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

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ABSTRACT

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

A growing number of American children are exposed to gun violence at their schools, but little is known about the impacts of this exposure on their human capital attainment and economic well-being. This paper studies the causal effects of exposure to shootings at schools on children's educational and economic outcomes, using individual-level longitudinal administrative data from Texas. We analyze the universe of shootings at Texas public schools that occurred between 1995 and 2016 and match schools that experienced shootings with observationally similar control schools in other districts. We use difference-in-difference models that leverage within-individual and across-cohort variation in shooting exposure within matched school groups to estimate the short- and long-run impacts of shootings on students attending these schools at the time of the shooting. We find that shooting-exposed students have an increased absence rate and are more likely to be chronically absent and repeat a grade in the two years following the event. We also find adverse long-term impacts on the likelihood of high school graduation, college enrollment and graduation, as well as employment and earnings at ages 24–26. Heterogeneity analyses by student and school characteristics indicate that the detrimental impacts of shootings are universal, with most sub-groups being affected.

JEL Classification: I24, I31, J13

Keywords: school shootings, childhood trauma, human capital development

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1 Introduction

Questions about human capital development have been central in economics research for many decades (Schultz, 1961; Becker, 1975; Romer, 1986). Because an important part of human capital accumulation takes place at schools, a large literature investigates how educational inputs, such as teacher quality and enrichment programs, affect children's human capital outcomes (Hanushek et al., 2011, 2016). At the same time, an emerging literature shows that the formation of human capital can be disrupted by adverse shocks—such as exposure to violence.[1] While nearly 40 percent of American children experience or witness violence over the course of their childhood (Federal Interagency Forum on Child and Family Statistics, 2018), little is known about the long-term impacts of such exposure on children's human capital accumulation and subsequent economic outcomes.

In this paper, we use longitudinal, administrative microdata from Texas to provide the first comprehensive analysis of the short- and long-run impacts of an increasingly common source of exposure to violence among children: shootings at schools. The number of shootings at U.S. schools has doubled in the last two decades, with more than 100,000 American children attending a school at which a shooting took place in 2018 and 2019 alone.[2] While public discussions often center on the tragic deaths resulting from such events, we find that experiencing gun violence at school has lasting economic implications for survivors. Our results illustrate 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.

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

[1] See: Cook and Ludwig (2002); Aizer (2007); Sharkey (2010); Sharkey et al. (2012); Burdick-Will (2013); Caudillo and Torche (2014); Sharkey et al. (2014); McCoy et al. (2015); Monteiro and Rocha (2017); Duque (2017); Heissel et al. (2018); Gershenson and Tekin (2018); Sharkey (2018); Ang (2020).

[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 by the average enrollment at schools that experienced a shooting as reported in the *Washington Post* school shooting database.

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 is negatively associated with children's educational and behavioral outcomes (Cook and Ludwig, 2002; Aizer, 2007; Sharkey, 2010; Sharkey et al., 2012; Burdick-Will, 2013; Caudillo and Torche, 2014; Sharkey et al., 2014; McCoy et al., 2015; Monteiro and Rocha, 2017; Duque, 2017; Heissel et al., 2018; Gershenson and Tekin, 2018; Sharkey, 2018; Ang, 2020). At the same time, the literature on child resilience argues that children can "bounce back" after trauma (see, e.g., Agaibi and Wilson, 2005; Goldstein and Brooks, 2005; Garrett et al., 2019), suggesting that exposure to violence during childhood may not have lasting effects.

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 Blacks adversely affects the mental health of Black adults (Bor et al., 2018) and the educational outcomes of Black 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. Relative to violence in other settings, shootings that occur at schools may also cause greater disruption to students' learning by influencing other educational inputs. For example, school shootings may lead to curriculum disruptions, resource diversion, or reductions in teaching quality. Finally, since peer effects have been shown to be particularly important in a school setting, 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).

Our analysis uses longitudinal, administrative microdata 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 moderately traumatic events frequently occurring in schools. Our short-term analysis focuses on

[3]See, for example: 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; Miller et al., 2018; Heissel et al., 2018.

the 33 Texas public schools that experienced a shooting on school grounds during school hours 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 in students from control schools that are matched based on institutional and student characteristics.

We find that school shootings 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.[4] We find no significant effects on the frequency of disciplinary actions such as suspensions, expulsions, or in-school detentions.[5] 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.[6]

For the long-term analysis, we make use of the linkage between the individual-level public school records and (1) college enrollment and graduation files from the Texas Higher Education Coordinating Board and (2) employment and earnings data from the Texas Workforce Commission. We study the impacts of the eight shootings that took place at Texas public high schools over the period 1998–2006 on individual outcomes through age 26. Since these long-term outcomes are only observed after each 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 in matched control schools.

[4] 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.

[5] 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.

[6] We also have access to data on reading and math scores from standardized tests. However, since Texas has used different standardized tests that have been administered to different grades over the course of our analysis time period, we are unable to examine test scores as an outcome.

We find that school shootings 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 and 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 through 11 are 4.4 percentage points (6.3 percent) less likely to be employed and have $2,779.84 (13.5 percent) lower average annual earnings between the ages of 24 and 26. This estimate implies a $115,550 reduction (in 2018 dollars) in the present discounted value of lifetime earnings per shooting-exposed student.

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. Finally, 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.[7]

Our study contributes to three strands of literature. The first is a small but growing set of

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

studies on the impacts of school shootings.[8] Recent work documents that school shootings can have detrimental effects on the mental health (Rossin-Slater et al., 2020; Levine and McKnight, 2020a)[9] and short-run educational outcomes (Poutvaara and Ropponen, 2010; Abouk and Adams, 2013; Beland and Kim, 2016)[10] of surviving youth. Notably, a contemporaneous study by Levine and McKnight (2020a) uses school and district-level data on test scores and absences and shows that these outcomes deteriorated substantially in the years following the Sandy Hook Elementary School shooting in Newtown, Connecticut. We build upon this work by using comprehensive, individual-level administrative data that allows us to examine both the short- and long-term effects of a diverse set of school shooting events. Our use of individual-level data enables us to identify students exposed to school shootings, precisely estimate the impacts of this exposure over time, and investigate heterogeneity in these impacts across student, school, and shooting characteristics. 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 the event. 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 more typical shootings at schools that may be more comparable to other forms of violence

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

[9] Rossin-Slater et al. (2020) analyze 44 school shootings that occurred in the United States between 2008 and 2013 and find that fatal shootings lead to large and persistent increases in antidepressant use among local youth. Relatedly, Levine and McKnight (2020a) document an increase in external-cause mortality—including suicides and accidents—among local residents who were aged 14–18 at the time of the 1999 shooting at Columbine High. Recent work by Soni and Tekin (2020) documents similar mental health effects of mass shootings more generally, showing that community well-being declines after an event. Brodeur and Yousaf (2020) show that these reductions in community well-being are accompanied by decreases in local employment, earnings, and housing prices. 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.

[10] Beland and Kim (2016) use school-level data from California high schools and find that 9th grade enrollment and standardized test scores drop in the years following a deadly shooting. Abouk and Adams (2013) use state-level data on school enrollment and document that private school enrollment increases following a school shooting. Outside the United States, Poutvaara and Ropponen (2010) analyze the effects of a 2008 university shooting in Finland that took place during the high school graduation exam period and find a decline in performance among exam-takers.

to which children are commonly exposed.[11]

Our work further contributes to a growing literature on the effects of gun violence more generally. Ang (2020) documents that police killings in a large urban district in the Southwest adversely affect the grades and rates of high school graduation and college enrollment among students living nearby. Our study also complements recent work on exposure to a single, non-school-based mass shooting in Norway by Bharadwaj et al. (2020), which finds adverse impacts on teenage survivors' test scores, health visits, educational attainment, and earnings. Our study suggests that much less deadly shootings—which are significantly more widespread, especially in the United States—nevertheless generate large human capital and economic costs for the many children who are present on school grounds when they occur.

Finally, our work contributes to a broad literature investigating the long-run effects of childhood circumstances and educational inputs. Prior work has investigated the long-run effects of 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 the empirical strategies. Section 4 provides main results, heterogeneity analyses, robustness exercises, and a discussion. Section 5 concludes.

[11] In fact, no mass shootings occurred in Texas public schools 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.

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).[12] 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 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.[13] 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, 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*

[12] 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/`.

[13] 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/`.

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.[14] For the long-run analysis, we consider the eight shootings that took place at Texas 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. While no shootings 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 are not as deadly as those typically covered in the media. Nevertheless, these shootings may affect the thousands of students who are at school when they occur.

Figure 1 displays the locations of the shootings used in our analyses. The spread of shootings across the state largely reflects the distribution of Texas's population. In addition, Appendix Figure A1 depicts the number of shootings per academic year. 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.[15] 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).[16] The data further contain information on student characteristics—such as age, gender, race/ethnicity, and receipt of free or reduced-

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

[15] 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/`.

[16] As previously noted, data on disciplinary actions is only available from the academic year 1998–1999 onward.

price lunch—and the number of different types of staff employed at each school in each year.

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.[17] We also obtain information on whether a student graduated high school—and if so, at which age—from these records.[18]

Second, we use administrative microdata on enrollment and graduation from all public institutions of higher education in the state of Texas from the Texas Higher Education Coordinating Board (THECB).[19] The THECB data are linked to the TEA data at the individual level, allowing us to follow students from elementary school through college. 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

[17]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 "natural" 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 any significant effects.

[18]As previously noted, 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 has used different standardized tests that have been 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 A1). While we therefore do not consider test scores as an outcome, we do use test scores in a robustness exercise. In particular, as outlined in Section 4, we augment the matching algorithm used to select control schools to include school-level average test scores and the share of students with non-missing test scores as matching variables.

[19]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, 2010). We note that our research design includes cohort fixed effects, which allows us to control for changes in data coverage over time.

