NBER WORKING PAPER SERIES

SOCIETAL DISRUPTIONS AND CHILD MENTAL HEALTH: EVIDENCE FROM ADHD DIAGNOSIS DURING THE COVID-19 PANDEMIC

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Working Paper 30909
http://www.nber.org/papers/w30909

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2023

We would like to thank Lauren Jones, Martin Anderson, Tony LoSasso, Bill Brinkman, and the participants of the American Society of Health Economists conference, Association for Public Policy Analysis and Management conference, Mid-Midwest Applied Microeconomics Workshop, and the special issue conference hosted by the Center for Health and Wellbeing at Princeton for valuable comments and suggestions. We also thank the Regenstrief Institute for facilitating access to data from the Indiana Network for Patient Care. The views here do not represent those of the Federal Reserve Bank of Chicago or the Federal Reserve System. All errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Societal Disruptions And Child Mental Health: Evidence From ADHD Diagnosis During The COVID-19 Pandemic
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NBER Working Paper No. 30909
February 2023
JEL No. I0,I12

ABSTRACT

We study how the societal disruptions of the COVID-19 pandemic impacted diagnosis of a prevalent childhood mental health condition, Attention Deficit Hyperactivity Disorder (ADHD). Using both nationwide private health insurance claims and a single state's comprehensive electronic health records, we compare children exposed to the pandemic to same aged children prior to the pandemic. We find the pandemic reduced new ADHD diagnoses by 8.6% among boys and 11.0%among girls nationwide through February 2021. We further show that higher levels of in-person schooling in Fall 2020 dampened the decline for girls but had no moderating effect for boys.

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1 Introduction

Recognizing and diagnosing mental and behavioral health conditions in childhood has important implications for child well-being. It can affect health and other outcomes related to education, risky behaviors, crime, and long-run economic outcomes (see Currie 2020 for a recent review and discussion). In practice, diagnosing children with mental or behavioral health conditions involves interactions between a diverse and decentralized collection of people, including parents, teachers, and physicians. Understanding if and how the COVID-19 pandemic disrupted these interactions and affected discovery of children with mental and behavioral health needs is essential for efforts to improve child well-being in a post-pandemic world.

This paper examines how child mental health diagnosis changed during the COVID-19 pandemic with a focus on diagnosis of initial cases of a common child mental health condition: Attention Deficit Hyperactivity Disorder (ADHD). We use a nationwide private health insurance claims database and a state-specific electronic health records database to analyze how the flow of new ADHD diagnoses was impacted over the first year of the COVID-19 pandemic. Our nationwide analysis uses Optum's de-identified Clinformatics® Data Mart system, a comprehensive commercial claims database with nationwide coverage. Our state specific analysis uses the Regenstrief Institute's Indiana Network for Patient Care (INPC) research database. These two data sets are complementary, as one provides nationwide coverage from a large private insurer, and the other includes all payers in a single state with finer geographic detail.

In both data sets, our research design compares two cohorts of elementary school children: one from February 2019-February 2021 that was affected by the pandemic beginning in March 2020 and a previous cohort from February 2018-February 2020 that was not affected by the pandemic. We compare changes in new diagnosis rates between these two cohorts, where members of the pre-pandemic cohort serve as a control group for members of the pandemic cohort at the same age. In other words, we compare how diagnosis rates evolved over a nineteen month period for children of the same age but at different points in calendar time. We estimate both event study and difference-in-difference models, and we conduct each analysis separately by gender given evidence that ADHD presents differently in boys and girls.

We find a substantial decline in new ADHD diagnosis starting in March 2020 and extending into early 2021. Overall, our estimates imply that new ADHD diagnosis fell by 8.6% among boys and by 11.0% among girls in our nationwide analysis. The fall in diagnosis was concentrated among white and Hispanic children with a negligible decrease among Black children. In Indiana, we find a higher effect among boys (18% decrease) and a not statistically significant 7% decrease among girls. We provide a detailed discussion of potential mechanisms that likely contribute to this decline in initial ADHD diagnosis rates, and emphasize the role of schools in the diagnosis process.

We explore heterogeneity by in-person school activity to examine how the sudden contraction and subsequent reopening of in-person schooling – one of the main elements of the child mental health system – affected patterns of initial ADHD diagnosis during the COVID-19 pandemic. We compare the size of the ADHD diagnosis fall across areas with higher vs lower levels of in-person schooling, using SafeGraph Mobility data to measure physical activity near schools as a proxy for the level of in-person activity. In our nationwide Optum analysis we can only compare outcomes across states with different opening levels. In our Indiana analysis, we can measure school activity at a much finer geographic level in which we can identify the student's likely zoned school. The results suggest that the pandemic depressed ADHD diagnosis more in states with lower levels of in-person school activity. The moderating effect of in-person schooling is strongest for girls, in our nationwide analysis. In particular, there is some evidence that – for girls – the cumulative ADHD diagnosis rates recovered in states that went back to in-person instruction earlier. Within Indiana, which itself was a relatively open state in Fall 2020, we find similar patterns comparing counties with different in-person schooling levels.

Finally, we analyze the potential welfare effects of these declines in diagnosis by plotting the observed ADHD diagnosis rate and the counterfactual values based on predictions from our estimated model. We then compare these diagnosis paths to estimates of true ADHD prevalence for boys and girls. While the proportional decline for boys and girls is similar, our counterfactual analysis suggests the pandemic may have worsened the under-diagnosis problem for girls and potentially mitigated some of the over-diagnosis problem for boys. Future research will be necessary to better understand the implications of the drop in cumulative ADHD diagnosis that we document in this paper.

These results on how societal disruption affects child mental health contribute to the recent literature on the importance of schools as a determinant of child well-being during the COVID-19 pandemic. Baron et al. (2020) and Bullinger et al. (2021) show that reports of child-maltreatment fell at the beginning of the COVID-19 pandemic, partly driven by school closures and the associated decline in child maltreatment referrals from school personnel, which has been shown to be a crucial source of early maltreatment identification in pre-pandmemic times (Benson et al., 2022). Surveys that rely on parent perspectives of child behaviors report that school closures and disruptions are associated with worsening child behavior and increased mental health concerns (Hawrilenko et al., 2021; Gassman-Pines et al., 2022). All of these likely impact education outcomes, and can partially explain the emerging research showing lower academic test scores and pass rates due to schooling disruptions early in the pandemic (Engzell et al., 2021; Halloran et al., 2021; Agostinelli et al., 2022). By measuring in-person school activity with SafeGraph mobility data, we also contribute to the related and overlapping area of research that examines pandemic effects by using detailed cellphone location data to capture mobility patterns at various geographic levels (e.g. Parolin and Lee, 2021; Andersen, 2020; Gupta et al., 2020; Buckee et al., 2020). We discuss the benefits of these data as measures of societal disruption that may not be captured in administrative or survey data.

Our paper also relates to the literature on accuracy, costs, and benefits of ADHD diagnosis more broadly. ADHD is defined by symptoms of inattention, hyperactivity, and impulsivity, which can negatively impact child-welling and human capital development. This condition is associated with lower educational attainment (Currie and Stabile, 2006), risky behaviors (Chorniy and Kitashima, 2016), and longer-run labor market outcomes (Knapp et al., 2011; Fletcher, 2014). While accurate diagnosis and subsequent treatment can help manage ADHD symptoms and reduce negative consequences, over-diagnosis and over-prescribing are also a cause of concern. Many studies show that a child's birthdate in relation to school entry cut-off date is a strong predictor of ADHD diagnosis. This suggests that teachers may be subjectively comparing younger and older students in the same class, and mistaking immaturity for ADHD symptoms (Elder, 2010; Evans et al., 2010; Schwandt and Wuppermann, 2016; Layton et al., 2018; Persson et al., 2021). Over-diagnosis of marginal children can have detrimental effects as well. Stimulant medications used to manage ADHD symptoms can have significant side effects, and potential long-run negative effects, especially if not used according to clinical guidelines (Currie et al., 2014). While data limitations prevent us from studying prescriptions, our paper relates to this literature by highlighting the change in diagnosis rates during the pandemic and the potential role of schools in influencing diagnosis. We also conduct a welfare exercise which examines how observed and counterfactual diagnosis rates compare to true ADHD prevalence estimates, finding different welfare impacts for boys and girls.

Finally, we also add to the literature on disparities and bias in child mental health identification by analyzing heterogeneity in ADHD diagnoses by child gender and race/ethnicity. While boys are more likely than girls to have ADHD, the literature suggests that the diagnostic gap is larger than the true prevalence gap due to an over-diagnosis of boys and under-diagnosis of girls (Marquardt, 2022). There is less consensus on true prevalence differences based on individual race and ethnicity. Figure B1 plots the national trends in ADHD diagnosis rates since 2000. While white children are more likely to be diagnosed with ADHD on average, there are significant changes in the race-based diagnostic gaps. Possible explanations for these changing diagnostic disparities include barriers to care, stigma, cultural differences, and implicit biases in the clinical setting.[1] Besides parents and physicians, research shows that school personnel also differ in their perceptions or awareness of behaviors when comparing boys and girls or minority students to non-minority peers (Sciutto et al., 2004; Elder et al., 2021). Our research extends these research lines by exploring gender and racial heterogeneity in diagnoses changes due to pandemic related disruptions.

The remainder of this paper is structured as follows. In Section 2, we describe the ADHD diagnosis process and provide a detailed discussion on possible mechanisms contributing to the change in diagnosis rates observed during the COVID-19 pandemic. Section 3 outlines our general empirical strategy. Section 4 describes the three sources of data: the Optum national sample, the Indiana state sample, and SafeGraph mobility data. This section also introduces our definition of *new* ADHD diagnosis and how we measure Fall 2020 school activity. In Section 5, we present both our nationwide and within-state results. Finally, Section 6 concludes with discussion and a welfare exercise.

2 Background and Mechanisms

2.1 Attention Deficit Hyperactivity Disorder (ADHD)

According to the 2019 National Health Interview Survey, approximately 10% of school-aged children have Attention Deficit Hyperactivity Disorder (ADHD), making it the most diagnosed mental health condition in the United States. The American Academy of Pediatrics

[1] For a further discussion, see Nigg (2022), and cites within.

(2019) presents the best-practice guidelines for ADHD treatment, and strongly recommends both FDA-approved medication for ADHD along with behavioral therapy and/or educational interventions. However, in order to obtain these treatments, children must first receive a behavioral assessment and clinical diagnosis.

Initial diagnosis of ADHD in childhood is a complex process that involves a chain of events starting with symptom development, behavior recognition, and clinical diagnosis (described further in Figure 1). Symptoms of ADHD often develop at ages 3-12, with a median diagnosis age of 7 (Wolraich et al., 2019). There are three sub-types of ADHD: inattentive, hyperactive/impulsive, and combined (which includes symptoms of both other types). Males are more likely than females to have ADHD, and conditional on having ADHD, are more likely to experience the hyperactive or combined sub-type relative to females (Hinshaw, 2018; Hinshaw et al., 2022). Behaviors are typically first recognized by family members or teacher/school staff. While school psychologists may diagnose students with ADHD, they are not allowed to prescribe medication, and thus many are referred to a pediatrician for clinical assessment and treatment.[2] The national shortage of school psychologists further pushes children to receive diagnosis and treatment outside of the school setting or may even limit care entirely (National Association of School Psychologists, 2021).

During assessment, physicians should consult both the official diagnostic criteria for ADHD (the DSM-V) and request input from parents, teachers, or other school support staff. The DSM-V criteria requires at least six symptoms, and that these symptoms be present in at least two environments over at least a six month period. Typically, these two environments are at home and at school. However, clinical research shows signification heterogeneity in adherence to these formal guidelines.[3]

[2] Only 5 states have psychologist prescriptive authority laws, but these are only allowed under limited circumstances that, at the current time, do not extend to the school-setting Curtis et al. (2022).

[3] Chan et al. (2005) finds that only 28% of physicians strictly follow the DSM guidelines in diagnosing ADHD. Via chart reviews, Epstein et al. (2014) find that teacher and parent rating scales were used in just over half of all ADHD assessments, though they do find higher rates of DSM documentation than previous studies.

2.2 Potential Mechanisms

The COVID-19 pandemic has likely impacted the pathways to initial diagnosis in a variety of ways. We describe potential impacts at each point in the diagnosis process. In terms of symptoms themselves, the heightened general stress and environmental disruptions that were caused by the COVID-19 pandemic could exacerbate ADHD symptoms or trigger gene expression. Changes to caregiver employment and work-from-home status may also impact child symptoms. The impact of school shut-down and subsequent re-opening on symptoms is ambiguous and likely heterogeneous. While added stress due to lack of school structure and in-person instruction could exacerbate ADHD symptoms, it is also possible that online learning reduces external distractions and helps some children focus. On the other hand, more time within the (often disrupted) home environment could create additional distractions for other children.[4]

In addition to symptom expression itself, the recognition of symptoms by teachers, parents, and caregivers was likely impacted. Given the important role of schools and teachers/staff in the ADHD diagnosis process, it is likely that school closures and subsequent schooling disruptions due to COVID-19 had effects on symptom recognition. The impact on behavior recognition could also be positive or negative. If students are attending school from home (virtually), parents and caregivers may have a new view on child behavior during the school day, leading to increase in recognition. On the other hand, referrals from schools likely declined early in the pandemic due to limited face-to-face time between students and teachers and restrictions in supply of school-based mental health services.[5]

The pandemic may also directly impact the clinical diagnosis stage. The most immediate

[4]See https://www.cdc.gov/ncbddd/adhd/features/adhd-and-school-changes.html for additional discussion on ADHD and School Changes.

[5]67% of schools increased mental health services to students to address rising mental health concerns during the COVID-19 pandemic, however less than half reported hiring more mental health staff. Recent government policies and funding have been aimed at helping school-based mental health support, though the majority of these were not used or implemented until the 2021/2022 school year, which is outside the scope of our analysis (Panchal et al., 2022).

is the likely reduction in clinical diagnosis due to canceled or delayed behavioral assessments. In the initial months of the pandemic, local health care systems followed state guidelines in suspending elective and non-urgent medical care to prevent the spread of COVID-19. This made it difficult to provide the types of health services required to diagnose and treat child mental health conditions, as it did for other conditions (Helsper et al., 2020). Appendix Figure B4 shows this visually, with both adult and child general health care visits falling in March of 2020 and then returning to pre-pandemic levels by summer.

School disruptions could also indirectly impact the clinical diagnosis stage. For example, the instability in school attendance or instruction mode likely constrained teacher/school staff attention, further limiting their ability to respond to requests for input from physicians. Given the importance of schools in the diagnostic criteria, even children who have had recognized symptoms may be less likely to be formally diagnosed due to school disruptions.

Taken together, there are many factors that could impact initial diagnosis of ADHD due to the COVID-19 pandemic. In this paper, we document overall trends in cumulative rates of initial ADHD diagnosis, separately for elementary school-aged boys and girls. We focus on the early stage of the pandemic and examine heterogeneity in effects by school-openness status.

