From Craddle to Court: The Effect of Early Childhood Education on Crime

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1 Introduction

Does adult criminal activity have roots in childhood education? This policy-relevant question has been investigated by many empirical papers, but long term estimates of the impacts of early childhood education programs have been limited. In an influential paper (Anders et al., 2023), Anders, Barr, and Smith examined the impacts of the Head Start and Smart Start programs, two large-scale early childhood support programs, on crime in adulthood in North Carolina. By using administrative crime data and Head Start and Smart Start funding data in North Carolina, the authors use both a fixed effects estimate and an event study specification as empirical identification strategies. Since the two programs rolled out in different counties at different times in North Carolina, the authors exploit within-county variation in exposure and funding levels generated by the rollout of each program to to determine long-term effects on crime.

Separating the analysis by poverty of counties, the authors find that both programs are responsible for reducing the conviction rate in high-poverty areas. Head Start availability reduces the likelihood of a serious conviction by age 35 by 1.3 percentage points while Smart Start exposure reduces the likelihood of a serious conviction by age 24 by 1.18 percentage points, both in high-poverty areas.

The evidence of crime reduction as a result of early childhood program participation leads to hypothesis of the channels through which early childhood education may affect crime. The authors interestingly states that cognitive measures alone may not be predictive of long run effects of criminal behavior and that there is evidence of a weak relationship between test scores and criminal behavior. Indeed, the role of non-cognitive factors in determining crime remains unclear(Anders et al., 2023). In this paper, I aim to explore whether early childhood education affects crime through the non-cognitive channel. In particular, what is the effect of the Head Start Program on the behavioral development of children? How does non-cognitive factors such as behavioral problems relate to early childhood education programs and crime?

I attempt to answer these questions using a regression discontinuity design that exploits the income eligibility cutoffs of the Head Start Program for different household sizes and public data from the NLSY79 survey. After conducting Instrumental Variables analysis through a fuzzy regression discontinuity design, I do not find any relationships between behavioral factors and program participation. My results are inconclusive likely because of missing values in the data and simplification of design.

2 Literature Review

Research on the effect of early educational programs on crime mostly focus on the short term due to data limitations. Two famous childhood education interventions are the Perry Preschool and the Abcedarian Project, and evaluations of these programs provide mixed results. Heckman et al. (2010) find that participation in the Perry Preschool program led to significant reductions in criminal behavior. In a following study, Heckman and Karapakula (2019) examined intergenerational effects of the Perry Preschool Program and found that children of participants had lower levels of crime participation. However, studies of the Abcedarian Project such as Campbell et al. (2002) very limited effects of the project on self-reported crime behavior. Well known studies of the Head Start program such as Currie and Thomas (1993) find positive effects of program participation on health and educational outcomes, but few studies analyze the impact of Head Start on crime. Literature on the Smart Start program, which was more recently implemented in North Carolina than Head Start, is also limited.

Anders et al. (2023) contribute to this literature by using a novel dataset and empirical method. Instead of focusing on small-scale or single-site interventions, the breadth of the administrative data used by the authors allows them to improve internal validity by estimating long-run effects of criminal behavior. Their investigation also compares two programs introduced in different time periods in the same state to account for external validity. Their findings are important in informing policymakers' decisions on investing in early childhood interventions that may have accumulated effects in later stages of life.

Although a number of studies use the regression discontinuity design to estimate impacts of the Head Start program on educational improvement Ludwig and Miller (2007), few investigate whether the Head Start program affects non-cognitive behavioral factors. In one particularly relevant study, Carneiro and Ginja (2014) uses an RD design to estimate long term impacts of Head Start participating and found that participation in the program reduces the incidence of behavioral problems, lowers depression, and reduces engagement in criminal activities and idleness for young adults. Inspired by the empirical design and use of the NLSY79 data in Carneiro and Ginja (2014), I attempt to perform a simplified regression discontinuity design without confidential state data in this paper to analyze the effect of Head Start participation on behavioral problems.

3 The Head Start Program

Head Start was initiated in 1965 and quickly rolled out throughout North Carolina in subsequent years. As a response to the War on Poverty, Head Start was aimed at improving school readiness the children of the poor. Head Start admitted children of ages 3 - 5 and provided a package of health and nutritional service along with educational services. Because the Head Start Program rolled out in different counties in different years, Anders et al. (2023) took advantage of the variation in year of Head Start implementation among counties with similar baseline characteristics and reasonably assumed variation in Head Start access to be exogenous.

