

Crime and (a Preference for) Punishment: The Effects of Drug Policy Reform on Policing Activity*

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Abstract

We still know very little about the incentives of police. Using geocoded crime data and a novel source of within-city variation in punishment severity, I am able to shed light on enforcement behavior. I find that in parts of a city where drug penalties were weakened, there is a 13% decrease in drug arrests within a year; there is no displacement of non-drug offenses and majority black neighborhoods have a larger decline in drug arrests. If offenders were significantly deterred by harsher penalties, as the law intended and Becker's (1968) model predicts, there should have been an increase in drug arrests. My results are therefore consistent with police treating enforcement effort and punishment severity as complementary. I also find that citywide crime and drug use do not increase following the weakening of drug penalties. Taken together, my results call into question the "War on Drugs" view of punishment and suggest that certain types enforcement can be reduced without incurring large public safety costs.

Keywords: crime, enforcement, deterrence, punishment, enhanced penalty zones

JEL Codes: K40, K42, H40

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While criminal behavior is relatively well understood, we still know very little about the incentives of police (Doleac 2020). A primary challenge in researching this topic is that most of our proxies for crime are a function of both criminal activity *and* enforcement, which makes it difficult to disentangle police behavior from offender behavior. Moreover, criminal justice reform often changes policy uniformly for an entire state, leaving researchers with few options for valid comparison groups. I am able to address both of these challenges by using geocoded incident report data and exploiting within-city spatial and temporal variation in drug penalties near schools; these unique features allow me to study how police respond to changes in punishment severity.

All U.S. states apply some form of enhanced penalties for drug offenses committed near schools. The goal of these drug-free school zones (DFSZs) is to deter drug dealers and users from entering schoolyards with often mandatory minimum sentences; the same offense could carry a weaker punishment if committed just outside the enhanced penalty zone. While DFSZs are generally not salient to potential offenders (Ciarmella and Krisai 2018), they create non-linearities in punishment across space. This gives significant power to police, as they can expose offenders to additional risk for the same criminal activity, such as through undercover drug buys inside school zones. Moreover, despite being based on proximity to a school, DFSZ policies can result in enhanced penalties for offenses that do not involve children, take place outside of school hours, and are far from schools (Porter and Clemons 2013).¹ Advocacy work has therefore supported reforms seeking to shrink DFSZ sizes and remove mandatory minimum sentencing.

One of the few reforms occurred in Kentucky, where the enhancement zone distance around every school in the state shrank from 1000 yards to 1000 feet in March 2011; Porter and Clemons (2013) indicate that the original buffer size was a mistake, as all other states impose a zone measured in feet rather than yards.² By exploiting these exogenous spatial changes in punishment for drug sales in Kentucky, I can examine both 1) the effects of DFSZs on reported crime and 2) the impact of lowering sanctions on the tendency of police to arrest drug offenders. The sudden change in area covered by DFSZs helps overcome a significant challenge that arises when studying the complex relationship between punishment severity, criminal activity, and policing. Because the reform in Kentucky removed penalty enhancements from only certain parts of every city in the state, I can separate enforcement from criminal activity.

Using a difference-in-differences strategy for Kentucky's largest city, Louisville, I find a 13% decrease in drug crime incidents in "de-zoned" census blocks (those that lost their penalty enhancement) relative to those that had no change in their DFSZ status; note that 94% of reported drug incidents in Louisville end in arrest.³ This decrease stems from fewer arrests for possession and paraphernalia offenses, which carry no penalty enhancement in Kentucky.

¹Additionally, defendants may face two distinct penalties for a single drug offense (Porter and Clemons 2013).

²Google Trends analysis in Section A.3 shows little change in search behavior for relevant terms.

³No other city in Kentucky has incident-level or geocoded crime data. The control group combines areas that always had a penalty enhancement (within 1000 feet of schools) and those that never had an enhancement (greater than 1000 yards of schools). Additionally, using a different dataset for Louisville, which contains no geographic information and cannot be linked to the primary dataset, I find that approximately 94% of drug crime incidents end in an arrest both pre- and post-reform. Lastly, the majority of drug crime incident reports in Louisville are officer-initiated.

Only drug sales receive a harsher punishment if committed near schools, which suggests that the results are driven by changes in enforcement behavior.

The effects are negative and large one year after the reform, but dissipate thereafter and are unique to drug crimes; while there was an increase in some non-drug offenses in those de-zoned areas, the results are quite imprecise and I cannot reject the null of no effect. I do, however, find that there were fewer drug arrests in census blocks with a larger share of black residents after the reform, regardless of whether these areas were de-zoned or not. Taken together, these findings suggest that police shifted their use of discretion away from black neighborhoods, but that they did not become more effective at deterring other types of crime. Instead, the reform seems to have led to a targeted but temporary slowdown of enforcement effort, similar to the findings of Bohn et al. (2015) and DeAngelo and Owens (2017).⁴

Enhanced penalty zone policies often focus solely on deterrence, and are designed without allowing for enforcement effects. Becker's (1968) model predicts an *increase* in criminal activity in the formerly enhanced penalty areas, as offenders should move to places with weaker punishments; enforcement is implicitly assumed fixed. If harsher punishments instead have little or no deterrence effect, as the literature suggests, criminal activity should not move. Since I find empirically that reported drug crime *decreased* in areas that lost their DFSZ status, I conclude that police respond to weaker penalties with a reduction in enforcement: there was a differential decrease in drug arrests but seemingly no shift in the geographic distribution of patrolling, as there was no significant displacement of non-drug offenses. The conceptual framework in the following section formalizes this conclusion by drawing parallels between the labor supply decision and the enforcement decisions of police.

This paper adds to the literature on the responsiveness of crime to punishment severity; studies examining non-linearities in punishments that arise from age-based classifications (e.g., Hansen and Waddell 2014; Lee and McCrary 2017) and capital offenses (e.g., Hjalmarsson 2009; Kovandzic et al. 2009) similarly find little empirical support for the deterrence effect of harsher penalties.⁵ However, there are several potential costs associated with DFSZs. First, a significant amount of discretion is inherent in policing drug crime, as, for example, the margin between drug possession and intention to sell is often ill-defined. Ba et al. (2020) find that police with poor misconduct records are more likely to find or pursue drug crimes, while those with better misconduct records pursue more property and violent crimes, which often require an investigation. Greater police presence can also have significant negative impacts on the educational outcomes and psychological well-being of students (Owens 2017; Weisburst 2019; Ang 2021). Since DFSZs disproportionately cover densely populated urban areas, the concern is that this burden falls mainly on low-income and minority residents (Greene et al. 2006). Lastly, the use of enhanced sentencing has contributed to the growth in the U.S. prison population

⁴Automatic Vehicle Locator (AVL) data for Louisville are not available, but discussions with a former lieutenant and officer summarized in Section 1.1 provide some context. The officer described the weakening of drug penalties as “kneecapping” the police, as it reduced their ability to do their job. The lieutenant stated that DFSZs provided a lot of “bang for your buck,” as they allow police to give the harshest penalty possible. Both agreed that removing this enforcement mechanism was frustrating, and the lieutenant suggested that this displeasure likely led to a temporary “subconscious slowing down” of enforcement. They returned to their baseline enforcement after “regrouping and reassessing,” as no additional reforms were made.

⁵Gonzalez et al. (2020) find that crime drops in areas where punishments are more severe, but only if the monitoring intensity is at intermediate levels.

(Raphael and Stoll 2013).

This paper also contributes to a growing literature that finds that police can often reduce marginal enforcement without incurring public safety costs. Chandrasekher (2016) observes large decreases in misdemeanor and traffic ticket-writing during a long police slowdown in New York City that led to only minor changes in three categories of crime. Sullivan and O’Keeffe (2017) find that reported crime decreased during a shorter slowdown in New York City, which they suggest led to an increase in the quality of life for those living in disproportionately targeted communities. Cho et al. (2021) find that a different source of police slowdowns, line-of-duty deaths of officers, led to short-term but significant reductions in arrests of all types. However, the authors similarly find no evidence of an increase in crime. Lastly, Owens et al. (2018) find that an intervention aimed at slowing down police decision-making processes led to a reduction in arrests but did not lead to citywide crime increases.

Existing work has focused mainly on criminal incentives, yet we still know little about what drives police behavior. There are, however, two related papers that find that changes to criminal justice policy can cause a temporary reduction in enforcement. The first is by Bohn et al. (2015), who use spatial variation in expected penalties for immigration violations to help shed light on otherwise unobserved changes in police behavior. In particular, they determine that police changed arresting behavior in response to the immigration reform. The second is DeAngelo and Owens (2017), who use speeding ticket data linked to law changes for traffic violations to show that after a reform, some troopers are less likely to issue citations; the effect is temporary and driven by less experienced officers.⁶

Similar to Bohn et al. (2015) and DeAngelo and Owens (2017), I observe that changes to punishment severity can temporarily affect the behavior of law enforcement. More specifically, I find that drug arrests decreased following the weakening of drug penalties in certain parts of the city. I also find that both citywide non-drug offenses and drug overdose hospitalization trends remained unchanged. Therefore, two major concerns associated with weakening the penalties for drug crime, that criminal activity and drug use would increase, are not substantiated in this setting. Put differently, my findings suggest that certain types enforcement can be reduced without incurring large public safety costs.

1 Conceptual Framework

1.1 Conversations with a Lieutenant and an Officer

This subsection helps motivate both the research questions and the conceptual framework (Section 1.2), as well as provide context on policing activity. In January 2021, one former lieutenant and one officer responded to my request for information on the impacts of the 2011 criminal justice reform. The officer described the weakening of drug penalties as “kneecapping” the police, as it compromised their authority and reduced their ability to do their job. The lieutenant stated that DFSZs provide good “bang for your buck,” as they provide an opportunity to give the harshest penalty possible. While officers know where schools are, both stated that the moti-

⁶There is also a literature on how fiscal incentives can distort law enforcement behavior, such as Harvey (2020), Makowsky et al. (2019), and Mas (2006).

vation for using DFSZ enhancements was not necessarily to protect children, and that removing this mechanism was frustrating. The lieutenant stated that displeasure with the reform may have led to a “subconscious slowing down” of enforcement. They returned to their baseline behavior after “regrouping and reassessing,” as no additional reforms occurred.

With regards to what police did with the excess “time” that may have freed up with the weakening of drug penalties, the officer stated that they still had the same amount of work and that drug possession, paraphernalia, and sales remained crimes. Furthermore, they both stated that the department already spent a significant amount of resources investigating more serious crimes. Therefore, it would be unlikely to see any changes to non-drug crimes.

1.2 Theory and Predictions

I first present some background information on the standard theory of criminal behavior, then I transition into what helps frame and interpret the empirical results in the following paragraph. According to Raphael and Tahamont (2017), if the use of enhanced sentencing affects crime, one channel is through the deterrence of individuals from committing an offense in the first place.⁷ This conclusion arises from standard models of criminal behavior, such as Becker (1968). Suppose that Y_C is the gain from a successful crime, p is the probability of being apprehended, f is the extent of punishment or fine, and Y_L is the earnings from legitimate work, then these models suggest that an individual chooses to commit a crime if $pU(f) + (1-p)U(Y_C) > U(Y_L)$, where $U(\cdot)$ is the utility function. This setup treats p and f as substitutes: in order to keep criminal activity fixed, decreasing the penalty for a given crime needs to coincide with an increase in the probability of apprehension. However, the empirical research examining criminal deterrence has demonstrated that the perceived probability of punishment p is a much stronger deterrent than perceived severity of punishment f (Abel 2013; Chalfin and McCrary 2017).⁸

While the decision of a potential offender has been examined thoroughly since Becker (1968), there has been limited discussion on what drives police behavior.⁹ I therefore focus on modeling the enforcement decision, where I use the labor supply model as the primary framework. However, instead of labor, leisure, and a wage, there is enforcement of drug offenses, other policing duties, and punishment severity f , respectively. The drug policy reform in Kentucky weakens penalties for drug offenses committed in certain parts of a city, and thus I consider how a decrease in punishment severity affects the decision to enforce there.

