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In-Person Schooling and Youth Suicide

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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. We document three key findings. First, using data from the National Vital Statistics System from 1990-2019, we document the historical association between teen suicides and the school calendar. We show that suicides among 12-to-18-year-olds are highest during months of the school year and lowest during summer months (June through August) and also establish that areas with schools starting in early August experience increases in teen suicides in August, while areas with schools starting in September don't see youth suicides rise until September. Second, we show that this seasonal pattern dramatically changed in 2020. Teen suicides plummeted in March 2020, when the COVID-19 pandemic began in the U.S. and remained low throughout the summer before rising in Fall 2020 when many K-12 schools returned to in-person instruction. Third, using county-level variation in school reopenings in Fall 2020 and Spring 2021—proxied by anonymized SafeGraph smartphone data on elementary and secondary school foot traffic—we find that returning from online to in-person schooling was associated with a 12-to-18 percent increase teen suicides. This result is robust to controls for seasonal effects and general lockdown effects (proxied by restaurant and bar foot traffic), and survives falsification tests using suicides among young adults ages 19-to-25. Auxiliary analyses using Google Trends queries and the Youth Risk Behavior Survey suggests that bullying victimization may be an important mechanism.

Keywords: Teenage suicide; COVID-19; schools; bullying; mental health

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1. Introduction

The existence of substantial private and social returns to education is one of the most robust findings in labor economics. Additional years of schooling are associated with higher earnings (Angrist and Krueger, 1991), delays in fertility (McCrary and Royer, 2011), improvements in health (Lleras-Muney, 2005; Jayachandran and Lleras-Muney, 2009), and lower crime (Machin et al. 2011, Anderson, 2014). Time in public schools offers other benefits to children and their families as well. For instance, school is an important source of childcare, allowing for increases in parental labor supply (Gelbach, 2002; Cascio, 2009; Fitzpatrick, 2012), and educators play a key role in identifying child abuse (Benson et al., 2022).[1]

Despite the well-documented benefits of education, in-person schooling may also contribute to social inequities. Some of these mechanisms are well-known, such as how school discipline policies may create a "pipeline to prison" (Jacob and Lefgren, 2003; Bacher-Hicks et al. 2019). Less well-known is that teen suicides consistently rise during the academic year, consistent with the hypothesis that depression and stress related to time in school may lead to increases in suicide risk for youth.

Hansen and Lang (2011) were the first to identify that youth suicides consistently decrease in summer months and (to a lesser extent) over December holidays, while suicides for young adults remain unchanged. They find the seasonal decline in suicides is evident for every region of the United States and is evident in recession and booms. They investigate several potential causes including seasonal affective disorder (SAD), economic conditions, and geography. One possibility that Hansen and Lang (2011) are unable to rule out is that time spent in school could be an important contributing factor to teen suicide.

[1] Other school-related investments –such as the provision of school-subsidized meals—also provide important benefits that improve child nutrition and may spillover to improvements in learning Kuhn, 2018),

A deeper investigation into the association between school attendance and teen suicides has been hampered by the lack of exogenous variation in school calendars. School calendars are remarkably stable over time and the lack of national reporting of school calendars presents a challenge in identifying plausibly exogenous shocks to school starting dates. While there have been subtle changes in the start date of a few districts (Sims, 2008) — for instance, a modified four day calendar (Anderson and Walker, 2015) or year-round schools in rural locations (Graves 2010) — most Kindergarten through 12th grade (elementary and secondary schools) start each year between early August and early September, and finish between Memorial day and mid-June.

In this paper, we offer new evidence on the effect of in-person schooling on youth suicide using several different sources of identifying variation. First, we use cross-sectional differences in school calendars to dissect historical differences in the seasonality of youth suicide. As there are no official records of school calendars for the entire country, we use smartphone "point of interest" foot-traffic data provided by Safegraph to proxy for school calendars (Garcia and Cowan 2022; Hansen, Sabia and Schaller 2022; Parolin 2021). That is, we use data on smartphone pings at elementary and secondary schools to capture on-property activity by teachers, administrators, students, and staff to proxy for in-person instruction. We show that there is substantial geographic heterogeneity in August foot traffic both across and within states and use this variation to document differences in the seasonality of youth suicides across jurisdictions with early versus late start and end dates for the school year.

Second, we exploit the unique circumstances of the COVID-19 pandemic to capture a large national shock to school calendars. The broad long-standing societal coordination of school calendars in the United States changed abruptly in March of 2020. When the novel coronavirus SarsCov2 became recognized as a global pandemic, schools closed across much of the world, including in essentially all of the United States. While the public health trade-offs of these school closures remain uncertain, particularly with concerns around youth isolation and mental health (Mayne et al., 2021), this represents the largest national deviation from normal school calendars ever seen. We exploit this national shock to see if the pattern of seasonality of teen suicides changed in 2020.

Third, as the pandemic wore on, local school districts in the U.S. had the option to continue in an online format in Fall 2020 and Spring 2021 or to move toward hybrid learning (i.e., limited number of days/hours per-week of in-person instruction and the remainder online) or full in-person learning. Our SafeGraph data document that relative to the pre-pandemic period, smartphone foot traffic at K-12 schools varied considerably across jurisdictions and time during this period. We exploit this variation to estimate the effect of K-12 school reopenings on youth suicide, with careful attention to disentangling the effect of school reopenings from other pandemic-related shocks.

Across all of our analyses, our results repeatedly and convincingly that in-person schooling is a contributor to teen suicide. We reproduce the general seasonality identified by Hansen and Lang (2011), showing that youth suicides (suicides to those ages 18-and-under) dropped during summer months (and to a lesser extent during December holidays) in all prior years from 1990 through 2019. We also show that having despite similar overall national trends, suicides among young adults (ages 19-to-25) showed no evidence of a decline during summer months over the same period. Exploiting regional differences in the timing of school start dates (i.e., early August versus early September) and end dates (late May/early June versus late June), we find that school districts that start the academic year in August see suicides rise in August, while those that begin in September experience low youth suicide rates in August and increases in September when school begins.

When we explore the 2020 pandemic year, our results reflect a startling break in the seasonality trend in youth suicides from prior decades. We find evidence of a relative decline in youth suicides in March 2020, which is the first time a drop in youth suicides began in the spring rather than June over the span of our sample period going back to 1990. This is in fact the first time such a pattern has been observed since 1980 (Hansen and Lang 2011).

Having documented the early-pandemic decline, we proceed to study the role of school reopenings during Fall 2020 and Spring 2021. Using a difference-in-differences approach and exploiting variation across jurisdictions and over time in school reopenings, we consistently find that increased K-12 foot traffic is associated with a significant increase in teenage suicides. A causal interpretation of these estimates is supported by our use of census division-by-time fixed effects and controls for other pandemic-related shocks (through conditioning on K-12 foot traffic at restaurants and bars, macroeconomic conditions, and COVID-19 deaths). We also find little evidence that K-12 foot traffic has any effect on suicides of young adults ages 19-to-25. Our preferred estimates suggest that a move from fully closed to fully reopened schools was associated with an approximate 15 percent increase in youth suicides.

Evidence for a decrease in teen suicides in response to school closings stands in contrast to some popular narratives about teen mental health during the pandemic. However, we note that suicide captures only one part of the distribution of youth mental health. The average youth's mental health may be very differently affected from those who may be contemplating completing suicide Bacher-Hicks et al. (2022) also provide evidence that school closures may have disrupted long-standing bullying patterns, based upon decreases in bullying related queries when schools first shut down, which suggests that social conditions for some children may have improved with pandemic closures.

We close by investigating and discussing several mechanisms that could be at work to explain our findings. First, we explore the role of access to firearms (Lang and Lang 2021; Lang 2013) by examining proxies for gun ownership and laws that increase liability to parents from negligently or recklessly allowing youths' firearm access (Anderson and Sabia 2020; Anderson, Sabia, and Tekin 2021). Second, we explore the role of parental supervision by exploring whether K-12 foot traffic differently affected teen suicides completed on a weekday or weekend (the latter for which we might expect greater immediate parental monitoring). Finally, we investigate the potential contribution of bullying victimization by using GoogleTrends related queries linked with our proxies for school reopenings (Bacher et al. 2022). While we cannot rule out other mechanisms, such as changes in parental support, our results suggest that changes in exposure to bullying, which has been documented to be an important trigger for teenage suicide (Rees, Sabia, and Kumpas 2022), may be a key factor in the association between school calendars and teen suicide.

2. Data

2.1 National Vital Statistics System Mortality Data

We measure youth (ages 13-to-18) and young adult (ages 19-to-25) suicides over the period 1990 through 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). These data include individual death certificates with identifying information on the deceased persons' county of residence, cause(s) of death, as well as month and year of death.[2]

We generate county-by-month counts of suicides among school-aged teenagers ages 12-to-18. For a comparison group we follow Hansen and Lang (2018) and focus on young adults ages 19-to-25, who are no longer in middle or high school and who are either attending university, in the labor force, or idle. Figure 1a shows trends in the overall teen suicide rate over the period 1990 through 2020. From 1990-2007, there was a sharp decline in the teen suicide rate, plummeting from

[2] 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 through Sunday).

a high of 7.0 suicides per 100,000 teenagers in 1990 to 3.9 suicides per 100,000 teenagers in 2007. The post-2007 period saw a reversal in that trend, with the teen suicide rate doubling to 7.9 suicides per 100,000 teenagers in 2018. There was a 9 percent decline in the teen suicide rate from 2018 to 2019 (to about 7.1 suicides per 100,000 population), and the teen suicide rate remained steady in 2020.

The young adult (ages 19-to-25) suicide rate followed a roughly similar pattern, as shown in Figure 1b. 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 has been a sharp increase in their suicide rate through 2019 which continued through 2020.

2.2 SafeGraph Foot Traffic Data

To proxy for local school reopening policies, we use anonymized smartphone data from SafeGraph, Inc. to capture foot traffic at elementary and secondary schools. These smartphone data have been used by economists to study social mobility prior to and during the COVID-19 pandemic in the U.S. (see, e.g., Allcott et al. 2020; Cronin and Evans 2020; Dave et al. 2021; Goolsbee and Syverson 2021), and more recently by scholars studying the impact of school reopening/closing policies on health and economic wellbeing (Garcia and Cowen 2022; Hansen, Sabia, and Schaller 2022; Bravata et al., 2021; Fuchs-Schffndeln et al., 2021).

School foot traffic data are drawn from SafeGraph point-of-interest (POI) data, available for the years 2019 and 2020. These data include location-specific "pings" from 40 million anonymized cellphones whose owners did not opt out of sharing geocoded data. SafeGraph provides researchers with daily data on cellphone pings at over 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 twelfth grade schools. These data are then merged to county-by-month-year death certificate data on age-specific completed suicides.

First, we use K-12 school foot traffic in 2019 to create proxies for historical school calendars for each county. To measure when school begins, we calculate the aggregate school foot traffic on weekdays in August of 2019 for each county and divide it by the average foot traffic in September and October. To measure when school ends, 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 1 would suggest schools are fully open throughout the month, and values close to 0 would suggest schools are fully shut down.

To measure school reopenings in 2020, we follow Hansen, Sabia, and Schaller (2022), we calculate the treatment variable *K-12 Foot Traffic*, a year-specific 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. In 2020, January and February capture months prior to the onset of the pandemic in the United States in Match 2020. For example, if *K-12 Foot Traffic* took on a value of 10 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 largely online education.[3] As the value of school foot traffic increases beyond values closer to 0, this implies a mix of online and in-person schooling (hybrid teaching) while values approaching January-February levels (100) would suggest 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.[4]

[3] We omit weekends from our calculation of average K-12 school foot traffic.
[4] 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 (i.e., community members using school grounds for athletic activities) and also is subject to some GPS drift. These factors will contribute noise to our estimates.

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 is important in that it will allow us to partially disentangle the effect of school foot traffic from other pandemic-related phenomenon, including shelter-in-place orders (SIPOs), non-essential business closures (NEBCs) as well as beliefs and 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.

2.3 COVID-19 Death Data

To more fully capture pandemic-related correlates of teenage suicides, we also measure county-by-month COVID-19 cumulative deaths (*COVID-19 Deaths*), as provided by the *New York Times* from January 2020-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 et al. 2021; Gupta et al. 2020; 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 teen 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.

2.4 Other Economic and Policy Data

We collect data on county-by-year unemployment rates (*URate*) from the United States Census Bureau. We further collect information on the state-by-year divorce rate (*DivRate*), which could influence teen suicides (Hansen and Lang 2017), from the CDC. And finally, we collect data on state anti-bullying laws (*ABL*), which may affect psychological health of historically marginalized populations of students (Rees, Sabia and Kumpas 2021), from the Department of Education (2011), Sabia and Bass (2017), and Rees et al. (2021).

2.5 Data on Mechanisms

We explore one potential mechanism in depth: the role of bullying. For this we use primarily two data sources. Once source is the Youth Risk Behavior Study (YRBS) as conducted by the Centers for Disease Control. The YRBS is a nationally representative survey. We use the YRBS from 2009-2019 to studying the association between bullying behavior and youth suicidality. We also use bullying related queries from GoogleTrends to measure changes in bullying, similar to Becher et al. (2022).

