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To What Extent Does In-Person Schooling Contribute to the Spread of COVID-19?

Evidence from Michigan and Washington

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Abstract

In this paper we use data from two states—Michigan and Washington—on COVID case rates at the county level linked to information on the type of instructional modality offered by local public school districts to assess the relationship between modality and COVID outcomes. We focus primarily on COVID case rates, but also provide estimates for hospitalizations (in Washington only) and deaths. Our preferred district and month fixed effects models exploit within district (over time) variation in instructional modality and account for time-invariant district factors. In both states, we find evidence that instructional modality does lead to increases in COVID spread in communities with moderate to high levels of pre-existing COVID cases, although the causal effect is small in magnitude.

1. Introduction

In the spring of 2020, the COVID-19 pandemic forced all K12 public schools in the United States to close school buildings and shift to remote instruction. Concerns about whether in-person schooling leads to increased spread of the SARS-COV2 virus that causes COVID-19 (henceforth we use COVID as a shorthand for both the virus and the disease) led to considerable debates about whether public schools should open for in-person instruction in the fall of 2020. There was substantial variation in districts' re-opening decisions over the 2020-21 school year, and by the late spring of 2021 the vast majority of U.S. public schools were educating students in-person at least a few days a week. This alongside rising vaccination and falling COVID rates have schools planning for in-person instruction in fall 2021.[1] There nonetheless remains uncertainty about fall plans, in particular how school systems should handle localized COVID outbreaks or the emergence of new vaccine-resistant COVID variants.[2] The disease also continues to proliferate in other countries, especially those where vaccine access remains limited. Policymakers across the world are reckoning with questions about the extent to which in-person schooling contributes to the spread of COVID (e.g., Wallen, 2021).

At the same time, there is a growing consensus that remote learning has not worked well for most students (e.g., Kaufman & Diliberti, 2021).[3] This is one reason that a number of professional associations and the U.S. Centers for Disease Control and Prevention (CDC) previously recommended that students return to in-person learning (American Academy of Pediatrics, 2020; CDC, 2020). These recommendations included the qualification that in-person schooling occur only with appropriate safety measures to mitigate the risk of COVID transmission. There is of course no guarantee that schools adhere to recommended safety protocols, and, indeed there are documented cases of COVID outbreaks tied to transmission inside school buildings (Furfaro & Bazzaz, 2020; Hicks, 2021; Martin & Ebbert, 2020; Razzaq, 2020; Stein-Zamir et al., 2020; Wisely, 2020). On the other hand, while it may be natural to assume that removing students from contexts in which they are in close quarters in school buildings will allow for greater social distancing and COVID mitigation practices, keeping school buildings closed does not necessarily reduce community COVID spread; the impact of in-

[1] Even though most U.S. school districts offered some in-person schooling by the end of the 2020-21 school year (Sparks, 2021), many districts operated remotely for part or all of the year, and even those that offered in-person learning opportunities often did so in a "hybrid" or part-time format. Substantial portions of students did not return to in-person schooling either because they had no option or they chose not to take it (French & Wilkinson, 2021; Hopkins et al., 2021; Johnson, 2021; Lorch, 2021). See Rahimi (2021) for a summary of school modality trends using publicly available weekly school status data from the 200 largest school districts compiled by Burbio since November 2020.

[2] See Meckler and St. George (2021) and Allen and Mina (2021) for discussion of fall 2021 plans and a likely COVID surge in unvaccinated adults and children in the fall and winter of 2021 that could impact school operations.

[3] Early research suggests that, on average, remote instruction during the COVID pandemic has been harmful to students, both because of missed opportunities to learn academic content resulting in reductions in student achievement growth over the year (e.g., Kaufman & Diliberti, 2021; Kogan & Lavertu, 2021; Sass & Goldring, 2021) and because of the adverse effects on students' mental health and wellness (e.g., Martin & Sorensen, 2020; Sprang & Silman, 2013; Xie et al., 2020). These impacts appear to be greater for historically underserved student populations (e.g., Dorn et al., 2020; EmpowerK12, 2020; Hart et al., 2019; Hoffman & Miller, 2020; Kuhfeld, Soland, Tarasawa, Johnson, Ruzek, & Lewis, 2020; Kuhfeld, Soland, Tarasawa, Johnson, Ruzek, & Liu, 2020; Sass & Goldring, 2021).

person schooling on community spread depends not only on safety practices in schools, but also on what students and staff are doing under the counterfactual condition of no in-person schooling. It is possible, for instance, that COVID mitigation practices in schools are more effective than practices in the contexts students are exposed to when school buildings are not open. In short, it is not possible to say ex-ante that the risk of COVID spread is higher or lower with a particular school modality (i.e., in-person, hybrid, remote).

In this paper we use data from two states—Michigan and Washington—on COVID case rates at the county level linked to information on the type of instructional modality offered by local public school districts to assess the relationship between modality and COVID outcomes.[4] While we focus primarily on COVID case rates, in an online appendix we also provide estimates for hospitalizations (in Washington only) and deaths.[5]

As we describe in the next section, there is evidence that COVID transmission is correlated with various district- and county-level factors, such as political views, which are correlated both with attitudes about the need for COVID mitigation and school modality decisions. To address this concern, we rely primarily on district and month fixed effects models that exploit within district (over time) variation in instructional modality and account for time-invariant district factors. In both states, we find evidence that instructional modality does lead to increases in COVID spread in communities with moderate to high levels of pre-existing COVID cases, although the causal effect is small in magnitude.

We further estimate two additional types of models: 1) event studies that allow us to assess how COVID rates change over time relative to when the district first reported offering in-person instruction; and 2) models that consider differential impacts based on school district estimates of modality take-up rates when in-person or hybrid instruction is offered (e.g., the percent of students in districts attending in-person schooling). Our event study models show that, in Michigan, but not in Washington, there is an initial increase in COVID rates in the months after districts begin offering in-person schooling after which effects slowly fade out. There is little consistent evidence of differential effects in districts with lower or higher percentages of students reported to be in school buildings.

Our preferred two-way fixed effects (TWFE)—district and month fixed effects—estimation strategy identifies causal impacts of school modality on COVID spread in the case that treated districts would have followed the same trends in outcomes as untreated districts if they had not been in-person or hybrid (the parallel trends assumption). Recent research, however, has indicated that these estimates could be biased under heterogenous treatment effects (Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2020). To address this concern, we provide estimates and results of tests for parallel pre-trends using methods developed by de Chaisemartin and D'Haultfœuille (2020) and Sun and Abraham (2021). These analyses suggest our TWFE estimate are unlikely to suffer

[4] Note the distinction between offer type and the choice by parents to send children to schools. We use "in-person," "hybrid," and "remote" modality throughout to refer to offer type, and to describe evidence about the percentage of students that are reported to be in schools in hybrid or in-person modalities.

[5] As we describe below, the findings for hospitalizations and deaths are broadly consistent with the case rate but since they are low incidence, it is hard to detect effects and most estimates are small and insignificant. Hospitalization data are not available at the county level for Michigan hence we only include Washington for in these analyses.

from this type of bias.[6] Nonetheless, caution about drawing strong causal conclusions remains warranted since we cannot directly account how changes in school modality might be correlated with the subsequent selection into COVID testing. This could lead to bias from non-classical measurement error. We do, however, note that when we examine deaths and hospitalizations, which are less likely to be affected by this measurement error, the pattern is similar—there are not statistically significant relationships between school modality and these outcomes at low pre-existing case rates.

The remainder of the paper proceeds as follows: Section 2 briefly reviews the literature to date on the relationship between public schooling and COVID spread. Section 3 outlines our data from Michigan and Washington, highlighting similarities and differences across the two contexts. Section 4 outlines our methods of estimating the relationship between instructional modality and COVID community spread. Section 5 describes our results. Section 6 concludes with a discussion of the results and implications for decisionmakers.

2. Background on School Modality and COVID Spread

Much of the early evidence on whether COVID spreads in schools, based on international studies, reached mixed conclusions (e.g., Dub et al., 2020; Fontanet, Grant, et al., 2020; Stein-Zamir et al., 2020).[7] Several U.S.-based studies examining COVID spread relied on contact tracing to assess the extent to which the virus is spread within schools. Contact-based studies on North Carolina and Wisconsin have shown that within-school transmission of COVID is limited when risk mitigation strategies (e.g., mask wearing and distancing) are employed (Falk et al., 2021; Zimmerman et al., 2021).

Although contact-tracing studies are useful in understanding the mechanisms through which instructional modality may impact COVID spread, they provide less value for understanding how modality affects *community* COVID spread. In particular, missed contacts can lead to inadequate tracing and attribution of infections. Moreover, as noted above, assessing spread only *within* schools does not speak to the counterfactual condition. To that end, a study by Oster et al. (2021) used data from three states to assess the correlations between three mitigation strategies—in-person density in school buildings, ventilation upgrades, and mask mandates – and COVID spread among students, teachers, and school staff. The authors found that masking was not correlated with COVID rates and that ventilation upgrades were correlated with lower COVID rates in Florida but not in the other states. Counterintuitively, they showed that greater in-person density in schools was correlated with lower COVID rates for students. A potential explanation for this finding is that parents and administrators may choose to send students back into in-person schooling at higher rates when COVID rates are lower, thus biasing downwards any relationship between in-person schooling and in-person density and observed COVID rates.

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[6] Since these estimators do not allow us to easily incorporate interactions and multiple treatments, we use them as a check on our main results and show that they are similar to findings using the traditional TWFE methods and satisfy pre-trend tests.

[7] Variation in mitigation strategies such as mask-wearing and small classroom cohorts may contribute to some of the differences across studies. Another potential explanation for differences across studies is the age of the children in the schools as there is evidence that the probability of becoming infected, and in some cases transmitting the virus, is lower for younger school-aged children and increases with older children and adults (Dattner et al., 2021; Fontanet, Grant, et al., 2020; Fontanet, Tondeur, et al., 2020; Park et al., 2020). Some of this evidence suggests older children transmit COVID at rates similar to adults (Park et al., 2020). Finally, differences in testing may lead to an undercount of cases among children (Couzin-Frankel et al., 2020).

A growing literature has begun to examine the role of instructional modality in community spread of COVID. Lessler et al. (2021), for instance, used responses to a large online survey—with questions about COVID symptoms, children in the household, and schooling experiences—to assess whether school modality was associated with increased risk of COVID spread. The study concluded that living in a household with a student receiving in-person schooling was associated with a large increase in the odds of reporting a COVID-like illness, but this risk was greatly reduced with a greater number of school virus mitigation strategies reported.[8] This finding is broadly reflective of the contract-tracing studies, but is also limited in that it relied on self-reports of mitigation and COVID-like illnesses (which may also include severe colds and flu) from a survey sample that may not be representative of the larger population.

Several recent papers use administrative data to understand the relationship between in-person schooling and COVID community spread. They draw on data from different contexts, use somewhat different empirical strategies, assess different outcomes, and reach different conclusions. Three of these are based in the United States. Rauscher and Burns (2021) exploited variation across states in the timing of school closures to study the impact of modality on COVID death rates. They tracked the number of days from the first reported COVID case in spring 2020 to the time of state mandated closures, using matching strategies to account for heterogeneity of observed county characteristics. They found that later school closures in the United States were associated with higher death rates, and that the relationship was stronger in counties with high poverty and high proportions of Black residents. By contrast, Harris and colleagues, (2021) who also used national data, found more limited evidence connecting modality to COVID outcomes. Using county fixed effects and matched difference-in-differences methods, the authors found robust evidence that school openings were not statistically associated with increased hospitalizations when baseline hospitalization rates were low. But when baseline hospitalization rates were high, there was inconsistent evidence across model specifications. (Courtemanche et al. (2021) applied an event study framework to data on the timing of school openings in Texas in the beginning of the 2020-21 school year. They found consistent and quite large impacts of earlier openings on both COVID cases and deaths.[9] The study also suggested that the impact of school re-openings is, in part, driven by increased adult mobility in response to school re-openings rather than just spread from the school buildings. [10]

All the above studies (and ours, described in detail below) face the same central issue in estimating the relationship between school modality and COVID outcomes: the potential that unobserved heterogeneity amongst communities is correlated both with school modality decisions and COVID outcomes. This is a challenge given that both school openings and general COVID responses have been highly politicized (e.g., Valant, 2020). For instance, there has been shifting rhetoric about whether children transmit COVID. Research has now clearly established that children, while typically having milder reactions to COVID infection, can transmit the virus (Lopez et al., 2020). But communities are geographically heterogeneous in their views on the need to worry about COVID transmission and the degree to which they practice risk mitigation

[8] If seven or more of the right strategies are employed, the relationship between in-person schooling and a report of a COVID-like illness was no longer statistically significant.

[9] The study estimates that the openings of schools led to over 40,000 additional COVID cases and 800 deaths.

[10] There are also two relevant studies based on data from Germany. Both use event study models to exploit plausibly random variation in timing of school openings and find openings are not associated with increased community spread (Isphording et al., 2020; von Bismarck-Osten et al., 2020).

strategies such as physical distancing and mask-wearing, in schools or otherwise (Adolph et al., 2021; Bruine de Bruin et al., 2020). These differing attitudes are correlated with both voting patterns and school modality decisions (Adolph et al., 2021; Brenan, 2020; Clinton et al., 2021; Gollwitzer et al., 2020; Grossman et al., 2020a; Schneider, 2020; Van Kessel & Quinn, 2020).[11]

The above clearly raises concerns that unobserved community heterogeneity might lead to biased estimates in the relationship between school modality decisions and COVID spread—but assigning the likely direction of bias is not straightforward. On the one hand, schools in urban areas and with high rates of low-income families were more likely to have schools operating remotely (Gross et al., 2020; Hopkins et al., 2021). Lower income workers are also less likely to have the opportunity to work from home, arguably increasing the likelihood of COVID spread among adults at work (Gould & Shierholz, 2020; Schaner & Theys, 2020). Given that we do not directly observe work arrangements, we might therefore expect an upward bias on the estimated relationship between remote schooling and COVID incidence. On the other hand, forward-looking policymakers may have unobserved information about the risks of opening schools that influences their decisions, presumably resulting in a negative bias in the relationship between remote learning and COVID outcomes.

A related issue is that selection into a modality decision could be correlated with the subsequent frequency of COVID testing; and hence identified COVID cases. This could arguably affect other COVID outcomes as well. For all these reasons, it is important to be cautious when interpreting findings about the role of instructional modality in COVID spread. In the next section, we describe how we attempt to account for the various non-school factors that could influence community spread, and account for the potential of heterogeneous effects across counties.

3. Data and Measures

We use several sources of data to understand how districts' instructional modality decisions (fully in-person and fully remote schooling at the extremes) and students' uptake of the offered modalities influence the spread of COVID in Michigan and Washington. Data on reported COVID cases are collected by the states' respective state health agencies, the Michigan Department of Health and Human Services (MDHHS) and the Washington Department of Health (WADoH). District-level information on educational modality is collected by each of the states' departments of education via monthly or weekly surveys administered to school districts, in Michigan from the Michigan Department of Education and the Center for Educational Performance Information and in Washington from the Washington Office of the Superintendent of Public Instruction. The data used for the analysis are relatively consistent across both states, though below we provide details on slight differences as well as contexts surrounding COVID incidence and school modality. Finally, in each state, we merge the above sources of information onto other publicly available information about community characteristics.

COVID-19 Data in Michigan and Washington

[11] For more on the broader impacts of the pandemic, politicization of COVID and attitudes about the pandemic response, as well as the policy and political debates about schools being open for in-person instruction, see: Green et al. (2020); Grossman et al. (2020); Miller, 2020; and Zamarro and Prados (2021).

Daily counts of newly confirmed COVID cases are available publicly for all counties in both Michigan (*N*=83) and Washington (*N*=39). As we show in Figure 1, both states experienced growth in COVID cases early in 2020 (though given low testing capacity, these are underestimates of the true incidence), and, after plateauing in the summer, infections again rose rapidly in October, peaking in November (Michigan) and December (Washington). In Michigan there was a third wave that peaked in April 2021. While both states followed a similar pattern of COVID growth and decline, it is notable that COVID case rates were generally far lower in Washington than in Michigan. For instance, from September 2020 through May 2021—our period of study—the 50th percentile of 7-day average daily case rates per 100,000 residents was 21 in Michigan and 15 in Washington.[12]

We also use data on COVID-related hospitalization and death rates in models that estimate the impact of modality on county incidence for each outcome. Daily counts of new COVID deaths in Michigan, as well as weekly counts of new COVID hospitalizations and deaths in Washington, are available publicly from the Michigan Department of Health and Human Services (MDHHS) and Washington Department of Health (WADoH), respectively.[13] COVID hospitalization data in Michigan are available only by Healthcare Coalition regions, which contain between 3 and 19 unique counties, and thus are not shown here.

