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Can Community Crime Monitoring Reduce Student Absenteeism?

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In this paper we study the impact on student absenteeism of a large school-based community crime monitoring program that employed local community members to monitor and report crime on designated city blocks during students' travel to and from school. We find that the program resulted in a 0.78 percentage point reduction in the school-level absence rate (11 percent effect). We explore two potential channels to explain this: we find improvements "outside of the school walls" in the form of reduced crime near treated schools and "inside of the school walls" in the form of reduced incidents of serious student misconduct.

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Can Community Crime Monitoring Reduce Student Absenteeism? *

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Abstract

In this paper we study the impact on student absenteeism of a large school-based community crime monitoring program that employed local community members to monitor and report crime on designated city blocks during students' travel to and from school. We find that the program resulted in a 0.78 percentage point reduction in the school-level absence rate (11 percent effect). We explore two potential channels to explain this: we find improvements "outside of the school walls" in the form of reduced crime near treated schools and "inside of the school walls" in the form of reduced incidents of serious student misconduct.

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1 Introduction

Exposure to community and neighborhood violence is a significant problem in the United States (U.S.), where in a nationally-representative survey of children, 18.4 percent report witnessing an assault during the past year (Finkelhor et al., 2015). This percent rises to 27.7 for lifetime exposure to assault among children of all ages and increases to 57.9 percent for lifetime exposure to assault among 14-17 year-old children (Finkelhor et al., 2015). These rates of exposure to violence are particularly troubling due to the growing evidence on the effects of exposure to neighborhood and community violence on student outcomes. An extensive literature documents the correlation between exposure to neighborhood crime and violence and negative child outcomes (Osofsky, 1995, 1999; Margolin and Gordis, 2000; Buka et al., 2001), and a growing literature in education provides strong evidence to suggest that the relationship is indeed causal. Recent work has demonstrated the negative impact of acute exposure to localized violence on student achievement and other education outcomes and has begun to explore the individual- and community-level mechanisms underlying these effects (Sharkey, 2010; Sharkey et al., 2012, 2014; McCoy et al., 2015; Schwartz et al., 2016; Laurito et al., 2019).

In addition to the documented prevalence of exposure to community and neighborhood violence, the percentage of students in the U.S. who report missing school due to concerns about their personal safety has risen steadily over the past two decades. Recent results from a nationally-representative survey of students reveal that 6.7 percent of students report that they did not attend school on at least one day in the 30 days preceding the survey due to the belief that they would be unsafe at school or on their way to or from school (Kann et al., 2018).[1] This represents a 52 percent increase relative to students' responses to the same question in the early-1990s (Kann et al., 2018). Somewhat puzzlingly, this increase in the share of students who report being absent due to fears about safety occurred during a period when crime rates fell dramatically and school safety generally improved (James, 2018; Musu-Gillette et al., 2018). Failure to attend school due to concerns about safety is troubling from an education policy perspective because of the well-documented relationship between student attendance and achievement (Sims, 2008; Fitzpatrick et al., 2011; Aucejo and Romano, 2016).

Despite the accumulating evidence in these two areas, relatively little work has investigated effective policy interventions designed to limit or prevent exposure to crime and violence in community and neighborhood settings. Although there is growing use of and increasing research evidence on school-based practices that mitigate the negative impacts of exposure to violence after the fact (e.g., trauma-informed teaching) (Overstreet and Chafouleas, 2016; Powell and Bui, 2016; Dorado et al., 2016), less work has been devoted to

[1] Unfortunately the survey questions in the Youth Risk Behavior Surveillance System (YRBSS) are not detailed enough to separate out the proportion of students who are absent due to concerns about safety at school versus travel to and from.

understanding effective policies to limit or prevent exposure to crime and violence in the first place.

At the same time, much of the recent work on effective strategies to reduce student absenteeism has focused on providing information and outreach to students and parents. One set of papers in the emerging literature investigates the effectiveness of interventions that cultivate supportive relationships with teachers, mentors, or other school staff (Guryan et al., 2016; Cook et al., 2017; Smythe-Leistico and Page, 2018, 2019). These strategies, while promising, are time- and resource-intensive. They often require the sustained efforts of trained school personnel, access to high-frequency data on student absences, and a time-horizon long enough to allow for the development of trusting relationships. Other recent work studies the effects of low-cost information treatments (e.g., personalized mailings) targeting parents' inaccurate beliefs about their child's absences (Rogers and Feller, 2018; Robinson et al., 2018). These are also quite promising and are much lower in cost. But despite the growing research evidence in these two areas, very little work focuses on how to address community-wide factors that contribute to high rates of absenteeism.

We address these substantive gaps in the literature by investigating the effects of a community crime monitoring intervention on student absenteeism. This unique intervention, implemented at-scale, assigned community monitors – unarmed adults employed by neighborhood-based, non-profit organizations – to keep watch on specific city blocks around designated public schools in Chicago. These community monitors served as a physical presence and performed basic surveillance and crime reporting tasks on assigned blocks during student arrival and dismissal times. Formally called the Safe Passage Program (SPP), the community monitors who carried out these crime monitoring tasks around public schools were neither trained police nor school personnel. They received annual training on general topics (e.g., first aid, CPR), wore bright neon vests while on duty, and were equipped with basic communication technologies (e.g., cellphone or two-way radio).

In this paper we estimate the causal impact of the SPP on absences using difference-in-differences and event-study approaches that exploit the staggered rollout of the SPP across public elementary schools in Chicago. We find that the SPP decreased school-level rates of student absences by around 0.78 percentage points, which is a 11 percent decrease relative to the baseline mean. In practical terms, this translates into around 696 additional student attendance-days for the average-sized elementary school in CPS, or around 1.4 additional attendance-days per student per year. This effect is similar in magnitude to other estimates in the absenteeism intervention literature that come from programs designed to reduce absences through the provision of information and intensive individualized supports (Guryan et al., 2016; Cook et al., 2017). Given the limited scope for interaction between community monitors and students, our paper adds to this literature by shedding light on an alternative policy approach to reducing absenteeism that addresses the broader neighborhood and community context in which schools operate.

In addition to our findings for absenteeism overall, we also find some evidence of heterogenous impacts when we examine the effects of the SPP on absence rates for student subgroups. Point estimates for the effect of the SPP on absences among black, low-income, and disabled students are similar to the overall effect, but the point estimate for Hispanic students is considerably larger. In relative terms these subgroup effects translate into a 10.5 percent decrease in absences among black students, a 16.5 percent decrease among Hispanic students, a 11.8 percent decrease among low-income students, and a 9.1 percent decrease among disabled students, respectively. This pattern of results among student subgroups is consistent with other findings in related literature on the effects of exposure to crime and violence on student outcomes, a point we return to in the discussion of subgroup heterogeneity.

To gain insight into the specific mechanisms through which the SPP decreased rates of student absences, we undertake an in-depth exploration of two channels that are suggested by previous literature: the neighborhood context ("outside the school walls") and the school context ("inside the school walls"). To investigate the relative importance of these channels empirically, we examine the impacts of the SPP on a host of intervening variables related to each of these two contexts. Our findings suggest that both channels contribute to reducing student absenteeism. Specifically, we find evidence that the SPP led to improvements "outside of the school walls" in the form of reduced crime rates near treated schools and to improvements "inside of the school walls" in the form of reduced incidents of serious student misconduct. Importantly, we find that these improvements in the school context were not accompanied by increases in rates of police involvement at SPP schools, a policy-relevant finding that lends support to the notion that community monitoring can improve school outcomes without increasing police presence in schools.

Despite the existence of a robust and growing literature on the effects of the SPP on crime, which we summarize in Section 2.2, very little is known about the effects of the SPP on student outcomes. The single exception in the existing literature is McMillen et al. (2019), who devote one table to reporting estimated effects of the SPP on *changes* in attendance rates. They find that changes in attendance rates were larger in SPP schools relative to propensity-score matched controls and that this effect was primarily driven by high schools in the program.[2] Our paper adds to this lone finding in three ways. First, we present new evidence on the effects of the SPP on absences at the elementary school level. In contrast to findings reported in McMillen et al. (2019), we find significant evidence of large effects of the SPP on absenteeism in treated elementary schools. Second, our focused investigation at the elementary school level provides insight into broader questions about the effects of exposure to violence on student outcomes that are obscured in analysis that combines elementary and high schools together. Estimates that include high schools cannot disentangle

[2]See Column(4) of Table 11 of McMillen et al. (2019). Due to the choice of dependent variable (*changes* in attendance rates) we are unable to directly compare these effects to our estimates. We are also unable to translate this effect into relative terms, since the authors report baseline attendance rates in levels.

whether results are explained by reductions in neighborhood exposure to crime or individual-level crime. The elementary school context is less likely to suffer from this confounding and thus provides a cleaner setting in which to investigate the impacts of community monitoring on absenteeism that flows through reduced exposure to neighborhood crime. Finally, our context of elementary schools is less likely to be contaminated by time-varying, school-level policies that coincided with the SPP at the high school level. Nearly coincident with the SPP, CPS rolled out the "Culture of Calm" Initiative in CPS high schools to improve student attendance and school climate (Stevens et al., 2015). The elementary context should be unaffected by these policies and therefore provide estimates that capture the effect of the SPP and not other time-varying policies that were correlated with SPP implementation.

Our empirical evidence on the effects of the SPP on student absenteeism and our exploration of the potential channels through which this program operates provide broad insight into the far-reaching effects of exposure to crime and violence on student outcomes. This work contributes to the growing recognition that exposure to crime and violence negatively impacts student performance in schools and highlights the potential to address this issue by intervening using a preventive and relatively inexpensive strategy. This paper also adds a new dimension to the growing literature on interventions designed to reduce student absenteeism by investigating the effects of a previously unexplored policy lever. Community monitoring offers the potential to address one of the underlying community and neighborhood determinants of absenteeism by reducing crime near schools. This approach to addressing absenteeism is particularly important in light of growing attention under the Every Student Succeeds Act (ESSA) to the issue of chronic absenteeism and to the role that this measure will increasingly play in school accountability systems. As a growing number of states incorporate measures of chronic absenteeism into their plans for school accountability, education policymakers will undoubtedly be searching for effective community- and school-based strategies to address this issue.

2 Context

2.1 Chicago Public Schools and the Safe Passage Program

Chicago Public Schools (CPS) is the third largest school district in the U.S., with current enrollment estimated to be around 371,000 students (Chicago Public Schools, 2018). Like many other large, urban districts, CPS serves a large number of racial and ethnic minority and economically-disadvantaged students. Recent estimates indicate that the district is 37 percent African-American, 47 percent Hispanic, and 10 percent white, and that around 78 percent of students in the district are economically-disadvantaged (Chicago Public Schools, 2018). In 2017/18, the district's operating budget was estimated to be around $5.7 billion

(Chicago Public Schools, 2018).

Students in CPS report experiences of crime and violence at school at rates that are higher than national averages. In a survey from 2013, 16.9 percent of CPS high school students reported being in a physical fight at school (versus 8.1 percent nationally), 9.1 percent reported being threatened or injured with a weapon at school (versus 6.9 percent nationally), and 12.9 percent reported missing at least one school day – within the past 30 – due to concerns about their own safety (versus 7.1 percent nationally) (Kann et al., 2014).[3] Detailed surveys of CPS teachers and students confirm these findings and demonstrate the primacy of neighborhood safety as a concern among students and teachers. Relative to other schools in the state of Illinois, CPS teachers and students regularly express high levels of concern about safety, trust, and support in their local communities and neighborhoods, despite favorable responses about school leadership, teacher effectiveness, and curricular instruction in the classroom (Klugman et al., 2015).

CPS introduced the SPP in response to a series of violent incidents that directly affected CPS students. These violent incidents received widespread media attention and prompted district action in response to student and parent concerns about safety (Davey, 2013; Zubrzycki, 2013). The SPP is a community-based crime monitoring intervention that utilizes paid, adult civilians employees who work for local non-profit organizations (henceforth "community monitors"). At designated SPP schools, community monitors provide the guarantee of supervision and an adult's presence on established routes (city blocks around the school) during arrival and dismissal times on regular attendance days during the academic year. Community monitors wear neon vests that identify them as SPP workers and are present on routes for between 2-3 hours each morning (arrival time) and for between 2-3 hours each evening (dismissal time). The exact times of coverage are determined by each school's bell schedule.

