
In-Person Schooling and Youth Suicide

Evidence from School Calendars and Pandemic School Closures

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ABSTRACT

This study explores the effect of in-person schooling on youth suicide in the United States. We show that youth suicide rates historically declined during summers and rose again earlier in counties with an August school starting date. We document a departure from this pattern at the onset of the COVID-19 pandemic: youth suicides fell 25 percent in March 2020, when schools closed, and remained low throughout summer. Leveraging county variation in the timing of reopening, we find that returning to in-person instruction increased youth suicides by 12–18 percent. Analysis of Google search data suggests that bullying is a likely mechanism.

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Special Issue: Causes and Consequences of Child Mental Health

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I. Introduction

Young people in the United States are increasingly grappling with severe mental health disorders. In 2019, 15.7 percent of children ages 12–17 experienced a major depressive episode, compared with just 7.9 percent in 2006 (SAMHSA 2021). As mental disorders have surged, the United States has seen a troubling increase in suicidality among youths. Since 2007, suicide deaths among individuals ages 15–19 have increased by over 70 percent (Curtin and Heron 2019), making suicide the second leading cause of death among youths. A large cross-disciplinary literature has explored the determinants of youth suicide, pointing to a wide range of contributing factors, which include adverse childhood experiences (Dube et al. 2001), social stressors (Cutler et al. 2001), and substance use (Carpenter 2004). One area of policy focus has been the role of schools in youth suicide, with emphasis on risk factors such as in-school bullying (Kim and Leventhal 2008) and the efficacy of interventions to prevent teen suicide (Brann et al. 2021).

The role of school attendance as a risk factor for youth suicide was first highlighted by Hansen and Lang (2011), who identified seasonality in youth suicide rates in the United States that largely follow the traditional academic calendar. Hansen and Lang (2011) found that suicides consistently decreased dramatically in summer months (and less so in December) among individuals ages 12–18, while remaining largely unchanged among young adults, ages 19–25.¹ While this national seasonality pattern is also found in each region of the United States, it has been difficult to generate a concrete link between school attendance and youth suicide at a more granular level. Generally, U.S. students begin their summer vacation between Memorial Day and late June and return to school between early August and early September, and there have historically been few deviations from this pattern.² A few studies have used limited changes in school calendars or collected local education agency data for a particular state.³ However, to our knowledge, an administrative database documenting geographic and temporal variation in local school calendars across the entire United States does not exist, and there has

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1. Hansen and Lang (2011) rule out several potential alternative causes for seasonality, including seasonal affective disorder (SAD), economic conditions, and geography.

2. Price and Wasserman (2023) provide evidence from the Current Population Survey to support this general school calendar pattern for U.S. schools using employment patterns of teachers and enrollment of students.

3. For example, Sims (2008) uses changes in the start date of a few districts in Wisconsin, Anderson and Walker (2015) study modified four-day calendars in rural Colorado, and Graves (2011) focuses on year-round schools in rural California.

been no large-scale nationwide examination of the association between local school calendars and youth suicide.

We offer new evidence on the effect of in-person schooling on youth suicide using cell phone point-of-interest data (that is, smartphone “pings” at specific locations) made available by SafeGraph, Inc. As documented by Garcia and Cowan (2022); Hansen, Sabia, and Schaller (2022); and Parolin and Lee (2021), SafeGraph foot traffic data provide an extraordinary proxy that captures daily variation in the physical presence of individuals on elementary and secondary school campuses, thus capturing when schools are likely open and closed at granular local and temporal levels. We use SafeGraph data in two novel ways, described below, to identify the causal effects of in-person schooling on youth suicide.

First, we reproduce evidence of seasonality in youth suicide rates (and the lack of seasonality for young adults) originally identified by Hansen and Lang (2011). We then expand on their analysis by using school foot traffic patterns from 2019 to identify plausibly exogenous cross-county differences in school district calendars and use this variation to explore pre-pandemic differences in youth suicide seasonality. In particular, we explore how youth suicide patterns differ in summer months (June, July, and August) across counties with different school year starting times (for example, August start times vs. September start times). We find that youth suicide rates rise in August in counties predicted to have early August school starting dates, but do not rise until September in counties predicted to have September school starting dates. Similarly, youth suicide rates decline earlier in counties where summer vacation starts earlier (May) and later in counties with later release (June). This finding suggests an important link between in-person school attendance and youth suicides.

We provide further evidence on the causal effects of in-person schooling on youth suicide by exploiting the unprecedented changes in in-person attendance that occurred during the COVID-19 pandemic. When the novel coronavirus SARS-CoV-2 became recognized as a global pandemic, schools closed across the United States. While the public health trade-offs of these school closures remain uncertain—particularly given concerns about youth isolation and mental health (Mayne et al. 2021)—this enormous national deviation from normal school calendars provided an important new opportunity to study the psychological effects of in-person schooling. Furthermore, in the months that followed the onset of the pandemic, the timing of reopening of schools to in-person instruction varied considerably across the United States, driven by both state and local school district decisions. We exploit both the sudden drop in school attendance in March and the subsequent staggered reopening (as identified by changes in school foot traffic) to study their effects on youth suicide.

Using each source of variation, we consistently find that in-person schooling is associated with increases in youth suicide rates. We find evidence of a sudden and dramatic decline in youth suicide rates in March 2020, three months earlier than the typical summer drop, which is sustained through the summer. Then, using staggered reopening, proxied by changes in local school foot traffic, and a difference-in-differences approach, we find that moving from likely closed to likely fully reopened schools is associated with a 12–18 percent increase in youth suicide rates. This finding is robust to a variety of alternative specifications, including those that control for other proxies for local pandemic severity, economic impacts, and lockdown responses.

We conclude by investigating and discussing several mechanisms. One possible mechanism is changes in access to firearms. More than half of all suicides involve a gun,⁴ and young people could obtain firearms through networks at school.⁵ We examine firearm suicides and nonfirearm suicides separately and show that in-person schooling effects are concentrated among nonfirearm suicides. We next consider the role of parental supervision by exploring whether time at home with parents reduced immediate suicide risk. Given that parental exposure increases the most on weekends (when parents are less likely to be working), we examine whether the effect of school foot traffic on youth suicide differed by the day of the week on which the suicide is completed. We do not find that the effects are measurably stronger for weekday as compared to weekend suicides. Finally, we study bullying (including in-person bullying and cyberbullying) as a potential mechanism using data on search queries obtained from Google Trends. Like Bacher-Hicks et al. (2022), we find that bullying related queries decreased with school closures. Difference-in-differences estimates show that a return to full-in-person schooling was associated with a 137–243 percent increase in Google searches related to bullying. Descriptive evidence from the 2021 National Youth Risk Behavior Survey provides further evidence consistent with the hypothesis that in-person bullying may be an important mechanism.

II. Data

A. National Vital Statistics System Mortality Data

We measure suicides over the period 1990–2020 using restricted-use data from the multiple-cause of death mortality files. These data are obtained from the National Center for Health Statistics' (NCHS) Division of Vital Statistics at the Centers for Disease Control and Prevention (CDC). They include individual death certificates with identifying information on the deceased persons' county of residence, cause(s) of death, and month and year of death.⁶

We generate county-by-month counts of completed suicides among school-aged youth ages 12–18. Following Hansen and Lang (2011), we use a comparison group of young adults ages 19–25 who are no longer in middle or high school and who are either attending university, in the labor force, or idle. [Online Appendix Figure A1](#) shows trends in the overall youth suicide rate over the period 1990–2020. Between 1990 and 2007, there was a sharp decline in the youth suicide rate, plummeting from a high of 7.0 suicides per 100,000 youth in 1990 to 3.9 suicides per 100,000 youth in 2007. The post-2007 period saw a reversal in that trend, with the youth suicide rate doubling to 7.9 suicides per 100,000 youth in 2018. There was a 9 percent decline in the youth suicide rate from 2018 to 2019 (to about 7.1 suicides per 100,000 population), with the youth suicide rate remaining steady in 2020.

4. This is according to Pew Research Center <https://pewrsr.ch/448q4hU> (accessed November 1, 2023).

5. On the other hand, firearm ownership increased substantially during the pandemic, which would likely have counteracted the sudden beneficial effects of school closures in March 2020. It is possible but unlikely that changes in firearm ownership were correlated with local school reopening once we control for other pandemic effects.

