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Going to Pot?
The Impact of Dispensary Closures on Crime *

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Abstract

Jurisdictions that sanction medical or, more recently, recreational marijuana use often allow retail sales at dispensaries. Dispensaries are controversial as many believe they contribute to local crime. To assess this claim, we analyze the short-term mass closing of hundreds of medical marijuana dispensaries in Los Angeles. Contrary to popular wisdom, we find an immediate increase in crime around dispensaries ordered to close relative to those allowed to remain open. The increase is specific to the type of crime most plausibly deterred by bystanders, and is correlated with neighborhood walkability. We find a similar pattern of results for temporary restaurant closures due to health code violations. A likely common mechanism is that"eyes upon the street" deter some types of crime.

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1 Introduction

One of the most dramatic shifts in public opinion in the U.S. over the past four and a half decades has been a surge in support for marijuana legalization, both medical and, increasingly, recreational. Currently 60% of adults in the U.S. favor broad-based marijuana legalization, compared to only 12% in 1969 (Swift 2016), and nearly 90% think adults should be allowed to use marijuana for prescribed medical purposes (CNN/ORC 2014). Despite this support, 44% indicate that they would be somewhat or very concerned if a "store that sold medical marijuana" opened in their area (The Pew Research Center 2010). In particular, many maintain that these stores, usually called dispensaries, attract or, even, cause crime (McDonald and Pelisek 2009; National Public Radio 2010; Reuteman 2010).

The idea that marijuana dispensaries attract crime has proved influential with policy-makers. For example, an Oregon state senator argued that a law allowing cities to ban dispensaries was important to "empower them to protect our children and families" (Zheng 2014). In Los Angeles, the setting for this study, the city council cited crime in its 2010 decision to cap the number of dispensaries in the city.[1] Yet, empirical evidence to support any link (positive or negative) between marijuana dispensaries and crime is quite limited. State difference-in-differences estimates find no relationship between medical marijuana laws and crime rates (Morris et al. 2014). Since not all medical marijuana states have operational dispensaries, however, these estimates do not speak directly to the impact of dispensaries on crime. The density of dispensaries across 95 census tracts in Sacramento, CA is uncorrelated with either violent or property crime rates (Kepple and Freisthler 2012).[2] Well-known limitations of cross-sectional analyses and a general lack of statistical power in that study suggest the importance of continued work on the topic.

How, in theory, might medical marijuana dispensaries affect crime? First, marijuana use, which may be concentrated around dispensaries if some buyers consume onsite or nearby, may be criminogenic. Similar effects have been cited for alcohol outlets, where

[1] See the fifth paragraph of Ordinance 181069 `http://clkrep.lacity.org/onlinedocs/2008/08-0923_ord_181069.pdf`

[2] The Denver and Colorado Springs Police Departments each analyzed the number of crimes around dispensaries and compared them to the numbers around banks, pharmacies, and other businesses (Ingold, 2010; Rodgers, 2010). Neither found that dispensaries attract crime, although recent work demonstrates that dispensaries in Denver tend to be located in high crime neighborhoods (Boggess et al. 2014).

openings and availability in Los Angeles and other jurisdictions are associated with increases in crime (Teh 2008; Scribner et al. 1995; Gorman et al., 1998; Scribner et al., 1999; Gruenewald and Remer 2006; Gruenewald et al. 2006; Franklin et al. 2010; Grubesic and Pridemore 2011). In contrast to alcohol, however, some work suggests marijuana may not increase crime commission per se (Pacula and Kilmer, 2003) and may even inhibit aggressive behavior (Myerscough and Taylor 1985; NAS 1994; Hoaken and Stewart 2003).[3]

Second, given the quasi-legal status of these stores and their products, dispensary customers, employees or owners may resort to violence to resolve disputes (Miron 1999; Resignato, 2000).[4] If so, we might expect increases in crimes such as aggravated assault, which increased for such reasons with the emergence of crack cocaine (Grogger and Willis 2000).

Third, crime could increase near dispensaries as individuals try to finance their purchases through the proceeds of crime (Grogger and Willis 2000). If so, we would expect theft or other property crimes to increase with dispensaries. Finally, marijuana users and the dispensaries they frequent, which are a direct source of drugs and cash, may offer opportunities that attract criminals. Anecdotal evidence suggests that dispensaries have been subject to break-ins and robberies (e.g., see McDonald and Pelisek, 2009). Thus, we would expect an increase in robbery and burglary around dispensaries.[5]

While these channels seem plausible and have captured public attention, dispensaries could, in principle, decrease crime. Dispensaries tend to have their own security systems and often security guards to protect their assets and resolve disputes. Analyses of business improvement districts find that private security can have large returns in terms of crime

[3]The correlation between marijuana use and non-drug crime, although positive, is generally small (Bennett et al. 2008) and largely inconclusive (Pedersen and Skardhamar 2010; Farrington 2010). Longitudinal studies that find clearer positive relationships, such as Green et al. (2010), cannot rule out the role of third factors that affect both the commission of non-drug crime and marijuana use (Caulkins et al. 2012).

[4]We describe dispensaries as quasi-legal for several reasons. First, although medical marijuana use is legal in California, large-scale production and sales are not. Second, while cooperative are allowed under California law, Los Angeles and other localities can tightly regulate and, in some cases ban, their operations. Finally, under federal law, it remains illegal to manufacture, distribute or possess marijuana. Consequently, dispensaries have been targeted and raided by federal law enforcement.

[5]In this case, dispensaries may affect the spatial distribution of crime rather than increase the overall level. Such a change has clear negative implications for dispensary neighbors, but may not have broader societal implications. The welfare impact of geographic redistribution of crime depends on such factors as heterogenous effects (e.g., different costs across neighborhoods), multiplicative effects (if two crimes in one area impose higher costs than one crime in each of two areas) and economies of scale (if two crimes in one area is less costly than one crime in each of two areas).

reduction (Brooks, 2008; Cook and MacDonald 2011). Likewise, if police allocate more patrols around dispensaries, they might reduce crime as in Di Tella and Schargrodsky (2004). To the extent that dispensaries increase foot traffic through a neighborhood, they might prevent crime by increasing "eyes on the street" (Jacobs 1961). In addition, by legitimizing the marijuana trade, actors in this market may have legal channels to resolve disputes. This last possibility is somewhat less plausible given the ambiguous legality of many aspects of the medical marijuana market, such as large scale distribution.

Finally, if marijuana is a substitute for alcohol, as suggested by Anderson, Hansen and Rees (2013) and Crost and Rees (2013), increased access to marijuana could reduce crime since drinking increases arrests for both property crime (Carpenter 2007) and violent crime (Carpenter and Dobkin 2015). Ultimately, given the range of theoretical predictions, the impact of dispensaries on crime is an empirical question.

To evaluate the claim that dispensaries attract or otherwise contribute to crime, we exploit a plausibly exogenous source of variation in dispensary activity – the temporary shutdown of medical marijuana dispensaries in the City of Los Angeles. On June 7, 2010, roughly 70% of the nearly 600 shops operating in the city of Los Angeles were ordered to close (Hoeffel 2010a). The shutdown came after years of concern and indecision over how to handle the burgeoning medical marijuana dispensary business in the city. In September 2007, the city adopted an "Interim Control Ordinance" (ICO), placing a temporary moratorium on new dispensaries and requiring existing dispensaries to register with the city by November 13, 2007 (see Appendix Table 1 for a timeline).

Given the limited time that dispensaries had to submit a registration form along with the required city business tax registration certificate, registration was quite ad hoc. How the city would use the registrations was unclear and the market continued to grow for several years despite the moratorium. In January 2010, final regulations, including closure orders, were adopted. The new ordinance set the number of dispensaries in the city at 70. Dispensaries that had registered between September and November 2007 and had been operating legally since that time were grandfathered, meaning that the number of legal dispensaries in the city could exceed 70 in the short run.

Consistent with the seeming arbitrariness of the closure criteria, we find that dispensaries ordered to close and those allowed to remain open look similar on observable dimensions. In other words, closure orders were not correlated with observable dispensary characteristics (including the level of or trend in crime around specific dispensaries) that might have otherwise made them of specific interest to law enforcement. We leverage the quasi-random nature of closure orders using a difference-in-differences framework and detailed data on exact dispensary locations and crime reports by city block to compare daily crime counts within varying radii (as small as 1/8 of a mile) around dispensaries ordered to close and those allowed to remain open. If dispensaries attract crime, then crime should decrease around dispensaries subject to closure relative to those allowed to remain open.[6]

Contrary to conventional wisdom, we find no evidence that closures decreased crime. Instead, we find a significant relative increase in crime around closed dispensaries. Like compliance with the closures orders themselves, which first was high, fell off with legal challenges and collapsed after a December 2010 injunction (Hoeffel 2010b), the increase in crime is temporary. Relative crime rates return to normal within four weeks. The increase is also very local – the estimated crime effects decrease rapidly and monotonically with distance around dispensaries. Bearing in mind that our analysis captures short-run effects, these findings imply that closing medical marijuana dispensaries is unlikely to reduce crime. Although there may be a myriad of reasons to regulate the number of marijuana dispensaries, protection from crime is one that seems difficult to substantiate.

We perform several analyses to better understand how dispensary closures affect crime. First, we analyze crime by categories. We find that the increase in crime is strongest and most precise for the type of crime most plausibly deterred by the presence of bystanders – property crime and theft from vehicles, specifically. Second, we analyze the interaction between closures and neighborhood foot traffic. We proxy for foot traffic using Walk Scores, a proprietary measure that scores each address based on the walking time to amenities, population density, block length and the density of street intersections. We find that the the magnitude of the crime effect varies in a non-linear way with Walk Scores. Specifically, the magnitude of the closure effect varies negatively with walkability, except in the most

[6] An alternative question, not explicitly evaluated here, is how dispensaries affect crime relative to alternative business types (e.g., ice cream parlors, convenience stores or banks.) While we cannot speak to this directly, our analyses of temporary restaurant closures can help shed light on this issue.

geographically isolated areas for which closures have no measurable effect on crime.

To shed further light on mechanisms, we explore the generalizability of the findings. Specifically, we analyze the impact of temporary restaurant closures due to public health code violations on crime in Los Angeles County. Despite the very different nature of these businesses, the reason for and timing of their closures, and the identifying assumptions, we find a nearly identical pattern of results. Crime increases in the local neighborhood around closed restaurants, the increase is driven by property crime, the effect is concentrated in areas without a high volume of foot traffic, and the effect disappears as soon as the restaurant reopens.

The common pattern of results for dispensaries and restaurants suggests that business closures in general exert a significant negative crime externality. By extension, businesses offer very local protection against some types of crime. Given that police are unlikely to systematically change their behavior in response to temporary restaurant closures, this analysis further suggests that changes in policing cannot explain the common pattern of results. Rather, a likely common mechanism may be "eyes upon the street" (Jacobs 1961), meaning that the presence of individuals helps deter crime. While part of the canon of modern urban design and crime prevention, this theory is virtually unsupported by rigorous empirical evidence. In addition, Jane Jacob's original 1961 formulation of the hypothesis makes clear that the impact of additional individuals on local crime is theoretically ambiguous; crowds provide some form of natural policing but also more perpetrators of and opportunities for crime. Our findings suggest that the first channel dominates, at least in the case of medical marijuana dispensaries and restaurants in urban environments.

The rest of the paper is organized as follows. In section 2 we discuss the June 2010 closure of medical marijuana dispensaries in Los Angeles and describe our data. In section 3 we describe our analytic approach for the dispensary analysis. In section 4 we present our main results. In section 5 we discuss spatial and temporal displacement. In section 6 we presents the institutional details of and results from our analysis of temporary restaurant closures due to health code violations in Los Angeles County. In section 7 we explore potential mechanisms behind the shared pattern of findings. In section 8 we conclude.

2 Medical Marijuana Dispensaries in Los Angeles

In 1996, voters in California approved Proposition 215, the state's medical marijuana law. Marijuana dispensaries opened to serve the patients newly qualified to use the drug under the law. Like the state as a whole, the City of Los Angeles saw rapid growth in dispensaries after the 2004 passage of a bill (SB 420) that clarified several operational aspects of the state's medical marijuana law.[7] At its peak, some estimates put the number of dispensaries in the City of Los Angeles at over 800 (McDonald and Pelisek 2009).

Not all Los Angeles residents welcomed these stores. Many believed that dispensaries attract crime and law enforcement fueled these concerns. In a July 2005 report, the LAPD cited several felony narcotics arrests made at dispensaries and speculated that "crimes such as theft, robbery and assault have occurred and will occur along with the sale of marijuana from these locations." As a result, they called for restricting dispensaries to commercial areas in the city if not banning them outright.[8] A later report by the Los Angeles Police Commission argued that the increase in dispensaries within the city (from 4 to 98 between July 2005 and November 2006) was tied to an increase in crime in reporting districts that had received complaints about dispensaries.[9] While these crime changes were not compared to that around other businesses or areas, many found the argument persuasive.

In 2006, the City Attorney's Office laid out options for regulating dispensaries – a land use ordinance establishing zoning requirements, an interim moratorium until state law was "further clarified" or an outright ban. Almost a year later, in September 2007, the city adopted an "Interim Control Ordinance" (ICO) that temporarily banned new dispensaries and required existing ones to register with the city by November 13, 2007. The ICO aimed to pacify constituents concerned with the growth of dispensaries while the city drafted permanent legislation.

While in principle the ICO should have stopped the growth in dispensaries, in practice it had the opposite effect. Hundreds of dispensaries opened after the moratorium by filing

[7] Among other things, SB 420 recognized a patient's right to cultivate marijuana through nonprofit collectives and cooperatives, i.e., dispensaries, a right that was later affirmed in People v. Urziceanu. See http://caselaw.lp.findlaw.com/data2/californiastatecases/C045276.PDF

[8] See http://clkrep.lacity.org/onlinedocs/2005/05-0872_rpt_atty_10-19-06.pdf

[9] See http://californiapolicechiefs.org/site/uploads-calchiefs/2012/02/fact_sheet.pdf

applications for "hardship exemptions" allowed under the ICO (McDonald and Pelisek, 2009).[10] The large number of applicants stemmed in part from the recognition that the city would not prosecute dispensaries until their hardship applications had been reviewed and that the city council was in no hurry to review applications. By June 2009, when the city council first began to rule on the hardship exemption applications, over 500 applications had been submitted (Hoeffel, 2009). On June 19, 2009, the city passed an ordinance amending the ICO to eliminate the hardship exemption.[11]

Although intended as a stop-gap measure, the ICO remained in place for more than a year and half. On January 26, 2010 the city council approved final legislation limiting the number of dispensaries in the city to 70 but grandfathering in those that had registered and been operating legally since the ICO.[12] Based on the 2007 registrations, 187 dispensaries were initially deemed eligible to apply for permits to remain operational. All other dispensaries were to cease operation by June 7, 2010. On May 4, 2010, the city sent "courtesy notices" to the 439 dispensaries that were being ordered to shut their doors.[13] Several listed establishments were later identified as ancillary businesses (e.g., clinics offering medical marijuana recommendations or smoke shops selling paraphernalia).[14] Our own scrutiny of the city's lists eliminated several duplicate listings, yielding 180 dispensaries eligible to remain open and 417 dispensaries ordered to close.

Some dispensaries and patient advocates responded to the city's notices by filing temporary restraining orders to prevent the closures. Efforts to win temporary restraining orders proved unsuccessful (Yoshino 2010; Kim 2010). That the appeals continued up to 3 days before June 7, 2010, however, suggests that dispensaries were not preparing weeks in advance to close. While the city declined to detail how the law would be enforced, it noted that it would "rely on reports from police, neighbors and building inspectors to identify violators" (Hoeffel 2010a). This characterization suggests that special patrols and

[10] Hardship exemption requests often cited delays beyond a dispensary's control, such as in receiving a city business tax registration certificate required for registering. However, many later applicants cited failure to register because of fear imposed by federal authorities (Hoeffel 2009).

