

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

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Abstract

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

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1 Introduction

Gun violence in the United States has been rising over the past two decades and is significantly higher than in other high-income nations ([IHME, 2021; JHU, 2022](#)).¹ Gun violence that takes place *at schools*—which is especially concerning for parents, educators, and policymakers—has not been immune to this trend: the number of shootings at U.S. schools doubled between 2000 and 2019, with more than 100,000 American children attending a school at which a shooting took place in 2018 and 2019 alone.² Although mass shootings at schools tend to receive significant media attention, 95 percent of shootings at schools between 2018 and 2019 resulted in fewer than two deaths, and nearly three-quarters of shootings led to no fatalities at all ([Riedman and O'Neill, 2020](#)). Despite the prevalence of these less highly publicized acts of gun violence at schools—*institutions whose central purpose is to promote human capital development*—surprisingly little is known about the impacts of these events on the educational and labor market trajectories of surviving students.

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

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

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

find that a shooting leads to an increase in the number of leadership staff and the turnover rate among teachers and teaching support staff, highlighting that school gun violence can impact many aspects of the school environment.

We begin by introducing a conceptual framework that draws on the existing interdisciplinary literature on exposure to childhood trauma and outlines the mechanisms through which school shootings can impact student outcomes. Like other types of violence and trauma, school shootings can affect students by influencing their own health and well-being through neurobiological and stress-related channels. But compared to traumatic events that take place in students' neighborhoods or homes, shootings at school may be particularly disruptive to students' human capital accumulation because students are expected to learn in the environment in which the trauma occurred. Moreover, any individual-level effects on students may be tempered or amplified through peer effects within shooting-exposed schools: while the collective experience of trauma could generate more peer and community support for survivors and mitigate harmful effects, disruptions caused by other shooting-exposed students might instead exacerbate the adverse impacts of a student's own trauma. Lastly, compared to violence in other settings, shootings that occur at schools may cause greater disruption to students' learning by influencing other educational inputs, such as teaching quality, classroom resources, and continuity of instruction.

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

on institutional and student characteristics.

We find that shootings at schools adversely impact the educational outcomes of exposed students in the short run. 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 (27.6 percent) increase in the likelihood of being chronically absent, and a 1.3 percentage point (126.4 percent) increase in the likelihood of grade repetition. We find no significant effects on the frequency of disciplinary actions such as suspensions, expulsions, or in-school detentions. 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.

To examine effects on students' long-run outcomes, we make use of linkages between the individual-level public school records and college enrollment and graduation files from the Texas Higher Education Coordinating Board as well as employment and earnings data from the Texas Workforce Commission. We study the impacts of the universe of eight shootings that took place at Texas public high schools between 1998 and 2006 on individual-level educational and economic outcomes through age 26. Since these long-term outcomes are only observed after a shooting, we cannot measure within-student changes in them. We therefore compare outcomes among cohorts of exposed students to outcomes among cohorts who attended the same schools before the shooting occurred.³ As in the short-run analysis, we compare these differences in cohort outcomes to the analogous differences among students at matched control schools. We examine the statistical significance of our long-run estimates using permutation tests in addition to inference based on conventional standard errors. We further show that our results are unlikely to be influenced by differential attrition.

We find that shootings at schools have lasting implications for the educational and labor market trajectories of exposed students. Students who are exposed to a shooting at their school in grades 10–11 are 2.7 percentage points (3.4 percent relative to the control school mean) less likely to graduate high school, 4.0 percentage points (6.2 percent) less likely to enroll in any college, 5.0 percentage points (13.3 percent) less likely to enroll in a 4-year college, and 3.5 percentage points (14.6 percent) less likely to obtain a bachelor's degree by age 26.

³Our use of a shorter time window for event inclusion in the long-run analysis is necessary to ensure that we observe all outcomes for both the cohorts enrolled at the time of the shooting as well as comparison cohorts who were enrolled five years before the shooting took place.

At ages 24–26, students exposed to shootings in grades 9–11 are 3.0 percentage points (3.8 percent) less likely to be ever employed and 3.8 percentage points (5.6 percent) less likely to be employed for at least four consecutive quarters. Further, exposed students have \$2,622 (11.3 percent) and \$2,422 (7.8 percent) lower average annual earnings at ages 24–26 unconditional and conditional on working, respectively. Our estimates imply that shootings at schools lead to a \$100,439 reduction (in 2018 dollars) in the present discounted value of lifetime earnings per shooting-exposed student. Additional analyses suggest that at most a quarter of the earnings effect can be explained by the estimated reduction in college completion, suggesting that shootings have impacts on labor market outcomes that operate through channels beyond educational attainment.

We explore heterogeneity in the impacts of shootings at schools by student characteristics, school resources, and type of shooting. When considering student characteristics such as race and gender, we find that the detrimental consequences of school shootings are relatively universal, with all sub-groups being affected. That being said, non-Hispanic Black students and those who receive free or reduced-price lunch experience relatively larger adverse effects on some outcomes, suggesting that shootings at schools may exacerbate pre-existing disparities in outcomes between more and less advantaged groups. We also conduct heterogeneity analysis based on whether schools had a higher or lower number of various health professionals (e.g., school counselors, psychologists, social workers, and nurses) and other types of staff members (e.g., teachers, teaching support staff, and school leadership) per student in the year before the shooting and find that access to different staff resources does not appear to offset the negative impacts of shootings. We further do not find evidence of significant heterogeneity in effects across shooting types as categorized by [Levine and McKnight \(2020b\)](#).

Additionally, we analyze the impacts of shootings at schools on the employment and retention of teachers and other school staff. Examining effects on the number of personnel, we find that schools increase the number of full-time equivalent (FTE) school leadership staff by an average of 0.55 per 1,000 students (18.9 percent relative to the pre-shooting mean) following a shooting on school grounds. This effect is driven predominately by an increase in the number of assistant principals, who are the staff typically responsible for dealing with safety and disciplinary issues at schools. While we find no effects on the total number of FTE teachers,

teaching support staff, or social support staff per 1,000 students, we observe an increase in the turnover rate among teachers and teaching support staff in the years following a shooting. Disruptions to the continuity of instruction could therefore contribute to the negative effects on student outcomes that we find.

Finally, we explore spillover effects on students at neighboring schools. For both short- and long-run outcomes, we find some evidence that the adverse impacts of shootings extend to students at the nearest schools. However, the effects of shootings at schools fade with distance and are not detectable beyond the fourth closest school. Moreover, impacts on students at the closest schools are notably smaller than the estimated impacts on students attending the school where the shooting occurred and are in line with magnitudes documented in prior work on the impacts of community-level gun violence (e.g., [Bor et al., 2018](#); [Ang, 2020](#); [Koppensteiner and Menezes, 2021](#); [Brodeur and Yousaf, 2022](#)). These findings are therefore consistent with the idea that shootings at nearby schools are more comparable to shootings in the community than to a shooting at one's own school.

Our study contributes to three strands of literature. The first is a small but growing set of studies on the impacts of school shootings on student outcomes.⁴ Recent work documents that school shootings can have detrimental effects on the mental health ([Rossin-Slater et al., 2020](#); [Levine and McKnight, 2020a](#))⁵ and short-run educational outcomes ([Beland and Kim, 2016](#); [Poutvaara and Ropponen, 2018](#); [Levine and McKnight, 2020a](#)) of surviving youth. We add to this literature in three ways. First, while previous work has predominantly relied on school- or district-level data, our use of individual-level data enables us to identify students exposed to each event, precisely estimate the impacts of this exposure over time, and investigate heterogeneity in these impacts across student, school, and shooting characteristics. Moreover, while previous studies have focused largely on near-term effects of shootings, our linked educational and labor market data provide a unique opportunity to examine the effects of shootings up to a decade after they occur.⁶ Finally, while attention is often focused on

⁴A related literature examines the determinants of gun violence at schools; see, e.g., [Pah et al. \(2017\)](#) and [Livingston et al. \(2019\)](#).

⁵For recent overviews of the broader interdisciplinary literature on the mental health impacts of school and mass shootings, see [Lowe and Galea \(2017\)](#), [Travers et al. \(2018\)](#), [Iancu et al. \(2019\)](#), and [Rowhani-Rahbar et al. \(2019\)](#).

⁶Recent work by [Deb and Gangaram \(2023\)](#) studies the impacts of exposure to school shootings at the county level. They use survey data from the Behavioral Risk Factor Surveillance System and analyze whether

indiscriminate mass shootings at schools that result in numerous fatalities (e.g., Columbine, Sandy Hook, Parkland, Uvalde), mass shootings are rare, and most shootings that take place at schools result in no deaths. Our analysis captures the effects of gun violence that is more common in schools and may be more comparable to other forms of violence to which children are frequently exposed.⁷

Our work further contributes to a growing literature on the effects of gun violence more generally. Recent work by Bharadwaj et al. (2022) finds that exposure to the 2011 massacre on the island of Utøya in Norway led to adverse impacts on teenage survivors' test scores, health visits, educational attainment, and earnings. Other work has documented how community violence, such as police killings (Ang, 2020) and other homicides (Jarillo et al., 2016; Koppensteiner and Menezes, 2021), impact educational outcomes, as well as how mass shootings affect community-wide mental health (Soni and Tekin, 2023) and local economic factors (Brodeur and Yousaf, 2022). Our work shows that less deadly shootings—which are 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 an expansive literature investigating the long-run effects of childhood circumstances and educational inputs. Prior work has investigated the impacts 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

a school shooting that occurred in the adult respondent's current county of residence when they were an adolescent (i.e., when the respondent may have been living elsewhere) influences risky behaviors, mental health, and educational and labor market outcomes. They find evidence of reductions in earnings, a positive effect on college attendance, and no impacts on mental health.

⁷In fact, no mass shootings occurred in the Texas public school system during the two decades spanned by our sample of shootings. In this way, our work complements Levine and McKnight (2020a)'s study of the impacts of the Sandy Hook Elementary School shooting—a large mass shooting event—on student absences and test scores. Our combined body of evidence suggests that all types of shootings at schools have detrimental impacts on survivors' educational outcomes.

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 introduces a conceptual framework outlining how school shootings can affect student outcomes. Section 3 provides additional details on the data, and Section 4 outlines our empirical strategies. Section 5 provides main results, heterogeneity analyses, robustness exercises, and extensions. Section 6 provides a discussion and concludes.

2 Conceptual Framework

How might gun violence at schools affect students’ human capital development and later economic outcomes? In this section, we present a conceptual framework that outlines potential mechanisms through which shootings at schools might affect student outcomes in the short and long run. We structure our discussion around three channels: (1) effects on students’ own health and well-being, (2) effects on their peers and parents, and (3) effects on school-level resources and educational inputs. Throughout this discussion, we highlight how shootings at schools both relate to and differ from other forms of trauma previously studied in the literature.

Individual students. Shootings at schools might affect student outcomes by influencing their own health and well-being. This mechanism is consistent with a large interdisciplinary literature documenting that trauma affects children through various biological pathways.⁸ Numerous studies show that exposure to adverse childhood experiences—such as physical, emotional, and sexual child abuse; domestic violence between parents or other household members; and parental substance abuse—is associated with higher rates of mental and physical health problems in later childhood and adulthood (see, e.g., Danese et al., 2009; Chartier et al., 2010; Burke et al., 2011; Kerker et al., 2015). Focusing on school shootings specifically,

⁸See, e.g., De Bellis, 2001; Garbarino, 2001; Perry, 2001; Carrion et al., 2002, 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.

recent work shows that such events have sizable and persistent negative impacts on youth mental health (Travers et al., 2018; Iancu et al., 2019; Rowhani-Rahbar et al., 2019; Levine and McKnight, 2020a; Rossin-Slater et al., 2020).⁹ As poor mental health during childhood can lead to lower educational attainment and worse economic prospects in adulthood (Currie and Stabile, 2006; Goodman et al., 2011), exposure to a school shooting might lead to worse long-term outcomes by adversely affecting a student's mental health.

Shootings at schools may be particularly detrimental for student mental health due to the locations in which they occur and the attention that they receive. Educators have long conceived of schools as "safe spaces," such that children who are exposed to violence in their homes or in their neighborhoods can nevertheless feel safe and supported at school (Fisher et al., 2019). A school shooting fundamentally disrupts this notion and causes a loss of trust in the institution's ability to protect and support children.¹⁰ This loss of trust is likely to occur even after shootings that result in no deaths or injuries, as such events can remind students and their families that much more deadly shootings—like the widely publicized events at Columbine, Sandy Hook, Parkland, and Uvalde—are possible at their own schools (Lowe and Galea, 2017).

Shootings at schools may also be especially disruptive to human capital accumulation because students continue to be exposed to—and are expected to learn in—the environment in which the trauma occurred. Students spend a significant amount of their time in school, and our findings indicate that students frequently continue attending the same school after a shooting. This means that most students cannot easily avoid the site of the shooting, which can make it more difficult to overcome the associated trauma (Trigg, 2009; Schonfeld and Demaria, 2020). Moreover, compared to other locations with gun violence such as neighborhood streets, shootings at schools may be more disruptive to learning because schools are environments in which human capital acquisition is expected to take place. Put simply, it may be uniquely

⁹The relatively universal impacts of exposure to school shootings on student outcomes that we uncover are in contrast to effects of positive educational interventions that are often larger among students from disadvantaged backgrounds (see Cascio, 2015 for an overview). This might be because poor mental health has the potential to impact all populations whereas positive interventions often only lead to improvements among students with existing deficiencies. Previous work studying the mental health impacts of school shootings finds similarly universal impacts (Rossin-Slater et al., 2020).

¹⁰This shattering of a belief in the safety of an institution echoes reactions to killings of unarmed individuals by the police—that the institutions entrusted by society to keep people safe are instead the ones that can cause harm (Bor et al., 2018; Ang, 2020).

difficult for students to concentrate and learn in the same environment in which the shooting occurred.

Peers and parents. Individual-level effects on students may be amplified or tempered through peer effects within shooting-exposed schools. In particular, school-based gun violence might result in “collective trauma,” similar to other violent events that impact groups or communities, such as mass shootings, wars, and terrorist attacks.¹¹ If the collective experience of trauma generates more peer and community support than individual exposure, then events that lead to collective trauma may have smaller effects. For example, the 2011 mass shooting in Utøya, Norway may have had even larger adverse impacts if the government had not provided substantial financial and mental health resources to the survivors in its aftermath (Bharadwaj et al., 2022). However, given that there are no systematic policy responses that direct resources toward survivors following school shootings in the United States, any mitigating effects stemming from the collective nature of school shootings may be limited in our setting.

Alternatively, if witnessing other people’s trauma intensifies one’s own distress, then the detrimental consequences of collective trauma may be larger than those of individual trauma. This amplification of collective trauma may be particularly relevant for understanding the impacts of school shootings on student outcomes. Students tend to remain classmates and peers with other shooting-exposed students for years after the shooting, and prior evidence suggests that peer effects play an important role in shaping student learning (see, e.g., Sacerdote, 2011; Feld and Zolitz, 2017). Most relevant to our context of violence exposure, Carrell et al. (2018) find that classroom peers of students experiencing domestic violence at home have lower earnings later in life. Analogously, we might expect the adverse impacts of a student’s own trauma from experiencing a shooting at school to be amplified by the impacts on other shooting-exposed peers.

Beyond students’ peers, parents might also be affected by school shootings. If gun violence

¹¹School shootings might also affect students at neighboring schools who hear about the shooting from peers or through social and traditional media. The existing literature suggests that gun violence impacts not only those who are directly exposed to a shooting but also those in the broader community (e.g., Bor et al., 2018; Ang, 2020; Koppensteiner and Menezes, 2021; Brodeur and Yousaf, 2022). We analyze spillover effects on students at neighboring schools in Section 5.4 and find that the impacts on students who are near to the event but not directly exposed are smaller than the impacts on students who attend shooting-exposed schools.

at schools leads to worse mental health among parents, then parents may be less able to help children cope with their grief and may add additional stress due to their own poor mental health. We unfortunately do not have information on parents in our data, and thus cannot examine effects on parents directly. However, other work suggests that difficulties among parents might contribute to the negative effects on students that we observe. While [Rossin-Slater et al. \(2020\)](#) find no impacts of school shootings on antidepressant use among adults, [Nabors \(2022\)](#) finds that parents and other family members suffer from worse mental health in the aftermath of a school shooting. Similarly, [Bharadwaj et al. \(2022\)](#) find evidence of worsening maternal mental health following the 2011 mass shooting in Utøya, and thus effects on parents might exert additional adverse impacts on the trajectories of children.

School environment. In addition to affecting the health and well-being of individual students, their peers, and their parents, shootings at schools can affect student outcomes by impacting school-level resource decisions and classroom instruction. The potential for school-level responses distinguishes school shootings from gun violence that takes place in other settings that are unlikely to influence school resources.

Following a shooting at school, the administration and other school staff can respond in ways that will either mitigate or exacerbate adverse impacts on student outcomes. On the one hand, schools might respond to a shooting by directing resources to help buffer against negative impacts on student outcomes. For example, schools could hire additional social support staff (e.g., counselors and school psychologists) following a shooting to help students cope with their trauma. On the other hand, schools and their staff may respond to a school shooting in ways that further harm student outcomes. For example, a school may close for a period of time or teachers and other staff may take time off or leave their jobs entirely, leading to a loss of continuity or changes in instruction that have been shown to negatively affect student performance ([Ronfeldt et al., 2013](#); [Atteberry et al., 2017](#)). School administrators may also respond with more strict disciplinary procedures or institute preventive measures such as metal detectors, security guards, and frequent school shooting drills. All of these responses may, in turn, affect children's learning and psychosocial adjustment in the aftermath of a school shooting.

Using the detailed nature of our data, we examine impacts on the employment and retention of school staff in Section 5.4. We find that school shootings lead to increased turnover among teachers but have no impacts on the hiring of social support staff. These responses suggest that impacts on school resources may contribute to—rather than help mitigate—impacts on student outcomes in our setting.

3 Data

3.1 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, or 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 data, the *Washington Post* database is updated as facts emerge about individual cases; the

¹²The 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/>.

¹³To 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/>.

version of the database used in our analysis was downloaded in April 2019.

As outlined in Section 3.2, our outcome data span the academic years 1992–1993 to 2017–2018. During this time period, there were 66 shootings at Texas public schools. Two schools experienced two shootings over our sample period; we only consider the first shooting at a given school (64 shootings). Since we are interested in studying the impacts of exposure to shootings on student outcomes, we further limit the sample to the 43 shootings that occurred *during school hours* (i.e., we drop shootings that occurred on weekends, evenings, or during school breaks) and *on school grounds* (i.e., we drop shootings that occurred off school property). In addition, in order to measure outcomes three years before to two years after a shooting in the short-run analysis, we focus on the 33 shootings that took place between the academic years 1995–1996 and 2015–2016.¹⁴ For the long-run analysis, we consider the universe of eight shootings that took place at Texas public high schools between the academic years 1998–1999 and 2005–2006. This narrower event window is required to allow us to measure outcomes at all ages between 18 and 26 for all cohorts used in our long-run analysis.

The 33 shootings included in our analysis vary in severity and situation (see Appendix Table A1 for a description of each event).¹⁵ While no shooting in our sample led to multiple deaths, 15 of the shootings 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.

Figure 1 displays the locations of the shootings used in our analyses, and Appendix Figure A1 depicts the number of shootings per academic year. The geographic distribution of school shootings across the state largely reflects the distribution of Texas’s population. Moreover,

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

¹⁵Appendix Table A2 compares the types of shootings across the eight shootings included in the long-run analysis, the 33 shootings included in the short-run analysis, and all 375 school shootings that took place across the United States during our analysis period. There is a slightly higher share of personally-targeted and crime-related shootings among the eight incidents in our long-run analysis sample than in the sample of 33 shootings used in our short-run analysis, although these differences are small and not statistically significant. Differences are even smaller when the eight shootings from the long-run analysis are compared to all 375 school shootings that occurred during the sample period. Moreover, we show in Section 5.2 that we find very similar estimates when we repeat the short-run analysis using the eight shootings that are included in the long-run sample. These results suggest that the shootings included in the long-run analysis are representative in terms of the distribution of shooting types and their effects on students.

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.

3.2 Educational and Labor Market Outcomes

Our outcome data come from three sources.¹⁶ 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). The data further contain information on student characteristics, including age, gender, race/ethnicity, and receipt of free or reduced-price lunch.

We use the TEA records to create five outcomes for each student at an annual (academic year) level: (1) a continuous absence rate, measured as the ratio of the number of days a student is absent relative to the number of days a student is enrolled in any school in our data; (2) an indicator denoting chronic absenteeism, which we define as an absence rate of greater than 10 percent; (3) an indicator denoting grade repetition; (4) the number of days of disciplinary action taken against a student;¹⁷ and (5) an indicator denoting whether the student switched schools.¹⁸ We further obtain information on whether a student graduated high school—and if so, at which age—from these records.¹⁹

We also use information on school staff from the TEA. These data contain annual records for each staff member at each school, and include information such as their FTE units and job

¹⁶We 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/>.

¹⁷The disciplinary actions variable in the TEA data includes 34 possible categories, including various types of suspensions (e.g., out-of-school, in-school, or part-day suspensions) and various types of expulsions (e.g., court-ordered or expulsions to various off-campus locations or alternative schools). Data on disciplinary actions is only available from the academic year 1998–1999 onward. We winsorize the number of days of disciplinary action at the 99th percentile to reduce the influence of outliers.

¹⁸We measure school switches with an indicator denoting whether a student is enrolled in a school at the beginning of the academic year that is different from the one in which he/she was enrolled in at the beginning of the previous academic year, excluding transitions from elementary to middle and middle to high school.

¹⁹We also have information on standardized test scores, although it is difficult to use these variables as outcomes in our analysis. Texas used different standardized tests that were administered to different grades over the course of our analysis period: the Texas Assessment of Academic Skills (TAAS) was used until 2002, the Texas Assessment of Knowledge and Skills (TAKS) was used from 2003–2011, and the State of Texas Assessments of Academic Readiness (STAAR) have been used since 2012. Moreover, 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 A3).

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

Second, we use administrative microdata on enrollment and graduation from all public and most private institutions of higher education in the state of Texas from the Texas Higher Education Coordinating Board (THECB).²⁰ The THECB data are linked to the TEA data at the individual level. We measure three outcomes in the THECB data for each individual at age 26: (1) an indicator for ever having enrolled in college, (2) an indicator for ever having enrolled in a 4-year college, and (3) an indicator for ever having obtained a bachelor's degree. We do not have information on out-of-state college enrollment or enrollment at some private institutions in Texas; as discussed in Section 4.3, this is unlikely to bias our results.

Finally, we use quarterly, administrative data on employment and earnings for all workers covered by the Unemployment Insurance (UI) program from the Texas Workforce Commission (TWC).²¹ As with the THECB data, the TWC data are linked to the TEA data at the individual level, thereby allowing us to follow students from school to the labor market.²² We

²⁰The 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.txhigheredata.org/Interactive/CBMStatus/> for additional information on participating institutions. Enrollment at independent colleges and universities (private technical colleges) accounted for approximately 11% (3%) of Texas college enrollment in 1999 ([THECB, 2000](#)). Our research design includes year fixed effects, which allows us to control for changes in data coverage over time.

²¹UI covers all workers whose employers pay at least \$1,500 in gross earnings or have at least one employee during twenty different weeks in a calendar year. Federal employees are not covered. See <https://www.twc.texas.gov/tax-law-manual-chapter-3-employer-0> for more details.

²²The TEA records are linked to the THECB and TWC records using a unique identifier, which is an anonymized version of an individual's social security number (see: https://texaserc.utexas.edu/wp-content/uploads/2016/03/Matching_Process.pdf). Individuals with invalid identifiers cannot be matched to the THECB and TWC data and are thus excluded from our long-run analysis of college and labor market outcomes. Approximately 8.8 percent of students eligible for our long-run analysis sample (outlined in Section 4.3) have invalid identifiers in the TEA data. Reassuringly, we find no systematic difference in the likelihood of having a valid identifier between shooting-exposed and non-exposed students. Students with valid identifiers in the TEA data who do not appear in the THECB or TWC data are included in our long-run analysis but are considered to not have attended college in Texas and to not be employed in Texas, respectively.

use the TWC data to create four outcomes, all of which are measured once for each individual when they are aged 24–26: (1) an indicator for being ever employed, measured by having positive earnings in any quarter; (2) an indicator for having stable employment, measured by having positive earnings in any four consecutive quarters; (3) average real annual earnings, measured in 2018 dollars; and (4) 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, although we do not expect this limitation to significantly influence our estimates (see Section 4.3).

4 Empirical Design

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

4.1 Matching Schools with Shootings to Control Schools

As noted in Section 3.1, 33 public schools in Texas experienced a shooting during school hours and on school grounds over the academic years 1995–1996 to 2015–2016. To reduce concerns about differential trends between schools with and without shootings biasing our estimates, we choose control schools that are similar on a set of observable characteristics using a “nearest-neighbor” matching procedure.

Specifically, for each school with a shooting, we first identify all other schools that are in different districts but offer the same grade levels (e.g., high schools are only matched with other high schools), have the same “campus type” (one of 12 categories based on population size and proximity to urban areas), and have the same charter school status.²³ We exclude schools in

²³The National Center for Education Statistics classifies schools into 12 campus types: City-Large, City-Midsize, City-Small, Suburban-Large, Suburban-Midsize, Suburban-Small, Town-Fringe,

the same district as we find some evidence of spillover effects of shootings on students at nearby schools.²⁴ We then use the nearest-neighbor matching algorithm to select the two most similar control schools based on a fuzzy match on the following school-level characteristics (not spatial proximity): share female students, share students receiving free or reduced-price lunch, share non-Hispanic White students, share non-Hispanic Black students, share Hispanic students, and total enrollment. We measure these variables in the first six-week grading period of the academic year of the shooting. As discussed in Section 5.3, our results are robust to the use of alternative matching strategies.

Appendix Table A3 presents average school characteristics for schools that experience a shooting (column (1)), matched control schools (column (2)), and all Texas public schools (column (3)). The fourth column presents p -values from tests of differences between mean characteristics of shooting and matched control schools, while the fifth column presents p -values from tests of differences between mean characteristics of shooting schools and all Texas public schools. Panels A and B present statistics separately for high schools and non-high schools, respectively.

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

4.2 Short-Run Analysis

In the short-run analysis, we focus on outcomes that can be measured both before and after a shooting for a given student in the TEA data (e.g., attendance and disciplinary actions).

Town-Distant, Town-Remote, Rural-Fringe, Rural-Distant, Rural-Remote. 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.

²⁴We also show that our results are not sensitive to excluding the six nearest schools in terms of distance (regardless of district) from the pool of control schools. See Sections 5.3 and 5.4 for more details.

To construct our short-run analysis sample, we begin by considering all students who were enrolled in the 33 shooting and 66 control schools in the academic semester during which a shooting took place.²⁵ We further restrict our sample to students who are observed in the data three years before to two years after the shooting (i.e., a six-year period); this requirement leads us to study students who were in grades 3–10 at the time of the shooting. Importantly, we do not require that students stay in the same school over their six years in the TEA data. Our final short-run analysis sample consists of 61,357 students (22,362 at shooting schools and 38,995 at matched control schools).

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

$$Y_{isgt} = \beta \text{ShootingSchool}_s \times \text{Post}_t + \alpha_i + \theta_{gt} + \epsilon_{isgt} \quad (1)$$

where Y_{isgt} is an outcome in academic year t for student i who was enrolled in school s in match group g at the time of the shooting. ShootingSchool_s is an indicator denoting schools that experienced a shooting, and Post_t is an indicator denoting observations in the academic year of the shooting and the following two years.²⁶ We include individual fixed effects, α_i , which account for all time-invariant differences between shooting-exposed and non-exposed students. We also include a full set of match group-by-academic year fixed effects, θ_{gt} , which flexibly account for match group-specific trends in outcomes. Standard errors are clustered by school (i.e., we account for $33 + 66 = 99$ clusters of shooting and control schools). The key coefficient of interest is β , which measures the difference in the change in student outcomes following a shooting between shooting and control schools within each match group.

Causal interpretation of β relies on a standard parallel trends assumption. That is, we

²⁵Enrollment information is available for every student for six six-week grading periods per academic year. We define the fall (spring) semester as containing the first (last) three six-week periods. We include all students who are enrolled in the shooting and control schools at any point in the semester of the shooting (e.g., a student who is enrolled in a shooting school at the beginning of the semester of a shooting, but switches to a different school by the end of the semester, is included in our sample). Students at control schools who were ever enrolled in a shooting school (5 percent of all students at the control schools) are excluded from our sample.

²⁶Since grade repetition reflects academic performance in the previous academic year, we exclude the year of the shooting from Post_t when analyzing this outcome. We also include a separate interaction term between ShootingSchool_s and an indicator for the year of the shooting.

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

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

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 matched control schools at the time of the shooting. As discussed in Section 5.1, the raw data plots and event study estimates reveal no evidence of differential pre-trends between students at treatment and control schools.

An additional concern for our short-run analysis is that of possible differential attrition from the sample. It is possible that students systematically leave the Texas public school system—either because they switch to private schools or because they move out of state—as a result of exposure to a shooting at school. This type of response has been documented in prior studies analyzing aggregate data on school enrollment (Abouk and Adams, 2013; Beland and Kim, 2016). Our primary short-run analysis focuses on a balanced panel of students and includes individual fixed effects, ensuring that our estimates are not driven by compositional differences in time-invariant factors between those in shooting and control schools. Nevertheless, our baseline estimates could be biased if there is differential attrition from the Texas public school system that is correlated with changes in the outcomes that we analyze. To address this concern, we examine whether students in schools that experience a shooting are more likely to leave the Texas public school system following the event relative to students at matched control schools. To do so, we consider an unbalanced sample that is constructed in the same way as our primary analysis sample except that we only require students to be observed in the Texas public school system in the year of the event (rather than three years before to two years after).

Appendix Figure A2(a) plots the share of students who appear in the TEA data in each year surrounding a shooting, separately for students at shooting and control schools. While about 7 percent of students are missing in a given year on average, we find no significant difference in the rate of attrition between students at shooting and control schools. Moreover, we estimate equation (2) using an indicator denoting whether each student appears in the TEA data in a given year as the outcome. As shown in Appendix Figure A2(b), while students at shooting schools are slightly less likely to be observed in the data than those at control schools, this difference is similar in years before and after the shooting. Thus, there is no evidence of differential attrition out of the Texas public school system that is caused by exposure to a shooting at school. In addition, we show in Section 5.3 that our short-run estimates are very similar if we use a balanced or unbalanced panel.

4.3 Long-Run Analysis

Our long-run analysis focuses on outcomes that can only be observed after the shooting in the TEA, THECB, or TWC data (e.g., high school graduation by age 26 and employment at ages 24–26). Since we only observe each outcome after the event, we cannot examine within-student changes in them. Instead, our difference-in-difference models compare differences in cohort outcomes between students who were enrolled in treatment schools at the time of the shooting and students who were enrolled in the same schools five years earlier, relative to analogous differences in cohort outcomes at matched control schools. As outlined in Section 3.1, our long-run analysis considers the universe of eight shootings that took place at Texas public high schools between the 1998–1999 and 2005–2006 academic years. This allows us to observe outcomes between the ages of 18 and 26 for all cohorts included in our analysis.

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 additionally include students who were enrolled in grades 9–12 at the same schools *five years before the year of the shooting*.²⁷ We label these “too-old-to-be-exposed”

²⁷We 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 5.1). Students who are enrolled in a shooting school four years before the shooting may still be there at the time of the shooting if they repeat a grade.

