Community Monitoring and Crime: Evidence from Chicago's Safe Passage Program

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Abstract

This paper examines the effect of community-based monitoring on crime. We study this question empirically in the context of a school safety initiative – the Chicago Public Schools' Safe Passage program. This community-based monitoring intervention employed community members to monitor designated areas around public schools in Chicago during students' travel to and from school. We combine detailed block-level data on violent, property, and minor crimes along with information on each block's Safe Passage designation between the 2010/11-2015/16 school years. We investigate the effect of community monitoring on crime and the degree to which community monitoring led to spatial and intertemporal reallocations of crime. We find evidence that Safe Passage community monitoring had a significant effect on crime. Safe Passage blocks experienced about a 29% drop in all crimes relative to neighboring non-treated blocks. Violent and property crimes declined by 31% and 16%, respectively. We find evidence of modest cross-crime elasticities: although crimes declined overall, burglaries and motor vehicle thefts increased slightly. In the case of intertemporal substitution of crime, we find no evidence of shifts in crime towards non-monitored periods. Finally, with the exception drug offenses, we find no evidence of spatial crime spillovers into neighboring non-treated blocks. Instead, our findings suggest significant diffusion of treatment benefits to non-treated areas via monitoring spillovers.

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1 Introduction

In recent years, grassroots participation in crime monitoring (often called "community monitoring") has received increased attention as an alternative crime-deterring policy option as compared to more conventional public policies in this area.[1] One reason for this increased attention is that community monitoring can be a cheaper alternative relative to more conventional forms of monitoring, such as police deployment. Additionally, community monitoring has the potential to be more effective than other conventional forms of monitoring, since relative to police officers, individual community members may have better information on the nature and extent of criminal activities in their communities as well as stronger incentives to monitor and report crime.

Despite their popularity, however, evidence on the responsiveness of crime to community monitoring initiatives is scarce.[2] Most of the economics literature in the area of criminal deterrence has focused on conventional or "top-down" monitoring methods, which rely primarily on police manpower and policing intensity. For the most part, findings within this research literature indicate that crime levels decline in response to increases in police manpower.[3] In contrast, research literature on the effects of community monitoring is more limited.[4] In the case of community-based or "bottom-up" monitoring, however, not only is the empirical literature sparse, but the direction of the effect on crime predicted by economic theory is ambiguous. Community monitoring can have a deterring effect on crime by increasing the probability of detection and hence lowering the expected gain from criminal activity. On the other hand, as with any collective action intervention, the impact of community monitoring on crime may be trivial in the presence of free-riding by monitors or if individuals engaging in criminal behavior can manipulate the monitoring process. This could happen, for instance, if criminals could bribe, intimidate, or exert undue influence in the selection of community monitors.[5] In the worst case scenario, community monitoring could lead to increased crime, if local police respond to community monitoring by reallocating manpower away from monitored areas. This may lead to a decline in the probability of apprehension, even if the likelihood of detection via community monitoring increases.

The aim of this paper is to address this unresolved empirical and theoretical question by providing rigorous evidence on the effect of community monitoring on crime. We study this question in the context of a unique

[1]Some examples of community-oriented programs are Boston's CeaseFire, CeaseFire-Chicago, Baltimore's Safe Streets, Pittsburgh's One Vision, and more recently, Chicago's Safe Passage. Refer to Wilson and Chermak (2011) for a summary and comparisons among some of these programs.

[2]We provide a more detailed description of the relevant literature in section 2.1 .

[3]It is worth noting that the magnitudes and precision of the estimates vary significantly by study. Refer to Skogan and Frydl (2004), Levitt and Miles (2006), Chalfin and McCrary (2017) for reviews of the literature on police manpower and crime.

[4]We note that there are studies within the development economics literature examining the impact of grassroots interventions in the context of corruption and public malfeasance in general. Refer to Olken and Pande (2012) for a review of this literature.

[5]For example, issues like free-riding and elite capture of the monitoring process have been shown to dampen the effectiveness of community monitoring interventions designed to curb corruption. See, for instance, Olken (2007)

community monitoring program – Safe Passage – carried out in an education setting by the Chicago Public Schools (CPS). The Safe Passage program places members from the community along designated routes around public schools in Chicago during school arrival and dismissal times on regular attendance days during the school year. The main objective of the program is to deter crime by means of increased presence and crime-reporting by these community monitors. To investigate the effect of Safe Passage community monitoring on crime, we combine block-level data on reported violent, property, and minor crimes in Chicago with block-level information on monitored Safe Passage routes. Using these two data sources, we address: (i) the effect of Safe Passage community monitoring on crime, (ii) whether and to what extent community monitoring leads to a spatial and temporal reallocation of crime, and (iii) whether we can detect any evidence of cross-crime elasticities. We investigate these issues by employing a Difference-in-Differences (DD) approach that exploits the staggered roll-out of Safe Passage routes across public schools in the city of Chicago.

Our results provide evidence that community-based monitoring has a significant effect on crime. Specifically, when looking at all crimes reported on regular attendance days during the school year, we find that Safe Passage blocks experienced about a 29% decrease in crime relative to neighboring non-treated blocks. In the case of violent and property crimes, the observed decline in violent crimes is considerably higher than in property crimes (31% versus 16%). This result is consistent with the program's objective as well as with previous findings from the "top-down" monitoring literature (Chalfin and McCrary, 2013). Within violent crimes, cases of aggravated assault and battery declined by about 36%, while the number of robberies dropped by 28%. In the case of property crimes, while reports of larceny declined significantly, burglary and other property crimes, such as motor vehicle thefts, slightly increased in Safe Passage blocks. Such findings point to the possibility that individuals responded to the community monitoring intervention by substituting to alternative types of criminal activity. Our main findings are robust to several alternative definitions of the control group, alternative block length weights, and to the introduction of neighborhood-specific linear time trends. Furthermore, inference is not significantly affected when adjusting for multiple hypothesis testing issues resulting from the high number of outcomes being tested.

We build on these main findings by investigating the degree to which individuals respond to community monitoring by reallocating criminal activity across time and space. In the case of intertemporal responses, we compare crime levels in Safe Passage and control blocks during four periods when monitors are absent: evening hours, weekends, non-attendance days (e.g., breaks, holidays, teacher professional development days, etc.), and summer months. In all four instances, there is no evidence of significant intertemporal substitution in violent, property, or non-index crimes. In fact, differences in crime levels between Safe Passage and control blocks are small and statistically insignificant in the absence of monitoring. To investigate the spatial reallocation of crime, we explore crime spillovers into neighboring areas by comparing crime levels on blocks that are adjacent, within 0.25-miles, and within 0.50-miles of Safe Passage blocks to blocks that are more than one-half mile from Safe Passage blocks but within the same neighborhood. For the most part, we find no evidence of spatial spillovers in crime. Instead, we uncover evidence of positive monitoring spillovers into neighboring areas: non-treated blocks near Safe Passage blocks experienced drops in total crimes that range between 4% to 44%. It is important to highlight, however, that we find a significant increase in drug offenses in blocks that are more than one-half mile away from Safe Passage blocks.

2 Background

2.1 Previous Work

This paper contributes to a growing effort to understand the effectiveness of crime-deterring interventions.[6] In particular, our work adds to the literature exploring the sensitivity of crime to changes in the probability of apprehension. Early literature on the impact of police manpower on crime generally found elasticities that were close to zero and imprecise (e.g., Ehrlich (1973), Wilson and Boland (1978)). Later research addressing simultaneity and unobserved heterogeneity found larger, negative elasticities, although the range of the estimates varied significantly across studies and many estimates lacked statistical significance (e.g., Levitt (1997), Levitt (2002), DiTella and Shargrodsky (2004), Lin (2009), Machin and Marie (2011)). More recently, Chalfin and McCrary (2013) further address issues with measurement error in crime reporting and police deployment numbers and find economically and statistically significant elasticities. Note, however, that while previous work within this literature has primarily focused on "top-down" interventions such as policing intensity, our paper is novel in exploring a "bottom-up", community-based approach. Economic studies on these "bottom-up" approaches are scarce, with most empirical evidence emerging from the criminology literature.[7] For example, Braga et al. (2001) and Skogan et al. (2011) find a significant decrease in violent crimes as a result of Boston's CeaseFire and CeaseFire-Chicago programs, respectively. On the other hand, Webster et al. (2009) and Webster et al. (2013) find mixed results: although Baltimore's Safe Streets program did lead to reductions in homicides in some areas of the city, other areas reported increases in homicides and non-fatal shootings. Wilson and Chermak (2011) find similar instances of crime spillovers as a result of Pittsburgh's One Vision initiative.

Despite the insight provided by the existing criminology literature on this topic, two points are worth noting: First, even though all of the programs studied in this literature had an important grassroots-

[6] Refer to Chalfin and McCrary (2017) for a recent review of the literature on crime-reducing interventions.

[7] We note that there are a number of studies in the development economics literature examining the impact of grassroots interventions. However, the focus is on corruption and public malfeasance in general. Furthermore, the degree to which these interventions have a significant impact on malfeasance remains an open question. Olken (2007), Björkman and Svensson (2009), Banerjee et al. (2010), Serra (2011). Refer to Olken and Pande (2012) for a review of this literature.

monitoring component, most also incorporated additional components as part of their crime reduction efforts. These additional components included improved data collection, increased intelligence gathering by law enforcement, youth counseling, and better access to job training and substance abuse programs (Wilson and Chermak, 2011). Our intervention, Chicago's Safe Passage, is simpler in the sense that it only involves the deployment of community members to monitor designated routes during students' travel to and from school. This, in turn, allows us to better isolate the effect of community monitoring on crime. Second, in addition to the difficulties associated with disentangling the effect of community monitoring from the effects of other program components, empirical findings from the existing literature on community monitoring are mixed. Some studies in this literature find a deterring effect while others find trivial effects – and even increases in certain types of crimes after implementation. These points – along with the theoretical arguments mentioned before – suggest that the degree to which grassroots-monitoring affects crime and improves neighborhood safety remains an open question.

In addition to contributing to the literature on the effects of community monitoring, our setting and results also shed light on the ongoing question on the relative importance of deterrence *versus* incapacitation as crime-reducing mechanisms. Although we find considerable drops in crime from Safe Passage community monitoring, we find no significant differences in arrest rates between treated and control blocks. This key result, along with the nature of the monitoring technology, lends support to the idea that the effects of community monitoring on crime arise through a deterrence channel rather than through incapacitation. This finding has important implications in terms of public policy because policies that aim to reduce crime through deterrence are typically more cost-effective than those that work through incapacitation. Lastly, our work expands upon an initial evaluation of Chicago's Safe Passage program in Curran (2017), which concludes that Safe Passage did not produce any meaningful impacts on crime in the vicinity of CPS schools. In contrast to this existing study, our work takes advantage of the entire lifetime of the program and addresses potential issues, such as temporal and spatial spillovers of crime, that may mask the true impact of the program on crime.

Aside from the crime-related outcomes that are typically examined in assessments of crime policy, we make note of recent work from psychology that highlights the potential for crime policies to influence outcomes in other domains. Recent quasi-experimental work in psychology and related social sciences has documented the negative impact of exposure to community violence on children's cognitive outcomes, even when this exposure is indirect(Sharkey et al., 2012; McCoy et al., 2015). Work in this area has also found significant increases in parental stress following community-level violence, thus suggesting a potential pathway through which neighborhood crime could indirectly affect children's outcomes. Finally, a recent paper in this literature has also found that exposure to community-level violence negatively influences reading achievement in schools(Sharkey et al., 2014). These papers suggest that community monitoring has the potential to affect a broader range of outcomes than those typically examined in the crime literature. Although we do not address this in this paper, we highlight the possibility that a broad range of benefits that may emanate from community monitoring.

2.2 Safe Passage Community Monitoring

Chicago Public Schools (CPS) is the third largest school district in the United States (U.S.), with current enrollment estimated to be around 380,000 students (CPS, 2017). Like many other large, urban districts in the U.S., CPS serves a large number of racial/ethnic minority and economically-disadvantaged students. Recent estimates indicate that the district is 38 percent African-American, 46 percent Hispanic, and 10 percent White, and that around 80 percent of students in the district are economically-disadvantaged (family income is less than or equal to 185 percent of the federal poverty line) (CPS, 2017). In 2015/16, the district's operating budget was estimated to be around $5.7 billion (CPS FY2016 Budget Book, 2016).

Chicago Public Schools (CPS) introduced Safe Passage community monitoring at the beginning of the 2010/11 school year in response to a series of violent incidents that affected CPS students on the way to and from school. These violent incidents – including the murder of CPS high school student Derrion Albert in 2009 – received significant media attention and catalyzed district-level action in response to heightened student and parental concerns about safety and crime (Davey, 2013; Zubrzycki, 2013).

