out of North Carolina, at least as measured by administrative schooling data through grade 3 (Ladd et al., 2014). While we have been unable to determine a way to use publicly available data to test for differential migration at older ages (18 to 24), we have conducted similar analyses (as discussed above) with restricted-use ACS data that contain county of birth. While the results are undergoing the disclosure process, they are consistent with the conclusion of (Ladd et al. (2014) regarding the effect of Smart Starting funding on migration.[42]

Given the modest level of migration out of one's state of birth (and the lack of evidence of any differential out-migration), we view our estimates as providing a lower bound for the overall effect on criminal behavior.

5.4 Magnitude of Effect on Criminal Behavior

Our estimates suggest that the availability and funding of early childhood education causes substantial reductions in adult criminal behavior. Our preferred estimates from studying both Head Start and Smart Start indicate reductions in the likelihood of any serious conviction of approximately 1 percentage point (approximately 20 percent) for the average program in high poverty counties. While both programs generate similar reductions in serious criminal convictions in high poverty counties, Head Start is somewhat more expensive in real terms.[43] Scaling by the average cost of Head Start in high poverty counties and adjusting everything to 2015 dollars, we find that Head Start availability reduced the likelihood of a serious criminal conviction by 0.636 percentage points per $1,000 (in 2015 dollars), while Smart Start reduced the likelihood by roughly 0.635 percentage points per $1,000 (in 2015

[42] If anything, exposure to additional Smart Start funding in early childhood appears to make individuals less likely to leave North Carolina, suggesting that the magnitude of our estimated effects on overall crime may be slightly attenuated.

[43] Around $2,000per individual in the cohort for Head Start and around $1,600per fully treated individual for Smart Start in high poverty counties (all in 2015 dollars).

dollars).[44] Alternatively put, Head Start and Smart Start cost about $1,500 per percentage point reduction in serious criminal convictions.

While the specific criminal outcome measures differ, the implied cost per percentage point of crime reduction for Head Start and Smart Start spending are similar to but somewhat smaller than those implied by evaluations of the Perry Preschool program: $1,800 per percentage point *arrest* reduction (authors' calculations from Heckman et al. (2010) estimates). When we scale arrests to convictions, we find that this cost-effectiveness advantage for Head Start and Smart Start increases.[45]

5.5 Quantifying the Benefits

How do the future benefits of crime reduction compare to the costs of these programs? To enable this comparison, we present back-of-the-envelope estimates of the discounted future value of crime reduction by offense for various choices of discount rates (Table 5). Column 2 shows the reduction in number of convictions per child implied by our difference-in-differences estimate for high poverty counties.[46] Column 3 shows the reduction in the number of *crimes* associated with this reduction in *convictions*.[47] We apply McCollister et al (2010) estimates of the social cost (2015 dollars) of each type of crime (Column 1) to arrive at our estimates

[44] The Smart Start treatment is denoted in nominal dollars so we adjust to $1,000 in 2015 dollars.

[45] Another approach to comparing magnitudes is to calculate implied treatment-on-the-treated (TOT) effects of each intervention. While this isn't possible in the case of Smart Start, our Head Start estimates imply treatment-on-the-treated (TOT) effects of 6 to 9 percentage points, somewhat smaller than those reported in evaluations of the Perry Preschool program for somewhat similar measures (See Appendix B for additional discussion.)

[46] We convert changes in the likelihood of property crime conviction to changes in the number of convictions per child for a specific crime by multiplying our coefficient estimate by the mean convictions for the given crime at each age, among individuals convicted of a property crime by the relevant age (35 for Head Start and 24 for Smart Start). Column 2 shows the sum of these conviction changes for each specific crime across the relevant age ranges. We repeat this calculation for violent crime convictions.

[47] We convert the number of convictions to the number of crimes using the overall ratio of these measures for property or violent crimes in North Carolina during the period relevant for each set of cohorts (1982-2003 for Head Start and 2004-2014 for Smart Start). The number of violent and property crimes for North Carolina is obtained from the FBI's Uniform Crime Reports (2014 was the last year available). The comparable number of violent and property crime convictions is constructed using our data from the North Carolina Department of Public Safety.

for the benefits generated per exposed individual in high poverty counties (Columns 4-6).[48] For Head Start, an exposed individual is anyone in a birth cohort for whom Head Start was available in the county, regardless of their eligibility for or participation in the program. For Smart Start, this is anyone in a birth cohort for whom Smart Start funding was provided in the county between the ages of zero and five. For both programs we find that, in high poverty counties, the crime-reduction benefits are substantial, ranging from $408-$1,116 for Head Start and $3,514-$8,594 for Smart Start per affected individual (2015 dollars). For Head Start, these benefits amount to between one and three fifths of the cost of the program in high poverty counties, and exceed the costs if we assume that our violent conviction effects represent true effects.[49] For Smart Start, these benefits are more than twice the cost of the funding provided. Even in low-poverty counties, where Smart Start's benefits are smaller, they remain equal to or greater than the cost of the funding provided. These results support the conclusion that individual investments in early childhood education are inefficiently low without subsidies, particularly given the large impacts of these programs on other outcomes with substantial social benefits (e.g. education).

6 Conclusion

We contribute to the sparse literature on the developmental factors that influence an individual's likelihood of becoming a criminal by exploring the effect of early childhood education on criminal behavior. This relationship has become increasingly relevant given recent expansions in the share of children attending public preschools and widespread efforts to improve

[48]Benefits are calculated for each age (18-35 for Head Start and 18-24 for Smart Start) and then discounted back to childhood (age 4 for Head Start and age 2.5 for Smart Start) at the given rate for comparison with the program cost.

[49]Costs are similarly measured per individual in each affected birth cohort, separately for Smart Start and Head Start. For Head Start, costs are produced per four year old, following Barr and Gibbs (2017), and inflated to $2015 dollars for comparability with benefits. For Smart Start, costs are simply measured using the treatment variation inflated to $2015 dollars.

the quality of early childhood education. These expansions have been driven in large part by policymakers' belief that early childhood education has large impacts later in life. Crime reduction in particular is central to the widely publicized benefit-cost analyses of these programs (e.g., crime reduction accounts for 40-65% of the benefits estimated in the context of Perry preschool). However, inconsistent findings across similar studies, and an inability to account for the potential peer spillovers of a large-scale program, among other concerns, make the generalizability of crime effect estimates from earlier studies unclear. We bring new evidence to this literature by using administrative crime data to investigate the effects of two large-scale early childhood education programs operating in two different time periods. This approach yields substantial external validity improvements over prior long-run crime effect estimates by relying on a much broader treated population and enabling a direct comparison of similar adult crime reductions from two different interventions implemented in very different contexts.

We show that early childhood education reduces adult criminal behavior across two different programs and time periods. We find that Head Start availability in the 1960s and 1970s reduces the likelihood of a serious conviction by age 35 by 1.3 percentage points in high-poverty counties, but has no measurable effect in low-poverty counties. Implemented two to three decades later, Smart Start generates similar reductions in adult criminal behavior, with effects similarly concentrated in high-poverty counties and among black children. Taken together, these results suggest the general capacity of early childhood education to reduce the propensity for criminal behavior in those areas and among those individuals with the fewest resources. We also find that Smart Start's effects are largest in counties without Head Start access, which suggests that there are diminishing returns to early childhood education funding.

Finally, we show that the discounted benefits generated by early childhood education's later crime reduction are substantial relative to the costs of the education provided. This is especially noteworthy considering that later crime reduction was not the stated objective of either program and that these benefits likely accrue in large part to those who did not experience the program themselves. The magnitude of these external benefits implies that the social benefits of early childhood education vastly exceed the private benefits. That said, these benefits accrue disproportionately to high-poverty areas and to those lacking access to other early childhood education subsidies. Taken together, our results provide evidence in support of recent state efforts to expand and improve early childhood education, but point to large potential gains from targeting these efforts toward areas with the greatest need.

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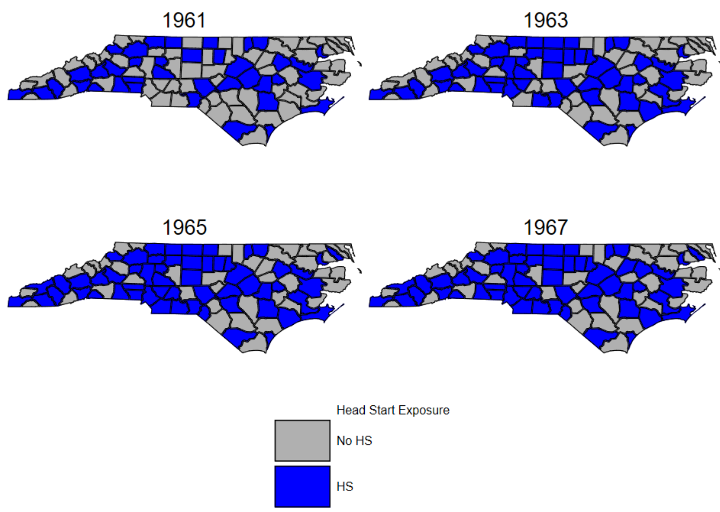
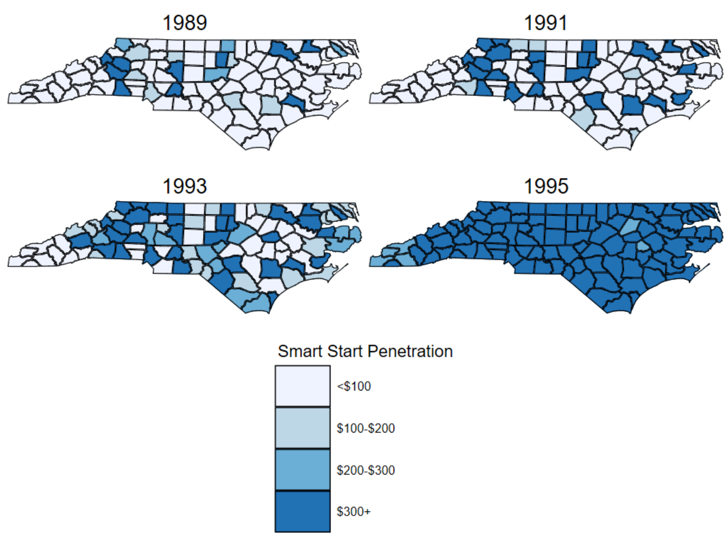
Figure 1: County by Birth Cohort Early Childhood Program Rollout in North Carolina



