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BACK TO SCHOOL:
THE EFFECT OF SCHOOL VISITS DURING COVID-19 ON COVID-19 TRANSMISSION

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ABSTRACT

Schools across the United States and the world have been closed in an effort to mitigate the spread of COVID-19. However, the effect of school closure on COVID-19 transmission remains unclear. We estimate the causal effect of changes in the number of weekly visits to schools on COVID-19 transmission using a triple difference approach. In particular, we measure the effect of changes in county-level visits to schools on changes in COVID-19 diagnoses for households with school-age children relative to changes in COVID-19 diagnoses for households without school-age children. We use a data set from the first 46 weeks of 2020 with 130 million household-week level observations that includes COVID-19 diagnoses merged to school visit tracking data from millions of mobile phones. We find that increases in county-level in-person visits to schools lead to an increase in COVID-19 diagnoses among households with children relative to households without school-age children. However, the effects are small in magnitude. A move from the 25th to the 75th percentile of county-level school visits translates to a 0.3 per 10,000 household increase in COVID-19 diagnoses. This change translates to a 3.2 percent relative increase. We find larger differences in low-income counties, in counties with higher COVID-19 prevalence, and at later stages of the COVID-19 pandemic.

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1. INTRODUCTION

The COVID-19 pandemic has the potential to expose everyone to a deadly virus. One potentially important vector of transmission that has been under increasing scrutiny is within- and between-household viral transfer from school reopenings (Kim et al. 2020; Ludvigsson 2020; Lessler et al. 2021). Early in the pandemic, school closures were seen as a mitigation and containment strategy to prevent the spread of COVID-19 (Donohue and Miller 2020). By April 2020, all 50 states closed elementary and secondary public schools for the remainder of the academic year to slow COVID-19 transmission (Kaufman et al. 2021), although many schools eventually reopened. There were two primary rationales for the enactment of these closures. First, the risk of COVID-19 infection to children was not known during the initial stage of the pandemic. Second, children were assumed to be important vectors for the spread of COVID-19 (Christakis, Cleve, and Zimmerman 2020). Both factors were critical given that between 33.9 million and 44.2 million adults with risk factors for severe COVID-19, as defined by the Centers for Disease Control and Prevention (CDC), have direct or within-household connections to schools (Selden, Berdahl, and Fang 2020).

The extent to which school reopenings contribute to COVID-19 transmission among both children and adults remains unclear. As discussed below, the social proximity of in-person schools creates opportunities for disease spread. At the same time, schools and households may respond to increased risks of school transmission by reducing other sources of COVID-19 risk (Arrow 1951; Peltzman 1975; Arrow et al. 1996; Becker 1976). For example, households exposed to risk of COVID-19 infection in schools might be more likely to limit social gatherings, wash hands more frequently, and wear masks more consistently. Likewise, school-based gatherings are likely to occur in regulated settings that may be less risky than some unregulated settings, such as at home child care (Doolittle 2020). In addition, knowledge of the virus has improved over time, and mitigation measures by schools have been responsive to this new information (National Academies of Sciences, Engineering, and Medicine 2020).

Despite potential increased risk of COVID-19 transmission, in-person school creates substantial benefits (Levinson, Cevik, and Lipsitch 2020). First, prolonged school closures will hurt long-term human capital formation and exacerbate disparities in educational attainment between high- and low-income areas (Chetty et al. 2020; Bacher-Hicks, Goodman, and Mulhern 2021; Psacharopoulos et al. 2020; Kuhfeld and Tarasawa 2020). Second, schools are a critical food source and provider of nutrition assistance for children (Poole, Fleischhacker, and Bleich 2021; Sharfstein and Morphew 2020; Masonbrink and Hurley 2020). Third, schools are an important source of health care for children, and school-based health centers are associated with improved health care and preventive services and better health outcomes (Martin and Sorensen 2020; Sharfstein and Morphew 2020; Masonbrink and Hurley 2020). Finally, schools provide critical childcare functions that allow parents to engage in the workforce (National Academies of Sciences, Engineering, and Medicine 2020). There are also critical equity concerns given that children from low-income families often depend more on school-based services than higher-income families (Donohue and Miller 2020).

A challenge to examining the impact of school closure policies on COVID-19 risk is the non-uniform nature of school closures and reopenings. Some schools have fully returned to in-person instruction (Meckler 2021). Other schools have implemented hybrid models. Several large school districts (e.g., Chicago, Seattle, and San Francisco) have not completely returned to in-person instruction (Fuller and Taylor 2021; Gates 2021; Issa 2021). In addition, within many school districts, public and private schools often operate differently, with some private schools reopening while public schools remain closed (Carpenter and Dunn 2021). To date there is no national database of in-person school attendance rates, community testing, and case rates (Oster 2021). There is also no national database on the mitigation strategies employed by schools and school districts (Goldhaber et al. 2021).

In this paper, we leverage two large sources of national data to add to the knowledge base on how school reopening might affect spread of COVID-19. We leverage mobile phone tracking data provided by SafeGraph.[1] SafeGraph collects global positioning system (GPS) data from approximately 45 million U.S. mobile phones. The SafeGraph data has been used in several prior studies to measure social distancing behaviors or trends in visits to specific locations during the COVID-19 pandemic (Allcott et al. 2020; Dave, McNichols, and Sabia 2021; Gao et al. 2020; Goolsbee and Syverson 2020; Jay et al. 2020; Nguyen et al. 2020; Gupta, Simon, and Wing 2020; Cantor et al. 2020; Cantor, Stein, and Saloner 2020; Lasry et al. 2020). A unique feature of the SafeGraph data is the ability to identify the number of visits to specific locations, which are mapped to industry codes (e.g., NAICS code 622110 identifies "General Medical and Surgical Hospitals"). We use the industry code for schools to measure weekly county-level visit patterns to primary and secondary schools, using an approach similar to that of Goolsbee and Syverson (2020).[2] This approach allows us to identify geographic and temporal variation in visits to schools during the COVID-19 pandemic. We use this linkage to construct visit measures for approximately 131,000 U.S. schools.

We then link the county-level schools visits data to medical claims data collected from a nationwide sample of approximately 7 million individuals (3 million households) with employer-

[1] Safegraph. www.safegraph.com
[2] We do not consider visits to colleges and other post-secondary schools (M. S. Andersen et al. 2020; Mangrum and Niekamp 2020).

sponsored health insurance. The medical claims information allows us to identify the occurrence and timing of individual COVID-19 diagnoses and tests. As of December 9, 2020, we observe almost 93,000 patients with COVID-19 diagnoses, including 10,255 children and 82,180 adults. We also observe each individual's county of residence, which allows us to link COVID-19 outcomes to measures of the number of weekly school visits. The data covers 99 percent of U.S. counties. An important limitation of this data is that we do not observe households covered by public insurance or without insurance. Those with public insurance in one health system were at increased likelihood of being hospitalized with COVID-19 (Price-Haywood et al. 2020).

Even with this linkage, measuring the causal effect of school visits on risk of COVID-19 infection is challenging given the endogenous nature of school reopenings. Schools may be less likely to reopen when population-level COVID-19 cases and the risks of transmission are highest or trending upwards. In addition, several school districts reopened but then closed following increases in community cases (Shapiro 2020). Thus, measuring the effect of school reopenings by comparing COVID-19 cases between reopened and non-reopened schools, or gradients of reopening using school visits data, is likely to lead to biased results.

Instead, we leverage a unique feature of the medical claims data that allows us to identify households and household composition. The employer-sponsored insurance plans represented in our data provide both individual and family health insurance coverage. For family coverage policies, all covered individuals are linked to the same household plan. We use this linkage to identify households. Under the assumption that families with school age children are more likely to be exposed to school-based COVID-19 transmission, we use a triple-differences model to compare how changes in school visits within a county over time lead to changes in household-level COVID-19 infection rates for households with and without school age children within the same county.

We find that increased school visits lead to increases in COVID-19 infections for households with children, relative to households without children. However, the increase is modest in magnitude. Our results imply that a one-log unit increase in school visits leads to a 0.4 per 10,000 (0.003 percentage point) increase in a household's risk of COVID-19 infection. Based on the distribution of school visits and baseline infection rates during the COVID-19 pandemic, these results imply that moving from the 25th percentile of school visits to the 75th percentile leads to an approximately 3 percent increase in risk of COVID-19 infection in a household with children. A change in school visit of the magnitude induced by county-level shelter-in-place (SIP) policies leads to a 1.3 percent increase in household COVID-19 infection rates. As secondary outcomes, we do not find changes in COVID-related medical spending and a 2.7% increase in COVID-related hospitalizations.

These results are robust to several additional tests, including controls for other policy changes that may be correlated with restricted visits to schools (Courtemanche et al. 2020; Cook, Newberger, and Smalling 2020), mobility for other sources of transmission (Pray et al. 2021), variation in the intensity of the COVID-19 pandemic (Cantor et al. 2020), and unobserved household-level differences that may impact risks of COVID-19 transmission (Goldstein, Lipsitch, and Cevik 2020). In additional tests, we find that increased school visits were associated with lower rates of transmission during the initial months of the COVID-19 pandemic (March to June), when COVID-19 prevalence was lower, and that nearly all of the main effect is driven by higher transmission rates during the later months (September to December). We also find that school visits have larger effects on infection rates in counties with higher COVID-19 prevalence. The increased infection rates in counties with higher COVID-19 prevalence is important to highlight given that a recent CDC finding indicated that schools with proper mitigation techniques can have reduced transmission where community transmission is high (Falk et al. 2021; Doyle et al. 2021; Hershow et al. 2021; Dawson et al. 2021).

We also find that the increase in school-based COVID-19 transmission is driven by households that reside in counties in the bottom income quartiles. This finding is particularly relevant, as it suggests that the socioeconomic disparities observed in other studies also exist for school reopening policies (Panovska-Griffiths et al. 2020; Jay et al. 2020; Lee et al. 2021). Because our data comes from households covered by employer-based insurance, our results likely understate the income-based disparities in COVID-19 infections and hospitalizations. At the same time, students from lower-income households are more likely to have learning disruptions than students from higher-income households, leading to concerns about growing disparities in childhood education (Bacher-Hicks, Goodman, and Mulhern 2021; Dorn et al. 2020; Engzell, Frey, and Verhagen 2020).