[11] http://clkrep.lacity.org/onlinedocs/2009/09-0964_ord_180749.pdf

[12] See http://clkrep.lacity.org/onlinedocs/2008/08-0923_ord_181069.pdf

[13] For a sample letter, see http://blogs.laweekly.com/informer/2010/05/pot_shops_warned_to_close.php

[14] E.g., see http://www.latimes.com/local/la-me-closing-dispensaries-htmlstory.html.

enforcement units were not allocated, although we know of no data on this point.

Early reports on compliance with the law indicated that most of the dispensaries ordered to close on June 7, 2010 did so.[15] Within weeks, however, compliance seemed to break down and legal challenges to the law mounted (Wei and Romero 2010; Guerrero, 2010). In December 2010 an injunction was issued against the law and in January 2011 the city's dispensary closures were formally invalidated.[16]

2.1 Crime Data

To assess the relationship between medical marijuana dispensaries and crime, we analyze incident level crime data provided by the Los Angeles Police Department (LAPD) and the Los Angeles Sheriff's Department (LASD) to The Los Angeles Times (LAT) as part of its "Mapping L.A." project."[17] The LAPD provides police services to neighborhoods throughout the city while the LASD provides primary police services to all unincorporated parts of Los Angeles County, to all Metropolitan Transportation Authority (MTA) stations within the city and beyond, nine community college campuses throughout the county, as well as to numerous contract cities in the county.[18] The crime data include the date, time and location of reported crimes at the block level.[19]

Crimes not reported to the LAPD or LASD are not in our dataset. Thus, some crime committed in adjacent jurisdictions, such as the City of Santa Monica, is not captured

[15] The LA City Attorney's office estimated that only 20-30 stores defied the initial closure order (Rubin and Hoeffel 2010).

[16] Specifically, a Los Angeles County Superior Court Judge issued an injunction barring the city from enforcing many aspects of the medical marijuana ordinance, including dispensary closures based on registration (or lack thereof) at the time of the moratorium (Hoeffel 2010a).

[17] See `http://maps.latimes.com/crime/` for details on the data. As noted in the FAQ, the LAPD and LASD provide the raw data directly to the LAT.

[18] According to the LAT, they capture data from 42 LASD contract cities: Agoura Hills, Artesia, Avalon, Bellflower, Bradbury, Calabasas, Carson, Cerritos, Commerce, Compton, Cudahy, Diamond Bar, Duarte, Hawaiian Gardens, Hidden Hills, Industry, La Canada Flintridge, La Habra Heights, La Mirada, La Puente, Lakewood, Lancaster, Lawndale, Lomita, Lynwood, Malibu, Maywood, Norwalk, Palmdale, Paramount, Pico Rivera, Rancho Palos Verdes, Rolling Hills, Rolling Hills Estates, Rosemead, San Dimas, Santa Clarita, South El Monte, Temple City, Walnut, West Hollywood, Westlake Village. See `http://maps.latimes.com/about/#why-no-crime-reports`

[19] For mapping purposes, the LAT data repeat some crimes that occur at the boundaries of neighborhoods. See `http://maps.latimes.com/about/#double-crime-counting`. We clean the data to eliminate any multiple counts introduced for mapping purposes.

here.[20] Nonetheless, the LAT data should contain the vast majority of crimes occurring around the City and County of Los Angeles.[21] In addition, the dataset has gone through a rigorous vetting process by the Los Angeles Times, including the correction of numerous data omissions and flaws.[22] More importantly for our estimation, since any data omissions are determined by geographic coverage not dispensary closures and occur both pre and post closure, the missing data should not bias our findings.

The LAT crime data capture Part I offenses, defined as "serious crimes [that] occur with regularity in all areas of the country, and are likely to be reported to police" (FBI 2010). We analyze total Part I crimes and subcategories of Part I crimes defined by either the LAPD's Crime Class Code Hierarchy or the FBI's coding, which is used by the LASD and in national level datasets such as the Uniform Crime Reports. The LAPD's categories differ somewhat from the FBI's coding; specifically, the LAPD breaks out theft into general theft and theft from vehicles because of the high share of crime in the theft from vehicles category.[23] Since the LAPD comprises 98% of our data in the dispensary analysis and much of data for the restaurant analysis, we have adopted this as our main coding system.

2.2 Dispensary Data

We use information from the Los Angeles City Attorney's Office (LACAO) on the exact location of dispensaries subject to closure or allowed to remain open. Of the 597 dispensaries in our dataset, 417 were ordered to close and 180 were allowed to remain open.[24] Figure 1 shows the geographic distribution of dispensaries by closure status.

We code dispensaries based on their closure order status, adopting an intent-to-treat (ITT) approach to the analysis. As described above, the city initially reported that nearly

[20] Some crime in these areas are included because the LASD had jurisdiction, e.g., at an MTA stop, or because the LAPD was called.

[21] Together the LAPD and LASD provide police services for about 63% of the 9.9 million LA County residents – about 3.8 million within the city, 1.1 million in unincorporated parts of the county and another 1.4 million in contract cities (based on authors' calculations).

[22] For details see `http://maps.latimes.com/about/#crime-data-sources`

[23] For a discussion of the LAPD's categories for Part I offenses, see `http://projects.latimes.com/mapping-la/about/#what-crimes`

[24] The initial number of dispensaries cited by the LACAO was closer to 640 shops. However, careful scrutiny of the official list of dispensaries ordered to close and allowed to remain open revealed many duplicate listings as well as listing of shops that were subsequently deemed not to be dispensaries (these were generally clinics offering medical marijuana recommendations or smoke shops selling paraphernalia).

all shops complied with their orders to close. However, a small number of dispensaries that were supposed to close were later raided by the LAPD (see Rubin and Hoeffel 2010) or reported to be operating by the LA Weekly, (see Wei and Romero 2010). While the LACAO later indicated that the closure orders were not nearly as effective as they originally claimed and that many dispensaries defied their orders, it is unclear whether the revised statements apply to the short or long run. If indeed a substantial number of dispensaries failed to close immediately after the June 7, 2010, the results may significantly understate the true effect of dispensary closures on crime. In sensitivity checks, we replicate our analysis but recode as open or drop entirely dispensaries that, according to reports from the Los Angeles Times and the LA Weekly, defied the city's orders to close (Rubin and Hoeffel 2010, Wei and Romero 2010). Such revisions do not materially affect our results.

3 Empirical Strategy

We estimate the effect of dispensary closures on crime using a regression of the following basic form:

$$C_{it}^{d} = \alpha_i + \beta * 1(closed_{it}) + \delta_t + \epsilon_{it} \tag{1}$$

where C_{it}^{d} is the number of crimes within a distance d of a dispensary i on date t, α_i is a dispensary fixed effect, δ_t is a date fixed effect and closed is an interaction between $1(date \geq June7)$, an indicator for dates after and including the June 7, 2010 closures, and 1(closed), an indicator for dispensary closure status, as determined by city orders and in some sensitivity checks by reports of defying these orders. The main post June 7 and closure indicators are subsumed in fixed effects for date and dispensaries, respectively.

Given the (non-negative) count nature of the crime data, we estimate equation (1) using a Poisson regression model. All standard errors allow for two-way clustering to account for serial correlation of an arbitrary structure at the dispensary level as well as correlation across dispensaries on a given day (Cameron, Gelbach and Miller, 2011). With robust standard errors, the Poisson model is a quasi-maximum likelihood estimator that does not impose the equality of mean and variance condition and leads to consistent standard errors (Winkelmann and Zimmermann 1992). Allowing for overdispersion with a negative binomial model, which we show in sensitivity checks, yields similar results.

We use the 10 days prior to and 10 days after (but not including) the closure date (June 7) for the main analysis. We drop June 7 because of the possibility of enhanced police presence to enforce closures, the general confusion over the meaning of the date (e.g., whether stores had to shutter on June 7 or the day after), and potential protests against closing on the proscribed date. In addition, since there can be a lag between the commission and reporting of a crime, crime reports for the closure date may be contaminated by crime committed in the pre-closure period, which would attenuate the estimates. As we show in sensitivity checks, the results are robust to the inclusion of the closure date.

We focus on a short (20 day) time window because many closures were temporary. As the legality and enforceability of the measure came under question, many dispensaries reopened or were replaced by other businesses. In addition, the short window increases our confidence that our results are due to dispensary closures and not differences in longer run crime trends around open and closed dispensaries. Analyses that extend the window around June 7 but decompose the post-closure period into smaller time periods confirm the immediate but temporary impact of closure orders on crime.

The identifying assumption for our analysis is that, in the absence of closures, crime and the factors that impact crime in the immediate area around dispensaries subject to closure would be similar (or at least not differentially different post-closure) to those in the immediate area around dispensaries allowed to remain open. While the somewhat arbitrary process the city took to determine closure status suggests this should be the case, we present several pieces of evidence to support this claim.

First, in Table 1, we show that daily crime counts at 1 or 1/3 mile around dispensaries ordered to close (col (1)) and allowed to remain open (col (2)) are virtually indistinguishable in the pre-period.[25] This remains true even when we narrow to 1/4 or 1/8 mile around dispensaries (not shown), and for total Part I crime as well as for Part I property and violent crime, and across subcategories of Part I crime.

Second, we compare crime trends around dispensaries ordered to close and those al-

[25]We use 1/3 of a mile because that is the distance where we detect changes in crime across both dispensaries and restaurants, i.e., this distance best balances the trade-off between local crime effects and the loss of power from considering a very small area with few crimes on any given day.

lowed to remain open. As shown in Figure 2a, at 1 mile, average daily Part I crimes are indistinguishable by closure status in both the pre and post period. A different pattern emerges when we consider crime in the immediate area around a dispensary. Specifically, at 1/3 of a mile (Figure 2b), average Part I crime tracks closely in the pre-period but diverges after June 7. This is suggestive of a closure effect on localized crime. The divergence in Part I crime trends after June 7 can also be seen at 1/8 of a mile (Figure 2c).[26]

Third, we find that dispensaries are indistinguishable based on closure status across a range of zip code characteristics from the 2010 census and 2011 ACS. Dispensaries are located in zip codes of about 42,000 people or 15,500 households, irrespective of closure status. Median household income, median age, housing occupancy rates and the share foreign-born are also independent of closure status. We also consider Walk Scores, a walkability measure that rates an address based on a weighted function of walking distance to amenities in 9 different categories, such as grocery, restaurants, and entertainment. (Walk Score 2011). Scores, which are on a scale of 0 to 100, are adjusted for pedestrian-friendliness, such as block connectivity. Dispensaries in Los Angeles tend to be in very walkable areas (scores of 70-89 are considered 'Very Walkable') and while we fail to reject zero difference in Walk Scores across dispensary types at the 10% level (p-value 0.056), this difference is quite small in magnitude – about 2.3 points (74.9 vs. 77.2) or less than 1/5 of a standard deviation (13.7 points) in Walk Scores. A comparison of the distribution of Walk Scores by closure status in Appendix Figure 2 shows that the mean difference is driven by a few dispensaries in very low Walk Score areas that were ordered to close.[27] Our results are not sensitive to these outliers. In short, the descriptive statistics are consistent with de-facto random closures, with open dispensaries serving as good controls for closed dispensaries.

Next we consider the possibility of spatial clustering of dispensary closures. If closure status is geographically clustered, it could impact inference and lead to over-rejection of the null of no effect of closures on crime (Barrios et al. 2012). In the last row of Table 1,

[26]Consistent with the patterns at 1 mile, we see no obvious impact of dispensary closures on citywide Part I crime counts or counts by region (see Appendix Figures 1a and 1b). This null effect is consistent with either the change in crime around dispensaries being too small to show up in citywide crime counts or a displacement of crime from areas farther to nearer to closed dispensaries.

[27]Excluding the 31 dispensaries characterized as "Car-Dependent," the difference in mean Walk Scores for dispensaries ordered to close and those allowed to remain drops to 0.5 with values of 76.9 and 77.4 respectively.

however, we demonstrate that the likelihood that a closed dispensary's nearest neighbor is open is similar to the likelihood that an open dispensary's nearest neighbor is open. Both probabilities are about a third and are statistically indistinguishable from each other, suggesting that closure status is not geographically clustered, and standard approaches to inference (e.g., two-way clustering) should be valid in this setting.

A separate but related issue is the overlap of crime catchment areas. Because dispensaries often locate close to one another, a given crime may be assigned to multiple dispensaries, particularly as we widen the catchment area. This overlap will likely bias our results towards zero, and potentially affect inference. To deal with this issue, below (in section 5.1) we present analyses using only dispensaries without nearby neighbors. Consistent with the predicted downward bias, the results are larger in magnitude but still statistically significant when we reduce or eliminate overlap, despite a greatly reduced sample size.[28]

Finally, we run a series of placebo regressions that analyze crime 1, 2, or 3 months *prior* to the closure orders taking effect (section 5.1). These regressions provide another check on whether our results are driven by differential crime trends around dispensaries that registered with the city in 2007 (and thus were eligible to remain open) and those that failed to do so. As discussed below, the placebo checks provide further support for the identification strategy.

4 Main Results

Our main Poisson regression model estimates of the impact of dispensary closures on total Part I crimes are in Table 2. In col (1), we show the pre-closure mean of Part I crime at 1/8, 1/4, 1/3, 1/2, 1 and 2 miles around dispensaries ordered to close. For crime at each of these distances, we show the results of the intent-to-treat (ITT) analysis (col (2)), the analysis that recodes dispensaries that were known to have defied closure orders as open (col (3)) and the analysis that drops known defiers (col (4)).

At distances of 1 to 2 miles, the estimated effects of closure on Part I crime are rather precisely estimated zeros. At 1/2 of a mile the effects are larger but insignificantly different from zero. At 1/3 of a mile, we detect increases in Part I crime of about 12 to 14% around

[28]We thank Steve Raphael for suggesting this check for a related, unreleased RAND report.

dispensaries ordered to closed relative to those allowed to remain open.

The point estimates imply increases between 14 to 16% at 1/4 of a mile and 23 to 24% at 1/8 of a mile around dispensaries ordered to close relative to those allowed to remain open. These findings suggest that while dispensary closures affect (increase) total Part I crime, they do so only in a very localized fashion (i.e., in the immediate vicinity of the affected business). Columns 2 and 3 show a similar pattern of results when we recode known defiers or drop them from our sample.[29]

4.1 Robustness Checks and Sensitivity Analyses

In Table 3 we present results from placebo regressions in which we repeat the ITT analysis but code the closure period as 1, 2 or 3 months prior to the actual June 7 closure date. As in our main analysis, we use 10 days each of pre and post data and drop the placebo closure date. Col (1) in Table 3 repeats our main ITT result from Table 2, while cols. (2) - (4) show results of the ITT analysis using placebo closure dates. Unlike the true closure period results, we find no clear pattern of results using placebo dates (e.g., monotonically increasing/decreasing with distance). The placebo estimates are sometimes positive and sometimes negative and are never significant at short distances (less than 0.5 miles) around a dispensary. These findings suggest our main results are not driven by systematic differences in crime trends by closure orders.

Next, we check whether the results are sensitive to model choice. First, we re-run the regressions using a negative binomial model in place of a Poisson model to deal with over dispersion. Second, we use a zero-inflated Poisson regression model to handle excess zeros in the data. As shown in Appendix Table 2, the estimates from both models are quite similar to our main Table 2 estimates.

In Appendix Table 3, we explore the impact of extending the study window. Specifically, we present (ITT) results that lengthen the study period to 60 days but include 3 separate indicators for closure days 1-10 (col 1), 11-20 (col 2), and 21-30 (col 3). We break up the

[29]Since defiant dispensaries may differ systematically from other dispensaries, recoding them as open or dropping them could introduce bias. In practice, since we know of only nine defiant dispensaries, we use this exercise to demonstrate that the results are not driven by the treatment of defiant dispensaries. As expected, the treatment of defiers has little effect on the magnitude or significance of the coefficients.

extended post-period into three parts because lengthening the post-closure period likely introduces control days to the treatment period since, as documented in McDonald and Pelisek (2009), some dispensaries reopened within a couple of weeks of closure.[30] Col (1) shows that increasing the pre-period generates results similar to our main specification with slightly tighter confidence intervals: the estimated effect of the first 10 days of closure on total Part I crime at 1/8 of a mile is almost 30% and is significant at the 1% level. At 1/4 and 1/3 of a mile, the first 10-day estimates are 12 and 9%, respectively, consistent with a decreasing monotonic relationship between the distance around dispensaries and the change in crime. Col (2) shows the effects of dispensary closures 11-20 days after the event. We find effects that are both smaller in magnitude and only significantly different from zero (at the 10% level) at 1/3 of a mile. Estimates for the 21-30 day closure period in col (3) are much less precise and are inconsistent in sign. This analysis confirms that pre-period trends are not driving our findings and that temporary dispensary closures had an immediate and temporary impact on crime.

