Climate Change and Learning Loss: Evidence from Wildfire School Closures in California *

Yifeng Luo[1] and Ying Xu[2]

[1]Shenzhen University

[2]Columbia University

May 26, 2024

Abstract

This paper examines the causal effect of unexpected school closures due to wildfires on student academic achievement. We exploit exogenous variation in the intensity of wildfire school closures in California between 2009 and 2017 as a natural experiment. We find that wildfire school closures have negative effects on both ELA and math test scores. Students with lower socioeconomic status experience larger negative effects from such unexpected closures. Furthermore, we show that school time loss and air pollution are two important mechanisms contributing to the decline we measure in student achievement.

*We are deeply grateful to Alex Eble's support throughout this project. This paper would not have been possible without the helpful suggestions of Peter Bergman, Sarah Cohodes, Jordan Matsudaira, Kirsten Slungaard Mumma, Sebastián Otero, R. Jisung Park, Douglas Ready, Sean Reardon, Randall Reback, Jonah Rockoff, Judith Scott-Clayton, and Pietro Tebaldi. We also thank participants at the Russell Sage Foundation & William T. Grant Foundation Conference and Columbia University Economics of Education seminars for their helpful suggestions. Funding for this project was generously provided by the Russell Sage Foundation and William T. Grant Foundation. All errors are our own.

1 Introduction

Schools are one of the key actors in the provision of human capital. Indeed, several aspects of schooling, including longer school time, better teachers, and more funding, can improve student outcomes (Angrist and Krueger, 1991; Angrist et al., 2013; Hanushek, 1986; Jackson et al., 2015). At times, however, adverse events such as natural disasters and infectious disease outbreaks force schools to close to ensure public safety, thus disrupting students' education. While such disruptions likely have negative ramifications for students, there is relatively little empirical research on the effects of such unexpected school closures on student achievement.

In this paper, we use administrative data from California to conduct one of the first empirical analyses of the causal effect of sudden school closures due to wildfires on student achievement. The impact of wildfires has drawn significant attention in the United States because these fires have become larger, more frequent, and more widespread (Burke et al., 2021). In California, wildfires are one of the most common reasons for school closures; in the 2017-18 academic year, for example, wildfire closures accounted for 91% of all closure days. Further, wildfire-related school closures are similar to other unexpected school closures such as the COVID-19 school shutdowns that began in March 2020 and persisted for multiple academic years in some areas of the country. Both types of closures led to learning interruptions, strain on teachers, economic losses for parents, and emotional distress from exposure to the disaster. Given these repercussions, uncovering the effects of wildfire school closures has important policy implications.

The current study focuses on wildfire school closures that occurred in California between the 2009-10 and 2016-17 academic years. We link school closure data to both district-level and school-level test score data and use a school-grade/district-grade fixed effects model to exploit exogenous variation in shock intensity across time. Compared to extreme winter conditions, which are often used as exogenous shocks causing school closures (Goodman, 2014; Marcotte and Hemelt, 2008), wildfires are less predictable and vary more widely across years, and thus teachers are less likely to consider the impact of closures due to wildfires when developing their teaching plans. This unpredictability and temporal variation in wildfires facilitate the identification of causal effects in the model and allow us to extend the empirical evidence on the consequences of school closures beyond prior results on weather-based closures.

We first estimate the overall impact of wildfire school closures on students' academic performance. The results reveal that these closures have a negative impact on test scores in both English language arts (ELA) and math. Our preferred specification shows that on average, one wildfire school closure day decreases both math and ELA scores by 0.02 standard deviations (SD) relative to the scores of the national reference cohort in the same grade. Because wildfire school closures last for 2.4 days on average, a typical closure caused by a wildfire leads to a loss of 0.048 SD in both ELA and math scores. Such a magnitude is equivalent to at least 12% of the total yearly learning according to the estimates of Betthäuser et al. (2023), or an approximately 40 percent of a SD change in teacher quality, which can translate into a 1.34% change in lifetime earnings according to the estimates of Chetty et al. (2014). However, these effects are transitory—the negative effects that emerge in one year do not persist into the following year. We also find that closures lasting 2-5 school days have more severe impacts on test scores, compared to closures that last for only one day or more than 5 days.

Next, we explore heterogeneity in the effects of wildfire school closures on student achievement by gender and socioeconomic status (SES). We find that for a similar number of wildfire school closure days, learning loss is greater among students from low-SES districts than among those from high-SES districts, which is consistent with related work (Goodman, 2014; Groppo and Kraehnert, 2017). This heterogeneity suggests that policymakers apportioning resources in the aftermath of sudden school closures should consider targeting low-income students. The results do not identify a gender difference in the effect of wildfire school closures on student test scores in either ELA or math.

We then explore two possible mechanisms driving these learning losses: loss of school time and air pollution. The loss of school time can lead to both less time for instruction and out-of-school environment. To examine this mechanism, we exclude grade-year cells that experienced large-scale wildfires or closures due to severe air pollution, so that the treated group suffered primarily from a loss of school time rather than psychological trauma, financial loss, or the health impacts of air pollution. The results for this restricted sample show that the loss of school time led to a substantial decrease in test scores, which suggests that the loss of school time is an important channel through which wildfire school closures harmed student learning and has policy implications for alleviating learning loss during unexpected school closures.

We also explore whether air pollution caused by wildfires underlies the link between school closures and learning loss. Estimating the causal effect of air pollution on student test scores is complicated by potential omitted variable bias (e.g., student sorting into residential districts partly based on environmental quality) and measurement error. We overcome these challenges by combining an instrumental variables (IV) approach that is similar to Deryugina et al. (2019), exploiting variation in air pollution attributable to changes in wind direction, and using Childs et al. (2022) wildfire smoke data that specifically measures wildfire-driven PM2.5. Our IV estimates show that a 1-microgram per cubic meter ($\mu g/m^3$) increase in the cumulative wildfire smoke PM 2.5 (particulate matter that is 2.5 microns in diameter or smaller) concentration decreases both math and ELA scores by 0.022 SD, indicating that wildfires also lead to learning loss via their effect on air pollution.

In addition, we examine whether schools adjust their spending after wildfire school closures, which can both reflect schools' remediation strategies and be another mechanism of the learning loss we find. Results show that wildfire school closures did not affect total spending. We also break down total spending into nine categories and find that most of them are not affected by wildfire school closures, except the small impacts

($8-$31 per-pupil per-closure day annually) on instruction administration, ancillary services, and maintenance & operation. However, few studies, to the best of our knowledge, have found associations between these categories of spending and student achievement. Therefore, these adjustments are unlikely to be used for remediating student learning loss or one of the mechanisms of the learning loss we find.

We demonstrate that our identification strategy is effective at eliminating confounding effects from unobserved school/school district characteristics. We show that wildfire school closures in the future years are not predictive of student achievement in the current year, suggesting a lack of preexisting trends in student achievement. Results also show that the number of wildfire school closure days does not influence the number of other school closure days. Additionally, we show that student mobility after wildfires does not bias our estimates. Wildfire school closures do not affect enrollment and have small effects on the racial/ethnic composition of students. The small changes in student racial/ethnic composition only explain a negligible proportion of the estimated impact of wildfire school closures on test scores.

Our study contributes to several streams of literature. First, we contribute to the literature on the effects of severe natural disasters on human capital development. We extend this literature by providing the first, to the best of our knowledge, well-identified evidence on the impact of wildfire school closures—which have become increasingly common in recent years—on student test scores and inequality in academic achievement. Prior studies on the impacts of natural disasters on human capital development have mainly focused on developing countries (Deuchert and Felfe, 2015; Herrera-Almanza and Cas, 2020; Paudel and Ryu, 2018), especially low-income families whose income depends heavily on weather conditions (Groppo and Kraehnert, 2017). However, given the increasing impact of climate change and infectious diseases around the world, there is a need to extend this research to developed countries.[1] In addition, the current study reveals the channels through which natural disasters affect human capital. Our findings suggest that loss of school time and air pollution are two important mechanisms behind the observed impacts.

Our research also adds to a growing body of literature on the impact of air pollution on human well-being. Research has shown that air pollution is associated with many significant outcomes in life (Aguilar-Gomez et al., 2022).[2] We exploit plausibly exogenous variation in wind direction to provide one of the first quasi-experimental evidence that wildfire smoke harms student test scores, complementing studies exploring whether interventions that improve air quality increase test scores(Ebenstein et al., 2016; Gilraine, 2020; Roth, 2016). Our estimates also suggest that the endogeneity problem can lead to under-estimation of the negative impact of air pollution, which is consistent with Deryugina et al. (2019)

Our findings also contribute to the small but growing literature on the economic and social costs of wildfires. Research has documented the correlation between wild-

[1]A few studies have explored this topic in the context of developed countries. Sacerdote (2012), for example, finds that Hurricanes Katrina and Rita negatively affected New Orleans students' test scores in the first year following the storms. These two hurricanes, however, almost completely destroyed the entire public school system—more frequent but less disastrous events may have different effects. In addition, the Covid-19 pandemic prompted studies on the impact of the pandemic on college graduation rates and job placement (Aucejo et al., 2020), student math progress on an online platform (Chetty et al., 2020), and predicted learning loss (Kuhfeld et al., 2020).

[2]Studies have shown the relationship between air pollution and health outcomes, (Deryugina et al., 2019; He et al., 2020; Pullabhotla and Souza, 2022), productivity (Adhvaryu et al., 2022; He et al., 2019), cognitive performance (Ebenstein et al., 2016; La Nauze and Severnini, 2021), and violent crimes (Burkhardt et al., 2019).

fires and air quality that translates into health deterioration (Burke et al., 2023), house price declines, credit card and mortgage defaults (An et al., 2023), and lower earnings (Borgschulte et al., 2022). Our findings that wildfire school closures negatively affect student test scores, especially those with lower socio-economic status have important implications for the evaluation of the social cost of wildfire-related disasters.

Finally, our results extend the literature on how sudden school closures affect student outcomes. Prior studies show that heavy snow-driven school closures have mixed effects on student test scores (Fuller, 2013; Goodman, 2014; Groppo and Kraehnert, 2017; Hansen, 2011; Marcotte, 2007; Marcotte and Hemelt, 2008). Our study explores the impact of school closures resulting from wildfires, a type of more unpredictable natural disaster that is happening at an increasing frequency and severity and is more variable across years, facilitating the identification of causal effects. We leverage these important features of wildfire school closures to provide one of the first implications for evaluating the impact of unexpected school closures resulting from natural disasters and infectious disease outbreaks, such as the Covid-19 pandemic.

The rest of the paper is organized as follows. Section 2 reviews the study's background and data. Section 3 describes the empirical strategy. Section 4 presents the main results. Section 5 concludes.

2 Background and Data

Unexpected wildfires affect a large number of students in the United States each year. In California, for example, wildfires affected more than 867,000 students in 2017; further, the number of California schools affected by wildfires increased dramatically over the past decade (see Figure 1). Figure 2 illustrates the geographic distribution of California wildfires between 2007 and 2019, showing that many public schools across a widespread geographical area are located close to wildfire events. Further, researchers have predicted that climate change will generate more frequent extreme weather events, such as heatwaves, heavy precipitation, and droughts, in at least some regions of the world (Seneviratne et al., 2012). Given these trends, unexpected school closures will likely become more common and thus deserve more empirical attention.

In California, wildfire school closures predominantly occur during the Fall semester. For instance, within our sample, 92% of such closures happened between August and December, as shown in Figure 3. Considering that California's statewide assessment of student performance is conducted from January to early July, the majority of wildfire school closures in our sample occurred prior to these assessment dates. The decision to shut down schools during wildfire emergencies typically arises from a collaborative process involving district-level decision-making and guidance from local and state government authorities.

