Measuring Inappropriate Medical Diagnosis and Treatment in Survey Data: The Case of ADHD among School-Age Children

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Abstract:
We exploit the discontinuity in age when children start kindergarten generated by state eligibility laws to examine whether relative age is a significant determinant of ADHD diagnosis and treatment. Using a regression discontinuity model and exact dates of birth, we find that children born just after the cutoff, who are relatively old-for-grade, have a significantly lower incidence of ADHD diagnosis and treatment compared with similar children born just before the cutoff date, who are relatively young-for-grade. Since ADHD is an underlying neurological problem where incidence rates should not change dramatically from one birth date to the next, these results suggest that age relative to peers in class, and the resulting relative behavior, directly affects a child's probability of being diagnosed with and treated for ADHD.

I. Introduction

Nearly all critics of the U.S. healthcare system note that the U.S. spends far more on health care than any other developed country yet performs poorly in international comparisons on aggregate outcomes such as life expectancy and infant mortality.[1] Some interpret these statistics as an indication that the U.S. health care system is on the "flat-of-the-curve" (Fuchs, 2004) in the health production

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[1] In 2006, per capita spending on health in the U.S. was $6,714, more than twice the median value for OECD countries. Despite this spending, in 2005, the U.S. ranked 25th of 29 countries in average life expectancy and the U.S. had the fourth highest infant mortality rate of 28 reporting countries. All data is from the OECD's frequently requested data series, http://www.irdes.fr/EcoSante/DownLoad/OECDHealthData_FrequentlyRequestedData.xls

function meaning the marginal health care dollar is of little or questionable medical value. The notion that a large fraction of health care spending produces little return is bolstered by data from the Dartmouth Atlas which shows that per capita Medicare reimbursements across hospital referral regions vary by a factor of three (Wennberg et al., 2008), yet there is little evidence that these differences in spending lead to better quality of care (Baicker and Chandra, 2004) or better mortality outcomes (Fisher et al., 2003). This same research program suggests that the U.S. could reduce Medicare spending by 30 percent without any drop in medical outcomes. Similarly, the Institute of Medicine (2007) estimates that nationwide less than half of all treatments delivered are supported by evidence.

The statistics reported above have lead to a greater emphasis on reducing waste and improving the quality of clinical decisions as cornerstones of any health care reform initiative. For example, $1.1 billion was earmarked for cost-effectiveness research as part of the American Recovery and Reinvestment Act, signed into law on February 19, 2009 by President Obama.[2]

The difficulty in implementing procedural reforms is identifying what is and is not medically appropriate. Utilization review is now commonplace in medicine and there is a large volume of research that uses chart review to identify procedures that are appropriately indicated by medical conditions. Unfortunately, chart reviews are expensive and in many instances review can only indicate whether the treatment was appropriate given the diagnosis, not whether the diagnosis itself was correct in the first place. In this paper, we implement a statistical procedure to examine the medical appropriateness of one specific diagnosis (attention-deficit/hyperactivity disorder) and its most frequent treatment (stimulants). The procedure is implemented using information typically gathered in claims data files or reported in surveys, which greatly reduces the data needs compared to other forms of utilization review.

Attention-deficit/hyperactivity disorder (ADHD) is a neurological disorder characterized by delayed brain development. According to the National Institute for Mental Health (NIMH) ADHD Booklet (2008), children with ADHD are hyperactive and tend to have difficulty staying focused and controlling behavior. The ADHD Booklet explains (p. 2) "it is normal for all children to be inattentive, hyperactive, or impulsive sometimes, but for children with ADHD, these behaviors are more severe and occur more often." Not only is ADHD difficult to diagnosis, but often the diagnosis is made by a pediatrician or family physician without consultation with a mental health specialist (Safer and Malever, 2000). In the United States about 5 to 10 percent of children aged 6 to 18 have been diagnosed with ADHD and some estimates suggest this number increased by 500 percent between the late 1980's and early 2000's (Zuvekas, Vitiello and Norquist, 2006).

In this paper, we provide evidence that the diagnosis and treatment of ADHD is heavily influenced by the relative age of children in school. Most public schools in the United States have an

[2] PL 111-5, http://www.gpo.gov/fdsys/pkg/PLAW-111publ5/pdf/PLAW-111publ5.pdf.

official "age of start" date that indicates the time by which a child must turn five years old in order to start kindergarten. Age at school start laws create quasi-experimental variation in the age of children where those born just before the kindergarten eligibility date may enter school in a given year, while children born only a few days later must wait an entire year to enter school. The children born just before the cutoff date are younger than their classroom peers, on average. The relative immaturity of these young-for-grade children may be mistaken as ADHD due to the nature of the diagnostic guidelines that suggest a comparison with a child's peers. According to the medical guidelines described by the NIMH ADHD Booklet health professionals are asked to consider whether the observed behaviors (p. 6) "happen more often in this child compared with the child's peers?" Given age-of-start laws, a typical kindergarten class may contain a child who just turned five and someone almost six, a difference of 20 percent. Using a regression discontinuity model, we exploit the discrete jump in school enrollment generated by kindergarten eligibility laws to examine whether children's relative age influences their probability of being diagnosed with ADHD and, as a result, to be prescribed stimulants.

ADHD is an underlying neurological problem and incidence rates should not change dramatically from one birth date to the next. If diagnosis rates do shift appreciably based on small changes in birth dates, then the diagnosis is not based entirely on underlying conditions. Evidence consistent with increased diagnosis of ADHD for younger children is provided in Elder and Lubotsky (2009) who used samples from the Early Childhood Longitudinal Study – Kindergarten cohort (ECLS-K) data to document persistent negative consequences for younger children in school.

In this paper, we use data on ADHD diagnosis from the 1997 to 2006 National Health Interview Survey (NHIS), plus data on prescription drug use of stimulants from the 1996 to 2006 Medical Expenditure Panel Survey (MEPS) and a nationwide private health insurance company over the 2003 through 2006 time period. In all three samples, we find evidence children whose fifth birthday fell just after the school eligibility cutoff date, who are therefore more likely to be older-for-grade, have significantly lower chances of being diagnosed with, and treated for, ADHD. The effect sizes are large. Children born just after the cutoff date are 1 to 3 percentage points less likely to be diagnosed with or chemically treated for ADHD. The results imply that being young for your grade increased the chance of taking stimulants by about 25 percent.

The basic results in this paper are quite similar to those in Elder (2009), who used the same techniques employed here and data from the ECLS-K to demonstrate that children born just before the state's age-of-start cutoff date are 50 percent more likely to be diagnosed with ADHD than those born just after. The fact that the basic results in this paper can be replicated in four different data sets should be reassuring.

II. Background on ADHD

According to the National Institutes of Mental Health (NIMH) ADHD Booklet, the characteristic behaviors associated with ADHD are inattention, hyperactivity and impulsivity. These symptoms typically appear early in life and in many cases last into adulthood. Accurate identification of ADHD is critical since children with ADHD are at an increased risk of academic difficulties such as a greater incidence of learning disabilities (Mayer et al., 2000), a higher chance of repeating a grade and lower test scores (Currie and Stabile, 2006), and a higher dropout rate (Trampush et al., 2009). Outside the classroom, children with ADHD have higher rates of illegal drug use (Biederman et al., 1998), greater motor vehicle accident rates (Woodward et al., 2000; Barkley et al., 1993), and a greater likelihood of having other psychiatric conditions (Pliszka, 1998; Jensen et al., 1997). Data from the National Survey of Children's Health indicate that among youths 4 to 17 years of age, 7.8 percent reported an ADHD diagnosis, with boys having a 2.5 greater incidence rate than girls (Visser, Lesene, and Perou, 2007).

Treatments options for children with ADHD include medication management, behavioral treatment, routine community care, or some combination of these regimens. In a random assignment clinical trial, financed by the National Institutes of Mental Health, the Multimodal Treatment Study for Children with ADHD (MTA) found a combination treatment of medication management and behavioral treatment and medication management alone produced superior results to behavioral treatment or routine community care (MTA Cooperative Group, 1999).

Despite the variety of treatment options, we focus on prescription stimulant medication for the following reasons. First, stimulants have been demonstrated to be extremely effective at controlling the symptoms of ADHD, but stimulants do not treat the underlying disorder or provide a cure for ADHD. As we document below, stimulants also have a number of potential negative side effects. Finally, prescription medications such as these are easy to identify in standard claims data bases.

Data from the Medical Expenditures Panel Survey indicates that roughly 3 percent of children under the age of 18 were prescribed stimulants such as Ritalin in 2002, which is roughly five times the prescription rate in 1987 (Zuvekas, Vitiello and Norquist, 2006). Visser, Lesene, and Perou (2007) note that in 2003 roughly 55 percent of children diagnosed with ADHD were taking stimulants. Using data from a large sample of privately-insured children, Castle et al. (2007) estimate that by 2005 4.4 percent of children aged 0 to 19 in their sample were using stimulants to treat ADHD, with usage rates increasing by roughly 12 percent per year over the 2000 through 2005 period. Zito et al. (2000) note a rapid increase in stimulant use among pre-school children.

Perhaps due to this striking increase in the diagnosis and treatment of ADHD, concern has been raised by the medical community, popular press, and parent support groups that this rise may be due to over-diagnosis. There is no pathognomonic marker for ADHD and the intensity of symptoms may fluctuate over time (Angold et al., 2000), making accurate diagnosis of ADHD difficult. Moreover, diagnosis of ADHD is often made without consulting a mental health specialist. Safer and Malever (2000) found that of Maryland public school students taking methylphenidate (i.e., Ritalin) at school 63 percent had prescriptions from pediatricians, 17 percent from family practitioners, and only 11 percent received a prescription from a psychiatrist. Diagnoses are generally made after a medical professional considers a child's behavior in multiple contexts, as reported by the parent, teacher, and child.

Stimulant use and ADHD diagnosis rates vary tremendously across groups of similarly defined youths, possibly suggesting that clinical guidelines for diagnosis are not being applied consistently. For example, researchers have found tremendous variation in stimulant use by children across regions of the United States,[3] by race and ethnicity, and by gender.[4] Comparing stimulant use among children in two southeastern Virginia cities, LeFever et al. (1999) found tremendous heterogeneity in stimulant use both within and between cities and conclude that the (p. 975) "criteria for diagnosis of ADHD vary substantially across U.S. populations, with potential over-diagnosis and overtreatment of ADHD in some groups of children." Similarly, in a study of children in the Great Smokey Mountains, Angold et al. (2000) found that the presence of ADHD symptoms is not well correlated with the treatment of ADHD through prescription medication and thus conclude that (p. 135) "stimulant treatment was being used in ways substantially inconsistent with current diagnostic guidelines."

This heterogeneity in diagnosis and treatment rates across gender and race has been documented in many settings. In a large-scale study specifically designed to assess the disparity in treatment, Safer and Malever (2000) collected data on all children that received medication for the treatment of ADHD during school hours in the State of Maryland in 1998. They found that the boys in elementary school were 3.5 times as likely to be receiving treatment as girls, and that black and Hispanic students were about half as likely to be receiving treatment relative to non-Hispanic white students.

