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Do more eyes on the street reduce Crime? Evidence from Chicago's safe passage program[☆]

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ABSTRACT

Chicago's Safe Passage program attempts to ensure the safety of student traveling to and from schools by placing civilian guards along specified routes. The program was launched during the 2009-2010 school year and was expanded to 140 schools by 2015-16. We use data from more than 10 years of geocoded Chicago police reports and school level data to analyze the Safe Passage program's effects on crime rates and the rate of absenteeism from schools. Our findings suggest that the program is an efficient and cost effective alternative way of policing with direct effects on crime and student's outcomes. Exploiting both spatial and temporal variation in the implementation of the program, we find that the presence of guards results in lower levels of crime, with violent crime declining by 14% on average. The rate of absenteeism is estimated to decline by 2.5 percentage points. We find no evidence of spillovers of crime to areas that are not along the Safe Passage routes.

1. Introduction

Students routinely encounter a wide range of safety issues when commuting to and from schools across the country. According to Office of Juvenile Justice and Delinquency Prevention, most (63%) violent crimes committed by juveniles occur on school days and nearly one-fifth (19%) of juvenile violent crimes occur in the 4 h between 3 p.m. and 7 p.m. on school days. Additionally, the rate of victimization at schools is high, with 60.6% of nonfatal victimization for students aged 12–18 occurring at school in 2015 (National Crime Victimization Survey). Studies such as Mathews et al. (2009), Schwartz and Gorman (2003), Grogger (1997), and Billings and Phillips (2017) have found that exposure to crime, especially violent crime, may affect educational achievement, and implications for long-term outcomes have been established by Braga et al. (2012), Nagin (2013), Lum and Koper (2014), and Chalfin and McCrary (2017). Increasing public safety and crime prevention has long been at the center stage of policy debate. Previous empirical studies suggest that increasing or redeploying of police to specific geographic areas (or "hotspots") is an effective means of reducing crime.[1] However, most of these studies restrict their analysis to police enforcement practices, such as short term exogenous changes in the deployment of police following a terror attack (Di Tella and Schargrodsky, 2004; Klick and Tabarrok, 2005; Draca et al., 2011), or short term randomized experiments, such as "crackdowns" (Weisburd et al., 2009; Braga et al., 2012; Lum and Koper, 2014). Research on social interaction and safety suggests that community involvement can help reduce crime (Krivo, 2014).

This paper examines an alternative way of policing to increase student safety: hiring civilians to guard schools for a few hours each day. To study this alternative strategy, we use the Chicago Safe Passage program. The program places civilian guards around schools during arrival and dismissal times. The Safe Passage program is jointly run by the Chicago Public School and the Chicago Police Department, along with community organizations. The Safe Passage program began with 35 schools in the 2009-2010 school year and has expanded to cover

[1] See for example Braga et al., 2012; Nagin, 2013; Lum and Koper, 2014; Chalfin and McCrary, 2017.

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about 20% of Chicago public schools in the 2015-2016 school year. Safe Passage guards are expected to be knowledgeable of the community area they serve. The guards receive a background check and are trained on various de-escalation strategies and safety protocols. The guards patrol streets designated as “Safe Passage Routes” for approximately two and a half hours in morning and afternoon times when students commute to and from school. The guards wear neon jackets, and the designated routes have signs indicating that they are Safe Passage Routes.

Previous studies have criticized such “hot spot” policing because 1) any crime reduction may only be short term, and 2) there may be spatial spillovers to neighboring areas. We evaluate these two criticisms in the context of the Safe Passage Program. More specifically, we analyze the effectiveness of this alternative policing strategy in reducing crime, whether the effects are persistent, and whether the program leads to spatial spillover of crime.

The key challenge in estimating the effects of Safe Passages on crime is identifying the counterfactual scenario, i.e. what would have happened to crime if guards were not present? The exact location of these Safe Passage routes allows us to exploit variation in crime within adjacent small geographic areas. We combine detailed geo-located crime data with the location of guards and exploit the timing of the start of the program and the location of the guards to estimate the effect on crime. The exact start date of the program and the duty times of the Safe Passage guards allows us to control for preexisting differences.

Our results suggest that the presence of guards is effective at reducing crime in the surveilled areas, and that crime is not displaced to nearby areas. Guarded areas experience a significant reduction in crime as compared to neighboring areas, with the effect being strongest for violent crime at 14%. The effect is limited to the times of day when they are on duty. The sharpest reduction in violent crime is associated with early Safe Passage routes, while the expansion of the program is the time when property crime is reduced. Similar to results found in Sanfelice (2018), we find that the decline in crime is stronger for high schools as compared with elementary and middle schools.[2] Moreover, our results are consistent with the finding that place-based initiatives do not generally lead to spatial displacement (Sorg et al., 2013), and in the relatively rare cases where displacement does occur the spillover effects are small (Guerette and Bowers, 2009).

An important difference between the policing variation studied in previous research and our context is that the presence of Safe Passage guards is more likely to be permanent rather than temporary. This feature allows us to compare the long and short term effects. Sherman (1990) shows that the effectiveness of initial crackdowns declines over time, i.e., an “initial deterrence decay” that he suggests is caused by criminals learning over time that they had overestimated the risk of being caught. Consistent with Sherman’s (1990) theory of initial deterrence decay, Sorg et al. (2013) find evidence that deterrent effects of Philadelphia’s Foot Patrol experiment slowed down over the course of the experimental period, with the effect fading to zero after foot patrolling had continued for 22 weeks. In contrast, we find that Chicago’s Safe Passage program has had a persistent reduction in crime three years after the experiment began. We find that the effects are persistent over time and continue to lower crime throughout the implementation period. Schools that had the program for more than two school years show a significant reduction in crime, with an approximate 20% decline in violent crime.

In addition, we find improvements in attendance, with Safe Passage schools witnessing an annual rate of change in attendance of about 2.5 percentage points. To identify the effect of the Safe Passage guards on school attendance, we supplement our data with school level information. To address potential concerns of selection bias of the guarded schools, we use propensity score matching to find suitable controls. These results suggest that the presence of Safe Passage guards acts as a deterrent for criminals and helps to encourage students to attend schools more regularly, which also has an incapacitation effect. The Safe Passage program is a relatively cheap way of increasing safety.

Our results suggest that placing civilian guards around schools is both an inexpensive and effective way of increasing safety and attendance. Safe Passage guards work at an hourly wage of $10 for about 5 h a day on weekdays when schools are in session, which is a significant savings relative to the costs of training or redeploying additional police officers. Moreover, the program provides an interesting insight into policies aimed at reducing crime. While the program directly deters crime, it also increases the probability that students will attend school, which in turn reduces the number of potential perpetrators of the crimes. This reduction in the number of potential perpetrators is a form of “self-incapacitation”: time spent in school is time not spent in criminal activities (Tauchan et al., 1994; Jacob and Lefgren, 2003; Luallen 2006; Machin et al., 2011). The presence of guards will also produce quicker police response times, which is likely to increase the likelihood of arrests and eventual incarceration (Blanes I Vidal and Kirchmaier, 2017).

The guards are not equivalent to police, and they do not have the tools or training to incapacitate criminals. However, they do have the ability to intervene to defuse potential incidents, call 911, or simply make their presence known. Thus, our paper provides evidence of the effectiveness of an alternative policing strategy in which civilians are used for patrolling instead of police officers, and our findings can help guide policy makers around the country who have adopted or are considering adopting similar programs.[3] Our study is also relevant to the broader literature on private policing, and more specifically on the literature showing that university policing has the potential to significantly reduce crime rates (e.g., MacDonald et al., 2016 and Heaton et al., 2016).

The remainder of the paper is organized as follows: In Section 2, we provide background information on the Chicago Safe Passage program. Next, Section 3 presents our main results, including the effect of Safe Passage guards on crime and a cost-benefit analysis. In Section 4, we analyze the effect of the program on attendance rate, while Section 5 concludes.

2. Chicago’s safe passage program

The Chicago Safe Passage program started in the 2009-2010 school year with 35 schools. Since then, the program has been expanded to cover new schools almost every year, with about 20% of Chicago Public Schools (CPS) covered in the 2015-2016 school year.[4] The Safe Passage program is jointly run by the CPS and the Chicago Police Department (CPD), along with community organizations. In 2015-16, 22 vendors worked for the program. The vendors are responsible for on-ground enforcement of the program and hiring neighborhood residents to patrol the Safe Passage routes.

In this setting, “Safe Passage guards” are very different from police officers. They are civilians, primarily parents and grandparents of the children who attend the schools or community members who are knowledgeable of the local community area and interested in making the neighborhood safer. They are subject to thorough background

[2] Sanfelice (2018) finds similar declines in crime along Safe Passage routes for the initial 35 schools, which are mostly high schools. Her identification is similar to our robustness check presented in Section 4.5 where future Safe Passage route serve as controls. Our study is more general in timing and aim: rather than restricting the analysis to the initial set of schools, we examine the overall effect of the program on crime for all the schools that were part the program in the 2009-10 to 2015-16 school years, and we also analyze the effect of the program on attendance.

[3] Los Angeles, Philadelphia and New Britain (CT) have in place similar programs designed to offer safe routes in Public schools (https://www.cga.ct.gov/2013/rpt/2013-R-0396.htm).

[4] The CPS system comprises about 650 schools. Our analysis analyzes the expansion up to the 2015-2016 school year including crime data up to August of 2016.

Table 1
Safe passage program rollout.

School Year	No. passages	No. schools
2009-2010	35	35
2012-2013	3	4
2013-2014	51	55
2014-2015	32	39
2015-2016	3	7
Total	124	140

Source: Chicago Public Schools via Freedom of Information Act (FOIA) action.

checks and received specialized training, although neither the background checks nor the training are as rigorous as those expected of public police officers. Guards are trained during the summer to provide them with relationship-building skills, de-escalation strategies, and thorough knowledge of other safety protocols. This comprehensive training enables employees to proactively identify and report safety risks. The guards have strict protocols to follow in terms of reporting any crime or suspicious activity that they witness and have cellphones to enable them to report crime by either contacting 911 or a 24-h student safety number. Also, unlike public police, they are unarmed and do not have the power to make arrests. Employees work part time in the morning and afternoon when students commute to and from school.

The guards patrol "Safe Passage Routes" that are determined by identifying the paths that most students take when walking to school from their homes or bus stops. The preliminary route map is shared with parents, school personnel, and the local community to get feedback before deciding on the final routes. The catchment area of the guards is approximately one block along each of the routes. As of 2015–2016, the Safe Passage program employed about 1300 workers, who were paid approximately $10 per hour to work for about 5 h a day on weekdays when schools are in session. They work for two and a half hours in the morning and again in the afternoon around school dismissal time.[5] The total cost of the program was $17.8 million for the 2016 fiscal year.

The Safe Passage program was rolled out in three major phases. Table 1 shows the number of Safe Passages rolled out by school year and the number of schools they cover, while Fig. 1 shows the location by roll-out year.[6] The program was rolled out in three major phases. The program was introduced in the 2009-2010 school year, when 35 schools in areas with relatively high crime rates became part of the program. The largest and most advertised expansion took place in the 2013-2014 school year. In that year, 50 schools were closed and schools receiving these students, designated as "welcoming" schools, were added to the program.[7] The expansion of the program to these schools was the response to some safety concerns for the children who had to switch schools and, in their commuting, may have to cross gang boundaries. The last major program expansion was in 2014–2015 school year covering 39 additional schools.

Prior to the implementation of the Safe Passage program in the 2009-2010 school year, the CPS rolled out the pilot program in 2006-2007 and 2007-2008 school years, covering around 20 high schools. The pilot program proposed two strategies aiming to increase safety in and around the selected high schools. The first strategy involved patrolling and monitoring areas surrounding the high schools between 1 p.m. and 5 p.m. on school days. Secondly, micro-pod cameras were installed, with officers serving as monitors during afternoon school hours. According to research carried out by the Chicago Police Department, the pilot program led to a 20% decline in criminal incidents around Safe Passage schools, a 27% drop in incidents among students, and a 7% increase in attendance over the past two years in high schools that implemented the pilot program.

3. Data sources

Our empirical analysis is based on crime incident reports, Safe Passage location data, characteristics of the schools, and census block groups. The crime incident data are based on police reports between January 2001 and August 2016, as provided by the City of Chicago Data Portal. This information was extracted from the Chicago Police Department's Citizen Law Enforcement Analysis and Reporting (CPD CLEAR) system. The data set provides the date, time, classification of the type of crimes committed.[8]

The classification of each incident follows the Illinois Uniform Crime Reporting (IUCR) code, which is compliant with the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting (UCR) program. All crimes are classified into categories following a hierarchy. FBI's UCR program only collects statistics on violent and property crime, with violent crime having the highest hierarchy followed by property crime. The hierarchical categorization also implies that in case of multiple offenses, the incident is classified as one which is the highest in the hierarchy.[9] As a result of this classification procedure, reports for crime lower in the hierarchy will be biased downwards. We restrict our attention to violent and property crimes because they have higher priorities in the coding and thus are more likely to be reported to the police.[10]

The data set has several limitations. First, the CPD CLEAR data set reflects only incidents in which the police responded and completed a case report. Thus, it reflects the number of reported crimes rather than being an exhaustive list of the number of incidents. A second limitation is that there are some recording errors in the reports data set regarding the precise date and time of the incident. If the address of the incident is not present we exclude the observation from the final data set. Crime incidents are recorded on the hour when the reporter cannot reasonably estimate the exact time of the crime.[11]

Data on the schools and the Safe Passage routes were obtained through the CPS web site and the City of Chicago Data Portal. The school data includes demographic information for the student body, the proportion of students eligible for free lunch, the proportion of students

[5] We do not have data on actual police deployment along the Safe Passage Routes. According to our discussions with CPS, there was no additional deployment of police in these areas. Although it is possible that there is some additional police monitoring of these areas, our discussions indicate that there was no increase in police officers after the roll out of the program. Additionally, regressions for the number of police officers in a district do not indicate any increase after the implementation of the Safe Passage Program, as shown in Table A4.

[6] Appendix Fig. A1 shows each Safe Passage roll out phases separately. Since some schools where close to each other, some Safe Passages cover more than one school.

[7] A map of the welcoming schools can be found here: http://cps.edu/qualityschools/Pages/WelcomingSchoolsMap.aspx (last access April 11, 2018).

[8] The last two digits of the address are withheld in the crime data, which means that addresses are recorded at approximately the block level.

[9] For example, if a burglar breaks into a house and steals several items and hurts the homeowner, the incident is classified as violent, although it also includes a property crime.

[10] Violent crimes are defined by the FBI's UCR as those that involve force or threat of force and include murder and non-negligent manslaughter, forcible rape, robbery, and aggravated assault. Property crime includes burglary, larceny-theft, motor vehicle theft, and arson.

