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 now serves 140 schools. We use data from more than 10 years of geocoded Chicago police reports and school level data to analyze the Safe Passage programs 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.

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1. Introduction

Students routinely encounter a wide range of safety issues when communing to and from schools across the country. Studies have shown that exposure to crime, especially violent, may impact educational outcome and have implications for long term outcomes.[2] 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.[3] However, most of these studies restrict their analysis to police enforcement agencies, like short term exogenous changes in deployment of police following a terror attack (Di Tella and Schargrodsky 2004; Klick and Tabarrok 2005; Draca, Machin, and Witt 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 suggest that community involvement can help reduce crime (Krivo 2014).

This paper examines an alternative way of policing to increase student safety: hiring civilians to stand guard near 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. We find that the guards' presence is effective at reducing crime in the surveilled areas, and that this crime does not displace to nearby areas. The effect is restricted to the times they are on duty. The effects are persistent over time and are mainly explained by the decline of crime in high schools. We also find that schools with Safe Passage guards experience an increase in attendance rates. These results suggest that the presence of Safe Passage guards acts as a deterrent for criminals, and help to encourage students to attend schools more regularly.

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. Major expansions of the program took place in the 2013-2014 and 2014-2015 school years, when the program was expanded to cover 55 and 39 additional schools, respectively. The largest and most advertised expansion of the program was in the 2013-2014 school year when it was expanded to increase the

[2] See for example Mathews et al. 2009; Schwartz and Gorman 2003; Grogger 1997 and, Billings and Phillips, 2017
[3] See for example Braga et al. 2012; Lum and Koper 2014; Chalfin and McCrary 2015

safety of students displaced for the closing of 50 schools. The schools receiving these students were designated as “welcoming” schools. Safe Passage guards are expected to be knowledgeable of the community area they wish to serve. The guards are trained on various de-escalation strategies, and safety protocols.

The key challenge in estimating the effects of Safe Passages on crime is identifying the counterfactual scenario, i.e. what would have happened with crime if guards were not present. We combine detailed crime geo-located data with the location of guards. By exploiting the timing of the start of the program and the location of the Safe Passage guards we can estimate their effect on crime. The exact location of these Safe Passage guards allows us to exploit variation in the threat of crime within adjacent small geographic areas. 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 show the Safe Passage program is an effective strategy for reducing crime. Guarded areas experience a significant reduction in crimes, especially violent crime. Although the effect is only local to when and where guards are placed, we find no crime displacement to adjacent areas or times. 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 two 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, while the reduction in property crime is explained by the two latter expansions.

In addition, we find that Safe Passage schools increase their attendance rates by 2.5%. To identify the effect of the Safe Passage guards on school attendance we complement our data with school level information. To address potential concerns of selection bias of the guarded schools, we show that our results are robust to the selection of the control schools. Results remain unaltered when we restrict our comparison to schools in the same communities or when we use propensity score matching to find suitable controls.

The Safe Passage program is a relatively cheap way of increasing safety. We compute the benefits accrued for the crimes that are avoided. Based on estimates for the willingness to pay for reduced crime, we estimate that the benefits of the program are around $100 million a year. In contrast, the estimated total cost of the program was $17.8 million for the 2015-2016 school year.

Our results suggest that placing civilian guards around schools is both an inexpensive and effective way of increasing safety and attendance. The program provides an interesting insight into policies aimed at reducing crime. The reduction in crime is driven through deterrence with guards rather than incapacitation. The guards are not equivalent to police, and they do not have the tools or training to incapacitate criminals. However, they can intervene to defuse potential incidents, call 911, or simply make their presence known. Our findings can help guide policy makers around the country who have adopted or are considering adopting similar programs.[4]

The remainder of the paper is organized as follows: In Section 2, we provide background information on the Chicago Safe Passage program. Next, we explore whether more eyes on the street provided by Safe Passage guards reduce crime. In section 4 we focus on their effect on attendance. In section 5 we discuss costs and benefits of the program and Section 6 concludes.

2. Chicago's Safe Passage Program

The Chicago Safe Passage program started in the 2009-2010 school year to increase the safety of students traveling to and from public schools. The program started with 35 schools participating. Since then, the program has been expanded to cover new schools almost every year, with about 20% of CPS schools covered in the 2015-2016 school year.[5][6] Table 1 shows the number of Safe Passages rolled out by school year and the number of schools they cover, while Figure 1 shows the location by roll-out year. Given that some schools are located close together, some Safe Passages cover more than one school.

[4] Los Angeles, Philadelphia and New Britain (CT) have in place similar programs designed to offer safe routes in Public schools (Sullivan 2013).
[5] The CPS system covers encompass about 650 schools.
[6] Our analysis expands up to the 2015-2016 school year including crime data up to August of 2016. We do not include the 2016-2017 school year where two more Safe Passage routes to cover two schools were added.

Major expansions of the program took place in the 2013-2014 and 2014-2015 school years, when the program was expanded to cover 55 and 39 additional schools, respectively. The largest and most advertised expansion of the program took place in the 2013-2014 school year when it was expanded to cover most of the "welcoming" schools.[7] Schools designated as "welcoming" are those that received students from 50 schools that were closed.[8] There were some safety concerns for the children who had to be enrolled in the welcoming schools as they did not necessarily belong to the neighboring area and might be required to cross gang boundaries when traveling to and from their school.

Prior to the implementation of the Safe Passage program in the 2009-2010 school years, Chicago Public School (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. Second micro-pod cameras were installed, with officers serving as monitoring them during afternoon school hours. According to research carried out by the Chicago Police Department (CPD), 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.[9]

The Safe Passage program is jointly run by the CPS and the CPD, along with community organizations. Currently, 22 vendors work for the program. The vendors are responsible for hiring neighborhood residents to patrol the Safe Passage routes. Employees are expected to be knowledgeable of the community area they wish to serve, and they must pass a background check. Employees are trained during the summer to provide them with relationship-building skills, de-escalation strategies, and thorough knowledge of other safety protocols. This comprehensive

[7] A map of the welcoming schools can be found here: http://cps.edu/qualityschools/Pages/WelcomingSchoolsMap.aspx (last access March 13, 2017)
[8] https://www.dnainfo.com/chicago/20130522/downtown/cps-closings-school-board-decide-schools-fates-today
[9] Details of the analysis were not provided by the Chicago Police Department (CPD). We analyzed the pilot program but our results were not consistent with those of the CPD.

training enables employees to proactively identify and report safety risks. Employees work part time in the morning and afternoon when students commute to and from the school. The guards support the safety of the students by being vigilant and ensuring that the students get to and from school safely.

As of 2015-2016, the Safe Passage program employs about 1300 workers, who were paid approximately $10 per hour to work for a about five hours a day on weekdays when the school is in session. They work for a few hours in the morning when the school starts and again in the afternoon around school dismissal time. The exact start and end times when the guards are present varies by school. The total cost of the program is $17.8 million for the 2016 fiscal year.

3. Data Sources

The empirical analysis is based on crime incident reports, Safe Passage location data, characteristics of the schools and census block groups. The crime incident data is based on police reports between January 2001 and August 2016 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, location[10] of the crime, along with a classification of the type of incident.

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 highest in the hierarchy[11]. As a result of this classification procedure, reports for crime lower in the hierarchy will be biased downwards.

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[10] In the crime data, the last two digits of the address are withheld. Thus, we can code the data up to 100s-level of the block address, which is approximately one eighth of a mile.

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

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[12].

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.

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 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 also use the community area and census tract boundaries to control for varying time trends. Community areas and census tract boundaries come from the City of Chicago Data Portal. We use the Census 2010 definitions for the Census Tract boundaries.

[12] Violent crimes are defined by 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. While property crime is when crime is committed with a motive to obtain money, property or some other benefit and includes burglary, larceny-theft, motor vehicle theft and arson.

3. Do more eyes on the street reduce crime?

In this section, we exploit the location of the Safe Passage guards to explore whether having more eyes on the street reduce crime. First, we describe how we leverage our detailed geo-located crime data and location of Safe Passage guards to identify their effects on crime. Then we show that their presence does reduce crime, particularly violent crime. Moreover, their presence does not shift crime to nearby areas. Next, our falsification tests show that the reduction in crime is driven by the presence of guards and no other factors. Finally, we test the robustness of the results.

3.1. Empirical Strategy

Our objective is to identify the change in crime due to the presence of Safe Passage guards. Accomplishing this objective requires us to identify the counterfactual scenario, i.e., what would the crime trends have been had the guards not been present on the Safe Passage route.

Two issues arise when identifying the effect of the Safe Passage guards on crime. First, the overall decline in crime in the city of Chicago during the period of our analysis. Thus, it is essential to isolate the effect of the program from the overall declining trend in crime.[13] Second, schools were not randomly chosen to participate in the program. Instead, the CPS started the program in schools that were in areas of particularly high vulnerability. Table 2 and 3 show that CPS implemented the program in more vulnerable schools located in high crime areas, where students tend to be mostly minorities and low income.

The correlation between crime and both observable and unobservable characteristics of the Safe Passage routes' locations is a challenge for the identification of the causal effect of Safe Passage routes on crime. As a result of this correlation, schools that are not part of the program cannot be used as counterfactuals for schools that were "treated". To overcome this issue, we focus on small geographic areas surrounding the Safe Passages. Our strategy follows Di Tella and Schargrodsky

[13] Although shootings have had a dramatic increase in 2016 compared to 2015 (http://crime.chicagotribune.com/chicago/shootings/) Figure A.2 in the Appendix shows that the overall number of criminal incidents have declined at the city level

(2004) in replacing street blocks[14] by cells of one eighth by one eighth miles (i.e. 1/64 of a square mile). Figure 2 illustrates the strategy, where cells that have a Safe Passage route are designated as *Safe Passage Cell.* The strategy leads to areas of equal size, which are approximately the same length as a standard Chicago block. Given the small area of these cells, we are confident that a guard standing on the Safe Passage route is able to monitor it.

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. Consequently, we label these areas as *One Cell Over*, *Two Cells Over*, and *Three Cells Over*. In turn, the cell definition allows us to analyze the potential spatial displacement effects of crimes into neighboring areas.

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 from evening hours (5:30 pm to 6:30 am) when they are not present. Furthermore, we 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.

We focus on the number of violent and property crimes rather than per capita rates for several reasons. First, we are interested in analyzing how the program affects the number of incidents rather than the intensity of crime for a given number of people. Second, since our area of interest is a set of 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.

[14] As a robustness check, we repeat the exercise at the Census Block level (2010 Census Block definitions) and get similar results which are presented in table A.II.1 in the appendix.

We also match cells with other attributes of the Safe Passages, such as the corresponding school for the Safe Passage route. The school location allows us to identify when the program started in the route contained in the cell. During the expansion of the program, especially the 2013-2014 school year, the program was expanded to cover schools that were already close to an existing Safe Passage route. Thus, some of the Safe Passage routes are very close to each other and some cells have more than one Safe Passage guard.

The key assumption to our approach is that cells with Safe Passage guards have similar underlying crime trends as the non-guarded adjacent cells. Figure 3 presents the average number of (3a) violent and (3b) property crimes during daytimes for week days of the school year. We distinguish further by $Safe\ Passage\ Cell$s, $One\ Cell\ Over$, $Two\ Cells\ Over$, 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 five pre-program years and for three post-program years. Figure 3a and 3b show that the program was indeed implemented in areas with higher crime incidents but there are no obvious differences in trends before the program implementation. Furthermore, control cells show no significant differences in levels or trends. What is more, after the implementation in the program there's a drop in the average number of crimes when compared to control areas.