Although these studies effectively demonstrate the heterogeneity in diagnosis and treatment rates across different demographic groups, it is difficult to know from these results whether this heterogeneity is a result of genetic or environmental factors, rather than a reflection of inappropriate diagnosis. Because the etiology of ADHD is not well understood, risk factors for ADHD are often based on population averages, such as a being male or having a lower socioeconomic status. While these population averages are somewhat consistent over time and across geographies, there is no clear medical evidence that higher diagnosis and treatment rates are due to a higher prevalence of the disorder in these populations.

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[3] Cox et al. (2003) demonstrated tremendous regional variation in stimulant use in a sample of children with private insurance.

[4] Castle et al. (2007) found that boys ages 0 to 19 were 2.3 times more likely to receive stimulant medications than girls in a comparable age range for a sample of children in a private prescription claims database. Visser et al. (2007) found gender and race/ethnicity are related to ADHD diagnosis, but not to ADHD medication treatment.

Comparing diagnosis rates across populations may confound issues such as access to and quality of care for any disease. This is particularly problematic for ADHD diagnosis (and the diagnosis of other mental disorders in childhood) since there is no objective clinical test.

The potential of inappropriate diagnosis and treatment is most troubling when considering the biological effects of the commonly prescribed stimulants. The side effects of methylphenidate use include insomnia, stomachache, headache, dizziness, and decreased appetite (Ahmann et al., 1993). More importantly, stimulants have been shown to increase heart rates and blood pressure (Nissen, 2006). Less is known about the longer term effects. Because the stimulants act to inhibit the dopamine receptors in the brain, there is some concern and speculation that long term changes in cell function might result from chronic exposure to stimulant medication, particularly during brain development in childhood and adolescence (Volkow and Insel, 2003). In addition to these important medical side effects of stimulant use, there is also an economic cost associated with diagnosis and treatment. Pelham et al. (2007) use a cost of illness framework to estimate the economic impact of ADHD and they conclude that the cost of ADHD is between $12,005 and $17,458 per child in 2005 dollars.

ADHD is often diagnosed after a teacher observes a child in his/her classroom and refers the parent to have the child evaluated. In a survey of physicians in the Washington, DC metro area, Sax and Kautz (2003) found that in 52 percent of all cases, teachers and other school personnel are the first to suggest a diagnosis of ADHD. It seems natural that teachers should compare the behavior of children within a class and recent research suggests that ADHD diagnosis rates are in fact correlated with the relative age of students within a class. In the most detailed study to date, Elder and Lubotsky (2009) used data from the Early Childhood Longitudinal Study – Kindergarten cohort (ECLS-K) to examine the impact of being older for a grade on a long list of outcomes. The Elder-Lubotsky paper serves as the template for our work in that they use the variation in student age generated by age of school start models to identify their model. Specifically, using an instrumental variables framework, the authors find that children who are an additional year of age older at school entry have superior educational outcomes. For example, these older children tended to have higher test scores and fewer behavioral problems. More importantly for our work, the authors demonstrated that starting school later reduces the chance of being diagnosed with ADHD by 50 percent.

This work is part of a larger literature in labor economics that explores the beneficial cognitive and labor market effects of being among the oldest children in the classroom. Many studies have exploited the variation in school start eligibility laws across states, over time, and even between countries.[5] For example, using international data, Bedard and Dhuey (2006) demonstrate that being young for your class produces lower test scores through the eighth grade. In recent work, Dhuey and Lipscomb (2010) find that relative age in the classroom causes a higher risk of being labeled as having a learning disability. Given this large literature on age effects and given the stark change in ADHD diagnoses rates based on age of school start found in the Elder and Lubotsky paper, we also suspect a similar disparity in stimulant use rates. In this paper, we replicate the basic results in Elder and Lubotsky (2009) using restricted-use data from the National Health Interview Survey and state data on age of school start legislation. We then extend these basic models to include data on stimulant use.

While completing the work for this paper, we came across the independent work of Elder (2009), who used the same techniques employed here and data from the ECLS-K to demonstrate that children born just before the state's age-of-start cutoff date are 50 percent more likely to be diagnosed with ADHD than those born just after. The robustness nature of the results across samples in this paper and the work of Elder is encouraging and suggests that the results presented below are not spurious but represent true misdiagnosis of ADHD.

III. Empirical Specifications

The primary question we consider is whether children that are older for their grade are less frequently diagnosed with and treated for ADHD. A similar set of questions has been addressed in a variety of disciplines about whether delayed entry into school helps or hinders academic promise. The underlying structural equation for both questions is essentially the same. Let the unit of observation be the individual child, indexed by i, and let Y_i be a dummy variable that equals 1 if a student is diagnosed (or treated) for a particular condition such as having developmental problems. The focus of this paper is ADHD and therefore, in our context, Y would equal 1 if a child is diagnosis (or treated) for ADHD. A student is defined as young for their grade ($Young_i$) if they are below some threshold age, such as the median, for children in the same state, grade, and year. The primary equation of interest is therefore

$$(1) \qquad Y_i = \beta_0 + x_i\,\beta_1 + Young_i\beta_2 + h(z_i) + \kappa_i$$

where x is a vector of observed characteristics and κ is a random error. The function h(•) is a smooth function in z, a variable that measures the difference in days between the child's birth date and the state cutoff date when that child was age five. Given a state with a September 1st age at start cutoff, a

[5] See, for example, Bedard and Dhuey (2006), Datar (2006), Elder and Lubotsky (2009), Dobkin and Ferreira (2007), Fertig and Kluve (2005), Goodman et al. (2003), Lincove and Painter (2006), McEwan and Shapiro (2009), Puhani and Weber (2007), Angrist and Krueger (1992), and references therein.

September 1st birth date would have a value of z=-1, a September 2nd birth date would be z=0 and an October 1st birth date would be have a value of z_i =29. Following previous RDD applications, we capture h(z) with polynomial terms in z and interactions of these polynomials with the treatment indication $I(z_i \geq 0)$.

If children of different ages were randomly assigned to classes, ordinary least squares (OLS) estimates of the parameter of interest (β_2) would be consistent. There is, however, good reason to suspect that single-equation estimates of equation (1) are subject to an omitted variables bias. Parents often decide their child is not ready for kindergarten and enroll their child in school later than others from the same birth cohort. This behavior is often referred to as "academic redshirting." If parents delay a child's entrance into kindergarten because they have difficulty sitting still or focusing on school work, which in turn signals a greater likelihood of an ADHD diagnosis in the future, then redshirting signals reverse causation from diagnosis to age relative to peers and OLS estimates of equation (1) would then understate the coefficient on β_2.

The available evidence suggests this is a real concern. West et al. (2000) estimate that during the mid 1990s, roughly 9 percent of students delayed entry into kindergarten. Males were 30 percent more likely than females to have delayed entry and children with diagnosed development problems were more than twice as likely as those without such diagnoses to have delayed entry. The number of academic redshirts and the role that developmental issues play in the decision suggests that estimating equation (1) by OLS will lead to inconsistent and potentially misleading estimates of the effect of relative age on ADHD diagnosis. Despite these concerns, many such models have been estimated in the past (Byrd et al., 1997; Sipek and Byler, 2001; Lincove and Painter, 2006).

We could obtain a consistent estimate for β_2 if we could somehow mimic random assignment and alter the relative ages of children in classes in a way that conveys no direct information about underlying ADHD incidence. In just this fashion, we use the distance between a child's birthday and the age at school entry as an instrument for relative age in class within a regression discontinuity design (RDD) or an instrumental variables (IV) model.

Children born a few days apart should be, on average, similar along all characteristics (e.g., underlying intelligence, parental backgrounds, home environment, etc.) yet because of age of school start laws these children will have vastly different ages when they start school. Consider a state that has a September 1st cutoff date. In this state, children born on August 31st are more likely to begin school as a five year old, but those students born just a few days later, on September 2nd, must wait a year to begin school. This age difference in a class is relatively large in early grades. Around the start of the school year, a class containing students with an August 31st and a September 2nd birth date will differ in age by 20 percent in kindergarten, 14 percent in second grade and 10 percent in fifth grade. The sharp break in age at school start generated by the interaction of child birthdates and the assumed similarity of children born just before and just after the school cutoff suggests that any observed difference in ADHD diagnosis and treatment between these two groups can be attributed to the difference in ages of the children in school.

IV estimates of equation (1) can be obtained in two steps. The initial step is to examine the first-stage relationship between age relative to the state cutoff date and the relative age in class. This model can be represented by the equation

$$(2) \qquad \text{Young}_i = \gamma_0 + x_i\,\gamma_1 + \gamma_2 I(z_i \geq 0) + h(z_i) + v_i\ ,$$

where h(z) and x are defined as above, v is a random error and the dummy variable $I(z_i \geq 0)$ equal 1 if the student has a birth dates after the age at school start. The impact of the age at start laws on whether the child is young for the class is captured by the parameter γ_2. The key assumption of the RDD model is that in the absence of the treatment (in this case, the student's birth date occurs after the school start cutoff) the outcome of interest is "smoothly" changing in z (the child's age). The polynomial h(z) captures the secular trend in the probability a child has the outcome Y_i. In our specifications, we model h(z) as a linear, quadratic, and cubic, and include an interaction with the treatment variable for a fully flexible model. Given h(z), we assume that people on either side of z_i are functionally identical, controlling for observable characteristics x.

The second step in the process is to examine the reduced-form relationship between a child's age relative to the school start dates and their diagnosis and/or treatment of ADHD. This relationship can be captured by the following equation

$$(3) \qquad Y_i = \alpha_0 + x_i\,\alpha_1 + \alpha_2\, I(z_i \geq 0) + h(z_i) + \zeta_i$$

where ζ_i is a random error and all remaining variables are defined as above. Given the assumptions above, the coefficient α_2 measures the impact of being born just after the cutoff on the propensity of students to experience the outcome Y_i. Because this is an exactly identified model with one endogenous variable, the IV estimate of β_2 in equation (2) is obtained by simply dividing α_2, the impact being born after the age of start on ADHD diagnosis, by the fraction of people impacted by the age of start (γ_2 from equation 2), or

$$(4) \qquad \hat{\beta}_2 = \hat{\alpha}_2 / \hat{\gamma}_2 .$$

Arithmetically, this is also equivalent to estimating equation (2) by two-stage least-squares and using $I(z_i \geq 0)$ as an instrument for $Young_i$.

