School Reopenings, COVID-19, and Employment

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Abstract

Using a panel of United States counties, this study compares outcomes before and during the 2020-2021 school year between locations that started K-12 instruction on campus, remotely, or through a hybrid approach. Corroborating recent studies, we find comparatively larger increases of COVID-19 cases and deaths in locations using any in-person instruction. Within the same empirical framework, we present robust new evidence that employment was unaffected by this choice, even in counties with more vulnerable populations. We posit that opening schools did not improve employment due to policy uncertainty, supported by the fact that one-quarter of schools changed teaching methods mid-year.

JEL: D78, H75, I18, J21

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1 Introduction

The COVID-19 pandemic wedged policymakers between a rock and a hard place. A first-order priority was to reduce the spread of the deadly disease. However, governments and other planners also sought to sustain the economic livelihoods of households and businesses during this global recession. Balancing these objectives required making difficult decisions based on limited information about how various policy options would affect targeted outcomes.

One particularly salient choice was whether to "reopen" schools and provide at least some instruction in person rather than teaching entirely remotely. While bringing people together onto a campus increases the potential scope for viral transmission, schools have long served an important role in facilitating parents' access to employment markets. Indeed, the U.S. Bureau of Labor Statistics suggests that the 2020 "decline in labor force participation among parents, especially mothers, likely reflects not only pandemic-related job losses, but also the shift of many schools to distance learning" (BLS, 2021).

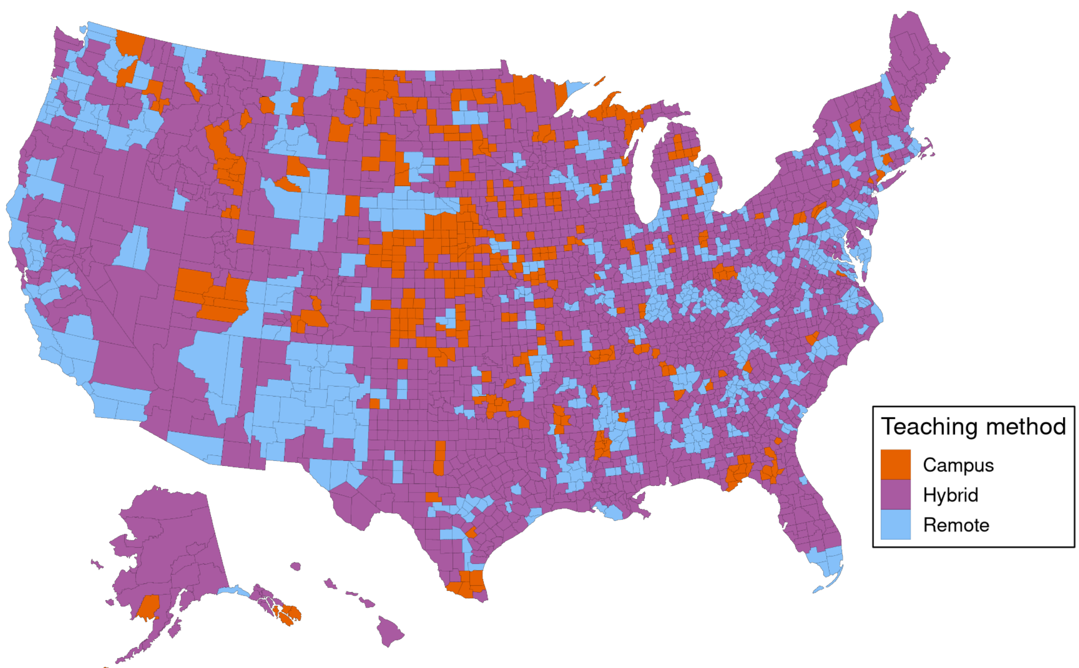
Ultimately, policymakers across the world chose to operate local schools during the 2020-2021 academic year using a variety of approaches. In this paper, we use a panel of United States counties to study the impacts of the choice of teaching method on COVID-19 cases and deaths and on employment. We employ difference-in-differences and event study designs to isolate the variation in the outcomes that is most likely attributable to the teaching method chosen at the start of the school year. We acknowledge that a limitation of this approach is that even the initial choice of teaching method was not (quasi-)randomly assigned, and thus the causality of these findings should be interpreted with caution. Nonetheless, we provide a broad set of supporting evidence for the robustness of our approach and findings.

Several recent studies also explore whether in-person schooling is associated with greater COVID-19 transmission (e.g. Chernozhukov et al., 2021; Goldhaber et al., 2021) or parental employment (e.g. Amuedo-Dorantes et al., 2020; Collins et al., 2021). However, we provide the first evaluation, to our knowledge, that uses a consistent empirical framework to estimate how the local teaching method relates to both of these outcomes. There are also some key differences between our analysis and the related literature. Our study uses data for virtually the entire U.S. population, rather than the geographically limited survey evidence from some prior research. Moreover, much of the related research pertains to school closures in March 2020, whereas we focus on the effects of the teaching methods selected during the pandemic for the 2020-2021 academic year.

Finally, while some related literature also uses a difference-in-differences empirical strategy, these studies allow the choice of instructional modality to vary over time—thereby estimating rather short-run effects identified within-location from schools changing teaching methods. In contrast, our approach captures the longer-run intent-to-treat effect of the initially selected teaching method across the full school year. While both methods can have merit depending on the specific research question(s), this is an important distinction given that many school districts revised their teaching method after instruction had started. In Appendix A, we provide a more comprehensive discussion of the related literature and comparisons to our paper.

2 Data and methods

Figure 1: United States counties map by predominant initial K-12 teaching method



Note: Hybrid can entail some schools teaching on campus while others are remote, or a mixed schedule.

This study uses a balanced panel of U.S. counties from March 2020 through May 2021—effectively spanning the start of the pandemic through the end of the 2020-2021 school year. We source data on the daily COVID-19 deaths and cases (positive tests) from the *New York Times*, which compiles data from health departments. For monthly employment, we use the Bureau of Labor Statistics' Local Area Unemployment Statistics. We obtained nationwide K-12 school district policies from MCH Strategic Data. Because some districts changed teaching methods mid-year—potentially endogenously—we use a database version of policies in October 2020, just after the school year started. As counties can host multiple school districts, we spatially joined districts to counties and assign teaching policies based on the highest-enrollment district. Figure 1 shows counties' predominant teaching method: Campus, Hybrid, or Remote.

Table 1: Summary statistics by county teaching method

	Average across counties		
	Campus	Hybrid	Remote
Panel [A] Pre-pandemic covariates in 2019			
Latitude	40.57	38.2	38.14
Longitude	-95.6	-91.96	-91.13
Population	28,687	100,357	151,604
Population share under-18 (%)	22.66	22.17	22.25
Adult labor force part. rate (%)	76.13	73.48	72.92
Female labor force part. rate (%)	71.82	69.69	69
Mothers labor force part. rate (%)	75.51	73.94	73.52
GDP per person ($)	56,478	65,197	67,363
Median income ($)	54,182	55,629	56,691
Poverty rate (%)	13.51	14.43	15.02
Influenza vaccinations rate (%)	40.19	43.15	43.23
Hospital beds per 1k population	2.67	2.64	2.56
ICU beds per 1k population	0.14	0.19	0.21
Panel [B] Dependent variables during March 2020 - May 2021			
Daily COVID-19 cases per 1m pop.	238	228	214
Daily COVID-19 deaths per 1m pop.	4.91	4.61	4.36
Employment per 1k population	465	431	428
Start of 2020-2021 school year	August 26	August 30	August 30
Number of counties	357	2,087	696

Note: See Appendix B for details.