3 Empirical Strategy

Our study is organized around cohort-based event study and difference-in-difference designs. We focus on two cohorts: the pandemic exposed cohort, and the pre-pandemic unexposed cohort. The pandemic exposed cohort is followed over a 19 month study window that runs from August 2019 to February 2021. The pre-pandemic unexposed cohort is followed over the 19 month period from August 2018 to February 2020. Throughout, we use $t = 1...19$ to index months measured in *event time*. Event time is assigned so that calendar months are always aligned in the two cohorts: period $t = 1$ refers to August (2019) in the exposed group and August (2018) in the unexposed cohort. The key idea is that the typical operation of the ADHD surveillance and treatment system was disrupted by the onset of the COVID-19 pandemic beginning in event time period $t = 8$ for the exposed cohort (in March 2020), but not for the unexposed cohort (March 2019).

We construct the exposed and unexposed cohorts by building a balanced panel data set on children ages 6 to 11 with no record of an ADHD diagnosis during a look-back period covering the six months immediately before the start of the cohort-specific study window. We describe the cohort inclusion criteria for each of our data sets in more detail in section 4. This means that the exposed cohort consists of elementary school aged children who were ADHD-naive during the six month period preceding August 2019. The unexposed cohort is defined the same way except that they are required to be ADHD-naive during the six month period preceding August 2018.

For each member of the two cohorts, we observe the child's age at the beginning of the panel, geographic location of residence, and race/ethnicity.[6] Let a_{ict} be an indicator variable set to 1 if child i from cohort c has ever been diagnosed with ADHD as of event time period t. Since both cohorts are ADHD-naive at baseline, a_{ict} is a cumulative measure that turns on if and when a child is "identifed" as having ADHD. We use the age information to assign each child to a likely elementary school grade at baseline, and then collapse the individual panel data into cohort $\times$ event time $\times$ grade $\times$ race $\times$ geography cells.[7] After collapsing, A_{grzct} represents the cumulative fraction of the children in grade g with race/ethnicity r living in geography z in cohort c who have been diagnosed with ADHD by event time period t. This is simply the mean of a_{ict} in the grade-race-geography-cohort-period cell. A_{grzct} starts out at zero in the period just prior to the initial study period in each cell, and then rises over the 19 months of the study period as ADHD cases are identified over time.

[6] In the nationwide Optum data, we observe the child's state of residence. In the Indiana electronic health records, all of the children reside in one state and we observe the county of residence.

[7] In our Indiana analysis, we do not collapse by race due to small cell sizes.

We study the effects of the pandemic shock on the discovery of new ADHD cases in ADHD-naive cohorts using event study and difference-in-differences regressions that allow for an exponential conditional mean function, which we estimate with fixed effect Poisson regression models.[8] The goal of these models is to use the growth rate of cumulative new diagnoses in the unexposed cohort as a counterfactual for the growth rate of cumulative new diagnoses in the exposed cohort. Importantly, we also control for grade fixed effects. Therefore, all of our comparisons are within grade level. In other words, we compare first graders in the exposed cohort to first graders in the unexposed cohort. We note that our cohort construction allows some children in the unexposed cohort, particularly those not diagnosed with ADHD through the end of August 2019 and meeting our other inclusion criteria, to also appear in the exposed cohort. However, any students who do appear in both cohorts will have aged into a different grade between the years. Therefore they will be part of the control group for one grade level comparison and the treatment group for a different grade level comparison. We do not believe this materially impacts our identification strategy, though we do show results where we restrict the sample to each grade level separately in Appendix Table B6 and B7.

Because we are tracking cumulative new diagnoses, the trend in our dependent variable will be weakly monotonically increasing over event time. We therefore identify whether or not new diagnoses grow more slowly in the post period for the exposed cohort relative to the unexposed cohort. This implies that the key identifying assumption is that the exposed and unexposed cohorts experience a common growth rate, which we test by checking if they were experiencing common growth rates over the pre-period – the first seven months of event time.

[8] We use the fixed effect Poisson only to model the conditional expectation function. We relax the assumption that the cumulative diagnosis rates are actually distributed Poisson by allowing for a heteroskedasticity and cluster robust variance matrix. Sometimes this approach is called a pseudo-Poisson model or a generalized linear model with a log link function (Wooldridge, 2010).

The workhorse specification in our analysis is:

$$ln\Big(E[A_{grzct}|g,r,z,c,t]\Big) = \beta_0 + Exposed_c \times \Big[\sum_{j=1}^{6} \alpha_j 1\{t=j\} + \sum_{k=8}^{19} \alpha_k 1\{t=k\}\Big]$$
$$+ X_{grzct}\gamma + \mu_g + \mu_r + \mu_z + \mu_c + \mu_t \quad (1)$$

In the model, $Exposed_c$ is a dummy variable set to 1 if the cell belongs to the pandemic exposed cohort and set to 0 if it belongs to the control cohort. μ_g is a grade fixed effect, μ_r is a race/ethnicity fixed effect, μ_z is a geography fixed effect, μ_c is a cohort fixed effect, and μ_t is an event time fixed effect. X_{grzct} is a covariate vector that – in some of our analysis – includes the cumulative rate of well-child visits in the cell and adult evaluation and management visits in the cell's geographic area to proxy for health care supply constraints and avoidance behavior during the pandemic. In our Indiana analysis, where we do not use race based cells, we omit μ_r and control for the race composition of the cell. The event study covers a 19 month window. The first 7 months (August through February) represent the pre-period of the event study. The final 11 months (March through February) represent the post period. The coefficients of interest are the set of αs, which trace out the relative difference in the growth rates of cumulative diagnoses between the cohorts relative to the reference period, February of the cohort base year ($t = 7$). In other words,

$$\alpha_m = ln\Big(\frac{E[A_{grzct}|g,r,z,c=Exposed,t=m]/E[A_{grzct}|g,r,z,c=Exposed,t=7]}{E[A_{grzct}|g,r,z,c=Unexposed,t=m]/E[A_{grzct}|g,r,z,c=Unexposed,t=7]}\Big) \quad (2)$$

In addition to this event study model, to summarize the effects, we estimate the following difference-in-differences model:

$$ln\Big(E[A_{grzct}|g,r,z,c,t]\Big) = \beta_0 + \beta_1 Exposed_c \times PostMarch_t$$
$$+ X_{grzct}\gamma + \mu_g + \mu_r + \mu_z + \mu_c + \mu_t \quad (3)$$

$PostMarch_t$ is equal to one beginning in $t = 8$, which is March 2020 for the exposed cohort and after March 2019 for the unexposed cohort. The difference-in-differences model is similarly interpreted as in Equation 2, but comparing the pre and post-period rather than two specific event months.

We also estimate heterogeneous effects by race/ethnicity in our nationwide analysis. Here we interact the event-study variables or $Exposed_c \times PostMarch_t$ with indicators for whether the cell represents Asian, non-Hispanic Black, or Hispanic children, with non-Hispanic White cells as the reference group. In these specifications, we also interact the race indicators with all design-based fixed effects, which include the cohort and event time fixed effects.

Finally, to examine heterogeneity by in-person school availability, we estimate similar interaction models where we interact the event-study variables or $Exposed_c \times PostMarch_t$ with measures of school mobility in geographic area z. As described in more detail in Section 4, we divide geographic areas into those with low, medium, and high in-person school activity. We treat low school activity areas (i.e. those that stayed closed) are the reference group, and we include cohort-by-openness level fixed effects and event-month-by-openness level fixed effects.

In all of these regressions we weight by the number of children in the cell and cluster the standard errors at the geographic area (state or county) by cohort level, to allow arbitrary correlation of the error term both over time and across groups within a state-cohort.

4 Data

Our analysis leverages three different data sets. To measure changes in ADHD diagnosis nationwide, we use de-identified medical claims from Optum's Clinformatics® Data Mart Database (Optum; 2020-2021). We also use electronic health records data from the Indiana Network for Patient Care (INPC). These two data sets are complementary. Optum data is nationwide in coverage, but only among those with a single private insurer, whereas INPC covers only one state, but all payment types. In addition, the INPC data allow local, rather than state, level variation in school activity. Finally, we use SafeGraph mobility data to measure in-person schooling activity at the state and and school level during the COVID-19 pandemic.

4.1 Health Data Sources

Optum

Our nationwide analysis uses de-identified medical claims from Optum's Clinformatics® Data Mart Database (Optum; 2020-2021). This database captures approximately 20% of the commercially insured population nationwide. Demographic data indicates that individuals represented are comparable to the US commercially insured population (Lee et al., 2021), and we show this to be true among children in our sample.

Table 1 compares basic demographics of our pre-pandemic cohort to nationally representative statistics from the National Health Interview Survey (NHIS), restricted to children with private health insurance. Our sample has a similar gender, income, and race/ethnicity composition to the NHIS; although, the Optum sample has more white and fewer Hispanic children. Compared to the NHIS, diagnosis rates for ADHD are only slightly lower for boys and girls in Optum.

INPC

Our Indiana specific analysis uses data from the Indiana Network for Patient Care research database (INPC). INPC includes data from electronic health records from providers across the state collected through the Indiana Health Information Exchange. This information exchange is one of the longest standing in the country and includes most major health care providers in the state. About two thirds of the state population have at least one encounter within the INPC (Regenstrief Institute, RDS).[9] It is difficult to know how many patients receive all or most of their healthcare from INPC participating providers. However, Indiana is dominated by a small handful of large healthcare systems covered by the network.

Our INPC pre-pandemic cohort includes all payers, and therefore includes Medicaid covered children not captured in the Optum data. Compared to Optum, the ADHD diagnosis rates are only slightly larger in INPC. Additional sample composition comparisons to Optum and NHIS are provided in Table 1.

4.2 Defining Cohorts and *New* ADHD Diagnoses

In both the Optum national sample and the INPC sample, we define two cohorts of elementary school-aged children.[10] In our Optum analysis, the "exposed" cohort is continuously enrolled in with the insurance provider from February 2019 through February 2021, and thus experienced the pandemic beginning in March 2020. The "unexposed" cohort is continuously enrolled from February 2018 through February of 2020 and did not experience the pandemic. In INPC we can only observe when patients receive healthcare from INPC providers. We cannot directly observe when they "enter" or "exit" the data. We therefore define the exposed cohort as children with at least one INPC encounter during the look back period represented by the first six months of each cohort, between February 2019 and July 2019, and then follow these children through February 2021. The "unexposed" cohort includes children with at least one INPC encounter between February 2018 and July 2018,

[9] For more information, see Biondich and Grannis (2004)).

[10] ADHD is often first diagnosed in elementary school-aged children, which is why we subset our analysis to children aged 6-11.

and then followed through February 2020.

To determine new diagnoses, we first separate the panel for each cohort into a six month look back period (February-July 2019 for the exposed cohort and February-July 2018 for the unexposed cohort) and the remaining study period (beginning in August 2019 for the exposed cohort and August 2018 for the unexposed cohort).[11] We label a child as receiving a new ADHD diagnosis in month t of the study period if s/he has a medical visit with a recorded ADHD diagnosis code in month t but no visits with ADHD diagnosis codes during the look back period.[12] ICD-10 diagnosis codes that indicate ADHD are those with the first three digits of F90. For the main analysis, we group all ADHD sub-types into a single diagnosis category, and we discuss heterogeneity by sub-type in Section 6.

To analyze the effect of the pandemic on cumulative new ADHD diagnoses, we essentially calculate how the cumulative rate of new ADHD diagnoses for the exposed cohort between August 2019 and February 2021 deviates from its pre-pandemic trend in the unexposed cohort between August 2018 and February 2020. Figure 3 plots our main outcome variable over time for each cohort, with months lined up in "event time." From August of 2019 to March of 2020, the growth of new ADHD diagnosis was following a similar trend as the year before; however, the trend starts to deviate for both boys and girls once the pandemic begins and schools begin to shut-down in March and April of 2020, with the exposed cohort accumulating fewer new diagnoses.[13] This is true in both the nationwide Optum sample

[11] We believe the six month look back period is sufficient as the majority of children with an ADHD diagnosis will be observed with an ADHD-related claim every 1 to 3 months for 'medication management'. This is due to federal policies surrounding the prescribing and dispensing of stimulant medication, which is a schedule II controlled substance.

[12] We use visit diagnosis codes rather than prescriptions as the latter may not be fully captured in our data sets. In Optum, prescription coupons (e.g., GoodRx discounts) or alternative prescription insurance plans means that we may not observe all ADHD-specific prescription fills in the data. In fact, we find that prescription rates in Optum are much lower than in NHIS, despite the similarity in diagnosis rates. In INPC, collection of prescription-specific data changed significantly during our sample period, which results in under-counting in later sample months. In Appendix Figures B6-B7 and Table B1 , we show that nationwide analysis of new ADHD prescriptions are similar but noisier than our main results.

[13] The analogous nationwide cumulative ADHD diagnosis rates by race/ethnicity and by school-openness grouping are shown in Appendix Figures B2 and B3, respectively.

(panel a) and the Indiana INPC sample (panel b).

4.3 Additional Control Variables

In some specifications we control for the cumulative rate of well-child visits and adult evaluation and management visits to proxy for health care supply constraints and avoidance behavior during the pandemic. In the nationwide Optum analysis, we identify these visits based on CPT codes for new and established patient well child visits (99383, 99384, 99385, 99393, 99394, 99395) and new and established adult evaluation and management visits (99202, 99203, 99204, 99205, 99212, 99213, 99214, 99215). In the Indiana INPC analysis, we do not observe CPT codes, so we use ICD10 Z codes to identify child and adult general health care utilization. Appendix Figure B4 compares the flow of well-child visits and adult evaluation and management visits in both exposed and unexposed cohorts, and the similar measures in Indiana. These types of visits fell during March of 2020 and returned to pre-pandemic levels by Summer 2020.

In the Indiana analysis, we do not have a sufficient number of observations to stratify by race, but we do include controls for cell-based racial composition: percent non-Hispanic white, non-Hispanic black, Hispanic, and Asian. We also control for cell-level health insurance composition as INPC includes all payers whereas the nationwide Optum analysis includes only one private health insurer: percent Medicaid, percent Privately Insured, and percent other insurance.

4.4 Measuring Fall 2020 School-Openness Activity

To measure in-person schooling at the state and school level over the course of 2020 and 2021, we follow (Parolin and Lee, 2021) by using data from "smart" devices that record the level of activity by location and date. Unlike administratively reported or school-district-website scraped data, cell-device mobility measures cover all schools and can measure both intensive and extensive margins.

There was a clear uniform closing of schools for all states right after the start of the pandemic, but we see substantial geographic variation in the availability of in-person schooling at the start of the next school-year in Fall 2020. For our nationwide analysis, we examine heterogeneity in ADHD diagnosis by state school-opening level. To do so, we we aggregate visits across all elementary schools within a state, and then partition states into three equal-sized groups according to their relative school-openness levels: High, Medium, and Low.[14] Low Opening states are those with Fall 2020 visit levels less than 54.4%, Medium Opening states range from 54.4% to 70.4%, and High Opening states are above 70.4% relative to their 2019 levels. Figure 2 displays this geographic variation in school-openness levels in Fall 2020.