6. The data available to us outside of a Research Data Center (RDC) do not include information on the exact day of death, but only the day of the week on which the death occurred (Monday–Sunday).

Though higher overall, young adult (ages 19–25) suicide rate followed a similar pattern. Between 1995 and 1999, there was a sharp decline in the young adult suicide rate from about 15.5 suicides to 12.0 per 100,000 young adults. After remaining roughly steady through 2009, there was a sharp increase in their suicide rate through 2019 and continuing through 2020. The overall patterns suggest that youth and young adults show similar trends in suicide despite having different seasonality and supports the use of young adults as a counterfactual for youth in our analyses.

B. SafeGraph Foot Traffic Data

To identify cross-county variation school calendars in the pre-pandemic period (2019) and county-by-month variation in in-person school attendance during the 2019–2020 period (which includes the pandemic), we use anonymized smartphone data from SafeGraph, Inc. These data allow us to capture foot traffic (cell phone pings) at elementary and secondary schools. These smartphone data have been used by economists and other researchers to study social mobility prior to and during the COVID-19 pandemic in the United States (for example, Allcott et al. 2020; Cronin and Evans 2020; Dave, McNichols, and Sabia 2021; Goolsbee and Syverson 2021) and more recently by scholars studying the impact of school reopening/closing policies on health and economic well-being (Garcia and Cowen 2022; Hansen, Sabia, and Schaller 2022; Bravata et al. 2021; Fuchs-Schündeln et al. 2021).

School foot traffic data are drawn from SafeGraph point-of-interest (POI) files for the years 2019 and 2020. These data include location-specific pings from 40 million anonymized cell phones whose owners did not opt out of sharing geocoded data. SafeGraph provides researchers with daily data on cell phone pings at more than four million POIs aggregated to the census block group, county, and state levels. We use the North American Industry Classification System (NAICS) identifier to flag elementary and secondary schools (NAICS code 611110) to construct county-by-month counts of smartphone pings at kindergarten through 12th grade (K–12) schools. These data are then merged to county-by-month-year death certificate data on age-specific completed suicides.

First, we use K–12 foot traffic in 2019 to create proxies for school calendars for each county. To measure when the school year begins, we calculate aggregate school foot traffic on weekdays in August of 2019 for each county and divide this number by the average foot traffic in September and October of 2019. To measure the end of the school year, we likewise calculate the aggregate school foot traffic on weekdays in June of 2019 and compare it to the average of weekday foot traffic in May and April. Values close to one suggest schools are fully open throughout the month, and values close to zero suggest schools are fully shut down. This requires the assumption that school calendars remained constant from 1999 to 2019. This assumption is supported by measures of enrollment from the Current Population Survey and echoes the approach of Price and Wasserman (2023).⁷

7. Using the measures of current enrollment of 16- and 17-year-old youth in the Current Population Survey, we find strong evidence of calendar stability, shown in [Online Appendix Figure A2](#). Measures of school enrollment in the summer from 1990–2004 show a correlation of 0.83 with similar measures for the time period 2005–2019.

To capture the local timing of school reopening in 2020, we follow Hansen, Sabia and Schaller (2022). We calculate the treatment variable *K–12 Foot Traffic*, a county-by-month measure of K–12 school foot traffic relative to monthly averages for January and February, when nearly all U.S. primary and secondary schools were in session just prior to the pandemic. For example, if *K–12 Foot Traffic* took on a value of ten in September 2020, this means that county-level school foot traffic in September was approximately 10 percent of what it was in January–February 2020, suggestive of a high degree of remote learning.⁸ As the value of school foot traffic increases beyond values closer to 50, this implies a mix of online and in-person schooling (hybrid teaching), while values approaching January–February levels (100) would suggest a return to full in-person schooling. During 2019, the (population weighted) mean of the *K–12 Foot Traffic* treatment measure was 66.3; in 2020, it was 37.6, reflective of substantial school closings.⁹

In addition to measuring school foot traffic, we also measure foot traffic at restaurants and bars in a manner comparable to our school foot traffic measure. This measure helps to disentangle the effect of school foot traffic from other pandemic-related phenomenon, including shelter-in-place orders (SIPOs), nonessential business closures (NEBCs), and beliefs or risk preferences of the local population with respect to COVID-19 contagion. *Restaurant–Bar Foot Traffic* is a year-specific county-by-month measure of relative smartphone pings at restaurants (NAICS code 7225) and drinking places (NAICS code 7224), as compared to foot traffic at such establishments in January and February.

C. COVID-19 Death Data

To capture local pandemic-related correlates of youth suicides more fully, we also measure county-by-month COVID-19 cumulative deaths (*COVID-19 Deaths*), as provided by the *New York Times* from January 2020 through December 2020. These data, which have been used by health economists and public health researchers to track variation across counties over time in COVID-19 spread (see, for example, Courtemanche et al. 2020; Dave McNichols, and Sabia 2021; Gupta et al. 2021; Hansen, Sabia, and Schaller 2022), are, like *Restaurant–Bar Foot Traffic*, designed to disentangle the effect of school reopening/closing policies from other pandemic-related effects on youth suicide. In the period following the onset of COVID-19 deaths, the average cumulative COVID-19 death rate was 2.83 per 100,000 population, reaching 8.14 deaths per 100,000 population by December 2020.¹⁰

8. We omit weekends from our calculation of average K–12 school foot traffic.

9. We acknowledge that our foot traffic measure may be measured with error, picking up trends in staff presence on school campuses as well as the presence of others (for example, community members using school grounds for athletic activities). As Hansen, Sabia, and Schaller (2022) note, “Many factors could affect foot traffic other than school closures and reopenings, and those will generate noise in our variable. For instance, while foot traffic drops on the weekends and during the summer, it does not drop to zero, potentially due to individuals passing by school grounds or families using school facilities for recreation when schools are not open for instruction. Moreover, even when schools were remote, staff were likely working on campus, and families may have stopped by to pick up lunches (which many districts still provided). In addition, there is some measurement error due to GPS drift.”

10. We collect data on the business cycle using the county-by-year unemployment rate (*URate*) collected from the United States Census Bureau. We further collect information on the state-by-year divorce rate (*DivRate*) from the CDC. And finally, we also collect data on state anti-bullying laws, which may affect psychological health of historically marginalized populations of students, from Sabia and Bass (2017); Rees, Sabia, and Kumpas (2022); and Llang et al. 2023.

III. Empirical Methods

A. Seasonality of Suicides over Time

We begin by pooling county–months over the pre-pandemic period of 1990–2019 and then in 2020 (the first COVID-19 pandemic year in the United States) and estimate a Poisson regression of the following form:

$$(1) \quad E(\text{Suicide}_{cmt} | \mathbf{X}_{cmt}) = \exp[\beta_0 + \beta_m + \tau_t + \gamma_c + \ln(\text{days} * \text{pop}) + \beta_4 \text{URate}_{cmt} + \beta_5 \text{DivRate}_{cmt} + \beta_6 \text{ABL}_{cmt}]$$

where Suicide_{cmt} is the number of suicides for youth ages 12–18 (or young adults ages 19–25) residing in county c at month m in year t . The exposure variable (for which the coefficient is restricted to be 1) is the product of the age-by-county-by-year population and the number of days in a month. Our coefficients of interest, β_m , show the seasonality of suicides, with the reference month of January, when all schools are generally in session. Given our particular interest in how the seasonality of suicides may have changed during the COVID-19 pandemic, we estimate Equation 1 separately for the years 1990–2019 and 2020, allowing all the parameters to differ in the pre- and post-pandemic periods.¹¹

Poisson regressions are well suited to our setting given the count nature of suicides, the possibility that some counties have no youth suicides in some months, the ability to constrain the estimated effect on exposure variables to reflect differences in counts due to population levels or the number of days in a month, and the general robustness of the model to misspecification. While the model assumes under maximum likelihood the equality of the mean and variance, this assumption is easily relaxed, and the estimator is consistent provided the conditional mean is correctly specified (Gourieroux, Monfort, and Trognon 1984; Wooldridge 2014). However, we also estimate ordinary least squares (OLS) regressions using the youth suicide rate as the left-hand-side variable, with a pattern of estimates qualitatively similar to those obtained when using our preferred Poisson model.