⁷Chalfin and McCrary (2017) suggest that there is little empirical support for other channel, the incapacitation effect, which is mechanical and arises from removing criminals from the general population.

⁸To better understand this finding, Polinsky and Shavell (1999) and Lee and McCrary (2009) extend Becker’s framework to include alternative time preferences. They conclude that the behavior of present-biased individuals is often unresponsive to changes in the intensive margin of punishment, as the additional punishment generally occurs in the future. Note, however, when we test these models empirically, we only observe a subset of all criminal activity and cannot see many changes in police behavior. Therefore, we often implicitly assume that the most common proxies for crime, data on arrests and police incident reports, are a function of the two and are increasing in both criminal activity and the detection of that activity. Put another way, when we observe more arrests or crime reports, this can be attributed to either an increase in criminal activity or an increase in police enforcement. If both move simultaneously but in opposite directions, then an increase in arrests or crime reports results from one effect dominating the other.

⁹The exception is Owens (2020), who describes “optimal” policing as a tradeoff between what police produce, broadly defined as public safety, and the costs associated with that public safety provision. She further states that policing scholars have frequently referred to this “impossible task” as the warrior-guardian tradeoff, where officers must be warriors against crime as well as guardians who protect and defend potential victims.

When examining the impacts of the DFSZ reform on police behavior, one must consider two offsetting effects. The first is a substitution effect, as when f decreases in de-zoned areas, officers cannot punish criminals as severely and thus may substitute towards other duties, such as patrolling or administrative work. This behavior is consistent with police officers wanting to catch criminals and punish them as severely as possible; while it may be unclear why individual police officers should care what the punishment is for a given crime, people who select into being officers may have a taste for punishment (Niskanen 1968). I define the substitution effect as “punitive” for simplicity, but it may also be driven, for example, by police departments having budgets, and thus wages, tied to arresting behavior. Alternatively, officers may be taking their cue from policymakers as to what is important to enforce and what is not. Empirically, if this effect dominates, then following the reform, there would be a decrease in drug arrests in de-zoned areas. In the labor supply model, this would be similar to a decrease in wages leading to fewer hours worked.

The second is the “income” effect of a decrease in f : enforcing the law to the same extent as before the reform would provide a lower level of deterrence. Therefore, a police officer must increase their level of enforcement in order to maintain the previous level of criminal activity. If this effect dominates, a decrease in punishment severity for drug offenses would lead to an increase in drug arrests. Note that this effect is more consistent with Becker’s (1968) framework, where punishment severity and enforcement (the probability of apprehension) are substitutes. In the labor supply framework, this would be similar to a wage decrease leading to more hours worked in order to compensate for lost income.

As the primary goal of this paper is to determine how police respond to changes in punishment severity, which is an open empirical question, I summarize the predictions of this section in Table 1. Potential offenders either do not respond to changes in punishment severity (A), as the literature suggests, or if they do, their response would follow from standard models of criminal behavior (B); for example, Becker (1968) would predict more drug offenses committed in de-zoned areas after the reform, as the opportunity cost of committing a crime decreased there. The other relevant component is which preference or effect dominates for police. As seen in Table 1, if we observe a decrease in arrests following the reform, we can rule out the fixed level of deterrence hypothesis. A decrease in arrests would therefore suggest that the substitution effect is stronger, where either criminals do not respond (A), or the police response dominates any criminal behavior change (B) (as they move in opposite directions).¹⁰

Table 1: Empirical Predictions for Drug Arrests in De-Zoned (“Treated”) Areas

Behavior	Police Response to Punishment ↓	Criminal Behavior	
		(A) Fixed Predictions	(B) Becker (1968)
(SE) Punitive	↓ enforcement	↓ arrests in de-zoned areas	ambiguous
(IE) Fixed Level of Deterrence	↑ enforcement	↑ arrests in de-zoned areas	↑↑ arrests in de-zoned areas

¹⁰Police could also technically not change their behavior, which would be similar to the IE effect in terms of outcomes: we would observe an increase in arrests in (B), but instead no effect on arrests in (A).

2 Background and Empirical Strategy

DFSZ laws were first enacted in the 1980s to protect children from exposure to drugs. They can vary by state in terms of zone size, the locations they surround, offenses covered, and penalties imposed, but all states impose harsher punishments for drug offenses committed near schools.¹¹ The zones typically extend for a fixed linear distance around a school and thus create a spatial non-linearity in punishment severity at the zone's boundary. Over the past few decades, some states have expanded the sizes of these zones and included additional landmarks, such as parks, libraries, and public housing, as a way to limit problems associated with drug abuse (Porter and Clemons 2013). The result is that large areas of cities are now covered by DFSZs.¹²

Other states have reduced the size of their drug-free school zones, such as Kentucky, where the distance around every school in the state went from 1000 yards (3000 feet) to 1000 feet in March 2011. This change was part of a larger criminal justice reform (HB 463), which is generally how DFSZ laws are adjusted. The bill included reforms to pretrial detention and bail, probation and parole, and jail oversight. While the sentencing guidelines for possession of a small amount of drugs were reduced and the classification guidelines for repeat drug law violations were changed, drugs remained illegal, and all of the aforementioned changes occurred for the entire state. Put differently, there was no differential change to punishments for DFSZs, and the sale of drugs remains the only crime with a DFSZ penalty enhancement.¹³ The only element that changed with the reform was where the enhancement was enforced.

In order to understand the dynamics associated with the component of the reform that induced variation in punishment across space, I utilize geocoded crime report data from Kentucky's largest and most populated city, Louisville; no other city in the state has incident-level or geocoded crime data. These data are available from 2007 from the Louisville Metro Police Department (LPMD). They include information such as the crime description, date of occurrence, premise type, and address of the incident. I aggregate these data to the census block for the empirical analysis, which is described in further detail in Section 2.1.¹⁴

Panel A of Figure 1 shows DFSZ coverage in Louisville, where 72% of the city was covered by DFSZs until March 2011 and approximately 15% after. Panel B of Figure 1 presents the distributions of drug crime distances to schools and DFSZ boundaries. The left figure of Panel B suggests that drug incidents did not significantly respond to the reform, as the distribution of distances to schools is similar one year before and after it, but that there was a slight shift away from the 1000 to 3000 foot zone. However, the right figure of Panel B shows that offenders face significantly less risk due to the loss in eligibility for harsher punishment across space after the reform. Put differently, drug crime incidents remain a similar distance away from schools, which remain fixed during the sample period, but because the DFSZ boundaries shifted inwards, these

¹¹Figure A.1.1 shows a US map of enhanced penalty zone sizes. See Porter and Clemons (2013) for a detailed description of DFSZ laws by state.

¹²Figure A.1.2 shows cities that are the most covered by DFSZs.

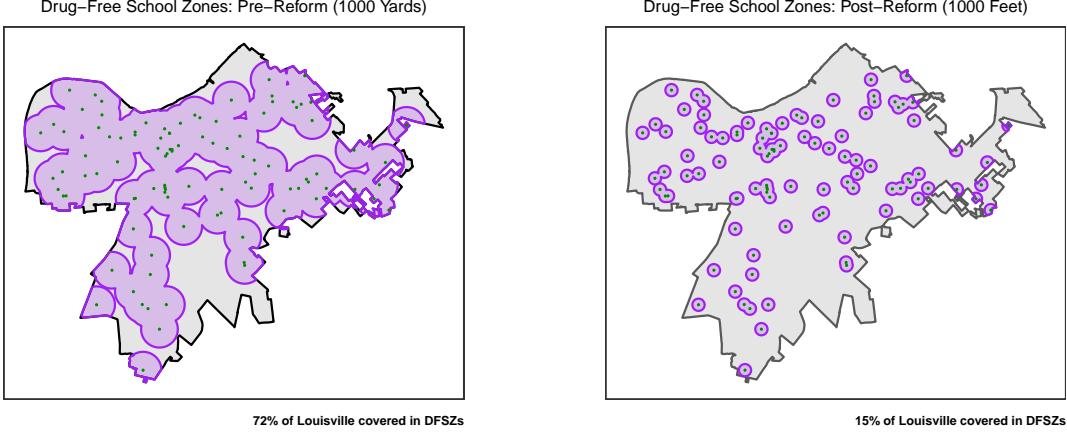
¹³The DFSZ punishment is Class D felony for all drug sales within school zone, but varies depending on quantity and whether it was your first offense if outside a school zone. In all parts of the state, paraphernalia and possession are Class A and B misdemeanors, respectively; more details are in Table A.1.1.

¹⁴Table A.1.2 provides summary statistics for Louisville. Drug crime locations and common violations can be found in Table ???. Data on schools and shapefiles were obtained through Louisville's Open Data Portal.

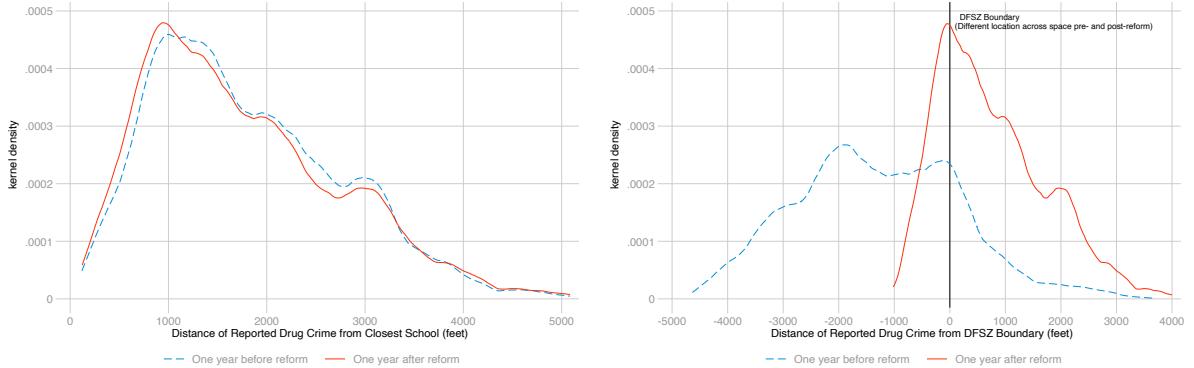
incidents are now closer to a DFSZ border.¹⁵

Figure 1: Exposure to Changes in Drug-Free School Zones

Panel A: Drug-Free School Zones in Louisville



Panel B: Distributions of Drug Crime Distances to Schools and DFSZ Boundaries in Louisville



Panel A Notes: Green dots capture all private and public schools in Louisville. In the left figure, the purple buffers capture the borders of drug-free school zones prior to March 2011, which were 1000 yards, while in the right figure, the purple buffers capture the 1000 foot zones enacted after March 2011. Drug crime heat maps can be found in Figure A.1.5.