In Appendix Table 4, we test the sensitivity of the results to confusion over the closure date and potential lags in crime reporting. Specifically, we drop June 6-8, 2010 from the analysis. Because this significantly limits our sample, we show results using 9, 19 and 29 days on either side of the June 7, 2010 but excluding June 6-8. Those results are quite similar and, in many cases, more precisely estimated than our main Table 2 results.

Finally we examine the effect of the multiple counting of crimes due to geographic overlap in dispensary neighborhoods. Because closure status is not geographically clustered, the main effect of this overlap is to mechanically bias our estimates towards zero, leading to an underestimate of the magnitude of the closure effect. To see this, we would ideally analyze dispensaries that have no neighbors within a wide radius, e.g., 1 mile. In practice, less than 5% of dispensaries are so geographically isolated. Consequently, in Appendix Table 5, we show sensitivity checks using the less restrictive requirements that dispensaries have a nearest neighbor more than 1/3 mile or more than 1/2 mile away. Using these restrictions leaves us with 158 dispensaries with a nearest neighbor more than 1/3 mile

[30] In addition, lengthening the post-period could reduce our estimates if they capture a generic business effect, as suggested by the restaurant analysis below, and new businesses open at the site of closed dispensaries.

away and 79 dispensaries with a nearest neighbor more than 1/2 mile away.

Across both restricted samples, the magnitude of the change in Part I crime is consistently larger than in the sample as a whole. The results for crime at 1/3 and 1/4 of a mile are statistically significant, despite the greatly reduced sample size. Restricting to dispensaries with a nearest neighbor more than 1/3 mile away, the estimates imply that Part I crime within a radius of 1/4 mile was about 47% higher around dispensaries ordered to close compared to those allowed to remain open, more than triple the main estimate in Table 2. When we restrict to the 79 dispensaries with a nearest neighbor more than 1/2 mile away, the estimates imply that Part I crime within 1/4 mile is 93 percent higher around dispensaries ordered to close compared to those allowed to remain open. While the results in Appendix Table 5 follow the expected pattern of increasing in magnitude as we reduce catchment overlap, the set of geographically isolated dispensaries may differ on other unaccounted for dimensions. As such, we cannot use the difference in these coefficients relative to the full sample to measure the average downward bias. Rather, these results provide suggestive evidence that our main results underestimate the true effect sizes.

4.2 Results for Crime by Type

We next analyze categories of Part I crimes, which are divided by the FBI into property and violent crimes. We estimate separate models for the following property crimes: burglary, grand theft auto, and larceny theft. Larceny theft is separately broken out as thefts from vehicles and other theft. Arson, a sub-category of Part I property crime is too rare to analyze separately. For violent Part I crime, we analyze aggravated assault and robbery. Murder and rape, which are included in total Part I violent crimes, are also too rare to analyze separately (see Appendix Table 6 for pre-closure mean).[31]

Table 4 shows the impact of dispensary closures on crime by type using the preferred ITT approach that codes closures according to order status. These results show that the effect of dispensary closures loads on property crimes, specifically larceny, and, breaking that out further, theft from vehicles. As with total crime, the effects are very local and

[31] Appendix Table 6 makes apparent the difficulty in analyzing rare crimes. For example, even at 2 miles, there were only an average of 0.04 murders per day around dispensaries ordered to close. At 1/8 of a mile, the count is 0.0002.

monotonically decrease with catchment area radii. This monotonic decrease in the closure estimates and confidence intervals can be seen clearly in Figures 3 and 4, which plot the implied percent change in Part I crimes and theft from vehicles, respectively, along with 95 percent confidence intervals at distances from 1/8 to 2 miles. At distances of 1/2 mile or greater we find no effect of closures on crime, and the small coefficients with relatively tight confidence intervals means we can explicitly rule out even small increases in crime at these larger distances. At 1/3 of a mile the models imply that property crimes increase by 12%, largely driven by increases in larceny and, specifically, theft from vehicles. Even more locally, the estimated effects imply that thefts from vehicles increase by almost 30% at 1/4 of a mile and by 100% at 1/8 of a mile around dispensaries ordered to close relative to those allowed to remain open. While the percent increase in crime near closed dispensaries is large, proper interpretation of these effects must take into account the low number of crimes around each dispensary on any given day. For example, combining the results of Tables 1 and 2, we see that closing a dispensary leads to just 0.0512 additional crimes (0.0399 additional property crimes) per day within a third of a mile of the closed dispensary.

Burglary is the one exception to the general monotonic pattern. Here we find a large, *negative* and marginally significant (p-value=0.07) coefficient for closures at 1/8th of a mile, positive and statistically insignificant coefficients at 1/4th, 1/3rd and 1/2 of a mile, a small negative and statistically insignificant coefficient at 1 mile, and a small negative statistically significant coefficient at 2 miles. While intriguing, this non-monotonic pattern does not admit to an obvious explanation. In addition, unlike the results for total crime or larceny, the burglary results do not hold up in robustness checks and are based on a very small number of events, with an average of 0.0245 burglary per day at 1/8 of a mile. As such, this result should be interpreted with caution.

As with our main results, we find that results for crime by type are insensitive to the treatment of defiers (see Appendix Table 7, which drops defiers, and Appendix Table 8, which recodes them as open) or the inclusion of the closure date (see Appendix Table 9).

5 More crime or displaced crime?

A crucial question in determining the social costs of crime associated with dispensary closures is whether the changes represent an increase (or decrease) in total crime or a shift of crime across either space or time. If crime is spatially displaced, then the increase in crime near a closed dispensary may be offset by decreases in crime further away. Since our main results show that closures lead to significant crime increases at distances of 1/4 to 1/3 of a mile around a dispensary, spatial displacement would imply corresponding decreases in crime at distances of greater than 1/4 to 1/3 mile. To check for this type of displacement, we examine the impact of closures on crime in concentric rings around each dispensary.[32]

Specifically, in Table 5 we analyze crime occurring between 1/4 and 1/3 of a mile, 1/3 and 1/2 of a mile, 1/2 to 1, 1/2 to 2 and 1 to 2 miles around dispensaries. At distances of 1/4 to 1/3 of a mile (a band fully contained within the radii where we find increases in crime) the coefficient on closure is, with the exception of violent crimes, positive. The increase within this band is not statistically distinguishable from zero, however. At 1/3 to 1/2 of a mile, the property crime estimate is negative but close to zero, albeit with a wide confidence interval. Since the overlap issue discussed previously should be exacerbated at larger radii, the magnitude of the estimates within the larger rings could be more downward biased than those at smaller distances. But given that these coefficients are never significant, these results do not provide strong evidence for (or against) spatial displacement.

Analogous to spatial displacement, temporal displacement of crime would mean that the changes in crime associated with closures are offset by changes in crime either before or after the closure period. While the dispensary closure date was well known in advance, there are no clear "re-opening" dates.[33] As such if criminal activity exhibited a significant ex-ante temporal elasticity, we would expect a decrease in crime around dispensaries scheduled to

[32]An alternative approach to checking for displacement would aggregate our data to larger geographic levels as in Freedman and Owens (2011) or sum results across areas as in Aliprantis and Hartley (2015). We choose not to take this approach for several reasons. First, dispensaries may border neighborhoods or police reporting districts, two potential levels of analysis. In this case, aggregation can mask displacement as an increase in crime in an area assigned the dispensary appears bigger when measured relative to a decrease in a neighboring area. In addition, the effects we observe here may be too small relative to the city or region to statistically detect in aggregated data, even in the absence of any actual displacement.

[33]Note that re-opening could be due to either the dispensary deciding to re-open (as many did) or the space itself being taken over by another business.

close but prior to actual closures as criminals waited until June 7 to commit crimes.

We find little evidence of pre-closure differences in either the level or trend in daily crime around dispensaries ordered to close relative to those allowed to remain open. Most directly, since extending the pre-period window around June 7, 2010 yields similar results (see Appendix Table 3), it is unlikely that a pre-period decline in crime in anticipation of future crime commission can explain our results. In other words, criminals do not appear to postpone (or move forward) crimes in anticipation of the mass closure of dispensaries. Given the variation in pre-closure crime levels, we can generally rule out economically significant temporal displacement in the period just prior to the June 7, 2010 closures.

6 Restaurant Closures and Crime

While the results above demonstrate that crime increased near dispensaries that closed relative to those allowed to remain open, such an increase is consistent with multiple mechanisms (see Section 2). Here, we attempt to disentangle some of the possible mechanisms by testing whether this closure effect is unique to dispensaries or reflects a more general business closure phenomenon. We do this by performing a parallel analysis for temporary restaurant closures due to public health code violations in Los Angeles County.

6.1 Background on Restaurant Closures

In Los Angeles County, the Department of Public Health (DPH) is charged, under the California Uniform Retail Food Facilities Law (CURFFL), with enforcing uniform statewide health and sanitation standards for retail food facilities according to the"science-based standards." DPH inspects all facilities that provide food to the public (restaurants, bakeries and markets). Based on the guidelines outlined in the California Retail Food Code (Cal-Code), DPH environmental health specialists grade restaurants on various health and sanitation measures including improper holding temperatures, poor personal hygiene of food employees, contaminated equipment and the presence of vermin and, depending on the outcome, may order a temporary shutdown for remediation.

Based on a Food Official Inspection Report (FOIR), restaurants receive a numerical score between 0-100. Restaurants that score 70 and above are given a grade card that must be posted in an easily visible location (90-100 is an "A", 80-89 a "B", 70-79 a "C"). Restaurants that score less than 70 receive a numerical score card rather than a grade. Restaurants that score less than 70 twice in any twelve month period are subject to closure and the filing of a court case. Such closures are rare. More commonly, if the inspection turns up a "major violation," meaning a violation, such as vermin harborage or infestation, sewage disposal problems or food temperature problems, that poses an imminent health hazard, the restaurant is subject to immediate closure without a permit suspension hearing.[34] Restaurants closed for major violations remain closed until a subsequent follow-up inspection confirms that the situation has been satisfactorily resolved. Follow-up inspections generally take place within two-days but can take up to a week.[35]

Restaurants are inspected twice a year, although those that handle large quantities of "risky foods" (e.g., meat) or consistently score low may be inspected three times a year. The DPH may conduct an additional inspection in response to consumer complaints. Individual inspectors work specific geographic areas determined by the local environmental health office. They work with supervisors to set a schedule for restaurant inspections in increments of one or more months. While inspection scheduling is not standardized, inspections are, depending on the specific supervisor, scheduled weeks to months ahead of time. As such, although the timing of inspections are not explicitly randomized, the process makes it highly unlikely that the exact timing of inspections are correlated with trends in crime in the immediate area around each restaurant. In addition, DPH officials have stated that local conditions (including crime) have no bearing on the timing of inspections.

6.2 Restaurant Data

Data on restaurant closures are from the Environmental Health Division (EHD) of the Los Angeles County DPH, the enforcement division in charge of inspecting retail food facilities. The EHD data include the name and exact location of restaurants closed by the agency, the date of closure, the reason for closure, and in most cases a reopen date. In total, we have 888 restaurant closures during our study period, February 1, 2010 to October 31, 2010.[36]

[34]See: `http://publichealth.lacounty.gov/eh/docs/RetailFoodInspectionGuide.pdf`
[35]This timing is based on conversations with LA County Department of Public Health officials.
[36]The study period was determined by the original data made available to us by the LAPD.

Most closures are caused by "major violations," with roughly two-thirds of the closures in our sample due to vermin harborage or infestation. The next most common offense is a lack of potable or hot water, which accounts for 12 percent of closures. Of the 888 closures, 766 or 86% of them have valid reopen dates. In all the cases we investigated, restaurants with no-reopen dates were in fact open and operational. In multiple conversations with EHA, we were unable to obtain any official reason for missing reopen dates. As described below, we take three approaches to dealing with restaurants with missing restaurant reopen dates – assigning the median closure period of 2 days, treating them as permanently closed or dropping them from the sample. Our primary approach uses the median closure period but, as shown below, the results are not sensitive to this choice.

6.3 Restaurant Analysis

We focus on the universe of Los Angeles County restaurants that were closed for heath code violations between February 1, 2010 to October 31, 2010.[37] Using the same basic specification as in equation (1), we define 1(*closed*) as the period between a restaurant's closure and reopen date. Because we restrict the sample to restaurants with health code violations, the identifying assumption for this analysis is that the *timing* of closures is uncorrelated with crime in the area immediately around the affected restaurant.

Paralleling our dispensary analysis, we drop each first closure day in the analysis. In addition to the concern that crimes reported on closure dates may have occurred prior to that date, many restaurants will be closed for only part of the first closure day. In other words, some restaurants ordered to close temporarily remain open for part of the first closure day – both before and during the inspection. However, as with dispensaries, the results are similar when we include the first closure day in the analysis (shown below).

Appendix Table 10 shows summary statistics for restaurants in the 10 days prior to closure. Since all restaurants in our sample were subject to closure, there are no separate time-invariant restaurant characteristics for closed and open restaurants. Rather, these summary statistics show pre-closure characteristics of neighborhoods around restaurants

[37] Since we will require 10 days of pre and post closure data, the restriction is actually restaurants closed between February 10, 2010 and October 21, 2010.

subject to closure during our sample period. In general, the neighborhoods around restaurants do not look dramatically different from that around dispensaries (in Table 1). The most noteworthy differences are that these neighborhoods are slightly more populous, with larger families (i.e., fewer households, despite more people) and lower family incomes. And, consistent with the fact that restaurant closures occur across the county, not just in the city of Los Angeles, the average Walk Score is slightly lower (71.1) around restaurants than either dispensaries ordered to close (74.9) or allowed to remain open (77.2).

While the inspection scheduling process makes it unlikely that inspections are correlated with crime (since it would require that the DPH be able to predict crime at a very disaggregated level), a related concern is that the probability of closure *conditional* on an inspection is correlated with local crime conditions.[38] If the probability of closure is affected when crime in the immediate vicinity of a restaurant is rising – because, for example, the inspector does a less rigorous review in order to minimize his exposure to crime – it could bias our results. To assess these concerns, we run placebo regressions to test for differences in crime within 1/4, 1/3, 1/2, 1 or 2 miles around restaurants in the days leading up to a closure. In other words, we estimate a regression of the form in (1) but define a placebo closed dummy equal to 1 for the same length of time as the actual closure for the days prior to the closure event (see columns 1-3 in Appendix Table 11). As an alternate test, we define a placebo closed indicator for the day prior to, or the 2 days prior to the closure date (see columns 4-5 in Appendix Table 11). In all cases, we find no statistically significant relationship between the placebo closures and crime. The point estimates are also small in magnitude, with the exception of the 1 day dummy (column 4), which, representing the shortest placebo time period, also has the largest standard errors. In short, we find no evidence of systematic changes in crime in the days leading up to these restaurant closures.

[38]The importance of plausibly exogenous restaurant closures status is made clear by earlier work documenting a the complex relationship between crime and the business activity. For example Greenbaum and Tita (2004) find that surges in violence leads to less business formation and downsizing, while Sloan, Caudill and Mixon Jr. (2015) find that criminal activity is positively correlated with restaurant openings.

