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Not in my backyard? Not so fast. The effect of marijuana legalization on neighborhood crime☆

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ABSTRACT

This paper studies the effects of marijuana legalization on neighborhood crime and documents the patterns in retail dispensary locations over time using detailed micro-level data from Denver, Colorado. To account for endogenous retail dispensary locations, we use a novel identification strategy that exploits exogenous changes in demand across different locations arising from the increased importance of external markets after the legalization of recreational marijuana sales. The results imply that an additional dispensary in a neighborhood leads to a reduction of 17 crimes per month per 10,000 residents, which corresponds to roughly a 19 percent decline relative to the average crime rate over the sample period. Reductions in crime are highly localized, with no evidence of spillover benefits to adjacent neighborhoods. Analysis of detailed crime categories provides insights into the mechanisms underlying the reductions.

1. Introduction

After Colorado and Washington became the first U.S. states to legalize recreational marijuana in 2012, the number of states legalizing or decriminalizing the sale and use of marijuana quickly expanded. After a wave of ballot initiatives in 2016, the sale and use of marijuana for recreational purposes was legal in 7 states, and another 22 states had legalized medical use. As states legalize the manufacturing, distribution, and sale of marijuana, the local health, economic, crime, and safety effects of marijuana dispensaries have become an important public policy issue. In addition to the aggregate effects of legalization, understanding the local effects of marijuana dispensaries on neighborhoods is important for designing policies to address concerns of residents who are broadly open to legalization but have a "not in my backyard" attitude toward dispensaries near their homes.[1]

The economic welfare and public policy implications of marijuana legalization are broad in scope and stem from several primary sources. First, given that legalization improves access to marijuana and presumably reduces prices, in the long run, legalization could affect local health, economic, crime, and safety outcomes due to increased marijuana use.[2] Second, legalization may displace illicit markets affecting neighborhood outcomes, including crime or access to other illegal drugs.[3] Third, marijuana dispensaries may have social or economic spillover effects that may affect welfare. Finally, there are direct implications for public finance through increased tax revenue and decreased enforcement costs.[4]

☆ The views expressed here are those of the authors and do not represent the views of the Federal Reserve Bank of Philadelphia, the Federal Reserve System, or Rocky Mountain Health Plans.

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[1] The *Denver Post* reported that, "So far, the conflicts over pot sales … have been confined to individual neighborhoods and are driven largely by a 'not in my backyard' mentality rather than by a wider denunciation of the entire industry" Aguilar (2015).

[2] Becker et al. (2006) provide an economic theory of prohibition that considers price elasticities of supply and demand.

[3] Displacement of illicit markets for marijuana or other illegal drugs can have important implications given that drug markets are subject to returns to scale in both supply and demand. See Jacobson (2004) for detailed discussion of the interaction between crime and drug markets.

[4] We provide a review of recent related literature and the broader policy implications of legalization in Section 2.

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Our paper focuses on the short-run causal effects of marijuana legalization on neighborhood crime.[5] To date, we are unaware of any research that studies the effects of full marijuana legalization on local crime, although several papers have analyzed local effects of decriminalization polices or legalization of medical marijuana.[6] Two papers study aggregate effects of crime from decriminalization and legalization policies. Adda et al. (2014) study the effects of a depenalization policy in a borough of London, exploiting time variation from a policy change. The authors found that the decriminalization policy led to an aggregate decrease in crime arising from reallocation of police resources but also a decrease in home values, suggesting a welfare loss. Huber et al. (2016) use cross-sectional variation in state policies and panel data between 1970 and 2012 and find evidence that the legalization of medical marijuana reduces robberies, larcenies, and burglaries, although they find that decriminalization has no effect on crime. We expand on these aggregate studies by considering local variation in crime outcomes within a jurisdiction that has legalized marijuana.

Our approach is related to that of Freishtler et al. (2016) who study the effect of medical marijuana on neighborhood crime in Long Beach, California, and exploit a change in policy that led to the closing of dispensaries. They show that there was no change in crime locally, but found positive correlations between increased crime and the dispensary density in adjacent neighborhoods.

Chang and Jacobson (2017) also exploit the unexpected closing of dispensaries in California to identify the effect on crime. They find the somewhat different result; specifically that there is a temporary increase in crime very near the dispensaries after they have closed that dissipates over time.[7] Our research expands on this work in an important way by accounting for the endogenous location of dispensaries in neighborhoods. In addition, we study both recreational and medical marijuana dispensaries and utilize panel data that capture both dispensary openings and closings.

In this paper, we investigate the local effects of marijuana legalization on neighborhood crime in Denver, Colorado, which was the first state to fully legalize marijuana use, sales, and production for medical and recreational purposes. The baseline analysis compares year-over-year changes in dispensaries to year-over-year changes in crime rates using monthly tract-level data from Denver. To account for the endogenous location of dispensaries, we exploit the change in policy that allowed for recreational sales starting in January 2014. This policy changed the spatial demand for marijuana by allowing for sales to out-of-state residents and increased demand from residents from neighboring municipalities that only allowed medical sales. Proxies for market access are used to instrument for changes in dispensary density around the time of the policy change.

One contribution of our research is the construction of a unique and rich geospatial data set.[8] To measure dispensary locations, we use panel data from the State of Colorado that provide exact locations of dispensary licenses at monthly frequencies. Our primary measure of crime comes from the city of Denver and includes the location, date, and type of crime committed. We construct a detailed location-specific measure of available land using data on zoning, geographic features, and legal restrictions on dispensary locations, which we augment with demographic and employment data provided by the U.S. Census.

Initial analysis of the data shows that the locations of dispensaries are not randomly allocated across space or neighborhood characteristics. Dispensaries are more concentrated in areas with higher poverty, higher minority populations, and higher initial employment density. Correlations between the growth of new dispensaries and neighborhood characteristics strengthened over time.

It is also likely that dispensary locations and crime are correlated with unobservable neighborhood characteristics, which creates a challenge for causal analysis. For example, after the legalization of recreational marijuana sales in 2014, new dispensaries were subject to public hearings. Neighborhoods with more social cohesion, could potentially prevent the opening of new dispensaries. Previous studies on local crime effects have not directly addressed the endogeneity of dispensary locations.

An important contribution of our paper is that we employ a novel identification strategy that exploits shifts in demand across locations over time to analyze causal effects of marijuana legalization on crime. While the legalization of recreational marijuana in 2014 applied to the entire state, many municipalities within Colorado prohibit sales within their own jurisdictions. Residents living in municipalities near Denver that prohibit recreational sales often travel to Denver to purchase marijuana. Therefore, locations within Denver that have more access to demand from neighboring municipalities show more growth in their dispensary density, ceteris paribus. In addition, out-of-state tourists could purchase marijuana starting in 2014, further increasing the demand for dispensaries in locations with access to broader outside markets. In the empirical analysis, we use two geospatial variables to proxy for access to outside demand: a neighborhood's proximity to municipal borders and proximity to major roads or highways. These variables are then used to instrument for changes in locations of dispensaries over time.[9] We combine our instrumental variables (IV) with our panel data to compare year-over-year changes in crime with year-over-year changes in dispensary density and use time fixed effects to control for aggregate crime trends.

Note that a particular advantage of our identification strategy relative to others is that it relies on variation within a single jurisdiction. Studies that use differences in policies across jurisdictions suffer from endogeneity if the policy decision is correlated with unobserved characteristics of a municipality. *In our setting, the policy change is the same for all locations in the study, and the variation in treatment is due to an exogenous shift in external demand.*

Our main IV results imply that receiving a dispensary in a neighborhood causes a reduction in crime; specifically, an additional dispensary per 10,000 residents is associated with a reduction of 17 crimes per 10,000 residents per month. The average number of crimes per 10,000 residents in Denver is 90 per month, so an additional dispensary is associated with roughly a 19 percent decline in crime. These IV results are robust across a number of specifications. The results from the ordinary least squares (OLS) specification, on the other hand, are positive, reflecting that dispensaries are on average selected into neighborhoods with increasing crime.[10]

In addition to finding an overall reduction in crime when a dispensary is added to a neighborhood, we also find that there is some variation across crime categories. The effect is generally strongest for nonviolent crimes; specific crimes most affected include criminal trespassing,

[5] Other recent papers have studied the local effects of establishments or institutions on crime including Steinberg et al. (2019) (schools); Nicols and Tosun (2017) (casinos), and Montolio and Planells-Struse (2016) (football matches).

[6] In a paper developed concurrently with this one, Burkhardt and Goemens (2018) use an event study design, and find reductions in crime in some specific categories. In addition, two recent papers have looked at the local effects of legalization on house prices: Cheng et al. (2016) and Conklin et al. (2016).

[7] We also find that the presence of a dispensary results in a reduction in crime, but our results suggest the effect is more permanent. Our results are also similar in terms of the geographic extent of the effects and the types of crime that are affected.

[8] See Section 5 for a detailed description of the data.

[9] Although the details of identification strategy are different, this approach is related to the literature that uses policy discontinuities across jurisdictional borders for other applications. Examples include Harding et al. (2012) (cigarette taxes); Agrawal (2015) (local sales taxes); Holmes (1998) (manufacturing activity); and Card and Krueger (1994) (minimum wage).

[10] Bhuller et al. (2016), studying the effects of incarceration, provide an example from the literature where selection effects are important in studying criminal behavior.

public-order crimes, criminal mischief, and simple assault. There are also reductions in violent crimes driven by a decrease in aggravated assault, although results are not statistically significant. Reductions in these crimes are consistent with disruption of illicit markets and with a substitution away from alcohol use. However, we do not find strong evidence that legalization disrupts the sale of other illicit drugs. While our point estimates suggest that sales of other drugs decline, the estimates are not statistically significant. In addition, we do not find significant increases in marijuana-related crimes that are tracked separately by the City of Denver, which implies that there are not large crime effects from increased marijuana use itself.