The SPP officially began at 26 CPS high schools in the 2010/11 school year, although in this paper we focus exclusively on the effects of the SPP in CPS elementary schools.[4] Figure 1 documents the rollout of the SPP across CPS elementary schools over the course of the 2013/14-2016/17 school years. Panel (a) depicts the cumulative number of elementary schools that were covered by the SPP. The program started with 53 elementary schools in the 2013/14 school years and was subsequently expanded in each subsequent year to a total of 84 schools in the 2016/17 school year. Panel (b) depicts the share and number of CPS elementary students who attended schools with SPP coverage during the same time period. The small declines in the number of participating schools, the share of CPS elementary students who attended SPP schools, and the

[3] Chicago Public Schools is one of the large urban school districts in the U.S. that participates in the Youth Risk Behavior Surveillance (YRBS) survey. The questions about physical fighting and threats/injuries with a weapon have a 12-month recall period, while the question about missing school has a 30-day recall period. The sample of respondents includes only high school students.

[4] In this paper, we study the effects of the SPP on elementary schools in CPS. We do this because we have limited data on absences at the high school level and because we do not have any "pre"-SPP data on reported student misconduct or suspensions for high schools.

number of CPS elementary students who attended SPP schools between the 2015/16 and 2016/17 school years is due to the closure of Marshall Middle School, which was designated as a SPP school starting in the 2013/14 school year. Aside from this closure, all other treated schools remained in the program.

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.[5] Despite this centralized funding and oversight by the district, however, Safe Passage community 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 are paid $10 per hour for between five and six hours of work each day. Safe Passage community monitors receive standardized training at the beginning of the school year from CPS on topics such as first aid, CPR, and conflict de-escalation. CPS provides standardized policies and procedures to all Safe Passage community monitors, which cover topics such as monitoring and reporting criminal activity and communicating with school officials (Zubrzycki, 2013). Safe Passage community monitors are unarmed but are issued CPS-provided cellular telephones or two-way radios (Zubrzycki, 2013).

During the period we study, CPS carried out one of the largest mass school closings in history. At the end of the 2012/13 school year, CPS closed 47 elementary schools on the South and West sides of Chicago, resulting in re-assignment to new schools for around 12,000 students (Ahmed-Ullah and Secter, 2013; Gordon et al., 2018). District officials and Chicago Mayor Rahm Emanuel cited low enrollments and a district-wide budget deficit as the main reasons for the closings and projected that the closings would save CPS $560 million over 10 years (Yaccino and Rich, 2013).

In the school year following the mass closings, CPS designated 46 other elementary schools in the district as Welcoming Schools, which were the intended recipient schools for students affected by mass closings (Gordon et al., 2018). These schools were given new technology, facility renovations, and additional discretionary funds (Gordon et al., 2018). The first year in which the SPP was expanded to CPS elementary schools – 2013/14 – followed these mass closings and overlapped considerably with the designation of Welcoming Schools. In 2013/14, 46 out of the 53 SPP schools were also Welcoming Schools. In subsequent years, the SPP was expanded to other elementary schools that were not designated as Welcoming Schools. In the

[5] Authors' calculations. We obtained information on annual Safe Passage expenditures from the Chicago Public Schools (CPS) using a Freedom of Information Act (FOIA) request.

analysis that follows, we use several empirical approaches to separate out the school-level impacts of being designated as a Welcoming School from the effects of the SPP.

2.2 Previous Work

Existing work on the effects of the SPP on crime is considerably nuanced and includes papers by Curran (2018), Gonzalez and Komisarow (2019), McMillen et al. (2019) and Sanfelice (2019). Of these existing papers, only Curran (2018) reports results in which schools are the unit of analysis, which is the approach we take later in the paper when we investigate crime reduction as a potential mechanism through which the SPP reduces absenteeism. In this early evaluation, Curran (2018) leveraged the expansion of the SPP in a single school year (2013/14) to estimate the effect of the SPP on crime within one-quarter mile of treated elementary schools. Using treated schools from this single year compared to schools treated later, Curran (2018) found that the relative difference in before-and-after comparisons across these two groups was statistically insignificant or actually positive, indicating that crime was higher in treated schools following the SPP. This finding is likely explained by his use of the single year that coincided with the aftermath of mass school closings in CPS and the designation of Welcoming Schools. We address these concerns by using more years of the program rollout and by estimating our models with and without Welcoming Schools.

Although Curran (2018) did not find evidence of reduced crime using the school-based approach, his analysis of individual treated street segments did yield evidence of localized crime reductions relative to nearby street segments without the SPP. Building on this street segment approach, Gonzalez and Komisarow (2019), McMillen et al. (2019) and Sanfelice (2019) all implemented slightly different identification strategies and similarly found robust evidence of reduced crime. Aggregating city blocks into larger cells, McMillen et al. (2019) report reduced violent crime (14 percent) in treated cells compared to nearby control cells following the introduction of the SPP. Likely due to aggregation of city blocks into these larger cells, however, this paper did not detect evidence of crime displacement into nearby areas, which is reported in both Gonzalez and Komisarow (2019) and Sanfelice (2019). Although both of these latter papers examine the effect of the SPP on a wide variety of crime outcomes and report evidence of reduced violent, property, and non-index crime (Gonzalez and Komisarow, 2019) or sub-categories thereof (Sanfelice, 2019), they also found evidence of spatial displacement – albeit small in magnitude – of property and non-index crimes (Gonzalez and Komisarow, 2019) and total crime, theft, and criminal damage (Sanfelice, 2019).

Aside from contributing to the existing SPP literature on crime, this paper also contributes to the broader literature on the relationship between exposure to crime and violence on student outcomes, and nascent literature in education on effective strategies to reduce absenteeism. Early empirical work on the relationship between exposure to localized violence and student outcomes documented a robust negative correlation and highlighted the challenge in distinguishing these effects from other dimensions of childhood disadvantage, such as poverty, parental education, and violence in schools (Grogger, 1997; Aizer, 2008). More recent papers have built on this work by utilizing causal designs to refine estimates of the reduced-form relationship and delve into the underlying mechanisms. Gershenson and Tekin (2018) exploit variation in school-level exposure to the Beltway Sniper attacks that occurred in the Washington D.C.-area in 2002. They find that close proximity to one or more of the attacks associated with this weeks-long random shooting spree resulted in lower school-level proficiency rates on mathematics achievement exams. They also find weaker but suggestive evidence of negative effects on proficiency rates for reading achievement exams. Given that the attacks involved shootings at multiple locations, the authors were able to estimate dosage models to probe whether effects were larger in schools where students were exposed with violence more intensely. They found that effects were indeed larger (i.e., more negative) in schools that were in close proximity to two (versus one or zero) attacks.

Related work by Gershenson and Hayes (2018) examines the effect of exposure to civic unrest from the police shooting of an unarmed black teenager in Ferguson, Missouri. They find that math and reading test scores in exposed schools declined relative to schools farther away in the years following the exposure. One hypothesized mechanism for these declines is chronic absenteeism, which they find increased in exposed schools relative to counterfactual schools in the years following the incident. Closely related work examines the impact of school shootings on student outcomes. Abouk and Adams (2013) find that school shootings in the U.S. lead to increases in private school enrollment and Beland and Kim (2016) find that school shootings lead to decreases in test scores among students who remain enrolled in schools affected by shootings.

A growing literature at the intersection of education and development exploits variation in student exposure to localized violent crime. Caudillo and Torche (2014) exploit variation in exposure to localized violence induced by variation in municipality-level homicide rates in Mexico. They find that localized exposure to homicide rates increased grade failure rates at the school-level among primary grade students, although once again these effects were quite short-lived. Monteiro and Rocha (2016) exploit variation induced by conflicts between drug gangs in favelas in Rio de Janeiro. They find that conflict during the school year results in lower math achievement (0.054 SDs) for fifth grade students and that this effect is increasingly negative with respect to conflict intensity, duration, and closeness to standardized exam dates. The authors present evidence to show how schools and staff responded to these incidents: teacher absences, principal turnover, and temporary school closings all increased in the wake of localized violence. Although this does not rule out the possibility of psychological or trauma-related mechanisms, this work highlights the role that supply-side channels play in producing negative achievement effects. Finally, Koppensteiner and Menezes

(2019) leverage variation in exposure to homicides in Brazil. Using information on individual students' routes between home and school, the authors find that exposure to homicides on the path to school raises the probability of dropout by 3 percentage points (20 percent effect).

By exploiting variation in the timing of exposure to violent crime relative to scheduled achievement tests, Sharkey et al. (2014) demonstrate that exposure to violent crime in the week prior to testing decreases student performance on English Language Arts (ELA) tests by about 0.026 standard deviations. Related work with younger children provides confirmatory evidence of this phenomenon. Sharkey (2010) finds that exposure to homicide in the week prior to an assessment of children's vocabulary and reading skills leads to lower performance in both domains. Further investigation into the psychological mechanisms underlying these negative effects suggests that likely mechanisms are cognitive disruption and family stress. Studies with similar designs exploiting preschool-aged children's exposure to localized incidents of homicide and violent crime have demonstrated short-run effects of children's impulse control, memory and attention (McCoy et al., 2015) as well as acute psychological distress among caregivers (Sharkey et al., 2012).

Two recent papers in this area have expanded upon these previous studies to consider two related questions: how might these acute effects generalize to circumstances in which student exposure to violence is chronic, and to what extent do institutions – such as schools – mitigate the negative effects exposure. With respect to the first question, Schwartz et al. (2016) find that acute exposure effects are largest for students with more past exposure, thus suggesting that chronic exposure to crime and violence results in heightened sensitivity (or "sensitization") to adverse events. With respect to the second question, Laurito et al. (2019) find that the negative effects of exposure to violent crime are largest among students who attend schools with low levels of perceived safety or a weak sense of community. In contrast, students who attend relatively safer schools do not experience negative short-run effects of exposure to local violence. These results highlight the important role that schools – and perhaps community institutions more broadly – play in moderating the negative effects of exposure to crime and violence.

A final literature that is related to our study is the growing evidence-base on effective, school-based interventions designed to reduce student absenteeism. Guryan et al. (2016) report the results from a randomized controlled trial (RCT) of Check & Connect, a program in which students were assigned to receive structured mentoring, engagement, and regular check-ins from full-time school employees. The results from the RCT indicate that treated students missed around 1.7 fewer days per year (Intent-to-Treat estimates). Additional evidence on importance of strong relationships comes from recent work on the Early Truancy Prevention Project. Cook et al. (2017) report the results from an RCT of this program, which provided first and second grade teachers with smartphones to text and email parents regularly about their child's attendance. The authors found that the program reduced the share of students with four or more absences by around 6 percentage points (8.9 percent reduction relative to baseline) and identified the closeness of the relationship between the parents and the teacher as one of the potentially important contributors to the success of the program.

3 Data

In our main analysis, we investigate the effects of the SPP by combining a novel, hand-constructed database of school-level participation in the SPP with outcome data on school-level rates of student absence. To provide insight into the mechanisms through which the SPP influenced absences, we augment our main analysis with an in-depth exploration of the effects of the SPP on several intermediate outcomes of interest, including crime rates in the vicinity of CPS elementary schools, reported incidents of serious student misconduct and suspension rates, school security practices, and several other school-level outcomes. We obtained these outcome measures from several different sources. We discuss each data source in the sections below and provide more detailed information in Appendix B.

3.1 Safe Passage Program Data

To document the rollout of the SPP across elementary schools in CPS, we hand-constructed a unique database by combining information from the following four sources: (1) Procurement Contracts from the Chicago Board of Education (CBOE)[6] that outlined agreements with the neighborhood non-profit organizations that provided community monitoring services around designated SPP schools, (2) historical snapshots of the CPS SPP webpage, (3) street-level maps of SPP routes made available by CPS through the City of Chicago Data Portal, and (4) official press releases from the CPS Office of Communication. The information contained in these sources allowed us to identify the exact school year in which CPS introduced the SPP to specific elementary schools in the district. When considered in isolation, we found that none of these sources exhaustively captured the SPP rollout. When combined together, however, we were able to identify the exact timing of introductions at the school-level and match our counts of participating schools with the number of schools enumerated in official announcements from the district. For more information about these sources and our methods, please see Appendix B.