Safe Passage is a school-based intervention that provides community monitoring on the grounds, streets, and sidewalks surrounding designated schools in the district. At designated Safe Passage schools, community monitors patrol established routes during student arrival and dismissal times on regular attendance days during the school year. Community monitors wear brightly colored, neon vests that identify them as Safe Passage community monitors. These community monitors are present on school days during arrival and dismissal, but the Safe Passage routes are marked with street signs indicating "Safe Passage" at all times of day, even when community monitors are not physically present. Beginning in the 2010/11 school year, CPS established Safe Passage routes around 26 high schools in the district. In subsequent school years, the Safe Passage program was expanded to serve 140 schools in the district, including elementary, middle, and high schools. Currently, around one-quarter of all CPS schools are designated as Safe Passage schools, resulting in community monitoring coverage for more than 75,0000 CPS students each day (CPS, 2017).

Safe Passage is funded at the district-level but is separate from other school-based budgeting procedures. District expenditures on Safe Passage community monitoring totaled around $16 million during the 2016/17 school year.[8] Despite this centralized funding and oversight by the district, however, Safe Passage community

[8]We obtained information on annual Safe Passage expenditures from the Chicago Public Schools (CPS).

monitors are drawn from local neighborhoods and communities whenever possible. To cultivate connections between Safe Passage community monitors and the students and communities they serve, CPS contracts with local, neighborhood-based non-profit organizations to provide community monitoring services. These non-profits – which typically partner with local communities and neighborhoods in other capacities, such as tutoring, social assistance, and after school programming – employ and manage the Safe Passage community monitors.

Safe Passage community monitors work between five and six hours per day and are paid \$10 per hour. They receive training from CPS on topics such as first aid, CPR, and conflict de-escalation. All Safe Passage community monitors follow CPS-designed protocols for monitoring and reporting criminal activity and for communicating with school officials (Zubrzycki, 2013; CPS, 2017). Safe Passage community monitors are unarmed but have CPS-provided cellular telephones or two-way radios that allow them to communicate with Safe Passage supervisors and with the city's emergency services (i.e., police, fire, and medical personnel) (Zubrzycki, 2013).

3 Data

We investigate the impact of Safe Passage community monitoring on crimes in the vicinity of CPS schools by combining two main data sources: block-level data on reported crimes collected by the City of Chicago Police Department (CPD) and street-level maps of the routes covered by Safe Passage community monitors.

3.1 Chicago Crime Data

We obtained data on crimes in Chicago from the Chicago Police Department (CPD) Citizen Law Enforcement Analysis and Reporting (CLEAR) system. We restrict attention to all crimes during the 2007/08-2015/16 school years, a period of time that spans the staggered rollout of Safe Passage across CPS schools and that includes several years of pre-treatment data. Each observation in the crime data corresponds to a single reported crime during this time period and includes information on the type of crime, the date and time of the report, the latitude/longitude coordinate corresponding to the crime's location (aggregated up to the city block level), and an indicator for whether the reported crime resulted in an arrest.

To provide results that are directly relevant to our setting, we aggregate these block-level crimes into three mutually exclusive and exhaustive categories: Violent Crimes, Property Crimes, and Non-Index Crimes. Violent Crimes include Homicide (1st and 2nd Degree), Criminal Sexual Assault, Robbery, Aggravated Assault, and Aggravated Battery. Property Crimes include Burglary, Larceny, Motor Vehicle Theft, and Arson. Non-Index Crimes include all other remaining crimes in the data, encompassing crimes such as

Simple Assault, Simple Battery, Drug-Related Offenses, Fraud, and Embezzlement, among others. For a complete list of all crimes in the Non-Index category, please see Appendix B.

To investigate the effects of Safe Passage community monitoring on crimes in the vicinity of CPS schools, we divide the data in several analytic samples based on the days and dates on which the crimes were reported. In our main analysis, we restrict our sample to include only crimes reported on weekdays (Monday-Friday) during the school year, which roughly spans early-September to mid-June each year. Using detailed information drawn from CPS calendars, we further exclude designated non-attendance days, including breaks, holidays, and staff professional development days, from consideration. In addition to this main sample, we construct several additional samples to exploit data on crimes occurring during days of the week when community monitors were not present. These include designated non-attendance days, weekends during the school year, and summer breaks. Finally, in addition to restricting our analysis to particular days of the week during the school year, we also take advantage of the timing of reported crimes during these attendance and non-attendance days. In some of our analysis, we divide our main sample by time-of-day, paying particular attention to day (6am-6pm) versus night (6pm-6am).

Figure 3 plots the number of average daily reported crimes between 2008-2016, for all crimes and then separately for Violent, Property, and Non-Index crimes. Each panel presents two time series: one for the city of Chicago as a whole and one for areas within a 0.25-mile distance of CPS schools. All four panels of this figure illustrate marked downward trends in reported crimes over this period, a phenomenon that has been well-documented elsewhere. In all cases, we note that the trend in the time series for blocks near CPS schools tracks the trend in the time series for the city as a whole very closely, although the average number of daily crimes is higher near CPS schools for all crimes and for our three separate categories.

3.2 Safe Passage Data

We documented the staggered rollout of Safe Passage routes at the street-level by combining information from the following sources: Procurement Contracts from the Chicago Board of Education (CBOE), street-level maps of Safe Passage routes from CPS, historical snapshots of the CPS Safe Passage webpage, and press releases from the CPS Office of Communication. For more detailed information about these sources, please see Appendix B. Using the information from these sources, we documented the school year in which community monitoring services began at each designated Safe Passage school within CPS. We then recorded the specific city blocks included in each school-level Safe Passage route, separately by school year. Figure 1 depicts the staggered rollout of Safe Passage routes across CPS schools during the 2010/11-2016/17 school years.[9]

To assess the impact of community monitoring on crime, we defined treatment blocks in our sample based on street-level Safe Passage route maps that we obtained from CPS. We highlight that treatment blocks varied at the school-level (route) over time along both the extensive and intensive margins. Using expansions of the Safe Passage program across schools, we captured changes in treatment blocks along the extensive margin, while block-level changes within school-level routes allowed us to capture changes in treatment blocks along the intensive margin.

Relative to our treatment blocks, we defined control blocks in our sample in several ways. First, we defined control blocks as those blocks located within 0.25-mile distance of any block that was treated by Safe Passage during the period of our study. We experimented with alternative definitions of this distance (we present results using a 0.50-mile distance as a robustness check) but found that our main results were not sensitive to this choice. Second, as an alternative to a distance-based measure, we imposed a more restrictive definition of control blocks and limited our control group to include only those blocks that were directly adjacent (i.e., intersecting) to any block that was treated by Safe Passage during the period of our study. Figure 2 provides an illustrative example of these alternative definitions of the control group using a street-level Safe Passage route map from the 2014/15 school year. Panel (a) visually depicts the blocks contained in our treatment and control groups for the entire sample of Safe Passage schools during the 2014/15 school year, while Panel (b) provides a close-up view of several Safe Passage routes to illustrate both the control blocks contained within fixed distances (0.25 and 0.50 miles) of treatment blocks and the more narrowly defined control group based on blocks directly adjacent to Safe Passage routes.

Table 1 summarizes Safe Passage program characteristics and basic descriptive information about Safe Passage community monitoring between the 2010/11-2015/16 school years. CPS introduced Safe Passage community monitoring to 26 high schools in the 2010/11 school year and gradually expanded the program to serve 140 schools – including elementary, middle, and high Schools – during the 2015/16 school year. We combine elementary and middle schools together the typical school configuration in CPS is K-8. Columns (3) and (4) summarize the number of treated blocks and the number of community monitors, separately by school year. Column (5) presents the average "intensity" of treatment during this period, which declined slightly from around 1.26 to 0.85 community monitors per block over this period.

[9] We exclude charter schools from our analysis since only one charter school has ever been designated as a Safe Passage school.

4 Empirical Strategy

We use a Difference-in-Differences (DD) approach that takes advantage of the staggered introduction of Safe Passage routes to estimate the effect of community monitoring on crimes. In this section, we present the baseline estimating equation along with discussion of our identifying assumptions.

To estimate the impact of Safe Passage community monitoring on crime in the vicinity of CPS schools, we use an estimating equation of the following form:

$$Y_{bt} = \alpha + \beta \times SPCM_{bt} + \theta_b + \lambda_t + \varepsilon_{bt} \tag{1}$$

where Y_{bt} is a block-level crime outcome for block b in year t. $SPCM_{bt}$ is a binary indicator (0/1) that takes on the value of one when the block is covered by Safe Passage community monitoring during year t. The model includes block fixed-effects, θ_b, which control for observable and unobservable block-level differences that are constant over time. These include, for example, time-invariant characteristics such as the physical environment. The model also includes year fixed-effects, λ_t, which control for factors that are common to all blocks in specific years, such as city-wide economic conditions and district-wide policy changes. Some specifications also include neighborhood-specific linear time trends. These flexibly capture time-varying observables and unobservables that change linearly at the neighborhood-level. The error term, ε_{bt}, is assumed to be uncorrelated with other determinants of the outcome. The key coefficient of interest, β, represents the impact of Safe Passage community monitoring on block-level crimes. We cluster standard errors at the neighborhood level. To account for differences in block size, all specifications are weighted using block lengths.

Identification of β requires that treatment and control blocks exhibit similar crime trends prior to the introduction of Safe Passage community monitoring. Violations of this assumption may result from systematic differences in observable and unobservable characteristics between treatment and control blocks. In our setting, a source of concern is the lack of data on time-varying characteristics at the block level that can help mitigate this issue. We compensate for this by restricting our comparison group to blocks that are geographically close to treated blocks. Specifically, we experiment with blocks that are adjacent, within 0.25-miles, and within 0.50-miles from treated blocks as our alternative comparison blocks.[10] An additional source of concern that may result in differential trends is the possibility of anticipatory effects. Crimes may decline just prior to implementation of Safe Passage community monitoring if it is expected or common knowledge among potential offenders that a given block will be part of the monitoring intervention in the near future. This is particularly concerning given the widespread coverage of the program in general media.

[10] Most results presented in this paper use the 0.25-mile threshold but results using other thresholds are available upon request.

5 Results

5.1 Main Results

Table 2 presents the results of estimating Equation (1). The four main crime categories presented in the table are measured as the total number of crimes in a given block occurring during the school year. In other words, we exclude summer months, weekends, and scheduled non-attendance days (e.g., holidays, breaks, and staff professional development days). The first row presents the results for all crimes at the block-level, while the second through fourth rows disaggregate all reported crimes into three mutually exclusive and exhaustive categories: Violent crimes, Property crimes, and Non-Index crimes. Column (2) presents the estimation results from our main empirical specification using the sample comprised of treated Safe Passage blocks and control blocks located within a 0.25-mile distance. The point estimate in the first row of this column demonstrates that Safe Passage community monitoring decreased the total number of block-level crimes reported near CPS schools by 1.647 crimes annually, and that the result is statistically significant. This effect translates into a 33 percent reduction in annual block-level crimes relative to the control mean (5.042 crimes).

The point estimates in the second through fourth rows disaggregate this effect by crime type: we find that Safe Passage community monitoring resulted in 0.153 fewer Violent Crimes, 0.220 fewer Property Crimes, and 1.274 fewer Non-Index crimes annually in monitored blocks. These effects translate into 33 percent fewer Violent Crimes (control mean is 0.464), 17 percent fewer Property Crimes (control mean is 1.335), and 40 percent fewer Non-Index crimes (control mean is 3.243), respectively. These point estimates are all highly statistically significant at conventional levels, but to be conservative we reconsider our results by implementing two corrections for multiple hypothesis testing. Appendix Table A1 presents alternative p-values, calculated using the Benjamini and Hochberg (1995) correction and the Bonferroni-Holm correction (Holm, 1979). We find that our conclusions regarding the statistical significance of our estimates are unaffected by these corrections.

Columns (3) and (4) present two alternative sets of estimation results that serve as robustness checks on our main findings. In Column (3) we augment our main specification with the addition of neighborhood-specific linear time-trends, but we find that the results are remarkably similar to those obtained from our main specification. In all cases, the point estimates are slightly smaller in magnitude, but we note that they are very close to the estimates in the previous column. In Column (4) we expand our analytic sample to include more control blocks by including all blocks located within a 0.50-mile radius of treated Safe Passage blocks. The results in this column are also remarkably similar to our main findings, with point estimates that are slightly larger than those we obtained from our main specification. Taken together, these robustness checks demonstrate that our main findings are not sensitive to model specification and that they are robust to alternative definitions of the control group.

We highlight an important result from these main findings. In all specifications, the relative drop in violent crimes is much larger than in property crimes. For instance, in Column (2), the decrease in violent crimes is almost double the drop in property crimes (33 *versus* 17 percent). This result is consistent with previous findings that examine the impact of increases in police manpower and policing intensity on crime (Chalfin and McCrary, 2013). This may lend support to the idea that grassroots monitoring operates through a similar channel and hence may serve as an efficient alternative to top-down policing.

