Teen Suicide and the Outcomes of Surviving Peers*

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August 4, 2026

Abstract

By age 17, a quarter of U.S. students have experienced a peer suicide. Using linked administrative data from South Carolina and a matched difference-in-differences design, we find that exposure to a peer's self-harm death increases the probability of a self-harm diagnosis by nearly 50% and both the incidence and frequency of mental health visits. Effects on care use and criminal behavior are concentrated among white boys, and responses diverge by prior mental health history: students without a prior diagnosis increase felony offending rather than care-seeking. Deaths from assault and transportation accidents produce no comparable rise in self-harm, consistent with contagion.

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We are grateful to Kosali Simon as well as seminar and conference participants at the American Society of Health Economists, Gothenburg, ITAM, NBER Children, the Triangle Applied Microeconomics Conference, and Tinbergen. Asenat Mantovani provided outstanding research assistance. Janet Currie thanks the NOMIS Foundation and the Jacobs Foundation for financial support. We are also grateful to the Princeton Industrial Relations Section for financial support. We thank Sarah Crawford and Kaowao Strickland for help fulfilling data requests. This paper uses information from the records of the South Carolina Revenue and Fiscal Affairs Office, Health and Demographics Division. Their authorization to release this information does not imply endorsement of this study or its findings by either the Revenue and Fiscal Affairs Office or the data owners.

1 Introduction

Suicide is the second leading cause of death among children ages 10–14 and the third leading cause among adolescents ages 15–19 in the United States (National Institute of Mental Health, 2025). By the time they are 17, 24% of teens have experienced a schoolmate's suicide (Swanson and Colman, 2013). Public attention often centers on suicide clusters, which account for 1–5% of teen suicides (National Research Council, 2013). Yet, every suicide leaves behind hundreds of surviving schoolmates. This paper provides the first causal evidence that exposure to a schoolmate's suicide increases self-harm among surviving students.

Whether suicidal behavior is contagious has been debated for over a century, yet the question remains unresolved because existing evidence is largely correlational. As early as 1897, Durkheim (2005) argued that suicide was a "social fact" driven by external social forces and explicitly rejected the possibility of suicide contagion. Since then, researchers have documented suicide spikes following media coverage of celebrity suicides (Phillips, 1974; Bollen and Phillips, 1982; National Research Council, 2013),[1] and have used vital statistics to document spatial and temporal suicide clusters. Suicides among young people are two to four times more likely to be part of a cluster than among older people (Gould et al., 1990; Hawton et al., 2020). A recent case study of one town's teen suicide cluster suggested that suicide exposure normalizes the idea that suicide is a valid way to escape unendurable pressure (Mueller and Abrutyn, 2024).

Survey evidence corroborates the possibility of suicide contagion. Prior studies demonstrate that teens with friends or family members who attempt suicide are at higher risk for attempts (Cutler et al., 2001; Abrutyn and Mueller, 2014; Mueller and Abrutyn, 2015), and the association between suicide exposure and suicide risk is stronger for people with pre-existing mental health conditions and those who identify more closely with the deceased (Swanson and Colman, 2013; Mueller and Abrutyn, 2015; Cerel et al., 2016). Using the National Longitudinal Study of Adolescent Health, Fletcher (2017) exploits variation across

[1] Suicide spikes have followed other exceptional events, such as the Chernobyl meltdown (Hoffman and Bearman, 2015), and do not necessarily reflect suicide contagion *per se*.

grades within schools and finds that adolescents whose grademates had a family member attempt suicide report more suicidal thoughts, with transmission running within gender lines.

Despite this evidence, establishing a causal relationship between exposure to a suicide death and suicide-related behavior is challenging for two reasons. First, a cluster might reflect a common shock rather than contagion (Joiner, 2003; Crepeau-Hobson and Leech, 2014). Second, suicides are rare in most data, which limits possibilities for causal inference. In contrast, self-harming behavior, which is itself among the strongest risk factors for suicide, is far more common (Geulayov et al., 2019). In 2021, there were 394 emergency department visits for self-harm per 100,000 youth ages 10–19, compared to seven suicides (Choi et al., 2025). We therefore study the effect of exposure to a peer suicide death using self-harm as the primary outcome measure.

To estimate causal effects, we use linked individual-level administrative records from South Carolina, allowing students to be followed from ages 10–18 across Medicaid claims, school records, and criminal justice data. Students in 30 schools in which a student died by suicide are compared to students in control schools matched on grade span, urbanicity, and enrollment. We employ a difference-in-differences design with person-by-match and match-by-time fixed effects, following the approach used to study school shootings (Cabral et al., 2026). This strategy permits the construction of a plausible counterfactual for treated students and accounts for the possibility that schools experiencing a suicide death may differ systematically from other schools. Treated and control students are balanced on pre-period characteristics and outcomes, with no differential pre-trends before the focal death.

The headline finding is that students in exposed schools are nearly 50% more likely to receive a self-harm diagnosis in the 24 months following the death. The distress is not confined to self-injury: the probability of having a mental health visit in a given month increases by 5%, the likelihood of having two or more mental health visits in a month increases by 10%, and visits related to substance use rise significantly. Furthermore, affected teens are more likely to exhibit behaviors that bring them into contact with the justice system: they are more likely to be referred for violent crimes and detained in the juvenile system, and more likely to be arrested for drug-related offenses in the adult system. Estimates are

robust to alternative ways of selecting control schools and weighting schemes.

Two findings strengthen the contagion interpretation. First, there is no increase in mental health evaluations or in the probability of seeing a medical professional, so it is unlikely that the estimates represent greater awareness of mental health due to, for example, the provision of grief counselors. Second, we estimate effects around deaths from transportation accidents and assault. While suicide deaths increase self-harm, deaths from these other causes do not, consistent with self-harm contagion.

The average effects mask two forms of policy-relevant heterogeneity. First, white boys—the demographic group at highest baseline suicide risk—show the broadest behavioral response in terms of deteriorating mental health and increased criminal involvement.[2] Following a peer suicide, these boys have a higher likelihood of a mental health visit and a higher likelihood of multiple visits per month. They also experience a 30% increase in the likelihood of any criminal offending, and a 40% increase in felony offending.

Second, responses diverge sharply by prior mental health history. Students with a mental health history show an increase in self-harm diagnoses and intensified mental healthcare-seeking. Students without a history show a significant increase in self-harm but no increase in care-seeking; instead, they exhibit increases in felony offending. That self-harm rises even among students *without* a prior mental health history suggests that peer effects extend beyond at-risk populations, providing further support for contagion. These results also imply that, for some students, the effects of peer suicide first surface in the justice system and may not be detected by grief-based screening.

Our work is most closely related to two papers examining another traumatic school-based experience: school shootings. Rossin-Slater et al. (2020) shows that school shootings in Texas increase antidepressant use in affected areas, and Cabral et al. (2026) finds that affected students experience worse educational and labor market outcomes relative to a matched control group. We study suicide, which is more common than school shootings,

[2] We split the sample into Black and non-Black students. Most non-Black students are classified as non-Hispanic white, so "white" and "non-Black" are used interchangeably throughout the paper.

accounting for approximately one-fifth of deaths among youths ages 14–18 (Gaylor et al., 2023), and which may spread through contagion. We also extend the literature by examining behavioral outcomes rarely studied after a student death, including criminal justice contacts.

This paper also complements concurrent work studying the peer effects of self-harm and substance use using variation in peer composition in Swedish residential treatment facilities (Helénsdotter, 2025). That study finds that exposure to peers with a history of self-harm increases post-discharge self-harm hospitalizations among teens with prior self-harm histories. In contrast, we study a sudden shock that affects a broad school population not selected on prior risk.

Finally, whereas a large literature documents the lasting effects of early childhood circumstances (Almond et al., 2018), evidence on shocks experienced in adolescence is comparatively limited. Few adolescent experiences are as common—and understudied—as exposure to a peer's suicide. We show that self-harm is contagious and that its consequences extend beyond self-harm to other self-destructive behaviors, including substance use and conduct that draws criminal justice involvement. Our results point to heightened risks for white boys and teens with existing mental health conditions, findings that may prove helpful for targeting preventive services.

2 Data

2.1 Student-level Data

The primary data source is linked individual-level administrative records spanning multiple South Carolina state agencies, which capture a range of individual characteristics and outcomes. We construct a panel dataset for each student at the year-month level and track individuals starting at age 10 over the period 2000–2018. Appendix B provides details about data sources as well as sample and variable construction.

The sample is predominantly low-income youth born between 1990 and 1999. To construct the sample, the South Carolina Revenue and Fiscal Affairs (SC RFA) Office identified high schools that were above the median in terms of the share of students receiving free or reduced-price lunch from 2008–2013. Any student ever enrolled in one of these high schools was included. This low-income sample is of particular interest given the well-documented

relationship between poverty and teen suicide (Hoffmann et al., 2020): suicide rates are almost 40% higher in counties in the highest poverty quintile compared to those in the lowest poverty quintile.

Department of Education records identify the school in which each individual is enrolled each year, as well as their grade level. Although the sample is conditioned on high school enrollment, the data show where students were enrolled before high school, which allows us to consider deaths from self-harm that occurred in middle school.