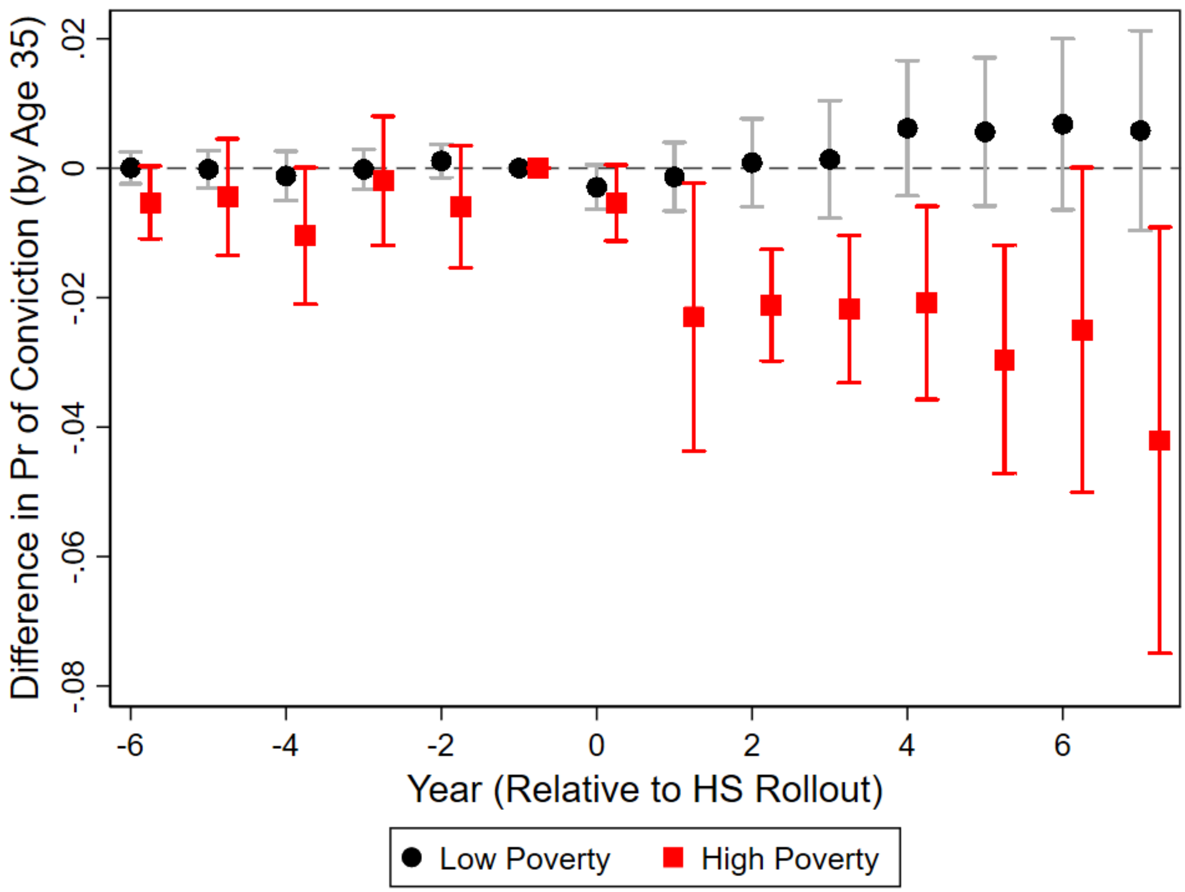
(a) Head Start Rollout



(b) Smart Start Rollout

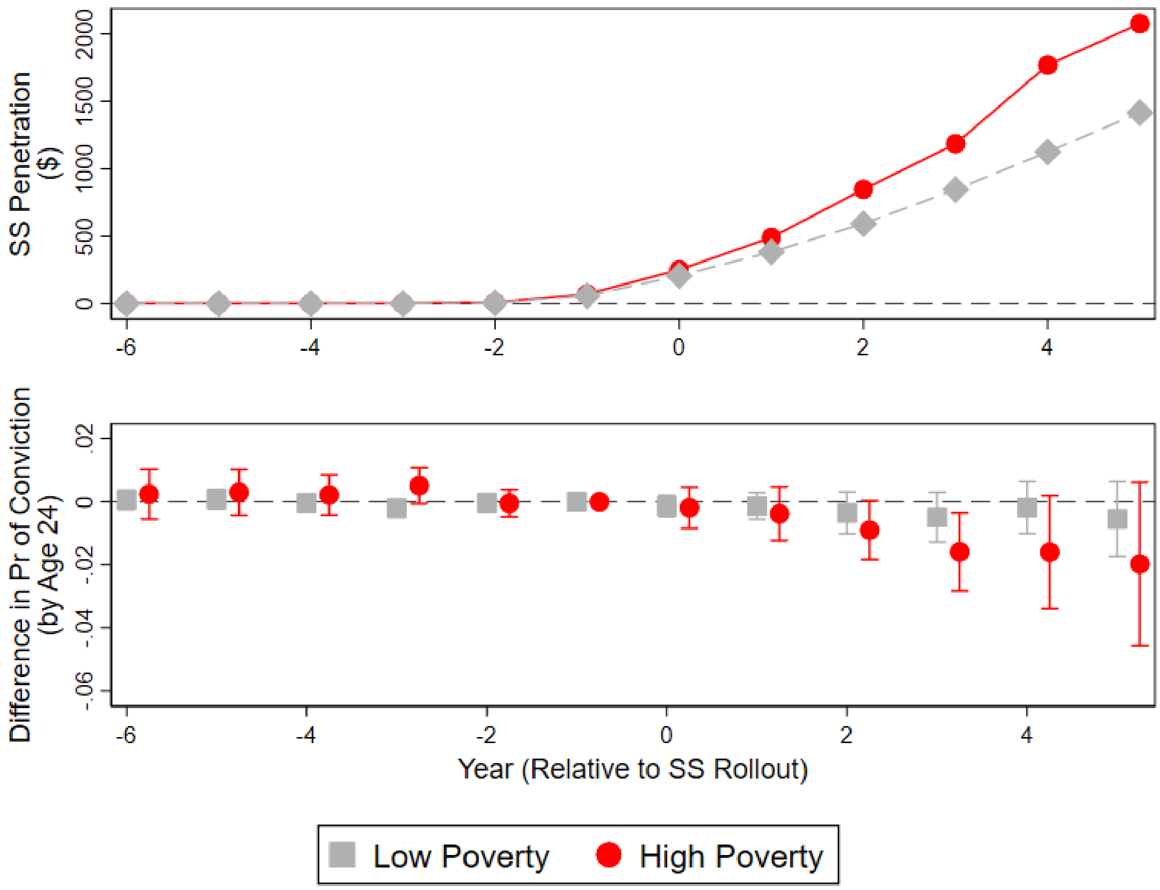
Note: Figure shows which birth-cohorts born to which counties had Head Start and Smart Start available to them in North Carolina. Panel (a): Prior to the 1961 birth cohort no counties had Head Start available. Head Start availability is identified from county by year level Head Start funding data following Barr and Gibbs (2017). Head Start funding levels are obtained from Head Start Historical Records. Panel (b): Prior to the 1989 birth cohort no counties had Smart Start funding penetration. Smart Start penetration is defined following Ladd et al. (2014) and depicted in nominal terms. Smart Start funding levels are obtained from the Smart Start organization.

Figure 2: Event Study of Head Start's Impact on Criminal Conviction



Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 separately for high and low poverty counties. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure 3: Event Study of Smart Start's Impact on Criminal Conviction



Note: In the top panel, figure shows trends in Smart Start funding penetration in nominal dollars defined following Ladd et al. (2014), separately for high and low poverty counties. In the bottom panel, figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 separately for high and low poverty counties. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects. Binary Smart Start availability, the independent variable of interest, is defined as Smart Start penetration level above the 25^{th} percentile of penetration. Those counties whose poverty rate in 1980 was above the median in North Carolina (17.3% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to cohorts who were born between 1980 and 1994.

Table 1: Descriptive Statistics

	All	High Poverty	Low Poverty
Panel A: Head Start Sample			
First Cohort With Head Start	1962.3	1962.3	1962.3
HS Funding (2015$ per 4 year old)	893.8	2061.1	605.7
Criminal Conviction	0.0476	0.0469	0.0478
Observations	882	308	574
Panel B: Smart Start Sample			
First Calendar Year of Smart Start	1995.5	1996.7	1995.0
SS Penetration (2015$)	818.9	750.0	838.3
Criminal Conviction	0.0514	0.0492	0.0522
Observations	1500	750	750

Note: Panel A contains summary statistics of crime outcome variables for the sample of birth cohorts born from 1955 to 1968, which is used in the Head Start analysis. Each observation is at the county birth-cohort level. Head Start availability and funding are reported in the first two rows. Funding levels are given in 2015 dollars and averaged over exposed county-cohorts only, so that only non-zero values are included. Criminal conviction by adulthood is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All variables are further broken down by county level poverty status. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. Panel B contains summary statistics of crime outcome variables for the sample of birth cohorts born from 1980 to 1994, which is used in the Smart Start analysis. Each observation is at the county birth-cohort level. Smart Start availability and funding penetration are reported in the first two rows of Panel B. Penetration measures are defined following Ladd et al. (2014) reported in 2015 dollars and averaged over exposed county-cohorts only, so that only non-zero values are included. Criminal conviction by adulthood is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. All variables are further broken down by county level poverty status. Those counties whose poverty rate in 1980 was above the median in North Carolina (17.3% poverty) are called "High Poverty", while those below the median are called "Low Poverty". Data sources are the NC Department of Corrections, the NC Department of Corrections, Head Start Historical Records, and Smart Start Records.

Table 2: Effect of Early Childhood Education on Criminal Conviction

	(1)	(2)	(3)
	All	High Poverty	Low Poverty
Panel A: Head Start			
Head Start Availability	-0.0018	-0.0131**	0.0026
	(0.0031)	(0.0057)	(0.0032)
Observations	882	308	574
Mean	0.0476	0.0469	0.0478
Panel B: Smart Start			
SS ($1000s)	-0.0065**	-0.0104**	-0.0040
	(0.0030)	(0.0051)	(0.0036)
Observations	1500	750	750
Mean	0.0514	0.0492	0.0522

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects. Panel A reports results using the Head Start sample. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. Panel B reports results using the Smart Start sample. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014). See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table 3: Effect of Smart Start Funding on Criminal Conviction - By Presence of Head Start

	(1)	(2)	(3)	(4)
	All		High Poverty	
SS ($1000s)	-0.0065**	-0.0146***	-0.0104**	-0.0154***
	(0.0030)	(0.0048)	(0.0051)	(0.0055)
Observations	1500	555	750	435
Mean	0.0514	0.0503	0.0492	0.0505
Head Start	All	No Head Start	All	No Head Start

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of either UCR Part 1 property crimes or Part 1 violent crimes in North Carolina by age 24. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014). See the notes to Table 1 for additional sample restrictions and definitions. Columns (2) and (4) further restrict the sample to counties without a Head Start program by 1980. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table 4: Effect of Early Childhood Education on Criminal Conviction- By Race

	(1)	(2)	(3)
	All	High Poverty	Low Poverty
Panel A: Head Start			
White			
Head Start Availability	-0.0030	-0.0098	-0.0001
	(0.0035)	(0.0061)	(0.0037)
Observations	667	252	415
Mean	0.0270	0.0262	0.0272
Non-White			
Head Start Availability	0.0011	-0.0129*	0.0115
	(0.0075)	(0.0068)	(0.0152)
Observations	667	252	415
Mean	0.0988	0.0733	0.1142
Panel B: Smart Start			
White			
SS ($1000s)	-0.0029	-0.0029	-0.0015
	(0.0019)	(0.0051)	(0.0021)
Observations	1329	674	655
Mean	0.0313	0.0269	0.0326
Non-White			
SS ($1000s)	-0.0188***	-0.0213***	-0.0127
	(0.0065)	(0.0059)	(0.0120)
Observations	1329	674	655
Mean	0.0954	0.0812	0.1058