B. School Foot Traffic and Suicides 2019 and 2020

Next, we turn to our school foot traffic data available for the 2019–2020 period and estimate the following regression:

$$(2) \quad E(\text{Suicide}_{cmt} | \mathbf{X}_{cmt}) = \exp[\beta_0 + \beta_1 K_{12} \text{Foot Traffic}_{cmt} + \tau_t + \gamma_c + \ln(\text{days} * \text{pop}) + \beta_4 \text{URate}_{cmt} + \beta_5 \text{DivRate}_{cmt} + \beta_6 \text{ABL}_{cmt}]$$

where β_1 , the parameter of interest, is the partial effect of relative K–12 school foot traffic on youth suicides. To ease interpretation of our regression results, we follow the approach of Hansen, Sabia, and Schaller (2022) and rescale this measure so that a one-unit change reflects a move from the fifth to the 95th percentile of reopening (representing a change of around 75.1 points in 2020) to approximate the difference between counties where schools were most likely to be fully closed (fifth percentile) as compared to schools with likely full in-person instruction (95th percentile). We also

11. We also estimate regressions where we aggregate foot traffic and suicides at the state level and obtain a qualitatively similar pattern of results, as described below.

allow for nonlinearities in the effect of K–12 school foot traffic by estimating models with indicator variables taking on the value of one if foot traffic passes a threshold likely indicative of school reopening.

To estimate the effect of K–12 school foot traffic separately from seasonality effects, we also augment Equation 2 with controls for summer fixed effects to isolate the effect of county trends in school foot traffic during the academic year when schools chose differing reopening policies. In some specifications, we also add controls for census division-by-year fixed effects. These flexible time controls allow for unique trends for counties in the same census divisions, which may have had comparable COVID-19 mitigation policies.

To explore descriptively the common trends assumption, we take several approaches. First, we estimate Equation 2 for young adults ages 19–25, who should be less affected by in-person K–12 schooling. Second, we present findings from two event study analyses. The first uses the continuous school foot traffic measure in Equation 2. Following Hansen, Sabia, and Schaller (2022) and Schmidheiny and Sieglöcher (2019),¹² we estimate:

$$(3) \quad E(\text{Suicide} | \mathbf{X}_{cmt}) = \exp \left[\beta_0 + \sum_{j \neq 1} \delta_j D_{cmt}^j + \beta_1 \text{RestuarantFootTraffic}_{cmt} \right. \\ \left. + \beta_2 \text{COVID19Deaths}_{cmt} + \beta_3 \text{URate}_{cmt} + \tau_t + \gamma_c + \ln(\text{days} * \text{pop}) \right. \\ \left. + \beta_6 \text{Summer}_m + \varepsilon_{cmt} \right]$$

where j denotes event time and D_{cmt}^j is a set of variables that measure the difference between county-level K–12 school foot traffic in month-by-year t and $t-1$ occurred j periods from t . Each δ_j can be interpreted as estimated effect of school foot traffic (scaled as described above) in event time relative to $j(i,s,t) = -1-2$ (one to two months prior to the change).

The second event study approach focuses on increases in school foot traffic beyond a “prominent” relative threshold of 90 percent in the post-pandemic period, representing mostly in-person or fully in-person instruction. We then employ the novel estimator developed by Sun and Abraham (2021) to account more fully for heterogeneous and dynamic treatment effects (Goodman-Bacon 2021). The specification includes the same set of controls described in Equation 3, as well as controls for relative foot traffic of 20–89 percent. In this analysis, the counterfactual is composed of counties that never exceeded 90 percent of pre-pandemic foot traffic during the post-pandemic period.

IV. Results

Our main results are shown in Tables 1–5 and Figures 1–7. Standard errors are corrected for clustering at the state level.

A. Historic Seasonality

We first explore the historic seasonality of suicides, comparing patterns for youths ages 12–18 and young adults ages 19–25. Figure 1 shows this comparison of suicide rates. We find the same pattern first identified by Hansen and Lang (2011). Youth suicides decline in summer months and December, times when student are generally not in

12. See also Rees, Sabia, and Margolit (2021).

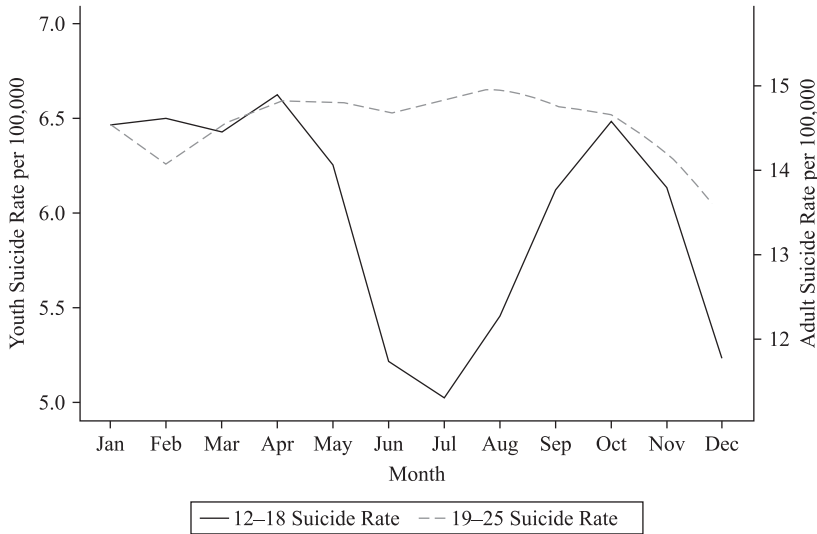


Figure 1

Monthly Suicide Rate per 100,000 Population, 1990–2019

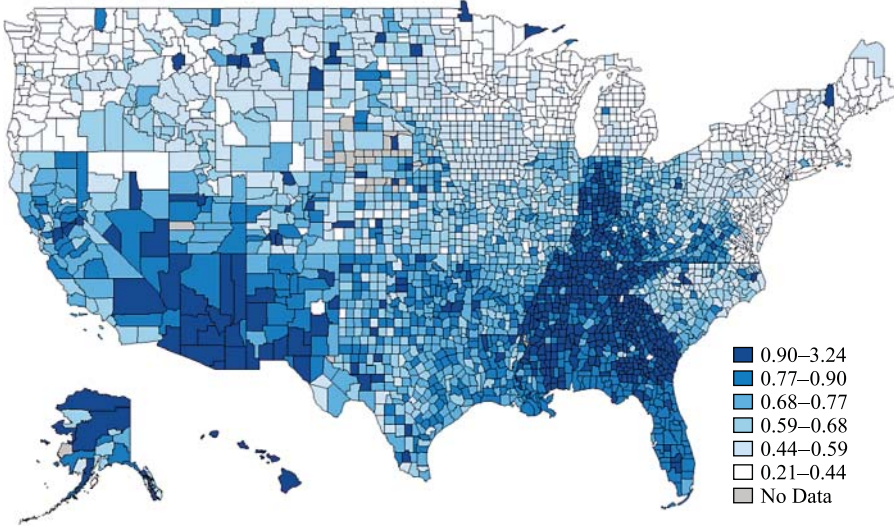
Notes: Based on annualized suicide rates from the multiple cause of death records from the National Center of Health Statistics, 1990–2019.

school, while young adult suicide rates are relatively flat throughout the year (with a slight increase in summer months and a modest decline in December).

In Figure 2, we highlight cross-county differences in school calendars using relative foot traffic patterns from SafeGraph for the pre-pandemic year of 2019. We construct two measures: (i) *August Relative Foot Traffic*, which measures the ratio of average daily foot traffic on nonholiday weekdays to foot traffic in September–October, and (ii) *June Relative Foot Traffic*, which measures the ratio of average daily nonholiday foot traffic in June to that in April–May. Panel A highlights large differences in school starting dates across the country, with some counties having schools start at the beginning of August (or perhaps end of July), denoted in darker shades (many counties in the southeast and southwest), while other regions have schools that stay closed throughout August and instead open in early September (northeast and northwest). A similar pattern emerges for school foot traffic in June (shown in Panel B), with some counties showing essentially no foot traffic in June, while others have significant in-person attendance throughout the first month of summer. In [Online Appendix Figure A3](#), we show these two measures of relative foot traffic are negatively correlated (correlation is -0.73). This finding is expected, as schools that start early also tend to end sooner. This negative correlation also aids in our confidence that our K–12 foot traffic proxy reflects actual differences in school start and end dates.