3.2 School-Level Data

We obtained school-level data from the Illinois State Board of Education (ISBE) and from CPS. The ISBE data came from two sources: the Illinois Report Card program and the state's annual published Directory of

[6] The Chicago Board of Education (CBOE) is the financial arm of CPS.

Educational Entities. The CPS data came from publicly-available CPS Employee Position Files and from the CPS website.[7] From the Illinois Report Card program, we obtained annual school-level absence rates and the following time-varying school characteristics: total enrollment and the percentages of black, Hispanic, and low-income students enrolled in the school. From the state's annual Directory of Educational Entities, we obtained each school's address. We used this address to restrict our crime data, which we discuss in more detail below, to only those crimes reported in the vicinity of public elementary schools.

From publicly-available CPS Employee Position Files, we computed the number of school security officers assigned to each school, separately by school year. We also obtained school-level data on student behavior from CPS, including counts of serious reported student misconducts and suspensions at the school-level.[8] Reported student misconducts were separated into categories based on their severity. Our primary measure at the school-level is the rate (adjusted by student enrollment) of reported misconducts that resulted in suspension (in-school or out-of-school), which for brevity we refer to as a "serious misconduct." School-level suspension counts were broken out into in-school and out-of-school suspensions, respectively. For both in- and out-of-school suspensions, we obtained two separate measures: one measuring the unique number of students receiving each type of suspension and one measuring the total number suspension events of each type (not unique to individual students). We normalized all misconduct and suspension counts by student enrollment and created rates per one thousand students enrolled in the school.

Student misconduct in CPS is governed by the Student Code of Conduct (SCC), and misconducts are classified in six levels, each of which corresponds to an increasing level of severity. The least severe includes "behaviors that are inappropriate" (Level 1) and includes behaviors such as leaving the classroom without permission, failing to attend class without a valid excuse, and unauthorized use or possession of cellular telephones or other information technology devices. In contrast, the most severe category includes "behaviors that are illegal and most seriously disrupt" (Level 6), such as bomb threats, robbery, arson, and use, possession, and/or concealment of a firearm (CPS SCC, 2018).[9] For a complete list of student behaviors in Levels 1-6, see Appendix B. Individual schools within CPS are given authority to develop their own rules for addressing student behavior, so long as these rules are consistent with the SCC.[10] We focus on serious misconducts (i.e. those that result in suspension) because we can validate these reported counts against

[7] CPS data are available here: https://cps.edu/SchoolData/Pages/SchoolData.aspx.

[8] School-level data on misconducts and suspensions were only available for the 2011/12-2015/16 school years

[9] The following definitions are used to differentiate the six categories of offenses: "behaviors that are inappropriate" (Level 1), "behaviors that disrupt" (Level 2), "behaviors that seriously disrupt" (Level 3), "behaviors that very seriously disrupt" (Level 4), "behaviors that most seriously disrupt" (Level 5), and "behaviors that are illegal and most seriously disrupt" (Level 6).

[10] CPS schools are explicitly prohibited from including academic performance with student behavior. The SCC states, "However, poor academic achievement is not an inappropriate behavior. The SCC and school rules may not be used to discipline students for poor academic progress or failure to complete in-class and homework assignments. Instead, struggling students should be considered for academic or behavioral interventions to help them improve. Also, students must not be disciplined for the parents/guardians' refusal to consent to the administration of medication" (CPS SCC, 2018).

counts of student suspensions within the same school. In contrast, we are somewhat skeptical about the counts of reported student misconduct by levels (Levels 1 and 2, Levels 3 and 4, Levels 5 and 6). We find the reported values somewhat implausible, although for completeness we report our estimation results using these data in Appendix A.

3.3 Crime Data

We obtained block-level data on reported crimes in Chicago from the Chicago Police Department (CPD).[11] To restrict attention to the vicinity of elementary schools in CPS, we limited our sample to crimes reported within one-quarter mile of our sample of schools based on the school's address, which we obtained from the state's annual Directory of Education Entities. We further restricted the sample to include only those crimes reported on weekdays (Monday-Friday) during the school year, using official start- and end-dates reported in published CPS school year calendars.

We divided all reported crimes (total) into three mutually exclusive and exhaustive categories: violent, property, and non-index. 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. We normalized these counts by student enrollment, thus forming crime rates rates that captured the annual number of reported crimes per one thousand students enrolled in the school.

3.4 Descriptive Statistics

Table 1 presents summary statistics for school characteristics and absence outcomes, separately by eventual SPP treatment status, during the 2007/08 school year.[12] Columns (1) and (2) present means and standard deviations for the 391 untreated control schools and the 83 eventually-treated Safe Passage schools in our sample.[13] Column (3) presents the difference in means for these two groups and the associated standard error (in parentheses), and Column (4) presents the p-value associated with a two-tailed t-test of the difference in means.

[11] These data are available from the Chicago Police Department (CLEAR) through the City of Chicago Data Portal. For more information, see `https://data.cityofchicago.org/Public-Safety/Crimes-2018/3i3m-jwuy/data`.

[12] For most of the variables reported in Table 1 the baseline year is 2007/08. The following variables are exceptions: absence rates for Hispanic students and all misconduct and suspension variables. The baseline year is 2008/09 for absence rates among Hispanic students, since more than half of the observations were missing from 2007/08 year and because non-missing values were implausible (e.g., absence rates of 100 percent). The baseline for misconduct rates and suspensions is the 2011/12 school year, since this is the first year of data we have for these variables.

[13] We constructed a panel of elementary schools in CPS and restricted the sample to those elementary schools that were open continuously for at least 6 years between the 2007/08-2015/16 school years.

Elementary schools that eventually participated in the SPP were observably different than their non-SPP counterparts at baseline. Panel (A) indicates that prior to the introduction of SPP, eventually-treated SPP elementary schools had fewer enrolled students (113 student difference), lower percentages of white (8.0 percentage point difference) and Hispanic (15.1 percentage point difference) students, and larger percentages of black (27.5 percentage point difference) and low-income (11.6 percentage point difference) students. All of these differences are statistically significant at the 5 percent-level. SPP elementary schools also had more school security officers assigned to them, although the difference is small (0.23 officers) and only marginally statistically significant.

Elementary schools that participated in the SPP also had worse absence outcomes than their non-SPP counterparts at baseline. Panel (B) indicates that eventually-treated SPP schools had higher rates of absence overall (6.9 percent vs. 5.6 percent) and higher rates of absence among student subgroups: black students (7.2 vs 6.7 percent), Hispanic students (8.7 vs. 5.6 percent), low-income students (6.8 vs. 5.6 percent), and disabled students[14] (8.3 vs. 7.1 percent). These differences are all statistically significant at the 5 percent-level.

These descriptive statistics highlight pre-existing observable differences between the treatment (SPP) and control (non-SPP) schools in our sample. They also illuminate the relative disadvantage of the SPP schools as compared to their non-SPP counterparts. On average, SPP schools served more racial/ethnic minority students and more low-income students. They also had worse outcomes in terms of student absences prior to the introduction of the SPP. We confirm further evidence of this relative disadvantage at baseline: SPP schools had higher crime rates in their vicinities (one-quarter mile radius) and had worse outcomes in terms of student misconduct and suspensions. SPP schools had higher rates of total crime (830 vs. 547 incidents per one thousand enrolled students), violent crime (76 vs. 42 incidents per one thousand enrolled students), property crime (198 vs. 151 incidents per thousand enrolled students), and non-index crime (555 vs. 353 incidents per one thousand enrolled students). They also had higher overall rates of reported serious misconduct (235 vs. 145 per one thousand enrolled students), higher in-school suspension rates (31 vs. 19 per one thousand enrolled students), and higher out-of-school suspension rates (203 vs. 125 per one thousand enrolled students). Although not the primary focus of this paper, we investigate these intermediate outcomes later in the paper when we discuss plausible mechanisms. Summary statistics for these outcomes are reported in Appendix Table A1.

[14]In this paper, we refer to students with Individualized Education Plans (IEPs) as disabled students.

4 Empirical Strategy

In this paper, we use a difference-in-differences approach to estimate the effects of the SPP on school-level rates of student absenteeism. By exploiting variation in the timing of the introduction of the SPP across public elementary schools in CPS, our estimates capture the extent to which the introduction of community monitoring affected school-level outcomes. In addition to obtaining these estimates, we also implement a flexible event-study approach to examine the pre- and post-SPP changes in the evolution of outcomes at treatment and control schools.

In Section 3.4, we documented pre-existing observable differences between the SPP and non-SPP elementary schools in CPS, but these differences do not invalidate our difference-in-differences strategy. Instead, our approach relies on the assumption of common (or "parallel") trends. Specifically, we assume that in the absence of the SPP, school-level absence outcomes in treated (SPP) schools would have followed the same trend as school-level outcomes in control (non-SPP) schools, despite differences in the levels of these outcomes at baseline. To provide empirical support for this assumption, we implement a flexible event-study specification to examine the pre- and post-SPP changes in school-level outcomes around the years in which the program was implemented. Importantly, the evidence from the pre-SPP periods provides support for the validity of our difference-in-differences approach by demonstrating that outcomes in SPP and non-SPP schools were trending similarly prior to the introduction of the SPP.

4.1 Difference-in-Differences Specification

In this paper, we use a difference-in-differences approach to estimate the impact of the SPP on school-level outcomes. To do this, we estimate an equation of the following form:

$$Y_{st} = \beta \times SPP_{st} + \theta_s + \lambda_t + \phi X_{st} + \varepsilon_{st} \tag{1}$$

Y_{st} is a school-level outcome for school s in year t. SPP_{st} is a binary indicator (0/1) that takes on the value of one for all years including and following the introduction of the SPP at school s in year t. The model includes school fixed-effects, θ_s, which control for observable and unobservable school-level differences that are constant over time, such as time-invariant differences in the school environment, curricular differences, school-level policies, and neighborhood characteristics. The model also includes year fixed-effects, λ_t, which control for factors that are common to all schools in specific years, such as city-wide economic conditions and district-wide policy changes. X_{st} contains time-varying school characteristics and policy controls. The time-varying school characteristics include the natural logarithm of enrollment, the percentage of black students, the percentage of Hispanic students, and the percentage of low-income students. Time-varying policy controls include the number of school security officers assigned to the school and whether the school was designated as a "Welcoming School," due to the closing of another school nearby. All regressions are weighted by total school enrollment, and we report heteroskedasticity-robust standard errors that are clustered at the school level.

In addition to our main specification outlined in Equation (1), we report results from several specification checks designed to probe the sensitivity of our main results to assumptions about functional form. Specifically, we report estimation results from unweighted regressions, models excluding time-varying covariates, models with alternate coding for the "Welcoming Schools" dummy, models augmented with zip code-specific linear trends, and models augmented with school-specific linear trends. Estimates from unweighted regressions provide insight into whether effects are heterogeneous with respect to school size (Solon et al., 2015) and models excluding time-varying covariates provide some reassurance that the introduction of the SPP was not correlated with other time-varying determinants of absenteeism at the school-level. Our re-coding of the "Welcoming School" dummy attempts avoid bias in our estimates of the effect of the SPP that are due to compositional or other changes in schools affected by mass closings.[15] The augmentation of our basic specification with zip code-specific and school-specific linear trends probes the sensitivity of our estimates to these unobserved sources of heterogeneity.

As a final set of robustness checks, we re-estimate our basic specification using two alternative samples: first, we exclude "Welcoming Schools," to show that our results are not driven by the set of schools that received both a Welcoming School and SPP designation. Second, we re-estimate our main specification on a strongly balanced panel of elementary schools to show that our results are not driven by the small number of schools that attrit from our panel due school closures carried out after the mass closings at the end of the 2012/13 school year.