3. Empirical Methods

3.1 Seasonality of Suicides Over Time

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

$$E(Suicide_{cmt}|\boldsymbol{X_{cmt}}) = exp\big[\beta_0 + \boldsymbol{\beta_m} + \boldsymbol{\tau_t} + \boldsymbol{\gamma_c} + \ln{(days * pop)} + \beta_{UR}URate_{cmt} + \beta_{DV}DivRate_{cmt} + \beta_{ABL}ABL_{cmt_i}\big] \quad (1)$$

where $Suicide_{cmt}$ is the number of suicides for teenagers ages 12-to-18 (or young adults ages 19-to-25) residing in county *c* at month *m* in year *t*. The exposure variable (the variable for which the coefficient is restricted to be 1) is the age-specific county-by-year population multiplied by the number of days in a month. Our coefficients if 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 through 2019 and 2020, allowing all of the parameters to differ in the pre- and post-COVID-19 periods.[5]

Poisson regressions are well suited to our setting given the count nature of suicides, the possibility that some counties have no 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, C., Monfort, A., and Trognon, A. 1984; Wooldridge, 2014).

4.2 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:

$$E(Suicide_{cmt}|\boldsymbol{X_{cmt}}) = exp\big[\beta_{0t} + \beta_1 K_{12}Foot\ Traffic_{cmt} + \boldsymbol{\tau_t} + \boldsymbol{\gamma_c} + \ln{(days * pop)} + \beta_{UR}URate_{cmt} + \beta_{DV}DivRate_{cmt}\ \big] \qquad (2)$$[6]

where β_1 is the parameter of interest on relative K-12 school foot traffic. 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 5th to the 95th percentile of reopening

[5] 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.
[6] We note that no state changed their anti-bullying law during the 2019-2020 peeiod, which is why this variable is omitted from the set of controls in equation (2).

(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 (5^{th} percentile) as compared to schools with likely full in-person instruction (95^{th} percentile). We also allow for non-linearities in the effect of K-12 school foot traffic by including separate indicator variables for K-12 school foot traffic above 80 percent of what it was in the pre-pandemic period, 50 to 80 percent of what it was, 20 to 50 percent of what it was, and less than 20 percent of what it was in January/February 2020.

Next, to isolate the effect of K-12 school foot traffic from the COVID-19 pandemic and seasonality effects, we also augment equation (2) with controls for restaurant and bar foot traffic, COVID-19 deaths, and summer fixed effects. The former controls will help to isolate the effect of K-12 foot traffic from other pandemic-era related shocks (i.e., lockdowns, voluntary demand-side responses to fears of infectious disease spread). The latter controls are designed to isolate the effect of county trends in school foot traffic during the academic year when schools chose differing reopening policies:

$$E(Suicide_{cmt}|\boldsymbol{X_{cmt}}) = exp\big[\beta_0 + \beta_1 K_{12} Foot\ Traffic_{cmt} + \ln(days * pop) + \beta_2 Restaurant\ Foot\ Traffic_{cmt} + \beta_3 COVID19_{Deat\quad cmt} + \beta_4 URate_{cmt} + \boldsymbol{\gamma_c} + \beta_5 Summer_m\big]^{7} \qquad (3)$$

Moreover, in some specifications, we also add controls for census division-by-year fixed effects to force "close controls" whereby we compare differences in K-12 school foot traffic within states located in the same census divisions, which may have had more similar COVID-19 mitigation policies. However, we acknowledge that controlling for common regional shocks may come at a

[7] To get a sense of the variation in K-12 relative school foot traffic that can be explained by our fixed effects, we note that in 2020 when we regress county-by-month K-12 school foot traffic on county fixed effects, we obtain an R^2 of 0.379. The addition of controls for restaurant-bar foot traffic and COVID-19 deaths increases the R^2 from the regression to 0.460. The inclusion of controls for seasonal fixed effects increases the R^2 to 0.521.

cost if neighboring jurisdictions are not the most appropriate counterfactual (i.e., because there are spillover effects of local policies or because neighboring jurisdictions follow less similar pre-treatment trends in youth suicides than non-geographically proximate jurisdictions).

To descriptively explore the common trends assumption, 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 Siegloch (2019)[8], we estimate:

$$E(Suicide_{cmt}|\boldsymbol{X_{cmt}}) = exp\big[\beta_0 + \sum_{j\neq-1} \delta_j D^{j}_{cmt} + \beta_{\mathrm{RF}} Restaurant\ Foot\ Traffic_{cmt} + \beta_{\mathrm{CV}} COVID19_{Deaths_{cmt}} + \beta_{3UR} URate_{cmt} + \boldsymbol{\tau_t} + \boldsymbol{\gamma_c} + \ln\,(days * pop) + \beta_S Summer_m + \varepsilon_{cmt}\big] \quad (4)$$

where *j* denotes event time and D^{j}_{iat} 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 across 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 80 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 more fully account for heterogeneous and dynamic treatment effects (Goodman-Bacon 2021). The specification includes the same set of controls described in equation (4), as well as controls for relative foot traffic of 20 to 80 percent. In this case, the counterfactual is defined as those counties that never exceeded 80 percent of relative foot traffic in January-February 2020 in the post-pandemic period (March 2020 to December 2020).

[8] See also Rees, Sabia, and Margolit (2021).

5. Results

Our results are shown in Tables 1 through 9 and Figures 2 through 10. Standard errors are corrected for clustering at the state level.[9]

5.1 Historic Seasonality

We first estimate the historic seasonality of suicides, comparing patterns for youth ages 12-to-18 and young adults ages 19-to-25 in Figure 2. The results, consistent with those of Hansen and Lang (2011), show that youth suicides decline in summer months and in December, times when students are generally not attending K-12 schools, while young adult suicide rates are relatively flat throughout the year (with a slight increase in summer months).

In Figure 3, we highlight the cross-sectional heterogeneity in school calendars across counties identified using relative foot traffic patterns from SafeGraph for 2019.[10] Panel A highlights large cross sectional heterogeneity across the country, as with some counties have school districts starting school at the beginning of August (or perhaps end of July) while other counties have schools that stay closed throughout August and instead open in early September. A similar pattern emerges for school foot traffic based in June (shown in Panel B), with some counties showing essentially no foot traffic June, while others have significant foot traffic throughout the first month of summer. In Panel C, we show these two measures of relative foot traffic are negatively correlated (correlation is -0.73). This is expected, as schools which start early tend to end sooner. This negative correlation also gives us confidence the foot traffic proxy reflects actual differences in school start and end dates.

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[9] A less conservative approach of clustering at the county level produces a statistically similar pattern of results.

[10] August relative foot traffic is based on average daily foot traffic on non-holiday weekdays divided by foot traffic in September and October, and June relative foot traffic is based average non-holiday week foot traffic in June divided by that of May and April.

We next examine if these regional differences in foot traffic are linked with differences in suicidality seasonality for youth. Figure 4 shows the point estimates and confidence intervals from Poisson regression models used to estimate the seasonality of suicide. The first column is based on counties in the lower tercile of foot traffic in August, which representing regions where schools likely open in September and stay open later in June. The figure in the middle column is for counties in the middle tercile, representing regions where schools likely open in the middle of August and close in early June. The third column is for counties in the upper tercile which are regions where the schools likely begin at the beginning of August or late July and shut down before Memorial Day.[11]

The findings in Figure 4 are indicative of heterogeneity in the summer decline in suicides that aligns with differences in local school districts' setting of school calendars.[12] Counties with schools that open in September (and therefore close later in June) see suicide rates remain low in August, and they also see a somewhat attenuated dip in June (column I). On the other hand, counties with schools that open at the beginning of August (column III), show an increase in suicides in August, a strong dip in June. The counties in the inter-quartile range show patterns which rely between these two extremes. Likewise, Panel B shows young adults aged 19-25 show remarkably stable suicide patterns across the entire year independent of when schools begin and end.

5.2 Changes in Seasonality in the Pandemic

The first three columns of Table 1 show estimates of the β_1s from equation (1) over the period 1990-2019. Consistent with the findings in Figure 2, we find strong evidence of seasonality

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[11] We note, this is based on foot traffic from 2019, under the assumption that school calendars have not changed much in the last 30 years. To the extent that there has been changes, then this decomposition may understate how strong the differences would be if we had school calendars for the entire 30 years to rely on.

[12] We show only the estimates for June, July, August in Figure 2, but dummy variables for each month of the year are included in the model (except for January which is the omitted category). Appendix Figure A1 shows the estimates and confidence intervals for the entire year.

in teen suicides. Without any controls (column 1), Poisson estimates show that relative to January, when nearly all K-12 schools in the United States are in session (see relative K-12 school foot traffic in January and February in Appendix Figure A3), the teen suicide rate is 15.6 ($1 - \exp^{-0.169}$) to 22.0 ($1 - \exp^{-0.248}$) percent lower during the summer months of June, July, and August, with the largest reduction observed in July. We also observe a relative decline in teenage suicides of approximately 19.0 percent in December, in which most students are not attending in-person schooling for the latter part of the month for Christmas/winter break (Appendix Figure A4). Controlling for county and year fixed effects (column 2), as well as the county unemployment rate, state divorce rate, and state anti-bullying laws (column 3) has no effect on this finding. Together, these results are consistent with the finding of Hansen and Lang (2011) suggesting that teenage suicides follow the typical U.S. academic calendar.

In the remaining columns of Table 1 (columns 4 and 5), we repeat the exercise for 2020, when the first wave of COVID-19 began in the U.S. 2020. Consistent with the observed trends in Figure 3, we find that teen suicides decline dramatically as the COVID-19 pandemic — along with school closings, shelter-in-place orders, and non-essential business closures — arrived in March, April, and May 2020. Relative to January 2020, prior to the start of the U.S. pandemic, the teen suicide rate was 25.4 ($1 - \exp^{-0.292}$) to 38 ($1 - \exp^{-0.478}$) percent in March through May of 2020. Moreover, teenage suicide rates were also somewhat lower during June and July of 2020 relative to the same comparison in the prior year. Figure 5 plots the point estimates of Table 1 scaled so the point estimates represent semi-elasticities.

In Table 2, we pool the years 1990 through 2020 and explore whether the estimated coefficients on month dummies were significantly different in 2020 relative to Januarys from 1990 and 2020, which all correspond to the pre-pandemic era. These findings suggest that the COVID-19 pandemic ushered in a very different pattern of seasonality in teenage suicides, consistent with a redistribution of suicides away from months of the year that coincided with lockdowns and school closings (March, April, and May 2020). However, while the difference is statistically indistinguishable from zero, it does appear that some of these suicides were redistributed to the Fall, where we teen suicides are observed to be approximately 5 to 10 percent higher, which coincided with partial (or full) reopening of some elementary and secondary schools.

There could be a number of explanations for these findings. Families spending time together during a period of fear and uncertainty may have generated important mental health benefits for teenagers. Moreover, increased communication between parents and teenagers, as well as increased monitoring (both of behaviors and psychological health) may have yielded important psychological gains for teenagers. In addition, the absence of in-person schooling could have helped teenagers avoid negative peer effects of in-person bullying. Or, it may be that reductions in pressures associated with high-stakes exams, athletics, or romantic relationships may have reduced triggers for suicide ideation. In the final section of this paper, we empirically explore a few potential channels.

As was clear in the descriptive statistics, the findings in Table 3 confirm that the pattern of young adult (ages 19-to-25) suicides over the year is quite different from that of teenagers. We find that over the 1990-2019 period (columns 1 through 3), young adult suicides *rose* by approximately 1-3 percent during summer months relative to Januarys. Moreover, young adult suicides were somewhat lower during Novembers and Decembers (roughly 2 to 7 percent) relative to Januarys.

We find that this pattern largely continued in 2020 (columns 4 and 5), with relative suicide increases in July and August that were even larger than in prior years, ranging from 10.2 ($\exp^{0.0975} - 1$) to 22.3 ($\exp^{0.202} - 1$) percent higher relative to January 2020. Moreover, unlike in prior years, relative suicides were also somewhat higher in September and October (on the order of 15 to 20 percent higher). As shown in Table 4, these differences in the month coefficients are significantly different in 2020 versus the 2019-2020 period. Together, the pattern of findings we uncover for young adults in the COVID-19 year of 2020 is somewhat different from that observed for teenagers and suggests that the patterns we observe for teenagers may be, 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.

5.2 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. As noted above, the coefficient can be interpreted as the effect of moving from the 5^{th} (likely closed) to the 95^{th} percentile (likely fully opened) of K-12 school foot traffic. Columns (1) through (4) focus on teenagers ages 12-to-18. For the year 2019, we find that school openings are associated with a 17.5 percent increase in teenage suicides, an effect that does not change when we control for local restaurant and bar foot traffic. 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 (approximately 24 to 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 K-12 school foot traffic effect is not simply capturing overall pandemic-related shocks.

