I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 17190

The Impact of Children's Access to Public Health Insurance on Their Cognitive Development and Behavior

Marie Hull
Ji Yan

JULY 2024

DISCUSSION PAPER SERIES

IZA DP No. 17190

The Impact of Children's Access to Public Health Insurance on Their Cognitive Development and Behavior

Marie Hull
University of North Carolina at Greensboro and IZA

Ji Yan
Appalachian State University

JULY 2024

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Impact of Children's Access to Public Health Insurance on Their Cognitive Development and Behavior*

While a large literature examines the immediate and long-run effects of public health insurance, much less is known about the impacts of total program exposure on child developmental outcomes. This paper uses an instrumental variable strategy to estimate the effect of cumulative eligibility gain on cognitive and behavioral outcomes measured at three points during childhood. Our analysis leverages substantial variation in cumulative eligibility due to the dramatic public insurance expansions between the 1980s and 2000s. We find that increased eligibility improves child cognitive skills and present suggestive evidence on better behavioral outcomes. There are notable heterogeneous effects across the subgroups of interest. Both prenatal eligibility and childhood eligibility are important for driving gains in the test scores at older ages. Improved child health is found to be a mediator of the impact of increased eligibility.

JEL Classification: H51, I13, I38, J13, J24

Keywords: Medicaid, state children's health insurance program, health insurance, human capital, cognitive development, non-cognitive skills

Corresponding author:
Marie Hull
Department of Economics
University of North Carolina at Greensboro
Greensboro, NC 27402
USA

E-mail: mchull2@uncg.edu

* We are grateful for comments from Martin Andersen, Susan Averett, Adam Biener, Sarah Cohodes, Tom DeLeire, Chloe East, Dora Gicheva, Donna Ginther, Maithreyi Gopalan, Matt Larsen, Ben Schwab, David Slusky, seminar/workshop participants at Furman University, Lafayette College, University of North Carolina at Greensboro, Wake Forest University, and participants at presentations at American Society of Health Economists, Association for Education and Finance Policy, Association for Public Policy Analysis and Management, Carolina Region Empirical Economics Day (CREED), and Kansas Health Economics Conference. This research was conducted with restricted access to Bureau of Labor Economics (BLS) data. The views expressed here do not necessarily reflect the views of the BLS.

1. Introduction

It has been well established that childhood conditions can shape the accumulation of human capital and health capital, as well as impact later-life outcomes (Almond et al., 2018). A particularly important childhood intervention is improving access to health services through expansions of public health insurance programs. Created by the Social Security Amendments of 1965, Medicaid provides public health insurance to eligible low-income individuals and families. After dramatically expanding eligibility for pregnant women and children between the 1980s and 2000s, Medicaid and the related State Children's Health Insurance Program (SCHIP) covered 39.8 million children under age 19, or about 50 percent of children in the United States, in 2010 when the Affordable Care Act (ACA) was passed (MACPAC, 2013). Total spending by Medicaid and SCHIP on children, the largest group of beneficiaries, exceeded $80 billion in 2010 (MACPAC 2012, 2013, 2014). Given the magnitude of expenditures and the sizable number of child recipients, it is crucial to assess how public health insurance expansions influence children's developmental processes.

A large literature documents notable short-run impacts of Medicaid and SCHIP on health care utilization, health status, crowding out, and other outcomes (Gruber and Simon, 2008; Finkelstein et al., 2012; Currie and Duque, 2019). Recently, a growing body of research considers the long-term effects of cumulative program exposure during childhood. Several studies exploit eligibility variation from the introduction of Medicaid or a birthdate discontinuity to find salient later-life benefits (Boudreaux et al., 2016; Wherry et al., 2018; Goodman-Bacon, 2021; Noghanibehambari, 2022). Newer research which revisits the expansions in the 1980s and 1990s shows positive long-run effects (Cohodes et al., 2016; Thompson, 2017; Miller and Wherry 2019; Brown et al., 2020; East et al., 2022; Hendrix and Stock, 2022).

In contrast, less attention has been paid to the medium-term effects of children's access to public health insurance. A few studies examine health outcomes of children and adolescents but report mixed findings (Currie et al., 2008; De la Mata, 2012; Wherry and Meyer, 2016). Even less is known about the relationship between public insurance expansions and childhood human capital development. Levine and Schanzenbach (2009) focus on the effect of eligibility changes at birth and report modest gains in reading scores in the 4th and 8th grade. They only exploit eligibility changes due to the income-based Medicaid expansions after 1987, and so their analysis does not leverage the earlier expansions to relatively poorer children, for whom the effect of public insurance might be larger. Moreover, measurement error in eligibility due to inaccuracies in their eligibility calculator could bias the results. Qureshi and Gangopadhyaya (2021) find that students who gain eligibility are less likely to be below grade-for-age when they are 12-14 years old. Because the test scores in their data are grade-specific, the authors are unable to disentangle the effects on test scores from changes in grade-for-age. In addition, neither study examines development of non-cognitive skills.

The scant literature on medium-run outcomes is striking since such analyses could lend insights into skill development more generally. Moreover, the cognitive and noncognitive outcomes considered in this paper are interesting in their own right. A growing literature demonstrates the importance of noncognitive or socioemotional skills for a variety of adult outcomes, and thus it would be useful to know if public health insurance fosters them (Heckman et al., 2006; Deming, 2017; Edin et al., 2022). From a policy evaluation standpoint, the impact of multi-year exposure to eligibility on developmental outcomes may prove to be a significant component of program benefits.

Regarding the mechanisms through which expanding eligibility affects child development, first, public insurance expansions could aid early diagnosis and treatment through facilitating access to care (Chorniy et al., 2018). As a result, Medicaid and SCHIP help remove health barriers to learning and limit their persistence into adolescence. Furthermore, many covered pediatric health services protect children at young ages from future health problems that undermine human capital acquisition (Wherry and Meyer, 2016). Second, although crowding out weakens the health mechanism, switching to public insurance would free up resources that were previously spent on private insurance. These additional resources could be directed toward other home investments in children (Boudreaux et al., 2016). Moreover, additional outcomes examined in the short-term literature, such as household financial security and maternal stress, are also potential channels (Gross and Notowidigdo, 2011; Guldi and Hamersma, 2023).

This paper contributes to the small but important literature on the medium-run effects, leveraging substantial variation in cumulative eligibility which comes from public insurance expansions between the 1980s and 2000s. We focus on multi-year eligibility rather than point-in-time eligibility because: one, human capital production is affected by the cumulative influence of investments over time; two, there could be cross-reinforcement of skill capacity and health at different ages (Cunha and Heckman, 2007; Cunha et al., 2010). The baseline analysis estimates the impact of increased eligibility on both cognitive and non-cognitive outcomes measured at three age ranges, using matched mother-child data from the National Longitudinal Survey of Youth 1979 (NLSY79). To achieve identification, we instrument multi-year total actual eligibility with total simulated eligibility, using a strategy pioneered by Currie and Gruber (1996a,b). We also provide rich evidence on the heterogeneous effects by child gender, race/ethnicity, and the age at which a child is eligible. Our results suggest increased eligibility improves child cognitive skills and present suggestive evidence that it leads to better behavioral outcomes. The magnitude of our estimates compares favorably with prior studies on other policies which influence child development.

2. The Medicaid Expansions and Implementation of SCHIP

Jointly funded by the federal and state governments, Medicaid is administered by states. States follow certain federal mandates but have flexibility to determine eligibility and services. Until the early 1980s, Medicaid eligibility for pregnant women and children was tied to participation in the program Aid to Families with Dependent Children (AFDC). This linkage generally limited Medicaid eligibility to single-parent families. In addition, the AFDC income eligibility thresholds which varied by state were typically well below the federal poverty line (FPL). There were other optional state programs under which poor children and pregnant women could qualify for Medicaid. They encompass the AFDC-Unemployed Parent (AFDC-UP) program, state Ribicoff program, Medically Needy program, and other state options for pregnant women such as provision of Medicaid coverage for first-time pregnant women.[4] Beginning in 1984, the linkage between AFDC coverage and Medicaid eligibility was gradually weakened. This occurred both at the state level (for instance, through expansion of the Ribicoff option) and at the federal level through the Deficit Reduction Act of 1984 (Gruber, 2003). More dramatic Medicaid expansions were seen from 1987 to the mid-1990s. These expansions substantially

[4] The AFDC-UP program allowed two-parent families where the primary earner was unemployed to qualify for AFDC and Medicaid. Under the Ribicoff program, states can cover children with family incomes below the AFDC income standards but who did not qualify due to family structure. The Medically Needy program allowed children with family incomes above the AFDC income standards to qualify for Medicaid, if their families had very large medical expenditures. Regarding the other state options for pregnant women, please refer to Miller and Wherry (2019).

increased the income eligibility thresholds for children and pregnant women, and meanwhile provided higher eligibility levels to all family structures.

By the mid-1990s, all children under age 19 born after September 20, 1983, with family incomes below 100 percent of FPL were mandatorily eligible. In addition, states were required to cover children younger than age 6 and pregnant women up to 133 percent of FPL and were allowed to provide more generous coverage up to 185 percent of poverty. In 1997, SCHIP was passed into law as part of the Balanced Budget Act to augment Medicaid for children. The introduction of SCHIP was one of the largest expansions of public health insurance to date. This program provides matching funds to states to expand their health insurance coverage beyond Medicaid levels, up to 200 percent of FPL or higher. In implementing SCHIP, all states are also given flexibility to choose to either expand Medicaid or create a separate child health program which mimics private health insurance (Gruber and Simon, 2008).[5]

These expansions in public insurance programs were phased in with different generosity levels for children of different ages within states and at different times across states. Therefore, there is a great deal of cross-state and cross-cohort variation in cumulative program exposure of children and adolescents, which our study will exploit. To illustrate the point, we use data from the March Current Population Survey (CPS) and the eligibility rules to calculate the state-level total years of simulated eligibility from birth to age 5 for the 5-year-old children who were born in 1981 and 1996.[6] Simulated eligibility isolates the variation in eligibility due only to policy (more discussion below). Then, the difference in the years of simulated eligibility between the two birth cohorts is worked out and illustrated in Figure 1. While cumulative early childhood eligibility goes up in all states as we compare the two birth cohorts, there is considerable heterogeneity in the changes across states. The same pattern emerges in Figure 1, when we plot

[5] The major federal legislations for the eligibility expansions in the 1980s and 1990s discussed above are the Deficit Reduction Act of 1984, the Consolidated Omnibus Budget Reconciliation Act of 1985, the Omnibus Budget Reconciliation Acts (OBRA) of several years (1986, 1987, 1989, 1990), the Medicare Catastrophic Coverage Act of 1988, the Personal Responsibility and Work Opportunity Act of 1996, and the Balanced Budget Act of 1997. The legislations are described in Currie and Gruber (1996a), Gruber (2003), and Miller and Wherry (2019).

[6] We consider the children aged 5 from the 1981 and 1996 birth cohorts to make our cross-cohort discussion more comparable with section 4, which will present the overall cohort trend of similar children who were born between 1981 and 1996. A similar point can be made on why we use the 1976 and 1996 cohorts in Figure 1 for the 11- or 14-year-old children.

the state-level cross-cohort changes in cumulative eligibility for the 11- and 14-year-old children born in 1976 and 1996.

[Insert Figure 1 Here]

3. Literature Review

The short-run effects of Medicaid and SCHIP have been examined extensively. For instance, previous studies provide robust evidence of increased access to and utilization of health care immediately following changes in eligibility or insurance coverage (Currie and Gruber, 1996b; Card and Shore-Sheppard, 2004; Dafny and Gruber, 2005; De La Mata, 2012; Finkelstein et al., 2012). Findings of this literature are less consistent for health status (Howell and Kenney, 2012). However, there is strong evidence of a decline in infant and child mortality associated with both the expansions since the early 1980s and Medicaid's original introduction (Currie and Gruber, 1996a,b; Howell et al., 2010; Goodman-Bacon, 2018).[7]

An emerging literature explores the long-term impacts of children's access to public health insurance. Recent work documents improvements in health, labor supply, earnings, and birth outcomes in adulthood, using variation in childhood cumulative program exposure from the rollout of Medicaid (Boudreaux et al., 2016; Goodman-Bacon, 2021; Noghanibehambari, 2022). Wherry et al. (2018) exploit a birthdate discontinuity in the 1980s expansions and find additional childhood eligibility lowers adult hospitalizations. A series of studies follow cohorts who experienced eligibility changes in utero or childhood due to the expansions in 1980s and 1990s. As adults, the affected cohorts have better educational attainment, health, and labor market performance; pay more in tax; rely less on government transfers; commit fewer crimes; and have healthier offspring (Cohodes et al., 2016; Thompson, 2017; Miller and Wherry, 2019; Brown et al., 2020; East et al., 2022; Hendrix and Stock, 2022).

Nevertheless, the medium-term effects have received much less attention. Currie et al. (2008) find that greater point-in-time eligibility at ages 2, 3, and 4 reduces poor self-rated health for children at ages 9-17. Wherry and Meyer (2016) link increased childhood eligibility to a later

[7] This literature also examines a range of other outcomes, including substitution of public insurance for private insurance or crowding out (Cutler and Gruber, 1996; Gruber and Simon, 2008), household financial security (Gross and Notowidigdo, 2011; Finkelstein et al., 2012), maternal labor supply (Strumpf, 2011), asset holdings and consumption (Gruber and Yelowitz, 1999; Leininger et al., 2010), contraceptive use and fertility (Kearney and Levine, 2009; Zavodny and Bitler, 2010), and maternal stress (Guldi and Hamersma, 2023).

decline in teenage mortality. However, De la Mata (2012) uses a regression discontinuity approach to find no significant improvement in child health status and obesity five years after a given year of eligibility. But the imperfect control assumption for the assignment variable (income) may not hold around the eligibility cutoffs (Buchmueller et al., 2015).

As far as we know, only two medium-run evaluations exist regarding the effect of child insurance expansions on development of cognitive skills, and none directly investigates noncognitive skills.[8] Levine and Schanzenbach (2009) focus on the effect of eligibility changes at birth and estimate a triple-difference model, using data from the National Assessment of Educational Progress. This study does not exploit variation from eligibility changes before 1987, when expanding public insurance may be more impactful. Mismeasured eligibility is also concerning, because not all eligibility pathways are considered and their eligibility calculator does not fully account for significant differences in the income-counting methodologies across states. In addition, they examine a limited number of cohorts because data of test scores exist for sporadic years. Qureshi and Gangopadhyaya (2021) apply a design of difference-in-discontinuities to school children in North Carolina born around September 30, 1983. They find that cumulative eligibility gain near the discontinuity reduces the risk of being below grade-for-age. There is no evidence of an effect on documented disabilities or absences from school. Although the study reports a reduction in reading scores and no effect in math scores for students with additional eligibility, those estimates are contaminated by changes in grade-for-age.

This gap in the literature is notable, given that both cognitive and noncognitive abilities strongly influence schooling, labor market performance, and risky behaviors in adulthood (Heckman et al., 2006; Edin et al., 2022). Medium-term analyses on child development and behavior, such as our study, will help researchers understand how developmental trajectories are shaped by public insurance expansions. Moreover, this study is closely related to Thompson (2017), which uses the NLSY79 data to examine health outcomes in early adulthood. Eligibility

[8] We do note a handful of studies on parental eligibility for public health insurance and child development. One analysis on pregnancy-related Medicaid expansion in the late 1980s demonstrates a positive impact on child's Denver Developmental Scale Score at age 3 (Guldi and Hamersma, 2023). Moreover, Bullinger et al. (2023) examine the short-run impact of parental Medicaid eligibility under the ACA Medicaid expansions on child development. They find small gains in reading scores but no effect on math scores or socioemotional skill development. However, exploiting similar variation in parental eligibility, Gangopadhyaya and Schiman (2023) find significant short-term spillover effects on math scores but not English-language arts (ELA) achievement scores among older children.

calculation in Thompson (2017) is subject to similar limitations as Levine and Schanzenbach (2009). As a result, similar errors in actual and simulated eligibility would be introduced, which likely biases his instrumental variable (IV) results. The present work largely surmounts this problem using a comprehensive list of Medicaid and SCHIP rules to determine eligiblity. In addition, to improve performance of the IV, Thompson (2017) constructs a subsample from the Current Population Survey that mirrors the NLSY sample and then uses this subsample to impute simulated eligibility. Our study does not impose this strong sample restriction, as the rich set of eligibility policies we consider already results in a generally strong first-stage relationship.

4. Data

Our primary data source is the NLSY79 and the corresponding survey of their children (NLSCYA), which are sponsored by the U.S. Bureau of Labor Statistics. The NLSY79 follows a nationally representative sample of over 12,000 American youth born between 1957 and 1964. They were interviewed annually from 1979 to 1994 and biennially from 1996 to the present. In 1986 a biennial survey of the biological children of women in the NLSY79 was launched. Importantly for our study, the NLSCYA survey includes a battery of age-appropriate tests to assess cognitive and socio-emotional development for children.

In addition, we employ data from the CPS March Supplement for each year to construct measures of simulated eligibility for public health insurance. These measures will be used as instrumental variables in the analysis below. The CPS is a monthly survey of about 60,000 U.S. households administered by the Census Bureau to yield estimates of employment, unemployment, and other characteristics of the general labor force. The March CPS provides detailed information on income, work experience, and household demographic characteristics.

4.1. Public Health Insurance Eligibility

We estimate actual eligibility for pregnant women and children under age 19 for the years 1979-2011, utilizing the eligibility calculator developed by Miller and Wherry (2019). The calculator applies detailed federal and state eligibility rules to individual information, such as child's age and birthday, state of residence, and family structure. There were six different pathways to Medicaid prior to welfare reform (1979-1996). The first was traditional AFDC eligibility, where a child's family must satisfy a net income test and a gross income test.[9] The second pathway provided coverage to pregnant women and children with family incomes below specified percentages of FPL.[10] The other four pathways were the AFDC-UP program, state Ribicoff rules, the Medically Needy program, and additional state options for pregnant women. From 1997 onward, eligibility was calculated under the post-welfare reform rules for three eligibility pathways: (1) Medicaid section 1931 eligibility, (2) continuing state Medicaid expansions, (3) separate state programs under SCHIP.[11]

After determining point-in-time eligibility at different ages for NLSCYA respondents by the calculator, we take averages of point-in-time eligibility over various portions of childhood. Then, we convert each average into total actual eligibility by a certain age for the main analysis. For instance, to construct total actual eligibility for a 5-year-old child, we follow Thompson (2017) to calculate the mean of imputed point-in-time eligibility from ages 0 through 5 and multiply this average by 6. We construct other total eligibility measures in a similar manner. One exercise below uses eligibility at different age ranges (such as eligibility at ages 5-9, 10-14) and pregnancy-related eligibility. As in Miller and Wherry (2019), we refer to the measure of pregnancy-related eligibility as prenatal eligibility, which captures total exposure to eligibility in utero and during the first year of life.

