Flight from Fights:
School shootings, mobility and neighborhood sorting

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Abstract

School shootings are an extreme form of violence and a devastation for students, staff, families and the broader community. Seeking safety, a shooting could incentivize some families to switch schools. If these fleeing students come from socioeconomically stronger families, this could exacerbate sorting by socioeconomic status between schools. This paper estimates the effects of shootings in U.S. K-12 public schools between 1990 and 2022 on enrollment and socioeconomic segregation within school districts using a staggered Difference-in-Differences design that exploits the exogenous timing of shootings. At the school-level, I find a decrease of approximately 4% in enrollment, driven by non-poor students—those not receiving Free or Reduced Lunch. The decline is present in both lowest and highest grades, but is greater in the highest grade. At the school-district level, between-school segregation sees modest increases following a shooting. This implies non-random mobility following a shooting. I perform a series of heterogeneity analysis highlighting the role of ease of switching schools, the fatality of the shooting and spillover effects.

Keywords— school shootings, crime, education, segregation
JEL— I24, I31, J11, R23

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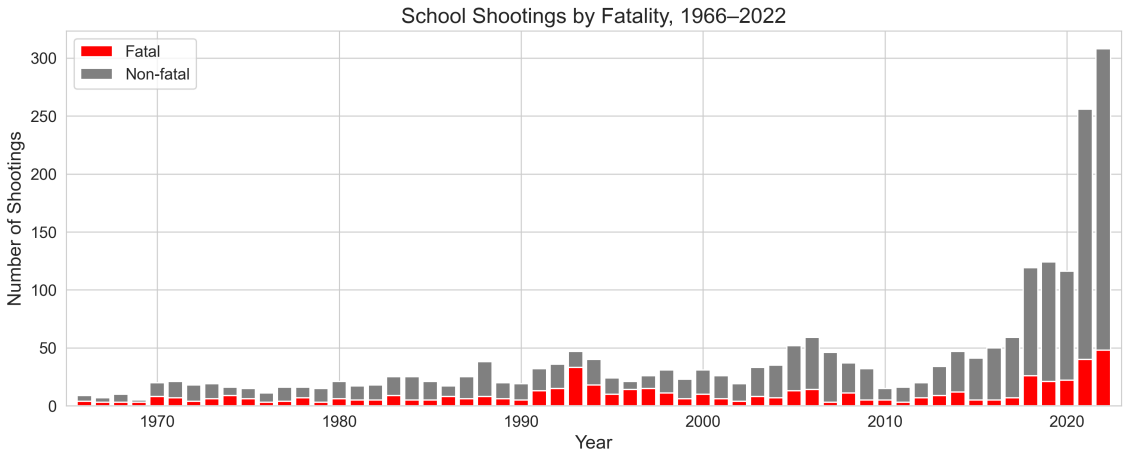
Introduction

Safe neighborhoods are vital for children. We know this from the Moving To Opportunity literature that showed the importance of good schools and safe neighborhoods on children's educational attainment and future labor market outcomes (Chetty and Hendren 2018; Chetty et al. 2016; Chetty et al. 2014; Chyn 2018). While that literature studied moves away from disadvantaged neighborhoods, I focus on a much more acute source of mobility—fleeing violence.

School shootings are some of the worst types of violence in the United States. In 2023 alone, there were approximately 300 shootings, of which 50 had at least one fatality (as plotted in Figure 1) They are more likely to occur in bigger schools, often high schools, but they result in greater fatalities in elementary schools. These shootings are more likely to occur in urban areas, but are not restricted to any other specific geography (as seen in Figure 2). It is extremely difficult to predict the location of a shooting and impossible to predict the timing. These events create strong incentives for families to reconsider their school and residential choice. This begs the question, to what extent does household mobility react to these shocks? And does this mobility response exacerbate socioeconomic segregation within the neighborhood?

Figure 1. School Shootings per Year by Fatality



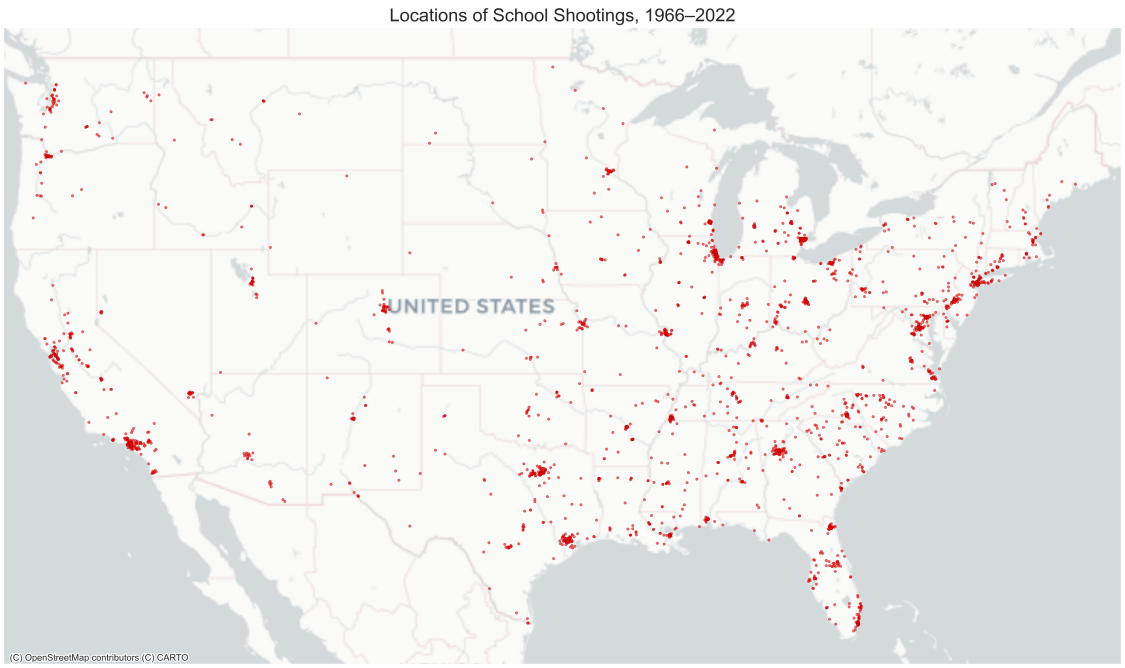
Notes: Source: K-12 School Shootings Database (Riedman 2024). Red bars indicate fatal shootings.

There is mixed evidence from previous literature regarding mobility responses. Recent research by Deb and Gangaram (2024), Sezer (2022), and Cabral et al. (2021) finds no effect of school shootings on enrollment or mobility, while Yang and Gopalan (2023) and Abouk and Adams (2013) do find mobility responses. Beland and Kim (2016) find mixed evidence.

At the same time, there is a growing concern over a rise in socioeconomic segregation (Mijs and Roe 2021; Owens et al. 2022; Watson 2009). I examine mobility responses to shootings as well as the effects on neighborhood segregation. Mobility responses include changes in student enrollment, full-time equivalent teachers as well as between-school sorting by socioeconomic and racial groups.

I use a comprehensive dataset of shootings, combined with school-level enrollment data, as well as school-district level socioeconomic and segregation data. Again, while it may be conditionally random which schools experience a shooting, there are no reliable predictors for the timing of a shooting at the school-level. Therefore, my main empirical approach is a staggered Difference-in-Differences (Callaway and Sant'Anna 2021) that relies on the exogeneity in timing of shootings. I report the dynamic Average Treatment Effect on the Treated (ATT) in an event-study style plot as well as the overall ATT.

Figure 2. Shootings Heatmap



Notes: Shootings heatmap for contiguous United States, 1970–2023, based on data from Riedman 2024.

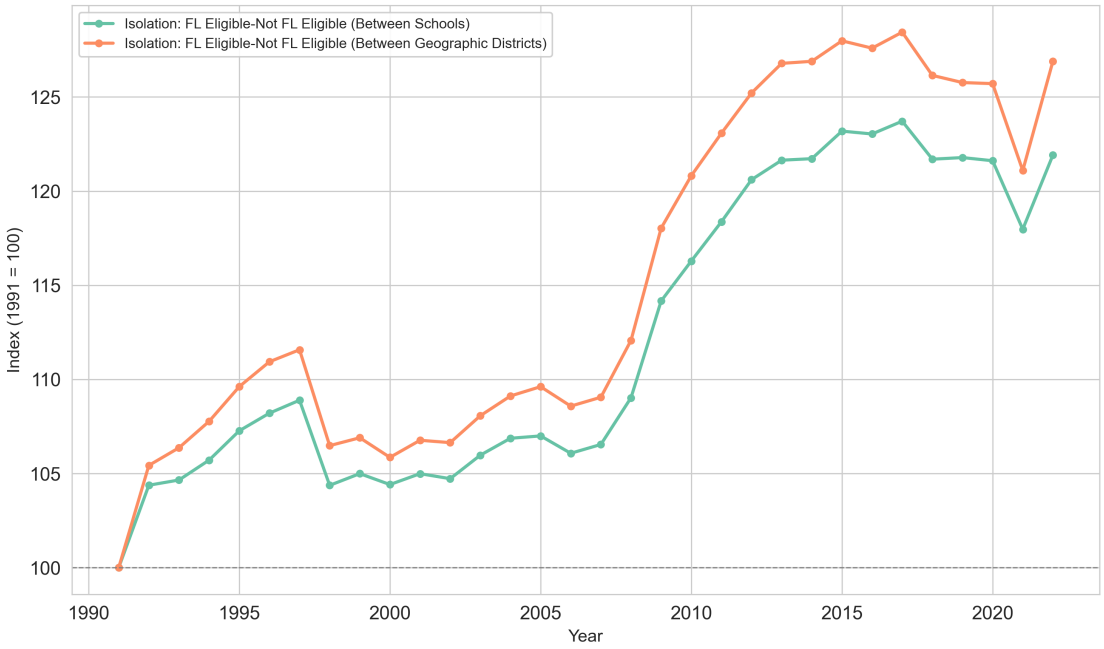
A huge advantage of studying within-district changes is that while the total number of families in the neighborhood may be unchanged, there could still be significant sorting across schools within a school district. While previous research focused primarily on enrollment changes, I also estimate causal effects on the sorting of students within a district. The more aggregate the level of analysis, the more difficult it is to capture mobility responses due to the smaller samples and for multiple confounding factors. For example, going from student-level to district-level (or in the extreme state-level). Thus, we can expect to have less precisely estimated results than if we would have had student-level data. The literature finds mixed evidence for this.

My main contribution is two-fold. First, the school shootings literature has focused heavily on enrollment responses to a shooting. I build on this and contribute to the crime and school shootings literature by estimating effects on between-school socioeconomic segregation. The driving hypothesis is that there is a non-random out-mobility of students following a shooting, and therefore, there may also be a selective *sorting* of students into other schools post-shooting. Second, this paper speaks to a broader 'importance of neighborhood' literature. Considering safety at school as a neighborhood amenity, the paper also relates to the moving by preferences literature (Clark 1991; Lee and Lin 2017; Schelling 1969). In segregation studies, oftentimes both preferences

and neighborhood amenities are endogenous. School shootings are an exogenous violence shock, allowing us to study the effects of extreme violence on sorting and segregation.

At the school-level, I find a decrease in enrollment by approximately 4%, driven by fewer non-poor students staying in the school. There is a drop in both lowest and highest grade enrollment following a shooting, but the decline is greater in the highest grade. At the school-district level, school-district segregation sees modest increases following a shooting. This implies that remaining families are worse off economically and that there is non-random mobility following a shooting.

Figure 3. Isolation Index: Free Lunch eligible



Notes: A Free Lunch to Non-Free Lunch eligible isolation value of 0.5 means that the average Free Lunch Recipient student attends school that has 50% Free Lunch eligible. An increase in the isolation index implies greater clustering of Free Lunch eligible. This value is interpreted in relation to the existing share of Free Lunch eligible.

1 Previous Literature

The School Shootings Literature in Economics

Despite the importance of this topic, the school shootings literature in economics is quite limited, with no more than 20 papers in total. The consensus in the literature is that school shootings affect educational outcomes negatively, but the effects on mobility are unclear.

Several papers have studied the effects on educational, labor market and health outcomes. As expected, the effects are mostly negative—reducing test scores (e.g. Beland and Kim 2016; Bharadwaj et al. 2021; Poutvaara and Ropponen 2010), increasing anti-depressant prescriptions (Rossin-Slater et al. 2020), lowering likelihood of high-school graduation, college enrollment, and future employment (Cabral et al. 2021; Sezer 2022).

There are two sets of circumstantial evidence suggesting that school shootings may have an effect on mobility—perceived safety and neighborhood economic conditions. Regarding perceived safety: On an individual level, Hodges et al. (2023) find that students exposed to school shootings were more likely to avoid school due to feeling unsafe. On a neighborhood level, Soni and Tekin (2023) document a decrease in the likelihood of having excellent community wellbeing and emotional health. Regarding property values, Muñoz-Morales and Singh (2023) document a negative effect of school shootings on property values in affected school districts. Brodeur and Yousaf (2022) find that residents in counties experiencing mass shootings develop pessimistic views of local business conditions. Taken together, these results suggest that if people act on those feelings, school shootings should lead to an outflow of students from the affected schools or neighborhoods.