[11] Burglary and vehicle crime are among the most common types of crime where the exact time of occurrence is most likely to be unknown because the crime may happen when the victim is not present. Thus, many agencies record the time as the range of time from the point when the victim was last home until the crime was discovered. Some methods such as aoristic analysis have been suggested to overcome the imprecision inherent in spatial temporal crime data (Ratcliffe, 2000).

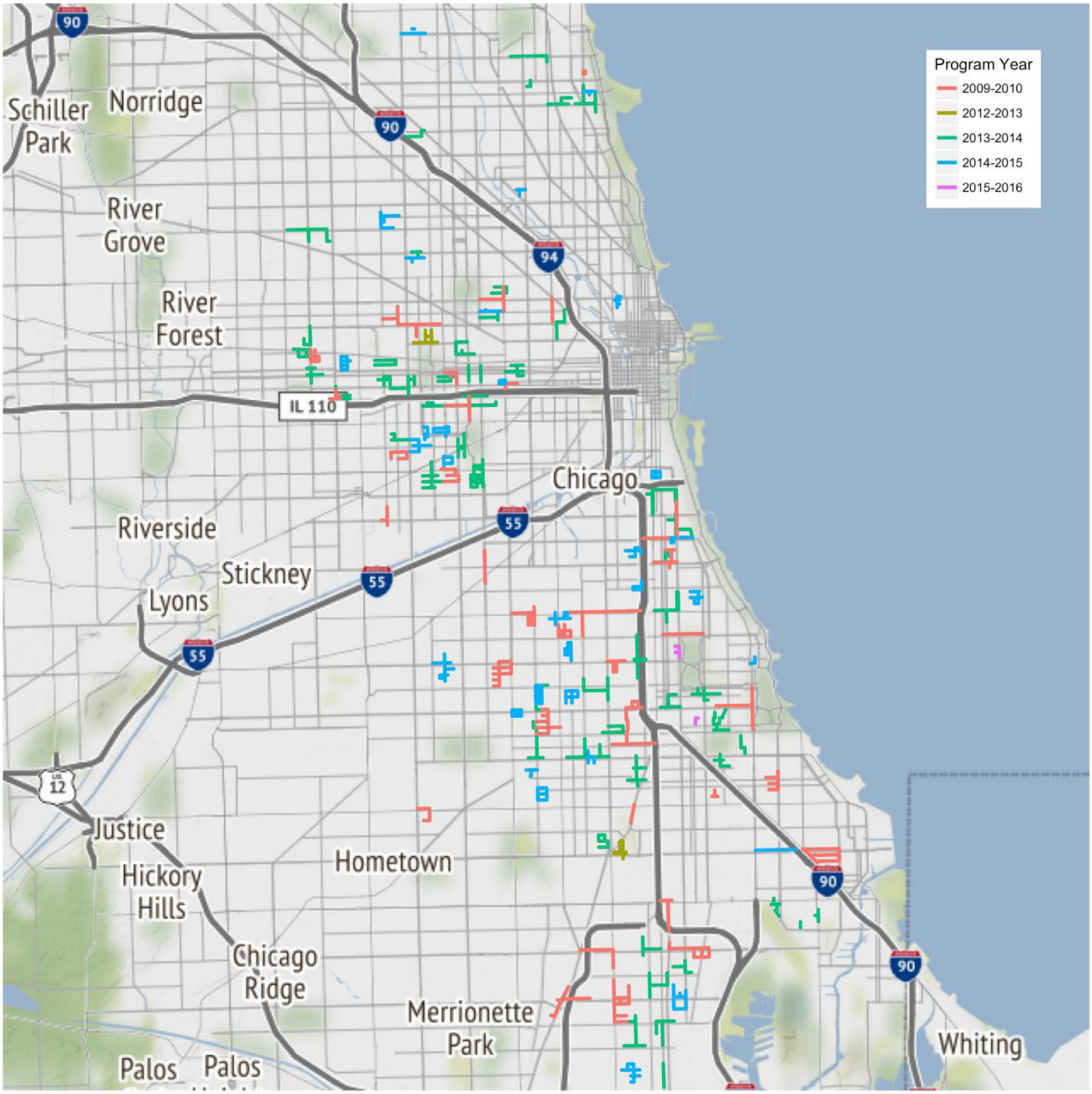


Fig. 1. Safe Passage Routes, by year of program adoption.
Note: Shapefiles with Safe Passage shape and location where obtained from the Chicago Data Portal and year that the program was launched at each location through a FOIA request.

who are bilingual, and overall attendance records. Shapefiles with the location of the Safe Passage routes are available through the City of Chicago Data Portal. The information on the year in which the Program was started in each of the schools was obtained from the Chicago Public School via Freedom of Information Act (FOIA) action.

Finally, we also use the American Community Survey (ACS) for the period 2009–2014 to obtain data on census block group characteristics. The demographic data include median income, average education, unemployment rates, poverty rates, and housing characteristics. Additionally, we use community area and census tract boundaries to control for varying time trends. Community areas, census tract boundaries, and data on traffic counts come from the City of Chicago Data Portal. We use the Census 2010 definitions for the Census Tract boundaries.

4. Do more eyes on the street reduce crime?

4.1. Empirical strategy

Our objective is to identify the change in crime due to the presence of Safe Passage guards. In an ideal setting, an experiment could be conducted by randomly placing guards near some schools and not others. In this setting, we would unambiguously identify the effects of guards on crime by comparing how crime changes in areas that are guarded relative to unguarded areas. The main concern with our non-experimental setting is the fact that schools were not randomly chosen to participate in the program. Tables 2 and A14 shows that more vulnerable schools in high crime, low-income neighborhoods were chosen to be part of the program.

Table 2
Chicago public schools with and without safe passages.

	Descriptive statistics		
	CPS with Safe Passages (1)	CPS without Safe Passages (2)	Diff. (3)
Attendance (in 2008)	87.24	93.44	−6.20***
	(10.85)	(4.77)	(0.68)
Total enrollment	677.95	672.83	5.13
	(480.54)	(461.13)	(45.83)
Prop. white	1.98	10.87	−8.89***
	(3.32)	(17.18)	(1.48)
Prop. African American	75.06	45.01	30.04***
	(34.65)	(41.08)	(3.89)
Prop. Hispanic	21.90	40.20	−18.30***
	(32.36)	(36.36)	(3.48)
Prop. bilingual	0.90	0.56	0.35***
	(1.01)	(0.80)	(0.08)
Prop. Individualized education program	0.14	0.12	0.02*
	(0.12)	(0.10)	(0.01)
Prop. free lunch	0.94	0.84	0.1***
–	(0.08)	(0.21)	(0.02)

Note: The table presents descriptive statistics for Chicago Public Schools (CPS) with Safe Passages (column 1) and without Safe Passages (column 2). Column (3) presents the difference in means.
$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

The correlation between crime and both observable and unobservable characteristics of the Safe Passage schools thus poses a challenge for the identification of the causal effect of Safe Passage guards on crime (see Table A14). To overcome this issue, we focus on small geographic areas around Safe Passage routes, namely, cells of one eight by one eight mile (i.e. 1/64 of a square mile) in the neighboring areas of a Safe Passage school.[12] Fig. 2 illustrates our gridding strategy for the designated Safe Passage route associated with Kelly High School. Cells that have a Safe Passage route are designated as *Safe Passage Cells*. To avoid unbalanced location of treated and control areas (Donohue et al., 2013), we construct our control areas as cells that are contiguous in any direction up to three cells over.[13] This strategy naturally implies a spatial difference-in -differences approach that compares crime counts in cells that are on a Safe Passage route with adjacent cells before and after the program started. The spatial difference-in-differences approach helps to account for confounding unobserved neighborhood attributes (Diamond and McQuade, 2016; Pope and Pope, 2015; Cui and Walsh, 2015; Ellen et al., 2013; Linden and Rockoff, 2008)

Leveraging the geolocation of crime, we match violent and property crime incidents to each cell. We identify violent and property crimes that take place during the day when Safe Passage guards are present, and also in evening hours (5:30 pm to 6:30 am) when guards are not present. We also distinguish between school and non-school days (i.e. weekends and summer months). Given the small size of the geographic areas, we aggregate the number of incidents to months to avoid an excess of zero counts. In our main specification, we exclude crime that occurred during weekends, night and summer break.

Our baseline specification is then:

$$\#Crimes_{it} = \beta\ Safe\ Passage\ Cell_i * Post_t + \ \theta\ One\ Cell\ Over_i * Post_t + \phi\ Two\ Cells\ Over_i * Post_t + \gamma_i + \delta_t + u_{it} \quad (1)$$

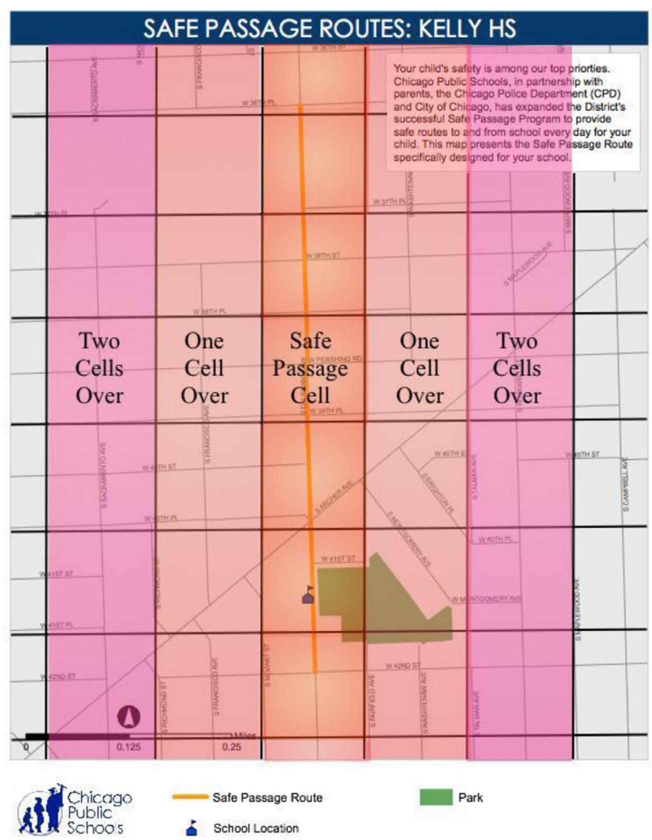


Fig. 2. Identification Strategy.
Note: Figure shows the neighborhood map available to the public on the CPS website. We overlay Cells of one eight by one eight mile (i.e. 1/64 of a square mile) that illustrates our identification strategy. Cells that are on a Safe Passage route are designated as Safe Passage Cells. Cells directly adjacent to the Safe Passage Cells, are designated “One Cell Over”, cells that are two cells from a Safe Passage Cell, are designated “Two Cells Over”.

[12] This approach is similar to Di Tella and Schargrodsky (2004), but we replace street blocks by cells of one eighth by one eighth mile. The advantage of this approach is that it gives us areas of equal size that are approximately the same length as a standard Chicago block. Moreover, given the small area of these cells we are confident that a guard standing on the Safe Passage route is able to monitor it. The results remain robust when we replace cells by street blocks (see Table A1).

[13] The cell definition has the added advantage that allows us to analyze the potential spatial displacement effects of crimes into neighboring areas. If the cell is “One Cell Over” to one Safe Passage and “Two Cell Over” to another Safe Passage, we denote the cell as “One Cell Over”.

Table 3
Effect of the Safe Passage Program on Crime, base results.

	Number of violent crimes			Number of property crimes		
	(1)	(2)	(3)	(4)	(5)	(6)
Safe Passage Cell*Post	−0.1437***	−0.1421***	−0.1410***	−0.0343	−0.0306	−0.0277
	(0.0316)	(0.0341)	(0.0370)	(0.0237)	(0.0242)	(0.0262)
One Cell Over*Post		0.0052	0.0062		0.0129	0.0158
		(0.0262)	(0.0294)		(0.0213)	(0.0231)
Two Cells Over*Post			0.0029			0.0084
			(0.0268)			(0.0187)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	508,376	508,376	508,376	552,896	552,896	552,896

Note: Columns show coefficient and standard errors (in parenthesis) from Eq. (1) using a Poisson regression of the number of crimes per cell (one eighth by one eighth of a mile) per month when schools are in session for the period January 2006–August 2016. Safe Passage Cell*Post equals one for cells that have a Safe Passage in the months after the program was enacted. The same is true for One Cell Over*Post and Two Cells Over*Post (for cells one cell away and two cells away from the nearest Safe Passage, respectively). Regressions include up to Three Cells Over. Standard errors are clustered by Safe Passage. The average monthly number of violent and property crimes in the preprogram period in the Safe Passage Cell were 0.17 and 0.53 respectively.
$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

where $\#Crimes_{it}$ is the monthly number of crimes (violent or property) in cell i in month-year t. $Safe\,Passage\,Cell_i$ is an indicator variable with a value of one for cells that have Safe Passage route. $One\,Cell\,Over_i$ is an indicator for cells that are adjacent to a Safe Passage Cell, and $Two\,Cells\,Over_i$ indicates that the cell is two cells from the Safe Passage Cell. We interact these spatial indicators with an indicator $Post_t$, which equals one after the Safe Passage program started in a school. We complete the specification with a cell fixed effect (γ_i), month-year fixed effects (δ_t), and the error term u_{it}. The omitted category is three cells over in our preferred specification. Thus, in the difference-in-differences specification, crime in treated cells is being compared to crime in three cells over.

The parameter of interest, β, captures the causal effect of the Safe Passage route on crime. The hypothesis is that cells on a Safe Passage route should see a decrease in crime at times that Safe Passage guards are present. The advantage of this approach is that by including spatial fixed effects and focusing on crimes before and after the Safe Passage implementation, we can difference out time invariant omitted variables. Our main identifying assumption is that Safe Passage Cells have similar underlying trends as the non-guarded adjacent cells. In Section 4.4, we provide evidence suggesting that this assumption holds.

As has become standard in the literature, we estimate count data models for Eq. (1) using a Poisson regression. However, our results do not change if we use other estimators such as negative binomial regression or ordinary least squares.[14] To account for the possible correlation between errors at the treatment level, we cluster our standard error at the route level in all of our regressions.[15]

4.2. Base results for crime

In this section, we present our main results for the overall effect of the Safe Passage program on crime. Table 3 presents our results for violent and property crimes.[16] We start with a basic specification in column (1) that shows a more basic measure of proximity to guard presence, $Safe\,Passage\,Cell_i{}^{*}Post_t$, which has a value one for every month after the program was implemented for every cell that has a Safe Passage route. This regression uses adjacent cells up to the third one over as controls. Results show that cells that have a Safe Passage route see a decrease in violent and property crime after the program implementation.[17] Violent crimes decline by 14.3% and property crimes by 3.4%, but the estimates for property crime are noisy.