Lastly, we find that the reduction in crime is very localized and contained within the census tract of the dispensary's location. We test for spillover effects by regressing changes in crime on the predicted change in dispensary density both within that tract and from neighboring tracts (from our first-stage regression). We find no significant effects of neighboring dispensary density on crime.

Overall, our results suggest that dispensaries cause an overall reduction in crime in neighborhoods, with no evidence of spillovers to surrounding neighborhoods. The local nature of these effects is consistent with increased policing or private security response near the dispensaries. These findings may point to an aggregate reduction in crime due to legalization, but further investigation would be needed to rule out a reshuffling of crime to other neighborhoods. Effects on specific crimes vary and are weakly consistent with the theory that legalization could disrupt illicit markets and also support evidence that marijuana could be a substitute for alcohol consumption. Lastly, there is no evidence that increased marijuana use itself results in additional crime. More generally, there is potential for further research to understand the underlying mechanisms that lead to the change in crime after legalization.

The rest of the paper is organized as follows. Section 2 discusses the policy implications of legalization and related literature. Section 3 provides background and descriptive data analysis of legalization in Colorado. Section 4 outlines the empirical methodology and identification strategy. Section 5 gives a detailed description of data collection and construction. Section 6 presents the main results. Section 7 provides some additional analysis and discussion. Finally, Section 8 concludes.

2. Policy implications and related literature

This section outlines the important policy implications of legalization and summarizes existing literature. Caulkins et al. (2016), Anderson and Rees (2014), and Miron and Zweibel (1995) provide more comprehensive summaries of the broad issues surrounding legalization than are provided here. Recent research highlights some of the potential negative effects of legalization. Given that legalization will likely lead to increased consumption, the effects of marijuana use itself could become more prevalent.[11] Previous research suggests that increased consumption could decrease educational obtainment, negatively affect health and well-being, inhibit brain development, or increase fatalities and injuries from drugged driving.[12] Dispensaries could also be perceived to be a disamenity by the public, thus decreasing local housing prices or harming local businesses.

In contrast, legalization could also have positive effects. Increased accessibility could provide positive medicinal effects or may be a substitute for the consumption of other more harmful drugs.[13] There is also increasing evidence that marijuana can act as a substitute for alcohol consumption.[14] The substitutability with alcohol may have particular implications for neighborhood crime, given the findings of Twinam (2017) that show that bars and liquor stores are an important source of neighborhood crime. Legalization might also improve social outcomes from decreases in incarceration rates and enforcement costs.[15] In addition, there are direct impacts on local and state budgets from increased tax revenues. The state sales tax rate in Colorado is substantial at 27.9 percent for recreational marijuana (plus municipal taxes where applicable). This amounted to revenues each year between 2014 and 2016 of $44.9, $76.2, and $102.7 million, respectively.[16]

In this paper, we focus on the short-run effects of legalization on crime. Existing theories provide different predictions for the effects of legalization on crime. Crime may increase if drug use itself contributes to criminal behavior. Grogger and Willis (2000), using cross-city comparisons, find evidence of increased crime during the crack epidemic in the 1980s and 1990s. Luca et al. (2015) also find that prohibition of alcohol reduces violence against women. Despite the link between alcohol legalization and crime, previous researchers, including Resignato (2000), have found little evidence of a connection between marijuana use and criminal behavior. Increases in crime may also occur from legalization if marijuana arrests lead to incarceration of individuals prone to criminal activity. The evidence for this is indirect and mixed. Kuziemko and Levitt (2004) find a small correlation between increased incarceration and reduced crime, while Green and Winik (2010) find that incarceration has no effect on recidivism, using variation from random judge assignments. Legalization can increase local crime if dispensaries become targets for crime. An important characteristic of marijuana dispensaries is that they are primarily a "cash only" business. Since marijuana is illegal at the federal level, dispensaries are banned from accessing traditional banking services. Therefore, marijuana dispensaries could potentially have a differential impact on crime compared to other types of retail establishments.

Legalization will reduce crime if it disrupts illicit markets, which are susceptible to criminal activity. This is the conventional wisdom for explaining the increase in crime during alcohol prohibition in the 1920s. Owens (2014) indeed finds that prohibition of alcohol led to increases in organized crime, even if there were some benefits due to decreased alcohol consumption. Similarly, Bisschop et al. (2017), using evidence from legal prostitution zones in Dutch cities, find that legalization of prostitution leads to a reduction of crime. In addition, crime reductions may occur from legalization due to the shifting of resources away from marijuana enforcement toward other criminal activity. Crime within a neighborhood may decrease after a new dispensary is opened if the new dispensary increases private security or if

[11] Jacobi and Sovinsky (2016) estimate the effects of increased access on consumption using Australian data and predict that consumption could increase by nearly 50 percent under legalization. In the case of alcohol, Dills et al. (2005) find evidence of decreased consumption during the prohibition in the U.S. between 1920 and 1933.

[12] Van Ours and Williams (2009) find small negative effects of early cannabis use with educational obtainment. Van Ours and Williams (2012) use data from the Netherlands, and their "results suggest that cannabis use reduces the mental well-being of men and women and the physical well-being of men." Blake and Finlaw (2014) outline public policy concerns over drugged driving.

[13] The Institute of Medicine released a comprehensive report on the medical benefits of marijuana in 1999, which was summarized by Watson et al. (2000). The report found moderate evidence of some medicinal benefits, and encouraged increased research. Bachuber et al. (2014) find decreased opioid overdoes and mortality rates in states with medical marijuana.

[14] Anderson et al. (2013) show that marijuana legalization is associated with an 8 to 11 percent decrease in traffic fatalities resulting from a substitution effect between marijuana and alcohol. Using a discontinuity design from based on age restrictions on alcohol, Crost and Guerrero (2012) find that alcohol and marijuana use are substitutes.

[15] Research includes work by Kling (2006), who finds no effect on labor market outcomes using variation in prison sentence duration, and Charles and Luoh (2010), who find negative effects for women from male incarceration through general equilibrium effects in the marriage market.

[16] Data provided by the State of Colorado. "Marijuana Tax Data." Department of Revenue. Accessed February 9, 2017. https://www.colorado.gov/pacific/revenue/colorado-marijuana-tax-data.

police change their enforcement responses around dispensaries.[17]

The contradicting theories for how legalization will affect crime rates call for further research. Initial evidence on legalization suggests a reduction in crime.[18] Our research complements and builds on this literature while accounting for both the endogeneity of policy and the endogeneity of retail dispensary locations. In addition, we provide further insights into the mechanisms that lead to this reduction in crime.

3. Background and descriptive statistics

3.1. Legalization in the U.S.

Legalization of marijuana is becoming increasingly common, with many countries adopting varying decriminalization or legalization policies. In the United States, while marijuana is still technically illegal under federal law, the federal government largely defers to states with regard to local enforcement, particularly since 2009.[19] Legalization at the state level has since accelerated. While decriminalization of marijuana possession became common in the 1970s, California was the first state to formally legalize marijuana use for medical purposes in 1996 under California Proposition 215. The law allowed for the cultivation, manufacture, and sale of marijuana through retail dispensaries but required a medical reason to possess or use the drug. In subsequent years, other states followed suit by enacting medical marijuana laws in varying forms.

In 2012, Colorado and Washington became the first states to legalize marijuana for recreational use, with sales permitted to anyone over the age of 21 regardless of state of residence, and the first recreational dispensaries began appearing in Colorado in January 2014. The new laws also allowed for legal production of marijuana.

3.2. Legalization in Colorado

On November 7, 2000 Colorado residents voted for Amendment 20, which would allow patients with "debilitating medical conditions" and their caregivers to possess up to two ounces of marijuana and six cannabis plants.[20] To become a medical user, potential patients have to acquire a written diagnosis from a physician, register with the Colorado Department of Public Health and Environment (CDPHE), 2016a, 2016b, and pay an administrative fee of $110; if approved, patients received a registry ID card. In 2000, each caregiver could have at most five patients, creating an effective prohibition on commercial distribution of marijuana by caregivers. In 2006, decriminalization for the possession of marijuana (under an ounce) was extended to those over the age of 20 but left the sale, growing, and public use of marijuana illegal.[21]

In 2007, the Denver District Court struck down the five-to-one patient-to-caregiver ratio cap, leading to the creation of an informal medical marijuana market where medical marijuana stores could operate under the legal standing of "caregivers." By January 2010, there were over 250 businesses functioning as "dispensaries" under the role of a caregiver, and over 53,000 individuals held registry ID cards.[22]

In November 2012, Colorado voters passed Amendment 64 legalizing and regulating the growth, manufacturing, and sale of marijuana in a system of legal establishments as well as allowing individuals over the age of 20 to possess, use, display, purchase, transport, and transfer up to one ounce of marijuana and own up to six marijuana plants.[23] On January 1, 2014 retail marijuana stores were open to the public for nonmedical (recreational) use, and non-Colorado residents could purchase marijuana from legal businesses for the first time.

Amendment 64 gave substantial local control to cities and counties to regulate and/or prohibit the cultivation, production, and sale of marijuana.[24] Localities have been heterogeneous in their approach to local rule. Some localities allow only medical sales, while other localities created new zoning requirements, fees/special taxes, or fire codes for marijuana-related businesses.[25] Many localities delayed the licensing of recreational stores hoping to learn lessons from other localities, while other localities enacted "liberal" regulations early to benefit from excise taxes and business spillovers.

In January 2014, 18 municipalities had at least one recreational dispensary license and by December 2014, the number municipalities nearly doubled to 29, covering 42 percent of Colorado residents. Fig. 1 shows the growth of licenses in Denver (panel 1a) and Colorado (panel 1b). The growth of recreational licenses after 2014 was modest, rising to cover 32 municipalities and 47 percent of residents. By municipality, medical licenses are more prominent; 50 and 51 municipalities, respectively, had at least one license in 2014 and 2015. In Denver, most dispensaries offer both medical and recreational sales.