We next examine if regional differences in K–12 foot traffic are linked to differences in suicidality seasonality for youths. Figure 3 shows point estimates and confidence intervals from Poisson regression models based on Equation 1. The first column shows

Panel A: August K–12 Foot Traffic (Relative to September and October)



Panel B: June K–12 Foot Traffic (Relative to April and May)

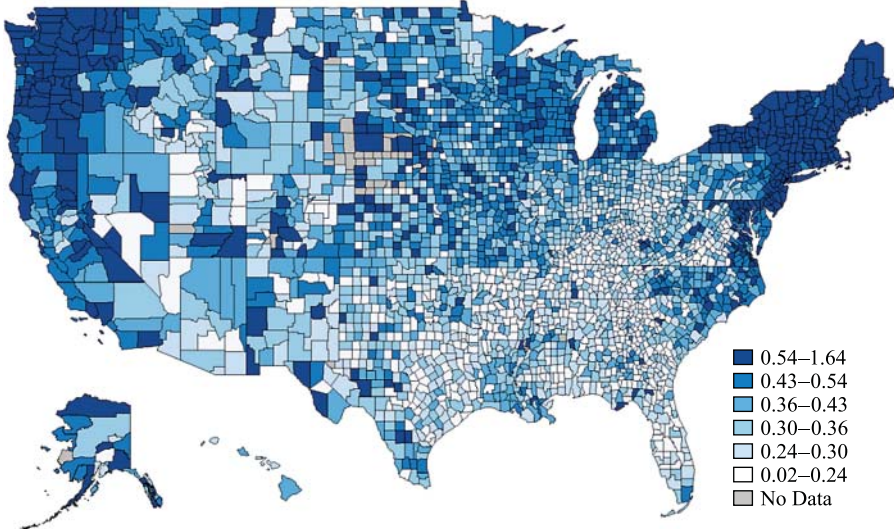


Figure 2
School Calendars Based on Relative Foot Traffic

Notes: Based on SafeGraph foot traffic at K–12 schools in 2019 aggregated to the county level. Relative foot traffic in August compares the aggregate of average nonholiday weekday foot traffic in August to average no-holiday weekday foot traffic in September and October. Relative foot traffic in June compares average nonholiday weekday foot traffic in June to average nonholiday weekday foot traffic in April and May.

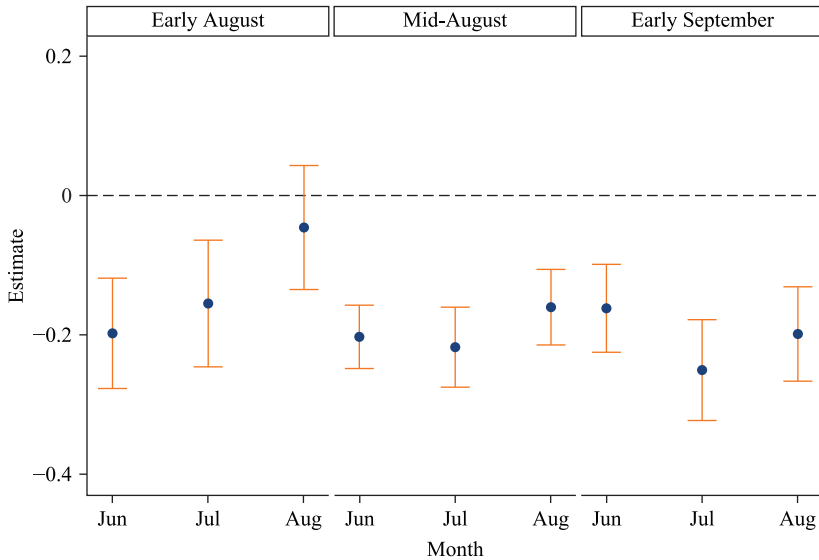


Figure 3

School Start Dates and Historic Seasonality of Youth Suicide, 1990–2019

Notes: Based on point estimates and 95 percent confidence intervals of the differences in suicide rates for calendar month of the year from Poisson regression models using suicides from 1990–2019. January is the omitted category. School calendar start dates are based on terciles of K–12 August foot traffic relative to foot traffic in September and October. All models control for county fixed and year fixed effects and cluster at the state level. Population*days in a month is used as an exposure variable.

monthly seasonality estimates for counties in the top tercile of *August Relative Foot Traffic*, which represent “early start, early release” regions (that is, areas where schools likely began their year in August and ended in May). The middle column isolates counties in the middle tercile, representing regions where schools likely open in the middle of August and close in early June. The third column shows counties in the bottom tercile of the *August Relative Foot Traffic* distribution, which are “late start, late release” regions where the schools likely begin in September and close in late June.¹³

We find that counties with early August start dates see youth suicide increase in August. Likewise, counties with mid-August starts instead exhibit a decrease in suicides in August, although it is not as pronounced as the June or July decreases. Finally, counties with September school calendar beginnings show a decrease in August that is close to July’s magnitude, and an attenuated drop in June. The smaller drop in June for this group is consistent with time in school increasing youth suicide risk, as September starts lead to a school year that does not end until the third or fourth week of June.

13. We note, these categorizations are based on school foot traffic data from 2019, with the assumption that school calendars have not changed substantially in the last 30 years. To the extent that there have been changes, then this decomposition may tend to understate how strong the differences would be if we had precise school calendars for the entire 30 years.

B. Changes in Seasonality during the Pandemic

Our previous analyses show that youth (but not young adult) suicides fall in the summer, and this drop varies depending on when the school year begins. While suggestive, the variation is cross-sectional in nature, as there has been limited variation in school calendars over time.¹⁴ The unprecedented changes in school policies during the pandemic—first the sudden closure in March 2020 and the subsequent staggered reopening in fall 2020—provide an important opportunity for additional insight into the causal effects of in-person schooling on youth suicide.

In Figure 4, we compare the seasonality of suicide in 2020 against the period 1990–2019. The point estimates are based on models following Equation 1. During the period 1990–2019, suicide rates fell in the summer for youth. Strikingly, in 2020, suicide rates instead fell in March, the start of the pandemic in the United States. Suicide rates for young adults remain relatively constant throughout the months of the year and likewise do not fall abruptly like youth rates.

In [Online Appendix Table A1](#), we show that the inclusion of controls has little bearing on this key finding. When adjusted to represent semi-elasticities, our estimates suggest that youth suicides fell by 25–38 percent (relative to January) from March to May of 2020. In [Online Appendix Table A2](#), we formally test whether the seasonal variation in suicides observed in 2020 is different than the variation during the 1990–2019 period.¹⁵ We are able to reject the hypothesis of equivalent seasonality for the months March through May, providing compelling evidence that the seasonality of youth suicide changed with the onset of the pandemic.

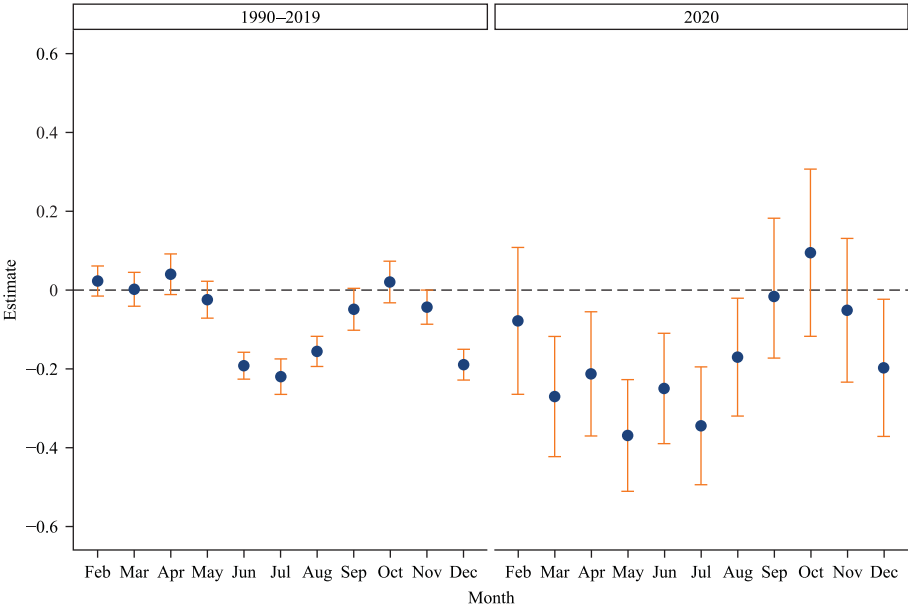
Interestingly, beginning in June of 2020, we no longer reject the hypothesis of identical suicide effects. This finding suggests whatever effects the pandemic had on the aggregate seasonal pattern of youth suicides, this ended in the month when the school year typically concludes. Importantly, the pattern of findings we uncover for young adults in the COVID-19 year of 2020 ([Online Appendix Tables A3 and A4](#)) is different from that observed for youth and suggests that the patterns we observe for youth may, at least in part, be due to the academic calendar for primary and secondary education. We next turn to a direct test of this hypothesis with K–12 school foot traffic.