The strategy naturally leads to a difference in difference estimator. We estimate the following model

$$\#Crimes_{it} = \beta_{sp} Safe\ Passage\ Cell_{it} + \beta_{oco} One\ Cell\ Over_{it}$$

$$+\beta_{tco} Two\ Cells\ Over_{it} + \gamma_i + \delta_t + u_{it} \quad (1)$$

where $\#Crimes_{it}$ is either the is the monthly violent or the property crime count in cells of one eighth mile by one eighth mile at school times. $Safe\ Passage\ Cell_{it}$ is an indicator variable taking one for cells in the months that are guarded by Safe Passage personnel. We use this fixed effects model since the Safe Passage program was rolled out in a phased manner and the guarded cells started the program at different points in time. The coefficient of interest is β_{sp}. Our hypothesis is that the implementation of the Safe Passage should reduce crime in a cell that has a Safe Passage, which implies that β_{sp} is negative. The control areas are the three adjacent cells, thus the counterfactual change in crime for guarded cells is estimated using slightly farther away areas. The additional terms in the equation, $One\ Cell\ Over_{it}$ and $Two\ Cells\ Over_{it}$, are indicators for the months after the Safe Passage was enacted if the cells are one or two cells over. One key advantage of this specification is that identifies whether there's displacement of crime to nearby areas. For instance, if crimes are simply transferred to nearby areas then estimates that omit the variables $One\ Cell\ Over_{it}$ and $Two\ Cells\ Over_{it}$might overstate the effectiveness of the Safe Passage program. We complete the model with cell fixed effects (γ_i) to control for persistent differences in crime across the cells and, time fixed effects (δ_t) that control for secular trends in crime. u_{it} is the error term, i.e. the unobserved characteristics of crime in cell i at time t. As it has become standard in the literature, we estimate our count data model in equation (1) using a Poisson regression.

3.2. Base Results for Crime

In this section, we present the results of the overall effect of the Safe Passage program on crime. We estimated our model for the period January 2006 to August 2016.[15] Results are presented in Table 4.[16] Column (1) uses only the more basic measure of proximity to guard presence, $Safe\ Passage\ Cell_{it}$, which takes value one for every month after the program was implemented for every cell that has a Safe Passage route. This regression takes as control the adjacent cells (up to the third one) as control groups. The coefficient on the Safe Passage cell is negative and significant, implying that the presence of Safe Passage guards reduce crime. [17] Violent crimes see a statistically significant decline of 14.3%, while property crimes a non-significant decrease of 3.4%.[18]

[15] The program started in the 2009-2010 school year, so we use few years before the program as the pre-treatment period. We estimate the model using the entire crime data set available to us (January 2001 onward). Results are very similar and are presented in the Appendix in Table A.1.1.

[16] We also use an alternative clustering strategy, which is clustering at the cell level. The results are presented in appendix Table A.1.2 and the standard errors are lower in this case.

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

[18] A possible concern might be that spatial displacement could be driving our results. To address this, as robustness we drop observations for which the variables *One Cell Over* and *Two Cells Over* equal one, and keep only *Three Cells*

The above results indicate that the Safe Passage program did result in significant reductions in crime. However, instead of reducing crime, the presence of Safe Passage guards could potentially have displaced crime to nearby, unguarded areas. To determine whether there are significant displacement effects, in columns (2) and (6) of Table 4 we control for areas that are one cell over in the post treatment period. Results for the guarded areas remain unchanged with no evidence of displacement to the adjacent areas.

The complete model is presented in Columns (3) and (6). The magnitude of the effect of Safe Passage routes on crime is reduced marginally as the reference group for comparison is changed from all the adjacent cells to the third adjacent cell, but the results remain statistically significant. Although violent crime is estimated to decline by 14.1%, the decline in property crime is not statistically significant at conventionally accepted levels. The estimated coefficients for *One Cell Over* and *Two Cells Over* indicate that there is not a significant increase in crime in the adjacent cells. The coefficients are marginally positive in a few specifications, but statistically insignificant. Overall results remain robust, with no evidence of displacement to adjacent areas.

These results suggest a decline in overall crime, with violent having a statistically significant decline of 14%. However, the estimated effect on property crime is not statistically significant.[19] The hierarchical classification procedures for crime may account 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 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.

Over as a control variable. The idea behind this analysis is to leave a buffer zone around the treated cells, which is excluded from the analysis. The results when this buffer zone is deleted are presented in Table A1.3. Again, the results do not differ significantly from the base specification.

[19] Table A1.4 on the Appendix shows the same analysis using OLS instead of a Poisson regression. Results are consistent with the ones presented here.

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 community trends, we include in columns (4) and (8) of Table 4 community-specific time trends. Results do not change significantly under this specification.[20]

As the program 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 the models in Table 4 controlling for the intensity of treatment by including a variable representing the number of Safe Passage routes in a cell.[21] Results of this specification are shown in Appendix Table A1.6. The coefficients obtained under this specification are consistent with our main results and reaffirm our finding that results are not driven by certain areas which had more intensive policing.

3.3. Additional Results for Crime: Falsification and Robustness Tests

In this section, we present a set of tests of our base results for the effect of Safe Passage Routes on Crime. We first check for no preprogram effects or at times that guards are not on duty. Next, we show the robustness of our results to the definition of control groups, length of sample, geographical definitions of study area and estimating equation.

3.3.1. Falsification Tests

We begin presenting a set of falsification tests. As a first validity check we test our equal trends assumption by testing the program before it started. We compare changes in violent and property crimes five, six and seven years before the beginning of the Safe Passage program. Next, we exploit the timing of incidents and the times when guards are present and show that there are no

[20] Although the boundaries of Chicago's community areas were drawn in the 1930s and they are still used locally to refer to various areas of the city today, they are quite large and sometimes quite heterogeneous. As a robustness test, we also include census tract and Safe Passage specific time trends. Results presented in Appendix Table A1.5 remain robust.

[21] 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.

effects at these times: nights, weekends, and summer months. Finally, we show that the decline does not take place before the guards arrive, but they last after they leave.

Placebo Safe Passage Programs. We leverage the length of our data set to conduct placebo experiments in pre-program periods. Instead of using the program year, we define a placebo program year of five, six or seven years prior to the implementation of the program. The results are summarized in Table 5. The effect of these placebo experiments is positive but statistically insignificant for both violent and property crime. These are consistent with the fact that Safe Passage routes where effectively placed in high crime corridors. And the lack of a significant decline in crime during placebo years suggests that the decline in crime during times when a cell is part of the Safe Passage program is indeed a result of the program itself.

Falsification Tests: Non-school times. A possible threat to our identification strategy 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 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 counts in Safe Passage cells relative to adjacent cells could have been produced indirectly by improvements in the conditions of these cells rather than as a direct result of the presence of the program's guards.

If a general improvement in the condition of Safe Passage cells led to a reduction in crime counts in the cells, then there should not be any differential effects on crime for times when Safe Passage guards are present relative to times when schools are not in session. We use this timing to conduct a series of falsification tests. We test whether there is a reduction in crime rates in Safe Passage Cells relative to adjacent cells during times when guards are not present – nighttime (5:30 pm – 6:30 am), during summer months when schools are not in session (July and August), and on weekends.

The results are shown in Table 6. Columns (1) and (4) summarize the results for night time, columns (2) and (5) present the results for the summer months, and columns 3 and 6 present the results for weekends. None of the results are statistically significant at conventional levels of significance. These results suggest it is the presence of Safe Passage guards that produced our finding that crime counts declined in Safe Passage cells relative to adjacent cells.

Variation within School-day Times. We can further exploit our data with the timing of the incidents and look at differential effects within school-days. To do so, we note that guards are present on the Safe passage routes around 2.5 hours before school starts and 2.5 hours after it ends. For simplicity, we round to three hours. We divide our data into three groups times: 3 hours before the guards are present, the time when guards are present, and three hours after they have left. We estimate the following equation:

$$\#Crimes_{it} = \sum_j \beta_j \, Safe\ Passage\ Cell_j + \sum_j \beta_j \, One\ Cell\ Over_j + \sum_j \beta_j \, Two\ Cell\ Over_j + \gamma_i + \delta_t + u_{it} \quad (3)$$

where *j=3 hours before, while guards are present, and 3hours after*. As before, *Safe Passage Cell* is a binary variable that takes one at *j* times in the months after the program was implemented, and zero otherwise. As before we include controls for *One Cell Over*, *Two Cells Over*, and the control group are *Three Cells Over*. We include cell fixed effects and time of day-school year fixed effects. The time of day effects capture the hourly trends within the day, that is, if the Before, Guarded Time, and After times are in the morning or afternoon. In this specification, we aggregate to the school year, as a monthly/hourly aggregation would produce excess of zeros.

Figure 4 plots the coefficients of estimating equation (3) and the full results are summarized in Table 7. Results show that violent crimes decrease when guards are present and after they leave. Property crime. show a similar pattern but the effect is not statistically significant. We also plot the coefficients of the effect on weekends. We find no effects on weekends for the same hours during which guards are present during the week.

3.3.2. Robustness Checks

We begin a set of robustness checks by exploring alternative ways of constructing control groups: Propensity Score Matching and using later years as controls for earlier ones. Next, we check whether the results are robust to the definition of geographic area. Our main results remain unchanged.

Construction of Control Group: Propensity Score Matching. Our first robustness check is the construction of control groups. We use a matching procedure to identify control areas. In our main specification, we compare areas that have Safe Passage routes to the adjacent non-guarded areas.

As an alternative, we identify the 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.[22] 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.[23] We match the neighboring schools to the cell and classify the schools as either in the cell, one cell adjacent or two cells adjacent. Including school and census block group characteristics for in the matching procedure ensures that the cells that are used as controls are similar to the ones that got the treatment.

Table 8 presents the results when the control group is obtained using propensity score matching. Under this specification, we find results consistent with our earlier analysis, with violent crime declining by 11.0% and no significant effect on property crime. The results are also similar when we include community specific time trends (columns 2 and 4).

Finally, we use the night and weekend timings as the basis for falsification tests using the control areas identified by our matching procedure. The results obtained are robust to these identification tests: crime does not decline significantly in times when guards are not present.

[22] Table A.1.7 in the appendix summarizes the covariate balance for the matched sample. In the appendix Table A.1.8, we also include the results of matching to the closest neighbor with no replacements (and common support). The results obtained in this analysis are consistent with the ones obtained by using the two closest neighbors. Covariate balance for this is presented in Table A.1.9.

[23] 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 randomly assigns the cell to one of the census blocks.

Construction of Control Group: Asynchronous Program Rollover. Given that the program was rolled out in a phased manner, we can exploit it as an additional robustness check. To exploit this variation, we restrict the time period of the data to the 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". As before we use a difference in differences strategy like the one described in equation (1).

Results under this alternative strategy remain consistent with the results obtained earlier and are summarized in Table 9. Column (1) shows that violent crime declined by 16.2% during day time periods in areas where guards are present. However, there was no significant effect on property crime. Columns (5)-(8) present falsification tests with the same specification for night times and weekends when the guards are not present. Results are not statistically significant, reaffirming our finding that there was a significant decline in crime around the guarded schools when guards are present.

Robustness to the definition of geographic area: Census blocks. Next, we test the robustness of our results to the choice of geographic units on our results by using census blocks as the unit of analysis. Rather than using equal-sized cells as the unit of analysis, we focus here on census blocks, which may be a more natural unit of reference. We use the 2010 census block boundaries provided by the City of Chicago Data Portal. Like Di Tella and Schargrodsky (2004) and following our main approach, we identify blocks with *Safe Passage* guards, *One Block*, *Two Block*, and *Three Blocks adjacent* to the Safe Passage blocks.

The results of estimating equation (1) using the alternative geographic unit are shown in Table 10. Columns (1)-(4) show results for violent crime and columns (4)-(8) present the results for property crime. Again, our preferred specifications are columns (3) and (6). The results are consistent with our previous findings: the Safe Passage program produces a 14% decrease in violent crimes for census tracts with a passage relative to adjacent tracts, and again the effect on property crime is not statistically significant.

3.4. Heterogeneity in the Results for Crime

We begin this section by exploring whether the effects are short-lived or not. Next, we investigate if the Safe Passage guards had a different effect in high crime areas than low crime areas. We close this section by looking at the effect that the Safe Passage guards had on adjacencies of High Schools.

Effect by Duration of Treatment. We begin by analyzing the effects of the Safe Passage program on crime by duration of treatment. The analysis helps to determine whether the program has long term effects or if the decline in crime is limited to the first few years of treatment. For this estimation, we split the data into subsamples based on the length of time in which the program has been implemented. We divide the overall treatment effect into the effect for the first year of program, the second year, and more than two years after the program was started on a route.