To provide more detailed insight into the specific types of crimes that were affected by Safe Passage community monitoring, we further divided our measures of annual Violent, Property, and Non-Index crimes at the block-level into a more refined set of crimes and sub-categories of crimes. We re-estimated our main specification using these refined categories and present the results in Table 3. Panel (A) presents the results for Violent Crimes, which we sub-divide into Aggravated Assaults and Batteries, Robberies, and Other Violent crimes. Our estimates from this exercise suggest that Safe Passage community monitoring resulted in fewer Aggravated Assaults and Batteries and fewer Robberies at the block-level, but that there were no meaningful effects on the sub-category of Other Violent Crimes (Homicides and Criminal Sexual Assault).

Panel (B) presents the results for Property Crimes, which we sub-divide into Burglary, Larceny, and Other Property Crimes. The results from re-estimating our main specification on these sub-categories demonstrate the effect of Safe Passage community monitoring on Property Crimes was driven entirely by decreases in the average number of larcenies at the block-level. Notice that signs on the point estimates for burglaries and other property crimes are actually positive, although the estimates are not statistically significant, in Safe Passage blocks.[11] This opens the possibility that offenders may respond to Safe Passage community monitoring by substituting to alternative types of crime.

Panel (C) presents the results for Non-Index Crimes, which we sub-divide into Simple Assaults and Batteries, Drug crimes, Weapons Violations, and Other Non-Index crimes. Our results suggest that most of the effect on Non-Index crimes was driven by a decreased number of Simple Assaults and Batteries, although our evidence suggests that Safe Passage community monitoring decreased all types of crimes within this broader category. For completeness, we present alternative p-values that correct for multiple hypothesis testing in Appendix Table A2, although we note that once again our conclusions regarding the statistical significance of our estimates are largely unaffected by these corrections.

[11] The Other property crimes category is mainly comprised of Motor Vehicle Theft.

5.2 Pre-Trends and Dynamics of the Monitoring Effect

We take advantage of the multiple years of data and the staggered rollout the program to explore: (i) whether there are common trends in crime rates between treated and control blocks prior to the introduction of Safe Passage community monitoring and (ii) whether and how the monitoring effect evolves in the years following Safe Passage community monitoring adoption. To implement this, we modify Equation (1) by including leads and lags of Safe Passage community monitoring introduction.[12] Specifically, we estimate:

$$Y_{bt} = \alpha + \sum_{g=0}^{6} \beta_{-g} \times SPCM_{b,t-g} + \sum_{g=1}^{6} \beta_{+g} \times SPCM_{b,t+g} + \theta_b + \lambda_t + \varepsilon_{bt} \tag{2}$$

where $SPCM_{b,t-g}$ equals 1 if block b adopted Safe Passage community monitoring in year $t-g$ (lags) and $SPCM_{b,t+g}$ equals 1 if block b will adopt Safe Passage community monitoring in year $t+g$ (leads). Therefore, $SPCM_{b,t-g}$ with $g = \{0, \ldots, 6\}$ can be interpreted as a dummy variable equal to 1 if there have been g years since adoption of Safe Passage community monitoring while $SPCM_{b,t+g}$ with $g = \{1, \ldots, 6\}$ can be interpreted as a dummy variable equal to 1 if there are g years prior to the adoption of Safe Passage community monitoring. Since some blocks can have more than 6 years prior to the implementation of SPP, $SPCM_{b,t+6}$ refers to 6 years *or more* before the introduction of Safe Passage community monitoring. In order to assess the validity of the common trends assumption, we expect the coefficients on the leads to be jointly zero (i.e., no significant differences in crime levels in years prior to Safe Passage community monitoring implementation). Furthermore, we assess the dynamics of the monitoring effect by exploring the coefficients on the lags.

The results from this estimation are presented in Table 4. The year prior to the implementation is left as the comparison category. First, in order to rule out the existence of differential pre-trends, we assess both the magnitude and statistical significance of the coefficients on the leads, which measure whether there are differences in outcomes between control (non-Safe Passage) blocks and would-be Safe Passage blocks. We find that most coefficients are small in magnitude and statistically insignificant. Furthermore, the F-tests for joint significance presented in the table confirm that for most crime categories, we cannot reject the null hypothesis that there are no differences between treatment and control blocks in the years prior to the implementation of Safe Passage community monitoring.

With respect to the dynamics of the monitoring effect, we find that the year of the intervention (i.e., $g = 0$) there is a significant negative effect on crime and that in subsequent years this effect grows in magnitude. To assist in the interpretation of these coefficients, Figure 4 uses the estimated coefficients to plot the percent change in crimes relative to the average level in control blocks. This allows the results from

[12] This approach is equivalent to the lags and leads specification used in Autor (2003).

the four main crime categories to be on a similar scale. We find that the percent differences between control blocks and would-be Safe Passage blocks is close to zero for most crime categories for the years prior to intervention but that a sharp decline in crime occurs in the year of implementation and then increases in the subsequent years.

5.3 Spatial Reallocation of Crime

To explore the possibility that criminals responded to Safe Passage community monitoring by relocating their activities to blocks nearby, we expanded the sample to include all blocks located within neighborhoods that contained at least one Safe Passage route. We then divided this sample of blocks into five categories: treated Safe Passage blocks, blocks directly adjacent to Safe Passage blocks, blocks within a 0.25-mile distance of Safe Passage blocks, blocks within a 0.50-mile distance of Safe Passage, and all other blocks in the neighborhood. Figure 2 depicts the example of one neighborhood. We then estimated a version of Equation (1) in which we introduced four mutually exclusive dummy variables capturing whether a block is treated (Safe Passage), adjacent, within 0.25 miles, or within 0.50 miles. We used blocks located more than 0.50 miles from a Safe Passage route as the omitted comparison category. In the case that Safe Passage community monitoring produce crime spillovers into neighboring blocks, we would expect significant jumps in crime in nearby blocks (adjacent, within 0.25, and 0.50 miles) relative to blocks further away.

Table 5 presents the results of this estimation exercise, in which we investigated the effects of Safe Passage community monitoring on all blocks within neighborhoods with Safe Passage routes. In summary, not only do we find no evidence of spatial reallocation, but instead we find strong evidence of diffusion of the treatment benefits into nearby untreated blocks. In general, we find that the point estimates on our dummy variable for treated Safe Passage blocks is large, negative, and statistically significant for All Crimes and for Aggravated Assaults and Batteries, Robberies, Larcenies, Simple Assaults and Batteries, Weapons Violations, Drug Crimes, and Other Property Crimes. The pattern of these results are entirely consistent with what we found in our main specification even though the effect is measured relative to a slightly different control group. What is more interesting, however, is the pattern that we find: our point estimates decline monotonically as the distance from the Safe Passage blocks increases, but with few exceptions, the results strongly suggest that the effects of Safe Passage community monitoring are diffused into nearby areas.

We further explore the question of the spatial reallocation of crimes by asking whether the monitoring spillovers presented in the previous section could be driven by increases in crime in blocks that are farther away than 0.50 miles from the treated blocks but that are located within Safe Passage neighborhoods. In other words, we explore whether crimes are reallocated to the comparison blocks used in our previous analysis. To investigate this, we re-estimate Equation (1) by assigning blocks that are more than 0.50 miles away from treated blocks but that are located within Safe Passage neighborhoods as "treated," and we assign blocks in non-Safe Passage neighborhoods as comparison blocks. If crime is moving to the outskirts of the Safe Passage neighborhoods, then we would expect a positive coefficient on the "treatment" dummy. Table 1 presents the results from this estimation exercise. Notice in Column (1) that overall crimes actually increase by about 8 percent relative to blocks in non-Safe Passage neighborhoods. Interestingly, note in Column (10) that the increase in crimes seems to be solely driven by drug offenses. Drug offenses increase by about 24 percent in blocks that are more than one-half mile away from treated Safe Passage blocks.

5.4 Intertemporal Reallocation of Crime

To empirically investigate the possibility that criminals responded to Safe Passage community monitoring by reallocating their activities intertemporally, we re-estimated Equation (1) using several subsamples of our data. In Table 7 we present the results from several exercises in which we split or restrict our sample based on the dates and times of reported crimes. To test the possibility that criminals changed their behavior and avoided times of day when Safe Passage community monitors were present, we re-estimated our main model using a data on regular attendance days during the school year but divided this sample into crimes reported during the day (6AM-6PM) and crimes reported during the night (6PM-6AM). Columns (2) and (3) present the results from this re-estimation of our main model using these subsamples. We find strong evidence that our main coefficient estimates (reproduced in Column (1)) are driven by effects on crimes reported during the day. The point estimates in Column (2) are all large, negative, and highly statistically significant. Moreover, their magnitudes can all account for more than 80 percent of the total reduction that we observe across each crime type. In contrast, the point estimates in Column (3) are small in magnitude and statistically insignificant. Only the estimate for Property Crime is positive, although it is quite small.

Aside from the possibility that criminals could have reallocated their activities within days over time, we also investigate the possibility that criminals responded to Safe Passage community monitoring by shifting their activities to days of the week when community monitors were not present. To explore this possibility, we take advantage of reported crimes on days that we previously omitted from our main estimation: weekends during the school year and scheduled non-attendance days (e.g., Holidays, Professional Development Days, Winter Break, etc.). Column (4) presents the estimated effects for Safe Passage community monitoring on weekends (Saturdays and Sundays) during the school year. The estimated effects are small in magnitude and statistically indistinguishable from zero. We repeat this analysis using data on crimes reported on scheduled non-attendance days and report the results in Column (5). We find no evidence to suggest that criminals responded to by shifting their behavior to weekdays when Safe Passage community monitors were not present due to scheduled non-attendance for CPS students.

As a final way to explore the intertemporal reallocation of crime, we investigate whether criminals responded to Safe Passage community monitoring by shifting their activities by season. Using a sample of data on crimes reported during the summer (approximately mid-June to mid-September each year), we re-estimate our main model. The point estimates in Column (6) are all negative and statistically insignificant, which suggests that criminals did not shift their activities to alternative times of the year when community monitors were not physically present.

To further assess the possibility that criminals changed their behavior by shifting their activities intertemporally, we divided our main categories of crimes into more granular sub-categories that allowed us to focus on specific crimes. We present these results in Table 8. Column (2) presents weak but suggestive evidence demonstrating not only that there is no evidence of intertemporal allocation, but that the negative effect Safe Passage community monitoring on Violent Crimes and Drug Crimes persisted into hours of the night when community monitors were not physically present.

In addition to exploring reallocation of crime to different times of day, we also investigate whether criminals reallocated activities to days of the week when monitors were not present for sub-categories of crime. The evidence we present in Columns (4) and (5) tell a less favorable story. We find some evidence to suggest that Other Property Crimes (Arson and Motor Vehicle Theft) actually increased on weekends and scheduled non-attendance days during the school year, which suggests that criminals avoided detection by shifting their activities to days of the week when community monitors were not present. Despite the positive and statistically significant point estimates, we note that the magnitudes of these effects are quite small. Finally, in Column (6) we present results re-estimating our main specification for crimes reported during summer months. We find some evidence to suggest that the effect of Safe Passage community monitoring on Robberies persisted into summer months when community monitors were not present.

6 Cost-Benefit Analysis

Our empirical analysis suggests that Safe Passage community monitoring resulted in fewer crimes at the block-level. In this section, we present the results from back-of-the-envelope calculations designed to compare the benefits achieved through Safe Passage community monitoring – as measured by the monetary value of reduced crime – to the costs of implementing the program. To carry out this exercise, we made use of standard estimates from the research literature on the social cost of crime victimization and information on annual Safe Passage program costs.

To estimate the annual cost of Safe Passage community monitoring at the block-level, we obtained data on Safe Passage program expenditures, separately by school year, from CPS. These annual expenditures included labor, administrative, and personnel-related costs (e.g., reimbursement for fingerprinting and background checks) that were paid directly to the non-profit organizations that were contracted to provide monitoring services, and they included expenditures on CPS provided equipment (two-way radios and cellular telephones), uniforms, and CPS-sponsored annual training. We calculated the average annual cost of community monitoring at the block-level by dividing the dollar value of annual Safe Passage expenditures by the number of treated blocks in each school year.

Using the data on annual Safe Passage program expenditures between 2010/11-2015/16, we estimated that the average annual cost of community monitoring was between 10,230 and 24,149 dollars per block, with a median estimate of 15,138 dollars (2015 dollars). We believe that there are at least three reasons for this wide range in block-level costs across school years. First, CPS varied the number of monitored hours per day between five and six during the course of the program, which produced variation in daily labor costs associated with block-level community monitoring. Second, even though the number of treated blocks increased monotonically as the program expanded across CPS schools during the period of our study, the average "intensity" of community monitoring decreased over this period. Based on aggregate counts of the number of community monitors contracted to provide Safe Passage services during this period, we calculate that the average intensity of monitoring decreased from 1.26 to 0.84 monitors per block. This decrease in monitoring intensity further contributed to variation in labor costs, which are by far the largest share of costs associated with the program. Finally, CPS placed different caps on the share of expenses that non-profit providers could bill in the form of administrative costs (which was also a function of the number of community monitors employed) at various points in time, and this contributed to variation in the cost of community monitoring across school years during the period of our study.

