The Annals of Regional Science (2026) 75:42
https://doi.org/10.1007/s00168-026-01471-0

ORIGINAL PAPER

Beyond crime: evidence on policing and marijuana dispensaries

Sian Mughan[1]

Received: 4 March 2025 / Accepted: 4 February 2026

1 Introduction

Marijuana legalization enjoys broad support among the American public. Yet within communities the sale of marijuana remains controversial as residents and public officials debate how dispensaries will impact their communities. The primary source of contention is public safety. Law enforcement officials and a share of residents oppose marijuana dispensaries arguing that they attract crime, increase police workload, and complicate enforcement (Stanton et al. 2022; Stohr et al. 2020). Others maintain that there is no evidence that dispensaries increase crime and claim that dispensaries will improve public safety by enabling police to allocate resources to areas of greater need (Di Tella and Schargrodsky 2004), reducing the consumption of alcohol and other substances that are associated with aggressive behavior (Miller and Seo 2021; Anderson and Rees 2023), and eliminating the need for individuals to obtain marijuana on the black market (Dong and Tyndall 2024).

The existing literature generally finds no effect of dispensaries on violent crime, while evidence on property crime is mixed. Using the random assignment of marijuana licenses through a state lottery in Washington, Thomas and Tian (2021) find that dispensaries increase reports of nuisance-related offenses such as loitering and disorderly conduct but have no effect on property or violent crime. Studying the same lottery, Dong and Tyndall (2024) also find no overall effect on crime, though they document modest increases in property crime in low-income areas with dispensaries. Estimating treatment effects across varying treatment bandwidths, they show that the magnitude of these effects increases with larger bandwidths. These findings contrast with Chang and Jacobson (2017), who find that medical dispensaries in Los Angeles reduce property crime and that this effect dissipates as the treatment bandwidth increases.

These studies focus on the narrow question of how the retail sale of marijuana impacts crime. Little attention is paid to the mechanisms driving this relationship.

✉ Sian Mughan
smughan@asu.edu

[1] School of Public Affairs, Arizona State University, Phoenix, USA

Published online: 16 March 2026

An exception is Chang and Jacobson (2017), who find that dispensaries are associated with increased foot traffic in surrounding areas, which may deter crime. However, there are other channels through which dispensaries might deter crime. Because they are typically required to adopt outward-facing security measures—such as surveillance cameras, enhanced exterior lighting, and private security personnel—dispensaries alter the physical environment of neighborhoods in ways that previous research has linked to reductions in crime (Freisthler et al. 2013). Community responses to dispensaries may have a deterrent effect if residents, perceiving increased risks, become more engaged in informal policing efforts. This can include heightened monitoring of public spaces and greater use of non-emergency reporting systems to address behaviors viewed as problematic (Harris et al. 2020).

Perhaps most importantly, the sale of marijuana may prompt law enforcement to reallocate resources to areas surrounding dispensaries or to adopt or intensify the use of proactive policing techniques which focus on identifying and addressing potential criminal activity before it occurs by, for example, expanding foot patrols, or raising the frequency of citizen interactions (Majmundar and Weisburd 2018). Given that one of the more consistent findings in the policing literature is that police presence (Di Tella and Schargrodsky 2004; Draca et al. 2011) and enforcement (Luca 2015) reduce offending, such changes are expected to deter crime.

This study seeks to enhance our understanding of how dispensaries influence public safety by estimating the short and medium term impact of medical marijuana dispensaries on crime and policing in Chicago between 2012 and 2017. Using a stacked differences-in-differences (DiD) model, I begin by analyzing the effect of dispensary entry on crime counts in police beats with a dispensary, relative to those without. To address potential bias arising from systematic differences in neighborhood characteristics correlated with dispensary location, propensity score matching (PSM) is used to construct an alternate control group of beats similar to treated beats along observable pretreatment covariates. The results do not suggest a strong effect on violent or property crime. There is limited evidence of a decline in theft—a relatively minor property crime that has often been the focus of concerns surrounding dispensaries—which decreases by between six and 12 percent. However, a lack of precision in these estimates necessitates caution in interpretation.

To identify the effect of dispensary entry on policing—the primary focus of this study—I exploit the institutional distinction between two types of enforcement personnel: sworn officers of the Chicago Police Department (CPD) and civilian Parking Enforcement Aides (PEAs). PEAs are civilian employees whose authority is limited to enforcing parking and vehicle related violations. Because dispensaries are unlikely to meaningfully affect the demand for their services, any observed changes in PEA outcomes would suggest that unobserved neighborhood characteristics—such as gentrification or redevelopment—are biasing the treatment effect estimates.

Using detailed parking ticket data and a stacked difference-in-differences (DiD) design with a matched control group, I find that dispensary entry has a positive and significant effect on police presence. In the average quarter, the number of CPD police officers issuing traffic tickets in neighborhoods surrounding dispensaries increases by between 4.9 (matched control group) and 15.1 (unmatched control group) relative to neighborhoods not in close proximity to a dispensary. In addition, the number of parking tickets written by these officers increases by between 85 (matched control group) and 108 (unmatched control group), indicating an increase in observable police activity. An event study specification reveals a clear upward trend in the post-treatment period that is particularly pronounced for the number of officers, the measure that best captures police presence.

I next examine traffic stop data to identify changes in policing behavior. Drivers often violate traffic laws, and police officers have significant discretion in deciding when to initiate a stop. This provides officers with wide latitude to stop and question drivers (Skogan 2022). Consequently, traffic stops for non-moving violations (i.e., equipment failures) are frequently used as a pretext for investigating unrelated criminal activity (Epp et al. 2017; Petersen et al. 2023).[1] Referring to marijuana specifically, officers have complained that legalization means that they can no longer use marijuana as a 'search tool' (Stohr et al. 2020). If, as the preceding analysis suggests, police perceive dispensaries as increasing local crime risk, they may respond by adopting strategies aimed at detecting criminal activity before it occurs. This is expected to manifest in more traffic stops for violations unrelated to driving conduct (non-moving violations henceforth).

Because police may concentrate proactive enforcement efforts during nighttime hours—when the likelihood of criminal activity is higher (Doleac and Sanders 2015)—and because increased enforcement may have heterogeneous effects across racial groups, I estimate treatment effects by race (White, Black, and Hispanic) and time of day. I find no evidence that the sale of marijuana influences the total number of traffic stops or stops for moving (i.e., speeding) or non-moving violations. Similarly, there are null effects for Black and Hispanic drivers. However, nighttime stops of White drivers for non-moving violations increase following dispensary entry. Taken together, these results suggest that changes in police behavior are targeted rather than uniform, helping to explain the imprecision observed in the aggregate estimates.

The primary contribution of this study is to deepen our understanding of how marijuana dispensaries affect local communities by providing causal evidence on their impact on public safety and policing. To my knowledge, this is the first study to empirically examine the relationship between the retail sale of marijuana and policing, a key mechanism through which dispensaries impact crime. I show that dispensaries increase police presence, and in doing so, generate unintended consequences. Specifically, they raise the likelihood of sanction for minor violations such as parking tickets, which may be associated with additional fines, court debt, vehicle seizure, and the transfer of debt to collection agencies. While such actions may improve public safety, prior research shows that these relatively minor events can push financially fragile households into distress and increase the likelihood of future justice system involvement (Mello 2018; Mughan and Carroll 2021).

Another contribution of this study is its location. Other studies are clustered in a handful of states and tend to estimate effects of the sale of marijuana in markets where dispensaries are already operating (Chang and Jacobson 2017; Brinkman and

[1] The literature also refers to these as investigatory or pretextual stops.

Mok-Lamme 2019; Dong and Tyndall 2024; Burkhardt and Goemans 2019). By examining a context in which legal marijuana sales were entirely new, this analysis estimates institutional responses to the entry of marijuana businesses and offers important insights for communities weighing whether to permit dispensaries to operate within their boundaries.

In the sections that follow, I discuss the theoretical basis for my analysis of dispensaries and policing and the relevant literature. Section 3 gives the relevant institutional background and Sect. 4 describes the data. I outline my empirical approach and present my main findings in Sect. 5. Section 6 contains supplemental analyses, and Sect. 7 concludes.

2 Literature and theory

In a rational choice model of crime, an actor's decision to break the law is based on the expected benefits (financial gains, convenience, etc. and so on) and the expected costs (the probability of arrest, financial penalties, time spent in jail, etc.) (Becker 1968). In this framework, dispensaries are predicted to increase the benefits of criminal activity in several ways. They are cash businesses and thus create opportunities for theft, either from the dispensary or its patrons. Bernasco et al. (2013, 123) note that 'victims with cash are best found around cash economies,' highlighting the risk posed by such businesses. Dispensaries also increase the relative returns to criminal activity by lowering search costs, attracting individuals who are more likely to engage in crime or resort to illegal means to fund their marijuana consumption. This, in turn, could attract others seeking to target criminals drawn to the dispensary.

However, as previously discussed, the existing empirical literature generally does not support these concerns. One possible explanation is that while dispensaries increase the returns to criminal activity, they also increase the costs. Enhanced security, foot traffic, law enforcement presence, and citizen vigilance associated with dispensaries raise the probability of detection. These conflicting predictions concerning how dispensaries affect the returns to criminal activity make the effect of the sale of marijuana on crime ultimately an empirical question.

The same logic can be applied to police officer decision making. When deciding whether to initiate an interaction with a citizen, officers weigh the potential costs and benefits of that action (Knowles et al. 2001). If the costs of an action increase relative to the benefits, the officer becomes less likely to perform the action. For example, increased scrutiny of traffic enforcement led New Jersey state troopers to dramatically reduce the number of traffic stops because the officers became concerned about the consequences of knowingly or unknowingly failing to follow procedure (Tully 2024). Similarly, if officers believe there is a higher chance that a driver is engaging in illegal activity, they become more likely to initiate a stop, irrespective of driving behavior.

Dispensaries may influence officers' cost–benefit calculations by raising the costs of inaction. Evidence suggests that the average law enforcement officer believes that marijuana increases the probability an individual is committing or is going to commit a crime (Ward et al. 2019). Assuming the modal officer considers preventing a crime from occurring or apprehending a criminal as enhancing public safety, this model yields clear predictions about the relationship between policing and dispensaries; police presence, visibility, and activity will increase when dispensaries are present.

There is reason to expect that dispensary-induced changes in the officer cost–benefit calculation are not uniform. Dispensaries are more likely to led to increases in police visibility in areas surrounding dispensaries during nighttime hours where darkness and fewer onlookers reduce the probability of detection, making the risk of crime is more salient (Calandrillo and Buehler 2008). Moreover, research on racial bias in policing suggests that officers' thresholds for initiating a stop may vary by race (Knowles et al. 2001). In other words, assuming the marginal Black and White drivers are identical, the Black driver is stopped, whereas the counterfactual White driver is not. Thus, dispensary-induced changes in enforcement may have differential effects across racial groups.