Unfortunately, schools do not always make up closure days. In the United States, states have varying requirements regarding the number of instructional days and hours in an academic year. The majority of states set the school year at 180 days. However, when extreme situations prevent schools from having enough school days, states may not require schools to make up all missed school days. In California, for example, Education Code § 41422 provides that in extreme circumstances, if a school district can show, to the satisfaction of the superintendent, that specific reasons prevented the school from holding classes for at least 175 school days during a fiscal year it can still "receive the same apportionment from the State School Fund as it would have received." Therefore, schools in California may not have enough incentive to make up for unexpected closure days. Consequently, school closures caused by unexpected disastrous events can potentially lead to unfinished teaching plans.

We combine multiple data sources to assess the effects of wildfire school closures on student achievement. First, we use grade-specific district-level test score data from the Stanford Education Data Archive (SEDA). These data include average district-level test scores in English language arts (ELA) and math as well as the achievement gap by gender for students in grades 3 through 8 for each school district in each academic year (AY) from 2009-10 to 2017-18, with the exception of AY 2013-14 (California did not administer standardized tests in 2014).[3] A one-unit increase in these scores refers to 1 grade-specific standard deviation above the average test score of the national reference cohort, which consists of four cohorts of students who were in 4th grade in 2009, 2011, 2013, and 2015. The scores are comparable across school districts and years but not grades. The SEDA dataset also includes information on district enrollment (in grades 3 through 8), the percentage of students eligible for free/reduced-price lunch, and racial/ethnic composition of the student body.

Second, we use school-level test score data from the California Department of Education (CDE) for the 2009-10 through 2018-19 AYs. These test score data are not directly comparable across years because the state changed its assessment in 2014. We follow the procedures developed by Reardon et al. (2017) to re-scale these school-level test scores.[4] Specifically, we link reliability-adjusted California school test score scales to the NAEP scales for ELA and math using the following equation:

$$\hat{\mu}_{sygb}^{n\hat{a}ep} = \hat{\mu}_{cygb}^{naep} + \frac{\hat{\mu}_{sygb}^{state}}{\sqrt{\hat{\rho}_{cygb}^{state}}} * \hat{\sigma}_{cygb}^{naep}$$

where $\hat{\mu}_{sygb}^{n\hat{a}ep}$ is the estimated test score for school **s**, in year **y**, grade **g**, and subject **b** $\in \{ELA, math\}$, on the NAEP scale. $\hat{\mu}_{cygb}^{naep}$ and $\hat{\sigma}_{cygb}^{naep}$ are the NAEP test score means

[3]The SEDA converted these scores to a comparable national scale based on the National Assessment of Education Progress (NAEP) test scores.

[4]Reardon et al. (2017) developed a method to link test score data from states to NAEP data, which provides comparable state-level scores in reading and math for students in 4 and 8 in odd years. Reardon et al. (2017) use this linking method to estimate test scores that are comparable across years and states.

and standard deviations of year **y**, grade **g**, and subject **b** in California. $\hat{\mu}^{state}_{sygb}$ and $\hat{\rho}^{state}_{cygb}$ represent the original school-level test score and the reliability of the state test. To ease interpretation, we then standardize the re-scaled scores using the cohort standardization technique recommended in Reardon et al. (2017). For the resulting scores, 1 unit refers to 1 grade-specific standard deviation above the average test score of the California reference group, which consists of five cohorts of California students who were in 4th grade in 2007, 2009, 2011, 2013, and 2015. Like the district-level test scores, these standardized school-level scores are comparable across schools and years but not across grades.

We also obtained information on several school-level and district-level characteristics from the CDE. At the school level, we use data on school location, grade-level enrollment, gender and racial/ethnic composition of the student body, and percentage of students eligible for free/reduced-price lunch. At the district level, we use expenditure data for nine specific functions: instruction, instructional administration, school administration, guidance and counseling, psychological services, health services, ancillary services, maintenance and operation, and facilities acquisition.

School closure data are drawn from the California Public School Closure Database, which contains information on the number of school closure days due to natural disasters and weather, wildfires, and student safety for AY 2002-03 to 2018-19 for each school. Figure 4 shows that the number of closure days caused by wildfires is highly variable across years, compared to those caused by extreme winter conditions, which has been used frequently as exogenous shocks to study the impact of sudden school closures. We also calculate enrollment-weighted closure days due to wildfires and other factors at the district-year level because SEDA has district-year-grade test score data but not school-year-grade test score data.

Our analytic sample combines wildfire school closures between AY 2007-08 and 2018-19, and test scores between AY 2009-10 and 2016-17. Incorporating wildfire school closures in AY 2007-08 and 2008-09 allows us to (1) examine the lagged effects of wildfire school closures; and (2) include the 2007 Santa Barbara wildfire, which affected more than 1 million students. Further, we perform placebo tests by examining whether wildfires affect student test scores realized one and two years before the wildfire school closures.[5]

We also use California Department of Forestry and Fire Protection data to obtain information on the timing and geographical coverage of all wildfire events in California during the focal period. We match this information with data on school locations and wildfire closure timings to match each wildfire school closure to a specific wildfire event.[6] The information on acres burned allowed us to distinguish between schools that experienced damaging wildfires and those that were closed only as a precaution.

To examine the mechanisms by which wildfire school closures impact test scores, we analyze data on air pollution and wind direction. The wildfire smoke PM2.5 data comes from Childs et al. (2022), which generates daily estimates of wildfire-driven PM2.5 across the contiguous US from 2006 to 2020 based on ground, satellite, and reanalysis data sources. We use the cumulative smoke exposure during the school year at the local level and match the smoke data to school data based on the zipcodes of schools [7]. We use

[5]Placebo tests include school closures from AY 2017-18 and 2018-19. However, test scores from these AYs are not included in the main analysis because we cannot test their pre-trends.

[6]In 2017 Southern California Fires, no fire was documented in Santa Barbara County in December but multiple schools were affected. We match these closures to the wildfire happened in the closest county, Ventura. We also matched January 2018 closures in Ventura to the December 2017 Ventura Fire. In 2007 Southern California Fires, no fire was documented in Ventura County but multiple schools were affected. We match these closures to the fire happened in the closest county, Los Angeles. All of these are consistent with fire documentation.

[7]We first merge the wildfire smoke PM2.5 data to schools based on the 5-digit zipcodes. For schools that are not successfully matched to the smoke data (around 17% school-year cells), we further use the 4-digit and 3-digit zipcodes to impute the smoke data.

the daily smoke data to calculate averages and percentiles of wildfire-driven PM2.5 by academic year for each school. We also use hourly wind direction data obtained from the California Air Resources Board to calculate relative frequencies of four wind directions (NE, SE, SW, and NW) at each monitor at both the month level and the year level.[8] We then match each school to the nearest monitor by year.[9]

Table 1 presents summary statistics of test scores, demographic characteristics, and the number of wildfire school closure days in the analytic sample. Column 1 of Panel A shows that during the sample period, students from California performed worse than the national average (i.e., mean math and ELA scores are both negative). In addition, average math scores are slightly lower for students in the closure sample (i.e., school-grade-year or district-grade-year cells affected by wildfires) than for the overall California sample, although this simple comparison does not necessarily indicate a causal relationship between school closures and test scores.

As shown in Panel B, about 60% of students in the analytic sample are eligible for free or reduced-price lunch. About half of the sample members are Hispanic, while 39% are White, 8% are Asian, and 4% are Black.[10] A comparison of Columns 1 and 2 (or Columns 3 and 4) shows that schools/districts with a higher proportion of White students are more likely to be affected by wildfires, while those with a higher proportion of Hispanic students are less likely to be affected.

Among the 8,246 schools in the analytic sample, 1,297 were affected by wildfire school

[8]These monitors can be different from the ones used to get air pollution data.

[9]Wind direction is reported on a 0° - 360° scale, where 360° means a due north wind and 90° means a due east wind. If the nearest monitor was missing wind direction data, we use the wind direction data from the second nearest monitor, and so forth.

[10]Columns 1 and 3 show different racial/ethnic compositions because these mean statistics are calculated without weights.

closures during the study period. This translates into 9,980 affected school-year-grade cells among the 201,869 cells in the analysis sample. Panel C in Table 1 also shows that on average, these school-grades experienced 2.83 days of closure due to wildfires.

3 Empirical Strategy

We estimate the impact of wildfire school closures via a two-way fixed effects approach. It eliminates biases from cross district-grade differences that are constant over time, such as location.[11] Specifically, we estimate the causal effects of wildfire school closures using the following equation:[12]

$$\begin{aligned} Score_{dgt} = & \alpha_0 + \Sigma_{k=-2}^{2} \beta_k Days\_Closure\_Fire_{dg(t+k)} + \\ & \Sigma_{k=-2}^{0} \eta_k Days\_Closure\_other_{dg(t+k)} + \\ & \gamma X_{dgt} + \mu_{dg} + \phi_{t-g} + \lambda_t + t \cdot \omega_c + \epsilon_{dgt} \end{aligned} \tag{1}$$

where $Score_{dgt}$ denotes the mean standardized test scores in math or ELA for school

[11] Although wildfire school closures are sudden and likely perceived as idiosyncratic events by schools and students, the likelihood of such closures may be correlated with the characteristics of schools and students. For example, schools in cities may suffer less from wildfires than schools in mountain areas. School districts may also have control over the length of school closures: schools with more resources are likely more capable of recovering from disasters and shortening the length of their school closures.

[12] Our setting is not appropriate for using a standard event study model because our treatment, the number of wildfire school closure days, is continuous and reflects the varying intensity of unexpected school closures. Converting this continuous treatment to a binary one misses the rich variation in treatment dosages in identifying the causal effects of school closures.The properties of applying event study models in the setting where the treatment is continuous are still unclear (Monarrez et al., 2022; Sandler and Sandler, 2013).

district d, grade g, and year t. $Days\_Closure\_fire_{dg(t+k)}$ represents the average number of school closure days of district d in year $t+k$ weighted by school enrollment, $k \in \{-2, -1, 0, 1, 2\}$. β_0, β_{-1}, and β_{-2} are the coefficients of interest, which measure the impact of one wildfire school closure day that occurred in the current (academic) year, one year ago, and two years ago, respectively. Models also include a control variable, $Days\_Closure\_other_{dg(t+k)}$, for the number of closure days in the past two years caused by other factors, such as heavy rain, snows, impassable roads, and broken/damaged infrastructures.

One may be worried that the estimated effects of wildfire school closures on student achievement could be biased by pre-trends in student achievement. We test for this possibility by examining whether wildfire school closures in the future influence current levels of student achievement. Specifically, β_1 and β_2 capture the effects of wildfire school closures in years $t = 1$ and $t = 2$ on student achievement in year $t = 0$. β_1 and β_2 are expected to be indistinguishable from 0. In Section 4.4, we show that the results cannot reject the null hypothesis that there is no pre-trends.

μ_{dg} denotes the district-by-grade fixed effects. The district-by-grade fixed effects restrict comparisons to the same grades within a school district. Specifically, they difference out the time-invariant variation in student achievement across grade levels within school districts as well as the time-invariant confounding variation in student achievement across school districts.

Although district-by-grade fixed effects alleviate the endogeneity problem caused by time-invariant confounders, omitting time-variant variables (e.g., student body composition) may still bias the estimates. To address this problem, we add district-grade-year measures of the gender and racial/ethnic composition of the student body as well as the proportion of students eligible for free/reduced-price lunch (X_{dgt}). Year fixed effects, λ_t, and cohort fixed effects, ϕ_{t-g}, are also included in the equation to control for both annual shocks that affect students statewide and cohort-specific shocks. To control for county-specific trends, we add linear time trends, $t \cdot \omega_c$, for each county. We weight observations using the test-taking population at the district-grade-year-subject level, a conventional weighting scheme (Levine and McKnight, 2021).