4.2 Event-Study Specification

To complement the presentation our main difference-in-differences results, we also present results from a flexible event-study specification. These results allow for investigation of changes in school-level absence (and other) outcomes around the years in which the SPP was introduced. For example, declines in absences in the treatment group prior to the introduction of the SPP may indicate that our estimates overstate the impact of the SPP by picking up pre-trends. At the same time, evidence of pre-SPP positive shocks to

[15] In our main coding of the Welcoming School dummy, we code only those schools that received the designation in the year following the mass closings, the 2013/14 school year. In our re-coding we allow this designation to persist from 2013/14 until the end of the sample. We know that the introduction of the SPP was positively correlated with the designation of Welcoming Schools, although the effect of this designation on student absences is theoretically ambiguous.

school-level absence outcomes may indicate endogenous policy response and that our estimates overstate the impact of the SPP by picking up mean reversion in the outcome.

To estimate the impact of the SPP on school-level absence rates, we estimate an event-study model of the following form:

$$Y_{st} = \sum_{\substack{k=\underline{k} \\ k \neq -1}}^{1} \left[\pi_k \cdot SPP_s \cdot \mathbf{1} \cdot \left(t - T_s^* = k \right) \right] + \theta_s + \lambda_t + \phi X_{st} + \varepsilon_{st} \tag{2}$$

In Equation (2), the variable Y_{st} is a school-level absence (or other) outcome in school year t. T_s^* is the year in which the SPP was introduced at school s. As in the first estimating equation, θ_s is a set of school fixed-effects and λ_t is a set of year fixed-effects. X_{st} contains the same time-varying school characteristics and policy controls as above. We use a dummy variable, SPP_s, to characterize whether the SPP was ever introduced at school s during the sample period. The estimated π_k coefficients illustrate the effects of the SPP in the years prior to its introduction, $k = \underline{k}, ..., -2$, and following its introduction, $k = 0, 1$ (we note that $k = -1$ is omitted). The lower bound, $\underline{k}$ varies slightly by outcome according to data availability. For crime rates, absences overall, absences for black, low-income, and disabled students, $\underline{k} = -6$. For absences for Hispanic students, $\underline{k} = -5$. For misconduct rates, suspension rates, and police notifications, $\underline{k} = -3$. The year of the introduction of the SPP in school s is $k = 0$. We note that observations occurring more than one year following the introduction of the SPP are binned at the endpoint and thus $k = 1$ captures the average effect of treatment in one (or more) years following the introduction.

5 Results

5.1 The Impact of the SPP on Absences

Table 2 presents results from estimating Equation (1) for school-level absence rates overall and by student subgroup. Our preferred specification in Column (2) indicates that the introduction of the SPP at the school-level resulted in a 0.781 percentage point reduction in the school-level absence rate overall. In relative terms, this translates into an 11 percent effect relative to the baseline school-level absence rate of 6.88 percent.[16] The point estimate in Column (3) comes from an unweighted regression and is slightly smaller than the point estimate from the weighted version, although we note that there is substantial overlap between the ninety-five percent confidence intervals. The small difference in magnitude suggests that the weighted result

[16]For completeness we present an estimate in Column (1) from a model that excludes time-varying covariates. This results in a smaller effect on absences in absolute terms, a 0.575 percentage point reduction. We believe that models without covariates are subject to omitted variable bias and erroneously pick up the effects of time-varying school-level factors that negatively affect absence rates and are positively correlated with the SPP (e.g., time-varying measures that capture aspects of school disadvantage). The inclusion of these covariates corrects for this bias toward zero.

is not driven by schools with large enrollments. The estimate in Column (4) re-estimates our preferred specification using a restricted sample of CPS elementary schools that excludes Welcoming Schools. The point estimate from this regression is very close to the full sample result, thus suggesting that our results are not driven by the subset of schools that received this designation in the wake of the mass school closings at the end of the 2012/13 school year.

To aid in the interpretation of these percentage point effects, we rescale our estimates based on average enrollment in treated elementary schools in our sample (495 students) and a 180-day school year calendar. A school with 495 students and a 180-day school year has 89,100 potential student attendance days. A 0.781 percentage point reduction in absences translates into around 696 additional student attendance days per school year. If absences were distributed uniformly across students in the school, this would result in around 1.4 additional attendance days per student per year. Even if the assumption of uniformity is unlikely to hold, we find the exercise instructive nonetheless. This rescaling allows for clear comparisons other other policy interventions – expressed in terms of average effects – that are designed to decrease student absenteeism, which we discuss in more detail below.

The second and third rows reveal some weak but suggestive evidence of heterogeneous treatment effects by racial/ethnic subgroup. Although the point estimates for the effect of the SPP on absences among black students are similar to the results for the full sample, the point estimate for Hispanic students is substantially larger. The 0.769 and 1.445 percentage point reductions in absences among black and Hispanic students translate into 10.5 percent and 16.5 percent effects in relative terms, respectively. Columns (3) and (4) present results from the same robustness and sensitivity checks as those the full sample, and we find that the results are qualitatively similar to our preferred estimates. Although we do not have a good explanation for why the estimated effects for Hispanic students are larger, we note that this pattern of findings is consistent with previous work in a related literature. Laurito et al. (2019) find that impacts of acute exposure to neighborhood violence on student test scores scores are larger and more negative among Hispanic students who attend schools with low levels of safety. It stands to reason then that Hispanic students stand to benefit the most from interventions designed to reduce this exposure.

In addition to investigating heterogeneity by race/ethnicity, we also investigate heterogeneous treatment effects by gender and for two vulnerable student populations: students from low-income backgrounds and students with disabilities. We do not find any evidence to suggest that the effect of the SPP differs along these dimensions. In the fourth and fifth rows of the table, we present estimation results for absence rates by student gender. Our results are similar to the results for the full sample. In the sixth and seventh rows, we present estimation results for absence rates for low-income and disabled students. Our results are once again quite similar to those that we find for the full sample of students overall.

5.2 Event-Study Results for Absences

Panel (a) of Figure 2 depicts coefficient estimates and associated ninety-five percent confidence intervals for the sequence of π_k coefficients for $k = -6, ..., 1$ in Equation (2). The pattern of coefficients in the pre-SPP years supports our identifying assumption of no pre-trend in treated SPP schools since three out of the five event-time coefficients are statistically indistinguishable from zero. We further expect that the coefficients in the years prior to the introduction of the SPP should be jointly zero (i.e., no significant differences in absence outcomes in years prior to the SPP). The p-value from the F-test for joint significance of these coefficients is 0.11, which means that we cannot reject the null hypothesis that all of the event-time dummies are jointly zero. The evidence in this plot further suggests that the effect of the SPP on absences is instantaneous. We find that in the year of the intervention (i.e., $k = 0$) there is a decrease in the school-level absence rate and that the effect increases with time. As a further check of our identifying assumption, we include a raw plot of average absence rates for the following four groups of CPS elementary schools: 2014 SPP schools, 2015 SPP schools, 2016 SPP schools, and untreated (control) schools. This plot is in Panel (a) of Appendix Figure A1 and illustrates remarkably parallel trends (prior to treatment) across the four groups of CPS elementary schools. To further show that pre-trends are not driven by a specific group of SPP schools in our treatment group, we also produce three additional Event-Study plots in which we sequentially exclude one of the three waves of treatment schools (2014, 2015, and 2016). These plots are depicted in Panels (b)-(d) of Appendix Figure A1 and provide further empirical evidence of no pre-trends.

Panels (b)-(g) depict our estimates of π_k from Equation (2) for school-level absence rates by student subgroup. We observe similar patterns in the coefficients for student subgroups. In the years prior to the introduction of the SPP, coefficients are small and are mostly statistically insignificant. This pre-SPP pattern provides evidence to support our identifying assumption: namely, that there were no pre-trends in absence outcomes in our treatment schools prior to the introduction of the SPP. Most of the evidence suggests that the effect of the SPP on absences is nearly instantaneous. We observe sudden drops in school-level absences rates – by subgroup – in the year in which the program was introduced. Whether the effect increases with time or remains constant is more difficult to discern. The pattern following the year of introduction is not entirely consistent across subgroups groups and the estimates are fairly noisy.

5.3 Robustness, Falsification, and Alternate Inference

To further investigate the sensitivity of our main results to difference choices of functional form and to alternative sample restrictions, we present the results from four additional checks in Appendix Table A2. When using an alternative coding scheme for Welcoming Schools, school-specific linear trends, 5-digit zip code-specific linear trends, and a strongly balanced panel of elementary schools, we find results that are qualitatively similar to those presented in the main table.

In addition to these robustness checks, we also carry out a series of falsification tests to bolster the causal interpretation of our main findings. To do this, we explore the effects of the SPP on school-level outcomes that should be unaffected by the introduction of the program. The results from these exercises provide reassurance that we are not picking up the effects of other unobserved improvements in schools that are correlated with the SPP. If, for example, unobserved improvements in school climate, safety policies, teaching practice (e.g., pedagogy or instructional methods), or other inputs to education production were correlated at the school-level with the SPP and also reduced student absenteeism, then our estimates of the effects of the SPP would be overstated. To explore this possibility, we estimate Equation (1) using several school-level outcomes that should not have been affected by the SPP: namely, average number of minutes per day spent on instruction (Math, English, and Social Studies) and class size. Finding impacts of the SPP on these outcomes would suggest that other unobserved factors at the school-level were driving changes in school-level absences during this period.

Table 3 presents estimation results for the school-level outcomes mentioned above. The results indicate the introduction of the SPP had no discernible impact on any of these school-level outcomes. The point estimates across all columns in this panel are small and statistically insignificant with one single exception. The point estimate for average class size in Column (1) is 0.747 and statistically significant, although this finding is not robust across other specifications in Columns (2)-(4). The single significant point estimate is consistent with what would be expected by chance. The baseline means for these variables in eventually-treated SPP schools are 52.9 minutes per day in Math, 120.5 minutes per day in English, 36.0 minutes per day in Social studies, and class size of 22.4 students, respectively. This means that even though our estimated coefficients are imprecise, the associated ninety-five percent confidence intervals are narrow enough to rule out any meaningful effects in either the positive or negative directions. We interpret these results as suggestive evidence that the SPP was uncorrelated with other improvements across SPP and non-SPP schools and thus that our estimated effects of the SPP are not picking up the effects of these unobservables.

As a final exercise to bolster the validity of our main findings related to absences, we present two alternative approaches to conducting statistical inference in Table 4. First, we present standard errors clustered at the 5-digit zip code level. In contrast to our main method of clustering at the school level, this approach allows for arbitrary serial correlation in error terms at a higher level. As expected, these standard errors are mostly larger than those we obtained when we clustered at the school level, although we note that our conclusions about statistical significance are unaffected by this alternative approach. Second, we utilize randomization inference to calculate permutation p-values. To do this, we randomize assignment to treatment and control ("placebo" SPP status) across all schools in our sample in a way that mimics the real-world rollout of the SPP program We then re-estimate our models using the same functional forms and samples as before and compute the effect of the "placebo" SPP on absenteeism outcomes. We repeat this procedure 1,000 times and calculate the permutation p-value as the fraction of placebo estimates that exceed (in magnitude) our estimated effect. Our conclusions regarding statistical significance are unaffected by this alternative procedure, as our permutation p-values lead us to the same conclusions about the statistical significance of our estimates.

6 Mechanisms

To gain insight into the specific mechanisms through which the SPP decreased rates of student absences, we explore two channels suggested by previous literature: the neighborhood context ("outside the school walls") and the school context ("inside the school walls"). Generally speaking, these refer to a broad set of factors that promote a safe environment in and around schools and that can therefore affect a student's or parents' school attendance decision.

To explore the potential mechanisms, we exploit data on a number of intervening variables related to each of the two channels. First, to investigate improvements in the neighborhood context as a contributor to reduced student absenteeism, we examine whether the SPP leads to a meaningful decline in crime in neighborhoods and areas surrounding schools. To do this, we re-estimate our models using crime rates in vicinity of CPS elementary schools as our outcomes of interest. We do this for crimes overall and separately for violent, property, and non-index crimes. Second, to investigate improvements in the school context as a contributor to reduced student absenteeism, we examine whether the SPP leads to decreases in reports of serious student misconduct. We also examine measures of in-school suspension, out-of-school suspension, and police notification rates (all adjusted for the number of students enrolled in the school).

