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TRAUMA AT SCHOOL:
THE IMPACTS OF SHOOTINGS ON STUDENTS' HUMAN CAPITAL AND ECONOMIC OUTCOMES

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Trauma at School: The Impacts of Shootings on Students' Human Capital and Economic Outcomes
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ABSTRACT

We examine how shootings at schools—an increasingly common form of gun violence in the United States—impact the educational and economic trajectories of students. Using linked schooling and labor market data in Texas from 1992 to 2018, we compare within-student and across-cohort changes in outcomes following a shooting to those experienced by students at matched control schools. We find that school shootings increase absenteeism and grade repetition; reduce high school graduation, college enrollment, and college completion; and reduce employment and earnings at ages 24–26. We further find school-level increases in the number of leadership staff and reductions in retention among teachers and teaching support staff in the years following a shooting. The adverse impacts of shootings span student characteristics, suggesting that the economic costs of school shootings are universal.

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1 Introduction

Gun violence in the United States has been rising over the past two decades and is significantly higher than in other developed nations (IHME, 2021; JHU, 2022).[1] Of particular concern for parents, educators, and policy makers, gun violence that takes place *at schools* has not been immune to this trend: the number of shootings at U.S. schools doubled between 2000 and 2019, with more than 100,000 American children attending a school at which a shooting took place in 2018 and 2019 alone.[2] Although mass shootings at schools tend to receive significant media attention, 95 percent of shootings at schools between 2018 and 2019 resulted in fewer than two deaths, and nearly three-quarters of shootings led to no fatalities at all. Despite the prevalence of these less highly publicized acts of gun violence at schools—institutions whose central purpose is to promote human capital development—surprisingly little is known about the impacts of these events on the educational and labor market trajectories of surviving students.

In this paper, we use longitudinal, individual-level administrative data from the state of Texas to provide the first analysis of the short- and long-run educational and economic impacts of shootings at schools. Comparing within-student and across-cohort changes in outcomes following a shooting to those experienced by students at matched control schools, we find that experiencing gun violence at school has lasting implications for survivors. Our results indicate that exposure to a shooting at school disrupts human capital accumulation in the near-term through increased absences, chronic absenteeism, and grade retention; harms educational outcomes in the medium-term through reductions in high school graduation, college attendance, and college graduation; and adversely impacts long-term labor market outcomes including reductions in employment and earnings at ages 24–26. Heterogeneity analyses indicate that these detrimental effects are wide-reaching and span student characteristics. We further find

[1] The United States is an outlier relative to other high-income nations with respect to gun violence. In 2019, the age-adjusted firearm homicide rate in the United States was 22 times greater than in the European Union (IHME, 2021). High rates of gun violence in the United States have been linked to the country's relatively lenient gun control laws and higher rates of gun ownership (Lee et al., 2017; GBDC, 2018).

[2] Information on the number of school shootings per year comes from the Center for Homeland Defense and Security (CHDS) K-12 school shooting database. To approximate the number of children who attended a school where a shooting took place in 2018 and 2019, we multiply the number of shootings that took place on school grounds during school hours as reported in the CHDS data by the average enrollment at schools that experienced a shooting as reported in the *Washington Post* school shooting database.

school-level increases in the number of leadership staff and turnover among teachers and teaching support staff following a shooting, highlighting that gun violence at schools can impact many aspects of the school environment.

Our study is motivated by a large interdisciplinary body of research that characterizes the neurological and physiological mechanisms through which trauma from exposure to violence can impact young people, highlighting its influence on both the biological stress system and brain development.[3] Accordingly, a number of studies set in different countries and contexts have shown that exposure to local community and domestic violence has negative impacts on children's educational and behavioral outcomes.[4] At the same time, the literature on child resilience argues that children can "bounce back" after trauma, suggesting that exposure to violence during childhood may not have lasting effects.[5]

Moreover, the effects of shootings that take place at schools may differ from the effects of other types of violence previously examined in the literature. Recent work finds that local exposure to police killings of unarmed minorities adversely affects the mental health of Black adults (Bor et al., 2018) and the educational outcomes of Black and Hispanic youth (Ang, 2020). Analogously, children exposed to school shootings may suffer more severely because of their connection to the victims and the loss of trust in their schools' ability to keep them safe. Gun violence in schools may also be particularly traumatic if it triggers fears rooted in past, highly publicized mass shootings in schools (Lowe and Galea, 2017). Compared to violence in other settings, shootings that occur at schools may also cause greater disruption to students' learning by influencing other educational inputs, such as teaching quality, classroom resources, and continuity of instruction. Finally, since peer effects have been shown to be particularly important in school settings, the adverse impacts of a student's own trauma from experiencing a shooting may be amplified due to disruptions caused by other shooting-exposed peers (Carrell et al., 2018).

[3] See: Osofsky (1999); De Bellis (2001); Garbarino (2001); Perry (2001); Carrion et al. (2002); Murali and Chen (2005); Carrion et al. (2007); Lieberman and Knorr (2007); Carrion et al. (2008); Taylor et al. (2009); Carrion and Wong (2012); De Bellis and Zisk (2014); McDougall and Vaillancourt (2015); Romano et al. (2015); Russell et al. (2017); Heissel et al. (2018); Miller et al. (2018).

[4] See: Cook and Ludwig (2002); Aizer (2007); Sharkey (2010); Sharkey et al. (2012, 2014); Caudillo and Torche (2014); McCoy et al. (2015); Duque (2017); Monteiro and Rocha (2017); Bor et al. (2018); Gershenson and Tekin (2018); Sharkey (2018); Ang (2020); Koppensteiner and Menezes (2021); Bharadwaj et al. (2022).

[5] For example, see: Agaibi and Wilson (2005); Goldstein and Brooks (2005); Garrett et al. (2019).

Our analysis uses longitudinal, individual-level administrative data on all Texas public school students from the Texas Education Agency linked to data on the universe of school shootings from the Center for Homeland Defense and Security and the *Washington Post* school shootings databases. Importantly, these data sets include shootings both with and without fatalities, therefore capturing less severe incidents that may be more comparable to other forms of violence that frequently occur in schools. Our short-run analysis focuses on the 33 shootings that took place on school grounds during school hours at Texas public schools between 1995 and 2016. Since shootings are not distributed randomly across schools, we analyze changes in educational outcomes within the same students in the years before and after a shooting. In order to control for general time trends, we compare these within-student changes to changes among students from control schools that are matched based on institutional and student characteristics.

We find that shootings at schools adversely impact the educational outcomes of exposed students in the short run. In particular, exposure to a shooting leads to a 0.4 percentage point (12.1 percent relative to the pre-shooting mean) increase in the share of school days that a student is absent, a 1.8 percentage point (27.8 percent) increase in the likelihood of being chronically absent, and a 1.3 percentage point (124.5 percent) increase in the likelihood of grade repetition.[6] We find no significant effects on the frequency of disciplinary actions such as suspensions, expulsions, or in-school detentions.[7] We further find no effects on the likelihood of changing schools within the Texas public school system or of leaving the Texas public school system altogether.[8]

For the long-run analysis, we make use of linkages between the individual-level public school records and (i) college enrollment and graduation files from the Texas Higher Education Coordinating Board and (ii) employment and earnings data from the Texas Workforce Commission. We study the impacts of the eight shootings that took place at Texas public

[6]Absences are measured by the ratio of the number of days a student is absent relative to the number of days a student is enrolled in any school in our data in each academic year. Chronic absenteeism is an indicator denoting an absence rate of greater than 10 percent.

[7]Data on disciplinary actions is only available from 1998 onward. We therefore analyze the impacts of 26 school shootings that took place between 2001 and 2016 when studying these outcomes.

[8]We also have access to data on reading and math scores from standardized tests. However, most shootings take place in high schools, and no consistent standardized tests were administered to high school students over our sample period.

high schools over the period 1998–2006 on individual outcomes through age 26. Since these long-term outcomes are only observed after a shooting, we cannot measure within-student changes in them. We therefore compare cohorts of exposed students to cohorts that attended the same schools in the years before the shooting occurred. As in the short-run analysis, we compare these differences in cohort outcomes to the analogous differences among matched control schools. We examine the statistical significance of our long-run estimates using permutation tests in addition to inference based on conventional standard errors. We further show that our results are unlikely to be influenced by differential attrition.

We find that shootings at schools have lasting implications for the educational and labor market trajectories of exposed students. In particular, students who are exposed to a shooting at their school in grades 10–11 are 2.9 percentage points (3.7 percent relative to the pre-shooting mean) less likely to graduate high school, 4.4 percentage points (9.5 percent) less likely to enroll in any college, 5.5 percentage points (17.2 percent) less likely to enroll in a 4-year college, and 3.1 percentage points (15.3 percent) less likely to obtain a bachelor's degree by age 26. We also find that students exposed to shootings in grades 9–11 are 4.4 percentage points (6.3 percent) less likely to be employed and have $2,779.84 (13.5 percent) and $2,349.91 (7.5 percent) lower average annual earnings at ages 24–26 unconditional and conditional on working, respectively. Our estimates imply a $115,550 reduction (in 2018 dollars) in the present discounted value of lifetime earnings per shooting-exposed student. Additional analyses suggest that at most a quarter of the earnings effect can be explained by the estimated reduction in college completion, suggesting that shootings have impacts on labor market outcomes that operate through channels beyond educational attainment.

We explore heterogeneity in the impacts of shootings at schools by student characteristics, school resources, and type of shooting. When considering student characteristics such as race and gender, we find that the detrimental consequences of school shootings are relatively universal, with all sub-groups being affected. That being said, non-Hispanic Black students and those who receive free or reduced-price lunch experience relatively larger adverse effects on some outcomes, suggesting that shootings at schools may exacerbate pre-existing disparities in student outcomes between more and less advantaged groups. Heterogeneity analyses by school resources highlight that differences in access to mental health care treatment on campus are unlikely to account for these patterns: we find no significant differences in the short-run impacts of shootings across schools with higher versus lower availability of different types of health professionals (school counselors, psychologists, social workers, physicians, and nurses) in the year before the shooting. Additionally, when we follow Levine and McKnight (2020b) and assign shootings into four mutually exclusive categories (suicides, personally-targeted, crime-related, and other), we find that the adverse short-term effects on absenteeism are particularly large for personally-targeted shootings, while the impacts on grade repetition are largest for crime-related shootings.[9]

Finally, we analyze the impacts of shootings at schools on the employment and retention of teachers and other school staff. Shootings at schools are unique relative to gun violence in other settings in that they have the potential to impact many aspects of the school environment. Examining effects on the number of personnel, we find that schools increase the number of full-time equivalent (FTE) school leadership staff by an average of 0.55 per 1,000 students (18.8 percent relative to the pre-shooting mean) following a shooting on school grounds. This effect is driven predominately by an increase in the number of assistant principals, who are the staff typically responsible for dealing with safety and disciplinary issues at schools. Moreover, while we find no effects on the total number of FTE teachers, teaching support staff, or social support staff per 1,000 students, we observe an increase in the turnover rate among teachers and teaching support staff in the years following a shooting. Disruptions to the continuity of instruction could therefore contribute to the negative effects on student outcomes that we find.

Our study contributes to three strands of literature. The first is a small but growing set of studies on the impacts of school shootings.[10] Recent work documents that school shootings can have detrimental effects on the mental health (Rossin-Slater et al., 2020; Levine and McKnight, 2020a)[11] and short-run educational outcomes (Poutvaara and Ropponen, 2018; Beland and Kim, 2016; Levine and McKnight, 2020a) of surviving youth. We add to this literature in

[9]We have too few shootings in the long-run analysis sample to examine heterogeneity by shooting type or school resources.

[10]A related literature examines the determinants of gun violence at schools; see, e.g., Pah et al. (2017) and Livingston et al. (2019).

[11]See Lowe and Galea (2017), Travers et al. (2018), Iancu et al. (2019), and Rowhani-Rahbar et al. (2019) for recent overviews of the broader interdisciplinary literature on the mental health impacts of school and mass shootings.

three ways. First, while previous work has predominantly relied on school- or district-level data, our use of individual-level data enables us to identify students exposed to each event, precisely estimate the impacts of this exposure over time, and investigate heterogeneity in these impacts across student, school, and shooting characteristics. Moreover, while previous studies have focused largely on near-term effects of shootings, our linked educational and labor market data provide a unique opportunity to examine the effects of shootings up to a decade after they occur. Finally, while attention is often focused on indiscriminate mass shootings at schools that result in numerous fatalities (e.g., Columbine, Sandy Hook, Parkland), mass shootings are rare, and most shootings that take place at schools result in no deaths. Our analysis captures the effects of gun violence that is more common in schools and may be more comparable to other forms of violence to which children are frequently exposed.[12]

Our work further contributes to a broader literature on the persistent effects of exposure to gun violence in non-school settings. Recent work by Bharadwaj et al. (2022) finds that Norwegian teenagers exposed to the 2011 massacre on the island of Utøya experienced adverse impacts on test scores, health visits, educational attainment, and earnings. Focusing on police violence in Los Angeles, Ang (2020) documents a deterioration of educational outcomes among minority students living close to the location of a police killing.[13] Despite differences in the analyzed contexts (see Section 5 for a more detailed comparison), we find that gun violence at schools has similar long-term consequences for exposed students. This collective evidence therefore uncovers just how vulnerable children are to exposure to gun violence. Our findings highlight that shootings that take place at schools—especially those that are more common but less highly publicized than events with many fatalities—are an increasingly important yet under-researched source of such exposure.

Finally, our work contributes to an expansive literature investigating the long-run effects of childhood circumstances and educational inputs. Prior work has investigated the impacts of

[12] In fact, no mass shootings occurred in the Texas public school system during the two decades spanned by our sample of shootings. In this way, our work complements Levine and McKnight (2020a)'s study of the impacts of the Sandy Hook Elementary School shooting—a large mass shooting event—on student absences and test scores. Our combined body of evidence suggests that all types of shootings at schools have detrimental impacts on survivors' educational outcomes.

[13] Other recent work on community violence in the United States has documented the impacts of mass shootings on community-wide mental health (Soni and Tekin, 2020) and local economic factors such as employment, earnings, and housing prices (Brodeur and Yousaf, 2020). Koppensteiner and Menezes (2021) and Jarillo et al. (2016) document educational impacts of exposure to community homicides in Brazil and Mexico, respectively.

preschool programs like Head Start and the Perry Preschool (e.g., Garces et al. 2002; Ludwig and Miller 2007; Heckman et al. 2013), neighborhood quality (Chetty et al., 2016; Chetty and Hendren, 2018), kindergarten classroom assignment (Krueger and Whitmore, 2001; Chetty et al., 2011; Dynarski et al., 2013), teacher value-added (Chetty et al., 2014), elementary school class rank (Denning et al., 2020), and the age at which a child starts school (Bedard and Dhuey, 2006; Black et al., 2011). While much of this research identifies positive impacts of school- or classroom-level educational interventions in early grade levels, our results suggest that an increasingly common adverse school-level shock in later grades—exposure to a shooting—can offset substantial advantages from earlier inputs. Our work demonstrates that policy discussions about improving children's long-term economic outcomes through the school system should go beyond traditional educational inputs and consider how to prevent—and mitigate the harmful effects of—exposure to trauma at school.

The remainder of the paper proceeds as follows. Section 2 provides additional details on the data, and Section 3 outlines our empirical strategies. Section 4 provides main results, heterogeneity analyses, and robustness exercises. Section 5 contains a discussion of the results. Section 6 concludes.

2 Data

2.1 Shootings at Schools

Our data on shootings at schools come from two sources. First, we use the Center for Homeland Defense and Security (CHDS) K-12 school shooting database, which is a comprehensive account of all incidents in the United States in which "...a gun is brandished, is fired, or a bullet hits school property for any reason, regardless of the number of victims, time, day of the week" (Riedman and O'Neill, 2020).[14] The database includes incidents from 1970 onward and is continuously updated with new information; the version of the database used in our analysis was downloaded in July 2019. The data contain information on the school name and location, date and time of the incident, information on the number of deaths and physical

[14] The CHDS data are compiled from more than 25 different original sources including peer-reviewed studies, government reports, media, non-profit organizations, private websites, blogs, and crowd-sourced lists. Additional information is provided here: `https://www.chds.us/ssdb/about/`.

injuries, and a summary of the event (e.g., "Teen fired shot at another group of teens during a dispute").

Second, we cross-check and augment the shootings observed in the CHDS data with those listed in the *Washington Post* school shootings database. The *Washington Post* data contain information on acts of gunfire at primary and secondary schools since the Columbine High massacre on April 20, 1999.[15] The database excludes shootings at after-hours events, accidental discharges that caused no injuries to anyone other than the person handling the gun, and suicides that occurred privately or posed no threat to other students. As with the CHDS data, the *Washington Post* database is updated as facts emerge about individual cases; the version of the database used in our analysis was downloaded in April 2019.

As outlined in Section 2.2, our outcome data span the academic years 1992–1993 to 2017–2018. During this time period, there were 66 shootings at Texas public schools. Two schools experienced two shootings over our sample period; we only consider the first shooting at a given school (64 shootings). Since we are interested in studying the impacts of exposure to shootings on student outcomes, we further limit the sample to the 43 shootings that occurred *during school hours* (i.e., we drop shootings that occurred on weekends, evenings, or during school breaks) and *on school grounds* (i.e., we drop shootings that occurred off school property). In addition, in order to measure outcomes three years before to two years after a shooting in the short-run analysis, we focus on the 33 shootings that took place between the academic years 1995–1996 and 2015–2016.[16] For the long-run analysis, we consider the eight shootings that took place at Texas public high schools between the academic years 1998–1999 and 2005–2006. This allows us to measure outcomes at all ages between 18 and 26 for all cohorts.

The 33 shootings included in our sample vary in severity and situation (see Appendix Table A1 for a description of each event). While no shooting in our sample led to multiple deaths, approximately half of the shootings (15) resulted in one fatality. Among the 18 non-fatal shootings, 11 led to at least one (physically) injured victim, with 1.45 victims being injured on average. These statistics underscore the fact that most shootings that occur in schools

[15] To compile the *Washington Post* database, reporters used *LexisNexis*, news articles, open-source databases, law enforcement reports, information from school websites, and calls to schools and police departments. The data are available for download here: `https://www.washingtonpost.com/graphics/2018/local/school-shootings-database/`.

[16] Among these 33 shootings, 32 (9) are included in the CHDS (*Washington Post*) data. Eight of the shootings are included in both data sets.

are not as deadly as those typically covered in the media. Nevertheless, these shootings may affect the many students who are at school when they occur.

Figure 1 displays the locations of the shootings used in our analyses, and Appendix Figure A1 depicts the number of shootings per academic year. The spread of shootings across the state largely reflects the distribution of Texas's population. Moreover, all but three years over our analysis period had at least one shooting, with the 2006–2007 academic year witnessing the maximum of six shootings.

2.2 Educational and Labor Market Outcomes

Our outcome data come from three sources.[17] First, we use individual-level, administrative data from the Texas Education Agency (TEA). The TEA data cover all students in all public K–12 schools in Texas over the academic years 1992–1993 through 2017–2018 and include information on students' attendance, graduation, and disciplinary actions (i.e., suspensions, expulsions, and in-school detentions).[18] The data further contain information on student characteristics—such as age, gender, race/ethnicity, and receipt of free or reduced-price lunch.

We use the TEA records to create five outcomes for each student at an annual (academic year) level: (1) a continuous absence rate, measured as the ratio of the number of days a student is absent relative to the number of days a student is enrolled in any school in our data; (2) an indicator denoting chronic absenteeism, which we define as an absence rate of greater than 10 percent; (3) an indicator denoting grade repetition; (4) the number of days of disciplinary action taken against a student; and (5) an indicator denoting whether the student switched schools.[19] We further obtain information on whether a student graduated

[17] We access these data through the Education Research Center at The University of Texas at Austin. Additional information is available here: https://research.utexas.edu/erc/.

[18] Data on disciplinary actions is only available from the academic year 1998–1999 onward.

[19] The number of days of disciplinary action is windsorized at the 99th percentile to reduce the influence of outliers. We measure school switches with an indicator that is set to one when a student is enrolled in a school at the beginning of the academic year that is different from the one in which he/she was enrolled in at the beginning of the previous academic year, excluding transitions from elementary to middle and middle to high school. We have further considered an indicator denoting that a student is in a special education program as an outcome. However, since only 0.1 percent of all student-year observations in our analysis sample are enrolled in special education, we are underpowered to detect effects.

high school—and if so, at which age—from these records.[20]

We also use information on school staff from the TEA. These data contain annual records for each staff member at each school, and include information such as their FTE units and job title. We construct school-level measures of the number of FTE staff per 1,000 students across different staffing categories in each year: teachers, school leadership (principals and assistant principals), teaching support (e.g., educational aides), and social support (e.g., counselors and school psychologists). We use these data to explore heterogeneity in our main effects by baseline levels of social support staff. We further use this information to estimate effects on school staff, focusing on aggregate annual employment and turnover rates as outcomes.

Second, we use administrative microdata on enrollment and graduation from all public and most private institutions of higher education in the state of Texas from the Texas Higher Education Coordinating Board (THECB).[21] The THECB data are linked to the TEA data at the individual level. We measure three outcomes in the THECB data for each individual at age 26: (1) an indicator for ever having enrolled in college, (2) an indicator for ever having enrolled in a 4-year college, and (3) an indicator for ever having obtained a bachelor's degree. We do not have information on out-of-state college enrollment or enrollment at some private institutions in Texas; as discussed in Section 3.3, this is unlikely to bias our results.

Finally, we use quarterly, administrative data on employment and earnings for all workers covered by the Unemployment Insurance (UI) program from the Texas Workforce Commission

[20] We further have access to data on reading and math scores from standardized tests. However, it is difficult to examine test scores as an outcome for two reasons. First, Texas used different standardized tests that were administered to different grades over the course of our analysis period: the Texas Assessment of Academic Skills (TAAS) was used until 2002, the Texas Assessment of Knowledge and Skills (TAKS) was used from 2003–2011, and the State of Texas Assessments of Academic Readiness (STAAR) have been used since 2012. Second, while 3rd and 8th grade test scores are comparable over time, the majority of the shootings in our analysis sample occurred in high schools (see Appendix Table A2). While we therefore do not consider test scores as an outcome, we do use test scores in a robustness exercise (see Section 4.3).

[21] The THECB collects data from (1) all public institutions of higher education in Texas and (2) private institutions of higher education in Texas that participate in data sharing. More specifically, the THECB data contain all public community, technical, and state colleges; all public universities and health-related institutions; almost all independent colleges and universities (available from 2002 onward); and some private technical colleges (available from 2003 onward). See http://www.txhighereddata.org/Interactive/CBMStatus/ for additional information on participating institutions. Enrollment at independent colleges and universities (private technical colleges) accounted for approximately 11% (3%) of Texas college enrollment in 1999 (THECB, 2000). We note that our research design includes year fixed effects, which allows us to control for changes in data coverage over time.

(TWC).[22] As with the THECB data, the TWC data are linked to the TEA data at the individual level.[23] This allows us to follow students from school to the labor market. We use the TWC data to create three outcomes, all of which are measured once for each individual when they are aged 24–26: (1) an indicator for being employed, measured by having positive earnings in any quarter; (2) average real annual earnings, measured in 2018 dollars; and (3) average non-zero annual earnings (i.e., conditional on having positive earnings in a given year). While we do not observe information about employment outside of Texas, we do not expect this limitation to significantly influence our estimates (see Section 3.3).

3 Empirical Design

Our goal is to analyze the causal effects of exposure to a shooting at school on students' short- and long-term outcomes. We use two sets of difference-in-difference strategies to deliver these estimates, comparing either within-student or across-cohort changes in outcomes among students at schools that experienced a shooting to analogous changes in outcomes among students at schools that did not experience any shootings. In this section, we begin by describing our process for choosing control schools. We then present our samples and empirical strategies for the short- and long-run analyses.

