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### Does the Chicago Safe Passage Program Reduce Reported Crime Around Elementary Schools? Evidence from Longitudinal, Geocoded Crime Data

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#### Abstract

The Chicago Public School's Safe Passage program is a large-scale intervention designed to improve the safety of students as they travel to and from schools. By placing hundreds of adult monitors on designated streets around schools, the program has the potential to reduce crime. This study evaluates Safe Passage's impact on crime around primary schools during the 2013-14 expansion of the program. Using longitudinal, geocoded crime data and a difference-in-differences and triple-difference methodology, this study finds suggestive though not conclusive evidence that Safe Passage may have contributed to lower crime on designated routes despite some increases in crime around designated "welcoming schools" during the 2013-14 school year. Implications for the continued expansion of the program and further directions for research of the program are discussed.

**Keywords:** safety, commute, prevention, crime, school, urban, education

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## **Does the Chicago Safe Passage Program Reduce Reported Crime Around Elementary Schools? Evidence from Longitudinal, Geocoded Crime Data**

### **Introduction**

In recent years, the city of Chicago has drawn national headlines for violence against students before and after school (ABC News, 2009; NPR, 2013). In an effort to ensure the safe transit of students to and from schools, Chicago Public Schools (CPS) implemented the Safe Passage (SP) program. Started as a pilot program in the late 2000s and greatly expanded in 2013, SP represents one of the most robust interventions for improving student safety while commuting to and from school. Though multifaceted in design, the core of SP involves the hiring and placement of a large number of adult monitors along designated “safe passage” streets leading to and from schools. In 2014, nearly 600 SP workers were working around CPS schools with this number increasing to over 1,000 in more recent years (Associated Press, 2013; CPS, 2017).

These monitors, who are present for several hours in both the morning and afternoon, provide an official presence that may potentially reduce crime around schools. Indeed, claims of reduced crime have been made by school officials, the police department, and the media in recent years (Ali, 2015; CPS, 2014; 2015; Gulasingam, 2016; Masterson, 2017). For instance, a recent news headline read “Safe Passage Works, Data Shows” while another stated “CPS Says Crime Down by One-Third Along Safe Passage Routes” (Ali, 2015; Masterson, 2017). Likewise, the Chicago Public Schools’ website has made claims of decreased criminal incidents along SP routes (CPS, 2015). Similarly, other organizations, implementing SP-type programs in other locales, have also made claims regarding their ability to reduce crime (Urban Peace Institute, n.d.).

Such claims, however, have not been rigorously evaluated. During the same period of time that crime was decreasing along SP routes, reported crime was also dropping in the city of Chicago overall (Bernstein & Isackson, 2014, April 7; Papachristos, 2013). To date, little work has attempted to disentangle the impacts of SP on crime from general trends in crime within the city. Given the cost of SP, understanding the effects of the program are important as policymakers consider continued support and expansion. SP employees work approximately five hours a day for five days a week at a rate of 10 dollars per hour (CPS – Safe Passage, 2013). With over 1,000 Safe Passage employees hired (CPS, 2017) the weekly cost of wages alone is over \$250,000. Over the course of a year, this expense is nearly equivalent to hiring an additional 90 full-time teachers. In short, then, the SP program represents a costly intervention whose impacts are largely unknown.

The purpose of this study is to estimate the relationship between SP and the frequency of reported crime around Chicago public elementary schools (K-8<sup>th</sup> grades in Chicago) and on SP routes. This study focuses on the 2013-14 expansion of SP, in which the program was expanded to include elementary schools for the first time. In response to the closing of a large number of city schools due to under-enrollment, CPS designated a number of schools that were receiving displaced students as “welcoming schools” and provided most of these with a SP route (Torre et al., 2015). This study seeks to understand the impact of these SP routes on community crime both around these elementary schools in general as well as on the SP routes. In particular, this study addresses the following research questions:

- 1) What is the relationship between the presence of SP routes around Chicago primary schools and the number of crimes reported around the schools?

2) What is the relationship between the presence of SP workers on particular street segments and the number of crimes reported on those street segments?

To answer these questions, this study leverages several rigorous methodological approaches including difference-in-differences and triple-difference longitudinal designs comparing SP schools and streets to both schools without SP and to street segments without SP workers. In doing so, the study attempts to disentangle general trends in reported crime within the city from the impact of the SP program on reported crime rates thereby allowing for an empirical test of the claims that the media, school officials, and other policymakers have put forth regarding SP's ability to decrease reported crime around schools (Ali, 2015; CPS, 2015; Masterson, 2017).

## **Theoretical Framework and Prior Literature**

### ***Theoretical Framework***

Routine activity theory, a commonly invoked framework for understanding criminal activity, suggests that crime occurs when there is a “convergence of likely offenders and suitable targets in the absence of capable guardians” (Cohen & Felson, 1979; Felson & Boba, 2010; Wikström & Treiber, 2015). By placing responsible adults on the streets before and after school, SP offers an organized adult presence that can preempt crime that might otherwise occur (Eck, 1998). In this manner, SP workers serve as capable “guardians” that can disrupt the situation that gives rise to crime and therefore represent an opportunity-blocking crime prevention strategy (Wikström & Treiber, 2015). In doing so, SP workers closely resemble the foot patrol approach to policing seen in problem oriented policing and may serve as an opportunity-blocking mechanism to improve community safety (Clarke, 1995; Kelling & Wilson, 1982; Wilson & Kelling, 1989). A review of the empirical research on prior opportunity-blocking interventions

finds that over 90% of such studies across a range of environments report success at reducing crime (Eck, 1998). In summary, routine activity theory suggests that, by removing the opportunity to commit a crime through the placement of capable guardians, SP has the potential to reduce reported crime.

### ***Why Safety around Schools Matters***

Schools are influenced by the environment in which they reside and in which their students live. Unfortunately, many students live in or commute to school through environments that are unsafe and attractive for crime. In the 1990s, about one third of students nationally had experienced a personally threatening situation in their neighborhood within the last 30 days (Bowen & Bowen, 1999). Such unsafe environments in the neighborhoods surrounding schools can lead to unsafe environments within schools. In the early 1990s, a substantial proportion of American high school students reported having carried a weapon to school (Everett & Price, 1995; U.S. Department of Health and Human Services, 1996). The carrying of weapons may in part be motivated by perceived danger outside of school. For instance, in the MET Life survey, a national survey of 7<sup>th</sup> through 12<sup>th</sup> graders, researchers found that 38% of students believed the need for safety on the way to and from school prompted the carrying of weapons while at school (Everett & Price, 1995).