3 Medical marijuana dispensaries in illinois

In August 2013, the Illinois governor signed the Compassionate Use of Medical Cannabis Pilot Program Act (the MCPP Act henceforth), making Illinois the 20th state to legalize medical marijuana and the first to do so through the legislative process. To purchase medical marijuana, individuals must provide written certification from a physician testifying to their need for medical marijuana to treat a qualifying condition, fill out an application for a medical marijuana card with the Illinois Department of Public Health, and pay a fee of between \$50 and \$125. Once registered, patients may purchase a maximum of 2.5 oz of marijuana in any 14-day period (410 ILCS 130/130).

The MCPP Act also establishes the parameters for marijuana cultivation centers and dispensaries which are overseen and regulated by the Illinois Department of Financial and Professional Regulation (IDFPR). The Act allows for 60 medical marijuana dispensary licenses, 13 of which are allocated to Chicago. To further ensure patient access, licenses were distributed by township, five of the city's eight townships received two licenses while the remaining townships received one (Section 1920.20).

The city received over 350 applications for medical marijuana dispensary licenses. The IDFPR evaluated and scored each applicant based on factors such as security and business plans, financial requirements, and community benefits. Applicants were also incentivized to consider the location of existing dispensaries as, given the goal of patient access, the probability of being selected decreased significantly if the proposed dispensary was near an existing dispensary. These scores were then used by the governor in allocating the licenses.

In addition to obtaining a license from the state, applicants were required to secure zoning approval from the city. To do so, the applicant was required to propose an exact location which, per the Act, could not be within 1,000 feet of preschool, elementary or secondary school, daycare center or daycare home area and could not be located in a house, apartment, or area zoned for residential use (410

ILCS 130/130). Given the considerable number of people running daycares out of their homes, the daycare restriction was particularly limiting (Arruza and Keenehan 2015).

Dispensaries were also required to obtain a special zoning permit rather than a standard business permit. The objective was to make the local licensing process more stringent and to create more space for community input (Schlikerman 2014). This requirement, combined with the restrictions in the Act, limited the number of eligible properties. Referring to Illinois legislation regarding recreational marijuana, one industry group wrote 'The intent of the legislation was to maximize local control over siting and zoning options since *the very prescriptive language in the medical pilot program created significant challenges for licensees to find eligible properties, particularly in more densely populated areas* (Howard n.d.) [italics added].'

4 Data and descriptive statistics

4.1 Crime data

I rely on crime data available through Chicago's data portal to analyze the relationship between dispensaries and reported crime. These data captures every incident reported to the Chicago Police Department in which an incident report is completed. It does not capture crimes that are not reported to the police or incidents where the police respond but do not fill out an incident report. Crimes are classified using Illinois Uniform Crime Reporting Codes and are divided into index and non-index offenses. The former are consistent with the FBI's Uniform Crime Reporting codes while the latter are unique to the Illinois reporting system. In addition to violent and property crime, this study uses burglary, theft, motor vehicle theft, narcotics (index offenses) as well as deceptive practices and criminal trespass (non-index offenses) as outcome variables.

The incident-level data are aggregated to the police beat, the smallest organizing unit of police activity.[2] As part of the city's alternative policing strategy program (CAP), each beat has a small number of designated officers who provide the baseline police presence in that area. A central objective of CAPS is the early detection and prevention of criminal activity, which is pursued through an emphasis on proactive policing. These activities include foot and bicycle patrols, as well as targeted enforcement efforts, and are intended to increase police visibility, respond to local concerns, and foster trust between police and the community.

Beats are grouped into 22 police districts, the primary administrative units for management, staffing, and deployment. Each district is led by a district commander and operates out of a district station. While some officers are assigned to specific beats, the majority have a district-wide assignment. For instance, in July 2023, 6,339 sworn police officers were assigned to districts. Of these 33 percent were listed as belonging to a specific beat, averaging approximately 7.8 officers per beat, while the

[2] The average (median) Chicago police beat is 0.79 (0.58) square miles.

remaining 67 percent were assigned to district-wide patrol.[3] Officers with district-wide assignments report to district headquarters at the start of each shift and are deployed to beats or to district-wide patrol as needed, typically in response to areas of highest demand.

4.2 Dispensary data

The names, locations, and license issuance dates of medical marijuana dispensaries are publicly accessible on the IDFPR's website. The first license was granted in November 2015, two licenses were issued in 2018, and the rest were awarded in 2016 and 2017. However, information regarding the exact opening dates of the dispensaries is not publicly available. Significant delays between the license issuance and the start of sales would attenuate treatment effect estimates as marijuana sales were not occurring during a substantial portion of the treatment period. However, available evidence suggests that the gap between licensing and opening dates is minimal.[4]

There are 11 dispensaries operating in the city, two fewer than the 13 licenses available, as no applications were submitted in two townships. Figure 1 shows the locations of the dispensaries analyzed in this study. Two additional licenses were awarded in 2018, after the study period, and the corresponding beats are therefore treated as controls in Fig. 1.

The polygons represent police beats, and the gray solid lines denote police district boundaries. Beats containing a dispensary are shaded black, while gray beats indicate the control group. A subset of control beats selected through a matching procedure, discussed in Sect. 5, are represented by gray striped polygons. Five police beats located around O'Hare International airport, the empty circle in Fig. 1, are excluded from the sample as these areas have atypical policing and crime patterns due to airport-specific operations and dedicated CPD resources. The result is an unbalanced panel of 270 beats across 24 quarters beginning in January 2012 and ending in December 2017.[5]

4.3 Policing data

I supplement the crime data with incident-level traffic stop data, also aggregated to the beat level. These data were collected and made available by the Stanford Open Policing Project and include information on the police beat where the stop occurred,

[3] These data were retrieved from staffing dashboard operated by the City of Chicago Inspector General and do not include higher-level positions (i.e., sergeants, lieutenants, commanders, or captains). July 2023 is the first date for which these data are available. It can be accessed at https://igchicago.org/information-portal/data-dashboards/sworn-cpd-member-staffing-map-2/.

[4] Table 8 of the Appendix provides details on the dates when licenses were granted and, when available, the dates when marijuana sales commenced. This information was collected by the author through an exhaustive internet search of newspaper articles.

[5] Six beats did not report stop data in 2012; otherwise, the panel is balanced. Results are not sensitive to the exclusion of these six beats.

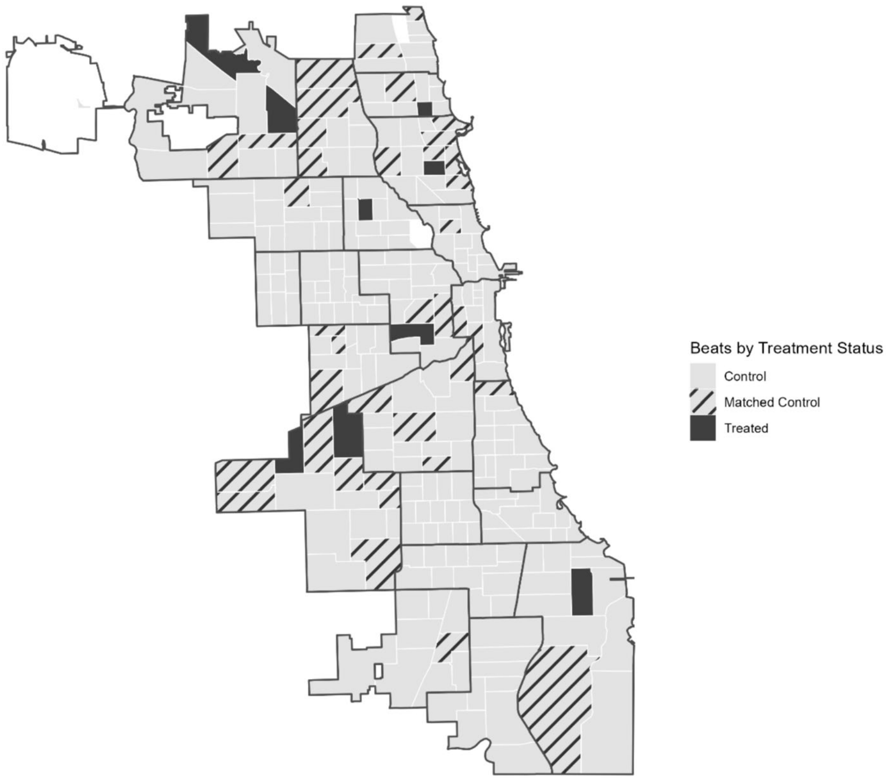

Fig. 1 Location of Chicago dispensaries by beat. The figure displays police beat boundaries. Beats shaded in black contain a dispensary. Gray beats constitute the control group; striped beats indicate the propensity score–matched subset. Solid gray lines denote police district boundaries. The white circle in the upper left highlights beats surrounding O'Hare International Airport, which are excluded from the analysis

the time and date of the stop, a short description of the stop, the year of the vehicle and the race and gender of the driver and the stop outcome—whether it resulted in a warning or a ticket (Pierson et al. 2020). Stops are classified as moving or non-moving. Moving stops are made in response to driving behaviors such as speeding, tailgating, or running a red light. The most common reasons for non-moving stops are equipment failures (e.g., broken headlights) and expired registration.

Parking ticket data obtained through a Freedom of Information Act request filed by ProPublica provide additional measures of police presence. This dataset offers two advantages. First, it contains a unique identifier for individual officers, allowing me to determine how many officers are operating in a specific beat at a specific time. Second, it distinguishes between the issuers of parking tickets: 39 percent of tickets are written by Chicago Police Department (CPD) officers and 61 percent are issued by parking enforcement aides (PEAs). PEAs work for the Department of Finance.

They are not sworn police officers; their sole responsibility is enforcing the city's parking and vehicle ordinances.[6]

4.4 Descriptive statistics & evidence on investigatory stops

One challenge to identification is the spatial endogeneity of dispensary location; that is, characteristics guiding dispensary location are also correlated with crime and policing. In Illinois, zoning regulations limiting the number of dispensary locations, coupled with the requirement that medical dispensaries are geographically dispersed, mitigate this concern to some extent. To assess potential endogeneity, Table 1 presents statistics for the outcome variables and beat characteristics in the pretreatment period, defined as the time before the first dispensary license was issued.[7]

Treated beats tend to be whiter, higher income, less densely populated and more commercially active. And while treated and control beats are statistically indistinguishable in aggregate crime and traffic stop measures, there are significant differences in the race-specific stop measures. White drivers are stopped more frequently in treated beats, whereas Black drivers are stopped more often in control beats. Additionally, PEA agents, who primarily patrol commercial corridors, downtown areas, and business districts rather than residential areas, write more tickets in treated beats, while CPD officers issue more tickets in control beats. Taken together, these statistics suggest that dispensaries tend to locate in relatively prosperous commercial areas, an interpretation that is consistent with zoning regulations.

Returning to traffic stops, although there is no difference in aggregate stop counts, there is significant heterogeneity in the distribution of stop types across racial groups; 81 percent of traffic stops of White drivers are made for moving violations while 19 percent are made for non-moving violations. The equivalent statistics for Black drivers are 57 and 43 percent. Despite comprising a minority share of the average beat population, Black drivers are subject to non-moving stops at more than three times the rate of White drivers. This pattern aligns with existing research indicating that Black drivers are more frequently the subject of investigatory traffic stops (Epp et al. 2017). It also lends support to the argument that moving and non-moving stops are used for distinct purposes; specifically, non-moving stops are used as a pretext to search for illegal activity. Figure 2 provides additional evidence on this point.