In constructing simulated eligiblity, we aim to isolate the variation in eligibility due only to rule changes. If we used state-specific fixed samples for simulation, the corresponding variation in simulated eligibility would still abstract from changes in state sociodemographic characteristics. But it would be affected by state-level fixed differences in demographics and economic conditions, thereby failing to reflect only the true policy changes in program

[9] To pass the net income test, the monthly total family income (excluding income from public assistance or welfare, the same below) less the total disregard must be no more than the state need standard (Currie and Gruber, 1996a). We use the work-related expense deduction, earnings disregard, and childcare disregard to calculate the total disregard. The gross income test specifies that the monthly total family income must not exceed a given multiple of the state need standard.

[10] This pathway is related to the federal and state Medicaid eligibility expansions beginning in 1984, such as OBRA 1989. We consider a child eligible for Medicaid if the monthly total income minus work-related expense deduction was no more than either the federal or the state eligibility threshold.

[11] To determine SCHIP eligibility for a child, we compare the monthly countable family income to the assigned eligibility threshold (given the date of eligibility determination, child's age, family size, and state). Moreover, the monthly countable family income comes from subtracting the state- and SCHIP-specific work expense deduction per worker from the monthly total family income.

generosity across states.[12] Applying a national fixed sample for simulation overcomes this problem.

We calculate simulated eligibility during childhood for the main analysis as follows: First, a national random sample of 1,000 children at each age is drawn from the March 1991 CPS.[13] Assigning this sample to each state, we adjust income in each year (1979-2011) for inflation using the Consumer Price Index for All Urban Consumers. We apply the eligibility calculator to the fixed CPS sample and then collapse eligiblity of all the children into unique state-year-age cells, which give us within-cell fraction of children of each age who are eligible for public health insurance in each year and state. Finally, we link the cells of simulated eligibility to NLSCYA respondents, take averages of simulated eligibility over various portions of childhood, and convert them into measures of total simulated childhood eligibility.

For robustness checks below, we also construct two other measures of total simulated eligibility, using national random samples from the March 1981 CPS and March 2001 CPS. In addition, to estimate simulated eligibility in utero, we use a national sample of 3,000 women aged 15-44 from the March 1991 CPS, assign them to each state and year, and apply the calculator. Then, we use simulated eligibility in utero and during the first year of life to construct simulated prenatal eligibility.

4.2. Measures of Child Development and Health

Our measures of children's cognitive development come from the Peabody Picture Vocabulary Test, revised edition (PPVT-R) and the Peabody Individual Achievement Test (PIAT)

---

[12] Consider a simplified example with two states: A and B. Both states only compare countable family income to 100 percent of FPL to determine eligibility and cover children younger than age six from the corresponding poor families. Now, the two states raise the threshold to 133 percent of poverty for this child age group. Before the policy change, suppose a larger fraction of the families in State A with children younger than age six had countable income between 100 and 133 percent of FPL, compared with State B. If we use state fixed samples for simulation, the isolated effect will indicate State A implemented a more generous rule than State B, as seen in a larger increase in simulated eligibility for children younger than six. But in fact, changes in program generosity are the same in the two states. Moreover, it is problematic to use state-year-age fixed effects to address the bias in the isolated eligiblity changes, since these interactions remove much legislative variation for eligibility in the data.

[13] Since the CPS asks about all income received in the prior calendar year, we effectively use income information from 1990.

for Mathematics, Reading Recognition, and Reading Comprehension.[14] For non-cognitive development, we use the Behavioral Problem Index (BPI) and its associated subscores. These assessment instruments are widely used and have high validity and reliability. The ages at which children were given different assessments changed across survey waves from 1986 to 2012. As such, the availability of particular assessments across survey rounds informs our choice of three age ranges: 4-5, 10-11, and 13-14.

The BPI is derived from 28 questions that ask mothers about their children's behavior. The NLSCYA survey provides one overall BPI score and six subscores based on the dichotomized recoding of the original three answer responses.[15] To adjust for multiple inference, we construct a composite index with mean 0 and standard deviation 1 from the six subscores. This index is robust to overtesting, since it represents a single test and the risk of false rejection will not rise when additional subscores are included (Anderson, 2008).[16] Moreover, there are three additional scales based on trichotomous (TRI) recodes of the original responses: one overall scale and the others two for internalizing and externalizing behaviors. We refer to them as "TRI-based" scores. Increases to the overall BPI scores, BPI subscores, or BPI composite index indicate more behavioral problems. In addition, BPI scores are available for all the three age ranges, except that immature dependency is not measured at ages 13-14.

In our dataset, scores of the cognitive tests and BPI are total standard scores, which come from applying norms of the corresponding national norming samples with mean 100 and standard deviation of 15 to the raw scores. We then rescale these total scores by subtracting 100 and dividing by 15 to arrive at the standard scores used in this analysis. Moreover, to investigate whether child health is an important mechanism, we use health measures based on the survey questions about health limitations or conditions.

[14] The PPVT-R measures children's hearing vocabulary knowledge; the PIAT Mathematics assesses children's achievement in math as taught in mainstream education; the PIAT Reading Recognition tests children's ability to read words aloud; and the PIAT Reading Comprehension measures children's ability to derive meaning from sentences.

[15] The domains for the six subscores are: antisocial behavior, anxiousness/depression, headstrongness, hyperactivity, immature dependency, and peer conflict/social withdrawal.

[16] This index, which accounts for correlation among the subscores, also improves test efficiency. Moreover, we mostly follow the procedure in Anderson (2008) to create the index. However, one step outlined in Anderson (2008) requires switching signs for bad outcomes so that the positive direction always indicates a better outcome. We skip this step to ensure larger values of our BPI composite index correspond to more behavior problems, just as higher BPI subscores do. Our index can be regarded as a mirror image of the Anderson-type composite index.

4.3. Descriptive Evidence

The following restrictions are imposed to construct the analytic samples of different age ranges. One, we exclude the NLSCYA respondents born to the oversampled disadvantaged non-Black, non-Hispanic women, because these women were not retained in the sample following the 1990 interview. Two, we drop the children who have fewer than three valid values of actual point-in-time eligibility from birth to an age of interest, so that our imputed total actual eligibility better captures true eligibility over the corresponding portions of childhood. Three, in the age 4-5 sample, we measure each outcome using the last valid observation which occurred after children turned 4 and before they turned 6 and then remove the children without a valid outcome value over this age range. We apply a similar rule for the outcomes measured at older ages.

Table 1 shows summary statistics of the three working samples.[17] The mean total simulated eligibility is greater than the mean total actual eligibility by about a year across the three age groups, when we use the March 1991 or March 1981 CPS for simulation. The pattern is not surprising. In a random national sample used for simulation from one of these early years, family incomes are generally lower than more recent periods (such as the 2000s). To construct our instrument, we then assign this fixed sample to each state and adjust income by CPI for each year. By construction, family income levels used for the simulated instrument tend to be fairly low across all years, making a large fraction of children eligible at any given age. As a result, the average total simulated eligibility appears larger than average total actual eligibility. Consistent with this explanation, the gap between the means of total simulated and actual eligibility is substantially reduced if we use the March 2001 CPS to create simulated eligibility since family incomes are higher. Moreover, the cross-cohort changes in cumulative eligibility which we exploit are very similar across the three simulated measures (not shown).

[Insert Table 1 Here]

The maternal control variables on marriage, highest level of education, and number of children are all constructed over the corresponding periods of childhood. For instance, in the age

[17] We do not expect the sample means of our standardized scores to be zero or close to zero, and similarly for the standard deviations being close to one, for two reasons. One, the NLSY79 used norming samples from 1968 (PIAT), 1979 (PPVT), and 1981 (BPI), whereas the assessments of NLSCYA respondents took place later. Two, while a norming sample was nationally representative of all children in the specific year, the NLSCYA sample is designed to be representative of the biological children of women born between 1957 and 1964.

4-5 sample, 25 percent of the children have mothers whose highest level of education was bachelor's degree or higher during the period from birth to age 4 or 5. The Armed Forces Qualifying Test (AFQT) is an aptitude test that was administered to all the NLSY79 respondents. Since the NLSCYA survey began in 1986, children of younger mothers were not surveyed or assessed at young ages. At later survey years, children of older mothers had not yet reached the older age ranges. As such, children in the age 4-5 sample have more advantaged mothers in terms of marital status, education, and AFQT, relative to the age 10-11 and 13-14 samples.

Appendix Figure A1 provides descriptive evidence that children with more cumulative program exposure tend to have better cognitive outcomes. We graph the cohort-level average years of simulated eligibility and test scores for NLSCYA children of three age ranges. Excluding a few small cells on the tails of birth cohort distributions, we focus on the 1981-1996 cohorts in the age 4-5 sample and 1976-1996 cohorts in the other two samples. Across the three age ranges, the mean years of simulated eligibility rose by 1.5 to 2 times between the 1976/1981 and 1996 birth cohorts. The figure also reveals upward trends in nearly all the test scores of the three samples.[18] As shown in each panel of Appendix Figure A2, concurrent to the substantial eligibility expansion, there are marked declines in the three aggregate BPI measures. Of course, various factors can contribute to the gains in child developmental outcomes, such as family environment, economic conditions, public investments in schools and social programs. This study focuses on disentangling the impact of public insurance expansion from the other factors.

5. Empirical Strategy

We begin our analysis on child developmental outcomes by estimating a model of the following form:

$$Y_{iast} = \alpha_0 + \alpha_1 ELIG_{iast} + \alpha_2 X_{iast} + \gamma_s + \theta_a + \varepsilon_{iast} \quad (1)$$

where Y_{iast} is a cognitive or behavioral outcome observed at age a and in year t for child i who resided in modal state s from birth up to age a. The parameter of interest is α_1 which measures the change in Y_{iast} associated with one additional year of eligibility. $ELIG_{iast}$ denotes total

[18] There are fluctuations in test scores along the trends, especially for the children born in the early 1990s who took the PPVT at ages 4-5. The degree of the fluctuations could be related to how much children of adjacent cohorts differ in the ages of taking the tests and how the assessment tests adjust the level of difficulty according to the test-taking ages.

actual eligibility, or the number of years child i was eligible for Medicaid or SCHIP up to age a. Moreover, X_{iast} consists of demographic controls for children (gender, race/ethnicity, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), and a set of indicators of whether actual eligibility could be imputed at every age up to age a and in each year between $t-a$ and t. These indicators effectively control for birth cohorts but do so more flexibly than birth cohort fixed effects (Thompson, 2017). A set of state fixed effects (FE) γ_s and age FE θ_a are also included.

Variation in total actual eligibility can be driven by changes in eligibility rules or changes in the economic circumstances of households with given eligibility criteria. However, with the second source of variation, ordinary least squares (OLS) estimation of equation (1) is unlikely to produce unbiased estimates of α_1 for several reasons. One, simultaneity bias arises if parents reduce labor supply to care for children with behavioral or other developmental problems but this reduction increases the likelihood of being eligible for public health insurance. Two, there may be unobserved family environment factors or changes in state-level sociodemographic characteristics that affect both family economic well-being (thus, actual eligibility) and child development. Three, actual eligibility is measured with error, due to response errors in the survey or inaccuracies in our eligibility calculator.

To surmount these problems, we utilize a strategy of simulated instruments pioneered by Currie and Gruber (1996a, b).[19] The measure of point-in-time simulated eligibility in each state-year-age cell calculated in section 4 offers a convenient parameterization of legislative differences affecting children in different states, years, and age groups. The corresponding total simulated eligibility, our instrumental variable, reflects the average generosity of public health

[19] Another possibility is to estimate a simpler model with cumulative exposure to exogenous eligiblity thresholds as the policy variable, rather than employing instruments. However, there are many more dimensions or rules for eligibility than the thresholds at play, as seen in the different eligibility pathways outlined in section 2. For instance, the AFDC program also requires a net income test which is independent of the thresholds and the parameters for the test vary across states over time. We also see state-level changes in the AFDC-UP, Ribicoff, and Medically Needy programs. These programs have additional rules concerning family structure, unemployment status, and their own thresholds for eligibility. Moreover, even with the same thresholds, states that implement different income-counting rules after welfare reform may have different proportions of children eligible for public insurance. As a result, using only the thresholds does not sufficiently capture the true variation in eligibility.

insurance rules to which a child is exposed until a certain age. The following two equations demonstrate the IV approach:

$$ELIG_{iast} = \beta_0 + \beta_1 SIMELIG_{iast} + \beta_2 X_{iast} + \eta_s + \psi_a + \upsilon_{iast} \quad (2)$$

$$Y_{iast} = \alpha_0 + \alpha_1 \widehat{ELIG}_{iast} + \alpha_2 X_{iast} + \gamma_s + \theta_a + \varepsilon_{iast} \quad (3)$$

where $\widehat{ELIG}_{iast}$ in equation (3) is the predicted total eligibility from equation (2), the first stage regression of $ELIG_{iast}$ on the instrument $SIMELIG_{iast}$ (total simulated eligibility for child i). The corresponding reduced form model is analogous to equation (2), regressing Y_{iast} on $SIMELIG_{iast}$, conditional on the other independent variables. Note that data limitations of the NLSY prevent us from directly estimating program take-up and the effects of program enrollment. We estimate an intent-to-treat effect, which is the focus of much of the Medicaid literature and is also the most policy relevant.

As discussed in section 4, within-state variation in $SIMELIG_{iast}$ results only from changes to policy. Therefore, $SIMELIG_{iast}$ is not correlated with unobserved family characteristics or individual response errors. It is then credibly excluded from equation (1) and helps achieve identification. Furthermore, when looking at the heterogeneous effects of eligibility by age, we use simulated eligibility of different age groups to instrument for the corresponding actual eligibility measures.

If we had not considered all the eligibility pathways or adjusted for the state-varying rules about disregards, the resulting notable inaccuracies in our calculator would introduce similar errors in actual and simulated eligibility, as in several prior studies (Currie et al., 2008; Levine and Schanzenbach, 2009; Thompson, 2017). Then, a valid concern arises on the potential correlation between the IV and errors in equation (3). However, since our calculator uses a comprehensive list of eligibility rules, measurement error in our actual and simulated eligibility should stem mainly from random response errors. As such, measurement error in our CPS instrument is unlikely correlated with measurement error in actual eligibility from the NLSY sample. Another threat to our identification strategy is that public insurance expansions may be related to the underlying cross-cohort trends in child outcomes at the state level. On this front, East et al. (2023) use an alternative strategy which accounts for state pre-trends and find the new estimates are highly consistent with the original results in Currie and Gruber (1996b) from the IV approach. This finding lends credence to the validity of using state-level variation in the expansions' magnitude and timing as a natural experiment.

While there is a rich framework for the baseline IV analysis, we will conduct specification checks, adding state-level characteristics such as other state policies to the model. Moreover, to assess whether the results are sensitive to the potential correlated secular trends in child outcomes and public insurance eligiblity, we estimate an IV model with the second stage taking the form:

$$Y_{icarst} = \alpha_0 + \alpha_1 \widehat{ELIG}_{icarst} + \alpha_2 X_{icast} + \lambda_r \cdot C + \mu_{ra} + \gamma_s + \theta_a + \varepsilon_{iast} \quad (4)$$

where c indexes birth cohort and r indexes region. Relative to equation (3), equation (4) adds region-specific birth cohort trends $\lambda_r \cdot C$ and region-by-age effects μ_{ra}. These additional terms are also included in the first stage specification. Two variants of this enriched IV model are utilized in additional checks: one, we further add state-level controls and replace the indicators of the ages and years when actual eligibility could be imputed by the cohort FE; two, we instead include state-specific birth cohort trends and state-by-age effects. As these models are also more demanding of the data, we have less statistical power to estimate the parameter of interest and the results tend to be less precise (Cohodes et al., 2016).

6. Results

Before analyzing child developmental outcomes by OLS and IV estimation, we test for covariate balance, since well-balanced controls make the standard IV independence assumption more credible. Our preferred test regresses a child or mother characteristic on indicators for the first and second tercile of the distribution of total simulated eligibility, while adjusting for other factors. In the two model specifications we consider, such factors are cohort, state, and age FE, region (or state)-by-age FE, and region (or state)-specific cohort trends. The results are reported in Appendix Table A1, where most of the coefficient estimates for each age group are insignificant.[20] Using other specifications, such as models without trends, does not materially change the results (not shown). In summary, we find little evidence of covariate imbalance.

6.1. Baseline Results

[20] A similar pattern is seen when we code a set of indicators for the dependent variables with more than two possible values, such as number of children in household. Moreover, in a series of joint F tests, we generally fail to reject the null hypothesis that the coefficients on the indicators for the first and second tercile are jointly zero. The results are available upon request.

Table 2 presents the OLS, reduced form (RF), first-stage (FS), and IV estimates on cognitive outcomes from the baseline specification in section 5. The OLS estimates are all negative and mostly statistically significant. This pattern could be driven by negative selection into public insurance by unobserved family-level characteristics or simultaneity bias, since developmental outcomes could influence family income and program eligibility. However, after implementing the IV strategy, we find an additional year of public insurance eligibility significantly improves all the test scores across the three age ranges by 0.046 to 0.17 standard deviations (SDs), except the score of PIAT Reading Comprehension measured at ages 10-11.

[Insert Table 2 Here]

We use the effective F statistic from the first stage regression for a clustered-robust weak-instrument test (Olea and Pflueger, 2013).[21] For the outcomes measured at ages 10-11 and 13-14, We reject the null hypothesis of having a weak instrument which leads to a 10 percent of the worst-case bias in the IV estimates. When we consider a 20 percent of the worst-case bias, the effective F statistics lead to a pass of the test for all the age groups. The RF results suggest that cumulative exposure to more generous eligibility rules leads to better cognitive outcomes. It is unsurprising that the RF and IV estimates are similar, since the FS coefficients are generally close to 1. In addition, most of the significant IV estimates appear to be statistically distinct from the OLS counterparts, as displayed in Figure 2.