(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; we discuss in Section 3.3 how this is expected to influence 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 (TWC).[20] As with the THECB data, the TWC data are linked to the TEA data at the individual level. 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 ever having positive earnings; (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). We do not observe information about employment outside of Texas; see Section 3.3 for a discussion of how we expect this to influence our estimates.[21]

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 types of difference-in-difference (DD) models 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 regression models for the short- and long-run analyses.

[20] UI covers all workers whose employers pay at least $1,500 in gross earnings to employees 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.

[21] 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 the 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) 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).

3.1 Matching Schools with Shootings to Control Schools

As noted in Section 2, 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 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 a different district 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.[22] 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: 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. In additional specifications, we explore the sensitivity of our estimates to alternative matching strategies (see Section 4 for more details).

Appendix Table A1 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 school 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

[22]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.

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 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 occurs for a given student in the TEA data (e.g., attendance). 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.[23] 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.[24] 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 DD models in which we compare within-student changes in outcomes following a shooting between the shooting and matched control schools. Our regression models 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

[23]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 in 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).

[24]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 our Texas public schools database. Students at control schools who have ever been enrolled in a shooting school (3.6 percent of all students at the control schools) are excluded from our sample.

that experienced a shooting, and $Post_t$ is an indicator denoting observations in the academic year of the shooting and the following two years.[25] 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 in the DD framework. 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 models 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 event study estimates and the raw data plots reveal no evidence of differential pre-trends that would bias our estimates.

An additional concern for our short-run analysis is that of possible selective attrition from the sample. That is, our short-run estimates would be biased if students systematically left the Texas public school system—either because they switched to private schools or because they moved out of state—as a result of exposure to a shooting. This type of response has been documented in prior studies analyzing aggregate data on school enrollment (Abouk and

[25] Since grade repetition reflects academic performance 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.

Adams, 2013; Beland and Kim, 2016).

To assess the importance of this concern, we analyze an unbalanced panel of students. This sample is constructed in the same way as our primary analysis sample described above except that we do not make any restrictions on the number of years that a student must be observed in the data. In Appendix Figure A2, we plot 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 difference in the rates of attrition between students at shooting and control schools. Thus, it does not appear that selective attrition out of the Texas public school system is likely to bias our estimates. In addition, we show in Section 4 that our short-run estimates are very similar whether 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, 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 once for each student in the TEA, THECB, or TWC data (e.g., high school graduation by age 26 or employment at ages 24–26). Since we only observe each outcome once, we cannot include individual fixed effects in our regression models. Instead, our DD models compare differences in cohort outcomes between students who were enrolled in shooting 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, we limit our analysis to the set of eight shootings that took place at Texas 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 of the 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*.[26] That is, our "too old" cohorts would be in "expected" grades 14–17 at the time of the shooting.

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 a model 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. 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×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 trends in cohort outcomes between students at shooting and control schools among cohorts who are too old to have been exposed to the shooting. However, by estimating separate interaction coefficients π_d for these "too old" cohorts, we cannot additionally include school

[26] 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). 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.

fixed effects. Therefore, we also estimate models of the form:

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

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×cohort level. The key coefficients of interest are ψ_d, which measure the "double difference" in outcomes, or 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 school shooting does not lead individuals to be more or less likely to continue enrollment in Texas public primary and secondary schools, suggesting that exposure to a shooting does not impact whether a student moves out of state in the short-run. If exposure to a school shooting makes a student less likely to leave Texas in the long-run (e.g., less likely to pursue out-of-state college or labor market opportunities), our analysis will underestimate the effect of school shootings on long-run outcomes such as college attendance, college completion, and labor force participation.

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. 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. Results from event study models 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 interaction terms 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 strongly supports the parallel trends assumption that is required for the validity of our research design. Furthermore, sub-figures (a) and (b) demonstrate that the absence rate and the 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, although the individual event study coefficients are statistically insignificant, sub-figures (d) and (e) suggest that the number of days of disciplinary action and likelihood of school switching, respectively, might increase slightly following 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.[27] 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. That said, the point estimates suggest there may be a 0.25 day increase in the annual number of disciplinary action days ($p-$value=0.145) and a 1.3 percentage point increase in the likelihood of switching to a different Texas public school ($p-$value=0.241) following a shooting.

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, crime-related, and other.[28] For each category of shootings, Figure 4 displays coefficients and associated 95% confidence intervals from estimation of equation (1).[29] Our baseline results for the full set of shootings, first presented in Table 1, are displayed at the top of each subfigure for reference. While many of the confidence intervals overlap across the 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

[27]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.

[28]The CHDS data assigns 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.

[29]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.

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/reduced price lunch in the pre-shooting period.[30] Figure 5 displays the coefficient estimates and associated 95% confidence intervals.[31] 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 the year prior to a shooting. Specifically, the CHDS data provide information on the amount of full-time employment (FTE) allocation of these professionals for each school campus. If a given school psychologist splits his/her time between four schools equally, for example, then each school receives a 0.25 value for the FTE school psychologist allocation. We split schools into categories based on whether they have an above- or below-median number of per-pupil counselors and physicians/nurses. Since only seven out of the 33 shooting schools have any positive FTE allocation of school psychologists or social workers in the year before a shooting, we also split schools based on whether they have any positive FTE allocation of school psychologists or social workers. Figure 6 presents coefficients and 95% confidence intervals from estimation of equation (1) for each school type.[32] We find no evidence of differential impacts based on the presence of health professionals at schools.

[30] 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).

[31] Figure 5 reports raw estimates; the pattern of results is very similar if we instead report estimates relative to sub-group specific outcome means (see Appendix Figure A3).

[32] Since we have fewer clusters in these sub-group analyses, we calculate standard errors using a wild cluster bootstrap.

4.2 Long-Run Effects on Educational and Economic Outcomes

Figure 7 presents results from our long-run models on educational outcomes. In each sub-figure, the graph on the left-hand side presents the coefficients and 95% confidence intervals on the interaction terms 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 interaction terms from estimation of equation (4). An advantage of specification (3) is that we can explicitly examine the possibility of differential cohort trends by estimating coefficients on 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 10th and 11th grades. 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 12th grade 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.

Table 2 presents results from estimation of equation (4) for each of the long-run educational outcomes. Averaging across the coefficients on exposure in grades 10 and 11, we estimate that experiencing a school shooting 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 and 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 coefficients for the "too old" cohorts, while exposure in grades 9 through 11 negatively affects economic well-

being. Averaging across coefficients for exposure in grades 9 through 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 to 26. 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).[33]

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 the long-term educational and labor market outcomes, respectively. As in our analysis of short-run outcomes, we find no evidence of significant differences in long-run impacts across student characteristics. Instead, it appears that the adverse long-term impacts of exposure to shootings 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 results are not driven by 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 the DD coefficients and 95% confidence intervals from estimation of equation (1) using different samples of control schools selected from alternative matching strategies. In particular, we make the following adjustments to the matching strategy: (1) we add average 8th grade standardized test scores

[33] Our results on short-run outcomes using the sample of eight long-run analysis schools are very similar to our baseline estimates. If anything, the magnitudes of the effects on short-run outcomes are somewhat larger in the sample of eight schools. See Appendix Table A2 for the DD estimates, and Appendix Figure A4 for the event study graphs.

for math and reading before the shooting to the set of "fuzzy" match variables;[34] (2) in addition to the variables in (1), we do an exact match on the 10 educational regions in Texas;[35] (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; (4) we use the same matching variables as in our baseline strategy but select four control schools instead of two; (5) we use the same matching variables as in our baseline strategy but match in reverse order; (6) we use same matching variables as in our baseline strategy but 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 for each outcome at the top of each sub-figure. Reassuringly, our results are robust across all of these alternative matching strategies.

4.4 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 school 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 on annual earnings at ages 24–26 (Table 2). Assuming that the average effect of exposure across grades 9–11 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.[36]

It is helpful to compare our effect magnitudes to those found in recent work on exposure to other types of violence. Using administrative data on elementary school students in a

[34] 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.

[35] See: `http://www.txhighereddata.org/Reports/Performance/P16data/TxEdregionslist.pdf`.

[36] To calculate the present discounted value (PDV) of lifetime earnings, we sum over the distribution of earnings from ages 15 to 64 in the 2019 March Current Population Survey (CPS), and assume that earnings are discounted at a 3 percent real rate (i.e., a 5 percent discount rate with 2 percent wage growth) back to age 15 (i.e., around the start of high school). 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).

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 teacher quality and other school resources—which have been shown to impact long-run student outcomes.

Our findings can also be compared to Ang (2020)'s estimates of the impacts of local exposure to police violence. Using data from a large urban school district in the Southwest, 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 to 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, if the control group of students living slightly further away from police brutality is also impacted.

We can further compare our effect sizes to those reported in Bharadwaj et al. (2020)'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 attacks, we note that our study populations and settings are also very different. Children who were attending a youth camp in Utøya at the time of the shooting came from overwhelmingly high socio-economic status backgrounds; moreover, relative to Texas, Norway has a broader

safety net and more affordable higher education. There may therefore be less scope for large, adverse effects on long-run human capital and economic outcomes in the Norwegian setting than for the Texas public school students that we study.

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 through 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 improvements in educational inputs in younger grades.

5 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.[37] 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 their potential harms and for

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

informing policy discussions that compare the costs of different types of gun violence.

This paper draws on comprehensive 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 leverage within-individual and across-cohort variation within matched school groups using difference-in-difference and event study specifications. We find that exposure to a shooting at school leads to increased absences and higher rates of chronic 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. Heterogeneity analyses indicate that the effects are broad and are reach across nearly all of the sub-groups analyzed.

Our results therefore demonstrate that students exposed to a shooting at their school experience substantial reductions in their human capital accumulation and detrimental effects on their labor market outcomes in early adulthood. 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). As shootings may influence other measures of well-being not captured in our data, this is likely a lower bound of the total cost of experiencing a shooting at school in the United States.

