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PEER EFFECTS AND HUMAN CAPITAL ACCUMULATION: THE EXTERNALITIES OF ADD

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ABSTRACT

Although recent work has shown that peers affect human capital accumulation, the mechanisms are not well understood. Knowing why high achieving peers matter, because of their innate ability, disciplined behavior or some other factor, has important implications for our understanding of the education production function and for how we organize schools and classrooms. In this paper I provide evidence that peer behavior is an important mechanism. To identify the impact of peer behavior on achievement separate from ability or other characteristics, I exploit exogenous improvements in classmates' inattention/impulsivity that result from a diagnosis of ADD. After children with ADD are diagnosed, I show that their behavior improves, but that no other characteristics, including achievement, change. I find that peer behavior significantly affects cognitive achievement and that resources such as class size can overcome the negative peer effects observed, consistent with the model of education production proposed by Lazear (2001). These findings have important implications for our understanding not only of peer effects but also of the relationship between health, productivity and growth.

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I. Introduction

Research on the determinants of human capital accumulation has focused increasingly on the role of peers. But estimating peer effects is hindered by selective sorting across schools and potential omitted variable bias. Recent work employing novel identification strategies has estimated significant peer effects, but because peer achievement is a function of multiple characteristics, including both innate ability and behavior, these estimates do not allow one to draw any conclusions about the mechanisms. It could be that peer achievement matters because students learn from their high ability peers or because high achieving peers exert greater effort and concentration and are less disruptive in class. Knowing whether ability, behavior or some combination of the two is responsible for observed peer effects is necessary for the development of an accurate model of education production and has important implications for how we organize schools and classrooms. If ability, which is generally considered fixed or difficult to modify, is solely responsible for observed peer effects, then the appropriate policy response would be to re-organize classrooms. However, if behavior which is more malleable than ability, proves to be an important factor (though not necessarily the only one), then this would suggest the optimal response may be to design policies that alter student behavior as they are likely easier to implement than policies that redistribute students based on "ability" - a poorly measured and often unobserved characteristic.

In this paper I provide strong evidence that peer behavior, separate from ability or other child characteristics, matters by comparing the outcomes of the same children over time as the level of inattention or impulsivity of the peer group (defined as classmates) changes. To identify the impact of inattention or impulsivity separate from ability I estimate the impact of having classmates with ADD before and after diagnosis. I show that before students are diagnosed with ADD they display greater externalizing behavior problems and worse self-control. After diagnosis, their behavior improves but their cognitive achievement does not, thereby enabling identification of the impact of peer behavior separate from ability or other characteristics. To address issues of selection into peer groups, I include individual fixed effects (which subsumes school fixed effects). I also instrument for the timing of diagnosis using expansions in public health insurance through Medicaid/SCHIP. Medicaid/SCHIP expansions increase the probability of health insurance coverage and lower the cost of diagnosis and treatment of ADD but otherwise should have no effect on classroom composition, teacher quality or student test scores.

There are two advantages to this identification strategy. First, the policy used to instrument is not an education policy and thus is more arguably exogenous in this context. Second, by using a diagnosis of ADD to identify peer effects one can identify the impact of a specific peer characteristic (inattention/impulsivity), without changing other peer characteristics such as ability, race, gender or income. Many of the existing studies that identify peer effects rely on exogenous changes in classroom composition (eg Angrist and Lang, 2004; Hoxby, 2000). In such cases, multiple peer characteristics change so that it is not possible to identify which characteristic is responsible for the estimated effect.

There are four main findings. First, children with undiagnosed (and therefore untreated) ADD generate negative externalities in the classroom, lowering the reading test scores of their ADD-free classmates: if 8.5 percent of the class have undiagnosed ADD (the standard deviation in these data), test scores will be 2 points, 20 percent of a standard deviation, lower. Once diagnosed, students with ADD generate no such negative externalities. This represents a moderate impact given previous findings that a one standard deviation increase in peer test scores increases individual test scores by 35 percent of a standard deviation (Hanushek et al, 2003). Second, once diagnosed, children with ADD see significant improvements in their own behavior but small and mostly insignificant effects on their cognitive achievement, consistent with medical evidence and suggesting that the primary mechanism by which students with undiagnosed ADD affect others is through their disruptive behavior. Third, these effects are concentrated among boys. This can potentially be explained by the fact that peer groups at early ages are largely gender-specific (Sage and Thomason) and ADD is a disorder that mostly affects boys. However, it may also be that girls are simply less disturbed by disruptive behavior in the classroom. Finally, I find that resources (class size and teacher human capital) can overcome the negative peer effects observed, consistent with the model of peer effects proposed by Lazear (2001).

These findings have a number of important implications. First, they contribute to the existing literature on peer effects in the classroom, shedding light on one potential mechanism through which peer effects operate. While these estimates of the impact of inattentive/impulsive behavior are derived from students with ADD, they likely generalize to other problematic or disruptive behavior in the classroom, suggesting that the total peer effect due to behavior exceeds these estimates. Second, the finding that achievement of girls is less affected by disruptive behavior in the classroom can potentially explain part of the widening gender gap in cognitive achievement. Third, the results suggest that peer effects should be considered within their institutional framework: schools and teachers can both affect peer behavior and mitigate the negative effects of peer behavior. As such, policy discussions need not be limited to how best to compose classrooms to maximize peer effects. Rather, policies that also consider the ways in which teacher, school, and community resources (health care in this case) influence or mitigate peer effects via student behavior may ultimately be easier to implement and just as effective.

Finally, the results of this paper contribute to our understanding of the relationship between health, productivity and growth. Previous work has linked children's physical and mental health with their own human capital accumulation (Grossman and Kaestner, 1997; Currie and Stabile, 2007; Fletcher and Wolf, forthcoming). Other work (Weil, 2007; Shastry and Weil, 2005) have estimated the effect of physical health on income per capita. Results presented here suggest that mental health may also play an important role in explaining growth – not only through its impact on the human capital accumulation of those with a mental disorder, but also through externalities imposed on others.

The rest of the paper is organized as follows: section II contains background information on ADD and the peer effects literature; section III describes the data and how peer characteristics are measured; section IV presents estimates of the impact of diagnosis on one's own achievement and behavior; sections V and VI contain the fixed effect and IV estimates of the externalities associated with untreated ADD, respectively; section VII includes two robustness checks and section VIII concludes.

II. Background

A. ADD: Symptoms, Prevalence and Etiology

ADD is characterized by inattention, impulsivity and hyperactivity. For a medical diagnosis of ADD, the symptoms must be more frequent or severe than in other children the same age and at least some of the symptoms must have been present before age 7 (Diagnostic and Statistical Manual of Mental Disorders IV). Data from the National Health Interview Survey (NHIS) 1997-2004 show that the proportion of children diagnosed with ADD has remained fairly constant at about 6 percent of children (Figure 1). ADD is much more common among boys and rates of diagnosis increase with age until age 11-12 when they plateau. In 2003, prevalence among boys between the ages of four and six was five percent, increasing to 11 percent for those aged 11-12, and remaining steady at 12 percent for those age 13-17.

Children with ADD are characterized by worse behavior and lower cognitive achievement (see Mannuzza and Klein, 2000 for a review; Currie and Stabile, 2007). The negative impact of ADD on behavior is significant. Barkley et al (1990) finds that almost half of students with ADD had been suspended from school. Greene et al (2002) find that students with ADD consume a significantly higher percentage of teacher attention and that teachers report significantly greater stress in their interactions with them.

There is mounting evidence in the medical literature that ADD is biologically determined, with much of the evidence based on brain imaging studies (Swanson et al 2001; Castellanos, 2001; Waldman et al, 1998; Rowe et al, 1998). This is consistent with recent work that suggests that children with ADD display many of the symptoms associated with the disorder in preschool (Campbell and Ewing, 1990) even though most children are not diagnosed with the disorder until later.

B. Diagnosis, Treatment and Cognitive and Behavioral Outcomes

Of youths diagnosed with ADD, an estimated 78% are prescribed one or more stimulants (Guevara et al, 2002).[1] Medical evidence suggests that diagnosis and treatment of ADD positively affects behavior in 70-80 percent of children but has little impact on cognitive achievement. In a recent review of the literature, Spira and Fischel (2005) conclude that for children with ADD "stimulants may increase on-task behavior, decrease disruptive behavior, and even increase the amount of class work completed, but they do not appear to have a significant effect on the accuracy of that work." Recent work by Currie and Stabile (2007) and Fletcher and Wolf (forthcoming) based on large datasets of children followed over time for many years is consistent with these findings.

C. Endogeneity of Peer Achievement and Existing Peer Effects Literature

Most of the empirical literature on peer effects estimates the impact of peer achievement on own achievement. The primary challenge to identifying peer effects lies in overcoming the endogeneity of one's peer group. Specifically, issues of self-selection, simultaneity and omitted variables may confound or bias estimates of peer effects. Selection refers to the fact that students select their peer groups largely through their choice of school. Omitted variables might include unobserved aspects of teacher quality that affect both the student and his peers. Finally, simultaneity refers to the fact that while

[1] For those not prescribed stimulants, antidepressants, antipsychotics, and clonidine are often prescribed.

a student is influenced by his peers, he also influences his peers (Brock and Durlauf, 2001; Manski, 1993; Moffitt, 2001.)