To calculate the total value of benefits achieved through reductions in crime, we scaled our estimated coefficients by empirical estimates (dollar values) from the research literature on the social cost of crime victimization. As is typical in this research literature, the estimates we relied upon were generated using one of two methods: contingent valuation and direct (bottom-up) accounting. While contingent valuation methods capture ex ante assessments of willingness-to-pay to avoid future (hypothetical) crime victimization, direct accounting methods rely on ex post assessments of the costs associated with victimization using information such as medical bills, property damage, criminal justice costs, lost work hours, and jury awards, which capture intangibles such as "pain and suffering" and diminished quality of life for crime victims (Dominguez and Raphael, 2015). While neither of these two methods is without controversy, critics often point to the large values generated from direct accounting methods as an area of particular concern for cost-benefit analyses of criminal justice policy, particularly because of the difficulties associated with quantifying concepts like "pain and suffering" and diminished quality of life (Cohen et al., 2004; Dominguez and Raphael, 2015). For this reason, we present a range of possible results for our cost-benefit calculations and employ multiple estimates of the social cost of crime victimization from the current research literature.[13] Furthermore, when we employ direct accounting estimates, we rely on those generated without incorporating intangibles. We focus our cost-benefit analysis on the subset of crimes for which multiple estimates of the social cost of crime victimization were available and for which we found meaningful effects from Safe Passage community monitoring. These crimes include: Aggravated Assault/Battery, Robbery, Larceny, and Simple Assault/Battery.

Table 9 presents the results of our cost-benefit calculations. Using estimates on the social cost of crime from Miller et al. (1996), Cohen et al. (2004), and Cohen and Piquero (2008), we find that the estimated benefits of Safe Passage community monitoring typically outweighed the costs, although in some cases – depending on the assumptions we make – our calculated ratio is less than one. Comparing an estimate of the benefits achieved from averted crimes – which we generated using our preferred estimates from the research literature on the social cost of crime victimization – and a median estimate of annual Safe Passage community monitoring costs at the block-level ($15,138 per year), we find that benefits exceeded costs by a factor of 2.8 (42,611/15,138), but possibly by as little as 1.5 or as much as 4.1. Columns (1) and (2) reproduce our coefficient estimates from Column (3) of Table 2 in this paper. Column (3) presents our preferred estimates of the value of each averted crime from Cohen et al. (2004) and Cohen and Piquero (2008). In Columns (4) and (5) we scale these dollar values by our estimated coefficients and the upper and lower bounds of each coefficient estimate's 95% confidence interval. In the bottom row, we sum the total dollar value of the averted crimes arising from Safe Passage community monitoring. We find that Safe Passage community monitoring generated annual benefits around $42,611 per block.

As a sensitivity check, we recalculated the benefits associated with averted crimes using smaller estimates from the research literature on the social cost of crime. Column (6) presents an alternative set of estimates of dollar value of each averted crime. These estimates are from Miller et al. (1996) and Cohen and Piquero (2008), although in the cases of Larceny and Simple/Assault Battery, no alternative estimates were available. In Columns (7) and (8) we scale these dollar values by our estimated coefficients and present upper and lower bounds of the associated 95% confidence interval. In the bottom row, we again present the total value of averted crimes arising from Safe Passage community monitoring. By using these smaller, more conservative dollar values, we find that Safe Passage community monitoring generated annual benefits around $22,466

[13] In addition to using estimates from these two methods, we acknowledge the literature on the social cost of homicide, which borrows heavily from related work on the Value of a Statistical Life (VSL). We ignore this area of work in our cost-benefit calculations, as the large dollar values ascribed to averted homicides – which are based on VSL estimates – can dominate benefit calculations.

per block. In this case, the ratio of benefits to costs is closer to 1.5, although we acknowledge that the ratio would be less than one if we were to use the maximum annual per-block cost estimate.

7 Conclusion

Our paper provides evidence that community-based monitoring can have a significant impact on reducing crime. This lends support to the idea that grassroots alternatives to monitoring may be an efficient way to curb crime relative to more conventional forms such as policing manpower. In the case of Safe Passage community monitoring in Chicago, we found that blocks patrolled by community monitors during school commuting hours exhibited an average decline in all crimes of about 29%, while violent, property, and minor crimes also showed significant declines as well. We document a decrease in violent crimes that almost doubles the decline in property crimes. Our findings suggest that the program's impacts were consistent with one of the major goals of the program: to improve safety and the security environment around public schools in Chicago.

We highlight that, although the intervention generally succeeded in reducing crime in the vicinity of schools, it also led to some negative externalities. Specifically, we find suggestive (albeit somewhat weak) evidence that offenders substituted to other types of crimes once a block was monitored as part of the Safe Passage program. This is particularly evident for certain property crimes such as burglaries and motor vehicle thefts, although the magnitude of the increase in alternative crime types is quite modest. Similarly, along these lines, we find significant evidence that drug-related crimes spilled over into blocks that were more than one-half mile from monitored blocks. In particular, drug-related crimes increased by more than 20% in non-treated blocks in the outskirts of neighborhoods with Safe Passage community monitoring. For other types of crime, we find no evidence of spatial reallocations resulting from the monitoring intervention.

Perhaps surprisingly, however, our findings suggest that untreated blocks located near treated blocks benefitted from the monitoring intervention even if though they were not directly treated. Nearby blocks exhibited declines of up to 40% in certain crime categories, suggesting important monitoring spillovers. In the case of intertemporal substitution of crime, we find no evidence of shifts in crime towards non-monitored periods. Instead, we find strong evidence that community monitoring is the main driver behind the observed drops in crime. Differences in most crime categories between treated and non-treated blocks are negligent during non-monitoring hours (evenings, weekends, school holidays, and summer months).

Finding interventions that deter crime is an important public policy issue. We provide evidence that a simple, cost-effective community-based intervention like Chicago's Safe Passage program can be effective in curbing crime. This is also an intervention that can be readily replicated in other communities and settings.

Additionally, such interventions can have desirable externalities. For instance, in the case of Chicago's Safe Passage program, community monitoring may lead to positive benefits for schoolchildren by reducing children's exposure to crime and improving safety around schools.

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Tables and Figures

Table 1: Safe Passage Community Monitoring Program Characteristics, Separately by School Year, 2010/11-2015/16

	CPS Schools			Characteristics			
	Elementary	High School	Total	Treated Blocks	Community Monitors	Treatment Intensity	Hours Per Day
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
2010/11	0	26	26	473	599	1.26	6
2011/12	0	34	34	533	640	1.20	6
2012/13	0	35	35	541	635	1.17	5.5
2013/14	54	36	90	1,237	1,251	1.01	5.5
2014/15	79	51	130	1,335	1,289	0.97	5.5
2015/16	86	54	140	1,551	1,316	0.85	5

Notes: Program characteristics come from Chicago Board of Education (CBOE) procurement contracts, street-level maps of Safe Passage routes from Chicago Public Schools (CPS), historical snapshots of the CPS Safe Passage webpage, and official press releases from the CPS Office of Communication. For more information about these sources, please see Appendix B. Columns (1) and (2) contain the total number of Safe Passage-designated CPS schools in each school year. We combine K-8, PK-8, and 6-8 grade configurations in the elementary school category.

Table 2: The Effect of Safe Passage Community Monitoring on Crimes, Overall and Separately by Category

	(1) Control Mean	(2) Within 0.25 miles	(3) Within 0.25 miles	(4) Within 0.50 miles
All Crimes	5.042	-1.647***	-1.481***	-1.716***
		(0.310)	(0.299)	(0.331)
Violent Crimes	0.464	-0.153***	-0.144***	-0.160***
		(0.029)	(0.029)	(0.031)
Property Crimes	1.335	-0.220***	-0.209**	-0.248***
		(0.082)	(0.081)	(0.083)
Non-Index Crimes	3.243	-1.274***	-1.128***	-1.307***
		(0.230)	(0.220)	(0.248)
Neighborhood time trends		No	Yes	Yes
Observations		153,472	153,472	243,210
Clusters		64	64	74

Notes: Outcome variables measure the number of crimes (overall and separately by category) per block in a given school year (regular attendance days only). All specifications include block and year fixed-effects, and some specifications include linear neighborhood-level time trends (as noted). Standard errors are clustered at the neighborhood-level in all specifications. Column (1) reports average block-level crime for untreated blocks within 0.25-miles of treated Safe Passage blocks. Columns (2), (3), and (4) restrict the sample to treated Safe Passage blocks and control blocks within 0.25- and 0.50-miles, respectively, of Safe Passage blocks. Individual crimes contained in each category are enumerated in Appendix B. Asterisks denote statistical significance: *** p<.01, ** p<.05, * p<.10.

Table 3: The Effect of Safe Passage Community Monitoring on Crimes, Separately by Sub-Category

	(1) Control Mean	(2) Within 0.25 miles	(3) Within 0.25 miles	(4) Within 0.50 miles
Panel A: Violent Crimes				
Aggr. Assault/Battery	0.226	-0.089***	-0.081***	-0.088***
		(0.016)	(0.016)	(0.016)
Robbery	0.211	-0.059***	-0.059***	-0.067***
		(0.020)	(0.019)	(0.021)
Other	0.027	-0.005	-0.005	-0.006
		(0.004)	(0.004)	(0.004)
Panel B: Property Crimes				
Burglary	0.306	0.023	0.025	0.018
		(0.017)	(0.017)	(0.018)
Larceny	0.812	-0.253***	-0.250***	-0.276***
		(0.071)	(0.070)	(0.072)
Other	0.217	0.010	0.016	0.010
		(0.013)	(0.013)	(0.013)
Panel C: Non-Index Crimes				
Simple Assault/Battery	1.101	-0.706***	-0.675***	-0.731***
		(0.112)	(0.110)	(0.114)
Drug Crimes	0.715	-0.271***	-0.194***	-0.259***
		(0.065)	(0.057)	(0.071)
Weapons Violation	0.069	-0.052***	-0.052***	-0.053***
		(0.013)	(0.013)	(0.013)
Other	1.358	-0.245***	-0.206**	-0.264***
		(0.092)	(0.089)	(0.099)
Neighborhood time trends		No	Yes	Yes
Observations		153,472	153,472	243,210
Clusters		64	64	74

Notes: Outcome variables measure the number of crimes (separately by sub-category) per block in a given school year (regular attendance days only). All specifications include block and year fixed-effects, and some specifications include linear neighborhood-level time trends (as noted). Standard errors are clustered at the neighborhood-level in all specifications. Column (1) reports average block-level crime for untreated blocks within 0.25-miles of treated Safe Passage blocks. Columns (2), (3), and (4) restrict the sample to treated Safe Passage blocks and control blocks within 0.25- and 0.50-miles, respectively, of Safe Passage blocks. Individual crimes contained in each sub-category are enumerated in Appendix B. Asterisks denote statistical significance: *** p<.01, ** p<.05, * p<.10.

Table 4: The Effect of Safe Passage Community Monitoring on Crimes, By Year, Overall and Separately by Category

	(1) All Crimes	(2) Violent	(3) Property	(4) Non-Index
$SPCM_{t+6}$	0.392	0.029	-0.200**	0.564
	(0.559)	(0.042)	(0.093)	(0.503)
$SPCM_{t+5}$	0.331	0.055	-0.126	0.402
	(0.438)	(0.057)	(0.120)	(0.330)
$SPCM_{t+4}$	0.086	0.064	-0.099	0.120
	(0.245)	(0.040)	(0.087)	(0.211)
$SPCM_{t+3}$	-0.246	-0.046	-0.183**	-0.018
	(0.234)	(0.046)	(0.081)	(0.175)
$SPCM_{t+2}$	-0.253	0.021	-0.041	-0.233
	(0.210)	(0.046)	(0.058)	(0.195)
$SPCM_0$	-1.014***	-0.099**	-0.280***	-0.635***
	(0.227)	(0.044)	(0.097)	(0.151)
$SPCM_{t-1}$	-1.209***	-0.101**	-0.364***	-0.745***
	(0.313)	(0.047)	(0.102)	(0.226)
$SPCM_{t-2}$	-1.463***	-0.123**	-0.235*	-1.105***
	(0.356)	(0.057)	(0.126)	(0.244)
$SPCM_{t-3}$	-1.958***	-0.155**	-0.423***	-1.380***
	(0.395)	(0.064)	(0.113)	(0.292)
$SPCM_{t-4}$	-3.205***	-0.323***	-0.624***	-2.259***
	(0.657)	(0.069)	(0.191)	(0.465)
$SPCM_{t-5}$	-3.647***	-0.251***	-0.744**	-2.652***
	(0.859)	(0.060)	(0.299)	(0.575)
$SPCM_{t-6}$	-3.165***	-0.259***	-0.600**	-2.306***
	(0.983)	(0.087)	(0.234)	(0.723)
F-test	0.363	0.026	0.130	0.456
Control Mean	7.370	0.654	1.770	4.940
Observations	153,472	153,472	153,472	153,472
Clusters	64	64	64	64

Notes: Outcome variables measure the number of crimes (separately by category) per block in a given school year (regular attendance days only). All specifications include block fixed-effects, year fixed-effects, and linear neighborhood-level time trends. Standard errors are clustered at the neighborhood-level in all specifications. All specifications restrict sample to blocks in neighborhoods with Safe Passage program. "Control Mean" refers to average number of crimes reported during the school year in blocks further than 0.50 miles from Safe Passage blocks. Asterisks denote statistical significance: *** p<.01, ** p<.05, * p<.10.