Although these results show that crime goes down, a potential concern is that, instead of reducing crime, the presence of Safe Passage guards is displacing crime to the nearby unguarded areas. We test this hypothesis by adding controls for adjacent areas. Columns (2) and (5) control for spillovers in the first adjacent cell. Results for Safe Passage Cell remain unchanged, and suggest that violent crime in the adjacent cell did not change with respect to the control cells (i.e. cells that are two or three cells adjacent to the Safe Passage Cell).

The results of our preferred specification are presented in columns (3) and (6) of Table 3 and in Fig. 3. The coefficients on *One Cell Over* and *Two Cells Over* are insignificant across the specifications, suggesting that crime is not being displaced to the adjacent cells. Panel (a) of Fig. 3 shows that violent crime declines by 14.1% with no evidence of spatial displacement of crime. Panel (b) shows a decline in property crime in guarded cells and a small increase in adjacent cells, none of which are statistically significant.[18] The hierarchical classification procedure for crime might be a possible explanation for this insignificant estimate. Crimes are classified according to the highest category, with severe offenses classified as violent and less serious offenses classified as property crimes. If the severity of the crimes tended to decline after the implementation of the program, then a higher proportion of offenses

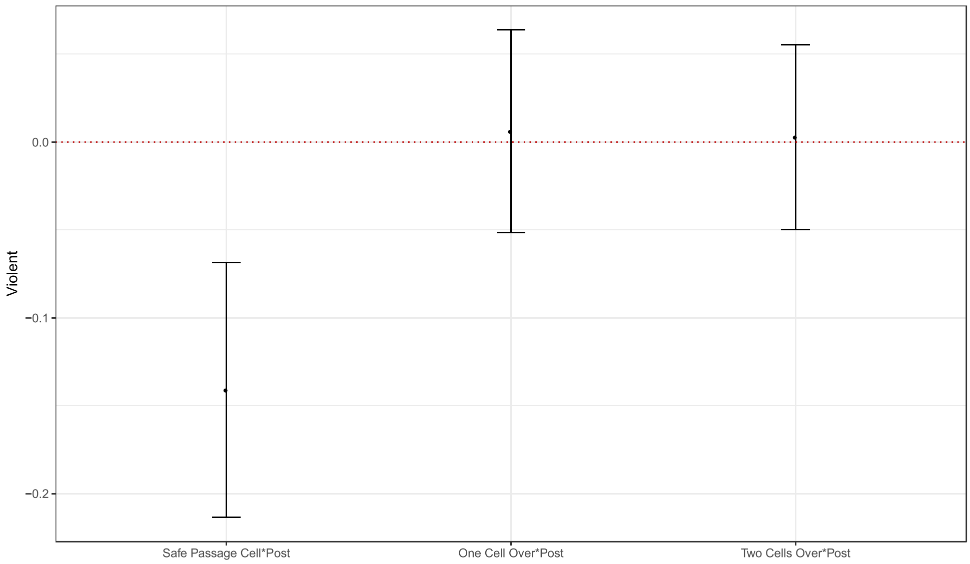
[14] See Table A2 for results using negative binomial regression and OLS.

[15] Our results also hold if we cluster at the cell level. The results are presented in Table A1 columns (2) and (6).

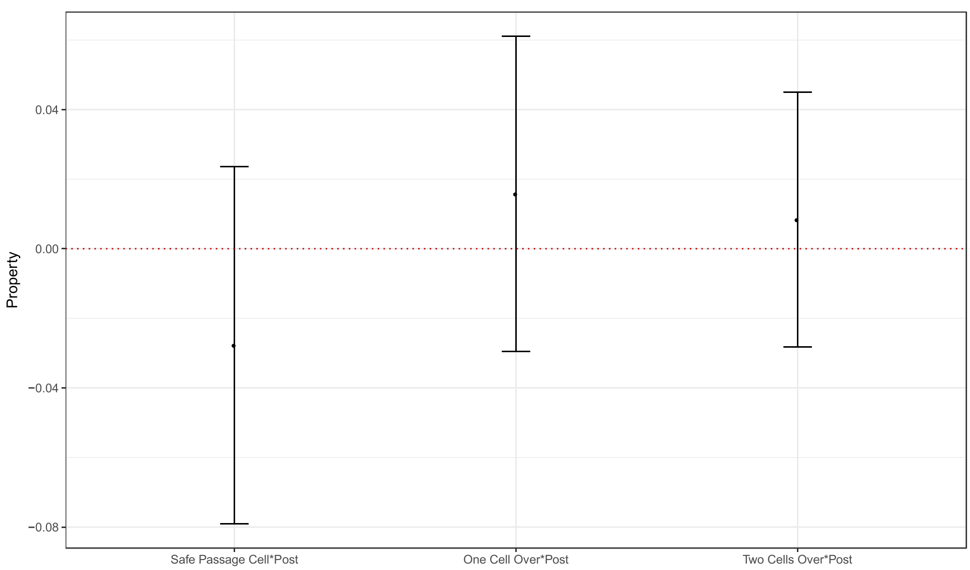
[16] We restrict our sample to the period January 2006–August 2016. The choice of pre-program period does not play a significant role. Results presented in Table A1 columns (3) and (7) are similar when using an extended sample (January 2001–August 2016).

[17] The interpretation of a difference-in-differences coefficient from a Poisson regression is $exp(\beta)-1$, although the approximation $exp\,(\beta)-1\approx\beta$ is valid for small enough β.

[18] Sorg et al., 2014 point out that police officers make adjustments to boundaries during an intervention, as observed in the Philadelphia Foot Patrol Experiment, which might cause researchers to incorrectly estimate the actual effect of the program and mis-measure the spatial displacement of crime. Our discussions with the CPS suggest that guards mostly stand on the Safe Passage routes assigned to them. However, if they stray into the neighboring areas, our estimates might be a lower bound of the true estimates as the decrease in crime will be lower in the treated area. We do not this issue to affect crime three cells over as these areas are far enough away from the Safe Passage routes that it is unlikely that guards would stay to those areas.



a:



b:

Fig. 3. Effect of the Safe Passage Program on Crime. a: Violent Crimes. b: Property Crimes. *Note:* Point estimates and 95% confidence intervals for estimating Eq. (1) using dummy variables indicating proximity to Safe Passage route. Safe Passage Cell*Post indicates the cell is in a Safe Passage route after the program was implemented during week days when school is in session. One Cell Over*Post indicates that the cell is one cell adjacent to a Safe Passage Cell after the program was implemented. Similarly, Two Cells Over*Post indicates that the cell is two cells adjacent to the Safe Passage Cell. The ommitted category is Three Cells Over * Post.

will tend to be classified as property crime, and as a result, there may be some increase in the number of property crimes after the program started. Moreover, our estimated effects for property crime might be a lower bound due to over reporting of crime in the presence of guards. Na and Gottfredson (2013) find that as schools increase their use of police officers, the percentage of crimes involving non-serious violent offenses that are reported to law enforcement increases.

We use Poisson models for our preferred specification. However, we show in Appendix Table A2 that our results are robust to alternative specifications such as Negative Binomial regression and OLS. We focus on the number of violent and property crimes rather than per capita rates for several reasons. First, our objective is to analyze how the program affects the number of incidents rather than the intensity of crime for a given number of people. Second, we are interested in analyzing very small geographic areas; the zones can include areas where residents do not live even though they may travel through the zones frequently. Third, precise population estimates for such a small geographic level is not available at a monthly frequency. Moreover, Ihlanfeldt and Mayock (2010) argue that crime per unit of land is a better measure of crime intensity than crime rates when analyzing geographic areas smaller than city level. However, for robustness we also estimate OLS models with crime rates as the dependent variable (Appendix Table A2). We use two alternative measures of crime rates by dividing counts of crime by population in the cell and traffic counts.[19] The estimates are consistent with our earlier results.

Crime rates and trends vary substantially across Chicago neighborhoods (Papachristos, 2013). To account for this variation and to verify that our results are not driven by time-varying neighborhood trends we include various trends in Appendix Table A3. Our specifications in-

[19] We use the population estimates from census and traffic counts from Chicago Data Portal. We assume uniform population density in the census tract. We estimate population by multiplying the ratio of area in the cell to census tract by population in the tract.

Table 4
Effect of the Safe Passage Program on Crime, spillovers.

	Number of violent crimes				Number of property crimes			
	(1)	(2)	(3)	(5)	(6)	(7)	(8)	(10)
Safe passage cell*post	−0.1542***				−0.0350			
	(0.0363)				(0.0259)			
One cell over*post		−0.0012		−0.0064		0.0130		0.0168
		(0.0267)		(0.0299)		(0.0221)		(0.0244)
Two cells over*post			−0.0098	−0.0125			0.0043	0.0103
			(0.0255)	(0.0286)			(0.0183)	(0.0204)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	246,874	407,888	407,888	407,888	267,968	450,818	450,818	450,818

Note: Columns show coefficient and standard errors (in parenthesis) from Eq. (1) using a Poisson regression of the number of crimes per cell (one eighth by one eighth of a mile) per month when schools are in session for the period January 2006–August 2016. Safe Passage Cell*Post equals one for cells that have a Safe Passage in the months after the program was enacted. The same is true for One Cell Over*Post and Two Cells Over*Post (for cells one cell away and two cells away from the nearest Safe Passage, respectively). Regressions include up to Three Cells Over. Standard errors are clustered by Safe Passage. Column (1) and (6) we drop cells that are Once Cell Over and Two Cells Over, that is we keep only Safe Passage Cells and Three Cells Over. In the remaining columns we drop Safe Passage Cells.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

clude alternative definitions of neighborhoods, including community areas and census tracts, which are interacted with month-year dummies. We also include police district or police beat dummies interacted with month-year dummies to account for heterogeneity in policing strategies. Our findings are robust to these alternative specifications.

As the program was expanded to include more schools, some Safe Passages became very close to each other. As a result, some cells may contain more than one route, and thus might have been more intensely guarded. We re-estimate Eq. (1) controlling for the intensity of treatment by including a variable representing the number of Safe Passage routes in a cell.[20] Results are consistent with our previous findings and are shown in Appendix Table A6. Thus, our findings do not appear to be driven by areas with more intensive guarding.

4.3. Additional results for crime

In this section, we provide further evidence that our results are not spurious. A possible concern could be that crime is actually simply being displaced. We explore this hypothesis and do not find evidence of spatial displacement. In columns (1) and (6) of Table 4, we present estimates of Eq. (1) after dropping the first and second adjacent cell. This approach creates a buffer area between treatment and control areas. Point estimates suggest that our previous findings are not driven by an increase in crime in the adjacent areas. In the remaining columns, we assess the impact of the program on the adjacent areas. For these specifications, we drop the treated area, i.e. cells with Safe Passage routes. Results show no significant changes in crime in the adjacent areas as a consequence of the presence of guards, giving us reassurance that crime is not being displaced to nearby areas.

Another potential problem is the possibility of time-varying unobserved characteristics that have a different effect on crime in Safe Passage Cells relative to adjacent cells. Differential effects could occur if, for example, the city chose to invest in areas with Safe Passage Cells by securing and/or demolishing buildings, cleaning vacant lots, removing instances of graffiti, replacing and repairing street lights, etc. In such an event, a decline in crime in Safe Passage cells relative to adjacent cells could have been produced indirectly by improvements in the conditions of these areas (Aliprantis and Hartley, 2015), rather than as a direct result of the presence of the program's guards.

If these general improvements in the condition of Safe Passage cells are the actual source of the reduction in crime, there should not be any differential effect on crime for times when Safe Passage guards are present relative to times they are not. To test this potential concern, we use the information on the timing of incidents and run the same specification as Eq. (1) for times when guards are not present – night times (5:30 pm–6:30 am), summer months when schools are not in session (July and August), and on weekends.

Results shown in Table 5 suggest that the presence of Safe Passage guards is the source of the reduction in crime rather than general improvements in the Safe Passage route areas. Columns (1) and (4) summarize the results for night times, columns (2) and (5) present the results for summer months, columns (3) and (6) present the results for weekends, and columns (4) and (8) presents the results for all three placebo times combined. None of the results is statistically significant at conventional levels of significance.

Our results could also be questioned if the city had increased police presence in the Safe Passage areas. Responses from the city could have either been on the extensive or intensive margin i.e. increase in the number of police officers in districts with Safe Passages or, redeploying forces within districts.[21] Table A5 shows that our results remain the same when controlling for the number of officers in the district. Finally, it could be that police forces are redeployed within the district. We cannot test this hypothesis as data on the time and location of police deployment is not available, but our discussions with CPD and CPS suggest that there was no additional deployment of police officers in the Safe Passage areas. Overall, our results measure the effect of the Safe Passage program as implemented, inclusive of any police redeployments that may have occurred.

To explore the effects over time, we perform an event study analysis. We classify cells by bins of school years with respect to when the program started. Fig. 4 shows point estimates and 95% confidence intervals. We see similar patterns in violent and property crime: before the

[20] There are a few cells that have more than two Safe Passage routes running through them. Thus, we lose power when we try to estimate the varying effects by intensity of treatment.

[21] We obtained data from the CPD and found that there were not more police officers in a given police district as a result of the number of Safe Passage in the district, results are shown in Table A4. These data were obtained by a FOIA request and the sample period was restricted by the records kept by CPD. The data contains the number of sworn Chicago police personnel by police district and by month between January 2008 and September 2016.

Table 5
Effect of the Safe Passage Program on Crime, unguarded times.