The coefficients of interest are presented in Table 11 for violent and property crime. As indicated by the negative and significant coefficients for violent crime, the Safe passage program has a persistent effect on crime. The effect of guards continues to be negative and significant even after two years of being designated as a Safe Passage area. The coefficient on the interaction term "*Safe Passage Cell * 2 or More School Years*" is high, showing a 19% decline.

The strong estimated effects of the program could potentially be confined primarily to the schools that were first part of the program because they were in the most crime-ridden neighborhoods. It also is possible that the results are primarily associated due to the major expansion in 2013 as this expansion incorporated all the welcoming schools. To investigate these issues, we test whether the estimated effects vary by school year.

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 second phase was in the 2103-2014 school year when the program was expanded to 51 welcoming schools to alleviate safety concerns of students. Students from recently closed schools faced a severe risk of potential crime as they often had to cross gang boundaries to go to the welcoming schools. The third major expansion of the program took place in the 2014-2015 school year, when 32 Safe Passage routes were added. A few schools were also added in other years,but we focus on these major expansion periods to when testing for potential heterogeneous effects.

The identification strategy is the same as before but we allow for the effects to differ depending on the year when a Safe Passage route was added to the program. The treated cells for this analysis are the cells containing the Safe Passage route during a year, while the control cells are the first, second and third adjacent cells to the treated cells. In constructing the data set, 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.

Figure 5 is similar to Figure 3, showing the average number of violent and property crimes before and after the program for each of the three major program phases. The descriptive evidence suggests a reduction in violent crime in treated areas for all three phases and a reduction in property crime for the latter two times. Figure 5b suggests that the effect of the program on violent crime disappears during the second year of implementation, and Figure 5c shows some evidence of a displacement effect on violent crime during the second year.

Table 10 summarizes regression results for the heterogeneous effects of the program. There is a significant reduction in crime for the three major phases of the program, with violent crime declining by 12.5% to 15.4%. As the descriptive evidence suggests, the rollout of the program in 2013-2014 and 2014-2015 also led to a decline in property crime. However, the first 35 Safe Passage routes did not witness a decline in property crime. Although we did not find an overall reduction in property crime, this analysis does suggest that there was a reduction in property crime for the later phases of the program.

To explore these concerns, we estimate our basic model described in equation (1) for the three major expansions. We begin presenting graphical evidence in Figure 6. The descriptive evidence shows reduction in violent crime in Safe Passage areas for all three expansions. For property crime, the reduction only happens in the two latter ones. Note on Figure 6 (c) that the effect of the program on violent crime disappears on the second year of implementation. In Figure 6 (e) there is a little evidence of the program effectiveness and evidence of displacement effect of violent crime on the second year.

Table 12 presents regression results for the three program expansions. The identification strategy is the same as in Table 4.[24] Our estimates show a significant reduction in crime for the three major rollouts of the program, with violent crime declining by 12.5% to 15.4%. As the descriptive evidence, our estimates show drops in property crimes for schools that adopted the program in 2013-2014 and 2014-2015.

These results clarify our previous results on the dynamics of the program. 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.

Effects on High Crime Areas To analyze the differential effect that the Safe Passage program had on high crime areas, we classify high crime areas as those with higher than average crime for the three years before the program started, that is 2006-2008.[25] We then estimate Equation 2 including an interaction term for being in a high crime area and having a Safe Passage route to capture the additional effect in the high crime areas. Results are shown on Table 12. We find that the program is less effective in reducing violent crimes in high crime areas than in low crime areas. The average reduction is 18%. Perhaps a more interesting result is that we find a statistically significant reduction in property crimes of about 6% in high crime areas.

Effects on High Schools Finally we focus our attention on high schools.[26] The results of the differential effect on high schools is summarized in column four of Table 13. We find that the overall reduction in crime is primarily driven by high schools. Guarded areas around High Schools show a 17% decrease in violent crimes and an 11% in property crimes.

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[24] While constructing the data set, we exclude all cells that were treated in the other program years. For instance, cells which are control cells for the program year 2009-2010 but get the Safe Passage route in the latter periods are excluded from the analysis for the year 2009-2010.

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

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

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

It has been seen that the presence of Safe Passage guards reduces violent crime without displacing it to neighboring areas. Next, we explore whether having more eyes on the streets improves attendance. We begin this section by describing our empirical strategy. Then we show that schools in Safe Passage areas see an increase in their attendance.

4.1. Empirical Strategy

The focus now is exploring the changes in attendance driven by the Safe Passage program. To do so we augment our crime data with school level data on attendance rates and other school level characteristics.[27] The model we use to identify the changes in attendance rates mimics our analysis of crime rates. We use a difference in differences estimator of the form,

$$\Delta Attendance_{it} = \beta\ Safe\ Passage\ School_{it} + \gamma_i + \delta_t + u_{it} \qquad (3)$$

Where $\Delta Attendance_{it}$ represents the change in attendance rate for school i in year *t*. $Safe\ Passage\ School_{it}$ equals one if school *i* has a Safe Passage program in place in year *t*, and it equals zero otherwise. The control group for the analysis comprises other public schools that are not yet part of the Safe Passage program. To get the fixed effects differences in differences we complement the equation with school and year fixed effects. Standard errors are clustered at the school level.

4.2. Base Results for Attendance

Table 14 presents the estimates for the effect of the Safe Passage program on the change in attendance.[28] We find that schools in the Safe Passage program experience a 1.6 percentage points

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[27] 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

[28] We also exclude the schools which 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.

increase in attendance (Column (1)). Which implies that attendance in the participating schools increased at a faster rate than schools not enrolled in the program.

A potential explanation is that the effect is driven by 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 change in attendance is much higher at 2.5 percentage points. When controlling for welcoming schools we see a decrease in their attendance.[29] This suggest that as new students coming from the closed schools enrolled in the welcoming schools, the change in composition of students led to higher rates of absenteeism.

Our identification depends on the relative similarity of schools. Thus, a potential concern is whether public schools not in the program are a good control group. In columns (4) to (6) of Table 14 we restrict the sample to include only schools in the same community areas as the Safe Passage schools. The assumption is that public schools within a community area are likely to be similar. Results remain robust showing an increase of 2.5% in attendance rate.

For robustness, we also 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.[30] We use the propensity scores to identify the two closest schools to a treated school in the range of common support.[31] The results using this matching approach, presented in Table A.1.10 in the appendix, are consistent with our earlier results.[32]

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[29] 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.
[30] If any of these characteristics is missing for a school, we replace the missing data with the average value for the sample.
[31] The covariate balance table for this matching analysis is presented in Table A.1.11.
[32] These results use schools as control once. As an additional check, we use weighted least squares where control schools appear twice and the results remain robust.

Our underlying assumption in Table 14 is that the other public school used as controls have similar underlying attendance trends. Figure 6 shows attendance rates and changes in attendance rates trends for our sample period. Before the year 2007 all schools display similar trends in attendance. However, for the first schools that were included in the Safe Passage program, there's a significant drop in the previous years of the program. This potentially explains why this schools were the first to be included in the program. After the introduction of the program there's a significant gain in attendance rates to beginning of the sample rates. Schools that received the program with the 2013-2014 expansion present similar changes in attendance rates with a significant gain around the same time that the previous schools saw a significant drop, suggesting students moving from these schools to the other. With the beginning of the program, we see again a reduction in attendance for this schools suggesting students going back to the safer schools.

Finally, schools in the later major expansion of the program look as a cleaner experiment to assess the effect of the Safe Passage guards. These schools show similar preprogram trends in attendance rates and in changes in attendance rates. The plot also shows significant gains in attendance after the implementation of the program

Table A1.12 presents results of estimating equation (3) by major expansions of the program. Like the graphical evidence shows there's significant gain for the earlier treated schools, and moderate gains for the later expansions. Once we control for welcoming schools in the 2013 expansion, the effects are bigger but also more imprecise. These effects are combining those students joining the safer schools but also leaving for the now more safer schools that started the program in 2013. It can be seen in Figures 1 and A1 that many of these schools are in the same neighborhoods and close together. Since some of the schools in the neighboring area had joined the program in 2009, this would have made the area safer and led to a slight gain in attendance even before they became part of the program. For those that joined the 2014-2015 year the effect is much more precise but lower. Overall, the presence of the Safe Passage guards shows a positive and significant effect on schools. Migration of students between Safe Passage schools as the program rolls out may attenuate the effects. However, the gains in attendance are well identify in the "cleaner" quasi-experiments.

5. Cost Benefit Analysis of the Program

Cost benefit analysis of the Chicago's Safe Passage program can help improve policies elsewhere. The results discussed earlier provide strong evidence that there is significant decline in crime around the Safe Passage schools. The estimation strategy described in Section 4 can then be used to estimate the direct benefit of the program due to crime reduction. In this section, we use these results to estimate the direct benefit of reduction in crime near the Safe Passage schools.

A starting point for the benefit analysis involves estimating the potential benefits accrued for the avoided crimes. The costs of crime literature suggest that the relevant measure for policy analysis is the willingness to pay or ex-ante approach measure of 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 using the victim costs or ex-post approach. These costs are often derived by using civil jury awards and capture direct costs like injuries sustained during the incident and indirect costs where jurors try to compensate the victims for their pain and suffering. We use Cohen and Piquero's (2009) cost of crimes estimates which report both approaches and have become the standard reference in the literature (Chalfin and McCrary 2015). Table 15 column (1) and (2) shows these estimates in 2015 dollars.

To calculate the counterfactual number of crimes avoided by the program, we estimate the effect of the program on each subcategory of crime: murder, rape, robbery, assault, burglary, larceny and motor vehicle theft. We use the estimation strategy described in Section 3. The point estimates and clustered standard errors are shown in columns (3) and (4) of Table 15. Column (5) completes the table with the preprogram averages for each type of crime for the Safe Passage cells.

The estimated effects for each crime subcategory are more imprecise and thus we conduct a simulation exercise to account for the number of potentially reduced crimes. 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 Figure 7. The figure shows estimates using Cohen and Piquero's (2009) willingness to pay estimates illustrated in Column (1) of Table 15.

Results from the simulation show that the mean benefit of the program based on willingness to pay due to reduced crime is about $100 million per year, while the total cost of the program is $17.8 million for the 2015-2016 school year.[33] Simulations show that the probability that the program's benefits do not exceed its costs for the 2015-2016 school year is about 2%. We also include simulation results with more conservative ex-post cost of crimes estimates. Figure A.3 summarizes the results for the same simulation exercise using the. The estimated benefits of the program are still quite large, with the estimated mean benefits almost doubling the cost of the program, and the likelihood of that the benefits do not exceed the cost of about 15%. Overall, our simulation results show that placing civilian guards around schools is a relatively cheap way of reducing crime.

6. Conclusion

In this paper, we examine an alternative way of policing to increase student safety: hiring civilians to stand guard near schools for a few hours each day. To study this alternative strategy, we focus on the Chicago 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

[33] Simulations contain 100,000 iterations.

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.

The program provides an interesting insight of policies to increase safety. By placing civilian guards, the reduction in crime is driven through deterrence rather than incapacitation. One of the important questions for deterrence research is the "degree of correspondence between actual and perceived risks" (Chalfin and McCrary, 2015). We believe that for the Safe Passage program, perceived risks are more closely aligned to actual risk as the program is well advertised. Safe Passage routes have yellow hoardings which read "Safe Passage", indicating that the place is being monitored during the school hours. Also, the guards are very prominent as they wear neon jackets and are thus easily identifiable. These routes are also available on the on the City Data Portal, School websites and the CPS website.

In addition, we 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 restricted 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.

Overall, our results suggest that placing civilian guards around schools is an inexpensive and effective way of increasing safety and attendance. We evaluate the cost effectiveness of the program in terms of crime avoided. We find that the Safe Passage program is a relatively cheap way of increasing safety. We estimate that based on estimates for willingness to pay to avoid crime the benefits of the program are around $100 million a year, whereas the estimated total cost of the program was $17.8 million for the 2015-2016 school year. Although indirect benefits – test scores, graduation rates, future job outcomes – are harder to measure and beyond the scope of this paper, they are likely to be considerable. Together, these estimates suggest that the program's direct benefits are substantial, and are much greater than the costs.