However, there is no consensus on the effects of school shootings on mobility. Sezer (2022) finds that survivors of school shooting are less likely to move out of the shooting neighborhood into an affluent district, county or state. Deb and Gangaram (2024) find "no evidence of differential probabilities of migration by exposure to school shootings" for US shootings between 1970–2019.[1] In contrast, Abouk and Adams (2013) document an increase in private school enrollments the school year following a school shooting as well as a decline in public school enrollments for shootings in the US between 1998–2009.[2] Using school-level data, Yang and Gopalan (2023) find that there is a decline in enrollment following a shooting driven by richer students (i.e. students that are not Free or Reduced Lunch Recipients).[3] Yang and Gopalan (2023) do not find that private schools gain enrollment from public schools suffering from shootings, rather that "shootings reduce the desirability of the community" as a whole. Beland and Kim (2016) find a reduction in grade 9 enrollment following a shooting in states with Open Enrollment, but no reduction in states

1. Deb and Gangaram (2024) write "if families with children affected by school shootings (or the children themselves as young adults), assuming negative causal effects of exposure, subsequently moved out of those counties, then the estimated effect would be biased toward zero." Their migration analysis "using the ACS shows that there is no evidence of differential migration between exposed and unexposed individuals." They do not clarify in detail how they estimate this or what the control group is, or when this is estimated.

2. Abouk and Adams (2013) use a population weighted WLS regression on the state-year level with state and year FE for the outcome log state {private, public} enrollment.

3. Yang and Gopalan (2023) use an event-study and a Difference-in-differences approach with the outcomes log total enrollment, log FRPL enrollment and log private school enrollment.

without open enrollment. Cabral et al. (2021), using a balanced individual-level panel, find ”no significant difference in the rate of attrition [out of the Texas Education Agency] between students at shooting and control schools.” They also do not find that students at treated schools have higher rates of school switching than at control schools. This, I argue, is not surprising since Texas has no state-wide mandatory open enrollment laws. Each paper investigates mobility from a slightly different angle.

A potential explanation for the differences between estimates of mobility responses within-student (e.g. Cabral et al. 2021) and between-schools (e.g. Yang and Gopalan 2023) is that while the within-student mobility estimates are low, since shooting schools on average have more students, these small individual effects may aggregate to a sizeable effect on the school-level. [4]

This seemingly paradoxical result could also be explained as follows: the likelihood of students in exposed schools to switch schools is similar to a comparable control group, but the schools that experience shootings could experience smaller incoming cohorts in the years following the shooting, thus resulting in a negative effect on total enrollment at the school-level. In this framework, we may also have unchanged total enrollment due to the changes in incoming and outgoing students cancelling out. I explore this further in Appendix C.

For there to be any mobility response, there must be opportunities to switch schools. Importantly, none of the previous literature accounts for differences in open-enrollment laws. The closest would be Beland and Kim (2016), who run a Difference-in-Differences regression of grade 9 enrollment on school shootings, splitting the sample between schools in open enrollment and non-open enrollment states. Their treated group consists of schools that experience deadly homicidal shootings, and the control group is ”all other high schools in the same district.” For this to be a fair counterfactual, one would have to believe that other high schools in the same district are not affected by the shooting. They rightly identify that results at the school-level identify ”total effect” (i.e. ”sum of compositional change and individual effect”), but only conclude that since there is an individual level effect, we know that the total effect is not just compositional. Simply including school fixed effects does not account for the difference in propensity of moving schools within and between districts—because it assumes a common treatment effect across schools abiding by different open enrollment laws. Thus, there is room to consolidate the findings on mobility. The previous literature does not explicitly document the changed SES composition of neighborhoods following a school shooting.

The school choice literature discusses three main mechanisms by which students sort into schools. Families can choose among public schools through their residential choice, families can opt out of the public system entirely and enter a private school, and finally, some public schools may sort students through tracking (Nechyba 2006). Families factor in the perceived safety at school when making a school choice (Hailey 2025). For example, exploiting geographic catchment area boundaries cutting through neighborhoods, Black 1999 finds that families have a 2.5% higher willingness to pay for a 5% increase in test scores. All this implies that parents may value school safety and quality enough to switch their childs' school if that amenity is negatively shocked.

4. Individual fixed-effects do not require baseline levels of mobility propensity to be the same in treated and control groups, rather, it requires that the counterfactual trend behavior of treatment and control groups are the same (Angrist and Pischke 2009.) The question we should be asking regarding the relation between attending a shooting school and mobility is whether students attending a shooting school would have had higher propensities to move anyways (i.e. without exposure to a shooting). If that is the case, then a school-fixed fixed effect would absorb this baseline difference.

Anecdotal evidence from news reporting (Appendix Figure .1) backs this up, with parents threatening to pull their kids out of the affect school.

2 U.S. School System and School Shootings

In school year 2015–16, the U.S. had approximately 50.4 million students enrolled in public elementary and secondary schools, with a total of 18,328 school districts and 98,456 schools.[5] The figures for California, the largest state in the country, were 6.2 million students, 1,163 school districts and 10,303 schools.

Each school district has one or more attendance areas. These attendance areas, also known as catchment areas, are used in assigning students to schools. This speaks to the strong connection between residential and school choices. For example, most school districts in the northern Bay Area have several attendance areas, as seen in Figure 4.[6] Different states have different open enrollment rules that give a certain autonomy to school districts regarding the placement and transfer of students. In short, some states allow districts to decide whether to accept students from outside the district, or to allow students to switch schools within the district, and other states mandate how the districts should handle these situations. This matters because if families can switch schools without having to relocate their residence, we may find stronger effects of shootings on enrollment. On the other hand, if open enrollment is not permitted, families have no option but to relocate in order to switch schools. Thus, it may be the most economically well-off that can afford to switch schools by affording to move.

School shootings

Some of the deadliest shootings are Columbine ('99, 13 killed), Sandy Hook ('12, 26 killed), Parkland ('18, 17 killed), Santa Fe ('18, 10 killed), and Uvalde ('22, 21 killed). There are several definitions of school shootings and several data sources. Irrespective of the source, there is geographic variation in the number of school shootings. Following The American School Shootings Study (TASSS), as developed by Freilich et al. 2022, a conservative estimate of school shootings in the U.S. between 1990 and 2016 is 652 shootings. This has an average of 18 intentional shootings and 9–12 lethal shootings per year. Most shootings occur in high schools, but those that occur in elementary schools are deadlier. Freilich et al. 2022 find no clear time trends in the types or number of shootings during the observed time-period. Since 2016, unfortunately, no matter how we count shootings, the number of shootings has gone up dramatically.

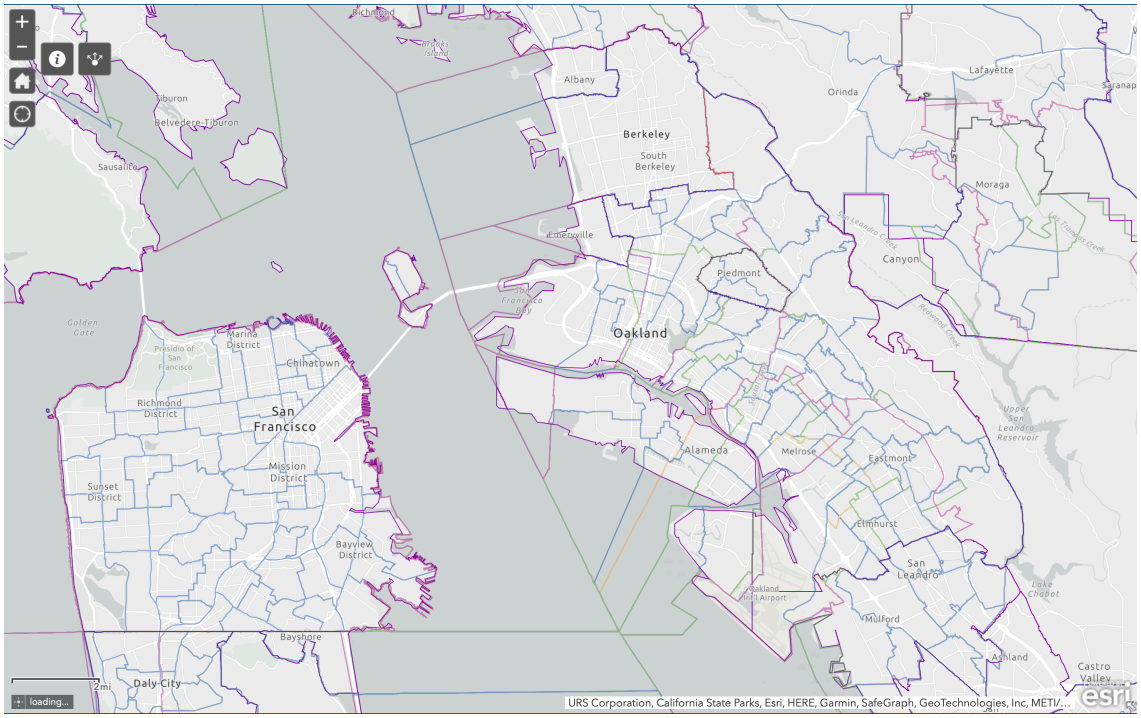
I use a dataset on school shootings that includes events where a gun was brandished or fired, or bullets hit school property, irrespective of time or day. These amount to approximately 2,600 school shootings, from 1966 to 2024 (Riedman 2024). My estimation period is 1990–2022.

Importantly, there is a consensus in the literature that the timing of school shootings is plausibly exogenous. Even if there is are so-called imitation shootings (long-term) or contagion (short-term), these are at the national or state-level, not at the school-level. The contagion models are

5. Source: Common Core of Data, via NCES

6. The Berkeley Unified School District has three elementary school attendance zones for its 12 public elementary schools since June 2022, as opposed to the single attendance zone shown in the figure.

Figure 4. School Districts and Attendance Areas in the upper Bay Area



Notes: School district boundaries from the 2021–2022 academic year from NCES overlayed with public school catchment area boundaries in light blue lines from 2015–2016, the latest year available, by the NCES, Education Demographic and Geographic Estimate program.

based on granular data (daily or weekly) while this analysis is at an annual-level, thus any contagion is fully captured by the annual data. There is mixed evidence for the presence of contagion in mass shootings (Kissner 2016 find slightly higher probability of a shooting in a state if there has been a shooting in the past two weeks). The imitation shootings are long-term and cannot be temporaly pinned down. For example, many shootings are later found to have been inspired by the Colombine 1999 shooting, many years later. Most of the imitation or the contagion models are for mass shootings. The mechanism by which contagion occurs is often argued to be traditional or social media. There is mixed evidence for the role of media (Fox et al. 2021). In our setting, we study both mass shootings and non-fatal shootings that are much less likely to be picked up by any media, let alone national media. Imitation and contagion are therefore not a strong threat to identification in this paper. To the extent that the location of shootings is not random, its predictors (such as being in an urban area) are well-known and can be controlled for. This means that the

location of school shootings is as-if random conditional on covariates.

3 Schooling, Neighborhood and Shootings Data

I rely on two sources for school-level information. For basic administrative data, I use the Common Core of Data (CCD) by the National Center for Educational Statistics.[7] This covers all public elementary and secondary schools in the United States. The CCD contains school identifiers (such as NCESSCH codes, Local Education Agency code, latitude and longitude, etc.). It also contains information on the number of enrolled students in each grade, number of full-time equivalent teachers, and number of Free or Reduced Lunch Recipients (a proxy measurement for number of poor students). Students from households with incomes at or below 130% of the federal poverty level are eligible for free lunch. For students from households earning between 130% to 185% of the federal poverty level, a reduced price lunch is available (U.S. Department of Agriculture, Food and Nutrition Service 2025). Students above these thresholds that are not in other lunch programs purchase a paid meal according to prices set by their school district. Biennial data on the racial composition of schools and other socioeconomic indicators from 2009–2022 come from The Educational Opportunity Project at Stanford (Reardon et al. 2024). [8]

Data at the school-district level also come from the Stanford project. At this level, there are measures of segregation, such as exposure indices between poor and non-poor students. This data therefore allows us to indirectly measure selection into schools within a district after a shooting. The following three indices are most relevant: the Information Theory index, the SES Composite index, and the Relative Diversity index. Socioeconomic segregation in a school district is captured through the between-school Exposure index. The Information Theory index is the average deviation of each student's school's socioeconomic diversity from the district-wide socioeconomic diversity. A value of 0 indicates no segregation and a value of 1 indicates complete segregation (Theil 1972). The Socioeconomic Composite index is a normalized score for districts overall socioeconomic status. The index is composed of median income, proportion of adults with bachelor's degree or higher, poverty rate among households with 5–17 year-olds, proportion of households receiving food stamps, and proportion single mother headed households. The Relative Diversity index captures the Free or Reduced Lunch Recipient to non-Free or Reduced Lunch Recipient gap in exposure to Free or Reduced Lunch Recipient students (Reardon and Firebaugh 2002).