The difficulty with equation (4) is that our data are not well suited for estimating the first-stage model outlined in (2). As we describe below, our large sample of private claims data, which measures drug use to treat ADHD, does not contain data on a child's current grade. The two nationally representative samples, the National Health Interview Survey (NHIS) and the Medical Expenditure Panel Survey (MEPS), do include some limited information on grade-level in school, but it is poorly measured. Respondents are asked the highest grade completed, which requires that we impute current grade by adding one to the recorded value for children currently enrolled in school. This is problematic for two reasons. First, we will overstate current grade for those who have completed but must repeat a grade. Second, as we outline below, it appears that some parents are reporting the child's current grade rather than the highest grade completed, meaning that by imputing the grade, we will have too many respondents that are young for their class.

To verify this point, we extracted a sample of children aged 7 to 16 from the 2000 to 2002 October Current Population Survey (CPS) data sets. These data contain a school enrollment supplement which identifies the current grade enrolled for all respondents. In Appendix Figure 1, we report the distribution of grades relative to age for this sample. Almost 70 percent of students are in a grade that is five years lower than their age (most eight year olds enrolled in school in October are in the third grade) with the next largest group enrolled in a grade that equals age minus six, and a few students are young for their grade, enrolled in a grade that is four years lower than age.

We compared these numbers to those who responded to the NHIS in the fourth quarter of the year. For this sample, we take data from the 2000-2002 NHIS, and use reported month and year of birth to impute the respondent's age as of October 1st to make this sample as comparable as possible to the October CPS. We add one year to the highest grade completed in order to estimate the current grade enrolled. Graphing the implied distribution of grades for age from this sample in Appendix Figure 1, we see that the NHIS overstates by a factor of three the number of students that are young for their class (in grade=age-4) and understates by 40 percent the fraction who are older for their class (in grade =age-6).

In practice, the systematic measurement error in the NHIS will tend to understate the first-stage coefficient γ_2, which will overstate the implied IV estimate in equation (4). For this reason, we will not estimate the instrumental variables model suggested by equation (1).[6] Instead, we rely on the results from

[6] While this limitation could theoretically be addressed by using the two-sample instrumental variables procedure of Angrist and Krueger (1992), it is difficult to isolate the appropriate population given the confidentiality of our datasets.

the reduced-form models in equation (3) to signal the casual relationship between being young for class and ADHD diagnosis and treatment.

There is both between state variation in the age at school start and within state variation in these laws over time.[7] A summary of the cross-sectional and time series variation in these laws is shown in Table 1. Seven states (CO, MA, NH, NJ, NY, PA, and VT) had no statewide age at school entry law in 2005, but rather allowed local education authorities (LEA) to determine age at school entry standards. Twenty-five states (including the District of Columbia) have had the same age at school start date since 1984, while the rest have had changes at some point in the period. In the 2005/2006 school year, the age at school start cutoff dates vary anywhere from July 1st in IN until January 1st in CT.

IV. Data

The data requirements for the RDD model outlined above are substantial. Naturally we need a data set that identifies whether a child has been diagnosed with ADHD and/or whether that child uses a prescription stimulant medication to treat ADHD. In addition, we must identify a child's exact date of birth and state of residence so that we can calculate his/her age relative to the kindergarten eligibility cutoff date. These last set of descriptors are identifying variables that are not typically available on public use versions of data sets. Consequently, we estimate the empirical models on three separate restricted-access data sources: the National Health Interview Survey (NHIS), the Medical Expenditures Panel Survey (MEPS), and a private insurance prescription drug claims data set. Even though our data cover different time periods and populations, we find similar results in each data set, confirming the robustness of our findings.

The NHIS is an annual survey of roughly 60,000 households that collects data on the extent of illness, disease, and disability in the civilian, non-institutionalized population of the United States. The NHIS includes detailed demographic and socioeconomic information, as well as the self-reported medical conditions of respondents. Information on ADHD diagnosis has been included in the Sample Child Supplement within the NHIS since 1997. Our empirical strategy relies on the ability to identify the exact cutoff date that each child faced when they first entering kindergarten, plus their birth date. We therefore use the more detailed geographic data and the exact date of birth that is available only in the restricted use version of the NHIS.[8] The dependent variable for the NHIS analysis is the child's parent's report of

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[7] For a discussion of the individual state statutes and a detailed breakdown of the age of school entry laws in the U.S. from the early 1980s through the present time, see Morrill (2008).

[8] These data are available for use through the National Center for Health Statistics Research Data Center: http://www.cdc.gov/nchs/r&d/rdc.htm. We access the data at the Triangle Census Research Data Center through a data sharing agreement made between the Census Bureau and the National Center for Health Statistics.

whether the child has ever been diagnosed with ADHD by a doctor or health professional. ADHD incidence rates from the NHIS are comparable to results from other national surveys from similar periods.

Our second data source, the Medical Expenditure Survey (MEPS), is a series of surveys administered since 1996 by the Agency for Healthcare Research and Quality and the National Center for Health Statistics. The MEPS sample is drawn from the NHIS sample, although there are restrictions on merging these two datasets. There are three components of the survey completed by households, medical providers, and insurance companies. Individuals are asked questions over a series of five rounds detailing two years of medical expenditures and services utilization. Each year of the MEPS contains respondents from two overlapping panels. The MEPS full year consolidated data file (CDF) contains socio-demographic information for respondents including age, sex, race, and basic economic characteristics, plus their date of birth. We have access to the restricted version of the MEPS, which allows us to identify the exact eligibility as described above.[9] While the MEPS is a smaller sample than our private claims data, as with the NHIS, it has the advantage that it contains children with any health insurance type, including those that are uninsured. The dependent variable for this part of the analysis is whether a child has received a prescription for a stimulant to treat ADHD. We rely on the ICD-9 codes that identify whether the child received any medication for the treatment of ADHD (ICD-9 code 314).[10]

We have also obtained a proprietary claims data base constituting private insurance contracts for nearly 1 million covered lives and representing at least 40 of the 50 U.S. states. The data set contains claims and health insurance enrollment data for the 2003 through 2006 years of service. The data provide specific information on an insured's date of birth, age, gender, zip code of residence, insurance contract type (e.g., single, two person, family) and premium paid by the insured. Claims data elements of interest include date of service, ICD9 diagnosis and CPT4 procedure code (if medical care) and NDC drug code (if pharmacy). In addition, the pharmacy data provides information on days of supply and refill rates. Both medical and pharmacy data describe the amount paid by the insurer as well as the insured. All of the insured ID information has been encrypted and stripped of any identifying information.[11]

When using the private claims data, the dependent variable is whether the child had a claim for a prescription drug that is typically used to treat ADHD. Although Ritalin is the most common drug

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[9] The restricted access MEPS is available at regional Research Data Centers through a data sharing agreement made between the Census Bureau and the Agency for Healthcare Research and Quality. We access the data at the Triangle Census Research Data Center.

[10] Note that we only include medication that was not imputed and that was recorded as being for the primary diagnosis of ADHD. Relaxing these two restrictions increases the mean rate of treatment but does not affect the qualitative conclusions from the regression results.

[11] Because the encrypted Social Security number was missing for a number of dependent children, we could not use that variable to uniquely identify respondents in this sample. Instead, we used the employee's Social Security number and the dependent's date of birth, which necessitated that we delete twins and higher parity births from the sample. Our results are not sensitive to this restriction.

prescribed to treat ADHD, there are many drugs on the market and in recent years, several new drugs have been developed to treat this condition. We identify stimulants through the National Drug Codes (NDC) which are 10-digit, 3-segment numbers that identify the manufacturer, item and size/type, respectively. The list of stimulants includes popular drugs such as Ritalin, Metadate, Methylin, Daytrana, and Concerta (methylphenidate), Adderall (amphetamine and dextroamphetamine), and Dexedrine (dextroamphetamine).

We do not pool these three datasets together, but rather present estimates from each separately. The NHIS includes a measure of diagnosis only. The private claims data only measure prescriptions, not diagnosis. The MEPS data also measure prescriptions, but for a nationally representative sample that is not directly comparable to the private claims sample. Because all three of our data sources have significant restrictions on accessing the data and reporting statistics, it is not possible to combine them. In order to ensure that the children in our samples are currently enrolled in school, in all three samples we restrict our attention to children ages 7 to 17. Most states require that children ages 7 to 17 be enrolled in public school full-time. We also limit the sample to those observations where there was a state-wide age at school start law in force when the child was five years of age. We include in the sample only children born within 120 days of the school eligibility cutoff date in their state and year. The final estimation sample used from the NHIS includes 35,343 children. The final sample size from the MEPS is 31,641 observations representing 18,559 children.

Given the geographic distribution of the insurance company and eliminating states with no age at school start law, and states with less than 200 person/year observations, in the private claims data, we are left with 48,206 observations from 32 states representing data for 22,371 children aged 7 to 17. Although these data are for individuals with private health insurance and therefore are not nationally representative, having a sample this large enables us to get more precise estimates and to explore potential heterogeneity in the effects across gender and age. One limitation of the claims data relative to our other two datasets is the lack of demographic information outside of gender and age. However, as we indicate below where we test the sensitivity of our results to the inclusion of a richer set of covariates in the MEPS and NHIS samples, because people born just before and after the age of school start dates are similar on observed dimensions, the addition of demographic controls does not materially alter the statistical results.

Note that ideally we would like to have information on what state the child was residing in during the fall of the year they turned five. We do not have this information in any of the three data sources. In all three we do observe the current state of residence. The NHIS also includes the child's state of birth.[12]

[12] In the NHIS, approximately ten percent of the sample is missing state of birth. Of those that have both state of residence and state of birth, approximately 10 percent report being born in a different state than they currently reside.

In the empirical section we present results that confirm estimates are not sensitive to using state of birth rather than state of residence or restricting to children who reside in the same state in which they were born. Not having state of residence at age five is not a significant limitation for two reasons. First, there is little cross-state movement among school-aged children. In a sample of children age 6 to 18 from the 2000 Census 1Percent Public Use Micro Samples (PUMS), only 7.7 percent moved across state lines in the past five years.[13] Interstate moves will only contaminate the analysis if they occur differentially for children born just before or after the age of start cutoff. We have limited information on this fact to date, but data from the 1980 Census 1Percent PUMS indicates that there is little variation in within state moves based on a child's quarter of birth. In that sample, we estimate that among children 6 to 18 years of age, the fraction that moved in the past five years for those born in quarters 1 through 4 are 4.5, 4.7, 4.7 and 4.5 percent, respectively.[14] The small fraction of children that move after they start school and the lack of large variation across birth quarters suggest that not having the state of residence at the time a child enters school should not bias our results.

V. Results

Table 2 reports sample means and descriptive statistics for each of the three different data sets. In each case, we begin with a sample of children aged 7 to 17 on June 1st of the survey year. We call this our full sample. Although incidence rates vary by gender, we begin by initially pooling results for males and females. Next, to create the regression sample, we first restrict each sample to children who live in states with a clearly defined kindergarten eligibility cutoff date.[15] Table 2 demonstrates the effect of restricting the sample in this way. While the percent male and average ages are identical in the full and eligible state sample, there is a slightly higher incidence of ADHD diagnosis and treatment in the states used for analysis. As was discussed in Section II, this is consistent with the geographic variation in ADHD treatment and diagnosis rates widely documented in the literature. Next we further restrict the sample to children whose birth date is within 120 days of the cutoff date. While this effectively removes one-third of the sample, we find that the regression sample is very similar to the eligible states sample in each data set. Note that because the private claims data are from a later time period and are, by definition, for a sample of children with private health insurance, we find higher rates of stimulant use than in the MEPS.