Such perceived danger outside of school can have impacts on schooling outcomes. For instance, Bowen & Bowen find that increased student reported school and neighborhood danger factors are significant predictors of decreased attendance, lower grades, and more behavior incidents (1999). Work from Chicago also finds a predictive relationship between neighborhood safety and student achievement (McCoy, Roy, & Sirkman, 2013). Other recent evidence suggests a causal relationship between neighborhood violence exposure and student academic

achievement. Sharkey (2010) finds that exposure to a violent act in the neighborhood predicts a nearly half standard deviation decrease in student achievement, a very large impact relative to other educational interventions (Sharkey, 2010). These findings demonstrate the importance of promoting student safety in the neighborhoods surrounding schools.

### ***Safe Passage Expansion of 2013-14***

Prior to 2013-14, SP had been in place exclusively at the high school level in Chicago. Due to mounting fiscal concerns and under-enrollment of many district schools, Chicago embarked on a process of closing 47 underutilized elementary schools prior to the start of the 2013-14 school year. As a result, students displaced by these closings were routed into a number of other schools. While students had choice as to which school to attend, the district designated certain schools as “welcoming schools” that displaced students were encouraged to attend. There were 48 designated welcoming schools, and about 66% of displaced students attended their designated welcoming school (Torre et al., 2015).

A concern at this time was the potential for displaced students to be able to safely access their new school. As a result of the closures, many students had to travel a greater distance to school and, in some cases, had to traverse neighborhoods that might have rival gang affiliations or, at the least, community contexts with which they were unfamiliar. As a result of these concerns, CPS scaled up the SP program to include elementary schools for the 2013-14 school year, with almost all of the new SP routes being placed at welcoming schools.

The new SP routes were geographically distributed around the city, reflecting the location of the school closures and new welcoming schools. Panel A of Figure 1 shows the 2013-14 SP routes and their corresponding schools. As shown, routes varied in size but were generally localized to within a quarter or half mile of the school they served. While to date there are no

published quantitative analyses of SP, qualitative research on the 2013 school closings suggests that families viewed SP routes favorably and, in some cases, may have chosen a school based on the availability of a SP route (Torre et al., 2015).

### ***Safe Passage-Type Programs***

SP, while unique in its scale and use of employed monitors, is not the first program designed to provide designated safe routes to school. Similar programs have been implemented on a smaller scale in Philadelphia, New York, and in higher education environments (Stokes, Donahue, Caron, & Greene, 1996; Sochet, 2001; McLean, Worden, Kim, Garmley, & Bonner, 2010). Similarly, the Urban Peace Institute is currently promoting Safe Passage-type programs in locations such as Los Angeles (Urban Peace Institute, n.d.). Many of these prior programs differ from SP in that they relied on police officers or community volunteers rather than paid employees of the program. This section provides a brief overview of prior research on school-based SP-type programs.

One of the earliest uses of a SP-type program took place in Philadelphia in 1995 (Stokes et al., 1996). Implemented in a single middle school for a six week period, Philadelphia's Safe Corridor program grew out of efforts to encourage "problem-oriented policing" for the improvement of student safety. The program used crime mapping to identify crime hot spots and to appropriately locate designated safe corridors for student travel. Unlike SP, Philadelphia's Safe Corridor initiative did not hire extra street monitors but instead relied on increased police patrols provided by a collection of local law enforcement agencies. Students and families were educated about the program and encouraged to make use of the designated routes (Stokes et al., 1996).

An evaluation of the Philadelphia Safe Corridor program revealed few impacts on student victimization (Stokes et al., 1996). The authors speculated that the lack of influence may have been in part due to an under-awareness of the corridor on the part of students and due to the nature of victimization. Specifically, the authors found that much of the victimization arose from other students and may have originated in the school building, a location less amenable to influence from the presence of the Safe Corridor (Stokes et al., 1996). Overall crime rates were not directly examined.

In a second example, New York City's Bushwick cluster of schools implemented a SP-type program as a result of a school and community partnership for safety. Like the Philadelphia Safe Corridor, the NYC program involved increased police presence and student education. Additionally, the NYC implementation involved the designation of businesses along the route as safe havens where children could go for help (Sochet, 2001). The impact of the program on crime rates, however, has not been evaluated.

Finally, McLean and colleagues (2010) evaluated a SP-type program for college students around an unidentified college campus. The program was created in response to college student victimization while travelling from residences, the campus, and off-campus venues such as bars. The safe corridor program involved designated routes, safe haven businesses, increased police presence, physical improvements such as increased lighting, and education initiatives to improve student awareness. Using an interrupted time series design, the authors found some impact of the program on property crimes but no influence on crimes against persons (McLean et al., 2010).

Outside of this limited research, few studies have examined the impact of SP-type programs on crime outcomes. However, few school-based interventions to increase student

safety outside of the school building have been as large as SP, and few have hired employees specifically for this purpose. This study seeks to fill this gap in the literature by evaluating the impact of SP on crime around schools and on street segments with SP workers. In doing so, it empirically tests the media and policymakers' claims that SP reduces crime (Ali, 2015; CPS, 2015; Masterson, 2017).

### Methods

The impact of SP on the number of reported crimes is assessed through several approaches, all of which broadly adhere to a difference-in-differences methodology. Difference-in-differences approaches estimate treatment effects through the use of longitudinal data on treated and untreated groups.

In practice, difference-in-differences is implemented using interaction terms in longitudinal data. The base model estimated in this analysis was as follows:

$$1) \text{Crime}_{ij} = \beta_1 \text{After}_{ij} + \beta_2 \text{SafePassage}_i + \beta_3 \text{SafePassage}_i * \text{After}_{ij} + \gamma_i + u_{ij}$$

Where *Crime* represents the number of reported crimes occurring in a specific area *i* on day *j*, *SafePassage* is a binary variable representing whether or not the area received the SP treatment, *After* is a binary variable representing whether or not the observation is in a time point after the start of the SP, *SafePassage\*After* is the interaction term between *SafePassage* and *After*,  $\gamma$  is a vector of school-area or street segment fixed effects, and *u* is the error term. As the *SafePassage* variable is time-invariant, it is absorbed in the school-area or street segment fixed effects and therefore does not result in an estimated coefficient. The coefficient of interest is that of the interaction term,  $\beta_3$ .

In addition to the standard difference-in-differences approach, a triple-difference model exploiting differential trends between weekday and weekends was used as a further means of controlling for temporal trends in reported crime rates. Given that SP workers are only present on weekdays when school is in session, weekend days provide an untreated time period that can be used to account for other contributors to changes in reported crime rates. Such a triple-difference approach using untreated days as an additional comparison has been used in the empirical literature to assess the effects of other policies that are day specific (i.e. Heaton, 2012). The triple-difference model takes on the following form:

$$\begin{aligned}
 2) \text{ Crime}_{ij} = & \beta_1 \text{After}_{ij} + \beta_2 \text{SafePassage}_i + \beta_3 \text{WeekDay}_{ij} + \beta_4 \text{SafePassage}_i * \text{After}_{ij} \\
 & + \beta_5 \text{SafePassage}_i * \text{WeekDay}_{ij} + \beta_6 \text{After}_i * \text{WeekDay}_{ij} \\
 & + \beta_7 \text{SafePassage}_i * \text{After}_{ij} * \text{WeekDay}_{ij} + \gamma_i + u_{ij}
 \end{aligned}$$

Where the variables are the same as those in equation 1 with the exception that a binary indicator of whether the day is a weekday has been added to the model along with corresponding interaction terms. The coefficient of interest in the triple-difference model is  $\beta_7$ .