I summarize the many data sources for school shootings in Table A.1. The most comprehensive dataset is the K-12 School Shooting Database (abbreviated K12SSD, Riedman 2024). An event counts as a school shooting if a gun was brandished or fired, or bullets hit school property, irrespective of time or day. These amount to approximately 2,600 school shootings, from 1966 to 2024. In the study period, 1990–2022, there are approximately 2,300 schools that experience a shooting. This data is gathered continuously and extensively from government agencies, media or advocacy groups, websites, and blogs. Each incident contains information about the perpetrator, the setting, and is tagged by its geolocation in latitude and longitude.

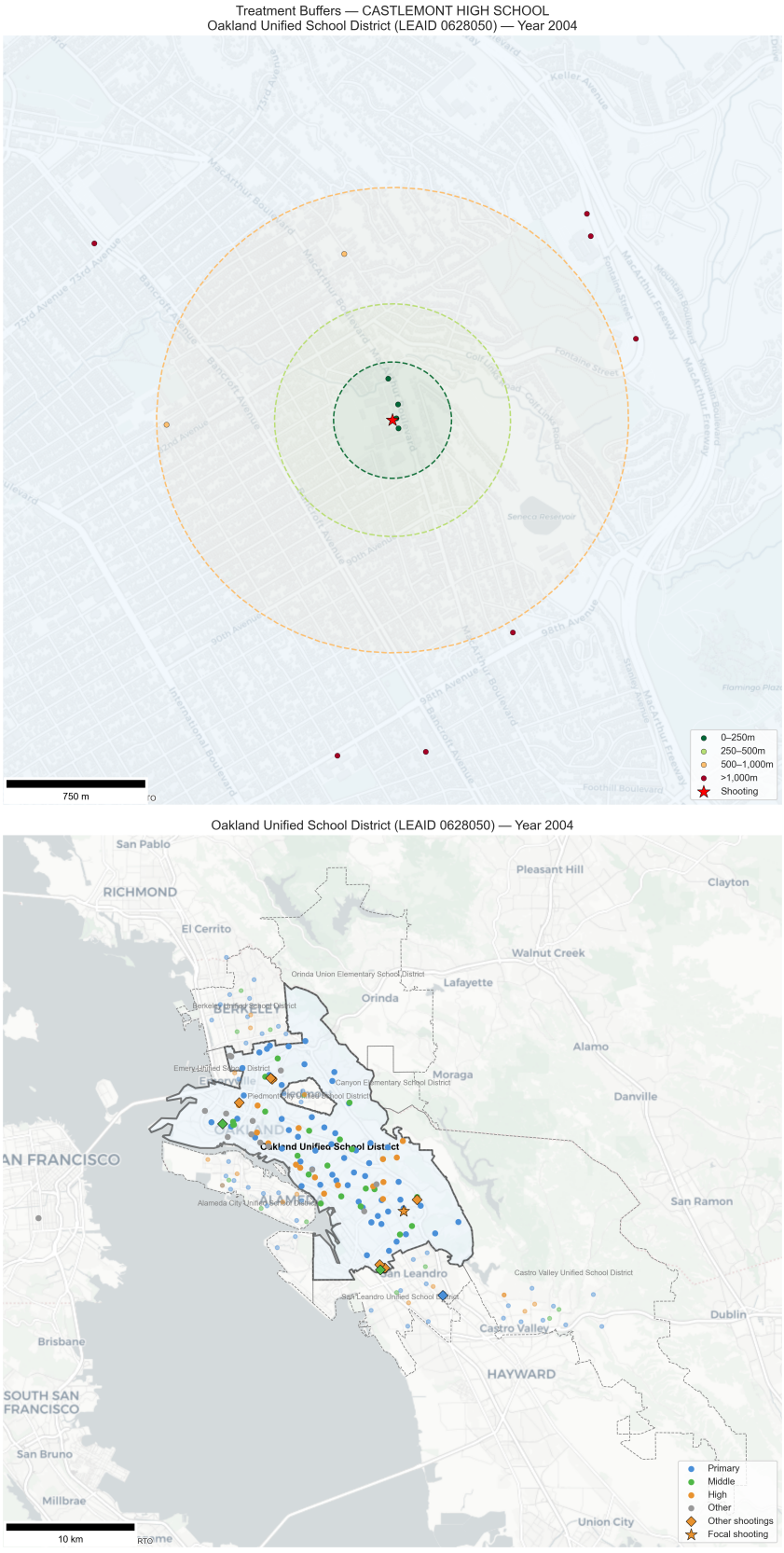
As shown in Figure 5, I perform a spatial join of the school and shooting data. The top panel shows my preferred treatment radius of 500m as well as two other radii. A robustness and spillover

7. As of June 2025, data can be accessed here: https://nces.ed.gov/ccd/.

8. The 2019/20 release was delayed due to the Covid-19 Pandemic. The years available are therefore 2011/12, 2013/14, 2015/16, 2017/18, 2021, 2022/23.

analysis is performed with other radii later in this paper. The bottom panel shows the shooting school (star marker) with other schools color-coded based on their school-level in the same district and in neighboring districts. In this example, there are quite a few alternative high-schools not too far from the focal school.

Figure 5. School Districts and Shootings in the upper Bay Area



Notes: Zoomed in picture showing a school shooting in the focal point and 250m, 500m, and 1000m concentric rings. (Left). This school is also shown in its district and the surrounding districts. (Right)

4 Empirical Approach

I employ a staggered Difference-in-Differences (DiD) approach that exploits exogenous variation in the timing of shootings. This approach presupposes that school shooting incidents are quasi-random events to estimate their causal effects on mobility as captured by enrollment and within-district sorting.

My identification strategy relies on the key assumption that conditional on observed covariates, enrollment and other mobility outcomes would have had the same trends in schools that experience a shooting as in schools that have not yet experienced a shooting, had there been no shooting.

At the school-level, there is good annual data on enrollment. To capture effects on sorting, we turn to the school-district level. I run similar analyses on the school-level and the school-district level, but with outcomes relating to poverty and segregation.

If the treatment changes over time, i.e. the characteristics of schools that experience a shooting over time changes, then we would be worried that the not-yet-treated schools are an imperfect control group. For that, we would need an alternative control group.

The main challenge is to find a suitable control group for the shooting schools. We could think that other schools in the neighborhood would provide good control schools. After all, they share the same neighborhood characteristics and other potential confounders. However, it is very likely that these other schools in the same neighborhood are directly or indirectly affected by the shooting. A neighboring school might be directly affected, by experiencing the same threat and engaging in lock-downs and other measures to protect their students. This would be a direct effect where neighboring schools are essentially themselves treated schools. Alternatively, neighboring schools could be a refuge for students that leave shooting schools. The neighboring school could temporarily be taking in students from the affected school. Therefore, although the neighboring school is not itself treated, there could be spillover effects making it an unsuitable control school. I perform a spillover analysis to answer this (Section 5.3).

4.1 Staggered Difference-in-Differences Exploiting Exogenous Timing

The traditional two-way fixed effects (TWFE) estimator can be biased in settings with staggered treatment and heterogeneous treatment effects across cohorts or time. School shootings are such a setting. In the case of heterogenous treatment effects, the TWFE would give us biased estimates as it uses already-treated schools as controls (leading to the so-called forbidden comparisons, as discussed in De Chaisemartin and d'Haultfoeuille 2020).

I employ the staggered Difference-in-Differences estimator of Callaway and Sant'Anna 2021, which addresses these issues by computing group-time average treatment effects. Units are grouped by their treatment timing (cohort g), and effects are estimated separately for each cohort at each post-treatment period t.

The group-time average treatment effect for cohort g at time t is:

$$ATT(g,t) = \mathbb{E}\left[Y_t(g) - Y_t(0) \mid G_g = 1\right] \tag{1}$$

where $Y_t(g)$ is the potential outcome under treatment at time g, $Y_t(0)$ is the potential outcome without treatment, and $G_g = 1$ indicates membership in cohort g.

To summarize treatment effects, I aggregate the group-time ATTs into event-study parameters. For each event-time e (periods since treatment), I compute a weighted average of cohort-specific effects:

$$ATT^{dyn}(e) = \sum_{g \in \mathcal{G}} \omega_g \cdot ATT(g, g+e) \tag{2}$$

where $\mathcal{G}$ is the set of treatment cohorts, and $\omega_g = P(G = g \mid G \leq T)$ weights each cohort by its relative size among ever-treated units. This aggregation produces event-study style estimates showing how effects evolve with time since treatment, averaging across cohorts that experienced shootings in different years. I report the dynamic Average Treatment Effect on the Treated (ATT) in an event-study style plot. For analyses that are run on separate samples, I also report the overall ATT by sample in a plot. The coefficients in the event-study plots for log-transformed outcomes are interpreted as average percentage changes in the outcome a certain number of periods after exposure to a shooting. For percentage outcomes, the coefficients are interpreted as percentage point changes.

5 School-Level Mobility

I first focus on mobility as captured by log enrollment at the school-level.

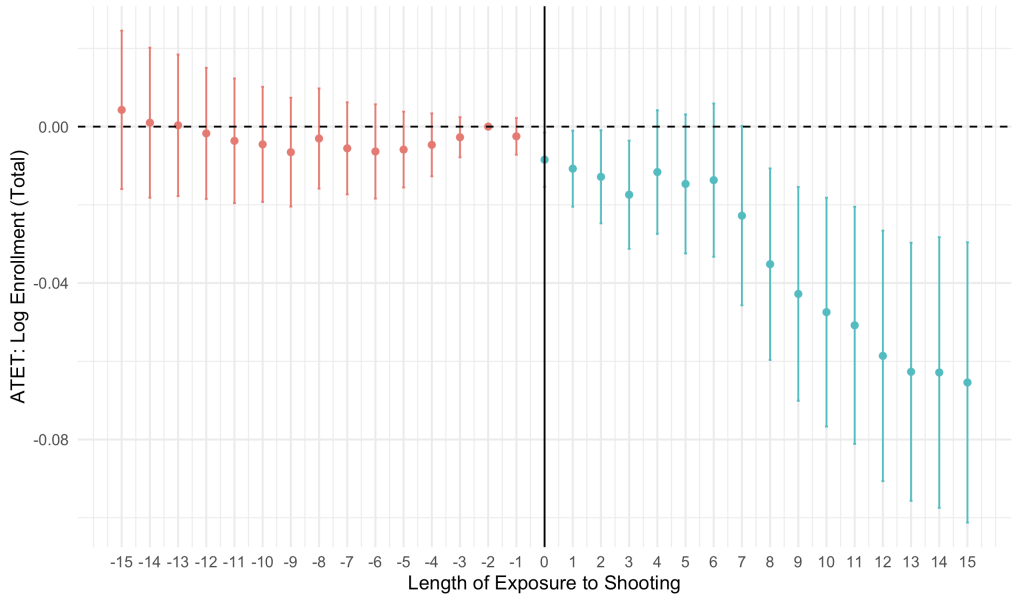
A dynamic aggregation by length of exposure to shootings in Figure 6 shows an average negative effect of -0.043. Given that the average shooting school had approximately 632 students (log enrollment of 6.45) pre-shooting, this is a decrease of about 4% or 25 students.

Putting these estimates in perspective, enrollment following the biggest mass shootings in the U.S. declined by 5%, 12%, 1% and 7% for the Columbine, Sandy Hook, Parkland, and Santa Fe shootings. Considering that we are looking at a collection of mass shootings as well as non-fatal gun brandishing, an average effect of around -4% is within a reasonable range of estimates.

These results are in line with the enrollment declines found in Yang and Gopalan 2023 (5%, with schools in non-shooting districts as comparison). Similar to them, I find that the long-run effects are more negative than the average effect. Studying high-schools rather than K-12, Abouk and Adams 2013 found a 0.4%–1.3% decline in public high schools state-wide while Beland and Kim 2016 found 5.8% decline in earliest grade enrollment in high schools at the school-level.