In sharp contrast, we find no evidence that K-12 school foot traffic is related to suicides among young adults ages 19-25. The estimated effects are relatively small and are as often positive (2019) as they are negative (2020). Together, the pattern of results in Table 5 suggests that K-12 school foot traffic is likely capturing changes in suicide behaviors among those most likely to be affected.[13]

In Table 6, 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 a 17.7 ($\exp^{0.163} - 1$) percent increase in teenage suicides. The 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 — we continue to find that K-12 school openings are associated with a 14.3 percent increase in teenage suicides. Finally, in column (5), we add controls for census division-by-year fixed effects, comparing treatment and control jurisdictions across states within the same census division. In this specification, we find that moving to a full K-12 school reopening is associated with a 16.1 percent increase in youth suicides (column 5)

Panels (a) and (b) of Figure 6 show event-study analyses using our continuous foot traffic measure, following Schmidheiny and Siegloch (2019). Our results show that there is little evidence of a differential pre-treatment trend in teenage suicides between treatment and control jurisdictions, consistent with the parallel trends assumption. Following a school reopening however, we see a substantial rise in the teenage suicide rate relative to non-reopened schools. The differential is largest in the period up to 4 months following the reopening and then falls to pre-treatment levels by 5 or more months following the reopening.

[13] Appendix Table 5 shows results using state-level K-12 school foot traffic and teenage and young adult suicides with a qualitatively similar pattern as those shown in Table 5.

In columns (6) through (10) of Table (6), we show estimates of the effect of K-12 school openings on the young adult suicide rate during the 2019-2020 period. In sharp contrast to the results for teenagers, we find no evidence of a significant increase in young adult suicides following a full opening. The effects are consistently small and nowhere near statistically distinguishable from zero at conventional levels. Event-study analyses in panel (b) of Figure 6 largely confirms this null pattern of findings.

Table 7 explores whether there are any non-linearities in the effects of K-12 school foot traffic. The results provide the strongest evidence that schools that nearly fully reopen (K-12 school foot traffic >= 80% of January-February 2020 K-12 school foot traffic) see the largest increases in teenage 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 full reopening is associated with a 17.6 percent increase in teenage suicides relative to counties that did not reopen (K-12 relative school foot traffic < 20% of January-February 2020) (column 3), but with no significant change in young adult suicides (column 6).

We also note that the size of the coefficients on "hybrid" (or partial in-school instruction, or some schools within the county operating full-in person and some hybrid or online) education (>= 20% and < 80%) are still indicative of a 10 to 18 percent increase in teenage suicides, though the estimated treatment effects are imprecisely estimated. This could suggest that the margin of school reopenings that is most relevant is any in-person schooling, though the effects also indicate a modest dose-response relationship. Together, the pattern of results in Tables 6 and 7, as well as Figure 6 provide strong support for the hypothesis that school opening policies are positively associated with teenage suicides.

5.3 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 reopenings. Note, that the prior evidence suggests this concern is likely a second-order concern, as. the percentage reduction in suicides when school is not meeting in person and the riming of the effect is nearly immediate (see Figure 3). This suggests there is at least limited spatial and temporal heterogeneity in treatment effects in the past. However, to address the possibility that there could be bias introduced in our estimated effects of school reopenings during the COVID-19 pandemic due to spatial heterogeneity and dynamic treatment effects, we first isolate prominent changes in school opening policies that appear to "bite" with respect to teenage 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 80 percent relative foot traffic in the post-pandemic period (March 2020-December 2020). We control for the full set of observables described in equation (4).

In panel (a) of Figure 7, event study coefficients for teen suicides are presented. Our results suggest that in the pre-treatment period, the pattern of teenage suicide differentials between treatment and control jurisdictions is consistent with the common trends assumption. Following a prominent school reopening (an "all absorbing" state set equal to one following the first reopening), we detect evidence of an increase in teenage suicides relative to jurisdictions that remained largely closed, though we note that the estimated effects in the post-treatment period vacillate in magnitude, perhaps because not all prominent reopenings actually remain reopened in the future In contrast to the event studies shown in Figure 6, which accounts for multiple reopening/closing events by each treated unit, the event study based on Sun and Abraham (2021) coefficients requires the treatment variable turn on at the period of the first prominent increase and remain turned on for the duration of the panel. Still, in the main, our event-study findings using Sun and Abraham (2021) estimators result are consistent with our event studies shown in Figure 6 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-to-25, our event-study analysis in panel (b) provides no support for the hypothesis that K-12 school foot traffic has an important impact on their suicides.

5.3 Robustness in Main Estimates

In Appendix B, we explore the robustness of the above estimates Tables B1-B5 reproduce earlier tables using state level aggregations and variation in school traffic rather than county level variation. If county level variation is driven more by noise than signal, than state aggregations may produce estimates with less attenuation bias (Lindo, 2015). As the aggregations involve larger populations, these estimates are from OLS regressions with suicides per 100,000 as the key outcomes. The resulting estimates are similar quantitatively and qualitatively. We find that youth ages 13-to-18 show a 23 percent decline in suicides when school lets out in June. This pattern changes in 2020, with youth suicide instead declining in March and remaining at below average levels until schools reopen. In Appendix Table B5, we again study this relationship directly using school foot traffic as a proxy for school openings and closings. The results show that teenage suicide rates increase as K-12 schools move from likely fully closed to likely fully reopened. We continue to find no evidence that young adults' suicide patterns follow the K-12 school calendar.

In Appendix Tables B6-B9, we cluster the standard errors at the county level rather than the state level. The conclusions of our hypothesis testing in our main reported tables are the same.

5.4 Heterogeneity in Suicide Effects by Demographics, Substance Use, and Firearm Use

We explore heterogeneity of estimated teen suicide effects of school reopenings in Figure 8.[14] In Table C1 we focus on gender. We find that the relationship between K-12 school foot traffic for males and females is qualitatively similar, though disaggregation does decrease the precisions of the estimated relationship. This pattern of findings is consistent with the results of Hansen and Lang (2011).

In the main, the estimated relationship between school foot traffic and suicidality is similar for non-Hispanic white youth and Black youth (Table C2), but as with our gender-specific findings, disaggregation decreases precision. The estimates for Hispanic youth (Table C3), are generally smaller in magnitude (and less precisely estimated).

Tables C4 explores heterogeneity in the effects of school reopenings by age among young adults. Columns (1) through (5) focus on youth ages 12-to-15 and columns (6) to (10) focus on 16-to-18. Each column shows coefficient estimates from models similar to those described above (first with parsimonious controls and later saturated with fixed effects and observable controls). For nearly all specifications, the estimated effect for school foot traffic are larger for younger ages varying from 55 percent to 89 percent larger.

In Tables C5 and C6, we disaggregate the estimated relationship between K-12 foot traffic and suicide by suicide type. Table C5 focuses on intentional drug related suicide and non-intentional drug-related suicide. While the estimated relationship is typically larger for intentional drug related suicide, generally the relationship is also less precise. A more telling relationship shows for firearm-involved suicides vs non-firearm involved suicides. For all specifications, the estimated effects are generally larger for non-firearm involved suicides.

[14] The tables which produce this figure are in Appendix C.

5.5. Potential Mechanisms

Time spent at home with parents increased during the COVID-19 pandemic. This was driven both by the move to remote education of children, the shift to remote work by some parents, and layoffs or temporary leaves taken by to some 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.[15]

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. One useful descriptive test of the hypothesis of changed parental monitoring can be uncovered by analysis of impacts of K-12 foot traffic on completed youth suicides by whether the suicide occurred on a weekday versus weekend. Weekday vs. weekend provides a useful dimensional of heterogeneity, as Covid19 school closures and increases in work from home from increased the total amount of time families spend in the same location disproportionately on weekdays.[16]

The estimates in Table 8 suggest that there are limited differences in the estimated effect school foot traffic on youth suicidality based on day of the week. While this finding is not consistent with parental exposure as a key mechanism, 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 COVID-19 related school closures.

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[15] Leslie and Wilson (2020) find evidence that 911 calls related to domestic violence increased with the early lock-downs during the pandemic. Moreover, Baron et al. (2020) suggests that child abuse detection decreased due to school closures).

[16] Work from home may have also changed the typical work hours and days of families. However work by McDermott and Hansen (2022) suggests the increases in work on weekends was limited to around 2 hours on the weekend among a sample of workers able to work remotely.

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. Klomek et al. (2007) find evidence bullying increases depression and lowers mental health of youth and Rees, Sabia, and Gokhan (2022) show that state anti-bullying laws are associated with reductions in teenage suicide behaviors, particularly for historically vulnerable groups such as females, non-whites, and lesbian, gay, bisexual, and questioning. Eriksen, Nielsen, and Simonsen (2014) find the negative effects of bullying victimization extend to learning, as bullying worsened a variety of student academic outcomes.

We further investigate the relationship between bullying and suicidality using data from the 2009-2019 National Youth Risk Behavior Study (YRBS). The YRBS is a nationally representative survey of high school youth for a a variety of health-related behaviors and outcomes. The YRBS first asked about bullying in 2009, adding additional questions about cyber-bullying a few years later.

Figure 9 shows teen suicidality (considered, planned and attempted) based on the teen's reported bullying victimization. For each measured of suicidality, bullying victimization is associated with much higher rates of suicidality. Respectively for teens who reported considering, planning and attempting suicide, bullying victimization is associated with a 269, 290, or 320 percent increase in the risk of suicide behaviors.

While bullying is associated with substantial increases in suicide risk, the YRBS does not inform about how these risks change when school is or is not in session, as the survey is only implemented in school. Recently, Bacher-Hicks (2022) proposed an alternative proxy based on Google Trends. Through GoogleTrends for search, Google provides information on the relative search frequency of a variety of different user specified searches.

Following Bacher-Hicks et al. (2022), in Figure 10, we reproduce the association between SafeGraph K-12 school foot traffic and GoogleTrends for the search "My child is bullied." Our results show that during summer vacations in the pre-pandemic era, searches related to bullying fell. Then, when schools shut down during the beginning of the pandemic, searches for terms related to teenage bullying victimization fall in March of 2020 instead of June. Queries related to bullying then began to rise as schools reopened in person. The earlier heterogeneous effects we estimate also support this is as a mechanism, as bullying victimization is higher for younger ages.

We directly estimate the relationship between school attendance and bullying (measured by GoogleTrends proxies) in Table 8. GoogleTrends restricts geography based on how common a search is, so we focus on the searches "Bullying", "Cyber-Bullying" and "School Bullying." We rescale every state so the maximum search during 2019-2020 is 100 and estimate Poisson regressions controlling for state fixed effects, year fixed effects, and state level controls similar to those used in our earlier models. In columns (1) through (3) of Table 9, we estimate the (linear) relationship between K-12 foot traffic and queries related to bullying victimization. We find that a move from closed to fully in-person schooling is associated with a 103 percent ($\exp^{0.71} - 1$) increase bullying queries, a 75 percent increase cyber bullying queries, and 182 percent increase in school bullying queries.

In columns (4) through (6), we allow for a non-linear relationship between school foot traffic and bullying queries. A sharp non-linear relationship emerges for K-12 foot-traffic over 50 percent. Using an 80 percent relative cutoff for school foot traffic as a reasonable proxy for full reopening, we find that reopening of schools is associated with 52 percent increase in bullying queries, a 42 percent increase in cyber bullying queries, and a 93 percent increase in school bullying queries.

Could changes in youth access to firearms explain our findings? Lang and Lang (2021) find evidence the demand for guns surged during the COVID-19 pandemic, and Lang (2013) finds firearm suicides for youth increase with increased access to firearms. So if anything we would expect the surge in gun purchases during the pandemic would have increase suicide risk, and we find it decreased. Moreover we find decreases for both firearm and non-firearm suicides, as shown in Figure 8.

6. Conclusions

The findings of this study suggest that youth suicides are closely tied with in-person school attendance. We find evidence of this link based historic cross-sectional differences in school calendars and recent school closures and reopenings that occurred during the COVID-19 pandemic. Among the mechanisms we test include (1) the interrupted cycle of bullying and other stresses related to in-person schooling (Baher-Hicks et al. 2022), and (2) protective effects from more frequent interactions with family members.

While youth suicides declined when schools closed during the COVID-19 pandemic, the interruption in the rise of youth suicides was short lived. We find youth suicides levels have increased as schools have reopened. Moreover, this comes at a time when there has also been a consistent upward trend in youth suicide since 2006 that raises many concerns. Despite the promise that anti-bullying laws may have in reducing marginal bullying victimization (Rees, Sabia, and Kumpas 2022), the seasonal pattern in youth suicide and bullying related queries existed prior the pandemic and have reemerged as schools have reopened.

Lastly, our results should not be interpreted as supporting a policy of school closures to reduce youth suicide risks. There are substantial long-term benefits to education, including, but not limited to, higher earnings, improved health, and reduced criminality. A growing body of research shows school shut-downs had many other adverse spillover effects including decreases in human capital acquisition for children (Bacher-Hick et al. 2021, Halloran et al. 2021; Kofoed et al. 2021) and reduced attachment to the labor force for married mothers (Hansen, Sabia, and Schaller 2021). Moreover, it is possible that the average student saw decreases in her/his mental health due to school closures, and only a small, but important, subset of the most vulnerable saw improvements due to the stress than in-person schooling created for them. Indeed, analyses of real-time hospital surveillance data by Yard et al. (2021) suggests suicide attempts rose by 50 percent among young women during the pandemic. Meanwhile, self-reported major episodes of depression rose among both youth and young adults (see data from the National Survey on Drug Use and Health in Figure A6). Rather our research shines a light on the continued need for more research on youth mental health and deeper investigation why it declines for some students when school is in session. Likewise, future research and policy could focus on the determinants and consequences of bullying victimization, and the role that other policies — such as access to mental health care and safe storage of guns — could play in reducing these risks.