Our empirical evidence on mechanisms adds new dimensions to the growing literature on effective policy tools to decrease student absenteeism. Although the evidence on effective, school-based interventions designed to decrease student absenteeism is limited, most of the school-based programs in the literature emphasize one of two primary mechanisms: provision of information to parents/caregivers and supportive relationships with school personnel (e.g., teachers, coaches, counselors, or other staff designed to interface with parents about student absenteeism). The evidence here investigates other potential channels and provides policy-relevant insight into other points of intervention that might be effective ways to reduce student absenteeism.

6.1 The Impact of the SPP on the Neighborhood Context

To examine improvements in the neighborhood context as a potential channel through which the SPP reduced student absenteeism, we investigate the impacts of the SPP on crime rates in the vicinity of CPS elementary schools. By examining crime rates within one-quarter mile (radius) of schools using only crimes that were reported during school hours (6AM-6PM), we can empirically measure changes in the neighborhood context that emanated from the SPP.

There are at least three ways in which improvements in the neighborhood context via decreased crime rates could potentially decrease absences. First, lower rates of crime in the vicinity of SPP schools during school hours – particularly during arrival and dismissal times – could reduce students' exposure to crime and violence. Related literature suggests that this would improve students' mental health outcomes and cognitive functioning (Sharkey, 2010; Sharkey et al., 2012; McCoy et al., 2015), and these improvements might themselves also lead to reduced absences. Second, decreased crime rates could potentially improve students' perceptions of safety en route to and from school. Survey evidence suggests that this is an important factor in students' attendance decisions (Kann et al., 2018). Third, reduced crime in the vicinity of CPS elementary schools could improve parents' perceptions of their children's safety during travel to and from school and reduce parental/caregiver stress from exposure to crime. Reduced exposure to crime has the potential to improve family functioning, which is critical to ensuring that children attend school regularly. In previous work, Sharkey et al. (2012) find evidence that exposure to local homicides increases psychological distress and mental health symptoms among parents. For children who rely entirely on parents (or other caregivers) to get to and from school each day, reductions in these negative outcomes could translate into fewer missed days of school.

Table 5 presents estimates of Equation (1) for the impact of the SPP on crime rates within one-quarter mile of CPS elementary schools in our sample. Each cell in the table comes from a separate regression for the total crime rate or crime rate by category specified in the table row. Results in the first row of Column (1) indicate that the introduction SPP at the school-level resulted in 123.7 fewer crimes per one thousand enrolled students (14.9 percent effect relative to the baseline mean of 830.2). The point estimate from estimation without weights (Column (2)) is slightly larger while the point estimate from estimation without covariates (Column (3)) is slightly smaller, although we note that the ninety-five percent confidence intervals from each of these estimates have substantial overlap with the ninety-five percent confidence interval from our main result. Column (4) presents estimation results from a restricted sample that excludes Welcoming Schools. This point estimate is substantially smaller, although the upper bound of the ninety-five percent confidence interval is close to the lower bound of the ninety-five percent confidence interval for our main result, which suggests that the effects of the SPP on crime rates were larger in Welcoming Schools.

The remaining rows disaggregate this main result by crime category and provide confirmatory evidence of previous findings in the literature. Results from our basic model indicate that violent, property, and non-index crime rates declined by 9.7 percent, 14.9 percent, and 15.6 percent, respectively. These are qualitatively similar to the effects reported in Curran (2018), Sanfelice (2019), Gonzalez and Komisarow (2019), and McMillen et al. (2019). We present additional evidence on the robustness of these results using additional specification checks and analytic samples in Appendix Table A3. We also plot the results from a flexible event-study framework in Figure 3, where we see visual evidence of decreased crime rates coinciding with the introduction of the SPP. As a final validity check, we present the same results for crime rates calculated within a one-half mile radius of elementary schools. Our conclusions from this exercise are the same, and the results are in Appendix Table A4.

The estimation results in this section provide strong empirical support for an effect of the SPP on crime rates in the vicinity of schools and suggest that improvements in the neighborhood context could be an important mechanism for reducing student absenteeism. This underscores the importance of factors "outside the school walls" in shaping students' attendance behavior.

6.2 The Impact of the SPP on the School Context

In addition to examining the impacts of the SPP on the neighborhood context, we also examine effects of the SPP on the school context. The SPP was implemented against the back-drop of district-wide reforms in CPS that were designed to reduce the use of exclusionary discipline. These included annual revisions to the the Student Code of Conduct (SCC) and a district-wide a plan to reduce the severity and frequency of suspensions in the 2013/14 school year (Stevens et al., 2015; Hinze-Pifer and Sartain, 2018). These district-wide initiatives highlight the importance of year fixed-effects in our analysis, which should capture district-wide impacts of these policies that are common across all elementary schools in a given school year.

To examine improvements in the school context as a potential channel through which the SPP reduced student absenteeism, we investigate the impacts of the SPP on rates of serious student misconduct, exclusionary discipline (in-school and out-of-school suspension rates), and police notification.

There are several reasons we expect the school context to influence student absenteeism. First, the presence of community monitors' from the SPP could deter student conflict or prevent situations from escalating to the level of a serious incident. Reduced incidents of serious student misconduct – generated via deterrence or de-escalation from community monitors – could improve students' perceptions of safety and thereby encourage regular attendance. The reduction in incidents of serious student misconduct could also lead to small mechanical decreases in student absenteeism through lower rates of exclusionary discipline – namely, out-of-school suspensions – although the magnitudes of our estimates (presented below) are not nearly large enough to explain the reductions in absences that we observe. We believe it is important to investigate this outcome to ensure that decreased exclusionary discipline is not the primary driver of our absence results. Finally, we investigate the impact of the SPP on rates of police notification to gain insight into whether and how the SPP affected police presence in CPS schools.

Table 6 presents results from estimating Equation (1) for school-level outcomes related to reported student misconduct and suspension. Results in the first row of Column (1) indicate that the introduction SPP at the school-level resulted in 63.0 fewer misconducts resulting in suspension per one-thousand enrolled students, which translates into a 27 percent decline relative to the baseline mean of 235.1. Columns (2) through (4) present the results from two specification checks and an alternate sample, where the point estimates and conclusions about statistical significance are similar to the result from our preferred specification. To aid in the interpretation of this effect, we rescale this estimate based on average enrollment in treated elementary schools in our sample (495 students). A decrease in the rate of reported misconduct of 63 per one thousand enrolled students translates into around 31 fewer misconducts resulting in suspension in an average-sized CPS elementary school.

The second through fifth rows of the table report estimated effects of the SPP on in-school and out-of-school suspension rates. In each case, we present rates of suspension calculated among students (unique) and then for incidents resulting in suspension at the school overall. We do not find any evidence of effects on in-school suspension rates, but we do find evidence of effects on out-of-school suspension rates. Estimates for in-school suspension rates in the second and third rows of the table are small and statistically indistinguishable from zero. The estimate in the fourth row of Column (1) indicates that the introduction of the SPP at the school-level resulted in 26.0 fewer unique students receiving out-of-school suspensions per one-thousand students enrolled in the school. This translates into around 12.9 fewer unique students receiving out-of-school suspensions in the average-sized CPS elementary school or a 26.3 percent effect in relative terms (baseline mean is 109.8). The point estimates and our conclusions about statistical significance are very similar in Columns (2)-(4). The estimate in the fifth row of Column (1) indicates that the introduction of the SPP at the school-level resulted in 63.8 fewer out-of-school suspensions per one-thousand enrolled students. At an average-sized CPS elementary school, this translates into around 31 fewer out-of-school suspensions, or a 31 percent decline in relative terms (baseline mean is 203.9).

The sixth and seventh rows of the table present results from estimating our model specifications using the police notification rate as the outcome variable. We do not find any evidence of effects of the SPP on police notification rates. Our coefficient estimates are small and statistically indistinguishable from zero.

Although our null results are somewhat imprecisely estimated, the ninety-five percent confidence intervals are narrow enough to rule out meaningful effects in the positive or negative direction.

To explore the effects of the SPP on reported misconduct, suspension, and police notification rates over time, we once again present results from an event-study specification. Figure 4 plots the point estimates and associated ninety-five percent confidence intervals (the year prior to introduction is omitted). The plots suggest that the effect of the SPP was immediate and that it increased over time. To further probe the sensitivity of our results to model specification and choice of sample schools, we present the results from four additional checks in Appendix Table A5. Our conclusions are unchanged based on these additional robustness checks. As a further validity check on our misconduct and suspension rates, we present results for reported rates of misconduct disaggregated by severity. Although our estimates are noisy, the sign patterns are consistent with the SPP program leading to decreases in the most severe types of misconduct (those offenses that are mostly likely to result in suspension). These results are in Appendix Table A6.

The results in this section suggest that improvements in the school context could be an important channel for reducing student absenteeism. The SPP results in fewer reported incidents of serious student misconduct in school, likely due to community monitors' presence on and near school grounds, which could improve students' perceptions of safety at school. Although we also find decreases in the use of out-of-school suspensions, these effects are not large enough to explain the effects of the SPP on student absenteeism.[17] We believe that these decreases in out-of-school suspensions are a related outcome of the SPP – via decreases in serious student misconduct – but not the primary channel through which the SPP reduced absenteeism. Finally, we present evidence on the effects of the SPP on police involvement in schools, which demonstrates that the SPP contact with and involvement of police was not the primary channel through which other impacts of the SPP were realized.

7 Conclusion

This paper exploits the staggered rollout of a unique community crime monitoring intervention implemented at scale – the SPP – to estimate the causal effects of community monitoring on student absenteeism. By using difference-in-differences and event-study approaches, we find that the SPP decreased school-level rates of student absences by around 0.78 percentage points, an 11 percent decrease relative to baseline. In practical terms, this effect translates in 696 additional student attendance-days for the average-sized elementary school in CPS, or around 1.4 additional attendance-days per student per year. We find limited evidence

[17] On average, the typical length of an out-of-school suspension was around 2.1 days. A reduction in 63 out-of-school suspension events per one thousand enrolled students would result in around 31 fewer out-of-school suspension events at an average-sized school. This would result in around 65 additional student-attendance days per school per year.

of heterogenous impacts by student demographic characteristics. Point estimates for the effect of the SPP on absences among black, low-income, and disabled students are similar to the effect for all students, but the point estimate for Hispanic students is considerably larger. These subgroup effects translate into a 10.5 percent decrease among black students, a 16.5 percent decrease among Hispanic students, a 11.8 percent decrease among low-income students, and a 9.1 percent decrease among disabled students, respectively.

We follow our presentation of main results for student absenteeism with an exploration of the potential mechanisms through which the SPP operated. Specifically, we explore the neighborhood context ("outside the school walls") and the school context ("inside the school walls"). Our findings suggest that both channels are important. We find that the SPP led to improvements "outside of the school walls" in the form of reduced crime rates near treated schools and to improvements "inside of the school walls" in the form of reduced incidents of serious student misconduct within treated schools. These findings provide new insight into the mechanisms and channels through which school-based interventions might effectively address the issue of student absenteeism.

This paper contributes a new perspective on how to address the far-reaching effects of exposure to crime and violence. With growing recognition of the detrimental effects of exposure to crime and violence, this paper offers evidence on a new approach to prevention using a relatively inexpensive strategy: community monitoring. This paper also adds a new insight to the growing evidence-base on interventions designed to reduce student absenteeism. By investigating a previously unexplored policy lever, we show that community monitoring offers the potential to address underlying community and neighborhood determinants of absenteeism.