District Instructional Modality Data

Both states' departments of education surveyed school districts regularly to collect information about the mode of instruction delivered during the pandemic. In Michigan, districts were asked to indicate how they planned to deliver instruction in each upcoming month, while in Washington, districts reported the modality that was delivered on the final day of the month in fall 2020 and then the planned modality for the upcoming week starting January 18, 2021. To align the timing of the surveys as closely as possible across states, we assign Washington end-of-month surveys in the fall to the subsequent month (e.g., Michigan districts' modalities at the beginning of October are compared to Washington districts' modalities on September 30) and in the surveys for the last week of the previous month for February–April 2021.[14,15]

[12] We calculate average daily COVID incidence counts across a rolling 7-day window to smooth out random fluctuations due to reporting differences by day of week and holidays and different daily tendencies for people to get tested (e.g., people tend to be more likely to get tested during the week). We then use county population estimates from the 2014-2018 American Community Survey to convert these average counts to relative rates per 100,000 county residents. The resulting 7-day average rates per 100,000 residents form the basis of the main outcome measure of COVID growth used in our analysis: the 7-day average rate on the first day of the month.

[13] Similar to how we calculate our COVID case rate measures, we calculate average daily COVID hospitalizations and deaths per 100,000 residents in Washington by converting weekly counts to daily averages and then use county population estimates to calculate relative rates per 100,000 county residents. In Michigan, we calculate average daily COVID death counts across a rolling 7-day window before converting averages to relative rates per 100,000 county residents.

[14] We use Michigan modality from September through April of the 2020-21 school year. Because the first Washington survey was conducted on the last day of September 2020 (which we infer as representing instructional modalities for the beginning of October), and the first survey of the spring semester was not conducted until January 18, 2021, we only use the months of October through December 2020 and February through April 2021. For the event studies in Washington (described in Section 4), we apply the October modality to September and use the January 18 modality as the modality for the entire month of January.

[15] In both Michigan and Washington, the far majority of districts reside in a single county. In Michigan, 32 districts draw students from two counties. However, 99% of students attend a school that is in the same county as the district's central office. In Washington, there are fewer than 10 districts with schools located in different counties

The definitions of instructional modalities vary slightly between the two states due to differences in the ways their surveys are structured. For Michigan, these definitions are based on what districts offer to their general education students. We define "in-person" districts as those that provide general education students with the opportunity to receive full-time in-person instruction; in some cases, students may opt for either hybrid or remote instruction. "Hybrid" districts are those that offer some or all of their general education students in-person schooling at least some portion—usually two to three days—of a week; hybrid districts do not offer their students the option of fully in-person instruction, but here too students may opt for remote instruction if districts provide this offering. "Remote" districts provide all instruction in a remote or virtual format for all their general education students. These definitions are mutually exclusive and based only on the mode of instruction provided to general education students, i.e., they may not necessarily reflect the modality provided to special populations of students.[16]

Washington districts are classified as "in-person" if they indicated they provided "typical/traditional in-person" instruction to elementary, middle, and/or high school students; classified as "remote" if all of their students, or all except small subgroups of students,[17] received fully remote instruction; and classified as "hybrid" if all students received "partially in-person" instruction or the district used a "phase-in" approach where some students received partially or fully in-person instruction while others still received remote instruction.

Districts in each state were also asked to approximate the share of students who received different modes of instruction (Michigan districts were asked to estimate in-person and hybrid separately, and Washington districts were asked whether students were "receiving some level of in-person instruction" which we interpret as the share of students receiving either in-person or hybrid instruction). In Michigan, districts were asked to select one of the following percentage ranges: 24% or less, 25-49%, 50-74%, 75-99%, or 100%. The Washington survey was structured similarly in the fall semester, but used slightly different ranges: 0%, 1-10%, 11-25%, 26-50%, 51-75%, or 76-100%. In the spring, the Washington survey asked districts to provide an exact estimate of the proportion of students receiving some level of in-person instruction. We aggregate both of Washington's fall categories and the continuous spring measure so that they are comparable to the wider categories used in Michigan. In some of the specifications described below, we use these categorical estimates of student enrollment by modality to assess the relationship between estimated in-person or hybrid enrollment and COVID case rates.

Community Characteristics

In some models, we include a set of covariates hypothesized to influence both instructional modality and COVID incidence. These include the following demographics: county population share below 18, share of residents 65 or older,[18] the number of individuals living in

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than their district central offices. Similar to Michigan, over 99% of Washington students attend schools in the same county as the district. In our analyses, we attribute the instructional modality of and students enrolled in a district to the county in which the district's central office resides.

[16] For instance, if a district provides fully remote instruction to all general education students and fully in-person instruction to all special education students, it would be classified as a remote district.

[17] Washington districts offering alternative instruction for small groups are primarily providing partial or full in-person instruction for only a small subset of students. Most commonly these subsets include students with disabilities (or with 504 plans), English language learners, and students with extreme economic disadvantage (e.g., students experiencing homelessness).

[18] We use 1-year estimates of county population and age distribution from the 2019 American Community Survey (ACS).

long-term care facilities per 100,000 residents,[19] the number of religious congregations per 100,000 residents and religious adherents per 1,000 residents[20], indicators for having a correctional facility or college/university in the county,[21] district-level share of students who are underrepresented minorities, and whether a school district is urban, rural, or suburban.[22] However, because these variables are all time-invariant, they are not included in our preferred district fixed effects specifications.

We also include controls for economic and political contextual factors believed to shape local responses to the pandemic. These include average monthly unemployment rates in 2019,[23] poverty rates,[24] and 2016 presidential vote share for Donald Trump.[25] To account for compliance with social distancing we also include county-level estimates of mask usage in July 2020.[26] All of the aforementioned variables are time-invariant, but we also include two variables that do vary over time: the share of people in a county who stay at home in a given day,[27] and cumulative vaccination doses administered.[28]

Descriptive Statistics

Table 1 provides summary statistics by state and modality averaged across all months. School districts in Michigan were far more likely than those in Washington to offer in-person schooling. Approximately 62% of Michigan district-months offered fully in-person instruction, typically as one possible option available to parents along with hybrid or fully remote instruction. Twenty-one percent provided only remote instruction, and most of these districts are in or near large urban areas. The remaining 17% adopted a hybrid model where students attended in-person for part of the week and participated in remote instruction for the remainder of the week. By contrast, in Washington, most districts were either in a fully remote (28%) or hybrid (57%) model; the remaining 15% of districts, located predominantly in rural areas, were in-person.[29]

Since we weight our regressions by student enrollment, all summary statistics in Table 1 are enrollment-weighted. The third row of the table provides weighted modality exposure and

[19] Calculated as number of occupied beds from the COVID-19 Nursing Home Dataset (Centers for Medicare and Medicaid Services) in August 2020 divided by total population from the ACS.
[20] U.S. Religion Census, Religious Congregations and Membership Study, 2010.
[21] Michigan Department of Corrections Directory, Washington Department of Corrections, and the Integrated Postsecondary Education Data System.
[22] Michigan Department of Education and Washington State Office of Superintendent of Public Instruction.
[23] Local Area Unemployment Statistics, US Bureau of Labor Statistics.
[24] American Community Survey 2014-2018.
[25] County Presidential Election Returns 2000-2016, MIT Election Data and Science Lab.
[26] Dynata via the *New York Times*, https://github.com/nytimes/covid-19-data/tree/master/mask-use.
[27] Bureau of Transportation Statistics, US Department of Transportation, https://data.bts.gov/Research-and-Statistics/Trips-by-Distance/w96p-f2qv.
[28] Michigan Department of Health and Human Services and Washington Department of Health.
[29] In each state there are a small number of districts that did not report their instructional modality in each month, as shown in Table A1 in the online appendix. Less than 2% of district-months are missing in Michigan and less than 8% of district-months in Washington. Table A2 of the appendix shows that districts with missing modality data are relatively similar to districts without missing data, although in Michigan the former are located in counties with lower Trump vote shares, higher shares of people staying at home regularly, larger population shares above 65, larger nursing home populations, higher unemployment and poverty rates, more likely to have a county correctional facility. In Washington, they have higher mask usage, lower vaccination rates, larger population shares below 18 and above 65, larger nursing home populations, more religious adherents, and high COVID case, death, and hospitalization rates per 100,000 residents.

indicates that the share of students enrolled in in-person districts is much smaller than the share of districts offering that modality, reflecting the fact that these districts tend to be smaller and more rural. In Michigan, while 62% of district-months were offered in-person, only 54% of student-months were exposed to an in-person option. Similarly, in Washington, only 4% of students were enrolled in the 15% of district-months that offered in-person instruction.

In both states, there are notable differences between districts offering the various schooling modalities. For instance, remote districts in Michigan tend to have larger shares of Black and Hispanic students than do in-person and hybrid districts. We see similar trends in Washington, where nearly 6% of students in remote districts are Black (compared to 1% and 3% in in-person and hybrid districts, respectively) and 24% are Hispanic (16% and 24% for in-person and hybrid, respectively). Remote districts in both states also tend to be in counties with lower shares of votes cast for Donald Trump, more frequent mask usage compared with in-person districts, and tend to have more people staying at home regularly.

As we describe below, some of our models rely on district changes in modality. These changes, which are documented in transition matrices in the appendix (Table A1) tended to be in the direction of more in-person instruction (i.e., remote to hybrid or hybrid to in-person).[30] For instance, in a typical month in Michigan, 9.3% of districts moved to a more in-person status in month *t+1* (e.g., hybrid to in-person) while 5.6% of districts (those above the diagonal) moved to a less in-person status (e.g., hybrid to remote; those districts below the diagonal of the matrix for which there is not missing data). Similarly, in Washington, 13.0% of districts moved to a more in-person status relative to 3.2% that moved to a more remote status.

4. Methods

We begin by estimating naïve Ordinary Least Squares (OLS) models of the relationship between COVID incidence at the beginning of month *t+1* and instructional modality in month *t*. Our models interact modality with COVID rates in a county four weeks prior to when the modality is observed. We consider models both with and without district fixed-effects. The first model is:

$$COVID_{c,t+1} = \alpha + f\left(IP_{jct}, H_{jct}\right) + \gamma_1 Mask_c + \gamma_2 ShareAtHome_{\mathrm{ct}} + \gamma_3 TrumpShare_c + \gamma_4 Vaccinations_{\mathrm{ct}} + \boldsymbol{\Omega}'\boldsymbol{X}_c + \delta_t + \varepsilon_{jct} \quad (1)$$

where

$$f\left(IP_{jct}, H_{jct}\right) = \beta_1 IP_{jct} + \beta_2 H_{jct} + \beta_3 COVID_{c,t-1} + \beta_4 COVID_{c,t-1}^2 + \beta_5 IP_{jct} \times COVID_{c,t-1} + \beta_6 H_{jct} \times COVID_{c,t-1} + \beta_7 IP_{jct} \times COVID_{c,t-1}^2 + \beta_8 H_{jct} \times COVID_{c,t-1}^2$$

and $COVID_{c,t+1}$ is the daily average (based on a rolling 7-day window) COVID incidence per 100,000 individuals for county *c* at the beginning of month *t*+1. All regressions are weighted by district enrollment and standard errors are clustered at the county level.

IP_{jct} and H_{jct} represent the instructional modality *offered* by district *j* in county *c* and month *t*, where *IP* is fully in-person instruction for general education students and *H* is hybrid. The $f\left(IP_{jct}, H_{jct}\right)$ equation is a key part of our estimation strategy as it incorporates the role of

[30] Table A1 aggregates modality transitions across all months in the study period. Since districts can make multiple transitions throughout the school year, it is possible for a single district to appear in multiple cells within the table.

existing case rates in the potential impacts of modality on COVID. We model this as a quadratic function of daily average COVID case rates per 100,000 residents four weeks prior to the month for which modality information is collected. For example, if modality is observed as of November 1, the outcome is the daily average COVID case rate on December 1, and equation $f(IP_{jct}, H_{jct})$ includes daily moving average COVID rates as of October 10 (four weeks before the first day of November). The interactions between in-person (IP_{jct}) and hybrid (H_{jct}) modalities with prior case rates allow us to estimate heterogenous school modality treatment effects for different baseline case levels. This is important, as many epidemiologists argue that in-person schooling is less likely to risk health and safety if cases in the community are low, but it is considerably riskier when cases are high (Boyle, 2020). There are also indications from simulations that the impact of school modality could be larger when cases are higher (Cohen et al., 2020).

In Equation (1), the coefficients are identified by cross-sectional variation in school modality. Under certain assumptions (described below), the estimates in $f(IP_{jct}, H_{jct})$ capture an intent-to-treat (ITT) effect rather than the average effect of the treatment-on-the-treated (ATT). This is because we observe the modality districts are *offering* rather than individual student participation in a particular modality. If the policy is to require a single modality (in-person, hybrid, or remote) for all students, then the ITT would equal the ATT, but during our period of observation the vast majority of schools at least offered a remote option.[31] Nonetheless, the ITT estimate is important from a policy perspective because districts can decide the policy to implement but are less able to control uptake.

It is also important to note that effects are likely to be heterogeneous as a function of the characteristics of families whose students enroll in a given district (e.g., parents who cannot work remotely may be more likely to choose to send their children back in person) and what the counterfactual activities are of children in remote environments (e.g., the degree to which school-age children social distance at home relative to when they are at school). Hence, the estimates provide an average of these heterogenous effects across various contexts.

We also include control variables to account for non-schooling risk factors for COVID spread. $Mask_c$ is the share of individuals in county *c* who reported "always" wearing face masks when in public as of July 2020. $ShareAtHome_{\mathrm{ct}}$, which is calculated by the U.S. Department of Transportation, represents the estimated share of people in a county who stayed home in the fourth week prior to the first day of month t (as a 7-day average of the daily estimated rates). These variables serve as proxies for compliance with social distancing measures. $TrumpShare_i$ is the share of the 2016 presidential election vote for President Trump, which serves as a measure of political leanings in the county. We also include $Vaccinations_{ct}$ which is the cumulative number of COVID vaccinations administered per 100,000 people in county *c* in month *t*. Finally, $\boldsymbol{X}_c$ is a vector of additional time-invariant county-level characteristics including the 2019 county unemployment rate, the 2018 individual poverty rate, and the share of the population that is age 65 or older, lives in a nursing home, and is not White or Asian (underrepresented minority groups). The vector also includes the share of public school students relative to the population of each county as we might expect the risk of community COVID spread due to in-person schooling to depend on how important the student population is in a

[31] Between 93% and 96% of Michigan districts offered a remote option to at least some general education students in each month of the 2020-21 school year. In Washington, approximately 89% of districts offered a remote option throughout the year. Many students choose to learn remotely even when presented with an in-person or hybrid option (e.g., Hopkins et al., 2021).

county. Further, it includes the number of religious congregations per 100,000 residents and religious adherents per 1,000 residents in county c in 2010 (the most recent year of data available), whether the county has a college or university, and whether the county has a prison.[32] Urbanicity is also included at the school district level. δ_t is a month fixed effect and ε_{jct} is an error term.[33]

A potential problem with the approach used in Equation (1) is that it relies on a selection on observables assumption—that after accounting for the social distancing, political, and vaccination controls as well as the demographic and economic information in $\boldsymbol{X}_c$, $Cov(\varepsilon_{cjt}, InPerson_{cjt}) = 0$ and $Cov(\varepsilon_{cjt}, Hybrid_{cjt}) = 0$. If this assumption fails then the estimates could be biased. Indeed, as described in Section 2, there are reasons to believe that instructional modality decisions are correlated with unobserved COVID mitigation strategies and that our controls likely do not fully capture these. Given this concern, our preferred specification adds district fixed effects, ω_j, which will account for any time-invariant unobserved factors that affect both COVID rates and modality:

$$COVID_{c,t+1} = \alpha + f\left(IP_{jct}, H_{jct}\right) + \gamma_1 ShareAtHome_{ct} + \gamma_2 \text{Vaccinations}_{ct} + \omega_j + \delta_t + \varepsilon_{jct} \quad (2)$$

When we include district fixed effects, time-invariant covariates necessarily drop from the model. In this specification, the effect of school modality is identified based on within-district changes in school modality offerings (Appendix Table A1 reports the frequencies of modality switches in each state). We also estimate the same models using COVID-related hospitalizations (in Washington only) and deaths as outcomes.

As discussed above, these models provide the ITT estimate of the effect of modality on COVID incidence in the surrounding community. As noted in Section 3, both states surveyed districts about the share of their students attending schools in-person or hybrid. Districts reported estimated shares in ranges, although the exact ranges differ across states. We use these district estimates to estimate supplemental variations of models (1) and (2) with the following modality variables:

$$f\left(IP_{jct}, H_{jct}\right) = \sum_g \begin{pmatrix} \beta_1 IP_{jct}^g + \beta_2 H_{jct}^g + \beta_3 COVID_{c,t-1} + \beta_4 COVID_{c,t-1}^2 + \beta_5 IP_{jct}^g \times COVID_{c,t-1} \\ + \beta_6 H_{jct}^g \times COVID_{c,t-1} + \beta_7 IP_{jct}^g \times COVID_{c,t-1}^2 + \beta_8 H_{jct}^g \times COVID_{c,t-1}^2 \end{pmatrix} \quad (3)$$

where IP_{jct}^g and H_{jct}^g are indicators for whether the district-reported student enrollment for in-person and hybrid, respectively, falls in modality group g. In Michigan $g \in (1\% - 24\%, 25\% - 49\%, 50\% - 74\%, 75\% - 100\%)$ Washington $g \in (1\% - 25\%, 26\% - 50\%, 51\% - 75\%, 76\% - 100\%)$. In both states, remote-only districts are the omitted category.