(or, “too old”) cohorts as being in grades 9*–12*, where the starred number corresponds to the grade a student was in five years before the shooting occurred. Our final long-run analysis sample consists of 31,237 students who were in grades 9–12 at the time of the shooting (11,335 at treatment schools and 19,902 at matched control schools) and 28,571 “too old” students who were in grades 9*–12* (10,834 at treatment schools and 17,737 at matched control schools).

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

$$Y_{isdg} = \sum_{d=9}^{12} \pi_d \text{ShootingSchool}_s \times \mathbf{1}_d + \sum_{d=9^*}^{12^*} \pi_d \text{ShootingSchool}_s \times \mathbf{1}_d + \lambda_{dg} + \delta' X_i + \varepsilon_{isdg} \quad (3)$$

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 and 9*–12* 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 , for student race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other) and gender. We cluster standard errors at the school-by-cohort level. The key coefficients of interest are π_d , which measure the differences in outcomes between students in shooting and control schools in each cohort d within each match group.

Equation (3) allows us to examine whether there are pre-existing differences in long-run outcomes between students at treatment and control schools by looking at the π_d coefficients for the “too old” cohorts (i.e., coefficients on the interactions between the shooting school indicator and indicators for grades 9*–12*). As we find some, albeit limited, evidence of pre-existing differences in outcomes among these “too old” cohorts in Section 5.2, we further consider specifications that exclude the separate interaction coefficients π_d for the “too old” cohorts and instead include school fixed effects to account for these differences. That is, we

additionally estimate specifications of the form:

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

where τ_s are school fixed effects, and all other variables are defined similarly to those in equation (3). Here, the “too old” cohorts are included in the analysis as the within-school comparison group for each of the exposed cohorts who were in grades 9–12 at the time of the shooting. We again cluster standard errors at the school-by-cohort level. The key coefficients of interest are ψ_d , which measure differences in outcomes between exposed versus “too old” cohorts across shooting and matched control schools.

As noted in Section 3.2, we do not observe college enrollment and completion information for out-of-state colleges and some private institutions in Texas. We also do not observe labor market information for individuals who leave Texas. From our short-run analysis, we find that exposure to a shooting at school does not lead students to be more or less likely to continue enrollment in Texas public primary and secondary schools in the two years after the event, suggesting that exposure to a shooting does not impact whether students move out of state in the short run. Nevertheless, it is possible that students exposed to a shooting at school may be more or less likely than unexposed students to move out of state in the long run. We discuss this issue in more detail in Section 5.2 and conclude that differential mobility among shooting-exposed students is unlikely to bias our long-run results.

5 Results

5.1 Short-Run Effects on Student Outcomes

We begin by examining effects on student outcomes in the short run. Figure 2 presents raw trends in our short-run outcomes over the six years surrounding each shooting, separately for students at shooting-exposed and matched control schools. For the first four outcomes—the continuous absence rate (sub-figure (a)), an indicator denoting chronic absenteeism (sub-figure (b)), an indicator denoting grade repetition (sub-figure (c)), and the number of days of disciplinary action (sub-figure (d))—we observe increasing trends in both the shooting

and control schools in the three years before the shooting. These (parallel) upward pre-trends reflect the fact that all of these outcomes tend to increase as students age, and average student age is increasing with event time in these plots.

While the pre-trends are similar across the shooting and control schools, we begin to see a divergence in trends for many outcomes starting with the academic year of the shooting (denoted by year 0 on the x -axis). Sub-figures (a) and (b) show that students at schools that experience a shooting have higher rates of absences and chronic absenteeism in the two years following the event relative to students at matched control schools. Similarly, while rates of grade repetition (sub-figure (c)) are almost identical in shooting and control schools in the years before a shooting, they are substantially higher in schools that experience a shooting in the two years after the event. In sub-figure (d), we observe that the difference in levels in days of disciplinary action between students at shooting and control schools becomes more pronounced in the year of and the year after a shooting, although the gap returns to pre-shooting levels two years after the event. Lastly, when we consider school switching in sub-figure (e), we find similar trends for students in shooting and control schools both before and after a shooting.

The raw trends provide suggestive evidence that: (1) there are no noticeable differences in pre-trends between students at shooting and control schools, and (2) several student outcomes deteriorate following a shooting at their school. Event study estimates shown in Figure 3 demonstrate that these conclusions are robust to the inclusion of individual and match group-by-academic year fixed effects.²⁸ In particular, Figure 3 plots the coefficients and 95% confidence intervals on the interactions between the indicator denoting a shooting school and the indicators denoting each of the years before and after a shooting from estimation of equation (2). Importantly, there are no statistically significant differences between shooting and matched control schools in the pre-shooting period; this supports the parallel trends assumption that is required for the validity of our research design. Furthermore, sub-figures (a) and (b) demonstrate that the average absence rate and likelihood of chronic absenteeism, respectively, increase in the year of a shooting and remain at elevated levels for the following two

²⁸Appendix Figures C1 and C2 show that these results are also robust to linear and non-linear violations of the parallel trends assumption, using the method proposed by [Rambachan and Roth \(2023\)](#). See Appendix C for additional details.

years. When we analyze grade repetition in sub-figure (c), an effect materializes in the year after a shooting, which is the earliest academic year when we could see an effect on an outcome that reflects inadequate academic progress in the prior year. Finally, sub-figures (d) and (e) indicate that there are no statistically significant changes in the average number of days of disciplinary action and likelihood of school switching, respectively, in the two years after a shooting.

Table 1 presents results from estimation of equation (1), in which we pool the post-shooting years to capture the average effects of shootings at schools on our short-run outcomes. As shown in column (1), exposure to a shooting at school leads to an average increase in the absence rate of 0.4 percentage points ($p\text{-value}=0.012$), or 12.1 percent relative to the pre-shooting mean of 3.7 percent. Exposure to a shooting further increases the rate of chronic absenteeism: column (2) indicates that chronic absenteeism rises by 1.8 percentage points ($p\text{-value}=0.016$), or 27.6 percent relative to the pre-shooting mean of 6.5 percent. Moreover, the rate of grade repetition increases by 1.3 percentage points (column (3); $p\text{-value}=0.008$) in the two years following a shooting, which represents more than a doubling of the baseline grade repetition rate. As shown in columns (4) and (5), estimates of the effects of shootings on days of disciplinary action and school switching rates, respectively, are not statistically significant at conventional levels.²⁹

Heterogeneity analyses. Having shown that shootings at schools impact several short-run student outcomes, we explore heterogeneity in these estimates across student, school, and shooting characteristics. To explore heterogeneity in effects by student characteristics, we use information on individual-level socio-demographics available in our data and estimate equation (1) separately for sub-groups defined by the following characteristics: gender, race/ethnicity, grade level at the time of the shooting (high school or non-high school), and ever receiving free or reduced-price lunch in the pre-shooting period.³⁰ Figure 4 displays the estimated coefficients and associated 95% confidence intervals; the pattern of results is very similar if we

²⁹The impact on disciplinary actions is marginally significant ($p\text{-value}=0.105$). While shootings at school might therefore also lead to increases in days of disciplinary actions, the corresponding event study estimates suggest that this is a continuation of a slight pre-trend (see Figure 3(d)).

³⁰In 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).

instead report estimates relative to sub-group specific outcome means (see Appendix Figure A3). Strikingly, there appear to be substantial impacts on each of the sub-groups analyzed, highlighting 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.

We further analyze heterogeneity in effects across schools with different resources. We begin by focusing on resources that might help students cope with trauma, as measured by the availability of various health professionals on campus. In particular, we split schools based on whether they have an above- or below-median FTE allocation of different types of health professionals per student in the year before the shooting. Since only seven out of the 33 shooting schools have any positive FTE allocation of school psychologists or social workers at baseline, we split schools based on whether they have any positive FTE allocation of school psychologists or social workers when analyzing heterogeneity by these types of health professionals. Figure 5 presents coefficients and 95% confidence intervals from estimation of equation (1) for each school type.³¹ We find no evidence of differential impacts based on the presence of different types of health professionals in schools at baseline. We further consider heterogeneity by other baseline school-level resources, including whether schools had an above- or below-median number of FTE teachers, school leadership staff, and teaching support staff per pupil. As shown in Appendix Figure A4, we also find no consistent evidence of differential effects across these other measures of school-level resources at baseline.

Finally, using the categorization suggested by Levine and McKnight (2020b), we classify shootings into four mutually exclusive categories: suicides, personally-targeted, crime-related, and other.³² For each category of shootings, Figure A5 displays coefficients and associated 95%

³¹Since we have few clusters in some of these sub-group analyses, we calculate standard errors using a wild cluster bootstrap.

³²The CHDS data assign each shooting into one of 19 categories; we use this information to form the four aggregate groups from Levine and McKnight (2020b). In particular, “personally-targeted” shootings include escalation of dispute, anger over grade/suspension/discipline, bullying, domestic disputes with a targeted victim, and murder; “crime-related” shootings include gang-related, hostage standoffs, illegal drug related, and robberies; and “other” shootings include mental health-related, intentional property damage, officer-involved, racial, self-defense, accidental, and unknown. Among our 33 shootings, 11 are suicides, four are personally-targeted, and two are crime-related. Since we have relatively few shootings—and therefore few clusters—in some of these categories, we present 95% confidence intervals based on a wild cluster bootstrap.

confidence intervals from estimation of equation (1). The confidence intervals overlap across estimates, suggesting that there is no evidence of meaningful heterogeneity across shooting types.

5.2 Long-Run Effects on Educational and Economic Outcomes

Recall that our long-run analysis focuses on the eight shootings that took place at Texas public high schools over the period 1998–2006. As these shootings are a subsample of the 33 shootings included in the short-run analysis above, we begin by replicating our short-run analysis using these eight shooting events. As shown in Figure A6 and Appendix Table A4, the results from this analysis are similar to our baseline estimates presented in Figure 3 and Table 1. If anything, the short-run effects are somewhat larger among the shootings included in the long-run analysis.

We now turn to an analysis of educational and economic outcomes in the long run. Appendix Figures A7 and A8 present raw cohort-level means of our long-run educational and labor market outcomes, respectively, for each of the eight cohorts of students included in our long-run analysis: those who were in grades 9, 10, 11, and 12 at the time of the shooting (displayed to the right of the vertical line on each plot) and those in the “too old” cohorts who were in grades 9*, 10*, 11*, and 12* at the same schools five years earlier (displayed to the left of the vertical line on each plot).³³ As expected, the figures show some differences in outcomes based on the grade that determines sample inclusion. For instance, educational attainment and labor market outcomes measured at ages 24–26 are typically higher among individuals who were observed enrolled in grade 12 than those whose inclusion in the sample only requires that they were observed in grade 9. This is because students who make it to grade 12 (rather than potentially dropping out earlier) on average attain more education and earn higher wages once they enter the labor market.

Reassuringly, however, we see limited evidence of differences in outcomes among the “too old” cohorts between the shooting-exposed and control schools. In contrast, there are noticeable differences in cohort-level outcomes among students who were attending the shooting-

³³As discussed in Section 4.3, the star labeling for the “too old” cohorts signifies that these cohorts were not exposed to the shooting as they were in these grades five years before the event occurred.

exposed and matched control schools at the time of the event. For all outcomes, we see differences between treatment and control students in the exposed cohorts that are greater than the cross-school differences in long-run outcomes among the “too old” cohorts. While these raw data plots are consistent with negative long-term impacts from being exposed to a shooting at school, these plots do not account for either match group or school fixed effects. Thus, we next consider results from estimation of equations (3) and (4) that do.

Figure 6 presents estimates of the effects of exposure to a shooting at school on students’ educational outcomes measured at age 26. In each sub-figure, the graph on the left-hand side presents the coefficients and 95% confidence intervals on the interactions between the indicator denoting a shooting school and the cohort indicators from estimation of equation (3), while the graph on the right-hand side presents the coefficients and 95% confidence intervals on these interaction terms from estimation of equation (4).³⁴ Although not statistically significant, there is some evidence in the left-hand side panel that there may have been slight differences in long-run educational outcomes, such as high school graduation, between the “too old” cohorts at the shooting and control schools. This evidence motivates our inclusion of school fixed effects in the right-hand side panel. Comparing within-school, across-cohort outcomes between treatment and matched control schools shows that there are significant adverse impacts of exposure to a shooting at school on long-run educational outcomes, especially when exposure occurs in grades 10 and 11. We do not observe significant impacts of exposure in grade 12, which 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 our long-run educational outcomes by grade level of exposure, and Panel A of Appendix Table A5 provides summary estimates pooling across exposure in grades 10–11. Focusing on the pooled estimates, we see that experiencing a shooting at school in grades 10–11 leads to a 2.7 percentage point reduction in the likelihood of graduating high school by age 26 (column (1); p -value=0.034), a 3.4 percent reduction relative to the mean among the cohorts enrolled at matched control schools

³⁴Recall that equation (3) allows us to examine whether there are pre-existing differences in long-run outcomes between students at shooting and control schools (i.e., differences among the “too old” cohorts). Equation (4) then includes school fixed effects to control for any pre-existing differences.

at the time of the shooting. Moreover, exposure to a school shooting in grades 10–11 leads to a 4 percentage point reduction in enrollment in any college (column (2); p -value=0.002) and a 5 percentage point reduction in enrollment in a 4-year college (column (3); p -value<0.001), reductions of 6.2 percent and 13.3 percent of the respective control group means. Lastly, as shown in column (4), we find that exposure to a shooting at school in grades 10–11 leads to a 3.5 percentage point decline in the likelihood of receiving a bachelor’s degree by age 26 (p -value<0.001), a reduction of nearly 15 percent relative to the control group mean.

Figure 7 and Table 3 present analogous results for labor market outcomes measured at ages 24–26. Looking first to Figure 7, we see little evidence of differences in long-run labor market outcomes between “too old” cohorts at the treatment and control schools. In contrast, we see that shooting exposure in grades 9–11 negatively affects economic well-being. Averaging across coefficients for exposure in grades 9–11 (see Panel B of Appendix Table A5 for these summary estimates), we find that shootings at schools lead to a 3.0 percentage point reduction in the likelihood of any employment (column (1); p -value<0.001) and a 3.8 percentage point reduction in the likelihood of stable employment (column (2); p -value<0.001) at ages 24–26. Relative to the respective control group means, these effects reflect reductions in employment of 3.8 percent and 5.6 percent, respectively. Moreover, we find that exposure to a school shooting in grades 9–11 leads to a reduction of \$2,622 in annual earnings at ages 24–26 (column (3); p -value<0.001), an 11.3 percent reduction relative to the control group mean. While some of this reduction in annual earnings is driven by reductions in labor supply on the extensive margin, we also observe sizable reductions on the intensive margin as measured by non-zero earnings (i.e., conditional on employment; see column (4)).

These estimated adverse impacts on both education and labor market outcomes might indicate that the reduction in college attendance is a key mechanism driving the lower adult earnings. To assess how much of the reduction in earnings is explained by lower college attendance, we do a back-of-the-envelope benchmarking exercise. We do not have an independent instrument to causally estimate the returns to education in our data, but, under the assumption of positive selection into college, the ordinary least squares regression of earnings on college attendance provides an upper bound. Controlling for gender and race/ethnicity, we find that attending a 4-year college is associated with \$12,387 higher earnings at ages 24–26

among students in the control group. Multiplying this estimate by the effect of exposure to shootings on 4-year college attendance (-5.0 percentage points for exposure in grades 9–11) suggests that at most a quarter (\$619) of the overall earnings reduction can be explained by lower rates of college attendance.

Moreover, there is an important education-earnings trade-off in early adulthood, as individuals who are completing higher education experience lower earnings while they are in school. As such, the returns to education increase as individuals get older (Bhuller et al., 2017). Since we measure earnings when individuals are in their mid-20s, it is possible that our estimates represent lower bounds for the adverse impacts of shootings on their permanent incomes over the life cycle. Additionally, the fact that we see decreases in both educational attainment and earnings in response to a shooting at school points to the potential importance of other channels through which labor market effects may arise (e.g., through a deterioration in mental health).

Potential differential attrition. We code our long-run outcomes as zeros for those who have not obtained the indicated level of education or who do not have earnings in the Texas data. Since our data only contain information from the state of Texas, we necessarily assign zeros for students who leave Texas before an educational milestone (such as high school graduation) is reached or an employment outcome (such as employment at ages 24–26) is measured. If shooting-exposed students are differentially more or less likely to migrate out of Texas than their unexposed counterparts, then any resulting compositional changes would complicate the interpretation of our estimates of long-run effects.³⁵ While our primary data do not allow us

³⁵If there were differential migration in the long run, we note that the sign of the resulting bias would be ambiguous. For instance, if exposure to a shooting at school makes students less likely to leave Texas in the long run because they are less likely to pursue out-of-state college or labor market opportunities, our analysis would underestimate the effects of shooting exposure on long-run outcomes such as college attendance, college completion, and labor force participation. If exposure to a shooting instead makes students more likely to move away from Texas and work out-of-state in the long run, then the bias would go in the opposite direction. To assess this potential bias, we estimate Lee (2009) bounds assuming differential attrition in response to a school shooting of 0.83 percentage point (the attrition gap between shooting and control schools shown in Figure A2(b)). The estimated bounds are presented in Appendix Tables A6 and A7 and reassuringly exclude zero for most of the outcomes with significant estimates in our baseline specifications. We consider this bounding strategy to be a conservative assessment of potential bias in the long-run analysis due to the evidence of no differential attrition between exposed and “too old” cohorts (who are included in our baseline specifications as controls), and given that almost half of students leaving the Texas public school data in the short run are actually observed in the later labor market data.

to quantify the impacts of shootings at schools on migration out of Texas in the long run, four sets of evidence suggest that differential migration is unlikely to meaningfully influence our results.

First, while Appendix Figure A2(c) indicates that students at shooting schools are slightly less likely to be observed in the two years following a shooting compared to control schools, Appendix Figure A2(d) shows that the rate of leaving the Texas public school system is the same among the “too old” cohorts at shooting schools. As this pattern suggests that there is no differential attrition from the public school system in response to a shooting in the short run (in line with the discussion in Section 4.2), it is plausible that there is also no differential migration out of Texas in the longer run. Second, the estimated adverse effects of exposure to a shooting at school are observed across outcomes measured over different time horizons since the shooting—from high school graduation to labor market outcomes at ages 24–26. This consistency further suggests that differential mobility among shooting-exposed students is not driving the long-run results. Third, we obtain similar effects on earnings when we include or exclude individuals with no earnings in Texas. Thus, the estimated negative consequences of being exposed to a shooting at school extend to the subset of individuals who remain and work in Texas at some point between the ages of 24 and 26.

Finally, as outlined in Appendix D, we use data from the American Community Survey (ACS) to examine out-of-state and within-state migration in Texas surrounding shootings in our analysis sample.³⁶ As shown in Appendix Figure D1(a), migration out of Texas is low compared to within-state migration; that is, most individuals who change their county of residence in adolescence and young adulthood remain in Texas, and thus we can follow them in our data. Out-migration rates are also low relative to our estimated effect size for labor force participation: out-migration rates among shooting-exposed individuals would have to differentially increase by over 100 percent relative to baseline rates to explain the decreases in labor force participation that we observe. Moreover, as shown in Appendix Figure D1(b), we find no evidence of differential changes in out-migration rates in shooting-exposed counties

³⁶In particular, we use ACS data for 2005–2019 and restrict the sample to individuals who were residing in a county in Texas in the prior year. Using indicators denoting whether an individual’s current county of residence is outside the state of Texas or in a different county within Texas than in the prior year, we then analyze the impacts of shootings on out-migration among individuals who were residing in counties that contain treatment schools, counties than contain control schools, and all counties in Texas both in the raw data and using an event study specification similar to equation (2). See Appendix D for additional details.

relative to counties containing matched control schools in the years following an event. Taken together, this evidence strongly suggests that differential migration out of Texas is unlikely to meaningfully affect our long-run results.

Heterogeneity analyses. Since we only have eight shootings in our long-run analysis sample, we are unable to explore heterogeneity by school or shooting 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 A9 and A10 present these results for our long-run educational and labor market outcomes, respectively. As in our analysis of short-run outcomes, we find negative impacts of school shootings across sub-groups, suggesting near universal impacts. Although the estimated impacts are not statistically different, the point estimates suggest that the impacts on high school graduation, college enrollment, and employment may be more pronounced for non-Hispanic Black students. This pattern is consistent with the larger point estimates for these students in our analysis of short-run outcomes.

Magnitude comparison. It is helpful to compare our effects to those found in recent work examining the impacts of exposure to other types of violence. Our finding that exposure to a school shooting reduces high school graduation by 3.4 percent is comparable to the 3.5 percent reduction found by [Ang \(2020\)](#) in response to local police killings, whereas the 6.2 percent reduction in college enrollment that we document is larger than the 2.5 percent reduction found in [Ang \(2020\)](#). Moreover, our estimated 11.3 percent reduction in earnings at ages 24–26 is larger than the 3 percent reduction in earnings at ages 24–28 found by [Carrell et al. \(2018\)](#) from having an additional classroom peer who experiences domestic violence at home. As outlined in Section 2, our larger effect sizes are consistent with the idea that shootings at schools are more disruptive to student learning than violence that takes place in other settings. Moreover, as entire schools are exposed to shootings rather than individual students, our effect sizes are further consistent with the possibility that the harm of a student’s own trauma from experiencing gun violence at school may be amplified by the effects of being exposed to other shooting-exposed peers.

We can further compare our effects to those reported in [Bharadwaj et al. \(2022\)](#)’s study

on the impacts of exposure to the 2011 mass shooting in Utøya, Norway. They found that survivors of the mass shooting were 12 percent less likely to complete college and had 12 percent lower earnings than a matched control group. Our estimated 14.6 percent decrease in the likelihood of receiving a bachelor’s degree and 11.3 percent reduction in earnings are quite similar. Although the Utøya massacre was more severe than the shootings that we study, differences in compensating resource provision might help explain the similarly of effect sizes.³⁷ As Bharadwaj et al. (2022) note, the Norwegian government provided substantial resources and support to the survivors of the Utøya attack, which likely buffered against some of the detrimental long-term effects. In contrast, we are not aware of governmental responses to the school shootings that we study, and we do not see any changes in observable mental health resources to support students at the school level (see Section 5.4 below).

Lastly, our estimates can be put in context of the broader literature on the long-run impacts of educational inputs on adult earnings. Chetty et al. (2011) find that a one standard deviation increase in “class quality” (a measure that includes teachers, peers, and any class-level shocks) for one year among students in kindergarten through 3rd grade leads to a 9.6 percent increase in earnings at age 27. Furthermore, Chetty et al. (2014) estimate that a one standard deviation increase in teacher quality for one year among students in grades 4–8 results in a 1.3 percent increase in earnings at age 28. Our estimated 11.3 percent reduction in earnings at ages 24–26 is thus equivalent to a 1.2 standard deviation decrease in class quality for one year or a one standard deviation reduction in teacher quality for nine years. Given that our long-run estimates capture the effects of exposure to a shooting in high school, our findings suggest that adverse shocks at older grade levels can offset large advantages in educational inputs in younger grades.

5.3 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

³⁷Unprecedented tragedies such as the Utøya attack might also impact “control group” youth who were not directly involved, potentially diminishing the estimated net effects. In contrast, the events that we study were not widely covered by media outlets, and we show in Section 5.4 that the effects fade quickly as distance to the exposed school increases.

Appendix Figure A11, we explore the sensitivity of our estimates to using an unbalanced panel. In particular, we overlay our baseline event study estimates with results derived from a sample in which we do not make any restrictions on the number of years that students must be observed in the data. The results across the two samples are very similar, indicating that our main estimates are not sensitive to our balanced panel restriction.

We also test the robustness of our estimates to alternative ways of matching schools that experience shootings to control schools. Appendix Figure A12 presents coefficients and 95% confidence intervals from estimating equation (1) using samples of control schools selected from eight alternative matching strategies; Appendix Figures A13 and A14 present analogous results from estimation of equation (4) for long-run educational and labor market outcomes, respectively. We make the following adjustments to the matching strategy in these figures: (1) we add average 8th grade standardized test scores for math and reading before the shooting to the set of fuzzy match variables;³⁸ (2) in addition to the variables in (1), we do an exact match on the 10 educational regions in Texas;³⁹ and (3) in addition to the variables in (2), we add the share of students who are in gifted programs, have limited English proficiency, and are immigrants to the set of fuzzy match variables. Additionally, we use the same matching variables as in our baseline strategy but: (4) select four control schools instead of two, (5) match in reverse order, (6) match using characteristics measured in the year before the shooting rather than the year of the shooting, (7) add measures of FTE school staff in the year before the shooting to the set of fuzzy match variables, and (8) exclude the six closest schools (in terms of distance) from the pool of potential control schools. For ease of comparison, we provide our baseline estimate in each sub-figure. Reassuringly, both our short-run and our long-run results are robust across all of these alternative matching strategies.

Additionally, we examine the robustness of our estimates to using a more parsimonious set

³⁸In 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.

³⁹That is, we only pick control schools that are in relative geographic proximity to the schools that experience shootings. The TEA data provide information on the Educational Service Center (ESC) associated with each campus. There are 20 ESCs, and we assign these ESCs to the 10 education regions using the crosswalk between ESCs and education regions provided by the TEA (available at: <http://www.txhighereddata.org/Reports/Performance/P16data/TxEdregionslist.pdf>). We introduce four additional categories for the four ESCs that are matched to two education regions, and thus in practice we do an exact match on 14 education region categories.

of matching variables. In Appendix Figure A15 and Appendix Figures A16–A17, we demonstrate the robustness of our short- and long-run results, respectively, to starting with a basic set of matching variables and gradually adding additional variables to our matching procedure. Specifically, we begin by only matching on grade levels and the urbanicity categories. We then select two control schools for every shooting-exposed school from this set of potential matches by selecting the two schools that are the most similar to the exposed school in terms of student enrollment. To move toward our baseline specification, we then progressively add the following fuzzy match variables: share of students by gender, share of students by race/ethnicity, and share of students on free or reduced-price lunch. Overall, the pattern of results for both short-run and long-run outcomes is very similar when matching relies on a more parsimonious set of characteristics.

Finally, we further probe the statistical likelihood of our long-run effects using a permutation test. For each iteration, we begin by randomly selecting eight schools from the 664 high schools that did not experience a shooting and were observed over our entire sample period. We then run our matching procedure to identify two control schools for each of our eight placebo “treatment” schools. Randomly assigning the eight shooting dates observed in our sample to the eight placebo “treatment” schools, we then re-run our long-run empirical design (equation (4)) comparing changes in outcomes following a placebo event in the “treatment” schools to those experienced at the matched control schools. We repeat this analysis 1,000 times and compare the treatment effects presented in Tables 2 and 3 to the distributions of placebo estimates. As shown in Figures A18–A25, our effects are in the tails of these distributions and therefore retain statistical significance using this alternative method of inference.

5.4 Supplemental Evidence

Effects on school staff. Shootings at schools have the potential to impact many aspects of the school environment, setting gun violence in schools apart from violence in other settings like students’ communities or homes. As outlined in Section 2, a school might respond to a shooting on school grounds by increasing the quantity of instructional, support, or leadership services to mitigate the potential harms. At the same time, a shooting at school may adversely impact the staff themselves and lead to higher turnover rates or lower the quality of instruction

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

As introduced in Section 3.2, we begin this analysis by categorizing school staff into four groups: teachers, school leadership (principals and assistant principals), teaching support (e.g., educational aides), and social support (e.g., counselors and school psychologists).⁴⁰ We define employment and retention rates at the school-by-academic year level and estimate versions of equations (1) and (2) that use school fixed effects in place of individual fixed effects. We weight the school-by-academic year cells by total enrollment and cluster standard errors at the school level.⁴¹

Results for effects on the total number of FTE staff per 1,000 students across employment groups are shown in Figure 8 and Panel A of Appendix Table A9. As shown in sub-figures (a), (c), and (d) of Figure 8, shootings at schools do not affect the aggregate employment of teachers, teaching support staff, or social support staff, respectively. However, sub-figure (b) suggests that the number of FTE leadership staff increases following a shooting.⁴² This estimated increase is large: as shown in column (2) of Panel A of Appendix Table A9, the number of school leadership staff per 1,000 students rises by 0.55 following a shooting (p -value=0.064), an effect of 18.9 percent relative to the baseline mean of 2.9. As assistant principals—who contribute much of the variation in the number of school leadership positions in the data—are often in charge of discipline, safety, and interventions for behavioral issues at schools, this increase could reflect schools' responses to the disruption caused by a shooting.

Results for effects on the retention of full-time employees across employment groups are shown in Figure 9 and Panel B of Appendix Table A9. In these analyses, we consider staff that were employed full time at each of the shooting and control schools at the time of

⁴⁰Appendix Table A8 presents descriptive statistics for the school staff groups and the individual staff types included in each group.

⁴¹Total enrollment is measured in the first six-week grading period of the academic year of the shooting. Because our annual staffing data capture employment as of a snapshot date in October, we restrict our sample to shootings that took place in or after November in a given academic year and treat the year of the shooting as a pre-shooting period in this analysis. We also restrict our sample to match groups in which all three schools are consistently observed from three years before to two years after the shooting. Our final staff analysis sample includes 24 school shootings.

⁴²In order to make effect sizes more comparable across the four groups of staff, the y-axes in these figures are scaled to range from -50 percent to +50 percent of the pre-period mean of each outcome. Raw data trends for FTE staff by employment group are shown in Appendix Figure A26.

the shooting and examine how the probability of full-time employment at the same school evolves before and after the shooting.⁴³ As shown in sub-figures (a) and (c) of Figure 9, we find that shootings at schools lead to a reduction in the probability of retention for teachers and teaching support staff. The effect sizes are meaningful: Panel B of Appendix Table A9 shows that the retention rates of teachers and teaching support staff decline by 0.4 percentage points (column (1); p -value=0.012) and 2.0 percentage points (column (3); p -value=0.008), respectively, reflecting reductions of 5.7 percent and 32.7 percent relative to the respective baseline means. Since we do not find a change in the number of FTE staff per 1,000 students for these employment groups, we interpret the reduction in retention as evidence of increased turnover at shooting-exposed schools. Given the negative impacts of teacher turnover on student performance (Ronfeldt et al., 2013), these changes and disruptions to the school environment are one potential mechanism that may contribute to the adverse effects that we find among shooting-exposed students. While new staff have less school-specific experience by definition, we do not find evidence of a change in the composition of teachers and teaching support staff in terms of gender, race/ethnicity, or educational background.

Spillover effects on students at neighboring schools. We have thus far focused on studying the impacts on students who were attending a school in which a shooting occurred. The large literature documenting that exposure to violence outside of one's own school (i.e., in the local community) has lasting impacts on human capital formation suggests that the impacts of school shootings may extend to children who live and go to other schools nearby. We investigate this possibility by studying effects on students who attended the six closest schools (in terms of spatial distance) to the schools that experienced shootings in our sample. After re-running the baseline matching algorithm to identify two control schools for each neighboring school, we re-estimate our short- and long-run specifications for students at each of the following three groups of schools: the first and second closest, the third and fourth closest, and the fifth and sixth closest.⁴⁴

⁴³In this analysis, teachers who are included in multiple match groups (1.2 percent of all teachers in the sample) are excluded. We drop match groups in which either a shooting school or both control schools had no full-time employees in a given staff group at the time of the shooting. Raw data trends for retention rates by employment group are shown in Appendix Figure A27.