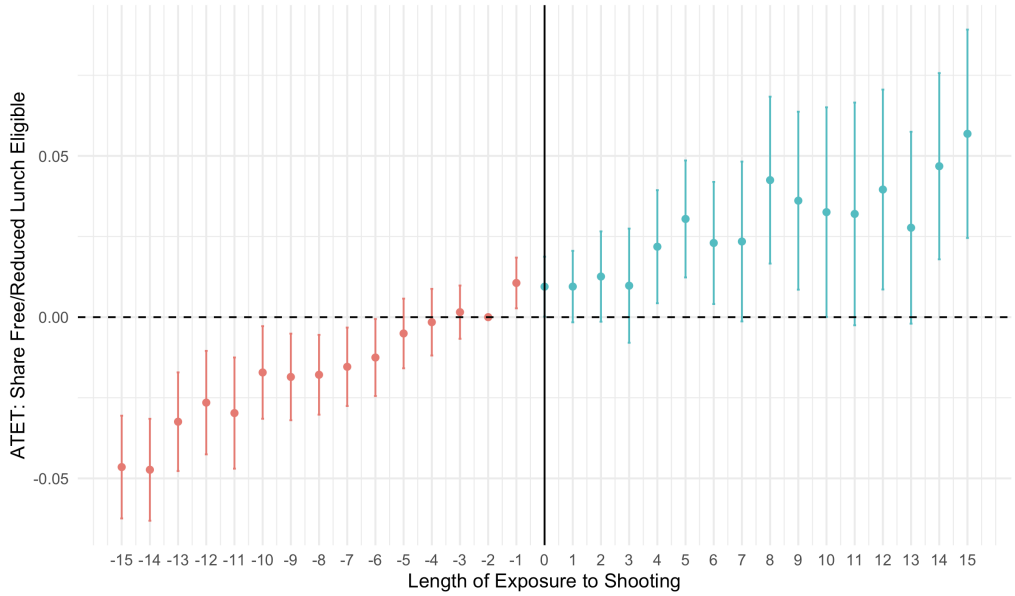
How does the school's student composition change in terms of socioeconomic backgrounds? Figure 7 shows a positive effect on the share of Free or Reduced Lunch Eligible students following a shooting, though imprecisely estimated.

Figure 6. Log Enrollment



Notes: Dynamic Difference-in-Differences estimates using not-yet-treated comparison schools relative to first shooting year

Figure 7. Free Lunch Eligible



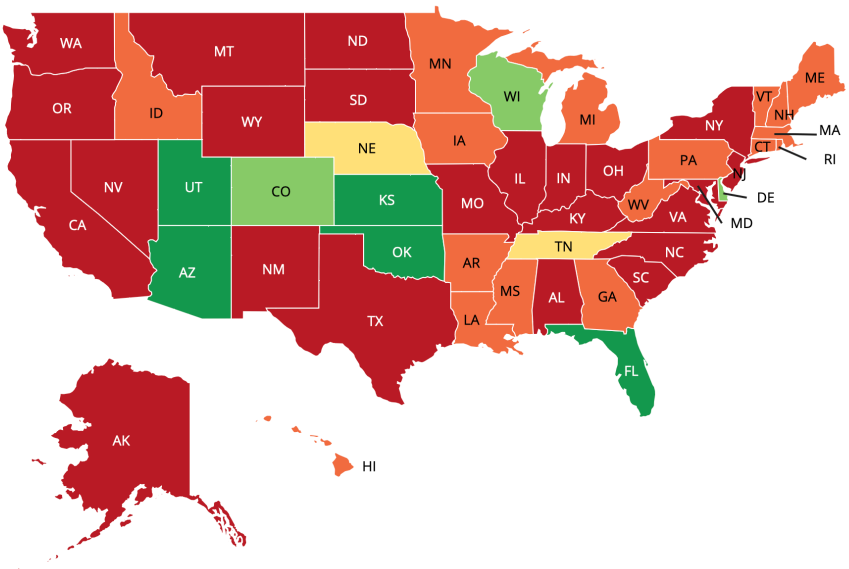
Notes: Dynamic Difference-in-Differences estimates using not-yet-treated comparison schools relative to first shooting year

5.1 Heterogeneity by Ease of Switching Schools

Open Enrollment laws. There may be institutional reasons for why we see a strong mobility response in some schools and not in others. In the U.S., the ease of switching schools is a state-matter. Figure 8 shows which states have the most stringent open enrollment laws. We can broadly categorize states into those that allow students to switch school within a school-district, and those that allow students to switch between school-districts. Note that the exact regulations are set by school-districts. For example, while a state might permit intra-district change that does not mean that the district must allow intra-district changes—only that it may.

Figure 9 estimates the dynamic average treatment effects on log enrollment, splitting the sample between states that permit within-district school switching and those that don't permit that. In states where intradistrict open enrollment is permitted, we see a stronger outflow of students from schools following a shooting, consistent with the idea that ease of switching facilitates mobility responses. Figure 10 splits the sample by states that allow between-district school switching and those that don't. Interestingly, for interdistrict open enrollment the pattern is reversed: states where school switching between districts is not permitted have more negative effects on enrollment. However, inspecting the number of shootings per state over time (Figure B.1) shows that—with the exception of Florida—9 of the 10 most affected states have strict interdistrict open enrollment rules. Figure 11 puts these overall ATT in relation to the full sample ATT.

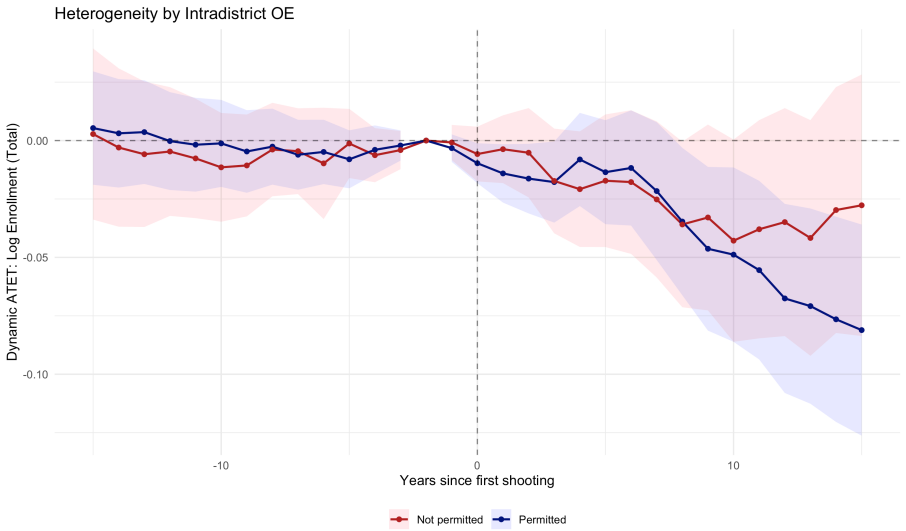
Figure 8. Open Enrollment Best Practices by state



Notes: 5-point system for Mandatory Cross-District OE, Mandatory Within-District OE, Transparent Reporting by State Education Agency, Transparent and Public Posting of Vacancies, and (Tuition) Free Access to All Public Schools. Dark green indicates 5/5, dark red indicates 0/5. Source: https://reason.org/policy-brief/public-schools-without-boundaries-a-50-state-ranking-of-k-12-open-enrollment/#california-diversions

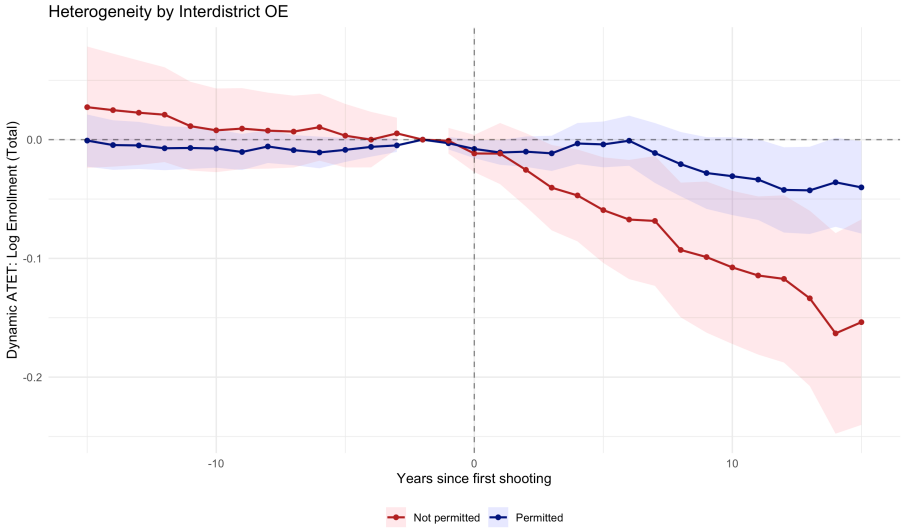
Number of Schools in District. A central thesis is that we cannot expect students to switch schools if it is difficult to do so. One dimension of ease of switching schools is the number of outside options, i.e. other schools of the same school-level within the same district. For example,

Figure 9. Effects on Log Enrollment by School Choice *Within* District



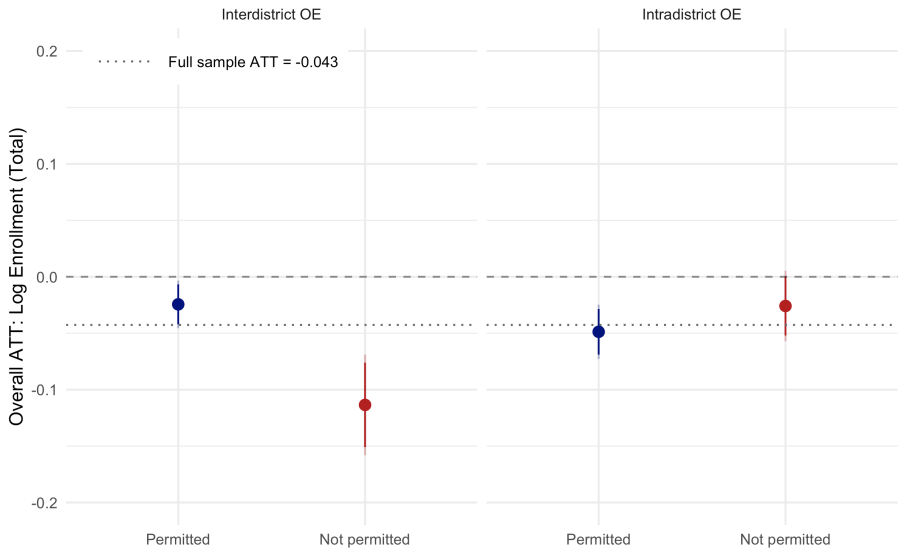
Notes: Dynamic Difference-in-Differences estimates using not-yet-treated comparison school-districts relative to first shooting year. 21 of 50 states permit intradistrict open enrollment.

Figure 10. Effects on Log Enrollment by School Choice *Between* District



Notes: Dynamic Difference-in-Differences estimates using not-yet-treated comparison school-districts relative to first shooting year. 43 of 50 states permit interdistrict open enrollment.

Figure 11. Effects on Log Enrollment by School Choice Regimes



Notes: Overall ATT for different Open Enrollment policies: interdistrict permitted, interdistrict not permitted, intradistrict permitted, intradistrict not permitted.

if a shooting happens in an elementary school and it is the only elementary school in the district, mechanically, we cannot expect any student to switch schools *within* the district. As the number of schools of the same school-level increases, we would expect a stronger negative effect on log enrollment.

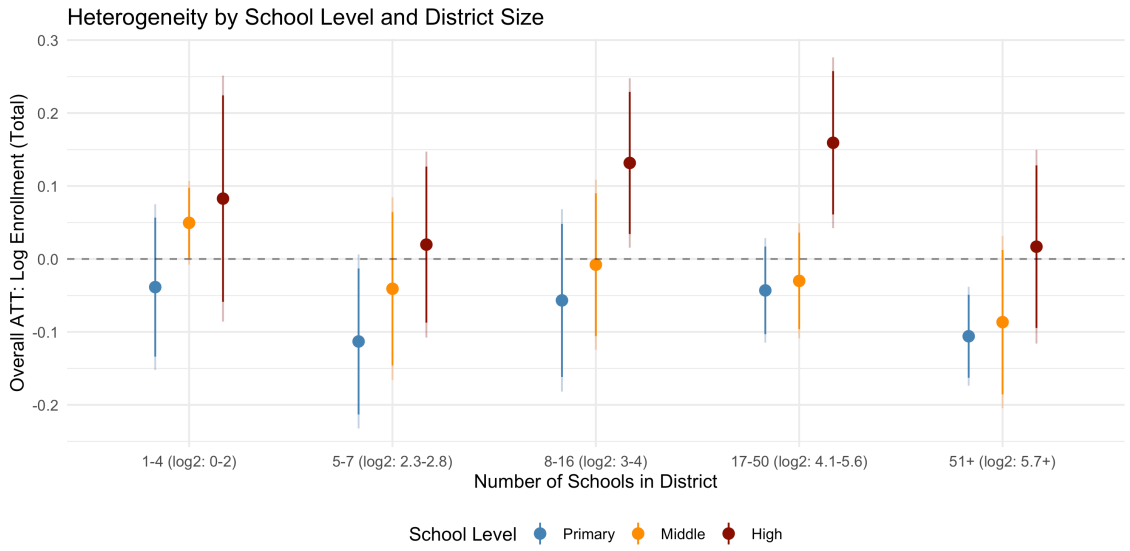
To test this, I bin the number of schools and run the same staggered difference-in-differences estimation separately per bin and school-level. The distribution of number of schools per district is heavily right-skewed, with very few big districts and many small ones. The biggest geographic school districts are New York City Public Schools (1,600 unique schools), Los Angeles Unified (1,100 unique schools) and the City of Chicago School District (1,000 unique schools). To put this in perspective, the median district has 3 schools and the mean district has 6 schools. To account for the skewness of the distribution of count of schools per district, I calculate quintiles on the $\log_2$ scale. This gives the bins roughly equal numbers of school-observations in the $\log_2$ space. For example, a district with 1–4 schools has log_2 between 1 and 2, while a district with 51 schools has a $\log_2(51) = 5.7$.

