The Impact of Preschool Entry Age on Children's Behavioral and Developmental Health in Medicaid*

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Abstract

We find that public preschools facilitate early diagnosis and treatment of conditions that can hinder learning. Low-income children born shortly before their state's school-entry cutoff date are 16.9, 9.3, and 14.8 percent more likely to be diagnosed with Attention Deficit Hyperactivity Disorder, a speech or language disorder, and a hearing or vision condition at ages three and four, compared to children born after the cutoff. They are also more likely to receive downstream services. Findings emphasize the role of earlier and longer exposure to public preschool in driving diagnostic gaps previously attributed to elementary school-entry and within-grade peer comparisons.

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1 Introduction

Children who are young for their grade level are more likely to be diagnosed with certain mental and behavioral conditions, like Attention Deficit Hyperactivity Disorder (ADHD), than their older classmates. This diagnosis gap has been documented across many countries and contexts using school-entry cutoff dates that require a child to turn five before a specific date to enter kindergarten. Researchers typically interpret the gap as reflecting differences in maturity between children who are nearly a year apart in age and are also in the same grade in elementary school.[1]

However, comparisons of children with birth dates just before versus after the kindergarten entry cutoff may capture earlier differences in preschool exposure rather than effects of relative age in elementary school. As of 2023, nearly half of all three- and four-year-olds were enrolled in preschool in the United States, with low-income children typically attending publicly-funded programs (USA Facts, 2023). In addition to providing early education, public preschools offer healthcare services through screenings and co-located providers (Hong, Dragan, and Glied, 2019). Public preschools follow state and district rules and use the same entry cutoff dates as elementary schools. As a result, children born just before the cutoff gain access to formal educational and healthcare resources through preschool up to one year earlier than those born just after it.

In this paper, we document a significant gap in diagnoses at ages three and four—that is, *before* kindergarten entry. We focus on children born in 2008–2009 and 2013–2014, whom we can observe from age three to five in administrative Medicaid data.[2] We additionally restrict to children residing in the 32 states with a consistent state-wide school-entry cutoff date throughout our analysis period. We use a regression discontinuity design to compare the diagnosis rates of children with birth dates just before and after the cutoff. We also study discontinuities in the receipt of treatment and other resources including ADHD medication, speech-language pathology, therapy, school-based services, and Supplemental Security Income (SSI).

We find that children born just before the cutoff date—who are eligible to enter preschool soon after turning three—are 0.4 percentage points (16.9 percent relative to the sample mean) more likely to be diagnosed with ADHD at ages three and four compared to children born shortly after the cutoff date. They are also 0.7 percentage points (9.3 percent) more likely to be diagnosed with a speech or language disorder, and 0.1 percentage points more likely to be diagnosed with a hearing or vision condition (14.8 percent, only marginally significant at the 10 percent level).

An earlier diagnosis leads to earlier treatment. We find that children born right before the

[1]See, for example: Elder, 2010; Evans, Morrill, and Parente, 2010; Dalsgaard et al., 2012; Morrow et al., 2012; Zoëga, Valdimarsdóttir, and Hernández-Díaz, 2012; Halldner et al., 2014; Krabbe et al., 2014; Pottegård, Hallas, and Zoëga, 2014; Chen et al., 2016; Schwandt and Wuppermann, 2016; Layton et al., 2018; Whitely et al., 2018; Root et al., 2019; Furzer, 2020; Furzer, Dhuey, and Laporte, 2022; Persson, Qiu, and Rossin-Slater, 2025.

[2]We select these two cohorts because we can observe them from ages three to five entirely within the same Medicaid data format. Medicaid transitioned from the Medicaid Analytic eXtract to the Transformed Medicaid Statistical Information System Analytic Files format in 2015, and there are known data quality problems around the transition period (Schpero et al., 2025). See Section 2 for more details.

cutoff are 0.2 percentage points (13.3 percent) more likely to be prescribed ADHD medication, 1.0 percentage points (11.4 percent) more likely to receive speech-language pathology, and 0.4 percentage points (11.9 percent) more likely to receive physical or occupational therapy at ages three and four.

Earlier diagnoses and treatment extend to downstream services. Children with birth dates before the cutoff are 1.9 percentage points (26.6 percent) more likely to receive school-based services, which include diagnostic, therapeutic, case management, and screening services provided by schools as part of students' Individual Education Program plans.[3] Children born before the cutoff are also 0.2 percentage points (7.0 percent, marginally significant at the 10 percent level) more likely to receive SSI. Because severe ADHD, speech, learning, vision, and hearing impairments may be SSI-qualifying conditions, this result suggests that earlier diagnosis facilitated by preschool attendance may allow families to access financial benefits sooner than they otherwise would.

When we consider age-specific outcomes separately, we find significant gaps in diagnoses occurring between children's fourth and fifth birthdays for all three main outcomes. Since children born before and after the cutoff are equally eligible to enroll in preschool at age four, this result suggests that the length of exposure to public preschool (rather than any attendance on the extensive margin) is likely a key mechanism. Further, we find that a discontinuity in new diagnoses persists at age five, suggesting that public preschools identify diagnoses that would not have occurred otherwise, rather than simply shifting diagnoses that would have happened in kindergarten. When we consider heterogeneity across states with and without universal public preschool during the analysis period, we find larger effects in the former group than in the latter, providing additional support for the role of preschool.

More broadly, diagnosis gaps suggest that public preschool programs play a role in connecting low-income children to healthcare and social services that facilitate early diagnoses and, importantly, treatment of conditions that impede learning. Further, they imply that the well-documented effects of elementary school-entry age may partially reflect differences in the timing and duration of *preschool* exposure between young-for-grade and old-for-grade children.

This paper contributes to and bridges the gap between two distinct strands of literature. Numerous studies have used school-entry cutoffs to document differences in ADHD and other mental health-related diagnoses between younger-for-grade and older-for-grade children in the US (Elder, 2010; Evans, Morrill, and Parente, 2010; Layton et al., 2018), Canada (Morrow et al., 2012; Furzer, 2020; Furzer, Dhuey, and Laporte, 2022), Denmark (Dalsgaard et al., 2012; Pottegård, Hallas, and Zoëga, 2014), Germany (Schwandt and Wuppermann, 2016), Iceland (Zoëga, Valdimarsdóttir, and Hernández-Díaz, 2012), the Netherlands (Krabbe et al., 2014), Sweden (Halldner et al., 2014; Persson, Qiu, and Rossin-Slater, 2025), Taiwan (Chen et al., 2016), and the United Kingdom (Root et al., 2019). These studies measure diagnosis gaps at elementary school ages (and older), and

[3]Medicaid pays for school-based services for Medicaid enrollees, which means we can measure them in our Medicaid claims data.

point to mechanisms related to mis- or over-diagnoses of children who are less mature than their classmates due to being relatively young-for-grade (see also Whitely et al., 2018 for a systematic review of the literature across nine countries). We show that diagnostic gaps appear before children begin kindergarten, which are likely driven by earlier exposure to public preschool. While relative-age differences may contribute to some ADHD diagnoses among preschoolers—since just-turned-three-year-olds are less mature than children nearing four—the discontinuities we observe in conditions unlikely to stem from peer comparisons, such as hearing and vision problems, suggest that public preschools connect low-income families to health and social services.

We thus also build on the literature about the effects of public preschool on children's outcomes. Studies have shown that the federal Head Start program improves short-term outcomes such as test scores (Currie and Thomas, 1995; Kline and Walters, 2016), longer-term outcomes like high school graduation, teen parenthood, college enrollment, adult economic well-being, mortality, and crime (Garces, Thomas, and Currie, 2002; Ludwig and Miller, 2007; Deming, 2009; Gibbs, Ludwig, and Miller, 2013; Carneiro and Ginja, 2014; Walters, 2015; Thompson, 2018; Johnson and Jackson, 2019; Bailey, Sun, and Timpe, 2021), and even the outcomes of the next generation (Barr and Gibbs, 2022). The literature on the effects of state-funded universal preschool programs extends these insights, finding positive effects on educational outcomes, especially among lower-income children (Gormley and Gayer, 2005; Fitzpatrick, 2008; Wong et al., 2008; Weiland and Yoshikawa, 2013; Lipsey, Farran, and Hofer, 2016; Gray-Lobe, Pathak, and Walters, 2022; Cascio, 2023).[4] As mechanisms, most of this literature emphasizes the importance of early education curricula that promote the development of cognitive and, especially, non-cognitive skills, as well as changes in parent–child interactions and parental involvement (Heckman and Mosso, 2014; García and Heckman, 2023).

Much less attention has been paid to the role of public preschools in connecting children and their families to healthcare and social services, especially in modern times.[5] An important exception is Hong, Dragan, and Glied (2019)'s analysis of the impacts of New York City's universal preschool program on the diagnoses of physical health conditions among Medicaid-enrolled children. They combine variation from a January 1st school-entry cutoff with the implementation of the program in 2014 to measure the change in outcomes in a difference-in-regression-discontinuities design. They find that eligibility for the program increased asthma and vision-related diagnoses, immunizations and screenings for infectious diseases, and the treatment of hearing and vision

[4]Related, a very large literature on older small-scale "model" interventions including the Perry Preschool Program and the Carolina Abecedarian Project find substantial evidence regarding lasting positive impacts of targeted high-quality preschool programs (see, e.g., Heckman et al., 2010; Heckman, Pinto, and Savelyev, 2013 as well as reviews in Almond and Currie, 2011 and Almond, Currie, and Duque, 2018).

[5]Studies of the implementation of Head Start in the 1960s point to the importance of the program's health and nutrition services, including vaccinations, screenings, referrals to medical providers, and healthy meals and snacks (Ludwig and Miller, 2007; Bailey, Sun, and Timpe, 2021). However, it is difficult to assess whether these aspects of the program are as relevant fifty to sixty years later.

conditions.

Our study builds on this work by using Medicaid claims data covering low-income children from 32 states, and by focusing on behavioral and developmental diagnoses that may be especially amenable to early intervention and treatment. We also directly measure the receipt of treatment and resources, such as medication, therapy, and other school-based and social services. Our study thereby sheds light on the role public preschools play in facilitating early diagnosis and treatment of conditions that can hinder children's learning, highlighting an underexplored set of mechanisms through which public preschool may influence long-term well-being.

