In-Person Learning During the Pandemic: Student Take-up, Student Outcomes and Aggregate School Effects of Remote and Hybrid Education

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Abstract

We examine student enrollment responses to district decisions to offer in-person learning during the 2020-21 school year in Connecticut Public Schools, and the impact of in-person learning on attendance and student test scores during this year. Offering a higher share of days in-person was negatively associated with schools that had a large share of high need students. Further, student take-up of in-person options was lower in schools with larger shares of high need students. Using difference-in-differences approaches, we find that in-person opportunities reduce pandemic associated declines in elementary school attendance and in 5^{th}-8^{th} grade test scores with larger protective effects in lower grades, and the treatment on the treated estimates are typically around twice as large as the intent to treat estimates documented in previous studies. Further, we find that the benefits of in-person learning appear to arise at the school, rather than the student, level. Consistent with the negative effects of remote learning arising from an increased burden on teachers, we also find that the benefits of in-person learning are larger among schools that delivered remote days of learning more heavily through hybrid, rather than fully remote education.

Note. I apologize that the Appendix Tables are not yet available. Much of the new analysis on the mechanism behind in person learning effects has been completed very recently and the Tables have not yet been updated. I have included some of the reduced form event study figures at the end of the paper.

Introduction

The 2020-21 school year was a major challenge for states, school districts, teachers and students as many districts started the school year with either hybrid or entirely remote education in response to the COVID-19 pandemic. These states had only a few months to prepare between the end of school in fully remote mode in spring 2020, mostly asynchronous education, and the start of the next school year. These challenges may have resulted in poorer educational outcomes for K-12 students for several reasons. First, schools often serve as a protective factor for students, especially those that provide safe environments with supportive educators (Wang & Fredericks, 2014). Second, the interpersonal nature of in-person learning can foster important social-emotional skills in students that enhance academic learning (Cipriano et al, 2023). Third, with some rare exceptions, in-person instruction has historically been the means through which teachers are trained to teach and students are used to learning in. As a result, remote and hybrid education may have imposed resource and time costs on schools and teachers reducing the availability of resources for broader support of student learning (add cites). Accordingly, a sudden shift to an unfamiliar learning mode that an education system is unprepared to accommodate can diminish the effectiveness of instruction. Qualitative research has documented that teachers found providing hybrid education to be especially challenging during the pandemic (add cites including on teacher burn-out).

Research has documented significant declines in enrollment during the 2020-21 school year, especially in kindergarten and elementary school (Musadiqq et al. 2021; Bassok and Shapiro 2021; Chatterji and Li 2021). Several studies have also documented significant learning losses during the pandemic by analyzing discussion forum posts (Bailey et al. 2021), using parental survey responses on the number of lessons received and hours spent on school work (Major et al. 2021), and examining test scores that were administered before and after school closures in spring 2020 (Engzell et al. 2021). More recently, two studies (Jack et al. 2023; Goldhaber et al. 2022) examined student test scores during the 2020-21 school year and documented worse performance compared to pre-pandemic levels and higher learning loss when students had less in-person instruction. Further, many of the studies above document larger negative effects for disadvantaged student populations, including Jack et al. (in press) who found larger negative effects of standardized test score pass rates for Black students partly because of less access to in-person learning and Goldhaber et al. (2022) who found that for districts that went remote during 2020-21, achievement growth was lower for students from high-poverty schools.

The Current Study. While existing research has examined the availability of different learning modes on student outcomes, no research to date has examined the extent to which students took up in-person learning when it was made available and the effect of this take-up on learning loss. This is an important factor because the variation in take-up may help explain which schools, such as schools with large disadvantaged student populations, experienced the greatest losses. In this study, we use data from the State of Connecticut public school system to examine the relationship between Local Education Agencies' (LEA) decisions to offer in-person learning opportunities and student in-person enrollment, and to examine the impact of in-person enrollment on student attendance and test scores during the 2020-21 school year using a difference-in-differences identification strategy and district in-person offerings as an instrument for in-person enrollment.

Our study makes several unique contributions to the existing literature. First, it is the first to our knowledge to document how students in different schools responded when provided the opportunity for in-person learning during the 2020-21 school year. Second, using school level measures of student take-up as a mediating factor between in-person offering and in-person learning, we document the difference between the variation in student outcomes based on intent-to-treat (ITT) estimates of the effect of in-person learning, as estimated by previous studies (Jack

et al. in press; Goldhaber et al. 2022), and the variation implied by new treatment-on-the-treated (TOT) estimates. Notably, these treatment on the treated estimates capture the combined effect of both direct student exposure to in-person learning and any aggregate effects arising because schools with higher rates of in-person learning had to devote less resources towards remote or hybrid learning. To separate the individual level and aggregate school effects, we use two approaches: 1. We estimate the post/pre-pandemic difference in the conditional correlation between student outcomes and student share of days enrolled in person conditional on separate school fixed effects for prior to and during the pandemic to remove any aggregate school effects,[1] and 2. We estimate the treatment on the treated measure for school level share of days in person conditional on actual student in-person enrollment to absorb individual level effects.[2] Finally, we further distinguish the effects of in-person enrollment between schools that offer remote days primarily through hybrid or through fully remote teaching, to capture the possibility that aggregate effects might be especially large for districts that relied on hybrid teaching. These aggregate effect estimates provide some of the very first evidence on the mechanism behind improved academic performance due to in-person learning during the pandemic. These contributions are possible because we have both student level data on in-person versus remote enrollment and student level outcomes over attendance and test scores, data not available for previous studies of this question.

Results

We find surprisingly low take-up rates when in-person learning is offered, especially among students from schools with a large share of students who are either free and reduced price lunch eligible, English language learners and/or students with disabilities (State of Connecticut describes as high need students). Our estimates suggest that on average students are only enrolled in-person for about 35 percent of school days between October and March and 65 percent of days between April and June, respectively. For districts with an above median share of students who are high needs, the share of days enrolled in-person falls to around 25-27% of days and 53% of days enrolled in-person between October and March and April and June, respectively. Students from schools with a large share of high need students also appear less responsive to increases in the share of days being offered. In schools with below median share of high needs students, a 10 percentage point increase in the share of days offered in-person is associated with over a 7 point increase in share of days enrolled in-person between October and March, while among schools with an above median share of high needs students a 10 point increase is only associated with a 4.5 point increase in share of days enrolled in-person.

We next estimate difference-in-differences models of student attendance and test score performance first following the existing literature and regressing student outcomes on district decisions to provide in-person learning opportunities (in our case share of days offered in-person), but then we also use share of days offered in-person as an instrument to estimate the direct effect of in-person enrollment (as opposed to district policies) on outcomes. Given the low take-up rates, the treatment on the treated estimated effects on attendance and test scores tend to be about twice the size of the intent to treat estimates. For attendance in elementary school, moving from the 10th to the 90th percentile of in-person enrollment at the school level (about a 60

[1] We document that the difference between pandemic and pre-pandemic fixed effect estimates is quite stable as student level controls are added.

[2] See the methods section and the appendix for a more complete discussion of this estimation strategy. As shown in the appendix, the assumption of a linear-in-means model to capture the aggregate effect allows us to identify a lower bound for the aggregate effect estimate, but the model can also be interpreted as simply running a horse race between a student level measure of in-person enrollment and district level offerings.

percentage point increase) implies a reduction in attendance rate declines during the pandemic from 2.0 percentage points to 0.6 points. In terms of test scores, the treatment on the treated estimates imply losses at the 10th percentile falling between 0.21 and 0.34 standard deviations for ELA and 0.38 and 0.56 for Math, and at the 90th percentile losses are substantially smaller at around 0.11 for all grades for ELA and between 0.21 and 0.25 for math.