3.3. Dispensary locations

In this section, we provide a description of the geographic distribution of dispensaries and correlations between dispensary locations and neighborhood characteristics. A complete description of the data used here and in subsequent empirical results is found in section 5. The City of Denver is the clear mecca of recreational marijuana sales in Colorado. In January 2014, Denver had approximately 68 percent of 147 total recreational licenses despite only accounting for approximately 12 percent of the state's population. Fig. 1a shows that stores that sell recreational marijuana make up a larger share of all stores in Denver than in other counties and that co-location of medical and recreational stores is much more prevalent in Denver.

The contrast in dispensaries between Denver and its neighboring municipalities is particularly pronounced. Of the 18 municipalities/unincorporated areas that border Denver, only three municipalities (Wheat Ridge, Lakeside, and Edgewater) had at least one retail license by June 2014. Since the legalization of recreational marijuana, the per capita number of dispensaries in Denver has been at least three times higher than the per capita rate in neighboring municipalities. Fig. 2 shows the contrast between the per capita number of recreational stores in Denver compared with neighboring municipalities. The "+" indicates a store location (medical or recreational) that opened for the first time between 2014 and 2016. The result of heterogeneous local regulation is that individuals travel from "prohibition localities" to "nonprohibition localities" to purchase recreational marijuana, a pattern documented by

[17] Freisthler et al. (2013) show that security measures implemented by dispensaries may have had the effect of reducing crime in those neighborhoods.

[18] See research by Chang and Jacobson (2017); Freisthler et al. (2016); Huber et al. (2016); and Adda et al. (2014) mentioned previously.

[19] The U.S. Department of Justice issued a memorandum in 2009 that deprioritized enforcement of marijuana offenses that were in compliance with state laws.

[20] Colorado Constitution, Article XVIII S. 14.

[21] Colorado Legislative Council (2006).

[22] Rocky Mountain High Intensity Drug Trafficking Area (2013) and CDPHE(2016b).

[23] Colorado Constitution, Article XVIII S. 16.

[24] Specifically, Amendment 64 allowed localities to enact ordinances or regulations "governing the time, place, manner, and number of marijuana establishment operations" and allowed localities to "prohibit the operation of marijuana cultivation facilities, marijuana product manufacturing facilities, marijuana testing facilities, or retail marijuana stores through the enactment of an ordinance or through an initiated or reffed measure" (Colorado Constitution, Article XVIII S. 16).

[25] Aguilar (2014a).

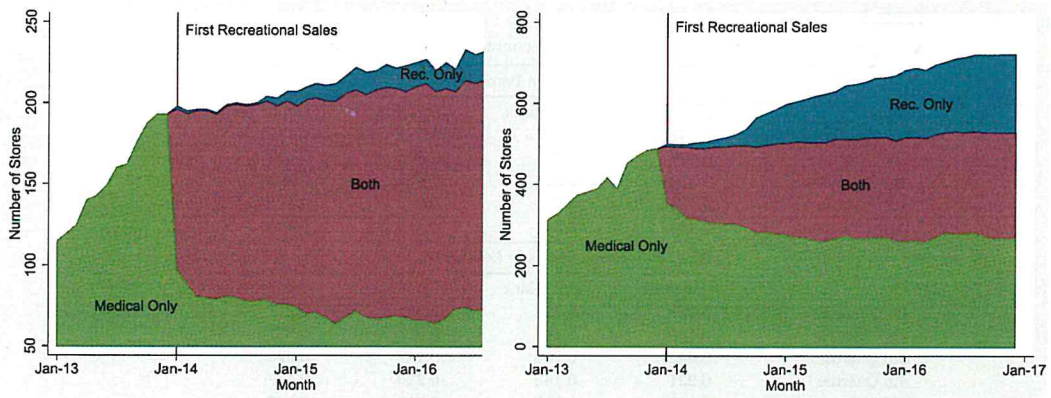


Fig. 1. Dispensary Growth 2013–2016. The panels of this figure show the total number of store fronts (calculated using data from Colorado Department of Revenue) cross tabbed by stores that sell only medical marijuana (green), both medical and recreational marijuana (red), and only recreational marijuana (blue). The vertical red line shows the first date (January 1, 2014) when recreational sales were legal. (For interpretation of the references to color in this figure legend, the reader is referred to the Web version of this article.)

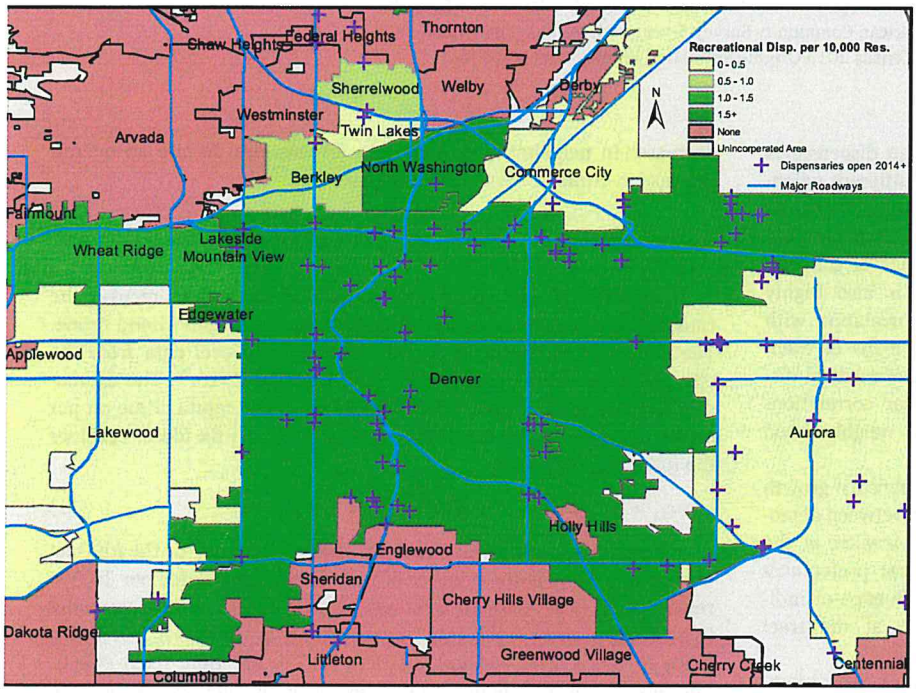


Fig. 2. Recreational Store Density and New Store Openings. This heat map shows the average number of recreational dispensaries per 10,000 residents (between 2014 and 2016) for municipalities in the Denver area. The "+" symbols indicate the opening of a store front after 2014 in a location where there was no store front in 2013. Major roadways are shown in blue. Data from the Colorado Department of Revenue. (For interpretation of the references to color in this figure legend, the reader is referred to the Web version of this article.)

business owners in the Denver area.[26]

Within Denver, the location of new dispensaries became highly regulated starting in 2014. During 2014, 2015, licenses could only be granted to owners of medical dispensaries in good standing, and owners of medical licenses could choose to colocate their medical and recreational stores or to open a recreational store in a new location. Under the bill, any new location of a dispensary (medical or recreational) must be at least 1,000 feet from any school, child care establishment, alcohol or drug treatment facility, or another marijuana dispensary. New location proposals also require public hearings where local stakeholders can object to the new license. If the director of the Denver Department of Excise and Licenses finds "evidence that the issuance of the license will adversely impact the health, welfare or public safety of the neighborhood" then she can reject the application for a license. Medical licenses for stores in new locations are subject to the same location restrictions and public hearings (Council Bill 13-0570, 2013). Given these requirements, it is likely that the locations of new stores would be correlated with neighborhood characteristics.

[26] An owner of a medical marijuana dispensary in Lakewood (a municipality on the western border of Denver) told the *Denver Post* that residents of Lakewood are driving to Denver to purchase marijuana (Aguilar, 2014a). An owner of a recreational store in a western suburb of Denver says that her location is better than locations in central Denver because "many of her customers hail from [other] western suburbs, such as Lakewood and Golden, where moratoriums on recreation pot shops are in effect, or Morrison, where the businesses are banned" (Aguilar, 2014b). One gas station in Denver partnered with a dispensary to provide discounted gas to drivers who purchase marijuana (McClure, 2016) and a restaurant owner located along Interstate-76 reported an increase in customers from people traveling between cities to purchase marijuana (Aguilar, 2016).

Table 1
Average change in dispensary density by local characteristics.

	Panel A: Across Tracts in Denver			
	Pov. Rate	Pct. Black	Pct. Hisp	Employment
1st Quartile	0.097	0.212	0.077	0.170
2nd Quartile	0.095	0.389	0.094	0.082
3rd Quartile	0.373	0.504	0.575	0.323
4th Quartile	0.823	0.270	0.541	0.811
Denver Average	0.344	0.344	0.344	0.344
	Panel B: Across Counties in Colorado			
	Pov. Rate	Pct. Black	Pct. Hisp	Employment
1st Quartile	0.080	0.112	0.081	0.217
2nd Quartile	0.142	0.113	0.071	0.031
3rd Quartile	0.042	0.106	0.142	0.076
4th Quartile	0.221	0.145	0.220	0.147
Colorado Average	0.119	0.119	0.119	0.119

This table shows the average 12-month change in dispensary density (between 2014 and 2016) cross-tabbed by local characteristics. Panel A shows dispensary growth for census tracts in Denver and panel B shows dispensary growth for counties in Colorado. In column two, we divide tracts/counties into four quartile bins based on their poverty rate (lowest to highest using populations weights) and show the average dispensary growth within each bin. We repeat this process in column three using percent of the population that is black, in column four using percent of the population that is Hispanic, and in column five using total employment. Dispensary data come from Colorado Department of Revenue, demographic data comes from the U.S. Census 2014 American Community Survey 5-year sample, and employment data comes from the U.S. Census 2013 Origin-Destination Employment Statistics.

Table 1 shows the average year-over-year changes in dispensaries (per 10,000 residents) cross-tabbed over quantiles for different neighborhood characteristics. The average growth in dispensaries per capita in the top quartile for poverty rate and for percentage Hispanic population is eight times and five times, respectively, that of the bottom quartile for each measure. Dispensary growth is also highly correlated with employment, but there is no clear correlation with the percentage of black residents. Fig. 3 shows heat maps of tract-level characteristics compared with the initial location of medical dispensaries in December 2013. These maps show similar correlations between store locations in 2013 and cross-sectional neighborhood characteristics.