2 Data and Sample

We use administrative data from the Medicaid program, the primary source of public insurance coverage for low-income individuals in the United States. In 2025, Medicaid—and its extension, the Children's Health Insurance Program (CHIP), which is also included in our data—covered approximately 78 million Americans (Centers for Medicare & Medicaid Services, 2025). The data are collected and curated by the Centers for Medicare & Medicaid Services (CMS), and capture all Medicaid and CHIP enrollment spells and claims from 2011 to 2019 (i.e., a 100% sample). Two different data formats were used over this period. In 2011–2014, states submitted their data via Medicaid Analytic eXtract (MAX) files, while in 2016–2019, states used Transformed Medicaid Statistical Information System Analytic Files (TAF). Most states shifted formats in 2015 and there are known data quality issues associated with the transition (Schpero et al., 2025).

Therefore, we focus on two cohorts of children born in March 2008–February 2009 and March 2013–February 2014, and restrict our observation windows to fall entirely within the MAX (2011–2014) and TAF (2016–2019) periods, respectively.[6] We include children who are ever enrolled in full-scope Medicaid or CHIP at age three.[7] Our baseline sample is comprised of 2,432,225 children, of which 1,221,361 are in the first cohort, and 1,210,864 are in the second cohort. The estimation sample is smaller because it only includes children born in narrow bandwidths around the school-entry cutoff, as described in Section 3. Additionally, we test the robustness of our results to restricting our analysis to the sub-sample of children who are continuously enrolled in Medicaid throughout the observation period (1,432,303 children).

CMS uses social security numbers to assign anonymized unique identifiers to all enrollees.[8] We use the demographic enrollment file to construct our cohort-based samples. The file contains information on beneficiaries' birth date, race, ethnicity, sex, and residence ZIP code. We assign

[6] We use March to February cohorts in order to center the birth dates around the modal school-entry cutoff date of September 1.

[7] We drop beneficiaries who only receive partial benefits (e.g., undocumented immigrants in some states) as we want to be able to observe their full set of claims.

[8] There are state-specific Medicaid identifiers for individuals without a social security number, but these identifiers are not necessarily unique over time. We therefore omit children without a CMS unique identifier, since our analyses require the ability to follow children over time.

beneficiaries to their state of residence as of the year they turn three, and we define a beneficiary's race and ethnicity as the most frequently reported non-missing value over 2011–2019.

We then use the beneficiary's identifier to link to inpatient (IP), other services (OT), and prescription drug (RX) claims over the window spanning the first day of the month in which the beneficiary turns three to the last day of the month before the beneficiary turns five.[9,10] Crucially for our research design, all children in our analysis sample are observed at the exact same ages, which means that any discontinuities at the school-entry cutoff cannot be explained by differences in outcome observation windows.

Outcomes. We focus on conditions likely to be flagged by teachers and other staff in a formal early childhood learning environment based on children's behavior and development. These conditions impede learning and are known to benefit from early intervention. Our main outcomes are binary indicators for a child ever having a claim with diagnoses of Attention Deficit Hyperactivity Disorder (ADHD), speech and learning disorders, and hearing and vision conditions over the observation window. We categorize downstream treatment, resources, and services to include receiving ADHD medication (e.g., stimulants), speech-language pathology, and occupational and physical therapy. We also measure receipt of broader services: an indicator for school-based services (SBS), Medicaid claims filed by schools, and an indicator for Medicaid enrollment through Supplemental Security Income (SSI). We also include three placebo outcomes that are unlikely to be affected by preschool attendance: congenital anomalies (e.g., Down syndrome), intellectual disabilities, and sickle cell disease. Appendix A.1 and Table A1 expand on how we define outcomes in the claims data.

Analysis states. We focus on the 32 states with a state-level legislated school-entry cutoff date that remained the same throughout our analysis period. Table A2 illustrates the relevant cutoff dates by state-cohort. We exclude states with cutoff dates that would compare beneficiaries born in different calendar years, i.e., Connecticut (January 1st cutoff) and Hawaii (December 31st cutoff). We also exclude four states—California, Michigan, Nebraska, and Tennessee—which changed their cutoff date over our period.[11]

[9]The IP files contain hospital stay records, including International Classification of Diseases (ICD) diagnosis and procedure codes, as well as dates of service. The OT files contain claims for services occurring in many settings, including physician offices, emergency departments, and outpatient clinics, along with the associated ICD diagnosis and procedure codes. The RX files contain records of filled prescriptions and their National Drug Codes (NDCs). We classify drugs based on conditions they are most frequently used to treat using a crosswalk to map NDC codes to Anatomical Therapeutic Chemical Classification codes.

[10]We have fee-for-service claims and "encounter" records for individuals enrolled in Medicaid managed care plans. We broadly refer to all of these data as "claims" although the encounter information does not include any payment information.

[11]Cutoff dates for kindergarten entry for each state were primarily sourced from the National Center for Education Statistics (2018) and the Education Commission of the States (2014, 2011, 2013). Whenever the sources provided conflicting information or a cutoff date changed, we referred to legislative documents and newspaper articles for clarification, as described in Table A3. To determine whether state-funded preschool programs follow the same cutoff dates, we used National Institute for Early Education Research (2012-2019) and secondary sources

Descriptive statistics. Table 1 presents the means of observable characteristics of the children in our sample in Panel A, diagnosis rates in Panel B, and shares receiving different types of treatment and services in Panel C. We split the sample into children who are born before and after the school-entry cutoff, and use a 70-day bandwidth around the cutoff.[12]

Our sample is evenly split between the two cohorts and balanced in terms of child sex. About 23 percent of the children are Black, 32 percent are Hispanic/Latino, 34 percent are white, and just over 11 percent belong to another race and ethnicity group or are missing this information. Around 19 percent reside in a rural ZIP code.[13]

The Table shows that while the observable characteristics of children born before and after the cutoff are very similar, the rates of behavioral and developmental diagnoses and of treatment and service receipt differ. Just over two percent of the children in our data are diagnosed with ADHD at ages three and four, while about seven percent are diagnosed with a speech or language disorder and one percent with a hearing or vision condition at these ages, respectively. To better understand how diagnosis rates vary with child age, Appendix Figure A1 uses data on all diagnoses that occur between a child's birth and sixth birthday and plots histograms of the ages at which they appear. We find that, conditional on being diagnosed before age six, only 1.4 percent of ADHD diagnoses occur before age three. In contrast, about a quarter of speech and language disorder diagnoses and slightly more than half of hearing and vision condition diagnoses occur before age three. These patterns suggest that while preschool age is a common diagnostic age, earlier diagnosis is also possible.

Rates of congenital anomalies, intellectual disabilities, and sickle cell disease—our placebo outcomes—are lower than one percent (Table 1, Panel B). Finally, slightly more than one percent of children are prescribed ADHD medication, while around nine percent receive speech-language pathology, and four percent receive occupational or physical therapy. Between seven and nine percent of children receive school-based services, and around three percent receive SSI benefits. We explore these differences more formally using the regression discontinuity design approach, as described below.

3 Empirical Framework

To estimate the impacts of preschool entry age, we use a regression discontinuity design that compares children with birthdays shortly before and after the school entry cutoff date in their state of residence. We estimate the following regression model using individual-level data, with

in Table A3. Head Start Program Performance Standards (2024) outlines that Head Start programs adhere to local district cutoff dates for school-entry.

[12] This is the maximum bandwidth selected by the optimal bandwidth algorithm used in estimation, as described in Section 3.

[13] We merge in data from the 2010 Census to categorize ZIP codes by urban/rural status (U.S. Census Bureau, 2011).

the running variable measuring the number of days between a child's exact birthday and the school entry cutoff:

$$Y_{its} = \beta_0 + \beta_1 \mathbf{1}[D_i < c_s] + f(D_i - c_s) + \mathbf{1}[D_i < c_s] \times f(D_i - c_s) + \mathbf{x}'_i\gamma + \delta_t + \rho_s + \epsilon_{its} \tag{1}$$

for each child i born in cohort t (either 2008–2009 and 2013–2014) and residing in state s. Y_{its} is the outcome of interest, such as an indicator for having an ADHD diagnosis at some point between a child's third and fifth birthdays. $\mathbf{1}[D_i < c_s]$ is an indicator for a child's birthday D_i being before the cutoff c_s in their state, and $f(D_i - c_s)$ is a function of the running variable, which captures the difference in days between a child's birthday and the cutoff, and which we allow to differ on opposite sides of the cutoff. $\mathbf{x}_i$ is a vector of individual-level controls, which include indicators for female sex, residing in an urban ZIP code, and race and ethnicity (white, Black, Hispanic, and other or missing), and whether one's birthday falls on a weekend or holiday. We also include fixed effects for the child's birth cohort, δ_t, and state of residence, ρ_s, to account for aggregate time trends and differences in policy environments and data quality across states, respectively. We cluster standard errors on the running variable (Lee and Card, 2008).

Our key coefficient of interest, β_1, captures the magnitude of the discontinuity in the outcome variable between children born just before and just after the cutoff. We estimate model (1) non-parametrically with a triangular kernel and bias-corrected inference procedures, and applying an optimal bandwidth selection algorithm (Calonico, Cattaneo, and Titiunik, 2014a,b; Calonico et al., 2017, 2019). We obtain optimal bandwidths ranging from 35.58 to 70.99 days around the cutoff in our primary specifications.

Identification and interpretation. The regression discontinuity design relies on the assumption that only the treatment variable changes discontinuously at the cutoff, and all other variables related to outcomes should be continuous functions of the running variable (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In our setting, treatment is measured by whether a child's birthday is before the school-entry cutoff date, as this makes the child eligible for preschool soon after turning three.