3.1 Matching Schools with Shootings to Control Schools

As noted in Section 2.1, 33 public schools in Texas experienced a shooting during school hours and on school grounds over the academic years 1995–1996 to 2015–2016. To reduce

[22] UI covers all workers whose employers pay at least $1,500 in gross earnings or have at least one employee during twenty different weeks in a calendar year. Federal employees are not covered. See `https://www.twc.texas.gov/tax-law-manual-chapter-3-employer-0` for more details.

[23] The TEA records are linked to the THECB and TWC records using a unique identifier, which is an anonymized version of an individual's social security number (see: `https://texaserc.utexas.edu/wp-content/uploads/2016/03/Matching_Process.pdf`). Individuals with invalid identifiers cannot be matched to the THECB and TWC data and are thus excluded from our long-run analysis of college and labor market outcomes. Approximately 8.8 percent of students eligible for our long-run analysis sample (outlined in Section 3.3) have invalid identifiers in the TEA data. Reassuringly, we find no systematic difference in the likelihood of having an invalid identifier between shooting-exposed and non-exposed students. Note that students with valid identifiers in the TEA data who do not appear in the THECB or TWC data are still included in our long-run analysis (and are considered to not have attended college in Texas and to not be employed in Texas, respectively).

concerns about differential trends between schools with and without shootings biasing our estimates, we choose control schools that are similar on a set of observable characteristics using a "nearest-neighbor" matching procedure.

Specifically, for each school with a shooting, we first identify all other schools that are in different districts but offer the same grade levels (e.g., high schools are only matched with other high schools), have the same "campus type" (which is one of 12 categories based on population size and proximity to urban areas), and have the same charter school status.[24] We exclude schools in the same district as there could be spillover effects of shootings to nearby schools. We then use the nearest-neighbor matching algorithm to select the two "nearest" control schools based on a fuzzy match on the following school-level characteristics (not spatial proximity): share female students, share students receiving free or reduced-price lunch, share non-Hispanic white students, share non-Hispanic Black students, share Hispanic students, and total enrollment. We measure these variables in the first six-week grading period of the academic year of the shooting. As discussed in Section 4.3, our results are robust to the use of alternative matching strategies.

Appendix Table A2 presents average school characteristics for schools that experience a shooting (column (1)), matched control schools (column (2)), and all Texas public schools (column (3)). The fourth column presents $p-$values from tests of differences between mean characteristics of shooting and matched control schools, while the fifth column presents $p-$values from tests of differences between mean characteristics of shooting schools and all Texas public schools. Panels A and B present statistics separately for high schools and non-high schools, respectively.

Comparing columns (1) and (3), it is evident that schools that experience shootings are not randomly selected. Relative to the average public high school in Texas, high schools that experience shootings have higher enrollment, are located in more urban areas, and have higher shares of non-Hispanic Black students. Non-high schools with shootings are also larger and have lower shares of non-Hispanic white students than the average public elementary or middle

[24]The National Center for Education Statistics classifies schools into different 12 "campus types": City-Large, City-Midsize, City-Small, Suburban-Large, Suburban-Midsize, Suburban-Small, Town-Fringe, Town-Distant, Town-Remote, Rural-Fringe, Rural-Distant, Rural-Remote. Note that schools in the same district can have different campus types. See `https://tea.texas.gov/reports-and-data/school-data/campus-and-district-type-data-search` for more details.

school in Texas. Reassuringly, our matching algorithm is successful at selecting control schools that are similar to schools that experience shootings: as shown in column (4), there are no significant differences in these characteristics across treatment and control schools.

3.2 Short-Run Analysis

In the short-run analysis, we focus on outcomes that can be measured both before and after a shooting for a given student in the TEA data (e.g., attendance and disciplinary actions). To construct our short-run analysis sample, we begin by considering all students who were enrolled in the 33 shooting and 66 control schools in the academic semester during which a shooting took place.[25] We further restrict our sample to students who are observed in the data three years before to two years after the shooting (i.e., a six-year period); this requirement leads us to study students who were in grades 3–10 at the time of the shooting. Importantly, we do not require that students stay in the same school over their six years in the TEA data.[26] Our final short-run analysis sample consists of 62,228 students (22,363 at shooting schools and 39,865 at matched control schools).

We use this sample to estimate difference-in-difference models in which we compare within-student changes in outcomes following a shooting between the shooting and matched control schools. Our regressions take the form:

$$Y_{isgt} = \beta ShootingSchool_s \times Post_t + \alpha_i + \theta_{gt} + \epsilon_{isgt} \tag{1}$$

where Y_{isgt} is an outcome in academic year t for student i who was enrolled in school s in match group g at the time of the shooting. $ShootingSchool_s$ is an indicator denoting schools that experienced a shooting, and $Post_t$ is an indicator denoting observations in the academic

[25] Enrollment information is available for every student for six six-week grading periods per academic year. We define the fall (spring) semester as containing the first (last) three six-week periods. We include all students who are enrolled in the shooting and control schools at any point in the semester of the shooting (e.g., a student who is enrolled in a shooting school at the beginning of the semester of a shooting, but switches to a different school by the end of the semester, is included in our sample).

[26] That is, we keep students who attend other schools either before or after the academic year of the shooting, as long as they are in the TEA data. Students at control schools who were ever enrolled in a shooting school (3.6 percent of all students at the control schools) are excluded from our sample.

year of the shooting and the following two years.[27] We include individual fixed effects, α_i, which account for all time-invariant differences between shooting-exposed and non-exposed students. We also include a full set of match group–by–academic year fixed effects, θ_{gt}, which flexibly account for match group–specific trends in outcomes. Standard errors are clustered by school (i.e., we account for $33 + 66 = 99$ clusters of shooting and control schools). The key coefficient of interest is β, which measures the difference in the change in student outcomes following a shooting between shooting and control schools within each match group.

Causal interpretation of β relies on a standard parallel trends assumption. That is, we must assume that outcomes would have evolved similarly for students enrolled at the shooting and control schools within each match group in the absence of a shooting. To assess the validity of this assumption, we compare raw trends in outcomes between shooting and control schools. In addition, we estimate event study specifications of the following form:

$$Y_{isgt} = \sum_{t=-3, t\neq -1}^{2} \rho_t ShootingSchool_s \times \mathbf{1}_t + \sigma_i + \kappa_{gt} + \eta_{isgt} \tag{2}$$

where academic year t is measured relative to the year of the shooting in each match group, and all other variables are defined similarly to those in equation (1). The key coefficients of interest are ρ_t, which capture the year-by-year differences in within-student changes among students enrolled in shooting schools compared to those enrolled at control schools at the time of the shooting. As discussed in Section 4.1, the raw data plots and event study estimates reveal no evidence of differential pre-trends between treatment and control schools.

An additional concern for our short-run analysis is that of possible differential attrition from the sample. That is, it is possible that students systematically leave the Texas public school system—either because they switch to private schools or because they move out of state—as a result of exposure to a shooting at school. This type of response has been documented in prior studies analyzing aggregate data on school enrollment (Abouk and Adams, 2013; Beland and Kim, 2016).

Our primary short-run analysis focuses on a balanced panel of students and includes in-

[27] Since grade repetition reflects academic performance in the previous academic year, we exclude the year of the shooting from $Post_t$ when analyzing this outcome. We also include a separate interaction term between $ShootingSchool_s$ and an indicator for the year of the shooting.

dividual fixed effects, ensuring that our estimates are not driven by compositional differences in time-invariant factors between those in shooting and control schools. Nevertheless, our primary estimates could be biased if there is differential attrition from the Texas public school system that is correlated with changes in the outcomes that we analyze. To address this concern, we examine whether students in schools that experience a shooting are more likely to leave the Texas public school system following the event relative to students at matched control schools. To do so, we consider an unbalanced sample that is constructed in the same way as our primary analysis sample except that we only require students to be observed in the Texas public school system in the year of the event (rather than three years before to two years after).

Appendix Figure A2(a) plots the share of students who appear in the TEA data in each year surrounding a shooting, separately for students at shooting and control schools. While about 8 percent of students are missing in a given year on average, we find no significant difference in the rate of attrition between students at shooting and control schools. Moreover, we estimate equation (2) using an indicator denoting whether each student appears in the TEA data in a given year as the outcome. As shown in Appendix Figure A2(b), while students at shooting schools are slightly less likely to be observed in the data than those at control schools, this difference is similar in years before and after the shooting. Thus, there is no evidence of differential attrition out of the Texas public school system that is caused by exposure to a shooting at school. In addition, we show in Section 4.3 that our short-run estimates are very similar if we use a balanced or unbalanced panel.

Finally, an advantage of using individual-level data covering the entire Texas public school system is that we can observe students switching across Texas public schools. Using the same balanced panel of students as in our main analysis, we compare school switching rates between students enrolled at the shooting and control schools at the time of a shooting. As we discuss in Section 4.1, we do not find any evidence that students enrolled at schools that experience a shooting are more or less likely to switch to other Texas public schools after the event.

3.3 Long-Run Analysis

Our long-run analysis focuses on outcomes that can only be observed after the shooting in the TEA, THECB, or TWC data (e.g., high school graduation by age 26 and employment at ages 24–26). Since we only observe each outcome after the event, we cannot examine within-student changes in these outcomes. Instead, our difference-in-difference models compare differences in cohort outcomes between students who were enrolled in treatment schools at the time of the shooting and students who were enrolled in the same schools five years earlier, relative to analogous differences in cohort outcomes at matched control schools. As outlined in Section 2.1, our long-run analysis considers the eight shootings that took place at Texas public high schools between the 1998–1999 and 2005–2006 academic years. This allows us to observe outcomes between the ages of 18 and 26 for all cohorts.

We construct our long-run analysis sample by first considering all students who were in grades 9–12 in the academic year of a shooting at one of the shooting or matched control schools. We then include students who were too old to be exposed to the event by including students who were enrolled in grades 9–12 at the same schools *five years before the year of the shooting*.[28] That is, our "too old" cohorts would be in "expected" grades 14–17 at the time of the event. Our final long-run analysis sample consists of 31,414 students who were in grades 9–12 at the time of the shooting (11,447 at treatment schools and 19,977 at matched control schools) and 28,197 students who were in grades 9–12 five years earlier (10,917 at treatment schools and 17,280 at matched control schools).

We use this sample to estimate two types of models. First, we examine within–match group differences between cohorts at shooting and control schools using specifications of the form:

$$Y_{isdg} = \sum_{d=9, d\neq 13}^{17} \pi_d ShootingSchool_s \times \mathbf{1}_d + \lambda_{dg} + \delta' X_i + \varepsilon_{isdg} \tag{3}$$

where Y_{isdg} is an outcome for student i in cohort d who was enrolled in school s in match group g at the time of the shooting (or five years before the shooting for the "too old" cohorts). $ShootingSchool_s$ is again an indicator denoting schools that experienced a shooting.

[28] We use students enrolled five years before the shooting as our "too old" cohorts because we want to account for the effect on grade repetition that we uncover in our short-run analysis (see Section 4.1). Students who are enrolled in a shooting school four years before the shooting may still be there at the time of the shooting if they repeat a grade.

We include a full set of match group–by–cohort fixed effects, λ_{dg}, where the set of cohort indicators denote each of the possible grade levels at the time of the shooting (9–12 for those enrolled at the time of the shooting; 14–17 for the "too old" cohorts). These match group–by–cohort fixed effects flexibly account for trends in outcomes across cohorts within each match group. We also include a vector of individual-level controls, X_i, indicating student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. We cluster standard errors at the school-by-cohort level. The key coefficients of interest are π_d, which measure the differences in outcomes between students in shooting and control schools in each cohort d within each match group.

An advantage of equation (3) is that we can explicitly examine whether there are pre-trends in outcomes by looking at the π_d coefficients for the cohorts who are in expected grades 14–17 at the time of the shooting. We should not expect to see statistically significant differences in outcomes between students at treatment and control schools among cohorts that left these schools before the shooting took place. However, by estimating separate interaction coefficients π_d for these "too old" cohorts, we cannot additionally include school fixed effects. Therefore, we also estimate the following specification:

$$Y_{isdg} = \sum_{d=9}^{12} \psi_d ShootingSchool_s \times \mathbf{1}_d + \nu_{dg} + \tau_s + \omega' X_i + u_{isdg} \tag{4}$$

where τ_s are school fixed effects, and all other variables are defined similarly to those in equation (3). We again cluster standard errors at the school-by-cohort level. The key coefficients of interest are ψ_d, which measure differences in outcomes between exposed versus "too old" cohorts across shooting and matched control schools.

As noted in Section 2.2, we do not observe college enrollment and completion information for out-of-state colleges and some private institutions in Texas. We also do not observe labor market information for individuals who leave Texas. From our short-run analysis, we find that exposure to a shooting at school does not lead students to be more or less likely to continue enrollment in Texas public primary and secondary schools in the two years after the event, suggesting that exposure to a shooting does not impact whether students move out of state in the short run. Nevertheless, it is possible that students exposed to a shooting at school may be more or less likely than unexposed students to move out of state in the long run. We discuss this issue in more detail in Section 4.2 and conclude that differential mobility among shooting-exposed students is unlikely to bias our long-run results.

4 Results

4.1 Short-Run Effects on Student Outcomes

Figure 2 presents raw trends in our short-run outcomes over the six years surrounding each shooting, separately for shooting and matched control schools. For both the continuous absence rate and an indicator denoting chronic absenteeism in sub-figures (a) and (b), respectively, we observe very similar trends in the three years before a shooting across the shooting and control schools. Absences are increasing in event time for both groups, reflecting the fact that absenteeism increases with age. However, starting with the academic year of a shooting (denoted as year 0 on the $x-$axis), we see a divergence in these trends, with students at schools that experience a shooting having higher rates of absences and chronic absenteeism. This divergence persists for two years following the event. Similarly, rates of grade repetition (sub-figure (c)) are almost identical in shooting and control schools in the years before a shooting but are substantially higher in schools that experience a shooting in the two years after the event. In sub-figure (d), we see that students in shooting schools tend to have more days of disciplinary action than students in control schools before a shooting, although the pre-shooting trends are similar. This difference in levels becomes more pronounced in the year of and the year after a shooting, with the gap in days of disciplinary action between shooting and control schools returning to pre-shooting levels two years after the event. Lastly, when we consider school switching in sub-figure (e), we find similar trends for students in shooting and control schools both before and after a shooting.

The raw trends provide suggestive evidence that: (1) there are no noticeable differences in pre-trends between students at shooting and control schools, and (2) several student outcomes deteriorate following a shooting at their school. Event study estimates demonstrate that these conclusions are robust to the inclusion of individual and match group–by–academic year fixed effects. In particular, Figure 3 plots the coefficients and 95% confidence intervals on the interactions between the indicator denoting a shooting school and the indicators denoting each of the years before and after a shooting from estimation of equation (2). Importantly, there are no significant differences between shooting and matched control schools in the pre-shooting period; this supports the parallel trends assumption that is required for the validity of our research design. Furthermore, sub-figures (a) and (b) demonstrate that the average absence rate and likelihood of chronic absenteeism, respectively, increase in the year of a shooting and remain at elevated levels for the following two years. When we analyze grade repetition in sub-figure (c), an effect materializes in the year after a shooting, which is the earliest academic year when we could see an effect on an outcome that reflects inadequate academic progress in the prior year. Finally, sub-figures (d) and (e) indicate that there are no statistically significant changes in the average number of days of disciplinary action and likelihood of school switching, respectively, in the two years after a shooting.

Table 1 presents results from estimation of equation (1), in which we pool the post-shooting years to capture the average effects of shootings at schools on our short-run outcomes. As shown in column (1), exposure to a shooting at school leads to an average increase in the absence rate of 0.4 percentage points ($p-$value=0.022), or 12.1 percent relative to the pre-shooting mean of 3.65 percent. Exposure to a shooting further increases the rate of chronic absenteeism: column (2) indicates that chronic absenteeism rises by 1.8 percentage points ($p-$value=0.027), or 27.8 percent relative to the baseline mean of 6.43 percent, following a shooting. Moreover, the rate of grade repetition increases by 1.3 percentage points ($p-$value=0.016) in the two years following a shooting, which represents more than a doubling of the baseline grade repetition rate.[29] As shown in columns (4) and (5), estimates of the effects of shootings at schools on days of disciplinary action and school switching rates, respectively, are not statistically significant at conventional levels.

Heterogeneity analyses Having shown that shootings at schools impact several short-run student outcomes, we explore heterogeneity in these estimates across shooting, student, and school characteristics. Using the categorization suggested by Levine and McKnight (2020b), we classify shootings into four mutually exclusive categories: suicides, personally-targeted,

[29] Because the earliest academic year in which grade repetition could be affected is the year after a shooting, we exclude the shooting year itself from the $Post_t$ indicator when considering grade repetition as an outcome.

crime-related, and other.[30] For each category of shootings, Figure 4 displays coefficients and associated 95% confidence intervals from estimation of equation (1).[31] Our baseline results for the full set of shootings, first presented in Table 1, are displayed at the top of each sub-figure for reference. While many of the confidence intervals overlap across estimates, a few patterns are worth noting. First, the effects on absences and chronic absenteeism are largest for personally-targeted shootings. Second, the coefficient estimates for grade repetition are particularly large for crime-related and personally-targeted shootings. For the number of days of disciplinary action and school switching, the confidence intervals for all sub-group estimates include zero, precluding us from detecting clear patterns in heterogeneity by shooting type.

Our individual-level data further allows us to explore heterogeneity in effects by student characteristics. In particular, we estimate equation (1) separately for sub-groups defined by the following characteristics: gender, race/ethnicity, grade at the time of the shooting (high school or non-high school), and ever receiving free or reduced-price lunch in the pre-shooting period.[32] Figure 5 displays the estimated coefficients and associated 95% confidence intervals; the pattern of results is very similar if we instead report estimates relative to sub-group specific outcome means (see Appendix Figure A3). Strikingly, there appear to be substantial impacts on each of the sub-groups analyzed; this highlights the wide-reaching effects of shootings at schools on exposed students. While absences, chronic absenteeism, and grade repetition are affected for all sub-groups, the point estimates suggest that the effects may be particularly pronounced for non-Hispanic Black students and students who have ever received free or reduced-price lunch.

Lastly, we analyze heterogeneity in effects across schools with different resources to help students cope with trauma, as measured by the availability of various health professionals on campus. In particular, we split schools based on whether they have an above- or below-median

[30] The CHDS data assign each shooting into one of 19 categories; we use this information to form the four aggregate groups from Levine and McKnight (2020b). In particular, "personally-targeted" shootings include escalation of dispute, anger over grade/suspension/discipline, bullying, domestic disputes with a targeted victim, and murder; "crime-related" shootings include gang-related, hostage standoffs, illegal drug related, and robberies; and "other" shootings include mental health-related, intentional property damage, officer-involved, racial, self-defense, accidental, and unknown. Among our 33 shootings, 11 are suicides, four are personally-targeted, two are crime-related, and 16 are other shootings.

[31] Since we have relatively few shootings—and therefore few clusters—in some of the categories, we present 95% confidence intervals based on a wild cluster bootstrap.

[32] In these analyses, we drop schools in which there are fewer than 10 students in a particular category and only use match groups that contain three schools (one shooting and two control schools).

FTE allocation of different types of health professionals per student in the year before the shooting. Since only seven out of the 33 shooting schools have any positive FTE allocation of school psychologists or social workers at baseline, we split schools based on whether they have any positive FTE allocation of school psychologists or social workers when analyzing heterogeneity by these types of health professionals. Figure 6 presents coefficients and 95% confidence intervals from estimation of equation (1) for each school type.[33] We find no evidence of differential impacts based on the presence of different types of health professionals in schools at baseline.

4.2 Long-Run Effects on Educational and Economic Outcomes

Figure 7 presents estimates of the effects of exposure to a shooting at school on students' educational outcomes by age 26. In each sub-figure, the graph on the left-hand side presents the coefficients and 95% confidence intervals on the interactions between the indicator denoting a shooting school and the cohort indicators from estimation of equation (3), while the graph on the right-hand side presents the coefficients and 95% confidence intervals on these interactions from estimation of equation (4). An advantage of specification (3) is that we can explicitly examine the possibility of differential trends by estimating effects of placebo exposure for "too old" cohorts who are in expected grades 14–17 at the time of the shooting. Across all four of the educational outcomes shown in Figure 7—high school graduation, enrollment in any college, enrollment in a 4-year college, and receipt of a bachelor's degree—we find no evidence of significant impacts of placebo exposure. This provides support for the validity of our research design. At the same time, we observe significant adverse impacts of exposure to a shooting at school on long-run educational outcomes, especially when exposure occurs in grades 10 and 11. As shown in the plots on the right-hand side of each sub-figure, these impacts are robust to the inclusion of school fixed effects. Note that the lack of significant coefficients on exposure in grade 12 is consistent with the long-run effects operating through a deterioration in high school performance in earlier grades that is consequential for meeting high school graduation and college admission requirements.

[33] Since we have few clusters in some of these sub-group analyses, we calculate standard errors using a wild cluster bootstrap.

Table 2 presents results from estimation of equation (4) for each of our long-run educational outcomes. Averaging across the coefficients on exposure in grades 10 and 11, we find that experiencing a shooting at school leads to a 2.9 percentage point (3.7 percent relative to the pre-shooting mean) reduction in the likelihood of graduating high school by age 26. We also find that exposure to a shooting in grades 10–11 leads students to be 4.4 percentage points (9.5 percent) less likely to enroll in any college, 5.5 percentage points (17.2 percent) less likely to enroll in a 4-year college, and 3.1 percentage points (15.3 percent) less likely to receive a bachelor's degree by age 26.

Figure 8 and Table 3 present the analogous results for labor market outcomes measured at ages 24–26. Again, we see no evidence of statistically significant placebo effects for the "too old" cohorts, while exposure in grades 9–11 negatively affects economic well-being. Averaging across coefficients for exposure in grades 9–11, we find that shootings lead to a 4.4 percentage point (6.3 percent) reduction in the likelihood of employment and $2,779.84 (13.5 percent) lower annual earnings at ages 24–26 (estimated including zero earnings). While some of the reduction in annual earnings is driven by reductions in labor supply on the extensive margin, we also observe reductions on the intensive margin as measured by non-zero earnings (i.e., conditional on employment).[34]

One natural question given the effect patterns we find is how much of the earnings effect can be explained by the reduction in college attendance. We do not have an independent instrument to causally estimate the returns to education in our data, but, under the assumption of positive selection into college, the ordinary least squares (OLS) regression of earnings on college attendance provides an upper bound. We find that, controlling for gender and race/ethnicity indicators, attending a 4-year college is associated with a $14,281 increase in earnings at ages 24–26. Multiplying this estimate by the effect of exposure to shootings on 4-year college attendance (−5.1 percentage points for exposure in grades 9–11) suggests that at most a quarter ($721) of the overall earnings reduction can be explained by the impact on college attendance.

[34]Our results on short-run outcomes using the sample of eight shootings included in our long-run analysis are very similar to our baseline estimates (see Appendix Table A3 and Appendix Figure A4). If anything, the short-run effects are somewhat larger among the subset of eight shootings.

Potential differential attrition We code our long-run outcomes as zeros for those who have not obtained the indicated level of education or who do not have earnings in the Texas data. Since our data only contain information from the state of Texas, we necessarily assign zeros for students who leave Texas before an educational milestone (such as high school graduation) is reached or an employment outcome (such as employment at ages 24–26) is measured. If shooting-exposed students are differentially more or less likely to migrate out of Texas than their unexposed counterparts, then any resulting compositional changes would complicate the interpretation of our estimates of long-run effects. While we are unaware of any data that would allow us to quantify the effects of shootings at schools on migration out of Texas in the long run, three pieces of evidence suggest that differential migration is unlikely to meaningfully influence our results.