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth cohort fixed effects. Panel A reports results using the Head Start sample for white cohorts and non-white cohorts separately. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals of a given race in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. (Sample sizes are smaller for these specifications because the natality files for 25% of counties in North Carolina do not have race breakdowns before 1969 and we do not know the race of approximately 13% of births in our sample.) The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. Panel B reports results using the Smart Start sample for white cohorts and non-white cohorts separately. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals of a given race in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014). (Sample sizes are smaller for these specifications because from 1989 to 1993 the natality files for 25% of counties in North Carolina do not have race breakdowns. For these years, race is available only for counties in which 1980 populations for the non-white group formed at least 10 percent of the total population or numbered at least 10,000.) See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

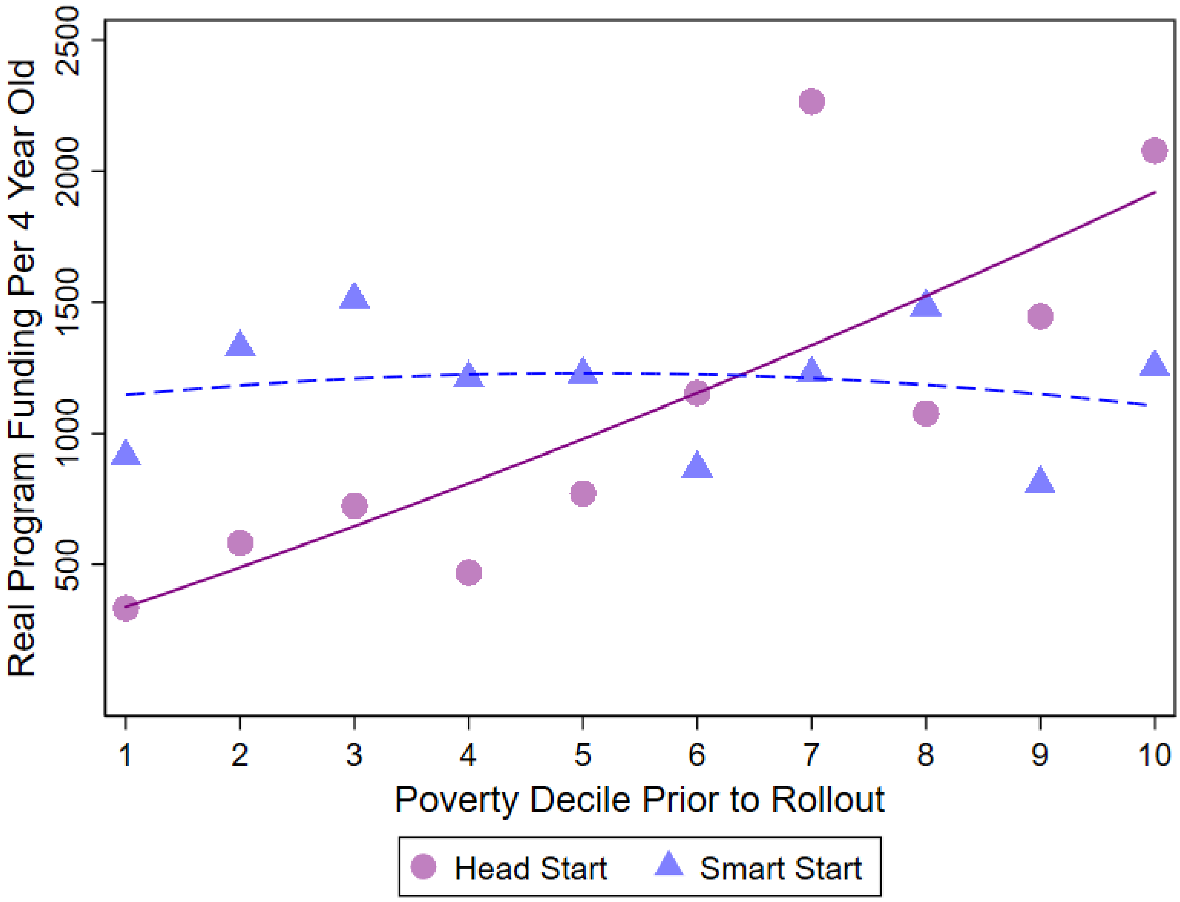
Table 5: Estimates of the Social Benefits of Crime Reduction from Early Childhood Education

	Cost Estimate ($ 2015)	Est. Δ Convictions	Est. Δ Crimes	Discounted Social Benefits 0%	3%	5%
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Head Start Availability						
Property Crime:						
Larceny	3,911	-0.0082	-0.2817	1,102	579	386
Burglary	7,155	-0.0003	-0.0091	65	34	22
Violent Crime:						
Murder	9,945,842	-0.0003	-0.0015	14,632	7,702	5,130
Assault	118,492	-0.0024	-0.0130	1,536	766	493
Robbery	46,845	-0.0008	-0.0043	202	109	73
			CRIME BENEFITS:	17,536	9,190	6,104
			BENEFIT-COST RATIO:	8.6	4.5	3.0
			CRIME BENEFITS (Property Only):	1,166	613	408
			BENEFIT-COST RATIO (Property Only):	0.6	0.3	0.2
Panel B: Smart Start Funding Exposure						
Property Crime:						
Larceny	3,911	-0.0032	-0.0779	305	179	126
Burglary	7,155	-0.0002	-0.0042	30	18	13
Violent Crime:						
Murder	9,945,842	-0.0002	-0.0007	7,020	4,073	2,863
Assault	118,492	-0.0026	-0.0087	1,028	599	422
Robbery	46,845	-0.0013	-0.0045	211	125	89
			CRIME BENEFITS:	8,594	4,994	3,514
			BENEFIT-COST RATIO:	5.2	3.0	2.1

Note: This table shows back-of-the-envelope calculations of the discounted social benefits of later crime reductions due to Head Start and Smart Start in high poverty counties. Social cost estimates for each crime type (Column 1) are adopted from McCollister et al. (2010). These estimates include victimization costs, criminal justice system costs, and the lost value of criminals' time, but do not include private expenditures on crime prevention. In Column 2, we report the estimated change in convictions by crime type, which we obtain by first dividing our property crime coefficient estimate by our estimated first stage and multiplying by mean number of property crimes of a particular type given any property conviction in North Carolina. In Column 3, we report the estimated change in criminal offenses associated with the given change in convictions. North Carolina has roughly 5.4 burglary and larceny arrests per conviction and roughly 5.8 reported burglary and larceny offenses per arrest (authors' calculations using statistics from the NC State Bureau of Investigation's "Crime in North Carolina -1995" report). Estimates of the discounted social benefit, contained in Columns 4-6, are produced by multiplying the dollar value of each offense's social cost by the change in offenses implied by our estimates (by age for ages 18-35 (18-24 for Smart Start)) discounting back to age 4 (for comparison with the program cost) at the given rate. All monetary values are in 2015 dollars.