4 The Smart Start Program

The Smart Start program is local to North Carolina and was created in 1993 to help parents prepare their children for school readiness. Like the Head Start program, the Smart Start program improved the quality of early education programs and provided health and food support. However, unlike Head Start, individuals did not enroll directly in the program. The program operates by funds distributed at the community level and are not specifically target to poor children. Anders et al. (2023) also exploits the variation in rollout of the Smart Start program in different counties in different years to estimate long-terms effects of Smart Start funding exposure on crime.

5 Data used in Anders et al. (2023)

5.1 Outcome Variable

The authors use data from the North Carolina Department of Public Safety, which contains the type of crime, name, date of birth, sex, and race of all individuals convicted of a crime between 1972 and 2018. The authors combined this data with information on birth rate in each county in North Carolina from the North Carolina Department of Health and Human Services to construct conviction rates at the birth cohort and county-year level. For example, the cohort conviction rate by age 35 for children born in Chapel Hill in 1961 would be the number of individuals born in Chapel Hill and convicted by age 35 divided by the total number of individuals born in Chapel Hill in 1961.

5.2 Independent Variables

For Head Start analysis, the authors restrict their sample to individuals born between 1955 and 1968 to leverage variation in Head Start availability up to 1972. Head Start availability is measured four years after birth. For Smart Start analysis, the authors restrict their sample to individuals born between 1980 and 1994 which allows them to observe criminal convictions through age 24 for all cohorts in the sample. My replication of summary statistics of conviction rates at the county-birth cohort level for Head Start and Smart Start is found in Table 1. [1]

6 Main Findings Replication

6.1 Fixed Effects Estimates

Anders et al. (2023) exploits within-county variation in exposure generated by rollout of each program. That is, after controlling for birth county and birth year fixed effects, participation or exposure to the programs is assumed to be exogenous. For both interventions, the authors use the following specification:

$C_{ct} = \beta * EC_{ct} + \gamma(X_c * t) + \alpha_c + \sigma_t + \epsilon_{ct}$ (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, α_c are birth county fixed effects, and σ_t are birth year fixed effects. $\gamma(X_c * t)$ are controls used in robustness checks for baseline birth county characteristics interacted with a time trend to account for differences in crime trends over time for counties with different characteristics. Standard errors in this specification are clustered at the county-of-birth level. The key identifying assumption is that variation in criminal conviction rate in each cohort, conditional on birth county and birth year fixed effects, is explained only by variation in program exposure and not through unobserved factors that are correlated with program exposure. The authors conduct analyses separately for high poverty counties, which are counties whose poverty rate in 1960 was above the median poverty level in North Carolina (40.2 percent poverty), and low poverty counties, whose poverty rate was below the median.

I replicated the estimates of β in (1) in Table 2. Panel A reports Head Start estimates and Panel B reports Smart Start estimates. Table 2 shows that Head Start availability reduced the likelihood of a criminal conviction by age 35 by 1.3 percentage points in high-poverty counties. The effect is not significant for all counties and for low-poverty counties. For the Smart Start program, the authors estimate the effect of average funding provided per zero-to-five-year-old over the first five years of life on the likelihood of conviction by age 24. Panel B of Table 2 demonstrates a 0.6 percentage point reduction in the likelihood of a serious criminal conviction by age 24 due to an additional 1000 exposure in funding. As with Head Start, the estimate is larger for high-poverty counties with a 1.2 percentage point reduction, while no significant effect is found for low-poverty counties. One explanation of the difference in effects conditional on poverty level may be that children in high-poverty areas have fewer options of educational services outside of Head Start, so they benefit more from early education services.

[1] The replication for tables and figures from Anders et al. (2023) are completed using data and do files in the replication package provided on the AEA website: https://www.aeaweb.org/articles?id=10.1257/pol.6.4.135

6.2 Event Study

To understand the dynamics of the Head Start and Smart Start program and to test for robustness of the exogeneity assumption of program exposure, Anders et al. (2023) conducts an event study design for each program. For Head Start analysis, the treatment is centered at the first year that the program is available and effects of leads and lags of program availability is estimated. For instance, 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. By taking a sample of counties with similar characteristics a difference-in-differences (DD) estimate can be used to compare reductions in likelihood of conviction between counties with and without Head Start rollout before and after a chosen year.