The bars with black borders show the distribution of stops for moving violations throughout the day. This distribution peaks during the morning (7–10 AM) and evening (approximately 4–6 PM) commute periods—a pattern consistent with expectations if these stops primarily reflect violations of traffic laws.

In contrast, stops for non-moving violations, represented by the dark gray bars without borders, are concentrated between 7 PM and 2 AM. If nighttime drivers were more likely to violate traffic laws relative to daytime drivers, moving stops

6 See Appendix Part I for additional information on the treatment of various data sources.

7 A full set of descriptive statistics are presented in Appendix Table 9.

Table 1 Comparison of pretreatment means by dispensaries status

	Treated	Control	*p*
Crime[a]			
Violent	67.6	76.6	0.01*
Property	91.1	88.4	0.48
Burglary	17.0	15.8	0.24
Theft	63.0	61.2	0.60
Motor vehicle theft	11.1	11.3	0.69
Criminal trespass	7.0	7.3	0.62
Deceptive practices	13.0	13.8	0.46
Narcotics	19.0	29.2	0.01***
Stops[b]			
Total	74.8	72.6	0.67
Moving	51.2	48.1	0.44
Moving–White	19.1	13.4	0.00***
Moving–Black	13.7	20.7	0.00***
Moving–Hispanic	14.8	11.5	0.02***
Non-moving	23.7	24.5	0.67
Non-moving–White	4.6	3.3	0.014**
Non-moving–Black	11.6	14.7	0.06*
Non-moving–Hispanic	6.7	5.9	0.30
Parking Tickets[c]			
CDP officers issuing tickets	122.2	156.7	0.00***
PEA officers issuing tickets	53.2	42.3	0.00***
CDP issued tickets	625.0	749.5	0.00***
PEA issued tickets	1587	1126	0.00***
Characteristics[d]			
Total population	3589	3,767	0.18
Black population share	11.9	43.0	0.00***
Population over 18	80.2	76.8	0.00***
Median income	45,970	55,336	0.00***
Household density	2366	2974	0.08*
New business licenses	15.2	11.7	0.02**
Business licenses renewed	25.5	22.6	0.53

The pretreatment period is defined as the time prior to quarter 4 of 2015, when the first dispensary opens. Data:

[a]Chicago Data Portal, available at https://data.cityofchicago.org

[b]Stanford Chicago Open Policing Project

[c]Propublica, available at https://www.propublica.org/nerds/download-chicago-parking-ticket-data

[d]License and crime data are sourced from the City of Chicago's data portal. All other variables are drawn from the American Community Survey. All data span 2012 through 2017

*p < .10; **p < .05; ***p < .01

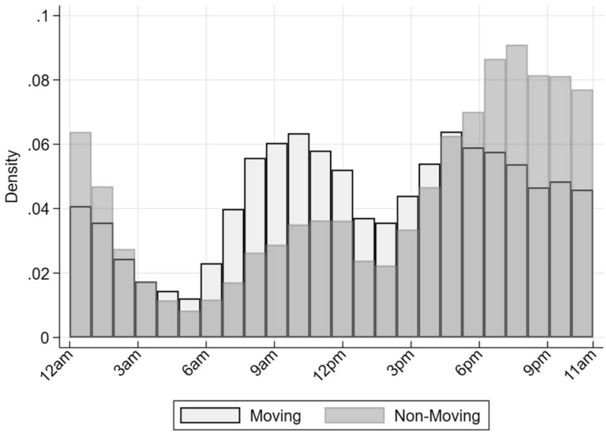

Fig. 2 Distribution of traffic stops by hour of the day. Figure depicts the distribution of stops for moving violations and stops for non-moving violations by the hour of the day the stop was made. Data: Stanford Chicago Open Policing Project, 2017–2017

would show a similar pattern. Instead, Fig. 2 indicates that police use non-moving traffic stops as a pretext to search for other illegal activity during hours when crime is more likely to occur.

A final piece of evidence suggesting police use non-moving traffic stops as part of a proactive policing strategy is ticketing rates, the proportion of stops resulting in an

Table 2 Descriptive information on traffic stop outcome variables

	# Stops	Percent receiving a citation (%)
Moving stops		
Traffic sign or signal	190,075	52.3
Other	133,629	52.6
Speed	46,947	83.0
Lane violation	28,043	45.2
Seat belt	14,270	8.1
Follow too close	1041	60.8
Non-moving stops		
Equipment	196,762	28.8
License plate/registration	131,130	46.7
Commercial vehicle	11	100

Column '# stops' report the total number of traffic stops while column 'ticket proportion' reports the percentage of stops that result in an official citation. All information in the table is drawn from the crime and stop datasets prior to aggregation. Data: Stanford Chicago Open Policing Project, 2017–2017 official citation. Table 2 reports the number of tickets and ticketing rates by offense type in the raw data.

The violations most likely to result in a citation are speeding (83 percent) and following to close (61 percent), both of which are moving violations and arguably represent the most dangerous driving behaviors. In contrast, only 29 percent of stops for equipment failure and 47 percent of stops for license plate/registration issues, both non-moving violations, result in a citation. If the primary purpose of police traffic stops is to address the cited offense, citation rates would resemble those observed for moving violations.

5 Empirical strategy and findings

5.1 Two-way fixed effects

I begin my analysis with a two-way fixed effects model examining the relationship between dispensary entry, crime, and policing. The model takes the following form:

$$\lg Y_{it} = \beta_0 + \beta_1 \text{Dispensary}_{it} + \beta_2 Z_{ij} + \eta_i + \partial_t + \in_{it} \tag{1}$$

where Y_{it} is the log-transformed value of the outcome variable in beat i in quarter t. There are three sets of outcome measures: crime, traffic stops, and parking tickets. Details on each set of outcome variables are provided in the 'Findings' sections that follow. Dispensary_{it} captures beat i's treatment status and is equal to one if a dispensary license has been issued in beat i and quarter-year t. Z_{ij} is a vector of annual covariates: the percent of population that is male, Black and over 18, percentage of renters, household density, median income, total population as well as the number of new business licenses, which is measured quarterly. Districts develop their own cultures and practices such that there is a district-level effect on patrol practices (Hassell 2007). Beat fixed effects, represented, η_i, account for these types of unobservable, time-invariant characteristics that may vary across districts, as well as environmental factors that vary at a local level. The inclusion of quarter-year fixed effects, ∂_t, absorbs variation through time in outcome variables across the study area.

To minimize potential bias arising from the non-random placement of dispensaries, propensity score matching is used to construct a control group that closely resembles the treatment group along relevant observable characteristics. Separate control groups are constructed for crime, traffic stops, and parking tickets analyses. Table 3 presents the covariates used in the matching procedure, along with the group means and the associated p-values.

The matching procedure resulted in strong covariate balance across the treatment and three control groups, most notably race and income, characteristics that are typically thought to be associated with dispensary location (Boggess et al. 2014). This, however, does not preclude the possibility that treated and control groups differ along unobservable dimensions. Finally, many decisions regarding strategy, patrol activities and resource allocation take place at the police district, raising the concern that the error structure is correlated within districts. Therefore, errors are clustered

Table 3 Propensity score matching, group means with matched control groups

		Crime & stop analysis		Parking ticket analysis	
	Treatment	Control	p	Control	p
Black population share	11.99	10.90	0.887	10.08	0.803
Median income (log)	11.60	11.68	0.579	11.64	0.788
Rental unit share	53.04	53.37	0.967	50.46	0.769
Share of population over 18	80.35	82.87	0.460	81.50	0.733
Household density	2378	3110	0.577	2883	0.713
New business licenses (log)	2.85	2.99	0.643	3.01	0.593

The table reports mean values for police beats with a dispensary (treatment group) and matched control beats constructed using propensity score matching (nearest neighbor, six matches per treatment beat). The same matched control group is used for the crime and traffic stop analyses. The control group used in the parking ticket analysis differs slightly due to missing observations in the parking ticket data. Reported p-values test for differences in means between treatment and control groups, indicating no statistically significant differences across covariates after matching

$^{*}p<.10$; $^{**}p<.05$; $^{***}p<.01$

at the district level rather than the beat level where treatment is assigned. To account for the small number (22) of police districts I implement the wild cluster bootstrap procedure for robust inference with few clusters articulated in Cameron et al. (2008).

5.2 Stacked differences-in-differences

DiD specifications relying on the staggered timing of treatment for identification may be biased if treatment effects vary over time or across treated cohorts (Goodman-Bacon 2021; Sun and Abraham 2021; Borusyak et al. 2024). The stacked differences-in-differences identification strategy developed by Cengiz et al. (2019) avoids potential biases by creating event-specific datasets that exclude early and late adopters from other time periods, generating treatment effect estimates based on within-event comparisons. To implement the stacked DiD I create nine unique datasets, each representing a unique treatment date, which are stacked to create the final dataset. I then estimate a regression relating the timing of treatment to traffic stops of the following form:

$$Y_{ict} = \beta_0 + \sum_{k=-12}^{5} \text{Dispensary}_{k,ict}\gamma_k + \beta_2 X_{it} + \beta_3 Z_{ij} + \eta_i + \partial_j + \in_{ict} \tag{2}$$

where Y_{ict} is the log-transformed number of outcomes in beat i, for event c, in quarter t. Dispensary$_{k,ict}$ is a series of indicators representing the quarters before and after a license is awarded and is recentered so that the reference category ($k=0$) is equal to one in the quarter the license is awarded. The pretreatment indicators (γ_{-12+} $-\gamma_{-1}$) capture differences in the outcome variable between treated and non-treated units in the pretreatment period. γ_{-12+} is a binned indicator interpreted as 'a dispensary opened in beat i 12 or more years ago.' One advantage of this approach is that

Table 4 Effect of dispensary entry on crime

	Panel A: Violent crimes		
	(1)	(2)	(3)
Unmatched OLS Sample	−0.342*	−0.045	−0.001
	(0.133)	(0.122)	(0.034)
PSM Matched Sample	−0.166	−0.141	−0.033
	(0.099)	(0.079)	(0.054)
	Panel B: Property crimes		
	(1)	(2)	(3)
Unmatched OLS Sample	−0.142	−0.184	−0.042
	(0.134)	(0.129)	(0.029)
PSM Matched Sample	−0.333**	−0.256*	−0.057
	(0.151)	(0.107)	(0.034)
Controls	x	✓	✓
Beat fixed effects	x	x	✓
N (unmatched sample)	6432	6432	6432
N (matched sample)	1104	1104	1104

Results are presented using the full group of control beats (Unmatched OLS Sample) and a control group generated using propensity score matching (PSM Matched Sample). Every model includes a full set of controls, beat and quarter-year fixed effects. Robust standard errors, shown in parenthesis, are clustered at the police district. Standard errors are presented in parentheses and are calculated from wild cluster bootstrap *t*-statistics and *p*-values, clustering at the police district level with 19,999 replications. Data: Chicago Data Portal, available at https://data.cityofchicago.org

$^{*}p<.10$; $^{**}p<.05$; $^{***}p<.01$

these indicators offer an empirical test of the parallel trends assumption the central identifying assumption of the DiD framework.[8] The lead treatment indicators (γ_1 to γ_{5+}) capture the treatment effect of interest.

