The Unintended Cost of Distance Learning: An Analysis of Child Maltreatment*

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Abstract

This paper provides the first causal evidence on how the pandemic-induced remote learning disrupted schools' capacity to detect child maltreatment. Leveraging county-level variation in remote instruction during the 2020–21 school year, I find that counties with higher exposure to remote instruction experienced a 7.2% greater decline in child maltreatment reports involving school-aged children, but a 13.4% increase in maltreatment-related child fatalities within this group. The decrease in reports by education personnel primarily drove the overall decline. Effects on maltreatment-related fatalities persisted even after schools resumed in-person instruction. These results highlight an unintended cost of distance learning: remote instruction impaired the detection of child maltreatment, leading to fewer reports but more severe cases. Prompt policy interventions could safeguard children who remain undetected.

Keywords: Child Maltreatment, Maltreatment-Related Fatality, Education Personnel, Mandated Reporter, Remote Learning

JEL Codes: I28, I31, J13

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1 Introduction

Child maltreatment is associated with profound and long-lasting impacts on nearly every dimension of children's lives, with around 1 in 3 children in the United States undergoing a child protective services (CPS) investigation by the age of 18 (Kim et al., 2017).[1] Education personnel, as mandated reporters, play a crucial role in identifying and reporting child maltreatment (Benson et al., 2022). Teachers have been the most frequent report source, accounting for over 20 percent of all maltreatment reports in the United States in recent years. (U.S. Department of Health & Human Services, 2022).

The COVID-19 pandemic disrupted this reporting mechanism by impeding the daily contact between children and mandated reporters. Emerging evidence suggests that stay-at-home orders and school closures in spring 2020 led to substantial under-reporting of child maltreatment (Baron et al., 2020; Cabrera-Hernández and Padilla-Romo, 2020; Prettyman, 2024), even as hospital and emergency department visits for maltreatment-related injuries increased during he same period (Bullinger et al., 2021; Kovler et al., 2021; Rebbe et al., 2023; Sidpra et al., 2021; Cappa and Jijon, 2021).

This paper provides causal analyses of the consequences of prolonged disruptions to in-person schooling during the 2020-21 school year on child maltreatment outcomes. As districts implemented a range of instructional models (from fully remote to fully in-person) over the year, the extent of remote instruction varied substantially, as illustrated in Figure A1. While prior work focuses on the immediate aftermath of universal closures, this paper extends the analysis to assess how the uneven disruptions in the following school year affected maltreatment reports, the cases that were substantiated, and, importantly, on maltreatment-related child fatalities, used as a proxy to capture the extent of latent maltreatment risk that may have gone undetected.

[1] Studies have shown that child maltreatment is linked to elevated rates of crime and incarceration, greater substance abuse, worse educational outcomes, lower employment and income levels, and poorer behavioral and mental health outcomes (Berger et al., 2016; Slade and Wissow, 2007; Currie and Spatz Widom, 2010; Currie and Tekin, 2012; Cicchetti and Handley, 2019; Eckenrode et al., 1993; Raitasalo and Holmila, 2017).

Empirically, I exploit variation in instructional modalities across school districts during the 2020-21 school year. Using a difference-in-differences design, I compare counties with higher versus lower exposure to remote learning. Because universal school closures and stay-at-home orders had ended by the start of the 2020-21 school year, the measure of remote learning exposure in my analysis is less likely to capture effects from disconnection with other mandated reporters outside the school system. To alleviate concerns that instructional mode decisions were linked to public health thresholds, such as local COVID-19 case rates, and evolved alongside other pandemic-related stressors including heightened stress and unemployment, I incorporate a set of fixed effects and include controls for time-varying covariates. The main outcomes are drawn from the National Child Abuse and Neglect Data System (NCANDS) and the National Vital Statistics System (NVSS) (National Center for Health Statistics, 2023).

I find that counties with greater exposure to remote learning during the 2020-21 school year experienced an additional 7.2% decline in the number of reported maltreatment cases involving school-aged children (aged 5-17), while simultaneously seeing an 13.4% increase in maltreatment-related fatalities for the same population. The reduction in reports was primarily driven by those from education personnel and was not observed among young children (aged 0-4). While reported cases declined, substantiated cases (those deemed credible of maltreatment by child protective services) did not differ significantly between high-remote and low-remote counties. Moreover, high-remote counties continued to experience increased maltreatment-related fatalities even after schools reopened for in-person learning. These patterns suggest that the decline in reports did not reflect a true reduction in maltreatment, but rather a breakdown in detection. The disruption to in-person schooling severed a key connection between children and their primary mandated reporters, coinciding with a rise in severe, even fatal, cases. Together, the findings suggest that remote learning may have inadvertently exposed children to a greater risk of maltreatment at home by limiting children's contact with school-based adults positioned to detect and report signs of harm.

This paper offers a new lens on the growing literature exploring how modes of learning shape children's lives—not only in the classroom, but beyond. While early studies explored the effects of virtual instruction and sporadic school closures even before the pandemic (Bueno, 2020; Puls et al., 2021), more recent research has centered on the academic and health consequences of pandemic-era remote learning and the return to in-person instruction (Kuhfeld et al., 2022; Aucejo et al., 2020; Copeland et al., 2021). A smaller but emerging set of studies has turned to more severe outcomes. For instance, Bacher-Hicks et al. (2022) and Hansen et al. (2024) find that distance learning reduced bullying and cyberbullying and may have lowered suicide risks, suggesting that school closures offered protection from school-based harms. This study broadens the conversation by documenting a different kind of harm—one that occurs not at school, but at home. I show that remote learning coincided with increased risks of severe and even fatal maltreatment, highlighting a critical trade-off in child safety: while physical absence from school may reduce peer-related risks, it can simultaneously weaken the schools' protective role in identifying and reporting maltreatment. My findings complement and complicate prior work, underscoring the need to weigh the full spectrum of consequences, both visible and hidden, when evaluating the impacts of remote instruction.

Second, this paper deepens and broadens the literature on COVID-19 and child maltreatment in three important ways. First, by extending the analysis beyond the initial crisis period into the 2022-23 school year, I move past the immediate disruption to capture the lingering effects of remote learning on both reporting and underlying risk. While early studies documented the sharp drop in maltreatment reports during school closures (Baron et al., 2020; Cabrera-Hernández and Padilla-Romo, 2020; Cappa and Jijon, 2021), less is known about whether the impacts of disruptions persisted after schools formally reopened. I analyze data through the 2022-23 school year and find that fatality stayed up while reported cases had rebounded. Second, I go beyond reported and substantiated cases by incorporating indicators of maltreatment severity—specifically, maltreatment-related fatalities—to assess whether reductions in reported cases may have obscured more severe underlying harm. Lastly, this study expands on Wolf et al. (2024)'s examination of learning modality and reporting outcomes for children aged 0-17 in Virginia. I extend this analysis in two key ways. First, I leverage national data to assess broader patterns across diverse geographic contexts. Second, I focus specifically on school-aged children (ages 5–17), for whom changes in school modality are most salient. While effects may be detectable across all age groups, particularly when school-aged children and their younger siblings are jointly involved in a maltreatment report, a subgroup analysis confirms that the effects are concentrated among school-aged children, with no detectable impacts among younger children (ages 0–4).

Third, this study underscores the vital role that educators play in the child protection system. Prior work has consistently shown that school personnel serve as key mandated reporters of child maltreatment in pre-pandemic settings (Benson et al., 2022; Puls et al., 2021), and that the sharp decline in reports during the early pandemic was largely driven by school closures and stay-at-home orders (Baron et al., 2020; Prettyman, 2024). By exploiting variation in remote learning exposure after the initial closures, this paper provides the first causal evidence on how reduced in-person contact with educators impaired the detection of maltreatment as well as the potential consequences for child safety. The findings underscore the protective function of schools, not just as academic institutions, but as critical sites of child safety, and highlight the vulnerability of that function when educator-student ties are weakened.

2 Background

2.1 Pandemic-Induced School Closure and Return to In-Person Learning in the U.S.

The COVID-19 pandemic, caused by the SARS-CoV-2 virus, began in late 2019 in Wuhan, China and rapidly spread worldwide, leading to a global health crisis. In March 2020, as the pandemic escalated in the United States, most schools across the country suspended in-person instruction to curb the spread of the virus. The majority of schools—about 77 percent of public schools and 73 percent of private schools—shifted to remote learning in early 2020 and remained closed for the remainder of the 2019-20 school year, gradually resuming in-person learning throughout the following school year (National Center for Education Statistics, 2022). The phased return to in-person instruction varied considerably across states, counties, and school districts. Figure A1 shows the county-level distribution of the average percentage of instructional weeks conducted remotely during the 2020-21 school year, with a median (mean) of 42% (39%).[2] Most schools had returned to in-person instruction by the beginning of the 2021-22 school year, as shown in Figure A2.

2.2 Child Maltreatment Report and Fatality Trends

Child abuse and neglect reporting laws are in place across all 50 states, the District of Columbia, and the U.S. Territories, requiring certain professionals and institutions to report suspected maltreatment to a child protective services (CPS) agency. When a CPS agency receives an allegation of maltreatment, it is either screened in for a response by CPS, becoming a report, or screened out. Once screened-in, the case is assigned to CPS workers for a detailed investigation. If the investigation finds credible

[2] Geographic variation in remote instruction is presented in figures A3 and A4.

evidence that abuse or neglect has occurred, the report is classified as substantiated. Professional reporters—such as educators, healthcare providers, and law enforcement personnel—accounted for nearly two-thirds of all child maltreatment reports in 2019, while the remainder came from non-professional sources such as neighbors, relatives, or anonymous individuals (U.S. Department of Health & Human Services, 2019).

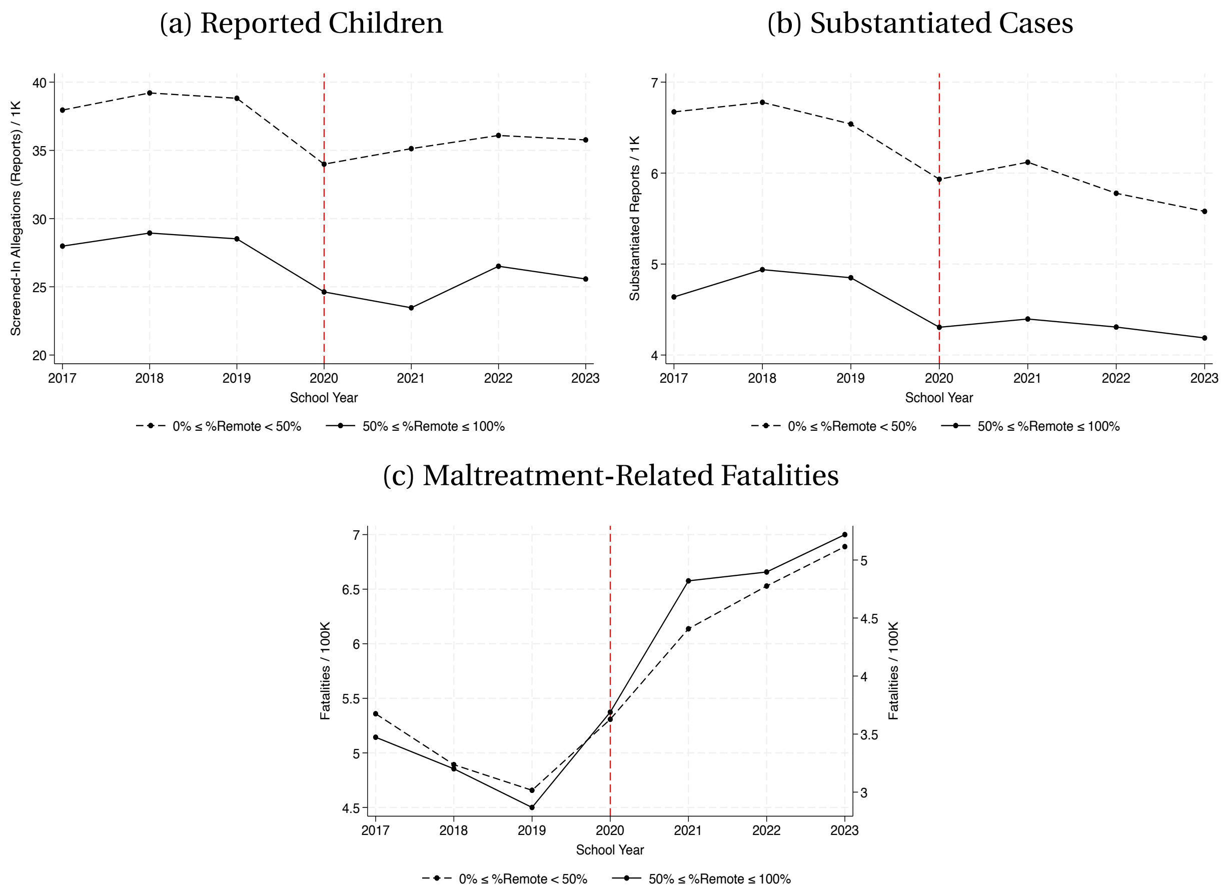
In federal fiscal year 2019, CPS agencies received 4.4 million referrals alleging maltreatment, involving approximately 7.9 million children. Of these referrals, 54.5 percent were screened in, representing a rate of 32.2 per 1,000 children in the national population (U.S. Department of Health & Human Services, 2019). Note that the number of children can differ from the number of reports, as a single report can involve one or more children. Throughout this paper, I use the number of children reported, rather than the number of reports, and refer to this as "cases" to distinguish it from report counts.