The effect of program availability relative to the year of initial rollout is estimated using the following specification, run separately for high- and low-poverty counties:

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

where $\mathbf{1}[t = T_c + \tau]$ are indicators 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 and other variables are defined as in (1). My replication of the results are shown in Figure 1. We observe that the difference in probability of conviction by age 35 between cohorts follows a flat trend before Head Start rollout for both low- and high-poverty counties, strengthening the absence of differential pre-existing trends. As time relative to initial Head Start rollout lengthens, we see significant decreases in conviction rates of the treated cohorts relative to the control cohorts in high-poverty counties, but there is almost no evidence of change in low-poverty counties. This supports the authors findings in Table 2.

Figure 2 is my replication of the event study for the Smart Start Program. The top panel plots smart Start exposure measured by the intensity of treatment a cohort experiences based on their age relative to the maximum targeted age when the funds became available in their counties. The panel shows that smart start exposure increases steadily five-to-six years following initial availability of funding. The bottom panel of Figure 2 represents event study estimates of the effect of Smart Start funding exposure on the likelihood of conviction. Once again, we observe a flat trend in cohort conviction rates before Smart Start funding arrival in a county, and cohorts in high-poverty counties experiencing a significant reduction in likelihood of conviction by age 24 over time. This means that even though Smart Start did not specifically target high-poverty counties, effect of early childhood education on crime reduction remains larger for high-poverty areas.

7 Extension: Non-cognitive factors

Anders et al. (2023) present promising evidence of effects of early childhood education on crime reduction. It is then natural to explore channels through which early childhood educational programs are linked to criminal behavior. A popular explanation for why early childhood programs affect crime is that these programs improve school outcomes and return to work in later stages of life, thereby increasing the opportunity cost of committing crime. Indeed, many studies(for example Fitzpatrick (2008)) that evaluate modern childhood programs find correlations between program participation and test score improvement. However, Anders et al. (2023) states that improvements in test scores may not translate into crime reduction in the absence of non-cognitive skills. Early childhood education may also influence behavioral or temperamental factors of a child, which then leads to lower propensity for crime.

In the following sections, I attempt to analyze the impact of the Head Start program on behavioral problems, which is a part of non-cognitive factors, and their combined relationships to crime. I follow the general strategy of Carneiro and Ginja (2014) in using a regression discontinuity design that exploits the eligibility guidelines from the Early Childhood Knowledge and Learning Center[2]. In each year, children from ages 3-5 are eligible to participate in Head Start if their family income falls below a certain federal income eligibility threshold that is dependent on family size. Children whose household incomes fall below these thresholds are more likely to participate in Head Start. I can then use a fuzzy RD to compare behavioral outcomes for children who are just below and just above eligibility thresholds.

7.1 Data and Data Cleaning

My main data source is the NLSY79 and the NLSY79 Child and Young Adult data. Carneiro and Ginja (2014) uses the NLSY79 Child and Young Adult data to construct multiple discontinuities in eligibility using total family income as well as state-dependent income tests. Since state of birth of participants in NLSY79 are restricted from access due to confidentiality, I only use net family income to determine eligibility and construct a RD design with many simplifications.

The NLSY79 Bureau of Labor Statistics (2019) is a longitudinal survey of nationally representative sample of 8,984 men and women born during the years 1980 through 1984 and living in the United States at the time of the initial survey in 1997. Participants were ages 12 to 16 as of December 31, 1996. Interviews were conducted annually from 1997 to 2011 and biennially since then. The NLSY79 Child and Young Adult data Bureau of Labor Statistics et al. (2019) follows the biological children of the women in the NLSY79. The survey runs from 1986 to 2020 and contains a wealth of relevant information about education, crime, and behavior.