Figure 12 shows the overall ATT of shootings on log enrollment run separately by bin and school-level. [9] Two results stand out. First, the effects are always more negative for primary schools (grades K–3) than middle or high schools. Secondly, in above-average sized districts, the effects on high schools are positive and statistically significant. This garners further investigation.

Lowest vs. Highest Grade Enrollment. Another dimension of changes in total enrollment is whether they are driven by fewer incoming students or by fewer graduating students. Naturally, both forces may be at play here. We can still investigate separately how enrollment changes in the lowest grade offered by the school versus the highest. Splitting school enrollment by lowest

9. The CCD raw data does not include the variable school level before 2000, but school level is constructed based on information about the the lowest grade offered, available from 1986. A school level is determined solely by the lowest grade offered.

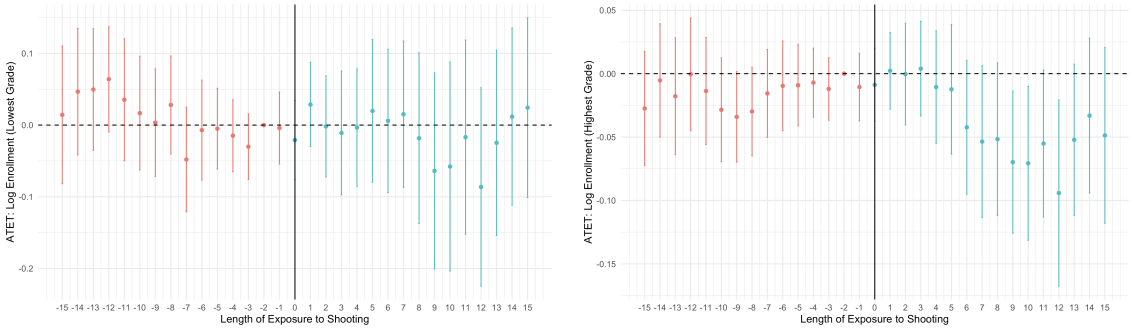
Figure 12. Effects on Log Enrollment by Number of Same-Level Schools in District



Notes: Overall ATT of effects on log enrollment by number of same-level schools in a district. The x-axis the range of number of schools in each bin, followed by their $\log_2$ equivalent in parentheses.

versus highest grade in Figure 13 shows an interesting pattern. There seems to be an immediate drop in the highest grade enrollment in the year of the shooting, though the estimates are noisy.

Figure 13. Log Enrollment by Grade Level



(a) Lowest Grade

(b) Highest Grade

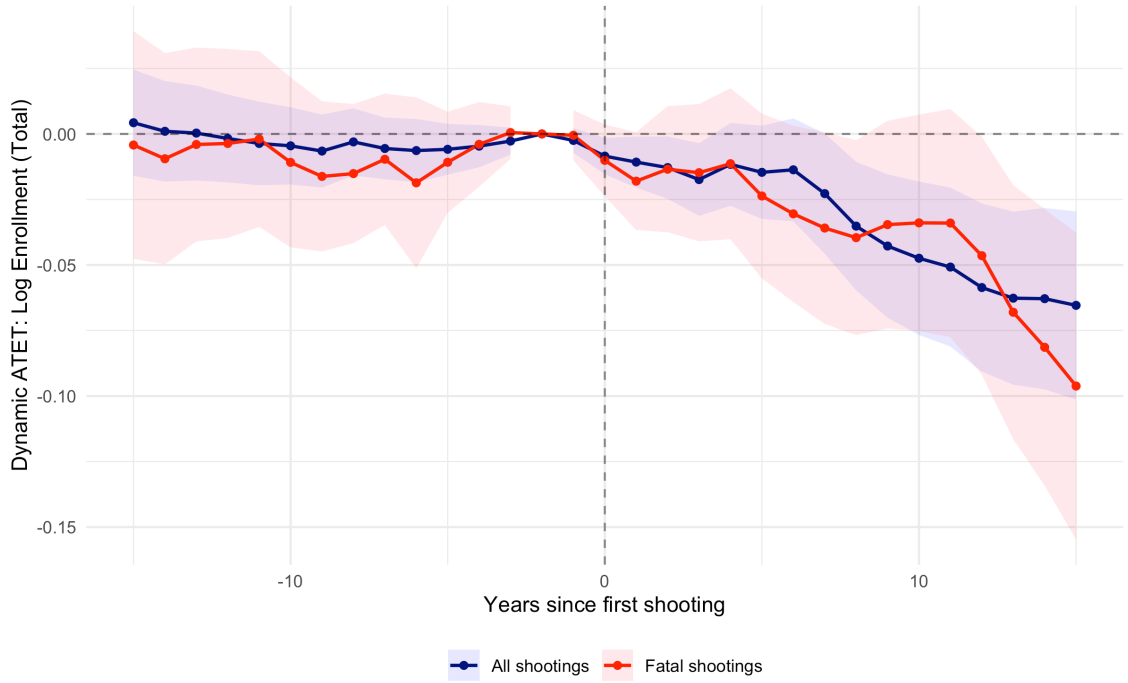
Notes: Dynamic Difference-in-Differences estimates using not-yet-treated comparison schools relative to first shooting year. Left: lowest grade enrollment. Right: highest grade enrollment.

5.2 Heterogeneity by Fatality and Underlying Violence Level

Shooting Fatality The results so far pool all shootings together. A natural question is whether the effects are driven by fatal shootings. Figure 14 overlays the enrollment estimates from the full sam-

ple with those restricted to fatal shootings. Focusing on shootings that result in at least one fatality, the decline is more pronounced, particularly at longer horizons where it reaches approximately a 10% decline in enrollment, compared to about 4% decline on average for all shootings.

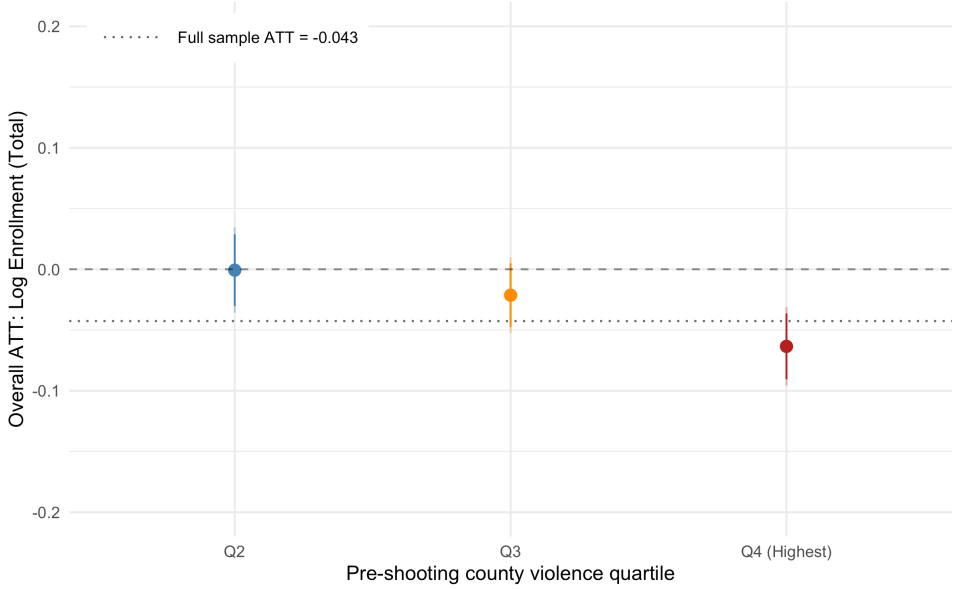
Figure 14. Log Enrollment: All Shootings vs Fatal



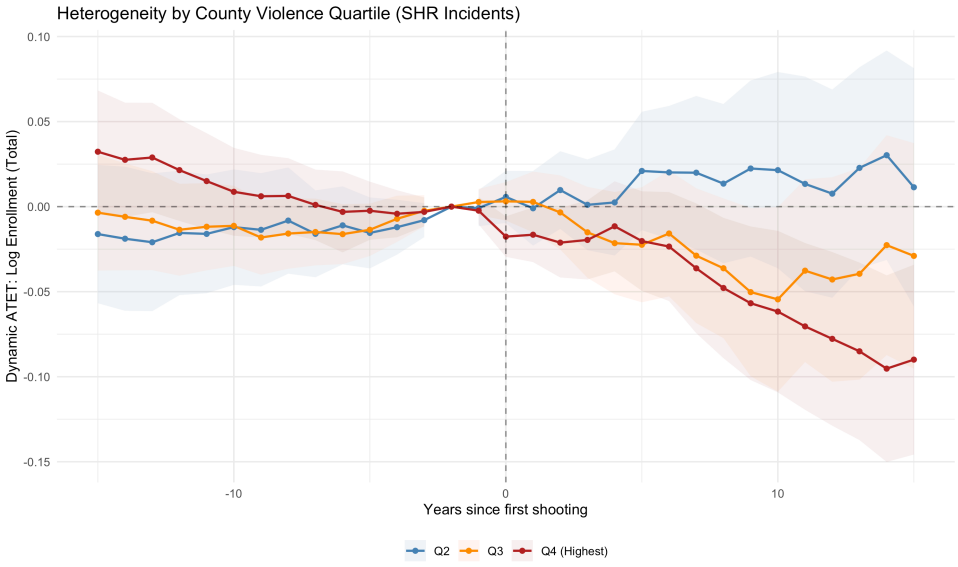
Notes: Dynamic Difference-in-Differences estimates using not-yet-treated comparison schools relative to first shooting year. Overlays all shootings with fatal shootings only.

Historical county violence. The enrollment response may also vary by the level of pre-existing violence in the county. In a peaceful neighborhood, a school shooting may be seen as an extreme outlier event, not prompting any action on the parents part. In an otherwise violent, one might also expect a school shooting not to be as salient given the high levels of violence, and therefore not prompting any action on the part of the parents. That is unless a school shooting is seen as the event that triggers a response to an otherwise violent environment. Figure 15 splits the sample by quartiles of pre-shooting county violence (as measured by the Supplementary Homicide Reports victim count). For treated schools, I use the average pre-treatment SHR county victim count. Never-treated schools get their mean value of SHR victim count based on their whole period in the data. Once there are is a unique pre-treatment (or overall) mean value of SHR county victim count, I split the sample into five quantiles. There is strong indication that schools in the most violent counties (Q4) see the largest enrollment decline following a shooting, while schools in less violent counties (Q2) show near-zero effects. The dynamic plot shows that Q4 counties drive the overall negative effect, with enrollment declining steadily in the years following a shooting.

Figure 15. Effects on Log Enrollment by Historical County Violence



(a) Overall ATT



(b) Dynamic ATT

Notes: Dynamic and overall Difference-in-Differences estimates using not-yet-treated comparison schools relative to first shooting year, split by quartile of pre-shooting county violence (SHR victim count).