[Insert Figure 2 Here]

Table 3 reports the baseline results for behavioral problems. As with the cognitive outcomes, the OLS results imply that cumulative eligibility results in worse behavior. Turning to the IV results, we find no significant impact on any behavioral outcome for children aged 4-5 or 10-11. For ages 4-5, many IV estimates are similar to the OLS counterparts, but the standard errors for all the IV estimates are much larger and we cannot rule out effect sizes of potential interest. For ages 10-11, our IV estimates are more precise, although the IV approach also generally results in much smaller coefficient estimates. For instance, the 95% confidence interval for the impact on the BPI composite index is (-0.064, 0.070), and thus we rule out large effects.

[21] The null hypothesis of this test is that we have a weak instrument, so that the IV estimator's bias exceeds a fraction of the "worst-case" bias. For a given fraction (say 10 percent) of bias, the null hypothesis will be rejected if the effective F statistic is greater than the corresponding critical value. Four commonly used critical values are shown in the notes of Table 2.

[Insert Table 3 Here]

In contrast, for six behavioral problem outcomes at ages 13-14, we find statistically significant improvements due to an additional year of eligibility. They include the overall BPI score, BPI composite index, internalizing behavior score, and three subscores. The RF estimates on the same six behavioral problem outcomes are also statistically significant (Appendix Table A2). The size of these significant IV estimates ranges from 0.051 to 0.101 SDs of these scores. For brevity, only the results on the three aggregate BPI measures are plotted in Figure 2. The OLS and IV estimates for the composite index and the overall BPI score measured at ages 13-14 are statistically different from each other, as suggested by the non-overlapping confidence intervals.[22] Furthermore, we see strong first-stage relationships in Appendix Table A2 for the behavioral outcomes.[23]

6.2. Heterogeneity by Gender and Race/Ethnicity

In Figure 3, we plot IV results when we estimate our baseline specification by gender. The OLS and IV point estimates for cognitive outcomes are reported in Appendix Table A4. The figure shows that impacts on cognitive outcomes are larger for male children across the three age ranges. Indeed, all the IV estimates for females are insignificant at the two younger age ranges, though the estimates vary in precision, and thus the results suggest that the impact of public health insurance at younger ages is driven by male children. At ages 13-14, while the impacts for males are larger, there is mixed evidence for how much larger they are, and some impacts for females are statistically significant.

[Insert Figure 3 Here]

[22] We have performed additional analysis regarding longer-term educational outcomes at ages 22-24 for the affected children. Broadly consistent with Cohodes et al. (2016) and Brown et al. (2020), our results suggest additional childhood eligibility increases college enrollment and completion. Thus, better developmental outcomes documented above contribute to improvements in adult educational attainment.

[23] The standard assumptions of parallel trends and no anticipatory effect are imposed when we estimate equation (3) for the baseline analysis. These conditions are unlikely to hold if additional eligibility due to future eligibility expansions is significantly associated with contemporaneous outcomes. We estimate two types of model for this falsification test. One, we augment the original IV specification for the outcomes at ages 4-5 (or 10-11) with future total eligiblity at ages 6-11 (or 12-14). Two, we alternatively assess whether increased eligibility at ages 10-11 (or 13-14) predicts the outcomes at ages 4-5 (or 10-11). As shown in Appendix Table A3, increased future eligiblity is not significantly associated with tests scores and aggregate BPI measures in either model.

Appendix Table A5 reports estimates of the impact on behavioral problems by gender; only IV results are shown to save room. For ages 4-5, estimates for both genders are insignificant but very imprecise. For ages 10-11, estimates are also insignificant (with one exception), but standard errors are smaller. At ages 13-14, we find statistically significant improvements in problem behavior for males across almost all aggregate measures and subscores. For example, an additional year of eligibility for public insurance reduces the BPI composite index by 0.14 SDs. For females aged 13-14, impacts are negative but smaller and thus rarely statistically significant. Accordingly, Figure 3 displays overlapping confidence intervals for the aggregate BPI measures.

With respective to research on long-run heterogeneous impacts by gender, Brown et al. (2020) document a larger increase in contemporaneous wage for females aged 23-28 but a similar positive effect on college enrollment for both genders. Yet two other studies find a more salient improvement in adult educational attainment and a larger decline in criminal activity for males (Cohodes et al., 2016; Hendrix and Stock, 2022). Our results suggest development of both cognitive and noncognitive skills during childhood for males are likely to matter for their later-life benefits.

Figure 4 plots IV results by race/ethnicity for test scores and aggregate BPI measures, and Appendix Tables A6 and A7 present point estimates by race/ethnicity for cognitive and behavioral outcomes, respectively. Using the NLSY79 screener variable of "Racial/Ethnic Cohort" for the mother, we classify children as "Black or Hispanic" or "non-Black, non-Hispanic".[24] Our IV estimation yields positive effects of increased eligibility on test scores for both subgroups, in contrast to the OLS estimates which are all negative. Overall, we do not see a consistent pattern of results for test scores being stronger for one subgroup over the other. At the two younger age ranges, standard errors are large relative to coefficients. At ages 13-14, we find positive and significant effects for non-Black, non-Hispanic children; at these ages, effects for children who are Black or Hispanic are positive but insignificant (except the score of PIAT Reading Recognition). For the behavioral outcomes, we again obtain imprecise estimates at ages 4-5 for both subgroups. For the older age ranges, coefficient estimates are generally more precise and several coefficient estimates for each subgroup are negative and statistically significant. We

[24] Arcidiacono et al. (2015) find that the race of the mother is more predictive than child race for educational outcomes.

find improvements in behavior outcomes are generally larger for non-Hispanic, non-Black children at ages 13-14, but confidence intervals for both subgroups overlap substantially.

[Insert Figure 4 Here]

Moreover, we consider an alternative ethnic split of "White" and "non-White" children, as some prior studies document noticeable later-life benefits for non-White children with cumulative program exposure (Cohodes et al., 2016; Goodman-Bacon, 2021; Noghanibehambari, 2022). To construct the non-White subsample, we combine the children identified as Native American, Asian, and Hawaiian/Pacific Islander with the children who are Black or Hispanic children. The other children with non-missing self-identification for the racial/ethnic origin are included in the White subsample, most of whom are children of European origin. Again, we focus on the corresponding IV results in Appendix Tables A8 and A9. The general pattern of results is similar to the previous ethnic split. The main exception is that we find a stronger pattern of negative and significant results on behavioral outcomes for non-White children at ages 13-14 relative to children who are Black or Hispanic at the same ages. In addition, there are more significant gains in the cognitive outcomes for non-White children than children who are Black or Hispanic.

6.3. Effects of Eligibility at Different Age Ranges

Next, we investigate whether the effects of insurance access vary by the age at which a child is eligible. Table 4 presents the IV estimates from this analysis, which focuses on the test scores and aggregate BPI measures for children aged 13-14. In Panel A, we estimate an IV model in which years of eligibility over three age ranges (0-4, 5-9, and 10-14) enter simultaneously.[25] For this exercise, we limit the sample to children with at least one valid value of point-in-time eligibility during each age range. Of course, highly correlated eligibility at different ages and the reduced sample size could lower precision. Eligibility in early childhood (ages 0-4) is the main driver for the gains in the test scores measured at ages 13-14. We generally find no evidence that a particular period of childhood drives any improvement to behavior for adolescents aged 13-14.

[25] Alternatively, we can estimate an IV model where eligibility at each age range is included separately. But this approach is less preferable due to an identification issue. Several legislative changes affected childhood eligiblity in multiple age ranges above. For this reason, simulated eligibility for a given age range, which serves as the IV in such an alternative model, could be correlated with omitted actual eligibility of another age range in the error term.

[Insert Table 4 Here]

In Panel B, we consider a similar model that includes prenatal eligibility. For this analysis, our sample necessarily excludes quite a few children with missing prenatal eligibility.[26] Nevertheless, when we repeat the exercise above with childhood eligibility using the smaller sample, the results are similar to Panel A (not shown). The standard errors for the coefficient estimates of prenatal eligibility are quite large, though we still find a significant and positive impact on the PIAT Reading Comprehension score. Otherwise, the estimates in Panel B are generally similar to those in Panel A, which suggest that eligibility in early childhood is the most important for later cognitive outcomes. There is no pattern of note for the behavioral outcomes.

Prior research is inconclusive about the effects by age of eligibility. Two studies show only prenatal eligibility is associated with improved test scores and educational attainment (Levine and Schanzenbach, 2009; Miller and Wherry, 2019). However, Cohodes et al. (2016) find childhood eligibility at older ages rather than at birth drives long-term human capital outcomes. Our findings indicate that prenatal eligibility and childhood eligibility both seem important for cognitive development. Moreover, though the heterogeneity in effects by age is consistent with sensitive periods of development, there are alternative explanations for this pattern, such as age-based differences in duration of program enrollment or covered health services.

To put our estimates in perspective, first, Levine and Schanzenbach (2009) find that an increase of 50 percentage points in simulated eligibility at birth improves reading scores in the 4th and 8th grade by 0.09 SDs. We rescale it into 0.11 SDs in terms of actual eligibility at birth, using the first-stage coefficient 0.84 from Cutler and Gruber (1996). Since our measure of prenatal eligibility is total exposure to eligibility in utero and during the first year of life, an increase of 50 percentage points is roughly one additional year of actual prenatal eligibility. Using the estimate in Table 4, we find it leads to an increase in the PIAT Reading Comprehension score by 0.59 SDs, much larger than 0.11 SDs. However, as discussed earlier, mismeasured eligibility could bias downward the estimates in Levine and Schanzenbach (2009).

[26] For a given child, total eligibility of an age group beyond infancy (such as ages 1-4) will be missing if data limitations prevent us from imputing eligibility at any age within the range and aggregating these age-specific measures. This could only happen when information on key variables for eligibility calculation is lacking throughout multiple survey years, which is uncommon. In contrast, constructing prenatal eligibility requires information over a shorter time frame. It is not unusual that data is unavailable during either the prenatal period or first year of life. Therefore, missing values for prenatal eligibility occur more often than the other age groups.

Moreover, while they only consider eligibility changes after 1987, about 40 percent of the children in our sample were born in very poor families before 1986 and may be particularly sensitive to prenatal eligibility expansions in the early 1980s (Currie and Gruber, 1996; East et al., 2023).

6.4. The Health Effect as a Potential Mechanism

We next investigate if improved child health is a mediator of the positive impacts on developmental outcomes. To the extent that health enters the production function of cognitive and socio-emotional skills, the health effect of public insurance expansions could lead to better developmental outcomes. We focus on children aged 13-14, for whom the effects of increased eligibility documented above are most pronounced. For adolescents, the survey collected information on any physical, emotional, or mental condition which limits school attendance, schoolwork, or play activities, and on any condition which requires a doctor, regular use of medicine, or special equipment.[27]

Increased utilization of effective medical care due to public insurance expansions could improve childhood health. However, additional eligibility could also increase detection of health conditions. Since some conditions are difficult to fully resolve, it would result in more reported conditions at later ages. Our IV estimates in Table 5 capture the net effect. We find children with increased eligibility are significantly less likely to have a health limitation which affects school attendance or a health condition that requires a doctor or special equipment at older ages. Moreover, there is a significant reduction in having any of the three types of health conditions (Row 9).[28] The results differ from two previous studies which document no medium-run effect on teenager health (De la Mata, 2012; Qureshi and Gangopadhyaya, 2021).[29]

[27] The specific health limitations or conditions coded in the dataset include learning disability, minimal brain dysfunction, hyperkinesis, asthma, respiratory disorder, speech impairment, serious hearing difficulty, serious difficulty in seeing, serious emotional disturbance, allergic conditions, crippled, mental retardation, heart trouble, chronic nervous disorder, chronic ear problems, blood disorder or immune deficiency, epilepsy/seizures, and others.

[28] The findings in Table 5 are generally in line with Thompson (2017) which focuses on health of young adults. Yet unlike our result in Row 9, his study reports an insignificant effect on having any chronic health condition.

[29] With respect to other potential mechanisms, cumulative exposure to the program could influence child development through changes in home inputs. We then carry out similar empirical tests, using mother-

[Insert Table 5 Here]

6.5. Sensitivity Analyses

We examine the sensitivity of our IV results to a wide array of alternatives. First, we control for state Earned Income Tax Credit (EITC) amounts, AFDC/Temporary Assistance for Needy Families (TANF) maximum monthly benefit, unemployment rate, and school expenditures per pupil (only for analyzing adolescent outcomes), all of which were average values over the corresponding periods of childhood.[30] The estimated effects on cognitive outcomes in Table 6 are similar to the baseline. But the results on behavioral outcomes at ages 13-14 are more sensitive to these additional controls, as shown in Table 7. Second, Table 6 shows that adding region-specific cohort trends and region-by-age FE generally reduces the size and precision of the estimated impacts on cognitive outcomes.[31] Alternatively, including state-specific cohort trends and state-by-age FE, we see further reduction in precision but somewhat larger effects. Additional analysis also reveals a weaker first-stage relationship in this case (not shown). Repeating the two exercises for the behavioral outcomes in Table 7, we find the resulting estimates are largely insignificant. The other two robustness checks in Tables 6 and 7 are: limiting the sample to children whose state of birth is the same as their state of residence to address endogenous mobility and constructing the instrument by the March 1981 CPS. The findings conform to those in Tables 2 and 3.

[Insert Table 6 Here]

[Insert Table 7 Here]

Appendix Figure A3 plots the results from the above five robustness checks for test scores and aggregate BPI measures and compares them with the baseline. Moreover, using the samples

---

reported individual item scores from the Home Observation Measurement of the Environment-Short Form (HOME-SF). The estimates are mostly imprecise.

[30] The state EITC data are taken from the National Bureau of Economic Research's database of State EITC Provisions 1977-2018. Data on AFDC/TANF maximum monthly benefit for a 3-person family are drawn from the University of Kentucky Center for Poverty Research. We use unemployment rate data from the Bureau of Labor Statistics and data on expenditures per pupil from the Digest of Education Statistics.

[31] These region-level trends and region-by-age FE are constructed over 9 regional divisions: New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific. Using indicators of four larger regions (Northeast, Midwest, South, West) for the trends and interaction terms produces similar results (suppressed for brevity).

of ages 4-5 and 10-11, we perform the same exercises for the behavioral outcomes in Appendix Tables A10 and A11. The results change little from those in Table 2. The only exception is that there are now significant improvements in several BPI measures at ages 10-11, when we add state-specific cohort trends and state-by-age FE. Appendix Tables A12 and A13 carry out another set of sensitivity analyses. We suppress the results for the BPI subscores to save room. One, we limit the sample to children whose family incomes were persistently below 400 percent of FPL, as higher income families, which were minimally impacted by the eligibility changes, may not yield a reliable counterfactual. We see a pattern analogous to Tables 2 and 3 but smaller effects for the outcomes at ages 13-14.[32] Two, using a richer specification with the cohort FE, region-specific cohort trends, region-by-age FE, and additional state-level controls, we find the estimated impacts on cognitive outcomes are typically substantively smaller and less precise. As to behavioral outcomes, we see significant yet adverse effects on the overall BPI and composite index for children aged 10-11; but none of the corresponding estimates are precise for the other two age ranges. The other three analyses in Appendix Tables A12 and A13 are: employing the March 2001 CPS for the instrument, focusing on children born in 1980-1999 (the birth cohorts common in the three child samples), and estimating the model without sampling weights. Results of these exercises are similar to the benchmark findings.[33]

Finally, we apply corrections of false discovery rate (FDR) to the results of test scores and aggregate BPI measures. As with the three BPI measures, the test scores are based on all the assessment items. We calculate sharpened q-values of the listed outcomes for each age group, using a two-stage procedure for tighter control of FDR (Anderson, 2008). Such a sharpened value gives the smallest FDR level at which a hypothesis would be rejected. In Appendix Table A14, we find the significant results at ages 13-14, suggested by the unadjusted p values, retain

[32] In a related placebo exercise, we look at children from higher income families. The IV approach is not appropriate here, since such children mostly have zero years of actual eligibility. In addition, it is possible that some families were generally poor but occasionally had incomes above 400 percent of FPL. We exclude these families using additional restrictions (such as requiring that a family's income is not less than 300% of FPL over half of the corresponding portions of childhood). In addition, we use a more robust reduced form specification with state-specific cohort trends for this falsification test. Across the three age groups, we find little evidence that total simulated eligiblity significantly affects developmental outcomes for children of higher income families (not shown).

[33] We also vary the lower limit of valid point-in-time eligibility counts to form alternative samples or require at least one valid value of point-in-time eligibility in ages 0-4, 5-9, and 10-14 for the age 13-14 sample. None of these sample restrictions materially change the results. The results are available upon request.

significance when controlling FDR for multiple inference. The significant estimates for children aged 4-5 or 10-11 appear less robust. When we experiment with dropping the BPI composite index or pooling the list of outcomes across age groups, the resulting sharpened q-values yield a similar pattern. Taken together, our findings on cognitive outcomes, especially those measured at older ages, are more robust than behavioral outcomes.

7. Conclusion

This paper examines the effects of expanding public health insurance on child cognitive development and behavior. We find cumulative eligibility gain results in significant improvements in cognitive skills, especially for young teenagers. There is also suggestive evidence that additional eligibility leads to better noncognitive outcomes. The estimated effects are stronger for male than female children. There are also larger impacts on test scores for non-Black, non-Hispanic children. With a slightly different ethnic split, we find more notable behavioral improvements for non-White children than White children. Both prenatal eligibility and childhood eligibility are important for driving gains in the test scores measured at older ages. In addition, improved child health is found to be a mediator of the impact of increased eligibility.

Our estimates compare favorably with prior findings on other policies which influence child cognitive and noncognitive outcomes. Dahl and Lochner (2012) show an increase of $1,000 (in 2000 dollars) in family income due to EITC expansions improves the combined math and reading score by 0.06 SDs for children aged 8-14. Table 1 shows similar gains in test scores of 0.02-0.11 SDs for children aged 10-11 and 13-14, resulting from one more year of eligibility which costs about $484 (in 2000 dollars) suggested by Brown et al. (2020). Deming (2009) estimates children aged 3-5 who participate in the Head Start program for a maximum of two years have test score indices which are 0.06-0.15 SDs higher at ages 5-14.[34] Carneiro and Ginja (2014) find male children eligible for the Head Start later have fewer behavioral problems at ages 12-13, with -0.27 SDs on the overall BPI score. The magnitude of our estimates are again on the order of these Head Start studies.