The use of model (2), whether using district-offered modality or student take-up of that modality, creates a separate potential bias issue inherent in the use of two-way fixed effects

[32] Our measures of COVID case rates exclude cases in prisons, but we nonetheless control for whether the county has a prison as cases in prisons could themselves spread to local communities.
[33] Covariates with missing values are set to the overall mean. We include indicators for whether a variable is missing in the regressions.

(TWFE, as we include both time and district fixed-effects). Recently, researchers have shown that these models may not provide an unbiased estimate of the ATT. Instead, they estimate a variance-weighted ATT that can be biased under heterogenous treatment effects even when the parallel trends assumption is satisfied (Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2020). Further, models that suffer from this bias may still pass the standard pre-trends tests for the parallel trends assumption, rendering such tests suspect.

New estimators have been developed to address some of these issues by restructuring the regression models to use comparison units that are not yet (or never will be) treated at the time the treated units are (Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfœuille, 2020; Sun & Abraham, 2020). To assess the robustness of our TWFE results to these potential biases, we apply the estimator proposed by de Chaisemartin and D'Haultfoeuille (2020) which allows for districts to move in and out of treatment multiple times.[34] A limitation of this method is that it can only easily accommodate a single treatment and cannot accommodate the lagged interactions in $f(IP_{jct}, H_{jct})$. Hence, we rely on the standard TWFE model to investigate interactions with existing COVID rates and produce specification checks that compare TWFE with the single treatment indicator and no interactions to the de Chaisemartin and D'Haultfœuille (2020) estimates.

Next, we estimate event study models to investigate how the potential impacts of remote schooling on COVID cases propagate over time. Since some districts moved into and out of different modalities multiple times, we must structure our event study analyses as intent-to-treat (ITT) estimates. To do this, we identify treatment as starting when the district first reported providing in-person or hybrid modality regardless of when this occurs during the 2020-21 school year and estimate the models as if that modality remained stable for the rest of the observation period. This creates a staggered event study that is subject to the same biases as the TWFE models. Hence, we use the estimator developed by Sun and Abraham (2020) that corrects for this bias under modified parallel trends and no anticipatory response assumptions.[35]

Since the event study limits the power of our estimates and the Sun and Abraham (2020) method cannot accommodate interaction effects or multiple treatments, we focus on the single combined in-person/hybrid treatment that excludes interactions with prior cases that we use in the de Chaisemartin and D'Haultfœuille models discussed above. That is, we estimate:

$$COVID_{c,t+1} = \alpha + \sum_{l=-K}^{-2} \beta_l \, \mathrm{IPH}_t^l + \sum_{l=0}^{T} \beta_l \, \mathrm{IPH}_t^l + \gamma_1 ShareAtHome_{\mathrm{ct}} + \gamma_2 \mathrm{Vaccinations}_{\mathrm{ct}} + \omega_{\mathrm{j}} + \delta_t + \varepsilon_{jct} \quad (6)$$

where $\mathrm{IPH}_t^l = 1$ if the district adopted in-person or hybrid schooling in period *t* or at any time prior, and *l* denotes the lag or lead from the time of initial adoption.

We note that these methods rely on assumptions that are variations on the parallel trends assumption for difference-in-differences models—that outcomes for treated observations would have continued on a path equivalent to the path observed in untreated observations. While this cannot be tested directly, a necessary condition is that trends in the outcome variable prior to treatment look similar across both groups. But as de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021) point out, standard parallel trends tests are insufficient for

[34] We use the *did_multiplegt* package in Stata (de Chaisemartin et al., 2021).
[35] We use the *eventstudyinteract* package in Stata (Sun, 2021).

checking this. We therefore use placebo and pre-trends tests developed by de Chaisemartin and D'Haultfœuille (2020) and Sun and Abraham (2020) to check for these pre-trends and find little evidence of significant pre-trends.

Another potential concern is that our focus on case rates, while being a clear and high-incidence measure of disease spread, potentially suffers from non-classical measurement error due to selection into whether people get tested. This was a particularly large concern for data from early in the pandemic when testing resources were scarce, though less of an issue by late summer 2020 when our data begin. We have no direct way to assess the extent of this bias, but we do provide results for other measures such as hospitalizations and deaths, which are low incidence but likely less subject to this same kind of measurement error.

5. Results

We perform several types of analyses to describe the relationship between instructional modality and community spread of COVID. Section 5.1 describes results from models that use the modality *offered* by the district as the policy variable. In Section 5.2, we estimate event studies to see how impacts develop over time after a district adopts in-person or hybrid instruction. In Section 5.3, we discuss findings for models that instead use the percentage of students who are reported to participate in a particular modality type as our policy variable; in-person, hybrid, and remote for Michigan, and in-person/hybrid versus remote for Washington.

5.1 COVID Spread and Instructional Modality

In Table 2 we present results from models predicting COVID community spread (cases per 100,000) in Michigan and Washington. Columns (1) and (3) of Panel A provide estimates of equation (2) where we include the set of controls described in Section 4. Before turning to the treatment effects, is worth noting some of the estimates on the control variables in this model. Those not shown in Table 2 are provided in online Appendix Table A3.[36] In particular, the estimates on our social distancing measures are mostly in the expected direction. For Michigan, mask wearing in July 2020 and the share of people staying at home four weeks prior to the month in which modality is observed are negatively correlated with case rates while the share of the county that voted for Trump in 2016 is positively correlated. In Washington, masking is, counterintuitively, positively correlated with case rates while the other measures are the same sign as those in Michigan. Vaccination rates in both states are only weakly correlated with case rates after conditioning on other factors.[37]

Columns (2) and (4) of Table 2 provide estimates from the TWFE model shown in equation (3) that includes both district and month fixed effects. Focusing on the in-person and hybrid school modality indicators along with the interactions with prior cases, the estimates are very similar to those in columns (1) and (3), hence we focus on the TWFE results. Since the impacts on case rates are estimated using a quadratic function of prior case rates, the overall impact of modality on COVID incidence can be hard to discern from the coefficients in the table. Thus, in Figures 2 and 3, we show the TWFE model estimates and 95% confidence intervals at

[36] We also estimated models that include the share of students in a county enrolled in private schools and the results were unaffected.
[37] It is clear from more recent data that case rates are dropping in areas with higher rates of vaccinations (Keating et al., 2021). By March 2021, in the last month of lagged COVID case data used in these analyses, vaccination rates were only 17% in Michigan and 24% in Washington.

each value of cases four weeks prior to the date instructional modality is observed in Michigan and Washington, respectively.

In Michigan, the estimates suggest a small and, at most prior case levels, statistically insignificant increase in COVID cases of between 0 and 2 per 100,000 residents when schools are in-person. The estimated effects are only significant for case rates between 12 and 19 per 100,000, which is between the 40th and 60th percentiles of the pre-existing case rate distribution in Michigan. For hybrid schooling, there is no point in the pre-existing case rate distribution where school modality is statistically significant.

In Washington, while there are no significant impacts when pre-existing cases are low, the point estimates suggest that COVID rates grow with both in-person and hybrid schooling throughout much of the pre-existing case rate distribution. The relationship between hybrid modality and COVID spread becomes statistically significant between 8 and 26 cases per 100,000, which corresponds to the 50th to 93rd percentiles of the pre-existing case rate distribution in Washington. The increase in case rates from hybrid instruction peak at around 10 per 100,000 (a 48% increase) at approximately the same value. There is no significant impact for in-person districts at any pre-existing case rate, although the direction of the effect is positive but imprecisely estimated.

In Appendix Figures A1 through A3 we provide results where the outcomes are hospitalizations (in Washington) and deaths. Here, we see no statistically significant impact of school modality on either of these outcomes. Regardless, deaths and hospitalizations are very low frequency events and so it is unclear that there is sufficient statistical power to detect meaningful impacts on deaths.[38]

As noted in section 4, recent research has shown that TWFE models can be biased under heterogenous treatment effects and methods have been developed to help address the bias. Thus, in columns (1) and (3) of Panel B, we provide TWFE models with in-person and hybrid combined into a single indicator and remove the interaction effects. This provides a standard TWFE specification equivalent to that shown in columns (2) and (4) using the de Chaisemartin and D'Haultfœuille (2020) estimator that accounts for this bias. The combination of the in-person and hybrid modality and the removal of the interactions with prior cases are necessary as the treatment in this estimator needs to be binary. Due to this limitation, we use it as a check on our standard TWFE estimator. For Michigan, the estimates for in-person or hybrid modality in columns (1) and (2) are both small, statistically insignificant, and not significantly different from each other. Further, de Chaisemartin and D'Haultfœuille (2020) provide a placebo test that tests for parallel pre-trends. The estimate for this test, provided at the bottom of column (2), is negative, relatively small, and not statistically significant. For Washington, the estimates on in-person or hybrid modality in columns (3) and (4) are very similar to each other and again the placebo is small and statistically insignificant. Given these results, and the similarity of the TWFE model to the OLS model without district fixed effects, we use the more flexible TWFE model as our preferred model to investigate effect heterogeneity.

5.2. Event Study Analyses

[38] Note, the hybrid findings in Washington had the largest point estimate with a maximum estimate of 10 at 21 pre-existing COVID cases. This produces a 95% confidence interval of -0.1320 to 0.1018. Hence, we can rule out impacts on deaths higher than 0.1 additional death per 100,000 residents from a shift from remote to hybrid modality.

In Figure 4 we provide event studies that define treatment as starting when the district first reports being in-person or hybrid. Since this is a staggered treatment adoption approach, we use the Sun and Abraham (2021) estimation method. Since some districts returned to remote status after initially being open, this is essentially an intention-to-treat (ITT) rather than average effect of treatment on the treated (ATT) effect. First, it is worth noting that Figure 4 shows little evidence of pre-trending in cases prior to adoption for Michigan or Washington, consistent with the placebo tests for the de Chaisemartin and d'Hautfouille specification in Table 2.[39]

A month after schools open in Michigan, there is a statistically significant increase in daily COVID rates of about 4 cases per 100,000 residents. This impact jumps to 10 cases per 100,000 residents for in-person or hybrid districts two months after re-opening, after which point the effects slowly fade out. It is unclear whether these fadeout effects are due to the impacts of in-person/hybrid modality themselves diminishing over time or to schools switching modalities later—although the smaller estimated effects from our baseline TWFE models suggest that some of the fadeout is indeed due to the impacts of modality on case rates growing smaller over time.

We also run event studies examining COVID-related hospitalizations (in Washington) and deaths (in both states). These are provided in Online Appendix Figures A1–A3 and show little evidence of significant impacts at any level of existing cases. Combined, our event study estimates in Michigan are somewhat consistent with the two extant studies using administrative data to assess the impact of U.S. school re-openings on COVID outcomes. While we find some indications of a positive impact of modality on COVID cases, like Harris and colleagues (2021), we find largely null effects on hospitalizations, certainly at lower levels of pre-existing rates of cases in the community. However, in contrast to Courtemanche et al. (2021), we do not find an impact of openings on COVID-related deaths.[40]

In Washington, there are no statistically significant effects at any time post-adoption. While we find positive estimates in the baseline TWFE model, the estimates in this figure are mostly negative because they are all relative to the period immediately prior to adoption, while the baseline model in Table 2 is relative to *all* periods prior to adoption. Even so, due to the smaller number of districts in Washington, the model is imprecisely estimated, leaving us unable to draw conclusions regarding the evolution of effects of modality on COVID rates over time in Washington after schools initially move to in-person or hybrid instruction.[41]

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[39] We also estimate models that mirror those used in Harris et al. (2021) and Courtemanche et al. (2021) that assign the modality as of September 2020 for the duration of the school year. Since in these models we define the treatment timing as of a specific date for all districts, the biases that can arise from staggered event study designs do not apply and we make the standard difference-in-differences assumptions of parallel trends and no anticipatory effects. Overall, 76% of schools in Michigan and 36% of schools in Washington adopted in-person or hybrid schooling options at the start of the school year. In these models (available by request) we also see little indication of pre-trending and similar temporary and fading post-adoption increases in COVID case rates. We further note that when estimating a similar event study with COVID test positivity as the outcome (available by request), we find no evidence of pre-trends.

[40] It is worth noting that in the time period included in Courtemanche et al.'s (2021) analysis, baseline case rates in Texas averaged 21 per 100,000 residents per day (148 per week) prior to school start, somewhat larger than in Washington or Michigan. Given this discrepancy, our results are not directly comparable. Nonetheless, when we estimate the September modality-based event studies, we find a peak increase of 8 cases per day four weeks after school opening in Michigan, similar to their estimate of approximately 8.5 cases per day (60 per week) increase.

[41] Another benefit of estimating an event study is that we can examine how the relationship between modality and COVID outcomes differs in months during which COVID cases surged, such as the winter or, in Michigan's case, late spring. In both states, the event study models suggest that the effect of in-person or hybrid modality on COVID outcomes did not substantially shift during the winter or spring (in Michigan) COVID surges.

5.3 Using District Estimates of Students Attending School Hybrid and/or in Person

The above estimates of how district instructional modality affects COVID spread are useful in assessing the implications of district-wide modality decisions. However, the relatively crude modality measures may mask variation in the degree to which public school students are physically present in schools. For instance, in Michigan districts categorized as "in-person," it is unclear what percentage of students who are offered in-person instruction take up that mode and spend time in the school building. Similarly, in Washington, the definition of "phase-in" suggests that some students are receiving instruction, at least partially, in-person, but which grades are brought back (and whether they are fully or partially in-person) is unclear.

To understand if finer-grained information about the proportion of students who are attending schools suggests a different picture than the above modality findings, we turn to data collected in each state of the proportion of students who are either in-person or in hybrid settings. Specifically, we replace the district modality measures used in Table 2 with a vector of categorical indicators for the proportion of students who are attending schools in a particular modality.[42]

In Figures 5 and 6, we report the coefficient estimates from the TWFE models for Michigan and Washington, respectively. Figures 5a, b, and c show estimates from the fully interacted models and their 95% confidence intervals for the effects of in-person and hybrid enrollment in Michigan districts on COVID cases in the surrounding counties when county-level pre-period COVID rates are at the 25th, 50th, and 75th percentiles of each state's COVID rate distributions, respectively.[43] Figure 6 provides the same estimates and confidence intervals for Washington, but with in-person and hybrid enrollment combined as the data only provide the combined enrollment rates.

There are a few cases where we see marginally significant findings,[44] but looking across the different take-up rates in both states there is little evidence of a pattern related to the proportion of students who are estimated to be in-person. Moreover, the confidence intervals for all of the take-up categories for each level of pre-existing case rates are overlapping.

6. Discussion and Conclusion

Using district- and county-level data from Michigan and Washington, we investigate how the instructional modality in public K12 schools—in-person, hybrid, or remote—in the wake of the COVID-19 pandemic influences the spread of COVID in the wider community. In Michigan, we find evidence that, averaged across the 2020-21 academic year, in-person schooling led to small but significant increases in COVID spread at moderate levels of pre-existing COVID in the surrounding community. There is no evidence that hybrid schooling impacted COVID spread.

[42] As we describe in Section 3, the Michigan data are more precise than in Washington about the percentages of students attending hybrid versus in-person schooling, but the Washington data include more categories for the percentage of students attending in-person. Washington also does not distinguish between fully in-person and hybrid attendance for this measure.

[43] The percentiles equal 8.8 cases per 100,000 residents for the 25th percentile, 14.5 per 100,000 for the 50th percentile, and 30.1 for the 75th percentile in Michigan. The values for Washington are 5.0, 8.3, and 14.9 per 100,000 residents, respectively.

[44] For instance, in Michigan (Figure 5a) when case rates are low (25th percentile) and less than 25% of students are in-person or hybrid, COVID rates decrease relative to fully remote schooling (significant at the 10% level). And, in Washington (Figure 6c), the results are statistically significant and positive when pre-existing case rates are high (75^{th} percentile).

Event study models that assess how COVID rates change over time relative to districts' first reported offer of in-person schooling suggest that, in Michigan, in-person schooling led to increases in community COVID spread in the first months after opening, but effects faded thereafter. In Washington, it appears that re-opening districts for in-person or hybrid instruction led to modest increases in community COVID spread at moderate to high (over the 40th percentile) levels of pre-existing COVID cases. Event studies in Washington are quite imprecise, but they do not suggest the same increase in COVID spread stemming from districts' initial re-opening.

Although our results suggest that there were case thresholds—about 12 to 16 cases per 100,000 in Michigan and 8 to 26 cases per 100,000 in Washington—at which in-person or hybrid schooling led to community COVID spread, we hesitate to offer specific recommendations about exact case rate thresholds. This is because, as with any econometric model, there is uncertainty in our estimates. Moreover, given sample size and associated power considerations, we are unable to rule out small changes in COVID spread that may result from decisions to offer or enroll in in-person schooling. In addition, statistical tests of significance rely on the use of arbitrary confidence thresholds, which means that we can assert with a certain degree of confidence that our results do not show significant average relationships between in-person schooling modalities and COVID spread. Ultimately, policymakers must judge the risks of spread given the evidence against other considerations, such as the learning consequences associated with particular school modalities.

