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Remote Learning in 2020-21 and Student Attendance Since the COVID-19 Pandemic

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Student attendance declined during the COVID-19 pandemic and remains lower than pre-pandemic levels. This study examines the role of remote learning in these post-pandemic declines in student attendance. I find that remote learning in 2020-21 led to persistent declines in post-pandemic attendance, with generally larger negative effects for students exposed to longer periods of remote learning, though this relationship between remote learning duration and post-pandemic attendance was not linear. Compared to students who were never provided with remote-only learning in 2020-21, students provided with remote-only learning for one, two, or three months had no statistically significant decline in attendance post-pandemic; those with remote-only learning for four, five, or six months subsequently missed a few additional days of school; and those with remote-only learning for seven, eight, or nine months missed substantially more. I also find some heterogeneity by race/ethnicity and economic status in 2021-22, but little persistence of these subgroup differences. These results clarify the impact of remote learning on post-pandemic attendance declines: substantial and persistent negative effects for students with prolonged remote-only exposure, but only a modest impact on Michigan's overall post-pandemic attendance, given that the large majority of students experienced relatively short periods of remote-only instruction.

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Abstract

Student attendance declined during the COVID-19 pandemic and remains lower than pre-pandemic levels. This study examines the role of remote learning in these post-pandemic declines in student attendance. I find that remote learning in 2020-21 led to persistent declines in post-pandemic attendance, with generally larger negative effects for students exposed to longer periods of remote learning, though this relationship between remote learning duration and post-pandemic attendance was not linear. Compared to students who were never provided with remote-only learning in 2020-21, students provided with remote-only learning for one, two, or three months had no statistically significant decline in attendance post-pandemic; those with remote-only learning for four, five, or six months subsequently missed a few additional days of school; and those with remote-only learning for seven, eight, or nine months missed substantially more. I also find some heterogeneity by race/ethnicity and economic status in 2021-22, but little persistence of these subgroup differences. These results clarify the impact of remote learning on post-pandemic attendance declines: substantial and persistent negative effects for students with prolonged remote-only exposure, but only a modest impact on Michigan's overall post-pandemic attendance, given that the large majority of students experienced relatively short periods of remote-only instruction.

Keywords: student attendance, chronic absenteeism, COVID-19 pandemic, remote learning

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Remote Learning in 2020-21 and Student Attendance Since the COVID-19 Pandemic

Student attendance declined during the COVID-19 pandemic and has not recovered to pre-pandemic levels. Chronic absenteeism (i.e., missing 10% or more of the school year) nearly doubled in the United States from about 15% in 2018-19 to about 28% in 2021-22 (Dee 2024). Since then, attendance has improved across the country, though only partially and unevenly, with chronic absenteeism rates still substantially above pre-pandemic rates on average nationwide (Malkus 2024). There are a wide range of theories for why student attendance has declined, and why lower attendance rates have persisted: pandemic-era disruptions to daily routines; weakened school-family relationships; worsening student mental health; shifting parental attitudes such as greater caution about illness or less urgency about daily attendance; new economic and logistical barriers; and the normalization of absenteeism through changing work routines, remote learning, and relaxed school policies (MacGillis 2024; Mervosh and Paris 2024; Rapaport et al. 2025).

There is very little research, however, to help explain the post-pandemic decline in student attendance. This gap in research is largely due to a lack of longitudinal data (either quantitative or qualitative) to capture changes over time associated with declines in student attendance. For example, findings from a nationwide representative survey show that students with greater mental health challenges (which have grown since the pandemic) missed more school in 2023-24; but the study does not connect changes in student mental health to changes in student attendance over time (Rapaport et al. 2024). Likewise, a study looking at Detroit student attendance found that students who experienced greater computer and internet issues during remote learning and with greater socioeconomic disadvantages missed more school in 2020-21, but the findings do not explain changes in student attendance after the return to in-person learning (Lenhoff and Singer 2024). One study of schools in Illinois found that schools with stronger pre-pandemic school-family relationships (as measured by the 5Essentials survey) experienced smaller increases in absenteeism in 2021-22 compared to schools with weaker school-family relationships (Learning Heroes and TNTP 2024). Those findings suggest that existing school-family relationships mediated post-pandemic declines in attendance but do not explain whether changes in relationships (or other family, school, or community factors) explain the declines.

This study examines the role of remote learning in the post-pandemic declines in student attendance. Specifically, I ask: *What is the effect of remote learning duration in 2020-21 on post-pandemic student attendance?* To answer this question, I combine publicly available data on district learning modalities in 2020-21 with longitudinal student-level administrative data from Michigan, and I use two different quasi-experimental research designs (difference-in-differences and instrumental variables). I find that remote learning in 2020-21 led to persistent declines in post-pandemic attendance, with generally larger negative effects for students exposed to longer periods of remote learning, though this relationship between remote learning duration and post-pandemic attendance was not linear.

Compared to students who were never provided with remote-only learning in 2020-21, students provided with remote-only learning for one, two, or three months had no statistically significant decline in attendance post-pandemic; and those provided with remote-only learning for four, five, or six months missed a few additional days per school year on average, with effects that were persistent across post-pandemic years. The few students in Michigan who were provided with remote-only learning for seven, eight, or nine months missed substantially more days in 2021-22, 2022-23 than other students, though the magnitude of that negative effect partly faded in 2023-24. I also find some heterogeneity by race/ethnicity and economic status, with sharper attendance declines in 2021-22 for economically disadvantaged and racially minoritized students, but little persistence of these subgroup differences in later school years. These results clarify the impact of remote learning on post-pandemic attendance declines. Exposure to remote learning appears to have led for the small subset of students with prolonged remote-only exposure to miss substantially more days of school in subsequent years. Yet, remote learning explains only a modest share of Michigan's overall post-pandemic attendance decline, given that the large majority of students experienced relatively short periods of remote-only instruction.

Literature Review

In March 2020, governors across the country ordered schools to cease in-person learning in an effort to mitigate the spread of COVID-19 (Grossmann et al. 2021). In 2020-21, districts varied in which learning modalities they offered and when (if at all) they returned to fully in-person learning (Singer 2025). Most students in the United States participated in remote learning at some point during the 2020-21 school year, and most returned to in-person learning by the end of that school year (United States Department of Education 2021). Researchers have documented a number of strong predictors of remote learning duration in 2020-21, such as political partisanship, district demographics, and local COVID-19 infection rates. Districts tended to offer remote learning for longer during 2020-21 if they had a more heavily Democratic constituency (often measured by the 2020 presidential election vote share), if they served a larger share of low-income students and racially minoritized students, and (to some extent) if they had greater COVID-19 infection rates (Singer 2025).

Researchers have highlighted several consequences associated with remote learning during the pandemic. Districts that offered remote-only instruction at the start of 2020-21 experienced a sharper decline in enrollment than those offering in-person instruction at the start of the school year, though enrollment of Black and low-income students declined more in in-person districts than remote-only districts (Baum and Jacob 2024; Dee et al. 2022; Musaddiq et al. 2022). Student mental health also declined during the pandemic (Rapaport et al. 2024), though the role of learning modality is nuanced. One study found that for historically advantaged students, remote and hybrid instruction were associated with worse mental health outcomes than in-person instruction, but that these associations did not hold for historically disadvantaged students (Silver et al. 2025). Relatedly, one study suggests that school-based bullying and cyberbullying incidences decreased due to the shift to remote learning (Bacher-Hicks et al. 2022). Finally, students who spent more time in remote learning experienced greater levels of "learning loss" (Goldhaber et al. 2023). Importantly, decreases in student attendance also help to explain the extent of student learning loss during the pandemic: according to one analysis, increased chronic absenteeism may explain 16-27% of math test score declines and 36-45% of reading test score declines (Council of Economic Advisors 2023).

There are two studies on the role that remote learning played in declining student attendance. Evans et al. (2024) use national district-level data on learning modality in 2020-21 and chronic absenteeism rates to estimate the relationship between remote learning and chronic absenteeism for the 2021-22 school year. Controlling for pre-pandemic (2018-19) chronic absenteeism rates, they find that a longer duration in virtual instruction (rather than in-person instruction) was associated with greater increases in absenteeism, though there was no difference between hybrid and in-person instruction. They also find that this association between virtual learning duration and chronic absenteeism was stronger in higher-poverty districts than in lower-poverty districts, suggesting some heterogeneity based on the socioeconomic context. Anders et al. (2025) use data from the PISA to compare changes in truancy since the pandemic internationally among OECD counties. They rely on student self-reported measures from participating fifteenyear-olds—a repeated question about how often they skipped school in the past two weeks, and a one-time question about the duration of COVID-19 related school closures (complemented by internationally compiled data on school closure policies). While the authors uncover variation in the extent to which countries' truancy rates have increased, they find no evidence of an association between school closure duration and increased truancy. Taken together, these studies offer mixed evidence about how school closures and remote learning during the pandemic have shaped student attendance. Further, the strength of the evidence from both studies is limited (among other reasons) by the reliance on cross-sectional (rather than longitudinal) data.