Figure 1 presents unadjusted trends in key child maltreatment outcomes from the 2016-17 through the 2022-23 school years, including the reported and substantiated cases per 1,000 school-aged children (aged 5-17), as well as maltreatment-related fatalities per 100,000 school-aged children.[3] Counties are stratified into two groups based on their average exposure to remote learning during the 2020–21 school year: solid lines represent counties where 50% or more of instruction weeks were delivered remotely, and dashed lines represent counties with less than 50% remote learning.

Across all three outcomes, the figure shows that pre-treatment trends were parallel between the two groups, suggesting no discernible pre-trends prior to the pandemic. Both groups experienced a sharp decline in reported cases and substantiation rates during the 2019-20 school year, coinciding with the nationwide school closures that began in March 2020. However, the trends diverge in the subsequent period. In counties with greater exposure to remote instruction, reported case rates continue to decline through

[3]A school year in my analysis sample covers September of the previous year to May of the current year. For instance, 2020-21 school year spans September 2020 to May 2021. Details on the sample year and how counties are classified into treatment and comparison groups are provided in section 4.

Figure 1: Trends in Child Maltreatment Outcomes, by Remote Learning Exposure



Notes: These figures show trends in child maltreatment outcomes across counties grouped by remote learning exposure during the 2020-21 school year. Panel (a) shows the number of children reported per 1,000 school-aged children (ages 5-17). Panel (b) plots the number of children whose cases were substantiated per 1,000 school-aged children. Panel (c) shows maltreatment-related child fatalities per 100,000 school-aged children. Solid black lines represent treatment counties, with an average remote learning share of 50% or higher during the 2020-21 school year, and dashed black lines represent comparison counties, with the remote share less than 50%. The vertical dashed line at 2020 marks the year prior to the emergence of variation in instructional modality in the 2020-21 school year.
Source: Author's analysis of child maltreatment report and substantiation data from the NCANDS Child File and fatality data from the NVSS for school years 2017-2023.

the 2020-21 school year, whereas counties with lower remote exposure begin to show signs of recovery. By the 2021-22 school year, when most districts had resumed in-person instruction, both groups appeared to return to an upward trajectory in report rates.

While substantiated reports are generally expected to track the pattern of overall reports, the rates in high-remote counties do not decline in proportion to the drop in reported cases. Fatality trends also diverge. While maltreatment-related fatalities in low-remote counties rise at a relatively steady pace after initial school closures, high-remote counties experience a sharper increase, eventually surpassing the trend of low-remote counties.

3 Data

3.1 Maltreatment and Fatality

The key outcome variables are drawn from multiple sources. Data on maltreatment cases and substantiation are obtained from the National Child Abuse and Neglect Data System (NCANDS). NCANDS is a federally maintained dataset that compiles annual records from child welfare agencies in all 50 states, the District of Columbia, and Puerto Rico. I use the NCANDS Child File, which provides case-level information on each report handled by CPS, including the date of report, geographic identifiers (state and county), demographic characteristics of each child involved in a report, alleged maltreatment types (e.g., physical abuse, neglect, sexual abuse), reporter type (professional or non-professional reporters), substantiation outcomes, and whether the case involved a child fatality.

For the analysis, I aggregate the case-level data to the school year-county level and calculate the counts of total reported and substantiated cases. As NCANDS masks county Federal Information Processing Series (FIPS) codes for counties with fewer than 750 cases, I follow the approach in Evans et al. (2022) and aggregate all masked counties within a state into a single hypothetical county to ensure all cases are counted. Substantiated cases are counted based on the year of initial report, not the year of CPS decision, to better capture contemporaneous maltreatment risk.

While NCANDS Child File provides data on maltreatment-related fatalities, two key challenges complicate their use for child fatality analysis: (1) geographic identifiers are masked for all cases involving child deaths, and (2) fatalities not reported to CPS are not recorded. To address these limitations, I use mortality data from the National Vital Statistics System (NVSS). NVSS contains complete death records for the U.S. population, including the date and cause of death, age at death, along with the decedent's county of residence and demographic information. Following Heath (2024), I construct county-level counts of deaths due to homicides and accidents for school-aged children (ages 5-17) and young children (ages 0-4), as these causes are more likely to reflect child maltreatment. I further identify maltreatment-related fatalities using the International Classification of Diseases (ICD-10) codes for underlying causes of death, based on the approaches used in the medical and child maltreatment literature (Schnitzer et al., 2008; Parks et al., 2012).[4] Since ICD-10-based coding alone cannot perfectly identify maltreatment-related deaths and may result in misclassification (Scott et al., 2009; Liu et al., 2023; Walter et al., 2025), I supplement the analysis with state-level counts of maltreatment-related fatalities from the U.S. Department of Health and Human Services' annual *Child Maltreatment* reports as a robustness check.

3.2 School Learning Modality

I construct measures of remote learning exposure using school district-level data on weekly instructional modalities for the 2020-21 school year, obtained from the Centers for Disease Control and Prevention's (CDC) School Learning Modalities dataset.[5] This dataset provides weekly information on the predominant mode of instruction (in-person, hybrid, or remote learning) for K-12 public and independent charter school dis-

[4] List of the ICD-10 codes used to identify maltreatment-related deaths and the corresponding analysis results are provided in Table E3.

[5] Retrieved from `https://healthdata.gov/National/School-Learning-Modalities-2020-2021/a8v3-a3m3/about_data`.

tricts from September 2020 to May 2021.[6] I restrict the analysis sample to K-12 public school districts, identified using district type indicators from the Common Core of Data (CCD) of the National Center for Education Statistics (NCES).

Since the unit of analysis is at the school year-county level, I map school districts (unit for defining remote learning exposure) to counties. However, school district boundaries do not always align with county lines. For example, a single school district may span multiple counties, and a single county may include multiple school districts. To assign districts to counties, I merge the CCD Public Elementary/Secondary School Universe Survey Data (2020-21 school year) with the HUD-USPS ZIP Code Crosswalk Files and follow several steps.[7] Further details on the construction of remote learning exposure measures are provided in section 4.

3.3 County-Level Statistics and Final Analysis Sample

To control for time-varying county characteristics, I incorporate annual labor force statistics from the Bureau of Labor Statistics (BLS), local poverty rates and median household income from the Small Area Income and Poverty Estimates (SAIPE) Program of the U.S. Census Bureau, and demographic composition of county population (share of the population by single-year age group, race/ethnicity, and sex) from the National Institutes of Health's (NIH) Surveillance, Epidemiology, and End Results Program (SEER).[8] I also use county-level weekly COVID-19 case and death data from the CDC.[9]

Observations in the final sample are at the school-year-by-county level, with an unbalanced panel of 1,085 counties across all 50 states and the D.C. (including aggregated hypothetical counties) covering the 2016-17 through 2022-23 school years. The

[6] When multiple modalities were reported within the same week, the CDC assigns the primary mode based on the modality offered for the majority of instructional days.

[7] Retrieved from https://nces.ed.gov/ccd/files.asp and https://www.huduser.gov/portal/datasets/usps_crosswalk.html.

[8] Retrieved from https://seer.cancer.gov/popdata/download.html.

[9] Retrieved from https://data.cdc.gov/dataset/Weekly-United-States-COVID-19-Cases-and-Deaths-by-/yviw-z6j5/about_data.

Table 1: Pre-School-Closure Summary Statistics

	Comparison Counties		Treatment Counties		*p*-value
	Mean	SD	Mean	SD	
Dependent Variables					
Maltreatment Report	42.30	23.09	35.35	22.73	0.000
Maltreatment Substantiation	7.19	5.30	6.42	6.09	0.000
Maltreatment-Related Fatality	4.33	5.80	3.82	4.99	0.004
Independent Variables					
Percent Remote (SY 2020-21)	0.26	0.16	0.63	0.12	0.000
Labor Force Participation	0.69	0.07	0.69	0.07	0.045
Unemployment	4.52	1.31	5.09	1.89	0.000
Median Household Income	56,320	12,705	62,115	18,424	0.000
Percent in Poverty (0-17)	18.81	6.94	18.87	7.67	0.787
Percent Asian	0.02	0.02	0.05	0.08	0.000
Percent Black	0.10	0.11	0.14	0.15	0.000
Percent Hispanic	0.10	0.11	0.14	0.15	0.000
Percent White	0.86	0.12	0.79	0.17	0.000
Percent Female	0.50	0.01	0.51	0.01	0.000
Number of Counties	699		386		

Notes: This table presents summary statistics for the analysis sample, the periods before the pandemic-induced school closures by learning mode. Treatment counties are those with average share of remote learning of 50% or higher during the 2020-21 school year, and comparison counties are those with share below 50%. Maltreatment report and substantiation is report and substantiation rates per 1,000 school-aged children (ages 5-17). Maltreatment-related fatality is rate per 100,000 school-aged children. Unemployment and Labor Force Participation rates data are sourced from the Bureau of Labor Statistics (BLS). Percent of children (0-17) in poverty (the proportion of the population living in poverty) and Median Household Income data are sourced from the Census Bureau's Small Area Income and Poverty Estimates (SAIPE). Demographics composition data are sourced from the Census Bureau. *p*-values reported in the table are from t-tests.

sample is restricted to counties with at least two years of data, including the 2020-21 school year.[10] As remote learning data are not available for the summer months (June–August 2021), I define each school year as September through May to ensure consistent comparisons across years. Summary statistics for the key outcome and control variables are presented in Table 1.

[10] Counties with complete data for all seven years account for 94% of the final sample.

4 Empirical Strategy

My preferred estimation approach is a traditional two-way fixed effects (TWFE) difference-in-differences (DID) model, comparing counties with higher versus lower exposure to remote learning based on the average share of instructional weeks delivered remotely during the 2020-21 school year. Specifically, I define treatment counties as those in which students spent 50% or more of instructional weeks in remote or hybrid mode. Given that the outcome variables—child maltreatment reports, substantiated reports, and maltreatment-related fatalities—are nonnegative counts with a nontrivial share of zeros at the county level, I estimate a Poisson pseudo-maximum likelihood (PPML) model. This model yields consistent estimates even in the presence of heteroskedasticity and many zero counts (Silva and Tenreyro, 2010; Correia et al., 2020).

I estimate the following DID specification:

$$E(y_{ct}) = \exp\left\{\beta(remote_c \times post_t) + \gamma\mathbf{X}_{ct} + \alpha_c + \delta_t + \ln(childpop_{ct})\right\} \tag{1}$$

where y_{ct} denotes the number of children reported, substantiated cases, or maltreatment-related fatalities in county c and school year t. $childpop_{ct}$ is the population of children aged 0-17 (or other age groups depending on outcomes), included in the model as an exposure with its coefficient constrained to one. $remote_c$ is an indicator equal to 1 for treatment counties, with 50% or higher share of remote instructional weeks in the 2020-21 school year, and 0 otherwise. $post_t$ is a post-treatment indicator equal to 1 for the 2020-21 school year (September 2020 to May 2021), and 0 for the pre-treatment period (school years 2016-17 through 2019-20, each defined as September through May). To mitigate concerns that instructional modality decisions were potentially correlated with county-specific characteristics and evolving local health and economic conditions, I include county and school year fixed effects (α_c and δ_t) and control for a rich set of time-varying county-level covariates in $\mathbf{X}_{ct}$. These includes unemployment and labor force participation rates, the percentage of children in poverty, median household income, COVID-19 case and death rates per 100,000 population, and demographic shares by age (18-29, 30-39, 40-49, 50-59, 60-69, 70-79, 80+), sex, and race/ethnicity (Asian, Black, Hispanic, and White). The coefficient of interest, β, captures the causal effect of exposure to remote learning. $\exp(\beta)-1$ can be interpreted as the percentage change in the outcomes in treatment counties during the 2020-21 school year relative to comparison counties. Robust standard errors are clustered at the county level in all regressions.

To construct the county-level treatment indicator ($remote_c$), I begin with weekly school district-level data on instructional modality from September 2020 through May 2021. As the data is recorded categorically (remote, hybrid, or in-person) depending on the predominant mode of instruction of each week, I assign a value r_{dw} for each school district d in week w:

$$r_{dw} = \begin{cases} 1 & \text{if remote in week } w, \\ 0.5 & \text{if hybrid in week } w, \\ 0 & \text{if in-person in week } w. \end{cases}$$

As school district boundaries do not always align perfectly with county lines, I map districts to counties using school-level enrollment data from the 2020-21 CCD Public School Universe Survey, linked with the HUD-USPS ZIP Code Crosswalk File, which connects school ZIP codes and LEA (school district) identifiers to corresponding county FIPS codes. Each school's enrollment is allocated to one or more district-county pairs based on location, resulting in district-county-level student counts.

I then merge these enrollment counts into the district-week instructional modality data and compute the county-level average share of remote learning during the 2020-21 school year. Specifically, I calculate:

$$mode_c = \frac{\sum_{\{d:\text{district } d \text{ in county } c\}} \sum_w r_{dw} \cdot s_{dc}}{\sum_{\{d:\text{district } d \text{ in county } c\}} \sum_w s_{dc}}$$

where s_{dc} denotes the number of students enrolled in district-county pair dc.[11] This yields the proportion of weeks that the students in county c spent in remote or hybrid learning, weighted by student enrollment.