Panel B Notes: The raw incident data is used in these figures, where the distances represent author's calculations; the underlying histograms are transparent and kernal densities used instead for ease of visualization. Negative values in the right figure of Panel B capture offenses committed within the DFSZ boundary while positive ones are for those outside the DFSZ. Note that because of overlapping boundaries, distances can be greater than 1000 yards pre-reform (or 1000 feet post-reform) within a school zone. Furthermore, in the right figure, the 0 (black vertical line) is normalized pre- and post-reform, but note that they represent different locations across space. For example, a crime committed 500 feet *outside* the DFSZ border post-reform would have been approximately 1500 feet *inside* the DFSZ border pre-reform.

2.1 Empirical Strategy

The goal of this paper is to examine criminal activity and enforcement behavior. The main empirical strategy is a difference-in-differences (DID) framework for Louisville that exploits temporal and spatial changes in punishment for drug sales created by DFSZs in Kentucky. I also perform three complementary empirical analyses to check stability of the main results. The first utilizes an analogous DID strategy but separately moves the change in policy to one

¹⁵ DFSZ coverage for the entire state is in Figure A.1.3, while monthly crime trends for Louisville are in Figure A.1.4 and Table A.1.3.

year earlier, the second uses random locations instead of actual school addresses, and the third sequentially removes fixed effects; the results from these tests are in the Online Appendix and support the main results presented in Section 3.

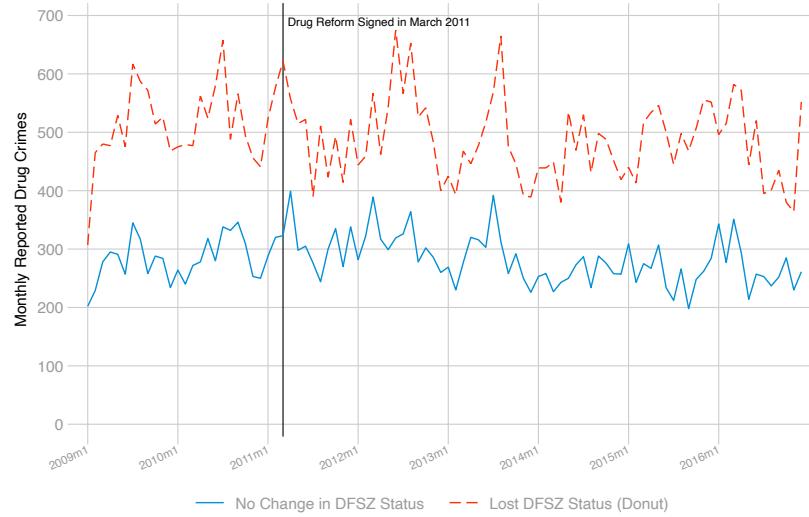
In order to examine how reported crime changed in Louisville following the passing of the DFSZ reform, I estimate

$$Y_{bt} = \gamma_b + \tau D_{bt} + \lambda_t + \epsilon_{bt}, \quad (1)$$

where Y_{bt} is the count of crimes for census block b in time t , D_{bt} is a dummy variable equal to one if census block b is in an area that lost its DFSZ status after the reform, and γ_b and λ_t are census block and year fixed effects, respectively.¹⁶ Census blocks are the preferred unit of analysis because they allow for a straightforward geographical treatment assignment.¹⁷

To highlight both the seasonality of drug crime and parallel pre-trends, Figure 2 presents the time series of monthly reported drug incidents.¹⁸ An alternative to the visual inspection of the parallel trends assumption is Callaway and Sant'Anna's (2020) pre-trend test, where I obtain a p-value of 0.336. This implies that the parallel trends assumption holds in pre-treatment periods, but it does not actually tell you if it holds in the current period.

Figure 2: Monthly Drug Crime Trends by DFSZ Status Change



Notes: This figure captures monthly totals for drug crime based on where it was reported, either in an area that lost its DFSZ status after the reform or in one that had no change to its DFSZ status.

Figure 3 helps illustrate the treatment assignment and empirical strategy. Every school in Kentucky (black square at the center of the figure) had a 1000-yard buffer around it (solid circle) prior to March 2011 that enhanced penalties for drug sales. After March 2011, the zone

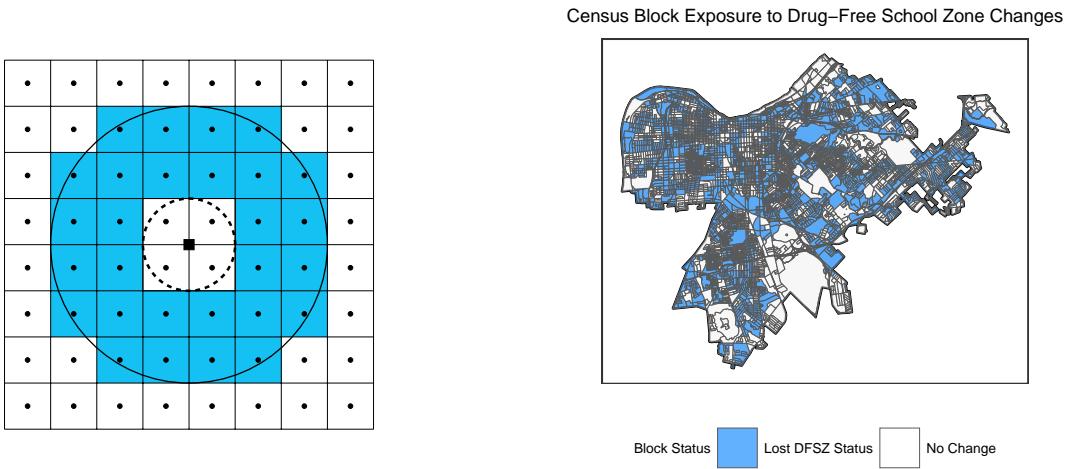
¹⁶Given that there is a point-in-time treatment, there is no need for any staggered treatment adoption DID estimators.

¹⁷Aggregating all crimes committed within de-zoned areas and those that had no change to their DFSZ status would produce a mechanical change, as the de-zoned areas are considerably larger and thus more arrests are made there pre-reform.

¹⁸The remainder of the paper utilizes yearly block-level data for the sake of consistency and clarity, but all monthly-level regression results arrive to the same conclusions; the relevant ones are in the Online Appendix.

size decreased to 1000 feet (dashed circle). The inner white area always had enhanced penalties for drug sales and the outer white never had any enhanced penalties, and thus only the shaded blue section had any change in their exposure to the DFSZ law (lost DFSZ status). Therefore, the parameter of interest from equation (1), τ , captures the net effect on reported crime of blocks losing their DFSZ status (light blue areas) after the reform, relative to the blocks that had no change in their DFSZ status (the white areas).¹⁹ Note that individual census blocks are relatively small and are classified as being in each zone based on their centroid, but as a robustness check, I also use the percentage of the census block that is within a zone and keep census blocks that are either 50% within or completely within a given zone.

Figure 3: Block-Level Treatment Assignment for the Empirical Analysis



Notes: In the left figure, the black square at the center is a school, where the solid circle is the 1000-yard pre-reform boundary and the dashed circle is the 1000-foot post-reform boundary. The small circles represent the centroid of a census block. In Louisville, 20% of all census blocks are within 1000 feet of schools (white “donut hole” region), 63% in the 1000 foot to 1000 yard region (light blue or “donut” region), and 17% are in the greater than 1000 yard region (outer white region). The right figure shows what this treatment assignment looks like for the entire city, where lightly shaded gray lines capture Louisville’s approximately 4000 census blocks. Figure A.1.6 zooms in on one school in Louisville to show what actual treatment assignment looks like.

2.2 Impacts of the DFSZ Reform on Drug Overdoses

A concern with reducing the penalties for drug offenses is that it may lead to an increase in drug use, possibly by weakening the incentive for sellers to be secretive or by removing the stigma associated with drug use. Obtaining information on actual drug use is generally not possible and thus one common proxy is drug overdoses. To test whether there was any change in drug use after the DFSZ reform, I utilize overdose hospitalization records for Kentucky residents treated in Kentucky acute care hospitals. I find no significant increase in overdose hospitalizations one year after the reform for both Louisville residents treated statewide and for Kentucky residents treated in Louisville; when the time horizon is extended to check whether there was a delay in

¹⁹I also run the analysis with three groups (always had a penalty enhancement, lost enhancement, never had enhancement) and find qualitatively similar results that are found in the Online Appendix.

response, I again find no significant change.²⁰

3 Results and Discussion

3.1 Impacts of the DFSZ Reform on Crime in Louisville

The results of estimating equation (1) for drug crimes are in Panel A of Table 2, which show that drug arrests decreased in areas that lost their DFSZ status. More specifically, column (1) shows that relative to census blocks that had no change in their penalties for drug sales, there were 0.372 fewer yearly drug crime incidents per block in de-zoned areas one year after the reform; this represents a 13% decrease relative to the mean. There is a similar sign and magnitude change when drug arrests are disaggregated into the categories of possession, paraphernalia, and sales in columns (2)-(4).

I perform several robustness checks, such as only keeping census blocks that are either 50% within or completely within a given zone, and I split the binary treatment variable into three groups to examine spatial spillovers. All these results can be found in the Online Appendix (Section A.2), which show that treatment assignment by centroid is the most conservative approach and that the results are driven by changes in the de-zoned blocks.

Table 2: The Impact of the DFSZ Reform on Yearly Crimes at the Block Level

Panel A: Drug Offenses

	Specific Offenses			
	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
Lost DFSZ Status=1 × postreform=1	-0.372** (0.149)	-0.204** (0.0891)	-0.139*** (0.0505)	-0.0295 (0.0365)
Observations	16226	16226	16226	16226
Outcome mean	2.801	1.671	0.722	0.358

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Non-Drug Offenses

	Specific Offenses						
	(1) Non-Drug	(2) Larceny	(3) Burglary	(4) Assault	(5) Mischief	(6) Vehicle Theft	(7) Weapon
Lost DFSZ Status=1 × postreform=1	0.207 (0.237)	0.174 (0.113)	0.0424 (0.0544)	0.0260 (0.0702)	0.0606 (0.0514)	-0.0139 (0.0162)	0.0194 (0.0205)
Observations	16226	16226	16226	16226	16226	16226	16226
Outcome mean	9.459	2.770	1.320	1.469	1.235	0.158	0.171

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. Treatment assignment is based on the centroid of the census block.

As described in Section 1, if enforcement was held fixed and criminals responded significantly to harsher punishments, the coefficients in Panel A of Table 2 should be positive due to a

²⁰This data comes from the state's Office of Health Policy, where a hospitalization is considered a drug overdose if any of the ICD-9-CM diagnosis codes in the range of 960 to 979 (drug poisoning) are in either the principal or secondary diagnosis fields. The results are in the Online Appendix.

deterrence effect: Becker's (1968) model predicts that criminal activity would move to areas that lost their harsher punishment. If police increased enforcement there as well, the results would be more positive. On the other hand, if criminal activity remained fixed, and police enforcement decreased in the de-zoned areas, the results would be negative. It is possible that both offender and police behavior changed, but the strong consistently negative effects suggest that the decrease in enforcement dominated any changes in criminal activity.