The fact that we find large adverse impacts of exposure to shootings on 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 investigate effective interventions that may help affected students overcome the trauma associated with experiencing gun violence at their schools.

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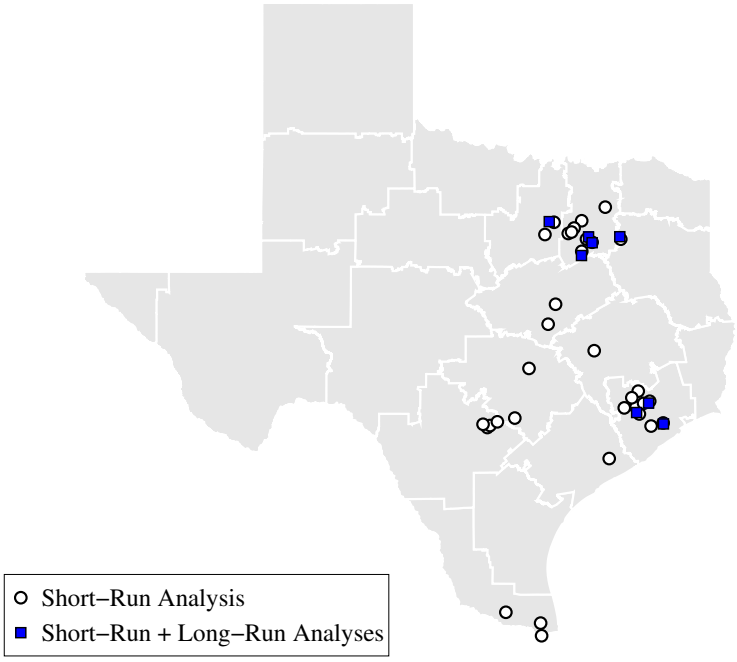
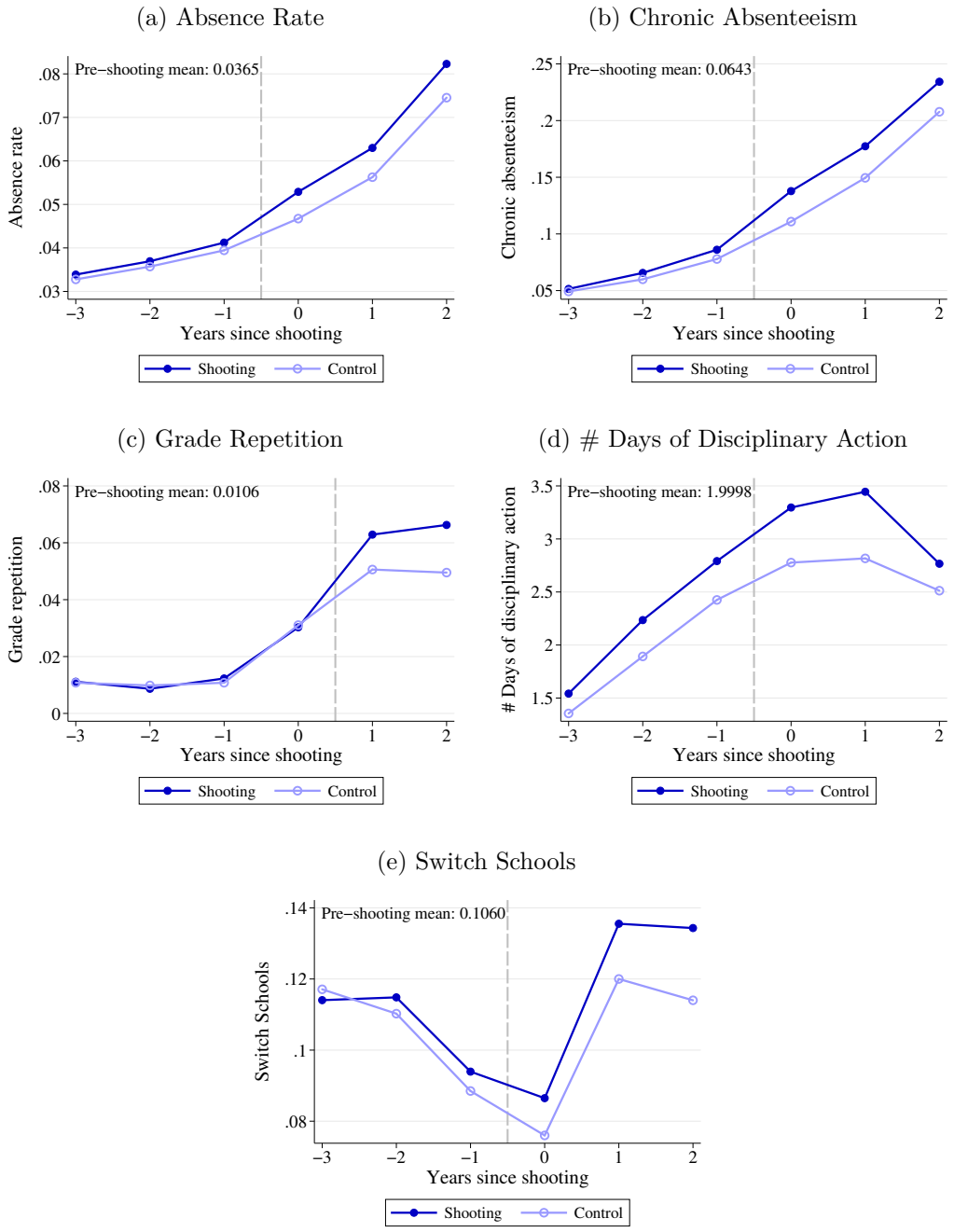
6 Figures and Tables

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



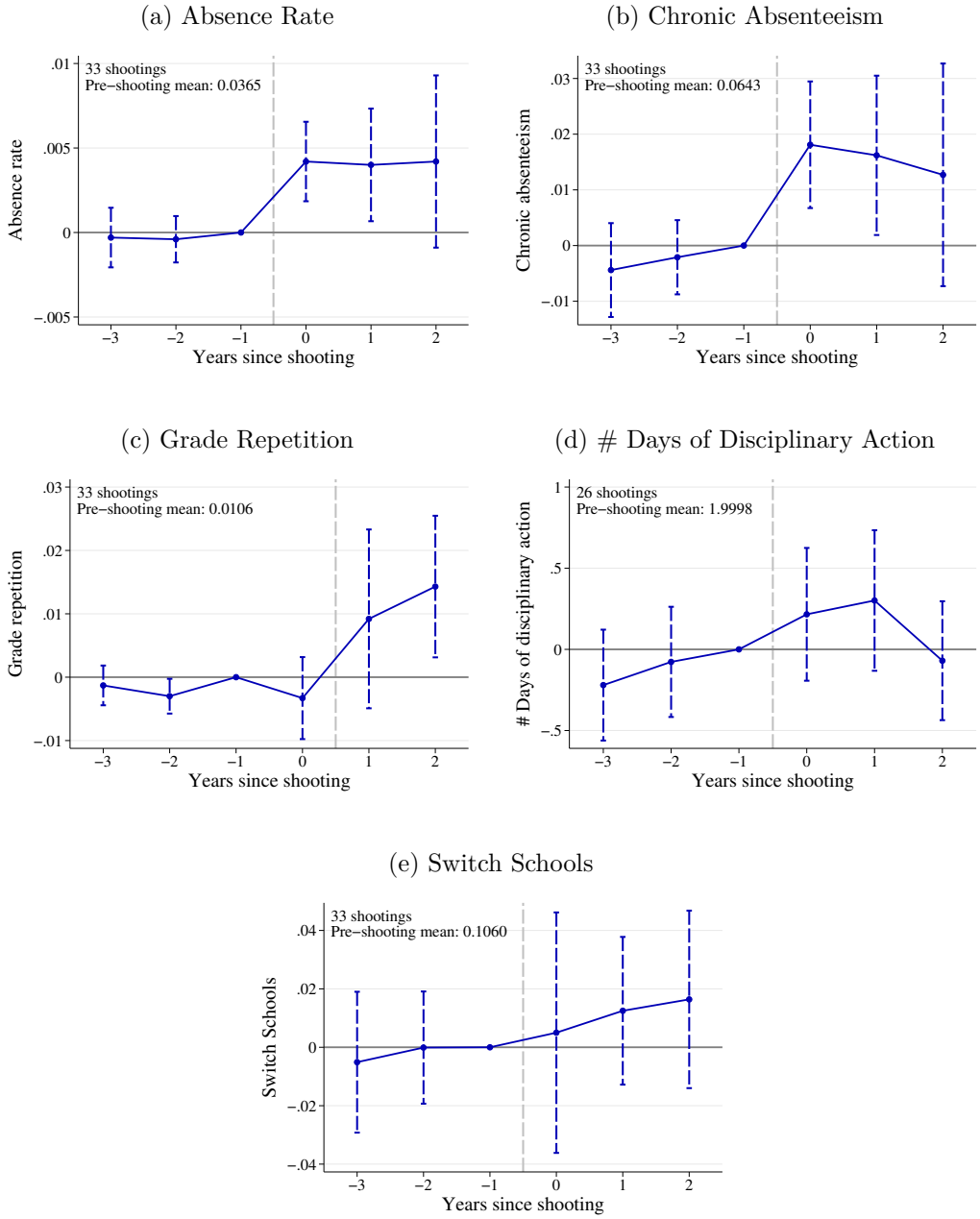
Notes: This figure shows the locations of the 33 (8) shootings at Texas public schools used in our short-run (long-run) analysis. These shootings occurred during school hours and on school grounds between the academic years 1995–1996 and 2015–2016. The data are compiled from the Center for Homeland Defense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