Finally, in evaluating the cost-effectiveness of a childhood intervention, one practical guide is whether it could increase test scores by at least 0.025 SDs per child per $1,000 spending (Ludwig

[34] The take-up rate among eligible children is about 0.7 in a recent experiemntal study of the Head Start (Ludwig and Phillips, 2008).

and Phillips, 2008). The Tennessee's Project STAR passes this cost-effectiveness test, as Krueger (2003) finds a discounted total cost of $7,417 (in 1998 dollars, 5 percent discount rate) of randomly assigning a child to a small class results in a total gain of 0.4 SDs in math and reading scores. In the context of the Medicaid and SCHIP expansions, we now consider a very robust specification in Table A12 (column 2) which gives only one precise and conservative estimate of 0.053 SDs for the PIAT Mathematics score measured at ages 13-14. When we rescale it by 1/0.63 using the take-up results from Brown et al. (2020), the resulting 0.084 SDs indicates the effect for an additional year of enrollment.[35] About 5 additional years of enrollment can also generate a test-score gain of 0.4 SDs. The corresponding discounted total cost is only $3,152 (in 1998 dollars, 5 percent discount rate), when we use the enrollment cost estimate from Brown et al. (2020). Clearly, expanding public insurance also passes the test and appears quite cost-effective relative to Project STAR. Overall, our findings underscore the role of increasing children's access to public health insurance in development of their human capital.

[35] The original take-up estimate is 0.59 for one more year of simulated eligibility in Brown et al. (2020). We adjust this coefficient using their first-stage result of 0.94 to find one more year of actual eligibility increases enrollment in Medicaid/SCHIP at some point during the entire childhood by 63 percent. Since we focus on the take-up rate by age 14, the 0.63 could be an overstatement.

References

Almond, D., Currie, J. and Duque, V., 2018. Childhood circumstances and adult outcomes: Act II. *Journal of Economic Literature*, *56*(4), pp.1360-1446.

Anderson, M.L., 2008. Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association*, *103*(484), pp.1481-1495.

Arcidiacono, P., Beauchamp, A., Hull, M. and Sanders, S. 2015. Exploring the racial divide in education and the labor market through evidence from interracial families. *Journal of Human Capital*, *9*(2), pp. 198-238.

Boudreaux, M.H., Golberstein, E. and McAlpine, D.D., 2016. The long-term impacts of Medicaid exposure in early childhood: Evidence from the program's origin. *Journal of Health Economics*, *45*, pp.161-175.

Brown, D.W., Kowalski, A.E. and Lurie, I.Z., 2020. Long-term impacts of childhood Medicaid expansions on outcomes in adulthood. *The Review of Economic Studies*, *87*(2), pp.792-821.

Buchmueller, T., Ham, J.C. and Shore-Sheppard, L.D., 2015. The Medicaid Program. In R.A. Moffitt (Ed.), *Economics of Means-tested transfer programs in the United States*. Chicago, IL: University of Chicago Press.

Bullinger, L.R., Gopalan, M. and Lombardi, C.M., 2023. Impacts of publicly funded health insurance for adults on children's academic achievement. *Southern Economic Journal*, *89*(3), pp.860-884.

Card, D. and Shore-Sheppard, L.D., 2004. Using discontinuous eligibility rules to identify the effects of the federal Medicaid expansions on low-income children. *Review of Economics and Statistics*, *86*(3), pp.752-766.

Carneiro, P. and Ginja, R., 2014. Long-term impacts of compensatory preschool on health and behavior: Evidence from Head Start. *American Economic Journal: Economic Policy*, *6*(4), pp.135-173.

Chorniy, A., Currie, J. and Sonchak, L., 2018. Exploding asthma and ADHD caseloads: The role of Medicaid managed care. *Journal of Health Economics*, *60*, pp.1-15.

Cohodes, S.R., Grossman, D.S., Kleiner, S.A. and Lovenheim, M.F., 2016. The effect of child health insurance access on schooling: Evidence from public insurance expansions. *Journal of Human Resources*, *51*(3), pp.727-759.

Cunha, F. and Heckman, J., 2007. The technology of skill formation. *American Economic Review*, *97*(2), pp.31-47.

Cunha, F., Heckman, J.J. and Schennach, S.M., 2010. Estimating the technology of cognitive and noncognitive skill formation. *Econometrica*, *78*(3), pp.883-931.

Currie, J., Decker, S. and Lin, W., 2008. Has public health insurance for older children reduced disparities in access to care and health outcomes?. *Journal of Health Economics*, *27*(6), pp.1567-1581.

Currie, J. and Duque, V., 2019. Medicaid: what does it do, and can we do it better?. *The ANNALS of the American Academy of Political and Social Science*, *686*(1), pp.148-179.

Currie, J. and Gruber, J., 1996a. Health insurance eligibility, utilization of medical care, and child health. *The Quarterly Journal of Economics*, *111*(2), pp.431-466.

Currie, J. and Gruber, J., 1996b. Saving babies: The efficacy and cost of recent changes in the Medicaid eligibility of pregnant women. *Journal of Political Economy*, *104*(6), pp.1263-1296.

Cutler, D.M. and Gruber, J., 1996. Does public insurance crowd out private insurance?. *The Quarterly Journal of Economics*, *111*(2), pp.391-430.

Dafny, L. and Gruber, J., 2005. Public insurance and child hospitalizations: access and efficiency effects. *Journal of Public Economics*, *89*(1), pp.109-129.

Dahl, G.B. and Lochner, L., 2012. The impact of family income on child achievement: Evidence from the earned income tax credit. *American Economic Review*, *102*(5), pp.1927-1956.

De La Mata, D., 2012. The effect of Medicaid eligibility on coverage, utilization, and children's health. *Health Economics*, *21*(9), pp.1061-1079.

Deming, D., 2009. Early childhood intervention and life-cycle skill development: Evidence from Head Start. *American Economic Journal: Applied Economics*, *1*(3), pp.111-134.

Deming, D.J., 2017. The growing importance of social skills in the labor market. *The Quarterly Journal of Economics*, *132*(4), pp.1593-1640.

East, C.N., Miller, S., Page, M. and Wherry, L.R., 2023. Multi-generational impacts of childhood access to the safety net: Early life exposure to Medicaid and the next generation’s health. *American Economic Review*, *113* (1): 98-135.

Edin, P.A., Fredriksson, P., Nybom, M. and Öckert, B., 2022. The rising return to noncognitive skill. *American Economic Journal: Applied Economics*, *14*(2), pp.78-100.

Finkelstein, A., Taubman, S., Wright, B., Bernstein, M., Gruber, J., Newhouse, J.P., Allen, H., Baicker, K. and Oregon Health Study Group, 2012. The Oregon health insurance experiment: Evidence from the first year. *The Quarterly Journal of Economics*, *127*(3), pp.1057-1106.

Gangopadhyaya, A. and Schiman, J.C., 2023. Does subsidized public health insurance for parents improve children's human capital and close achievement gaps?. *Economics of Education Review*, *93*, pp.1-19.

Goodman-Bacon, A., 2018. Public insurance and mortality: evidence from Medicaid implementation. *Journal of Political Economy*, *126*(1), pp.216-262.

Goodman-Bacon, A., 2021. The long-run effects of childhood insurance coverage: Medicaid implementation, adult health, and labor market outcomes. *American Economic Review*, *111*(8), pp.2550-2593.

Gross, T. and Notowidigdo, M.J., 2011. Health insurance and the consumer bankruptcy decision: Evidence from expansions of Medicaid. *Journal of Public Economics*, *95*(7-8), pp.767-778.

Gruber, J., 2003. Medicaid. In R.A. Moffitt (Ed.), *Means-tested transfer programs in the United States*. Chicago, IL: University of Chicago Press.

Gruber, J. and Simon, K., 2008. Crowd-out 10 years later: Have recent public insurance expansions crowded out private health insurance? *Journal of Health Economics*, *27*(2), pp.201-217.

Gruber, J. and Yelowitz, A., 1999. Public health insurance and private savings. *Journal of Political Economy*, *107*(6), pp.1249-1274.

Guldi, M. and Hamersma, S., 2023. The effects of pregnancy-related Medicaid expansions on maternal, infant, and child health. *Journal of Health Economics*, 87, p.102695.

Heckman, J.J., Stixrud, J. and Urzua, S., 2006. The effects of cognitive and noncognitive abilities on labor market outcomes and social behavior. *Journal of Labor Economics*, 24(3), pp.411-482.

Hendrix, L. and Stock, W.A., 2022. Investing in health and public safety: Childhood Medicaid eligibility and later life criminal behavior. *Journal of Human Resources*, forthcoming.

Howell, E., Decker, S., Hogan, S., Yemane, A. and Foster, J., 2010. Declining child mortality and continuing racial disparities in the era of the Medicaid and SCHIP insurance coverage expansions. *American Journal of Public Health*, *100*(12), pp.2500-2506.

Howell, E.M. and Kenney, G.M., 2012. The impact of the Medicaid/CHIP expansions on children: a synthesis of the evidence. *Medical Care Research and Review*, *69*(4), pp.372-396.

Kearney, M.S. and Levine, P.B., 2009. Subsidized contraception, fertility, and sexual behavior. *The Review of Economics and Statistics*, *91*(1), pp.137-151.

Krueger, A.B., 2003. Economic considerations and class size. *The Economic Journal*, *113*(485), pp.F34-F63.

Leininger, L., Levy, H. and Schanzenbach, D., 2010. Consequences of SCHIP expansions for household well-being. *Forum for Health Economics & Policy,* 13(1), pp. 1-30.

Levine, P.B. and Schanzenbach, D., 2009, May. The impact of children's public health insurance expansions on educational outcomes. *Forum for Health Economics & Policy, 12*(1).

Ludwig, J. and Phillips, D.A., 2008. Long-term effects of Head Start on low-income children. *Annals of the New York Academy of Sciences*, *1136*(1), pp.257-268.

Medicaid and CHIP Payment and Access Commission (MACPAC). 2012. *Report to the Congress on Medicaid and CHIP*. March. Washington, DC: Medicaid and CHIP Payment and Access Commission.

Medicaid and CHIP Payment and Access Commission (MACPAC). 2013. *Report to the Congress on Medicaid and CHIP*. June. Washington, DC: Medicaid and CHIP Payment and Access Commission.

Medicaid and CHIP Payment and Access Commission (MACPAC). 2014. *Report to the Congress on Medicaid and CHIP*. June. Washington, DC: Medicaid and CHIP Payment and Access Commission.

Miller, S. and Wherry, L.R., 2019. The long-term effects of early life Medicaid coverage. *Journal of Human Resources*, *54*(3), pp.785-824.

Noghanibehambari, H., 2022. Intergenerational health effects of Medicaid. *Economics & Human Biology*, *45*, p.101114.

Olea, J.L.M. and Pflueger, C., 2013. A robust test for weak instruments. *Journal of Business & Economic Statistics*, *31*(3), pp.358-369.

Qureshi, J.A. and Gangopadhyaya, A., 2021. Childhood Medicaid eligibility and human capital. *Economics of Education Review*, *82*, pp.1-13.

Strumpf, E., 2011. Medicaid's effect on single women's labor supply: Evidence from the introduction of Medicaid. *Journal of Health Economics*, *30*(3), pp.531-548.

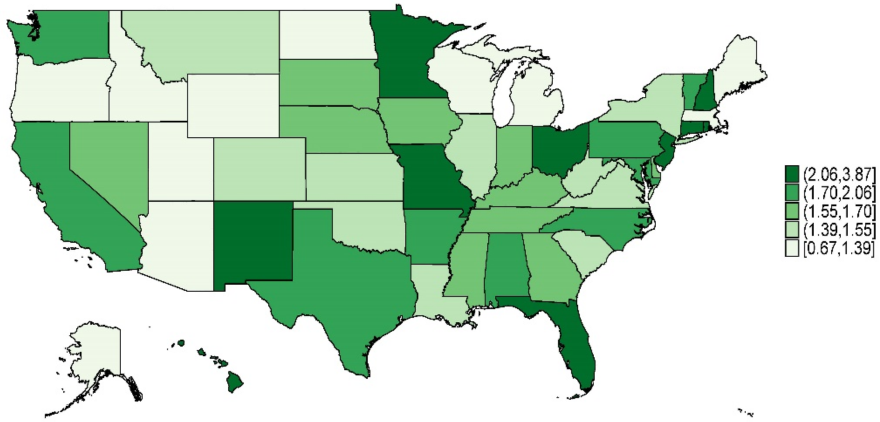
Thompson, O., 2017. The long-term health impacts of Medicaid and CHIP. *Journal of Health Economics*, *51*, pp.26-40.

Wherry, L.R. and Meyer, B.D., 2016. Saving teens: using a policy discontinuity to estimate the effects of Medicaid eligibility. *Journal of Human Resources*, *51*(3), pp.556-588.

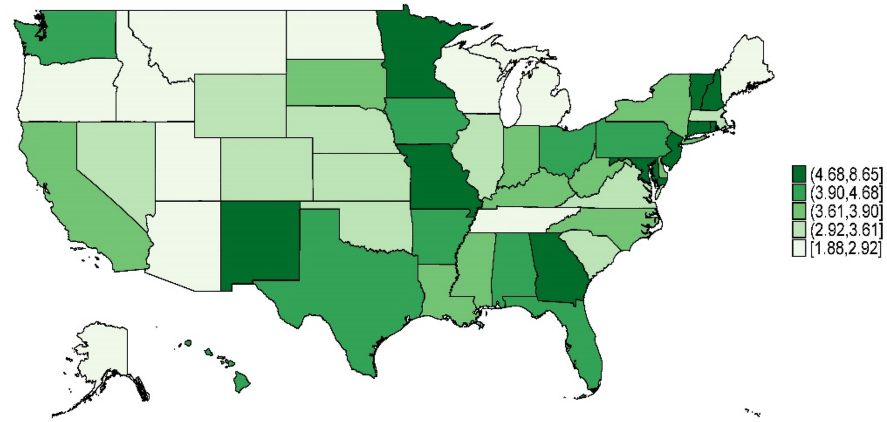
Wherry, L.R., Miller, S., Kaestner, R. and Meyer, B.D., 2018. Childhood Medicaid coverage and later-life health care utilization. *Review of Economics and Statistics*, *100*(2), pp.287-302.

Zavodny, M. and Bitler, M.P., 2010. The effect of Medicaid eligibility expansions on fertility. *Social Science & Medicine*, *71*(5), pp.918-924.

Panel 1. Changes in years of simulated eligiblity from birth to age 5 across two birth cohorts (1981 and 1996), by state



Panel 2. Changes in years of simulated eligiblity from birth to age 11 across two birth cohorts (1976 and 1996), by state



Panel 3. Changes in years of simulated eligiblity from birth to age 14 across two birth cohorts (1976 and 1996), by state

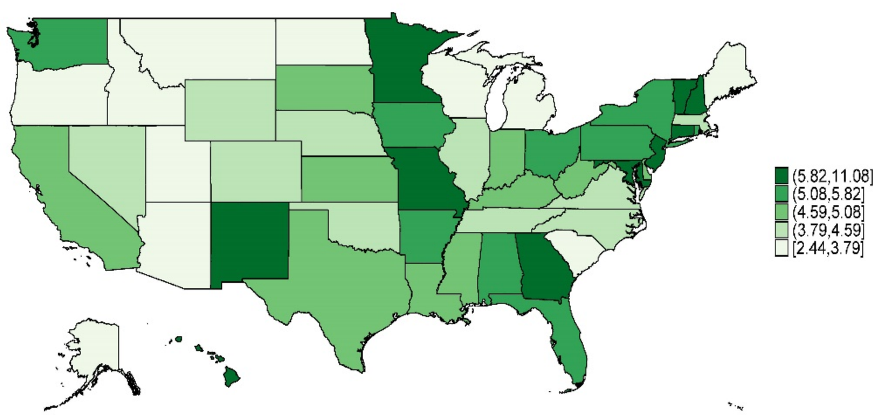
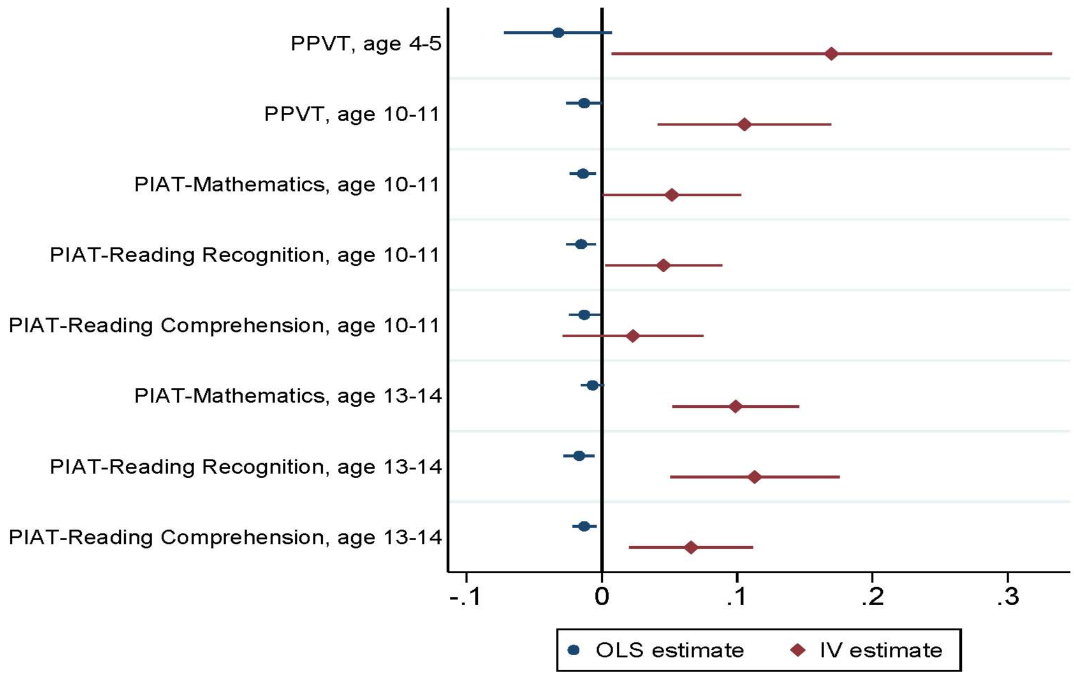


Fig.1. Changes in the years of simulated eligiblity across two March CPS birth cohorts (1976/1981 and 1996), by state.
Notes: The cross-cohort changes in the years of simulated eligibility are calculated using March CPS data from the survey years of 1980-2012.