C. K–12 School Foot Traffic

To further probe the role of schools in the pattern of suicides over the year, we next turn to our K–12 school foot traffic measure to proxy for local school opening/closing policies in Table 1. The point estimates shown are based on Equation 2. As noted above, the coefficient can be interpreted as the effect of moving from the fifth (likely closed) to the 95th percentile (likely fully opened) of K–12 school foot traffic. Columns 1–4 focus on youth ages 12–18. For the year 2019, we find that school openings are associated with a 17.5 percent increase in youth suicides. The findings in Columns 3 and 4 suggest that this effect of K–12 school foot traffic remains in 2020, with a similarly sized effect

14. Reasons for historical differences in school starting and ending times are a matter of some conjecture and include farm cycles related to the agrarian calendar across regions, urban versus rural make-up of regions, and differential demand across regions for cooler weather.

15. We explore young adult suicides in similar models in [Online Appendix Tables A3 and A4](#), finding little evidence of any seasonal variation or changes with the onset of the pandemic.



Panel B: Young Adult 19–25 Suicides

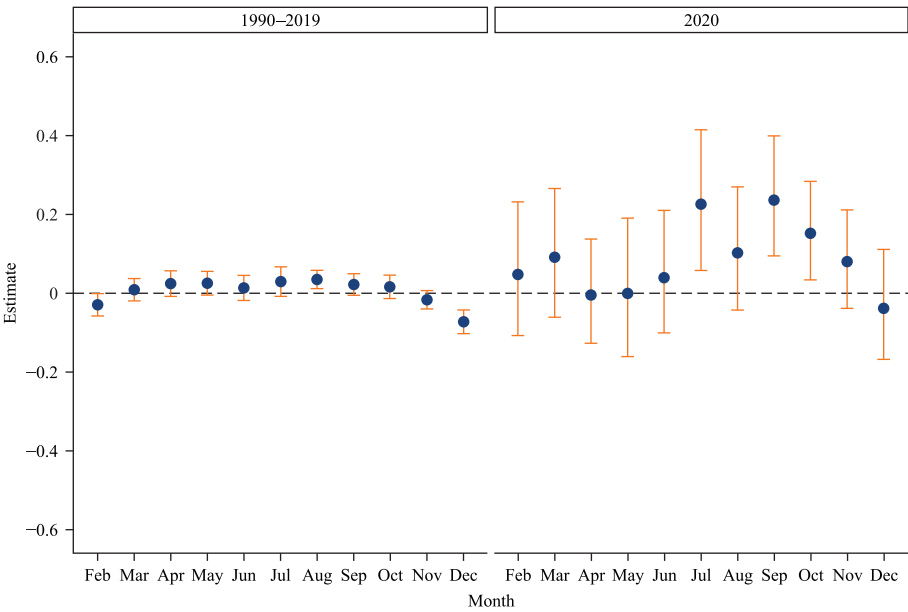


Figure 4
Historic Seasonality of Suicides, 1990–2019 vs. 2020

Notes: Based on estimates and 95 percent confidence intervals of the differences in suicide rates for calendar month of the year from Poisson regression models using suicides from 1990–2019. January is the omitted category. All models control for county fixed and year fixed effects and economic conditions and cluster at the state level. Population*days in a month is used as an exposure variable.

Table 1
Poisson Estimates of Effect of County-Level K-12 Foot Traffic on Youth and Young Adult Suicides 2019 vs. 2020

	Youth Ages 12-18				Young Adults Ages 19-25			
	2019	(2)	(3)	2020	2019	(5)	(6)	2020
	(1)			(4)			(7)	(8)
K-12 foot traffic	0.169** (0.0687)	0.166** (0.0693)	0.219*** (0.0604)	0.234*** (0.0879)	0.0454 (0.0423)	0.0489 (0.0429)	-0.0308 (0.0361)	-0.0438 (0.0486)
Restaurant-bar foot traffic		-0.0558 (0.0747)		0.110 (0.153)		0.0640 (0.0551)		0.0731 (0.0570)
Any COVID deaths				0.0555 (0.0549)				0.0235 (0.0455)
County death rate				1.633*** (0.594)				0.287 (0.534)
Observations	37,704	37,704	37,704	37,704	37,704	37,704	37,704	37,704
County fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Restaurant-bar foot traffic?	No	Yes	No	Yes	No	Yes	No	Yes
COVID-19 deaths?	No	No	No	Yes	No	No	No	Yes

Notes: Standard errors are clustered at the state level. Each regression uses population in each county times the number of days in a month as an exposure variable. COVID-19 deaths are coded as zero until the first documented COVID-19 related deaths, which occurred in March 2020. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

(approximately 23–26 percent). Importantly, the estimated effect of school openings persists even after controlling for restaurant and bar foot traffic and COVID-19 deaths, suggesting that the school attendance effect is not simply capturing overall pandemic-related shocks.

In sharp contrast to the results for youth, we find no evidence that K–12 school foot traffic is related to young adult suicides (Columns 5–8). The estimated effects are relatively small and are as often positive (2019) as they are negative (2020). Together, the pattern of results in Table 1 suggests that K–12 school foot traffic is likely capturing true changes in suicide behaviors among those most likely to be affected by school closures.

In Table 2, we pool data from 2019 and 2020 and use January–February 2020 as our anchor for relative foot traffic. Controlling for only county fixed effects (Column 1), we find that over this two-year period, school openings are associated with an 18.4 ($\exp^{0.169} - 1$) percent increase in youth suicides. The magnitude of the estimated effect does not substantially change after controlling for year fixed effects (Column 2) or restaurant and bar foot traffic, COVID-19 deaths, macroeconomic controls, and the divorce rate (Column 3). Importantly, we also find that after controlling for seasonality effects via summer month fixed effects (Column 4)—which ensures that identifying variation is coming from within-academic year changes in foot traffic—full in-person school openings are associated with a 14.3 percent increase in youth suicides. This finding also persists after controlling for census division-by-year fixed effects, which forces geographically proximate controls (Column 5).

Panels A of [Online Appendix Figure A6](#) show event study analyses using our continuous foot traffic measure, following Schmidheiny and Siegloch (2019). Our results show little evidence of a differential pre-treatment trend in youth suicides between treatment and control jurisdictions, consistent with the parallel trends assumption. Following an increase in K–12 school foot traffic (scaled to be from the 5th to 95th percentile), we see a substantial rise in the youth suicide rate. The differential is largest in the period up to four months following the reopening and then falls to pre-treatment levels by five or more months following the reopening.