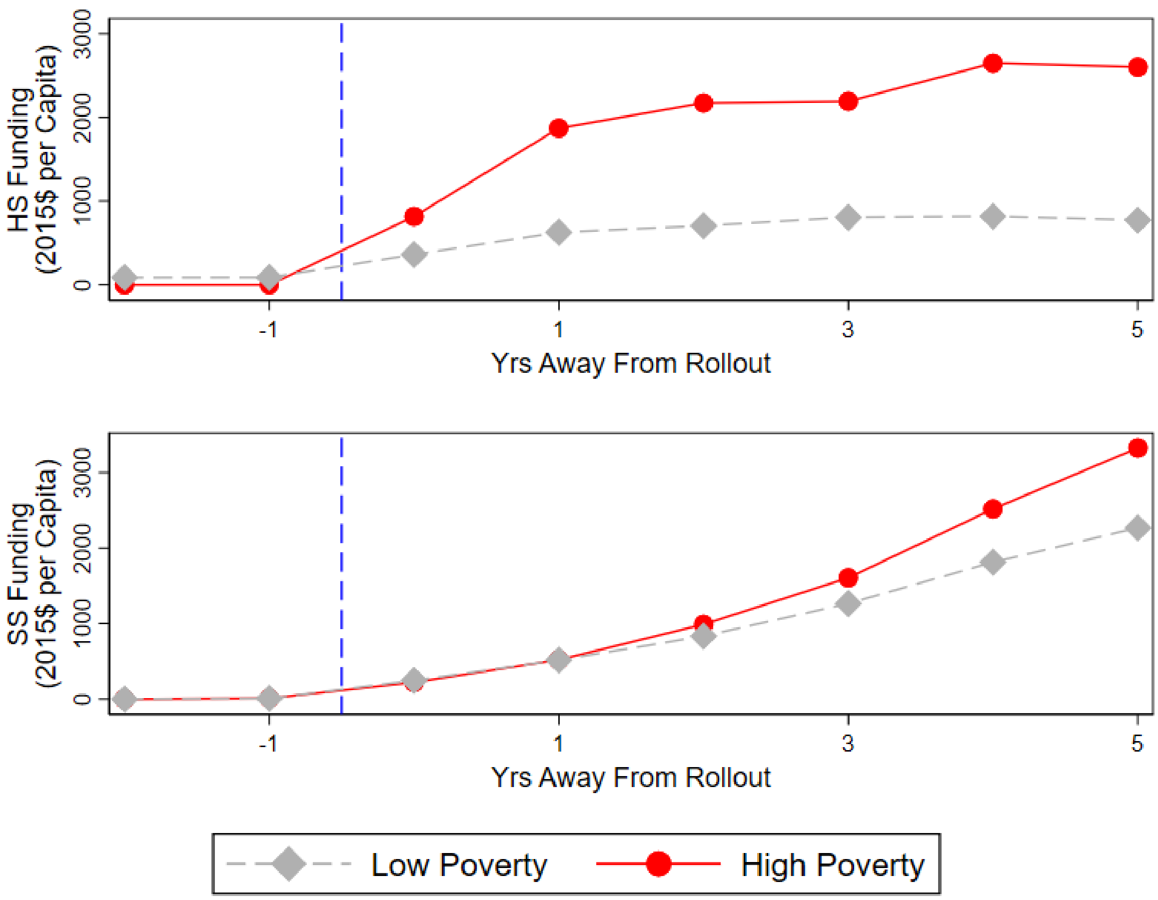
Appendix A: Supplementary Figures

Figure A1: Targeting of Head Start and Smart Start at Poverty



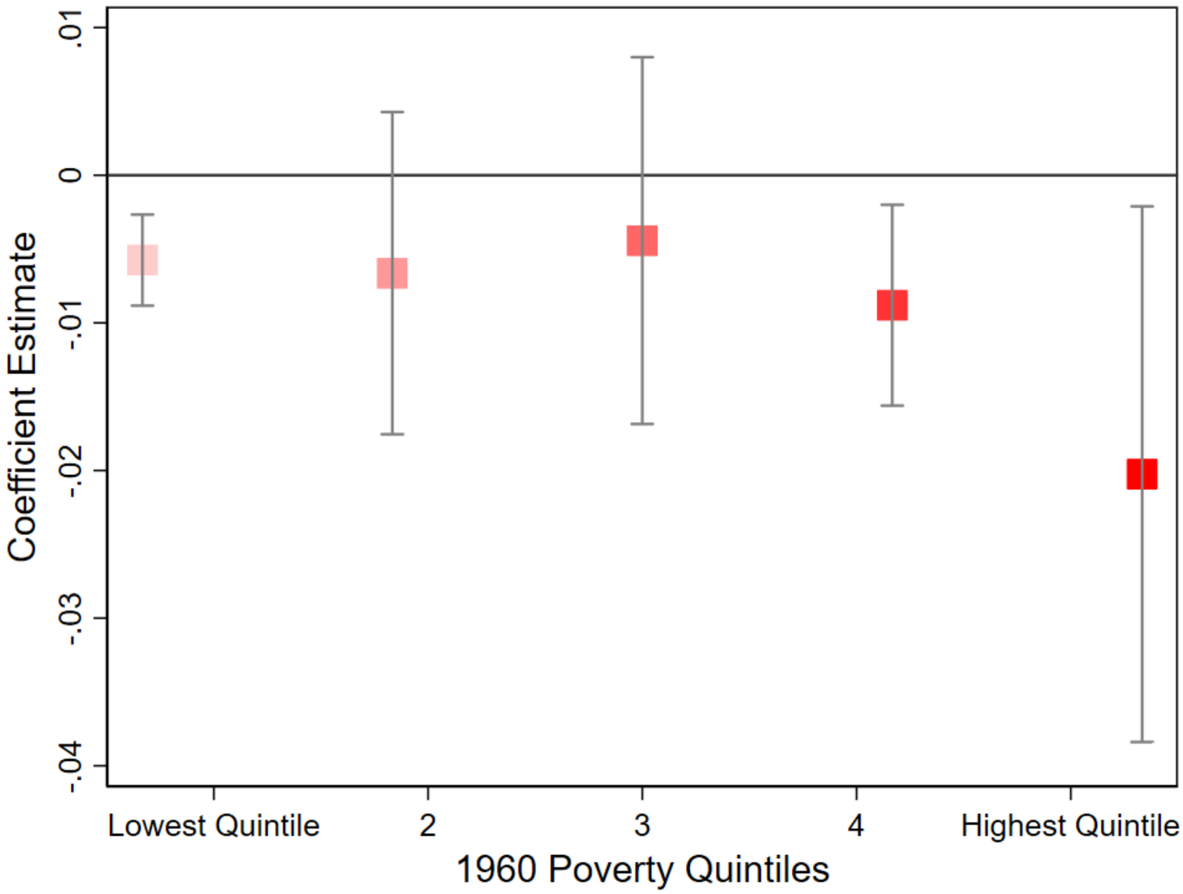
Note: Figure shows per capita county level Head Start and Smart Start funding (given in $ per 4 year olds) by county poverty deciles before program rollout. For Head Start 1960 poverty deciles are used, while for Smart Start 1980 poverty deciles are used. All values are in 2009 dollars. The sample is restricted to counties with nonzero funding amounts.

Figure A2: Head Start Funding By County Poverty Level



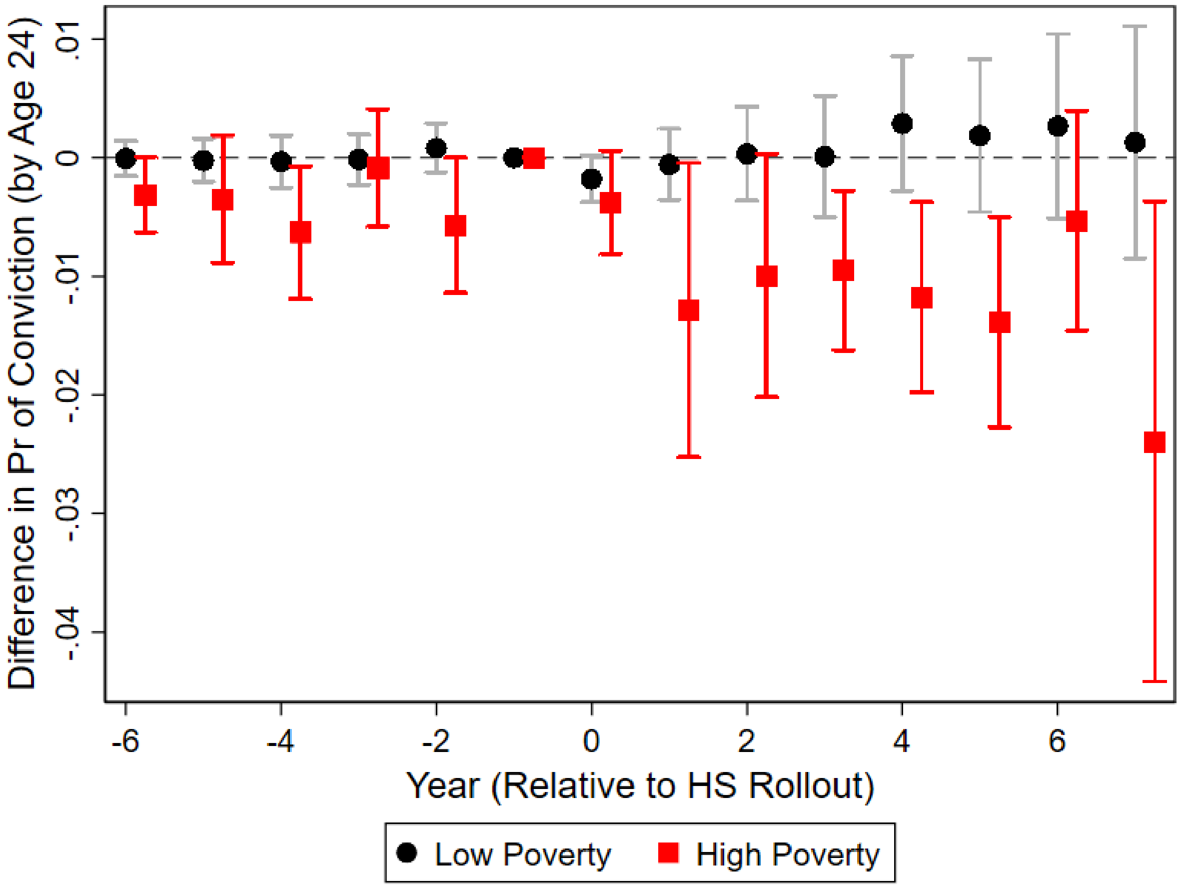
Note: Figure shows per capita county level Head Start and Smart Start funding (given in $ per 4 year olds) separately for high and low poverty counties. In the upper panel which shows Head Start funding, high poverty counties are those counties with a 1960 poverty rate above the median in North Carolina (40.2% poverty), while low poverty are those with a below median 1960 poverty rate. In the lower panel which shows Smart Start funding, high poverty counties are those counties whose poverty rate in 1980 was above the median in North Carolina (17.3% poverty) , while low poverty are those below the median. (There exist small, non-zero funding levels in the year prior to Head Start rollout for two reasons: first, following Barr and Gibbs (2017), county birth cohorts with very low funding levels are treated as not having Head Start availability, and, second, we do not count 1965 as the first year of availability since the Head Start program was introduced only as a pilot program over the Summer in that year.)

Figure A3: Head Start Estimates by Quintiles



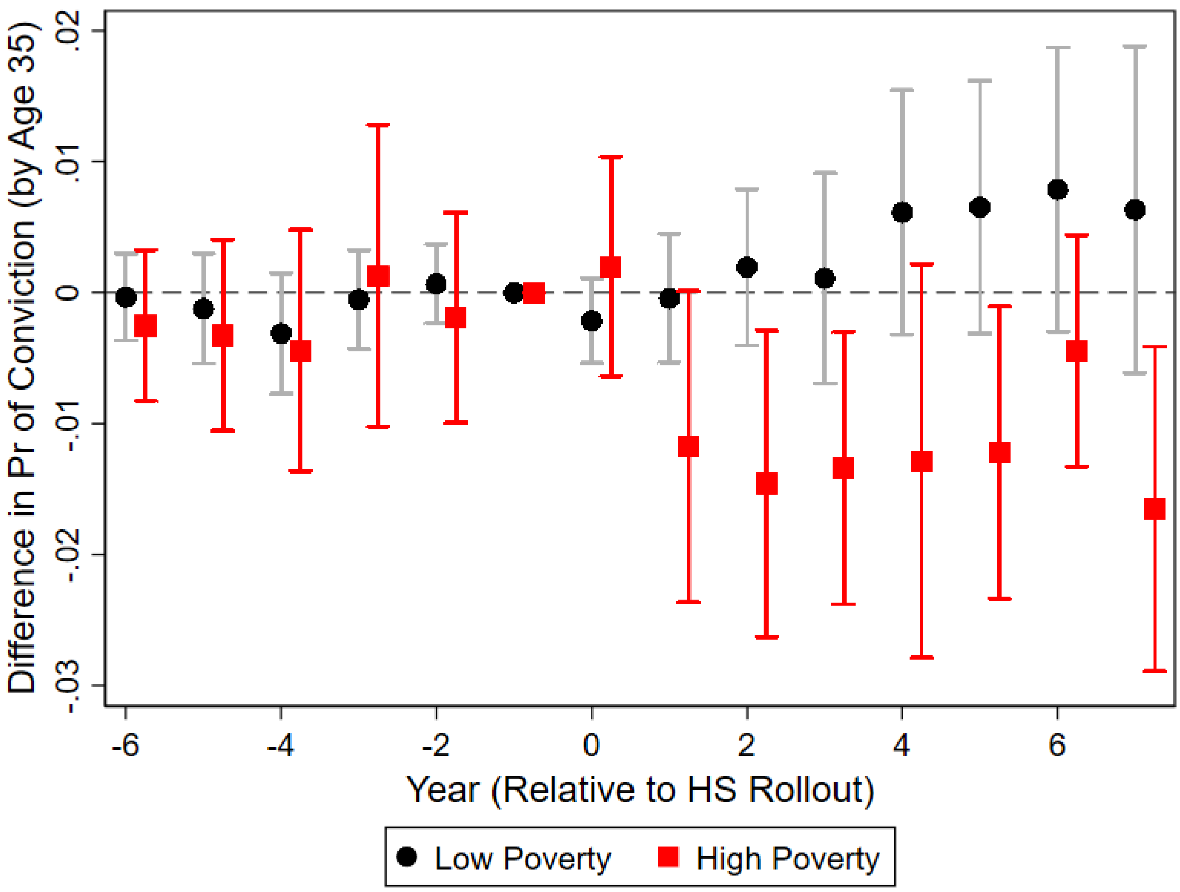
Note: Figure shows the coefficient estimates and 95% confidence intervals from estimating our basic difference-in-differences specification separately for counties in each quintile of the 1960 North Carolina poverty rate. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure A4: Event Study of Head Start's Impact on Criminal Conviction (by age 24)