Because the primary outcome variable in this study is a count variable of the number of crimes reported per day, both the difference-in-differences and triple-difference models were estimated using Poisson regression. Poisson regression is an appropriate methodological choice for modeling count outcomes and is preferable to ordinary least squares regression for this reason. With Poisson regression, the coefficient of interest is interpretable as the difference in the expected log count of crimes between SP and non-SP areas or as a percentage change in crime.

It is worth noting that another approach for modeling count data is the use of negative binomial regression, an approach that is appropriate when data are over-dispersed, having a variance greater than the mean. Unfortunately, negative binomial regression is not well suited to the inclusion of fixed-effects. As has been described by Allison & Waterman (2002), the standard fixed effects implementation of negative binomial regression is not a true fixed-effects model, as group in-variant variables are not absorbed. The approach described by Allison (2012) to address this issue, namely the inclusion of individual group dummy variables, is computationally complex and failed to converge in the models estimated in this study. As a result, Poisson regression results are shown, and the lack of these models to fully address over-dispersion is noted as a limitation.

In a difference-in-differences framework, the outcomes in the period during treatment are generally compared to the outcomes in the period prior to treatment. The primary analyses in this study compared reported crime during school days of the 2013-14 school year to reported crime during school days of the 2011-2012 and 2012-13 school years. Summer break, holiday breaks, and other non-instructional days were dropped from the analysis. In models using the difference-in-differences approach, weekend days are also dropped. In models using the tripe-difference approach, weekend days were retained in order to provide an additional source of variation that can account for temporal trends in crime.

### **Quarter Mile Radii Analysis**

Several different versions of equations 1 and 2 were estimated. First, efforts were made to explore the impact of SP on the general levels of crime around an elementary school. Rather than estimating the impact of SP on the particular street where workers are present, this analysis examined whether their presence had a broader impact on the general safety of a school's

immediate environment. To do so, the number of reported crimes within a quarter mile of each CPS elementary school were calculated. A quarter mile was chosen as it roughly approximated the general size of many of the SP routes and represented the immediate area of a school. Panel B of Figure 1 shows SP routes and quarter mile radii for a portion of the city. As shown, the quarter mile radii generally cover many of the SP routes. While some routes extend beyond this range, a quarter mile was still ideal insofar as this portion of the analysis sought to examine impacts on the general safety around the school with a SP route rather than around the route itself. As Chicago is a densely populated city, many elementary schools are within close proximity to each other. As seen in the figure, using a larger radius would result in substantial overlap across schools thereby rendering comparisons difficult.

In these analyses, the comparison group consisted of Chicago elementary schools that did not have SP routes in the 2013-14 school year. Given that SP routes were purposefully placed in schools in higher crime neighborhoods, the primary analyses restrict comparisons to such schools ( $n=34$ ) that would receive SP in later years (2014-15 through 2016-17). The use of non-SP elementary schools that would later implement SP provides a more convincing approximation of the counterfactual than the use of all Chicago elementary schools insofar as these later adopting schools are located in areas that have crime rates that are similar to those of the 2013-14 SP schools. Panel B of Figure 1 demonstrates examples of the quarter mile radii and their overlap with SP routes.

A potential confounder when exploring general crime rates around schools in the 2013-14 school year is the previously described closing of schools and redistribution of displaced students to “welcoming schools”. In 2013-14, over 80% of the K-8 SP routes were placed at schools that were also designated “welcoming schools”. These schools, then, were experiencing

a sizeable influx of new students, many of whom came from neighborhoods outside of the schools' original boundaries. It is possible that welcoming schools then would experience changes in crime as more students from adjacent neighborhoods travelled to school and as students navigated intermingling with students new to their school community. In fact, such a concern for heightened crime is part of what motivated the expansion of SP routes to these schools. To account for this analytically, models were also estimated in which an indicator for whether a school was serving as a welcoming school in 2013-14 was included in the quarter mile radii estimates.

### **Street Segment Analysis**

While the first analytic approach (quarter mile radii) informs SP's impact on general crime around schools, it fails to directly identify the impact of SP on crime on the routes themselves. To explore the extent to which SP influenced crime for the specific streets with SP workers, models were estimated in which the unit of analysis was street segments rather than areas around a school. In this analysis, each street segment on which SP workers were placed was coded by starting and ending street number. For example, if SP workers were assigned to the 1600 block of South Street, then one unit of analysis would be 1601-1699 South Street. To create a comparison group, parallel street segments on which SP workers were not placed were coded in a similar fashion.

Two sets of comparison street segments were identified. First, streets that were directly parallel to a SP route were identified and labeled as being one block over from a SP route. These parallel street segments represent a convincing counterfactual insofar as they are as close as possible to actual SP routes and would therefore be expected to be subject to similar trends in crime regardless of the presence of SP. Panel C of Figure 1 provides an example of the street

segment identification for parallel streets one block over. Though these streets provide a reasonable counterfactual, one limitation of using street segments one block away is that they may be subject to spill-over from SP. For example, a SP worker placed on a street corner on 45<sup>th</sup> street may be visible from 46<sup>th</sup> street and therefore influence crime rates on both streets. To account for this possibility, a second set of comparison street segments were identified by coding parallel streets that were two blocks away from the SP street segments. While potentially a bit less similar to SP routes, these street segments are theoretically less susceptible to spill-over effects and may therefore be a more justifiable counterfactual.

### **Sensitivity Analyses**

To establish the robustness of the findings, this study also reports estimates from a variety of sensitivity analyses. In addition to reporting impacts on total crime, this study also reports estimates of the relationship between SP and crime occurring between hours during which students would be travelling to or from school or would be in school (6AM to 6PM) as well as crimes during those hours that occurred outside, representing crime in the area and time that SP workers would be most able to influence. In addition to these alternative outcomes, estimates from models that include month by year fixed effects rather than a binary indicator of being in the 2013-14 school year were estimated. These models allow for a more flexible estimation of for non-linear time trends. Estimates from models aggregating crime to the month rather than day are also reported for the difference-in-differences models. Finally, in addition to estimating all of the primary and robustness check models with Poisson regression, this study reports results from models using ordinary least squares (OLS) regression.