Panel 1. Cognitive outcomes



Panel 2. Behavioral outcomes

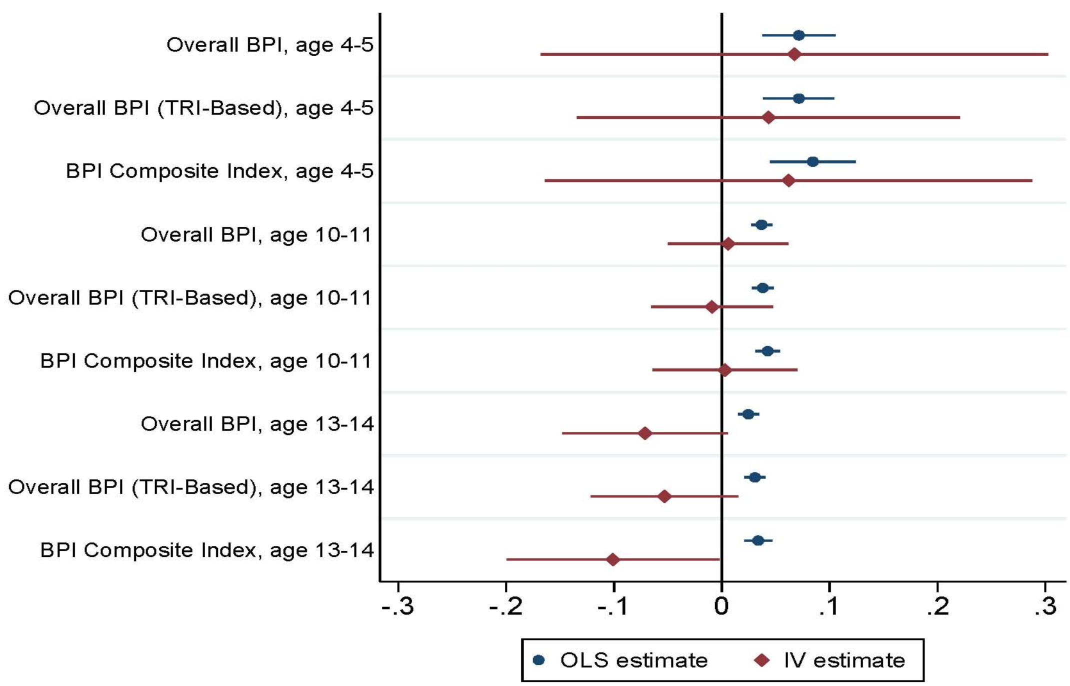
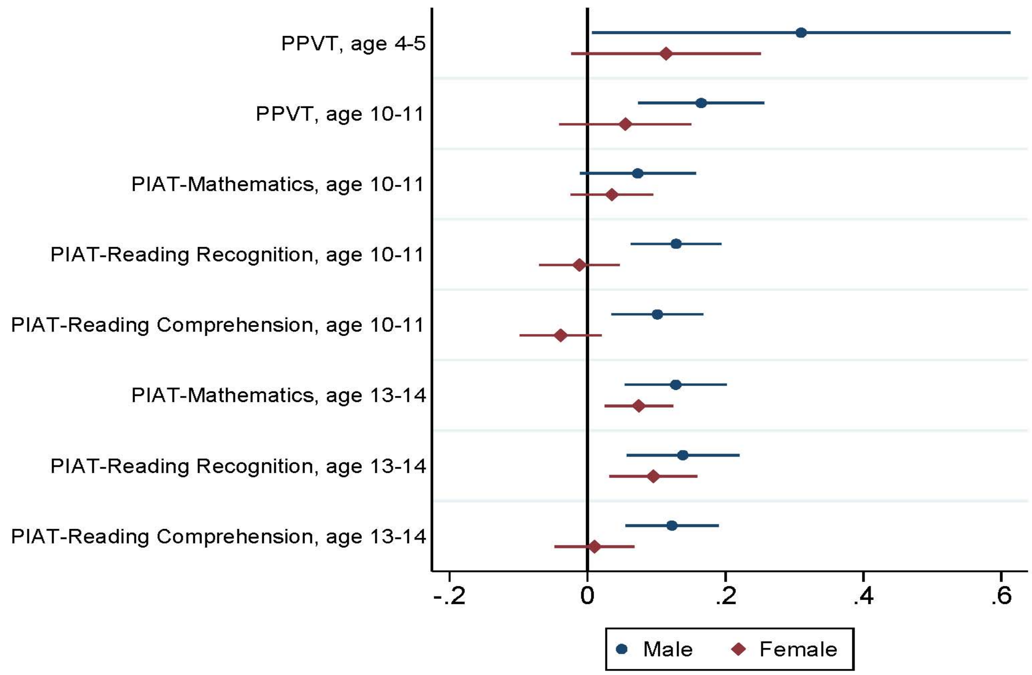


Fig.2. The impact of public insurance eligibility on child developmental outcomes.
Notes: The figure plots the OLS and IV estimates from Tables 2-3 and their respective 95% confidence intervals.

Panel 1. Cognitive outcomes



Panel 2. Behavioral outcomes

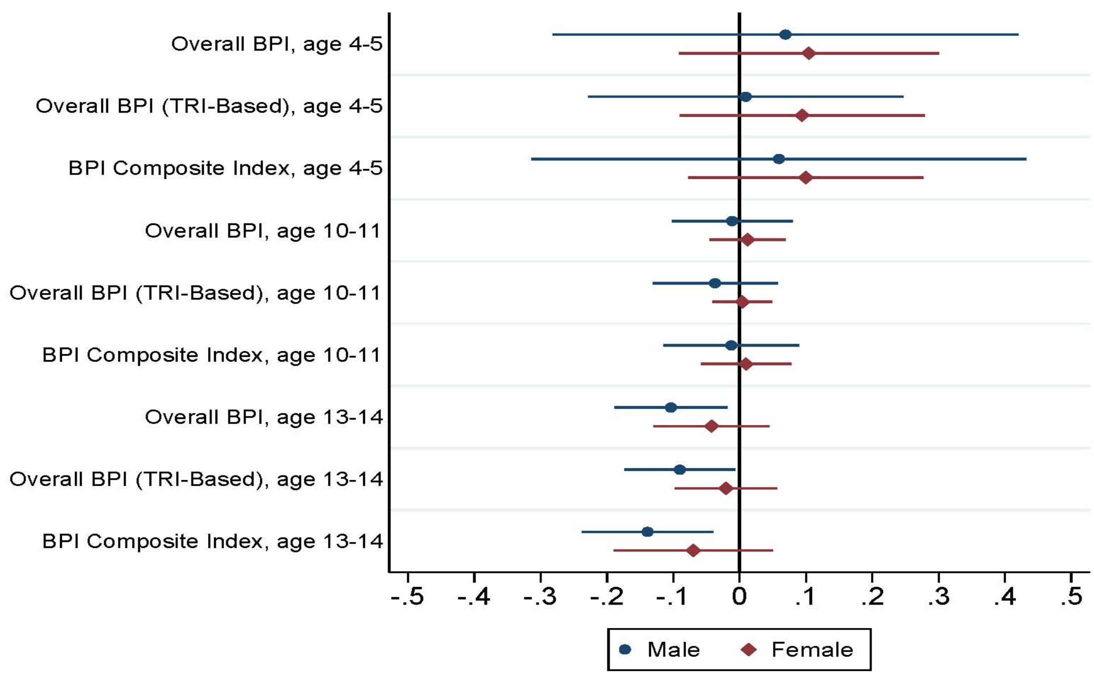
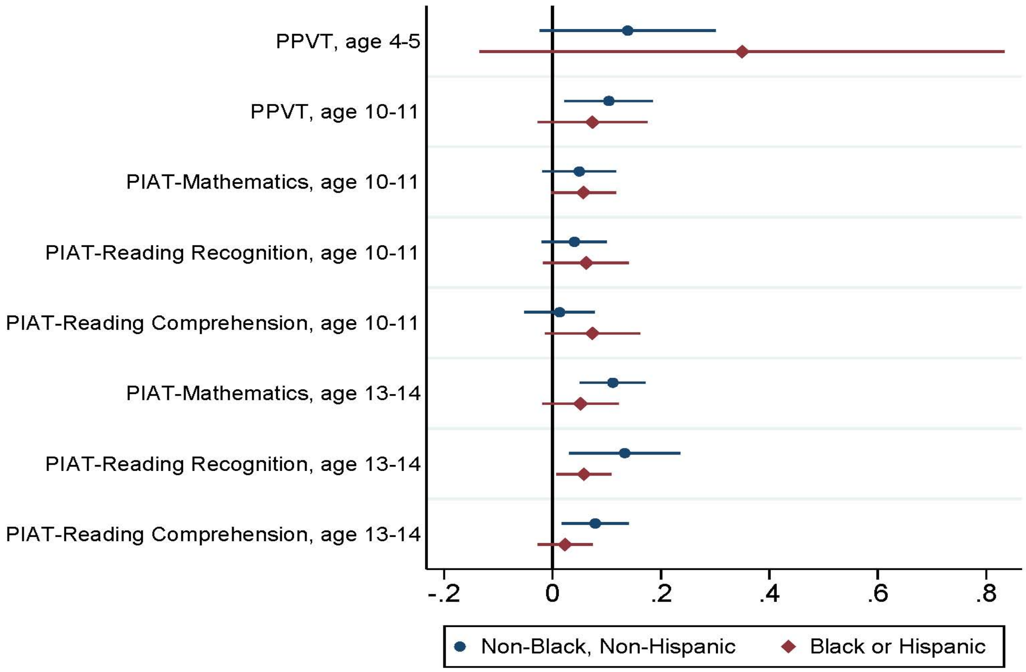


Fig.3. The impact of public insurance eligibility on child developmental outcomes by gender, IV estimates.

Notes: The figure plots the IV estimates for male children and female children from Appendix Tables A.4 and A.5 and their respective 95% confidence intervals.

Panel 1. Cognitive outcomes



Panel 2. Behavioral outcomes

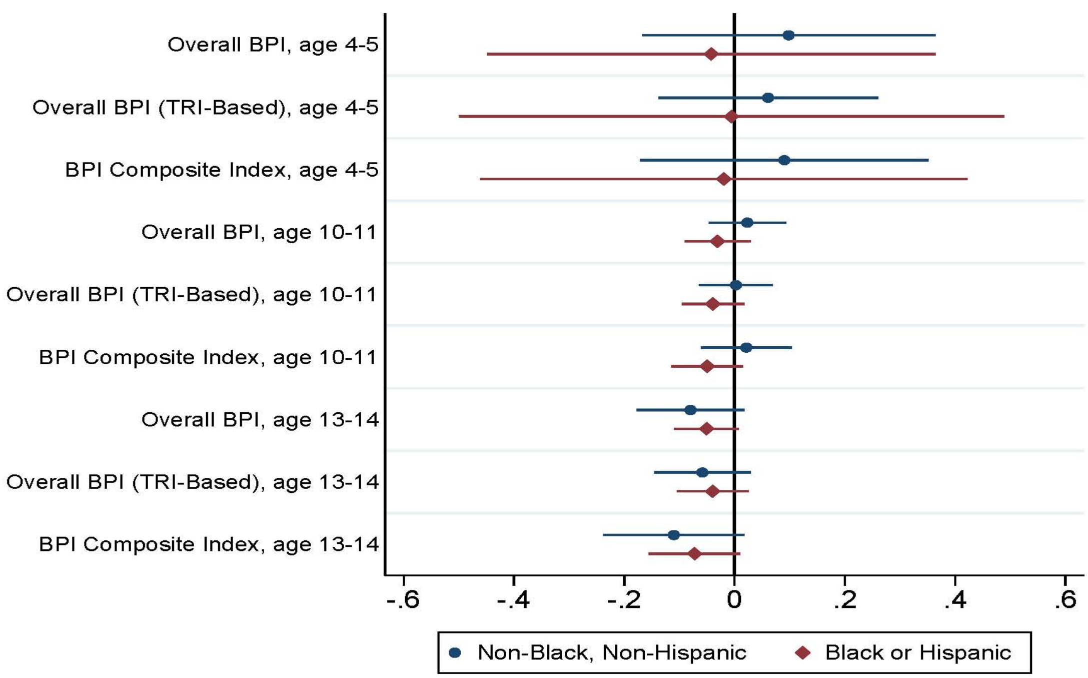


Fig.4. The impact of public insurance eligibility on child developmental outcomes, by race/ethnicity.
Notes: The figure plots the IV estimates for non-Black, non-Hispanic children and children who are Black or Hispanic from Appendix Tables A.6 and A.7 and their respective 95% confidence intervals.

Table 1. Summary statistics.

	Age 4-5		Age 10-11		Age 13-14	
	Mean	SD	Mean	SD	Mean	SD
Panel A. Outcome Variables						
PPVT	-0.37	1.30	-0.07	1.30		
PIAT Mathematics			0.33	0.98	0.19	0.99
PIAT Reading Recognition			0.42	1.01	0.40	1.07
PIAT Reading Comprehension			0.12	0.91	0.08	0.89
Overall BPI	0.17	0.98	0.30	0.99	0.36	0.99
BPI Composite Index	-0.04	1.14	-0.07	1.14	-0.03	1.36
Overall BPI (TRI-based)	0.19	0.97	0.19	0.96	0.26	1.08
Panel B. Child Characteristics						
Years of Eligibility	1.10	1.90	2.91	4.10	3.86	5.13
Years of Simulated Eligibility (1991 CPS)	1.91	0.77	3.94	1.71	4.89	2.15
Years of Simulated Eligibility (1981 CPS)	1.83	0.86	3.79	1.87	4.75	2.32
Years of Simulated Eligibility (2001 CPS)	1.54	0.74	3.20	1.65	3.95	2.06
Non-Black, Non-Hispanic	0.80	0.40	0.76	0.43	0.74	0.44
Black	0.12	0.33	0.17	0.37	0.18	0.38
Hispanic	0.07	0.26	0.08	0.27	0.08	0.27
Female	0.48	0.50	0.49	0.50	0.50	0.50
Year of Birth	1988.92	5.72	1987.65	6.08	1987.22	6.16
Birth Order	2.02	1.08	1.95	1.08	1.93	1.08
Panel C. Mother's Characteristics						
Mother's Year of Birth	1960.60	2.21	1960.46	2.29	1960.38	2.30
Unmarried	0.11	0.32	0.10	0.30	0.09	0.29
Always Married	0.77	0.42	0.62	0.49	0.58	0.49
Completed High School or Equivalent	0.43	0.50	0.43	0.50	0.42	0.49
Completed Some College	0.22	0.41	0.24	0.42	0.24	0.43
Completed Four Years College	0.25	0.44	0.23	0.42	0.23	0.42
AFQT Percentile	51.79	28.11	48.34	28.80	47.55	29.00
Children in Household	2.24	1.05	2.41	1.02	2.46	1.03
Max N	3,074		5,562		4,218	

Source: NLSY79 (1979-2010 waves) and NLSCYA (1986-2012 waves for the samples of age 4-5, 10-11, and 13-14). All the means and standard deviations are calculated with NLSCYA custom survey weights. The test scores and overall BPI scores have been standardized using national norming samples. For each analytic sample, the maximum sample size is determined by the dependent variable with the most non-missing values.

Table 2. The impact of public insurance eligibility on cognitive outcomes.

	OLS	RF	FS	Effective FS F-Statistic	IV	N
Panel A: Age 4-5						
PPVT	-0.032	0.168**	0.990***	19.453	0.170**	3,074
	(0.020)	(0.081)	(0.224)		(0.083)	
Panel B: Age 10-11						
PPVT	-0.013*	0.102***	0.971***	41.775	0.105***	5,562
	(0.007)	(0.027)	(0.150)		(0.033)	
PIAT Mathematics	-0.014***	0.050**	0.971***	41.807	0.052**	5,536
	(0.005)	(0.024)	(0.150)		(0.026)	
PIAT Reading Recognition	-0.015***	0.044*	0.965***	41.284	0.046**	5,526
	(0.005)	(0.023)	(0.150)		(0.022)	
PIAT Reading Comprehension	-0.013**	0.022	0.967***	41.316	0.023	5,475
	(0.006)	(0.025)	(0.150)		(0.027)	
Panel C: Age 13-14						
PIAT Mathematics	-0.007	0.086***	0.873***	30.280	0.099***	4,218
	(0.004)	(0.017)	(0.159)		(0.024)	
PIAT Reading Recognition	-0.017***	0.098***	0.868***	29.532	0.113***	4,196
	(0.006)	(0.020)	(0.160)		(0.032)	
PIAT Reading Comprehension	-0.013***	0.058***	0.872***	32.664	0.066***	4,165
	(0.004)	(0.018)	(0.153)		(0.023)	

Notes: The OLS column reports coefficient estimates on total actual eligibility from equation (1) in the text. The RF column reports coefficient estimates on total simulated eligibility from a specification analogous to equation (2), where the dependent variable is a test score. The FS column reports estimates of β_1 from equation (2). The IV column reports estimates of α_1 from equation (3). Critical values of the effective FS F-statistic when there is one endogenous variable and one instrument: 37.418 for 5% of the worst case bias, 23.109 for 10% of the worst case bias, 15.062 for 20% of the worst case bias, and 12.039 for 30% of the worst case bias. All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators of the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table 3. The impact of public insurance eligibility on behavioral outcomes.

	Age 4-5		Age 10-11		Age 13-14	
	OLS	IV	OLS	IV	OLS	IV
Overall BPI	0.071***	0.067	0.037***	0.006	0.025***	-0.071*
	(0.017)	(0.120)	(0.005)	(0.028)	(0.005)	(0.039)
Antisocial Behavior	0.038***	0.130	0.031***	-0.014	0.017***	-0.035
	(0.012)	(0.096)	(0.005)	(0.023)	(0.004)	(0.026)
Anxiousness/Depression	0.042**	0.029	0.029***	-0.007	0.019***	-0.058*
	(0.016)	(0.076)	(0.004)	(0.027)	(0.004)	(0.035)
Headstrongness	0.051***	0.045	0.028***	0.015	0.014***	-0.048
	(0.012)	(0.082)	(0.004)	(0.027)	(0.004)	(0.032)
Hyperactivity	0.059***	0.020	0.023***	-0.013	0.017***	-0.063*
	(0.014)	(0.083)	(0.005)	(0.023)	(0.004)	(0.033)
Immature Dependency	0.042***	-0.041	0.021***	0.008		
	(0.014)	(0.071)	(0.005)	(0.029)		
Peer Conflict/Social Withdrawal	0.052***	0.042	0.022***	0.005	0.017***	-0.051**
	(0.016)	(0.084)	(0.005)	(0.019)	(0.004)	(0.022)
BPI Composite Index	0.084***	0.062	0.043***	0.003	0.034***	-0.101**
	(0.020)	(0.115)	(0.006)	(0.034)	(0.007)	(0.050)
Overall BPI (TRI-based)	0.071***	0.043	0.038***	-0.009	0.031***	-0.053
	(0.016)	(0.090)	(0.005)	(0.029)	(0.005)	(0.035)
Internalizing Behavior (TRI-based)	0.058***	0.059	0.035***	0.002	0.030***	-0.059*
	(0.018)	(0.082)	(0.005)	(0.029)	(0.005)	(0.036)
Externalizing Behavior (TRI-based)	0.073***	0.074	0.037***	0.001	0.028***	-0.055
	(0.014)	(0.094)	(0.005)	(0.025)	(0.005)	(0.035)
Max N	3,074	3,074	5,562	5,562	4,218	4,218

Notes: The OLS columns report coefficient estimates on total actual eligibility from equation (1) in the text. The IV columns report estimates of α_1 from equation (3). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table 4. The impact of public insurance eligibility on cognitive and behavioral outcomes by age at eligibility, IV estimates.