Moreover, the penalty enhancement in Kentucky is only for drug sales, so observing any changes in possession and paraphernalia would not be expected within Becker's (1968) framework. Put differently, unless there are complementarities between these crimes or compounding of drug offenses, which I find no evidence of, those who commit possession or paraphernalia offenses would not be expected to change their behavior. Moreover, the decrease for each category of drug offense is similar, ranging from a 10% to 20% change relative to the mean, and thus I conclude that the effects are driven by enforcement behavior.

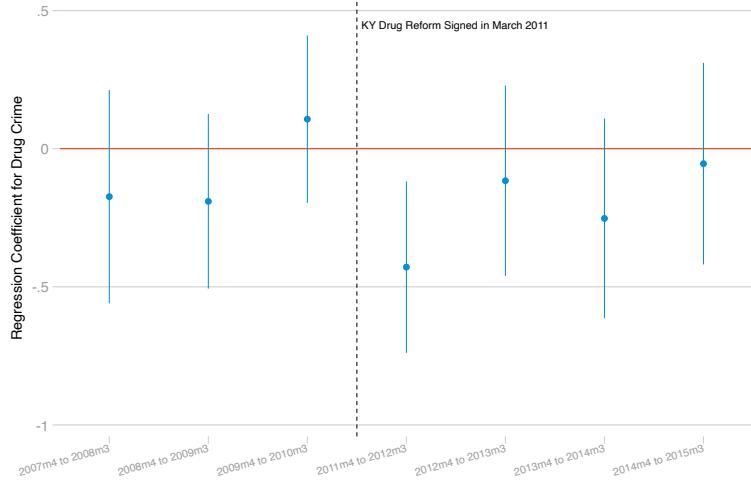
One rationalization for the findings in Panel A of Table 2 is that police officers would like to catch criminals and punish them as severely as possible, or that punishment severity and enforcement are complements. Conversations with law enforcement officials summarized in Section 1.1 suggest that altering the size of enhanced penalty zones may have changed the relative returns to policing between the areas inside and outside DFSZs. An alternative explanation is that officers take their cue from policymakers as to what behavior people care about, and thus penalize with harsher penalties, and what they do not. However, similar to Chandrasekher (2016), Cho et al. (2021), DeAngelo and Owens (2017), and Sullivan and O'Keeffe (2017), Figure 4 shows that the net effects dissipate after a temporary decline.²¹ A likely explanation is that they returned to their baseline behavior, as drugs remained illegal and there were no other reforms.

The results for drug crime suggest that enforcement behavior changed more than criminal activity, but it is not clear if it led to police becoming more effective at deterring other types of crime. There is also the concern that weakening drug penalties leads to more property crime by drug users. Therefore, I re-estimate equation (1) with the most common crimes in the city, which include a mix of crimes that are both "discretionary" in their enforcement and ones that are not. The results are in Panel B of Table 2 and suggest that the police may have substituted towards enforcing other crimes, as five of the six coefficients on major non-drug offenses are positive, but the data are too noisy to confidently reject the null. Furthermore, while also imprecise and positive, column (1) shows that total non-drug offenses changed by only 2%, which is similar in magnitude to the disaggregated non-drug crime results (2-7).²² Put differently, in that all non-drug coefficients are insignificant, I can only conclude that there was a targeted reduction in the arresting behavior for drug crime in the de-zoned areas.

²¹Additional event studies can be found in the Online Appendix, such as the inverse hyperbolic sine analogs. The motivation for these tests is there may have been a concern that the base rates are higher in the treated group, or that the seasonal swings may be larger in levels.

²²I use the same estimation strategy but with a different data source on traffic violations. Traffic offenses are generally more discretionary in terms of their enforcement, but Table A.2.7 shows the effects are small and insignificant after the DFSZ reform.

Figure 4: Impact of DFSZ Reform on Yearly Drug Crimes at Block Level (Event Study)



Note: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status and the year from 2010m4 to 2011m3 (year preceding the drug reform). Vertical black lines represent the 95% confidence intervals.

3.2 Policy Targeting within Louisville (Heterogeneity Analysis)

Given that DFSZs disproportionately cover densely populated urban areas and thus a major concern is that they primarily harm minority and low-income residents, I also examine heterogeneity by race and income. The results for race are in Panel A of Table 3, where I interact the treatment variable D_{bt} from equation (1) with a measure of racial composition: a dummy variable equal to one if 50% or more of the census block is comprised of black residents.²³ The coefficients on these triple interaction terms show that there was a slight increase in drug arrests in areas that lost their DFSZ status and have a higher percentage of black residents, but the coefficients are quite imprecise.

However, the results do show that there were significantly fewer drug arrests in majority black neighborhoods after the reform, suggesting that police shifted their use of discretion away from these neighborhoods. Figure A.2.4 in the Online Appendix shows temporal trends in drug arrests across neighborhoods with different racial compositions; it highlights that the decrease in drug arrests comes from neighborhoods that are almost entirely black, but that the drop is short-lived. This may be because DFSZs were so large pre-reform that police spent their time in areas that had a higher percentage of black residents, similar to what the literature on public housing demolitions finds. However, since I find limited evidence of crime displacement, and Automatic Vehicle Locator (AVL) data are not available, I do not know where police are otherwise spending their time.

The results for income are in Panel B of Table 3, where I split the data by median census block group income. It shows that the decrease in drug consumption crimes (possession and paraphernalia) is stronger for poorer neighborhoods. In census block groups that have a median

²³Table A.2.8 presents results using a continuous measure of the census block that is black. For reference, 32% of census blocks that were de-zoned (1000 to 3000 feet of schools) are in majority black neighborhoods. 35% of the census blocks within 1000 feet of schools are majority black, while 24% of those greater than 3000 feet are.

Table 3: The Impact of the DFSZ Reform on Yearly Drug Crimes at the Block Level

Panel A: Primary DID Results for Race

	(1)	(2)	(3)	(4)
	Drugs	Possession	Paraphernalia	Sales
Lost DFSZ Status=1 × postreform=1	-0.505*** (0.174)	-0.261*** (0.0999)	-0.174*** (0.0673)	-0.0620 (0.0386)
postreform=1 × More than 50% of cb black=1	-0.847*** (0.249)	-0.357** (0.157)	-0.387*** (0.0861)	-0.0830 (0.0670)
Lost DFSZ Status=1 × postreform=1 × More than 50% of cb black=1	0.366 (0.319)	0.0838 (0.198)	0.128 (0.109)	0.124 (0.0872)
Observations	13932	13932	13932	13932
Outcome mean	2.740	1.622	0.711	0.358

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Primary DID Results for Income

	CBG Median Income of $\leq \$25,000$				CBG Median Income of $\geq \$60,000$			
	Drugs	Possession	Paraphernalia	Sales	Drugs	Possession	Paraphernalia	Sales
Lost DFSZ Status=1 × postreform=1	-0.801*** (0.263)	-0.578*** (0.164)	-0.175* (0.0913)	-0.0360 (0.0737)	0.280	0.234* (0.204)	0.105 (0.124)	-0.0416 (0.0564)
Observations	5732	5732	5732	5732	1613	1613	1613	1613
Outcome mean	4.037	2.416	1.016	0.554	0.904	0.523	0.300	0.0701

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. In Panel A, I use a binary variable for a census block if it is greater than 50% black. Table A.2.8 uses a continuous measure. Approximately 15% of the census blocks in Louisville (23% in Jefferson County) have a population of zero, and singletons are removed by the FE regression. Therefore, to provide a more accurate comparison to the main results in Panel A of Table 2, I drop those census blocks in Table A.2.9. Panel B uses income of census block groups to split the sample, where the results are robust to other cutoffs.

income less than or equal to \$25,000, there was a decrease in drug crimes of about 20%. However, the effects are generally insignificant in blocks with an income of greater than \$60,000. Taken together, the policy seems to have led to a decrease in arresting for drug crime for black and poorer neighborhoods.²⁴

4 Conclusion

We still know very little about the incentives of police, primarily because of data constraints and the underlying policy environment. I address both of these challenges by using geocoded incident report data and exploiting within-city variation in punishment created by drug-free school zone (DFSZ) laws; these unique features allow me to study how police respond to changes in punishment severity. I use a difference-in-differences strategy for Louisville and find a 13% decrease in drug arrests in areas that lost their DFSZ status relative to those that did not. The effect remains strong and robust after one year, but dissipates over time and is unique to drug crimes.

If enforcement were fixed, Becker's (1968) model predicts an *increase* in criminal activity farther from schools, given that punishment decreased in areas that had the enhancement for drug sales removed. I find empirically that they *decrease* in those areas. This suggests that the decrease in drug arrests in the de-zoned areas is due to police changing their enforcement

²⁴I also examine whether there is variation across district in Table A.2.10, where the results suggest that it was not a department-wide mandate, and rather an individual police response.

behavior. While most existing work focuses on criminal incentives, this paper highlights the importance of considering a different margin, i.e., that changing punishments can also affect police behavior. In fact, the results suggest that police enforcement and punishment severity are complements.

Descriptive evidence suggests that DFSZs are not salient to potential offenders (Ciarmella and Krisai 2018), as distance is measured in feet and “as the crow flies.” These offenders are instead often only exposed to additional risk by prosecutors later in the criminal justice process. Additionally, low-income and minority residents have been disproportionately affected by the “War on Drugs” (Greene et al. 2006), and any change in policing activity around schools creates a potential concern about the “School-to-Prison Pipeline” (Owens 2017).

The sheer size of the enhanced penalty zones in Kentucky prior to the reform, covering almost 72% of Louisville, may seem surprising, but significant DFSZ coverage is common to many cities. Upon removal of the penalty enhancements from certain parts of Louisville, there was a large decrease in reported drug crime with no significant change to non-drug offenses. These results coincide with the literature examining police slowdowns, which find citywide decreases in arrests following such an event but with no subsequent increase in crime. Additionally, I find that there was a stronger shift away from policing drug crimes in black communities and no change in drug overdoses in the city. Therefore, the perceived negative impacts of weakening drug penalties associated with the “War on Drugs” view of punishment are not substantiated in this setting.

While I can confidently state that drug arrests decreased in de-zoned areas following the removal of penalty enhancements for drug sale offenses, I do not know what officers did otherwise with their time given the imprecise estimates for non-drug offenses and lack of additional data sources. In the extreme, if police officers completely stopped enforcing drug offenses and continued to patrol in the same way, we would predict large increases in non-drug arrests. I do not find significant evidence of such displacement, but it may be that such a reform freed up time for police to engage more with the public, such as community building. This is generally difficult to measure, but documenting what police are doing throughout their workday and how their daily activity responds to changes in criminal justice reform and social events is an important area for future research.