In addition to within-household transmission originating from schools, another concern is increased risk of infection for teachers and school staff following school reopenings (Gold et al. 2021; Vlachos, Hertegård, and B Svaleryd 2021). However, we find that increased school visits are not associated with increased transmission rates for households in which the primary insurance subscriber is employed by a firm in the education industry relative to other industries. In addition to within-household transmission originating from schools, another concern is increased risk of infection for teachers and school staff following school reopenings (Gold et al. 2021; Vlachos, Hertegård, and B Svaleryd 2021).

Finally, we leverage the medical claims data to estimate the health care spending impacts of school reopenings. We do not find a change in health care costs following school reopenings.

Our findings make two important contributions. First, our findings inform the ongoing policy debate on school reopenings (Goldhaber-Fiebert, Studdert, and Mello 2020; Lordan, FitzGerald, and Grosser 2020). Our findings suggest that school reopenings do contribute to COVID-19 transmission, but the magnitude of the effect is modest. When compared to other epidemiological literature on school-related risks, our estimated effects are comparable to rates of child emergency department admissions for school-related injuries (Zagel et al. 2019). Likewise, on average the cost for administering school-based mitigation strategies to properly combat COVID-19 transmission ranges from $55 to $442 per student (Rice et al. 2020). Policymakers, households, and educators should weigh the modest risks of COVID-19 transmission combined with the limited costs of implementing mitigation strategies within schools against the significant benefits of in-person schooling.

Second, we also contribute to the broader economics literature on risky behaviors. An important finding from related studies is how economic agents compensate for increased risk. Most notably, the introduction of automobile safety laws lead to an increase in unsafe driving, and thus more pedestrian deaths (Peltzman 1975; Cohen and Dehejia 2004). Related studies have found that medical advances and expanded payment for health technology can lead to increases in risky behavior (Lakdawalla, Sood, and Goldman 2006; Cawley and Ruhm 2011; Doleac and Mukherjee 2018). School reopenings may have the opposite effect if increased risks of COVID-19 transmission lead families and policymakers to reduce risks elsewhere. How economic agents and institutions respond to risk of potentially deadly infection in the face of these benefits is not clear. Further, the tradeoff between potential increased COVID-19 transmission and the education benefits of in-person schooling are also not clear, but depend on causally estimating the effect of school reopenings on COVID-19 transmission.

The rest of the paper proceeds as follows. Section 2 provides an overview of the existing evidence on school-related COVID-19 transmission. Section 3 describes the data used in the study. Section 4 presents our empirical approach. Section 5 reports our main results, and Section 6 presents additional results that serve as robustness checks. Section 7 concludes.

2. BACKGROUND ON SCHOOL-RELATED COVID-19 TRANSMISSION

COVID-19 infections are significantly less harmful for children when compared to adults (Rabinowicz, Leshem, and Pessach 2020). For example, the CDC estimates that compared to adults 40 to 49 years of age, children 5 to 17 years of age have 160 times lower risk of death from COVID-19 and 27 times lower risk of hospitalization from COVID-19 (CDC 2020). Other estimates suggest that younger children might be less likely to contract the virus. For example, a recent meta-analysis of 32 studies concluded that children and adolescents younger than 20 years had 44 percent lower odds of secondary infection with SARS-CoV-2 compared with adults 20 years and older. The study noted that data were insufficient to conclude whether transmission of SARS-CoV-2 by children is lower than by adults (Viner, Mytton, et al. 2021).

The lower harm from COVID-19 in children and the reduced risk of contracting COVID-19 has led to a larger policy debate on whether schools should continue to be closed in order to reduce the transmission of COVID-19 (Christakis, Cleve, and Zimmerman 2020). Survey estimates from June 2020 indicated that close to 49 percent of parents would send their child to school in the fall, while roughly 31 percent stated they would not (Kroshus et al. 2020). Other studies find that racial and ethnic minorities are more likely to be concerned with school reopenings (Gilbert et al. 2020). The American Academy of Pediatrics has also encouraged in-person education during the pandemic (American Academy of Pediatrics 2021). Nevertheless, an early report by the National Academies of Sciences, Engineering, and Medicine (2020) concluded that there is limited evidence as to how easily children and youth contract the virus, how contagious they are once they do, and what strategies for limiting transmission within a school setting are most effective. The limited evidence made it difficult to both assess the health risks of opening schools and to develop strategies for limiting transmission of the virus once open.

More recently, CDC researchers have proposed that schools can reopen safely provided actions are taken to reduce community transmission and the limiting of school-related activities such as indoor sports practice that increase the risk of transmission (Honein, Barrios, and Brooks 2021), and the CDC has posted guidelines for school reopenings (CDC 2021).[3] Other estimates from the CDC find that COVID-19 cases in those aged 0-17 have increased since the summer of 2020 and that weekly incidence was higher in each older age group (Leidman et al. 2021; Leeb et al. 2020).

Existing empirical studies that have examined the impact of school reopenings or closures are limited and have generally found mixed results. A study by Goldhaber et al. (2021) uses data from Michigan and Washington to examine changes in county-level COVID-19 case rates due to in-person schooling. The authors use a variety of ordinary least squares specifications that include school district or county fixed effects. They find that school districts offering hybrid or in-person instruction are not a significant contributor to COVID-19 spread in communities with low case rates. It is only in communities with the highest case rate (95th percentile in Michigan and 75th percentile in Washington) where the relationship was statistically significant (Goldhaber

[3] Similarly, one recent study in 17 rural Wisconsin schools was able to attribute that only approximately four percent of cases were due to in-school spread of the virus (Falk et al. 2021).

et al. 2021). Second, a national study used interrupted time series analysis and found that school closures in the United States between March and May 2020 were associated with a decline in COVID-19 incidence and mortality, and that the magnitude of the effect varied based on the cumulative incidence of COVID-19 (Auger et al. 2020). However, it is difficult to interpret findings from an interrupted time series analysis as causal given the lack of a control group to model the counterfactual course of the epidemic in the absence of school closure.

Related work finds that school reopenings in both in-person and hybrid forms are not associated with increases in COVID-19 hospitalizations (Harris, Ziedan, and Hassig 2021). A study based in Japan also used a similar interrupted time series design and found that school closure did not affect COVID-19 incidence (Iwata, Doi, and Miyakoshi 2020). A report by the Public Health Agency of Sweden compared data on COVID-19 incidence in children from Finland and Sweden, two similar countries who followed different strategies – schools were open in Sweden, but Finland closed schools from March 18 until May 13. The report finds little or no difference in overall incidence of COVID-19 in children as of June 14, 2020 with incidence in Sweden being 49 per 100,000 children and incidence in Finland being 52 per 100,000 children. A potential issue with the analysis is that Finland and Sweden are also different along other dimensions. For example, Finland swiftly imposed a two-month lockdown after the emergence of COVID-19, so the trajectory of the epidemic in Finland was likely different than Sweden, which hardly imposed any restrictions (Höppner 2020). More recent work finds that the opening of schools in Sweden increased the likelihood of a positive COVID-19 diagnosis. The effect was found in teachers but weaker for parents and teachers' parents (Vlachos, Hertegård, and B Svaleryd 2021). Similar work finds no change in COVID-19 infections following school reopening in Germany (Isphording, Lipfert, and Pestel 2020). Finally, a recent study using national survey of U.S. adults in November-December 2020 and January-February 2021 found that living in a household with a child who is in full-time in-person schooling is associated with increased risk of having a positive COVID-19 test. The effect was larger for older children, and separately was smaller for each mitigation technique employed by the child's school (Lessler et al. 2021). However, the analysis was limited to a sample that self-selected into COVID-19 testing and exploited variation in school attendance across counties or geographies with different COVID-19 prevalence and incidence. Also, it is important to note that this study is limited to two data points of collection that do not capture the peak of childhood infections in the summer of 2020 (Leeb et al. 2020; Leidman et al. 2021). Several modelling studies have also estimated the impact of school closures and other non-pharmaceutical interventions. We do not review this literature here as modeling studies are suitable for generating hypotheses and do not directly assess the impact of school closures. A related literature examines the impact of college openings on transmission and finds that colleges reopening in-person college instruction leads to increases in transmission. However, this evidence does not generalize to school reopenings given the large differences in the number of visits, living conditions, and risk of infection for college versus school student populations.

3. DATA

3.1. Number of School Visits to Measure School-Based Physical Mobility

A challenge with examining the effect of school reopenings and COVID-19 transmission is the lack of nationwide data on school reopenings. To address this limitation, we use phone mobility data provided by SafeGraph. The SafeGraph data aggregates global positioning system pings from 45 million mobile phones. The data are from mobile applications that obtain opt-in consent from its users to collect anonymous location data. These data have been used to track the number of visits to businesses (Goolsbee and Syverson 2020), and health care professionals (Cantor et al. 2020; Jay et al. 2020; M. Andersen, Bryan, and Slusky 2020; Kranz et al. 2021). Within the SafeGraph data we identify elementary and secondary schools using NAICS code 611110 for "Elementary and Secondary Schools". This process identifies 131,080 U.S. schools. Then, using the data, we collapse the total number of visits at the week-year level for each county. We use all visits to schools to measure visits to schools. As a sensitivity test, we also restrict visits to those lasting at least four hours.

With the mobile phone data, we construct two measures of the number of visits to schools. First, for each county g and week t, we use a modified version of the approach outlined in Goolsbee and Syverson (2020) and calculate the log-transformed county-level difference in the number of visits to schools between each 2020 week and the same week in 2019.

$$\ln \Delta school_{gt} = \ln(school_{gt2020}) - \ln(school_{gt2019}) \tag{1}$$

This approach effectively measures the weekly percent change in visits to schools for each county relative to pre-pandemic level.