### **Data**

The data used in this study came from several public data sources. Data on reported crimes came from the City of Chicago's Data Portal, which provides access to historical crime data (City of Chicago Data Portal, n.d.). Each reported crime is coded with date and time, contains a category for nature of offense, includes a block and street number, and is geocoded with latitude and longitude. For the purposes of this study, data were restricted to reported crimes occurring during the 2011-12 to 2013-2014 school years. This time period was chosen to correspond to the time period of the first major expansion of SP to elementary schools (2013-14) as well as to include two years of prior data to establish existing trends in crime.

Data on SP schools and street locations came from the CPS website in addition to the City of Chicago Data Portal (CPS, 2015; City of Chicago Data Portal, n.d.). The Data Portal offered GIS coded shape files while the CPS website offered visual maps of SP streets. These data were used to identify SP schools and street segments.

The unit of observation reflects one calendar day and further reflects either the quarter mile radius around a given school or, for street-level analyses, reflects a single street segment. In other words, the units reflect the number of crimes reported within a quarter mile of a given elementary school on a given day or the number of crimes reported on a given street segment on a given day. As such, the analytic sample varies depending on whether the analysis is at the school area level or at the street segment level. Additionally, the size of the analytic sample varies depending on whether comparisons include weekends (triple-difference) or not (difference-in-differences).

## **Results**

The findings suggest that schools with SP tend to have similar crime levels to those schools that implemented SP in a later year but that street segments with SP workers tend to have

higher reported crime on average than street segments both a block and two blocks over without SP workers. Furthermore, findings indicate that reported crime has dropped over time across both SP and non-SP schools and street segments. There is evidence that schools with SP routes experienced higher relative crime than comparison schools in 2013-14 but that much of this difference is explainable by the designation of many of these schools as welcoming schools. With regard to crime on SP street segments, there is suggestive evidence that SP reduced crime relative to street segments two blocks over, though this reduction is present on weekends as well as school days. Compared to street segments one block over, no effects are seen, possibly suggesting spill-over of SP effects to nearby streets. This section outlines the findings that support these claims.

#### ***Results for Crime around Schools (Quarter Mile Radii)***

Models comparing reported crime in the general vicinity (quarter mile radius) of elementary schools with SP routes to future SP elementary schools without such routes during the 2013-14 school year are presented first. As shown in Table 1, approximately 61% of the sampled schools (12% of all of the district's elementary schools) had Safe Passage routes during the 2013-2014 school year. In the two years prior to SP implementation, schools with SP routes tended to experience slightly more overall crime around their schools than schools without SP routes though the rates were practically quite similar. For instance, as shown in columns 4 and 5 of Panel A of Table 1, non-SP schools averaged 1.428 crimes per day within a quarter mile while SP schools averaged 1.535 in years prior to SP implementation. For crimes occurring between 6AM and 6PM, non-SP schools averaged 0.746 crimes per day within a quarter mile while SP schools averaged 0.813 crimes per day. The similarity of these crime rates in the period prior to

SP implementation suggests that the non-SP schools that later implemented SP are a reasonable comparison group for those implementing SP in the 2013-14 school year.

Both SP and non-SP schools experienced decreases in crime over time. Panel A of Figure 2 presents the average number of total crimes per day within a quarter mile from the 2011-2012 school year through the 2013-2014 school year. As expected, there is a cyclical pattern to the crime, with more crime occurring during the summer season; however, the average reported crime around all schools generally decreases over the time period. This is further shown in Table 1 in columns comparing crime during the 2013-2014 school year (columns 6 and 7) to the prior school years (columns 4 and 5). What is notable in Figure 2 is that, when linear approximations are graphed both before and after SP implementation for SP and non-SP schools, there is a visible decrease in crime rates for non-SP schools relative to SP schools after SP implementation. This indicates that, while crime around SP schools continued to decrease after SP implementation, it did not do so at the same rate as non-SP schools.

Table 2 shows results from both the difference-in-differences models (columns 1 and 3) and the triple-difference models (columns 2 and 4) estimated using Poisson regression. Panel A (columns 1 and 2) show models that do not account for the designation of many of the SP schools as welcoming schools while Panel B (columns 3 and 4) include a binary indicator of whether a school was acting as welcoming school. Each model also includes school area fixed-effects, implicitly controlling for any time-invariant aspects of the schools. In each model, the impact of the SP program is represented by the coefficient in the greyed box.

As shown, the results suggest that SP schools experienced higher relative crime than non-SP comparison schools in 2013-14 but that much of this difference is attributable to the designation of many of these schools as welcoming schools. In columns 1 and 2, which do not

account for the welcoming school status of schools, the coefficient on SP is positive and statistically significant. The magnitude, 0.06, indicates that crime rates were approximately 6% higher around SP schools than the comparison schools after taking into account all of the covariates in the models. This impact is seen in both the difference-in-differences (column 1) and triple-difference models (column 2).

This positive relationship between SP and crime appears to be explainable by the designation of many SP schools as welcoming schools. Columns 3 and 4 of Table 2 show results from models that include an indicator for being a welcoming school. In these models, the coefficient on SP is no longer positive and is statistically insignificant. In contrast, the coefficient on the welcoming schools indicator is positive. This suggests that the relatively higher rates of crime around SP schools is attributable to their status as welcoming schools and the influx of new and different students experienced as a consequence.

### ***Results for Crime on Street Segments***

The most direct test of SP's impact on crime is to look at the relationship between whether a specific street segment was a SP route and rates of crime. By comparing SP street segments to nearby street segments (in this case, one and two blocks away), other differences in the school and neighborhood, such as whether it was a welcoming school, are implicitly controlled for. Panel B of Figure 2 presents the average number of total crimes per day per street segment from the 2011-2012 school year through the 2013-2014 school year. Table 3 presents descriptive statistics on street segment reported crime, both for street segments with SP workers and for parallel street segments one and two streets away from SP routes. Figure 2 and Table 4 provide evidence that reported crime is higher on street segments with SP workers as compared

to street segments without SP workers. Likewise, crime is seen to decrease over time across both types of street segments.

While Panel B of Figure 2 shows decreases in crime rates for both SP and non-SP street segments, the linear estimates show some clear differences in levels of crime before and after SP implementation. In particular, for SP street segments (shown at the top in black), there is a visible discontinuity or decrease in crime rates from the period prior to until the period after SP implementation. A similar discontinuous drop in crime is visible for parallel street segments one street over (medium grey). Notably, the trend in crime rates for parallel street segments two streets over (light grey) shows little sign of a discontinuity. These trends suggest a decrease in crime on SP routes relative to street segments two blocks away and suggest that parallel street segments one block away may have experienced some spill-over from SP monitoring.

