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The Revealed Preferences for School Reopening: Evidence from Public-School Disenrollment

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Before the 2020-21 school year, educators, policymakers, and parents confronted the stark and uncertain trade-offs implied by the health, educational, and economic consequences of offering instruction remotely, in person, or through a hybrid of the two. Most public schools in the U.S. chose remote-only instruction and enrollment fell dramatically (i.e., a loss of roughly 1.1 million K-12 students). We examine the impact of these choices on public-school enrollment using unique panel data that combine district-level enrollment trajectories with information on their instructional modes. We find offering remote-only instead of in-person instruction reduced enrollment by 1.1 percentage points (i.e., a 42 percent increase in disenrollment from -2.6 to -3.7 percent). The disenrollment effects of remote instruction are concentrated in kindergarten and, to a lesser extent, elementary schools. We do not find consistent evidence that remote instruction influenced middle or high-school enrollment or that hybrid instruction had an impact.

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In the summer of 2020, educators and parents in the U.S. faced an exceptionally difficult choice with regard to how they should deliver instruction for the 2020-21 school year (i.e., remote, in-person, or a hybrid of the two). The number of new confirmed COVID-19 cases had risen to record heights in July of 2020 and an even more substantial “second wave” of infections was (as it turned out, correctly) anticipated for the coming winter. In addition, at that time, the timeline for availability and the effectiveness of the currently available vaccines were highly uncertain. These factors motivated grave concerns about the health risks of bringing students, teachers, and staff together for a conventional academic year of in-person instruction.¹ However, the potentially serious consequences of not doing so were also widely discussed (e.g., Oster, 2020). In particular, for many parents, having children spend more or all of the 2020-21 school year at home would seriously amplify the disruption to their labor-force engagement and exacerbate household stress by denying child-care and other supports. Furthermore, there was wide concern that the absence of full-time, in-person instruction would dramatically harm both the intellectual development and the social and emotional well-being of children while conveying only modest health benefits to younger children who appeared less susceptible to COVID-19.

Faced with these difficult trade-offs, most states chose not to make a universal decision about reopening public K-12 schools and instead left the decision to local school districts, who largely chose to offer remote instruction (Kurtz, 2020). Specifically, in the fall of 2020, 57 percent of students in district public schools experienced remote-only instruction with 24 percent in in-person instruction and 19 percent in a hybrid model (Henderson, Peterson, West, 2021).² Surveys also suggest that these instructional choices largely tracked both parents’ and teachers’ preferences (Collins and Nuamah, 2020; Will, 2020). A contemporaneous report from the National Academies of Science, Engineering, and Medicine (2020) struck a contrarian note and emphasized the importance of in-person instruction for younger students. Specifically, the report concluded, on the basis of competing health and educational risks, that schools “should prioritize reopening with an emphasis on providing full-time, in-person instruction in grades K–5.” However, the reopening modes chosen by U.S. public schools exhibited relatively little variation by grade. For example, 48 percent K-2 students were in remote-only models while 37 percent were in-person (Henderson, Peterson, and West, 2021).

The short and longer-term economic and educational consequences of these school-reopening decisions (and the COVID-19 pandemic, more generally) is an important question and will be carefully studied as diverse sources of relevant data become available. However, one important leading indicator reflects the

¹ Subsequent research (e.g., Bravata et al., 2021; Courtemanche et al., 2021, Goldhaber et al., 2021; Harris et al., 2021) has provided mixed evidence on the role of in-person schools in COVID-19 transmission.

² By way of contrast, only 18 percent of private-school students reported they were fully remote at this time.

enrollment decisions made by parents for the 2020-21 school year. Public-school enrollment at the K-12 level, which typically increases by a fraction of a percentage point annually, fell by two percent in the fall of 2020, a striking one-year loss of roughly 1.1 million students. These declines were larger in elementary and middle-school grades and particularly dramatic in kindergarten, which is not required in most states.

This study examines the impact of the school-reopening decisions made by public-school districts (i.e., remote only, in person, or a hybrid) on their enrollment levels, drawing on unique data sources that track district enrollment trajectories by grade level as well as the instructional mode chosen by districts for the 2020-21 school year. Theoretically, the effects of these policy variables on enrollment decisions are uncertain. For example, some parents may have been comparatively likely to keep their child enrolled in a district that offered only remote (or hybrid) schooling if they viewed it as a way to safeguard the health of their children and their families (i.e., by reducing the risk of COVID-19 infection and household transmission implied by in-person schooling). Furthermore, district decisions to offer alternatives to traditional instruction could have reinforced this sort of response by creating a salient signal of the risks associated with face-to-face instruction (i.e., an inferred recommendation).

Alternatively, the child-care burdens created when school districts did not offer in-person instruction may have encouraged some public-school parents to disenroll their children. This factor could be considerably more relevant in households with younger children to the extent that younger children both require more intensive care and attention and are also less likely to contract or transmit COVID-19. Another potential mechanism involves perceptions of school quality. Parents of public-school students may have viewed virtual and hybrid schooling options as inferior (again, particularly for younger children) and, as a result, disenrolled their children when face-to-face instruction was not offered. Finally, in the spirit of the “Peltzman Effect” (Peltzman, 1975), parents may have responded to the externally imposed risk reduction implied by a switch to hybrid and remote-only instruction with risk-compensating behaviors (i.e., disenrolling and seeking in-person instruction for their children).

Understanding the impact of these district choices on the disenrollment phenomenon is highly relevant for several reasons. First, enrollment changes in response to the instructional mode chosen by school districts provide direct evidence on the revealed instructional preferences of parents across different grade levels and within the context of their local communities. However, the enrollment effects of instructional modes can also provide insight into the varied educational consequences of these difficult policy choices. Such findings are relevant both for understanding the consequences of the exceptional 2020-21 school year and for informing the instructional challenges schools face in the coming school year as schools navigate a context of uneven vaccinations and the growing prevalence of COVID-19 variants.

For example, the expectation that many, if not most, of the students who have left public schools will not return soon (or ever) has serious fiscal consequences for school districts. Because most of the departing students appear to be younger, these financial consequences are also likely to be long-lived. The grade-specific effects of school-reopening policies can also provide indirect evidence on their implications for teaching and learning. Disenrollment from public schools implies that students have switched schools (e.g., to private schools or home-schooling), intend to skip or delay kindergarten, and dropped out or become truant.³ The negative consequences of dropping out of school are well established (e.g., Card, 1999). Similarly, the available research suggests that switching schools (i.e., in particular, “reactive” moves) has negative consequences for student outcomes (e.g., Welsh, 2017). Additionally, students who skipped kindergarten may bring unique learning challenges to first grade (i.e., their first year of formal schooling). However, if a substantial number of students “redshirt” into kindergarten this fall rather than attending first grade, it would imply an unusually large cohort with resulting consequences for the classroom context (i.e., older peers, large class sizes). In the next two sections, we describe the data and research designs we use to examine the enrollment effects of the instructional choices districts made for the 2020-21 school year before turning to the results and conclusions.

I. Data

Our analysis relies on three broad types of data. First, using both state and federal sources, we constructed enrollment data by school district for each of the six school years from 2015-16 through 2020-21. The federal reporting requirements ask states to identify their enrollment counts on or close to October 1, a convention that is used in state-sourced data as well. For the first five years, these enrollment data are based on the federal source – the Common Core of Data (CCD) from the National Center for Education Statistics (NCES) – and state sources. Because the CCD has not yet reported the district-by-grade enrollment data for the 2020-21 school year, we rely on leading data that we collected directly from state-level departments of education (i.e., through web searches and staff contacts) nationally as part of the Stanford School Enrollment Project at Big Local News. Specifically, the key outcome measures are total district enrollment (i.e., K-12 and ungraded students, excluding pre-kindergarten and adult students) as well as enrollment by grade level (i.e., kindergarten, grades 1-5, grades 6-8, and grades 9-12). This sample only includes traditional public-school districts and excludes administrative units and standalone charter-school networks. However, we note that these data include enrollment in charter schools authorized by traditional school districts.

³ A decomposition of the enrollment decline in Massachusetts (Dee and Murphy, forthcoming) suggests that more than 10 percent may be due to dropping out and truancy. This reflects the residual decline that cannot be explained by pre-kindergarten and kindergarten disenrollment, transfers to private schools or home-schooling, out-of-state mobility, and demographic change.

In the data appendix, we describe these data in more detail. The appendix also discusses three overlapping quality-control assessments and the corresponding edits we implemented to ensure the accuracy and comparability of these data. First, we constructed year-on-year percent changes in enrollment for every school district in our sample and examined individual districts where the absolute value of these changes met or exceeded a threshold of 10 percent. Second, we collected state-sourced district enrollment data for the 2019-20 school year and compared these data to their federally sourced counterparts (Figure A1). Third, we compared state-sourced enrollment data for the 2020-21 school year to a recent, preliminary NCES release of 2020-21 data, which only reported PK - 12 totals at the district level. For most of the districts in our sample, these checks suggested both the reliability of within-district enrollment changes over time and a tight correspondence between data based on federal and state sources. However, this analysis also surfaced systemic differences in reporting conventions across these sources for school districts in fourteen states as well as several instances of misreporting in specific district-year observations. We describe these issues and the corresponding edits in the appendix.

Our second key data source identifies district-level opening plans. Specifically, we rely on tracking data collected by Burbio (<https://cai.burbio.com/school-opening-tracker/>). Burbio monitors the instructional mode status in nearly 1,200 school districts (i.e., including the nation's largest 200 districts) by auditing data from "school district websites, Facebook pages, local news stories and other publicly available information." They characterize districts by "the most in-person option available to the general student population" (i.e., in-person, remote-only, or a hybrid). However, these data also capture the differentiated adoption of *grade-specific* instructional modes within districts as well (e.g., in-person at the elementary level but remote-only at the high school level). Through an agreement with Burbio, we have acquired their raw district-level audit data on instructional mode at different points in time and by school level. We compared the Burbio data to other independent sources on school-reopening policies and found it to be accurate (e.g., identifying districts that received waivers from state policies). Burbio surveyed districts twice prior to the October 2020 enrollment census (i.e., August 24th and September 8th). Our variable construction reflects the fact that district opening plans changed over time and that we are interested in the district choice most relevant to parents' fall enrollment decision. Therefore, we measure district opening plans using the audit on or directly preceding the school year start date recorded by Burbio. For districts that began instruction prior to August 24th (n = 264) we use the August survey.