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



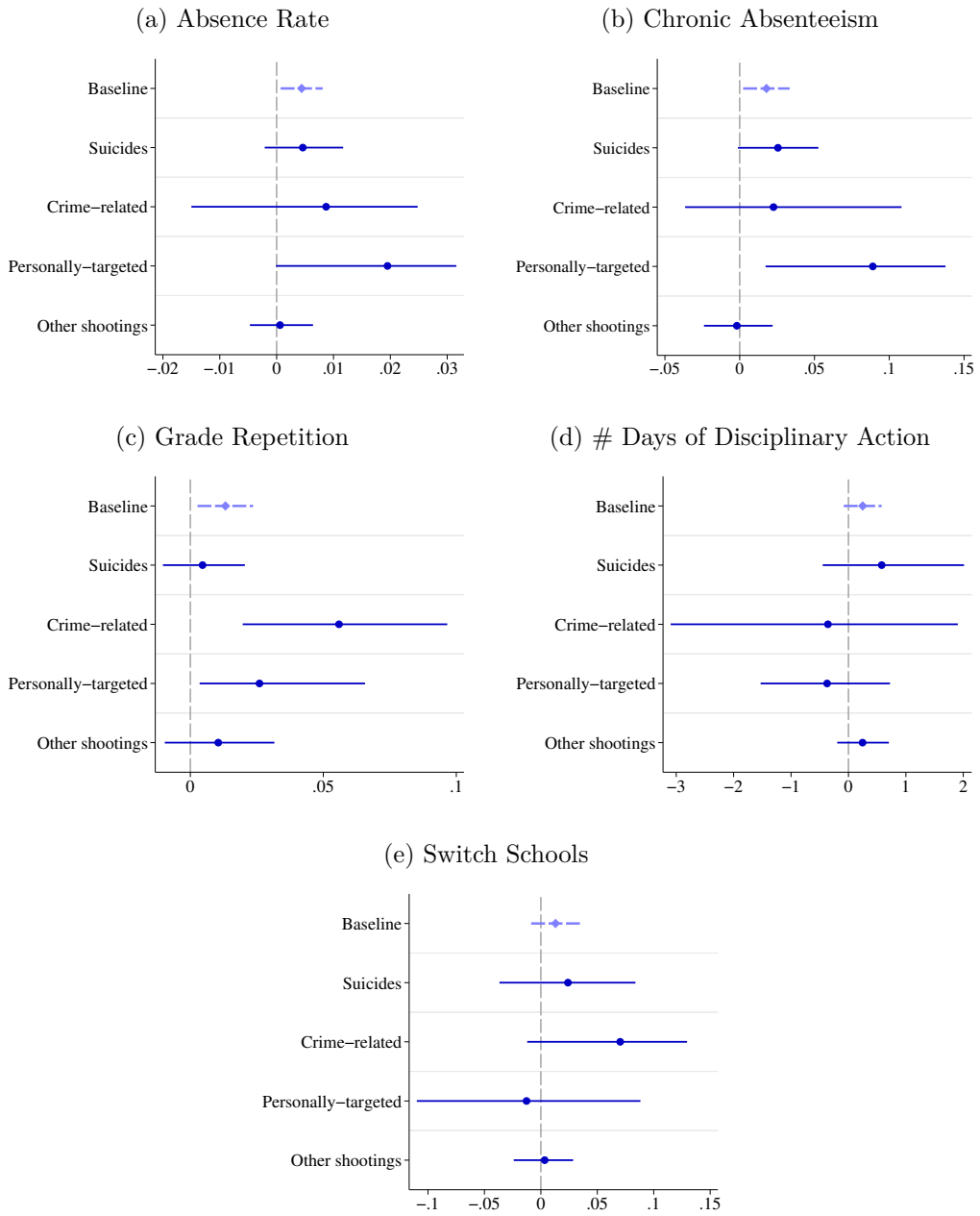
Notes: These figures plot raw trends in our short-run outcomes over the six years surrounding a school shooting, separately for treatment and matched control schools. Sub-figures (a)–(c) and (e) include 33 shooting and 66 control schools; since data on disciplinary actions is not available for our entire 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).

Figure 3: Event Study Estimates of Effects of School Shootings on Short-Term Outcomes



Notes: These figures present output from estimation of equation (2). In particular, they plot the coefficients and 95% confidence intervals on the interaction terms 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 4: Heterogeneity in Effects of School Shootings on Short-Term Outcomes by Shooting Type

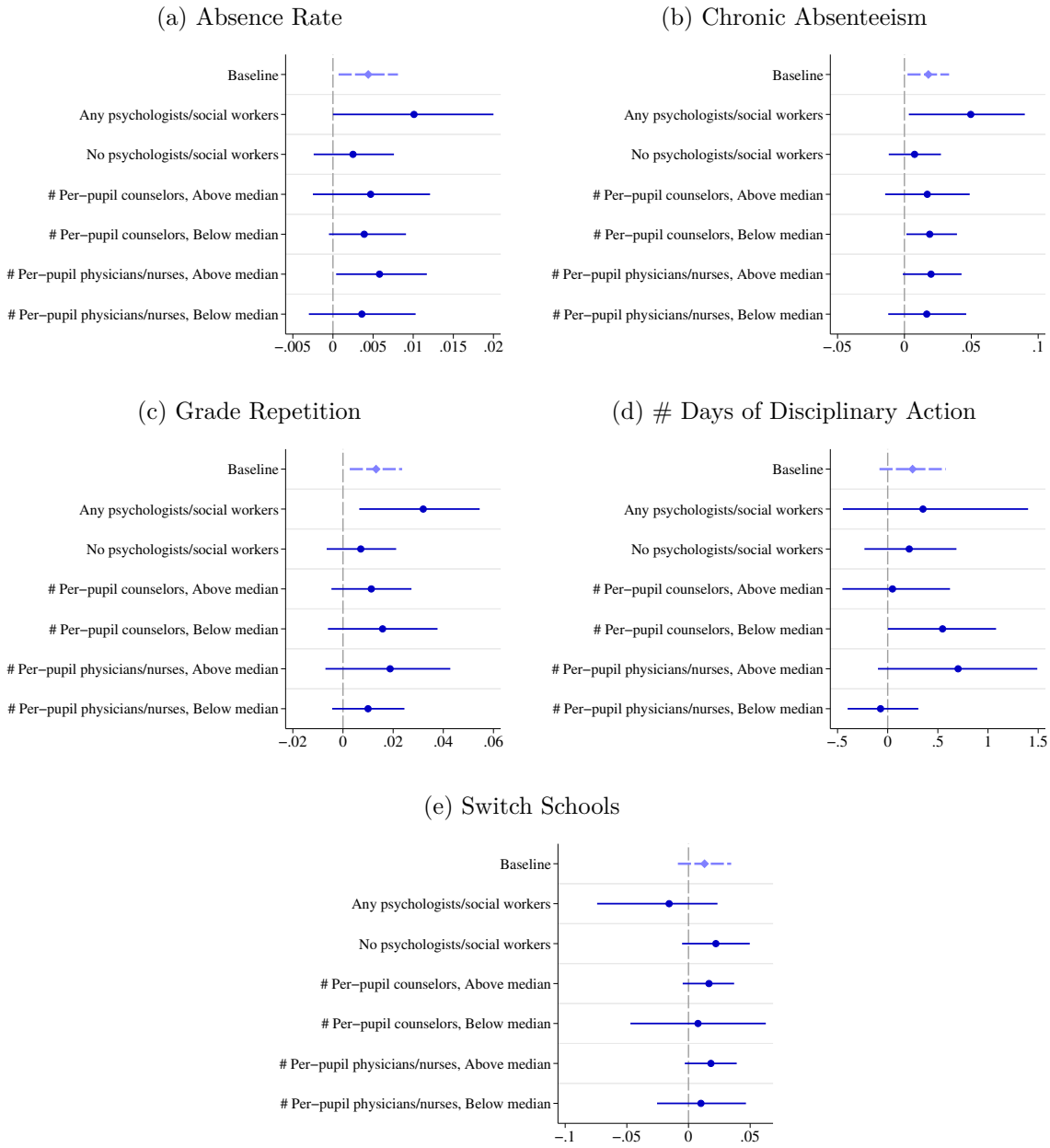


Notes: These figures present the DD coefficients and 95% confidence intervals (based on a wild cluster bootstrap) from estimation of equation (1) for the shooting type denoted on the y-axis. The shooting categories follow those suggested by Levine and McKnight (2020b) and are mutually exclusive. Our baseline DD estimates—which use the entire sample of shootings—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 5: Heterogeneity in Effects of School Shootings on Short-Term Outcomes by Student Characteristics

Notes: These figures present the DD coefficients and 95% confidence intervals from estimation of equation (1) for students belonging to the sub-group denoted on the y-axis. 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 DD 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.