Because our instrument operates at the district level for each grade, we should interpret these estimates as the combination of the direct effect of an individual student enrolling in-person and any indirect effects arising due to aggregate patterns of enrollment for a given grade and school. We next examine whether these effects are in part explained by aggregate effects at the school level that benefit all students regardless of learning mode. First, we examine within school differences in the relationship between student outcomes and student in person enrollment. Next, we essentially run a horse race between student level in-person enrollment and district by grade offerings of in-person learning. Surprisingly, using both approaches, we find that both the attendance and test score effects of in-person enrollment appear to be driven primarily by school level variation, which would be consistent with the gains arising from a reduction in the burden of remote and hybrid education on staff and schools, rather than a student's specific in-person learning experiences. Finally, given concerns that hybrid education was especially burdensome on teachers, we estimate models separately by districts and grades where remote days were provided primarily as part of hybrid learning, versus primarily through fully remote learning. We find that the gains from in-person learning are substantially larger when remote learning days were primarily offered as part of a hybrid format, again consistent with a conclusion that gains from in-person learning may have been driven by reduced burdens on teachers, schools and districts.

A critical caveat is that the Local Average Treatment Effect of the within school individual effect and aggregate impact after controlling for student level enrollment are both conditional on student enrollment decisions that arise from students not complying with the in-person offerings provided at their school. Therefore, we cannot rule out individual level benefits of in-person learning for students that complied with the school/district offerings. However, the estimates of individual in person learning are stable as controls for student attributes are added inconsistent with large, heterogeneous effects of in person learning. Also, the substantial benefit of in-person enrollment when remote days arise as part of hybrid learning points to a potential role for aggregate school effects in explaining learning losses.

Student Take-up of In-person Learning. We estimate models of a student's days enrolled in-person as a share of total enrollment days for each student on the share of days offered in-person by the grade and the LEA of the school in which they were enrolled in October of 2020. The estimate on share of days offered in-person captures student take-up in response to districts offering more in-person days. Further, we estimate a constant term that captures the share of enrollment days in-person evaluated at the mean share of days offered by subtracting the overall sample mean of 0.65 from the share of days offered in-person. The results are illustrated in Figure 1 and point estimates are presented in Appendix Table A3. The light grey bars show the student mean share of days enrolled in-person for each time period when evaluated at the common mean number of days offered in-person, and the red bars present the share of days enrolled based on number of days being offered at one standard deviation or a 0.21 share above the mean. These bars are presented in sets based on the portion of the school year covered: October-December, January-March, and April-May. For each time period, two pairs of bars are presented with the

pair on the left presenting results for schools that are below the median in terms of share of high need students and the pair on the right presenting results for schools that are above the median.

Our estimates suggest that students in a school and grade that offers the mean share of days in-person are only enrolled in-person for about 44 percent days between October and March and 56 percent of days between April and June. These differences entirely capture the effect of enrollment decisions because we hold the mean share of days offered fixed for all models, and actual differences over time are larger because the number of days offered in-person also increased in the later part of the year. We also see a very large gap between districts based on student composition. For districts with an above median share of high needs students, the share of days enrolled in-person at the sample mean share offered is 33%, 34% and 49% of days between October and December, January and March, and April and June, respectively. While for districts with a below median share of high needs students, the shares enrolled in-person over time are 50%, 47% and 63%. Again, average enrollment differences are larger in practice because high needs districts tended to offer a lower share of days in-person and the figures are based on the population average.

Further, student take-up in response to attending a school that offers more days in-person is relatively low at 65% (or a 6.5 percentage point increase in share of days enrolled in-person when the share of days offered in-person increases by 10 percentage points) between October and March and falls to below 60% between April and June as the share of days offered by most LEA's increases. Students from schools with a large share of high need students also appear less responsive to whether their school/district offered a higher share of days in-person for their grade. The gap between the light grey and the red bars, the effect of a one standard deviation increase in share of days offered, is substantially larger between October and March for students in schools with an above average share of high needs students. In schools with below median of share high needs students, a 10 percentage point increase in the share of days offered in-person is associated with over a 7 point increase in share of days enrolled in-person between October and March, while among schools with an above median share of high needs students a 10 point increase is only associated with a 4.5 point increase. In the later part of the year, between April and June, these across school differences in take-up fall substantially, but that decline is entirely because take-up rates are falling for schools with a below median share of high needs students. So, rather than take-up improving in the spring among schools with disadvantaged populations, take-up was declining in more advantaged schools as students transitioned back to in-person and potentially the remaining remote students were less likely to respond to new in-person offerings.

In-person Learning and Pandemic Learning Losses. Figure 2 presents estimates of attendance and test score effects of in-person learning through the end of the year for attendance and through the end of April for test scores. Panel A presents results for Attendance, while panels B and C present results for \standardized test scores on universally administered Smarter Balanced English Language Arts (ELA) and Mathematics tests. These estimates are based on a difference-in-differences strategy by interacting share of days enrolled in-person with a dummy variable for the 2020-21 school year conditioning on the lagged student outcome, school and year fixed effects plus controls that allow the effects of the pandemic to vary across schools, i.e. an interaction between the pandemic year dummy and both pre-pandemic school share of students with high needs and school mean outcomes (attendance rates for attendance models and average test scores for each test score model) and the interaction of these variables with the type of Local Education Agency.

Each figure presents three estimates for each grade or grade level. The first estimates shown in the grey bars are the intent to treat estimates based on share of days offered in-person at a student's grade level and school and are comparable to estimates by Jack et al. (In Press) and Goldhaber et al. (2022). The second estimates (red bars) present the treatment on the treated estimates based on using share of days offered in-person for each district and grade level to

instrument for school by grade average of students' share of days enrolled in-person,[3] and the third estimates (light brown bars) present aggregate effect estimates that will be discussed later in the section.

Figure 2 Panel A shows the results for attendance rates. We only observe effects of in-person learning on attendance for elementary school grades. The intent to treat estimates of share of days offered in-person are relatively modest at 1.2 percentage points or less than 0.3 points for a one standard deviation change of 0.22 in share of days offered. After accounting for take-up the treatment on the treated, estimates are approximately double the size of the reduced form estimates at 2.3 percentage points. The reduced form model implies for elementary school grades that moving from schools in the 10th percentile LEA with 47% of days offered in-person to the 90th at 100% of days offered reduces the attendance losses from 1.5 percentage points to 0.9 points, while moving from the 10th percentile of school average in-person enrollment of 26% of days to the 90th of 85% implies a reduction in attendance rate losses from 2.0 to 0.6 percentage points. Please note that while estimates are smaller for middle and high school, we cannot rule out effects in either direction because the standard errors are also much larger at those higher grade levels. Parameter estimates are presented in Appendix Tables A4 and A5.

Figure 2 Panels B and C show the results for Smarter Balanced ELA and Math test scores for 5th through 8th grade.[4] For both ELA and Math test scores, reduced form estimates of the effects of in-person learning decline with grade level (grey bars), similar to Jack et al. (2023), with the estimates ranging between 0.11 and 0.22 standard deviations for ELA tests and between 0.14 and 0.30 standard deviations for Math. Again, the treatment on the treated estimates (red bars) are approximately two times larger than the intent to treat with estimates ranging between 0.22 and 0.40 for ELA tests and between 0.27 and 0.54 for Math, see Appendix Tables A6-A9. The reduced form estimates imply pandemic losses at the 10th percentile of share of days offered in-person as of the end of April (prior to the conclusion of standardized testing) ranging between 0.18 and 0.28 of a standard deviation improvement for ELA tests and between 0.33 and 0.46 for Math tests, while at the 90th percentile estimates are between 0.11 and 0.14 for ELA and 0.26 and 0.29 for Math. For the treatment on the treated estimates the 10th percentile losses based on school level enrollment are larger falling between 0.21 and 0.34 for ELA and 0.38 and 0.56 for Math, and at the 90th percentile losses are mostly smaller than Intent to treat at around 0.11 for all grades for ELA and between 0.21 and 0.25 for Math. Further, we no longer find any systematic evidence of larger test score losses at lower grades at the 90th percentile of in-person enrollment.[5]

We also examine whether the effect of in-person learning is heterogeneous between schools based on our measure of student disadvantage. In comparison to earlier studies, we find less evidence of larger positive effects of in-person learning on test scores for students in schools

[3] We instrument for the school average to capture the combined impact of both individual and school level effects. In the appendix, we show that under the linear in means assumption the estimates capture the sum of the individual and school level effects. The estimates are also virtually identical whether one instruments for the individual or school average in person enrollment, and would be identical mathematically except for the inclusion of the control for each student's lagged outcome.