	Number of violent crimes				Number of property crimes			
	(1) Night	(2) Summer	(3) Weekend	(4) All	(5) Night	(6) Summer	(7) Weekend	(8) All
Safe passage cell*post	0.0134	0.0327	0.0091	0.0156	−0.009	−0.034	−0.005	−0.0111
	(0.0273)	(0.0566)	(0.0457)	(0.0220)	(0.0253)	(0.0433)	(0.0388)	(0.0246)
One cell over*post	0.0235	0.0603	−0.0225	0.0272	−0.0011	−0.0406	−0.002	−0.0045
	(0.0246)	(0.0496)	(0.0379)	(0.0200)	(0.0204)	(0.0323)	(0.0304)	(0.0200)
Two cells over*post	0.0241	0.0469	0.0171	0.0194	0.0146	−0.0071	0.0372*	0.0099
	(0.0238)	(0.0522)	(0.0380)	(0.0199)	(0.0169)	(0.0269)	(0.0226)	(0.0158)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	521,308	77,220	419,548	2088,464	547,384	109,956	533,392	2141,244

Note: Columns show coefficient and standard errors (in parenthesis) from Eq. (1) using a Poisson regression of the number of crimes per cell (one eighth by one eighth of a mile) per month when schools are not in session for the period January 2006–August 2016. Safe Passage Cell*Post equals one for cells that have a Safe Passage in the months after the program was enacted. The same is true for One Cell Over*Post and Two Cells Over*Post (for cells one cell away and two cells away from the nearest Safe Passage, respectively). Regressions include up to Three Cells Over. Standard errors are clustered by Safe Passage. Each column corresponds to a particular time frame referred to in the heading. Night time refers to incidents that took place between 5:30 pm to 6:30 am. Columns (2), and (6) Summer months, and (3) and (7) for Weekends we omit night times. Columns (4) and (8) combine all times that guards are not present. Standard errors clustered by Safe Passages are reported in parentheses.
$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

adoption of the program we do not observe any significant differences in cells that are on a Safe passage route when compared to our control group.[22] This provides suggestive evidence of our main identifying assumption, which is that trends evolved similarly in cells that are on Safe Passage routes and in adjacent cells. Effects on violent crime are almost immediate, with violent crime decreasing by 10% in the implementation year, 11% in the first year after implementation, and 19% from the second year onward. For property crime, the effect takes about a year to appear, but we see decreases in the second year of implementation of around 7% and 10% from the third year on. These results suggest that at least within three years we see no "deterrence decay" (see Sherman, 1990; Sorg et al., 2013) on the effects of the program.[23]

4.4. Robustness for Crime: differential trends

The identification of the effects of Safe Passage guards on crime depends on the assumption that in the absence of guards crime would have evolved similarly in the treated and control areas. Although we cannot test this assumption directly, in this section we provide evidence that supports our assumption.

Descriptive evidence in Fig. A2, shows that the program was indeed implemented in areas with higher crime incidents but there are no obvious differences in trends before the program implementation. Control cells show no significant differences in levels or trends after the program was implemented. Additionally we include in Eq. (1) an indicator Safe Passage Cell * One year pre-event which equals one for one year before the area had a Safe Passage route. Similarly, we define Safe Passage Cell * Two year pre-event for two years before the area had a Safe Passage route. The results are summarized in Table 6: none of the pre-event coefficients is significant, indicating absence of trend before the event. These results are consistent with our event study analysis presented in Fig. 4, and suggests that there is no systematic difference in trends between the treated and control groups before the event.

As a robustness check, we also take advantage of the length of our crime data set to conduct a placebo Safe Passage program in pre-program periods. By going back five, six or seven years prior to the implementation of the program we can simulate the entire program without any overlap with the actual start dates. Thus, for example, for five years prior, we define the start of the program as 2004-2005 rather than the actual start in 2009-2010, with expansions coming at the same intervals following 2004-2005 as those following the 2009-2010 start date. The results, which are summarized in the Appendix Table A7, show that Safe Passage routes were effectively placed in high crime corridors, but the lack of decline in crime in the placebo years suggests that the decline we see in our main results is in fact due to the Safe Passage program. Overall, these results suggest that our findings are not caused by pre-trends.

4.5. Alternative Strategies: different control groups

A potential concern with our spatial difference-in-differences strategy could be that our control areas are slightly "farther away" from the school and may be fundamentally dissimilar to areas near schools. To mitigate this concern, we show that our results are not sensitive to the choice of control groups.

Our first approach is to identify control areas by using propensity score matching (Rosenbaum and Rubin, 1985). We choose the two closest neighbors to the treated cell with common support as controls. Our match is based on three broad categories: pre-program crime counts, school characteristics of the school close to that cell, and census block group characteristics.[24] We match the neighboring schools to the cell and classify the schools as either in the cell, one cell, or two cells ad-

[22] The p-value of the joint test that all the pre-program coefficients are equal to zero is 0.8946 for violent crime and 0.0724 for property crimes.

[23] We thank one of the referees for pointing us to this literature.

[24] For crime, we use the total number of violent and property crime in the cell during the period 2001–2008. For school characteristics, we assign the average characteristics of the adjoining schools to the cell. We also include school characteristics in identifying counterfactual cells, including the proportion of students eligible for individualized education programs, the proportion receiving free lunches, the share of students who are bilingual, and the percentage of African American, and percentage of Hispanic students. In addition to the characteristics of the schools, we augment our data with census block group characteristics like demographics, education, unemployment rate and housing characteristics coming from the 2009–2014 ACS (5 year estimates). When a cell belongs to multiple census blocks, our algorithm assigns the cell to one of the census blocks.

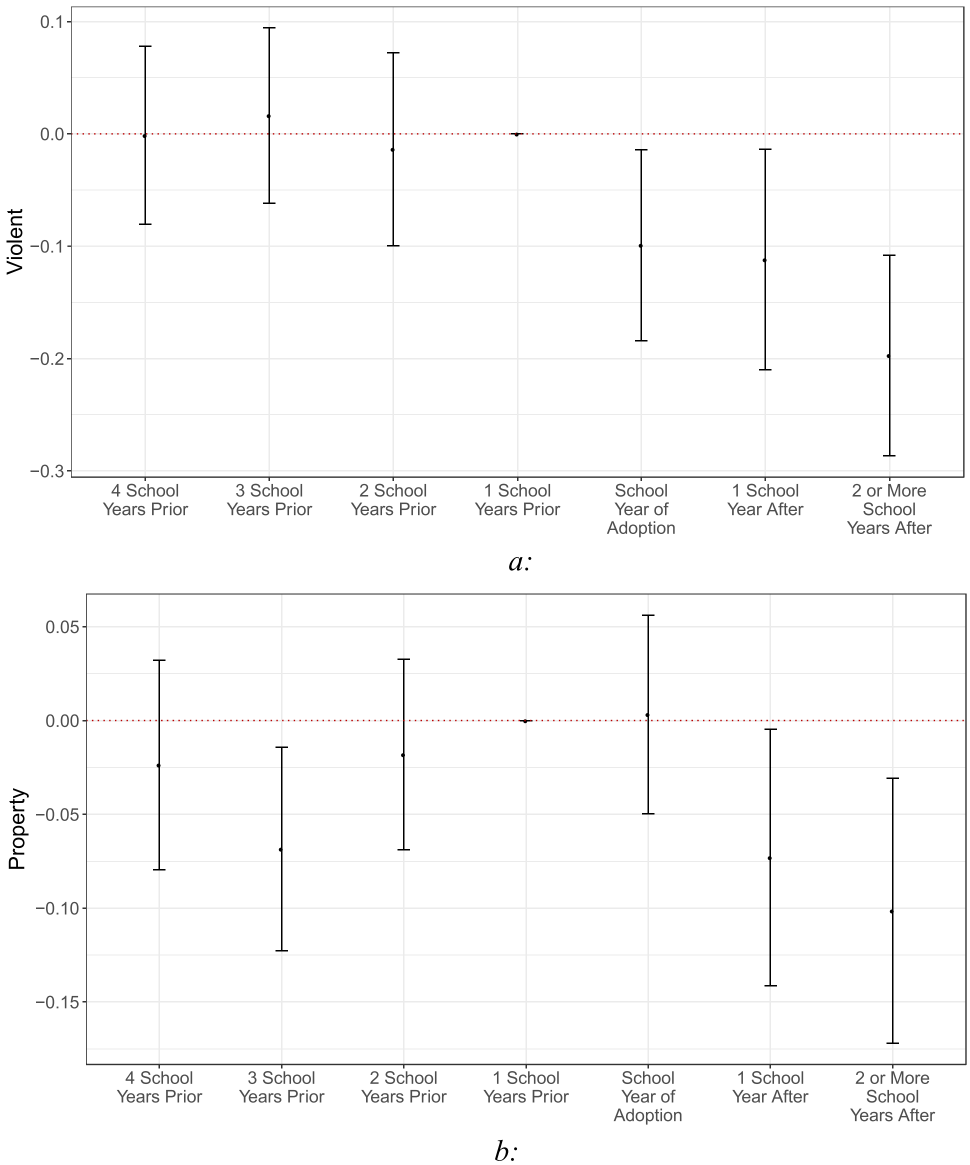


Fig. 4. Effect of the Safe Passage Program on Crime, Event Study. a: Violent Crimes. b: Property Crimes.
Note: Point estimates and 95% confidence intervals for coefficients Safe Passage Cell*Post from estimating Eq. (1) using dummy variables indicating timing with respect to school year of adoption. The ommitted category is the school year prior to the implementation of the program.

jacent. Including school and census block group characteristics in the matching procedure ensures that the cells that are used as controls are similar to the ones that received the treatment. Columns (2) and (5) in Table 7 summarize the results when the control group is identified using propensity score matching.[25] Under this specification, we find results consistent with our earlier analysis, with violent crime declining by 11.0%.[26]

A second alternative to matching is to use future Safe Passage route areas as controls by exploiting the phased manner in which the program was rolled out. In this way our control group comprises areas that receive the treatment in future. To exploit this variation, we restrict the time period of the data to 2009-2010 through 2013-2014 school years. The Safe Passage routes that received the treatment during this period are considered “treated” routes, while the routes that received the treatment in the 2014-2015 and 2015-2016 are used as “controls”. Results under this alternative strategy remain consistent with the results obtained earlier and are summarized in Table 7 in columns (3) and (6). Overall, the evidence presented in this section assures us that the effects are driven by the presence of guards and not by either pre-trends or the choice of our control groups.[27]

4.6. Heterogeneity in the results for crime

Our results show that the presence of the Safe Passage guards have on average a positive impact of reducing crime in the guarded areas. In this section, we further explore these results and show that our results are likely to be the result of Safe Passage guards deterring and dispersing potential offenders from the guarded areas.

To begin, we analyze whether our estimated effects are confined to a reduction in crime in schools in high crime neighborhoods. To analyze this differential effect, we classify areas as high crime if they had above average crime for the three years before the program started i.e.

[25] Appendix Table A8 summarizes the covariate balance for the matched sample. We also repeat this exercise using the closest neighbor with no replacements and common support and find similar results.

[26] In Appendix Table A9 we show that results hold when using alternative estimators as in Appendix Table A2.

[27] We repeat the experiment for non-guarded times in Appendix Table A10 and the point estimates suggest that there is no effect in the guarded areas when the guards are not present.

Table 6
Effect of the Safe Passage Program on Crime, with Leads.

	Number of violent crimes			Number of property crimes		
	(1) Base	(2) 1 year before	(3) 2 years before	(4) Base	(5) 1 year before	(6) 2 years before
Safe Passage Cell*Post	−0.1410***	−0.1410***	−0.1369***	−0.0277	−0.0183	−0.0223
	(0.0370)	(0.0397)	−0.0379	(0.0262)	(0.0282)	(0.0260)
One Cell Over*Post	0.0062	0.0034	0.0174	0.0158	0.0139	0.0157
	(0.0294)	(0.0311)	−0.0308	(0.0231)	(0.0251)	(0.0234)
Two cells Over*Post	0.0029	(0.0022)	0.0082	0.0084	0.0138	0.0141
	(0.0268)	(0.0275)	−0.0270	(0.0187)	(0.0211)	(0.0197)
Safe Passage Cell* 1or 2 year pre event		0.0063	0.0014		0.0383	0.0187
		(0.0364)	(0.0401)		(0.0263)	(0.0212)
One Cell Over* 1or 2 year pre event		(0.0059)	0.0419		−0.0173	−0.0071
		(0.0352)	(0.0305)		(0.0257)	(0.0183)
Two Cells Over* 1or 2 year pre event		(0.0219)	0.0188		0.0211	0.0235
		(0.0301)	(0.0317)		(0.0220)	(0.0158)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	508,376	508,376	508,376	552,896	552,896	552,896

Note: Columns show coefficient and standard errors (in parenthesis) from Eq. (1) using a Poisson regression of the number of crimes per cell (one eighth by one eighth of a mile) per month when schools are in session for the period January 2006 to August 2016. Safe Passage Cell*Post equals one for cells that have a Safe Passage in the months after the program was enacted. The same is true for One Cell Over*Post and Two Cells Over*Post (for cells one cell away and two cells away from the nearest Safe Passage, respectively). Regressions include up to Three Cells Over. Standard errors are clustered by Safe Passage. Each column corresponds to a particular lead specification referred in the heading. Columns (1) and (4) show our prefer specification of Table 3. Columns (2) and (5) include a dummy for one-year before, and (3) and (6) for two-year before.
$^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7
Effect of the Safe Passage Program on Crime, alternative control groups.

	Number of violent crimes			Number of property crimes		
	(1) Base	(2) Matching	(3) Asynchronous	(4) Base	(5) Matching	(6) Asynchronous
Safe Passage Cell*Post	−0.1410***	−0.1128***	−0.1619***	−0.0277	−0.0227	−0.0287
	(0.0370)	(0.0279)	(0.0529)	(0.0260)	(0.0181)	(0.0291)
Cells FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	508,376	167,162	406,264	552,896	172,250	447,716

Note: Columns show coefficient and standard errors (in parenthesis) from Eq. (1) using a Poisson regression of the number of crimes per cell (one eighth by one eighth of a mile) per month when schools are in session for the period January 2006 to August 2016. Safe Passage Cell*Post equals one for cells that have a Safe Passage in the months after the program was enacted. Each column corresponds to a particular strategy of choosing control groups. Columns (1) and (4) show the coefficient of the interaction of Safe Passage cells from our preferred specification of Table 3. Columns (2) and (5) refers to using a matching strategy as a way to choose controls groups (Table A8 summarizes the covariate balance for the matched sample). Columns (3) and (6) refers to the asynchronous strategy.
$^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

2006–2008.[28] The results are presented in Table 8. We do not find any differential effect for violent crime across low and high crime neighborhoods. However, for property crime the program leads to an additional (statistically significant) 6% reduction in high crime areas.

The first major expansion of the program (2009) was targeted at schools in the most crime-ridden neighborhoods. It is also possible that the results are primarily associated with the major expansion in 2013 as this expansion incorporated all the welcoming schools. Thus, we test whether the estimated effects vary by major expansions of the program.[29] Our results presented in Table 9 show that there are similar effects of the program in all three phases, with violent crimes declining by 12.5%–15.4%. There also is suggestive evidence of a decline in property crime but the point estimates are insignificant.

Having established that Safe Passage guards reduce crime, we now explore the mechanism behind this reduction. We exploit the timing of the incidents and determine whether there are differential effects within school-days. We divide the day into 2.5 h bins – before guards arrive, while they are present, and after they leave, in both the morning and afternoon. We aggregate the crime data to the school year level and

[28] We based the calculations on monthly averages for census blocks containing Safe Passage routes.

[29] To focus on the three major expansions, we exclude all cells that were treated in other program years. For instance, cells which serve as controls for the program year 2009-2010 but which get the Safe Passage route in a later year are excluded from the analysis for 2009-2010.

Table 8
Heterogeneous Effect of the Safe Passage Program on Crime, High Crime Areas, Welcoming Schools, and School Level.