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[13]Author's calculations from the Census PUMS files.

[14]Author's calculations from the Census PUMS files.

[15] Data confidentiality restrictions prohibit the delineation of which states are included in these tables. We have a large enough sample from many states and years to assure a reasonably representative population.

The last two columns of Table 2 demonstrate the basic relationship hypothesized above when comparing the fraction of children with an ADHD diagnosis for those born before the cutoff date (young-for-grade) and children born just after the cutoff date. Notice that in all three data sets the samples of children born just before the cutoff date have nearly identical demographic characteristics when compared with children born just after the cutoff date. However, we find large differences in ADHD diagnosis and treatment rates. In the NHIS, children born before the cutoff experience a 9.7 percent diagnosis rate compared with only 7.6 percent for those born after. Stimulant usage in the MEPS indicates a 0.5 percentage point difference between children born before and children born after the cutoff date. Similarly, in the private claims data the percentage of children with any stimulant use drops from 6.5 percent to 5.2 percent across the kindergarten eligibility cutoff date.

Figure 1 presents the graphical equivalent to the means presented in Table 2 and described above. We see that the difference in ADHD diagnosis and treatment rates is large for all samples in all three data sets.[16] In Figure 2 we present means for progressively smaller sample of children, those born within 120, 60, and 30 days of the kindergarten eligibility cutoff date, respectively. Note that the NHIS is measuring diagnosis, while the MEPS and private claims data include only children receiving prescription stimulants to treat ADHD.

Figure 3 presents a similar design using six different common childhood ailments found in the NHIS and two other classifications of drugs in the private claims data. The pattern shown in Figure 1 is unique to ADHD; there is no statistically significant difference in means across kindergarten eligibility cutoff dates for any of these other childhood diseases and other common children's prescription medications. In Figure 4 we present a graphical display of the reduced-form model in the NHIS, namely, the impact of being born after the cutoff on being diagnosed with ADHD. In this figure, we see around a 2 percentage point difference in incidence rates between those children that were born just before the cutoff date when compared to those born just after, which is about 25% of the sample mean. In Table 3 we present the regression equivalent to this figure for each data set.

As discussed in Section III, the data are not sufficient to specify a full instrumental variables specification because grade level is not well measured in the NHIS and MEPS and not measured at all in the private claims data. The NHIS includes a variable measuring the last grade completed for each household member, but this value may not be well defined for children currently enrolled in school. In Section III, when comparing data from the 2000-2002 public use NHIS and data from the 2000-2002 October CPS, we found that the NHIS underestimates the last grade completed. This comparison is illustrated in Appendix Table 1.

[16] The differences are statistically significant, results available upon request.

Although we believe that grade level is measured with considerable error such that the first-stage estimates will suffer from attenuation bias, in Figure 5 we illustrate that our instrument, days from the eligibility cutoff, does influence relative age. Using the NHIS restricted data we first define the child's grade as of January 1st of the interview year. To do this, we add one to the last grade completed for those interviewed in the first or second quarter.[17] We then drop observations where the grade level is greater than 12 or where the grade is more than three years from the age-appropriate grade level.[18,19] Given these restrictions, the sample size for Figure 5 is 34,173 children. As in Equations (1) and (2) in Section III, we define *Young* as an indicator for whether the child is below the median age in her grade by state by year cell.

Figure 5 clearly shows that children born after the kindergarten eligibility cutoff date in their state by year are considerably less likely to be young for their grade. The difference is approximately 30 percentage points. In results not shown, the regression equivalent of Figure 5 indicates that children born after the cutoff are 38.4 percentage points (standard error of 0.022) less likely to be younger than the median age in their state by grade by year cell. Adding controls does not change this number appreciably. If compliance with the kindergarten eligibility cutoff dates were perfect and if grade level was perfectly measured, the coefficient on *born after* should be 100 percent. It is not possible to know within our data the extent of non-compliance with the kindergarten cutoff dates. Parents may choose not to enroll an eligible child or may apply for a waiver to allow an ineligible child to enter early. Also, children who are immature may be held back or children who are developmentally advanced may skip a grade. These choices would result in the instrument, *born after*, having less predictive power for relative age, *Young*. Using data from the Early Childhood Longitudinal Study (ECLS) and the National Education Longitudinal Study (NELS), Bedard and Dhuey (2006) found that in the United States relative age (birth month relative to the school cutoff date) predicts the observed age. For the sample of 4th graders from the ECLS, they found a coefficient of 0.774, while the 8th grade sample from the NELS had a coefficient of only 0.438. These results suggest that compliance with the cutoff date declines as children age, potentially due to grade retention or promotion policies. Although our results clearly indicate that date of birth relative to the cutoff date is an important determinant of relative age, gaining an exact measure of the first stage estimate is not possible within our data. We have therefore chosen to present only the

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[17] In quarter 2 we only add one year if the interview month is May or earlier (when available) or assignment week 9 or earlier (when available). The results are not sensitive to these adjustments. The difficultly in determining when the school year would have ended, and thus when the "last grade completed" is equal to the "grade level on January 1st," illustrates the larger problem that grade level is not well measured in the NHIS.

[18] Recall the sample consists of children ages 7 to 17, where age is defined as the child's age on June 1st of the survey year. The age range allowed in each grade is: Grade 1 (Age 7-9), Grade 2 (Age 7-10), Grade 3 (Age 7-11), Grade 4 (Age 7-12), Grade 5 (Age 7-13), Grade 6 (Age 8-14), Grade 7 (Age 9-15), Grade 8 (Age 10-16), Grade 9 (Age 11-17), Grade 10 (Age 12-17), Grade 11 (Age 13-17), and Grade 12 (Age 14-17).

[19] Note that the measure of age relative to median uses age measured in days.

reduced form model, as illustrated in Figure 4. To the extent that compliance is not perfect, the reduced form coefficients will underestimate the relative age effect.

The main NHIS results are presented in the top panel of Table 3. Each column includes additional covariates. Because the mean demographic characteristics do not differ across the cutoff date, as reported in Table 2, it is not surprising to note that adding covariates to the model does not significantly affect the coefficient of interest. In column 4 we include a linear polynomial in the variable z, as in equation (1). This include the z variable itself plus an interaction of z times the treatment dummy $I(z_i \geq 0)$. The estimate reported in column 4 indicates that children born in the 120 days after the cutoff have a 2.1 percentage point lower probability of being diagnosed with ADHD. This corresponds to approximately 24 percent of the average diagnosis rate across the sample.

Next, results using data from the MEPS are presented in the middle panel of Table 3. We find that being born after the cutoff leads to between a 0.6 and 0.8 percentage point reduction in the probability of being treated for ADHD. This is approximately 13 to 19 percent of the mean treatment rate of 4.3 percent in the sample. Note that in column 4 the results become imprecise and not statistically significantly different from zero once the linear polynomial is included, although the magnitude of the coefficient does not change much.

The bottom panel of Table 3 presents the equivalent set of results using the private claims database. As in our other data sets, we find that the estimates are not sensitive to the inclusion of demographic characteristics, state and birth cohort fixed effects, or a linear polynomial in days from cutoff. The baseline result in column 4 indicates that children born just after the cutoff experience a 1.6 percentage point lower risk of receiving stimulants to treat ADHD, approximately 27 percent of the average rate of stimulant usage. The main results reported in Table 3 indicate a large and robust relationship between being born after the kindergarten eligibility cutoff date and being diagnosed with or receiving prescription treatment for ADHD. We find that being born after the cutoff, and therefore being relatively old for grade, is associated with an 13 to 27 percent lower risk of ADHD treatment and a 24 percent lower risk of ADHD diagnosis.

To explore the robustness of these findings, we perform a variety of specifications checks. It should be noted that for there to be an effect of age relative to the cutoff date on treatment or diagnosis two relationships must be present. First, it must be the case that the kindergarten eligibility laws influence enrollment behavior and therefore age for grade, which is demonstrated in the first-stage regression discussed above. Second, relative age must determine diagnosis and/or treatment for some portion of the population. Given the statistical significance found in Table 3, we can infer that both effects are occurring and that there is medically inappropriate diagnosis. It is important to consider heterogeneity in the results to determine whether this average effect is in fact concentrated among a very selected or unusual portion of the population. Often in the context of instrumental variable estimation this issue is referred to as determining the Local Average Treatment Effect (LATE), implying that the effect is only measured for individuals that are responsive to the instrument.

In our analysis, we would like to confirm whether the inappropriate diagnosis and treatment we detect is seen across subsets of the population, as well as to confirm whether the empirical results hold with alternative specifications. However, caution must be used in interpreting the relative size of coefficients. It may be the case that some populations are more compliant with the instrument. For example, we know that girls are much less likely to be held back in kindergarten than boys, so are more compliant with the instrument. In that case we might expect to find larger differences across the eligibility cutoff dates, since those dates were more binding for girls than boys.[20] However, it might also be the case that relative age is less important for girls than for boys, due to faster maturation of young girls. In that case, we would expect to see a smaller effect of relative age for girls than for boys. Theoretically, then, it is not obvious whether the coefficient for girls should be smaller or larger than that for boys, or how to interpret any differences between the two. We therefore present these results merely to explore whether the effect holds in subpopulations, but strongly caution against interpreting differences in the coefficients as indicating a stronger or weaker relative age effect. It may simply be that our instrument is more effective at determining relative age for some populations than others.

Table 4 presents the specification checks for ADHD diagnosis using the NHIS data. Since the coefficient of interest did not change across the columns of Table 3, it is not surprising that in Table 4 we find the estimate is robust to a host of specification checks. These results use the same specification as Table 3, column 4, repeated in the top row of Table 4 for comparison. First, we restrict the window of the sample to children born within 90, 60, and 30 days of the cutoff date. While the estimates become less precise as the sample size decreases, we find that the effect of being born just after the cutoff is an approximately 1.8 to 3.2 percentage point decrease in the probability of being diagnosed with ADHD. This effect is again 21 to 37 percent of the total ADHD diagnosis rate. The results are also insensitive to including higher order polynomials. These results confirm that the findings cannot be due to season of birth effects.[21]

The third set of sensitivity tests demonstrates that approximating state of residence at age five with state of birth rather than current state of residence produces nearly identical estimates. When we restrict the sample to children that report being born in the same state where they currently reside, a

[20] Indeed, in results not shown, the "first-stage" estimates of the effect of being *born after* on being *Young* are 0.4 for girls compared with 0.3 for boys.
[21] Note that in results not shown but available upon request, estimates are similar when birth month fixed effects are included.

sample much more likely to have been living in that same state at age five, we again find that the results are nearly identical.