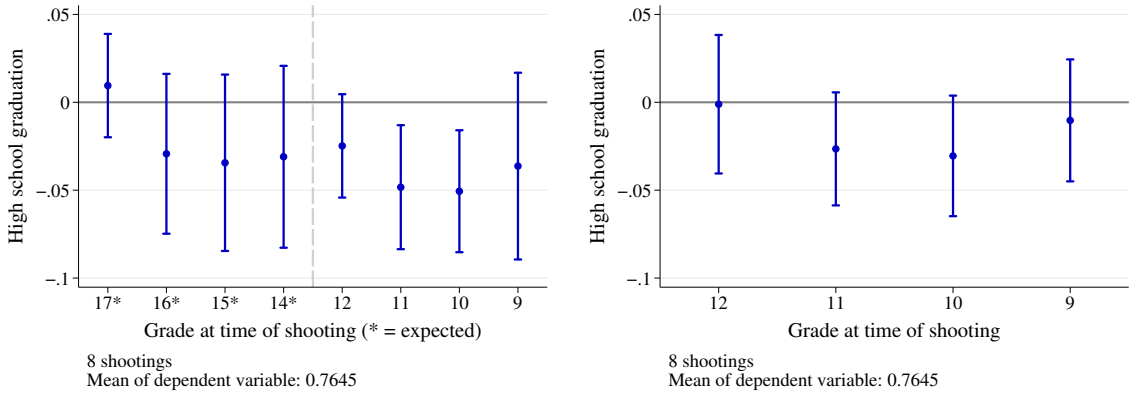
Figure 6: Heterogeneity in Effects of School Shootings on Short-Term Outcomes by School Mental Health Resources



Notes: These figures present the DD coefficients and 95% confidence intervals (based on a wild cluster bootstrap) from estimation of equation (1) for shootings at schools with differing availability of health professionals. Our baseline DD estimates—which use the entire sample of 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 7: Effects of School Shootings on Long-Term Educational Outcomes

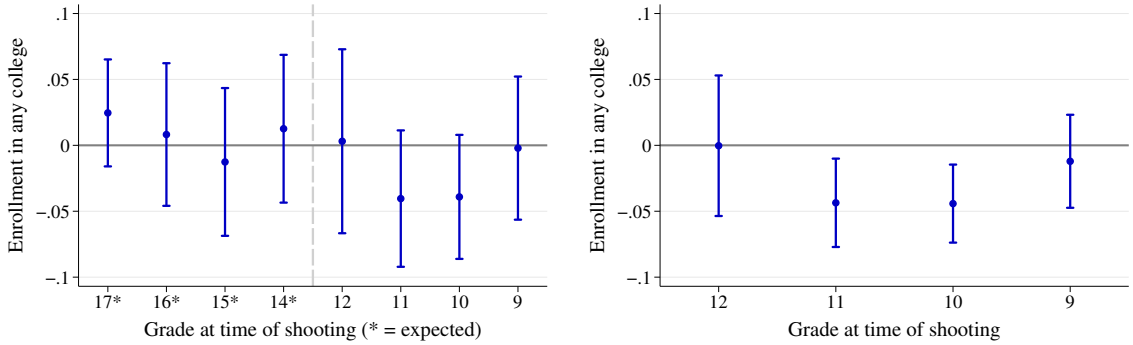
(a) High School Graduation



8 shootings
Mean of dependent variable: 0.7645

8 shootings
Mean of dependent variable: 0.7645

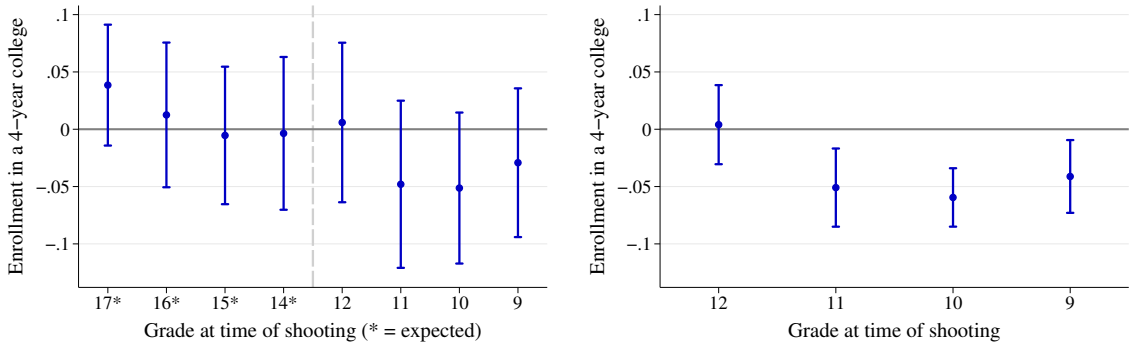
(b) Enrollment in Any College



8 shootings
Mean of dependent variable: 0.4642

8 shootings
Mean of dependent variable: 0.4642

(c) Enrollment in a 4-Year College

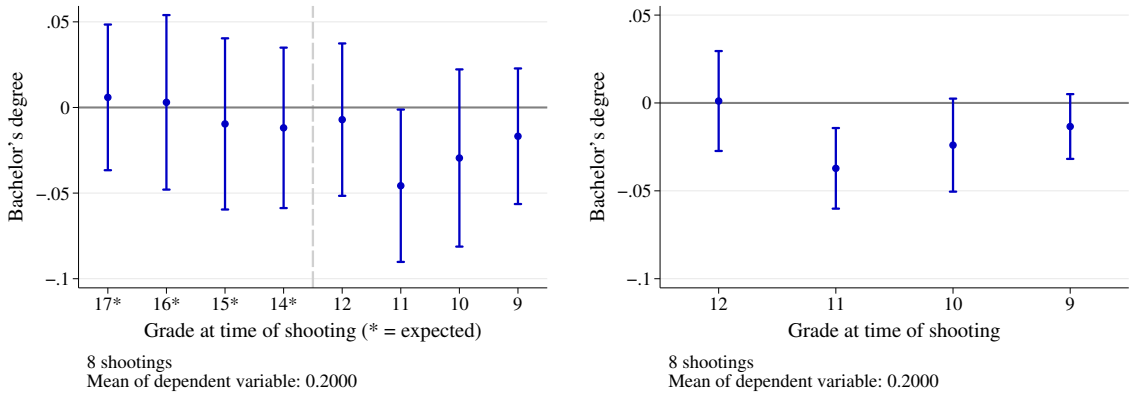


8 shootings
Mean of dependent variable: 0.3212

8 shootings
Mean of dependent variable: 0.3212

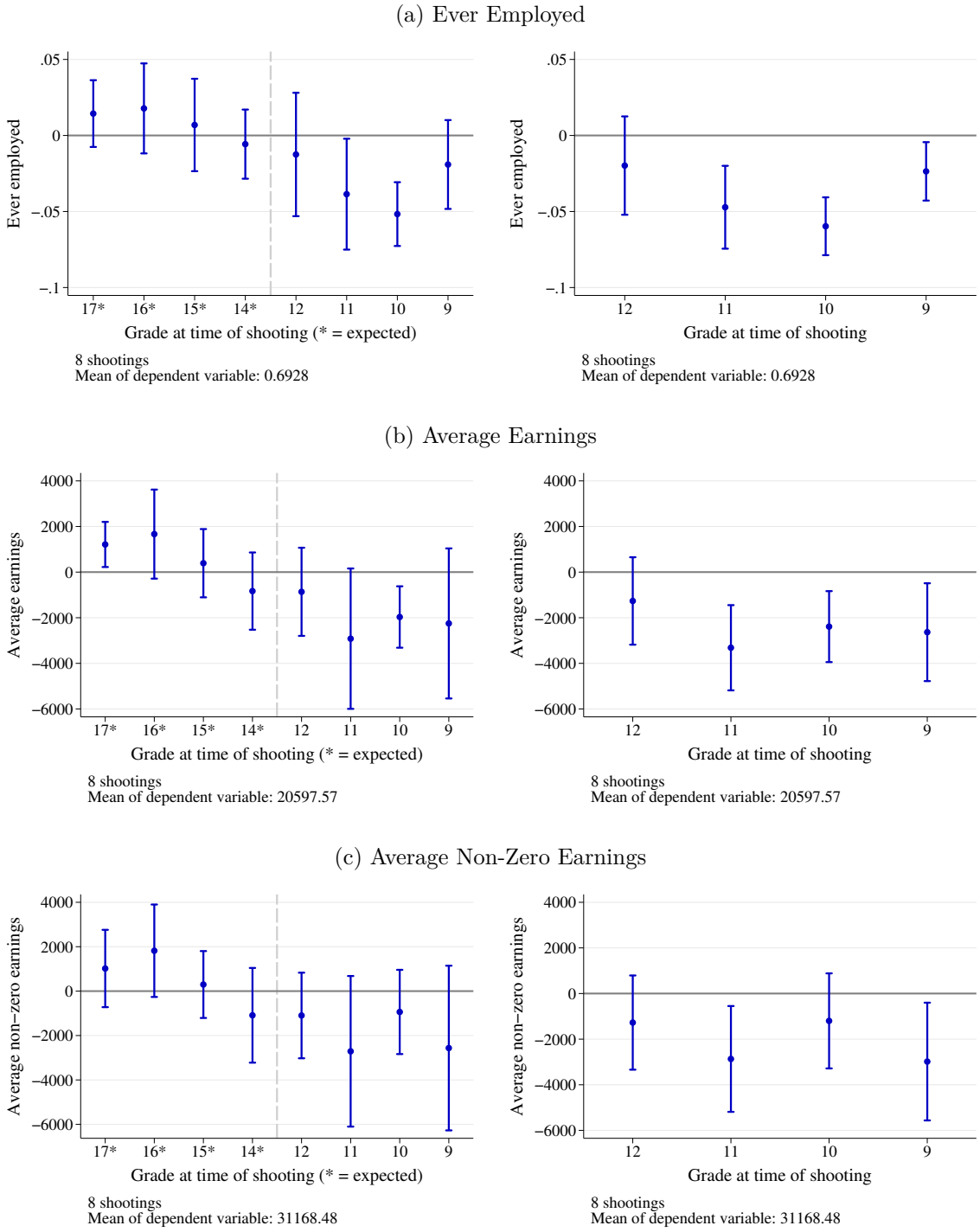
Figure 7: Effects of School Shootings on Long-Term Educational Outcomes (continued)

(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 interaction terms between the indicator denoting a shooting school and the 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: Effects of School Shootings on Labor Market Outcomes



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 interaction terms between the indicator denoting a shooting school and the 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.

Table 1: Effects of Shootings on Students' Short-Run 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 mean of dependent variable	0.0365	0.0643	0.0106	1.9998	0.1060
Student x academic 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 the DD coefficients, standard errors (in parentheses), and $p-$values (in brackets) from estimating model (1). All regressions include individual fixed effects, as well as a full set of match-group by academic year interactions. Because the earliest academic year where grade repetition could be affected is the year after the shooting, we exclude the shooting year itself from the $Post_t$ indicator variable in the specification that uses grade repetition as the outcome. Standard errors are clustered on the school level.