If remote learning experiences have indeed had some lasting impact on student attendance post-pandemic, there are several possible explanations. Remote learning may have been less engaging than in-person learning or may have ruptured relationships between students and their peers and teachers (Hollister et al. 2022). Extended time out of school and in remote learning may have also been detrimental for student mental health (Rapaport et al., 2024). These negative experiences in 2020-21 could have led to greater disengagement with school, resulting in a decline in attendance. Remote learning experiences may have also impacted parents and guardians, who play a central role in attendance. For example, more time in remote learning may have led parents to increasingly discount the importance of in-person attendance (Mervosh and Paris 2024; Rapaport et al. 2025). Alternatively, parents in districts with longer periods of remote learning might have felt a greater degree of caution about student health (Singer et al. 2023; Rapaport et al. 2025). These changes in parent orientation toward in-person attendance might lead to increased absences in subsequent school years. Relatedly, school-based health measures, such as testing for COVID-19 and imposing quarantines, may led to lower attendance rates for at least one more school year after 2020-21 (e.g., Bakuli 2023). Finally, there may have been broader cultural, social, or economic shifts associated with the pandemic that have influenced student attendance (Wallace-Wells 2025), though these kinds of macro-level changes may not necessarily be connected to an individual student's remote learning duration.

Methodology

Since the duration of remote learning that districts adopted in 2020-21 was not random, it is difficult to credibly estimate a causal effect of remote learning duration on subsequent student attendance. While the findings in this study may not be strictly causal, I use two quasi-experimental methods in order to produce plausibly causal estimates. For my primary estimates, I use a difference-in-differences (DiD) design with robust student-level controls and year fixed effects to estimate the effect of between remote learning duration and attendance. To corroborate these findings, I use an instrumental variables (IV) design, first predicting remote learning duration based on a well-established predictor of school reopening (political partisanship) in the first stage, then using those first-stage results to estimate the effect of remote learning duration on post-pandemic attendance. Below, I describe the data and analyses in detail.

Data

The state of Michigan collected instructional modality data from districts on a monthly basis during the 2020-21 school year, through a legislatively mandated questionnaire about COVID-19 learning plans (Hopkins et al. 2020). Each month, districts indicated whether they planned to offer only in-person instruction, a mix of in-person and remote learning options (i.e., hybrid), or only remote learning options. These data are publicly available on MI School Data, the state's education data website (https://mischooldata.org/covid-dashboard). Using the instructional modality data, I created a measure of remote learning duration, which is the *number of months* during which students were provided with *remote learning only* by their district in 2020-21. In months that a district was not remote-only, schools may have provided either in-person only or hybrid instruction. The number of remote-only months provided to students thus captures the minimum amount of time they spent in remote learning.

I linked the instructional modality data to longitudinal, student-level administrative data. This dataset includes demographic, enrollment, and school attendance data for all Michigan K-12 public school students from 2017-18 through 2023-24. Based on enrollment data from 2020-21, I matched students with the number of months their districts provided remote-only learning in 2020-21. I also calculated students' pre-pandemic attendance rate (days attended divided by days enrolled) for the 2017-18, 2018-19, and 2019-20 school years as baseline measures, and their post-pandemic attendance rates for the 2021-22, 2022-23, and 2023-24 school years as outcomes.[1]

The data also included a number of student demographic characteristics, including student grade level, gender, race/ethnicity, special education status, English language learner status, and school locale (i.e., urban, suburban, or rural). I also used two measures of student disadvantage: the state of Michigan's indicator of student economic disadvantage, which is based on proxy measures of student income; and an indicator of school mobility, or whether a student changed schools during the school year (Goldhaber, Koedel, et al. 2022; Welsh 2017).[2] These demographic characteristics are important to account for because they are correlated with student attendance rates (e.g., students in high school miss more school than in middle school, students who switch schools miss more than students who don't); and they are correlated with remote learning duration

[1] Attendance data are missing for 2020-21 because Michigan adopted a modified criteria for counting student attendance during the COVID-19 pandemic in that school year. Attendance data for the 2019-20 schools are used with caution, given that the Michigan school year (and thus regular attendance-taking) was cut short due to school closures at the start of the COVID-19 pandemic.

[2] It is also important to control for within-year student mobility since students may have changed schools during the 2020-21 school year, leading to measurement bias. In fact, consistent with prior research (Cordes et al. 2023; Schueler and Miller 2024), within-year student mobility substantially declined during the 2020-21 school year (especially in districts that offered remote learning for three month or longer) before returning to pre-pandemic baselines. This reduces the number of students who might be assigned with the incorrect remote learning duration (Appendix A).

(e.g., racially minoritized students, lower-income students, and students in urban districts were more likely to learn remotely for longer) (Singer et al. 2023; Lenhoff and Singer 2025).

Finally, I collected additional district-level data related to districts' selection into remote learning during the 2020-21 school year, to construct an instrument for the IV estimates. Most importantly I added a measure of local partisanship, which researchers have shown to be the strongest and most consistent predictor of reopening status (Singer 2025). I used the Redistricting Data Hub's dataset of 2020 presidential results by Census block (Redistricting Data Hub 2023), which includes the total number of votes per block as well as the number of votes per block for each major party candidate. I matched the 2020 presidential voting data with students' school districts using the Geographic Relationship Files provided by the National Center for Education Statistics' Education Demographic and Geographic Estimates (EDGE) program (Geverdt 2019). The EDGE files include a geographic crosswalk between Census block groups and school districts in the United States. I aggregated the block-level voting data from the Redistricting Data Hub to the block group level, then matched each block group to its corresponding school district and further aggregated the voting data to the district-level. With district-level counts of total votes and votes for each major candidate, I created a measure of the percentage of voters in each school district who voted for Donald Trump (i.e., Republican) in 2020.

To complement this measure of local partisanship, I also incorporated data on COVID-19 case rates and district demographics, which are predictors of learning modality decisions during 2020-21 as well as correlates of local partisanship (Singer 2025). For COVID-19 case rates, I used the COVID-19 data repository maintained by the New York Times, which includes a daily county-level rolling average of COVID-19 case rates (i.e., cases per 100,000 residents) from March 2020 through 2023 (The New York Times 2023). I construct a measure of average county-level case rates for approximately before the 2020-21 school year (March 2020 through August 2020) and approximately during the school year (September 2020 through June 2021). Prior studies have found that more granular data—at school or district level, and in a closer timeframe to reopening decisions—is more predictive of reopening decisions (Darling-Aduana et al. 2022; Christian et al. 2024). Still, these rough measures are useful for incorporating some degree of COVID-19 exposure in the IV estimates. For district-level characteristics, I included measures of each student's district racial composition (percentage of white students) and district economic composition (percentage of "economically disadvantaged") from 2020-21. I created these demographic measures by aggregating the student-level data to the district level and matching those aggregates back to students based on their 2020-21 district enrollment.

Sample

To be included in the study, students had to be enrolled in a Michigan public school during the treatment (i.e., remote learning in 2020-21) for at least one pre-treatment period, at least one post-treatment period (over one million students). As a result, slightly fewer students are observed in the earliest and latest grade levels (i.e., K-2 and 10-12), as they were less likely to include both pre-treatment and post-treatment observations. In addition, I excluded students for whom data was not available on their district's remote learning duration during the 2020-21 school year.[3] Only about two percent of students from the study population were excluded due to missing data on remote learning duration. In total, the sample included 5,177,053 observations of 992,582 unique students over the 2017-18 through 2023-24 school years.

[3] I followed What Works Clearinghouse (2022) guidelines to address other missing data. The only field with missing data was baseline attendance, as some students were observed in some but not all baseline years (e.g., in 2018-19 and 2019-20 but not 2017-18). I use "dummy imputation," which is acceptable for baseline measures in quasi-experimental research (p. 97). Specifically, I used a flag for missing baseline attendance by year, which allowed me to include those students observed in less than all baseline years without dropping them from the analytic sample.

Table 1 presents the distribution of students and districts in 2020-21 by remote learning duration. Most students in the sample were either never in remote-only learning (about 38%) or provided with remote-only learning for a short time, such one month (about 18%) or two months (about 17%). Of the remaining students, some were provided with remote-only learning for longer than others. The other most common lengths were three months or six months (about 7.5% each), though some were in remote-only learning for four months (about 3%), five months (about 4%). Few students were in remote-only learning for seven months (about 1%) or eight month (less than 1%), and only about 2% of students were in remote-only learning for the entire school year. The distribution of districts by remote learning duration largely mirrors that of students, though a larger share of districts (about 48%) were never in remote-only learning, suggesting that this category disproportionately includes small (and rural) districts across the state.

[Table 1 here]

Table 2 compares students by their 2020-21 remote learning duration. For these comparisons, I combined students into four groups: 0-2 months of remote-only learning, 3-5 months, 6-7 months, and 8-9 months. (I use the 0-2 months group as the reference category to consider the statistical significance of differences by group.) The table includes the average duration of in-person, hybrid, and remote learning; individual student characteristics; and district characteristics in 2020-21 related to school reopening decisions.

