# When Does Crime Respond to Punishment?: Evidence from Drug-Free School Zones

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December 4, 2020

**Abstract**

Economic theory suggests that crime should respond to punishment severity. However, empirical evidence on this link is ambiguous. We propose one explanation for this discrepancy: Punishments deter crime but only when the probability of detection is moderate. Using increases in punishment severity in drug-free school zones along with changes in the probability of detection resulting from a community crime-monitoring program, we demonstrate that drug-related crime drops in blocks just within the drug-free school zones, where punishments are more severe, but only if the monitoring intensity–and hence the probability of detection–is at intermediate levels.

*Keywords*: crime, deterrence, punishment, fines
*JEL Classification*: K40, K42, H40

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*"Crimes are more effectually prevented by the certainty than the severity of punishment."*

Cesare Beccaria, *On Crimes and Punishments*, 1764

# 1 Introduction

Starting with Becker (1968), the standard economic model of criminal behavior predicts that crimes should respond to the severity of punishments. However, empirical evidence on this link is ambiguous. In a recent survey of the economics literature on the responsiveness of crime to various deterrents, Chalfin and McCrary (2017) conclude that "while there is considerable evidence that crime is responsive to police and to the existence of attractive legitimate labor-market opportunities, there is far less evidence that crime responds to the severity of criminal sanctions".

In this paper, we propose and empirically test one potential explanation for the apparent discrepancy between the theory and the empirics: crime does respond to the severity of punishment but only when the perceived probability of facing those punishments (henceforth, the probability of detection) is moderate. Using the Becker (1968) model, we show that the effect of severe punishments is trivial in settings where the probability of detection is either relatively high or relatively low. In contrast, when the probability of detection takes on values in an intermediate range, potential offenders do respond to the severity of punishments. Intuitively, there is no incentive for offenders to respond to the possibility of harsher punishments if the likelihood of facing those punishments is trivial or almost certain.

We produce empirical evidence in support of this argument by exploiting a novel context in Chicago, Illinois with two unique and independent sources of variation in the severity of punishments and the probability of detection. Our first source of variation comes from Illinois state law, which mandates that drug-related crimes carry more severe punishment when committed in close proximity to schools. Under Illinois state law, drug-related crimes committed within 1,000 feet of a school carry an automatic increase in punishment of one felony class, as compared to the same crimes committed beyond that distance.[1] Termed "drug-free school zones" in this paper, we exploit this sharp change in the severity of punishments both across space, using the boundary of the 1,000-foot radius around schools; and across time, using changes in the presence of drug-free school zones that emanate from school closures and openings of schools during our study period.

For our second source of variation, we follow Gonzalez and Komisarow (2020) in exploiting the phased rollout of a school-based, community crime monitoring program in Chicago, Illinois. This program, the Safe Passage Program (SPP), deployed non-police personnel from the community – i.e., adult civilians who were paid by the hour – to monitor and report crime on designated city blocks surrounding public schools during students' travels to and from school.[2] The phased rollout of this program over time and across schools in Chicago induces spatial and temporal variation in the intensity of monitoring at the city block-level, which we use as a proxy for potential offenders' perceptions of the probability of detection. Specifically, we exploit differences in monitoring intensity across SPP-designated blocks (high perceived probability of detection), SPP-adjacent blocks (intermediate perceived probability of detection), and unmonitored blocks (low perceived probability of detection). SPP-adjacent blocks are those city blocks that directly abut the SPP blocks where community monitors routinely surveilled, monitored, and reported on crime. Previous research has demonstrated that the direct effects of monitoring on SPP blocks spilled over into these SPP-adjacent blocks,[3] although our argument that offenders perceived an increase in the likelihood of detection in these SPP-adjacent blocks does not depend on this.

[1] In 1985, the Illinois state legislature passed Public Act 84-1075, which amended the Illinois Controlled Substances Act to create enhanced penalties for drug crimes committed within 1,000 feet of a school or school property.

[2] For evidence on the successes of the SPP in reducing the incidence of crime near schools in Chicago, see Curran (2018), McMillen et al. (2019), Sanfelice (2019), and Gonzalez and Komisarow (2020).

[3] Section 4 in the paper provides evidence on the monitoring spillovers extending into adjacent blocks. For further evidence on the spillover benefits of SPP monitoring to adjacent city blocks, see Gonzalez and Komisarow (2020).

We combine these two sources of variation to classify all blocks in Chicago as high vs. low punishment blocks, using their location inside or outside of a drug-free school zone, and as high/intermediate/low probability of detection blocks, using their SPP-, adjacent-, or unmonitored designations.

We find evidence consistent with the predictions from the standard model of crime. Drug-related crime decreases in response to increases in the severity of punishments but only in blocks where the probability of detection is at an intermediate level. By comparing neighboring blocks within a fixed bandwidth of the drug-free zone boundaries, we find that the sharp increase in punishments across the drug-free zone boundary leads to 0.14 fewer drug-related crimes in Safe Passage-adjacent blocks per year (71 percent decrease relative to baseline mean of 0.19). In contrast, in blocks where the probability of detection is high or low, we do not find any evidence to suggest that crimes respond to increases in the severity of punishments. Consistent with previous literature, results from a naive specification that ignores the interaction between the severity of punishments and the probability of detection yield a null effect of punishments. The null effect is bounded by a confidence interval that is narrow enough to rule out meaningful effects on drug-related crime in either the positive or negative directions.

We corroborate our main findings by presenting results from two sets of falsification tests and from a recent – albeit short – and not yet studied natural experiment embedded within our broader setting. In our first set of falsification tests, we re-estimate our main specification using data on drug-related crimes that occurred when the probability of detection was low everywhere and did not vary across blocks (e.g., non-attendance school days and time periods including nights, weekends, and days when school was not in session). We find that the negative effect of severe punishments on drug-related crimes previously found in blocks with an intermediate probability of detection disappears entirely. This finding is consistent with previous literature examining the effect of the severity of punishment on crime in settings where the likelihood of detection is very low. In our second set of falsification tests, we assess the extent to which the pattern of effects across blocks with different perceived probabilities of detection holds for crimes that are not drug-related. In principle, this pattern should only be observed for drug-related crimes, as severe punishments in drug-free school zones do not apply to other types of crime. We find that increases in the severity of punishments have no effect on violent crime or property crime.

We also present results from a short natural experiment that occurred during the last year of our data. In 2018, a state-level policy change in Illinois altered the size of drug-free school zones. We exploit this policy change as an additional source of variation in the presence of severe punishments within city blocks over time. Although our results from this natural experiment are less precise, we find a pattern that is consistent with our main findings: the effect of severe punishments is most evident in blocks where the probability of detection is moderate and is absent in blocks where it is low and high, respectively.

We conclude our empirical analysis by estimating bounds for the intermediate range of probabilities of detection in which potential offenders respond to changes in the severity of punishments. Broadly speaking, higher baseline sanctions, less profitable crimes, higher outside options, and risk aversion lower the minimum probability required for a "no-crime" decision. Using back-of-the-envelope calculations, we show that the range of probabilities of detection in which severe punishments matter can be quite small. If the probability of detection is too low, then crime is still marginally profitable, or if it is too high, then changes in punishments do not matter as crime was already not profitable. Simply put, the accompanying probability of detection has to be "just right" for severe punishments to work. This might explain why much of the previous research fails to detect a significant effect of severe punishments and possibly why policies that solely rely on severe punishments often fail.

The remainder of this paper proceeds as follows. In Section 2, we sketch the neoclassical model of Becker (1968) and use it to illustrate our basic argument. In Section 3, we provide background information on drug-free school zones, briefly discuss the phased rollout of community-based crime monitoring, the Safe Passage Program (SPP), in Chicago, and review previous literature. Sections 4 and 5 describe our data and empirical strategy, respectively. Section 6 presents our main findings. In Section 7 we contextualize our main findings, and in Section 8, we conclude.

# 2 Conceptual Framework

This section uses the Becker (1968) model of crime to illustrate how the effect of severe punishments depends closely on the probability of detection. Suppose a potential offender has to decide whether to commit a crime or not. This entails facing a lottery $[(-f, Y) : (p, 1-p)]$ where the individual can expect to succeed with probability $1-p$ and keep profits from illicit activity $Y$, or be detected with probability $p$ and receive a punishment $f$.[4] He will abstain from crime as long as:

$$U(Y^0) > pU(-f) + (1-p)U(Y) \quad (1)$$

where $U(.)$ is the utility function of the individual and $Y^0$ is the income received from non-criminal behavior.

We highlight a key result from Equation 1: Punishments $f$ have an unambiguous effect on reducing the likelihood of crime as long as the probability of detection is not too small or too large. If the probability of detection is very low ($p \longrightarrow 0$), severe punishments are irrelevant in the optimization problem of the potential offender. Intuitively, in a setting where it is very unlikely that offenders will be detected, increasing punishments will not deter their decisions to commit crimes. Similarly, if the probability of detection is close to 1, then the inequality in 1 will hold for any reasonable value of $f$. Thus, the individual will decide to refrain from crime regardless of how much $f$ changes. Intuitively, in a setting where potential offenders know they will be caught with certainty, increasing punishments

[4]For simplicity, we assume that the offender does not keep any profits from crime when caught. However, the analysis yields similar results if one assumes that profits are kept even when caught.

will not affect their decision to abstain from committing crime.

Figure 1 illustrates the main predictions from this model by depicting a simple calibration of Equation 1.[5] We assume risk neutrality and $Y^0 = 0$ as the outside option. In both Figures 1a and 1b, the solid blue line depicts the set of points where a potential offender is indifferent between committing a crime and not, while the shaded area depicts the bundles, $(p, f)$, for which the commission of crime is profitable.

Figure 1a illustrates the main predictions of our model for three hypothetical values of $p$, $0 < p_l < p_m < p_h < 1$. When the probability of detection is low (i.e., when $p = p_l$), doubling the penalties from $f_0$ to $f_1$ does not affect the potential offender's decision of whether or not to engage in crime. The offender chooses to commit a crime regardless of the severity of punishment, since both $a$ and $a'$ lie within the shaded region. A similar result holds when the probability of detection is high (i.e., when $p = p_h$), as the offender will choose to refrain from committing a crime even after doubling the penalties. We note that both $c$ and $c'$ lie outside the shaded region. However, for moderate values of $p$ (i.e., when $p = p_m$), the potential offender's decision to engage in crime clearly responds to a change in the severity of punishments as illustrated by the movement from $b$ to $b'$.

Note that we argue that severe punishments can be trivial if the probability of detection is close to zero but not the other way around, i.e., that the probability of detection is trivial if punishments are close to zero. Even if punishments are zero, increasing the probability of detection lowers the expected crime payoff and therefore can still deter crime. Looking at Equation (1) even if $f$ is near zero, increases in $p$ can still deter crime (since $U(Y^0) > (1-p)U(Y)$). Intuitively, an individual can either pursue the outside option ($Y^0$) or commit a crime. If detected, even if there is no punishment (i.e., $f = 0$), detection means that the individual does not receive crime payoff $Y$. Therefore, by increasing $p$ enough, the individual might actually be better off just getting the outside option even if punishments

[5] This calibration assumes risk neutrality, an outside option $Y^0 = 0$, crime profit $Y = 1$, and a punishment matching crime profits (i.e., $f = 1$). $f'$ represents a doubling of the punishment $f$.

are zero. With that being said, an important and policy-relevant implication of this is that increasing the detection probability can be effective even in low punishments settings. We demonstrate this empirically by showing that the effect of a high probability of detection is consistently negative, even in a low punishment environment. Section 6.1 presents these results.