The bottom portion of Table 1 shows average dispensary growth cross-tabbed across Colorado counties. The relationship between demographic factors and changes in dispensary density is weaker at the county level. This might be explained by the fact that preferences toward store locations and the social and political influence of individuals are more diluted at the county level than at the tract level.

The correlation between neighborhood characteristics and dispensary location has important policy implications in and of itself given that local effects of legalization will clearly have unequal consequences among different subpopulations. In addition, this presents a challenge for identifying the causal effect of legalization on neighborhood outcomes given that the locations of dispensaries are highly nonrandom. This suggests a quasi-experimental approach is necessary.

3.4. Crime trends

Fig. 4 shows a scatter plot of the number of crimes per 10,000 residents for Denver and all other Colorado counties between January 2011 and December 2014 (the lines show a 3-month moving average). Crime per 10,000 residents in Denver increased by 1.7 percent between 2013 and 2014 from 56.2 to 57.2 incidents a month, while crime per 10,000 residents in other counties decreased by 0.2 percent from 38.4 to 38.3 incidents a month. Crime rates follow seasonal patterns, are persistently higher in Denver, and show no major changes when retail marijuana sales started in January 2014. It is of interest that we find that crime decreases in neighborhoods that gain dispensaries despite an overall increase in crime in Denver after legalization.[27]

4. Methodology

In this section, we outline our estimation strategy to recover the causal effect of retail marijuana dispensaries on neighborhood crime. The data used in the analysis are monthly tract-level data from the city of Denver from January 2013 to December 2016.[28] The estimation strategy is based on a linear regression of per capita crime on per capita dispensaries for individual census tracts. To fix ideas, consider the following regression:

$$crime_{j,t} = \beta_0 + \beta_1 disp_{j,t} + \beta_2 \mathbf{X}_j t + \beta_3 \mathbf{X}_j + \delta_t + \epsilon_{j,t}, \quad (1)$$

where $crime_{j,t}$ is the crime rate per 10,000 residents for the jth tract in month t; $disp_{j,t}$ is the number of marijuana dispensaries per 10,000 residents; $\mathbf{X}_j$ is a vector of tract-level time-constant control variables (e.g., race, poverty level, retail employment, available land, and distance to the central business district); and δ_t are time fixed effects. The interaction of time-invariant measures with time, $\mathbf{X}_j t$, is included to allow for heterogeneous time trends with respect to time-invariant neighborhood characteristics.[29]

Our baseline specification uses a first-difference approach, comparing year-over-year changes for each month and tract. By using year-

[27] In order to compare Denver to the rest of the state, the data used here come from the Federal Bureau of Investigations' National Incident-Based Reporting System accessed through the National Archive of Criminal Justice Data (University of Michigan). These are different from the more detailed data used in the main analysis, which come from the city of Denver.

[28] More specifically, given that we are using year-over-year changes in the baseline model, the time period for the analysis starts with the year-over-year change from January 2013 to January 2014, the first month where recreational dispensaries are legal. The last year-over-year change in the sample is from December 2015 to December 2016. The data is described in more detail in section 5.

[29] One example might be the diverging long-run trends in suburban and urban crime rates in U.S. metropolitan areas noted by Kneebone and Raphael (2011).

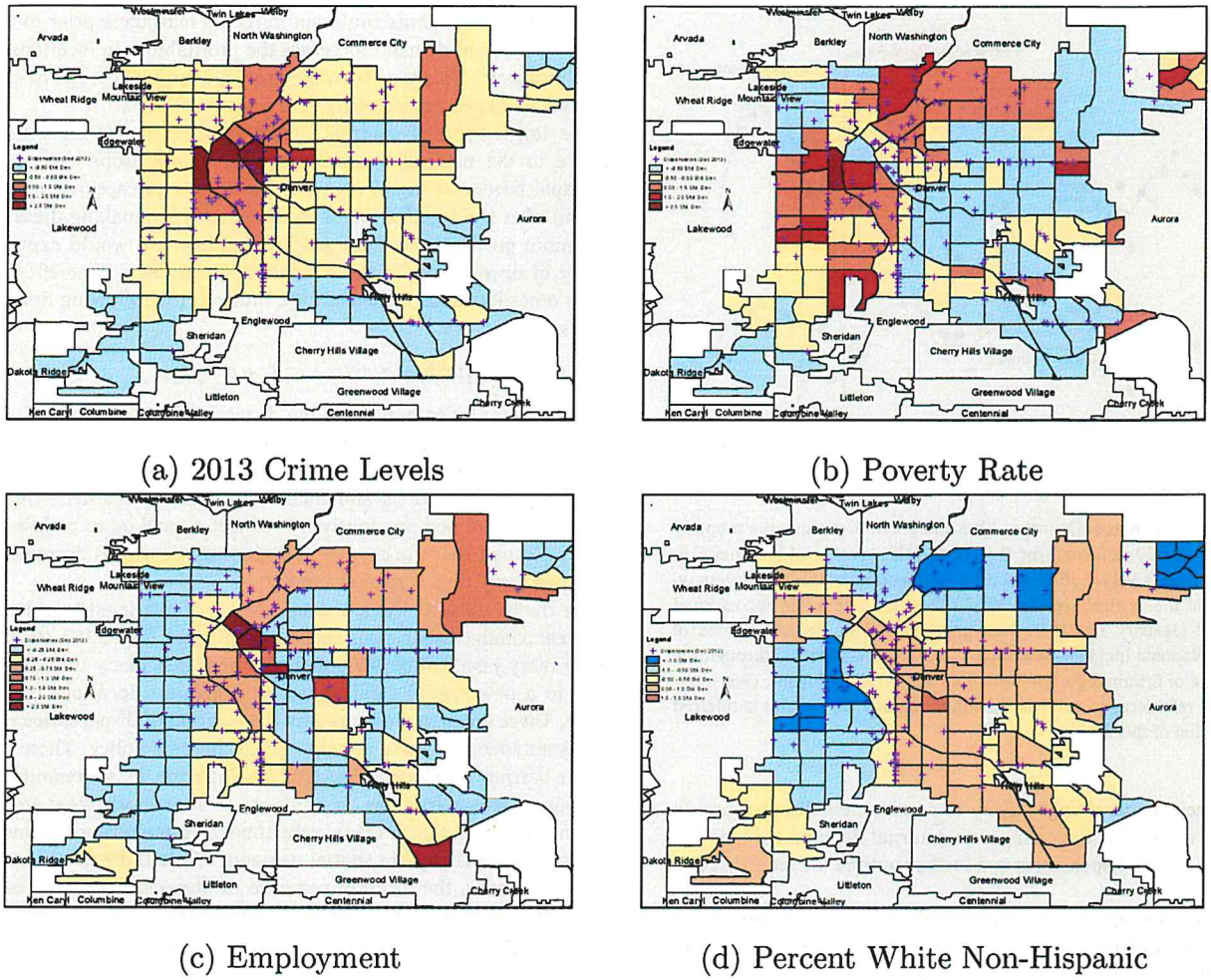


Fig. 3. Neighborhood Characteristics with 2013 Dispensaries. These heat maps compare neighborhood characteristics with dispensary locations before the legalization of recreational marijuana. 2013 medical dispensary locations are shown using "+" symbols. In panel (a), we show average crime rates in 2013; beige-colored tracts have crime rates within half a standard deviation of the average tract's crime rate, red tracts have a crime rate that is more than half a standard deviation above the average, and blue tracts show tracts that are below half a standard deviation of the average. The other panels show tract-level poverty rates (b), employment levels (c), and percent of the population that are white and non-Hispanic (d) (we use the same standard deviation color schemes in all panels). Dispensary data come from Colorado Department of Revenue, demographic data come from the Census' 2014 American Community Survey 5-year sample, and employment data comes from the Census' 2013 Origin-Destination Employment Statistics. (For interpretation of the references to color in this figure legend, the reader is referred to the Web version of this article.)

over-year changes in the regression, we are able to control for all time-invariant effects at the tract level. Furthermore, the use of 12-month changes addresses prominent seasonal patterns in crime data, which can be seen in Fig. 4. The baseline estimating equation is given by the following.

$$\Delta crime_{j,t} = \beta_0 + \beta_1 \Delta disp_{j,t} + \beta_2 \mathbf{X}_j + \Delta\delta_t + \epsilon_{j,t} \quad (2)$$

$\Delta crime_{j,t}$ and $\Delta disp_{j,t}$ represent year-over-year changes in crime rates and dispensary density respectively. Time fixed effects, $\Delta\delta_t$, account for aggregate crime shocks and ensure that identification arises from differences between tracks within the same time period as opposed to differences across time periods.[30]

In the absence of endogeneity concerns, β_1 would be interpreted as the average treatment effect of a per capita change in dispensaries on per capita crime. However, we suspect that the OLS estimate of β_1 is biased by unobserved tract characteristics that are correlated with changes in the dispensary density and changes in crime rates. A particular concern is that, after recreational dispensaries were legalized, all new locations were subject to public hearing. Therefore locations with less political power or social cohesion could be more likely to get new dispensaries and also have increasing crime rates. Another example of potential bias could arise from the fact that declining neighborhoods could have more retail space availability.

To address the potential bias in the OLS estimates, we exploit the fact that the legalization of recreational sales changed the market access of different locations. Specifically, sales to customers outside of Denver became a more important component of demand. Therefore, we propose using the smallest distance between a tract to the Denver municipal border and a tract to a major roadway as instrumental variables for changes in dispensary density. We then use these instruments in a standard two-stage least-squares approach.