[Table 2 here]

Remote learning duration in 2020-21 was inversely related to the amount of time schools provided in-person learning options (Table 2). On average, students exposed to remote-only learning for 0-2 months (71.56%) were provided with in-person only learning for about six months and hybrid learning for about two months. In comparison, students exposed to remote-only learning for 3-5 months (17.44%) were provided with in-person only for about three months and hybrid for about two months on average; and students exposed to remote-only learning for 6-7 months (8.41%) were provided with in-person only for less than one month and hybrid for about two months on average. Those few students who were provided with remote-only learning for 8-9 months (2.60%) were never provided with in-person learning only.

Student characteristics also differed on average by remote learning duration. These differences reflect well-documented patterns of school closures and remote learning participation (Singer 2025). Students who were provided with remote learning for more of the school year were more likely to be economically disadvantaged, Black, and attend an urban school. Those students who were provided with remote-only learning for longer also had lower baseline attendance rates. Students who were provided with remote-only learning for 0-2 months had an average attendance rate in 2018-19 of 95%, compared to 92% for students who were provided with remote-only learning for 3-5 months, 93% for 6-7 months, and 91% for 8-9 months.

Finally, as with student characteristics, 2020-21 district characteristics by remote-learning duration also reflect differences that are well-documented in the school reopening literature (Singer 2025). Students who were in remote-only for longer in 2020-21 were in districts that year with a larger share of economically disadvantaged and racially minoritized students. In addition, the district-level Trump (i.e., Republican) vote share in 2020 was lower for students that spent longer in remote-only. Finally, countywide COVID-19 case rates before the 2020-21 school year were slightly higher for students in remote-only learning for 3-5 months and 6-7 months than those in remote-only learning for 0-2 months, but were actually slightly lower for students in remote-only learning for 8-9 months. COVID-19 case rates during the 2020-21 school year were actually slightly higher for students whose districts spent less time in remote-only learning, which reflects (at least in part) the documented impact of remote learning as a mitigation strategy (Goldhaber, Imberman, et al. 2022). Taken together, these observable student and district differences by remote learning duration highlight the need for quasi-experimental research designs to address selection-into-treatment bias. In the following sections, I describe my DiD and IV approaches.

Difference-in-Differences (DiD) Approach

I use a dosage DiD design to estimate changes in student attendance from pre-pandemic to post-pandemic over time. The treatment is the number of months that a student spent in remote learning in 2020-21. First, I estimated a pre/post DiD. For student *i* in year *t* and grade *g*, and who was enrolled in district *d* during the 2020-21 school year, I predicted a student's attendance rate (Y), based on the following equation:

$$Y_{itg} = \beta_0 + \beta_1(Remote)_d + \beta_2(Post)_t + \beta_3(Remote_d \; x \; Post_t) + X_{it}\alpha + Z_{it}\delta + \gamma_g + \varepsilon_{itg}$$

where *Remote* is the measure of remote learning duration, *Post* is a binary indicator of the post-treatment period (i.e., 2021-22 through 2023-24), *X* is the student's baseline pre-attendance rates (i.e., 2017-18, 2018-19, and 2019-20), *Z* is a vector of student characteristics used as controls, γ is grade-level fixed effects, and ε is an idiosyncratic error term (with standard errors clustered at the district level).[4]

Then, I estimated an event study DiD. The event study estimates allow me to examine pre-trends to assess the parallel trends assumption. A parallel trend in the outcome for treated students (i.e., students who experienced some amount of remote-only learning in 2020-21) and not-treated students (i.e., students who were never in remote-only learning in 2020-21), this strengthens our

[4] I use a robust set of student-level covariates instead of student fixed effects, because the treatment is at the district level rather than student level and the model with student-level covariates allows for a formal test of heterogeneity by race/ethnicity and income status. In addition, the use of student-level covariates offers a parallel model structure to the instrumental variables model (which relies on the same student-level covariates). I estimated a student fixed effects model as a robustness check and found nearly identical results (Appendix B).

ability to draw inferences about the relationship between the treatment and outcome in the post-treatment estimates. The event study estimates also demonstrate whether the effect of remote learning duration changes (e.g., fades out) or remains stable over time. The event study estimates are based on the following equation:

$$Y_{itg} = \beta_0 + \beta_1(\mathit{Remote})_d + \beta_2(\mathit{Period})_t + \beta_3(\mathit{Remote}_d \ x \ \mathit{Period}_t) + X_{it}\alpha + Z_{it}\delta + \gamma_g + \varepsilon_{itg}$$

where *Remote* is the measure of remote learning duration, *Period* is a indicator of the specific pre-treatment or post-treatment (i.e., 2017-18, 2018-19, 2019-20, 2021-22, 2022-23, and 2023-24), X is the student's baseline pre-attendance rates, Z is a vector of student characteristics used as controls, γ is grade-level fixed effects, and ε is an idiosyncratic error term (with standard errors again clustered at the district level). To test for heterogeneity by student socioeconomic status and race/ethnicity, I also estimate effect of remote learning duration based on interactions between the treatment, period, and student subgroup.

In both the pre/post and event study estimates, the coefficient of interest is β_3, which indicates the average effect of remote learning duration on post-pandemic attendance. Specifically, the coefficient shows the percentage-point change in attendance associated with an increase in the remote learning duration. For example, a coefficient of -1 would correspond to a 1 percentage point (pp) decrease in a student's attendance rate post-pandemic (or an additional 1.8 days missed in a 180-day school year). In the pre/post estimates, β_3 represents the average effect of remote learning duration on subsequent attendance for the post-treatment period overall; and in the event study estimates, β_3 represents the average effect of remote learning duration on subsequent attendance for each post-treatment period. Controlling for baseline student attendance (which is similar to incorporating student fixed effects), treatment period (i.e., time fixed effects), grade-level fixed effects, and other relevant control variables allows for a precise estimate of β_3.

I modeled the measure of remote learning duration (i.e., the treatment variable) two different ways. First, I treated remote learning duration as a continuous variable (ranging from zero months to nine months), essentially predicting a linear effect on attendance. Second, I treated remote learning duration as a categorical variable (with zero months remote-only as the reference category), to assess whether there was a non-linear effect on attendance based on different remote learning durations. While the continuous measure is useful for simplicity and parsimony, including for testing pre-trends and interactions, the categorical measure is more likely to offer an accurate estimate of the treatment effect (Callaway et al. 2024).

Instrumental Variables (IV) Approach

IV models use a two-stage least squares (2SLS) estimator, which identifies causal effects by first predicting selection into treatment to adjust for selection bias (first-stage) and then estimating the effect of the treatment based on those predicted treatment values (second-stage). The first-stage prediction requires a strong "instrument"—a variable (or set of variables) that strongly and statistically significantly predict the treatment. Importantly, IV estimates reflect the causal effect for only the subset of students whose likelihood of treatment is shifted by the instrument (i.e., compliers). Thus, while the IV estimates provide plausibly causal, the estimated local average treatment effect apply only to this subgroup of compliers and may not generalize to all students. I therefore use the IV approach primarily as a robustness for the DiD estimates, assessing whether the main findings are sensitive to residual selection into treatment may not be adequately addressed by the DiD design. Accordingly, the IV results are not interpreted as the primary estimand, but rather as evidence on whether residual selection into treatment (beyond what is accounted for by DiD) affects the conclusions.

For my instrument, I used a combination of several district-level predictors of reopening from the 2020-21 school year—the share of voters in each district that voted for Donald Trump (i.e., Republican) in the 2020 presidential election, along with COVID-19 rates before and during the 2020-21 school year, and the percentage of students in the district in 2020-21 who were white and who were economically disadvantaged (i.e., racial and socioeconomic composition). For student *i* in year *t* and grade *g*, and who was enrolled in district *d* during the 2020-21 school year, I predicted a student's remote learning duration in 2020-21 (R), based on the following equation:

$$R_d = \theta_0 + \theta_1(\mathit{Trump})_d + \theta_2(\mathit{COVID})_d + \theta_3(\mathit{Demographics})_d + X_{it}\alpha + Z_{it}\delta + \gamma_g + \lambda_t + \varepsilon_{itg}$$

where *Trump*, *COVID*, and *Demographics* are the instrumental variables described above. I also included student-level controls used in both the first and second stages: X is the student's baseline pre-attendance rates, Z is a vector of student characteristics used as controls, γ is grade-level fixed effects, and λ is year fixed effects. Finally, ε is an idiosyncratic error term with standard errors clustered at the district level.

For the second stage, I predicted a student's post-pandemic attendance rate (Y) based on the following equation:

$$Y_{itg} = \beta_0 + \beta_1\hat{R}_d + X_{it}\alpha + Z_{it}\delta + \gamma_g + \lambda_t + \varepsilon_{itg}$$

where $\hat{R}$ is the instrumented value of a student's 2020-21 district remote learning duration predicted from the first stage. Again, the estimates include a student's baseline pre-attendance rates (X), a vector of student characteristics used as controls (Z), grade-level fixed effects (γ), year fixed effects (λ), and an idiosyncratic error term (ε) with standard errors clustered at the district level.