6.4 Restaurant Results

In Table 6 we show restaurant results that (i) recode those with missing reopen dates as having been closed for the median number of days closed across the sample, 2 days (col (2)), (ii) treat those with missing reopen dates as closed through the entire post-period (col (3)), or (iii) drop those restaurants with missing reopen dates (col (4)). As with the dispensary analysis, we limit this analysis to the 10 days prior to and 10 days after any restaurant's closure.[39] Since results at 1/8 of a mile generally do not converge, we show results for crime at 2, 1, 1/2, 1/3 and 1/4 mile around restaurants. Pre-closure means for Part I crime at each of these distances are provided in col (1).

Table 6 indicates that total Part I crime increases during temporary restaurant closures. At 1/3 of a mile, total Part I crime increases by about 9 to 12% around closed restaurants relative to open restaurants that were temporarily shut down within plus or minus 10 days. The results are similar irrespective of the treatment of restaurants without re-open dates. In addition, the results show a monotonic increase in the effect size as distance narrows up until 1/4 of a mile, at which point the coefficient is small and statistically insignificant.[40]

Table 7 presents results for the breakdown of crime by type, where restaurants with missing reopen dates are coded as closed for the median length of time in the data. As with dispensaries, we find that the effects of closures are concentrated on property crimes, specifically thefts from vehicles. The estimates imply an almost 30% increase in thefts from vehicles at 1/4 of a mile – generally the smallest radii we can analyze for restaurants. Again as with the dispensaries results, the effects quickly diminish with distance, becoming not just insignificant but also small in magnitude at distances of 1 mile and greater. As

[39]In addition to making this analysis as similar as possible to the dispensary closure analysis, the short time window addresses a concern regarding clustering in inspections. Specifically, the use of a short window around each closure helps ensure that identification is not affected by any gross correlations between the timing of inspections and local crime and reduces the possibility that any results are due to differences in medium or long-run crime trends. The focus on restaurants with similar closure dates also mechanically reduces overlap in the catchment areas simply by reducing the number of restaurants examined on any given day, which as previously discussed introduces a downward bias in our estimates.

[40]The difference in the crime change-distance pattern for restaurants and dispensaries likely reflects differences in catchment overlap and statistical power and not necessarily any difference in the magnitude of the effect across establishment types. While we have more restaurants than dispensaries (888 restaurants vs. 597 dispensaries), restaurants are generally closed for only a couple days and, more importantly, these closures are spread out over 250 calendar days as opposed to just one period for dispensaries.

detailed in the appendix, these results are robust to several additional sensitivity checks: lengthening the window of time around restaurant closures (Appendix Table 12), including closure days (Appendix Table 13), coding restaurants with missing re-open dates as closed for the full post-closure period (Appendix Tables 14) and dropping restaurants with missing reopen dates (Appendix Table 15).

We next check for the displacement of crime either spatially or temporally in response to temporary restaurant closures. As with dispensaries, we check for spatial displacement by examining changes in crime in rings of various sizes around closed restaurants. Table 8 shows the crime changes occurring between 1/4 and 1/3 of a mile, 1/3 and 1/2 of a mile, 1/2 to 1 mile, 1/2 to 2 miles and 1 to 2 miles around closed restaurants. At 1/4 to 1/3 of a mile, which is fully contained within the radii where we find increases in crime around closed restaurants, the coefficient on closure is positive. The increase within this band is significant only for total crimes. The point estimates then drop and are both small in magnitude and not distinguishable from zero at 1/3 to 1/2 of a mile, suggesting that the increase in crime is localized to distances of less than 1/3 of a mile.

To test for temporal displacement, we re-run our standard regression but supplement the restaurant closure period indicator with dummies for both the re-open date and the re-open date plus 1. We focus on the reopening period since restaurant closures are unexpected and thus could not have caused pre-closure shifts in criminality. Rather, the temporary restaurant closures could have led criminals to shift crime earlier in time to the closure period. Such a shift would decrease crime after a reopening. Instead, as shown in Table 9, we find significant increases in crime at 1/3 of a mile around restaurants during the closure period but no compensating decrease in crime on either the re-open day or the day after.

The similarity in the broad pattern of results for restaurants and dispensaries despite the differences in the nature of these businesses, the reason for and timing of their closures, and the identifying assumptions of the analyses, provides additional evidence that the increase in crime following dispensary closures is not spurious. Furthermore, it suggests that the mechanism behind the decrease in crime is not dispensary-specific but indicative of a more general effect of business closures on crime.

7 Modes and Mechanisms

The results presented above show that temporary dispensary closures increase crime in the short-run and that temporary restaurant closures affect crime in a similar fashion. While the increase in crime after both dispensary and restaurant closures may be unrelated, it seems more likely that a common factor drives the shared pattern of results. Under this assumption, we can rule out dispensary-specific mechanisms such as the substitution of alcohol for marijuana or diminished access to formal dispute resolution channels in medical marijuana markets, as the driving force behind the increase in crime. Below, we explore the evidence for and against several possible common factors.

7.1 Walkability and the Role of "Eyes Upon the Street"

One potential common factor affecting crime may be a reduction in foot traffic. If dispensary and restaurant closures reduce foot traffic, informal policing or "eyes upon the street" (Jacobs 1961) may also be diminished and crime could increase. This hypothesis requires that the impact of business closures on crime be mediated through customer foot traffic. Such a connection seems intuitive since a closed business necessarily has fewer customers than an open one. The ideal data to test this would include measures of foot traffic by location. Given that such measures are unavailable, we use neighborhood characteristics to proxy for the *relative* impact of business closures on foot traffic in an area.

To proxy for foot traffic by location, we collect "Walk Scores" from www.walkscore.com by exact business address.[41] Scores range from 0 to 100, and are based on walking paths to amenities. Amenities within a 5 minute walk are given maximum points. More distant amenities receive points based on a decay function, with zero points after a 30 min walk. Pedestrian friendliness is comported into the measure based on population density, block length and intersection density. While Walk Scores do not capture the presence of sidewalks, street lights or speed limits, which likely improve the walking experience, they have been shown to be a useful measure of walkability (Hirsch et al. 2013).

[41] A complementary approach might be to use Dunn and Bradstreet data on the level of employment and the composition (retail vs. wholesale) of establishments at the address and block level as in Rosenthal and Ross (2010).

Walk Score identifies four categories of addresses based on their scoring system: Car-Dependent (0-49), Somewhat walkable (50-69), Very walkable (70-89) and Walker's paradise (90-100). Walkability is determined by the number and proximity of restaurants, bars, coffee shops, grocery stores, and so on. An address with a high Walk Score has many businesses and other features that generate foot traffic nearby whereas one with a low Walk Score has few businesses nearby and relatively little foot-traffic.

How should the Walk Score interact with business closures to affect crime? Since a business with a high Walk Score is located near many other businesses, its customers likely represent a small share of local foot traffic. On the other hand, the closure of a business in a low Walk Score area should have a proportionally large impact on total foot traffic. As such, the eyes upon the street hypothesis (hereafter EUS) would predict that, all else equal, the impact of business closures on crime should be negatively related to Walk Scores (i.e., that a closure should increase crime more in low Walk Score areas.)

A more complete consideration of foot traffic must acknowledge that people are both crime deterrents and crime targets. For very isolated, car dependent areas with little foot traffic, a business closure could reduce crime in the area by removing the few existing crime targets. As an extreme example, consider a business that is the only feature for 1/3 of a mile (i.e., in an extremely car dependent area) and that its closure decreases the number of people in the area from N to zero. Such a closure would substantially decrease foot traffic. But, since there are virtually no remaining crime targets in the immediate area, crime would likely decline despite the loss of crime-deterring eyes upon the street.[42] In this way, EUS predicts a non-monotonic relationship between business closures and Walk Scores: business closures will have smaller (and in the case of isolated areas possibly even negative) effects on crime in the most and least walkable areas and larger, positive effects in moderately walkable areas.

In Table 10 we explore the interaction of business (dispensary or restaurant) closures and walkability on crime. Panel A shows results for dispensary closures and Panel B for restaurant closures. Column 1 shows the impact of closures on total crime within 1/3 of a

[42]Along these lines Sandler (2012) finds that the eviction of residents from, and subsequent demolition of Chicago public housing led to a decrease in crime in the area immediately surrounding the demolition.

mile for dispensaries or restaurants with Walk Scores above versus below 70, corresponding to walkscore.com's cutoff between 'Very' and 'Somewhat' walkable.[43] We find a significant positive closure effect on crime for both dispensaries (Panel A) and restaurants (Panel B) with low Walk Scores, with effect sizes approximately double that found in the full sample (i.e., compared to Tables 2 and 6). When we examine crime by type (columns 3-6), we see that, as in the full sample, the interaction effect is driven by increases in property crime, specifically larceny and theft from vehicles. In low Walk Score areas, dispensary or restaurant closures have more than double the impact on property crime than they do in high Walk Score areas.

In column 2, we further divide up businesses using separate closure dummies for the Car-dependent, Somewhat walkable, Very walkable, and Walker's paradise categories. Here again we find that the closure effect is smaller in highly walkable areas (i.e., areas where a single business closure has little impact on total foot traffic) and larger and positive in the "somewhat walkable" areas. For "Car-dependent" areas, the sign of the coefficient flips and becomes negative; it is also both small in magnitude and statistically indistinguishable from zero. With the caveat that the coefficients for "car-dependent" and "somewhat walkable" areas are only marginally statistically different (p-values of 0.074 and 0.110 for restaurants and MMDs respectively), this pattern is consistent with a non-linear relationship between closures and walkability as predicted by EUS.[44]

While the Walk Score findings suggest that our main results are driven by changes in customer foot-traffic, interpreting the elasticities of these effects with respect to foot traffic is difficult since we have no measure of a business's customer base or, by extension,

[43] Nearly identical results are obtained by dividing the sample into above and below median Walk Scores.

[44] As an alternative proxy for foot traffic, we interacted the closure indicator with indicators for whether the dispensary's ZIP code was above or below median for the density of employees in all dispensary ZIP codes based on the 2010 census ZIP Code Business Patterns data. The results of this analysis are qualitatively consistent with out Walkscore results (see Appendix Table 16): closure effects are larger in less dense areas, where the closure of a dispensary represents a larger proportional decrease in foot traffic. That said, the estimates are somewhat difficult to directly compare to the Walkscore results. In particular, while the density of employees in a ZIP code is a better measure of business activity, it suffers, as a measure of foot traffic, from several shortcomings. First, it cannot distinguish between retail vs non-retail establishments (e.g., business parks and factories). More significantly, ZIP codes capture relatively large areas (over 3.6 (5.8) square miles for the median (mean) ZIP Code in either sample) while our business closure estimates (and foot traffic more generally) are localized; we observe crime effects within a radius of 1/3 of a mile or less, representing an area of approximately 0.35 square miles.

the change in the number of "eyes" associated with a closure. With this very strong caveat in mind, we can nevertheless generate a simple back of the envelope calculation as a face validity check on the magnitude of our coefficients with respect to EUS. Specifically if we assume that a business has 50 customers per day, the results for the 1/3rd mile catchment area suggest that it would take approximately 1,250-1,800 customers to deter one property crime. If we further assume that each customer effectively contributes 15 minutes of monitoring time, then our estimates suggest that it takes roughly 300-450 eyes-upon-the-street hours to deter one property crime.

7.2 Private Security and Public Policing

While our results are consistent with EUS, they may be consistent with several alternative explanations. Perhaps the most plausible alternative is that businesses provide formal, direct on-site security that deters crime. Studies of business improvement districts in Los Angeles have demonstrated the deterrent effect of paid security services (Brooks, 2008; Cook and MacDonald, 2011). Thus, closures may increase crime by removing security services. But, while dispensaries typically employ many forms of security, most restaurants have little more than security cameras, if anything at all. These cameras may not be external, as they often are for dispensaries. And, assuming they are external, it is unclear why restaurants would remove or disable cameras during temporary closure periods. As such, the dismantling of private security seems unlikely to be the main driver of crime effects that are common to both dispensaries and restaurants.

A second alternative explanation relates to changes in police presence, which have been shown to affect similar categories of crime. Klick and Tabarrok (2005) find that a large increase in police presence, combined with increases in closed-circuit surveillance cameras in Washington D.C., led to a significant decrease in a combined category of theft from vehicles and auto-thefts. Draca, Machin and Witt (2011) find that a 50% increase in police presence in London led to a reductions in larceny theft of around 20% in the affected neighborhoods. If closures decrease police presence, they could plausibly increase crime.

Like private security, changes in police service allocations seem unlikely to be a significant driver of our results. For restaurants, it is hard to imagine that police would formally or informally change their behavior in response to temporary restaurant closure. For dispensaries, anecdotal reports suggest that, if anything, police presence may have increased in the days following the closures in order to check for compliance. This increase would be predicted to decrease rather than increase crime. Even if police presence did change in response to temporary restaurant and dispensary closures, the change, based on existing evidence from the literature, would have to be implausibly large to generate our findings. For example Draca et al. (2011) find effects similar in magnitude to our findings in response to a *50% increase* in police presence.

8 Discussion and Conclusions

Analyzing medical marijuana dispensary closures in the City of Los Angeles, we find no support for the idea that closing dispensaries reduces crime. Rather, temporary closures deter some types of Part I crime. To understand the mechanism, we evaluate the impact of temporary restaurant closures due to public health code violations. We find a nearly identical impact of these closures. Both temporary dispensary and restaurant closures increase Part I property crime, specifically theft from vehicles, in a very localized area around closed businesses. The magnitude of the closure effect is correlated with the relative impact of the closure on area foot traffic. Specifically, the increase in crime due to closures (dispensary or restaurant) is negatively correlated with neighborhood walkability in relatively walkable areas but reverses sign or is nonexistent in areas classified as car dependent.

That the pattern of results is so similar across the temporary closure of two different establishment types suggests a common factor may be at play. In other words, the main findings may not capture a dispensary specific effect on crime but rather a more general retail specific effect. Since temporary restaurant closures should have no meaningful effect on policing patterns, police presence is unlikely to be this common factor. On the other hand, the results on neighborhood walkability suggest that "eyes upon the street" may account for the common pattern of results

Taken together, our results provide support for the hypothesis that retail establishments, when operational, provide informal security through their customers. That is, to the extent that businesses bring foot traffic or as Jane Jacobs once famously proclaimed, "eyes upon the street" to a neighborhood, they may deter certain types of "dark alley" crimes. While the idea that "eyes belonging to those we might call the natural proprietors of the street" (Jacobs 1961) can provide public safety on city streets has spawned many studies on the role of urban design and architecture on crime (Newman 1972; Hunter and Baumer 1982; Glaeser and Sacerdote 2000; Foster and Giles-Corti 2008), albeit not all in support of the hypothesis, credible empirical evidence on the impact of local activity on crime remains quite limited. Our results are consistent with a somewhat nuanced view: increased foot-traffic appears to decrease crime but only above a certain threshold of traffic.

Our findings have direct policy implications for regulating marijuana sales in the U.S. They imply that dispensary closures, and potentially the closure of other types of retails establishments, exert a significant negative externality in terms of neighborhood criminality. A quick back of the envelope cost calculation using the change in larceny theft at 1/3 of a mile (from Table 4) and crime costs from McCollister et al. (2010) suggests that an open dispensary provides over $30,000 per year in social benefit in terms of larcenies prevented.[45] This calculation ignores potential offsets in terms of quality-of-life issues, such as loitering, graffiti, double parking and noise.[46] In addition, the current study is underpowered to detect any impact of closures on high-cost, low frequency crimes such as robbery, aggravated assaults, homicide, rape or arson. Future research on the impact of dispensaries on these low-frequency crimes and on quality-of-life issues are crucial for understanding the full economic impact of these establishments.

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[45] A similar calculation using the point estimates for aggravated assault and robbery at 1/3 of a mile from Table 4 suggest social benefit of approximately $350,000 per annum. Given the relatively large standard errors on these estimates, this number should be mainly considered illustrative of the potential *relative* costs associated with property vs violent crimes.

[46] Indeed, these types of NIMBY issues may be the real cost of a dispensary to area residents.