To capture adolescent distress and behavioral responses to a peer's death, we construct outcome measures spanning mental health and criminal behavior. Self-harming behavior and mental health visits offer direct measures of distress. However, many adolescents do not seek or receive formal care (e.g., due to stigma). We therefore complement mental health measures with criminal justice contacts, which capture behavioral responses that surface outside the healthcare system.

Measures of self-harm and mental health visits are constructed using Department of Health and Human Services Medicaid records. Medicaid is the state-run health insurance program for low-income individuals; in 2023, the program covered 43.4% of South Carolina children, about 92% of those eligible (Center for Children and Families, 2025). These data include individual enrollment spells as well as claim-level information about prescription drugs and inpatient and outpatient medical visits. Diagnosis codes are used to classify visits.[3]

The primary mental health outcomes are whether an individual had a Medicaid claim with a self-harm diagnosis, whether they had any claim with a mental health diagnosis, and whether they had two or more claims with a mental health diagnosis, all measured in a given month. The latter measure captures changes in mental health severity that increase visit frequency. More specific mental health diagnoses are also considered, as discussed below.

Criminal behavior is measured using records from three agencies. Department of

[3] Mental health visits are those with a diagnosis in the mental disorders chapter of the International Classification of Diseases (ICD-9 and ICD-10). Self-harm-related diagnoses are included in this definition, and developmental conditions other than attention deficit hyperactivity disorder (ADHD) are excluded.

Juvenile Justice (DJJ) data provide information about youth referred to the agency, including the offense type and whether the individual was detained. Over the sample period, the age of criminal majority was 17, so children typically interact with DJJ unless they have committed a serious crime. Data from the South Carolina Law Enforcement Division include information about arrests, including the offense type and whether the individual was detained in a correctional facility. Data on state prison spells come from the Department of Corrections.

The two main criminal outcomes are whether an individual was referred to the juvenile justice system or arrested by law enforcement in a given month, and an analogous measure focusing on referrals and arrests for felony offenses. The latter captures changes in serious offending. We also consider the reason for involvement with the justice system, such as violent or drug-related offenses.

2.2 Data on Student Deaths

Student deaths are identified using a bespoke dataset constructed by SC RFA that records adolescent deaths from 2000–2018. These data include the date, cause of death, and limited demographic information about the deceased. Deaths with undetermined intent are not classified as self-harm, so this definition, which includes only deaths with an underlying cause of intentional self-harm, is conservative. Crucially, the file identifies the deceased individual's last public school and the corresponding academic year.

We begin by identifying schools where a student died from self-harm in grades 6–12. If more than one self-harm death occurred within a four-year period, only the first death is retained.[4] Deaths occurring too late for us to observe a two-year post-period are also excluded. The baseline sample includes 30 deaths from self-harm spanning 2004–2016.[5] Appendix Table B1 shows how the sample of deaths evolves as each restriction is imposed.

[4] The four-year window reflects the fact that most students are enrolled in high school for four years; we wanted to consider the *first* instance in which students were impacted. Six schools had more than one death in a two-year period, leaving too few clusters to analyze.

[5] We also exclude six deaths at schools where fewer than half of enrolled students appear in the microdata. Low coverage is a mechanical consequence of the sample design: the microdata include only 1990–1999 birth cohort students who attended a low-income high school, so coverage is low at schools enrolling many students outside this frame (see Appendix B.3). The same coverage requirement applies to control schools.

Schools where these deaths occurred are the "treated" schools.

Over three-fourths of students who die by suicide are male, nearly three-fourths of deaths occur in high school, and two-thirds of deceased individuals are white (Appendix Table A1). We compare these characteristics with those of teens who died from assaults or transportation accidents. These individuals are also disproportionately likely to be male, but they are more likely to be Black. The racial contrast is particularly stark for assault deaths, where 86% of the deceased were Black compared to roughly one-third for self-harm deaths.

The 30 schools with a self-harm death are larger than schools without a self-harm death (Appendix Table A2). This pattern reflects both the concentration of deaths in high schools, which tend to be larger than middle schools, and a mechanical relationship between the probability of at least one death and school size. Treated schools have slightly higher shares of white students and are more rural. Despite these differences, schools with and without a self-harm death are located in counties with similar poverty rates, median income, unemployment rates, and Medicaid coverage. To ensure that treated and comparison schools are comparable, we implement a matching procedure.

3 Empirical Strategy

3.1 Matching Procedure

For each treated school, a set of control schools is chosen that share the same grade span, urban/rural classification, and above/below median enrollment bin, and that did not experience a self-harm death in the two years before or after the focal event.[6] Matching on grade span and rurality ensures that we compare students of similar ages living in similar environments (e.g., with comparable access to mental healthcare and crime exposure). Matching on enrollment accounts for the mechanically higher probability of a self-harm death in larger schools, as well as size differences in school environment. All eligible schools serve as controls for a treated school, and the same school may serve as a control in multiple

[6] Characteristics are measured in the school year prior to the death. For details about matching, see Appendix B.7.

matches.[7] Each control school is assigned a placebo event date equal to the date of the self-harm death in the corresponding treated school, so that treated and control groups are observed over the same calendar window within each match.

Students who attended these schools in the school year of the focal event can then be identified. Since healthcare utilization is only observed for Medicaid beneficiaries, the sample is restricted to the approximately 45% of students who were continuously enrolled in Medicaid for the two years before and two years after the focal event date. Section 7 examines robustness to alternative ways of selecting control schools and relaxing the continuous Medicaid enrollment requirement.

Weights are chosen so that each event contributes equally to the estimation. Specifically, students in each treated school are re-weighted by one over the number of sample students attending that school. Control students are re-weighted by one over the number of sample students selected as controls for event m. In practice, each treated school receives a total weight of one and control students collectively receive a total weight of one for each event. Within the control group, larger schools contribute more weight given their larger enrollment.

The treatment and control groups are balanced on school-level characteristics (panel (a) of Appendix Table A3). Although we do not match on student characteristics, both groups are comparable in terms of student demographics (roughly 70% are Black), socioeconomic status, and pupil-teacher ratios as well as in terms of access to mental health providers and local crime rates.

There are also no significant differences between treated and control students in the year before the event (panel (b) of Appendix Table A3). The two groups are comparable in terms of the frequency of mental health visits in the year before the event, including for self-harm, as well as in their contacts with the justice system. Approximately three-fourths of both treatment and control students receive Supplemental Nutrition Assistance Program benefits.

[7] There are, on average, 64 control schools per treated school. Reuse of control students is limited: over 80% appear in three or fewer matches.

Raw outcome means are plotted in Figure 1. Treatment and control students had similar levels of all outcomes before the focal event. Previewing the main result, panels (a)–(c) show spikes in the probability of self-harm among treated students in the two years after the event as well as increases in the incidence and frequency of mental health visits.

3.2 Empirical Specification

The causal effect of a death from self-harm is estimated using a difference-in-differences regression of the form:

$$Y_{imt} = \alpha + \beta\big(\text{Treat}_i \times \text{Post}_t\big) + \delta_{im} + \gamma_{tm} + \epsilon_{imt}, \tag{1}$$

where Y_{imt} is student i's outcome, in match m, at time t. Treat_i is an indicator equal to one for treated students. The unit of observation is the person-month, with 48 observations per person (24 months before and after the event). The terms δ_{im} and γ_{tm} indicate person-by-match and match-by-time fixed effects, respectively.[8] The error ϵ_{imt} is clustered at the level of the school the student attended at the time of the (placebo) death. The coefficient β captures the average within-student change in outcome Y for treated students relative to control students in the two years following the self-harm death. Outcomes are multiplied by 100 for ease of exposition and weights are used throughout the analysis.

The identifying assumption underlying this research design is that students at schools with a self-harm death would have continued to trend similarly to students in control schools in the absence of the death. Figure 1 displays the raw data on outcomes for treated and control students, showing that outcomes were trending similarly in the pre-period.

We also estimate a fully dynamic version of the main specification:

$$Y_{imt} = \alpha + \sum_{\substack{k=-4 \\ k\neq -1}}^{3} \beta_k \left(\text{Treat}_i \times \mathbf{1}[\tau_{mt} = k]\right) + \delta_{im} + \gamma_{tm} + \epsilon_{imt}, \tag{2}$$

where τ denotes time relative to the self-harm death (or placebo death) in six-month bins

[8] Because a school may serve as a control for several treated schools, the same student could be part of multiple matches, so we include person-by-match fixed effects.

(i.e., $\tau = 0$ denotes the first six-month period after a death from self-harm for each match). Here, the coefficients of interest, β_k, represent the average within-student change in outcome Y at period τ among students in treated schools, relative to students in control schools. Because the six-month period before the focal death is omitted, β_k measures the difference in the outcome relative to $\tau = -1$.