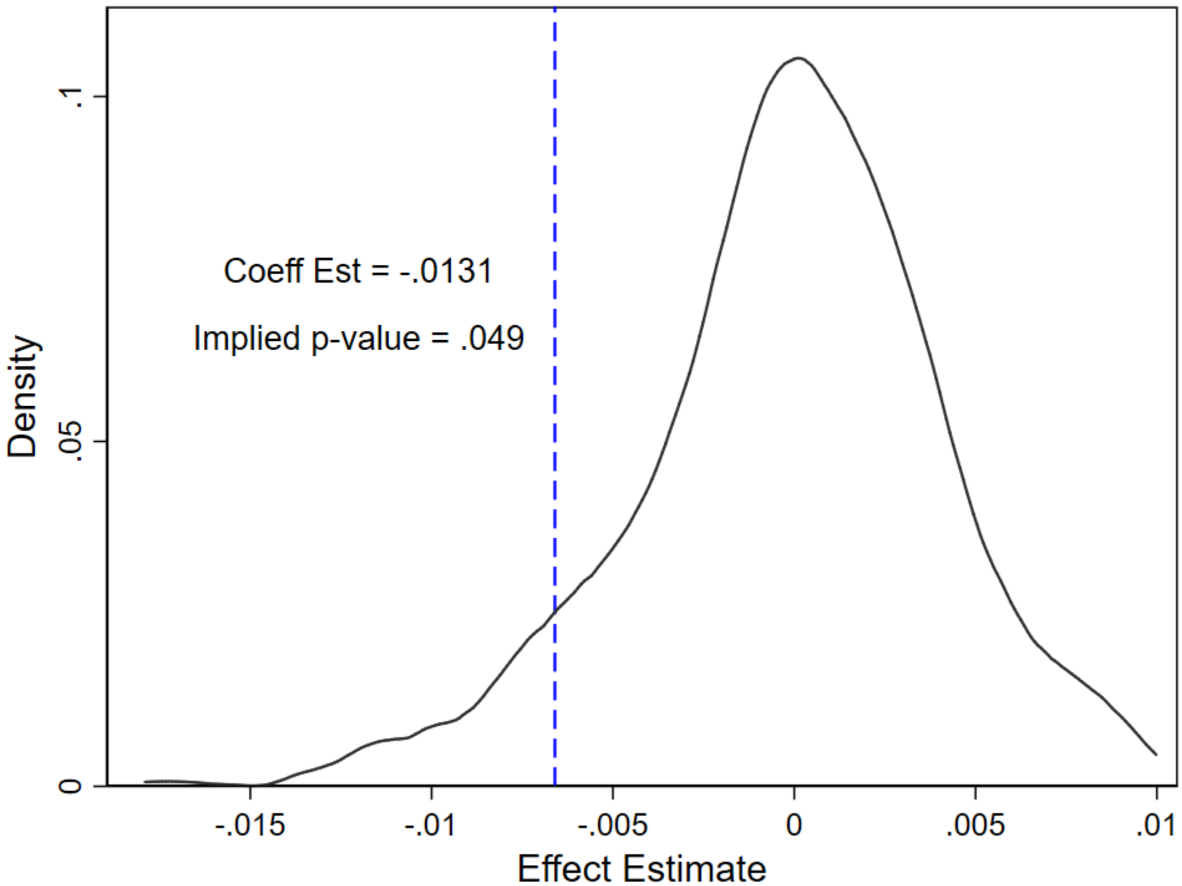
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 separately for high and low poverty counties. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure A5: Event Study of Head Start's Impact on Criminal Conviction – Robustness to Inclusion of All Counties



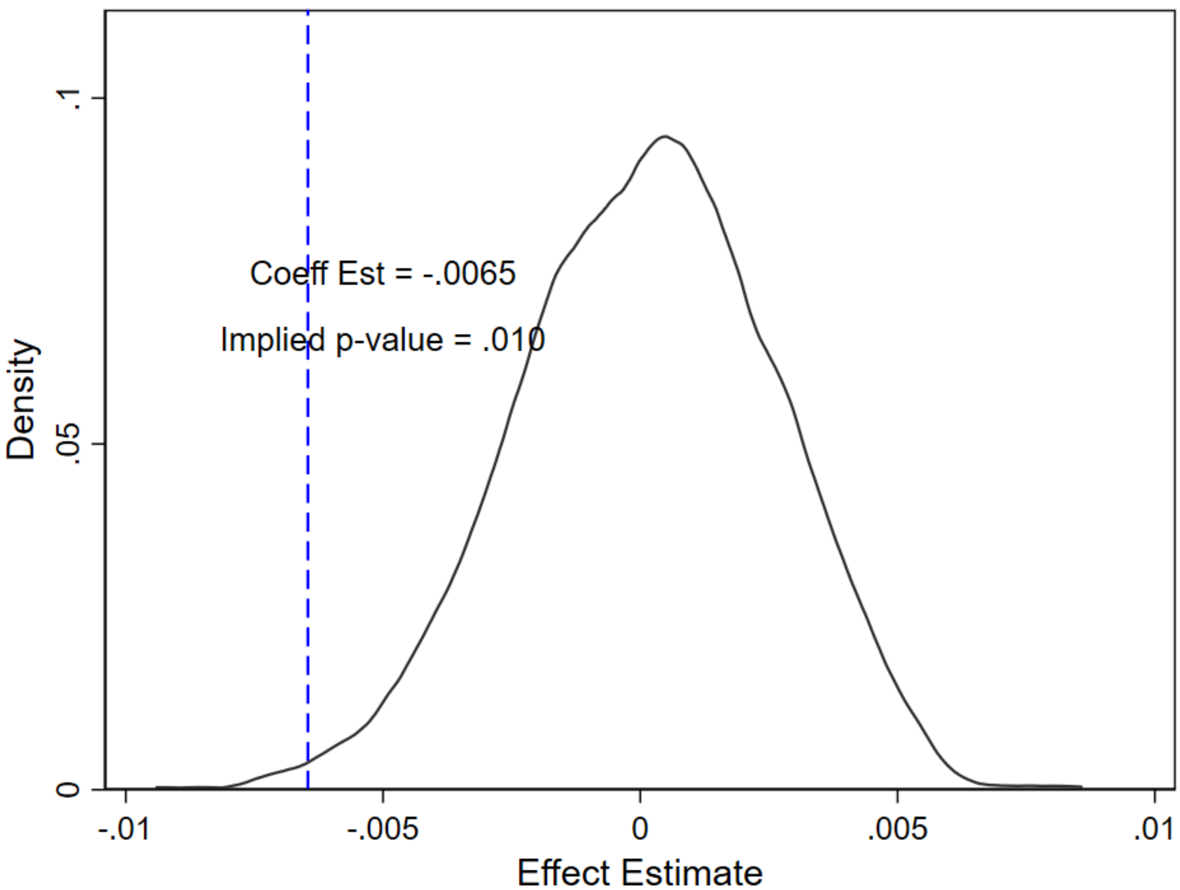
Note: Figure shows the coefficient estimates and 95% confidence interval from estimating Equation 2 separately for high and low poverty counties. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955 multiplied by the inverse propensity to receive Head Start as defined by Stuart (2010). The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. UCR Part 1 crimes include violent crimes (those in which the description of the offense contains the words "murder", "assault", or "robbery" (rape not being included), and property crimes (those in which the description of the offense contains the words "burglary" or "larceny"). All specifications include birth county and birth-cohort fixed effects as well as 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". The sample is restricted to counties that ever received Head Start between 1965 and 1976. The sample is further restricted to cohorts who were born between 1955 and 1968.

Figure A6: Head Start Randomization Inference (High-Poverty Counties)



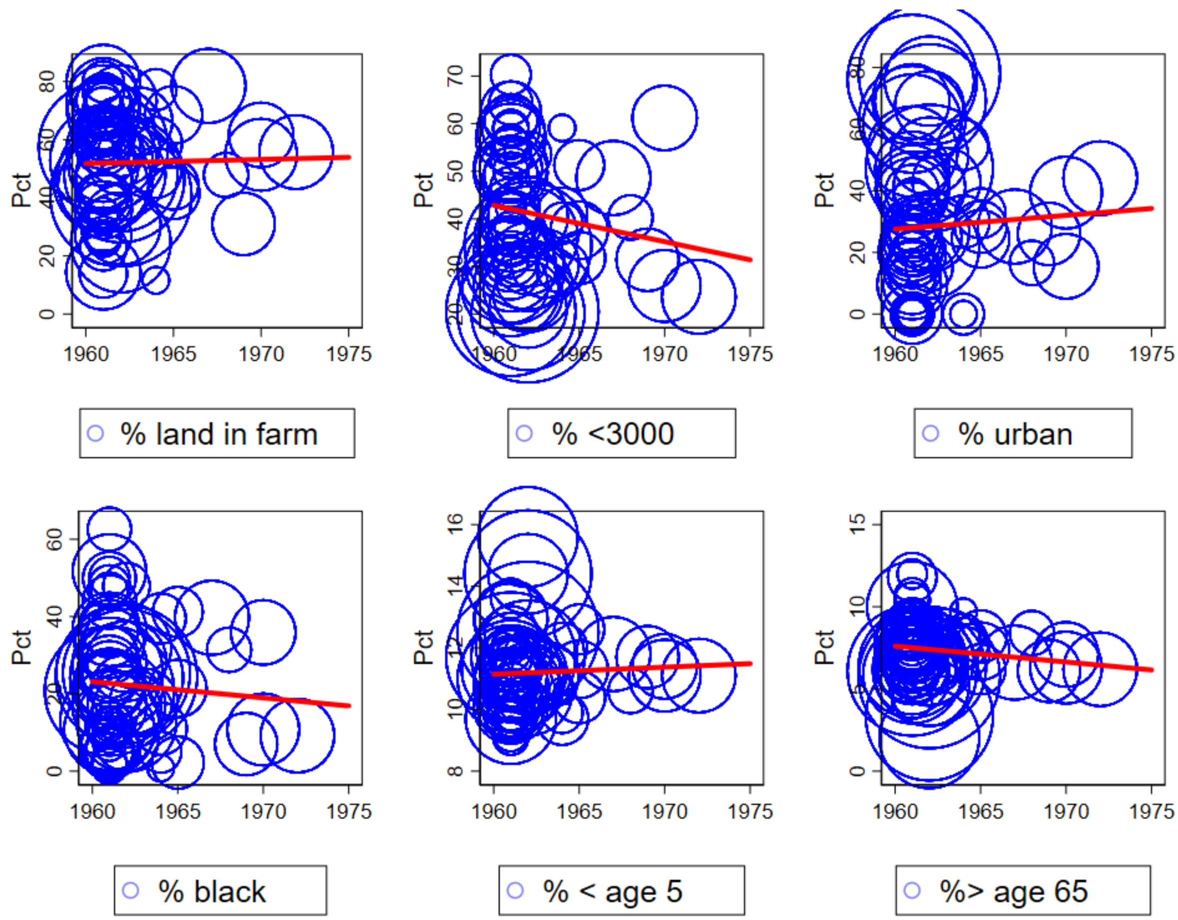
Note: Figure shows the kernel density of coefficient estimates under random assignment of Head Start availability to high poverty counties. 1000 repetitions were performed. The vertical line indicates the coefficient estimate obtained using the actual rollout of Head Start (See Table 2). A two-tailed test statistic is calculated as the share of estimates whose absolute value is greater than or equal to the estimate obtained using the actual rollout. Calculating this statistic gives an implied p-value of .049 as compared with the p-value of .038 given by the standard errors clustered at the county level.