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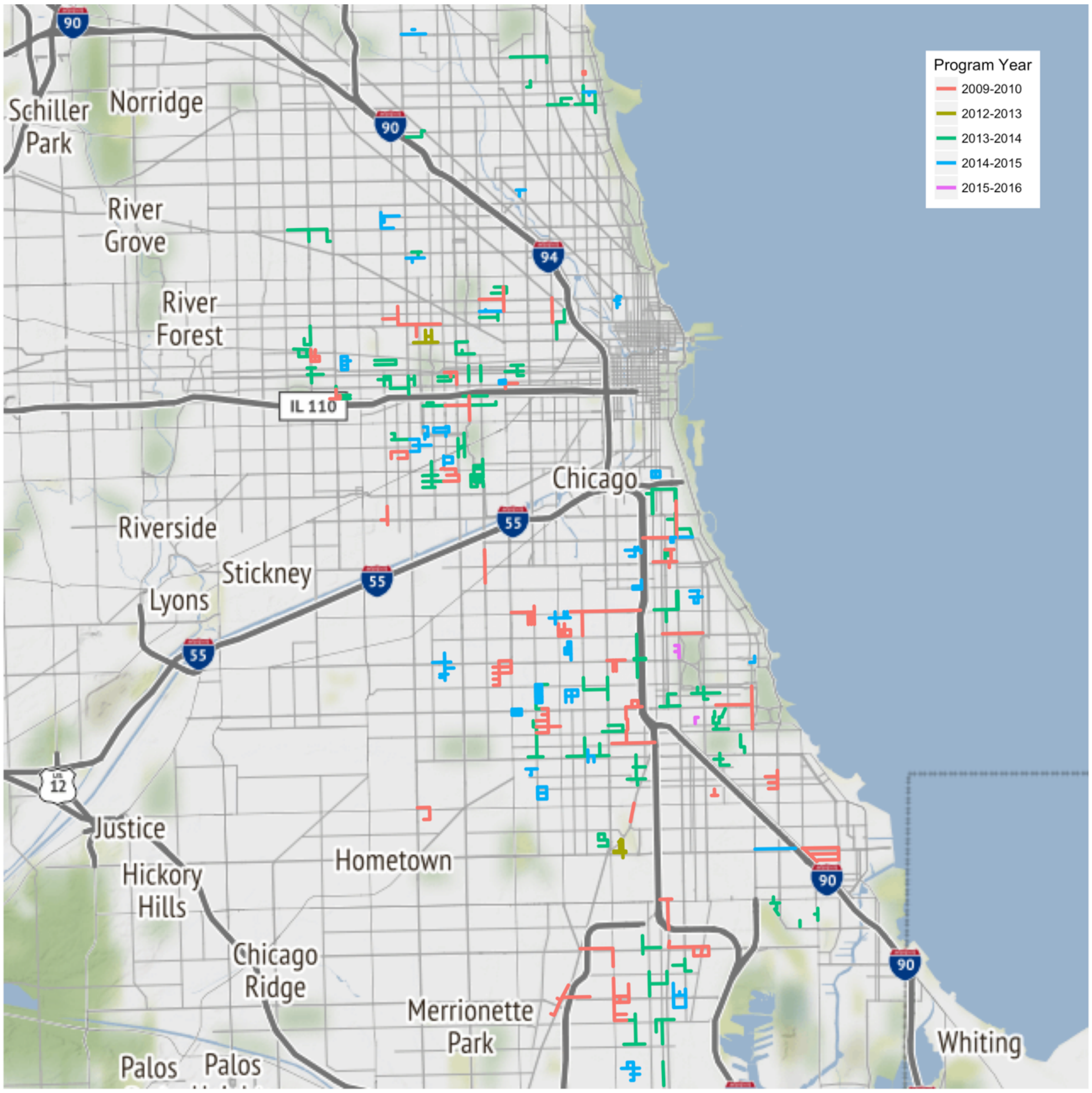
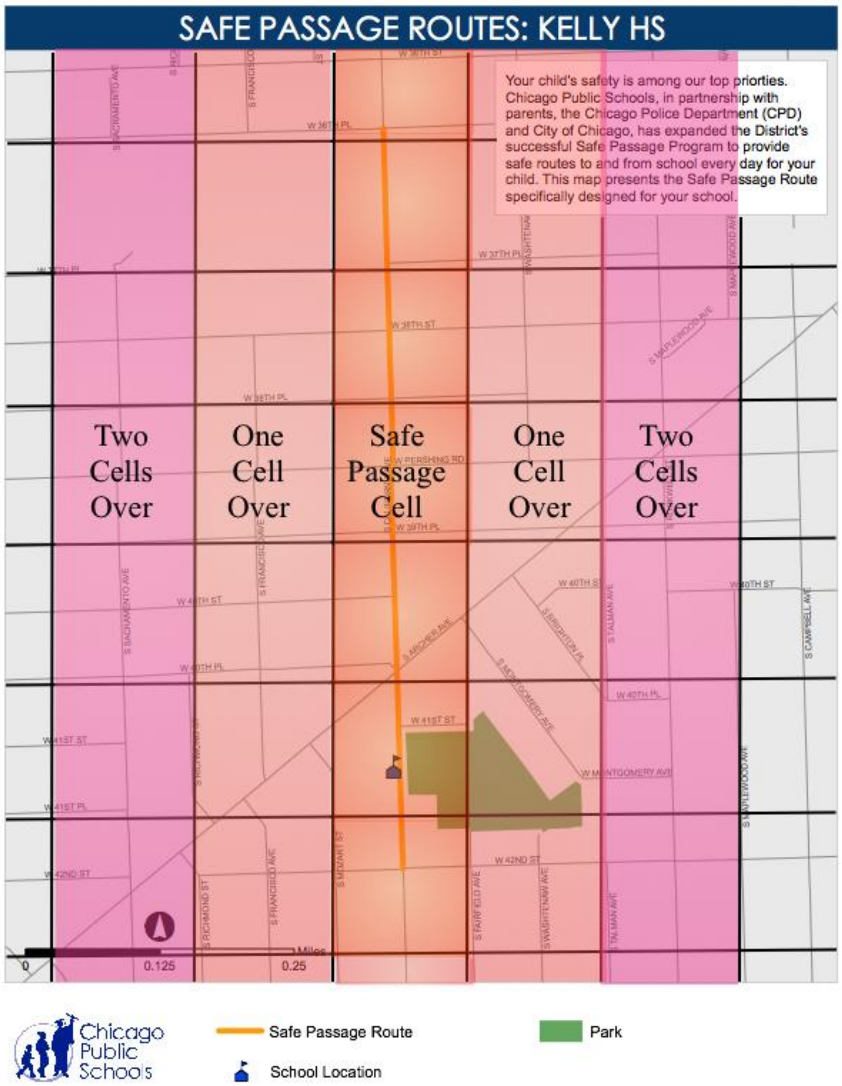
Figure 1: Safe Passage Routes



Source: Chicago Data Portal and FOIA request.

Figure 2: Identification Strategy



Source: CPS website and authors' modifications

Figure 3: Crime Trends

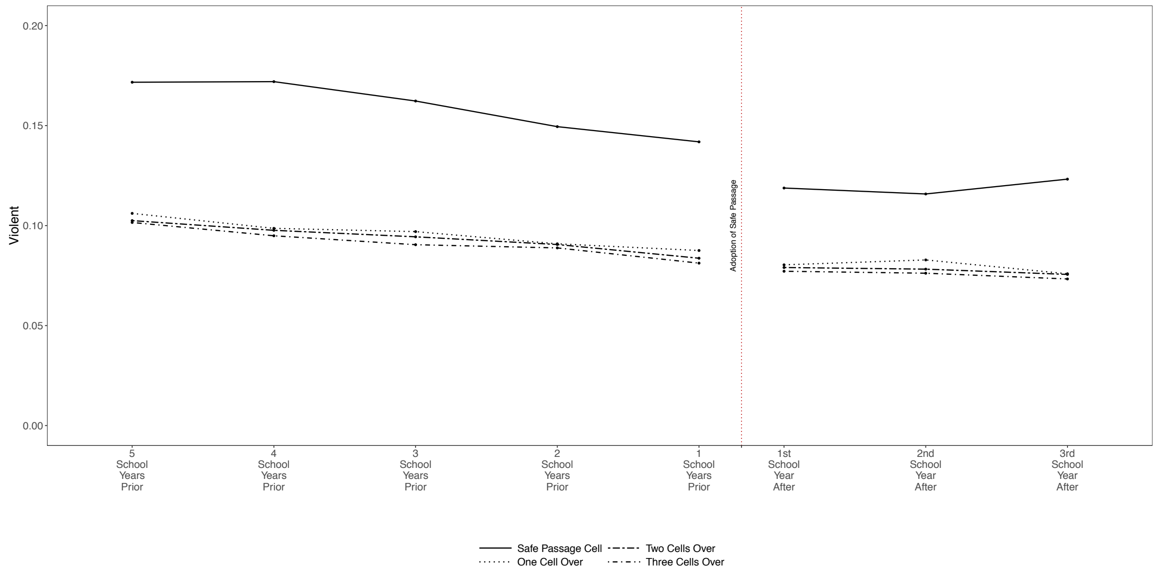


Fig 3a: Violent Crimes

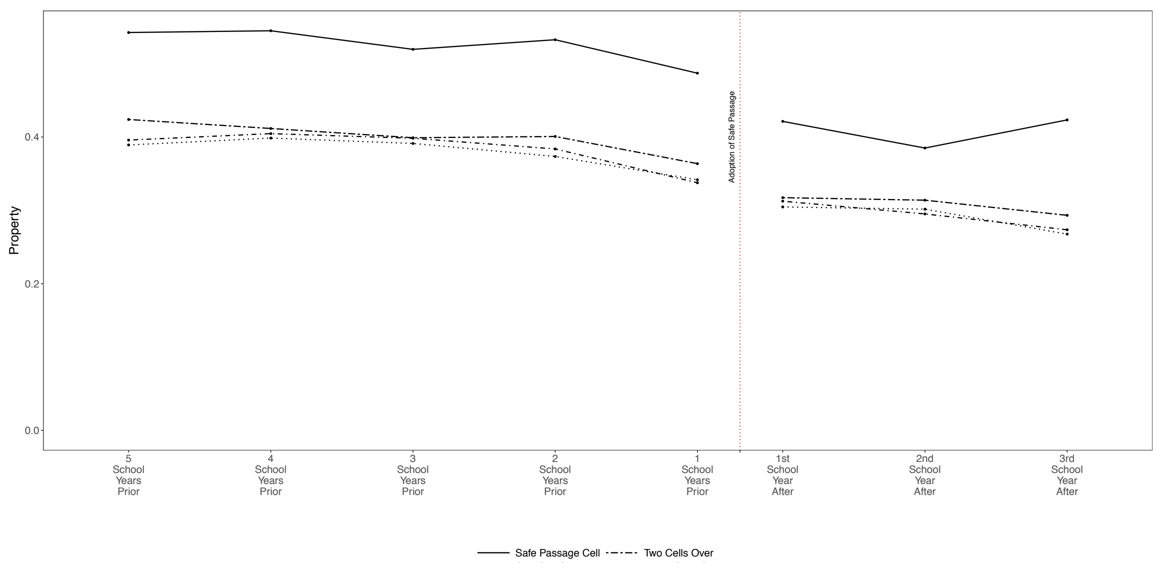
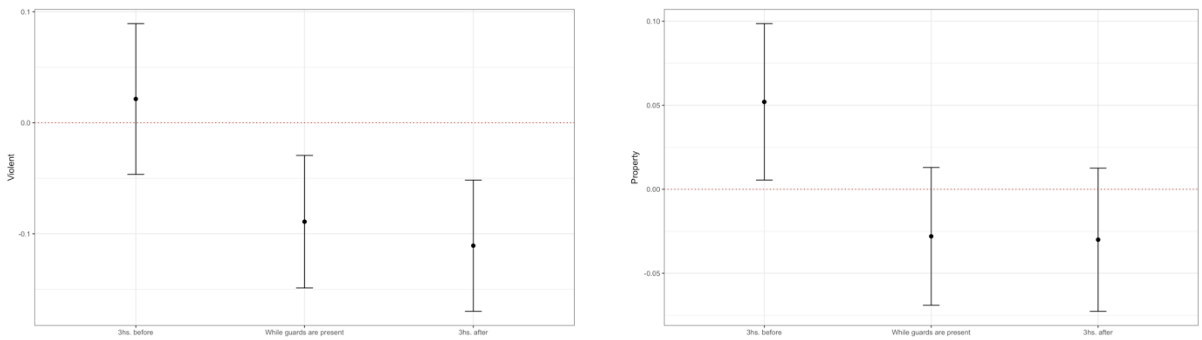