Many papers employ novel techniques to identify the causal impact of peers. Hoxby (2000) exploits variation in cohort size to estimate the impact of peer race and gender on achievement. Hanushek et al (2003) employ a similar strategy, identifying peer effects by estimating the impact of differences in peer characteristics for cohorts of students within the same school. They find that peer achievement does have a significant and positive impact on achievement. Whitmore (2005) use randomization to small classrooms as part of the Tennessee project STAR to estimate the impact of peer achievement on test scores. Interestingly, she does find positive peer effects, but even conditional on peer achievement, she finds that more girls in a classroom generates positive effects. Though she does not speculate why – the evidence presented here suggests that this could be because girls are characterized by less disruptive behavior. Sacerdote (2001) and Zimmerman (2003) overcome the issue of self selection by focusing on randomly assigned roommates in college. They find that the high school achievement of roommates affect students' grades in the first year of college. Angrist and Lang (2004) study Metco, a desegregation program in Boston which dramatically increased the number of low-performing black students in predominantly white suburban schools, and find little effect. By focusing on a policy of forced desegregation they too overcome issues of self-selection and other omitted variables. Cooley (2006) estimates the impact on high achieving students of a change in policy that raises the bar for promotion for low achieving students.[2]

[2] Other work on peer effects include Evans, Oates and Schwab (1992), Betts and Morell (1998), Epple and Romano (1998), Vigdor and Nechyba (2005). Of these, Evans Oates and Schwab (1992) and Vigdor and

The above-mentioned natural experiments, however, do not lend themselves to identification of the mechanism by which peers affect student achievement. High achieving peers might matter because students learn from them (ability), or because they are less disruptive (behavior). The latter would be consistent with the model proposed by Lazear (2001) in which the ability of a student to learn depends on the behavior of his classmates because it reduces effective teaching time or directly interferes with their work. Distinguishing between the potential mechanisms has proven difficult. In a recent empirical paper on peer effects, Hanushek et al (2003) write "In general there has been limited attention given to the mechanism through which peers affect outcomes…Most analyses have focused on the identification of the "reduced form" relationship between outcomes and specific measures of peer group quality, typically ignoring the precise structure of the underlying causal relationship."

Three recent empirical papers provide evidence on the role of peer behavior in determining cognitive outcomes. Figlio (2005) uses the presence of a boy with a feminine name to instrument for classroom disruption, arguing that such boys are more prone to fighting but are not characterized by lower cognitive ability. He finds large negative effects on achievement. Lavy, Paserman and Schlosser (2007) find that low-achieving peers negatively affect student achievement, particularly those at the bottom of the distribution, and suggests that this due to the fact that they are more disruptive and negatively affect the ability of teachers to teach. Finally, Neidell and Waldfogel (2008) find that having peers who have been to preschool affects cognitive achievement in

Nechyba (1998) find that peer effects estimated via OLS are not robust under simultaneous equation estimations for the former or the inclusion of teacher fixed effects for the latter. Gavira and Raphael (2001) look at peer effects in the context of juvenile behavior.

kindergarten and provide some evidence that the effect is working through externalizing behavioral problems.

In this paper I estimate the impact of a specific peer behavior (in attention/impulsivity) on cognitive achievement and exploit an identification strategy, described below, that allows me to hold all other peer characteristics constant.

D. Overview of Identification Strategy

To identify the impact of peer inattention or impulsivity separate from ability I estimate the impact of having classmates with ADD before and after diagnosis. I argue and provide evidence that before students are diagnosed with ADD they display greater externalizing behavior problems. After diagnosis, their behavior improves but their cognitive achievement does not, thereby enabling identification of the impact of peer behavior separate from ability or other characteristics. This identification strategy has the additional advantage of overcoming the problem of simultaneity as the medical evidence suggests that classroom characteristics (including peers) do not influence the probability that a student has ADD. Rather, the preponderance of evidence suggests that the origins of ADD are biological. But several potential threats to identification remain which I address below.

The first concern is that children may be diagnosed with ADD shortly after an especially acute period of disruptive behavior and as such, we might expect their behavior to improve afterwards in a mean-reverting fashion (as in an Ashenfelter dip). To address this, I compare the improvements in behavior of those diagnosed with ADD with those evaluated but not diagnosed. If the observed improvement in behavior simply reflects mean reversion, one would expect similar improvement among those evaluated for ADD but not diagnosed. We do not.

A second concern might be non-random selection into peer groups. To address this, I include individual fixed effects. If peer selection is manifest mostly through selection of school, including individual fixed effects implicitly controls for school choice and thus choice of classmates (the peer group).

But there could still be non-random assignment of students to classrooms within schools based, in part, on behavior. If this occurs one might find that peer characteristics, including achievement, change with diagnosis, though the relationship would not be causal. To address this, I show that observed peer characteristics (gender, race, behavior, income, special education status) do not change appreciably after diagnosis.

Omitted variables might also bias the estimates if the probability of diagnosis is affected by a third factor such as teacher quality that also affects peer achievement. To address this I both control for observed school, teacher and classroom characteristics in the regressions and provide evidence that such characteristics do not significantly affect when a child is diagnosed with ADD as part of the robustness checks in section VII.

A second and more conservative approach to identification that addresses many of these concerns is to drop the assumption of random timing of diagnosis altogether and instrument for the timing of diagnosis using expansions in publicly provided health insurance. Expansions in health insurance coverage reduce the cost of medical diagnosis and treatment but are uncorrelated with peer or teacher characteristics that might independently affect both diagnosis and treatment. An advantage of this identification strategy is that the policy used to instrument for diagnosis is not an education policy. As such, we may be less concerned that the policy change coincides with other changes affecting students, teachers or schools. The empirical methods are described in greater detail in sections V and VI.

III. Data

A. Data Description

The data come from the restricted use Early Childhood Longitudinal Survey – Kindergarten Cohort (ECLS-K). The ECLS-K cohort consists of a nationally representative group of roughly 20,000 children who entered Kindergarten in the Fall of 1998, drawn from roughly 1000 schools. Data are collected for students in kindergarten, first, third and fifth grades. Teachers, parents and school administrators are surveyed each year. The data include information on family background, teacher characteristics, classroom composition, curriculum and school resources, as well as behavioral and cognitive assessments. The behavioral assessment consists of teacher scores on an externalizing behavioral problem scale (scale 1-4 with 4 indicating worse behavior). Cognitive assessments consist of standardized reading and math scores on tests developed especially for the ECLS but based on existing instruments.[3]

The data include both individual survey data for multiple children per class (6 on average for this analysis sample) and teacher reports so that one can characterize a student's teachers and classmates. Specifically, information on classroom composition, teacher qualifications, class size, racial, gender and special education status of the class come from teacher reports while information on average income and evaluation and

[3] These include: the Children's Cognitive Battery, Peabody Individual Achievement test –Revised, the Peabody Picture Vocabulary Test-3, Primary Test of Cognitive Skills and the Woodcock-Johnson Psycho-Educational Battery-Revised.

diagnosis of ADD come from parental reports. The panel nature of the data allows one to follow the same child over time and determine when he was evaluated and diagnosed with ADD.

Data on whether the child is currently prescribed medication for ADD is not collected until the fifth grade. In the fifth grade, three quarters of children diagnosed with the disorder are reportedly treated with a stimulant, consistent with existing medical evidence on treatment rates. In this paper I focus on diagnosis and not treatment because treatment is not reported before fifth grade and because diagnosis is arguably more exogenous than treatment in this context. The focus on diagnosis, not treatment, likely results in downward bias of the estimates.

B. Characteristics of Children with ADD

Children with and without a diagnosis of ADD by fifth grade are similar in terms of racial composition, per capita household income, and school, teacher and classroom characteristics (Table 1). But those diagnosed by fifth grade are more likely to be male (74 percent) and more likely to have health insurance (.91 vs .83). In terms of child outcomes, children with ADD suffer worse reading test scores and worse ratings in terms of externalizing behavior. They are also less likely to be rated by their teachers as “always working to the best of their ability.”

C. Characterizing Peers

Peer characteristics were measured from teacher and parent reports. From teacher reports, I generate measures of the gender and racial composition of the class as well as class size. These measures are based on the whole class. From parent surveys, I generate measures of the share of students in the class with diagnosed and undiagnosed ADD and the average income of the students in the class. These measures are based on the subset of the class included in the ECLS-K sample (6 on average). I classify a child as having undiagnosed ADD if he or she is diagnosed with ADD in the future but is not currently diagnosed. This classification assumes that children who are ultimately diagnosed with ADD display symptoms of the disorder prior to their diagnosis, which is consistent with the medical evidence. Indeed, children cannot be diagnosed with ADD unless they displayed at least some symptoms before age 7.

This characterization of peers with undiagnosed ADD introduces three sources of measurement error which will lead to a downward bias of any estimated effect. The first arises if those with undiagnosed ADD exhibit few symptoms prior to diagnosis. Even though evidence based on the ECLS-K and elsewhere suggests that on average, those with undiagnosed ADD are characterized by greater inattention, this does not necessarily hold for all children. For example, in the ECLS-K, among those who are not yet diagnosed with ADD but will be in the future, 40 percent reportedly "have trouble paying attention relative to other children their age" compared with 10 percent of those who are never diagnosed with ADD. Second, because students in the ECLS-K are only followed through fifth grade, students diagnosed with ADD later would be incorrectly classified as not having ADD. However, since data from NHIS suggests that most children with ADD are diagnosed by age 11-12 (corresponding to fifth and sixth grades), this should not introduce much error.[4] The third source of measurement error results from the fact that

[4] For example, in the 2003 NHIS, 4.6 percent of 4-6 year old boys were diagnosed with ADD, increasing to 8.4 percent of 7-8 year olds, 9.7 percent of 9-10 year olds , 11.3 percent of 11-12 year olds and 12.1 percent

this measure is derived from the parent surveys and thus are calculated over six children, on average, per class (average class size is 21).[5] Ammermueller and Pischke (2006) show that when peer characteristics are measured over a sub-sample of students in the class, estimates of peer effects will be biased down by a factor of $(N_{sample}-1)/(N_{actual}-1)$ which is 1/4 in this sample. This suggests that the instrumental variable estimates will be considerably larger than OLS estimates.