First, while Appendix Figure A2(c) indicates that students at shooting schools are slightly less likely to be observed in the two years following a shooting compared to control schools, the rate of leaving the Texas public school system is the same in "too old" cohorts at shooting schools (see Appendix Figure A2(d)). As this pattern suggests that there is no differential attrition from the public school system in response to a shooting in the short run (in line with the discussion in Section 3.2), it is plausible that there is also no differential migration out of Texas in the longer run.[35] Second, the estimated adverse effects of exposure to a shooting at school are observed across outcomes measured over different time horizons since the shooting—from high school graduation to labor market outcomes at ages 24–26. This consistency further suggests that differential mobility among shooting-exposed students is not driving the long-run results. Lastly, we obtain similar effects on earnings when we include

[35] If there were differential migration in the long run, we note that the sign of the resulting bias in the long-run analysis would be ambiguous. For instance, if exposure to a shooting at school makes students less likely to leave Texas in the long run because they are less likely to pursue out-of-state college or labor market opportunities, our analysis would underestimate the effects of shooting exposure on long-run outcomes such as college attendance, college completion, and labor force participation. If exposure to a shooting instead makes students more likely to move away from Texas and work out-of-state in the long run, then the bias would go in the opposite direction. To assess this potential bias, we estimate Lee (2009) bounds assuming differential attrition in response to a school shooting of 0.86 percentage point (the attrition gap between shooting and control schools shown in Figure A2(b)). The estimated bounds are presented in Appendix Tables A6 and A7. While these Lee bounds cover a range of estimates, the bounds exclude zero for most of the outcomes with significant estimates in our baseline specifications. We consider this bounding strategy to be a conservative assessment of potential bias in the long-run analysis due to the evidence of no differential attrition between exposed and "too old" cohorts (who are included in our baseline specifications as controls), and given that almost half of students leaving the Texas public school data in the short run are actually observed in the later labor market data.

or exclude individuals with no earnings in Texas. Thus, the estimated negative consequences of being exposed to a shooting at school extend to the subset of individuals who remain and work in Texas at some point between the ages of 24 and 26.

Heterogeneity analyses Since we only have eight shootings in our long-run analysis sample, we are unable to explore heterogeneity by shooting or school characteristics. We can, however, examine heterogeneity in long-run impacts by student gender, race/ethnicity, and receipt of free or reduced-price lunch. Appendix Figures A5 and A6 present these results for our long-run educational and labor market outcomes, respectively. As in our analysis of short-run outcomes, we find no evidence of significant differences in impacts across student characteristics. Instead, it appears that the adverse impacts of exposure to a shooting at school on students' long-run educational and economic outcomes are relatively universal.

4.3 Sensitivity Analysis

Our short-run analysis uses a balanced panel of students who are observed in the TEA data in each of the six years surrounding a shooting (three years before to two years after). In Appendix Figure A7, we explore the sensitivity of our estimates to using an unbalanced panel. In particular, we overlay our baseline event study estimates with results derived from a sample in which we do not make any restrictions on the number of years that students must be observed in the data. The results across the two samples are very similar, indicating that our main estimates are not sensitive to our balanced panel restriction.

We also test the robustness of our estimates to alternative ways of matching schools that experience shootings to control schools. Appendix Figure A8 presents coefficients and 95% confidence intervals from estimating equation (1) using samples of control schools selected from six alternative matching strategies. In particular, we make the following adjustments to the matching strategy: (1) we add average 8th grade standardized test scores for math and reading before the shooting to the set of fuzzy match variables;[36] (2) in addition to the variables in (1), we do an exact match on the 10 educational regions in Texas (that is, we only

[36]In particular, we include average scores among students who took the test as well as the share of students with non-missing 8th grade test scores. Since average 8th grade test scores among middle school students could be endogenous to the shooting, we only add these variables when matching high schools. If a student repeated 8th grade, we use the first observed test score.

pick control schools that are in relative geographic proximity to the schools that experience shootings);[37] and (3) in addition to the variables in (2), we add the share of students who are in gifted programs, have limited English proficiency, and are immigrants to the set of fuzzy match variables. Additionally, we use the same matching variables as in our baseline strategy but: (4) select four control schools instead of two, (5) match in reverse order, and (6) match using characteristics measured in the year before the shooting rather than the year of the shooting. For ease of comparison, we provide our baseline estimate at the top of each sub-figure. Reassuringly, our results are robust across all of these alternative matching strategies.

Finally, we further probe the statistical likelihood of our long-run effects using a permutation test. For each iteration, we begin by randomly selecting eight schools from the 679 high schools that did not experience a shooting and were observed over our entire sample period. We then run our matching procedure to identify two control schools for each of our eight placebo "treatment" schools. Randomly assigning the eight shooting dates observed in our sample to the eight placebo "treatment" schools, we then re-run our long-run empirical design (equation (4)), comparing changes in outcomes following a placebo event in the "treatment" schools to those experienced at the matched control schools. We repeat this analysis 1,000 times and compare the treatment effects presented in Tables 2 and 3 to the distributions of placebo estimates. As shown in Figures A11–A17, our effects are in the tails of these distributions and therefore retain statistical significance using this alternative method of inference.

4.4 Effects on School Staff

Shootings at schools have the potential to impact many aspects of the school environment, setting gun violence in schools apart from violence in other settings like students' communities or homes. Following a shooting on school grounds, a school might respond with efforts to mitigate the trauma by increasing the quantity and quality of instructional, support, or leadership services. At the same time, a shooting at school may adversely impact the staff themselves and lead to higher turnover rates or lower the quality of instruction and other ser-

[37] For more details about these educational regions, see: `http://www.txhighereddata.org/Reports/Performance/P16data/TxEdregionslist.pdf`.

vices provided. To shed light on these potential impacts, we study the effects of shootings at schools on the employment and retention of teachers, administrators, and non-teaching staff.

As introduced in Section 2.2, we begin this analysis by categorizing school staff into four groups: teachers, school leadership (principals and assistant principals), teaching support (e.g., educational aides), and social support (e.g., counselors and school psychologists).[38] We define employment and retention rates at the school–by–academic year level and estimate versions of equations (1) and (2) that use school fixed effects in place of individual fixed effects. We weight the school–by–academic year cells by total enrollment, and cluster standard errors at the school level.[39]

Results for effects on the total number of FTE staff per 1,000 students across employment groups are shown in Figure 9 and Panel A of Appendix Table A5. As shown in sub-figures (a), (c), and (d) of Figure 9, shootings at schools do not affect the aggregate employment of teachers, teaching support staff, or social support staff. However, sub-figure (b) suggests that the number of FTE leadership staff increases following a shooting.[40] This estimated increase is large: as shown in Panel A of Appendix Table A5, the number of school leadership staff per 1,000 students rises by 0.55 following a shooting ($p-$value=0.065), an effect of 18.8 percent relative to the baseline mean of 2.9. As assistant principals—who contribute much of the variation in the number of school leadership positions in the data—are often in charge of discipline, safety, and interventions for behavioral issues at schools, this increase could reflect schools' responses to the disruption caused by a shooting.

Results for effects on the retention of full-time employees across employment groups are shown in Figure 10 and Panel B of Appendix Table A5. In these analyses, we consider staff that were employed full-time at each of the shooting and control schools at the time of the shooting and examine how the probability of full-time employment at the same school evolves before and

[38] Appendix Table A4 presents descriptive statistics for the school staff groups and the individual staff types included in each group.

[39] Total enrollment is measured in the first six-week grading period of the academic year of the shooting. Because our annual staff data capture employment as of a snapshot date in October, we restrict our sample to shootings that took place in or after November in a given academic year and treat the year of the shooting as a pre-shooting period in this analysis. We also restrict our sample to match groups in which all three schools are consistently observed from three years before to two years after the shooting. Our final staff analysis sample includes 24 school shootings.

[40] In order to make effect sizes more comparable across the four groups of staff, the y-axes in these figures are scaled to range from -50 percent to $+50$ percent of the pre-period mean of each outcome. Raw data trends for FTE staff by employment group are shown in Appendix Figure A9.

after the shooting.[41] As shown in sub-figures (a) and (c) of Figure 10, we find that shootings at schools lead to a reduction in the probability of retention for teachers and teaching support staff. The effect sizes are meaningful: Panel B of Appendix Table A5 shows that the retention rates of teachers and teaching support staff decline by 0.4 percentage points ($p-$value=0.028) and 2.0 percentage points ($p-$value=0.008), respectively, reflecting reductions of 5.6 percent and 32.9 percent relative to the respective baseline means. Since we do not find a change in the number of FTE staff per 1,000 students for these employment groups, we interpret the reduction in retention as evidence of increased turnover at shooting-exposed schools. That is, if teachers and teaching support staff are more likely to leave schools that experience shootings, but aggregate employment remains constant, then exiting staff members must be replaced with new staff. While new staff have less school-specific experience by definition, we do not find evidence of a change in the composition of teachers and teaching support staff in terms of gender, race/ethnicity, or educational background.

5 Discussion

The magnitudes of our estimates suggest that the costs of shootings at schools—even those that have few or no deaths—are large. In addition to effects on short-run educational outcomes like absenteeism, we find that shootings have lasting implications for the human capital and economic trajectories of exposed students. We conduct a back-of-the-envelope calculation based on our estimates of the effects of shooting exposure in grades 9–11 on annual earnings at ages 24–26 (Table 2). Assuming that the average effect of exposure persists through age 64, our estimates imply a reduction of $115,550 (in 2018 dollars) in the present discounted value of lifetime earnings per shooting-exposed student.[42] Given that more than 50,000 American

[41]In this analysis, teachers who are included in multiple match groups (1.2 percent of all teachers in the sample) are excluded. We drop match groups in which either a shooting school or both control schools had no full-time employees in a given staff group at the time of the shooting. Raw data trends for retention rates by employment group are shown in Appendix Figure A10.

[42]To calculate the present discounted value (PDV) of lifetime earnings, we discount the stream of earnings from ages 15–64 in the 2019 March Current Population Survey (CPS) back to age 15 (i.e., around the start of high school), assuming that earnings are discounted at a 3 percent real rate (i.e., a 5 percent discount rate with 2 percent wage growth). This calculation yields a total PDV of $888,844. We then multiply this number by the average percent effect of exposure to a shooting in grades 9–11 on annual earnings (13 percent). This yields $115,550. The CPS data are downloaded from the Integrated Public Use Microdata Series (IPUMS) (Flood et al., 2020).

students experienced a school shooting annually in recent years (see footnote 2), the aggregate present discounted value of the cost of school shootings is more than $5.8 billion annually.

It is helpful to compare our effects to those found in recent work on exposure to other types of violence. Using administrative data on elementary school students in a Florida county, Carrell et al. (2018) find that exposure to an additional classroom peer who experiences domestic violence at home leads to a 3 percent reduction in earnings at ages 24–28. Our estimated 13.5 percent reduction in earnings is thus equivalent to the impact of having approximately 4.5 violence-exposed peers. Our larger effect size is consistent with the possibility that the harm of a student's own trauma from experiencing gun violence at school may be amplified by the peer effects of other shooting-exposed peers. Moreover, relative to violence that occurs in children's homes, shootings that take place at schools are more likely to influence other educational inputs—such as the increases in teacher turnover that we document—that have been shown to impact student outcomes in other settings (see, e.g., Ronfeldt et al., 2013; Hanushek et al., 2016; Atteberry et al., 2017).

Our findings can also be compared to Ang (2020)'s estimates of the impacts of local exposure to police violence. Using data from Los Angeles, he finds that students who live within 0.5 miles of a police killing have a 3.5 percent lower likelihood of high school graduation and a 2.5 percent lower likelihood of college enrollment relative to students who live 0.5–3 miles away. These effects are concentrated among students who are exposed in grades 10 and 11. We find 3.7 and 9.5 percent reductions in high school graduation and college enrollment, respectively, which are also driven by 10th and 11th grade exposure. Our relatively larger magnitudes are again consistent with the idea that shootings at schools are more disruptive to educational inputs than violence that takes place elsewhere. Moreover, it is possible that the estimates reported in Ang (2020) represent lower bounds, as the control group of students living slightly further away from a police killing might also be impacted.

We can further compare our effects to those reported in Bharadwaj et al. (2022)'s study on the impacts of exposure to the 2011 mass shooting in Utøya, Norway. They find that survivors of the mass shooting are 12 percent less likely to complete college and have 12 percent lower earnings than a matched control group. Our estimated 15.3 percent decrease in the likelihood of receiving a bachelor's degree and 13.5 percent reduction in earnings are quite similar.

While the shooting incidents that we study are much less deadly than the 2011 Norway massacre, we note that our setting differs in ways that could generate relatively large adverse effects for shooting-exposed students in Texas. As Bharadwaj et al. (2022) point out, the Norwegian government provided substantial resources and support to the survivors of the Utøya attack, which likely buffered against some of the detrimental long-term effects. In comparison, we are not aware of governmental responses to the school shootings that we study, and we do not see any changes in observable mental health resources to support students (e.g., as measured by the number of social support staff) at the school level. Moreover, unprecedented tragedies such as the Utøya attack can impact "control group" youth who were not directly involved, potentially diminishing the estimated net effects. In contrast, the events that we study were not widely covered by media outlets and thus were unlikely to affect control students in other districts. Lastly, given that gun violence is much more common in the United States than in Norway, one might hypothesize that we should expect smaller effects in our context if children are "desensitized" to such violence. At the same time, shootings remain rare and uncommon events for any given student; moreover, the relatively higher rate of highly publicized mass shootings in the United States can make the occurrence of a less deadly shooting more traumatic for a student if it triggers fears rooted in past publicized tragedies (Lowe and Galea, 2017).

Lastly, our estimates can be put in context of the broader literature on the long-run impacts of educational inputs on adult earnings. Chetty et al. (2011) find that a one standard deviation increase in "class quality" (a measure that includes teachers, peers, and any class-level shocks) for one year among students in kindergarten through 3rd grade leads to a 9.6 percent increase in earnings at age 27. Furthermore, Chetty et al. (2014) estimate that a one standard deviation increase in teacher quality for one year among students in grades 4–8 results in a 1.3 percent increase in earnings at age 28. Our estimated 13.5 percent reduction in earnings at ages 24–26 is thus equivalent to a 1.4 standard deviation decrease in class quality for one year or a one standard deviation reduction in teacher quality for ten years. Given that our long-run estimates capture the effects of exposure to a shooting in high school, our findings suggest that adverse shocks at older grade levels can offset large advantages in educational inputs in younger grades.

6 Conclusion

Mass shootings receive significant media attention and incite vigorous policy debates about how such tragedies can be prevented. At the same time, these high-profile events account for a very small fraction of all gun deaths in the United States (Gramlich, 2019). If policymakers want to curb the costliest gun violence in terms of the number of lives lost, one might argue that they should focus their attention on "everyday" gun violence occurring in people's homes, communities, and schools.[43] Furthermore, decades of research on exposure to trauma suggests that the costs of gun violence extend beyond the death toll. Hundreds of thousands of American children have been exposed to a shooting at their school and have survived, and these shootings vary substantially in their circumstances, number of injuries, and number of deaths. Quantifying the causal effects of shootings at schools on students' short- and long-run outcomes is critical both for targeting resources to help mitigate potential harms and for informing policy discussions that compare the costs of different types of gun violence.

This paper draws on rich administrative data from Texas to investigate the impacts of shootings at schools on students' educational and economic outcomes through age 26. We study the universe of shootings that occurred on school grounds during school hours at Texas public schools between 1995 and 2016 and examine within-student and across-cohort changes in outcomes relative to changes at matched schools. We find that exposure to a shooting at school leads to higher rates of absenteeism and grade repetition in the following two years. We also document adverse long-run impacts of exposure to a shooting at school, with reductions in the likelihood of high school graduation, college enrollment, and college graduation, as well as a decreased likelihood of employment and lower earnings at ages 24–26. Our estimates imply that a shooting at school reduces the present discounted value of lifetime earnings of each exposed student by $115,550 (in 2018 dollars). Given that more than 50,000 students experienced a school shooting per year in 2018 and 2019, the aggregate present discounted value of the cost of school shootings is more than $5.8 billion annually. Heterogeneity analyses indicate that the detrimental effects of exposure to a shooting at school on students' educational and economic trajectories are broad and reach across nearly all analyzed sub-groups.

[43]For an example of such an argument, see, e.g.: `https://www.vox.com/2015/10/1/18000524/mass-shootings-rare`.

The fact that we find large, adverse impacts of exposure to shootings on students' long-term outcomes indicates that current interventions and resources devoted to helping survivors of school shootings are not sufficient to counteract the negative effects. Future research is needed to identify effective interventions that can help mitigate the lasting consequences of exposure to gun violence in schools. Moreover, our results increase the urgency to identify and adopt policies, such as stricter regulation surrounding gun ownership, that can prevent these tragic events from occurring.

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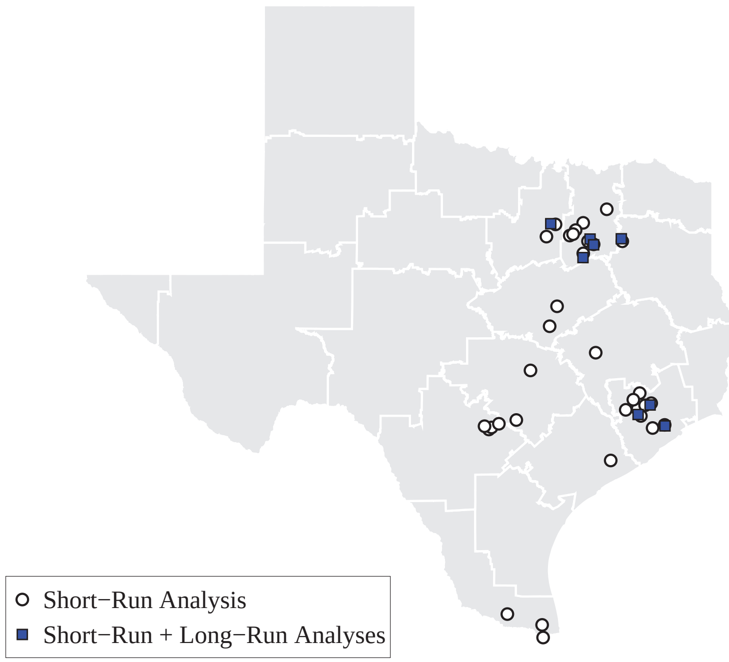
7 Figures and Tables

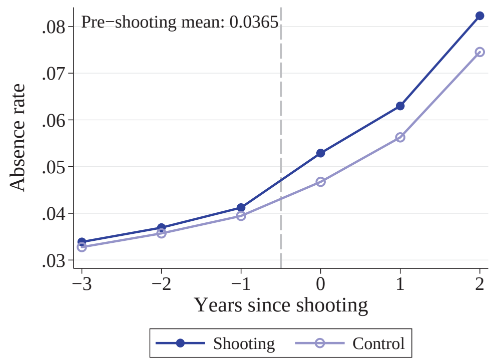
Figure 1: Map of Shootings at Texas Public Schools: Academic Years 1995–1996 to 2015–2016



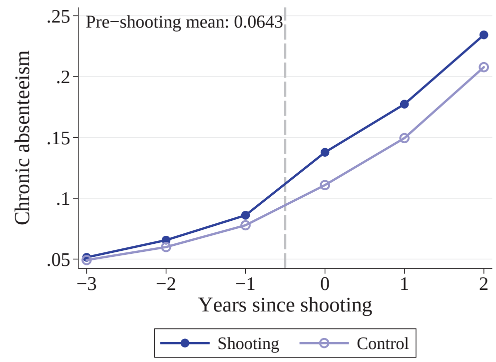
Notes: This figure shows the locations of the 33 (8) shootings at Texas public schools used in our short-run (long-run) analysis. These shootings occurred during school hours and on school grounds between the academic years 1995–1996 and 2015–2016. The data are compiled from the Center for Homeland Defense and Security K-12 school shooting database and the *Washington Post* school shootings database.

Figure 2: Raw Trends in Short-Run Outcomes Across Shooting and Control Schools

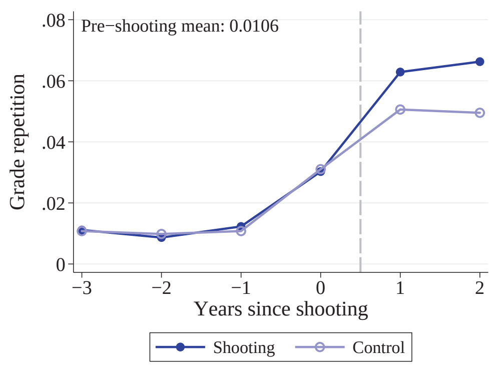
(a) Absence Rate



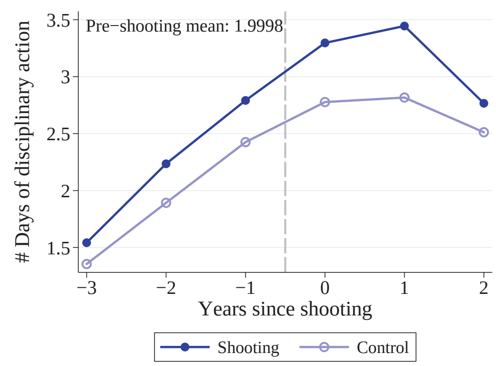
(b) Chronic Absenteeism



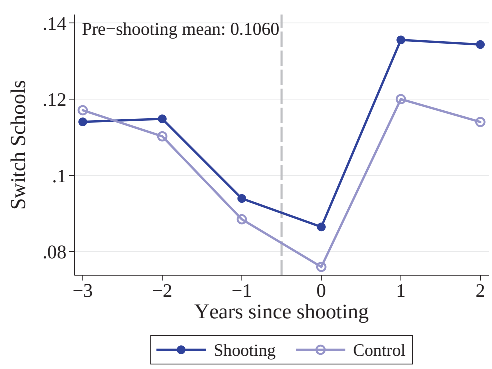
(c) Grade Repetition



(d) Days of Disciplinary Action



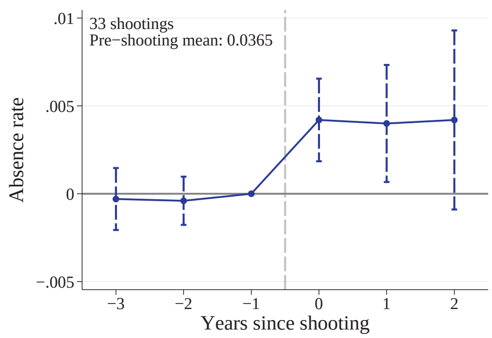
(e) Switch Schools



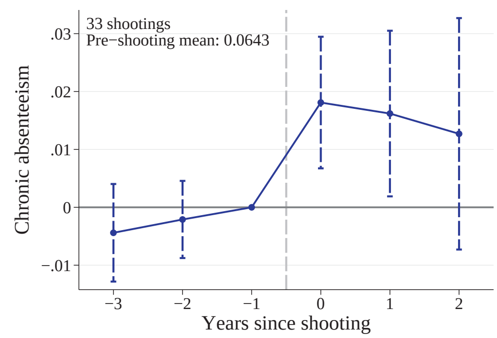
Notes: These figures plot raw trends in our short-run outcomes over the six years surrounding a school shooting, separately for treatment and matched control schools. Sub-figures (a)–(c) and (e) include 33 shooting and 66 control schools; since data on disciplinary actions is not available for our entire sample period, sub-figure (e) includes a subset of 26 shooting and 52 control schools. We restrict the sample to students who are observed in the data over the period of three years before to two years after a shooting (i.e., the panel is balanced). See Appendix Figure A7 for results using an unbalanced panel.