I use multiple test statistics to establish the validity of the instrument. First, I report the F-statistic from the first-stage regression, which tests whether the instrumental variables sufficiently predict the treatment. Higher F-statistic values indicate a stronger instrument (Angrist and Pischke

2009), and instruments are typically considered "weak" if the F-statistic is below ten (Staiger and Stock 1997). In addition, one assumption of the IV approach is the exclusion restriction, which requires the instrumental variables to influence the outcome only indirectly by influencing their selection into treatment (Angrist and Pischke 2009). While the exclusion restriction cannot be directly tested, its plausibility can be assessed through placebo tests. Here, I test whether the instrumental variables predict the outcome in pre-treatment periods (i.e., student attendance in 2018-19 or 2019-20). If they did predict the outcome prior to the treatment, this would suggest that they have a direct relationship with the outcome, violating the exclusion restriction. Together, the F-statistic and placebo tests help establish the causal plausibility for the IV estimates.

Limitations

There are some notable limitations to this study. First, the measure of exposure to remote learning is at the district level, rather than the student level. The measure captures the number of months that a district provided remote-only learning in 2020-21. For schools that provided a hybrid learning modality, however, it is possible that some students remained in remote learning while their peers returned to in-person learning.[5] Second, some districts may have switched learning modalities dynamically throughout the year, which is not captured by a static aggregate measure of remote learning duration (Christian et al. 2024); and some districts may have returned to remote learning temporarily during the 2021-22 school year during subsequent COVID-19 spikes (e.g., Bakuli, 2022), but this is not captured in the available data. Finally, the measure simply captures remote learning status, rather than other potentially important qualitative features of the remote

[5] I am also unable to further differentiate among districts' hybrid arrangements because I do not have access to more granular data on their specific operational arrangements during hybrid learning. Notably, as shown in Table 2, for all students whose districts with less than eight or nine months in remote learning, students on average were exposed to about two months of hybrid learning. This suggests that hybrid functioned similarly across districts as a transitional modality before returning to in-person instruction, whether districts offered in-person learning for just a few months or for most of the school year.

learning experience or other school operations, which likely differed between schools (Bartlett 2022; Schueler et al. 2023; Singer et al. 2023). Therefore, there may be measurement error related to exposure to remote learning.

Also, since students were not randomly or quasi-randomly exposed to remote learning (Singer 2025), the estimates do not strictly identify a causal effect. It is hard to disentangle the effect of remote learning duration from other factors correlated with the duration of remote learning, such as heightened family concerns about health and safety, school capacity or resource constraints, or subsequent changes to school policies and practices, which are unobserved in the available data (Christian et al. 2024; Darling-Aduana et al. 2022; Jacob 2024; Jacob and Stanojevich, 2024; Weber and Baker, 2025).[6] Interpretations must therefore account for these potentially confounding factors. Still, the corroboration of findings across multiple quasi-experimental research designs does suggest a plausibly causal interpretation of the results.

Findings

Descriptive trends in student attendance by remote learning duration suggest a relationship between remote learning and post-pandemic attendance that is durable but attenuated over time. In Figure 1, the first panel shows average attendance rates over time for students in remote learning for 0-2 months, 3-5 months, 6-7 months, and 8-9 months; and the second panel shows the percentage point change in attendance relative to the 2017-18 baseline for each group.

[Figure 1 here]

On average, students whose schools provided remote learning for longer in 2020-21 had greater declines in attendance post-pandemic. In 2021-22, the average attendance rate for students exposed to 0-2 months in remote-only learning was about 5pp lower than in 2017-18 (from 95%

[6] The estimates are, however, robust to county-level COVID-19 case rates, as evidenced by the IV results and the consistently of the DiD model with and without COVID-19 variables (Appendix C).

to 90%), compared to about 6pp lower for 3-5 months (from 93% to 87%), about 7pp lower for 6-7 months remote-only (from 93% to 86%), and about 10pp lower for 8-9 months remote-only (from about 91% to 81%). In 2022-23, attendance rates for all students improved by about 1.5pp on average. In 2023-24, however, attendance rates only improved on average for students who were in remote-only learning for longer—slightly (about 0.5pp) for 6-7 months, and substantially (about 2pp) for 8-9 months.

In sum, students with longer remote learning durations in 2020-21 had greater decline in attendance relative to their pre-pandemic baseline, though this difference narrowed over time. By the end of the 2023-24 school year, the average attendance rate for students exposed to 0-2 months in remote-only learning remained about 3pp lower than in 2017-18 (i.e., about 5-6 more absences in a 180-day school year), compared to about 5pp lower for 3-5 months and 6-7 months remote-only (i.e., 9 more absences), and about 7pp lower for 8-9 months remote-only (i.e., 12-13 more absences). In the following sections, I present formal estimates of the effect of remote learning duration on post-pandemic attendance from the DiD and IV.

Difference-in-Difference (DiD) Results

Table 3 presents the DiD estimates for the effect of remote learning duration on post-pandemic attendance. The first two columns show results for the pre/post model, first using the continuous measure of remote learning duration (i.e., linear estimates) and second using the categorical measure (i.e., non-linear estimates). The third column shows the results for the event study model using the continuous measure, and results for the event study model using the categorical measure are located in Appendix D and E.

[Table 3]

Pre/Post Estimates

The pre/post estimates suggest a substantial and increasingly negative but non-linear impact of remote learning duration on post-pandemic attendance. The linear estimates (column 1) suggest that each additional month of remote learning led to a 0.46pp lower attendance rate (i.e., about 1 additional absence in a 180-day school year) in the post-pandemic period. The non-linear estimates (column 2), however, reveal some important nuances in the effect of remote learning duration. First, there is no statistically significant effect of spending one, two, or three months in remote learning (compared to no months of remote learning). Second, spending four months or more in remote learning led to substantially lower attendance rates post-pandemic, with these negatively effects mostly increase as remote learning duration increases, but not monotonically. Rather, students who spent four, five, or six months in remote learning had about 2pp lower attendance rates post-pandemic (i.e., about 3-4 additional absences)—with larger negative impacts for seven months of remote learning (about 3.5pp lower attendance rates, or 6 additional absences), eight months of remote learning (about 4pp lower attendance rates, or 7 additional absences), or nine months of remote learning (about 5pp lower attendance rates, or 9 additional absences).[7]

The results are similar when using the probability of chronically absence as the outcome (Appendix F). There is no practically meaningful or statistically significant difference for students with remote-only learning for one, two, or three months, compared to students who were never provided remote-only learning. However, remote-only learning led to a substantial increase in the probability of being chronically absent post-pandemic for four, five, or six months (5-6pp); and an

[7] To formally test whether the effects declines differed across instructional modalities, I conducted a joint test of the modality-by-post interaction terms for the non-linear results (column 2). The results indicate that the estimates are not statistically significant difference for students who spent zero, one, two, and three months in remote learning; for students who spent four, five, and six months in remote learning; or for students who spent seven, eight, and nine months in remote learning.

increasingly higher probability for students with remote-only learning for seven months (8pp), eight months (9pp), or nine months (10pp).

Event Study Estimates

The event study estimates suggest a persistent effect of remote learning duration on post-pandemic attendance over time. The linear estimates (column 3) show essential identical effects in 2021-22 and 2022-23, with slight decline in 2023-24. The non-linear estimates show a similar trend over time (Appendix D and E). There remains no statistically significant effect on students in remote-only learning for one, two, or three months over time; and a persistent negative effect for students in remote-only learning for four or more months, though in 2023-24 the negative effects fade more substantially for students who had eight or nine months or remote-only learning.

The event study estimates also offer support for a causal interpretation of the results—the coefficients for the pre-pandemic periods (i.e., 2018-19 and 2019-20) are not statistically significant in the linear estimates (column 3) and for the most part in the non-linear estimates (Appendix D and E), suggesting that these estimates satisfy the parallel trends assumption.

Heterogeneity Analyses

The findings also suggest some notable heterogeneity in the effects (immediately and over time) of remote learning duration. Table 4 presents interaction term coefficients for the event study DiD estimates by race/ethnicity and economic status (using the continuous treatment measure for parsimony). Each column indicates the student subgroup for which an interaction was included, for a model with race/ethnicity interactions (first set of columns) and economic status interactions (last column). The coefficients represent the effect of the treatment (i.e., remote learning duration) by period (i.e., school year) compared to the reference groups (White students for race/ethnicity and “not economically disadvantaged” students for economic status).

[Table 4]

The results show a larger negative effect for racially minoritized and lower-income students in the immediate next school year, though these differences mostly faded over time. In the pre-treatment periods, there were no statistically significant differences by race/ethnicity or economic status. In 2021-22, however, the estimates (coefficients ranging from -0.30 to -0.35) show a larger negative effect of remote learning duration for students who are Black, Hispanic, Asian, or another race, compared to White students. Similarly, for the economic status model, the interaction coefficient in 2021-22 (-0.30) shows a larger negative effect for students who are economically disadvantaged, compared to non-economically disadvantaged students. Though the magnitude of these interaction terms is hard to interpret in the absence of baseline treatment effects, they can be seen as substantively meaningful differences when considering the overall post-treatment effects estimated previously (ranging from -0.44 to -0.53; see Table 3, column 3).

In 2022-23 and 2023-24, however, there is little evidence of differences by race/ethnicity or economic disadvantage. The interaction terms remain statistically significant (and smaller but still substantially negative) for students in the "other" race/ethnicity category (about 90% of whom are identified as multiracial). For other racially minoritized students and for economically disadvantaged students, however, the interaction terms are statistically insignificant and fade in magnitude over time. These results suggest that these racial/ethnic and economic disparities in the effect of remote learning duration were acute but temporary, with sharp initial differences quickly fading for almost all subgroups.