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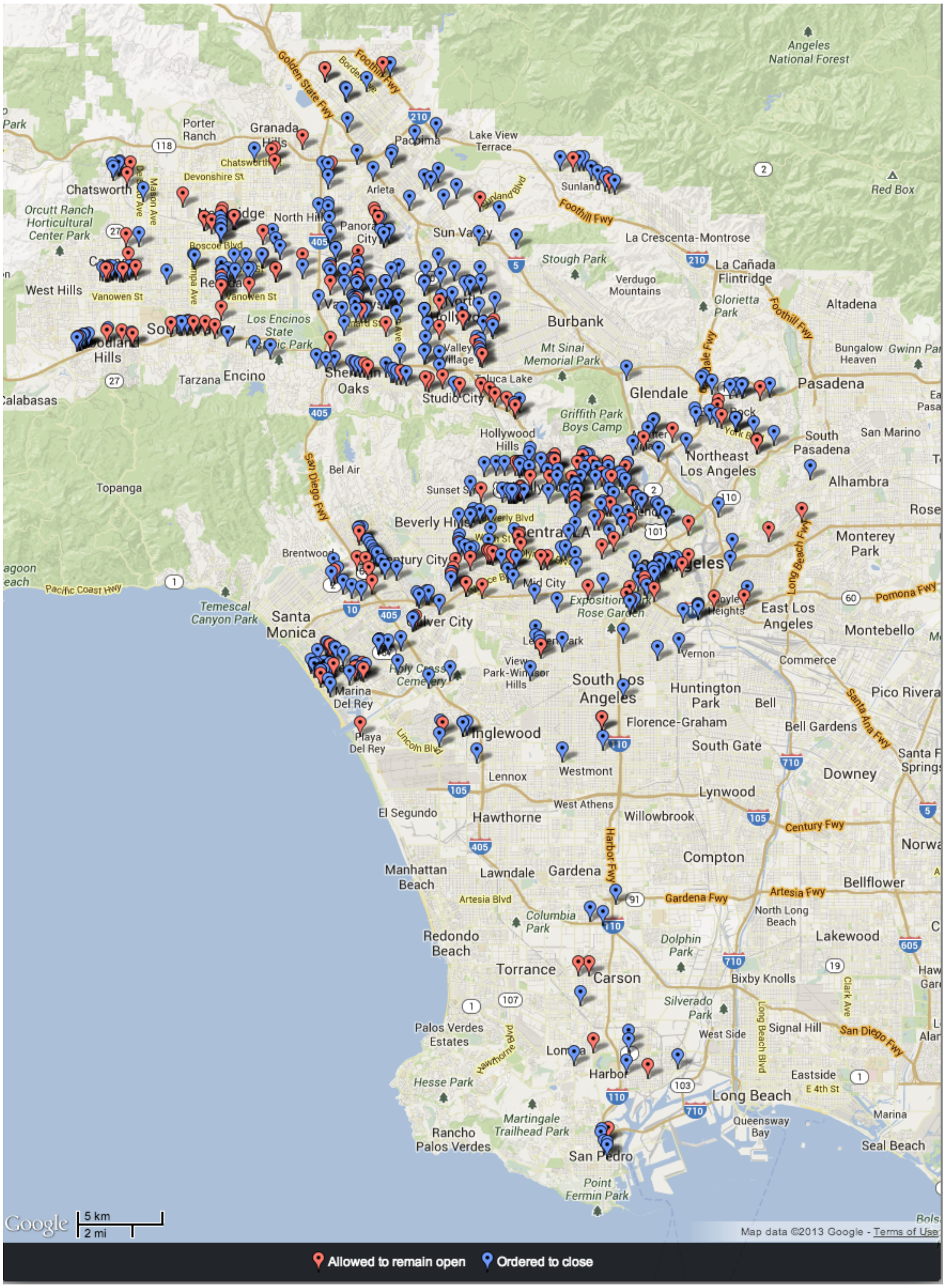


Figure 1. Dispensary location by closure order status.

Figure 2a. Mean Daily Part I Crime at 1 Mile by Closure Status

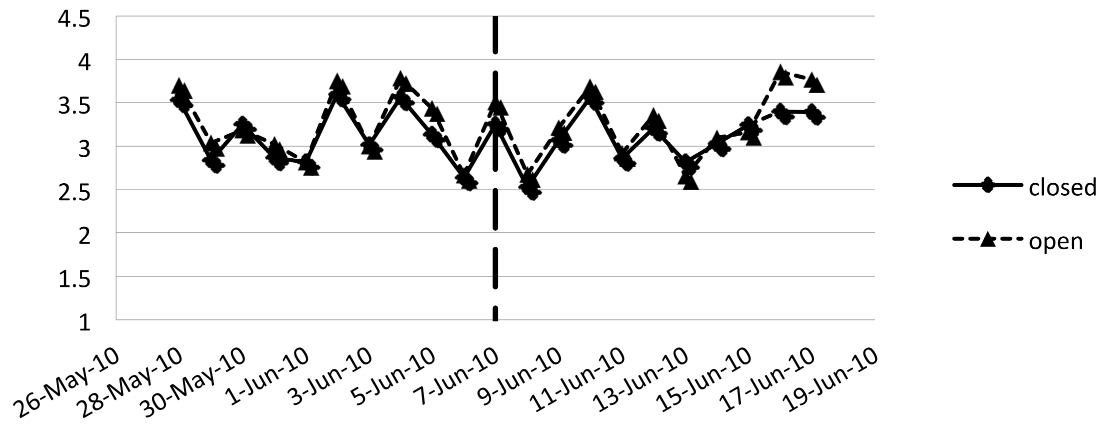


Figure 2b. Mean Daily Part I Crime at 1/3 Mile by Closure Status

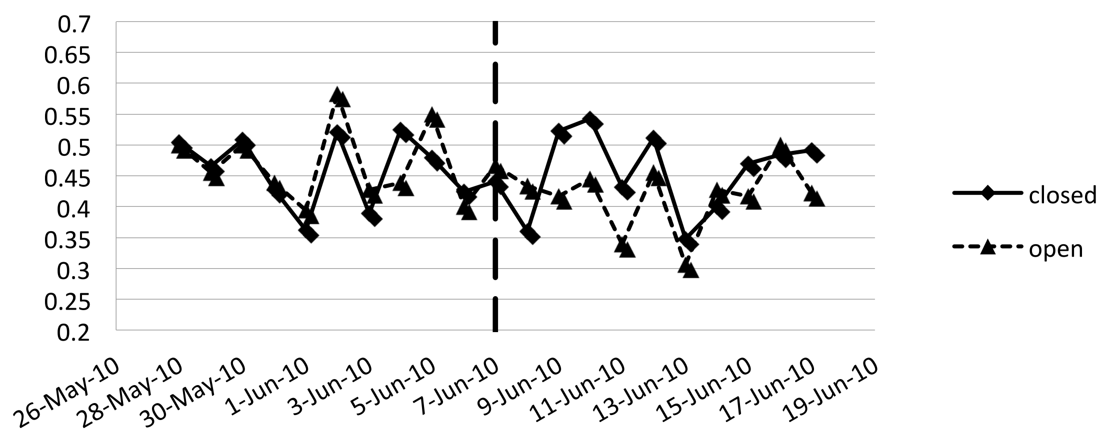


Figure 2c. Mean Daily Part I Crime at 1/8 Mile by Closure Status

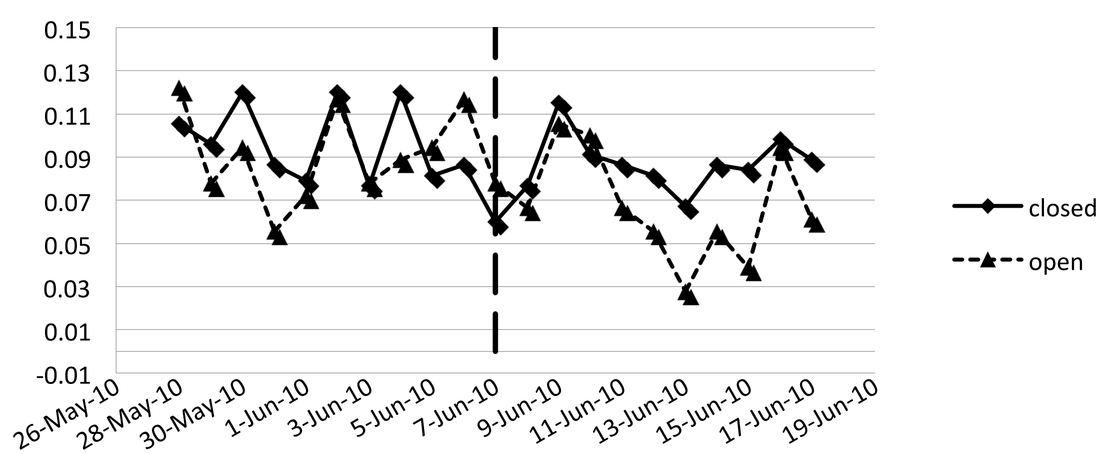


Figure 3. Dispensary Closure Impacts on Part I Crime by Distance

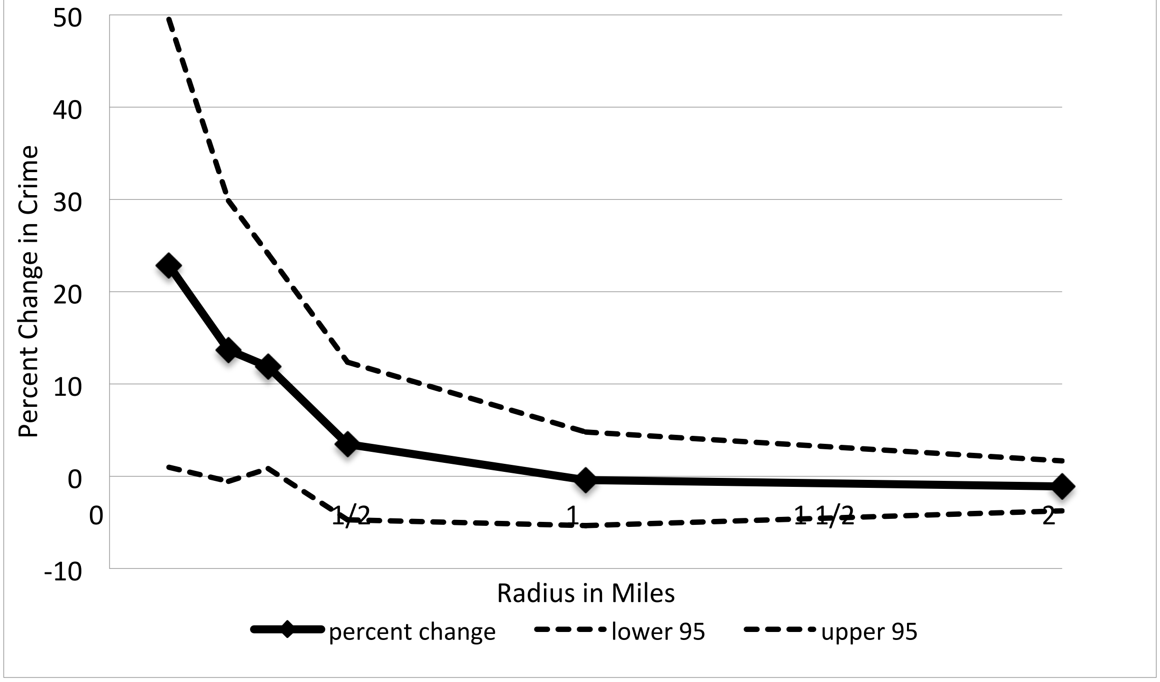


Figure 4. Dispensary Closure and Theft From Vehicles by Distance

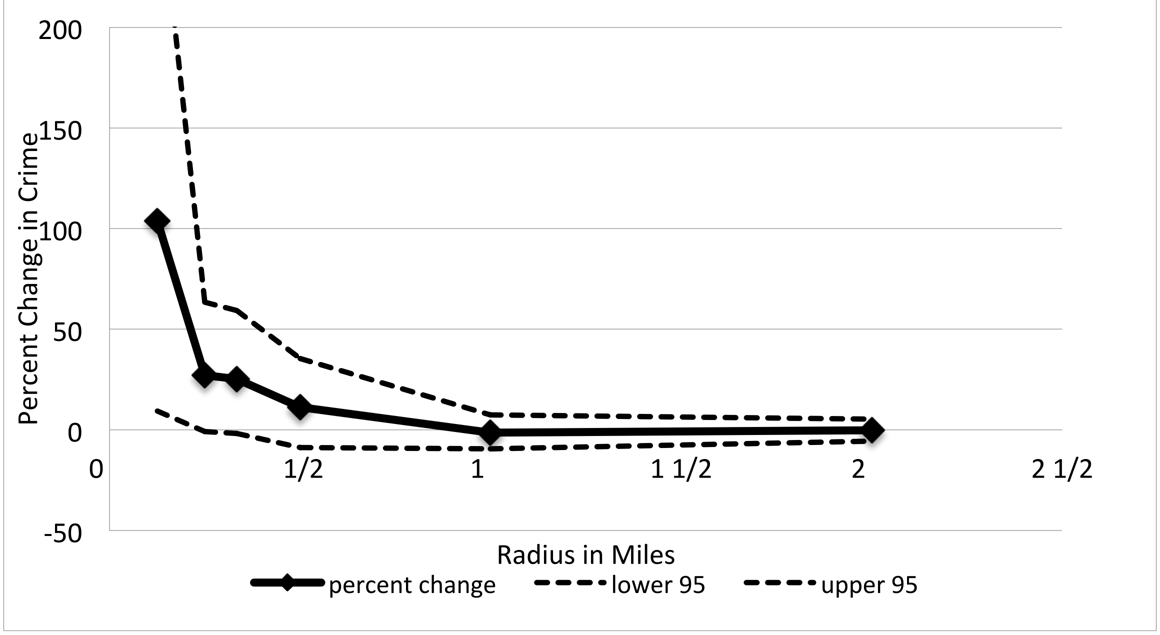


Table 1. Dispensary Summary Statistics Pre versus Post June 7, 2010

	Dispensaries ordered to clos, Pre-June 7	Dispensaries allowed to remain open, Pre-June 7	p-value
Daily Crimes			
Part I Crimes < 1 Mile	3.12	3.24	0.122
Part I Property Crime <1 Mile	2.47	2.56	0.117
Part I Violent Crime < 1 Mile	0.657	0.679	0.466
Part I Crimes < 1/3 Mile	0.466	0.479	.563
Part I Property Crime <1/3 Mile	0.356	0.362	0.751
Part I Violent Crime < 1/3 Mile	0.104	0.106	0.813
Daily Part I subcategories < 1/3 mile			
Aggravated assault	0.049	0.054	0.535
Auto Theft	0.061	0.054	0.276
Burglary	0.052	0.054	0.713
Homicide	0.0014	0.0017	0.835
Rape	0.0046	0.0044	0.957
Robbery	0.048	0.046	0.724
Larceny Theft	0.243	0.254	0.480
Theft	0.144	0.145	0.960
Theft from Vehicles	0.099	0.109	0.254
Zip Code Characteristics			
Population	41960	41947	0.994
Households	15414	15669	0.609
Median Household Income	54621	54900	0.867
Median Age	35.5	35.7	0.480
Occupancy Rate	0.930	0.930	0.772
Share Foreign born	0.376	0.381	0.592
Other			
Walkscore	74.9	77.2	0.056
Closest Neighbor Allowed Open	0.326	0.354	0.514

The p-value is for a two-sided test of differences in means for dispensaries ordered to close vs. allowed to remain open. We compare crime counts in radii of 1 and 1/3 mile around dispensaries in the 10 days prior to June 7, 2010. Zip Code characteristics are from the 2010 Census and the 2011 American Community Survey. Walkscores are from walkscore.com and are matched to dispensaries by exact address. Walkscore.com chategorizes its scores as followss: (1) 0-49 = Car Dependent; (2) 50-69= Somewhat Walkable; (3) 70-89 = Very Walkable and (4) 90-100 = Walker's Paradise.