Table 2: Long-Run Effects of Shootings on Students' Educational Outcomes

	(1)	(2)	(3)	(4)
	Graduate HS	Enroll Any Col	Enroll 4yr Col	Bachelor's Degree
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]	[0.000]	[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]
Mean of dependent variable	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 the coefficients, standard errors (in parentheses), and $p-$values (in brackets) on the interaction terms between the shooting school indicator and the cohort indicators from estimating model (4). All specifications control for 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 on the school by cohort level.

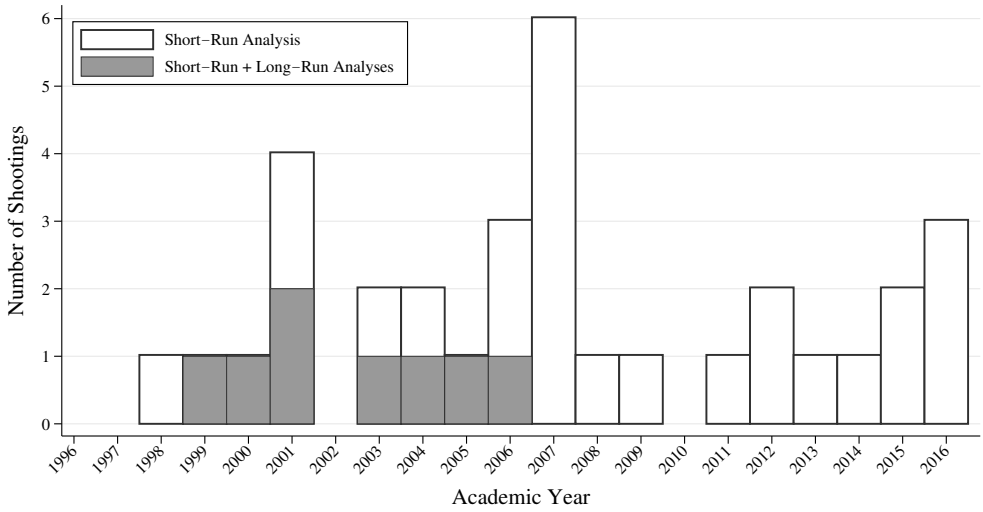
Table 3: Long-Run Effects of Shootings on Students' Labor Market Outcomes

	(1)	(2)	(3)
	Ever Employed	Earnings	Non-Zero Earnings
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)
	[0.000]	[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]
Mean of dependent variable	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: See notes under Table 2.

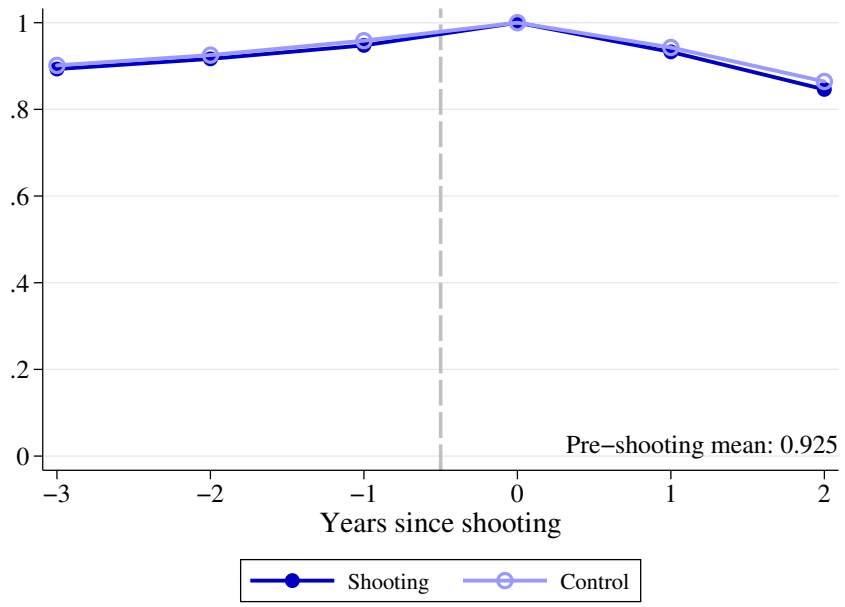
A Appendix Figures

Figure A1: Number of Shootings by Academic Year in Texas Public Schools: 1995–1996 to 2015–2016 Academic Years



Notes: This figure plots the number of school shootings at Texas public schools per academic year over our analysis time period. The subset of shootings used in the analysis of long-run outcomes is denoted with gray-filled bars. The data on school shootings are compiled from the Center for Homeland Defense and Security (CHDS) K-12 school shooting database and the *Washington Post* school shootings database.

Figure A2: Trends in Sample Attrition Rates in Treatment Versus Control Schools

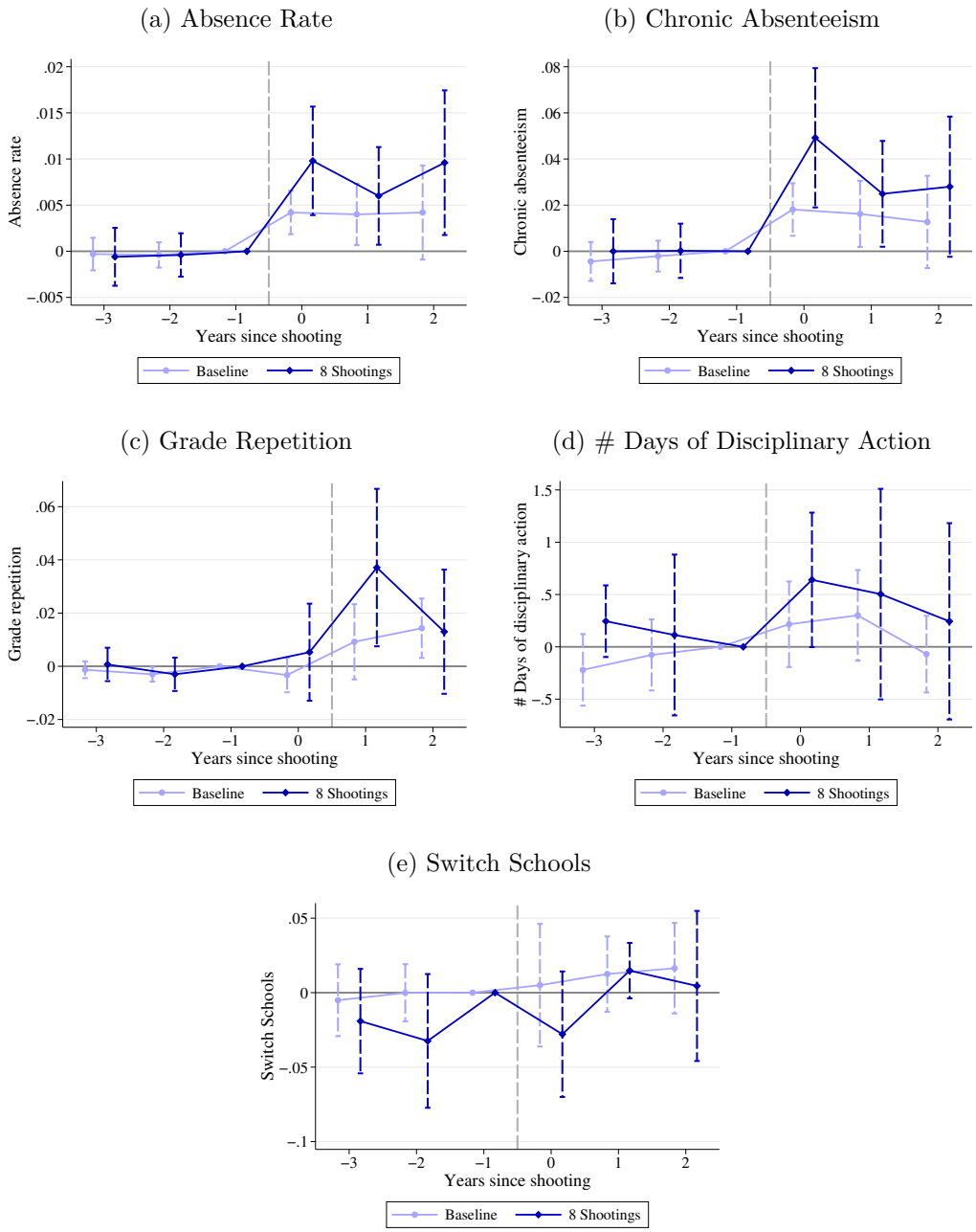


Notes: This figure uses a sample of all students who were enrolled in the 33 shooting and 66 control schools in the academic semester during which the shooting takes place (denoted by time 0 on the $x-$axis on the graph). It then plots the share of these students who are observed in our data (i.e., they are enrolled in any Texas public school) in each of the three years before the shooting and two years after the shooting, separately for students at treated (shooting) and control schools.

Figure A3: Heterogeneity in the Effects of School Shootings on Short-Term Outcomes, by Student Characteristics, Normalized Relative to Sub-Group Means

Notes: These graphs present the DD coefficients and 95% confidence intervals from estimating model (1), which are scaled relative to the baseline outcome means for each sub-group. See notes under Appendix Figure 5.