	Number of Violent Crimes				Number of Property Crimes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Cell*Post	−0.1410***	−0.1849***	−0.1671***	−0.0536	−0.0277	0.0172	−0.0181	0.0301
	(0.0374)	(0.0514)	(0.0428)	(0.0431)	(0.0261)	(0.0305)	(0.0317)	(0.0319)
One Cell Over*Post	0.0062	0.0067	0.0120	0.0115	0.0158	0.0153	0.0141	0.0191
	(0.0295)	(0.0296)	(0.0301)	(0.0296)	(0.0231)	(0.0230)	(0.0234)	(0.0232)
Two Cells Over*Post	0.0029	0.0033	0.0069	0.0063	0.0084	0.0079	0.0073	0.0107
	(0.0267)	(0.0268)	(0.0268)	(0.0269)	(0.0187)	(0.0187)	(0.0189)	(0.0190)
Safe Passage Cell*Post *High Crime Areas		0.0583				−0.0631*		
		(0.0614)				(0.0366)		
Safe Passage Cell*Post *Welcoming			0.0628				−0.0229	
			(0.0458)				(0.0370)	
Safe Passage Cell*Post *High School				−0.1688***				−0.1116***
				(0.0644)				(0.0427)
Cell FE	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Time - Year FE	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Sample size	508,376	508,376	508,376	508,376	552,896	552,896	552,896	552,896

Note: Columns show coefficient and standard errors (in parenthesis) from Eq. (1) using a Poisson regression of the number of crimes per cell (one eighth by one eighth of a mile) per month when schools are in session for the period January 2006–August 2016. Safe Passage Cell*Post equals one for cells that have a Safe Passage in the months after the program was enacted. Standard errors are clustered by Safe Passage. Columns (1) and (5) show the coefficient of the interaction of Safe Passage cells from our preferred specification of Table 3. Columns (2) and (6) add an interaction for high crime areas, where an area is designated as high crime if it had above average incidents for the years previous to the program. Columns (3) and (7) add the interaction for welcoming schools and (4) and (8) if the Safe Passage is associated with a high school.
$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

Table 9
Heterogeneous Effect of the Safe Passage Program on Crime, by Program year.

	Number of Violent Crimes			Number of Property Crimes		
Program Year	(1) 2009	(2) 2013	(3) 2014	(4) 2009	(5) 2013	(6) 2014
Safe Passage Cell*Post	−0.1254***	−0.1535***	−0.1432*	−0.0361	−0.0893	−0.0915
	(0.0462)	(0.0561)	(0.0852)	(0.0341)	(0.0633)	(0.0570)
One Cell Over*Post	0.0215	−0.0001	0.0556	0.0128	−0.0459	−0.0447
	(0.0378)	(0.0578)	(0.0824)	(0.0302)	(0.0606)	(0.0614)
Two Cells Over*Post	0.0278	0.0117	0.0397	0.0013	−0.0764	−0.0172
	(0.0364)	(0.0618)	(0.0721)	(0.0271)	(0.0520)	(0.0570)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	280,794	302,206	187,302	300,298	326,692	201,930

Note: Columns show coefficient and standard errors (in parenthesis) from Eq. (1) using a Poisson regression of the number of crimes per cell (one eighth by one eighth of a mile) per month when schools are in session for the period January 2006–August 2016. Safe Passage Cell*Post equals one for cells that have a Safe Passage in the months after the program was enacted. Standard errors are clustered by Safe Passage. Each column corresponds to a major program expansion denoted in the header.
$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

estimate the following equation:

$$\begin{aligned}\#Crimes_{ith} = &\sum_{h\,=\,before,while,after} \beta_h Safe\ Passage\ Cell_i * Post_{th} \\ &+ \sum_{h\,=\,before,while,after} \theta_h One\ Cell\ Over_i * Post_{th} \\ &+ \sum_{h\,=\,before,while,after} \phi_h Two\ Cell\ Over_i * Post_{th} + \gamma_i + \delta_{th} + u_{ith}\end{aligned} \tag{2}$$

where $\#Crimes_{ith}$ is the number of incidents in cell i, in school year t, in time bin h. The time bin indicates whether a crime takes place 2.5 h before guards are present, while they are present, or 2.5 h after the guards have left.[30] For example, $Safe\ Passage\ Cell_i$ is an indicator for a cell on a Safe Passage Route, while $Post_{th}$ takes one for years following the start of a Safe Passage route, with separate effects for times before, during, and after the presences of guards. Thus, the coefficient β_{before} captures the change in crime after the program was implemented for times before guards are present, as compared to the control area. The interpretation is similar for times during and after the presence of guards. We exclude all other time periods, including nights and weekends. Thus, similar to our earlier analysis the control group is third cell over. To complete the specification, we include controls for both cell and time of day-school year fixed effects.[31] The time of day effects account for trends within

[30] For the morning, the "before" window takes a value of 1 between 4:00 am and 6:30 am, and the "after" is 9:00 am and 11:30 am; guards are present from 6:30 am to 9:00 am. For the afternoon session, "before" denotes the time between 12:30 pm and 3:00 pm, guards are present from 3:00 pm to 5:30 pm, and the "after" window is 5:30 pm to 8:00 pm. We exclude the time window 11:30 am to 12:30 pm from our analysis.

[31] In this specification, we aggregate to the school year, as monthly/time of day aggregation would produce a lot of zeros.

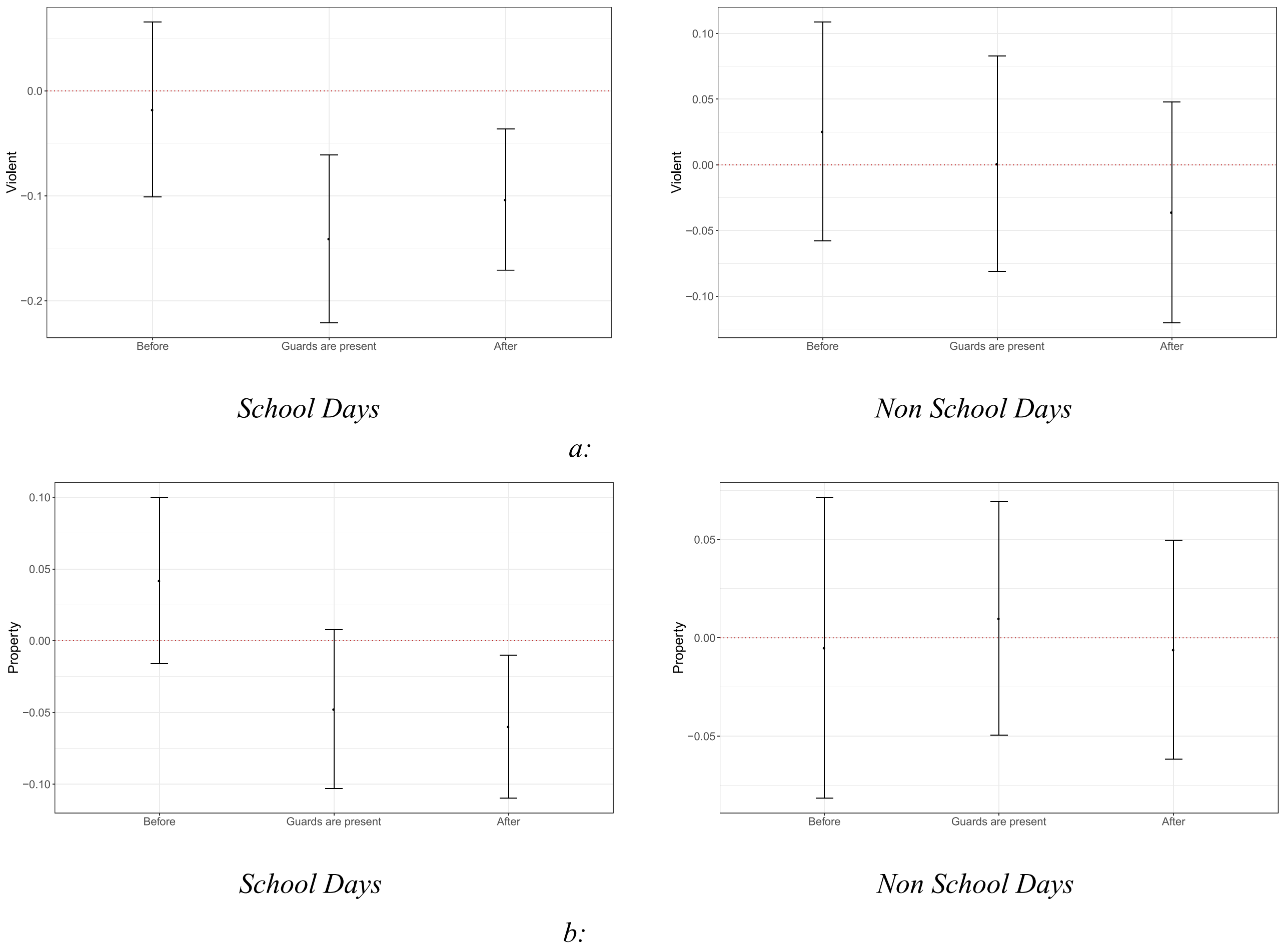


Fig. 5. Effect of the Safe Passage Program on Crime, Intraday Variation of Crime. a: Violent Crimes. b: Property Crimes.
Note: Panels (a) and (b) presents estimates for Violent and Property Crimes and for School Days and Non School Days (where we combine Summer and Weekends when guards are not present). Point estimates and 95% confidence intervals for difference-in-differences coefficients on time of day. "Before" denotes interaction between being a Safe Passage Cell after the program implementation for 2.5 h in morning and afternoon before guards arrive, "Guards are present" indicates 2.5 h in morning and afternoon during which guards are present, and "After" indicates 2.5 h in morning and afternoon after the guards have left.

the day by controlling for the before, during and after times in both the morning and afternoon.[32]

We plot the point estimates and 95% confidence intervals for the Safe Passage Cell coefficients in Fig. 5. Panel (a) shows a clear pattern for violent crimes: estimated effects are insignificant before guards arrive, but there is a statistically significant reduction in crime while guards are present and also afterward. Property crimes show a similar pattern in panel (b). These findings are reinforced by insignificant effects during non-school days when guards are never present.

In Appendix Fig. A3 we show estimates for morning and afternoon times separately. There is evidence of a decline in violent crimes when guards are present, while the effect on property crime is negative but imprecise. Moreover, we find stronger effects in the afternoon, which may indicate that guards are leading to declines in crime by dispersing students after school lets out.

If the presence of guards encourages students to attend school, we might expect to see a higher impact of the Safe Passage guards on high school students. These results are summarized in columns (4) and (8) of Table 8.[33] We find that the overall reduction in crime is primarily associated with high schools. Guarded areas around high schools witness an additional 17% decrease in violent crimes and 11% reduction in property crimes. These give us some evidence that the reduction is driven by dispersing these juvenile offenders.

In Table 10 we explore these results by subcategory of crime and distinguishing again by type of school. Our results for violent crimes are driven by robbery and aggravated battery whereas for property crimes it is driven by larceny and motor vehicle theft around high schools. These are also the types of crime, which are committed by juveniles.[34] These results show that our results are driven by crimes around high schools and by types of crimes that are more likely to be committed by juveniles.

5. Do more eyes on the street reduce school absenteeism?

Our analysis shows that the presence of Safe Passage guards reduced violent crime without displacing it to neighboring areas. If the program indeed is making school trips safer we might expect to see immediate consequences on school attendance. In this section, we show that this is indeed the case with the Safe Passage program.

[32] We bunch together times to gain statistical power.

[33] There were a few middle schools which have been combined in the elementary school category, so the elementary and middle schools serve as the base group.

[34] According to NIBRS data, among serious offenses, the most common crimes committed by juveniles are arson (32.7%) of all offenses between 2012 and 2016, robbery (19.5%), motor vehicle theft (17.2%) and burglary (16.7%).

Table 10
Effect of the Safe Passage Program on Crime, Individual Categories and School Level.

	Violent crimes				Property crimes			
	(1) Homicides	(2) Sexual assault	(3) Robbery	(4) Aggravated assault	(5) Aggravated battery	(6) Burglary	(7) Larceny	(8) Motor vehicle theft
Safe Passage Cell*Post	−0.1694	0.2354	−0.0721	−0.0844	−0.0413	0.0469	0.0336	−0.0058
	(0.2354)	(0.1461)	(0.0663)	(0.0673)	(0.0665)	(0.0589)	(0.0370)	(0.0599)
Safe Passage Cell*Post*High School	−0.6310	−0.2259	−0.1479*	−0.0073	−0.2648***	0.0183	−0.1357***	−0.1312*
	(0.3881)	(0.2192)	(0.0874)	(0.0849)	(0.0914)	(0.0841)	(0.0513)	(0.0739)
One Cell Over*Post	−0.1162	0.1208	−0.0119	0.0380	0.0218	0.0264	0.0259	−0.0104
	(0.1952)	(0.0985)	(0.0392)	(0.0526)	(0.0530)	(0.0333)	(0.0305)	(0.0384)
Two Cells Over*Post	−0.4014**	0.0904	−0.0172	0.0488	0.0184	0.0021	0.0270	−0.0440
	(0.1953)	(0.1093)	(0.0366)	(0.0452)	(0.0545)	(0.0309)	(0.0245)	(0.0359)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	52,208	141,404	446,578	368,138	379,692	498,412	548,762	501,698

Note: Columns show coefficient and standard errors (in parenthesis) from Eq. (1) using a Poisson regression of the number of crimes per cell (one eighth by one eighth of a mile) per month when schools are in session for the period January 2006–August 2016. Safe Passage Cell*Post equals one for cells that have a Safe Passage in the months after the program was enacted. Standard errors are clustered by Safe Passage. Each column corresponds to individual crime category denoted in the header.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 11
Effect of the Safe Passage Program on Attendance, Propensity Score Matching.