In Appendix A we compare our SafeGraph derived measure of school activity to reported learning model policies collected by the COVID-19 School Data Hub (CSDH). We show that once aggregated to the state level, there is strong correlation between in-person school policies and actual in-person school visits. However, CSDH only reports for a subset of states, whereas SafeGraph mobility data includes all states.

Because in-person school availability was typically made at the school/district level and not the state level, the average state level analysis may potentially miss important variation in school availability *within* a state. For example, the average aggregate school-openness in Indiana for Fall 2020 is 84%, making it a "High Opening State" for the nationwide analysis. However, Figure 2 shows there is significant variation in in-person availability for elementary schools across the state during the Fall of 2020.

When we conduct the within state analysis in Indiana, we proceed in a slightly different way for partitioning individuals into different school-openness groupings, because in INPC data we have access to very detailed geographical location (census tract and zip5) of the patient. Specifically, we construct a measure of the opening level of schools serving the loca-

[14] Openness level is based on the state's average number of elementary school mobile visits in Fall 2020 relative to baseline visits in Fall 2019, where Fall is defined as September through November.

tion where a child lives using the census-tract/zip5 combination. School educational mode decisions are often at the school level, but school catchment areas do not necessarily coincide with census track borders or zip5 borders. Using GIS software, we overlay the elementary school catchment areas obtained from the National Center for Education Statistics (NCES) on top of zip5 and census tract borders. We then calculate the fraction of a zip-census tract combination pairing that is covered by each school catchment area. We then construct the weighted average relative school opening level for the zip-census tract pair. We attach school-openness measures to each individual based on their zip-tract reported in INPC, then collapse to the county level and divide into three equal sized groups. Low Opening counties are those with an opening level less than 76%, Medium Opening counties range from 76% to 93%, and High Opening counties are above 93% relative to their 2019 activity levels.

Similar to the state-level comparison, we also compare our zip-tract SafeGraph derived school-openness measures to CSDH in-person learning mode at the school and/or district level in Indiana. We show that at these finer geographic regions, administrative policies and realized activity differ. This is likely driven by both local demand for in-person schooling and somewhat arbitrary definitions of in-person learning during the Fall of 2020. In Indiana, learning mode is determined by whether the majority of students receive at least three-quarters of their instruction in-person, which leaves room for a lot of variation in actual attendance even in schools coded as open by the CSDH data set. See Appendix A for further discussion and implications of these differences.

5 Results

5.1 Nationwide Results (Optum)

Figure 4 plots the event study estimates for boys and girls. We translate the event-time Poisson coefficients ($\hat{\alpha}_j$) from Equation 1 into percent changes by exponentiating and subtracting 1. Before the pandemic started, diagnosis rates for the "exposed" and "unexposed" cohorts trended similarly. However, for the group of children that were exposed to the pandemic, we see a sharp decline in new diagnoses that starts in March 2020 and continues to the end of our sample period. Though slightly smaller, the decline in diagnosis persists even after controlling for child well-visits and adult E&M visits, suggesting that the fall in general healthcare utilization which occurred throughout the beginning of the pandemic is not driving our results. We note here that the decline is of similar proportional magnitude for boys and girls, and the gap persisted even through the winter of 2020/2021 when many had returned to in person schooling. In other words, after an initial decrease in new diagnosis, the exposed cohort did not "catch up" by receiving more new diagnoses later in the year.

The difference-in-difference specification results are presented in Table 2, columns 1 and 3. For both boys and girls, we see a statistically significant overall decline in diagnoses in the exposed cohort relative to the unexposed cohort (labeled Pandemic in the table). Consistent with our event studies, Panel B translates the Poisson coefficients into overall percent changes. Cumulative ADHD diagnosis fell by 8.58% for boys and 11.0% for girls. Appendix Table B6 presents difference-in-difference estimates and transformed percent changes separately for each grade (kindergarten through fifth). The decline in ADHD diagnosis rates for boys is similar across all grades, though the effect is largest among those in kindergarten. For girls, the decline is largest among third and fifth graders.

Columns 2 and 4 of Table 2 (a) present heterogeneity by race/ethnicity. Here, the reference group is white children, so the table presents differential declines in diagnosis among other race and ethnicity groups *compared* to the decline for white children. While results are noisy, we see a positive interaction effect that implies the decrease in ADHD diagnosis is smaller for Black children than for white children.

To better summarize the heterogeneity, we translates the Poisson coefficient estimates into percent changes by group- see panel (b) of Table 2.[15] Though not statistically different

[15]Mathematically, we first estimate the race-interacted main effect coefficients in equation 3 with non-

from each other, both white children and Hispanic children experienced significant declines in new ADHD diagnosis during the pandemic. This is not the case for Black children, on the other hand, where percent changes are not significantly different from 0 and point estimates are positive in magnitude. The associated event-study estimates are provided in Appendix Figure B5.

Next, we analyze the importance of school opening status on cumulative ADHD diagnosis. Figure 5 plots event study estimates based on a model with school opening interactions. Panel A of Table 3 summarizes these results numerically, with Low Opening states as the reference group. Compared to states that stayed closed the most, children in high opening states experienced smaller declines in cumulative ADHD incidence, though the difference is only statistically significant for girls. Translated into percent changes (panel B), we see that cumulative ADHD diagnosis for girls fell significantly in states with low and medium open levels but not for states with schools that were largely open. For boys, the cumulative decline is largest in medium-open states, but statistically different from 0 in all open categories. In the event study figure, we see that girls in high opening states did experience a decline in new diagnoses during the spring and summer of 2020, but began to catch back up to the unexposed cohort during the fall and winter of 2020.

5.2 Indiana Results (INPC)

Figure 6 presents the within-state INPC event study estimates. Besides two months of higher-than-average diagnosis rates for boys well before the start of the pandemic, we see that new monthly diagnosis rates trended similarly for "exposed" and "unexposed" cohorts prior to March of 2020. Given the Poisson specification, the event studies are comparing ratios of diagnosis rates. Because the base rate of new diagnoses are very low in early months by construction, small absolute differences in new diagnosis changes between the two groups

Hispanic white children as reference group. We then calculate the decline for non-Hispanic white children as $e^{\hat{\beta}_1} - 1$, and the decline for each other race group as $e^{\hat{\beta}_1 + \hat{\beta}_r} - 1$.

can be exaggerated when looking at relative changes in these early months. So while the event study estimates appear to show differential relative trends over the first two months, the raw data plotted in Figure B1 shows that the absolute differences were very small, and therefore unlikely to impact our difference in difference estimates, which aggregate the pre- and post- period. Similar to the nationwide analysis, we see a decline in new ADHD diagnoses that starts in March 2020 and continues throughout the sample period. Within Indiana, however, the magnitude and strength of the decline is larger for boys than for girls.

Table 4 presents the difference-in-difference specification results. Columns 1 and 3 summarize the main effect for boys and girls, respectively. The overall decline is statistically significant for boys but not for girls. The transformed percent changes (in Panel B) show there was a statistically significant 18% decline in new ADHD diagnoses for boys and a non-significant 7% decline for girls. Appendix Table B7 presents difference-in-difference estimates and transformed percent changes by grade. As with the nationwide results, effects are similar across grades.

Columns 2 and 4 of Table 4 presents the heterogeneous effects by school-openness level within the state. The associated event-study estimates are shown in Figure 7. Noise and pre-trend differences in some school-openness level groups limit our interpretation of differential effects. However, in both the event study and difference-in-differences regressions, we find no evidence of substantial heterogeneity by school-openness for boys. For girls, the event study suggests that - consistent with the nationwide analysis- ADHD diagnosis rates did not fall as much in high-opening areas, though due to a downward relative pre-trend this is not apparent in the difference-in-difference estimates.

6 Discussion & Conclusion

Despite reports that child mental health is *worsening* during the COVID-19 pandemic (Gassman-Pines et al., 2022), we find diagnosis rates *fall* for Attention Deficit Hyperactivity Disorder, a very common child mental health condition. While it is possible that the pandemic conditions impacted ADHD symptoms, it is most likely to have exacerbated them. Therefore, we do not interpret this decline as driven by a reduction in the number of children with ADHD specific symptoms, but rather as a change in the inputs to diagnosis.

We document a statistically significant and large in magnitude decline in cumulative new ADHD diagnosis starting in March of 2020 and extending into early 2021. We use two different data sets to show this decline appears for both individuals within a specific state captured by electronic health records and across states nationwide captured by private health insurance claims. Taking into account the gender differences in baseline ADHD diagnosis rates, we show the proportional decline in cumulative new diagnoses is similar for boys and girls nationwide, but larger for boys within Indiana. In our nationwide analysis, we also estimate heterogeneous effects by child race/ethnicity and find that Black children were significantly less likely than white children to experience falls in ADHD diagnosis rates, and overall percent declines were largest among Hispanic and white children.

Because schools and teachers are important inputs of the ADHD diagnosis process, we examine how the lack of in-person schooling during the start of the COVID-19 pandemic may have contributed to the fall in cumulative new diagnosis rates. We explore this idea using SafeGraph mobility data at elementary schools and analyze heterogeneity in the ADHD diagnosis decline based on school-openness groupings in the Fall of 2020.

When analyzing differences at the sate level, we find that girls in states with low and medium open levels had larger declines in diagnoses than girls in high open states, suggesting that in-person interactions are especially important for ADHD identification in girls. This does not appear to be the case for boys, however, where diagnosis rates fell similarly regardless of school-openness grouping. Within Indiana, which is a state with a high level of school-openness over all, we find similar patterns, though the results are noisier and less conclusive.

It is worth noting that even the low-opening areas of Indiana still experienced modest in-person school availability, between ∼50%-70% of pre-pandemic activity. It may be the case that Medium-Opening and High-Open states (such as Indiana) are also states that experience instability in school instruction caused by constant opening and closing of schools and/or varying attendance due to increased exposure.

To examine this possibility at the national level, we analyze the interaction between school open level and school visit stability, where stability is based on the within-state standard deviation in SafeGraph school visits across weeks over the Fall of 2020. The idea is that states that were more open on average may have also experienced more disruptions on a weekly basis due to rising COVID-19 infections or concerns. So, while the in-person schooling could be beneficial for children behaviors and learning, the instability could have counteracting effects (and vice-versa).

Table 5 presents the results from this exercise for boys (panel a) and girls (panel b). Values represent the transformed overall percent changes based on difference-in-differences estimates with school openness and school stability interactions. We find that stability of school visits is indeed important for ADHD identification, though it differs by gender. Within a given open level status, boys in states with high stability experience lower declines than those in low stability states. Conversely, high visit stability is still associated with large declines in diagnosis for girls, except within high opening states. These findings suggest that school visit stability is important for ADHD diagnosis in boys, while girl diagnosis may require both stability and face-to-face interaction.

One possible explanation for these various school-openness and stability results could be due to the way in which ADHD presents differently in boys and girls. As discussed in Section 2, girls with ADHD are more likely to have internalizing symptoms (e.g., Inattentive sub-type) which are less salient to observers, whereas boys are more likely to have externalizing symptoms (e.g., Hyperactive sub-type). In Appendix Tables B2-B5, we present the difference-in-difference estimates by ADHD sub-type. While we do not place too much emphasis on these results given some inconsistencies in how ADHD sub-types are reported in the data (especially within INPC), we do note some overall trends in the nationwide results. Hyperactive diagnoses fell the most for boys, especially in medium and low opening states. Girls experienced similar declines in Hyperactive and Inattentive diagnoses. Interestingly, when we run these separate regressions by sub-type, we find that for both boys and girls, declines in diagnosis are largest in Medium Opening states. This again relates back to the importance of school-mode stability, which is likely fluctuating in Medium-opening states.

6.1 Welfare Exercise

If we assume that all children would have been accurately diagnosed in the absence of the COVID-19 pandemic, our finding of a substantial decline in new cumulative ADHD diagnoses indicates vast under-identification of ADHD in both boys and girls. These "missed diagnoses" can be extremely costly both at the individual level and to society as a whole. Such costs arise from lack of symptom management which can explain lower test scores and educational attainment in childhood (Currie and Stabile, 2006) and influence labor market outcomes in the long-run (Fletcher, 2014). Missed diagnosis may also have spillover costs, especially if un-managed symptoms cause disruptions in the home or classroom (Aizer, 2009). At the other extreme, if all children would have been *over*-diagnosed in the absence of the COVID-19 pandemic, the cumulative decline may be welfare increasing as misdiagnosis is also costly due to excess medical and education spending, exacerbated by the added side-effects and lack of evidence supporting long-run benefits of stimulant treatment (Currie et al., 2014).

However, due to the subjective nature of diagnosis, it is unlikely that the decline in ADHD diagnoses summarized in this paper are either all "missed" diagnoses or all children who would have been misdiagnosed. Further, the welfare effects may differ by gender given the literature suggesting boys are more often over-diagnosed with ADHD and girls under-diagnosed (Bruchmüller et al., 2012; Marquardt, 2022).

To visualize this, Figure 8 uses our nationwide estimates to plot observed ADHD diagnosis rates and counterfactual diagnosis rates, with the latter summarizing what diagnosis rates would have been had the COVID-19 pandemic not occurred. For each month, we use our event study model estimates to calculate the counterfactual diagnosis rate by predicting monthly outcomes with exposed cohort-time interactions set to 0. In other words, we estimate what the diagnosis rate would have been in each month had the pandemic never happened. We also calculate the predicted monthly total diagnosis rate from our model[16], with the difference between the counterfactual and predicted rates being comparable to our event study estimates. We focus on *total* diagnosis rates, and therefore incorporate the number of children who already had an ADHD diagnosis during our lookback period.[17]

For reference, we also plot the range of true ADHD prevalence estimates for boys and girls from the medical literature shaded in gray.[18] We also include the gender-specific prevalence estimate from Marquardt (2022), the horizontal black dashed line, which takes a more quantitative/modeling approach to finding ADHD true prevalence based on text analysis of doctor note text and selection adjustments into mental healthcare.

Compared to the Marquardt (2022) true prevalence estimates, Figure 8a shows that while boys are on net over-diagnosed, the final observed rate is closer to the true prevalence estimate than the counterfactual. This would imply that the decline in cumulative ADHD diagnosis for boys may be welfare increasing if it reduced the number of boys misdiagnosed, and therefore limited the costs associated with misdiagnosis such as adverse side-effects and dependence on stimulant medications typically used to manage ADHD in children. However,

[16] Average observed and predicted diagnoses are almost identical.

[17] Total diagnosis rate is equal to $(\#PreviouslyDiagnosed + NewDiagnosisRateX\#NotPreviouslyDiagnosed)/(\#PreviouslyDiagnosed + \#NotPreviouslyDiagnosed)$ where we replace the New Diagnosis Rate with either the predicted or counterfactual versions.