[4] Models are not estimated for 3rd and 4th grade due to the inclusion of a control for student lagged outcomes and the cancellation of standardized testing in the 2019-20 academic year. We move results for the universally administered 11th grade SAT tests to the appendix because we never find significant effects of in-person learning on SAT test scores.

[5] The 10th percentile is around 40% of days offered in-person for 5th through 8th grade and between 16% and 19% for school average in-person enrollment with higher enrollment at lower grade levels, and the 90th percentile was between 93% and 100% for offerings and between 69% and 83% for enrollment again with higher levels of in-person at lower grades. These shares are based offerings and enrollment through the end of April since 2020-21 standardized testing in Connecticut was conducted between late April and mid-May.

with disadvantaged student populations, except for 6th grade test scores where the benefits of in-person learning are heavily concentrated among schools with an above median share of high needs students. The loss of in-person learning may have had substantially worse consequences when students were transitioning between elementary and middle school. Given the lower take-up rates in disadvantaged schools, we could not have detected this effect using our intent to treat estimates, i.e. in person offering as used in earlier studies, see Appendix Tables 10-12.

The validity of the use of district share of days offered in-person as an instrument is evaluated first by conducting event study analyses of the reduced form or intent to treat models, and showing strong evidence of parallel trends in attendance and test scores in the years running up to the pandemic between districts offering low and high shares of days in-person, see Appendix Figures A1-A13. Further, similar to Jack et al. (2023), we examine the stability of our estimates by comparing the estimates in Figure 2 that allow the effects of the pandemic on student outcomes to be heterogeneous to estimates from models that omit the interaction of district type and school attributes with the pandemic indicator. Attendance effect estimates are sensitive to adding controls for share of high needs students eliminating any effects of in-person learning in higher grades and reducing the estimates without such controls for elementary school grades by 65 to 75 percent, but effect estimates do not erode in magnitude when adding additional controls for lagged student attendance rates. Like Jack et al. (2023), our estimated effects for test scores are very stable as these controls are added with only estimates for 5th grade ELA declining by about 10% as controls for school share high needs are added and no declines in estimates when additionally adding controls for pre-pandemic school average test scores. See the Appendix Tables A4-A9.

Aggregate and Individual Effects of In-person Learning. The estimates above should be interpreted as the combined effect of an individual student's in-person enrollment share and any aggregate effects arising based on the level of in-person enrollment at the school because our instrument only varies by grade by district and so cannot disentangle the direct effects of in-person learning on the student and broader effects on an entire cohort of students at a school. The light brown bars in Figure 2 present IV estimates of the effect of school level measures of average share of days enrolled in-person after conditioning on each student's share of days enrolled in-person. While the student level estimates in this new model are biased by the endogeneity of student choices, the student level control captures both the causal effect of a student's in-person learning experiences and any effects of student unobservables that are correlated with this choice, and the remaining correlation between our instrument for school level in-person enrollment can be attributed to aggregate effects across all students regardless of their individual learning mode. As discussed below, conditional on the assumption of linear-in-means aggregate effects, the IV estimates in the light brown bars represent a lower bound on the aggregate effects. As an alternative interpretation, these results can be viewed as a horse race between student level in-person enrollment and the in-person opportunities offered at the district by grade level where IV estimation assures that estimates are comparable between red and light brown bars.

In panel A for attendance, the aggregate estimates (light brown bar) in elementary school are somewhat larger in magnitude at 3.8 percentage points than the overall effect of 2.3 points (red bar) consistent with all attendance gains arising at the school by grade level, rather than due to individual in-person enrollment. In fact, given our interpretation of the aggregate estimates as a lower bound, they imply that the direct effect of student in-person enrollment is negative, consistent with an increase in attendance from remote learning possibly in part because attending is relatively easy when it simply requires logging in during school hours and does not necessarily require student effort or engagement, as compared to physically attending school. As above, attendance estimates for middle school and high school are very noisy. In panels B and C for test scores, the aggregate effect estimates can explain a majority or even all of the overall treatment on the treated estimates.

For 5th grade, the aggregate estimate is 60 to 70% of the overall estimate, and or higher grade levels the aggregate is 80 to 100% of the overall.

Table 1 presents the point estimates from first the overall estimates presented by the red bars in Figure 2, and then the point estimates of the lower bound of the school level effects from the light brown bars. Row 3 then presents estimates associated individual student in person enrollment based on within school comparisons by allowing for separate fixed effects by school year, and the fourth row adds the estimates from rows 2 and 3 to capture an estimate of the upper bound on the total effect for comparison to the total effect estimates in row 1. For attendance, the individual level effect estimate is a negative estimate of 2.6 percentage points, consistent with the light brown bar in Figure 2 Panel A being taller than the red bar. Adding this negative effect brings the total estimate to 1.2 percentage points, well below the total estimate of 2.3 points shown by the red bars. Similarly, the individual level estimates for both ELA and Math are modest in size and comparable in magnitude to the difference between the overall estimate in row 2 and the aggregate effect lower bound in row 3 with the largest estimates for 5th grade and estimates near zero for 7th and 8th grades. Admittedly, the upper bounds in row 4 are a bit higher (1/20th of a standard deviation) than the estimates in row 1 for 5th and 6th grade ELA test scores, but this might arise from estimation error given the alternative sources of variation used for identifying the estimates.

These findings point to a new possible mechanism behind improved student outcomes during the pandemic where the informal and formal resource requirements of remote and hybrid learning possibly put a strain on the educational system, both administratively and on teachers and staff, reducing academic performance across the entire student population, both those learning in-person and those learning remotely. While we cannot rule a mechanism arising from students who are not harmed by remote learning selecting into remaining remote, the school by year fixed effect estimates are stable in response to the inclusion of student level controls for free lunch, English language learner and special education status with estimates never varying by more that two and one half percent of a standard deviation and usually by one percent or less upon adding these controls. See Appendix Tables A13-A15 for parameter estimates.

Hybrid versus Fully Remote Learning. As discussed above, hybrid learning may impose additional burdens on teachers due to the need to simultaneously manage two modes of instruction over and above the direct cost of remote teaching approximately half of the time or days in a hybrid week. To further investigate the aggregate effects presented above, we split the sample between districts and grades where the number of days of remote learning is heavily associated with fully remote education and districts and grades where hybrid learning plays a larger role in the number of days remote. We then estimate separate models for each subsample for the outcomes where in-person learning was shown to influence educational outcomes: elementary school attendance and both ELA and Math test scores. Specifically, Figure 3 Panels A and B present treatment on the treated estimates of the effect of in-person learning on ELA and Math test scores separately for grades and districts that had a below or above median share of remote days arising from weeks when districts reported hybrid, rather than fully remote, learning. The parameter estimates are presented in Appendix Tables A16-17. For attendance for elementary grade levels, the pattern was consistent with the test score models, but the estimates were relatively noisy.[6]

Using a one-sided hypothesis test based on our expectation that hybrid education may impose a larger burden on teachers, we find significantly larger effects of in-person learning for both ELA and Math test scores for the subsample of districts and grades where a larger share of

[6] We present the simple treatment on the treated IV estimates for school level in-person enrollment, rather than IV models that also condition on the students' actual in-person enrollment, to focus on the total effect of in-person enrollment.

days remote occurred via hybrid learning for 7th and 8th grade in ELA and 5th and 8th grade in Math. These estimated differences range between 15 and 30 percent of a standard deviation larger effect size when hybrid education played a larger role in remote learning. The ELA differences are significant at the 0.025 level for 7th grade and 0.005 level of 8th grade, while the Math differences are significant at the 0.08 and 0.06 level for 5th and 8th grade.[7] For nine outcomes (four test scores for ELA and Math each and attendance for elementary grades), we observe four rejections at the 10 percent level or better while type 1 error would imply around 1 rejection, and the ELA 8th grade differences are still significant at the 5 percent level using the conservative Bonferroni correction of multiplying the p-value by 9. Of the 9 differences, all but one of the point estimates, 6th grade ELA, are consistent with our hypothesized sign, and the 6th grade ELA difference is less than 10 percent of a standard deviation and the z-score is below one. These findings suggest that the positive effects of in-person learning are larger for districts where remote learning days arose from hybrid education, consistent with hybrid learning imposing a substantial time and resource cost on teachers.