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Figures and Tables

Figure 1. Month-by-Year Teenage (Ages 12-to-18) Suicide Rate Per 100,000 Population

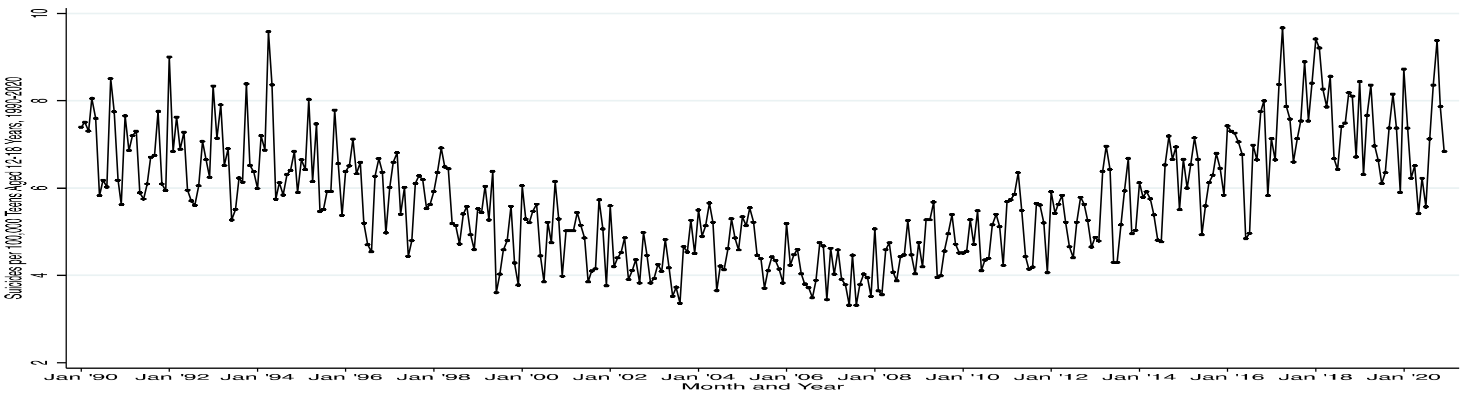
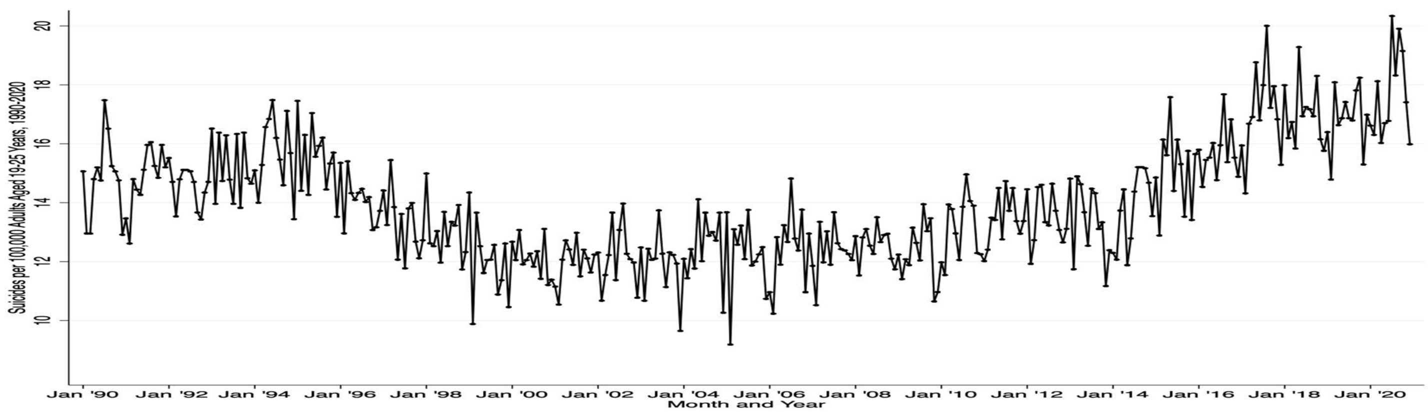
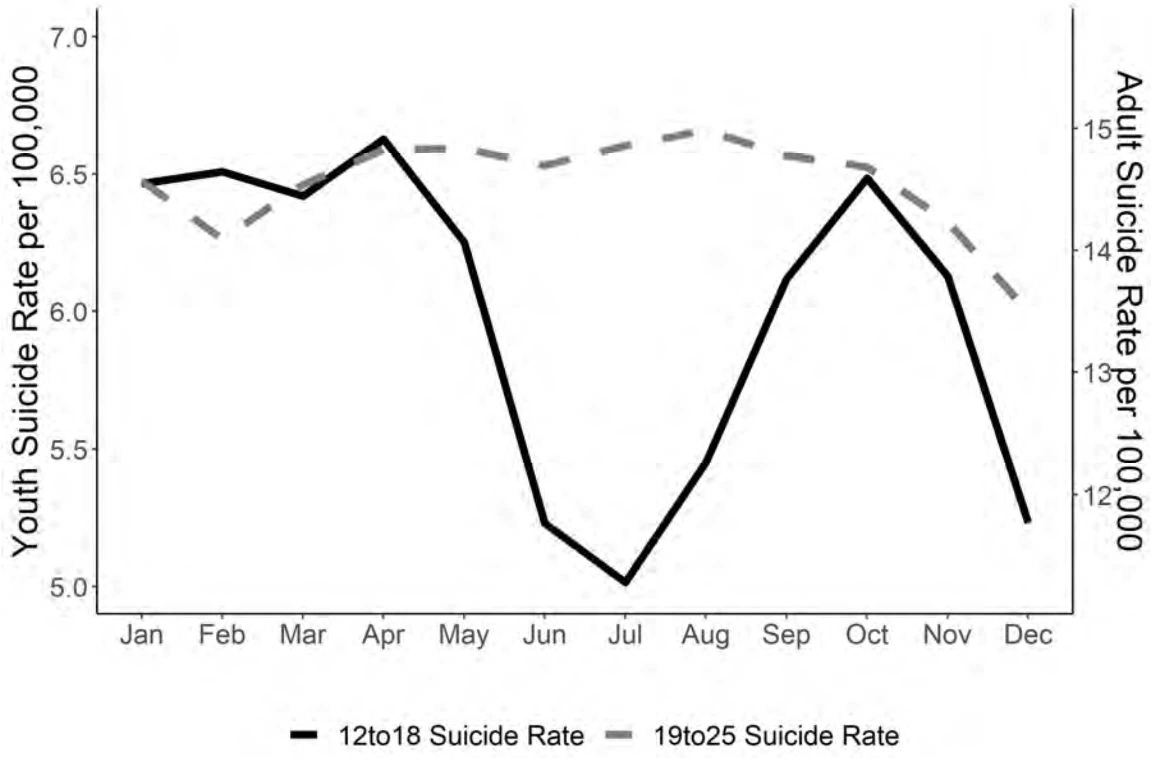


Figure 1b. Month-by-Year Young Adult (Ages 19-to-25) Suicide Rate Per 100,000 Population



Notes: Based on multiple cause of death files collected by the National Center for Health Statistics.

Figure 2. Monthly Suicide Rate Per 100,000 Population, 1990-2019

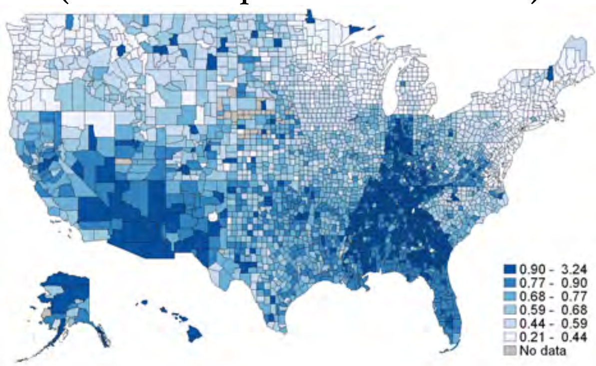


Notes: Based on annualized suicide rates from the multiple cause of death records from the National Center of Health Statistics, 1990-2019.

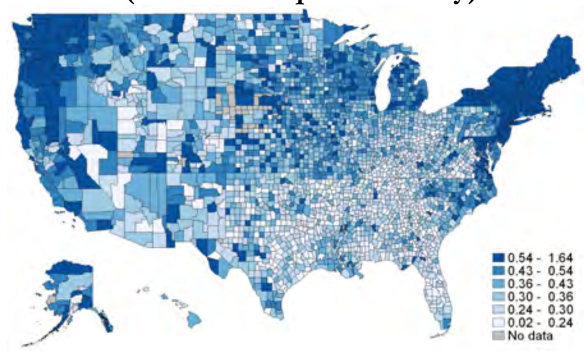
Figure 3:

Differences in School Calendars Based on Relative Foot Traffic

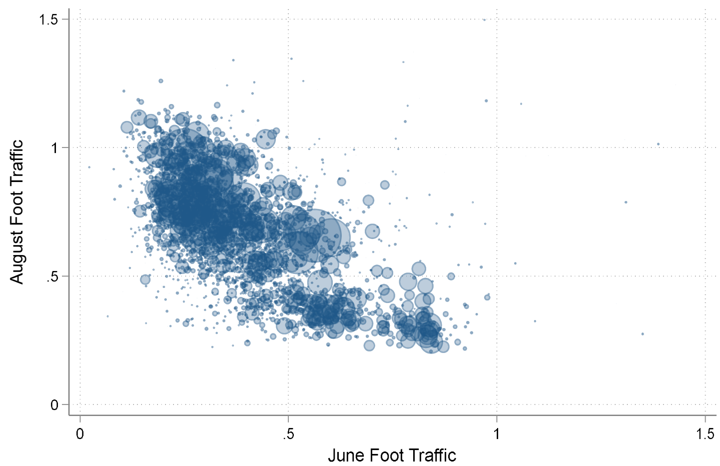
Panel A
August K-12 Foot Traffic
(relative to September and October)



Panel B
June Foot K-12 Foot Traffic
(relative to April and May)



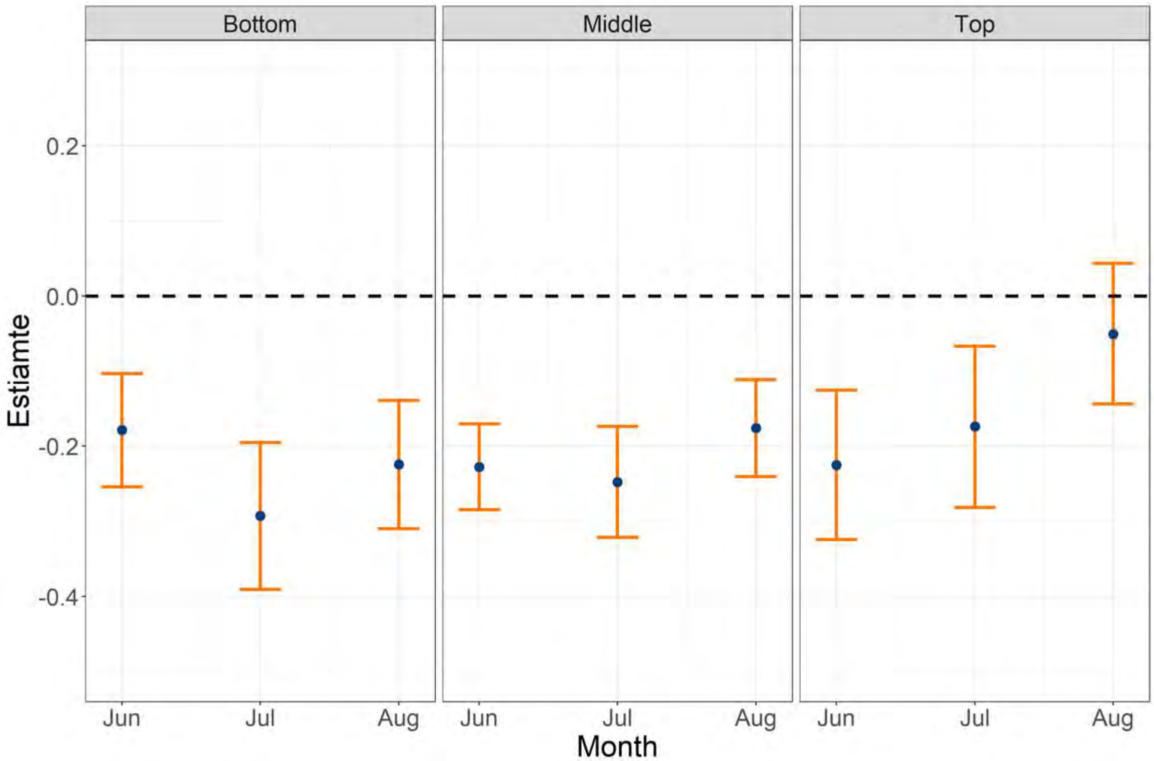
Panel C
Correlation of Relative August and Relative June Foot Traffic



Note: 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 non-holiday weekday foot traffic in August to average non-holiday weekday foot traffic in September and October. Relative foot traffic in June compares average non-holiday weekday foot traffic in June to average non-holiday weekday foot traffic in

Figure 4:

Terciles of Relative August Foot Traffic in 2019
and Historic Seasonality of Youth Suicide, 1999-2019