District-level analysis may overlook the variation in wildfire school closures within school districts. In large districts such as the Los Angeles Unified School District, geographic dispersion of schools can lead to variation in wildfire school closure days within a district-year cell. To take advantage of this variation, we repeat the analysis using the school-level test score data described above. The school-level analysis uses the same methodology but replaces district-grade level variables with school-grade level variables (test scores, closure days due to wildfires and other reasons, and gender and racial/ethnic composition) or school-level variables (proportion of students eligible for free/reduced-price lunch), depending on data availability. District-grade fixed effects are replaced with school-grade fixed effects, while county-specific linear time trends are also replaced with district-specific linear time trends. We use the following model to analyze the school-level data:

$$\begin{aligned} Score_{sgt} = & \alpha_0 + \Sigma_{k=-2}^{2} \beta_k Days\_Closure\_Fire_{sg(t+k)} + \\ & \Sigma_{k=-2}^{0} \eta_k Days\_Closure\_other_{sg(t+k)} + \\ & \gamma X_{sgt} + \mu_{sg} + \phi_{t-g} + \lambda_t + t \cdot \omega_d + \epsilon_{sgt} \end{aligned} \tag{2}$$

where all the variables are defined similarly as in Equation 1, except that the subscript s denotes school s.

One potential concern of our empirical strategy is that the number of wildfire school closure days may affect the number of other types of school closures. We test for this possibility by regressing the total number of other types of school closures on the number of wildfire school closure days using the following model:

$$\begin{aligned} Days\_Closure\_other_{s(t+k)} = & \alpha_0 + \Sigma_{k=-2}^{2} \beta_k Days\_Closure\_Fire_{s(t+k)} + \\ & \mu_s + \lambda_t + \epsilon_{st} \end{aligned} \tag{3}$$

Table A.I reports the results. The impacts of wildfire school closure days in the past two years on other types of school closure days are both statistically insignificant and small in magnitude in almost all cases. The only exception is that one wildfire school closure day in the previous year can increase other types of school closures in the current year by 0.007 days, which is unlikely to contaminate our main results. Therefore, we do not find evidence that the number of wildfire school closure days in our sample influences other types of school closure days.

3.1 Heterogeneity by gender and socioeconomic status

We also explore heterogeneity in the impact of school closures to identify student groups that may need extra support in the face of these disruptions. First, we examine heterogeneity by gender by estimating Equation 1 but replacing the dependent variable with the district-year-grade level gender achievement gap.

We then examine heterogeneity by district-level SES. We calculate the average proportion of free/reduced-price lunch students in district d between 2009 and 2017; divide the distribution of average proportions into terciles; and categorize districts in the analytic sample as having low, medium, or high SES. We estimate equation 1, replacing $Days\_Closure\_Fire_{dg(t+k)}$ with three interaction terms between (1) indicators of whether district d belongs to each of the three SES groups and (2) the number of closure days caused by wildfires (i.e., low SES x number of closure days, medium SES x number of closure days, high SES x number of closure days). We also run the same regression using school-level data and categorizing schools in the analytic sample as having low, medium, or high SES based on the average proportion of students eligible for free/reduced-price lunch between 2009 and 2017.

3.2 Exploring possible mechanisms of learning loss

School closures caused by wildfires may affect student outcomes through the loss of school time, financial losses, psychological trauma, or bad air quality. We examine whether two of these factors—the loss of school time and bad air quality caused by wildfires—are mechanisms through which wildfire school closures affect student learning in California.

Schooling environment and instructional time are important determinants of a child's academic success (Chabrier et al., 2016; Eble et al., 2021). To identify the effect of school time loss, we estimate Equation 3 but exclude areas that experienced large-scale wildfires or poor air quality. Specifically, we exclude school-year-grade cells that either specified bad air quality as the reason for the closure or experienced wildfires that burned more than 1,000 acres.[13] This leaves two groups of school-year cells: (1) those that did not suffer from school closures, and (2) those that experienced preemptive or precautionary school closures but did not encounter severe air pollution, financial losses, or psychological trauma. This restricted sample allows us to identify the impact of losing school time on test scores.

To explore whether air pollution caused by wildfires, measured by the wildfire-driven PM 2.5 concentrations, is one of the mechanisms driving learning loss, we estimate the following equation:

$$\begin{aligned} Score_{sgt} =& \alpha_0 + \Sigma_{k=-2}^{2} \beta_k Days\_Closure\_Fire_{sg(t+k)} \\ &+ \Sigma_{k=-2}^{0} \eta_k Days\_Closure\_other_{sg(t+k)} + \theta PM25_{st} \\ &+ \gamma X_{sgt} + \mu_{sg} + \phi_{t-g} + \lambda_t + t \cdot \omega_d + \epsilon_{sgt} \end{aligned} \tag{4}$$

where all the variables are the same as Equation 3, except an additional term $PM25_{st}$,

[13] As a point of comparison, the city of San Francisco has an area of about 30,016 acres and the Central Park in New York City has an area of about 843 acres.

which is the average PM 2.5 exposure driven by wildfire smoke of school s in year t. θ is the coefficient of interest. In addition, we also use the 75th, 90th, and 95th percentiles of wildfire smoke-drive PM 2.5 exposure as measures of air pollution. We also include weather controls, including temperature, precipitation, and wind speed at the school level as a robustness check in Section 4.4.[14]

One potential concern is that $PM25_{st}$ might be endogenous because families may self-select into locations with high/low air pollution. To address this concern, we employ the instrument variable (IV) method by using the variation in wind direction; this approach is similar to the one used by Deryugina et al. (2019). Specifically, we use yearly relative frequencies of NE, SE, and SW wind as instruments of $PM25_{st}$. The omitted category is the yearly relative frequencies of NW wind.

The first stage specification is:

$$\begin{aligned} PM25_{st} =& \Sigma_{g \in G} \Sigma_{b=0}^{2} \beta_{bg} 1[G_s = g] * WD_{st(9-12)}^{90b} \\ & + \Sigma_{k=-2}^{2} \beta_k Days\_Closure\_Fire_{sg(t+k)} \\ & + \Sigma_{k=-2}^{0} \eta_k Days\_Closure\_other_{sg(t+k)} \\ & + \gamma X_{sgt} + \mu_{sg} + \phi_{t-g} + \lambda_t + t \cdot \omega_d + \epsilon_{sgt} \end{aligned} \tag{5}$$

The excluded instrument variables are $1[G_s = g] * WD_{st}^{90b}$, where $1[G_s = g]$ is an indicator of whether school s is classified into geographical area g, and WD_{st}^{90b} is the frequency of NE (when b = 0), SE (when b = 1), and SW (when b = 2) wind of school s in year t. Other variables are defined in the same way as Equation 4.

The validity of the IV requires that (a) the IVs (wind directions) are sufficiently predictive of PM 2.5, and (b) these IVs only influence student test scores through altering PM

[14] We include yearly maximum and minimum temperatures as temperature controls, represented by indicators for deciles of maximum and minimum temperatures at the 5-digit zip-code level. We do the same for precipitation and wind speed.

2.5. Prior research has found that wind directions and PM 2.5 are correlated (Deryugina et al., 2019)—wind can transport pollution caused by wildfires to school locations. Table 2, which reports the estimates of first-stage Equation 5, also shows that these IVs are strong predictors of PM 2.5 levels in our focal sample. Further, the F-statistics in the IV regression results (Table 7) show that there is not a weak-IV bias problem. To verify condition (b), we need to assume that conditional on school-grade fixed effects (e.g., within a county), variation in wind direction frequencies across years is not correlated with other determinants of student outcomes.[15]

One challenge to measuring pollution induced by wildfires is that the PM 2.5 measure may reflect variations in local sources of air pollution other than wildfires. We alleviate this concern in two ways. First, as mentioned in Section 2, the PM 2.5 data in the current study directly measures wildfire-driven PM2.5, which improves the reliability of our measure of air pollution induced by wildfires. Second, following the idea of Deryugina et al. (2019), we divide the State of California into three geographic areas, as shown in Figure 2, and forcing Equation 5 to estimate a common effect of wind that carried air pollution from wildfires on the PM 2.5 measures within each of the geographic areas. As shown in Figure 2, within each geographic area, most wildfires, especially the most destructive ones, were located on one side of the schools. For example, in North California, wildfires mostly took place to the east of the schools. This is consistent with Column 3 of Table 2, which indicates a higher frequency of east winds increases air pollution levels. These estimates of the impacts of wind on air pollution are also consistent with Deryugina et al. (2019). Therefore, Equation 5 provides a reliable estimation of the effects of wind

[15]One potential concern is that wind directions also affect air pollution caused by reasons other than wildfire smoke, which may also affect test scores. As a robustness check, we estimate Equation 4 controlling for the yearly average PM 2.5 from all sources at the school level. In Section 4.4, we show that our estimates are robust to the inclusion of such controls.

directions that brought air pollution from wildfires.

4 Results

4.1 Impact of Wildfire School Closures on Test Scores

Columns 1 and 2 in Table 3 present the estimation results for Equation 1 using district-grade level data. The results show that wildfire school closures have a negative impact on both ELA and math scores during the concurrent year but not the following years (i.e., the coefficients on $Closure\ days_{t-1}$ and $Closure\ days_{t-2}$ are non-negative or indistinguishable from 0). Columns 3 and 4 show the parallel results using school-grade level data for the same group of students using Equation 3. The school-grade results show similar patterns as the district-grade results.[16] Our preferred specifications, shown in Columns 1 and 2, indicate that on average, one wildfire closure day in the current school year decreases both ELA and math scores by approximately 0.02 standard deviations relative to the scores of the national reference cohort in the same grade. One potential concern of our estimates is that the academic assessments were administered before the occurrence of wildfire school closures, which is a potential measurement error issue in test scores and may lead to attenuation biases. However, about 90% of the wildfire school closures in our sample happened before January, it is therefore unlikely that considering such a possibility can substantially affect our results. We also show in Section 4.4 that excluding wildfire school closures after December does not change our results.

Inspired by Herrmann and Rockoff (2012), we test whether the duration of closures matters for achievement loss, using school-level data. Specifically, we estimate Equation

[16]Because the district-level test score data is standardized at country-grade-subject level while the school-level test score data is standardized at the state level. Therefore, the magnitudes in Columns 1 and 2 are not directly comparable to those in Columns 3 and 4.

3 with dummies for four closure durations: 1 day, 2-3 days, 4-5 days, or more than 5 days (rather than the number of wildfire school closure days). The results (see Table 4) show that 1-day closures do not have a statistically significant impact on test scores. For ELA scores, wildfire school closures lasting 2-3 school days are the main drivers of the negative impact, while for math scores, closures lasting 4-5 days are the main drivers of the decrease in test scores. Wildfire school closures lasting for more than 5 days have a statistically significant impact on test scores, but the magnitude of the per-day effect (i.e., the coefficient divided by an integer larger than 5) is smaller than the magnitude of the per-day effect of closures lasting 2-5 days. One reason 2-5 day closures have a greater impact than longer closures may be that very long closures highlight the severity of the interruption in teaching and learning and thus are more likely to prompt schools to implement remediation measures.

4.2 Heterogeneity

To explore the heterogeneous effects of wildfire school closures by SES and gender, we categorize the schools/districts in the analytic sample as having low, medium, or high levels of SES (see Section 3). We then estimate Equations 1 and 3, replacing wildfire closure days with three interaction terms between (1) indicators of whether a district/school belongs to each of the three SES groups (upper, middle, or lower third) and (2) the number of closure days caused by wildfires. Table 5 reports the estimated coefficients on these interaction terms. The negative effects of closures on test scores are clearly concentrated among districts/schools with a high proportion of free/reduced-price lunch students; the negative effects are much smaller in magnitude among districts/schools with a middle or low proportion of free/reduced-price lunch students.