A primary threat to identification is that other events, such as a deteriorating school environment or elevated peer distress, might coincide with the self-harm death and differentially affect treated students. The clinical literature suggests that adolescent suicides typically occur suddenly, unexpectedly, and impulsively in response to acute, individual-level stressors rather than collective ones (Bilsen, 2018). However, the dynamic specification allows us to further check for evidence of confounding school-level events, which should lead to differences in outcomes in the months leading up to the focal death.[9]

Two possible confounders merit attention. The first is other deaths, which could independently affect student well-being. In Section 7, we restrict the sample to matches where the treated schools had no other deaths in the two years preceding the focal death; the takeaways are highly similar. The second is bullying, which is associated with adolescent suicidality (Arseneault et al., 2010; Van Geel et al., 2014; Holt et al., 2015). If treated schools have more hostile peer environments, then elevated self-harm among surviving peers could reflect shared victimization rather than exposure to the peer suicide. Several factors mitigate this concern: treated and control students are balanced on outcomes in the year prior to the death, including comparable levels of mental health service use and referrals to DJJ for school-related incidents, which are outcomes that would likely diverge if treated schools had meaningfully more hostile peer environments (Appendix Table A3). Section 4 also shows no differential pre-trends in our primary outcomes in the 24 months preceding the self-harm death. Last, if the estimates reflected exposure to a hostile peer environment, rather than the self-harm death itself, we might expect similar effects following an assault death, a strong

[9] Moreover, the self-harm deaths take place in 22 school districts across 12 years. For unobserved shocks (e.g., area-level changes in economic circumstances) to confound the estimates, it would have to be the case that these shocks only affect treated students and that the timing of these shocks coincided with the self-harm death.

marker for such an environment. We find no such effect on self-harm (Section 6), suggesting that the estimates are not simply picking up a hostile school climate.[10]

An additional concern is that if students' Medicaid enrollment is affected by a peer's self-harm death, the resulting sample could reflect differential selection of treated and control students. For example, if the most affected students are more likely to remain enrolled in Medicaid, then the baseline estimates could overstate the effects of exposure to self-harm. In Section 7, we show no evidence of changes in Medicaid enrollment and find that the estimates are highly similar when the continuous enrollment criterion is relaxed.

4 Main Results

The effects of a peer self-harm death over the following 24 months are displayed in Table 1. Analogous event-study graphs are in Figure 2. Panel (a) of Figure 2 shows no evidence of a systematic lead-up in self-harming behavior in the months preceding the death, providing reassurance that there were no confounding school-level shocks impacting self-harm around the focal event. The first row of Table 1 shows that the monthly probability of a self-harm diagnosis rises by 0.028 percentage points, a 47% increase over the baseline rate of 0.06%. This result provides direct evidence of contagion in self-harming behavior. Over the two-year window, there are roughly seven additional person-months with a self-harm diagnosis per 1,000 exposed students, on a baseline of about 14.

The likelihood of any mental health visit increases by 0.351 percentage points, indicating a 5% increase in the monthly probability. The probability of two or more mental health visits also rises by 0.400 percentage points, or 10%. This is the largest effect in absolute terms, implying 96 additional person-months of intensive mental healthcare use per 1,000 exposed students in the two years following the death. These results indicate that surviving peers experience a deterioration in mental health, especially on the intensive margin.

Mental health diagnoses can be categorized into internalizing and externalizing disorders, trauma- and stressor-related diagnoses, and substance use disorders, with externalizing

[10] Unfortunately, microdata on school-level measures of peer conflict (e.g., suspensions, expulsions) are only available from 2007–2009.

disorders further divided into ADHD and other behavioral diagnoses. There are significant increases in mental health visits for substance use disorders and large (but insignificant) coefficients for ADHD and behavioral diagnoses (Appendix Table A4). These results suggest that surviving peers experience worsening substance use and, potentially, increases in externalizing behaviors.[11]

Students may have more contact with mental health professionals following a suicide, so increases in mental health visits could reflect greater contact with medical providers, rather than deteriorating mental health. To assess this possibility, equation (1) is estimated using any mental health evaluation and any visit to a mental health provider, primary care provider, or other provider as outcomes. Table 2 shows that there are no statistically significant impacts on these measures, indicating that there is no increase in *overall* medical contacts. The only increases are in claims for mental health services (such as therapy) and in the probability of having two or more such claims in a month (Appendix Table A4).

Effects on criminal behavior appear in the final two columns of Table 1. The first row shows that there is no significant overall effect on the likelihood of arrest or juvenile justice referral, either for any offense or for felony offenses. But affected teens are significantly more likely to be detained as juveniles and referred to DJJ for violent crimes (Appendix Table A5). There is also an increase in drug-related arrests in the adult system, which, together with the increase in substance use diagnoses, suggests an increase in substance use severe enough to draw both clinical and criminal justice attention.

[11] Epidemiological studies suggest that youth bereaved by a family member's suicide have higher rates of alcohol and substance use (Cerel and Roberts, 2005; Brent et al., 2009).

5 Heterogeneity

5.1 By Race and Sex

The next four panels of Table 1 split the sample by sex and race.[12] Self-harm rises in three of the four groups: the increase is statistically significant for Black boys (63% relative to baseline), large but imprecisely estimated for white girls, and positive but insignificant for white boys, with Black girls showing no change.

Across the other outcomes, white boys stand out, exhibiting both worse mental health and increases in criminal behavior following the self-harm death. This group has both a significantly higher likelihood of any mental health visit and a rise in multiple visits per month; no other subsample exhibits comparable increases. White boys also experience a roughly 30% increase in the likelihood of any offending, and a 40% increase in the likelihood of a felony offense. Given that the deceased are disproportionately likely to be white males (Appendix Table A1), these results are consistent either with greater vulnerability among white males or with impacts being larger among more similar students.

5.2 By Prior Mental Health History

The last two panels of Table 1 examine heterogeneity by a student's prior mental health history, defined as having any mental health diagnosis in the two years before the death. Self-harm rises in both groups. Among students with a prior history, the monthly probability of a self-harm diagnosis increases by 0.076 percentage points, a 42% increase over baseline, or roughly 18 additional person-months with a self-harm diagnosis per 1,000 exposed students over the two-year window.

Among students without a prior history, the increase is 0.011 percentage points—a 110% increase relative to this group's very low baseline, but only about three additional person-months per 1,000 students. Contagion is therefore not confined to students already

[12] We also estimated effects separately for middle and high schools, finding a positive and significant effect on self-harm in both subsamples (Appendix Table A6). The point estimates are consistently larger for middle schools across outcomes, suggesting effects may be larger among younger students. A caveat is that there are few treated middle schools.

known to the mental health system, though in absolute terms the burden falls overwhelmingly on those with a prior history. The increase in mental health visits is likewise driven by the prior-history group, consistent with worsening symptoms among those already in care.

Students without a prior history do not have any statistically significant increase in mental health visits but do exhibit a significant 27% increase in felony offenses, or roughly six additional person-months per 1,000 students over the two-year window. Hence, distress among students outside the mental health system manifests in behavior that brings them into contact with the justice system, rather than as additional care.

5.3 Examining Concordance by Sex and Grade

Previous studies have used sex and grade as proxies for peer networks in schools (see, e.g., Black et al., 2013). Hence, to examine concordance, equation (1) is augmented with a triple interaction term, $\text{Treat}_i \times \text{Post}_t \times \text{Concordant}_i$, where the last variable is an indicator for whether student i is the same sex as the deceased. In addition to the triple interaction term, match-by-time fixed effects are allowed to vary with concordance. We do not use race to define concordance because roughly a third of the suicide deaths lack information about the race of the deceased (Appendix Table A1).

There is a 15% increase in the probability of multiple mental health visits per month among students of the same sex as the deceased, compared with a statistically insignificant 6% increase among non-concordant students (panel (a) of Appendix Table A7). The triple-interaction term for self-harming behavior is large and positive (two-thirds the size of the main coefficient), though it is not statistically significant. We also experiment with defining concordance using sex *and* grade, which yields broadly similar conclusions for mental health-related outcomes (panel (b) of Appendix Table A7).[13]

Overall, the findings in this subsection are consistent with a contagion mechanism in which proximity to the deceased amplifies the effects of peer self-harm deaths, particularly for mental health outcomes. White male students show effects on many outcomes, consistent

[13] The triple interaction term on felony offenses is negative and statistically significant in this specification, suggesting that it is students in *other* grades who are more likely to commit serious crimes following a death from self-harm.

with the fact that the deceased are disproportionately male and white. While the concordance estimates are imprecise, reflecting both limited statistical power and the fact that sex and grade are imperfect proxies for social networks, the consistency of the positive point estimates supports the idea that closeness to the deceased matters.

6 Comparison with the Impacts of Assault and Transportation Deaths

Do suicide deaths differentially affect self-harming behavior compared to other peer deaths? To consider this, we estimate models analogous to equation (1) using peer deaths from transportation accidents and assault as the focal events, applying the same procedures for defining treated and control schools.

Table 3 shows that self-harm is especially sensitive to self-harm deaths. Transportation deaths involving pedestrians and car passengers have a negative coefficient for self-harm, ruling out an increase of comparable magnitude to the effects of a self-harm death (panel (b)).[14] There are no statistically significant effects on any of the remaining outcomes. Panel (c) shows estimates for deaths due to assault. Here, the only significant impact is on felony offenses, plausibly consistent with escalation of violence in a school. Overall, the results suggest that self-harm deaths are uniquely likely to trigger self-harm.

Why do self-harm deaths have distinct effects on self-harm? One possibility is that they are particularly unexpected and therefore more distressing to surviving peers. Assault deaths may be less shocking in communities where violence is prevalent. More fundamentally, self-harm deaths may trigger imitation and social learning—mechanisms that are central to the contagion hypothesis—in ways that accidental or violent deaths do not, though the data do not allow us to directly test this hypothesis.