Table 5: The Effect of Safe Passage Community Monitoring on Spatial Crime Spillovers, Overall and Separately by Sub-Category

		Violent Crimes			Property Crimes			Non-Index Crimes			
	All Crimes	Aggr. Assault	Robbery	Other	Burglary	Larceny	Other	Simple Assault	Weapons Viola-tion	Drug crimes	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Safe Passage	-2.305***	-0.113***	-0.080***	-0.006	-0.019	-0.362***	-0.014	-0.869***	-0.054***	-0.392***	-0.397***
	(0.359)	(0.017)	(0.021)	(0.004)	(0.022)	(0.083)	(0.016)	(0.122)	(0.013)	(0.089)	(0.109)
Adjacent	-1.368***	-0.062***	-0.042***	-0.003	-0.067**	-0.173***	-0.034**	-0.383***	-0.016	-0.279***	-0.308***
	(0.221)	(0.017)	(0.014)	(0.004)	(0.027)	(0.061)	(0.015)	(0.084)	(0.012)	(0.068)	(0.057)
Quarter mile	-0.392**	-0.018**	-0.008	0.001	-0.025	-0.054	-0.016	-0.092**	0.004	-0.110**	-0.073**
	(0.156)	(0.008)	(0.008)	(0.002)	(0.018)	(0.053)	(0.011)	(0.044)	(0.003)	(0.053)	(0.036)
Half mile	-0.113	-0.014**	0.003	-0.001	-0.021	-0.009	-0.012	-0.028	0.002	-0.011	-0.022
	(0.085)	(0.006)	(0.007)	(0.001)	(0.014)	(0.037)	(0.008)	(0.021)	(0.002)	(0.025)	(0.023)
Control mean	3.120	0.106	0.114	0.014	0.215	0.704	0.145	0.583	0.029	0.305	0.901
Observations	337,304	337,304	337,304	337,304	337,304	337,304	337,304	337,304	337,304	337,304	337,304
Clusters	51	51	51	51	51	51	51	51	51	51	51

Notes: Outcome variables measure the number of crimes (separately by sub-category) per block in a given school year (regular attendance days only). All specifications include block fixed-effects, year fixed-effects, and linear neighborhood-level time trends. Standard errors are clustered at the neighborhood-level in all specifications. "Control Mean" refers to the average number of crimes reported on blocks located more than 0.50 miles from treated Safe Passage blocks. The sample is restricted to neighborhoods with at least one Safe Passage route. Individual crimes contained in each category are enumerated in Appendix B. Asterisks denote statistical significance: *** p<.01, ** p<.05, * p<.10.

Table 6: The Effect of Safe Passage Community Monitoring on the Spatial Reallocation of Crime, Overall and Separately by Sub-Category

		Violent Crimes			Property Crimes			Non-Index Crimes			
	All Crimes	Aggr. Assault	Robbery	Other	Burglary	Larceny	Other	Simple Assault	Weapons Viola-tion	Drug crimes	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
SPCM	0.239**	-0.010	0.006	-0.001	0.007	0.061	0.019	0.032*	-0.001	0.070**	0.055
	(0.114)	(0.006)	(0.008)	(0.002)	(0.023)	(0.060)	(0.016)	(0.019)	(0.004)	(0.030)	(0.042)
Mean	3.090	0.101	0.109	0.013	0.210	0.716	0.142	0.572	0.027	0.296	0.906
Observations	425,372	425,372	425,372	425,372	425,372	425,372	425,372	425,372	425,372	425,372	425,372
Clusters	98	98	98	98	98	98	98	98	98	98	98

Notes: Outcome variables measure the number of crimes (separately by sub-category) per block in a given school year (regular attendance days only). All specifications include block fixed-effects, year fixed-effects, and linear neighborhood-level time trends. Standard errors are clustered at the neighborhood-level in all specifications. "Control Mean" refers to the average number of crimes reported on blocks located more than 0.50 miles from treated Safe Passage blocks. The sample is restricted to neighborhoods with at least one Safe Passage route. Individual crimes contained in each category are enumerated in Appendix B. Asterisks denote statistical significance: *** p<.01, ** p<.05, * p<.10.

Table 7: The Effect of Safe Passage Community Monitoring on the Intertemporal Reallocation of Crime, Overall and Separately by Category

	Baseline	Day (6am-6pm)	Night (6pm-6am)	Weekends	Non-Att. Days	Summer
	(1)	(2)	(3)	(4)	(5)	(6)
All Crimes	-1.481***	-1.359***	-0.121	-0.046	-0.020	-0.067
	(0.299)	(0.254)	(0.095)	(0.086)	(0.032)	(0.087)
Violent Crimes	-0.144***	-0.123***	-0.021	0.000	-0.009	-0.014
	(0.029)	(0.021)	(0.015)	(0.014)	(0.005)	(0.013)
Property Crimes	-0.209**	-0.234***	0.025	0.007	0.002	-0.036
	(0.081)	(0.069)	(0.022)	(0.024)	(0.014)	(0.037)
Non-Index Crimes	-1.128***	-1.002***	-0.125	-0.053	-0.013	-0.017
	(0.220)	(0.190)	(0.076)	(0.067)	(0.021)	(0.061)
Observations	153,472	153,472	153,472	153,472	153,472	139,520
Clusters	64	64	64	64	64	64

Notes: Outcome variables measure the number of crimes (overall and separately by category) per block in a given school year or in the summer months as in Column (6). All specifications include block fixed-effects, year fixed-effects, and linear neighborhood-level time trends. Standard errors are clustered at the neighborhood-level in all specifications. The sample is restricted to treated Safe Passage blocks and control blocks within 0.25-miles of Safe Passage blocks. “Baseline” refers to total crimes on regular attendance days during the school year. “Day” refers to total crimes on regular attendance days within the school year occurring between 6AM and 6PM, while “Night” refers to total crimes on regular attendance days within the school year occurring between 6PM and 6AM. “Non-Attendance Days” refers to total crimes in days the school is off due to holidays, professional development days, breaks, etc. “Summer” refers to total crimes during summer vacation (approximately mid-June to mid-September each year). Individual crimes contained in each category are enumerated in Appendix B. Asterisks denote statistical significance: *** p<.01, ** p<.05, * p<.10.

Table 8: The Effect of Safe Passage Community Monitoring on the Intertemporal Reallocation of Crime, Separately by Sub-Category

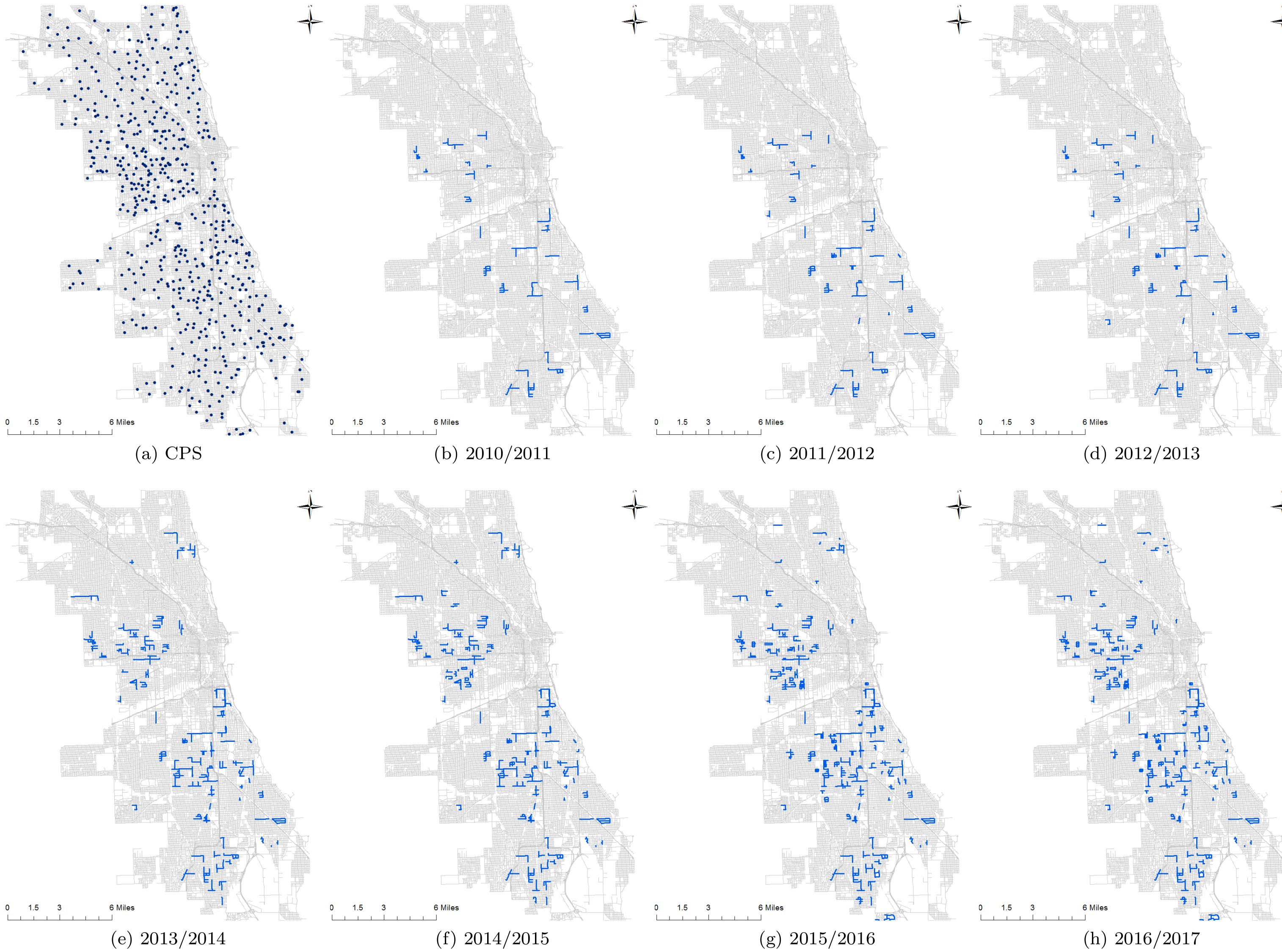
	Baseline	Day (6am-6pm)	Night (6pm-6am)	Weekends	Non-Att. Days	Summer
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Violent Crimes						
Aggr. Assault/battery	-0.081***	-0.067***	-0.014*	-0.003	-0.002	0.002
	(0.016)	(0.014)	(0.008)	(0.009)	(0.004)	(0.009)
Robbery	-0.059***	-0.055***	-0.004	0.010	-0.007**	-0.021**
	(0.019)	(0.013)	(0.011)	(0.010)	(0.003)	(0.010)
Other	-0.005	-0.001	-0.004	-0.007**	0.000	0.006*
	(0.004)	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)
Panel B: Property Crimes						
Burglary	0.025	0.024*	0.001	0.011	0.000	-0.009
	(0.017)	(0.013)	(0.008)	(0.010)	(0.004)	(0.010)
Larceny	-0.250***	-0.262***	0.011	-0.022	-0.006	-0.036
	(0.070)	(0.062)	(0.017)	(0.015)	(0.012)	(0.028)
Other	0.016	0.004	0.012	0.018**	0.008**	0.009
	(0.013)	(0.010)	(0.008)	(0.008)	(0.004)	(0.011)
Panel C: Non-Index Crimes						
Simple Assault/battery	-0.675***	-0.646***	-0.030	-0.023	0.010	0.011
	(0.110)	(0.102)	(0.024)	(0.027)	(0.010)	(0.024)
Drug Crimes	-0.194***	-0.119**	-0.075**	-0.032	-0.020*	-0.012
	(0.057)	(0.051)	(0.030)	(0.025)	(0.010)	(0.023)
Weapons Violation	-0.052***	-0.049***	-0.003	0.004	-0.001	-0.002
	(0.013)	(0.011)	(0.005)	(0.004)	(0.003)	(0.005)
Other	-0.206**	-0.188***	-0.018	-0.002	-0.002	-0.014
	(0.089)	(0.066)	(0.038)	(0.031)	(0.011)	(0.037)
Observations	153,472	153,472	153,472	153,472	153,472	139,520
Clusters	64	64	64	64	64	64

Notes: Outcome variables measure the number of crimes (separately by sub-category) per block in a given school year or in the summer months as in Column (6). All specifications include block fixed-effects, year fixed-effects, and linear neighborhood-level time trends. Standard errors are clustered at the neighborhood-level in all specifications. The sample is restricted to treated Safe Passage blocks and control blocks within 0.25-miles of Safe Passage blocks. “Baseline” refers to total crimes on regular attendance days during the school year. “Day” refers to total crimes on regular attendance days within the school year occurring between 6AM and 6PM, while “Night” refers to total crimes on regular attendance days within the school year occurring between 6PM and 6AM. “Non-Attendance Days” refers to total crimes in days the school is off due to holidays, professional development days, breaks, etc. “Summer” refers to total crimes during summer vacation (approximately mid-June to mid-September each year). Individual crimes contained in each category are enumerated in Appendix B. Asterisks denote statistical significance: *** p<.01, ** p<.05, * p<.10.