⁴⁴For every shooting-exposed school in our analysis sample, we select the six closest schools that offered the same grade levels and did not experience a shooting over our sample period. We do not make restrictions

Appendix Figure A28 presents the results on spillover effects for our short-run outcomes. We plot the coefficients and 95% confidence intervals from estimation of equation (1), separately for students at shooting-exposed schools and students at the three groups of nearest schools. These results demonstrate some evidence of spillover effects on students in the two closest schools, with effects fading to zero as we consider students at schools further away. Students at the first and second closest schools experience increases in their rate of absenteeism and chronic absenteeism that are marginally significant and roughly half the size of the effects on absences observed among those enrolled at the shooting schools themselves. Similarly, there is suggestive evidence of an increase in grade repetition among students in the first through fourth closest schools. We additionally find that there is a negative effect on the likelihood of switching schools among those attending the closest schools at the time of the shooting, which could reflect a decrease in the likelihood of switching to the school that experienced a shooting after the shooting takes place. We do not find any statistically significant or economically meaningful impacts on short-run outcomes among students at the fifth and sixth closest schools.

Analogously, we study spillover effects on long-run outcomes for students in neighboring schools. We estimate equation (4) and compare the average effects of exposure in grades 9–11 among students at the shooting-exposed schools to those among students in the three groups of nearest schools. Consistent with the effects on short-run outcomes, Appendix Figures A29 and A30 show that the impacts of school shootings on long-run educational and labor market outcomes, respectively, are strongest among students at the exposed schools. The effects are smaller in magnitude for students in the first two to four nearest schools, and we find no impacts on long-run outcomes among students in the fifth and sixth nearest schools.

The magnitudes of spillover effects on students attending the closest neighboring schools are in line with prior evidence on the impacts of gun violence in the broader community on similar outcomes (e.g., [Bor et al., 2018](#); [Ang, 2020](#); [Koppensteiner and Menezes, 2021](#); [Brodeur and Yousaf, 2022](#)). This aligns with intuition, as a shooting at a nearby school may be more

on school districts when defining neighboring schools, thereby allowing neighboring schools to be located in different districts. We then run our matching procedure to identify two control schools for each neighboring school; here, schools within the same districts as the shooting-exposed school or the neighboring school under consideration are excluded from the pool of potential control schools. Unlike in our baseline matching procedure, we allow matched control schools to be included in more than one match group and treat control schools in multiple match groups as separate schools.

comparable to a shooting in the community rather than to a shooting at one’s own school. The fact that we find evidence of these spillovers therefore echoes the conclusions of the prior literature on the lasting impacts of community-level violence.

6 Conclusion

Mass shootings receive significant media attention and spark 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). Therefore, 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.⁴⁵ Hundreds of thousands of American children have been exposed to a shooting at their school and have survived, and these shootings vary substantially in their circumstances, number of injuries, and number of deaths. Quantifying the causal effects of shootings at schools on students’ short- and long-run outcomes is critical both for targeting resources to help mitigate potential harms and for informing policy discussions that compare the costs of different types of gun violence.

We study the universe of shootings that occurred on school grounds during school hours at Texas public schools between 1995 and 2016 and examine within-student and across-cohort changes in outcomes relative to changes at matched control schools. Our estimates suggest that the costs of shootings at schools—even those that have few or no deaths—are large. In addition to effects on short-run educational outcomes like absenteeism, we find that shootings have lasting implications for the human capital and economic trajectories of exposed students.

We conduct a back-of-the-envelope calculation based on our estimates of the effects of shooting exposure in grades 9–11 on annual earnings at ages 24–26. Assuming that the average effect of exposure persists through age 64, our estimates imply a reduction of \$100,439 (in 2018 dollars) in the present discounted value of lifetime earnings per shooting-exposed student.⁴⁶

⁴⁵For an example of such an argument, see: <https://www.vox.com/2015/10/1/18000524/mass-shootings-rare>.

⁴⁶To calculate the present discounted value of lifetime earnings, we discount the stream of earnings from ages 15–64 in the 2019 March Current Population Survey (CPS) back to age 15 (i.e., around the start of high school), assuming that earnings are discounted at a 3 percent real rate (i.e., a 5 percent discount rate with 2 percent wage growth). This calculation yields a total present discounted value of \$888,844. We then multiply

Given that more than 50,000 American students experienced a school shooting annually in recent years (see footnote 2), the aggregate present discounted value of the cost of school shootings based on long-run earnings losses alone is more than \$5 billion annually.

It is useful to benchmark these back-of-the-envelope calculations to the costs of educational investments. Notably, our estimate of the annual cost of school shootings in terms of lost earnings is more than twice as large as the nationwide annual spending on school resource officers (i.e., police officers stationed at schools, often with a goal of preventing gun violence; [Avila-Acosta and Sorensen, 2023](#)). Moreover, these effects are sizable when compared to general educational expenditures: the aggregate annual cost in terms of lost earnings is 0.7% of total annual spending on primary and secondary education in the United States, and the estimated impact on lifetime earnings among exposed students is roughly six times average annual educational expenditures per student.⁴⁷ As these figures do not take into account the costs of school shootings resulting from the loss of human life, physical injuries, and the broader mental health impacts ([Rossin-Slater et al., 2020](#); [Levine and McKnight, 2020a](#)), they represent substantial underestimates of the value of the investment that society should be willing to make to avoid school shootings.

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

this number by the average percent effect of exposure to a shooting in grades 9–11 on annual earnings (11.3 percent, see Appendix Table A5). The CPS data are downloaded from the Integrated Public Use Microdata Series (IPUMS; [Flood et al., 2020](#)).

⁴⁷For the 2018–2019 academic year, the National Center for Education Statistics (NCES) reports that aggregate expenditures on primary and secondary education in the United States were \$769.1 billion (NCES, [2023a](#)) and expenditures per pupil were \$16,146 (NCES, [2023b](#)).

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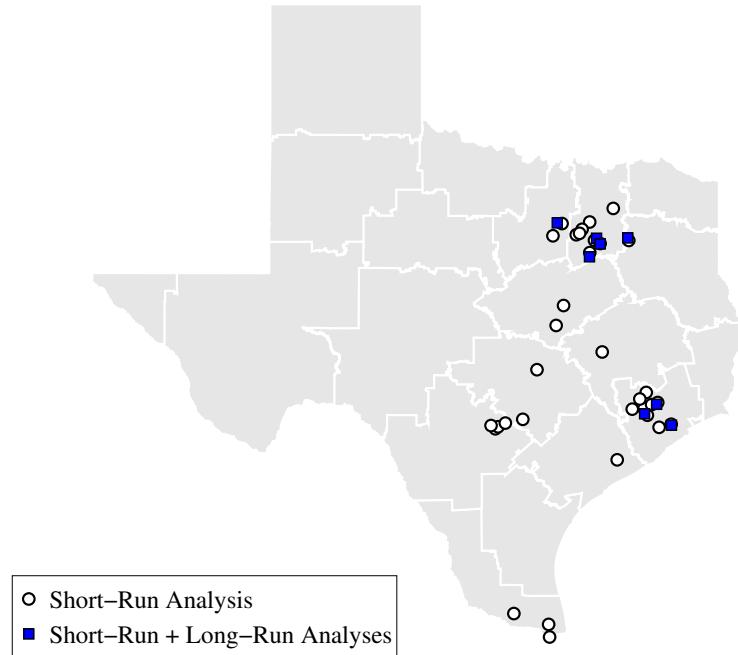
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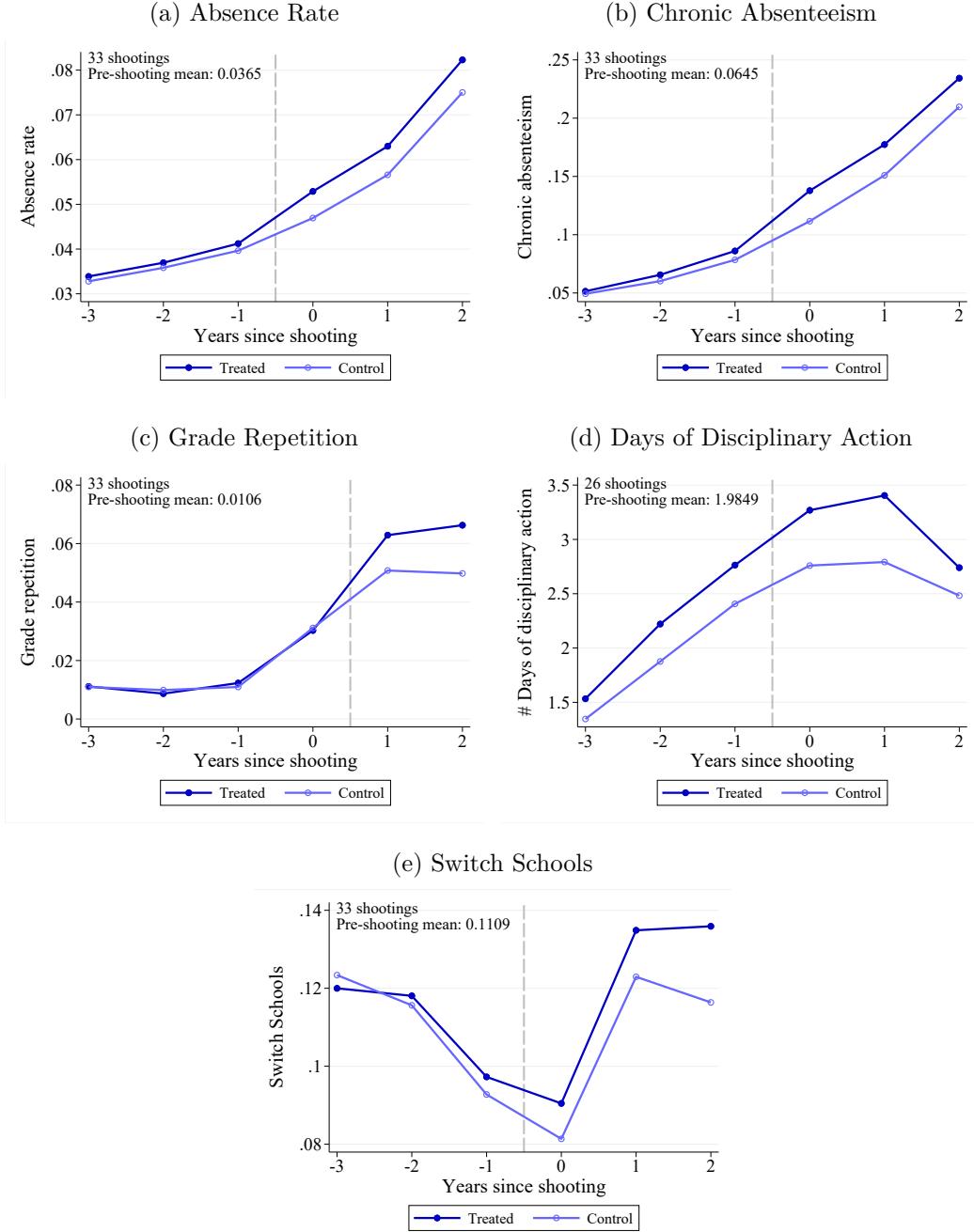
7 Figures and Tables

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



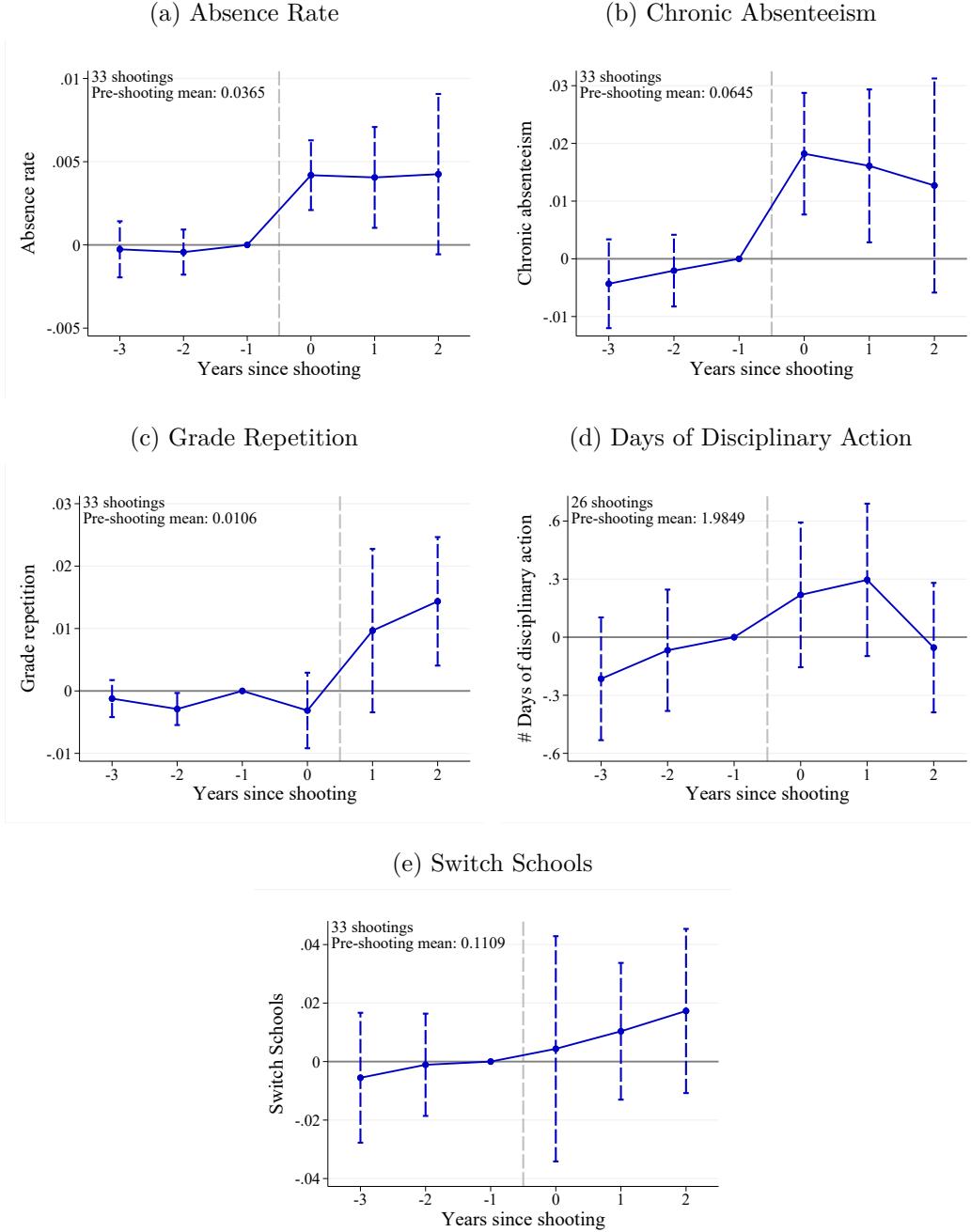
Notes: This figure shows the locations of the 33 (8) shootings at Texas public schools used in our short-run (long-run) analysis. These shootings occurred during school hours and on school grounds between the academic years 1995–1996 and 2015–2016. The data are compiled from the Center for Homeland Defense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

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



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 (d) 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). Pre-shooting outcome means are calculated based on both the treatment and control group schools.

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



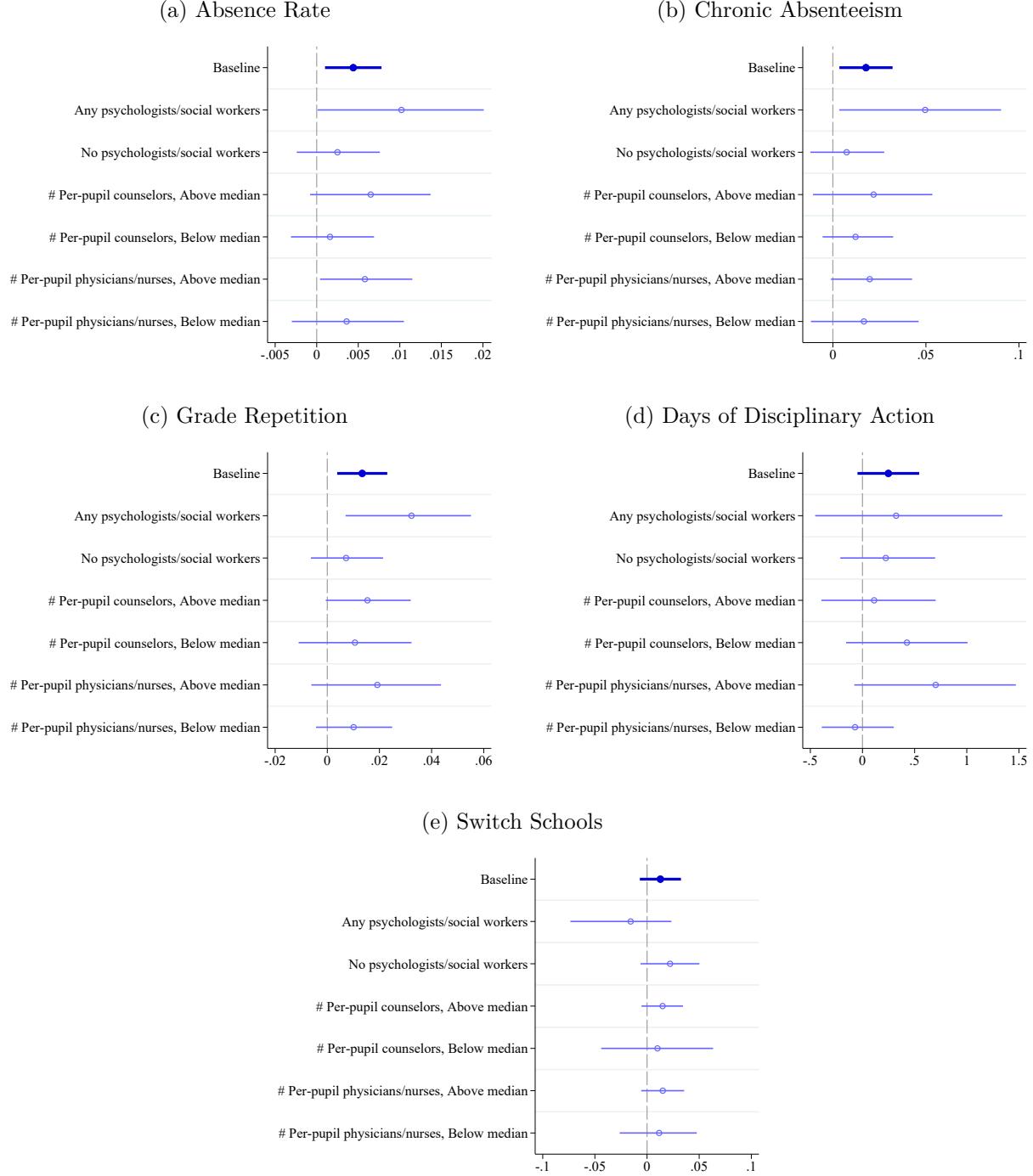
Notes: These figures present output from estimation of equation (2). In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the indicators denoting each of the years before and after a shooting. The academic year before the shooting is the omitted category. The regressions include individual and match group-by-year fixed effects. Standard errors are clustered by school. 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 (d) includes a subset of 26 shooting and 52 control schools. We restrict the sample to students who are observed in the data over the period of three years before to two years after a shooting (i.e., the panel is balanced); see Appendix Figure A11 for results using an unbalanced panel. Pre-shooting outcome means are calculated based on both the treatment and control group schools.

Figure 4: Short-Run Effects on Educational Outcomes: Heterogeneity by Student Characteristics



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

Figure 5: Short-Run Effects on Educational Outcomes: Heterogeneity by School Mental Health Resources



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

Figure 6: Long-Run Effects of Shootings at Schools on Educational Outcomes by Age 26

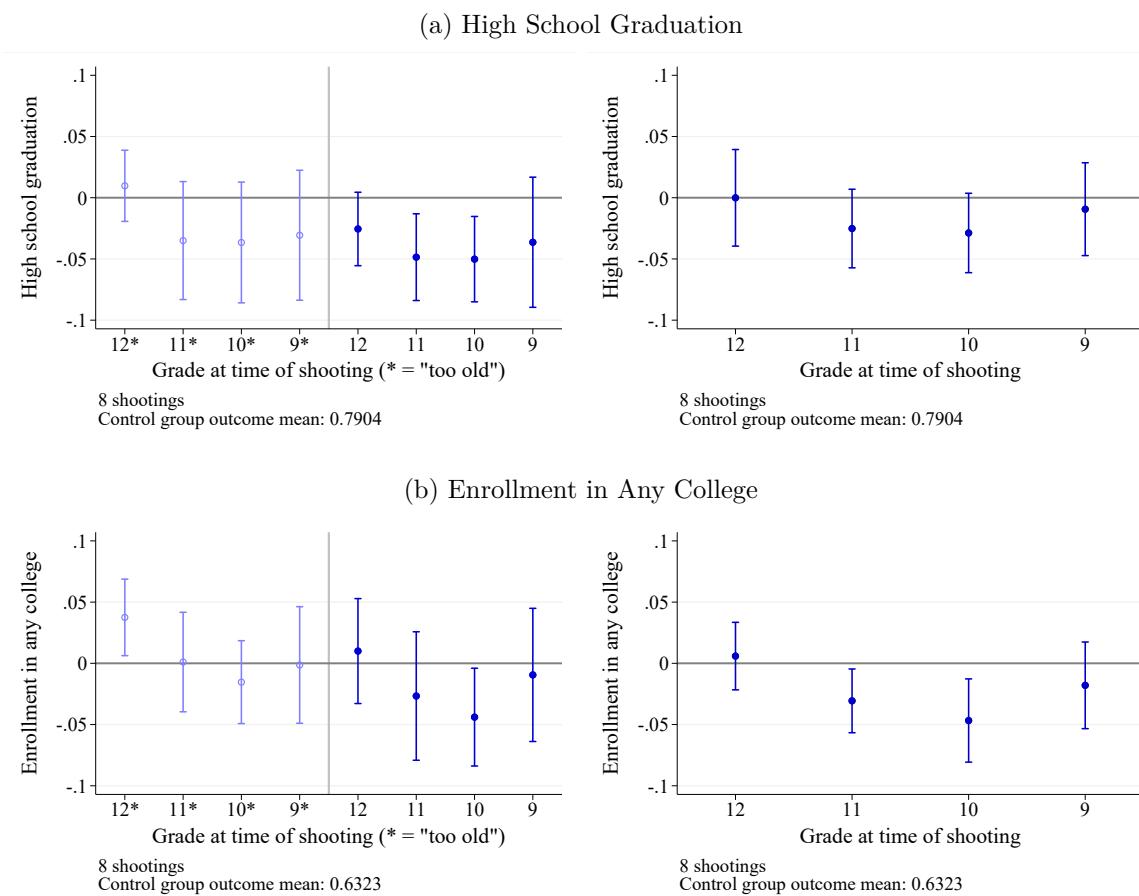
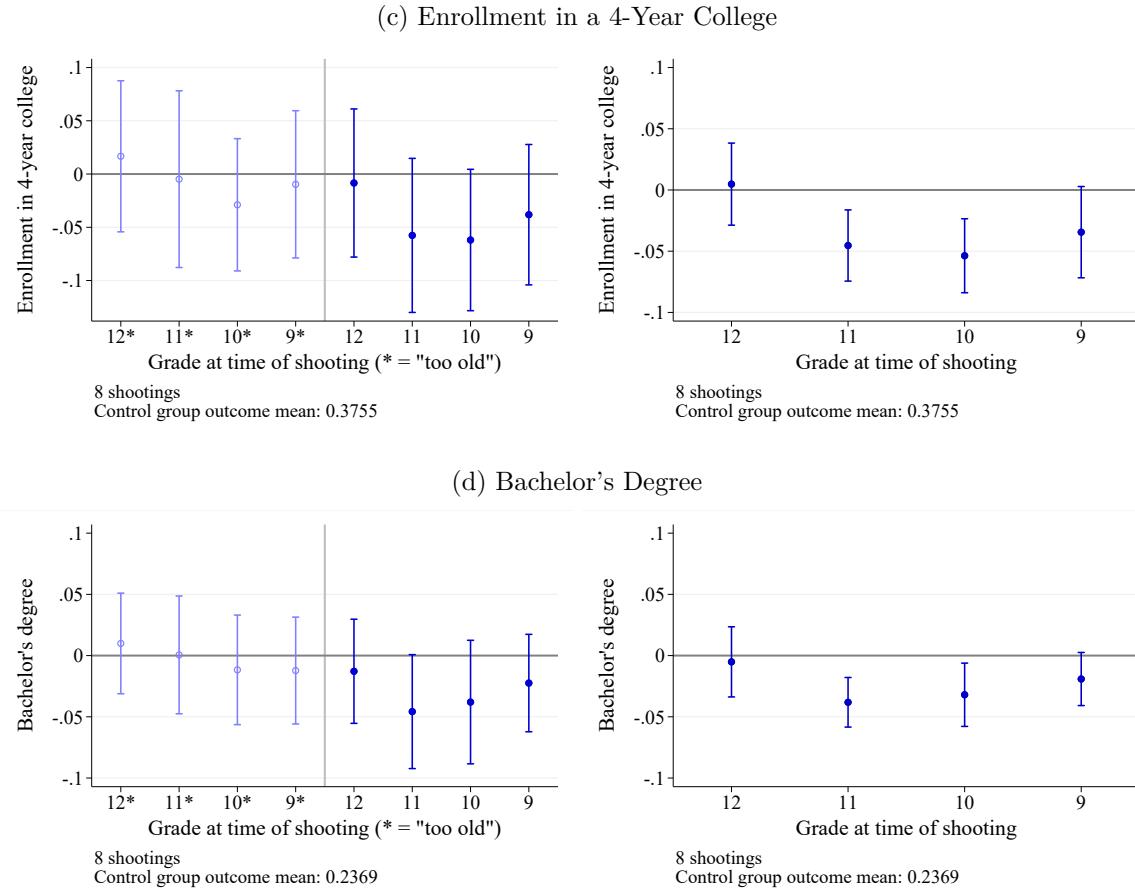


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Figure 6: Long-Run Effects of Shootings at Schools on Educational Outcomes (continued)



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

Figure 7: Long-Run Effects of Shootings at Schools on Labor Market Outcomes at Ages 24–26

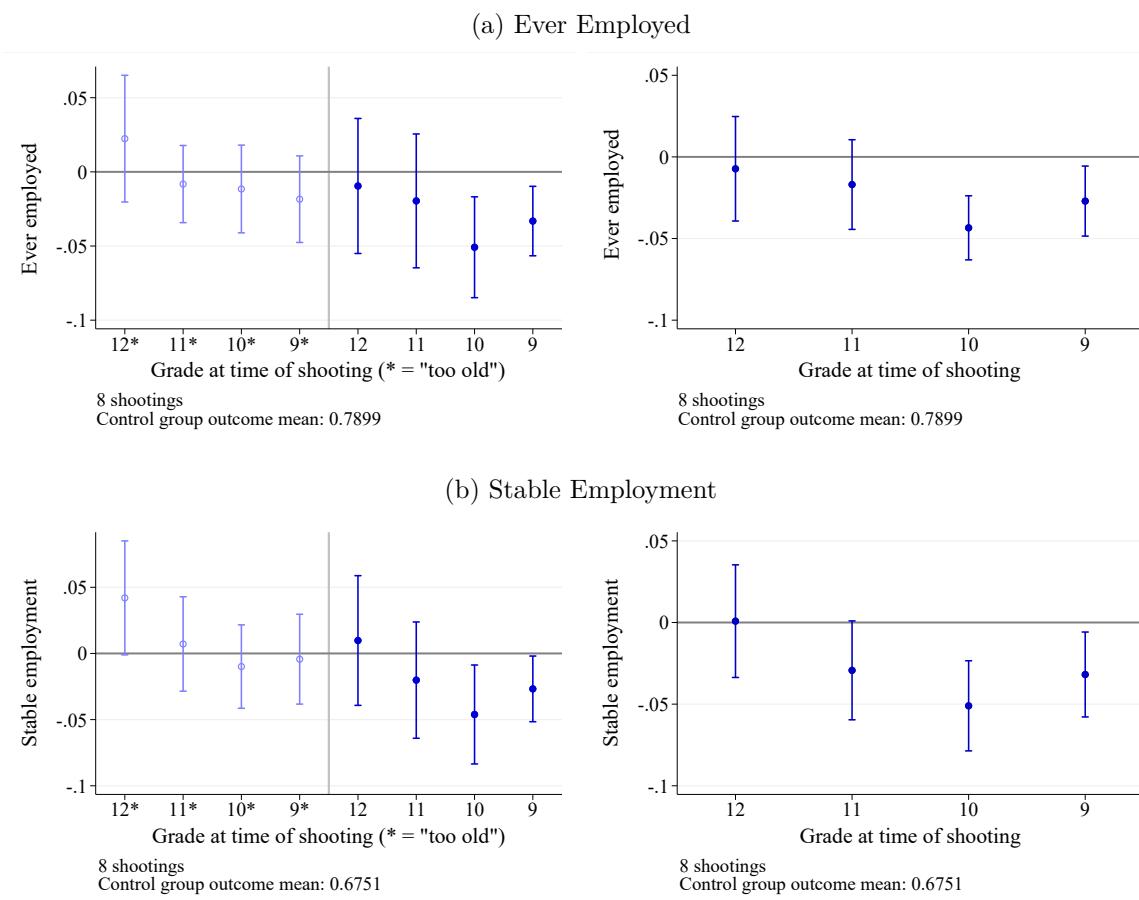
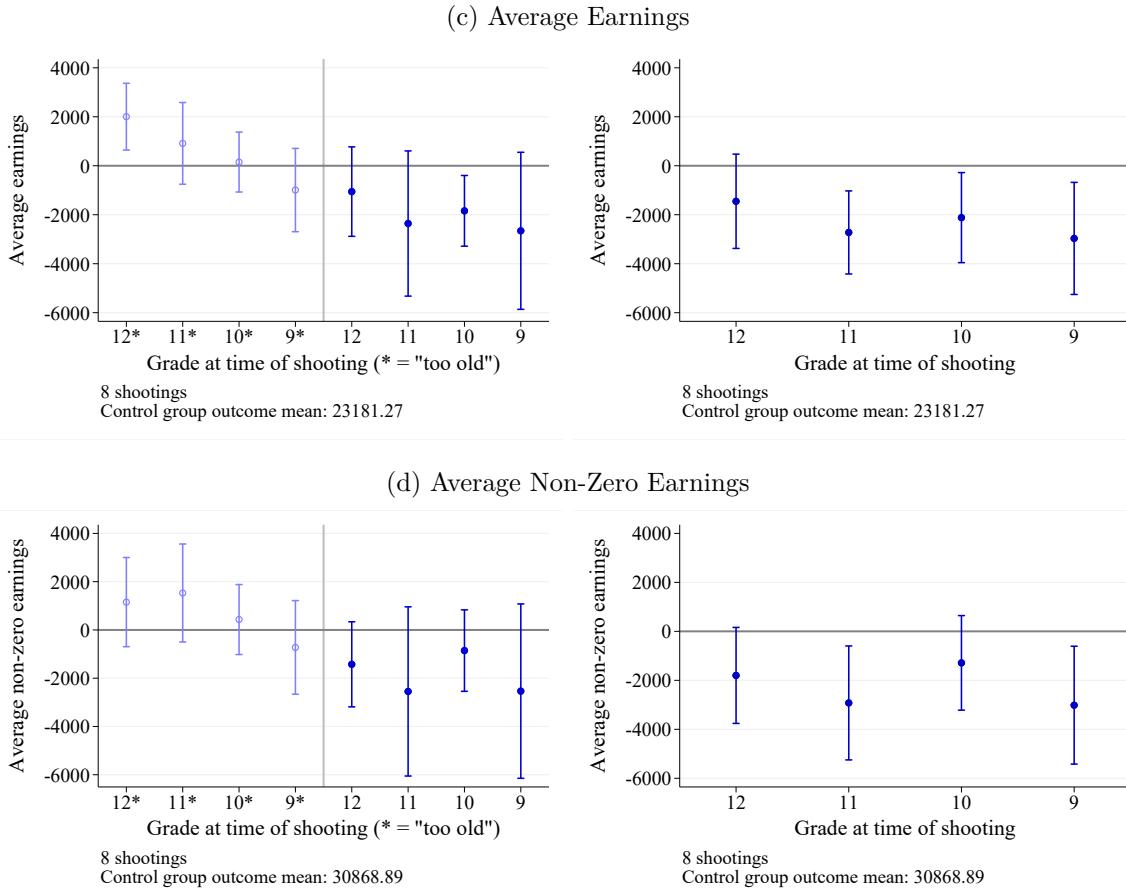