We present distributions of each measure across counties in Figure 1, using kernel density plots for the weeks of March 1, May 10, and September 27, 2020. The distribution of the main measure, the difference in the natural log number of visits in 2020 and 2019, looks clustered around zero for the week of March 1, 2020 (mean=0.35, SD=0.57) given that we would not expect there to be significant changes in the number of visits to schools prior to declaration of COVID-19 as a public health emergency on March 13, 2020. By the week of May 10, 2020, the distribution is significantly skewed, showing large declines in the difference in the number of visits (mean=-1.48, SD=0.62). By September 27, 2020 the number of visits is again distributed around zero (mean=-0.30, SD=0.62).

Figure 3 maps geographic variations in the school visit measure. There is substantial geographic variation in the county-level visits measures for the three weeks of March 1, May 10, and September 27, 2020. Areas shaded in blue exhibited the largest declines in school visits, which would indicate school closures.

3.2. Medical Claims Data

Our data on COVID-19 related outcomes comes from medical claims data from approximately seven million people collected by Castlight Health, which provides a health benefits and price transparency platform to approximately 200 employer-sponsored insurance plans. For participating employers, the data includes medical and pharmacy claims for all employees and their dependents. A unique feature of the data is that it allows for a linkage of members to households for households with a family insurance plan. Participating employers represent a wide range of industries, including education, transportation, retail, and financial services, and employees are located in all U.S. states. These data have been previously used to track health care utilization during the pandemic (Cantor et al. 2021; McBain et al. 2021; C. M. Whaley et al. 2021), and existing studies have found that this population is representative of the broader U.S. population with employer-sponsored insurance (Cantor et al. 2020; C. M. Whaley et al. 2020).

For each household, we identified any COVID-19 diagnoses in that week using ICD-10 codes U07.1, B34.2, and B97.29. Other studies have found that the sensitivity and specificity of these administrative diagnosis codes for COVID-19 is 98.0 percent and 99.0 percent, respectively (Kadri et al. 2020). An important limitation of using clinical diagnoses for COVID-

19 is that we potentially missed asymptomatic cases or infections that do not require interactions with health care providers. Thus, our results should be interpreted as the effect of county-level school visits on moderate-to-severe COVID-19 infections.

Table 1 describes characteristics of households with (N=84,494, 2.8 percent of sample) and without (N= 2,892,046, 97.2 percent of sample) a COVID-19 diagnosis. For each characteristic, we report the standardized difference between the two household types (Rosenbaum and Rubin 1985; Austin 2009). The geographic and industry composition of households with and without a COVID-19 diagnosis are relatively similar, with most standardized differences below a 0.1 standard deviation difference. Households with a COVID-19 diagnosis are more likely to have a child in the household, have a mean of 0.7 additional household members, and are likely to live in areas with lower overall changes in visits to schools. Households with a COVID-19 diagnosis incur a mean $2,320 in COVID-19 related medical costs, with a standard deviation that is approximately an order-of-magnitude larger than mean spending. This variance is due to the wide variation of patients impacted by the COVID-19 pandemic. Some patients can be treated in a low-intensity outpatient setting, while a small number of patients require hospitalization, and in some cases ventilatory support and critical care.

Due to a lag period of when a medical event occurs and the completeness of the medical claims data, we used data through the 46th week of 2020, ending on November 15, 2020. The claims data were pulled in March 2021.

3.3. COVID-19 Social Distancing Policies and Exposure Data

We used data on county-level shelter-in-place policies that was collected by Cook et al. (2020). First, the authors collected the dates for statewide orders from the New York Times (The New York Times 2020). Second, for states that lacked a state-wide order, the authors determined whether an individual county had a shelter-in-place policy by searching local news and government sites. Finally, the counties for which the authors did not find information on a shelter-in-place policy were assumed to have followed the state's guidance (Cook, Newberger, and Smalling 2020).

However, given that many of these policies were enacted in response to COVID-19 cases and deaths, we also collected data on COVID-19 incidence rates. The data come from USAFacts and have been frequently used by researchers (Cantor et al. 2020; Adhikari et al. 2020; Brown and Ravallion 2020). The USAFacts data come from the CDC, which collects the number of cases and deaths from state public health websites. The USAFacts data has the total number of cases and deaths in each county for each day. From these data, we calculated the week of the first COVID-19 case and of the first COVID-19 death for each county.

4. EMPIRICAL APPROACH

With these data, we estimate the effect of school reopenings using within-county variation in household structure. We estimate the effects of changes in school visits on COVID-19 outcomes using a triple-differences model of the form:

$$\begin{gathered} covid_{igt} = \alpha + \gamma_1 child_i + \gamma_2 \Delta school_{gt-2} + \delta child_i \times \Delta school_{gt-2} \\ + \tau \boldsymbol{week_t} + \psi \boldsymbol{county_g} + \varepsilon_{igt} \end{gathered} \tag{3}$$

In this model, $covid_{igt}$ represents the dichotomous COVID-19 related outcome of interest—a COVID-19 diagnosis for household i in county g during week t. The $child_i$ term indicates that the household has a school-age child. The $\Delta school_{gt-2}$ represents the week and county-specific measure of visits to schools from the SafeGraph data. Given the incubation period of the SARS-

CoV-2 virus, we use a two-week lag between changes in the number of visits to schools and household-level outcomes. As a sensitivity test, we also use a one-week lag and find similar results (Appendix Table A2).

Our primary coefficient of interest, δ, captures the differential change in COVID-19 outcomes between households with and without children following changes in county-level visits to school. In all specifications, we include fixed effects for week and county. Thus, the δ coefficient measures the change in COVID-19 infections between households with and without children in the same county, and relative to all other households in that week. Due to these fixed effects and because our school visits measures are county and week specific, we do not include main post-reopening or treatment county indicators. We estimate all models using ordinary least squares and cluster standard errors at the county level.

Across all specifications, the δ coefficient measures the intent-to-treat effect of changes in county-level school visits on COVID-19 diagnoses and other COVID-19 related outcomes. We are unfortunately unable to link data on household-specific school visits and infections, which would allow us to fully estimate the first-stage effect that would allow for a local-average treatment effect calculation of school visits on COVID-19 cases. We are also unable to account for differences in school-specific policies designed to limit infection spread (e.g., mask and social distancing stringency), which are important contributors to mitigating the spread of COVID-19 (Falk et al. 2021; Lessler et al. 2021; Dawson et al. 2021; Volpp et al. 2021). Instead, this estimate captures both the direct effect of school-related COVID-19 transmission (e.g., children becoming infected at school) and the potential indirect effect of risk avoidance by households and schools.

Our identification strategy compares trends in COVID-19 diagnoses for household with and without children, which we measure using household insurance enrollment structures. To adjust for differences based on household composition, we control for the number of people enrolled on a household's insurance plan. As a sensitivity test, we also estimate a specification that includes a household fixed effect to control for time invariant differences in COVID-19 risk across households. A potential challenge with this approach is that some households with private insurance may also have children covered by Medicaid or other forms of public insurance. For such households, the household will be misclassified as not being exposed to school visits. In the Appendix, we provide additional support that these cases are likely rare and do not appear to be driving our main results. As an additional sensitivity test, we decompose the triple-differences estimate into separate difference-in-differences regressions for households with and without children. As discussed in the Appendix, doing so allows us to assess the validity of our triple-differences approach, and test for potential contamination of the control group, which will lead to a downward bias on our estimates. In the separate difference-in-differences regressions, we find much larger changes in COVID-19 diagnoses for households classified as having child household members than without child members.

A key empirical challenge is the potential endogeneity of changes in school visits. Schools may be less likely to reopen in areas with high COVID-19 prevalence as measured by positivity rates or where contact tracing is difficult (Vogel and Couzin-Frankel 2020). To address this concern, we perform several additional tests. First, we control for the introduction of county-level shelter-in-place policies. These policies are designed to slow COVID-19 spread, and may impact the rate of infection in a county (Courtemanche et al. 2020; Berry et al. 2021). Second, we follow the approach outlined in previous work and non-parametrically control for the variations in the trajectory of the COVID-19 pandemic (Cantor et al. 2020). For each county and week, we include separate fixed effects for the number of weeks since the first COVID-19 case and death in that county, under the assumption that counties with earlier exposure are at higher risk of transmission.

Third, we control for visits to other locations—bars and restaurants- associated with COVID-19 risk. We construct similar measures of number of visits to bar and restaurant locations in the Safegraph data. Social gatherings at bars and restaurants are an important potential source of COVID-19 infection transmission (Pray et al. 2021). Fourth, we construct measures of weekly changes in visits to all locations in a county, excluding schools. We define these as any location without the NAICS code 611110. We believe this is an accurate measure of total movement that is occurring within the county for a given week.

Fifth, we add fixed effects for county-by-week interactions, which account for time-varying trends at the county level. This specification absorbs any broader changes in policies, COVID-19 spread, or physical mobility. This measure also absorbs our main effect of school-related visits. Within a given county, it isolates the differential impact of changes in school-related visits between households with and without children. Finally, we use a similar approach and interact fixed effects for the industry of the primary insurance subscriber's employer with week fixed effects. These industry-by-week fixed effects control for industry-specific shocks that lead to differences in COVID-19 transmission, such as ability to work remotely.[4] Each of these additional controls address potential confounders that are related to both county-level school-related visits and COVID-19 diagnoses.

[4] For example, the trajectory of work-related infection risks is likely higher for retail workers than it is for office workers.

As discussed below, we also estimate several additional tests that examine differences within counties and between different types of households. We estimate models that non-linearly test the effects of changes in county-level school visits on COVID-19 infections. We also test for differences based on COVID-19 prevalence in a county. We separately estimate event studies that test for trends in COVID-19 infections based on county-level school visits.

