Local Violence and School Attendance in Chicago

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Abstract

In the first chapter I examine how local homicides affect both attendance and enrollment in elementary schools. I use publicly available data from Chicago Public Schools and the City of Chicago. To estimate treatment effects, I use a difference-in-differences estimator that allows for both heterogeneity in treatment-timing and intensity. I find no effect at lower homicide levels, but for two homicides per year in a school's catchment area I find an approximately 0.33 percentage point increase in average daily attendance in the initial year the homicides occurred. This is a relatively large effect, about 14.7% of the standard deviation for all elementary schools. For three or more homicides I find a relatively smaller positive but insignificant effect. There also appears to be a relatively small and insignificant decrease in enrollment at the bottom of the homicide distribution, but towards the top the decrease in enrollment becomes larger in magnitude and significant. These enrollment estimates are not robust to the inclusion of pre-treatment homicides as a control, however, but the point estimates do remain negative. With the inclusion of controls, the attendance results remain similar and the estimates for the very top of the homicide distribution become significant. Overall, these findings suggest that the effect of homicides on attendance may behave in a counter-intuitive way, particularly at the levels of homicides studied in this chapter.

Keywords: Schooling, attendance, local violence, homicides, murders, crime, continuous difference-in-differences, DiD.

JEL Codes: K40 - Legal Procedure, The Legal System, and Illegal Behavior; I21 - Analysis of Education.

1 Introduction

Local, or neighborhood, violence has many negative effects on children living in the surrounding community.[1] In this paper, I will look at a specific type of violence, namely homicides, affect elementary school attendance and enrollment. Often when people think of violence and its effects, people focus more on its direct effects. Children dying too soon and others, often similar in age, being sent to jail. Local violence has other secondary effects as well. For example, the psychological effects it has on people living in a dangerous community or the cognitive effects it has on children.

Violence also has the potential to affect human capital in other ways as well, for example through attendance and enrollment. Attendance is an important contributor to human capital development. If a student is absent, they are likely not learning at the rate they would have had they attended. This will be reflected later in life through, among other things, lower wages. Local violence has the potential to change attendance by altering the payoffs to schooling by increasing or decreasing the marginal cost of attendance. For example, if the journey to school is dangerous or if the school itself is unsafe, then staying home may be the safest option for the student, thus reducing their attendance. The opposite may also happen, however. School may be seen as a relatively safer option, perhaps because the student does not have access to their house during the day. As a result, there is ambiguity in the direction of the effect that local violence has on attendance and this becomes an empirical question.

The other outcome that I will look at, enrollment, is often used to capture changes in human capital investment. Because I focus on elementary school students it is unlikely that this is capturing changes in human capital. Instead, any causal differences are likely to be due to students moving between schools, for example due to a student's parents sending them to a safer school in a different neighborhood. This may be more of a medium-run effect; parents will have to find a new school and go through the enrollment process or potentially move neighborhoods which could take time. As a result, putting their child in the safest place possible, be it at home or in school, may be the short-run effect with the final goal of moving their child to a safer neighborhood if/when this is possible.

A collection of papers, which I discuss in much greater detail in the section that follows, look at these secondary effects. These papers, however, often do not make causal claims and simply report correlations. Of the papers that do make causal claims, the focus is often not on attendance. Instead, they usually focus on test scores, another important outcome. In this paper, I seek to contribute to the literature studying an understudied albeit equally important outcome, attendance. Particularly, I will look at how homicides affect elementary school attendance and enrollment in Chicago.[2] I present a simple conceptual framework below to show why the direction of the effect is ambiguous and depends upon how students perceive their safety both to/from and at school.

In this chapter I focus on Chicago because of the reputation that the city has for violence.

[1]See for example Boynton-Jarrett et al. (2013) and Sharkey (2010)

[2]I use the term homicide to mean an event where at least one person was killed. I will use the term homicide victims to refer to the number of people killed.

It is often perceived within the US as one of the most violent cities (Monkovic and Asher 2021). Although it is not the most violent city in the US in per capita terms, there are an average of 441 homicides per year during the period that I look at below. Because of this, it seems important to understand the full effects that this level of violence has on schoolchildren residing in Chicago.

For this chapter I use publicly available average daily attendance data and census day enrollment data from Chicago Public Schools (CPS) by grade, school, and school year.[3] I then combine this with school catchment area data from the City of Chicago, which allows me to assign local homicide data, also from the City of Chicago, to a given school. To estimate these effects, I focus on the subgroup of elementary schools that experienced zero homicides in the first period of the data that I have, the 2006/07 school year, to create a pre-treatment period where both treated and control groups experienced no homicides in their local area. I then use a difference-in-differences (DiD) estimator that allows for both variation in treatment-timing and some heterogeneity in the initial number of homicides a school was exposed to in order to estimate local treatment effects for switchers of a given dosage.[4]

I find that for the switcher elementary schools, treated schools that went from zero to some positive number of homicides, going from zero to one homicide does not have much of an effect on grade level average daily attendance. I also find that switcher schools that went from zero to two homicides experienced in an increase in grade level average daily attendance by approximately 0.33 percentage points in the initial year of exposure. Additionally, I find that grade level enrollment for elementary school switchers that experienced two or more homicides falls by between approximately 1.68 and 1.79 students in the initial year of the homicides, although this becomes smaller in magnitude and insignificant when controlling for the sum of pre-treatment homicides. Also, while controlling for pre-treatment homicides, the attendance estimates for two or more homicides become slightly larger in magnitude and the point estimate for the group of schools that experience three or more homicides becomes significant at the 10% level. Overall, homicides appear to have a positive effect on attendance, at least for the data and time period observed, and potentially a negative effect on enrollment.

This paper has several limits worth mentioning here. First, it relies on the identifying assumptions discussed below. Second, my main identification strategy focuses on a subset of elementary schools which may or may not behave similarly to the remaining sample of elementary schools. Third, this paper only looks at the city of Chicago and even if the subset of data that I look at is representative of Chicago, it may not be that these results generalize to the rest of the US. Even with these limits in mind, I believe that it contributes an important missing piece to the existing literature on local violence and schooling outcomes.

In the sections of the paper that follow I will discuss the related literature, focusing particularly on papers that also look at attendance. I will then present a theoretical framework and discuss how the effect of homicides on attendance is *a priori* ambiguous. After this,

[3]Census day for Chicago is the twentieth day of the school year.

[4]I limit my estimation of treatment effects to the initial year a school experienced some positive number of homicides to simplify estimation. See the methodology section for further details.

I will present some background information on Chicago, both the city and the educational system. I will also discuss the data that I use and present summary statistics for the subset of the data that I use below. Then I will present my estimation results along with robustness checks. Finally, I will conclude the paper with a discussion of my findings.

2 Literature Review

In this section I will discuss two general strands of literature related to this paper. The first of these strands looks at how violence, in particular homicides, affects children and schools. There are many papers on how violence affects children and teens and they look at a variety of outcomes relating to health, psychology, and education. I further separate these papers in this strand into three subcategories based upon the type of violence the children are exposed to: conflict, school shootings, and local/neighborhood violence, of which I will focus particularly on the papers that look at attendance and absenteeism. The second strand of literature that I will discuss are papers analyzing the effect of the Safe Passage Program (SPP) on crime and attendance.

All of these papers face a similar identification issues to this one. Local homicides are not randomly assigned to areas and are likely correlated with many different unobservable factors, such as poverty its determinants. A majority of the papers that I will discuss below attempt to estimate causal effects, however, by exploiting different types of variation under different assumptions. The most common estimator used in these papers is the Two-Way Fixed Effects (TWFE) estimator, which suffers from several issues when the explanatory variable of interest is continuous. I discuss the problems with this estimator further in the methodology section below.

2.1 Violence and Student Outcomes

The first group of papers that I will discuss are the papers that look at the educational outcomes of students in conflict zones. These papers cover a broad range of conflicts such as the civil war in Tajikistan (Shemyakina 2011), cartel violence in Mexico (Brown and Velásquez 2017), conflict in Israel and Palestine (Brück et al. 2019), and conflict between gangs in Brazil (Monteiro and Rocha 2017). They all focus on similar student outcomes, primarily test scores, but Brown and Velásquez (2017) and Shemyakina (2011) look at how conflict affected school completion. In all of the papers the authors find that violence has a negative effect on the student outcomes that they studied. The most common identification strategy used in these papers is a DiD, although some of these papers do not explicitly state this.

The second group of papers that I will discuss looks at how school shootings affect a variety of student and school outcomes. School shootings are potentially different than general neighborhood violence because the violence occurs at the school. If a school is perceived as a safe place by students, a school shooting may reduce or eliminate this feeling of safety. As a result, students may believe that their alternative options are relatively safer and adjust their behavior accordingly.

The literature on school shootings focuses on a much broader range of outcomes as well. For example, the effect that a school shooting has on staffing and retention change (Cabral et al. 2020) and how school finances and student composition change due to the shooting (Yang and Gopalan 2023). Other papers look at student related outcomes, such as test scores (Levine and McKnight 2020; Poutvaara and Ropponen 2010) and private school enrollment (Abouk and Adams 2013). Similar to the other papers on conflict, these papers on school shootings are in agreement that they have a negative effect on student outcomes.

Next, I will discuss three papers in greater detail because they discuss, among other outcomes, attendance changes arising from a school shooting. The first of these papers is by Beland and Kim (2016), which looks at how school shootings resulting in death affected attendance rates in addition to enrollment and test scores of high school students. To estimate these effects, the authors use a simple DiD with two groups and two time periods. Aside from enrollment and test scores both decreasing, the authors do not find any significant effect of the school shootings on attendance. This paper, and the next paper that I will discuss, both suffer from a similar problem by not accounting for the staggered nature of school shootings. This could potentially bias their estimates, a problem which I will discuss in the methodology section below.

The second paper that I will discuss by Cabral et al. (2020) looks at how school shootings affected both shorter and longer run outcomes. Like the previous paper, the authors in this paper estimate effects using DiD, either in the form of simple DiD or event studies. For the longer run outcomes, the authors found school shootings negatively affected high school, college, and earnings outcomes. For the shorter run outcomes, which are more related to this paper, the authors found that the school shootings resulted in increased absenteeism. As I mentioned above, this paper does not account for the staggered nature of school shootings and implicitly assumes that treatment effects are constant across treatment-timing cohorts. If this is not the case, these estimates will be biased.

The third and final paper that I will discuss on school shootings is by Munoz (2023), who focuses on a single high school shooting in Florida. Different to the two previous papers discussed, the author in this paper uses a synthetic control estimator to estimate treatment effects on a variety of student outcomes. Also different to the other papers, this author did not find any negative effect on the outcomes that he examined. For example, no negative effect on attendance, decreased chronic absenteeism, and increased enrollment. The author suggests these findings, which are very much in contrast to the other papers on school shootings, may be due to a policy implemented after the school shooting which focused on increasing school safety. In addition to this policy, which may conceal some the effects of the school shooting, several of the pre-treatment synthetic control estimates diverge from the actual pre-treatment outcome trends, highlighting a potentially serious issue that synthetic control group may not be constructing a good counterfactual to estimate treatment effects with.

Finally, I will discuss third and final group of papers on violence and student outcomes. This group of papers looks at how children and teens are affected by violence when it occurs in their local community. Like the other groups of papers, these focus on a variety of outcomes. The majority focus on test scores (Aizer 2008; Burdick-Will 2013, 2016; Facchetti

2021; Sharkey 2010), but other papers look at outcomes ranging from health and behavior (Boynton-Jarrett et al. 2013; Cooley-Quille et al. 1995; Hurt et al. 2001), IQ (Delaney-Black et al. 2002), and high school completion and college attendance (Grogger 1997). Similar to school shootings, the findings in these papers are largely in agreement that local violence is detrimental to children and teens, with students experiencing lower test scores, worse mental health outcomes, and reduced college enrollment.

Within this final group of papers, several look at how attendance is affected by violence. Some of these papers don't make causal claims about the effect and simply measure correlation (N. K. Bowen and G. L. Bowen 1999; Burdick-Will 2017; Burdick-Will et al. 2019; Hurt et al. 2001). These papers find a negative correlation between both violence and attendance. Other papers, three of which I will discuss in the paragraphs that follow, do estimate causal effects.

The first paper by Ang (2020) examines how police officer killings of civilians in Los Angeles affects a variety of high school students' outcomes, one of which is absenteeism. Absenteeism is not the focus of this paper, but the author does provide some convincing graphical evidence showing a large increase in absenteeism for students living within 0.5 miles of a shooting while there is no such spike for students living farther away. It is possible that the public responds differently to police killings than violence committed by civilians in a neighborhood. This could be due to certain police killings receiving significant amounts of police coverage, although the author argues that this is largely not the case, at least for the time period they covered in Los Angeles. Beyond this, it is also possible that communities respond differently to police killings because these acts are committed by an individual they entrust with their protection. The killing of a civilian by a police officer could potentially be viewed as a violation of this trust as opposed to when the killing is done by another civilian who does not have the same duty to protect the public.

Like the previous paper, the second which I will discuss by Sharkey et al. (2014) does not focus on absenteeism or attendance. Instead, they look at how test scores change due to local homicides in New York City by exploiting randomness in the timing of homicides. To do so, the authors compare outcomes of students exposed to a homicide in the week before an exam to students exposed to a homicide in the week after. In addition to looking at test scores, Sharkey et al. also look at whether homicides changed the probability of a student taking the exam, which is essentially exam day attendance. The authors find no effect on the probability of taking the exam, but this could potentially be due to the importance of the exam. It seems possible that other days around the exam day may have experienced a change in attendance, but this measure of attendance is too narrowly defined to detect any such changes.

The last paper that I will discuss by Koppensteiner and Menezes (2021) is the most similar to mine in this collection of papers. In this paper, the authors examine how local violence, which they measure as homicides, affects attendance, in addition to a range of other student outcomes, for students in Brazil. Like the previous papers I have discussed, attendance is not the focus of this paper, but it is presented as a robustness check. To identify the effect of violence on attendance the authors us a TWFE estimator. Koppensteiner and Menezes find that an additional homicide within a 25-meter radius circle around a school reduces attendance by around 1%. There are several shortcomings with this paper. First, a 25-meter radius circle around a school is small and may not capture the full effect of a homicide because it is so small. Second, identification with the TWFE requires a strong parallel trends assumption, which I will discuss in greater detail in my methodology section. The third and final shortcoming is the age of the students and potential for reverse causality. The sample of students that the authors use for this analysis includes the equivalent to fifth, ninth, and twelfth graders in terms of length of schooling. The ninth and twelfth graders in this sample have the most potential for reverse causality because it seems plausible that older students are more likely engage in violent and criminal behavior than the younger students. If these older students are missing school and engaging in criminal activity, such as homicides, the estimates presented in this paper would be biased. This is something that I deal with in this paper as well, which I will discuss further below, by focusing primarily on elementary school students.

2.2 Safe Passage Program (SPP)

In this subsection I will discuss the second strand of related literature that looks at the introduction of the SPP in Chicago. The SPP is an initiative that places paid staff along designated travel routes to and from schools in Chicago with the intent to make these routes safer for children as they commute to and from school. Most of the papers looking at the SPP focus on how various types of crime were affected by its introduction (Curran 2018; Gonzalez and Komisarow 2020; Sanfelice 2019). These papers are largely in agreement that the SPP reduced crime during the hours these travel routes were staffed, but there are some minor differences in findings regarding spillover effects and whether it caused crime to move to hours outside of the SPP.