Because our core analysis relies on the school-opening data collected by Burbio, their sampling strategy merits particular attention. The first stratum consists of the 232 most highly populated counties, which represent approximately 28 percent of all public-school enrollment in the nation. Within the counties in this

stratum, they sampled school districts that represented about 90 percent of county enrollment (e.g., often the only school district in the county). The second stratum consists of another 228 highly populated counties, which account for 47 percent of all public-school students. Generally, Burbio audited 3 to 10 school districts in each of these counties. The final stratum consists of all other counties, which account for roughly 25 percent of the student population. Within this third stratum, Burbio audited a total of 130 school districts. This sampling strategy implies that the Burbio districts cover 47 percent of the public-school enrollment in the U.S. (i.e., roughly 35,000 schools in all 50 states). Our analytic sample includes, in turn, just over three quarters of the Burbio districts (i.e., those in states where 2020-21 enrollment data are available, n=875), which serve 35 percent of all public-school students. We discuss below how this sample compares to school districts nationally as well as how our analysis explores the generalizability of the findings based on this sample.

Our third set of key data focuses on variables that varied over both location and time and may be relevant to district reopening plans and to parents' decisions about public-school enrollment. To account for COVID-19 prevalence we use a county-level rolling 7-day average of identified cases per 100,000 population from The New York Times Covid-19 data repository (<https://github.com/nytimes/covid-19-data>). In our preferred specification, we use the value at the district start date. We also include state-level variables describing non-school policy responses to Covid-19 compiled in the Oxford Covid-19 Government Response Tracker (OxCGR) by the Blavatnik School of Government (<https://www.bsg.ox.ac.uk/covidtracker>). These measures track state decisions with respect to stay-at-home orders, maximum gathering size, restrictions on public transportation and internal movement, workplace closures and other restrictions and recommendations that could influence school instructional mode and parent perception of local COVID-19 risk. As with the infection rate covariate, we estimate results using the policies in place at the school year start date.

Table 1 presents descriptive statistics for the key variables based on these three sources defined for our analytical sample of 875 districts observed over each of six school years (n=5,250). We note that the enrollment data in our sample largely parallel national trends (i.e., slightly positive annual growth before declining roughly 3 percent in 2020-21). In our sample, we find that half of the districts chose remote-only instruction while the fractions choosing in-person and hybrid models were 27 percent and 23 percent, respectively. Most of these districts were also in states where at some workplaces were closed and public events cancelled while 39 percent had recommended restrictions on public transit. We relied on other time-invariant data to characterize the districts in our sample and to examine potentially heterogeneous responses to school-reopening choices. Specifically, we identified the share of children in poverty within each district using data from the Census Bureau's Small-Area Income and Poverty Estimates (SAIPE) program defined for our baseline year, 2015-16. We also identified the racial/ethnic composition of each district's students at

baseline using CCD data. Finally, we used federal Education Demographic and Geographic Estimates (EDGE) data to classify district locales (i.e., urban, suburban, town/rural). Relative to the nation, the districts in our sample are more likely to be in urban or suburban settings (Table A1). They also serve lower concentrations of white students and slightly higher concentrations of students in poverty (Table A1). In ancillary analyses, we examine how our results vary across subsets of the analytical sample defined by these traits.

II. Methods

Our first approach to examining the determinants of the natural log of enrollment in district d in year t (i.e., Y_{dt}) relies on a conventional “difference in differences” (DD) specification that conditions on both district-specific and year-specific fixed effects (i.e., α_d, γ_t):

$$Y_{dt} = \alpha_d + \gamma_t + \beta_1(R_d \times Post_t) + \beta_2(H_d \times Post_t) + \kappa X_{dt} + \varepsilon_{dt} \quad (1)$$

We allow remote-only (i.e., R_d) and hybrid (i.e., H_d) to have distinct effects relative to in-person instruction. However, we also test the null hypothesis that remote-only and hybrid instructional models have the same effects (i.e., $H_0: \beta_1 = \beta_2$). In some specifications, we compare remote-only instruction to all others (i.e., $\beta_2 = 0$). The variable, X_{dt} , captures other candidate determinants varying within districts over time (e.g., COVID-19 case rates and pandemic-related policies). The standard errors allow for clustering at the district level. We also explore the parallel-trends assumption implicit in the DD specification through estimating event-study specifications of the following form:

$$\begin{aligned} Y_{dt} = & \alpha_d + \gamma_t + \sum_{\tau=2}^5 \delta_\tau^R R_d I_{t+\tau} + \sum_{\tau=2}^5 \delta_\tau^H H_d I_{t+\tau} \\ & + \beta_1(R_d \times Post_t) + \beta_2(H_d \times Post_t) + \kappa X_{dt} + \varepsilon_{dt} \end{aligned} \quad (2)$$

In this specification, the binary indicator, $I_{t+\tau}$, equals 1 for school years τ years before t . The coefficients of interest (i.e., $\delta_\tau^R, \delta_\tau^H$) identify how enrollment in districts that chose remote-only and hybrid instruction varied in the years before that policy choice (i.e., fall 2015 through fall 2018) relative to a reference year (i.e., fall 2019) and districts that chose in-person instruction for fall 2020. We examine the statistical significance of these policy leads by testing $H_0: \delta_5^R = \delta_4^R = \delta_3^R = \delta_2^R = 0$ and $H_0: \delta_5^H = \delta_4^H = \delta_3^H = \delta_2^H = 0$.

For the enrollment outcomes we study, we see clear evidence that the parallel-trends assumption is sometimes violated. In particular, school districts that chose remote-only instruction for the 2020-21 school year tended to be larger, urban districts, which had distinctive comparative trends towards decreasing enrollment that preceded the COVID-19 pandemic (Figure 1, Table A2). Failing to account for this prior

trend could imply a negative bias in the estimated effect of remote-only instruction on enrollment. To address the implied internal-validity threat, we present estimates that control for prior, group-specific trends. In the program-evaluation literature, particularly in the fields of health and education, this approach is commonly understood as a “comparative interrupted time series” (CITS) design (Cook, Campbell, and Shadish, 2002; Jacob et al., 2016). By way of contrast, the DD approach effectively relies on the comparison group’s change from its baseline mean as the relevant counterfactual. However, the CITS approach instead uses the comparison group’s change from its baseline trend. Specifically, a basic CITS specification for our context takes the following form:

$$\begin{aligned} Y_{dt} = \alpha_1 V_d + \alpha_2 H_d + \gamma_1 Year_t + \gamma_2 (V_d \times Year_t) + \gamma_3 (H_d \times Year_t) + \gamma_4 Post_t + \\ \beta_1 (V_d \times Post_t) + \beta_2 (H_d \times Post_t) + \kappa X_{dt} + \varepsilon_{dt} \end{aligned} \quad (3)$$

where $Year_t$ is a linear trend. We also estimate a version of this specification that, like a DD design, conditions on both district and year fixed effects as well as linear trends unique to each group:

$$\begin{aligned} Y_{dt} = \alpha_d + \gamma_t + \gamma_2 (R_d \times Year_t) + \gamma_3 (H_d \times Year_t) + \\ \beta_1 (R_d \times Post_t) + \beta_2 (H_d \times Post_t) + \kappa X_{dt} + \varepsilon_{dt} \end{aligned} \quad (4)$$

The coefficients of interest (i.e., β_1, β_2) identify the 2020-21 enrollment change unique to districts that chose an alternative to in-person instruction and conditional on prior trends unique to each group. Fry and Hatfield (2021) note that the difference between such a DD design with group trends and a generalized CITS specification is “semantic” in that “both construct the same counterfactual outcomes for the treated group.”

One potential limitation of the specifications in equations (3) and (4) is the maintained assumption that the prior trends unique to remote-only and hybrid districts are indeed linear. We explore the validity and empirical relevance of this assumption in five ways. One is to consider visual evidence of the enrollment trends by district type both to assess the linearity of the prior, group-specific trends and the credibility of the impact estimates we report. Second, we also discuss the results of specifications that allow for a quadratic term in the group-specific trends. Third, as a falsification exercise, we discuss the estimated effects of a district’s chosen instructional mode on kindergarten enrollment across states where kindergarten was and was not mandatory. In the absence of specification error, we would expect the impact of district instructional mode on kindergarten to be attenuated in states where kindergarten was mandatory.

Fourth, we also explore the robustness of our findings by implementing the “honest” DD sensitivity analysis recently introduced by Rambachan and Roth (2020). Specifically, we calculate 95% confidence intervals for our key impact estimates under varying assumptions of the value, M (i.e., the largest allowable change in the slope of an underlying linear trend between two consecutive periods). Our preferred estimates

(i.e., equation 4) impose the assumption that $M = 0$ (i.e., a constant linear trend). However, these sensitivity analyses allow us to assess how wrong this assumption could be while still allowing us to reject the null hypothesis (i.e., the “breakdown” value of M). Fifth, while our results generally suggest that inferences based on equation (4) are valid, we also present complementary results based on a less restrictive “generalized synthetic control” (GSC) approach that accommodates violations of the parallel-trends assumption (Xu, 2017; Liu et al., 2020). The GSC estimates are based on an interactive fixed effects specification that models time-varying confounds as latent factors and form a counterfactual for each treated district in the treatment period. The GSC-based estimate of the average treatment effect on the treated (ATT) is the average of the difference between the observed outcome and its counterfactual estimate for all the treated observations.

III. Results

Figure 1 illustrates average K-12 district enrollment by year and fall 2019 reopening status (i.e., in-person, hybrid, or remote-only). In order to assess these comparative changes more clearly, these data are indexed so that enrollment for fall 2015 equals 100. These conditional means show that fall-2019 enrollment fell dramatically across all three types of districts. However, they also indicate distinct (and quite linear) trends across these district types prior to the focal 2019-20 school year. In particular, between fall 2015 and fall 2019, the enrollment gap between in-person and remote-only districts grew by 2.07 percent (i.e., an annual differential trend of 0.52 percent). Between fall 2019 and 2020, enrollment in in-person and remote-only districts fell by 1.99 and 3.32 percent, respectively. Combining these data suggests that, enrollment in remote-only districts fell by 0.81 percent relative to in-person districts, after adjusting for their comparative trends. In contrast, districts that chose hybrid instruction show a more modest prior trend and a fall-2019 decline in enrollment of 2.7 percent.