5. RESULTS

5.1. Effect of School Visits on COVID-19 Diagnoses

Table 2 presents our main results that measure the effect of visits to schools on COVID-19 cases. Panel A uses the modified Goolsebee and Syverson (2020) log-transformed measure as our main measure of school visits. To allow for an easier comparison with epidemiological studies of the COVID-19 pandemic, we express probabilities in per 10,000 units. In column 1, which is the most parsimonious model and does not include the full set of controls, we estimate that a one-log unit increase in visits to schools leads to a 0.4 per 10,000 household increase in the weekly likelihood of COVID-19 infection for households with children relative to households without children. The two main effects of being a household with a child and an increase in county-level school visits are both positive, and at 3.6 and 2.2 per 10,000, respectively, much larger than the interaction term of interest. The main effect coefficient of school reopenings is challenging to interpret causally, as school reopenings are endogenously related to the trajectory of the COVID-19 pandemic.

In column 2, we control for the number of people in the household. Because this variable is correlated with the child indicator variable, doing so changes the sign of the child indicator variable, but the magnitudes of the interaction term is similar, 0.4 per 10,000. In column 3, we add household fixed effects, which is our most restrictive specification. When accounting for all household-level variation, the magnitude of the school visits-child interaction decreases to 0.2 per 10,000 but remains statistically significant.

In columns 4 to 9, we perform additional robustness tests and add additional controls to address potential confounders for visits to schools. In column 4 we add an indicator for county-level shelter-in-place policies and also find a 0.4 per 10,000 increase. Column 5 adds fixed effects for the county-level weeks since the first COVID-19 case and death, which does not change the school visits-child interaction. The result is similar when adding controls for non-school visits (column 6), bar and restaurant visits (column 7), county-week fixed effect interactions (column 8), and industry-week fixed effect interactions (column 9).

Across the different specifications, the household indicator and school visits interaction coefficients are stable, suggesting that the potential threats to validity may be limited. While these results show a positive increase in COVID-19 cases, the relative magnitude is limited. Using our main result of 0.4 per 10,000 and relative to the weekly median household diagnosis rate between the 10th and 46th weeks of 2020 of 8.3 cases per 10,000 households, the increase in household diagnoses from Panel A translates to a 4.8 percent relative increase. During the same period, the median difference between the 75th and 25th percentiles of school visits is 0.67 log units. Thus, a move from the 25th percentile to the 75th percentile of school reopenings is associated with a 3.2 percent increase in COVID-19 cases.

6. ROBUSTNESS TESTS

6.1. Differences Between Children and Adults

Our main results pool children and adults, but the impacts of county-level school visits on COVID-19 outcomes may differ between children and adults. Children may be more susceptible to school-related COVID-19 transmission, but are more likely to be asymptomatic carriers (Vermund and Pitzer 2020). Adults may be less likely to receive school-related infections, but may also be vulnerable to an infection due to school reopenings (Vlachos, Hertegård, and B Svaleryd 2021). Thus, as an additional test, we estimate separate models that examine age-specific diagnoses. We test for change in diagnoses among all children (ages 0 to 18) and all adults (ages 19 to 64) and among specific age groups (ages 0 to 4, 5 to 18, 19 to 29, 30 to 39, 40 to 49, 50 to 59, and 60 to 64).

As shown in Table 3, we find slightly larger increases in pediatric diagnoses. A one-unit increase in visits to schools leads to a 0.3 per 10,000 increase in the likelihood of a child COVID-19 diagnosis. We find no change in COVID-19 diagnoses among children under age 5, and the entirety of the effect is concentrated among children ages to 5 to 18. For adult diagnoses, we find an overall increase of 0.2 per 10,000. However, the increase is concentrated among adults ages 19 to 29 and 30 to 39 (approximately 0.1 per 10,000 increase for both).

These results are consistent with school reopenings leading to both between household transmission to children, but also within household transmission from children to adults. However, the direct effect of between household transmission (e.g., children infecting other children) is approximately three times larger than the indirect effect of within-household transmission (e.g., children infecting adult household members).

6.2. Differences Based on Intensity of COVID-19 Pandemic

School-based COVID-19 transmission is likely to have a larger effect in areas with higher underlying rates of COVID-19 transmission. As a test of this assumption, we add the weekly number of new COVID-19 cases per 10,000 population, $cases_{gt}$, to our regression model and interact COVID-19 prevalence with our DDD treatment effect in a quadruple-differences model:

$$\begin{gathered} covid_{igt} = \alpha + \gamma_1 child_i + \gamma_2 \Delta school_{gt-2} + \gamma_3 cases_{gt} + \gamma_4 cases_{gt} \times \Delta school_{gt-2} \\ + \gamma cases_{gt} \times child_i + \delta_1 child_i \times \Delta school_{gt-2} + \delta_2 cases_{gt} \times child_i \times \Delta school_{gt-2} \\ + \tau \boldsymbol{week_t} + \psi \boldsymbol{county_g} + \varepsilon_{igt} \end{gathered} \tag{4}$$

As shown in Table 4, we find that, perhaps unsurprisingly, county-level increases in new COVID-19 cases leads to increases in household COVID-19 cases, and these increases are higher for households with children than households without children. When interacting all measures, we find that the one-unit increase in mobility to schools and a 1-person per 10,000 increase in new county-level cases leads to a 0.2 per 10,000 household increase in household-level COVID-19 diagnoses for households with children, relative to households without children. The primary $\delta_1 child_i \times \Delta school_{gt-2}$ interaction is small and is not statistically significant, suggesting that our overall effect is driven by variations in COVID-19 prevalence.

6.3. Differences Across the COVID-19 Pandemic

As a similar test, we next examine differential impacts across the evolution of the COVID-19 pandemic. Changes in visits to schools during the early phases of the pandemic, when baseline infection rates are low, are less likely to contribute to transmission than changes in school visits when infection rates are high. Risk-mitigation approaches have been developed and changed over the course of the pandemic (Centers for Disease Control and Prevention 2021), and so the net effect of increased exposure to the pandemic on school-based transmission is unclear.

To capture these changes, we separately estimated equation (3) for each week of 2020, through week 46. This test allows us to examine temporal trends in the triple-differences treatment effect and if the difference in COVID-19 infection rates between households with and without children is different between early weeks of the pandemic and later weeks.

As shown in Figure 3, we find that most of the differences in household transmission occur in later weeks. In the first portion of 2020, we find that changes in county-level visits to schools do not lead to changes in household infection. This null finding is consistent with the previous sensitivity test that found increased transmission rates in counties with higher COVID-19 prevalence. We find that the DDD coefficients are driven by changes that occur starting in week 40, which corresponds to the last week of September. The number of school visits is measured with a one-week lag, and so school-based visits beginning in mid-September 2020 drives our main results. By the 46^{th} week of 2020, which corresponds to the second week of November, the estimated DDD implies that a one-unit increase in log school visits leads to an 8.5 increase per 10,000 households in COVID-19 transmission for households with children, relative to those without. This effect is more than an order-of-magnitude larger than our main results in Table 2.

This result, paired with the results in Table 4, suggests that the impact of school-based transmission interacts with the intensity of the COVID-19 pandemic. Not surprisingly, when overall COVID-19 risks are low, the risks of school-based transmission are also low. When community cases are high, schools are another mode of transmission, and higher levels of community spread enable higher levels of school-based spread.

6.4. Differences for Households with Education Workers

Additionally, an important consideration is the potential differential impact among teachers, administrators, and other school workers. To test for differences among education workers, we identify households in which the primary insurance subscriber works within the education sector. We are unable to account for households in which an education worker receives insurance coverage from a non-education family member. We are also unable to identify education worker job type, and changes in mobility for the worker.

We estimate a similar model to equation (3) but interact county-level school visits with an indicator for the household receiving insurance coverage from an organization in the education sector. As shown in Table 5, we do not find that households in which the primary insurance subscriber is employed by an educational organization are disproportionally likely to have a COVID-19 diagnosis after school reopening.

6.5. Differences by County-Level Income Composition

A substantial literature documents race and income disparities in the health and non-health impacts of the COVID-19 pandemic (Ogedegbe et al. 2020; Jay et al. 2020; Khazanchi, Evans, and Marcelin 2020; Moore 2020; Lee et al. 2021). The effects of school reopenings on COVID transmission may also show similar disparities. To test for disparities, we also examined differences based on the income composition of the county in which patients live. We lack information on household-level income. For income, we categorize county-level household income into quartiles using the pooled 2013 to 2018 American Community Survey (ACS).

As shown in Table 6, we find that the $child_i \times \Delta school_{gt-2}$ coefficient for households in the bottom quartile of the income distribution is 1.3 per 10,000 households, compared to 0.7 per

10,000 households for the second quartile, 0.5 per 10,000 households among the third quartile, and a non-statistically significant 0.2 per 10,000 among the top quartile. When interacting the income quartiles with the $child_i \times \Delta school_{gt-2}$ coefficient, similar to equation (4), the effects of school visits are statistically different at the p=0.05 level between the first and second quartiles, and at the p<0.01 level between the first quartile and both the third and fourth quartiles.

7. IMPACTS ON HEALTH CARE SPENDING AND COVID-19 HOSPITALIZATIONS

Our primary results use COVID-19 diagnoses as the outcome of interest. A potential challenge for using diagnoses as the outcome is the possibility that school reopening policies include testing strategies to mitigate COVID-19 spread. Such testing is likely to catch asymptomatic COVID-19 cases that would have remained undiagnosed in the absence of increased testing. In such a case, it is challenging to test if our results reflect increased COVID-19 transmission or simply more accurate diagnoses. As a solution to this challenge, we use COVID-19-related medical spending and hospitalizations as an alternative dependent variable. Measuring impacts on spending allows an approximation of the health care costs of school reopenings. While there are some estimates of COVID-19 spending, they are mainly focused on total nationwide aggregates (Miller et al. 2020), or at the state-level (McWilliams, Russo, and Mehrotra 2021). We follow the same empirical approach outlined in equation (3), but measure household COVID-19 related health care costs by summing costs for all medical claims with a COVID-19 diagnosis or procedure code. We include all COVID-related spending, including spending by patients and spending by the employer/insurer. To account for skewness, we also estimate models that use log-transformed spending.[5] Likewise, hospitalizations are unlikely to

[5] We use $\ln(spending_{it} + 1)$ for household i in week t.

reflect testing strategies, as COVID-19 cases that require hospitalization are unlikely to be induced by increasing testing.