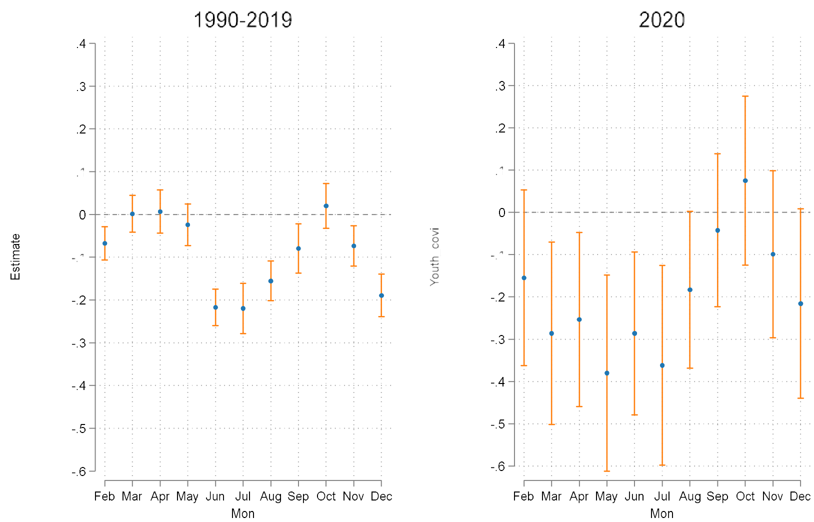


Notes: Based on point estimates and 95 percent confidence intervals of the differences in suicide rates for calendar month of the year from Poisson regression models using suicides from 1990-2019. January is the omitted based category. All models control for county fixed and year fixed effects, and cluster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

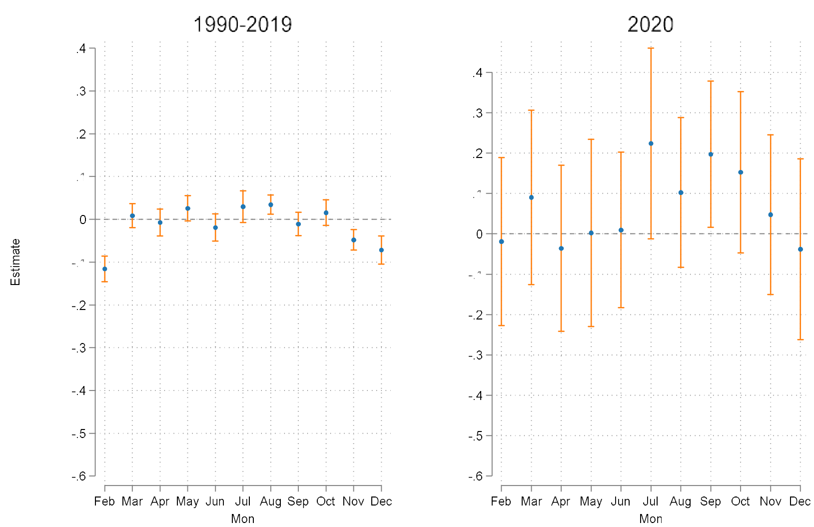
Figure 5:

Historic Seasonality of Suicides 1990-2019 vs. 2020

Panel A: Youth 12-18 Suicides



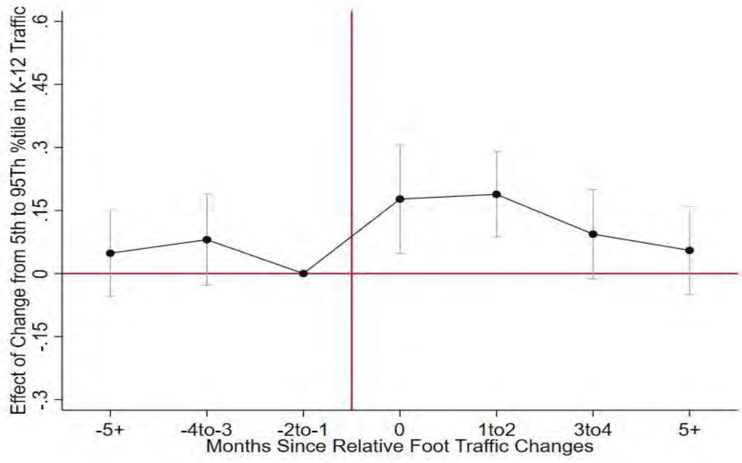
Panel B
Young Adult 19-25 Suicides



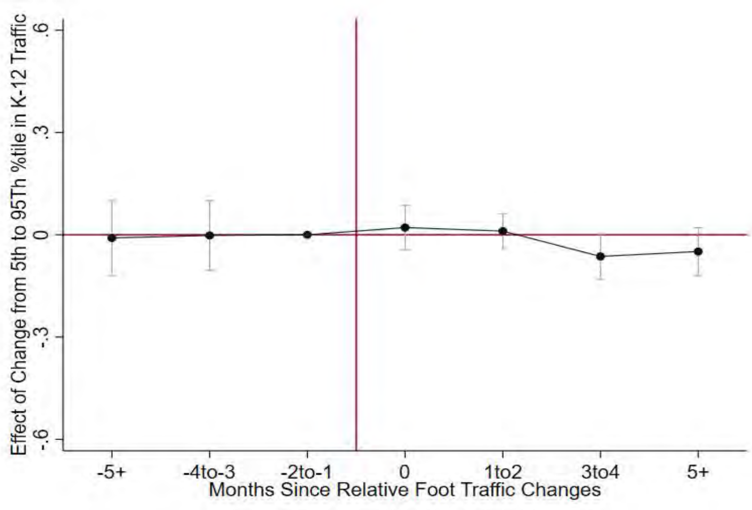
Notes: Based on estimates and 95 percent confidence intervals of the differences in suicide rates for calendar month of the year from Poisson regression models using suicides from 1990-2019. January is the omitted based category. All models control for county fixed and year fixed effects, and cluster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

Figure 6: Event-Study Analysis of K-12 Foot Traffic and Suicides, Using Two-Way Fixed Effects Estimates

Panel A Teenagers Ages 12-to-18



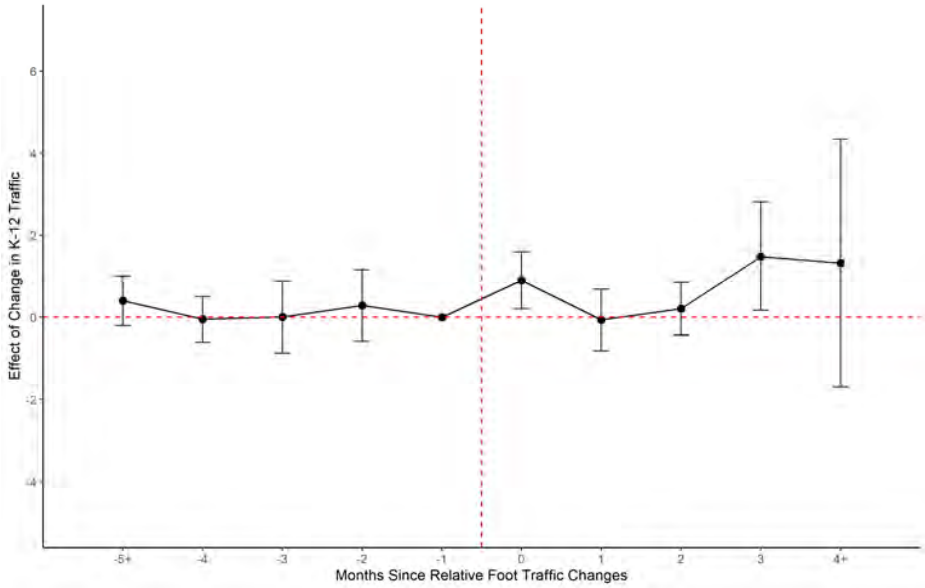
Panel B Young Adults Ages 19-to-25



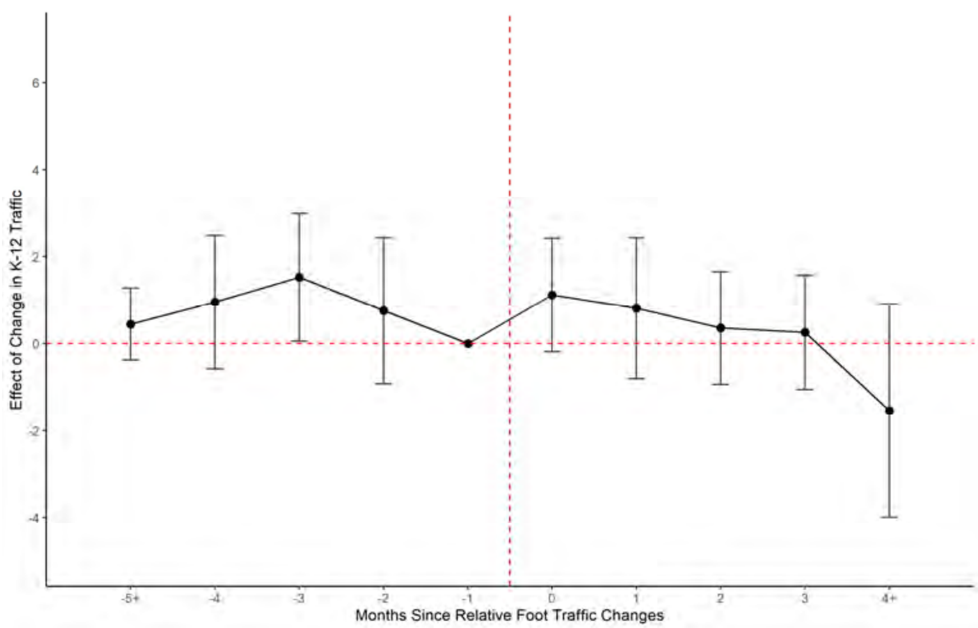
Notes: Each regression is weighted by the population in each county. All models include controls for foot traffic into restaurants, COVID-19 mortality, income, unemployment rate, divorce rate, and summer months fixed effect. COVID-19 deaths are coded as zero until the first documented COVID-19 related deaths, which occurred in March 2020. The vertical bars represent 95% confidence interval generated using standard errors clustered at the state level.

Figure 7: Event-Study Analysis of Prominent Increases in K-12 School Foot Traffic ("Full Reopening", Using Sun and Abraham (2020) Estimates