Table 9: The Estimated Benefits of Safe Passage Community Monitoring

	Estimates		Preferred			Sensitivity		
	Coeff (1)	95% CI (2)	Dollar Value (3)	Overall (4)	95% CI (5)	Dollar Value (6)	Overall (7)	95% CI (8)
Aggravated Assault/Battery	-0.081	[-0.112, -0.050]	96,600	7,824	[4,830, 10,819]	49,128	3,979	[2,456, 5,502]
Robbery	-0.059	[-0.096, -0.022]	320,160	18,889	[7,043, 30,735]	43,884	2,589	[965, 4,212]
Larceny	-0.250	[-0.387, -0.113]	4,600	1,150	[520, 1,780]	4,600	1,150	[520, 1,780]
Simple Assault/Battery	-0.675	[-0.891, -0.459]	21,850	14,748	[10,029, 19,468]	21,850	14,748	[10,029, 19,468]
Weapons Violation	-0.052	[-0.077, -0.027]	–	–	–	–	–	–
Total			443,210	42,611	[22,422, 62,802]	119,462	22,466	[14,150, 30,962]

Notes: The coefficient estimates and associated confidence intervals in Columns (1) and (2) are reproduced from Column (3) of Table 2 in this paper. The monetary estimates for the social cost of crime victimization come from Miller et al. (1996) [Column (6): Aggravated Assault/Battery and Robbery], Cohen et al. (2004) [Column (3): Aggravated Assault/Battery and Robbery], and Cohen and Piquero (2008) [Columns (3) and (6): Larceny and Simple Assault/Battery]. All monetary values are measured in 2015 dollars.

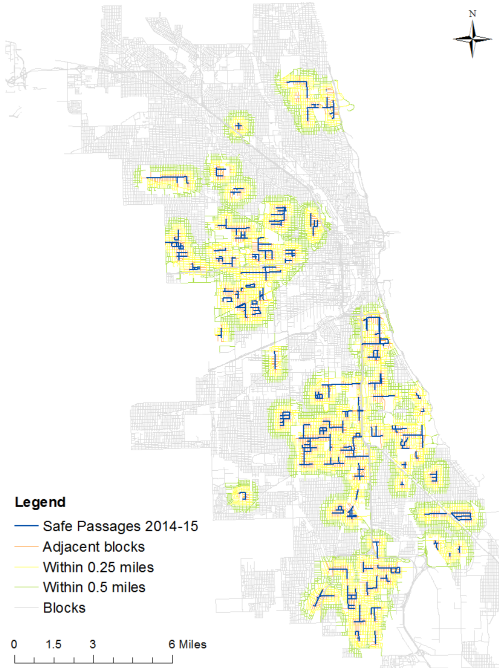


(a) CPS (b) 2010/2011 (c) 2011/2012 (d) 2012/2013

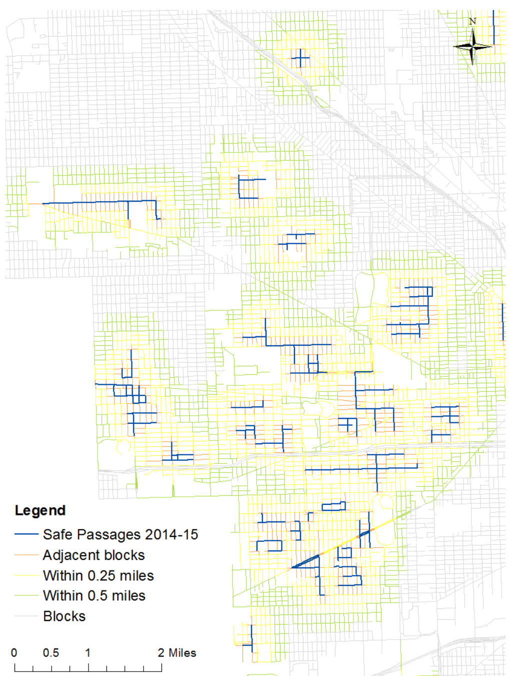
(e) 2013/2014 (f) 2014/2015 (g) 2015/2016 (h) 2016/2017

Figure 1: Locations of Elementary, Middle, and High Schools in Chicago Public Schools (CPS) and Safe Passage Routes, 2010/11-2016/17 School Years

Notes: Panel (a) presents the locations of traditional elementary, middle, and high schools in Chicago Public Schools (CPS), 2010/11-2016/17. Panels (b)-(h) present Safe Passage routes, separately by school year, for 2010/11-2016/17.



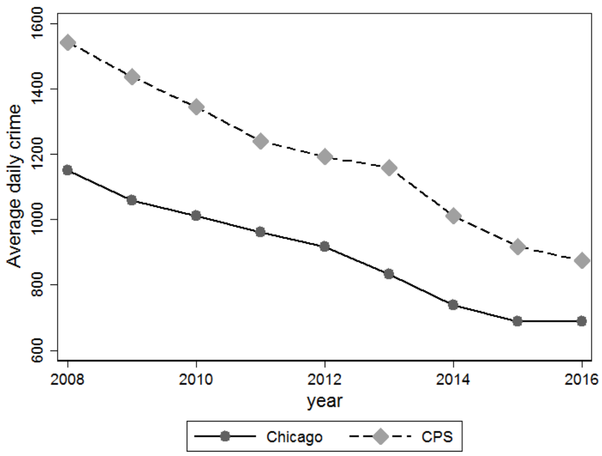
(a) Treatment and Control blocks



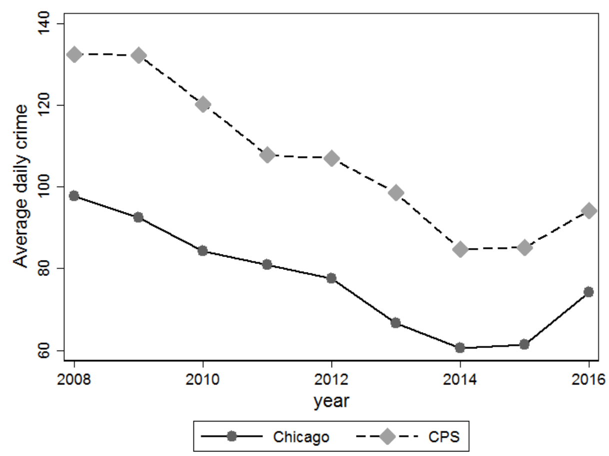
(b) Treatment and Control blocks

Figure 2: Treatment and Control blocks (2014/15 School Year)

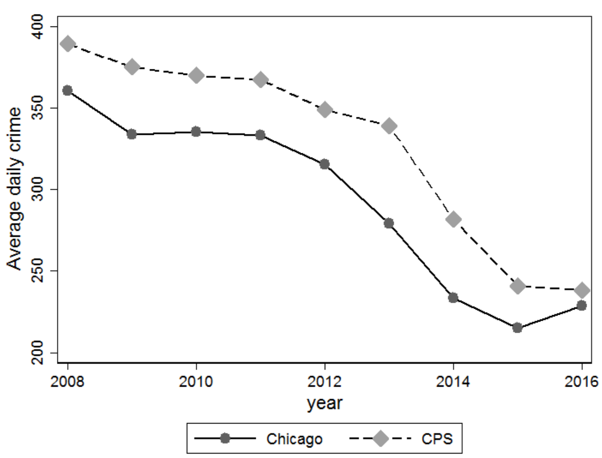
Notes: Treatment blocks include those directly covered by Safe Passage community monitoring. Adjacent blocks refer to blocks that are directly adjacent to Safe Passage blocks, Within 0.25- and 0.50-miles refer to blocks located within 0.25- and 0.50-miles of treated Safe Passage blocks. Panel (a) depicts all Safe Passage routes for the 2014/15 School Year, while Panel (b) provides a close-up illustration of several alternative definitions of the control group.



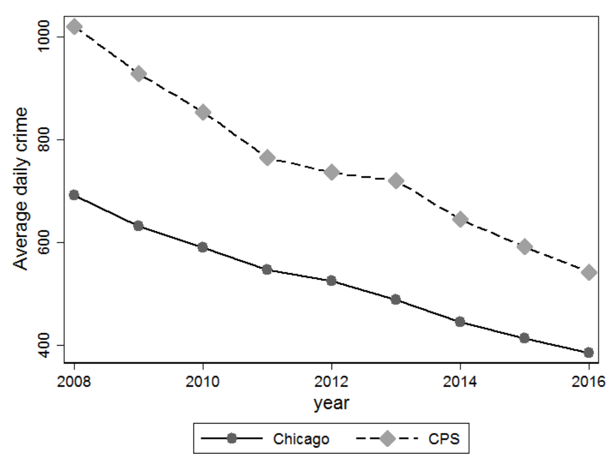
(a) All Crimes



(b) Violent Crimes



(c) Property Crimes



(d) Non-Index Crimes

Figure 3: Trends in Average Daily Crime, City of Chicago vs. 0.25-Mile Radius around Chicago Public Schools (CPS), 2008-2016

Notes: Dashed lines labeled "CPS" depict the average number of daily crimes reported within a 0.25-mile radius around elementary, middle, and high schools in Chicago Public Schools (CPS). Solid lines labeled "Chicago" depict the average number of daily crimes reported within the city of Chicago. Individual crimes contained in each category are enumerated in Appendix B.

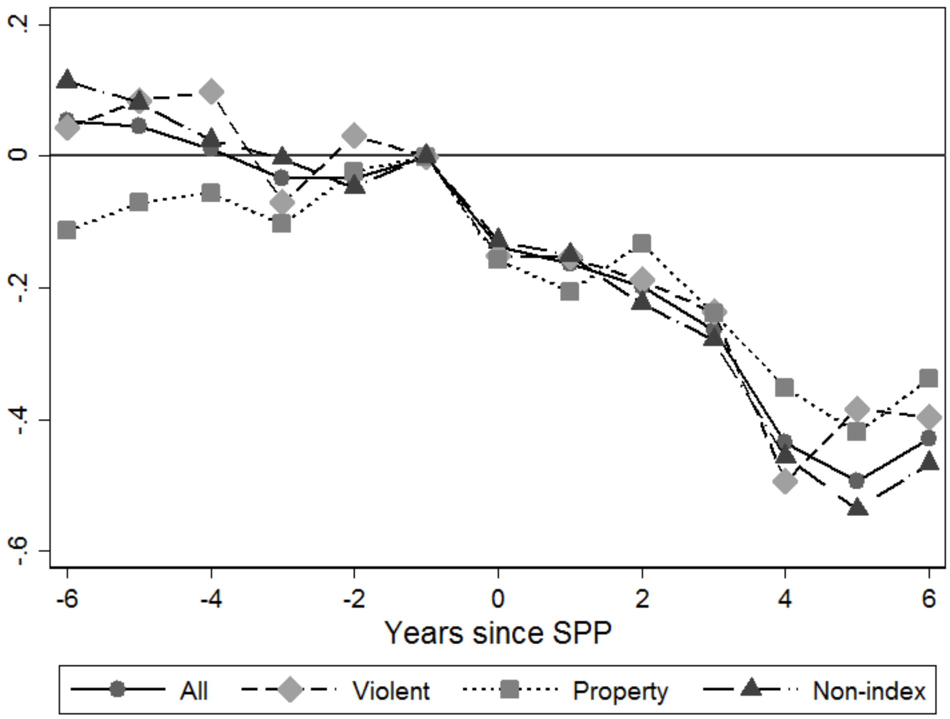


Figure 4: All Crimes and Crimes by Category
Notes: Figure notes go here.