7 Robustness

The analysis relies on a matched control group of students to estimate counterfactual outcome paths. This section discusses robustness to changes in the selection and weighting

[14] Deaths where the driver died are excluded because some may have been acts of self-harm.

of the control group.

In the baseline, control students within a match receive equal weight regardless of school characteristics. As a first check, the weights are adjusted to give greater weight to students attending control schools with more similar enrollment and racial profiles. The results are highly similar (Appendix Table A8, row two).

Self-harm deaths may have spillover effects within a school district, or common district-level shocks may precipitate both the focal death and a general deterioration in student well-being. The baseline estimates do not require control schools to be in different districts than treated schools. Dropping control schools from the same district as the treated schools yields similar results (Appendix Table A8, row three).

Of the 30 treated schools, 11 experienced at least one death from other causes in the two years preceding the focal death, which could contribute to student malaise. Re-estimating our models excluding these treated schools and the corresponding control schools yields somewhat larger estimates compared to the baseline (Appendix Table A8, row four), suggesting that prior deaths in treated schools do not account for the main findings.

The baseline sample focuses on students continuously enrolled in Medicaid for the two years before and after the focal death. Estimating equation (1) on students continuously enrolled during the pre-period, with Medicaid enrollment as the outcome, shows no evidence of differential attrition after the event (p-value = 0.76). Re-estimating equation (1) using students who were continuously enrolled for two years preceding the focal event, and assuming that students who did not appear in the data in the post-period did not receive mental healthcare, yields similar estimates (Appendix Table A8, row five).[15]

Finally, we consider non-health outcomes in a broader sample that includes students who were not continuously enrolled in Medicaid around the focal event. These students are, on average, less economically disadvantaged than those in the baseline sample. In this larger sample, the estimated effect on felony offenses becomes statistically significant at the

[15] In South Carolina, children with household incomes up to 208% of the federal poverty level are eligible for Medicaid, so it is reasonable to assume that eligible adolescents who required care would reappear in the claims data.

5% level, suggesting an increase in serious criminal offending following the self-harm death, though there is no significant effect on the overall probability of offending (Appendix Table A8, row six).

8 Conclusions

To our knowledge, this study provides the first causal evidence that a peer suicide increases self-harm among surviving students and that effects spill over onto broader measures of mental health and criminal offending. Importantly, the response is not uniform: effects on mental health and criminal behavior are concentrated among white boys, and responses differ by prior mental health history. Students known to the mental health system seek more care, suggesting worsening symptoms. Students without prior mental health contacts exhibit increases in criminal offending, rather than seeking care.

This divergence matters for how schools respond to a student suicide. Standard practice is for staff and crisis teams to identify at-risk students based on visible warning signs and grief reactions; expert consensus has explicitly stopped short of recommending systematic screening of every exposed student (Cox et al., 2016). Our results suggest that this approach will miss some of the students most affected by a peer suicide. Broadening the lens beyond monitoring for depression and suicidal ideation to consider substance use and externalizing behaviors as possible grief reactions may help schools and providers reach at-risk students who would otherwise first come to their attention through the justice system. Given that law enforcement contacts in adolescence can have lasting effects on human capital and future offending (Aizer and Doyle, 2015; Weisburst, 2019), preventing these consequences could be especially valuable.

The disparate effects of a peer suicide by sex add to a growing body of evidence that boys are especially vulnerable to adverse childhood experiences, from family disadvantage (Autor et al., 2019) and non-intact families (Bertrand and Pan, 2013) to neighborhood conditions (Chetty et al., 2016). Our finding that boys suffer increases in criminal justice involvement and worsening mental health fits this broader pattern and speaks to recent concerns about boys' well-being (see, e.g., Reeves, 2022).

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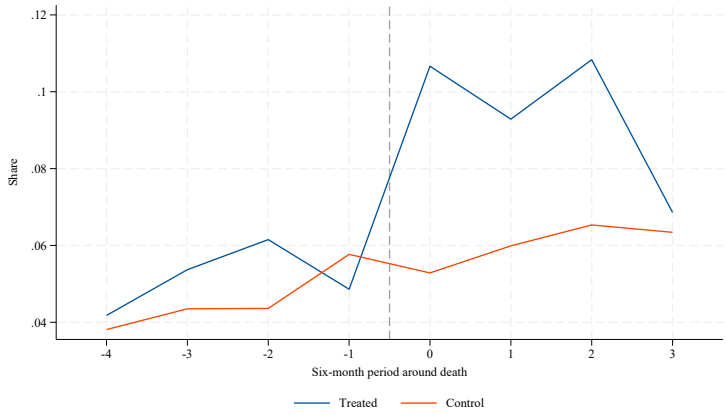
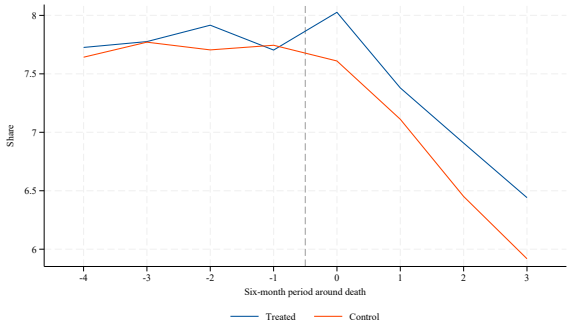
Figures and Tables

Figure 1: Raw Means of Primary Outcomes, Around Self-Harm Death

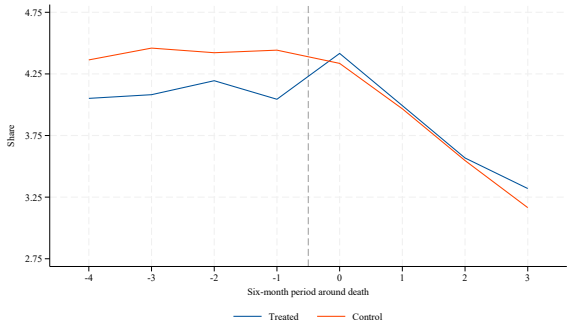
(a) Self-harm diagnosis



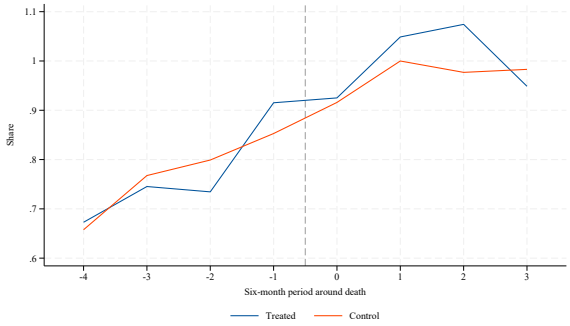
(b) Any mental health visit



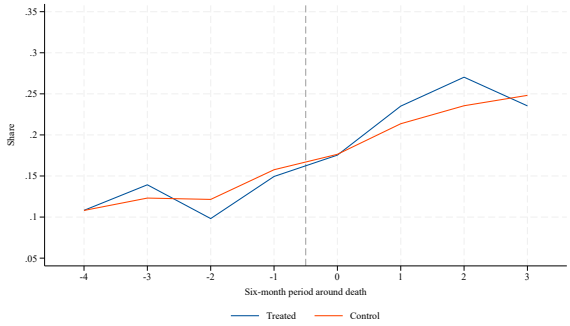
(c) Two or more mental health visits



(d) Any arrest or referral



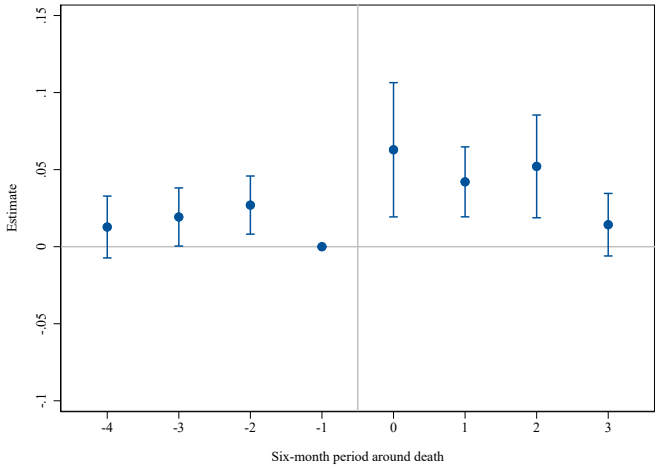
(e) Arrest or referral for felony



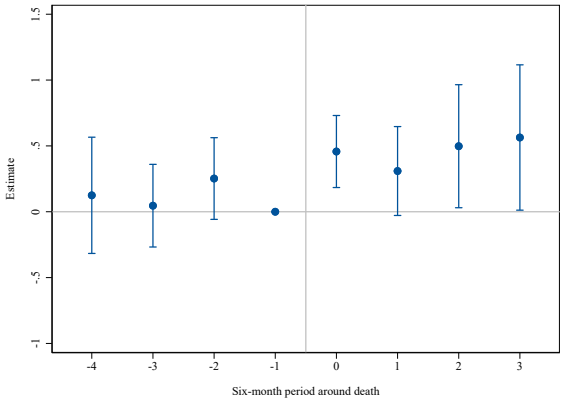
NOTES: This figure plots the average outcome in the two years before and after treatment, using treated and control individuals in the baseline sample described in Section 3. The solid blue line represents the mean for treated individuals and the orange line represents the mean for control individuals. Individuals in the comparison group are weighted using the corresponding weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition.