These visible trends are somewhat confirmed in the estimates from the difference-in-differences model but are less clear in the triple-difference model. Table 4 shows results from Poisson regression predicting crime on street segments for SP segments compared to parallel streets one block away (Panel A) and parallel streets two blocks away (Panel B). All of these models include street segment fixed effects. The estimates from models comparing to street segments one block away (columns 1 and 2), demonstrate statistically insignificant relationships between SP and crime rates (boxes shown in grey). In contrast, estimates from models comparing to street segments two blocks away show suggestive evidence of a decrease in crime. In particular, in the difference-in-difference model (column 3), SP predicts a marginally significant ( $p < 0.10$ ) negative relationship with crime. In the triple-difference model (column 4), SP predicts a statistically significant and large (-0.193) lower level of crime, though this effect is less pronounced on weekdays. The positive coefficient (0.136) on the triple difference may

indicate that SP made these street segments safer both on school days and on weekends but could also be evidence that the relationship in the difference-in-differences model was not fully accounting for a temporal trend in crimes. The results then, overall, are suggestive of a possible decrease in crime on street segments with SP workers with a spillover to streets one block away, but, given the marginal significance in the difference-in-differences model and the results of the triple-difference model, are not entirely conclusive.

### **Sensitivity Analyses**

To further probe the street segment results, a number of sensitivity analyses were conducted. As previously described, models were estimated using OLS, predicting crime between 6AM and 6PM, predicting crime at these times that occurred outdoors, substituting month-year fixed effects for the binary indicator of 2013-14, and aggregating data to the month level. For parsimony, results of these sensitivity analyses are shown in a condensed form in Table 5. This table replicates Table 4 insofar as it shows results from the difference-in-differences and triple-difference models for comparing to street segments one block away (Panel A) and two blocks away (Panel B). Rather than showing all coefficients, only the coefficient of interest (impact of SP) is presented. For difference-in-differences models, this reflects the interaction between SP and 2013-14 school year indicator while for the triple-difference models it reflects the triple interaction. For ease of comparison, the corresponding coefficients of interest from the primary models (from Tables 2 and 4) are repeated in the first row.

While there is some variability from model to model, the sensitivity analyses are generally supportive of the primary findings. In particular, estimates comparing to street segments one block away (columns 1 and 2) are consistently insignificant. The relationship between SP and crime in the difference-in-differences model comparing to street segments two

blocks away (column 3) is consistently negative and is often statistically significant at either the marginal ( $p < 0.10$ ) or traditional ( $p < 0.05$ ) levels. The estimate on the triple-difference (column 4) comparing to two blocks away is statistically insignificant in all cases except the primary model and that aggregated to the month-year. Perhaps most notable is the large relationship between SP and crime seen in the difference-in-differences model predicting outdoor crime from 6AM to 6PM. The estimate (-0.178) suggests a sizeable decrease in the rate of crime during the hours and locations which would be most likely to be impacted by the presence of the SP workers. As in the primary models, however, the lack of a negative relationship in the triple-difference model provides some cautionary interpretation to the difference-in-difference estimates, suggesting either that SP has reduced crime on these street segments during the weekend as well or that there were other temporal trends that are driving the results seen in the difference-in-differences model.

### **Summary**

In summary, the results provide mixed evidence on the impact of SP on crime. On the one hand, rates of crime in the general vicinity (quarter mile) of elementary schools were not statistically different from those around non-SP schools after accounting for the welcoming school status of schools. While there was suggestive evidence that SP may reduce crime on the actual streets on which SP monitors are placed and may contribute to positive spillovers for streets one block away, this relationship was as strong or stronger on weekends than on weekdays, a finding that suggests the possibility of other temporal trends contributing to the relationship. On the other hand, in the sensitivity analyses, models predicting street segment crime outdoors during day time hours, suggested a large impact on crime at the time and location

when SP would be expected to have an impact. In short, the evidence is suggestive of possible effects on street level crime, though not conclusive.

### **Discussion**

Policymakers and the media have made claims of the impact of SP on crime rates around schools and on SP routes in particular (Ali, 2015; CPS, 2014; 2015; Masterson, 2017). During the time of the 2013-14 expansion of SP, CPS stated that the SP program had contributed to a “20 percent decline in criminal incidents around Safe Passage schools” (CPS, 2015). This claim cited unpublished analysis by the Chicago police department and gave no indication of the rigor of the analysis. The results of this study suggest that such claims may be inflated relative to any observable impact from the 2013-14 expansion but that the SP program may have reduced crime on SP street segments.

In particular, the results of this study suggest that rates of crime around SP schools (defined as a quarter mile radius) generally did not decline as a result of the program. While there is evidence that crime may have decreased on particular street segments as a result of SP, the magnitude of this decrease was generally smaller than that suggested by CPS. There are several possible reasons why SP’s impact on crime may not be more pronounced. First, the deterrent effect of SP workers may be minimal given that they are not law enforcement officers and that their primary purpose is to assist with students’ transit to school rather than police the communities. As such, perpetrators of crime may view them in little different light than a parent or other adult that is assisting with student transit to school.

A second possibility is that SP workers have a larger impact on crime but that the impact is mitigated by an increased likelihood for crime to be reported to law enforcement. A limitation of this study and many studies that examine crime outcomes is that they rely on measures of

crime reported to law enforcement rather than actual crime committed. It is possible that the impact of SP on crime is attenuated due to SP workers increasing the likelihood that a crime is reported to law enforcement. Given their role in promoting safety, SP workers may be identifying and reporting incidents that otherwise would have gone unreported. If a decrease in actual crime is counterbalanced by an increase in the probability that a crime is reported, the impact of SP on actual crime could be driven towards zero resulting in a smaller observed impact of SP. As a result, the suggestive evidence that SP reduces crime seen in the street segment analysis or the null results in the quarter mile analysis might be thought of as a lower bound on SP's true impact on crime.

The SP program remains a relatively costly expenditure and would be so for any other school district or city considering the implementation of a similar program. It is relevant then to consider the potential costs of the program relative to the potential benefits of the program in terms of crime reduction. Criminal activity has been shown to have high costs for society due to the costs of law enforcement, the court system, and incarceration (Henrichson & Delaney, 2012; McCollister, French, & Fang, 2010; Wickramasekera, Wright, Elsey, Murray, & Tubeuf, 2015). If SP reduces crime, then the program may contribute to considerable economic benefits through reduced crime related costs.

Taking the estimate from the difference-in-difference street segment comparison to street segments two blocks over, the result suggests an approximately 6% reduction in reported crime per day per street segment. Taking the pre-SP average of 0.12 crimes per SP street segment per day and the presence of 184 SP street segments in 2013-14, a back of the envelope calculation would suggest that about 1.3 fewer crimes ( $0.12 * 0.06 * 184$ ) would be committed per day on SP street segments or 6.5 fewer crimes per week. At the staffing levels present in 2013-14, SP

roughly cost \$150,000 dollars per week to staff meaning that each crime prevented would need to save approximately 23,000 dollars to fully support the cost of the program. While estimates of the cost of crime vary significantly in the literature, it is possible for reduced crime to outweigh this cost. For instance, estimates of the cost of murder, rape, sexual assault, and robbery all exceed the 23,000 dollar figure, in some cases by orders of magnitude (McCollister, French, & Fang, 2011).