Since the school-entry cutoff date is known, parents may strategically time births in a way that leads to non-random sorting, violating the identification assumption. To assess this, Appendix Figure A2 plots the distribution of births of Medicaid enrollees in seven-day bins. There does not appear to be any major sorting around the cutoff visually, but the Cattaneo, Jansson, and Ma (2018) RD manipulation test yields a significant $t-$statistic of 7.91. While it is possible that some parents time births, prior research suggests that birth timing manipulation around school-entry cutoffs only occurs when these cutoffs coincide with holidays (Dickert-Conlin and Elder, 2010). However, given that we focus on Medicaid-covered children, some of the sorting may be driven by differences in the propensity to enroll in Medicaid.

We take several steps to address the possibility of non-random sorting around the cutoff. First, we check whether there are discontinuities in children's observable characteristics. Appendix Figure A3 shows that there are no significant jumps in the shares of female, Black, Hispanic/Latino, or white children at the cutoff. The share of children residing in an urban ZIP code shows a slight and only marginally significant discontinuity of 0.5 percentage points, which is very small compared to the sample mean of 81 percent. As noted in Section 2, Panel A of Table 1 provides further support for the comparability of children born before and after the cutoff by showing that the means of observable characteristics are similar across the two groups.

Second, we implement a "doughnut-RD" in a robustness check by dropping children with birthdays within a week of the cutoff date. Since our analysis uses a running variable with a daily frequency, the main concern is regarding strategic manipulation of the timing of birth among families who conceive around the same time. This can happen through, for example, planned cesarean sections or inductions, and can alter the timing of birth by no more than a few days to a couple weeks. Therefore, by dropping children born one week before to one week after the cutoff, we can remove any such manipulations. As we discuss in Section 4, our results are very similar when using this alternative specification. Third, as already noted in Section 3, we include individual-level controls in our primary regression models, but we also test the sensitivity of our results to excluding them. Fourth, we examine placebo outcomes that should not be influenced by the school entry cutoff, but can also be diagnosed over the ages observed in our data. Fifth, we check the sensitivity of our results to restricting to children continuously enrolled in Medicaid over the entire observation window. Overall, our sensitivity analyses support the identifying assumption and suggest that non-random sorting is unlikely to influence our key findings.

4 Results

We begin by presenting the results for our main outcomes capturing diagnoses of behavioral and developmental conditions observed between children's third and fifth birthdays. Figure 1 contains raw data plots using seven-day bins of mean diagnosis rates of ADHD in sub-figure (a), speech and language disorders in sub-figure (b), and hearing and vision conditions in sub-figure (c), respectively. For both ADHD and speech and language disorders, there is a clear discontinuity in the likelihood of having a diagnosis at the cutoff date. The discontinuity for hearing and vision conditions is somewhat less stark, but nevertheless still noticeable. We report the corresponding regression model estimates for these outcomes in Panel A of Table 2, and print the estimate of β_1 from equation (1) on each graph for reference.[14]

Our results indicate that children born before the cutoff are 0.4 percentage points (16.9 percent relative to the sample mean) and 0.7 percentage points (9.3 percent) more likely to be diagnosed

[14] Our regression models use the optimal bandwidth for each outcome (reported in the last column of Table 2), while our graphs use a consistent 70-day bandwidth for visual ease.

with ADHD and speech and language disorders, respectively, at ages three and four, compared to children born shortly after the cutoff date. They also have a 0.1 percentage point (14.8 percent) higher probability of being diagnosed with a hearing or vision condition, but this result is marginally significant at the 10 percent level, consistent with the smaller visual gap in Figure 1.

In Figure 2, we examine how the receipt of treatment and other resources varies at the cutoff, with corresponding regression estimates reported in Panel B of Table 2. We demonstrate that the higher diagnosis rates among children born before the cutoff translate into earlier treatment. Our results show that, at ages three and four, children born right before the cutoff are 0.2 percentage points (13.3 percent) more likely to be prescribed ADHD medication, 1.0 percentage points (11.4 percent) more likely to receive speech-language pathology, and 0.4 percentage points (11.9 percent) more likely to receive physical or occupational therapy.

These effects extend to downstream services: children with birth dates before the cutoff are 1.9 percentage points (26.6 percent) more likely to receive school-based services at ages three and four. We also find a marginally significant 0.2 percentage point (7.0 percent) higher likelihood of receiving SSI. All in all, these results underscore that public preschools facilitate access to resources for families whose children are diagnosed with behavioral and developmental conditions.

We explore heterogeneity in the effects on our main outcomes across individual characteristics in Appendix Figures A6 and A7. Specifically, we present sub-group-specific β_1 coefficients and 95% confidence intervals in absolute terms in Appendix Figure A6 and as relative effects scaled by the respective sub-group outcome means in Appendix Figure A7. The effects are broadly similar across children from different racial and ethnic sub-groups, boys and girls, and those living in urban and rural ZIP codes, with overlapping confidence intervals for all outcomes.

Mechanisms. While our Medicaid data do not allow us to observe preschool attendance directly, we present two additional findings that suggest that earlier entry into preschool is a main mechanism. First, we study effects on *new* diagnoses at ages three, four, and five. Specifically, we construct three binary indicators for: (i) having a diagnosis between the third and fourth birthday, (ii) not having a diagnosis at age three, but having one between the fourth and fifth birthday, and (iii) not having a diagnosis at ages three or four, but having one between the fifth and sixth birthday.[15] For outcomes (i) and (ii), the sample of analysis is the same as our main sample. For outcome (iii), we expand the observation window by one year.[16]

[15]Given our sample construction, we are unable to observe diagnoses before age three in our data. Thus, it is possible that diagnoses measured at age three are not new. That said, for children whom we can follow in our data from birth through their fourth birthday, more than half of the diagnoses that occur at age three are first diagnoses. Specifically, 90 percent of ADHD, 55 percent of speech and language disorder, and 66 percent of hearing and vision condition diagnoses, respectively, occur for the first time between children's third and fourth birthdays.

[16]For the second cohort—those born from March 2013 to February 2014—we face a censoring issue since we can only observe children born in January and February of 2014 until the ages of five years and 11 or 10 months, respectively (since our data end in 2019). In practice, this means that outcomes are censored if we use bandwidths greater than 77 days (because we center the data on the school cut-off date, the latest of which is October 15th).

Table 3 shows that for ADHD, the magnitudes of the discontinuities in new diagnoses increase from age three to five. However, this pattern does not hold for speech and language disorders or for hearing and vision conditions. That said, since children born on both sides of the cutoff are equally eligible to attend preschool between their fourth and fifth birthdays, the fact that we observe a significant discontinuity in new diagnoses over this window suggests that the length of exposure to preschool—rather than any attendance on the extensive margin—is the driver of these gaps. Moreover, at least for ADHD and speech and language disorders, the significant discontinuities in new diagnoses observed between children's fifth and sixth birthdays indicate that preschools do not just shift diagnoses that would have otherwise happened in kindergarten to occur earlier, but rather generate additional diagnoses that perhaps would have otherwise not happened at all.

Second, we compare children living in three groups of states: those with an existing state-funded preschool program throughout our analysis period, those in which a program was implemented between 2011 and 2019, and those with no such program (National Institute for Early Education Research, 2012-2019). Table A4 illustrates that our effects are concentrated among children living in states with pre-existing programs. Importantly, 24 of the 32 states included in our analysis are in this group, suggesting that universal public preschool programs are likely key mechanisms for the overall effects that we find. Four states implemented a program over the analysis period (IN, MN, MS, and MT), and just four small states had no program at all (ID, SD, UT, and WY). The federal Head Start program, which is available in all states, is likely also relevant given that 41 percent of children in poverty are enrolled (Friedman-Krauss, Barnett, and Duer, 2023).

Robustness. We conduct a number of analyses probing the sensitivity of our results and addressing concerns regarding non-random sorting around the school-entry cutoff. Appendix Figure A4 and Table A5 show that our results are robust to using alternative optimal bandwidth algorithms and different bandwidths.[17]

We present results for our placebo outcomes—congenital anomalies, intellectual disabilities, and sickle cell disease—in Panel C of Table 2 and Appendix Figure A5. These are severe and mostly genetically-influenced conditions, for which we do not expect preschool attendance to impact the likelihood of diagnosis. Consistent with this conjecture, we do not find any significant discontinuities at the school-entry cutoff for these outcomes.

Appendix Table A6 shows results from the "doughnut-RD" model that omits beneficiaries born in a one-week bandwidth around the cutoff. The results are similar to our main findings. Interestingly, the discontinuity in SSI receipt becomes larger and more significant in the "doughnut-RD" specification, consistent with there being an outlier point immediately to the right of the threshold in Figure 2(e).

As a result, we use the bandwidths from Table 2 to directly calculate age-specific estimates, with no bandwidth exceeding 77, thereby ensuring equal numbers of observations across age bins.

[17]Specifically, we use all of the possible options available in the `rdrobust` command.

Appendix Table A7 presents estimates of model (1) without the individual-level controls, while Appendix Table A8 presents the results for the subset of beneficiaries who are continuously enrolled throughout the observation period from their third to fifth birthday. All specifications largely confirm our main results.

That said, we note that a few of the outcomes—hearing and vision conditions, ADHD medications, and SSI—are slightly sensitive to specification, although all point estimates are directionally similar. In some of the robustness tests, the sample sizes change substantially. For instance, when we restrict to continuously enrolled beneficiaries, the sample size is 40 percent smaller, likely reducing our statistical power. Given that some of the outcomes are quite rare—for example, ADHD medications are not frequently prescribed to three- and four-year-olds—it is not surprising that we lose statistical significance in these cases.

5 Conclusion

The value of early intervention for children with behavioral and developmental conditions has been recognized for decades (e.g., Conroy and Brown, 2004; Petrenko, 2013; Britto et al., 2017). Yet low-income families often face barriers to accessing these interventions for their children due to multiple structural factors, including a lack of information, time, and money necessary to seek appropriate diagnoses and follow-up care and resources. This paper suggests that public preschool programs in the United States link families and health and social services by facilitating earlier diagnoses of conditions that hinder learning.