5.3 Findings: dispensary entry and crime

I begin the analysis by presenting the effect of dispensary operations on violent and property crime in Table 4.

Results using three modeling choices are presented. First, I report estimates using all available control units (unmatched OLS sample) and the subset of control units selected via propensity score matching (PSM matched sample). Second, I report results without covariates or unit fixed effects (col 1), with only covariates (col 2)

[8] In the pretreatment period, event years are binned at year 12 because this is the complete year, i.e., the last year every beat that will eventually host a dispensary is equal to one. In the post treatment period event years 1, 2 and 3 are complete. I bin at event year 5 to maximize the number of post treatment periods.

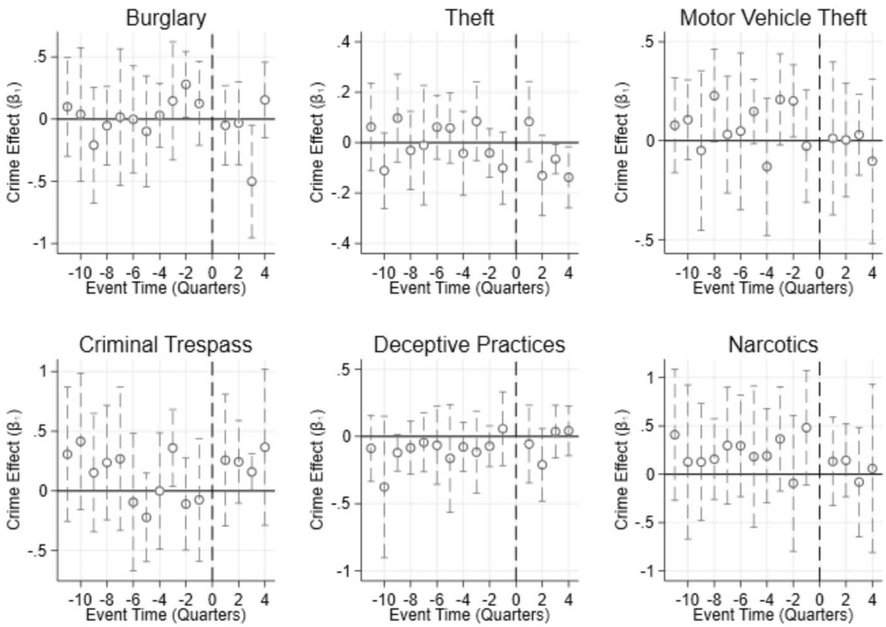

Fig. 3 The effect of dispensary entry on disaggregated crime. The figure reports year-specific treatment effect estimates of medical marijuana dispensary entry on crime. Assault, battery, and robbery are classified as violent crimes; all remaining offenses are classified as property crimes. The omitted period is event quarter 0, corresponding to the quarter in which the license is issued. Pretreatment periods beyond 12 quarters are binned. The figure reports 90 percent confidence intervals. Robust standard errors, shown in parentheses, are clustered at the police district level. *p*-values are obtained using a wild cluster bootstrap with 19,999 replications to account for the small number of clusters. Data: Chicago Data Portal, available at https://data.cityofchicago.org

and with covariates and beat fixed effects (col 3). Finally, *p*-values are derived from the wild cluster bootstrap (WCB) procedure and robust standard errors clustered at the district level are reported in parentheses.

Treatment effect estimates are uniformly negative but generally imprecise. Estimates from the preferred specification exploiting within-beat variation (column 3) suggest property crime declines of three to six percent; however, these effects are not statistically significant.

Prior research suggests that the effects of marijuana are highly localized (Chang and Jacobson 2017; Brinkman and Mok-Lamme 2019). However, it is possible that beats close to dispensaries but without a dispensary themselves see changes in criminal activity. To account for this, I present results excluding beats that do not host a dispensary but share a border with a beat that does. These estimates are presented in Table 10. Estimates remain negative but insignificant.

Prior research suggests that dispensaries may affect specific categories of crime. It is also possible that the average treatment effect understates the true effect magnitude since dispensaries were not operating in a portion of the post-treatment period. To investigate these possibilities, I estimate year-specific treatment effects for six subcategories of property crime using Eq. 2 : burglary, theft, motor vehicle theft, criminal trespass, deceptive practices, and narcotics. Results using the PSM control group are presented in Fig. 3.

The vertical dashed line marks the reference quarter ($k=0$), corresponding to the quarter in which the license was awarded. Across most categories, estimated coefficients fluctuate around zero both before and after treatment, suggesting that treated and control beats followed similar crime trajectories prior to dispensary entry and that dispensary openings did little to alter those trends.

Year-specific treatment effect estimates reveal a one-quarter delay, which is expected given that the treatment date is defined by dispensary license issuance rather than the commencement of retail sales. One exception to this pattern is theft, where coefficients begin trending downwards in event year $k=2$, likely reflecting the lag between license issuance and dispensary opening. Consistent with this interpretation, estimates for $k=3$ and $k=4$ are more precisely estimated than those for $k=2$. In these years, the estimated effects imply that dispensary openings are associated with declines in theft of approximately 6.6 percent ($k=3$, $p<0.1$) and 13.7 percent ($k=4$, $p<0.05$). For context, in the average beat this equates to between six and 12 fewer theft offenses per quarter. These results align with Chang and Jacobson (2017), who find that abrupt dispensary closings increase larceny offenses by nine to 12 percent within a half to one third mile radius.

This analysis is limited by the relatively short post-treatment observation window. A longer period would enable a clearer assessment of whether the observed decline in thefts persists or instead reflects imprecision in the treatment effect estimates. Nonetheless, the consistently negative estimates on theft—particularly in contrast to the absence of such patterns in other crime categories—suggest that dispensaries may have a modest but meaningful effect on a specific type of crime. The lack of any detectable change in narcotics offenses further supports the conclusion that dispensaries are not the ‘crime magnets’ some have feared.

In the next section, I turn to the question of how dispensary entry influences police activity.

5.4 Findings: dispensary entry and police presence

In this section, I use the parking ticket data to examine how dispensaries affect police presence, measured by the number of parking tickets issued by CPD officers and the number of CPD officers issuing tickets. To provide a more comprehensive assessment of the impact of dispensaries, I also estimate effects on the average fine amount, the number of unpaid tickets, the number of tickets incurring additional fees, the number of vehicle seizures, and the number of cases sent to collections.

I address potential confounding from local area characteristics correlated with dispensary location by taking advantage of a natural comparison group—parking enforcement aides. If dispensaries systematically locate in areas becoming more commercial or affluent, we expect to observe changes in the presence and activity of both CPD officers and PEAs. The absence of such parallel effects provides further

Table 5 Effect of dispensary entry on parking ticket outcomes

	# tickets	Fine amount	# unpaid tickets	# tickets with extra fees	# vehicle seizures	# cases sent to collections	# of officers
Panel A: Chicago police department officers, unmatched OLS sample							
Dispensary license awarded	108.262	0.083	33.078	29.832*	16.176	8.985	15.124**
	(52.095)	(1.337)	(22.230)	(10.892)	(10.800)	(5.390)	(4.292)
N	6312	6312	6312	6312	6312	6312	6312
Panel B: Chicago Police Department Officers, PSM Matched Sample							
Dispensary license awarded	84.707	0.068	22.984	17.736	5.618	10.757*	4.897*
	(44.154)	(1.762)	(18.859)	(8.957)	(9.993)	(4.266)	(2.715)
N	1224	1224	1224	1224	1224	1224	1224
Panel C: Parking Enforcement Aides, Unmatched OLS Sample							
Dispensary license awarded	−50.486	0.933	−1.679	−0.414	−6.608	0.622	−0.299
	(73.076)	(1.082)	(31.320)	(31.140)	(15.137)	(8.063)	(1.800)
N	6312	6312	6312	6312	6312	6312	6312
Panel D: Parking Enforcement Aides, PSM Matched Sample							
Dispensary license awarded	−90.191	0.231	−40.284	11.337	−8.872	−2.967	0.198
	(116.402)	(1.065)	(32.079)	(25.448)	(23.249)	(10.424)	(1.463)
N	1224	1224	1224	1224	1224	1224	1224

Outcome variables are listed in column heads. Other than fine amount, which is a dollar value, outcomes are counts. Tickets with additional fees are those that include financial penalties that are in addition to the amount of the initial citation. These are typically late fees and/or collection fees. Results are presented using the full group of control beats (Unmatched OLS Sample) and a control group generated using propensity score matching (PSM Matched Sample). Every model includes a full set of controls, beat and quarter-year fixed effects. Robust standard errors, shown in parenthesis, are clustered at the police district. Standard errors are presented in parentheses and are calculated from wild cluster bootstrap t-statistics and *p*-values, clustering at the police district level with 19,999 replications. Data: ProPublica, available at https://www.propublica.org/nerds/download-chicago-parking-ticket-data

$^{*}p<.10$; $^{**}p<.05$; $^{***}p<.01$

evidence that the areas where dispensaries locate do not systematically differ from police beats without dispensaries with respect to public safety.

Table 5 reports the estimated effects of dispensary openings on various parking enforcement outcomes from the DiD analysis of Eq. 1. Panels A and B present results for Chicago Police Department (CPD) officers, while Panels C and D present results for parking enforcement aides (PEAs). Estimates for both the unmatched and PSM samples are provided for both groups.

Dispensary openings are associated with increases in both the total number of tickets and the number of tickets in specific outcome categories; however, these estimates are generally not statistically significant. In contrast, the number of officers issuing tickets increases following dispensary openings. Estimates in Panel A indicate an increase of approximately 15 officers, although this effect falls to about five officers when using the matched control group. By comparison, no statistically significant effects are observed for parking enforcement aides in either the unmatched or matched samples (Panels C and D). Again, I explore these results graphically.

Panel A presents treatment effect estimates for the number of officers, while Panel B reports estimates for the number of tickets. For PEA agents, effect sizes are statistically insignificant and close to zero in magnitude across the study period. Estimates for CPD officers are likewise null in the pretreatment period but exhibit a clear upward trend following treatment. Officers also issue more tickets, with evidence of a delayed treatment response: there is no detectable effect in the quarter immediately following dispensary entry ($k=1$), while approximately 166 additional tickets are issued in k = 2 ($p < 0.05$), suggesting that ticketing estimates in Table 5 are attenuated. Appendix Fig. 6 reports corresponding results for fine amounts, unpaid tickets, tickets with additional fees, vehicle seizures, and cases sent to collections. While there is no effect on fine amounts for tickets issued by CPD officers (Panel A), all other outcomes increase following dispensary entry and exhibit a similar delayed response. No effects are observed for PEAs across any outcome (Panel B). (Fig. 4)

5.4.1 Findings: redistribution or changes in behavior?

One explanation for the increase in the number of police officers observed in areas surrounding dispensaries is a reallocation of patrol resources within districts as officers are shifted towards areas surrounding dispensaries. An alternative explanation is that dispensary entry induces changes in officer behavior, with officers engaging in more proactive policing in and around dispensary locations. Such a response is consistent with a core objective of Chicago's CAPS strategy.