Discussion

This study provides the first analysis of in-person enrollment, as opposed to opportunities, on educational outcomes using detailed longitudinal student data including student data on enrollment in-person or on-line and weekly administrative reports that identify the learning mode used by each district or Local Education Agency. The student level administrative data allows us to examine how responsive students are to the provision of in-person learning opportunities and more accurately assess the magnitude of the effects of in-person versus remote learning. We document relatively low take-up of in-person opportunities with students enrolled in-person for only about 35% of days between October and March and about 65% of days between April and June, while districts on average in Connecticut offered students the opportunity to attend in-person for around 60% of days between October and March and 87% of days between April and June. We also document stark differences between districts with schools that have an above median share of high needs students offering less days in-person, about 50% until April when the average rises to 80%, and experiencing lower student take-up of in-person opportunities with students in these high needs schools responding to increased in-person opportunities by increasing enrollment by only 50% of in-person days offered between October and March and only 67% of days offered between April and June..

These low take-up rates imply that the reduced form or intent to treat effects of in-person learning in papers like Halloran et al. (2021) and Goldhaber et al. (2022) are smaller than the actual effect of in-person learning on outcomes. Specifically, our treatment on the treated estimates tend to be about twice the size of our intent to treat estimates, and the range of outcomes between schools with low and high levels of in-person learning is substantially wider when learning is measured as enrollment rather that in-person offerings. For attendance, reduced form estimates imply that schools at the 10th and 90th percentiles of in-person offering had losses of 1.5 and 0.9 percentage points, respectively, while moving from the 10th to 90th percentile of school average in-person enrollment and using treatment on the treated estimates implies attendance rate losses changing from 2.0 to 0.6 percentage points. Across 5th-8th grade test scores, reduced form losses at the 10th and 90th percentiles averaged 0.22 and 0.13 standard deviations for ELA and 0.40 and 0.27 for Math, respectively, while treatment on the treated estimates implied

[7] For elementary school attendance, the difference in the estimates is 1.8 percentage points of attendance rates with larger effects of in-person learning for districts where remote days were more associated with hybrid education, which is sizable and in the hypothesized direction, but the z-score for this estimated difference only 1.26, which just misses significance at the 10 percent level even for a one-sided test.

losses at the 10th and 90th of 0.27 and 0.12 for ELA and 0.48 and 0.23 for Math. Unlike earlier studies, we find less evidence of heterogeneity in effects based on student disadvantage as measured by the share of high needs students. However, we do identify much larger effects on 6th grade test scores in more disadvantaged districts suggesting that in-person learning in disadvantaged districts might be especially important when students were transitioning between schools.

Finally, we also provide new evidence on the mechanism behind the improved student outcomes at higher rates of in-person learning. Reduced pandemic attendance and test score losses from increased in-person enrollment appear primarily attributable to broad performance improvements regardless of the individual student's own enrollment choices, especially for attendance and middle school test scores. Further, we find that the effects are largest in districts and grades where remote days were offered primarily via hybrid education, consistent with qualitative evidence that hybrid education was especially burdensone on teachers. Remote and especially hybrid learning may impose resource costs of districts and time and emotional burden on teachers. A higher share of in-person enrollment may free up resources overall contributing to broad improvements in performance in a school and/or grade and school, or simply make it more feasible for teachers and staff to invest additional resources in students who are remote or hybrid, minimizing any losses resulting from remote learning during the pandemic. As noted above, the aggregate effects are estimated conditional on individual student take-up decisions of in-person offerings. If students who gain the least from in-person offerings systematically remain remote, then our aggregate estimates would include the larger individual benefits that accrued to compliers who benefit the most from in-person. However, we find minimal evidence of selection into compliance on observables when estimating the conditional correlation between outcomes and student take-up of in-person learning.[8]

Materials and Methods

Data and Samples. The Connecticut State Department of Education provided administrative data covering the academic years from the 2014-15 to the 2020-21 academic years. These include 1. Fall enrollment files identifying each student's school and district as of October 1st along with their status on key indicators of whether the student qualifies for free or reduced price lunch (FRPL), whether the student is classified as disabled (SWD), i.e. has an individualized education plan, and whether the student is classified as an English Language Learner (ELL) defined as of December of that year; 2. End of year enrollment file identifying end of year school and district, as well as the total number of days enrolled and attended at this final school, but only through March in 2019-20; 3. Scaled test scores on spring Smarter Balanced English Language Arts (ELA) and Mathematics (MATH) tests for 3rd through 8th grade students and universally administered SAT tests in 11th grade, except in the 2019-20 school year when no tests were administered, and 4. Monthly data starting in October 2020 by student for number of days enrolled in-person and remote and number of days attended in-person and remote for the last school in which the student was observed that month. We define two variables for in-person enrollment at the student level calculating the total number of days enrolled in-person between October and June divided by total enrollment days during the same period and the same variable for the period between October and April for use with the test score analyses. We also create two

[8] For each outcome, we estimate the difference between the coefficient on student take-up between the pandemic year and our earlier control years conditional on school by year FE's to condition out aggregate effects, and the inclusion of student attributes has minimal impact on these estimates.

variables at the school by grade level measuring the mean share of days enrolled in-person for each of these periods, as well.

The state collected weekly data from Districts and other Local Education Agencies (LEA's) on the learning mode separately by grade for each week during the 2020-21 school year. Each LEA reports whether the learning mode is remote, hybrid, or in-person where LEA's were given guidance that hybrid should be considered to be between 25 and 75% of time in-person. LEA's were obligated to establish a weekly learning mode that students were entitled to utilized, and also provide fully remote learning to any student that requested remote learning. Except for a small number of heavily disabled students, students did not have access to fully in person learning unless that was the mode being provided by the LEA. LEA's included town based public school districts, regional school districts that combine multiple small towns in order to deliver education at higher grades either providing high school or both middle and high school education, Regional Education Service Centers (RESC's) that primarily act as the district administrator for public magnet schools that operate separately from city and town based school districts, individual charter schools or small systems of charter schools, individual endowed schools that historically operated within a single district providing an alternative option in middle or high school, the Connecticut Technical Education and Career System (CTECS) that manages stand-alone technical education high schools throughout the state, and finally Approved Private Special Education Schools (APSES) that are primarily very small schools, servicing students with specific and often severe disabilities. All samples described below exclude the approved special education schools, but estimates including these students are very similar to the results presented below.[9]

Given that all students had the right to select all remote learning during the 2020-21 school year, we characterize LEA's based on whether they provided students with a hybrid defined as 25-75% in-person or a predominantly in-person option 75%, although in many cases hybrid was 2-3 days a week and in-person was 5 days a week. We then define two variables for in-person offering by LEA by grade: the approximate share of days offered in-person through April for examining test score outcomes that occur between mid-April and mid-May and through the end of the school year for examining attendance outcomes. For this variable, we count all days in a week as in-person if the LEA learning mode for the week is classified as in-person, half of the days in a week as in-person if the week is classified as hybrid, and zero days for remote, summing over all weeks for each LEA. This state collected data is the same data used by Jacks et al. (In Press) in their study of aggregate district data over multiple states including Connecticut.

We construct repeated cross-sections of students by grades and schools for the school years of 2016-17 through 2020-21 pooling the cross-sections of students across these years and omitting the 2019-20 school year where standardized tests did not occur and attendance was only recorded through March. We restrict our cross-sectional samples to students who were observed in the state public school system in both the given year and two years earlier to allow for the inclusion of two-year student lagged outcomes. The lagged controls are included to minimize concerns of bias from selective attrition from the sample during the pandemic year. For the 11th grade test, we must use a three-year lag to control for 8th grade test scores, and those models can only include 2017-18 and 2018-19 as pre-pandemic control years.

[9] The State of Connecticut also has non-approved special education schools. These schools are even smaller than the approved special education schools and students in those schools are recorded as being part of their traditional public school district, as opposed to a separate LEA. As a result, we do not observe the actual in-person learning opportunities of these students and they are filtered out of the sample by eliminating schools with 10 or less students. Also, sometimes students appear as enrolled in a grade that is not covered by that school and we eliminate non-standard grades by dropping school by grade cells with 5 or fewer students.