Again, in sharp contrast to our findings for youths, the findings in Columns 6–10 of Table 2 and Panel B of [Online Appendix Figure A6](#) provide little evidence that K–12 school foot traffic is related to young adult suicides. The effects are consistently small and nowhere near statistically distinguishable from zero at conventional levels.¹⁶

Table 3 explores whether there are any nonlinearities in the effects of K–12 school foot traffic. The results show that schools with K–12 school foot traffic with at least 80 percent of its January–February 2020 levels (and likely largely reopened) see the largest increases in youth suicides. After controlling for summer fixed effects (identifying the treatment effect during the academic year) and requiring within-census-division county comparisons (Columns 3 and 6), we find that a likely full in-person reopening is associated with a 17.6 percent increase in youth suicides relative to counties that likely did not reopen at all (K–12 relative school foot traffic <20 percent of January–February 2020) (Column 3). Again, we find no evidence that K–12 school foot traffic is associated with a change in young adult suicides (Columns 3–6). Together, the pattern of results in

16. We find similar results when using OLS models, which are available upon request.

Table 2
Poisson Estimates of Effect of County-Level K–12 Foot Traffic on Youth and Young Adult Suicides, Pooled 2019 and 2020

	Youth Ages 12–18					Young Adults Ages 19–25				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
K–12 foot traffic	0.163*** (0.0549)	0.198*** (0.0580)	0.231*** (0.0556)	0.134* (0.0740)	0.149** (0.0717)	-0.0231 (0.0213)	-0.00427 (0.0191)	-0.00694 (0.0241)	0.00430 (0.0239)	0.00377 (0.0240)
Restaurant foot traffic			0.0773 (0.0560)	0.155*** (0.0556)	0.154*** (0.0545)			0.0790** (0.0361)	0.0689* (0.0371)	0.0635* (0.0374)
Any COVID deaths			0.0786 (0.0513)	0.0835 (0.0512)	0.0694 (0.0527)			0.0497 (0.0432)	0.0465 (0.0437)	0.0455 (0.0441)
County death rate			0.913** (0.461)	0.717 (0.453)	1.048** (0.514)			0.282 (0.484)	0.319 (0.488)	0.265 (0.453)
Personal income			-0.0639 (0.210)	-0.0564 (0.208)	0.0174 (0.198)			-0.288* (0.149)	-0.290* (0.149)	-0.155 (0.180)
Unemployment rate			0.0389** (0.0174)	0.0400** (0.0171)	0.0447*** (0.0165)			-0.00111 (0.0110)	-0.00130 (0.0111)	0.00259 (0.0103)
Divorce rate			0.0979 (0.134)	0.0919 (0.132)	0.196* (0.109)			0.115* (0.0617)	0.116* (0.0618)	0.118 (0.0765)

(continued)

Table 2 (continued)

	Youth Ages 12–18				Young Adults Ages 19–25					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Observations	74,660	74,660	74,660	74,660	74,660	74,660	74,660	74,660	74,660	74,660
County fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Restaurant–bar foot traffic?	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
COVID-19 deaths?	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Macro econ controls?	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Summer months FE?	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Census division-by-year FE?	No	No	No	No	Yes	No	No	No	No	Yes

Notes: Standard errors are clustered at the state level. Each regression uses population in each county times the number of days in a month as an exposure variable. COVID-19 deaths are coded as zero until the first documented COVID-19 related deaths, which occurred in March 2020. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3

Exploration of Nonlinear Effects of K–12 School Foot Traffic on Youth and Young Adult Suicides, Pooled 2019 and 2020

	Youth Ages 12–18			Young Adults Ages 19–25		
	(1)	(2)	(3)	(4)	(5)	(6)
K–12 foot traffic $\geq 80\%$	0.251*** (0.0591)	0.148* (0.0778)	0.162** (0.0754)	-0.0130 (0.0371)	0.00203 (0.0392)	-0.000518 (0.0393)
50% \leq K–12 foot traffic $< 80\%$	0.200*** (0.0708)	0.107 (0.0868)	0.111 (0.0863)	-0.0291 (0.0282)	-0.0153 (0.0327)	-0.0156 (0.0327)
20% \leq K–12 foot traffic $< 50\%$	0.105* (0.0576)	0.0735 (0.0606)	0.0717 (0.0598)	-0.0302 (0.0234)	-0.0259 (0.0235)	-0.0289 (0.0236)
Observations	74,660	74,660	74,660	74,660	74,660	74,660
County fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Restaurant–bar foot traffic?	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 deaths?	Yes	Yes	Yes	Yes	Yes	Yes
Macro econ controls?	Yes	Yes	Yes	Yes	Yes	Yes
Summer months FE?	No	Yes	Yes	No	Yes	Yes
Census division-by-year FE?	No	No	Yes	No	No	Yes

Notes: Standard errors are clustered at the state level. Each regression uses population in each county times the number of days in a month as an exposure variable. COVID-19 deaths are coded as zero until the first documented COVID-19 related deaths, which occurred in March 2020. The reference group K–12 school foot traffic less than 20 percent of the January–February 2020 level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tables 2 and 3 provide strong support for the hypothesis that in-person schooling is positively associated with youth suicides.

D. Spatial Heterogeneity and Dynamic Treatment Effects

One concern with our fixed effects Poisson estimates is that they may be subject to bias in the presence of heterogeneous and dynamic effects of school reopening. Note, the evidence presented so far suggests this concern is likely second order. The percentage reduction in suicides when school is out of session is of similar in magnitude across the entire country, as shown both in this paper and in Hansen and Lang (2011). Likewise, as shown in Figure 3, the timing of the increase in suicides with respect to changes in in-person schooling is nearly immediate.¹⁷

Nonetheless, to address this possibility in the present with school reopening following pandemic era school closure, we first isolate prominent changes in school opening

17. For example, youth suicide increases materialize in August for schools that start in early August, and decreases are apparent in June for schools that end by late May/early June. This suggests there is limited spatial and temporal heterogeneity in treatment effects in the past.

policies that appear to “bite” with respect to youth suicides and then generate new event studies using the new estimator proposed by Sun and Abraham (2021) to mitigate bias caused by heterogeneous and dynamic treatment effects. For this approach, we restrict the set of counterfactuals to those jurisdictions that did not attain at least 90 percent relative foot traffic in the post-pandemic period (March 2020–December 2020). We control for the full set of observables described in Equation 3, along with smaller foot traffic changes.¹⁸

[Online Appendix Figure A7](#) presents estimated event study coefficients. Our results suggest that in the pre-treatment period, the pattern of youth suicide differentials between treatment and control jurisdictions is consistent with the common trends assumption. Following a prominent school reopening, we detect evidence of an increase in youth suicides relative to jurisdictions that remained largely closed. This result is consistent with our event studies shown in [Online Appendix Figure A6](#) (which make use of the full distribution of changes in K–12 school foot traffic) and suggest that our estimated K–12 school foot traffic effects are not biased by heterogeneous and dynamic treatment effects by timing of reopening. With respect to young adults ages 19–25, our event study analysis in Panel B provides no support for the hypothesis that prominent increases in K–12 school foot traffic have an important impact on their suicides.¹⁹

E. Heterogeneity in Suicide Effects by Demographics, Substance Use, and Firearm Use

In Figure 5, we explore heterogeneity in the estimated effects of school reopening on youth suicides.²⁰ The findings suggest little evidence that K–12 foot traffic differentially affects youth suicides by race or gender. The estimated effects are generally larger for nonfirearm-involved suicides relative to firearm suicides, suggesting that firearms are an unlikely mechanism.²¹ We further find that the estimated treatment effects are, if anything, larger for younger as compared to older children. This would tend to cast doubt on the hypothesis that high-stakes exams or tumultuous school-involved romantic relationships (and breakups) drive our in-person schooling effects.

F. Potential Mechanisms

While economic conditions are predictive of adult suicides, the COVID-19 recession was short-lived, and controlling for economic conditions had little effect on our model estimates. Moreover, access to guns and economic conditions both operate in the “wrong direction” to account for the decline in youth suicides at the onset of the pandemic.

18. The use of alternative cutoffs, including 70 percent K–12 relative foot traffic, 85 percent relative foot traffic, and 95 percent relative foot traffic generated a qualitatively similar pattern of findings.

19. We have also explored the robustness of the main estimates using state-level aggregation and different levels of clustering. Those estimates are nearly identical, and our conclusions are unchanged. Results are available upon request.

20. These estimates based on models following Equation 2 that include the full set of observable controls, county fixed effects, year fixed effects, and summer fixed effects.

21. While Lang (2013) finds firearm suicides for youth increase with increased access to firearms, Lang and Lang (2021) also find that the demand for guns surged during the pandemic, which would tend to be inconsistent with our evidence on youth suicides.