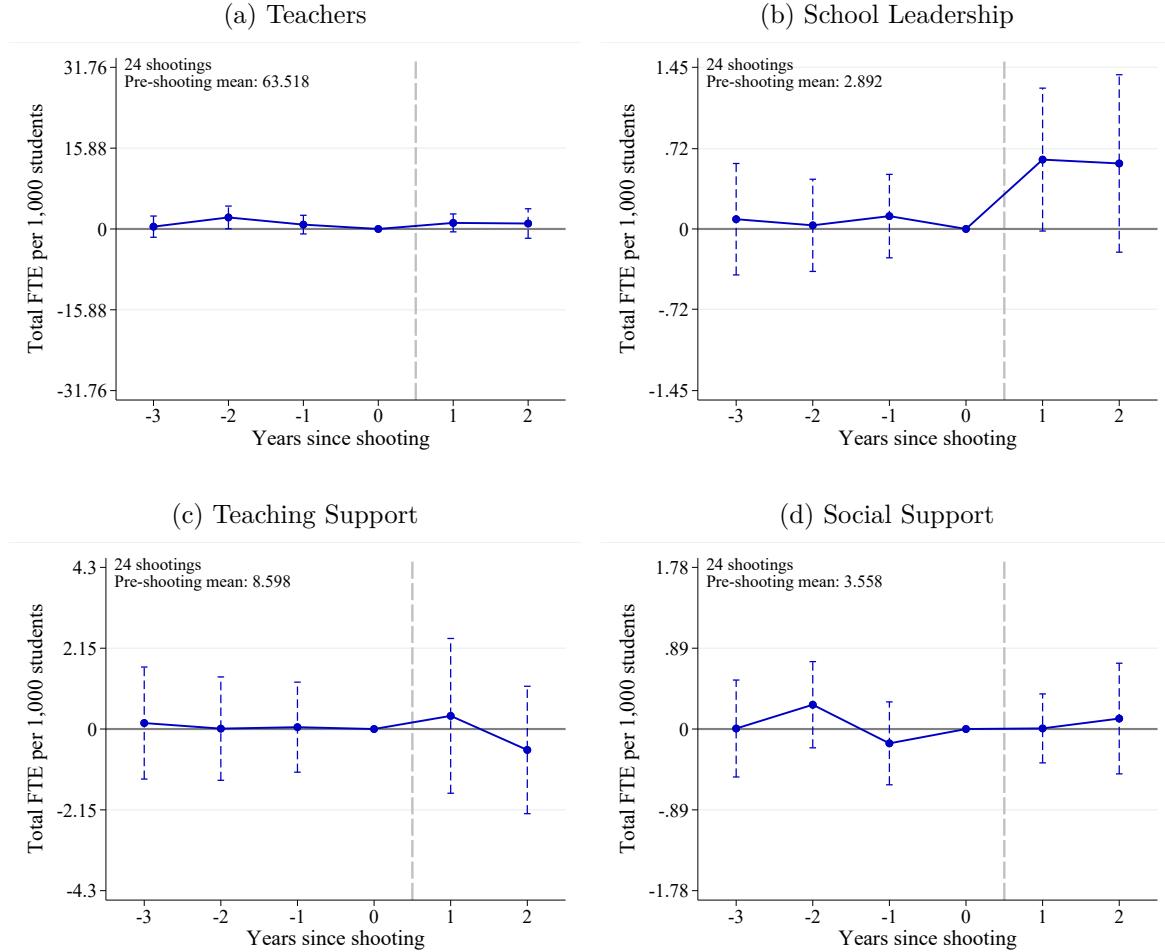
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Figure 7: Long-Run Effects of Shootings at Schools on Labor Market Outcomes at Ages 24–26
(continued)



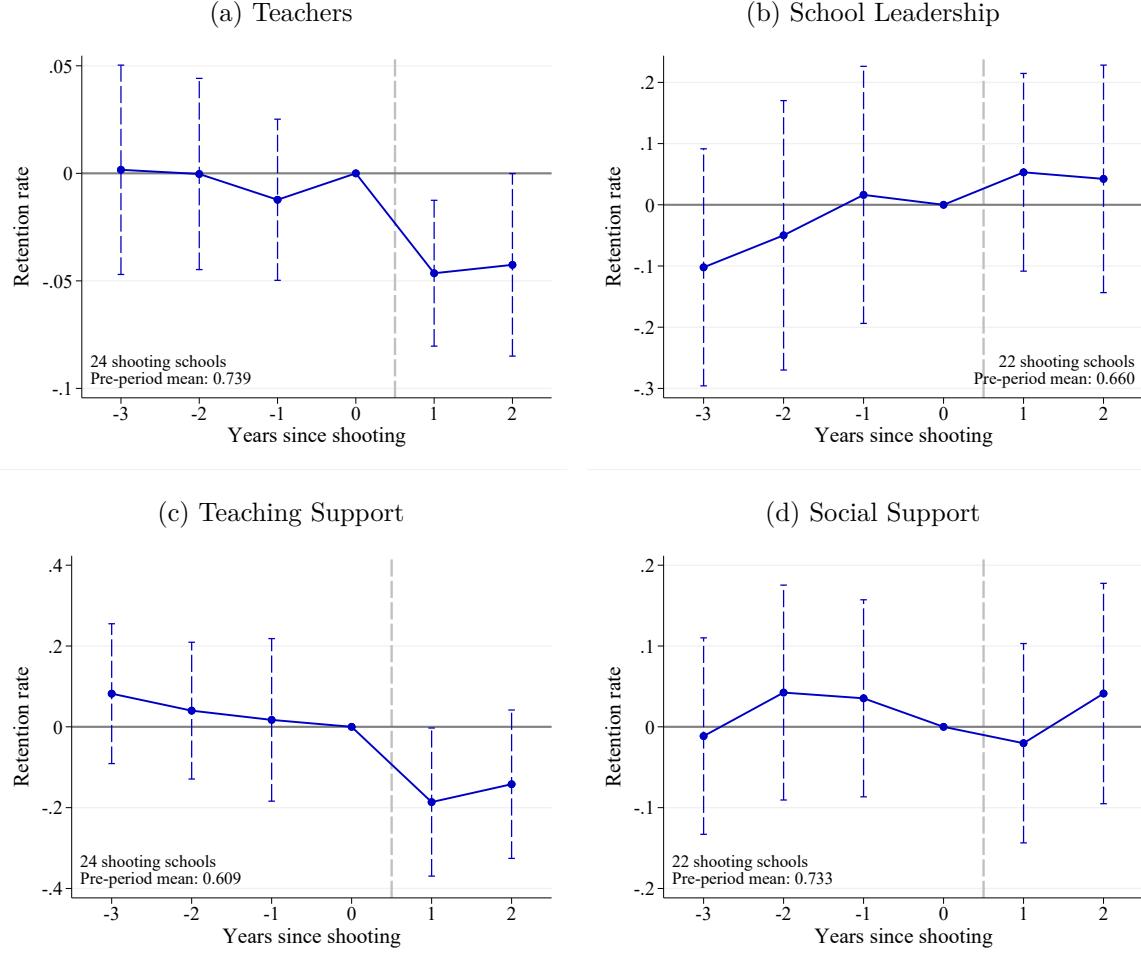
Notes: In each sub-figure, the graph on the left-hand side presents output from estimation of equation (3), while the graph on the right-hand side presents output from estimation of equation (4). In both cases, we plot the coefficients and 95% confidence intervals on the interactions between the indicator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

Figure 8: Effects of Shootings at Schools on School Staff Employment



Notes: These figures present output from estimation described in Section 5.4. In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the indicators denoting each of the years before and after a shooting. The academic year of the shooting is the omitted category. The regressions include school and match group-by-year fixed effects. School-by-academic year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting, and standard errors are clustered by school. In order to make effect sizes more comparable across the four groups of staff, the y-axes in these figures are scaled to range from -50 percent to $+50$ percent of the pre-period mean of each outcome. As outlined in footnote 41, the staffing analysis includes 24 shooting and 48 control schools. Pre-shooting outcome means are calculated based on both the treatment and control group schools.

Figure 9: Effects of Shootings at Schools on Retention of Full-Time Staff



Notes: These figures present output from estimation described in Section 5.4. In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the indicators denoting each of the years before and after a shooting. The academic year of the shooting is the omitted category. We focus on the staff that were employed full time at each of the shooting and control schools in our staff analysis sample at the time of the shooting and analyze changes in the probability of full-time employment at the same school both before and after the shooting. The regressions include school and match group-by-year fixed effects. School-by-academic year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting, and standard errors are clustered by school. Sub-figures (a) and (c) include all 24 shooting and 48 control schools included in our staffing analysis; since we drop match groups in which either a shooting school or both control schools had no full-time employees in a given staff group at the time of the shooting, sub-figures (b) and (d) only include 22 match groups. Pre-shooting outcome means are calculated based on both the treatment and control group schools.

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

| | Absence Rate (1) | Chronic Absenteeism (2) | Grade Repetition (3) | Days of Disc. Act. (4) | Switch Schools (5) |
|---------------------------|-------------------------------|-------------------------------|-------------------------------|-------------------------------|-------------------------------|
| Shooting School x Post | 0.0044 (0.0017) [0.012] | 0.0178 (0.0073) [0.016] | 0.0134 (0.0049) [0.008] | 0.2478 (0.1510) [0.105] | 0.0128 (0.0101) [0.205] |
| Pre-shooting outcome mean | 0.0365 | 0.0645 | 0.0106 | 1.9849 | 0.1109 |
| Student-year observations | 368,142 | 368,142 | 368,142 | 276,114 | 365,675 |
| R-squared | 0.553 | 0.481 | 0.233 | 0.429 | 0.283 |

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

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

| | Graduate HS (1) | Enroll Any Col (2) | Enroll 4yr Col (3) | Bachelor's Degree (4) |
|-----------------------------|--------------------------------|--------------------------------|--------------------------------|---------------------------------|
| Shooting School x Cohort 12 | -0.0001 (0.0200) [0.997] | 0.0059 (0.0140) [0.673] | 0.0048 (0.0170) [0.779] | -0.0051 (0.0145) [0.725] |
| Shooting School x Cohort 11 | -0.0252 (0.0163) [0.124] | -0.0306 (0.0132) [0.021] | -0.0454 (0.0147) [0.002] | -0.0381 (0.0103) [<0.001] |
| Shooting School x Cohort 10 | -0.0288 (0.0164) [0.081] | -0.0467 (0.0172) [0.007] | -0.0537 (0.0153) [0.001] | -0.032 (0.0131) [0.016] |
| Shooting School x Cohort 9 | -0.0094 (0.0192) [0.627] | -0.018 (0.0179) [0.317] | -0.0345 (0.0189) [0.070] | -0.0191 (0.0110) [0.083] |
| Control group outcome mean | 0.7904 | 0.6323 | 0.3755 | 0.2369 |
| Student observations | 59,808 | 54,526 | 54,526 | 54,526 |
| R-squared | 0.119 | 0.082 | 0.092 | 0.080 |

Notes: This table presents coefficients, standard errors (in parentheses), and p -values [in brackets] from estimation of equation (4). The regressions include match group-by-cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level. Outcome means are calculated based on the cohorts enrolled at the matched control schools at the time of the shooting.

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

| | Ever Employed (1) | Stable Employment (2) | Earnings (3) | Non-Zero Earnings (4) |
|-----------------------------|---------------------------------|---------------------------------|------------------------------------|------------------------------------|
| Shooting School x Cohort 12 | -0.0073 (0.0162) [0.655] | 0.0008 (0.0175) [0.962] | -1,450.93 (976.28) [0.139] | -1,799.61 (993.58) [0.072] |
| Shooting School x Cohort 11 | -0.0169 (0.0139) [0.225] | -0.0293 (0.0154) [0.058] | -2,723.49 (859.93) [0.002] | -2,922.87 (1,179.62) [0.014] |
| Shooting School x Cohort 10 | -0.0434 (0.0100) [<0.001] | -0.0510 (0.0140) [<0.001] | -2,117.97 (932.32) [0.024] | -1,286.74 (978.83) [0.190] |
| Shooting School x Cohort 9 | -0.0271 (0.0109) [0.014] | -0.0318 (0.0132) [0.017] | -2,967.47 (1,159.92) [0.011] | -3,014.35 (1,220.21) [0.014] |
| Control group outcome mean | 0.7899 | 0.6751 | 23,181.27 | 30,868.89 |
| Student observations | 54,526 | 54,526 | 54,526 | 42,942 |
| R-squared | 0.017 | 0.019 | 0.016 | 0.021 |

Notes: This table presents coefficients, standard errors (in parentheses), and p -values [in brackets] from estimation of equation (4). The regressions include match group-by-cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level. Outcome means are calculated based on the cohorts enrolled at the matched control schools at the time of the shooting.

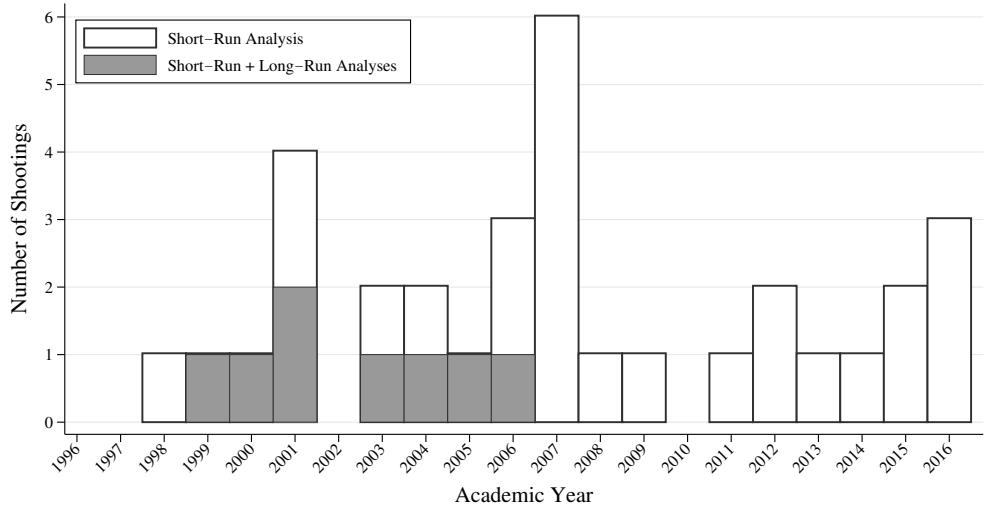
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Trauma at School: The Impacts of Shootings on Students' Human Capital and Economic Outcomes

Cabral, Kim, Rossin-Slater, Schnell, Schwandt (2024)

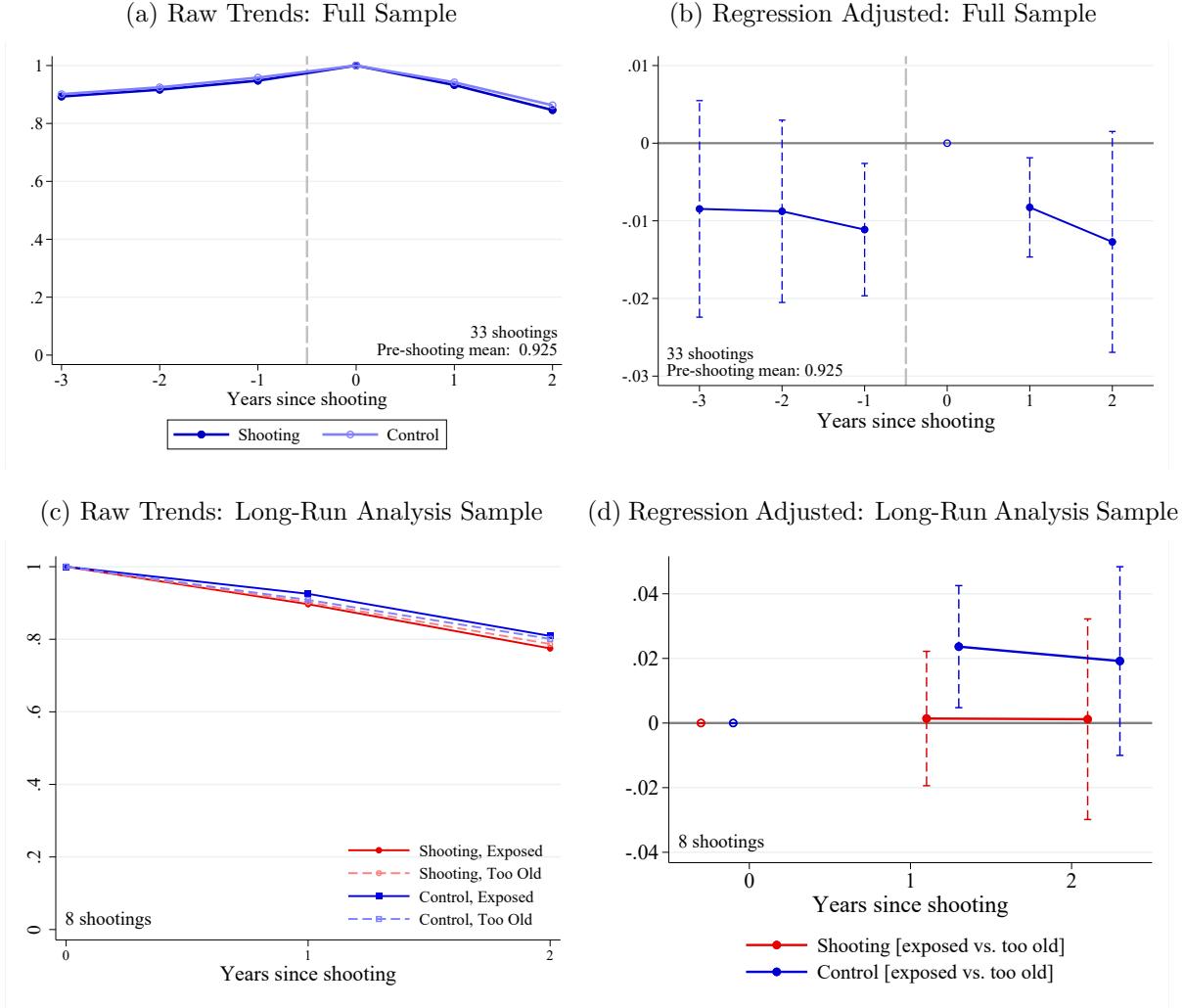
A Appendix Figures

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



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

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



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

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



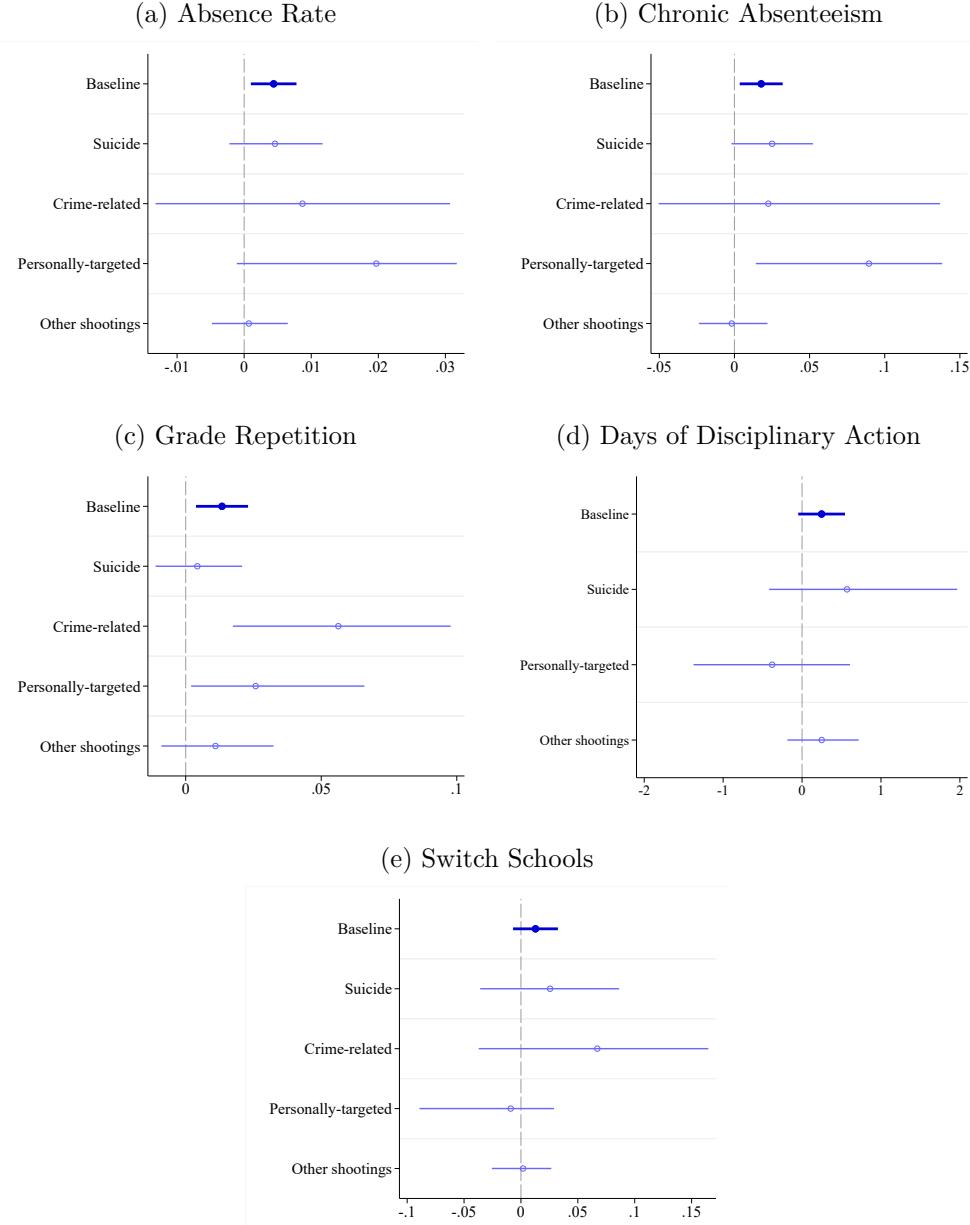
Notes: These figures present output from estimation of equation (1) for students belonging to the sub-group denoted on the y-axis. In particular, we plot the coefficients and 95% confidence intervals on the interaction between the indicator denoting shooting schools and the post indicator; coefficient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

Figure A4: Short-Run Effects on Educational Outcomes: Heterogeneity by Baseline School Resources



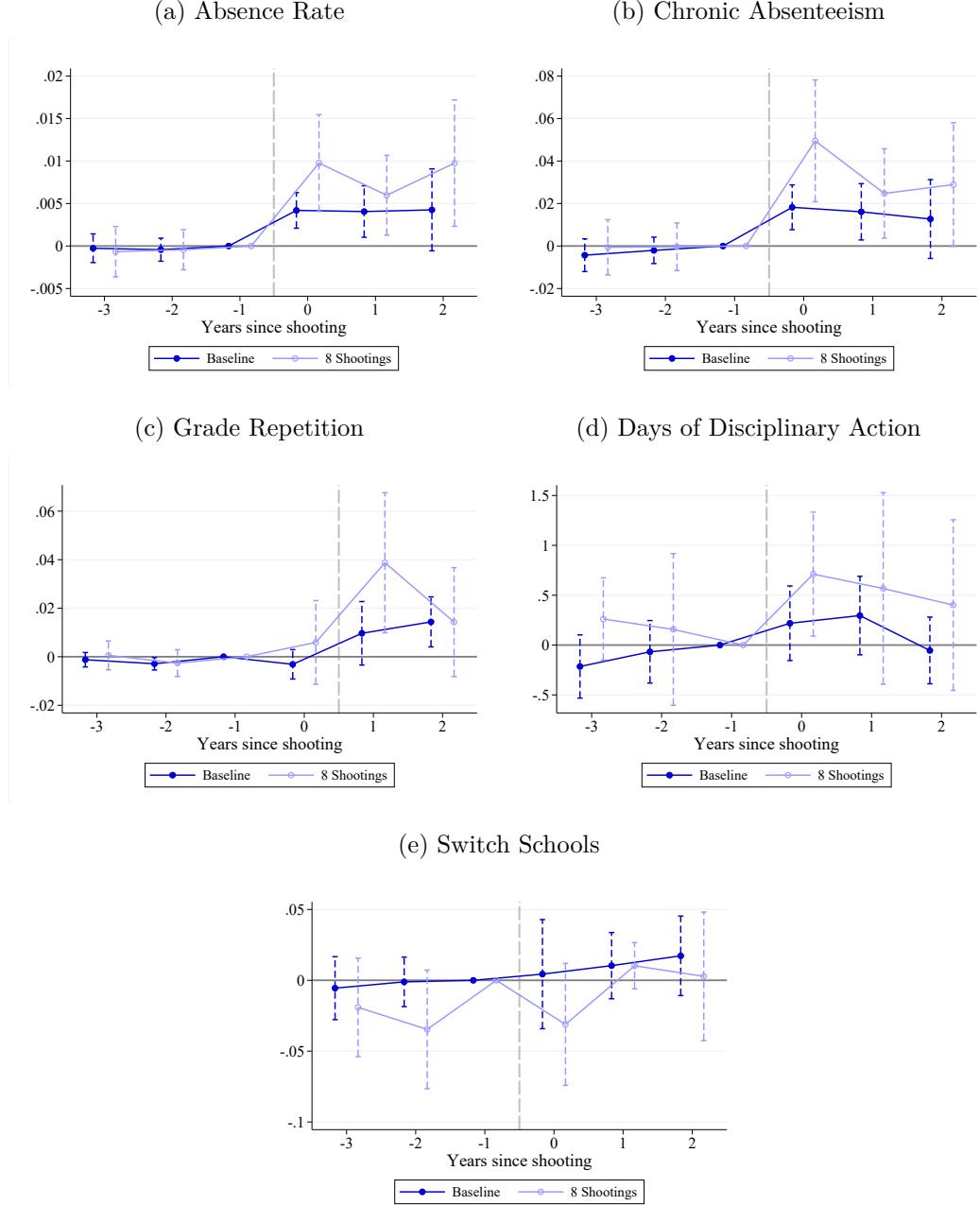
Notes: These figures present output from estimation of equation (1) for shootings at schools with differing availability of school-level resources. In particular, we plot the coefficients and 95% confidence intervals on the interaction between the indicator denoting shooting schools and the post indicator.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

Figure A5: Short-Run Effects on Educational Outcomes: Heterogeneity by Shooting Type



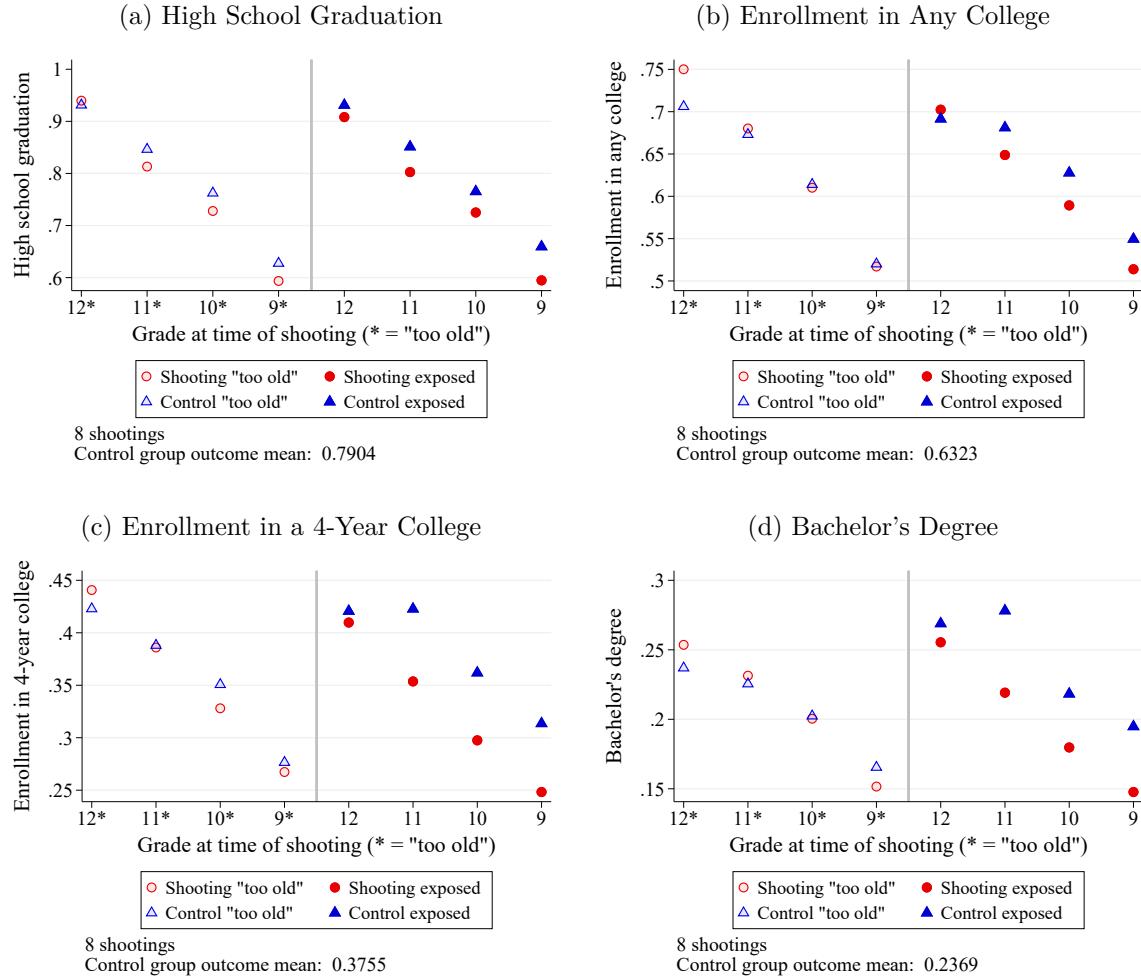
Notes: These figures present output from estimation of equation (1) for the shooting type denoted on the y-axis. In particular, we plot the coefficients and 95% confidence intervals on the interaction between the indicator denoting shooting schools and the post indicator. Confidence intervals are based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

Figure A6: Short-Run Effects on Educational Outcomes: Long-Run Versus Short-Run Analysis Sample



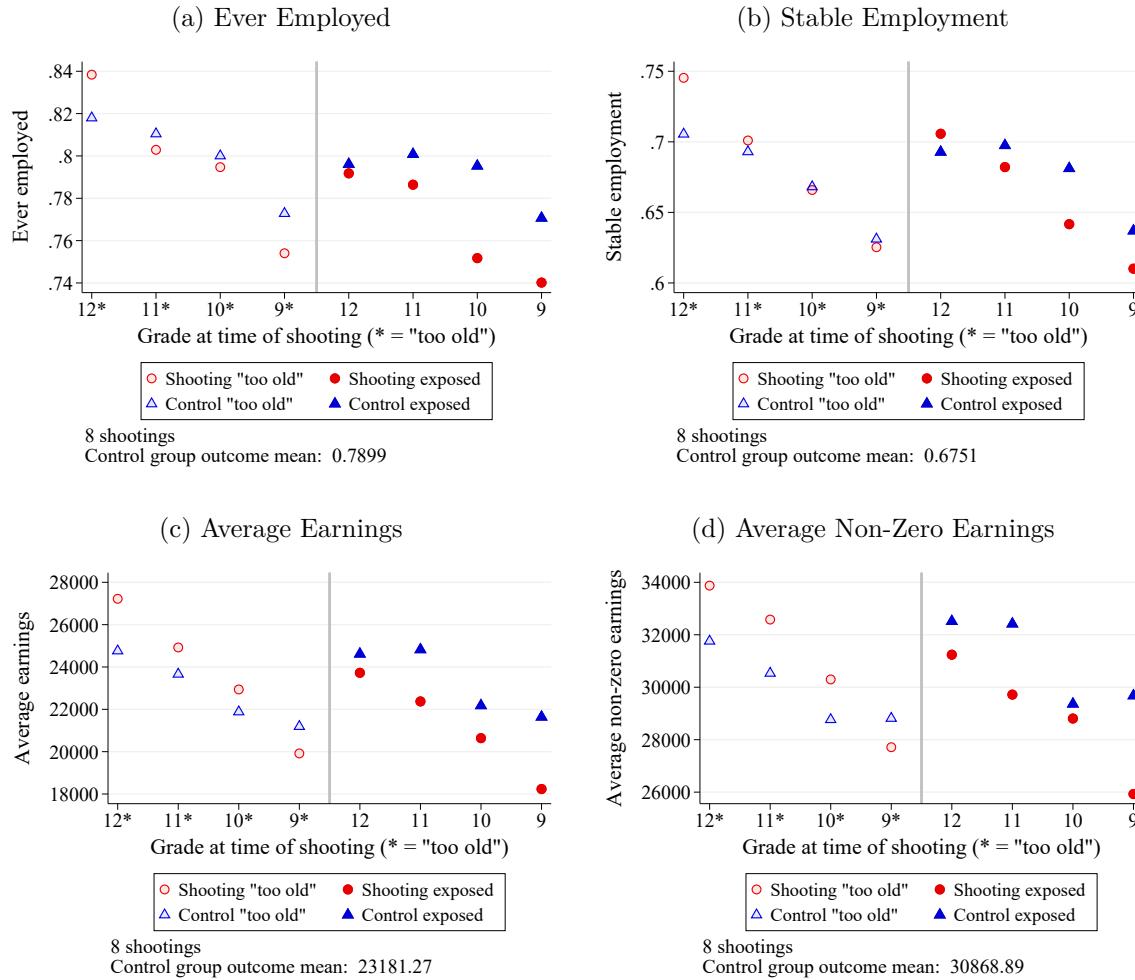
Notes: These figures present output from estimation of equation (2) using the 33 (8) shootings in our short-run (long-run) analysis sample. In both cases, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting a shooting school and the indicators denoting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

Figure A7: Raw Trends in Long-Run Educational Outcomes Across Shooting and Control Schools



Notes: These figures plot raw cohort-level means of our long-run educational outcomes for each of the eight cohorts of students included in our long-run analysis: those who were in grades 9, 10, 11, and 12 at the time of the shooting (displayed to the right of the vertical line on each plot) and the “too old” cohorts who are observed in our data at the same schools in these grades five years earlier (displayed to the left of the vertical line on each plot). Outcome means are calculated based on the cohorts enrolled at the matched control schools at the time of the shooting.