TABLES AND FIGURES

Table 1: Descriptive Statistics, CPS Elementary Schools by SPP Status, 2007/08 School Year

	(1) No SPP	(2) SPP	(3) Difference	(4) p-value
Panel A. School Characteristics				
Enrollment	633.00	495.53	-137.47***	0.00
	(336.43)	(229.78)	(30.68)	
Percent White	9.65	1.07	-8.58***	0.00
	(16.73)	(2.68)	(0.92)	
Percent Black	47.82	76.84	29.02***	0.00
	(42.51)	(38.21)	(4.73)	
Percent Hispanic	35.51	19.70	-15.82***	0.00
	(36.29)	(35.37)	(4.31)	
Percent Low-Income	81.94	94.17	12.22***	0.00
	(22.41)	(6.25)	(1.36)	
School Security Officers	1.45	1.64	0.19	0.11
	(0.94)	(0.98)	(0.12)	
Panel B. Absences				
All	5.62	6.88	1.26***	0.00
	(1.97)	(2.00)	(0.24)	
Black	6.75	7.29	0.54**	0.03
	(2.54)	(1.89)	(0.25)	
Hispanic	5.61	8.72	3.11***	0.01
	(3.53)	(9.42)	(1.15)	
Low-Income	5.66	6.80	1.14***	0.00
	(1.85)	(1.96)	(0.24)	
Disabled	7.14	8.36	1.22***	0.00
	(2.55)	(2.42)	(0.30)	
Observations(Schools)	*391*	*83*		

Notes: This table presents school characteristics and absence rates for 474 elementary schools in CPS. Columns (1) and (2) present means and standard deviations in parentheses. Column (3) presents the difference in means and associated standard errors in parentheses. Column (4) presents the p-value on the difference in means in the previous column. The baseline year is 2007/08 for all variables except the absence rate for Hispanic students, where the baseline year is 2008/09. All CPS elementary schools are divided into two groups (SPP and Non-SPP) based on their eventual participation in the SPP during the 2013/14-2016/17 school years.

Table 2: The Effect of the Safe Passage Program on Student Absenteeism

	(1) No Cov.	(2) Basic	(3) Unweighted	(4) No W.S.	(5) Baseline Mean
All	-0.575***	-0.781***	-0.693***	-0.733***	6.878
	(0.193)	(0.189)	(0.187)	(0.260)	
	4,421	4,421	4,421	4,233	
Black	-0.563**	-0.769***	-0.678***	-0.994***	7.290
	(0.262)	(0.261)	(0.227)	(0.323)	
	4,404	4,404	4,404	4,216	
Hispanic	-1.418***	-1.445***	-1.442***	-1.524**	8.719
	(0.459)	(0.484)	(0.485)	(0.695)	
	3,809	3,809	3,809	3,621	
Female	-0.552***	-0.748***	-0.671***	-0.672**	6.458
	(0.192)	(0.190)	(0.190)	(0.270)	
	4,418	4,418	4,418	4,230	
Male	-0.604***	-0.825***	-0.716***	-0.794***	7.282
	(0.200)	(0.194)	(0.192)	(0.257)	
	4,418	4,418	4,418	4,230	
Low-Income	-0.594***	-0.803***	-0.708***	-0.771***	6.798
	(0.193)	(0.189)	(0.187)	(0.260)	
	4,418	4,418	4,418	4,230	
Disabled	-0.536**	-0.764***	-0.664***	-0.740***	8.360
	(0.231)	(0.233)	(0.233)	(0.255)	
	4,418	4,418	4,418	4,230	

Notes: Each coefficient comes from a separate regression, where the dependent variable is the aggregate absence rate at the school-level for the full sample of students (Row 1) or for the subgroup of students indicated in the row label (Rows 2-7). The sample is comprised of the 2007/08-2016/17 school years, except absence rates for Hispanic students, which is comprised of the 2008/09-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects and school fixed-effects. Models with time-varying covariates in Columns (2)-(4) include percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. Weighted regressions in Columns (1), (2), and (4) are weighted by school enrollment. Relative to Column (2), which is the specification in Equation (1), Column (1) omits time-varying covariates, Column (3) is unweighted, and Column (4) restricts the sample to exclude Welcoming Schools. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: The Effect of the Safe Passage Program on Other School Outcomes

	(1) No Cov.	(2) Basic	(3) Unweighted	(4) No W.S.	(5) Baseline Mean
Minutes on Math	-0.069	0.105	0.267	0.297	52.976
	(1.212)	(1.308)	(1.069)	(2.372)	
	4,405	4,405	4,405	4,217	
Minutes on English	-1.517	-1.101	-0.828	-1.590	120.522
	(1.205)	(1.284)	(1.103)	(2.301)	
	4,405	4,405	4,405	4,217	
Minutes on Social Studies	0.101	-0.171	-0.181	-0.167	36.004
	(0.561)	(0.605)	(0.525)	(1.034)	
	4,405	4,405	4,405	4,217	
Average Class Size	0.747**	0.489	0.159	0.707	22.483
	(0.365)	(0.439)	(0.419)	(0.617)	
	4,420	4,420	4,420	4,232	

Notes: Each coefficient comes from a separate regression, where the dependent variable is the school-level outcome indicated in the row label. The sample is comprised of the 2007/08-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects and school fixed-effects. Models with time-varying covariates in Columns (2)-(4) include percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. Weighted regressions in Columns (1), (2), and (4) are weighted by school enrollment. Relative to Column (2), which is the specification in Equation (1), Column (1) omits time-varying covariates, Column (3) is unweighted, and Column (4) restricts the sample to exclude Welcoming Schools. Column (5) presents baseline means for these outcome variables in the 2007/08 school year in eventually-treated SPP schools. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: The Effect of the Safe Passage Program on Student Absenteeism

(1) All	(2) Black	(3) Hispanic	(4) Female	(5) Male	(6) Low-Income	(7) Disabled
-0.781***	-0.769***	-1.445***	-0.748***	-0.825***	-0.803***	-0.764***
(0.192)	(0.243)	(0.414)	(0.203)	(0.188)	(0.197)	(0.235)
[0.000]	[0.009]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Each column reports the results from a separate regression, where the dependent variable is the aggregate absence rate at the school-level for the full sample or subgroup of students listed in the column heading. The sample is comprised of the 2007/08-2016/17 school years, except absence rates for Hispanic students, which is comprised of the 2008/09-2016/17 school years. The specification is outlined in Equation (1) and includes year fixed-effects, school fixed-effects, and the following time-varying covariates: percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. Regressions are weighted by school enrollment. Robust standard errors are clustered at the 5-digit zip code level in parentheses. Permutation p-values are reported in brackets. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: The Effect of the Safe Passage Program on Crime Rates Near Elementary Schools (One-Quarter Mile Radius)

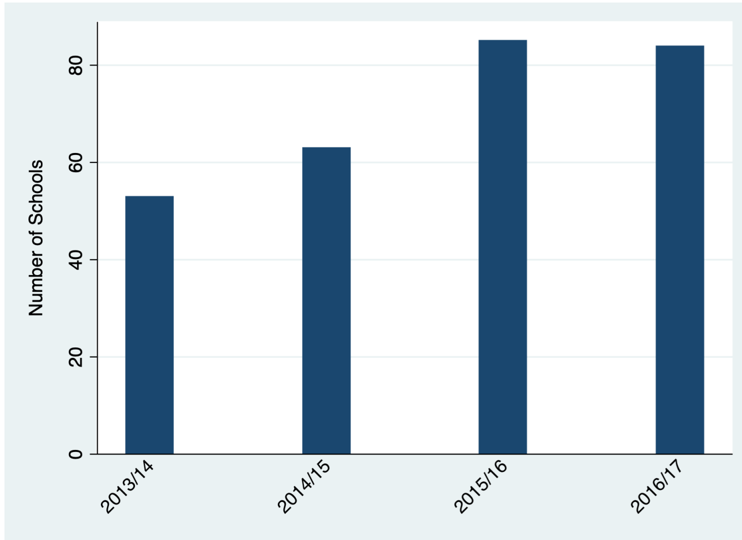
	(1) No Cov.	(2) Basic	(3) Unweighted	(4) No W.S.	(5) Baseline Mean
Total	-183.782***	-123.763***	-89.067**	-37.413**	830.232
	(40.370)	(29.855)	(41.572)	(14.523)	
	4,421	4,421	4,421	4,233	
Violent	-14.345***	-7.383**	-4.629	2.363	76.058
	(3.880)	(2.930)	(3.799)	(2.300)	
	4,421	4,421	4,421	4,233	
Property	-48.766***	-29.731***	-21.971***	-10.741**	198.767
	(9.311)	(6.610)	(8.275)	(5.302)	
	4,421	4,421	4,421	4,233	
Non-Index	-120.670***	-86.649***	-62.468**	-29.035***	555.408
	(28.816)	(22.327)	(31.680)	(11.221)	
	4,421	4,421	4,421	4,233	

Notes: Each coefficient comes from a separate regression, where the dependent variable is the crime rate (crimes per one thousand enrolled students) within a one-quarter mile vicinity of each school for total crimes or the category of crimes indicated in the row label. The sample is comprised of the 2007/08-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects and school fixed-effects. Models with time-varying covariates in Columns (2)-(4) include percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. Weighted regressions in Columns (1), (2), and (4) are weighted by school enrollment. Relative to Column (2), which is the specification in Equation (1), Column (1) omits time-varying covariates, Column (3) is unweighted, and Column (4) restricts the sample to exclude Welcoming Schools. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

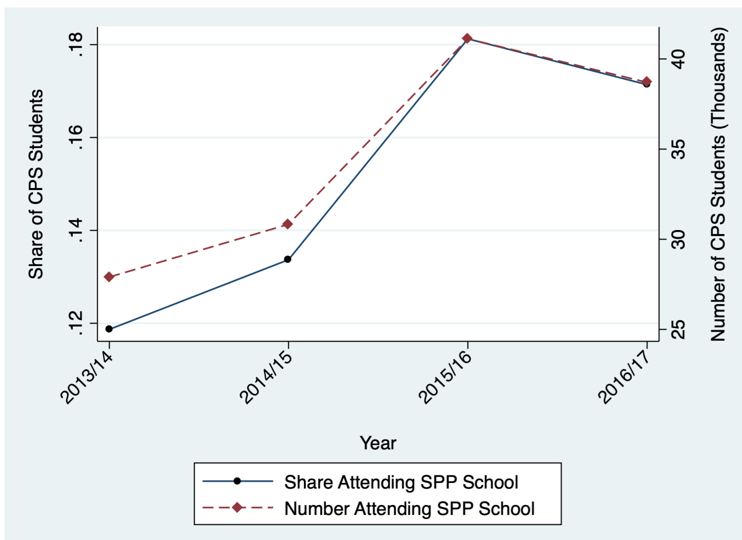
Table 6: The Effect of the Safe Passage Program on Serious Misconduct, Suspensions, and Police Involvement

	(1) No Cov.	(2) Basic	(3) Unweighted	(4) No W.S.	(5) Baseline Mean
Serious Misconduct	-53.025**	-63.050***	-67.642***	-72.615**	235.134
	(20.549)	(21.874)	(22.978)	(31.748)	
	2,570	2,570	2,570	2,382	
In-School Suspension (Unique)	1.768	1.791	-0.429	5.374	21.899
	(4.733)	(4.686)	(5.306)	(6.932)	
	2,570	2,570	2,570	2,382	
In-School Suspension (All)	1.651	1.133	-2.130	8.895	31.208
	(7.976)	(7.905)	(8.966)	(11.842)	
	2,570	2,570	2,570	2,382	
Out-of-School Suspension (Unique)	-22.374***	-26.000***	-26.343***	-29.778***	109.884
	(7.166)	(7.520)	(7.894)	(10.720)	
	2,570	2,570	2,570	2,382	
Out-of-School Suspension (All)	-54.458***	-63.872***	-65.896***	-81.179***	203.927
	(18.212)	(19.487)	(20.212)	(28.219)	
	2,570	2,570	2,570	2,382	
Police Notification (Unique)	0.060	0.297	0.975	-0.530	8.324
	(1.016)	(1.029)	(1.094)	(1.471)	
	2,570	2,570	2,570	2,382	
Police Notification (All)	0.369	0.701	1.835	-0.288	9.959
	(1.351)	(1.368)	(1.443)	(1.935)	
	2,570	2,570	2,570	2,382	

Notes: Each coefficient comes from a separate regression, where the dependent variable is the misconduct, suspension, or police notification rate listed in the row label. The sample is comprised of the 2011/12-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects and school fixed-effects. Models with time-varying covariates in Columns (2)-(4) include percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. Weighted regressions in Columns (1), (2), and (4) are weighted by school enrollment. Relative to Column (2), which is the specification in Equation (1), Column (1) omits time-varying covariates, Column (3) is unweighted, and Column (4) restricts the sample to exclude Welcoming Schools. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



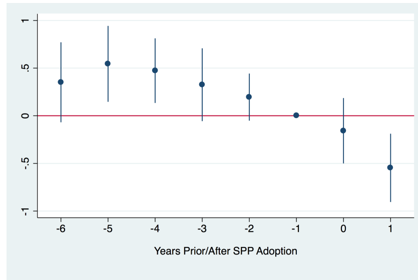
(a) Number of SPP Elementary Schools by School Year



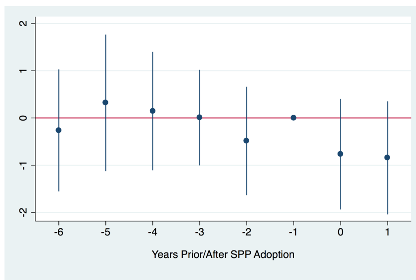
(b) Share and Number of CPS Students Attending SPP Elementary Schools by School Year

Figure 1: Rollout of the Safe Passages Program (SPP) in CPS Elementary Schools, 2013/14-2016/17

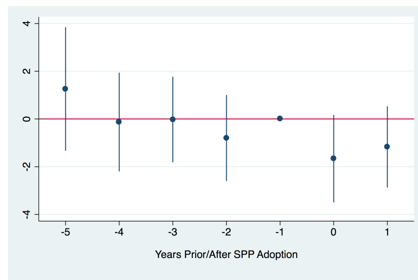
Notes: Panel (a) depicts the cumulative number of CPS elementary schools in the SPP, separately by school year. Panel (b) depicts the share and number of elementary schools students in CPS who attended an SPP school, separately by school year.