Finally, I define the treatment indicator $remote_c$ as a binary variable equal to 1 if $mode_c$ is greater than or equal to 0.5. The 50% threshold reflects a majority exposure definition: counties where students experienced remote or hybrid learning for at least half of the school year are classified as treatment counties. This binary classification provides a straightforward and policy-relevant interpretation of exposure while facilitating DID estimation .[12] Although remote learning may have fluctuated month to month, Figure A5 shows that this classification remains stable over time. Throughout the 2020–21 school year, treatment counties consistently maintained higher shares of remote learning compared to comparison counties, suggesting that the groupings reflect sustained differences in learning modality exposure rather than temporary fluctuations. I also use alternative thresholds (four groups: 0-25%, 25-50%, 50-75%, 75-100%, with 0-25% as the reference group) and estimate DID models with a continuous treatment specification to verify the consistency of the results.

The robustness of the identification strategy relies on the standard DID assumptions of parallel trends and no anticipation. To assess whether these assumptions hold, I first conduct a joint hypothesis test using only pre-treatment period observations to determine whether trends in the outcomes differ systematically between treatment and comparison counties. Specifically, I estimate an event study model that replaces the

[11]While instructional modality varies weekly, enrollment counts are assumed to remain stable during the school year.

[12]Percentage thresholds, rather than percentile thresholds, have been used in prior COVID-19 learning research (see Jack et al. (2023); Sass and Goldring (2021) for example).

post-treatment indicator in Equation 1 with a set of year-specific indicators, using the first sample year (2016–17) as the reference. I then test whether the coefficients for all pre-treatment years are jointly equal to zero.

Using the same sample period, I also perform placebo tests by assigning false treatment years in 2018, 2019, and 2020 to assess whether treatment effects arise before the actual intervention. Additionally, I plot event study coefficients for the 2016-17 through 2020-21 school years, using the 2019–20 school year as the reference year, to visually assess the plausibility of the parallel trends assumption. Detailed discussion on the robustness check results are documented in section 5.

To further address potential confounding, I present unadjusted trends in all time-varying control variables, which provide evidence that underlying differences in covariates are unlikely to drive the estimated treatment effects.[13] Collectively, these tests support the credibility of the identifying assumptions and help mitigate concerns that post-treatment differences are attributable to pre-existing disparities rather than the treatment itself.

5 Results

This section summarizes and interprets the results of the analysis. I start by reporting the main estimation results for maltreatment outcomes during the 2020–21 school year. I then explore heterogeneity through subgroup analysis by report source, maltreatment type, and child demographics, offering interpretation of the observed patterns. Next, I assess whether these effects persisted in the longer term, focusing on estimates of the 2021-22 through 2022-23 school years. Finally, I report results from a series of sensitivity checks to confirm the robustness of the findings, including a supplemental analysis of child fatalities using state-level data, additional pre-trend tests, and alternative model specifications.

[13] See figures C1 through C3.

5.1 Main Analysis Results

Table 2 presents the DID estimates of remote learning on child maltreatment outcomes based on the PPML model specified in Equation 1. $\exp(\beta) - 1$ represents the relative percentage change in the outcomes in treatment counties during the 2020-21 school year, compared to comparison counties. Column (1) reports results for all children using the total child population (ages 0–17) as the exposure. Columns (2) and (3) present age-specific estimates (5–17 and 0–4) while still using the total child population as the exposure, allowing for across-age group comparisons. Columns (4) and (5) also report age-specific estimates but use the population of each respective age group as the exposure to measure within-group changes. Robust standard errors, clustered at the county level, and *p*-values from pre-trend joint hypothesis tests ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$, with the 2016-17 school year as a reference year) are reported for each regression result. In addition to the main specification with a binary treatment, I use an alternative threshold (four groups: 0–25%, 25–50%, 50–75%, 75–100%, with 0–25% as the reference) to examine heterogeneous impacts across exposure levels. The results from the four-group specification are reported in tables D1 through D3 in Appendix D.

5.1.1 Reported Children

Panel A of Table 2 presents the estimates for reported maltreatment cases. Treatment counties experienced an additional 5.7% ($\approx \exp(-0.059) - 1$) decline for children aged 0–17 relative to comparison counties. The effect is concentrated among school-aged children (5–17), who experienced a 7.2% ($\approx \exp(-0.075) - 1$) decrease, whereas no statistically significant change is observed for children aged 0–4. Within-age group analyses using age-specific exposure measures show consistent patterns, confirming that reductions in reported cases were primarily driven by school-aged children.

Table 2: Difference-in-Differences Results

		Across Age Group		Within Age Group	
	(1)	(2)	(3)	(4)	(5)
	0-17	5-17	0-4	5-17	0-4
Panel A. Reported Children					
β	-0.059***	-0.075***	-0.023	-0.075***	-0.022
	(0.016)	(0.017)	(0.015)	(0.017)	(0.014)
p-value	0.8453	0.9366	0.4003	0.9232	0.2438
Observations	5,360	5,360	5,360	5,360	5,360
Unique Counties	1,085	1,085	1,085	1,085	1,085
Panel B. Substantiated Cases					
β	0.003	-0.003	0.011	-0.004	0.013
	(0.026)	(0.027)	(0.025)	(0.027)	(0.025)
p-value	0.2191	0.1242	0.5428	0.1537	0.4319
Observations	5,360	5,360	5,360	5,360	5,360
Unique Counties	1,085	1,085	1,085	1,085	1,085
Panel C. Maltreatment-Related Fatalities					
β	0.106***	0.126**	0.050	0.126**	0.050
	(0.038)	(0.049)	(0.057)	(0.050)	(0.057)
p-value	0.0162	0.2366	0.0106	0.2399	0.0128
Observations	5,249	5,031	4,959	5,031	4,959
Unique Counties	1,060	1,015	1,000	1,015	1,000
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	0-17	0-17	0-17	5-17	0-4

Notes: This table reports DID estimates derived from Equation 1, with the number of reported children (Panel A), substantiated cases (Panel B), and maltreatment-related (Panel C) as dependent variables. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes in treatment counties during the 2020-21 school year compared with comparison counties. Column (1) presents counts for children aged 0-17, columns (2) and (4) for school-aged children (ages 5-17), and columns (3) and (5) for ages 0-4. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported p-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.1.2 Substantiated Cases

Estimates for substantiated cases (Panel B of Table 2) show small and statistically insignificant effects across all age groups, with relative changes near zero. In prior work, the substantiation rate has often been used as a proxy for maltreatment outcomes in pre-pandemic settings, when overall reporting rates were relatively stable (Paxson and Waldfogel, 2002; Lindo et al., 2018). In the context of this study, however, such an interpretation is more challenging because there was a sharp decline in total number of reported cases, and substantiated cases are a subset of those ever reported to CPS. Under the assumption that the substantiation process and underlying risk of maltreatment is comparable across treatment and comparison counties, the absence of a decline in substantiated cases can suggest two possibilities: first, that redundant or less severe cases were filtered out, or second, that there was a shift in reporting source composition that offsets the decline in reporting, with maltreatment cases identified through other channels.[14] While the characteristics of the cases driving this decrease can be examined (discussed in subsection 5.2), the true level of maltreatment risks associated with unreported cases remains unknown.

5.1.3 Maltreatment-Related Fatalities

Given the limitations of substantiated cases in capturing unobserved maltreatment risk, I examine maltreatment-related fatalities (Panel C of Table 2) as an extreme yet concrete indicator of severe maltreatment outcomes. While I do not claim that the reduction in reports directly caused the observed fatalities, it is notable that treatment counties—particularly among school-aged children (5–17)—experienced concurrent increases in maltreatment-related deaths. Relative to comparison counties, total fatalities rose by 11.2% ($\approx \exp(0.106) - 1$), with school-aged children seeing a 13.4% ($\approx$

[14] Literature has documented racial disparities in reporting patterns, including the overrepresentation of Black children and underrepresentation of White children in child welfare systems (Merritt, 2020; Harris, 2020; Palusci and Botash, 2021).

$\exp(0.126) - 1$) increase. The youngest group (0–4) exhibited a smaller, non-significant increase of 5%. Within-age group analyses using age-specific exposure measures show consistent patterns.

I also employ an alternative classification of deaths using ICD-10 codes to identify maltreatment-related fatalities, following the approaches in the medical and child maltreatment literature (Schnitzer et al., 2008; Parks et al., 2012). These deaths constitute a subset of those classified as homicides and accidents. Tables E1 and E2 report the analysis results, and Table E3 provides a complete list of ICD-10 codes used in this alternative classification. Estimates based on this alternative approach are consistent with the main findings, except for the estimates for female and Hispanic children.

Taken together, these findings suggest that the decline in reported cases should not be interpreted as evidence of lower underlying maltreatment risk. Instead, the concurrent rise in child fatalities in counties with higher remote exposure raises concern that serious maltreatment incidents may have gone unnoticed or unreported during this period.

5.2 Heterogeneity Analysis

Given that the main effects were most pronounced among school-aged children(5-17), the heterogeneity analyses in this section focus on this age group. Fatality results are reported only by child demographics, as the corresponding report source or maltreatment type cannot be identified.

5.2.1 Report Source

Table 3 presents DID estimates of changes in maltreatment cases among school-aged children by source of report, each column representing estimates by each reporter group. As before, $\exp(\beta) - 1$ represents the relative percentage change in the number of children reported (Panel A) and substantiated cases (Panel B) in treatment coun-

Table 3: DID Results by Report Source, Ages 5-17

	(1) Educ.	(2) Legal	(3) Social	(4) Medical	(5) Other Pro.	(6) Non-Pro
Panel A. Reported Children						
β	-0.283***	0.003	0.024	-0.046	-0.079***	0.049**
	(0.039)	(0.018)	(0.032)	(0.028)	(0.028)	(0.021)
p-value	0.8564	0.0299	0.0685	0.3280	0.2006	0.0236
Observations	5,360	5,360	5,360	5,360	5,360	5,292
Unique Counties	1,085	1,085	1,085	1,085	1,085	1,071
Panel B. Substantiated Cases						
β	-0.247***	0.031	0.047	0.003	-0.042	0.075**
	(0.051)	(0.029)	(0.044)	(0.037)	(0.044)	(0.034)
p-value	0.2245	0.3331	0.0227	0.7985	0.1607	0.1275
Observations	5,360	5,360	5,337	5,360	5,277	5,277
Unique Counties	1,085	1,085	1,080	1,085	1,067	1,068
α_c	Y	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	5-17	5-17	5-17	5-17	5-17	5-17

Notes: This table reports DID estimates from Equation 1 by source of reported cases, with the number of children reported (Panel A) and substantiated cases (Panel B) among school-aged children (ages 5-17) as dependent variables. Columns (1) through (4) report estimates for cases reported by professional reporters (mandated reporters), including education personnel, legal and law enforcement personnel, social services personnel, and medical personnel. Columns (5) and (6) report estimates for cases from other professional reporters and non-professional reporters, respectively. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes in treatment counties during the 2020-21 school year compared with comparison counties.Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population (ages 5-17) as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported p-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ties during the 2020–21 school year, compared to comparison counties. Columns (1) through (4) report results for professional reporters, including education personnel, legal and law enforcement personnel, social services personnel, and medical personnel. Columns (5) and (6) present estimates for other professionals (e.g. mental health personnel, child daycare providers, foster care providers) and non-professional reporters (e.g. parents, friends, relatives, neighbors), respectively.

The largest and most statistically significant decline is observed among education personnel, with a 24.6% decrease in reported children and a 21.9% decrease in substantiated cases proportionally with reported cases decrease compared to the comparison counties. Cases reported by legal, social services, and medical personnel show no statistically significant differences. Cases reported by other professionals also declined modestly, though no statistically significant effects in substantiated cases. In contrast, reported cases (substantiations) from non-professional reporters increased by 5% (7.8%). However, the reported case estimate warrants caution, as the joint pre-trend test does not support the parallel trends assumption (p = 0.024). Nonetheless, the decline in reported and substantiated cases from education personnel and other professionals, alongside the increase in substantiated cases from non-professional reporters, suggests a potential shift in the composition of reporters in treatment counties. This underscores the role that alternative reporters may have played in maintaining contact with children during school disruptions. As students in remote counties were less connected to educators, maltreatment incidents may have come to light through these alternative channels, consistent with the findings.

5.2.2 Maltreatment Type

DID estimates disaggregated by type of maltreatment are reported in Table 4. Columns (1) through (4) present estimates for cases involving physical abuse, neglect (including medical neglect), sexual abuse, and emotional or psychological maltreatment, respectively. These categories are not mutually exclusive; a single case may involve multiple types of maltreatment.