We are also limited by the available data. We rely on COVID case rates at the county level as it provides us a high frequency and high variability measure of COVID incidence. Case rates, however, are not just a function of incidence but also testing. Hence if the likelihood of getting tested at a given underlying incidence varies with modality, that could bias our estimates. This concern is mitigated by the fact that unlike early in the pandemic, testing was widely available during the period we study, but we do not know the degree or ways in which testing might vary across communities. We also estimate models predicting hospitalization and death rates and find no significant effect of in-person modalities on these more distal outcomes.

Another important concern is that the COVID pandemic has hit communities of color and low-income communities much harder than others (e.g., Sandoiu, 2020). Ideally, we would be able to conduct analyses that focus on the modality impacts in these communities as they may be at higher risk of COVID spread. Unfortunately, our outcome data—which is at the county level and not broken down by racial, ethnic, or income subgroups—make this infeasible. We attempted to address this question by examining counties with large minority population shares. However, these estimates primarily rely on cross-county variation and very few counties have high enough minority populations to provide accurate evidence on this—in Michigan the county at the 75th percentile of underrepresented minority share (non-white and non-Asian) has only 12% underrepresented minorities, while in Washington there is only a 5% underrepresented minority population in counties at the 75th percentile of the distribution. Further, when we estimated models restricted to these counties with (marginally) larger shares of underrepresented minorities, our estimates are too imprecise to draw clear conclusions. Hence, we caution that our overall results may not hold in these populations, but we are unable to determine the extent to which this may or may not be the case.

Similarly, there is reason to believe that there may be differences in the likelihood of COVID spread across age groups, and that younger children may have been less likely to contract the disease than older students and adults (e.g., Dattner et al., 2021; Fontanet, Grant, et al., 2020; Fontanet, Tondeur, et al., 2020; Park et al., 2020). This might suggest that individuals of different ages would be more or less sensitive to COVID spread as a result of instructional modality. Although we attempted to estimate our models using monthly data on COVID cases broken down by age groups in each county, these data were difficult to compare over time and across states. As such, we are unable to discern if instructional modality differentially impacted younger or older school-age children or adults.

Even with these limitations, however, we believe this work can be useful to decision-makers concerned with how best to balance protecting students, school staff, and the greater community from COVID while working to ensure the academic and socioemotional well-being of children. In particular, while at the time of writing the COVID case and death rate in the United States is trending downward, internationally COVID cases are near an all-time peak. Moreover, prominent health experts believe that COVID variants combined with vaccine hesitancy make it quite unlikely that the U.S. will reach herd immunity, making future sporadic COVID outbreaks likely (Mandavilli, 2021). The evidence we present is thus likely to be relevant to communities in the future that experience outbreaks and therefore must consider the degree to which instructional modality decisions might influence COVID community spread.

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Table 1: Summary statistics for districts with modality data, average of all district-month observations, Michigan and Washington

	Michigan				Washington			
	All	In-person	Hybrid	Remote	All	In-person	Hybrid	Remote
Total districts	814				295			
Districts with some modality data	809	--	--	--	295	--	--	--
% district-months, unweighted	--	61.68	17.04	21.27	--	15.40	56.58	28.02
% district-months, weighted	--	54.29	22.28	23.43	--	3.98	45.77	50.24
County								
Percent always wears a mask	63.38	61.15	65.56	65.19	69.71	66.78	69.05	70.55
Trump vote share 2016	47.06	51.25	44.07	40.18	40.20	52.58	44.87	34.96
Share to stay at home	25.10	23.91	25.49	27.48	26.60	25.29	25.48	27.73
Vaccinations administered per 100,000	5,490.26	4,328.13	3821.12	9770.42	7,800.07	11,296.36	13,033.80	2,754.26
School to county population ratio	0.15	0.15	0.15	0.15	0.15	0.16	0.16	0.15
Nursing home residents per 100,000	320.14	337.84	298.58	299.63	175.68	209.01	181.91	167.36
65+ population per 100k	17,532.88	18,190.16	16,958.01	16,556.61	15,877.03	17,332.90	16,760.92	14,956.24
Unemployment rate, 2019	4.14	4.16	4.05	4.18	4.49	5.60	4.96	3.98
Individual poverty rate	15.08	14.53	14.75	16.66	11.70	15.33	12.49	10.69
Religious congregations per 100k	95.08	107.88	79.79	79.97	91.82	115.80	99.02	83.35
Religious adherents per 1k	424.54	423.04	422.99	429.49	348.74	363.63	347.00	349.15
Correctional facility (%)	43.08	36.41	48.20	53.67	71.92	64.08	59.40	83.94
College/University (%)	89.92	85.76	91.23	98.33	93.81	85.99	90.60	97.36
Lagged cases per 100k	22.53	20.74	18.59	49.36	10.59	17.26	12.21	8.58
District (%)								
Underrepresented minority share	31.26	24.72	28.16	49.36	37.82	24.91	36.23	40.29
Urban	24.11	13.66	26.97	45.63	39.15	39.21	34.41	43.47
Suburban/Town	58.17	60.88	61.23	48.99	53.21	33.30	55.45	52.73
Rural	17.72	25.46	11.80	5.39	7.64	27.49	10.14	3.80
Outcomes								
Cases per 100,000	26.85	28.99	27.97	20.83	17.44	21.08	18.23	16.43
	(18.64)	(19.92)	(18.78)	(13.42)	(12.67)	(16.84)	(13.40)	(11.44)
Deaths per 100,000	0.44	0.43	0.38	0.49	0.20	0.25	0.17	0.23
	(0.52)	(0.60)	(0.38)	(0.43)	(0.25)	(0.50)	(0.24)	(0.22)
Hospitalizations per 100,000	197.45	196.62	210.31	187.13	0.95	1.40	0.99	0.88
	(130.43)	(132.91)	(136.24)	(117.41)	(0.69)	(1.06)	(0.76)	(0.56)

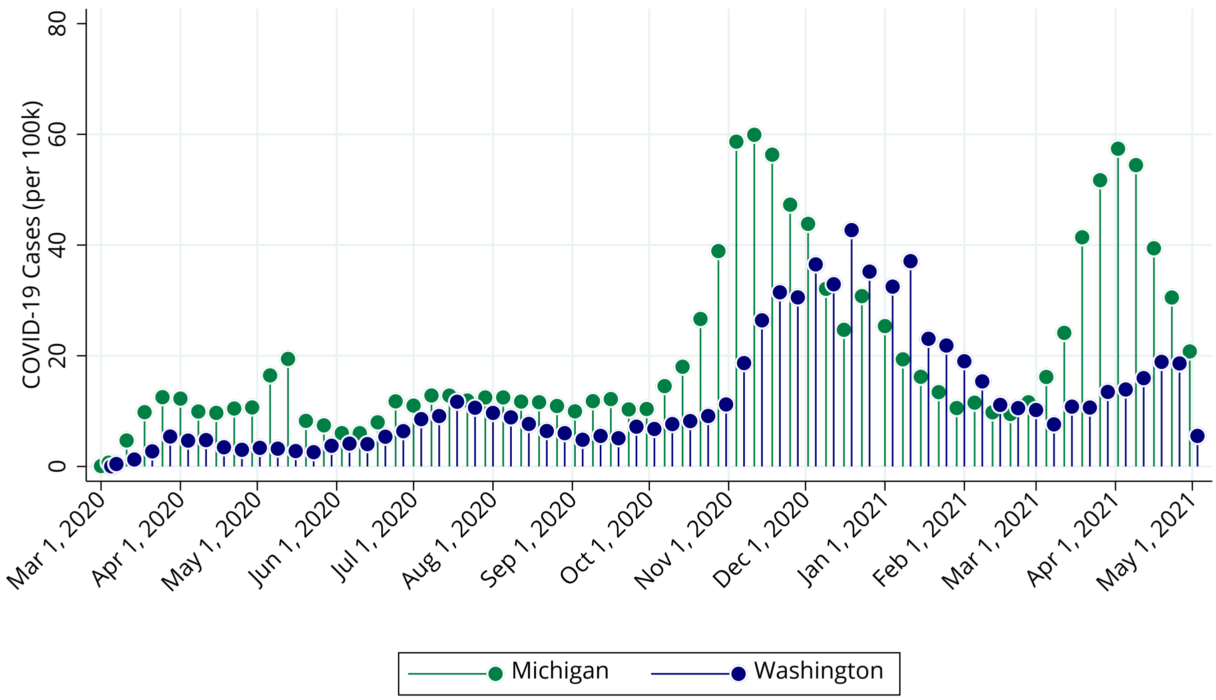
Notes: County variables are county-level characteristics assigned to districts. District variables are means of schools within districts. All county and district variable means are weighted by district size unless noted otherwise. The in-person, hybrid, and remote modalities are mutually exclusive. Outcomes include standard deviations in parentheses. COVID-19 hospitalization data in Michigan are only available by Healthcare Coalition region. Each region contains between 3 and 19 Michigan counties. These region-level variables are assigned to districts.

Table 2. The impact of modality on county COVID-19 cases per 100,000 residents, Michigan and Washington

	Michigan		Washington	
	Month FE	Month and District FE	Month FE	Month and District FE
	(1)	(2)	(3)	(4)
Panel A: Baseline OLS models				
In-Person	1.4538	1.5655	1.3839	-0.9870
	(1.0702)	(1.6551)	(3.6247)	(4.4551)
Hybrid	2.0607	1.3467	-3.2907	-2.8054
	(1.2511)	(1.5673)	(3.4714)	(4.1911)
In-Person* Prior month cases	-0.0125	0.0093	0.2964	0.5054
	(0.0791)	(0.1102)	(0.5303)	(0.6279)
Hybrid* Prior month cases	-0.0954	-0.0703	1.0339*	1.2051*
	(0.0701)	(0.0752)	(0.4367)	(0.4694)
In-Person* Prior month cases squared	-0.0002	-0.0005	-0.0048	-0.0088
	(0.0007)	(0.0010)	(0.0112)	(0.0139)
Hybrid* Prior month cases squared	0.0010	0.0007	-0.0233**	-0.0282**
	(0.0009)	(0.0010)	(0.0084)	(0.0091)
Prior month cases	-0.0726	-0.2578	-1.1781	-1.2370
	(0.1340)	(0.1916)	(0.7715)	(0.8367)
Prior month cases squared	0.0007	0.0020	0.0206	0.0210
	(0.0009)	(0.0013)	(0.0124)	(0.0135)
Share to stay at home	-0.9767**	-3.1484**	-0.0512	1.3056
	(0.2951)	(1.1301)	(0.4339)	(1.1590)
Vaccinations administered per 100,000	0.0000	0.0002	-0.0001	-0.0001
	(0.0002)	(0.0003)	(0.0001)	(0.0001)
Observations	6439	6439	1649	1649
R^2	0.759	0.784	0.661	0.703
	Michigan		**Washington**	
	Month and District FE	de Chaisemartin & d'Haultfoeuille (2020)	Month and District FE	de Chaisemartin & d'Haultfoeuille (2020)
	(1)	(2)	(3)	(4)
Panel B: TWFE correction check				
In-person or hybrid	0.5932	-0.1417	5.2363*	2.5595
	(0.7216)	(0.9905)	(2.3388)	(2.6868)
Prior month cases	-0.2491^{+}	-	-0.5689	-
	(0.1493)	-	(0.6623)	-
Prior month cases squared	0.0016	-	0.0062	-
	(0.0011)	-	(0.0105)	-
Share to stay at home	-3.1610**	-	1.1492	-
	(1.1284)	-	(1.3102)	-
Vaccinations administered per 100,000	0.0002	-	-0.0001	-
	(0.0003)	-	(0.0001)	-
Placebo test	-	-1.9868	-	0.5534
	-	(1.1482)	-	(1.9838)
Observations	6439	6439	1649	1649
R^2	0.784	--	0.690	--

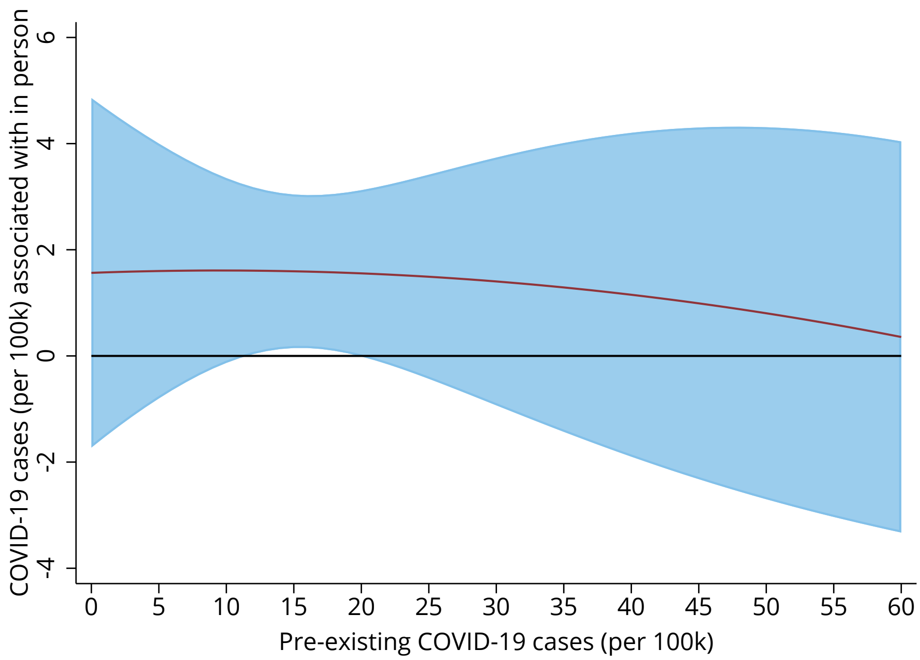
Notes: Robust standard errors clustered at the county level are in parentheses. Estimates represent the impact of district modality in month t on county 7-day average COVID-19 cases per 100,000 residents on the 1st of month $t+1$. All regressions are weighted by district size. September and January modalities are not included for Washington. The month fixed effects models in panel A also control for county share to always wear a mask, Trump 2016 vote share, school to county population ratio, nursing home residential population, population over the age of 65, 2019 unemployment rate, individual poverty rate, religious congregations per 100,000 residents, religious adherents per 1,000 residents, indicators for correctional facility or college/university in a county, underrepresented minority population share, and urbanicity. Refer to Appendix Table A3 for the full regression output. The (Kaufman & Diliberti, 2021) models in panel B also include controls for prior month cases, prior month cases squared, share to stay at home, and vaccinations administered per 100,000 residents. The placebo tests in the de Chaisemartin and D'Haultfœuille (2020) models test for the existence of a hypothetical treatment effect the month prior to a district first offering in-person or hybrid instruction. For the de Chaisemartin and D'Haultfœuille (2020) models, the main effect in columns (2) and (4) are calculated from 913 and 273 switchers, respectively. Similarly, the placebo effect is calculated from 621 and 173 switchers, respectively.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 1: Statewide trends in county COVID-19 cases per 100,000 residents, Michigan and Washington

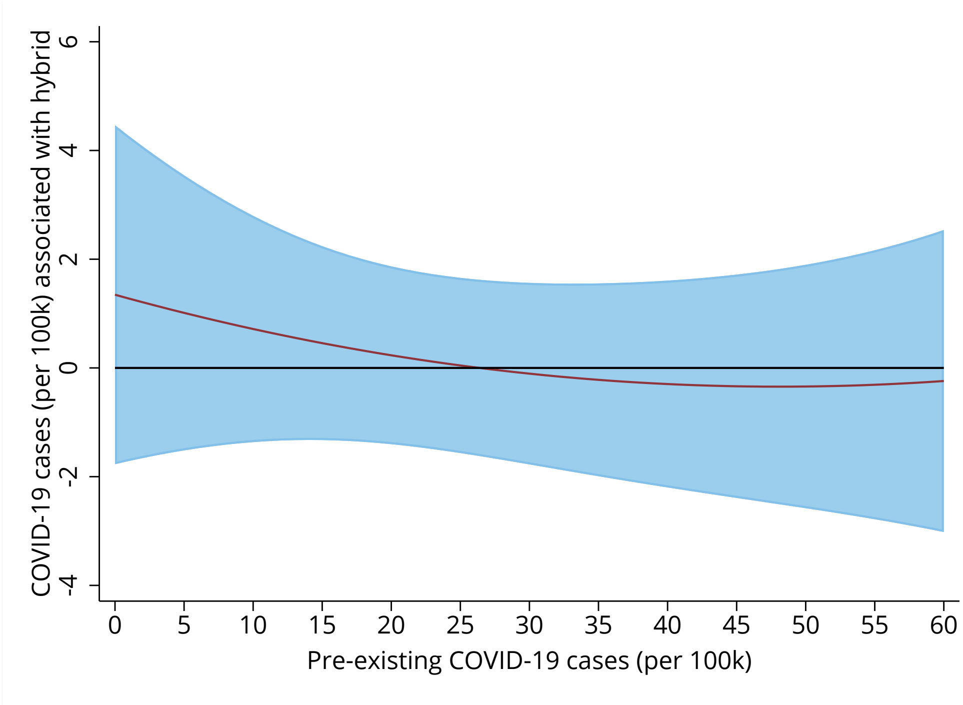


Notes: Marker heights represent seven-day average daily case rate per 100,000 residents by week.