Figure 3: Short-Run Effects of Shootings at Schools on Educational Outcomes

(a) Absence Rate

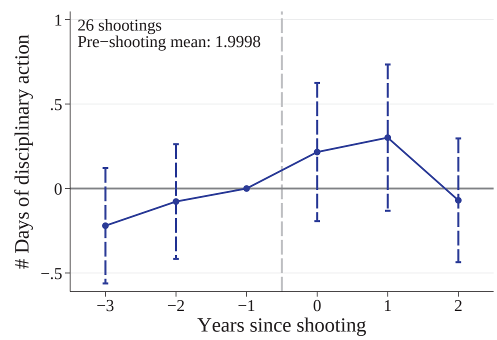


(b) Chronic Absenteeism



(c) Grade Repetition

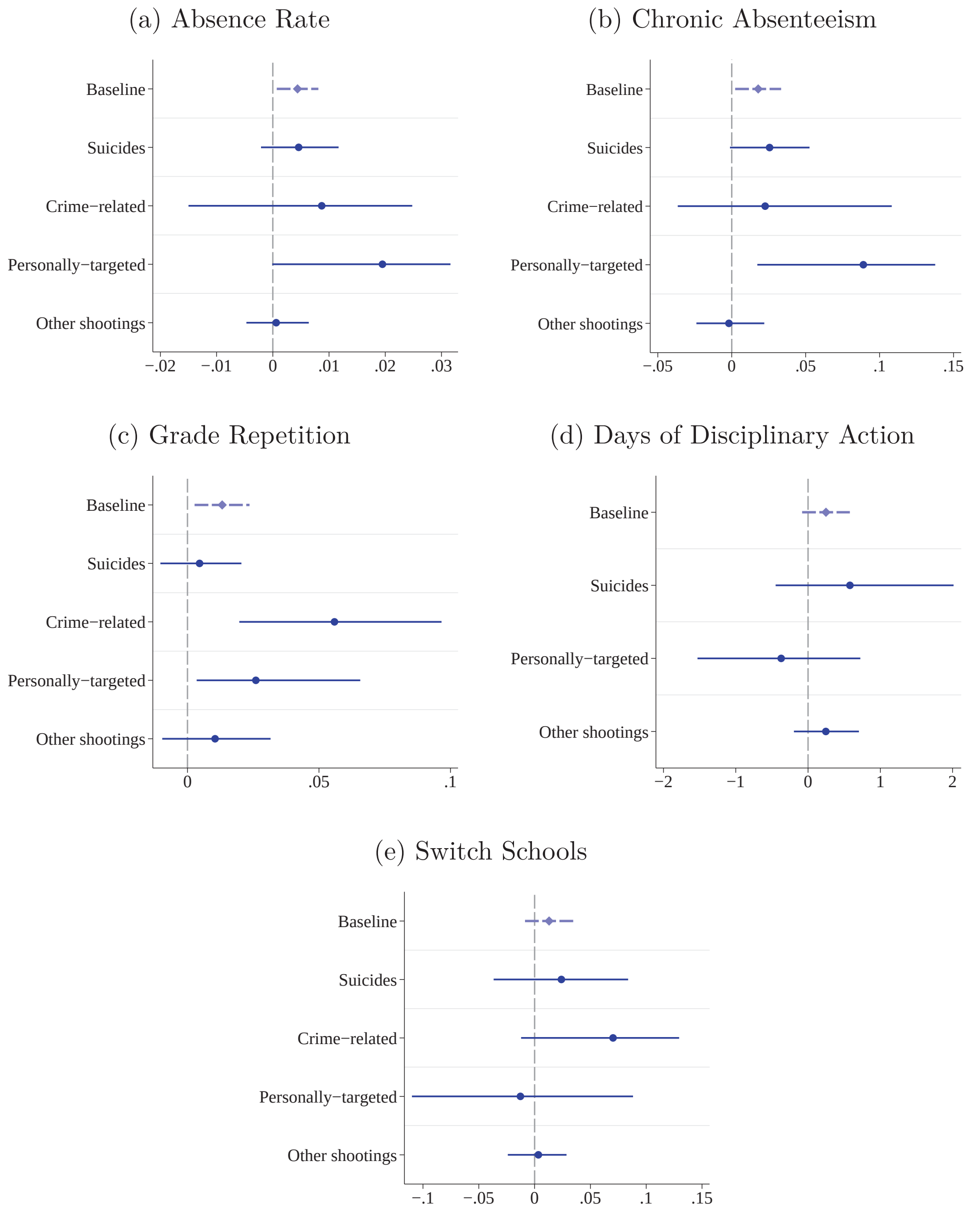
(d) Days of Disciplinary Action



(e) Switch Schools

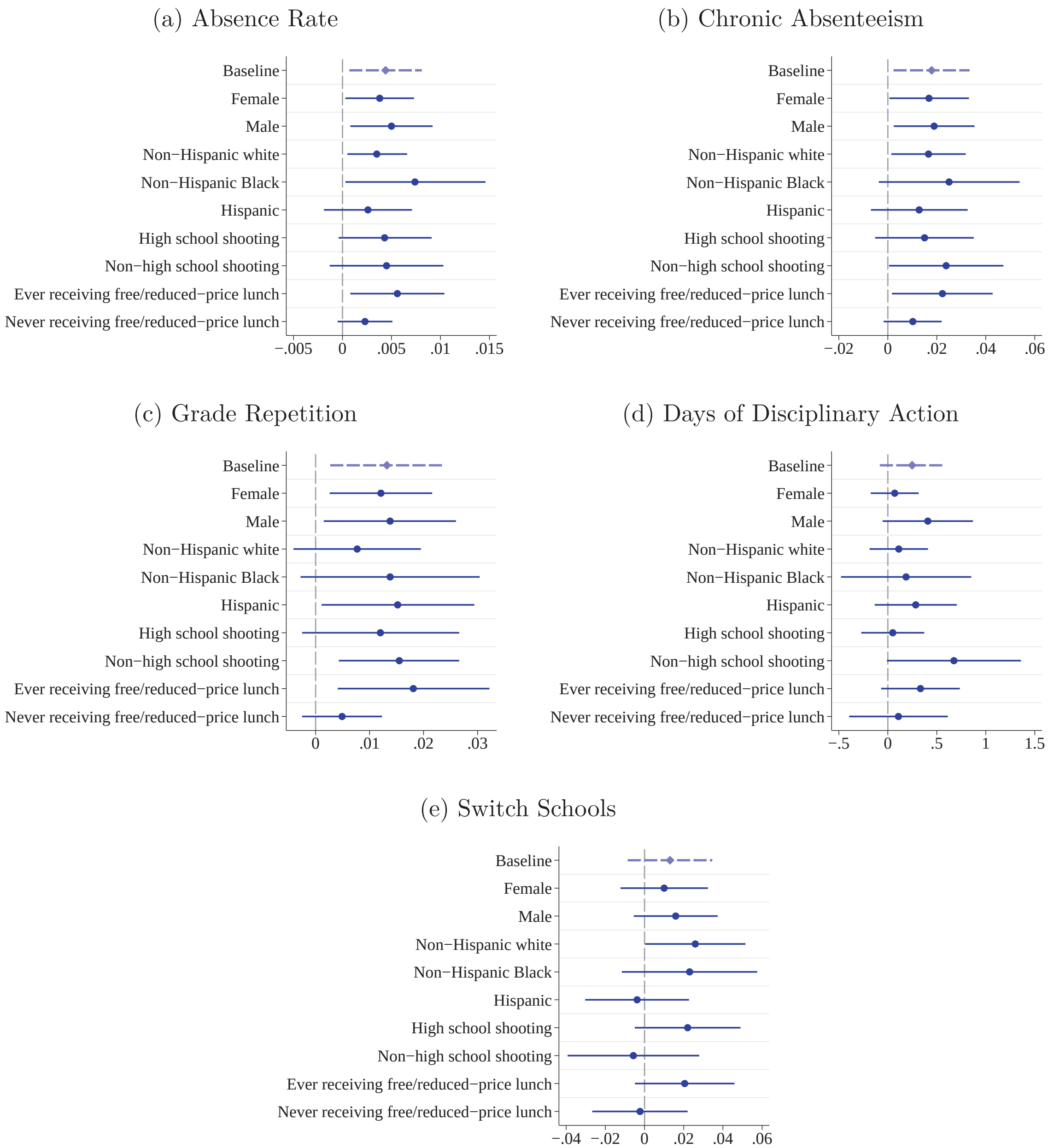
Notes: These figures present output from estimation of equation (2). In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the indicators denoting each of the years before and after a shooting. The academic year before the shooting is the omitted category. The regressions include individual and match group–by–year fixed effects. Standard errors are clustered by school.

Figure 4: Short-Run Effects on Educational Outcomes: Heterogeneity by Shooting Type



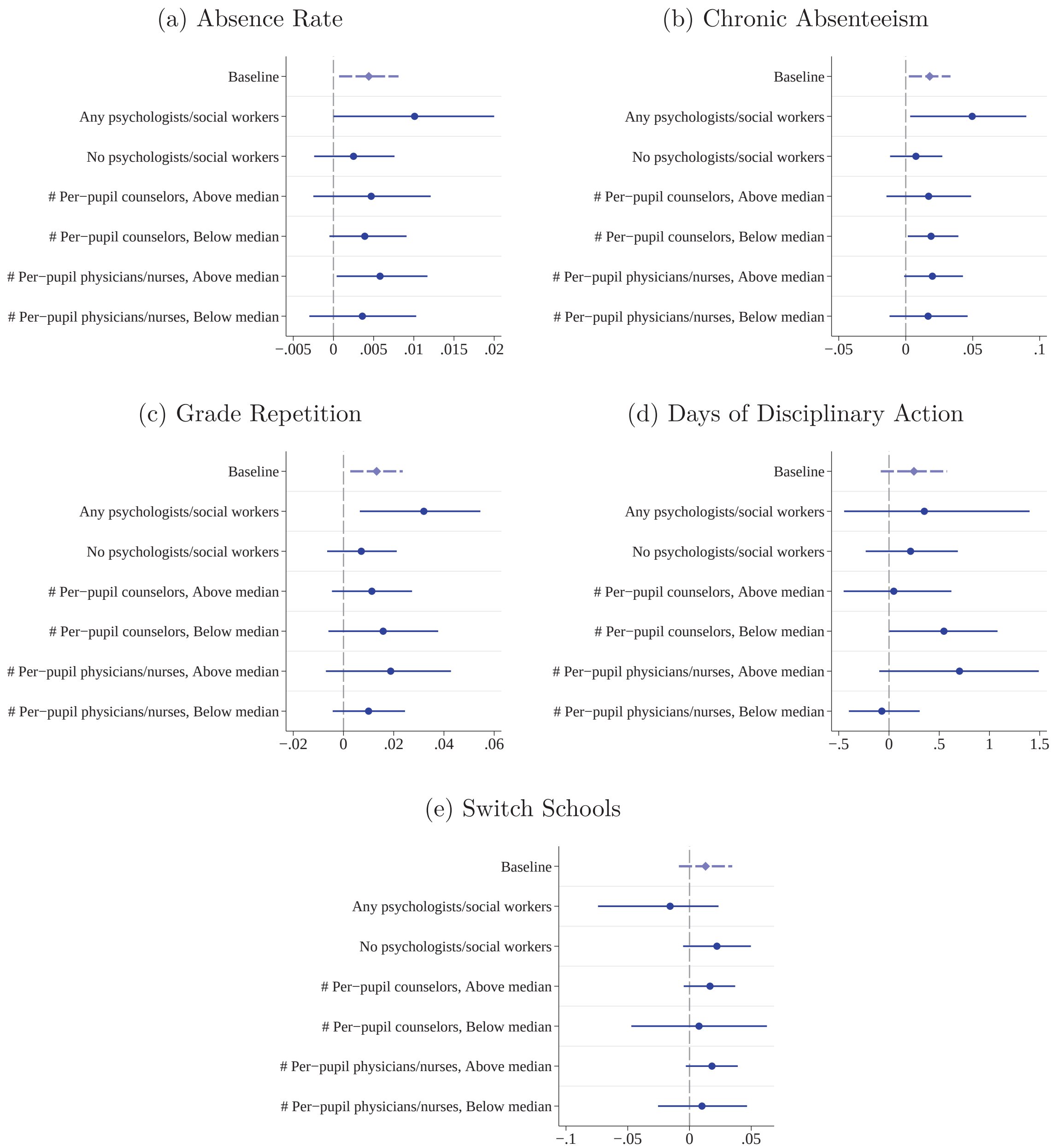
Notes: These figures present output from estimation of equation (1) for the shooting type denoted on the y-axis. In particular, we plot the coefficients and 95% confidence intervals on the interaction between the indicator denoting shooting schools and the post indicator. Confidence intervals are based on a wild cluster bootstrap for standard errors clustered at the school level; they are not centered around the coefficient estimates because the bootstrap method does not assume normality. The shooting categories follow those suggested by Levine and McKnight (2020b) and are mutually exclusive. Our baseline estimates—which use the entire sample of 33 shootings—are presented at the top of each sub-figure. The baseline estimate presented at the top of sub-figure (d) uses a subset of 26 shootings covering the time period for which data on disciplinary actions is available (1998 onward); since only one shooting among this subset was crime-related, we do not present an estimate for crime-related shootings in sub-figure (d). The regressions include individual and match group–by–year fixed effects.

Figure 5: Short-Run Effects on Educational Outcomes: Heterogeneity by Student Characteristics



Notes: These figures present output from estimation of equation (1) for students belonging to the sub-group denoted on the y-axis. In particular, we plot the coefficients and 95% confidence intervals on the interaction between the indicator denoting shooting schools and the post indicator. We drop schools in which there are fewer than 10 students in a particular sub-group and only use match groups that contain three schools (one shooting and two control schools). Our baseline estimates—which use the entire sample of students—are presented at the top of each sub-figure. The regressions include individual and match group–by–year fixed effects. Standard errors are clustered by school.

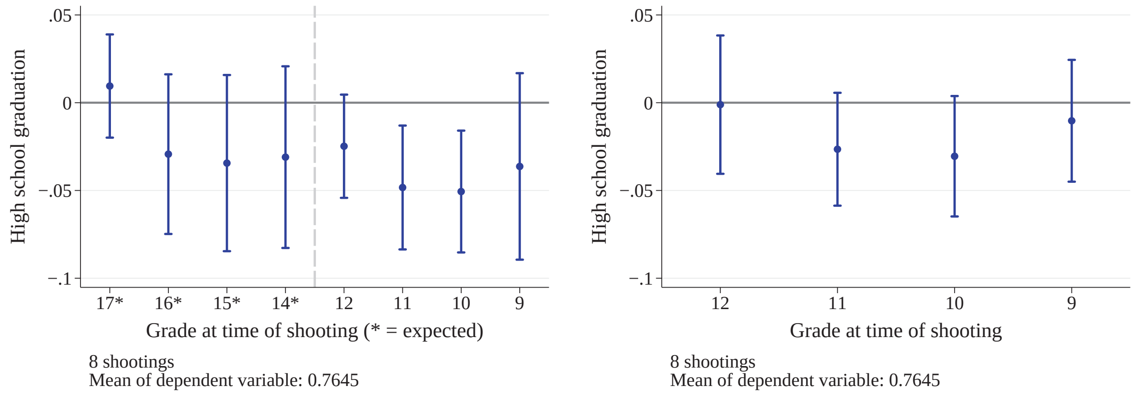
Figure 6: Short-Run Effects on Educational Outcomes: Heterogeneity by School Mental Health Resources



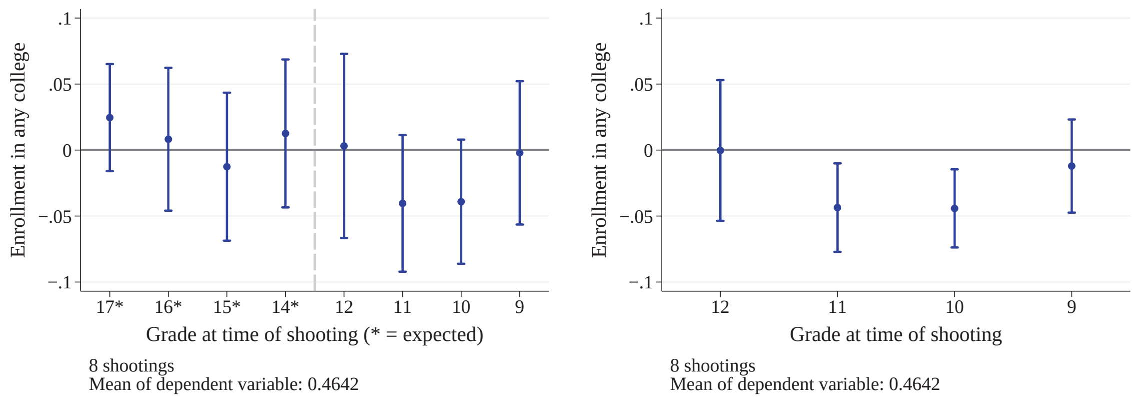
Notes: These figures present output from estimation of equation (1) for shootings at schools with differing availability of health professionals. In particular, we plot the coefficients and 95% confidence intervals on the interaction between the indicator denoting shooting schools and the post indicator. Confidence intervals are based on a wild cluster bootstrap for standard errors clustered at the school level; they are not centered around the coefficient estimates because the bootstrap method does not assume normality. Our baseline estimates—which use the entire sample of schools—are presented at the top of each sub-figure. The regressions include individual and match group–by–year fixed effects.

Figure 7: Long-Run Effects of Shootings at Schools on Educational Outcomes by Age 26

(a) High School Graduation



(b) Enrollment in Any College



(c) Enrollment in a 4-Year College

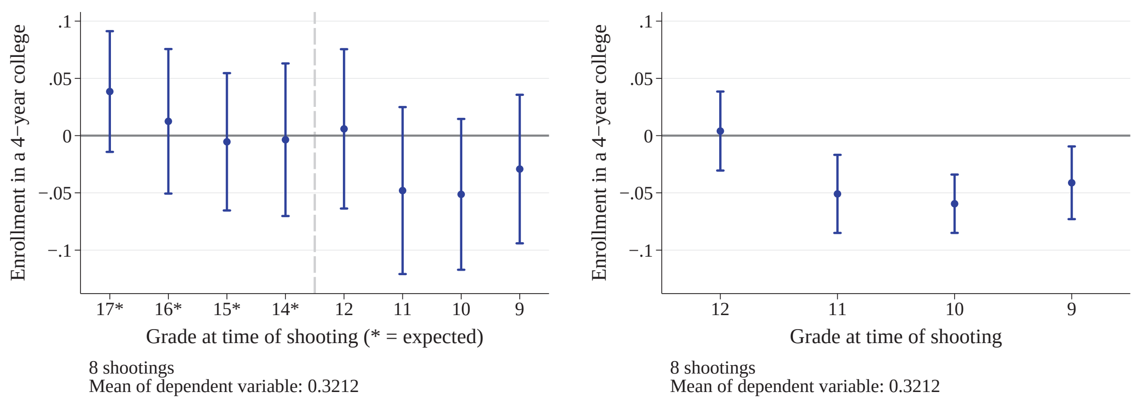


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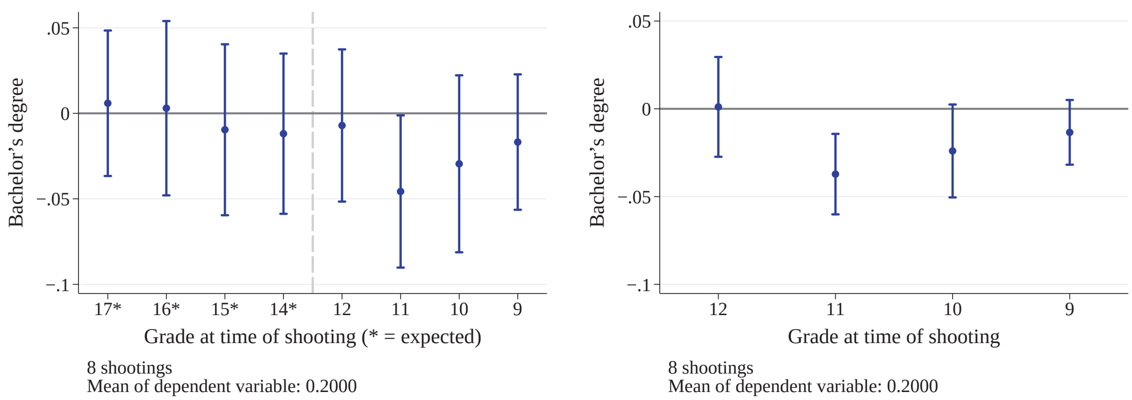
Figure 7: Long-Run Effects of Shootings at Schools on Educational Outcomes (continued)

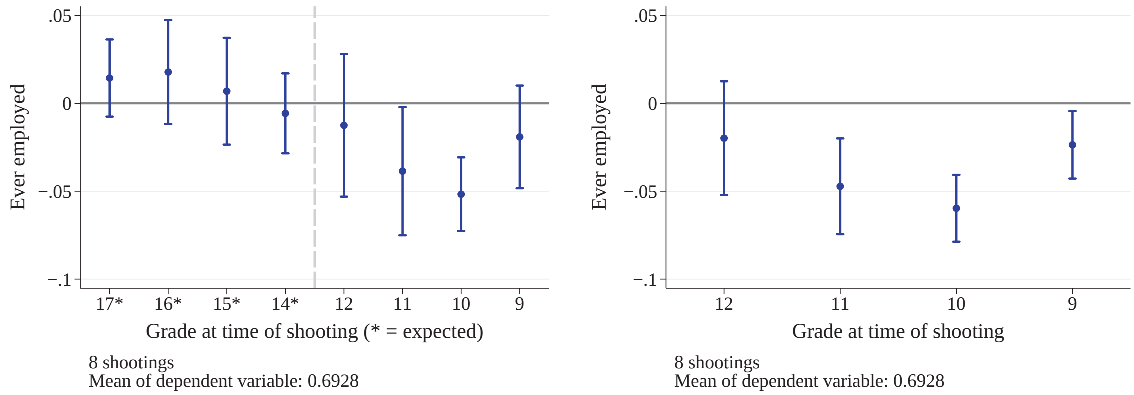
(d) Bachelor's Degree



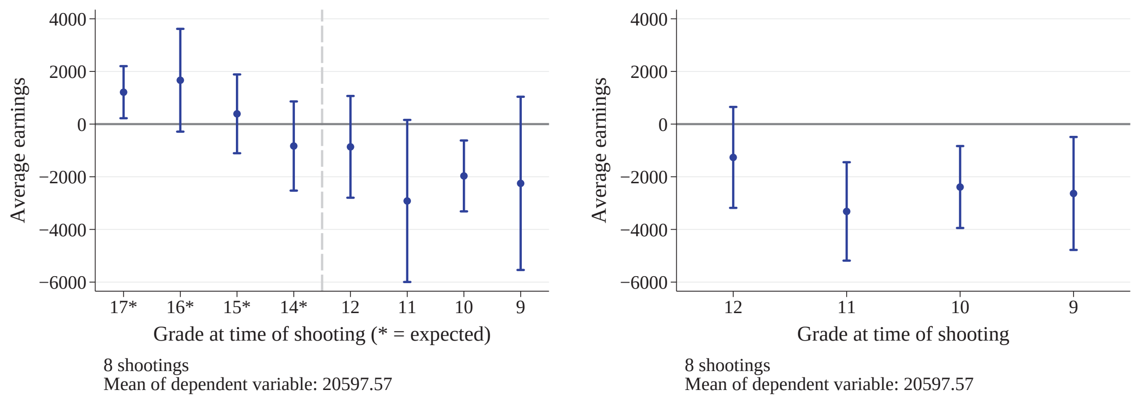
Notes: In each sub-figure, the graph on the left-hand side presents output from estimation of equation (3), while the graph on the right-hand side presents output from estimation of equation (4). In both cases, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the set of cohort indicators. Both specifications control for match group–by–cohort fixed effects and a vector of individual-level controls for student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. Equation (4) additionally includes school fixed effects. Standard errors are clustered at the school-by-cohort level.