Fig 3b: Property Crimes

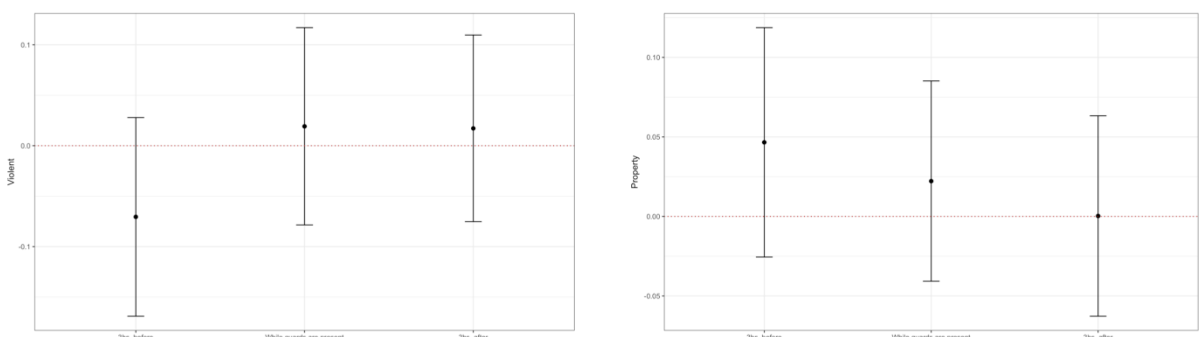
Note: The figures show average violent and property crime in Safe Passage Cells, One Cell Over, Two Cells Over and Three Cells during week days when school is in session. Given the asynchronicity of the program's rollover of the program, we normalize to zero the last school year and the first year of the program on each cell. The dotted red line marks the end of the pre Safe Passage years and the start of the program.

Figure 4: Intraday Variation in Crime



School Days *Weekends*

Fig 4a: Violent Crimes

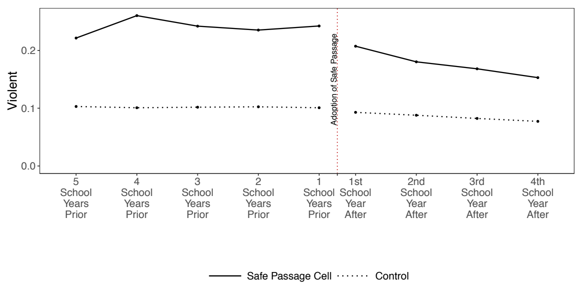


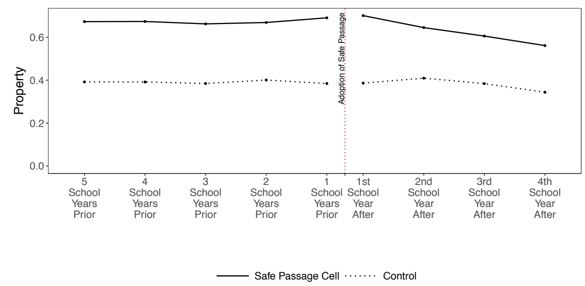
School Days *Weekends*

Fig 4b: Property Crimes

Note: The dot represents the estimated coeffcient in Table 9, while bars denote 95% confidence intervals. Figures (a) and (c) present estimates of Violent Crimes and Property Crimes respectively on School Days for 3 hours before the guards arrive, the 3 hours that guards are present and for the 3 hours after they have left. Figures (b) and (d) repeat the experiment for Weekends.

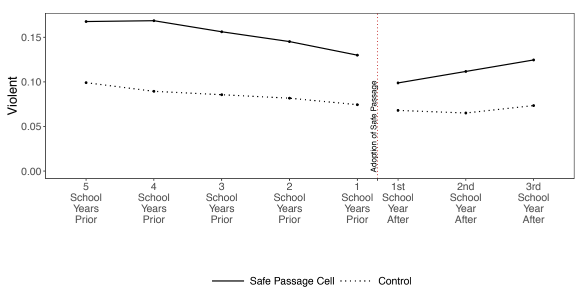
Figure 5: Crime Trends: By Year of Major Program Expansions

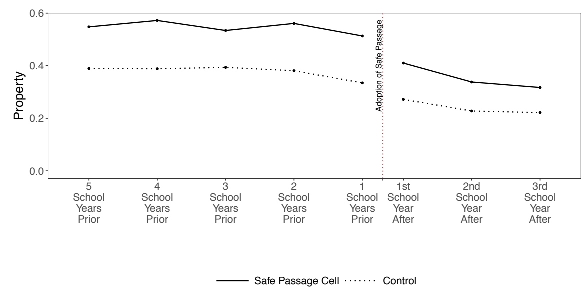




Violent Crime *Property Crime*

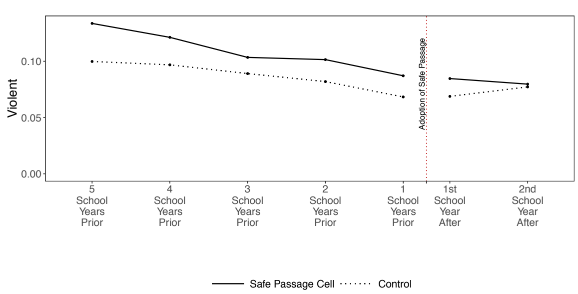
Fig 5a: 2009-2010 School Year

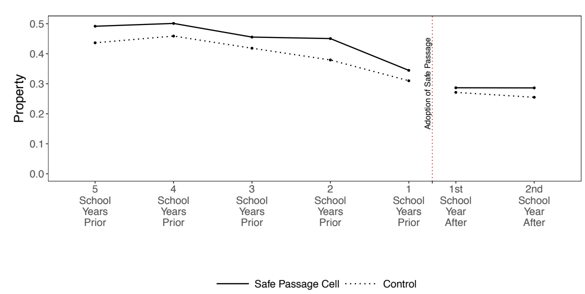




Violent Crime *Property Crime*

Fig 5b: 2013-2014 School Year



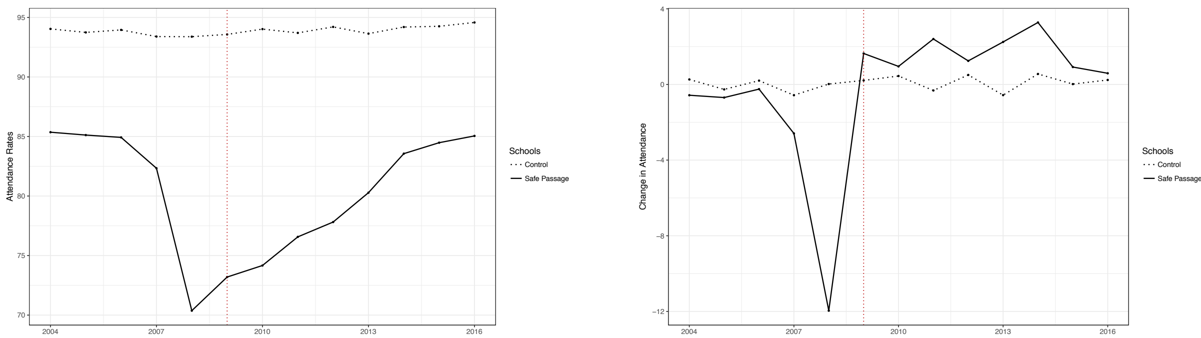


Violent Crime *Property Crime*

Fig 5c: 2014-2015 School Year

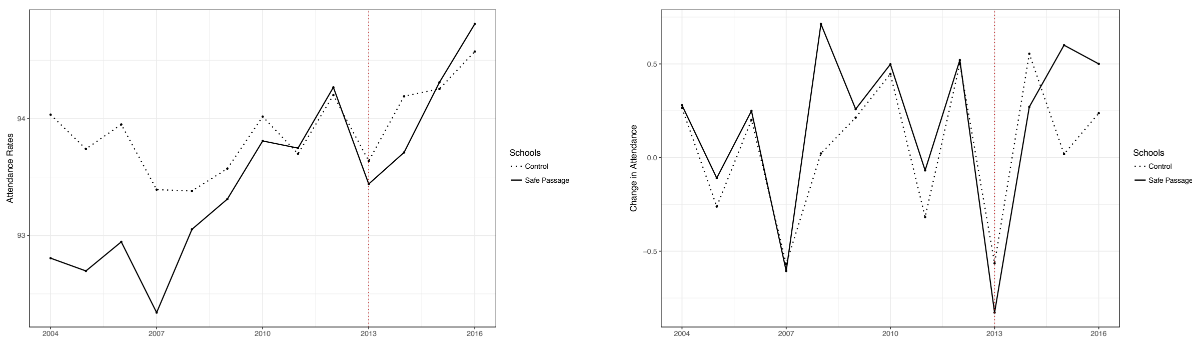
Note: The plots show average violent and property crime in the Safe Passage Cells, One Cell Over, Two Cells Over and Three Cells Over during week days when school is in session. Adjacent cells (One Cell Over, Two Cells Over and Three Cells Over) are bunched toghether in the Control category. We normalize to zero the last school year and the first year of the program on each cell. The trends are plotted for the three major expansions, the 2009-2010 school year (a)-(b), the 2013-2014 school year (b)-(c) and the 2014-2015 school year (d)-(e).

Figure 6: Attendance Trends: By Year of Major Program Expansions



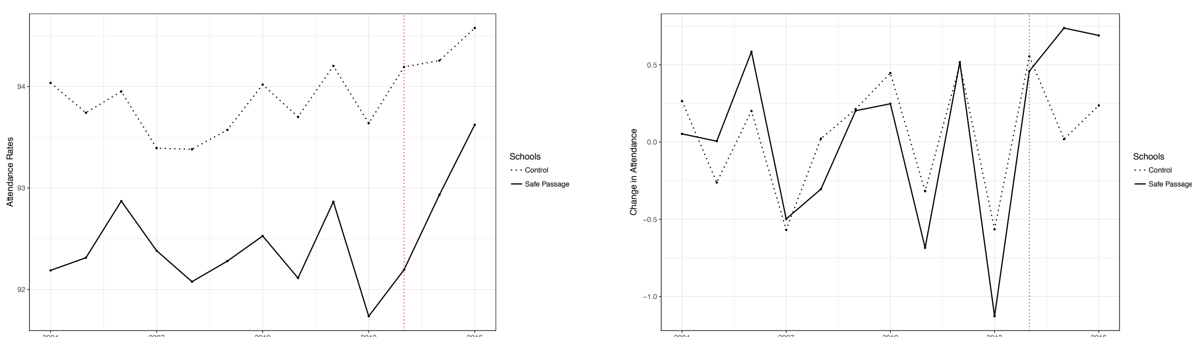
Attendance Rates *Change in Attendance Rates*