Next, we explore the heterogeneity of the estimates across subsets of the population. Note that we include the mean of the dependent variable in the table, which highlights the large differences in diagnosis rates across different groups. We find that nearly 13 percent of boys have ever been diagnosed with ADHD, compared with 5 percent of girls. However, we see a similar effect of being born after the cutoff for both boys and girls. This result is not found in our other data sets, where the girls sample does not experience statistically significant effects. The estimated effect for girls is not only slightly larger in magnitude, but is considerably larger in percentage terms. Note that there may be a power loss when attempting to detect smaller effects on treatment rates, as described below in the discussion of Table 5. When comparing between different racial/ethnic groups, we find the highest rates of ADHD diagnosis among white non-Hispanic children. While the mean diagnosis rates differ by race, we again find similar coefficients on being born after the cutoff in all samples, with the largest effects for children with Hispanic ethnicity.

So far our estimates have pooled together children ages 7 to 17. Because a one year difference in age represents a larger fraction of a child's life at younger ages, we might expect that the relative age differences cause larger effects for children ages 7 to 12 compared to teenage children. Note that although all specifications do include child's age and birth cohort fixed effects, we may still find that the rising rates of ADHD diagnosis lead to a larger estimate for the younger age group due to year effects as well.[22] In Table 4, comparing across age groups we find that the largest effect is seen for the youngest age group in the sample.

We then divide the sample into survey years 1997 to 2001 versus 2002 to 2006. Consistent with other studies we see that ADHD diagnosis rates rose between these two time periods from 8.0 percent to 9.3 percent, or about a 16 percent rise. The effect of being born after the cutoff is larger in the later time period, 2.3 percentage points (25 percent) versus 1.8 percentage points (23 percent) in the earlier time period. This result suggests that the effects of relative age on inappropriate diagnosis may be increasing over time as ADHD diagnosis and treatment become more prevalent.

Because our dependent variable is dichotomous, we confirm that using a limited dependent variable model produces nearly identical results. The second to last panel in Table 5 provides the marginal effects from a probit model. Finally, the bottom panel of Table 4 estimates a similar model

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[22] Note that in results not shown, similar to the findings of Bedard and Dhuey (2006) discussed above, we find that for children age 13 to 17 the first stage coefficient is only -0.28 compared with a coefficient of -0.41 for the children age 7 to 12. This is consistent with eligibility being less binding as children age due to differential promotion and retention. It may also be due to using current state of a residence as a proxy for the state where the child lived at age five. As children age it will be more likely that they have moved since age five, so an additional form of measurement error is introduced which may cause some attenuation bias.

using other childhood diseases as outcomes, as in Figure 2. Because children born before the cutoff will have experienced more years of school on average, one might worry that it is exposure to years of school, rather than relative age, that is causing the difference in diagnosis rates.[23] The first two childhood ailments we consider as falsification tests, chicken pox and respiratory allergies, may also be a function of years of exposure to school. Another concern might be that the stress of being younger than one's classroom peers actually causes ADHD. Although we are not aware of any evidence that ADHD is stress-induced, we explore the possibility that children who are relatively young may suffer from stress-induced ailments. To test if a stress-induced mechanism is at work, we consider other childhood ailments that may be exacerbated by stress. For all four childhood ailments we consider, chicken pox, respiratory allergies, hay fever, and frequent headaches, we find no statistically significant effects of relative age. This also further confirms that differences in susceptibility to diseases by children born at different times of year cannot explain the effects.

In all the estimates of the effect of being born after the age of school start date are large and statistically significant across a host of specifications and in almost all subsamples. We find no similar effect for four other childhood diseases. This suggests that the nature of the diagnostic guidelines, which recommend a comparison with classroom peers, leads to medically inappropriate ADHD diagnosis.

Table 5 reports a similar set of specification and heterogeneity checks considering stimulant prescription as the outcome of interest. In Table 3 we found that the estimated effect of being born after the cutoff was strikingly similar across the specifications as additional covariates were added for all three datasets. The private claims data source has a large enough sample size to explore alternative specifications and heterogeneity within the sample. However in the MEPS data, the main result, presented in column 4 of Table 3, is not statistically significant. Still, we explore whether the qualitative results in the MEPS hold across specifications and within subsamples as further evidence supporting the findings in the larger private claims data. Although it is a smaller data set, the MEPS sample is nationally representative and allows for controls for race and ethnicity.

The top row of Table 5 repeats the main specification, Table 3 column 4, for the MEPS and private claims data sets. In the first set of specification tests, we find that the coefficient is insensitive to narrowing the window of birth days included in the sample. In the next panel of results in Table 5 we include higher order terms of the polynomials h(z). The specification with the quadratic yields a puzzling result. Using the MEPS data we find the coefficient on *born after* more than doubles, while in the private claims data the coefficient goes to zero. Once higher order terms are added the coefficients are again similar to the baseline result. Recall also that this anomalous result is not found in the NHIS results

[23] Note that in results not shown but available upon request, estimates are similar when grade fixed effects are included.

reported in Table 4. Note that Porter (2003) argues that odd-numbered polynomials have better econometric properties in regression discontinuity design models.

We next consider heterogeneity within the samples. As was found with diagnosis rates, treatment rates for boys are much higher than for girls in both samples. The effect of being born after the cutoff is only statistically significant for boys in the private claims data and reflects an over 2 percentage point decreased risk of ADHD treatment for boys born just after the eligibility cutoff. Note that the estimate for girls is not statistically significant, but this may simply be due to insufficient power. Again similar to the estimates in Table 4, we find that the effect is largest for children ages 7 to 12. Near the bottom of Table 5 we estimate a model in the MEPS data for a subset of the population that is most similar to that from the private claims data base. We find that among children with private health insurance in survey years 2003 to 2006, 4.8 percent have a prescription medication to treat ADHD. We find that children born just after the cutoff date have a 2.7 percentage point lower risk of being treated for ADHD in this group.

Finally, we consider another set of falsification tests at the bottom of Table 5. Similar to the tests reported in Table 4, if the stress-induced illness or exposure to school mechanism were influencing ADHD treatment, we would expect to see a negative and significant effect of being born after the cutoff on asthma medication use or antibiotic use as well. The estimates at the bottom of Table 5 show a positive and statistically insignificant effect of relative age on asthma medication and antibiotic use.

In summary, if one assumes that the true incidence rate of ADHD is uniform over a small window around the age at school start cutoff, these estimates provide compelling evidence that a large fraction of ADHD diagnoses are not the result of an underlying medical condition. Rather, children that were born just after the kindergarten eligibility cutoff date in their state in the year they turned five years old, who therefore were more likely to wait an additional year to enter school, are at a much lower risk being diagnosed with ADHD and being prescribed stimulants. This provides strong evidence that medically inappropriate diagnosis and treatment is occurring.

The diagnosis rates for children born on either side of the kindergarten eligibility cutoff date should only be different if that cutoff date actually corresponds to initial school enrollment behavior. Not only do many states allow exemptions for early entry, in general states do not require children attend school until they are seven years old. In addition, more advanced children may skip grades, while children who are struggling may repeat grades. This non-compliance with the age at school start laws should only serve to dampen the difference between children born before and after the cutoff date. As described above, we cannot estimate the effect of being relatively young directly due to data limitations. Still, the reduced form analysis presented here indicates that, as long as the underlying medical risk of having ADHD does not differ across the eligibility cutoff date, there is a significant amount of medically inappropriate diagnosis and treatment of ADHD.

VII. Conclusions

The evidence presented above indicates that for some children, a diagnosis of ADHD is not solely based upon underlying biological conditions. Rather, relative maturity influences ADHD diagnosis. This is not a surprise given the difficulty of diagnosing ADHD and the explicit consideration that health care providers are advised to give to whether the behaviors in question “happen more often in this child compared with the child’s peers?”[24] As Elder and Lubotsky (2009) demonstrate, younger children in classes are more likely to have educational and behavioral problems compared to their peers, and therefore, some children who are relatively young compared to their classroom peers are more likely to be diagnosed with ADHD. These results suggest that the comparison sample for diagnosis should not be other children in class but rather, other children of a similar age within a class.

According to a 2007 FDA review, the stimulant medication used to treat ADHD may have serious and significant side effects including cardiovascular problems and psychiatric problems. Others studies have suggested potential long-term consequences on young children’s brain development. According to our estimates, approximately 9 percent of all children are diagnosed with ADHD and approximately 4 to 6 percent of children current take a prescription stimulant to treat ADHD. According the population estimates provided by the U.S. Census Bureau,[25] on July 1, 2006 there were approximately 53 million children ages 5 to 17 in the United States. To put our estimates into perspective, an excess of 2 percentage points implies that approximately 1.1 million children received an inappropriate diagnosis and over 800,000 received stimulant medication due only to relative maturity. Recognizing the pattern of inappropriate diagnosis should help to better target treatments. In addition, this may help to avoid treatments with potentially serious short-term and long-term consequences.

International comparisons that indicate the United States spends more yet achieves lower health outcomes when compared to other OECD countries. This and other evidence has prompted criticism of wasteful spending and over-treatment in the U.S. healthcare system. However, identifying inappropriate diagnosis and treatment can be difficult and generally involves costly chart reviews or extensive case studies. In this paper we document inappropriate medical diagnosis and treatment using survey data. Using variation in relative age induced by age of school start laws, we are able to clearly identify a source of differential diagnosis that cannot be due to true underlying differences in disease incidence.

[24] NIMH, ADHD Booklet, Page 6.
[25] Source: Population Division, U.S. Census Bureau, Table 2: Annual Estimates of the Resident Population by Sex and Selected Age Groups for the United States: April 1, 2000 to July 1, 2008 (NC-EST2008-02), Release Date: May 14, 2009, accessed November 16, 2009.