Table 2. Effect of Dispensary closures on Total Part 1 crime

		Treatment of Defiant Dispensaries		
Radius (miles)	Pre-closure Mean	Intent to Treat	Recoded	Dropped
1/8	0.097	0.206*	0.207*	0.213+
		(0.1)	(0.097)	(0.115)
1/4	0.286	0.128+	0.150*	0.137*
		(0.068)	(0.063)	(0.068)
1/3	0.466	0.112*	0.131*	0.121*
		(0.053)	(0.057)	(0.055)
1/2	0.938	0.0341	0.073	0.047
		(0.042)	(0.031)	(0.043)
1	3.12	-0.004	0.004	-0.001
		(0.026)	(0.017)	(0.026)
2	10.6	-0.011	-0.008	-0.010
		(0.014)	(0.01)	(0.013)
N	4170	11940	11940	11760

Notes: + p<0.10, * p<0.05, ** p<0.01.

Pre-closure means are for crime around dispensaries ordered to close at each distance are shown in col (1). All other cols show point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering by dispensary and by date. All regressions include date and dispensary fixed effects. We include 10 days of data pre and post-closure but drop the actual closure date. To handle dispensaries known to be defiant col (2) does nothing and estimates the intent to treat, col (3) recodes dispensaries known to be defiant and col (4) drops known defiers.

Table 3. Placebo Checks of Dispensary closures on Total Part 1 crime

	Dispensaries			
	Period Relative to June 7th Closure Orders			
Radius (miles)	Actual	-1 month	-2 months	- 3 months
1/8	0.206*	-0.086	0.093	0.105
	(0.1)	(0.142)	(0.106)	(0.137)
1/4	0.128+	-0.034	0.078	-0.045
	(0.068)	(0.077)	(0.067)	(0.076)
1/3	0.112*	-0.039	0.008	-0.037
	(0.053)	(0.043)	(0.053)	(0.047)
1/2	0.0341	-0.028	0.076+	-0.050+
	(0.042)	(0.020)	(0.044)	(0.026)
1	-0.004	0.004	0.014	-0.001
	(0.026)	(0.015)	(0.024)	(0.014)
2	-0.011	.0126+	0.000	0.003
	(0.014)	(0.007)	(0.011)	(0.01)
N	11940	11940	11940	11940

Notes: + $p<0.10$, * $p<0.05$, ** $p<0.01$.

The table shows point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering by dispensary and by date. All regressions include date and dispensary fixed effects. Column (1) is from Table 2. It shows our main results for the June 7th closure orders. Placebo results are in colums 2 - 4. Column 2 assumes a May 10 closure date; column 3 assumes an April 12 closure date and column 4 a March 15 closure data. In all cases, we drop the data from the closure day (actual or placebo) and use 10 days of data on either side of thise date.

Table 4. Effect of Dispensary Closures by Crime Type

	Radius Around Dispensaries (miles)					
	1/8	1/4	1/3	1/2	1	2
Property	0.209+	0.107	0.112*	0.050	-0.005	-0.018
	(0.113)	(0.07)	(0.05)	(0.034)	(0.022)	(0.014)
Burglary	-0.518+	0.106	0.160	0.006	-0.013	-0.056*
	(0.284)	(0.199)	(0.195)	(0.142)	(0.053)	(0.022)
Auto Theft	0.617	-0.071	0.061	-0.072	-0.036	0.016
	(0.509)	(0.318)	(0.2)	(0.136)	(0.065)	(0.029)
Larceny	0.288+	0.145	0.126*	0.091*	0.002	-0.017
	(0.159)	(0.09)	(0.063)	(0.052)	(0.033)	(0.023)
Thefts from Vehicle	0.712*	0.241+	0.224+	0.106	-0.014	-0.003
	(0.318)	(0.128)	(0.123)	(0.101)	(0.044)	(0.028)
Theft	N/A	0.080	0.057	0.079	0.016	-0.030
		(0.13)	(0.078)	(0.067)	(0.035)	(0.024)
Violent	0.188	0.177	0.099	-0.024	0.002	0.020
	(0.274)	(0.162)	(0.147)	(0.106)	(0.062)	(0.035)
Agg. Assault	N/A	0.257	0.200	-0.130	-0.014	-0.000
		(0.245)	(0.241)	(0.166)	(0.083)	(0.065)
Robbery	N/A	-0.001	-0.040	0.072	-0.004	0.030
		(0.214)	(0.194)	(0.126)	(0.088)	(0.042)
N	11940	11940	11940	11940	11940	11940

Notes: + $p<0.10$, * $p<0.05$, ** $p<0.01$.

The table shows point estimates from Poisson regression models as well as standard errors, in parenthesis, that allow for twoway clustering by dispensary and by date. All regressions include date and dispensary fixed effects. Standard errors allow for twoway clustering by dispensary and by date. Regressions are estimated using 10 days pre and post the June 7th closure orders. June 7th is not included in the sample. Arson is included in total property crime and rape and murder are included in total violent crime; we do not estimate separate count models for these 3 types of crimes because they are too rare to allow for convergence. Aggravated assault and robbery do not convege at 1/8 mile.

Table 5. Spatial Displacement of Crime due to Dispensary Closures

Crime Within Rings of…	1/4-1/3	1/3-1/2	1/2-1	1/2-2	1-2
All Part 1	0.080	-0.036	-0.019	-0.015	-0.014
	(0.115)	(0.065)	(0.029)	(0.014)	(0.014)
Property	0.120	-0.010	-0.028	-0.025+	-0.023
	(0.117)	(0.06)	(0.025)	(0.014)	(0.015)
Violent	-0.085	-0.149	0.014	0.024	0.028
	(0.188)	(0.126)	(0.085)	(0.037)	(0.048)
N	11940	11940	11940	11940	11940

Notes: + p<0.10, * p<0.05, ** p<0.01.

The table shows point estimates from Poisson regression models as well as standard errors, in parenthesis, that allow for twoway clustering by dispensary and by date. All regressions include date and dispensary fixed effects. Standard errors allow for twoway clustering by dispensary and by date. Regressions are estimated using 10 days pre and post the June 7th closure orders. June 7th is not included in the sample.

Table 6. Effect of restaurant closures on Total Part 1 crime

Radius (miles)	Pre-closure Mean	Restaurants Treatment of Missing Re-Open Date Use Median Closure Period	 Assume Ongoing Closure	 Drop Those with Missings
1/4	0.234	0.061	0.013	0.003
		(0.063)	(0.053)	(0.043)
1/3	0.331	0.110*	0.094*	0.094+
		(0.045)	(0.038)	(0.049)
1/2	0.803	0.053*	0.052+	0.050
		(0.027)	(0.027)	(0.031)
1	2.75	0.000	0.013	-0.003
		(0.018)	(0.015)	(0.02)
2	9.52	0.014	0.002	0.018
		(0.01)	(0.009)	(0.011)
N	8880	17760	17760	14600

Notes: + p<0.10, * p<0.05, ** p<0.01.

Pre-closure means are for crime at each distance are shown in col (1). The table shows point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering by restaurnt and by date. All regressions include date and business fixed effects. Standard errors allow for twoway clustering by business and by date. We include 10 days pre and post-closure. To handle missing reopen dates, col (2) codes them as closed through the rest of the sample period, col (3) uses the median number of days closed and col (4) drops these cases. Poisson regression for restaurants for distance of <1/8 mile do not converge.

Table 7. Effect of Restaurant Closures by Crime Type

	Radius Around Restaurants (miles)				
	1/4	1/3	1/2	1	2
Property	0.082	0.096+	0.046	0.000	0.018
	(0.07)	(0.051)	(0.028)	(0.02)	(0.011)
Burglary	0.021	-0.038	0.007	-0.031	0.039
	(0.196)	(0.139)	(0.097)	(0.05)	(0.024)
Auto Theft	-0.008	0.038	0.024	-0.011	0.007
	(0.208)	(0.156)	(0.103)	(0.042)	(0.025)
Larceny	0.096	0.128*	0.067+	0.012	0.016
	(0.08)	(0.064)	(0.038)	(0.026)	(0.015)
Thefts from Vehicle	0.255*	0.227*	0.100	0.007	0.007
	(0.124)	(0.096)	(0.066)	(0.04)	(0.022)
Theft	0.012	0.054	0.050	0.022	0.024
	(0.105)	(0.086)	(0.055)	(0.032)	(0.018)
Violent	0.001	0.157	0.078	-0.002	0.004
	(0.146)	(0.101)	(0.067)	(0.039)	(0.018)
Agg. Assault	-0.209	0.181	0.016	-0.007	-0.026
	(0.228)	(0.166)	(0.127)	(0.068)	(0.03)
Robbery	N/A	N/A	0.097	-0.005	0.026
			(0.084)	(0.05)	(0.026)
N	17760	17760	17760	17760	17760

Notes: + p<0.10, * p<0.05, ** p<0.01.

The table shows point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering by restaurant and by date. All regressions include date and restaurant fixed effects.. The regressions us e10 days of datat pre and post-closure. We use the median number of days closed to handle missing reopen dates. Poisson regression for restaurants for distance of <1/8 mile do not converge. Robbery does not converage at 1/4 or 1/3 of a mile. While arson is included in property crime and rape and murder are included in violent cime counts, these crimes are too few to separately estimate changes due to business closures.

Table 8. Spatial Displacement of Crime due to Restaurant Closures

Panel A: Spatial Displacement	1/4-1/3	1/3-1/2	1/2-1	1/2-2	1-2
All Part 1	0.172*	0.002	-0.021	0.011	0.020+
	(0.086)	(0.041)	(0.023)	(0.011)	(0.012)
Property	0.109	0.005	-0.018	0.016	0.025+
	(0.096)	(0.043)	(0.027)	(0.012)	(0.013)
Violent	N/A	0.007	-0.038	-0.004	0.005
		(0.103)	(0.103)	(0.021)	(0.023)
N	17760	17760	17760	17760	17760

Notes: + p<0.10, * p<0.05, ** p<0.01.

Notes: The table shows point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering by business and by date. All regressions include date and restaurant fixed effects. We include 10 days pre and post-closure. We use the median number of days closed to handle restaurants with no reopen date.

Table 9. Temporal Displacement of Crime at 1/3 Mile around Restaurant Closures

	All	Property All	Larceny	Vehicle	Violent All
closed	0.111*	0.096+	0.128*	0.233*	0.161
	(0.045)	(0.052)	(0.065)	(0.1)	(0.099)
re-open	0.034	-0.006	0.063	-0.005	0.157
	(0.061)	(0.081)	(0.093)	(0.176)	(0.12)
re-open + 1	-0.021	0.002	-0.058	0.086	-0.113
	(0.062)	(0.083)	(0.097)	(0.153)	(0.127)
N	17760	17760	17760	17760	17760

Notes: + p<0.10, * p<0.05, ** p<0.01.

Notes: The table shows point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering b ybusiness and by date. For both datasets - restaurants and dispensaries - we include 10 days pre and post-closure. We use the median number of days closed to handle restaurants with no reopen date. For dispensary regressions, we exclude June 7 from the data. Re-open and re-open + 1 are dummies equal to one on the day a restaurant is allowed to re-open and the day after a restaurant is allowed to re-open respectively.

Table 10. Dispensary or Restaurant Closures and Walkscores: Crime at 1/3 of a Mile Around Establishments

Panel A: Dispensary Closures	Part I Crime		Property			Violent
			Total Property	Larceny	Theft from Vehicle	Total Violent
Closed (Low Walk Score)	0.194*		0.180+	0.285*	0.367+	0.284
	(0.083)		(0.094)	(0.106)	(0.203)	(0.236)
Closed (High Walk Score)	0.083		0.091	0.085	0.176	0.022
	(0.069)		(0.058)	(0.071)	(0.146)	(0.157)
Closed * Car-dependent		-0.063				
		(0.197)				
Closed * Somewhat Walkable		0.233*				
		(0.092)				
Closed * Very Walkable		0.104				
		(0.077)				
Closed * Walker's Paradise		0.042				
		(0.11)				
N	11940	11940	11940	11940	11940	11940

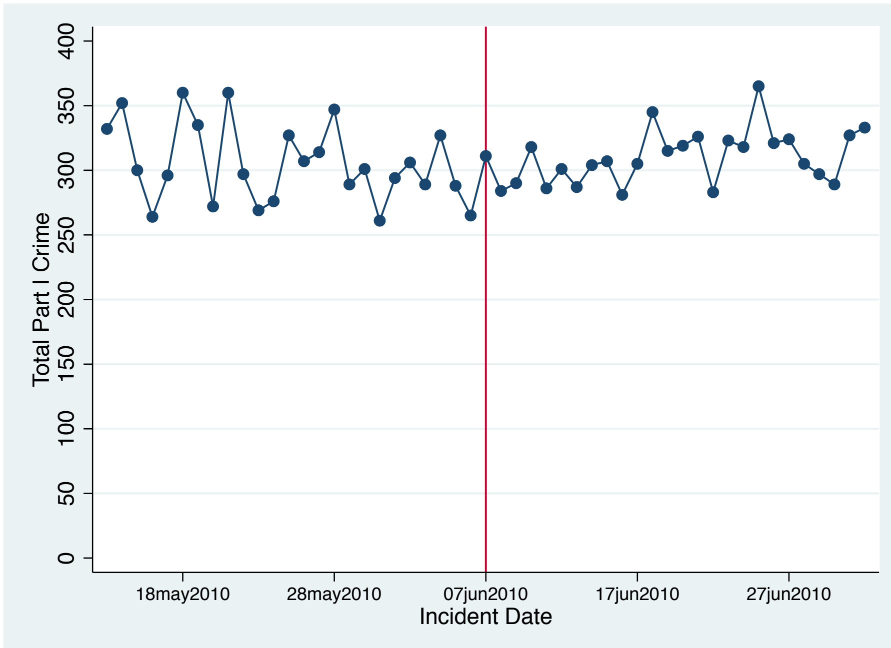
Panel B: Restaurant Closures	Part I Crime		Property			Violent
			Total Property	Larceny	Theft from Vehicle	Total Violent
Closed * Low Walk Score	0.187+		0.180	0.347*	0.513*	0.194
	(0.103)		(0.114)	(0.152)	(0.212)	(0.207)
Closed * High Walk Score	0.083		0.068	0.071	0.135	0.141
	(0.051)		(0.056)	(0.069)	(0.108)	(0.155)
Closed * Car-dependent		-0.155				
		(0.241)				
Closed * Somewhat Walkable		0.244*				
		(0.114)				
Closed * Very Walkable		0.084				
		(0.069)				
Closed * Walker's Paradise		0.082				
		(0.073)				
N	17760	17760	17760	17760	17760	17760

Notes: + $p<0.10$, * $p<0.05$, ** $p<0.01$.

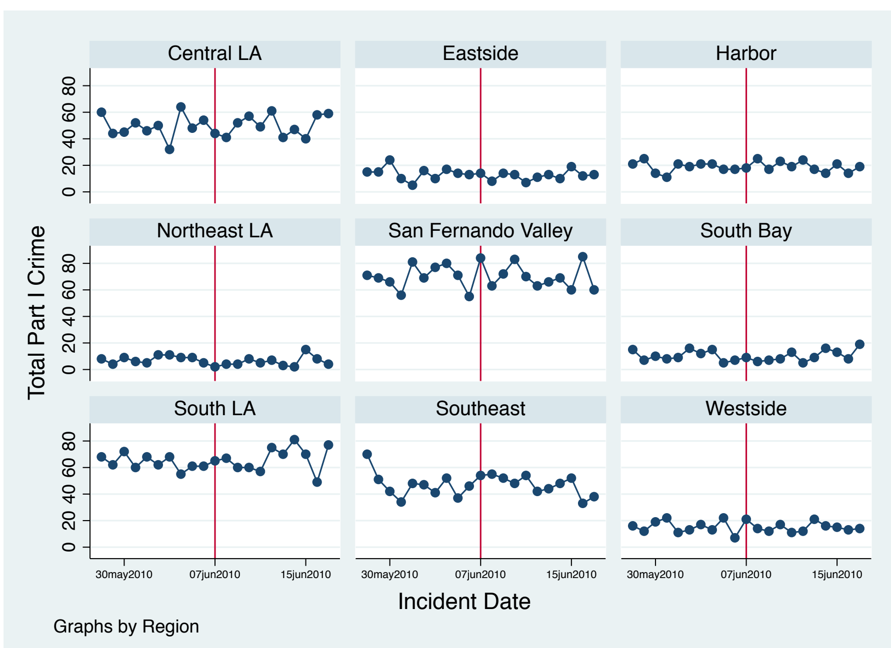
Notes: The table shows point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering by business and by date. For both datasets - restaurants and dispensaries - we include 10 days pre and post-closure. We use the median number of days closed to handle restaurants with no reopen date. For dispensary regressions, we exclude June 7 from the data. Low Walk Scores are those that are defined as either "Car-dependent" or "Somewhat Walkable", while High Walk Scores are those defined as "Very Walkable" and "Walker's Paradise". Dividing the sample into businesses above and below the median Walk Score generate qualitatively similar results.