Specifically, using administrative data from the Medicaid program and a regression discontinuity design, we show that, at ages three and four, children born shortly before their state's preschool-entry cutoff date are 16.9, 9.3, and 14.8 percent more likely to be diagnosed with ADHD, a speech or language disorder, and a hearing or vision condition, respectively, than their counterparts born shortly after. Children eligible to start preschool at a younger age are also 13.2, 11.4, and 11.9 more likely to receive ADHD medication, speech-language pathology, and physical or occupational therapy, respectively, at these same ages. Outside the healthcare system, children exposed to preschool at a younger age are 26.6 and 7.0 percent more likely to get school-based services and SSI, respectively. These effects appear to be relatively universal in the Medicaid population, spanning across all race, ethnicity, sex, and urban sub-groups.

This evidence provides a new perspective on the large literature documenting differences in diagnosis rates between young-for-grade and old-for-grade children in kindergarten and beyond. Many of the existing studies, which have mostly focused on ADHD, suggest that the extra diagnoses among children born before school-entry cutoffs reflect potential misdiagnoses based on peer comparisons of children almost one year apart in age (Whitely et al., 2018). We show that these diagnostic gaps arise even before children start kindergarten and affect conditions that are less likely to be misdiagnosed. Moreover, we show that earlier diagnosis leads to additional services and resources for low-income families. For instance, receiving SSI benefits from a younger age translates into a meaningful increase in income—for 2026, the annual SSI payment is $11,929 (Social Security Administration, 2025).

While our findings are strongly consistent with the possibility that attending public preschool at a younger age leads to earlier diagnosis and treatment of behavioral and developmental conditions, our data are limited by a lack of information on actual preschool enrollment. Future research with better data can assess this more directly. Moreover, an analysis of the longer-term outcomes of children who receive diagnoses and treatments at preschool ages would help illuminate the long-term value of public preschool for human capital development and well-being.

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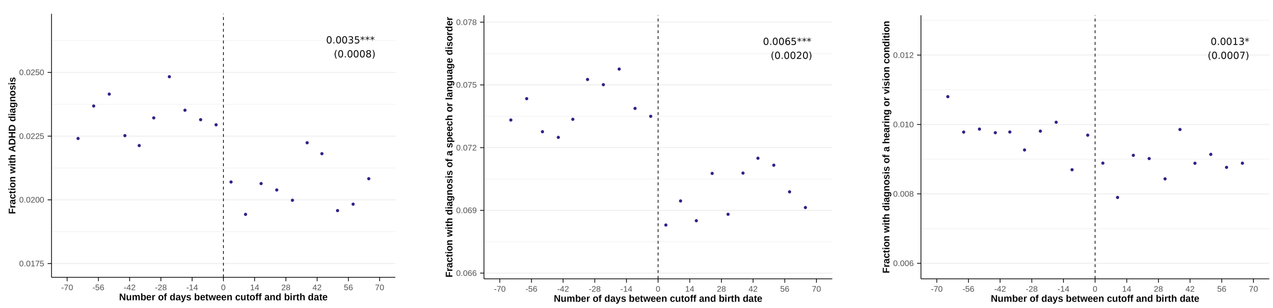
6 Figures

Figure 1: Fraction of children with a diagnosis at ages three and four



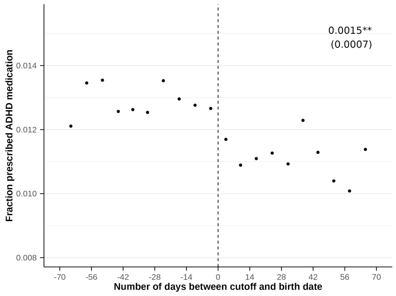
(a) ADHD

(b) Speech and language disorders

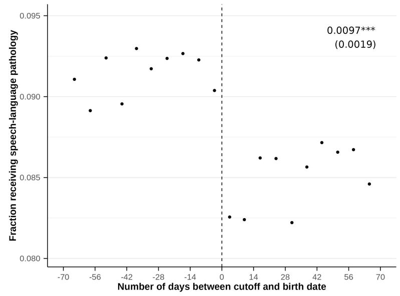
(c) Hearing and vision conditions

Note: The plots display the fractions of diagnosis in seven-day bins within a bandwidth of 70 days around the cutoff. Figures include the regression discontinuity estimate and standard errors from Panel A of Table 2.

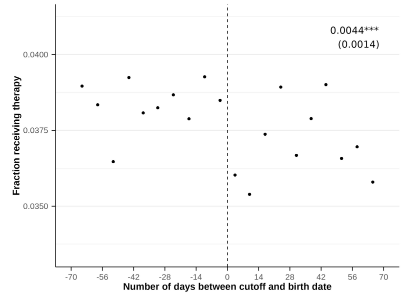
Figure 2: Fraction of children receiving treatment or services at ages three and four



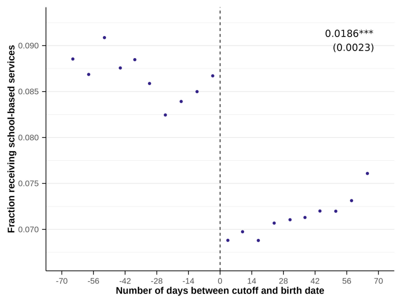
(a) ADHD medication



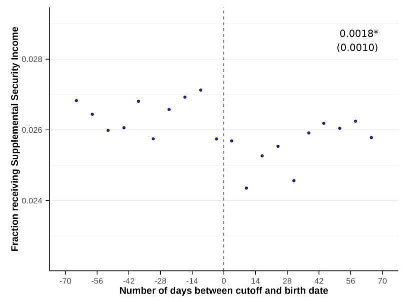
(b) Speech-language pathology



(c) Therapy



(d) School-based services



(e) Supplemental Security Income

Note: The plots display the fractions of receipt of treatment or services in seven-day bins within a bandwidth of 70 days around the cutoff. Figures include the regression discontinuity estimate and standard errors from Panel B of Table 2.

7 Tables

Table 1: Characteristics of the study population

	Children Born [-70, 0] Days Relative to the Cutoff	Children Born [1, 70] Days Relative to the Cutoff
Panel A. Observable characteristics		
Birth cohorts: percent born in...		
2008-2009	50.4	50.4
2013-2014	49.6	49.6
Percent female	48.8	48.9
Percent Black	22.8	22.8
Percent Hispanic/Latino	32.0	31.6
Percent white	33.9	33.6
Percent other or missing race/ethnicity	11.3	11.9
Percent residing in rural ZIP code	19.1	19.1
Panel B. Percent diagnosed with condition		
ADHD	2.3	2.1
Speech and language disorders	7.4	7.0
Hearing and vision conditions	1.0	0.9
Congenital anomalies	0.6	0.6
Intellectual disabilities	0.1	0.2
Sickle cell disease	0.1	0.1
Panel C. Percent receiving treatment/services		
ADHD medication	1.3	1.1
Speech-language pathology	9.1	8.5
Therapy	3.8	3.7
School-based services	8.6	7.1
Supplemental Security Income	2.6	2.6
Number of observations	491,803	479,411

Notes: “ADHD” denotes attention-deficit/hyperactivity disorder. “Therapy” includes physical and occupational therapy.

Table 2: Regression discontinuity estimates of the effect of being born before the cutoff date

	Mean	Estimate	Bandwidth
Panel A: Behavioral and developmental health			
ADHD	0.0207	0.0035*** (0.0008)	51.95
Speech and language disorders	0.0699	0.0065*** (0.0020)	49.74
Hearing and vision conditions	0.0088	0.0013* (0.0007)	51.74
Panel B: Treatment, resources, and services			
ADHD medication	0.0113	0.0015** (0.0007)	51.94
Speech-language pathology	0.0848	0.0097*** (0.0019)	52.37
Therapy	0.0371	0.0044*** (0.0014)	43.08
School-based services	0.0699	0.0186*** (0.0023)	35.58
Supplemental Security Income	0.0256	0.0018* (0.0010)	70.99
Panel C: Placebo outcomes			
Congenital anomalies	0.0058	0.0000 (0.0005)	70.77
Intellectual disabilities	0.0015	0.0001 (0.0002)	50.53
Sickle cell disease	0.0011	0.0002 (0.0002)	56.07

Note: Standard errors (in parentheses) are bias-corrected and clustered by the running variable, number of days between cutoff and birth date. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and control for sex, race/ethnicity, and urban/rural ZIP code. Means are calculated among beneficiaries born after the cutoff. "BW" denotes bandwidth. "Therapy" includes physical and occupational therapy.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 3: Regression discontinuity estimates of the effect of being born before the cutoff date, at specific ages

	Age 3		Age 4		Age 5		
	Mean	Estimate	Mean	Estimate	Mean	Estimate	BW
ADHD	0.0074	-0.0006 (0.0006)	0.0133	0.0032*** (0.0006)	0.0197	0.0171*** (0.0008)	51.95
Speech and language disorders	0.0464	0.0036* (0.0021)	0.0235	0.0018* (0.0010)	0.0146	0.0034*** (0.0008)	49.74
Hearing and vision conditions	0.0053	0.0009 (0.0007)	0.0036	0.0005 (0.0004)	0.0029	0.0005 (0.0005)	51.74

Note: Standard errors (in parentheses) are bias-corrected and clustered by the running variable, number of days between cutoff and birth date. We set both the main and bias bandwidths equal to the optimal bandwidths from the main specification. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and control for sex, race/ethnicity, and urban/rural ZIP code. Means are calculated among beneficiaries born after the cutoff. "BW" denotes bandwidth.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

A Appendix

A.1 Defining outcomes

We define diagnoses of speech and language disorders, hearing and vision conditions, congenital anomalies, and intellectual disabilities following the Children With Disabilities Algorithm (Chien et al., 2015, 2023). We define ADHD medications to be stimulants and guanfacine, or Anatomical Therapeutic Chemical (ATC) Classification codes that start with "N06BA" except "N06BA07" and the code "C02AC02" following Persson, Qiu, and Rossin-Slater (2025). We define physical or occupational therapy and speech-language pathology following Morris et al. (2025). We measure the receipt of school-based services (SBS), which are claims filed by schools, and an indicator for receiving Medicaid due to being enrolled in SSI, following the methodology outlined in Harmon Sanchez, and Pomerantz (2023) and Centers for Medicare & Medicaid Services (2022, 2016).