Figure 2: Event-Time Estimates of Impact of Self-Harm Death

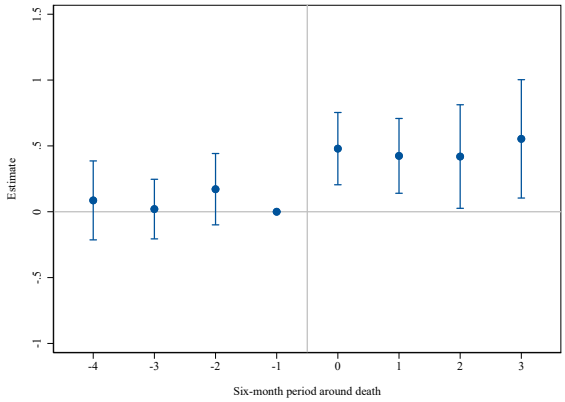
(a) Self-harm diagnosis



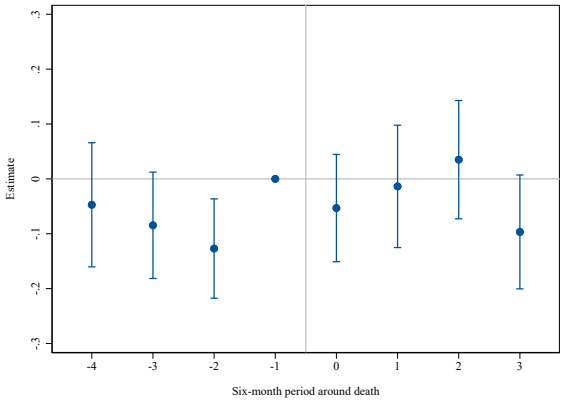
(b) Any mental health visit



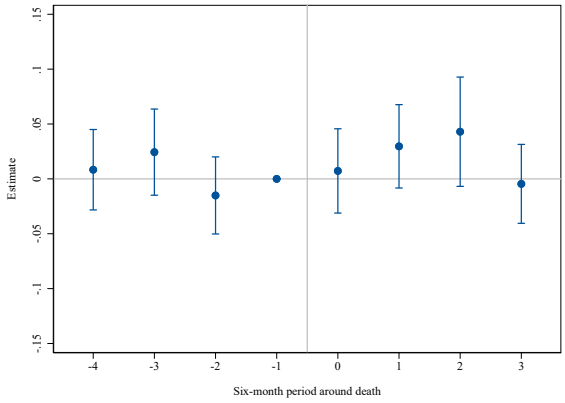
(c) Two or more mental health visits



(d) Any arrest or referral



(e) Arrest or referral for felony



NOTES: This figure plots difference-in-differences estimates using equation (2) and the baseline sample described in Section 3. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Individuals in the comparison group are weighted using the weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition.

Table 1: Difference-in-Differences Monthly Estimates of Impact of Self-Harm Death on Mental Health and Criminal Behavior

	(1) Self-harm diagnosis	(2) Mental health visit	(3) 2+ mental health visits	(4) Any arrest or referral	(5) Felony arrest or referral
A. All students (N = 135,247)					
Treat × Post	0.028***	0.351**	0.400***	0.033	0.014
	(0.008)	(0.172)	(0.145)	(0.039)	(0.010)
Mean	0.06	6.98	3.84	0.83	0.16
B. Non-Black male students (N = 17,416)					
Treat × Post	0.018	0.923**	1.340***	0.258***	0.077**
	(0.015)	(0.395)	(0.365)	(0.070)	(0.039)
Mean	0.08	11.55	5.73	0.83	0.18
C. Black male students (N = 45,502)					
Treat × Post	0.021***	0.108	0.292	0.012	-0.013
	(0.007)	(0.272)	(0.216)	(0.067)	(0.025)
Mean	0.03	7.44	4.28	1.30	0.35
D. Non-Black female students (N = 17,817)					
Treat × Post	0.161	0.382	0.314	-0.054	0.005
	(0.082)	(0.376)	(0.310)	(0.043)	(0.019)
Mean	0.11	8.81	4.57	0.48	0.04
E. Black female students (N = 54,512)					
Treat × Post	0.005	0.264	0.148	-0.008	0.002
	(0.011)	(0.195)	(0.141)	(0.045)	(0.009)
Mean	0.05	4.55	2.63	0.55	0.05
F. No pre-period MH diagnosis (N = 100,449)					
Treat × Post	0.011**	0.137	0.061	0.052	0.024***
	(0.005)	(0.089)	(0.052)	(0.030)	(0.009)
Mean	0.01	0.90	0.46	0.49	0.09
G. Any pre-period MH diagnosis (N = 34,798)					
Treat × Post	0.076***	1.619***	1.805***	-0.011	-0.014
	(0.022)	(0.479)	(0.437)	(0.083)	(0.029)
Mean	0.18	24.53	13.59	1.79	0.38

NOTES: **p< 0.05, ***p< 0.01. This table reports estimates using equation (1). Panel (a) uses the baseline sample described in Section 3. Panels (b)–(e) and (f)–(g) restrict the sample by a student's race/sex and mental health diagnosis in the two years prior to the self-harm (placebo) death, respectively. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Comparison students are weighted using the weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition. "Mean" refers to the average outcome in the sample. "N" reports the number of individuals (person-match) in the regression.

Table 2: Difference-in-Differences Monthly Estimates of Impact of Self-Harm Death on Medical Provider Utilization

	(1) Mental health evaluation	(2) Mental health provider	(3) Primary care provider	(4) Other type of provider
Treat × Post	0.049	0.355	-0.339	-0.299
	(0.081)	(0.203)	(0.212)	(0.468)
Mean	1.93	5.17	15.07	22.23
N	135,247	135,247	135,247	135,247

NOTES: **p< 0.05, ***p< 0.01. This table reports difference-in-differences estimates using equation (1) and the baseline sample described in Section 3. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Individuals in the comparison group are weighted using the corresponding weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition. "Mean" refers to the average outcome in the sample. "N" reports the number of individuals (person-match) in the regression.

Table 3: Difference-in-Differences Monthly Estimates of Impact of Self-Harm, Transportation, and Assault Deaths on Mental Health and Criminal Behavior

	(1) Self-harm diagnosis	(2) Mental health visit	(3) 2+ mental health visits	(4) Any arrest or referral	(5) Felony arrest or referral
A. Self-harm deaths (30 deaths)					
Treat × Post	0.028***	0.351**	0.400***	0.033	0.014
	(0.008)	(0.172)	(0.145)	(0.039)	(0.010)
Mean	0.06	6.98	3.84	0.83	0.16
N	135,247	135,247	135,247	135,247	135,247
B. Transport deaths (32 deaths)					
Treat × Post	-0.016**	-0.046	-0.260	0.065	0.013
	(0.007)	(0.197)	(0.180)	(0.037)	(0.014)
Mean	0.07	7.37	4.08	0.87	0.18
N	150,089	150,089	150,089	150,089	150,089
C. Assault deaths (39 deaths)					
Treat × Post	0.013	-0.384	-0.305	-0.037	0.034**
	(0.008)	(0.227)	(0.167)	(0.043)	(0.014)
Mean	0.06	7.25	4.07	0.88	0.18
N	152,017	152,017	152,017	152,017	152,017

NOTES: **p< 0.05, ***p< 0.01. This table reports difference-in-differences estimates using equation (1). Each panel considers a different type of death, from self-harm, transportation, and assault, respectively. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Individuals in the comparison group are weighted using the corresponding weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition. "Mean" refers to the average outcome in the sample. "N" reports the number of individuals (person-match) in the regression.

A Supplemental Appendix Figures and Tables

Table A1: Characteristics of Deceased Individuals, by Cause of Death

	(1) Self-harm deaths	(2) Assault deaths	(3) Transport deaths
A. Sex of individual who died			
Male	77% (23)	90% (38)	67% (24)
Female	23% (7)	10% (4)	33% (12)
B. Race of individual who died			
White	67% (14)	14% (5)	19% (5)
Black	33% (7)	86% (30)	77% (20)
Hispanic	0% (0)	0% (0)	4% (1)
Total deaths with race	70% (21)	83% (35)	72% (26)
C. Grade of individual who died			
6th	7% (2)	5% (2)	11% (4)
7th	0% (0)	5% (2)	8% (3)
8th	17% (5)	12% (5)	8% (3)
9th	13% (4)	12% (5)	17% (6)
10th	30% (9)	24% (10)	22% (8)
11th	20% (6)	19% (8)	6% (2)
12th	13% (4)	17% (7)	17% (6)

NOTES: This table reports the composition of deaths by cause. Columns (1)–(3) correspond to self-harm, assault, and transportation deaths, respectively. Each row displays the percentage of deaths in that category, with the total number of deaths in parentheses. Statistics in panels (b) and (c) are computed among deaths with non-missing race and non-missing grade information, respectively.