Figure A8: Raw Trends in Long-Run Labor Market Outcomes Across Shooting and Control Schools



Notes: These figures plot raw cohort-level means of our long-run labor market outcomes for each of the eight cohorts of students included in our long-run analysis: those who were in grades 9, 10, 11, and 12 at the time of the shooting (displayed to the right of the vertical line on each plot) and the “too old” cohorts who are observed in our data at the same schools in these grades five years earlier (displayed to the left of the vertical line on each plot). Outcome means are calculated based on the cohorts enrolled at the matched control schools at the time of the shooting.

Figure A9: Long-Run Effects on Educational Outcomes by Age 26: Heterogeneity by Student Characteristics

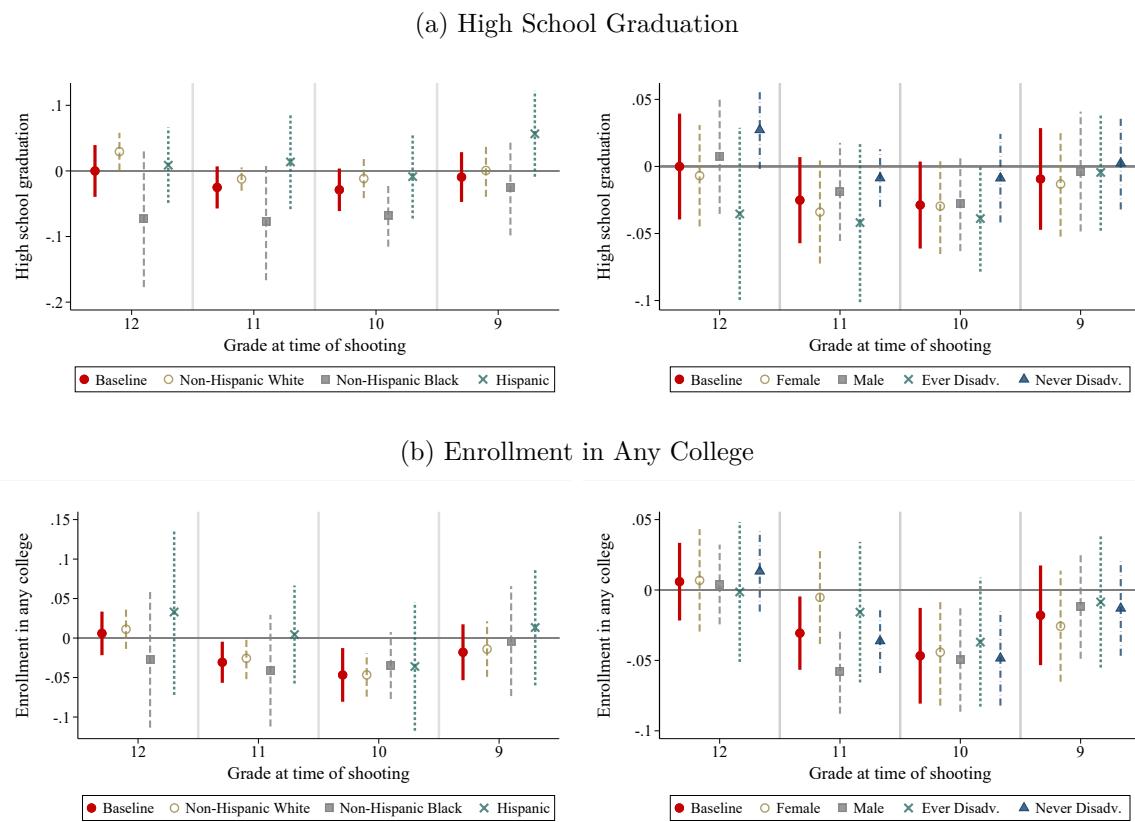
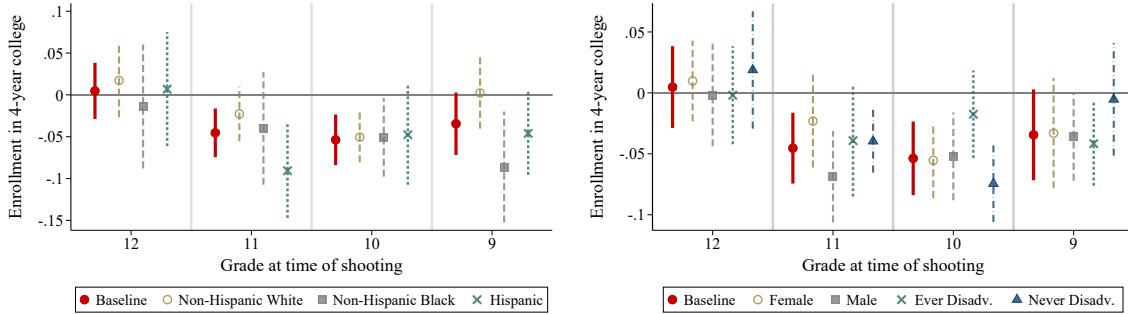


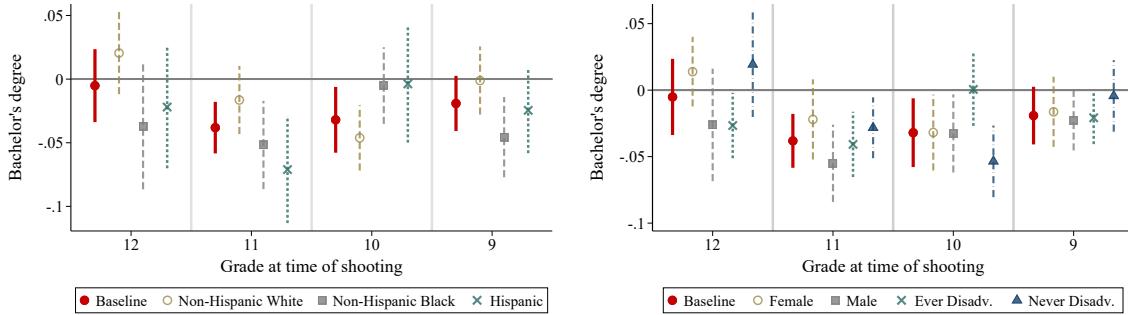
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Figure A9: Long-Run Effects on Educational Outcomes by Age 26: Heterogeneity by Student Characteristics (continued)

(c) Enrollment in a 4-Year College



(d) Bachelor's Degree



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

Figure A10: Long-Run Effects on Labor Market Outcomes at Ages 24–26: Heterogeneity by Student Characteristics

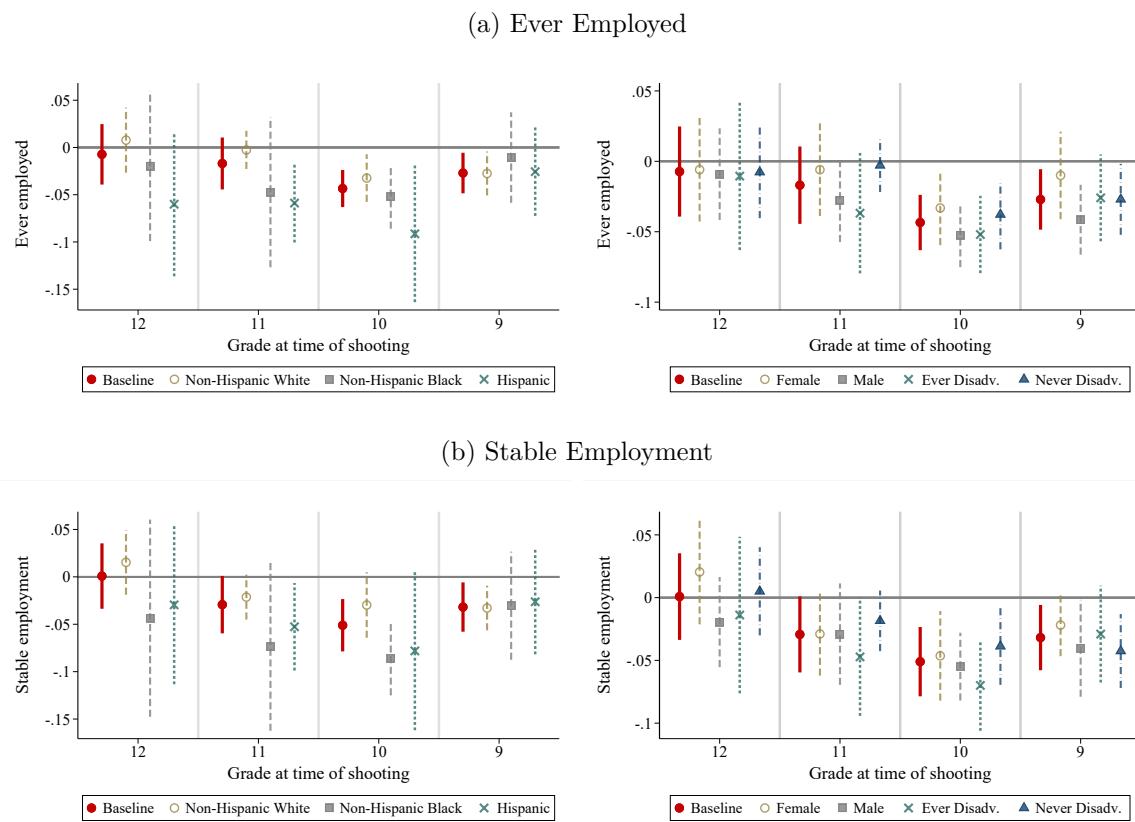
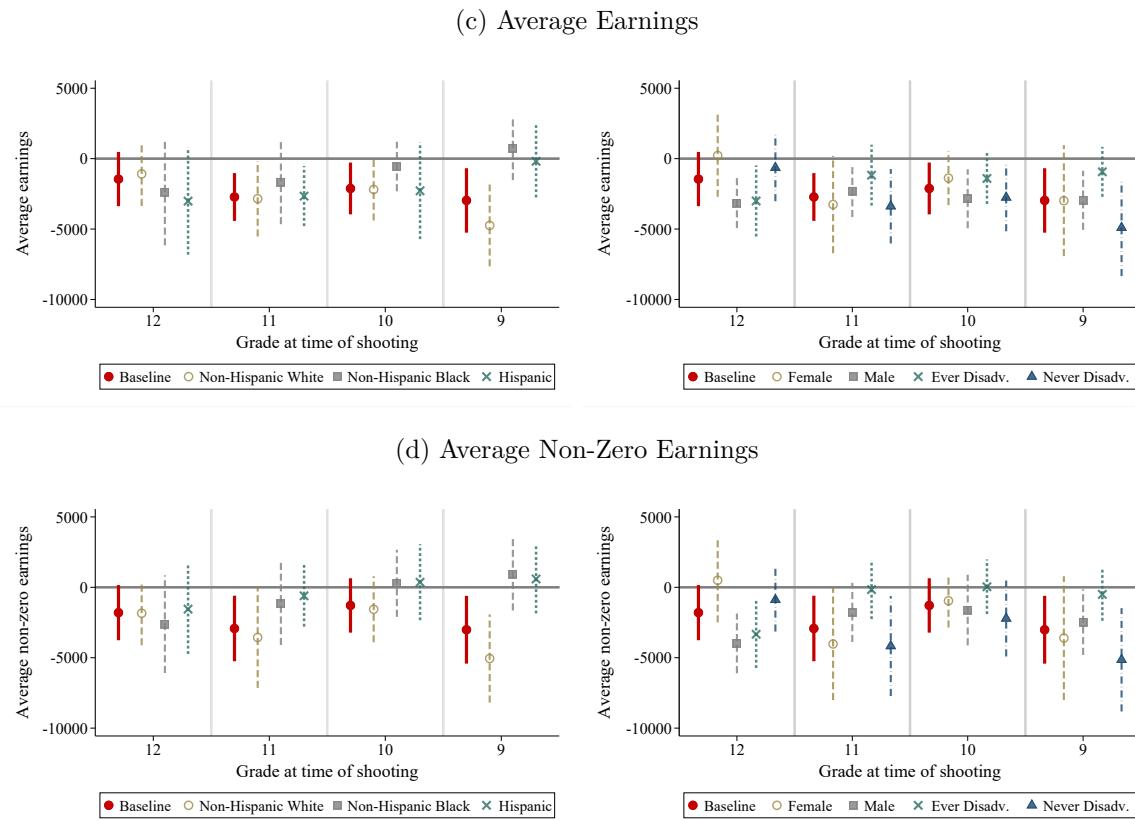


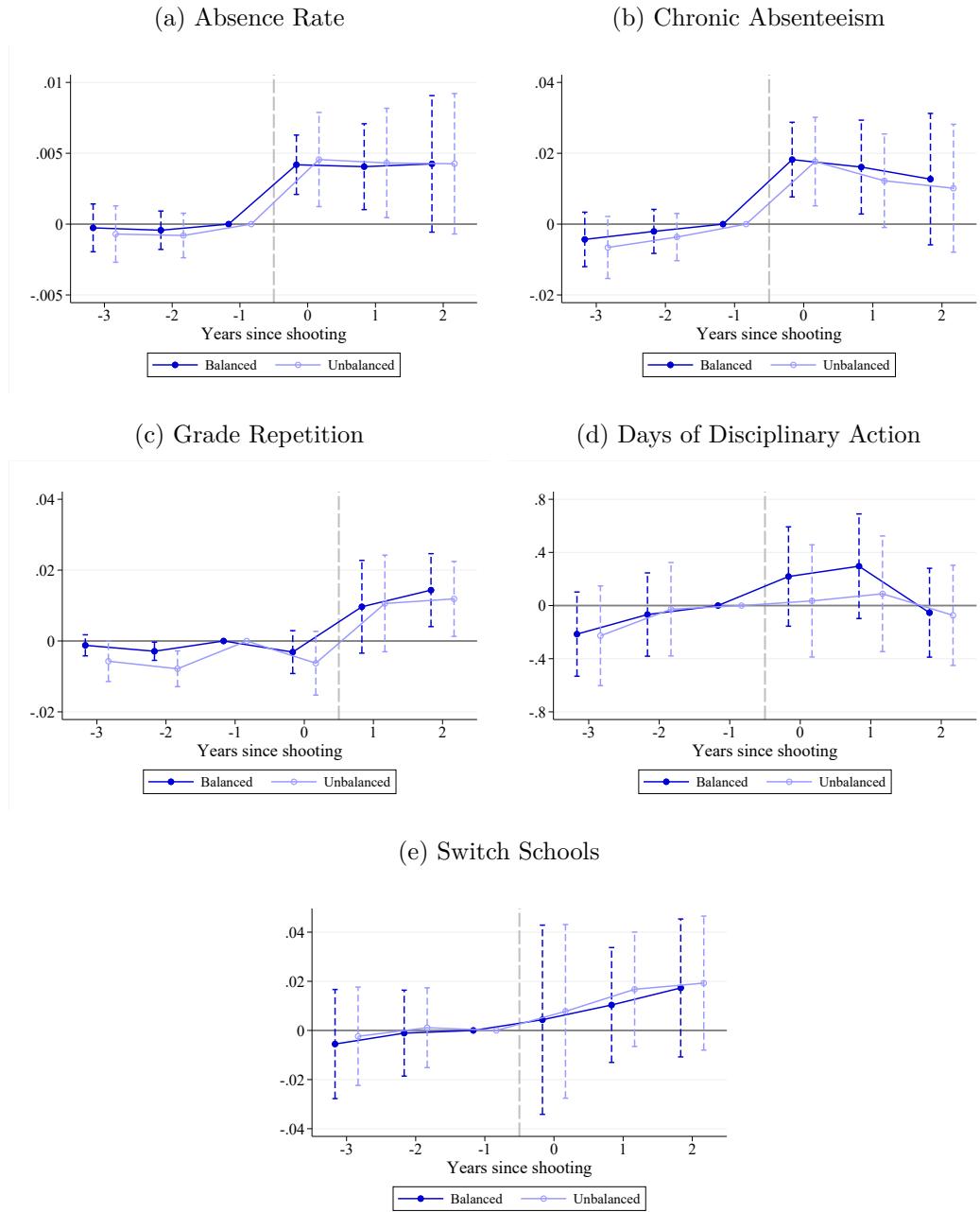
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Figure A10: Long-Run Effects on Labor Market Outcomes at Ages 24–26: Heterogeneity by Student Characteristics



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

Figure A11: Short-Run Effects on Educational Outcomes: Balanced Versus Unbalanced Panels



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

Figure A12: Short-Run Effects on Educational Outcomes: Alternative Matching Strategies



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

Figure A13: Long-Run Effects on Educational Outcomes by Age 26: Alternative Matching Strategies

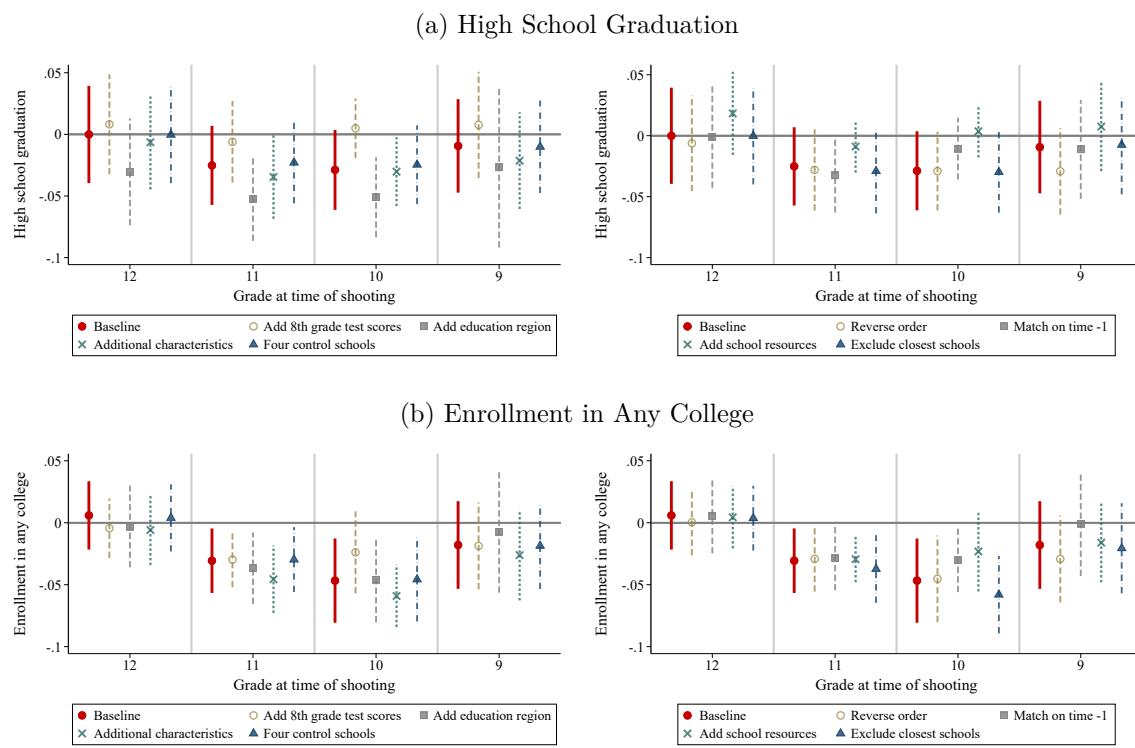


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Figure A13: Long-Run Effects on Educational Outcomes by Age 26: Alternative Matching Strategies (continued)

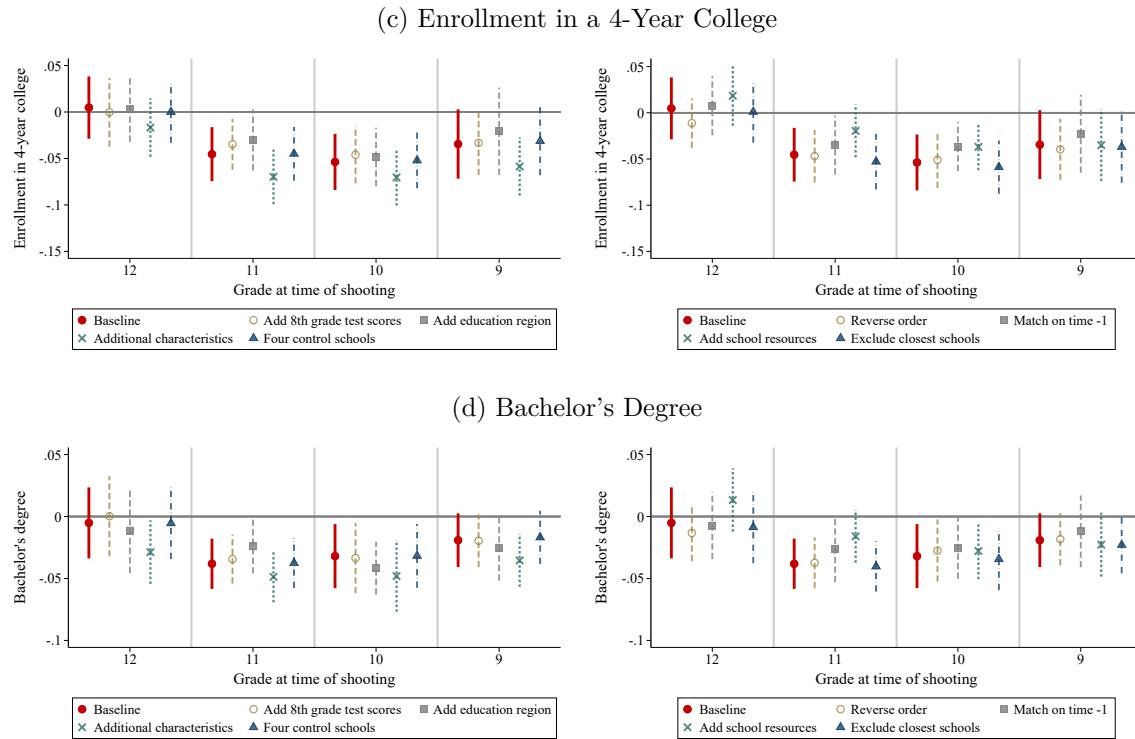


Figure A14: Long-Run Effects on Labor Market Outcomes at Ages 24–26: Alternative Matching Strategies

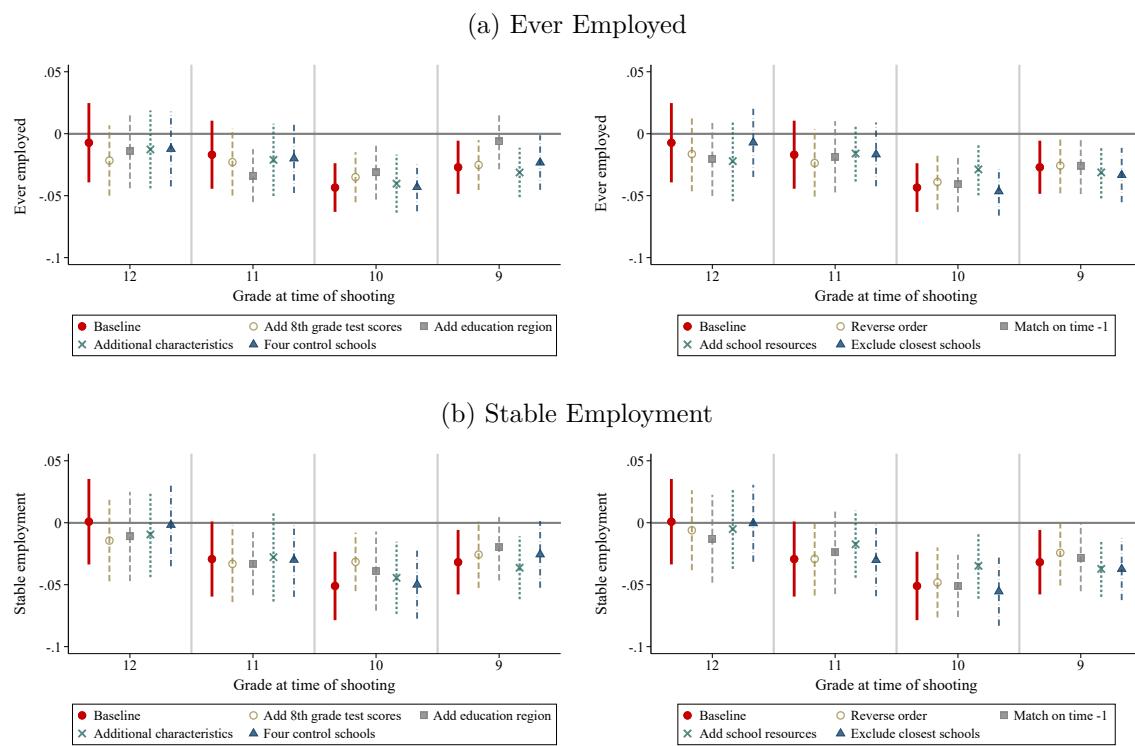
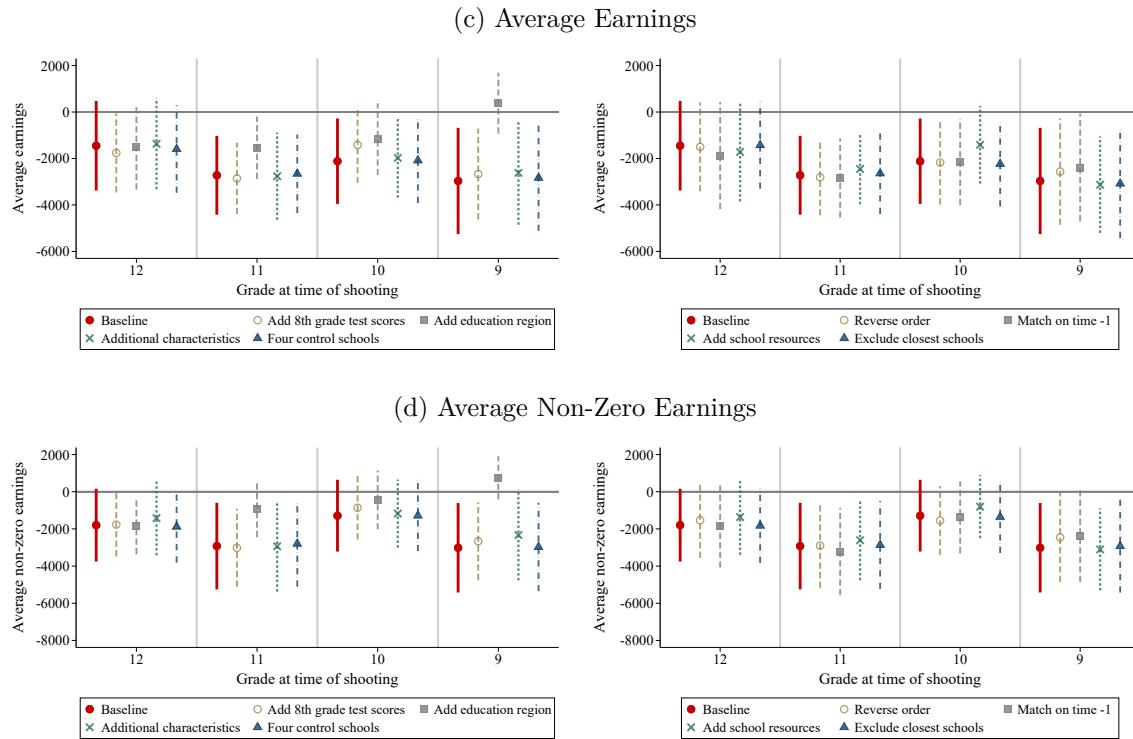