The most pronounced declines are observed in reported cases of physical abuse and neglect, which fell by approximately 11% and 4%. No significant changes are detected for other maltreatment types, and there are no statistically significant effects on substantiated cases across any category. These patterns are consistent with earlier find-

Table 4: DID Results by Maltreatment Type, Ages 5-17

	(1) Physical	(2) Neglect	(3) Sexual	(4) Emotional
Panel A. Reported Children				
β	-0.113***	-0.039*	-0.021	0.004
	(0.024)	(0.022)	(0.021)	(0.029)
p-value	0.7265	0.0096	0.7237	0.0001
Observations	5,360	5,360	5,360	4,814
Unique Counties	1,085	1,085	1,085	975
Panel B. Substantiated Cases				
β	-0.014	-0.008	0.012	0.023
	(0.037)	(0.031)	(0.030)	(0.042)
p-value	0.4028	0.0097	0.2504	0.1157
Observations	5,352	5,360	5,360	4,663
Unique Counties	1,083	1,085	1,085	943
α_c	Y	Y	Y	Y
δ_t	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y
exposure($childpop_{ct}$)	5-17	5-17	5-17	5-17

Notes: This table reports DID estimates from Equation 1 by maltreatment type, with the number of children reported (Panel A) and substantiated cases (Panel B) among school-aged children (ages 5-17) as dependent variables. Columns (1) through (4) present estimates for cases involving physical abuse, neglect (including medical neglect), sexual abuse, and psychological or emotional maltreatment, respectively. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes in treatment counties during the 2020-21 school year compared with comparison counties.Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population (ages 5-17) as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported p-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ings that cases reported by education personnel declined most sharply: physical abuse and neglect are the most frequently reported forms of maltreatment and are often identified through in-person contact. As remote learning limited children's interaction with education personnel, these types of maltreatment were likely less visible and thus less frequently reported.

5.2.3 Child Demographics

Table 5: DID Results by Demographics, Ages 5-17

	(1) Female	(2) Male	(3) Asian	(4) Black	(5) Hispanic	(6) White
Panel A. Reported Children						
β	-0.075***	-0.078***	-0.132***	-0.087***	-0.126***	-0.040**
	(0.017)	(0.017)	(0.043)	(0.021)	(0.023)	(0.020)
p-value	0.6805	0.9889	0.1648	0.9303	0.6801	0.0695
Observations	5,360	5,360	4,924	5,356	5,342	5,360
Unique Counties	1,085	1,085	994	1,084	1,081	1,085
Panel B. Substantiated Cases						
β	-0.008	-0.001	-0.080	0.024	-0.044	-0.003
	(0.028)	(0.027)	(0.070)	(0.033)	(0.034)	(0.027)
p-value	0.2614	0.1020	0.1215	0.0227	0.8918	0.0445
Observations	5,360	5,360	3,983	5,344	5,316	5,360
Unique Counties	1,085	1,085	802	1,081	1,075	1,085
Panel C. Maltreatment-Related Fatalities						
β	0.177**	0.103*	-0.192	0.121	0.178*	0.105*
	(0.088)	(0.056)	(0.354)	(0.077)	(0.093)	(0.062)
p-value	0.5926	0.1555	0.9610	0.3240	0.3699	0.4189
Observations	4,235	4,759	853	2,718	2,540	4,911
Unique Counties	853	958	171	545	510	990
α_c	Y	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y	Y
exposure$(childpop_{ct})$	5-17	5-17	5-17	5-17	5-17	5-17

Notes: This table reports DID estimates from Equation 1 by child demographics, with the number of children reported (Panel A) and substantiated cases (Panel B) among school-aged children (ages 5-17) as dependent variables. Columns (1) through (6) present estimates for cases involving female, male, Asian, Black, Hispanic, and White children, respectively. $\exp(\beta)-1$ can be interpreted as the relative percentage change in the outcomes in treatment counties during the 2020-21 school year compared with comparison counties. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population (ages 5-17) of each demographic group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported p-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5 presents DID estimation results, disaggregated by demographic characteristics of reported children. Significant declines in reported cases are observed across all demographic groups, as reported in Panel A, with the largest decreases among Asian (–12.4%) and Hispanic children (–11.8%), followed by Black (–8.3%) and White children (–3.9%). Both boys and girls experienced similar reductions in reports (–7.5% and –7.2%, respectively). Again, these decreases did not extend to substantiated cases.

Estimates reported in Panel C show that maltreatment-related fatalities increased for all groups except Asian and Black children. While reports involving Asian and Black children fell significantly—more so than some other groups—there was no corresponding rise in fatalities. This may indicate that the reduction in reports for these groups disproportionately filtered out less severe or non-substantiable cases. For Black children, this aligns with concerns about historical overrepresentation in the CPS system. In contrast, both Hispanic and White children—groups often considered underrepresented in child welfare involvement—experienced significant declines in reports alongside increases in fatalities (Palusci and Botash, 2021). This pattern suggests that serious maltreatment among these children may have gone undetected during the remote learning period.

5.3 2022-2023 School Years

To assess whether its effects persisted after schools resumed in-person instruction, I estimate the following model:

$$E(y_{ct}) = \exp\left\{\beta_s(remote_c \times short_t) + \beta_l(remote_c \times long_t) + \gamma \mathbf{X}_{ct} + \alpha_c + \delta_t + \ln(childpop_{ct})\right\} \quad (2)$$

where $short_t$ equals 1 for the 2020–21 school year and $long_t$ equals 1 for the 2021–22 and 2022–23 school years. All other terms are defined as in Equation 1. The coefficients of interest, β_s and β_l, estimate the causal effects of exposure to remote learning during the period of variation in instructional modality (2020–21 school year) and after schools returned to fully in-person instruction (2021–22 and 2022–23 school years), respectively.

The analysis focuses on reported and substantiated cases (overall and those by education personnel) as well as maltreatment-related fatalities among school-aged children.

Table F1 presents the DID estimates from this model. Most effects of remote learning exposure did not persist beyond the first year, except for the fatality, as all districts returned to in-person instruction. In particular, the sharp drop in education personnel reports—the largest decline observed during remote learning—fully disappeared once in-person instruction resumed in the following school year. These estimates are consistent with the temporary disconnection between students and educators during remote instruction, which was largely resolved as schools returned to in-person operations.

In contrast, the increase in maltreatment-related fatalities persists to some extent, pointing to possible longer-lasting consequences of earlier disruptions. The persistence of elevated fatalities beyond the 2020-21 school year may reflect cumulative harm from undetected maltreatment during the period of remote learning, as well as delayed system responses. Unlike reports or substantiations—which rely on external detection—fatal outcomes may continue to rise when severe cases remain unaddressed over time. Moreover, the post-pandemic surge in public school exit and chronic absenteeism may have further weakened the protective role of schools, limiting early intervention opportunities even after in-person instruction resumed (Chatterji and Li, 2021; Bacher-Hicks et al., 2024; Dougherty et al., 2025). Together, these dynamics suggest that disruptions to school-based surveillance may have had enduring implications for children facing persistent household risks, such as caregiver stress or economic instability.

5.4 Sensitivity Checks

This section documents a series of sensitivity checks conducted to assess the robustness of the main findings. Beyond the previously reported trends in outcome and control variables and the pre-trend joint tests, the following analyses offer further validation of the results.

5.4.1 State-Level Analysis of Maltreatment-Related Fatalities

As ICD-10-based coding alone cannot perfectly identify maltreatment-related deaths and may result in misclassification (Scott et al., 2009; Liu et al., 2023; Walter et al., 2025), I supplement the main analysis of maltreatment-related fatalities with state-level counts of maltreatment-related fatalities from the U.S. Department of Health and Human Services' annual *Child Maltreatment* reports.

The annual reports provide official counts of maltreatment-related child fatalities that combine those recorded in NCANDS Child File with additional maltreatment-related deaths reported in the state-level NCANDS Agency File. The Agency File gather information on deaths related to maltreatment from agencies external to CPS, including vital statistics departments, child death review teams, law enforcement agencies, and offices of medical examiners or coroners (U.S. Department of Health & Human Services, 2022). Two contextual notes are worth highlighting: (1) the unit of analysis is state by federal fiscal year because the NCANDS data are collected on a federal fiscal year basis; and (2) the fatality counts are aggregated for all children aged 0–17, making it impossible to isolate deaths among school-aged children. To address the latter, I present a supplemental death trend by age group using the Child File (Figure G4), which suggests that the observed patterns are likely driven by school-aged children.

The estimation is derived using the same specification as in Equation 2, but at the state level. Results from this supplemental analysis are reported in Tables G1 and G2 in Appendix G, showing robust and consistent findings, with effect sizes even larger than those in the county-level analysis.

5.4.2 Additional Pre-Trend Tests

To further evaluate the plausibility of the parallel trends assumption and the robustness of the findings, I conduct additional pre-trend tests for reported cases and maltreatment-related fatalities among school-aged children, which directly capture the mechanism of interest: the role of school personnel in reporting child maltreatment and the potential risks associated with reduced contact. Specifically, I use the DID specification of Equation 1, replacing the post-treatment indicator with year-specific indicators. First, I plot the event study regression coefficients over the 2016-17 to 2020-21 school years, using 2019-20 as the reference year, to visually assess pre-treatment trends. Second, using only pre-treatment years, I run placebo tests by assigning false treatment to years 2018, 2019, and 2020 to examine whether any treatment effects appear prior to the actual intervention.

Figure H1 plots the event study estimates, with the 2019–20 school year as the baseline year. The pre-treatment coefficients are generally flat and centered around zero. An exception appears in fatalities due to homicides and accidents (Panel C), which show a dip in 2019; however, this deviation appears to be a one-off fluctuation rather than part of a broader trend. A pronounced increase emerges in 2020–21, aligning with the onset of remote learning variation. ICD-10-coded fatalities (Panel D) show no evidence of pre-treatment trends.

The placebo test results are presented in Table H1. I find no statistically significant effects in reported cases or fatalities in 2018 or 2019. A modest increase in fatalities in 2020 is observed; this may partly reflect early disruptions during the spring of that year, when approximately 80% of public schools shifted to full remote instruction beginning in March 2020. Given potential differences in early remote learning uptake between treatment and comparison counties during this initial period, some divergence in 2020 is not unexpected. However, no anticipatory effects are observed in earlier years.

5.4.3 Alternative Specifications

Finally, I test the stability of the main findings under alternative model specifications. These include: (1) using analysis sample excluding aggregated hypothetical counties within each state; (2) analyses restricted to counties with complete data across all seven school years in the sample; and (3) DID specifications using a continuous measure of remote learning exposure. All results are reported in Tables I1 through I3. The estimates remain stable across all models and directionally consistent with the primary results.

6 Conclusion

This study provides causal evidence on how the COVID-19-induced shift to remote learning affected child maltreatment outcomes. I find that reported maltreatment cases, particularly those filed by educators, declined substantially during the 2020–21 school year in counties with higher remote learning exposure. While reported cases fell, I do not find a corresponding decline in substantiated cases. At the same time, maltreatment-related fatalities rose and remained elevated beyond the initial disruption. These patterns suggest that the decline in reported cases likely reflects reduced detection, not a true decrease in maltreatment. When schools—central hubs for mandated reporting—are not operating in person, serious cases may go unnoticed, with the most tragic consequence being an increase in child deaths.

While it is possible that the observed decline in maltreatment reports partially reflects a reduction in marginal or redundant cases, particularly among racial groups historically overrepresented in the CPS system such as Black children, this is unlikely to fully explain the pattern. As shown in the first figure in Figure 1, reporting in treatment counties rebounded after schools resumed in-person instruction during the 2021–22 school year, indicating that the temporary decline was more likely due to disrupted detection by education personnel. Importantly, the sustained rise in maltreatment-related fatalities beyond the remote learning period raises further concerns about systemic vulnerabilities. Post-pandemic increases in school disengagement, including chronic absenteeism and public school exit, may have further weakened children's contact with mandated reporters, warranting future research on the long-term consequences for child safety.

These findings carry important policy implications. They reaffirm the critical role of schools and educators in the child protection system. Yet they also highlight vulnerabilities in relying on a single reporting channel. Disruptions to in-person education are not unique to the pandemic; school closures routinely occur during summer and winter breaks (Benson et al., 2022; Puls et al., 2021), and climate change is projected to increase the frequency of closures due to extreme weather events (World Bank Group, 2024). To strengthen the resilience of reporting systems, policymakers should invest in more diversified and adaptive detection mechanisms. For instance, maintaining structured communication between children, parents, and educators during remote learning—such as virtual wellness check-ins—can help maintain visibility into children's safety. In parallel, improving coordination between educators and other mandated reporters, including health professionals and community organizations, can help ensure continuity in identifying and responding to maltreatment even under disrupted conditions.