# 3 Background

## 3.1 Drug-Free School Zones

Drug-free zones around schools in the U.S. date back to the 1970s and 1980s, when the U.S. Congress and individual state legislatures created these zones through federal and state statutes as part of broader efforts to increase the harshness or severity of penalties for drug-related crimes (Bateman, 1995; The Sentencing Project, 2013; Wiltz, 2016). In the legal literature, these laws are sometimes called "school-zone statutes" or "schoolyard statutes," and drug-free school zones are sometimes discussed as part of a broader class of what are often called "enhanced penalty zones" (Bateman, 1995; Wiltz, 2016). Drug-free school zones are specific geographic areas surrounding schools in which offenders face more severe punishments for the commission of drug-related crimes. Found in all 50 states in the U.S., these zones typically extend for a fixed linear distance (e.g., 1,000 feet) relative to a specific landmark (e.g., a school, church, nursing home, playground, etc.) (The Sentencing Project, 2013). Drug-related crimes committed within the boundaries of a drug-free school zone typically carry a more severe punishment than the same crimes committed just slightly farther away.

In 1985, the Illinois State Legislature passed legislation to create drug-free school zones within 1,000 feet of school property.[6] Appendix Figure A1 plots prison terms and fines for drug offenses by distance to schools. In the three decades that followed this initial law, the Illinois state legislature passed additional legislation to expand the application of the law to

[6]See Illinois Public Act 84-1075.

other public spaces and clarified that the law could be applied regardless of the time of day, time of year, or presence of schoolchildren (Kane-Willis et al., nd).

At present, state lawmakers across the country are making strikingly different changes to drug-free school zone policy. In the wake of the opioid crisis, some states are expanding the number of protected places and broadening the applicability of drug-free zones, while others are passing legislation to decrease the number of protected places and reduce the applicability of drug-free zones (Wiltz, 2016). Although empirical evidence on the effectiveness of drug-free school zones is limited, there is growing concern among lawmakers that drug-free school zones do not achieve their stated objective of deterring drug-related crime. Using case-level data from three large cities in Massachusetts, Brownsberger et al. (2004) report that the incidence of drug crime is not lower in drug-free zones when compared to areas outside drug-free zones. At the same time, descriptive studies have raised concerns about the disproportionate impact of these zones on residents of poor neighborhoods. Greene et al. (2006) document that drug-free zones disproportionately cover densely populated urban areas, which raises the concern that they are more likely to affect low-income and minority residents of urban communities. Reports from many states, including Florida, Illinois, Massachusetts, and New Jersey, find that minorities are disproportionately represented in the number of offenses and violations within drug-free zones (Greene et al., 2006).

## 3.2 The Safe Passage Program

Following several violent incidents that involved Chicago Public Schools (CPS) students and mounting public concern about students' safety in the district, CPS introduced the Safe Passage Program (SPP) at 26 CPS high schools during the 2010/11 academic year (Zubrzycki, 2013). This school-based, community crime monitoring program utilizes paid, adult civilians who perform surveillance and crime reporting tasks on established routes during students' arrival at and dismissal from school on regular attendance days during the academic year. Following its initial introduction at the high school level, the SPP was expanded in subsequent school years to serve around 145 elementary, middle, and high schools by the end of the 2016/17 academic year. Figure 2a depicts SPP routes in Chicago from the 2016/17 school year in red.

SPP community monitors wear neon vests that identify them as part of the program and are present on designated routes for between 2-3 hours each morning and each afternoon on regular attendance days during the academic year. SPP community monitors annually receive basic training from CPS covering topics such as first aid, CPR, and conflict de-escalation. SPP community monitors follow district-designed protocols for reporting criminal activity and communicating with school officials about crime in the vicinity of monitored schools (Zubrzycki, 2013). SPP community monitors are unarmed but are issued cellphones or walkie-talkies for communication purposes (Zubrzycki, 2013). The SPP has received recent attention in the economics literature, where a number of papers have demonstrated the efficacy of the program in reducing crime in the vicinity of participating schools.[7]

Gonzalez and Komisarow (2020) point out that schools could not self-select into Safe Passage. Instead, selection was centrally managed and did not follow any specific algorithm or formula. Furthermore, they show evidence that past crime levels do not necessarily predict entrance into the program or the number of years until a school enters the program. Furthermore, in Section 6.1 below, we show that Safe Passage blocks and nearby blocks exhibited similar trends in crime prior to the introduction of the program.

## 3.3 Previous Work

This paper contributes to the economics of crime literature focused on understanding the responsiveness of crime to the severity of punishments. Our main contribution finds the relationship between punishments and crime may be more nuanced that previously believed, and specifically that the effect of the severity of punishments interacts with the perceived probability of detection. We propose a potential explanation for the weak empirical evidence

[7] See Curran (2018), McMillen et al. (2019), Sanfelice (2019), and Gonzalez and Komisarow (2020).

on the deterring effect of punishments. Existing work likely fails to account for the nonlinear nature of the effect as described above or focuses on settings where the likelihood of detection is perceived to be low or very high.

This broad literature has three major areas within it, which we summarize in turn. The first area exploits nonlinearities in punishment sanctions that arise from age-based classifications. Papers in this literature that exploit discontinuities in punishment sanctions that arise at the age of majority typically find small effects.[8] One plausible explanation for these findings is that the increase in the probability of being incarcerated at the age of majority is perceived to be much smaller than the actual increase (Hjalmarsson, 2009a). Consistent with our argument, the low perceived probability of punishment conditional on offense could explain why there is only a small deterrent effect of increases in the severity of punishment.

A second and related line of research studies capital punishment and the death penalty as a source of variation in the severity of punishment (Grogger, 1991; Hjalmarsson, 2009b; Zimring et al., 2003; Kovandzic et al., 2009).[9] For the most part, these studies find a small or no deterrent effect of capital punishment and execution risk. Following our argument, a plausible explanation for these findings is that potential offenders perceive the probability of punishment conditional on offending to be low. In fact, between 1973-2013 only 16 percent of prisoners with a death penalty conviction were eventually executed in the United States, while in Illinois, the state studied in this paper, this number was only 3 percent (Snell, 2014).

A final line of research uses arguably exogenous changes in criminal sentencing enhance-

[8] See for instance Lee and McCrary (2017) or Chalfin and McCrary (2017) for a summary of this literature.

[9] Grogger (1991) and Hjalmarsson (2009b) use executions as event studies to see effects on homicides, and find small deterrent effects using variation in crime in the days around an execution. In these studies, it is hard to separate deterrent effect from temporal displacement. Zimring et al. (2003) compare Singapore which has death penalty with Hong Kong (control) and finds no evidence of deterrent effects. Panel studies find no deterrent effect of capital punishment and execution risk (Kovandzic et al., 2009). Analysis using state-level shocks to capital punishment regimes may have endogeneity concerns.

ments to study the effect of changes in the intensive margin of punishment on deterring crime. These studies find mixed results but mostly point to small deterrent effects on crime. The Three Strikes policy has been a major area of focus within this line of research and overall findings mainly point to small decreases in crime (Helland and Tabarrok, 2007; Iyengar, 2008; Zimring et al., 2003; Shepherd, 2002; Marvell and Moody, 2001).

Another set of papers examines policy changes affecting the severity of punishment for certain crimes to measure deterrent effects compared to crime types unaffected by policies (Bell et al., 2014; Kessler and Levitt, 1999).[10]

# 4 Data and Sample

Data on reported crimes in Chicago came from the Citizen Law Enforcement Analysis and Reporting (CLEAR) system of the Chicago Police Department (CPD).[11] These incident-level data include detailed information on all reported crimes in Chicago for our sample period, 2006-2017. For each reported crime (incident), the data include information on crime type, the latitude and longitude coordinates corresponding to the block of occurrence.

For our main analysis, we restricted our sample to the subset of crimes that were drug-related.[12] For a detailed list of crime subtypes included in our sample, please see Appendix B. We imposed this restriction based on the classification code assigned to the incident

[10]Exploiting a natural experiment after the London 2011 riots which resulted in harsher sentences for riots 6 months after riots, Bell et al. (2014) find a decrease in riot crimes compared to non-riot offenses. Kessler and Levitt (1999) study the effects of sentence enhancements from Proposition 8 in California which increased sentences on a specific set of felonies, right after the passage of the law. Difference-in-Difference using felonies ineligible for Proposition 8 as a control group finds sentence enhancements decreased eligible crimes rates.

[11]These data are public-use and can be accessed here: `https://data.cityofchicago.org/Public-Safety/Crimes-2001-to-present/ijzp-q8t2`.

[12]Formally, these crimes are defined as, "[t]he violation of laws prohibiting the production, distribution, and/or use of certain controlled substances and the equipment or devices utilized in their preparation and/or use."

by the CPD, which follows the Federal Bureau of Investigation (FBI) National Incident-Based Reporting System (NIBRS). We then further restricted the sample to include only drug-related crimes that were reported during the day (6AM-6PM) on days during the academic year when school was in session. We did this by using published versions of the official Chicago Public Schools (CPS) calendar for each school year and removing all drug-related crimes reported on weekends, school holidays, school breaks, teacher professional development days, and during the summer months.

We collapsed the incident-level data to obtain counts of the number of drug-related crimes reported on each city block, separately by year. Our main analytic sample was thus comprised of all drug-related crimes that were reported on regular attendance days during the 2006/07 to 2016/17 school years. In a similar fashion, we separately constructed several auxiliary samples of drug-related crimes reported on regular attendance days at night (6PM-6AM), on designated non-attendance days (e.g., school holidays, teacher professional development days, school breaks), and on weekends. We used these auxiliary samples to perform several falsification checks as a complement to our main analysis.

We note that as with most crime studies, ours relies on *reported* crime rather than actual crime. An inherent problem with this is the potential for increased reporting of crime in monitored areas, which, in the case of our study, would likely result in estimates that are biased toward zero. In our setting, since we are comparing crime across drug-free and nearby control areas, this would be concerning if the degree of reporting varied across these areas. There is no reason to believe that monitors changed their reporting behavior based on whether they were within a drug-free zone or not. However, at the end of section 6.1 we discuss results showing no evidence of differential monitoring behavior across control and drug-free zones.

To assign monitoring levels to each city block in our sample, we took advantage of route-level information from Chicago's Safe Passage Program (SPP). This route-level information came from a unique database that we assembled by combining SPP route maps with information on the timing that SPP routes were rolled out across CPS schools.[13] Information on the exact timing of the rollout came from a combination of three sources: (1) Procurement Contracts from the Chicago Board of Education (CBOE), (2) historical snapshots of the CPS Safe Passage Program webpage, and (3) official press releases from the CPS Office of Communication.

We divided city blocks in our sample into three categories to characterize offenders' perceptions of the probability of detection: low, medium, or high. City blocks with SPP monitoring (i.e., those blocks that were part of an official SPP route) were classified as "high," city blocks adjacent (i.e., directly abutting) to an SPP route were classified as "medium," and the remainder of city blocks (those that were not covered by SPP monitoring nor directly adjacent) were classified as "low." Classifying SPP-adjacent blocks as those in which offenders perceived the probability of detection to be moderate is quite plausible. Notice in Figures 2 and 2b that for many SPP-adjacent blocks there is a direct line-of-sight to Safe Passage blocks, although we note that no SPP monitor was assigned to the block directly. Thus, it is reasonable that offenders perceived the probability of detection to be lower than in Safe Passage blocks themselves but at the same time perceived the probability of detection to be higher than in blocks located farther away from the SPP routes.

In order to empirically validate our assumption that potential offenders perceived the probability of detection in SPP-adjacent blocks to be lower than in SPP blocks yet but higher than in non-SPP blocks, we report the results from a regression in which we regressed our outcome of interest – total drug-related crimes at the block-year level – on a set of four indicators: (1) SPP blocks, (2) SPP-adjacent blocks, (3) blocks within 0.25 miles of any SPP block but not SPP-adjacent blocks, and (4) blocks between 0.25-0.50-miles of SPP blocks.[14] If there were SPP monitoring spillovers into SPP-adjacent blocks as previous research suggests – i.e., potential offenders perceived SPP-adjacent blocks to be partially monitored due to

[13]SPP route maps are public-use and can be accessed here: https://data.cityofchicago.org/.