I will discuss two papers on the SPP by Komisarow and Gonzalez (2023) and McMillen et al. (2019) in greater detail because they both look at how attendance was affected by the policy. The papers are similar in their identification strategy, both using DiD estimators. They are also similar in their findings, with McMillen et al. finding attendance rate growth increased due to the program and Komisarow and Gonzalez finding it reduced absence rates. There is some disagreement about the magnitude of the effects, but overall their findings both point in a similar direction.

There are also several shortcomings with these papers. McMillen et al. focuses on treated high schools and assigns elementary and middle schools to the control group. As Komisarow and Gonzalez discuss, this is problematic because of a nearly concurrent policy aimed at increasing high school attendance. Further, both of these papers do not account for the staggered adoption of the policy. Although, this is more likely to affect the paper by McMillen et al. because of greater variation in treatment-timing for high schools.

I believe that it is worth emphasizing the difference between the SPP papers and mine. Although they are related, due to similar data and outcome variables, I focus on changes in attendance due to local homicides. The papers looking at the SPP are focusing on changes in attendance that arise due to a policy. There is a subtle difference between the two, but the questions we are attempting to answer are different. The SPP papers would capture all changes that arise due to changes in local safety, not just homicides but also other types of violence as well. I focus specifically on the effect that local homicides have on attendance and enrollment.

To conclude this section, nearly all of the papers that I have discussed in the previous subsection find that violence has a negative effect on children and teens. Most of the papers focus on other outcomes, but there are a few that look at attendance. Of those that do look at attendance, it is commonly estimated as a simple correlation, foregoing any attempt at identifying a causal effect, or it is treated as an afterthought and included for completeness as an additional table. This paper's contribution to the literature is giving attention to an understudied outcome in the context of local violence.

3 Conceptual Framework

In this section, I will present a simple model for student attendance. First, I will discuss the definitions and assumptions of the model. Then I will discuss how attendance may potentially be affected by an increase in violence, specifically, local homicides, and why this effect is *a priori* ambiguous.

Suppose that all students receive some net benefit from attending amount of school A in a given school year, where A is normalized by the length of the school year.[5] The net benefit for a given school year can be written as:

$$\pi(A) = B(A) - C(A), \tag{1}$$

where $A \in [0, 1]$. The functions $B(\cdot)$ and $C(\cdot)$ are the total benefit and total cost of schooling, respectively. Further assume that $B(\cdot)$ and $C(\cdot)$ are continuously differentiable in the interval $(0, 1)$.

The function $B(\cdot)$ captures the benefit of attending school. This is not just the academic and labor market benefits, but other benefits such as socializing with peers. Assume that for all A in $(0, 1)$

$$MB \equiv B'(A) > 0$$

and

$$B''(A) < 0,$$

so that students benefit from attending more school at a decreasing rate. As an example, for the intuition behind this assumption, imagine a student with near perfect attendance missing an additional day of school because they are sick. This is unlikely to have a large impact on their grades, future employment, etc. As a result, the change in total benefit will be relatively small. Conversely, imagine the opposite scenario for a student with poor attendance. An additional day of schooling is likely to have a much larger effect on their

[5] A can be thought of as a continuous measure of attendance that includes partial days as well so that derivatives can be taken.

total benefits. It could be the difference between having to repeat a grade or not, for example if they show up on a test day instead of missing it.

The function $C(\cdot)$ captures the total cost of attending school and is not just the monetary cost of going to school but also the mental cost of studying so many days and the opportunity cost of not being able to do other activities that the student may enjoy as well. I will also assume that for all A in $(0, 1)$

$$MC \equiv C'(A) > 0$$
$$\text{and}$$
$$C''(A) > 0,$$

so the cost of going to school increases as they attend more school, and at an increasing rate this time. Returning to the previous example, a student with near perfect attendance may face a higher cost than a student with worse attendance because the days that they would miss are very costly. For example, if these are days that a student would miss because they are sick, these days would have a very large physical and mental cost to the student. On the other hand, an additional day of schooling for a student with poor attendance is likely less costly because they are rarely there.

I will also make the additional assumptions that

$$\lim_{x \to 1^-} B'(x) < \lim_{y \to 1^-} C'(y)$$
$$\text{and}$$
$$\lim_{x \to 0^+} B'(x) > \lim_{y \to 0^+} C'(y).$$

Students maximize Equation 1, the net benefit of schooling, by selecting a level of attendance for a given school year

$$\max_{A \in (0,1)} \pi(A).$$

First order conditions imply that at the maximum, the attendance will be such that the marginal benefit is equal to the marginal cost

$$MB(A) = MC(A).$$

Therefore, for a given student in a given school year the optimal attendance for them is

$$A^* \equiv \operatorname*{argmax}_{A \in (0,1)} \pi(A).$$

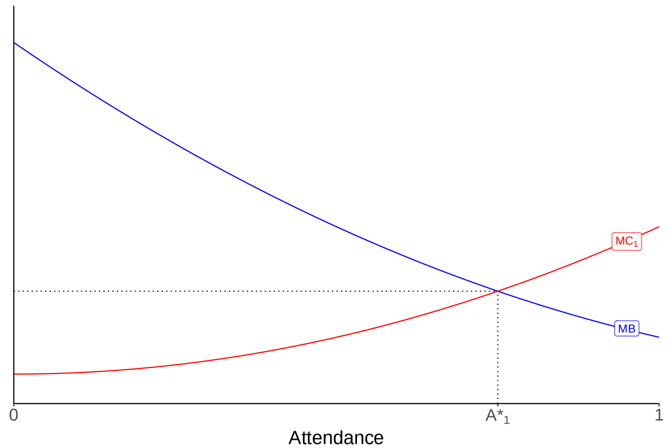
Also, for simplicity, assume that $\pi(A^*) > \pi(0)$ and $\pi(A^*) > \pi(1)$ so that the solution is on the interior of $[0, 1]$ and is unique.

Figure 1 Panel a plots the marginal benefit and marginal cost curves together, where A_1^* denotes the initial optimal attendance for the given student. An increase in homicides in the area surrounding a school has several potential effects on students, the direction of which depends on how students perceive their safety both in transit to and at school compared with their alternative option. A student's alternative option could be staying at home or, if they do not have access to their house during the day, it could be something like sitting in a park or going to a shopping mall. Changes in the safety of the alternative option will be reflected in changes of the opportunity cost of the net benefit in Equation 1.

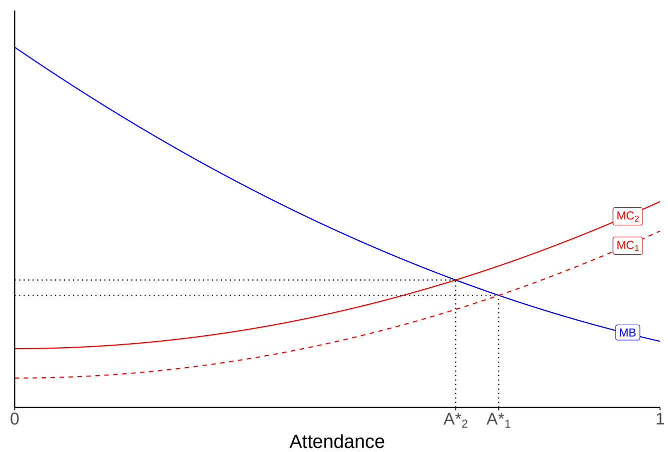
If a student believes that their journey to/from school or spending time at school has become more dangerous than their next best alternative, the marginal cost of attendance will increase at all attendance levels. This may occur if a student's alternative option is staying at home or if they view their alternative as relatively less risky following the increase in violence. Diagrammatically, this would appear as an upward shift of the marginal cost curve as in Figure 1 Panel b and the optimal attendance would decrease from A_1^* to A_2^*.

Figure 1: Optimal Attendance

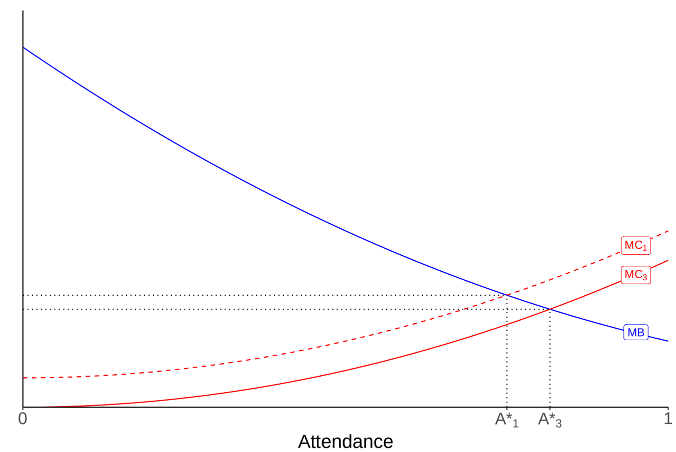
(a) Initial Attendance



(b) Upward Shift



(c) Downward Shift



This figure plots the MC and MB curves discussed above. The top panel shows the initial optimal attendance level, A_1^*. The bottom left and bottom right panel show the effect that an upward or downward shift of the MC curve has, respectively, on optimal attendance.

Alternatively, students may view school as a relatively safe place. This may occur if a student perceives their alternative as relatively risky compared with attending school. For example, if a student does not have access to their house during the day, then going to school may be the relatively safe option for them. In this scenario, the marginal cost curve would shift downwards as in Figure 1 Panel c and optimal attendance would increase from A_1^* to A_3^*.

It is also possible that students react differently to violence. For example, students may have different risk perceptions and be more or less sensitive to changes in violence. Another possibility is that differences may arise as students get older. This could happen for several reasons. It may be due to students' risk tolerance changing as they age. It could also be due to students gaining more autonomy and having different educational preferences than their parents. In this latter scenario, the marginal benefit and cost curves for a primary school student may be more reflective of their parents' attitude towards schooling, but as the student ages they become more reflective of their own views.

As I mentioned in the introduction above, changes in attendance are potentially more of a short-run solution. Parents likely react to violence by putting their child in the safest place possible until they can send them to a different school by either moving neighborhoods or enrolling them in a different school if they can do so. As a result, changes in enrollment would be more of a medium-run effect. Any changes in enrollment, beyond fluctuations due to variation in cohort size, should reflect students moving schools as they are too young to drop out. But it takes time to move neighborhoods or find a new school and enroll. Therefore, regardless of the direction of the change in attendance, any changes in enrollment will likely be negative as parents move their children away from schools in violent areas.

4 Background

In this section I will discuss some background information pertaining to Chicago. First, I will discuss background information related to the city of Chicago. Then I will similarly discuss background information about the schools in Chicago.

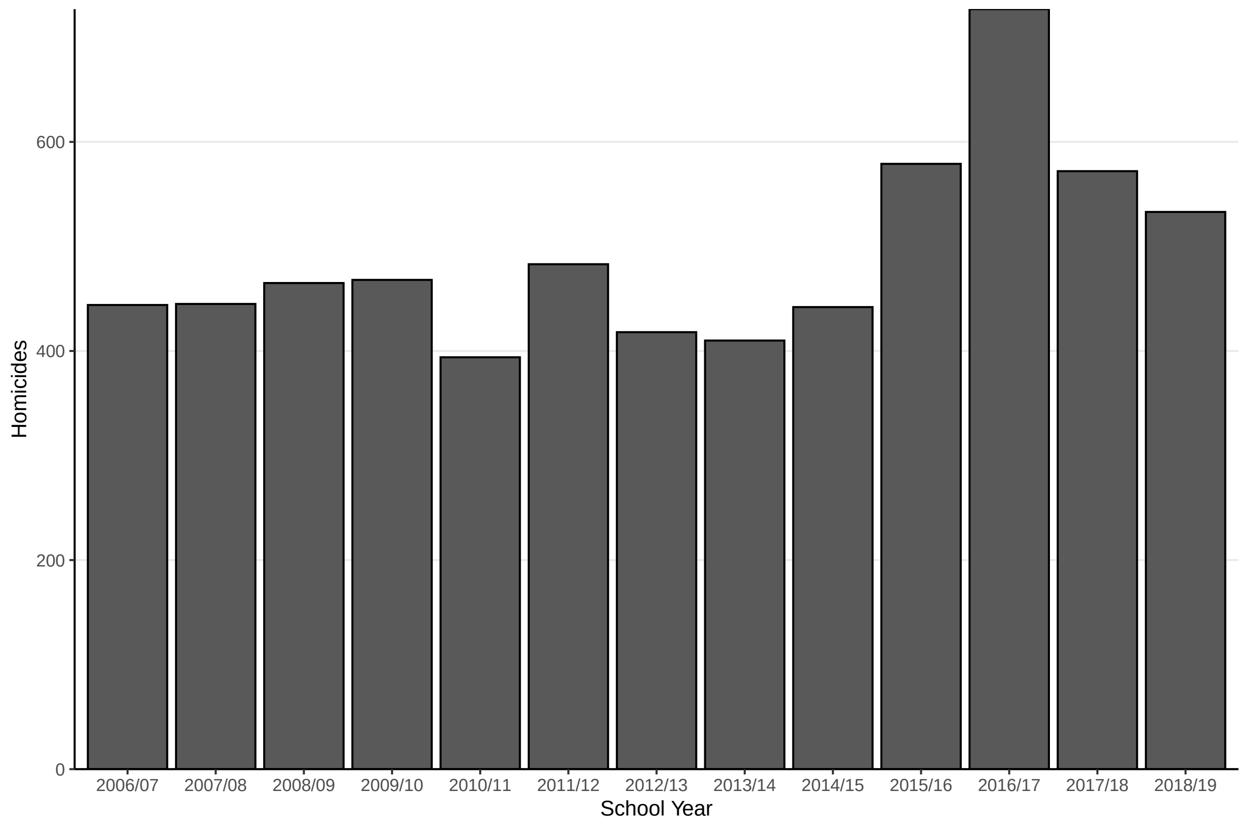
4.1 Chicago

Chicago is one of the largest cities in the US, with a population of approximately 2.7 million people spread across 227.73 square miles (589.82 square kilometers) as of the 2020 census. For comparison, New York City is roughly 300 square miles with a population of about 8.8 million people. Chicago is also a relatively diverse US city. In the 2020 census around 42.4% of residents were white, 28.8% black, 29% Latino, and 17.5% other.[6]

Chicago also has a reputation for being one of the most violent cities in the US in terms of murder rate. This reputation is largely undeserved, but it is a common belief within the

[6]These percentages do not add up to 100% because in the US Latino is an ethnicity and not a race. A person can be a white Latino, black Latino, etc.

Figure 2: Homicides by School Year



This figure plots the number of homicides by school year for the city of Chicago.

US.[7] For example, in 2020 there were approximately 778 homicides in Chicago. Based upon the population at that time the murder rate was 28.32 murders per 100,000. Detroit, a city less than one quarter the size of Chicago, saw 51.16 murders per 100,000 in the same year.

Homicides within Chicago vary largely across neighborhoods. The majority of homicides in the city are concentrated in poor, predominantly black neighborhoods. As discussed by Ander (2021) in their US Senate testimony, the four most violent police districts had a homicide rate of approximately 113.5 murders per 100,000 in 2020 while the four least violent districts had a homicide rate of about 4.6 per 100,000. Because of the large differences in homicide rates in Chicago, residents, and students, have vastly different experiences in terms of violence depending upon where they live.