In terms of distinguishing between schools with advantaged and disadvantaged student populations, we focus on a single measure recommended by the State of Connecticut to mitigate concerns about type 1 error. We define a measure of share of high need students as the fraction of students in a school that are classified as either Free and Reduced Price Lunch (FRPL) eligible students, Students with Disabilities (SWD) and/or English Language Learners (ELL). Individual measures of share FRPL, ELL, Hispanic students, and African-American students are all very highly correlated with share high needs. We base the school share of high needs students on the three pre-pandemic cohorts from the fall enrollment data in 2017-18 to 2019-20 to average out year to year variation in composition.

Both in-person offerings and student in-person enrollment exhibit substantial heterogeneity across the state. The share of days offered in-person over the 2020-21 school year is substantially at lower at 58% of days in schools that have an above median share of high needs students, as defined as free or reduced price lunch eligible, English language learner or student with a disability, as compared to 72% of days in the below median subsample. Further, student take-up of those days is lower with student days enrolled in-person in above median schools being only 60% of the days, compared to 78% for students below median schools. We also see substantial heterogeneity across grade levels and across Local Education Agencies with regional school districts having the highest share of days offered in-person at 87% in elementary school grades and 64% in high school. Town districts offered somewhat lower share of days at 72% of days in person in elementary grades and 57% of days in high school. Together, the regional education service centers that administer non-town magnet schools and charter schools offered a substantially lower share of days in-person in elementary school at 65%, but a higher share in high school at 69%. Finally, the differences in in-person offerings between high needs and lower needs districts was largest for the RESC's and charters, e.g. in elementary school 83% of days on average were offered in-person for below median districts on share of high needs students, while in above median LEA's the mean share of days in-person was only 46%. Appendix Table 1 shows descriptive statistics overall and by above and below median share high needs, and Appendix Table A2 shows descriptive statistics by grade level.

Methods. We begin by estimating cross-sectional models of student i enrollment by month in grade g, school s and district or LEA d using share of days enrolled in-person (E_{igsd}), during the pandemic year of 2020-21 as a function of the share of days offered in-person by district in the student's grade (S_{gd}). We subtract the sample mean from S_{gd} so that the constant term captures the share of days enrolled in-person at the mean share of days offered in-person.

$$E_{igsd} = \beta_0 + \beta_1 (S_{gd} - \bar{S}) + \mu_{igsd} \quad (1)$$

We estimate models separately by subsamples based on three subsamples of months, October-December, January-March and April-June and separately for subsamples of schools above and below the median share of high need students. However, $\bar{S}$ is based on the entire sample and held constant across subsamples so that the constant estimates in the take-up model are based on the same share of days offered and so are comparable across the subsamples.

We then develop difference-in-differences models of attendance and test scores (Y) that include controls for lagged student outcomes, a dummy variable for the pandemic, school fixed effects and the interaction of the pandemic dummy with the in-person offerings provided by each LEA in each grade (Z_{sd}). This model represents a reduced form or intent to treat model where outcomes depend upon a measure of in-person opportunities provided by the LEA to which the students' school belongs, and is comparable to the existing literature.

$$Y_{isdt} = \alpha_{sd} + \gamma_0 Y_{idt-s} + \gamma_1 P_t + \gamma_2 P_t \, Z_{sd} + \varepsilon_{isdt} \quad (2)$$

where *s* represents the lag applied to the previous outcome control, which is two years for attendance and Smarter Balanced test scores and three years for 11th grade SAT test scores. Given the inclusion of school fixed effects, all difference-in-differences models are clustered at the school level. All test score models are estimated separately by grade, while attendance models pool grades into elementary (2nd-5th), middle (6th-8th) and high school (9th-12th) levels.

To estimate the effect of in-person learning during the pandemic or the treatment on the treated effect, we estimate an Instrumental Variable model for

$$Y_{isdt} = \alpha_{sd} + \gamma_0 Y_{idt-s} + \gamma_1 P_t + \gamma_2 P_t \bar{X}_{sd} + \varepsilon_{isdt} \quad (3)$$

where $\bar{X}_{sd}$ is the share of school days that a student was actually enrolled in-person, and the first stage is modelled as

$$P_t \, \bar{X}_{sd} = \delta_{sd} + \rho_0 Y_{idt-s} + \rho_1 P_t + \rho_2 P_t \, Z_{isd} + \pi_{isdt} \quad (4)$$

where in the pandemic year district by grade offering of in-person learning opportunities is used as an instrument for student in-person enrollment.

Because the instrument varies at the school by grade level, the estimate of γ_2 in equation (3) in principle could capture both the direct effect of in-person enrollment on a student's own outcomes and any aggregate effects on all students arising from the overall extent of in-person versus remote learning for a school and grade. Under the assumption that aggregate effects follow a linear in means process,[10] we use a parsimonious model to demonstrate that γ_2 in equation (3) captures the sum of the coefficients on student in-person learning and on school by grade mean in-person learning across all students, see the mathematical appendix. We also show that the estimate of γ_2 is the same whether we instrument for $P_t \, \bar{X}_{sd}$ or the pandemic interaction with the student level in person enrollment variable (X_{isd}).[11]

To gain some insight into the importance of this aggregate effect, we include the student level share of days enrolled in-person as a control for the direct effect and instrument for the school by grade average of in-person days enrolled using share of days offered in-person yielding

$$Y_{isdt} = \alpha_{sd} + \gamma_0 Y_{idt-s} + \gamma_1 P_t + \gamma_2 P_t \, X_{isd} + \gamma_3 P_t \, \bar{X}_{sd} + \varepsilon_{isdt} \quad (5)$$

but the first stage in equation (4) is unchanged. In the mathematical appendix, we show that the IV estimate of γ_3 from equation (5) provides a lower bound estimate of the aggregate effect. A second more intuitive interpretation of equations (5) and (6) is that γ_3 represents the result of a horse race between the student's own share of days enrolled in-person and the share of days that the student's district offered in-person where IV estimation is used for $\bar{X}_{sd}$ so that the magnitude of γ_3 is comparable to the estimated combined effect of student in-person enrollment and school overall in-person enrollment from γ_2 in equation (3).

Alternatively, we can utilize a fixed effect approach to estimate the individual level impact of X_{isd} based on within school variation

$$Y_{isdpt} = \alpha_{sdp} + \gamma_{0p} Y_{idt-s} + \gamma_{tp} \, X_{isd} + \varepsilon_{isdpt} \quad (6)$$

[10] Plus a more technical assumption of linear conditional expectations.

[11] This result is not robust to conditioning on individual level variables, such as student lagged test scores, but in practice estimates are very similar.

where the *p* index on the parameters allow for separate estimation of models for the pre-pandemic and the pandemic period, and the difference between the estimates on γ_{tp} during and before the pandemic provide a difference-in-differences estimate of the effect of a student's in person enrollment share.

Finally, to demonstrate the validity of the instrument, we estimate event studies of the following form

$$Y_{isdt} = \alpha_{sd} + \gamma_0 Y_{idt-s} + \sum_{s=2016}^{2020} (\gamma_{1t} D_s + \gamma_{2t} Z_{sd} D_s) + \varepsilon_{isdt} \quad (7)$$

where δ_s is a binary variable that is one for year *s* for the fall of the school year. However, while the event study allows us to examine whether schools exposed to small and large shares of days offered in-person have parallel trends relative to the pandemic, this approach does nothing to address concerns that the effects of the pandemic could be heterogeneous across schools based on both the level of student disadvantage and the type of school/type of LEA to which the school belongs. Therefore, we interact a vector of controls based on the type of LEA and the school composition W_{sd} with the pandemic dummy and examine whether these controls erode the magnitude of our estimates.

$$Y_{isdt} = \alpha_{sd} + \delta Y_{idt-s} + \gamma_1 P_t + \gamma_2 P_t Z_{sd} + \gamma_3 P_t W_{sd} + \varepsilon_{isdt} \quad (8)$$

This strategy is similar to models estimated in Jack et al. (2023) that interact the pandemic indicator with district composition over student race, ethnicity and free and reduced price lunch status.