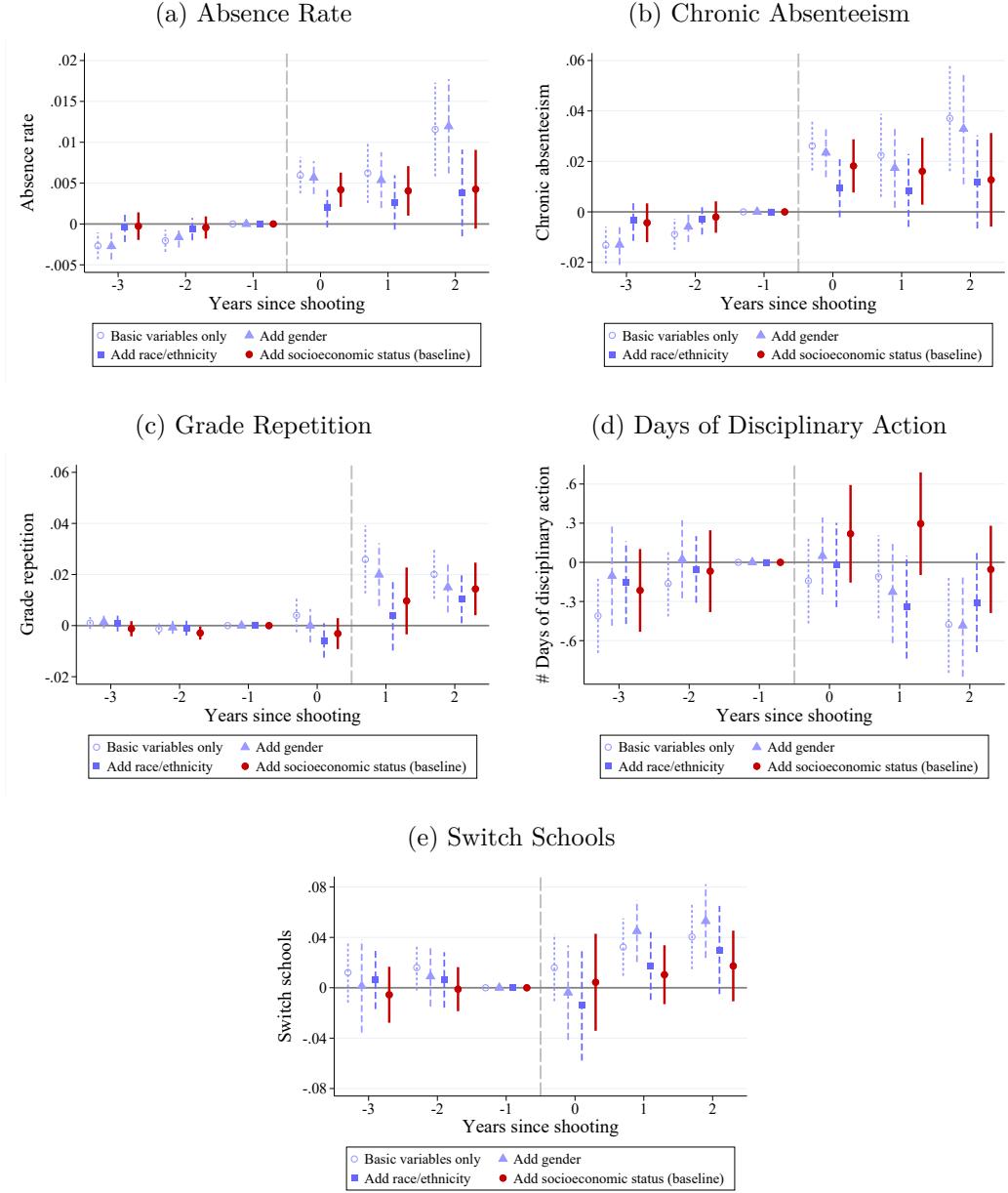
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Figure A14: Long-Run Effects on Labor Market Outcomes at Ages 24–26: Alternative Matching Strategies



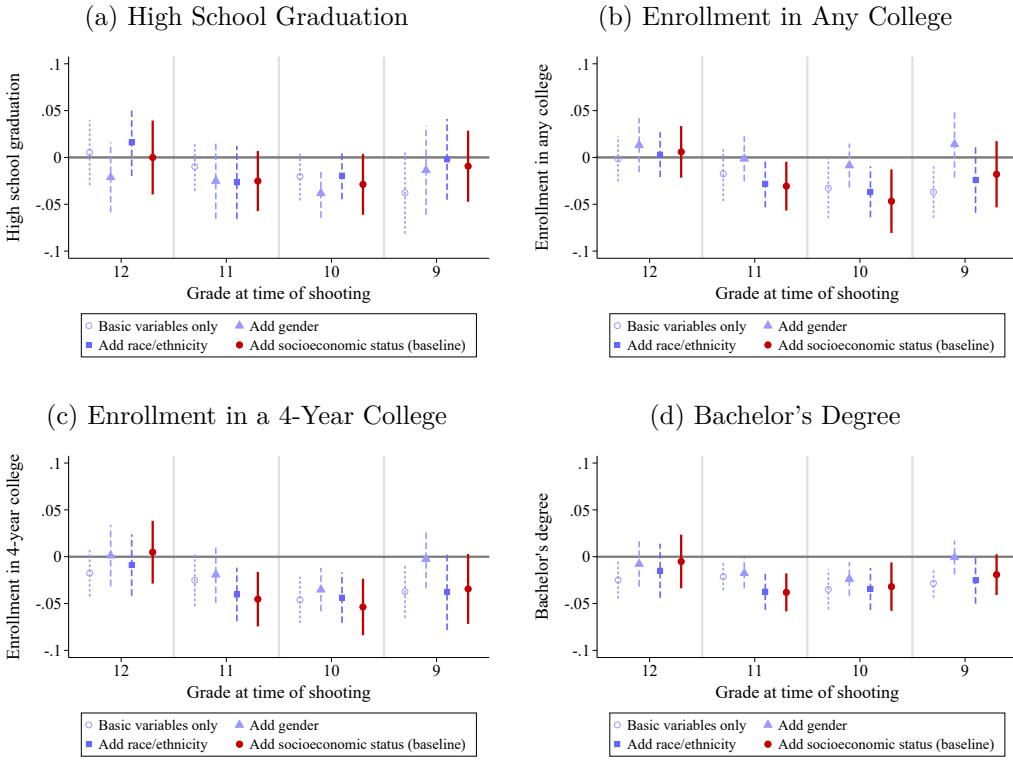
Notes: These figures present output from estimation of equation (4) using control schools selected from the matching strategy denoted in the graph legends. In particular, we plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the set of cohort indicators. Our baseline estimates—which use our baseline sample of matched control schools—are presented in solid red at the left of each sub-figure. The specification includes match group-by-cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level.

Figure A15: Short-Run Effects on Educational Outcomes: Layering in Matching Variables



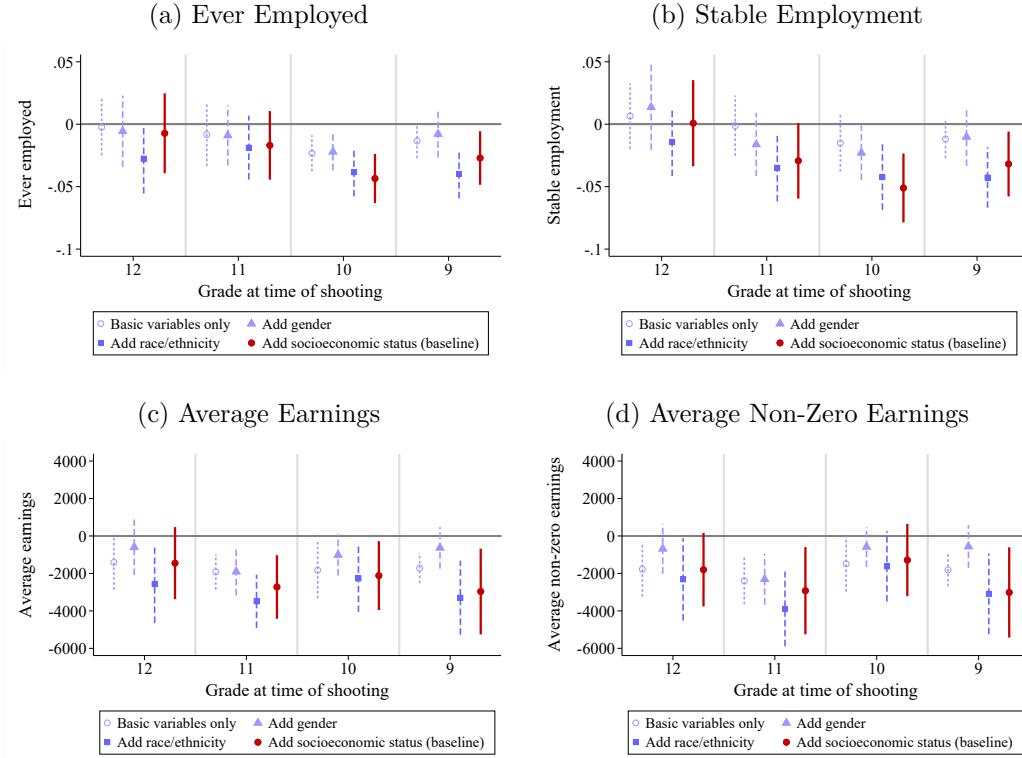
Notes: These figures present output from estimation of equation (2) using control schools selected from the matching procedure with the fuzzy match variables specified in the graph legends. Specifically, we begin by only matching on grade levels and the urbanicity categories. We then select two control schools for every shooting-exposed school from this set of potential matches by selecting the two schools that are the most similar to the exposed school in terms of student enrollment. We refer to this procedure as “Base variables only” in the figures. To move toward our baseline specification, we then gradually add the following fuzzy match variables: share of students by gender, share of students by race/ethnicity, and share of students on free or reduced-price lunch. Our baseline estimates—which use our baseline set of fuzzy match variables—are presented in solid red at the right of each sub-figure. We plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the indicators denoting each of the years before and after a shooting. The academic year before the shooting is the omitted category. The regressions include individual and match group-by-year fixed effects. Standard errors are clustered by school.

Figure A16: Long-Run Effects on Educational Outcomes: Layering in Matching Variables



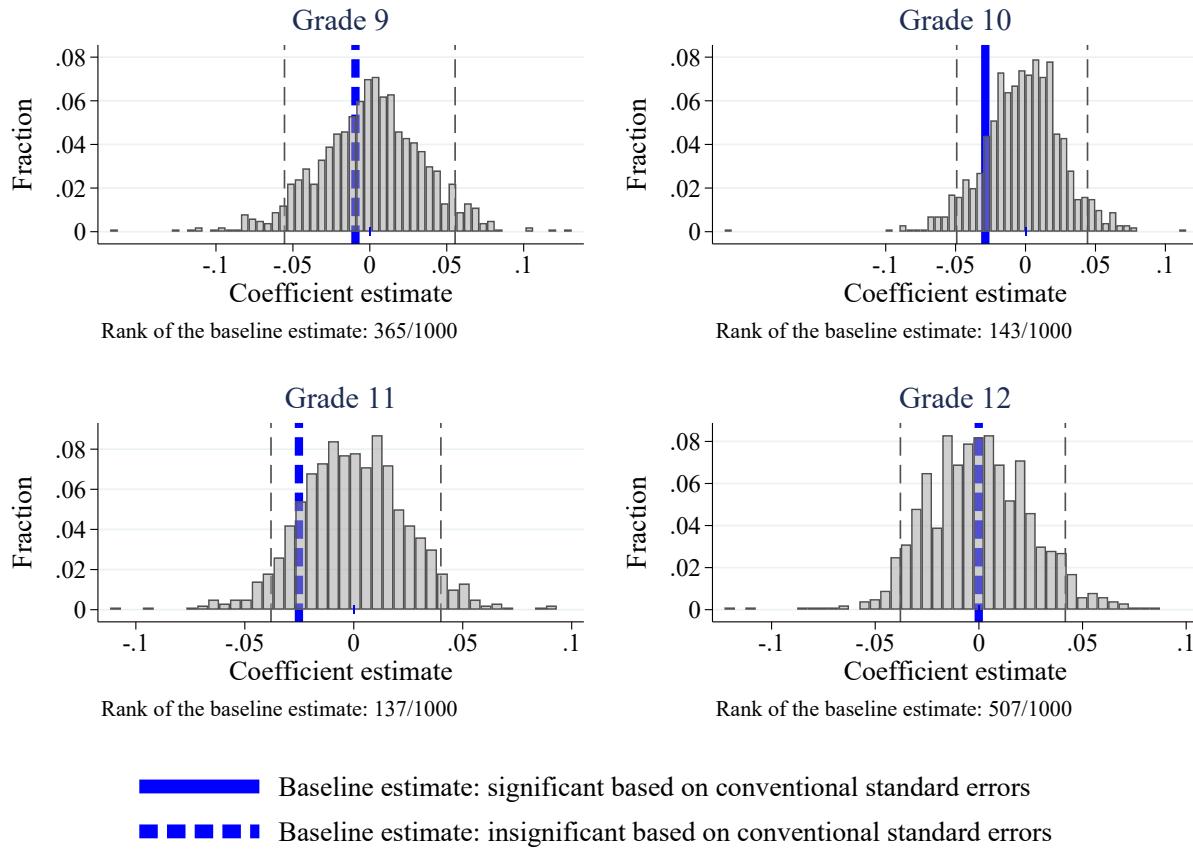
Notes: These figures present output from estimation of equation (4) using control schools selected from the matching procedure with the fuzzy match variables specified in the graph legends. Specifically, we begin by only matching on grade levels and the urbanicity categories. We then select two control schools for every shooting-exposed school from this set of potential matches by selecting the two schools that are the most similar to the exposed school in terms of student enrollment. We refer to this procedure as “Base variables only” in the figures. To move toward our baseline specification, we then gradually add the following fuzzy match variables: share of students by gender, share of students by race/ethnicity, and share of students on free or reduced-price lunch. Our baseline estimates—which use our baseline set of fuzzy match variables—are presented in solid red at the right of each sub-figure. We plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the set of cohort indicators. The specification includes match group-by-cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level.

Figure A17: Long-Run Effects on Labor Market Outcomes: Layering in Matching Variables



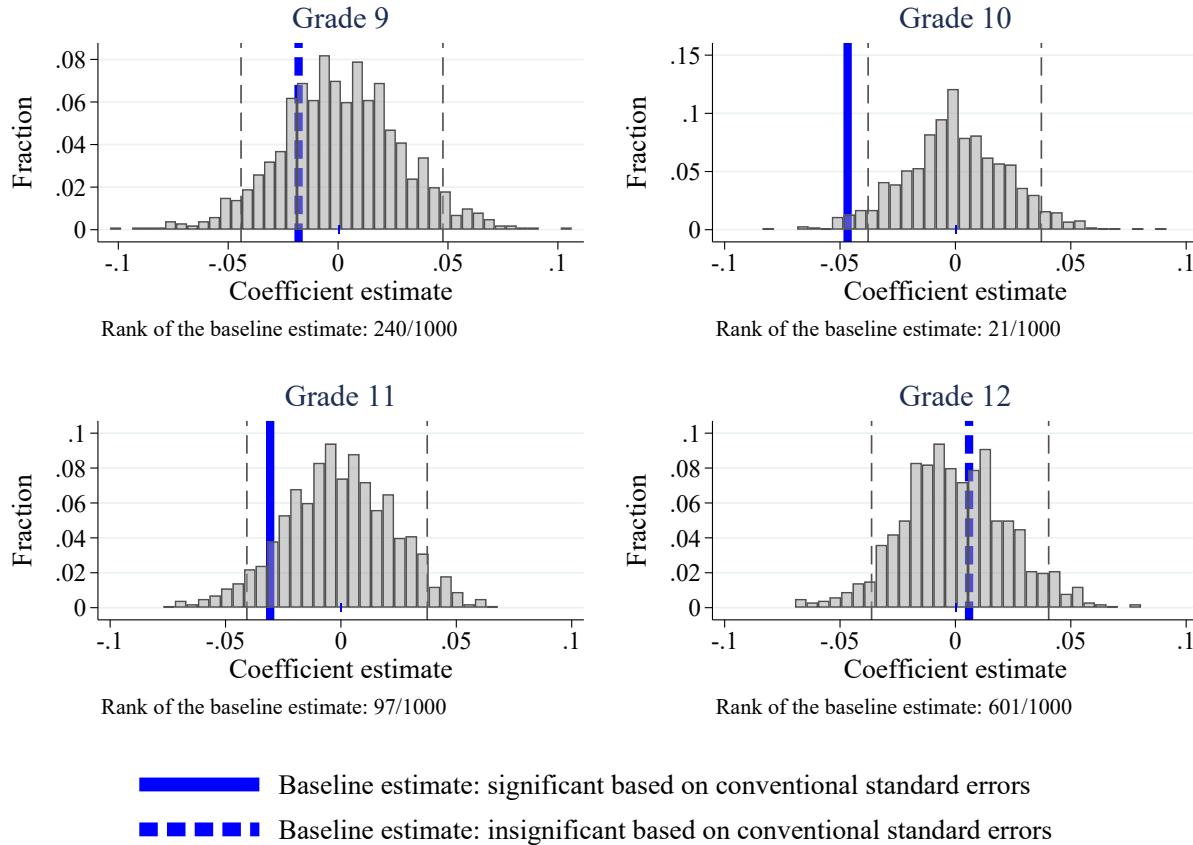
Notes: These figures present output from estimation of equation (4) using control schools selected from the matching procedure with the fuzzy match variables specified in the graph legends. Specifically, we begin by only matching on grade levels and the urbanicity categories. We then select two control schools for every shooting-exposed school from this set of potential matches by selecting the two schools that are the most similar to the exposed school in terms of student enrollment. We refer to this procedure as “Base variables only” in the figures. To move toward our baseline specification, we then gradually add the following fuzzy match variables: share of students by gender, share of students by race/ethnicity, and share of students on free or reduced-price lunch. Our baseline estimates—which use our baseline set of fuzzy match variables—are presented in solid red at the right of each sub-figure. We plot the coefficients and 95% confidence intervals on the interactions between the indicator denoting shooting schools and the set of cohort indicators. The specification includes match group-by-cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level.

Figure A18: Permutation Tests: High School Graduation



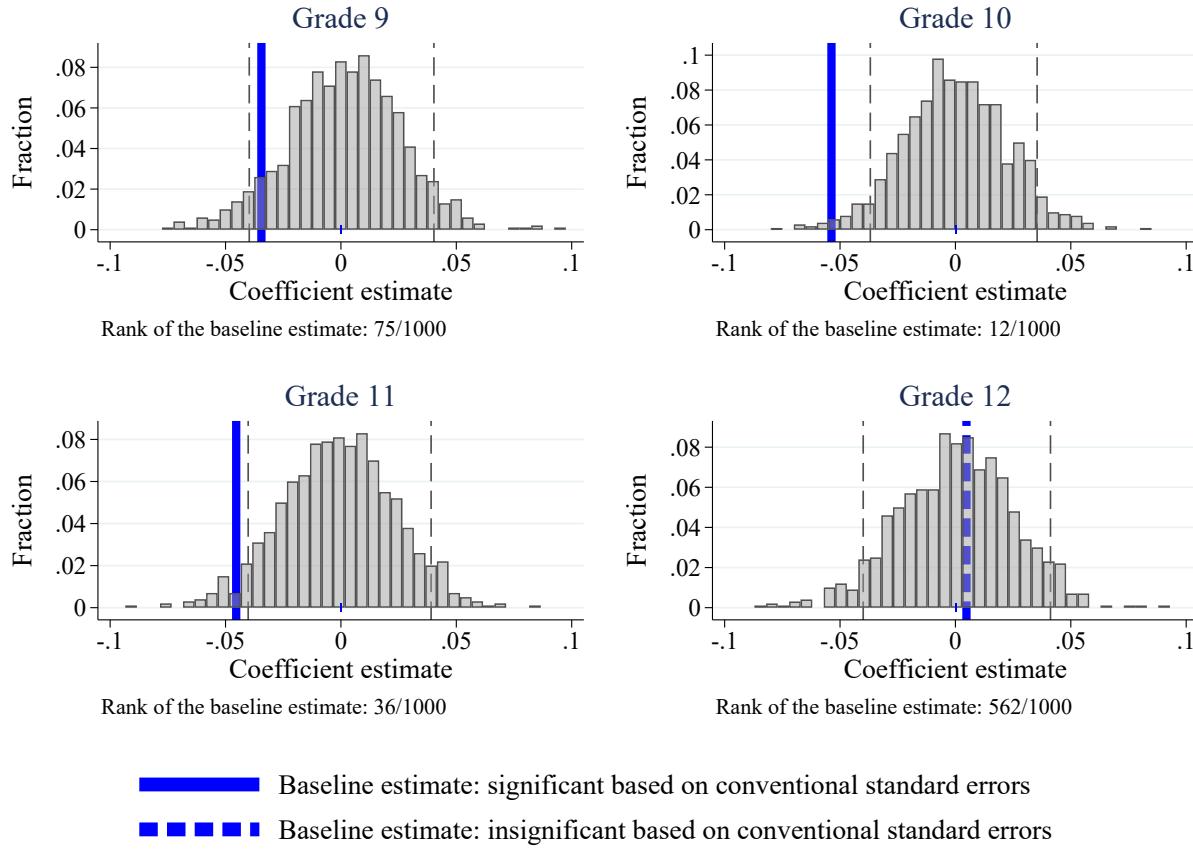
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline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

Figure A19: Permutation Tests: Enrollment in Any College



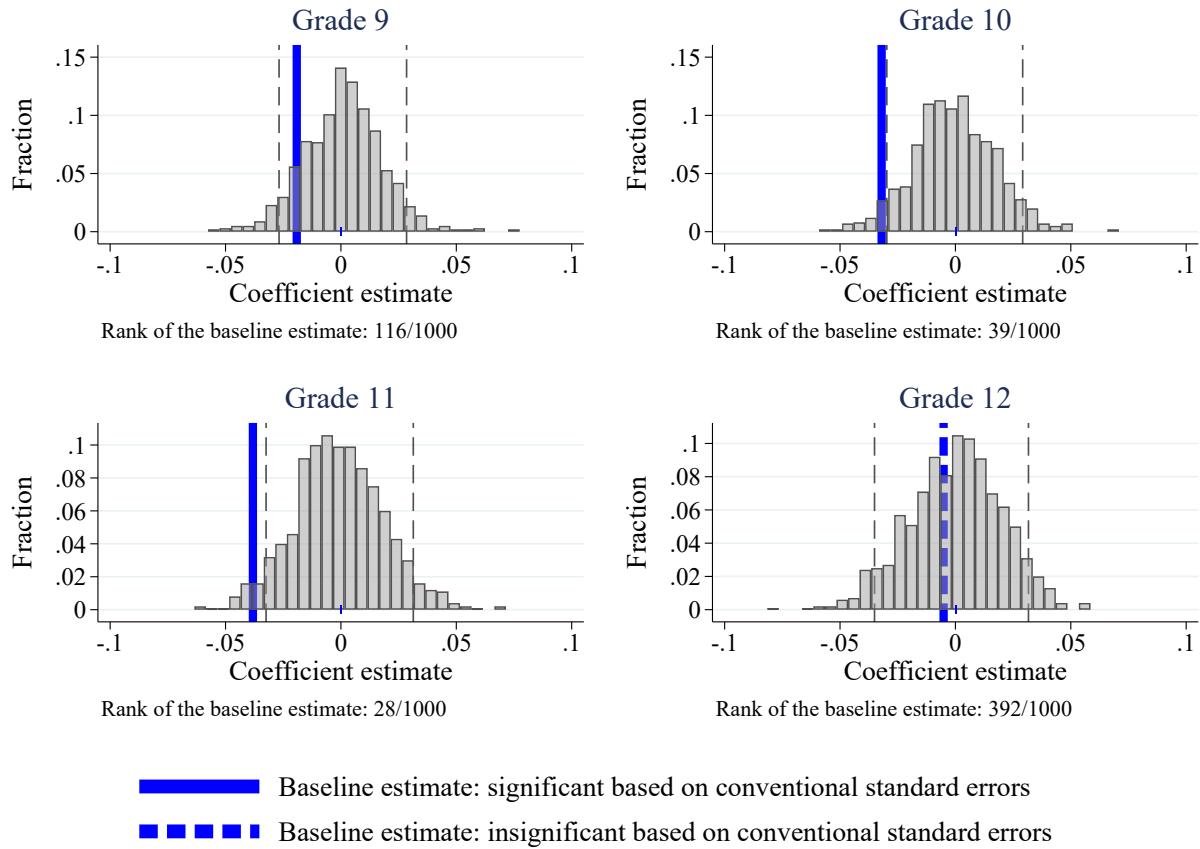
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline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

Figure A20: Permutation Tests: Enrollment in a 4-Year College



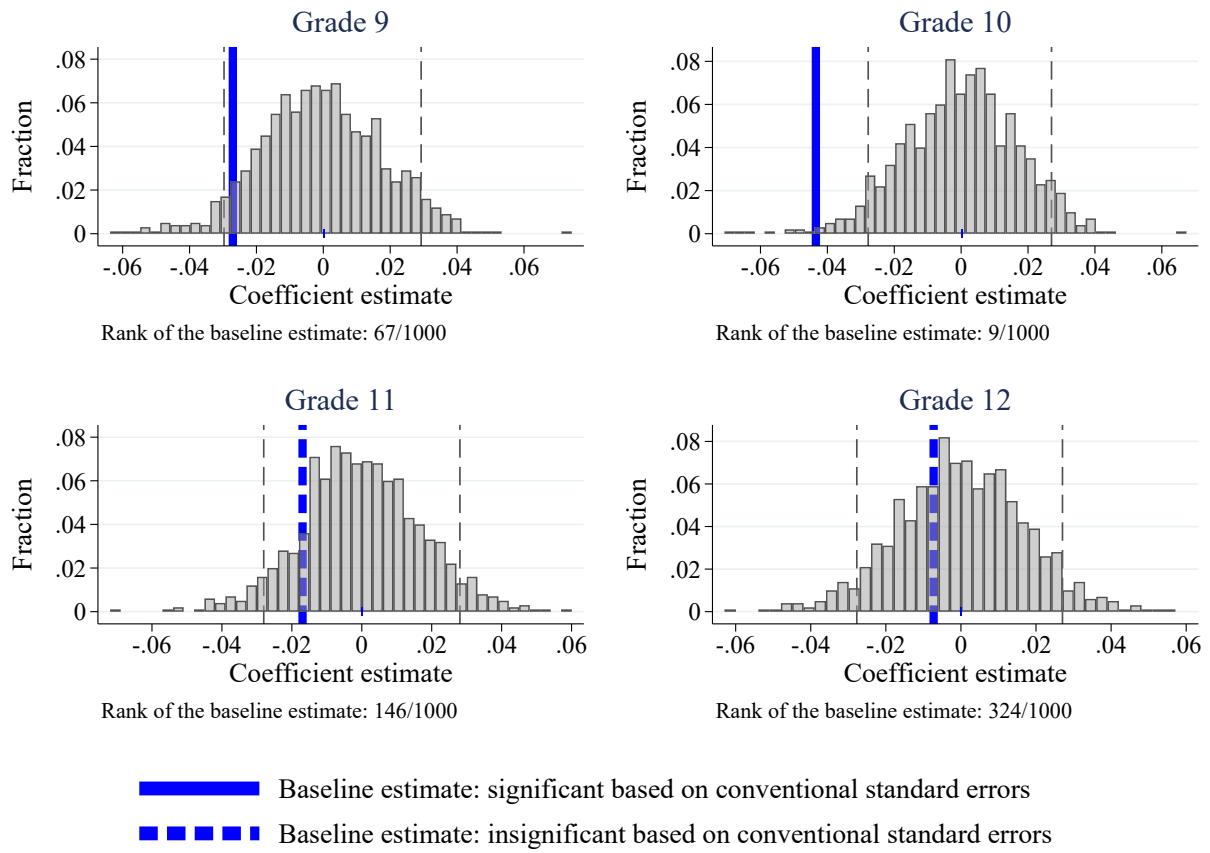
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline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

Figure A21: Permutation Tests: Bachelor's Degree



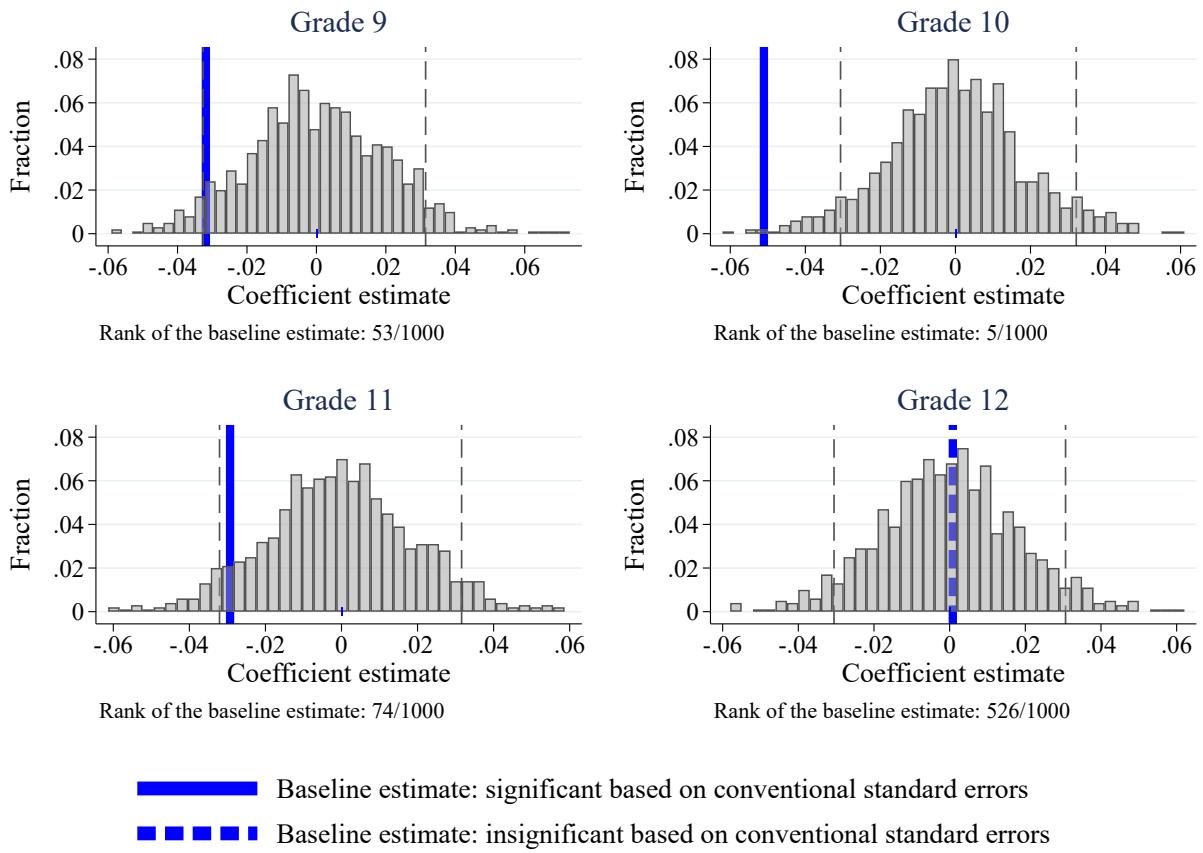
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline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