Table A2: Characteristics of Schools in South Carolina With and Without a Self-Harm Death

	(1) Experience self-harm death	(2) Never experience self-harm death
A. School characteristics		
Total number of students	816	550
Total number of teachers (FTE)	55	36
Percent male	50.6	51.3
Percent non-Hispanic white	36.2	32.9
Percent non-Hispanic Black	57.0	60.1
Percent Hispanic	4.4	4.2
Percent FRL recipients	72.2	74.6
Rural	60.0	57.3
B. County characteristics		
Percent with Medicaid	21.8	21.4
Percent below federal poverty level	21.4	20.9
Median income	38,494	39,140
Unemployment rate	12.0	12.0
Number of schools	30	218

NOTES: This table compares the 30 schools that experienced a self-harm death in our baseline sample to schools that did not. The schools in column (2) are restricted to non-correctional schools in our sample that span grades 6–12. The top panel uses data from National Center for Education Statistics (2024) measured in 2012 or in the closest available year. The second panel uses county-level data from Manson et al. (2024) measured in 2012 or in the closest available year. For more details, see Appendix B.

Table A3: Summary Statistics of Treated and Matched Control Groups

	(1) Treatment group	(2) Comparison group	(3) P-value
A. School characteristics			
Percent male	50.6	50.7	0.708
Percent non-Hispanic white	37.0	30.8	0.147
Percent non-Hispanic Black	58.1	64.1	0.160
Percent Hispanic	3.4	3.4	0.993
Percent free or reduced price lunch recipients	70.1	73.1	0.120
Pupil-teacher ratio	15	15	0.860
Total number of students	878	860	0.650
Located in city	13.3	9.0	0.482
Located in suburb	20.0	14.1	0.439
Located in town	20.0	28.6	0.267
Located in rural area	46.7	48.3	0.811
Travel time to nearest mental health provider	14.1	12.8	0.254
Number of providers within 30-minute drive	1.9	1.8	0.557
Violent crime (per 100k)	85.0	84.4	0.940
Property crime (per 100k)	509.0	536.7	0.480
B. Student characteristics			
Percent male	45.9	46.6	0.401
Percent non-Hispanic Black	69.1	74.3	0.196
Average age	13.9	13.9	0.963
Any inpatient or ER hospital visit	31.9	31.1	0.523
Any mental health visit	21.3	20.2	0.270
2+ mental health visits	12.9	12.9	1.000
Any self-harm related diagnosis	0.6	0.5	0.635
Any ADHD diagnosis	11.3	10.9	0.649
Any mental health service	20.9	20.7	0.811
Any mental health evaluation	11.4	11.1	0.686
Enrollment in TANF	9.6	9.7	0.762
Enrollment in SNAP	74.5	75.1	0.485
Any school related referral	1.6	1.4	0.665
Any arrest or referral	7.3	7.5	0.724
Any arrest or referral for felony	1.3	1.5	0.300

NOTES: Panel (a) displays school-level characteristics measured in the school year before treatment. Treated schools receive a weight of one and control schools receive fractional weights proportional to their contribution within the match. Panel (b) displays average student outcomes in our baseline sample, measured in the year prior to treatment (except age, which is measured 12 months before the (placebo) death) and uses the baseline weights described in Section 3.1. Column 3 presents p-values from a regression of each characteristic on a treatment indicator using the respective weights and clustering standard errors at the match. Details on variable definitions are available in Appendix B.

Table A4: Difference-in-Differences Monthly Estimates of Impact of Self-Harm Death on Mental Health Diagnoses and Healthcare Utilization

	Treat × Post	(S.E.)	Mean
A. Mental health diagnoses			
Externalizing	0.284	(0.156)	5.21
Internalizing	-0.026	(0.086)	2.03
ADHD	0.034	(0.119)	3.43
Behavioral	0.145	(0.088)	1.44
Trauma or stress	0.022	(0.055)	0.76
Substance use	0.093**	(0.045)	0.60
B. Healthcare utilization			
Any claim	-0.203	(0.404)	40.91
Hospital or ER visit	-0.009	(0.094)	4.37
Mental health service	0.630***	(0.206)	4.97
2+ mental health service	0.394***	(0.144)	2.99
Mental health drug	-0.144	(0.129)	6.57
ADHD drug	-0.053	(0.141)	4.86
Mental health non-ADHD drug	-0.046	(0.076)	3.09

NOTES: **p< 0.05, ***p< 0.01. This table reports difference-in-differences estimates using equation (1) and the baseline sample described in Section 3. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Individuals in the comparison group are weighted using the corresponding weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition. "Mean" refers to the average outcome in the sample. For more details on diagnosis and drug classifications, see Appendix B.

Table A5: Difference-in-Differences Monthly Estimates of Impact of Self-Harm Death on Criminal Behavior

	Treat × Post	(S.E.)	Mean
A. Juvenile Records			
Referral	0.024	(0.044)	0.62
Substance-related offense	-0.001	(0.005)	0.05
Violent offense	0.015***	(0.004)	0.03
Detention	0.024***	(0.008)	0.06
B. Adult Records			
Arrest	0.009	(0.019)	0.22
Drug-related arrest	0.011**	(0.005)	0.04
Violent crime arrest	0.000	(0.006)	0.04
Incarceration	0.018	(0.018)	0.05

NOTES: **p< 0.05, ***p< 0.01. This table reports difference-in-differences estimates using equation (1) and the baseline sample described in Section 3. Panel (a) refers to referrals to the Department of Juvenile Justice. Panel (b) refers to records from the South Carolina Law Enforcement Division and the Department of Corrections. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Individuals in the comparison group are weighted using the corresponding weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition. "Mean" refers to the average outcome in the sample. For more details on offense classifications, see Appendix B.

Table A6: Difference-in-Differences Monthly Estimates of Impact of Self-Harm Death, by School Level

	(1) Self-harm diagnosis	(2) Mental health visit	(3) 2+ mental health visits	(4) Any arrest or referral	(5) Felony arrest or referral
A. Middle school deaths (7 deaths)					
Treat × Post	0.064**	0.479	0.600	0.114	0.050
	(0.026)	(0.418)	(0.330)	(0.109)	(0.026)
Mean	0.04	8.78	4.87	0.82	0.16
N	20,186	20,186	20,186	20,186	20,186
B. High school deaths (22 deaths)					
Treat × Post	0.020***	0.343	0.307	-0.004	-0.000
	(0.006)	(0.191)	(0.164)	(0.039)	(0.011)
Mean	0.06	6.65	3.65	0.83	0.17
N	113,736	113,736	113,736	113,736	113,736

NOTES: **p< 0.05, ***p< 0.01. This table reports difference-in-differences estimates using equation (1) and the baseline sample described in Section 3. Panel (a) pertains to the seven treated schools serving grades 6–8 or 5–8 and their corresponding matches. Panel (b) pertains to the 22 treated schools serving grades 9–12 or 10–12 and their corresponding matches. In this table, we exclude the one treated school serving grades 6–12 and its corresponding control schools. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Individuals in the comparison group are weighted using the corresponding weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition. "Mean" refers to the average outcome in the sample. "N" reports the number of individuals (person-match) in the regression.

Table A7: Difference-in-Differences Monthly Estimates of Impact of Self-Harm Death, Examining Concordance

(a) Sex Concordance

	(1) Self-harm diagnosis	(2) Any MH visit	(3) 2+ MH visits	(4) Any arrest or referral	(5) Felony arrest or referral
Treat × Post	0.022**	0.241	0.224	0.023	0.027**
	(0.009)	(0.202)	(0.143)	(0.044)	(0.011)
Treat × Post × Concordant	0.014	0.194	0.370**	0.019	-0.028
	(0.011)	(0.254)	(0.166)	(0.041)	(0.020)
Mean	0.06	6.98	3.84	0.83	0.16
N	135,247	135,247	135,247	135,247	135,247

(b) Sex and Grade Concordance

	(1) Self-harm diagnosis	(2) Any MH visit	(3) 2+ MH visits	(4) Any arrest or referral	(5) Felony arrest or referral
Treat × Post	0.026***	0.340	0.269	0.042	0.029***
	(0.009)	(0.177)	(0.139)	(0.043)	(0.011)
Treat × Post × Concordant	0.015	-0.004	0.886***	-0.071	-0.107***
	(0.015)	(0.371)	(0.219)	(0.070)	(0.029)
Mean	0.06	6.98	3.84	0.83	0.16
N	135,247	135,247	135,247	135,247	135,247

NOTES: **p< 0.05, ***p< 0.01. This table reports difference-in-differences estimates using the baseline sample described in Section 3. The regressions in this table modify equation (1) to include a triple interaction term, $\text{Treat}_i \times \text{Post}_t \times \text{Concordant}_i$, and allows the match-by-time fixed effects to vary by concordance. In panel (a), "Concordant" is an indicator equal to one if a student shares the sex of the deceased student in the event. In panel (b), "Concordant" is an indicator equal to one if a student shares the sex and grade of the deceased student in the event. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Individuals in the comparison group are weighted using the corresponding weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition. "Mean" refers to the average outcome in the sample. "N" reports the number of individuals (person-match) in the regression.