In addition to any impacts on crime, it is worth noting that the primary purpose of SP was not to reduce general rates of crime. Rather, SP was established for the purpose of improving the safety of students as they travel to and from school. It is possible that, while evidence on its impact on crime is mixed, SP nevertheless reduces students' exposure to victimization. For instance, SP workers may reduce incidents for students such as bullying or fights that would not necessarily result in a criminal report with the police. Furthermore, even if crime is not reduced and regardless of whether non-criminal victimization is reduced, students may nevertheless feel safer due to the presence of an adult school official on their route to school. Prior research suggests that if students feel safer traveling to school then they may be more likely to attend school and potentially less likely to bring weapons to schools (Bowen & Bowen, 1999; Everett & Price, 1995). Indeed, some recent qualitative work finds that families choosing schools during the time of the Chicago closures viewed SP routes favorably, despite still having concerns about the safety of students (Torre et al., 2015). To fully assess the merits of the SP program, then, requires further exploration of students' feelings of safety, student attendance, incidents of non-criminal victimization such as bullying, and other school-based outcomes. With information on the impacts on a wider set of outcomes, policymakers should then rigorously evaluate the costs

and benefits associated with the use of the program. Nevertheless, the results presented here suggest that it is not unreasonable to assume that SP may be a cost-effective program.

### **Limitations**

As with any study, this analysis has its limitations. First, as previously noted, the study relies on reported rates of crime rather than actual measures of crime. While the data limits analysis to reports of crime, it is recognized that such reports could escalate as a result of the placement of capable guardians on the streets. In other words, estimates of the true effect on crime could be biased upward due to greater reporting of crime. This is a limitation of most studies of criminal activity and may be thought to contribute to a lower bound on estimates of crime reduction in this study.

A second limitation pertains to the time period studied. While the large expansion of SP in 2013-14 provided an opportunity to examine the impact of the program, it is worth repeating that this expansion was restricted to primary schools (K-8). The impact of SP workers may be differential around primary schools as compared to high schools, particularly if the crime diverted arises from students themselves. In virtue of their age, high schoolers are significantly more likely to be involved in criminal activity than elementary aged students, so it is possible that analyses of SP's impact around high schools would result in larger estimates, as suggested by the unpublished Chicago police analysis cited by the school district (CPS, 2015).

Next, it is important to note the limitations to the generalizability of the findings. Chicago is a unique city, both in terms of its size as well as its rates of crime, which are among the highest in the nation. Like some other large cities, many students commute to school by walking or taking public transportation, rather than using transit provided by the school district. Additionally, within this context, SP routes were placed at schools with recognizable concerns

around safety, such as higher rates of crime. While the results presented here might be expected to generalize to schools in similar neighborhoods in other large urban areas, they may not generalize to contexts where crime is lower or where students commute to school through different modes of transit. Consequently, caution should be taken in extrapolating these results to other contexts.

This point is particularly important given ongoing efforts by CPS to expand the SP program. As of 2018, the district had announced plans to expand to a total of 159 schools, almost three times as many as those included at the time of this analysis (CPS, 2018). As such expansions are considered, care should be given to consider the context and needs of particular communities for such an intervention.

Finally, it is worth noting the remaining threats to the internal validity of this study. SP was not randomly assigned to schools or to street segments. While this study made use of a variety of strategies to address selection bias, it is nevertheless possible that there are remaining differences in schools or streets with SP and those without. While the analytic approach is strong, it cannot preclude the possibility that some unidentified time-varying change of a neighborhood or street segment also contributed to differential rates of crime. That reductions in crime were not consistently observed in the triple-difference model suggests this is a possibility.

### **Conclusion**

The issue of student safety represents a prominent concern for parents, educators, and policymakers. Providing safe environments during the commute to and from school is a necessary prerequisite for student learning and development. This paper has addressed Safe Passage, a large scale intervention aimed at improving student safety during the commute to and

from school, and has empirically tested claims from the district and the media regarding the impact of the program on general crime rates around SP schools.

The study has provided some suggestive evidence that SP may reduce crime on particular street segments though the impact is not consistently seen across all specifications and does not appear to translate to reduced crime in the general vicinity of a school. Nevertheless, it seems possible that SP may be a justifiable investment, particularly if other analyses can establish that it has positive impacts on student outcomes like attendance or achievement. To this end, this paper has outlined a series of further outcomes that policymakers and practitioners should evaluate as further expansions of the SP program are considered.

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Table 1: Means and standard errors of independent and dependent variables by Safe Passage school status for quarter mile radii comparisons

	Full Sample	Overall		Prior School Years		2013-2014 School Year	
		Not Safe Passage School	Safe Passage School	Not Safe Passage School	Safe Passage School	Not Safe Passage School	Safe Passage School
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Safe Passage School	0.616 (0.002)	0.000 (.)	1.000 (.)	0.000 (.)	1.000 (.)	0.000 (.)	1.000 (.)
2013-2014 School Year	0.333 (0.002)	0.333 (0.003)	0.333 (0.002)	0.000 (.)	0.000 (.)	1.000 (.)	1.000 (.)
Week Day	0.674 (0.002)	0.674 (0.003)	0.675 (0.002)	0.666 (0.004)	0.668 (0.003)	0.688 (0.005)	0.688 (0.004)
Welcoming School	0.182 (0.001)	0 (.)	0.296 (0.002)	0 (.)	0 (.)	0 (.)	0.887 (0.003)
Total crime within a quarter mile (#)	1.424 (0.006)	1.350 (0.009)	1.470 (0.008)	1.428 (0.011)	1.535 (0.009)	1.193 (0.014)	1.340 (0.013)
Crime between 6AM and 6PM within a quarter mile (#)	0.753 (0.004)	0.707 (0.006)	0.781 (0.005)	0.746 (0.007)	0.813 (0.006)	0.629 (0.009)	0.717 (0.008)
Outside crime between 6AM and 6PM within a quarter mile (#)	0.344 (0.003)	0.308 (0.004)	0.367 (0.003)	0.332 (0.005)	0.385 (0.004)	0.259 (0.006)	0.330 (0.006)
n	67,878	26,073	41,805	17,394	27,866	8,679	13,939

Note. Standard errors in parentheses are clustered by school.