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A Online Appendix

A.1 Descriptive Information

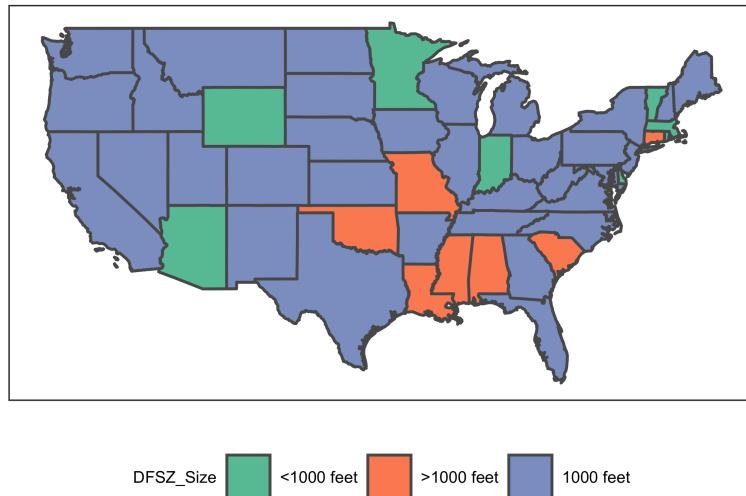


Figure A.1.1: DFSZ Sizes by State

Source: <https://sentencingproject.org/>

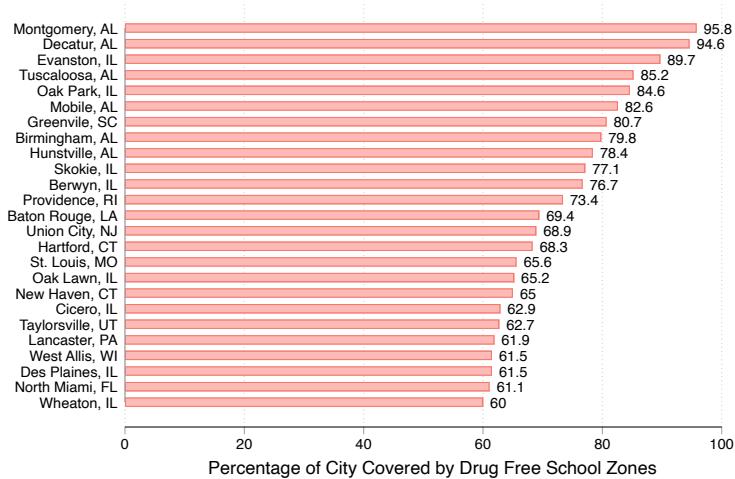


Figure A.1.2: Highest DFSZ City Coverage

Note: These are the top 25 cities with a population of 50,000 or greater. Source: projectknow.com

Table A.1.1: Drug Crime and Punishment in Kentucky

Offense Category	Offense	Penalty	Incarceration	Max. Fine
Possession	Less than 8 oz	Misdemeanor (B)	45 days	\$250
Sale or Trafficking	Less than 8 oz (first offense)	Misdemeanor (A)	1 year	\$500
	Less than 8 oz (subsequent offense)	Felony (D)	1 - 5 years	\$10,000
	8 oz - 5 lbs (first offense)	Felony (D)	1 - 5 years	\$10,000
	8 oz - 5 lbs (subsequent offense)	Felony (C)	5 - 10 years	\$10,000
	5 lbs or more (first offense)	Felony (C)	5 - 10 years	\$10,000
	5 lbs or more (subsequent offense)	Felony (B)	10 - 20 years	\$10,000
	To a minor (first offense)	Felony (C)	5 - 10 years	\$10,000
	To a minor (subsequent offense)	Felony (B)	10 - 20 years	\$10,000
	Within 1000 feet of a school or park	Felony (D)	1 - 5 years	\$10,000
Paraphernalia	Possession of paraphernalia	Misdemeanor (A)	1 year	\$500

Source: [NORML](#)

Note: The relevant Kentucky statute is KRS 218A.1441, which states that “Any person who unlawfully traffics in a controlled substance classified in Schedules I, II, III, IV or V, or a controlled substance analogue in any building used primarily for classroom instruction in a school or on any premises located within one thousand feet of any school building used primarily for classroom instruction shall be guilty of a Class D felony, unless a more severe penalty is set forth in this chapter, in which case the higher penalty shall apply. The measurement shall be taken in a straight line from the nearest wall of the school to the place of violation.”

Table A.1.2: Summary Statistics in Louisville by Exposure to DFSZ Law Change (2009-2012)

	(1)		(2)		(3)	
	No DFSZ Change mean	sd	DFSZ Change mean	sd	Louisville mean	sd
Drugs	2.80	5.83	2.75	5.37	2.77	5.54
Possession	1.66	3.53	1.65	3.31	1.65	3.39
Paraphernalia	0.73	1.74	0.70	1.57	0.71	1.63
Sales	0.36	1.07	0.35	1.01	0.35	1.03
Burglary	1.26	1.85	1.33	2.00	1.30	1.94
Mischief	1.25	1.93	1.20	1.85	1.22	1.88
Larceny	3.17	9.85	2.48	5.62	2.73	7.46
Assault	1.49	2.81	1.42	2.58	1.45	2.66
Vehicle Theft	0.17	0.49	0.15	0.44	0.16	0.46
Weapon	0.16	0.55	0.17	0.57	0.17	0.56
CB housing units	30.80	52.32	29.18	40.85	29.77	45.38
CB population	62.55	100.27	59.17	83.85	60.40	90.21
CB % black	36.02	38.51	37.80	38.22	37.16	38.34
CB % hispanic	2.91	6.45	3.19	7.73	3.09	7.29
Observations	6029		10473		16502	

Notes: The unit of observation is a census block-year.

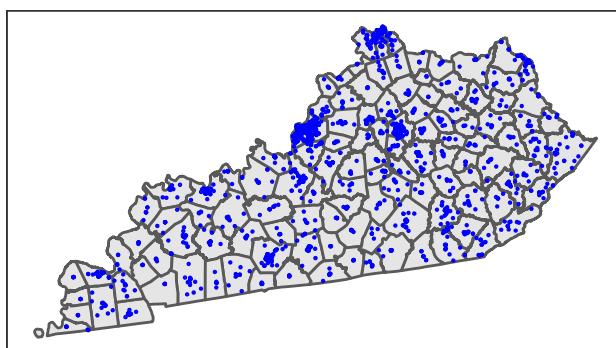


Figure A.1.3: Drug-Free School Zones in Kentucky

Note: Each blue dot represents an individual school with a 1000 yard buffer.

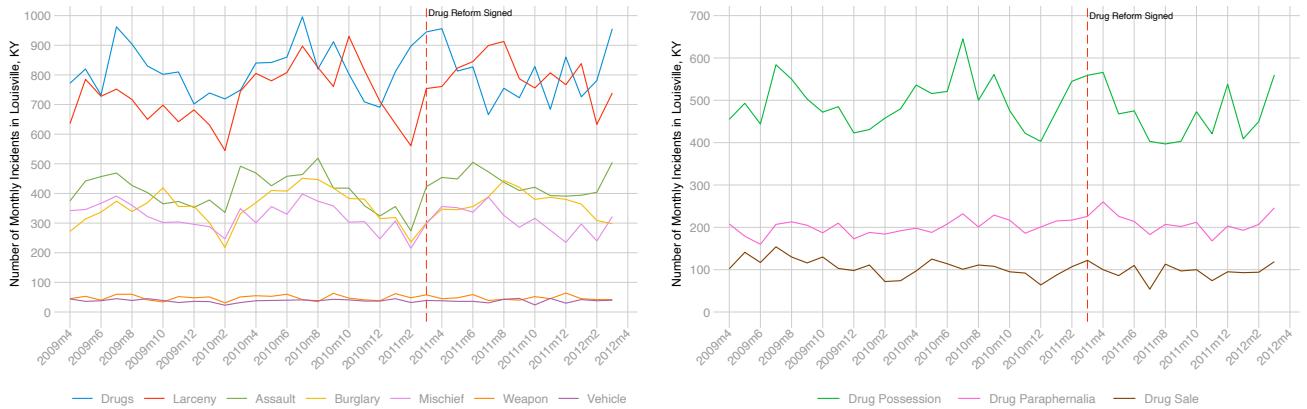
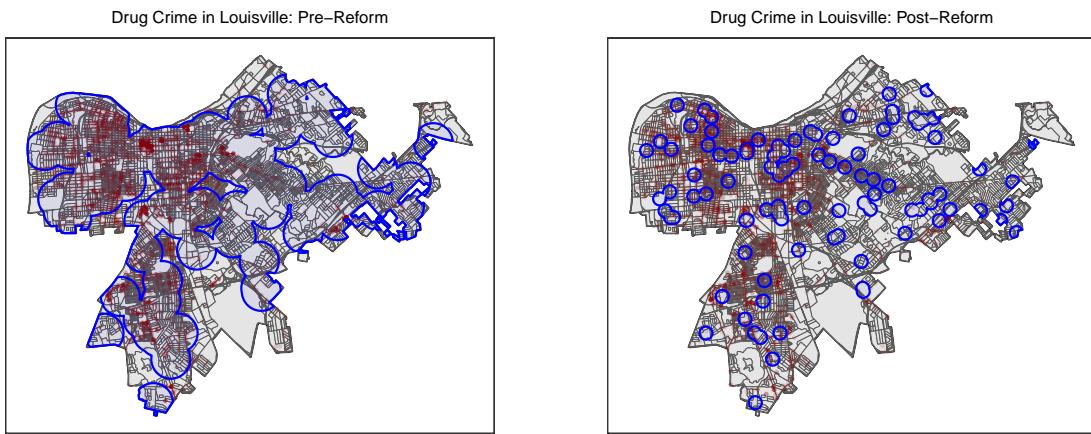


Figure A.1.4: Monthly Crime in Louisville

Notes: The left figure captures the seven most common crimes in Louisville, while the right disaggregates drug crimes into its three primary categories of crime.



Notes: The blue outline and shaded area represent drug-free school zones, which were 1000 yards and 1000 feet before and after the reform, respectively. Each red dot represents an individual drug crime, and thus darker shaded dots are when several crimes occurred in the same location over that year.

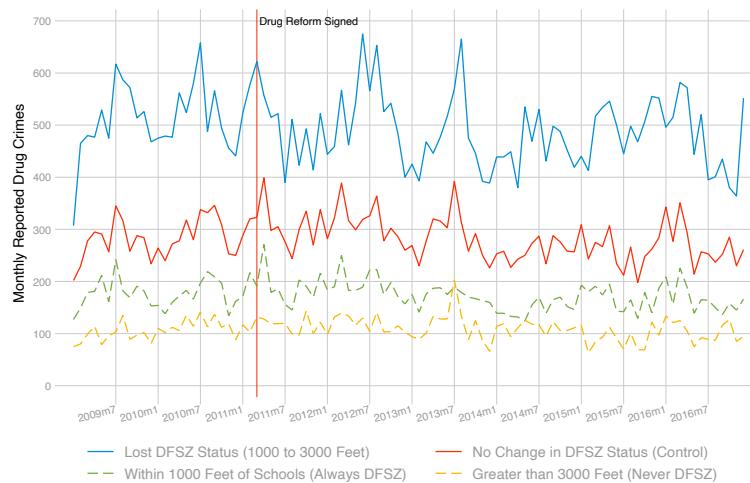
Figure A.1.5: Drug Crime Heat Maps in Louisville (One Year Before and After the Reform)

Table A.1.3: Monthly Crime Regressions for Louisville

	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales	(5) Burglary	(6) Mischief	(7) Larceny	(8) Vehicle Theft	(9) Weapon
postreform	-147.9 (63.57)	-94.10 (41.30)	-25.32 (15.97)	-23.83 (12.01)	36.51 (24.87)	9.746 (16.23)	11.71 (41.57)	-5.365 (4.861)	-9.111 (7.071)
Observations	96	96	96	96	96	96	96	96	96
Outcome mean	751.4	427.0	207.2	99.55	320.3	292.8	740.1	39.90	52.85

Standard errors in parentheses

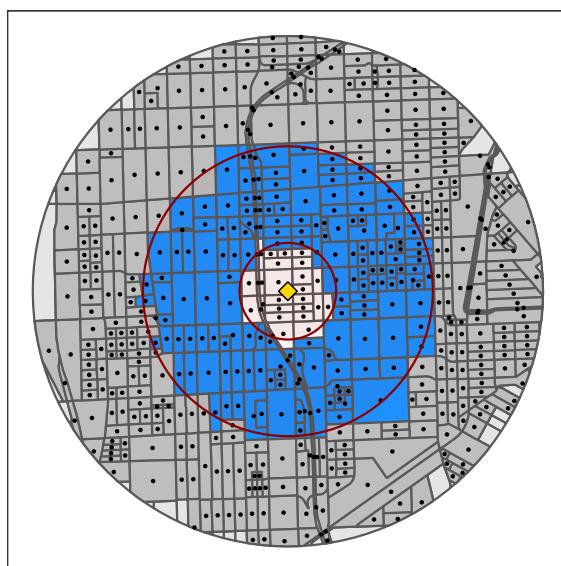
Notes: Year and month fixed effects are included in all models. The data represent monthly reported crime counts for the entire city from 2009 to 2017. I repeat this exercise by successively removing post reform years, and the results do not change. Put another way, reported drug crimes decrease post reform, and the environment otherwise remains stable.