The NLSY79 Child and Young Adult Bureau of Labor Statistics et al. (2019) contains a Behavior Problems Index (BPI)that computes a score for children aged 4-13 based on questions concerning behavioral problems and use of mental health services. I decide to use the BPI raw

[2]Eligibility guidelines can be found here:https://eclkc.ohs.acf.hhs.gov/ersea/articulo/poverty-guidelines-determining-eligibility-participation-head-start-programs

score as my outcome variable because it is a comprehensive aggregate measure of non-cognitive factors that may be related to crime. The survey also contains self reported data on whether an individual has convicted a crime other than traffic and an indicator for whether individual has ever enrolled in Head Start. To analyze necessary data, I first downloaded race, gender, date of birth, head start participation, BPI raw score, conviction, and household size data from NLSY79 Child and Young Adult from 1986 to 2016, the year range for which all these variables were available. Then, I downloaded data on net family income in previous calendar year from NLSY79. I used the mother identification ID in the child data to merge net family income data from the mother data. Next, I merged in CPI data from 1986 - 2016 from the Federal Reserve Bank of St. Louis CPI database to convert net family income to 2000 dollars, as the authors did in Carneiro and Ginja (2014). To determine eligibility of each individual, I merged household size dependent income eligibility thresholds data for Head Start from U.S. Federal Poverty Guidelines public data.[3] Finally, I was able to compute the income margin for each individual at age 4, which is the difference between the adjusted net family income and the eligibility threshold for different household sizes. Using these variables, I attempt to compare BPI scores for individuals who were just below and just above the eligibility thresholds for Head Start when they were four years old.

7.2 RD Model

Not all children who are eligible for Head Start actually participate in Head Start. Carneiro and Ginja (2014) states that take-up rates among those eligible are far below 100 percent. Factors such as shortage of funding and other barriers to enrollment means that priority is given to the neediest among the poor, which introduces the problem of incompliance. Since being below the family income eligibily threshold at age 4 only increases the likelihood of participating in Head Start, this is a fuzzy regression discontinuity design. I use family income eligibility as an instrument for Head Start participation to analyze the impact of Head Start participation on behavioral problems index. Similar to that in Carneiro and Ginja (2014), the regression model specifications are:

$Y_i = \alpha + \beta \times HS_i + g(Z_i, X_i) + \epsilon_i$ (3)

$HS_i = \mathbf{1}[\eta + \tau \times E_i + h(Z_i, X_i) + v_i > 0]$ (4)

where Y_i is raw BPI score, HS_i is an indicator of Head Start participation, E_i is an indicator of eligibility for Head Start, X_i is a set of determinants of eligibility for each child except for net family income (year, family size, family structure (presence of father in household), measured at age four), and Z_i is adjusted net family income (at age four). Eligibility is determined by whether adjusted net family income at age four is below the threshold income, or

$E_i = \mathbf{1}[Z_i \leq \bar{Z}(X_i)]$ (5)

where $\bar{Z}(X_i)$ is the income eligibility threshold that depends on family size, family structure,

[3]https://aspe.hhs.gov/topics/poverty-economic-mobility/poverty-guidelines

and year.

For β to estimate the causal effect of Head Start participation, several assumptions must hold. First, E_i must be correlated with HS_i, which is the relevance assumption. This can be tested using a first stage regression. Second, families are not able to manipulate household income around the eligibility cutoff. This can be explored by plotting the distribution of income margins. Third, eligibility to Head Start should not be correlated with eligibility to other programs which also affect child outcomes. Given more time, I would test this exogeneity assumption by collecting data on other early education programs.

8 RD Estimates

Overall, running IV and RDD analyses on my merged dataset yielded inconsistent and insignificant results. Table 3 shows the first stage estimates of the effect of eligibility on Head Start participation. Being eligible for head start increased Head Start Participation by 0.2 standard deviations. It seems that the instrument has a positive relationship to the treatment variable. However, an F-test would be better in determining whether the instrument is weak.

When I ran the IV regression of the impact of Head Start participation, represented in Table 4, I obtained very noisy estimates with large standard errors. This result is inconclusive about the relationship between BPI and Head Start participation due to large variations in the dataset.

Figure 3 shows the distribution of family income margin. We observe a long right tail, indicating that a small number of individuals have very high income in the data, and I exclude these individuals in my regression discontinuity plots. Even more problematic, however, is the high clustering around the cutoff point, which may indicate that families are able to manipulate their income to be just below the cutoff. If this is the case, then my specification would be biased.

Regression discontinuity analysis around the eligibility cutoff also yielded disappointing results. Figure 4 and Figure 5 plot BPI raw scores and crime respectively on income margin. After trying many different functional forms and bandwidths, no discontinuous jump seems to be present at the eligibility cutoff point. Coefficient estimates using optimal bandwidth are also insignificant.