Figure 2a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 cases per 100,000 residents by pre-existing case rates, Michigan



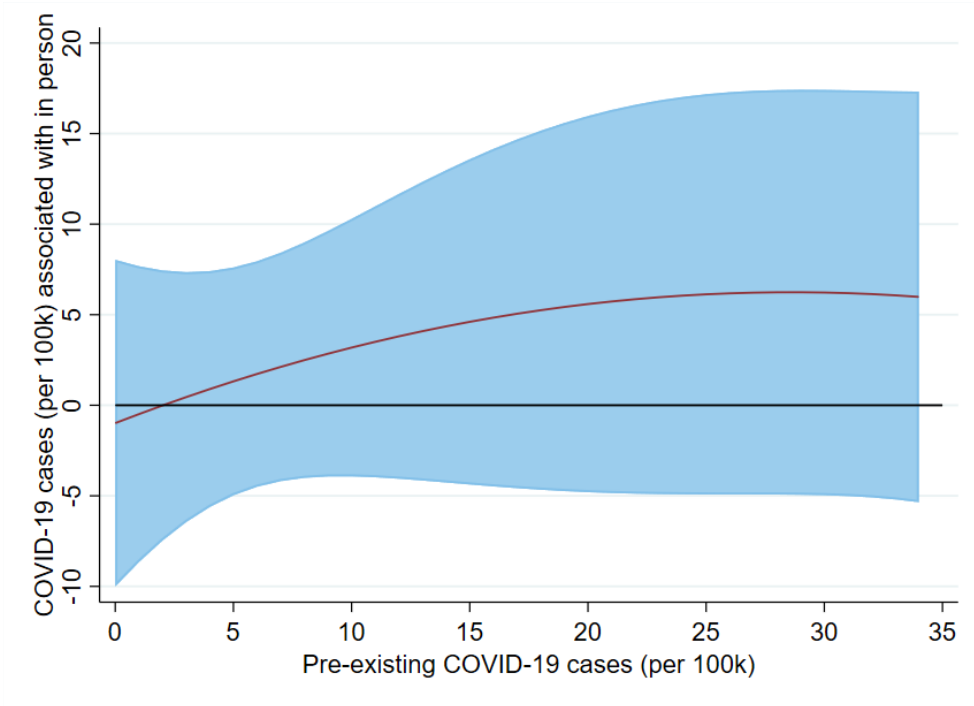
a) Impact if all districts in county switch from remote to in-person



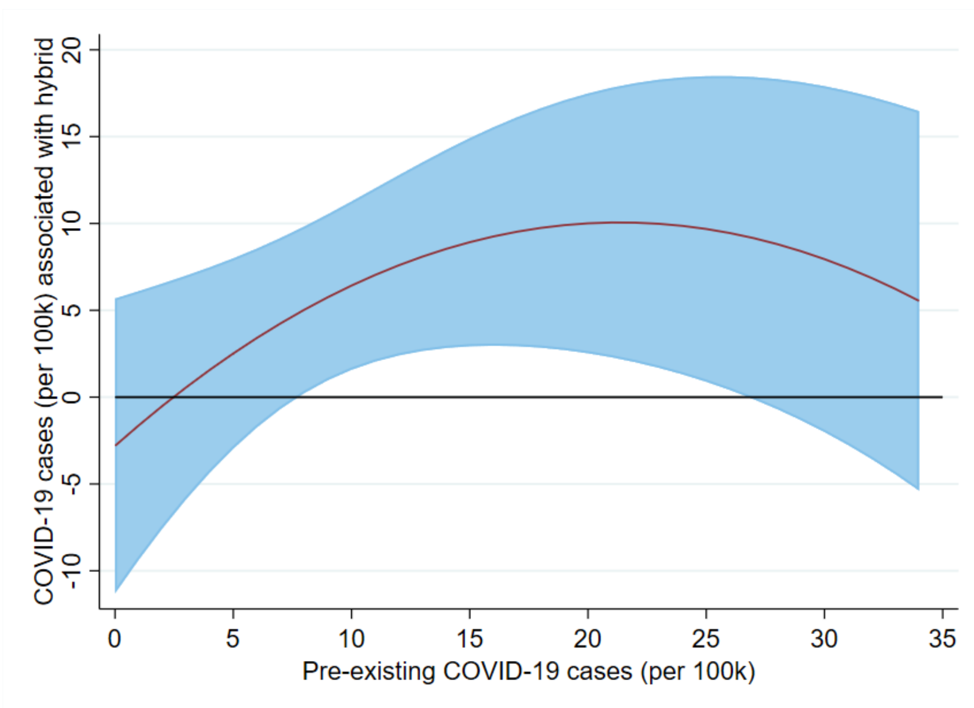
b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated using the Michigan month and district fixed effect model in Table 2, where school modality is interacted with a quadratic function of COVID-19 case rates in the prior month.

Figure 3a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 cases per 100,000 residents by pre-existing case rates, Washington



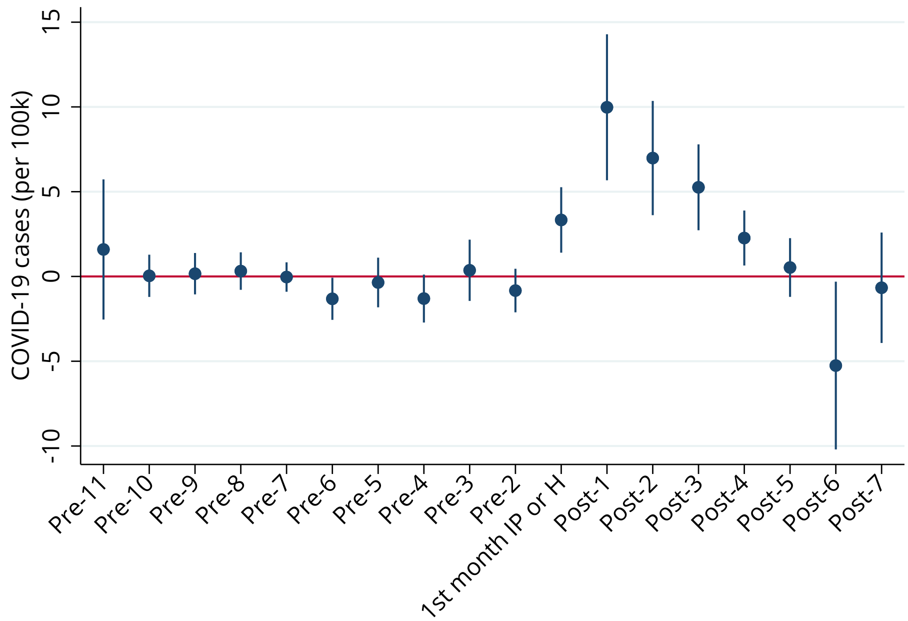
a) Impact if all districts in county switch from remote to in-person



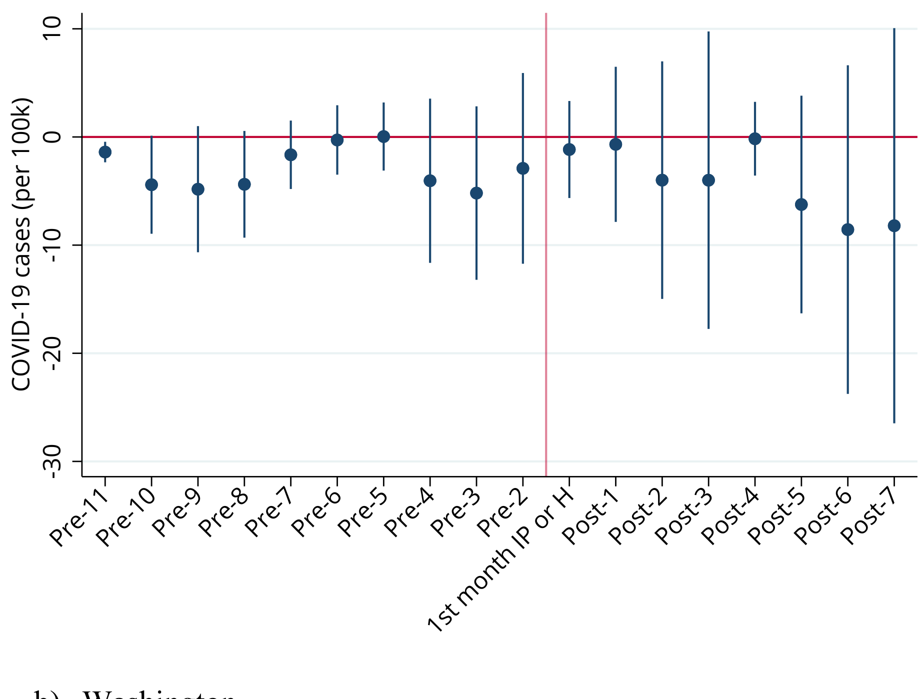
b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated using the Washington month and district fixed effect model in Table 2, where school modality is interacted with a quadratic function of COVID-19 case rates in the prior month.

Figure 4a, b: Sun and Abraham (2020) event studies of the impact of in-person or hybrid modality on COVID-19 cases per 100,000 residents, Michigan and Washington



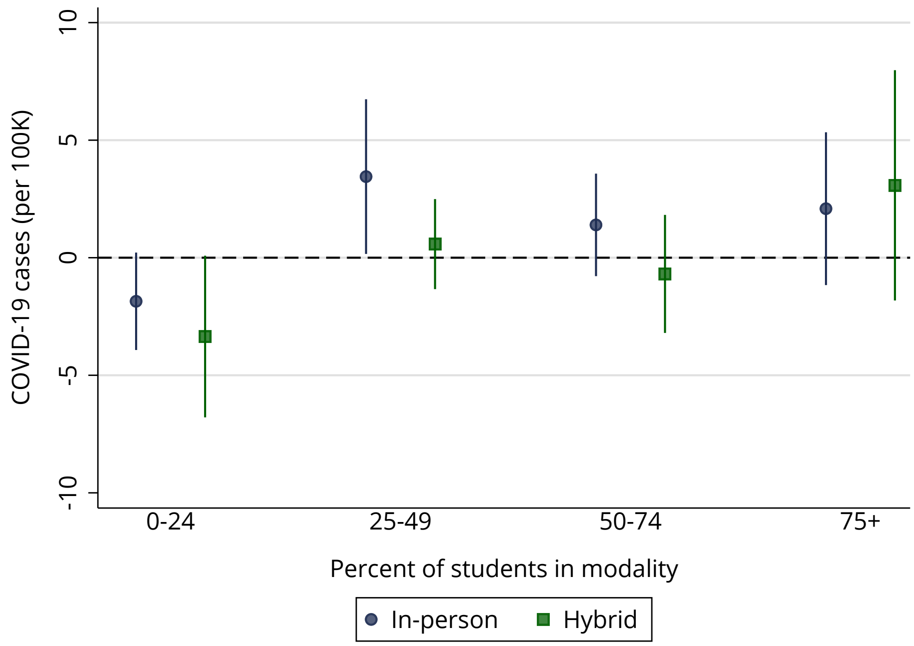
a) Michigan



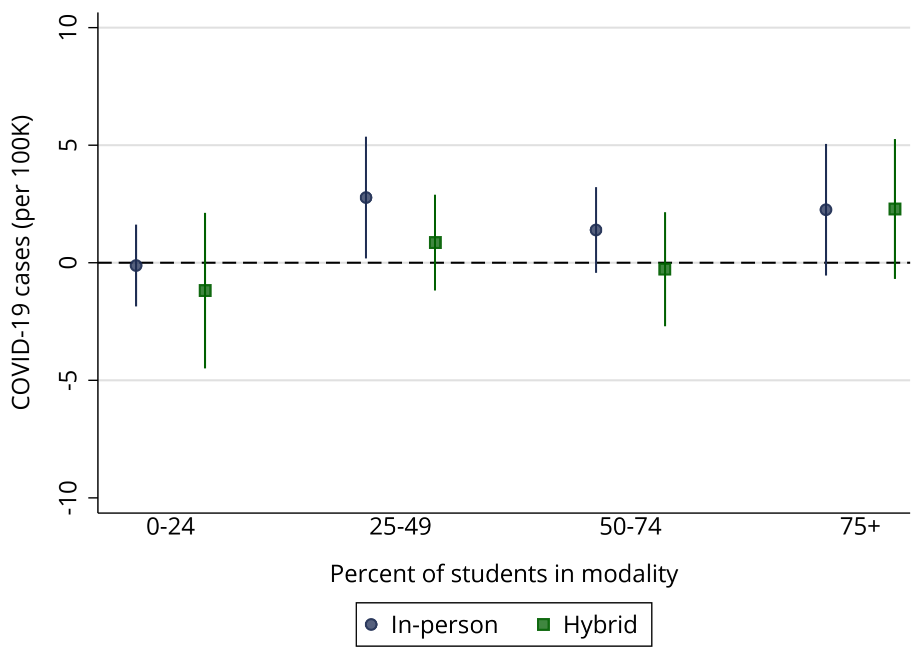
b) Washington

Notes: District modality is set as remote prior to September. If the modality changes to in-person or hybrid in any month between September 2020 and April 2021, the district is treated as in-person or hybrid for the remainder of the time series. Washington data for September are not available, hence we impute values using October modality. Washington modality data for January was collected on January 18, 2021, rather than December 31, 2020.

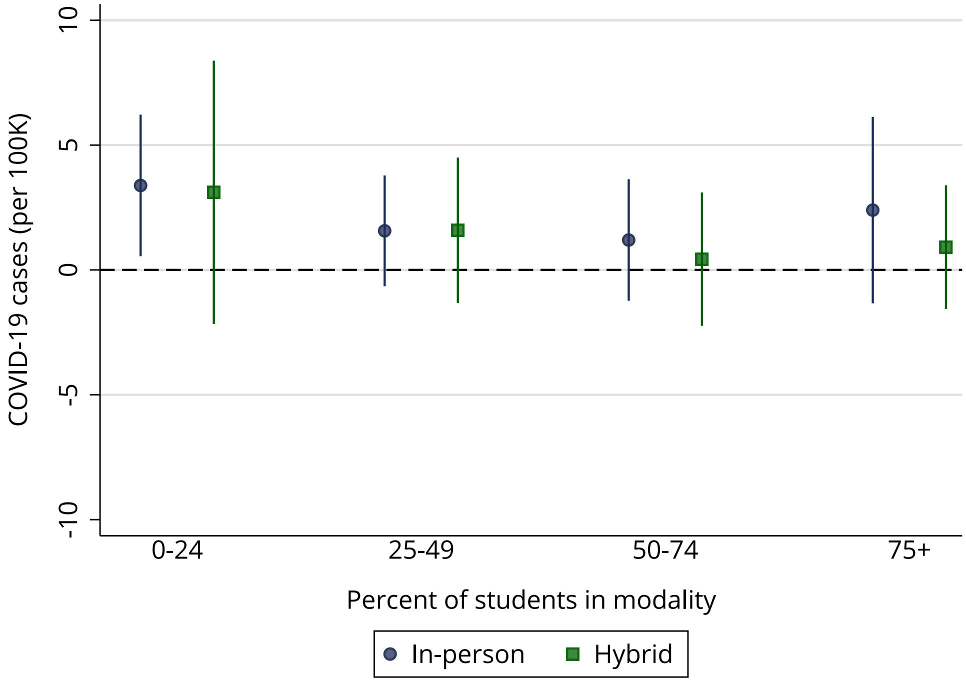
Figure 5a-c: Month and district fixed effect estimates of the impact of modality uptake on county COVID-19 cases per 100,000 residents by pre-existing COVID-19 case rates, Michigan



a) 25th percentile of pre-existing COVID-19 rates



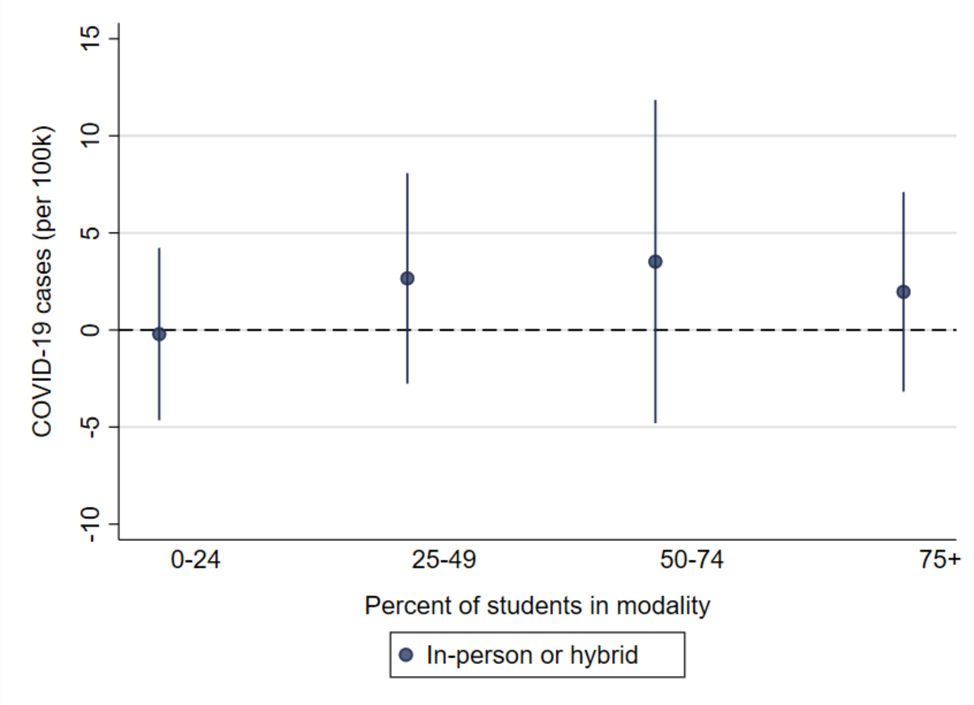
b) 50th percentile of pre-existing COVID-19 rates