Figure 8: Long-Run Effects of Shootings at Schools on Labor Market Outcomes at Ages 24–26

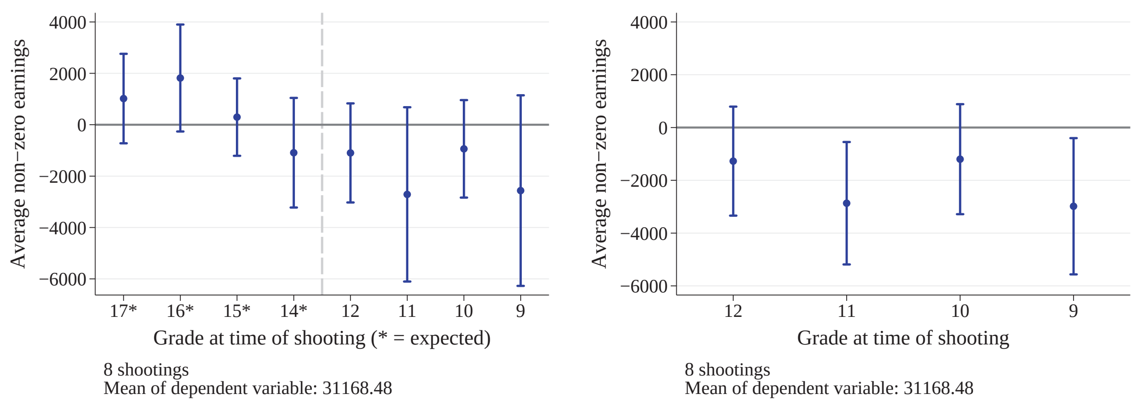
(a) Employed



(b) Average Earnings

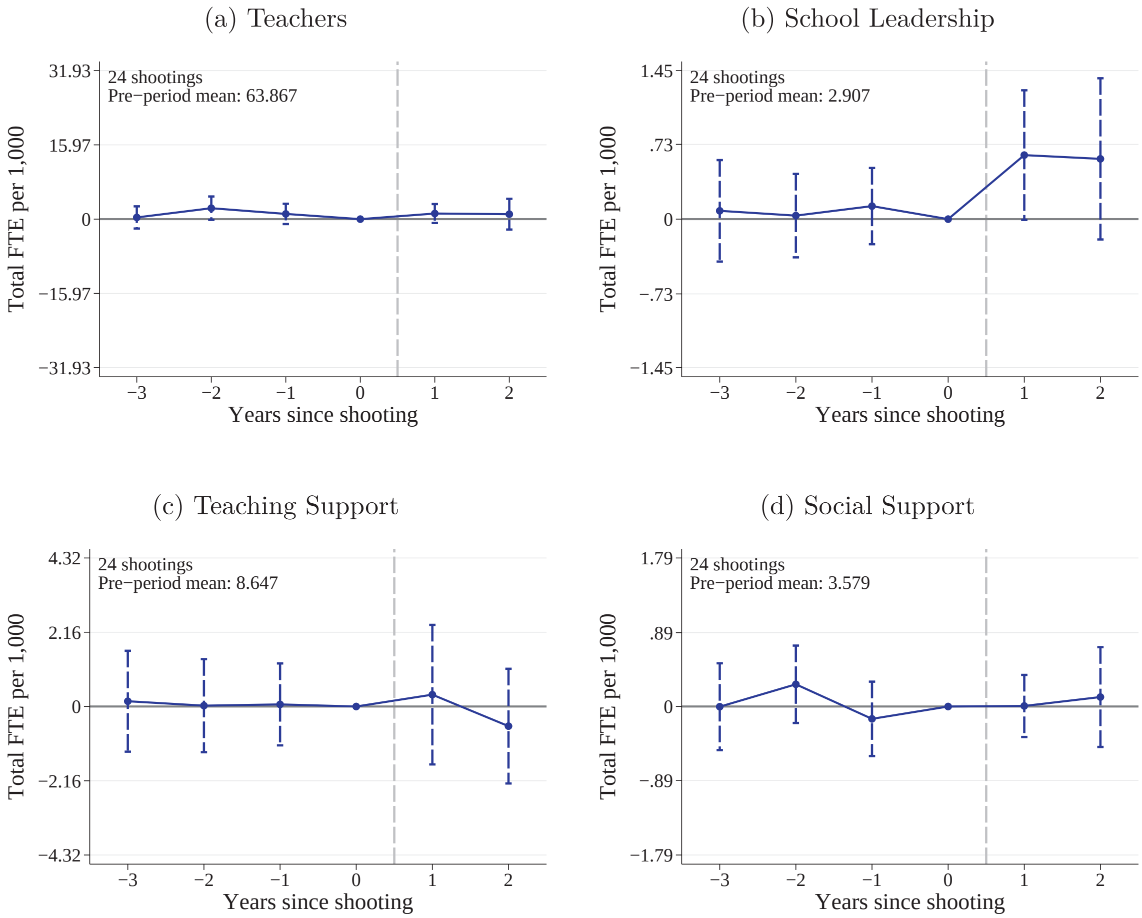


(c) Average Non-Zero Earnings



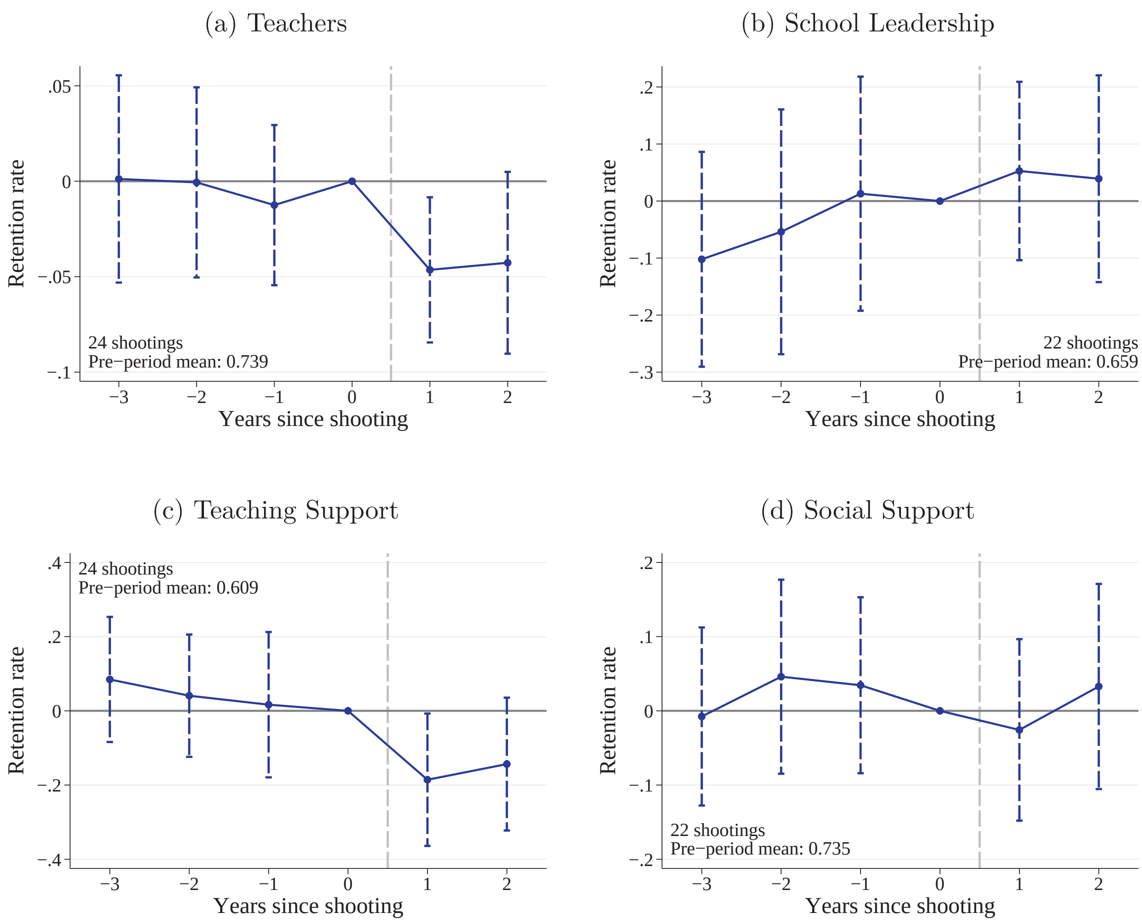
Notes: In each sub-figure, the graph on the left-hand side presents output from estimation of equation (3), while the graph on the right-hand side presents output from estimation of equation (4). In both cases, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the set of cohort indicators. Both specifications control for match group–by–cohort fixed effects and a vector of individual-level controls for student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. Equation (4) additionally includes school fixed effects. Standard errors are clustered at the school-by-cohort level.

Figure 9: Effects of Shootings at Schools on School-level Employment



Notes: These figures present output from estimation described in Section 4.4. In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the indicators denoting each of the years before and after a shooting. The academic year of the shooting is the omitted category. The regressions include school and match group–by–year fixed effects. School-by-academic-year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting. Standard errors are clustered by school. In order to make effect sizes more comparable across the four groups of staff, the y-axes in these figures are scaled to range from −50 percent to +50 percent of the pre-period mean of each outcome.

Figure 10: Effects of Shootings at Schools on Retention of Full-Time Employees



Notes: These figures present output from estimation described in Section 4.4. In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the indicators denoting each of the years before and after a shooting. The academic year of the shooting is the omitted category. We focus on the staff that were employed full-time at each of the shooting and control schools in our staff analysis sample at the time of the shooting, and analyze changes in the probability of full-time employment at the same school both before and after the shooting. Sub-figure (a) includes 24 and 48 control schools; since we drop match groups in which either a shooting school or both control schools had no full-time employees in a given staff group at the time of the shooting, sub-figures (b) and (d) include less than 24 match groups. The regressions include school and match group–by–year fixed effects. School-by-academic-year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting. Standard errors are clustered by school.

Table 1: Short-Run Effects of Shootings at Schools on Educational Outcomes

	Absence Rate (1)	Chronic Absenteeism (2)	Grade Repetition (3)	Days of Disc. Act. (4)	Switch Schools (5)
Shooting School x Post	0.0044	0.0179	0.0132	0.2482	0.0130
	(0.0019)	(0.0079)	(0.0053)	(0.1685)	(0.0110)
	[0.022]	[0.027]	[0.016]	[0.145]	[0.241]
Pre-period outcome mean	0.0365	0.0643	0.0106	1.9998	0.1060
Student-year observations	373,368	373,368	373,368	277,176	371,285
R-squared	0.553	0.481	0.233	0.426	0.276

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (1). The regressions include individual and match group–by–academic year fixed effects. Standard errors are clustered by school. Since grade repetition reflects academic performance in the previous academic year, we exclude the year of the shooting from the post period when analyzing this outcome.

Table 2: Long-Run Effects of Shootings at Schools on Educational Outcomes by Age 26

	Graduate HS (1)	Enroll Any Col (2)	Enroll 4yr Col (3)	Bachelor's Degree (4)
Shooting School x Cohort 12	-0.0011	-0.0003	0.0040	0.0011
	(0.0201)	(0.0272)	(0.0176)	(0.0145)
	[0.956]	[0.990]	[0.821]	[0.939]
Shooting School x Cohort 11	-0.0265	-0.0436	-0.0509	-0.0372
	(0.0164)	(0.0171)	(0.0174)	(0.0117)
	[0.108]	[0.012]	[0.004]	[0.002]
Shooting School x Cohort 10	-0.0305	-0.0442	-0.0595	-0.0240
	(0.0175)	(0.0151)	(0.0130)	(0.0135)
	[0.083]	[0.004]	[<0.001]	[0.077]
Shooting School x Cohort 9	-0.0103	-0.0121	-0.0412	-0.0134
	(0.0177)	(0.0180)	(0.0162)	(0.0094)
	[0.560]	[0.504]	[0.012]	[0.159]
Outcome mean	0.7645	0.4642	0.3212	0.2000
Student observations	59,146	53,927	53,927	53,927
R-squared	0.119	0.065	0.081	0.082

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (4). The regressions include match group–by–cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level.

Table 3: Long-Run Effects of Shootings at Schools on Labor Market Outcomes at Ages 24–26

	Employed	Earnings	Non-Zero Earnings
	(1)	(2)	(3)
Shooting School x Cohort 12	-0.0198	-1,265.02	-1,273.02
	(0.0165)	(977.91)	(1,053.43)
	[0.230]	[0.198]	[0.229]
Shooting School x Cohort 11	-0.0472	-3,316.19	-2,867.87
	(0.0139)	(953.31)	(1,183.25)
	[0.001]	[0.001]	[0.017]
Shooting School x Cohort 10	-0.0597	-2,389.54	-1,199.10
	(0.0097)	(793.63)	(1,063.11)
	[<0.001]	[0.003]	[0.261]
Shooting School x Cohort 9	-0.0236	-2,633.79	-2,982.86
	(0.0098)	(1,094.13)	(1,316.82)
	[0.017]	[0.017]	[0.025]
Outcome mean	0.6928	20,597.57	31,168.48
Student observations	53,927	53,927	37,363
R-squared	0.015	0.014	0.021

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (4). The regressions include match group–by–cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level.

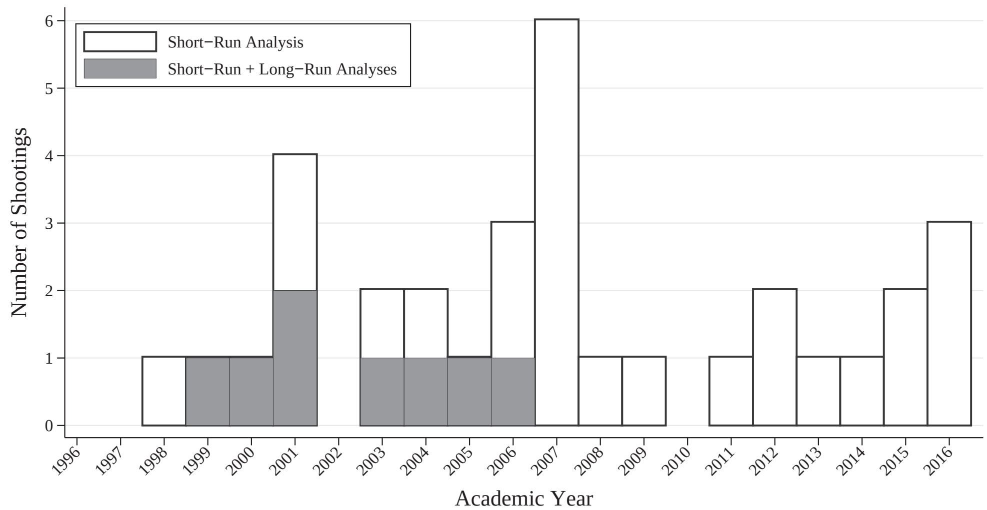
For Online Publication

Trauma at School: The Impacts of Shootings on Students' Human Capital and Economic Outcomes

Cabral, Kim, Rossin-Slater, Schnell, Schwandt (2022)

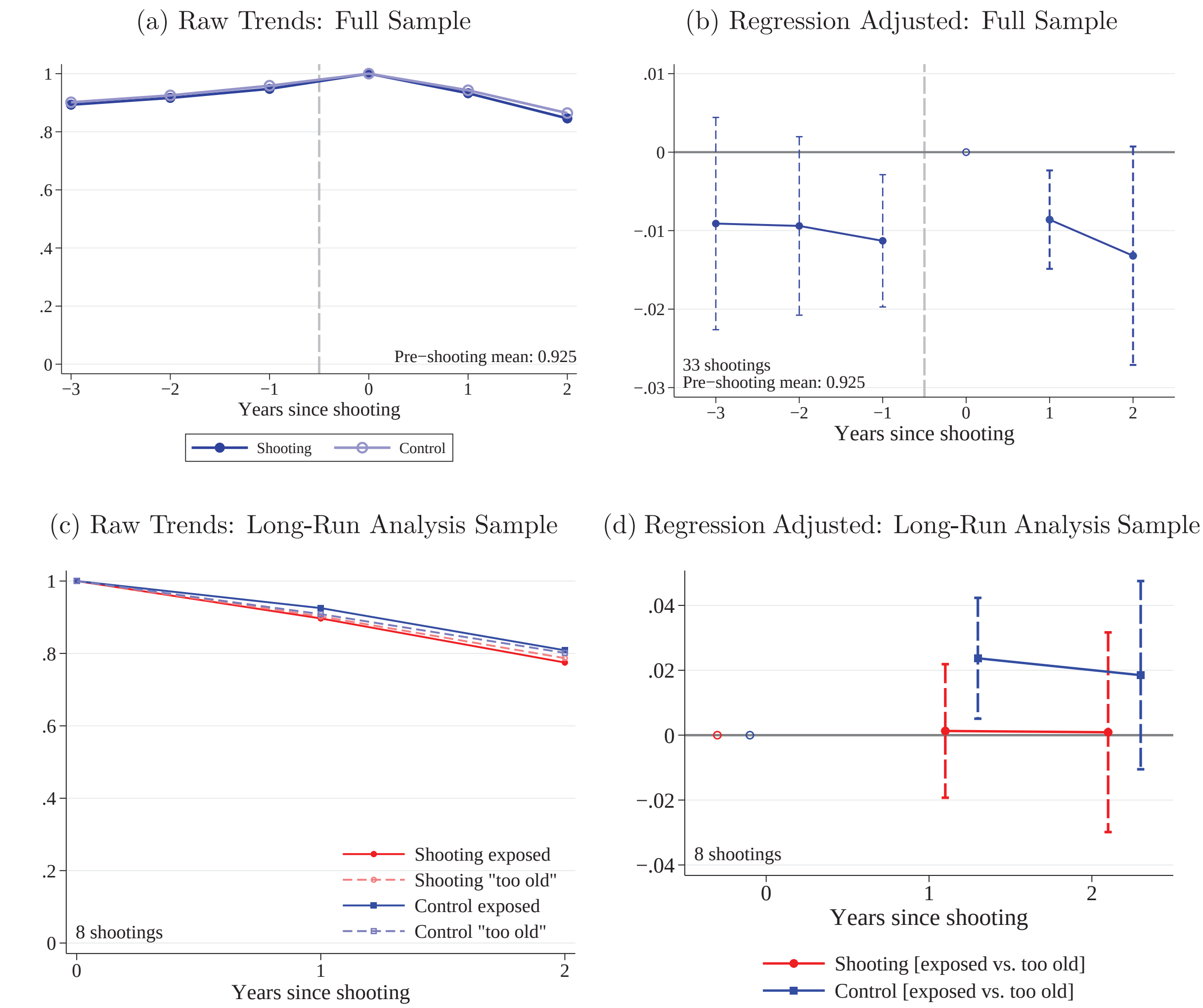
A Appendix Figures

Figure A1: Annual Number of Shootings at Texas Public Schools: Academic Years 1995–1996 to 2015–2016



Notes: This figure shows the distribution of the 33 (8) shootings at Texas public schools used in our short-run (long-run) analysis across the academic years 1995–1996 and 2015–2016. The data are compiled from the Center for Homeland Defense and Security K-12 school shooting database and the *Washington Post* school shootings database.

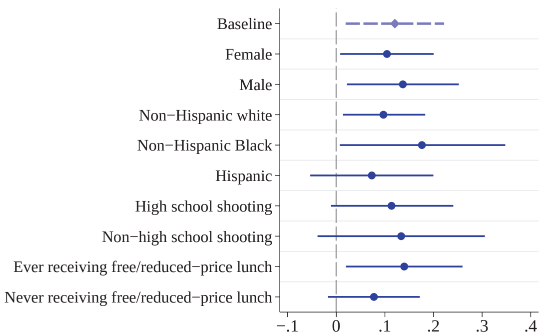
Figure A2: Trends in Sample Attrition Rates Across Treatment and Control Schools



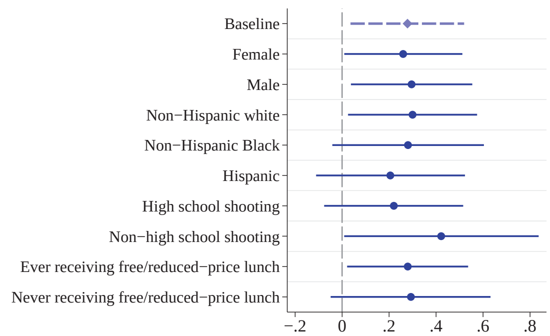
Notes: Sub-figures (a) and (b) consider all grades 3–10 students enrolled in the 33 shooting and 66 control schools in the academic semester of a shooting (denoted by time 0 on the $x-$axis). Sub-figure (a) plots the share of these students who are observed in the TEA data in the years surrounding the shooting, separately for students at shooting and control schools. Sub-figure (b) presents output from estimation of equation (2) using this sample, where the outcome is an indicator for being observed in the TEA data. We plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting a shooting school and the indicators denoting each of the years before and after a shooting. The academic year of the shooting is the omitted category. Standard errors are clustered by school. Sub-figure (c) considers all grades 9–10 exposed ("too old") cohorts enrolled in the eight shooting and 16 control schools included in our long-run analysis sample in the academic semester of a shooting (five years before a shooting), denoted by time 0 on the $x-$axis. Sub-figure (c) plots the share of these students who are observed in the TEA data in the years following time 0, separately for four groups—exposed cohorts in shooting schools, "too old" cohorts in shooting schools, exposed cohorts in control schools, and "too old" cohorts in control schools. Using this sample, sub-figure (d) presents output from estimation of a version of equation (2) that controls for (i) the interactions between the indicator denoting exposed cohorts in a shooting school and the indicators denoting years relative to time 0 (indicated by the red line), (ii) the interactions between the indicator denoting exposed cohorts in a control school and the indicators denoting years relative to time 0 (indicated by the blue line), (iii) individual fixed effects, (iv) academic year fixed effects, and (v) a full set of school-by-relative time fixed effects, where the outcome is an indicator for being observed in the TEA data. The red and blue lines present the coefficients and 95% confidence intervals on the interaction terms. Time 0 is the omitted category. Standard errors are clustered at the school-by-cohort level.

Figure A3: Short-Run Effects on Educational Outcomes: Heterogeneity by Student Characteristics (Effects Normalized Relative to Sub-Group Mean)

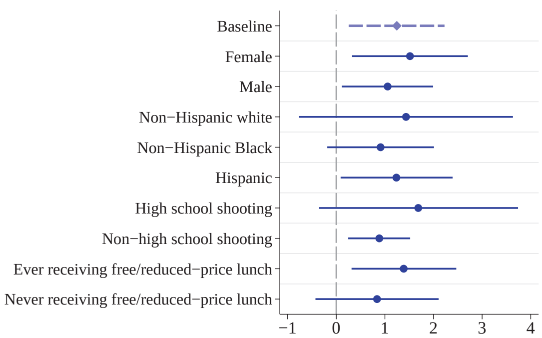
(a) Absence Rate



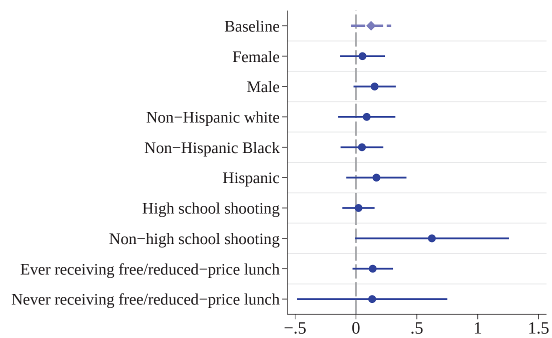
(b) Chronic Absenteeism



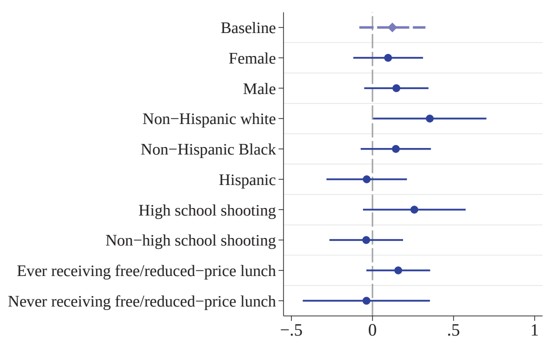
(c) Grade Repetition



(d) Days of Disciplinary Action



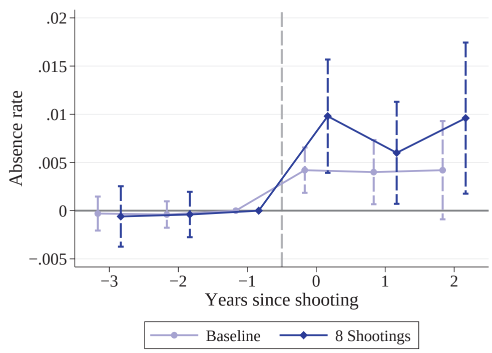
(e) Switch Schools



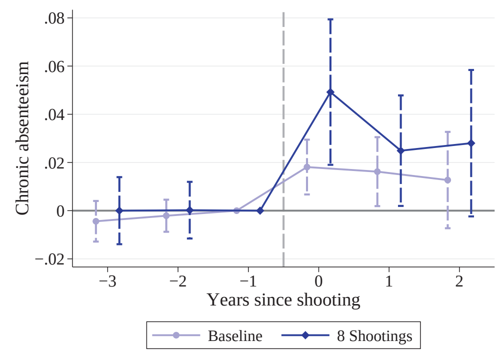
Notes: These figures present output from estimation of equation (1) for students belonging to the sub-group denoted on the y-axis. In particular, we plot the coefficients and 95% confidence intervals on the interaction between the indicator denoting shooting schools and the post indicator; coefficient estimates are scaled relative to the baseline outcome mean for each sub-group. We drop schools in which there are fewer than 10 students in a particular sub-group and only use match groups that contain three schools (one shooting and two control schools). Our baseline estimates—which use the entire sample of students—are presented at the top of each sub-figure. The regressions include individual and match group–by–year fixed effects. Standard errors are clustered by school.