5.3 Robustness: Spillovers and School Closures

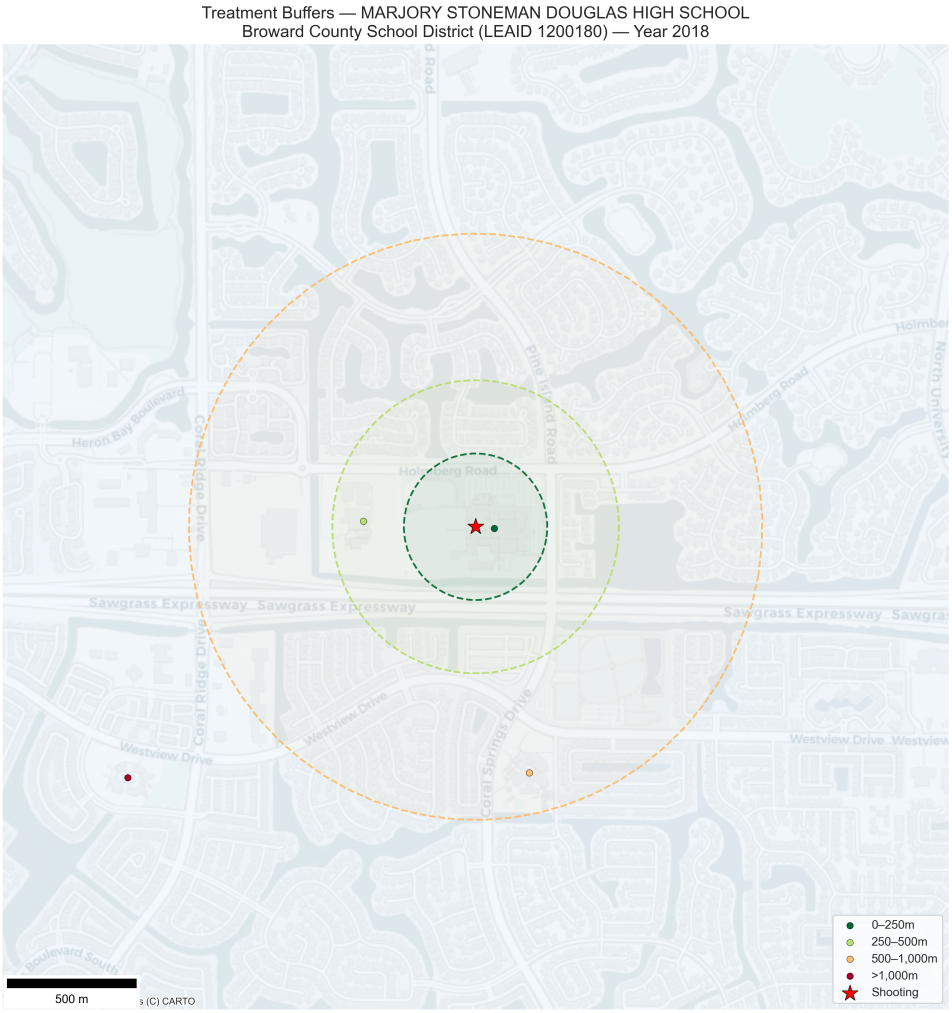
Spillovers and Treatment Radii. In the preceeding analyses, I used the proximity of schools to a school shooting to assign treatment. Schools within 500 meters of a school shooting are assumed to be treated. The alternative to a spatial join was to merge the school shootings dataset and the school directory based on string identifiers such as school name, city, and county. However, since some shootings occur in close proximity to multiple schools (e.g. on a yard for an elementary school and a high school), assigning these shootings to one school only would be problematic. We would risk assigning a school to the control group when it in fact also has experienced a shooting. This also applies to a school building that has multiple school entities (e.g. a school for the gifted and a regular school sharing the same building). My treatment assignment overcomes these issues by relying solely on schools' and shootings' geographic coordinates.

For example, the gruesome mass shooting at the Marjory Stoneman Douglas High School (serving grades 9–12) in Parkland, Florida in 2018 occurred in proximity to the Westglades Middle School (serving grades 6–8). The perpetrator of this shooting was himself enrolled in this nearby middle school in the years prior to carrying out this attack. In Figure 16, a map with increasing radius of treatment assignment shows that students of this middle school would not have been considered treated had the spatial join only captured school units within 250meters of a school shooting. Within 500 meters, both the focal high school and the middle school across the yard are considered affected. A three minute drive away lays the Country Hills Elementary School, captured by the 1km radius in the map. Testimony from a school police, Dave Dittman, jumping into action to help protect the students at Marjory Stoneman Douglas High School, shows how resources and individuals in the nearby community are affected.

The two natural follow-up questions are: How sensitive are the results to the exact treatment assignment of 500 meters? And can we learn anything about spillover effects? To answer this, I rerun the main analysis using six radii of increasing size. As seen in Figure 17, for radii between 250m and 5km, the results are robust, hovering around -4%. The two other prominent patterns are that the effects of shootings on log enrollment goes toward zero (and eventually turns positive) as the radius increases, and the estimates become more precise. For the latter pattern, as our treatment radius increases, so too does the number of treated schools and thereby increasing the precision. For the former pattern, these results tell a story where closeby schools are affected negatively, while a radius of 50km may reflect that schools far away from the focal shooting school may become *more* attractive and gain in student enrollment.

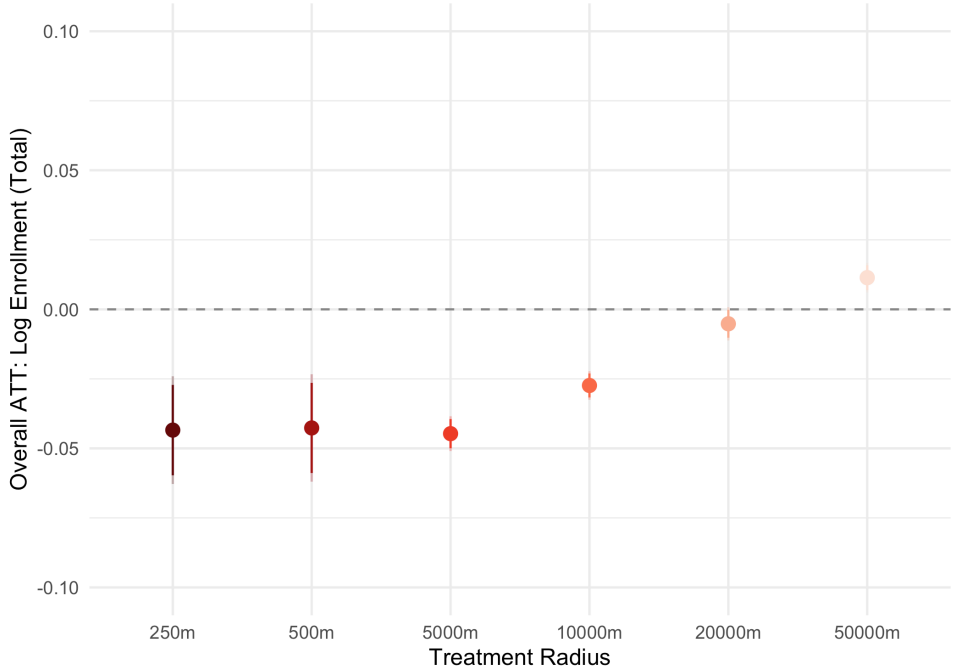
School closures. Do schools survive a shooting? To understand this, we need to know what happens to a school's operational status in the years following a shooting. This is important to know for evaluating what direction bias we may have due to survivorship. In a standard Difference-in-Differences setting, we get a meaningful result of regarding the intensive margin if our estimation sample is balanced. If we have non-random attrition, such that a shooting school is more likely to close and drop out of the dataset, and the remaining schools are positively selected (e.g. due to their good handling of the aftermath of a shooting), then a balanced estimation sample would lead us to underestimate the true negative effects on log enrollment. Figure 18 plots raw data showing that the share of ever-treated schools that close increases drastically after the shooting year. The average closure rate is close to zero pre-shooting, and jumps to around 1.5% five years post-shooting. That's equivalent to 1 in every 66 school that has any kind of bullets fired with intent closing down.

Figure 16. Expanding Treatment Radius Example Map

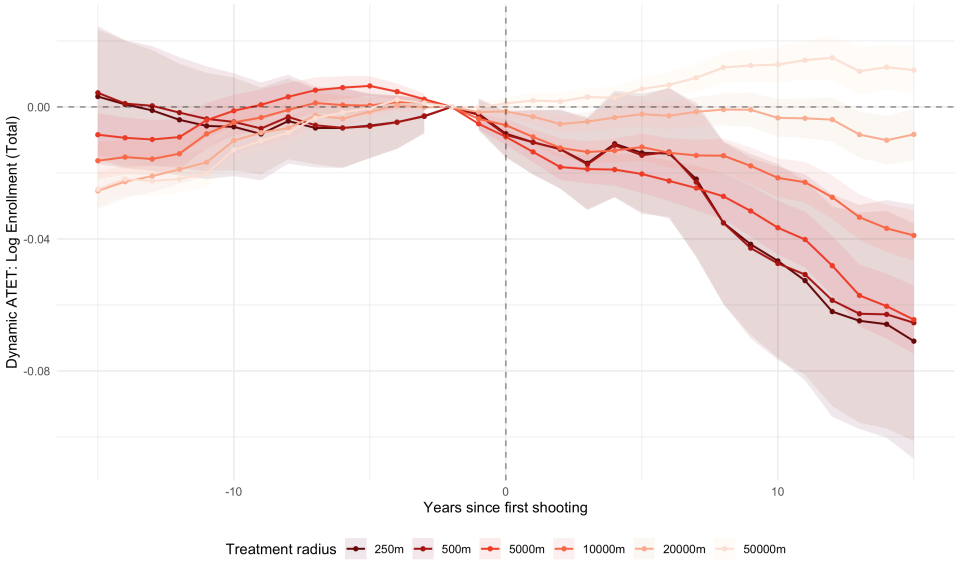


Notes: Map showing Westglades Middle School included in treatment of 500 meters but not in 250 meters. Rings correspond to increasing treatment radius from 250 meters to 1+ kilometers.

Figure 17. Expanding Treatment Radius



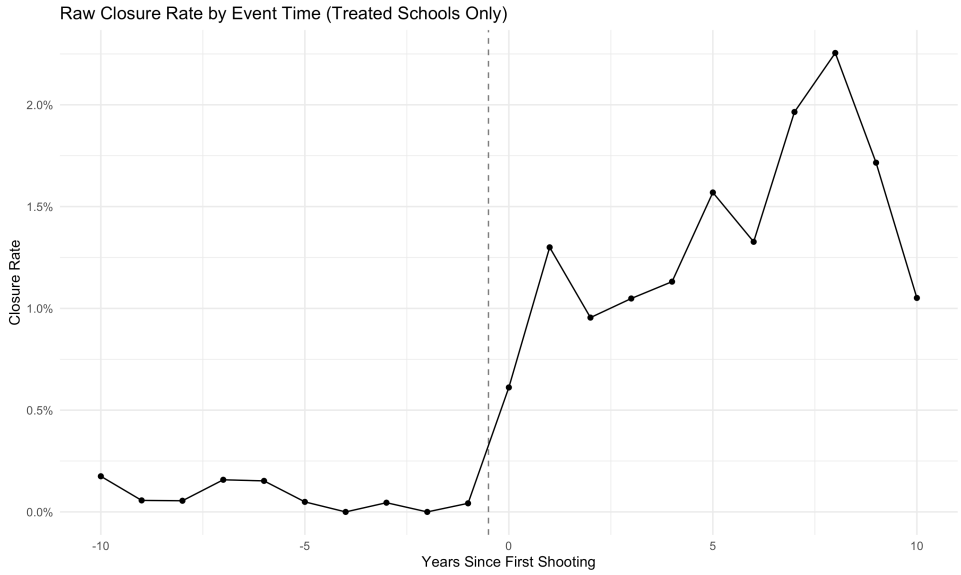
(a) Overall ATT



(b) Dynamic ATT

Notes: Dynamic and overall ATT for the effect of shootings on log enrollment when varying the radius of treated schools from 250 meters to 20km.

Figure 18. Closure Rates among Shooting Schools



Notes: Raw data plot for closure rates relative to shooting year among ever-treated schools.

In summary of the school level findings, a shooting causes a 4% decline in enrollment and an increase in the share of poor students (as proxied by being a recipient of free or reduced lunch). The decline is more pronounced over the long-term and is greater following a fatal shooting. School choice policies matter. In states where within- and between-district switching schools is permitted, there are statistically significant negative effects on enrollment. Where between-district switching schools is not permitted, we find an even greater negative effect. This can be explained by families in these states having a greater propensity to physically relocate in response to a shooting. Whether relocating or simply switching schools, the negative effect is most prominent in the highest-grade enrollment rather than lowest grade. The negative effects of shootings on enrollment are also greater for schools in counties with greater violence levels. In an otherwise violent county, a school shooting may be the final straw that brakes the camels back. As enrollment declines in a shooting school, the nearby schools (up to 10–20 kilometers) also experience declining enrollment. However, schools within 50km[10] from a shooting experience an increase in enrollment following a shooting. These are likely the alternatives at which relocating students end up.

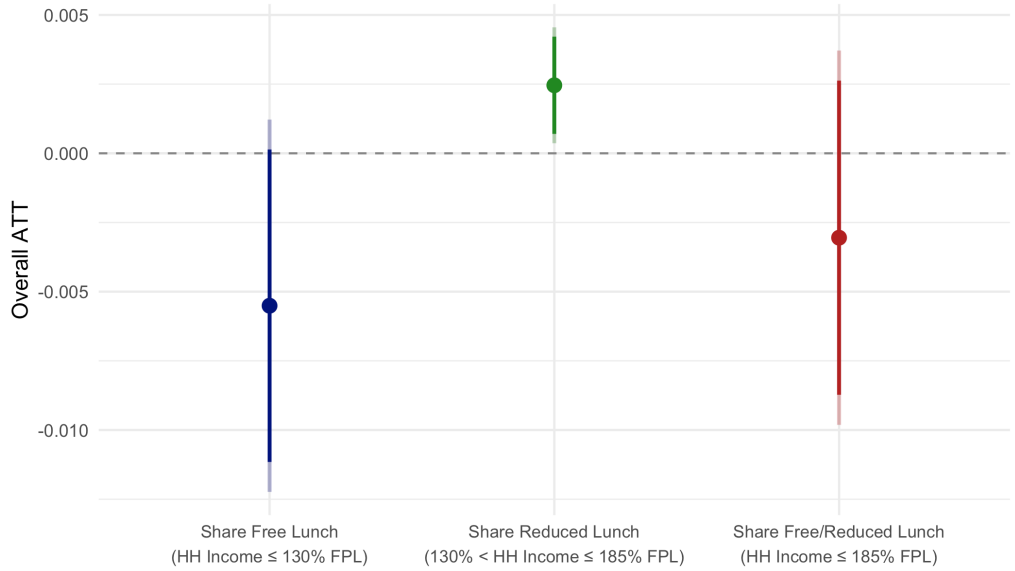
10. This is a distance equivalent to (1) Dallas to Fort Worth, Texas, (2) Philadelphia to Trent, New Jersey or (3) San Francisco to San Jose, California.