[14]Specifically, we estimate the following regression: $Y_{bt} = \beta_0 + \beta_1 SPP_{bt} + \beta_2 Adj_{bt} + \beta_3 Within0.25_{bt} + \beta_4 Within0.5_{bt} + \delta_b + \lambda_t + \varepsilon_{bt}$, where $\delta_b$ and $\lambda_t$ are block and year fixed effects.

the possibility of direct sight lines – then we should expect that drug-related crime responds even in those blocks. Appendix Table A5 presents these results. Crimes in SPP-adjacent blocks decline significantly. This is observed across all crime types (columns (2) and (3)). Importantly, however, the decline in crime in SPP-adjacent blocks is substantially lower than the drop observed in SPP blocks themselves but higher than the drop observed in blocks farther away. In fact, for our sample, blocks beyond SPP-adjacent blocks do not exhibit any monitoring spillovers from SPP blocks. This empirical finding gives us confidence that SPP-adjacent blocks are a good proxy for locations where offenders perceived the probability of detection to be moderate.

We classified all city blocks in Chicago as either "inside" or "outside" of a drug-free school zone using the locations of CPS schools paired with an appropriately-sized buffer (either 500 or 1,000 feet, depending on the time period). We then assigned each city block its "inside" or "outside" status based on the latitude and longitude coordinates of the block's centroid. For reference, refer to Figure 2a for a map of all drug-free school zones in the 2016/17 school year and to Figure 2c for a close-up depicting the block centroids along with the drug-free school zones. Using each city block's assignment to low, medium, or high perceived probability of detection combined with its classification as either inside or outside a drug-free school zone, we assigned each block to one of six categories based on pairwise combinations of $\{p_l, p_m, p_h\}$ and $\{DFSZ, Not\ DFSZ\}$.

# 5 Empirical Strategy

Our empirical strategy takes advantage of spatial and temporal variation in the severity of punishments and variation in offenders' perceptions of the probability of detection that come from drug-free school zone status and SPP monitoring, respectively. Temporal variation in the probability of detection comes from the phased rollout of the SPP, while temporal variation in drug-free school zones (and hence individual blocks' drug-free school zone status) comes from the school openings and closures in Chicago, which were significant during this time period. In later results (Section 6.3), we also exploit a temporal change in the size of drug-free school zones emanating from a change in Illinois state law.

We use these two sources of variation to estimate a flexible empirical specification that allows the effect of severe punishments to depend on the probability of detection.[15] Connecting this to our conceptual framework, we consider non-monitored, SPP-adjacent, and SPP blocks to be cases corresponding to offenders' perceived probabilities of detection of $p_l, p_m, p_h$ in Figure 1, respectively. At the same time, a block inside a drug-free school zone is equivalent to the movement from $f$ to $f'$. We estimate an equation of the following form:

$$Y_{bt} = \beta_0 + \beta_1 D_{bt} + \beta_2 S^{\perp}_{bt} + \beta_3 S_{bt} + \beta_4 D_{bt} S^{\perp}_{bt} + \beta_5 D_{bt} S_{bt} + \beta_6 dist_{bt} + \delta_b + \lambda_t + \varepsilon_{bt} \quad (2)$$

$Y_{bt}$ is the count of drug-related crimes for block $b$ in school year $t$. $D_{bt}$ is a binary indicator that equals one to indicate that block $b$ is inside of a drug-free school zone in school year $t$. $S^{\perp}_{bt}$ is a binary indicator that takes on the value of one when the block is directly adjacent to an SPP block during year $t$ (i.e., SPP-adjacent). This captures whether block $b$ has a moderate or intermediate probability of detection. $S_{bt}$ is a binary indicator that takes on the value of one when the block is an SPP block in year $t$. This captures whether block $b$ has a high probability of detection. $dist_{bt}$ denotes distance from block $b$ to the drug-free school zone boundary. This control allows for the comparison of blocks that are the same distance away from drug-free school zone boundary. Our baseline specification also includes a vector of year fixed-effects, $\lambda_t$, to capture city-wide shocks to drug-related crimes that are

[15]Refer to Figure 2 for evidence on the spatial variation in SPP and drug-free school zone status. Since opening or closure of schools is likely endogenous, Columns (1) and (2) of Appendix Table A4 replicate our main results by restricting the sample to blocks without any change in drug-free school zone status. These are essentially blocks where the school remained open for the entire study period and thus do not suffer from selection into opening/closure of schools. These results do not differ significantly from our main results. For the case of selection of blocks into SPP, refer to Gonzalez and Komisarow (2020) for a detailed discussion.

common to all blocks within calendar years, and a vector of block fixed-effects, $\delta_b$, to capture time-invariant characteristics of blocks that are common across all years in our sample. In addition to this baseline specification, as a robustness check we also present results that add neighborhood-level linear time trends. We report heteroskedasticity-robust standard errors that are clustered at the neighborhood level.

Following the discussion presented in Section 2, we expect coefficients $\beta_1$ and $\beta_5$ to be small in magnitude and statistically indistinguishable from zero, since these represent the effects of severe punishments in blocks where the probability of detection is low and high, respectively. In contrast, we expect $\beta_4$ to be negative, since theory predicts that the combination of severe punishments and a moderate probability of detection will have a negative effect on the occurrence of drug-related crimes. Note also that coefficient $\beta_3$ serves a key purpose. It implicitly tests for the counter-argument, described at the end of section 2, that the detection probability can also be trivial when punishments are low.[16] A consistently negative and significant estimate of $\beta_3$ would provide evidence against this and in favor of our argument that increasing the detection probability can be effective even in low punishment settings.

Coefficient $\beta_5$ also provides evidence on whether SPP community monitors potentially altered their monitoring behavior based on whether they were working within or outside of a drug-free school zone. If monitoring behavior changes based on whether a monitor is within a drug-free zone or not, then we should expect the coefficient estimate of $\beta_5$ to be either significantly negative (in case monitors increase their efforts within drug-free zones) or significantly positive (in case monitors shirk if placed within a drug-free zone).

To emphasize the importance of accounting for the interaction between the severity of punishment and the probability of detection, we present two sets of results: first, we present results using a version of Equation (2) that sets coefficients $\beta_2$ through $\beta_5$ to zero. We refer to this restricted version of the model as our naive specification, as this version of the model

[16] Recall that our main argument is that harsher punishments are trivial if the probability of detection is low

does not allow for any interaction between the severity of punishments and the probability of detection. Second, we present results using a regression discontinuity specification that compares changes in crime at the drug-free school zone boundary but once again fails to account for any interaction between the severity of punishment and the probability of detection.

Identification of the parameters in Equation (2) requires that, in the absence of treatment, trends in drug-related crime in SPP and drug-free school zone blocks would have paralleled those of control blocks. Broadly speaking, other than their treatment assignment, SPP and drug-free school zone blocks should not be systematically different from our choice of control blocks. We address these concerns in two ways. First, we restricted our analysis to blocks within a 1000 feet (about 0.19 miles) band around drug-free school zone boundaries.[17] Appendix Figure A2 presents an example of the boundary with the corresponding sample. We note that when experimented with even narrower bandwidths, our main results remained economically and statistically robust.[18]

Second, we performed a set of tests to shed light onto the validity of our identifying assumption. These tests assessed whether there was evidence of differential pre-trends between SPP, SPP-adjacent, and drug-free school zone blocks relative to our control blocks. We found no differential pre-trends in crime levels for SPP, SPP-adjacent, and drug-free school zone blocks relative to control blocks within our bandwidth of analysis. This is visually evident in Panels (a) and (b) of Figure 3 and further confirmed in Panels (c) and (d).[19]

[17]Similarly, we exclude blocks that are within the drug-free school zones but within 100 feet of school grounds. This ensures that blocks near schools which can be systematically different than other blocks are not included in the analysis.

[18]These results are presented in Columns (3)-(6) of Appendix Table A4.

[19]Panel (c) plots the coefficients from a regression using dummy variables for each number of years since implementation of Safe Passages. Specifically, we estimate: $Y_{bt} = \alpha + \sum_{g=0}^{6} \beta_{-g} S_{b,t-g} + \sum_{g=1}^{6} \beta_{+g} S_{b,t+g} + \delta_b + \lambda_t + \varepsilon_{bt}$, where $S_{b,t-g}$ equals 1 if block $b$ adopted Safe Passage community monitoring in year $t-g$ (lags) and $S_{b,t+g}$ equals 1 if block $b$ will adopt Safe Passage community monitoring in year $t+g$ (leads). We use the year prior to implementation as the reference year. For panel (d), we simply use the 2010-2011 school year as the "implementation" year and estimate the regression $Y_{bt} = \alpha + \sum_{g=-6}^{4} \beta_g D_{b,t+g} + \delta_b + \lambda_t + \varepsilon_{bt}$. In

It is also important to recognize that there may be steps in the criminal justice process impacted by the increased severity of punishment in drug-free school zones. For example, prosecutors can respond to sentencing enhancements in drug-free school zones by differentially prosecuting these drug crimes, increasing the likelihood of punishment in drug-free school zones. The only implication of this is that it increases the expected punishment within drug-free zones, therefore it does not alter our interpretation of drug-free blocks as enhanced punishment blocks in our empirical strategy.

# 6 Results

## 6.1 Effect of Punishments and the Probability of Detection

In Table 1, we present estimation results from multiple versions of Equation (2) using our primary outcome of interest: annual counts of drug-related crimes at the block-level. Column (1) shows results from our naive specification that fails to account for how the effect of severe punishments interacts with the probability of detection (i.e., $\beta_2$ through $\beta_5$ in Equation (2) are set to zero). Much like the previous literature, the point estimate on the drug-free school zone indicator (i.e., the blocks with severe punishments) is small and statistically insignificant. This result suggests that severe punishment just within drug-free school zones does not seem to deter drug-related crimes. Appendix Table A1 and Appendix Figure A3 confirm this result using an alternative methodology: a regression discontinuity design that compares blocks on each side of the drug-frees school zone boundary for a given bandwidth.[20]

the case of Safe Passage blocks please refer to Gonzalez and Komisarow (2020) for further corroboration on the absence of differential pre-trends.

[20]Specifically, we pool all years prior to the introduction of Safe Passages and estimate $Y_b = \beta_0 + \beta_1 D_b + f(Distance) + \lambda_t + \varepsilon_b$ where $f(Distance)$ is a polynomial in distance from the block centroids to the boundary of the drug-free zones and the remaining terms are defined as in Equation (2). Panel A uses a local linear specification for the RD polynomial and a bandwidth determined optimally following Calonico et al. (2014). Panels B and C use a wider bandwidth and a second degree RD polynomial. Panel C adds boundary fixed

Table A2 confirms this null result when dividing drug-related crimes by subtype: possession (Column 1) and manufacturing (Column 4).

Column (2) of Table 1 presents results in which we allow the effect of severe punishments to depend on the probability of detection. Consistent with our conceptual framework, we find significant evidence of this dependence: namely, an increase in the severity of punishments has a negative and significant effect on drug-related crime when the probability of detection is at moderate levels (i.e., in SPP-adjacent blocks) but not when the probability of detection is high (SPP blocks) or low (non-monitored blocks). Specifically, our estimate of $\beta_4$ indicates that when the probability of detection is at intermediate levels, an increase in the severity of punishments leads to 0.140 fewer annual drug-related crimes per block per year. In contrast, our estimates of $\beta_1$ and $\beta_5$ are both small and statistically insignificant. These results are consistent with the predictions of theory, which tells us that the effects of severe punishments should be negligible (i.e., close to zero) when the probability of detection is relatively low or high. We confirm that our results are essentially unchanged by the addition of neighborhood-level linear time trends to account for differing trends in crime across neighborhoods (Column 3).