	(1)	(2)	(3)	(4)
Safe Passage School* Post	1.6832***	2.5351***	2.4151***	0.2326*
	(0.2781)	(0.3287)	(0.3300)	(0.1297)
Welcoming*Post			−2.2053***	
			(0.3084)	
Safe Passage School * Post*High School				3.4931***
				(0.3917)
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Excludes welcoming schools		Yes		
Control group obtained by matching	Yes	Yes	Yes	Yes
Sample size	3324	2695	3324	3324

Note: Columns show coefficients and standard errors (in parenthesis) from Eq. (3) using OLS of the annual change in attendance rate for between 2006 and 2016. Safe Passage Cell*Post equals one for schools that have a Safe Passage in the school years after the program was enacted. Control group is constructed using Propensity Score Matching with two neighbors. Schools that were closed in 2013 are excluded from all the regressions. Standard errors clustered by school are reported in parentheses.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

5.1. Empirical strategy

To explore the effects of the Safe Passage program on attendance we gather school level data on attendance rates and other school level characteristics.[35] Given the nonrandom assignment of the school program we use propensity score matching (Rosenbaum and Rubin, 1985) to find suitable control schools. We match the schools based on three broad categories of variables: pre-program attendance, school characteristics, and Census block group characteristics. We use attendance for the pre-treatment program years of 2006, 2007 and 2008. For school and Census block group characteristics, we use the same variables used in constructing propensity score matches for crime.[36] We use the propensity scores to identify the two closest schools to a treated school in the range of common support. We then use a difference-in-differences estimator of the form,

$$\Delta Attendance_{it} = \beta\ Safe\ Passage\ School_i * Post_t + \gamma_i + \delta_t + u_{it} \tag{3}$$

where $\Delta Attendance_{it}$ represents the annual change in attendance rate for school i in year t. $Safe\,Passage\,School_i$ equals one if school i has a Safe Passage program ever, $Post_t$ is an indicator that takes one for the school years after the Safe Passage Program adoption. We also include school fixed effects (γ_i) and year fixed effects (δ_t). Standard errors are clustered at the school level. This identification strategy depends on the relative similarity of the treated and control schools.[37]

5.2. Base results for attendance

Table 11 presents the estimates for the effect of the Safe Passage program on the change in attendance.[38] We find that schools in the Safe Passage program experience a 1.68 percentage points increase in attendance (column (1)), which implies that attendance in the participating

[35] School level data comes from the CPS website and includes school level attendance rates, demographic information about the student body, proportion of student eligible for free lunch, proportion of bilingual students, and overall attendance records.

[36] If any of these characteristics is missing for a school, we replace the missing data with the average value for the sample.

[37] Appendix Table A11 summarizes the balance across covariates.

[38] We also exclude the schools that had more than two years of missing attendance data in the Safe Passage sample period. Results do not change significantly if we include these missing schools in our analysis.

schools increased at a faster rate than control schools not enrolled in the program.

A potential explanation for this result could be that the effect is due to the closing of some schools and the reallocation of students to the Safe Passage schools designated as welcoming schools. In column (2) we exclude welcoming schools from the sample. The estimated effects after excluding the welcoming schools is stronger, with change in attendance being much higher at 2.53 percentage points. When controlling for welcoming schools (column (3)), we see a decrease in their attendance.[39] This result suggests that as new students coming from the closed schools enrolled in the welcoming schools, the change in the composition of students led to higher rates of absenteeism.[40]

Furthermore, since our results on crime show a larger effect for High Schools, we expect a higher effect on attendance around High Schools. Column (4) shows that this is indeed the case. Together, these results suggest that the program led to significant increases in attendance rates as students felt safer while traveling to and from school.

6. Conclusion

In this paper, we examine an alternative way of policing to increase student safety: hiring civilians to serve as guards near schools for a few hours each day. To study this alternative strategy, we focus on Chicago's Safe Passage program. The Safe Passage program began with 35 schools in the 2009-2010 school year and has expanded to cover about 20% of Chicago public schools in the 2015-2016 school year.

By combining detailed crime geo-located data with location of guards, we exploit the timing of the start of the program and the location of the Safe Passage guards to estimate their effect on crime. Our results show the Safe Passage program is an effective strategy for reducing crime. Guarded schools experience a significant reduction in crime, especially violent crime, with no crime displacement to adjacent areas. In addition, the effectiveness of the program is not limited to the first year it is implemented but it continues to lower crime throughout the implementation period. Schools that had the program for more than 2 school-years show a significant reduction in crime with an approximate 20% decline in violent crime. The sharp reduction in violent crime is driven by the early adopters of the program. Whereas, the reduction in property crime is explained by the two latter expansions.

We also find positive effect of the Safe Passage guards on attendance. Safe passage schools increase their attendance rates by 2.5% on average when compared to other Chicago Public Schools. Schools that received the program earlier where not only in more dangerous areas but their attendance rate had dropped significantly. The presence of Safe Passage guards not only made those areas safer but also contributed to significant increases in attendance rates.

This improvement in attendance highlights the success of the program as it reflects that students and their parents now have a sense of increased safety around the school that results in students attending school more regularly. The increase in attendance is driven by a safer environment, and is likely to improve academic performance as earlier studies have shown that higher attendance has a positive effect on math and reading scores. However, it should be noted that our results show that crime incidents drop more in High Schools and that the drop is not limited only to the times the Safe Passage guards are on duty but also after they leave. This suggests another potential explanation: High school students who otherwise might loiter or be involved in criminal activities are not only deterred but also encouraged to go to class. This would explain the reduction in crime after guards leave and the increase in attendance.

The program provides an interesting insight of policies to increase safety. By placing civilian guards, the reduction in crime is driven mainly through deterrence. One of the important questions for deterrence research is the "degree of correspondence between actual and perceived risks" (Chalfin and McCrary, 2017). The perceived risks are more closely aligned to actual risk for the Safe Passage program as the program is well advertised, with signs clearly indicated that a route is being monitored during schools hours along with the visible presence of the guards The guards are easily identifiable by their bright neon jackets. The routes are also available on the on the City Data Portal, School websites, and the CPS website. Additionally, there could also be some incapacitation effect as presence of guards is likely to result in faster response time, which will increase the probability of criminal cases being cleared and incapacitation of the criminals. Also, the improvement in attendance indicates that there is a 'self incapacitation' effect as time spent in school is likely to reduce time spent in criminal activities (Tauchan et al., 1994; Machin et al., 2011).

Overall, our results suggest that placing civilian guards around schools is an inexpensive and effective way of increasing safety and attendance. The program involves placing civilians for 5 h a day on school days. These civilians are paid $10 dollars an hour. Cohen and Piquero (2009) suggest that people are willing to pay around $97,000 dollars to avoid battery, and even $13 million to avoid being killed. We show that the Safe Passage guards are efficient at reducing these types of crime, although the effects on homicides are very imprecisely estimated. Nevertheless, the program would be considered a major success even if it saves just one life. Apart from the direct benefits due to reduction in crime, there are also likely to be indirect effects – such as improvements in test scores, graduation rates, future job outcomes – which are harder to measure and beyond the scope of this paper. Sickmund and Synder (1999) estimate that allowing a youth to leave high school for a life of crime and drug abuse results in a cost to society of around $1.7 million-$2.3 million. Although a limitation of the analysis is that it does not take into account the general equilibrium effects of the program, our simulation results[41] strongly suggest that placing civilian guards around schools is a relatively inexpensive way of creating a safe environment where children and teenagers can attend school.

[39] We estimate a similar model for enrollment and do find a significant rise in change in enrollment for the welcoming schools, which provides evidence that the welcoming schools did absorb students from the schools that had closed.

[40] Appendix Table A12 presents results of estimating Eq. (3) by major expansions of the program.

[41] Refer to the appendix for the details of the Cost Benefit analysis.

Appendix: Cost benefit analysis of the program

In this section, we estimate the direct benefit of reductions in crime near the Safe Passage schools. A starting point for this analysis involves estimating the potential benefits accrued for the avoided crimes. The literature on the costs of crime suggests that the relevant measure for policy analysis is the willingness to pay or an ex-ante measure of the costs of crime (Ludwig, 2010; Cohen et al., 2010; Cohen and Piquero, 2009). The willingness to pay approach quantifies how much people are willing to pay to reduce the likelihood of becoming victims. A second approach for quantifying the costs of crime is to use the victim costs or ex-post approach. These costs are often derived from civil jury awards, and capture both direct costs such as injuries sustained during the incident and indirect costs such as pain and suffering. We use Cohen and Piquero's (2009) victim cost of crimes estimates, which is the more conservative of the two estimates.[42]

The estimated effects for each crime subcategory are more imprecise and thus we conduct a simulation exercise to account for the number of crimes that are potentially reduced. For each crime subcategory, we draw from a normal distribution with parameters described by our estimates. With the pretreatment averages and cost for each type of crime, we obtain a distribution of the benefits of the program shown in Fig. A4.

Results from the simulation show that the mean benefit of the program based on willingness to pay due to reduced crime is about $38.6 million per year, while the total cost of the program is $17.8 million for the 2015-2016 school year.[43] Simulations show that the probability that the program's benefits do not exceed its costs for the 2015-2016 school year is about 10%. We get much higher estimates when we use Cohen and Piquero's (2009) willingness to pay estimates.[44]

We should note at this point that our results are driven by the benefits associated with reductions in the number of murders. We believe that it is important to include murder in our estimates because the program would be considered a major success even if it saves just one life. The program was begun and subsequently expanded in large part because there were concerns that students going to school might become murder victims. Chaflin and McCrary (2017) note that even after accounting for the rarity of murder, its expected cost is 27 times higher than that of motor vehicle theft, with the latter being the most expensive property crime.

Although a limitation of the analysis is that it does not take into account the general equilibrium effects of the program, our simulation results strongly suggest that placing civilian guards around schools is a relatively inexpensive way of significantly reducing crime.

Table A1
Effect of the Safe Passage Program on Crime, Sample Robustness.

	Number of violent crimes				Number of property crimes			
	(1) Base	(2) Base	(3) Extended	(4) Blocks	(5) Base	(6) Base	(7) Extended	(8) Blocks
Safe Passage Block*Post	−0.1410***	−0.1410***	−0.1379***	−0.1438***	−0.0277	−0.0277	−0.0135	−0.0045
	(0.0370)	(0.0304)	(0.0272)	(0.0355)	(0.0262)	(0.0208)	(0.0237)	(0.0249)
One Block Over*Post	0.0062	0.0062	−0.015	0.0052	0.0158	0.0158	0.0254	0.0261
	(0.0294)	(0.0261)	(0.0256)	(0.0290)	(0.0231)	(0.0194)	(0.0233)	(0.0243)
Two Blocks Over*Post	0.0029	0.0029	0.0017	0.0438	0.0084	0.0084	0.0199	0.0166
	(0.0268)	(0.0235)	(0.0235)	(0.0274)	(0.0187)	(0.0160)	(0.0175)	(0.0248)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Std. error cluster level	Safe Passage	Cell	Safe Passage	Safe Passage	Safe Passage	Cell	Safe Passage	Safe Passage
Sample Size	508,376	508,376	778,596	783,340	552,896	552,896	823,524	982,832

Note: The specification is similar to Table 3 columns (3) and (6). Each column corresponds to a particular sample specification referred to in the heading. Columns (2) and (6) show results for standard errors clustered at the cell level. Columns (3) and (7) extend the preprogram period to 2001 instead of 2006 in the base regression. Columns (4) and (8) show results when the unit of analysis are census blocks.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

42 Appendix Table A11 column (1) and (2) shows these estimates in 2015 dollars.
43 Simulations contain 100,000 iterations.
44 Using willingness to paste estimates our mean benefits are about $97.4 million. See Appendix Table A13 and Fig. A4.

Table A2
Effect of the Safe Passage Program on Crime: Robustness to Estimator Used.

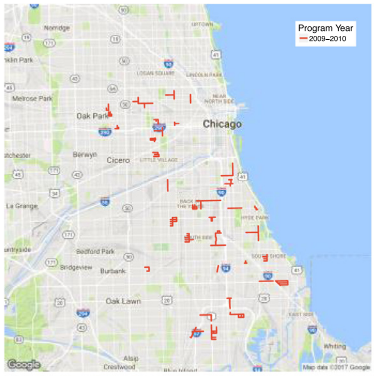
	Number of violent crimes				
	(1) Poisson	(2) Neg. Binomial	(3) OLS	(4) OLS-Population	(5) OLS-Traffic
Safe Passage Cell*Post	−0.1410***	−0.1400***	−0.0433***	−0.0034***	−0.0037***
	(0.0370)	(0.0303)	(0.0049)	(0.0007)	(0.0006)
One Cell Over*Post	0.0062	0.0062	−0.0059**	−0.0002	−0.0002
	(0.0294)	(0.0264)	(0.0025)	(0.0003)	(0.0003)
Two Cells Over*Post	0.0029	0.0030	−0.0012	0.0003	0.0001
	(0.0268)	(0.0243)	(0.0020)	(0.0002)	(0.0002)
Cell FE	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes
Sample Size	508,376	508,376	603,034	596,356	600,914
	Number of property crimes				
	(6) Poisson	(7) Neg. Binomial	(8) OLS	(9) OLS-Population	(10) OLS-Traffic
Safe Passage Cell*Post	−0.0277	−0.0181	−0.0659***	−0.0037**	−0.0055***
	(0.0262)	(0.0206)	(0.0126)	(0.0016)	(0.0015)
One Cell Over*Post	0.0158	0.0126	−0.0058	0.0021**	0.0008
	(0.0231)	(0.0171)	(0.0084)	(0.0009)	(0.0012)
Two Cells Over*Post	0.0084	0.0096	0.0073	0.0013	0.0023***
	(0.0187)	(0.0154)	(0.0061)	(0.0010)	(0.0007)
Cell FE	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes
Sample size	552,896	552,896	603,034	596,356	600,914

Note: The specification is similar to Table 3 columns (3) and (6). Each column corresponds to a particular estimator referred in the heading. Columns (1) and (6) repeat Table 3 columns (3) and (6). Columns (2) and (7) use Negative Binomial. Columns (3) and (8) OLS regression. Dependent variable in these cases is the count of crimes per cell (one eighth by one eighth of a mile) per month when schools are in session for the period January 2006–August 2016. Columns (4), (5), (9) and (10) show OLS results when the dependent variable are rates, in columns (4) and (9) rates are calculated as incidents per population in each cell, and in columns (5) and (10) the rates are calculated using traffic estimates for the streets in the cell.
$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.

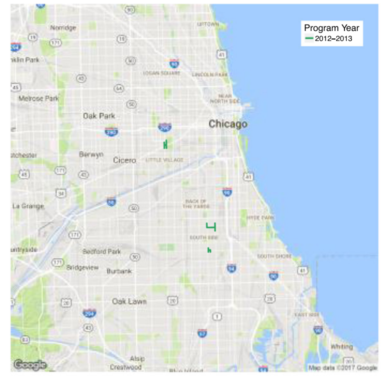
Table A3
Impact of the Safe Passage Program on Crime, alternative trends.