Monthly Drug Crime Trends by DFSZ Status Change

Notes: Similar to Figure 2, this figure captures monthly totals for drug crime based on where it was reported, either in an area that lost its DFSZ status or in one that had no change to its DFSZ statute. I disaggregate the "No change in DFSZ status" into its components ("always DFSZ" or less than 1000 feet and "never DFSZ" or greater than 3000 feet) to show why I chose to combine them; they satisfy the common trends assumption when combined.

Jefferson County High School



Treatment Status 0 to 1000 feet 1000 to 3000 feet (De-Zoned) Greater than 3000 feet

Note: the yellow diamond is the school, while black circles are census block centroids.

Figure A.1.6: An Example of the Primary DID Empirical Strategy with Census Blocks around One School

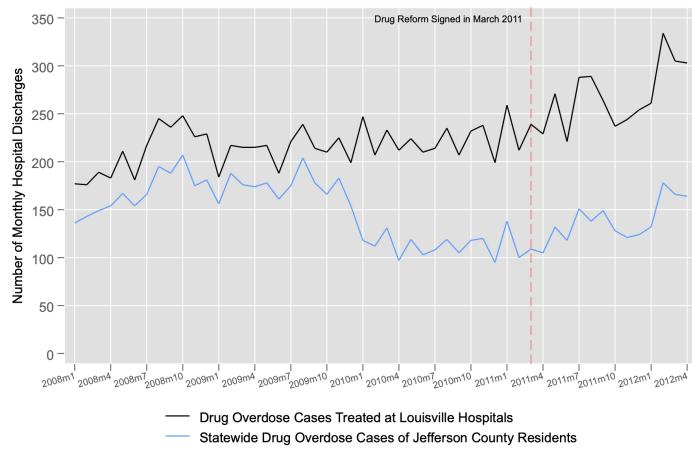


Figure A.1.7: Drug Overdose Cases Treated at Kentucky Hospitals

Table A.1.4: DID Results for Drug Hospitalizans

	(1) Louisville Facility Overdoses	(2) Louisville Resident Overdoses
postreform	-1.222 (18.02)	-3.969 (13.37)
Observations	93	93
Outcome mean	264.0	173.6

Standard errors in parentheses

Note: Year and month fixed effects are included in all models, and the constants are not reported.

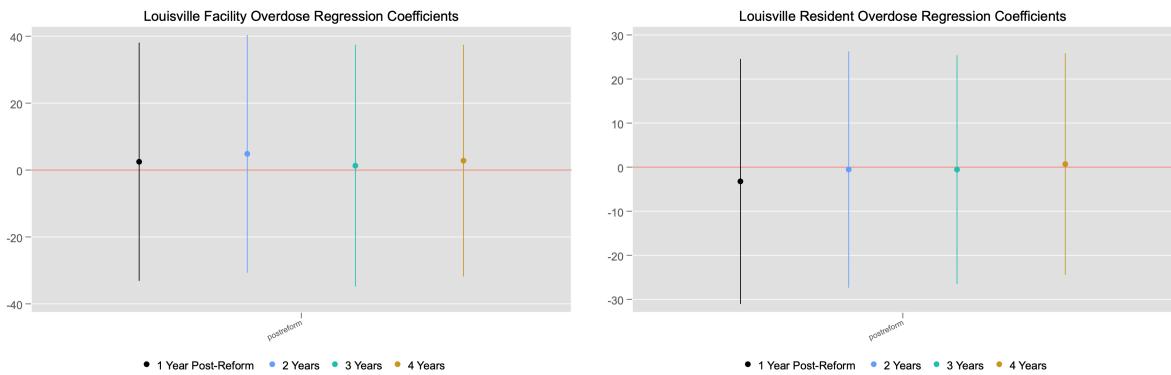


Figure A.1.8: Overdose Regression Coefficient Plots over Time

Notes: The outcome is count variable of hospitalizations for overdose, and year and month fixed effects are included in all models. Vertical lines represent a 95% confidence intervals.

Table A.1.5: Fiscal Year Expenditures by Louisville Metro Police (Primary Categories)

Fiscal Year	Contractual Services	Equipment_Capital_Outlay	Interdepartment_Charges	Personal_Services	Supplies	Total
2008	\$5,150,238.10	\$3,171,359.20	\$2,721.23	\$373,129.27	\$1,460,764.60	\$10,158,212.40
2009	\$5,171,447.30	\$2,489,897.30	\$528.62	\$980,301.08	\$1,292,024.90	\$9,934,199.20
2010	\$5,072,580.40	\$5,742,729.20	\$5,015.64	\$750,519.96	\$1,197,296.70	\$12,768,141.90
2011	\$3,951,257.20	\$2,948,061.50	\$3,090.61	\$343,181.86	\$1,208,505.60	\$8,454,096.77
2012	\$4,177,636.30	\$1,226,651.40	\$2,778.77	\$250,006.00	\$1,206,439.20	\$6,863,511.67

Table A.1.6: Fiscal Year Expenditures by Louisville Metro Police (Detailed Categories)

Department	2008	2009	2010	2011	2012
Community Relations	\$321,493.98	\$277,782.70	\$283,509.24	\$236,593.05	\$281,943.43
Crimes Against Children	\$16,404.20	\$4,401.00	\$2,885.47	\$3,651.55	\$1,623.01
Criminal Investigations	\$414,856.85	\$670,259.41	\$709,692.18	\$663,325.42	\$653,531.11
Director's Office	\$632,693.21	\$1,074,342.40	\$363,270.98	\$643,156.56	\$601,227.80
Finance Grant Management (Finance, Grants & Personnel)	\$158,364.62	\$249,484.21	\$723,412.02	\$203,320.68	\$79,908.34
Fleet (Vehicles)	\$304,971.12	\$331,228.26	\$236,775.61	\$431,798.67	\$713,785.33
Information Services	\$1,626,149.40	\$1,640,940.60	\$941,291.76	\$1,047,241.60	\$1,184,093.20
Narcotics/Vice/Intelligence	\$108,347.31	\$114,175.74	\$64,349.29	\$64,945.04	\$195,968.81
Patrol	\$776,473.09	\$722,915.56	\$699,842.84	\$508,220.14	\$345,287.26
Planning Analysis	\$11,089.61	\$68,238.95	\$7,880.46	\$10,812.23	\$6,057.86
Press Information Media Relations	\$734.41	\$416.12	\$512.65	\$708.03	\$827.36
Professional Standards	\$51,980.20	\$66,044.64	\$52,243.34	\$85,223.81	\$21,608.67
Property	\$106,421.91	\$30,804.49	\$26,294.66	\$16,043.89	\$22,673.45
Public Integrity	\$13,145.86	\$9,118.36	\$10,121.89	\$7,157.94	\$6,645.87
Records/Photo Lab	\$313,386.22	\$295,213.92	\$273,035.49	\$234,915.36	\$200,482.27
School Guards	\$5,858.09		\$7,691.82	\$2,793.01	
Special Operations	\$569,051.91	\$476,247.00	\$428,090.76	\$470,564.62	\$479,875.13
Training	\$922,656.21	\$937,071.86	\$860,398.72	\$836,591.13	\$781,672.81
Other	\$3,804,134.20	\$2,965,514.00	\$7,076,842.80	\$2,987,034.00	\$1,286,299.90
Total	\$10,158,212.40	\$9,934,199.22	\$12,768,141.98	\$8,454,096.73	\$6,863,511.61

Note: Based on invoices from Louisville's Office of Management and Budget, and a Fiscal Year starts July 1 and runs through June 30 of the given fiscal year. For example, Fiscal Year 2012 would include July 1, 2011 through June 30, 2012. Under other, new cars purchased late 2009 for \$4,002,994.30.

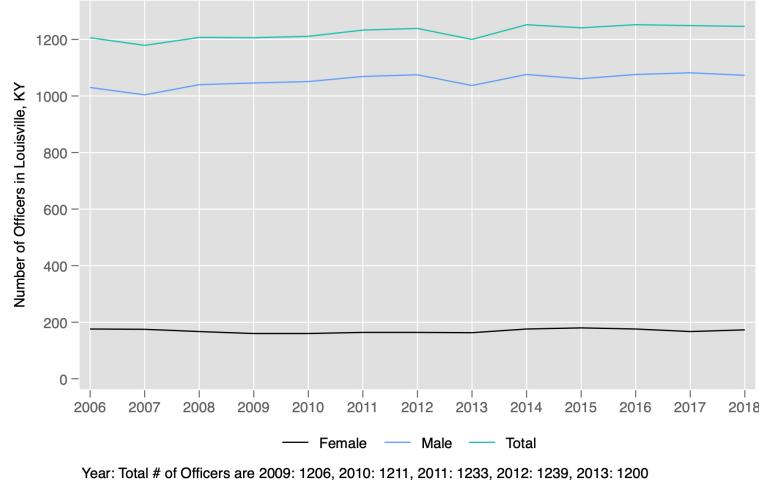


Figure A.1.9: Police Officers in the Louisville Metro Police Department

A.2 Robustness Checks and Additional Analysis

Table A.2.1: The Impact of the DFSZ Reform on Yearly Drug Crimes at the Block Level (50% within)

	Specific Offenses			
	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
Lost DFSZ Status \times postreform=1	-0.400*** (0.151)	-0.238*** (0.0909)	-0.137*** (0.0509)	-0.0236 (0.0365)
Observations	16226	16226	16226	16226
Outcome mean	2.801	1.671	0.722	0.358

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that lost DFSZ status (blocks within 1000 to 3000 feet of schools). Block assignment was based on whether the block was greater than or equal to 50% within a given zone.

Table A.2.2: The Impact of the DFSZ Reform on Yearly Drug Crimes at the Block Level (100% within)

	Specific Offenses			
	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
Lost DFSZ Status \times postreform=1	-0.589*** (0.168)	-0.297*** (0.0988)	-0.221*** (0.0615)	-0.0632 (0.0399)
Observations	10148	10148	10148	10148
Outcome mean	2.536	1.516	0.659	0.315

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that lost DFSZ status (blocks within 1000 to 3000 feet of schools). Block assignment was based on whether the block 100% within a given zone; blocks that were not completely within a zone were dropped from the analysis.