Appendix A: Supplemental Results

Table A1: P-value Adjustments for the Effect of Safe Passage Community Monitoring on Crimes, Overall and Separately by Category

	Within 0.25 miles		Within 0.25 miles		Within 0.50 miles	
	Coeff. (1)	p-values (2)	Coeff. (3)	p-values (4)	Coeff. (5)	p-values (6)
All Crimes	-1.647***	0.000	-1.481***	0.000	-1.716***	0.000
		(0.000)		(0.000)		(0.000)
		[0.000]		[0.000]		[0.000]
Violent Crimes	-0.153***	0.000	-0.144***	0.000	-0.160***	0.000
		(0.000)		(0.000)		(0.000)
		[0.000]		[0.000]		[0.000]
Property Crimes	-0.220***	0.009	-0.209**	0.012	-0.248***	0.004
		(0.009)		(0.012)		(0.004)
		[0.009]		[0.012]		[0.004]
Non-Index Crimes	-1.274***	0.000	-1.128***	0.000	-1.307***	0.000
		(0.000)		(0.000)		(0.000)
		[0.000]		[0.000]		[0.000]

Notes: "Coeff." refers to the coefficient estimates presented in Table 2. "p-values" present the unadjusted p-values along with p-values adjusted for multiple hypothesis testing. P-values reported in parentheses use the Benjamini and Hochberg (1995) method as described in Anderson (2008). P-values in brackets use the Bonferroni-Holm methodology. Individual crimes contained in each category are enumerated in Appendix B.

Table A2: P-value Adjustments for the Effect of Safe Passage Community Monitoring on Crimes, Separately by Sub-Category

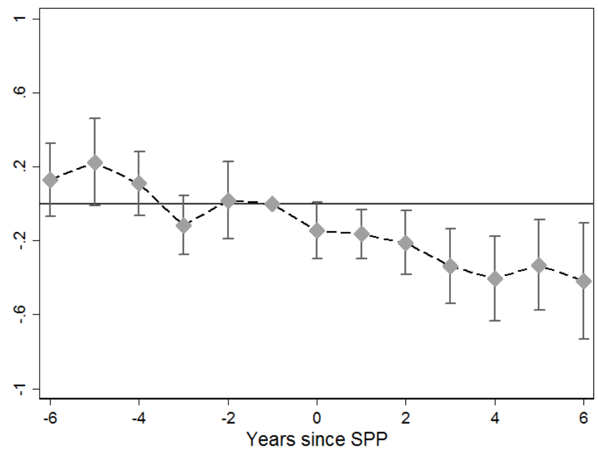
	Within 0.25 miles		Within 0.25 miles		Within 0.50 miles	
	Coeff.	p-values	Coeff.	p-values	Coeff.	p-values
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Violent Crimes						
Aggr. Assault/Battery	-0.089***	0.000	-0.081***	0.000	-0.088***	0.000
		(0.000)		(0.000)		(0.000)
		[0.000]		[0.000]		[0.000]
Robbery	-0.059**	0.004	-0.059**	0.003	-0.067***	0.002
		(0.007)		(0.006)		(0.003)
		[0.020]		[0.017]		[0.009]
Other	-0.005	0.199	-0.005	0.253	-0.006	0.177
		(0.221)		(0.253)		(0.221)
		[0.557]		[0.441]		[0.530]
Panel B: Property Crimes						
Burglary	0.023	0.186	0.025	0.147	0.018	0.321
		(0.221)		(0.184)		(0.356)
		[0.557]		[0.441]		[0.641]
Larceny	-0.253***	0.001	-0.250***	0.001	-0.276***	0.000
		(0.001)		(0.002)		(0.001)
		[0.004]		[0.005]		[0.002]
Other	0.010	0.433	0.016	0.214	0.010	0.443
		(0.433)		(0.237)		(0.443)
		[0.557]		[0.441]		[0.641]
Panel C: Non-Index Crimes						
Simple Assault/Battery	-0.706***	0.000	-0.675***	0.000	-0.731***	0.000
		(0.000)		(0.000)		(0.000)
		[0.000]		[0.000]		[0.000]
Weapons Violations	-0.052***	0.000	-0.052***	0.000	-0.053***	0.000
		(0.000)		(0.000)		(0.000)
		[0.001]		[0.001]		[0.001]
Drug Crimes	-0.271***	0.000	-0.194***	0.001	-0.259***	0.000
		(0.000)		(0.002)		(0.001)
		[0.001]		[0.007]		[0.003]
Other	-0.245**	0.010	-0.206*	0.024	-0.264**	0.010
		(0.014)		(0.034)		(0.014)
		[0.039]		[0.095]		[0.038]

Notes: "Coeff." refers to the coefficient estimates presented in Table 3. "p-values" present the unadjusted p-values along with p-values adjusted for multiple hypothesis testing. P-values in parentheses use the Benjamini and Hochberg (1995) method as described in Anderson (2008). P-values in brackets use the Bonferroni-Holm methodology. Individual crimes contained in each sub-category are enumerated in Appendix B.

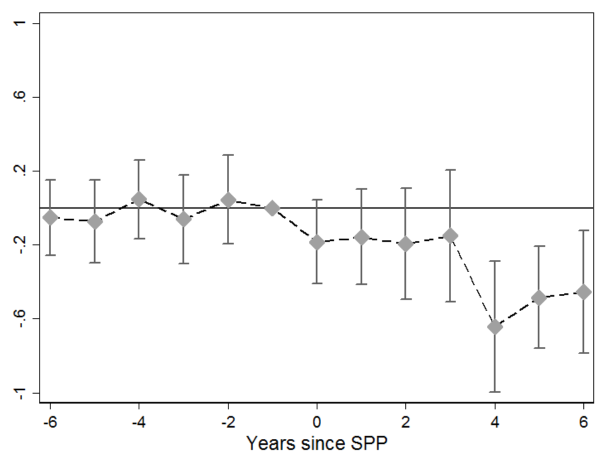
Table A3: The Effect of Safe Passage Community Monitoring on Crimes, By Year, Separately by Sub-Category

		Violent Crimes			Property Crimes			Non-Index			
	All Crimes	Aggr. Assault	Robbery	Other	Burglary	Larceny	Other	Simple Assault	Weapons Viola- tion	Drug crimes	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
$SPCM_{t+6}$	0.392	0.041	-0.016	0.003	-0.032	-0.177**	0.008	0.252	0.013	0.103	0.196
	(0.559)	(0.031)	(0.031)	(0.008)	(0.034)	(0.080)	(0.023)	(0.173)	(0.027)	(0.142)	(0.297)
$SPCM_{t+5}$	0.331	0.072*	-0.022	0.006	-0.023	-0.094	-0.008	0.301*	0.066*	0.112	-0.077
	(0.438)	(0.037)	(0.034)	(0.009)	(0.050)	(0.103)	(0.028)	(0.164)	(0.039)	(0.155)	(0.146)
$SPCM_{t+4}$	0.086	0.035	0.014	0.015	-0.013	-0.090	0.003	0.153	-0.000	0.021	-0.053
	(0.245)	(0.028)	(0.032)	(0.009)	(0.028)	(0.093)	(0.019)	(0.122)	(0.019)	(0.103)	(0.092)
$SPCM_{t+3}$	-0.246	-0.037	-0.019	0.010	-0.060*	-0.088	-0.034	-0.014	-0.001	0.025	-0.028
	(0.234)	(0.025)	(0.036)	(0.008)	(0.035)	(0.080)	(0.025)	(0.106)	(0.019)	(0.083)	(0.091)
$SPCM_{t+2}$	-0.253	0.006	0.014	0.001	-0.036	-0.026	0.020	-0.062	-0.038**	-0.060	-0.073
	(0.210)	(0.033)	(0.036)	(0.008)	(0.024)	(0.059)	(0.023)	(0.112)	(0.018)	(0.081)	(0.075)
$SPCM_{0}$	-1.014***	-0.046*	-0.056	0.003	-0.044	-0.232**	-0.004	-0.294***	-0.040**	-0.183***	-0.117*
	(0.227)	(0.024)	(0.035)	(0.007)	(0.028)	(0.097)	(0.024)	(0.083)	(0.018)	(0.059)	(0.069)
$SPCM_{t-1}$	-1.209***	-0.052**	-0.048	-0.001	-0.015	-0.341***	-0.007	-0.492***	-0.051***	-0.094	-0.108
	(0.313)	(0.021)	(0.039)	(0.009)	(0.028)	(0.103)	(0.021)	(0.117)	(0.018)	(0.062)	(0.106)
$SPCM_{t-2}$	-1.463***	-0.067**	-0.059	0.003	-0.001	-0.248**	0.014	-0.704***	-0.048**	-0.203***	-0.151
	(0.356)	(0.027)	(0.046)	(0.007)	(0.033)	(0.105)	(0.029)	(0.127)	(0.022)	(0.074)	(0.116)
$SPCM_{t-3}$	-1.958***	-0.107***	-0.045	-0.002	0.006	-0.444***	0.015	-0.804***	-0.061**	-0.204**	-0.312***
	(0.395)	(0.032)	(0.054)	(0.007)	(0.031)	(0.101)	(0.034)	(0.155)	(0.025)	(0.097)	(0.116)
$SPCM_{t-4}$	-3.205***	-0.129***	-0.195***	0.001	0.049	-0.717***	0.044	-1.341***	-0.081***	-0.243*	-0.594***
	(0.657)	(0.037)	(0.054)	(0.010)	(0.040)	(0.183)	(0.035)	(0.255)	(0.030)	(0.136)	(0.169)
$SPCM_{t-5}$	-3.647***	-0.106***	-0.147***	0.002	0.094**	-0.847***	0.009	-1.484***	-0.111***	-0.317**	-0.740***
	(0.859)	(0.039)	(0.042)	(0.010)	(0.036)	(0.277)	(0.042)	(0.267)	(0.035)	(0.151)	(0.250)
$SPCM_{t-6}$	-3.165***	-0.133**	-0.138***	0.011	0.079*	-0.757***	0.079**	-1.355***	-0.112***	-0.264	-0.575**
	(0.983)	(0.050)	(0.051)	(0.012)	(0.047)	(0.219)	(0.035)	(0.327)	(0.033)	(0.195)	(0.284)
F-test	0.363	0.005	0.505	0.533	0.531	0.269	0.226	0.259	0.008	0.251	0.905
Control Mean	7.370	0.318	0.303	0.033	0.278	1.290	0.207	2.120	0.117	0.972	1.740
Observations	153,472	153,472	153,472	153,472	153,472	153,472	153,472	153,472	153,472	153,472	153,472
Clusters	64	64	64	64	64	64	64	64	64	64	64

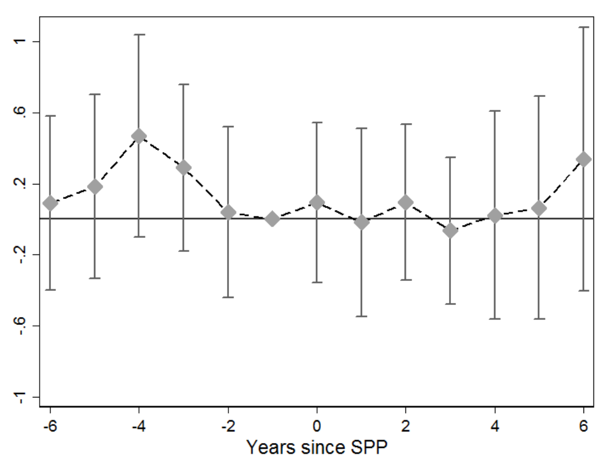
Notes: Outcome variables measure the number of crimes (separately by sub-category) per block in a given school year (regular attendance days only). All specifications include block fixed-effects, year fixed-effects, and neighborhood-level time trends. Standard errors are clustered at the neighborhood-level in all specifications. "Control Mean" refers to the average number of crimes during the school year in blocks more than than 0.50 miles from Safe Passage blocks. Individual crimes contained in each sub-category are enumerated in Appendix B. Asterisks denote statistical significance: *** p<.01, ** p<.05, * p<.10.