Figure A7: Smart Start Randomization Inference (High-Poverty Counties)



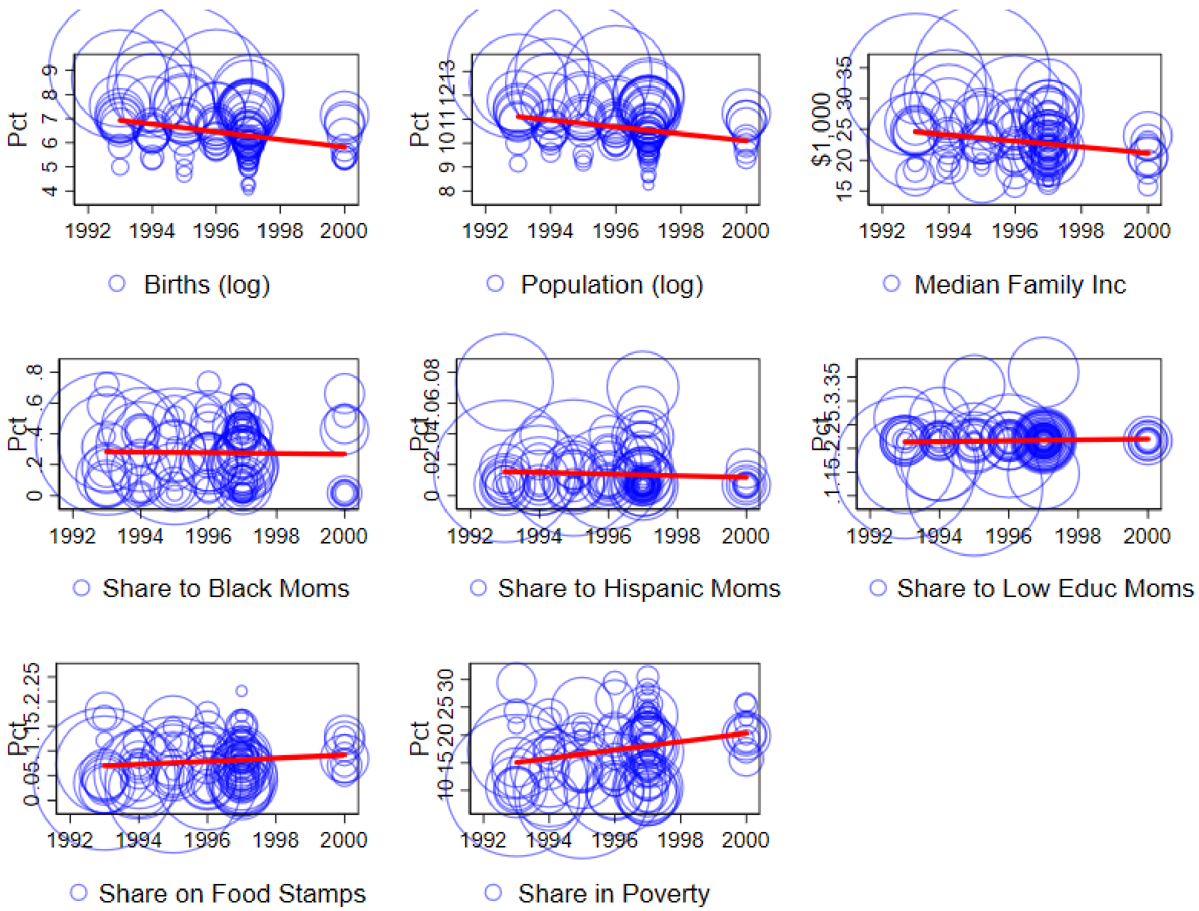
Note: Figure shows the kernel density of coefficient estimates under random assignment of Smart Start availability to high poverty counties. 1000 repetitions were performed. The vertical line indicates the coefficient estimate obtained using the actual rollout of Smart Start (See Table 2). A two-tailed test statistic is calculated as the share of estimates whose absolute value is greater than or equal to the estimate obtained using the actual rollout. Calculating this statistic gives an implied p-value of .01 as compared with the p-value of .033 given by the standard errors clustered at the county level.

Figure A8: Relationship between Year of Initial Head Start Funding and Baseline County Characteristics



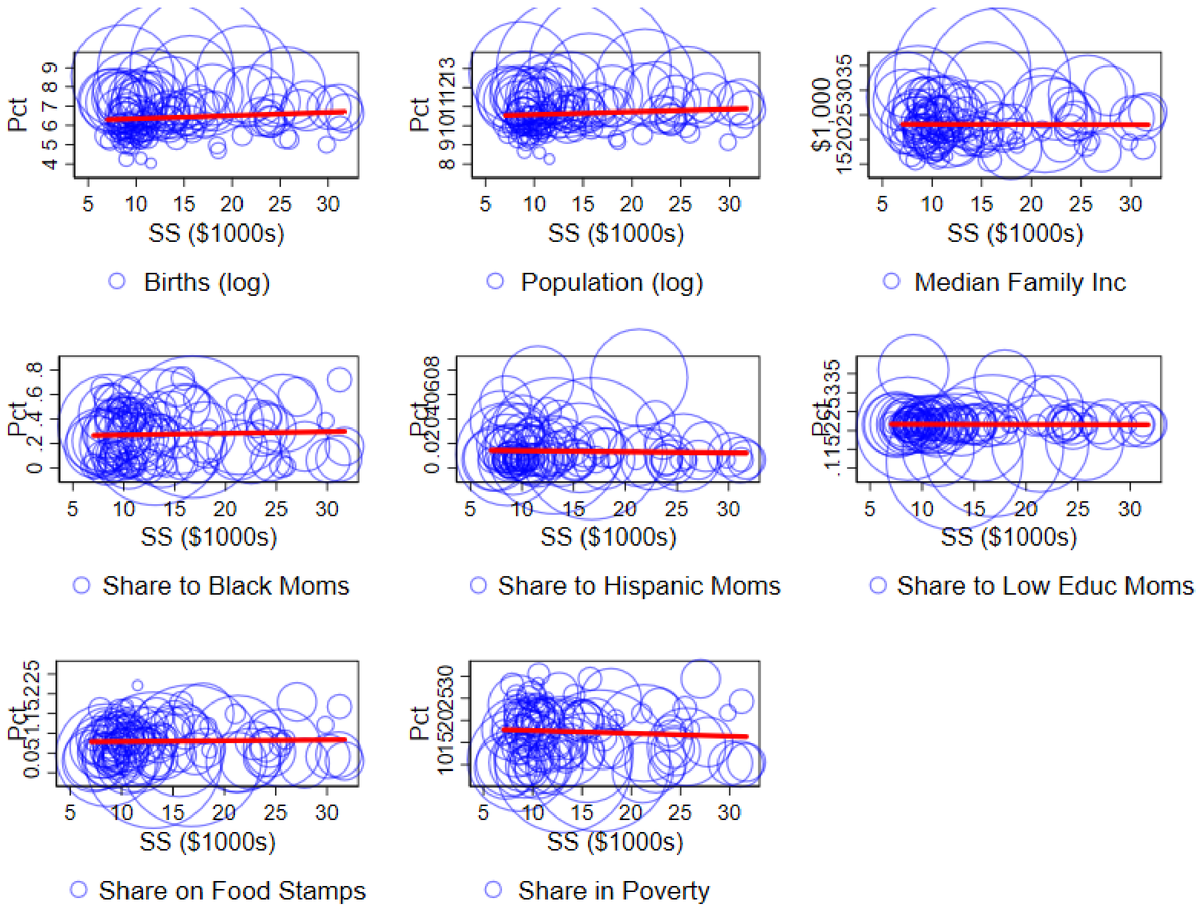
Note: Figure shows population weighted scatterplots of county characteristics against the year in which Head Start first became available in that county. Data are at the county level and weights are defined using 1955 births (represented by circle radius). A flat, horizontal fitted line suggests that the values of a given county characteristic are not systematically connected to the timing of Head Start availability.

Figure A9: Relationship between Year of Initial Smart Start Funding and Baseline County Characteristics



Note: Figure shows population weighted scatterplots of county characteristics against the year in which Smart Start first became available in that county. Data are at the county level and weights are defined using 1980 births (represented by circle radius). A flat, horizontal fitted line suggests that the values of a given county characteristic are not systematically connected to the timing of Smart Start availability.

Figure A10: Relationship between Total Smart Start Funding Levels and Baseline County Characteristics



Note: Figure shows population weighted scatterplots of county characteristics against the total amount of Smart Start funding received in a county over the sample period. Data are at the county level and weights are defined using 1980 births (represented by circle radius). A flat, horizontal fitted line suggests that the values of a given county characteristic are not systematically connected to total Smart Start funding.