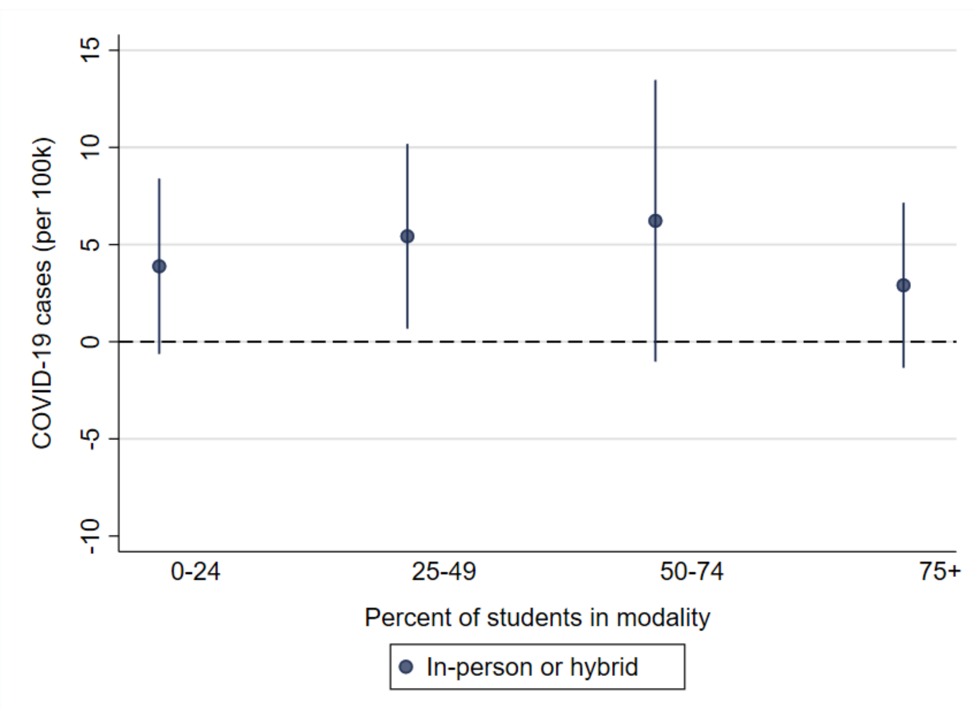
c) 75th percentile of pre-existing COVID-19 rates

Notes: Estimates from month and district fixed effect models interacting district modality measures with a vector of categorical indicators for the percent of students who are attending schools in a particular modality (represented by x-axis labels). Markers represent point estimates for in-person and hybrid, respectively, and spikes represent 95% confidence intervals. Panels are for the 25th, 50th, and 75th percentile of pre-existing COVID case rates. In Michigan, the 25th, 50th, and 75th percentiles represent 8.81, 14.45, and 30.13 pre-existing COVID cases per 100,000. Refer to Appendix Table A4 for the full regression output.

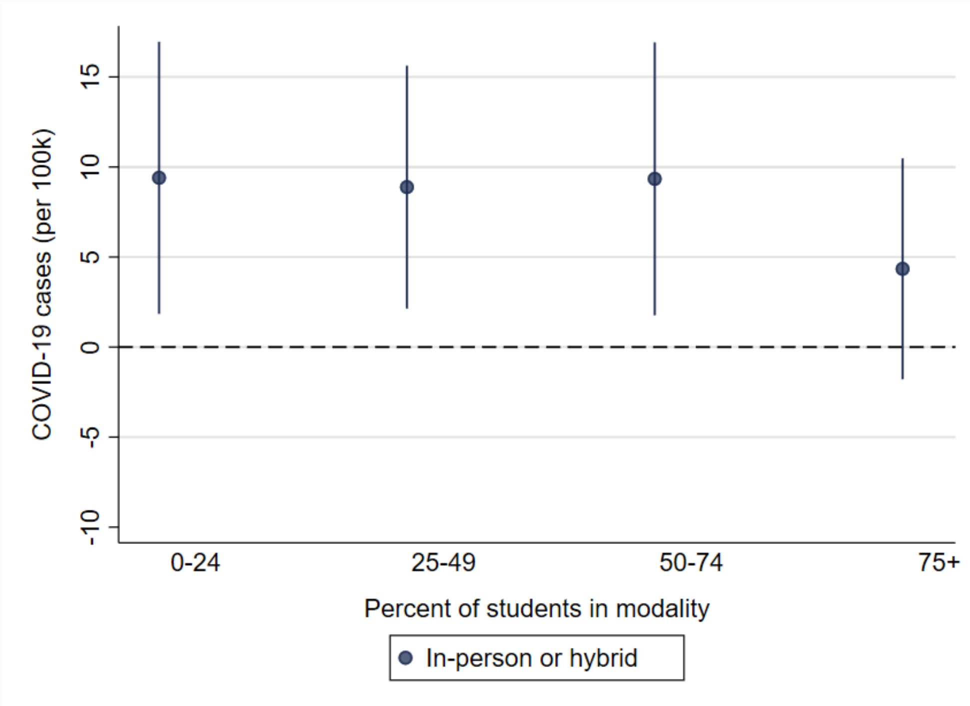
Figure 6a-c: Month and district fixed effect estimates of the impact of modality uptake on county COVID-19 cases per 100,000 residents by pre-existing COVID-19 case rates, Washington



a) 25th percentile of pre-existing COVID-19 rates



b) 50th percentile of pre-existing COVID-19 rates



c) 75th percentile of pre-existing COVID-19 rates

Notes: Estimates from month and district fixed effect models interacting district modality measures with a vector of categorical indicators for the percent of students who are attending schools in a particular modality (represented by x-axis labels). Markers represent point estimates for in-person and hybrid, respectively, and spikes represent 95% confidence intervals. Panels are for the 25th, 50th, and 75th percentile of pre-existing COVID case rates. In Washington, the 25th, 50th, and 75th percentiles represent 4.86, 8.31, and 14.93 pre-existing COVID cases per 100,000. Refer to Appendix Table A4 for the full regression output.

APPENDIX

Table A1. Transition matrix showing changes in district modality between month *t* and *t+1*, all months, Michigan and Washington

		Michigan (month *t+1*)					**Washington (month *t+1*)**				
		In-person	Hybrid	Remote	No data	Total	In-person	Hybrid	Remote	No data	Total
Month *t*	In-person	55.44	0.37	3.28	0.09	59.18	10.78	1.29	0.34	0.75	13.15
		(3,159)	(21)	(187)	(5)	(3,372)	(159)	(19)	(5)	(11)	(194)
	Hybrid	1.56	12.99	1.98	0.02	16.55	2.92	44.68	1.56	2.37	51.53
		(89)	(740)	(113)	(1)	(943)	(43)	(659)	(23)	(35)	(760)
	Remote	4.37	3.32	15.41	0.07	23.17	0.75	9.29	15.46	2.10	27.59
		(249)	(189)	(878)	(4)	(1,320)	(11)	(137)	(228)	(31)	(407)
	No data	0.05	0.05	0.02	0.98	1.11	0.95	2.78	1.63	2.37	7.73
		(3)	(3)	(1)	(56)	(63)	(14)	(41)	(24)	(35)	(114)
	Total	61.43	16.73	20.69	1.16	100.00	15.39	58.03	18.98	7.59	100.00
		(3,500)	(953)	(1,179)	(66)	(5,698)	(227)	(856)	(280)	(112)	(1,475)

Notes: Rows represent modality in month, *t*, and columns represent modality in month, *t+1*. Row-column entries represent the percentage of all district-month observations where districts either maintained or switched modalities between months *t* and *t+1*. Entries in parenthesis represent the total number of district-month observations where districts either maintained or switched modalities between months *t* and *t+1*. Month *t* represent any month in the sample between September and March in Michigan or October and March in Washington. Month *t+1* represents any month in the sample between October and April in Michigan and November and April in Washington. September and January modalities are not included for Washington.

Table A2: Summary statistics for districts with no modality data, average of all district-month observations, Michigan and Washington

	Michigan			**Washington**		
	No data	Analytic Sample	Difference (p value)	No data	Analytic Sample	Difference (p value)
N districts	16	809	--	81	295	--
County						
Percent always wears a mask	65.54	63.38	0.078	71.38	69.71	0.003
Trump vote share 2016	41.43	47.06	0.002	38.99	40.20	0.361
Share to stay at home	26.93	25.10	0.001	26.78	26.60	0.533
Vaccinations administered per 100,000	5,636.82	5,490.26	0.922	4,370.85	7,800.07	0.003
School to county population ratio	0.15	0.15	0.471	0.16	0.15	0.001
Nursing home residents per 100,000	359.84	320.14	0.023	150.84	175.68	0.001
65+ population per 100k	18,643.39	17,532.88	0.011	14,241.70	15,877.03	0.000
Unemployment rate, 2019	4.93	4.14	0.000	4.49	4.49	0.976
Individual poverty rate	18.95	15.08	0.000	12.15	11.70	0.165
Religious congregations per 100k	96.90	95.08	0.765	92.11	91.82	0.923
Religious adherents per 1k	414.43	424.54	0.402	364.17	348.74	0.002
Correctional facility (%)	70.66	43.08	0.000	77.71	71.92	0.197
College/University (%)	83.58	89.92	0.164	96.94	93.81	0.188
Lagged cases per 100k	22.72	22.53	0.949	11.92	10.59	0.107
District (%)						
Underrepresented minority share	43.66	31.26	0.005	42.58	37.82	0.009
Urban	12.08	24.11	0.062	26.84	39.15	0.011
Suburban/Town	75.34	58.17	0.020	65.77	53.21	0.012
Rural	12.58	17.72	0.374	7.39	7.64	0.925
Outcomes						
Cases per 100,000	24.20 (18.28)	26.85 (18.64)	0.347	25.22 (15.28)	17.44 (12.67)	0.000
Deaths per 100,000	0.50 (0.61)	0.44 (0.52)	0.377	0.28 (0.24)	0.20 (0.25)	0.001
Hospitalizations per 100,000	200.27 (140.75)	197.45 (130.43)	0.886	1.28 (0.66)	0.95 (0.69)	0.000

Notes: County variables are county-level characteristics assigned to districts. District variables are means of schools within districts. All county and district variable means are weighted by district size. Outcomes include standard deviations in parentheses. COVID-19 hospitalization data in Michigan are only available by Healthcare Coalition region. Each region contains between 3 and 19 Michigan counties. These region-level variables are assigned to districts

Table A3. The impact of modality on county COVID-19 cases per 100,000 residents, Michigan and Washington

	Michigan				**Washington**			
	Month FE	Month and District FE	Month FE	Month and District FE	Month and District FE	de Chaisemartin & d'Haultfoeuille (2020)	Month and District FE	de Chaisemartin & d'Haultfoeuille (2020)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
In-Person	1.4538	1.5655	1.3839	-0.9870				
	(1.0702)	(1.6551)	(3.6247)	(4.4551)				
Hybrid	2.0607	1.3467	-3.2907	-2.8054				
	(1.2511)	(1.5673)	(3.4714)	(4.1911)				
In-person or hybrid					0.5932	-0.1417	5.2363*	2.559452
					(0.7216)	(0.9905)	(2.3388)	(1.983797)
Prior month cases	-0.0726	-0.2578	-1.1781	-1.2370	-0.2491^{+}		-0.5689	
	(0.1340)	(0.1916)	(0.7715)	(0.8367)	(0.1493)		(0.6623)	
Prior month cases squared	0.0007	0.0020	0.0206	0.0210	0.0016		0.0062	
	(0.0009)	(0.0013)	(0.0124)	(0.0135)	(0.0011)		(0.0105)	
In-Person* Prior month cases	-0.0125	0.0093	0.2964	0.5054				
	(0.0791)	(0.1102)	(0.5303)	(0.6279)				
Hybrid* Prior month cases	-0.0954	-0.0703	1.0339*	1.2051*				
	(0.0701)	(0.0752)	(0.4367)	(0.4694)				
In-Person* Prior month cases squared	-0.0002	-0.0005	-0.0048	-0.0088				
	(0.0007)	(0.0010)	(0.0112)	(0.0139)				
Hybrid* Prior month cases squared	0.0010	0.0007	-0.0233**	-0.0282**				
	(0.0009)	(0.0010)	(0.0084)	(0.0091)				
Share to stay at home	-0.9767**	-3.1484**	-0.0512	1.3056	-3.1610**		1.1492	
	(0.2951)	(1.1301)	(0.4339)	(1.1590)	(1.1284)		(1.3102)	
Vaccinations administered per 100,000	0.0000	0.0002	-0.0001	-0.0001	0.0002		-0.0001	
	(0.0002)	(0.0003)	(0.0001)	(0.0001)	(0.0003)		(0.0001)	
Percent always wears a mask	-0.1408**		0.2754*					
	(0.0533)		(0.1322)					
Trump vote share 2016	0.2169*		0.3101***					
	(0.0860)		(0.0859)					
School to county population ratio	17.4922		-1.1096**					
	(21.6792)		(0.3247)					
Nursing home residents per 100,000	0.0062^{+}		0.0156*					
	(0.0033)		(0.0058)					
65+ population per 100k	-0.0004		-0.0011**					
	(0.0002)		(0.0004)					
Unemployment rate, 2019	-0.3432		2.2585*					
	(0.7999)		(1.0000)					
Individual poverty rate	0.2646^{+}		0.1948					
	(0.1519)		(0.2784)					
Religious congregations per 100k	-0.0522**		0.0058					
	(0.0191)		(0.0213)					
Religious adherents per 1k	0.0286***		0.0828***					
	(0.0050)		(0.0188)					
Correctional facility	1.2618		5.9557***					
	(1.0971)		(1.5126)					
College/University	0.5497		-4.7899^{+}					
	(1.1904)		(2.6108)					
Underrepresented minority share	0.0069		0.0118					
	(0.0047)		(0.0167)					
Urban	0.2134		-0.3698					
	(0.2345)		(0.2959)					
Town	-2.0788**		-0.8615					
	(0.7402)		(0.6769)					
Rural	-1.0008^{+}		-0.9979					
	(0.5286)		(0.6282)					
Placebo test						-1.9868		0.8706399
						(1.1482)		(1.599048)
Observations	6439	6439	1649	1649	6439	6439	1649	1649
R^2	0.759	0.784	0.661	0.703	0.784		0.690	

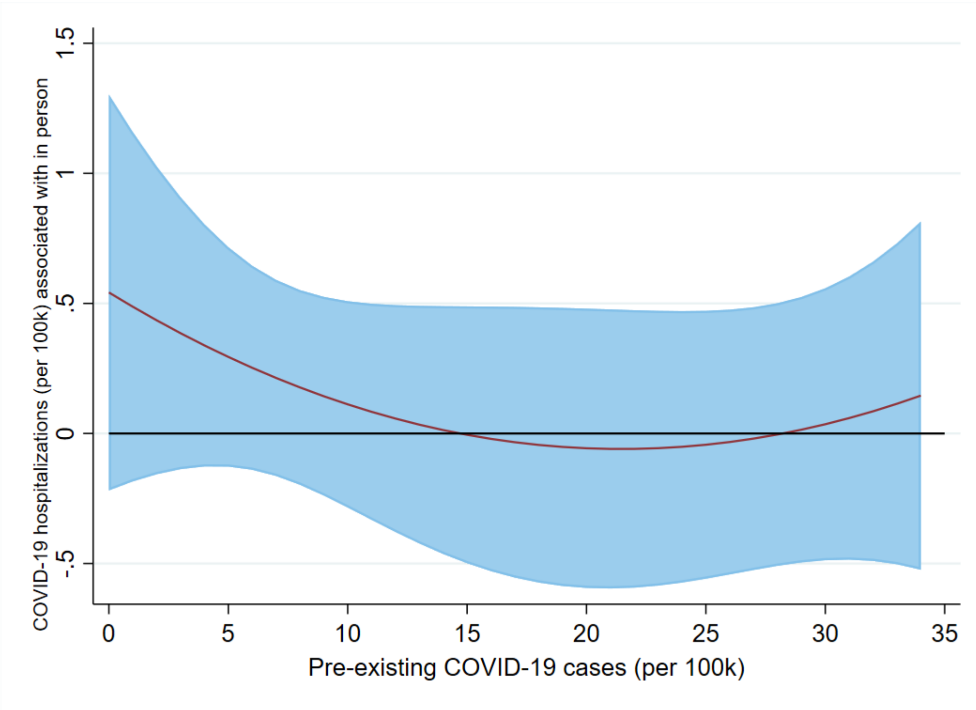
Notes: Robust standard errors clustered at the county level are in parentheses. Estimates represent the impact of district modality in month *t* on county 7-day average COVID-19 cases per 100,000 residents on the 1st of month *t+1*. All regressions include month fixed effects (November is the reference category) and are weighted by district size. *Share to stay at home* and *vaccinations administered per 100,000* are both time varying. September and January modalities are not included for Washington. The de Chaisemartin and D'Haultfœuille (2020) models in panel B also include controls for *prior month cases, prior month cases squared, share to stay at home, and vaccinations administered per 100,000*. The placebo tests in de Chaisemartin and D'Haultfœuille (2020) models test for the existence of a hypothetical treatment effect in months prior to a district first offering in-person or hybrid instruction. For the de Chaisemartin and D'Haultfœuille (2020) models, the main effect is calculated from 913 and 273 switchers in Michigan and Washington, respectively. Similarly, the placebo effect is calculated from 621 and 173 switchers, respectively. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4. The impact of modality uptake on county COVID-19 cases per 100,000 residents, Michigan and Washington