After January 2014, demand for retail marijuana from non-Denver residents drastically increased, and tracts with more access to *external demand* showed larger changes in the number of dispensaries per capita. Indeed, over the sample period, locations near highways and munici-

[30] Note that as a robustness check, we also estimate a specification that uses a fixed-effects model based on equation (1) that yields similar results. The fixed effects equation allows for more flexible time effects. However, this is not the preferred specification, given that identification relies on monthly changes in crime that may be influenced by seasonal patterns.

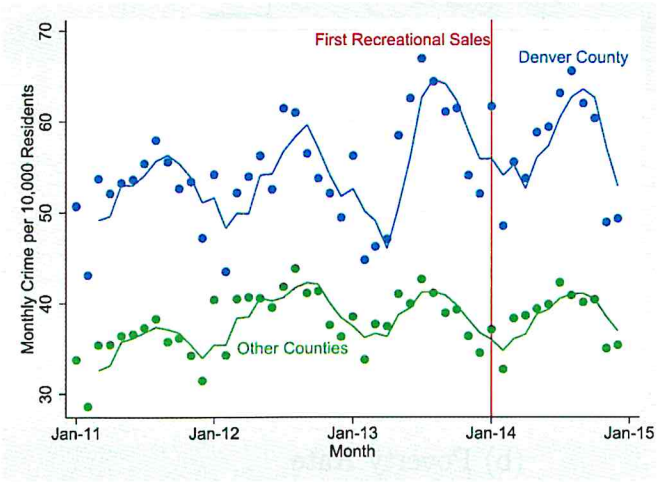


Fig. 4. Monthly Crime Across Counties. Blue and green dots show the monthly crime rates (per 10,000 residents) for Denver and the average of all other Colorado counties, respectively. Solid lines show corresponding three-month moving averages and the vertical red line shows the first date when recreational sales were legal (January 1, 2014). Data provided by the Federal Bureau of Investigations' National Incident-Based Reporting System accessed through the National Archive of Criminal Justice Data (University of Michigan). (For interpretation of the references to color in this figure legend, the reader is referred to the Web version of this article.)

pal borders became increasingly likely to receive new dispensaries. In what follows, we discuss the changes in external demand that lead to exogenous changes in dispensaries and further outline the identification argument.

The legalization of recreational marijuana in January 2014 increased the demand for retail marijuana across all Colorado locations. Between 2013 and 2014, total marijuana sales more than doubled from approximately $328 million in 2013 to over $700 million in 2014.[31] However, as documented in Section 3.3, Denver is the clear center for recreational marijuana sales in Colorado, and there is evidence that residents from neighboring municipalities travel to Denver to purchase retail marijuana.[32] This means that locations that have better access to neighboring municipalities are more profitable and that we would expect to see larger positive changes in dispensary densities near borders and major roadways.[33]

A second source of external demand comes from increased tourist demand. Using data submitted by dispensaries in March 2014, Light et al. (2014) estimated that 44.5 percent of purchases for recreational marijuana use were made by customers using an out-of-state ID. The majority of "out-of-state ID" purchases are made by tourists who travel along major roadways and stay in hotels located along these roadways. This would include customers traveling to and from the airport. Given that out-of-state residents could not purchase marijuana prior to 2014, this new source of demand increases the profitability in locations near major roadways. This tourist traffic provides further reasons to expect increases in dispensary densities near major roadways.

The importance of external demand in the recreational market relative to the medical market suggests that locations close to the municipal border or highways were better able to capture increasing demand after the legalization of recreational sales, making these locations more profitable for businesses. Therefore, we would expect the density of dispensaries in these locations to increase after 2014 relative to other locations in Denver. We thus run the following first-stage regression:

$$\Delta disp_{j,t} = \alpha_0 + \alpha_1 m2b_j + \alpha_2 m2r_j + \alpha_3 \mathbf{X}_j + \delta_t + \eta_{j,t}, \quad (3)$$

where $m2b_j$ (miles to border) is the distance of the centroid of a census tract to the nearest municipal border and $m2r_j$ (miles to road) is the shortest distance to a major roadway. We also include important observable characteristics and time fixed effects, consistent with the specification outlined previously. We expect α_1 and α_2 to both be negative, reflecting that the change in dispensary density is decreasing as you move away from a road or border.

For the baseline analysis, the sample is not restricted to only year-over-year changes that span the policy change (i.e. 2013 to 2014). The primary justification for doing so is that it is likely that the transition to a new spatial equilibrium for dispensary locations occurred slowly. There are many reasons why the location of dispensaries would not instantaneously move in response to the new policy. These could include barriers to relocation such as moving costs, lease commitments, or property search frictions. In addition, there is most likely an information friction, in that it would take time for some business owners to learn about the change in spatial demand. While there is a clean policy discontinuity, the location response of dispensaries would be less immediate. Therefore the change in dispensary density is correlated with the proximity to borders and highways throughout the sample period.[34]

This point is further illustrated in Fig. 5. The left panel shows the average distance of a dispensary to borders and the right panel shows the average distance to a highway over the sample period. The average distance of a dispensary to a border declines until the end of 2015, while the average distance to a highway declines until the end of the sample. Note that there is also a notable decline in the distance to a border before the policy went into effect, suggesting some location decisions occurred in anticipation of the law, which would be expected given that the law was passed in 2012.

We construct our IVs to exploit variation in the profitability of locations based on external demand, which we expect to be independent of unobserved neighborhood factors. The concerns regarding the endogeneity of the OLS regression come from unobserved neighborhood factors that affect the dispensary locations. These factors include local resistance at public hearings, availability of commercial space, and local demand for dispensaries. Our IVs are based on external demand and, therefore, are unlikely to be correlated with these unobservable characteristics. Nonetheless, there still may be some concern about the validity of the instruments. Next, we discuss and address some of these potential concerns.

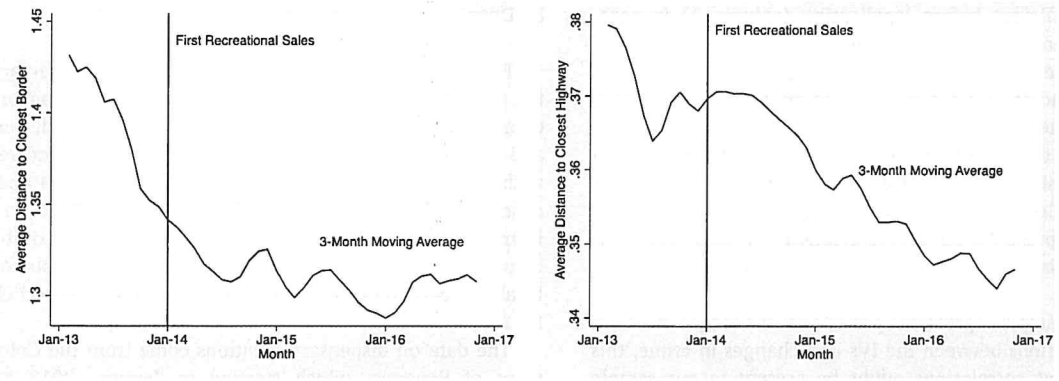
The general concern is that changes in crime are correlated with proximity to major roadways and borders. It is first useful to look at out-of-sample data to rule out persistent correlations between changes in crime and our instruments that might arise from broader trends.

[31] See Breathes (2014) and Ingraham (2015). Potential causes for the increase in demand include (1) individuals without a disabling condition being able to legally purchase marijuana, (2) legalization removing fixed costs associated with acquiring a registry ID (time, money, or perceived risk of being in a government data base of known marijuana users), (3) increased publicity and advertising, (4) changes in cultural norms making marijuana consumption more permissible, and (5) perceived decrease in risk of purchasing or consuming marijuana.

[32] In addition, recreational dispensaries were much more concentrated in Denver relative to medical dispensaries, as was seen previously in Fig. 1, making external demand even more relevant after the legalization of recreational sales.

[33] The idea that retail establishments locate to maximize demand based on the geographic distribution of customers (and competition) is related to the classic work by Hotelling (1929). Work by Davis (2006) represents a more recent application of this theory.

[34] Two additional specifications are presented in Section 6 as robustness checks that assume adjustment to the policy was immediate. The first specification uses only data from 2013 to 2014. The second uses all data and constructs instruments by interacting a post-policy dummy with a dummy for proximity to a highway or border.



(a) Distance to Border (b) Distance to Highway

Fig. 5. Change in dispensary locations over time. This figure shows the change in the average distance in miles to a municipal border (left) and to a highway (right) over the sample period.

Table 2
12-Month changes in crime and neighborhood characteristics before and after legalization.

	(1) Before	(2) After
Miles to Border	−0.698	1.572
	(1.200)	(3.669)
Miles to Major Road	1.378	7.507**
	(1.952)	(2.920)
Poverty Rate	−19.984**	81.615***
	(8.883)	(18.507)
Employment (1,000s)	0.022	2.042**
	(0.233)	(0.682)
Pct. White Non-Hispanic	−8.119*	−1.052
	(4.051)	(5.002)
Miles to CBD	−0.723	−1.487**
	(0.440)	(0.540)
Useable Land/10,000 Res.	3.541*	9.906***
	(1.941)	(2.716)
N	1704	1704

In the "before" column, we regress the 12-month change in crime rates during 2012 on miles to border, miles to a major roadway, and our control variables (including time fixed effects). In the "after" column, we repeat the exercise using crime data from 2014. Heteroskedastic robust standard errors are shown in parentheses, and significance is denoted by * = 0.10, ** = 0.5, and *** = 0.01. Crime data provided by the City of Denver in an October 2016 vintage. "Miles to Major Road" is the distance in miles to a major road; "Miles to Border" is the distance in miles to the nearest municipal border; "poverty rate" is the poverty rate as a percentage; "Employment" is total employment in the tract in thousands; "Pct. White" is the percentage of white/non-hispanic population in each tract; "Miles to CBD" is the distance to the central business district of Denver in miles; and "Useable Land" is the the total amount of land per 10,000 residents that is not restricted for use as a dispensary.