Table 2. Coefficients and standard errors from Poisson regressions predicting count of total crimes within quarter mile of schools from Safe Passage indicators with and without indicators for welcoming schools

	Panel A: Quarter Mile Radii Without Welcoming School Indicator		Panel B: Quarter Mile Radii with Welcoming School Indicator	
	(1)	(2)	(3)	(4)
2013-2014 School Year	-0.197** (0.0141)	-0.139** (0.0208)	-0.197** (0.0141)	-0.139** (0.0208)
Safe Passage * 2013-2014 School Year	0.0639** (0.0176)	-0.00546 (0.0264)	-0.0606 (0.0413)	-0.0801 (0.0581)
Weekday		0.0571** (0.0136)		0.0571** (0.0136)
Safe Passage * Weekday		0.0555** (0.0171)		0.0554** (0.0171)
2013-2014 School Year * Weekday		-0.0587* (0.0252)		-0.0587* (0.0252)
SP * 2013-2014 School Year * Weekday		0.0693* (0.0317)		0.0326 (0.0664)
Welcoming School in 2013-14			0.134** (0.0403)	0.0807 (0.0558)
Welcoming School in 2013-14 * Weekday				0.0395 (0.0628)
School Area Fixed Effects	Yes	Yes	Yes	Yes
Observations	45,778	67,878	45,778	67,878

Note. Standard errors in parentheses are clustered by school.; \*\* p<0.01, \* p<0.05, + p<0.1

Table 3: Means and standard errors of independent and dependent variables by SP and non-SP street segments

	Full Sample	Prior School Years			2013-2014 School Year		
		1 Street Over Street Segment	2 Streets Over Street Segments	Safe Passage Street Segment	1 Street Over Street Segment	2 Streets Over Street Segments	Safe Passage Street Segment
Safe Passage Street Segment	0.242 (0.001)	0.000 (.)	0.000 (.)	1.000 (.)	0.000 (.)	0.000 (.)	1.000 (.)
2013-2014 School Year	0.333 (0.001)	0.000 (.)	0.000 (.)	0.000 (.)	1.000 (.)	1.000 (.)	1.000 (.)
Week Day	0.675 (0.001)	0.668 (0.001)	0.668 (0.001)	0.668 (0.002)	0.688 (0.002)	0.688 (0.002)	0.688 (0.002)
Total crime on street segment (#)	0.0826 (0.000)	0.0791 (0.001)	0.0683 (0.001)	0.129 (0.001)	0.0628 (0.001)	0.0623 (0.001)	0.107 (0.002)
Crime between 6AM and 6PM on street segment (#)	0.0441 (0.000)	0.0414 (0.001)	0.0366 (0.001)	0.0693 (0.001)	0.0335 (0.001)	0.0336 (0.001)	0.0581 (0.001)
Outside crime between 6AM and 6PM on street segment (#)	0.0200 (0.000)	0.0202 (0.000)	0.0160 (0.000)	0.0306 (0.001)	0.0152 (0.000)	0.0155 (0.001)	0.0247 (0.001)
n	582,822	151,407	142,970	94,088	75,744	71,536	47,077

Note. Standard errors in parentheses are clustered by street segment.

Table 4. Coefficients and standard errors from Poisson regressions predicting count of total crimes on street segments from SP indicators compared to parallel street segments 1 and 2 streets over

	Panel A: SP Compared to Parallel Street Segments 1 Street Over		Panel B: SP Compared to Parallel Street Segments 2 Streets Over	
	(1)	(2)	(3)	(4)
2013-2014 School Year	-0.216** (0.0205)	-0.263** (0.0315)	-0.115** (0.0215)	-0.0411 (0.0333)
Safe Passage * 2013-2014 School Year	0.0445 (0.0284)	0.0290 (0.0447)	-0.0569+ (0.0292)	-0.193** (0.0461)
Weekday		0.0619** (0.0196)		0.166** (0.0221)
Safe Passage * Weekday		0.0851** (0.0278)		-0.0185 (0.0297)
2013-2014 School Year * Weekday		0.0468 (0.0375)		-0.0736+ (0.0397)
SP * 2013-2014 School Year * Weekday		0.0155 (0.0530)		0.136* (0.0545)
Street Segment Fixed Effects	Yes	Yes	Yes	Yes
n	214,534	318,726	199,092	298,251

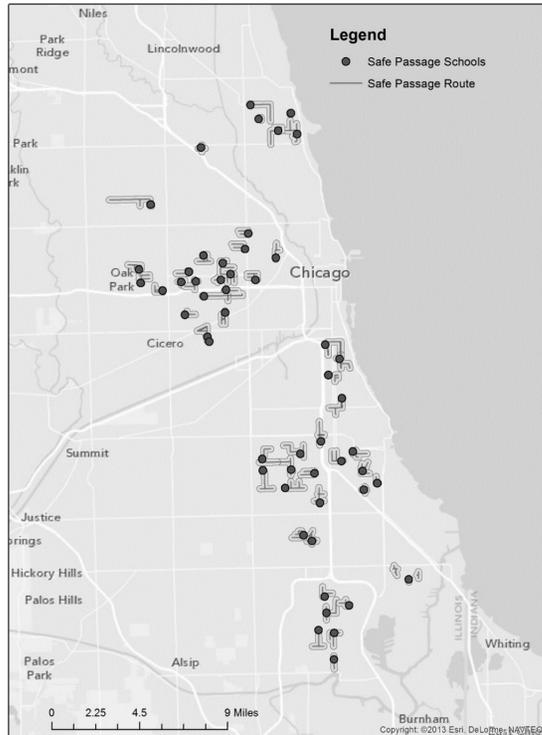
Note. Standard errors in parentheses are clustered by street segment.; \*\* p<0.01, \* p<0.05, + p<0.1

Table 5. Coefficients and standard errors from regressions predicting count of total reported crimes on street segments comparing to summer prior to implementation

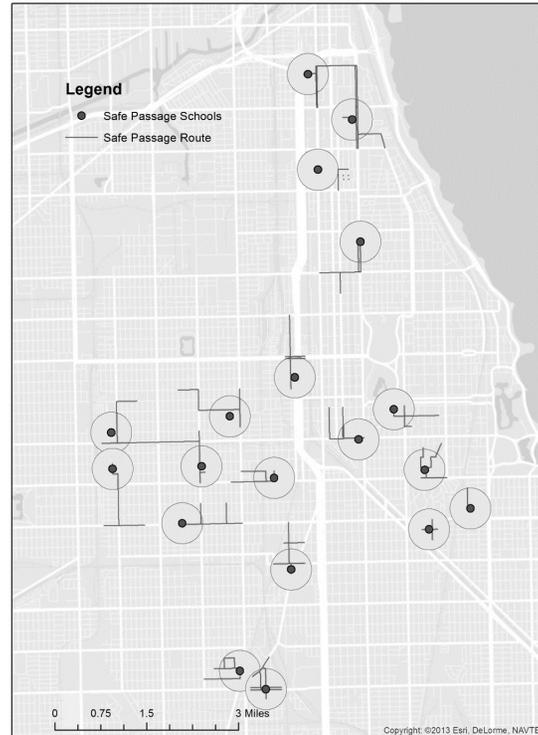
	Panel A:		Panel B:	
	SP Compared to Parallel Street Segments 1 Street Over		SP Compared to Parallel Street Segments 2 Streets Over	
	DiD (1)	TD (2)	DiD (3)	TD (4)
Primary models	0.0445 (0.0284)	0.0155 (0.0530)	-0.0569+ (0.0292)	0.136* (0.0545)
OLS	-0.00568 (0.00538)	0.00113 (0.00543)	-0.0135* (0.00546)	0.00837 (0.00559)
6AM to 6PM Crime	0.0423 (0.0382)	0.0374 (0.0743)	-0.0621 (0.0390)	0.114 (0.0767)
Outdoor Crime from 6AM to 6PM	0.0742 (0.0577)	0.0147 (0.109)	-0.178** (0.0596)	0.00682 (0.112)
Month-Year Fixed Effects	0.0445 (0.0284)	0.0155 (0.0530)	-0.0569+ (0.0292)	0.136* (0.0545)
Aggregated to month	0.0432 (0.0284)		-0.0604* (0.0292)	