D. Variation in Peer Characteristics

Five percent of the children surveyed in the ECLS-K are diagnosed with ADD by fifth grade. Diagnosis occurs uniformly over time. Of those ever diagnosed with ADD, 23 percent are diagnosed by kindergarten, another 25 percent are diagnosed in first grade, 28 percent between first and third grades and 24 percent between third and fifth grades. As such, the share of classmates with diagnosed and undiagnosed ADD varies considerably over time in this sample. For the sample of students without ADD, 23 percent have peers with undiagnosed ADD in kindergarten, in first grade 15 percent have peers with undiagnosed ADD, dropping to 7 percent in third grade and (by definition) no students have peers with undiagnosed ADD in fifth grade. There are three sources of this variation: 1) undiagnosed peers are diagnosed, 2) peers are designated special education (though only a subset move to different classrooms as a result) or 3) peers attrit from the sample. Most of the variation is due to diagnosis of those previously undiagnosed.

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of boys 13-17, suggesting that by stopping at grade 5, we are missing less than one percent of boys with ADD. For girls, the share with diagnosed ADD increases from 4.3 percent of 11-12 year olds to 4.7 percent of 13-17 year olds.

[5] The ECLS K users manual chapter 4 describes the sample design. Within each school a self weighting sample of students was selected in Kindergarten. The only subgroup that was oversampled was Asian Pacific Islanders. After Kindergarten with sample attrition, the remaining sample is not necessarily representative. For this reason I run regressions dropping the fifth grade (when attrition is highest) and also dropping the attriters.

While attrition is minimal up until third grade and the characteristics of the remaining sample remain stable, this changes in fifth grade: attrition increases and the characteristics of the remaining students change, they are less likely to be black and more likely to be upper income (Table 2). To assess whether and how the different sources of variation (diagnosis, designation as special education or attrition) may influence the results I present estimates from multiple specifications: including and excluding special education students, including and excluding the fifth grade, and including only non-attriters.

IV. Impact of Diagnosis on Own Cognitive and Behavioral Outcomes

To estimate whether diagnosing a child with ADD improves his behavior and cognitive achievement, I compare outcomes for the same child before and after a diagnosis. To do so, I regress cognitive achievement and behavioral outcomes on an indicator for whether the child has been diagnosed with ADD, child fixed effects and time-variant family income, (panel A Table 3). In panel B I include observed teacher and classroom characteristics (masters degree, years of teaching experience, average income of classmates, share female, black and Hispanic) as controls. Finally, in panel C, I test whether diagnosis affects future test scores and behavior. I also explore heterogeneity of the effects by defining the sample multiple ways.

In the first two columns of Table 3 the sample is unrestricted. Diagnosing a child with ADD does not appear to improve reading test scores but does improve behavior considerably more, decreasing the child's score on the "externalizing behavioral problem" scale by between 8 and 18 percent of a standard deviation, depending on the specification. This is consistent with the large medical literature and small economics literature on the topic which has generally found that treatment for ADD results in improved behavioral outcomes but little or no change in cognitive achievement.

Students diagnosed with ADD may become eligible for special education services at the same time – either because ADD makes them eligible for special education or because they are diagnosed with additional learning disabilities. If so, it may be the special education designation which affects outcomes, not diagnosis of ADD. To eliminate this possibility, I exclude those students who receive special education services from the sample in columns 3 and 4. The estimated impact on externalizing behavioral problems and reading test scores increases: externalizing behavior improves by 13 percent of a standard deviation, while reading test scores increase by less than 7 percent of a standard deviation (panel A). However, in panel B when we include class characteristics, the reading test score result falls by 40 percent and becomes insignificant, while the behavioral problem estimate falls by less but remains significant. Interestingly, within a year, the impact of diagnosis on reading falls to zero while the impact on behavior increases 60 percent (panel C).[6]

It may be, however, that it is not the diagnosis and presumed treatment of ADD that generates the positive impact on behavior, but rather the fact that the child is professionally evaluated. The act of evaluation may signal the presence of a concerned care-giver or some positive change in family circumstances which could explain the results. In columns 5-6 of Table 3 I limit the sample to those children who are ever evaluated for ADD (of whom one third are diagnosed with the disorder). By conditioning on whether evaluated, one controls for any underlying differences in care

[6] The small fleeting reading test score results could be consistent with mean reversion.

seeking behavior of parents that might bias the estimated impact of diagnosis on outcomes. This analysis also enables one to address the concern that children are diagnosed with ADD shortly after an increase in disruptive behavior and as such, the behavior might improve afterward due to mean-reversion, not treatment. If this were so, we would expect improvements in behavior among those evaluated but not diagnosed as well. When I condition on this sample, diagnosis still improves behavioral outcomes, suggesting that it is the act of diagnosis (and presumably treatment) that is responsible for the improvements in behavior, not the care-seeking behavior or mean-reversion.[7] For this sample, the impact of diagnosis on reading test scores is positive and significant. However, the effect is smaller (less than 1 point, or 7 percent of a standard deviation) and fleeting – disappearing completely in the year after while the impact on behavior actually increases (panel C column 5).

V. Externalities Associated with ADD

If children with undiagnosed ADD generate negative externalitites, these externalities should decline over time as diagnosis and treatment increase. In Table 4 column (1) I present results from a regression of reading test scores on the share of classmates with ADD (that is, who are ever diagnosed with ADD), grade level and an interaction between the two. For this analysis, the sample includes only those without ADD (those never

[7] Alternatively I also regressed the child's externalizing behavior problem index on two indicators: whether the child was evaluated for ADD but not diagnosed and whether the child was evaluated for ADD and diagnosed with ADD. I include all controls for classroom characteristics (class size, average income, share Hispanic, share black, share female) as well as child fixed effects and grade dummies. I find that being evaluated but not diagnosed has no impact on externalizing behavior (coefficient 0.005) but that being evaluated and diagnosed has a significant (at ten percent) impact on behavioral problems (coefficient -0.082). I repeat this for reading test scores and find that evaluation but not diagnosis is associated with a small negative and borderline significant impact on reading test scores and that evaluation and diagnosis has no impact on test scores.

diagnosed with ADD). In addition to individual fixed effects, I also include controls for class size, share black, Hispanic and female, average income of classmates, school type (public, private, parochial), and the share of special education students in class.

The estimated effect of having classmates with ADD is negative, but it declines significantly with grade progression. This is consistent with a hypothesis of peer behavior affecting cognitive achievement since children are increasingly diagnosed over time and diagnosis improves behavior. In columns 2 and 3 I drop the fifth grade and classes with special education students, respectively, and the results remain. In fact, they are larger when the fifth grade sample is dropped. This is likely due to the fact that those with the greatest behavioral problems are diagnosed earlier than fifth grade. As such, we would expect the greatest improvements between kindergarten and third grade. In columns 4 and 5 I stratify by gender: the effects are larger for boys than girls, I point to which I return.

A. Fixed Effects Estimation - Strategy

To estimate the impact of peers who exhibit disruptive behavior on student achievement, I estimate the following equation for the sample of children without ADD (defined as those who are never diagnosed with ADD):

$$Y_{ig} = \alpha + \beta_1 ADD_{-icg} + \beta_2 ADD\text{-}UNDIAG_{-icg} + \beta_3 X_{ig} + \beta_4 C_{-icg} + \beta_5 G_g + \beta_6 u_i + \varepsilon_{ig} \quad (1)$$

Where i indexes individual students, g grade and c classroom. Y_{icg} in the above equation refers to reading test scores taken in the Spring of each year; ADD_{-icg} refers to the share of students in the class with ADD (that is, who are ever diagnosed by 5th grade) and ADD-UNDIAG$_{-icg}$ refers to the share of classmates with undiagnosed ADD (and therefore untreated) excluding the focal child. X_{ig} refers to time varying student characteristics such as age and family income; C_{-icg} is a vector of classroom characteristics calculated over all students except the focal student and includes the share black, Hispanic and white, share female, average income and class size. G_g refers to a grade fixed effect (first, third and fifth grades – kindergarten omitted) and u_i to individual fixed effects. All regressions are weighted by the number of students sampled in the class.

The inclusion of individual fixed effects enables one to control for two important sources of omitted variables that could bias estimates of peer effects. The first is non-random selection into schools. The second is unobserved differences in family background of the child.

However, as previously noted there are two potential threats to identification that the fixed effect does not address. First, school administrators may non-randomly sort students across classrooms within grade based, in part, on behavior. As such, a diagnosis of ADD and an improvement in behavior may result in different peers. Second, the timing of diagnosis could be correlated with changes in teacher characteristics that could affect reading test scores directly in which case the resulting estimates would be biased and would not represent true causal effects. To address the former, I examine changes in peer characteristics (share of special education students in the class, share Hispanic, black and female, average BPI and log income) before and after a diagnosis of ADD (Table 5).