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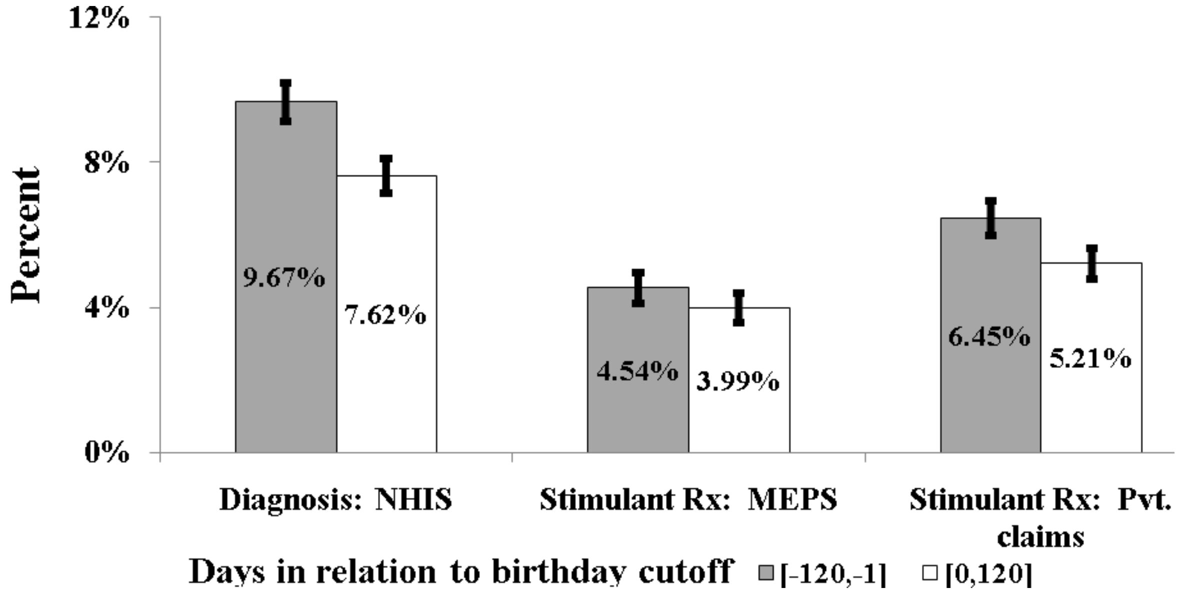
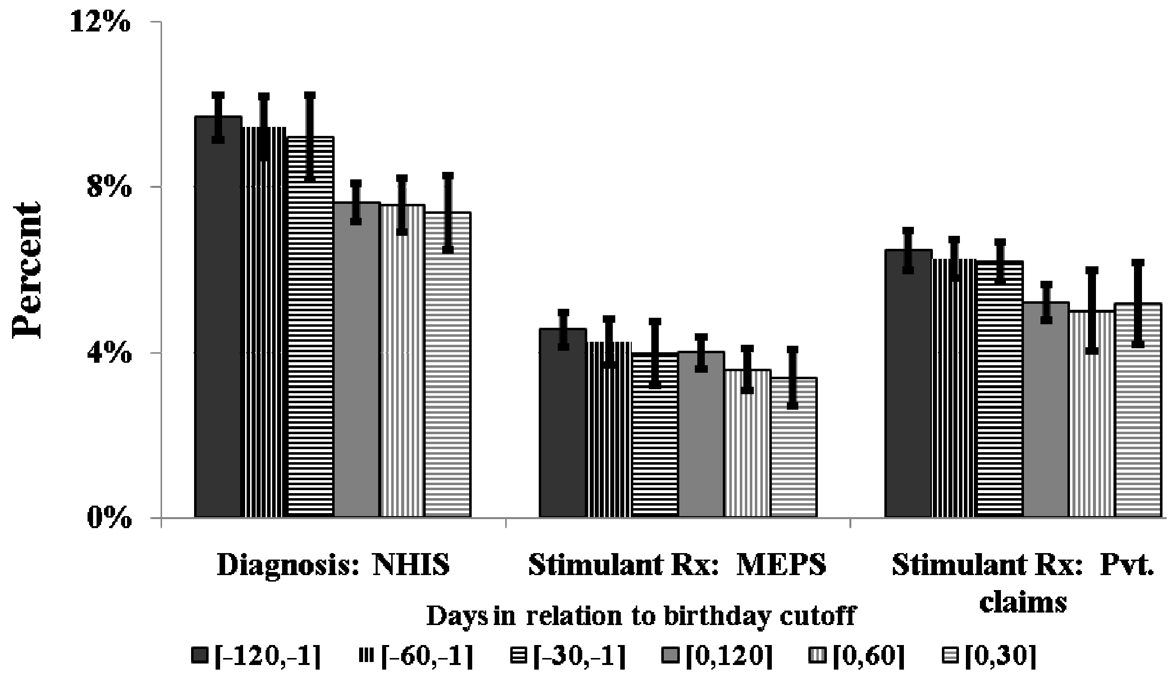
Figure 1: Means and 95% Confidence Intervals for Children Born Before and After Cutoff Dates



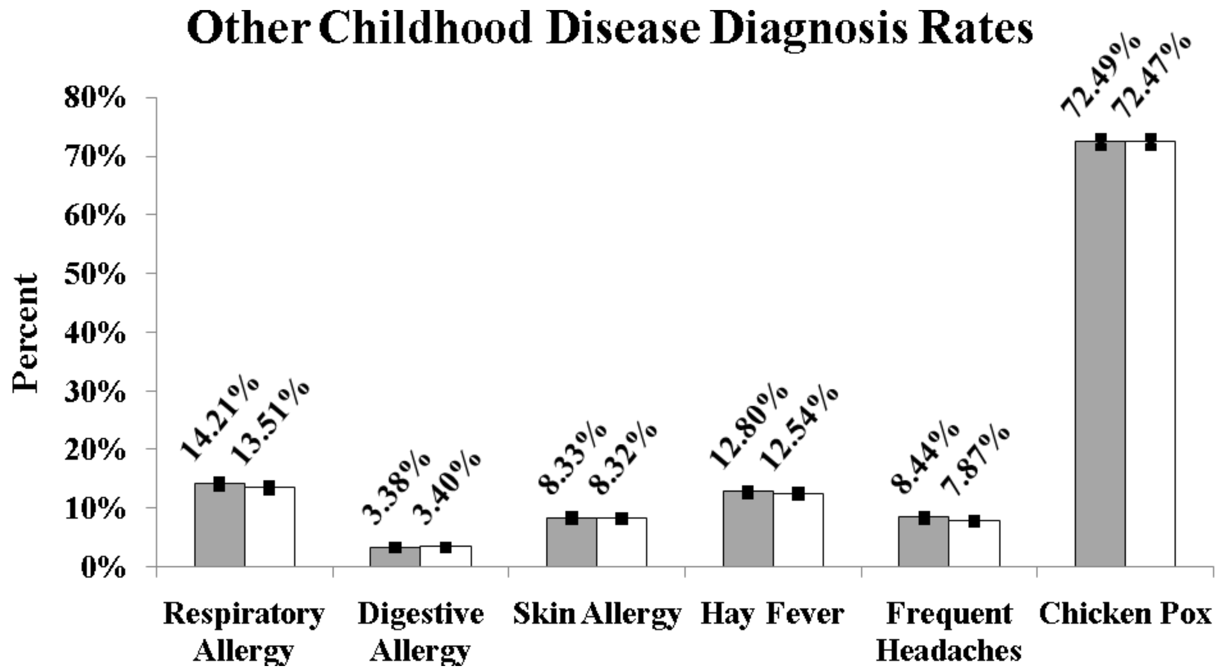
Notes: The means for children born before (dark color) versus after (light color) the kindergarten eligibility cutoff date in their state of residence is shown for children born within 120 days of the cutoff date. Data are from the restricted-access versions of the 1997-2006 National Health Interview Survey (NHIS), the 1996-2006 Medical Expenditure Panel Survey (MEPS), and a private insurance claims dataset. The sample includes children ages 7 to 17 on June 1st of the survey year born within 120 days of the kindergarten eligibility cutoff. The NHIS and MEPS results are weighted means.

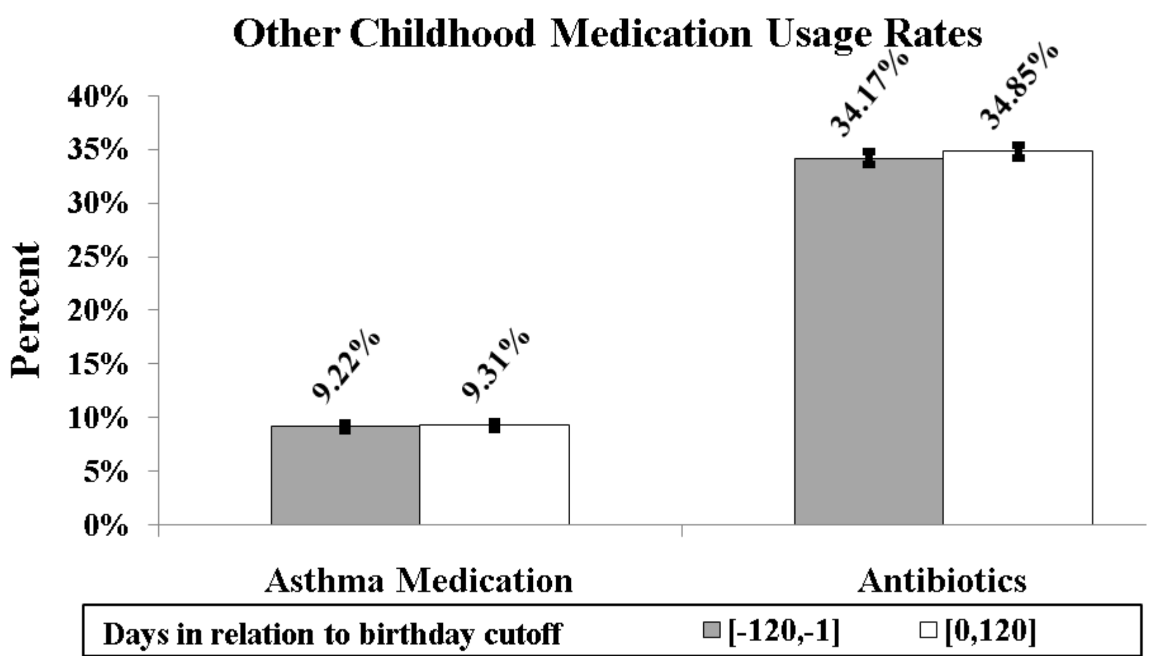
Figure 2: Means and 95% Confidence Intervals for Children Born Before and After Cutoff Dates



Notes: The means for children born before (dark color) versus after (light color) the kindergarten eligibility cutoff date in their state of residence is shown for children born within 120 (solid), 60 (vertical stripes), and 30 (horizontal stripes) days of the cutoff date. Data are from the restricted-access versions of the 1997-2006 National Health Interview Survey (NHIS), the 1996-2006 Medical Expenditure Panel Survey (MEPS), and a private insurance claims dataset. The sample includes children ages 7 to 17 on June 1st of the survey year born within 120 days of the kindergarten eligibility cutoff. The NHIS and MEPS results are weighted means.