In Table 5, I ran OLS regressions of whether individual was convicted of a crime on Head Start participation and on interactions of Head Start participation and BPI raw score. Once again, I do not find significant correlations between the three variables.

I attribute the randomness and inconclusive results to several factors. First, there were many missing values for Head Start participation when I cleaned the data, which could lead to a treatment sample that is too small. Secondly, the limitations of the NLSY as a self-reported data may lead to under-reporting of criminal conviction and behavioral problems. Furthermore, my model could be an oversimplification that doesn't consider other covariates that affect behavior, crime, and participation in early childhood education programs or it does not satisfy the identifying assumptions of an IV approach. Finally, there may be errors during data cleaning that altered or deleted relevant observations.

9 Conclusion

The relationship between early childhood education and adult criminality is vital amidst the widespread efforts to reduce crime. Anders et al. (2023) provides insightful empirical analysis on the long term impacts of programs such as the Head Start and Smart Start probability of being convicted in adulthood. Using administrative crime and funding data in North Carolina, they find that early education programs are effective in reducing crime in high-poverty regions. Their findings indicate that policies that target early education programs in high poverty areas may yield large social benefits that extend many years after the program.

In my extension, I attempt to explore non-cognitive channels through which early education affects criminal behavior. Exploiting the income eligibility cutoffs of Head Start for different family sizes, I implement a fuzzy regression discontinuity approach with Head Start eligibility as an instrument for program participation. I do not find any significant relationships between Head Start participation, behavioral development, crime, and the interactions between these variables. The role of non-cognitive factors in crime reduction remains ambiguous and relevant for future research.

10 Tables and Figures

Panel A: Head Start Sample	**All**	**High Poverty**	**Low Poverty**
First 4 year olds exposed to HS:	1962.3	1962.3	1962.3
HS Funding (2015$ per 4 year old)	893.8	2061.1	605.7
Criminal Conviction	0.0469	0.0462	0.0471
Observations	882	308	574
Individuals Represented	1487225	444848	1042377
Panel B: Smart Start Sample	**All**	**High Poverty**	**Low Poverty**
First Calendar Year of Smart Start	1995.5	1996.7	1995.0
SS Penetration (2015$)	818.9	750.0	838.3
Criminal Conviction	0.0516	0.0512	0.0517
Observations	1500	750	750
Individuals Represented	1407042	666073	740969

Table 1: Summary Statistics

Poverty	(1) All	(2) High	(3) Low
Panel A: Head Start			
Head Start Availability	-0.0017	-0.0128**	0.0026
	(0.0031)	(0.0058)	(0.0033)
Observations	882	308	574
Mean	0.0469	0.0462	0.0471
Poverty	All	High	Low
Panel B: Smart Start			
SS ($1000s)	-0.0064**	-0.0118**	-0.0030
	(0.0029)	(0.0051)	(0.0035)
Observations	1500	750	750
Mean	0.0516	0.0512	0.0517

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 2: Effect of Early Childhood Education on Criminal Conviction