Table A8: Robustness of Difference-in-Differences Monthly Estimates on Mental Health and Criminal Behavior

	(1) Self-harm diagnosis	(2) Mental health visit	(3) 2+ mental health visits	(4) Any arrest or referral	(5) Felony arrest or referral	N
Baseline estimates	0.028***	0.351**	0.400***	0.033	0.014	135,247
	(0.008)	(0.172)	(0.145)	(0.039)	(0.010)	
Enrollment & race weight	0.026***	0.391**	0.421***	0.042	0.017	135,247
	(0.008)	(0.175)	(0.145)	(0.039)	(0.010)	
Exclude same-district controls	0.028***	0.372**	0.411***	0.033	0.015	131,512
	(0.008)	(0.174)	(0.147)	(0.039)	(0.010)	
Exclude schools with prior deaths	0.038***	0.443**	0.526***	0.059	0.027	90,122
	(0.012)	(0.223)	(0.179)	(0.050)	(0.015)	
Pre-period enrollment	0.026***	0.303	0.371***	0.026	0.015	137,582
	(0.008)	(0.165)	(0.139)	(0.038)	(0.010)	
All affected individuals	–	–	–	0.047	0.012**	316,385
	–	–	–	(0.030)	(0.005)	

NOTES: **p< 0.05, ***p< 0.01. This table reports difference-in-differences estimates using equation (1). The first row reports baseline estimates using the sample described in Section 3; subsequent rows modify the weighting scheme or the sample as described in Section 7. Standard errors are clustered by the school the student was enrolled in at the time of the (placebo) death. Individuals in the comparison group are weighted using the corresponding weights from the matching procedure. Outcomes are multiplied by 100 for ease of exposition (scale 0 to 100). "N" reports the number of individuals (person-match) in the regression.

B Supplemental Data Appendix

This appendix provides additional detail on the data and variable construction used throughout the paper. Sections B.1–B.3 describe the linked administrative data, sample construction, and student deaths. Sections B.4–B.6 define the health outcomes, criminal justice outcomes, and other variables. Sections B.7–B.8 describe the matching procedure, analysis sample, and weighting scheme.

B.1 Linked Data

The individual-level data were assembled and linked by the South Carolina Revenue and Fiscal Affairs Office (SC RFA). SC RFA assigned each individual a unique research identifier, allowing records to be linked across agencies. Specifically, we obtained data from: school enrollment records from the South Carolina Department of Education (SCDE); Medicaid enrollment spells and claims (professional, hospital, and pharmacy files) from the Department of Health and Human Services; juvenile justice referrals from the Department of Juvenile Justice (DJJ); adult arrest records from the South Carolina Law Enforcement Division (SLED); state prison records from the Department of Corrections (DOC); Supplemental Nutrition Assistance Program (SNAP) and Temporary Assistance for Needy Families (TANF) participation spells from the Department of Social Services (DSS); and death certificates from Vital Records. SC RFA also constructed a file linking deaths of school-aged children to the deceased student's school, described in Section B.3.

B.2 Sample

Sample of Students. The sample consists of individuals born between 1990 and 1999 who attended a low-income high school in South Carolina. To define the set of low-income schools, the SC RFA first calculated, for each high school, the average share of students receiving free or reduced-price lunch over the 2008–2013 period. High schools in the top half of this distribution were classified as low-income. The analysis sample then includes all individuals who were ever enrolled in one of these schools during high school.

For each individual, we construct a balanced panel at the year-month level beginning in the calendar year they turn 10 and ending in 2018. All agency records are merged at the person-month level. Education records, which are reported by school year, are merged to all months of the

corresponding school year. Observations after an individual's death (per death certificate data) are dropped.

Sample of Schools. We define the set of "sample" schools as the high schools described in Section B.2 together with the middle and elementary schools that feed into them. Feeder schools are identified from observed student flows in the education data: a school qualifies as a feeder school if it appears in the data for at least three years and either enrolls at least 100 sample students on average (in its three highest-enrollment years) or covers at least 25% of the school's enrollment as reported by the National Center for Education Statistics (NCES) on average (in its three highest-coverage years).

Schools operated by the Department of Juvenile Justice or the Department of Corrections are excluded from the analysis. Specifically, deaths occurring at correctional schools are not treated as focal events, and correctional schools are never eligible to serve as control schools.

B.3 Student Deaths

The dataset on student deaths was constructed by SC RFA by linking death certificates to SCDE enrollment records. Each record includes the month and year of death, the underlying cause of death (using ICD-10 codes), the sex and grade of the deceased, and the school and school year of the student's most recent observable enrollment. The file does not include the race of the deceased; race is only observed for deceased students who can be matched to their own records in the microdata, which requires them to have been enrolled in high school prior to their death. For our analysis, we retain student deaths that occurred when the deceased was in grades 6–12.

Causes of death are classified using the underlying cause of death code. Deaths from intentional self-harm are those with codes X60–X84 or Y87.0 (sequelae of intentional self-harm). Deaths from assault are those with codes X85–Y09. Transportation deaths are those with codes V01–V99 or Y85. Within transportation deaths, we identify passenger deaths (V20–V29 with fourth digit .1 or .5, and V30–V79 with fourth digit .1 or .6) and non-train pedestrian deaths (V01–V04 and V06–V09); the analysis of transportation deaths in Section 6 is restricted to passenger and pedestrian deaths as some driver deaths may have been acts of self-harm.

A student's enrollment in the SCDE data reflects their school as of the March school enrollment census. When constructing the dataset of student deaths, SC RFA therefore needed to decide

how to combine enrollment information with the date of death to assign a school and school year to each death. Deaths occurring on or after August 20 are assigned to the following school year and to the school attended in that school year (e.g., a death on September 1, 2005 is assigned to the 2005–2006 school year and to the school attended in that school year). Deaths occurring before August 20 are assigned to the previous school year and to the school attended in that previous school year (e.g., a death on June 1, 2005 is assigned to the 2004–2005 school year and to the school attended in that school year). Students who died before the March enrollment census and could not be matched to a school in the school year of death are assigned to the school they last attended. We exclude deaths for which more than one school year elapsed between the student's last confirmed enrollment and the death.

One complication arises for students who die after leaving a feeder school but before appearing in a high school enrollment census. For these students, the last school observed in the data is their middle school even though they would have been attending high school at the time of death. We therefore identify the affected high school by looking at the modal high school among their middle school classmates (i.e., looking at the largest share of the deceased student's schoolmates in the following year and assigning the death to that high school).

Appendix Table B1 summarizes the restrictions used to construct the analysis sample of self-harm deaths. Starting from 46 self-harm deaths at sample schools, we retain only the first death at a school within a four-year window. We also exclude deaths occurring too late in the sample period to observe a full two-year post-period. Finally, we exclude deaths at schools where the microdata cover less than 50% of NCES-reported enrollment. Because the microdata include only students born between 1990 and 1999 who ever attended a low-income high school (Section B.2), coverage relative to NCES enrollment is mechanically low at schools enrolling many students outside this sampling frame—for example, feeder schools that send many students to non-sample high schools, or schools observed in early or late sample years, when several enrolled grades correspond to cohorts born outside the 1990–1999 window (e.g., 11th and 12th graders in the early years and 6th graders in the later years).

The resulting analysis sample contains 30 self-harm deaths occurring between 2004 and 2016. An analogous procedure is applied to construct the samples of assault and transportation deaths.

Table B1: Sample Restrictions on Self-Harm Deaths

	Number of Student Deaths
Deaths from self-harm in sample schools	46
Exclude higher-number deaths within four-year window	−8
Exclude deaths too late in sample period	−2
Exclude deaths where school coverage below 50% of NCES enrollment	−6
Number of self-harm deaths in analysis sample	30

B.4 Health Outcomes

Health outcomes are constructed from three claims files: a professional claims file, an inpatient/outpatient hospital claims file, and a pharmacy file. Records are collapsed to the visit level and then to the person-month level, yielding indicators for one or more qualifying visits in a month and counts of qualifying visits in a month. "Any claim" is an indicator for one or more professional, hospital, or pharmacy Medicaid claims in a month. Hospital and emergency room use is identified from inpatient and emergency claims in the hospital file, as well as professional claims at inpatient locations.

Medicaid enrollment measure. Medicaid enrollment is observed as enrollment spells with start and end dates, which we convert into person-month enrollment indicators. We then make three adjustments to this measure in order to avoid classifying short interruptions in administrative enrollment records as losses of coverage. First, if an individual submits a Medicaid claim for any type of healthcare (i.e., a visit or prescription), we classify them as enrolled in Medicaid in that month, even if no corresponding enrollment spell is observed. Second, if there is a gap of fewer than 24 months between observed enrollment spells or Medicaid claims, we treat the individual as continuously enrolled throughout the gap. Finally, if an individual's last observed enrollment spell ends less than 24 months before their 19th birthday, we treat them as continuously enrolled until their 19th birthday. This accounts for South Carolina's Medicaid eligibility rules, under which most individuals age out of coverage at age 19.

Using this measure of Medicaid enrollment, we construct indicators for diagnoses and health-

care utilization. For each person-month in which an individual is classified as enrolled, the indicator equals one if we observe the relevant diagnosis or healthcare utilization and zero otherwise. Person-months in which an individual is not classified as enrolled are coded as missing. This approach therefore assumes that, conditional on enrollment, the absence of an observed Medicaid claim indicates no healthcare utilization rather than healthcare obtained through another payer (e.g., private insurance).