6 Neighborhood SES and Sorting

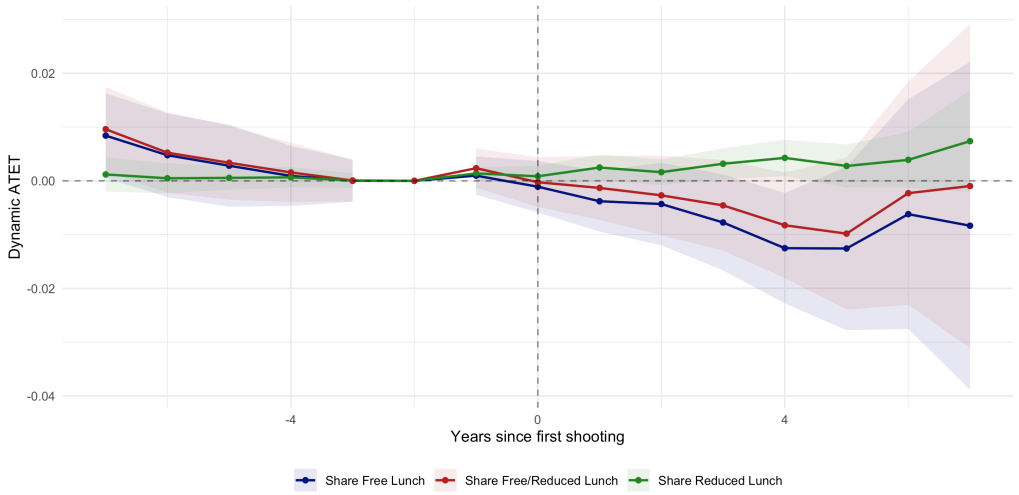
Does the increase in the share of poor students at the school level carry over to the district level? Here, we use the three available proxies for being poor: being a recipient of Reduced Lunch, a recipient of Free lunch, or their combined shares. Recall that free lunch eligibility corresponds to household incomes at or below 130% of the federal poverty level, and reduced-price lunch to 130–185%. In Figure 19, I estimate the effects of shootings separately for each of the three outcomes. We see that there is an increase in the share of Reduced Lunch students in the shooting districts, but a decrease in the share of Free Lunch recipients.

Given that students exit shooting schools, do families sort by socioeconomic status into other schools in the district? Figure 20 plots the Exposure Index for Free Lunch and Non-Free Lunch Recipients by length of exposure to a shooting. The index measures the exposure that Free Lunch and Non-Free Lunch Recipients have to Free Lunch Recipients, with higher values indicating more segregation. Districts that experience a shooting see an increase in between-school SES segregation relative to control districts—although not by much.

Figure 19. District-Level Shares of Proxy for Poor Students



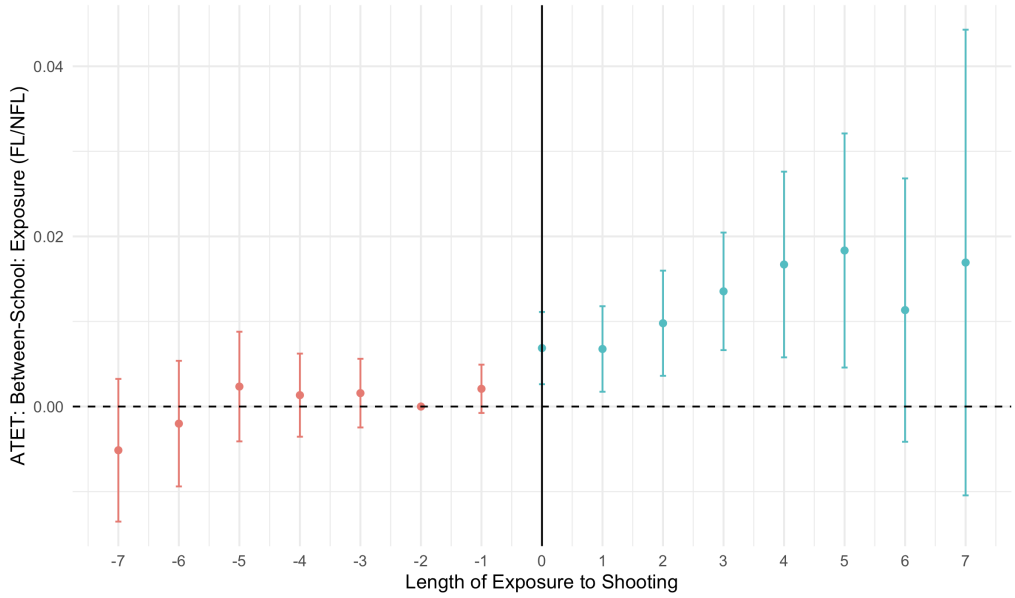
(a) Overall ATT



(b) Dynamic ATT

Notes: Difference-in-differences estimates for proxies of poor students: Reduced Lunch, Free Lunch, and Reduced or Free Lunch Recipients.

Figure 20. Exposure Index: Free Lunch and Non-Free Lunch Recipients



Notes: Exposure Index for Free Lunch and Non-Free Lunch Recipients by relative year since shooting. This measures the exposure that Free Lunch and Non-Free lunch recipients have to Free Lunch recipients. A value of 0 implies no segregation (there are equal shares of the two groups) while a value of 1 implies complete segregation (no non-Free Lunch student attends a school with a Free Lunch student).

7 Conclusion

This paper set out to understand some of the aftermath of school shootings, specifically regarding student mobility and socioeconomic sorting.

There is a clear divergence in enrollment trends post-shooting. Shooting schools had for years maintained a steady size of the student body, but experienced an average decrease of around 4% following a shooting. The decrease is immediate and growing over time, with fatal shootings driving declines of up to 10% at longer horizons. This is in line with previous school shootings papers that found similar estimates. The share of poor students at a shooting school increases post-shooting, implying that the exodus is non-random as higher-income families leave at greater rates.

The mobility response is not uniform. It is shaped by institutional context. In states where intradistrict open enrollment is permitted, the outflow of students is stronger, consistent with school choice policies facilitating mobility responses. One can remain agnostic about whether this is desirable or not. Similarly, districts with more same-level schools see larger enrollment declines. Mechanically, families need viable alternatives to act on their preference to leave. That is unless a school shooting is seen as the event that triggers a response to an otherwise violent environment: schools in the most violent counties see the largest enrollment declines, suggesting that a shooting may serve as a "last straw" rather than the effect being largest where shootings are most unexpected.

The next question is, do students with different socioeconomic backgrounds sort into different schools post-shooting? At the district level, the share of Reduced Lunch students increases while the share of Free Lunch recipients decreases, hinting at nuanced compositional shifts. The share of the poorest students (those who are Free Lunch Eligible) decreases by 0.5% while the share of the next poorest students (those who are Reduced Lunch Eligible) increases by 0.25%. This district-wide increase is small but persistent up to six years post-shooting. Between-school SES segregation sees modest increases following a shooting, indicating that the differential mobility contributes to socioeconomic stratification across schools within a district.

These results are robust across treatment radii from 250m to 5km. At very large radii, the effect reverses sign, consistent with a reallocation story where distant schools absorb the students that nearby schools lose. Shooting schools also close at higher rates, roughly 1 in 66 within five years, implying survivorship bias that would make the estimated enrollment declines a lower bound of the true effect.

Taken together, these findings carry a policy tension: open enrollment policies, while intended to expand opportunity, may also accelerate the compositional deterioration of schools hit by violence. There are yet more mechanisms driving the non-random out-mobility of students that should be explored. Every shooting is unique and traumatizing to that school. There is great heterogeneity in the types of shootings that could be explored in the hopes of assessing the damage and the potential for recovery by different types of shootings.

The issue of neighborhood violence in general and school shootings in particular should be of utmost importance to the research community and to policy makers. This paper contributes by documenting how school shootings reshape the composition of affected schools and districts,

adding one piece to the puzzle that is the aftermath of gun violence in schools.

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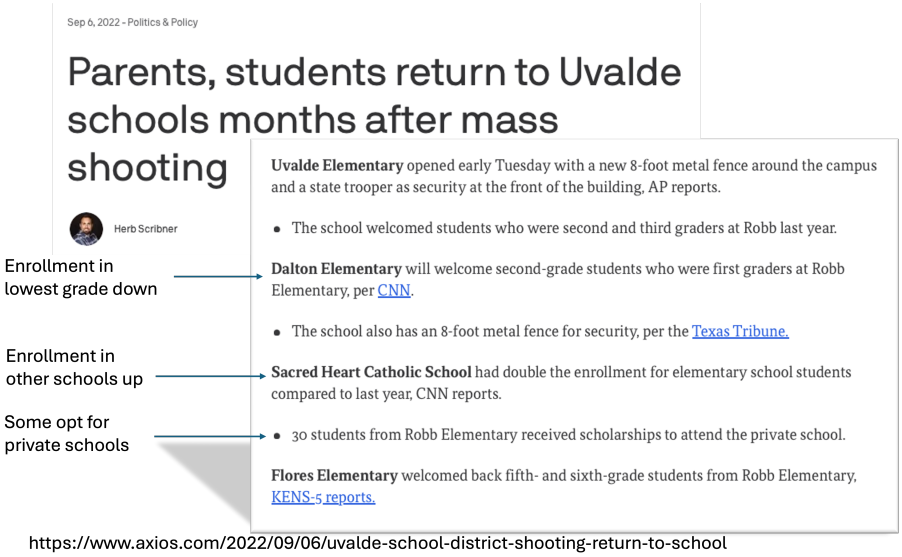
Appendix

Figure .1. News Reporting on Parental Responses and Nearby School Responses to Shooting

https://www.nbcbayarea.com/news/local/east-bay/parents-leaders-demand-change-oakland-school-shooting/3018366/

https://www.axios.com/2022/09/06/uvalde-school-district-shooting-return-to-school



https://www.axios.com/2022/09/06/uvalde-school-district-shooting-return-to-school

Notes: News reporting on parental responses to school shootings as well as enrollment spillovers to other schools.

A School Shootings databases

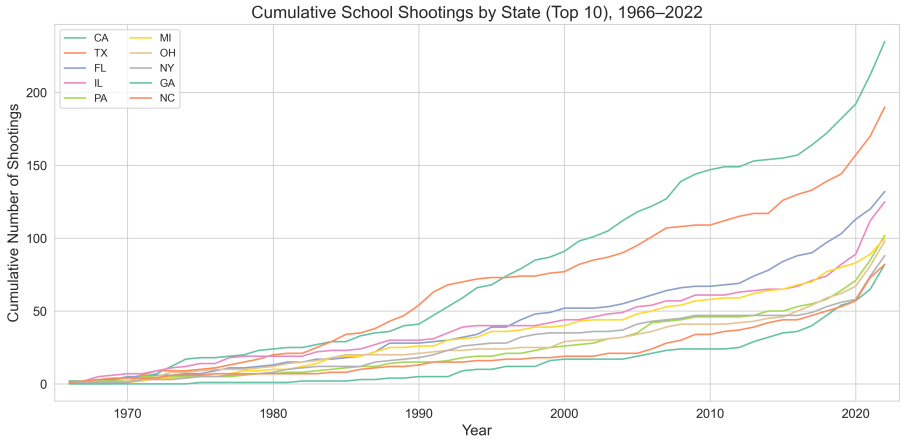
Table A.1. School Shootings Datasets Overview

Dataset	Period	# Events	Inclusion Criteria	Based on	Used by	Last accessed
Public K–12 School Shooting Database (K12SSD)	1970–2024	2,675	Gun brandished or gun fired or bullet hits school property irrespective of time or day.	Government agencies, media or advocacy groups, websites and blogs	(See CHDS)	February 9th 2024 (https://k12ssdb.org)
US School Gun Violence	1990–2013	381	firearm being discharged (inc. accidental), on a school campus, involves students or school employees, either as perpetrators, bystanders or victims.	Shultz dataset; Slate, Brady Campaign to Prevent Gun Violence, and Wikipedia.	Pah et al. 2017	October 23rd 2023 (https://amaral.northwestern.edu/resources/data-sets/gun-violence-us-schools)
Washington Post School Shootings Database	1999–2023	387	On campus immediately before/during/just after classes. Excludes: shootings at after-hours events, accidental discharges that don't injure anyone other than the perpetrator, suicides that occurred privately or posed no threat to other children	News articles, open-source databases, law enforcement reports, school websites, calls to schools & police	Cabral et al. 2021; Gujral et al. 2023; Livingston et al. 2019; Reeping et al. 2022; Rossin-Slater et al. 2020; Yang and Gopalan 2023	October 31st 2023 (https://github.com/washingtonpost/data-school-shootings)
CHDS K-12 School Shooting Database	1970–2022	2,070	Like K12SSD but not as up-to-date	(See K12SSD)	Cabral et al. 2021; Deb and Gangaram 2024; Gammell et al. 2022; Gujral et al. 2023; Hodges et al. 2023; Levine and McKnight 2020, 2021; Sezer 2022	January 23rd 2024 (www.chds.us/ssdb)
The American School Shooting Study (TASS)	1990–2016	652	Resulted in criminal justice response, firearm discharged, at K-12 school, on school grounds, & injured or killed at least one person	.	Freilich et al. 2022	N/A More information at (https://rockinst.org/wp-content/uploads/2022/08/Overview-American-School-Shooting-pdf)
School-Associated Violent Death	1992–2020	2,031	Homicide, suicide, or legal intervention death, fatal injury occuring on school campus, while victim to/from regular sessions or official school-sponsored event	.	Beland and Kim 2016	N/A (https://nces.ed.gov/programs/coe/indicator/a01/violent-deaths-and-shootings)
Stanford Mass Shootings of America	1966–2022	335	In an elementary, middle or high school, with 3+ victims excl. the perpetrator	Online reporting sources (at least 3 corrobarting)	Muñoz-Morales and Singh 2023	January 23rd 2024 (https://github.com/StanfordGeospatialCenter/MSA)

Notes: Summary of publically available datasets relating to school shootings.