[18] These prevalence estimates vary significantly based on sample size, setting (e.g., community vs clinical), time-frame, and review criteria. We pull these estimates from meta-analyses on ADHD prevalence that differentiate by gender: Froehlich et al. (2007); Polanczyk et al. (2007); Willcutt (2012); Kessler et al. (2012); Cordova et al. (2022).

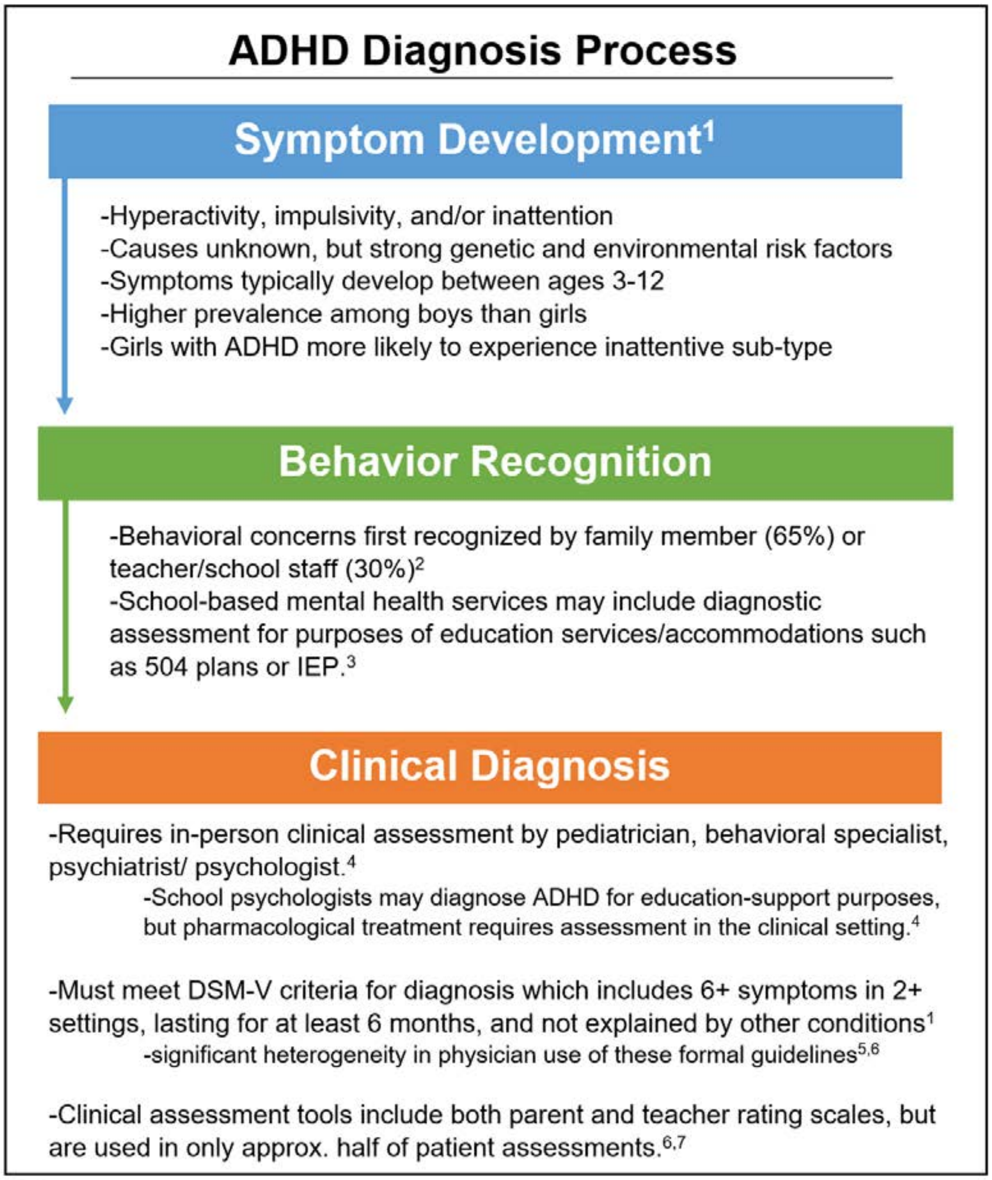
it is difficult to quantify this given the large range in ADHD prevalence among boys. For girls, on the other hand, Figure 8b shows that the pandemic may have made the under-diagnosis problem even worse. Though again, this is difficult to quantify given varying true prevalence estimates.

Whether the decline in ADHD diagnosis we identify in this paper is due to reduced over diagnosis, under diagnosis, or some combination, and to better understand the consequences of the drop in cumulative ADHD diagnosis documented in this paper, longer run study of additional outcomes will be necessary. In particular, we identify some important future questions. Do cumulative diagnoses rates rebound to pre-pandemic levels following our study period? How have test scores and other academic outcomes been impacted by the decline in new ADHD diagnosis? Are there long-run impacts of this decline such as compromised adult outcomes from a delay in initiation of treatment and/or benefits from reducing the potential to over-prescribe ADHD medication? And, how do these associated outcomes differ across states or demographic groups?

Our work also contributes to other research outside of ADHD documenting the tradeoffs made when diverting away from in-person schooling for public health protections; there are difficult tradeoffs that policy makers needed to make, based on little evidence about the implications for the short run let alone the longer run. The literature has been producing evidence along many lines–from test scores to mental health to physical fitness, as well as of cyberbullying of non COVID consequences from school closures and disruptions from shifting coursework to online format. This evidence base will be important for future management of national emergency decision making, as well as for economic studies tracing the role of parents, providers and schools in producing child health.

Tables and Figures

Figure 1: The Initial ADHD Diagnosis Process

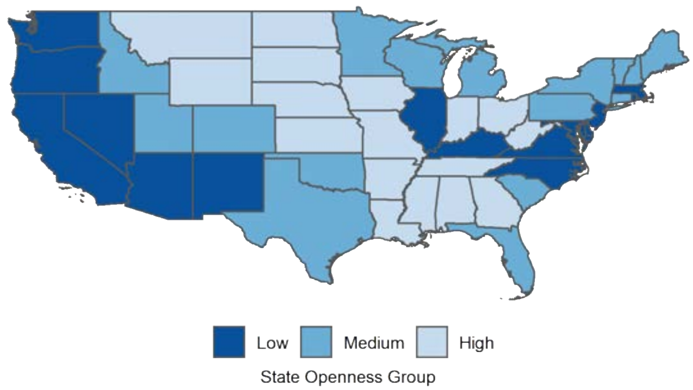


References:

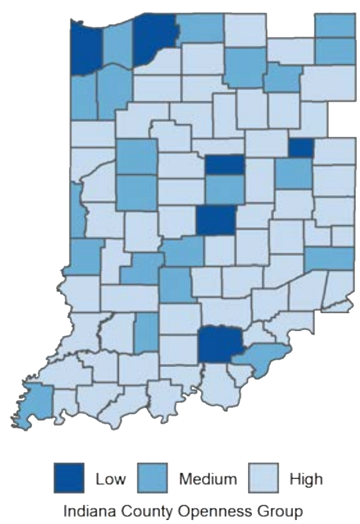
1: Mayo Clinic (2019)
2: Visser et al. (2015)
3: https://www.cdc.gov/ncbddd/adhd/school-success.html
4: Kessler, E. (n.d.)- www.smartkidswithld.org
5: Chan et al. (2005)
6: Epstein et al. (2014)
7: Gordon et al. (2020)

Figure 2: Fall 2020 School-Openness Groupings

(a) Nationwide



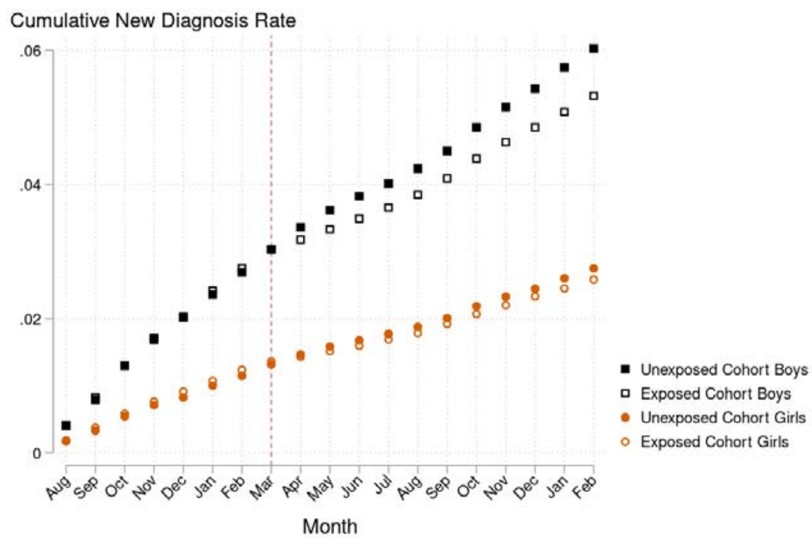
(b) Indiana



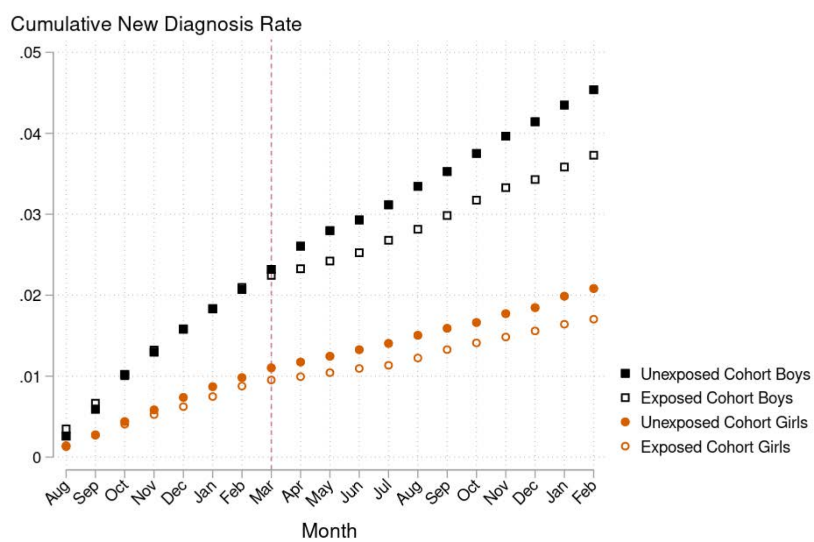
Note: Figure displays Fall 2020 school-openness groupings derived from SafeGraph mobility data as described in Section 4. In Panel A, Low Opening states are those with an opening level less than 54.4%, Medium Opening states range from 54.4% to 70.4%, and High Opening states are above 70.4% relative to their 2019 levels. In Panel B, Low Opening counties are those with an opening level less than 76%, Medium Opening counties range from 76% to 93%, and High Opening counties are above 93% relative to their 2019 activity levels.

Figure 3: Cumulative New Diagnoses by Cohort

(a) Nationwide (Optum)

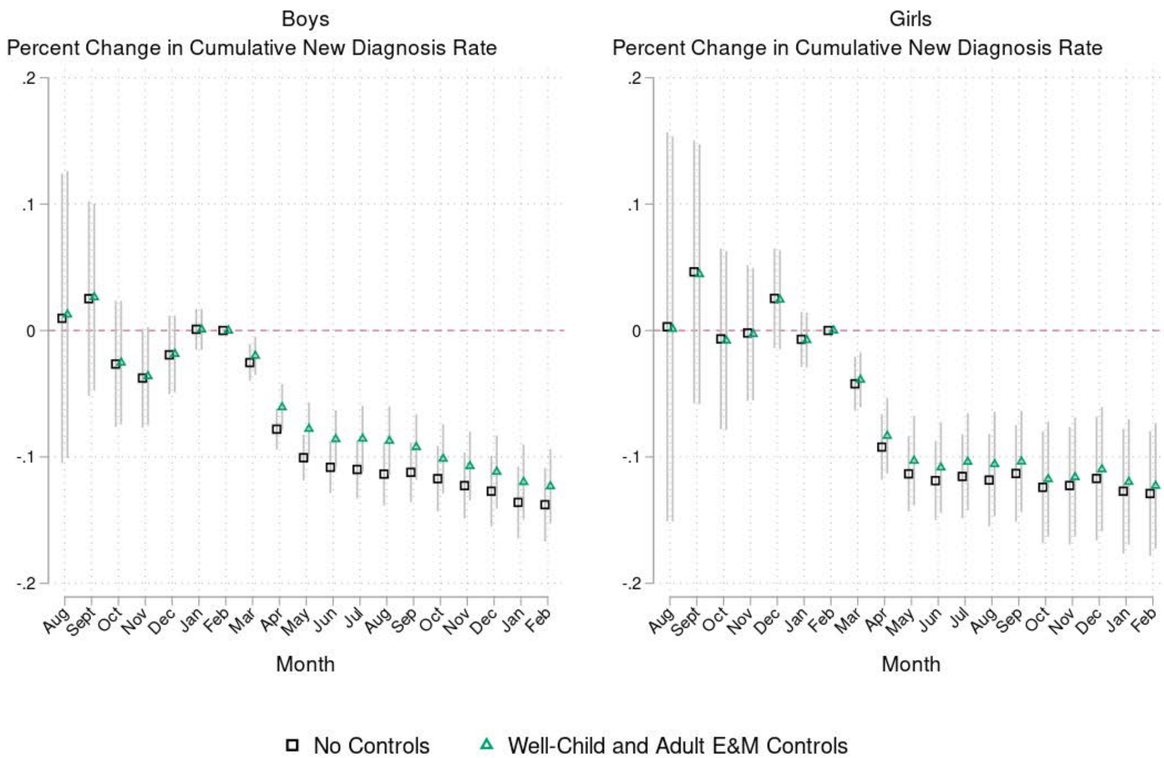


(b) Indiana (INPC)