Online Appendix

Appendix Figure 1a. Trends in Part I Crimes

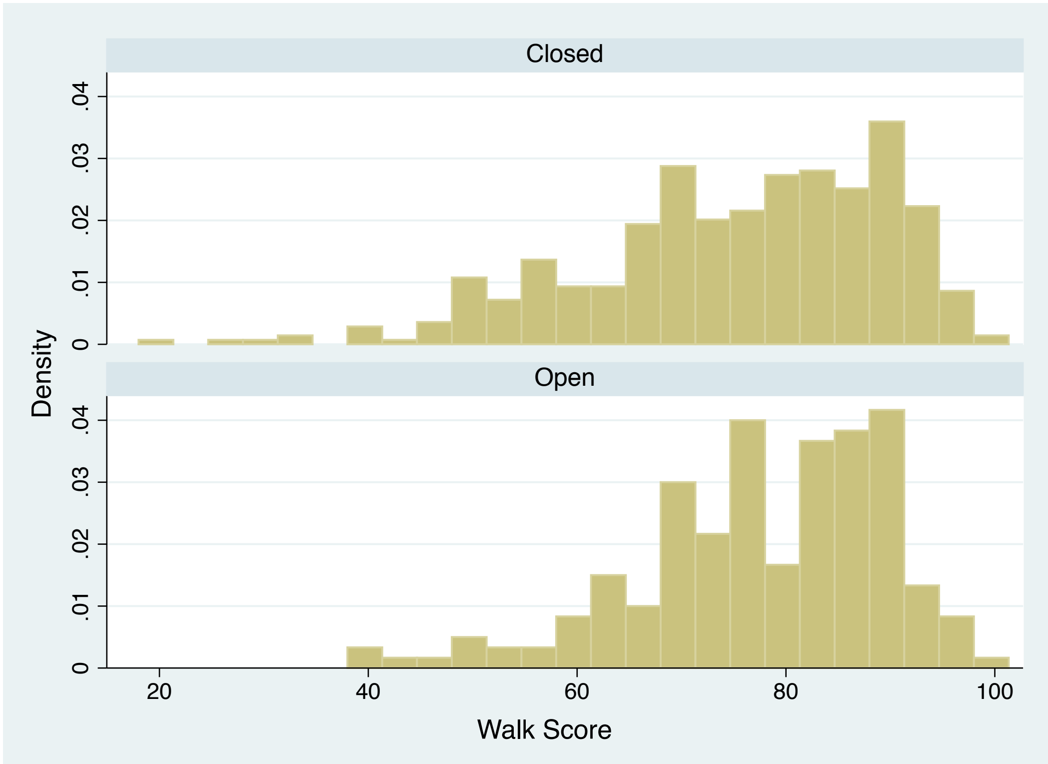


Appendix Figure 1b. Trends in Part I Crimes by Region



Notes: Data are from the LA Times Crime L.A. Project. The overall trends are restricted to the regions, as defined here http://maps.latimes.com/neighborhoods/, listed in Figure 1b. Excluded are crimes committed in the Angeles Forest, Antelope Valley, Northwest County, Pomona Valley, San Gabriel Valley, Santa Monica Mountains, Verdugos. We excluded them based on proximity to city dispensaries.

Appendix Figure 2. Distribution of Walk Scores by Dispensary Closure Orders



Notes: The figure above plots the distribution of Walk Scores by exact dispensary address for those dispensaries ordered to close (top panel) and those dispensaries allowed to remain open.

Appendix Table 1. Timeline of Events Impacting Medical Marijuana Dispensaries in Los Angeles

Date	Law/Event	Key Details
14-Dec-06	LAPD fact sheet released	Details the explosion of medical marijuana dispensaries in the City of Los Angeles, shows statistics to support the view that the dispensaries increase crime, and recommends a moratorium on new dispensaries and regulations for existing dispensaries
14-Sep-07	ICO:L.A. Ordinance 179027	Placed a temporary moratorium on the opening of new medical marijuana dispensaries in the City of Los Angeles. Allows for a hardship exemption.
13-Nov-07	ICO registration deadline	Deadline for dispensary registration under the ICO
24-Jun-09	ICO amended via L.A. Ordinance 180749	Eliminates hardship exemption
26-Jan-10	L.A. Ordinance 181069 to regulate medical marijuana collectives passes	Caps the number of dispensaries at 70. Allows dispensaries in excess of 70 to remain operational provided that they comply with the ICO and abide by new requirements. Dispensaries must be geographically distributed across L.A. community plan areas in proportion to the population; must be at least 1,000 feet from “sensitive use” buildings, such as schools and parks; and must not be located on a lot “abutting, across the street or alley from, or having a common corner with a residentially zoned area.”
7-Jun-10	L.A. Ordinance 181069, Chapter IV, Article 5.1, takes effect	The city shuts down the more than 400 dispensaries that had not registered by November 13, 2007. Offenders face civil penalties of $2,500 per day and may receive up to six months in jail . The remaining dispensaries have 180 days to comply with the new zoning requirements, which, in many cases, means moving.
25-Aug-10	Villaraigosa memo	City states that 128 of the remaining 169 dispensaries must shut down because they had changes in management, which were precluded under the ICO. City allows these dispensaries to remain open until the courts can rule on the decision’s legality.
24-Nov-10	Koretz-Hahn and other amendments to L.A. Ordinance 181069	City Council adopts amendments that clarify and effectively eliminate the “same ownership and management” requirements and extend the timeline for full compliance for “qualifying” dispensaries. Mayor has until December 6, 2010, to decide on the amendments.
10-Dec-10	Mohr injunction	Los Angeles County Superior Court Judge Anthony J. Mohr grants an injunction that bars the city from enforcing key aspects of L.A. Ordinance 181069, including closures based on the moratorium.
25-Jan-11	L.A. Ordinance 181530 takes effect	Amends L.A. Ordinance 181069 to cap the number of dispensaries at 100 among those continuously operating since September 14, 2007. Allocates permits by lottery.

SOURCES: Brown (2008), California Senate Bill 420 (2003), Compassionate Use Act of 1996, Council of the City of Los Angeles (2007), Council of the City of Los Angeles (2009), Council of the City of Los Angeles (2010), Hoeffel (2010a), Hoeffel (2010b), Hoeffel (2011d), Johnston and Lewis (2009), LACityClerk Connect (undated[b]), Lagmay (2010), and Los Angeles County Department of Regional Planning (2009), Los Angeles Police Department, Narcotics Division (2006), and United States Department of Justice (2009).

Appendix Table 2. Alternative Models of the Effect of Dispensary Closures on Total Part 1 crime

	Negative Binomial			Zero-inflated Poisson		
	Treatment of Defiant Dispensaries			Treatment of Defiant Dispensaries		
Radius (miles)	ITT	Recoded	Dropped	Ongoing	Recoded	Dropped
1/8	0.206+	--	--	0.205+	0.217+	0.216+
	(0.109)			(0.111)	(0.127)	(0.119)
1/4	0.127+	0.150*	0.137*	0.130+	0.156*	0.141*
	(0.068)	(0.063)	(0.067)	(0.07)	(0.066)	(0.070)
1/3	0.114*	0.134*	0.123*	0.116*	0.135*	0.123*
	(0.055)	(0.057)	(0.056)	(0.054)	(0.057)	(0.056)
1/2	0.036	0.073	0.049	0.038	0.071	0.049
	(0.042)	(0.046)	(0.043)	(0.041)	(0.044)	(0.042)
1	-0.005	0.004	-0.002	--	--	-0.001
	(0.027)	(0.026)	(0.026)			(0.026)
2	-0.011	-0.008	-0.010	-0.011	-0.009	-0.010645
	(0.014)	(0.013)	(0.013)	(0.013)	(0.012)	0.0132143
N	11940	11940	11760	11940	11940	11760

Notes: + p<0.10, * p<0.05, ** p<0.01.

Models in the first three columns are negative binomial and in the last three are zero-inflated poisson models. All regressions include date and business fixed effects. Standard errors allow for twoway clustering by business and by date. For both datasets - restaurants and dispensaries - we include 10 days pre and post-closure. To handle dispensaries known to be defiant cols (1) and (4) do nothing and estimate the intent to treat, cols (2) and (5) recode dispensaries known to be defiant and cols (3) and (6) drop known defiers. Negative Binomial Regressions do not converge at 1/8 mile if we recode or drop defiers (col(2) or col(3)). Zero-inflated Poisson models don't converge at 1 mile in the intent to treat and the recoded analysises (col(4) and col(5)).

Appendix Table 3. Effect of lengthening the study window to 60 Days

	Total Part I Crimes		
Radius (miles)	Days 1-10	Days 11-20	Days 21-30
1/8	0.263**	0 .048	0.146
	(0.09)	(0.114)	(0.145)
1/4	0.115+	0.057	-0.052
	(0.059)	(0.043)	(0.054)
1/3	0.089*	0.054+	-0.005
	(0.042)	(0.03)	(0.045)
1/2	0.024	0.014	0.004
	(0.026)	(0.028)	(0.027)
1	-0.003	-0.012	-0.001
	(0.018)	(0.021)	(0.012)
2	-0.003	-0.015	-0.010
	(0.012)	(0.014)	(0.011)

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and dispensary fixed effects and use 60 days of data -- 30 days on either side of June 7, 2010. The total number of observations in each regression is 35820 = 60 days * 597 dispensaries. Standard errors allow for twoway clustering by dispensary and by date. Within a crime category, each row represents a separate regression. For each crime, column (1) provides the coefficient on the first, column (2) the second and column (3) the third and last 10 days in the full 30 day post closure period.

Appendix Table 4. Effect of Dropping June 6-8 in Estimation of Closues on Total Part 1 crime

		Dispensaries Length of Window		
Radius (miles)	Pre-closure Mean	+/- 9 days	+/- 19 days	+/- 29 days
1/8	0.097	0.176	0.204+	0.230*
		(0.119)	(0.105)	(0.1)
1/4	0.286	0.145+	0.101+	0.130*
		(0.075)	(0.06)	(0.06)
1/3	0.466	0.144**	0.092**	0.105**
		(0.053)	(0.038)	(0.037)
1/2	0.938	.043156	.030805	.0341048
		(0.044)	(0.027)	(0.025)
1	3.12	-.0003429	.0043993	.0014057
		(0.028)	(0.021)	(0.02)
2	10.6	-.0123588	-.0009354	.0000517
		(0.015)	(0.012)	(0.012)
N	4170	10,746	22,686	34,626

Notes: + p<0.10, * p<0.05, ** p<0.01.

Pre-closure means are for crime around dispensaries ordered to close at each distance are shown in col (1). All other cols show point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering by dispensary and by date. All regressions include date and dispensary fixed effects. In all regressions, we drop the day of as well as the day before and day after the closure order took effect, i.e., June 6-Jun 8, 2010. In col (1), we include 9 days, in col (2) 19 and col (3) 29 days on either side of the closure orders. Thus, for the 597 dispensaries, col (1) captures 18 days (N=18*597=10746), col (2) 38 days (N=22686 = 38* 597) and col (3) 58 days (N=34626 = 58*597).

Appendix Table 5. Effect of limiting overlap - Total Part 1 crime

	Restricting to dispensaries with nearest neighbor more than…	
Radius (miles)	1/3 mile away	1/2 mile away
1/8	0.293	0.412
	(0.222)	(0.589)
1/4	0.382*	0.660*
	(0.178)	(0.319)
1/3	0.325**	0.491+
	(0.131)	(0.26)
1/2	0.196*	0.342
	(0.098)	(0.227)
1	0.035	0.045
	(0.049)	(0.094)
2	-0.006	0.023
	(0.024)	(0.033)
N	3160	1580

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and dispensary fixed effects. Standard errors allow for twoway clustering by business and by date. For both datasets - restaurants and dispensaries - we include 10 days pre and post-closure. Column (1) restricts to dispensaries with a nearest neighbor more than 1/3 of a mile away while column (2) restricts to dispensaires with a nearest neighbor more than 1/2 mile away.

Appendix Table 6. Pre-closure Summary Statistics for Dispensaries Ordered to Close

	Radius Around Dispensaries (miles)					
	1/8	1/4	1/3	1/2	1	2
Property	0.072	0.219	0.357	0.734	2.47	8.35
Burglary	0.011	0.032	0.052	0.111	0.418	1.47
Auto Theft	0.011	0.039	0.061	0.120	0.396	1.30
Larceny	0.050	0.148	0.243	0.502	1.65	5.58
Thefts from Vehicle	0.016	0.059	0.099	0.219	0.762	2.63
Theft	0.034	0.088	0.144	0.284	0.891	2.95
Violent	0.025	0.066	0.104	0.204	0.657	2.23
Agg. Assault	0.011	0.032	0.049	0.095	0.314	1.01
Robbery	0.013	0.031	0.048	0.096	0.306	1.08
Rape	0.0014	0.0026	0.0046	0.0094	0.0283	0.1053
Homicide	0.0002	0.0007	0.0014	0.0034	0.0084	0.0367
N	4170	4170	4170	4170	4170	4170

This table shows mean crime counts by type in the 10 days prior to June 7, 2010 for dispensaries ordered to close. Means are generally indistinguishable for those allowed to remain open.

Appendix Table 7. Effect of Dispensary Closures by Crime Type - Dropping Defiers

	Radius Around Dispensaries (miles)					
	1/8	1/4	1/3	1/2	1	2
All Part 1	0.213+	0.137*	0.121*	0.047	-0.001	-0.010
	(0.115)	(0.068)	(0.055)	(0.043)	(0.026)	(0.013)
Property	0.206+	0.110	0.116*	0.059+	-0.004	-0.017
	(0.114)	(0.069)	(0.051)	(0.034)	(0.022)	(0.014)
Burglary	-0.518+	0.102	0.117	0.013	-0.016	-0.057**
	(0.284)	(0.193)	(0.195)	(0.141)	(0.054)	(0.021)
Auto Theft	0.617	-0.072	0.069	-0.057	-0.031	0.022
	(0.509)	(0.324)	(0.204)	(0.139)	(0.067)	(0.031)
Larceny	0.281+	0.152+	0.126*	0.098+	0.003	-0.018
	(0.165)	(0.087)	(0.062)	(0.051)	(0.032)	(0.022)
Thefts from Vehicle	0.714*	0.241+	0.218+	0.111	-0.014	-0.004
	(0.323)	(0.128)	(0.123)	(0.101)	(0.044)	(0.027)
Theft	N/A	0.092	0.062	0.087	0.019	-0.030
		(0.132)	(0.076)	(0.064)	(0.034)	(0.024)
Violent	0.229	0.212	0.130	0.007	0.002	0.022
	(0.29)	(0.17)	(0.15)	(0.108)	(0.062)	(0.035)
Agg. Assault	N/A	0.279	0.235	-0.099	-0.012	-0.002
		(0.351)	(0.242)	(0.167)	(0.082)	(0.064)
Robbery	N/A	0.053	-0.007	0.105	0.014	0.034
		(0.215)	(0.197)	(0.131)	(0.088)	(0.042)
N	11760	11760	11760	11760	11760	11760

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and dispensary fixed effects. Standard errors allow for twoway clustering by dispensary and by date. Regressions are estimated using 10 days pre and post the June 7th closure orders. June 7th is not included in the sample and dispensaries known to have defied closure orders are dropped. Arson is included in total property crime and rape and murder are included in total violent crime; we do not estimate separate count models for these 3 types of crimes because they are too rate to allow for convergence. Theft, aggravated assault and robbery do not convege at 1/8 mile.