Table A.2.3: The Impact of the DFSZ Reform on Monthly Drug Crimes at the Block Level

	Specific Offenses			
	(1) drugs	(2) drugpossession	(3) drugparaphernalia	(4) drugsales
exposurechange=1 \times postreform=1	-0.0978*** (0.0278)	-0.0492*** (0.0171)	-0.0363*** (0.0101)	-0.0137* (0.00772)
Observations	45871	45871	45871	45871
Outcome mean	0.632	0.378	0.159	0.0803

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year, month, and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. Treatment assignment is based on the centroid of the census block.

Table A.2.4: The Impact of the DFSZ Reform on Yearly Drug Crimes at the Block Level (Inverse Hyperbolic Sine Transformed)

	(1) asinh(Drugs)	(2) asinh(Possession)	(3) asinh(Paraphernalia)	(4) asinh(Sales)
Lost DFSZ Status=1 × postreform=1	-0.0607* (0.0327)	-0.0621** (0.0268)	-0.0576** (0.0229)	-0.00774 (0.0193)
Observations	16226	16226	16226	16226
Outcome mean	1.080	0.805	0.452	0.237

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. Treatment assignment is based on the centroid of the census block. The inverse hyperbolic sine transformation was made, instead of the logarithm, to account for blocks with zero drug crimes.

Table A.2.5: The Impact of the DFSZ Reform on Yearly Drug Crimes at the Block Level (Different Reference Category)

	Specific Offenses			
	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
1000 to 3000 feet × postreform=1	-0.421** (0.211)	-0.238* (0.126)	-0.156** (0.0712)	-0.0367 (0.0503)
Greater than 3000 feet × postreform=1	-0.111 (0.232)	-0.0780 (0.139)	-0.0398 (0.0788)	-0.0165 (0.0569)
Observations	16226	16226	16226	16226
Outcome mean	2.801	1.671	0.722	0.358

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area within 1000 feet of schools (area that is always exposed to enhancements for drug sales). Note that if you drop "never treated" observations (those greater than 3000 feet from a school), the coefficients on 1000 to 3000 feet (lost DFSZ status) remain the same.

Table A.2.6: The Impact of the DFSZ Reform on Yearly Crimes at the Block Level (Different Reference Category)

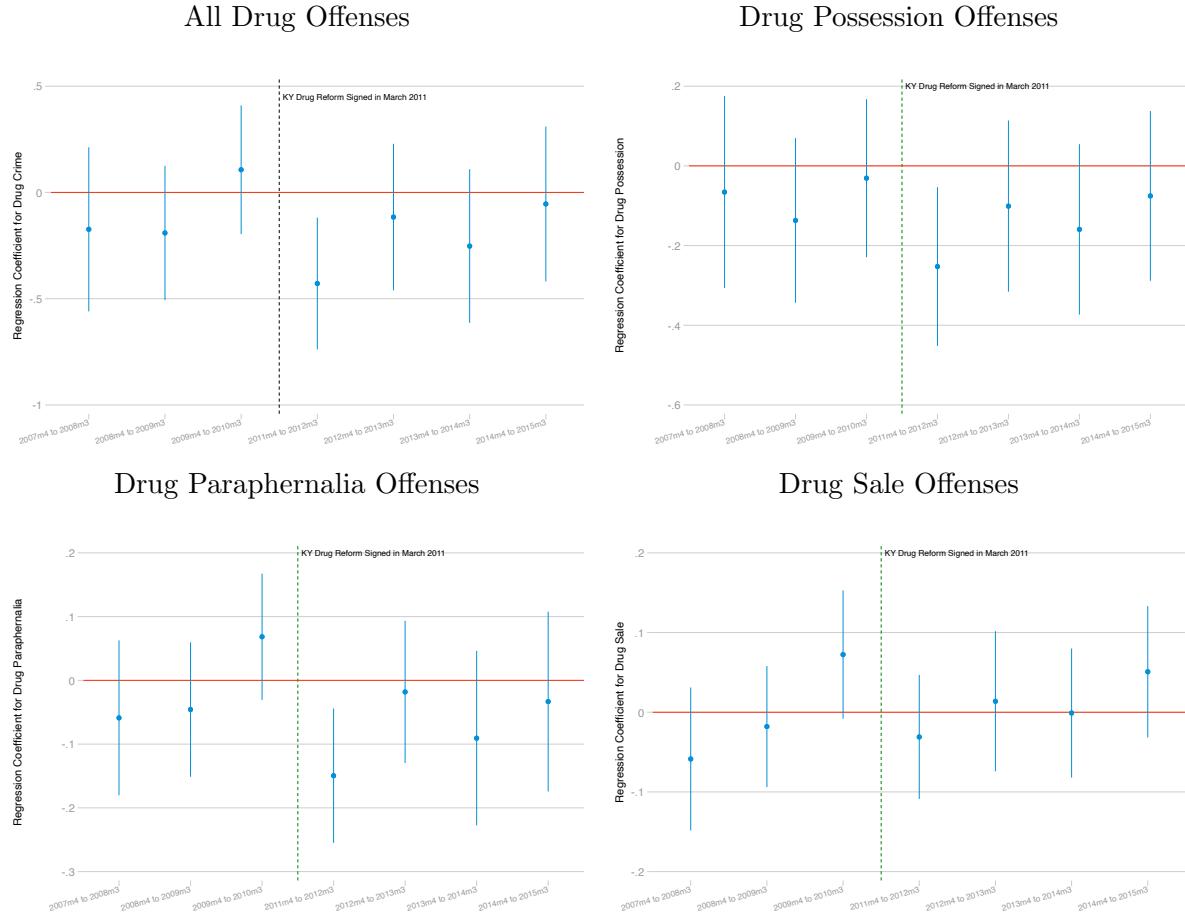
	(1) Larceny	(2) Burglary	(3) Assault	(4) Mischief	(5) Vehicle Theft	(6) Weapon
1000 to 3000 feet × postreform=1	0.153 (0.133)	0.0966 (0.0692)	0.0665 (0.0946)	0.114* (0.0664)	-0.00972 (0.0202)	0.0165 (0.0283)
Greater than 3000 feet × postreform=1	-0.0465 (0.186)	0.124 (0.0833)	0.0922 (0.111)	0.122 (0.0803)	0.00953 (0.0264)	-0.00658 (0.0313)
Observations	16226	16226	16226	16226	16226	16226
Outcome mean	2.770	1.320	1.469	1.235	0.158	0.171

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area within 1000 feet of schools (area that is always exposed to enhancements for drug sales).

Figure A.2.1: Impact of DFSZ Reform on Yearly Drug Crimes at Block Level (Event Study)



Note: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status and the year from 2010m4 to 2011m3 (year preceding the drug reform). Vertical black lines represent the 95% confidence intervals.

Table A.2.7: DID Results for Traffic Stops

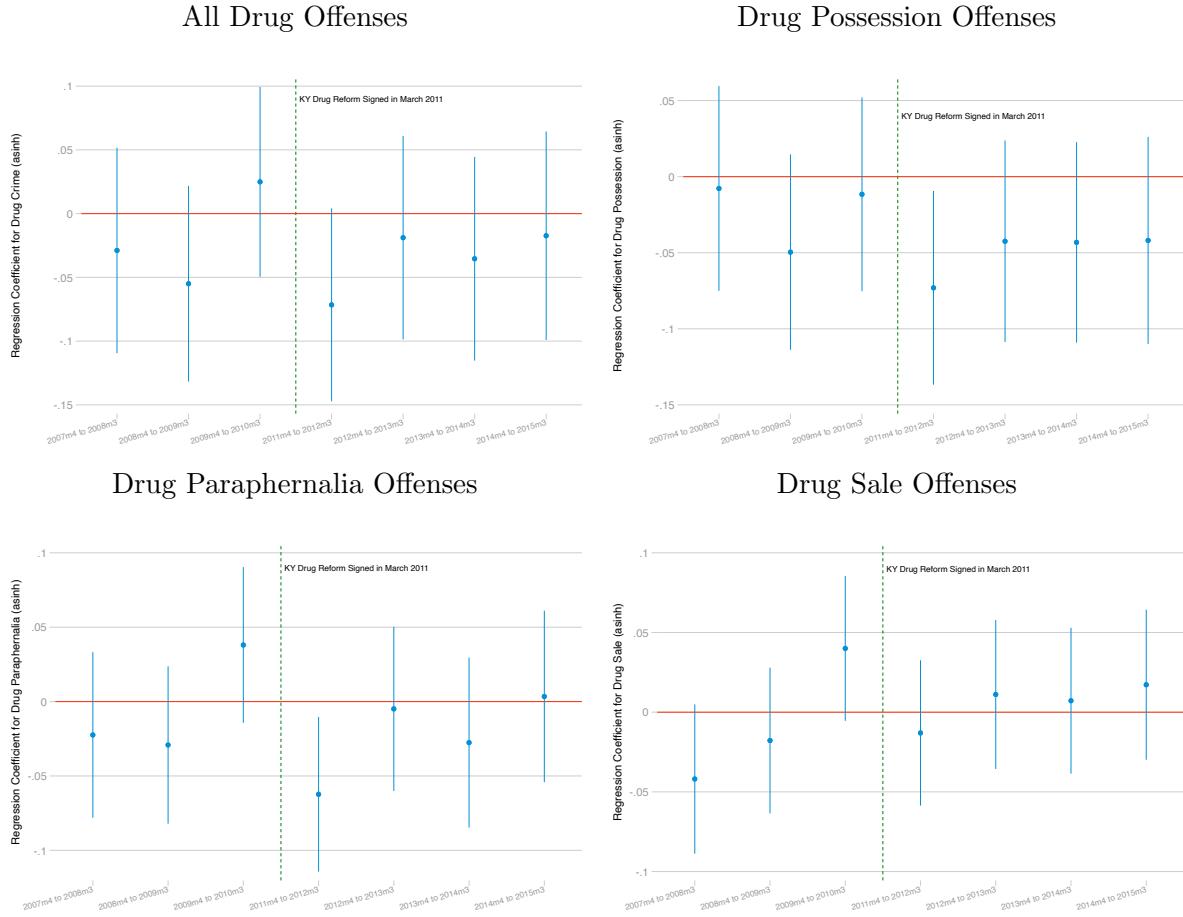
	(1)
	Yearly Traffic Violations
Lost DFSZ Status=1 \times postreform=1	0.633 (2.307)
Observations	3169
Outcome mean	12.67

Standard errors in parentheses

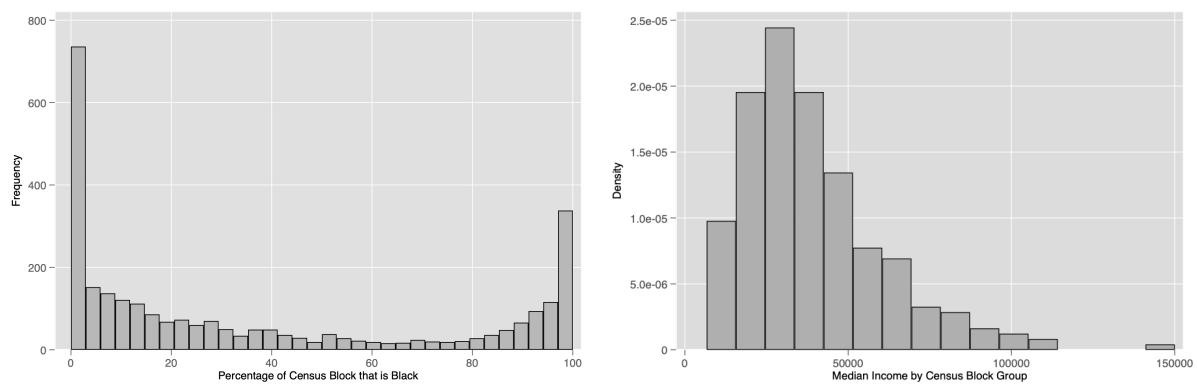
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Year and census block effects are included. The base category is the area that lost DFSZ status (1000 to 3000 feet). The source of this data is different than the rest of the paper and comes from Louisville Metro PD's Uniform Citation records.