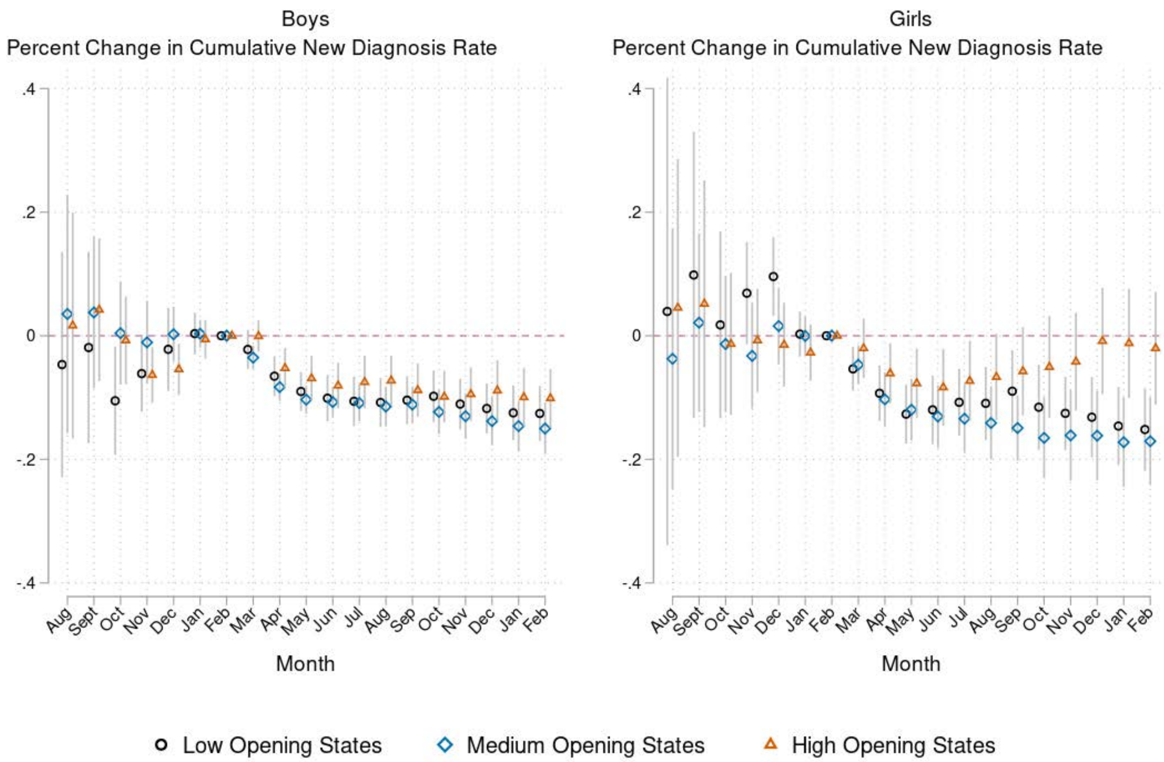
Note: In Panel A, exposed cohort is children continuously enrolled between February 2019 and February 2021. Unexposed cohort is children continuously enrolled between February 2018 and February 2020. In Panel B, exposed cohort is children with at least one INPC encounter between between February 2019 and July 2019. Unexposed cohort is children with at least one INPC encounter between between February 2018 and July 2018. In both panels, sample includes children without an ADHD diagnosis during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively).

Figure 4: Event Study Estimates, Nationwide (Optum)



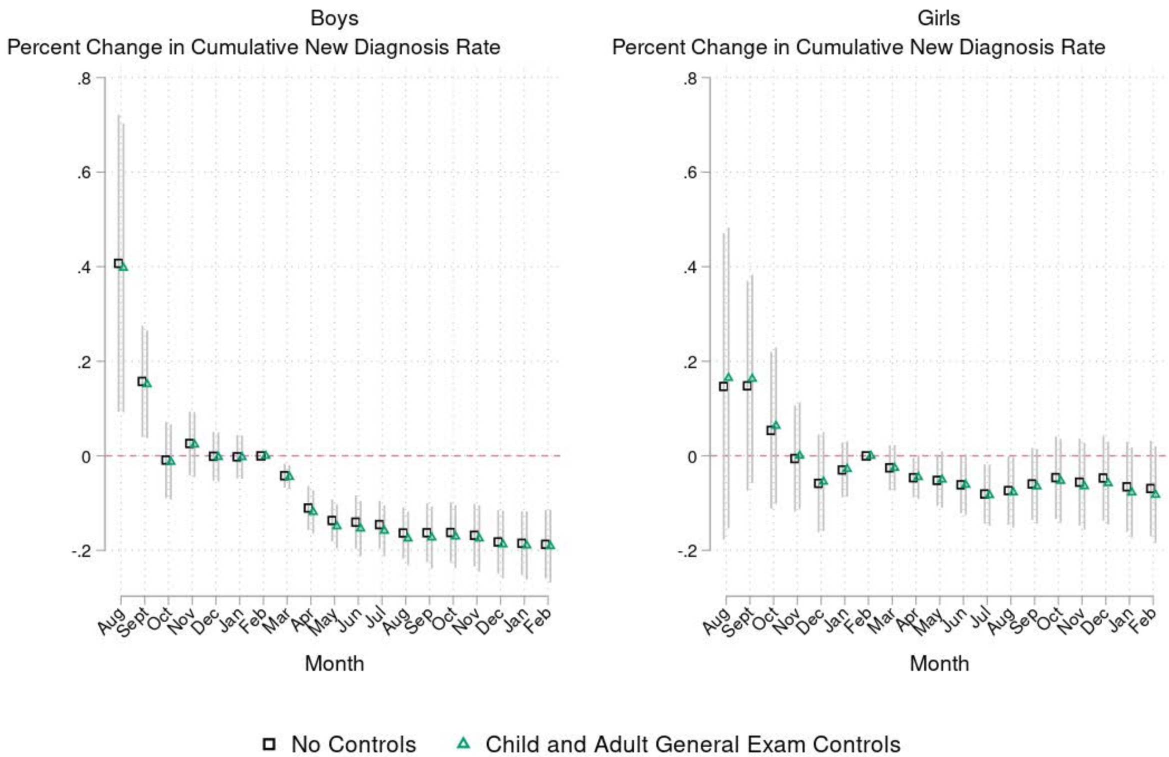
Note: This figure presents percent changes derived from event study estimates of changes in cumulative new diagnosis rate between the exposed and unexposed cohort. Exposed cohort is children continuously enrolled between February 2019 and February 2021. Unexposed cohort is children continuously enrolled between February 2018 and February 2020. Sample includes children without an ADHD diagnosis during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively). February 2019/2020 is the reference period for the unexposed/exposed cohorts. Plotted percent changes are the exponentiated event study coefficients minus one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Figure 5: Event Study Estimates by State School Opening Level, Nationwide (Optum)



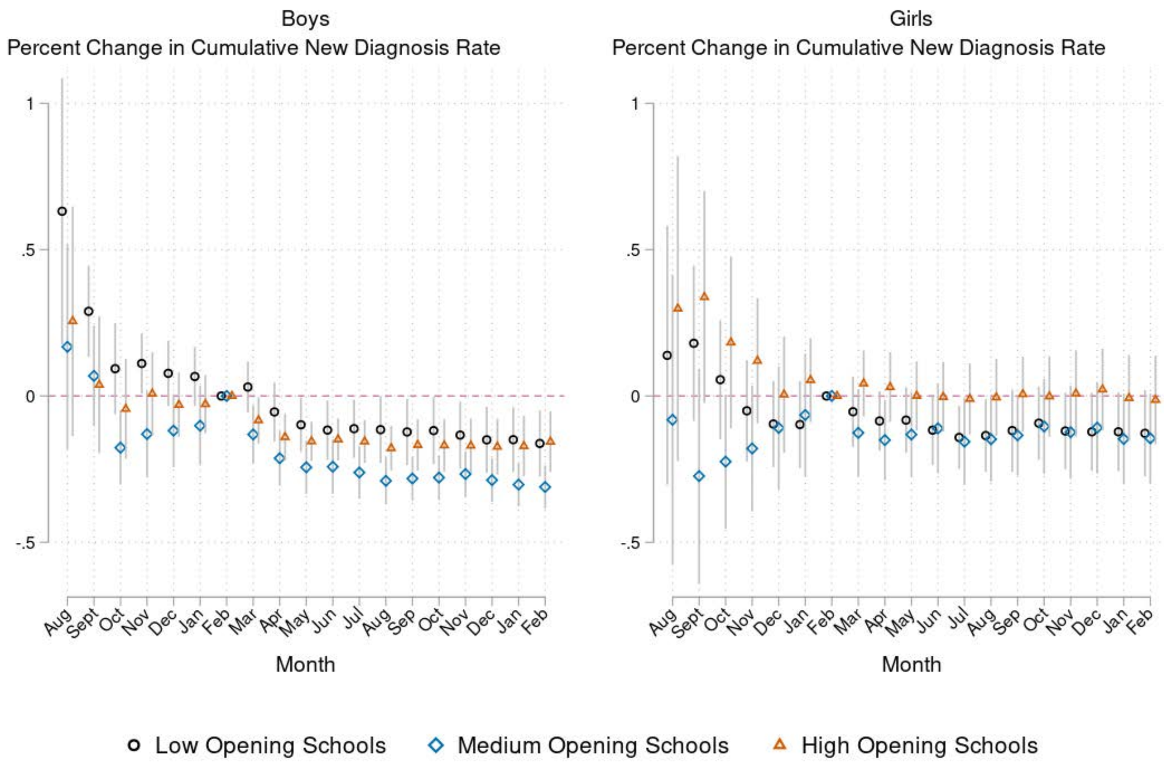
Note: This figure presents percent changes derived from event study estimates of changes in cumulative new diagnosis rate between the exposed and unexposed cohort, interacted with state school opening level. Exposed cohort is children continuously enrolled between February 2019 and February 2021. Unexposed cohort is children continuously enrolled between February 2018 and February 2020. Sample includes children without an ADHD diagnosis during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively). February 2019/2020 is the reference period for the unexposed/exposed cohorts. Plotted percent changes are the exponentiated event study coefficients for each state school opening group minus one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Figure 6: Event Study Estimates, Indiana (INPC)



Note: This figure presents percent changes derived from event study estimates of changes in cumulative new diagnosis rate between the exposed and unexposed cohort. Exposed cohort is children with at least one INPC encounter between between February 2019 and July 2019. Exposed cohort is children with at least one INPC encounter between between February 2018 and July 2018. Sample includes children without an ADHD diagnosis during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively). February 2019/2020 is the reference period for the unexposed/exposed cohorts. Plotted percent changes are the exponentiated event study coefficients minus one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the county by cohort level.

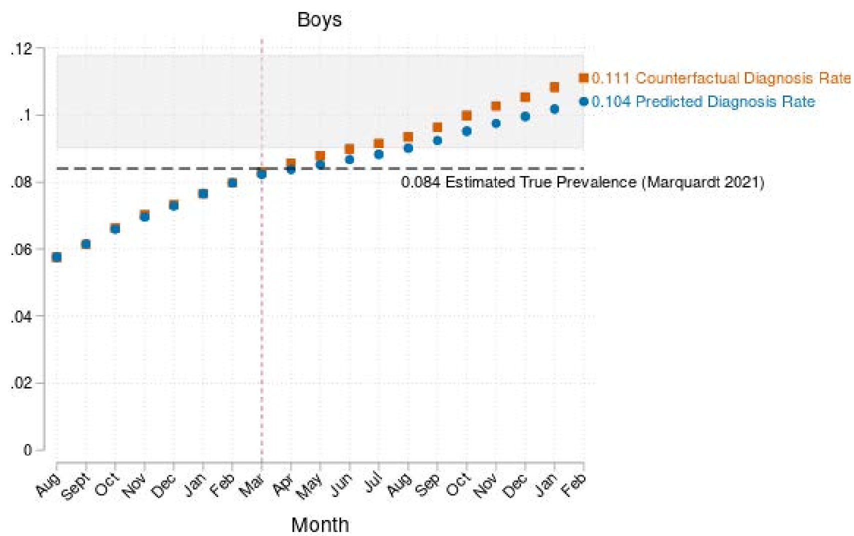
Figure 7: Event Study Estimates by School Opening, Indiana (INPC)



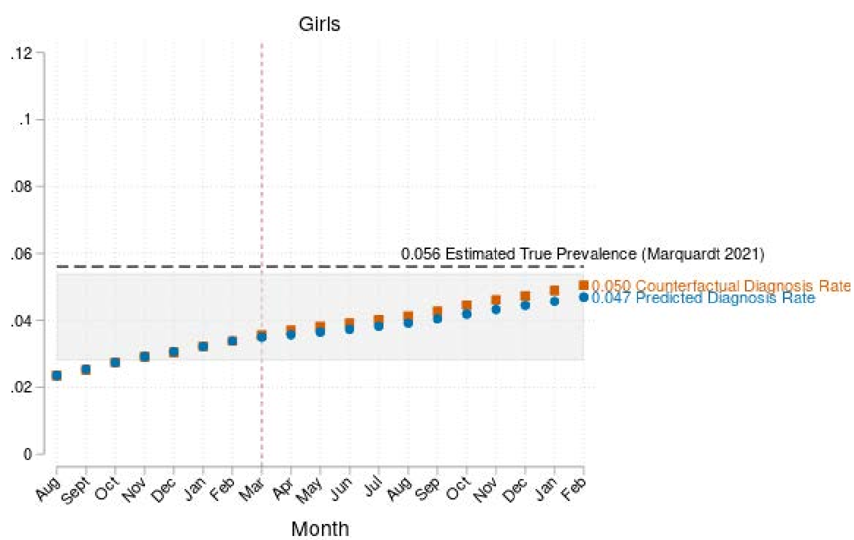
Note: This figure presents percent changes derived from event study estimates of changes in cumulative new diagnosis rate between the exposed and unexposed cohort, interacted with county school opening level. Exposed cohort is children with at least one INPC encounter between between February 2019 and July 2019. Exposed cohort is children with at least one INPC encounter between between February 2018 and July 2018. Sample includes children without an ADHD diagnosis during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively). February 2019/2020 is the reference period for the unexposed/exposed cohorts. Plotted percent changes are the exponentiated event study coefficients minus one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the county by cohort level.

Figure 8: Counterfactual Total Diagnoses

(a) Boys



(b) Girls



Note: This figure plots predicted and counterfacual diagnosis rates. We first calculate predicted and counterfactual (by setting the Exposed Cohort indicator to zero) cumulative new diagnosis rates for the exposed cohort based on the event study estimates. To translate these into total diagnosis rates we incorporate the number of children that did have an ADHD diagnosis during the lookback period. Total diagnosis rates are equal to (#PreviouslyDiagnosed + NewDiagnosisRate X #NotPreviouslyDiagnosed) / (#PreviouslyDiagnosed + #NotPreviouslyDiagnosed) where the New Diagnosis rate is replaced by either the predicted or counterfactual rate for each month. The black dotted line corresponds to the estimated true ADHD prevalence by gender from Marquardt (2022). The shaded area corresponds to the range of true ADHD prevalence found in the epidemiological and/or psychological literature.

Table 1: Sample Demographics and NHIS Comparisons

	Optum %	**NHIS (Private) %**	**INPC %**	**NHIS (All) %**
Female	48.69	49.77	48.55	48.76
White	72.04	64.17	66.04	51.25
Black	7.11	6.98	16.62	12.55
Asian	7.70	6.35	3.23	4.56
Hispanic	13.15	16.93	14.11	25.82
HH Income $\leq$ 74K	26.67	24.54		51.44
ADHD Dx, Boys	7.3	7.7	8.0	10.7
ADHD Dx, Girls	3.0	3.4	3.3	4.5

Note: This table presents demographic composition and ADHD diagnosis rates for the nationwide Optum sample, the INPC sample, and comparisons to random sample of children from the 2019 National Health Interview Survey (NHIS). Each sample include children born between 2009 and 2015. The second column restricts sample to children covered by private insurance whereas the fourth column includes all children, and all averages are weighted by the NHIS individual annual weights. ADHD diagnosis rates based on current diagnosis in year for NHIS samples and any diagnosis in year for Optum and INPC samples.