Disclaimer

The analyses presented in this paper were based on data from the National Child Abuse and Neglect Data System (NCANDS) Child File. These data were provided by the National Data Archive on Child Abuse and Neglect (NDACAN), and have been used with permission. The data were originally collected under the auspices of Duke University. The collector of the original data, the funder, NDACAN, Duke University, Cornell University, and the agents or employees of these institutions bear no responsibility for the analyses or interpretations presented here. The information and opinions expressed reflect solely the opinions of the authors. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

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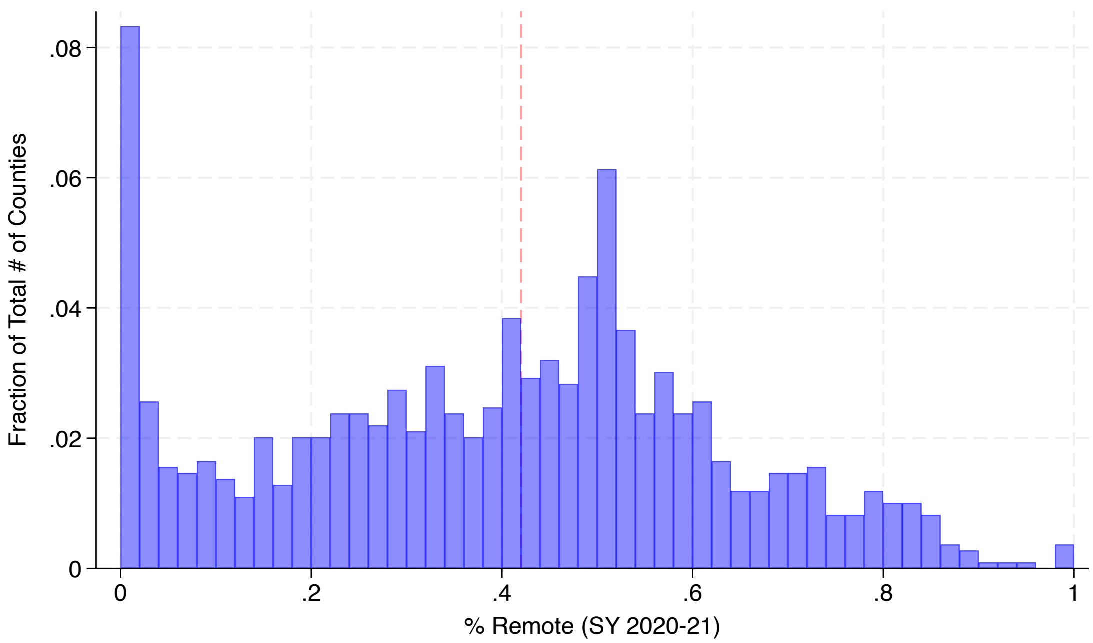
Appendix

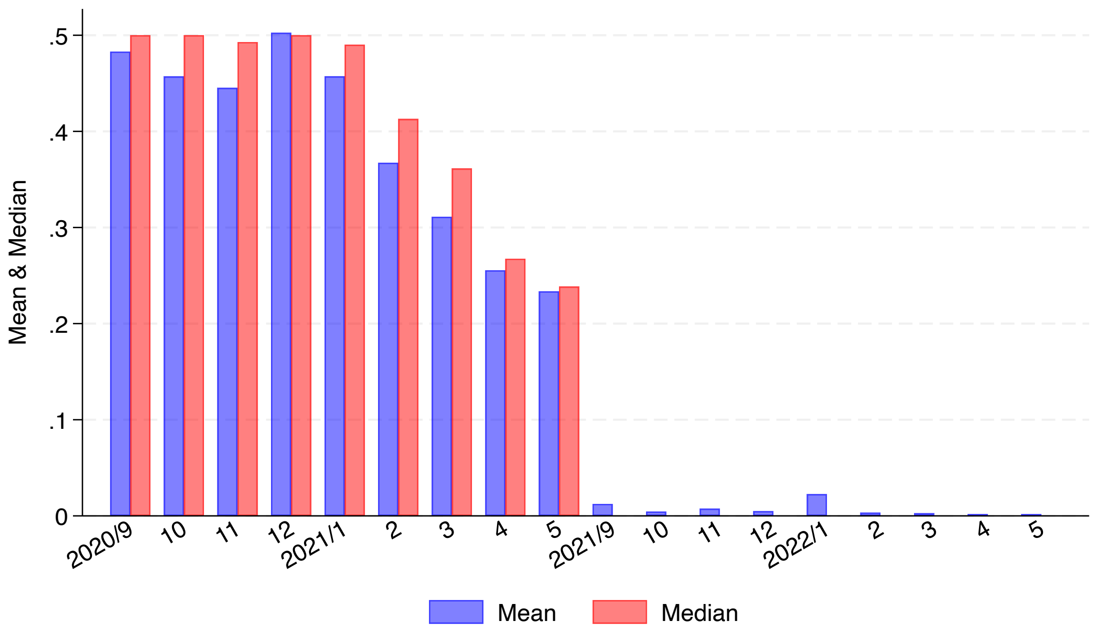
A. Exposure to Remote Learning

Figure A1: Distribution of Average % Remote Learning, SY 2020-21



Notes: This figure displays the distribution of U.S. counties in the analysis sample by the share of weeks in which instruction was delivered remotely during the 2020-21 school year. The dashed red line marks the median proportion of remote learning throughout the school year (0.42; mean = 0.39, standard deviation = 0.23).
Source: Author's analysis of CDC School Learning Modalities (2020-21 school year) data.

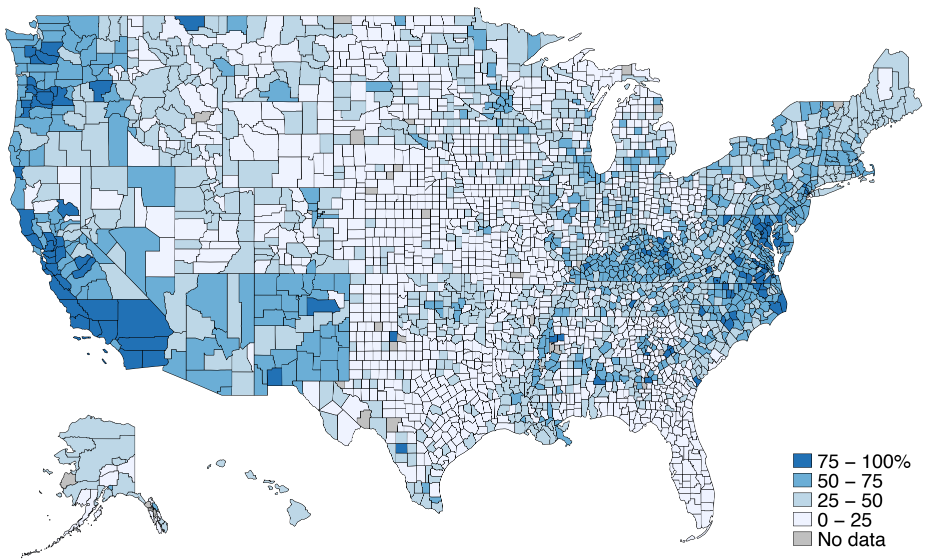
Figure A2: Mean and Median Shares of Remote Learning by Month



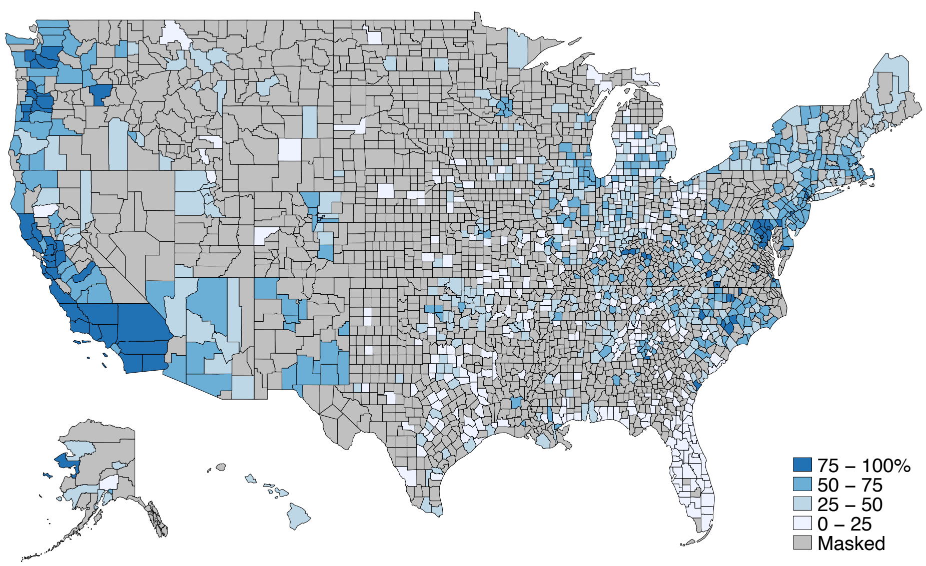
Notes: This figure shows the mean and median county-level share of weeks spent in remote instruction from September 2020 to May 2022. Summer months (June–August) are excluded due to the absence of data. Blue bars represent the mean, and red bars represent the median.
Source: Author's analysis of CDC School Learning Modalities (2020-21 and 2021-22 school years) data.

Figure A3: County Map of Remote Learning Percentage, 2020-21 school year

(a) All Counties

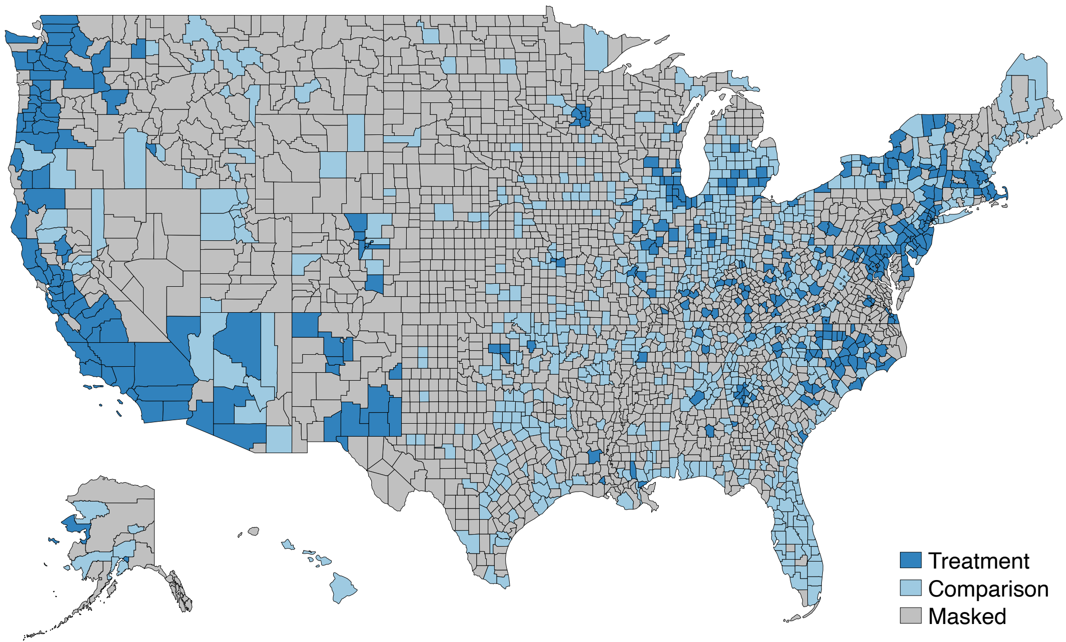


(b) Counties with NCANDS Identified



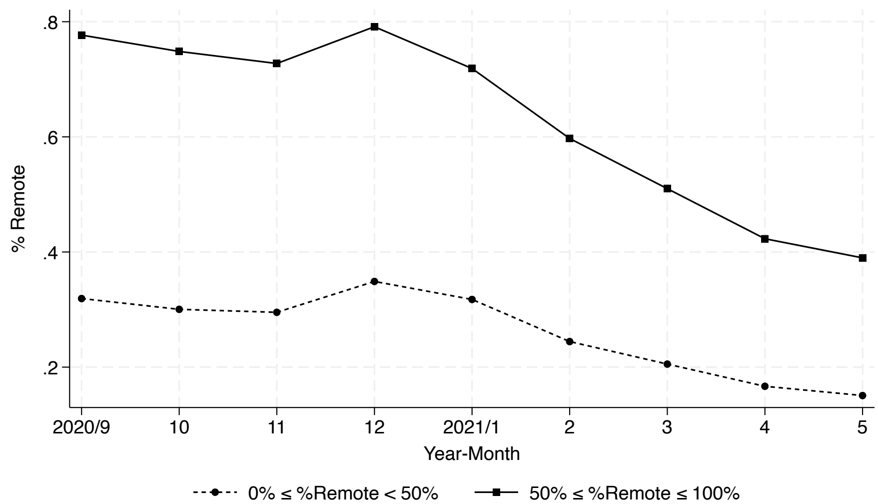
Notes: These maps illustrate the percentage of remote learning instruction during the 2020-21 school year at the county level. Counties are color-coded based on their remote share. Panel (a) includes all counties, while panel (b) only includes those with unmasked county FIPS codes in the NCANDS data. Counties shaded in grey indicate those with masked county FIPS codes. I follow the approach of Evans et al. (2022), constructing an aggregated hypothetical county for each state for the analysis.
Source: Author's analysis of CDC School Learning Modalities (2020-21 school year) and final analysis sample.

Figure A4: County Map by Treatment Group, 2020-21 school year



Notes: This map illustrates the geographical distribution of treatment and comparison counties during the SY 2020-21. Counties are color-coded based on their treatment status. Counties shaded in grey indicate those with masked county FIPS codes. I follow the approach of Evans et al. (2022), constructing an aggregated hypothetical county for each state for the analysis.
Source: Author's analysis of CDC School Learning Modalities (2020-21 school year) and final analysis sample.

Figure A5: Monthly Trends in Remote Learning Share by Treatment Group

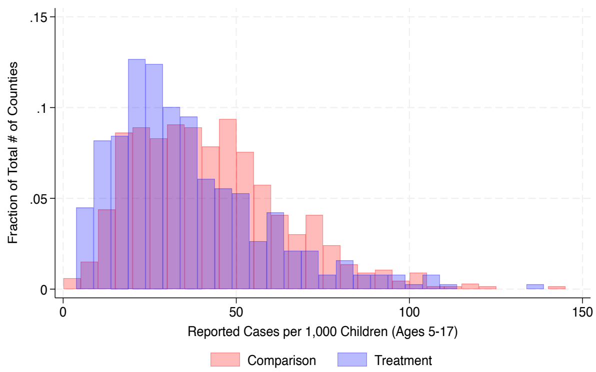


Notes: This figure shows monthly trends in the average percentage of instructional weeks delivered remotely. Solid black lines represent treatment counties, with an average remote learning share of 50% or higher during the 2020-21 school year, and dashed black lines represent comparison counties, with the remote share less than 50%.
Source: Author's analysis of CDC School Learning Modalities (2020-21 school year) and final analysis sample.