As shown in Table 7, we do not find that school reopenings lead to changes in COVID-19 related medical spending, when measured in raw dollars (Panel A). When measured in log-dollars, we find a small increase in spending. A full log-unit change in school visits leads to a 0.01 percent increase in household COVID-19 spending. While statistically significant in all specifications, the effect is small in magnitude. Thus, our results suggest that school reopenings are unlikely to lead to meaningful changes in COVID-19-related health care costs.

As shown in Table 8, we find that increased number of school-based visits leads to higher rates of COVID-19 hospitalizations among households with children, relative to households without children. The estimated DDD coefficient is approximately 0.03 per 10,000 households. Based on the mean COVID-related hospitalization rate of 0.8 per 10,000, this effect translates to an approximately 4% relative increase. These results imply that a move from the 25th to the 75th percentile of visits to schools leads to a 2.7% increase in COVID-related hospitalizations.

For both outcomes, we also estimate event studies that examine how the DDD estimates have changed over the COVID-19 pandemic. In Figure A1, we do not find a change in COVID-19 related medical spending. However, in Figure A2, we find that log-transformed COVID-19 medical spending increased in following week 40. In Appendix Figure A3, consistent with the small magnitude of the hospitalization regression results, we find small effects in each week. In most weeks, the confidence intervals overlap zero.

8. IMPACT OF SOCIAL DISTANCING POLICIES ON SCHOOL VISITS

We finally examine how changes in the number of school visits has responded to policies designed to limit social interactions. Understanding the behavioral responses to these policies allows for a “first stage” estimate that shows the size of the change in the number of weekly visits to schools after a shelter-in-place policy went into effect. To do so, we use the SafeGraph data and estimate the effect of social distancing policies on school visits using a two-way fixed effect model of the form:

$$\ln\left(visits_{jgt}\right) = \alpha + \delta policy_{gt} + \tau week_t + \tau year_t + \psi school_i + \varepsilon_{jgt} \qquad (6)$$

This regression measures the change in log-transformed mobility to school j in week t before and after the implementation of a social distancing policy in county g. We include fixed effects for week and for the 131,000 schools identified in the SafeGraph data based on their NAICS code. Unlike the other analyses, where we use just 2020 data and measure week-level changes in 2020 school visits relative to the same week in 2019, we use school visit data for both 2019 and 2020. We include a fixed effect for 2020, which allows us to estimate the changes in within-school visits between 2019 and 2020 for each calendar week.

As shown in Table 9, the introduction of county-level social distancing policies leads to a 0.28 log-unit change in school visits. This estimate is unchanged when including fixed effects for year-by-week interactions to control for week-specific changes in school visits and state-by-week fixed effects to control for temporal changes within states (e.g., state-level exposure to the COVID-19 pandemic). Tying these results to our earlier results suggests that the level of school visits induced by the introduction of social distancing policies leads to a 0.08 per 10,000 (0.0008 percentage point) decrease in the risk of household-level COVID-19 infection. Based on the baseline mean of nine COVID-19 diagnoses per 10,000 households, our results imply that county-level social distancing policies led to a 1.3 percent decrease in COVID-19 infections.

9. DISCUSSION

The COVID-19 pandemic has upended nearly all aspects of life. In response to concerns of COVID-19 infection, many schools have moved to remote instruction (Malkus and Christensen 2020). Remote instruction can lead to reductions in COVID-19 cases, but the effectiveness is not well quantified or understood (Christakis, Cleve, and Zimmerman 2020). Remote instruction is also likely to impose disproportionate costs on socioeconomically disadvantaged families (Kaufman et al. 2021). These families in particular may have less access to important resources that include high-speed Internet access, computers, and job flexibility (Kroshus et al. 2020; Reeves, Kneebone, and Rodrigue 2016). Remote instruction also imposes costs to parents, many of whom are also balancing remote work and childcaring responsibilities (Adams and Todd 2020; Amuedo-Dorantes et al. 2020), and preliminary results indicate that it leads to an increase in stress for parents (Verlenden et al. 2021).

Despite the large costs of school closure, the impacts of school reopenings on COVID-19 cases has not been well estimated. In this study, we leverage variations in household composition and apply novel data on both COVID-19 diagnoses and county-level visits to schools to estimate the effects of school reopenings on COVID-19 transmission. We find that a one-log unit change in visits to schools leads to an approximately 0.4 per 10,000 increase in COVID-19 cases. Our results further imply that moving from the 25th percentile of school reopenings to the 75th percentile translates to an approximately 3 percent increase in cases. These results are robust to several specifications. In addition, we find that the modest increase in cases impacts both children and adults, does not differentially impact households with education-industry workers, and has little impact on medical spending. However, the effect is largest for households in lower-income counties and is larger during the peak of the COVID-19 pandemic.

This study is not without limitations. For one, while broadly representative of the U.S. population with private insurance, we use a subset of the U.S. population. Importantly, our sample only includes those with employer-sponsored private insurance, and thus we do not include more economically vulnerable populations, such as uninsured individuals or those on public insurance, of which the pandemic has a disproportionately larger impact. We believe our estimates are a likely underestimate of the effect of school reopenings on more vulnerable populations. Likewise, we do not include individuals ages 65 and older, who are at increased risk to COVID-19 (Viner, Bonell, et al. 2021). Our models do not account for changes in the education labor force that may impact the number of visits to schools. Many educators have quit due to stresses of the COVID-19 pandemic (Diliberti, Schwartz, and Grant 2021). Finally, we do not include measures of school-level mitigation measures that have been shown to impact COVID-19 transmission (Falk et al. 2021; Lessler et al. 2021; Volpp et al. 2021; Dawson et al. 2021). This is a common challenge in the literature given there is no national database collecting data on schools (Oster 2021; Goldhaber et al. 2021). Only recently has the Department of Education announced a campaign to collect detailed survey data on schools during the pandemic (United States Department of Education 2021). In addition, recommendations for mitigation strategies have changed over the course of the pandemic given new research. For example, recommendations on minimum student distancing may be changing from six feet to three feet (van den Berg et al. 2021).

Despite these limitations, this study demonstrates that among the population we study, school reopenings lead to a small increase in COVID-19 cases. The key question for policymakers is the tradeoff between the costs of school reopenings and the costs of remote learning. It is important to place these results in context with other risks of daily life and social distancing. Recent estimates using similar physical mobility data from Unacast data have found that living in communities with the greatest social distancing leads to a 31 percent reduction in COVID-19 cases (Kwon et al. 2020). Research using the same data as this paper finds that household birthdays, which likely lead to social gatherings, lead to an approximately 30 percent increase in COVID-19 transmission (C. Whaley et al. 2021). In non-COVID contexts, other studies find that injuries received at school account for 21 percent of non-intentional emergency department visits and approximately 14 children per 10,000 receive a school injury per year (Zagel et al. 2019). Spread across 40 weeks, this risk rate translates to an approximately 0.4 per 10,000 child risk rate, which is comparable in magnitude to our main estimate of 0.4 per 10,000 households. The effects of school reopening on COVID-19 risk were estimated when COVID-19 vaccines were not available to teachers and the general population. With increased vaccine uptake the risk of school reopening on COVID-19 risk may be further diminished.

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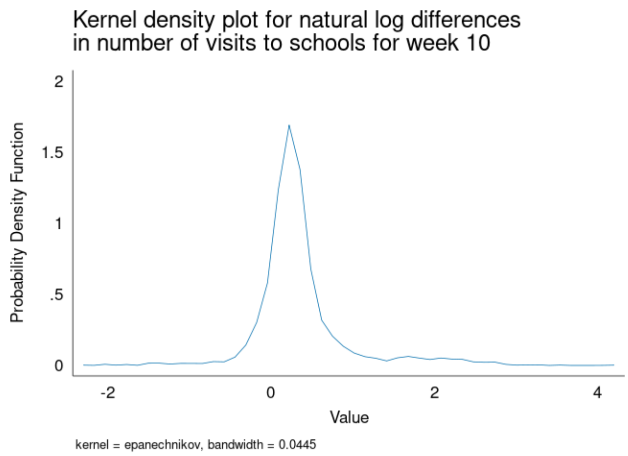
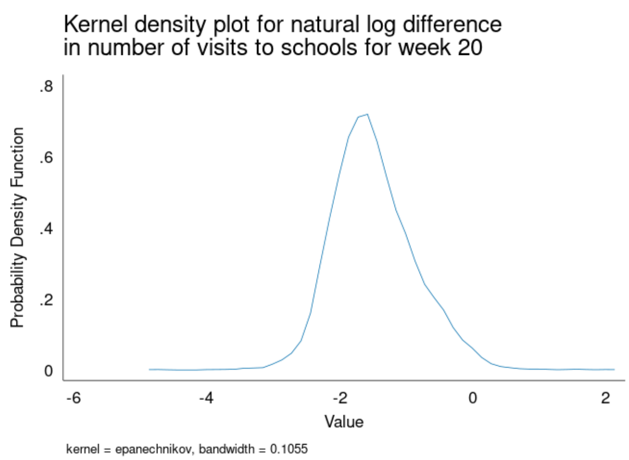
11. TABLES AND FIGURES/
Figure 1: Distribution of Log-Transformed School Visit Measures