We use achievement gap data at the district-year-grade-subject level to detect whether wildfire school closures affected the gender achievement gap. The results in Table 6 show that we cannot reject the null hypothesis that school closures have no impact on the gender achievement gap. Notably, this analysis focuses on the gender achievement gap within districts, while the previous analysis of heterogeneity by SES identifies differences in the effect of wildfire school closures across districts.

4.3 Mechanisms

Wildfire school closures may influence student learning through several possible channels, including loss of school time, loss of family income, mental stress, and air pollution. This subsection explores two potential mechanisms: school time loss and air pollution.

To examine the impact of school time loss, we estimate Equation 3 excluding school-grade-year cells that encountered large-scale wildfires or closures due to extreme air pollution (as described in Section 3). Results in Columns 5 and 6 of Table 3 indicate that a loss of school time significantly decreases math test scores. The impact of lost time on ELA scores is not statistically significant but has the expected negative sign and the magnitude is within the 95% confidence interval of the estimate in Column 3. Given differences in sample sizes and compositions, these results should not be interpreted as the fraction of the total impacts estimated in Columns 3 and 4 that can be attributed to the loss of school time. Rather, these results provide clear-cut evidence that a reduction in school time harms test scores even without the other negative shocks associated with extremely large wildfires.

To explore whether school time loss had a greater effect on students with lower SES than on students with higher SES, we again apply the method described in Section 4.2 (i.e we replace the number of wildfire school closure days with interaction terms in the regressions) and exclude school-years that did experience large-scale fires or air pollution. The results in Table A.II show that the negative impact of school time loss is most intense among students with lower SES.

Next, we explore whether air pollution is another channel through which wildfires affect test scores. Table 7 presents the estimation results of Equation 4. As mentioned above, we use the average wildfire-driven PM 2.5 concentration as well as the 75th, 90th, and 95th percentiles of PM 2.5 concentration during the academic year as measures of air pollution.

Panel A in Table 7, which reports the ordinary least squares (OLS) estimates of the relationship between wildfire-driven PM2.5 and test scores, suggests that an increase in wildfire-driven PM 2.5 is associated with small decreases in test scores. However, the OLS results may be subject to the omitted variable bias issue. Thus, Panel B presents the corresponding IV estimates of the causal effects of wildfire-driven PM 2.5 on test scores. Here, the coefficients on PM 2.5 are substantially larger in magnitude (2-7.3 times) and have the expected negative sign, except specification 7. Our preferred specifications, in Columns 1 and 5, show that, on average, each 1-$\mu g/m^3$ increase in wildfire-driven PM 2.5 concentration decreases math scores by 0.038 SD and ELA scores by 0.010 SD. These magnitudes (i.e., the change in test scores per 1-$\mu g/m^3$ fluctuation in PM 2.5) are comparable to those found in other studies that explore the relationship between wildfire-driven PM 2.5 and student test scores (see Gilraine, 2020 for a study that finds a 0.03 SD impact on math and ELA scores in the Los Angeles Unified School District, and Ebenstein et al., 2016, for a study that finds a 0.006 SD impact on high school matriculation exams in Israel).

Last, we examine whether wildfire school closures affect school spending. Adjustments in expenditures can both reflect schools' remediation strategies in response to closures and be another mechanism through which closures impact learning. To explore such effects, we estimate Equation 1 using district-level per-pupil expenditure as the dependent variable. Column 1 in Table 8 presents the impact of closures on total spending, while Columns 2 - 10 show the impact on 9 distinct categories of spending. The magnitudes of the coefficients can be interpreted approximately as proportional changes. Results in Column 1 show that we cannot reject the null hypothesis that wildfire school closures have no impact on total spending. When spending is broken into specific categories, the results show that school closures have no statistically significant impact on 6 of the 9 categories, including instruction, psychological services, and health services, which prior studies have found are associated with test score outcomes (Greenwald et al., 1996; Lafortune et al., 2018; Baron, 2022) and/or which are important for post-wildfire remediation services.

Wildfire school closures have a negative effect on per-pupil instruction administration spending (Column 3) and positive effects on ancillary services (Column 7) and maintenance & operation (Column 9).[17] However, given that few studies have found that these three categories of spending are associated with student test scores, it is unlikely that adjustments in these spending categories are responsible for the identified decreases in test scores. While such spending adjustments may be necessary for school operations, they are unlikely to be used for remediation strategies that address learning loss. The small magnitudes of the changes in per-pupil spending in these categories strengthen this argument—the estimated coefficients suggest a -$31 annual change (-0.083*293.79, per-pupil per-closure day) in instruction administration spending, a $8 annual change (0.077*99.89, per-pupil per-closure day) in ancillary services spending, and a $23 annual change (0.047*489.21, per-pupil per-closure day) in maintenance spending.

4.4 Robustness Analysis

We conduct several robust analyses to check (1) whether there exist pre-trends in test scores, (2) whether student migration after wildfires pollutes our estimates, (3) whether the possibility that tests may be taken prior to the occurrence of wildfires within an academic year affects our results, (4) whether estimated effects of school time loss are robust

[17]Instruction administration refers to activities that assist instructional staff within the planning, development, and evaluation of learning experiences for students. Ancillary services include co-curricular activities and athletics that are not directly related to test scores.

to using different criteria for identifying large-scale wildfires, (5) whether our estimates of the impact of smoke exposure are sensitive to the inclusion of weather controls and all-source air pollution controls, and (6) whether the estimated effects of wildfire school closures are biased due to the negative weights assigned to groups and periods in the two-way fixed effects model.

The validity of the estimates depends on whether there were pre-trends in test scores that can be mistakenly attributed to the effects of school closures. In our setting, because school closures cannot affect prior test scores, testing for pre-trends is equivalent to testing whether the coefficients on $Closure\ days_{t+1}$ and $Closure\ days_{t+2}$ in Equations 1 and 3 are indistinguishable from 0. The results in Figure 5 indicate that we cannot reject the null hypothesis that no pre-trends exist, either for our district-level or school-level data.

Wildfires may lead to migration of students' families, which changes the student body composition of schools and in turn affects their average test scores. If this mobility is random in terms of student characteristics, then the results are unbiased. However, it is possible that some types of families are more likely to move than others, for example, risk-averse families who do not want to experience wildfire closures again may be more likely to move to a new area.

To address this concern, we estimate whether wildfire school closures are associated with shifts in enrollment. Table A.III shows that the association between wildfire school closures and enrollment is positive but not statistically significant. On average, 1 wildfire closure day increases the school-grade level enrollment of the current year by only 0.3 students (the average enrollment in the sample is over 100). Wildfire school closures that occurred in the previous two years also have limited (i.e., small in magnitude and/or not statistically significant) associations with enrollment. Therefore, the results indicate that school closures caused by wildfires do not change total enrollment.

We also examine whether wildfire school closures are associated with changes in the racial/ethnic composition of the focal schools. The results in Table A.IV show that although wildfire school closures are associated with changes in student body racial/ethnic composition, the magnitudes of these shifts are quite small: on average, 1 wildfire closure day is associated with 0.06 - 0.46 percentage point change in the proportion of each of the four largest racial groups (Asian, Black, Hispanic, and White). These results translate into 0.7% - 2.8% changes in their mean proportions. Similarly, closures that occurred in the previous two years have limited (i.e., small in magnitude and/or not statistically significant) associations with changes in student body racial/ethnic composition.

In sum, wildfire school closures are associated with changes in student composition. However, these changes are so small in magnitude that they do not change the conclusions based on the main analyses. In addition, we control for both student racial/ethnic composition and the proportion of students eligible for free/reduced-price lunch throughout the analyses, which would alleviate the impact of any changes in student body composition. Further, calculations using the estimated coefficients on student racial/ethnic composition in Equation 3 show that changes in student body composition result in a much smaller impact on test scores than the estimated total impact of school closures on test scores.[18]

Another concern about the validity of our results is that tests in California can be scheduled between January and June, meaning that wildfires in the school year t may happen after the test in that school year. We test for this possibility by running the regression using Equation 3 and counting only wildfire school closures that happened before

[18]The regression results in Columns 1 and 2 of Table 3, for example, show that the coefficients on the percentage of Asian, Black, Hispanic, and White students are 0.99, -0.66, -1.03, and 0.32, respectively, for ELA scores and 1.2, -0.64, -1.25, and 0.20, respectively, for math scores. Thus, changes in racial/ethnic composition lead to a -0.003 SD (0.99*-0.059% - 0.66*-0.101% - 1.03*.158% + 0.32*-0.458%) change in ELA scores and a -0.003 SD (1.2*-0.058 - 0.64*-0.111 - 1.25*.16 + 0.2*-0.461) change in math scores. These magnitudes are only about 15 % of our estimated effect of per-day wildfire school closure.

January (within a school year). Results are shown in Columns (7) and (8) of Table 3. The estimates are almost identical to those in Columns (3) and (4), meaning that our results are robust to the possibility that tests may be taken prior to wildfires.

In the mechanism analysis, we identify the effect of school time loss by excluding observations that either specified poor air quality as the reason for closure or experienced wildfires that burned more than 1,000 acres. Here, we use alternative criteria for defining large-scale wildfires and check whether the estimated impact of school time loss remains stable. Results are shown in Table A.V. When large-scale wildfires are defined as ones that burned more than 500 acres or more than 5,000 acres, the estimated effects of wildfire school closures are quite similar to the original estimates in Table 3. When large-scale wildfires are defined more narrowly as ones that burned more than 15,000 acres, the estimated effects of wildfire school closures become slightly larger in magnitude for both ELA and math scores. This increase in the negative impact of wildfires likely occurs because wildfires that burn 5,000 - 15,000 acres can lead to economic loss and/or psychological trauma. Hence, the estimated effects of these wildfire school closures on test scores reflect the effects of not only school time loss, but also economic loss and psychological trauma. In sum, these estimated effects have similar magnitudes as the main analyses. We therefore conclude that the estimated effect of school time loss is robust to different criteria for identifying large-scale wildfires.

In addition, we test whether the inclusion of weather controls affects our results. Following Deryugina et al. (2019), we estimate Equation 4 with weather controls, including minimum and maximum temperature, precipitation, and wind speed. Table A.VI reports the results, which are similar to our main results in Table 7. We also control for all-source PM 2.5 to address the potential concern that our IVs (wind direction) may affect air pollution resulting from reasons other than wildfires that also influence student test scores, a situation where exclusion restriction may be violated. Results in Table A.VII show that controlling for the overall PM 2.5 does not affect our results. Therefore, our results are robust to these additional controls.

Numerous studies have shown that treatment effect estimates from two-way fixed effects models can be biased due to negative weights assigned to groups and periods when the treatment is implemented at different times and the treatment effect is heterogeneous (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway et al., 2021; Sun and Abraham, 2021). Because wildfire school closures occurred in different years across schools, we conduct several robustness analyses using the school-level data to check if our estimated effects of wildfire school closures suffer bias from the negative weights.

Following De Chaisemartin and d'Haultfoeuille (2020), we calculate the weights attached to the regressions with the *twowayfeweights* Stata package. We find that only 1.1% of the weights in the school-level sample are negative and the sum of the negative weights is -0.003. In addition, we compute the adjusted estimator developed by De Chaisemartin and d'Haultfoeuille (2020), which is valid even when there is heterogeneity in treatment effects, using the *did_multiplegt* Stata package. The adjusted estimates of the effects of wildfire school closures are -0.02 (standard error = 0.002) for math and -0.02 (standard error = 0.005) for ELA, which are quite close to our original estimates. Therefore, the negative weights issue in two-way fixed effect models is likely not a concern in the current analysis, and our estimated effects of wildfire school closures are not substantially biased by the negative weights issue.