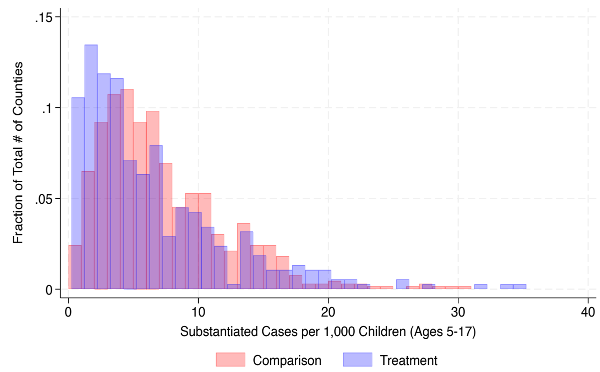
B. Distribution of Outcome Variables

Figure B1: Distribution of Child Maltreatment Outcomes, 2017-2020 school years

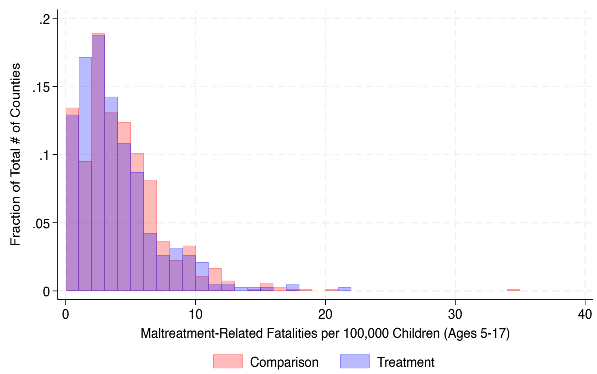
(a) Reported Case Rate



(b) Substantiated Case Rate



(c) Maltreatment-Related Fatality Rate

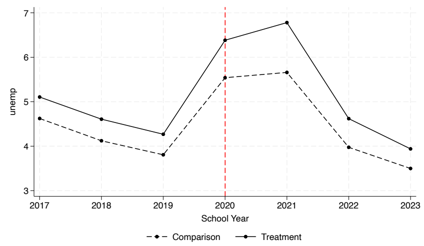


Notes: These figures show pre-treatment distributions of child maltreatment outcomes across counties grouped by remote learning exposure. Panel (a) shows the number of children reported per 1,000 school-aged children (ages 5-17). Panel (b) plots the number of children whose cases were substantiated per 1,000 school-aged children. Panel (c) shows maltreatment-related child fatalities per 100,000 school-aged children. Blue bars represent treatment counties, with an average remote learning share of 50% or higher during the 2020-21 school year, and red bars represent comparison counties, with the remote share less than 50%.
Source: Author's analysis of child maltreatment report and substantiation data from the NCANDS Child File and fatality data from the NVSS for 2017-2020 school years.

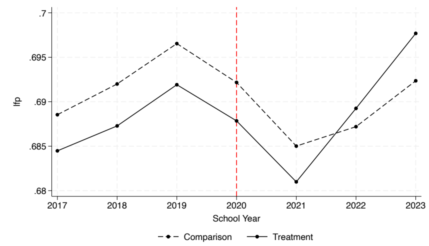
C. Trends in Control Variables

Figure C1: Control Variables Trends

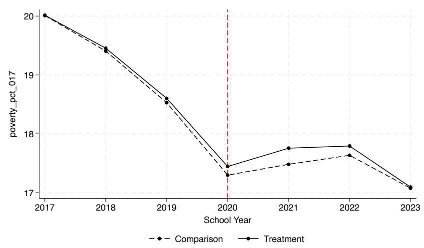
(a) Unemployment



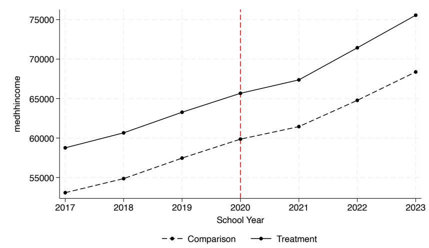
(b) Labor Force Participation



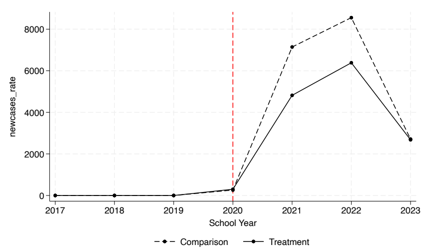
(c) Percent in Poverty, Ages 0-17



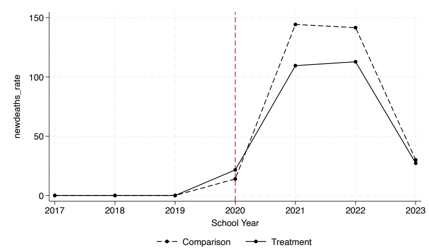
(d) Median Household Income



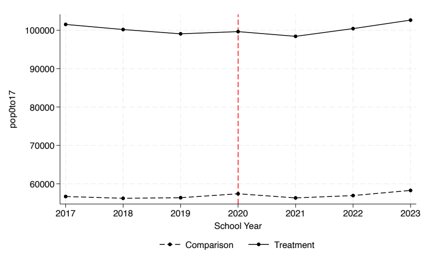
(e) COVID-19 Case Rate



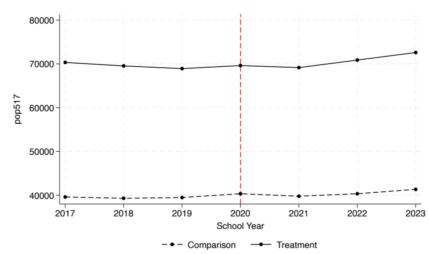
(f) COVID-19 Death Rate



(g) Child Population, Ages 0-17



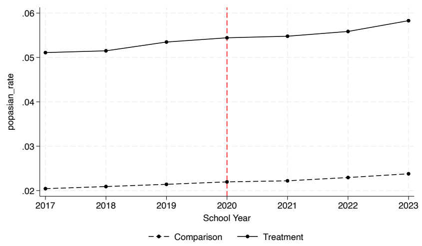
(h) Child Population, Ages 5-17



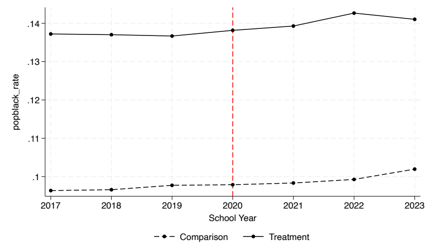
Notes: These figures present unadjusted trends from the 2017 to 2023 school years for (a) unemployment rate, (b) labor force participation rate, (c) percent of the child population living in poverty, (d) median household income (in USD), (e) COVID-19 case rate per 100,000 population, (f) COVID-19 death rate per 100,000 population, (g) total number of child population aged 0-17, and (f) the number of school-aged children (ages 5-17). Solid black lines represent treatment counties, with an average remote learning share of 50% or higher during the 2020-21 school year, and dashed black lines represent comparison counties, with the remote share less than 50%. The vertical dashed line at 2020 marks the year prior to the emergence of variation in instructional modality in the 2020-21 school year.

Figure C2: Control Variables Trends (cont.)

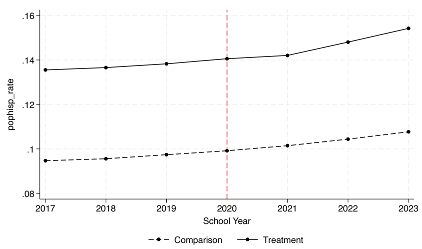
(a) Percent Asian



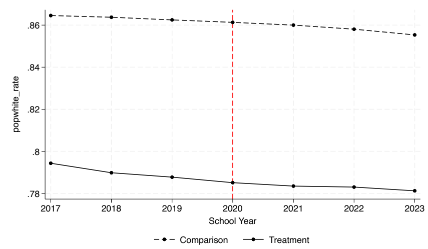
(b) Percent Black



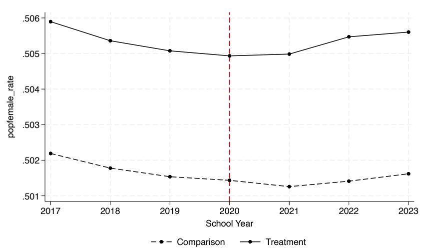
(c) Percent Hispanic



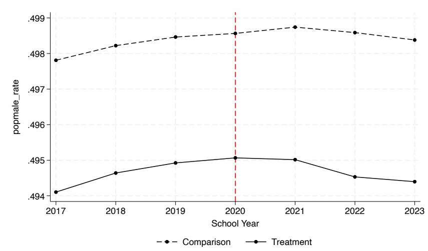
(d) Percent White



(e) Percent Female



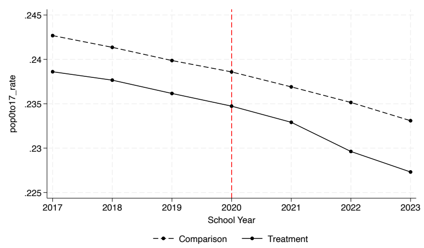
(f) Percent Male



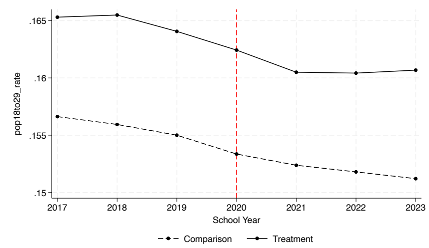
Notes: These figures present unadjusted trends from the 2017 to 2023 school years for the percentage of the population that is (a) Asian, (b) Black, (c) Hispanic, (d) White, (e) female, and (f) male. Solid black lines represent treatment counties, with an average remote learning share of 50% or higher during the 2020-21 school year, and dashed black lines represent comparison counties, with the remote share less than 50%. The vertical dashed line at 2020 marks the year prior to the emergence of variation in instructional modality in the 2020-21 school year.

Figure C3: Control Variables Trends (cont.)

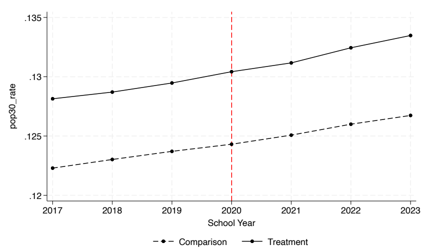
(a) Percent Age 0-17



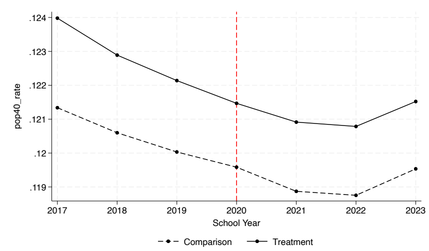
(b) Percent Age 18-29



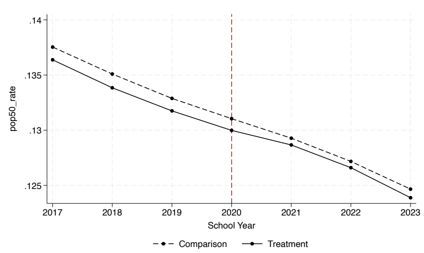
(c) Percent Age 30-39



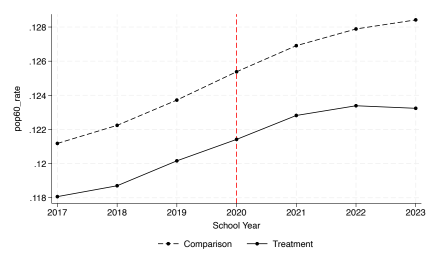
(d) Percent Age 40-49



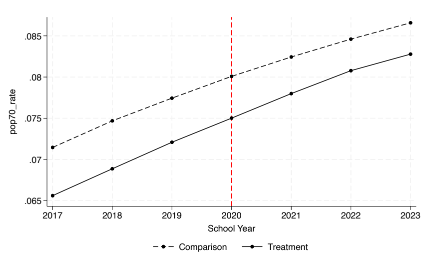
(e) Percent Age 50-59



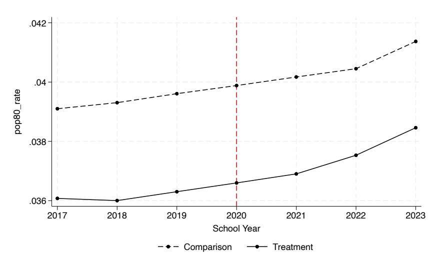
(f) Percent Age 60-69



(g) Percent Age 70-79



(h) Percent Age 80+



Notes: These figures display unadjusted trends in the percentage of the population by age group for each panel (a) through (h) over the 2017 to 2023 school years. Solid black lines represent treatment counties, with an average remote learning share of 50% or higher during the 2020-21 school year, and dashed black lines represent comparison counties, with the remote share less than 50%. The vertical dashed line at 2020 marks the year prior to the emergence of variation in instructional modality in the 2020-21 school year.