In Table 2, we present the key results of regressions that estimate these effects parametrically (i.e., equations (1), (3), and (4)). A basic DD specification suggests that remote-only instruction reduced enrollment by a statistically significant 2.4 percent relative to in-person instruction while hybrid instruction had small and statistically insignificant effects. However, the pre-existing trend towards lower enrollment in remote-only districts (i.e., more urban districts that were losing enrollment prior to the pandemic) implies that a DD estimator overstates the impact of this policy choice.⁴ The remaining results in Table 2 are consistent with that DD bias. That is, spare CITS specifications and DD specifications that condition on remote-only and hybrid linear trends (i.e., columns 2 through 5) indicate that the estimated impact of adopting remote-only

⁴ Event-study estimates based on equation (2) are presented in Table A2, column (1). The hypothesis of common year effects in the “pre” period is rejected for remote-only but not for hybrid instruction.

instruction is smaller but statistically significant (i.e., implying a 0.79 to 1.1 percent additional reduction in enrollment). These results also indicate that the pre-existing remote-only trend (i.e., enrollment decreases by 0.52 percent per year relative to in-person districts) is statistically significant. However, neither of the variables related to hybrid instruction are statistically significant. And F-tests consistently reject the hypothesis that remote-only and hybrid instruction had similar enrollment effects.

The estimated effects of remote-only instruction are slightly larger in specifications that include the district-year controls (i.e., columns 4 and 5). The cancellation of public events, in particular, implied a statistically significant *increase* in district enrollment (i.e., 1.5 percent) and is positively correlated with the adoption of remote-only instruction. We also explored the heterogeneity in these estimated effects by baseline district traits. We find that the effects of remote-only instruction do not vary across districts serving lower and higher concentrations of students in poverty (Table A3). However, we do find that the disenrollment effects of remote-only instruction are larger in districts serving more rural communities, higher concentrations of Hispanic students, and lower concentrations of Black students.

We explore the robustness of these results based on linear group trends in three additional ways. First, we find that adding quadratic group trends results in a similar estimate (Table A4). Second, we conducted the sensitivity analysis recommended by Rambachan and Roth (2020). This sensitivity analysis (Figure A2) indicates that the enrollment trend unique to remote-only districts would have to deviate from linearity by $M > 0.004$ in order to no longer reject the null hypothesis. This “breakdown” value is about 150 percent of the largest slope change observed in the pretreatment period. It is also equivalent to 67 percent of the upper limit on a one-sided 95% confidence interval for the largest slope change. Third, we find that generalized synthetic control (GSC) estimates also suggest the negative and statistically significant impact of remote-only instruction on enrollment with noticeably larger estimates for total enrollment (Table A5). Event-study estimates based on the GSC results (Figure A3) suggest this procedure performs well in addressing time-varying confounds, an inference supported by an equivalence test (Liu et al. 2020).

We turn next to the enrollment results by grade level. The comparative trend data in Figure 2 suggest that the enrollment declines attributable to remote-only instruction are concentrated in kindergarten and, to a lesser extent, elementary-school grades. The trends in kindergarten and elementary-school enrollment also suggest the existence of quite linear comparative trends by reopening status *prior* to fall 2020.⁵ Table 3 presents the key results from corresponding regressions that control for these linear trends and other time-varying covariates (i.e., equation (4)). These results suggest that remote-only instruction led to particularly large

⁵ Event-study estimates in Table A2 (columns 2 through 5) underscore the magnitude and statistical significance of these prior trends, particularly for remote-only districts.

enrollment reductions in lower grades and that these effects diminished monotonically in higher grade levels.⁶ Specifically, these estimates suggest that remote-only instruction reduced kindergarten enrollment by 3 to 4 percent and elementary-school enrollment by 1 percent while having smaller and statistically imprecise effects at the middle and high-school levels. These results also suggest that hybrid instruction did not have statistically significant effects with the exception of a small, positive estimate for high-school enrollment, a finding that does not appear consistent with either the visual evidence (Figure 2) or the event-study estimates (Table A2).

We explore the robustness of these grade-specific results in several ways. First, we assess the credibility of the substantial kindergarten effect of remote-only instruction by estimating these effects separately for districts in states where kindergarten was mandatory and where it was not. We find that the remote-only instruction had a smaller and statistically insignificant enrollment effect in states where kindergarten is mandatory (i.e., a 3.9 percent decrease, p -value = 0.125). In contrast, in states where kindergarten is optional, remote-only instruction reduced enrollment by 4.6 percent (p -value = 0.0001). This treatment heterogeneity is consistent with the hypothesis that the estimated effect of remote-only instruction on kindergarten enrollment is genuine. We also find that the kindergarten result is robust to conditioning on quadratic group-specific trends (Table A4). The elementary-school enrollment effect of remote-only instruction is sensitive to conditioning on quadratic trends. However, this appears to be due to an increase in imprecision rather than a substantial decrease in the point estimate. For example, the point estimate from Table A4 (i.e., -0.87 percent) would be statistically significant with the corresponding standard error from Table 3.

The Rambachan-Roth (2020) sensitivity analysis also suggests the robustness of the kindergarten result (Figure A2). Specifically, the deviation from trend linearity required to “breakdown” this result (i.e., M of roughly 0.0275 or larger) is nearly three times as large as the largest deviation from linearity observed in the pre-treatment period (and roughly equivalent to the 95% upper confidence limit on this deviation). However, for higher grade levels (i.e., elementary, middle, and high-school), this analysis does suggest more sensitivity to the presumed linearity of the group trends. Specifically, the results in Figure A2 indicate that rejection of the null hypothesis will no longer hold if deviations from linearity resemble the maximum deviations observed in the pre-treatment period. Generalized synthetic control (GSC) estimates (Table A5) provide complementary evidence on the robustness of these grade-specific estimates in Table 3. The GSC estimates also suggest a robust impact of remote-only instruction on kindergarten enrollment. These estimates also suggest that remote-only instruction led to smaller enrollment declines at higher grade levels, though some of

⁶ We also estimated these effects in models using enrollment by each grade. We plot these estimates and their confidence intervals in Figure A4. Interestingly, these results suggest that remote-only instruction led to a statistically significant decline in grade-9 enrollment but not in other high-school grades. This heterogeneity is consistent with the hypothesis that parents were particularly willing to switch their child’s school in response to remote-only instruction if they were already undertaking a school transition.

these inferences are sensitive to the inclusion of district-year controls (Table A5). The corresponding GSC-based event-study figures (Figure A5) and equivalence tests suggest that this estimation procedure performs well (Liu et al., 2020).

IV. Conclusions

During the first full school-year after the onset of the COVID-19 pandemic, enrollment in U.S. public schools fell dramatically (i.e., a decline of 1.1 million students or 2 percent of prior K-12 enrollment). This enrollment decline is an important leading indicator of the educational impact of the pandemic. In particular, the existence and magnitude of this enrollment decline implies a myriad of potential consequences for schools and students. These include the likely fiscal strain on public schools of declining enrollment as well as the diverse developmental implications for students related to the underlying behaviors driving enrollment declines such as kindergarten skipping and redshirting, school switching, truancy, and dropping out. Arguably, the major policy lever relevant to these enrollment decisions was (and continues to be) the instructional mode states and districts choose for their schools (i.e., in-person, remote-only, or a hybrid). In the fall of 2020, policymakers confronted an exceptionally difficult choice about how to reopen schools; one that required them to balance the public-health risks to their students and communities with the educational and economic harm of remote instruction.

In this study, we rely on several unique data sources to construct quasi-experimental evidence on how their fall 2020 reopening decisions influenced the disenrollment phenomenon. Our results suggest that the decision to offer remote-only instruction last fall contributed meaningfully to disenrollment from public schools. Specifically, our estimates suggest that offering remote-only instruction increased disenrollment by 42 percent (i.e., a change from 2.6 to 3.7 percent) relative to in-person instruction while hybrid instruction had small and statistically insignificant effects. To frame these effect sizes against the national decline in public-school enrollment, we note that public schools previously enrolled roughly 49 million students and that roughly 57 percent of students faced remote-only instruction as of November 2020 (Henderson, Peterson, and West, 2021). The additional enrollment decline in remote-only districts (i.e., 1.1 percentage points) implies that public schools lost roughly 300,000 K-12 students as a result of these decisions (i.e., 49 million \times 0.57 \times 0.011). This implies that the widespread adoption of remote-only instruction explains roughly a quarter of the disenrollment from public schools. Notably, we also find evidence that the effects of remote-only instruction on the decline in public-school enrollment were particularly concentrated in kindergarten and elementary-school grades.

Overall, these results suggest that significant numbers of parents, particularly parents of younger children, did not want their children to participate in remote instruction. Notably, this pattern of revealed preferences corresponds strongly with the conclusions of a National Academies of Science, Engineering, and Medicine (2020) panel that recommended full-time, in-person instruction for students in grades K-5. Beyond revealing these parental preferences, these results have at least two broad implications for understanding the consequences of school-reopening decisions. First, these results suggest that the decision to offer remote-only instruction will have negative fiscal consequences for school districts if disenrolled students do not return. In particular, because the disenrolled students are disproportionately younger, this effect may be long-lasting. Uncertainty about the instructional model local public schools will offer in the near future (e.g., in response to the growing prevalence of COVID-19 variants) may also contribute to sustained disenrollment.

Second, these results provide early evidence on the educational challenges created by these reopening decisions (and the pandemic, more generally). For example, the sharp drop in kindergarten enrollment suggests schools (i.e., particularly those that offered remote-only instruction) will face the challenge of educating rising first graders who are in their first experience of formal schooling. However, to the extent that a substantial number of students instead “redshirts” into kindergarten classes this fall, schools may face the staffing and pedagogical challenge of managing an unusually large, mixed-age cohort. In the absence of responsive staffing, these COVID-19 redshirters and their peers could experience unusually large class sizes throughout their schooling. The enrollment declines we document suggest that offering remote-only instruction could also have contributed to truancy, particularly among elementary-school students (i.e., to the extent, these students are not homeschooled or attending private schools). However, the absence of evidence that remote-only instruction reduced high-school enrollment suggests it did not contribute meaningfully to dropout behavior.