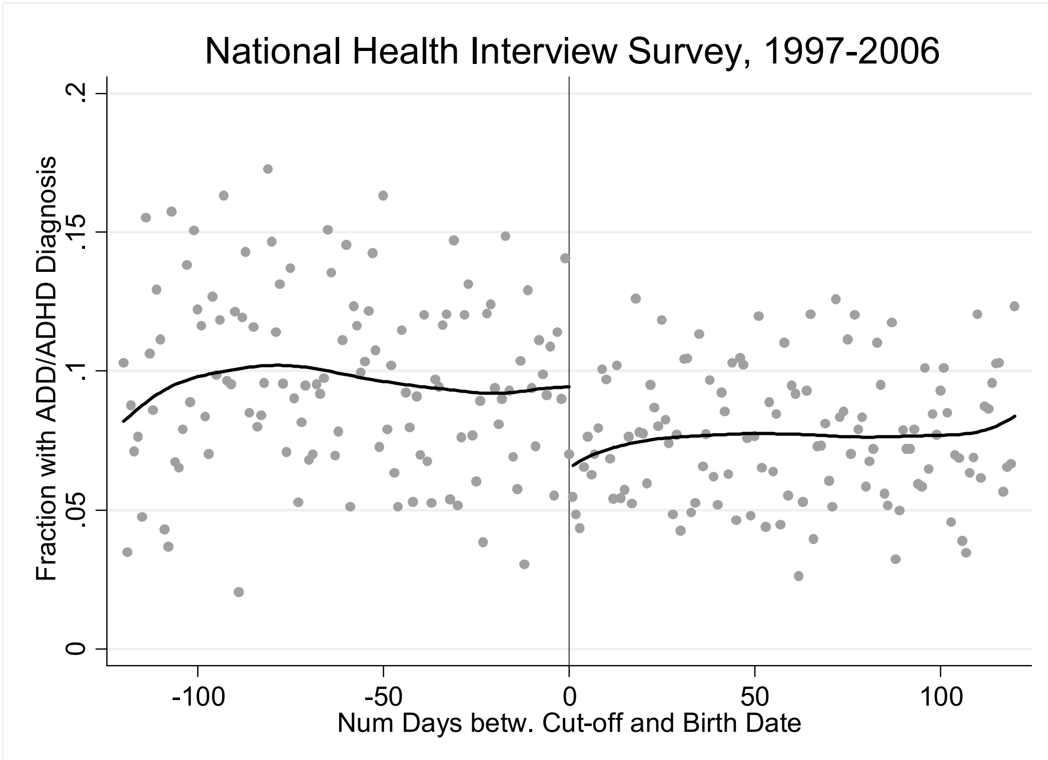
Figure 3: Falsification Tests, Means and 95% Confidence Intervals for Children Born Before and After Cutoff Dates





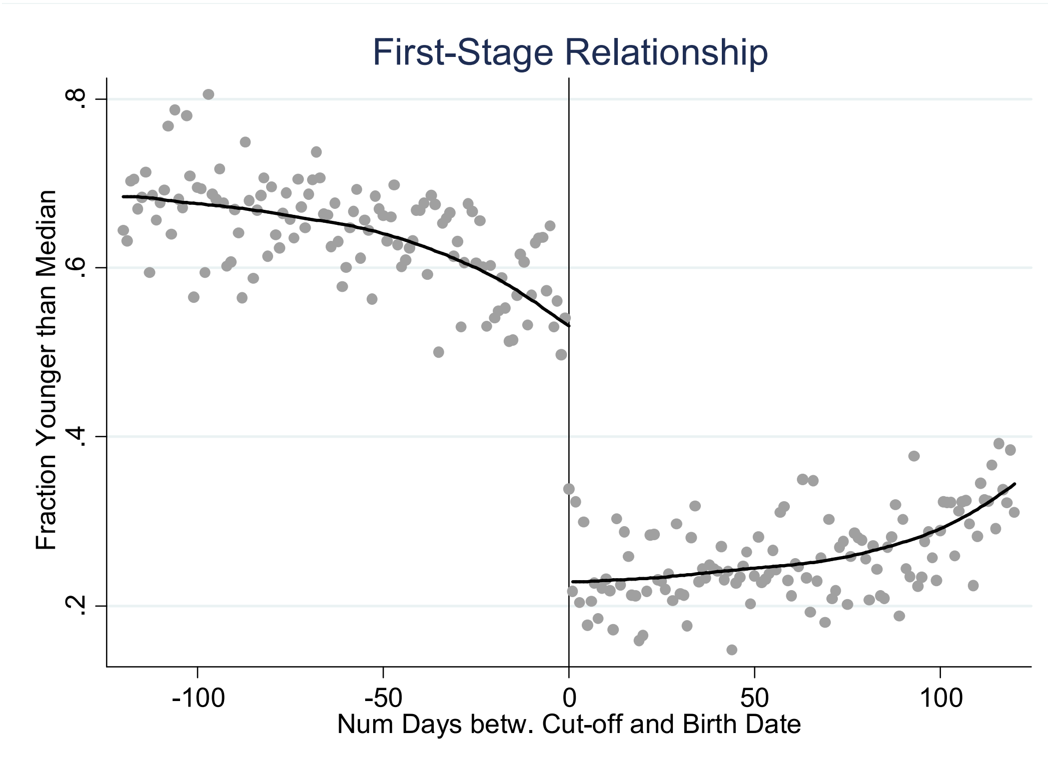
Notes: Childhood disease data are from the restricted-access National Health Interview Survey (NHIS) 1997-2006. Childhood medication use data are from a private claims data set. The sample includes children ages 7 to 17 on June 1st of the survey year born within 120 days of the kindergarten eligibility cutoff. For the NHIS data, all means are weighted.

Figure 4: ADHD Diagnosis by Days from Kindergarten Eligibility Cutoff Date



Notes: Data are from the restricted-access versions of the 1997-2006 National Health Interview Survey (NHIS). The horizontal axis indicates bins for children born each number of days from the kindergarten eligibility cutoff date. The dots are mean diagnosis rates. The lines are from locally weighted regression interpolation. The sample includes children ages 7 to 17 on June 1st of the survey year born within 120 days of the kindergarten eligibility cutoff.

Figure 5: Fraction of Children Younger than Median for State x Grade x Year Cell by Days from Kindergarten Eligibility Cutoff Date



Notes: Data are from the restricted-access versions of the 1997-2006 National Health Interview Survey (NHIS). The horizontal axis indicates bins for children born each number of days from the kindergarten eligibility cutoff date. The dots are the fraction of children in that bin that are younger than the median age for their grade x state x year cell. The lines are from locally weighted regression interpolation. The sample includes children ages 7 to 17 on June 1st of the survey year born within 120 days of the kindergarten eligibility cutoff.

Table 1: Kindergarten Eligibility Cutoff Dates

State	Cutoff 2005	Law Changes Since 1984
AL	1-Sep	1984-1989: 10/1
		1990+: 9/1
AK	1-Sep	1984-1987: 11/2
		1988-2003: 8/15
AZ	31-Aug*	
AR	15-Sep	1984-1997: 10/1
		1998: 9/1
		1999+: 9/15
CA	2-Dec	1984-1986: 12/1
		1987+: 12/2
CO	LEA	
CT	1-Jan	
DE	31-Aug	1984-1992: 12/31
		1993: 11/30
		1994: 10/31
		1995: 9/30
		1996+: 8/31
DC	31-Dec	
FL	1-Sep	
GA	1-Sep	Established 1985
HI	31-Dec	
ID	1-Sep	1984-1989: 10/16
		1990: 9/16
		1991-1992: 8/16
		1993+: 9/1
IL	1-Sep	1984-1985: 12/1
		1986: 11/1
		1987: 10/1
		1988+ : 9/1
IN	1-Jul	1984-1988: LEA
		1989: 9/1
		1990: 8/1
		1991: 7/1
		1992-2000: 6/1
		2001-2005: 7/1
IA	15-Sep	
KS	31-Aug	1984-1994: 9/1
		1995+: 8/31
KY	1-Oct	
LA	30-Sep	1984-1995: 12/31
		1996+: 9/30
ME	15-Oct	
MD	30-Sep	1984-2002: 12/31
		2003: 11/30
		2004: 10/31
		2005: 9/30
		2006+: 9/1
MA	LEA	
MI	1-Dec	
MN	1-Sep	
MS	1-Sep	
MO	31-Jul*	1984-1986: 8/31*
		1987: 7/31*
		1988-1996: 6/30*
		1997+: 7/31*
MT	10-Sep	
NE	15-Oct	
NV	30-Sep	
NH	LEA	
NJ	LEA	
NM	31-Aug*	
NY	LEA	
NC	16-Oct	
ND	31-Aug*	
OH	30-Sep	
OK	1-Sep	
OR	1-Sep	Changed 1986 from 11/15
PA	LEA	
RI	1-Sep	Changed 2004 from 12/31
SC	1-Sep	Changed 1993 from 11/1
SD	1-Sep	
TN	30-Sep	Change 1985 from 10/31
TX	1-Sep	1984-1994: ssy
		1995+: 9/1
UT	1-Sep*	1984-1987: ssy
		1988+: 9/1*
VT	LEA	1984-1990: 1/1
		1991+: LEA
VA	30-Sep	
WA	31-Aug	
WV	31-Aug*	
WI	1-Sep	
WY	15-Sep	

Notes: Data acquired from individual state statutes. LEA denotes that the state allowed the local education authority to determine the applicable cutoff, therefore there is no statewide date. Starred dates indicate that the statute specifies that the child must be born *before* a certain date, so we have adjusted the date in this table to reflect the date that the child must be born *on or before* to be consistent across states.

Table 2: Sample Characteristics

Variable	Full Sample	Eligible States	Regression Sample: +/-120 Days	Regression Sample: Born Before [-120, -1]	Regression Sample: Born After [0, 120]
National Health Interview Survey (NHIS) [1997-2006]					
Observations (person/year)	69,350	53,212	35,343	17,728	17,615
% Male	51.0%	50.8%	50.7%	50.3%	51.0%
Average age as of June 1	11.9	11.8	11.8	11.7	11.8
% White (Non-Hispanic)	64.6%	64.5%	64.2%	64.1%	64.4%
% Black (Non-Hispanic)	15.4%	16.0%	15.9%	16.0%	15.9%
% Hispanic	15.7%	15.0%	15.2%	15.1%	15.2%
% Other Race/Ethnicity	4.4%	4.5%	4.7%	4.8%	4.5%
% ADD/ADHD Diagnosis	8.4%	8.7%	8.6%	9.7%	7.6%
Medical Expenditure Panel Survey (MEPS) [1996-2006]					
Observations (person/year)	59,814	47,423	31,641	15,952	15,689
% Male	51.1%	51.1%	51.2%	50.9%	51.5%
Average age as of June 1	12.0	11.9	11.9	11.9	11.9
% White (Non-Hispanic)	62.5%	62.6%	62.4%	62.0%	62.7%
% Black (Non-Hispanic)	15.7%	15.9%	16.0%	15.9%	16.1%
% Hispanic	16.3%	15.7%	15.5%	15.8%	15.3%
% Other Race/Ethnicity	5.5%	5.9%	6.1%	6.2%	6.0%
% any stimulant use	4.2%	4.3%	4.3%	4.5%	4.0%
Private Claims Data [2003-2006]					
Observations (person/year)	121,352	72,885	48,206	24,380	23,826
% Male	50.3%	50.2%	50.2%	50.3%	50.1%
Average age as of June 1	12.3	12.4	12.4	12.4	12.4
% any stimulant use	5.2%	5.6%	5.8%	6.5%	5.2%

Notes: Data are from the restricted-access versions of the 1997-2006 National Health Interview Survey (NHIS), the 1996-2006 Medical Expenditure Panel Survey (MEPS), and a private insurance claims dataset. The NHIS and MEPS statistics utilize the survey sample weights. The full sample includes children ages 7 to 17 on June 1st of the survey year. The eligible sample includes children who live in states with clearly defined kindergarten eligibility cutoff dates in the state they reside in the year they turned five years old. The regression sample restricts this group to children whose birthdays are within 120 days of school start.