D.Four-Group Analysis

I estimate the following equation to estimate the impact by each remote group:

$$E(y_{ct}) = \exp\left\{\beta_{low}(low_c \times post_t) + \beta_{med}(med_c \times post_t) + \beta_{high}(high_c \times post_t) \right.$$
$$\left. + \gamma\mathbf{X}_{ct} + \alpha_c + \delta_t + \ln(childpop_{ct})\right\}$$

where low_c equals 1 for the counties with remote share between 25% and 50%, med_c equals 1 for the counties with remote share between 50% and 75%, and $high_c$ equals 1 for the counties with remote share between 75% and 100%. Counties with remote share between 0% and 25% are the reference group. All other terms are defined as in Equation 1. The coefficients of interest, β_{low}, β_{med}, and β_{high}, estimate the causal effects of exposure to remote learning for each remote group during the 2020-21 school year, comparing to the reference counties. Tables D1 through D3 present the four remote group analysis results.

Table D1: DID Results by Four Remote Groups: Reports

		Across Age Group		Within Age Group	
	(1)	(2)	(3)	(4)	(5)
	0-17	5-17	0-4	5-17	0-4
(Reference group: counties with 0%-25% remote share)					
β_{low}	-0.004	-0.006	-0.001	-0.006	-0.001
	(0.016)	(0.017)	(0.015)	(0.017)	(0.015)
β_{med}	-0.037**	-0.051***	-0.005	-0.051***	-0.006
	(0.018)	(0.019)	(0.017)	(0.019)	(0.017)
β_{high}	-0.158***	-0.184***	-0.098***	-0.186***	-0.094***
	(0.031)	(0.033)	(0.027)	(0.034)	(0.026)
Observations	5,360	5,360	5,360	5,360	5,360
Unique Counties	1,085	1,085	1,085	1,085	1,085
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	0-17	0-17	0-17	5-17	0-4

Notes: This table reports DID estimates from the four-group analysis, with the number of reported children as dependent variables. $\exp(\beta)-1$ can be interpreted as the relative percentage change in the outcomes in each remote group during the 2020-21 school year compared with the counties with lowest remote exposure. Column (1) presents counts for children aged 0-17, columns (2) and (4) for school-aged children (ages 5-17), and columns (3) and (5) for ages 0-4. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D2: DID Results by Four Remote Groups: Substantiations

		Across Age Group		Within Age Group	
	(1)	(2)	(3)	(4)	(5)
	0-17	5-17	0-4	5-17	0-4
(Reference group: counties with 0%-25% remote share)					
β_{low}	0.027	0.018	0.040	0.018	0.039
	(0.026)	(0.028)	(0.025)	(0.028)	(0.025)
β_{med}	0.030	0.022	0.039	0.022	0.038
	(0.031)	(0.033)	(0.030)	(0.033)	(0.030)
β_{high}	-0.021	-0.048	0.018	-0.050	0.023
	(0.044)	(0.048)	(0.041)	(0.048)	(0.040)
Observations	5,360	5,360	5,360	5,360	5,360
Unique Counties	1,085	1,085	1,085	1,085	1,085
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	0-17	0-17	0-17	5-17	0-4

Notes: This table reports DID estimates from the four-group analysis, with the number of substantiated cases as dependent variables. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes in each remote group during the 2020-21 school year compared with the counties with lowest remote exposure. Column (1) presents counts for children aged 0-17, columns (2) and (4) for school-aged children (ages 5-17), and columns (3) and (5) for ages 0-4. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D3: DID Results by Four Remote Groups: Maltreatment-Related Fatalities

		Across Age Group		Within Age Group	
	(1)	(2)	(3)	(4)	(5)
	0-17	5-17	0-4	5-17	0-4
(Reference group: counties with 0%-25% remote share)					
β_{low}	0.008	0.017	-0.014	0.017	-0.014
	(0.044)	(0.059)	(0.062)	(0.059)	(0.062)
β_{med}	0.109**	0.129**	0.064	0.129**	0.064
	(0.046)	(0.060)	(0.068)	(0.060)	(0.067)
β_{high}	0.124*	0.161*	-0.032	0.159*	-0.027
	(0.070)	(0.084)	(0.096)	(0.084)	(0.096)
Observations	5,249	5,031	4,959	5,031	4,959
Unique Counties	1,060	1,015	1,000	1,015	1,000
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	0-17	0-17	0-17	5-17	0-4

Notes: This table reports DID estimates from the four-group analysis, maltreatment-related child deaths (homicides and accidents) as dependent variables. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes in each remote group during the 2020-21 school year compared with the counties with lowest remote exposure. Column (1) presents counts for children aged 0-17, columns (2) and (4) for school-aged children (ages 5-17), and columns (3) and (5) for ages 0-4. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E. ICD-10-Based Fatality Analysis

Table E1: ICD-10-Based Fatality Analysis Results

		Across Age Group		Within Age Group	
	(1)	(2)	(3)	(4)	(5)
	0-17	5-17	0-4	5-17	0-4
β	0.167**	0.166*	0.098	0.165*	0.097
	(0.069)	(0.086)	(0.131)	(0.086)	(0.130)
p-value	0.1593	0.8252	0.0822	0.8205	0.0834
Observations	3904	3197	3111	3197	3111
Unique Counties	785	642	625	642	625
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	0-17	0-17	0-17	5-17	0-4

Notes: This table reports DID estimates from Equation 1, with maltreatment-related child deaths classified based on ICD-10 codes as dependent variables. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes in treatment counties during the 2020-21 school year compared with comparison counties. Column (1) presents counts for children aged 0-17, columns (2) and (4) for school-aged children (ages 5-17), and columns (3) and (5) for ages 0-4. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported *p*-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.

Table E2: ICD-10-Based Fatality Analysis Results by Demographics

	(1) Female	(2) Male	(3) Asian	(4) Black	(5) Hispanic	(6) White
β	0.184	0.170*	1.927	0.004	0.248	0.336**
	(0.187)	(0.098)	(1.233)	(0.111)	(0.191)	(0.137)
p-value	0.7776	0.7116	0.1745	0.3117	0.4848	0.5989
Observations	1,871	2,824	295	1,931	1,145	2,422
Unique Counties	375	567	59	387	229	486
α_c	Y	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	5-17	5-17	5-17	5-17	5-17	5-17

Notes: This table reports DID estimates from Equation 1 by child demographics, with maltreatment-related child deaths classified based on ICD-10 codes as dependent variables. Columns (1) through (6) present estimates for cases involving female, male, Asian, Black, Hispanic, and White children, respectively. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes in treatment counties during the 2020-21 school year compared with comparison counties. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population (ages 5-17) of each demographic group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported *p*-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.

Table E3: List of ICD-10 Codes for Identifying Maltreatment Related Fatalities

ICD-10 Code	Cause of Death
T74.0	Neglect or abandonment
T74.1	Physical abuse
T74.2	Sexual abuse
T74.3	Psychological abuse
T74.8	Other maltreatment syndromes
T74.9	Maltreatment syndrome, unspecified
X85	Assault (homicide) by drugs, medicaments, and biological substances
X86	Assault (homicide) by corrosive substance
X87	Assault (homicide) by pesticides
X88	Assault (homicide) by gases and vapors
X89	Assault (homicide) by other specified chemicals and noxious substances
X90	Assault (homicide) by unspecified chemical or noxious substance
X91	Assault (homicide) by hanging, strangulation, and suffocation
X92	Assault (homicide) by drowning and submersion
X93	Assault (homicide) by handgun discharge
X94	Assault (homicide) by rifle, shotgun, and larger firearm discharge
X95	Assault (homicide) by other and unspecified firearm discharge
X96	Assault (homicide) by explosive material
X97	Assault (homicide) by smoke, fire, and flames
X98	Assault (homicide) by steam, hot vapors, and hot objects
X99	Assault (homicide) by sharp object
Y00	Assault (homicide) by blunt object
Y01	Assault (homicide) by blunt object
Y02	Assault (homicide) by pushing or placing victim before moving object
Y03	Assault (homicide) by crashing of motor vehicle
Y04	Assault (homicide) by bodily force
Y05	Sexual assault (homicide) by bodily force
Y06.0	Neglect and abandonment, by spouse or partner
Y06.1	Neglect and abandonment, by parent
Y06.2	Neglect and abandonment, by acquaintance or friend
Y06.8	Neglect and abandonment, by other specified persons
Y06.9	Neglect and abandonment, by unspecified person
Y07.0	Other maltreatment syndromes, by spouse or partner
Y07.1	Other maltreatment syndromes, by parent
Y07.2	Other maltreatment syndromes, by acquaintance or friend
Y07.3	Other maltreatment syndromes, by official authorities
Y07.8	Other maltreatment syndromes, by other specified persons
Y07.9	Other maltreatment syndromes, by unspecified person
Y08	Assault (homicide) by other specified means
Y09	Assault (homicide) by unspecified means

Notes: This table lists the ICD-10 codes used to construct an alternative measure of maltreatment-related fatalities, following the approaches outlined in Schnitzer et al. (2008) and Parks et al. (2012). The full list of ICD-10 code was obtained from `https://www.cdc.gov/nchs/icd/icd-10/index.html`.

F. 2022-2023 School Years

Table F1: DID Results: Short-Term (SY21) and Long-Term (SY22-23), Ages 5-17

	(1)	(2)	(3)	(4)	(5)
	Reported Cases		Substantiated Cases		Fatality
	Total	Educ.	Total	Educ.	
β_s	-0.087***	-0.334***	0.002	-0.263***	0.152***
	(0.017)	(0.046)	(0.028)	(0.054)	(0.048)
β_l	-0.003	0.019	0.044	0.061	0.079**
	(0.021)	(0.026)	(0.033)	(0.039)	(0.040)
p-value	0.9232	0.8564	0.1537	0.2245	0.2399
Observations	7,467	7,467	7,467	7,467	7,203
Unique Counties	1,093	1,093	1,093	1,093	1,048
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	5-17	5-17	5-17	5-17	5-17

Notes: This table reports DID estimates from Equation 2. Columns (1) and (2) present estimates for total reported cases and those filed by education personnel, columns (3) and (4) present estimates for total substantiated cases and those originally filed by education personnel, and column (5) presents counts of maltreatment-related fatalities (homicides and accidents) of school-aged children. $\exp(\beta_s) - 1$ and $\exp(\beta_l) - 1$ can be interpreted as the relative percentage change in the outcomes in treatment counties compared to comparison counties, during the 2020-21 school year and the 2022-2023 school years respectively. All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population (ages 5-17) as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported *p*-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.

G. State-Level Analysis of Maltreatment-Related Fatalities

This appendix section provides supplemental fatality analyses corresponding to the description in subsection 5.4. Table G1 reports the estimates from the longer-term analysis model (Equation 2), implemented at the state level. Table G2 reports the estimates from the four-remote-group specification described in Appendix D, replacing the post-treatment indicator ($post_t$) with short-term and longer-term indicators.

Table G1: State-Level Fatality Analysis

	(1) Maltreatment-Related Fatality
β_s	0.336***
	(0.112)
β_l	0.329**
	(0.132)
p-value	0.4896
Observations	343
Unique States	49
α_c	Y
δ_t	Y
$\mathbf{X}_{ct}$	Y
exposure($childpop_{ct}$)	0-17

Notes: This table reports state-level DID analysis results of the number of maltreatment-related fatalities as dependent variables. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes in each period (short- and long-term) compared with the comparison counties. Short-term period covers October 2020 through September 2021 (federal fiscal year 2021), and long-term period includes 2022-2023 federal fiscal years. All regressions include state and federal-fiscal-year fixed effects (α_c, δ_t), time-varying state-level covariates ($\mathbf{X}_{ct}$), and child population (ages 0-17) as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

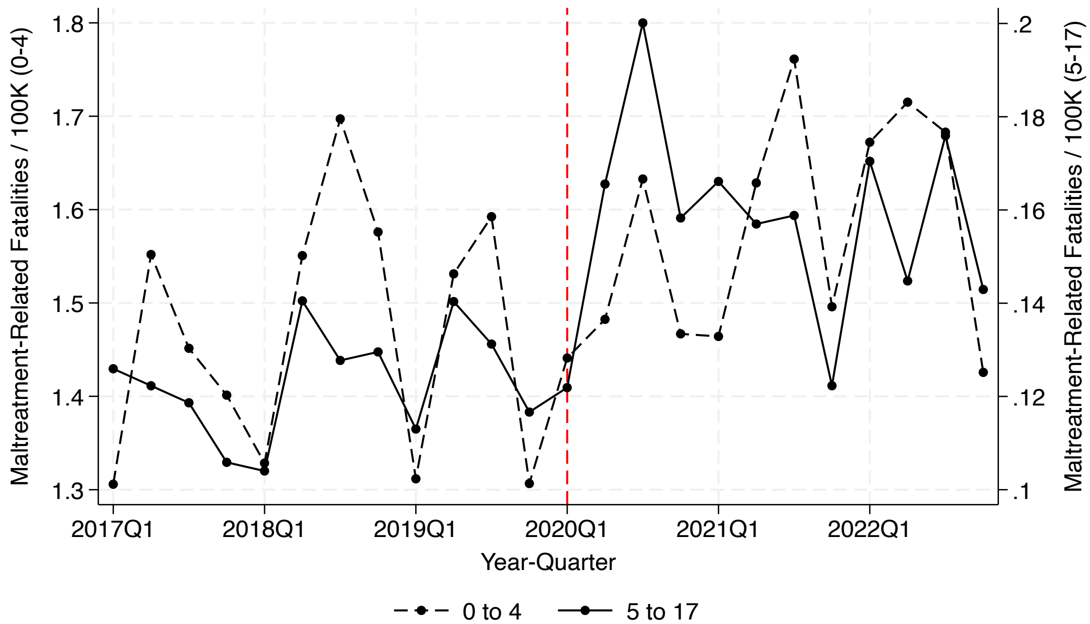
Table G2: State-Level Fatality Analysis

	(1) Maltreatment-Related Fatality
FFY21	
(Reference group: counties with 0%-25% remote share)	
β_{low}	-0.166
	(0.137)
β_{med}	0.189
	(0.115)
β_{high}	0.339**
	(0.157)
FFY22-23	
β_{low}	0.119
	(0.133)
β_{med}	0.402***
	(0.152)
β_{high}	0.421***
	(0.150)
Observations	343
Unique States	49
α_c	Y
δ_t	Y
$\mathbf{X}_{ct}$	Y
exposure($childpop_{ct}$)	0-17