5 Conclusion

Using administrative data from California, we investigate the effect of unexpected school closures due to wildfires on student achievement. We find that exposure to wildfire school closures negatively affects student test scores. The overall negative impact of per-day closure on both ELA and math test scores is approximately 0.02 SD. Such effects could develop into major learning losses, given that an increasing number of schools are experiencing closures (at least in California, see Figure 1) and disastrous events are occurring at an increasing frequency (Burke et al., 2021; Seneviratne et al., 2012). Moreover, the COVID-19 pandemic prompted extended school closures on a global scale.

We also explore heterogeneity in the effects of wildfire school closures on student test scores by SES. The effects of wildfire school closures are larger for students in schools/districts with a high proportion of free/reduced-price lunch students (i.e., students with lower SES). The heterogeneity identified herein is consistent with the existing literature (Goodman, 2014; Groppo and Kraehnert, 2017). Given that the negative effects of school closures are transitory, the current results suggest a worsening disparity in student achievement in the short term.

Further, we explore the channels through which wildfire school closures affect student test scores. First, we isolate the impact of lost school time from the impact of air pollution, financial loss, and psychological trauma, and find that the loss of school time did cause a reduction in test scores. The negative impact is stronger for students from schools with a high proportion of free/reduced price lunch students. While Groppo and Kraehnert (2017) find that in a developing country, negative impacts on low-income students are driven by a loss of family income, our findings suggest that low-SES students suffer more intensely than higher income students from a similar loss of school time.

In addition, our results show that wildfire school closures affect test scores, especially math scores, through their impact on air pollution, measured by the wildfire-driven PM 2.5 concentration. By exploiting variation in air pollution driven by changes in wind directions, we show that on average, each 1-$\mu g/m^3$ increase in PM 2.5 concentration decreases math scores by 0.038 SD and ELA scores by 0.010 SD.

Taken together, our findings show that wildfire school closures have a negative impact on student achievement. Further, there is substantial heterogeneity in this impact by socioeconomic status: students from low-SES school districts are particularly vulnerable to the negative effects of school closures caused by disastrous events. We also show that the loss of school time and air pollution caused by wildfires are two important mechanisms through which wildfire school closures affect student achievement.

These findings highlight the need for policymakers to develop and implement plans to reduce learning loss in the aftermath of disasters and ameliorate resulting increases in inequality. The findings reinforce other efforts in this area, such as the climate action plan released by the U.S. Department of Education in 2021, which focused on preparing the country to confront the rapidly changing climate and its impacts, as well as the work of Park et al. (2020), which suggested that extreme weather disproportionately affects students of color, leading to increases in inequality in the United States. Thus, the current findings should inform the design of any resulting remediation policies. By identifying the impact of unexpected school closures, the mechanisms driving this impact, and the resulting inequality, this study sheds light on the optimal designs for targeted remediation policies.

References

Adhvaryu, A., Kala, N., and Nyshadham, A. (2022). Management and shocks to worker productivity. *Journal of Political Economy*, 130(1):1–47.

Aguilar-Gomez, S., Dwyer, H., Graff Zivin, J., and Neidell, M. (2022). This is air: The "nonhealth" effects of air pollution. *Annual Review of Resource Economics*, 14:403–425.

An, X., Gabriel, S. A., and Tzur-Ilan, N. (2023). The effects of extreme wildfire and smoke events on household financial outcomes. *Available at SSRN 4353113*.

Angrist, J. D. and Krueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings? *The Quarterly Journal of Economics*, 106(4):979–1014.

Angrist, J. D., Pathak, P. A., and Walters, C. R. (2013). Explaining charter school effectiveness. *American Economic Journal: Applied Economics*, 5(4):1–27.

Aucejo, E. M., French, J., Araya, M. P. U., and Zafar, B. (2020). The impact of covid-19 on student experiences and expectations: Evidence from a survey. *Journal of public economics*, 191:104271.

Baron, E. J. (2022). School spending and student outcomes: Evidence from revenue limit elections in wisconsin. *American Economic Journal: Economic Policy*, 14(1):1–39.

Betthäuser, B. A., Bach-Mortensen, A. M., and Engzell, P. (2023). A systematic review and meta-analysis of the evidence on learning during the covid-19 pandemic. *Nature Human Behaviour*, 7(3):375–385.

Borgschulte, M., Molitor, D., and Zou, E. Y. (2022). Air pollution and the labor market: Evidence from wildfire smoke. *Review of Economics and Statistics*, pages 1–46.

Burke, M., Childs, M. L., De la Cuesta, B., Qiu, M., Li, J., Gould, C. F., Heft-Neal, S., and Wara, M. (2023). Wildfire influence on recent us pollution trends. Technical report, National Bureau of Economic Research.

Burke, M., Driscoll, A., Heft-Neal, S., Xue, J., Burney, J., and Wara, M. (2021). The changing risk and burden of wildfire in the united states. *Proceedings of the National Academy of Sciences*, 118(2):e2011048118.

Burkhardt, J., Bayham, J., Wilson, A., Carter, E., Berman, J. D., O'Dell, K., Ford, B., Fischer, E. V., and Pierce, J. R. (2019). The effect of pollution on crime: Evidence from data on particulate matter and ozone. *Journal of Environmental Economics and Management*, 98:102267.

Callaway, B., Goodman-Bacon, A., and Sant'Anna, P. H. (2021). Difference-in-differences with a continuous treatment. *arXiv preprint arXiv:2107.02637*.

Chabrier, J., Cohodes, S., and Oreopoulos, P. (2016). What can we learn from charter school lotteries? *Journal of Economic Perspectives*, 30(3):57–84.

Chetty, R., Friedman, J. N., Hendren, N., Stepner, M., et al. (2020). Real-time economics: A new platform to track the impacts of covid-19 on people, businesses, and communities using private sector data. *NBER Working Paper*, 27431.

Chetty, R., Friedman, J. N., and Rockoff, J. E. (2014). Measuring the impacts of teachers ii: Teacher value-added and student outcomes in adulthood. *American economic review*, 104(9):2633–79.

Childs, M. L., Li, J., Wen, J., Heft-Neal, S., Driscoll, A., Wang, S., Gould, C. F., Qiu, M., Burney, J., and Burke, M. (2022). Daily local-level estimates of ambient wildfire smoke pm2. 5 for the contiguous us. *Environmental Science & Technology*, 56(19):13607–13621.

De Chaisemartin, C. and d'Haultfoeuille, X. (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review*, 110(9):2964–96.

Deryugina, T., Heutel, G., Miller, N. H., Molitor, D., and Reif, J. (2019). The mortality and medical costs of air pollution: Evidence from changes in wind direction. *American Economic Review*, 109(12):4178–4219.

Deuchert, E. and Felfe, C. (2015). The tempest: Short-and long-term consequences of a natural disaster for children s development. *European Economic Review*, 80:280–294.

Ebenstein, A., Lavy, V., and Roth, S. (2016). The long-run economic consequences of high-stakes examinations: Evidence from transitory variation in pollution. *American Economic Journal: Applied Economics*, 8(4):36–65.

Eble, A., Frost, C., Camara, A., Bouy, B., Bah, M., Sivaraman, M., Hsieh, P.-T. J., Jayanty, C., Brady, T., Gawron, P., et al. (2021). How much can we remedy very low learning levels in rural parts of low-income countries? impact and generalizability of a multi-pronged para-teacher intervention from a cluster-randomized trial in the gambia. *Journal of Development Economics*, 148:102539.

Fuller, S. C. (2013). *The effects of natural disasters on birth and school outcomes of children in North Carolina*. PhD thesis, Duke University.

Gilraine, M. (2020). Air filters, pollution and student achievement. *Annenberg Institute at Brown University*.

Goodman, J. (2014). Flaking out: Student absences and snow days as disruptions of instructional time. Technical report, National Bureau of Economic Research.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277.

Greenwald, R., Hedges, L. V., and Laine, R. D. (1996). The effect of school resources on student achievement. *Review of educational research*, 66(3):361–396.

Groppo, V. and Kraehnert, K. (2017). The impact of extreme weather events on education. *Journal of Population Economics*, 30(2):433–472.

Hansen, B. (2011). School year length and student performance: Quasi-experimental evidence. *Available at SSRN 2269846*.

Hanushek, E. A. (1986). The economics of schooling: Production and efficiency in public schools. *Journal of economic literature*, 24(3):1141–1177.

He, G., Liu, T., and Zhou, M. (2020). Straw burning, pm2. 5, and death: Evidence from china. *Journal of Development Economics*, 145:102468.

He, J., Liu, H., and Salvo, A. (2019). Severe air pollution and labor productivity: Evidence from industrial towns in china. *American Economic Journal: Applied Economics*, 11(1):173–201.

Herrera-Almanza, C. and Cas, A. (2020). Mitigation of long-term human capital losses from natural disasters: Evidence from the philippines. *The World Bank Economic Review*.

Herrmann, M. A. and Rockoff, J. E. (2012). Worker absence and productivity: Evidence from teaching. *Journal of Labor Economics*, 30(4):749–782.

Jackson, C. K., Johnson, R. C., and Persico, C. (2015). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *The Quarterly Journal of Economics*, 131(1):157–218.

Kuhfeld, M., Soland, J., Tarasawa, B., Johnson, A., Ruzek, E., and Liu, J. (2020). Projecting the potential impacts of covid-19 school closures on academic achievement.

La Nauze, A. and Severnini, E. R. (2021). Air pollution and adult cognition: evidence from brain training. Technical report, National Bureau of Economic Research.

Lafortune, J., Rothstein, J., and Schanzenbach, D. W. (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics*, 10(2):1–26.

Levine, P. B. and McKnight, R. (2021). Exposure to a school shooting and subsequent well-being. Technical report, National Bureau of Economic Research.

Marcotte, D. E. (2007). Schooling and test scores: A mother-natural experiment. *Economics of Education Review*, 26(5):629–640.

Marcotte, D. E. and Hemelt, S. W. (2008). Unscheduled school closings and student performance. *Education Finance and Policy*, 3(3):316–338.

Monarrez, T., Kisida, B., and Chingos, M. (2022). The effect of charter schools on school segregation. *American Economic Journal: Economic Policy*, 14(1):301–340.

Park, R. J., Goodman, J., Hurwitz, M., and Smith, J. (2020). Heat and learning. *American Economic Journal: Economic Policy*, 12(2):306–39.

Paudel, J. and Ryu, H. (2018). Natural disasters and human capital: The case of nepal's earthquake. *World Development*, 111:1–12.

Pullabhotla, H. K. and Souza, M. (2022). Journal of environmental economics and management. *Journal of Environmental Economics and Management*, 115:102723.

Reardon, S. F., Kalogrides, D., and Ho, A. D. (2017). Linking us school district test score distributions to a common scale. cepa working paper no. 16-09. *Stanford Center for Education Policy Analysis*.

Roth, S. (2016). The contemporaneous effect of indoor air pollution on cognitive performance: Evidence from the uk.

Sacerdote, B. (2012). When the saints go marching out: Long-term outcomes for student evacuees from hurricanes katrina and rita. *American Economic Journal: Applied Economics*, 4(1):109–35.

Sandler, D. and Sandler, R. (2013). Multiple event studies in public finance and labor economics: A simulation study with applications. *Available at SSRN 2341638*.

Seneviratne, S., Nicholls, N., Easterling, D., Goodess, C., Kanae, S., Kossin, J., Luo, Y., Marengo, J., McInnes, K., Rahimi, M., et al. (2012). Changes in climate extremes and their impacts on the natural physical environment.

Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Tables

TABLE 1
Summary Statistics

	District-Level: (1) Full Sample	District-Level: (2) Closure Sample	School-Level: (3) Full Sample	School-Level: (4) Closure Sample
Panel A: Test Scores				
Math score	-0.23	-0.26	0.01	-0.03
ELA score	-0.18	-0.19	0.02	0.05
Panel B: Demographics				
Free/Reduced price lunch	0.56	0.55	0.60	0.55
Asian	0.08	0.05	0.08	0.05
Black	0.04	0.05	0.06	0.05
Hispanic	0.48	0.36	0.51	0.40
White	0.39	0.53	0.27	0.42
Panel C: Closure Days				
Closure days	0.02	2.40	0.02	2.83
Obs. (District-grade-year in Math)	25,827	1,248	-	-
Obs. (District-grade-year in ELA)	32,846	1,628	-	-
Obs. (School-grade-year in Math)	-	-	190,253	9,294
Obs. (School-grade-year in ELA)	-	-	201,869	9,980

TABLE 2
Impact of Wind Directions on Wildfire Smoke Exposure

	(1) South California	(2) Middle California	(3) North California
Northeast	-.118*	-.070	.523***
	(.071)	(.072)	(.104)
Southeast	.024	.205***	.446***
	(.072)	(.065)	(.116)
Southwest	.225***	.546***	-.137*
	(.058)	(.073)	(.078)
Mean Dependent Var.	2.542	3.053	3.030
Observations	45,302	54,732	67,880
R-squared	0.636	0.644	0.587

Notes: This table reports the first-stage results of the two-stage least squares regression, or the effect of variations in frequencies of wind directions on PM 2.5 driven by wildfire smoke. All regressions include school-x-grade fixed effects, cohort fixed effects, year fixed effects, and school-district linear time trends. Other controls include school-grade level student race and gender composition, school level percentage of students eligible for free/reduced price lunch, and closure days due to various reasons. Estimates are weighted by school-year-grade enrollments. Robust standard errors clustered at the school level are reported in parenthesis; *** p<0.01, ** p<0.05, * p<0.1

TABLE 3
Impact of School Closures on Test Score

	District-Level: Full Sample		School-Level: Full Sample		School-Level: Exclude Large-scale Fires		School-Level: Closures Before Jan.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ELA	Math	ELA	Math	ELA	Math	ELA	Math
Closure days	-.021***	-.019***	-.010**	-.010*	-.006	-.012**	-.011**	-.010*
	(.005)	(.006)	(.005)	(.006)	(.005)	(.006)	(.005)	(.006)
$Closure\ days_{t-1}$	.000	.002	.007**	.017***	.009***	.018***	.007**	.017***
	(.002)	(.004)	(.003)	(.004)	(.003)	(.004)	(.003)	(.004)
$Closure\ days_{t-2}$	-.001	.001	.004**	.012***	.005**	.012***	.004**	.012***
	(.002)	(.003)	(.002)	(.003)	(.002)	(.003)	(.002)	(.003)
Observations	32,846	25,827	201,869	190,253	191,049	179,912	201,869	190,253
R-squared	.961	.936	.917	.875	.918	.876	.917	.875

Notes: This table reports the effect of school closures on student achievements. All regressions include cohort fixed effects and year fixed effects. Columns (1) and (2) include district-x-grade fixed effects, race and gender composition, free/reduced price lunch percentage, closures days due to reasons other than wildfires, and county linear time trends. Columns (3) - (6) include school-x-grade fixed effects, race and gender composition, school level free/reduced price lunch percentage, closures days due to reasons other than wildfires, and district linear time trends. In Column (7) and (8), we only include school closures before January. Robust standard errors clustered at school level are reported in parenthesis; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 4
School Closure Duration and Learning Loss

	(1) ELA	(2) Math
Wildfire closure, 1 day	.012	-.010
	(.028)	(.033)
Wildfire closure, 1 - 3 days	-.069**	-.006
	(.024)	(.028)
Wildfire closure, 3 - 5 days	-.022	-.094**
	(.036)	(.033)
Wildfire closure, more than 5 days	-.052	-.075
	(.075)	(.087)
Observations	201,869	190,253
R-squared	.917	.875

Notes: This table reports the effect of school closures on student achievements. Both regressions include school-x-grade fixed effects, cohort fixed effects, year fixed effects, and school-district linear time trends. Other controls include school-grade level race and gender composition, school level free/reduced price lunch percentage, and closures days due to reasons other than wildfires. Robust standard errors clustered at school level are reported in parenthesis; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE 5
Impact of School Closures by
Proportion of Free/Reduced-price Lunch (FRPL) Students

		School-level		District-level	
		(1)	(2)	(3)	(4)
		ELA	Math	ELA	Math
Closure	* High FRPL	-.032***	-.044***	-.055***	-.050***
days		(.009)	(.011)	(.015)	(.015)
	* Medium FRPL	-.004	-.002	-.014**	-.011
		(.007)	(.007)	(.006)	(.008)
	* Low FRPL	-.009	.002	-.019	-.028
		(.007)	(.009)	(.010)	(.016)
Observations		200,713	189,309	32,846	25,827
R-squared		.917	.875	.961	.936

Notes: This table reports the effect of school closures on student achievement by the proportion of students eligible for free/reduced-price lunch at districts/schools. All regressions include cohort fixed effects and year fixed effects. Columns 1 and 2 controls for district-x-grade fixed effects, district-grade level student race and gender composition, the percentage of students eligible for free/reduced price lunch, closures days due to reasons other than wildfires, and county linear time trends. Estimates are weighted by district-year-grade enrollments. Columns 3 - 6 include school-x-grade fixed effects, school-grade level student race and gender composition, school level percentage of students eligible for free/reduced price lunch, closures days due to reasons other than wildfires, and district linear time trends. Estimates are weighted by school-year-grade enrollments. Robust standard errors clustered at district level for columns 1 and 2 and at school level for columns 3 - 4 are reported in parenthesis.

TABLE 6
Impact of School Closures on Gender Achievement Gap

	(1) ELA	(2) Math
Closure days	-.001	.004
	(.005)	(.004)
Closure days$_{t-1}$	.000	.000
	(.001)	(.001)
Closure days$_{t-2}$	.001	.001
	(.001)	(.001)
Observations	27253	21401
R-squared	.427	.439

Notes: This table reports the effect of school closures on district-grade level gender achievement gap. Both regressions include district-x-grade fixed effects, year fixed effects, cohort fixed effects, and controls for district-grade level student race and gender composition, the percentage of students eligible for free/reduced price lunch, and county linear time trends. The number of observations is smaller because some district-grade-year cells did not report gender achievement gap. Estimates are weighted by district-year-grade enrollments. Robust standard errors clustered at district level are reported in parenthesis.

TABLE 7
Impact of Air Pollution Caused by Wildfires in School Closures

	ELA				Math			
	(1) Mean	(2) 75th PCTL	(3) 90th PCTL	(4) 95th PCTL	(5) Mean	(6) 75th PCTL	(7) 90th PCTL	(8) 95th PCTL
	Panel A: OLS							
Closure days	-.010**	-.010**	-.010**	-.009**	-.009*	-.010**	-.009*	-.009*
	(.004)	(.004)	(.004)	(.004)	(.005)	(.005)	(.005)	(.005)
$Wildfire\_PM$ 2.5 ($\mu g/m^3$)	-.004***	-.003***	-.001***	-.001***	-.009***	-.006***	-.003***	-.001***
	(.001)	(.001)	(.000)	(.000)	(.001)	(.001)	(.000)	(.000)
Observations	195,392	195,392	195,392	195,392	184,304	184,304	184,304	184,304
	Panel B: IV Estimates							
Closure days	-.009**	-.009**	-.004	-.010**	-.004	-.006	.004	-.007
	(.004)	(.004)	(.005)	(.005)	(.005)	(.005)	(.006)	(.006)
$Wildfire\_PM$ 2.5 ($\mu g/m^3$)	-.010	-.011***	-.009***	-.001	-.038***	-.039***	-.022***	-.002
	(.007)	(.004)	(.003)	(.001)	(.008)	(.006)	(.003)	(.002)
Cragg-Donald Wald F-statistic	296.830	312.424	199.050	330.998	278.214	280.662	196.188	297.756
Observations	195,392	195,392	195,392	195,392	184,304	184,304	184,304	184,304

Notes: This table reports OLS and IV estimates of the effect of wildfire-driven PM 2.5 on student achievement. Columns (1) and (5) use yearly wildfire-driven PM 2.5 exposure as a measure of air pollution; columns (2) and (6) use the 75th percentile of this measure; similarly, columns (3) and (7) use the 90th percentile and column (4) and (8) use the 95th percentile of this measure. All regressions control for cohort fixed effects, year fixed effects, school-x-grade fixed effects, school level race and gender composition, percentage of students eligible for free/reduced-price lunch, closures days due to reasons other than wildfires, average PM 2.5 concentration between January and August, and district linear time trends. Estimates are weighted by school-year-grade enrollments. Robust standard errors clustered at the school level are reported in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

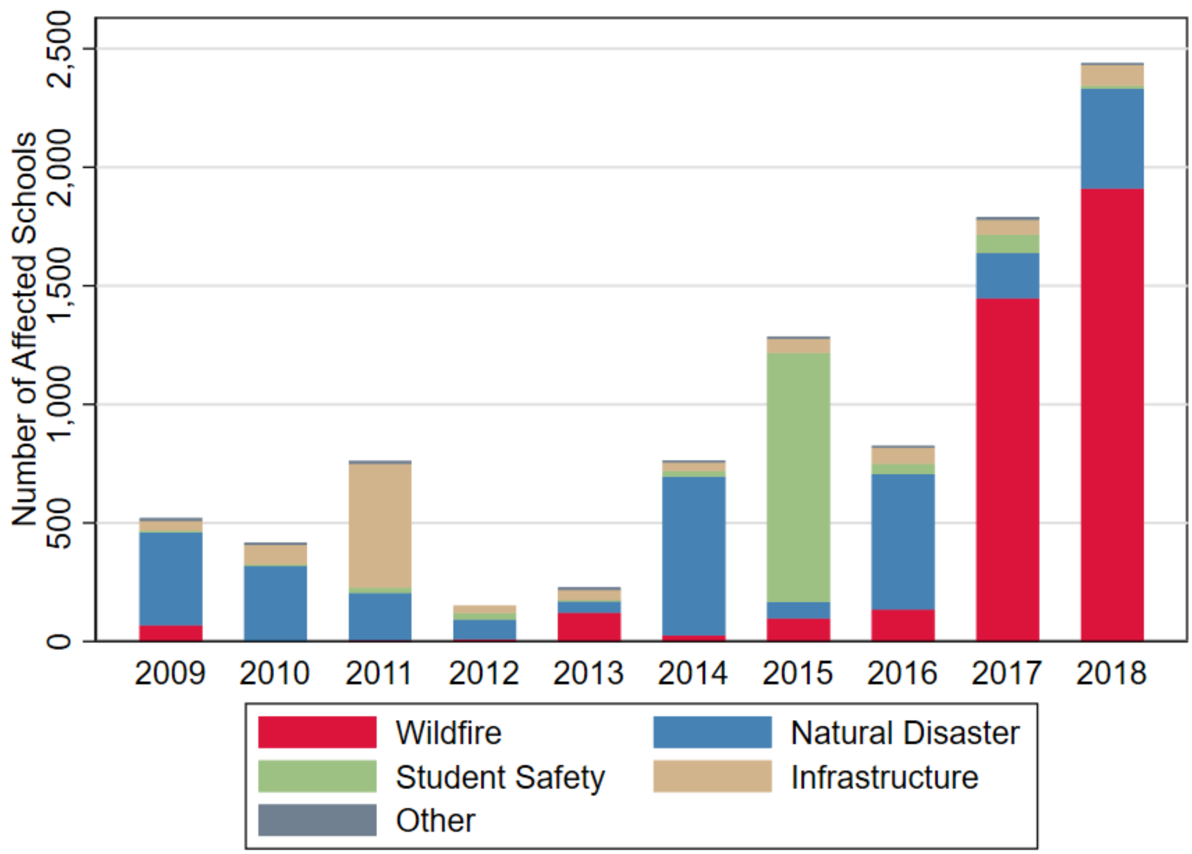
TABLE 8
Impact of School Closures on School Spending