Changes in these six characteristics are small and in all cases but one (share Black) insignificant.[8]

To address the latter, I include multiple measures of teacher and classroom characteristics in the regression. In addition, I estimate discrete time hazard models to time of diagnosis to determine whether observed teacher and classroom characteristics are associated with timing of diagnosis (section VII). I find that they are not. Following Altonji, Elder and Taber (2005), I conclude that unobserved characteristics of the classroom are also unlikely to be correlated with the timing of diagnosis of ADD, since it is largely fixed over time.

B. Fixed Effects Estimation - Results

The results from estimating equation (1) on the sample of those never diagnosed with ADD are presented in Table 6. As the share of students in one's class with undiagnosed ADD increases, the test scores of his classmates declines. There is no significant impact of the share with ADD on test scores (the coefficient is small and positive but insignificant). The null effects for the share of the class with ADD is likely due to the inclusion of individual (and therefore school) fixed effects which substantially reduces the variation in this measure.

Because ADD is a condition that disproportionately affects boys and peer groups are largely gender specific at this age, one might expect the impact to be greater among other boys in the class (results in Table 4 also suggest greater effects for boys). In column (2) are results from a regression which includes an interaction between share

[8] Even for share Black, the difference (-.02) is relatively small (the average share black in the class in these data are .18) and we might expect that in testing 6 characteristics, one would be statistically significant by chance.

undiagnosed and male: the impact is much greater for boys than girls (for girls the effect can be positive or negative but always small and insignificant).

While the estimated coefficients on the interaction term share undiagnosed*male are negative and significant in all regressions, the estimated impact is small. Recall, that due to measurement error in the construction of the measure of classmates with undiagnosed ADD, the OLS results are attenuated by at least a factor of four (Ammermueller and Pischke, 2006). Once we account for this, the estimates imply that if a boy moves from a classroom where 8.5 percent of the students have undiagnosed ADD to a class where all are diagnosed (the standard deviation in these data), his test scores will improve by 1 point, or 10 percent of a standard deviation, still a relatively small effect.

To address the possibility that children may be sorted in classrooms according to past achievement and that this sorting may be correlated with the timing of diagnosis, I present results that include (in addition to the individual child fixed effects) the child's reading score in the previous survey period in column 3. These regressions must exclude all kindergarten students. The estimated coefficient on the term share undiagnosed*male is actually larger once I control for lagged reading scores, suggesting that if there is any sorting on past achievement, it is not driving the results.[9]

In column 4 I present estimates of equation 1 weighting not by the number of students in the class surveyed but the share of the class surveyed and in column 5 I present unweighted regressions. The results are not sensitive to weighting. In columns 6 and 7 I drop 5th grade and restrict the sample to non-attriters, respectively, and the results are unchanged.

[9] The estimated coefficient is larger only because the sample changes (it excludes kindergarten students.)

In column 8, I redefine the measure of the share of peers with undiagnosed ADD to be taken over all students in the grade, not just the classroom. This specification addresses the potential issue of non-random classroom assignment of students with undiagnosed ADD. The results are similar, though larger and significant for girls. This may be attributable to reductions in measurement error when one takes averages over a larger number of students.

The analysis sample excludes those who are ever diagnosed with ADD in an attempt to estimate the impact of peers with externalizing behavioral problems on those without such problems. However, it is likely that those who are evaluated for ADD exhibit behavioral problems, even though they are not diagnosed with ADD. In column 9 of Table 6 I also exclude those who are ever evaluated for ADD. The results are unchanged.

C. Undiagnosed ADD and Teacher/Classroom Characteristics

To explore whether resources can overcome negative peer effects, I re-estimate equation (1) including interactions between share undiagnosed and measures of classroom and teacher characteristics (Table 7). To avoid including triple interactions and ease interpretation, I limit the sample to boys on whom the effects are concentrated. Smaller class sizes can overcome the negative peer effects associated with untreated ADD. If there are 30 students in a class, the impact of share undiagnosed is -8.7. This implies that if the share undiagnosed declines by 8.5 percent (and accounting for measurement error) test scores would increase by 3 points. But if there are only 20 students in the class, the impact drops to -3.6 (1.2 points). This is consistent with

Lazear's (2001) disruption model of education production which stipulates that small class size mitigates the impact of disruptive peers on a student's ability to learn. There is also some suggestive evidence that teacher human capital can overcome negative peer effects. The estimates of the interaction between teacher human capital (master's degree) and share undiagnosed is large and positive but very imprecisely estimated and therefore only suggestive that higher quality teachers may be better able to manage disruptive students. This too could be consistent with Lazear's model which posits that disruptive students lower educational output of their classmates because they reduce effective teaching time if higher quality teachers are more effective in dealing with disruptive students.

In the next section, I relax the assumption of the exogeneity of the timing diagnosis of ADD entirely, relying instead on instrumental variables for identification of the impact of classmates with undiagnosed ADD on reading test scores.

VI. Instrumental Variable Estimates

A. Instruments for Classmates with Undiagnosed ADD

To instrument for the share of the class with undiagnosed ADD, I use recent expansions in eligibility for publicly provided child health insurance (SCHIP).[10] In 1997 Congress authorized SCHIP, greatly expanding children's eligibility for publicly provided health insurance. Though SCHIP was federally authorized and subsidized, individual states were free to develop their own SCHIP programs, subject to federal

[10] I cannot instrument for externalizing behavioral problems of classmates because the first stage is too weak: SCHIP/Medicaid eligibility levels are not strong predictors of externalizing behavioral problems, which is not surprising given that behavioral problems likely have many causes, only some of which may be amenable to medical treatment (and thus greater insurance coverage).

approval. As a result there was considerable heterogeneity in both the timing and scope of SCHIP programs across the states and we rely on this heterogeneity to identify the impact of SCHIP on diagnosis. Thirty-seven percent of the children in the analysis sample are eligible for SCHIP.

The underlying assumption of using SCHIP eligibility expansions as an instrument for share undiagnosed is that by increasing health insurance coverage, SCHIP expansions lower the cost of medical care, thereby lowering the cost of a medical diagnosis of ADD. In section VII of the paper, I provide evidence supporting the use of SCHIP eligibility expansions as an instrument in this context. I do so by establishing that SCHIP eligibility significantly increased the probability of ADD diagnosis via increases in health insurance coverage.

B. IV Estimates of the Impact of Undiagnosed ADD on Peer Outcomes

The first stage results of the IV analysis are presented in Appendix Table 1. The instruments for the share of classmates with undiagnosed ADD are the Medicaid/SCHIP eligibility thresholds in the state and year and the threshold interacted with the child's age. The endogenous variable is measured two ways: share of classmates with undiagnosed add (column 1 Appendix Table 1) and share of those with ADD who are undiagnosed (column 2 Appendix Table 1). The latter is set to zero in classes that have no students with ADD. The IV regressions include all covariates included in the previous OLS regressions, including the individual fixed effects.[11] The results of the first stage suggest that the increase in the eligibility threshold reduces the share of the class with

[11] The IV regressions are unweighted because 1) weighting led to less precise first stage estimates (a weaker first stage) and 2) evidence presented in Table 6 suggests that weighting has no impact on the results.

undiagnosed ADD, with the impact increasing with age of the child. For example, increasing the threshold from 100 to 300 percent of the federal poverty line will reduce the share of the class with undiagnosed ADD by 12 percentage points (column 1). The same increase in eligibility thresholds will reduce the share of those with ADD who are undiagnosed by 10 percentage points (column 2).

The second stage estimates of the impact of peers with undiagnosed ADD on reading test scores are presented in Table 8. I follow the method outlined in Newey, Powell and Vella (1999) for instrumenting for endogenous interactions (share undiagnosed*male).[12] As with the OLS fixed effect estimates I define the sample multiple ways: columns 1 and 5 contain estimates based on the full sample, columns 2 and 6 include the lagged reading test score (value added model), columns 3 and 7 exclude special education students and columns 4 and 8 exclude special education and 5th grade.

The results are generally consistent across the different specifications. As with the OLS fixed effect estimates, the interaction term (share undiagnosed*male) is negative and significant in most specifications while the main effect is always insignificant, though it varies in magnitude.[13] The one insignificant effect occurs when I exclude the fifth grade and the sample falls by almost a third in column 4 (though it remains significant in column 8.) The results generally imply that if the share of peers with undiagnosed ADD falls by .085 (the standard deviation), test scores will increase by 1.3

[12] This method involves estimating a first stage (regressing the share of peers with undiagnosed ADD on the instruments and other exogenous variables), generating a predicted value and a residual, interacting the predicted value and residual with male, and regressing the outcome (reading test scores) on the predicted value, its interaction, the residual and its interaction in a second stage regression. The standard errors are bootstrapped.

[13] For the full sample, the estimate of the interaction term is -14 and significant while the estimate of the main term is 10.8, large, positive and insignificant. The positive estimate on the main term seems to be driven by the 1500 special education students: when they are removed in column 3, the interaction term remains, but the main effect falls to 1.86.

points, or 15 percent of a standard deviation. The results in column 5-8 based on the alternative measure of undiagnosed peers, suggest that going from a class in which all those with ADD are undiagnosed to one in which they are all diagnosed will increase test scores by 2 points, or 22 percent of a standard deviation.

These estimates represent a moderate effect given previous work estimating that a one standard deviation increase in peer cognitive achievement increases student achievement by 35 percent of a standard deviation (Hanushek et al, 2003). They also suggest that treating students with ADD may be more cost effective than other school-based interventions. Based on previous estimates, it would cost $1430 to directly increase the test scores of ten boys by .2 of a standard deviation. [14] In contrast, treating two kids with ADD with medication for one year would cost $1100 and generate a similar impact on their peers.