Appendix B discusses details for these panel data and county covariates from 2019. As Figure 1 indicates, Campus counties are more rural and concentrated in the Great Plains, whereas Hybrid and Remote counties are geographically representative. Table 1 provides summary statistics. The average population is lowest in Campus counties and highest in Remote counties. However, the three groups are quite similar in per-capita characteristics such as income, poverty, family demographics, and influenza vaccinations. Almost every county started teaching between mid-August and early September (typical for U.S. schools). Panel [B] shows statistics for the outcome variables, computed over our entire study period. Descriptively, counties teaching on Campus or Hybrid exhibit both relatively greater COVID-19 severity and employment, motivating our more formal analysis below.

We apply difference-in-differences and event study research designs to compare outcomes before and during the school year between locations that initially started teaching on Campus or Hybrid versus Remote. This choice of teaching method is not assigned at random, although our use of the initial teaching choice avoids some endogenous selection. In addition, Appendix B uses Google's Community Mobility Reports to demonstrate that the groups of counties exhibit similar movement patterns over time to various categories of places (e.g. retail and recreation), both before and during the school year.

3 Results

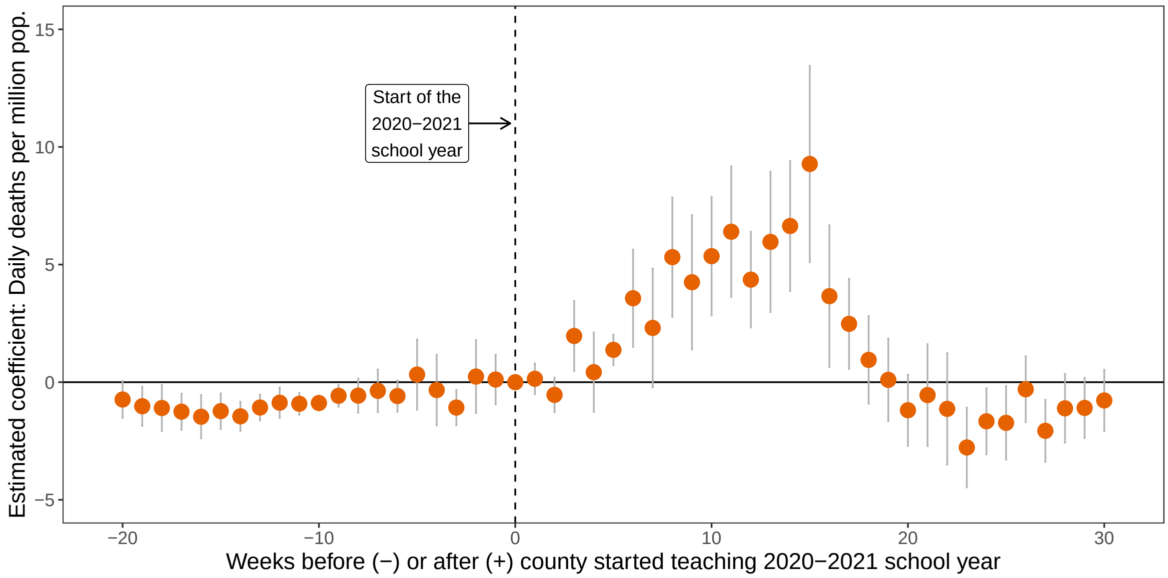
We first evaluate how in-person teaching relates to community COVID-19 outcomes. Appendix B includes graphs showing that per-capita cases and deaths trended fairly similarly before the school year across the three groups, especially for Hybrid and Remote counties. Outcomes diverge after schools opened, particularly for the initial few months, with Campus counties experiencing substantially more and Hybrid counties moderately more COVID-19, relative to areas teaching remotely. The difference-in-differences estimates shown in Appendix Table B1 echo these patterns.[1] Per million population, teaching on-campus leads to 54 more new cases and 1.7 more deaths each day, while teaching Hybrid leads to 22 more new cases and 0.5 more deaths per day.[2] These estimates are statistically significant and imply that, nationally, Campus and Hybrid teaching cause 123 additional deaths per day

[1] Recent econometrics literature suggests that differences-in-differences models may yield biased estimates for the effects of policies pertaining to COVID-19 (Korolev, 2021). Because growth rates can be exponential, parallel pre-trends might not serve as a credible identification test. In Appendix C, we address this concern by showing that results are robust to using synthetic controls for each county (Ben-Michael et al., 2021).

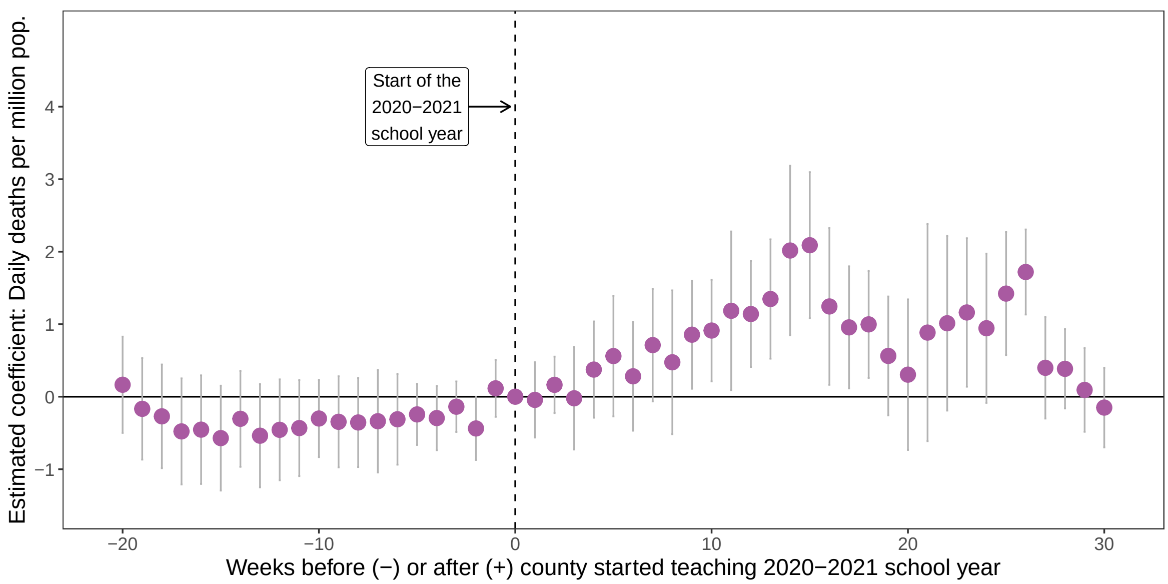
[2] Several parts of the country (e.g., the West Coast and the Northeast) have very few counties with purely in-person instruction, as shown in Figure 1. In Appendix Table B2, we show that these results are robust to including only the 36 states that had at least one county using the (fully) Campus teaching modality.

during the school year, a 6.3 percent increase.[3]

Figure 2: Event study: COVID-19 deaths in counties teaching Campus/Hybrid v. Remote