Results in Appendix Table A2 confirm a similar pattern when focusing on specific subtypes of drug-related crime: possession offenses (Columns (2) and (3)) and manufacturing offenses (Columns (5) and (6)).[21] Refer to Panel (a) of Appendix Figure A4 for a graphical representation of the results. Last, Appendix Table A3 confirms that our results are also robust when using a panel Poisson specification of Equation (2).

Note that our results confirm that there is no evidence in favor of the counter-argument described at the end of section 2 that the probability of detection is trivial when punishments

effects to ensure comparisons of blocks within the same drug-free zone. Panel A of Figure A3 visually confirms that there is no significant change in drug crimes. Panel B confirms that the sample of blocks is balanced across the drug-free zone boundary.

[21]Results for manufacturing offenses are less precise. This is likely due to the low number of drug crimes classified as "manufacturing" relative to drug possession offenses.

are low. If that was the case, then the estimate of the coefficient on SPP–the effect of monitoring in a low punishment environment–would be close to zero and statistically insignificant. Instead, we find a consistently negative and statistically significant effect of monitoring even when punishments are relatively low. Note also that the fact that we find the estimate of the coefficient on "Drug-free × SPP" to be small and statistically insignificant suggests that SPP community monitors were unlikely to change their behavior (i.e., monitoring intensity) based on whether they were working within or outside of a drug-free school zone.

## 6.2 Falsification Tests

Columns (4)-(6) of Table 1 present the results from a set of falsification tests in which we use several auxiliary samples of drug-related crimes that occurred during periods of the day and times of year when SPP monitors were not physically present, and thus the probability of detection did not vary across blocks. We therefore expect the effect of severe punishments not to vary across SPP, SPP-adjacent, and non-monitored blocks. We report results for drug-related crimes that occurred at night (Column (4)), on designated non-attendance days (e.g., school holidays and teacher professional development days) (Column (5)), and on weekends (Column (6)). The differential effect of severe punishments that we previously observed across blocks where the probability of detection was high, intermediate, and low, respectively, disappears entirely.[22] This is consistent with findings from other settings in which the probability of detection is low.

Columns (7)-(8) of Table 1 present results from a second set of falsification tests in which we examine types of crime that are not drug-related and thus should be unaffected by drug-free school zones. For both violent and property crimes, we find that the effects of severe punishments do not vary with the probability of detection. Instead the effects are relatively small in magnitude and statistically insignificant across SPP, SPP-adjacent, and

[22]Refer to Panels (b)-(d) in Appendix Figure A4 for a graphical representation of these results.

non-monitored blocks.[23]

## 6.3 Further Evidence from a Natural Experiment

We proceed by presenting results from a natural experiment embedded within our broader setting. In January of 2018, the state of Illinois passed a law that reduced the size of drug-free school zones. Specifically, the policy change decreased the size of drug-free school zones from 1000 feet (radius) around schools to 500 feet (radius). This recent and previously unexploited policy change removed severe punishments for a subset of blocks that had previously been in drug-free school zones, and had previously been subject to severe punishments from sentencing enhancements. Namely, blocks within 1,000 feet but beyond 500 feet of schools. We leverage the spatial variation in the severity of punishment induced by this policy change to further study the relationship between the severity of punishments and perceived probability of detection during the 2017/2018 and 2018/2019 school years.

Since the policy was introduced in the middle of the 2017/2018 school year, we disaggregate the crime data to the monthly level in order to have block variation in drug-free zone status within the school year. We thus proceed by estimating Equation (2) with subscript $t$ denoting month rather than school year. Just as before, we begin with the estimation of a naive specification that ignores the interaction between the severity of punishment and the probability of detection. Column (1) of Table 2 presents results from our naive specification. Once again, we find no evidence to suggest that drug-related crimes respond to severe punishments. Columns (2) and (3) present results from our interacted specification and provide additional evidence supporting our main findings. The point estimate for the effect of severe punishments in SPP-adjacent blocks is substantially larger than the estimates for the effects of severe punishments in blocks where the probability fo detection was either low or high. Specifically, the magnitudes of our estimates in SPP-adjacent blocks (-0.009 and -0.008 in Columns (2) and (3), respectively) are almost four times larger than the magnitudes of our

[23] Refer to panels (e) and (f) of Appendix Figure A4 for a graphical representations of these results.

estimates for blocks where the probability of detection is high (0.003). Although the -0.009 effect in Column (2) is not statistically significant and the -0.008 effect in Column (3) is only significant at the 10-percent level, we take the evidence from this recent natural experiment (occurring only in the last year of our data) as suggestive and consistent with our main findings.

The lack of statistical significance in these estimates compared to our main results in section 6.1 is to be expected. SPP status (and hence SPP-adjacent block status) only varies at the school-year level. This is why we use school years as the unit of time in our main results. The analysis in this section is disaggregated to the monthly level to measure the effect of the drug-free policy change that took place midway through the school year. Taken together with the main results, the results presented in this section provide additional – albeit weaker – evidence that severity of punishments interacts with the probability of detection.

# 7 Discussion

The results in this paper demonstrate that severe punishments work when the probability of detection, $p$, is at intermediate levels.[24] In this section, we present the results from a simple exercise designed to shed light on plausible values for "intermediate" levels of $p$ in this context. Using the model from Section 2 and a setting in which punishments for crime are assumed to double, we perform a series of back-of-the envelope calculations designed to estimate plausible values of $p$ for which an individual will refrain from choosing crime. In practice, we believe the more policy-relevant question is to determine the lower bound of this range and thus we mostly focus on answering the question: "Given a doubling of sanctions, what is the minimum probability $p$ required to guarantee a no-crime decision?"[25]

[24] We clarify that "intermediate" simply means that the probability should lie away from 0 and 1, not that it should be close to 0.5.

[25] A budget-constrained agency trying to deter crime from happening, would need to increase the probability of detection just enough to guarantee deterrence.

Assuming risk neutrality in Equation (1), it is clear from Figure 1b that this probability is given by:

$$p_0 = \frac{Y - Y^0}{Y + 2f} \tag{3}$$

Using our setting, Table 3 presents possible values of $p_0$ for various drug possession violations. We focus on possession violations since sanctions and crime profits are more straightforward than for manufacturing or trafficking violations.[26] In the case of marijuana possession, $p$ needs to be at least 0.41 for a minor offense and 0.22 for more serious offenses. Intuitively, more serious offenses tend to carry higher baseline punishments, thus the minimum probability required to deter them is not as high. This is also reflected with "harder" drugs: the required $p$ is lower and ranges from 0.04 for methamphetamine to 0.08 for heroin. These values are feasible given our setting: SPP-adjacent blocks are near heavily monitored blocks and notice from Figure 2b that for many there is a direct line-of-sight from monitored SPP blocks. Thus, it is quite plausible that offenders perceived the probability of detection in SPP-adjacent blocks to be within the ranges discussed above.

This exercise potentially provides real-world values for this intermediate probability of detection, but more generally, it shows that the minimum required probability for a "no-crime" decision is determined by several factors. Higher baseline sanctions, less profitable crimes, and higher outside options lower the minimum probability required for a "no-crime" decision. This is clearly illustrated in the comparative statics exercise presented in panels (a)-(c) in Appendix Figure 4. Intuitively, these three factors make crime less desirable and thus they should make the effect of an increase in severity of punishments bind more easily. Similarly, risk aversion also matters in panel (d) : more risk averse individuals tend to

[26] We make several assumptions: (i) crime profits $Y$ are assumed to be the utility gain from consuming the drug. We use street price as the value of $Y$. This can be interpreted as the price capturing this utility gain. We obtained these prices primarily from transactions data collected by the Drug Enforcement Agency using undercover agents (DEA, 2018). (ii) following the above, we assume the outside option $Y^0 = 0$. (iii) Since offenders do not always face the maximum punishment, we simply use the midpoint as the value for $f$. Data on maximum punishments were obtained from the Illinois Compiled Statutes (ILCS, 2019b).

respond to punishments at lower values of $p$.[27]

Also important, note from Figure 1b and the estimates in Table 3 that the range of $p$ for which offenders respond to severe punishments can be quite small. If $p$ is just to the left of $p_0$ then crime is still marginally profitable. If $p$ is just to the right, then changes in punishments do not matter as crime was already not profitable. This helps illustrate why it might be difficult to detect the effects of severe punishments in previous research and why policies that solely rely on harsher punishments are likely to fail. Simply put, the accompanying $p$ has to be in a specific range for severe punishments to have a deterring effect on crime. With this in mind, we proceed to discuss policy recommendations and potential avenues for future research.

If offenders systematically underestimate punishments and the probability of detection, then a policy like increasing fines is potentially useless in deterring criminal behavior. This can be seen in Figure 1a if we imagine the actual probability of detection is $p_m$ but a potential offender underestimates it to $p_l$. A doubling of punishments is supposed to deter crime under perfect information ($b$ to $b\prime$), however, in practice, it does not ($a$ to $a\prime$). This is aggravated if underestimation of $f$ or lack of awareness about the policy leading to more severe punishment is also present. Future research assessing whether offenders systematically underestimate $f$ and/or $p$; or more generally, how they form expectations on $f$ and $p$, can thus have important policy implications. Similarly, policies designed to inform about punishment severity and/or detection probabilities could be effective at deterring crime. Risk preferences can also affect the effectiveness of policies that enhance punishments. Note in panel (d) of Figure 4 that for a probability of detection somewhere between $p_0$ and $p_1$, doubling fines will be effective for risk-neutral individuals but ineffective for risk-loving individuals. Although there is an existing literature on the effects of violence on the risk preferences of victims, very little

[27] We use an exponential utility function, $U(c) = \alpha^{-1}[1 - exp(-\alpha c)]$ with $\alpha \in [-1, 1]$ denoting the risk aversion parameter and $U(c) = c$ when $\alpha = 0$. $\alpha > 0$ denotes risk aversion, $\alpha < 0$ denotes risk loving, $\alpha = 0$ denotes risk neutrality. Function chosen because it eases calculation of $U(-f)$ in Equation (1).

is known about the risk profile of potential offenders. With this in mind, studying risk preferences among criminal offenders seems like a policy-relevant area of research.

# 8 Conclusion

In this paper, we propose and test a new explanation for the weak relationship observed in previous literature between the severity of punishments and the responsiveness of crime. Using the standard model of crime in Becker (1968), we show that the responsiveness of crime to the severity of punishments depends on the probability of detection. Specifically, we propose that for severe punishments to have an effect on criminal behavior, the probability of detection has to be within an intermediate range, that is, not too high or not too low. We believe that the weak relationship found in previous work is accounted for by the fact that previous studies either failed to account for this interaction or focused on settings where the likelihood of detection was perceived to be very low or very high. We support this argument by providing empirical evidence from Chicago, Illinois using novel sources of variation in the severity of punishments and probabilities of detection.

Consistent with previous literature, we first show that if we ignore the interaction between harsh punishments and the probability of detection then our results are similar to previous findings. Point estimates from a naive specification in which we ignore the interaction produces results that are small and statistically insignificant. Our main results from an interacted specification show that more severe punishments do deter crime. Specifically, we find more severe punishments lead to a significant decrease in drug-related crimes in blocks where the probability of detection is in an intermediate range. In contrast, in blocks where the probability of detection is either high or low, we do not find any evidence to suggest that crimes respond to more severe punishments. Our main findings are supported by evidence from two sets of falsification tests and from a recent and not yet studied natural experiment from a state-level policy change that altered the size (area) of drug-free school zones. We find a pattern of results that is similar to our main findings: the effect of severe punishments is most evident in blocks with intermediate probability of detection and absent in blocks with relatively low or high probability of detection.

We conclude by performing a series of back-of-the-envelope calculations designed to estimate plausible ranges of the probability of detection for which offenders respond to severe punishments in our context. We find that this range can quite small. These results further highlight why it might be so hard to detect a significant effect of severe punishments in other settings. Moreover, our results underscore how policies that rely exclusively on severe punishments, without attention to the probability of detection, can be ineffective in deterring crime. We conclude by providing potential avenues for future research.