	Number of violent crimes				
	(1) Base	(2) Community Area	(3) Census Tract	(4) Police District	(5) Police Beats
Safe Passage Cell*Post	−0.1410***	−0.1417***	−0.1504***	−0.1674***	−0.1677***
	(0.0374)	(0.0371)	(0.0415)	(0.0367)	(0.0363)
One Cell Over*Post	0.0062	0.0053	−0.0160	−0.0136	−0.0189
	(0.0295)	(0.0296)	(0.0344)	(0.0289)	(0.0345)
Two Cells Over*Post	0.0029	0.0103	−0.0015	−0.0070	0.0038
	(0.0267)	(0.0245)	(0.0303)	(0.0260)	(0.0271)
Cell FE	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes
Region *Month-Year FE	Yes	Yes	Yes	Yes	Yes
Sample size	508,376	508,376	508,376	508,376	508,376
Number of property crimes					
	(6) Base	(7) Community Area	(8) Census Tract	(9) Police District	(10) Police Beats
Safe Passage Cell*Post	−0.0277	0.0059	−0.0069	−0.0108	−0.0236
	(0.0261)	(0.0251)	(0.0293)	(0.0244)	(0.0270)
One Cell Over*Post	0.0158	0.0393	0.0321	0.0272	0.0190
	(0.0231)	(0.0239)	(0.0246)	(0.0222)	(0.0216)
Two Cells Over*Post	0.0084	0.0194	0.0119	0.0064	0.0093
	(0.0187)	(0.0184)	(0.0187)	(0.0186)	(0.0186)
Cell FE	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes
Region *Month-Year FE	Yes	Yes	Yes	Yes	Yes
Sample size	552,896	552,896	552,896	552,896	552,896

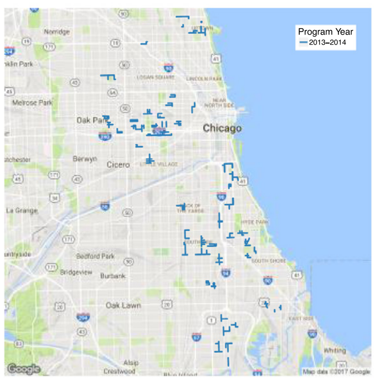
Note: Each column repeats Table 3 columns (3) and (6) and add controls for different geographic areas specific time trends referred to in the heading, except for Columns (1) and (6) which show our base results for comparison purposes.
$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$.



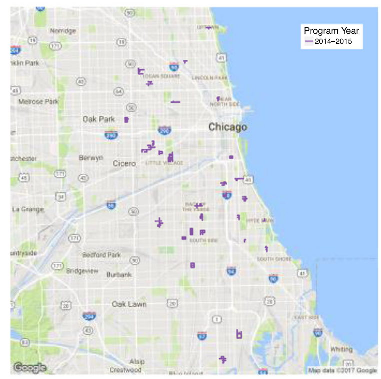
(a) 2009-2010 School Year



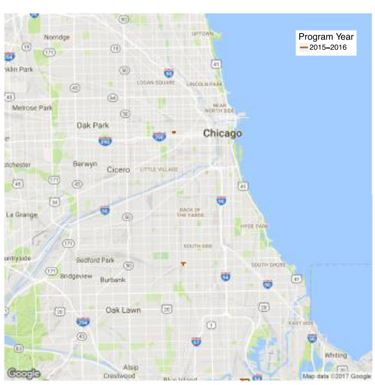
(b) 2012-2013 School Year



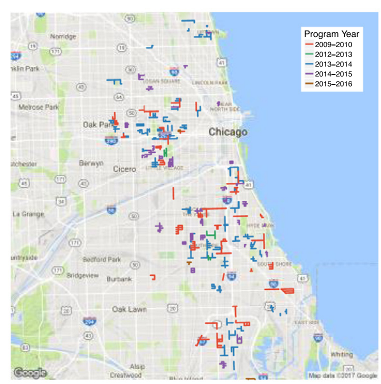
(c) 2013-2014 School Year



(d) 2014-2015 School Year



(e) 2015-2016 School Year



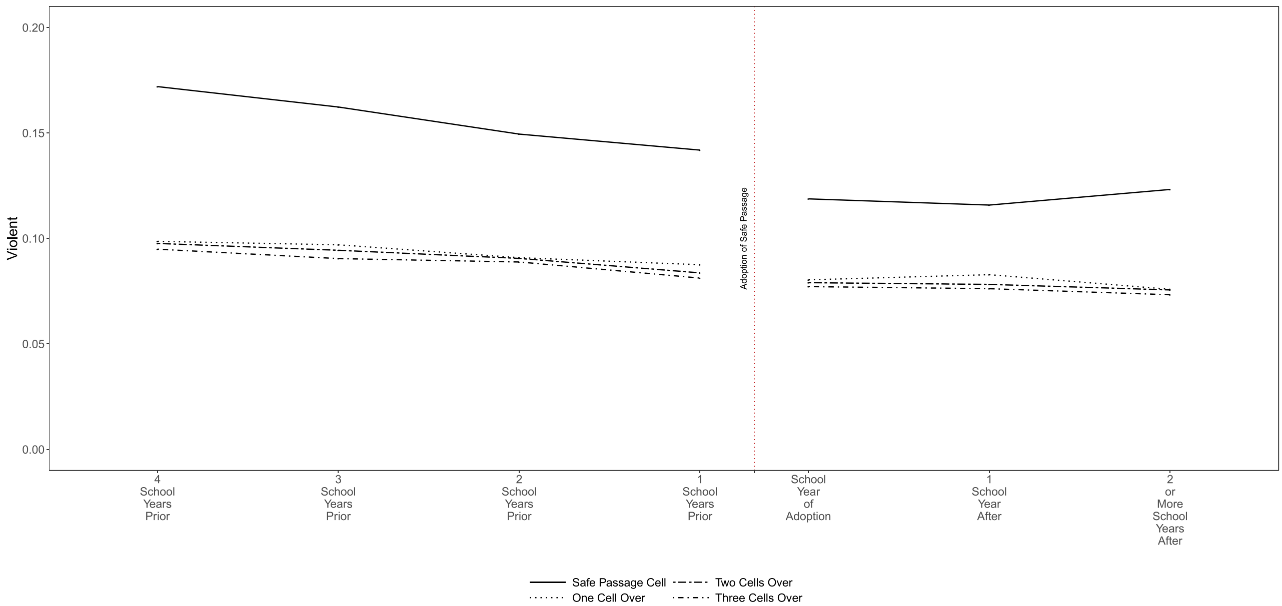
(f) All Safe Passages

Fig. A1. Safe Passages: By Starting Program Year.

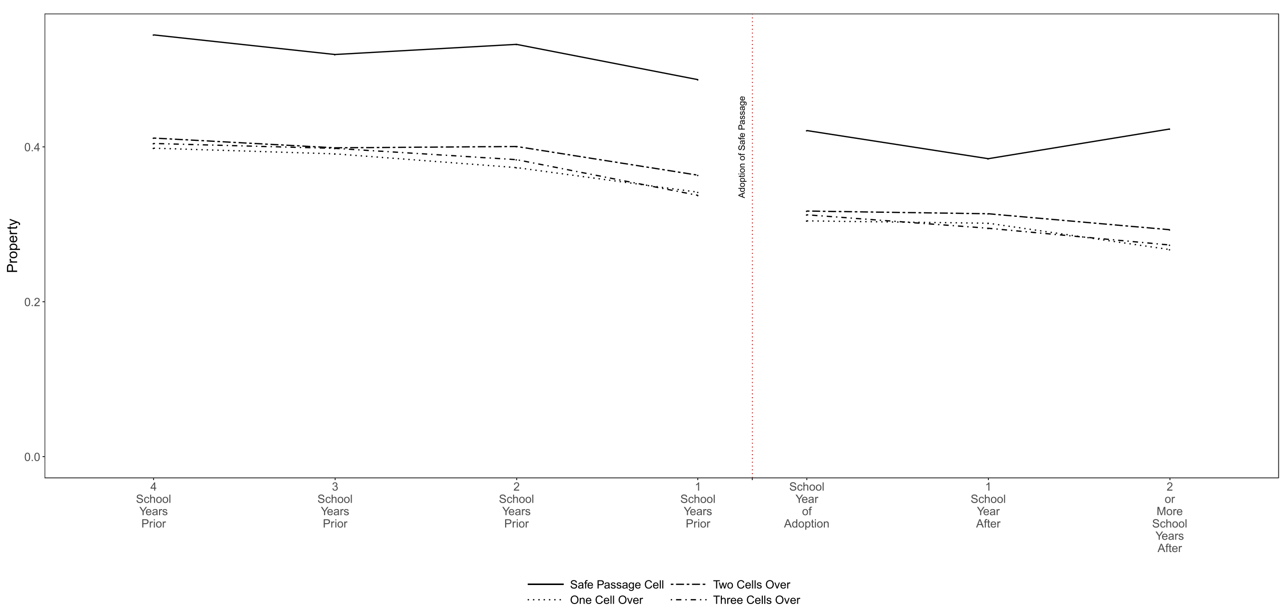
Table A4
Number of police officers by safe passage.

	Log of number of police officers		
	(1)	(2)	(3)
Number of Safe passages in district	−0.0001	−0.0002	−0.0002
	(0.0001)	(0.0003)	(0.0003)
Police district FE	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes
Sample size	2040	2040	2040

Note: Columns show coefficient and standard errors (in parenthesis) from an equation of log number of sworn Chicago police personnel by police district and by month between January 2008 and September 2016 on the number of Safe Passages in the police district.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.



a:



b:

Fig. A2. Violent and Property Crime trends relative to the start year of the Safe Passage program. a: Violent Crimes. b: Property Crimes.
Note: The Figure presents the average number of (A2a) violent and (A2b) property crimes when schools are in session (daytimes for week days of the school year). We distinguish by *Safe Passage Cells*, our treated cells, with our control cells, *One Cell Over, Two Cells Over*, and *Three Cells Over*. The vertical dotted line marks the start of the program. Given the phased way the program was implemented, we normalize to a common start and show the averages for the four pre-program years and for three post-program years.

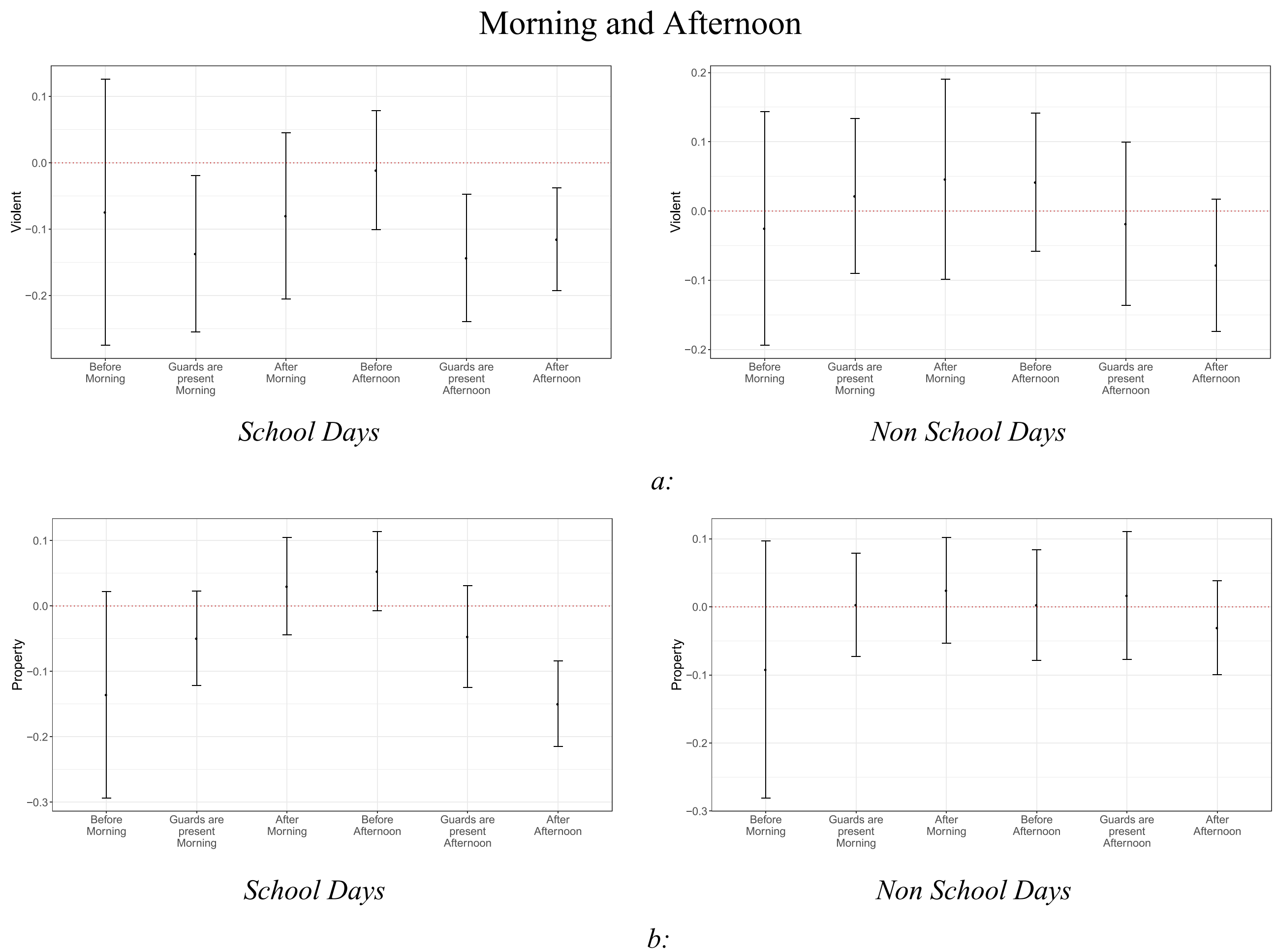


Fig. A3. Effect of the Safe Passage Program on Crime, Intraday Variation of Crime. Morning and Afternoon. a: Violent Crimes. b: Property Crimes.
Note: Panels (a) and (b) presents estimates for Violent and Property Crimes and for School Days and Non School Days (where we combine Summer and Weekends when guards are not present). Point estimates and 95% confidence intervals for difference-in-differences coefficients on time of day. "Before Morning" denotes interaction between being a Safe Passage Cell after the program implementation for 2.5 h in morning before guards arrive, "Guards are present Morning" indicates 2.5 h in morning and during which guards are present, and "After Morning" indicates 2.5 h in morning after the guards have left. Similar for afternoon times.