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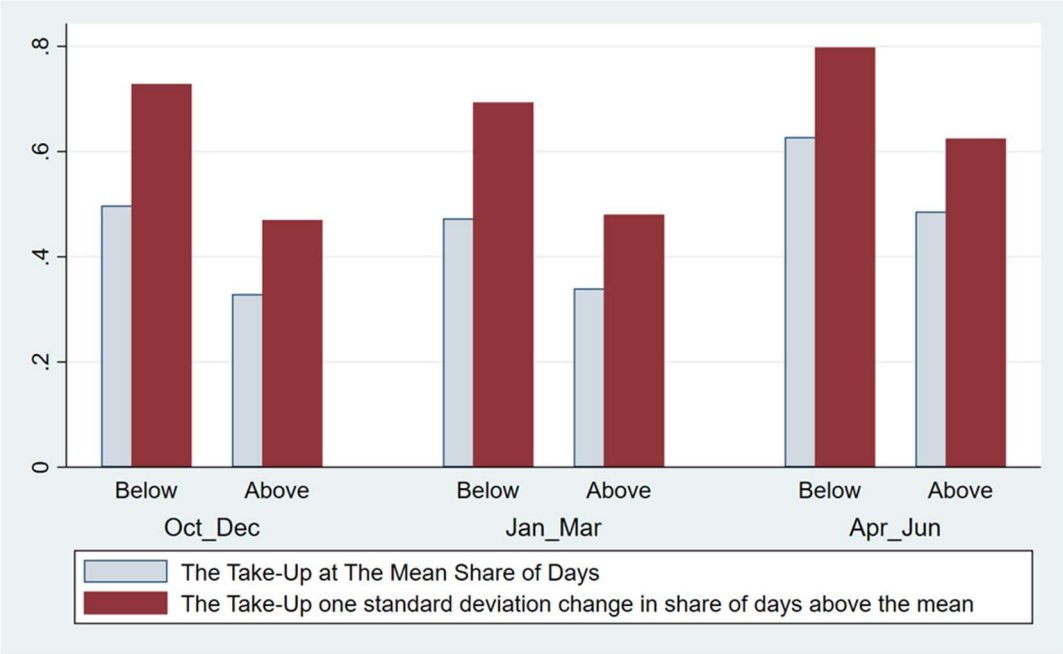
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Figures and Tables

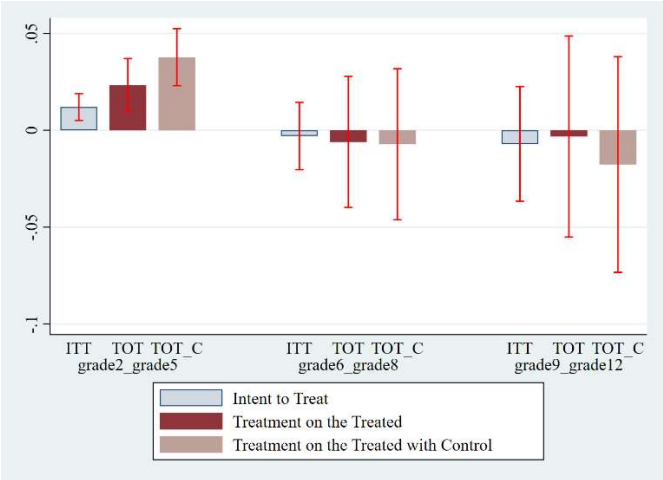
Figure 1. Student Take-up of In-Person Learning by Share of High Need Students



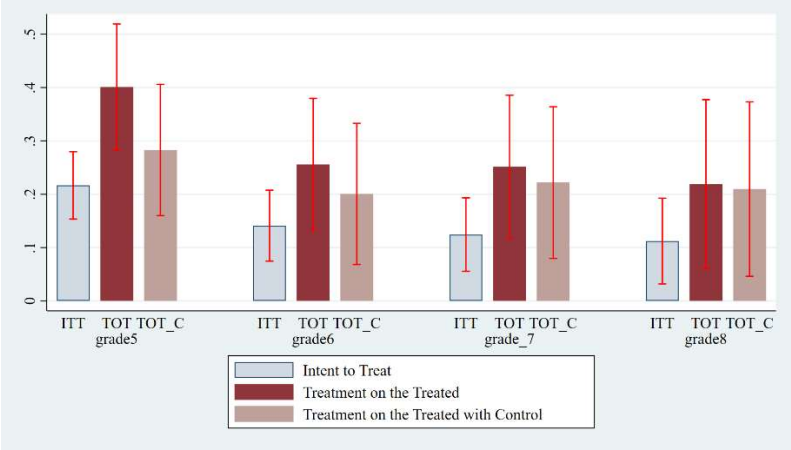
Notes. The grey bar presents the intercept in a regression of student share of days enrolled in person on district by grade share of days offered in person where days offered is mean differenced so the intercept captures in person enrollment at the sample mean. The red bar shows the predicted share of days enrolled in person at one standard deviation above the mean on share of days offered in person. From left to right, the results are presented separately for October to December, January to March and April to May. For each period, separate results are presented for schools that have either a below (on the left) or above (on the right) the median share of students defined as either free and reduced priced lunch, English language learner or special education.

Figure 2. Declines in Pandemic Effects from In-person Learning from 10th to 90th Percentile

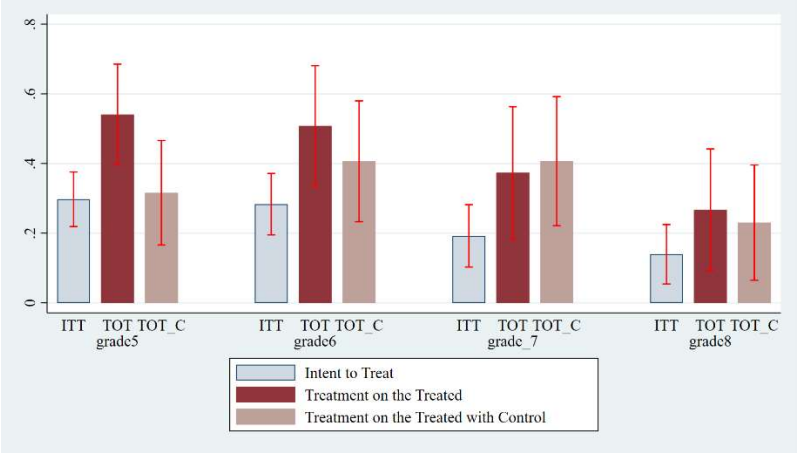
Panel A. Attendance Rates



Panel B. ELA Smarter Balanced Test Scores



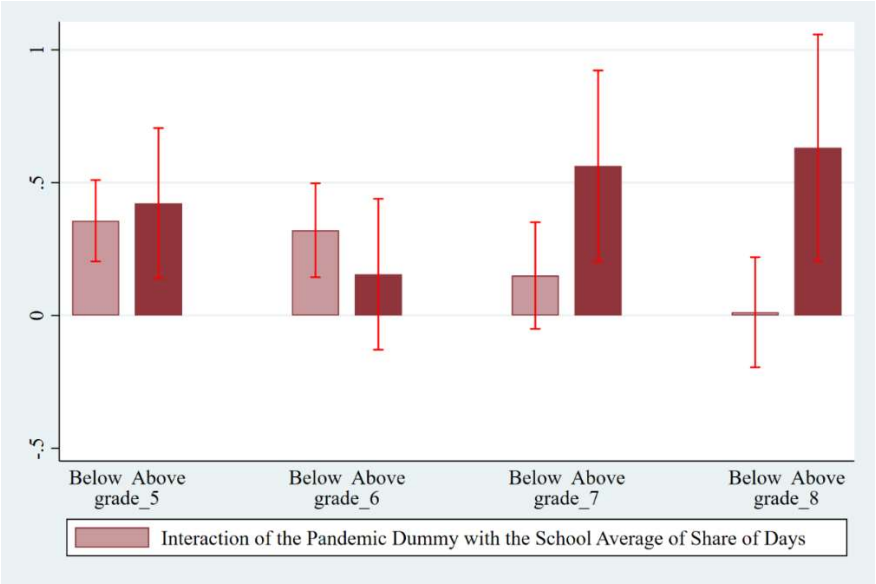
Panel C. Math Smarter Balanced Test Scores