Fig 6a: 2009-2010 School Year



Attendance Rates *Change in Attendance Rates*

Fig 6b: 2013-2014 School Year

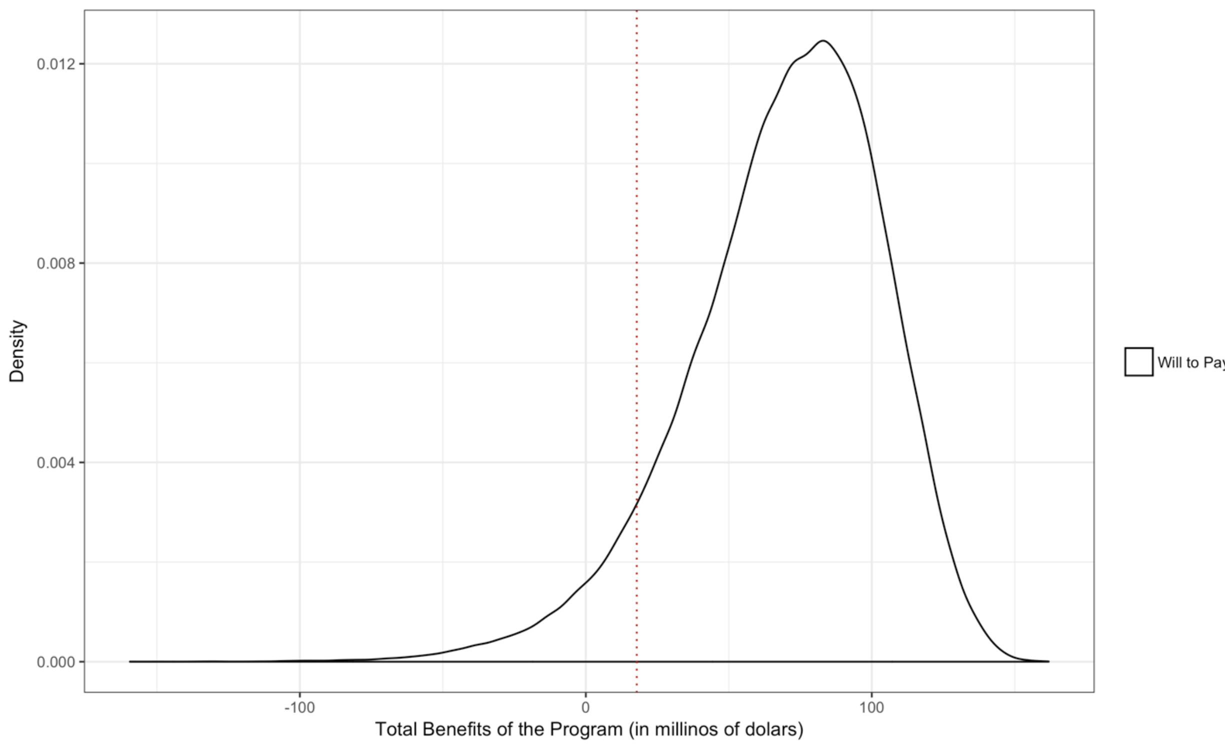


Attendance Rates *Change in Attendance Rates*

Fig 6c: 2014-2015 School Year

Note: The plots show average atendance rates and average changes in attendance for Safe Passage schools and control schools. Control schools are CPS schools that never had a Safe Passage. The vertical dotted line marks the year of the beginning of the program. The trends are plotted for the three major phases of the program, the 2009-10 school year (Panel a), the 2013-14 school year (Panel b) and the 2014-15 school year (Panel c).

Figure 7: Cost Benefit Analysis



Note: The solid line is the density of possible benefits of the program. The red dotted line is the cost 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. We first draw 100,000 estimates of the program effect of each crime category from a normal distribution with mean equal to the estimated coefficient and standard deviation equal to the standard errors listed in columns (2) and (3) of Table 14. Next, we calculate the benefits of the change in the number of crimes using the pre-program mean and Cohen and Piquero's (2009) cost estimates (in 2015 dollars).

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.

Table 2. Chicago Public Schools with and without Safe Passages.

Descriptive Statistics

	CPS with Safe Passages	CPS without Safe Passages	Diff.
	(1)	(2)	(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)

*p < 0.10, * * p < 0.05, * * *p < 0.01

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.

Table 3: 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	CBGs w. Schools but w/o Safe Passages	CBGs with Safe Passages	Diff.	Diff.
	(a)	(b)	(c)	(d)=(c)-(a)	(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)	(2,778.21)	(2,627.18)
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	1,948.3	1,946.78	-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)

*p<0.10, **p<0.05,* **p<0.01

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.

Table 4. Effect of the Safe Passage Program on Crime

	Number of Violent Crimes				Number of Property Crimes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Cell	-0.1437***	-0.1421***	-0.1410***	-0.1446***	-0.0343	-0.0306	-0.0277	-0.0020
	(0.0316)	(0.0341)	(0.0370)	(0.0363)	(0.0237)	(0.0242)	(0.0262)	(0.0269)
One Cell Over		0.0052	0.0062	-0.0025		0.0129	0.0158	0.0354
		(0.0262)	(0.0294)	(0.0296)		(0.0213)	(0.0231)	(0.0228)
Two Cells Over			0.0029	-0.0033			0.0084	0.0186
			(0.0268)	(0.0261)			(0.0187)	(0.0191)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trend				Yes				Yes
Sample Size	508,376	508,376	508,376	508,376	552,896	552,896	552,896	552,896

*p<0.10, **p<0.05,* **p<0.01

Note: Results are obtained from Poisson regressions 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. The Safe Passage Cell 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 and Two Cells Over (for cells one cell away and two cells away from the nearest Safe Passage, respectively). We include up to three cells over. Standard errors clustered by Safe Passages are reported in parentheses.

Table 5: Falsification Tests - Placebo Experiments

	Five years before rollout		Six years before rollout		Seven years before rollout	
	Violent	Property	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)	(5)	(6)
Safe Passage Cell	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	-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	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

*p<0.10, **p<0.05, * **p<0.01

Note: The specification is similar to Table 4. Standard errors clustered by Safe Passages are reported in parentheses.

Table 6. Falsification Test - Unguarded Times

	Number of Violent Crimes			Number of Property Crimes		
	(1)	(2)	(3)	(4)	(5)	(6)
	Night	Summer	Weekend	Night	Summer	Weekend
Safe Passage Cell	0.0134	0.0327	0.0091	-0.0090	-0.0340	-0.0050
	(0.0273)	(0.0566)	(0.0457)	(0.0253)	(0.0433)	(0.0388)
One Cell Over	0.0235	0.0603	-0.0225	-0.0011	-0.0406	-0.0020
	(0.0246)	(0.0496)	(0.0379)	(0.0204)	(0.0323)	(0.0304)
Two Cells Over	0.0241	0.0469	0.0171	0.0146	-0.0071	0.0372*
	(0.0238)	(0.0522)	(0.0380)	(0.0169)	(0.0269)	(0.0226)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	521,308	77,220	419,548	547,384	109,956	533,392

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. Night time is 5:30 pm to 6:30 am. For, columns (2), (3), (5) and (6) we omit night times. Standard errors clustered by Safe Passages are reported in parentheses.

Table 7: Intraday

	Number of Violent Crimes					Number of Property Crimes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	School Year	School Year	School Year	Summer	Weekend	School Year	School Year	School Year	Summer	Weekend
Safe Passage Cell*3hs. before	0.0231	0.0256	0.0215	0.0682	-0.0705	0.0404*	0.0491**	0.0520**	0.0218	0.0466
	(0.0305)	(0.0321)	(0.0347)	(0.0648)	(0.0502)	(0.0218)	(0.0224)	(0.0238)	(0.0421)	(0.0368)
Safe Passage Cell*While guards are present	-0.0832***	-0.0793***	-0.0891***	-0.0488	0.0192	-0.0375*	-0.0380*	-0.028	-0.1262***	0.0222
	(0.0275)	(0.0287)	(0.0305)	(0.0641)	(0.0499)	(0.0193)	(0.0197)	(0.0209)	(0.0370)	(0.0321)
Safe Passage Cell*3hs. after	-0.1251***	-0.1226***	-0.1107***	-0.0487	0.0172	-0.0335*	-0.0336	-0.03	0.0188	0.0003
	(0.0275)	(0.0284)	(0.0302)	(0.0564)	(0.0472)	(0.0204)	(0.0206)	(0.0217)	(0.0373)	(0.0321)
One Cell Over*3hs. before		0.0081	0.004	0.0216	-0.0985**		0.0305	0.0333	-0.0323	0.0735**
		(0.0294)	(0.0319)	(0.0590)	(0.0449)		(0.0208)	(0.0218)	(0.0397)	(0.0320)
One Cell Over*While guards are present		0.0124	0.0026	0.0011	-0.0155		-0.0019	0.0079	-0.0433	0.0309
		(0.0260)	(0.0281)	(0.0559)	(0.0474)		(0.0182)	(0.0192)	(0.0314)	(0.0313)
One Cell Over*3hs. After		0.0079	0.0198	-0.056	-0.0035		-0.0006	0.0028	-0.0311	0.0202
		(0.0237)	(0.0257)	(0.0519)	(0.0417)		(0.0179)	(0.0187)	(0.0319)	(0.0274)
Two Cells Over*3hs. before			-0.0113	0.0078	-0.0421			0.008	0.0209	0.0530*
			(0.0310)	(0.0595)	(0.0449)			(0.0194)	(0.0351)	(0.0291)
Two Cells Over*While guards are present			-0.0269	-0.05	0.0342			0.0285*	-0.034	0.0559**
			(0.0272)	(0.0578)	(0.0465)			(0.0165)	(0.0311)	(0.0280)
Two Cells Over*3hs. After			0.0318	-0.014	0.0347			0.0098	0.0222	0.0112
			(0.0259)	(0.0501)	(0.0391)			(0.0168)	(0.0298)	(0.0257)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	330924	330924	330924	270600	300630	346236	346236	346236	335412	337458

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. Standard errors clustered by cells are reported in parentheses.

Table 8: Using Matching to analyze the effect on crime

	Day				Night		Weekend	
	Violent	Violent	Property	Property	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Cell	-0.1306***	-0.1214***	-0.0061	0.0075	-0.0161	-0.0033	-0.0218	0.0289
	(0.0249)	(0.0261)	(0.0206)	(0.0180)	(0.0221)	(0.0200)	(0.0355)	(0.0262)
Cells FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trends		Yes		Yes				
Sample size	250224	250224	255216	255216	251472	253968	230100	251004

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. However, the control cells are those propensity score matching identifies as being similar to the treated cells but which did not receive treatment. Night time is defined as 5:30 pm to 6:30 am, while the weekend excludes night time. Standard errors clustered by Safe Passages are reported in parentheses.

Table 9: Asynchronous Rollout of the Program

	Day				Night		Weekend	
	Violent	Violent	Property	Property	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Cell	-0.1619***	-0.1466***	-0.0275	0.0007	-0.0374	-0.0168	-0.0510	-0.0716
	(0.0529)	(0.0523)	(0.0391)	(0.0339)	(0.0470)	(0.0319)	(0.0582)	(0.0508)
Cells FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trend		Yes		Yes				
Sample size	81184	81184	82818	82818	81528	82560	71724	81356

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4 for the period 2009-10 to 2013-14 school year. The control cells are the cells which received the treatment in 2014-15 and 2015-16 school year. Night time is 5:30 pm to 6:30 am and weekend excludes night time. Standard errors clustered by Safe Passages are reported in parentheses.

Table 10: Effect of the Safe Passage Program on Crime (using the Census Block definitions)

	Number of Violent Crimes				Number of Property Crimes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Block	-0.1556***	-0.1576***	-0.1438***	-0.1357***	-0.0144	-0.0096	-0.0045	0.0096
	(0.0337)	(0.0343)	(0.0355)	(0.0348)	(0.0246)	(0.0246)	(0.0249)	(0.0233)
One Block Over		-0.0081	0.0052	0.0064		0.0213	0.0261	0.0354
		(0.0277)	(0.0290)	(0.0282)		(0.0251)	(0.0243)	(0.0230)
Two Blocks Over			0.0438	0.0391			0.0166	0.0196
			(0.0274)	(0.0272)			(0.0248)	(0.0258)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trend				Yes				Yes
Sample Size	783,340	783,340	783,340	783,340	982,832	982,832	982,832	982,832

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. However, the unit of analysis for this table is the US Census Block. Standard errors clustered by Safe Passages are reported in parentheses.