(a) Campus counties v. Remote counties

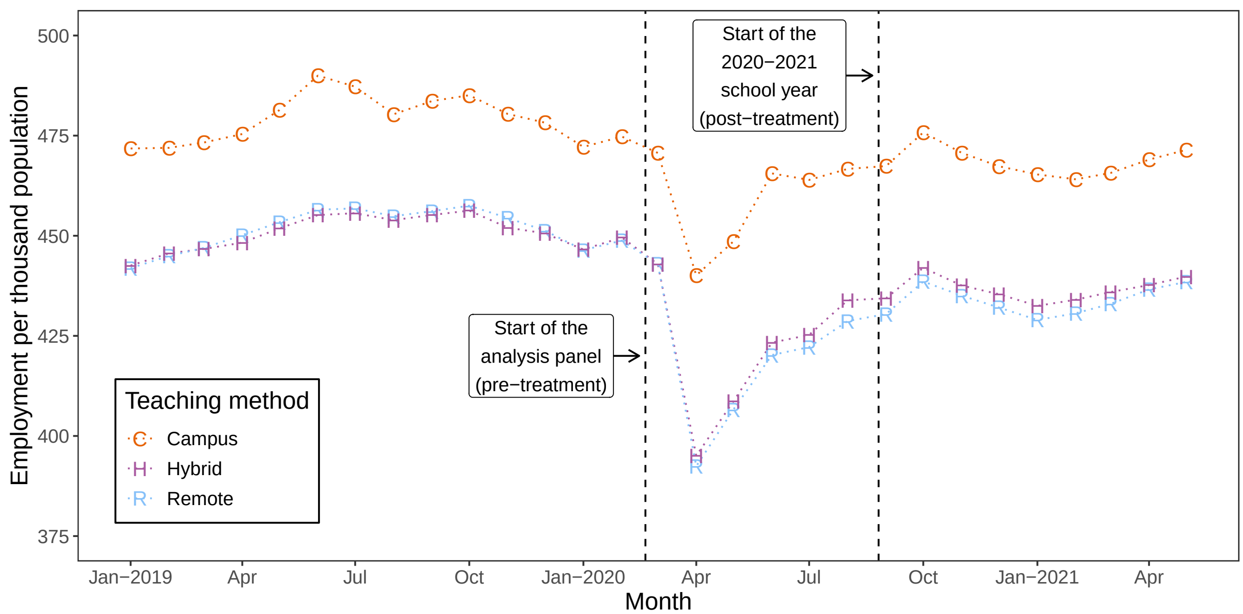


(b) Hybrid counties v. Remote counties

[3]As the daily data on COVID-19 cases and deaths might be subject to measurement error, we verify robustness of these results to aggregating the data at the monthly level in Appendix Table B3. All monthly estimates are around 30 times the magnitude of the daily estimates. Because we two-way cluster standard errors by state and month—which yields only 15 clusters in the temporal dimension—the standard errors are somewhat larger, but nearly all estimates remain statistically significant at the five percent level.

We reiterate that teaching methods are not randomly assigned, and some of this effect might result from other asymmetric behavioral changes across counties during the school year. In support of a causal interpretation, Figure 2 presents event study evidence with regression coefficients by weeks-to-open for counties teaching Campus versus Remote and for counties teaching Hybrid versus Remote.[4] Residual to county and date fixed effects, the groups are statistically indistinguishable just prior to the school year, then Campus/Hybrid areas show a marked increase in COVID-19 deaths. As in Chernozhukov et al. (2021), there is about a three-week lag before evidence of deaths.

Figure 3: Total employment per-capita by county teaching method



Using the same empirical framework, we find that opening schools in person does not boost local employment. Figure 3 shows monthly employment per-capita rates over time across the three county groups. These ratios trended very similarly before the pandemic, plummeted in sync in April 2020, and remained on parallel trends throughout May 2021.

[4]For counties i on dates t, the first panel of the graph uses only counties teaching either Campus or Remote and plots θ_w coefficients from the specification: $\text{deaths\_per\_1m}_{iwt} = \sum_{w=-20}^{-1} \theta_w \text{campus}_i + \sum_{w=1}^{30} \theta_w \text{campus}_i + \gamma \cdot 1\{w < -20\} \cdot \text{campus}_i + \phi \cdot 1\{w > 30\} \cdot \text{campus}_i + \mu_i + \tau_t + \epsilon_{iwt}$. The second panel uses only counties teaching either Hybrid or Remote and shows coefficients from the specification: $\text{deaths\_per\_1m}_{iwt} = \sum_{w=-20}^{-1} \theta_w \text{hybrid}_i + \sum_{w=1}^{30} \theta_w \text{hybrid}_i + \gamma \cdot 1\{w < -20\} \cdot \text{hybrid}_i + \phi \cdot 1\{w > 30\} \cdot \text{hybrid}_i + \mu_i + \tau_t + \epsilon_{iwt}$. The 95 percent confidence intervals use standard errors that are two-way clustered by state and weeks-to-open.

Table 2 provides quantitative evidence of this null effect.[5] In both natural logs and employment per-capita regressions, we estimate fairly precise zero difference-in-differences coefficients for the teaching method.[6] If anything, labor markets in fully in-person counties fare worse after schools open. The table shows that employment is unaffected even in poorer counties or those with higher pre-pandemic mother labor force participation—where people likely depend more on schools for childcare—and in counties with fewer COVID-19 deaths through August—where people may have changed behavior by more when schools opened.

Table 2: Difference-in-differences estimates for employment outcomes by teaching method

	Log-Emp.	Employment per 1k population			
	All counties	All counties	Abv. median mother LFPR in 2019	Abv. median poverty rate in 2019	Blw. median COVID deaths pre-Sep. 2020
	(1)	(2)	(3)	(4)	(5)
Teaching: Campus	-0.013	-5.55	-7.04	1.34	-6.47
	(0.008)	(3.66)	(4.02)	(2.24)	(4.04)
Teaching: Hybrid	0.001	0.11	-0.11	0.81	-0.68
	(0.003)	(1.14)	(1.35)	(1.17)	(1.57)
Dep. variable mean	9.42	433.84	459.23	394.25	447.49
County fixed effects	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Number of counties	3,139	3,139	1,568	1,560	1,568
Observations	47,085	47,085	23,520	23,400	23,520
Adj. R-squared	0.999	0.952	0.923	0.958	0.941

Notes: Each regression uses a balanced panel of counties from March 2020 through May 2021. "Teaching" is defined as zero for all counties prior to the start of the 2020-2021 school year. The omitted interaction category is Teaching: Remote. Standard errors are two-way clustered by state and month of sample.
*p$<$0.1; **p$<$0.05; ***p$<$0.01

[5] Appendix Table B4 shows that these results are robust to including only the 36 states that had at least one county using the (fully) Campus teaching modality.

[6] Although the pre-treatment trends are similar across county groups, there is a difference in levels. We again verify that synthetic control models provide similar estimates as the difference-in-differences model.

4 Conclusions

Policymakers faced many challenging tradeoffs during the COVID-19 pandemic. One difficult decision was whether to use in-person school instruction, risking the spread of the virus but facilitating parents' access to labor markets (in addition to benefits such as fostering children's development). As several other recent studies have documented, we find that opening schools increased community prevalence of the disease. Our evidence showing no effects on employment is more puzzling. Likely, there are several relevant explanations. One consideration is that the pandemic "employment deficit is explained by factors that affect workers more broadly, as opposed to challenges specific to working parents" (Furman et al., 2021).

We think that an additional important factor is policy uncertainty. In the data we study, at least one-quarter of schools changed teaching methods at some point over the fall or spring semesters, a pattern also shown at www.returntolearntracker.net. Although schools are undoubtedly related to parents' employment opportunities, beginning the school year in person may not be enough to support higher employment if parents cannot reliably anticipate the availability of consistent child care.