To check for this persistence, we compare reduced-form regressions before and after legalization. Specifically, we regress annual changes in tract-level crime on our vector of control variables $\mathbf{X}_{j,t}$, a tract's distance to a major road ($m2r_j$), and its distance to a municipal border ($m2b_j$) and include time fixed effects (δ_t). We run this regression using annual changes in crime from before legalization of recreational marijuana in 2012 and compare it to annual changes after legalization in 2014.

The results shown in Table 2 serve two purposes.[35] First, they provide a reduced from test of the correlation of the IVs to crime in the post period. The coefficients on miles to border and major roadways in the "after" regression (column 2) are positive, which is consistent with our main findings presented subsequently.[36] The estimate is statistically significant for the distance to a major road.

In addition, the results in Table 2 provide a placebo test by showing that the same correlation is not present in the pre-period. The

[35] Denver crime data from 2011 are not available for vintages published after 2016. Since this table requires data from 2011, we use a vintage of data downloaded in 2016 (other results use a vintage from 2017).

[36] We find that changes in crime are lower where there are positive changes in dispensary densities and that positive changes in dispensary densities occur closer to the Denver border and major roadways. Therefore, we would expect crime to be higher in locations that are further from the border or from major roadways, consistent with positive coefficients on miles to border and miles to major roadways.

miles to border coefficient before legalization (column 1) is negative. This suggests that the positive relationship between distance to the border and crime is unique to 2014, as opposed to some persistent unobserved factor in locations. The miles to roadway coefficient is positive in the "before" regression, however it is statistically insignificant and less than one-fourth the magnitude of the coefficient in the "after" regression. This suggests that the strong relationship between distance to the major roadway and changes in crime is a feature that is uniquely present after the legalization of recreational marijuana and not a persistent characteristic of neighborhoods near major roadways.

While the out-of-sample regressions provide evidence that there are not persistent correlations between the IVs and changes in crime, this does not rule out that correlations might be present in our sample period from one-time shocks occurring at the same time as legalization. Note that the coefficients on other variables also change significantly between the two time periods. These results suggest that correlations between year-over-year changes in crime and static variables may not be stable over time. Ideally, analyzing a longer period before the policy was instituted would allow for closer examination of these correlations. However, we are limited by the fact that the crime data are not available before 2011.

Given these concerns, we also control for a number of other factors. One concern is that there could be spatial correlations that arise from the particular geography of Denver.[37] To account for the particular city structure, we control for distance to the central business district as well as initial employment density. In addition, we include a specification with police precinct fixed effects which allows for identification through variation within different regions of the city rather than across regions. If there are institutional differences between districts, then they would be accounted for in this specification.[38]

Another potential confounding factor is that the location of roads and borders could be endogenous. While it is unlikely that road construction responded to the legalization of marijuana in the short term, there may be concerns that there were effects on annexation or secession of particular neighborhoods. However, records from the City and County of Denver show that there have been no significant changes to the borders of Denver since the land for the airport was annexed in 1988.[39]

To address more general endogeneity concerns, we run regressions for each IV in isolation and find similar results. This provides particularly strong evidence for the validity of the results, given that the IVs are orthogonal both statistically and in geographic space.[40] This orthogonality arises from the fact that major roads primarily radiate from the center of the city, while the border surrounds the city, meeting the roads at right angles. Therefore it is unlikely that the same source of unobserved heterogeneity is correlated with both IVs and changes in crime rates and is idiosyncratic to the sample period, which lends additional support to the validity of the instruments.

Lastly, given that we have two instruments, we are also able to perform an overidentification test. The results of the test provide further statistical evidence of instrument exogeneity in our baseline specification.

[37] More specifically, a problem could arise from the fact that borders are far from the central business district. Therefore, crime trends could be correlated with distance to the CBD (e.g. due to gentrification). Controlling for distance to the CBD allows identification through comparison of neighborhoods at similar distances from the CBD, but with different proximity to borders.

[38] Time constant effects of geography on crime are controlled through first differencing.

[39] These data were accessed at www.denvergov.org/opendata/dataset/city-and-county-of-denver-annexations in March 2019.

[40] The correlation of the two IVs is.032 and not statistically significant.

5. Data

For the empirical analysis, we require local time-varying data on the location of dispensaries and detailed information on crime. This data is used to construct year-over-year changes in dispensary density and crime rates at the census tract level for the city of Denver starting with changes between January 2013 to January 2014 and ending with changes between December 2015 and December 2016. In addition, the instrumental variable identification strategy, outlined above, requires data on major roads and municipal borders. We also collect data on local demographics, land use, and other neighborhood characteristics for use as controls.

The data on dispensary locations come from the Colorado Department of Revenue, which starting in January 2013 has published a monthly list of each active medical and recreational license.[41] We geocode the address associated with each license and use an algorithm to identify medical and recreational stores that are colocated.[42] This is aggregated to create a monthly panel of the number of store fronts within each census tract in Denver to be used in our primary empirical analysis.[43] Overall, our sample covers 48 months between 2013 and 2016 for 143 census tracts in the City of Denver.

We obtain data on crime from several sources. The local crime data for our analysis of Denver come from the City and County of Denver, which maintains an extensive online data catalog.[44] The data include the specific time and location of each incident, which we aggregate to the census tract level at a monthly frequency. The data also contain detailed information covering over 190 different crime categories, which allows us to use various levels of aggregation to get a better picture of the precise changes in neighborhood crime.[45] Each incident in the data may contain multiple charges. Therefore, we use the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting Program hierarchy to order the charges related to a single incident and assign the incident to the crime category of the highest crime.[46] In addition to the local crime data, we collect data at the county level for the entire

[41] The data were accessed at www.colorado.gov/pacific/enforcement/archived-med-medical-and-retail-marijuana-licensee-lists in January 2017.

[42] Geospatial data were processed primarily using ESRI ArcGIS. Geocoding was completed using the Census Geocoder accessed August 16, 2016 at www.census.gov/geo/maps-data/data/geocoder.html and supplemented with Google Maps at maps.google.com. Our algorithm utilizes a customized cross-walk that identifies licenses assigned to the same address (e.g. matches 250 W. Colfax to 250 West Colfax) and identifies the co-location of medical and recreational based on the name of the business holding the licenses (since recreational licensees were granted only to the license holders of medical license). We then manually update 11 instances where the algorithm cannot identify colocation.

[43] We exclude the tract that contains the Denver International Airport which is spatially disconnected from the rest of Denver.

[44] The crime data were accessed at www.denvergov.org/opendata/dataset/city-and-county-of-denver-crime in February 2017. The catalog contains detailed geographic information including data on municipal government, health, safety, and education for Denver. Much of the data is available in GIS compatible formats.

[45] One challenge in using this data is a change in data reporting starting in May 2013. At this time, Denver began using a "Unified Summons and Complaint Process" that changed the frequency of reporting for certain crime categories. In our analysis, we use time fixed effects to account for this change and conduct tests to that show that the change in data reporting does not bias our results (see Appendix A for details).

[46] For instance, if someone breaks into a store, steals money from the cash register, and kills the clerk with a gun, then the incident would be categorized as murder and not burglary or theft. The hierarchy comes from the U.S. Department of Justice. "Summary Reporting System User Manual." June 20, 2016, V1.0.

Table 3
First-stage regression results.

	μ [σ]	(1) m2b only	(2) m2r only	(3) all IV
Miles to Major Road	1.07 [0.84]		−0.269***	−0.182***
			(0.065)	(0.054)
Miles to Border	0.18 [0.12]	−0.171***		−0.138***
		(0.043)		(0.039)
Poverty Rate	2.97 [6.55]	3.273***	3.158***	3.114***
		(0.573)	(0.597)	(0.603)
Employment (1,000s)	0.56 [0.26]	0.021***	0.018***	0.021***
		(0.004)	(0.003)	(0.004)
Pct. White Non-Hispanic	4.36 [2.71]	−0.019	−0.093	−0.081
		(0.146)	(0.162)	(0.161)
Miles to CBD	0.64 [0.89]	−0.062***	−0.016**	−0.049***
		(0.015)	(0.007)	(0.012)
Useable Land/10,000 Res.	0.49 [0.34]	0.271***	0.261***	0.261***
		(0.033)	(0.034)	(0.034)
N		5112	5112	5112
F-Statistic		56.311	62.191	56.082
R^2		0.085	0.084	0.086

This table shows first-stage results for the 12-month change in store density (per 10,000 residents) using time fixed effects. The second column shows the mean and standard deviation for each covariate (between 2014 and 2016). Regression (1) only uses miles to border as an IV, regression (2) only uses miles to roads as an IV, and regression (3) uses both IVs. Heteroskedastic robust standard errors are shown in parentheses, and significance is denoted by * = 0.10, ** = 0.5, and *** = 0.01. "Miles to Major Road" is the distance in miles to a major road; "Miles to Border" is the distance in miles to the nearest municipal border; "poverty rate" is the poverty rate as a percentage; "Employment" is total employment in the tract in thousands; "Pct. White" is the percentage of white/non-hispanic population in each tract; "Miles to CBD" is the distance to the central business district of Denver in miles; and "Useable Land" is the the total amount of land per 10,000 residents that is not restricted for use as a dispensary.

state. For this, we use the FBI Uniform Crime Reporting Program data for cross-county comparisons.[47]

The data for our instrumental variables are both taken from the U.S. Census TIGER database.[48] For the borders, we use the border of Denver (excluding the airport) from the TIGER "Boundary" file, and for the major roads in Denver, we use the "Major Roadways" file. We then calculate distances as the distance from the geographic centroid of the census tract to the nearest border/roadway.

We use the 2014 American Community Survey (ACS) 5-year sample data, which provide information on race, ethnicity, income, and poverty to control for demographic factors.[49] These data are also used to calculate crime and dispensary density per 10,000 residents using the ACS total population counts. In addition, we collect employment data by industry from 2013 U.S. Census Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics.