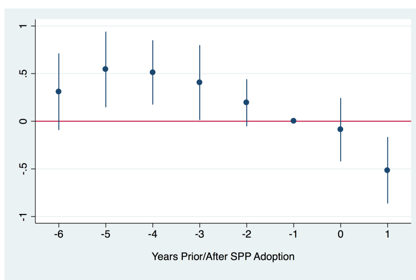
(a) All



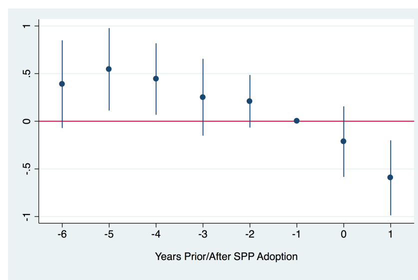
(b) Black



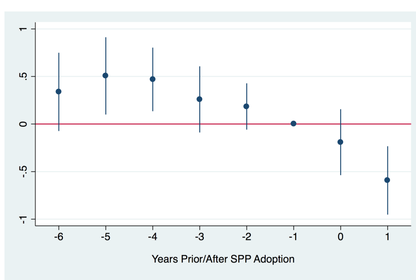
(c) Hispanic



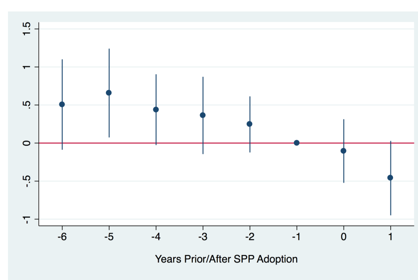
(d) Female



(e) Male



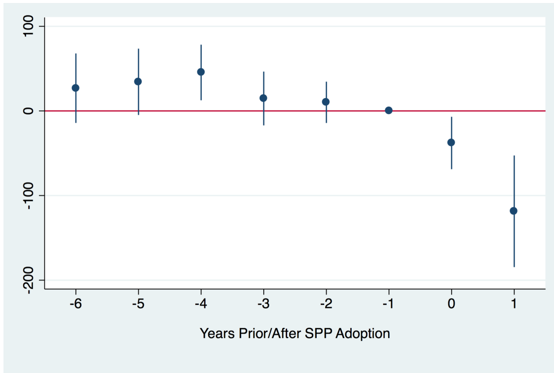
(f) Low-Income



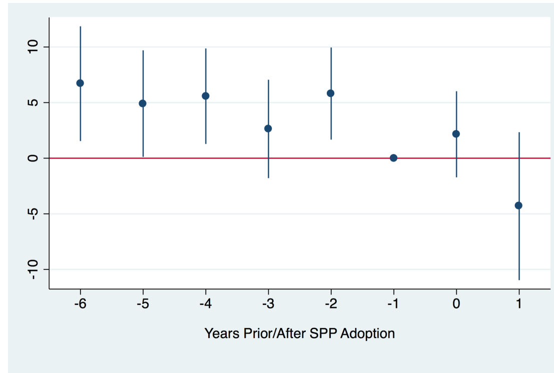
(g) Disabled

Figure 2: Event-Study, Absence Rates Overall and by Student Subgroup

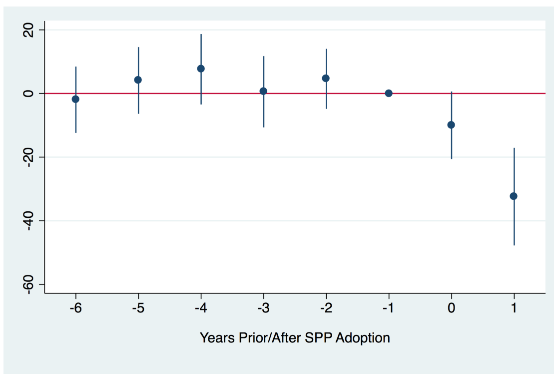
Notes: This figure depicts event-study results from Equation (2) for aggregate absence rates at the school-level. The event-study specification includes year fixed-effects, school fixed-effects, percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools and is weighted by school enrollment. $k = -1$ is omitted. Robust standard errors are clustered at the school-level.



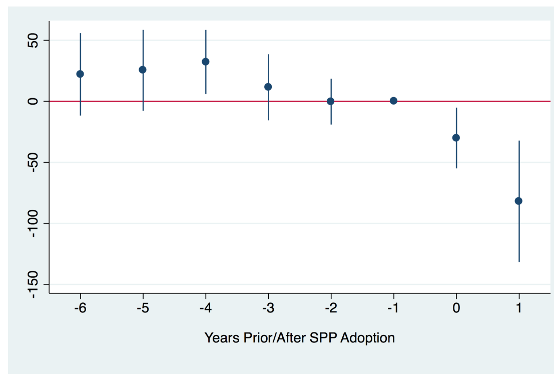
(a) Total



(b) Violent



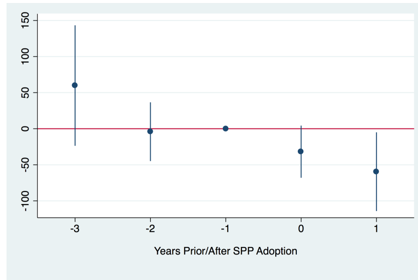
(c) Property



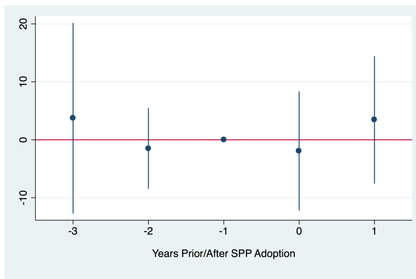
(d) Non-Index

Figure 3: Event-Study, Crime Rates Overall and by Category

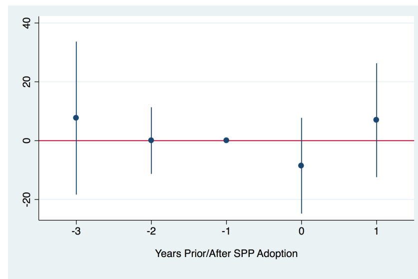
Notes: This figure depicts event-study results from Equation (2) for crime rates (crimes per one thousand enrolled students) within one-quarter mile (radius) of CPS elementary schools. The event-study specification includes year fixed-effects, school fixed-effects, percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools and is weighted by school enrollment. $k = -1$ is omitted. Robust standard errors are clustered at the school-level.



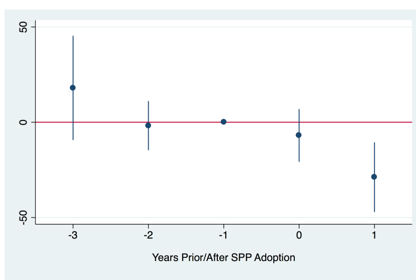
(a) Serious Misconduct



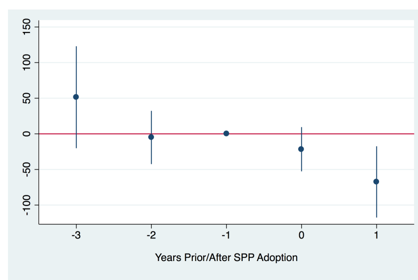
(b) In-School Suspension Rate (Unique)



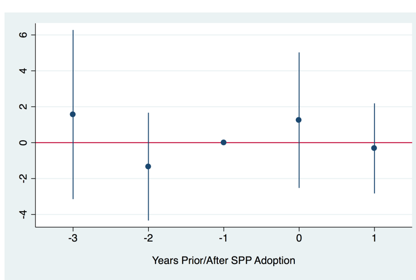
(c) In-School Suspension Rate (All)



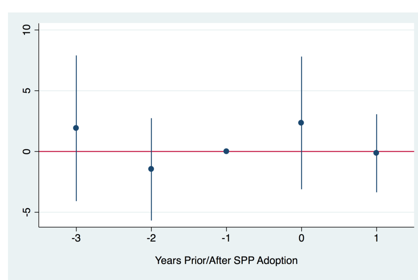
(d) Out-of-School Suspension Rate (Unique)



(e) Out-of-School Suspension Rate (All)



(f) Police Notification (Unique)



(g) Police Notification (All)

Figure 4: Event-Study, Reported Misconduct, Suspension, and Police Involvement Rates

Notes: This figure depicts event-study results from Equation (2) for reported rates of student misconduct, suspensions, and police notification. The event-study specification includes year fixed-effects, school fixed-effects, percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools and is weighted by school enrollment. $k = -1$ is omitted. Robust standard errors are clustered at the school-level.

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Appendix A: Supplemental Results

Table A1: Descriptive Statistics, CPS Elementary Schools by SPP Status, 2007/08 School Year

	(1) No SPP	(2) SPP	(3) Difference	(4) p-value
Panel A. Crime Rates				
All	547.48	830.23	282.75***	0.00
	(761.17)	(541.95)	(71.40)	
Violent	42.87	76.06	33.19***	0.00
	(63.05)	(57.30)	(7.08)	
Property	151.06	198.77	47.70***	0.00
	(145.56)	(117.08)	(14.90)	
Non-Index	353.55	555.41	201.86***	0.00
	(578.51)	(391.69)	(52.44)	
Panel B. Student Misconduct and Suspension				
Serious Misconducts (Rate)	144.79	235.13	90.34***	0.00
	(169.66)	(236.40)	(27.03)	
In-School Suspension Rate (Unique)	15.35	21.90	6.55	0.17
	(24.53)	(42.68)	(4.78)	
In-School Suspension Rate (Events)	19.04	31.21	12.17	0.14
	(32.97)	(75.47)	(8.33)	
Out-of-School Suspension Rate (Unique)	72.62	109.88	37.26***	0.00
	(78.50)	(90.15)	(10.56)	
Out-of-School Suspension Rate (Events)	125.77	203.93	78.16***	0.00
	(160.54)	(212.31)	(24.42)	
Observations(Schools)	*391*	*83*		

Notes: This table presents crime rates, misconduct rates, and suspension rates for 474 elementary schools in CPS. Columns (1) and (2) present means and standard deviations in parentheses. Column (3) presents the difference in means and associated standard errors in parentheses. Column (4) presents the p-value on the difference in means in the previous column. The baseline year is 2007/08 for all variables except for misconduct and suspension rates, where the baseline year is 2011/12. All CPS elementary schools are divided into two groups (SPP and Non-SPP) based on their eventual participation in the SPP during the 2013/14-2016/17 school years.