Table 2: Difference in Differences Estimates by Race and Ethnicity, Nationwide (Optum)

(a) Fixed Effect Poisson Coefficient Estimates

	(1) boys	(2) boys	(3) girls	(4) girls
Pandemic	-0.0897***	-0.0937***	-0.117***	-0.124***
	(0.0191)	(0.0213)	(0.0249)	(0.0268)
Pandemic X Asian		-0.0762		0.0208
		(0.101)		(0.104)
Pandemic X Black		0.124**		0.148*
		(0.0548)		(0.0807)
Pandemic X Hispanic		-0.0175		-0.0329
		(0.0443)		(0.0716)
Observations	42674	42674	42351	42351

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1) boys	(2) boys	(3) girls	(4) girls
Pandemic	-0.0858***		-0.110***	
	(0.0175)		(0.0222)	
White		-0.0895***		-0.116***
		(0.0194)		(0.0236)
Asian		-0.156*		-0.0979
		(0.0850)		(0.0923)
Black		0.0311		0.0246
		(0.0558)		(0.0808)
Hispanic		-0.105***		-0.145**
		(0.0353)		(0.0585)
Observations	42674	42674	42351	42351

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates over all in columns 1 and 3 and by race/ethnicity in columns 2 and 4. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Table 3: Difference in Differences Estimates by State School Opening Level, Nationwide (Optum)

(a) Fixed Effect Poisson Coefficient Estimates

	(1) boys	(2) girls
Pandemic	-0.0626**	-0.153***
	(0.0296)	(0.0409)
Pandemic X Medium Opening	-0.0561	0.00614
	(0.0423)	(0.0556)
Pandemic X High Opening	0.00180	0.117**
	(0.0378)	(0.0575)
Observations	42674	42351

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1) boys	(2) girls
LowOpening	-0.0607**	-0.142***
	(0.0278)	(0.0351)
MediumOpening	-0.112***	-0.137***
	(0.0275)	(0.0335)
HighOpening	-0.0590***	-0.0359
	(0.0229)	(0.0402)
Observations	42674	42351

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by state school opening levels. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Table 4: Difference in Differences Estimates by Local School Opening Level (INPC)

(a) Fixed Effect Poisson Coefficient Estimates

	(1) boys	(2) boys	(3) girls	(4) girls
Pandemic	-0.200***	-0.251***	-0.0732	-0.121*
	(0.0426)	(0.0160)	(0.0499)	(0.0696)
Pandemic X Medium		0.144**		0.213*
		(0.0695)		(0.116)
Pandemic X High		0.0439		-0.00150
		(0.0873)		(0.111)
Observations	16359	16169	13509	42351

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1) boys	(2) boys	(3) girls	(4) girls
Pandemic	-0.181***		-0.0706	
	(0.0349)		(0.0464)	
LowOpening		-0.222***		-0.114*
		(0.0124)		(0.0617)
MediumOpening		-0.101*		0.0957
		(0.0609)		(0.105)
HighOpening		-0.187***		-0.115
		(0.0665)		(0.0739)
Observations	16359	16169	13509	42351

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by county school opening levels. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the county by cohort level.

Table 5: Difference in Differences Estimates by State School Opening and Stability, Percent Changes

(a) Boys

	(1) LowStability	(2) MediumStability	(3) HighStability
LowOpening	-0.128***	-0.419***	-0.0144
	(0.0347)	(0.00823)	(0.0256)
MediumOpening	-0.174**	-0.122***	-0.105***
	(0.0697)	(0.0397)	(0.0275)
HighOpening	-0.0988***	-0.0815***	0.0258
	(0.0354)	(0.0257)	(0.0843)

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Girls

	(1) LowStability	(2) MediumStability	(3) HighStability
LowOpening	-0.0102	-0.0683	-0.210***
	(0.0672)	(0.124)	(0.0301)
MediumOpening	-0.0551	-0.174***	-0.145***
	(0.0994)	(0.0244)	(0.0512)
HighOpening	-0.0802**	-0.0783*	0.127
	(0.0407)	(0.0472)	(0.160)

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates interacted with both average school opening level and stability of school opening. The table presents the percent change for each opening by stability grouping by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

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SafeGraph Data: We use the Core Place of Interest (POI) and the Patterns files from SafeGraph, a data company that aggregates anonymized location data from numerous applications in order to provide insights about physical places, via the SafeGraph Community. To enhance privacy, SafeGraph excludes census block group information if fewer than two devices visited an establishment in a month from a given census block group.

Appendices

A SafeGraph and Administrative Policy Comparison

We first compare our state-level SafeGraph openness measure to state-level learning model policies as collected by COVID-19 School Data Hub (2022) (henceforth CSDH). We can only do this for the 39 states with weekly or monthly learning model data in CSDH, which we need to measure the share of Fall 2020 with in-person learning model (compared to virtual or hybrid learning options). Because our SafeGraph measure is based on visits to public elementary schools, we restrict to this level in the CSDH policy data where possible. There are 30 states that report school-level learning models, in which we keep only the public elementary schools as identified by their NCES School ID. For the 4 states with district-level-by-grade policy data, we keep only the learning model reported for Grade K-5. This leaves 5 states with only district-level policies in which we cannot separately identify elementary schools. After making these school or district restrictions, we calculate the share of the Fall 2020 semester that had a reported in-person learning model, and aggregate up to the state level. We then rank each state by in-person schooling policy and compare this rank to the analogous ranking based on our SafeGraph mobility measures. Figure A1 presents this state rank-rank comparison, with 45-degree line in red. The two ranking have a strong correlation of 0.867, suggesting that our school-openness measure derived from SafeGraph mobility data aligns closely with school or district level in-person school policies documented in the CSDH for the Fall of 2020.

With this analysis, we show that once aggregated to the state level, there is little difference in school-open policies and actual in-person school visits. We choose to use the SafeGraph-derived openness grouping for our preferred state-level analysis as it allows us to include all states whereas CSDH only has weekly and/or monthly reports for 39 states.

We conduct a similar exercise within Indiana, where we compare SafeGraph derived school-openness and CSDH in-person reporting at the zip-tract level. For each Indiana public elementary school with reports in CSDH, we calculate the share of Fall 2020 that offered in-person learning. We then use the same aggregation method described in Section 4 to construct a weighted average in-person policy share for each zip-tract comprised of schools with learning model reports in CSDH.

While SafeGraph-derived openness and CSDH in-person policy differences were minimal in the state-level analysis, we do see deviations within Indiana, especially for zip-tracts with low opening status based on our SafeGraph mobility measure. This can be seen visually in Figure A2, where we show the box-and-whisker of zip-tract CSDH in-person share, separately for each decile of our SafeGraph school openness measure. A majority of zip-tracts that we call "High-Opening" (those in SafeGraph decile 6-10) also report always in-person according to CSDH. However, for zip-tracts that we call "Medium-Opening" or "Low-Opening" (those in SafeGraph decile 1-5), there is significant heterogeneity in the share of Fall 2020 that reported in-person learning mode.

We think the main reason why the CSDH in-person share differs from our SafeGraph school-openness share, especially for the low and medium opening zip-tracts, is due to lower demand for in-person instruction even when available. The Indiana Department of Education defines a school to be "in-person" if over half of the students receive 75% of their instruction in-person.[19] This still allows for heterogeneity in visit levels to schools, which is likely captured by our SafeGraph school-openness measure, even when aggregated to the county-level.

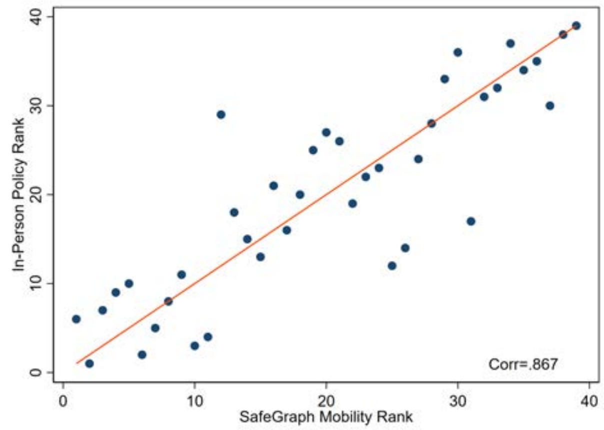
Our main specification for the within-Indiana analysis uses zip-tract school openness group derived from SafeGraph mobility data, aggregated up to the county level. This has an advantage over CSDH in-person policy reports because (1) it captures the variation in in-person school *utilization* rather than the somewhat ad-hoc definition of in-person school

[19]See the *Data Details for Indiana* sheet at www.covidschooldatahub.com/states/indiana for additional information on how learning models are defined by State and categorized by CSDH.

availability, and (2) it allows us to analyze almost the entire state whereas CSDH only has reports for schools that cover $\sim 50\%$ of the zip-tracts.[20] That said, we do note that there may be potential for this measure to bias our estimates in the low and medium opening groups if there is a correlation between likelihood of initial ADHD diagnosis and demand for in-person schooling. We think this is more likely to be correlated for children who already have ADHD rather than those not yet diagnosed, but we recognize there could be some confounding factors that link ADHD diagnosis potential and demand for in-person schooling. However, the direction of the bias is not clear.

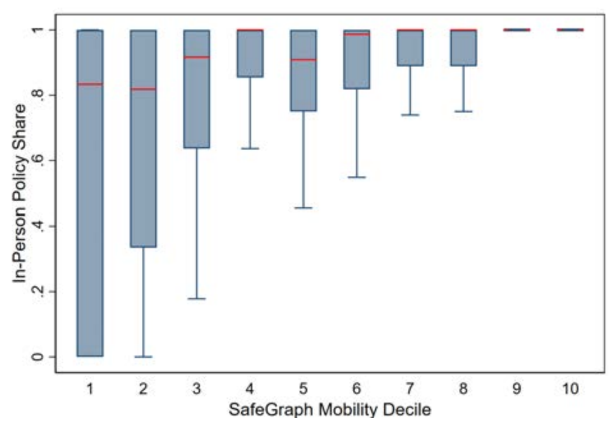
[20] 1,957 zip-tracts in Indiana are covered by schools reporting in CSDH and 4,122 zip-tracts are covered by schools with SafeGraph visit levels.

Figure A1: SafeGraph and CSDH State Rank-Rank Comparison, Fall 2020



Note: Figure plots the state-level rank of SafeGraph school-openness for Fall 2020 (x-axis) and state-level rank of CSDH share in-person for Fall 2020 (y-axis) for the 39 states in which CSDH rank could be determined. Higher ranking corresponds to higher school-openness and higher share in-person, respectively. 45-degree line in red.

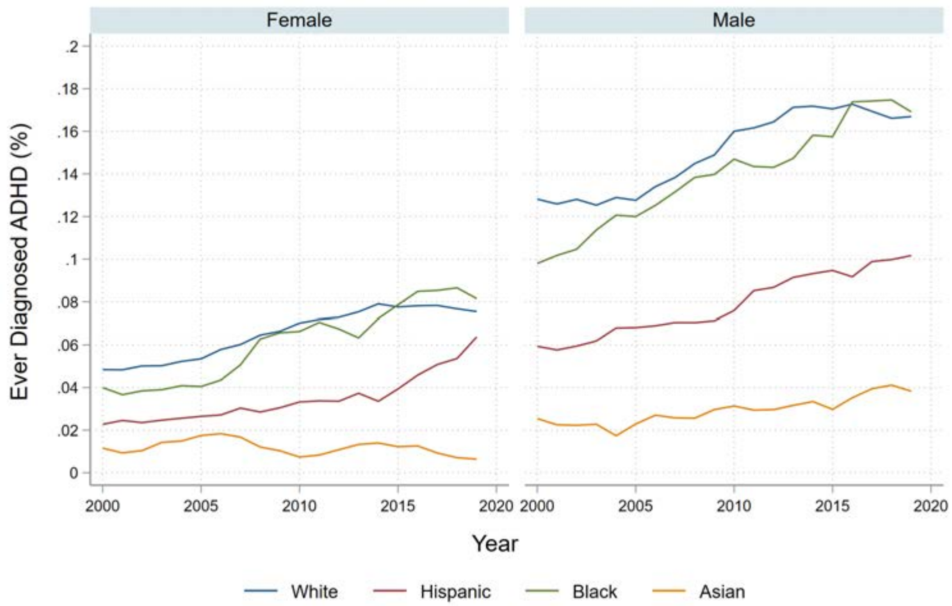
Figure A2: SafeGraph and CSDH County-Level Comparison, Fall 2020



Note: Box-and-whisker plot of CSDH in-person share for Fall 2020, by decile of SafeGraph school-openness for Fall 2020. Both CSDH in-person share and SafeGraph mobility are at the zip-tract level. Higher deciles correspond to higher rates of SafeGraph school-openness. Within each SafeGraph mobility decile, the median zip-tract's CSDH in-person share is denoted by the solid red line, the IQR in blue box, and the lower/upper adjacent values as whiskers.