We also need to control for existing land uses and restrictions, as these are important determinants of dispensary locations. Importantly, with the start of legalization, the City of Denver restricted new dispensaries from being located 1,000 feet from existing dispensaries, child care facilities, schools, and drug and alcohol treatment facilities, for which we collect data from several sources.[50] We collect geographic data zoning designations and bodies of water available through the City and County of Denver's Community Planning and Development Department. We then combine these data to develop a measure of useable area for dispensaries in each location.[51] Finally, we calculate the distance of each tract to the central business district and collect data on police districts as additional controls.

6. Results

In this section, we outline our main results. We start by presenting the results of the first-stage regressions, which are summarized in Table 3.[52,53] Columns (1) and (2) show the first-stage results using only miles to border or miles to major road as an instrumental variable. Column (3) shows results using both instruments. As expected, the coefficients on miles to border and miles to road are negative and statistically significant in each regression, which implies that locations near highways and borders saw a larger increase in dispensaries after 2014 than locations further away. For example, the coefficient in the first row, column (3) implies that the year-over-year change in dispensaries per 10,000 residents is 0.182 higher for locations next to a major road than for locations a mile away. Other location characteristics are also significant and consistent across all specifications. Dispensary densities after 2014 increased more in neighborhoods with higher poverty rates, with higher levels of employment, that are closer to the central business district, and where there is more useable land.

[47] Source: U.S. Department of Justice. Federal Bureau of Investigation. Uniform Crime Reporting Program Data: National Incident-Based Reporting System, 2014. ICPSR36398-v1. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2016-03-21. doi.org/10.3886/ICPSR36398.v1.

[48] U.S. Department of Commerce, Bureau of the Census, Geography Division. www.census.gov/geo/maps-data/data/tiger-line.html.

[49] We access data through the Minnesota Population Center. National Historical Geographic Information System: Version 11.0 [Database]. Minneapolis: University of Minnesota. 2016.

[50] The locations of childcare facilities were obtained from the license data available from the City and County of Denver, while school locations were obtained from a digitized map that was then georeferenced. Drug and alcohol treatment facility locations were obtained through a Google search.

[51] Specifically, we take the total land area of each tract and subtract locations within 1000 feet of restricted facilities, bodies of water, and locations that are restricted through zoning (e.g., residentially zoned locations). This area is then normalized by tract population to be consistent with the normalization of crime and dispensaries, which are per capita measures.

[52] Summary statistics for each variable are included in the second column.

[53] To check for serial correlation, we followed the test suggested by Wooldridge (2010). The F-statistic of the test was 1.771, with a p-value of 0.19, which provides evidence that serial correlation is not a large concern.

Table 4
Second-stage regression results.

	(1) OLS	(2) m2b - IV	(3) m2r - IV	(4) ALL IV
12-M Chg Store Den.	1.924***	−20.575**	−10.980*	−17.009**
	(0.459)	(8.742)	(6.617)	(7.035)
Poverty Rate	14.427	91.791**	58.798**	79.530**
	(13.247)	(37.686)	(29.991)	(32.448)
Employment (1,000s)	0.403	0.794*	0.627	0.732*
	(0.370)	(0.408)	(0.406)	(0.402)
Pct. White Non-Hispanic	0.579	0.689	0.642	0.671
	(3.799)	(5.881)	(4.880)	(5.472)
Miles to CBD	−1.302**	−1.848***	−1.615***	−1.762***
	(0.579)	(0.693)	(0.627)	(0.659)
Useable Land/10,000 Res.	5.390**	11.655***	8.983**	10.662***
	(2.225)	(4.011)	(3.558)	(3.688)
N	5112	5112	5112	5112
Weak IV Test Statatistic		17.560	13.393	10.415
Hansen J Statistic				1.468
R^2	0.034			

This table shows the second-stage results for the 12-month change in crime rate (per 10,000 residents) using time fixed effects. For comparison, we include OLS regression results in regression (1). In regression (2), we use only the miles to border IV, in regression (3) we use just the miles to roads IV, and in regression (4), we use both variables as IVs. Heteroskedastic robust standard errors are shown in parentheses and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. The bottom rows show the Kleibergen-Paap Wald F statistic for weak IV, as well as the results of the Hansen J overidentification test. "Miles to Major Road" is the distance in miles to a major road; "Miles to Border" is the distance in miles to the nearest municipal border; "poverty rate" is the poverty rate as a percentage; "Employment" is total employment in the tract in thousands; "Pct. White" is the percentage of white/non-hispanic population in each tract; "Miles to CBD" is the distance to the central business district of Denver in miles; and "Useable Land" is the the total amount of land per 10,000 residents that is not restricted for use as a dispensary.

Table 4 shows the primary second-stage results for several specifications. The table first shows the OLS regression (column 1) results, followed by IV regression results using only the miles to border IV (column 2), only the miles to major roadway IV (column 3), and both IVs together (column 4). At the bottom of the table, we provide test statistics for first-stage weak IVs and the results of an overidentification test for the specification using both IVs.[54]

The coefficient on the 12-month change in store density is significant and robust across all IV regressions (columns 2–4). Consider the estimated effect of the change in store density on crime using the specification with both IVs (column 4). The negative and significant coefficient means that, on average, census tracts that gained a dispensary (per 10,0000 residents) show a decrease of 17 crimes per month (per 10,000 residents) compared with a neighborhood that had no change in its dispensary density, ceteris paribus. The magnitude of these coefficients are substantial; the average number of crimes across our observation period is 90 crimes per 10,000 residents. When we use both IVs, we get a coefficient of 17, which is equivalent to a 19 percent decrease in crime over our observation period, for an increase of 1 store per 10,000 residents.

Our result is comparable to that of two recent studies on the effects of legalization on neighborhood crime. The most similar study is by Chang and Jacobson (2017) who look at unexpected medical dispensary closings in Los Angeles. The authors find that when a dispensary closed, crime increased by as much as 24 percent in the immediate neighborhood. In addition, Adda et al. (2014) also find that decriminalization in a borough of London resulted in a 9 percent reduction in crime.

Other control variables show similar relationships across the OLS and IV specifications. The positive coefficient on poverty rates shows that crime increased in areas with higher poverty rates. We also see a negative coefficient on distance to the central business district(CBD) showing that crime increased faster near the center of the city compared with those locations further from the city center. The coefficient on per capita useable land is positive, indicating that tracts with more useable land saw increases in their crime rate.[55]

It is of note that the OLS results for the effect of a change in dispensary density are substantially different than IV results. In the OLS results, we observe a small positive relationship between changes in store density and changes in crime. This supports concerns that changes in dispensary densities are correlated with unobserved neighborhood characteristics associated with increasing crime during the sample period. This result is consistent with the idea that dispensaries potentially selected into neighborhoods with a lack of social cohesion or political power and increasing crime rates that were unable to prevent the opening of new dispensaries through the public hearing process. The result is in an upward bias in the OLS estimates.

Table 5 shows a number of results that check the robustness of the econometric specification and functional forms of the baseline regression (shown in column (1) for reference). In all of the specifications, the estimated coefficient on the change in store density is negative, and it is statistically significant for all but one specification. Column (2) shows the results from a regression run in levels instead of year-over-year changes that includes tract-level fixed effects. Effectively, this estimates Eq. (1) with tract fixed effects.

In our baseline regression, we assume that changes in dispensary locations are correlated with our instruments throughout the sample period. The next two columns, (3) and (4), show the results of specifications that assume a clean treatment across the policy introduction in January 2014. The results in column (3) assume that the proximity of highways and borders influence the location of dispensaries only after the policy is enacted. Therefore, we use a post-policy dummy interacted with the proximity measures as instruments for dispensary density and run a fixed-effect model. This specification assumes that there a clean pre/post policy measure but still uses all of the data available. The estimate of the crime effect is smaller in this case, but still significant. In regression (4), we use year-over-year changes, as in the baseline specification, but only those that span the policy implementation in January 2014 (i.e. only 2013 and 2014 data). The estimate is still negative, but

[54] For the preferred specification the overidentification test provides evidence that the IVs are exogenous. See Angrist and Pischke (2008) and Parente and Silva (2011) for a discussion of the interpretation of overidentification tests.

[55] Large values of useable land are negatively correlated with the number of schools and child care programs in a neighborhood, which may help to suppress crime rates.

Table 5
Robustness to specifications.

	(1) Base	(2) Tract FE	(3) Post Dummy	(4) 13/14 Only	(5) dummy IVs	(6) w/quad
12-M Chg Store Den.	−17.009**	−18.990***	−13.926***	−4.717	−16.445***	−10.087**
	(7.035)	(4.947)	(5.067)	(4.074)	(5.704)	(4.619)
Poverty Rate	79.530**	6.764***	5.351***	63.016**	77.592***	60.033***
	(32.448)	(1.890)	(1.796)	(24.784)	(22.269)	(21.097)
Employment (1,000s)	0.732*	0.065***	0.058**	2.138***	0.722*	0.474
	(0.402)	(0.024)	(0.025)	(0.700)	(0.389)	(0.401)
Pct. White Non-Hispanic	0.671	−0.023	−0.007	−9.013	0.669	3.374
	(5.472)	(0.310)	(0.280)	(6.935)	(5.401)	(4.824)
Miles to CBD	−1.762***	−0.179***	−0.168***	−1.569**	−1.748***	−4.993***
	(0.659)	(0.031)	(0.030)	(0.760)	(0.668)	(1.466)
Useable Land/10,000 Res.	10.662***	1.004***	0.891***	7.056**	10.505***	8.913***
	(3.688)	(0.175)	(0.170)	(2.814)	(3.515)	(2.944)
N	5112	6816	6816	1704	5112	5112
Weak IV Test Statatistic	10.415	19.130	14.308	15.489	28.536	8.924

This table shows robustness checks of our main results (shown in regression (1)) for the 12-month change in crime rate (per 10,000 residents). Regression (2) shows the results using tract fixed effects rather than year-over-year changes. Regression (3) uses a post-policy dummy interacted with proximity to roads and borders as an instrument for dispensary density. Regression (4) show the results only using data from 2013 to 2014. Regression (5) uses dummy variables for the IVs, specifically an IV that equals one if a neighborhood is more than two miles from a border (and zero otherwise) and an IV that equals one if a neighborhood is than half a mile from a major roadway (and zero otherwise). Regression (6) includes a quadratic term in the distance to the highway and border in the first stage. All regressions use time fixed effects. Heteroskedastic robust standard errors are shown in parentheses, and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. The bottom row shows the Kleibergen-Paap Wald F statistic for weak IV.

not statistically significant. The weaker result could reflect the smaller sample size and the fact that the migration of dispensaries persisted after 2014.