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A Literature review appendix

Researchers in economics and other disciplines have been very active in studying various aspects of the COVID-19 pandemic. One important strand of research studies the effects of policy orders (or the lack thereof) and adoption of non-pharmaceutical interventions (NPI) on the transmission of cases and resulting deaths (Amuedo-Dorantes, Kaushal, and Muchow 2020; Dave et al. 2020; Friedson et al. 2020; Karaivanov et al. 2020). A related strand examines effects of congregations such as the Black Lives Matter protests (Dave et al. 2020) on social distancing and the spread of the pandemic.

A sub-strand of this literature specifically focuses on the impact of school closures during the 2019-2020 academic year and the choices of instructional modality made during the 2020-2021 academic year on COVID outcomes. A detailed review of this literature is presented in Table A1.

From the perspective of policymakers, the central challenge in deciding the response to COVID-19 has been to weigh the benefits of imposing movement restrictions and other NPIs to prevent the spread of the virus against the economic consequences of those restrictions. So, naturally, researchers are interested in studying the short-run impacts of COVID-19 policy orders on employment and economic activity. In this vein, studies explore the effects of the pandemic and social distancing on minority unemployment (Couch, Fairlie, and Xu 2020), unemployment risk faced by immigrant workers (Fasani and Mazza 2020), start-up activity (Camino-Mogro 2020), and families' strategies to mitigate labor market shocks (Crossley, Fisher, and Low 2020).

More directly related to our study, much has been written in the press about the school disruptions that resulted from the pandemic and their labor market effects on parents of young children. Noting a far steeper rise in unemployment for women than for men, the pandemic-induced recession has been dubbed by many as a "shecession" (The Fred Blog 2020). A commonly cited explanation for this phenomenon is the closure of schools and other childcare options that could heavily impact the labor force participation of mothers of young children, who often shoulder the larger parenting burden. However, research evidence on this question is mixed. A detailed review of this literature is presented in Table A2.

Contributing to both of these strands of the literature, we present the first evidence to our knowledge on the effects of the teaching method (in-person vs. hybrid vs. remote) chosen for the 2020-2021 school year on *both* COVID-19 outcomes and on employment within the same empirical framework.

Table A1: Review of literature on the effects of school closures and teaching methods on COVID-19 outcomes

Paper	Attributes of the study	Results/Findings
1. To What Extent Does In-Person Schooling Contribute to the Spread of COVID-19? Evidence from Michigan and Washington (Goldhaber et al. 2021)	• **Geography**: Counties in Michigan and Washington • **Date range**: March to November 2020 • **Main outcome**: COVID-19 diagnoses and growth rates • **Level of analysis**: School district-month • **Approach**: School district-level variation in instructional modality over time is used in a difference-in-differences model. The main independent variable is changing instructional modality, the evolution of which could be endogenous to COVID-19 spread.	• No discernible effect of instructional modality on the spread of COVID-19 overall and some models are imprecisely estimated • Some differences are found between MI and WA. • In-person and Hybrid schooling results in COVID spread in counties with the highest infection rates prior to the start of the school year.
2. Back to School: The Effect of School Visits During COVID-19 on COVID-19 Transmission (Bravata et al. 2021)	• **Geography:** Counties in all of the United States • **Date range:** January to November 2020 • **Main outcome:** COVID-19 diagnoses and growth • **Level of analysis:** County-week • **Approach:** A triple difference approach using weekly variation in county-level visits to schools to estimate the difference in COVID-19 diagnoses in households with school-age children relative to diagnoses in those without. The main independent variable is school visits, the evolution of which could be endogenous to COVID spread. As acknowledged by the authors, the strategy of comparing COVID-19 diagnoses in households with school-age children relative to diagnoses in those without could be affected by the spillover effects of school-related movement on households without school-age children. The estimates capture the effects of school closures in March 2020 and that of the choice of teaching method made in September 2020.	• Increases in county-level in-person visits to schools lead to an increase in COVID-19 diagnoses among households with children relative to households without school-age children. • Larger differences are found between the two types of households in low-income counties and counties with higher COVID-19 spread. The gap widens as the pandemic progresses.
3. School Reopenings,	• **Geography:** Counties in Texas	• School reopenings led to substantial

Mobility, and COVID-19 Spread: Evidence from Texas (Courtemanche et al. 2021)	• **Date range:** March to November 2020 • **Main outcome:** COVID-19 cases and deaths • **Level of analysis:** County-week • **Approach:** County-level variation in the choice of teaching method and the timing of school opening is used in difference-in-differences and event study approaches. The instructional modality is kept constant over time.	increases in the community spread of COVID-19. • A significant increase in time spent outside the home (by adults) on weekdays in neighborhoods with large numbers of school-age children is cited as a possible mechanism for this spread.
4. In-person schooling and COVID-19 transmission in Canada's three largest cities (Bignami-Van Assche et al. 2021)	• **Geography**: The three largest cities in Canada: Montréal, Toronto, and Calgary. • **Date range**: August 2020 to January 2021 • **Main outcome**: COVID-19 diagnoses • **Level of analysis**: City-week by age group. • **Approach**: Comparison of trends in COVID outcomes among school-age children with other age groups under different rules for in-person schooling. The methodology is more observational than quasi-experimental with descriptions of the progression of infection rates among different age groups	• In-person schooling without other mitigation strategies such as mask mandates is correlated with a greater spread of COVID-19 among the school-age population along with spillovers into other age groups.
5. The Association of Opening K-12 Schools and Colleges with the Spread of COVID-19 in the United States: County-Level Panel Data Analysis (Chernozhukov, Kasahara, and Schrimpf 2021)	• **Geography:** Counties in all of the United States • **Date range:** April to December 2, 2020 • **Main outcome:** COVID-19 diagnoses, growth, and deaths • **Level of analysis:** County-date • **Approach:** County/School district-level variation in school visits and instructional modality (interacted with mask mandates) over time is used in a difference-in-differences model. The changes in instructional modality and school visits over time could evolve endogenously. So, the paper presents a structural model that likely circumvents these issues to an extent. For instance, the model includes lags of COVID measures to control for the effect of information on	• Opening schools completely with in-person learning results in a 5 percentage point increase in the growth rate of cases. • The effect is exacerbated in counties without mask mandates.

	COVID spread on the protective behavior of the people.	
Our Paper: School Reopenings, COVID-19, and Employment	• **Geography:** Counties in all of the United States • **Date range:** March 2020 to May 2021 • **Main Outcome:** COVID-19 cases and deaths (and employment per capita – details in Table A2) • **Level of analysis:** County-date • **Approach:** County-level variation in the choice of teaching method is used in difference-in-differences and event study approaches. The teaching method is kept constant over time in the model, so the results represent an intent-to-treat effect of the initial teaching method chosen.	• Counties that predominantly chose in-person and hybrid modes of instruction experience greater rises in both COVID-19 cases and COVID-19 deaths. • Per million population, teaching fully on-campus leads to about 54 more new cases and 1.7 more deaths daily, while teaching Hybrid leads to about 22 more new cases and 0.5 more deaths per day. • In aggregate, these effects correspond to 123 additional deaths per day nationally, a 6.3 percent increase.