Clearly, there is still much to learn about the educational experiences of students who disenrolled during the 2020-21 school year (as well as those who remained in their schools) and that will be possible as richer data (e.g., longitudinal student-level data) become available (Oster, 2021). In that regard, we note that community and family contexts also appear to play an important role in moderating the enrollment impact of remote-only instruction. For example, the evidence that the disenrollment effects of remote-only instruction were larger in more rural communities and in districts serving higher concentrations of Hispanic students raises questions about the sources of this heterogeneity (e.g., differential access to high-speed Internet and relevant devices). Interestingly, the disenrollment effect of remote instruction in districts serving higher concentrations of Black students was nearly twice as large as in districts serving lower concentrations of Black students, a finding consistent with the survey evidence that Black parents disproportionately supported remote

learning (Harris, 2020). The drivers and policy implications of these varied contextual factors merit further scrutiny. Regardless, these results provide clear and leading evidence that offering remote-only rather than in-person instruction was a highly consequential choice for schools and students.

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Table 1 - Descriptive Statistics

Variables	Mean	SD	Minimum	Maximum
District enrollment at baseline (K-12)	19,599	48,580	42	955,003
Kindergarten	1,450	3,069	1	60,408
Elementary school (1-5)	7,456	14,709	16	249,809
Middle school (6-8)	4,235	8,203	9	139,555
High school (9-12)	5,802	11,263	16	189,565
<i>Fall 2020 Instructional Mode</i>				
Remote only	0.5	0.50	0	1
Hybrid	0.23	0.42	0	1
In person	0.27	0.45	0	1
<i>District-Year Covariates</i>				
COVID-19 case rate per 100,000 people	12.58	28.22	0	782.86
Workplaces closed for at least some sectors/workers	0.78	0.41	0	1
Public events cancelled	0.62	0.49	0	1
Public transit restrictions required or recommended	0.39	0.49	0	1

Notes: Our main analytical sample includes district-grade panels of 875 school districts observed annually over six years (i.e., fall 2015 through fall 2020, n = 5,250). Eight-hundred and sixty-four districts serve kindergarten and middle school grades (n = 5,184) and 826 serve high school grades (n = 4,956). See text for data sources.

Figure 1. Annual District K-12 Enrollment (Fall 2015 = 100)

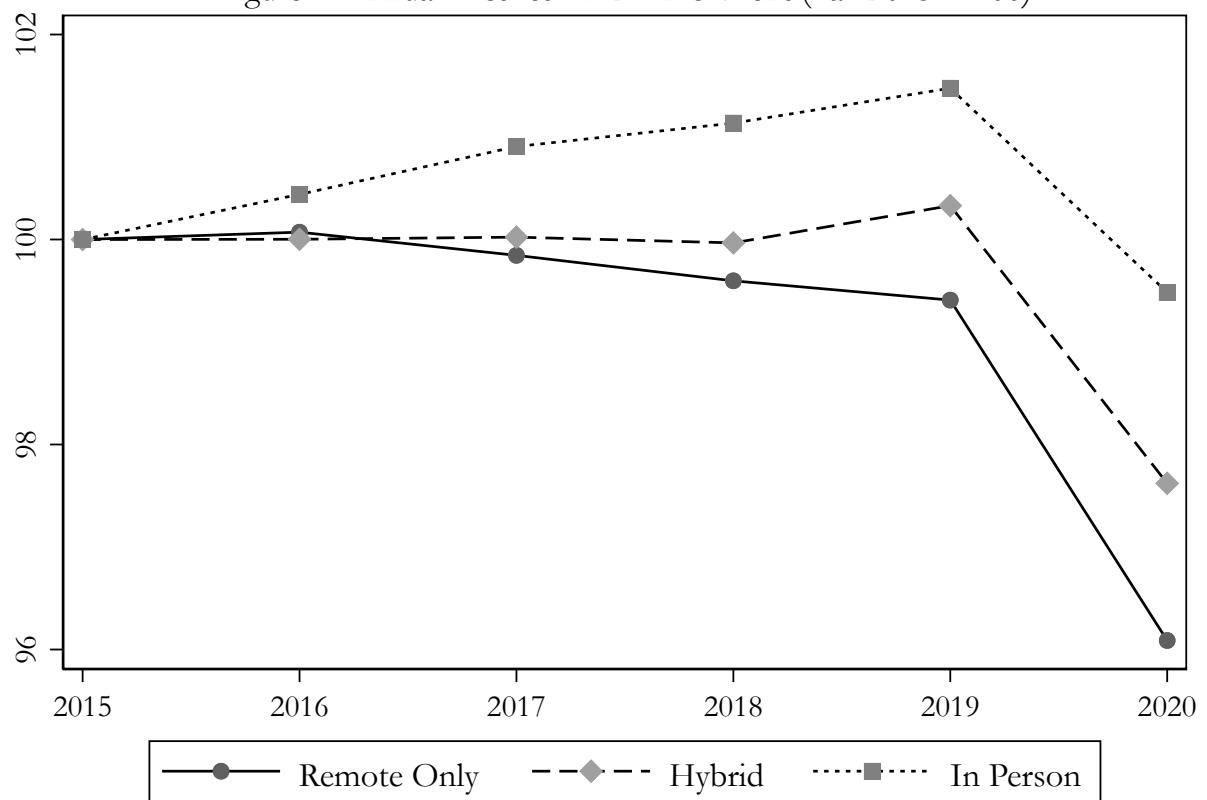


Table 2 - Estimated Determinants of K-12 District Enrollment

Independent Variable	(1)	(2)	(3)	(4)	(5)
Remote Only \times Post	-0.0236*** (0.0049)	-0.0079** (0.0025)	-0.0079** (0.0025)	-0.0105*** (0.0027)	-0.0110*** (0.0024)
Hybrid \times Post	-0.0100 (0.0053)	-0.0014 (0.0026)	-0.0014 (0.0026)	0.0009 (0.0029)	-
Remote Only \times Trend	- -	-0.0052*** (0.0015)	-0.0052*** (0.0015)	-0.0052*** (0.0015)	-0.0039** (0.0012)
Hybrid \times Trend	- -	-0.0029 (0.0017)	-0.0029 (0.0017)	-0.0029 (0.0017)	-
Remote Only District	- -	0.4069*** (0.1041)	- -	- -	- -
Hybrid District	- -	-0.5011*** (0.1163)	- -	- -	- -
Post	- -	-0.0256*** (0.0019)	- -	- -	- -
Trend	- -	0.0029* (0.0012)	- -	- -	- -
District Fixed Effects	Yes	No	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	Yes	Yes
District-Year Covariates	No	No	No	Yes	Yes
<i>p</i> value: ($H_0: \beta_1 = \beta_2$)	0.0021	0.0080	0.0080	0.0001	-

Notes: These estimates are based on data from 875 school districts observed over each of six years (i.e., fall 2015 through fall 2020, $n = 5,250$). The dependent variable is the natural log of enrollment. The district-year covariates include four pandemic-related variables: county-level COVID-19 case rate per person, workplace closures, public event cancellations, and public-transit restrictions (see Table 1 and text for details). Standard errors clustered at the district level are reported in parentheses.

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

Figure 2. Annual District Enrollment (Fall 2015 = 100)