Figure 2 plots the number of homicides by school year for the city of Chicago from the 2006/07 school year through to 2018/19. I exclude post-2019 years due to COVID-19. As can be seen in this figure, the number of homicides is relatively flat between 2006/07 and 2014/15, with an average of 441 per school year. Then in 2015/16, the number of homicides increases

[7] See The New York Times article (Monkovic and Asher 2021).

to 579 and peaks in 2016/17 with 727 homicides. The number of homicides then begins to fall back towards the 2006/07 to 2014/15 average in each of the subsequent years post spike.[8] In Figure 14 in the appendix, I plot the homicide data by calendar month, showing that most of the homicides happen in June, July, and August, with the peak occurring in July.[9]

4.2 Schools

CPS is one of the largest school districts in the US with an enrollment of 361,314 students in the school district for Chicago during the 2018/19 school year. These students were spread across approximately 657 schools, of which around 480 were elementary schools.[10] In the 2018/19 school year, the final year of my data, 46.7% of students were Latino, 36.6% black, and 10.5% white. Additionally, 19.2% of students were classified as English learners and 76.6% as economically disadvantaged.

There are 4 main types of elementary schools in Chicago (Chicago Public Schools 2025).[11] Neighborhood schools, which are the focus of this paper, are always open to students if they live in the catchment area. The second type of schools are selective enrollment programs, which are competitive schools that draw students from across the city. Admissions testing determines whether or not a student can attend this type of school. Third, charter schools, which operate according to their charter and admission is done via lottery. Lastly, choice programs, are schools which give students the chance to attend school outside of their zoned neighborhood school. There are several different types of choice schools, some have neighborhood boundaries and others do not. For example, magnet cluster and dual language programs typically have neighborhood boundaries. Magnet and open enrollment schools typically do not have boundaries, but open enrollment schools are neighborhood schools which allow for students in other catchment areas to apply although they serve local neighborhood children first.

Because choice, charter, and selective enrollment programs often pull students from across the city, I focus on neighborhood schools as this provides a straightforward way of assigning homicides to schools within a school catchment area and eliminates some of the heterogeneity between school types. In fact, the majority of students do not participate in school choice, which the CPS defines as not attending their zoned school. This can be seen in Figure 3 which plots the share of students that attend their local school relative to other school types. The earliest data I could find in the CPS Annual Regional Analysis was from the 2014/15

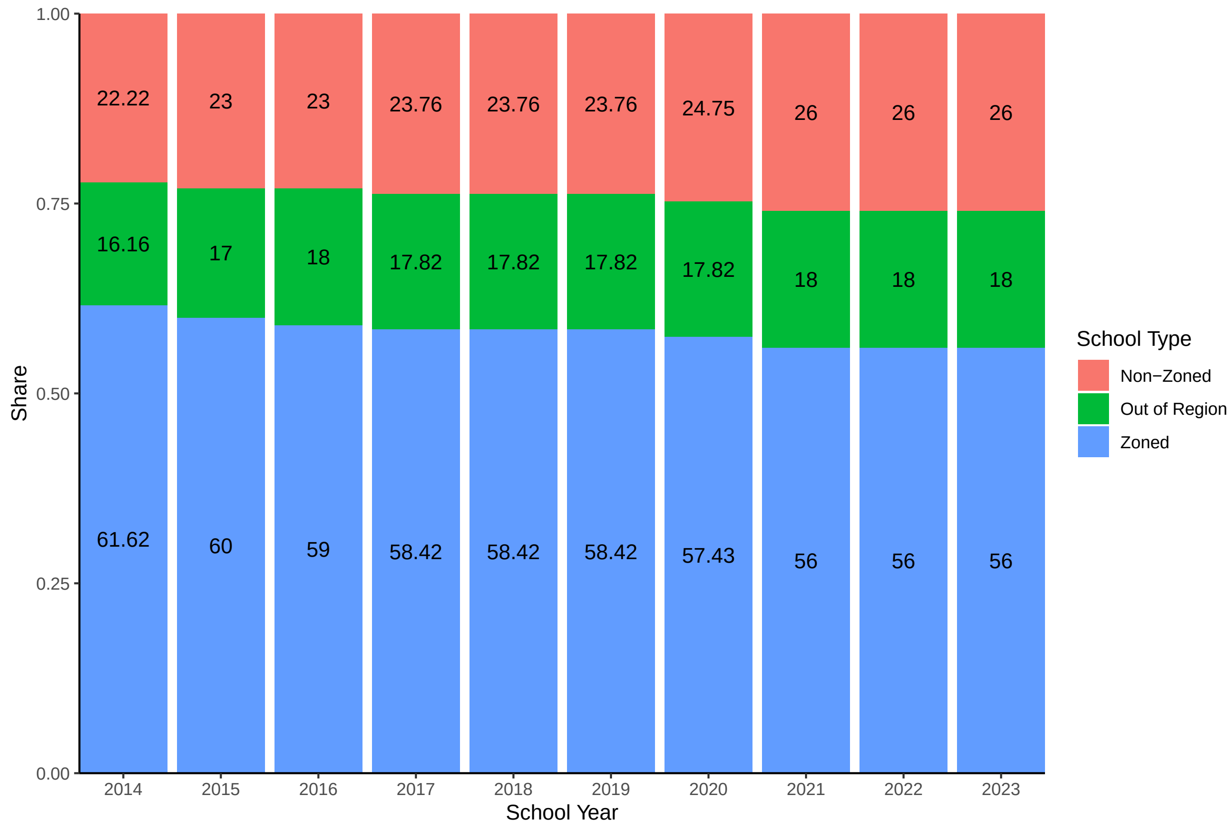
[8] The reason for this spike is not entirely known. See for example The Atlantic article interviewing the University of Chicago Crime Lab's then research director about the difficulty in attributing it to a cause (Ford 2017). Some of the possible causes mentioned in the article, which are difficult to verify, are increased gang feuds, an officer involved killing that sparked protests, or changes in police tactics.

[9] There are several studies that link warmer weather to increased violent crime. Reeping and Hemenway (2020) looks at shootings in Chicago and suggest that this could be due to more individuals being outside during hotter times of the year.

[10] CPS defines an elementary school as a school serving grades K through 8, a definition which I also use in this paper.

[11] Burdick-Will (2017) provides a brief overview of these school types.

Figure 3: School Choice by School Year



This figure plots the share of students attending a given school type by school year. The blue bars represent the share of students attending neighborhood or zoned schools, green bars represent the share of students attending schools outside their region, and the red are the share of students not attending zoned schools but that still attend school in their region. Data comes from the CPS Annual Regional Analysis. The size of each bar is reported within the bar in percent. Note there may be slight differences in the size of each group due to rounding.

school year. As can be seen from the figure, between the 2014/15 and 2023/24 school years the blue portion of the bars shows that majority of students still attend their neighborhood zoned schools. There is a slight downward trend in the share of students that do so, falling from around 62% to 56% in recent years. The green shaded area of the bars shows that students attending out of region schools has increased by nearly 2 percentage points. It is important to note here that a region is not equivalent to a catchment area boundary. Lastly, the chart also shows that the share of students not attending their local zoned school but that attend school in the same region. This is shown by the red shaded portion of the bar. This has also increased in recent years, from around 22% when the data became available in 2014/15 to 26% in 2023/24. Limits in access to these other school types, for example due to high demand or testing requirements may mean that the short-run and medium-run effects differ as I have mentioned in previous sections.

To get to school in Chicago, there are several possibilities available to students. In

Table 1: Travel to/from School

	Auto	Bus	Walk/Bike	Total
Auto	33.31	1.88	3.59	38.78
Bus	2.05	25.67	0.65	28.36
Walk/Bike	0.00	0.00	32.86	32.86
Total	35.36	27.54	37.10	100.00

[1] This table shows data on student travel to/from school in Chicago. Rows of the table indicate the method a student used to get to school, and columns indicate method of getting home. Elements of this table are reported in percent. Question specific survey weights are used, but there is a low number of underlying respondents after conditioning on Chicago.
[2] Data from the National Household Travel Survey of 2009.

addition to the usual methods of walking or being dropped off by a parent, CPS has school buses.[12] Chicago is also one of the few US cities that boasts a strong public transportation network. Table 1 shows the method of transportation to and from school for students in the Chicago area using data from the 2009 National Household Travel Survey.[13] The rows of the table show how students get to school and the columns show how students get home from school. Elements in the table are reported in percent, calculated using question specific weights from the survey. As can be seen from the table, just over 61% of students surveyed go to school either by walking, biking, or bus with the remainder going via some type of automobile. The numbers for returning from school are similar, with just under 63% not returning via automobile. If the dangerous part of attending school is the journey to/from it, then this would suggest that a large share of students are exposed to such danger because they walk or bike to school. Table 5 in the appendix shows that students who walk or bike to/from school are more likely to report that crime is a problem on the journey compared with those who take a bus or who are driven to/from school.

There were also several important policies affecting students in CPS that were enacted in or around the time period that I will look at. First, the compulsory schooling age for the state of Illinois was increased from 16 to 17 years old beginning in 2005. A second policy was the SPP, which I discussed in the literature review. This policy aimed to make transit to and from school safer by placing guards along designated roads near certain schools. In the 2013/14 school year this program was expanded to some elementary schools alongside the closure of 49 under-enrolled elementary schools. Around the same time as the SPP, the Culture of Calm was introduced with the aim to increase attendance, among other things,

[12] There has been a bus driver shortage recently, but neighborhood schools appear to be unaffected by this according to the CPS website.

[13] The lowest geography available in this data are Core Based Statistical Areas (CBSAs). After selecting the CBSA that includes Chicago, I am able to use an urban area size variable to select respondents living in a city with 1 million or more people and that has a subway or rail. This should limit respondents to those within Chicago.

for high school students. Nearly all of these policies target older students, apart from the large closing of elementary schools. Because of this, and due to potential reverse causality, which I discuss in the methodology section, I focus on elementary school students for my analysis below. I follow the CPS definition of elementary schools and include kindergarten through eighth grade, roughly students between five and fourteen years old.

5 Data

In this section I will discuss the data that I use for my analysis. The layout for this section is as follows. First, I will discuss my data sources and how the data was prepared for analysis. Then, I will present some summary statistics and figures for this data.

5.1 Data Sources

The first set of data that I use for this project comes from CPS.[14] Data from the CPS includes average daily attendance rates and enrollment, both of which are by grade, school, and school year. Attendance rate data is available from 2002/03 to 2022/23. Enrollment data, which is a census day enrollment count taken on the twentieth day of the school year, is available from 2005/06 to 2023/24.

I also include some demographic data in the summary statistics below. This data also comes from the CPS and is taken on census day as well. Included in this data are student race/ethnicity counts by school and school year. I include this data primarily for informative purposes because the race/ethnicity numbers do not always sum to the total enrollment. In addition, race/ethnicity categories were added and removed over time, making it difficult to create consistent categories for comparison.

In addition to the CPS data, I also use homicide and school catchment area data from the City of Chicago Data Portal. The homicide data is available from 1991 and began including non-fatal shootings in 2010. I focus on homicides to increase the length of my panel. This data set also contains detailed information about the time and location that the incident occurred, which I use to assign homicides to schools.

School catchment area data also come from the City of Chicago and is separated by school type (elementary, middle, and high school) and school year, as school boundaries did change over time. The earliest this data is available is for the 2006/07 school year. Because of this, and due to COVID-19, my analysis covers the 2006/07 through to 2018/19 school years. There is also some overlap of catchment areas within a school type, particularly for high schools, but this does not appear to be the case for elementary schools. I also supplement this data with additional data on the SPP. The earliest this data is available from the City of Chicago website is 2013/14 despite the program starting several years earlier. This is, however, the first year that the SPP was introduced for elementary schools.

[14] During the period that I look at, there have been issues regarding CPS data, particularly high school graduation rates (Perez Jr. 2015). I primarily focus on elementary schools, but it is unclear whether this problem also carried over to this data as well. There is little that can be done about this, but it is worth bearing in mind.

To construct the data set that I use for my analysis, I first assigned homicides to schools and school years based upon catchment areas and when the homicide occurred using QGIS. In assigning homicides to schools, I define a school year to include the summer leading up to that school year. This is because attendance in the prior year will have entirely been determined and any homicides occurring after the final day of class will have no effect on the attendance rate in the previous school year. As an additional benefit, it also allows for the possibility of some anticipation, which is important because the summer months are the most violent in terms of homicides.

Each of the individual data sets are merged together to create a single data set with school, attendance, enrollment, and homicide data. Match rates are relatively high. Of 53,137 school-grade-year observations from the attendance data for schools with catchment areas, 2,422 are missing attendance data (4.56%). Only 8 observations from the enrollment data for schools with catchment areas are unable to be matched to the attendance data (0.02%). Enrollment data is missing for 2,532 observations (4.77%). I remove any grades prior to kindergarten (4,912 observations) and any observations where either the grade observed is greater than the maximum grade offered at the school, or it is less than the minimum grade offered at the school (2,074 observations). Then I drop observations missing attendance or enrollment data (84 observations). Most of the observations dropped in this step are missing both, only 4 observations are missing attendance or enrollment. Finally, I remove any observations where enrollment was reported as zero, but attendance was reported as nonzero (42 observations).

After cleaning the data, I am left with 46,025 school-grade-year observations for grades kindergarten through 12. Observations come from 488 unique schools, 422 of which are elementary schools, from 2006/07 to 2018/19. Using a school type variable, which I construct based upon the catchment area data, I select elementary schools and I further limit the sample to include observations from kindergarten through to eighth grade. After doing so, I am left with 43,106 observations. Because of my main identification strategy, which I discuss in greater detail in the methodology section, I also exclude any observations with non-zero homicides in 2006, essentially excluding schools that were already treated at the beginning of the panel. Additionally, because treatment status varies depending upon the path of homicides over time, and the path that homicides take over time and across schools varies greatly, I focus solely on the initial year a school experienced positive murders to reduce the dimensionality of treatment statuses. After doing so I am left with 7,863 observations. This is the sample that I refer to as the "Main Sample" below.

5.2 Summary Statistics

Table 2 reports sample averages and standard deviations, in parentheses, for the cleaned data sets. Columns indicate the subsample that I use to calculate the summary statistics. The first column reports summary statistics from the sample that I use for my preferred identification strategy. The second and third columns are elementary schools and high schools, respectively. The final column contains the pooled data. The top panel of the table contains variables that vary at the school-grade-year level and the bottom panel contains

Table 2: Summary Statistics by School Type

	Sample			
	Main	Elementary School	High School	Total
School-Grade				
Attendance (%)	94.88	94.37	81.96	93.71
	(2.02)	(2.24)	(8.20)	(4.01)
Enrollment	63.93	62.71	295.75	75.08
	(37.22)	(36.80)	(188.79)	(76.76)
Observations	8,256	43,106	2,416	45,637
School				
Homicides	0.34	1.28	10.45	1.29
	(0.82)	(1.70)	(9.99)	(1.70)
White Students (%)	21.69	8.75	5.10	8.61
	(25.16)	(17.18)	(9.42)	(17.02)
Black Students (%)	30.31	50.59	50.53	50.12
	(38.18)	(43.72)	(39.97)	(43.75)
Hispanic Students (%)	51.98	40.63	44.77	41.57
	(48.26)	(45.01)	(41.61)	(45.79)
Observations	1,012	4,967	616	5,075

This table contains summary statistics, sample means with standard deviations in parentheses, for four data sets. The first column is the main data set that I use for my preferred estimation strategy. The elementary and high school columns contain cleaned elementary and high school data, respectively. The final column is elementary and high schools combined. The top panel of the table titled "School-Grade" reports summary statistics for variables that vary at the school-grade-year level. Attendance is reported in percent and enrollment is the number of students as counted on census day. The bottom panel titled "School" reports summary statistics for variables that vary at the school-year level. Homicides is the number of killings as defined earlier, not the number of victims. White, black, and Hispanic students are the percent of students belonging to the given ethnicity.

variables that vary at the school-year level. Before taking averages of variables in the bottom panel, I collapse the data to the school-year level first so that sample size is not misleading.