Figure A4: Short-Run Effects on Educational Outcomes: Long-Run Versus Short-Run Analysis Sample

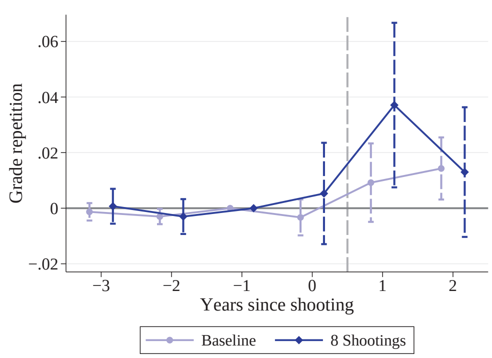
(a) Absence Rate



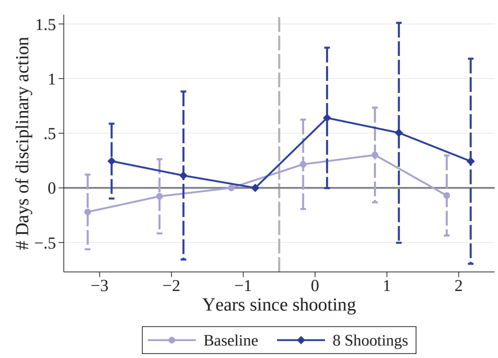
(b) Chronic Absenteeism



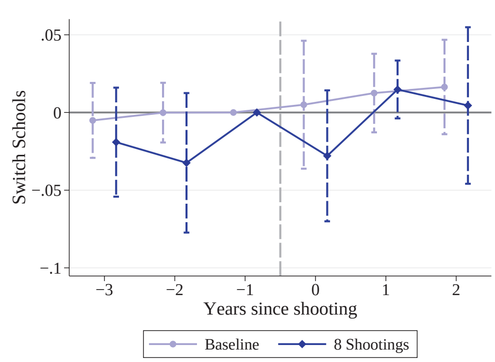
(c) Grade Repetition



(d) Days of Disciplinary Action



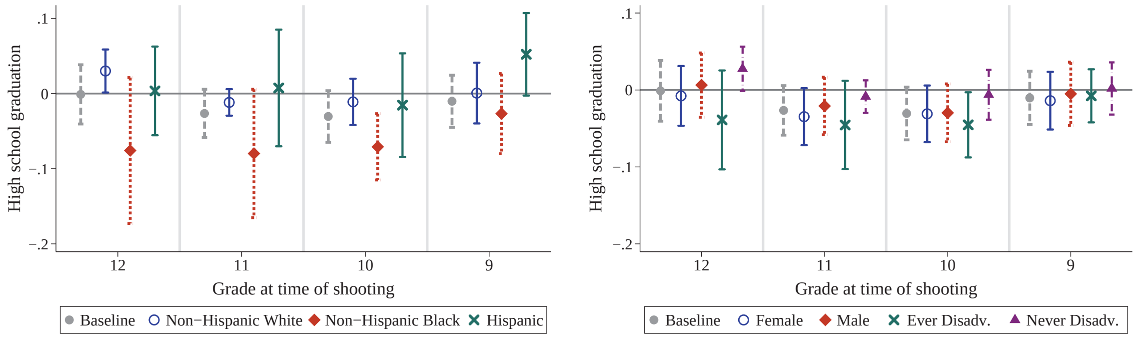
(e) Switch Schools



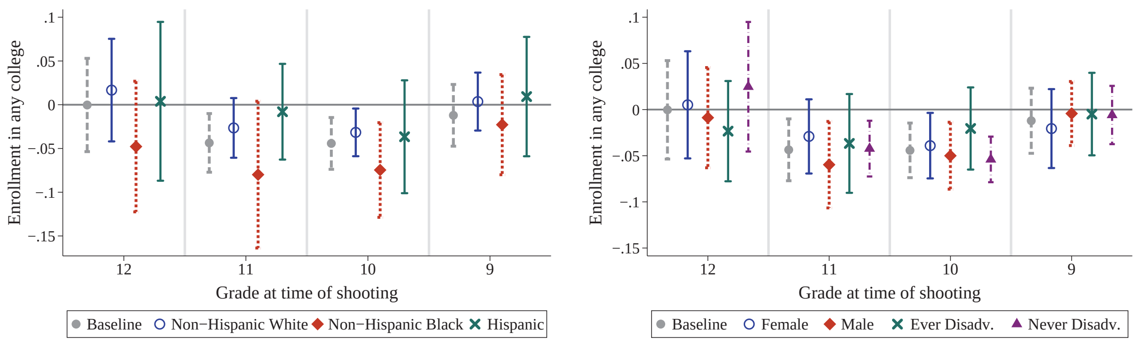
Notes: These figures present output from estimation of equation (2) using the 33 (8) shootings in our short-run (long-run) analysis sample. In both cases, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting a shooting school and the indicators denoting each of the years before and after a shooting. The academic year before the shooting is the omitted category. The regressions include individual and match group–by–year fixed effects. Standard errors are clustered by school.

Figure A5: Long-Run Effects on Educational Outcomes by Age 26: Heterogeneity by Student Characteristics

(a) High School Graduation



(b) Enrollment in Any College



(c) Enrollment in a 4-Year College

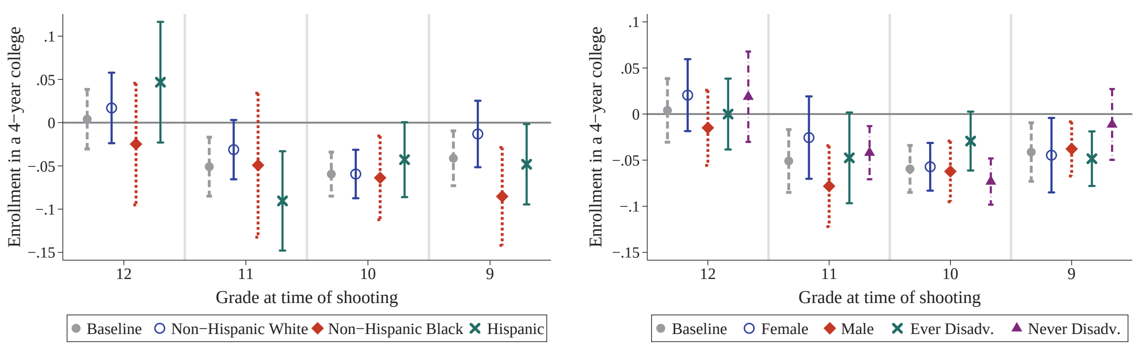


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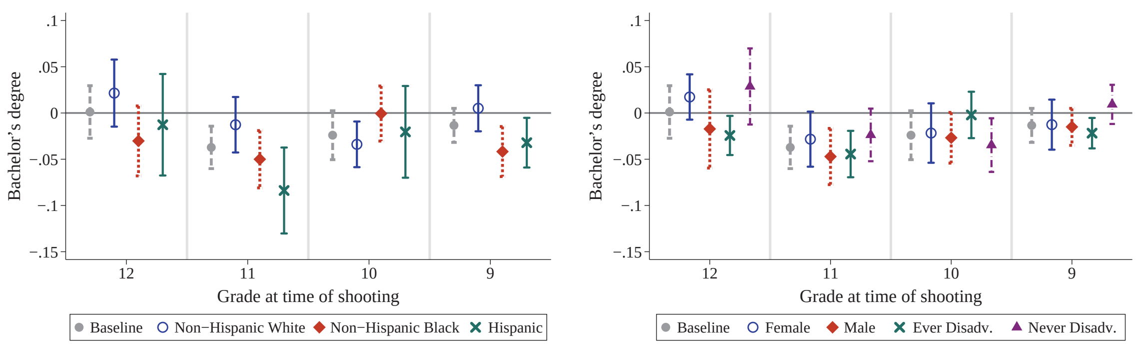
Figure A5: Long-Run Effects on Educational Outcomes by Age 26: Heterogeneity by Student Characteristics (continued)

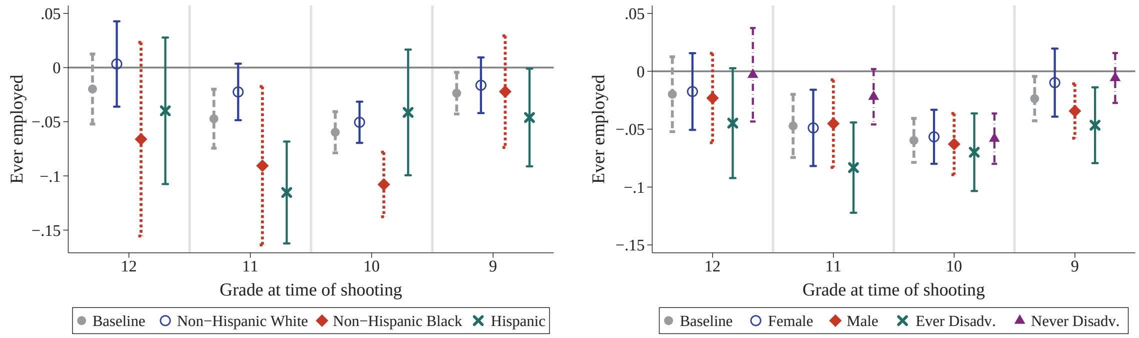
(d) Bachelor's Degree



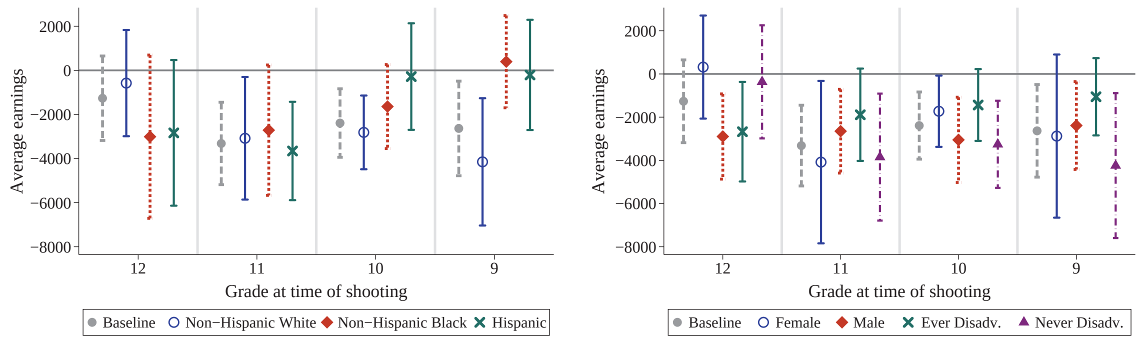
Notes: These figures present output from estimation of equation (4) for students belonging to the sub-group denoted on the y-axis. In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the set of cohort indicators. We drop schools in which there are fewer than 10 students in a particular sub-group and only use match groups that contain three schools (one shooting and two control schools). Our baseline estimates—which use the entire sample of students—are presented at the left of each sub-figure. "Ever (Never) disadvantaged" refers to students who ever (never) received free or reduced-price lunch in our data. The specification includes match group–by–cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level.

Figure A6: Long-Run Effects on Labor Market Outcomes at Ages 24–26: Heterogeneity by Student Characteristics

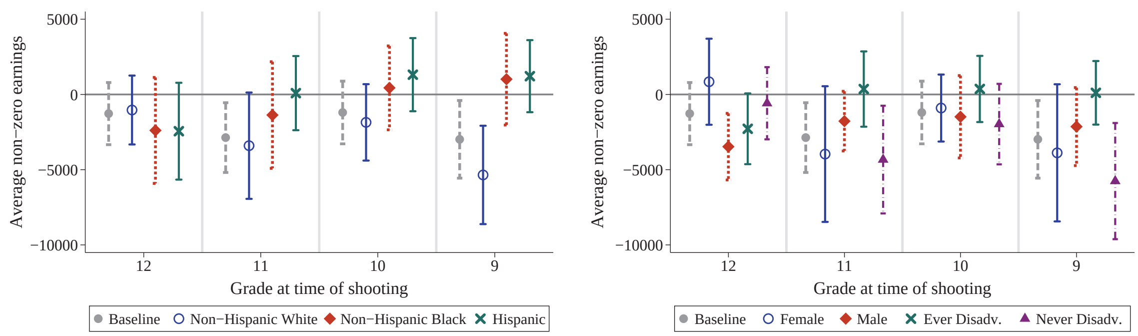
(a) Employed



(b) Average Earnings



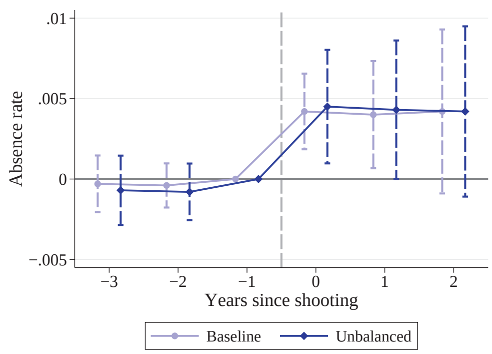
(c) Average Non-Zero Earnings



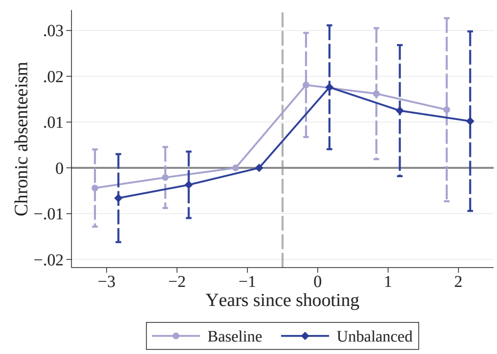
Notes: These figures present output from estimation of equation (4) for students belonging to the sub-group denoted on the y-axis. In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the set of cohort indicators. We drop schools in which there are fewer than 10 students in a particular sub-group and only use match groups that contain three schools (one shooting and two control schools). Our baseline estimates—which use the entire sample of students—are presented at the left of each sub-figure. "Ever (Never) disadvantaged" refers to students who ever (never) received free or reduced-price lunch in our data. The specification includes match group–by–cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level.

Figure A7: Short-Run Effects on Educational Outcomes: Balanced Versus Unbalanced Panels

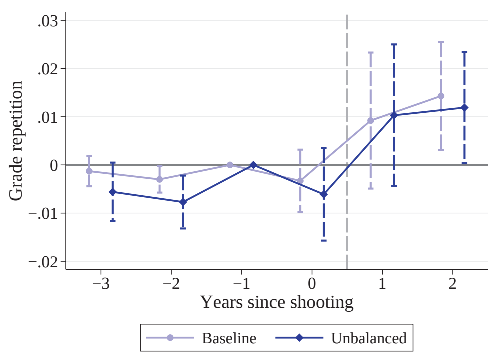
(a) Absence Rate



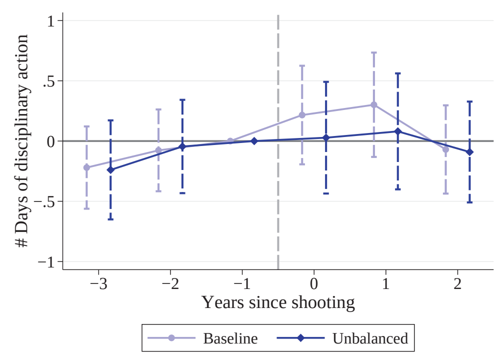
(b) Chronic Absenteeism



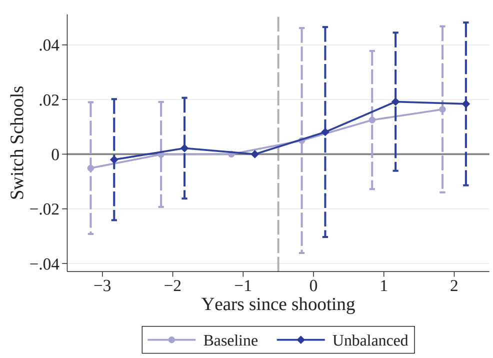
(c) Grade Repetition



(d) Days of Disciplinary Action

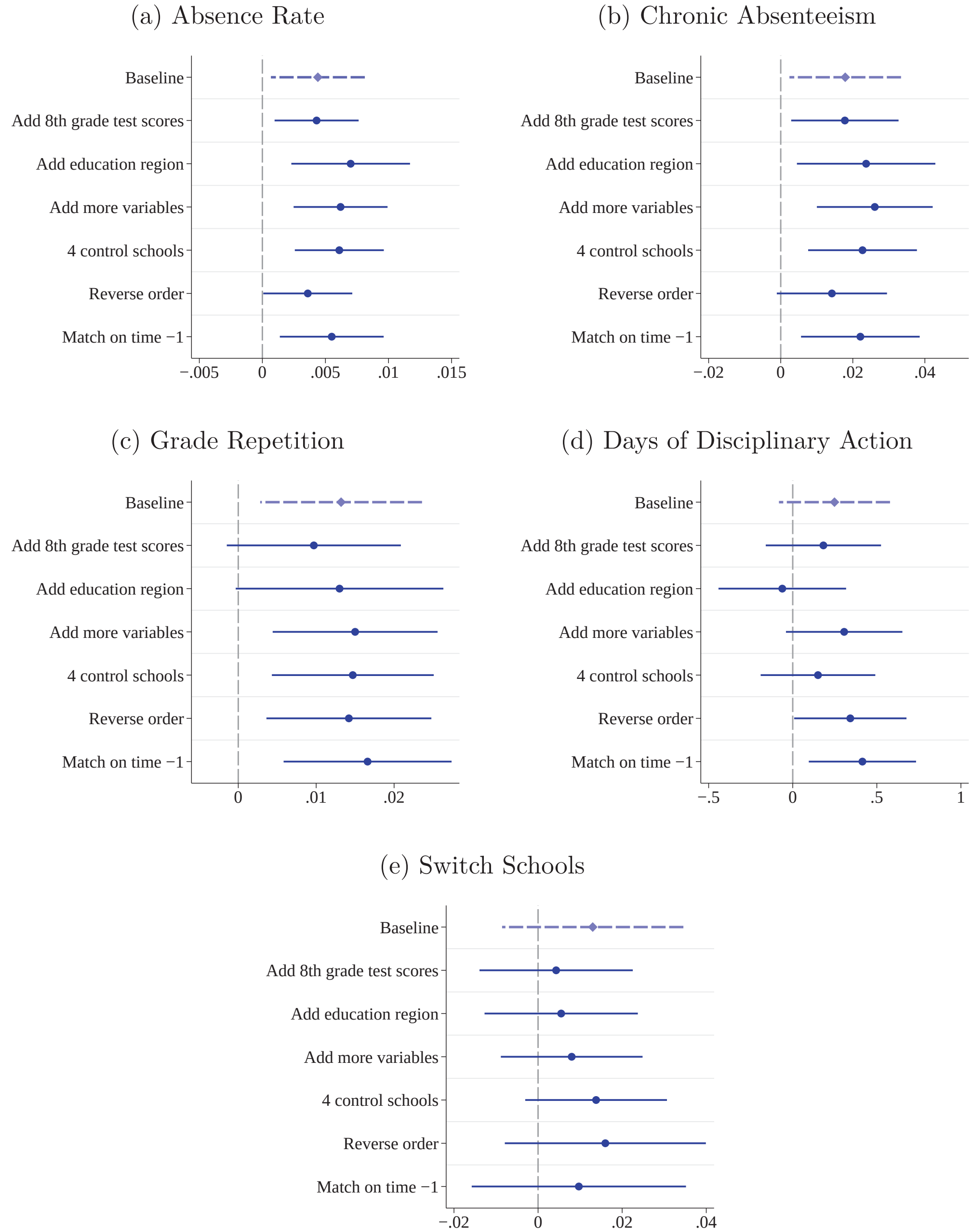


(e) Switch Schools



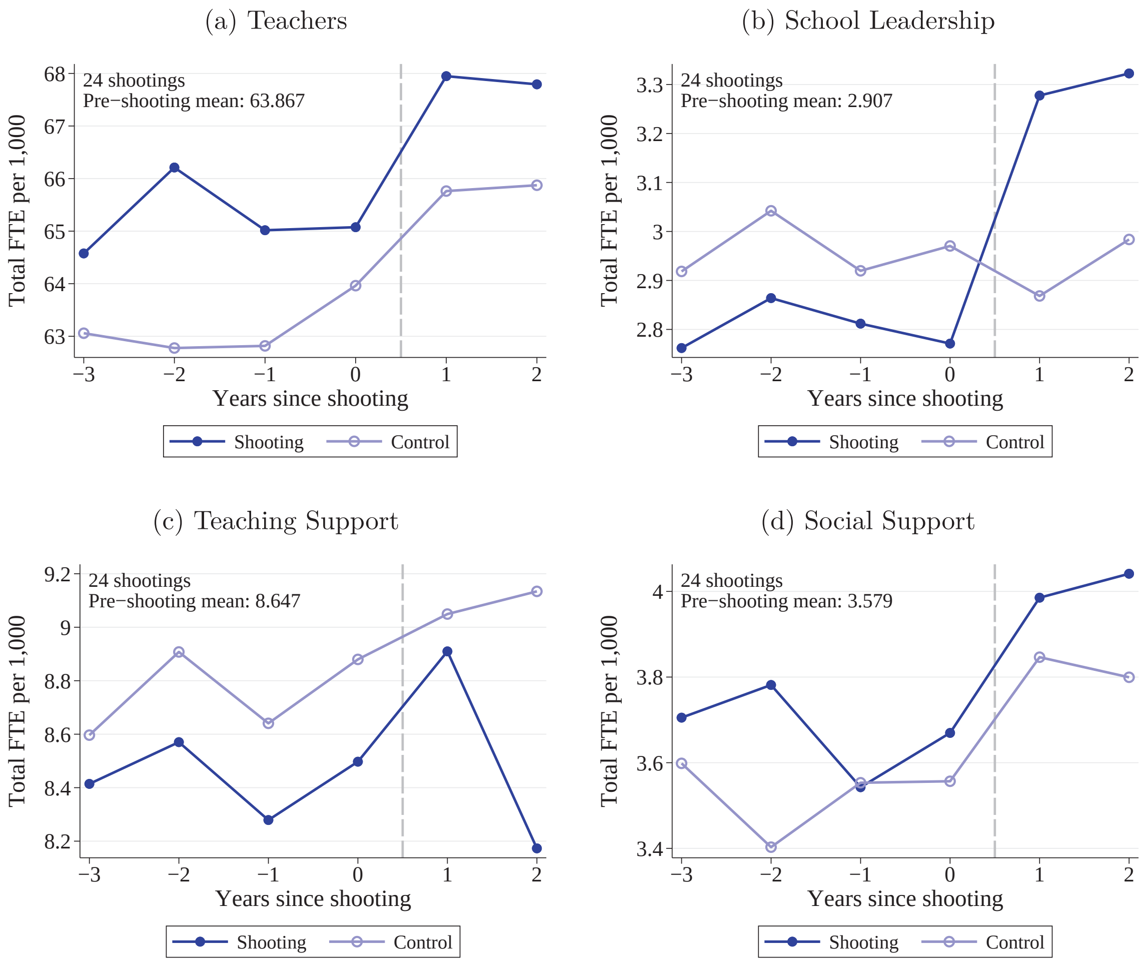
Notes: These figures present output from estimation of equation (2) using either a balanced or unbalanced panel. In each case, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the indicators denoting each of the years before and after a shooting. The academic year before the shooting is the omitted category. The regressions include individual and match group–by–year fixed effects. Standard errors are clustered by school.