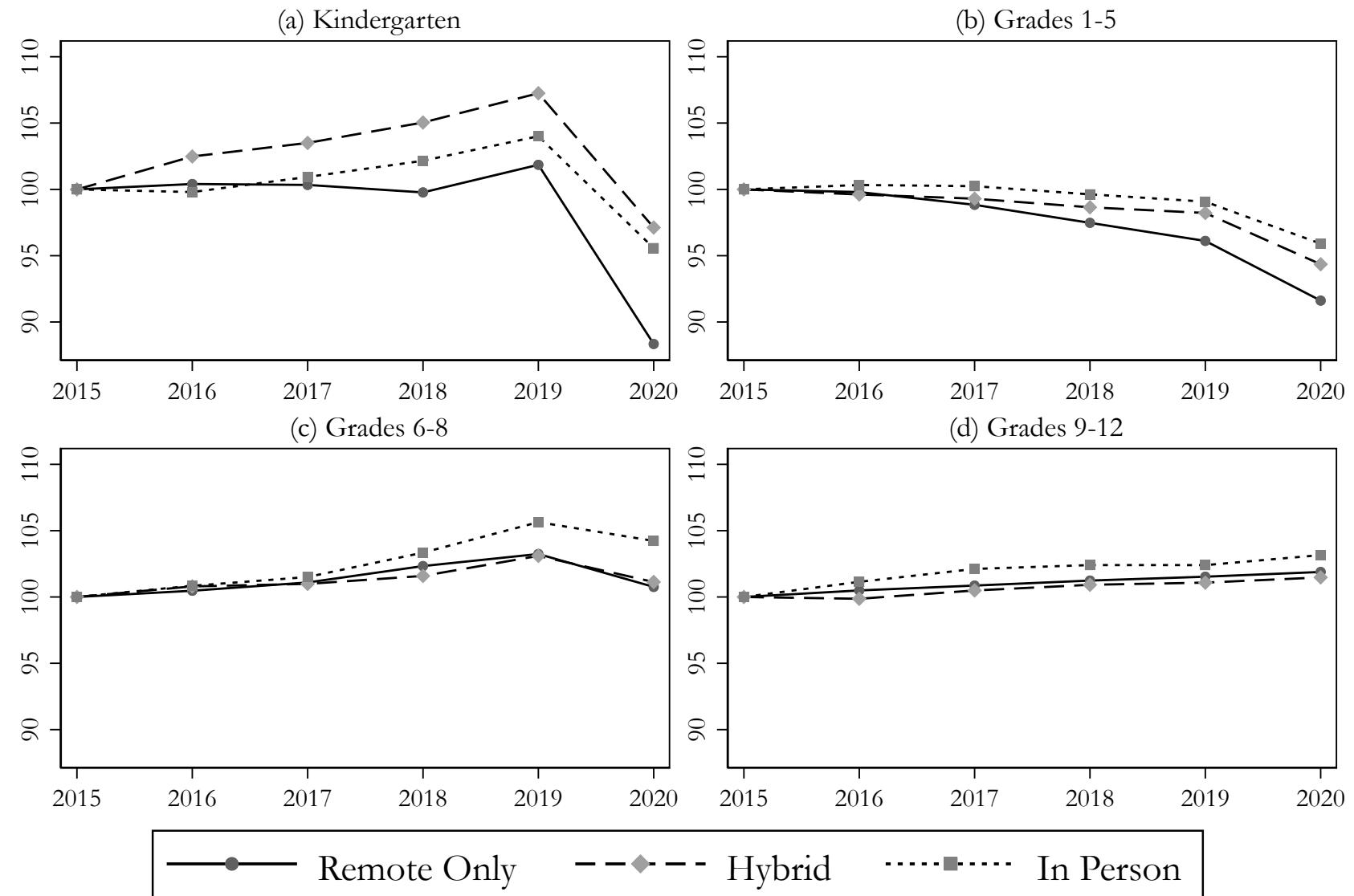


Table 3 - Estimated Determinants of Enrollment by Grade

Independent Variable	K-12		Kindergarten		Grades 1-5		Grades 6-8		Grades 9-12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Remote Only × Post	-0.0105*** (0.0027)	-0.0110*** (0.0024)	-0.0415*** (0.0104)	-0.0325*** (0.0096)	-0.0110** (0.0037)	-0.0111*** (0.0032)	-0.0083 (0.0043)	-0.0093** (0.0035)	-0.0004 (0.0041)	-0.0052 (0.0034)
Hybrid × Post	0.0009 (0.0029)	-	-0.0204 (0.0116)	-	0.0000 (0.0042)	-	0.0012 (0.0050)	-	0.0085* (0.0041)	-
Remote Only × Trend	-0.0052*** (0.0015)	-0.0039** (0.0012)	-0.0098** (0.0030)	-0.0118*** (0.0028)	-0.0078*** (0.0017)	-0.0068*** (0.0015)	-0.0052** (0.0019)	-0.0015 (0.0016)	-0.0021 (0.0021)	-0.0005 (0.0016)
Hybrid × Trend	-0.0029 (0.0017)	-	0.0043 (0.0038)	-	-0.0021 (0.0021)	-	-0.0067** (0.0023)	-	-0.0027 (0.0024)	-
<i>p</i> value: ($H_0: \beta_1 = \beta_2$)	0.0001	-	0.0786	-	0.0051	-	0.0256	-	0.0210	-

Notes: These estimates are based on data from 875 school districts observed annually over each of six years (i.e., fall 2015 through fall 2020, $n = 5,250$). Eight-hundred and sixty-four districts serve elementary and middle school grades ($n = 5,184$) and 826 serve grades 9-12 ($n = 4,956$). The dependent variable is the natural log of enrollment. All models include district fixed effects (FE), year FE, and district-year covariates. The district-year covariates include four pandemic-related variables: county-level COVID-19 case rate per person, workplace closures, public event cancellations, and public-transit restrictions (see Table 1 and text for details). Standard errors clustered at the district level are reported in parentheses.

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

DATA APPENDIX

I. Sample construction

The construction of our analytical sample of school districts begins with the nearly 1,200 school districts for which Burbio tracked school-enrollment modes. We excluded from the Burbio sample and our analysis nine observations representing charter-school networks and several entities that functioned as unconventional, administrative districts (e.g., a multi-county unit managing magnet schools, others managing vocational schools). As noted in the text, this sample of school districts draws from all 50 states, overrepresents larger school districts, and represents nearly half of all public-school students in the U.S. We matched these district data to federal and state data on district enrollment for the 2015-16 through 2020-21 school years. For the focal 2020-21 school year, we relied exclusively on state-sourced data collected through the Stanford School Enrollment Project as federal data are not yet available. Specifically, these 2020-21 data were available for 34 states. Our resulting sample of 875 traditional school districts covers 35% of all 2019-20 K-12 public-school enrollment in the U.S. (Table A1).

As noted in the text, we conducted three overlapping quality-control assessments of these enrollment data. One was to construct year-on-year percent changes in enrollment for every school district in our sample and to examine individual districts where the absolute value of these changes met or exceeded an arbitrary threshold (i.e., 10 percent). Second, we compared the state-sourced enrollment data for the 2020-21 school year to a recent and preliminary federal release of 2020-21 data, which only reported PK-12 totals at the district level. Third, we also constructed state-sourced data for the 2019-20 school year and compared these data to the CCD counts and examined individual districts where the absolute value of this gap exceeded 3% at any grade-level, or overall. We found that these counts were identical for 79% of grade-district observations in the 21 states where a longitudinal enrollment profile was constructed using both CCD and state sources. Furthermore, 98% of these observations exhibited a gap in student counts of less than one percent of the federally sourced value (see Figure A1). However, these checks also surfaced several issues with state and federal reporting conventions as well as some misreporting that guided further investigation and edits to the data.

Specifically, in 14 states (i.e., CA, CT, HI, ID, IN, MO, NJ, NM, NY, OH, OK, PA, UT and WA), we found evidence of fairly systemic misalignment between state and federally sourced enrollment counts for at least some districts. Our investigation indicated that the reasons for this incongruity varied by state. For example, in some cases, federal sources were inconsistent in identifying charter-school enrollment that was under the aegis of a traditional school district. In some states, these sources also differed in how they categorized the grade of special-education students, 4-year olds in the first year of a 2-year kindergarten, and students receiving services from alternative education programs or private vendors. To ensure the comparability of within-district enrollment changes over time, we relied exclusively on state-sourced data for the sampled districts in 12 of these states. State-sourced 2015-16 data were not available for OK; therefore, we combined a single year of federally sourced data with state data for 2016-17 through 2020-21. The federal enrollment data for OK during this period relied on reporting standards consistent with state data (e.g., we vetted this by comparing federal and state-sourced OK data for 2016-17). In New Mexico, the disparity between federal and state counts was due to the inclusion of some private-school students in the state-sourced district-level data. So, we used school-level 2020-21 enrollment data from the state to remove private-school students and vetted these results using the preliminary federal release of PK-12 enrollment data for 2020-21. We ultimately combine 2015-16 through 2019-20 federal data with the single year of edited state data for New Mexico.

The remaining edits guided by our quality-control checks and the resulting investigations focused on district-specific reporting issues. Specifically, in addition to the three checks described above, we also identified and investigated every district in our sample that started to report enrollment in grades they did not previously serve, as well as districts that reported totals that did not correspond to their NCES-defined grade spans. We found two districts, Norwich School District (CT) and Oroville City Elementary (CA) that reported sporadic non-zero counts in high school grades, but district websites stated they only served K-8 students and, in the case of Norwich, adult education students. We exclude grade 9 through grade 12 totals for these districts in all years. Relatedly, we note three districts in California (Redding Elementary, Chula Vista Elementary and Jefferson Elementary) that report small numbers of high school students in all years but are assigned “NA” status in the Burbio audit for grades 9-12 and, for Chula Vista Elementary, grades 6-8. We exclude totals from the grade levels without a defined fall 2020 instructional mode in all years. Similarly, grade 6 totals from a further two districts, Hope Elementary and Del Mar Elementary (both CA), are dropped due to “NA” middle school Burbio values for those districts.

We also identified three school districts where there appear to be data-entry errors in the federal files. Both state and federal sources indicate that Bensalem Township School District (PA) had 4 kindergarten students in 2015-16. However, they typically report over 400 kindergarten students. Therefore, we imputed this value using the grade-1 enrollment for 2016-17. Similarly, the federal data for Rochester School District (WA) dramatically understates for 2018-19 enrollment for grades 2, 5, and 8. We replaced these values with state-sourced data which are consistent with the longitudinal profile of the district. Additionally, the 2020-2021 state-sourced file for Pennsylvania enrollment reported 0 grade 3 students in Penns Valley School District (PA), when both federal and state data reported approximately 100 students for that grade in prior years. This value was replaced with the state-sourced student total for grade 2 in 2019-2020.

We also found that, for two school districts in New Jersey during the 2015-16 school (Branchburg Township and Commercial Township), the federal data reports positive enrollment in grades not served by the district. We exclude the enrollment counts for these grades. We also found that state but not federal sources for Pendleton School District (OR) reported small counts of students in alternative schools within district totals. We used school-level data from 2020-21 to manually remove these students from our totals.

Additionally, we observed a change in how enrollment in New York City totals were reported by grade in fall 2020. In prior years, New York City reported approximately 20,000 ungraded students per year. For 2020-21 the ungraded counted was under 500 students. As we cannot verify how these students who would previously have been classified as “ungraded” were distributed into the grade-level totals, we dropped New York City grade-level enrollment counts from our dataset but do retain the K-12 all-New York City total in our analytical sample.

We also note limited cases of student count suppression that exist in our enrollment data. First, there are five districts where the 2020-21 state reports suppressed the low-enrollment count for a single grade: Hitchcock ISD (TX), Mission CISD (TX), Ajo Unified School District (AZ), Littlefield Unified School District (AZ), Harrison County School District (MS). In all of these cases, we also find that our results are robust to excluding these 5 districts. In our analysis, we impute these values using the 2019-20 data for the prior grade with the exception of Mission CISD (TX). For that district we opt not to impute the value from the prior year (944 students) as the suppressed value is top-coded at 940

students. In this case, we retain the top-coded value. Additionally, for about half of sample districts in Ohio, fewer than 10 ungraded students were reported in each year. Because of these low counts, the exact value is masked. We impute a total of 4.5 for these observations. These imputations become part of the total K-12 enrollment variable, which is based on aggregating K through 12 and ungraded enrollment. However, our results are robust to dropping these districts where some data are masked.

To ensure that no conflation between suppressed, missing and zero totals had occurred, we investigated all zero counts within a district's federally defined grade-span excepting pre-kindergarten and adult education. NCES directory information indicates that El Dorado Union High serves grade 8, but it reported totals of 0 across all years for this grade level so those grade-level observations are dropped from the sample. There is also one school district in the state of Washington (Skykomish School District) which has zero enrollment in some grades and years. We impute 0.5 enrollment for those observations so that the log transform is defined and condition on a dummy variable that identifies imputed observations. Finally, multiple years of kindergarten (i.e., 4-year-old-K and 5-year-old-K) were reported in PA and WI. We classify only the 5-year-old cohort as kindergartners. We observe six years of enrollment data for all grades served within every district in our sample, so every district-grade-year panel is balanced

We also identified three districts with dramatic changes in school composition between the 2015-16 and 2020-21 school years: New Orleans Public Schools (LA), Maricopa Unified (CA) and Buckeye Union Elementary (CA). The governance of New Orleans schools changed in 2018, while the California districts became sites for rapidly growing charter schools in recent years. All three districts were dropped from our sample. We also exclude one district from Florida (Polk County) where federal and state data are not comparable, but a clear solution using school-level data could not be identified.