For the main data set, it can be seen that average attendance is fairly similar to the full sample of elementary schools. The difference between elementary schools and high schools is much larger, over 10 percentage points. Similarly for enrollment, the difference between the main sample and all elementary schools is small. Between elementary schools and high schools, however, the difference is larger. Average grade level enrollment for elementary schools is under one quarter the size of high school average enrollment.

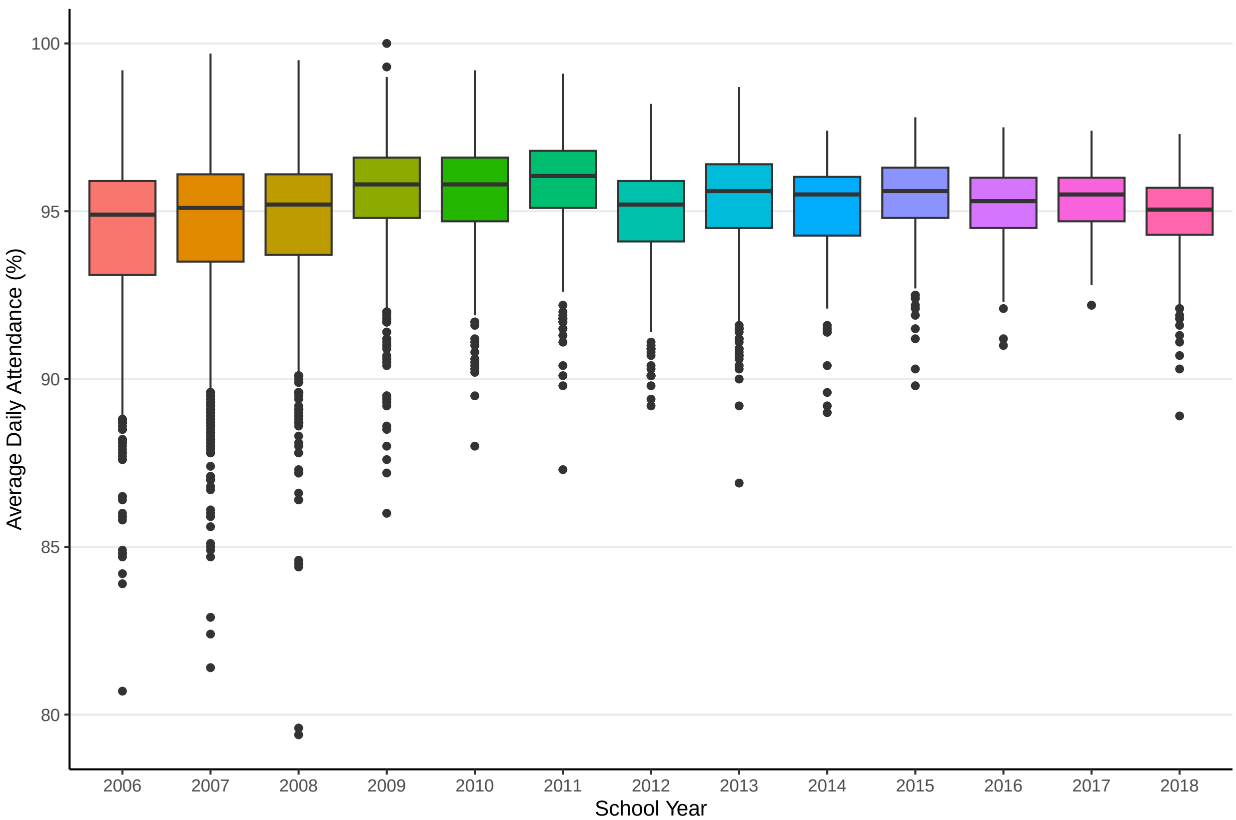
In the bottom panel of Table 2, I present summary statistics for the independent variable of interest, homicides. I define a homicide as an incident involving at least one person being murdered. For example, if two people were murdered by the same person on the same street, this would be counted as a single homicide. This is where differences between the main data set and elementary schools becomes more apparent. Average homicides in the main data set are almost one full homicide less than in the elementary school sample. This is because I drop schools treated in 2006/07 to create a pre-treatment period. I discuss my identification strategy more in the methodology section below. The mean number of homicides assigned to elementary schools is 1.28 and for high schools it is 10.45. Because elementary and high schools cover essentially the same area and the location and number of homicides does not change between school types, this difference in homicides is entirely due to high schools having larger catchment areas.

In the bottom panel of this table I also report racial shares. In the main data set there are almost 2.5 times as many white students, about 20 percentage points fewer black students, and 11 percentage points more Hispanic students as there are in the cleaned elementary schools data. This could indicate important differences between the main data set and all elementary schools in Chicago and that the findings may not generalize to the rest of the schools in the city. It is important to note that the racial shares did not add up to 100% in every year (but some years they did). There were further problems making the racial categories consistent over time because categories were added and removed.

Figure 4 plots average daily attendance rate box plots by school year for elementary schools in the main data set. Each observation is a grade in a school in a given year. For the full elementary school data set see Figure 15 in the appendix. As can be seen from Figure 4, the medians are relatively stable over time with a slight upward trend visible in the first six years of the panel. In the remaining years this appears to flatten out. The distributions of attendance over time also seem to become more compact towards the end of the panel as well, with relatively fewer outliers in later periods of the data set. Some of this is potentially due to changes in composition of the sample. After the initial year a school becomes treated it is removed from the sample because I only estimate treatment effects for this initial year.

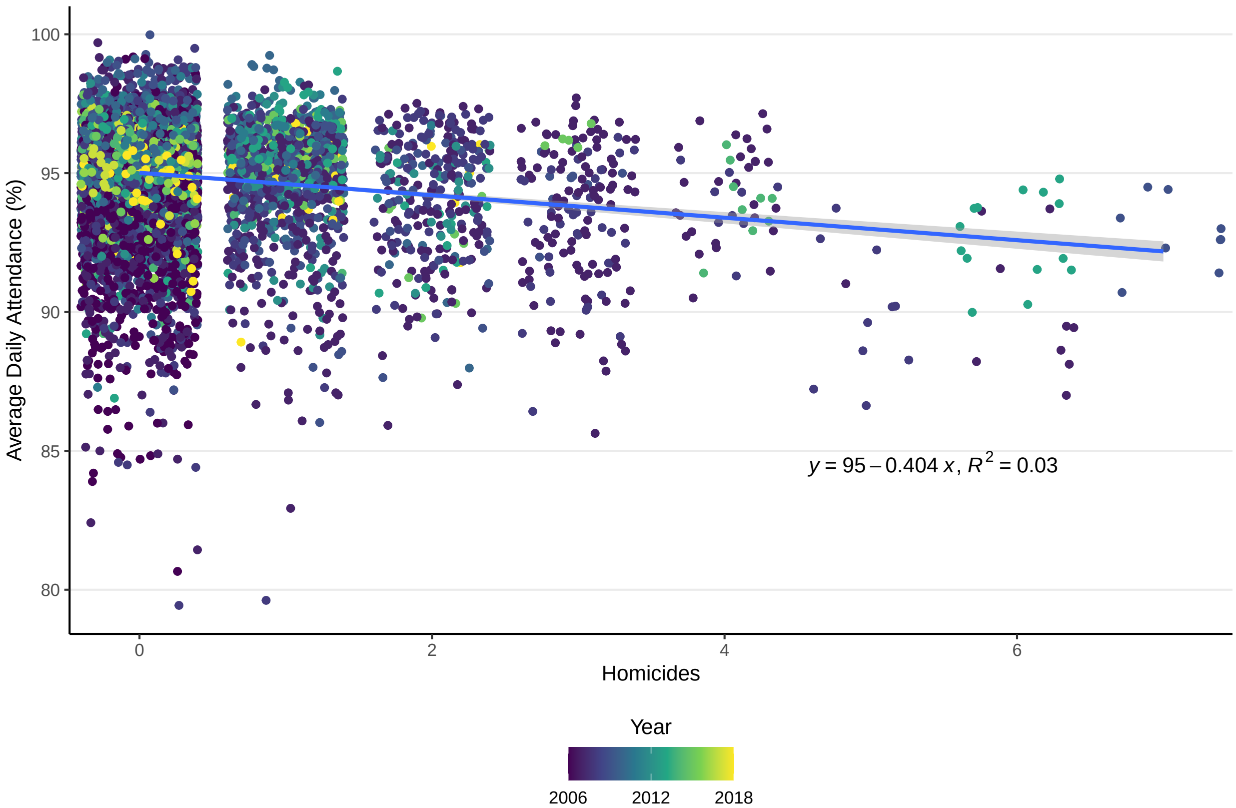
In Figure 5 I plot murders on the x-axis and average attendance on the y-axis for the main data set. Similarly, I include a similar plot using all elementary schools in the appendix Figure 16. Each data point in Figure 5 is a school-grade-year observation, with the color of the point indicating the year. Note that I have also jittered the data points to aid in legibility. As can be seen in the figure, there is a slight negative correlation between attendance and homicides. This figure, along with the figure that follows, also shows the distribution of

Figure 4: Main Sample Attendance by School Year



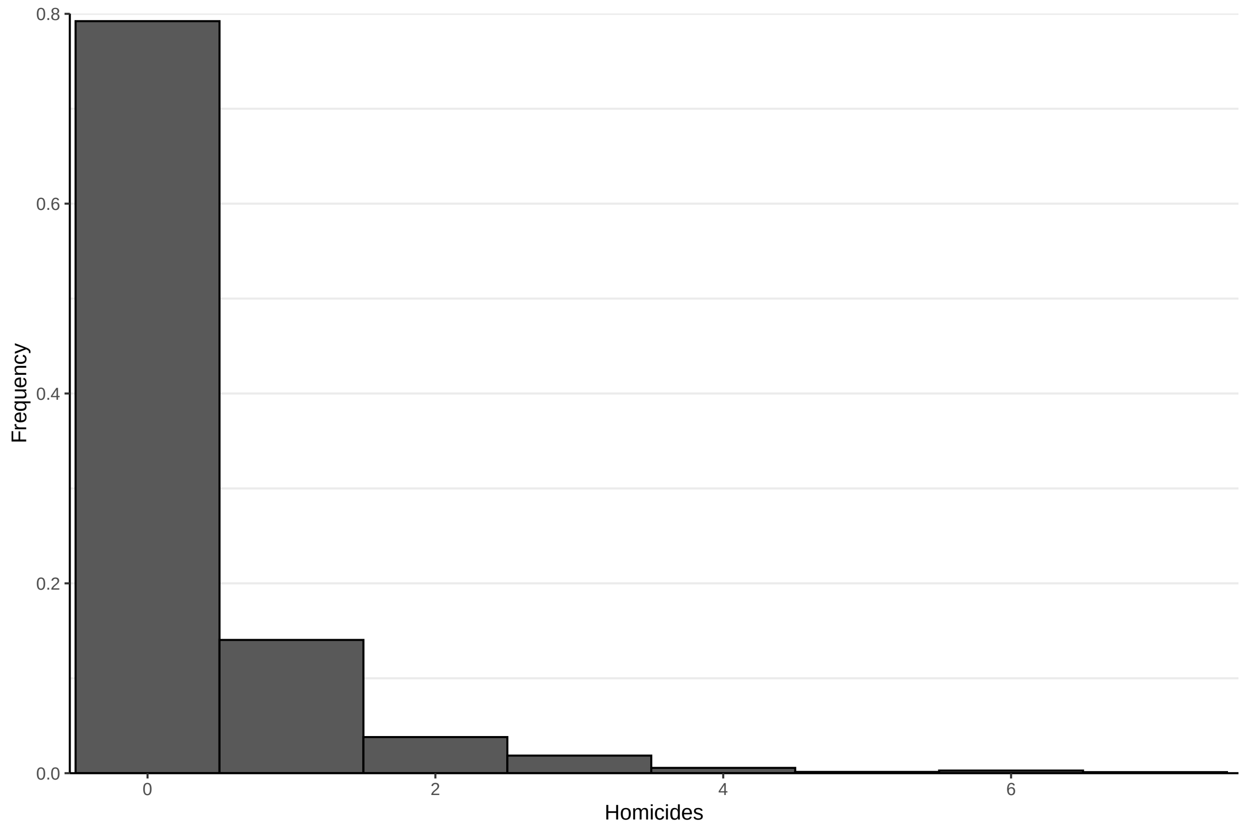
This figure contains box plots by year for average daily attendance. Observations are at the school-grade level. This figure plots only the main data. Figure 15 in the appendix is a similar plot containing all cleaned elementary schools.

Figure 5: Main Sample Attendance and Murders Scatter Plot



This figure plots homicides on the x-axis against average daily attendance on the y-axis. Each point in the scatter plot is a school-grade-year observation with the color indicating the school year. This figure plots only the main data. Figure 16 in the appendix is a similar plot containing all cleaned elementary schools.

Figure 6: Main Sample Homicides Distribution



This figure plots the homicide distribution for the main data. The x-axis shows the number of homicides a school-grade is exposed to in any given year and the y-axis is the frequency. Figure 17 in the appendix is a similar plot for all the cleaned elementary schools.

homicides for the data that I use.

Figure 6 plots the distribution of homicides that a school-grade-year observation is exposed to in the main data set. Figure 17 in the appendix is a similar histogram for all elementary schools. As can be seen in this figure here, around 78% of observations in the main data have zero homicides. For each additional homicide, the number of observations decreases. Just over 3% of observations have three or more homicides. This is largely due to my identification strategy.

6 Methodology

In this section I will discuss the traditional TWFE estimator that is typically used in this setting and the problems associated with it. I will then discuss the estimator that I will use to estimate the effect of homicides on both attendance and enrollment, along with the assumptions for identification. Finally, I will also discuss some challenges and drawbacks associated with this estimation method.

6.1 The TWFE Estimator

When estimating treatment effects for a continuous or discrete variable that takes on many values, the TWFE estimator is a popular choice. Treatment effects are estimated by estimating the model:

$$Y_{it} = \alpha_i + \lambda_t + \beta \text{Post}_t D_i + \varepsilon_{it}, \tag{2}$$

where Y_{it} is the outcome of interest for unit i in period t, α_i is a set of unit fixed effects, λ_t is a set of time fixed effects, Post_t is a post-treatment dummy variable, D_i is the dosage or treatment intensity of the continuous variable of interest, and ε_{it} is the error term. The parameter β is often interpreted as the marginal effect of treatment under parallel trends and the other usual DiD assumptions. This is the interpretation that I will focus on here.

There are several problems with the TWFE estimator, the first of which is not unique to the continuous case. This is due to the staggered timing of homicides; they do not happen across Chicago at the same time for every school in the data. As a result, there is a potential weighting problem with the TWFE estimator similar to the problem discussed by Goodman-Bacon (2021) for the binary treatment case (Callaway, Goodman-Bacon et al. 2024). This issue arises due to the TWFE estimator making bad comparisons and using already treated groups as control unit for later treated units. This can result in biased estimates, specifically if the treatment effects are not constant.