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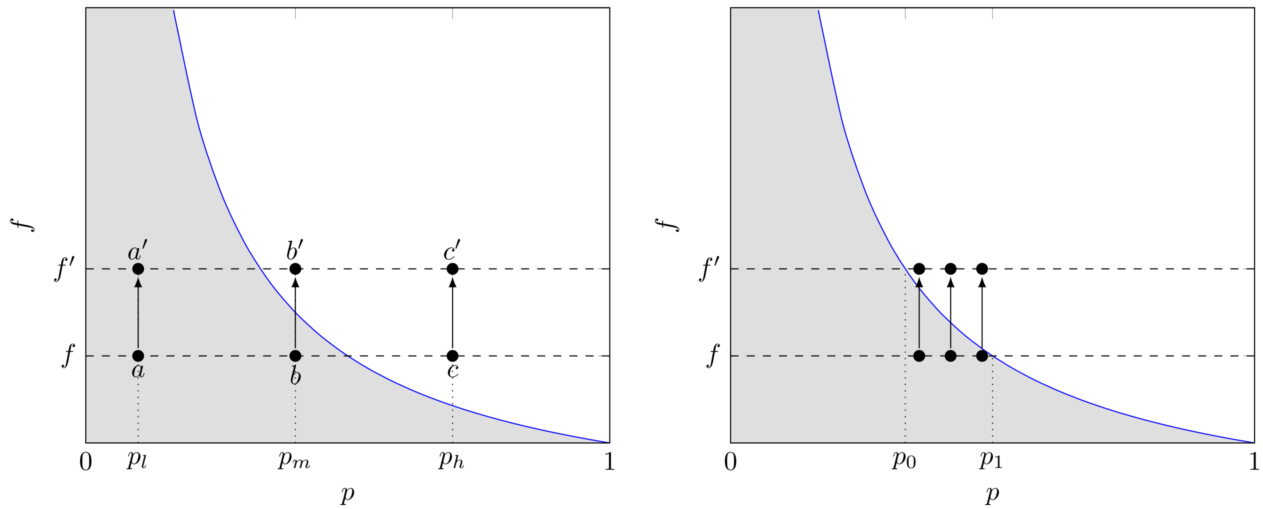
# Figures and Tables

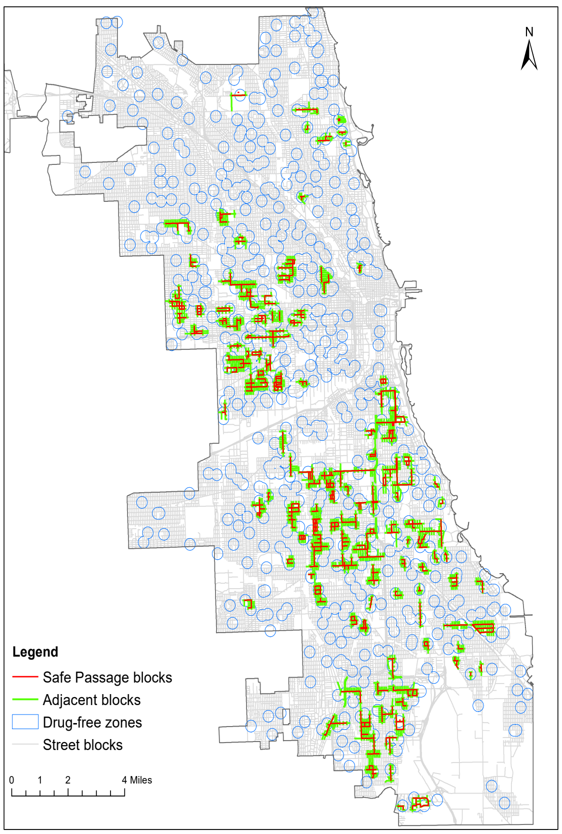



(a) Punishments and Probability of Detection

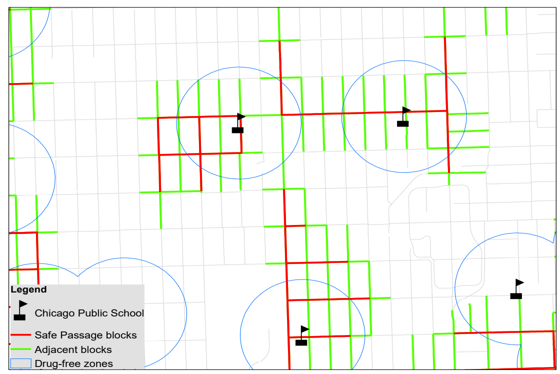
(b) Intermediate Probability of Detection

Figure 1: Punishments and Probability of Detection

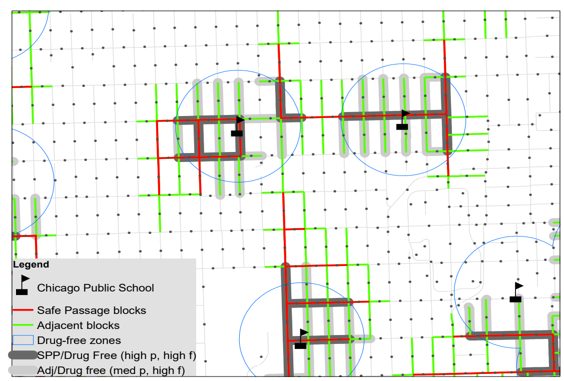
*Notes*: Calibration of Equation 1 assuming risk neutrality, an outside option $Y^0 = 0$, crime profit $Y = 1$, and a punishment matching crime profits (i.e., $f = 1$). $f'$ represents a doubling of the punishment $f$. $p_l$, $p_m$, and $p_h$ refer to a setting with low, medium, and high probability of detection, respectively. Blue line indicates the indifference points of the inequality in 1. "Intermediate Probability of Detection" refers to the range $p_0$ to $p_1$ for which a change (doubling) of fines leads to a change in the decision to commit the crime.




(a) Safe Passage Program (SPP) and SPP-Adjacent Blocks



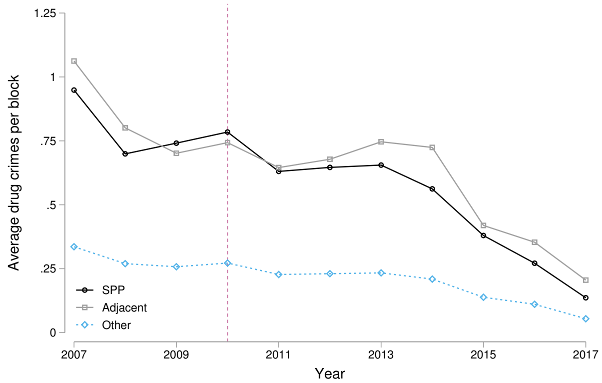

(b) Safe Passage Program (SPP) and SPP-Adjacent Blocks (close-up)



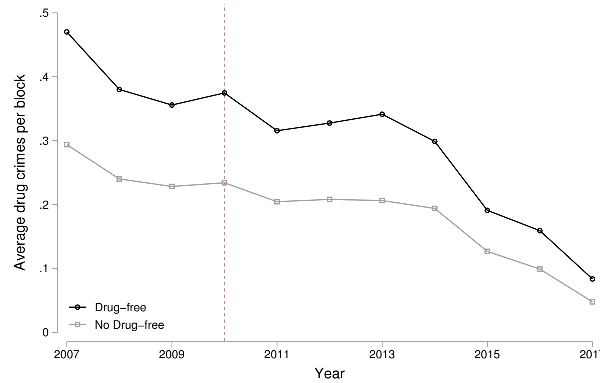

(c) Block Centroids and Treatment Assignment

Figure 2: Drug-Free School Zones, Safe Passage Program (SPP), and SPP-Adjacent Blocks

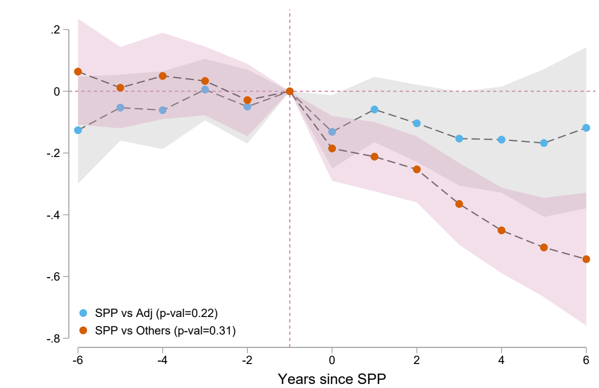
*Notes*: Drug-free school zones, Safe Passage Program (SPP) blocks, and SPP-adjacent blocks in Chicago during the 2016/17 school year.



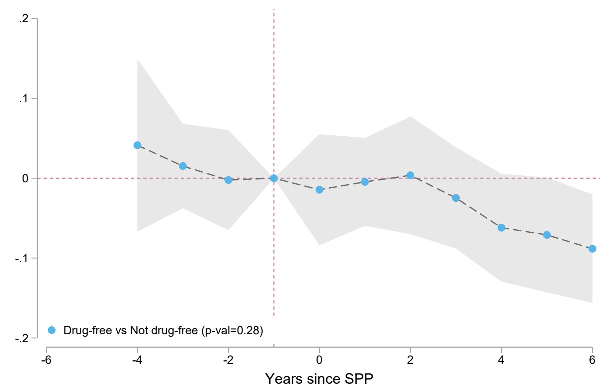

(a) SPP versus Other




(b) Drug-Free School Zone vs Not Drug-Free School Zone



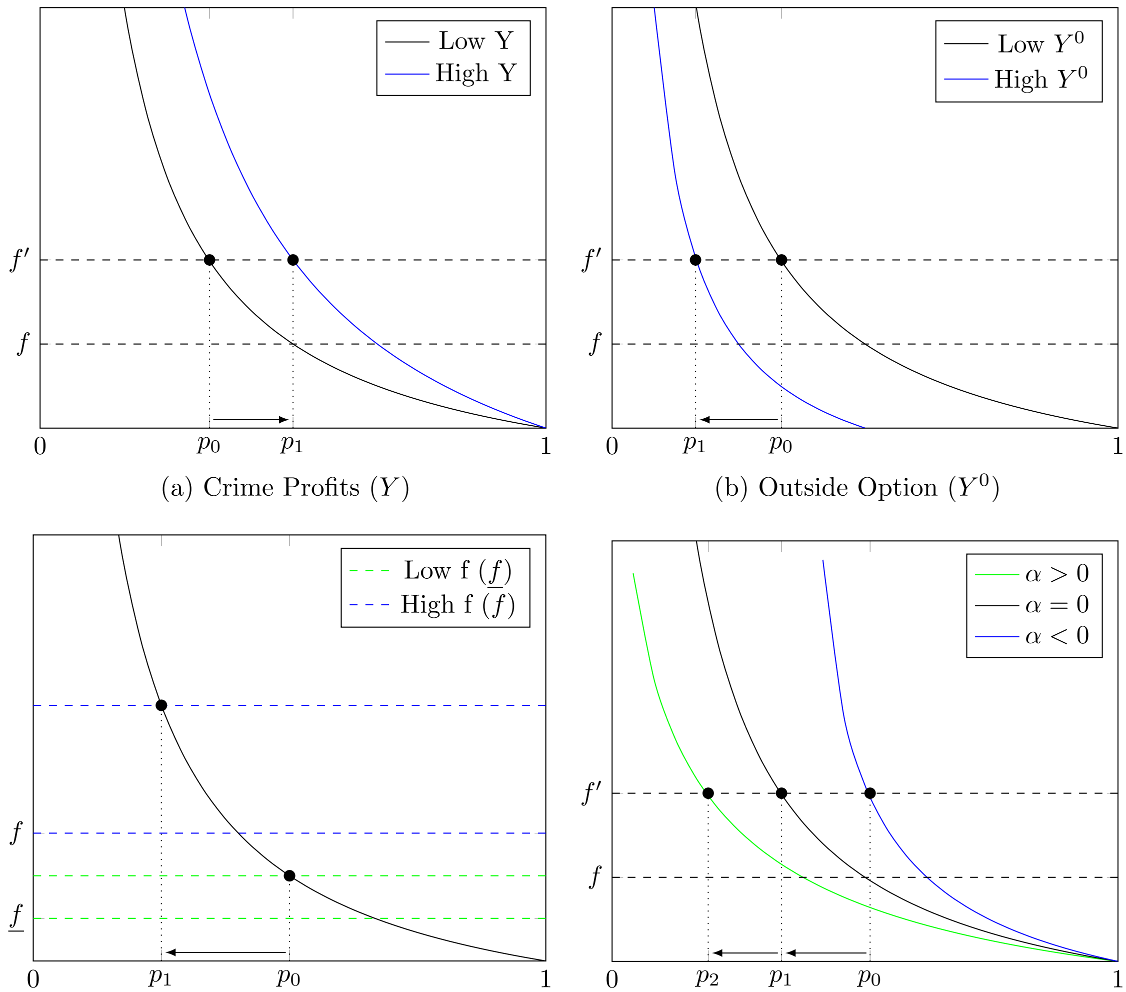

(c) Pre-trends: SPP versus Other




(d) Pre-trends: Drug-Free School Zone vs Not Drug-Free School Zone

Figure 3: Trends in Drug-Related Crimes, by Safe Passage Program (SPP) and Drug-Free School Zone Status