VII. Robustness

In this section I provide evidence to support the underlying assumptions of the previous analyses. The OLS fixed effect estimates assume that the timing of ADD diagnosis is exogenous or at least not correlated with changing characteristics of classmates, teachers or schools that may also improve reading test scores. In the first sub-section I provide evidence supporting this assumption.

In the second sub-section I provide evidence to support using SCHIP eligibility expansions to instrument for share with undiagnosed ADD. The underlying assumption behind this instrument is that by increasing health insurance coverage, SCHIP expansions

[14] This calculation is based on work by Hedges et al, 1994 suggests that it costs $500 per student to increase test scores by .7 of a standard deviation.

lower the cost of medical care thereby lowering the cost of a diagnosis of ADD. I show that SCHIP eligibility increases the probability of health insurance coverage and increases the probability of a diagnosis of ADD in individual fixed effect regressions.

A. Timing of Diagnosis and Classroom Characteristics

To identify the impact of having classmates with undiagnosed ADD, I included individual fixed effect and exploited variation in the timing of ADD diagnosis amongst one's classmates. This strategy assumes that the timing of diagnosis is not correlated with classroom characteristics that may independently affect reading test scores. Diagnosis could be positively correlated with unobserved teacher quality if higher quality teachers are more perceptive, in which case the estimated relationship between children with undiagnosed ADD and peer test scores would be biased upward. On the other hand, if diagnosis is negatively correlated with unobserved teacher quality which might happen if poor quality teachers compensate by diagnosing (and treating) more students, then the estimates would be biased downward.

To provide evidence in support of the identifying assumption of random timing of diagnosis, I examine whether teacher, school or classroom characteristics significantly predict 1) the share of students with a diagnosis of ADD or 2) the share of students first diagnosed with ADD. For this I characterize each classroom in each school in each grade in the survey, of which there are over 7000 with complete data, to estimate whether characteristics of the teacher (masters degree, amount of experience, race) the school (Title I funds, nursing staff, special education staff, reading specialists, gum/art/music teachers and school size) or classmates (share female, black, average income and class size) significantly predict the share with undiagnosed ADD or the share first diagnosed in the classroom.

The first column of Table 9A includes results for the outcome "Share with undiagnosed ADD in the classrooms" based on the full sample, the second column excludes all classrooms with any special education students and the third includes classrooms with 15 percent or fewer special education students. F tests for school, teacher and classmate characteristics are presented below the regression results. For the full sample in column 1, school and teacher characteristics do not significantly predict the share with undiagnosed ADD in the class, but classmate characteristics do (F=8.26). The latter is largely driven by the share female, which is not surprising since students with ADD are predominantly male. In column 2 when I exclude all classrooms with any special education students, the F statistic for classroom characteristics falls to 1.4 (p-value of .23). In column 3 I include classrooms with a small share of special education students (less than 15 percent) all teacher, school and classmate characteristics remain jointly insignificant.

In Table 9B I examine whether the same teacher, school or classroom characteristics predict the share of children first diagnosed with ADD. For this specification I also include the share ever diagnosed with ADD in columns 2, 4 and 6 of the table to control for the population at risk for diagnosis. Without this control, the share female in the class is highly significant (again, not surprising). When I control for the population at risk of diagnosis, the results suggest that diagnosis of ADD is not correlated with observed teacher, classroom or school characteristics that may independently affect cognitive achievement. Following Altonji, Elder and Taber (2005), I conclude that unobserved characteristics of the classroom are also unlikely to be correlated with diagnosis of ADD.

B. Supporting Evidence: SCHIP Eligibility, Health Insurance and ADD Diagnosis

In this sub-section I show that eligibility for SCHIP is associated with health insurance coverage and diagnosis by estimating the following equation:

$$Y_{it} = \alpha + \beta_1 \text{Eligble}_{it} + \beta_2 \text{Eligible}_{it} * \text{age} + \beta_2 \text{age}_{it} + \beta_3 \ln(\text{income})_{it} + \beta_4 \text{grade}_t + u_i + \varepsilon_{it} \quad (2)$$

Where Y is an indicator for any health insurance or for being diagnosed with ADD, depending on the regression; eligible is an indicator equal to one if the child is eligible for SCHIP and is interacted with age; income, age, and grade controls are included as well as individual fixed effects. The instruments for eligible and eligible*age in the above equation are the state SCHIP eligibility level (as a percent of the federal poverty line for a child of that age in that state) and the SCHIP eligibility level interacted with age. The first stage of this regression is presented in columns 4 and 5 of Appendix Table 2: expanding eligibility thresholds significantly increases the probability that a child will be eligible for SCHIP.

IV estimates suggest that becoming eligible for SCHIP does increase health insurance coverage and diagnosis (columns 1 and 2 of Appendix Table 2). For diagnosis, the impact of SCHIP eligibility increases with age. In column 3 I present reduced form estimates of the impact of SCHIP eligibility levels as a function of the FPL on the probability of diagnosis: increasing eligibility threshold from 100 to 200 percent of the federal poverty level increases the probability of diagnosis by .5 percentage points for five year olds and one percentage point for ten year olds. This represents a reasonable effect given an underlying rate of diagnosis of five percent for ten year olds.

VIII. Conclusions

After establishing the presence of peer effects in education, the literature is increasingly turning to understanding the mechanism(s) underlying the relationship. Recent theoretical and empirical work has focused on peer behavior as one of the potential mechanisms. In this paper I use a unique identification strategy to identify the impact of classmate behavior on cognitive achievement. Children with ADD are more likely to have behavioral problems. Once diagnosed, however, their behavior improves. By focusing on classmates with ADD before and after diagnosis, I can estimate the impact of a change in behavior holding family background and ability constant. In individual fixed effect regressions, I find that the classmates of those with undiagnosed ADD suffer worse scores on reading achievement tests, but the results are concentrated among boys. These results are robust to a number of alternative specifications and instrumental variable estimation. I also find that resources including class size and teacher human capital can overcome the negative peer effects observed, consistent with the “disruption” model of education production proposed by Lazear (2001).

These results have two important policy implications. First, the findings that schools and teachers can both affect peer behavior and mitigate the negative effects of peer behavior suggest that peer effects should be considered within their institutional framework. As such, policy discussions need not be limited to how best to compose classrooms to maximize peer effects. Rather, policies that also consider the ways in which teacher, school, and community resources (health care in this case) influence or mitigate peer effects via student behavior may ultimately be easier to implement and just as effective. A second implication regards the relationship between health, productivity and growth. Specifically, these results suggest that mental health may affect growth, through both its impact on the human capital accumulation of those with a mental disorder and the externalities imposed on others. As such, any policy debate over the adoption or payment of mental health treatment should consider these externalities in any cost-benefit analysis.

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Table 1 Summary Statistics Stratified by Whether Child Ever Diagnosed (by Fifth Grade) with ADD

	All		Male	
	Never Diagnosed	**Diagnosed**	**Never Diagnosed**	**Diagnosed**
Male	0.49	0.74		
Black	0.13	0.12	0.13	0.11
Hispanic	0.17	0.10	0.17	0.1
Any Insurance	0.83	0.91	0.83	0.91
Medicaid	0.18	0.24	0.18	0.23
Family Income	$60,120	$56,077	$60,675	$57,459
Per capita Income	$10,626	$9,902	$10,500	$10,542
Public School	0.80	0.81	0.8	0.82
Catholic School	0.12	0.12	0.12	0.1
Class size	21.34	20.70	21.3	20.6
Teacher has masters	0.33	0.35	0.33	0.35
Share black in class	0.10	0.09	0.1	0.09
Share female in class	0.50	0.48	0.48	0.47
Reading test score	51.39	47.74	50.66	47.8
Self control (scale 1-4)	3.22	2.77	3.12	2.72
Externalizing behavior problelms (scale 1-4)	1.63	2.16	1.75	2.24
Always works to best of Ability	0.29	0.08	0.22	0.06

Table 2 Follow up of the 19,985 Students Interviewed in Kindergarten

Last grade observed	Observations	Income	Male	Black	Ever diagnosed with ADD
Kindergarten	4040	$53,199	0.51	0.17	0.02
First	2443	$53,535	0.53	0.21	0.04
Third	3057	$53,085	0.53	0.20	0.07
Fifth	10445	$59,449	0.50	0.12	0.07
Total	19985				