Instrumental Variables (IV) Results

Table 5 presents the IV estimates for the effect of remote learning duration on post-pandemic attendance. The first column presents the results of an ordinary least-squares (OLS) regression to estimate the association between remote learning duration and post-pandemic attendance, without the use of the instrumental variables. This estimate serves as a reference to understand how the estimates change after addressing selection bias with the IV design. The second column presents the results of the 2SLS, as well as diagnostic statistics for the instrument (e.g., F-statistic, coefficients from the first-stage regression). Both the OLS and 2SLS use the continuous (rather than categorical) measure of remote learning duration, in part because the number of instrumented treatment variables (eight total when treated categorically) cannot exceed the number of instrumental variables in an IV model.

[Table 5]

The IV results confirm a strong negative effect of remote learning duration on post-pandemic attendance. In the OLS estimates, the coefficient for remote learning duration is -0.25, suggesting a more modest relationship between remote learning duration and post-pandemic attendance (i.e., 0.25pp lower post-pandemic attendance rate, or 0.5 more days absent in a 180-day school year, for each additional month in remote learning). In the 2SLS estimates, however, the coefficient is -0.77, which translates to an effect of a 0.77pp lower attendance rate (or about 1.5 more days absent in a 180-day school year) for each additional month in remote learning. The larger magnitude of the 2SLS estimates than the OLS estimates (three times as large) highlights reveal the impact of selection bias in the naïve OLS model and underscores the importance of accounting for selection into the treatment.

The IV estimates are also somewhat larger in magnitude than the DiD estimates (-0.46; see Table 3, column 1). While this may suggest that the DiD estimates are also somewhat biased due to selection, the IV estimates reflect a local average treatment effect for compliers only, and therefore are best interpreted as complementary evidence rather than a direct benchmark for the

DiD results. Most importantly, the overall pattern of results is consistent across approaches, strengthening confidence in the magnitude and direction of the estimated effects.

The IV diagnostics indicate that the instruments are strong predictors of the treatment and suggest that they plausibly satisfy the exclusion restriction. The F-statistic is 29.91, indicating a strong instrument. Based on the overall R^2 for the first-stage estimates, the instrumental variables and student-level controls explained 42% of the variation in remote learning duration. The partial R^2 indicates that together, the instrumental variables explained 19% of the variation in remote learning duration. In placebo tests using instrumental variables to predict pre-pandemic attendance, the local partisanship and COVID-19 measures are not statistically significant, which offers suggestive evidence that these variables are indeed only related to attendance indirectly through remote learning duration (Appendix G).[8]

Discussion

This study examined the effect of Michigan students' exposure to remote-only learning in 2020-21 on their school attendance in subsequent years. The findings show that students who spent longer periods in remote-only instruction experienced greater declines in their attendance post-pandemic, but also reveal important nuances due to the non-linear relationship between remote learning duration and post-pandemic attendance. For students who experienced only one, two, or three months of remote instruction, this had no statistically significant effect on attendance outcomes compared to those who spent no time in remote-only instruction. Those exposed to four, five, or six months of remote-only instruction experienced moderate but persistent declines in

[8] While the percentage of economically disadvantaged students in 2020-21 is a statistically significant predictor in the placebo test, it is likely due to the well-established negative relationship between concentrated economic disadvantage and chronic absenteeism (Singer et al. 2021). District-level economic disadvantage in one school year is highly correlated with other years (r=0.83 or above for the sample in this study, with r=0.95 for adjacent school year). So, the statistical significance of the 2020-21 measure is a reflection of the relationship between the district-level economic disadvantage in that year, rather than a true violation of the placebo test. The percentage of white students in 2020-21 is not a statistically significant predictor in the placebo tests.

attendance; and those in remote-only instruction in 2020-21 for seven, eight, or nine months experienced substantially larger declines, particularly in 2021-22 and 2022-23, though these negative effects became smaller (but did not completely fade out) by 2023-24.

These results help clarify the role that instructional modality in 2020-21 may have played in post-pandemic attendance declines, and more broadly contribute to the literature on the educational consequences of pandemic-era schooling disruptions. Michigan students who spent more time in remote learning did miss more school in subsequent school years, affirming that the experiences students had in remote learning were consequential for subsequent attendance. The findings suggest that for those who were in remote-only learning for most or all of the 2020-21 school year, their post-pandemic attendance would have been substantially better had their districts offered in-person learning sooner. Still, the effect on Michigan's overall attendance rates since the pandemic is relatively modest, given that the large majority of students (more than 80%) were in districts that only offered remote-only learning for three months or less. A quick back-of-the-envelope calculation based on the non-linear DiD estimates suggest that if no districts offered remote-only learning, Michigan's average daily attendance rate in 2023-24 would have been 0.5pp higher (91.3% instead of 90.8%), and its chronic absenteeism rate would have been 2.5pp lower (27% instead of 29.5%) (Singer 2024). This would represent a meaningful but modest improvement of the state's post-pandemic attendance and chronic absenteeism rates, which helps contextualize the magnitude of these effects of remote learning.

The findings of this study help clarify the mixed evidence in two prior studies on the relationship between remote learning and student attendance. This study is consistent with Evans et al.'s (2024) findings that longer durations of remote-only learning led to higher post-pandemic absenteeism in 2021-22, especially for economically disadvantaged and racially minoritized students; though my longitudinal results suggest that while the negative effects of remote learning generally have persisted, racial/ethnic and socioeconomic disparities have not. While Anders et al. (2025) find little evidence of a relationship between school closure duration and truancy, their outcome measure captures whether students ever skipped school and not the amount of school they have missed, which thus may limit their ability to capture the effects of remote learning duration on attendance rate documented here. (The reliance on a self-reported measure of truancy, with a survey question that emphasized skipping school rather than more generally being absent, may have also bias their results.) By leveraging longitudinal student-level data, this study shows that remote learning had meaningful and persistent consequences, primarily for the (relatively small) subset of students who were offered long periods of remote-only instruction.

There are several possible explanations for the effect of remote learning in 2020-21 on subsequent attendance (e.g., disrupted relationships, new precautions about health, increased student mental health problems, reduced urgency about daily attendance). The absence of effects for short remote exposures and the non-monotonic pattern across longer durations would suggest a threshold for these kinds of social or behavioral effects, in which extended time away from in-person schooling (rather than each additional month) was consequential. The persistence of the effects over time suggests relatively durable changes for students and families, though the fact that there was some fading-out for students in the remote learning for the longest (e.g., eight or nine months) suggests some readjustment over time (e.g., reestablishment of school routines, improvements in student well-being, or shifts in school engagement strategies) after particularly sharp negative impacts immediately after remote learning.

The findings here do not rule out the notion of a macro-level shift that explains the decreases in attendance—for example some broad cultural shift in student and parent attitudes or behaviors toward school (MacGillis 2024), durable changes to school policies and practices (Jacob 2024; Jacob and Stanojevich 2024), or other social and economic changes that continue to impact students and their families (Wallace-Wells 2025). They do, however, challenge the connection between such shifts and the specific amount of time districts offered remote-only learning. Rather, any explanation for the broad increases in student absenteeism post-pandemic must include multiple causes, with remote learning duration as only one factor. Future research could assess the extent to which attendance patterns have shifted for students never exposed to pandemic-era education (e.g., no immediate family members in school) compared to those partial exposed (e.g., sibling in school) or fully exposed (i.e., in school during 2020-21).

The heterogeneity in the effects of remote learning by race/ethnicity and economic status also suggest some distinct explanations for those effects. The brief but pronounced disparities in the effects of remote learning on economically disadvantaged and racially minoritized students 2021-22 may be explained by pandemic-related district policies. Districts with larger shares of economically disadvantaged and racially minoritized students had already offered longer periods of remote learning in 2020-21 on average; and these families often expressed greater health caution and stronger interest in remote learning options during the pandemic (e.g., Camp et al. 2023; Camp and Zamarro 2022). It is therefore likely that their districts maintained stricter quarantine expectations in 2021-22 (Bakuli 2023), and even that their districts shifted back into remote learning for part of the 2021-22 school year in response to new waves of COVID-19 (Bakuli 2022).

It is also possible that districts serving a larger share of economically disadvantaged and racially minoritized students were more effective in their post-pandemic efforts to improve attendance. It is true that the highest-absenteeism schools in Michigan (which also enroll a disproportionately large share of economically disadvantaged and racially minoritized students) have shown the greatest improvement in attendance post-pandemic (Singer 2024). These schools' specific strategies are not substantially different than others, but they are somewhat more likely to be focused on attendance, have organizational systems for improving attendance (e.g., as attendance teams, multi-tiered systems of support) in place, and have school leaders who spend time focused on attendance (Singer and Lenhoff 2025).