Callaway, Goodman-Bacon et al. (ibid.) discuss an additional issue under this interpretation of the TWFE estimator. It puts more weight on observations near the average dose. This could be problematic depending upon what the distribution of doses among the treated units looks like, for example a bimodal distribution with little density near the average. The authors do state that if the average change in outcomes is linear in dose, then this weighting issue disappears.

A final issue discussed by Callaway, Goodman-Bacon et al., is what they refer to as "selection bias" or more appropriately for this paper "heterogeneity bias". This occurs due to units having, on average, different responses to treatment for a given dose. They propose an alternative parallel trends assumption, which they refer to as stronger parallel trends. Instead of the usual parallel trends assumption that average untreated potential outcomes evolve similarly between treated and control groups, stronger parallel trends assumes that, for all doses the average change in potential outcomes for a given dosage group are similar to the average change in potential outcomes for the entire population if they were to experience the same dosage.

I present TWFE estimates of Equation 2 in the results section below. But the strong assumptions needed for identification should be kept in mind. Even then, there are still likely some weighting issues if treatment effects vary over time.

6.2 An Alternative Estimator

In the binary case, one solution to the staggered adoption of treatment is the DiD estimator proposed by Callaway and Sant'Anna (2021). The idea behind this estimator is that it allows for more treatment effect heterogeneity by estimating treatment effects by treatment-timing cohort, the initial year a group of units, g, was treated, and by time t for all $t \geq g$. In the binary case, this estimator without controls estimates each of the (g, t) treatment effects using a two group and two time period DiD, with $g-1$ as the pre-treatment period and t as the post-treatment period. The data is subset to include only observations in period $g-1$ and t belonging to either the control group or treatment group g. Then, the following model is estimated for each g and t combination where $t \geq g$:

$$Y_{it} = \alpha_i^{gt} + \lambda_t^{gt} + \beta^{gt} G_i T_t + \varepsilon_{it}^{gt}, \tag{3}$$

where α_i^{gt} is a set of unit fixed effects, λ_t^{gt} is a set of time fixed effects, G_i is a dummy variable equal to one if an observation is in treatment-timing cohort g and zero otherwise, T_t is a post-treatment dummy equal to one if $t \geq g$ and zero otherwise, and β^{gt} is the parameter of interest. For the discrete case it would be straightforward to extend this to allow for treatment-effects to vary by dosage if there are relatively few dosage levels and a sufficient number of observations in each dosage group.

This would solve the issues associated with staggered treatment, but there are still several problems that remain. The first issue is that there is no clear pre-treatment period. A second and related issue is that when a unit is exposed to treatment, some level of homicides, it does not necessarily stay the same in the following period and could go up or down.

To deal with these two remaining issues, I combine the ideas of the estimator proposed by Callaway and Sant'Anna that I discussed above with ideas from de Chaisemartin et al. (2022). For the first issue of no clear pre-treatment period, I define the control group as schools that experience zero homicides throughout the duration of the panel. I remove any schools with a positive number of homicides in the 2006/07 school year, the start of the panel, and use this as the pre-treatment period. This allows me to estimate a local effect for switchers relative to stayers. In other words, an effect for units that went from zero homicides to dosage $d > 0$ relative to units that remained at zero. An additional assumption is necessary, that prior treatment status does not have any effect on current outcomes.

Regarding the second issue, that treatment dosage is constantly fluctuating after a treated unit switches from zero homicides, I remove any post-treatment school years beyond the initial school year that a unit is treated. In doing so, I only focus on the initial effect that homicides have on outcomes. This makes estimates more tractable, as there are fewer to estimate, and it eliminates the need to worry about dosage changing from the initial level post-treatment. As a result, I modify Equation 3 to:

$$Y_{it} = \alpha_i^{gd} + \lambda_t^{gd} + \beta^{gd} G_i T_t + \varepsilon_{it}^{gd} \tag{4}$$

where the time superscripts have been removed because I only focus on the initial year of treatment g for dosage group d. I estimate this model similar to Equation 3, by subsetting the data to only include time periods $g-1$ and g for treated and control units for a given dosage and estimate each β^{gd}. Additionally, I include not yet treated units in the control group along with never treated units.

I estimate the effect of homicides on two outcomes, attendance and enrollment, both at the grade level. So here the subscript i represents a school-grade observation in school year t. To deal with relatively few observations at the top of the dosage distribution, I combine three or more homicides into the same bin and assume that they respond similarly to treatment. After estimating each of the β^{gd} parameters, I aggregate them by dosage group using the simple aggregation from Callaway and Sant'Anna (2021). This takes a weighted average across treatment-timing cohorts to create a single parameter estimate for each dosage group, $\hat{\theta}^{d}$. In addition to reducing the number of parameters reported, this increases the number of observations used to estimate the parameters.

The treatment effects that are identified depend upon which parallel trends assumption one is willing to make. In addition to the other usual DiD assumptions, if one invokes a typical parallel trends assumption between each treatment-timing group and each dosage group with the control group, then estimates could be interpreted as an average treatment effect on the treated for dose d for switching units that actually experienced dose d. If one is willing to invoke the stronger parallel trends assumption, which essentially generalizes responses for a given dose to all other dose groups that never experienced that dose, then the difference between two adjacent dosage groups could be viewed as the average marginal effect for moving from one dosage to another among the switchers.

Figure 7 plots the pre-treatment homicide distribution by year and by treatment status. Each panel is a pre-treatment year with red bars plotting the share of homicides for treated observations and the blue plotting the share for untreated observations.[15] As can be seen from the figure, the share of observations experiencing low levels of homicides was relatively similar between treated and control schools. The control group typically had a larger share of schools with zero homicides, but there is a good amount of overlap between these two groups at the dosage levels that I explore below. Because the treated schools that I examine experienced relatively low levels of homicides I believe that including "never treated" in the control group with "not yet treated" is a valid option.

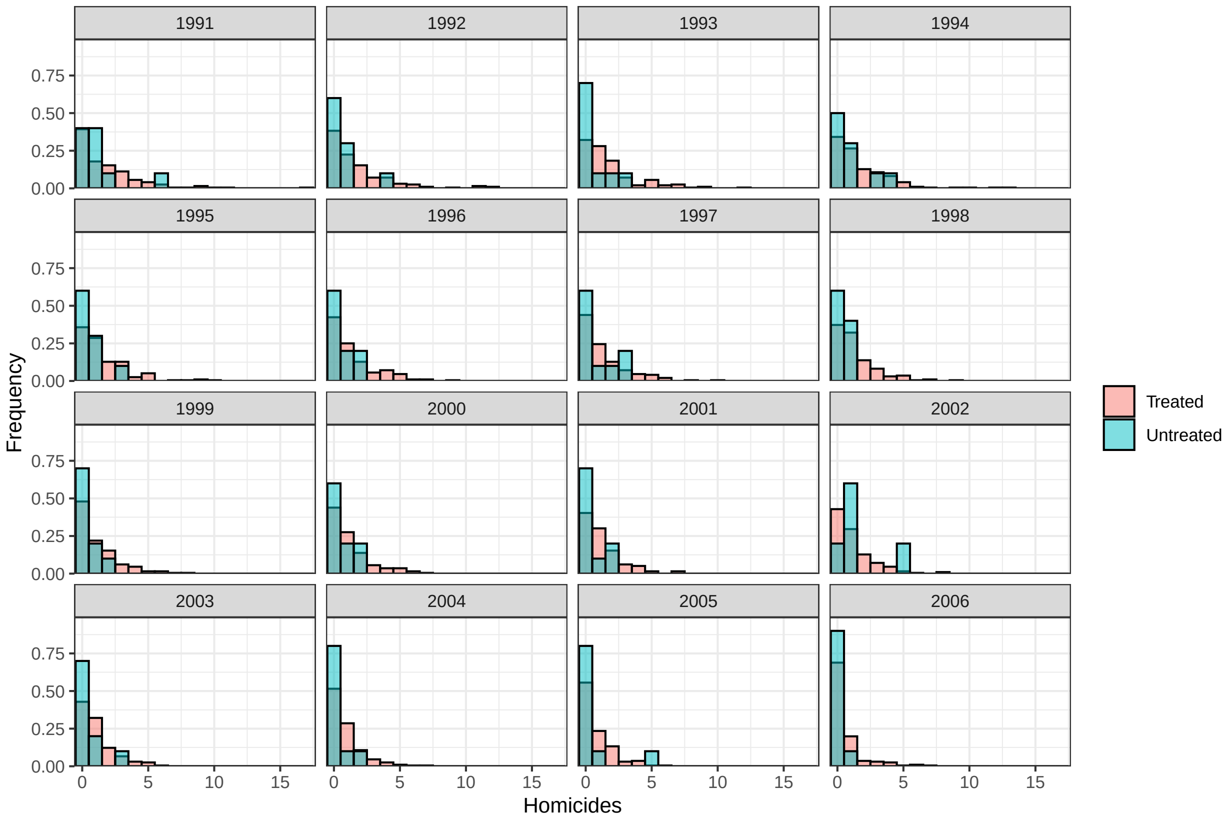
Table 3 contains pre-treatment (2006) summary statistics by treatment status. The first column contains summary statistics for untreated observations and the second column for treated observations. An observation is considered treated if at any point in the panel they experience some positive number of homicides. As can be seen from this table there are large, and in all but one case, statistically significant differences between the variables between the two groups.[16] Because of these often-large differences I also include not yet treated observations in the control group. These observations should be more similar to other treated units and, when combined with other never treated units, serve well to construct a plausible counterfactual.

Under a weak parallel trends the estimated effects can be interpreted as the average change in attendance or enrollment for schools that experienced dose d due to an increase in

[15] An important point worth mentioning is that the figure uses calendar years and not school years, this is why there are some treated and control observations with more than zero homicides in 2006.

[16] The only variable that is not significantly different between the two groups is attendance.

Figure 7: Pre-Treatment Homicide Distributions by Year and Treatment Status



This figure plots the homicide distribution by year and treatment status. Each panel represents a different year, and, within each panel, the red bars represent treated homicides and the blue bars represent control group homicides. An important note here is that the panels here are calendar years and not school years.

Table 3: Pre-Treatment Summary Statistics by Treatment Status, 2006

	Untreated	Treated
Attendance (%)	94.66	94.26
	(2.67)	(2.23)
Enrollment	45.93	65.04
	(21.52)	(35.62)
White (%)	36.67	9.41
	(24.47)	(14.97)
Black (%)	24.14	39.64
	(35.15)	(41.37)
Hispanic (%)	28.75	45.47
	(21.14)	(36.38)
Pre-Treatment Murders (Total)	9.11	18.97
	(8.82)	(17.96)
Safe Passage School	0.00	0.18
	(0.00)	(0.39)
Observations	116	1499

This table contains pre-treatment summary statistics, means with standard deviations in parentheses, by treatment status. An observation is treated if at any point in the panel it experienced $d > 0$. Untreated observations are those that did not experience any homicides throughout the duration of the panel. An observation here is at the school-grade level. Variables are defined identically to Table 2 with two additional variables reported here. First is pre-treatment murders, which is the sum of all murders from 1991 up to the 2005/06 school year. The second additional variable is a safe passage school dummy variable equal to one if a school participated in the safe passage project and zero otherwise.

Table 4: Pre-Treatment Summary Statistics by Dosage, 2006

	Dosage			
	0	1	2	3+
Attendance (%)	94.66	94.66	93.63	93.03
	(2.67)	(1.99)	(2.16)	(2.76)
Enrollment	45.93	63.12	69.73	68.59
	(21.52)	(34.38)	(34.00)	(42.49)
White (%)	36.67	12.54	3.32	1.57
	(24.47)	(16.73)	(6.79)	(4.24)
Black (%)	24.14	30.60	53.56	66.94
	(35.15)	(37.94)	(43.26)	(38.40)
Hispanic (%)	28.75	50.20	39.49	29.48
	(21.14)	(34.62)	(38.25)	(37.01)
Pre-Treatment Murders (Total)	9.11	14.50	27.47	30.00
	(8.82)	(14.40)	(23.71)	(16.35)
Safe Passage School	0.00	0.12	0.26	0.39
	(0.00)	(0.33)	(0.44)	(0.49)
Observations	116	1028	267	204

This table contains pre-treatment summary statistics, means with standard deviations in parentheses, by dosage group. An observation here is at the school-grade level. Variables are defined identically to Table 2 and Table 3.

homicides from 0 to d in student in a school's catchment area relative to stayer schools that either did not experience such a change in homicides in their catchment area or who have not yet experienced a change in homicides in their local area. Perhaps the most important threat to identification, as discussed in the preceding paragraph, comes from whether the selected control group serves as a valid comparison group for the various dosage levels. As can be seen in Table 3, there are large and significant differences between the never treated and treated groups. In Table 4 I report similar pre-treatment summary statistics by dosage group instead of simply by treatment status. As can be seen from this table, the lowest dosage groups are the most similar. As dose, or the level of homicides experienced, increases the differences between said dosage group and the never treated group becomes larger. The previous two tables highlight both the relatively large differences between the "never treated" and treated groups. I believe that this also highlights the importance of including "not yet treated" observations in the comparison group.

There are several additional identification issues that could bias the estimates. The first of these is reverse causality. It is possible that students who miss school commit crime (Jacob and Lefgren 2003), although it seems unlikely that many students would miss school to commit homicides. An additional identification issue is the various policies that were enacted in or around the time frame that I look at. These include the SPP, Culture of Calm, and raising the minimum schooling age. To deal with both issues, reverse causality and the simultaneous enactment of other policies, I follow a similar strategy to Komisarow and Gonzalez (2023) by focusing solely on grades K through 8 in elementary schools. This should reduce the likelihood of reverse causality because students this young are unlikely to be missing school to engage in violent crime. It also eliminates any changes in the outcomes that may arise due to the Culture of Calm or raising the minimum schooling age because these policies target high school students. To deal with the SPP I present an additional set of estimates that excludes all SPP schools after 2013, which is when the program was expanded to include elementary schools.

There are several other drawbacks to this identification strategy. First, I only estimate local effects between switchers and stayers. These two groups are likely different than the rest of the elementary schools in Chicago and it is possible that these estimates do not generalize to the rest of the city. A second and related point is that even if these effects do generalize to the entire city, it is possible that they do not generalize to the entire country. Chicago may have some unique policies in place that cause students and their parents to behave a certain way that are not present in other cities or states. Third, I only look at a single period of treatment because of the nature of homicides. Because of this, I am unable to say anything about dynamic effects that may occur beyond the initial time of treatment. Fourth, I focus solely on elementary schools. It is possible that older students behave differently as they gain more autonomy, and their parents begin making less decisions for them. Finally, because I use a relatively small subset of the data for identification, the number of observations towards the top of the homicide distribution is limited. These shortcomings are more difficult to deal with and I mention them so that the reader is aware.