Table 11: Effect on Crime by Duration of Treatment

	Violent Crimes (1)	Property Crimes (2)
Safe Passage Cell * School Year of Adoption	-0.0817*	0.0130
	(0.0479)	(0.0257)
Safe Passage Cell * 1 School Year After	-0.1185**	-0.0119
	(0.0464)	(0.0278)
Safe Passage Cell * 2 or More School Years After	-0.2190***	-0.0214
	(0.0595)	(0.0382)
One Cell Over * School Year of Adoption	0.0197	0.0019
	(0.0393)	(0.0313)
One Cell Over * 1 School Year After	0.0228	0.0303
	(0.0395)	(0.0306)
One Cell Over * 2 or More School Years After	-0.0279	0.0112
	(0.0407)	(0.0239)
Two Cells Over * School Year of Adoption	0.0069	-0.0101
	(0.0237)	(0.0144)
Two Cells Over * 1 School Year After	-0.0119	0.0252
	(0.0271)	(0.0181)
Two Cells Over * 2 or More School Years After	-0.0171	0.0315
	(0.0232)	(0.0222)
Cell FE	Yes	Yes
Month - Year FE	Yes	Yes
Sample Size	570660	628338

* p<0.10, ** p<0.05, *** p<0.01

Note: The table presents results obtained from poisson regressions of the number of crimes per cell per month on explanatory variables for the period January 2006 to August 2016. The unit of analysis in our study is cell of one eighth by one eighth of a mile. We drop the night times (5:30 pm to 6:30 am), weekend and the summer months when the guards are not present. Thus, the regression only includes times during which the school is in session. The Safe Passage Cell is a dummy variable that equals one for treated cells (i.e. cells that have a Safe Passage) in the months after the Safe Passage was enacted. The first and second rows estimate the effect in the first and second year of treatment. The third row captures the effect of being treated for more than two years. All regressions include cell fixed effects and month/year fixed effects. Standard errors clustered by Safe Passage are reported in parentheses.

Table 12: Heterogeneous Effect of the Safe Passage Program on Crime: By Program year

	Number of Violent Crimes			Number of Property Crimes		
Program Year	2009	2013	2014	2009	2013	2014
	(1)	(2)	(3)	(4)	(5)	(6)
Safe Passage Cell	-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	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	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	280794	302206	187302	300298	326692	201930

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. In columns 1 and 4, the Safe Passage Cell equals one for cells that received the program in 2009 in the months after the program was enacted. The same is true for One Cell Over and Two Cells Over (for cells one cell away and two cells away from the nearest Safe Passage, respectively). Similarly, columns 2 and 3 is for the program year 2013, and 3 and 6 for year 2014. Standard errors clustered by Safe Passages are reported in parentheses.

Table 13: Heterogeneous Effect of the Safe Passage Program on Crime: High Crime Areas and Welcoming Schools

	Number of Violent Crimes				Number of Property Crimes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Cell	-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	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	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*High Crime Areas		0.0583				-0.0631*		
		(0.0614)				(0.0366)		
Safe Passage Cell*Welcoming			0.0628				-0.0229	
			(0.0458)				(0.0370)	
Safe Passage Cell*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	508376	508376	508376	508376	552896	552896	552896	552896

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. High Crime areas are area with above median crime counts. Standard errors clustered by Safe Passages are reported in parentheses.

Table 14: Change in Attendance rate

	(1)	(2)	(3)	(4)	(5)	(6)
Treated * Post	1.6309***	2.5209***	2.4178***	1.6456***	2.5364***	2.4249***
	(0.2549)	(0.3187)	(0.3169)	(0.2627)	(0.3226)	(0.3215)
Welcoming*Post			-2.2449***			-2.2553***
			(0.3221)			(0.3199)
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Excludes welcoming schools		Yes			Yes	
Only include schools in neighborhoods which have a Safe Passage				Yes	Yes	Yes
Sample Size	6945	6316	6945	5148	4519	5148

*p<0.10, **p<0.05,* **p<0.01

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 to 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 that received 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.

Table 15: Welfare Analysis of the Program

Category	Willingness to Pay (in $ 2015)	Victim Costs (in $ 2015)	Coefficient	Se.	Preprogram Mean
	(1)	(2)	(3)	(4)	(5)
Violent Crime					
Murder	$13,488,827.80	$5,258,356.60	-0.42	-0.24	15
Rape	$331,505.09	$154,321.34	0.15	-0.13	52.3
Robbery	$44,581.72	$13,717.45	-0.15	-0.04	745
Aggravated assault	$97,165.29	$42,295.48	-0.09	-0.05	377.3
Property Crime					
Burglary	$40,009.24	$2,286.24	0.05	-0.03	1076
Larceney	$4,572.48	$514.40	-0.04	-0.03	3650.3
Motor Vehicle Theft	$19,433.06	$6,287.17	-0.07	-0.04	664

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 4 (Column 3 or 6). The Pre0program mean is the 2006-2008 average monthly crimes for Safe Passage Cells.

Appendix Tables

Table A1.1: Effect of the Safe Passage Program on Crime (Uses data from January 2001 to August 2016)

	Number of Violent Crimes				Number of Property Crimes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Cell	-0.1335***	-0.1386***	-0.1379***	-0.1304***	-0.0255	-0.0203	-0.0135	-0.0173
	(0.0253)	(0.0260)	(0.0272)	(0.0282)	(0.0224)	(0.0225)	(0.0237)	(0.0216)
One Cell Over		-0.0157	-0.015	-0.0164		0.0188	0.0254	0.0109
		(0.0238)	(0.0256)	(0.0248)		(0.0224)	(0.0233)	(0.0227)
Two Cells Over			0.0017	-0.0077			0.0199	0.0123
			(0.0235)	(0.0227)			(0.0175)	(0.0172)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trend				Yes				Yes
Sample Size	778596	778596	778596	778596	823524	823524	823524	823524

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4 for the period January 2001 to August 2016. Standard errors clustered by Safe Passages are reported in parentheses.

Table A1.2: Effect of the Safe Passage Program on Crime (Clustering Standard Errors by Cell)

	Number of Violent Crimes					Number of Property Crimes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Safe Passage Cell	-0.1437***	-0.1421***	-0.1410***	-0.1446***	- 0.1001** *	-0.0343*	-0.0306	-0.0277	-0.002	0.0048
	(0.0287)	(0.0293)	(0.0304)	(0.0302)	(0.0298)	(0.0193)	(0.0196)	(0.0208)	(0.0201)	(0.0201)
One Cell Over		0.0052	0.0062	-0.0025	0.0215		0.0129	0.0158	0.0354*	0.0386**
		(0.0245)	(0.0261)	(0.0259)	(0.0260)		(0.0185)	(0.0194)	(0.0192)	(0.0177)
Two Cells Over			0.0029	-0.0033	0.0049			0.0084	0.0186	0.0175
			(0.0235)	(0.0234)	(0.0235)			(0.0160)	(0.0161)	(0.0149)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trend				Yes	Yes				Yes	Yes
Census Tract specific time trend					Yes					Yes
Sample Size	508376	508376	508376	508376	508376	552896	552896	552896	552896	552896

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. Standard errors clustered by Cells are reported in parentheses.

Table A1.3: Effect of the Safe Passage Program on Crime (Using only third adjacent cell as control)

	Day				Night		Weekend	
	Violent	Violent	Property	Property	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Cell	-0.1437***	-0.1429***	-0.0343	-0.0182	-0.0008	-0.0126	0.0117	-0.0142
	(0.0318)	(0.0304)	(0.0238)	(0.0245)	(0.0235)	(0.0215)	(0.0426)	(0.0328)
Cells FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trend		Yes		Yes				
Sample size	508376	508376	552896	552896	521308	547384	419548	533392

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4, with only the third adjacent cell used as control. Standard errors clustered by Safe Passages are reported in parentheses.

Table A1.4: Effect of the Safe Passage Program on Crime: Using OLS

	Number of Violent Crimes					Number of Property Crimes				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Safe Passage Cell	-0.0412***	-0.0428***	-0.0433***	-0.0396***	-0.0344***	-0.0662***	-0.0686***	-0.0659***	-0.0478***	-0.0407***
	(0.0047)	(0.0047)	(0.0049)	(0.0049)	(0.0052)	(0.0117)	(0.0120)	(0.0126)	(0.0134)	(0.0143)
One Cell Over		-0.0055**	-0.0059**	-0.0034	-0.0011		-0.0084	-0.0058	0.0073	0.0122
		(0.0023)	(0.0025)	(0.0026)	(0.0026)		(0.0078)	(0.0084)	(0.0081)	(0.0078)
Two Cells Over			-0.0012	-0.0000	0.0002			0.0073	0.0131**	0.0129**
			(0.0020)	(0.0020)	(0.0021)			(0.0061)	(0.0061)	(0.0059)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trends				Yes	Yes				Yes	Yes
Census tract specific time trends					Yes					Yes
Sample Size	603034	603034	603034	600914	603034	603034	603034	603034	600914	603034

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4, with the results obtained from OLS regression. Standard errors clustered by Safe Passages are reported in parentheses.

Table A.1.5: Impact of the Safe Passage Program on Crime

	Violent Crimes	Violent Crimes	Property Crimes	Property Crimes
	(1)	(2)	(3)	(4)
Safe Passage Cell	-0.1001**	-0.1378***	0.0048	0.0035
	(0.0398)	(0.0374)	(0.0278)	(0.0268)
One Cell Over	0.0215	0.0015	0.0386**	0.0369
	(0.0292)	(0.0301)	(0.0213)	(0.0229)
Two Cells Over	0.0049	0.0007	0.0175	0.0213
	(0.0277)	(0.0261)	(0.0177)	(0.0195)
Cell FE	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes
Census Tract specific time trend	Yes	No	Yes	No
Safe Passage specific time trend	No	Yes	No	Yes
Sample Size	508,376	508,376	552,896	552,896

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4 Standard errors clustered by Safe Passages are reported in parentheses.

Table A.1.6: 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)	(7)	(8)
Safe Passage Cell	-0.1421***	-0.1317***	-0.1213***	-0.1266***	-0.0371	-0.0346	-0.0364	-0.0095
	(0.0324)	(0.0348)	(0.0374)	(0.0367)	(0.0240)	(0.0246)	(0.0263)	(0.0267)
One Cell Over		0.0277	0.0358	0.0219		0.0077	0.0063	0.0245
		(0.0227)	(0.0248)	(0.0250)		(0.0179)	(0.0197)	(0.0182)
Two Cells Over			0.0230	0.0127			-0.0043	0.0068
			(0.0217)	(0.0209)			(0.0136)	(0.0133)
Intensity of treatment	0.0969	0.0904	0.0823	0.0320	0.1447	0.1433	0.1446	0.1413
	(0.1232)	(0.1229)	(0.1240)	(0.1189)	(0.0900)	(0.0902)	(0.0907)	(0.0893)
Cell FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trend				Yes				Yes
Sample Size	508376	508376	508376	508376	552896	552896	552896	552896

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. Intensity of treatment is a dummy variable that takes value of 1 if the cell contains more than one Safe Passage. Standard errors clustered by Safe Passage are reported in parentheses.

Table A1.7: Crime: Covariate Balance for Matching with 2 closest neighbors

	Mean			t-test	
Variable	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	33861	33947	-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

*Number of schools that are in that cell, one block, two block or three blocks adjacent

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 A1.8: Crime: Robustness for Matching with one neighbor, no replacement

	Day				Night		Weekend	
	Violent	Violent	Property	Property	Violent	Property	Violent	Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Safe Passage Cell	-0.1095***	-0.0957***	-0.0224	0.0328	0.0104	0.0083	-0.0567	0.0001
	(0.0302)	(0.0300)	(0.0267)	(0.0210)	(0.0246)	(0.0267)	(0.0423)	(0.0400)
Cells FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month - Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community specific time trend		Yes		Yes				
Sample size	192602	192602	198750	198750	194192	197796	172038	194404

*p<0.10, **p<0.05,* **p<0.01

Note: The specification is similar to Table 4. Night time is 5:30 pm to 6:30 am and weekend excludes night time. Standard errors clustered by cells are reported in parentheses.