Table A1 lists all codes used to define these outcomes. "ICD-9" are International Classification of Diseases, Ninth Revision and "ICD-10" are International Classification of Diseases, Tenth Revision diagnosis codes. We use a crosswalk to map the National Drug Codes to ATC Classification codes, which are used to classify drugs based on conditions they are most frequently used to treat. "HCPCS" stands for Healthcare Common Procedure Coding System and "CPT" stands for Current Procedural Terminology, both of which classify procedure codes in the Medicaid data.

Table A1: Definition of outcomes

Diagnosis codes:	ICD-9	ICD-10
ADHD	314	F90
Speech and language disorders	31500, 31501, 31502, 3151, 31532, 31534, 31539, 78461	F800, F802, F804, F8082, F8089, F810, F812, H9325, R480
Hearing and vision conditions	36081, 36221, 36226, 36227, 36271, 36272, 36322, 36355, 36424, 36510, 36511, 36515, 36524, 36541, 36542, 36543, 36573, 36633, 36634, 36900, 36901, 36903, 36904, 36906, 36907, 36908, 3691 except for 36910, 36922, 36924, 3693, 3694, 37711, 37713, 37714, 3777, 37856, 37953, 38843, 38904, 3891 except for 38917, 38920, 38922, 3897	H20829, H26219, H26229, H30819, H3121, H35159, H35169, H35179, H36, H4010, H4011, H40159, H40249, H4089, H44829, H47219, H47239, H47299, H47619, H47629, H47639, H47649, H4930, H540X, H541, H542X12, H542X2, H543, H548, H5503, H902, H903, H904, H905, H906, H908, H913, H93299
Congenital anomalies	740, 741, 7423, 7424, 74251, 74259, 7428, 74300, 74306, 74312, 7432, 74342, 74345, 74348, 74400, 74402, 74405, 74409, 7467, 75011, 7535, 7542, 75533, 75534, 75651, 7580, 7581, 7852, 7583, 7585, 7594, 75981, 75983	G901, Q00, Q030, Q031, Q038, Q045, Q046, Q048, Q05 except for Q053 and Q059, Q060, Q061, Q062, Q063, Q068, Q0701, Q0702, Q0703, Q078, Q111, Q112, Q131, Q133, Q1389, Q150, Q161, Q165, Q169, Q234, Q383, Q6410, Q6419, Q675, Q7210, Q7240, Q763, Q7642 except for Q76429, Q780, Q871, Q894, Q909, Q913, Q917, Q928, Q933, Q934, Q937, Q9381, Q9388, Q9389, Q992
Intellectual disabilities	317, 318	F70, F71, F72, F73
Sickle cell disease	28241, 28242, 2826	D57 except for D573

Other codes:	
ADHD medication	ATC codes that start with "N06BA" except "N06BA07," and the code "C02AC02."
School-based services	HCPCS code T1018, HCPCS modifier TM, Place of Service code 03, benefit type code 060, CMS-64 Form category of service code 0039, or billing provider taxonomy codes 101YS0200X, 103TS0200X, 1041S0200X, 163WS0200X, 251300000X, 261QS1000X, 363LS0200X, 364SS0200X
Speech-language pathology	CPT codes 92507, 92508, 92520, 92521, 92522, 92523, 92524, 92525, 92526, 92606
Occupational and physical therapy	CPT codes 90912, 90913, 92548, 92549, 95831, 95851, 96000-96004, 97010, 97012, 97016, 97018, 97022, 97024, 97026, 97028, 97032-97037, 97039, 97110, 97112, 97113, 97116, 97124, 97129, 97130, 97139, 97140, 97150, 97161-97168, 97350, 97530, 97533, 97535, 97537, 97542, 97545, 97546, 97550-97552, 97597, 97598, 97602, 97605, 97610, 97696, 97750, 97755, 97760, 97761, 97763, 97799, and HCPCS codes G0237-G0239, G0515
Supplemental Security Income	Eligibility group code (`ELGBLTY_GRP_CD_mm`/`TMSIS_ELG_CD_MO_mm`) equal to 11, 12, or 13, or MAX eligibility code (`MAX_ELG_CD_MO_mm`) equal to 11 or 12

A.2 Preschool and kindergarten cutoff dates

Table A2: Preschool and kindergarten cutoff dates

	MAX Cohort		TAF Cohort	
State	Preschool	Kindergarten	Preschool	Kindergarten
AL	9/1	9/1	9/1	9/1
AR	8/1	8/1	8/1	8/1
AZ	N/A*	8/31	N/A*	8/31
DC	9/30	9/30	9/30	9/30
DE	8/31	8/31	8/31	8/31
FL	9/1	9/1	9/1	9/1
GA	9/1	9/1	9/1	9/1
IA	9/15	9/15	9/15	9/15
ID	—	9/1	—	9/1
IL	9/1	9/1	9/1	9/1
IN	—	8/1	8/1	8/1
KS	8/31	8/31	8/31	8/31
MD	9/1	9/1	9/1	9/1
ME	10/15	10/15	10/15	10/15
MN	—	9/1	9/1	9/1
MS	—	9/1	9/1	9/1
MT	—	9/10	9/10	9/10
NC	8/31	8/31	8/31	8/31
ND	—	7/31	7/31	7/31
NM	8/31	8/31	8/31	8/31
NV	9/30	9/30	9/30	9/30
OK	9/1	9/1	9/1	9/1
OR	9/1	9/1	9/1	9/1
RI	9/1	9/1	9/1	9/1
SC	9/1	9/1	9/1	9/1
SD	—	9/1	—	9/1
TX	9/1	9/1	9/1	9/1
UT	—	9/1	—	9/1
VA	9/30	9/30	9/30	9/30
WA	8/31	8/31	8/31	8/31
WI	9/1	9/1	9/1	9/1
WY	—	9/15	—	9/15

Notes: The dates are the latest birth date one can have to be eligible for school-entry. For example, a cutoff date of 9/1 means children must be 5 years old on or before 9/1 to enter kindergarten that year. Dashes indicate that the state did not have a public preschool program. *Arizona's state-funded early childhood program supports children from birth to age 5, so it has no minimum age. See Table A3 for sources.

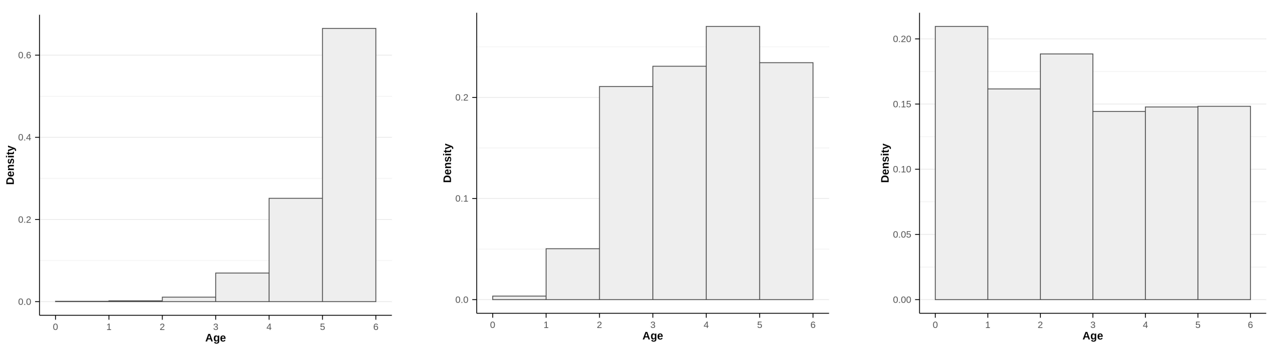
Table A3: Prioritization of sources for cutoff dates after main references

State	Preschool	Kindergarten
AZ	Arizona Department of Education (n.d.)	
DC	Code of the District of Columbia (2008)	
IN	Indiana Family and Social Services Administration (2015)	
MS	Mississippi Legislature (2013)	
ND	North Dakota Department of Health and Human Serices (n.d.)	
UT		Utah Administrative Code (2014)

Notes: Kindergarten cutoff dates were primarily determined from National Center for Education Statistics (2018) followed by the Education Commission of the States (2014, 2011, 2013). We referred to Utah's administrative code because NCES and ECS provided conflicting information on whether the cutoff date is inclusive or exclusive of September 2. Preschool cutoff dates were primarily sourced from the State of Preschool Yearbooks by the National Institute for Early Education Research (2012-2019). Whenever there were inconsistencies, or if a state enacted some change (such as implementing a new program or adjusting their cutoff date), we sought clarification from a secondary source.

A.3 Additional results

Figure A1: Age distribution of diagnoses



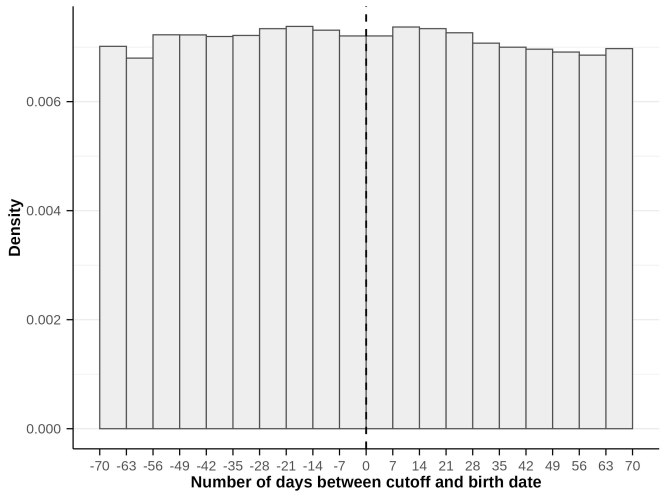
(a) ADHD

(b) Speech and language disorders

(c) Hearing and vision conditions

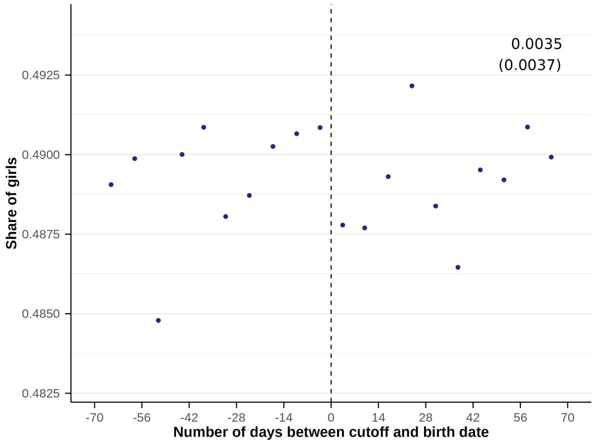
Note: This figure shows the distribution of ages at diagnosis, from birth to age five, in the Medicaid data from 2011 to 2019.