(a) Aggravated Assault and Battery

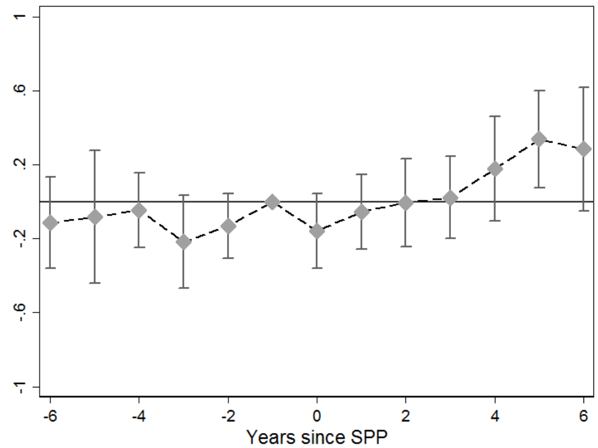


(b) Robbery

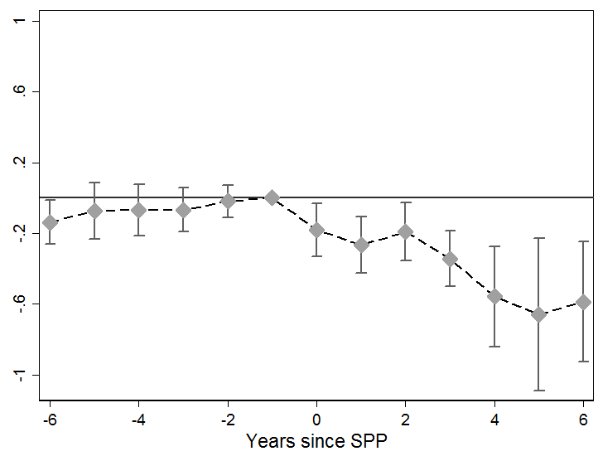


(c) Other Violent Crimes

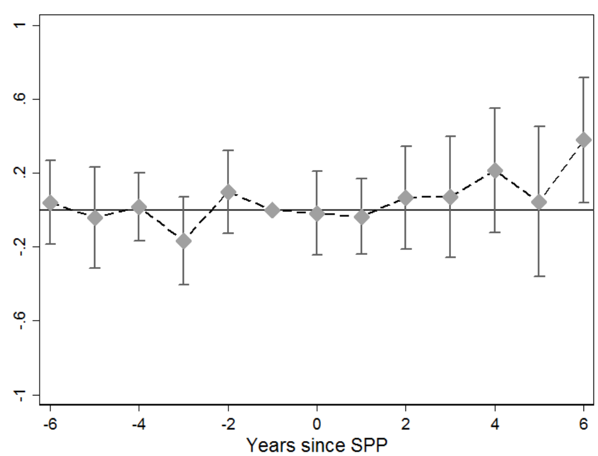
Figure A1: Violent Crimes within 0.25 miles of CPS Schools (2008-2016)
Notes: Data refers to reported crimes. SPP refers to crime near SPP schools. "SPP in neighborhood" refers to crime in non-SPP schools that are located within a neighborhood with other SPP schools. "No SPP" refers to crime near non-SPP schools in neighborhoods without any SPP school.



(a) Burglary



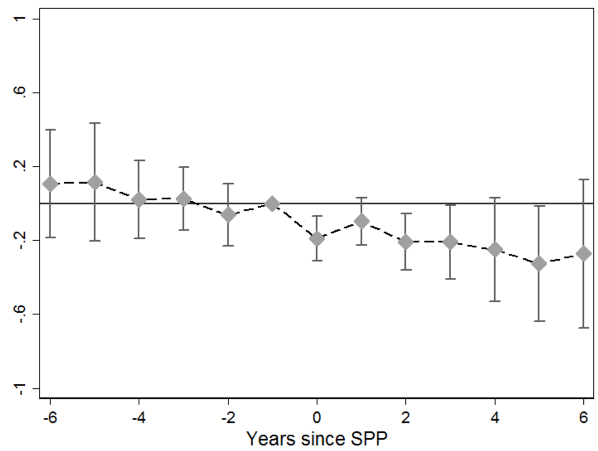
(b) Larceny



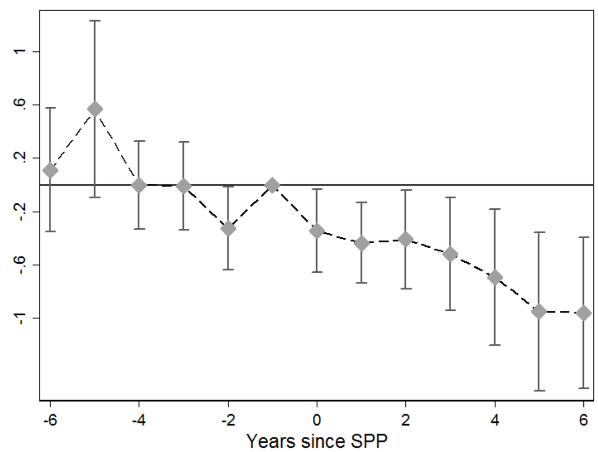
(c) Other Property Crimes

Figure A2: Property Crimes within 0.25 miles of CPS Schools (2008-2016)

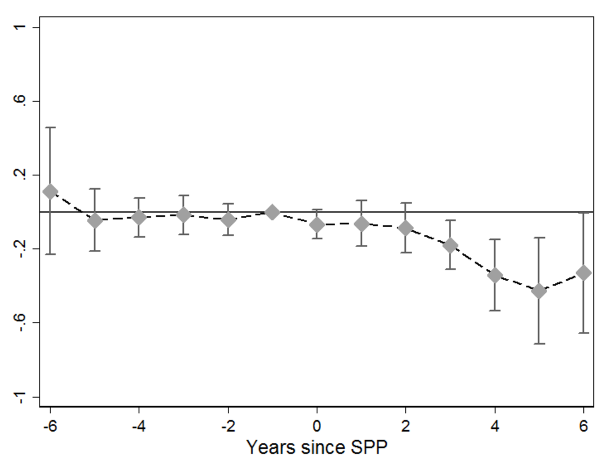
Notes: Data refers to reported crimes. SPP refers to crime near SPP schools. "SPP in neighborhood" refers to crime in non-SPP schools that are located within a neighborhood with other SPP schools. "No SPP" refers to crime near non-SPP schools in neighborhoods without any SPP school.



(a) Drug-Related Crimes



(b) Weapons-Related Crimes



(c) Other Non-Index Crimes

Figure A3: Non-Index Crimes within 0.25 miles of CPS Schools (2008-2016)

Notes: Data refers to reported crimes. SPP refers to crime near SPP schools. "SPP in neighborhood" refers to crime in non-SPP schools that are located within a neighborhood with other SPP schools. "No SPP" refers to crime near non-SPP schools in neighborhoods without any SPP school.

Appendix B: Data

Crime Data

We obtained data on crimes in Chicago from the Chicago Police Department (CPD) Citizen Law Enforcement Analysis and Reporting (CLEAR) system. These data contain information on all reported crimes in Chicago – to which the Chicago Police Department (CPD) responded and completed a case report – from 2001 to the present (the website is updated daily). These data are available for download through the City of Chicago Data Portal at: https://data.cityofchicago.org.

We divided all crimes into the following three mutually exclusive and exhaustive categories, following the Federal Bureau of Investigation (FBI) National Incident-Based Reporting System (NIBRS): Violent Crimes, Property Crimes, and Non-Index Crimes. For more information about these classifications, please see: http://gis.chicagopolice.org/clearmap_crime_sums/crime_types.html.

Main Crime Categories

Our three main crime categories were defined as follows:

- **Violent Crimes:**
  Homicide (1st and 2nd Degree), Criminal Sexual Assault, Robbery, Aggravated Assault, and Aggravated Battery
- **Property Crimes:**
  Burglary, Larceny, Motor Vehicle Theft, and Arson
- **Non-Index Crimes:**
  Involuntary Manslaughter, Simple Assault, Simple Battery, Forgery and Counterfeiting, Fraud, Embezzlement, Stolen Property, Vandalism, Weapons Violation, Prostitution, Criminal Sexual Abuse, Drug Abuse, Gambling, Offenses Against Family, Liquor License, Disorderly Conduct, and Miscellaneous Non-Index Offense.

Safe Passage Community Monitoring Data

We obtained information on Safe Passage Community Monitoring from the following sources:

Procurement Contracts from the Chicago Board of Education

The Chicago Board of Education (CBOE) is the financial arm of the Chicago Public Schools (CPS). We analyzed information contained in procurement contracts between the CBOE and the 501(c)(3) non-profit organizations that provided Safe Passage community monitoring services around designated CPS schools. The format and level of detail contained in these procurement contracts varied from year to year, but from the text of these contracts we obtained information on (1) the locations (schools) and (2) dates (school years) where Safe Passage community monitoring services were provided.

From these procurement contracts we also recorded information on the number of community monitors assigned to each school (or in later years to each neighborhood) and summed these to obtain the total number of community monitors contracted to provide Safe Passage community monitoring services by the CBOE each school year. We also summed the total anticipated payments to the non-profit organizations in order to estimate the annual costs of Safe Passage community monitoring each school year. Our assumptions about these costs and a discussion of how they compared to other sources of information on program costs are contained within the text of the paper.

The CBOE procurement contracts contained school-level information for the 2010/11-2013/14 school years (i.e., the contract specified which schools in CPS the provider would serve), but in later years, the 2014/15-2015/16 school years, the contracts only specified the number of community monitors assigned to each neighborhood.

In addition to this information, we also documented key Safe Passage program characteristics, separately by school year. These program characteristics included: the goals of the Safe Passage program, hourly pay for community monitors, the number of hours of daily coverage, the total number of school days for which coverage should be provided, the dates and topics of mandatory CPS-provided training for Safe Passage community monitors and for supervisors/managerial staff, the responsibilities for Program Administrators and other key personnel at each non-profit organization, rules for and limits on administrative costs associated with the program, details about CPS-provided equipment (two-way radios and cellular telephones), and information about CPS-provided uniforms.

Safe Passage Route Maps

For the 2013/14-2015/16 school years, CPS made detailed maps of Safe Passage routes available to the public by publishing the maps on the City of Chicago Data Portal. These maps can be accessed here: https://data.cityofchicago.org/. These route maps, which we exported as Shapefiles and analyzed in ArcGIS, displayed information on the streets where Safe Passage community monitors provided services around designated CPS schools. Using the attribute data contained in the ArcGIS Shapefiles, we were able to match street-level information to specific schools in CPS.

Historical Snapshots of the Safe Passage Website

Using the Wayback Machine of the Internet Archive, we obtained historical snapshots of the official CPS Safe Passage website from a series of dates corresponding to the beginning of each school year for the 2013/14-2015/16 school years (the website did not exist prior to the 2013/14 school year). The official CPS Safe Passage website is available here: http://cps.edu/Pages/safepassage.aspx. The Wayback Machine of the Internet Archive can be accessed here: https://archive.org/web/.

These historical snapshots of the CPS Safe Passage website showed us what information would have been available to CPS parents, students, and the public about Safe Passage community monitoring at the beginning of each school year between 2013/14-2015/16. The website snapshot from each date included a list of CPS schools with Safe Passage community monitoring coverage and – in most cases – a link to a .pdf map that displayed a Safe Passage route map for each CPS school in the program that year. We used the list of schools available on the CPS website to validate information in the procurement contracts and, when possible, we compared the school-level route in the .pdf maps to the street-level information that we obtained from the CPS Safe Passage Route Maps.

Press Releases from the Chicago Public Schools Office of Communication

Press releases from the CPS Office of Communication publicized the introduction and expansion of Safe Passage community monitoring within the district. These archived press releases are available here: http://cps.edu/News/Press_releases/Pages/Pressreleases.aspx. These CPS press releases underscored the district's commitment to student safety and informed parents about school-level participation in Safe Passage. We used information contained in these press releases to validate we found in other sources. This information was most useful for verifying the addition of new schools to the Safe Passage program and for obtaining the total number of elementary and high schools that were designated as Safe Passage schools each year.

Methods

Whenever possible, we used information from all of the sources available to us, and we found, for the most part, that these sources were largely consistent with one another. We first documented school-level participation in Safe Passage community monitoring by constructing a list of the CPS schools that were designated as Safe Passage schools, separately by school year. Once we had completed our list of schools with Safe Passage designation for the 2010/11-2015/16 school years, we then turned to the task of matching these schools to street-level route maps, separately by school year.

We began constructing our list of CPS Safe Passage schools, separately by school year, by qualitatively analyzing the CBOE procurement contracts. This was possible for the 2010/11-2013/14 school years. For the 2014/15-2015/16 school years, however, we relied on information on Safe Passage schools from historical website snapshots, because the CBOE procurement contracts no longer listed individuals schools (they instead listed neighborhoods). In the single overlapping school year for which we had a list of CPS schools from both sources (2013/14), we found that the information between the two sources was largely consistent. To complete our list of schools, we used information from CPS press releases to verify school-level designation (when possible) and also to confirm the total number of schools that participated in Safe Passage each year (it was common for CPS press releases to publicize the total number of elementary and high schools that were designated as Safe Passage schools each year). Once we had finalized our list of Safe Passage schools within CPS for for each school year, we then matched each school to its corresponding route map by using information contained in the attribute data of the ArcGIS Shapefiles. In cases where this information was not available, we relied on individual .pdf maps that we obtained from the historical website snapshots.