To test these competing explanations, I estimate treatment effects for control beats in close proximity to dispensaries. Appendix Fig. 7 maps police beats and classifies control beats according to whether they are located within 1 mile, 1–2 miles, or more than 2 miles from a dispensary. It also depicts police district boundaries and illustrates the lack of a dispensary in many districts (non-treated districts henceforth).

I use Eq. 1 to estimate the effect of dispensaries on police presence in control beats in treated districts within each distance category. Treated beats are excluded from the analysis and beats located in non-treated districts serve as controls.

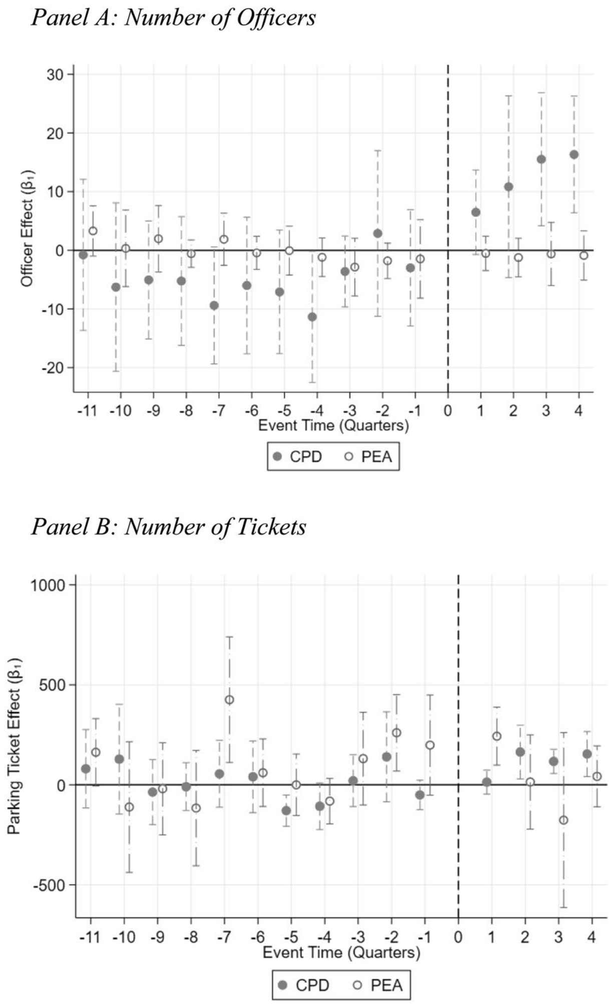

Fig. 4 The effect of dispensary entry on police presence. Panel A: Number of Officers. Panel B: Number of Tickets. Each panel reports treatment effect estimates for two distinct groups, Chicago Police officers (CPD) and by Parking Enforcement Aides (PEAs). The omitted period is event quarter 0, corresponding to the quarter in which the license is issued. Pretreatment periods beyond 12 quarters are binned. The figure reports 90 percent confidence intervals. Robust standard errors, shown in parentheses, are clustered at the police district level. *p*-values are obtained using a wild cluster bootstrap with 19,999 replications to account for the small number of clusters. Data: ProPublica, available at https://www.propublica.org/nerds/download-chicago-parking-ticket-data

Because police resources are allocated at the district level, there is no a priori reason to expect these beats to be directly affected by dispensary entry. The treatment date is defined as the quarter in which the first dispensary opens in the relevant district.

If officers are reassigned to areas with dispensaries, enforcement activity in nearby control beats should decline. If, on the other hand, changes in officer behavior are primarily responsible for the observed increase in officer presence then treatment effect estimates could be null (changes in police behavior strictly confined to beats with dispensaries) or positive (changes in police behavior not strictly confined to beats with dispensaries). In the case of the latter, positive effects are expected to diminish as distance from the dispensary increases.

Table 6 presents results.

Panels A and C report results for the number of tickets issued by CPD officers and the number of CPD officers issuing tickets, respectively. The equivalent estimates for PEAs are presented in Panels B and D. Finally, for reference, the first column presents results from the main analysis.

Treatment effect estimates in Panels A and C are uniformly positive, providing no evidence that dispensaries lead to a redistribution of officers within districts. Insteaed results suggest a change in police behavior that is most acute in areas surrounding dispensaries but extends beyond the geographic limits of the police beat. The number of officers issuing tickets increases by 13 in beats within two miles of a dispensary ($p < 0.05$). At greater distances, estimates are smaller and lack statistical significance. The estimates on number of tickets are positive but imprecise and exhibit no clear spatial pattern. Taken together, these findings suggest that changes in officer behavior, rather than a redistribution of officers, is responsible for increases in police presence following dispensary entry.

Table 6 Effects of dispensary entry on parking ticket outcomes in non-dispensary beats by proximity to dispensaries

		Distance from dispensary			
	Full sample	< 1 mile	1–2 miles	> 2 miles	> 3 miles
Panel A: Chicago Police Department Officers, Number of Tickets					
Dispensary license awarded	108.262	27.863	59.718	95.807	85.763
	(52.095)	(54.272)	(63.470)	(62.840)	(107.791)
Panel B: Parking Enforcement Aides, Number of Tickets					
Dispensary license awarded	−50.486	−89.120	−42.377	−26.743	−70.170
	(73.076)	(65.664)	(63.702)	(62.573)	(60.865)
Panel C: Chicago Police Department Officers, Number of Officers					
Dispensary license awarded	15.124**	13.344**	13.200**	9.995	4.063
	(4.292)	(4.916)	(4.805)	(9.827)	(20.727)
Panel D: Parking Enforcement Aides, Number of Officers					
Dispensary license awarded	−0.299	−4.265	−1.592	3.279	4.352
	(1.800)	(2.189)	(2.088)	(1.519)	(1.064)
N	6312	5112	4824	4728	4488
Number of treated beats	9	33	37	21	9

Every model includes a full set of controls, beat and quarter-year fixed effects. Robust standard errors, shown in parenthesis, are clustered at the police district. Standard errors are presented in parentheses and are calculated from wild cluster bootstrap t-statistics and *p*-values, clustering at the police district level with 19,999 replications. Data: ProPublica, available at https://www.propublica.org/nerds/download-chicago-parking-ticket-data

*$p < .10$; **$p < .05$; ***$p < .01$

Positive effects in control beats in close proximity to dispensaries raise the possibility that the main results are attenuated because a portion of the control beats are affected by the treatment. To address this concern, I repeat the main analysis using Eq. 1, excluding control beats located in treated districts so that only beats in non-treated districts serve as controls. Results are presented in Table 11. Treatment effect estimates for CPD officers increased modestly—to 18.99 officers ($p < 0.05$) and 124.9 tickets ($p < 0.1$)—relative to the corresponding estimates of 15.1 officers ($p < 0.05$) and 108.3 tickets reported in Table 5. This pattern suggests some attenuation in the full-sample estimates; however, the statistical significance and substantive conclusions remain unchanged.

5.5 Findings: dispensary entry and police behavior

Having established dispensary entry leads to an increase in police presence in the surrounding neighborhoods, I next analyze traffic stop data to examine potential changes in police behavior. To do so, I estimate the effect of dispensaries on the total number of traffic stops, traffic stops for moving violations and traffic stops for non-moving violations. Because moving and non-moving stops are often used for different purposes, analyzing them separately provides valuable insight into how police are responding to dispensary entry. One possibility is that dispensaries are not associated with changes in police activity. This could happen if dispensaries have no impact on the local environment; they do not increase traffic flow or lead to higher rates of dangerous driving, and therefore do not generate additional demand for police services. *In this case we would not expect there to be any change in the number of traffic stops following dispensary entry.*

A second possibility is that dispensaries boost traffic flow in the surrounding areas. Given the product they sell, dispensaries may also be linked to higher rates of impaired or dangerous driving. In this scenario, dispensaries create additional demand for police services. In this case, *dispensaries are expected to increase the number of traffic stops, with the most significant rise occurring in stops for moving violations.*

A final possibility is that dispensaries do not create additional demand for police services through altering road conditions. However, because dispensaries are viewed as detrimental to public safety, police supply more policing services to address the perceived threat. In this case, *we expect an increase in stops for non-moving violations without a corresponding increase in stops for moving violations.*

I use Eq. 1 to test these competing predictions. Results are reported in Table 7.

Panel C presents estimates for non-moving stops, which exhibit sensitivity to the inclusion of control variables. This likely reflects correlation between police activity and neighborhood characteristics rather than endogeneity in dispensary location—if endogeneity were the primary cause of sensitivity in the estimates, we would expect similar instability in the crime estimates reported in Table 4, which we do not observe. After controlling for covariates associated with both enforcement activity and neighborhood characteristics, most notably median income and Black population share, as well as time-invariant beat characteristics (column 3), the results

Table 7 Effect of dispensary entry on traffic stops

	Panel A: All stops		
	(1)	(2)	(3)
Unmatched OLS sample	−0.238	−0.142	0.116
	(0.176)	(0.156)	(0.119)
PSM matched sample	0.042	0.115	0.351***
	(0.140)	(0.124)	(0.100)
	Panel B: Moving violation stops		
	(1)	(2)	(3)
Unmatched OLS Sample	−0.166	−0.181	0.108
	(0.151)	(0.144)	(0.124)
PSM matched sample	0.028	0.119	0.368***
	(0.136)	(0.114)	(0.097)
	Panel C: Non− moving violation stops		
	(1)	(2)	(3)
Unmatched OLS Sample	−0.289	0.012	0.169
	(0.225)	(0.201)	(0.106)
PSM matched sample	0.104	0.135	0.300**
	(0.191)	(0.155)	(0.124)
Controls	x	✓	✓
Beat fixed effects	x	x	✓
N (unmatched sample)	6432	6432	6432
N (matched sample)	1104	1104	1104

Results are presented using the full group of control beats (Unmatched OLS Sample) and a control group generated using propensity score matching (PSM Matched Sample). Every model includes a full set of controls, beat and quarter-year fixed effects. Robust standard errors, shown in parenthesis, are clustered at the police district. Standard errors are presented in parentheses and are calculated from wild cluster bootstrap t-statistics and *p*-values, clustering at the police district level with 19,999 replications. Data: Stanford Chicago Open Policing Project, 2012–2017

$^{*}p < .10$; $^{**}p < .05$; $^{***}p < .01$

indicate increases in total stops, moving stops, and non-moving stops. However, these findings are limited to the weighted sample and do not withstand additional analysis. Year-specific estimates presented graphically in Appendix Fig. 8 reveal no consistent upward trend in any of the three outcomes and pretreatment estimates suggest that the parallel trends assumption does not hold.