Appendix: Supplementary Tables

Table A1: Head Start Availability and Criminal Conviction - Continuous Measure of Poverty Estimates

	All	
	(1)	(2)
HS Availability	0.0059	0.0044
	(0.0058)	(0.0041)
HS Availability X Poverty	-0.0202*	-0.0188**
	(0.0111)	(0.0078)
Observations	882	882
Mean	0.0476	0.0476
Baseline Chars X Trend		X

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort interacted with the county poverty rate in 1960. (The reported estimates are also scaled up by a factor of 100.). All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than \$3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. These regressions do not restrict the sample based on the county poverty rate in 1960. See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A2: Head Start Availability and Criminal Conviction – Effects by Age

	(1)	(2)	(3)
	All	High Poverty	Low Poverty
Conviction By Age 24			
Head Start Availability	-0.0005	-0.0043	0.0009
	(0.0015)	(0.0036)	(0.0015)
Mean	0.0238	0.0233	0.0239
Conviction By Age 30			
Head Start Availability	-0.0018	-0.0103**	0.0015
	(0.0024)	(0.0049)	(0.0025)
Mean	0.0375	0.0371	0.0377
Conviction By Age 35			
Head Start Availability	-0.0018	-0.0131**	0.0026
	(0.0031)	(0.0057)	(0.0032)
Mean	0.0476	0.0469	0.0478
Observations	882	308	574

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24, age 30 and age 35, respectively. The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A3: Smart Start Funding and Criminal Conviction - Continuous Measure of Poverty Estimates

	All	
	(1)	(2)
SS ($1000s)	-0.0005	0.0045
	(0.0047)	(0.0043)
SS ($1000s) X Poverty	-0.0408	-0.0713***
	(0.0283)	(0.0271)
Observations	1500	1500
Mean	0.0514	0.0514
Baseline Chars X Trend		X

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. The reported variable of interest is an indicator for whether Smart Start was available to a given county birth cohort interacted with the county poverty rate in 1980. (The reported estimates are also scaled up by a factor of 100.). All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1980 county characteristics interacted with a time trend in birth cohort. Following Ladd et al. (2014), the 1980 county characteristics include the share of births to black mothers, the share of births to Hispanic mothers, the share of births to low education mothers, the share of the population using food stamps, the total number of births, the total population, and the median family income. These regressions do not restrict the sample based on the county poverty rate in 1980. See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A4: Smart Start Availability and Criminal Conviction – Robustness to Conviction Age

	(1)	(2)	(3)
	All	High Poverty	Low Poverty
Conviction By Age 24			
SS ($1000s)	-0.0065**	-0.0104**	-0.0040
	(0.0030)	(0.0051)	(0.0036)
Observations	1500	750	750
Mean	0.0514	0.0492	0.0522
Conviction By Age 25			
SS ($1000s)	-0.0060*	-0.0107**	-0.0032
	(0.0032)	(0.0052)	(0.0039)
Observations	1400	700	700
Mean	0.0566	0.0537	0.0575
Conviction By Age 26			
SS ($1000s)	-0.0071*	-0.0122**	-0.0038
	(0.0041)	(0.0060)	(0.0050)
Observations	1300	650	650
Mean	0.0610	0.0576	0.0621

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24, age 25 or age 26, respectively. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014). See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A5: Effect of Early Childhood Education on Criminal Conviction - By Crime Type

	(1)	(2)	(3)
	All	High Poverty	Low Poverty
Panel A: Head Start			
Property Crime			
Head Start Availability	-0.0024	-0.0084***	-0.0000
	(0.0016)	(0.0028)	(0.0017)
Mean	0.0256	0.0255	0.0256
Violent Crime			
Head Start Availability	0.0005	-0.0046	0.0026
	(0.0017)	(0.0032)	(0.0018)
Mean	0.0212	0.0206	0.0214
Observations	882	308	574
Panel B: Smart Start			
Property Crime			
SS ($1000s)	-0.0024*	-0.0041*	-0.0009
	(0.0013)	(0.0024)	(0.0016)
Mean	0.0272	0.0257	0.0277
Violent Crime			
SS ($1000s)	-0.0041**	-0.0063**	-0.0031
	(0.0019)	(0.0030)	(0.0024)
Mean	0.0243	0.0235	0.0245
Observations	1500	750	750

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects. Panel A reports results using the Head Start sample. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of either a UCR Part 1 property crime or a Part 1 violent crime in North Carolina by age 35. Panel B reports results using the Smart Start sample. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of either a Part 1 property crime or a Part 1 violent crime in North Carolina by age 24. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014). See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A6: Effect of Smart Start Funding on Criminal Conviction - By Race, By Presence of Head Start

	(1)	(2)	(3)	(4)
	All		High Poverty	
White				
SS ($1000s)	-0.0029	-0.0065	-0.0029	-0.0066
	(0.0019)	(0.0046)	(0.0051)	(0.0059)
Observations	1329	470	674	372
Mean	0.0313	0.0296	0.0269	0.0262
Non-White				
SS ($1000s)	-0.0188***	-0.0270***	-0.0213***	-0.0286***
	(0.0065)	(0.0052)	(0.0059)	(0.0056)
Observations	1313	454	662	360
Mean	0.0954	0.0829	0.0812	0.0813
Head Start	All	No Head Start	All	No Head Start

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects. Results are reported separately for white cohorts and non-white cohorts. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals of a given race in a given birth county and birth year cohort that are later convicted of either UCR Part 1 crimes in North Carolina by age 24. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014). (Sample sizes are smaller for these specifications because from 1989 to 1993 the natality files for 25% of counties in North Carolina do not have race breakdowns. For these years, race is available only for counties in which 1980 populations for the non-white group formed at least 10 percent of the total population or numbered at least 10,000.) See the notes to Table 1 for additional sample restrictions and definitions. Columns (2) and (4) further restrict the sample to counties without a Head Start program by 1980. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A7: Effect of Early Childhood Education on Criminal Conviction – With Trends

	(1)	(2)	(3)	(4)	(5)	(6)
	All		High		Low	
Panel A: Head Start						
Head Start Availability	-0.0018	-0.0030	-0.0131**	-0.0131**	0.0026	0.0012
	(0.0031)	(0.0030)	(0.0057)	(0.0059)	(0.0032)	(0.0040)
Observation	882	882	308	308	574	574
Mean	0.0476	0.0476	0.0469	0.0469	0.0478	0.0478
Panel B: Smart Start						
SS ($1000s)	-0.0065*	-0.0060*	-0.0104*	-0.0097*	-0.0040	-0.0035
	(0.0030)	(0.0025)	(0.0051)	(0.0040)	(0.0036)	(0.0027)
Observations	1500	1500	750	750	750	750
Mean	0.0514	0.0514	0.0492	0.0492	0.0522	0.0522
Baseline Chars X Trend		X		X		X

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. In Panel A, observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. In Panel B, observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014) All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1980 county characteristics interacted with a time trend in birth cohort. Following Ladd et al. (2014), the 1980 county characteristics include the share of births to black mothers, the share of births to Hispanic mothers, the share of births to low education mothers, the share of the population using food stamps, the total number of births, the total population, and the median family income. See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A8: Effect of Early Childhood Education on Criminal Conviction - Robustness to Inclusion of Time-Varying Covariates

	(1)	(2)	(3)
	All	High Poverty	Low Poverty
SS ($1000s)	-0.0067**	-0.0098**	-0.0044
	(0.0028)	(0.0048)	(0.0033)
Observations	1500	750	750
Mean	0.0514	0.0492	0.0522
Covariates	Yes	Yes	Yes

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects, as well as a set of county-year level covariates. Following Ladd et al. (2014), the county-year level characteristics include the share of births to black mothers, the share of births to Hispanic mothers, the share of births to low education mothers, the share of the population using food stamps, the total number of births, the total population, and the median family income. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014) See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A9: Effect of Smart Start Funding on Criminal Conviction - By Presence of Head Start

	(1)	(2)	(3)	(4)
	All		High Poverty	
SS (\$1000s)	-0.0067**	-0.0161**	-0.0098**	-0.0154***
	(0.0028)	(0.0046)	(0.0048)	(0.0055)
Observations	1500	555	750	435
Mean	0.0514	0.0503	0.0492	0.0505
Head Start	All	No Head Start	All	No Head Start
Covariates	Yes	Yes	Yes	Yes

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects, as well as a set of county-year level covariates. Following Ladd et al. (2014), the county-year level characteristics include the share of births to black mothers, the share of births to Hispanic mothers, the share of births to low education mothers, the share of the population using food stamps, the total number of births, the total population, and the median family income. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1980. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 24. The reported variable of interest is a measure of Smart Start funding penetration for a given county birth cohort, constructed following Ladd et al. (2014). See the notes to Table 1 for additional sample restrictions and definitions. Columns (2) and (4) further restrict the sample to counties without a Head Start program by 1980. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A10: Other War On Poverty Programs and Head Start - High Poverty Counties

	High Poverty				
	(1)	(2)	(3)	(4)	(5)
Head Start Availability	-0.0131**	-0.0126**	-0.0126**	-0.0158**	-0.0153**
	(0.0059)	(0.0058)	(0.0059)	(0.0068)	(0.0061)
Observations	308	308	308	308	308
Mean	0.0469	0.0469	0.0469	0.0469	0.0469
Baseline Chars X Trend	X		X		X
WOP Controls	None	FS	FS	FS + Other WOP	FS + Other WOP

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than \$3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. In these specifications, controls for exposure to various War on Poverty programs, including the Food Stamp Program (FS) are also included. "Other War on Poverty Programs" are those recommended by Bailey and Goodman-Bacon (2015) and include per capita expenditures on Public Assistance Transfers, Medicaid expenditures, Community Health Centers and Community Action Agencies. See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$)

Table A11: Relationship between Head Start Availability and Possible Confounders

	All		High Poverty		Low Poverty	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: War on Povery Programs						
0-5 Food Stamp Exposure	-0.0429	-0.0429	0.0666	0.0666	-0.0639	-0.0639
	(0.0802)	(0.0802)	(0.1593)	(0.1593)	(0.0950)	(0.0950)
Public Assistance Transfers	6.9467	6.9467	0.7701	0.7701	11.5153**	11.5153**
	(5.7513)	(5.7513)	(6.7353)	(6.7353)	(4.5238)	(4.5238)
Medicaid	9.2430	9.2430	2.2806	2.2806	12.8272**	12.8272**
	(5.7438)	(5.7438)	(5.9320)	(5.9320)	(5.9322)	(5.9322)
Community Health Center Funds	681.9056	681.9056	-82.5776	-82.5776	916.6069	916.6069
	(521.5891)	(521.5891)	(502.3024)	(502.3024)	(720.6964)	(720.6964)
CAP Seniors Program Grant	0.0356	0.0356	0.0297	0.0297	0.0302	0.0302
	(0.0521)	(0.0521)	(0.0407)	(0.0407)	(0.0734)	(0.0734)
Legal Services Program Grant	0.0460	0.0460	-0.0013	-0.0013	0.0557	0.0557
	(0.0333)	(0.0333)	(0.0080)	(0.0080)	(0.0434)	(0.0434)
Panel B: Health						
Adjusted Mortality Rate, All Ages	0.8698	0.8698	19.5563	19.5563	-5.3087	-5.3087
	(9.0852)	(9.0852)	(17.5338)	(17.5338)	(9.3156)	(9.3156)
White, Infant Mortality Rate	0.4678	0.4678	0.2843	0.2843	0.1544	0.1544
	(0.8910)	(0.8910)	(1.3270)	(1.3270)	(1.4135)	(1.4135)
Nonwhite Infant Mortality Rate	-2.2424	-2.2424	-3.6512	-3.6512	-1.4567	-1.4567
	(2.3849)	(2.3849)	(3.8859)	(3.8859)	(3.2687)	(3.2687)
Infant Mortality Rate	-1.1731	-1.1731	-1.1761	-1.1761	-1.5512	-1.5512
	(0.9761)	(0.9761)	(1.1773)	(1.1773)	(1.0473)	(1.0473)
Neonatal Infant Mortality Rate	0.2524	0.2524	0.1171	0.1171	0.1792	0.1792
	(0.8686)	(0.8686)	(1.0237)	(1.0237)	(1.1750)	(1.1750)
Postneonatal Infant Mortality Rate	-1.4255*	-1.4255*	-1.2931	-1.2931	-1.7304**	-1.7304**
	(0.7816)	(0.7816)	(1.3091)	(1.3091)	(0.6643)	(0.6643)
Observations	882	882	308	308	574	574
Baseline Chars X Trend		X		X		X