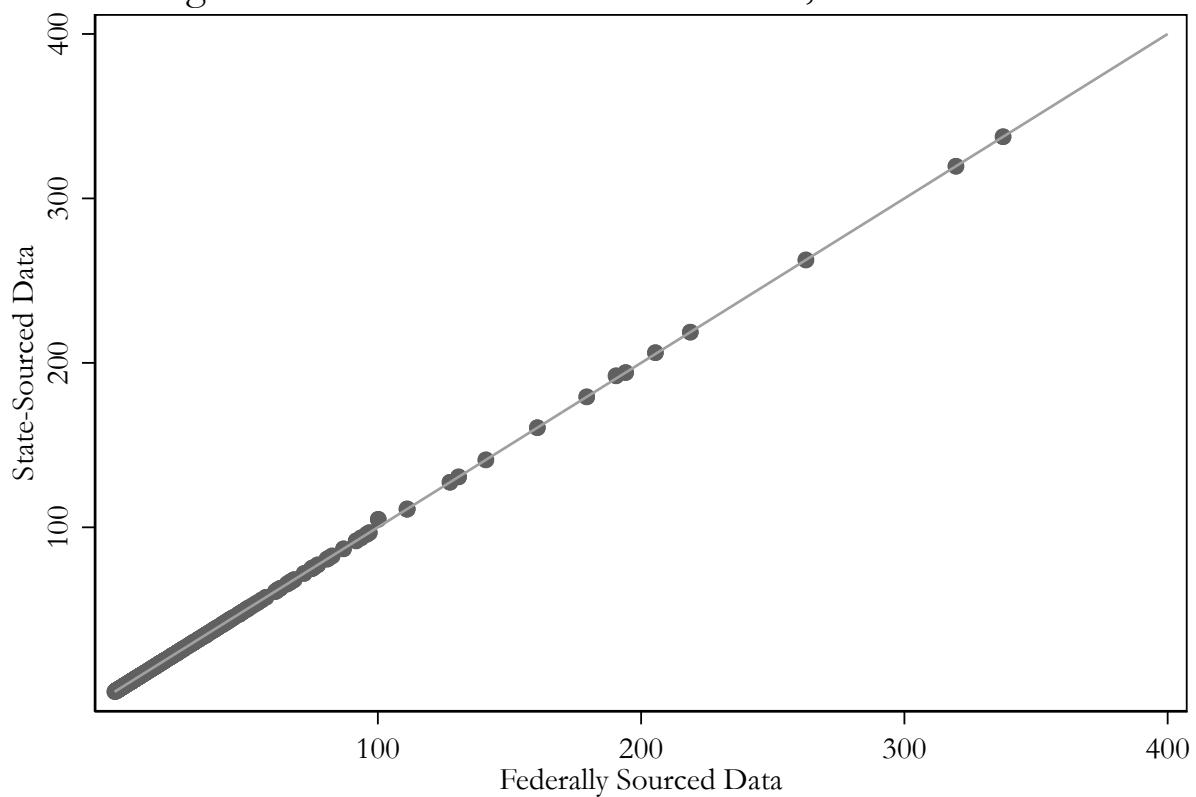
We further note that California, Nebraska, New Hampshire and Oklahoma did not directly report grade-specific counts at the district level, so we aggregated school-level totals by district to construct our outcome measures. In addition to the above quality checks, we spot-checked districts in these states using state data portals to confirm the aggregation yielded accurate totals.

After these corrections to the data, we repeated our outlier analysis to check for further issues and found none. Some districts retained in our sample did experience year-to-year enrollment changes greater than 10 percent, but when these changes appeared to be genuine (e.g., following a natural disaster) we made no adjustments.

II. COVID-19 Policy Data

Based on the policy variation observed in our sample, we dichotomized three categorical OxCGRT COVID-19 non-pharmaceutical intervention variables to use as district-year controls. First, “workclose” takes on a value of 1 when states “require closing (or work from home) for some sectors or categories of workers” and 0 when states merely recommend this. No other work closure policies were observed in our sample. Similarly, no states in our sample had “no measures” with respect to public events at the outset of the 2020-21 school year, so “nopublicevents” is 1 when canceling public events if required, and 0 if canceling is only recommend. Finally, we combine required and recommend closures (which may include substantial volume reduction) of public transit in defining the “closetransit” policy variable. The value of “closetransit” is 0 when no measures related to public transit are in place.

Figure A1. Fall 2019 K-12 Enrollment, 1000s of students



Note: This figure compares K-12 enrollment totals computed from both state and federal sources for the 352 districts in our sample draw from the 20 states for which we compare 2020-21 enrollment from state sources with prior years of enrollment data from the NCES CCD. These districts are encompassed by the following states: Alaska, Arkansas, Arizona, Colorado, Florida, Georgia, Louisiana, Massachusetts, Maryland, Minnesota, Mississippi, Nebraska, New Hampshire, Nevada, Oregon, South Carolina, South Dakota, Texas, Wisconsin, and Wyoming.

Table A1 - Comparison of Analytical Sample and All U.S. Public School Districts

	Analytical Sample	All U.S public school districts
No. States	34	51
No. Districts	875	13,584
No. K-12 Students	17,149,239	49,036,000
Average district enrollment	19,599	3,610
<i>Share of students attending districts classified as:</i>		
City	49.5%	30.2%
Suburban	46.2%	29.7%
Town	1.4%	11.3%
Rural	3.0%	18.7%
<i>Share of students attending districts in the:</i>		
Northeast	15.7%	15.7%
West	30.3%	24.6%
South	44.0%	39.3%
Midwest	10.0%	20.9%
<i>Share of students at baseline that are:</i>		
Black	18.9%	15.4%
White	36.9%	48.9%
Hispanic	32.8%	25.9%
AAPI	7.2%	5.4%
AI/NA	0.6%	1.0%
Two or more races	3.6%	3.4%
In Poverty	23.9%	19.1%

Notes: The sources for these variables are the NCES Digest of Education Statistics, NCES Common Core of Data, both defined for 2015-16, the baseline year in this study. The poverty data are based on the NCES' 2017-18 district-level EDGE data.

Table A2 - Estimated Determinants of Enrollment by Grade

Independent Variable	K-12	Kindergarten	Grades 1-5	Grades 6-8	Grades 9-12
	(1)	(2)	(3)	(4)	(5)
Remote Only \times 5-Year Lead	0.0204*** (0.0060)	0.0340** (0.0128)	0.0307*** (0.0070)	0.0232** (0.0078)	0.0081 (0.0086)
Remote Only \times 4-Year Lead	0.0166*** (0.0047)	0.0330** (0.0111)	0.0250*** (0.0057)	0.0193** (0.0066)	0.0016 (0.0071)
Remote Only \times 3-Year Lead	0.0097** (0.0035)	0.0198* (0.0091)	0.0161*** (0.0044)	0.0195*** (0.0053)	-0.0042 (0.0053)
Remote Only \times 2-Year Lead	0.0052* (0.0024)	0.0028 (0.0078)	0.0087** (0.0031)	0.0136*** (0.0039)	-0.0027 (0.0035)
Remote Only \times Post	-0.0157*** (0.0027)	-0.0530*** (0.0091)	-0.0182*** (0.0037)	-0.0088* (0.0040)	-0.0060 (0.0035)
Hybrid \times 5-Year Lead	0.0107 (0.0068)	-0.0238 (0.0155)	0.0089 (0.0082)	0.0247** (0.0088)	0.0127 (0.0095)
Hybrid \times 4-Year Lead	0.0064 (0.0054)	0.0006 (0.0141)	0.0016 (0.0067)	0.0243** (0.0075)	-0.0001 (0.0076)
Hybrid \times 3-Year Lead	0.0020 (0.0038)	0.0001 (0.0118)	-0.0008 (0.0049)	0.0196*** (0.0057)	-0.0034 (0.0056)
Hybrid \times 2-Year Lead	-0.0007 (0.0023)	-0.0039 (0.0104)	-0.0013 (0.0033)	0.0069 (0.0038)	-0.0018 (0.0035)
Hybrid \times Fall Post	-0.0040 (0.0029)	-0.0128 (0.0106)	-0.0045 (0.0040)	-0.0037 (0.0045)	0.0018 (0.0036)
<i>p value</i> : $H_0: \delta_5^R = \delta_4^R = \delta_3^R = \delta_2^R = 0$	0.0046	0.0062	0.0003	0.0060	0.0396
<i>p value</i> : $H_0: \delta_5^H = \delta_4^H = \delta_3^H = \delta_2^H = 0$	0.0512	0.1719	0.0912	0.0064	0.0009

Notes: These estimates are based on data from 875 school districts observed over each of six years (i.e., fall 2015 through fall 2020, n = 5,250). Eight-hundred and sixty-four districts serve elementary and middle school grades (n = 5,184) and 826 serve grades 9-12 (n = 4,956). The dependent variable is the natural log of enrollment. The reference category is relative to fall 2019, one year prior to post. All models include district fixed effects (FE) and year FE and district-year covariates. The district-year covariates include four pandemic-related variables: county-level COVID-19 case rate per person, workplace closures, public event cancellations, and public-transit restrictions (see Table 1 and text for details). Standard errors clustered at the district level are reported in parentheses. Standard errors clustered at the

* p<0.05 ** p<0.01 *** p<0.001

Table A3 - Estimated Determinants of K-12 Enrollment by District Traits

Independent Variable	Urbanicity			Share of Students in Poverty		Hispanic Enrollment Share			Black Enrollment Share		
	Urbanicity			Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Above Median	
	Town/Rural	Suburban	City	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Remote Only × Post	-0.0224** (0.0081)	-0.0077* (0.0037)	-0.0083 (0.0045)	-0.0109** (0.0042)	-0.0092* (0.0036)	-0.0069 (0.0044)	-0.0130** (0.0041)	-0.0129** (0.0045)	-0.0069* (0.0034)		
Hybrid × Post	-0.0040 (0.0098)	0.0038 (0.0031)	-0.0044 (0.0052)	0.0037 (0.0044)	-0.0006 (0.0045)	-0.0004 (0.0034)	0.0016 (0.0061)	0.0026 (0.0046)	0.0018 (0.0042)		
Remote Only × Trend	-0.0049 (0.0046)	-0.0031 (0.0018)	-0.0076*** (0.0023)	-0.0030 (0.0022)	-0.0058** (0.0018)	-0.0034 (0.0023)	-0.0096*** (0.0022)	-0.0044* (0.0022)	-0.0063** (0.0020)		
Hybrid × Trend	-0.0077 (0.0046)	-0.0011 (0.0020)	-0.0027 (0.0025)	-0.0046 (0.0024)	-0.0027 (0.0025)	0.0005 (0.0021)	-0.0090** (0.0028)	-0.0030 (0.0025)	-0.0025 (0.0023)		
p value: ($H_0: \beta_1 = \beta_2$)	0.0711	0.0046	0.3998	0.0010	0.0463	0.1474	0.0025	0.0012	0.0205		

Notes: These estimates are based on data from 875 school districts observed over each of six years (i.e., fall 2015 through fall 2020, n = 5,250). The dependent variable is the natural log of enrollment. All models include district fixed effects (FE), year FE, and district-year covariates. The district-year covariates include found pandemic-related variables: county-level COVID-19 case rate per person, workplace closures, public event cancellations, and public-transit restrictions (see Table 1 and text for details). The share of students who are Black, Hispanic or in poverty is measured at the fall 2015 baseline (see text for additional details). Data on urbancity are from Education Demographic and Geographic Estimates from NCES. Poverty data are from the Small-Area Income and Poverty Estimates (SAIPE) program by the U.S. Census Bureau. Race/ethnicity data are from the NCES' CCD. Information on state kindergarten policies is from the Education Commission of States (ECS). Standard errors clustered at the district level are reported in parentheses.