	(1) Total Spending	(2) Instruction Spending	(3) Instruction Admin.	(4) School Admin.	(5) Guidance & Counseling
Closure days	-.000	-.001	-.083***	.005	.002
	(.006)	(.005)	(.028)	(.006)	(.018)
Closure days$_{t-1}$	-.001	.005	-.035	.006	.000
	(.004)	(.003)	(.023)	(.005)	(.024)
Closure days$_{t-2}$	-.002	.003	-.014	.003	-.014
	(.005)	(.004)	(.015)	(.006)	(.025)
Mean per student ($)	14,295.61	5,176.87	374.30	727.82	165.28
Observations	5,950	5,945	4,634	5,950	4,786
	(6) Psychological Services	(7) Health Services	(8) Ancillary Services	(9) Maintenance & Operations	(10) Facilities Acquisition
Closure days	-.013	.005	.077***	.047***	-.054
	(.015)	(.015)	(.027)	(.017)	(.087)
Closure days$_{t-1}$	.006	-.002	.028	.028	-.068
	(.010)	(.015)	(.032)	(.018)	(.042)
Closure days$_{t-2}$	.009	.012	-.038	.003	-.017
	(.009)	(.014)	(.050)	(.019)	(.038)
Mean per student ($)	120.74	106.45	99.89	489.21	1161.70
Observations	4,789	5,337	1,758	5,010	5,367

Notes: This table reports the effect of school closures on school spending. The dependent variables are the natural log of per-pupil spending. All regressions control for district fixed effects, year fixed effects, district-grade level student race composition, the percentage of students eligible for free/reduced price lunch, and closure days due to reasons other than wildfires. The number of observations is different from each other because school districts did not report all categories of school spending. For each regression, we exclude observations that reported 0 in the outcome variable. Robust standard errors clustered at school-district level are reported in parenthesis.

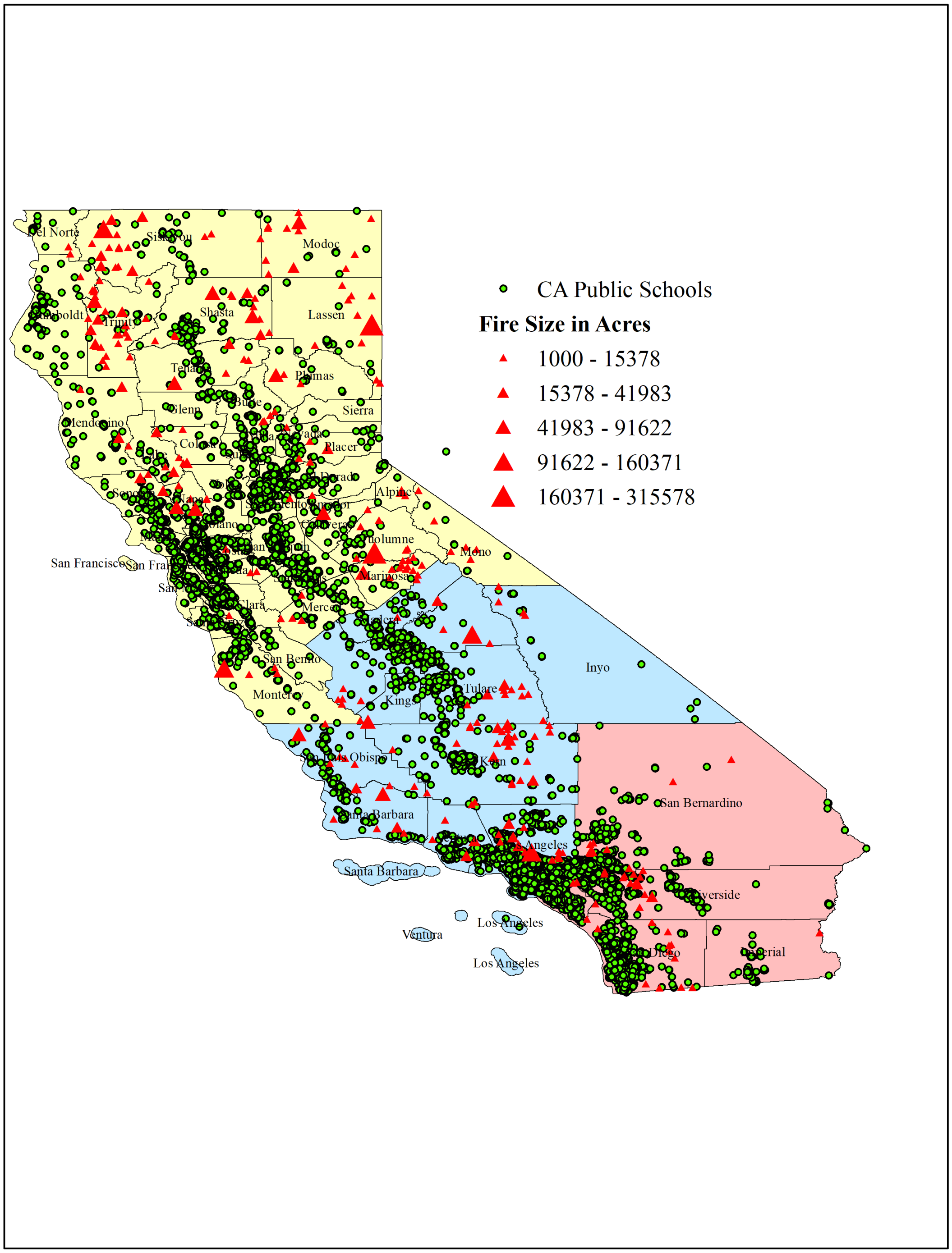
Figures

FIGURE 1
Number of schools impacted by school closures due to listed causes
in California



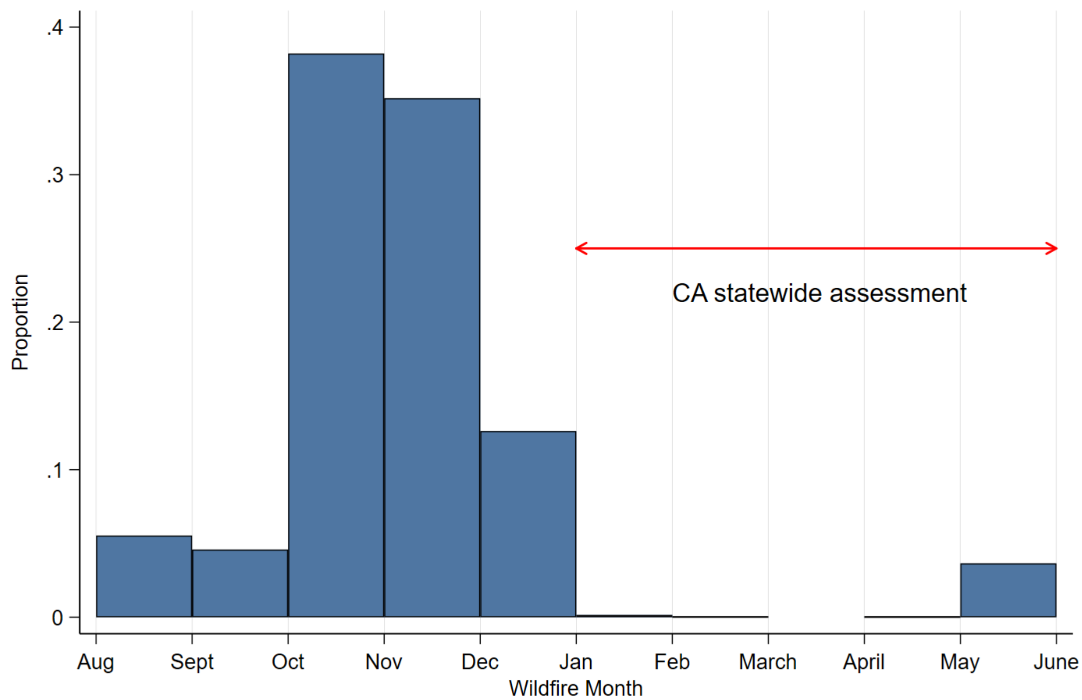
Notes: This figure shows the number of schools affected by wildfires, natural disasters & weather, student safety reasons, infrastructure, and other reasons between 2009 to 2018.

FIGURE 2
Location of Schools and Wildfires



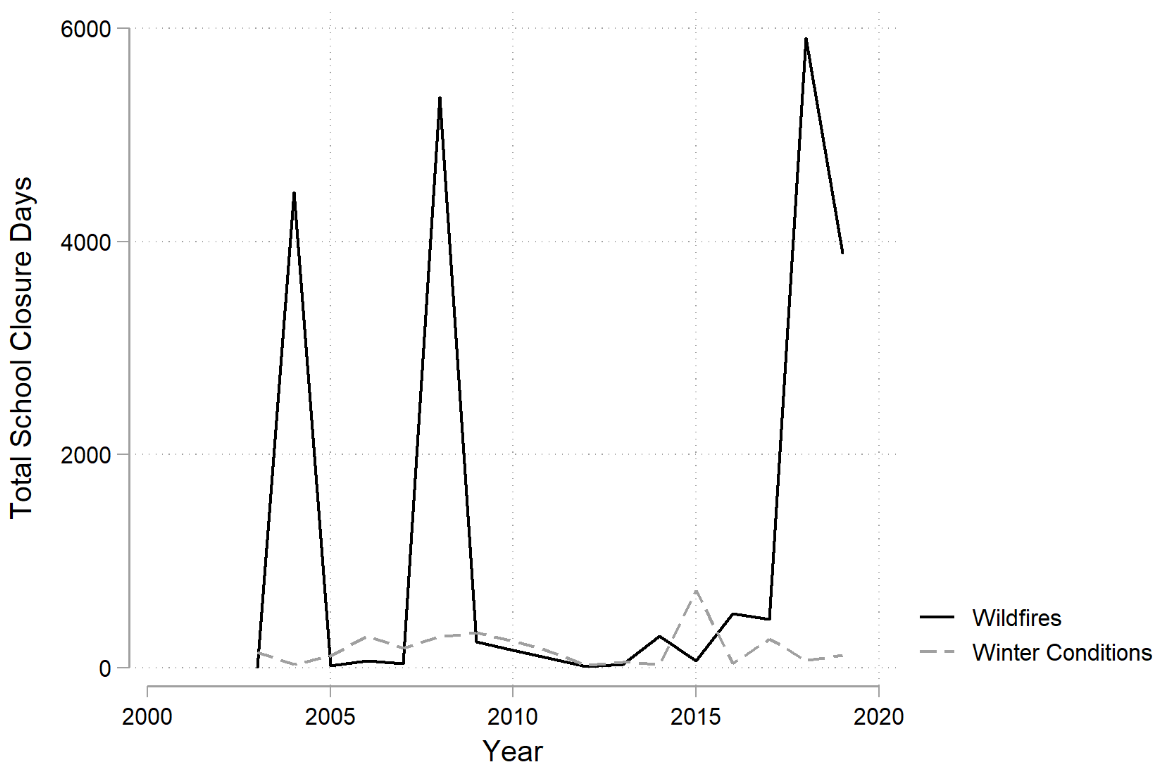
Notes: This figure shows the location of wildfires that burnt at least 1,000 acres between 2009 and 2017 in California.