Notes: This table reports state-level DID analysis results of the number of maltreatment-related fatalities as dependent variables. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes of each remote group in each period (short- and long-term) compared with the reference counties. Short-term period covers October 2020 through September 2021 (federal fiscal year 2021), and long-term period includes 2022-2023 federal fiscal years. All regressions include state and federal-fiscal-year fixed effects (α_c, δ_t), time-varying state-level covariates ($\mathbf{X}_{ct}$), and child population (ages 0-17) as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure G4: Fatality Trends by Age Group

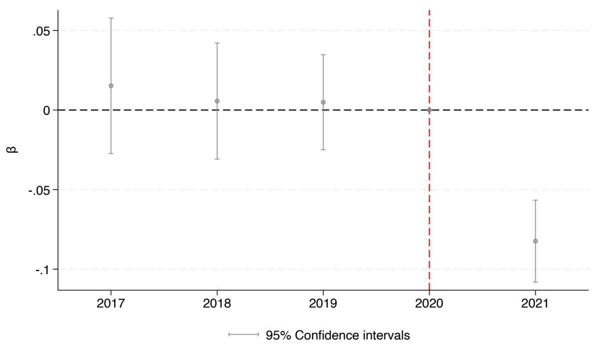


Notes: This figure presents quarterly trends in maltreatment-related fatality rates (per 100,000 children) by age group (0-4 and 5-17). The dashed black line represents the trend for children aged 0-4, and the solid black line represents the trend for children aged 5-17. The dashed red vertical line indicates January–March 2020, the last quarter that schools were mostly in session before the pandemic and subsequent school closures.
Source: Author's analysis of NCANDS Child File data.

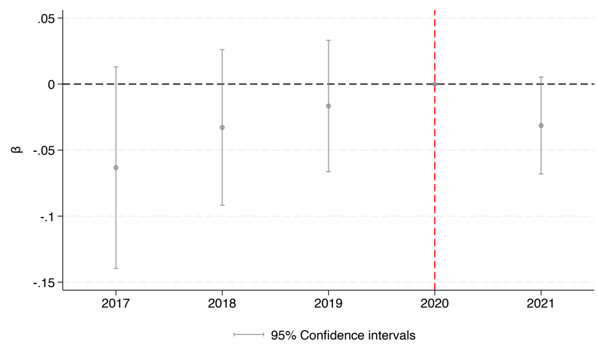
H. Additional Pre-Trend Tests

Figure H1: Event Study (2016-17–2020-21 School Years)

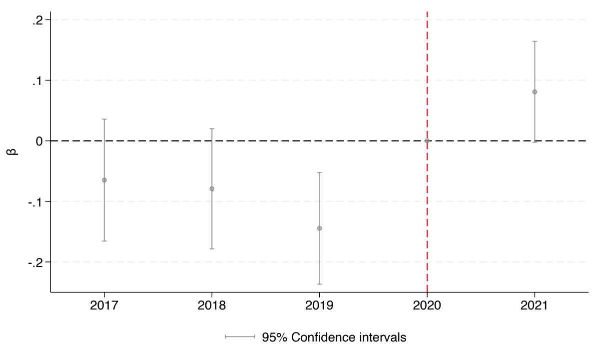
(a) Reported Children



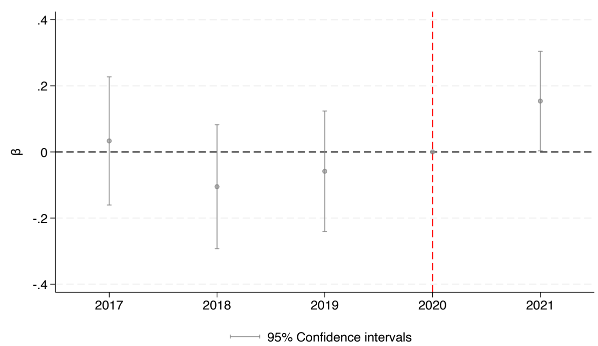
(b) Substantiated Cases



(c) Maltreatment-Related Deaths



(d) Maltreatment-Related Deaths (ICD-10)



Notes: These figures present event study estimates plots. Dependent variables are (a) the number of reported children, (b) substantiated cases, (c) maltreatment-related fatalities (homicides and accidents), and (d) maltreatment-related deaths identified using ICD-10 codes listed in Table E1. All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are clustered at the county level. Grey vertical bar around point estimates represent 95% confidence intervals. The vertical dashed line at 2020 marks the year prior to the emergence of variation in instructional modality in the 2020-21 school year.

Table H1: Placebo Test

	(1) 2018	(2) 2019	(3) 2020
Panel A. Reported Children			
β	-0.005	-0.001	-0.000
	(0.012)	(0.012)	(0.012)
Observations	4,271	4,271	4,271
Unique Counties	1,081	1,081	1,081
Panel B. Maltreatment-Related Fatalities (Heath, 2024)			
β	-0.002	-0.001	0.084*
	(0.054)	(0.043)	(0.050)
Observations	3,878	3,878	3,878
Unique Counties	977	977	977
Panel C. Maltreatment-Related Fatalities (Schnitzer et al., 2008; Parks et al., 2012)			
β	-0.089	-0.000	0.002
	(0.108)	(0.085)	(0.090)
Observations	2,274	2,274	2,274
Unique Counties	570	570	570

Notes: This table presents results from placebo tests using DID specification in Equation 1, where the analysis sample is restricted to pre-treatment years and post-treatment indicator is replaced with year-specific indicators. Each column shows estimates from assigning a false treatment to one of the pre-treatment school years: (1) 2017–18, (2) 2018–19, and (3) 2019–20. Panel A reports outcomes for reported children; Panels B and C report maltreatment-related fatalities based on Heath (2024) and Schnitzer et al. (2008); Parks et al. (2012), respectively. All regressions include county and school-year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population (ages 5-17) as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I. Alternative Specifications

Table I1: DID Results (Without Aggregated Hypothetical Counties)

		Across Age Group		Within Age Group	
	(1)	(2)	(3)	(4)	(5)
	0-17	5-17	0-4	5-17	0-4
Panel A. Reported Children					
β	-0.061***	-0.077***	-0.022	-0.078***	-0.021
	(0.015)	(0.016)	(0.014)	(0.017)	(0.014)
p-value	0.9434	0.8520	0.6443	0.8184	0.4673
Observations	5,132	5,132	5,132	5,132	5,132
Unique Counties	1,039	1,039	1,039	1,039	1,039
Panel B. Substantiated Cases					
β	0.015	0.009	0.022	0.009	0.022
	(0.024)	(0.026)	(0.024)	(0.026)	(0.024)
p-value	0.2541	0.2022	0.4020	0.2303	0.3377
Observations	5,132	5,132	5,132	5,132	5,132
Unique Counties	1,039	1,039	1,039	1,039	1,039
Panel C. Maltreatment-Related Fatalities					
β	0.124***	0.141***	0.074	0.140***	0.074
	(0.038)	(0.051)	(0.060)	(0.051)	(0.060)
p-value	0.0182	0.2454	0.0650	0.2446	0.0696
Observations	5,021	4,803	4,731	4,803	4,731
Unique Counties	1,014	969	954	969	954
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	0-17	0-17	0-17	5-17	0-4

Notes: This table reports DID estimates from Equation 1, with the number of reported children (Panel A), substantiated cases (Panel B), and child fatalities from homicides and accidents (Panel C) as dependent variables. The analysis sample excludes the aggregated hypothetical counties within each state. $\exp(\beta)-1$ can be interpreted as the relative percentage change in the outcomes during the 2020-21 school year, as a county hypothetically switch from fully in-person (0%) to fully remote (100%). Column (1) presents counts for children aged 0-17, columns (2) and (4) for school-aged children (ages 5-17), and columns (3) and (5) for ages 0-4. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported p-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table I2: DID Results (Counties with All Years Observations)

		Across Age Group		Within Age Group	
	(1)	(2)	(3)	(4)	(5)
	0-17	5-17	0-4	5-17	0-4
Panel A. Reported Children					
β	-0.061***	-0.077***	-0.025*	-0.077***	-0.024*
	(0.016)	(0.017)	(0.015)	(0.017)	(0.014)
p-value	0.8762	0.9569	0.4352	0.9500	0.2666
Observations	4,990	4,990	4,990	4,990	4,990
Unique Counties	998	998	998	998	998
Panel B. Substantiated Cases					
β	0.002	-0.004	0.011	-0.004	0.012
	(0.026)	(0.028)	(0.025)	(0.028)	(0.025)
p-value	0.2374	0.1368	0.5601	0.1684	0.4514
Observations	4,990	4,990	4,990	4,990	4,990
Unique Counties	998	998	998	998	998
Panel C. Maltreatment-Related Fatalities					
β	0.110***	0.138***	0.044	0.137***	0.045
	(0.039)	(0.050)	(0.057)	(0.050)	(0.057)
p-value	0.0144	0.1527	0.0154	0.1560	0.0188
Observations	4,935	4,745	4,710	4,745	4,710
Unique Counties	987	949	942	949	942
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	0-17	0-17	0-17	5-17	0-4

Notes: This table reports DID estimates from Equation 1, with the number of reported children (Panel A), substantiated cases (Panel B), and child fatalities from homicides and accidents (Panel C) as dependent variables. The analysis sample is restricted to counties with complete data for each school year from 2016–17 through 2022–23. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes during the 2020-21 school year, as a county hypothetically switch from fully in-person (0%) to fully remote (100%). Column (1) presents counts for children aged 0-17, columns (2) and (4) for school-aged children (ages 5-17), and columns (3) and (5) for ages 0-4. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported p-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table I3: Continuous DID Results

		Across Age Group		Within Age Group	
	(1)	(2)	(3)	(4)	(5)
	0-17	5-17	0-4	5-17	0-4
Panel A. Reported Children					
β	-0.134***	-0.167***	-0.059*	-0.168***	-0.057*
	(0.033)	(0.035)	(0.031)	(0.035)	(0.029)
p-value	0.7302	0.6671	0.7378	0.7024	0.5123
Observations	5,360	5,360	5,360	5,360	5,360
Unique Counties	1,085	1,085	1,085	1,085	1,085
Panel B. Substantiated Cases					
β	0.044	0.024	0.069	0.023	0.072
	(0.054)	(0.058)	(0.052)	(0.059)	(0.051)
p-value	0.0186	0.0084	0.0572	0.0120	0.0393
Observations	5,360	5,360	5,360	5,360	5,360
Unique Counties	1,085	1,085	1,085	1,085	1,085
Panel C. Maltreatment-Related Fatalities					
β	0.227***	0.295***	0.031	0.293***	0.032
	(0.077)	(0.102)	(0.115)	(0.102)	(0.115)
p-value	0.0014	0.1943	0.0069	0.1948	0.0085
Observations	5,249	5,031	4,959	5,031	4,959
Unique Counties	1,060	1,015	1,000	1,015	1,000
α_c	Y	Y	Y	Y	Y
δ_t	Y	Y	Y	Y	Y
$\mathbf{X}_{ct}$	Y	Y	Y	Y	Y
exposure($childpop_{ct}$)	0-17	0-17	0-17	5-17	0-4

Notes: This table reports continuous DID estimates from Equation 1, with the number of reported children (Panel A), substantiated cases (Panel B), and child fatalities from homicides and accidents (Panel C) as dependent variables. $\exp(\beta) - 1$ can be interpreted as the relative percentage change in the outcomes during the 2020-21 school year, as a county hypothetically switch from fully in-person (0%) to fully remote (100%). Column (1) presents counts for children aged 0-17, columns (2) and (4) for school-aged children (ages 5-17), and columns (3) and (5) for ages 0-4. Post-treatment period covers September 2020 through May 2021 (school year 2020-21), and pre-treatment period includes school years 2016-17 through 2019-20, excluding summer months (June–August). All regressions include county and school year fixed effects (α_c, δ_t), time-varying county-level covariates ($\mathbf{X}_{ct}$), and child population of each age group as the exposure with its coefficient constrained to one. Robust standard errors are in parentheses, clustered at the county level. The reported p-values are from a joint hypothesis test ($\beta_{2018} = \beta_{2019} = \beta_{2020} = 0$), with the first year in the sample (e.g. school year 2016-17) as the reference.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$