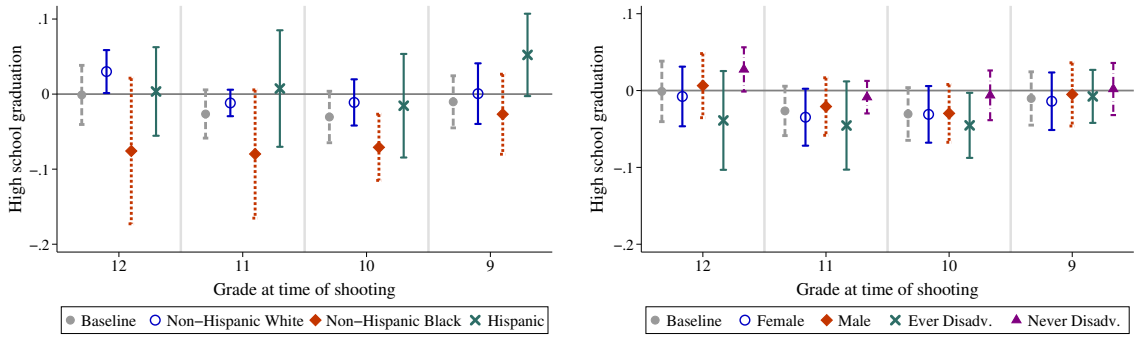
Figure A4: Event-Study Estimates of Effects of School Shootings on Short-Term Outcomes, Comparing Results from Baseline Models and Models Using Long-Run Analysis Schools



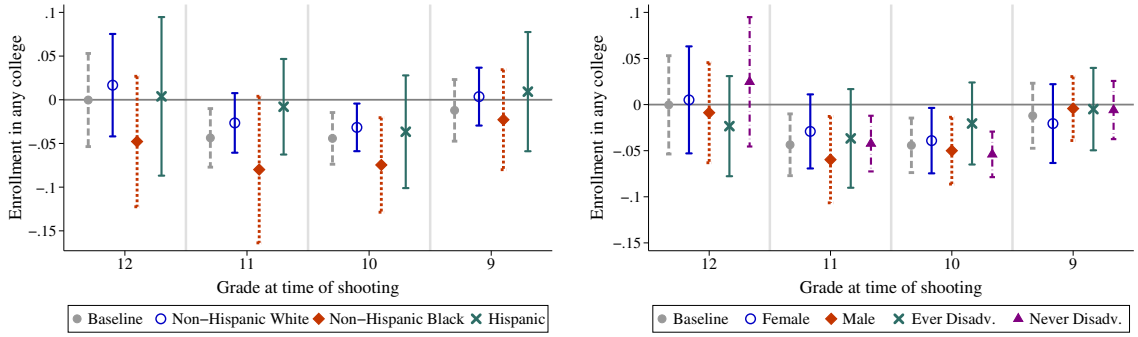
Notes: See notes under Figure 3. Here, we overlay our baseline event-study estimates with event-study estimates that use a sample of the 8 shooting schools and their matched control schools that we use in the long-run analysis.

Figure A5: Heterogeneity in Effects of School Shootings on Long-Term Educational Outcomes

(a) High School Graduation



(b) Enrollment in Any College



(c) Enrollment in a 4-Year College

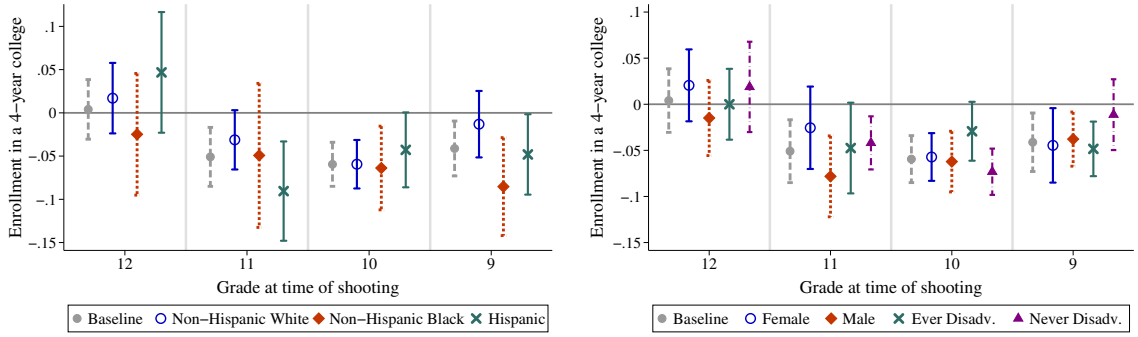
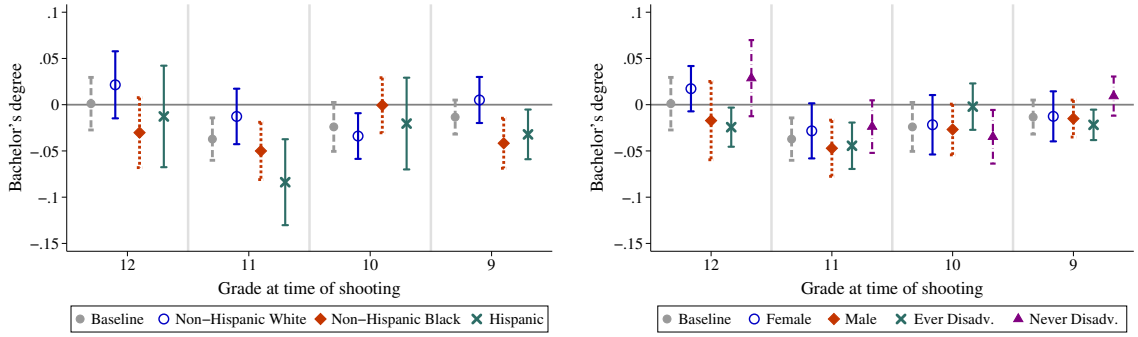


Figure A5: Heterogeneity in Effects of School Shootings on Long-Term Educational Outcomes (continued)

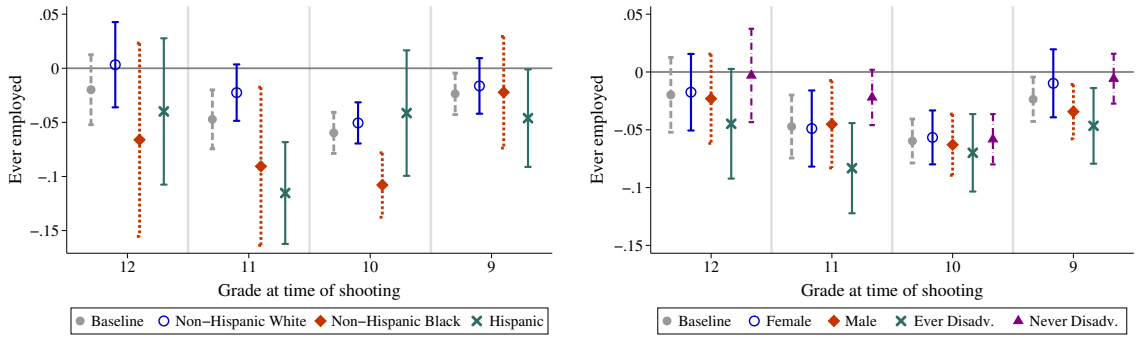
(d) Bachelor's Degree



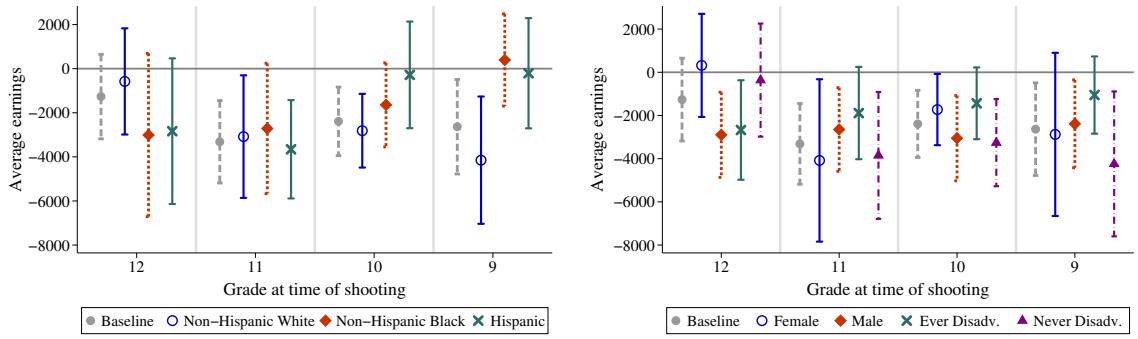
Notes: In each sub-figure, the graphs present the coefficients and 95% confidence intervals on the interaction terms between the shooting school indicator and the cohort indicators from estimating model (4), separately for students from different sub-groups denoted in the legends. The baseline estimates for the whole sample are depicted with gray dashed lines. See notes under Figure 7 for more details on the specifications. "Ever disadvantaged" refers to students who ever receive free or reduced-price lunch, while "never disadvantaged" refers to students who never receive free or reduced-price lunch in our data. 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).