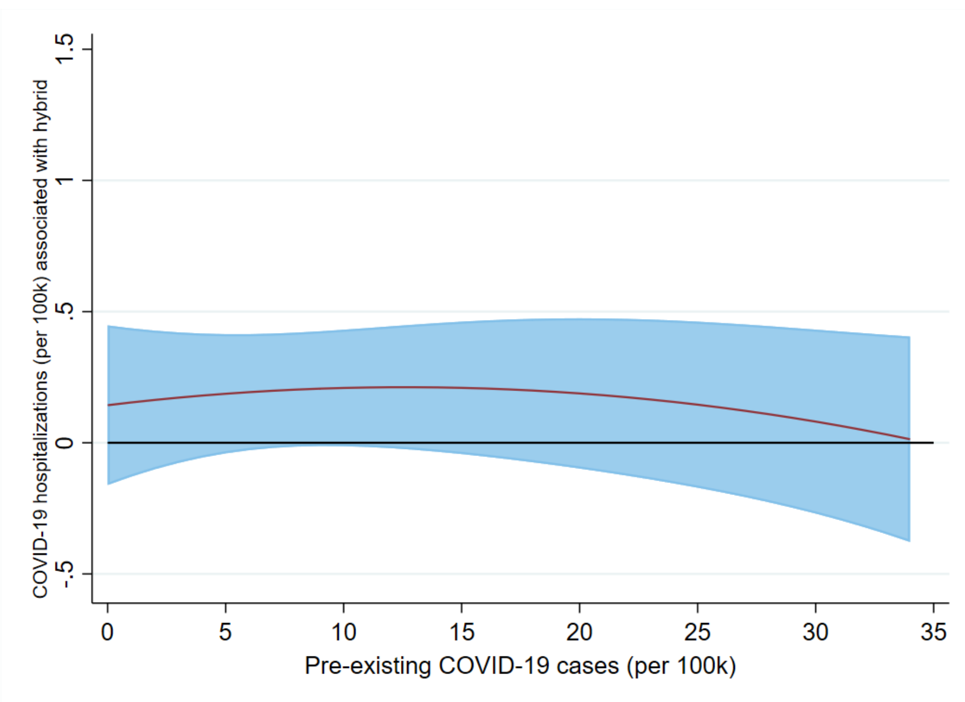
	Michigan Month and District FE	**Washington** Month and District FE
In-Person 0-24%	-5.0577**	-7.4084
	(1.8259)	(4.9405)
In-Person 25-49%	4.7625+	-2.3707
	(2.4861)	(4.3821)
In-Person 50-74%	1.3269	-1.5489
	(1.7288)	(5.6542)
In-Person 75-100%	1.6963	0.4213
	(2.4851)	(4.5212)
Hybrid 0-24%	-7.3860**	
	(2.7104)	
Hybrid 25-49%	0.1305	
	(1.1654)	
Hybrid 50-74%	-1.5001	
	(1.5444)	
Hybrid 75-100%	4.6304	
	(4.4986)	
Prior month cases*In-Person 0-24%	0.3986**	1.6519+
	(0.1303)	(0.8772)
Prior month cases*In-Person 25-49%	-0.1668	1.1702*
	(0.1250)	(0.5415)
Prior month cases*In-Person 50-74%	0.0128	1.1946*
	(0.1065)	(0.4912)
Prior month cases*In-Person 75-100%	0.0529	0.3445
	(0.1493)	(0.5907)
Prior month cases*Hybrid 0-24%	0.5034*	
	(0.2027)	
Prior month cases*Hybrid 25-49%	0.0520	
	(0.0786)	
Prior month cases*Hybrid 50-74%	0.1037	
	(0.0854)	
Prior month cases*Hybrid 75-100%	-0.1979	
	(0.2694)	
Prior month cases squared*In-Person 0-24%	-0.0039**	-0.0352
	(0.0013)	(0.0252)
Prior month cases squared*In-Person 25-49%	0.0020	-0.0279**
	(0.0015)	(0.0100)
Prior month cases squared*In-Person 50-74%	-0.0006	-0.0312**
	(0.0010)	(0.0101)
Prior month cases squared*In-Person 75-100%	-0.0010	-0.0055
	(0.0013)	(0.0120)
Prior month cases squared*Hybrid 0-24%	-0.0051*	
	(0.0020)	
Prior month cases squared*Hybrid 25-49%	-0.0001	
	(0.0009)	
Prior month cases squared*Hybrid 50-74%	-0.0013	
	(0.0010)	
Prior month cases squared*Hybrid 75-100%	0.0025	
	(0.0032)	
Prior month cases	-0.2871	-1.1647
	(0.1932)	(0.8221)
Prior month cases squared	0.0024+	0.0196
	(0.0014)	(0.0135)
Share to stay at home	-3.1719**	1.5121
	(1.1240)	(1.0660)
Vaccinations administered per 100,000	0.0001	-0.0002
	(0.0003)	(0.0001)
Observations	6439	1643
R^2	0.787	0.707

Notes: Robust standard errors clustered at the county level are in parentheses. Estimates represent the impact of district modality in month *t* on county COVID-19 positivity rate on the 1st of month *t+1.* All regressions include month fixed effects (November is the reference category) and are weighted by district size. *Share to stay at home* and *vaccinations administered per 100,000* are both time varying. September and January modalities are not included for Washington. The de Chaisemartin and D'Haultfœuille (2020) models in panel B also include controls for prior month cases, prior month cases squared, share to stay at home, and vaccinations administered per 100,000. The placebo tests in the de Chaisemartin and D'Haultfœuille (2020) models test for the existence of a hypothetical treatment effect in months prior to a district first offering in-person or hybrid instruction. For the de Chaisemartin and D'Haultfœuille (2020) models, the main effect is calculated from 913 and 273 switchers in Michigan and Washington, respectively. Similarly, the placebo effect is calculated from 621 and 173 switchers, respectively.
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A1a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 *hospitalizations* per 100,000 residents by pre-existing case rates, Washington



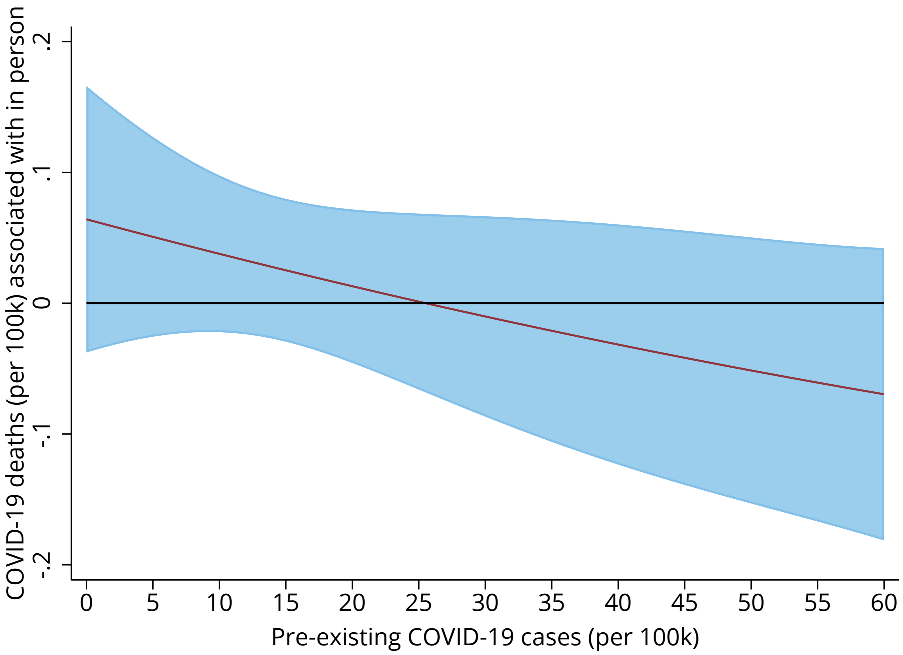
a) Impact if all districts in county switch from remote to in-person



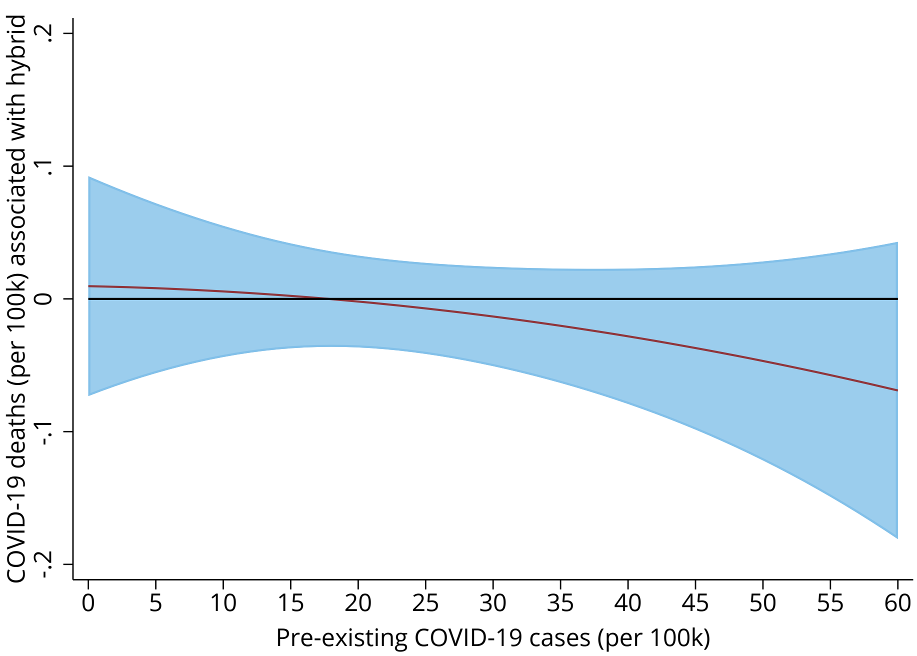
b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated using the Washington month and district fixed effect model where school modality is interacted with a quadratic function of COVID-19 case rates in the prior month.

Figure A2a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 *deaths* per 100,000 residents by pre-existing case rates, Michigan



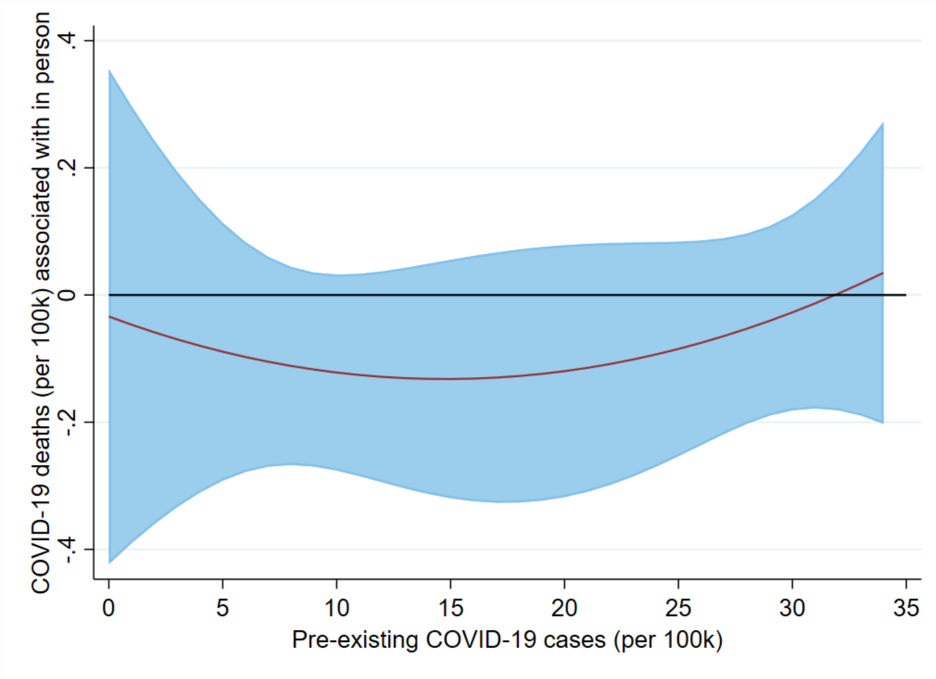
a) Impact if all districts in county switch from remote to in-person



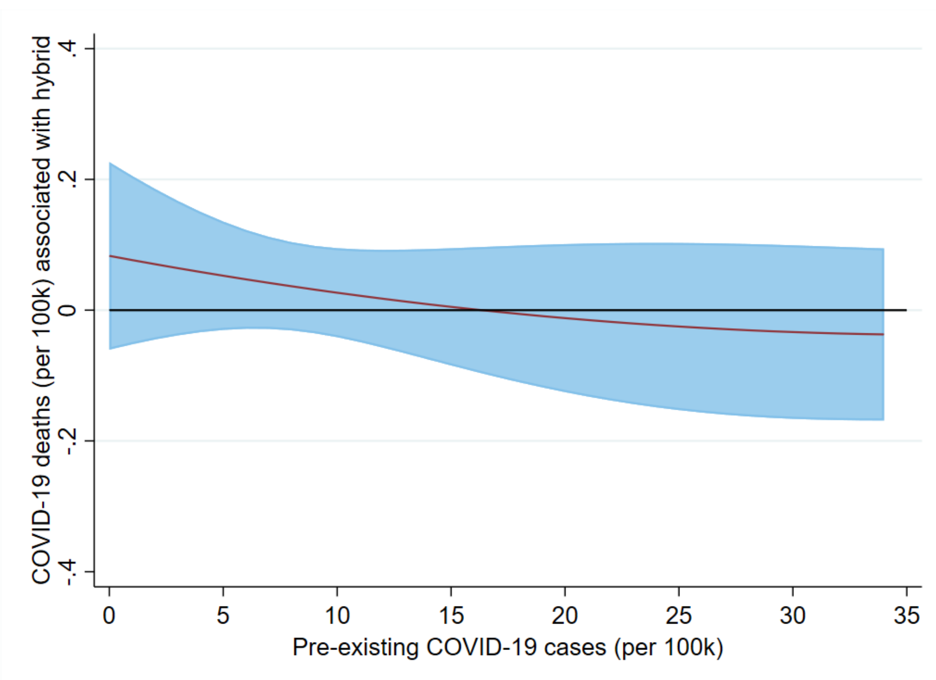
b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated using the Michigan month and district fixed effect model where school modality is interacted with a quadratic function of COVID-19 case rates in the prior month.