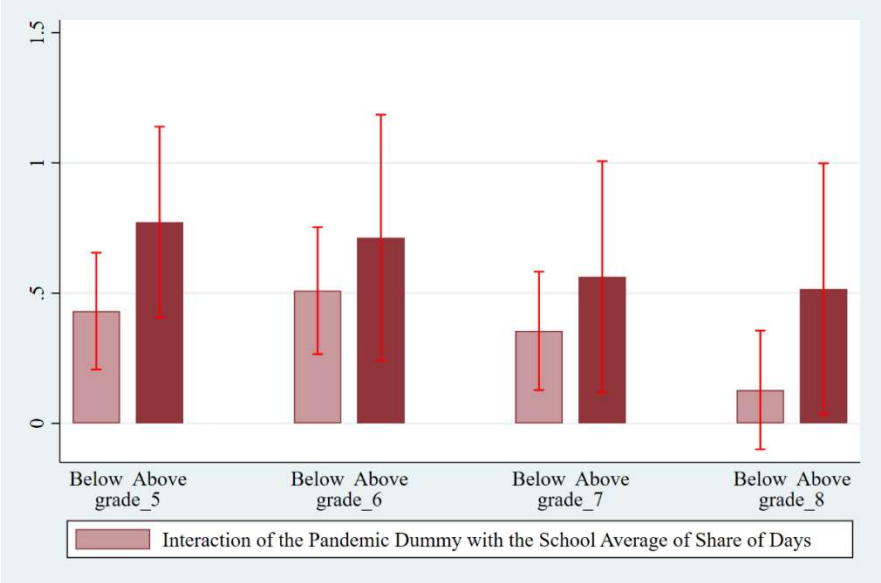
Notes. Panel A presents difference-in-differences effect estimates for Attendance separately for 2^{nd}-5^{th} grade, 6^{th}-8^{th} grade and 9^{th}-12^{th} grade. Panels B and C present ELA and Math Test score estimates separately for 5^{th}, 6^{th}, 7^{th} and 8^{th} grade. The grey bar shows the estimates by regressing outcomes on share of days offered in person at the district by grade level, the red bar shows the IV estimates for school by grade share of days enrolled in person instrumenting with the share of days offered, and the light brown bars show the same estimates after including the individual student's actual share of days enrolled in person to the second stage as an additional control.

Figure 3. In-person Learning Effects by Above or Below Median use of Hybrid Learning

Panel A. ELA Smarter Balanced Test Scores



Panel B. Math Smarter Balanced Test Scores



Notes. Panels A and B present ELA and Math Test score difference-in-differences estimates separately for 5th, 6th, 7th and 8th grade and by whether the district and grade was below (light brown bar) or above (red bar) the median share of remote days that arose from the district providing hybrid education.

Table 1. Seperating Individual Student from School Level Effects of In Person Enrollment									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Attendance	ELA Test Scores				Math Test Scores			
Pandemic Effects	2nd-5th Grade	5th Grade	6th Grade	7th Grade	8th Grade	5th Grade	6th Grade	7th Grade	8th Grade
Simple IV Estimate of Total	0.0234***	0.404***	0.259***	0.255***	0.223***	0.547***	0.516***	0.382***	0.272***
Effect	-0.007	(0.0605)	(0.0643)	(0.0699)	(0.0825)	(0.0747)	(0.0895)	(0.0989)	(0.0909)
Aggregate Effect Conditional	0.0377***	0.283***	0.201***	0.222***	0.210**	0.317***	0.403***	0.402***	0.230***
on Individual Enrollment	(0.0075)	(0.0628)	(0.0676)	(0.0725)	(0.0835)	(0.0764)	(0.0887)	(0.0939)	(0.0839)
Individual Enrollment using	-0.0257***	0.1579***	0.0941***	0.0133	-0.0211	0.2125***	0.0940***	-0.04692	-0.0111
School by Year FE	(0.0017)	(0.0206)	(0.0246)	(0.0249)	(0.0254)	(0.0228)	(0.0263)	(0.0241)	(0.0231)
Sum of Aggregate and	0.0120	0.441	0.295	0.235	0.189	0.530	0.497	0.355	0.219
Individual Effects									

Notes. The first row presents the IV estimates of the effect of in person enrollment (red bars in Figure 2), the secnd row presents the lower bound on the aggregate effect of in-person enrollment (light brown bars in Figure 2), and the third row presents the fixed effect estimates of the effect of student level in person enrollment for students who did not comply with school/district in person offerings. The final row sums the estimates in the second and third rows to capture a lower bound on the total effect for comparison to row one above.

Mathematical Appendix

The purpose of this appendix is to examine a model where outcomes depend upon both an individual level endogenous variable, in our case share of days enrolled in person by a student, and an aggregate affect where outcomes also depend upon the linear mean of this individual level variable, in our case the school by grade average of student share of days enrolled in person.

Throughout this exercise, we impose the assumption of linearity of conditional expectations or equivalently

$$E[A|B,C] = \upsilon B + \xi C$$

This assumption allows us to examine the effect of this underidentified model where only one instrument is available, share of days offered in person, but outcomes depend upon two endogenous variables. Specifically, after specifying estimating equations, we can calculate the conditional expectation on both sides of the equation allowing us to capture the effect of the unobservable on estimates.

Consider a model where an outcome y depends upon an attribute X of each observation i and also on the mean of that attribute over all observations in a cluster s. In our application, i is student and s is grade by school.

$$y_{is} = \beta X_{is} + \delta \bar{X}_s + \varepsilon_{is} \quad (1)$$

Note that all derivations below hold if all variables in equation (1) have either been mean differenced to remove a constant term or been conditioned on observables at the cluster level s. However, conditioning out variables that vary at the individual level would violate the linear in means assumption in the conditional model.

The two regressors are endogenous because student choices correlate with their unobservables and the school unobservables influence student selection into a school. As a result, the conditional expectation of ε_{is} is not zero, and we assume that the conditional expectation is separable so that

$$E[\varepsilon_{is}|X_{is}, \bar{X}_s\ ] = \varphi X_{is} + \omega \bar{X}_s \quad (2)$$

And estimates of equation (1) will be biased as

$$E[y_{is}|X_{is}, \bar{X}_s] = \beta X_{is} + \delta \bar{X}_s + E[\varepsilon_{is}|X_{is}, \bar{X}_s] = (\beta + \varphi) X_{is} + (\delta + \omega) \bar{X}_s \quad (3)$$

Because we condition on X_{is} and $\bar{X}_s$ the expectation of the first two terms is just their value, and the expectation of the error terms is defined in equation (2).

Assume that a single instrument Z_s is available where $E[\varepsilon_{is}|Z_s\ ] = 0$ and this instrument is used to estimate the following model

$$y_{is} = \beta X_{is} + \tilde{\varepsilon}_{is} \quad (4)$$

The resulting two stage estimates would be

$$E\left[y_{is}\middle|E[X_{is}|Z_s]\right] = \beta E\left[X_{is}\middle|E[X_{is}|Z_s]\right] + \delta E\left[\bar{X}_s\middle|E[X_{is}|Z_s]\right] + E\left[\varepsilon_{is}\middle|E[X_{is}|Z_s]\right] \quad (5)$$

Since the expectation of ε_{is} conditional on Z_s is zero, then the expectation of ε_{is} conditional on any linear function of Z_s is zero as well. The expectation of X_{is} on its own conditional expectation is just the conditional expectation, and the expectation of the mean of X_{is} within s conditional on a variable that does not vary within s will again just be the conditional expectation within each cluster.

Therefore,

$$E\left[y_{is}\middle|E[X_{is}|Z_s]\right] = (\beta + \delta)E\left[X_{is}\middle|E[X_{is}|Z_s]\right] = (\beta + \delta)E[X_{is}|Z_s] \quad (6)$$

which confirms the intuition that the combined effect of both individual student in-person enrollment and grade by school average in-person enrollment is captured by instrumenting for the individual level variable alone. If the linear in means assumption is violated, equation (6) still contains $\beta E[X_{is}|Z_s]$, but the second term $f(\delta)E[X_{is}|Z_s]$ would involve an unknown monotonically increasing function δ.