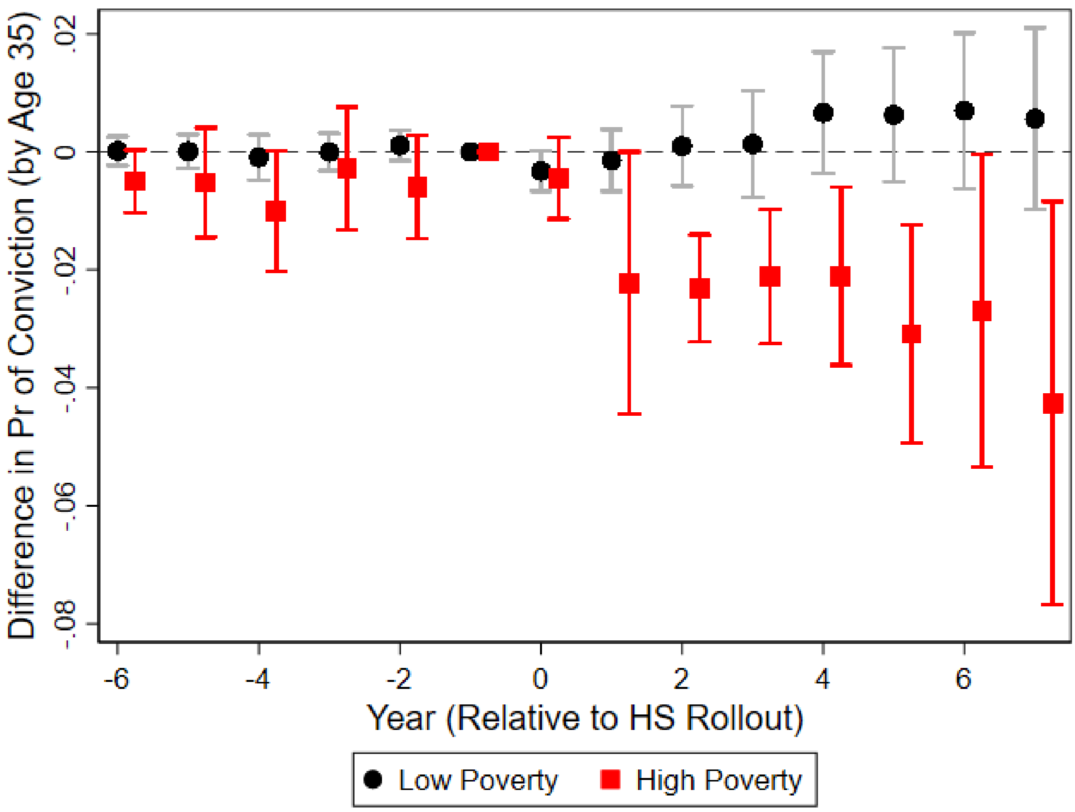

Figure 1: Event Study of Head Start's Impact on Criminal Conviction

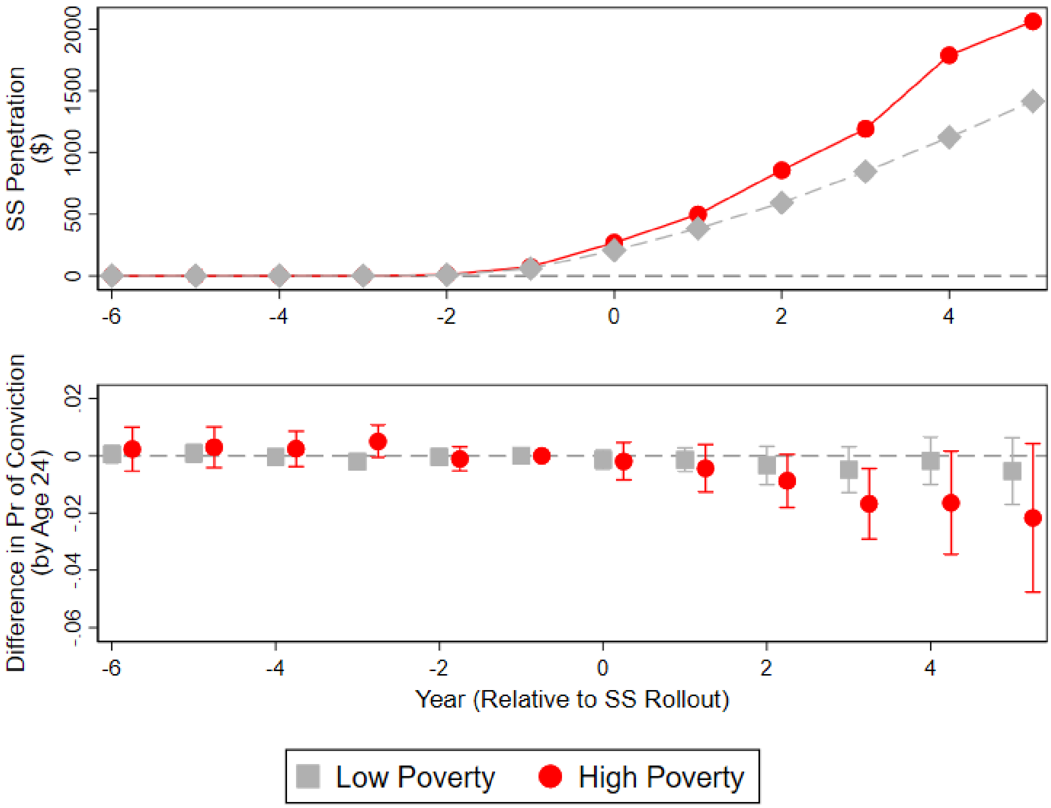

Figure 2: Event Study of Smart Start's Impact on Criminal Conviction

Table 3: First stage estimates of Head Start Participation on eligibility

	(1)	(2)
elig	0.203***	0.196***
	(0.0346)	(0.0414)
race		-0.0414***
		(0.0105)
sex		0.0403
		(0.0293)
dad_in_hh		-0.113***
		(0.0305)
_cons	0.0832***	0.114
	(0.0176)	(0.0633)
N	532	490
R^2	0.045	0.067
adj. R^2	0.043	0.059