Figure A8: Short-Run Effects on Educational Outcomes: Alternative Matching Strategies



Notes: These figures present output from estimation of equation (1) using control schools selected from the matching strategy denoted on the y-axis. In particular, we plot the coefficients and 95% confidence intervals on the interaction between the indicator denoting shooting schools and the post indicator. Our baseline estimates—which use our baseline sample of matched control schools—are presented at the top of each subfigure. The regressions include individual and match group–by–year fixed effects. Standard errors are clustered by school.

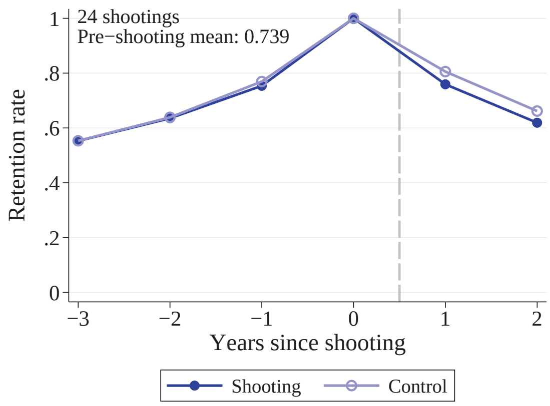
Figure A9: Raw Trends in School-level Employment Across Shooting and Control Schools



Notes: These figures plot raw trends in school-level employment over the six years surrounding a school shooting separately for treatment and matched control schools. Sub-figures (a)–(d) include 24 and 48 control schools. School-by-academic-year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting.

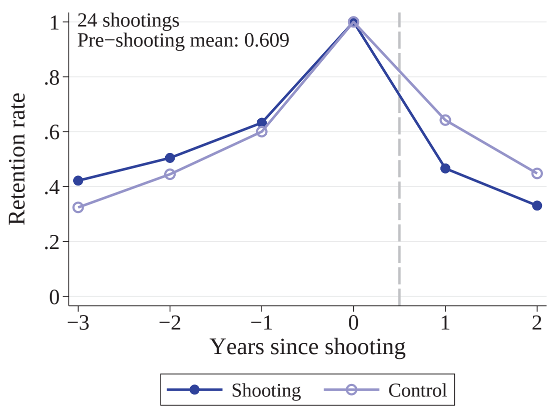
Figure A10: Raw Trends in Retention of Full-Time Employees Across Shooting and Control Schools

(a) Teachers

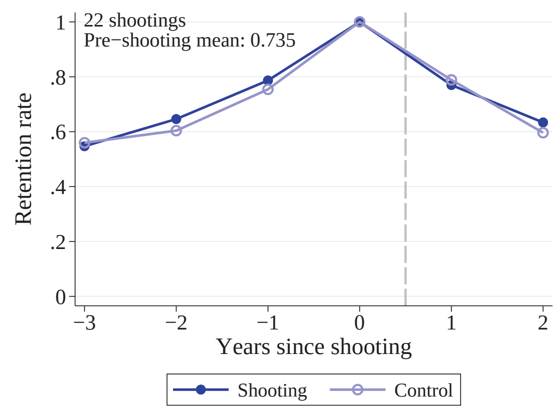


(b) School Leadership

(c) Teaching Support

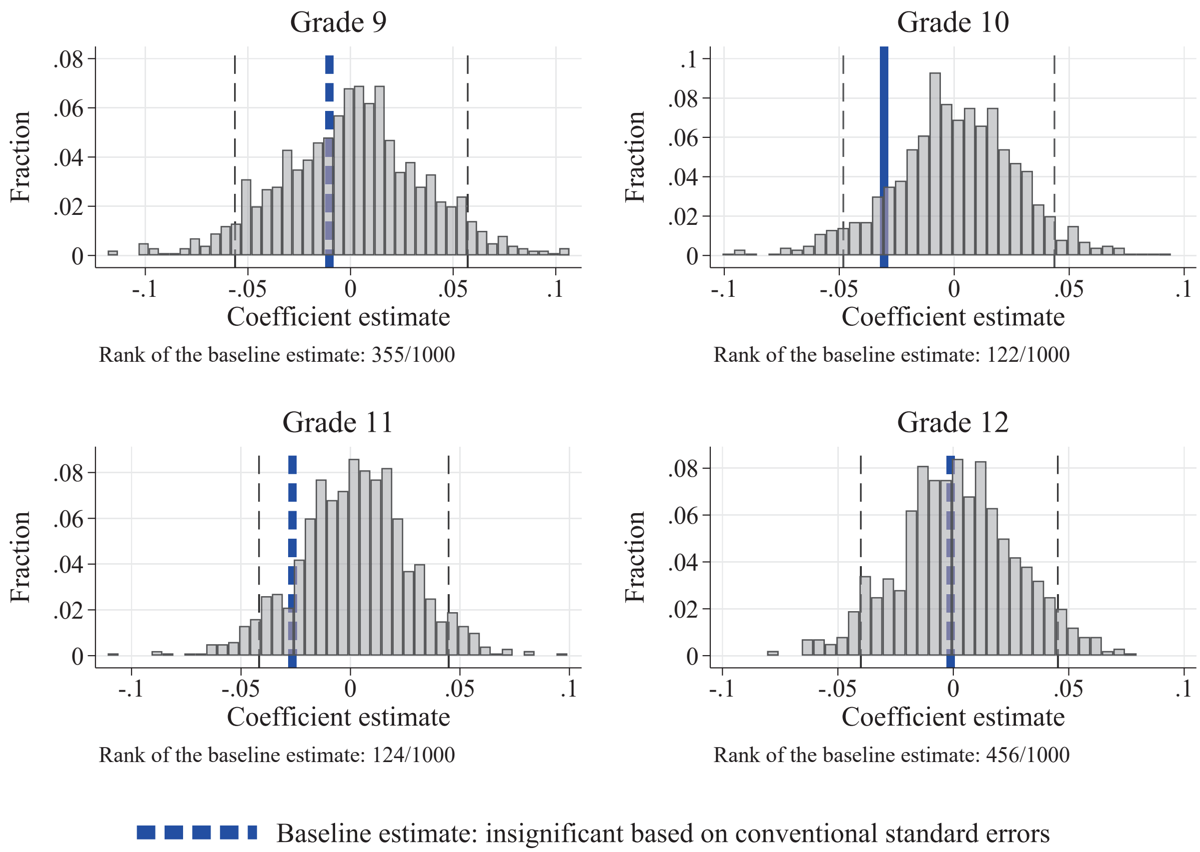


(d) Social Support



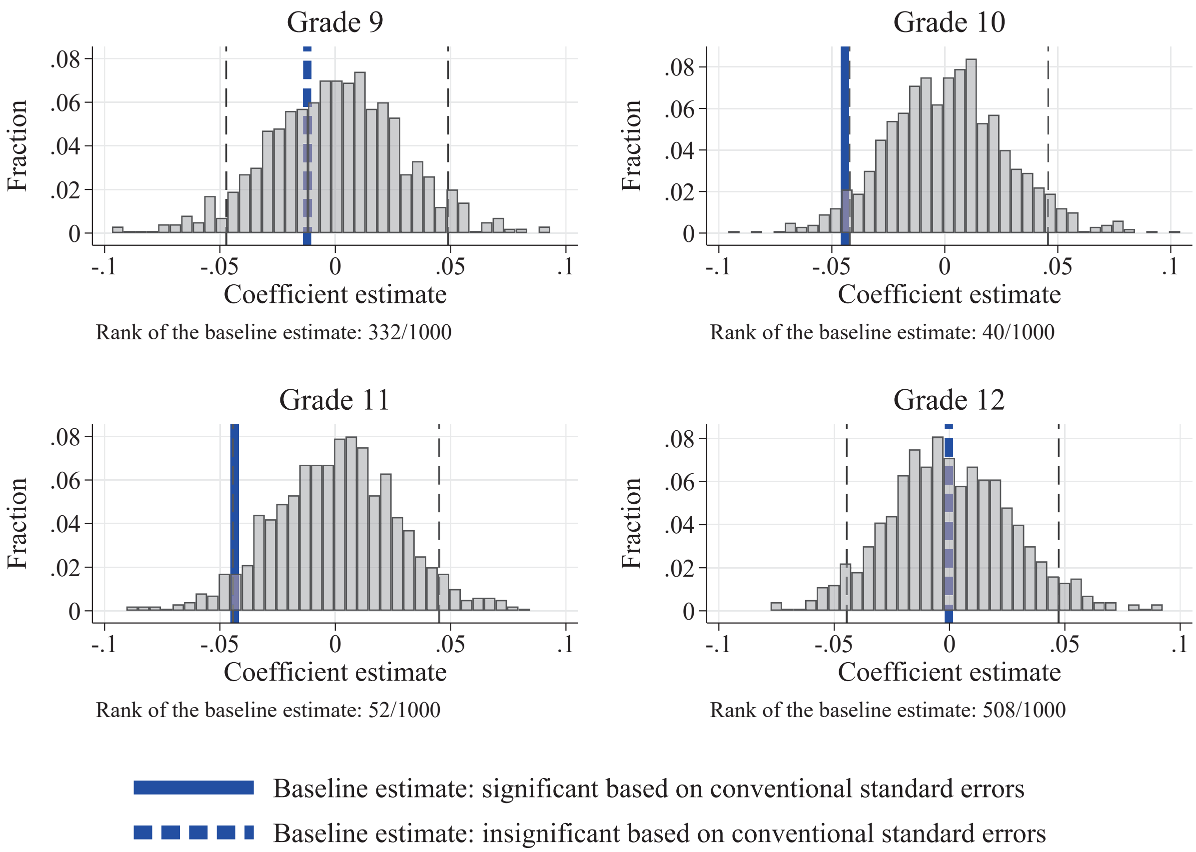
Notes: These figures plot raw trends in retention rates of full-time employees over the six years surrounding a school shooting separately for treatment and matched control schools. We focus on the staff that were employed full-time at each of the shooting and control schools in our staff analysis sample at the time of the shooting, and analyze changes in the probability of full-time employment at the same school both before and after the shooting. Sub-figure (a) includes 24 and 48 control schools; since we drop match groups in which either a shooting school or both control schools had no full-time employees in a given staff group at the time of the shooting, sub-figures (b) and (d) include less than 24 match groups. School-by-academic-year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting.

Figure A11: Permutation Tests: High School Graduation



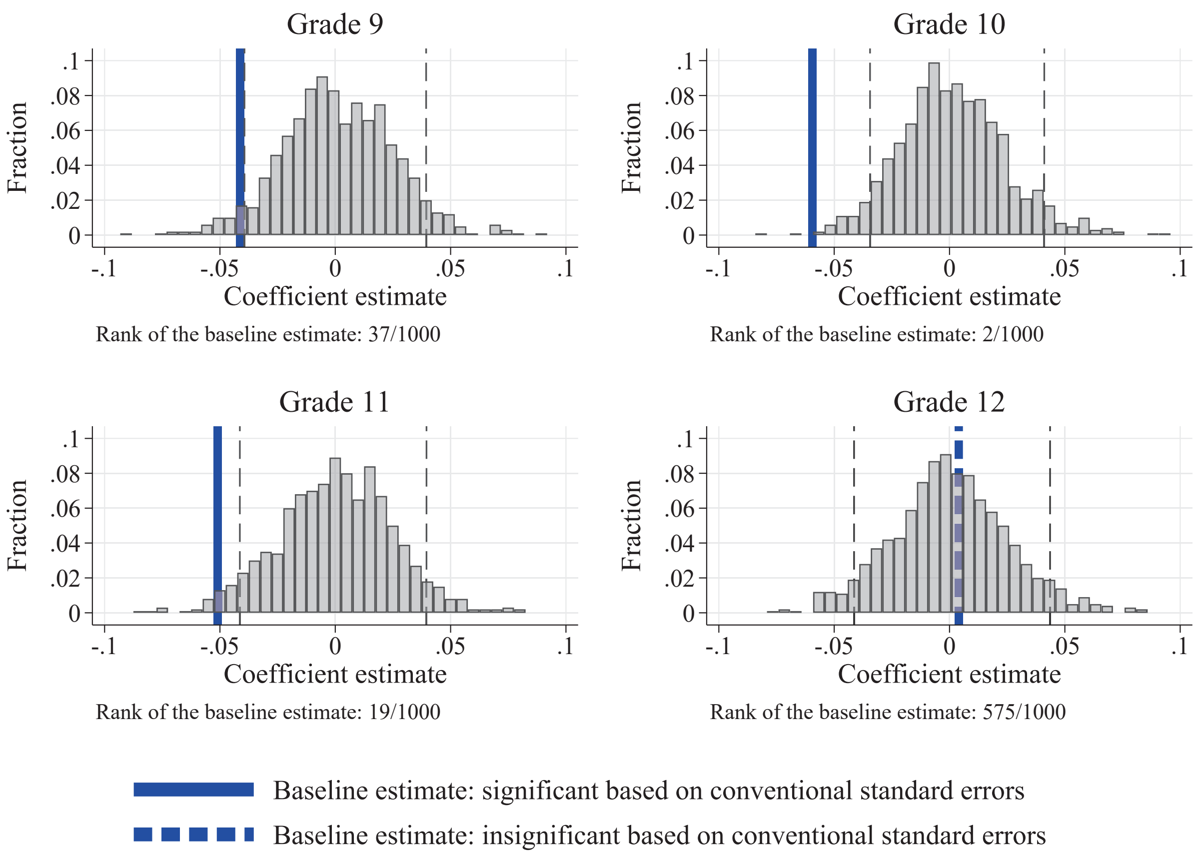
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline estimate (from Table 2) that is statistically significant at the 10% level based on conventional standard errors clustered at the school-by-cohort level, and the thick dashed blue vertical line is the baseline estimate (from Table 2) that is statistically insignificant at the 10% level based on conventional standard errors clustered at the school-by-cohort level. In each sub-figure, we report the rank of the baseline estimate, which is defined as the number of placebo coefficient estimates that are smaller (in relative value) than the baseline estimate.

Figure A12: Permutation Tests: Enrollment in Any College



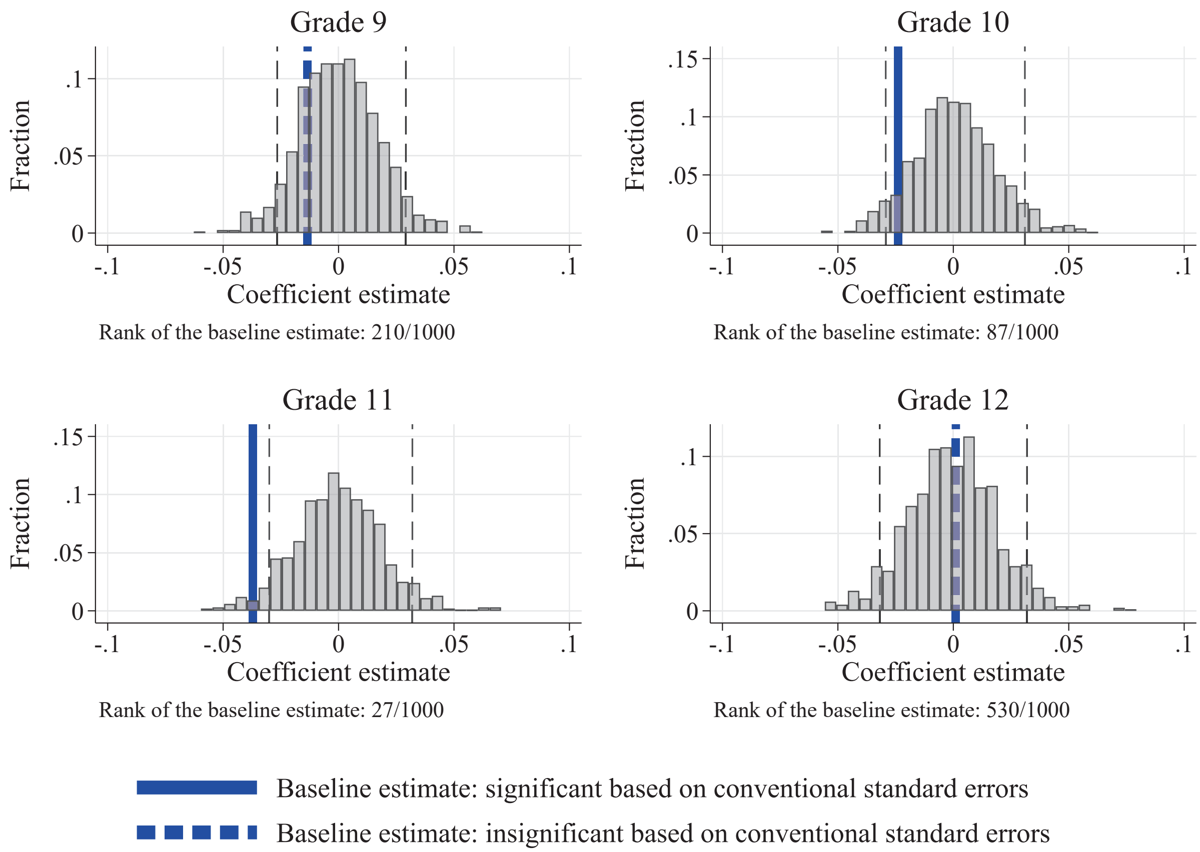
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline estimate (from Table 2) that is statistically significant at the 10% level based on conventional standard errors clustered at the school-by-cohort level, and the thick dashed blue vertical line is the baseline estimate (from Table 2) that is statistically insignificant at the 10% level based on conventional standard errors clustered at the school-by-cohort level. In each sub-figure, we report the rank of the baseline estimate, which is defined as the number of placebo coefficient estimates that are smaller (in relative value) than the baseline estimate.

Figure A13: Permutation Tests: Enrollment in a 4-Year College



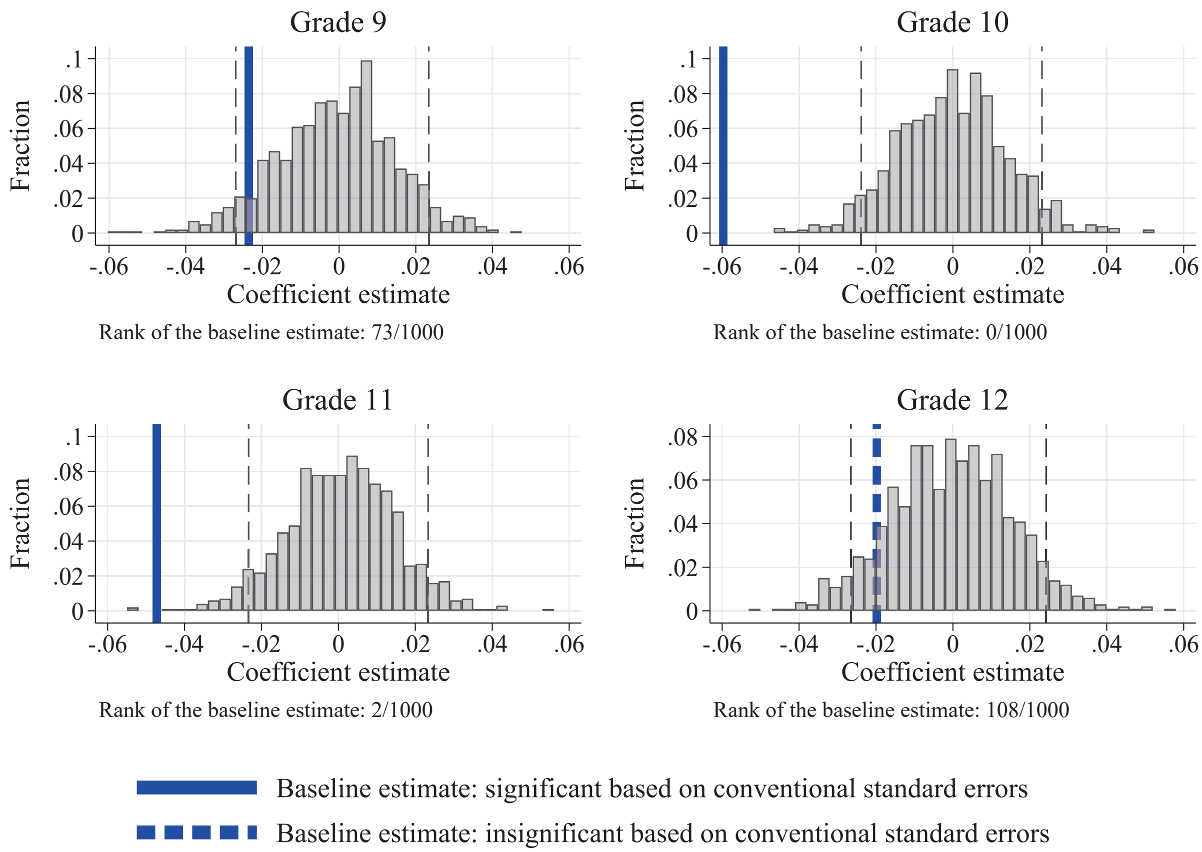
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline estimate (from Table 2) that is statistically significant at the 10% level based on conventional standard errors clustered at the school-by-cohort level, and the thick dashed blue vertical line is the baseline estimate (from Table 2) that is statistically insignificant at the 10% level based on conventional standard errors clustered at the school-by-cohort level. In each sub-figure, we report the rank of the baseline estimate, which is defined as the number of placebo coefficient estimates that are smaller (in relative value) than the baseline estimate.