7 Results

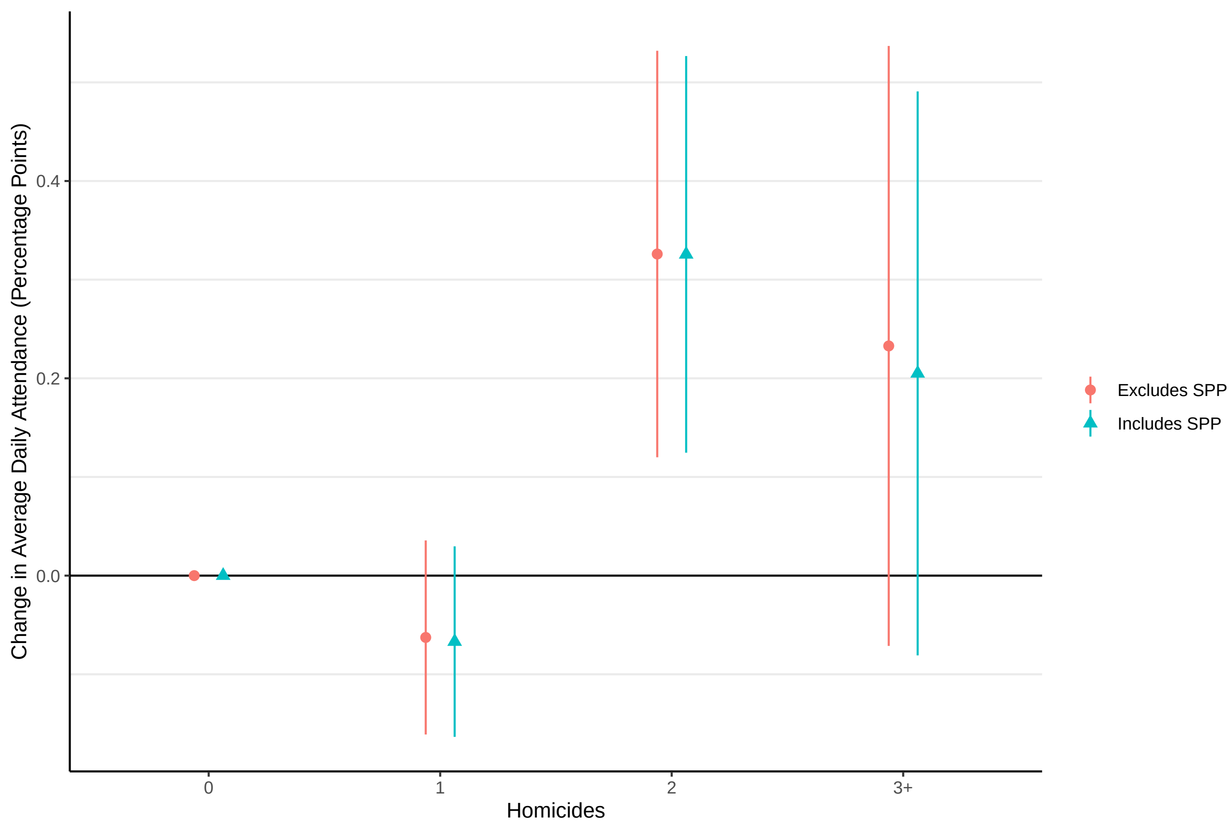
In this section I will present estimates for the effect that homicides has on both attendance and enrollment. The first subsection reports the estimates from my preferred identification strategy using Equation 4. Then, in the second subsection, I introduce additional robustness checks in addition to presenting TWFE estimates similar to Equation 2.

7.1 Main Results

Figure 8 plots the aggregated point estimates, $\hat{\theta}^d$, and their associated simultaneous 95% confidence intervals, from Equation 4 with attendance as the outcome. The x-axis is dosage, or number of homicides a school-grade observation was exposed to, and the y-axis is the change in average attendance in percentage points. The red circles are point estimates and confidence intervals estimated using the sample that excludes SPP schools and the blue triangles are estimates that include SPP schools. Zero homicides is the control group, these estimates are mechanically equal to zero.

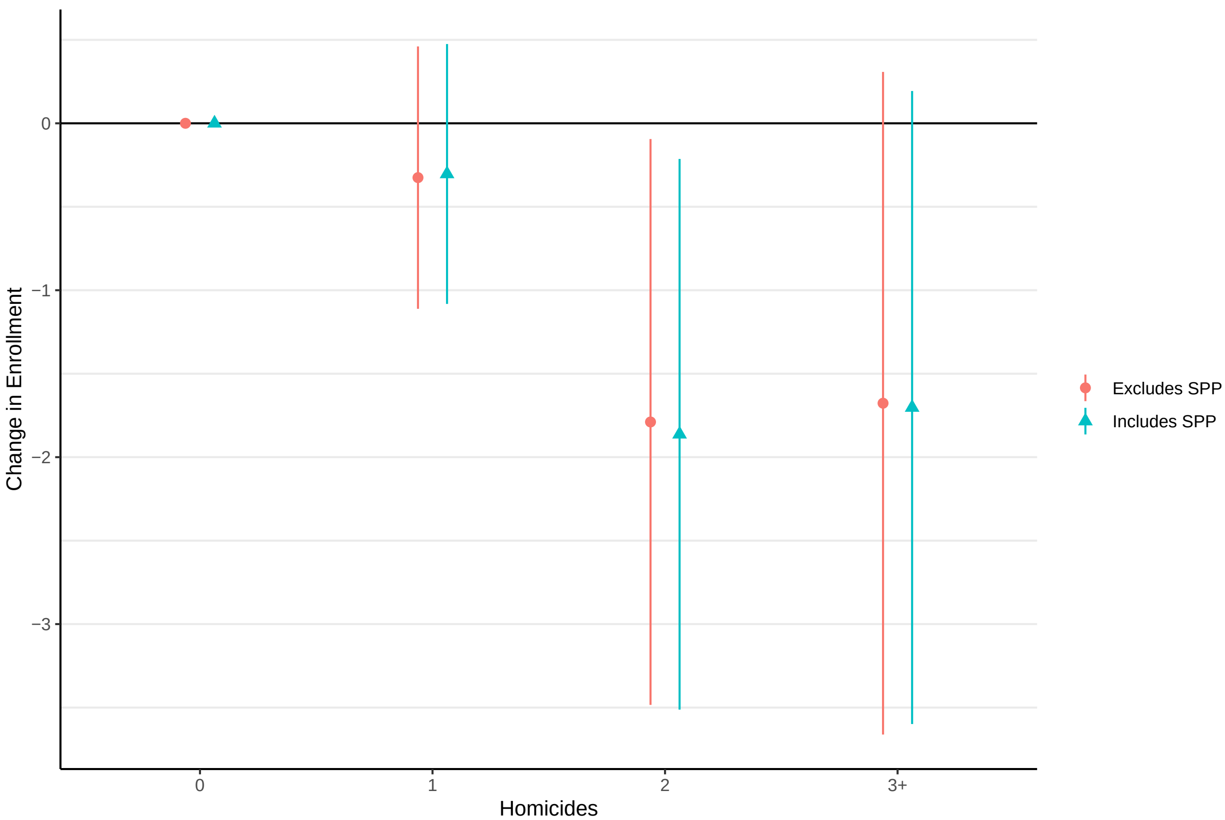
As can be seen from the figure, estimates with and without SPP schools are similar. I will focus on estimates that omit SPP here. For school-grades that went from zero to one

Figure 8: Effect of Homicides on Attendance



This figure plots aggregated treatment effect estimates for the effect that homicides has on attendance. The vertical bars are simultaneous, within a dosage group, 95% confidence intervals. The x-axis is the dosage, or number of homicides, and the y-axis is the change in average daily attendance in percentage points. Zero homicides is the control group so the change in attendance is mechanically zero for this dosage level. The red circles are estimates from regressions that exclude SPP schools from the sample and the blue triangles are from the sample that includes them. See Table 6 in the appendix for these results in table format.

Figure 9: Effect of Homicides on Enrollment



This figure plots aggregated treatment effect estimates for the effect that homicides has on enrollment. The vertical bars are simultaneous, within a dosage group, 95% confidence intervals. The x-axis is the dosage, or number of homicides, and the y-axis is the change in enrollment. Zero homicides is the control group so the change in enrollment is mechanically zero for this dosage level. The red circles are estimates from regressions that exclude SPP schools from the sample and the blue triangles are from the sample that includes them. See Table 7 in the appendix for these results in table format.

homicide, the point estimates are relatively small, negative, and insignificant. It seems that for this group of switchers, the first homicide does not have much of an effect on attendance. For school-grades that went from zero to two homicides, the point estimates become positive, larger in magnitude, and significant. The estimates imply that for switchers going from zero to two homicides, on average attendance increased by about 0.33 percentage points. For observations that went from zero to three or more homicides the result is slightly smaller in magnitude. For this group of switchers, the point estimates imply that this increased attendance by 0.23 percentage points on average, but this estimate is insignificant.

In Figure 9 I plot estimates like the previous figure with the only difference being enrollment is the outcome. Again, estimates including and excluding SPP schools are similar, so I will focus on the estimates that exclude SPP schools. The estimated treatment effect for switchers going from zero to one homicide is small, negative, and insignificant. For switchers going from zero to two homicides I find that enrollment decreased by 1.79 students in a school-grade. For switchers that went from zero to three or more homicides, the decrease is slightly smaller, implying enrollment fell by 1.68 students in a school-grade. This final estimate for the very top of the homicide distribution is not significant, however.

7.2 Robustness Check

In this subsection, I will present several additional sets of results. The first robustness check controls for the sum of pre-treatment homicides going back to 1991 as this is the earliest year in the data.[17] This changes the parallel trends assumption from an unconditional one to one conditional on covariates.

The second set of results that I will present as a robustness check are from an alternative estimator, essentially a TWFE estimator such as the one in Equation 2. For this set of estimates, I include a larger sample than the one that I use for my main set of estimates. I still exclude any schools with one or more homicides in the first year of the panel, but I do not drop observations after the initial year of treatment. The model that I estimate is:

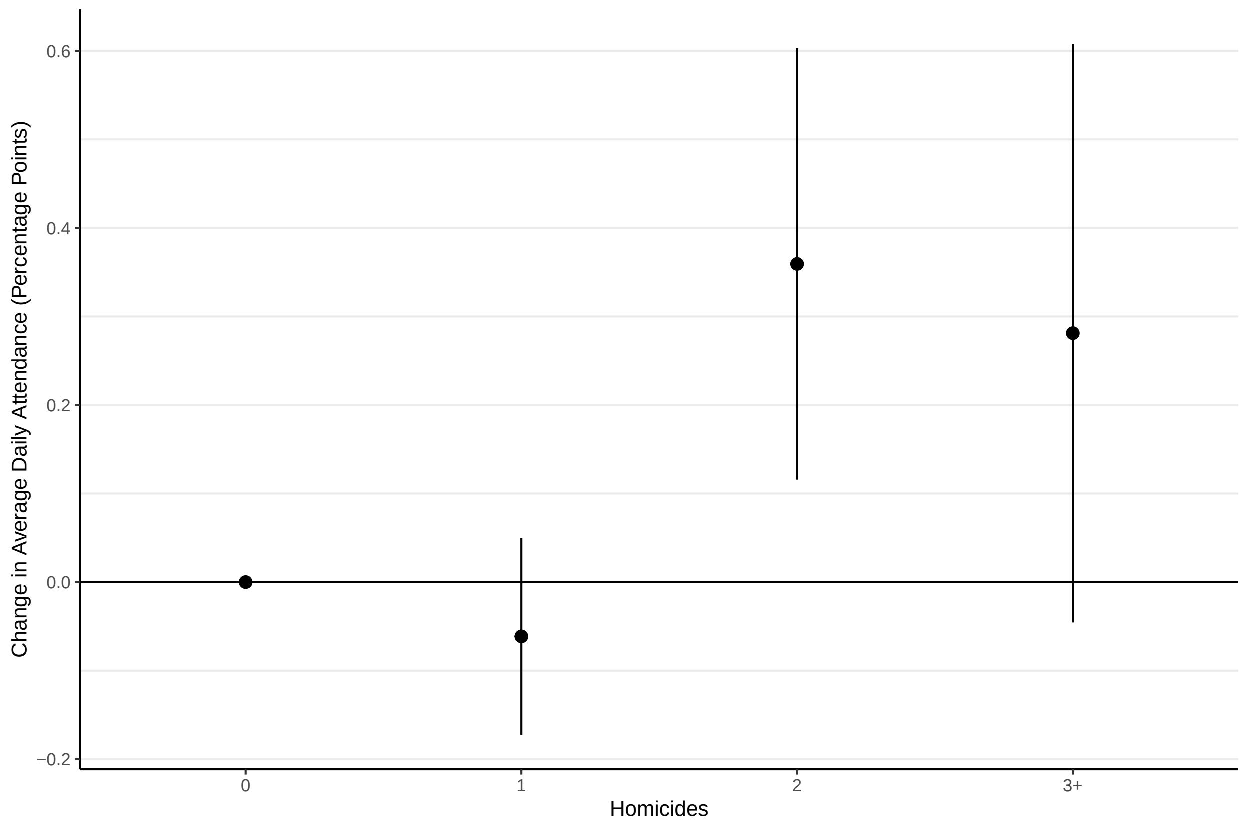
$$Y_{it} = \alpha_i + \lambda_t + \sum_{h>0} \beta_h H_{hit} + \upsilon_{it}. \tag{5}$$

The only difference between Equation 5 and Equation 2 is that in this model here, I allow for estimates to vary by the number of homicides. H_{hit} is a dummy variable equal to one if the number of homicides is equal to h in school-grade i in year t and zero otherwise. I exclude zero homicides as the reference group, and I group schools that experienced six or more homicides into a single group.

In Figure 10 for attendance and Figure 11 for enrollment I plot the point estimates and confidence intervals from the DiD estimator that uses the sum of pre-treatment homicides as a control. As can be seen from Figure 10, the estimates with attendance as the outcome are similar in terms of direction of the effect. Going from zero to one homicide has a small, negative, and insignificant effect on switchers in this dosage group. The estimates towards the

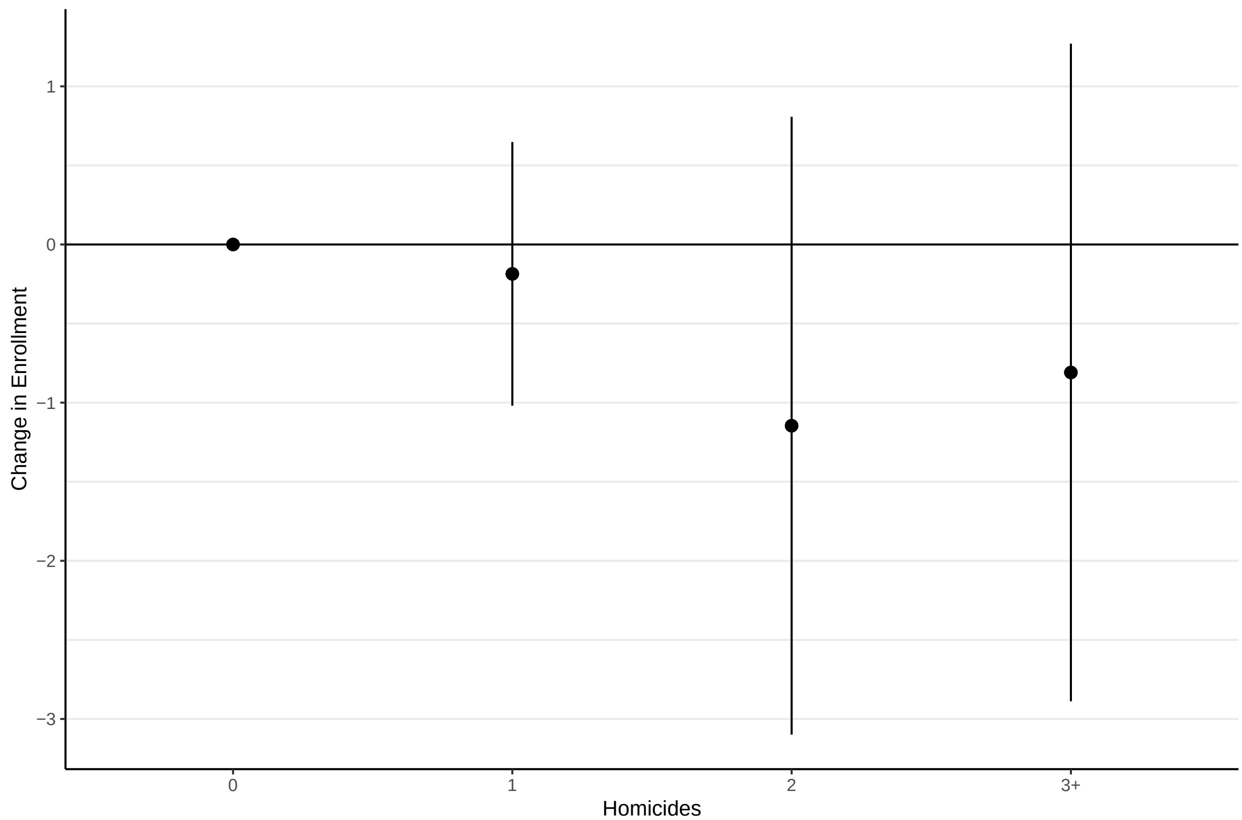
[17]Because the earliest date that catchment area data is available is the 2006/07 school year, I use this these boundaries to assign pre-treatment homicides to elementary schools.