Figure A3a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 *deaths* per 100,000 residents by pre-existing case rates, Washington



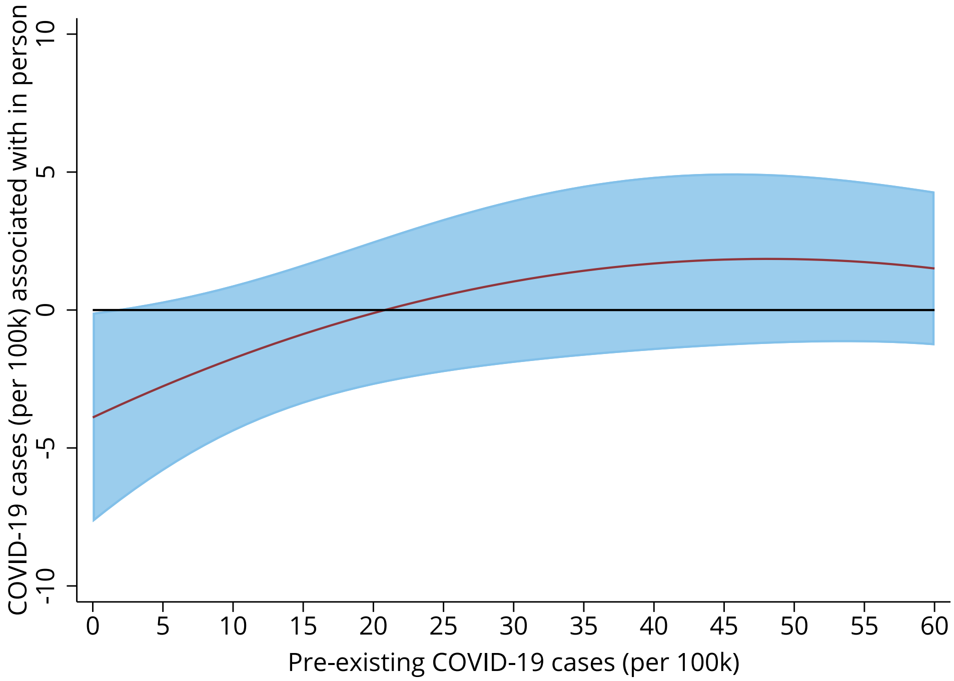
a) Impact if all districts in county switch from remote to in-person



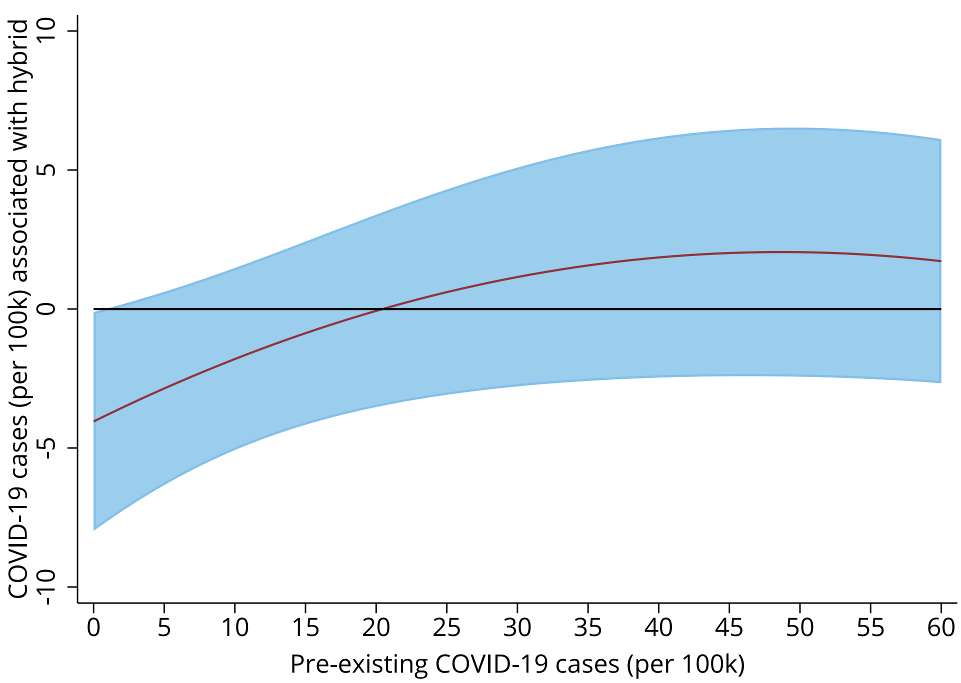
b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated using the Washington month and district fixed effect model where school modality is interacted with a quadratic function of COVID-19 case rates in the prior month.

Figure A4a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 *cases* per 100,000 residents by pre-existing case rates, fall semester only, Michigan



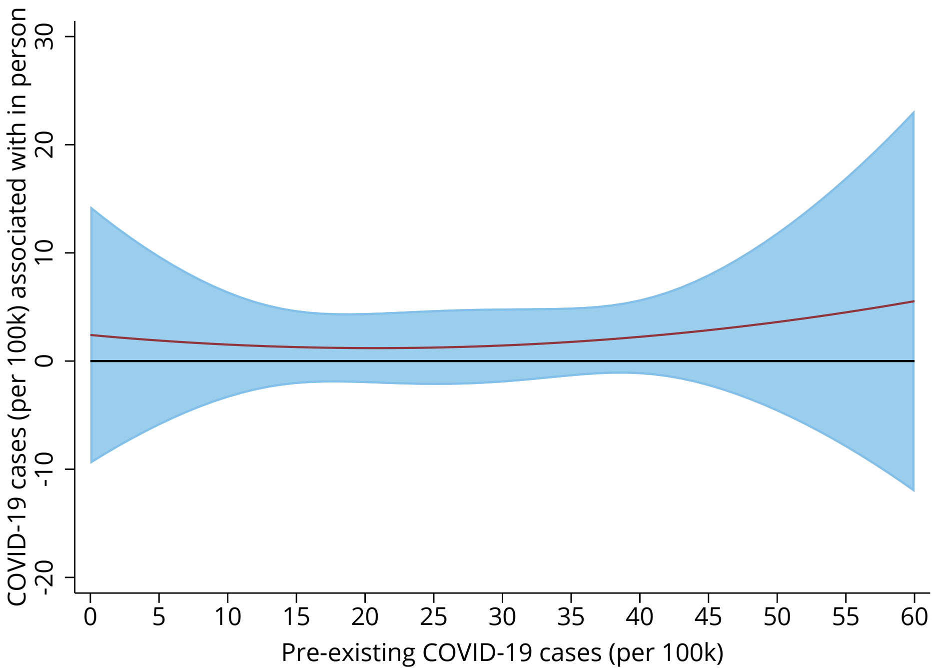
a) Impact if all districts in county switch from remote to in-person



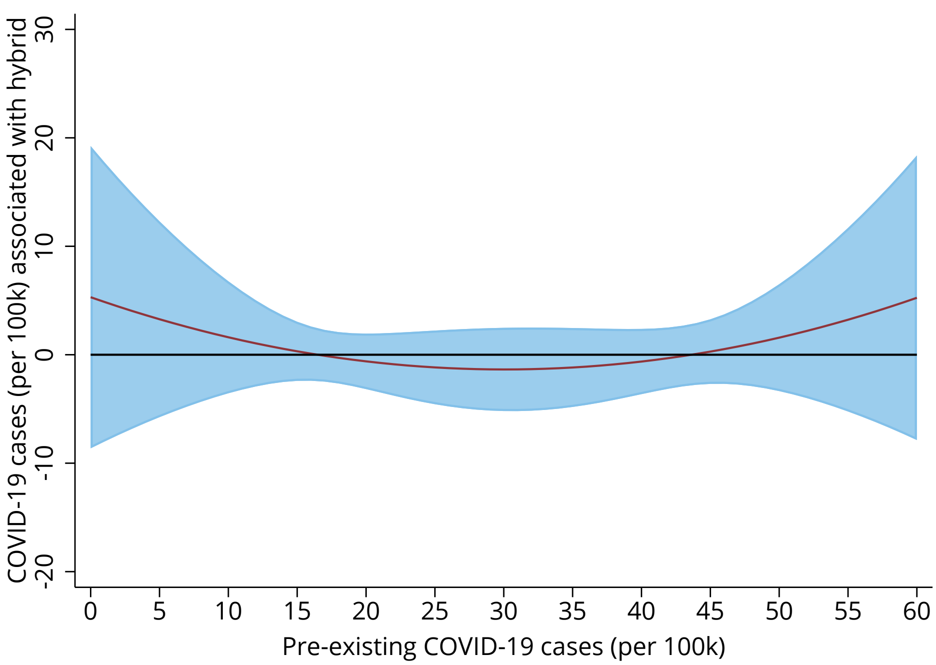
b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated by excluding January through April modality data and re-estimating the Michigan month and district fixed effect model in Table 2.

Figure A5a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 *cases* per 100,000 residents by pre-existing case rates, spring semester only, Michigan



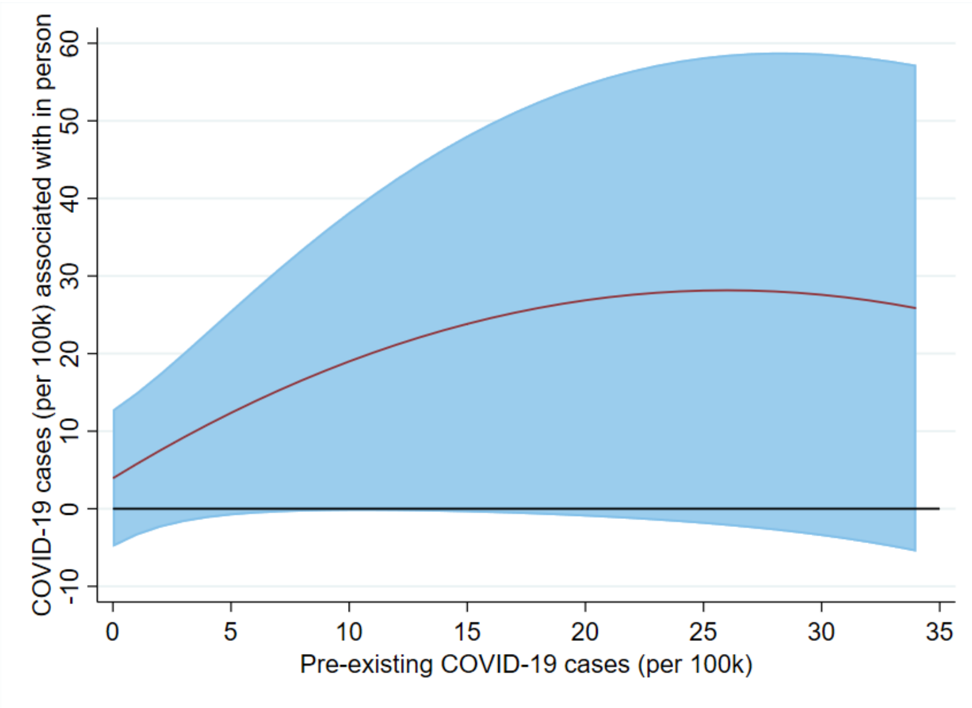
a) Impact if all districts in county switch from remote to in-person



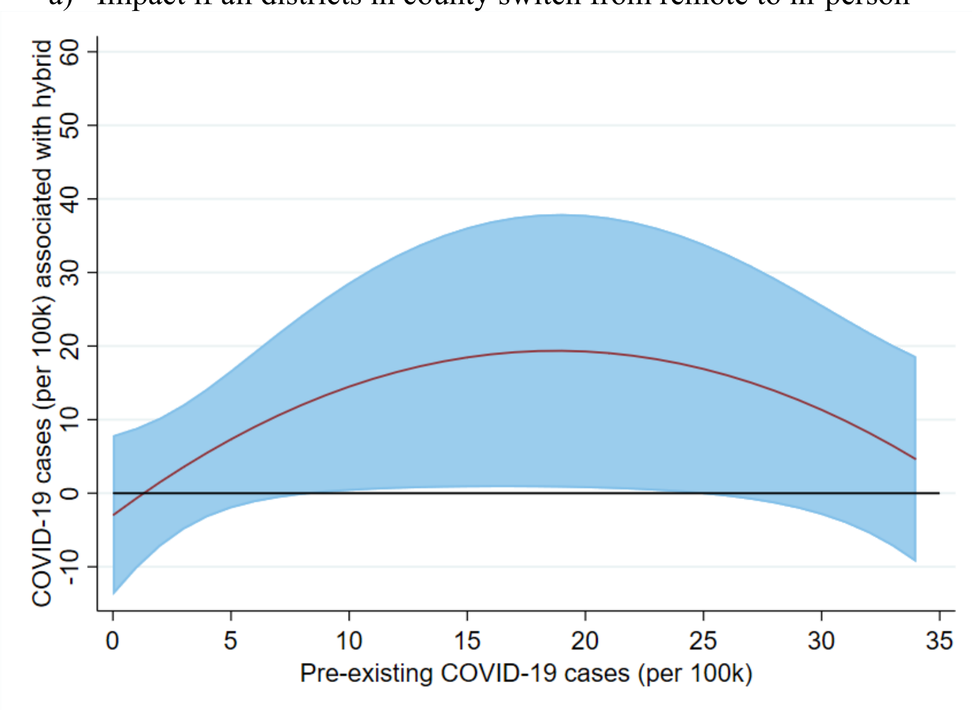
b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated by excluding September through December modality data and re-estimating the Michigan month and district fixed effect model in Table 2.

Figure A6a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 *cases* per 100,000 residents by pre-existing case rates, fall semester only, Washington



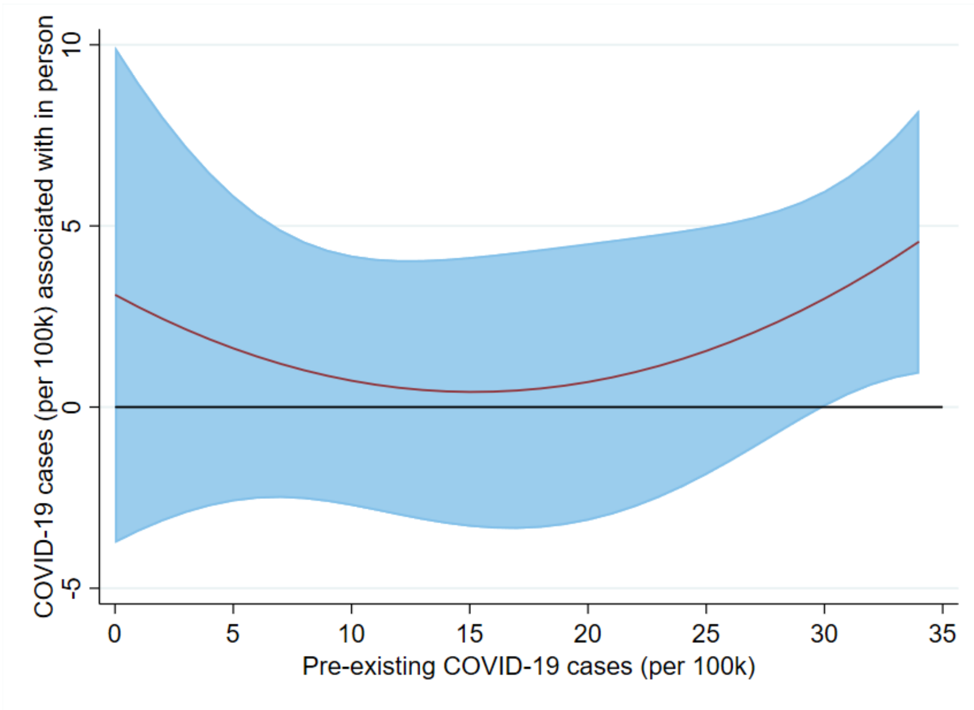
a) Impact if all districts in county switch from remote to in-person



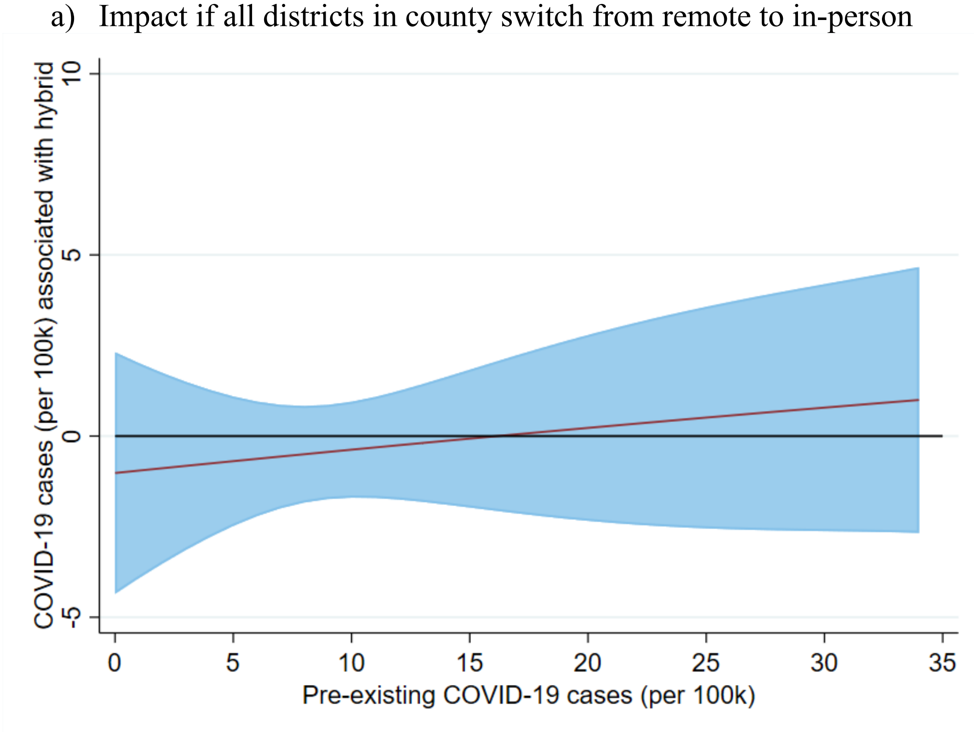
b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated by excluding September and February through April modality data and re-estimating the Washington month and district fixed effect model in Table 2.

Figure A7a, b: Month and district fixed effect estimates of the impact of modality on county COVID-19 *cases* per 100,000 residents by pre-existing case rates, spring semester only, Washington



a) Impact if all districts in county switch from remote to in-person



b) Impact if all districts in county switch from remote to hybrid

Notes: Estimates are calculated by excluding October through December modality data and re-estimating the Washington month and district fixed effect model in Table 2.