Table 3 Impact of Diagnosis on Own Outcomes

	All		No Sp Ed students		Evaluated for ADD	
Panel A Excluding Class Characteristics	**Read Score**	**Extern. Behav.**	**Read Score**	**Extern. Behav.**	**Read Score**	**Extern. Behav.**
Diagnosed with ADD/ADHD	0.165	-0.053	0.625	-0.082	0.703	-0.066
	[0.248]	[0.022]	[0.248]	[0.022]	[0.282]	[0.029]
Age	-1.218	0.036	-0.772	0.026	-1.86	0.075
	[0.206]	[0.018]	[0.201]	[0.017]	[0.604]	[0.063]
Ln(income)	0.009	-0.008	-0.016	-0.005	-0.039	-0.016
	[0.058]	[0.005]	[0.055]	[0.005]	[0.130]	[0.013]
First grade	2.357	-0.049	1.605	-0.038	3.62	-0.101
	[0.373]	[0.032]	[0.364]	[0.031]	[1.094]	[0.113]
Third grade	4.807	-0.063	3.245	-0.034	7.223	-0.223
	[0.786]	[0.068]	[0.765]	[0.066]	[2.304]	[0.238]
Fifth grade	7.425	-0.16	4.942	-0.118	11.19	-0.415
	[1.191]	[0.103]	[1.159]	[0.100]	[3.489]	[0.361]
Student in Special Ed	-0.251	-0.021				
	[0.191]	[0.017]				
Share Special Ed in class	0.356	-0.021				
	[0.235]	[0.021]				
Observations	42590	41148	45551	42914	7046	6600
R-squared	0.84	0.74	0.82	0.72	0.82	0.69
Panel B Including Class Characteristics						
Diagnosed with ADD/ADHD	-0.019	-0.041	0.396	-0.075	0.616	-0.064
	[0.268]	[0.023]	[0.273]	[0.024]	[0.310]	[0.031]
Observations	36664	37094	38341	38646	5900	5907
R-squared	0.85	0.75	0.83	0.73	0.83	0.7
Panel C Impact on Future Outcomes & Including Class Characteristics						
Diagnosed with ADD/ADHD	-0.547	-0.107	-0.039	-0.131	-0.082	-0.119
	[0.362]	[0.035]	[0.398]	[0.038]	[0.436]	[0.049]
Observations	25372	23353	25065	22921	3932	3626
R-squared	0.9	0.79	0.88	0.78	0.89	0.75

Standard errors in brackets
All regressions include individual child fixed effects

Table 4 Impact of Share Ever Diagnosed with ADD on Reading Test Scores of Peers Over Time

	All	Drop 5th	Drop Spec. Ed	Female	Male
Share of class ever diagnosed with ADD	-4.085	-6.455	-4.567	-3.508	-4.647
	[1.076]	[1.433]	[1.044]	[1.446]	[1.598]
Share ever diagnosed*grade	1.687	3.391	1.925	1.219	2.167
	[0.404]	[0.681]	[0.398]	[0.548]	[0.595]
Grade	0.195	0.099	0.207	0.156	0.244
	[0.046]	[0.076]	[0.044]	[0.063]	[0.068]
Teacher has masters degree	0.165	0.308	0.162	0.123	0.216
	[0.093]	[0.114]	[0.090]	[0.126]	[0.137]
Class size	-0.028	-0.039	-0.028	-0.025	-0.034
	[0.010]	[0.014]	[0.010]	[0.014]	[0.016]
Share Hispanic students in class	-0.405	-0.107	-0.468	-0.404	-0.438
	[0.315]	[0.362]	[0.297]	[0.422]	[0.473]
Share black students in class	-1.667	-1.484	-1.57	-1.611	-1.721
	[0.255]	[0.291]	[0.244]	[0.348]	[0.375]
Share female in class	0.685	0.81	0.581	0.489	0.935
	[0.514]	[0.635]	[0.496]	[0.714]	[0.745]
Class avg. income in $10000	-0.017	-0.043	-0.016	-0.041	0.008
	[0.020]	[0.023]	[0.018]	[0.027]	[0.029]
Public School	1.385	1.377	1.024	0.878	1.97
	[0.641]	[0.761]	[0.487]	[0.732]	[1.159]
Catholic School	2.3	1.811	2.076	3.066	1.433
	[0.725]	[0.848]	[0.638]	[0.935]	[1.144]
Student in Special Ed	-0.173	-0.007		0.556	-0.63
	[0.292]	[0.371]		[0.454]	[0.378]
Share Special Ed in class	0.77	0.777		0.936	0.604
	[0.354]	[0.489]		[0.479]	[0.523]
Observations	40623	33345	43465	20881	19742
R-squared	0.85	0.88	0.85	0.84	0.85

Robust standard errors in brackets

Table 5 Change in Peer Characteristics After Diagnosis

Share Special Education Students	0.0023
Share Hispanic	-0.000017
Share Black	-0.02
Share Female	0.0025
Average Log Income	-0.052
Average Externalizing Behavioral Problems	0.009

Table 6 Impact of Peer Diagnosis on Reading Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Lagged Read Score	Reweight	No Weight	Drop 5th Grade	Non-attriters	Share Grade	Drop Eval.
Share undiagnosed	-1.636	-0.673	1.015	-0.067	-0.008	0.3	0.324	-3.309	-0.663
	[0.699]	[0.868]	[1.054]	[0.781]	[0.699]	[0.903]	[0.805]	[1.307]	[0.866]
share unidagnosed*male		-1.959	-3.24	-1.978	-1.937	-1.929	-2.047	-3.372	-1.936
		[1.116]	[1.511]	[1.139]	[0.898]	[1.319]	[1.156]	[1.651]	[1.112]
Share of class ever diagnosed with ADD	0.674	0.664	0.684	0.638	0.452	1.088	0.481	1.908	0.734
	[0.478]	[0.477]	[0.514]	[0.486]	[0.377]	[0.694]	[0.473]	[0.942]	[.490]
Age	-0.775	-0.776							
	[0.381]	[0.382]							
Ln(income)	-0.016	-0.017							
	[0.087]	[0.087]							
First grade	1.978	1.979							
	[0.679]	[0.680]							
Third grade	3.624	3.627							
	[1.444]	[1.446]							
Fifth grade	5.436	5.441							
	[2.195]	[2.198]							
Teacher has masters degree	0.091	0.091							
	[0.104]	[0.104]							
Class size	-0.031	-0.031							
	[0.012]	[0.012]							
Share Hispanic students in class	-0.757	-0.759							
	[0.424]	[0.424]							
Share black students in class	-2.086	-2.087							
	[0.365]	[0.365]							
Share female in class	0.648	0.651							
	[0.578]	[0.578]							
Class avg. income in $10000	-0.008	-0.008							
	[0.023]	[0.023]							
Public School	2.049	2.057							
	[0.709]	[0.709]							
Catholic School	2.444	2.447							
	[0.772]	[0.772]							
Student in Special Ed	0.064	0.051							
	[0.330]	[0.329]							
Share Special Ed in class	0.629	0.64							
	[0.393]	[0.393]							
Observations	34355	34355	23517	34355	36664	27540	25936	39938	29932
R-squared	0.85	0.85	0.87	0.85	0.85	0.88	0.81	0.85	0.84
Robust standard errors in brackets									

Table 7 School Resources, Class Behavior and Peer Effects - Males Only

	(1)	(2)	(3)
Share of class ever diagnosed with ADD	1.69	1.713	1.686
	[0.714]	[0.714]	[0.714]
Share undiagnosed	-25.582	-3.822	-24.83
	[12.126]	[1.222]	[12.110]
Share undiagnosed*Teacher has Masters	1.941	1.925	
	[1.624]	[1.630]	
Share undiagnosed*ln((40-Classize)	7.35		7.349
	[4.050]		[4.061]
Age	-1.189	-1.169	-1.193
	[0.495]	[0.497]	[0.495]
Ln(income)	-0.076	-0.078	-0.074
	[0.124]	[0.124]	[0.124]
First grade	2.902	2.867	2.912
	[0.890]	[0.894]	[0.891]
Third grade	5.228	5.157	5.247
	[1.879]	[1.888]	[1.881]
Fifth grade	8.035	7.937	8.065
	[2.856]	[2.869]	[2.859]
Teacher has masters degree	0.11	0.098	0.161
	[0.158]	[0.158]	[0.152]
Class size	-0.02	-0.032	-0.02
	[0.018]	[0.018]	[0.018]
Share Hispanic students in class	-0.826	-0.781	-0.832
	[0.630]	[0.631]	[0.629]
Share black students in class	-2.17	-2.119	-2.16
	[0.559]	[0.557]	[0.560]
Share female in class	0.744	0.746	0.722
	[0.850]	[0.851]	[0.850]
Class avg. income in $10000	0.034	0.032	0.033
	[0.034]	[0.034]	[0.034]
Public School	2.293	2.302	2.323
	[1.280]	[1.280]	[1.279]
Catholic School	1.329	1.359	1.365
	[1.252]	[1.250]	[1.251]
Student in Special Ed	-0.473	-0.47	-0.484
	[0.413]	[0.417]	[0.414]
Share Special Ed in class	0.558	0.563	0.55
	[0.573]	[0.574]	[0.573]
Observations	16641	16646	16641
R-squared	0.85	0.85	0.85