Panel A: Teenagers Ages 12-to-18

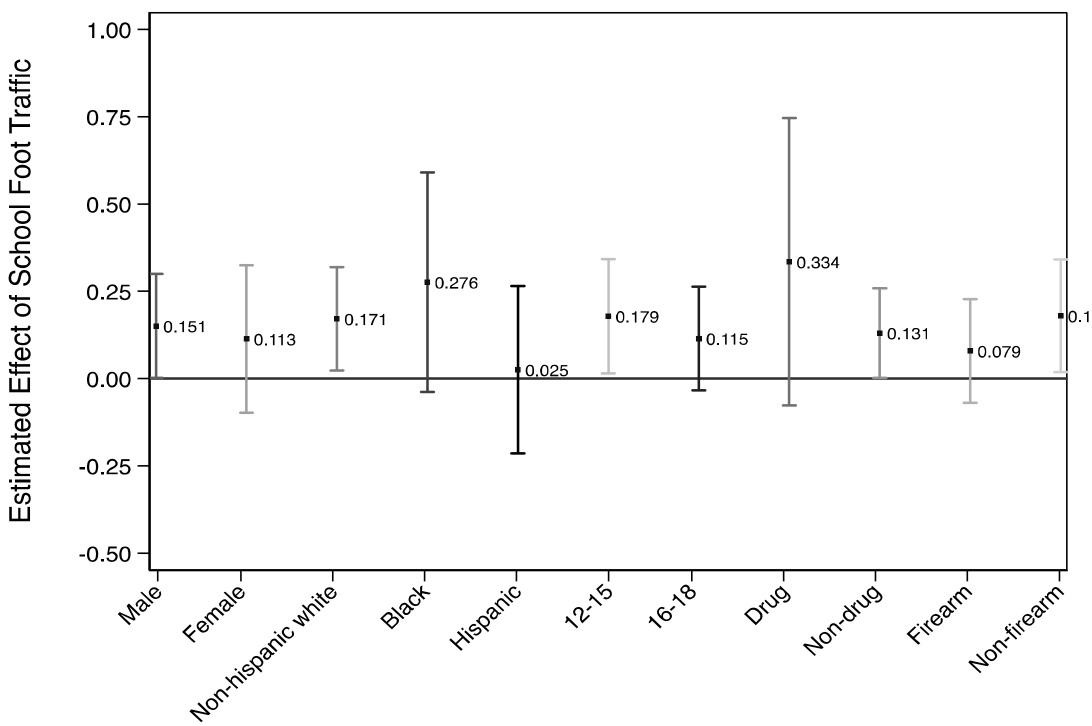


Panel B: Young Adults Ages 19-to-25



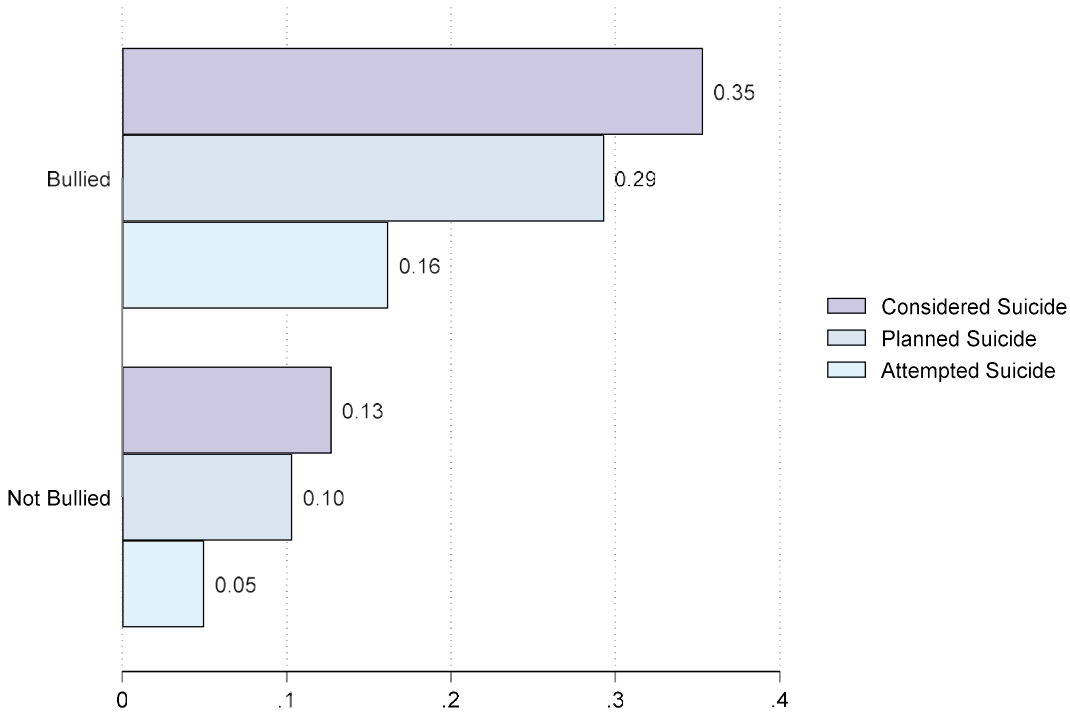
Notes: Each regression is weighted by the population in each county. All models include controls for foot traffic into restaurants, COVID-19 mortality, income, unemployment rate, divorce rate, summer months fixed effect, and smaller reopening status of 20% or higher between March and December of 2020. COVID-19 deaths are coded as zero until the first documented COVID-19 related deaths, which occurred in March 2020. The vertical bars represent 95% confidence interval generated using standard errors clustered at the state level

Figure 8: Heterogeneity in Estimated Effect of K-12 School Foot Traffic on Teenage (Ages 12-to-18) Suicides, by Demographic Characteristics and Suicide Circumstances



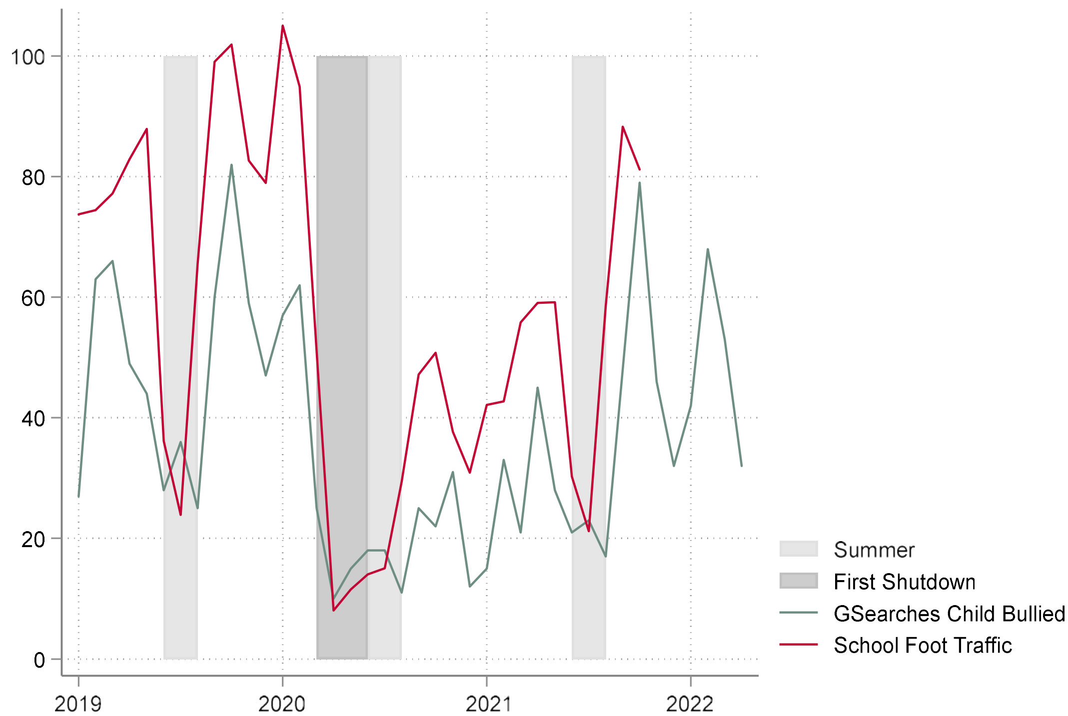
Notes: The figure presents estimated treatment effects and 90 percent confidence intervals around the estimated treatment effects of a move from the 5th to 95th percentile of relative school foot traffic on teenage suicides using monthly data for the period 2019-2020. The first two estimates present results by gender, the next three by race/ethnicity, the following 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 forearm. 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.

Figure 9: Teen Suicidality and Bullying



Notes: Based on self-reported bullying and suicidality from the National Youth Risk Behavior Study (YRBS) for 2009-2019. The YRBS is a representative survey collected bi-annually from the Centers of Disease Control.

Figure 10: Google Trends of Bullying Searches and Schools Foot Traffic



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

Table 1. Poisson Estimates of Seasonality of County-Level Teenage (Ages 12-to-18) Suicides, 1990-2019 vs. 2020

	1990-2019			2020	
	(1)	(2)	(3)	(4)	(5)
February	0.0233	0.0234	0.0236	-0.102	-0.102
	(0.0192)	(0.0192)	(0.0193)	(0.104)	(0.104)
March	0.00210	0.00192	0.00146	-0.337***	-0.337***
	(0.0217)	(0.0217)	(0.0216)	(0.108)	(0.108)
April	0.0397	0.0395	0.0395	-0.260**	-0.260**
	(0.0252)	(0.0252)	(0.0252)	(0.103)	(0.103)
May	-0.0243	-0.0244	-0.0244	-0.478***	-0.478***
	(0.0244)	(0.0244)	(0.0244)	(0.116)	(0.116)
June	-0.212***	-0.212***	-0.212***	-0.305***	-0.305***
	(0.0214)	(0.0214)	(0.0214)	(0.0963)	(0.0963)
July	-0.248***	-0.248***	-0.248***	-0.449***	-0.449***
	(0.0294)	(0.0294)	(0.0293)	(0.118)	(0.118)
August	-0.169***	-0.169***	-0.169***	-0.202**	-0.202**
	(0.0231)	(0.0231)	(0.0232)	(0.0928)	(0.0928)
September	-0.0502*	-0.0502*	-0.0501*	-0.0104	-0.0104
	(0.0288)	(0.0288)	(0.0289)	(0.0906)	(0.0906)
October	0.0199	0.0197	0.0198	0.0724	0.0724
	(0.0262)	(0.0262)	(0.0261)	(0.100)	(0.100)
November	-0.0434*	-0.0436*	-0.0438*	-0.0710	-0.0710
	(0.0235)	(0.0234)	(0.0237)	(0.0989)	(0.0989)
December	-0.210***	-0.210***	-0.210***	-0.243**	-0.243**
	(0.0246)	(0.0246)	(0.0249)	(0.112)	(0.112)
Observations	1,129,323	1,129,323	1,129,323	37,692	37,692
County Fixed Effects?	No	Yes	Yes	No	Yes
Year Fixed Effects?	No	Yes	Yes	No	Yes
County-State-by-Year Controls?	No	No	Yes	No	No

*** p<0.01, ** p<0.05, * p<0.1

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. Per-capita income has been adjusted to 2020 dollars. Due to perfect prediction of the outcome from indicator controls, the coefficients in columns (2) and (3) are identified from 91% and 76% of the listed observations in column (1), respectively. For column (5), the coefficients are identified from 30% of the observations listed in column (4). Observable annual controls include the county unemployment rate, county personal income (0000s of 2020$), the state divorce rate, and the presence of a state anti-bullying law.

Table 2. Poisson Estimates of Change in Seasonality of County-Level Teenage (Ages 12-to-18) Suicides, 2020 vs 2000-2019

	(1)	(2)	(3)
February*Year_2020	-0.125	-0.125	-0.125
	(0.105)	(0.105)	(0.105)
March* Year_2020	-0.340***	-0.339***	-0.339***
	(0.112)	(0.112)	(0.112)
April* Year_2020	-0.299***	-0.299***	-0.299***
	(0.110)	(0.110)	(0.110)
May*Year_2020	-0.454***	-0.454***	-0.454***
	(0.121)	(0.122)	(0.122)
June*Year_2020	-0.0925	-0.0923	-0.0925
	(0.0960)	(0.0960)	(0.0959)
July*Year_2020	-0.201	-0.201	-0.201
	(0.126)	(0.126)	(0.126)
August*Year_2020	-0.0337	-0.0335	-0.0331
	(0.0943)	(0.0943)	(0.0944)
September*Year_2020	0.0398	0.0398	0.0397
	(0.0953)	(0.0953)	(0.0953)
October*Year_2020	0.0526	0.0528	0.0526
	(0.109)	(0.109)	(0.109)
November*Year_2020	-0.0276	-0.0274	-0.0273
	(0.105)	(0.105)	(0.106)
December*Year_2020	-0.0335	-0.0334	-0.0332
	(0.114)	(0.114)	(0.114)
February	0.0233	0.0234	0.0236
	(0.0192)	(0.0192)	(0.0193)
March	0.00210	0.00192	0.00146
	(0.0217)	(0.0217)	(0.0216)
April	0.0397	0.0395	0.0395
	(0.0252)	(0.0252)	(0.0252)
May	-0.0243	-0.0244	-0.0244
	(0.0244)	(0.0244)	(0.0244)
June	-0.212***	-0.212***	-0.212***
	(0.0214)	(0.0214)	(0.0214)
July	-0.248***	-0.248***	-0.248***
	(0.0294)	(0.0294)	(0.0293)
August	-0.169***	-0.169***	-0.169***
	(0.0231)	(0.0231)	(0.0232)
September	-0.0502*	-0.0502*	-0.0501*
	(0.0288)	(0.0288)	(0.0289)
October	0.0199	0.0197	0.0198
	(0.0262)	(0.0262)	(0.0261)
November	-0.0434*	-0.0436*	-0.0438*
	(0.0235)	(0.0234)	(0.0237)
December	-0.210***	-0.210***	-0.210***
	(0.0246)	(0.0246)	(0.0249)

Observations	1,167,015	1,167,015	1,167,015
County Fixed Effects?	No	Yes	Yes
Year Fixed Effects?	No	Yes	Yes
County-State-by-Year Controls?	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1

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. Per-capita income has been adjusted to 2020 dollars. Due to perfect prediction of the outcome from indicator controls, the coefficients in columns (2) and (3) are identified from 89% and 79% of the listed observations in column (1), respectively. Observable annual controls include the county unemployment rate, county personal income (0000s of 2020$), the state divorce rate, and the presence of a state anti-bullying law.

Table 3. Poisson Estimates of Effect of Seasonality on County-Level Young Adult (Ages 19-to-25) Suicides, 1990-2019 vs. 2020

	1990-2019			2020	
	(1)	(2)	(3)	(4)	(5)
February	-0.0302**	-0.0300**	-0.0297**	0.0475	0.0475
	(0.0149)	(0.0149)	(0.0149)	(0.0823)	(0.0823)
March	0.00869	0.00867	0.00867	0.0866	0.0866
	(0.0142)	(0.0142)	(0.0142)	(0.0763)	(0.0763)
April	0.0252	0.0252	0.0254	-0.00358	-0.00358
	(0.0158)	(0.0158)	(0.0158)	(0.0671)	(0.0671)
May	0.0253*	0.0252*	0.0255*	0.00238	0.00238
	(0.0147)	(0.0147)	(0.0147)	(0.0891)	(0.0891)
June	0.0133	0.0133	0.0135	0.0423	0.0423
	(0.0158)	(0.0158)	(0.0158)	(0.0757)	(0.0757)
July	0.0294	0.0293	0.0293	0.202***	0.202***
	(0.0185)	(0.0185)	(0.0185)	(0.0739)	(0.0739)
August	0.0342***	0.0341***	0.0338***	0.0975	0.0975
	(0.0113)	(0.0113)	(0.0113)	(0.0722)	(0.0722)
September	0.0220	0.0220	0.0220	0.213***	0.213***
	(0.0135)	(0.0135)	(0.0135)	(0.0626)	(0.0626)
October	0.0156	0.0155	0.0155	0.142**	0.142**
	(0.0149)	(0.0149)	(0.0149)	(0.0552)	(0.0552)
November	-0.0164	-0.0164	-0.0164	0.0793	0.0793
	(0.0120)	(0.0120)	(0.0120)	(0.0583)	(0.0583)
December	-0.0745***	-0.0745***	-0.0744***	-0.0388	-0.0388
	(0.0164)	(0.0164)	(0.0164)	(0.0734)	(0.0734)
Observations	1,129,323	1,129,323	1,129,323	37,692	37,692
County Fixed Effects?	No	Yes	Yes	No	Yes
Year Fixed Effects?	No	Yes	Yes	No	Yes
County-State-by-Year Controls?	No	No	Yes	No	No

*** p<0.01, ** p<0.05, * p<0.1

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. Per-capita income has been adjusted to 2020 dollars. Due to perfect prediction of the outcome from indicator controls, the coefficients in columns (2) and (3) are identified from 96% and 86% of the listed observations in column (1), respectively. For column (5), the coefficients are identified from 47% of the observations listed in column (4). Observable annual controls include the county unemployment rate, county personal income (00000s of 2020$), the state divorce rate, and the presence of a state anti-bullying law.