Figure A2: Distribution of counts in seven-day bins

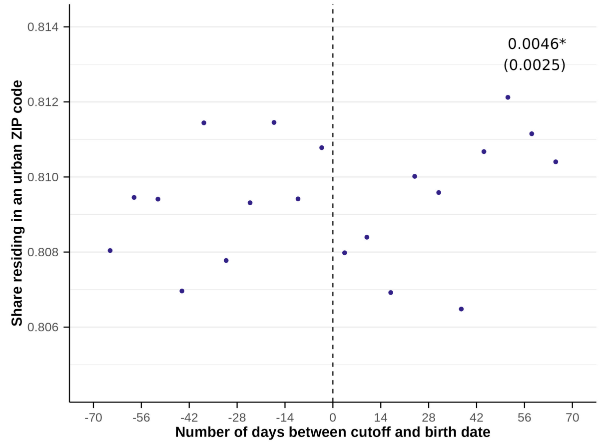


Note: The histogram shows the sample distribution of birth dates in seven-day bins, within a 70-day bandwidth around the cutoff.

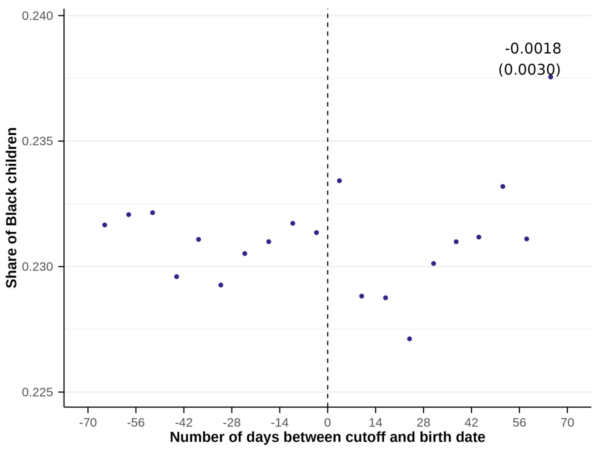
Figure A3: Gender, urban/rural, and race/ethnicity composition by birth date



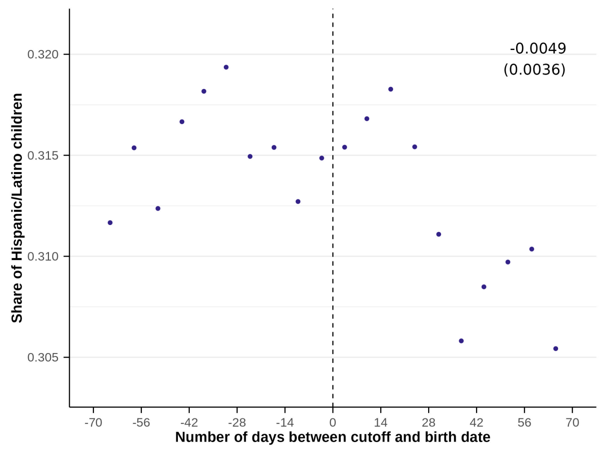
(a) Share of girls



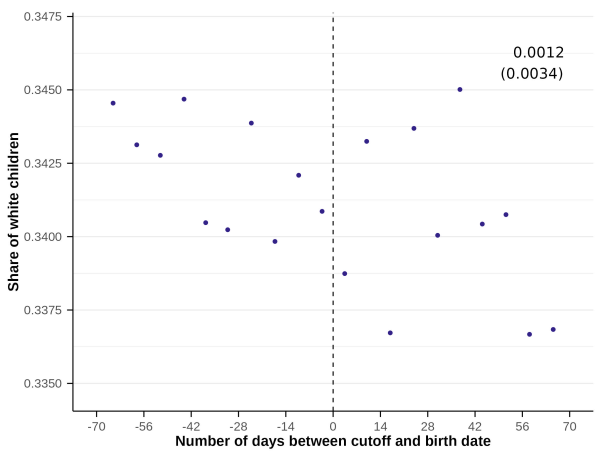
(b) Share residing in urban ZIP code



(c) Share of Black children



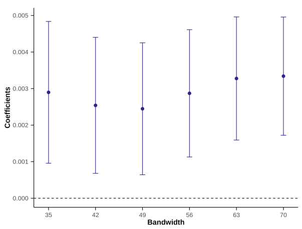
(d) Share of Hispanic/Latino children



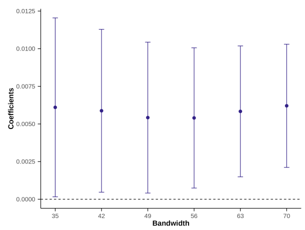
(e) Share of white children

Note: The plots display the shares in seven-day bins within a bandwidth of 70 days around the cutoff. Figures include the regression discontinuity estimate and standard errors, which are bias-corrected using the optimal bandwidth and clustered by the running variable: the number of days between the school-entry cutoff date and birth date.

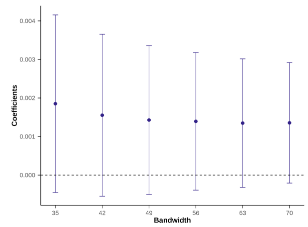
Figure A4: Regression discontinuity estimates of the effect of being born after the cutoff date, with varying bandwidth



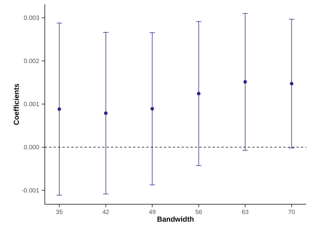
(a) ADHD



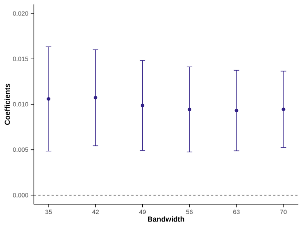
(b) Speech and language disorders



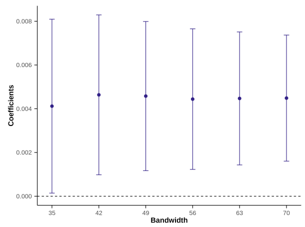
(c) Hearing and vision conditions



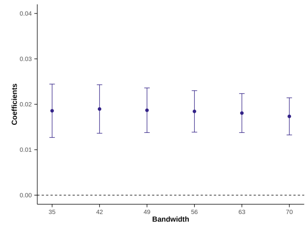
(d) ADHD medication



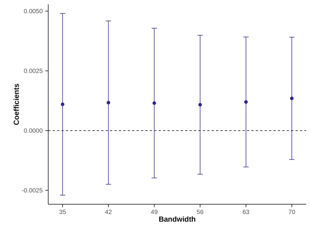
(e) Speech-language pathology



(f) Therapy



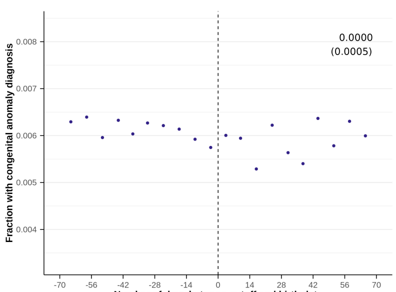
(g) School-based services



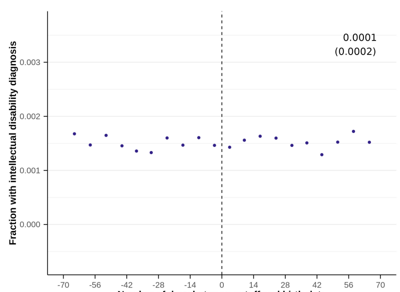
(h) Supplemental Security Income

Note: We plot the coefficients and 95% confidence intervals of the regression discontinuity estimates, varying the bandwidth from 35 to 70 days in seven-day increments. The running variable is the number of days between the school-entry cutoff and an individual's birth date. Standard errors are bias-corrected and clustered by the running variable and we set the bias bandwidth equal to the main bandwidth. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and control for sex, race, ethnicity, and whether the ZIP code is classified as urban. "Therapy" includes physical and occupational therapy.

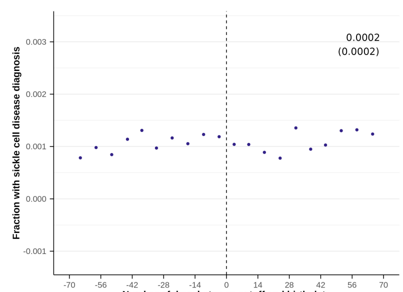
Figure A5: Fraction of children with a diagnosis at ages three and four, placebo outcomes



(a) Congenital anomalies



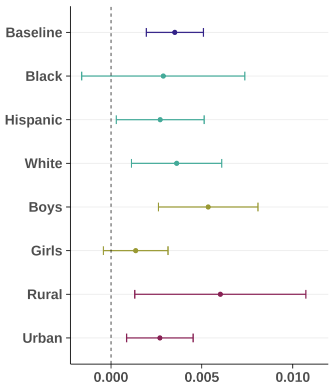
(b) Intellectual disabilities



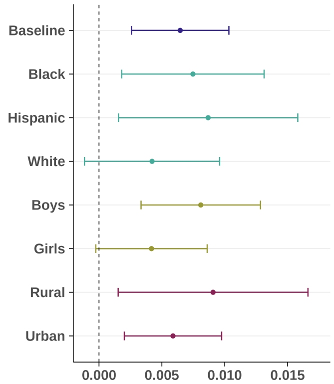
(c) Sickle cell disease

Note: The plots display the fraction of children with a specific diagnosis in seven-day bins within a bandwidth of 70 days around the cutoff. Figures include the regression discontinuity estimate and standard errors from Panel C of Table 2.