Figure A22: Permutation Tests: Ever Employed



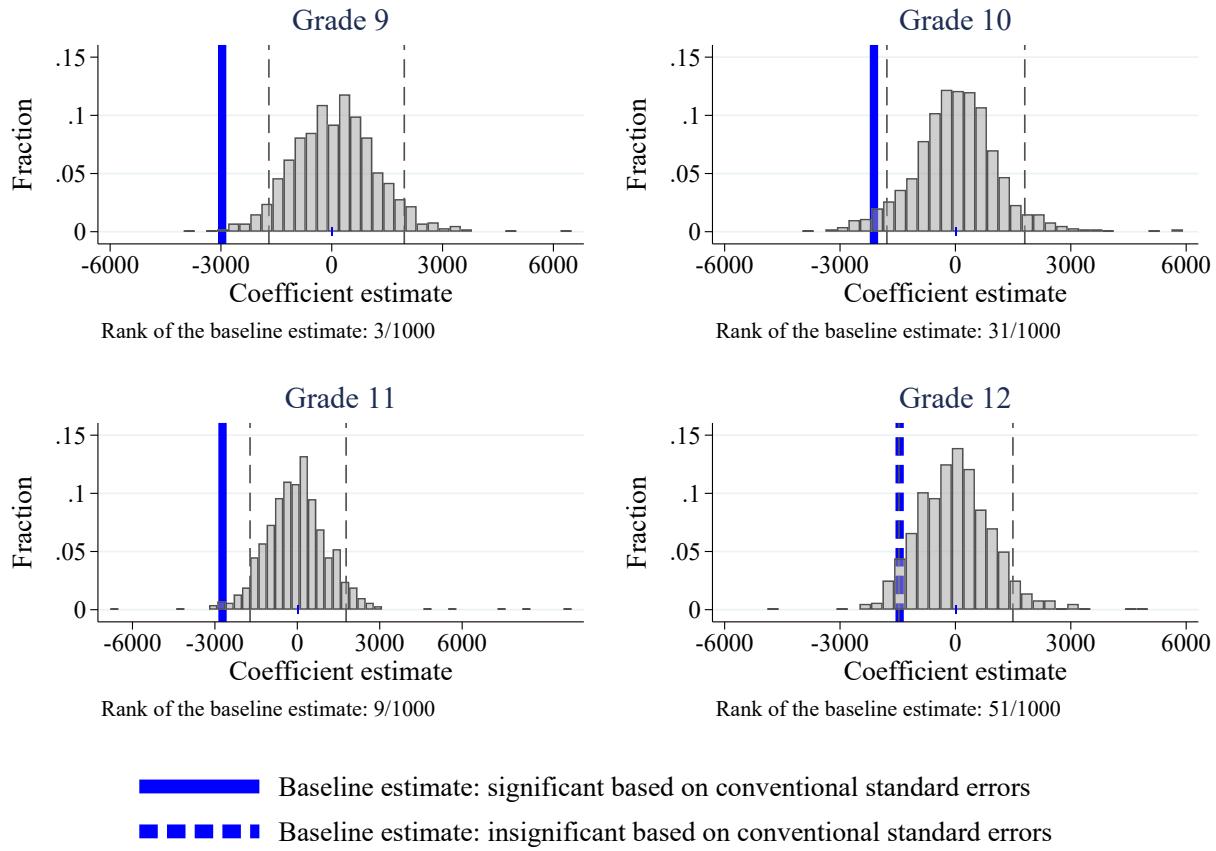
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline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

Figure A23: Permutation Tests: Stable Employment



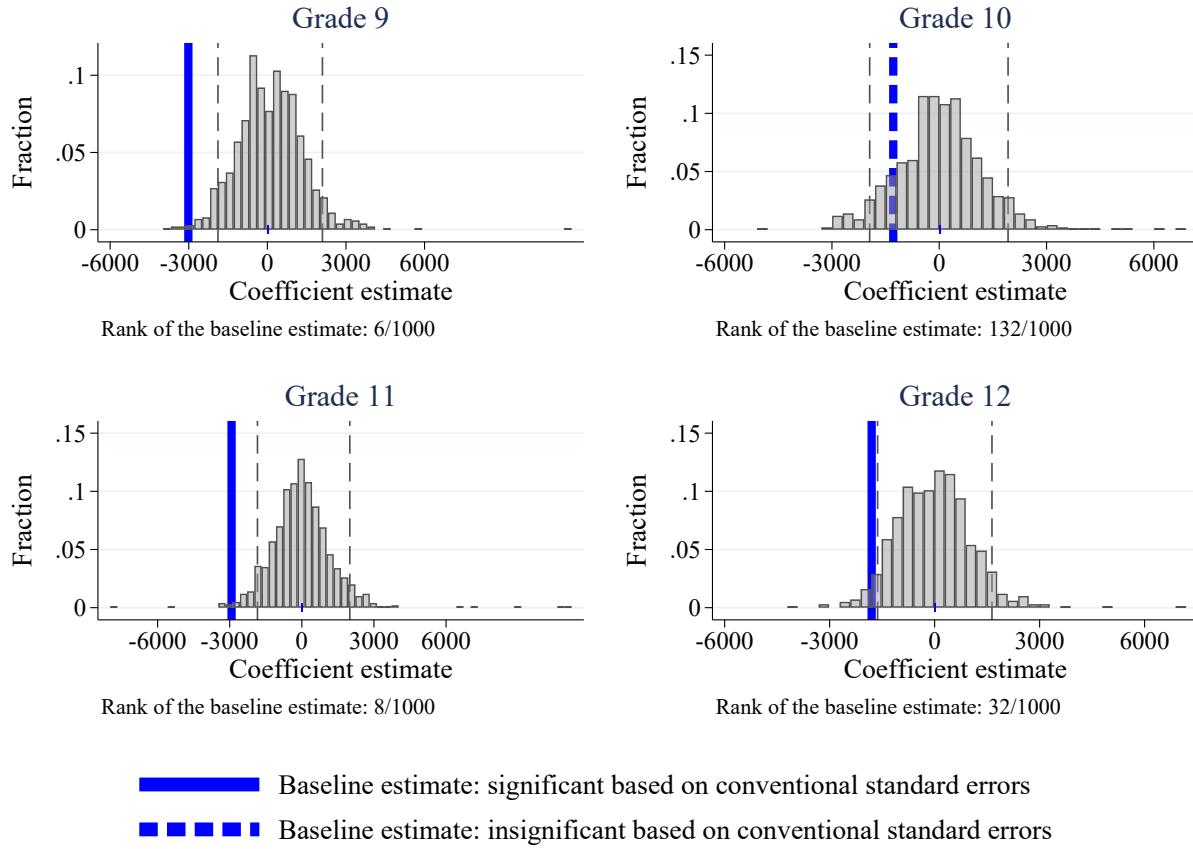
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline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

Figure A24: Permutation Tests: Average Earnings



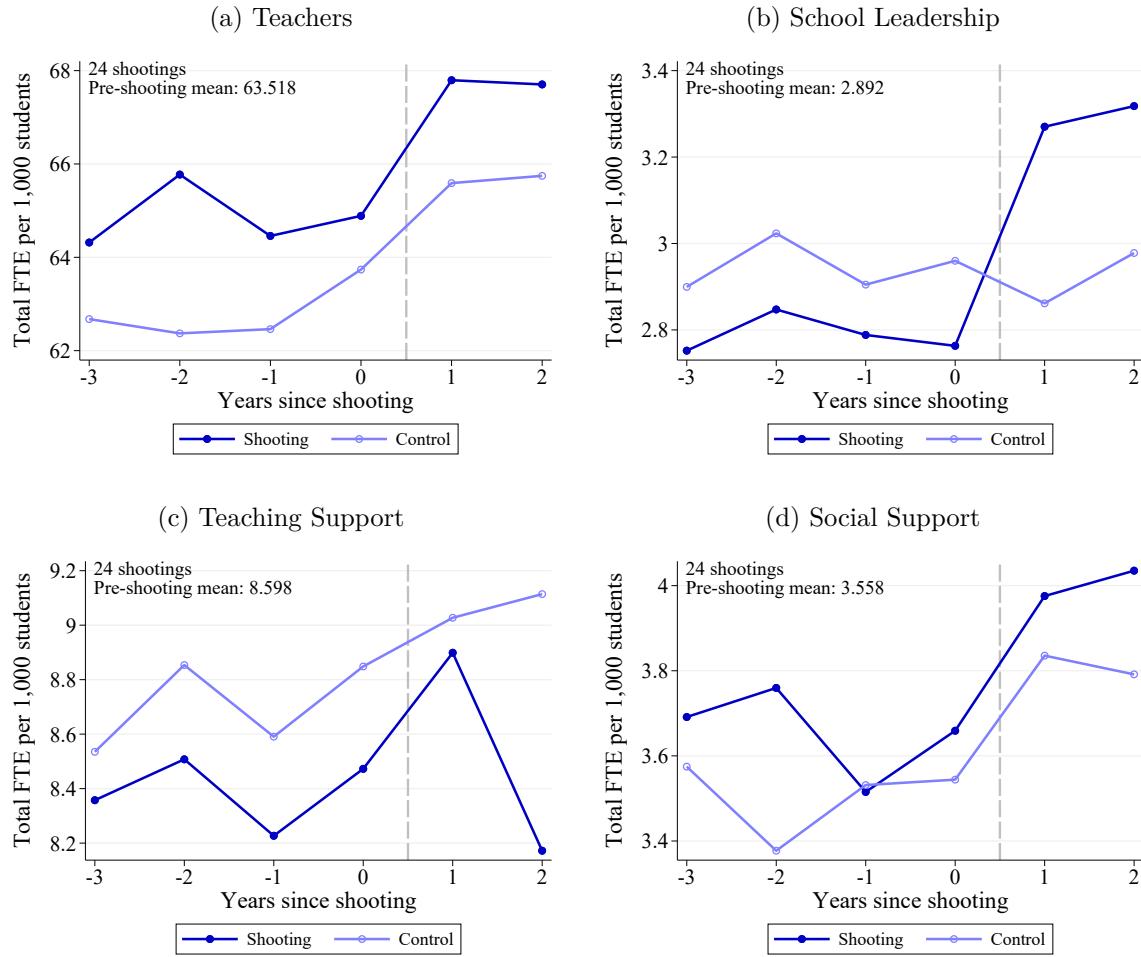
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline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

Figure A25: Permutation Tests: Average Non-Zero Earnings



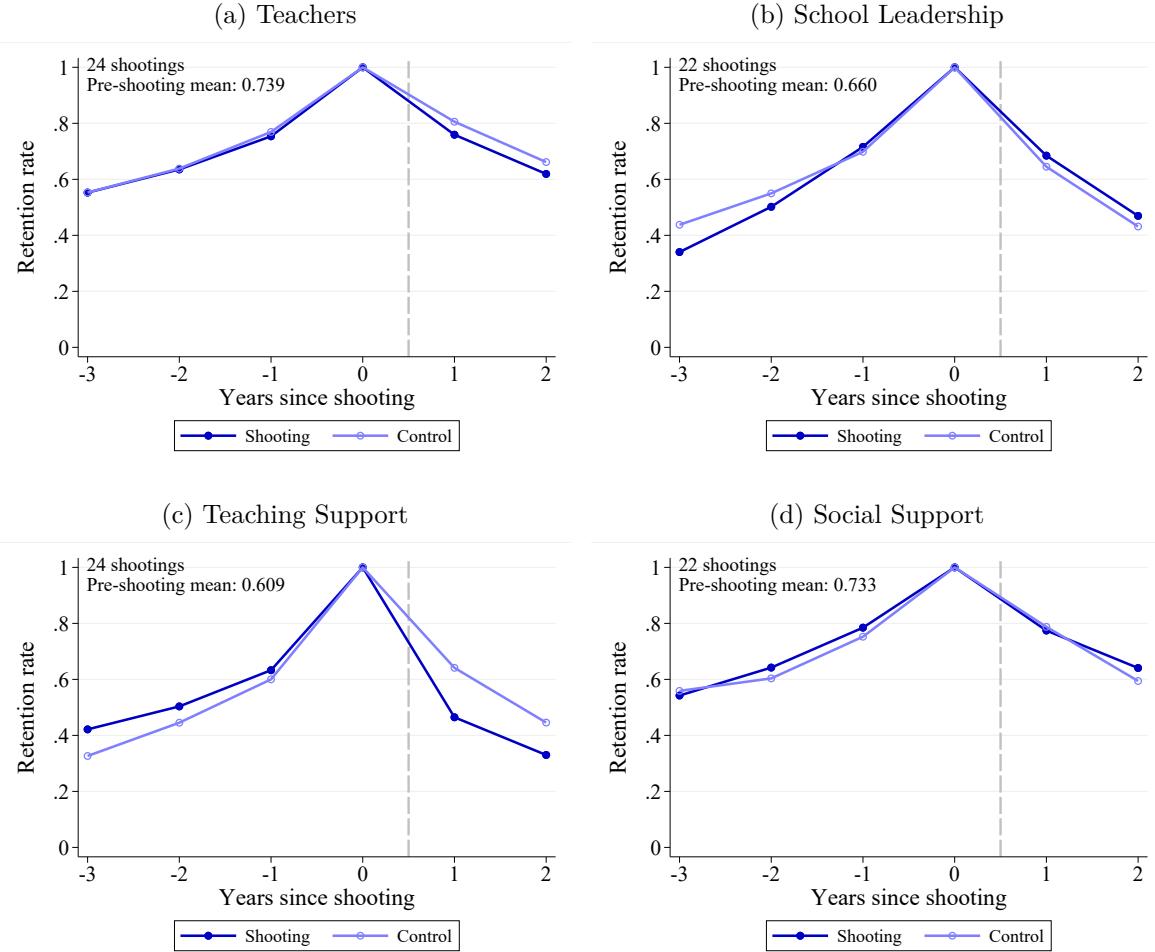
Notes: These figures present the distribution of placebo coefficient estimates from our permutation tests separately for each of the four grades. The dashed black vertical lines are the 5 and 95 percentiles of the placebo coefficient estimates. The thick solid blue vertical line indicates the baseline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

Figure A26: Raw Trends in School Staff Employment Across Shooting and Control Schools



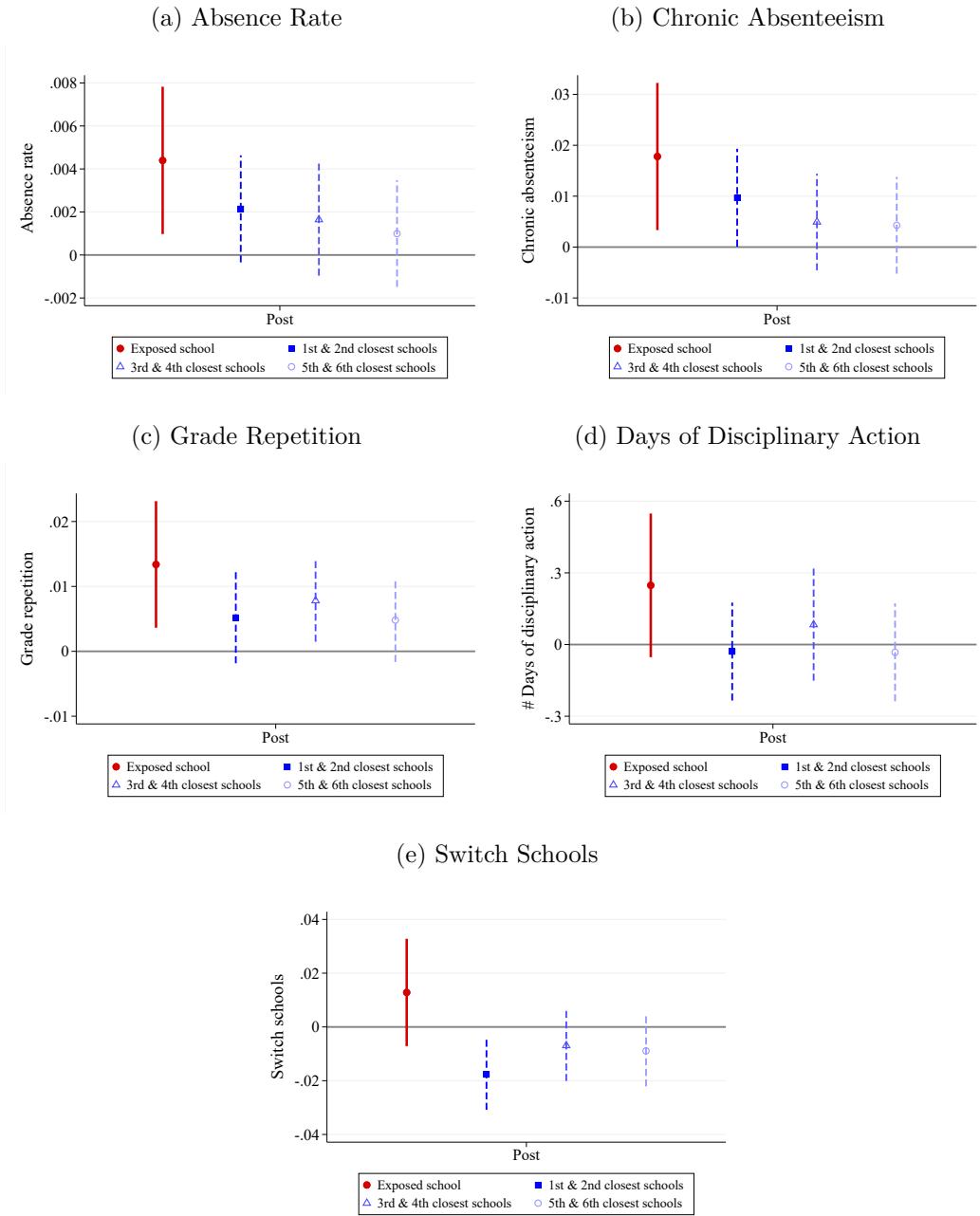
Notes: These figures plot raw trends in school staff employment over the six years surrounding a school shooting separately for treatment and matched control schools. School-by-academic year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting. As outlined in footnote 41, the staffing analysis includes 24 shooting and 48 control schools. Pre-shooting outcome means are calculated based on both the treatment and control group schools.

Figure A27: Raw Trends in Retention of Full-Time Staff Across Shooting and Control Schools



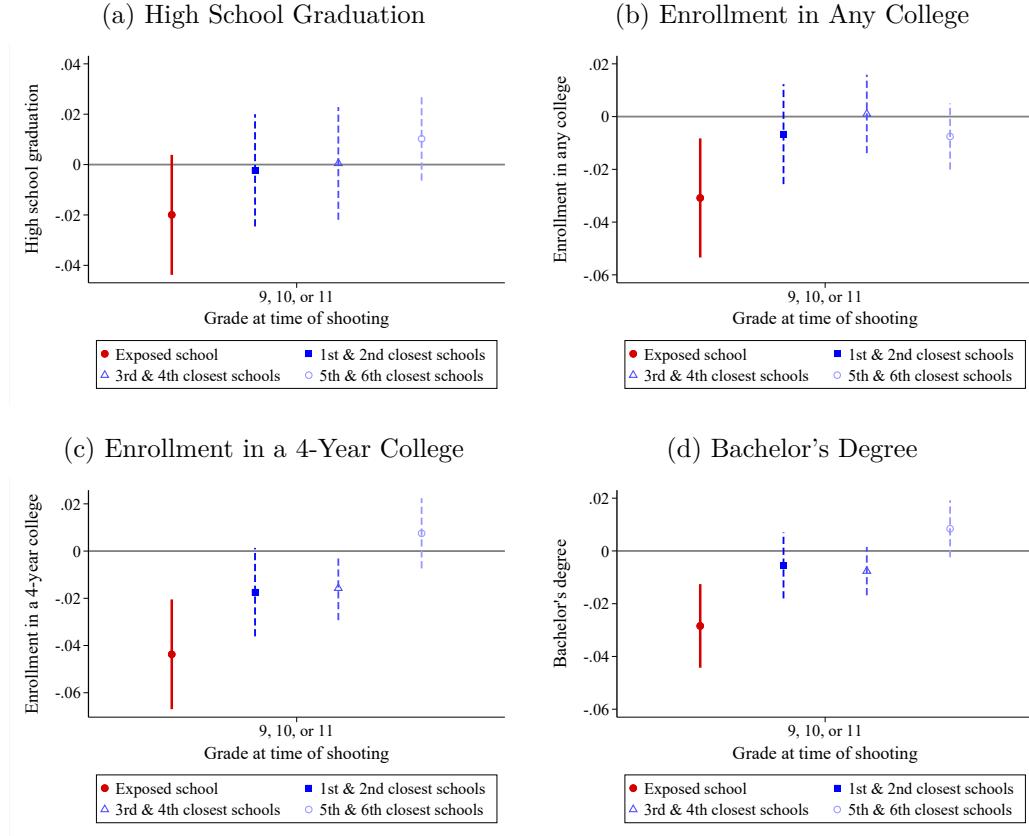
Notes: These figures plot raw trends in retention rates of full-time staff over the six years surrounding a school shooting separately for treatment and matched control schools. We focus on the staff that were employed full time at each of the shooting and control schools in our staff analysis sample at the time of the shooting and analyze changes in the probability of full-time employment at the same school both before and after the shooting. School-by-academic year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting. Sub-figures (a) and (c) include all 24 shooting and 48 control schools included in our staffing analysis; since we drop match groups in which either a shooting school or both control schools had no full-time employees in a given staff group at the time of the shooting, sub-figures (b) and (d) only include 22 match groups. Pre-shooting outcome means are calculated based on both the treatment and control group schools.

Figure A28: Short-Run Effects on Neighboring Schools



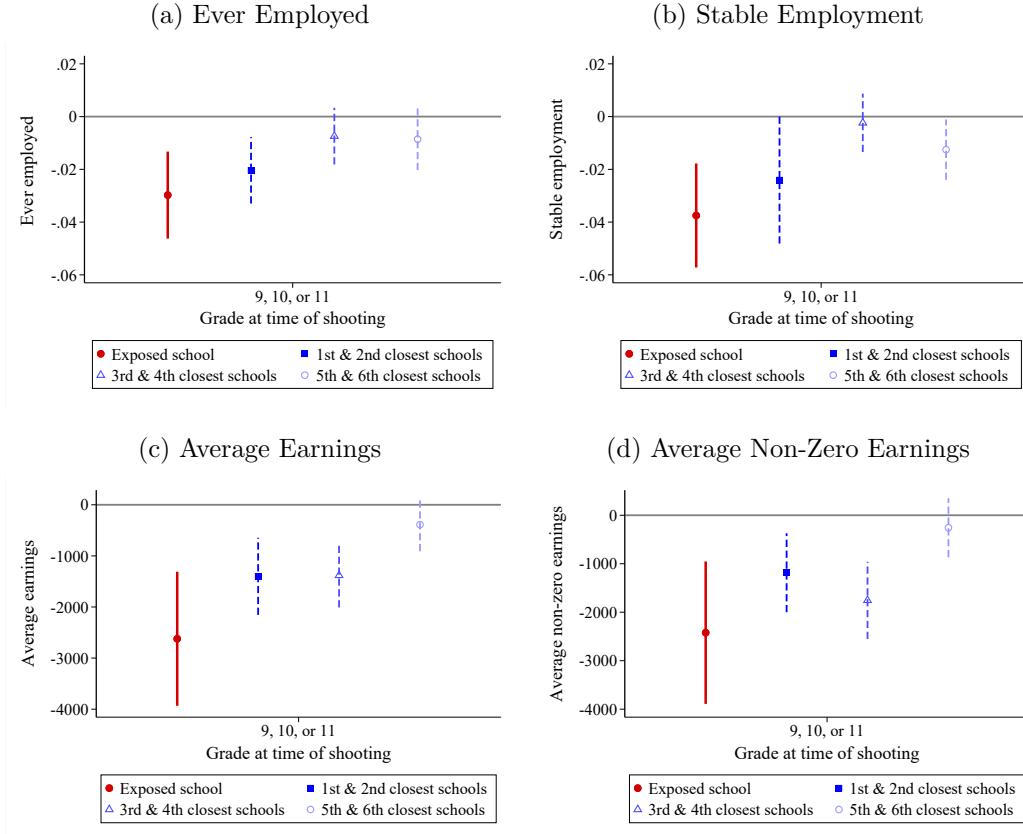
Notes: These figures plot the coefficients and associated 95% confidence intervals from estimation of equation (1), separately for students at the shooting-exposed schools (in solid red) and students at the three groups of closest neighboring schools (in dashed blue). The regressions include individual and match group-by-year fixed effects. Standard errors are clustered by school.

Figure A29: Long-Run Effects on Neighboring Schools: Educational Outcomes



Notes: These figures plot the coefficients and associated 95% confidence intervals from estimation of equation (4) that excludes the separate interaction coefficients for cohorts 9–11 and instead includes a single interaction coefficient for these cohorts, separately for students at the four groups denoted in the graph legend. For brevity, we do not report the estimates for cohort 12. The regressions include match group–by–cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school–by–cohort level.

Figure A30: Long-Run Effects on Neighboring Schools: Labor Market Outcomes



Notes: These figures plot the coefficients and associated 95% confidence intervals from estimation of equation (4) that excludes the separate interaction coefficients for cohorts 9–11 and instead includes a single interaction coefficient for these cohorts, separately for students at the four groups denoted in the graph legend. For brevity, we do not report the estimates for cohort 12. The regressions include match group–by–cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school–by–cohort level.

B Appendix Tables

Table A1: Description of Shootings Included in Our Analysis Sample

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

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

Table A2: Average Shooting Characteristics Across Long-Run Analysis, Short-Run Analysis, and Overall United States

| | Shooting Schools, Long-Run Analysis (1) | Shooting Schools, Short-Run Analysis (2) | Shooting Schools, Overall US (3) | <i>p-val</i> (1)-(2) (4) | <i>p-val</i> (1)-(3) (5) |
|---------------------|--|---|--|--------------------------------|--------------------------------|
| Indiscriminate | 0.000 | 0.000 | 0.067 | . | 0.451 |
| Suicide | 0.250 | 0.333 | 0.176 | 0.659 | 0.589 |
| Personally-targeted | 0.250 | 0.121 | 0.299 | 0.368 | 0.767 |
| Crime-related | 0.125 | 0.061 | 0.141 | 0.542 | 0.896 |
| Number of shootings | 8 | 33 | 375 | 41 | 383 |

Notes: This table presents average shooting characteristics for the 8 shootings in the long-run analysis, the 33 shootings in the short-run analysis, and all K–12 shootings across the United States. For all shootings in the United States (column (3)), we calculate averages using all K–12 shootings in the CHDS data that occurred during school hours and on school grounds over academic years 1997–1998 to 2015–2016 (specifically, between June 2017 and May 2016). We follow [Levine and McKnight \(2020b\)](#) to categorize shootings into five mutually exclusive categories (indiscriminate, suicides, personally-targeted, crime-related, and other).

Table A3: Average School Characteristics Across Treatment, Control, and All Schools

| | Shooting Schools (1) | Control Schools (2) | All Schools (3) | <i>p-val</i> (1)-(2) (4) | <i>p-val</i> (1)-(3) (5) |
|----------------------------|-------------------------|------------------------|--------------------|--------------------------------|--------------------------------|
| Matching Variables | | | | | |
| A. High Schools | | | | | |
| A.1. Exact Matching | | | | | |
| Lowest grade | 9.000 | 9.000 | 8.789 | . | 1.000 |
| Highest grade | 12.000 | 12.000 | 11.904 | . | 1.000 |
| Fraction Ciy | 0.364 | 0.364 | 0.273 | 1.000 | 1.000 |
| Fraction suburban | 0.364 | 0.364 | 0.171 | 1.000 | 1.000 |
| Fraction town | 0.136 | 0.136 | 0.164 | 1.000 | . |
| Fraction rural | 0.136 | 0.136 | 0.392 | 1.000 | 1.000 |
| A.2. Nearest Matching | | | | | |
| Female | 0.484 | 0.490 | 0.481 | 0.286 | 0.263 |
| Free/reduced-price lunch | 0.442 | 0.442 | 0.414 | 0.999 | 0.530 |
| Non-Hispanic White | 0.387 | 0.413 | 0.469 | 0.764 | 0.953 |
| Non-Hispanic Black | 0.222 | 0.204 | 0.124 | 0.785 | 0.818 |
| Hispanic | 0.359 | 0.356 | 0.388 | 0.966 | 0.815 |
| Number of students | 1,655.18 | 1,568.61 | 854.02 | 0.704 | 0.705 |
| Number of schools | 22 | 44 | 2,639 | 66 | 33 |
| B. Non-High Schools | | | | | |
| B.1. Exact Matching | | | | | |
| Lowest grade | 4.000 | 4.000 | 0.942 | 1.000 | 0.003 |
| Highest grade | 7.273 | 7.273 | 6.224 | 1.000 | 0.188 |
| Fraction Ciy | 0.636 | 0.636 | 0.395 | 1.000 | 0.102 |
| Fraction suburban | 0.182 | 0.182 | 0.251 | 1.000 | 0.595 |
| Fraction town | 0.000 | 0.000 | 0.130 | . | 0.201 |
| Fraction rural | 0.182 | 0.182 | 0.224 | 1.000 | 0.737 |
| B.2. Nearest Matching | | | | | |
| Female | 0.487 | 0.496 | 0.473 | 0.263 | 0.629 |
| Free/reduced-price lunch | 0.419 | 0.487 | 0.521 | 0.530 | 0.232 |
| Non-Hispanic White | 0.149 | 0.144 | 0.387 | 0.953 | 0.011 |
| Non-Hispanic Black | 0.196 | 0.173 | 0.142 | 0.818 | 0.356 |
| Hispanic | 0.637 | 0.666 | 0.448 | 0.815 | 0.049 |
| Number of students | 870.55 | 829.50 | 540.83 | 0.705 | 0.000 |
| Number of schools | 11 | 22 | 9,594 | | |

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

Table A4: Short-Run Effects on Educational Outcomes Among Long-Run Analysis Sample

| | Absence Rate (1) | Chronic Absenteeism (2) | Grade Repetition (3) | Days of Disc. Act. (4) | Switch Schools (5) |
|--|-------------------------------|-------------------------------|-------------------------------|-------------------------------|-------------------------------|
| A. Baseline sample (33 shootings) | | | | | |
| Shooting School x Post | 0.0044 (0.0017) [0.012] | 0.0178 (0.0073) [0.016] | 0.0134 (0.0049) [0.008] | 0.2478 (0.151) [0.105] | 0.0128 (0.0101) [0.205] |
| Pre-shooting outcome mean | 0.0365 | 0.0645 | 0.0106 | 1.9849 | 0.1109 |
| Student-year observations | 368,142 | 368,142 | 368,142 | 276,114 | 365,675 |
| R-squared | 0.553 | 0.481 | 0.233 | 0.429 | 0.283 |
| B. Long-run analysis sample (8 shootings) | | | | | |
| Shooting School x Post | 0.0089 (0.0023) [0.001] | 0.0347 (0.0092) [0.001] | 0.0272 (0.0119) [0.032] | 0.4207 (0.2472) [0.117] | 0.0119 (0.0081) [0.158] |
| Pre-shooting outcome mean | 0.0365 | 0.0615 | 0.0082 | 2.5853 | 0.0759 |
| Student-year observations | 76,188 | 76,188 | 76,188 | 24,342 | 75,697 |
| R-squared | 0.532 | 0.466 | 0.253 | 0.367 | 0.287 |

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

Table A5: Long-Run Effects of Shootings at Schools: Summary Estimates

| | Graduate HS (1) | Enroll, Any Col (2) | Enroll, 4yr Col (3) | Bachelor's Degree (4) |
|---------------------------------|---------------------------------|---------------------------------|-----------------------------------|----------------------------------|
| A. Educational Outcomes | | | | |
| Shooting School x Cohorts 10–11 | -0.0272 (0.0128) [0.034] | -0.0395 (0.0125) [0.002] | -0.0500 (0.0121) [<0.001] | -0.0347 (0.0093) [<0.001] |
| Control group outcome mean | 0.7904 | 0.6323 | 0.3755 | 0.2369 |
| Student observations | 59,808 | 54,526 | 54,526 | 54,526 |
| Percent relative to mean | -3.4% | -6.2% | -13.3% | -14.6% |
| | Ever Employed (1) | Stable Employment (2) | Earnings (3) | Non-Zero Earnings (4) |
| B. Labor Market Outcomes | | | | |
| Shooting School x Cohorts 9–11 | -0.0298 (0.0084) [<0.001] | -0.0375 (0.0100) [<0.001] | -2,622.04 (665.03) [<0.001] | -2,422.29 (744.62) [0.001] |
| Control group outcome mean | 0.7899 | 0.6751 | 23,181.27 | 30,868.89 |
| Student observations | 59,808 | 54,526 | 54,526 | 54,526 |
| Percent relative to mean | -3.8% | -5.6% | -11.3% | -7.8% |

Notes: Panel A (Panel B) of this table presents coefficients, standard errors (in parentheses), and p –values [in brackets] from estimation of equation (4) that excludes the separate interaction coefficients for cohorts 10–11 (cohorts 9–11) and instead include a single interaction coefficient for these cohorts. The dependent variables are as indicated in each panel. The regressions include match group–by–cohort fixed effects, school fixed effects, and a vector of individual-level controls for student race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic, other) and gender. Standard errors are clustered at the school-by-cohort level. Outcome means are calculated based on the cohorts 9–12 enrolled at the matched control schools at the time of the shooting.