Table 4. Poisson Estimates of Differential Effect of Seasonality on County-Level Young Adult Suicides, 2020 vs 2000-2019

	(1)	(2)	(3)
February*Year_2020	0.0765	0.0767	0.0764
	(0.0832)	(0.0833)	(0.0833)
March* Year_2020	0.0774	0.0777	0.0777
	(0.0794)	(0.0794)	(0.0794)
April* Year_2020	-0.0292	-0.0291	-0.0293
	(0.0688)	(0.0688)	(0.0688)
May*Year_2020	-0.0236	-0.0232	-0.0235
	(0.0893)	(0.0894)	(0.0894)
June*Year_2020	0.0275	0.0278	0.0275
	(0.0841)	(0.0841)	(0.0841)
July*Year_2020	0.172**	0.172**	0.172**
	(0.0798)	(0.0798)	(0.0798)
August*Year_2020	0.0624	0.0625	0.0629
	(0.0751)	(0.0751)	(0.0751)
September*Year_2020	0.191***	0.191***	0.191***
	(0.0622)	(0.0622)	(0.0622)
October*Year_2020	0.126**	0.126**	0.126**
	(0.0556)	(0.0556)	(0.0555)
November*Year_2020	0.0942	0.0944	0.0944
	(0.0587)	(0.0587)	(0.0587)
December*Year_2020	0.0345	0.0351	0.0350
	(0.0768)	(0.0768)	(0.0768)
February	-0.0290*	-0.0292*	-0.0290*
	(0.0150)	(0.0149)	(0.0149)
March	0.00925	0.00894	0.00894
	(0.0142)	(0.0142)	(0.0142)
April	0.0256	0.0256	0.0258
	(0.0158)	(0.0158)	(0.0158)
May	0.0260*	0.0256*	0.0259*
	(0.0147)	(0.0147)	(0.0147)
June	0.0148	0.0145	0.0148
	(0.0158)	(0.0158)	(0.0158)
July	0.0298	0.0297	0.0296
	(0.0185)	(0.0185)	(0.0185)
August	0.0351***	0.0349***	0.0346***
	(0.0114)	(0.0113)	(0.0114)
September	0.0224*	0.0220	0.0220
	(0.0135)	(0.0135)	(0.0135)
October	0.0159	0.0158	0.0158
	(0.0149)	(0.0149)	(0.0149)
November	-0.0149	-0.0151	-0.0151
	(0.0122)	(0.0122)	(0.0122)
December	-0.0734***	-0.0740***	-0.0739***
	(0.0165)	(0.0165)	(0.0164)

Observations	1,167,328	1,167,328	1,167,328
County Fixed Effects?	No	Yes	Yes
Year Fixed Effects?	No	Yes	Yes
County-State-by-Year Controls?	No	No	Yes

*** p<0.01, ** p<0.05, * p<0.1

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.Per-capita income has been adjusted to 2020 dollars. Due to perfect prediction of the outcome from indicator controls, the coefficients in columns (2) and (3) are identified from 95% and 84% of the listed observations in column (1), respectively. Observable annual controls include the county unemployment rate, county personal income (0000s of 2020$), the state divorce rate, and the presence of a state anti-bullying law.

Table 5. Poisson Estimates of Effect of County-Level K-12 Foot Traffic on Teenage and Young Adult Suicides, 2019 vs 2020

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Teenagers Ages 12-to-18				**Young Adults Ages 19-to-25**			
	2019		2020		2019		2020	
K-12 Foot Traffic	0.169**	0.166**	0.219***	0.234***	0.0454	0.0489	-0.0308	-0.0438
	(0.0687)	(0.0693)	(0.0604)	(0.0879)	(0.0423)	(0.0429)	(0.0361)	(0.0486)
Restaurant-Bar Foot Traffic		-0.0558		0.110		0.0640		0.0731
		(0.0747)		(0.153)		(0.0551)		(0.0570)
Any COVID Deaths				0.0555				0.0235
				(0.0549)				(0.0455)
County Death Rate				1.633***				0.287
				(0.594)				(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

*** p<0.01, ** p<0.05, * p<0.1

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.

Table 6. Poisson Estimates of Effect of County-Level K-12 Foot Traffic on Teenage and Young Adult Suicides, Pooled 2019 and 2020

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

*** p<0.01, ** p<0.05, * p<0.1
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.

Table 7. Exploration of Non-Linear Effects of K-12 School Foot Traffic on Teenage and Young Adult Suicides, Pooled 2019 and 2020

	Teenagers Ages 12-to-18			Young Adults Ages 19-to-25		
	(1)	(2)	(3)	(4)	(5)	(6)
K-12 Foot Traffic >=80%	0.251***	0.148*	0.162**	-0.0130	0.00203	-0.000518
	(0.0591)	(0.0778)	(0.0754)	(0.0371)	(0.0392)	(0.0393)
50% <=K-12 Foot Traffic <80%	0.200***	0.107	0.111	-0.0291	-0.0153	-0.0156
	(0.0708)	(0.0868)	(0.0863)	(0.0282)	(0.0327)	(0.0327)
20% <= K-12 Foot Traffic <50%	0.105*	0.0735	0.0717	-0.0302	-0.0259	-0.0289
	(0.0576)	(0.0606)	(0.0598)	(0.0234)	(0.0235)	(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

*** p<0.01, ** p<0.05, * p<0.1

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% of the Jan/Feb 2020 level.

Table 8. Heterogeneous Effects of School Foot Traffic on Teen and Young Adult Suicide Based on Weekdays/Weekends

	Teenagers Ages 12-to-18					**Young Adults Ages 19-to-25**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						Panel I: Weekdays (M-Th)				
K-12 Foot Traffic	0.165**	0.243***	0.246***	0.121	0.138	0.0102	0.0139	-0.000234	0.0260	0.0264
	(0.0646)	(0.0648)	(0.0668)	(0.0905)	(0.0891)	(0.0223)	(0.0225)	(0.0261)	(0.0287)	(0.0282)
						Panel II: Weekends (F-Sun)				
K-12 Foot Traffic	0.162**	0.125	0.204**	0.141	0.152	-0.0853**	-0.0428	-0.0256	-0.0653	-0.0666
	(0.0703)	(0.0787)	(0.0941)	(0.122)	(0.121)	(0.0369)	(0.0346)	(0.0483)	(0.0520)	(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

*** p<0.01, ** p<0.05, * p<0.1

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.

Table 9: School Foot Traffic And Google Searches for Bullying

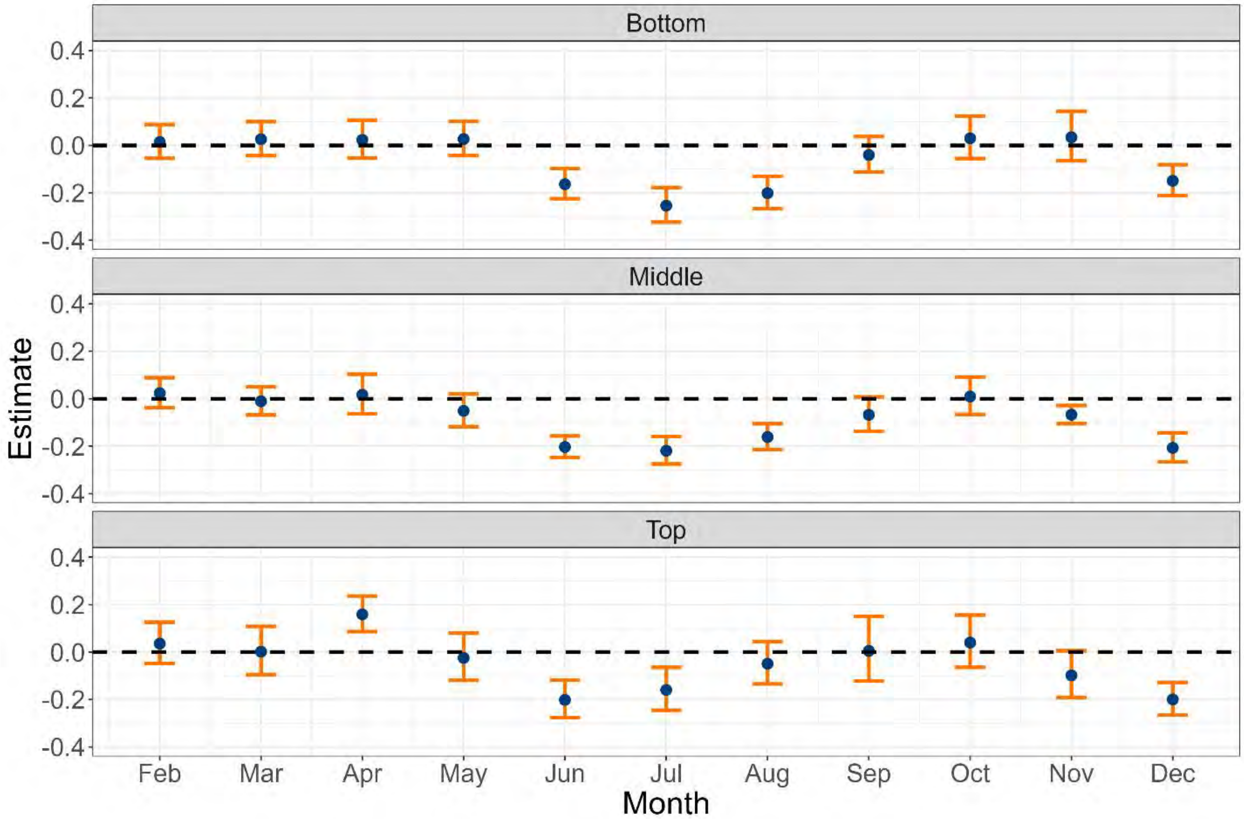
	Bullying	Cyber Bullying	School Bullying	Bullying	Cyber Bullying	School Bullying
	(1	(2)	(3)	(4)	(5)	(6)
K-12 Foot Traffic	0.71***	0.56***	1.04***			
	(0.065)	(.09)	(.12)			
K-12 Foot Traffic >=80%				.42***	.35***	0.66***
				(.04)	(.07)	(0.10)
50% <=K-12 Foot Traffic <80%				.32***	.32***	0.53***
				(.023)	(.06)	(.07)
20% <= K-12 Foot Traffic <50%				.07***	.04***	0.14***
				(.02)	(.04)	(.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

*** p<0.01, ** p<0.05, * p<0.1

Notes: Standard errors are clustered at the state level. Each estimated is from a Poisson regression. The reference group K-12 school foot traffic less than 20% of the Jan/Feb 2020 level for columns 4-6.