5.5.1 Heterogeneous effects

Prior research has examined heterogeneous effects of dispensaries by estimating changes in crime across neighborhoods that differ in income levels and racial composition (Dong and Tyndall; Burkhardt and Goemans 2019). I extend this approach by estimating heterogeneous effects along two dimensions that are particularly relevant to police enforcement patterns. The first dimension is driver race. Given documented racial disparities in stop and frisk (Hausman and Kronick 2023) and traffic enforcement (Mughan and Singla 2023), street-level enforcement mechanisms that police may utilize when responding to perceived criminal risk posed by dispensary entry, I estimate effects for White, Black and Hispanic drivers.

The second dimension is the time of day. Police may reasonably anticipate that dispensary-related criminal activity is more likely to occur at night and may therefore concentrate enforcement efforts during these hours. I distinguish between daytime stops (9:00 a.m. to 9:00 p.m.), which align with dispensary operating hours and daylight, and nighttime stops (10:00 p.m. to 8:00 a.m.), when dispensaries are closed. If dispensaries lead police to use non-moving stops as a pretext for detecting other criminal activity, treatment effects should be most pronounced outside of business hours, when the perceived risk of crime is higher and visibility is lower. DiD estimates of Eq. 2 along these dimensions are presented graphically in Fig. 5.

Panel A presents results for moving stops for White, Black and Hispanic drivers. Point estimates for nighttime stops are represented by the dark circles while the white circles report estimates for daytime stops. There is no observable change in moving stops following dispensary entry for drivers of any race, regardless of time of day. Equivalent results for non-moving stops are presented in Panel B. Again, there is no change in stops of Black or Hispanic drivers. However, there is a clear and significant increase in the number of stops of White drivers for stops for non-moving violations made between 10PM and 8AM. The magnitude of these effects ranges between 40 percent ($k=2$, $p<0.10$) and 80 percent ($k=4$, $p<0.01$). Given a pretreatment mean of five stops this only translates into an average of two to four more stops per quarter.

6 Discussion and conclusion

The objective of this study is to provide evidence on how marijuana dispensaries affect policing, both as a potential mechanism through which dispensaries influence crime and as a means of understanding their broader implications for public safety. I find no detectable change in reported crime following dispensary entry. However, it is not possible to determine whether dispensaries have no effect on crime or whether crime would have increased but for the other changes that accompany dispensary entry—most notably the increases in policing documented here. This is a limitation that applies to all studies of marijuana dispensaries and crime that do not account for changes in policing or other environmental factors such as security measures implemented by dispensaries that are likely correlated with dispensary entry.

The central finding of this research is that sale of medical marijuana results in an increase in police presence in areas surrounding dispensaries. This increase is reflected in higher numbers of parking tickets and a larger number of police officers issuing at least one parking citation. The absence of any changes in ticketing by parking enforcement aides who do not have law enforcement responsibilities suggest that these effects are not explained by changes in the local environment correlated with dispensary entry.

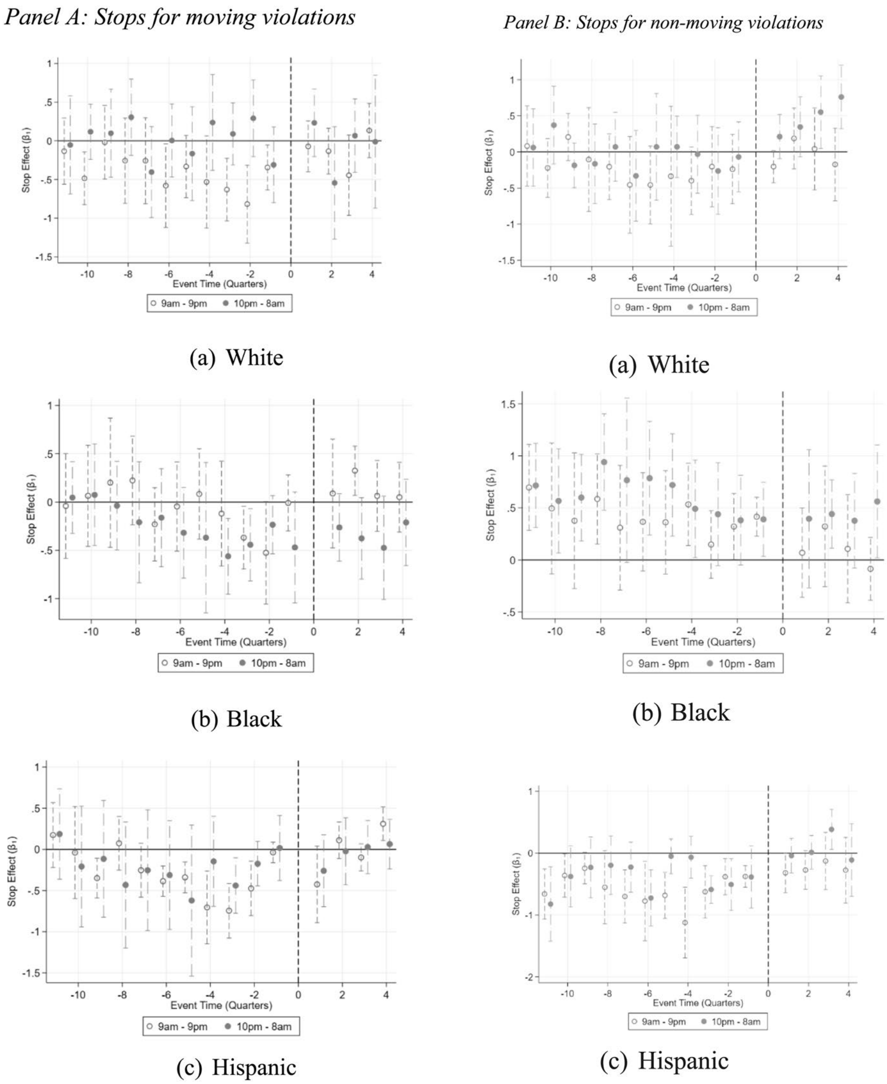

Fig. 5 Heterogeneous effects of dispensary entry on traffic stops by driver race and time of day. Panel A: Stops for moving violations. Panel B: Stops for non-moving violations. Panels A and B reports year-specific treatment effect estimates of the effect of medical dispensary entry on the number of moving and non-moving stops by driver race. '9AM–9PM' graphs report results made during typical dispensary hours with '10PM–8AM' results made when dispensaries are closed. The omitted period is event quarter 0, corresponding to the quarter in which the license is issued. Pretreatment periods beyond 12 quarters are binned. The figure reports 90 percent confidence intervals. Robust standard errors, shown in parentheses, are clustered at the police district level. *p*-values are obtained using a wild cluster bootstrap with 19,999 replications to account for the small number of clusters. Data: Stanford Chicago Open Policing Project, 2017–2017

Additional analysis indicates that the increase in police presence is not the result of the reassignment of officers to areas around dispensaries. Rather, results suggest

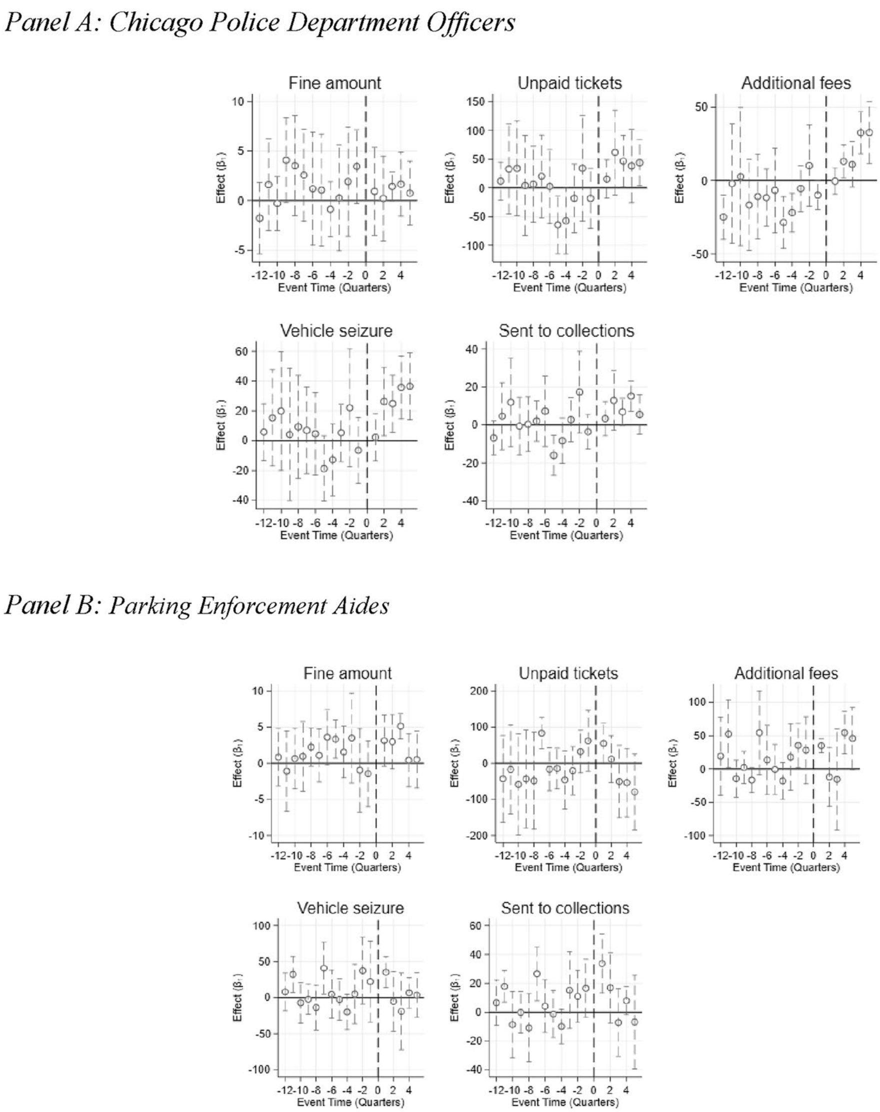

Fig. 6 Effect of dispensary entry on additional parking ticket outcomes. Panel A: Chicago Police Department Officers. Panel B: Parking Enforcement Aides. Fine amount is the dollar value of the fine associated with a ticket. All other outcomes are counts of the tickets that incurred the relevant outcomes (i.e. the unpaid fine was sent to collections). The omitted period is event quarter 0, corresponding to the quarter in which the license is issued. Pre-treatment periods beyond 12 quarters are binned. The figure reports 90 percent confidence intervals. Robust standard errors, shown in parentheses, are clustered at the police district level. *p*-values are obtained using a wild cluster bootstrap with 19,999 replications to account for the small number of clusters. Data: ProPublica, available at https://www.propublica.org/nerds/download-chicago-parking-ticket-data

increased use of proactive policing practices, which result in a more visible police presence. Although the data does not allow for identification of specific activities, plausible mechanisms include foot and bicycle patrols, pedestrian stops, and targeted enforcement efforts. There is limited evidence that traffic stops are used as one such tool. Although the results do not point to large, systemic changes in policing, they do reveal targeted adjustments. In particular, traffic stops of White drivers for non-moving violations increase during nighttime hours. The timing of these stops combined with the absence of comparable increases in stops for moving violations is consistent with the use of traffic stops as a pretext for investigating unrelated suspected criminal activity. And although the magnitude is modest, amounting to only a