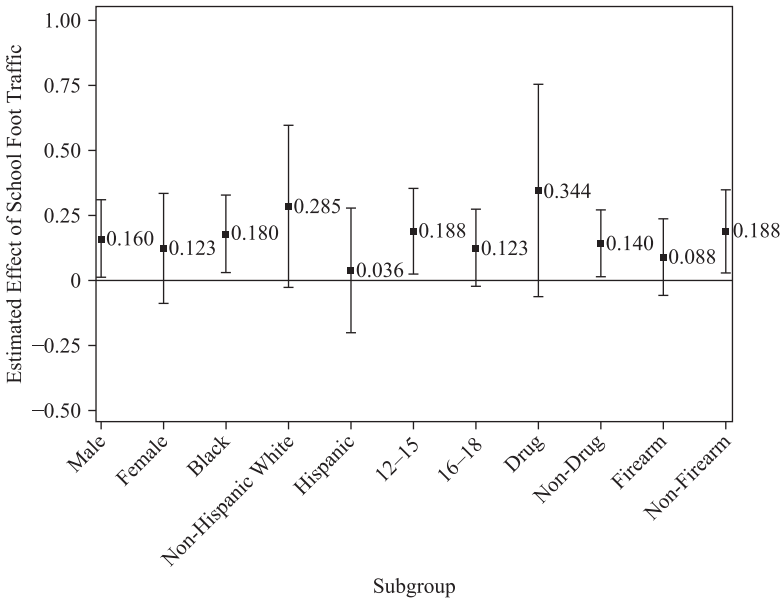


Figure 5

Heterogeneity in Estimated Effect of K–12 School Foot Traffic on Youth (Ages 12–18) Suicides, by Demographic Characteristics and Suicide Circumstances

Notes: The figure presents estimated treatment effects and 90 percent confidence intervals around the estimated treatment effects of a move from the fifth to 95th percentile of relative school foot traffic on youth suicides using monthly data for the period 2019–2020. The first two estimates present results by gender, the next three by race/ethnicity, the next two by age, the next two by whether the suicide was precipitated by intentional drug use or not, and the final two by whether the suicide involved a firearm. All regressions included county fixed effects, year fixed effects, summer month fixed effects, census division-by-year fixed effects, and the full set of observable controls.

We also consider that time spent at home with parents increased during the pandemic. This was driven both by the remote education of children and either the remote work of parents or (temporary) layoffs to parents. This increase in the amount of time families spend with each other could have diverse impacts on the mental health and well-being of children. For some families, the increase in supervision could reduce the amount of time children spend alone and hence could reduce suicide risk. For other families, the increased time together could increase family stress and lead to increases in child abuse.²²

Testing parental exposure as a mechanism is somewhat challenging, as early in the pandemic the amount of time parents and children spent with each other increased essentially everywhere across the country. However, weekdays versus weekend suicides

22. Leslie and Wilson (2020) find evidence that 911 calls related to domestic violence increased with the early lockdowns during the pandemic. Moreover, Baron, Goldstein, and Wallace (2020) suggest that child abuse detection decreased due to school closures.

provides a useful dimension of heterogeneity, as COVID-19 school closures and increases in remote work (or time spent at home due to a layoff or hours cut) increased the total amount of time families spent in the same location disproportionately on weekdays.²³

The estimates in Table 4 suggest that there are limited differences in the estimated effect school reopening on suicidality based on day of the week. This finding is inconsistent with parental exposure as a key mechanism. We note, however, this pertains only to parental exposure defined as physical proximity and time together, and it fails to capture other ways in which familiar interactions may have changed during pandemic-related school closures.

Bullying also stands out as a key potential mechanism that could explain part of the relationship between in-person schooling and youth suicidality. Prior work has shown bullying can have profoundly negative effects on youth. Card and Hodges (2008) and Klomek et al. (2007) find evidence bullying increases depression and lowers mental health of youth. Van Geel, Vedder, and Taniol (2014) recently conducted a thorough meta-analysis and find consistently across a variety of studies that bullying victimization is associated with a 95–334 percent increase in suicidal behaviors (ideation and attempts).²⁴

While bullying is associated with substantial increases in suicide risk, prior work generally does not inform about how these risks change when school is or is not in session, as most surveys are only implemented in school. Recently, Bacher-Hicks et al. (2022) proposed an alternative proxy based on Google Trends. Google provides information on the relative search frequency of a variety of user-specified searches. We reproduce the association Bacher-Hicks et al. (2022) find using data from both SafeGraph foot traffic and raw Google Trends for the search “My child is bullied” in Figure 6. During summer breaks prior to the COVID-19 pandemic, searches related to bullying fell. We replicate their key finding that when schools shut down at the start of the pandemic, searches fell in March of 2020 rather than in June. Moreover, as our time series track searches during the period of school reopening in fall of 2020 and beyond, we find that queries related to bullying began to rise as schools reopened to in-person instruction.

Next, we directly estimate the relationship between in-person school attendance and bullying (measured by county-by-month Google Trends proxies) in Table 5, using an estimation strategy identical to Equation 2. We focus on searches that include the terms “bullying,” “cyber-bullying,” and “school bullying.”²⁵ Difference-in-differences estimates in Columns 1–3 show that transitions from likely closed to likely reopened schools is associated with a 63 percent increase bullying queries ($\exp^{0.49} - 1$), a 48 percent increase cyber bullying queries, and a 107 percent increase in school bullying queries. In Columns 4–6, we allow a nonlinear relationship between K–12 school foot

23. Working from home may have also changed the typical work hours and days of families. However, McDermott and Hansen (2021) suggest the increases in work on weekends was limited to around two hours on the weekend among a sample of workers able to work remotely.

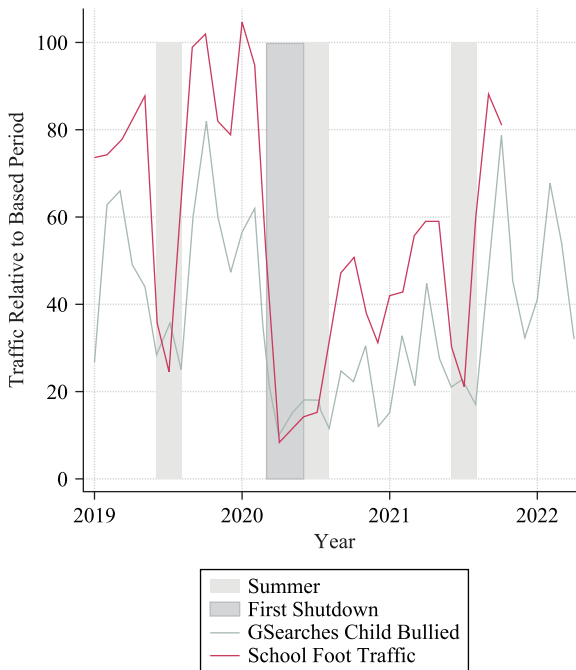
24. Rees, Sabia, and Kumpas (2022) find evidence that the adoption of anti-bullying laws is associated with a reduction in teen suicidal behaviors and completed suicides, particularly among those who are historically marginalized.

25. We rescale every state so the maximum search during 2019–2020 is 100.

Table 4
Estimated Effects of School Foot Traffic on Youth and Young Adult Suicides: Weekdays versus Weekends

	Youth Ages 12–18					Young Adults Ages 19–25				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel I: Weekdays (Monday–Thursday)										
K–12 foot traffic	0.165** (0.0646)	0.243*** (0.0648)	0.246*** (0.0668)	0.121 (0.0905)	0.138 (0.0891)	0.0102 (0.0223)	0.0139 (0.0225)	-0.000234 (0.0261)	0.0260 (0.0287)	0.0264 (0.0282)
Panel II: Weekends (F-Sun)										
K–12 foot traffic	0.162** (0.0703)	0.125 (0.0787)	0.204** (0.0941)	0.141 (0.122)	0.152 (0.121)	-0.0853** (0.0369)	-0.0428 (0.0346)	-0.0256 (0.0483)	-0.0653 (0.0520)	-0.0666 (0.0540)
Observations	74,660	74,660	74,660	74,660	74,660	74,660	74,660	74,660	74,660	74,660
County fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Restaurant/bar foot traffic?	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
COVID-19 deaths?	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Macro econ controls?	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Summer months FE?	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Census division-by-year FE?	No	No	No	No	Yes	No	No	No	No	Yes

Notes: Standard errors are clustered at the state level. Each regression uses population in each county times the number of days in a month as an exposure variable. COVID-19 deaths are coded as zero until the first documented COVID-19 related deaths, which occurred in March 2020. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Figure 6***Bullying Searches on Google and School Foot Traffic, 2019–2022*

Notes: Based on aggregate data collected from SafeGraph on foot traffic and searches for “My child is bullied” collected from Google Trends.