*Notes*: All figures restrict analysis to city blocks within 1,000 feet of Drug-Free School Zone boundaries. In Panels (a) and (b) the vertical dashed line denotes the 2009/10 school year (the school year before SPP introduction). Panels (a) and (b) present average annual drug crimes per block in SPP, SPP-adjacent, and other blocks (panel a) and Drug-Free School Zone/Not Drug-Free School Zone blocks (panel b). Panel (c) plots the coefficients from a regression using dummy variables for the number of years since implementation of the SPP. Specifically, we estimate: $Y_{bt} = \alpha + \sum_{g=0}^{6} \beta_{-g} S_{b,t-g} + \sum_{g=1}^{6} \beta_{+g} S_{b,t+g} + \delta_b + \lambda_t + \varepsilon_{bt}$, where $S_{b,t-g}$ equals 1 if block $b$ was covered by SPP community monitoring in year $t-g$ (lags) and $S_{b,t+g}$ equals 1 if block $b$ will be covered by SPP community monitoring in year $t+g$ (leads). We use the year prior to implementation as the reference year. SPP vs SPP-Adj in Panel (c) refers to analysis using SPP and SPP-Adjacent blocks. SPP vs Others in Panel (c) refers to analysis using SPP and blocks beyond SPP-Adjacent blocks but within the bandwidth of analysis (1,000ft). Panel (d) uses the 2010/11 school year as the "implementation" year and estimates the regression $Y_{bt} = \alpha + \sum_{g=-6}^{4} \beta_g D_{b,t+g} + \delta_b + \lambda_t + \varepsilon_{bt}$. P-values presented in graphs are for a joint test of significance for coefficients in pre-intervention period.




(a) Crime Profits ($Y$)

(b) Outside Option ($Y^0$)

(c) Baseline Fine ($f$)

(d) Risk Aversion ($\alpha$)

Figure 4: Effect of Changes in Crime Profits, Outside Option, Fines, and Risk Aversion

*Notes*: Panels (a)-(c) use calibration of Equation 1 assuming risk neutrality, an outside option $Y^0 = 0$, crime profits $Y = 1$, and a punishment matching crime profits (i.e., $f = 1$). High $Y = 2$. High $Y^0 = 0.5$. Low $f$=0.25. Curves indicate the indifference points of the inequality. Panel (d) uses an exponential utility function, $U(c) = \alpha^{-1}[1 - exp(-\alpha c)]$ with $\alpha \in [-1, 1]$ denoting the risk aversion parameter and $U(c) = c$ when $\alpha = 0$. $\alpha > 0$ denotes risk aversion, $\alpha < 0$ denotes risk loving, $\alpha = 0$ denotes risk neutrality. Calibration uses $\alpha = 0.5$ and $\alpha = -0.5$ for $\alpha > 0$ and $\alpha < 0$, respectively.

Table 1: Effects of Severe Punishments by Probability of Detection: 2007-2017

| | *Main Results* | | | *Falsification: Timing* | | | *Falsification: Other* | |
|---|---|---|---|---|---|---|---|---|
| | All Drug Offenses | | | Nights | Non-Att. Days | Weekends | Violent Crimes | Property Crimes |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Drug-free | -0.024 | -0.027 | -0.014 | -0.007 | 0.002 | -0.004 | 0.005 | -0.030 |
| | (0.033) | (0.037) | (0.023) | (0.017) | (0.005) | (0.015) | (0.009) | (0.037) |
| Adj. block | | -0.116** | -0.046 | -0.087** | -0.022 | -0.073** | -0.044** | -0.076 |
| | | (0.046) | (0.047) | (0.034) | (0.014) | (0.033) | (0.017) | (0.055) |
| SPP | | -0.279** | -0.199* | -0.138** | -0.049** | -0.152** | -0.065** | -0.149* |
| | | (0.106) | (0.107) | (0.067) | (0.022) | (0.058) | (0.030) | (0.076) |
| Drug-free × Adj. block | | -0.140** | -0.136** | 0.002 | -0.018 | 0.008 | -0.013 | -0.030 |
| | | (0.062) | (0.062) | (0.032) | (0.016) | (0.030) | (0.029) | (0.065) |
| Drug-free × SPP | | -0.017 | -0.005 | -0.020 | -0.001 | 0.063 | -0.066 | -0.093 |
| | | (0.108) | (0.115) | (0.078) | (0.023) | (0.063) | (0.046) | (0.092) |
| Mean | 0.197 | 0.197 | 0.197 | 0.183 | 0.057 | 0.152 | 0.150 | 0.705 |
| Linear trends | No | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 422174 | 422174 | 422174 | 422174 | 422174 | 422174 | 422174 | 422174 |

*Notes:* Drug-free is an indicator for whether a block is within a drug-free school zone area. SPP is an indicator for whether a block is a Safe Passage block (high probability of detection). Adj. block is an indicator for whether a block is adjacent to an SPP block. Columns (1)-(3) use total drug-related crimes at the block-year level occurring during school days and school hours. Columns (4)-(6) use total drug-related crimes at the block-year level occurring during nights, non-attendance days, and on weekends, respectively. Columns (7) and (8) use total violent and property crimes at the block-year level as outcome variables. All specifications control for distance to the drug-free school zone boundary, block fixed-effects and year fixed-effects, and include block-length weights. Standard errors are clustered at the neighborhood level and are in parentheses. There are 98 clusters. Sample restricted to blocks within 1,000 feet of drug-free school zone boundaries but more than 100 ft away from schools (to exclude crimes committed on school grounds). Stars signify: * p<0.10 ** p<0.05 *** p<0.01.

Table 2: Natural Experiment: Effects of Punishments by Probability of Detection

| | All Drug Offenses | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Drug-free | -0.002 | -0.001 | -0.001 |
| | (0.001) | (0.001) | (0.001) |
| Adj. block | | 0.008 | 0.007 |
| | | (0.018) | (0.018) |
| SPP | | -0.010 | -0.011 |
| | | (0.008) | (0.009) |
| Drug-free × Adj. block | | -0.009 | -0.008* |
| | | (0.006) | (0.005) |
| Drug-free × SPP | | 0.003 | 0.003 |
| | | (0.008) | (0.009) |
| Mean | 0.010 | 0.010 | 0.010 |
| Linear trends | No | No | Yes |
| Observations | 594170 | 594170 | 594170 |

*Notes:* Drug-free is an indicator for whether a block is within a drug-free school zone area. SPP is an indicator for whether a block is a Safe Passage block (high probability of detection). Adj. block is an indicator for whether a block is adjacent to an SPP block. Outcome variable is total drug-related crimes at the block-month level occurring during school days and school hours. All specifications control for block distance to the drug-free school zone boundary, block fixed-effects and month-year fixed effects, and include block-length weights. Standard errors are clustered at the neighborhood level and are in parentheses. There are 98 clusters. Sample restricted to blocks within 1,000 feet of drug-free school zone boundaries but more than 100 ft away from schools (to exclude crimes committed on school grounds). Stars signify: * p<0.10 ** p<0.05 *** p<0.01.

Table 3: Bounds for Intermediate Probability of Detection for Drug Possession Violations

| Violation | Drug Type | Quantity Used in Analysis | Value ($/g) | Maximum Punishment ($) | Punishment Used in Analysis ($) | Intermediate Probability of Detection |
|---|---|---|---|---|---|---|
| 10g or less | Marijuana | 10 | 14[a] | 200[d] | 100 | [0.41, 0.58] |
| 10g-30g | Marijuana | 30 | 14[a] | 1,500[e] | 750 | [0.22, 0.36] |
| 15g or less | Cocaine | 15 | 93[b] | 25,000[f] | 12,500 | [0.05, 0.10] |
| 15g or less | Heroin | 15 | 152[b] | 25,000[f] | 12,500 | [0.08, 0.15] |
| 15g or less | Methamphetamine | 15 | 70[c] | 25,000[f] | 12,500 | [0.04, 0.08] |
| 15g or less | Fentanyl | 15 | 80[a] | 25,000[f] | 12,500 | [0.05, 0.09] |

*Notes*: Quantity used in analysis is top quantity in definition of violation (column 1). Punishment used in analysis uses mid-value of maximum punishment in column (5). Price of Fentanyl is $40/pill with pill assumed to weight 0.5 grams. Intermediate probability of detection refers to the estimated range of the probability of detection for which a doubling of punishments leads offender to refrain from crime. Refer to section 7 for description of calculation of range of intermediate probability of detection.

[a] Soltesz and Bezrutczyk (2018)

[b] Heroin and Cocaine Prices in Europe and USA (UNODC, 2017)

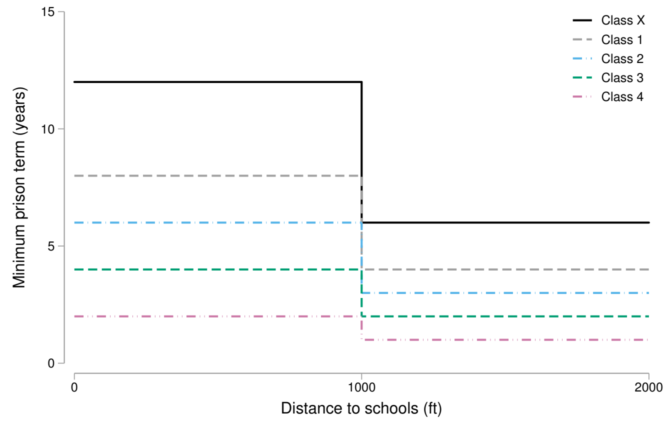
[c] National Drug Threat Assessment (DEA, 2018)

[d] Cannabis Control Act. 720 ILCS 550/4 (ILCS, 2019a)

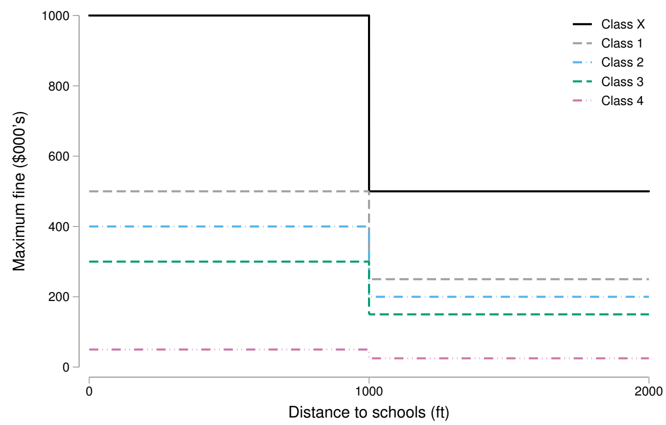
[e] Class B Misdemeanor: Cannabis Control Act. 720 ILCS 550/4 (ILCS, 2019a)

[f] Class 4 Felony: Illinois Controlled Substances Act. 720 ILCS 570/402 (ILCS, 2019b)

# Appendix A: Additional Figures and Tables




(a) Minimum Prison Term




(b) Maximum Fine

Figure A1: Prison Terms and Fines for Drug-Related Offenses by Distance to Schools

*Source*: Illinois Controlled Substances Act. 720 ILCS 570/407 ILCS (2020)
*Notes:* Graph uses the 1,000 feet radius for Drug-Free School Zones used prior to January 2018. After January 2018, the radius changed to 500 feet.