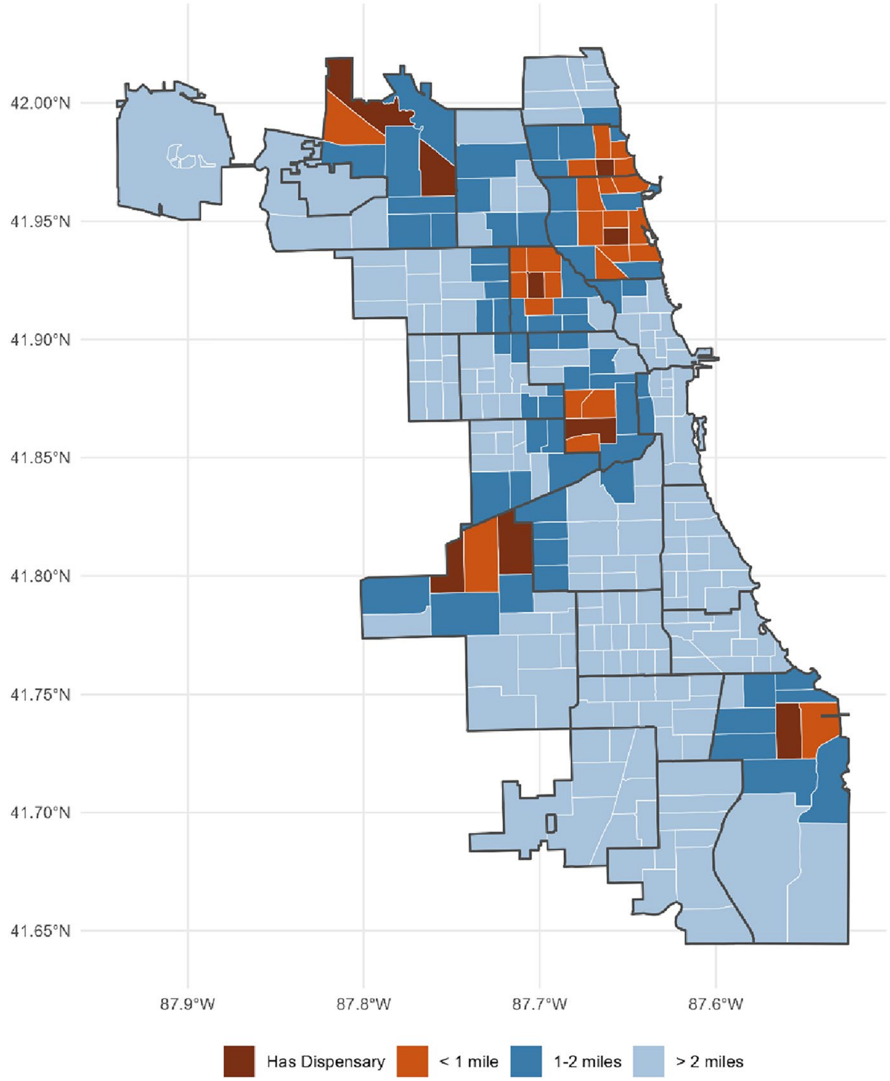

Fig. 7 The figure displays police beats, shaded according to dispensary status and proximity. The nine darkly shaded beats contain a dispensary. All remaining beats are coded by their distance to the nearest treated beat, measured from beat centroids. Solid dark gray lines denote police district boundaries

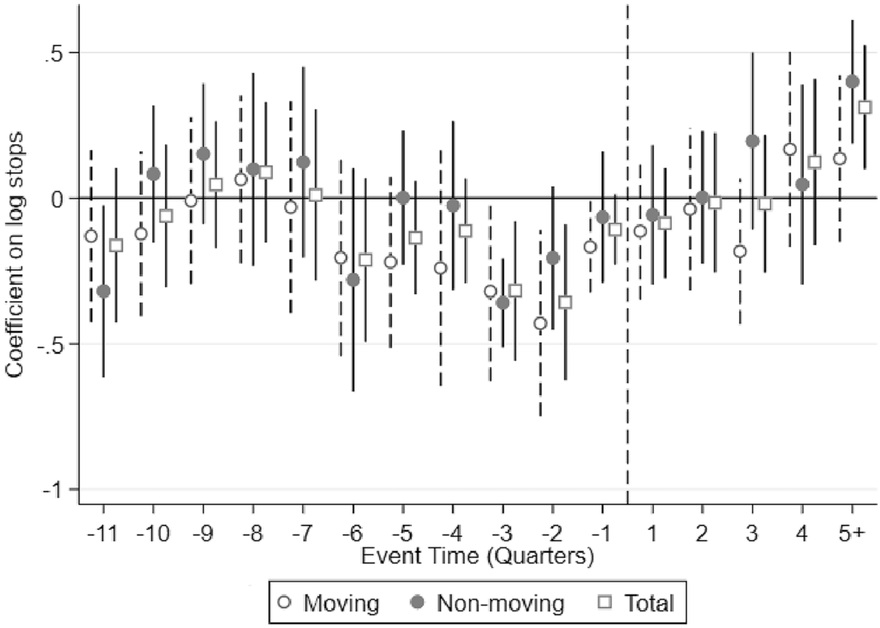

Fig. 8 Effect of marijuana sale on traffic stops. The omitted time period is event quarter 0, the period the medical marijuana dispensary license was issued. Pre-period values are binned at 16 years. For clarity only 12 pretreatment periods are presented. The vertical dashed line indicates the time prior to and following license issuance. 90% confidence intervals. Robust standard errors, shown in parenthesis, are clustered at the police district. A wild cluster bootstrap procedure is implemented at the police district level to account for the small number of clusters. The resulting *p*-values are based on 19,999 replications. Data: Stanford Chicago Open Policing Project, 2017–2017

few additional stops per beat per quarter, it nonetheless provides further evidence of meaningful changes in police activity surrounding dispensary entry.

By increasing police presence, dispensaries have unintended effects on neighborhoods. A heightened police presence is associated with more investigatory traffic stops. To the extent that investigatory traffic stops impose financial, physical, or psychological burdens, they represent a welfare loss for drivers. Increased police presence also results in additional citations and associated penalties. While seemingly minor events, nonpayment of these tickets have detrimental impacts, particularly for

Table 8 Dispensary names, license and opening dates

	License issue date	Opening date
Dispensary 33	11/19/2015	12/10/2015
Ascend MOCA-Logan Square	2/1/2016	9/22/2016
Midway by Ascend	4/13/2016	4/26/2016
Maribis of Chicago	7/27/2016	09/12/2016
Cannabist–Chicago	8/29/2016	09/6/2016
Consume: Chicago	11/10/2016	–
Sunnyside—Wrigleyville	1/13/2017	–
Zen Leaf Pilsen	1/13/2017	–
Mission Illinois	6/8/2017	–
Zen Leaf–Rogers Park	3/23/2018	–
NuEra Chicago	7/30/2018	–

Opening dates indicate the date the dispensary opened for business were sourced from local newspaper reporting and are provided where available. The date each issued were obtained from the Illinois Department of Financial and Professional Regulation's website (IDFPR). Data: License data from IDFPR. Opening dates was collected by the author through an exhaustive internet search of newspaper articles and were not located for all dispensaries

financially fragile households (Mello 2018); a parking ticket sent to collections may limit access to credit, losing the ability to drive may reduce job opportunities.

Further research on the non-crime-related costs generated by dispensaries is essential for understanding their aggregate effects on local communities. In particular, additional work is needed to examine how dispensaries affect enforcement levels and patterns, as well as the downstream consequences of those changes. Such evidence is critical for individuals and policy makers seeking to weigh the potential costs of marijuana retail in their communities against its potential benefits (public safety, economic, etc.). A community might welcome dispensaries if external security measures lead to a modest decrease in property crime. However, they might reject the same dispensary if that decrease is the result of a larger or more aggressive police presence. Situating this research in a more diverse group of localities is valuable as it would facilitate a fuller understanding of the relationship between marijuana dispensaries and crime and policing.

Table 9 Descriptive Statistics

	Mean	SD	Min	Max	N
Crime[1]					
Violent	75.8	38.8	6	266	3426
Property	89.9	44.4	12	427	3426
Burglary	15.4	10.5	0	78	3426
Theft	62.3	40.9	5	410	3426
Motor vehicle theft	11.4	7.3	0	51	3426
Criminal trespass	7.18	6.1	0	61	3426
Deceptive practices	13.9	11.5	0	112	3426
Narcotics	30.4	37.9	0	298	3426
Stops[2]					
Total	75.3	60.0	5	796	3426
Moving	50.4	45.9	1	749	3426
Moving–White	14.8	24.4	0	299	3426
Moving–Black	20.4	25.6	0	469	3426
Moving–Hispanic	12.4	15.6	0	124	3426
Non-moving	24.9	22.2	0	232	3426
Non-moving–White	3.7	6.3	0	85	3426
Non-moving–Black	14.3	19.0	0	223	3426
Non-moving–Hispanic	6.9	9.2	0	85	3426
Parking Tickets[3]					
CDP officers issuing tickets	158.7	74.4	28	696	3360
PEA officers issuing tickets	45.4	26.7	3	121	3360
CDP issued tickets	776	474	66	3325	3360
PEA issued tickets	1198	1336	14	9936	3360
CDP tickets per officer	5.0	2.5	1.5	21.1	3360
PEA tickets per agent	21.9	15.0	1.4	109.5	3360
Characteristics[4]					
Total population	3832	1542	995	15,584	3426
Black population share	39.1	36.6	0	97.9	3426
Population over 18	77.0	8.5	58.1	97.6	3426
Median income	46,376	21,791	16,654	119,657	3426
Household density	3155	4222	121	50,953	3426
New business licenses	12.5	17.5	0	178	3426
Business licenses renewed	24.2	55.7	0	1,007	3426

The pretreatment period is defined as the time prior to quarter 4 of 2015, when the first dispensary opens. Six beats did not provide parking ticket data in 2012. Data:

[a]Chicago Data Portal, available at https://data.cityofchicago.org [b]Stanford Chicago Open Policing Project

[c]Propublica, available at https://www.propublica.org/nerds/download-chicago-parking-ticket-data

[d]License and crime data are sourced from the City of Chicago's data portal. All other variables are drawn from the American Community Survey. All data span 2012 through 2017