FIGURE 3
Timing of Wildfire School Closures and CA Academic Assessment



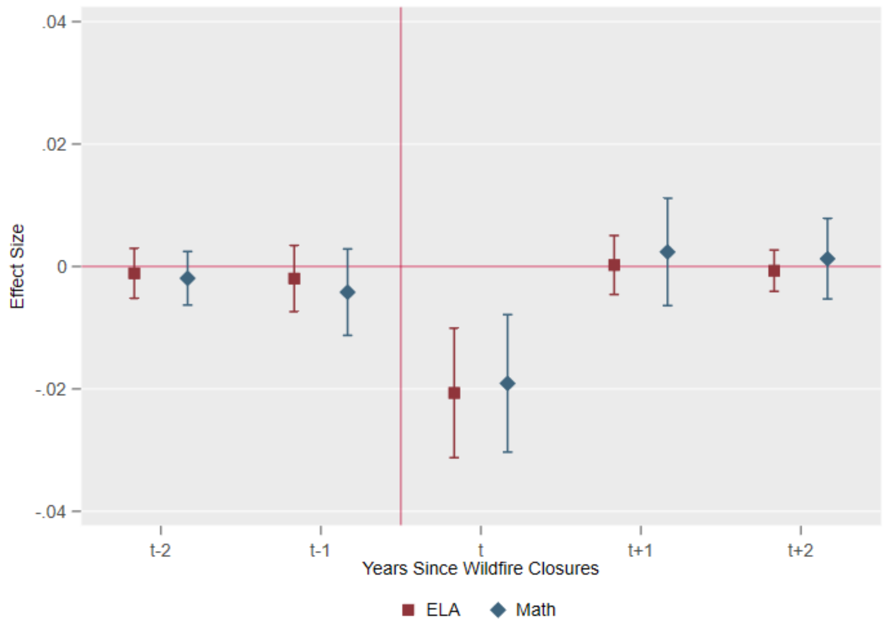
Notes: This figure shows the timing of wildfire school closures and state academic assessments in California.

FIGURE 4
Total School Closure Days Caused by Wildfires and Winter Conditions in California

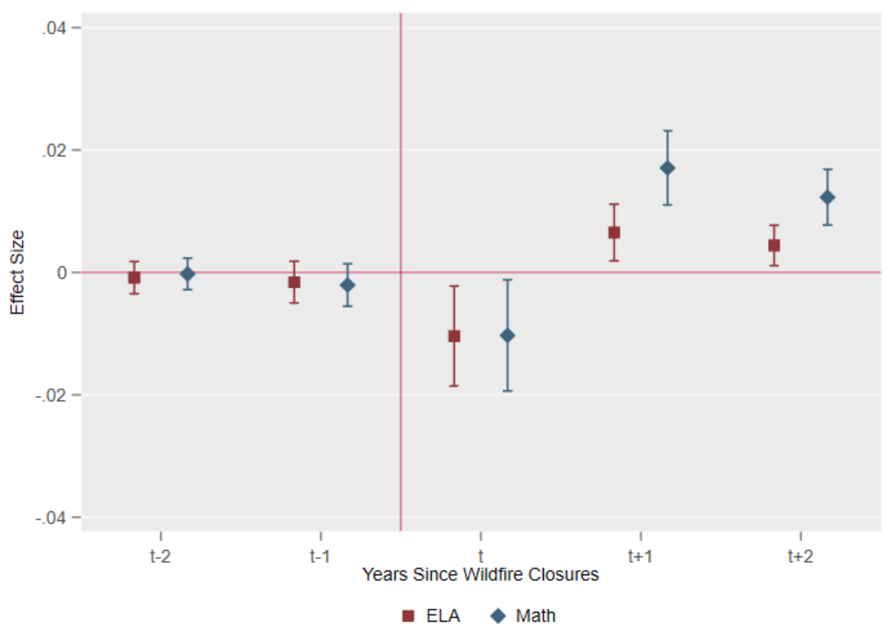


Notes: This figure shows the number of school closure days caused by wildfires and winter conditions between 2009 and 2018 in California.

FIGURE 5
Impact of Wildfire School Closures on Student Test Scores



(a) District-Level Sample



(b) School-Level Sample

A Supplementary Tables

TABLE A.I
Impact of Wildfire School Closures on Other Closures

	(1) ELA	(2) Math
Closure days	-.002	-.004
	(.006)	(.006)
Closure days$_{t-1}$	.007*	.006
	(.004)	(.004)
Closure days$_{t-2}$	-.002	-.002
	(.001)	(.002)
Observations	52,400	41,594
R-squared	.02	.02

Notes: This table reports the impact of wildfire school closures on other types of school closures. All regressions include year fixed effects. Robust standard errors are reported in parenthesis; *** p<0.01, ** p<0.05, * p<0.1

TABLE A.II
Impact of School Time Loss by Free/Reduced-price Lunch Status

		(1) ELA	(2) Math
Closure days	* High Free/Reduced-price Lunch	-.026***	-.037***
		(.009)	(.011)
	* Medium Free/Reduced-price Lunch	.000	-.003
		(.007)	(.008)
	* Low Free/Reduced-price Lunch	-.007	-.010
		(.008)	(.009)
Closure days$_{t-1}$	* High Free/Reduced-price Lunch	.020***	.017**
		(.005)	(.006)
	* Medium Free/Reduced-price Lunch	.006	.015***
		(.004)	(.005)
	* Low Free/Reduced-price Lunch	.005	.022***
		(.004)	(.005)
Closure days$_{t-2}$	* High Free/Reduced-price Lunch	.010**	.018***
		(.004)	(.005)
	* Medium Free/Reduced-price Lunch	.002	.011**
		(.003)	(.005)
	* Low Free/Reduced-price Lunch	.006*	.010**
		(.003)	(.004)
Observations		191,049	179,912
R-squared		.918	.876

Notes: This table reports the effect of school time loss on student achievement by the proportion of students eligible for free/reduced-price lunch at schools. All regressions control for cohort fixed effects, year fixed effects, school-x-grade fixed effects, school-grade level student race and gender composition, school-level percentage of students eligible for free/reduced price lunch, closures days due to reasons other than wildfires, and district linear time trends.Estimates are weighted by school-year-grade enrollments. Robust standard errors clustered at school level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

TABLE A.III
School Closures and Enrollment

	(1) ELA	(2) Math
Closure days	.347	.317
	(.220)	(.204)
Closure days$_{t-1}$	.250	.153
	(.171)	(.147)
Closure days$_{t-2}$	.279*	.195
	(.146)	(.131)
Mean Dept. Variable	110.239	104.946
Observations	201,869	190,253
R-squared	.967	.966

Notes: This table reports the impact of school closures on school-grade-year level enrollment. All regressions include year fixed effects, school-x-grade fixed effects, and closures days due to reasons other than wildfires. Robust standard errors clustered at school level are reported in parenthesis.

TABLE A.IV
School Closures and Racial Composition (in percentage point)

	Asian		Black		Hispanic		White	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ELA	Math	ELA	Math	ELA	Math	ELA	Math
Closure days	-.059***	-.058**	-.101***	-.111***	.158*	.160*	-.458***	-.461**
	(.023)	(.023)	(.038)	(.039)	(.095)	(.096)	(.135)	(.137)
Closure days$_{t-1}$	.032**	.038**	.076***	.072***	-.044	-.042	.093**	.089**
	(.015)	(.016)	(.019)	(.019)	(.044)	(.044)	(.045)	(.045)
Closure days$_{t-2}$	.015	.017	.019	.007	-.331***	-.367***	.453***	.484***
	(.014)	(.015)	(.017)	(.017)	(.073)	(.077)	(.076)	(.079)
Mean Dept. Variable	7.836	7.911	6.542	6.475	5.540	5.832	27.700	27.390
Observations	201,869	190,253	201,869	190,253	201,869	190,253	201,869	190,253
R-squared	0.963	0.963	0.937	0.940	0.969	0.970	0.962	0.963

Notes: This table reports the impact of school closures on school-grade-year level student racial composition. The outcome variables are the school-year-subject level percentages of students by race. All regressions control for year fixed effects, school-x-grade fixed effects, and closures days due to reasons other than wildfires. Robust standard errors clustered at school level are reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.1

TABLE A.V
Impact of School Time Loss: Robustness Check

	ELA			Math		
	(1)	(2)	(3)	(4)	(5)	(6)
	$<$ 500 acres	$<$ 5000 acres	$<$ 15000 acres	$<$ 500 acres	$<$ 5000 acres	$<$ 15000 acres
Closure days	-.005	-.006	-.009*	-.011*	-.011*	-.014**
	(.005)	(.005)	(.005)	(.006)	(.006)	(.006)
Closure days$_{t-1}$	.008***	.009***	.009***	.018***	.018***	.018***
	(.003)	(.003)	(.003)	(.004)	(.004)	(.004)
Closure days$_{t-2}$	.005**	.005**	.005**	.012***	.012***	.012***
	(.002)	(.002)	(.002)	(.003)	(.003)	(.003)
Observations	190,998	191,062	191,197	179,859	179,925	180,053
R-squared	0.918	0.918	0.918	0.876	0.876	0.876

Notes: This table reports the effect of school time loss on student achievement by different criteria for identifying large-scale wildfires. Column (1) and (4) exclude school-year-grade cells that either specified bad air quality as the reason of closure or experienced wildfires that burned more than 500 acres; column (2) and (5) exclude school-year-grade cells with wildfires burned more than 5,000 acres; column (3) and (6) exclude school-year-grade cells with wildfires burned more than 15,000 acres. All regressions include cohort fixed effects, year fixed effects, school-x-grade fixed effects, school-grade level student race and gender composition, school-level percentage of students eligible for free/reduced price lunch, closures days due to reasons other than wildfires, and district linear time trends. Estimates are weighted by school-year-grade enrollments. Robust standard errors clustered at school level are reported in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE A.VI
Impact of Air Pollution: IV Estimates with Weather Controls

	(1) ELA	(2) Math
Closure days	-.009*	-.000
	(.005)	(.006)
smoke	-.011	-.050***
	(.008)	(.010)
Cragg-Donald Wald F-statistic	190.405	184.674
Observations	178,179	168,118

Notes: This table reports IV estimates of the effect of air pollution on student achievement. Both regressions control for cohort fixed effects, year fixed effects, school-x-grade fixed effects, school level race and gender composition, percentage of students eligible for free/reduced-price lunch, weather controls including indicators for deciles of maximum and minimum temperatures, precipitation, and wind speeds, closures days due to reasons other than wildfires, and district linear time trends. Estimates are weighted by school-year-grade enrollments. Robust standard errors clustered at the school level are reported in parenthesis; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

TABLE A.VII

Impact of Air Pollution: IV Estimates with Overall Pollution Controls

	ELA				Math			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mean	75th PCTL	90th PCTL	95th PCTL	Mean	75th PCTL	90th PCTL	95th PCTL
Closure days	-.009**	-.009**	-.004	-.010**	-.003	-.006	.004	-.007
	(.004)	(.004)	(.005)	(.005)	(.005)	(.005)	(.006)	(.006)
smoke	-.010	-.011***	-.009***	-.001	-.039***	-.040***	-.022***	-.002
	(.007)	(.004)	(.003)	(.001)	(.008)	(.006)	(.003)	(.002)
Cragg-Donald Wald F-statistic	298.916	310.953	206.597	334.193	280.594	279.515	203.074	301.126
Observations	195,392	195,392	195,392	195,392	184,304	184,304	184,304	184,304

Notes: This table reports IV estimates of the effect of air pollution on student achievement. Columns (1) and (5) use the average of PM 2.5 concentration during the school year as a measure of air pollution; columns (2) and (6) use the 75th percentile of PM 2.5 concentration during the school year as a measure of air pollution; similarly, column (3) and (7) use the 90th percentile and column (4) and (8) use the 95th percentile of PM 2.5 concentration. All regressions control for cohort fixed effects, year fixed effects, school-x-grade fixed effects, school level race and gender composition, percentage of students eligible for free/reduced price lunch, closures days due to reasons other than wildfires, average PM 2.5 concentration, and district linear time trends. Estimates are weighted by school-year-grade enrollments. Robust standard errors clustered at the school level are reported in parenthesis; *** $p<0.01$, ** $p<0.05$, * $p<0.1$