* p<0.05 ** p<0.01 *** p<0.001

Table A4 - Estimated Determinants of Enrollment by Grade

Independent Variable	K-12	Kindergarten	Grades 1-5	Grades 6-8	Grades 9-12
	(1)	(2)	(3)	(4)	(5)
Remote Only \times Post	-0.0102** (0.0033)	-0.0378** (0.0128)	-0.0087 (0.0047)	0.0044 (0.0059)	-0.0133** (0.0047)
Hybrid \times Post	-0.0050 (0.0033)	0.0019 (0.0163)	-0.0095 (0.0048)	0.0117 (0.0065)	-0.0086 (0.0047)
Remote Only \times Trend	-0.0053* (0.0023)	-0.0119 (0.0067)	-0.0090** (0.0030)	-0.0125*** (0.0037)	0.0053 (0.0035)
Hybrid \times Trend	0.0005 (0.0023)	-0.0084 (0.0083)	0.0034 (0.0032)	-0.0127** (0.0039)	0.0071* (0.0036)
Remote Only X Trend ²	-0.0000 (0.0004)	-0.0005 (0.0016)	-0.0003 (0.0006)	-0.0018* (0.0008)	0.0018** (0.0006)
Hybrid X Trend ²	0.0008* (0.0004)	-0.0032 (0.0018)	0.0014* (0.0006)	-0.0015 (0.0008)	0.0024*** (0.0007)
p value: ($H_0: \beta_1 = \beta_2$)	0.1054	0.0116	0.8700	0.1439	0.2924

Notes: This table shows alternate specifications to the main results in Table 3 by adding mode-specific quadratic trends. These estimates are based on data from 875 school districts observed annually over each of six years (i.e., fall 2015 through fall 2020, n = 5,250). Eight-hundred and sixty-four districts serve elementary and middle school grades (n = 5,184) and 826 serve grades 9-12 (n = 4,956). The dependent variable is the natural log of enrollment. All models include district fixed effects (FE), year FE, and district-year covariates. The district-year covariates include four pandemic-related variables: county-level COVID-19 case rate per person, workplace closures, public event cancellations, and public-transit restrictions (see Table 1 and text for details). Standard errors clustered at the district level are reported in parentheses.

* p<0.05 ** p<0.01 *** p<0.001

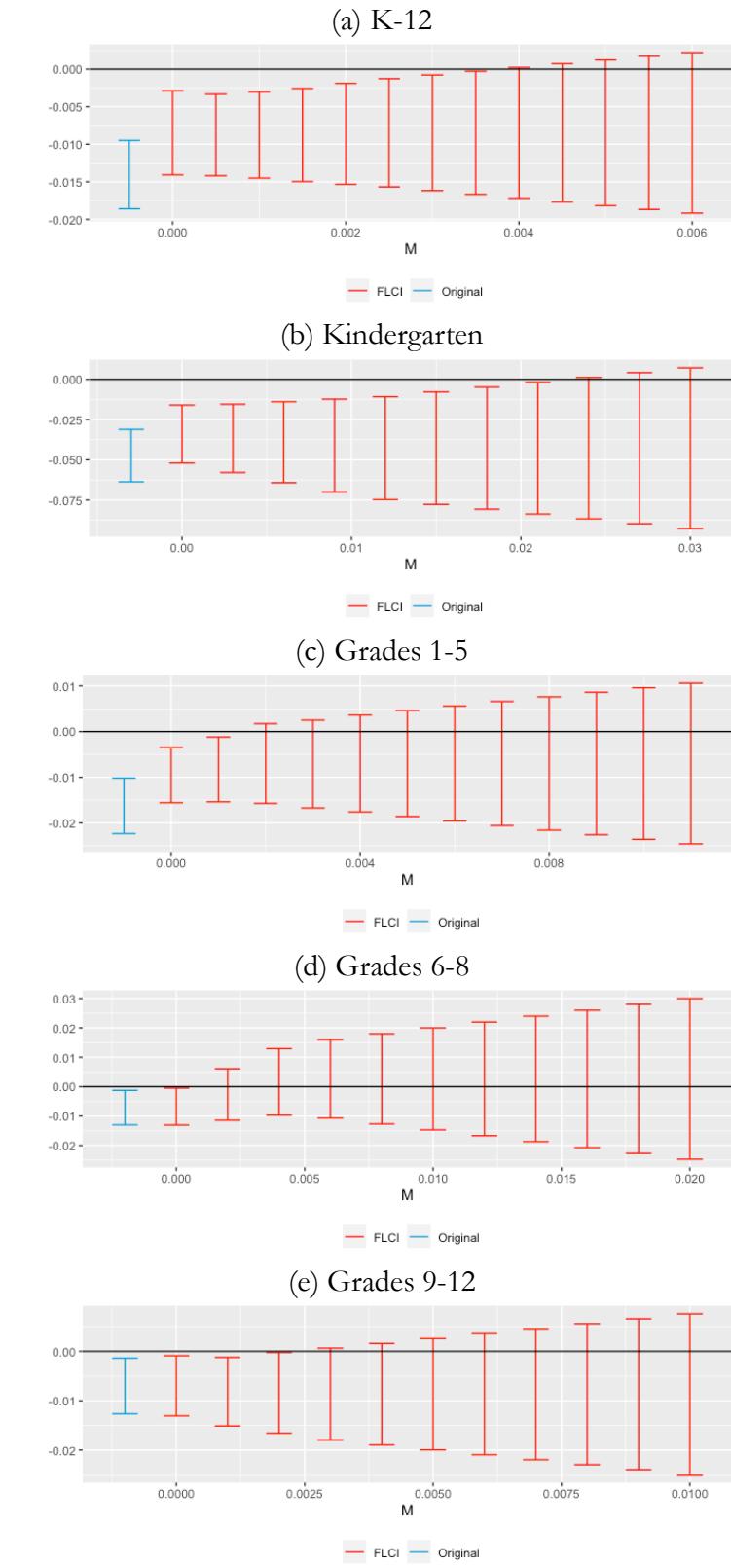


Figure A2 – Sensitivity Analyses (Rambachan and Roth, 2020)

Table A5 - Estimated Effect of Remote-Only Instruction on District Enrollment, Generalized Synthetic Control

	K-12	Kindergarten	Grades 1-5	Grades 6-8	Grades 9-12
Independent Variable	(1)	(2)	(3)	(4)	(5)
<u>(A) Without District-Year Covariates</u>					
Remote Only × Fall 2020	-0.0190*** (0.0039)	-0.0468*** (0.0135)	-0.0176** (0.0069)	-0.0194* (0.0082)	-0.0033 (0.0052)
<u>(B) With District-Year Covariates</u>					
Remote Only × Fall 2020	-0.0282*** (0.0059)	-0.0439*** (0.0135)	-0.0164 (0.0090)	-0.0255* (0.0113)	-0.0106 (0.0066)

Notes: Coefficients estimated for Remote Only X Post are estimated with 428 districts ($n = 2,568$) for kindergarten through grade 5 enrollment, 440 districts for grades 6-8 ($n = 2,640$), 414 districts for grades 9-12 ($n = 2,484$) and 437 districts ($n = 2,550$) for K-12 enrollment. The district-year covariates include found pandemic-related variables: county-level COVID-19 case rate per person, workplace closures, public event cancellations, and public-transit restrictions (see Table 1 and text for details). Standard errors are computed using 1,000 block bootstraps clustered at the district level.