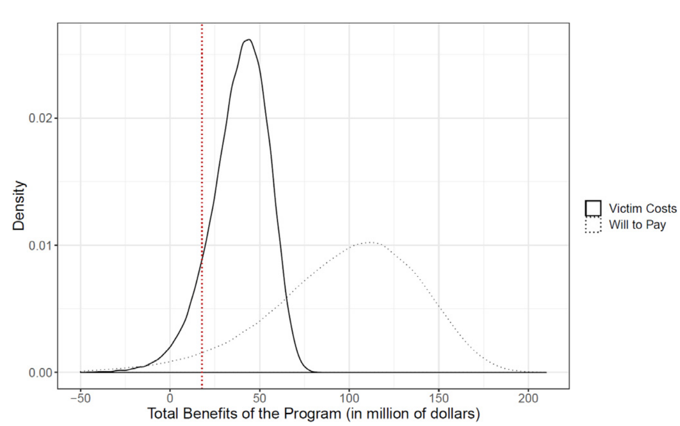


Fig. A4. Cost Benefit Analysis.
Note: The dotted line is the density of possible benefits of the program using victim cost estimates, with a mean of $38.6 million. The solid line denotes the density for possible benefits using willingness to pay estimate, which has a mean of $97.4 million. The red dotted line is the cost ($17.8 million) for the Safe Passage program for the 2015-2016 school year. The distribution of the potential benefits of the program is the result of a simulation exercise. First, we draw 100,000 estimates of the program effect for each crime category from normal distribution with mean equal to the estimated coefficient and standard deviation equal to the standard errors listed in Table A11 columns (2) and (3). Then we calculate the benefits of the program by using the preprogram mean and Cohen and Piquero's (2009) cost estimates (in 2015 dollars).

Table A5
Effect of the Safe Passage Program on Crime controlling for number of police officers.

	Number of violent crimes			Number of property crimes		
	(1)	(2)	(3)	(4)	(5)	(6)
Safe Passage Cell*Post	−0.1410***	−0.0889**	−0.0889**	−0.0277	−0.0260	−0.0262
	(0.0374)	(0.0394)	(0.0394)	(0.0261)	(0.0272)	(0.0272)
One Cell Over*Post	0.0062	0.0229	0.0230	0.0158	0.0227	0.0225
	(0.0295)	(0.0307)	(0.0307)	(0.0231)	(0.0232)	(0.0232)
Two Cells Over*Post	0.0029	0.0134	0.0134	0.0084	−0.0005	−0.0005
	(0.0267)	(0.0302)	(0.0302)	(0.0187)	(0.0190)	(0.0190)
Number of police officers			0.0011			−0.0016***
			(0.0010)			(0.0006)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	508,376	398,524	398,524	552,896	446,856	446,856

Note: Columns (1) and (4) show our preferred estimates from Table 3, Columns (2) and (5) repeat the exercise for the subsample that we have information on police officers in the district. Columns (3) and (6) include the number of police officers in the district as control.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A6
Effect of Safe Passage Program on Crime, Controlling for intensity of treatment.

	Number of violent crimes			Number of property crimes		
	(1)	(2)	(3)	(4)	(5)	(6)
Safe Passage Cell*Post	−0.1421***	−0.1317***	−0.1213***	−0.0371	−0.0346	−0.0364
	(0.0324)	(0.0348)	(0.0374)	(0.0240)	(0.0246)	(0.0263)
One Cell Over*Post		0.0277	0.0358		0.0077	0.0063
		(0.0227)	(0.0248)		(0.0179)	(0.0197)
Two Cells Over*Post			0.023			−0.0043
			(0.0217)			(0.0136)
Intensity of treatment	0.0969	0.0904	0.0823	0.1447	0.1433	0.1446
	(0.1232)	(0.1229)	(0.1240)	(0.0900)	(0.0902)	(0.0907)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	508,376	508,376	508,376	552,896	552,896	552,896

Note: Each column repeats Table 3 adding controls for Intensity of Treatment. Where intensity of treatment represents the number of Safe Passage routes in a cell.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A7
Effect of the Safe Passage Program on Crime, Placebo Safe Passage Programs.

	Violent	Property	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)	(5)	(6)
	Five years before rollout		Six years before rollout		Seven years before rollout	
Safe Passage Cell*Post	0.0035	0.0142	0.0232	0.0398	0.0154	0.0443
	(0.0278)	(0.0272)	(0.0286)	(0.0265)	(0.0280)	(0.0278)
One Cell Over*Post	−0.0011	0.0125	−0.0181	0.0252	−0.0285	0.0387*
	(0.0300)	(0.0204)	(0.0284)	(0.0207)	(0.0266)	(0.0218)
Two Cells Over*Post	0.0010	0.0147	−0.0137	0.0270	−0.0347	0.0439**
	(0.0252)	(0.0200)	(0.0265)	(0.0203)	(0.0263)	(0.0197)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	517,916	557,348	464,832	504,000	411,252	450,898

Note: The specification is similar to Table 3 columns (3) and (6). Standard errors clustered by Safe Passages are reported in parentheses. Columns show placebo Safe Passage programs in preprogram periods referred to in the heading.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A8
Crime: Covariate balance for matching with 2 closest neighbors.

Variable	Mean			*t*-test	
	Treated	Control	%bias	t	*p* > t
Crime count (Violent & property)					
Crime count (2006)	72.558	71.352	2.3	0.38	0.706
Crime count (2007)	69.367	69.233	0.3	0.04	0.965
Crime count (2008)	64.616	64.199	0.8	0.14	0.887
School characteristics: proportion of students					
Eligible for free lunch	0.93093	0.93212	−0.7	−0.36	0.721
Hispanic	21.286	20.29	3.1	0.7	0.487
Census block characteristics					
Share black	0.71265	0.72352	−2.8	−0.64	0.519
Proportion below high school	0.22222	0.22214	0.1	0.01	0.988
Median family income	33,861	33,947	−0.4	−0.1	0.917
Unemployment rate	0.2365	0.23731	−0.7	−0.14	0.891
Poverty rate	0.32372	0.32387	−0.1	−0.02	0.982
Owner occupancy rate	0.38467	0.38315	0.6	0.15	0.881
Vacancy rate	0.20213	0.20459	−2.1	−0.45	0.656
Median home value	1.70E+005	1.70E+005	0.1	0.03	0.976
Median gross rent	900.41	912.27	−4.4	−1.08	0.281
No. of schools in that area*	4.254	4.2213	1.7	0.33	0.74

Note: The table compares the mean of the treated and control for the matched sample obtained by propensity score matching using the two closest neighbors.
*Number of schools that are in that cell, one block, two block or three blocks adjacent.

Table A9
Effect of the Safe Passage Program on Crime: Robustness to Estimators using Matching.

	Number of violent crimes				
	(1) Poisson	(2) Neg. Binomial	(3) OLS	(4) OLS-Population	(5) OLS-Traffic
Safe Passage Cell*Post	−0.1128***	−0.1149***	−0.0336***	−0.0027***	−0.0031***
	(0.0279)	(0.0309)	(0.0047)	(0.0005)	(0.0005)
Cell FE	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes
Sample Size	167,162	167,162	177,868	177,126	177,868
	Number of property crimes				
	(6) Poisson	(7) Neg. Binomial	(8) OLS	(9) OLS-Population	(10) OLS-Traffic
Safe Passage Cell*Post	−0.0227	−0.0165	−0.0399***	−0.0036***	−0.0047***
	(0.0181)	(0.0163)	(0.0107)	(0.0014)	(0.0012)
Cell FE	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes
Sample Size	172,250	172,250	177,868	177,126	177,868

Note: The table presents a similar exercise as Appendix Table A2 with control groups were obtained using matching estimators presented in Table 7.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A10
Effect of the safe passage program on crime, alternative control groups, unguarded times.

	Number of violent crimes			Number of property crimes		
	(1) Base	(2) Matching	(3) Asynchronous	(4) Base	(5) Matching	(6) Asynchronous
Safe Passage Cell*Post	0.0156	0.0233	−0.0026	−0.0111	−0.0002	−0.0310
	(0.0220)	(0.0164)	(0.0230)	(0.0246)	(0.0155)	(0.0221)
Cells FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	2088,464	656,096	1667,164	2141,244	661,780	1713,130

Note: The table repeats the exercise in Table 7 combining all unguarded times.
*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A11
Attendance: Covariate balance for matching with 2 closest neighbors.

Variable	Mean			*t*-test	
	Treated	Control	%bias	t	$p > t$
No. of enrollments					
Enrollment in 2008	88.217	89.457	−15	−1.02	0.308
School characteristics: Proportion of students					
Eligible for free lunch	0.93908	0.93511	2.5	0.41	0.685
Hispanic	22.217	23.577	−3.9	−0.34	0.73
Census block group characteristics					
Share black	0.68126	0.67313	2	0.17	0.864
Proportion below high school	0.20957	0.23742	−20.2	−1.74	0.083
Median family income	35,455	30,960	19.7	1.89	0.06
Unemployment rate	0.24339	0.25928	−13.2	−0.93	0.352
Poverty rate	0.31763	0.35943	−26.9	−2.03	0.043
Owner occupancy rate	0.38232	0.3387	18.7	1.54	0.124
Vacancy rate	0.19331	0.20754	−12.7	−0.95	0.344
Median home value	1.80E+005	1.70E+005	6.8	0.58	0.56
Median gross rent	900.45	852.84	19.3	1.6	0.11

Note: The table compares the mean of the treated and control for the matched sample obtained by propensity score matching using the two closest neighbors.

Table A12
Change in attendance rates by expansion year.

	2009	2013	2013	2014
Treated* post	4.0820***	0.0562	0.2835	0.6678***
	(0.4436)	(0.1013)	(0.2682)	(0.1541)
Welcoming*post			−0.2674	
			(0.2921)	
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sample size	5694	5974	5974	5835

Note: Results are obtained from regressions of the change in average annual attendance of schools on the explanatory variables. The time period for this analysis is 2003–2016 school years. Schools that were closed in 2013 are excluded from all the regressions. The variable Safe Passage Schools * Post takes a value 1 for the schools which got the treatment in the post treated period. Welcoming * Post takes a value of 1 for the welcoming schools from 2014 school years onwards. Standard errors clustered by school are reported in parentheses.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A13
Welfare Analysis of the Program.

Category	Willingness to Pay (in $ 2015 mill.) (1)	Victim Costs (in $ 2015 mill.) (2)	Coefficient (3)	Standard error (4)	Preprogram mean (5)
Violent crimes					
Murder	13,488,827.80	5258,356.60	−0.4193*	(0.2391)	18.56
Sexual assault	331,505.09	154,321.34	0.1450	(0.1299)	65.99
Robbery	44,581.72	13,717.45	−0.1532***	(0.0503)	937.85
Aggravated assault	97,165.29	42,295.48	−0.0879	(0.0610)	471.40
Aggravated battery	97,165.29	42,295.48	−0.1799***	(0.0575)	731.22
Property crimes					
Burglary	40,009.24	2286.24	0.0548	(0.0462)	1347.38
Larceny	4572.48	514.40	−0.0399	(0.0307)	4609.23
Motor vehicle theft	19,433.06	6287.17	−0.0719	(0.0443)	834.33
Program benefit	**97,379,183**	**38,580,995**			
	(41,721,125)	**(16,176,968)**			

Note: Cost of crime estimates are taken from Cohen and Piquero (2009) and are updated to 2015 dollars. Coefficients for each type of crime are estimated using specification in Table 3 (Column 3 or 6). The Pre program mean is the 2006–2008 average yearly crimes in Safe Passage Cells.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A14
Average number of crimes and demographic descriptive statistics of Chicago block groups by the presence of schools and/or safe passages.

	CBGs w/o Schools and Safe Passages (a)	CBGs w. Schools but w/o Safe Passages (b)	CBGs with Safe Passages (c)	Diff. (d) = (c)-(a)	Diff. (e) = (c)-(b)
Violent crimes	39.89	57.62	100.28	60.39***	42.66***
	(43.81)	(50.40)	(60.86)	(4.42)	(5.58)
Property crimes	159.14	199.57	236.73	77.59***	37.15**
	(192.4)	(172.48)	(124.52)	(18.59)	(17.45)
Prop. of whites	0.51	0.42	0.21	−0.3***	−0.22***
	(0.35)	(0.34)	(0.29)	(0.03)	(0.04)
Prop. of blacks	0.32	0.39	0.69	0.37***	0.29***
	(0.40)	(0.42)	(0.39)	(0.04)	(0.04)
Prop. of female	0.52	0.52	0.54	0.01**	0.02**
	(0.07)	(0.07)	(0.07)	(0.01)	(0.01)
Median age	36.18	34.91	33.46	−2.72***	−1.45*
	(8.46)	(8.20)	(8.40)	(0.83)	(0.87)
Prop. with incomplete HS	0.16	0.19	0.21	0.05***	0.02
	(0.13)	(0.14)	(0.13)	(0.01)	(0.01)
Median income	53,671.68	47,909.2	34,260.25	−19,411.43***	−13,648.95***
	(28,789.77)	(26,085.67)	(18,035.05)	(2778.21)	(2627.18)
Prop. unemployed	0.14	0.16	0.25	0.11***	0.09***
	(0.11)	(0.11)	(0.14)	(0.01)	(0.01)
Poverty rate	0.20	0.23	0.33	0.13***	0.1***
	(0.15)	(0.15)	(0.16)	(0.01)	(0.02)
Prop. on food stamps	0.20	0.24	0.38	0.19***	0.14***
	(0.18)	(0.18)	(0.19)	(0.02)	(0.02)
Housing units	477.35	464.74	413.36	−63.99**	−51.38**
	(277.65)	(237.44)	(206.2)	(26.92)	(24.61)
Prop. of owners	0.50	0.47	0.37	−0.13***	−0.1***
	(0.25)	(0.25)	(0.21)	(0.02)	(0.03)
Prop. of renters	0.50	0.53	0.63	0.13***	0.1***
	(0.25)	(0.25)	(0.21)	(0.02)	(0.03)
Prop. of vacant	0.13	0.14	0.20	0.07***	0.06***
	(0.11)	(0.11)	(0.12)	(0.01)	(0.01)
Median number of rooms	4.97	5.02	4.99	0.03	−0.03
	(1.00)	(0.86)	(0.81)	(0.1)	(0.09)
Median year of construction	1949.81	1948.3	1946.78	−3.03**	−1.52
	(14.08)	(14.53)	(13.76)	(1.38)	(1.53)
Median contract rent	891.06	813.59	732.26	−158.8***	−81.33***
	(299.97)	(261.53)	(203.08)	(29)	(26.69)
Median gross rent	1023.02	956.07	882.48	−140.53***	−73.58***
	(295.87)	(262.48)	(235.82)	(28.76)	(27.35)
Median property value	242,785.41	230,345.44	176,533.36	−66,252.05***	−53,812.07***
	(135,144.49)	(131,430.85)	(84,791.77)	(13,041.92)	(13,148.86)

Note: Columns (a)–(c) present the mean and standard deviation of each variable for Census Block Groups (CBG) without Schools and Safe Passages (column (a)), with Schools but without Safe Passages (column (b)), and with Safe Passages (column (c)). Columns (d)-(e) presents the difference of means. Violent Crimes represent the average monthly number of crimes between 2006 and 2008 over the census block groups. The total number of crimes is calculated as the sum of violent and property crimes. Crime data come from the Chicago Data Portal, while the remaining variables come from 2009–2014 ACS.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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