Table A2: Review of literature on the effects of school closures and teaching methods on employment outcomes

Paper	Attributes of the study	Results/Findings
1. COVID-19 School Closures and Parental Labor Supply in the United States (Amuedo-Dorantes et al. 2020)	• **Geography**: States in all of the United States • **Date range**: January 2019 to May 2020 • **Main outcome**: Work hours conditional on employment • **Level of analysis**: State-month • **Approach**: School district-level variation in instructional modality over time is used in a difference-in-differences model. The main independent variable is a measure of disruption from school closures at the state level that is calculated as the population-weighted average of school district-level closures in the month. The estimated effect is of school closures in early 2020 as opposed to the estimated effect in our paper of opening of in-person or hybrid instruction in the 2020-2021 school year.	• 11% to 15% reduction in weekly work hours among parents of young school-age children due to school closures • The effects were more pronounced for mothers. • While other NPIs appear to affect employment at the extensive margin, school closures seem to impact the intensive margin.

2. The Gendered Consequences of a Weak Infrastructure of Care: School Reopening Plans and Parents' Employment During the COVID-19 Pandemic (Collins et al. 2021)	• **Geography**: States in all of the United States • **Date range**: September to November 2019 and September to November 2020 • **Main outcome**: Labor force participation rates of mothers and fathers • **Level of analysis**: State-level observations for the two time ranges • **Approach**: Comparison of maternal and paternal labor force participation rates (as captured by the Current Population Survey) during the first semester of schools (Sep to Nov) in 2020 to that in the same months of 2019, across states with different primary modes of instruction.	• In states where schools offered primarily remote instruction, the gap between maternal and paternal labor force participation rates grew by 5 percentage points in 2020 (from 18 pp in 2019)
3. Disentangling Policy Effects Using Proxy Data: Which Shutdown Policies Affected Unemployment During the COVID-19 Pandemic? (Kong and Prinz 2020)	• **Geography**: States in all of the United States • **Date range**: February 2020 to April 2020 • **Main Outcome**: Daily Google searches for "file for unemployment" • **Level of analysis**: State-date • **Approach**: Use the differential timing of the introduction of various non-pharmaceutical interventions (NPIs), including school closures, to analyze the daily variation in Google searches for claiming unemployment insurance across states. Google searches are used as a proxy for actual or expected job loss because data on Internet searches is available with less delay than official unemployment data. The estimated effect is of school closures in early 2020 as opposed to the estimated effect in our paper of opening of in-person or hybrid instruction in October 2020.	• No discernable effect of school closures in March on searches for UI claims • Restaurant and bar limitations and closure of non-essential businesses led to moderate increases in searches for UI claims.
Our Paper: School Reopenings, COVID-19, and Employment	• **Geography**: Counties in all of the United States • **Date range**: March 2020 to May 2021 • **Main outcome**: Employment per capita (and COVID-19	• The choice of teaching method is not found to have any impact on local employment. The confidence intervals for

	cases and deaths– details in Table A1) • **Level of analysis**: County-month • **Approach**: County-level variation in the choice of teaching method is used in difference-in-differences and event study approaches. The estimated effect is for the type of teaching method chosen when schools reopened in September 2020 as opposed to the effect of school closures estimated in some of the other papers. Also, the teaching method is kept constant over time in the model, so the results represent an intent-to-treat effect of the initial teaching method chosen.	our estimates support a precisely-estimated null effect of school re-openings on employment. • No discernable effect is found even in counties with above-median poverty rates or mother labor force participation.

Because we apply a consistent empirical approach to evaluate both types of outcomes, our results could serve as a guide to policymakers in weighing the short-term costs and benefits of the choice of instructional modality. As highlighted in Tables A1 and A2, there are a few key differences between our approach and that used in the related papers. First, there are differences in timelines and geographies. Our analysis spans across all of the United States and covers the period starting with essentially the first detected case of COVID-19 in the country (March 2020) and extending through the entire 2020-2021 school year (September 2020 to May 2021). Thus, our paper provides a comprehensive picture of the effects of schools' choice of teaching method on the primary outcomes of policy interest. Second, while some papers study the effects of school closures in March 2020, our paper evaluates the effects of school reopenings in one of three modes: fully on-campus vs. hybrid vs. fully remote learning. Third, many of the papers estimate relationships that allow the focal independent variable to vary during the school year. These revisions to schools' teaching methods might be made in response to the spread of the virus or mounting economic pressures and, hence, are subject to concerns of endogeneity. Though the initial choice of teaching method is also an independent variable that is not randomly assigned, by holding each school's teaching method constant in our models and estimating the intent-to-treat effect of that initial choice, our approach alleviates some of those endogeneity concerns.

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B Data and supplemental exhibits appendix

This appendix provides some additional details about the data used in our study. Altogether, we combine data from over a dozen sources.

The COVID-19 data for cases and deaths are from a GitHub repository maintained by the *New York Times*, which compiles data from state and local health departments. These data are provided at the county-by-date level, denoting the number of cases (positive COVID-19 tests) and deaths for each county-date observation.

The employment data are at the county-by-month level, provided in the Local Area Unemployment Statistics (LAUS) series of the U.S. Bureau of Labor Statistics. At this county-month level, the LAUS data include four measures: total employment, the size of the labor force, total unemployment, and the local unemployment rate. The LAUS data have several major advantages such as comprehensive county coverage with a relatively short delay before data is released; however, one downside is that no demographic or other characteristics are provided, in contrast with individual-level survey microdata such as that in the Survey of Income and Program Participation (SIPP).

We obtain nationwide K-12 school district policies for the 2020-2021 academic year from MCH Strategic Data. These data are a snapshot of school policies as of the date of the data extract. We use the snapshot describing policies in October 2020, just after nearly all schools started teaching. The MCH Strategic Data policies are at the school district level, and a county can host multiple districts. So, we spatially joined districts to counties using shapefiles from the National Center for Education Statistics and the U.S. Census Bureau. We then assign the school year start date and a teaching method to each county based on the largest district by enrollment within the county.

In addition to these data components—which we use to form our primary empirical panel—we incorporate some county-level covariates as control terms and identification checks for our empirical methodology. Google's Community Mobility Reports provide proxies for movement over time across six categories of places: retail and recreation, groceries and pharmacies, workplaces, residential, parks, and public transit. Note that the data provided for parks and for public transit are comparatively more sparsely populated. Google creates these movement indices from cell phones' location histories, aggregated to the county-by-date level of observation.