Because these possible underlying mechanisms are not directly measured in this study, however, these explanations remain speculative. Further, the non-random nature of the treatment means that the results could reflect other unobserved factors associated with remote learning duration (e.g., other school capacities, characteristics, policies, or practices, or the extent of exposure to COVID-19). Additional research is necessary to understand which of the complex set of changes during the COVID-19 pandemic have led students to miss more school in its wake. To the extent that longitudinal data are unable to capture these potential causes quantitatively, qualitative research will be necessary to capture changes in student and family perspectives, behaviors, and circumstances that have shaped student attendance. These insights can help inform schools prioritize the specific strategies they adopt to improve attendance, since recent evidence from Michigan indicates that while some schools have increased their focus on attendance since the pandemic, others report little change; and across contexts, schools tend to rely on a similar set (and wide variety) of practices, rather than tailoring strategies based on their students' specific barriers to attendance (Singer and Lenhoff 2025).

Finally, it is important to note that while most districts have experienced declines in student attendance since the pandemic, the relative distribution of absenteeism has remained largely the same. In other words, districts that had the highest rates of chronic absenteeism before COVID-19 still have among the highest rates of chronic absenteeism today (Singer 2024). Although researchers, practitioners, and policymakers are still trying to understand the specific drivers of post-pandemic declines in attendance, the deep-rooted social and economic inequities that have long contributed to chronic absenteeism are already well-documented and remain pressing issues to address (Lenhoff and Singer 2025).

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Tables

Table 1

Distribution of Students by District Remote Learning Duration in 2020-21

N Months Remote-Only	N Students	Pct. of Students	N Districts	Pct. of Districts
0	377,491	38.03	393	48.28
1	181,326	18.27	108	13.27
2	170,411	17.17	121	14.87
3	74,354	7.49	38	4.67
4	32,973	3.32	26	3.19
5	43,860	4.42	24	2.95
6	74,291	7.48	58	7.13
7	10,327	1.04	17	2.09
8	3,063	0.31	5	0.61
9	24,486	2.47	24	2.95
Total	992,582	*100%*	*814*	*100%*

Note: study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. Percentages may not sum precisely to 100% due to rounding.

Table 2

Student Characteristics by District Remote Learning Duration in 2020-21

	0-2 Months	3-5 Months	6-7 Months	8-9 Months
Modality in 2020-21				
In-person only (months)	6.14	2.69***	0.78***	0.00***
Hybrid (months)	2.07	2.49*	2.07	0.11***
Remote only (months)	0.72	3.80***	6.12***	8.89***
Baseline attendance rate (2017-18)	0.95	0.93***	0.93***	0.91***
Student Disadvantage				
Within-year school mobility	0.03	0.04	0.04***	0.05***
Economically disadvantaged	0.48	0.61***	0.64***	0.81***
Student Demographics				
Female	0.49	0.49	0.49	0.49
Special education	0.13	0.14***	0.14***	0.14***
English learner	0.05	0.11***	0.08***	0.09***
White	0.75	0.50***	0.37***	0.22***
Black	0.09	0.28***	0.43***	0.54***
Asian	0.03	0.04***	0.05***	0.02***
Hispanic	0.07	0.12***	0.08***	0.13***
Another race/ethnicity	0.05	0.05	0.06***	0.09***
School Locale				
Urban	0.13	0.35***	0.46***	0.68***
Suburban	0.45	0.57***	0.49***	0.28***
Town	0.15	0.01***	0.01***	0.01***
Rural	0.27	0.07***	0.05***	0.03***

Note: study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. **p<0.05, **p<0.01, ***p<0.001*, in comparison to “0-2 months remote-only.”

Table 3

Difference-in-Difference Estimates of the Effect of District Remote Learning Duration on Post-Pandemic Attendance

	(1)	(2)	(3)
Remote months (continuous) x Post-COVID	-0.46*** (0.07)	-	-
Remote months (categorical) x Post-COVID (ref. = 0 months)			
1 month	-	0.12 (0.27)	-
2 months	-	-0.09 (0.30)	-
3 months	-	-0.87 (0.66)	-
4 months	-	-2.41** (0.74)	-
5 months	-	-1.86*** (0.46)	-
6 months	-	-2.04*** (0.40)	-
7 months	-	-3.46*** (0.81)	-
8 months	-	-3.90*** (0.68)	-
9 months	-	-5.28*** (1.43)	-
Remote months (continuous) x School year (ref. = 2017-18)			
2018-19	-	-	-0.03 (0.02)
2019-20	-	-	-0.07 (0.05)
2021-22	-	-	-0.52*** (0.15)
2022-23	-	-	-0.53*** (0.09)
2023-24	-	-	-0.44*** (0.07)
N observations	5,177,053	5,177,053	5,177,053
N students	992,582	992,582	992,582
R^2	0.35	0.35	0.36
Student demographic controls	Yes	Yes	Yes
Baseline attendance rates	Yes	Yes	Yes
Grade level fixed effects	Yes	Yes	Yes

Note: Study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. Student demographics include grade level, race/ethnicity, low-income status, gender, special education status, English language learner status, school locale, and whether the student changed schools within the school year (i.e., school mobility). Baseline attendance includes student attendance rates in 2017-18, 2018-19, and 2019-20. Standard errors clustered at the district level. $^{+}p<0.10$, $*p<0.05$, $**p<0.01$, $***p<0.001$

Table 4

Modality-Year-Subgroup Interaction Coefficients from Event Study Difference-in-Difference Estimate of the Effect of District Remote Learning Duration on Post-Pandemic Attendance by Race/Ethnicity and Economic Disadvantage

	Black	Hispanic	Asian	Other Race	Economically Disadvantaged
2018-19	0.06	-0.01	-0.04	-0.01	-0.01
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
2019-20	0.06	-0.09	-0.04	-0.07	0.02
	(0.06)	(0.08)	(0.05)	(0.06)	(0.05)
2021-22	-0.35**	-0.32^{+}	-0.30**	-0.35*	-0.30**
	(0.13)	(0.19)	(0.10)	(0.18)	(0.10)
2022-23	-0.08	-0.19	-0.18^{+}	-0.26*	-0.13
	(0.10)	(0.12)	(0.09)	(0.11)	(0.11)
2023-24	0.00	-0.09	-0.08	-0.23**	-0.07
	(0.11)	(0.08)	(0.15)	(0.08)	(0.10)
N observations	5,177,053				5,177,053
N students	992,582				992,582
R^2	0.36				0.36

Note: Study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. Estimates include student demographic controls, baseline attendance rates, grade level fixed effects, and school year fixed effects. Student demographics include grade level, race/ethnicity, low-income status, gender, special education status, English language learner status, school locale, and whether the student changed schools within the school year (i.e., school mobility). Baseline attendance includes student attendance rates in 2017-18, 2018-19, and 2019-20. Reference category for months is 0 months. Reference category for school year is 2017-18. Reference group for race/ethnicity estimate is "White" and reference group for economic disadvantage estimate is "Not Economically Disadvantaged." Standard errors clustered at the district level. $^{+}p<0.10$, $*p<0.05$, $**p<0.01$, $***p<0.001$

Table 5

Ordinary Least Squares (OLS) and 2-Stage Least Squares (2SLS) Estimates of the Effect of District Remote Learning Duration on Post-Pandemic Attendance