Table A2: The Effect of the Safe Passage Program on Student Absences (Robustness Checks)

	(1) Alt W.S.	(2) Zip Trends	(3) School Trends	(4) Strong Bal.	(5) Baseline Mean
All	-0.683***	-0.311*	-0.376*	-0.838***	6.878
	(0.256)	(0.170)	(0.213)	(0.192)	
	4,421	4,421	4,421	4,000	
Black	-0.950***	-0.644*	-0.792	-0.810***	7.290
	(0.318)	(0.377)	(0.529)	(0.270)	
	4,404	4,404	4,404	3,984	
Hispanic	-1.509**	-0.592	-0.793	-1.553***	8.719
	(0.682)	(0.403)	(0.957)	(0.498)	
	3,809	3,809	3,809	3,476	
Female	-0.623**	-0.319*	-0.377*	-0.800***	6.458
	(0.267)	(0.167)	(0.207)	(0.193)	
	4,418	4,418	4,418	3,997	
Male	-0.745***	-0.315*	-0.379	-0.887***	7.282
	(0.253)	(0.180)	(0.231)	(0.197)	
	4,418	4,418	4,418	3,997	
Low-Income	-0.720***	-0.332**	-0.397*	-0.859***	6.798
	(0.256)	(0.168)	(0.215)	(0.192)	
	4,418	4,418	4,418	3,997	
Disabled	-0.675***	-0.221	-0.257	-0.820***	8.360
	(0.253)	(0.227)	(0.285)	(0.239)	
	4,418	4,418	4,418	3,997	

Notes: Each coefficient comes from a separate regression, where the dependent variable is the aggregate absence rate at the school-level for the full sample of students (Row 1) or for the subgroup of students indicated in the row label (Rows 2-7). The sample is comprised of the 2007/08-2016/17 school years, except absence rates for Hispanic students, which is comprised of the 2008/09-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects, school fixed-effects, and the following time-varying covariates: percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. All regressions are weighted by school enrollment. Relative to Equation (1), Column (1) uses an alternative coding procedure for Welcoming Schools, Column (2) includes linear trends at the 5-digit zip code, Column (3) includes school-specific linear trends, and Column (4) is a strongly balanced sample. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: The Effect of the Safe Passage Program on Crime Rates Near Elementary Schools (One-Quarter Mile Radius) (Robustness Checks)

	(1) Alt W.S.	(2) Zip Trends	(3) School Trends	(4) Strong Bal.	(5) Baseline Mean
Total	-40.040***	-75.533***	-49.127**	-129.952***	830.232
	(14.394)	(24.298)	(21.978)	(31.497)	
	4,421	4,421	4,421	4,000	
Violent	1.637	-3.845*	1.125	-9.024***	76.058
	(2.249)	(2.330)	(2.524)	(3.038)	
	4,421	4,421	4,421	4,000	
Property	-11.680**	-16.199***	-18.569***	-31.477***	198.767
	(5.266)	(5.746)	(5.841)	(6.763)	
	4,421	4,421	4,421	4,000	
Non-Index	-29.997***	-55.489***	-31.683*	-89.451***	555.408
	(11.099)	(18.486)	(16.996)	(23.591)	
	4,421	4,421	4,421	4,000	

Notes: Each coefficient comes from a separate regression, where the dependent variable is the crime rate (crimes per one thousand enrolled students) within a one-quarter mile vicinity of each school for total crimes or the category of crimes indicated in the row label. The sample is comprised of the 2007/08-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects, school fixed-effects, and the following time-varying covariates: percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. All regressions are weighted by school enrollment. Relative to Equation (1), Column (1) uses an alternative coding procedure for Welcoming Schools, Column (2) includes linear trends at the 5-digit zip code, Column (3) includes school-specific linear trends, and Column (4) is a strongly balanced sample. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: The Effect of the Safe Passage Program on Crime Rates Near Elementary Schools (One-Half Mile Radius)

	(1) No Cov.	(2) Basic	(3) Unweighted	(4) No W.S.	(5) Baseline Mean
Total	-709.430***	-471.471***	-326.191**	-179.119***	3,105.918
	(120.996)	(80.146)	(134.937)	(57.394)	
	4,421	4,421	4,421	4,233	
Violent	-49.024***	-23.427***	-11.627	7.233	275.383
	(11.773)	(8.332)	(12.075)	(7.202)	
	4,421	4,421	4,421	4,233	
Property	-189.272***	-114.151***	-79.976***	-48.568**	769.090
	(32.015)	(21.063)	(28.461)	(20.952)	
	4,421	4,421	4,421	4,233	
Non-Index	-471.133***	-333.892***	-234.588**	-137.784***	2,061.444
	(81.223)	(56.679)	(100.554)	(39.345)	
	4,421	4,421	4,421	4,233	

Notes: Each coefficient comes from a separate regression, where the dependent variable is the crime rate (crimes per one thousand enrolled students) within a one-half mile vicinity of each school for total crimes or the category of crimes indicated in the row label. The sample is comprised of the 2007/08-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects and school fixed-effects. Models with time-varying covariates in Columns (2)-(4) include percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. Weighted regressions in Columns (1), (2), and (4) are weighted by school enrollment. Relative to Column (2), which is the specification in Equation (1), Column (1) omits time-varying covariates, Column (3) is unweighted, and Column (4) restricts the sample to exclude Welcoming Schools. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: The Effect of the Safe Passage Program on Misconduct, Suspension, and Police Involvement (Robustness Checks)

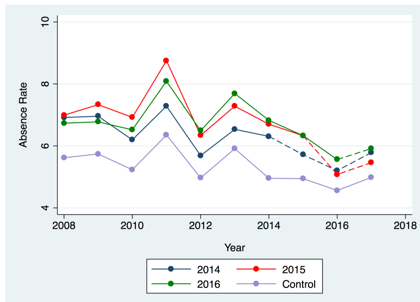
	(1) Alt W.S.	(2) Zip Trends	(3) School Trends	(4) Strong Bal.	(5) Baseline Mean
Serious Misconduct	-69.827**	-37.251*	-11.582	-58.546**	235.134
	(31.113)	(21.503)	(27.742)	(22.671)	
	2,570	2,570	2,570	2,378	
In-School Suspension (Unique)	5.448	-1.789	6.838	1.485	21.899
	(6.778)	(4.721)	(6.767)	(4.819)	
	2,570	2,570	2,570	2,378	
In-School Suspension (All)	8.950	-4.549	9.043	1.067	31.208
	(11.577)	(7.921)	(10.317)	(8.126)	
	2,570	2,570	2,570	2,378	
Out-of-School Suspension (Unique)	-28.660***	-11.380	-8.966	-23.627***	109.884
	(10.464)	(7.344)	(10.977)	(7.625)	
	2,570	2,570	2,570	2,378	
Out-of-School Suspension (All)	-78.479***	-33.171*	-21.217	-59.396***	203.927
	(27.594)	(19.251)	(24.836)	(20.001)	
	2,570	2,570	2,570	2,378	
Police Notification (All)	-0.553	0.676	1.013	0.488	8.324
	(1.438)	(1.105)	(2.088)	(1.009)	
	2,570	2,570	2,570	2,378	
Police Notification (Unique)	-0.340	1.315	1.192	0.887	9.959
	(1.893)	(1.438)	(2.859)	(1.355)	
	2,570	2,570	2,570	2,378	

Notes: Each coefficient comes from a separate regression, where the dependent variable is the misconduct, suspension, or police notification rate listed in the row label. The sample is comprised of the 2011/12-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects and school fixed-effects. Models with time-varying covariates in Columns (2)-(4) include percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. Weighted regressions in Columns (1), (2), and (4) are weighted by school enrollment. Relative to Column (2), which is the specification in Equation (1), Column (1) omits time-varying covariates, Column (3) is unweighted, and Column (4) restricts the sample to exclude Welcoming Schools. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

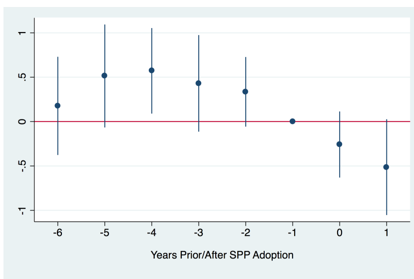
Table A6: The Effect of the Safe Passage Program on Student Misconduct, Separately By Misconduct Level

	(1) No Cov.	(2) Basic	(3) Unweighted	(4) No W.S.	(5) Baseline Mean
All	31.672	23.446	2.145	22.115	262.648
	(41.700)	(43.511)	(41.203)	(73.327)	
	2,570	2,570	2,570	2,382	
Levels 1 and 2	37.910	34.425	25.045	32.412	48.296
	(23.675)	(23.736)	(21.810)	(43.427)	
	2,570	2,570	2,570	2,382	
Levels 3 and 4	-4.907	-8.948	-20.302	-4.506	187.948
	(23.063)	(25.164)	(24.575)	(38.734)	
	2,570	2,570	2,570	2,382	
Levels 5 and 6	-1.331	-2.031	-2.598	-5.791	26.404
	(3.135)	(3.038)	(3.302)	(4.654)	
	2,570	2,570	2,570	2,382	

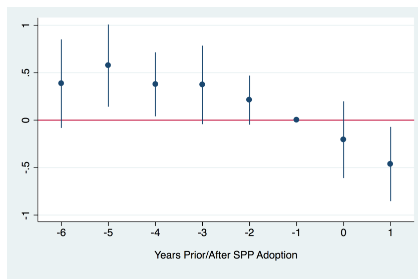
Notes: Each coefficient comes from a separate regression, where the dependent variable is the misconduct rate listed in the row label. The sample is comprised of the 2011/12-2016/17 school years. All specifications in Columns (1)-(4) include year fixed-effects and school fixed-effects. Models with time-varying covariates in Columns (2)-(4) include percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools. Weighted regressions in Columns (1), (2), and (4) are weighted by school enrollment. Relative to Column (2), which is the specification in Equation (1), Column (1) omits time-varying covariates, Column (3) is unweighted, and Column (4) restricts the sample to exclude Welcoming Schools. Column (5) reports the mean of the outcome variable among eventually-treated SPP schools at baseline. Robust standard errors are clustered at the school-level. Asterisks denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



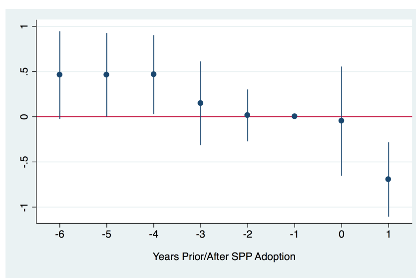
(a) Raw Absence Rates



(b) E-S Excluding 2014 SPP Schools



(c) E-S Excluding 2015 SPP Schools



(d) E-S Excluding 2016 SPP Schools

Figure A1: Raw Plot and Event-Study Detail for Absences

Notes: Panel (a) illustrates average absence rates for 2014 SPP, 2015 SPP, 2016 SPP, and control (untreated) schools respectively. Panels (b)-(d) depict event-study results from Equation (2) for aggregate absence rates at the school-level, excluding SPP schools in 2014, 2015, and 2016, respectively. The event-study specification includes year fixed-effects, school fixed-effects, percent black, percent Hispanic, percent low-income, the number of school security officers, and a dummy variable for Welcoming Schools and is weighted by school enrollment. $k = -1$ is omitted. Robust standard errors are clustered at the school-level.

Appendix B: Data

Safe Passage Program (SPP) Data

We obtained information on Safe Passage Program (SPP) from the following sources:

Procurement Contracts from the Chicago Board of Education

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

The CBOE procurement contracts contained school-level information for the 2010/11-2013/14 school years but only contained neighborhood information thereafter. In addition to documenting the timing and location of the introduction of schools, we also documented key SPP characteristics, separately by school year. These program characteristics included: the goals of the program, hourly pay for community monitors, the 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 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 2013/14-2016/17 school years, CPS made detailed, block-level maps of SPP routes available to the public through the City of Chicago Data Portal. These maps can be accessed here: https://data.cityofchicago.org/.

Historical Snapshots of the Safe Passage Website

Using the Wayback Machine of the Internet Archive, we obtained historical snapshots of the official CPS SPP website from dates corresponding to the beginning of each school year for the 2013/14-2016/17 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 snapshots of the SPP website showed us what information would have been available to CPS parents, students, and the public about the SPP at the beginning of each school year between 2013/14-2016/17. The snapshot from each date included a list of schools with SPP 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. We used information in press releases to verify and confirm we found in other sources.

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

- **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 Offenses.