Robust standard errors in brackets

Table 8 IV Impact of Undiagnosed ADD on Others' Reading Test Scores								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	Lagged Read	No Spec Ed	No Sped Ed/5th Grade	All	Lagged Read	No Spec Ed	No Sped Ed/5th Grade
Share undiagnosed (predicted)	10.83	1.367	1.856	-9.873				
	[23.300]	[36.289]	[25.180]	[40.237]				
Share undiagnosed(predicted)*male	-14.126	-11.457	-15.387	-11.994				
	[4.847]	[6.110]	[4.565]	[8.352]				
Share of those with ADD undiagnosed (predicted)					1.346	-2.917	0.236	-1.7
					[2.897]	[5.400]	[3.189]	[4.346]
Share of those with ADD undiagnosed(predicted)*male					-2.236	-1.92	-2.468	-1.952
					[0.682]	[0.970]	[0.694]	[1.037]
first stage residual	-0.066	1.165	-0.282	0.17	-0.043	0.323	-0.058	0.008
	[0.858]	[1.296]	[0.943]	[1.256]	[0.186]	[0.266]	[0.193]	[0.219]
first stage residual*male	-1.453	-3.754	-0.993	-1.695	-0.191	-0.82	-0.11	-0.244
	[1.497]	[1.659]	[1.406]	[1.974]	[0.267]	[0.366]	[0.288]	[0.336]
age	-0.967	2.933	-0.801	-1.147	-0.975	2.56	-0.811	-1.164
	[0.348]	[0.901]	[0.288]	[0.475]	[0.388]	[0.941]	[0.366]	[0.487]
ln(income)	0.028	0.161	0.001	-0.06	0.027	0.187	0.003	-0.052
	[0.073]	[0.127]	[0.085]	[0.123]	[0.083]	[0.147]	[0.067]	[0.108]
first grade	2.523		2.024	2.404	2.507		2.017	2.37
	[0.719]		[0.576]	[0.777]	[0.763]		[0.654]	[0.883]
third grade	4.384	-6.153	3.463	4.479	4.351	-5.679	3.472	4.467
	[1.450]	[6.431]	[1.139]	[1.642]	[1.559]	[6.018]	[1.341]	[1.790]
fifth grade	6.602	-11.773	5.229		6.554	-10.814	5.256	
	[2.189]	[6.574]	[1.709]		[2.379]	[6.004]	[2.033]	
Student in Special Ed	-0.582	-0.496			-0.585	-0.566		
	[0.300]	[0.481]			[0.344]	[0.413]		
Teacher has masters degree	0.031	-0.054	0.037	0.106	0.032	-0.078	0.032	0.086
	[0.089]	[0.102]	[0.084]	[0.107]	[0.094]	[0.111]	[0.116]	[0.122]
class size	-0.024	-0.02	-0.024	-0.031	-0.024	-0.009	-0.022	-0.02
	[0.010]	[0.014]	[0.011]	[0.017]	[0.015]	[0.021]	[0.015]	[0.027]
% Hispanic students in class	-0.902	0.106	-0.749	-0.264	-0.884	0.095	-0.723	-0.168
	[0.512]	[0.469]	[0.475]	[0.612]	[0.440]	[0.589]	[0.493]	[0.631]
% black students in class	-2.097	0.415	-1.906	-1.815	-2.077	0.375	-1.918	-1.882
	[0.356]	[0.720]	[0.361]	[0.456]	[0.378]	[0.676]	[0.368]	[0.460]
% female in class	0.476	0.147	0.3	0.165	0.443	-0.038	0.324	0.211
	[0.651]	[0.762]	[0.614]	[1.129]	[0.593]	[0.596]	[0.569]	[0.946]
Class avg. income in $10000	0.019	0.037	0.013	-0.015	0.018	0.037	0.014	-0.013
	[0.021]	[0.025]	[0.023]	[0.032]	[0.018]	[0.026]	[0.017]	[0.024]
Public School	0.66	0.926	0.876	0.495	0.709	0.961	0.825	0.289
	[0.556]	[0.910]	[0.551]	[0.762]	[0.422]	[0.577]	[0.459]	[0.656]
Catholic School	0.784	1.698	0.947	0.152	0.8	1.937	0.977	0.237
	[0.581]	[0.853]	[0.613]	[0.777]	[0.548]	[0.829]	[0.593]	[0.782]
Share Special Ed in class	0.385	0.989	1.381	1.678	0.393	1.29	1.412	1.797
	[0.409]	[0.824]	[0.659]	[1.401]	[0.460]	[0.646]	[0.603]	[1.250]
lagged reading score		0.004				0.002		
		[0.013]				[0.011]		
Observations	34998	26518	33564	25291	34998	26518	33564	25291

All regressions include individual fixed effects
Bootstrapped standard errors in brackets

Table 9A Relationship between Share with Undiagnosed ADD and Classroom/School Characteristics

	Full Sample	Excludes Sp Ed	Excludes >.15 Sp Ed
Teacher Characteristics			
Teacher has masters	0.00066	0.00365	0.00346
	[0.00269]	[0.00328]	[0.00325]
Teaching experience	-0.00006	-0.00022	-0.00022
	[0.00012]	[0.00015]	[0.00014]
Black teacher	0.01216	0.01898	0.01902
	[0.00647]	[0.00839]	[0.00832]
White teacher	0.01194	0.01303	0.01321
	[0.00471]	[0.00608]	[0.00602]
Hispanic teacher	-0.00085	0.00603	0.00602
	[0.00526]	[0.00726]	[0.00723]
School Characteristics			
School received Title I Funds	0.00044	-0.00015	-0.00011
	[0.00113]	[0.00120]	[0.00116]
Nurse FTE	-0.00015	0.00054	0.00045
	[0.00064]	[0.00099]	[0.00097]
Special Ed FTE	0.00051	-0.00009	-0.00007
	[0.00036]	[0.00049]	[0.00048]
Reading Specialists FTE	-0.00037	-0.00179	-0.00166
	[0.00053]	[0.00075]	[0.00074]
Gym/Art/Music teachers FTE	0.00058	0.00083	0.00081
	[0.00056]	[0.00076]	[0.00075]
Small School 0-149	0.00193	0.00479	0.00386
	[0.00863]	[0.01201]	[0.01162]
Moderate School 150-299	0.00385	0.00508	0.00556
	[0.00554]	[0.00666]	[0.00649]
Medium School 300-499	0.0011	0.00498	0.00505
	[0.00409]	[0.00471]	[0.00466]
Large School 500-749	0.0021	0.00528	0.00527
	[0.00356]	[0.00388]	[0.00386]
Classmate Characteristics			
% Female in class	-0.05808	-0.02073	-0.02276
	[0.01355]	[0.01511]	[0.01490]
% black in class	-0.01838	-0.01156	-0.01115
	[0.00553]	[0.00705]	[0.00697]
Average income in class	-0.0001	-0.00041	-0.00039
	[0.00032]	[0.00031]	[0.00030]
Class size	-0.0007	-0.00044	-0.00044
	[0.00026]	[0.00039]	[0.00038]
First grade	-0.01027	-0.01389	-0.01412
	[0.00239]	[0.00353]	[0.00348]
Third grade	-0.02612	-0.02587	-0.02572
	[0.00397]	[0.00521]	[0.00516]
Fifth grade	-0.0473	-0.0441	-0.04449
	[0.00334]	[0.00383]	[0.00379]
Observations	7120	6254	6345
R-squared	0.07	0.05	0.05
Regressions weighted by number of students sampled in each class			
F test school characteristics	0.58	1.41	1.47
p value	0.8184	0.2159	0.196
F test teacher characteristics	1.4	0.93	0.85
p value	0.2217	0.4933	0.5738
F test classmate characteristics	8.26	1.4	1.45
p value	0	0.2305	0.2146

Table 9B Hazard Model of Time to First Diagnosis

	Full Sample		Excludes All Sp Ed		Excludes >.15 Sp Ed	
Teacher Characteristics						
Teacher has masters	0.00012	-0.00271	0.00192	-0.00087	0.00168	-0.00101
	[0.00294]	[0.00241]	[0.00275]	[0.00226]	[0.00272]	[0.00223]
Teaching experience	0.00021	0.00019	0.00012	0.00014	0.00014	0.00015
	[0.00016]	[0.00013]	[0.00015]	[0.00012]	[0.00014]	[0.00012]
Black teacher	-0.00706	-0.00738	-0.00968	-0.00986	-0.00946	-0.00967
	[0.00867]	[0.00729]	[0.00877]	[0.00700]	[0.00870]	[0.00694]
White teacher	-0.00802	-0.00881	-0.01317	-0.01254	-0.01279	-0.01233
	[0.00644]	[0.00501]	[0.00674]	[0.00515]	[0.00667]	[0.00510]
Hispanic teacher	-0.00943	-0.00317	-0.00918	-0.00534	-0.0087	-0.00496
	[0.00495]	[0.00420]	[0.00459]	[0.00396]	[0.00457]	[0.00396]
School Characteristics						
School received Title I Funds	0.00132	0.00044	0.00073	0.00011	0.00087	0.00024
	[0.00085]	[0.00071]	[0.00073]	[0.00064]	[0.00072]	[0.00063]
Nurse FTE	-0.00048	-0.00057	-0.00022	-0.00049	-0.00034	-0.00057
	[0.00083]	[0.00065]	[0.00080]	[0.00064]	[0.00079]	[0.00063]
Special Ed FTE	-0.00009	-0.00013	-0.00005	-0.00001	0.00001	0.00004
	[0.00040]	[0.00029]	[0.00051]	[0.00035]	[0.00050]	[0.00034]
Reading Specialists FTE	0.00012	0.00069	0.00022	0.00101	0.00019	0.00095
	[0.00069]	[0.00057]	[0.00066]	[0.00055]	[0.00065]	[0.00054]
Gym/Art/Music teachers FTE	-0.00014	-0.00028	0.00008	-0.00031	0.00012	-0.00027
	[0.00055]	[0.00047]	[0.00052]	[0.00044]	[0.00052]	[0.00044]
Small School 0-149	-0.00212	-0.00063	0.00437	0.00218	0.00359	0.00205
	[0.00593]	[0.00549]	[0.00556]	[0.00544]	[0.00544]	[0.00529]
Moderate School 150-299	0.00597	0.00219	0.00795	0.00456	0.00858	0.00469
	[0.00524]	[0.00410]	[0.00506]	[0.00406]	[0.00491]	[0.00396]
Medium School 300-499	0.00447	0.00193	0.00295	0.00079	0.00292	0.00076
	[0.00389]	[0.00316]	[0.00349]	[0.00294]	[0.00346]	[0.00291]
Large School 500-749	0.0036	0.00014	0.00218	-0.00113	0.00239	-0.00097
	[0.00355]	[0.00270]	[0.00303]	[0.00246]	[0.00304]	[0.00246]
Classmate Characteristics						
Share female in class	-0.0577	-0.01406	-0.02037	-0.00498	-0.02123	-0.00476
	[0.01793]	[0.01458]	[0.01281]	[0.01039]	[0.01263]	[0.01026]
Share black in class	-0.01508	-0.00232	-0.00962	-0.0032	-0.00933	-0.00302
	[0.00732]	[0.00622]	[0.00622]	[0.00532]	[0.00618]	[0.00527]
Average income in class	-0.00084	-0.00039	-0.00058	-0.00032	-0.00056	-0.00031
	[0.00032]	[0.00025]	[0.00024]	[0.00021]	[0.00024]	[0.00021]
Class size	-0.00085	0.00012	-0.00032	0.00015	-0.00032	0.00015
	[0.00046]	[0.00035]	[0.00040]	[0.00031]	[0.00040]	[0.00031]
First grade	0.00874	0.00569	0.00461	0.00541	0.00433	0.00519
	[0.00268]	[0.00236]	[0.00249]	[0.00228]	[0.00245]	[0.00225]
Third grade	0.01857	0.01017	0.00768	0.00893	0.00804	0.00904
	[0.00485]	[0.00385]	[0.00428]	[0.00365]	[0.00426]	[0.00361]
Fifth grade	0.02032	0.00883	0.01627	0.01294	0.0158	0.0125
	[0.00513]	[0.00405]	[0.00534]	[0.00419]	[0.00527]	[0.00414]
Share in classroom ever diagnosed		0.33525		0.31879		0.3195
		[0.02329]		[0.02819]		[0.02806]
Observations	7120	7120	6254	6254	6345	6345
R-squared	0.02	0.33	0.02	0.32	0.02	0.32
Regressions weighted by number of students sampled in each class						