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

| | Graduate HS (1) | Enroll Any Col (2) | Enroll 4yr Col (3) | Bachelor's Degree (4) |
|---------------------------------------|--------------------------------|--------------------------------|---------------------------------|---------------------------------|
| A. Shooting School x Cohort 12 | | | | |
| Baseline estimate | -0.0001 (0.0200) [0.997] | 0.0059 (0.0140) [0.673] | 0.0048 (0.0170) [0.779] | -0.0051 (0.0145) [0.725] |
| Lee lower bound | -0.0081 (0.0220) [0.712] | -0.0015 (0.0143) [0.919] | 0.0011 (0.0170) [0.949] | -0.0073 (0.0147) [0.619] |
| Lee upper bound | 0.0002 (0.0199) [0.993] | 0.0101 (0.0141) [0.476] | 0.0131 (0.0176) [0.459] | 0.0046 (0.0148) [0.758] |
| B. Shooting School x Cohort 11 | | | | |
| Baseline estimate | -0.0252 (0.0163) [0.124] | -0.0306 (0.0132) [0.021] | -0.0454 (0.0147) [0.002] | -0.0381 (0.0103) [<0.001] |
| Lee lower bound | -0.0348 (0.0162) [0.033] | -0.0375 (0.0138) [0.007] | -0.0485 (0.0150) [0.001] | -0.0402 (0.0105) [<0.001] |
| Lee upper bound | -0.0239 (0.0163) [0.144] | -0.0270 (0.0130) [0.039] | -0.0376 (0.0149) [0.012] | -0.0293 (0.0104) [0.005] |
| C. Shooting School x Cohort 10 | | | | |
| Baseline estimate | -0.0288 (0.0164) [0.081] | -0.0467 (0.0172) [0.007] | -0.0537 (0.0153) [0.001] | -0.0320 (0.0131) [0.016] |
| Lee lower bound | -0.0366 (0.0171) [0.033] | -0.0527 (0.0177) [0.003] | -0.0568 (0.0156) [<0.001] | -0.0338 (0.0132) [0.011] |
| Lee upper bound | -0.0266 (0.0162) [0.102] | -0.0419 (0.0176) [0.018] | -0.0459 (0.0160) [0.005] | -0.0231 (0.0141) [0.103] |
| D. Shooting School x Cohort 9 | | | | |
| Baseline estimate | -0.0094 (0.0192) [0.627] | -0.0180 (0.0179) [0.317] | -0.0345 (0.0189) [0.070] | -0.0191 (0.0110) [0.083] |
| Lee lower bound | -0.0149 (0.0202) [0.460] | -0.0228 (0.0186) [0.222] | -0.0371 (0.0192) [0.055] | -0.0208 (0.0112) [0.064] |
| Lee upper bound | -0.0071 (0.0191) [0.709] | -0.0143 (0.0176) [0.419] | -0.0291 (0.0180) [0.108] | -0.0125 (0.0105) [0.234] |

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

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

| | Ever Employed (1) | Stable Employment (2) | Earnings (3) | Non-Zero Earnings (4) |
|---------------------------------------|---------------------------------|---------------------------------|------------------------------------|------------------------------------|
| A. Shooting School x Cohort 12 | | | | |
| Baseline estimate | -0.0073 (0.0162) [0.655] | 0.0008 (0.0175) [0.962] | -1,450.93 (976.28) [0.139] | -1,799.61 (993.58) [0.072] |
| Lee lower bound | -0.0161 (0.0164) [0.328] | -0.0071 (0.0179) [0.692] | -1,716.64 (990.32) [0.085] | -2,120.53 (1,002.80) [0.036] |
| Lee upper bound | -0.0049 (0.0165) [0.767] | 0.0045 (0.0176) [0.801] | -461.80 (797.50) [0.563] | -810.78 (784.42) [0.303] |
| B. Shooting School x Cohort 11 | | | | |
| Baseline estimate | -0.0169 (0.0139) [0.225] | -0.0293 (0.0154) [0.058] | -2,723.49 (859.93) [0.002] | -2,922.87 (1,179.62) [0.014] |
| Lee lower bound | -0.0261 (0.0142) [0.068] | -0.0369 (0.0159) [0.021] | -2,984.04 (866.14) [0.001] | -3,230.93 (1,187.44) [0.007] |
| Lee upper bound | -0.0146 (0.0139) [0.295] | -0.0257 (0.0153) [0.094] | -860.13 (621.20) [0.168] | -746.72 (670.60) [0.267] |
| C. Shooting School x Cohort 10 | | | | |
| Baseline estimate | -0.0434 (0.0100) [<0.001] | -0.0510 (0.0140) [<0.001] | -2,117.97 (932.32) [0.024] | -1,286.74 (978.83) [0.190] |
| Lee lower bound | -0.0520 (0.0105) [<0.001] | -0.0587 (0.0145) [<0.001] | -2,348.25 (943.65) [0.014] | -1,562.16 (959.51) [0.105] |
| Lee upper bound | -0.0411 (0.0098) [<0.001] | -0.0475 (0.0140) [0.001] | -1,175.45 (774.34) [0.131] | -350.65 (764.54) [0.647] |
| D. Shooting School x Cohort 9 | | | | |
| Baseline estimate | -0.0271 (0.0109) [0.014] | -0.0318 (0.0132) [0.017] | -2,967.47 (1,159.92) [0.011] | -3,014.35 (1,220.21) [0.014] |
| Lee lower bound | -0.0338 (0.0111) [0.003] | -0.0371 (0.0137) [0.007] | -3,145.35 (1,162.45) [0.007] | -3,268.44 (1,219.29) [0.008] |
| Lee upper bound | -0.0252 (0.0109) [0.022] | -0.0288 (0.0130) [0.028] | -1,010.62 (685.67) [0.142] | -821.80 (653.47) [0.210] |

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

Table A8: Descriptive Statistics for School Staff Groups and Individual Staff Types

| | Panel A | | | % Full Time | Panel C | | |
|---|-----------|-------------|-----------|---------------|-------------|-------------|-----------|
| | Total FTE | Mean (1) | SD (2) | Median (3) | Mean (4) | Mean (5) | SD (6) |
| Teacher | 110.069 | 51.383 | | 102.190 | 88.46 | 64.474 | 8.926 |
| School leadership | 4.845 | 2.889 | | 4.000 | 86.32 | 2.944 | 1.160 |
| Assistant principle | 3.855 | 2.799 | | 3.000 | 85.09 | 2.173 | 1.058 |
| Principal | 0.989 | 0.320 | | 1.000 | 91.87 | 0.771 | 0.613 |
| Teaching support staff | 13.449 | 7.379 | | 11.986 | 69.70 | 8.694 | 4.597 |
| Educational diagnostician | 0.681 | 0.906 | | 0.000 | 69.76 | 0.393 | 0.500 |
| Teacher supervisor | 0.104 | 0.405 | | 0.000 | 89.83 | 0.056 | 0.244 |
| Truant officer/visiting teacher | 0.044 | 0.262 | | 0.000 | 43.37 | 0.044 | 0.280 |
| Educational aide | 11.498 | 6.523 | | 10.000 | 71.78 | 7.516 | 4.233 |
| Teacher facilitator | 0.550 | 1.086 | | 0.000 | 72.97 | 0.363 | 0.764 |
| Substitute teacher | 0.573 | 1.567 | | 0.000 | 64.78 | 0.323 | 0.834 |
| Social support staff | 6.460 | 4.473 | | 5.031 | 76.34 | 3.666 | 1.332 |
| Art therapist | 0.002 | 0.049 | | 0.000 | 100.00 | 0.002 | 0.034 |
| Psychological associate | 0.037 | 0.176 | | 0.000 | 24.28 | 0.020 | 0.096 |
| Audiologist | 0.029 | 0.167 | | 0.000 | 100.00 | 0.009 | 0.053 |
| Corrective therapist | 0.007 | 0.067 | | 0.000 | 16.56 | 0.005 | 0.056 |
| Counselor | 4.578 | 3.056 | | 4.000 | 85.20 | 2.549 | 0.882 |
| Music therapist | 0.007 | 0.071 | | 0.000 | 13.61 | 0.005 | 0.051 |
| Occupational therapist | 0.012 | 0.047 | | 0.000 | 0.00 | 0.007 | 0.026 |
| Certified orientation and mobility specialist | 0.007 | 0.040 | | 0.000 | 0.00 | 0.005 | 0.028 |
| Physical therapist | 0.015 | 0.055 | | 0.000 | 0.00 | 0.007 | 0.027 |
| Recreational therapist | 0.013 | 0.114 | | 0.000 | 100.00 | 0.004 | 0.032 |
| School nurse | 0.941 | 0.576 | | 1.000 | 76.09 | 0.624 | 0.441 |
| LSSP/psychologist | 0.095 | 0.456 | | 0.000 | 71.17 | 0.035 | 0.150 |
| Social worker | 0.094 | 0.307 | | 0.000 | 48.05 | 0.052 | 0.185 |
| Speech therapist | 0.260 | 0.410 | | 0.000 | 24.49 | 0.177 | 0.321 |
| Certified interpreter | 0.365 | 1.280 | | 0.000 | 92.49 | 0.166 | 0.542 |
| School x academic year observations | | | | 432 | | | 432 |

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

Table A9: Effects on School Staff Employment and Retention of Full-Time Staff

| | Teachers (1) | School Leadership (2) | Teaching Support (3) | Social Support (4) |
|---|--------------------------------|-------------------------------|--------------------------------|--------------------------------|
| A. Effects on School Staff Employment (FTE per 1,000 students) | | | | |
| Shooting School x Post | 0.2374 (1.0648) [0.824] | 0.5461 (0.2908) [0.064] | -0.1582 (0.6262) [0.801] | 0.0323 (0.2125) [0.880] |
| Pre-shooting outcome mean | 63.518 | 2.892 | 8.598 | 3.558 |
| Observations | 432 | 432 | 432 | 432 |
| R-squared | 0.909 | 0.819 | 0.897 | 0.867 |
| B. Effects on Retention of Full-Time Staff | | | | |
| Shooting School x Post | -0.0418 (0.0163) [0.012] | 0.0817 (0.0892) [0.363] | -0.1989 (0.0731) [0.008] | -0.0061 (0.0705) [0.932] |
| Pre-shooting outcome mean | 0.739 | 0.660 | 0.609 | 0.733 |
| Observations | 432 | 384 | 414 | 390 |
| R-squared | 0.939 | 0.741 | 0.831 | 0.714 |

Notes: This table presents coefficients, standard errors (in parentheses), and p -values [in brackets] from estimation described in Section 5.4. Panel A reports the effects on school staff employment (the number of FTE staff per 1,000 students); Panel B presents the effects on the retention of school staff. In Panel B, we focus on the staff that were employed full-time at each of the shooting and control schools in our staff analysis sample at the time of the shooting, and analyze changes in the probability of full-time employment at the same school both before and after the shooting. The regressions include school and match group-by-year fixed effects. School-by-academic-year cells are weighted by the total enrollment measured in the first six-week grading period of the academic year of the shooting. Standard errors are clustered by school. Pre-shooting outcome means are calculated based on both the treatment and control group schools.

C The Parallel Trends Assumption

To test the robustness of our short-run event study results to violations of parallel trends, we use the method proposed by [Rambachan and Roth \(2023\)](#). In this section, we provide a brief overview of this method using their notation and discuss the application of this test to our results. [Rambachan and Roth \(2023\)](#) proceed as follows. First, in a difference-in-differences event study setting, they define δ as the difference in trends between the treated and control groups that would have occurred in the absence of treatment. Second, they assume that δ lies in a researcher-specified set Δ of possible violations of the parallel trends assumption. Third, given a chosen Δ , they construct robust confidence intervals associated with these violations for the parameters of interest. They introduce several possible choices of Δ , including the following two leading examples.

Relative magnitudes bounds. Their first approach is based on the assumption that the violations of parallel trends in the post-treatment periods are not substantially larger in magnitude than those in the pre-treatment periods. With this restriction, the set of possible differences in trends takes the following form:

$$\Delta^{RM}(\bar{M}) = \left\{ \delta : \forall t \geq 0, |\delta_{t+1} - \delta_t| \leq \bar{M} \cdot \max_{s < 0} |\delta_{s+1} - \delta_s| \right\}, \quad (5)$$

where the abbreviation RM is used to indicate “relative magnitudes.” $\Delta^{RM}(\bar{M})$ limits the maximum post-treatment violation of parallel trends between consecutive periods to \bar{M} times the maximum pre-treatment deviation from parallel trends.

Smoothness restrictions. Their second approach is based on the assumption that the differential trends evolve smoothly over time by bounding the changes in slope across consecutive periods up to a parameter $M \geq 0$. Under this constraint, the set of possible differences in trends takes the following form:

$$\Delta^{SD}(M) := \{ \delta : |(\delta_{t+1} - \delta_t) - (\delta_t - \delta_{t-1})| \leq M, \forall t \}, \quad (6)$$

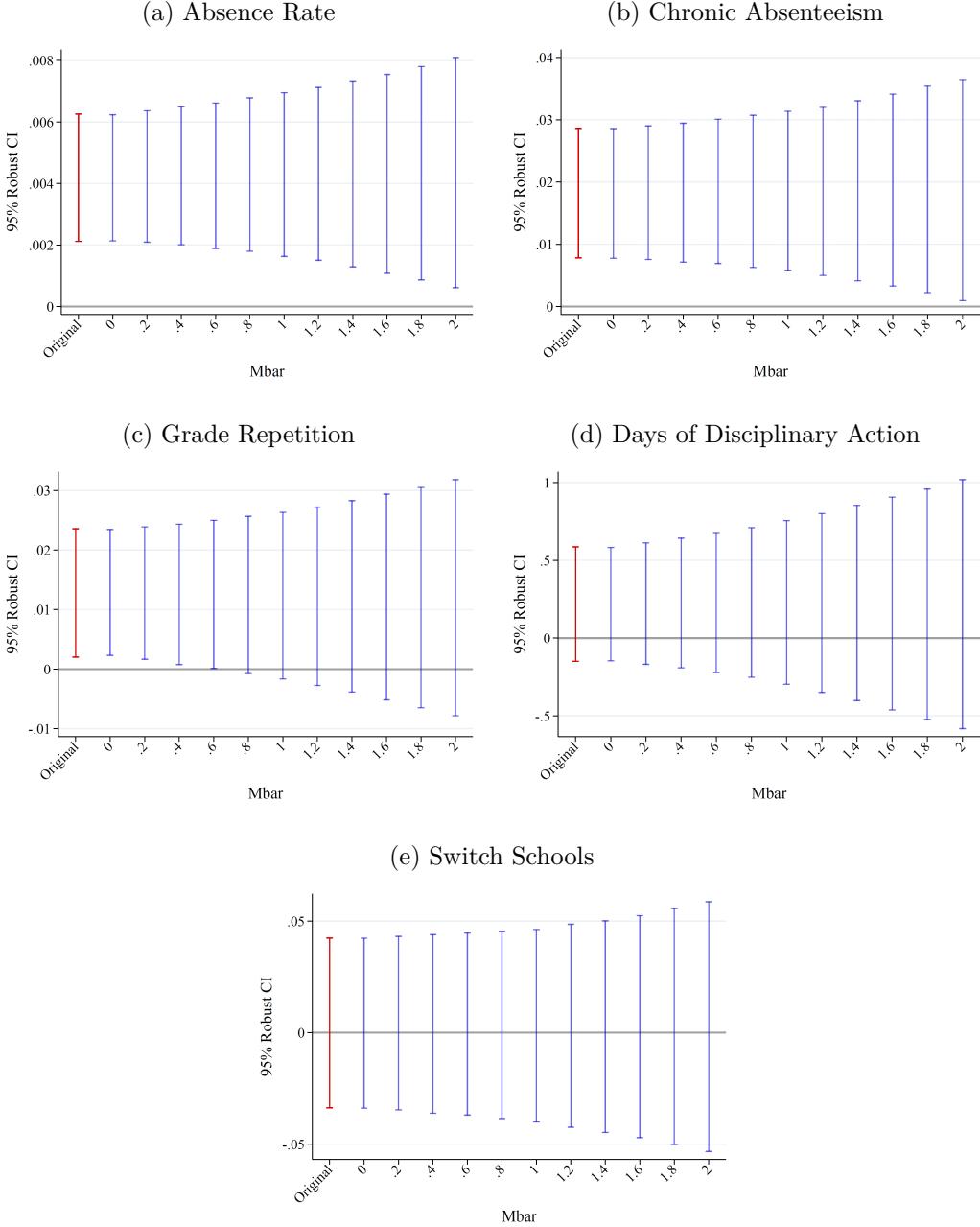
where the abbreviation SD stands for “second differences” or “second derivative.” When $M = 0$, $\Delta^{\text{SD}}(M)$ requires linearity in the difference in trends.

Using these two approaches, we test the robustness of our short-run event study results. For the “relative magnitudes” test, we choose a standard value of $\bar{M} = 2$, which allows for violations of common trends in the post-period that are up to twice as large as the biggest violation in the pre-period. For the “smoothness restrictions” test, which allows for additive trend violations in the post-period, we take the default values for M from the “honestDiD” Stata command, which is scaled relative to the outcome. Further following the default settings of the “honestDiD” Stata command, we apply these tests to the parameter for the first post-period, denoted as ρ_0 in equation (2).⁴⁸

The 95% confidence intervals from the “relative magnitudes” test and the “smoothness restrictions” test are presented in Appendix Figure C1 and Appendix Figure C2, respectively. Effects are robust to violations of the relative magnitudes restrictions of over 200 percent of the largest pre-period violation for the absence rate and chronic absenteeism and up to 60 percent for grade repetition. When implementing the smoothness restrictions test, outcomes are robust to a linear violation of parallel trends ($M=0$), and effects remain significant up to a value of $M = 0.002$, $M = 0.006$, and $M = 0.006$ for the absence rate, chronic absenteeism, and grade repetition, respectively.

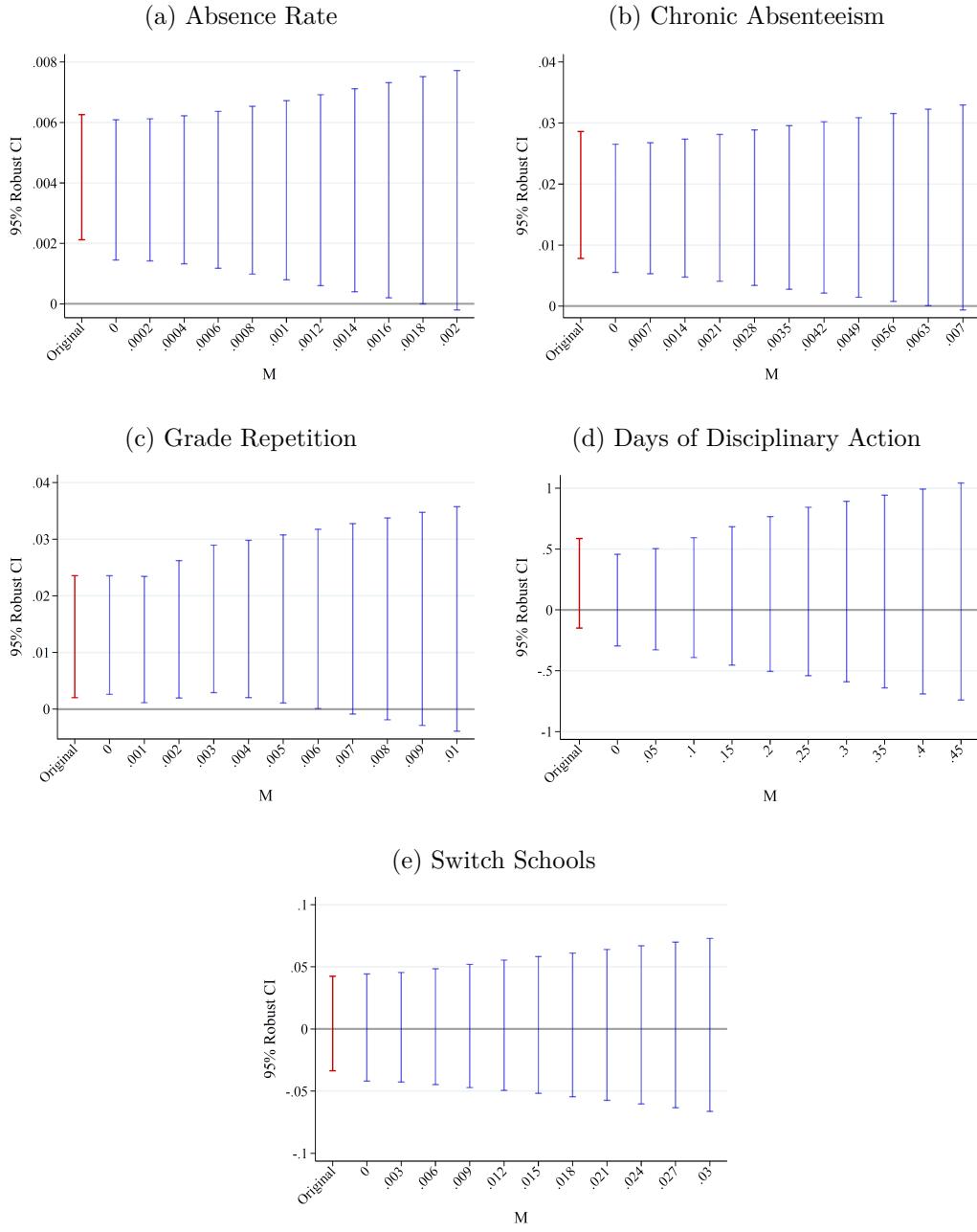
⁴⁸Since grade repetition reflects academic performance in the previous academic year, we treat the year of the shooting as a pre-period and apply these tests to ρ_1 in equation (2).

Figure C1: Sensitivity Analysis for Non-Parallel Trends: Relative Magnitudes Restrictions



Notes: The figures plot 95% robust confidence intervals of ρ_0 in equation (2), constructed following the “relative magnitudes bounds” approach proposed by [Rambachan and Roth \(2023\)](#). Since grade repetition reflects academic performance in the previous academic year, we treat the year of the shooting as a pre-period and apply this test to ρ_1 in equation (2) instead. Our baseline estimates—which are based on the parallel trends assumption—are presented in red at the left of each figure.

Figure C2: Sensitivity Analysis for Non-Parallel Trends: Smoothness Restrictions



Notes: The figures plot 95% robust confidence intervals of ρ_0 in equation (2), constructed following the “smoothness restrictions” approach proposed by [Rambachan and Roth \(2023\)](#). Since grade repetition reflects academic performance in the previous academic year, we treat the year of the shooting as a pre-period and apply this test to ρ_1 in equation (2) instead. Our baseline estimates—which are based on the parallel trends assumption—are presented in red at the left of each figure.

D Migration Analysis

In this section, we analyze migration flows using data from the American Community Survey (ACS), which provides information on each respondent’s county of residence in the prior year and in the current year starting in 2005. We use this information to calculate the share of the population who leaves a particular county and analyze the effects of school shootings in our analysis sample on county-level out-migration rates. This analysis supplements our analysis of attrition in our primary data discussed in Sections 4.2 and 5.2.

Specifically, we use ACS data for 2005–2019 and restrict the sample to individuals who were residing in a county in Texas in the prior year. We then create an indicator variable equal to one if an individual’s current county of residence is outside the state of Texas (i.e., the individual moved out of Texas since the previous year). We further construct an indicator denoting movement across counties within Texas. We then analyze the impacts of shootings on out-migration among individuals who were residing in counties that contain treatment schools, counties than contain control schools, and all counties in Texas. We stack the data for different shootings (e.g., counties that contain control schools for more than one shooting are included separately for each shooting). These data allow us to analyze the impacts on out-migration of 19 of the 33 shootings included in our short-run analysis; of the remaining 14 shootings, 11 occurred before 2005 and three involve schools in counties that are too small to be identified in the one-year ACS.

Appendix Figure D1(a) presents the raw data. In particular, we plot the raw annual shares of individuals migrating out of Texas (solid lines) and across counties within Texas (dashed lines) among individuals that previously resided in treatment counties (blue lines), control counties (green lines), and in overall Texas (gray lines) from three years before to six years after a shooting. We show these shares separately for cohorts that were aged 14–17 (exposed cohorts; left panel) and cohorts that were aged 19–22 at the time of the shooting (“too old” cohorts; right panel).

The solid lines in the left panel of Appendix Figure D1(a) show that out-migration rates among exposed cohorts in the years prior to a shooting are very similar across treatment counties, control counties, and for overall Texas (at a relatively low and stable level). Over the first four years following a shooting, out-migration rates rise somewhat. Again, this

increase is very similar across the three groups, which is consistent with the fact that these individuals reach college-going age and are more likely to move at that point. As shown in the right panel, out-migration rates peak at ages 19–22 and start to decrease as the four-year age window shifts to older ages. In sum, there is no indication of differential trends in out-migration rates across the treatment and control counties for exposed or “too old” cohorts. Moreover, the out-migration rates in the treatment and control counties are similar to the rates observed for all counties in the state.

We can further analyze differences in out-migration rates between treatment and control counties using an event study specification similar to equation (2). Specifically, using data only from counties with treatment and control schools, we regress the indicator denoting out-of-state migration in the individual-level data on event time dummies interacted with an indicator denoting the treatment status of an individual’s county of residence. The year before the shooting is the omitted category. The regressions include individual and match group-by-year fixed effects. Standard errors are clustered at the match group-by-county level.

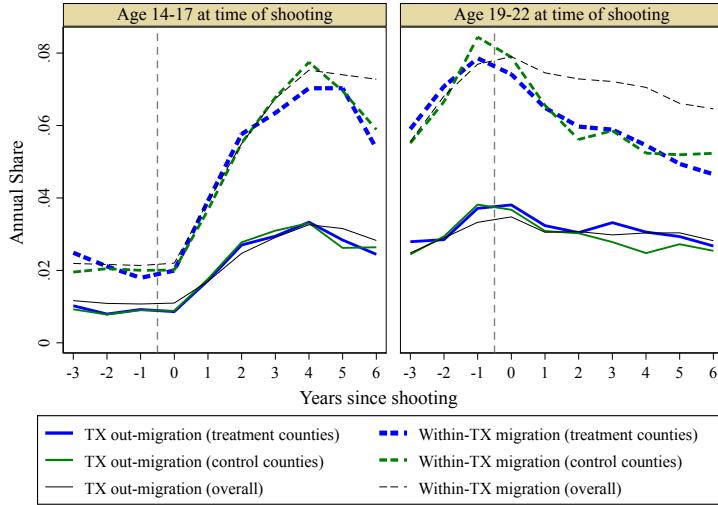
Appendix Figure D1(b) plots the coefficients and 95% confidence intervals on the interactions between the indicator denoting a shooting school county and the event time indicators. We present results both for the cohort aged 14–17 at the time of the shooting (left panel) and for the cohort age 19–22 at the time of the event (right panel). Consistent with the patterns observed in the raw data, there are no significant differences in out-of-state migration rates between treatment and control counties in the years before or after a shooting.

There are two additional points worth noting when considering the potential impacts of migration on our results. First, as shown by the dashed lines in Appendix Figure D1(a), in-state migration rates are much higher than out-of-state migration rates for all groups, and the increase in migration in young adulthood is much more pronounced when considering in-state rather than out-of-state migration. This suggests that most individuals moving out of their county of residence during the analyzed age range remain within the state of Texas, and thus we are able to follow their trajectories in our primary data sets. Second, rates of out-migration from Texas are low not only compared to within-Texas migration but also relative to our estimated effects on labor force participation. For differential migration out of

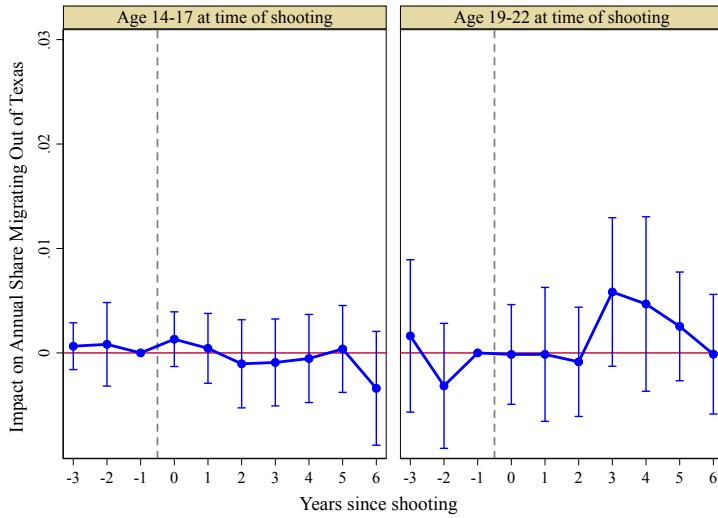
Texas to explain the 3 percentage point decrease in labor force participation resulting from exposure to a school shooting that we estimate, out-of-state migration rates among treated individuals would have had to differentially increase by more than 100 percent above the baseline rate. This amount of differential out-migration seems implausible, especially given that: (i) we do not find differential out-migration responses between treatment and control counties in the ACS, and (ii) we do not find differential effects on school switching between students at shooting and control schools in the Texas education data, which is consistent with no differential migration in the short run.

Figure D1: Effects on Migration Out of Texas

(a) Raw Trends



(b) Event Study Estimates



Notes: These figures present out-migration rates surrounding a school shooting. Sub-figure (a) plots the raw annual shares of individuals migrating out of Texas (solid lines) and across counties within Texas (dashed lines) among individuals that previously resided in treatment counties (blue lines), control counties (green lines), and in overall Texas (gray lines) from three years before to six years after a shooting. We show these shares separately for cohorts that were aged 14–17 (exposed cohorts; left panel) and cohorts that were aged 19–22 at the time of the shooting (“too old” cohorts; right panel). Sub-figure (b) plots the coefficients and 95% confidence intervals from estimation of an event study specification similar to equation (2). Specifically, using data only from counties with treatment and control schools, we regress the indicator denoting out-of-state migration in the individual-level data on event time dummies interacted with an indicator denoting the treatment status of an individual’s county of residence. The regressions include individual and match group-by-year fixed effects, and standard errors are clustered at the match group-by-county level. We present results both for the cohort aged 14–17 at the time of the shooting (left panel) and for the cohort age 19–22 at the time of the event (right panel).