Appendix A:
Ancillary Exhibits

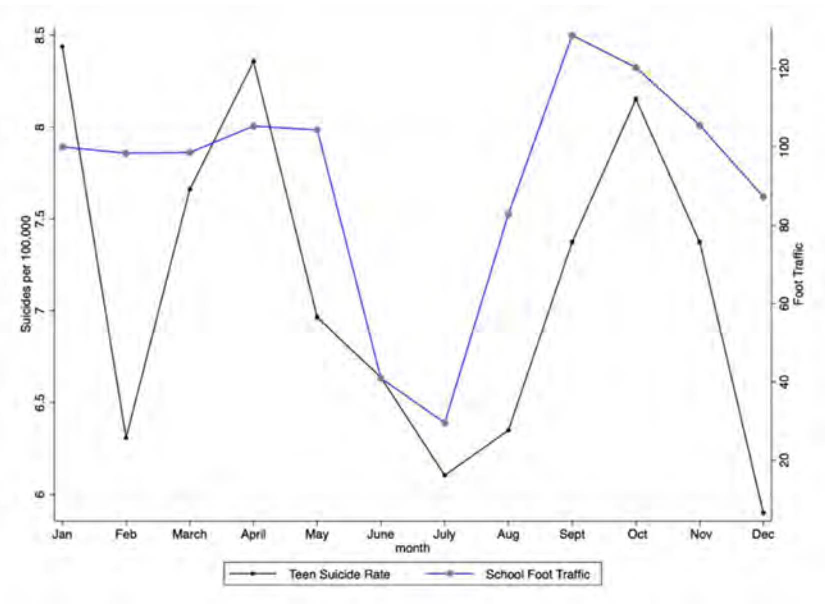
Appendix Figure A1 : Relative August Foot Traffic Terciles and Teen Suicide Seasonality



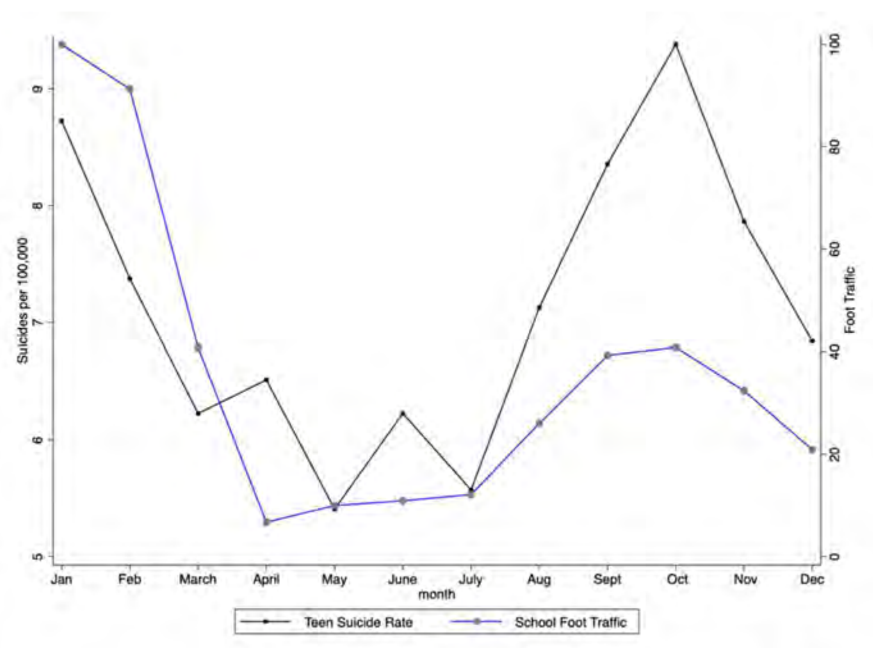
Notes: Based on point estimates and 95 percent confidence intervals of the differences in suicide rates for calendar month of the year from Poisson regression models using suicides from 1990-2019. January is the omitted based category. All models control for county fixed and year fixed effects, and cluster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

Appendix Figure A2. Monthly Teenage (Ages 12-to-18) Suicide Rate Per 100,000 Population and Relative School Foot Traffic, 2019 and 2020

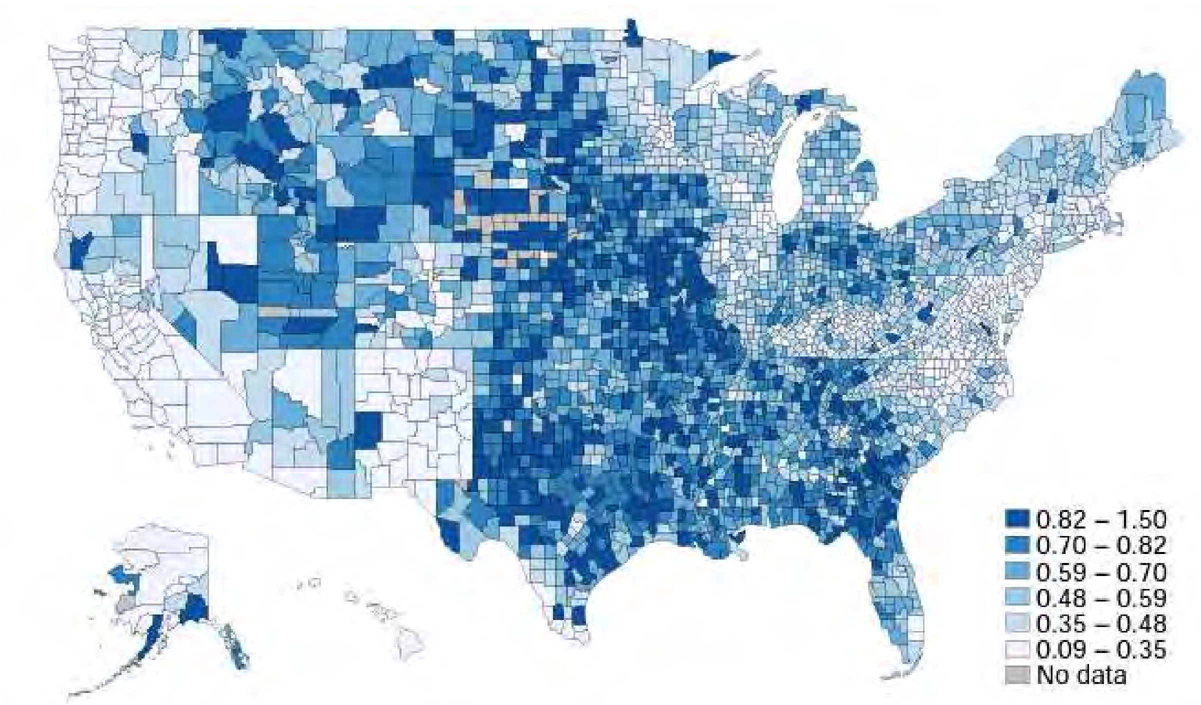
Panel (a): Monthly Teenage Suicide Rate, 2019



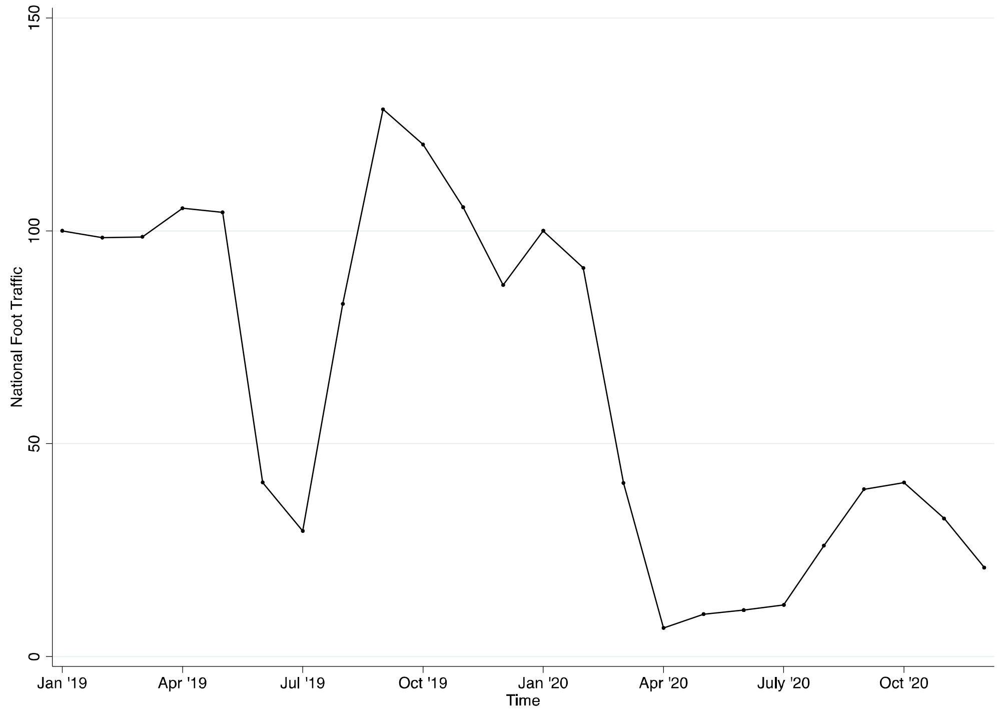
Panel (b): Monthly Teenage Suicide Rate, 2020



Appendix Figure A3. Variation in School Foot Traffic Relative to Jan/Feb 2020, Reproduced from Hansen, Sabia and Schaller (2022)

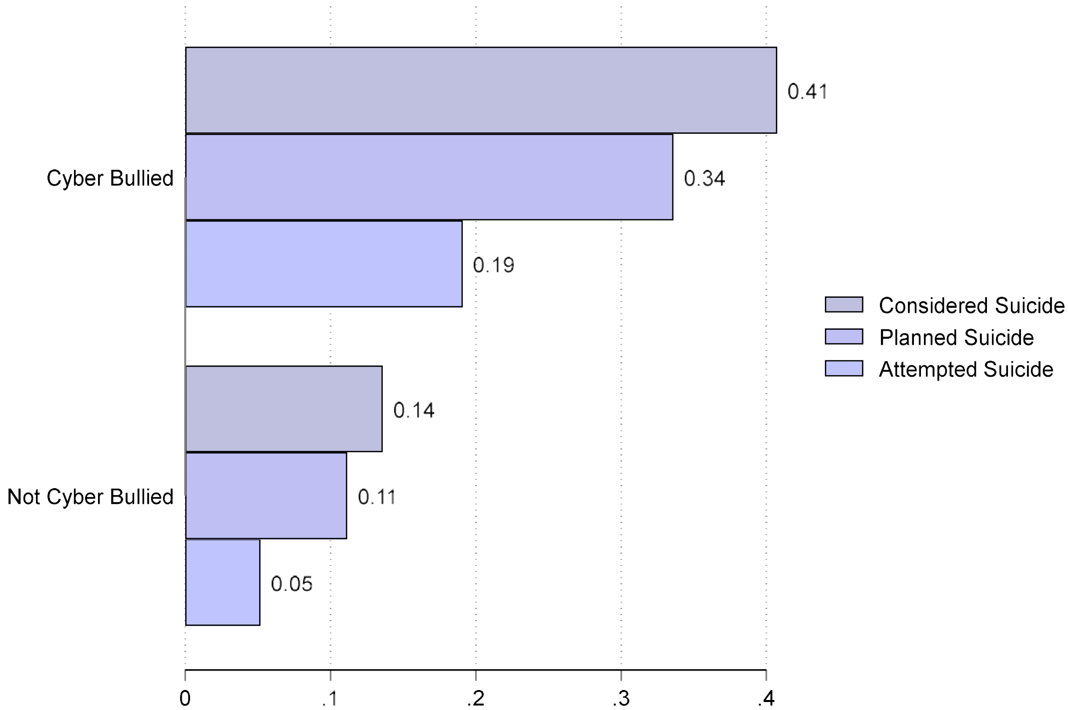


Appendix Figure A4. National Trends in K-12 Foot Traffic, 2019 and 2020



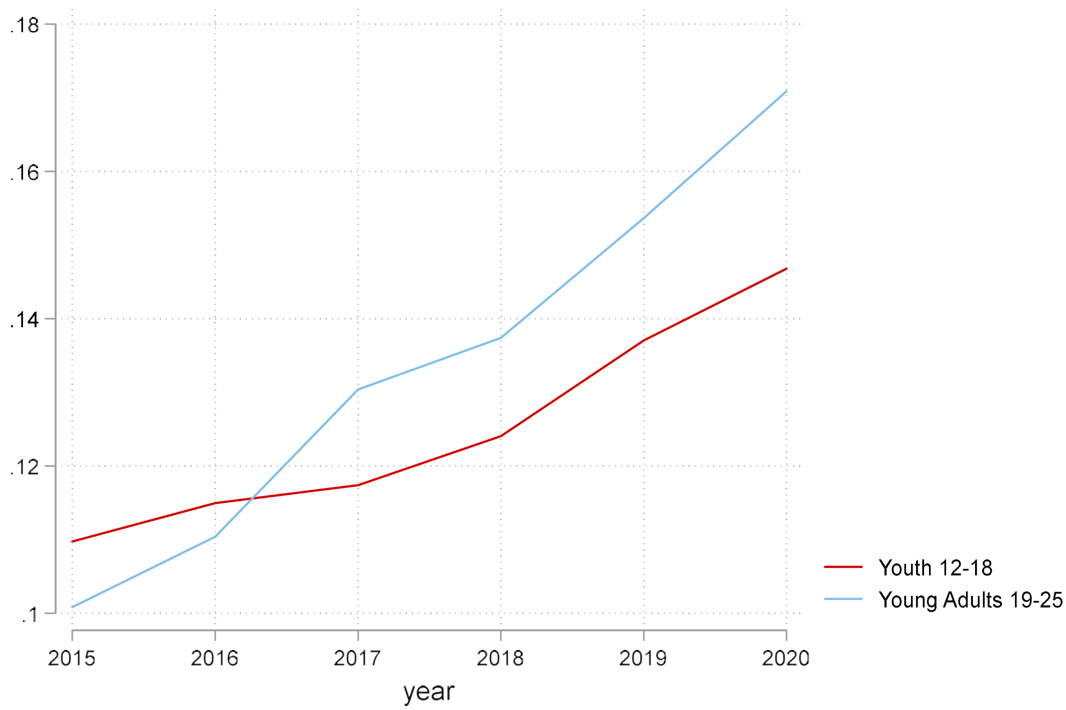
Notes: The K-12 relative foot traffic measure depicted above anchors baseline K-12 school foot traffic level at levels reported of January and February of each year (2019 and 2020, respectively).

Appendix Figure A5: Cyber-Bullying and Suicidality



Notes: Based on self-reported cyber-bullying and suicidality from the National Youth Risk Behavior Study (YRBS) for 2009-2019. The YRBS is a representative survey collected bi-annually from the Centers of Disease Control.

Appendix Figure A6: Trends in Major Depressive Episodes



Notes: Based on data on self-reports of major depressive episodes from the National Survey of Drug Use and Health 2015-2020.