Table A1.9: Crime: Covariate Balance for Matching with 1 closest neighbors

	Mean			t-test	
Variable	Treated	Control	%bias	t	p>t
Crime Count					
Total violent crime (2001-2008)	58.47	56.9	4	0.71	0.478
Total property crime (2001-2008)	127.68	127.86	-0.2	-0.03	0.973
School characteristics: Proportion of students					
Eligible for Individualized Education Program	0.13	0.13	-1.8	-0.37	0.71
Eligible for free lunch	0.93	0.93	-1.4	-0.68	0.494
Bilingual	0.94	0.91	0.3	0.42	0.675
African American	75.9	77.21	-3.6	-0.85	0.397
Hispanic	21.31	20.38	2.9	0.64	0.522
Census Block characteristics					
Share Black	0.71	0.73	-4.7	-1.11	0.267
Share Female	0.54	0.54	-3.7	-0.78	0.433
Median Age	34.12	34.59	-5.5	-1.16	0.247
Proportion below high school	0.22	0.22	5.3	1.2	0.23
Median Family Income	33886	33560	1.4	0.41	0.68
Unemployment Rate	0.24	0.23	1.6	0.33	0.743
Poverty Rate	0.32	0.32	2.6	0.57	0.568
Proportion on food stamps	0.38	0.38	2.1	0.46	0.646
Owner occupancy Rate	0.39	0.38	3.9	0.91	0.365
Vacancy Rate	0.2	0.2	1.5	0.31	0.758
Median no. of rooms	5.07	5.05	1.3	0.3	0.763
Median year of home construction	1947.8	1947.4	2.8	0.62	0.533
Median Home Value	170000	170000	0.4	0.11	0.915
Housing Units	397.82	400.6	-1.2	-0.33	0.745
Median Gross Rent	900.57	899.02	0.6	0.14	0.887
No. of schools in that area*	4.25	4.29	-2	-0.38	0.702

*Number of schools that are in that cell, one block, two block or three blocks adjacent

The table compares the mean of the treated and control for the matched sample obtained by propensity score matching using the closest neighbors, without replacement.

Table A1.10: Attendance: Propensity Score Matching with two neighbors

	(1)	(2)	(3)
Treated * Post	1.6832***	2.5351***	2.4151***
	(0.2781)	(0.3287)	(0.3300)
Welcoming*Post			-2.2053***
			(0.3084)
School FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Excludes welcoming schools		Yes	
Control group obtained by matching	Yes	Yes	Yes
Sample size	3324	2695	3324

Note: The specification is similar to Table 14. Standard errors clustered by school are reported in parentheses.

Table A1.11: Attendance: Covariate Balance for Matching

	Mean			t-test	
Variable	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	35455	30960	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 A1.12: Change in Attendance Rates by Expansion Year

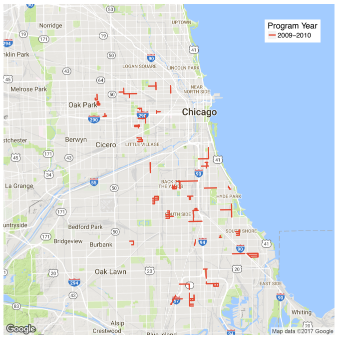
	2009	2013	2013	2014
Treated * Post	4.0820***	0.0562	0.2835	0.6678***
	(0.4436)	(0.1013)	(0.2682)	(0.1541)
Welcoming*Post			-0.2674	
			(0.2921)	
School FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sample Size	5,694	5,974	5,974	5,835

*p<0.10, **p<0.05,* **p<0.01

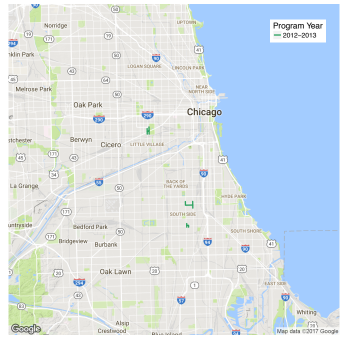
Note: Results are obtained from regressions of the change in average annual attendance of schools on the explanatory variables. The time period for this analysis is 2003 to 2016 school years. Schools that were closed in 2013 are excluded from all the regressions. The variable Safe Passage Schools * Post 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.

Appendix Figures:

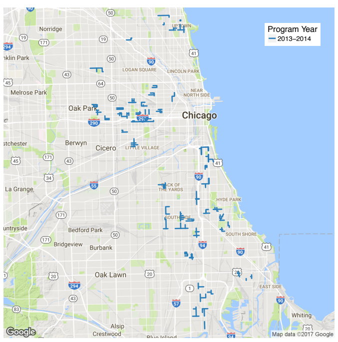
Figure A.1. Safe Passages: By Starting Program Year



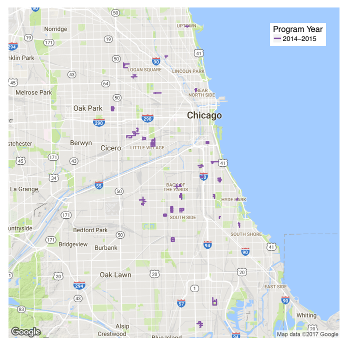
(a) 2009-2010 School Year



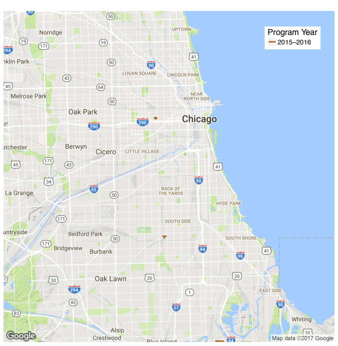
(b) 2012-2013 School Year



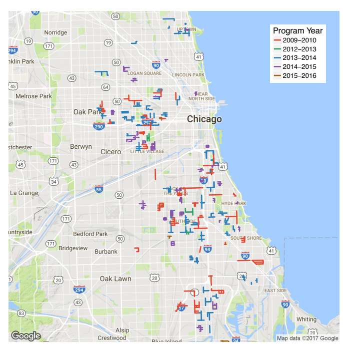
(c) 2013-2014 School Year



(d) 2014-2015 School Year

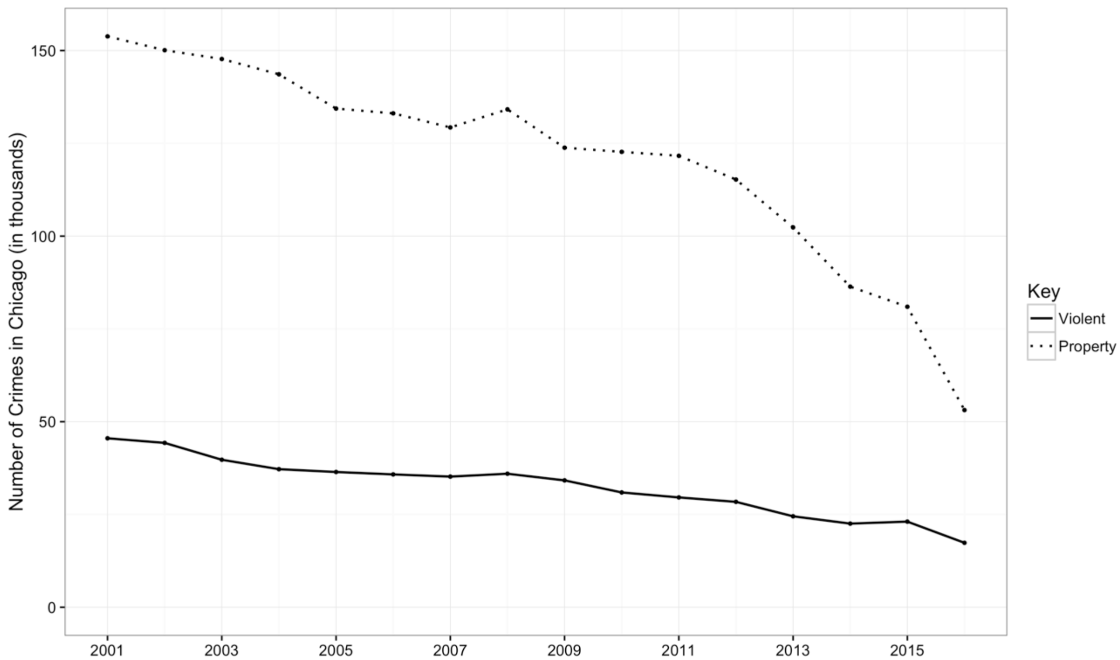


(e) 2015-2016 School Year



(f) All Safe Passages

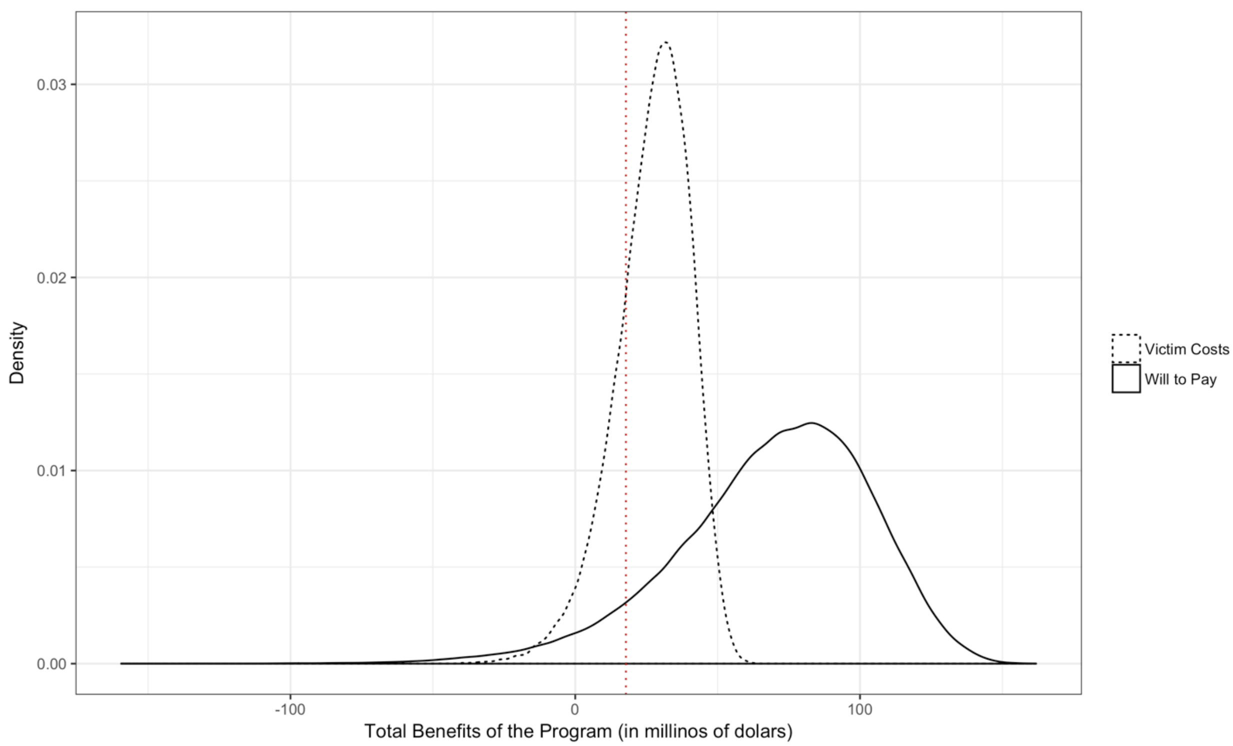
Figure A.2.: Crimes in Chicago by Year 2001-2016



Source: Author's calculations

Figure A.3.: Cost Benefit Analysis,

Victim Costs and Willingness to Pay



Note: The solid line is the density of possible benefits of the program. The red dotted line is the cost 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 100000 estimates of the program effect of each crime category from normal distribution with mean equal to the estimated coefficient and standard deviation equal to the standard errors listed in Table 15 columns (2) and (3). Then we calculate the benefits of the change in number of crimes using the preprogram mean and Cohen and Piquero's (2009) cost estimates (in 2015 dollars).