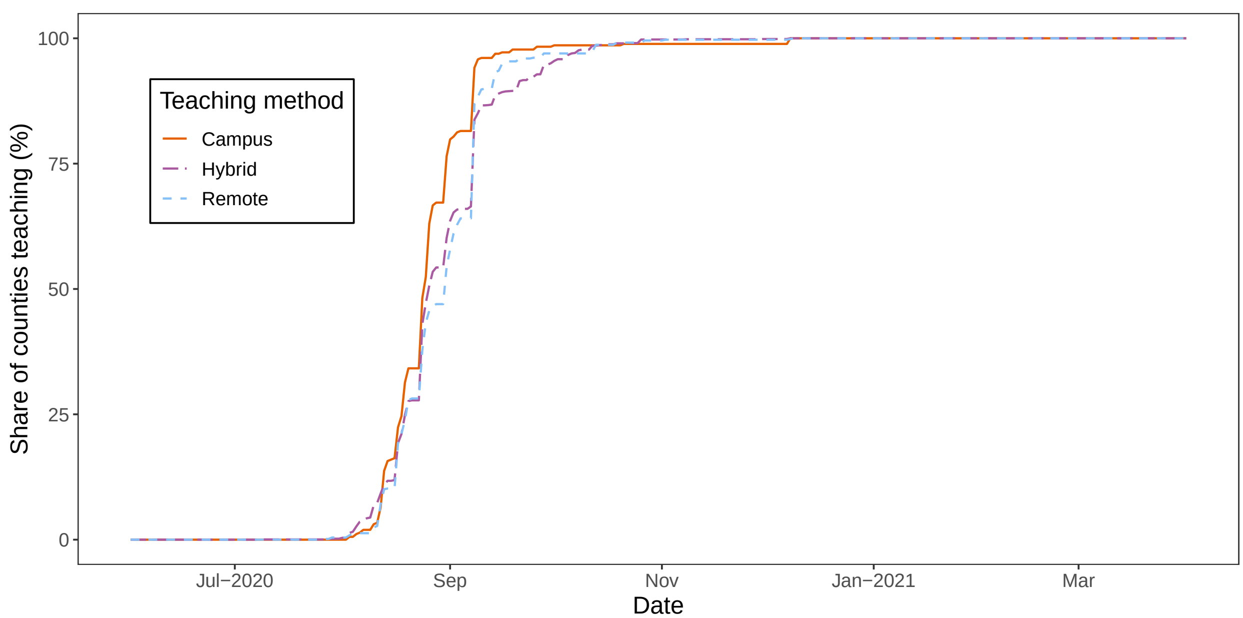
For information on state and county COVID-19 policy orders, we draw on a manually-curated dataset provided by the U.S. Department of Health and Human Services. These data are not very standardized but do provide a measure of the number of state- and county-level COVID-19 policies in effect in each county on each date. We also use data from the Delphi Research Group at Carnegie Mellon University on the number of COVID-19 tests run per day in each county.

Finally, we include some "cross-sectional" county characteristics from before the pandemic, i.e. variables for which we have exactly one observation per county. We use data from the U.S. Census Bureau on 2019 population, as well as 2019 income and poverty (from the Small Area Income and Poverty Estimates data). We also use the 2019 American Community Survey five-year estimates for selected county demographic composition and labor force participation rates, such as mothers' LFPR. We use data from the U.S. Bureau of Economic Analysis for county-level GDP in 2019. We source 2019 county-level health care statistics including 2019 influenza vaccination rates from the U.S. Centers for Medicare and Medicaid Services, and we obtain county-level counts of hospital and Intensive Care Unit (ICU) beds in 2019 and from Environmental Systems Research Institute (ESRI).

Figures mentioned in the *Data and methods* section

Appendix Figure B1, just below, shows the share of counties that had started the 2020-2021 school year by date. As is typical for schools in the United States, nearly every school started teaching between mid-August and early-September

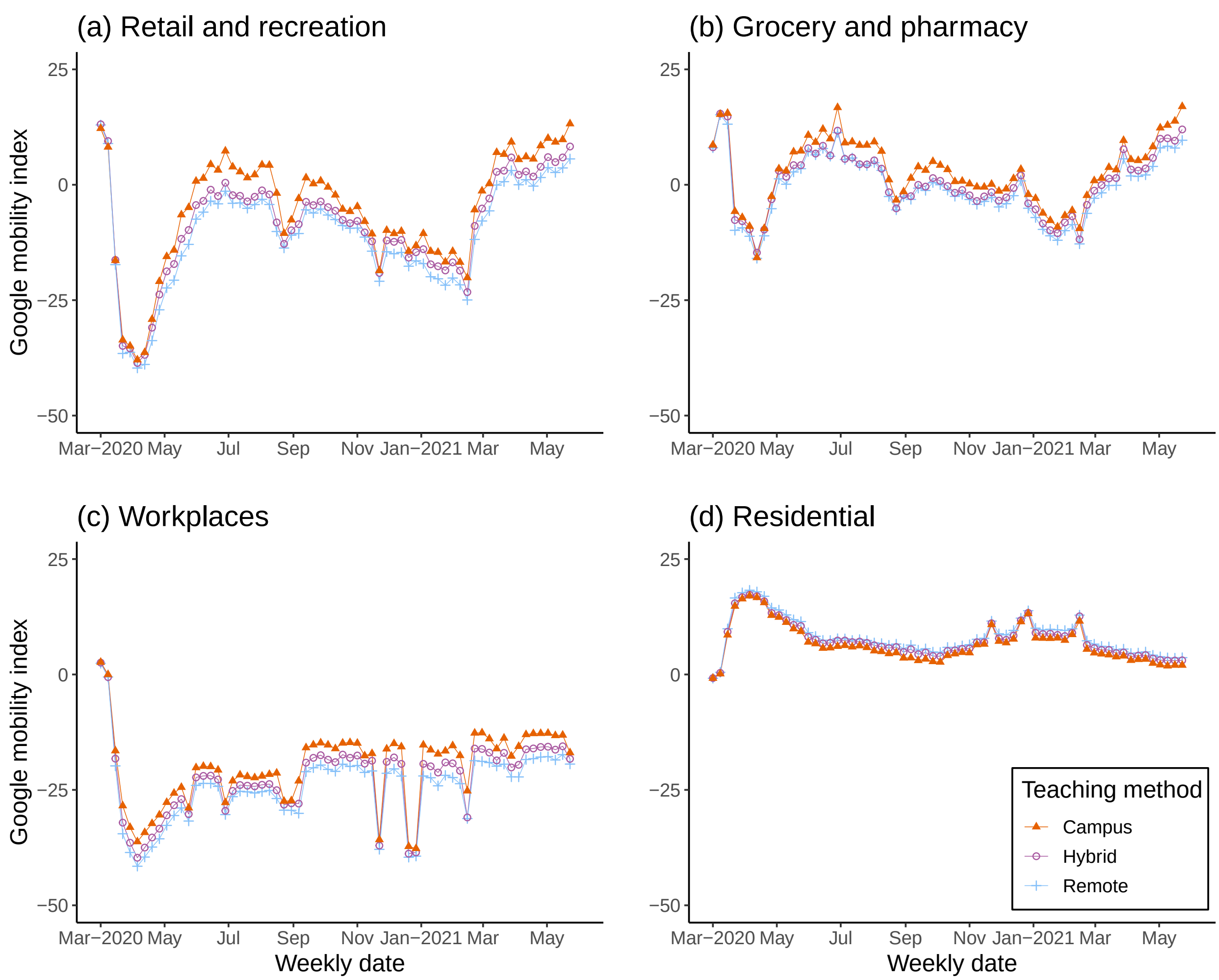
Figure B1: Timing for starting the 2020-2021 school year by county teaching method



Notes: Appendix Figure B1 plots the share of counties that had officially started teaching the 2020-2021 school year by date. Each county-date takes a value of either zero (before instruction started) or one. The lines plotted here show the unweighted average of these zero-or-one values grouped by teaching method.

Appendix Figure B2 shows four of the Google Community Mobility Report county movement indices (omitting the indices for Parks and for Public Transit). Each index can take values ranging from −100 to 100. Google sets the baseline zero-value for each county's indices using movement of people's cell phones to different places by day of the week during January 3 through February 6, 2020. We aggregated the county-date measures of movement by type of location to the weekly level by averaging across the seven days in each week (starting each week on Sunday). We the take the average weekly value across the counties in each group by teaching method, and plot these values over time in the figure.