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: IV Estimates of impact of head start paricipation on BPI raw score

	(1)	(2)
hs	64.89*	53.96
	(37.1938)	(40.4220)
sex		-2.032
		(5.4122)
race		8.043
		(5.1882)
dad_in_hh		37.16***
		(9.9908)
_cons	73.41***	56.82***
	(3.3366)	(17.0383)
N	532	490
R^2	.	.
adj. R^2	.	.

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

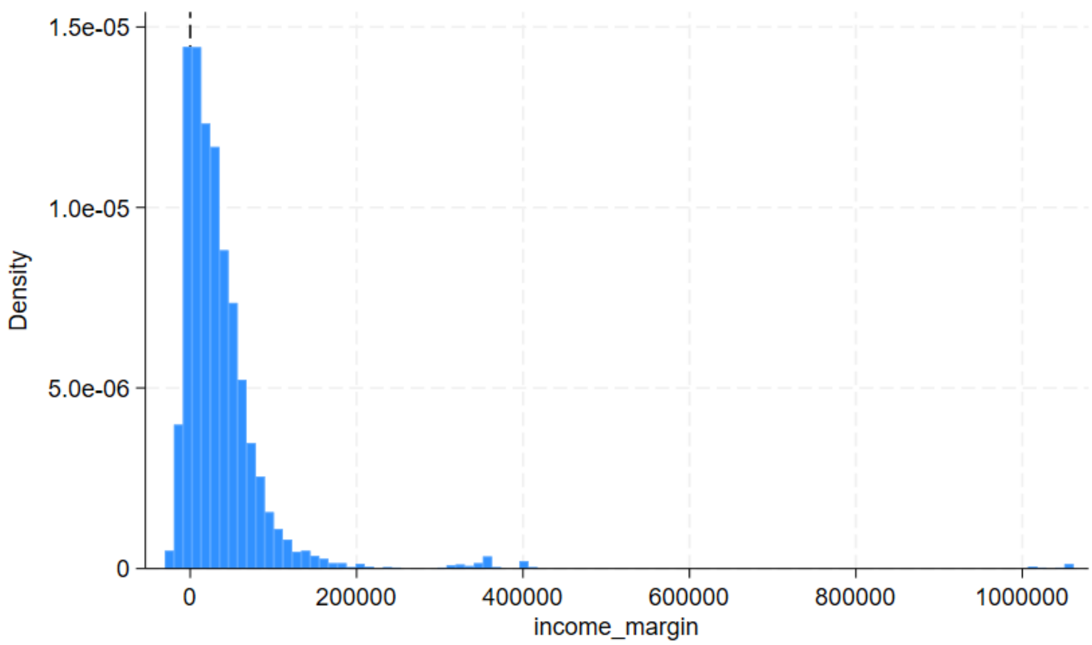

Figure 3: Distribution of family income margin

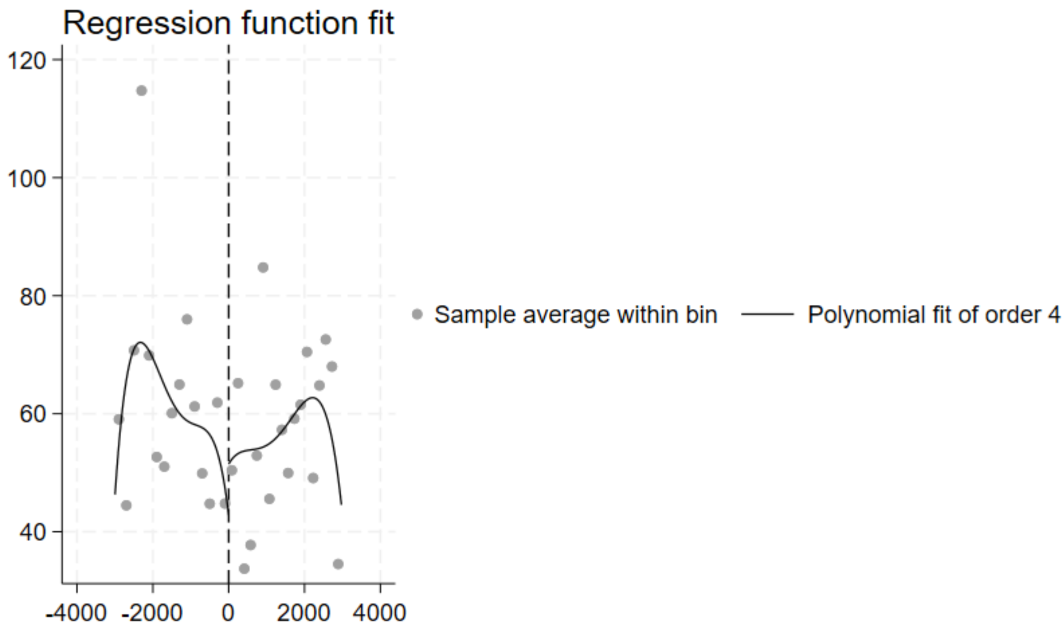

Figure 4: RDplot of BPI score on marginal income optimal bandwidth