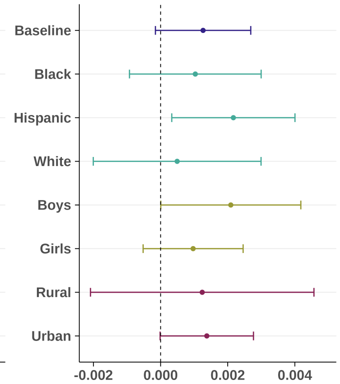
Figure A6: Heterogeneity by individual characteristics



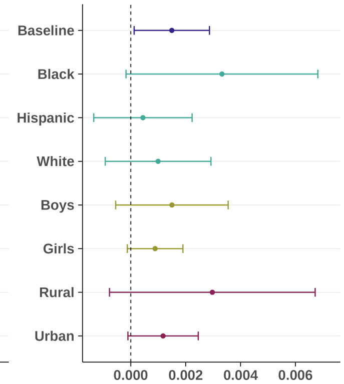
(a) ADHD



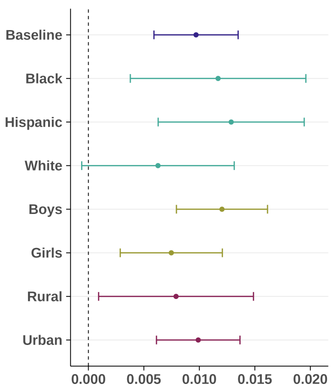
(b) Speech and language disorders



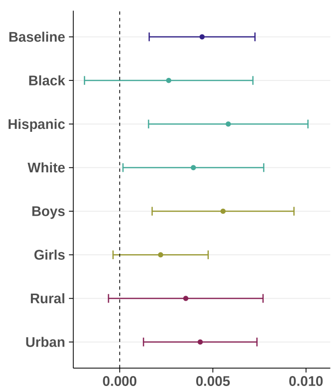
(c) Hearing and vision conditions



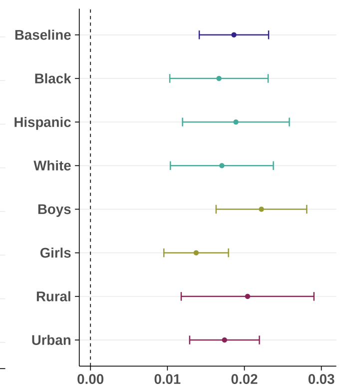
(d) ADHD medication



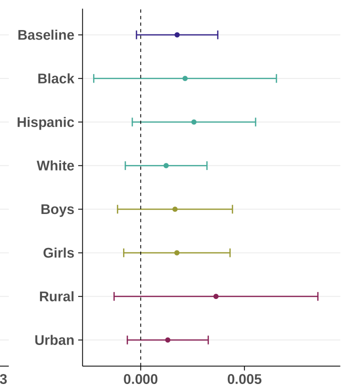
(e) Speech-language pathology



(f) Therapy



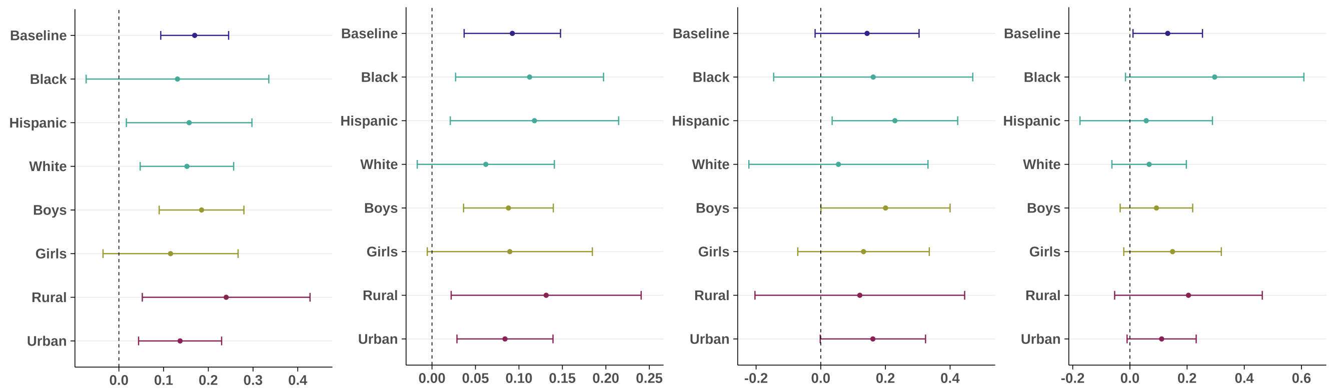
(g) School-based services



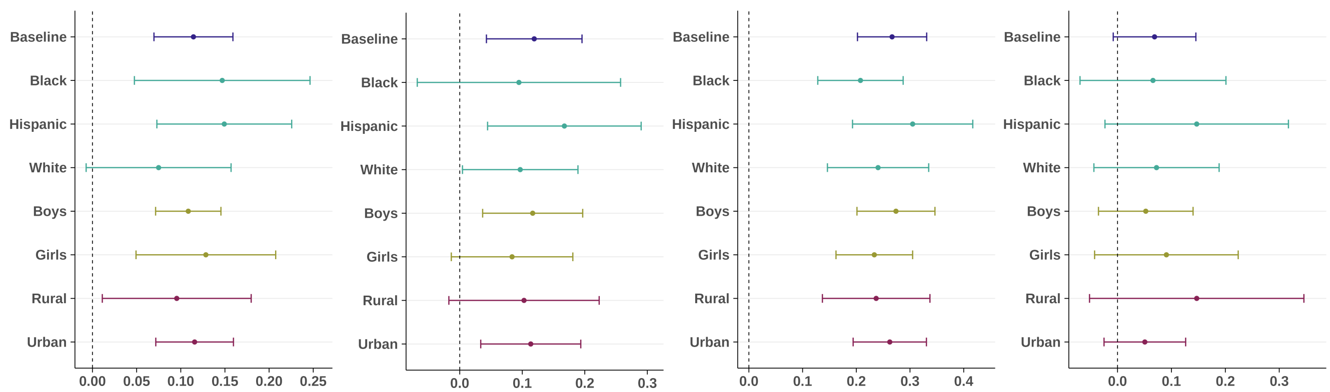
(h) Supplemental Security Income

Note: We plot the coefficients and 95% confidence intervals of the regression discontinuity estimates for children belonging to the sub-group denoted on the y-axis. The running variable is the number of days between the school-entry cutoff and an individual's birth date. Standard errors are bias-corrected and clustered by the running variable. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and all controls except for the characteristic being grouped on.

Figure A7: Heterogeneity by individual characteristics, normalized relative to sub-group mean



(a) ADHD (b) Speech and language disorders (c) Hearing and vision conditions (d) ADHD medication



(e) Speech-language pathology (f) Therapy (g) School-based services (h) Supplemental Security Income

Note: We plot the coefficients and 95% confidence intervals of the regression discontinuity estimates for children belonging to the sub-group denoted on the y-axis. Coefficient estimates are scaled relative to the baseline outcome mean for each sub-group. The running variable is the number of days between the school-entry cutoff and an individual's birth date. Standard errors are bias-corrected and clustered by the running variable. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and all controls except for the characteristic being grouped on.

Table A4: Regression discontinuity estimates of the effect of being born before the cutoff date, by implementation of state-funded preschool

	Pre-Existing Program			Implemented During Analysis Period			No Program		
	Mean	Estimate	BW	Mean	Estimate	BW	Mean	Estimate	BW
Panel A: Behavioral and developmental health									
ADHD	0.0209	0.0041***	52.36	0.0205	0.0002	64.39	0.0139	-0.0028	50.49
		(0.0008)			(0.0021)			(0.0037)	
Speech and language disorders	0.0707	0.0080***	50.43	0.0697	-0.0046	51.31	0.0463	-0.0013	41.75
		(0.0021)			(0.0050)			(0.0065)	
Hearing and vision conditions	0.0091	0.0016**	51.35	0.0071	-0.0019	55.53	0.0066	0.0017	58.25
		(0.0008)			(0.0016)			(0.0020)	
Panel B: Treatment, resources, and services									
ADHD medication	0.0112	0.0017***	58.10	0.0124	-0.0006	52.77	0.0062	-0.0027	44.24
		(0.0006)			(0.0020)			(0.0025)	
Speech-language pathology	0.0880	0.0112***	58.57	0.0652	-0.0060	43.45	0.0577	0.0044	40.84
		(0.0019)			(0.0054)			(0.0064)	
Therapy	0.0378	0.0050***	42.35	0.0338	-0.0031	52.83	0.0264	0.0074	51.64
		(0.0013)			(0.0042)			(0.0050)	
School-based services	0.0697	0.0199***	32.27	0.0749	0.0095	50.54	0.0599	0.0235***	34.05
		(0.0023)			(0.0059)			(0.0086)	
Supplemental Security Income	0.0259	0.0019*	74.82	0.0258	0.0023	52.75	0.0164	-0.0028	73.91
		(0.0011)			(0.0028)			(0.0041)	

Note: States that implemented a preschool program during our analysis period include: IN, MN, MS, and MT. States that never had a state-funded preschool program during our analysis period include: ID, SD, UT, and WY. The running variable is the number of days between the school-entry cutoff and an individual's birth date. Standard errors are clustered by the running variable and are bias-corrected by setting the bias bandwidth equal to the main bandwidth. Standard errors (in parentheses) are bias-corrected and clustered by the running variable, number of days between cutoff and birth date. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and control for sex, race, ethnicity, and whether the ZIP code is classified as urban. Means are calculated among beneficiaries born after the cutoff. "BW" denotes bandwidth.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A5: Regression discontinuity estimates of the effect of being born before the cutoff date, alternative optimal bandwidth algorithms