Please note that equations (4) through (6) would operate equivalently if equation (4) specified y_{is} as a function of $\bar{X}_s$ because naturally $E[X_{is}|Z_s]$ equals $E[\bar{X}_s|Z_s]$ so a single IV model using either X_{is} or $\bar{X}_s$ and an instrument at the cluster level will capture the combined effect of both the individual level variable and the cluster mean. Specifically, the estimate of the effect of in-person enrollment should be the same whether we are instrumenting for student level in-person enrollment or the school average, except for the inclusion of the individual control for lagged test scores, which in practice has minimal affect on two IV estimates.

Next, we wish to see if we can learn anything about the separate effects β and δ, e.g. in our case the effects of in-person enrollment on individual students and the effects of school by grade average levels of in-person enrollment . First, we decompose the unobservable into two components $\varepsilon_{is} = \pi_s + \mu_{is}$ where π_s is defined as $E[\varepsilon_{is}|s]$ and μ_{is} is a student level error that is orthogonal to school level observables and unobservables. Then, consider a model where we condition on the individual level variable X_{is} as a control function and use Z_s to instrument for the endogenous cluster, e.g. condition on student's remote enrollment and instrument for school by grade average remote enrollment by remote offerings district by grade.

$$E\left[y_{is}\middle|X_{is}, E[\bar{X}_s|Z_s]\right] = \beta X_{is} + \delta E\left[\bar{X}_s\middle|X_{is}, E[\bar{X}_s|Z_s]\right] + E\left[\pi_s + \mu_{is}\middle|X_{is}, E[\bar{X}_s|Z_s]\right] \quad (7)$$

The definition of π_s implies the conditional independence of μ_{is} from variation across clusters s. Therefore, the expectation of the error terms are separable and the conditional expectation of μ_{is} is not influenced by conditioning on $E[\bar{X}_s|Z_s]$. Therefore,

$$E\left[\pi_s + \mu_{is}\middle|X_{is}, E[\bar{X}_s|Z_s]\right] = E\left[\pi_s\middle|X_{is}, E[\bar{X}_s|Z_s]\right] + E[\mu_{is}|X_{is}] \quad (8)$$

The second term is defined as φX_{is} based on the linear conditional expectations assumption. As a result,

$$E\left[y_{is}\middle|X_{is}, E[\bar{X}_s|Z_s]\right] = (\beta + \varphi)X_{is} + \delta E\left[\bar{X}_s\middle|X_{is}, E[\bar{X}_s|Z_s]\right] + E\left[\pi_s\middle|X_{is}, E[\bar{X}_s|Z_s]\right] \quad (10)$$

Again imposing linear expectations, we can rewrite

$$E\left[\pi_s \middle| X_{is}, E[\bar{X}_s|Z_s]\right] = \rho X_{is} + \tau E[\bar{X}_s|Z_s] = \rho X_{is}$$

and since π_s is orthogonal to Z_s τ is zero. This decomposition also adds a second bias term to the estimate of X_{is} based on correlations with the cluster level unobservable.

$$E\left[y_{is} \middle| X_{is}, E[\bar{X}_s|Z_s]\right] = (\beta + \varphi + \rho)X_{is} + \delta E\left[\bar{X}_s \middle| X_{is}, E[\bar{X}_s|Z_s]\right] \quad (11)$$

In comparison, the expected contribution of the $\bar{X}_s$ based on equation (5) is $\delta E\left[\bar{X}_s \middle| E[X_{is}|Z_s]\right]$ or equivalently $E[\bar{X}_s|Z_s]$ because the expectation of X_{is} conditional on a cluster level variable is just the cluster level expected mean. Essentially, conditioning on the prediction of individual enrollment conditional on based on school by grade offering is the same as simply conditioning on school by grade offerings when calculating the expectation of school by grade enrollment.

Rewriting the last term in equation (11), we obtain

$$E\left[\bar{X}_s \middle| X_{is}, E[\bar{X}_s|Z_s]\right] = \gamma X_{is} + \theta E[\bar{X}_s|Z_s] = \gamma(\bar{X}_s - \tilde{X}_{is}) + \theta E[\bar{X}_s|Z_s] \quad (12)$$

where $\tilde{X}_{is}$ is within cluster variation in X_{is}, e.g. each students deviation from the school by grade mean in-person enrollment.

Then, we can consider $\tilde{X}_{is}$ as a form of measurement error in $\bar{X}_s$, which implies that θ is bounded between 0 and 1. If the variance of $\tilde{X}_{is}$ is zero, then θ is zero because conditioning $\bar{X}_s$ on $\bar{X}_s$ perfectly predicts itself. On the other hand, as $\tilde{X}_{is}$ become large, γ attenuates and θ limits monotonically towards one. The monotonicity of the bias arises because attenuation from measurement error (deviation of student in-person enrollment from the school by grade average) and the incidental bias in the correlated variable (school by grade in-person offerings) are both monotonic in the variance of the measurement error. Note that the linear in means assumption is critical in equation (12) because without that assumption $\tilde{X}_{is}$ could result in non-classical measurement error and we could not longer assume the sign and monotonicity of the bias.

As noted above, $E\left[\bar{X}_s \middle| E[\bar{X}_s|Z_s]\right]$ equals $E[\bar{X}_s|Z_s]$ by definition so the overall effect estimates from equations (5) and (6) can be written as

$$E\left[y_{is} \middle| E[X_{is}|Z_s]\right] = (\beta + \delta)E[\bar{X}_s|Z_s] \quad (13)$$

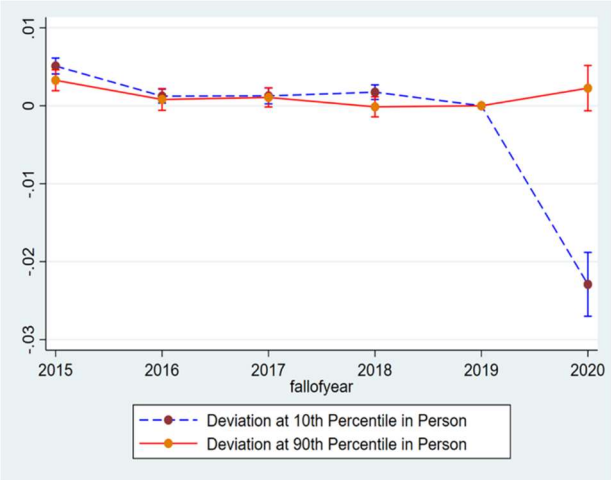
and the aggregate effect estimate from equations (7) through (11) can be written

$$E\left[y_{is} \middle| X_{is}, E[X_{is}|Z_s]\right] = (\beta + \varphi + \rho + \gamma)X_{is} + \delta\theta E[\bar{X}_s|Z_s] \quad (14)$$

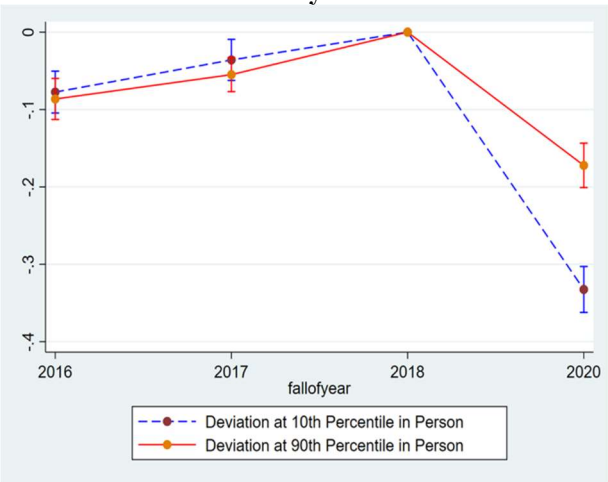
Therefore, IV estimates on $\bar{X}_s$ (school by grade average in-person enrollment) when conditioning on X_{is} (student level in-person enrollment) provides a lower bound on the aggregate effects of $\bar{X}_s$, and by combining equations (13) and (14) estimates of $(\beta + \delta) - \delta\theta$ provide an upper bound on the direct effect of the individual variable X_{is}. Note that equation (12) also implies a third bias term γ for the coefficient on X_{is} due to the correlation between X_{is} and Z_s.

Example Event Study Figures

1st-5th Grade Attendance by Share in Person



5th Grade ELA Scores by Share in Person



5th Grade Math Scores by Share in Person