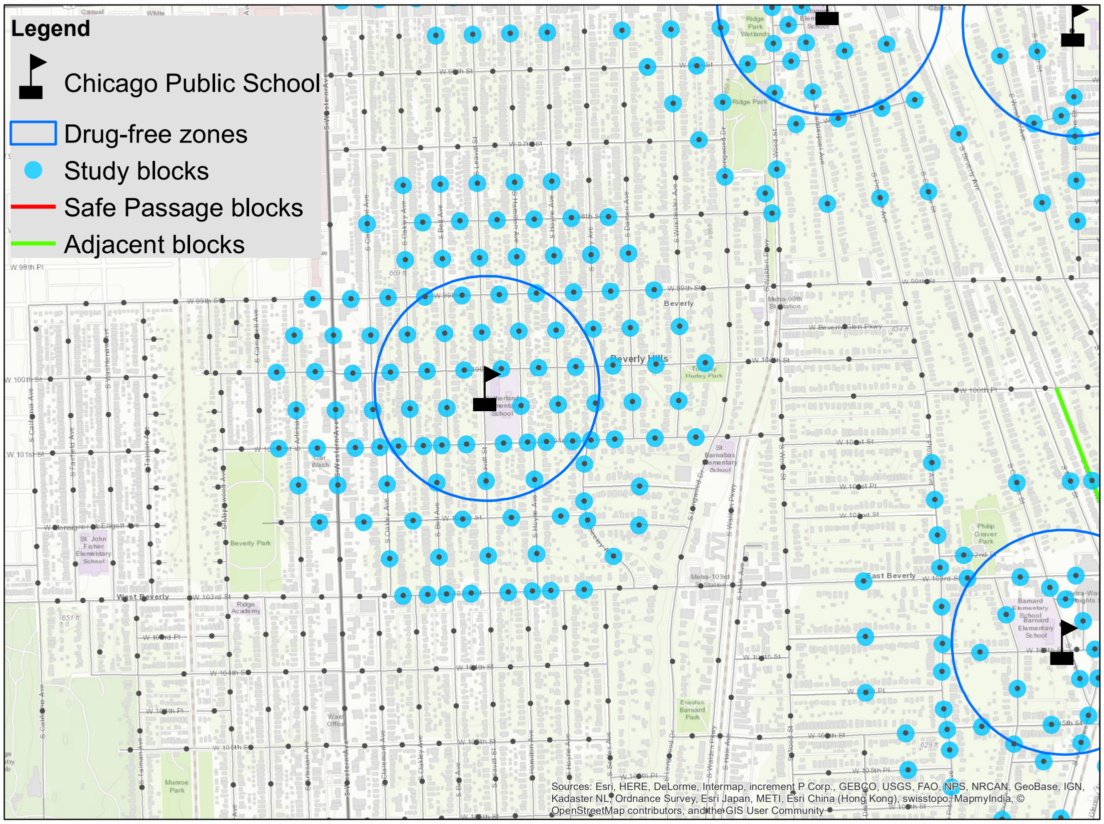
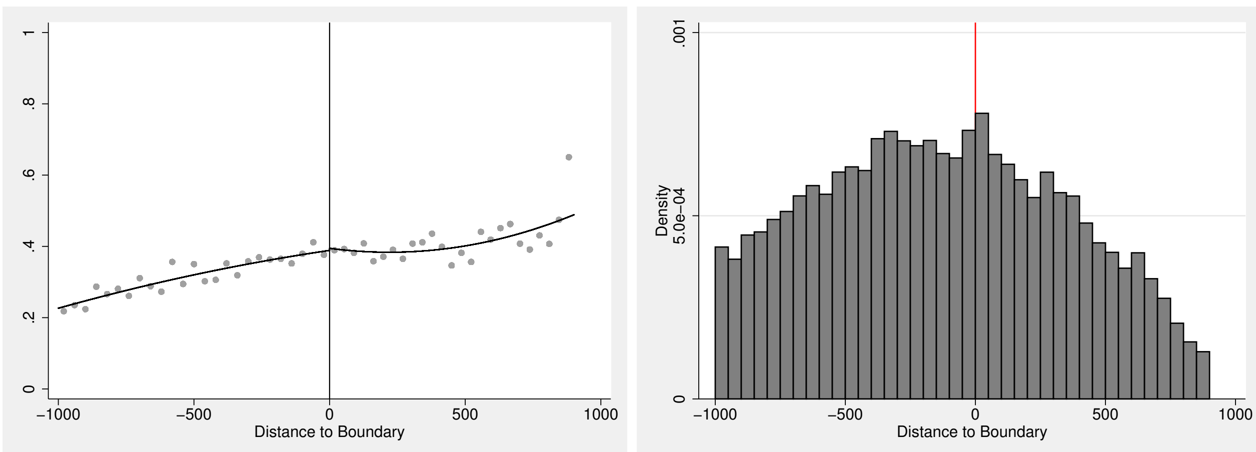



Figure A2: Sample of Blocks within 1,000 Feet of Drug-Free School Zone Boundary

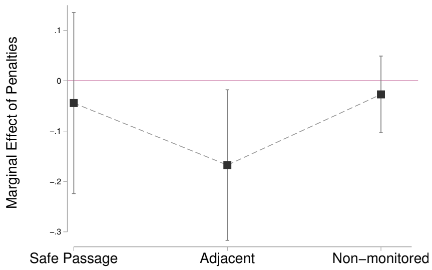
*Notes*: Grey dots indicate block centroids. Blue dots indicate blocks with centroids falling within 1000 feet of Drug-Free School Zone boundary. Blue line depicts the Drug-Free School Zone boundary.



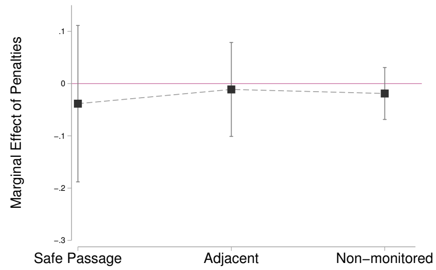

(a) Regression Discontinuity Plot (b) Histogram of Forcing variable

Figure A3: Regression Discontinuity Plot and Forcing Variable Histogram, Pre-Safe Passages

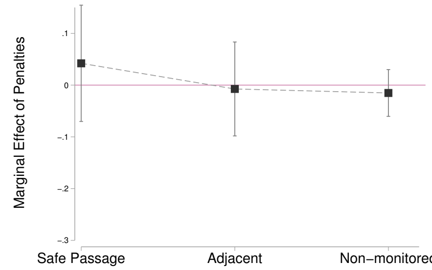
*Notes*: "Distance to Boundary" represents the closest distance from a block centroid to the boundary of a drug-free school zone. Vertical line at zero denotes the boundary of the drug-free school zone. "Positive" distances give the distance for blocks within the drug-free school zone while "negative" distances is for blocks outside of drug-free school zones. Solid dots in Panel (a) represent the mean of the outcome variable (total yearly drug crimes at the block level) for blocks within 50-foot distance bins. Trend lines are from a second degree polynomial in distance to the drug-free school zone boundary. Panel (b) presents a histogram of the forcing variable. Bin width in panel (b) is 50 feet.



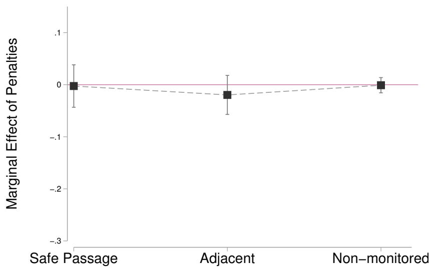

(a) Drug Crimes



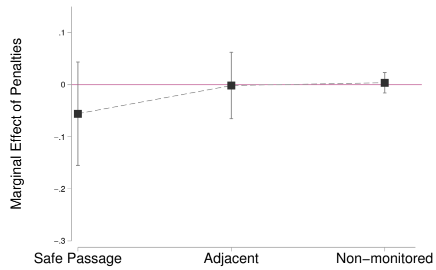

(b) Drug Crimes (Nights)



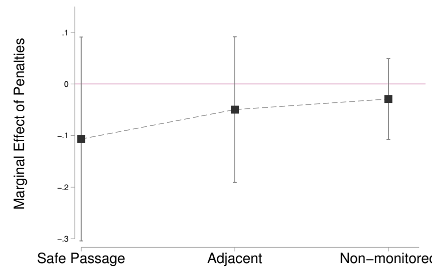

(c) Drug Crimes (Weekends)




(d) Drug Crimes (Non-Att. Days)




(e) Violent Crimes




(f) Property Crimes

Figure A4: Effect of Punishments by Probability of Detection

*Notes*: Margin plots for coefficients presented in Table 1. Spikes depict the 95% confidence interval for each coefficient estimate.

Table A1: Regression Discontinuity Estimates of the Effect of Drug-Free School Zones on Drug-Related Crimes

| | *Dep. Variable: Reported Drug Offenses* | | | | |
|---|---|---|---|---|---|
| | All years (1) | 2006-07 (2) | 2007-08 (3) | 2008-09 (4) | 2009-10 (5) |
| *Panel A: Local Linear Regression* | | | | | |
| Drug-free | 0.010 | -0.008 | 0.006 | 0.031 | 0.008 |
| | (0.15) | (0.20) | (0.13) | (0.14) | (0.13) |
| Bandwidth (ft) | 296 | 304 | 317 | 327 | 329 |
| Mean | .542 | .647 | .515 | .498 | .507 |
| Observations | 58,267 | 14,936 | 15,544 | 16,140 | 16,271 |
| *Panel B: Polynomial Regression with Wide Bandwidth* | | | | | |
| Drug-free | -0.001 | -0.039 | 0.023 | 0.004 | 0.007 |
| | (0.04) | (0.06) | (0.03) | (0.05) | (0.04) |
| Bandwidth (ft) | 950 | 950 | 950 | 950 | 950 |
| Mean | .474 | .544 | .450 | .446 | .456 |
| Observations | 148,288 | 37,071 | 37,071 | 37,073 | 37,073 |
| *Panel C: Polynomial Regression with Boundary FE* | | | | | |
| Drug-free | -0.015 | -0.053 | 0.012 | -0.010 | -0.008 |
| | (0.04) | (0.05) | (0.03) | (0.05) | (0.04) |
| Bandwidth | 950 | 950 | 950 | 950 | 950 |
| Mean | .474 | .544 | .450 | .446 | .456 |
| Observations | 148,288 | 37,071 | 37,071 | 37,073 | 37,073 |

*Notes*: Drug-free is an indicator for whether a block is within a designated drug-free school zone area. Outcome variable is total drug-related crimes at the block-year level occurring during school days and school hours. Analysis restricted to before 2010 to avoid changes in the effect of drug-free school zones due to the introduction of the Safe Passages Program (SPP). Local Linear Regression in Panel A uses triangular kernel and optimal bandwidth chosen as in Calonico et al. (2014). Choice of 950ft for the wide bandwidth is to take out crimes occurring on school grounds. Order of polynomial is two in Panels B and C. Choice based on RD plot fit. Standard errors are clustered at the neighborhood level (97 clusters). Boundary FE in Panel C adds a fixed effect for the drug-free school zone to ensure comparison of blocks across the boundary of the same drug-free school zone.