(A) Week 10



(B) Week 20



(C) Week 40

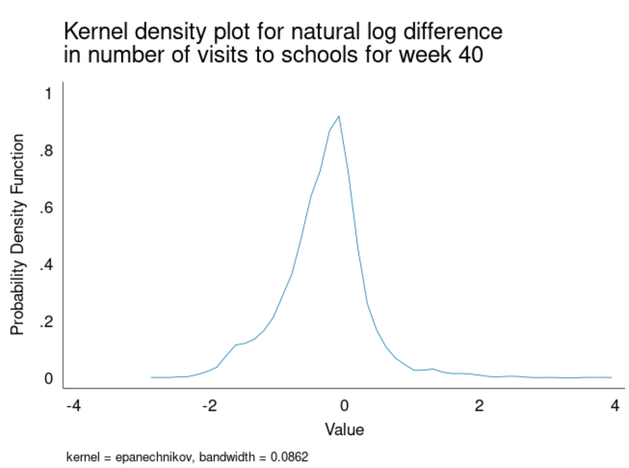
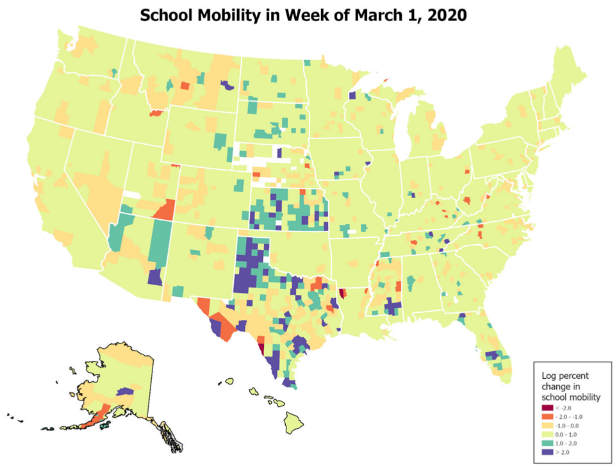


Figure 2: Geographic Distribution of Log-Transformed School Visits

(A) Week 10



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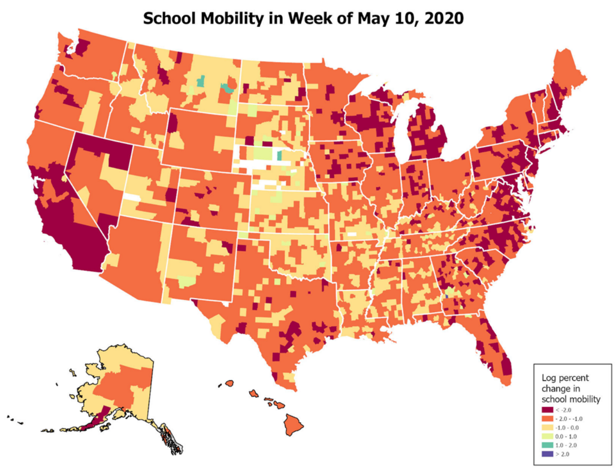

(C) Week 40

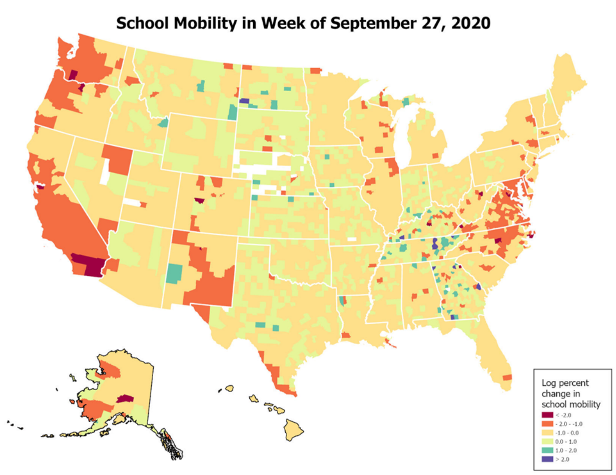

Table 1: Descriptive Characteristics of Study Population

	Households without COVID Diagnosis		Households with COVID Diagnosis		
	Mean	SD	Mean	SD	Standardized difference
Child in household (%)	28.5%	45.1%	37.7%	48.5%	-0.20
Number of household members	2.3	1.5	3.0	1.7	-0.44
COVID-related medical spending ($)	$0	$0	$2,320	$21,901	-0.15
log-transformed school visits change	-0.54	0.48	-0.45	0.48	-0.19
Census region (%)					
Pacific	19.5%	39.6%	20.0%	40.0%	-0.01
Mountain	5.6%	23.0%	6.5%	24.6%	-0.04
West north central	5.8%	23.4%	5.1%	22.0%	0.03
West south central	3.2%	17.5%	2.8%	16.6%	0.02
East north central	20.3%	40.2%	10.2%	30.3%	0.28
East south central	29.6%	45.6%	33.1%	47.1%	-0.08
New England	6.2%	24.1%	8.5%	27.9%	-0.09
South Atlantic	9.9%	29.8%	13.8%	34.5%	-0.12
Industry of primary subscriber (%)					
Aerospace & Defense	10.9%	31.2%	7.0%	25.6%	0.14
Apparel	0.1%	3.4%	0.0%	0.0%	0.05
Automotive	4.2%	20.1%	5.6%	23.0%	-0.06
Chemicals	0.2%	4.7%	0.3%	5.7%	-0.02
Construction	0.6%	8.0%	1.0%	9.9%	-0.04
Education	2.4%	15.2%	1.9%	13.8%	0.03
Electronics	1.2%	11.1%	1.3%	11.3%	0.00
Engineering	0.3%	5.2%	0.3%	5.7%	-0.01
Entertainment & Hospitality	0.4%	6.2%	0.4%	6.5%	-0.01
Financial Services	8.3%	27.6%	10.3%	30.4%	-0.07
Food & Beverage	4.2%	20.1%	6.3%	24.3%	-0.09
Government	9.7%	29.6%	14.7%	35.4%	-0.15
Grocery	3.0%	17.1%	3.1%	17.3%	0.00
Hospitals & Healthcare	5.3%	22.4%	5.9%	23.5%	-0.02
Insurance	4.5%	20.7%	6.3%	24.2%	-0.08
Legal Services	0.0%	2.0%	0.0%	2.2%	0.00
Manufacturing	6.8%	25.1%	8.6%	28.1%	-0.07
Media	0.0%	2.1%	0.0%	1.8%	0.01
Medical Devices	0.6%	7.4%	0.6%	7.7%	-0.01
Missing	0.4%	6.6%	0.3%	5.6%	0.02
Non-Profit	0.2%	4.8%	0.3%	5.5%	-0.01
Oil, Energy, & Utilities	2.3%	15.1%	3.7%	19.0%	-0.08
Other	0.4%	6.0%	0.4%	6.3%	0.00
Pharmaceuticals & Biotech	0.8%	8.8%	1.1%	10.3%	-0.03
Professional Services	1.5%	12.0%	1.3%	11.5%	0.01
Real Estate	0.1%	2.8%	0.1%	3.3%	-0.01
Retail	16.1%	36.7%	3.9%	19.4%	0.41
Security	0.4%	6.3%	0.4%	6.5%	0.00
Semiconductors	1.1%	10.4%	1.0%	10.1%	0.00
Software & Technology	0.8%	9.0%	0.6%	7.9%	0.02
Telecommunications	11.6%	32.0%	11.7%	32.1%	0.00
Transportation	1.5%	12.3%	1.2%	11.0%	0.03

Table 2: DDD Estimates on the Effect of School Visits on Household COVID-19 Diagnoses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log-transformed Visits									
Child in household X school visits	0.395***	0.390***	0.166**	0.389***	0.401***	0.390***	0.387***	0.369***	0.486***
	(0.0836)	(0.0835)	(0.0675)	(0.0834)	(0.0853)	(0.0840)	(0.0852)	(0.0863)	(0.0816)
Child in household	3.589***	-2.546***		-2.546***	-2.536***	-2.545***	-2.548***	-2.563***	-2.281***
	(0.190)	(0.150)		(0.150)	(0.151)	(0.150)	(0.151)	(0.148)	(0.155)
School visits	2.218***	2.219***	2.291***	2.227***	1.009***	2.242***	2.270***		2.526***
	(0.360)	(0.360)	(0.371)	(0.360)	(0.221)	(0.308)	(0.300)		(0.344)
Number of household members		2.853***		2.853***	2.853***	2.853***	2.853***	2.853***	2.645***
		(0.1000)		(0.1000)	(0.1000)	(0.1000)	(0.1000)	(0.1000)	(0.0884)
SIP policy				2.039***					
				(0.454)					
Bar mobility						-0.848***			
						(0.296)			
Restaurant mobility						0.731			
						(0.981)			
Non-school mobility							-0.504		
							(1.286)		
Observations	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976
HH FE			X						
SIP policy				X					
Weeks since first COVID case/death FE					X				
Bar/restaurant mobility						X			
Non-school mobility							X		
County-week FE								X	
Industry-week FE									X

This table presents regression results from equation 3 and measures effect of county-level school visits on the weekly change in household COVID-19 diagnoses between household with and without children. Standard errors clustered at the county-level in parentheses. Coefficients and standard errors are multiplied by 10,000 and can be interpreted as the change in weekly diagnosis rates per 10,000 households. ** p<0.01, ** p<0.05, * p<0.1.

Table 3: DDD Estimates on Effect of School Visits on Age-Specific Household COVID-19 Diagnoses

	Any child	Age 0-4	Age 5-18	Any adult	Age 19-29	Age 30-39	Age 40-49	Age 50-59	Age 60+
Log-transformed Visits									
Child in household X school visits	0.269***	0.00567	0.267***	0.172***	0.0905***	0.108***	0.00921	-0.0636**	0.0295
	(0.0499)	(0.0140)	(0.0519)	(0.0596)	(0.0203)	(0.0363)	(0.0519)	(0.0258)	(0.0222)
Observations	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976

This table presents results that measure the effect of county-level school visits on age-specific COVID-19 diagnoses between household with and without children. Standard errors clustered at the county-level in parentheses. Coefficients and standard errors are multiplied by 10,000 and can be interpreted as the change in weekly diagnosis rates per 10,000 households. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Differences Based on County-Level COVID-19 Prevalence

	(1)
	Log-transformed school visits
Child in household X school visits	-0.0377
	(0.0924)
Child in household	-5.851***
	(0.238)
School visits	-0.144
	(0.261)
Number of household members	3.047***
	(0.107)
New weekly cases per 10,000	0.614***
	(0.0410)
New weekly cases per 10,000 X school visits	0.196***
	(0.0271)
Child in household X new weekly cases per 10,000	0.562***
	(0.0378)
Child in household X new weekly cases per 10,000 X school visits	**0.192*** **
	(0.0283)
Observations	127,601,976

This table presents regression results from equation 4 and measures how the effect of county-level school visits on the weekly change in household COVID-19 diagnoses between household with and without children varies based on the prevalence of COVID-19 in that county. Standard errors clustered at the county-level in parentheses. Coefficients and standard errors are multiplied by 10,000 and can be interpreted as the change in weekly diagnosis rates per 10,000 households. *** p<0.01, ** p<0.05, * p<0.1.