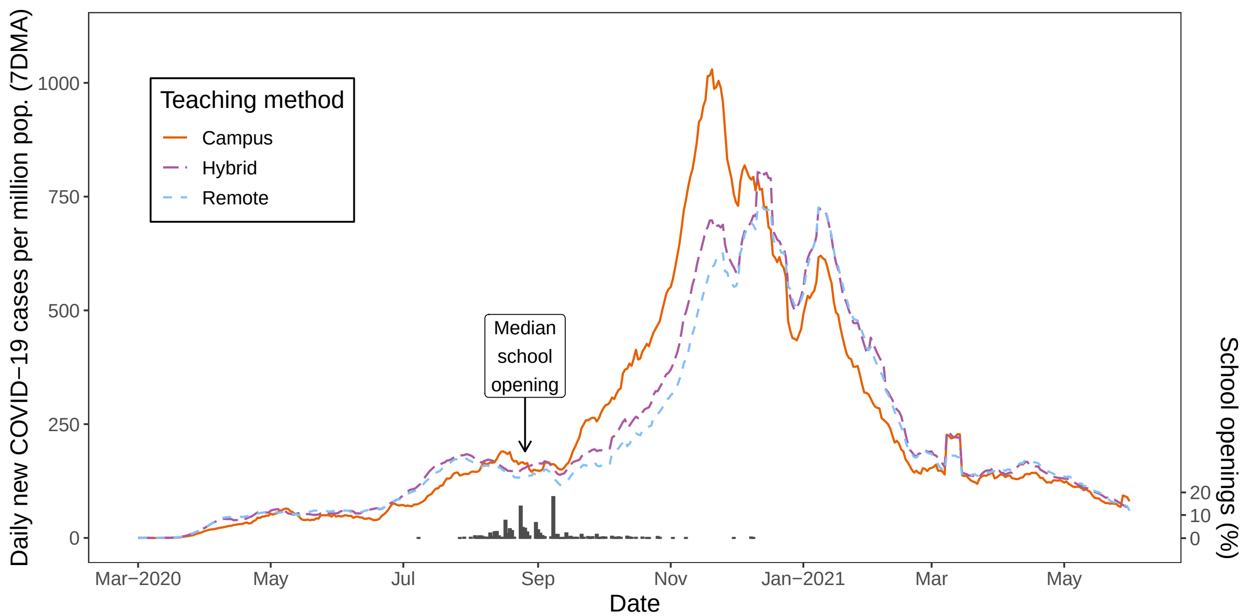
Figure B2: Community mobility indices by county teaching method



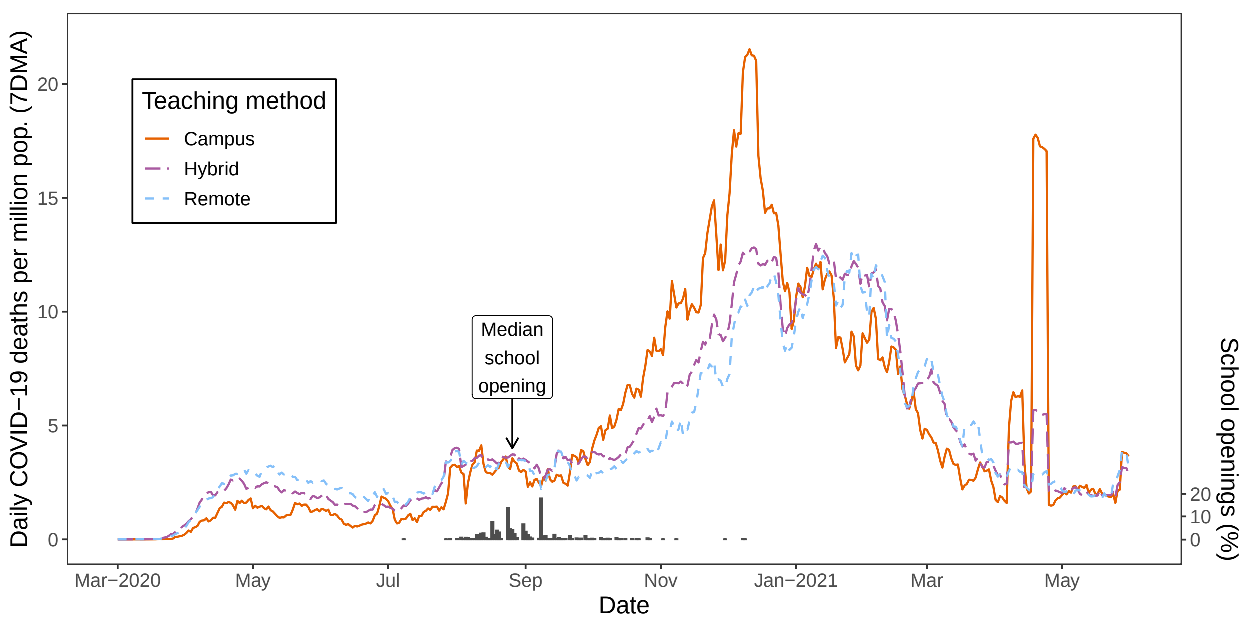
Notes: Appendix Figure B2 plots the four Community Mobility Indices indicated by the panel labels, grouped by county teaching method. Each index can take values ranging from −100 to 100. Google sets the baseline zero-value using the median level of movement by day of the week during January 3 through February 6, 2020. We aggregate each index to a weekly level by averaging across the days in each week.

Figures and table mentioned in the *Results* section

Figure B3: Daily per-capita COVID-19 outcomes by county teaching method



(a) New cases per-capita, seven-day moving average



(b) Deaths per-capita, seven-day moving average

Notes: Appendix Figure B3 plots seven-day moving averages for daily recorded new COVID-19 cases and deaths per million population, The value for each group of counties by teaching method is calculated by averaging across counties. The distribution of school opening dates is plotted using the right y-axis.

Table B1: Difference-in-differences estimates for COVID-19 outcomes by teaching method

	Population (m)	Daily new cases per 1m pop.		Daily deaths per 1m pop.	
		(1)	(2)	(3)	(4)
Teaching: Campus	10.24	57.61 ***	53.95 ***	2.07 ***	1.74 ***
		(14.73)	(14.11)	(0.51)	(0.48)
Teaching: Hybrid	209.45	22.24 ***	21.60 ***	0.62 **	0.50 **
		(6.58)	(6.86)	(0.26)	(0.24)
Aggregate effect size		5,248	5,077	150	123
Dep. variable mean (during school year)		324.49	324.49	6.39	6.39
U.S. daily total (during school year)		105,524	105,524	2,078	2,078
Controls		No	Yes	No	Yes
County fixed effects		Yes	Yes	Yes	Yes
Date fixed effects		Yes	Yes	Yes	Yes
Number of counties		3,140	3,140	3,140	3,140
Observations		1,434,980	1,434,980	1,434,980	1,434,980

Notes: Each column (1) - (4) presents results from an ordinary least squares regression using the dependent variables indicated by the column titles. The estimations use a balanced panel of counties daily from March 1, 2020 through May 31, 2021. "Teaching" is defined as zero for all counties prior to the start of the 2020-2021 school year. The omitted interaction category is Teaching: Remote. Where included, controls are the six daily Community Mobility Index measures on each date, the number of state or county-level ordinances pertaining to COVID-19 on each date, and the daily volume of COVID-19 tests run in the county on each date. Standard errors in parentheses are two-way clustered by state and date. The aggregate effect sizes are the national total additional cases (deaths) per day implied by the estimated coefficients and population of each county group.
*p<0.1; **p<0.05; ***p<0.01

Some related literature imposes structural assumptions of lagging deaths by fourteen to twenty-one days in panel analysis. In column (3) of Table B1, the outcome is not lagged, and the difference-in-differences estimates (s.e.) shown are 2.07 (0.51) and 0.62 (0.26). Imposing a structural lag of fourteen days, these estimates become 2.06 (0.57) and 0.59 (0.27). Using a twenty-one-day lag of deaths, the respective values are 1.96 (0.58) and 0.55 (0.29). Given that we use a panel around 455 days in length, with about 270 days post-treatment, it it unsurprising that the difference-in-differences estimates remain qualitatively similar regardless of the assumed lag structure.