B Additional Tables and Figures

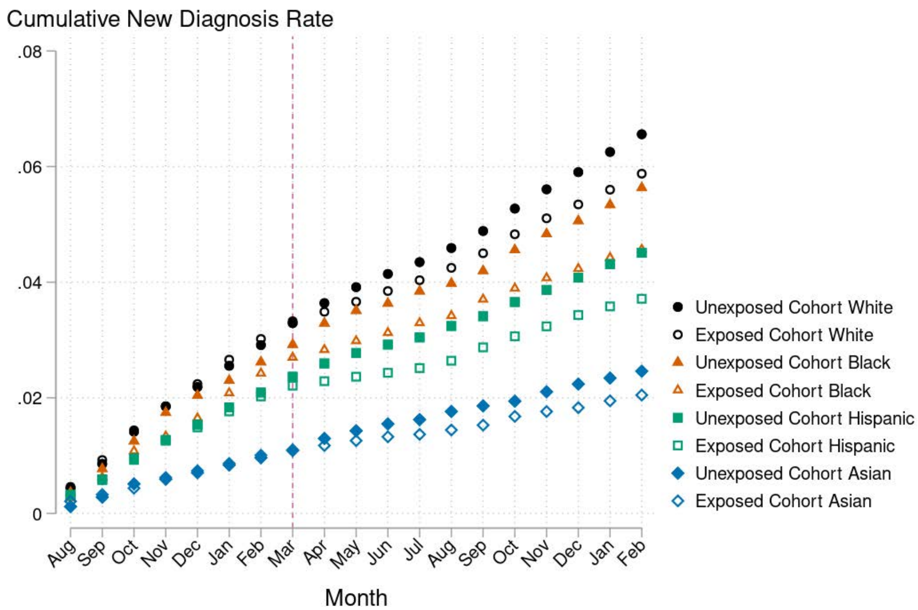
Figure B1: National ADHD Diagnosis Trends



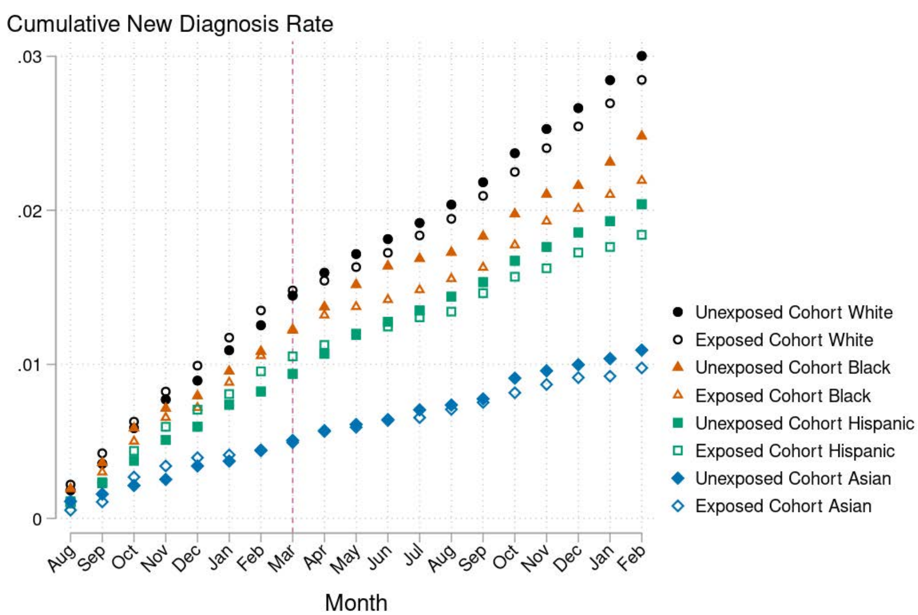
Note: This figure presents the percent of children aged 5-17 ever told they had ADHD by a medical professional, as identified within the National Health Interview Survey (NHIS). Gender and Race/ethnicity group averages are weighted by NHIS person sample weights. Trends are smoothed using 5-year moving averages.

Figure B2: Cumulative New Diagnoses by Cohort and Race

(a) Boys



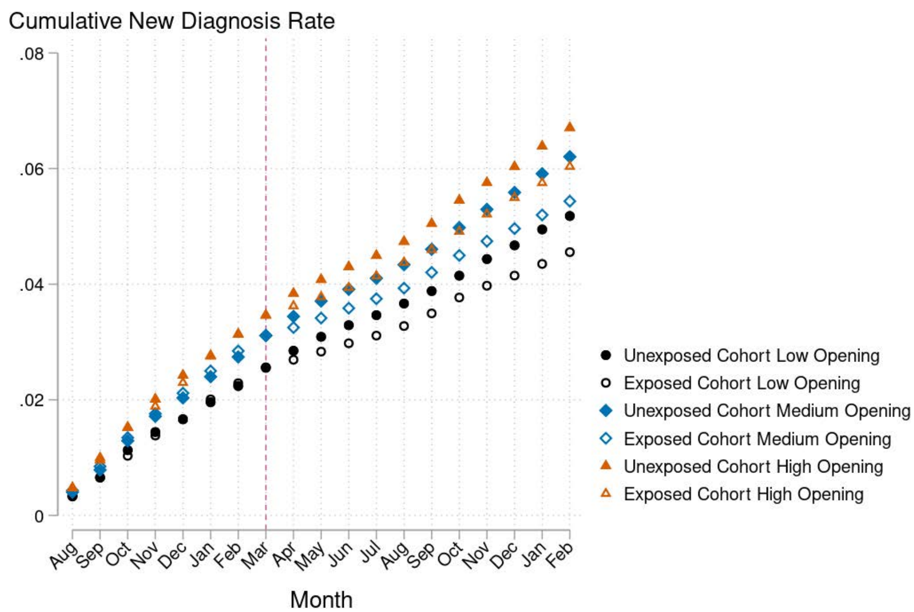
(b) Girls



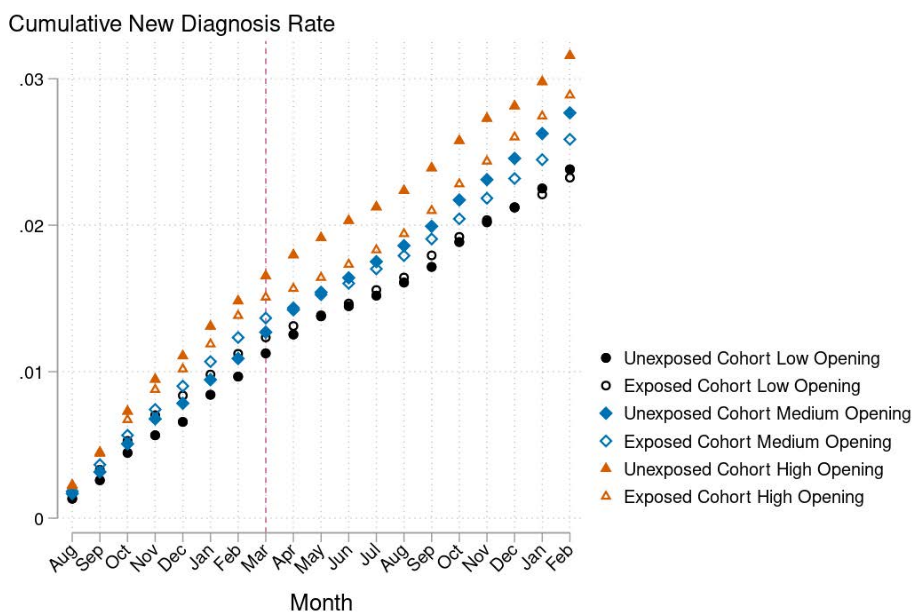
Note: Exposed cohort is children continuously enrolled between February 2019 and February 2021. Unexposed cohort is children continuously enrolled between February 2018 and February 2020. Sample includes children without an ADHD diagnosis during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively).

Figure B3: Cumulative New Diagnoses by Cohort and State School Opening Level

(a) Boys



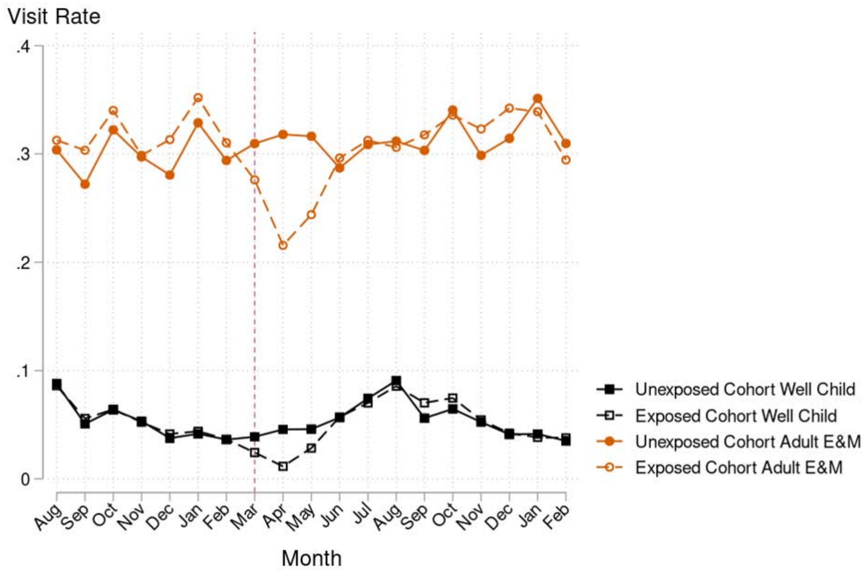
(b) Girls



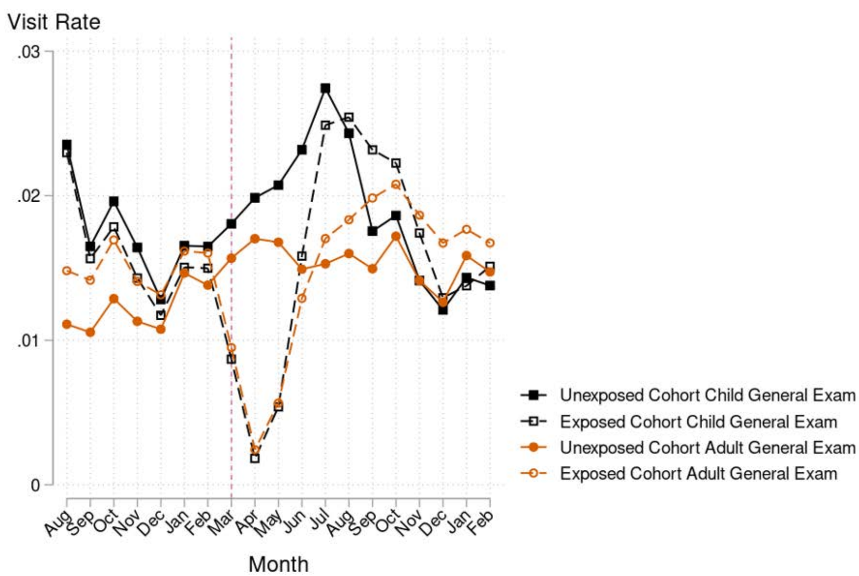
Note: Exposed cohort is children continuously enrolled between February 2019 and February 2021. Unexposed cohort is children continuously enrolled between February 2018 and February 2020. Sample includes children without an ADHD diagnosis during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively).

Figure B4: General Health Care Utilization

(a) Nationwide (Optum)

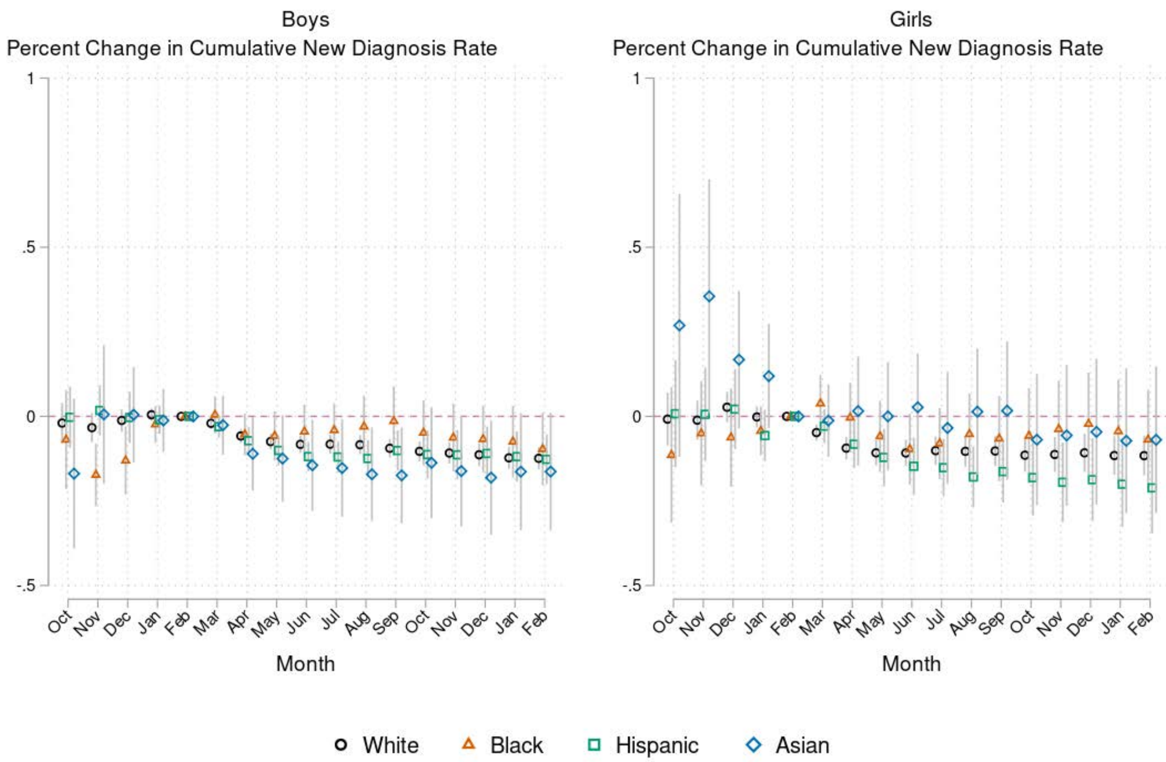


(b) Indiana (INPC)



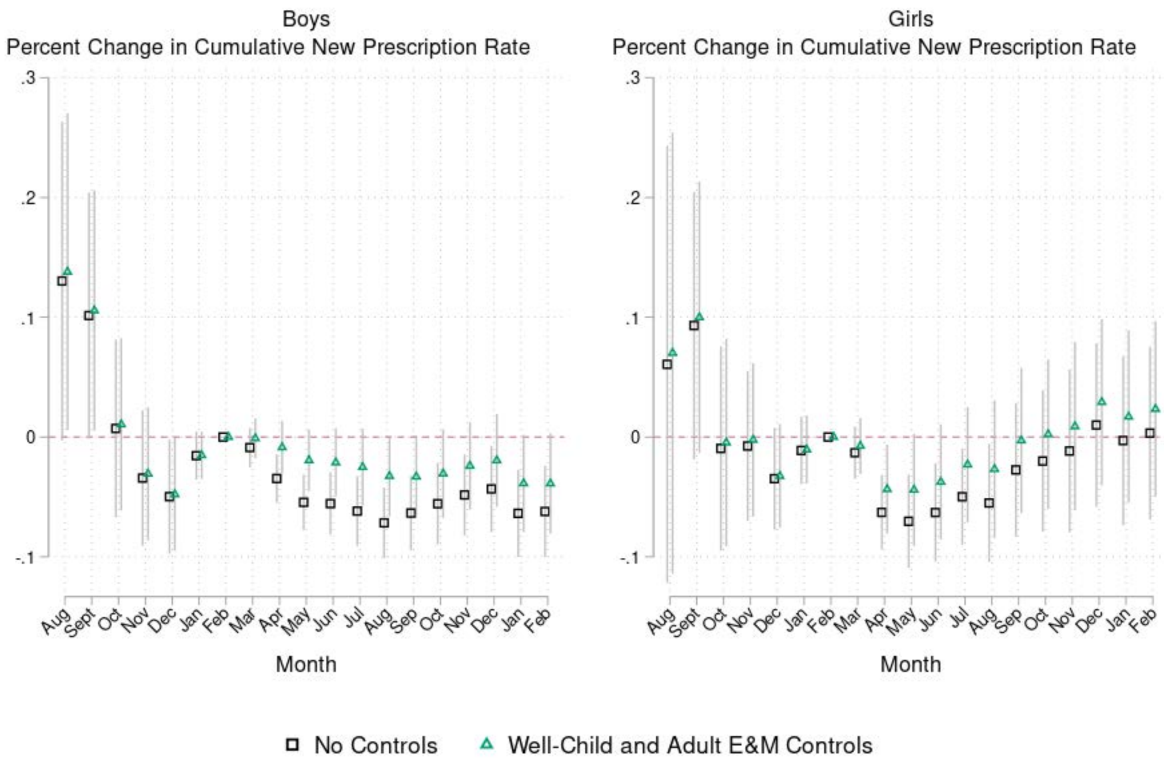
Note: In Panel A, exposed cohort is continuously enrolled between February 2019 and February 2021. Unexposed cohort is continuously enrolled between February 2018 and February 2020. Well Child visits and Adult E&M visits are identified using CPT codes. In Panel B, exposed cohort is patients with at least one INPC encounter between between February 2019 and July 2019. Unexposed cohort is patients with at least one INPC encounter between between February 2018 and July 2018. General Exams are determined by ICD10 Z codes associated with visits in INPC database.