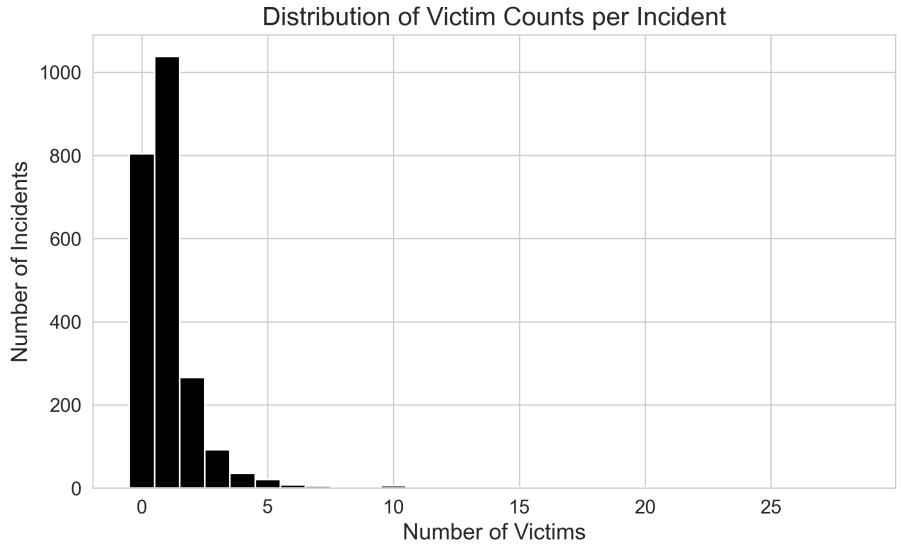
B Descriptive Statistics on Shootings

Figure B.1. Top 10 States with Shootings



Notes: Line plot of cumulative number of shootings per year and state for the top 10 states with most shootings in 2022.

Figure B.2. Distribution of Number of Victims



Notes: Bar plot with count of number of victims.

C Further notes on mobility

At the student-year level, Cabral et al. (2021) find that the probability of an individual student leaving the school is not statistically significantly different between students at shooting schools and control schools. I write this as follows:

$$\Pr\left(\text{Leave}_{i,t}|\text{ShootingSchool}_{s,t-1}=1\right) \approx \Pr\left(\text{Leave}_{i,t}|\text{ShootingSchool}_{s,t-1}=0\right) \tag{3}$$

First, I argue that since Texas has no state-wide mandatory open enrollment laws, the probability of any student switching schools is low to begin with (i.e. $\Pr\left(\text{Leave}_{it}\right)=0+\varepsilon$ for some arbitrarily small ε). The conditional probabilities of switching school given a school shooting would have to be very large to yield any significant difference.

Second, I compare this to school-year level estimates of enrollment. For example, Abouk and Adams (2013) estimates something that is similar to the following:

$$\text{E}(\Delta\text{Enrollment}_{s,t}|\text{ShootingSchool}_{s,t-1}=1) > \text{E}(\Delta\text{Enrollment}_{s,t}|\text{ShootingSchool}_{s,t-1}=0) \tag{4}$$

Specifically, I argue that Equation 3 does not imply an equality in Equation 4.

We can divide the expected change in enrollment into stayers, leavers and joiners.

$$\text{E}(\Delta\text{Enrollment}_{s,t}) = \text{E}\left(\sum_{i\in s}\text{Stayers}_{i,t} - \sum_{i\in s}\text{Leavers}_{i,t} + \sum_{i\in s}\text{Joiners}_{i,t}\right) \tag{5}$$

If we redo the Cabral et al. (2021) analysis at the school-year level, we might well find that there is no difference in the probability of students switching schools, but still find an overall negative effect on enrollment.

$$\text{E}(\Delta\text{Enrollment}_{s,t}|\text{ShootingSchool}_{s,t-1}=1) - \text{E}(\Delta\text{Enrollment}_{s,t}|\text{ShootingSchool}_{s,t-1}=0) \tag{6}$$

$$\begin{aligned}&\text{E}\left(\sum_{i\in s}\text{Stayers}_{i,t} - \sum_{i\in s}\text{Leavers}_{i,t} + \sum_{i\in s}\text{Joiners}_{i,t}|\text{ShootingSchool}_{s,t-1}=1\right)\\ &-\text{E}\left(\sum_{i\in s}\text{Stayers}_{i,t} - \sum_{i\in s}\text{Leavers}_{i,t} + \sum_{i\in s}\text{Joiners}_{i,t}|\text{ShootingSchool}_{s,t-1}=0\right)\end{aligned} \tag{7}$$

$$\begin{aligned}&\text{E}\left(\sum_{i\in s}\text{Stayers}_{i,t}|\text{ShootingSchool}_{s,t-1}=1 - \sum_{i\in s}\text{Stayers}_{i,t}|\text{ShootingSchool}_{s,t-1}=0\right)\\ &-\text{E}\left(\sum_{i\in s}\text{Leavers}_{i,t}|\text{ShootingSchool}_{s,t-1}=1 - \sum_{i\in s}\text{Leavers}_{i,t}|\text{ShootingSchool}_{s,t-1}=0\right)\\ &+\text{E}\left(\sum_{i\in s}\text{Joiners}_{i,t}\sum_{i\in s}|\text{ShootingSchool}_{s,t-1}=1 - \sum_{i\in s}\text{Joiners}_{i,t}|\text{ShootingSchool}_{s,t-1}=0\right)\end{aligned} \tag{8}$$

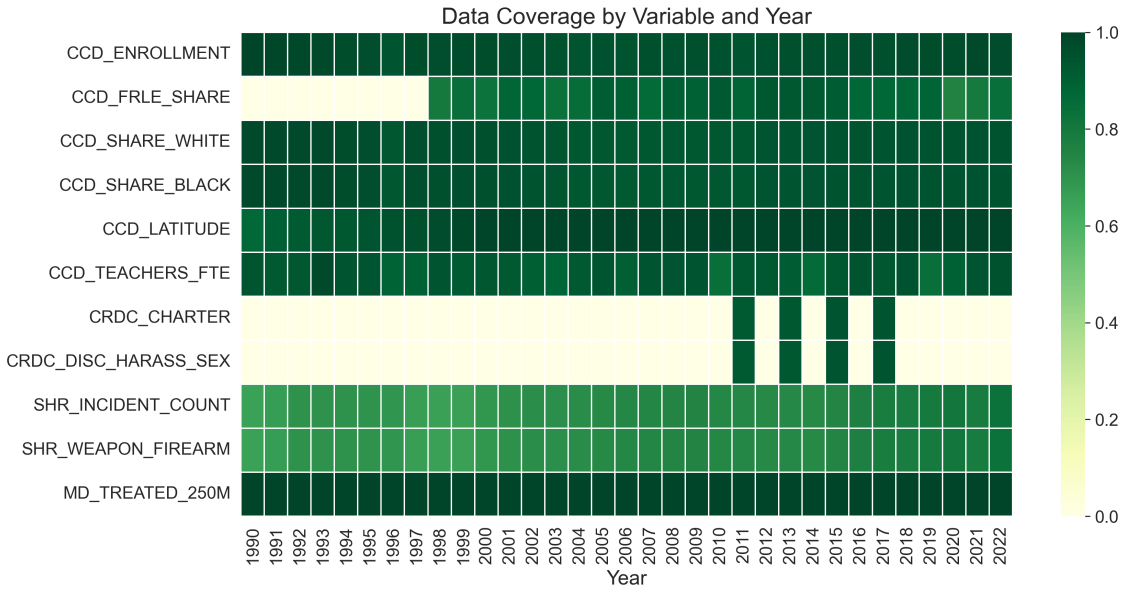
Looking at the last equation, it is possible to have a negative effect on total enrollment due to school shootings, while not having any difference in the probability of switching schools or

staying in schools, so long as there is a difference in the probability of joining a school that has experienced a shooting versus a comparable one that has not.

This is in line with anecdotal evidence of fewer incoming students in the lower grades of shooting schools.

D Data restrictions

Figure D.1. Missing data



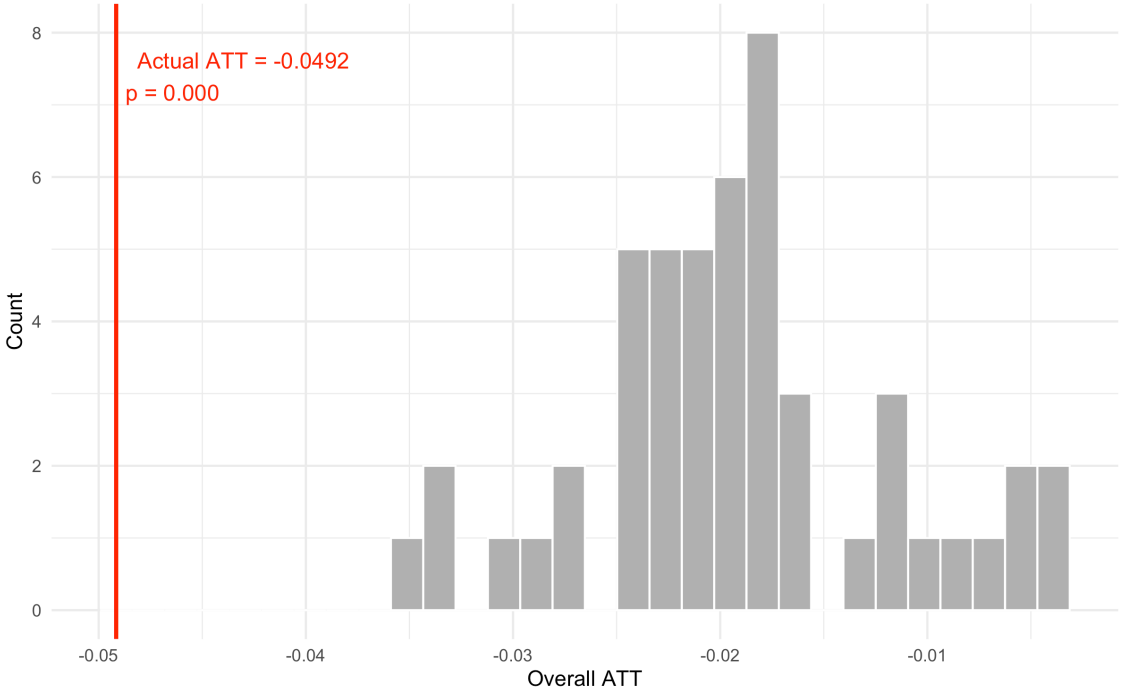
Notes: Plot of data completeness over time. The variable name prefix reflects the data source. Prefix "MD_" is author generated.

E Robustness Checks

Randomized Timing Assignments The Staggered Difference-in-Differences with the not-yet-treated and never-treated as a control group relies on the assumption of exogeneous timing of the shootings. If there were no true effect of shooting on log enrollment, then a reshuffling of treatment years among the treated units would produce similar ATT estimates, and our ATT would fall well within the range of ATTs produced by this reshuffling.

To test this, I reshuffled the treatment timing among the 1,700 treated units and reran my main analysis. Our actual overall ATT was -0.049. The exercise results in a two-sided permutation p-value of 0.000, indicating that if the true ATT were that of the mean ATT of our runs, and we received an ATT of -0.049, the likelihood of such an outlier value would be miniscule.

Figure E.1. Permutation Distribution of ATT



Notes: Plot overall ATT from 50 staggered Difference-in-Differences runs. The reference line is