	PIAT Mathematics	PIAT Reading Recognition	PIAT Reading Comprehension	Overall BPI	BPI Composite Index	Overall BPI (TRI-based)
Panel A. Single regression						
Eligibility at ages 0-4	0.186***	0.172**	0.198***	-0.095	-0.142*	-0.039
	(0.068)	(0.074)	(0.054)	(0.061)	(0.080)	(0.053)
Eligibility at ages 5-9	0.046	0.083*	0.076	0.059	0.044	0.044
	(0.058)	(0.049)	(0.060)	(0.076)	(0.105)	(0.076)
Eligibility at ages 10-14	-0.024	-0.044	-0.047	-0.081	-0.060	-0.062
	(0.063)	(0.049)	(0.052)	(0.091)	(0.103)	(0.084)
N	3,402	3,385	3,361	3,264	3,402	3,199
Panel B. Single regression including prenatal eligibility						
Prenatal eligibility	0.205	-0.051	0.594***	0.003	-0.211	-0.191
	(0.231)	(0.278)	(0.229)	(0.290)	(0.301)	(0.194)
Eligibility at ages 1-4	0.177**	0.203**	0.124	-0.071	-0.066	0.050
	(0.083)	(0.094)	(0.091)	(0.114)	(0.135)	(0.075)
Eligibility at ages 5-9	0.093	0.159***	0.058	0.044	0.040	0.020
	(0.061)	(0.055)	(0.078)	(0.092)	(0.119)	(0.085)
Eligibility at ages 10-14	0.034	-0.013	-0.026	-0.023	0.012	0.002
	(0.077)	(0.079)	(0.066)	(0.116)	(0.129)	(0.105)
N	2,435	2,426	2,410	2,354	2,435	2,297

Notes: The table reports IV estimates when years of actual eligibility over different age ranges are instrumented by the corresponding measures of simulated eligibility. The models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table 5. Child health limitations and conditions at ages 13-14, IV estimates.

	Mean	IV	N
1. Health limitation that affects school attendance	0.017	-0.005*	4,192
		(0.003)	
2. Health limitation that affects school work	0.033	-0.001	4,192
		(0.005)	
3. Health limitation that affects play activities	0.031	-0.010	4,192
		(0.008)	
4. Health condition that requires a doctor	0.072	-0.020***	4,192
		(0.008)	
5. Health condition that requires use of medicine	0.084	-0.015	4,192
		(0.010)	
6. Health condition that requires special equipment	0.012	-0.008**	4,192
		(0.004)	
7. Any of the health limitations/conditions 1-6 above	0.179	-0.018	4,192
		(0.013)	
8. Any of the health limitations 1-3 above	0.066	-0.009	4,192
		(0.009)	
9. Any of the health conditions 4-6 above	0.134	-0.024**	4,192
		(0.011)	

Notes: This table reports IV estimates of α_1 from equation (3). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table 6. Public insurance eligibility and cognitive outcomes, IV estimates from robustness checks.

	Robustness Checks				
	(1)	(2)	(3)	(4)	(5)
Panel A: Age 4-5					
PPVT	0.173**	0.263*	0.100	0.184**	0.166*
	(0.084)	(0.138)	(0.141)	(0.080)	(0.096)
Panel B: Age 10-11					
PPVT	0.094***	0.084**	0.088	0.072**	0.118***
	(0.028)	(0.036)	(0.060)	(0.032)	(0.038)
PIAT Mathematics	0.053**	0.026	0.091*	0.033	0.060**
	(0.024)	(0.028)	(0.048)	(0.029)	(0.030)
PIAT Reading Recognition	0.039*	0.018	0.079*	0.035	0.050**
	(0.021)	(0.030)	(0.047)	(0.026)	(0.024)
PIAT Reading Comprehension	0.033	0.039	0.030	0.001	0.029
	(0.027)	(0.027)	(0.035)	(0.024)	(0.029)
Panel C: Age 13-14					
PIAT Mathematics	0.080***	0.072***	0.084*	0.087***	0.108***
	(0.022)	(0.026)	(0.048)	(0.024)	(0.027)
PIAT Reading Recognition	0.106***	0.074**	0.126**	0.098***	0.121***
	(0.031)	(0.031)	(0.055)	(0.030)	(0.035)
PIAT Reading Comprehension	0.063***	0.036	0.028	0.036*	0.069***
	(0.021)	(0.024)	(0.048)	(0.022)	(0.026)
Controls: EITC, AFDC/TANF, unemployment rates, and school spending	X				
Controls: region-specific cohort trends, region-by-age FE		X			
Controls: state-specific cohort trends, state-by-age FE			X		
Sample: non-movers				X	
Simulated eligibility based on March 1981 CPS					X

Notes: This table reports IV estimates from equation (3) or (4). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. When indicated, models use different sets of additional controls, an alternative sample restriction, or simulated eligibility constructed from another survey year. In column (1), the school spending variable is only used for the outcomes measured at ages 10-11 or 13-14. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

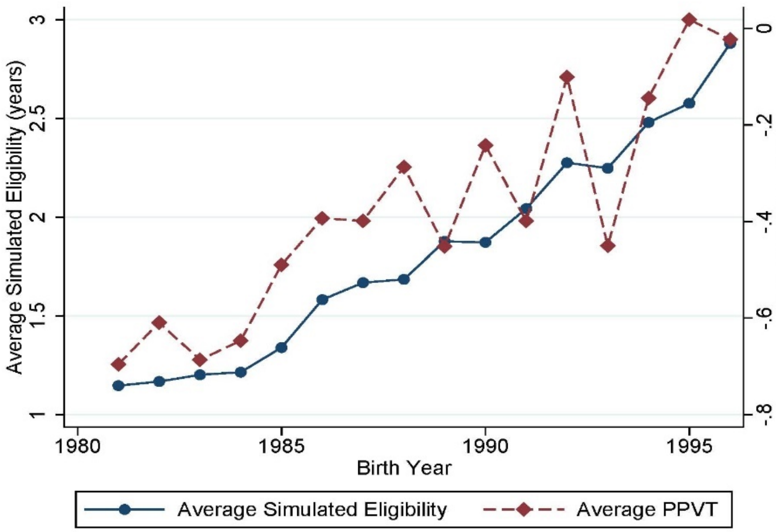
Table 7. Public insurance eligibility and behavioral outcomes at ages 13-14, IV estimates from robustness checks.

	Robustness Checks				
	(1)	(2)	(3)	(4)	(5)
Overall BPI	-0.034	0.012	-0.088	-0.048	-0.079*
	(0.030)	(0.047)	(0.078)	(0.036)	(0.042)
Antisocial Behavior	-0.011	0.017	-0.097*	-0.022	-0.039
	(0.023)	(0.036)	(0.053)	(0.024)	(0.028)
Anxiousness/Depression	-0.025	0.016	-0.062	-0.038	-0.065*
	(0.025)	(0.033)	(0.063)	(0.026)	(0.037)
Headstrongness	-0.016	0.027	-0.052	-0.032	-0.054
	(0.026)	(0.042)	(0.078)	(0.031)	(0.035)
Hyperactivity	-0.041	-0.018	-0.084	-0.059**	-0.070**
	(0.028)	(0.036)	(0.063)	(0.029)	(0.035)
Peer Conflict/Social Withdrawal	-0.028	0.026	0.022	-0.028	-0.055**
	(0.020)	(0.028)	(0.049)	(0.024)	(0.026)
BPI Composite Index	-0.050	0.028	-0.088	-0.071*	-0.113**
	(0.040)	(0.054)	(0.093)	(0.043)	(0.055)
Overall BPI (TRI-based)	-0.023	0.046	-0.047	-0.033	-0.059
	(0.029)	(0.045)	(0.082)	(0.032)	(0.037)
Internalizing Behavior (TRI-based)	-0.025	0.023	-0.020	-0.029	-0.065*
	(0.028)	(0.041)	(0.074)	(0.031)	(0.040)
Externalizing Behavior (TRI-based)	-0.024	0.033	-0.069	-0.040	-0.061
	(0.030)	(0.044)	(0.078)	(0.032)	(0.038)
Controls: EITC, AFDC/TANF, unemployment rates, and school spending	X				
Controls: region-specific cohort trends, region-by-age FE		X			
Controls: state-specific cohort trends, state-by-age FE			X		
Sample: non-movers				X	
Simulated eligibility based on March 1981 CPS					X

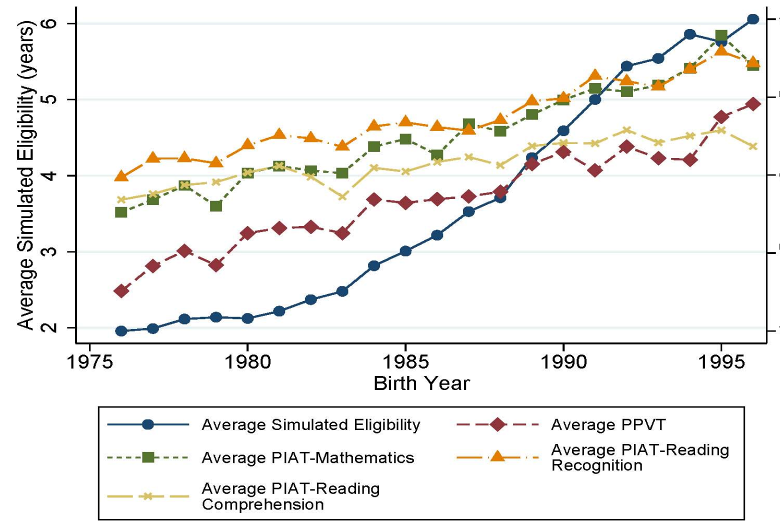
Notes: This table reports IV estimates from equation (3) or (4). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. When indicated, models use different sets of additional controls, an alternative sample restriction, or simulated eligibility constructed from another survey year. In column (1), the school spending variable is only used for the outcomes measured at ages 10-11 or 13-14. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Supplementary Materials: Appendix Figures and Tables

Panel 1. Age 4-5 sample



Panel 2. Age 10-11 sample



Panel 3. Age 13-14 sample

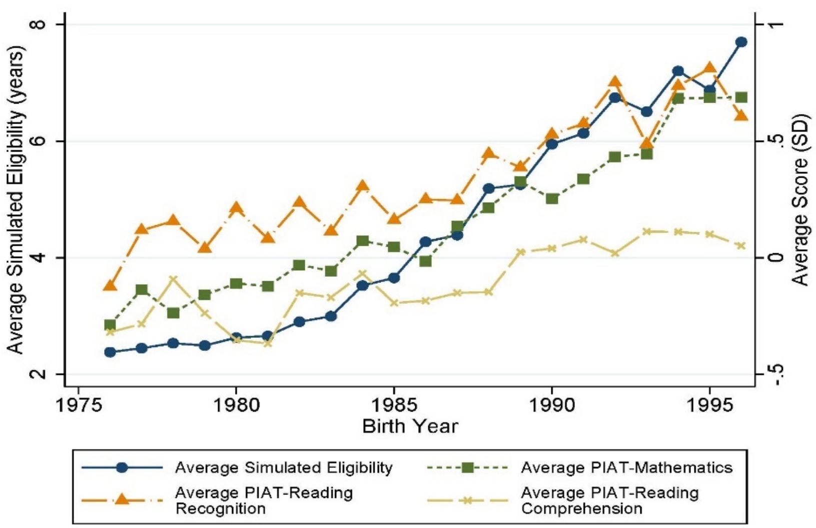
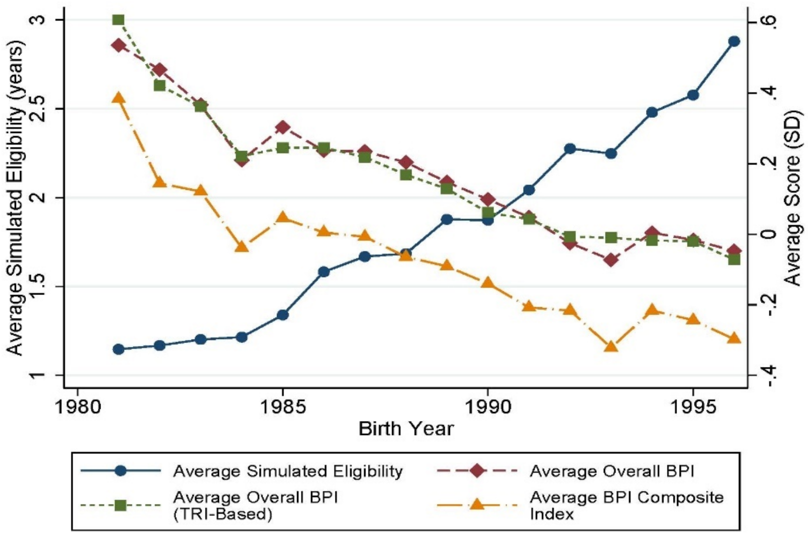


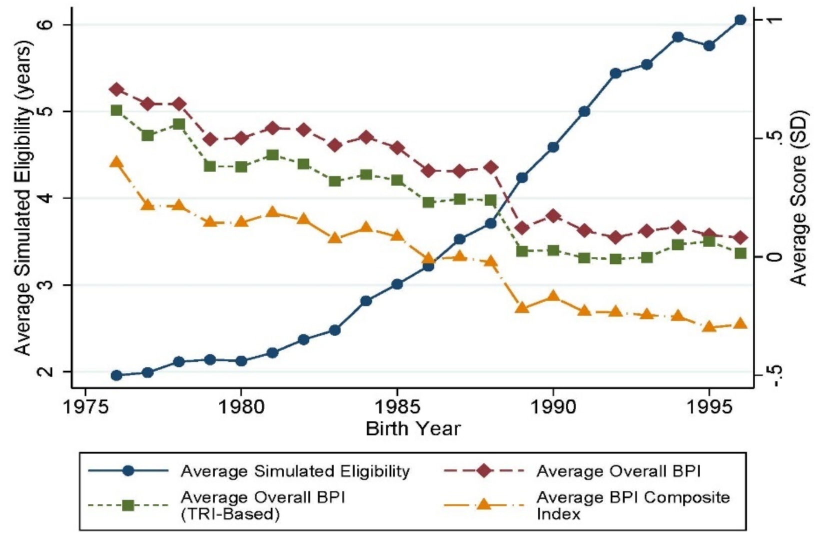
Fig. A1. Average years of simulated eligibility and test scores, by birth year.

Notes: All the means are calculated with NLSCYA custom survey weights.

Panel 1. Age 4-5 sample



Panel 2. Age 10-11 sample



Panel 3. Age 13-14 sample

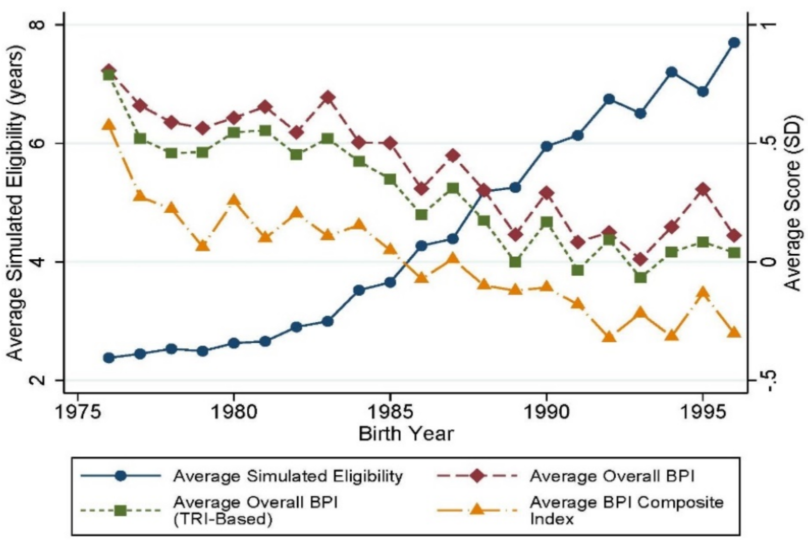
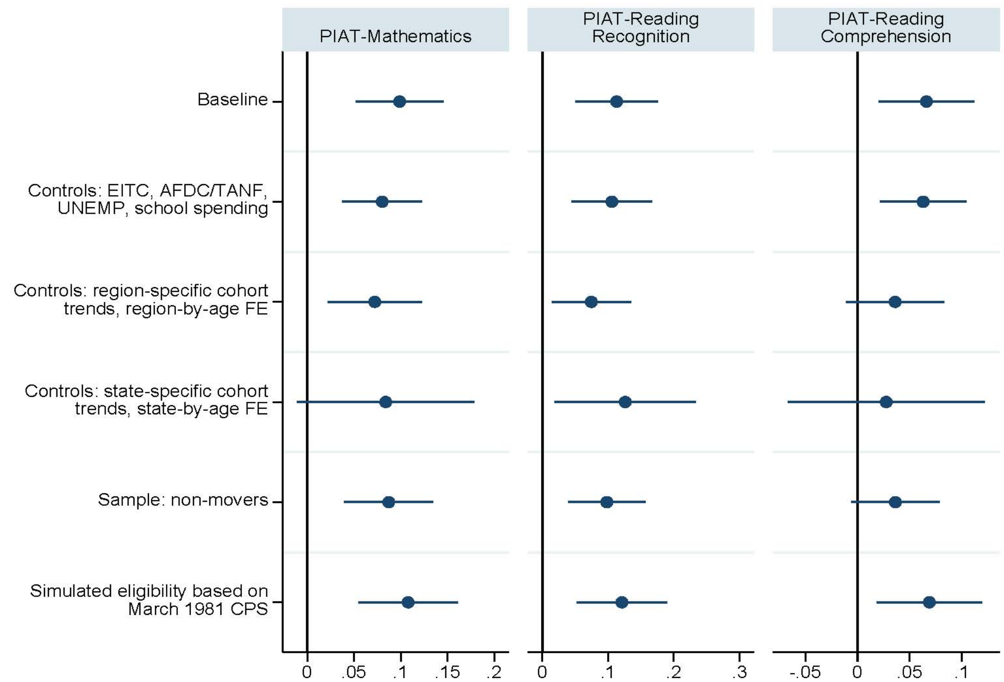


Fig. A2. Average years of simulated eligibility and aggregate BPI measures, by birth year.