* $p < 0.05$ ** $p < 0.01$ *** $p < 0.001$

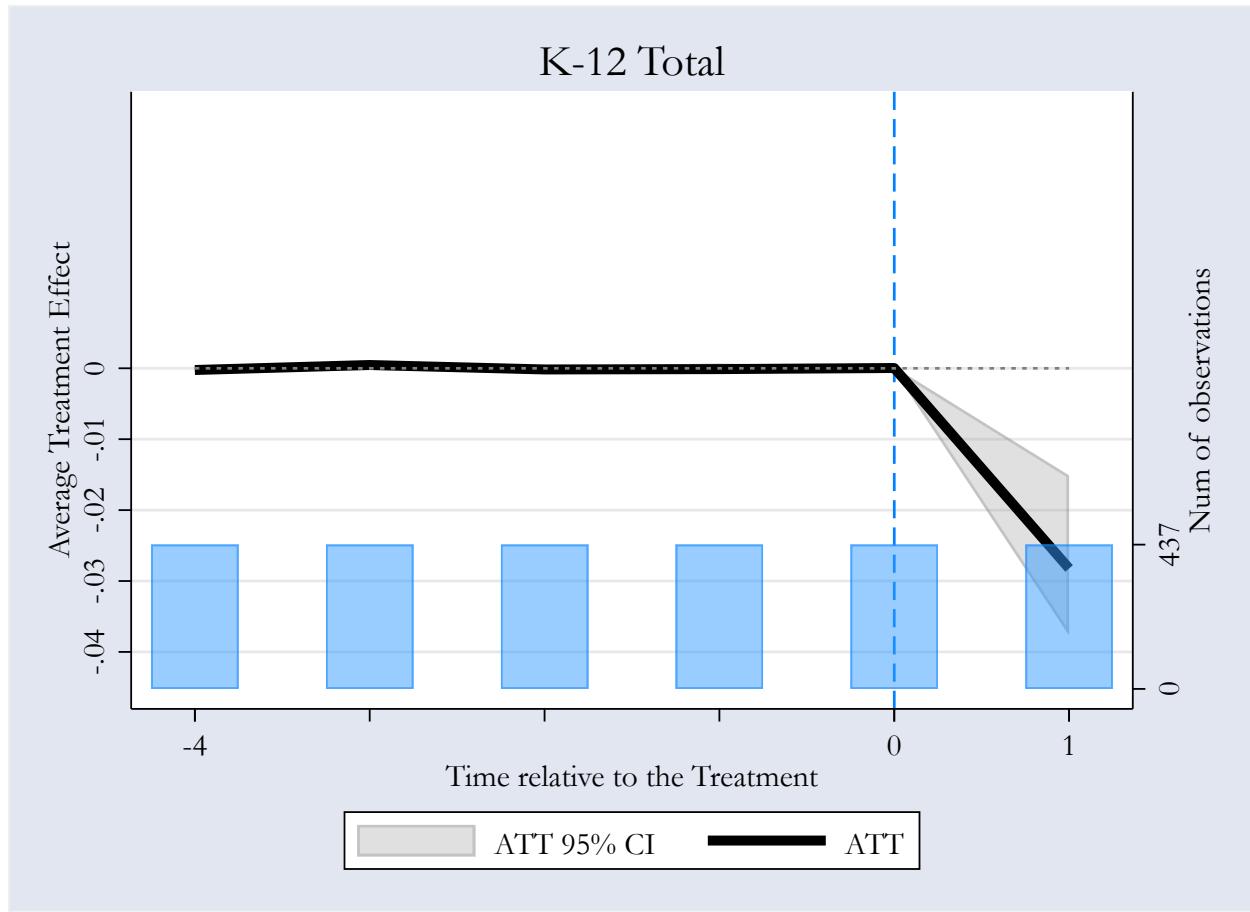
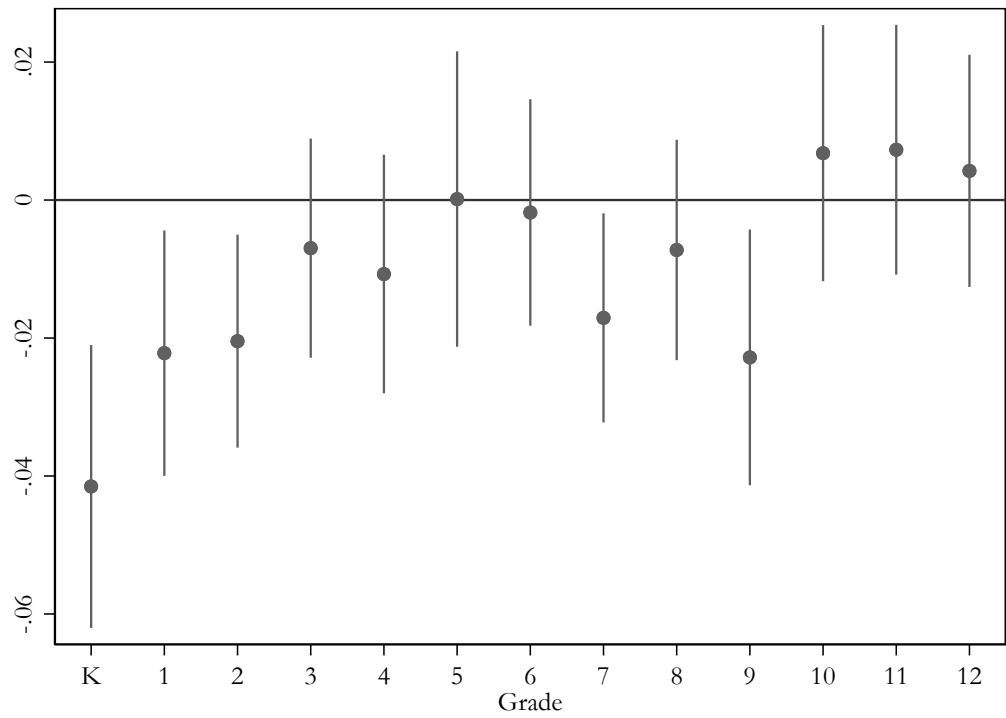
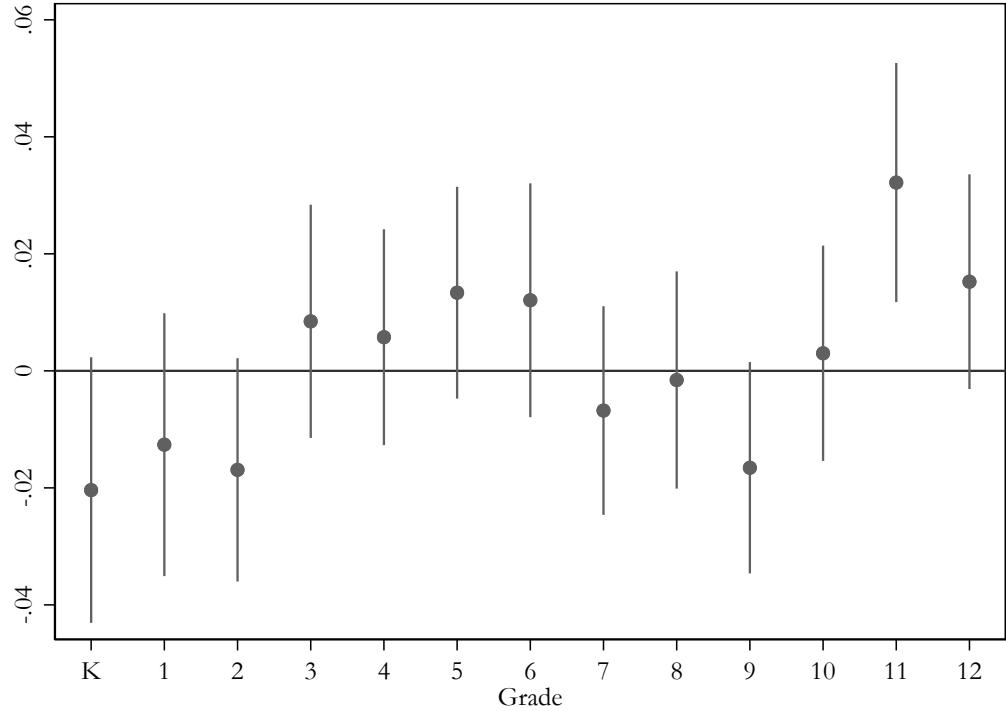


Figure A3 – GSC Event Study, K-12 Enrollment



(a) Remote Only



(b) Hybrid

Figure A4 – Estimated Effects of Instructional Mode on Enrollment by Single Grade

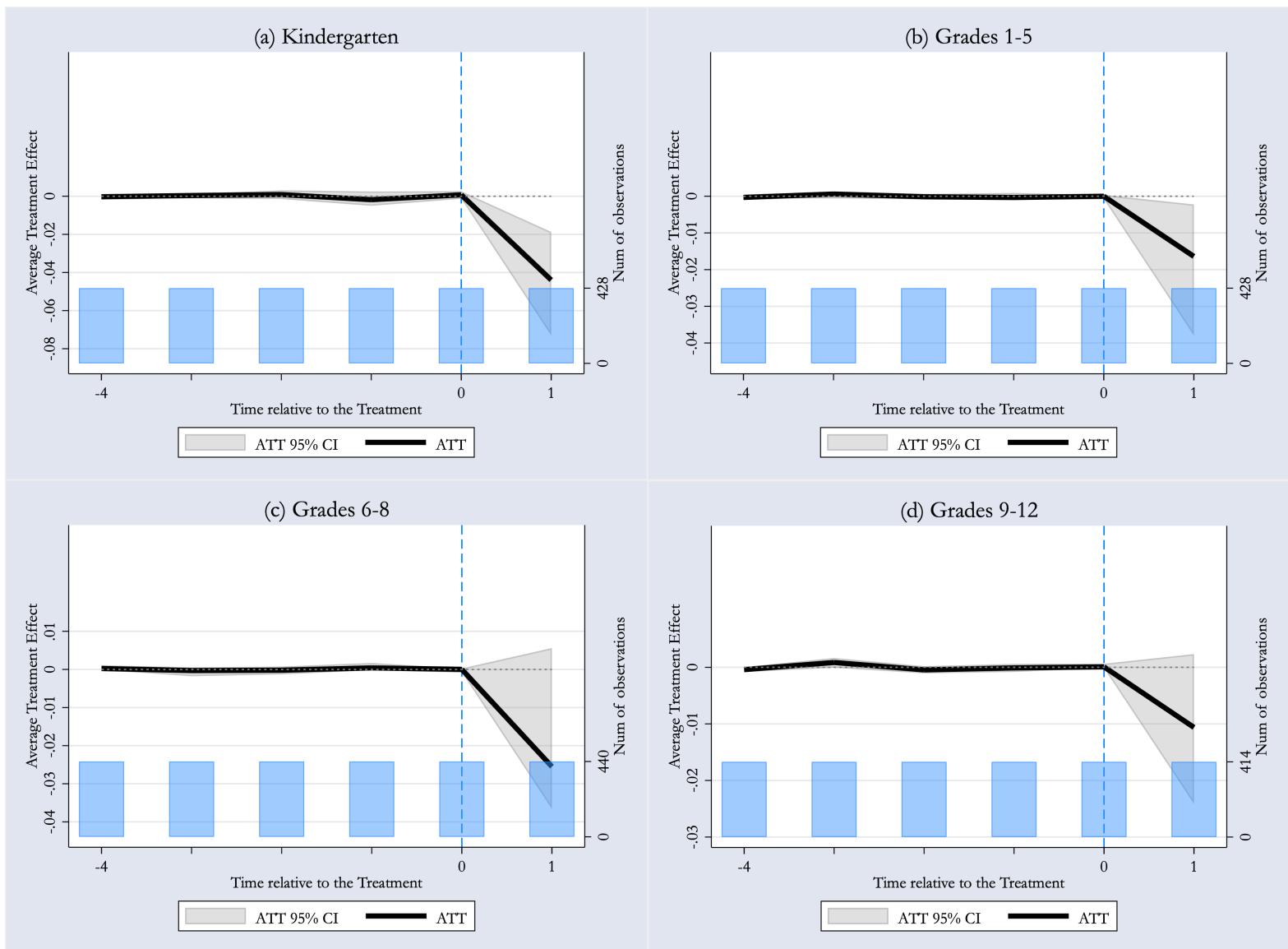


Figure A4 – GSC Event Studies by Grade Levels