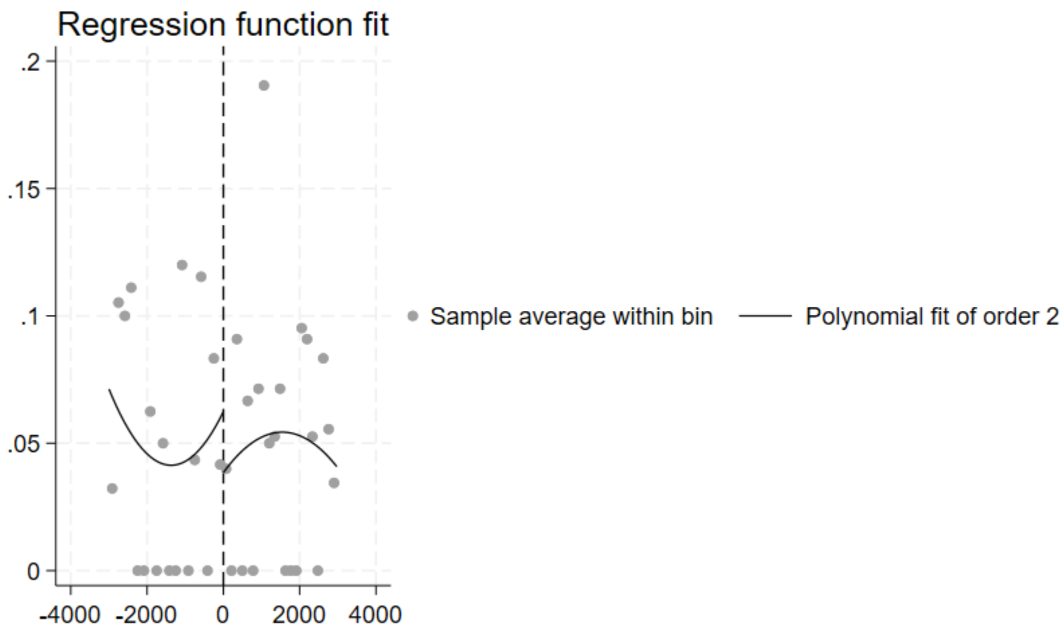

Figure 5: RDplot of crime on marginal income optimal bandwidth

Table 5: Estimates of impact of head start paricipation on crime

	(1) Convicted	(2) Convicted
hs	-0.0218	-0.0231
	(0.0229)	(0.0363)
sex	-0.00747	-0.00749
	(0.0135)	(0.0135)
race	0.0206**	0.0206**
	(0.0095)	(0.0095)
dad_in_hh	-0.00333	-0.00333
	(0.1638)	(0.1639)
hs_bpi		0.0000172
		(0.0004)
_cons	-0.00983	-0.00982
	(0.0321)	(0.0321)
N	585	585
R^2	0.012	0.012
adj. R^2	0.005	0.003

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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