Notes: All the means are calculated with NLSCYA custom survey weights.

Panel 1. Cognitive outcomes



Panel 2. Behavioral outcomes

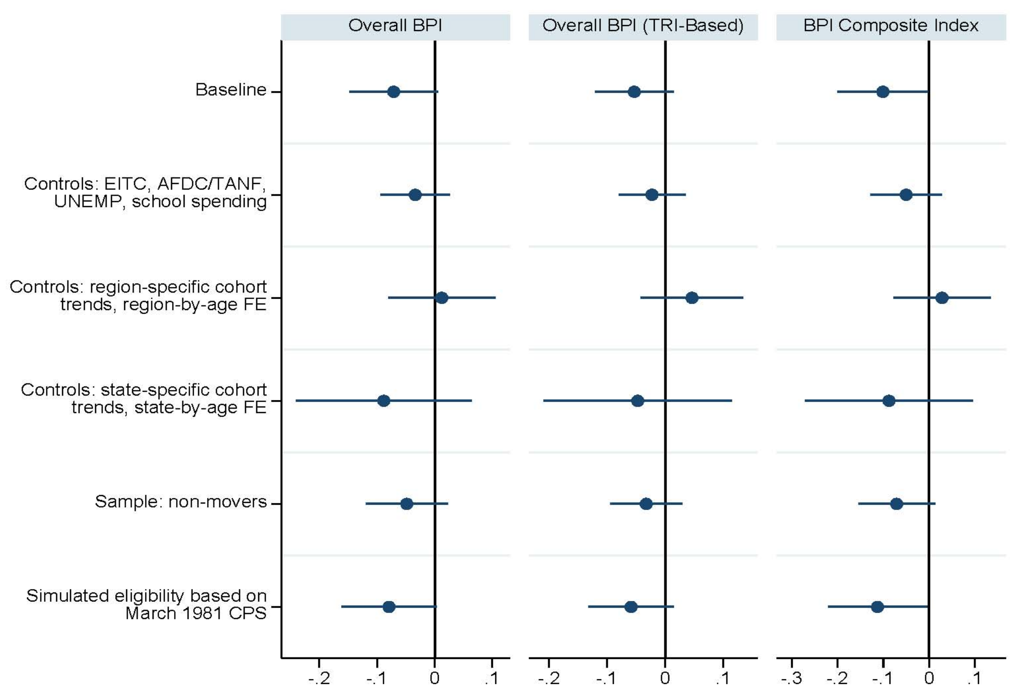


Fig. A3. Public insurance eligibility and child developmental outcomes at ages 13-14, robustness checks. Notes: The figure plots the IV estimates from Tables 6 and 7 and their respective 95% confidence intervals.

Table A1. Balance tests of the covariates

	Ages 4-5				Ages 10-11				Ages 13-14			
	(1)		(2)		(3)		(4)		(5)		(6)	
	Tercile 1	Tercile 2	Tercile 1	Tercile 2	Tercile 1	Tercile 2	Tercile 1	Tercile 2	Tercile 1	Tercile 2	Tercile 1	Tercile 2
1. Child Level Controls												
Black	0.043*	0.036**	0.037	0.035*	0.056	0.023	0.045	0.016	0.012	0.026	-0.006	0.026
	(0.025)	(0.017)	(0.025)	(0.018)	(0.037)	(0.022)	(0.039)	(0.023)	(0.035)	(0.020)	(0.042)	(0.022)
Hispanic	0.004	-0.013	0.005	-0.012	0.007	0.004	0.009	0.002	-0.015	-0.013	-0.003	-0.014
	(0.019)	(0.010)	(0.021)	(0.010)	(0.017)	(0.010)	(0.017)	(0.011)	(0.024)	(0.015)	(0.022)	(0.016)
Female	0.019	-0.016	-0.009	-0.026	0.046	0.040	0.036	0.033	0.019	-0.027	0.004	-0.032
	(0.068)	(0.047)	(0.078)	(0.055)	(0.052)	(0.040)	(0.058)	(0.043)	(0.065)	(0.043)	(0.077)	(0.046)
Birth Order	0.065	-0.012	-0.003	-0.025	0.138	0.145	0.160	0.143*	-0.062	-0.009	-0.030	-0.009
	(0.096)	(0.058)	(0.098)	(0.064)	(0.103)	(0.087)	(0.116)	(0.084)	(0.118)	(0.089)	(0.117)	(0.096)
2. Mother Level Controls												
Year of Birth	0.506**	0.281	0.622**	0.373*	0.203	0.116	0.175	0.062	-0.153	-0.025	-0.347	-0.069
	(0.220)	(0.175)	(0.251)	(0.198)	(0.210)	(0.170)	(0.249)	(0.181)	(0.296)	(0.223)	(0.339)	(0.239)
Unmarried	0.033	0.005	0.001	-0.013	0.015	-0.014	0.025	-0.006	-0.014	0.004	-0.006	0.013
	(0.029)	(0.018)	(0.033)	(0.020)	(0.026)	(0.017)	(0.029)	(0.018)	(0.037)	(0.020)	(0.038)	(0.021)
Always Married	-0.060**	-0.005	-0.045	-0.001	-0.078**	-0.030	-0.098**	-0.035	0.031	-0.031	-0.037	-0.054
	(0.028)	(0.021)	(0.033)	(0.024)	(0.035)	(0.025)	(0.036)	(0.026)	(0.062)	(0.039)	(0.072)	(0.039)
Completed High School or Equivalent	0.009	-0.013	-0.038	-0.034	0.014	-0.0002	-0.017	0.0003	0.023	0.008	-0.020	-0.001
	(0.065)	(0.042)	(0.071)	(0.043)	(0.056)	(0.040)	(0.058)	(0.042)	(0.077)	(0.056)	(0.077)	(0.060)
Completed Some College	-0.064	-0.020	-0.046	-0.019	-0.021	-0.023	-0.022	-0.030	-0.029	-0.045	-0.009	-0.043
	(0.043)	(0.023)	(0.048)	(0.023)	(0.050)	(0.039)	(0.051)	(0.043)	(0.072)	(0.043)	(0.073)	(0.044)
Completed Four Years College	0.019	0.020	0.049	0.035	0.009	0.026	0.035	0.031	0.032	0.034	0.057	0.044
	(0.044)	(0.034)	(0.044)	(0.035)	(0.057)	(0.040)	(0.056)	(0.042)	(0.051)	(0.045)	(0.055)	(0.047)
AFQT Percentile	-2.518	-1.591	-2.542	-1.576	-5.269	0.084	-4.694	-0.277	6.412	4.419*	6.682*	3.687
	(2.574)	(2.397)	(2.932)	(2.621)	(3.205)	(2.378)	(3.427)	(2.529)	(4.155)	(2.247)	(3.755)	(2.362)
Children in Household	0.046	0.008	-0.004	-0.007	-0.071	0.027	-0.097	0.005	-0.055	-0.017	-0.090	-0.016
	(0.091)	(0.057)	(0.095)	(0.066)	(0.118)	(0.094)	(0.131)	(0.094)	(0.107)	(0.095)	(0.111)	(0.100)

Notes: This table reports coefficient estimates for the effect of two dummy variables on total simulated eligibility (one for the first tercile of the total simulated eligibility distribution and the other for the second tercile) from a linear regression, where a child- or mother-level control is the dependent variable. For columns (1), (3), and (5), the other independent variables are cohort FE, state FE, age FE, region-specific cohort trends, and region-by-age FE. For columns (2), (4), and (6), the other independent variables are cohort FE, state FE, age FE, state-specific cohort trends, and state-by-age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2. The impact of public insurance eligibility on behavioral outcomes, reduced form and first stage results.

	Age 4-5		Age 10-11		Age 13-14	
	RF	FS	RF	FS	RF	FS
Overall BPI	0.064	0.944***	0.006	0.983***	-0.063*	0.889***
	(0.105)	(0.221)	(0.029)	(0.155)	(0.032)	(0.160)
Antisocial Behavior	0.123*	0.946***	-0.014	0.984***	-0.031	0.877***
	(0.071)	(0.222)	(0.022)	(0.153)	(0.022)	(0.162)
Anxiousness/Depression	0.028	0.954***	-0.007	0.975***	-0.051*	0.872***
	(0.070)	(0.220)	(0.027)	(0.154)	(0.027)	(0.161)
Headstrongness	0.043	0.950***	0.015	0.975***	-0.042	0.873***
	(0.073)	(0.218)	(0.027)	(0.153)	(0.026)	(0.161)
Hyperactivity	0.019	0.949***	-0.013	0.969***	-0.055**	0.876***
	(0.079)	(0.221)	(0.022)	(0.154)	(0.025)	(0.161)
Immature Dependency	-0.039	0.955***	0.007	0.965***		
	(0.075)	(0.218)	(0.029)	(0.157)		
Peer Conflict/Social Withdrawal	0.040	0.953***	0.005	0.979***	-0.045**	0.877***
	(0.077)	(0.217)	(0.019)	(0.150)	(0.018)	(0.161)
BPI Composite Index	0.061	0.990***	0.003	0.971***	-0.088**	0.873***
	(0.107)	(0.224)	(0.034)	(0.150)	(0.038)	(0.159)
Overall BPI (TRI-based)	0.041	0.938***	-0.009	0.990***	-0.047	0.892***
	(0.080)	(0.227)	(0.028)	(0.149)	(0.029)	(0.160)
Internalizing Behavior (TRI-based)	0.056	0.952***	0.002	0.968***	-0.052*	0.873***
	(0.072)	(0.219)	(0.029)	(0.154)	(0.029)	(0.160)
Externalizing Behavior (TRI-based)	0.070	0.948***	0.001	0.975***	-0.049	0.897***
	(0.080)	(0.218)	(0.025)	(0.155)	(0.029)	(0.158)
Max N	3,074	3,074	5,562	5,562	4,218	4,218

Notes: The RF column reports coefficient estimates on total simulated eligibility from a specification analogous to equation (2), where the dependent variable is a BPI measure. The FS column reports estimates of β_1 from equation (2). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A3. Falsification tests using future eligiblity

Outcome at ages 4-5	PPVT	Overall BPI	BPI Composite Index	Overall BPI (TRI-Based)
Model 1				
Eligibility at ages 0-4/5	0.186**	0.050	0.024	0.017
	(0.076)	(0.128)	(0.132)	(0.101)
Eligibility at ages 6-11	-0.033	-0.005	0.030	0.003
	(0.061)	(0.072)	(0.084)	(0.074)
Model 2				
Eligibility at ages 10-11	-0.038	0.075	0.130	0.056
	(0.166)	(0.105)	(0.120)	(0.110)

Outcome at ages 10-11	PPVT	PIAT Mathematics	PIAT Reading Recognition	PIAT Reading Comprehension	Overall BPI	BPI Composite Index	Overall BPI (TRI-Based)
Model 1							
Eligibility at ages 0-10/11	0.133***	0.068***	0.070**	0.036	-0.011	-0.021	-0.016
	(0.032)	(0.024)	(0.030)	(0.034)	(0.028)	(0.033)	(0.025)
Eligibility at ages 12-14	0.018	0.001	-0.128	0.010	0.078	0.043	0.020
	(0.108)	(0.085)	(0.079)	(0.079)	(0.083)	(0.083)	(0.076)
Model 2							
Eligibility at ages 13-14	0.153	0.079	-0.074	0.106	0.025	-0.066	-0.046
	(0.142)	(0.113)	(0.111)	(0.094)	(0.120)	(0.125)	(0.101)

Notes: The table reports IV estimates when years of actual eligibility over one or two age ranges are instrumented by the corresponding measures of simulated eligibility. The models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A4. The impact of public insurance eligibility on cognitive outcomes by gender.

	Male			Female		
	OLS	IV	N	OLS	IV	N
Panel A: Age 4-5						
PPVT	-0.042	0.310**	1,599	-0.019	0.114	1,475
	(0.028)	(0.155)		(0.021)	(0.070)	
Panel B: Age 10-11						
PPVT	-0.010	0.165***	2,797	-0.017**	0.055	2,765
	(0.010)	(0.047)		(0.008)	(0.049)	
PIAT Mathematics	-0.017**	0.073*	2,781	-0.010	0.035	2,755
	(0.007)	(0.043)		(0.006)	(0.031)	
PIAT Reading Recognition	-0.010	0.128***	2,775	-0.019***	-0.012	2,751
	(0.008)	(0.033)		(0.005)	(0.030)	
PIAT Reading Comprehension	-0.008	0.101***	2,743	-0.018**	-0.039	2,732
	(0.007)	(0.034)		(0.007)	(0.030)	
Panel C: Age 13-14						
PIAT Mathematics	-0.004	0.128***	2,093	-0.009	0.074***	2,125
	(0.005)	(0.038)		(0.006)	(0.025)	
PIAT Reading Recognition	-0.013*	0.138***	2,084	-0.021***	0.095***	2,112
	(0.007)	(0.042)		(0.007)	(0.033)	
PIAT Reading Comprehension	-0.009*	0.122***	2,064	-0.017***	0.010	2,101
	(0.005)	(0.034)		(0.006)	(0.029)	

Notes: The OLS columns report coefficient estimates on total actual eligibility from equation (1) when we estimate it separately for male and female children. The IV columns report estimates of α_1 from equation (3) when we estimate it separately for male and female children. All the models include the demographic controls of children (race/ethnicity, birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A5. The impact of public insurance eligibility on behavioral outcomes by gender, IV estimates.

	Male			Female		
	Age 4-5	Age 10-11	Age 13-14	Age 4-5	Age 10-11	Age 13-14
Overall BPI	0.069	-0.011	-0.104**	0.104	0.012	-0.042
	(0.179)	(0.046)	(0.043)	(0.100)	(0.029)	(0.044)
Antisocial Behavior	0.183	-0.098*	-0.070**	0.153	0.037	0.001
	(0.149)	(0.055)	(0.035)	(0.107)	(0.024)	(0.029)
Anxiousness/Depression	0.010	-0.005	-0.077**	0.047	-0.006	-0.038
	(0.105)	(0.027)	(0.039)	(0.083)	(0.038)	(0.043)
Headstrongness	0.058	0.008	-0.082**	0.053	0.017	-0.018
	(0.140)	(0.048)	(0.040)	(0.073)	(0.024)	(0.034)
Hyperactivity	0.053	-0.022	-0.090**	0.031	-0.005	-0.037
	(0.105)	(0.039)	(0.040)	(0.086)	(0.022)	(0.042)
Immature Dependency	-0.076	0.019		-0.011	-0.006	
	(0.112)	(0.039)		(0.060)	(0.034)	
Peer Conflict/Social Withdrawal	0.039	0.004	-0.047*	0.073	0.005	-0.058*
	(0.129)	(0.028)	(0.026)	(0.066)	(0.020)	(0.031)
BPI Composite Index	0.059	-0.013	-0.139***	0.100	0.010	-0.070
	(0.190)	(0.052)	(0.050)	(0.090)	(0.035)	(0.061)
Overall BPI (TRI-based)	0.009	-0.037	-0.090**	0.094	0.004	-0.020
	(0.121)	(0.048)	(0.043)	(0.094)	(0.023)	(0.039)
Internalizing Behavior (TRI-based)	0.040	-0.001	-0.064	0.093	-0.00005	-0.063
	(0.124)	(0.034)	(0.041)	(0.085)	(0.038)	(0.047)
Externalizing Behavior (TRI-based)	0.086	-0.032	-0.099**	0.096	0.022	-0.019
	(0.145)	(0.045)	(0.043)	(0.088)	(0.024)	(0.039)
Max N	1,599	2,797	2,093	1,475	2,765	2,125

Notes: This table reports IV estimates of α_1 from equation (3) when we estimate it separately for male and female children. All the models include the demographic controls of children (race/ethnicity, birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A6. The impact of public insurance eligibility on cognitive outcomes by race/ethnicity.

	Non-Black, Non-Hispanic			Black or Hispanic		
	OLS	IV	N	OLS	IV	N
Panel A: Age 4-5						
PPVT	-0.031	0.139*	1,696	-0.027*	0.350	1,378
	(0.028)	(0.083)		(0.014)	(0.247)	
Panel B: Age 10-11						
PPVT	-0.003	0.104**	2,742	-0.020**	0.073	2,820
	(0.010)	(0.042)		(0.008)	(0.052)	
PIAT Mathematics	-0.013*	0.049	2,735	-0.010*	0.057*	2,801
	(0.007)	(0.035)		(0.005)	(0.031)	
PIAT Reading Recognition	-0.014**	0.040	2,727	-0.011	0.062	2,799
	(0.007)	(0.030)		(0.008)	(0.040)	
PIAT Reading Comprehension	-0.010	0.013	2,709	-0.014**	0.073	2,766
	(0.008)	(0.033)		(0.005)	(0.045)	
Panel C: Age 13-14						
PIAT Mathematics	-0.002	0.111***	2,019	-0.014**	0.052	2,199
	(0.007)	(0.031)		(0.006)	(0.036)	
PIAT Reading Recognition	-0.009	0.133**	2,011	-0.028***	0.058**	2,185
	(0.007)	(0.052)		(0.008)	(0.026)	
PIAT Reading Comprehension	-0.007	0.079**	1,997	-0.020***	0.023	2,168
	(0.007)	(0.031)		(0.005)	(0.026)	

Notes: The OLS columns report coefficient estimates on total actual eligibility from equation (1) when we estimate it separately for non-Black, non-Hispanic children and children who are Black or Hispanic. The IV columns report estimates of α_1 from equation (3) when we estimate it separately for non-Black, non-Hispanic children and children who are Black or Hispanic. All the models include the demographic controls of children (gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A7. The impact of public insurance eligibility on behavioral outcomes by race/ethnicity, IV estimates.