F test teacher characteristics	1.05	0.96	1.34	1.27	1.31	1.29
p value	0.3881	0.4384	0.2429	0.2725	0.2588	0.266
F test school characteristics	0.93	0.45	0.5	0.63	0.61	0.59
p value	0.501	0.9075	0.8757	0.7719	0.7871	0.8072
F test classmate characteristics	4.58	0.8	2.46	0.6	2.41	0.55
p value	0.0011	0.5253	0.0438	0.6593	0.0477	0.6972

Appendix Table 1: First Stage Regressions		
	(1)	(2)
	Share of class with undiagnosed ADD	Share of those with ADD Currently Undiagnosed
Medicaid/SCHIP Eligibility Level	-0.00246	-0.01233
	[0.00271]	[0.01401]
Medicaid/SCHIP Eligibility Level*age	-0.00063	-0.00515
	[0.00026]	[0.00136]
age	-0.0021	-0.01499
	[0.00290]	[0.01616]
ln(income)	-0.00007	0.00195
	[0.00090]	[0.00449]
first grade	-0.00859	-0.06354
	[0.00522]	[0.02903]
third grade	-0.01442	-0.08884
	[0.01089]	[0.06073]
fifth grade	-0.02102	-0.11533
	[0.01650]	[0.09199]
Student in Special Ed	-0.00332	-0.01329
	[0.00328]	[0.01569]
Teacher has masters degree	-0.00013	-0.00714
	[0.00091]	[0.00433]
class size	0.00018	0.00344
	[0.00009]	[0.00048]
% Hispanic students in class	0.00869	0.07606
	[0.00344]	[0.01579]
% black students in class	0.0065	0.02338
	[0.00306]	[0.01619]
% female in class	-0.01574	-0.07394
	[0.00452]	[0.02279]
Class avg. income in $10000	-0.00039	-0.00136
	[0.00020]	[0.00095]
Public School	0.00946	-0.00933
	[0.00383]	[0.02038]
Catholic School	0.00567	0.05773
	[0.00434]	[0.02437]
Share Special Ed in class	0.00734	0.08257
	[0.00438]	[0.01932]
Observations	34998	34998
R-squared	0.48	0.52
All regressions include individual fixed effects		
Robust standard errors in brackets		

Appendix Table 2 Impact of Medicaid Eligibility Status on Insurance Coverage and Diagnosis- First Stage, Reduced Form and IV Estimates

	FE-IV	FE-IV	Reduced Form	First Stage	First Stage
	Any Health Insurance	**Diagnosed with ADD**	**Diagnosed with ADD**	**Eligible for Medicaid**	**Eligible for Medicaid*age**
Eligible for Medicaid/SCHIP	0.115	-0.025			
	[0.051]	[0.031]			
Eligible for Medicaid*age	-0.005	0.009			
	[0.006]	[0.004]			
Medicaid eligibility level			-0.003	0.129	-0.027
			[0.004]	[0.008]	[0.071]
Medicaid eligibility level*age			0.001	0	0.13
			[0.000]	[0.001]	[0.008]
Age	-0.002	0.005	0.004	-0.004	-0.089
	[0.004]	[0.003]	[0.003]	[0.006]	[0.050]
First grade	0.012	0.002	0.003	0.018	0.126
	[0.007]	[0.005]	[0.005]	[0.010]	[0.089]
Third grade	0.032	0.005	0.008	0.041	0.311
	[0.013]	[0.010]	[0.010]	[0.021]	[0.182]
Fifth grade	0.048	0.009	0.013	0.054	0.445
	[0.020]	[0.015]	[0.015]	[0.031]	[0.275]
Ln(income)	0.034	0.004	-0.002	-0.115	-0.959
	[0.002]	[0.002]	[0.001]	[0.002]	[0.017]
Observations	43194	43194	43194	43194	43194
Number of childid	14593	14593	14593	14593	14593

Standard errors in brackets

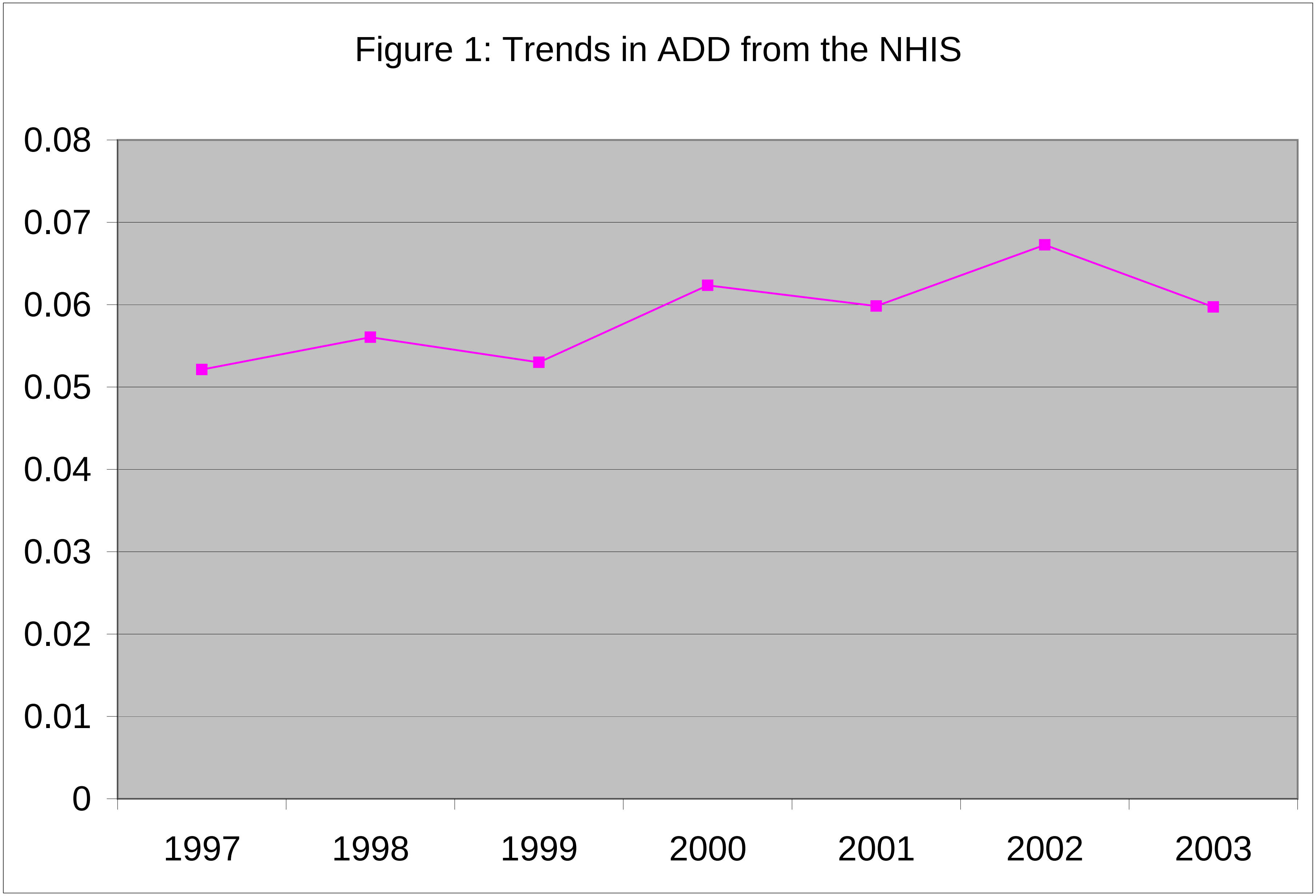
Figure 1: Trends in ADD from the NHIS
0.08
0.07
0.06
0.05
0.04
0.03
0.02
0.01
0
1997
1998
1999
2000
2001
2002
2003