Self-harm. The self-harm outcome is an indicator for one or more visits in a month with a diagnosis for suicidal ideation (ICD-9 V62.84; ICD-10 R45.851), suicide attempt (ICD-10 T14.91), or intentional self-harm. Intentional self-harm is identified in ICD-9 by E-codes E950–E958 and in ICD-10 by codes X70–X83 and by poisoning/injury codes T36–T65 and T71 whose intent character indicates intentional self-harm (excluding sequelae). Diagnoses of self-harm with undetermined intent are excluded.

Mental health visits. The mental health outcome is an indicator for one or more visits in a month with a diagnosis that falls within the mental disorders chapter of the ICD (ICD-9 codes 290–319 or ICD-10 chapter F). We exclude conditions attributable to known physiological causes and selected developmental conditions (e.g., intellectual disabilities, autism spectrum disorders, and disorders of speech, language, scholastic skills, and motor development). Visits with ADHD or self-harm diagnoses (as defined above) are also included. The outcome "2+ mental health visits" is an indicator for two or more such visits in a month.

Mental health categories. Categories of mental health conditions were constructed by identifying the ICD-10 codes corresponding to each Diagnostic and Statistical Manual of Mental Disorders, Fifth Edition (DSM-5) diagnosis and mapping them to their ICD-9 equivalents using a crosswalk. The categories include depression, anxiety, adjustment disorders, ADHD, oppositional defiant disorder, conduct disorder, bipolar disorder, post-traumatic stress disorder (PTSD), trauma- and stressor-related disorders, substance-related disorders (including alcohol and tobacco), intellectual disability, and other neurodevelopmental disorders. "Internalizing" diagnoses comprise depression, anxiety, bipolar disorder, PTSD, adjustment disorders without disturbance of conduct, and related conditions. "Externalizing" diagnoses comprise ADHD, conduct disorder, oppositional

defiant disorder, adjustment disorder with disturbance of conduct, and substance-related disorders. "Behavioral" diagnoses comprise oppositional defiant and conduct disorders. The diagnosis categories in Appendix Table A4 are indicators for one or more visits in a month with the corresponding diagnosis.

Mental health services. Mental health service outcomes are indicators for one or more visits in a month with the relevant service identified from Healthcare Common Procedure Coding System (HCPCS) codes. Services include therapy, pharmacologic management, substance abuse treatment, collaborative care management, residential and hospital-based mental healthcare, intensive outpatient treatment, rehabilitative and community support services, crisis intervention, case management, mental health evaluations and substance abuse tests. The "mental health service" outcome includes the above, except for mental health evaluations and substance abuse tests.

Mental health providers. Provider outcomes are indicators for one or more visits in a month with the relevant provider type, identified from Medicaid provider specialty codes. Mental health providers include therapists, psychiatrists, psychologists, social workers, counselors, alcohol and drug abuse providers, and Department of Mental Health clinics. Primary care providers include family and general practice, pediatrics, internal medicine, general clinics, and nurse practitioners. The remaining specialties are classified as "other" providers.

Prescription drugs. Prescription drug outcomes are indicators for one or more prescriptions in a month for the relevant drug class. Mental health drugs are grouped into antidepressants, anti-anxiety medications, antipsychotics (including mood stabilizers), and ADHD medications. ADHD drugs comprise stimulants and non-stimulant medications.

B.5 Criminal Justice Outcomes

Criminal justice outcomes are constructed from juvenile justice referrals to the Department of Juvenile Justice (DJJ), adult arrest records from the South Carolina Law Enforcement Division (SLED), and incarceration records from the Department of Corrections (DOC).

The main outcome "any arrest or referral" is an indicator for any juvenile referrals or adult arrests in a given month. The indicator "felony arrest or referral" refers to one or more referrals

or arrests for a felony offense.

We also classify offense-specific outcomes based on data from each agency. First, for DJJ, "substance-related offense" includes any referral or decision charge classified as drug-, alcohol-, tobacco-, or DUI-related. "Violent offense" equals one if either the referral or decision offense is classified by DJJ as violent. "School-related" equals one if either the referral or decision mentions a school-related incident (typically incidents such as disturbing schools, bringing weapons to school, or assaulting other students). "Detention" equals one if any record associated with the individual in that month indicates that the individual was detained.

Second, for SLED, "drug-related arrest" includes any arrest associated with a drug-, alcohol-, or DUI-related charge. "Violent-crime arrest" includes any arrest associated with murder, robbery, assault, a qualifying sex offense, or another charge classified as violent. Third, for DOC, we create an incarceration measure that equals one if the individual is in an incarceration spell during the month, based on recorded admission and release dates.

B.6 Other Data Sources and Variables

Student demographic characteristics, program participation, and mortality are drawn from state administrative sources. Student sex and race come from SCDE enrollment records, with students classified as Black if their recorded race is Black, and non-Black otherwise (i.e., white, Hispanic, other, or missing). Throughout the paper, we use the terms "non-Black" and "white" interchangeably (96% of students in the baseline sample are classified as either white or Black). SNAP and TANF participation are measured from DSS participation spells, converted to person-month indicators. These variables proxy for low-income status but are not considered as outcomes because in most cases the child's family rather than the child determines program participation. Death certificates are used to measure student mortality and to censor the panel after death.

School- and county-level characteristics come from external sources. Schools in the SCDE data are linked to National Center for Education Statistics (2024) by school code and year, providing information on student enrollment, full-time-equivalent teachers, pupil-teacher ratio, grade span, urbanicity, shares of students by race/ethnicity and sex, and the share receiving free or reduced-price lunch. School addresses are geocoded to assign county-level demographic characteristics, including the share of the population with Medicaid coverage, the share below the federal

poverty level, median household income, and the unemployment rate from American Community Survey data (Manson et al., 2024). County-level crime measures are constructed from FBI Uniform Crime Reports data (Kaplan, 2021). Because these data are at the agency level, we use the police department covering the largest population in each county and year.

We construct two measures of geographic access to public mental healthcare: (1) the road distance to the nearest provider and (2) the number of providers within a 30-minute drive. These measures vary at the school-year level. Provider locations are obtained from the South Carolina Department of Mental Health (SCDMH) directory of Community Mental Health Centers and satellite clinics. We restrict the provider list to locations offering outpatient mental health services to children or adolescents. School and provider addresses are geocoded and linked to county boundaries. Because SCDMH assigns patients to providers within their county of residence, school-provider pairs are restricted to those located in the same county. Road distances and travel times between each school and eligible providers are calculated using ArcGIS network routing tools. Distance to the nearest provider is defined as the minimum road distance among eligible providers, and provider supply is measured as the number of providers within a 30-minute drive.

B.7 Matching Procedure

For each treated school, the pool of eligible control schools consists of sample schools that satisfy four requirements: (1) the school did not experience a self-harm death within 24 months before or 24 months after the focal death; (2) the school year of the focal death is not the school's last observed year in the data (so that students can be followed after the placebo event); (3) the microdata cover at least 50% of the school's NCES-reported enrollment; and (4) the school serves the same (or similar) grades as the treated school.

Specifically, schools with typical grade spans (grades 6–8 or 9–12) are matched exactly on their lowest and highest grades in the NCES in the school year before the focal death. For schools with atypical grade spans, controls are instead required to fall within the same school category (e.g., middle or high school), defined by the school's grade span in the NCES in the school year before the focal death.

Once we select the set of eligible control schools for each treated school, we further refine this set using matching characteristics measured in the school year before the school year of the

death. In the baseline sample, we exactly match treated schools to eligible control schools on urban/rural classification and above/below-median enrollment bin. The urban/rural classification is based on the NCES locale code, harmonized across years into four categories (city, suburb, town, and rural), with schools classified as rural if assigned to the fourth category and urban otherwise. The enrollment medians are computed from the NCES enrollment numbers within school type (e.g., elementary, middle, or high school) and school year.

B.8 Analysis Sample and Weights

Each control school in a match is assigned a placebo event date equal to the date of the self-harm death in the corresponding treated school. The students associated with a match are those enrolled in the treated or control school in the school year of the (placebo) death. We then restrict this sample to students enrolled in Medicaid (per the adjusted measure described above) in the 24 months before and the 24 months after the (placebo) death. Because the same school can serve as a control for several treated schools, a student can appear in multiple matches. In practice, reuse of control students across matches is limited. Over 95% of students appear in five or fewer of the 30 matches, and because all of a student's appearances share a school, clustering at the school level accounts for any correlation across matches.

In the baseline weighting scheme, each student in the treated school receives a weight equal to one divided by the number of analysis-sample students in the treated school. Each control student receives a weight equal to one divided by the number of analysis-sample control students in the match. Thus, the treated school and the control group each receive a total weight of one within each match, and each event contributes equally to estimation. Within the control group, school identity does not enter the weights, so larger control schools implicitly contribute more.

We also report estimates using an alternative weighting scheme. Here, control schools receive weights proportional to the inverse Euclidean distance from the treated school, where distance is computed using standardized enrollment and share of students who are Black. Each control school's weight is divided equally among its students, and school weights are normalized to sum to one within each match.