Figure A6: Heterogeneity in Effects of School Shootings on Labor Market Outcomes

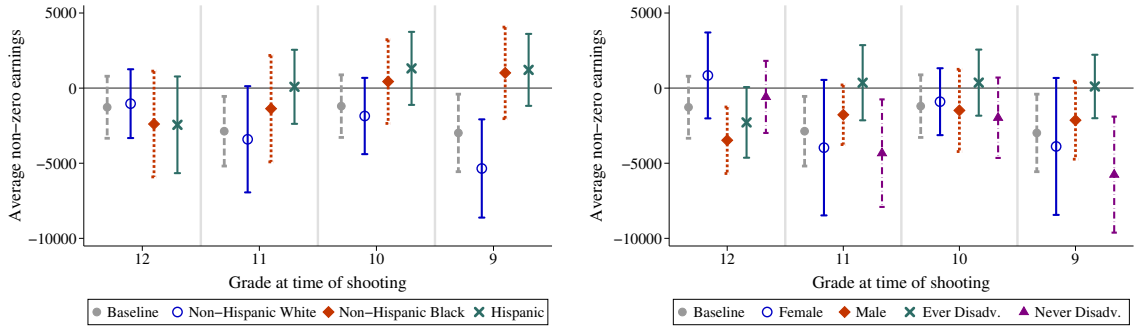
(a) Ever Employed



(b) Average Earnings

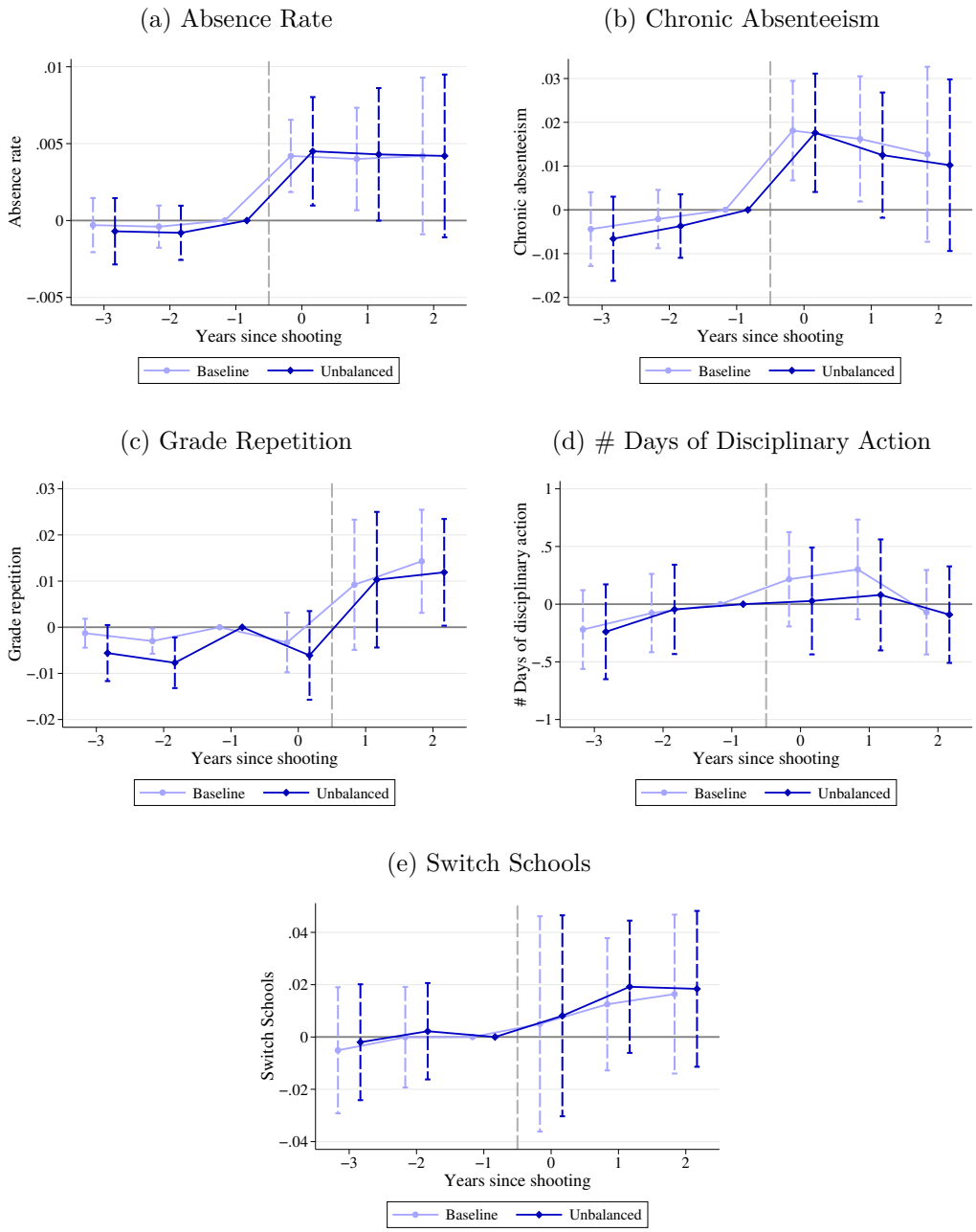


(c) Average Non-Zero Earnings



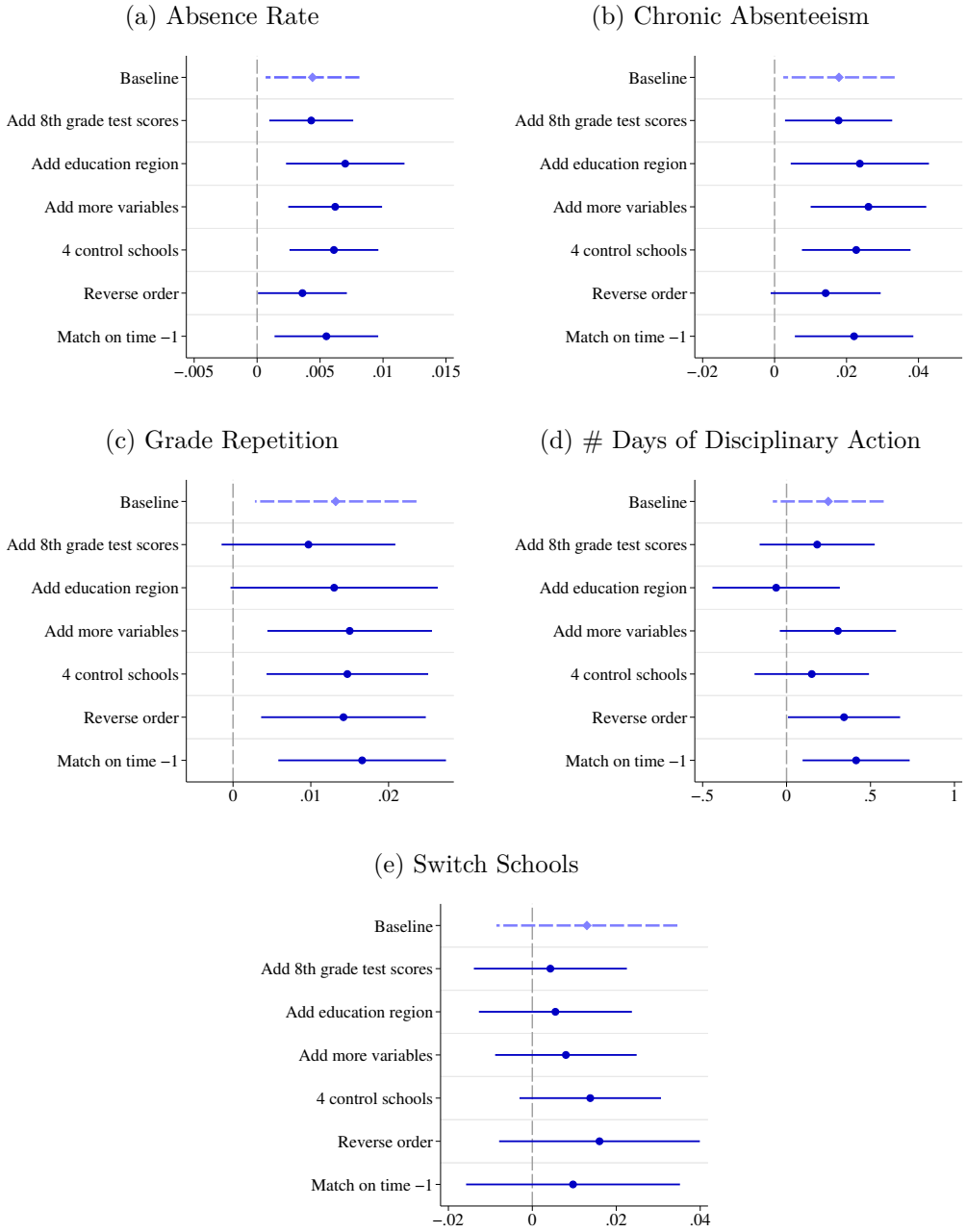
Notes: See notes under Appendix Figure A5.

Figure A7: Event-Study Estimates of Effects of School Shootings on Short-Term Outcomes, Comparing Results from Balanced and Unbalanced Panels



Notes: See notes under Figure 3. Here, we overlay our baseline event-study estimates with event-study estimates that use an unbalanced sample of students, in which we do not make any restrictions on the number of years that students are observed in the data.

Figure A8: Robustness of the Short-Run Results to Alternative Matching Strategies



Notes: These graphs present the DD coefficients and 95% confidence intervals from estimating model (1) using different alternative samples of schools based on alternative matching strategies of the 33 shooting schools to their two nearest-neighbor control schools. The baseline DD estimate using our baseline matching algorithm is presented at the top of each graph. These regressions control for individual fixed effects and match-group-by-year fixed effects. Standard errors are clustered on the school level.

B Appendix Tables

Table A1: Means of School Characteristics

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 means of school characteristics, separately for high schools and non-high schools in our data, in Panels A and B, respectively. Panels A.1 and B.1 present means of characteristics on which we do an exact match separately for the schools with shootings and their matched control schools in columns (1) and (2), respectively. These variables measured in the first six-week grading period of the academic year of the shooting. For all Texas public schools in column (3), we present averages of these variables calculated over academic years 1993-1994 to 2017–2018. In column (3), schools with any missing variables, except for campus type (which is only available for schools that existed in the last year of the data), are dropped. Panels A.2 and B.2 present the means for characteristics on which we do a "fuzzy match".

Table A2: Effects of 8 Long-Run Analysis Shootings on Students' Short-Run Outcomes

	Absence Rate (1)	Chronic Absenteeism (2)	Grade Repetition (3)	# Days of Disc.Act (4)	Switch Schools (5)
A. Baseline Estimates					
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 mean of dependent variable	0.0365	0.0643	0.0106	1.9998	0.1060
Student x academic 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. Effects of 8 Long-Run Analysis 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 mean of dependent variable	0.0366	0.0622	0.0083	2.5953	0.0719
Student x academic 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: Panel A repeats the baseline DD estimates from Table 1. Panel B presents the DD coefficients, standard errors (in parentheses), and $p-$values (in brackets) from estimating model (1) using only the 8 shooting schools and their matched control schools that are used in the long-run analysis. All regressions include individual fixed effects, as well as a full set of match-group by academic year interactions. Standard errors are clustered on the school level.