Note. Standard errors in parentheses are clustered by street segment.; Sample sizes vary by robustness check; Each cell represents the coefficient of interest from a different regression specification; DiD = difference-in-differences and TD = triple difference; \*\* p<0.01, \* p<0.05, + p<0.1

Panel A: Elementary Schools and Safe Passage Routes



Panel B: SP Routes and Quarter Mile Radii for Select Schools



Panel C: SP Routes and Adjacent Street Segments for Select Schools

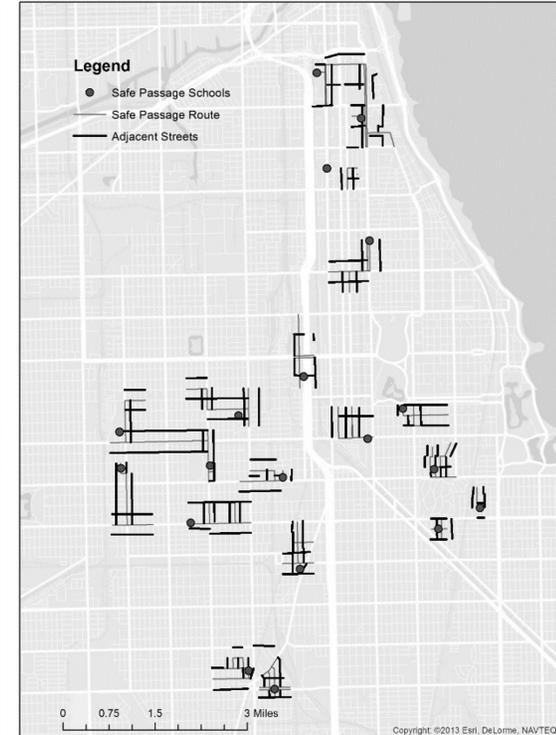


Figure 1. Maps showing 2013-14 Safe Passage schools and routes (panel A) as well as example quarter mile radii (panel B) and adjacent street segments (panel C)

Note. Base maps provided by Esri. "World Street Map". Scale Not Given. January, 2014. <http://www.arcgis.com/>. (January 1st, 2014).

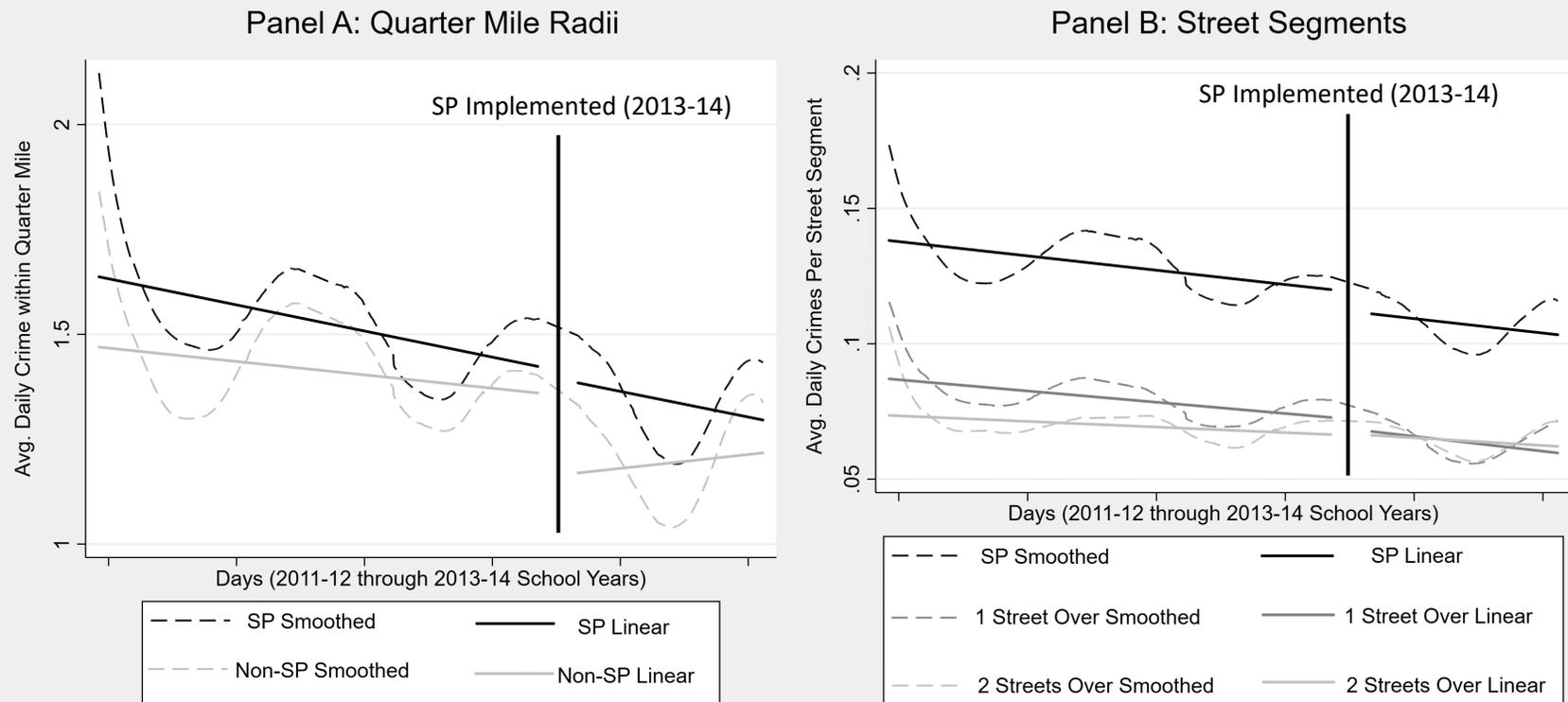


Figure 2. Daily crime averages for SP and non-SP schools for quarter mile radii (panel A) and street segments (panel B) from 2011-12 through the 2013-14 school years