Figure B5: Event Study Estimates by Race and Ethnicity



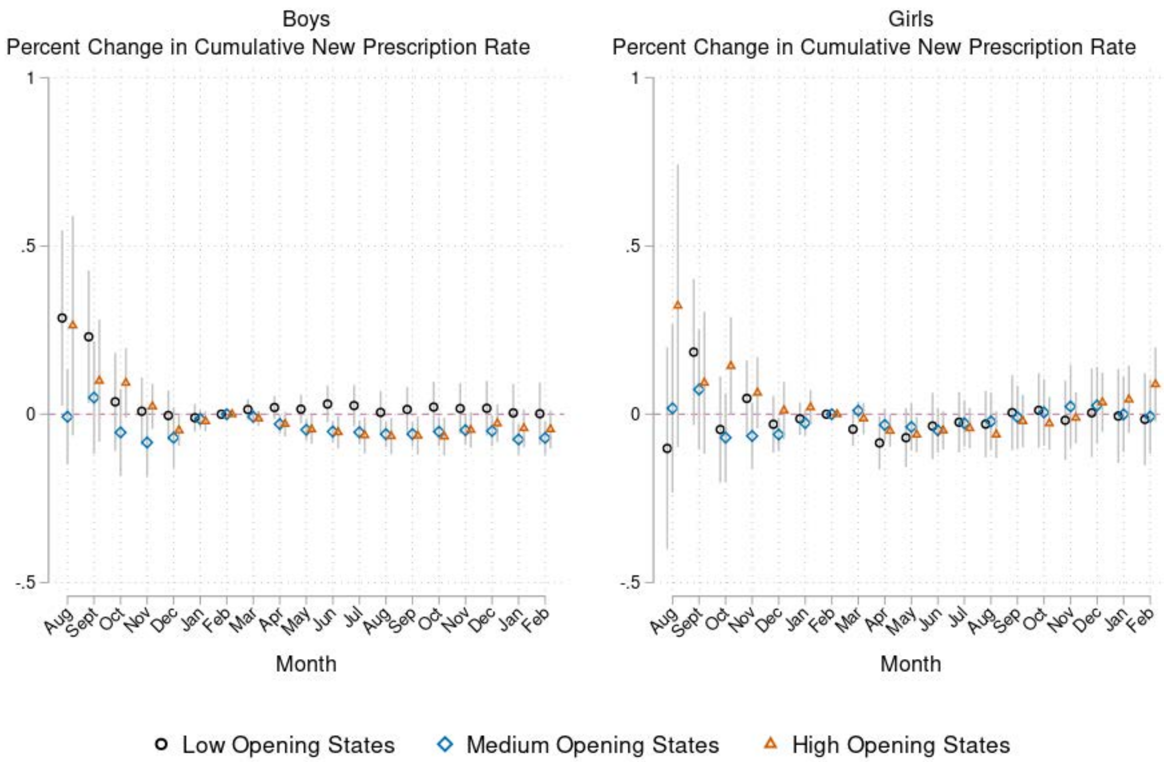
Note: This figure presents percent changes derived from event study estimates of changes in cumulative new diagnosis rate between the exposed and unexposed cohort, interacted with race and ethnicity indicators. Exposed cohort is children continuously enrolled between February 2019 and February 2021. Unexposed cohort is children continuously enrolled between February 2018 and February 2020. Sample includes children without an ADHD diagnosis during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively). February 2019/2020 is the reference period for the unexposed/exposed cohorts. Plotted percent changes are the exponentiated event study coefficients for each race/ethnicity group minus one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Figure B6: Event Study Estimates for New Prescriptions, Nationwide (Optum)



Note: This figure presents percent changes derived from event study estimates of changes in cumulative new prescription rate between the exposed and unexposed cohort. Exposed cohort is children continuously enrolled between February 2019 and February 2021. Unexposed cohort is children continuously enrolled between February 2018 and February 2020. Sample includes children without an ADHD prescription during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively). February 2019/2020 is the reference period for the unexposed/exposed cohorts. Plotted percent changes are the exponentiated event study coefficients minus one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Figure B7: Event Study Estimates by State School Opening Level for New Prescriptions, Nationwide (Optum)



Note: This figure presents percent changes derived from event study estimates of changes in cumulative new prescription rate between the exposed and unexposed cohort, interacted with state school opening level. Exposed cohort is children continuously enrolled between February 2019 and February 2021. Unexposed cohort is children continuously enrolled between February 2018 and February 2020. Sample includes children without an ADHD prescription during the six month lookback period (February 2019-July 2019 and February 2018-July 2018 for the exposed and unexposed cohorts, respectively). February 2019/2020 is the reference period for the unexposed/exposed cohorts. Plotted percent changes are the exponentiated event study coefficients for each state school opening group minus one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Table B1: Difference in Differences Estimates of New Prescriptions by State School Opening Level, Nationwide (Optum)

(a) Fixed Effect Poisson Coefficient Estimates

	(1) boys	(2) boys	(3) girls	(4) girls
Pandemic	-0.0220 (0.0204)	0.00581 (0.0362)	-0.00324 (0.0329)	-0.0108 (0.0620)
Pandemic X Medium Opening		-0.0183 (0.0483)		0.0397 (0.0842)
Pandemic X High Opening		-0.0633 (0.0468)		-0.0374 (0.0729)
Observations	42370	42370	41819	41819

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1) boys	(2) boys	(3) girls	(4) girls
Pandemic	-0.0218 (0.0199)		-0.00323 (0.0328)	
LowOpening		0.00583 (0.0364)		-0.0108 (0.0613)
MediumOpening		-0.0124 (0.0328)		0.0293 (0.0600)
HighOpening		-0.0559* (0.0293)		-0.0471 (0.0383)
Observations	42370	42370	41819	41819

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by state school opening levels. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Table B2: Difference in Differences Estimates by Type of New ADHD Diagnosis, Nationwide (Optum) Boys

(a) Fixed Effect Poisson Coefficient Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Inatentive		Hyperactive		Combined	
Pandemic	-0.0960**	0.0203	-0.138**	-0.146	-0.0864***	-0.0802
	(0.0424)	(0.0755)	(0.0535)	(0.0926)	(0.0252)	(0.0563)
Pandemic X Medium Opening		-0.182*		-0.0562		-0.0161
		(0.0932)		(0.111)		(0.0631)
Pandemic X High Opening		-0.0966		0.109		0.00409
		(0.106)		(0.140)		(0.0687)
Observations	41781	41781	41838	41838	42313	42313

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1)	(2)	(3)	(4)	(5)	(6)
	Inatentive		Hyperactive		Combined	
Pandemic	-0.0916**		-0.129***		-0.0827***	
	(0.0386)		(0.0466)		(0.0232)	
LowOpening		0.0205		-0.136*		-0.0771
		(0.0771)		(0.0800)		(0.0520)
MediumOpening		-0.150***		-0.183***		-0.0918***
		(0.0495)		(0.0526)		(0.0301)
HighOpening		-0.0735		-0.0356		-0.0733*
		(0.0702)		(0.106)		(0.0382)
Observations	41781	41781	41838	41838	42313	42313

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by state school opening levels. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Table B3: Difference in Differences Estimates by Type of New ADHD Diagnosis, Nationwide (Optum) Girls

(a) Fixed Effect Poisson Coefficient Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Inatentive		Hyperactive		Combined	
Pandemic	-0.166***	-0.122	-0.199***	-0.257	-0.0389	-0.0898
	(0.0478)	(0.0827)	(0.0726)	(0.160)	(0.0445)	(0.0779)
Pandemic X Medium Opening		-0.0760		-0.0437		-0.0106
		(0.101)		(0.190)		(0.101)
Pandemic X High Opening		-0.0221		0.259		0.184*
		(0.133)		(0.203)		(0.0981)
Observations	40546	40546	36404	36404	40983	40983

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1)	(2)	(3)	(4)	(5)	(6)
	Inatentive		Hyperactive		Combined	
Pandemic	-0.153***		-0.181***		-0.0382	
	(0.0405)		(0.0595)		(0.0428)	
LowOpening		-0.115		-0.226*		-0.0859
		(0.0732)		(0.123)		(0.0712)
MediumOpening		-0.180***		-0.260***		-0.0955
		(0.0534)		(0.0797)		(0.0605)
HighOpening		-0.134		0.00230		0.0991
		(0.0909)		(0.125)		(0.0714)
Observations	40546	40546	36404	36404	40983	40983

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by state school opening levels. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Table B4: Difference in Differences Estimates by Type of New ADHD Diagnosis, Indiana (INPC) Boys

(a) Fixed Effect Poisson Coefficient Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Inatentive		Hyperactive		Combined	
Pandemic	-0.196*	-0.104	-0.271	-0.694***	-0.175***	-0.224***
	(0.115)	(0.124)	(0.216)	(0.0757)	(0.0472)	(0.0288)
Pandemic X Medium Opening		0.0484		0.922***		0.0882
		(0.177)		(0.284)		(0.0957)
Pandemic X High Opening		-0.447*		0.562*		0.0506
		(0.249)		(0.313)		(0.0891)
Observations	5928	5928	5472	5472	11324	11324

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1)	(2)	(3)	(4)	(5)	(6)
	Inatentive		Hyperactive		Combined	
Pandemic	-0.178*		-0.238		-0.161***	
	(0.0948)		(0.165)		(0.0396)	
LowOpening		-0.0989		-0.500***		-0.200***
		(0.112)		(0.0378)		(0.0230)
MediumOpening		-0.0542		0.256		-0.127
		(0.126)		(0.339)		(0.0830)
HighOpening		-0.424***		-0.124		-0.159**
		(0.125)		(0.284)		(0.0687)
Observations	5928	5928	5472	5472	11324	11324

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by county school opening levels. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the county by cohort level.

Table B5: Difference in Differences Estimates by Type of New ADHD Diagnosis, Indiana (INPC) Girls

(a) Fixed Effect Poisson Coefficient Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Inatentive		Hyperactive		Combined	
Pandemic	0.259*	0.284**	-0.154	-0.195	-0.140*	-0.261***
	(0.143)	(0.110)	(0.273)	(0.192)	(0.0755)	(0.0557)
Pandemic X Medium Opening		0.718***				0.156
		(0.224)				(0.138)
Pandemic X High Opening		-0.291		-0.227		0.203
		(0.215)		(0.530)		(0.146)
Observations	6840	6840	3420	3258	8835	8835

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1)	(2)	(3)	(4)	(5)	(6)
	Inatentive		Hyperactive		Combined	
Pandemic	0.296		-0.143		-0.130**	
	(0.186)		(0.234)		(0.0657)	
LowOpening		0.328**		-0.177		-0.230***
		(0.147)		(0.158)		(0.0429)
MediumOpening		1.722***		-0.177		-0.100
		(0.532)		(0.158)		(0.119)
HighOpening		-0.00709		-0.344		-0.0563
		(0.208)		(0.331)		(0.124)
Observations	6840	6840	3420	3258	8835	8835

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by county school opening levels. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the county by cohort level.

Table B6: Difference in Differences Estimates by Grade, Nationwide (Optum)

(a) Fixed Effect Poisson Coefficient Estimates

	(1) K	(2) 1	(3) 2	(4) 3	(5) 4	(6) 5
Boys	-0.213**	-0.0920**	-0.0942**	-0.0901**	-0.0449	-0.102***
	(0.0842)	(0.0405)	(0.0367)	(0.0371)	(0.0343)	(0.0375)
Observations	6726	6745	6802	7163	7125	6992
Girls	-0.0927	-0.129*	-0.0338	-0.192***	-0.0615	-0.128***
	(0.114)	(0.0752)	(0.0583)	(0.0611)	(0.0481)	(0.0481)
Observations	6517	6821	6650	6612	6859	6840

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1) K	(2) 1	(3) 2	(4) 3	(5) 4	(6) 5
Boys	-0.192***	-0.0879**	-0.0899***	-0.0862**	-0.0439	-0.0970***
	(0.0680)	(0.0369)	(0.0334)	(0.0339)	(0.0328)	(0.0339)
Observations	6726	6745	6802	7163	7125	6992
Girls	-0.0886	-0.121*	-0.0333	-0.175***	-0.0596	-0.120***
	(0.104)	(0.0661)	(0.0563)	(0.0504)	(0.0452)	(0.0423)
Observations	6517	6821	6650	6612	6859	6840

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by grade. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.

Table B7: Difference in Differences Estimates by Grade, Indiana (INPC)

(a) Fixed Effect Poisson Coefficient Estimates

	(1) K	(2) 1	(3) 2	(4) 3	(5) 4	(6) 5
Boys	-0.161 (0.102)	-0.129 (0.136)	-0.254*** (0.0740)	-0.296*** (0.0684)	-0.173** (0.0724)	-0.192** (0.0928)
Observations	1672	1786	1672	1824	1710	1881
Girls	-0.140 (0.155)	0.0236 (0.106)	-0.149 (0.128)	-0.128 (0.155)	0.0977 (0.0963)	-0.151 (0.110)
Observations	1121	1292	1463	1292	1558	1330

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

(b) Overall Percent Changes

	(1) K	(2) 1	(3) 2	(4) 3	(5) 4	(6) 5
Boys	-0.149* (0.0868)	-0.121 (0.120)	-0.224*** (0.0574)	-0.256*** (0.0509)	-0.159*** (0.0609)	-0.175** (0.0766)
Observations	1672	1786	1672	1824	1710	1881
Girls	-0.131 (0.135)	0.0239 (0.108)	-0.139 (0.111)	-0.121 (0.137)	0.103 (0.106)	-0.140 (0.0947)
Observations	1121	1292	1463	1292	1558	1330

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Note: This table presents difference in difference estimates by grade. Panel A presents Poisson regression coefficients. Panel B presents the percent change for each group by exponentiating the appropriate sum of coefficients and subtracting one. 95% confidence intervals are derived using the delta method. Standard errors are clustered at the state by cohort level.