Figure 3: DDD Estimates of the Impact of School Visits on COVID-19 Infection Rates by Week

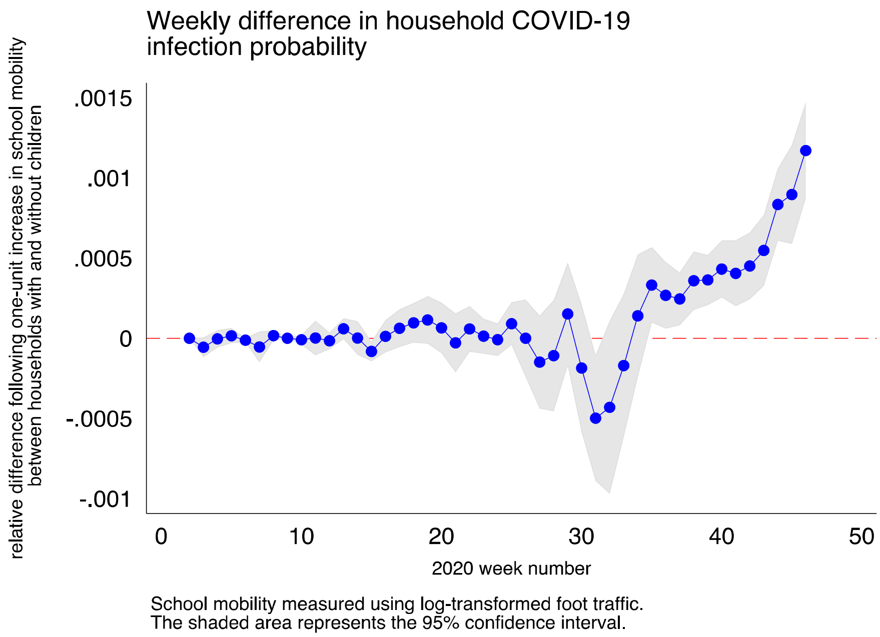

Table 5: DD Estimates of COVID-19 Infection Rates Among Households with Education Workers

	(1) Log-transformed school visits
School visits	2.343***
	(0.373)
Education industry	0.0626
	(0.529)
Education industry X school visits	-0.185
	(0.305)
Observations	127,601,976

This table presents results that measure the effect of county-level school visits on household-level COVID-19 diagnoses, and how the relationship varies among households in which the primary insurance subscriber is in the education industry. Standard errors clustered at the county-level in parentheses. Coefficients and standard errors are multiplied by 10,000 and can be interpreted as the change in weekly diagnosis rates per 10,000 households. *** p<0.01, ** p<0.05, * p<0.1

Table 6: DDD Estimates of the Impact of School Visits on COVID-19 Infection Rates by County Income Quartile

	(1)	(2)	(3)	(4)
Income quartile	1st	2nd	3rd	4th
Child in household X school visits	1.269***	0.669***	0.462***	0.181*
	(0.303)	(0.175)	(0.164)	(0.0999)
Observations	7,217,722	16,199,682	24,960,658	79,223,270
p-value of difference relative to first quartile	-	0.059	0.011	<0.01

This table presents results that measure the effect of county-level school visits on household-level COVID-19 diagnoses between household with and without children. Separate models test for differences in the first quartile (column 1), second quartile (column 2), third quartile (column 3), and fourth quartile (column 4) of county income. County income is measured using data from the American Community Survey. Standard errors clustered at the county-level in parentheses. Coefficients and standard errors are multiplied by 10,000 and can be interpreted as the change in weekly diagnosis rates per 10,000 households. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Effect of School Visits on Household COVID-19 Medical Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: COVID medical spending (Log-transformed visits)									
Child in household X school mobility	-0.0867	-0.0880	0.0117	-0.0884	-0.0833	-0.0847	-0.0864	-0.112	-0.0627
	(0.129)	(0.129)	(0.127)	(0.129)	(0.129)	(0.129)	(0.129)	(0.134)	(0.129)
Panel B: Ln(COVID spending +1) (Log-transformed visits)									
Child in household X school mobility	0.000125***	0.000131***	0.000137***	0.000130***	0.000129***	0.000157***	0.000149***	0.000112***	0.000138***
	(3.89e-05)	(3.88e-05)	(3.59e-05)	(3.88e-05)	(3.94e-05)	(4.08e-05)	(4.03e-05)	(3.96e-05)	(3.90e-05)
Observations	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976
Number of HH members		X							
HH FE			X						
SIP policy				X					
Weeks since first COVID case/death FE					X				
Bar/restaurant mobility						X			
Non-school mobility							X		
County-week FE								X	
Industry-week FE									X

This table presents results that measure the effect of county-level school visits on COVID-19 related medical spending between household with and without children. Standard errors clustered at the county-level in parentheses. Panel A uses COVID-19 related medical spending as the dependent variable. Panel B uses log-transformed COVID-19 medical spending as the dependent variable. Coefficients and standard errors are multiplied by 10,000 and can be interpreted as the change in weekly diagnosis rates per 10,000 households. *** p<0.01, ** p<0.05, * p<0.1

Table 8: DDD Estimates on the Effect of School Visits on Household COVID-19 Hospitalizations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Child in household X school visits	3.16e-06**	3.10e-06**	4.81e-06***	3.09e-06**	3.20e-06**	3.16e-06**	3.22e-06**	2.87e-06**	3.60e-06**
	(1.40e-06)	(1.40e-06)	(1.36e-06)	(1.40e-06)	(1.39e-06)	(1.40e-06)	(1.40e-06)	(1.41e-06)	(1.42e-06)
Child in household	1.16e-05***	-4.79e-05***		-4.79e-05***	-4.78e-05***	-4.78e-05***	-4.78e-05***	-4.81e-05***	-4.36e-05***
	(2.23e-06)	(3.32e-06)		(3.32e-06)	(3.30e-06)	(3.31e-06)	(3.31e-06)	(3.34e-06)	(3.02e-06)
School visits	1.64e-05***	1.64e-05***	1.60e-05***	1.65e-05***	5.84e-06*	1.44e-05***	1.42e-05***		3.44e-05***
	(3.89e-06)	(3.89e-06)	(3.93e-06)	(3.88e-06)	(3.51e-06)	(3.58e-06)	(3.65e-06)		(5.00e-06)
Number of household members		2.77e-05***		2.77e-05***	2.77e-05***	2.77e-05***	2.77e-05***	2.77e-05***	2.48e-05***
		(1.55e-06)		(1.55e-06)	(1.55e-06)	(1.55e-06)	(1.55e-06)	(1.55e-06)	(1.34e-06)
SIP policy				2.98e-05***					
				(7.66e-06)					
Bar mobility						-6.63e-06**			
						(3.13e-06)			
Restaurant mobility						2.26e-05**			
						(9.23e-06)			
Non-school mobility							2.17e-05**		
							(1.05e-05)		
Observations	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976	127,601,976
HH FE			X						
SIP policy				X					
Weeks since first COVID case/death FE					X				
Bar/restaurant mobility						X			
Non-school mobility							X		
County-week FE								X	
Industry-week FE									X

This table presents regression results from equation 3 and measures effect of county-level school visits on the weekly change in household COVID-19-related hospitalizations between household with and without children. Standard errors clustered at the county-level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Effect of Social Distancing Policies on School Visits

	(1)	(2)	(3)
Social distancing policy	-0.279***	-0.282***	-0.281***
	(0.0236)	(0.0236)	(0.0237)
year 2020	-0.422***		-0.421***
	(0.0188)		(0.0188)
Observations	11,307,112	11,307,112	11,307,112
R-squared	0.712	0.763	0.727
School FE	X	X	X
Week FE	X	X	X
Year-week FE		X	
State-week FE			X

This table presents regression results from equation 6 and measures the effect of county-level social distancing policies on school visits. Standard errors clustered at the county-level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

12. APPENDIX

12.1. Description and Representativeness of Study Sample

This study used medical and pharmacy claims data from employers who purchased access to the Castlight Health platform, which provides price transparency, wellness, and other health benefits tools. This analysis did not include the digital tools provided by Castlight Health, but instead used the medical claims data that participating employers provide to Castlight as a way to implement the digital tools. This data has been used in several papers by the study team related to the COVID-19 pandemic. For each of approximately 200 self-insured employers that provide access to this tool, the claims data covers all in-network procedures that are reimbursed through insurance. The claims data includes reimbursement amounts, procedure codes, and patient diagnoses. The data also includes demographic (e.g., geographic location, age, gender) and employer information (e.g., industry). We did not have access to individual-level data, but instead data aggregated to the year-month-state-gender-age group category level. The employers range in size (e.g., from a few thousand employees to over 50,000 employees) and industry, including manufacturing, government, and education.

One potential concern is that the population included in this sample may not be representative of the broader U.S. population. To assess differences between our study population and the broader commercially insured population, we used data from the American Community Survey (ACS). 2018 is the most recent year available in the ACS, and so we limited our comparison to 2018. We limited the ACS sample to individuals who receive insurance through an employer or union and are under the age of 65. We applied the nationally representative population weights in the ACS data. As shown in Table A1, the Castlight population is similar in gender, age, and geographic distribution to the ACS population.