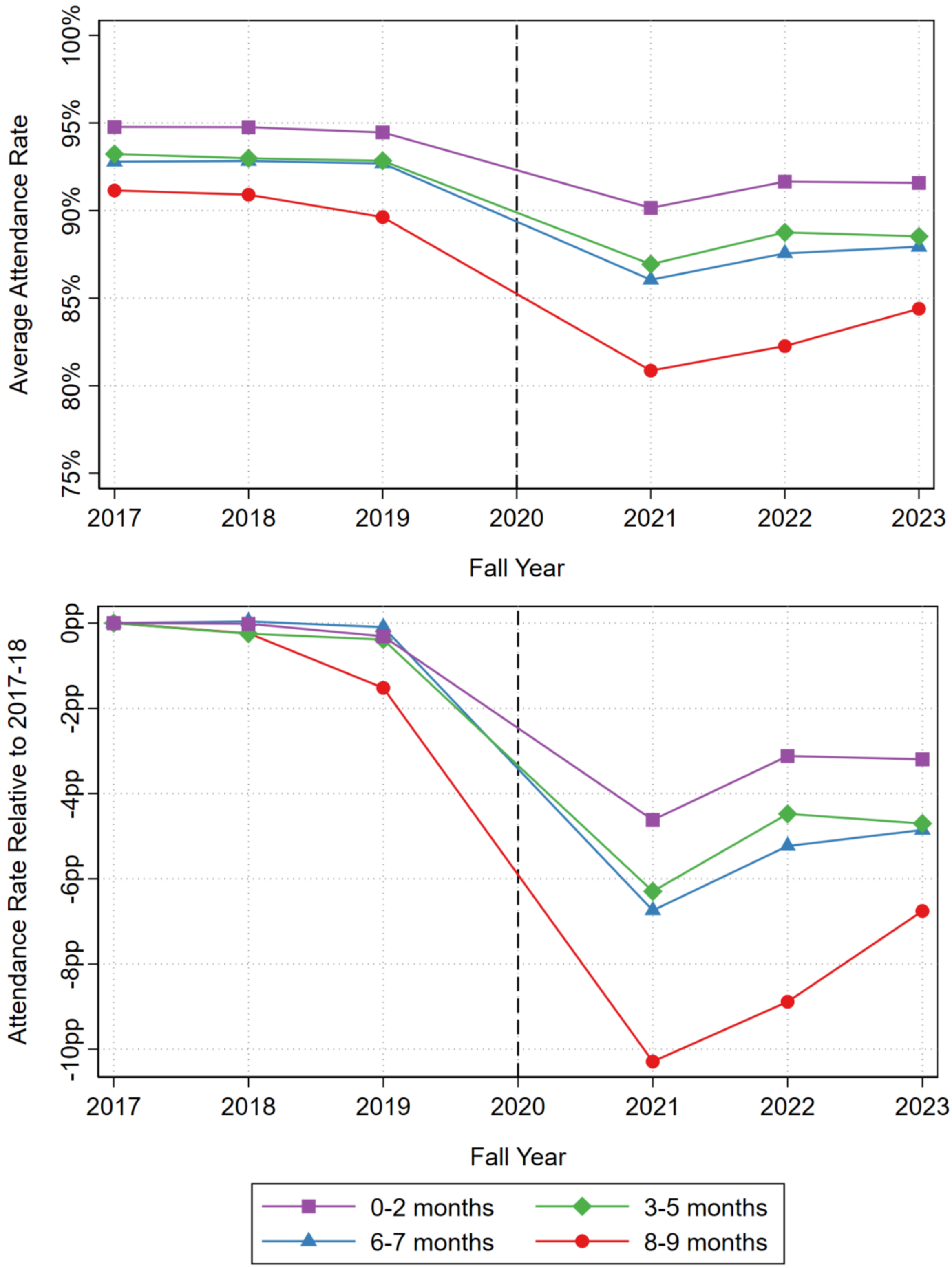
	OLS	2SLS
Remote months (continuous)	-0.25***	-0.77***
	(0.08)	(0.14)
First-stage instrument		
Pct. Trump in 2020	-	-3.19***
		(0.76)
District Pct. White in 2020-21	-	-2.05***
		(0.58)
District Pct. Economically Disadvantaged in 2020-21	-	1.25*
		(0.57)
COVID-19 Countywide Case Rate Prior to 2020-21	-	-0.05
		(0.05)
COVID-19 Countywide Case Rate During 2020-21		-0.06*
		(0.02)
F-statistic for IV	-	29.91***
Partial R^2 for IV	-	0.19
R^2 for first-stage estimates	-	0.42
Student demographic controls	Yes	Yes
Baseline attendance rates	Yes	Yes
Grade level fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
N observations	2,576,645	2,576,645
N students	989,724	989,724
R^2	0.23	0.23

Note: Study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. OLS and 2SLS estimates include post-treatment observations only (i.e., after 2020-21). Student demographics include grade level, race/ethnicity, low-income status, gender, special education status, English language learner status, school locale, and whether the student changed schools within the school year (i.e., school mobility). Baseline attendance includes student attendance rates in 2017-18, 2018-19, and 2019-20. Pct. Trump is measured at the district level based on an aggregate of block-level 2020 voting data. COVID-19 case rate is measured per 100,000 residents. Standard errors clustered at the district level. $^{+}p<0.10$, *$*p<0.05$, $**p<0.01$, $***p<0.001$*

Figures

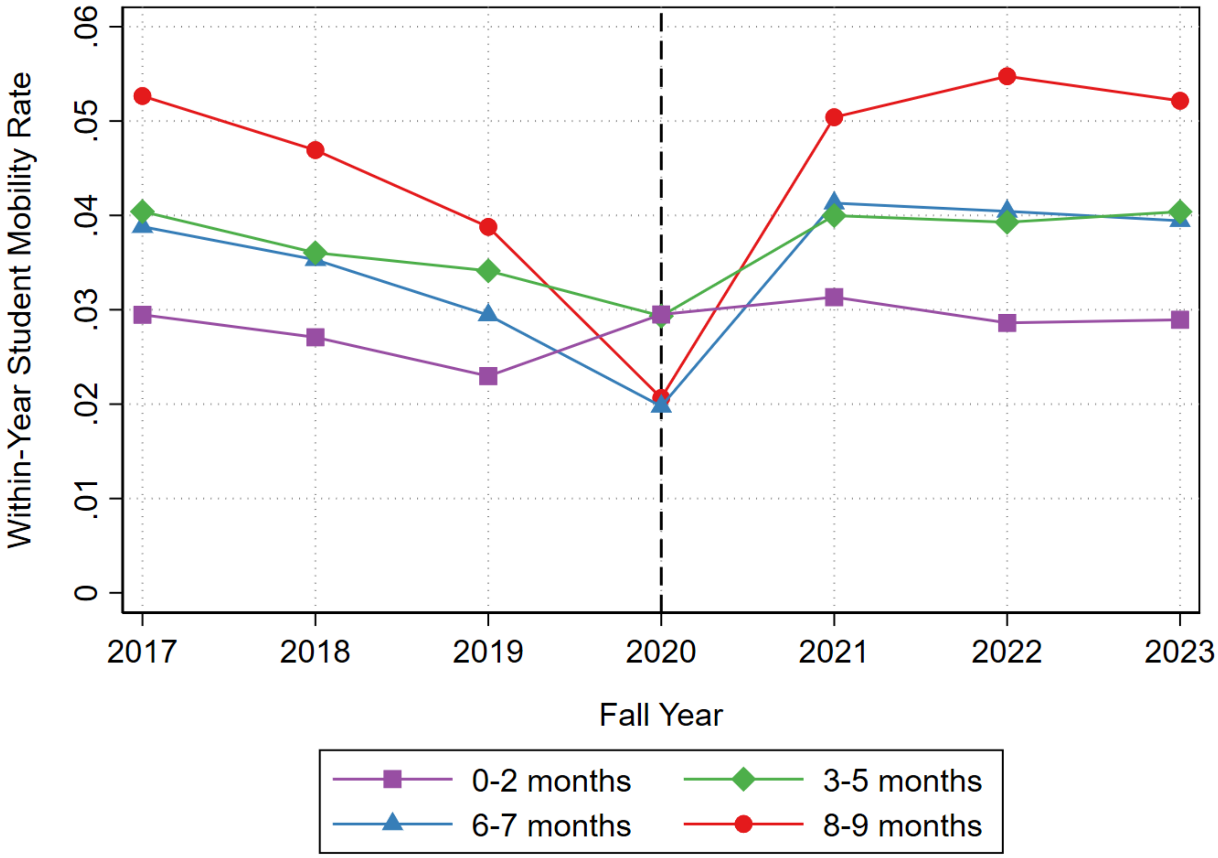
Figure 1

Post-Pandemic Attendance by District Remote Learning Duration in 2020-21



Appendix A

Within-Year Student Mobility by Remote Learning Duration Over Time



Appendix B

Difference-in-Difference Estimates of the Effect of District Remote Learning Duration on Post-Pandemic Attendance With and Without Student Fixed Effects

	Student-Level Controls	Student Fixed Effects
Remote months (continuous) x Post-COVID	-0.46***	-0.48***
	(0.07)	(0.08)
N observations	5,177,053	5,176,908
N students	992,582	992,437
R^2	0.35	0.42
Student demographic controls	Yes	No
Baseline attendance rates	Yes	No
Grade level fixed effects	Yes	Yes
Student fixed effects	No	Yes

Note: Study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. Student demographics include grade level, race/ethnicity, low-income status, gender, special education status, English language learner status, school locale, and whether the student changed schools within the school year (i.e., school mobility). Baseline attendance includes student attendance rates in 2017-18, 2018-19, and 2019-20. Standard errors clustered at the district level. $^{+}p<0.10$, $*p<0.05$, $**p<0.01$, $***p<0.001$

Appendix C

Difference-in-Difference Estimates of the Effect of District Remote Learning Duration on Post-Pandemic Attendance With and Without Instrumental Variables as Covariates

	Without IVs	With IVs
Remote months (continuous) x Post-COVID	-0.46*** (0.07)	-0.46*** (0.07)
Pct. Trump in 2020	-	1.46* (0.62)
District Pct. White in 2020-21	-	0.55 (0.43) (0.46)
District Pct. Economically Disadvantaged in 2020-21	-	-0.79* (0.31)
COVID-19 Countywide Case Rate Prior to 2020-21	-	0.05^{+} (0.03)
COVID-19 Countywide Case Rate During 2020-21	-	0.00 (0.01)
N observations	5,177,053	5,164,604
N students	992,582	991,544
R^2	0.35	0.35
Student demographic controls	Yes	Yes
Baseline attendance rates	Yes	Yes
Grade level fixed effects	Yes	Yes

Note: Study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. Student demographics include grade level, race/ethnicity, low-income status, gender, special education status, English language learner status, school locale, and whether the student changed schools within the school year (i.e., school mobility). Baseline attendance includes student attendance rates in 2017-18, 2018-19, and 2019-20. Pct. Trump is measured at the district level based on an aggregate of block-level 2020 voting data. COVID-19 case rate is measured per 100,000 residents. Standard errors clustered at the district level. $^{+}p<0.10$, $*p<0.05$, $**p<0.01$, $***p<0.001$