Figure A14: Permutation Tests: Bachelor's Degree



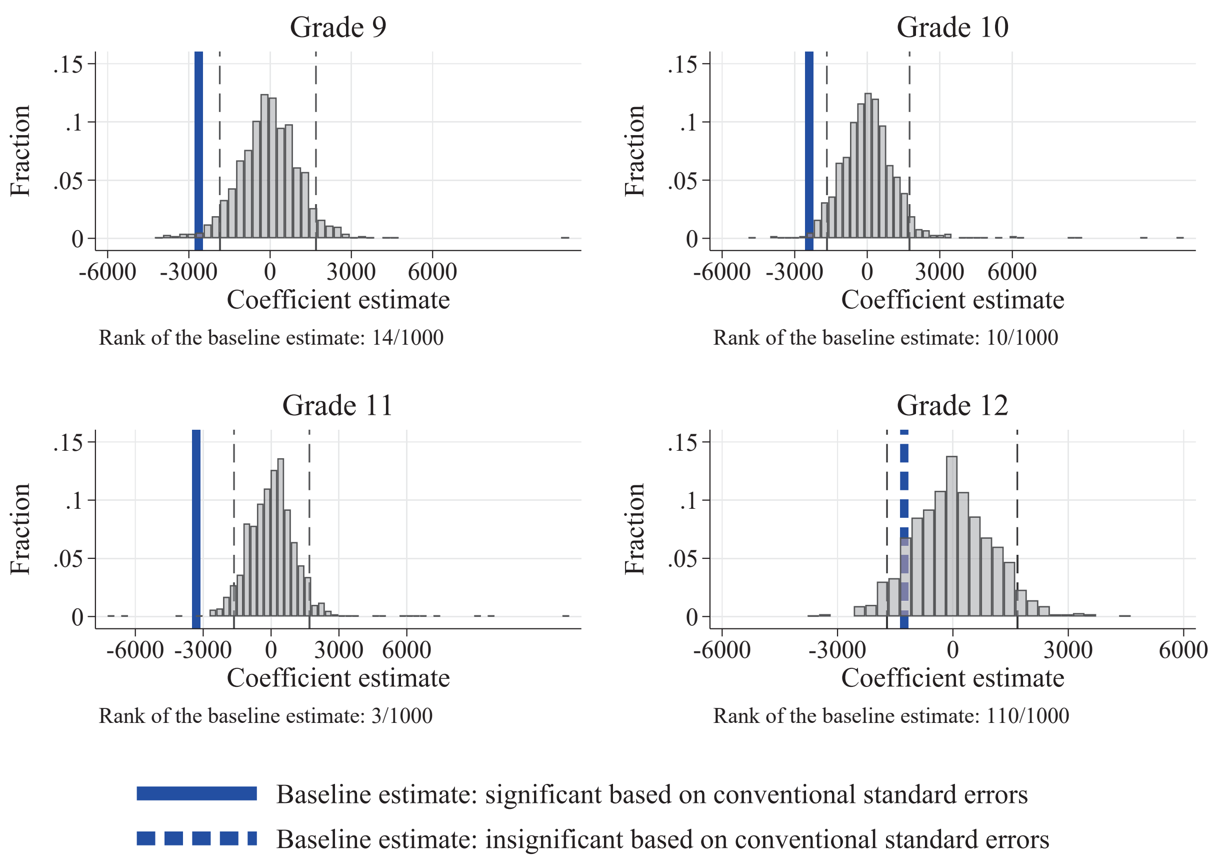
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline estimate (from Table 2) that is statistically significant at the 10% level based on conventional standard errors clustered at the school-by-cohort level, and the thick dashed blue vertical line is the baseline estimate (from Table 2) that is statistically insignificant at the 10% level based on conventional standard errors clustered at the school-by-cohort level. In each sub-figure, we report the rank of the baseline estimate, which is defined as the number of placebo coefficient estimates that are smaller (in relative value) than the baseline estimate.

Figure A15: Permutation Tests: Employed



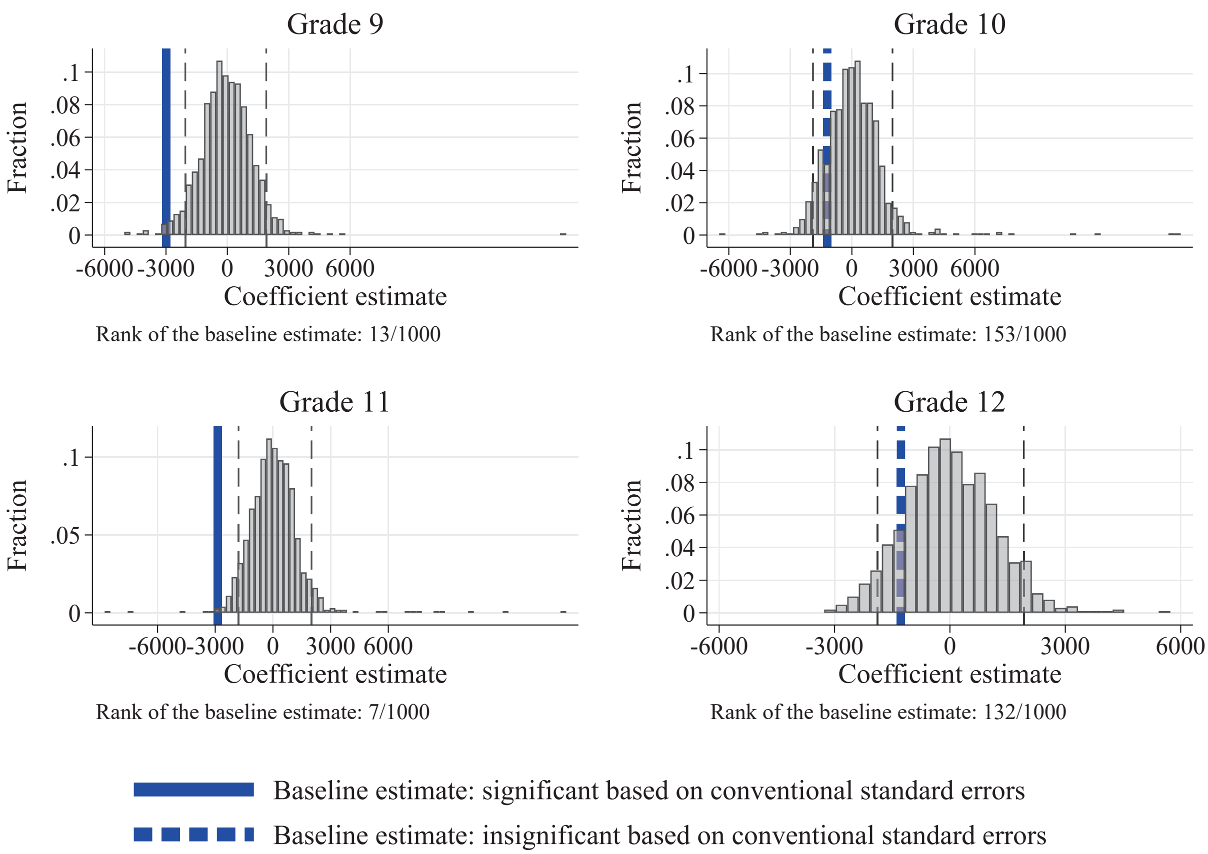
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline estimate (from Table 3) that is statistically significant at the 10% level based on conventional standard errors clustered at the school-by-cohort level, and the thick dashed blue vertical line is the baseline estimate (from Table 3) that is statistically insignificant at the 10% level based on conventional standard errors clustered at the school-by-cohort level. In each sub-figure, we report the rank of the baseline estimate, which is defined as the number of placebo coefficient estimates that are smaller (in relative value) than the baseline estimate.

Figure A16: Permutation Tests: Average Earnings



Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline estimate (from Table 3) that is statistically significant at the 10% level based on conventional standard errors clustered at the school-by-cohort level, and the thick dashed blue vertical line is the baseline estimate (from Table 3) that is statistically insignificant at the 10% level based on conventional standard errors clustered at the school-by-cohort level. In each sub-figure, we report the rank of the baseline estimate, which is defined as the number of placebo coefficient estimates that are smaller (in relative value) than the baseline estimate.

Figure A17: Permutation Tests: Average Non-Zero Earnings



Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline estimate (from Table 3) that is statistically significant at the 10% level based on conventional standard errors clustered at the school-by-cohort level, and the thick dashed blue vertical line is the baseline estimate (from Table 3) that is statistically insignificant at the 10% level based on conventional standard errors clustered at the school-by-cohort level. In each sub-figure, we report the rank of the baseline estimate, which is defined as the number of placebo coefficient estimates that are smaller (in relative value) than the baseline estimate.

B Appendix Tables

Table A1: Description of Shootings Included in Our Analysis Sample

No.	Description
1	Shot self in school bathroom
2	Shot self in school bathroom
3	Showing off gun in bathroom, accidental discharge
4	Held 19 students and teacher hostage for 30 minutes
5	Female student shot herself in bathroom
6	Held class hostage, shot TV, and then shot self
7	Shot herself in the school parking lot in front of other students
8	Former student with shotgun forced students into cafeteria, splashed gasoline, wanted to light school on fire
9	Showed off gun, fired when he put it into waistband of pants striking himself
10	Multiple shots fired outside of school, no injuries
11	Shot football coach for benching his son
12	Officer killed burglary suspect in parking lot
13	Fired gun in pocket during class
14	Man running across the street fired 2 shots that struck the bus
15	School resource officer fired at student while breaking up fight between 10 students
16	Student shot herself outside of school, school officials report it was an accident
17	BB gun fired while showing it off
18	Shot self in band room
19	Female student shot herself in the bathroom
20	Fired shot in bathroom, 4 hour standoff with police before surrendering
21	Rival gang members fired a 3 students in parking lot
22	Accidental shooting in school bathroom
23	Gun fell out of pocket of 6 YOM student in cafeteria, injured 3
24	Shooter was target practicing 1 mile away
25	Police officer killed student holding airsoft pistol
26	Student shot self on school tennis court during the school day
27	Suicide in school courtyard
28	Suicide outside of school building
29	Suicide in school
30	Police officer shot suspect after vehicle pursuit
31	Fired shots at principals car after friend reprimanded
32	Accidental discharge showing off gun
33	Male student put gun to the chest of a female student in the school parking lot; female student pushed gun away from the chest, and bullet grazed her hand

Notes: This table presents descriptions of the 33 shootings that are included in our analysis sample. The descriptions of the first 32 shootings are taken from the CHDS data. The 33rd shooting is only included in the *Washington Post* database, and its description is taken from there.

Table A2: Average School Characteristics Across Treatment, Control, and All Schools

Matching Variables	Shooting Schools (1)	Control Schools (2)	All Schools (3)	*p-val* (1)-(2)	*p-val* (1)-(3)
A. High Schools					
A.1. Exact Matching					
Lowest grade	9.000	9.000	8.356	.	0.072
Highest grade	12.000	12.000	11.916	.	0.377
Fraction city	0.364	0.364	0.284	1.000	0.408
Fraction suburban	0.364	0.364	0.175	1.000	0.020
Fraction town	0.136	0.136	0.161	1.000	0.757
Fraction rural	0.136	0.136	0.381	1.000	0.018
A.2. Nearest Matching					
Female	0.484	0.490	0.462	0.286	0.521
Free/reduced-price lunch	0.442	0.442	0.417	0.999	0.652
Non-Hispanic white	0.387	0.413	0.456	0.764	0.314
Non-Hispanic Black	0.222	0.204	0.129	0.785	0.020
Hispanic	0.359	0.356	0.397	0.966	0.577
Number of students	1,650.182	1,564.614	772.355	0.707	0.000
Number of schools	22	44	3,053		
B. Non-High Schools					
B.1. Exact Matching					
Lowest grade	4.000	4.000	0.869	1.000	0.002
Highest grade	7.273	7.273	6.091	1.000	0.121
Fraction city	0.636	0.636	0.395	1.000	0.101
Fraction suburban	0.182	0.182	0.252	1.000	0.593
Fraction town	0.000	0.000	0.130	.	0.200
Fraction rural	0.182	0.182	0.224	1.000	0.737
B.2. Nearest Matching					
Female	0.487	0.496	0.478	0.263	0.690
Free/reduced-price lunch	0.419	0.487	0.523	0.530	0.219
Non-Hispanic white	0.149	0.144	0.388	0.953	0.010
Non-Hispanic Black	0.196	0.173	0.141	0.818	0.342
Hispanic	0.637	0.666	0.447	0.815	0.047
Number of students	865.727	825.636	550.822	0.711	0.001
Number of schools	11	22	9,459		

Notes: This table presents average characteristics for treatment, control, and all Texas public schools. Panel A (B) presents averages for high schools (non-high schools); Panels A.1 (A.2) and B.1 (B.2) present means of characteristics on which we do an exact ("fuzzy") match. For shooting and matched control schools, characteristics are measured in the first six-week grading period of the academic year of the shooting; for all Texas public schools, averages are calculated over academic years 1993–1994 to 2017–2018.

Table A3: Short-Run Effects on Educational Outcomes Among Long-Run Analysis Sample

	Absence Rate (1)	Chronic Absenteeism (2)	Grade Repetition (3)	Days of Disc. Act. (4)	Switch Schools (5)
A. Baseline sample (33 shootings)					
Shooting School x Post	0.0044	0.0179	0.0132	0.2482	0.0130
	(0.0019)	(0.0079)	(0.0053)	(0.1685)	(0.0110)
	[0.022]	[0.027]	[0.016]	[0.145]	[0.241]
Pre-period outcome mean	0.0365	0.0643	0.0106	1.9998	0.1060
Student-year observations	373,368	373,368	373,368	277,176	371,285
R-squared	0.553	0.481	0.233	0.426	0.276
B. Long-run analysis sample (8 shootings)					
Shooting School x Post	0.0088	0.0339	0.0259	0.3433	0.0143
	(0.0026)	(0.0105)	(0.0129)	(0.3112)	(0.0097)
	[0.003]	[0.004]	[0.058]	[0.293]	[0.154]
Pre-period outcome mean	0.0366	0.0622	0.0083	2.5953	0.0719
Student-year observations	76,920	76,920	76,920	24,996	76,466
R-squared	0.531	0.465	0.253	0.362	0.281

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (1). Panel A reproduces our baseline estimates that use 33 shootings and their matched control schools (first presented in Table 1); Panel B considers the subset of eight shootings and their matched control schools that are used in our long-run analysis. The regressions include individual and match group–by–academic year fixed effects. Standard errors are clustered by school. Since grade repetition reflects academic performance in the previous academic year, we exclude the year of the shooting from the post period when analyzing this outcome.

Table A4: Descriptive Statistics for School Staff Groups and Individual Staff Types

	Panel A Total FTE			**Panel B** % Full Time	**Panel C** Total FTE per 1,000 Students		
	Mean (1)	SD (2)	Median (3)	Mean (4)	Mean (5)	SD (6)	Median (7)
Teacher	110.124	51.392	102.190	88.46	64.753	8.943	63.517
School leadership	4.846	2.889	4.000	86.30	2.957	1.163	2.885
Assistant principal	3.857	2.799	3.000	85.07	2.182	1.060	2.160
Principal	0.989	0.320	1.000	91.86	0.775	0.616	0.590
Teaching support staff	13.454	7.382	11.986	69.71	8.732	4.632	7.897
Educational diagnostician	0.681	0.907	0.000	69.81	0.395	0.501	0.000
Teacher supervisor	0.104	0.405	0.000	89.84	0.057	0.245	0.000
Truant officer/visiting teacher	0.044	0.259	0.000	43.01	0.044	0.281	0.000
Educational aide	11.502	6.525	10.000	71.79	7.547	4.259	6.825
Teacher facilitator	0.552	1.087	0.000	72.95	0.365	0.768	0.000
Substitute teacher	0.572	1.566	0.000	64.79	0.325	0.843	0.000
Social support staff	6.465	4.476	5.031	76.35	3.682	1.334	3.497
Art therapist	0.002	0.049	0.000	100.00	0.002	0.034	0.000
Psychological associate	0.037	0.176	0.000	24.41	0.021	0.096	0.000
Audiologist	0.029	0.167	0.000	100.00	0.009	0.053	0.000
Corrective therapist	0.007	0.068	0.000	16.56	0.005	0.056	0.000
Counselor	4.581	3.057	4.000	85.21	2.560	0.885	2.506
Music therapist	0.007	0.071	0.000	13.60	0.005	0.051	0.000
Occupational therapist	0.012	0.047	0.000	0.00	0.007	0.027	0.000
Certified orientation and mobility specialist	0.007	0.040	0.000	0.00	0.005	0.028	0.000
Physical therapist	0.015	0.056	0.000	0.00	0.007	0.028	0.000
Recreational therapist	0.013	0.114	0.000	100.00	0.004	0.032	0.000
School nurse	0.942	0.576	1.000	76.09	0.627	0.443	0.512
LSSP/psychologist	0.095	0.457	0.000	71.17	0.035	0.151	0.000
Social worker	0.095	0.307	0.000	48.08	0.052	0.186	0.000
Speech therapist	0.260	0.410	0.000	24.49	0.178	0.323	0.000
Certified interpreter	0.366	1.282	0.000	92.49	0.167	0.543	0.000
School-by-academic-year observations		432		432		432	

Notes: This table presents the mean, standard deviation, and median of school-level employment separately for the school staff groups and the individual staff types included in each group. Panel A (Panel C) presents the number of FTE staff (the number of FTE staff per 1,000 students); Panel B presents share full-time employees. Averages are calculated among 24 shooting and 48 control schools over the period from three years before to two years after the shooting (balanced panel, N=432). School-by-academic-year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting.

Table A5: Effects on School-level Employment and Retention of Full-Time Employees

	Teachers (1)	School Leadership (2)	Teaching Support (3)	Social Support (4)
A. Effects on School-level Employment (FTE per 1,000 students)				
Shooting School x Post	0.1771	0.5461	-0.1726	0.0312
	(1.2364)	(0.2917)	(0.6276)	(0.2121)
	[0.887]	[0.065]	[0.784]	[0.884]
Pre-period outcome mean	63.867	2.907	8.647	3.579
School-academic-year observations	432	432	432	432
R-squared	0.909	0.819	0.898	0.867
B. Effects on Retention of Full-Time Employees				
Shooting School x Post	-0.0416	0.0817	-0.2001	-0.0146
	(0.0185)	(0.0882)	(0.0727)	(0.0726)
	[0.028]	[0.358]	[0.008]	[0.841]
Pre-period outcome mean	0.739	0.659	0.609	0.735
School-academic-year observations	432	384	414	390
R-squared	0.939	0.743	0.830	0.710

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation described in Section 4.4. Panel A reports the effects on school-level employment (the number of FTE staff per 1,000 students); Panel B presents the effects on the retention of school staff. In Panel B, we focus on the staff that were employed full-time at each of the shooting and control schools in our staff analysis sample at the time of the shooting, and analyze changes in the probability of full-time employment at the same school both before and after the shooting. The regressions include school and match group-by-year fixed effects. School-by-academic-year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting. Standard errors are clustered by school.

Table A6: Lower and Upper Bounds on the Estimated Effect Sizes: Educational Outcomes

	Graduate HS	Enroll Any Col	Enroll 4yr Col	Bachelor's Degree
	(1)	(2)	(3)	(4)
A. Shooting School x Cohort 12				
Baseline estimate	-0.0011	-0.0003	0.0040	0.0011
	(0.0201)	(0.0272)	(0.0176)	(0.0145)
	[0.956]	[0.990]	[0.821]	[0.939]
Lee lower bound	-0.0098	-0.0065	0.0003	-0.0009
	(0.0223)	(0.0269)	(0.0176)	(0.0147)
	[0.661]	[0.809]	[0.988]	[0.952]
Lee upper bound	-0.0008	0.0056	0.0128	0.0107
	(0.0201)	(0.0273)	(0.0185)	(0.0150)
	[0.967]	[0.838]	[0.490]	[0.479]
B. Shooting School x Cohort 11				
Baseline estimate	-0.0265	-0.0436	-0.0509	-0.0372
	(0.0164)	(0.0171)	(0.0174)	(0.0117)
	[0.108]	[0.012]	[0.004]	[0.002]
Lee lower bound	-0.0366	-0.0492	-0.0541	-0.0386
	(0.0167)	(0.0177)	(0.0175)	(0.0116)
	[0.030]	[0.006]	[0.002]	[0.001]
Lee upper bound	-0.0251	-0.0379	-0.0427	-0.0273
	(0.0167)	(0.0171)	(0.0172)	(0.0117)
	[0.134]	[0.028]	[0.014]	[0.021]
C. Shooting School x Cohort 10				
Baseline estimate	-0.0305	-0.0442	-0.0595	-0.0240
	(0.0175)	(0.0151)	(0.0130)	(0.0135)
	[0.083]	[0.004]	[<0.001]	[0.077]
Lee lower bound	-0.0386	-0.0494	-0.0626	-0.0256
	(0.0171)	(0.0151)	(0.0134)	(0.0139)
	[0.026]	[0.001]	[<0.001]	[0.067]
Lee upper bound	-0.0283	-0.0383	-0.0516	-0.0150
	(0.0163)	(0.0147)	(0.0138)	(0.0146)
	[0.085]	[0.010]	[<0.001]	[0.307]
D. Shooting School x Cohort 9				
Baseline estimate	-0.0103	-0.0121	-0.0412	-0.0134
	(0.0177)	(0.0180)	(0.0162)	(0.0094)
	[0.560]	[0.504]	[0.012]	[0.159]
Lee lower bound	-0.0164	-0.0159	-0.0438	-0.0148
	(0.0199)	(0.0189)	(0.0171)	(0.0099)
	[0.412]	[0.400]	[0.011]	[0.139]
Lee upper bound	-0.0079	-0.0078	-0.0359	-0.0068
	(0.0187)	(0.0176)	(0.0161)	(0.0093)
	[0.672]	[0.656]	[0.026]	[0.465]

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (4). Each panel reproduces our baseline estimates (first presented in Table 2) and reports estimates from two trimmed samples, constructed following the Lee (2009) bounding procedure. We estimate Lee (2009) bounds assuming differential attrition in response to a school shooting of 0.86 percentage point (the attrition gap between shooting and control schools shown in Figure A2(b)). For each exposed cohort and each of the eight demographic groups (i.e., interactions between gender and race/ethnicity) in the control sample, we trim the observations by 0.86 percent to estimate the bounds. For continuous outcomes, we estimate the lower (upper) bound of the effect on each outcome by dropping observations that are in the bottom (top) 0.86 percent of the outcome distribution. For binary outcomes, the lower (upper) bound drops 0.86 percent of observations that all have a value of "0" ("1").

Table A7: Lower and Upper Bounds on the Estimated Effect Sizes: Labor Market Outcomes

	Employed	Earnings	Non-Zero Earnings
	(1)	(2)	(3)
A. Shooting School x Cohort 12			
Baseline estimate	-0.0198	-1,265.02	-1,273.02
	(0.0165)	(977.91)	(1,053.43)
	[0.230]	[0.198]	[0.229]
Lee lower bound	-0.0288	-1,543.57	-1,610.23
	(0.0169)	(1,010.76)	(1,080.35)
	[0.091]	[0.128]	[0.138]
Lee upper bound	-0.0168	-272.51	-244.82
	(0.0169)	(840.00)	(833.99)
	[0.321]	[0.746]	[0.769]
B. Shooting School x Cohort 11			
Baseline estimate	-0.0472	-3,316.19	-2,867.87
	(0.0139)	(953.31)	(1,183.25)
	[0.001]	[0.001]	[0.017]
Lee lower bound	-0.0555	-3,558.46	-3,189.74
	(0.0143)	(958.16)	(1,176.31)
	[<0.001]	[<0.001]	[0.007]
Lee upper bound	-0.0437	-1,406.34	-580.44
	(0.0135)	(622.38)	(583.69)
	[0.001]	[0.025]	[0.321]
C. Shooting School x Cohort 10			
Baseline estimate	-0.0597	-2,389.54	-1,199.10
	(0.0097)	(793.63)	(1,063.11)
	[<0.001]	[0.003]	[0.261]
Lee lower bound	-0.0678	-2,604.78	-1,499.71
	(0.0096)	(803.86)	(1,040.57)
	[<0.001]	[0.001]	[0.151]
Lee upper bound	-0.0566	-1,416.24	-138.49
	(0.0089)	(613.67)	(829.51)
	[<0.001]	[0.022]	[0.868]
D. Shooting School x Cohort 9			
Baseline estimate	-0.0236	-2,633.79	-2,982.86
	(0.0098)	(1,094.13)	(1,316.82)
	[0.017]	[0.017]	[0.025]
Lee lower bound	-0.0290	-2,789.45	-3,255.43
	(0.0098)	(1,077.60)	(1,299.36)
	[0.004]	[0.010]	[0.013]
Lee upper bound	-0.0207	-655.73	-623.68
	(0.0099)	(571.22)	(696.59)
	[0.038]	[0.252]	[0.372]

Notes: This table presents coefficients, standard errors (in parentheses), and $p-$values [in brackets] from estimation of equation (4). Each panel reproduces our baseline estimates (first presented in Table 3) and reports estimates from two trimmed samples, constructed following the Lee (2009) bounding procedure. We estimate Lee (2009) bounds assuming differential attrition in response to a school shooting of 0.86 percentage point (the attrition gap between shooting and control schools shown in Figure A2(b)). For each exposed cohort and each of the eight demographic groups (i.e., interactions between gender and race/ethnicity) in the control sample, we trim the observations by 0.86 percent to estimate the bounds. For continuous outcomes, we estimate the lower (upper) bound of the effect on each outcome by dropping observations that are in the bottom (top) 0.86 percent of the outcome distribution. For binary outcomes, the lower (upper) bound drops 0.86 percent of observations that all have a value of “0” (“1”).