Note: Each cell reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. All specifications include birth county and birth-cohort fixed effects. Observations are at the birth county by birth year level and are weighted by the number of births in each county in 1955. In each row the dependent variable is a county-year measure of spending or infant health that could potentially confound our estimates of the impact of Head Start. All dependent variables are taken from Bailey et al (2015). The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than \$3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A12: Head Start and Likelihood of Residing in One's State of Birth (Census)

	National				South			
	All		Men Only		All		Men Only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction with HS Avail.	-0.021	0.004	-0.017	0.009	-0.018	-0.009	-0.013	-0.005
	(0.016)	(0.008)	(0.017)	(0.008)	(0.029)	(0.020)	(0.029)	(0.021)
Obs	3,150,292	3,150,292	1,546,355	1,546,355	1,002,875	1,002,875	487,059	487,059
Mean	0.66	0.66	0.66	0.66	0.68	0.68	0.68	0.68
State Linear Trend		X		X		X		X

Note: Each cell represents a separate OLS regression with standard errors clustered at the state of birth level (in parentheses). Observations are at the individual level from the 1990 and 2000 Census. The dependent variable is whether an individual is currently living in his or her state of birth. The key explanatory variables are measures of Head Start availability for a birth cohort in a particular state. This is the weighted average of the Head Start availability variable across counties in a state, where the weights are the number of births in each county in 1960. All specifications include birth state and birth year fixed effects as well as indicators for race, age, and sex. Sample restricted to ages 18-35. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Table A13: Relationship Between Head Start Ever Available in Sample Period and Baseline County Characteristics

	All	High Poverty	Low Poverty
	(1)	(2)	(3)
Head Start Ever Available In County			
1960 CCDB: % of land in farming	0.00353	0.00782	0.00110
	(0.0177)	(0.0290)	(0.0265)
1960 CCDB: % of people living in families with $\leq$ \$3000	-0.0503	0.0939	-0.381***
	(0.0369)	(0.0945)	(0.123)
1960 CCDB: % of population urban	-0.0297	-0.00259	-0.0712
	(0.0283)	(0.0427)	(0.0621)
1960 CCDB: % of people black	0.00244	0.0233	0.00175
	(0.0259)	(0.0272)	(0.0472)
1960 CCDB: % of people $\leq$ age 5	-0.364	-0.766	0.696
	(0.404)	(0.488)	(0.678)
1960 CCDB: % of people $\geq$ age 65	-0.112	-0.568	1.137
	(0.337)	(0.423)	(1.014)
1960 CCDB: % of employment in agriculture	-9.870	-25.45	16.11
	(14.23)	(20.82)	(20.80)
1960 CCBD: log population	1.720**	0.988	4.548*
	(0.717)	(0.791)	(2.478)
Observations	100	50	50
Mean	0.630	0.440	0.820

Note: Each column reports a separate logistic regression of an indicator for whether a county ever got Head Start by 1976 against the eight county level characteristics recommended in Hoynes and Schanzenbach (2009) and drawn from the 1960 City and County Data Books (CCDB). Observations are at the county level. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$)

Table A14: Relationship Between Year of Initial Head Start Availability and Baseline County Characteristics

	All	High Poverty	Low Poverty
	(1)	(2)	(3)
First Birth Cohort in County To Have Head Start			
1960 CCDB: % of land in farming	0.00735	-0.0396	0.00827
	(0.0234)	(0.0658)	(0.0330)
1960 CCDB: % of people living in families with $\leq$ \$3000	-0.0375	0.0158	-0.138
	(0.0473)	(0.168)	(0.105)
1960 CCDB: % of population urban	-0.0122	-0.00119	-0.000130
	(0.0235)	(0.0358)	(0.0607)
1960 CCDB: % of people black	0.00251	0.0215	-0.0117
	(0.0328)	(0.0756)	(0.111)
1960 CCDB: % of people $\leq$age 5	-0.198	-0.187	0.115
	(0.387)	(1.374)	(0.582)
1960 CCDB: % of people $\geq$age 65	-0.418	-0.358	-0.350
	(0.259)	(0.898)	(0.266)
1960 CCDB: % of employment in agriculture	-4.219	-7.168	1.668
	(13.35)	(34.97)	(19.64)
1960 CCBD: log population	-0.397	1.003	-1.444
	(0.749)	(1.483)	(1.224)
Observations	63	22	41
Mean	0.381	0.0455	0.561

Note: Each column reports a separate OLS regression of the birth year (normalized to 1962) of the first birth cohort in a given county to which Head Start was available against the eight county level characteristics recommended in Hoynes and Schanzenbach (2009) and drawn from the 1960 City and County Data Books (CCDB). Observations are at the county level. Those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty) are called "High Poverty", while those below the median are called "Low Poverty". Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$)

Table A15: Relationship Between Smart Start Penetration in Sample Period and Baseline County Characteristics

	All	High Poverty	Low Poverty
	(1)	(2)	(3)
Total SS Funding ($1000s)			
1980 Share to Hispanic Moms	-0.1308	0.1115	-0.1408
	(0.3727)	(0.3814)	(0.5061)
1980 Share to Black Moms	-0.0009	-0.0170	0.0412
	(0.0323)	(0.0395)	(0.0647)
1980 Share to Low Educ Moms	-0.0102	-0.1082	-0.0093
	(0.1020)	(0.2400)	(0.1203)
1980 Births (log)	0.9310	-0.4084	0.0721
	(3.8694)	(3.6033)	(6.0506)
1980 Median Family Inc	-0.0319	-0.1871	-0.1373
	(0.1681)	(0.2787)	(0.2467)
1980 Population (log)	-0.7874	0.2502	-0.0541
	(4.0203)	(3.4664)	(6.2099)
1980 Share on Food Stamps	-0.0224	0.2944	-0.1224
	(0.1649)	(0.2359)	(0.2521)
Observations	100	50	50
Mean	2.0458	1.2738	2.2835

Note: Each column reports a separate OLS regression of total county-level Smart Start penetration funding against selected 1980 county characteristics. Following Ladd et al. (2014), the 1980 county characteristics include the share of births to black mothers, the share of births to Hispanic mothers, the share of births to low education mothers, the share of the population using food stamps, the total number of births, the total population, and the median family income. Observations are at the birth-county level. Those counties whose poverty rate in 1980 was above the median in North Carolina (17.3% poverty) are called "High Poverty", while those below the median are called "Low Poverty". Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$)

Table A16: Effect of Head Start Availability on Criminal Conviction - Robustness of High Poverty Estimates to Inclusion of Counties that Did Not Receive Head Start

	High Poverty			
	Baseline		Inverse Propensity Weighted	
	(1)	(2)	(3)	(4)
Head Start Availability	-0.0131**	-0.0131**	-0.0107**	-0.0093**
	(0.0057)	(0.0059)	(0.0040)	(0.0038)
Observations	308	308	700	700
Mean	0.0469	0.0469	0.0436	0.0436
Baseline Chars X Trend		X		X

Note: Each column reports a separate OLS regression with standard errors clustered at the birth county level and reported in parentheses. Observations are at the birth county by birth year level and are weighted by number of births in each county in 1955 multiplied by the inverse propensity to be receive Head Start as defined by Stuart (2010). The dependent variable is the fraction of individuals in a given birth county and birth year cohort that are later convicted of a UCR Part 1 crime in North Carolina by age 35. The reported variable of interest is an indicator for whether Head Start was available to a given county birth cohort. All specifications include birth county and birth-cohort fixed effects, and, where indicated, 1960 county characteristics interacted with a time trend in birth cohort. 1960 county characteristics include: percent of land in farming, percent of people living in families with less than $3,000, percent of population in urban area, percent black, percent less than age 5, percent greater than age 65, and percent of employment in agriculture. The sample is restricted to those counties whose poverty rate in 1960 was above the median in North Carolina (40.2% poverty), the "High Poverty" counties. In the first two columns, the sample is restricted to counties that ever received Head Start between 1965 and 1976. In the second two columns, the sample includes counties that never received Head Start between 1965 and 1976. See the notes to Table 1 for additional sample restrictions and definitions. Significance levels indicated by: *($p < 0.10$), **($p < 0.05$), ***($p < 0.01$).

Appendix B: Implied Treatment on the Treated

These TOT estimates are based on estimated Head Start participation rates in high poverty counties of 15 to 21 percent. The lower bound is based on OEO statistics on state-level North Carolina Head Start enrollment in 1966 and the upper bound is based on author's calculations assuming the national per participant funding level is fixed across North Carolina counties. Our implied TOT effects are between half and two-thirds of the size of effects on somewhat similar measures reported in evaluations of the Perry Preschool program (11 to 12 percentage points on any arrest (or any charges) by age 40).[50,51] As in the Perry evaluation, we find larger effects on property crimes; Head Start access reduces the likelihood of a serious property conviction by 0.8 percentage points, a TOT effect of 4 to 5 percentage points in high-poverty counties (Appendix Table A5). While there is no significant effect on serious violent convictions, the point estimate (0.0046) implies a TOT of 2 to 3 percentage points. In comparison, Schweinhart et al. (2005) find a 16 percentage point reduction in violent arrests by age 40 (32 versus 48 percent) and a 22 percentage point reduction in property arrests by age 40 (36 versus 58 percent) in their evaluation of Perry preschool, four to five times the size of our effects.[52] Perry Preschool enrolled a very particular type of student: extremely disadvantaged, black children in Ypsilanti, Michigan. If we split our property crime estimates by race, we find similar effects for whites and non-whites.

If there are important spillover effects of program availability it is not reasonable to interpret these scaled estimates as TOT effects. In this case, improving the behavioral trajectories of a significant 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. Especially in high-poverty areas, a substantial fraction of children enrolled in Head Start. As these children interacted with others in their cohort, effects of the program might have spilled over to the other children in a way that would have been unlikely with the smaller treatment and control groups in experimental evaluations of small-scale programs. Particularly when it comes to criminal behavior, it is likely that these spillovers, operating through peer effects, are substantial. We therefore focus our discussion on the estimated effects of Head Start availability rather than participation.

[50] The treatment effect of Perry Preschool on any felony arrest, the definition of which overlaps substantially with Part 1 crimes, is even larger (15 percentage points), but is reported only for males (Heckman et al. 2009).

[51] Our TOT estimates are less than half of the effects estimated for the Nurse-Family Partnership by age 19 (16 percentage points on likelihood of conviction or arrest) (Olds et al. 1998, 2007). Our effect sizes are similar to recent estimates of the effects of early childhood Food Stamp access (Barr and Smith 2018). In contrast to both of these health interventions, which found strong effects on violent criminal behavior, the effects of Head Start access appear to be somewhat stronger on property crimes.

[52] Although we note that these are effects on *any* arrest and thus may not be directly comparable to convictions for a serious violent or property crime. Treatment estimates of Perry Preschool on the *number* of felony arrests indicates no significant difference in the number of serious violent crimes and a 90 % reduction in the number of felony property arrests (0.31 versus 2.91 per individual).