Table B2: Difference-in-differences estimates for COVID-19 outcomes by teaching method using only states with any Campus teaching counties

	Population (m)	Daily new cases per 1m pop.		Daily deaths per 1m pop.	
		(1)	(2)	(3)	(4)
Teaching: Campus	10.24	51.35 ***	46.14 ***	1.81 ***	1.48 ***
		(14.92)	(14.15)	(0.49)	(0.46)
Teaching: Hybrid	168.24	21.39 ***	19.50 **	0.59 **	0.49 *
		(7.09)	(7.30)	(0.25)	(0.25)
Aggregate effect size		4,124	3,754	118	98
Dep. variable mean (during school year)		332.74	332.74	6.62	6.62
U.S. daily total (during school year)		79,619	79,619	1,585	1,585
Controls		No	Yes	No	Yes
County fixed effects		Yes	Yes	Yes	Yes
Date fixed effects		Yes	Yes	Yes	Yes
Number of counties		2,823	2,823	2,823	2,823
Observations		1,290,111	1,290,111	1,290,111	1,290,111

Notes: Each column (1) - (4) presents results from an ordinary least squares regression using the dependent variables indicated by the column titles. The estimations use a balanced panel of counties daily from March 1, 2020 through May 31, 2021. "Teaching" is defined as zero for all counties prior to the start of the 2020-2021 school year. The omitted interaction category is Teaching: Remote. Where included, controls are the six daily Community Mobility Index measures on each date, the number of state or county-level ordinances pertaining to COVID-19 on each date, and the daily volume of COVID-19 tests run in the county on each date. Standard errors in parentheses are two-way clustered by state and date. The aggregate effect sizes are the national total additional cases (deaths) per day implied by the estimated coefficients and population of each county group.

*p<0.1; **p<0.05; ***p<0.01

Table B3: Robustness checks for difference-in-differences estimates using monthly COVID-19 outcomes by teaching method

	New cases per 1m pop.		Deaths per 1m pop.	
	Daily	Monthly	Daily	Monthly
	(1)	(2)	(3)	(4)
Teaching: Campus	57.61 ***	1825.43	2.07 ***	69.16 **
	(14.73)	(1260.32)	(0.51)	(30.03)
Teaching: Hybrid	22.24 ***	747.83 **	0.62 **	22.87 **
	(6.58)	(262.13)	(0.26)	(9.87)
County fixed effects	Yes	Yes	Yes	Yes
Date fixed effects	Yes		Yes	
Month of sample FE		Yes		Yes
Observations	1,434,980	47,100	1,434,980	47,100

Notes: Each column (1) - (4) presents results from an ordinary least squares regression using the dependent variables indicated by the column titles. The estimations use a balanced panel of counties either daily (columns 1 and 3) or monthly (columns 2 and 4) from March 2020 through May 2021. "Teaching" is defined as zero for all counties prior to the start of the 2020-2021 school year. The omitted interaction category is Teaching: Remote. Standard errors in parentheses are two-way clustered by state and date (month) of sample.
*p<0.1; **p<0.05; ***p<0.01

Table B4: Difference-in-differences estimates for employment outcomes by teaching method using only states with any Campus teaching counties

	Log-Emp.	Employment per 1k population			
	All counties	All counties	Abv. median mother LFPR in 2019	Abv. median poverty rate in 2019	Blw. median COVID deaths pre-Sep. 2020
	(1)	(2)	(3)	(4)	(5)
Teaching: Campus	-0.012	-5.45	-7.40 *	1.95	-5.96
	(0.008)	(3.60)	(3.90)	(2.38)	(4.02)
Teaching: Hybrid	0.001	0.19	-0.08	1.14	-0.25
	(0.003)	(1.20)	(1.23)	(1.19)	(1.74)
Dep. variable mean	9.33	435.04	459.75	394.21	450.86
County fixed effects	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Number of counties	2,823	2,823	1,406	1,384	1,390
Observations	42,345	42,345	21,090	20,760	20,850
Adj. R-squared	0.999	0.954	0.925	0.959	0.943

Notes: Each regression uses a balanced panel of counties from March 2020 through May 2021. "Teaching" is defined as zero for all counties prior to the start of the 2020-2021 school year. The omitted interaction category is Teaching: Remote. Standard errors are two-way clustered by state and month of sample.
*p<0.1; **p<0.05; ***p<0.01

C Synthetic control estimations appendix

Korolev (2021) raises some concerns about the validity of estimated effects of policy interventions on COVID-19 outcomes from various reduced form approaches including two-way fixed effects. For our difference in differences study, a potential consideration is that the growth of cases and deaths is often exponential, such that testing for parallel pre-trends of cases in control and treatment counties might be inadequate support for identification. To address this possible concern, we additionally present robustness checks using the synthetic control method, which explicitly matches pre-treatment outcomes *in levels* in the treated and control counties.

However, using the synthetic control method is rather challenging here, as there are 3,140 unique units (counties), of which 357 are "treated" by Campus instruction and 2,087 by Hybrid instruction, with the remaining 696 Remote counties available to serve as the donor pool. Nevertheless, we perform synthetic control estimation using the augsynth R package developed by Ben-Michael et al. (2021).[7] This software package facilitates an approach to estimate average treatment effects pooled from separate synthetic control estimations for multiple treated units, as is the case for the context we evaluate. The scale and size of the data complicates this type of estimation, and it is not computationally feasible for us to estimate results using the daily data, even on a high-performance computing system. So, we use monthly data; specifically, as outcomes, we use monthly total new COVID-19 cases per million population and monthly total COVID-19 deaths per million population. We separately estimate the average treatment effects for Campus and Hybrid counties (always using only the Remote counties as the donor pool). Again, for computing reasons we are able to use only a random 50 percent subset (1021) of the Hybrid teaching counties for the synthetic control estimations. We use all 357 Campus counties.

The synthetic control estimated average treatment effects are strikingly similar to the corresponding monthly estimates from the difference-in-differences specifications as shown above in Tables 2 and B3. For monthly employment, the synthetic control estimate is an ATT of 1.035 for Campus counties, compared to -5.55 from the difference-in-differences model. For Hybrid teaching, the synthetic control estimate is 1.182, compared to 0.11 in the difference-in-differences specification. Note that the mean of county employment per thousand population is 434, so these are all trivial magnitudes. For monthly new COVID-19 cases per million population, the synthetic control estimates are 1706.133 for Campus counties and 1145.109 for Hybrid counties, compared to diff-in-diff estimates of, respectively, 1825.43 and 747.83 cases per million population. Finally, the synthetic control estimates for COVID-19 deaths per million population are 52.304 for Campus counties and 27.025 for Hybrid counties, compared to diff-in-diff estimates of 69.16 and 22.87, respectively. In summary, we are reassured of the strength of the identification in the difference-in-differences models, as we find these synthetic control estimated effects to be quantitatively similar.

[7] E. Ben-Michael, A. Feller, and J. Rothstein. The augmented synthetic control method. *Journal of the American Statistical Association*, Forthcoming, 2021.