Figure 10: Effect of Homicides on Attendance with Controls



This figure plots aggregated treatment effect estimates for the effect that homicides has on attendance controlling for the sum of pre-treatment homicides. The vertical bars are simultaneous, within a dosage group, 95% confidence intervals. The x-axis is the dosage, or number of homicides, and the y-axis is the change in average daily attendance in percentage points. Zero homicides is the control group so the change in attendance is mechanically zero for this dosage level. The sample used to estimate these parameters excludes SPP schools. See Table 8 in the appendix for results in a table.

Figure 11: Effect of Homicides on Enrollment with Controls

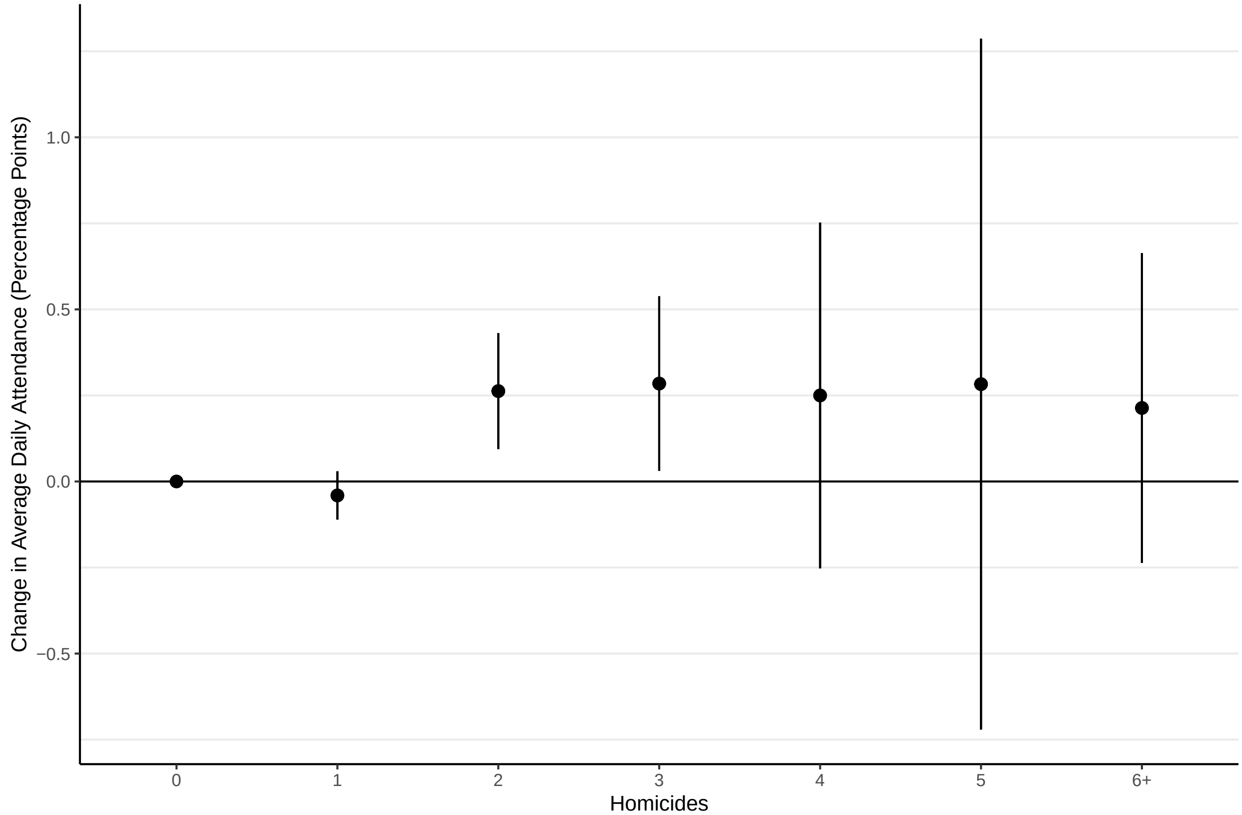


This figure plots aggregated treatment effect estimates for the effect that homicides has on enrollment controlling for the sum of pre-treatment homicides. The vertical bars are simultaneous, within a dosage group, 95% confidence intervals. The x-axis is the dosage, or number of homicides, and the y-axis is the change in enrollment. Zero homicides is the control group so the change in enrollment is mechanically zero for this dosage level. The sample used to estimate these parameters excludes SPP schools. See Table 8 in the appendix for results in a table.

top have both increased slightly. The point estimate for two homicides remains significant. At the very top, the point estimate for three or more homicides has become significant at the 10% level. From the enrollment figure, the point estimates all remain negative, but all of the estimates have decreased in magnitude and become insignificant. Overall, these findings are fairly similar to the main findings above, particularly for attendance although the estimates for enrollment point in a similar direction to above as well.

Finally, to conclude this section I present estimate from Equation 5 in the two figures that follow. The layout of these figures is similar to the previous ones. Figure 12 contains point estimates and 95% confidence intervals with attendance as the outcome variable. Standard errors are clustered at the school level. Overall, these results are fairly similar to the previous findings. The sign of the estimate is the same for the groups that I estimated above. The size of the estimates is also similar. Figure 13 is similar to the previous figure but with enrollment as the outcome now. Again, the results look relatively similar to the previous

Figure 12: TWFE Estimates for Attendance



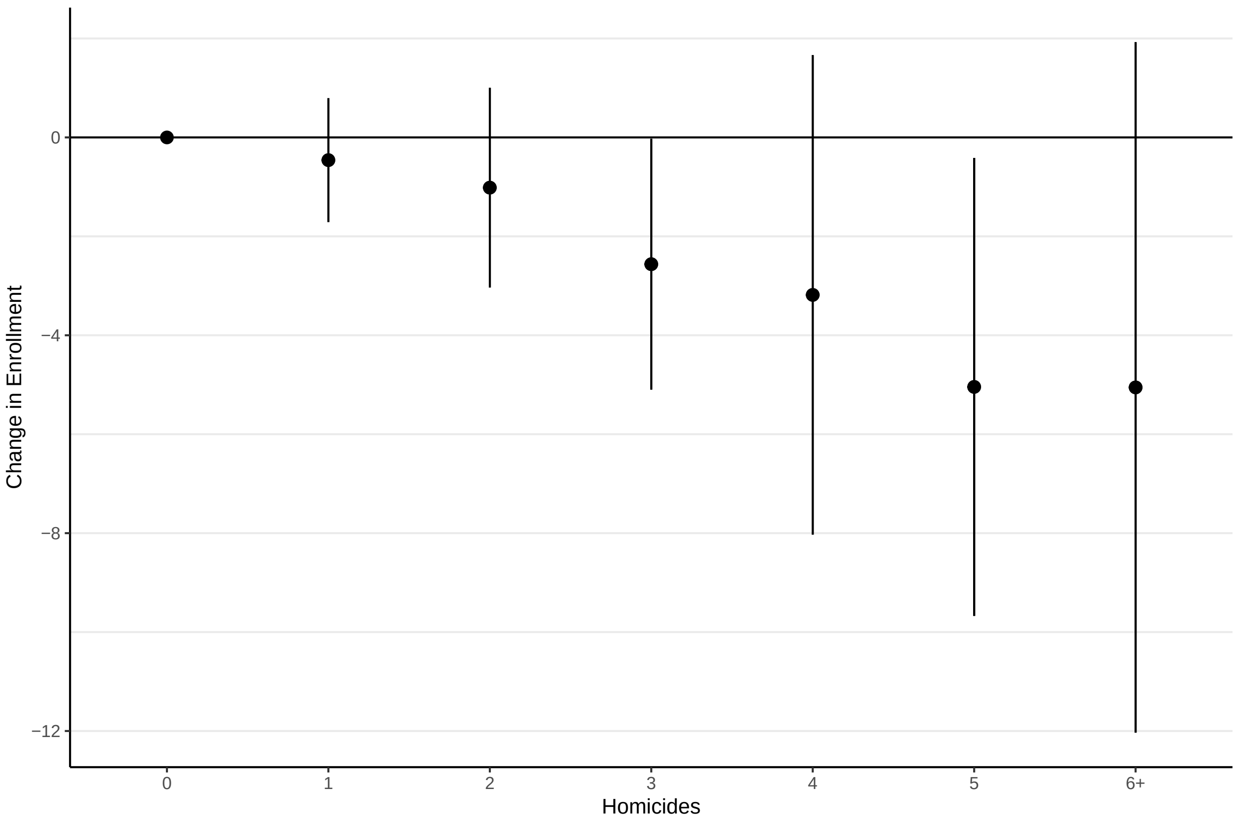
This figure plots point estimates from Equation 5 with attendance as the outcome. The vertical bars are 95% confidence intervals. Standard errors are clustered at the school level. The x-axis is the dosage, or number of homicides, and the y-axis is the change in average daily attendance in percentage points. Zero homicides is the control group so the change in attendance is mechanically zero for this dosage level. The sample used to estimate these parameters excludes SPP schools.

sets of estimates that I have discussed above. The estimates here are always negative and are increasing in size as the number of homicides increases. For the enrollment estimates, similar to the attendance estimates, the pattern is relatively similar to what I presented above at the levels of homicides that I observed using my main identification strategy.

7.3 Discussion

Under the DiD assumptions, the results suggest that for higher dosage schools, these higher levels of homicides reduced the marginal cost of attending schooling, at least in the short-run when parents have no other options. For schools that only experienced one homicide there does not appear to be much of an effect on attendance. For schools that experienced two or more homicides, however, the results indicate that the higher levels of homicides increased attendance for those high dosage groups relative to the control group. This is consistent with schools acting as a safe place for parents and students in the short-run.

Figure 13: TWFE Estimates for Enrollment



This figure plots point estimates from Equation 5 with enrollment as the outcome. The vertical bars are 95% confidence intervals. Standard errors are clustered at the school level. The x-axis is the dosage, or number of homicides, and the y-axis is the change in enrollment. Zero homicides is the control group so the change in enrollment is mechanically zero for this dosage level. The sample used to estimate these parameters excludes SPP schools.

There is also some evidence that enrollment decreased for high dosage schools with high levels of homicides. While this may appear contradictory at first glance that is not necessarily the case as I have explained above. These results are consistent with parents putting their students in the safest place possible in the short-run, in this case school, then moving their children to safer places if/when possible. Parents that can move their children to different schools do so, but not all parents will be able to. This is because selective schools with testing requirements are not open to everyone, charter schools and choice programs may be oversubscribed and require parents to enter into a lottery and hope for the best, and searching for a new home in a different area is expensive and takes time. As a result, enrollment is likely more of a medium-run effect. Further, because students cannot drop out of elementary school because they are too young, any changes in enrollment beyond the counterfactual enrollment level should be caused by students attending different schools, either within Chicago or potentially in another city.

This is one potential explanation for the pattern of results found here, but it is also possible that there is a violation of parallel trends and that this is driving the findings here. As mentioned in the methodology section, the lowest dosage group is more similar to the untreated control group than higher dosage groups. For this low dose cohort, the estimated effects for both outcomes are relatively small and statistically insignificant. For the higher dosage groups, it is not until two or more homicides that the point estimates start to become statistically significant. These findings could be driven by differences between dosage groups becoming larger as the dosage becomes larger. The inclusion of not-yet treated units in the control group should alleviate some of the concerns about whether they are a valid control group. Additionally, controlling for the sum of pre-treatment homicides should also help. It is still possible, however, that there is a violation of parallel trends.

8 Conclusion

In this chapter I have examined how homicides have affected both attendance and enrollment in Chicago elementary schools. To do so I have used publicly available data from CPS and the City of Chicago website. I have focused on estimating treatment effects of a subset of elementary schools that have switched from zero homicides to some positive amount during the period I observed. To estimate the treatment effects, I have used an estimator that allows for treatment effect heterogeneity in treatment-timing cohort and dosage of homicides. I have found that for switchers in dosage groups with two or more homicides, the estimated effect of homicides on attendance appears to be positive and the effect on enrollment is negative although this effect on enrollment is not robust to controlling for pre-treatment homicides.

These findings for attendance are in contrast with several other papers that I have discussed in the literature review but the enrollment estimates are not. For example, in the school shooting literature Cabral et al. (2020) finds an increase in absenteeism and Beland and Kim (2016) finds no effect on attendance. It is possible that students respond to violence at school and violence in the neighborhood differently. This could be caused by students viewing school as a safe place, in which case a shooting occurring at the school would harm their perception of safety at school. The latter paper does find a negative effect on enrollment similar to this paper. Negative enrollment effects for both school and local violence can potentially be explained by people removing themselves from violent areas and situations regardless of where the violence occurs, at school or in the neighborhood.

The estimates that I report here also differ from the papers that look at local violence. For example, Koppensteiner and Menezes (2021) looked at homicides in Brazil and found that attendance decreases. This could be due to differences between countries or even differences between cities and potentially the policies in place in Chicago. It could also be due to other things as well, such as the TWFE estimator that the authors use. The paper by Ang (2020) finds that absenteeism increase following a police killing. Regarding police killings there is the potential that this effect does not generalize to all homicides. Police killings are committed by individuals that are supposed to protect the community and when this happens it is possible that people respond differently to it than when there is a homicide committed by the member of the public.

There are several drawbacks to this paper which are worth reemphasizing. I only look at homicides, not any other violent crimes. I also only estimate the effect of homicides in the initial year of treatment. Lastly, my main identification strategy estimates effects for a subset of schools. This may be too limited and the results may not generalize well to the rest of Chicago or the US. Overall, additional research on this topic is needed, ideally using student level data to exploit better variation and determine the longer run attendance consequences of homicide exposure.