Table 3: Regression Discontinuity Estimates of the Effect of Being Born after the Cutoff Date

Covariates	Models (1)	(2)	(3)	(4)
NATIONAL HEALTH INTERVIEW SURVEY (NHIS) Outcome: ADD/ADHD Diagnosis N= 35,343 Children Mean of Dependent Variable = 0. 0864				
Born After Cutoff	-0.0204 (.0050)	-0.0209 (.0050)	-0.0206 (.0050)	-0.0208 (.0079)
Age Fixed Effects, Gender, Race/Ethnicity		X	X	X
State and Birth Cohort Fixed Effects			X	X
1st Order Polynomial				X
MEDICAL EXPENDITURE PANEL SURVEY (MEPS) Outcome: Receiving Medication to Treat ADD/ADHD N = 31,641 for 18,559 Children Mean of Dependent Variable = 0. 0427				
Born After Cutoff	-0.0055 (.0037)	-0.0059 (.0034)	-0.0063 (.0034)	-0.0079 (.0058)
Age Fixed Effects, Gender, Race/Ethnicity		X	X	X
State and Birth Cohort Fixed Effects			X	X
1st Order Polynomial				X
PRIVATE CLAIMS DATA Outcome: Prescription Claim for Ritalin or Other Drug for Treating ADD/ADHD N = 48,206 Observations for 22,371 Mean of Dependent Variable = 0.0584				
Born After Cutoff	-0.0124 (.0021)	-0.0123 (.0021)	-0.0122 (.0030)	-0.0156 (0.0057)
Age Fixed Effects, Gender		X	X	X
State and Birth Cohort Fixed Effects			X	X
1st Order Polynomial				X

Notes: Coefficients are from linear probability model regressions with standard errors in parentheses. All specifications include a constant term. All standard errors are clustered by current state of residence. Sample weights are used for the NHIS and MEPS data. The polynomial is defined as days from the cutoff and is modeled separately for days before and days after. The cutoffs are the kindergarten eligibility cutoff date in the child's current state of residence in the year the child turned five years old. The variable "Born After Cutoff" is T(i≥0). The sample includes children ages 7 to 17 on June 1st of the survey year born within 120 days of the kindergarten eligibility cutoff date.

Table 4: Sensitivity Tests, National Health Interview Survey

Specification	Sample	Num. of Obs	Mean of Dep. Var.	Coef. on Born After, T(i≥0)
Baseline results	+/- 120 days	35,343	0.0864	-0.0208 (0.0079)
Days in Sample	+/- 90 days	26,659	0.0861	-0.0178 (0.0101)
	+/- 60 days	17,826	0.0849	-0.0203 (0.0136)
	+/- 30 days	9,145	0.0826	-0.0316 (0.0155)
Order of Polynomial	2nd Order	35,343	0.0864	-0.0168 (0.0142)
	3rd Order	35,343	0.0864	-0.0381 (0.0181)
	4th Order	35,343	0.0864	-0.0364 (0.0205)
	5th Order	35,343	0.0864	-0.0523 (0.0255)
State of Birth	State of Birth (1997-2006)	30,476	0.0893	-0.0156 (0.0076)
	State of Birth = State of Residence	26,607	0.0875	-0.0205 (0.0098)
Gender	Male	18,014	0.1248	-0.0197 (0.0116)
	Female	17,329	0.0471	-0.0209 (0.0088)
Race/Ethnicity	White	19,538	0.1012	-0.0210 (0.0091)
	Black	6,000	0.0803	-0.0356 (0.0152)
	Hispanic	8,360	0.0462	-0.0220 (0.0134)
Age Group	7-12	19,345	0.0818	-0.0237 (0.0107)
	13-17	15,998	0.0926	-0.0158 (0.0114)
Survey Years	1997-2001	18,274	0.0798	-0.0180 (0.0096)
	2002-2006	17,069	0.0928	-0.0232 (0.0114)
Probit Model (Marginal Effects)		35,343	0.0864	-0.0197 (0.0068)
Falsification Tests	Chicken Pox	34,727	0.7248	-0.0057 (0.0121)
	Respiratory Allergies	35,233	0.1386	0.0059 (0.0086)
	Hay Fever	35,247	0.1268	-0.0046 (0.0089)
	Frequent Headaches	35,321	0.0815	0.0014 (0.0054)

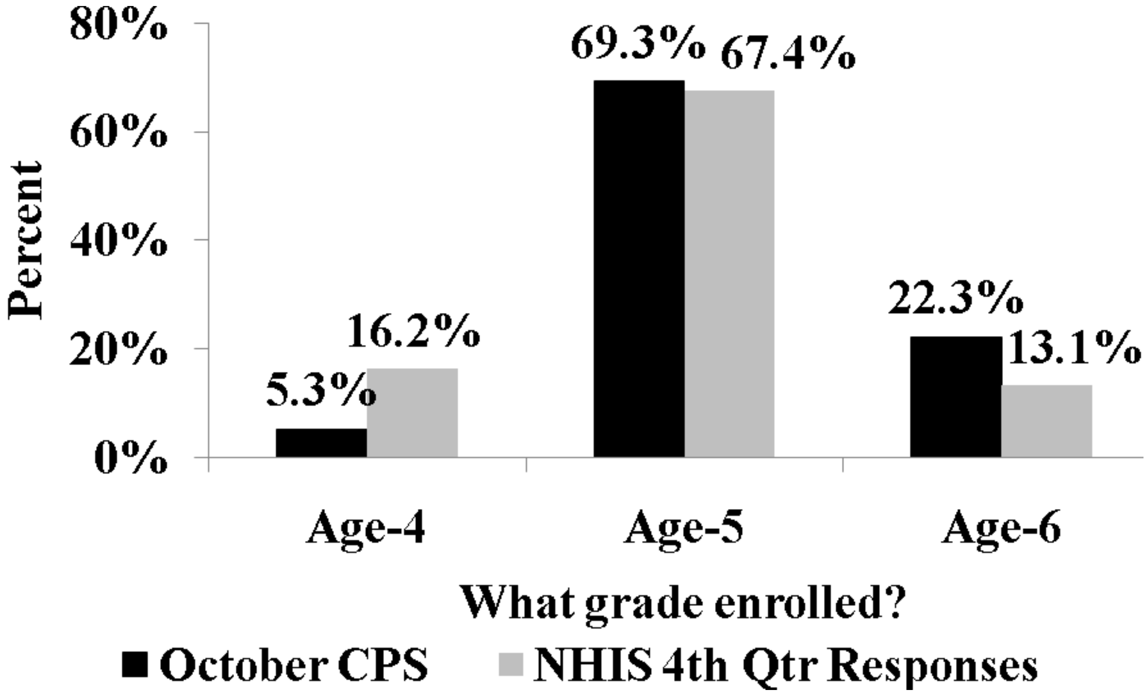
Notes: Data is from the 1997-2006 National Health Interview Survey, and the sample is restricted to children born ages 7 to 17 on June 1st of the survey year who were born within 120 days of the kindergarten eligibility cutoff. Unless otherwise specified, coefficients are from linear probability regressions with standard errors in parentheses, and all specifications include a constant, a linear polynomial in days from cutoff separately for days before and days after, child's age, state of residence and birth cohort fixed effects, and controls for gender and race/ethnicity. Population weights are used and the standard errors are clustered by state.

Table 5: Heterogeneity in Regression Discontinuity Estimates of Stimulant Treatment, Medical Expenditure Panel Survey (MEPS) and Private Claims Samples

		MEPS		Private Claims	
Specification	Sample	Obs. [$\bar{y}$]	Coef. on T(i≥0) (Std error)	Obs. [$\bar{y}$]	Coef. on T(i≥0) (Std error)
Baseline Results	+/- 120 days	31,641 [0.0427]	-0.0079 (0.0058)	48,206 [0.0584]	-0.0156 (0.0057)
Days in Sample	+/- 90 days	23,744 [0.0410]	-0.0129 (0.0065)	36,582 [0.0572]	-0.0129 (0.0059)
	+/- 60 days	16,034 [0.0391]	-0.0083 (0.0068)	24,809 [0.0563]	-0.0136 (0.0067)
	+/- 30 days	8,136 [0.0368]	-0.0104 (0.0125)	12,504 [0.0548]	0.0026 (0.0123)
Order of polynomial	2nd Order	31,641 [0.0427]	-0.0143 (0.0070)	48,206 [0.0584]	-0.0064 (0.0059)
	3rd Order	31,641 [0.0427]	-0.0033 (0.0111)	48,206 [0.0584]	-0.0103 (0.0132)
Gender	Male	16,109 [0.0610]	-0.0131 (0.0101)	24,216 [0.0803]	-0.0218 (0.0102)
	Female	15,523 [0.0235]	0.0003 (0.0081)	23,990 [0.0363]	-0.0092 (0.0072)
Age Group	7-12	18,424 [0.0523]	-0.0039 (0.0086)	23,703 [0.0584]	-0.0150 (0.0080)
	13-17	13,217 [0.0306]	-0.0110 (0.0077)	24,503 [0.0583]	-0.0154 (0.0062)
One observation per claimant	First Year in Data	16,986 [0.0420]	-0.0064 [0.0064]	19,857 [0.056]	-0.0108 (0.0059)
	Last Year in Data	14,655 [0.0435]	-0.0102 [0.0068]	19,696 [0.057]	-0.0196 (0.0065)
Probit model (marginal effect)		31,641 [0.0427]	-0.0055 (0.0047)	48,206 [0.0584]	-0.0149 (0.0055)
Private Insurance	2003-2006	6,570 [0.0481]	-0.0271 [0.0147]		
Falsification Tests	Asthma drug use	.	.	48,206 [0.093]	0.0117 (0.0075)
	Antibiotic drug use	.	.	48,206 [0.345]	0.0080 (0.0091)

Notes: Unless otherwise specified, coefficients are from linear probability regressions with standard errors in parentheses. All specifications include a constant, a linear polynomial, child's age fixed effects, and controls for gender, state, year of birth, and, when available, race/ethnicity. In the MEPS population weights are used. In all samples the standard errors are clustered by state. The samples include children ages 7 to 17 on June 1st of the survey year born within 120 days of the kindergarten eligibility cutoff date.

Appendix Figure 1: Current Grade for Children 7-16, 2000-2002 October CPS and 4th Quarter Responses to 2000-2002 NHIS



Notes: The NHIS fourth quarter responses are from the public use data. We impute the respondents' ages as of October 1st using information on month and year of birth. Both samples are from years 2000 to 2002 for children age 7 to 16.