Appendix D

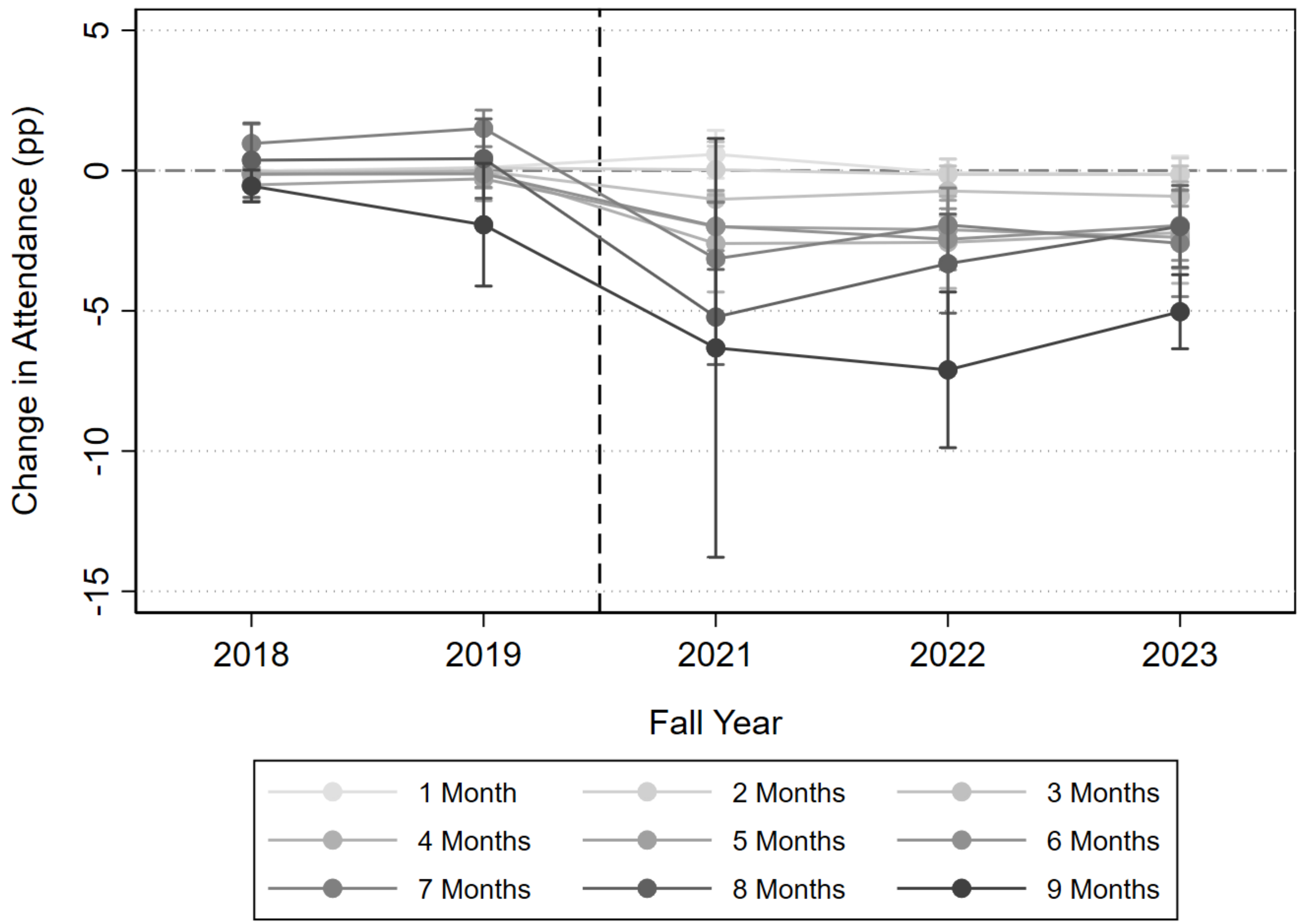
Modality-Year Interaction Coefficients from Event Study Difference-in-Difference Estimate of the Effect of District Remote Learning Duration (Categorical Measure) on Post-Pandemic Attendance

	1 month	2 months	3 months	4 months	5 months	6 months	7 months	8 months	9 months
2018-19	-0.01	-0.05	-0.05	-0.14	-0.51^{+}	-0.13	0.96**	0.37	-0.55^{+}
	(0.08)	(0.08)	(0.15)	(0.23)	(0.29)	(0.13)	(0.35)	(0.68)	(0.29)
2019-20	0.10	0.09	-0.02	-0.02	-0.29	-0.12	1.50***	0.43	-1.93^{+}
	(0.13)	(0.10)	(0.21)	(0.31)	(0.40)	(0.24)	(0.33)	(0.72)	(1.11)
2021-22	0.58	0.03	-1.03	-2.60**	-2.00**	-1.98***	-3.14**	-5.22***	-6.32^{+}
	(0.43)	(0.43)	(1.05)	(0.88)	(0.66)	(0.44)	(1.02)	(0.86)	(3.80)
2022-23	-0.08	-0.14	-0.73	-2.56**	-2.11***	-2.45***	-1.94**	-3.32***	-7.11***
	(0.24)	(0.28)	(0.46)	(0.84)	(0.53)	(0.56)	(0.67)	(0.90)	(1.41)
2023-24	-0.12	-0.14	-0.92+	-2.21*	-2.38***	-1.96**	-2.59**	-1.99**	-5.03***
	(0.33)	(0.30)	(0.55)	(0.92)	(0.56)	(0.63)	(0.97)	(0.74)	(0.67)
N observations					5,177,053				
N students					992,582				
R^2					0.35				

Note: Study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. Estimates include student demographic controls, baseline attendance rates, grade level fixed effects, and school year fixed effects. Student demographics include grade level, race/ethnicity, low-income status, gender, special education status, English language learner status, school locale, and whether the student changed schools within the school year (i.e., school mobility). Baseline attendance includes student attendance rates in 2017-18, 2018-19, and 2019-20. Reference category for months is 0 months. Reference category for school year is 2017-18. Standard errors clustered at the district level. $^{+}$*p<0.10, *p<0.05, **p<0.01, ***p<0.001*

Appendix E

Modality-Year Interaction Coefficient Plot from Event Study Difference-in-Difference Estimate of the Effect of District Remote Learning Duration (Categorical Measure) on Post-Pandemic Attendance



Appendix F

Difference-in-Difference Estimates of the Effect of District Remote Learning Duration on the Probability of Chronic Absenteeism Post-Pandemic

	(1)	(2)
Remote months (continuous) x Post-COVID	0.01*** (0.00)	-
Remote months (categorical) x Post-COVID (ref. = 0 months)		
1 month	-	-0.01 (0.01)
2 months	-	0.00 (0.01)
3 months	-	0.01 (0.01)
4 months	-	0.06** (0.02)
5 months	-	0.05*** (0.01)
6 months	-	0.06*** (0.02)
7 months	-	0.08** (0.02)
8 months	-	0.09** (0.03)
9 months	-	0.10** (0.04)
N observations	5,177,053	5,177,053
N students	992,582	992,582
R^2	0.35	0.35
Student demographic controls	Yes	Yes
Baseline attendance rates	Yes	Yes
Grade level fixed effects	Yes	Yes

Note: Study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. Student demographics include grade level, race/ethnicity, low-income status, gender, special education status, English language learner status, school locale, and whether the student changed schools within the school year (i.e., school mobility). Baseline attendance includes student attendance rates in 2017-18, 2018-19, and 2019-20. Standard errors clustered at the district level. $^{+}p<0.10$, $*p<0.05$, $**p<0.01$, $***p<0.001$

Appendix G

Placebo Test for Pct. Trump as an Instrument for District Remote Learning Duration

	2018-19 Attendance	2019-20 Attendance
Pct. Trump 2020	1.08	1.12
	(0.78)	(0.69)
District Pct. White in 2020-21	0.67	0.61
	(0.51)	(0.57)
District Pct. Economically Disadvantaged in 2020-21	-1.02**	-0.96**
	(0.30)	(0.36)
COVID-19 Countywide Case Rate Prior to 2020-21	-0.02	0.04
	(0.02)	(0.03)
COVID-19 Countywide Case Rate During 2020-21	0.00	0.01
	(0.01)	(0.02)
N observations	874,381	992,662
R^2	0.31	0.31
Student demographic controls	Yes	Yes
Baseline attendance rates	Yes	Yes
Grade level fixed effects	Yes	Yes

Note: Study population includes all students in Michigan observed in 2017-18 through 2019-20 (baseline attendance years), 2020-21 (remote learning year), and 2021-22 through 2023-24. Placebo OLS estimates include observations for a single school year only. Estimates for 2018-19 include 2017-18 attendance rates as a baseline, and estimates for 2019-20 include 2017-18 and 2018-19 attendance rates as a baseline. Student demographics include grade level, race/ethnicity, low-income status, gender, special education status, English language learner status, school locale, and whether the student changed schools within the school year (i.e., school mobility). Baseline attendance includes student attendance rates in 2017-18, 2018-19, and 2019-20. Pct. Trump is measured at the district level based on an aggregate of block-level 2020 voting data. COVID-19 case rate is measured per 100,000 residents. Standard errors clustered at the district level. $^+p<0.10$, $*p<0.05$, $**p<0.01$, $***p<0.001$