Table A2: Effects of Punishments by Probability of Detection: Subtypes of Drug-Related Crime, 2007-2017

| | Drug Possession Offenses | | | Drug Manufacturing Offenses | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Drug-free | -0.019 | -0.022 | -0.010 | -0.006 | -0.007 | -0.006 |
| | (0.027) | (0.030) | (0.019) | (0.008) | (0.009) | (0.006) |
| Adj. block | | -0.099*** | -0.040 | | -0.020 | -0.007 |
| | | (0.034) | (0.034) | | (0.017) | (0.016) |
| SPP | | -0.247** | -0.176* | | -0.034** | -0.021 |
| | | (0.098) | (0.097) | | (0.014) | (0.016) |
| Drug-free × Adj. block | | -0.120** | -0.115** | | -0.021 | -0.022 |
| | | (0.052) | (0.051) | | (0.015) | (0.015) |
| Drug-free × SPP | | -0.044 | -0.034 | | 0.021 | 0.023 |
| | | (0.102) | (0.107) | | (0.014) | (0.015) |
| Mean | 0.166 | 0.166 | 0.166 | 0.029 | 0.029 | 0.029 |
| Linear trends | No | No | Yes | No | No | Yes |
| Observations | 422174 | 422174 | 422174 | 422174 | 422174 | 422174 |

*Notes:* Drug-free is an indicator for whether a block is within a specified drug-free school zone area. Outcome variable in columns (1)-(3) is total drug possession crimes at the block-year level occurring during school days and school hours. Outcome variable in columns (4)-(6) is total drug manufacturing crimes at the block-year level occurring during school days and school hours. All specifications control for block distance to the drug-free school zone boundary, block fixed-effects and year fixed-effects, and include block-length weights. Standard errors are clustered at the neighborhood level and are in parentheses. There are 98 clusters.Sample restricted to blocks within 1,000 feet of drug-free zone boundaries but more than 100 ft away from schools (to exclude crimes committed on school grounds). Stars signify: * p<0.10 ** p<0.05 *** p<0.01.

Table A3: Effects of Punishments by Probability of Detection: Panel Poisson Estimates

| | *Main Results* | | *Falsification: Timing* | | | *Falsification: Other* | |
|---|---|---|---|---|---|---|---|
| | All Drug Offenses | | Nights | Non-Att. Days | Weekends | Violent Crimes | Property Crimes |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Drug-free | 0.033 | 0.019 | 0.046 | 0.059 | 0.034 | 0.067** | 0.033 |
| | (0.07) | (0.05) | (0.04) | (0.08) | (0.04) | (0.03) | (0.03) |
| Adj. block | | 0.172** | 0.047 | 0.173* | 0.080 | -0.009 | -0.032 |
| | | (0.07) | (0.06) | (0.10) | (0.06) | (0.04) | (0.04) |
| SPP | | -0.156 | -0.031 | -0.015 | -0.170* | -0.008 | -0.088 |
| | | (0.12) | (0.08) | (0.13) | (0.09) | (0.06) | (0.06) |
| Drug-free×Adj. block | | -0.204** | 0.071 | -0.149 | -0.003 | -0.013 | -0.029 |
| | | (0.09) | (0.09) | (0.13) | (0.09) | (0.05) | (0.04) |
| Drug-free×SPP | | 0.049 | -0.066 | -0.086 | 0.156 | -0.128* | -0.025 |
| | | (0.13) | (0.10) | (0.15) | (0.11) | (0.08) | (0.07) |
| Mean | .166 | .166 | .158 | .0483 | .13 | .13 | .13 |
| Observations | 170,372 | 170,372 | 187,238 | 101,845 | 167,816 | 198,991 | 314,641 |

*Notes:* Drug-free is an indicator for whether a block is within a specified drug-free school zone area. SPP is an indicator for whether a block is a Safe Passage block (high probability of detection). Adj. block is an indicator for whether a block is adjacent to an SPP block. Columns (1)-(3) use total drug crimes at the block-year level occurring during school days and school hours. Columns (4)-(6) use total drug crimes at the block-year level occurring during nights, non-attendance days, and weekends, respectively. Columns (7) and (8) use total violent and property crimes at the block-year level as outcome variables. All specifications control for distance to the drug-free school zone boundary, block fixed-effects and year fixed-effects, and include block-length weights. Standard errors are clustered at the neighborhood level and are in parentheses. There are 98 clusters. Sample restricted to blocks within 1,000 feet of drug-free zone boundaries but more than 100 ft away from schools (to exclude crimes committed on school grounds). Stars signify: * p<0.10 ** p<0.05 *** p<0.01.

Table A4: Effect of Punishments using Non-Closing Schools, Narrow Bandwidths

| | Non-Closing Schools | | Different Bandwidths | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Drug-free | | | -0.02 | -0.02 | -0.03 | -0.03 |
| | | | (0.04) | (0.04) | (0.04) | (0.04) |
| Adj. block | -0.07 | -0.00 | -0.11** | -0.11** | -0.12** | -0.14*** |
| | (0.06) | (0.06) | (0.05) | (0.05) | (0.06) | (0.05) |
| SPP | -0.30** | -0.23* | -0.26** | -0.26** | -0.27** | -0.30** |
| | (0.12) | (0.13) | (0.11) | (0.11) | (0.12) | (0.13) |
| Drug-free × Adj. block | -0.22** | -0.20** | -0.13* | -0.15** | -0.12* | -0.07 |
| | (0.09) | (0.09) | (0.07) | (0.07) | (0.07) | (0.05) |
| Drug-free × SPP | -0.03 | -0.00 | -0.02 | 0.00 | 0.03 | 0.11 |
| | (0.13) | (0.14) | (0.11) | (0.10) | (0.13) | (0.15) |
| Observations | 374,264 | 374,264 | 379,997 | 349,065 | 309,015 | 267,983 |
| Clusters | 98 | 98 | 97 | 97 | 97 | 97 |
| Linear Trends | No | Yes | No | No | No | No |
| Bandwidth (ft) | 1000 | 1000 | 800 | 700 | 600 | 500 |

*Notes*: Drug-free is an indicator for whether a block is within a specified drug-free school zone area. SPP is an indicator for whether a block is a Safe Passage block (high probability of detection). Adj. block is an indicator for whether a block is adjacent to an SPP block. All specifications use total drug crimes at the block-year level occurring during school days and school hours. All specifications include block and year fixed-effects and standard errors clustered at the neighborhood level. Columns (1) and (2) restrict the sample to blocks belonging to drug-free school zones of schools that did not close during the study period. Drug-free coefficient not identified due to block fixed effects (no variation in Drug-free status after dropping closed schools). Columns (3)-(6) use narrower bandwidths around the drug-free school zones boundaries. Linear trends refer to a neighborhood level year trend. Sample restricted to blocks within 1,000 feet of drug-free zone boundaries but more than 100 ft away from schools (to exclude crimes committed on school grounds). Stars signify: * p<0.10 ** p<0.05 *** p<0.01.

Table A5: Safe Passage Program (SPP) Monitoring Spillovers into Nearby Blocks

| | Drug Crimes (1) | Violent Crimes (2) | Property Crimes (3) |
|---|---|---|---|
| SPP | -0.221*** | -0.118*** | -0.209*** |
| | (0.059) | (0.022) | (0.072) |
| Adjacent | -0.150*** | -0.058*** | -0.091** |
| | (0.048) | (0.016) | (0.041) |
| Within 0.25 miles | -0.046 | -0.012 | -0.000 |
| | (0.034) | (0.009) | (0.040) |
| Within 0.5 miles | 0.001 | -0.004 | 0.012 |
| | (0.013) | (0.005) | (0.020) |
| Mean | .197 | .135 | .683 |
| Observations | 422,174 | 422,174 | 422,174 |
| Clusters | 98 | 98 | 98 |

*Notes*: Outcome variable in each column is total crimes at the block-year level occurring during school days and school hours."Adjacent", "Within 0.25 miles" and "Within 0.5 miles" refer to blocks that are adjacent, within 0.25 (but not adjacent), and within 0.5 miles (but farther than 0.25 miles) of SPP blocks. Control blocks are blocks beyond 0.5 miles of an SPP block. Standard errors are clustered at the neighborhood level. Sample restricted to blocks within 1,000 feet of drug-free zone boundaries but more than 100 ft away from schools (to exclude crimes committed on school grounds). Stars signify: * p<0.10 ** p<0.05 *** p<0.01.

# Appendix B: Data

## Crime Sub-Types in Drug Abuse Category

We obtained a list of all crime sub-types contained within the primary category of "Drug-Abuse" from http://gis.chicagopolice.org/clearmap_crime_sums/crime_types.html#N18.

- 1811 NARCOTICS POSS: CANNABIS 30GMS OR LESS
- 1812 NARCOTICS POSS: CANNABIS MORE THAN 30GMS
- 1821 NARCOTICS MANU/DEL:CANNABIS 10GM OR LESS
- 1822 NARCOTICS MANU/DEL:CANNABIS OVER 10 GMS
- 1840 NARCOTICS DELIVER CANNABIS TO PERSON ¡18
- 1850 NARCOTICS CANNABIS PLANT
- 1860 NARCOTICS CALCULATED CANNABIS CONSPIRACY
- 1900 OTHER NARCOTIC VIOLATION INTOXICATING COMPOUNDS
- 2010 NARCOTICS MANU/DELIVER:AMPHETAMINES
- 2011 NARCOTICS MANU/DELIVER:BARBITUATES
- 2012 NARCOTICS MANU/DELIVER:COCAINE
- 2013 NARCOTICS MANU/DELIVER: HEROIN(BRN/TAN)
- 2014 NARCOTICS MANU/DELIVER: HEROIN (WHITE)
- 2015 NARCOTICS MANU/DELIVER: HALLUCINOGEN
- 2016 NARCOTICS MANU/DELIVER:PCP
- 2017 NARCOTICS MANU/DELIVER:CRACK
- 2018 NARCOTICS MANU/DELIVER:SYNTHETIC DRUGS
- 2019 NARCOTICS MANU/DELIVER:HEROIN(BLACK TAR)
- 2020 NARCOTICS POSS: AMPHETAMINES
- 2021 NARCOTICS POSS: BARBITUATES
- 2022 NARCOTICS POSS: COCAINE
- 2023 NARCOTICS POSS: HEROIN(BRN/TAN)

- 2024 NARCOTICS POSS: HEROIN(WHITE)
- 2025 NARCOTICS POSS: HALLUCINOGENS
- 2026 NARCOTICS POSS: PCP
- 2027 NARCOTICS POSS: CRACK
- 2028 NARCOTICS POSS: SYNTHETIC DRUGS
- 2029 NARCOTICS POSS: HEROIN(BLACK TAR)
- 2030 NARCOTICS MANU/DELIVER:LOOK-ALIKE DRUG
- 2031 NARCOTICS POSS: METHAMPHETAMINES
- 2032 NARCOTICS MANU/DELIVER: METHAMPHETAMINES
- 2040 NARCOTICS POSS: LOOK-ALIKE DRUGS
- 2050 NARCOTICS CRIMINAL DRUG CONSPIRACY
- 2060 NARCOTICS FAIL REGISTER LIC:CONT SUBS
- 2070 NARCOTICS DEL CONT SUBS TO PERSON ¡18
- 2080 NARCOTICS CONT SUBS:FAIL TO MAINT RECORD
- 2090 NARCOTICS ALTER/FORGE PRESCRIPTION
- 2094 NARCOTICS ATTEMPT POSSESSION CANNABIS
- 2095 NARCOTICS ATTEMPT POSSESSION NARCOTICS
- 2110 NARCOTICS POS: HYPODERMIC NEEDLE
- 2170 NARCOTICS POSSESSION OF DRUG EQUIPMENT