Appendix Table 8. Effect of Dispensary Closures by Crime Type with Defiers Recoded

	Radius Around Dispensaries (miles)					
	1/8	1/4	1/3	1/2	1	2
All Part 1	0.207+	0.150*	0.131*	0.073	0.004	-0.008
	(0.118)	(0.064)	(0.057)	(0.046)	(0.025)	(0.012)
Property	0.180	0.112+	0.116*	0.073*	-0.002	-0.016
	(0.111)	(0.065)	(0.051)	(0.035)	(0.022)	(0.014)
Burglary	-0.357	0.088	0.130	0.027	-0.021	-0.058**
	(0.301)	(0.173)	(0.189)	(0.134)	(0.054)	(0.019)
Auto Theft	0.479	-0.073	0.086	-0.019	-0.020	0.036
	(0.474)	(0.326)	(0.209)	(0.139)	(0.07)	(0.033)
Larceny	0.235	0.156*	0.117*	0.105*	0.004	-0.018
	(0.165)	(0.078)	(0.056)	(0.048)	(0.031)	(0.021)
Thefts from Vehicle	0.696**	0.235+	0.192	0.116	-0.017	-0.006
	(0.318)	(0.124)	(0.124)	(0.098)	(0.043)	(0.025)
Theft	-0.027	0.107	0.067	0.097*	0.023	-0.028
	(0.207)	(0.112)	(0.067)	(0.058)	(0.032)	(0.024)
Violent	0.308	0.277	0.196	0.079	0.034	0.025
	(0.311)	(0.179)	(0.153)	(0.115)	(0.064)	(0.033)
Agg. Assault	N/A	0.257	0.285	-0.005	0.026	0.020
		(0.329)	(0.231)	(0.163)	(0.082)	(0.057)
Robbery	0.043	0.164	0.068	0.176	0.052	0.043
	(0.276)	(0.219)	(0.197)	(0.145)	(0.088)	(0.041)
N	11940	11940	11940	11940	11940	11940

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and dispensary fixed effects. Standard errors allow for twoway clustering by dispensary and by date. Regressions are estimated using 10 days pre and post the June 7th closure orders. June 7th is not included in the sample. Dispensaries known to have defied closure orders are recoded as open. Arson is included in total property crime and rape and murder are included in total violent crime; we do not estimate separate count models for these 3 types of crimes because they are too rate to allow for convergence. Aggravated assault do not convege at 1/8 mile.

Appendix Table 9. Effect of Dispensary Closures by Crime Type including Closure Date

	Radius Around Dispensaries (miles)					
	1/8	1/4	1/3	1/2	1	2
All Part 1	0.163	0.101	0.094+	0.021	-0.007	-0.009
	(0.109)	(0.07)	(0.054)	(0.042)	(0.025)	(0.013)
Property	0.193+	0.081	0.099+	0.033	-0.012	-0.017
	(0.109)	(0.071)	(0.052)	(0.037)	(0.023)	(0.014)
Burglary	-0.484+	0.066	0.072	-0.008	-0.005	-0.043+
	(0.285)	(0.199)	(0.194)	(0.137)	(0.052)	(0.024)
Auto Theft	0.484	-0.118	0.019	-0.084	-0.037	0.017
	(0.459)	(0.302)	(0.193)	(0.132)	(0.063)	(0.029)
Larceny	0.273+	0.128	0.125*	0.070	-0.009	-0.021
	(0.156)	(0.086)	(0.063)	(0.051)	(0.033)	(0.022)
Thefts from Vehicle	0.657*	0.217+	0.240*	0.099	-0.013	-0.032
	(0.303)	(0.123)	(0.116)	(0.091)	(0.041)	(0.026)
Theft	0.027	0.066	0.045	0.048	-0.005	-0.028
	(0.227)	(0.125)	(0.074)	(0.068)	(0.041)	(0.024)
Violent	0.015	0.139	0.081	0.021	0.013	0.026
	(0.294)	(0.159)	(0.135)	(0.099)	(0.059)	(0.063)
Agg. Assault	N/A	0.193	0.181	-0.106	0.018	0.020
		(0.31)	(0.221)	(0.158)	(0.082)	(0.057)
Robbery	N/A	-0.018	0.054	0.059	-0.011	0.026
		(0.2)	(0.172)	(0.114)	(0.08)	(0.04)
N	12537	12537	12537	12537	12537	12537

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and dispensary fixed effects. Standard errors allow for twoway clustering by dispensary and by date. Regressions are estimated using 10 days pre and post the June 7th closure orders. June 7th is not included in the sample. Dispensaries known to have defied closure orders are recoded as open. Arson is included in total property crime and rape and murder are included in total violent crime; we do not estimate separate count models for these 3 types of crimes because they are too rare to allow for convergence. Aggravated assault and robbery do not convege at 1/8 mile.

Appendix Table 10. Restaurant Summary Statistics

	Restaurants, 10-days pre-closure
Daily Crimes	
Part I Crimes < 1 Mile	2.75
Part I Property Crime <1 Mile	2.08
Part I Violent Crime < 1 Mile	0.674
Part I Crimes < 1/3 Mile	0.383
Part I Property Crime <1/3 Mile	0.285
Part I Violent Crime < 1/3 Mile	0.098
Daily Part I subcategories < 1/3 mile	
Aggravated assault	0.042
Auto Theft	0.048
Burglary	0.043
Homicide	0.0015
Rape	0.0030
Robbery	0.051
Larceny Theft	0.194
Theft	0.119
Theft from Vehicles	0.074
Zip Code Characteristics	
Population	44040
Households	13579
Median Household Income	50472
Median Age	34.6
Occupancy Rate	0.939
Share Foreign born	0.403
Other	
Walkscore	71.1
Closest Neighbor Allowed Open	N/A

Appendix Table 11. Restaurant Closure Placebo Checks

	Placebo based on Actual Closure Length			Placebo Based on Fixed Periods	
Radius (miles)	Assume Ongoing Closure	Use Median Closure Period	Drop Those with Missings	1 day prior	2 days prior
1/4	0.020	-0.003	0.013	-0.090	-0.006
	(0.054)	(0.052)	(0.061)	(0.09)	(0.065)
1/3	-0.007	-0.043	-0.012	-0.076	-0.009
	(0.043)	(0.042)	(0.05)	(0.074)	(0.05)
1/2	0.021	0.003	0.027	-0.007	0.020
	(0.032)	(0.031)	(0.035)	(0.051)	(0.035)
1	-0.004	-0.013	-0.003	-0.030	0.006
	(0.017)	(0.015)	(0.018)	(0.023)	(0.019)
2	-0.001	0.004	-0.006	0.001	-0.001
	(0.009)	(0.008)	(0.009)	(0.012)	(0.01)
N	17760	17760	14600	17760	17760

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and business fixed effects. Standard errors allow for twoway clustering by business and by date. Placebo closures indicators reverse the date of closures around the closure date. That is if a restaurant was closed for 2 days (the median closure length), then the placebo closure would be a dummy equal to one for the to days prior to the restaurant closure. Cols (2)-(4) use actual closure periods but vary in how they treat missing reopen dates. Cols (5) and (6) use fixed pre-closure periods.

Appendix Table 12. Effect of lengthening the study window - Total Part 1 crime

	Restaurant Closures		
Radius (miles)	+/-15 days	+/-20 days	+/-30 days
1/4	0.045	0.018	0.016
	(0.057)	(0.056)	(0.054)
1/3	0.094*	0.043	0.073+
	(0.042)	(0.047)	(0.041)
1/2	0.051+	0.032	0.044+
	(0.026)	(0.025)	(0.026)
1	0.001	-0.004	-0.005
	(0.016)	(0.015)	(0.016)
2	0.012	0.007	0.006
	(0.009)	(0.008)	(0.008)
N	26573	36696	52031

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and restaurant fixed effects. Standard errors allow for twoway clustering by restaurant and by date. We include either 15, 20 or 30 days on either side of the closure period (in contrast to the 10 on either side in the main tables). To handle missing reopen dates, we use the median number of days closed. Poisson regression for restaurants for distance of <1/8 mile do not converge.

Appendix Table 13. Effect of Restaurant Closures by Crime Type including Closure Date

	Radius Around Restaurants				
	1/4	1/3	1/2	1	2
All	0.040	0.082*	0.037	-0.004	0.008
	(0.051)	(0.037)	(0.024)	(0.015)	(0.008)
Property	0.066	0.098*	0.029	-0.000	0.013
	(0.059)	(0.043)	(0.026)	(0.018)	(0.009)
Burglary	0.114	0.031	-0.016	-0.020	0.026
	(0.162)	(0.11)	(0.077)	(0.041)	(0.019)
Auto Theft	0.109	0.149	0.040	-0.006	0.017
	(0.165)	(0.119)	(0.076)	(0.035)	(0.02)
Larceny	0.046	0.093+	0.044	0.009	0.009
	(0.068)	(0.052)	(0.035)	(0.022)	(0.012)
Thefts from Vehicle	0.141	0.169*	0.039	0.015	0.013
	(0.106)	(0.078)	(0.054)	(0.033)	(0.019)
Theft	-0.005	0.047	0.055	0.008	0.005
	(0.09)	(0.073)	(0.048)	(0.027)	(0.014)
Violent	-0.047	0.043	0.075	-0.015	-0.007
	(0.122)	(0.085)	(0.054)	(0.03)	(0.014)
Agg. Assault	N/A	0.052	0.064	-0.017	-0.045*
		(0.128)	(0.101)	(0.052)	(0.024)
Robbery	N/A	0.029	0.079	-0.016	0.029
		(0.123)	(0.073)	(0.038)	(0.022)
N	18648	18648	18648	18648	18648

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and restaurant fixed effects. Standard errors allow for twoway clustering by restaurant and date. The regressions use 10 days of data pre and post-closure. We also include the closure date even though the restaurant may have closed for only part of the day. We use the median number of days closed to handle missing reopen dates. Poisson regression for restaurants for distance of <1/8 mile do not converge. Robbery does not converage at 1/4 or 1/3 of a mile. While arson is included in property crime and rape and murder are included in violent cime counts, these crimes are too few to separately estimate changes due to business closures.

Appendix Table 14. Effect of Restaurant Closures by Crime Type with Missing Reopen Dates Coded as Continued Closure

	Radius Around Restaurants				
	1/4	1/3	1/2	1	2
All Part 1	0.061	0.110**	0.053**	0.000	0.014
	(0.063)	(0.045)	(0.027)	(0.018)	0.01
Property	-0.003	0.063	0.036	0.012	0.007
	(0.058)	(0.042)	(0.031)	(0.017)	(0.009)
Burglary	-0.185	-0.089	-0.022	-0.029	0.002
	(0.185)	(0.132)	(0.088)	(0.043)	(0.024)
Auto Theft	-0.007	0.110	0.056	0.004	-0.007
	(0.16)	(0.114)	(0.083)	(0.04)	(0.02)
Larceny	0.028	0.086	0.051	0.022	0.013
	(0.068)	(0.054)	(0.037)	(0.022)	(0.013)
Thefts from Vehicle	0.092	0.126	0.054	0.025	0.009
	(0.118)	(0.091)	(0.064)	(0.035)	(0.019)
Theft	-0.026	0.036	0.044	0.021	0.016
	(0.089)	(0.072)	(0.05)	(0.028)	(0.016)
Violent	0.100	0.201**	0.100*	0.014	-0.015
	(0.115)	(0.086)	(0.06)	(0.034)	(0.014)
Agg. Assault	0.001	0.238*	0.146	0.026	-0.025
	(0.178)	(0.14)	(0.107)	(0.059)	(0.025)
Robbery	N/A	N/A	0.047	-0.017	-0.015
			0.072	0.042	0.02
N	17760	17760	17760	17760	17760

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and restaurant fixed effects. Standard errors allow for twoway clustering by restaurant and by date. The regressions us 10 days of data pre and post-closure. We treat restaurants with missing re-open dates as permanently closed. Poisson regression for restaurants for distance of <1/8 mile do not converge. Robbery does not converage at 1/4 or 1/3 of a mile. While arson is included in property crime and rape and murder are included in violent cime counts, these crimes are too few to separately estimate changes due to business closures. Restaurants with missing reopen dates are coded as closed for the full post-closure observation period.

Appendix Table 15. Effect of Restaurant Closures by Crime Type, Dropping Restaurants with Missing Reopen Dates

	Radius Around Restaurants				
	1/4	1/3	1/2	1	2
All Part 1	0.000	0.084**	0.047*	-0.007	0.010
	(0.054)	(0.041)	(0.027)	(0.016)	(0.008)
Property	0.008	0.081*	0.032	-0.002	0.015
	(0.06)	(0.047)	(0.028)	(0.018)	(0.011)
Burglary	-0.101	-0.055	-0.043	-0.044	0.017
	(0.174)	(0.128)	(0.086)	(0.046)	(0.021)
Auto Theft	0.078	0.021*	0.046	-0.001	0.018
	(0.168)	(0.12)	(0.081)	(0.038)	(0.023)
Larceny	0.014	0.076	0.056	0.014	0.014
	(0.073)	(0.058)	(0.037)	(0.023)	(0.014)
Thefts from Vehicle	0.072	0.124	0.008	0.019	0.023
	(0.114)	(0.088)	(0.06)	(0.036)	(0.022)
Theft	-0.002	0.057	0.100**	0.015	0.006
	(0.1)	(0.081)	(0.05)	(0.03)	(0.016)
Violent	-0.027	0.096	0.106*	-0.024	-0.000
	(0.131)	(0.097)	(0.061)	(0.036)	(0.016)
Agg. Assault	N/A	N/A	0.089	-0.033	-0.028
			(0.113)	(0.058)	(0.028)
Robbery	N/A	0.101	0.121	-0.017	0.027
		(0.132)	(0.077)	(0.043)	(0.023)
N	15330	15330	15330	15330	15330

Notes: + p<0.10, * p<0.05, ** p<0.01.

All regressions include date and restaurant fixed effects. Standard errors allow for twoway clustering by restaurant and by date. The regressions us 10 days of data pre and post-closure. We drop restaurants that are missing reopen dates. Poisson regression for restaurants for distance of <1/8 mile do not converge. Robbery does not converage at 1/4 or 1/3 of a mile. While arson is included in property crime and rape and murder are included in violent cime counts, these crimes are too few to separately estimate changes due to business closures. Restaurants with missing reopen dates are excluded from the analysis.

Appendix Table 16. Dispensary or Restaurant Closures and Employee Density: Crime at 1/3 of a Mile Around Establishments

Panel A: Dispensary Closures					
	Part I Crime	Total Property	Larceny	Theft from Vehicle	Total Violent
Closed below median density)	0.128	0.21**	0.229	0.265**	-.164
	(0.078)	(0.087)	(0.125)	(0.107)	(0.275)
Closed (above median density)	0.094	0.049	0.064	0.183**	.289
	(0.08)	(0.06)	(0.079)	(0.065)	(0.202)
N	11900	11900	11900	11900	11900
Panel B: Restaurant Closures					
	Part I Crime	Total Property	Larceny	Theft from Vehicle	Total Violent
Closed below median density)	0.198*	0.195*	0.239+	0.17	0.109
	(0.087)	(0.087)	(0.137)	(0.144)	(0.181)
Closed (above median density)	0.048	0.050	0.088	0.261*	0.184
	(0.05)	(0.069)	(0.077)	(0.122)	(0.123)
N	15356	15356	15356	15356	15356

Notes: + p<0.10, * p<0.05, ** p<0.01.

Notes: The table shows point estimates from Poisson regression models. Standard errors, shown in parenthesis, allow for twoway clustering by business and by date. For both datasets - restaurants and dispensaries - we include 10 days pre and post-closure. We use the median number of days closed to handle restaurants with no reopen date. For dispensary regressions, we exclude June 7 from the data. To measure density, we use the number of employees per square mile in a dispensary or restaurant ZIP code.