	Non-Black, Non-Hispanic			Black or Hispanic		
	Age 4-5	Age 10-11	Age 13-14	Age 4-5	Age 10-11	Age 13-14
Overall BPI	0.098	0.023	-0.080	-0.042	-0.031	-0.051*
	(0.136)	(0.036)	(0.050)	(0.207)	(0.031)	(0.030)
Antisocial Behavior	0.151	0.007	-0.038	0.050	-0.074**	-0.033
	(0.101)	(0.027)	(0.032)	(0.157)	(0.030)	(0.030)
Anxiousness/Depression	0.059	-0.003	-0.065	-0.175	-0.010	-0.032
	(0.092)	(0.037)	(0.047)	(0.173)	(0.029)	(0.026)
Headstrongness	0.057	0.022	-0.053	0.085	0.001	-0.039*
	(0.089)	(0.033)	(0.041)	(0.157)	(0.028)	(0.022)
Hyperactivity	0.062	-0.002	-0.072*	-0.355	-0.045	-0.041
	(0.096)	(0.032)	(0.041)	(0.248)	(0.029)	(0.033)
Immature Dependency	-0.035	0.025		0.053	-0.038	
	(0.088)	(0.035)		(0.178)	(0.026)	
Peer Conflict/Social Withdrawal	0.048	0.007	-0.054*	0.105	-0.006	-0.039*
	(0.093)	(0.022)	(0.028)	(0.138)	(0.023)	(0.023)
BPI Composite Index	0.090	0.021	-0.110*	-0.020	-0.050	-0.073*
	(0.133)	(0.042)	(0.065)	(0.225)	(0.033)	(0.042)
Overall BPI (TRI-based)	0.061	0.002	-0.058	-0.006	-0.039	-0.040
	(0.102)	(0.034)	(0.045)	(0.252)	(0.029)	(0.033)
Internalizing Behavior (TRI-based)	0.073	0.009	-0.064	0.031	-0.015	-0.061
	(0.107)	(0.039)	(0.047)	(0.226)	(0.030)	(0.040)
Externalizing Behavior (TRI-based)	0.095	0.010	-0.062	-0.039	-0.026	-0.050
	(0.102)	(0.032)	(0.045)	(0.234)	(0.029)	(0.032)
Max N	1,696	2,742	2,019	1,378	2,820	2,199

Notes: This table reports IV estimates of α_1 from equation (3) when we estimate it seperately for non-black, non-Hispanic children and children who are black or Hispanic. All the models include the demographic controls of children (gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A8. The impact of public insurance eligibility and cognitive outcomes, White and non-White children.

	White			Non-White		
	OLS	IV	N	OLS	IV	N
Panel A: Age 4-5						
PPVT	-0.021	0.169**	1,619	-0.040**	0.327	1,416
	(0.027)	(0.086)		(0.016)	(0.235)	
Panel B: Age 10-11						
PPVT	-0.001	0.109**	2,587	-0.023***	0.048	2,893
	(0.010)	(0.045)		(0.007)	(0.045)	
PIAT Mathematics	-0.012	0.044	2,580	-0.011**	0.066**	2,874
	(0.007)	(0.035)		(0.005)	(0.030)	
PIAT Reading Recognition	-0.011	0.037	2,572	-0.012	0.065*	2,872
	(0.007)	(0.031)		(0.009)	(0.039)	
PIAT Reading Comprehension	-0.010	0.007	2,555	-0.014**	0.063	2,839
	(0.008)	(0.031)		(0.005)	(0.039)	
Panel C: Age 13-14						
PIAT Mathematics	-0.001	0.117***	1,894	-0.011**	0.061*	2,254
	(0.008)	(0.035)		(0.005)	(0.033)	
PIAT Reading Recognition	-0.006	0.161**	1,886	-0.028***	0.049**	2,240
	(0.008)	(0.068)		(0.008)	(0.023)	
PIAT Reading Comprehension	-0.009	0.082**	1,873	-0.017***	0.030	2,222
	(0.008)	(0.035)		(0.005)	(0.022)	(0.008)

Notes: The OLS columns report coefficient estimates on total actual eligibility from equation (1) when we estimate it separately for White and non-White children. The IV columns report estimates of α_1 from equation (3) when we estimate it separately for White and non-White children. All the models include the demographic controls of children (gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A9. The impact of public insurance eligibility on behavioral outcomes, White and non-White children.

	White			Non-White		
	Age 4-5	Age 10-11	Age 13-14	Age 4-5	Age 10-11	Age 13-14
Overall BPI	0.093	0.018	-0.079	-0.144	-0.032	-0.065**
	(0.139)	(0.038)	(0.053)	(0.211)	(0.030)	(0.026)
Antisocial Behavior	0.150	0.006	-0.040	-0.017	-0.071***	-0.034
	(0.106)	(0.028)	(0.037)	(0.160)	(0.027)	(0.025)
Anxiousness/Depression	0.046	-0.005	-0.064	-0.187	-0.014	-0.040*
	(0.092)	(0.038)	(0.051)	(0.168)	(0.034)	(0.025)
Headstrongness	0.065	0.021	-0.057	-0.018	-0.005	-0.053**
	(0.095)	(0.032)	(0.044)	(0.176)	(0.029)	(0.022)
Hyperactivity	0.051	-0.009	-0.069	-0.423	-0.039	-0.058**
	(0.098)	(0.035)	(0.043)	(0.263)	(0.027)	(0.029)
Immature Dependency	-0.038	0.025		-0.017	-0.050*	
	(0.090)	(0.036)		(0.155)	(0.026)	
Peer Conflict/Social Withdrawal	0.039	0.003	-0.056*	0.100	0.003	-0.043*
	(0.092)	(0.023)	(0.031)	(0.131)	(0.023)	(0.026)
BPI Composite Index	0.080	0.018	-0.111	-0.092	-0.052	-0.092**
	(0.136)	(0.045)	(0.073)	(0.199)	(0.032)	(0.037)
Overall BPI (TRI-based)	0.063	-0.002	-0.058	-0.137	-0.038	-0.048*
	(0.108)	(0.037)	(0.048)	(0.246)	(0.028)	(0.029)
Internalizing Behavior (TRI-based)	0.067	0.007	-0.067	-0.024	-0.024	-0.067*
	(0.109)	(0.041)	(0.051)	(0.215)	(0.029)	(0.036)
Externalizing Behavior (TRI-based)	0.097	0.005	-0.061	-0.163	-0.023	-0.058*
	(0.109)	(0.034)	(0.048)	(0.241)	(0.029)	(0.030)
Max N	1,619	2,587	1,894	1,416	2,893	2,254

Notes: This table reports IV estimates of α_1 from equation (3) when we estimate it separately for White and non-White children. All the models include the demographic controls of children (gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are also weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A10. Public insurance eligibility and behavioral outcomes at ages 4-5, IV estimates from robustness checks.

	Robustness Checks				
	(1)	(2)	(3)	(4)	(5)
Overall BPI	0.067	0.162	0.227	0.088	0.066
	(0.111)	(0.157)	(0.249)	(0.128)	(0.112)
Antisocial Behavior	0.127	0.138	0.138	0.153	0.143
	(0.092)	(0.124)	(0.166)	(0.104)	(0.098)
Anxiousness/Depression	0.030	0.095	0.082	0.056	0.032
	(0.072)	(0.092)	(0.147)	(0.085)	(0.074)
Headstrongness	0.045	0.149	0.218	0.074	0.029
	(0.079)	(0.119)	(0.176)	(0.089)	(0.074)
Hyperactivity	0.019	0.033	0.114	0.058	0.020
	(0.082)	(0.116)	(0.174)	(0.085)	(0.090)
Immature Dependency	-0.039	-0.037	-0.030	-0.055	-0.050
	(0.063)	(0.106)	(0.175)	(0.074)	(0.066)
Peer Conflict/Social Withdrawal	0.041	0.044	-0.016	0.034	0.050
	(0.084)	(0.105)	(0.162)	(0.088)	(0.083)
BPI Composite Index	0.063	0.105	0.110	0.080	0.063
	(0.108)	(0.156)	(0.240)	(0.126)	(0.109)
Overall BPI (TRI-based)	0.044	0.084	0.074	0.068	0.037
	(0.085)	(0.137)	(0.193)	(0.098)	(0.085)
Internalizing Behavior (TRI-based)	0.061	0.062	0.038	0.067	0.058
	(0.071)	(0.123)	(0.194)	(0.094)	(0.076)
Externalizing Behavior (TRI-based)	0.072	0.126	0.147	0.106	0.067
	(0.089)	(0.130)	(0.184)	(0.103)	(0.091)
Controls: EITC, AFDC/TANF, and unemployment rates	X				
Controls: region-specific cohort trends, region-by-age FE		X			
Controls: state-specific cohort trends, state-by-age FE			X		
Sample: non-movers				X	
Simulated eligibility based on March 1981 CPS					X

Notes: This table reports IV estimates from equation (3) or (4). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. When indicated, models use different sets of additional controls, an alternative sample restriction, or simulated eligibility constructed from another survey year. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A11. Public insurance eligibility and behavioral outcomes at ages 10-11, IV estimates from robustness checks.

	Robustness Checks				
	(1)	(2)	(3)	(4)	(5)
Overall BPI	0.006	0.045	-0.074	0.024	0.002
	(0.029)	(0.036)	(0.055)	(0.035)	(0.031)
Antisocial Behavior	-0.010	0.022	-0.101**	0.004	-0.015
	(0.024)	(0.031)	(0.047)	(0.021)	(0.024)
Anxiousness/Depression	0.001	0.046	-0.050	0.016	-0.010
	(0.022)	(0.029)	(0.054)	(0.028)	(0.029)
Headstrongness	0.014	0.048	-0.032	0.035	0.013
	(0.027)	(0.031)	(0.041)	(0.033)	(0.030)
Hyperactivity	-0.015	0.004	-0.057	0.013	-0.015
	(0.023)	(0.028)	(0.047)	(0.024)	(0.024)
Immature Dependency	0.002	0.025	-0.022	0.004	-0.0002
	(0.030)	(0.029)	(0.046)	(0.037)	(0.032)
Peer Conflict/Social Withdrawal	0.011	0.009	-0.070	0.009	0.005
	(0.018)	(0.024)	(0.044)	(0.023)	(0.020)
BPI Composite Index	0.004	0.043	-0.086	0.021	-0.002
	(0.034)	(0.042)	(0.067)	(0.040)	(0.037)
Overall BPI (TRI-based)	-0.007	0.020	-0.102**	0.009	-0.015
	(0.027)	(0.032)	(0.050)	(0.033)	(0.031)
Internalizing Behavior (TRI-based)	-0.001	0.017	-0.089*	0.022	-0.004
	(0.026)	(0.032)	(0.052)	(0.032)	(0.032)
Externalizing Behavior (TRI-based)	0.001	0.029	-0.091**	0.022	-0.003
	(0.023)	(0.033)	(0.045)	(0.030)	(0.027)
Controls: EITC, AFDC/TANF, unemployment rates, and school spending	X				
Controls: region-specific cohort trends, region-by-age FE		X			
Controls: state-specific cohort trends, state-by-age FE			X		
Sample: non-movers				X	
Simulated eligibility based on March 1981 CPS					X

Notes: This table reports IV estimates from equation (3) or (4). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. When indicated, models use different sets of additional controls, an alternative sample restriction, or simulated eligibility constructed from another survey year. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A12. Public insurance eligibility and cognitive outcomes, IV estimates from additional robustness checks.

	Additional Robustness Checks				
	(1)	(2)	(3)	(4)	(5)
Panel A: Age 4-5					
PPVT	0.253***	0.285*	0.168**	0.156*	0.152*
	(0.067)	(0.147)	(0.072)	(0.093)	(0.088)
Panel B: Age 10-11					
PPVT	0.061**	0.078**	0.100***	0.124***	0.096***
	(0.031)	(0.032)	(0.030)	(0.038)	(0.027)
PIAT Mathematics	0.054**	0.022	0.045*	0.063**	0.054***
	(0.027)	(0.023)	(0.025)	(0.027)	(0.019)
PIAT Reading Recognition	0.056*	0.002	0.045**	0.057**	0.049**
	(0.032)	(0.025)	(0.021)	(0.025)	(0.020)
PIAT Reading Comprehension	0.019	0.034	0.020	0.031	0.030
	(0.022)	(0.022)	(0.026)	(0.028)	(0.020)
Panel C: Age 13-14					
PIAT Mathematics	0.047**	0.053**	0.095***	0.112***	0.070***
	(0.019)	(0.023)	(0.021)	(0.031)	(0.018)
PIAT Reading Recognition	0.059***	0.028	0.106***	0.114***	0.089***
	(0.022)	(0.028)	(0.031)	(0.037)	(0.020)
PIAT Reading Comprehension	0.037***	0.022	0.063***	0.061***	0.044***
	(0.014)	(0.028)	(0.021)	(0.023)	(0.017)
Sample: families with income < 400% FPL	X				
Controls: cohort FE, region-specific cohort trends, region-by-age FE, state-level controls		X			
Simulated eligibility based on March 2001 CPS			X		
Sample: 1980-1999 cohorts				X	
Unweighted estimation					X

Notes: This table reports IV estimates from equation (3) or (4). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), state FE, and age FE. All the models also include indicators for the ages and years when actual eligibility could be imputed, except for column (2) which replaces these indicators by the cohort FE. When indicated, models use alternative sample restrictions, additional controls, simulated eligibility constructed from another survey year, or are unweighted. The state-level controls for column (2) are state EITC amounts, AFDC/TANF maximum monthly benefit, unemployment rate, and school expenditures per pupil. All estimates are weighted using NLSCYA custom survey weights in all but the last robustness check. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A13. Public insurance eligibility and behavioral outcomes, IV estimates from additional robustness checks.

	Additional Robustness Checks				
	(1)	(2)	(3)	(4)	(5)
Panel A: Age 4-5					
Overall BPI	0.113	0.253	0.034	0.119	0.057
	(0.101)	(0.187)	(0.107)	(0.119)	(0.097)
BPI Composite Index	0.123	0.182	0.032	0.117	0.072
	(0.102)	(0.184)	(0.104)	(0.119)	(0.099)
Overall BPI (TRI-Based)	0.124	0.201	0.012	0.091	0.049
	(0.083)	(0.178)	(0.077)	(0.096)	(0.084)
Panel B: Age 10-11					
Overall BPI	-0.027	0.068**	0.005	0.008	-0.021
	(0.025)	(0.032)	(0.027)	(0.028)	(0.026)
BPI Composite Index	-0.035	0.069*	0.004	0.008	-0.030
	(0.028)	(0.037)	(0.033)	(0.033)	(0.030)
Overall BPI (TRI-Based)	-0.039	0.044	-0.008	-0.008	-0.031
	(0.024)	(0.028)	(0.028)	(0.026)	(0.028)
Panel C: Age 13-14					
Overall BPI	-0.045*	0.040	-0.067*	-0.076*	-0.058*
	(0.025)	(0.043)	(0.038)	(0.042)	(0.030)
BPI Composite Index	-0.078**	0.050	-0.096*	-0.104*	-0.090**
	(0.032)	(0.050)	(0.049)	(0.056)	(0.039)
Overall BPI (TRI-Based)	-0.040	0.058	-0.050	-0.051	-0.044
	(0.026)	(0.039)	(0.034)	(0.038)	(0.029)
Sample: families with income <400% FPL	X				
Controls: cohort FE, region-specific cohort trends, region-by-age FE, state-level controls		X			
Simulated eligibility based on March 2001 CPS			X		
Sample: 1980-1999 cohorts				X	
Unweighted estimation					X

Notes: This table reports IV estimates from equation (3) or (4). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), state FE, and age FE. All the models also include indicators for the ages and years when actual eligibility could be imputed, except for column (2) which replaces these indicators by the cohort FE. When indicated, models use alternative sample restrictions, additional controls, simulated eligibility constructed from another survey year, or are unweighted. The state-level controls in column (2) are state EITC amounts, AFDC/TANF maximum monthly benefit, unemployment rate, and school expenditures per pupil. All estimates are weighted using NLSCYA custom survey weights in all but the last robustness check. Standard errors clustered at the state level are in parentheses. Significance levels: *p < 0.1, **p < 0.05, ***p < 0.01.

Table A14. Public insurance eligibility and developmental outcomes, IV results with unadjusted p values and sharpened q-values

Outcomes at ages 4-5	PPVT	Overall BPI	BPI Composite Index	Overall BPI (TRI-Based)		
Eligibility	0.170**	0.067	0.062	0.043		
	(0.083)	(0.120)	(0.115)	(0.090)		
P-Value	{0.041}	{0.575}	{0.590}	{0.632}		
Sharpened Q-Value	[0.196]	[0.901]	[0.901]	[0.901]		

Outcomes at ages 10-11	PPVT	PIAT Mathematics	PIAT Reading Recognition	PIAT Reading Comprehension	Overall BPI	BPI Composite Index	Overall BPI (TRI-Based)
Eligibility	0.105***	0.052**	0.046**	0.023	0.006	0.003	-0.009
	(0.033)	(0.026)	(0.022)	(0.027)	(0.028)	(0.034)	(0.029)
P-Value	{0.001}	{0.048}	{0.038}	{0.387}	{0.830}	{0.934}	{0.750}
Sharpened Q-Value	[0.010]	[0.107]	[0.107]	[0.632]	[1.000]	[1.000]	[1.000]

Outcomes at ages 13-14	PIAT Mathematics	PIAT Reading Recognition	PIAT Reading Comprehension	Overall BPI	BPI Composite Index	Overall BPI (TRI-Based)
Eligibility	0.099***	0.113***	0.066***	-0.071*	-0.101**	-0.053
	(0.024)	(0.032)	(0.023)	(0.039)	(0.050)	(0.035)
P-Value	{<0.001}	{<0.001}	{<0.001}	{0.057}	{0.048}	{0.120}
Sharpened Q-Value	[0.001]	[0.001]	[0.001]	[0.036]	[0.036]	[0.064]

Notes: This table reports IV estimates of α_1 from equation (3). All the models include the demographic controls of children (race/ethnicity, gender, birth order, interaction terms between gender and birth order), maternal characteristics (year of birth, marital status, highest level of education completed, AFQT percentile, number of children in the household), indicators for the ages and years when actual eligibility could be imputed, state FE, and age FE. All estimates are weighted using NLSCYA custom survey weights. Standard errors clustered at the state level are in parentheses. ***, **, and * indicate the estimates are significant at 1%, 5%, and 10%, based on the unadjusted p values in curly brackets. The sharpened q-values which are age-group specific are reported in square brackets.