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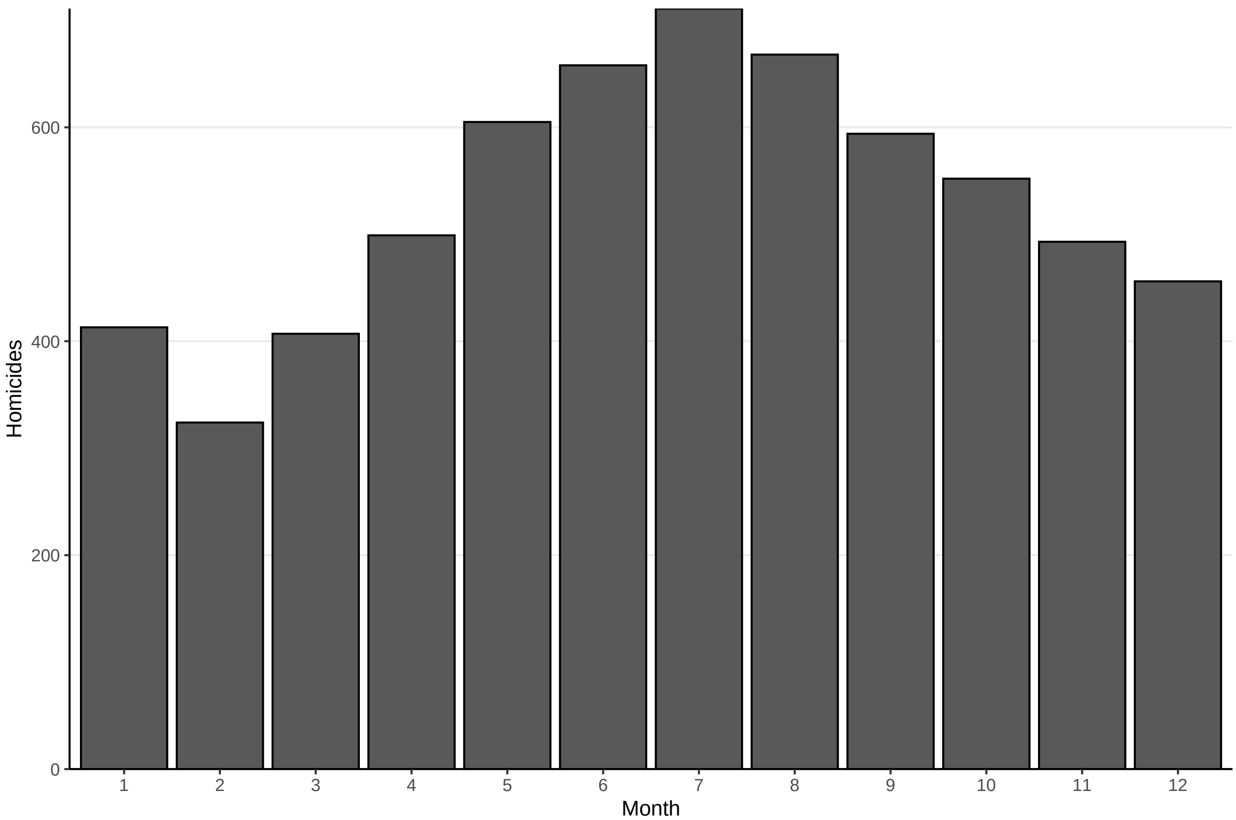
9 Chapter 1 Appendix

Figure 14: Homicides by Month



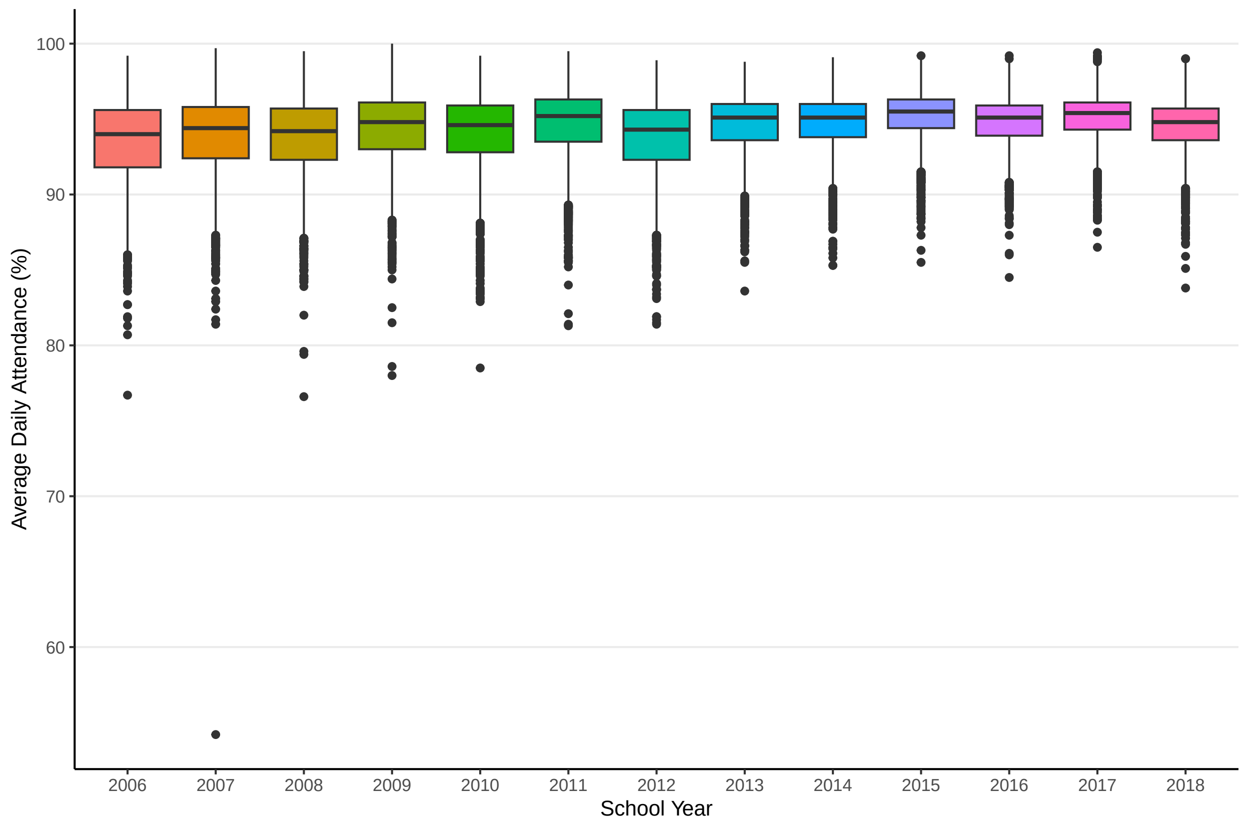
This figure plots the number of homicides by month for the city of Chicago from the beginning of 2006 through to the end of 2019.

Table 5: Safety Traveling to/from School

	Violence/Crime an Issue on Route to School					
	Not an Issue	Little Issue	Somewhat Issue	Very Much Issue	Serious Issue	Total
Doesn't Walk/Bike	33.06	2.66	13.19	2.24	12.92	64.07
Walks/Bikes	12.70	1.89	2.06	0.45	18.84	35.93
Total	45.76	4.55	15.25	2.68	31.76	100.00

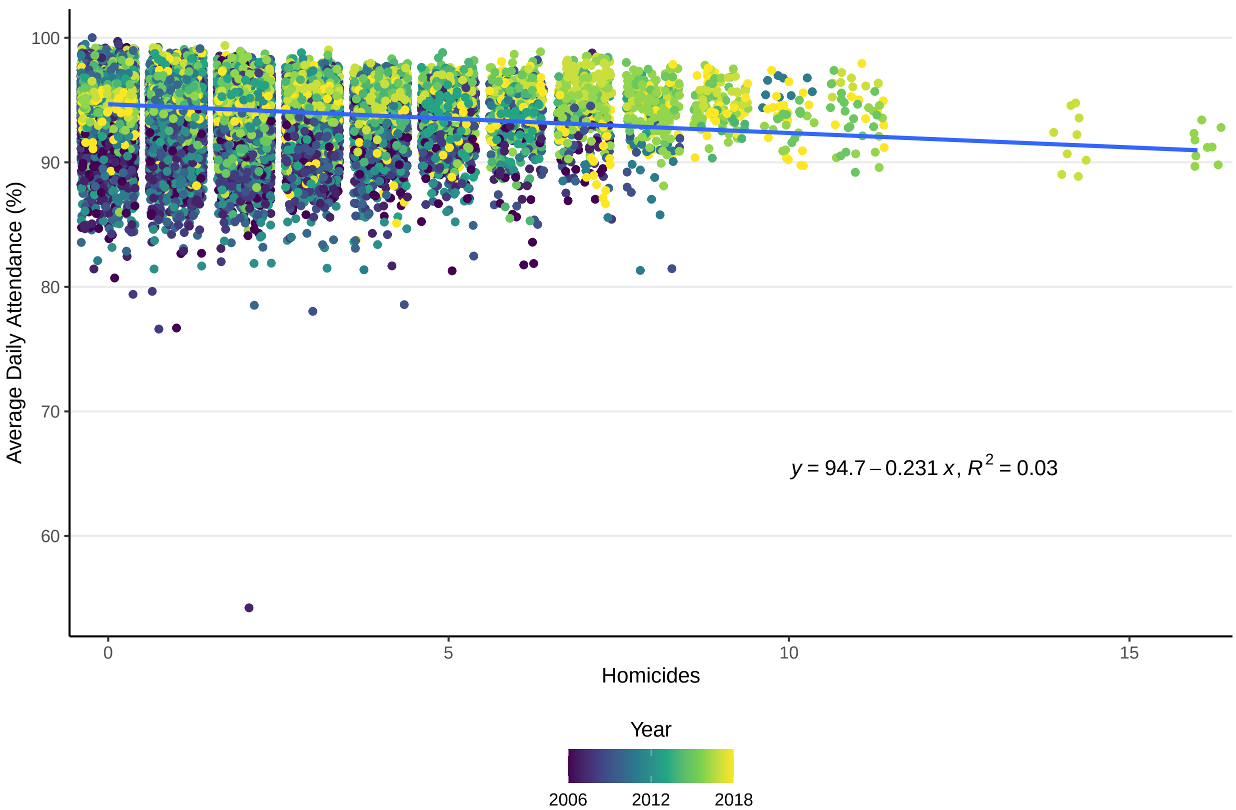
[1] This table shows data on how students travel to/from school in Chicago and how they feel about safety along their route to school. Rows of the table indicate whether the student walks or bikes either to or from school and the columns indicate how they feel about safety. Elements of this table are reported in percent. Question specific survey weights are used, but there is a low number of underlying respondents after conditioning on Chicago.
[2] Data from the National Household Travel Survey of 2009.

Figure 15: Elementary School Attendance by School Year



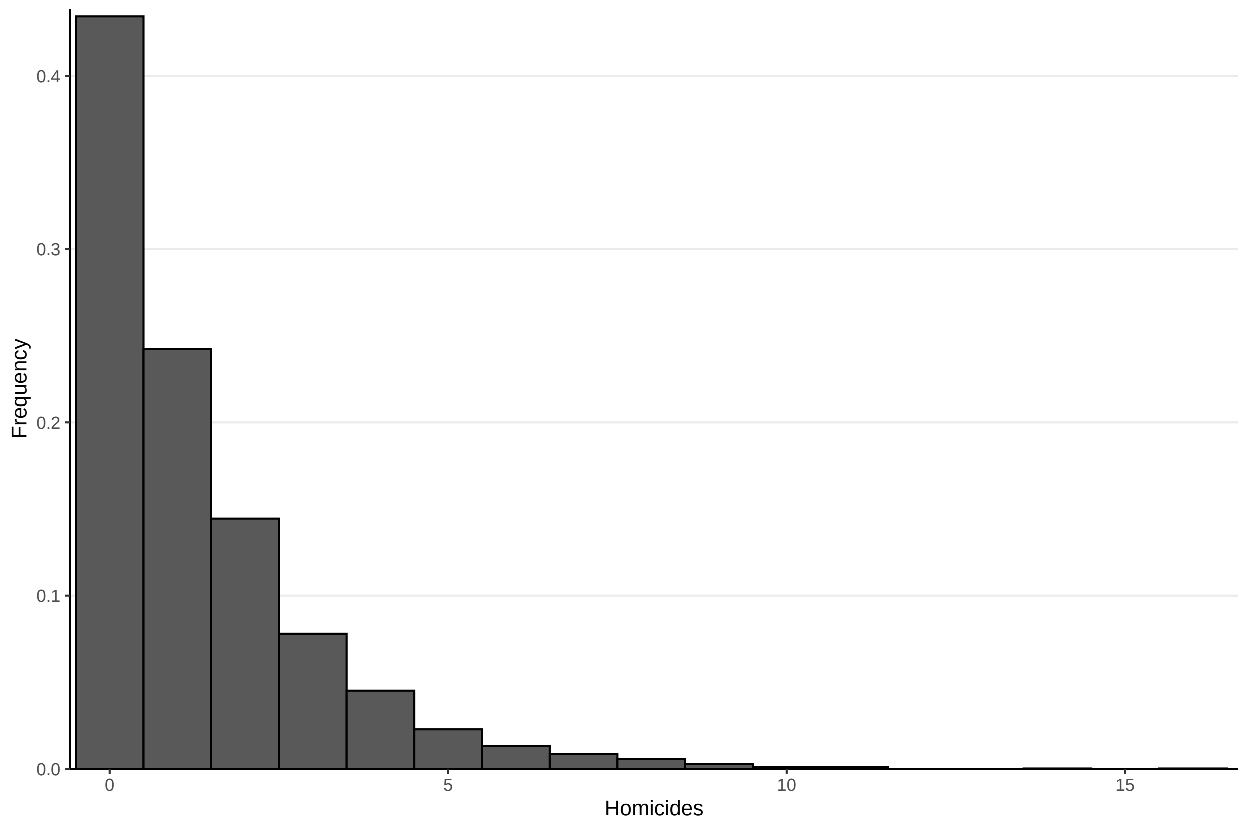
This figure contains box plots by year for average daily attendance. Observations are at the school-grade level. This figure is created using data from all cleaned elementary schools.

Figure 16: Elementary School Attendance and Murders Scatter Plot



This figure plots homicides on the x-axis against average daily attendance on the y-axis. Each point in the scatter plot is a school-grade-year observation with the color indicating the school year. This figure is created using data from all cleaned elementary schools.

Figure 17: Elementary School Homicides Distribution



This figure plots the homicide distribution for the cleaned elementary school data. The x-axis shows the number of homicides a school-grade is exposed to in any given year and the y-axis is the frequency.

Table 6: Attendance Estimates

	Homicides		
	1	2	3+
SPP	-0.067	0.326***	0.205
	(0.049)	(0.103)	(0.146)
No SPP	-0.063	0.326***	0.233
	(0.050)	(0.105)	(0.155)
Observations	6,972	5,923	5,730

[1] This table reports the treatment effect estimates from Figure 8. Columns indicate the number of homicides. The first row includes Safe Passage Program schools and the second row excludes them.
[2] * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 7: Enrollment Estimates

	Homicides		
	1	2	3+
SPP	-0.304	-1.863**	-1.703*
	(0.397)	(0.842)	(0.967)
No SPP	-0.325	-1.789**	-1.677*
	(0.401)	(0.865)	(1.013)
Observations	6,972	5,923	5,730

[1] This table reports the treatment effect estimates from Figure 9. Columns indicate the number of homicides. The first row includes Safe Passage Program schools and the second row excludes them.
[2] * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 8: Attendance and Enrollment Estimates, Conditional

	Homicides		
	1	2	3+
Attendance	-0.061	0.359***	0.281*
	(0.057)	(0.124)	(0.167)
Enrollment	-0.186	-1.146	-0.809
	(0.425)	(0.996)	(1.061)
Observations	6,172	5,158	5,100

[1] This table reports the treatment effect estimates from Figure 10 and Figure 11. Columns indicate the number of homicides. Both rows exclude Safe Passage Program schools.
[2] * significant at 10%, ** significant at 5%, *** significant at 1%.