Figure A.2.2: Impact of DFSZ Reform on Yearly Drug Crimes at Block Level (Event Study, Inverse Hyperbolic Sine Transformed)



Note: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status and the year from 2010m4 to 2011m3 (year preceding the drug reform). Vertical black lines represent the 95% confidence intervals.



Panel A: Census Block that is Black

Panel B: Census Block Group by Income

Figure A.2.3: Histograms by Race and Median Income

Table A.2.8: The Impact of the DFSZ Reform on Yearly Crimes at the Block Level (By Race)

	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
Lost DFSZ Status=1 × postreform=1	-0.551*** (0.197)	-0.277** (0.113)	-0.176** (0.0753)	-0.0791* (0.0424)
postreform=1 × CB % black	-0.0113*** (0.00293)	-0.00514*** (0.00176)	-0.00468*** (0.00108)	-0.00128* (0.000773)
Lost DFSZ Status=1 × postreform=1 × CB % black	0.00444 (0.00379)	0.00119 (0.00228)	0.00117 (0.00134)	0.00156 (0.00102)
Observations	13932	13932	13932	13932
Outcome mean	2.740	1.622	0.711	0.358

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. A continuous measure of percentage black in the census block (cb% black variable) is used. Approximately 15% of the census blocks in Louisville (23% in Jefferson County) have a population of zero, and singletons are removed by the FE regression, which explains the difference in observation totals relative to the main results in Panel A of Table 2.

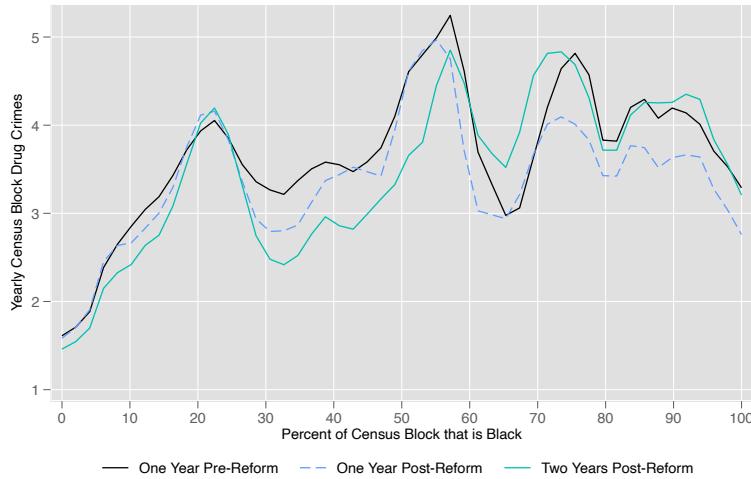


Figure A.2.4: Relationship between Percent of Census Block that is Black and Drug Crime

Notes: This figure uses kernel-weighted local polynomial regressions of drug crimes on percentage of black residents at the census block level, separately for each year.

Table A.2.9: The Impact of the DFSZ Reform on Yearly Crimes at the Block Level (Limited Sample)

	Specific Offenses			
	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
Lost DFSZ Status=1 × postreform=1	-0.402*** (0.147)	-0.241*** (0.0875)	-0.140*** (0.0539)	-0.0220 (0.0364)
Observations	13932	13932	13932	13932
Outcome mean	2.740	1.622	0.711	0.358

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. Approximately 15% of the census blocks in Louisville (23% in Jefferson County) have a population of zero, and singletons are removed by the FE regression. I remove those census blocks from the analysis in order to compare the results with Panel A of Tables 2 and 3.

Table A.2.10: The Impact of the DFSZ Reform on Yearly Crimes at the Block Level

	Specific Offenses			
	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
Lost DFSZ Status after Reform=1 × 1st District	-0.648*** (0.248)	-0.367** (0.148)	-0.258*** (0.0911)	-0.0336 (0.0639)
Lost DFSZ Status after Reform=1 × 2nd District	-0.530** (0.217)	-0.285** (0.138)	-0.252*** (0.0644)	0.0232 (0.0593)
Lost DFSZ Status after Reform=1 × 3rd District	0.514 (0.495)	0.178 (0.280)	0.151 (0.155)	0.121 (0.109)
Lost DFSZ Status after Reform=1 × 4th District	-0.158 (0.215)	-0.0789 (0.126)	-0.0527 (0.0740)	-0.0696 (0.0489)
Lost DFSZ Status after Reform=1 × 5th District	-0.258 (0.181)	-0.159 (0.117)	-0.0305 (0.0616)	-0.00624 (0.0400)
Lost DFSZ Status after Reform=1 × 6th District	-0.740*** (0.235)	-0.446*** (0.128)	-0.217*** (0.0748)	-0.0517 (0.0744)
Observations	16137	16137	16137	16137
Outcome mean	2.775	1.659	0.711	0.355

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. Treatment assignment is based on the centroid of the census block. The Seventh and Eighth Divisions are omitted due to small sample size.

Table A.2.11: The Impact of the DFSZ Reform on Yearly Drug Crimes at the Block Level (Placebo Reform)

	Specific Offenses			
	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
Lost DFSZ Status=1 × dummyreform=1	0.0650 (0.152)	0.0705 (0.0973)	0.00588 (0.0475)	-0.00416 (0.0349)
Observations	12816	12816	12816	12816
Outcome mean	2.799	1.681	0.717	0.365

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. The *dummyreform* variable moves the policy change to exactly one year earlier.

Table A.2.12: The Impact of the DFSZ Reform on Yearly Drug Crimes at the Block Level (Random Locations)

	Specific Offenses			
	(1) Drugs	(2) Possession	(3) Paraphernalia	(4) Sales
Lost DFSZ Status=1 × postreform=1	0.0693 (0.148)	0.0661 (0.0867)	0.00734 (0.0534)	0.00913 (0.0364)
Observations	11578	11578	11578	11578
Outcome mean	2.665	1.596	0.670	0.338

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. Year and census block fixed effects are included in all models, and the constants are not reported. The base category is the area that had no change in its DFSZ status. The 101 school addresses in Louisville are replaced by 101 random points for this model.

Table A.2.13: The Impact of the DFSZ Reform on Yearly Drug Crimes at the Block Level (Callaway and Sant'Anna, 2020)

Parameter	Outcome	Event	Treatment	Overall ATT (Simple)
Overall ATT	All Drug Crimes	Post-Reform	De-Zoned Blocks	-0.5294 (0.2006)
Overall ATT	Possession Crimes	Post-Reform	De-Zoned Blocks	-0.308 (0.1259)
Overall ATT	Paraphernalia Crimes	Post-Reform	De-Zoned Blocks	-0.1632 (0.0671)
Overall ATT	Drug Sale Crimes	Post-Reform	De-Zoned Blocks	-0.0584 (0.0483)

Notes:

Estimation Method: Doubly Robust

Control Group: Never Treated, Anticipation Periods: 0

Standard Errors: Clustered at Census Block and Computed using Multiplier Bootstrap

Table A.2.14: The Impact of the DFSZ Reform on Drug Crimes using Different Fixed Effects

	(1) Drugs, No FEs	(2) Drugs, Year FEs	(3) Drugs, Census Block FEs	(4) Drugs, Year and Census Block FEs
Lost DFSZ Status=1 × postreform=1	-0.296** (0.146)	-0.295** (0.146)	-0.373** (0.149)	-0.372** (0.149)
Observations	16502	16502	16226	16226
Outcome mean	2.770	2.770	2.801	2.801

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. The constants are not reported, and the base category is the area that had no change in its DFSZ status.

Table A.2.15: The Impact of the DFSZ Reform on Drug Crimes using Different Fixed Effects

	(1) Drugs, No FEs	(2) Drugs, Year FEs	(3) Drugs, Census Block FEs	(4) Drugs, Year and Census Block FEs
1000 to 3000 feet × postreform=1	-0.364* (0.208)	-0.362* (0.208)	-0.422** (0.211)	-0.421** (0.211)
Greater than 3000 feet × postreform=1	-0.145 (0.229)	-0.142 (0.229)	-0.113 (0.232)	-0.111 (0.232)
Observations	16502	16502	16226	16226
Outcome mean	2.770	2.770	2.801	2.801

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the census block. The constants are not reported, and the base category is the area within 1000 feet of schools (area that is always exposed to enhancements for drug sales).

A.3 Google Trends Analysis

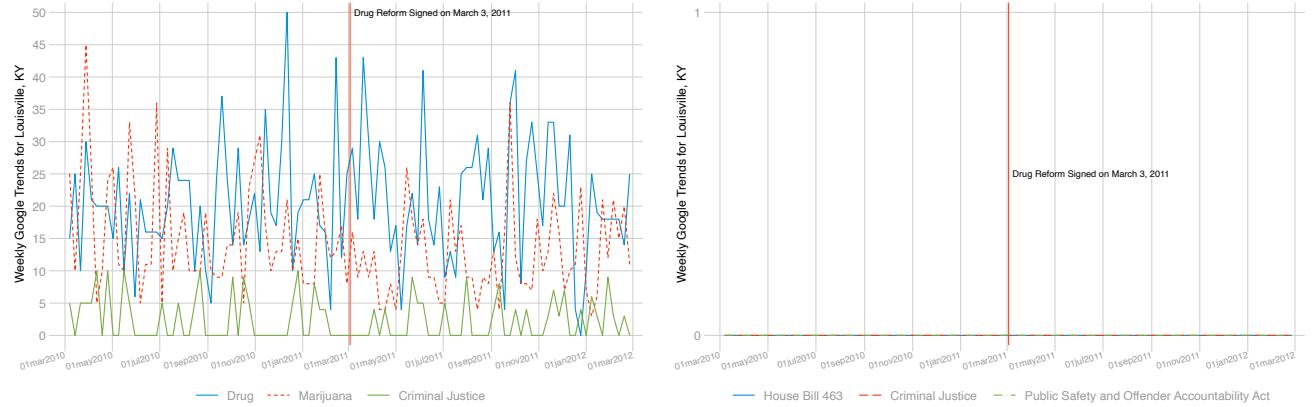


Figure A.3.1: Weekly Google Trends for One Year Pre- and Post-Reform in Louisville, KY

Notes: These two figures show six search terms that were used in Google Trends platform. Other terms were also checked, but the “search doesn’t have enough data to show here,” similar to what happened with the terms used in the right figure.

Table A.3.1: The Impact of the DFSZ Reform on Weekly Google Trends in Louisville, KY

	(1) Drug	(2) Marijuana	(3) Criminal Justice
postreform	7.115 (5.671)	-1.508 (3.318)	0.553 (1.401)
Observations	261	261	261
Outcome mean	46.54	15.01	2.471

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Year and month effects are included in all models, and the constants are not reported. The data run from January 2009 until December 2012.