Table 10 Effect of dispensary entry on crime, excluding adjacent police beats

	Panel A: Violent crimes		
	(1)	(2)	(3)
Unweighted Sample:			
Dispensary opens	−0.391*	−0.071	−0.005
	(0.146)	(0.136)	(0.033)
Weighted Sample:	−0.164	−0.073	−0.010
Dispensary opens	(0.121)	(0.089)	(0.040)
	Panel B: Property crimes		
	(1)	(2)	(3)
Unweighted Sample:			
Dispensary opens	−0.154	−0.215	−0.054*
	(0.146)	(0.138)	(0.029)
Weighted Sample:			
Dispensary opens	−0.270*	−0.191	−0.031
	(0.145)	(0.099)	(0.029)
Controls	x	✓	✓
Beat fixed effects	x	x	✓
N (unweighted sample)	5340	5316	5316
N (weighted sample)	840	840	840

The table reports average treatment effect estimates of the effect of medical marijuana dispensary entry on the logged transformed values of violent (Panel A) and property (Panel B) crime. Results are presented using the full group of control beats (Unmatched OLS Sample) and a control group generated using propensity score matching (PSM Matched Sample). Every model includes a full set of controls, beat and quarter-year fixed effects. Robust standard errors, shown in parenthesis, are clustered at the police district. Standard errors are presented in parentheses and are calculated from wild cluster bootstrap t-statistics and *p*-values, clustering at the police district level with 19,999 replications. Data: Stanford Chicago Open Policing Project, 2017–2017

*p < .10; **p < .05; ***p < .01

Table 11 Effect of dispensary entry on police presence: robustness to exclusion of affected control beats

Panel A: Chicago Police Department Officers		
Dispensary license awarded	# tickets	# of officers
	124.934*	18.988**
	(69.087)	(5.324)
Panel B: Parking Enforcement Aides		
Dispensary license awarded	−70.256	−1.201
	(80.914)	(2.546)
N	4512	4512

Every model includes a full set of controls, beat and quarter-year fixed effects. Robust standard errors, shown in parenthesis, are clustered at the police district. Standard errors are presented in parentheses and are calculated from wild cluster bootstrap *t*-statistics and *p*-values, clustering at the police district level with 19,999 replications. Data: ProPublica, available at https://www.propublica.org/nerds/download-chicago-parking-ticket-data

*p < .10; **p < .05; ***p < .01

Appendix

Part I. Data sources

Parking ticket data

I exclude tickets issued by airport police and those from unidentified or miscellaneous agencies. Each ticket is geocoded and mapped to a census tract. I drop approximately 870,000 observations (seven percent of the remaining data) where tract information is missing. Since police beats and census tracts do not align exactly, with each beat containing multiple tracts, I create a beat-level dataset by mapping tract boundaries to beat boundaries. The resulting dataset is a linear combination of overlapping tracts and beats, weighted by the share of each tract within a given beat. The data is aggregated to the quarter and covers 2012 through 2017. See Figs. 6, 7, 8.

Supplemental data

Retail activity has consistently been shown to be positively correlated with dispensary location (Brinkman and Mok-Lamme 2019). A large retail presence suggests a significant amount of land is zoned for commercial use, there is an existing customer base, and current residents are friendly towards retail activity (Boggess et al. 2014). Bars and alcohol retailers are considered high-risk business and have been shown to be positively associated with crime (Gruenewald and Remer 2006). One the other hand, these types of establishments are positively correlated with foot traffic which may deter crime. They are also more likely to locate in gentrifying neighborhoods, which suggests decreasing crime rates (Rascoff and Humphries 2015).

To account for these factors, I sourced crime and business license data, both of which are publicly available through the city of Chicago's data portal.[9] I use total offenses reported per 100 residents to control for changes in demand for police services. I construct two measures of commercial activity, the number of licenses issued, and the number of licenses renewed. Licenses missing a start date, or a geographic coordinate are dropped. These observations account for less than 0.01 percent of the sample. The address associated with each license is geocoded to determine its exact location. Data is then aggregated to the beat level. I also collect census tract data from the US Census Bureau's American Community Survey (ACS) to further control for police beat characteristics that might be correlated with crime. I use the same weighting procedure described above to create a beat level data set.

[9] It can be accessed at https://data.cityofchicago.org.

Part II. Additional analyses

Table 8 of the Appendix provides details on the dates when licenses were granted and when dispensaries opened. The opening dates were sourced though an extensive internet search conducted by the author. However, I was unable to locate a reliable opening date for every dispensary. See Tables 9, 10, 11

Author contribution I conducted all of work for this manuscript, these activities include originating the idea, collecting the data, performing the analysis and writing and reviewing the manuscript.

Data availability There are on restrictions on access to this data. Upon publication I will make the data available in a data repository.

Declarations

Competing interests The author declares no competing interests.

References

Anderson DM, Rees DI (2023) The public health effects of legalizing marijuana. J Econ Lit 61(1):86–143
Arruza E, Keehehan S (2015) Medical Marijuana Hits the Shelves at Chicago's First Dispensary. WRRW, 2015
Becker GS (1968) Crime and punishment: an economic approach. J Polit Econ 76(2):169–217
Bernasco W, Block R, Ruiter S (2013) Go where the money is: modeling street robbers'location choices. J Econ Geogr 13(1):119–143
Boggess LN, Pérez DM, Cope K, Root C, Stretesky PB (2014) Do medical marijuana centersbehave like locally undesirable land uses? Implications for the geography of health andenvironmental justice. Urban Geogr 35(3):315–336
Borusyak K, Jaravel X, Spiess J (2024) Revisiting event-study designs: robust and efficientestimation. Rev Econ Stud 91(6):3253–3285
Brinkman J, Mok-Lamme D (2019) Not in my backyard? Not so fast. The effect ofmarijuana legalization on neighborhood crime. Reg Sci Urban Econ 78:103460
Burkhardt J, Goemans C (2019) The short-run effects of marijuana dispensary openings onlocal crime. Ann Reg Sci 63(1):163–189
Calandrillo SP, Buehler, DE (2008) Time well spent: an economic analysis of daylight saving time legislation. Wake Forest L. Rev: 45–91
Cameron AC, Gelbach JB, Miller DL (2008) Bootstrap-based improvements for inference withclustered errors. Rev Econ Stat 90(3):414–427
Cengiz D, Dube A, Lindner A, Zipperer B (2019) The effect of minimum wages on low wagejobs. Q J Econ 134(3):1405–1454
Chang TY, Jacobson M (2017) Going to pot? The impact of dispensary closures on crime. J Urban Econ 100:120–136
Di Tella R, Schargrodsky E (2004) Do police reduce crime? Estimates using the allocationof police forces after a terrorist attack. American Econ Review 94(1):115–133
Doleac JL, Sanders NJ (2015) Under the cover of darkness: how ambient light influencescriminal activity. Rev Econ Stat 97(5):1093–1103
Dong X, Tyndall J (2024) The impact of recreational marijuana dispensaries on crime: evidencefrom a lottery experiment. Ann Reg Sci 72(4):1383–1414
Draca M, Machin S, Witt R (2011) Panic on the streets of London: police, crime, and theJuly2005 terror attacks. Am Econ Rev 101(5):2157–2181
Epp CR, Maynard-Moody S, Haider-Markel D (2017) Beyond profiling: the institutionalsources of racial disparities in policing. Public Adm Rev 77(2):168–178

Freisthler B, Kepple NJ, Sims R, Martin SE (2013) Evaluating medical marijuana dispensarypolicies: spatial methods for the study of environmentally-based interventions. Am J Community Psychol 51:278–288
Goodman-Bacon A (2021) Difference-in-differences with variation in treatment timing. J Econom 225(2):254–277
Gruenewald PJ, Remer L (2006) Changes in outlet densities affect violence rates. Alcohol Clin Exp Res 30(7):1184–1193
Harris AP, Ash E, Fagan J (2020) Fiscal pressures and discriminatory policing: evidencefromtraffic stops in Missouri. J Race Ethn Polit 5(3):450–480
Hassell KD (2007) Variation in police patrol practices: the precinct as a sub-organizationallevel of analysis. Polic Int J Police Strateg Manag 30(2):257–276
Hausman D, Kronick D (2023) The illusory end of stop and frisk in Chicago? Sci Adv 9(39):1–6
Howard T (n.d.) https://www.cannabisindustrylawyer.com/illinois-cannabis-license-timeline/
Knowles J, Persico N, Todd P (2001) Racial bias in motor vehicle searches: theory andevidence. J Polit Econ 109(1):203–229
Luca DL (2015) Do traffic tickets reduce motor vehicle accidents? Evidence from a naturalexperiment. J Policy Anal Manage 34(1):85–106
Majmundar MK, Weisburd D (eds) (2018) Proactive policing: effects on crime andcommunities. National Academies Press, Washington
Mello S (2018) Speed trap or poverty trap? Fines, fees, and financial wellbeing. Work. Pap FFJC, New York
Miller K, Seo B (2021) The effect of cannabis legalization on substance demand and taxrevenues. Natl Tax J 74(1):107–145
Mughan S, Carroll J (2021) Escaping the long arm of the law? Racial disparities in the effectof drivers' license suspensions on offense probabilities. South Econ J 87(4):1366–1389
Mughan S, Singla A (2023) Does revenue-motivated policing alter who receives trafficcitations? Evidence from driver race and income in Indiana. Public Adm Rev 83(2):353–366
Petersen K, Weisburd D, Fay S, Eggins E, Mazerolle L (2023) Police stops to reduce crime: asystematic review and meta-analysis. Campbell Syst Rev 19(1):1–42
Pierson E, Simoiu C, Overgoor J, Corbett-Davies S, Jenson D, Shoemaker A, Ramachandran V, Barghouty P, Phillips C, Shroff R, Goel S (2020) Alarge-scale analysis of racial disparities in police stops across the United States. NatureHuman Behaviour 4(7):736–745
Rascoff S, Humphries S (2015) Zillow talk: rewriting the rules of real estate. Grand Central Publishing
Schlikerman B (2014) Zoning Board signs off on marijuana farm, 6 dispensaries in city. Chicago Sun Times, Chicago
Skogan WG (2022) Stop & frisk and the politics of crime in Chicago. Oxford UniversityPress, Oxford
Stohr M, Makin D, Stanton D, Hemmens C, Willits D, Lovrich N, Meize M, Snyder J, Lu R, Wu G (2020) Anevolution rather than a revolution: cannabis legalization implementation from theperspective of the police in Washington state. Just Eval J 3(2):267–293
Stanton Sr DL, Makin D, Stohr M, Lovrich NP, Willits D, Hemmens C, Meize M, Bowers O, Snyder J (2022) Law enforcement perceptions of cannabis legalization effects on policing: challenges of major policy change implementation at the street level. Contemp Drug Probl 49(1):20–45
Sun L, Abraham S (2021) Estimating dynamic treatment effects in event studies withheterogeneous treatment effects. J Econom 225(2):175–199
Thomas D, Tian L (2021) Hits from the bong: the impact of recreational marijuanadispensaries on property values. Reg Sci Urban Econ 87:103655
Tully T (2024) For 8 Months traffic enforcement on New Jersey's highways plummeted. NewYork Times, New York
Ward KC, Lucas PA, Murphy A (2019) The impact of marijuana legalization on lawenforcement in states surrounding Colorado. Police Q 22(2):217–242

Publisher's Note Springer Nature remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.