Table 5*School Foot Traffic and Google Searches for Bullying*

	Cyber Bullying (1)	Cyber Bullying (2)	School Bullying (3)	Cyber Bullying (4)	Cyber Bullying (5)	School Bullying (6)
K–12 foot traffic	0.49*** (0.065)	0.39*** (0.09)	0.73*** (0.12)			
K–12 foot traffic $\geq 80\%$				0.42*** (0.04)	0.35*** (0.07)	0.66*** (0.10)
$50\% \leq$ K–12 foot traffic $< 80\%$				0.32*** (0.023)	0.32*** (0.06)	0.53*** (0.07)
$20\% \leq$ K–12 foot traffic $< 50\%$				0.07*** (0.02)	0.04*** (0.04)	0.14*** (0.04)
Observations						
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Level Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors are clustered at the state level. Each estimate is from a Poisson regression. Search terms are “bullying,” “cyber bullying,” and “school bullying.” The reference group K–12 school foot traffic less than 20 percent of the January–February 2020 level for Columns 4–6. State-level controls include COVID-19 deaths, macroeconomic controls, and restaurant–bar foot traffic. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

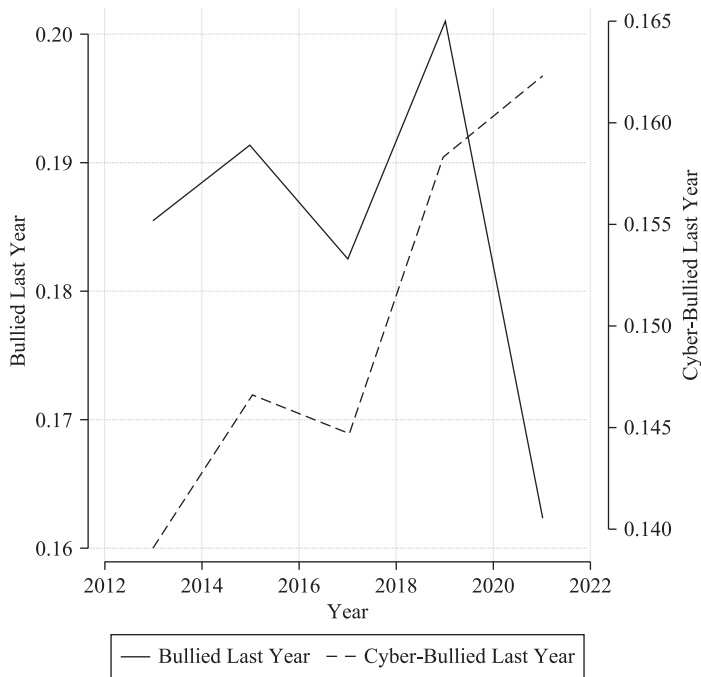


Figure 7

Bullying and Cyber-Bullying

Notes: Based on self-reported bully victimization rates in the Youth Risk Behavior Survey, 2013–2021. The 2021 survey was administered in fall of 2021, while the other surveys are administered in the spring.

traffic and bullying searches. We find the largest increases in searches for bullying terms for schools with the relatively higher K–12 school foot traffic.

As a final descriptive test of the role of bullying victimization, we draw data from the 2021 National Youth Risk Behavior Survey, a nationally representative school-based survey of students in Grades 9–12 collected by the Centers for Disease Control and Prevention. These data provide information on prior-year bullying victimization (in-school bullying as well as cyberbullying) among students who were interviewed in the fall of 2021.²⁶ Figure 7 shows that physical bullying victimization rates fell throughout the pandemic period, while cyberbullying remained relatively unchanged, suggesting little substitution toward online bullying.²⁷ When we link state identifiers in the National Youth Risk Behavior Survey to our measures of school reopening (at the state level), we

26. This measure of in-school bullying may minimize the survey's ability to identify changes in bullying victimization during the pandemic as more recent acts of victimization (that is, in fall of 2021) may also be captured. Moreover, the bullying victimization questions only capture extensive margins of bullying victimization.

27. Public health experts differ in their assessment of whether in-person versus cyberbullying is more detrimental to the psychological health of teens (Sticca and Perrin 2013).

find that states with higher average K–12 school foot traffic experienced higher levels of in-person bullying victimization among their students (see [Online Appendix Figure 8](#)).

Could bullying victimization explain much of the decline in youth suicide? Based on the average of estimates reported in Table 5, we find that bullying fell by approximately 63.2 percent when schools closed were closed. We find that 18.9 percent of students report bullying victimization in the National Youth Risk Behavior Survey for 2013–2019. Estimates from correlational studies (Van Geel, Vedder, and Tanilon 2014) suggest that bullying victimization is associated with a 123 percent increase in suicidality. Taken at face value, these estimates suggest that school closures would predict an approximately 14.69 percent decline in youth suicides, the majority of the decrease we identify when schools closed. We caution, of course, that this is not direct evidence that bullying victimization is *the* mechanism, just a potentially very important one.²⁸

V. Conclusion

This study finds consistent evidence that in-person schooling is positively related to youth suicides. We find evidence of this link based on historic cross-sectional differences in school calendars and recent school closure and staggered reopening during the COVID-19 pandemic. Our results support the conclusions of Bacher-Hicks et al. (2022) that school closures interrupted the cycle of bullying and other stresses related to in-person schooling.

However, this interruption was short-lived. We find youth suicides levels have increased as schools have reopened. Moreover, this increase in suicides comes as youth suicides have been on the rise since 2006 (Marcotte and Hansen 2023). Despite the promise that anti-bullying laws may have in reducing marginal bullying victimization (Rees, Sabia, and Margolit 2022; Liang et al. 2023), the seasonal patterns in youth suicide and bullying victimization (as proxied by Google searches) existed prior the pandemic and have reemerged as schools have reopened.

The decrease in youth suicides during the pandemic that we document stands in contrast to popular narratives about youth mental health during the pandemic. However, we note that suicide captures one (perhaps more extreme) dimension of youth mental health. Our findings do not rule out the possibility that the average youths' mental health declined, while the mental health for those who were suffering more extreme anxiety or depression in school improved. Indeed, analysis by Yard et al. (2021) based on hospitals providing real-time surveillance data to the CDC suggests suicide attempts rose by 50 percent among young women during the pandemic. Moreover, self-reported major episodes of depression in the National Survey on Drug Use and Health rose among both youth and young adults (shown in [Online Appendix Figure A8](#)). In addition, heterogeneous responses to in-person schooling are consistent with bullying as a mechanism, as a broader set of students experienced changes in parenting and their environment when schools closed—only those experiencing in-person bullying would benefit from relief from such a suicide risk factor.

28. We note that it is likely quite difficult to find an instrument for bullying that would satisfy the exclusion restriction. For example, anti-bullying laws could directly impact youth mental health by encouraging greater monitoring of students' well-being by school staff and parents.

Importantly, our findings do not suggest school closures are an appropriate policy strategy to reduce youth suicide risks. An extensive body of research has documented long-term benefits to education, including, but not limited to, higher earnings (Angrist and Krueger 1991), lower rates of crime (Machin, Marie, and Vujčić 2011; Anderson 2014), delays in fertility (McCrary and Royer 2011), and improvements in health (Lleras-Muney 2005; Jayachandran and Lleras-Muney 2009). Time spent in school offers other benefits as well, as educators play key roles in identifying child abuse (Benson, Fitzpatrick, and Bondurant 2023), school lunches provide subsidized food and improve nutrition (Kuhn 2018), and educational time provides childcare for families, increasing the labor supply of their parents (Gelbach 2002; Cascio 2009; Fitzpatrick 2012; Price and Wasserman 2023). Furthermore, other research has shown that school closures during the pandemic had adverse consequences for children, including decreases in human capital acquisition (Bacher-Hicks, Goodman, and Mulhern 2021; Halloran et al. 2021; Kofoed et al. 2021). Our study shines a light on the continued need for additional research on the determinants of youth mental health and deeper investigation of mechanisms through which in-person schooling affects suicidal behaviors. This study also highlights the potential roles that expanded access to mental health care, anti-bullying campaigns, and other policy interventions could play in reducing the risk of teen suicide.

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