	MSE	MSE-2	MSE-Sum	Min-MSE	Med-MSE	CER	CER-2	CER-Sum	Min-CER	Med-CER
ADHD										
Estimate	-0.0033***	-0.0030***	-0.0032***	-0.0033***	-0.0033***	-0.0023**	-0.0023**	-0.0027***	-0.0023**	-0.0023**
	(0.0009)	(0.0009)	(0.0008)	(0.0009)	(0.0009)	(0.0010)	(0.0010)	(0.0009)	(0.0010)	(0.0010)
Mean	0.0233	0.0233	0.0234	0.0233	0.0233	0.0235	0.0235	0.0233	0.0235	0.0235
N	695,301	663,867	837,971	695,301	709,180	523,203	495,551	629,819	523,203	530,521
Left BW	51.75	53.73	62.16	51.75	53.73	38.51	39.99	46.26	38.51	39.99
Right BW	51.75	44.97	62.16	51.75	51.75	38.51	33.46	46.26	38.51	38.51
Speech and language disorders										
Estimate	0.0065***	0.0068***	0.0067***	0.0065***	0.0068***	0.0059***	0.0063***	0.0060***	0.0059***	0.0061***
	(0.0020)	(0.0019)	(0.0018)	(0.0020)	(0.0019)	(0.0021)	(0.0021)	(0.0020)	(0.0021)	(0.0021)
Mean	0.0699	0.0698	0.0699	0.0699	0.0699	0.0694	0.0691	0.0694	0.0694	0.0694
N	668,600	715,988	749,982	668,600	710,269	508,557	534,986	564,224	508,557	538,362
Left BW	49.74	47.35	55.43	49.74	49.74	37.01	35.24	41.24	37.01	37.01
Right BW	49.74	58.63	55.43	49.74	55.43	37.01	43.63	41.24	37.01	41.24
Hearing and vision conditions										
Estimate	0.0013*	0.0012*	0.0011*	0.0013*	0.0012*	0.0012	0.0012*	0.0012*	0.0012	0.0012*
	(0.0007)	(0.0007)	(0.0006)	(0.0007)	(0.0007)	(0.0008)	(0.0007)	(0.0007)	(0.0008)	(0.0007)
Mean	0.0088	0.0089	0.0089	0.0088	0.0089	0.0088	0.0088	0.0088	0.0088	0.0088
N	694,517	778,014	905,759	694,517	778,014	523,182	576,330	680,850	523,182	576,330
Left BW	51.74	63.08	67.93	51.74	63.08	38.50	46.94	50.55	38.50	46.94
Right BW	51.74	52.25	67.93	51.74	52.25	38.50	38.88	50.55	38.50	38.88
ADHD medication										
Estimate	0.0015**	0.0013**	0.0012**	0.0015**	0.0013**	0.0012	0.0013*	0.0014**	0.0012	0.0013*
	(0.0007)	(0.0007)	(0.0006)	(0.0007)	(0.0007)	(0.0008)	(0.0007)	(0.0006)	(0.0008)	(0.0007)
Mean	0.0113	0.0111	0.0111	0.0113	0.0111	0.0113	0.0113	0.0112	0.0113	0.0113
N	694,517	753,309	982,440	694,517	759,782	523,182	561,773	737,544	523,182	569,091
Left BW	51.94	61.78	73.12	51.94	61.78	38.65	45.97	54.41	38.65	45.97
Right BW	51.94	50.40	73.12	51.94	51.94	38.65	37.50	54.41	38.65	38.65
Speech-language pathology										
Estimate	0.0097***	0.0095***	0.0098***	0.0097***	0.0096***	0.0096***	0.0093***	0.0095***	0.0096***	0.0093***
	(0.0019)	(0.0019)	(0.0017)	(0.0019)	(0.0019)	(0.0021)	(0.0020)	(0.0018)	(0.0021)	(0.0020)
Mean	0.0848	0.0851	0.0851	0.0848	0.0851	0.0843	0.0843	0.0849	0.0843	0.0843
N	709,193	726,325	876,949	709,193	740,205	523,182	539,960	657,157	523,182	547,278
Left BW	52.37	57.26	65.20	52.37	57.26	38.97	42.61	48.52	38.97	42.61
Right BW	52.37	50.48	65.20	52.37	52.37	38.97	37.57	48.52	38.97	38.97
Therapy										
Estimate	0.0044***	0.0045***	0.0044***	0.0044***	0.0044***	0.0047***	0.0046***	0.0046***	0.0047***	0.0047***
	(0.0014)	(0.0015)	(0.0014)	(0.0015)	(0.0015)	(0.0016)	(0.0016)	(0.0015)	(0.0016)	(0.0016)
Mean	0.0371	0.0370	0.0374	0.0371	0.0371	0.0369	0.0369	0.0370	0.0369	0.0369
N	588,022	644,000	657,157	588,022	624,199	444,524	476,830	494,854	444,524	470,322
Left BW	43.08	41.47	48.41	43.08	43.08	32.06	30.86	36.02	32.06	32.06
Right BW	43.08	53.13	48.41	43.08	48.41	32.06	39.54	36.02	32.06	36.02
School-based services										
Estimate	0.0186***	0.0188***	0.0170***	0.0186***	0.0186***	0.0187***	0.0184***	0.0180***	0.0187***	0.0186***
	(0.0023)	(0.0022)	(0.0020)	(0.0023)	(0.0021)	(0.0025)	(0.0023)	(0.0021)	(0.0025)	(0.0023)
Mean	0.0699	0.0703	0.0705	0.0699	0.0703	0.0699	0.0698	0.0699	0.0699	0.0698
N	482,548	500,974	644,906	482,548	543,027	364,368	375,115	482,548	364,368	404,446
Left BW	35.58	44.17	47.63	35.58	44.17	26.47	32.87	35.44	26.47	32.87
Right BW	35.58	29.95	47.63	35.58	35.58	26.47	22.29	35.44	26.47	26.47

Note: The running variable is the number of days between the school-entry cutoff and an individual's birth date. Standard errors (in parentheses) are bias-corrected and clustered by the running variable. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and control for sex, race/ethnicity, and urban/rural ZIP code. Means are calculated among beneficiaries born after the cutoff.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A6: Regression discontinuity estimates of the effect of being born before the cutoff date, doughnut RD

	Mean	Estimate	Bandwidth
Panel A: Behavioral and developmental health			
ADHD	0.0204	0.0044*** (0.0016)	37.18
Speech and language disorders	0.0701	0.0076*** (0.0025)	50.99
Hearing and vision conditions	0.0089	0.0008 (0.0007)	57.44
Panel B: Treatment, resources, and services			
ADHD medication	0.0112	0.0021* (0.0011)	36.43
Speech-language pathology	0.0853	0.0107*** (0.0025)	49.86
Therapy	0.0373	0.0062** (0.0025)	39.16
School-based services	0.0701	0.0171*** (0.0040)	38.70
Supplemental Security Income	0.0253	0.0038** (0.0017)	48.55

Note: The running variable is the number of days between the school-entry cutoff and an individual's birth date. Standard errors (in parentheses) are bias-corrected and clustered by the running variable. We exclude all beneficiaries born in the one-week bandwidth around the cutoff. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and control for sex, race, ethnicity, and whether the ZIP code is classified as urban. Means are calculated among beneficiaries born after the cutoff.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A7: Regression discontinuity estimates of the effect of being born before the cutoff date, without controls

	Mean	Estimate	Bandwidth
Panel A: Behavioral and developmental health			
ADHD	0.0205	0.0031*** (0.0007)	58.82
Speech and language disorders	0.0700	0.0064*** (0.0020)	48.91
Hearing and vision conditions	0.0091	0.0012* (0.0007)	48.53
Panel B: Treatment, resources, and services			
ADHD medication	0.0112	0.0011* (0.0006)	52.68
Speech-language pathology	0.0849	0.0095*** (0.0019)	52.30
Therapy	0.0372	0.0040*** (0.0014)	43.38
School-based services	0.0696	0.0176*** (0.0023)	36.39
Supplemental Security Income	0.0255	0.0019* (0.0010)	68.77
Panel C: Placebo outcomes			
Congenital anomalies	0.0059	-0.0000 (0.0005)	71.05
Intellectual disabilities	0.0015	0.0001 (0.0002)	48.20
Sickle cell disease	0.0010	0.0003* (0.0002)	55.97

Note: The running variable is the number of days between the school-entry cutoff and an individual's birth date. Standard errors (in parentheses) are bias-corrected and clustered by the running variable. Regressions include state, cohort, and weekend/holiday fixed effects. Means are calculated among beneficiaries born after the cutoff.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A8: Regression discontinuity estimates of the effect of being born before the cutoff date, among those continuously enrolled

	Mean	Estimate	Bandwidth
Panel A: Behavioral and developmental health			
ADHD	0.0267	0.0037*** (0.0014)	53.89
Speech and language disorders	0.0883	0.0075*** (0.0022)	52.79
Hearing and vision conditions	0.0114	0.0016 (0.0011)	53.31
Panel B: Treatment, resources, and services			
ADHD medication	0.0150	0.0019* (0.0012)	53.11
Speech-language pathology	0.1093	0.0128*** (0.0023)	62.82
Therapy	0.0494	0.0034** (0.0017)	70.89
School-based services	0.0924	0.0250*** (0.0034)	32.46
Supplemental Security Income	0.0356	0.0025 (0.0016)	62.83
Panel C: Placebo outcomes			
Congenital anomalies	0.0081	-0.0001 (0.0008)	73.12
Intellectual disabilities	0.0020	-0.0002 (0.0003)	47.46
Sickle cell disease	0.0013	0.0004* (0.0003)	56.03

Note: The running variable is the number of days between the school-entry cutoff and an individual's birth date. Standard errors (in parentheses) are bias-corrected and clustered by the running variable. Regressions include fixed effects for state, cohort, and weekend/holiday birth date, and control for sex, race, ethnicity, and whether the ZIP code is classified as urban. Means are calculated among beneficiaries born after the cutoff.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$