Table A1: Comparison Between Castlight and ACS Populations

Study year	Castlight (2018)	ACS (2018)
Number of enrolled persons	5,608,888	162,136,077
Gender, percent female (%)	50.0%	50.0%
Average age	34.3	33.5
Census region		
South	40.6%	35.70%
Midwest	23.0%	22.80%
Northeast	10.4%	18.20%
West	26.0%	23.40%

12.2. Alternative Lag Period

Our main analysis uses a two-week lag period between school visits and household COVID-19 diagnoses. As a sensitivity test, we also a one-week period. As shown in Table A2, this alternative lag period produces similar results to our main analysis. When using a one-week lag period, the estimated effect from a one-unit change in school visits on COVID-19 diagnoses between households with and without children is approximately 0.5 per 10,000 households. This effect is consistent, regardless of the additional controls.

Table A2: DDD Estimates on the Effect of One-Week Lagged School Visits on Household COVID-19 Diagnoses

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log-transformed Visits									
Child in household X school visits	0.492***	0.486***	0.243***	0.485***	0.496***	0.357***	0.476***	0.458***	0.568***
	(0.0836)	(0.0835)	(0.0676)	(0.0833)	(0.0851)	(0.0856)	(0.0852)	(0.0852)	(0.0818)
Child in household	3.763***	-2.508***		-2.509***	-2.499***	-2.666***	-2.517***	-2.533***	-2.252***
	(0.195)	(0.156)		(0.156)	(0.156)	(0.162)	(0.156)	(0.154)	(0.162)
School visits	1.689***	1.691***	1.772***	1.673***	0.505**	1.748***	1.895***		2.188***
	(0.358)	(0.358)	(0.370)	(0.357)	(0.215)	(0.332)	(0.302)		(0.345)
Number of household members		2.916***		2.916***	2.916***	2.894***	2.916***	2.916***	2.705***
		(0.102)		(0.102)	(0.102)	(0.110)	(0.102)	(0.102)	(0.0904)
SIP policy				1.885***					
				(0.448)					
Bar mobility						-0.661*			
						(0.370)			
Restaurant mobility						-1.080			
						(1.118)			
Non-school mobility							-2.281		
							(1.429)		
Observations	124,824,681	124,824,681	124,824,681	124,824,681	124,824,681	124,824,681	124,824,681	124,824,681	124,824,681
HH FE			X						
SIP policy				X					
Weeks since first COVID case/death FE					X				
Bar/restaurant mobility						X			
Non-school mobility							X		
County-week FE								X	
Industry-week FE									X

This table presents regression results from equation 3, but uses a one-week lag, and measures effect of county-level school visits on the weekly change in household COVID-19 diagnoses between household with and without children. Standard errors clustered at the county-level in parentheses. Coefficients and standard errors are multiplied by 10,000 and can be interpreted as the change in weekly diagnosis rates per 10,000 households. *** p<0.01, ** p<0.05, * p<0.1.

12.3. Differences-in-Differences by Household Type

Our main results use a triple-differences approach that compares changes in COVID-19 diagnoses following county-level changes in school visits, between households with and without children. This approach faces two potential threats to validity. First, our linkage of members to households relies on the household insurance purchasing decisions. Purchasing an insurance policy that covers all household members is less expensive than purchasing separate individual policies. However, some households may have members, and in particular, children, who are eligible for public insurance, for example Medicaid or the Children's Health Insurance Program (CHIP). Such households will be lower-income than the mean household, and thus may be particularly exposed to the COVID-19 pandemic. Second, our triple-differences identification assumption is that families with children will be more exposed to school-based visits than households without children. However, households with and without children may differentially interact following changes in school visits. If school-based COVID transmission also impacts households without children, then our results will be underestimated.

To test the potential magnitude of downward bias, we used data from the 2017 Medical Expenditure Panel Survey (MEPS). Overall, about 25% of households that have a member with employer or union sponsored insurance also have a child on CHIP/Medicaid, and there is a clear income gradient. While the Castlight data does not have income data, the employers in the data are likely higher-income than the general MEPS population. In the MEPS data, among households above the median household income, the share of dual employer-CHIP enrollment is about 13%. If 13% of the control group is exposed to school visits, and thus mis-classified due to our data construction, then the downward bias will still be small in absolute magnitude.

As an additional test of this mechanism, we decompose the triple-differences regression into two separate difference-in-differences regressions for households with and without children. As shown in Table A3, we find substantially large increases in COVID-19 diagnoses among households with children than households without children. The regression constants indicate that baseline diagnosis rates are also substantially higher for households with children than households without children. These regressions do not include the control for children in household or number of people in the household. The larger effect among households with children suggests that if school-based transmission spills over to then non-child household control group, the extent of the bias is limited.

Table A3: DD Estimates on the Effect of Lagged School Visits on Household COVID-19 Diagnoses, by Households with and Without Children

	(1) HH without children	(2) HH with children	(3) HH without children	(4) HH with children
Panel A: Log-transformed Visits				
School visits	1.837***	3.368***	1.858***	3.547***
	(0.296)	(0.589)	(0.250)	(0.499)
Non-school mobility			-0.217	-1.820
			(1.093)	(1.896)
Constant	7.273***	11.98***	7.229***	11.65***
	(0.255)	(0.506)	(0.422)	(0.777)
Observations	91,251,074	36,350,902	91,251,074	36,350,902

This table presents regression results that measure the effect of county-level school visits on the weekly change in household COVID-19 diagnoses. Columns 1 and 3 present results for households without children and columns 2 and 4 present results for households with children. Columns 3 and 4 control for all non-school mobility. Standard errors clustered at the county-level in parentheses. Coefficients and standard errors are multiplied by 10,000 and can be interpreted as the change in weekly diagnosis rates per 10,000 households. *** p<0.01, ** p<0.05, * p<0.1.

12.4. Alternate COVID Outcomes: COVID-Related Hospitalizations and Medical Spending

Figure A1: DDD Estimates of the Impact of School Visits on COVID-19 Medical Spending by Week

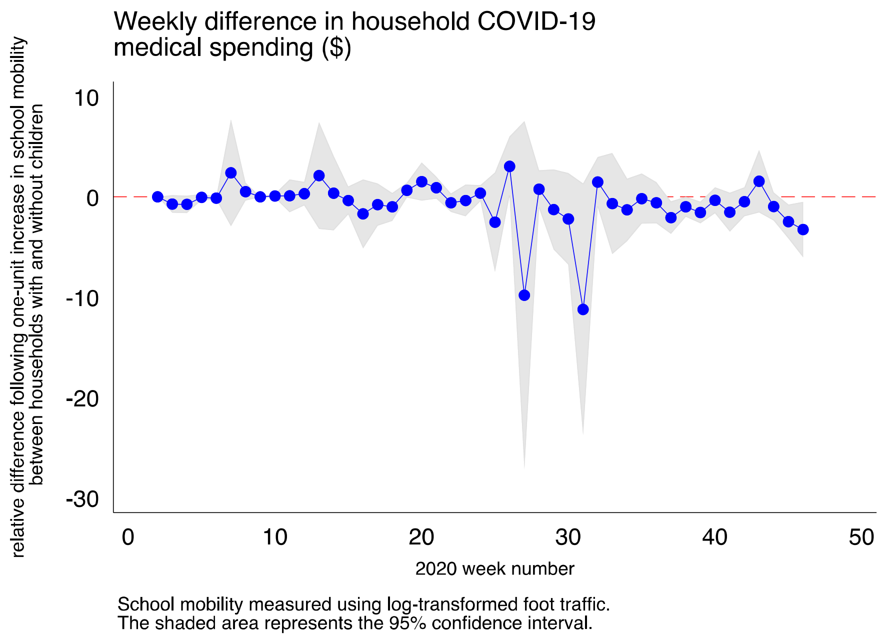

Figure A2: DDD Estimates of the Impact of School Visits on COVID-19 Log-transformed Medical Spending by Week

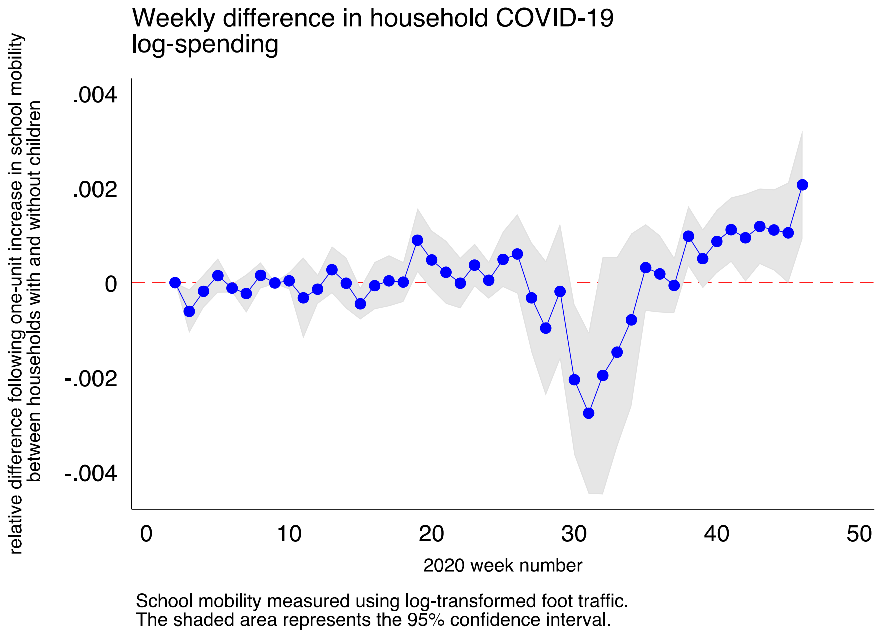

Figure A3: DDD Estimates of the Impact of School Visits on COVID-19 Hospitalization Rates by Week