The last two columns show results using different functional forms for the instruments, given that it is not obvious that the change in external demand is a simple linear function of distance. In regression (5) we use dummy variables for our IVs, specifically an IV that equals one if a neighborhood is more than two miles from a border (and zero otherwise) and an IV that equals one if a neighborhood is than half a mile from a major roadway (and zero otherwise). Column (6) shows the results using an additional quadratic term in distance for the instruments. In both cases, the coefficient is negative and significant.

Table 6 shows additional robustness checks including the use of different controls. Again, the baseline results are shown in Column (1). Column (2) shows the baseline regression with the inclusion of statistics for the number of schools, rehab centers, and child care licenses in each tract (per 10,000 residents). This specification is a check to ensure the useable land is not being used as a proxy measure for the density of schools, rehab centers, and child care licenses in each tract. In column (3), different control variables are used (median income replaces poverty rate, retail employment replaces employment, and percent black and percent Hispanic jointly replace percent white non-Hispanic). Column (4) excludes store locations that only sell recreational marijuana, controlling for the potential that store fronts without medical sales are different than those that include medical sales.[56] In column (5), we use police district fixed effects (crossed with monthly fixed effects).[57] Using district fixed effects controls for differences in police behavior across districts and controls for idiosyncratic differences across districts. This specification is of interest given the way that police districts are constructed (the central business district along with the surrounding area is its own district, and suburbs with similar characteristics are grouped into districts). In this case, the coefficient is similar, but statistical significance is lost. Finally, in column (6) we use population weights, to ensure that the results are not driven by variation in low-population tracts.

Lastly, a potential concern is that errors may be spatially correlated. Recently, a method for dealing with this issue is to estimate standard errors using a spatial HAC correction, as proposed by Conley (1999). However, this methods are not well developed for panel IV settings.[58] As an alternative, we present results in Appendix D where we cluster the standard errors at the police district level. The estimated standard errors remain similar in this case.

7. Discussion

We find that the overall effect of adding a dispensary to a neighborhood of 10,000 residents is a reduction of crime of around 17 crimes per month. In this section, we further analyze and decompose the data in order to provide a better sense of the underlying mechanisms that lead to crime reduction and to compare these findings with existing theories about the effect of legalization on crime. To do so, we first use the detailed nature of the crime data to look at how dispensaries affect different types of crime. Then we consider the geographic extent of these effects by looking at census tracts adjacent to neighborhoods that received dispensaries. The evidence presented here provides indirect evidence to support existing theories about the relationship between crime and legalization, and could inform future in-depth research.

7.1. Effects by crime type

While there is significant variation across crime categories in the response to legalization, some broad patterns emerge. In order to examine the effects on different types of crimes, we split the data into broad based categories defined by FBI's Uniform Crime Reporting Program.[59] Table 7 shows results using different subsets of crimes as the dependent variable (still using both IVs and time fixed effects). In columns (2) and (3), we divide the universe of crimes into violent and nonvi-

[56] Examining stores that only sell medical marijuana is not an appropriate robustness check, since most medical-only stores in 2013 converted to selling both types in 2014, and medical-only stores would be less sensitive to external demand. Examining stores that only sell recreational marijuana is not possible, since there were no recreational dispensaries in 2013.

[57] See Appendix C for a map of police districts.

[58] For a discussion, see Konig et al. (2017).

[59] A complete description of crimes, categories, and frequency of incidents during the sample period is provided in Appendix E.

Table 6
Additional robustness checks.

	(1) Base	(2) w/locations	(3) Diff. Contr.	(4) Excl. Rec.	(5) Police FE *	(6) Pop Weight
12-M Chg Store Den.	−17.009**	−17.286**	−11.487**	−35.555**	−13.857	−13.301**
	(7.035)	(7.011)	(4.837)	(14.702)	(10.242)	(6.522)
Poverty Rate	79.530**	63.947**		38.413**	62.620*	53.979***
	(32.448)	(27.602)		(16.967)	(36.316)	(18.226)
Employment (1,000s)	0.732*	0.604		0.571	0.656	0.788**
	(0.402)	(0.407)		(0.400)	(0.472)	(0.379)
Pct. White Non-Hispanic	0.671	−8.326		−6.347	−5.323	−3.311
	(5.472)	(5.862)		(5.366)	(8.773)	(3.556)
Miles to CBD	−1.762***	−1.984***	−2.894***	−1.864***	−1.554*	−1.167**
	(0.659)	(0.757)	(0.768)	(0.681)	(0.801)	(0.461)
Useable Land/10,000 Res.	10.662***	9.407***	10.021***	9.189***	9.939**	7.213***
	(3.688)	(3.386)	(3.337)	(3.209)	(4.496)	(2.627)
Schools/10,000 Res		−2.120***				
		(0.680)				
Rehab Centers/10,000 Res		4.060***				
		(1.457)				
Child Care License/10,000 Res		0.509***				
		(0.166)				
Median Income			−0.171***			
			(0.047)			
Retail Employment (1,000s)			0.004**			
			(0.002)			
Percent Black			22.431***			
			(7.648)			
Percent Hispanic			3.668			
			(3.251)			
N	5112	5112	5112	5112	5112	5112
Weak IV Test Statatistic	10.415	11.310	22.858	14.675	4.353	8.506

This table shows additional robustness checks of our main results (shown in regression (1)) for the 12-month change in crime rate (per 10,000 residents). Regression (2) includes controls for the density of schools, rehab centers, and child care licenses (per 10,000 residents) in each tract. Regression (3) substitutes control variables; specifically we substitute median income for poverty rate, retail employment for employment, and percent black and percent Hispanic for percent white non-Hispanic. Regression (4) shows results for a fixed effects model. Regression (4) only uses the store locations of medical marijuana, and regression (5) uses police district and time fixed effects. Regression (6) uses population weights. All regressions use time fixed effects. Heteroskedastic robust standard errors are shown in parentheses, and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. The bottom row shows the Kleibergen-Paap Wald F statistic for weak IV.

Table 7
IV results by crime category.

	(1) all	(2) violent	(3) nonviolent	(4) persons	(5) property	(6) society	(7) other
12-M Chg Store Den.	−17.009**	−1.125	−15.884**	−2.834**	−0.421	−2.845*	−8.440**
	(7.035)	(0.806)	(6.814)	(1.266)	(2.852)	(1.673)	(3.694)
Poverty Rate	79.530**	2.776	76.753**	9.215	13.733	11.364*	33.793**
	(32.448)	(3.532)	(31.352)	(6.252)	(11.326)	(6.835)	(16.260)
Employment (1,000s)	0.732*	0.070**	0.662*	0.208***	0.041	0.141	0.284
	(0.402)	(0.031)	(0.394)	(0.042)	(0.130)	(0.121)	(0.203)
Pct. White Non-Hispanic	0.671	−0.387	1.058	−0.456	2.564	−0.023	1.350
	(5.472)	(0.537)	(5.309)	(0.973)	(2.013)	(1.121)	(2.640)
Miles to CBD	−1.762***	−0.086	−1.676***	−0.224**	0.087	−0.308**	−0.779**
	(0.659)	(0.057)	(0.649)	(0.090)	(0.314)	(0.155)	(0.340)
Useable Land/10,000 Res.	10.662***	0.409	10.253***	0.912*	0.830	1.303	5.247***
	(3.688)	(0.320)	(3.626)	(0.506)	(1.697)	(0.840)	(1.881)
N	5112	5112	5112	5112	5112	5112	5112

This table divides crimes into subsets and regresses the 12-month change of each subset on the 12-month change in store density and our control variables (using month fixed effects and both distance to border and major roadway IVs). We provide our headline result in (1) for reference. In regressions (2) and (3) we regress the 12-month change in violent and nonviolent crimes (per 10,000 residents). In columns (4) through (7) we divide the universe of crimes into crimes against persons, property, society, and other. Heteroskedastic robust standard errors are shown in parentheses, and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. "Miles to Major Road" is the distance in miles to a major road; "Miles to Border" is the distance in miles to the nearest municipal border; "poverty rate" is the poverty rate as a percentage; "Employment" is total employment in the tract in thousands; "Pct. White" is the percentage of white/non-hispanic population in each tract; "Miles to CBD" is the distance to the central business district of Denver in miles; and "Useable Land" is the the total amount of land per 10,000 residents that is not restricted for use as a dispensary.

olent crimes.[60] The majority (93 percent) of the reduction in crime is due to a decrease in nonviolent crimes. We also divide the universe of crimes into crimes against persons, property, society, and "other" as shown in columns (4) through (7). The decrease in crimes against persons is mostly driven by declines in simple and aggravated assaults. Decreases in "other" crimes are driven by decreases in criminal tres-

[60] We use the FBI's Uniform Crime Reporting Program definition of violent and nonviolent crime.

Table 8
IV results by crime category - drugs.

	(1) All Drugs	(2) Marj.	(3) Marj. Rel.	(4) Meth	(5) Cocaine	(6) Heroine
12-M Chg Store Den.	−2.252	−0.076	0.143	−0.233	−0.059	−0.650
	(1.517)	(0.779)	(0.237)	(0.399)	(0.478)	(0.563)
Poverty Rate	8.532	0.631	−0.251	0.749	0.641	1.025
	(5.786)	(2.905)	(1.268)	(1.760)	(1.837)	(2.007)
Employment (1,000s)	0.110	0.022	−0.006	0.024	−0.001	0.037
	(0.116)	(0.089)	(0.005)	(0.022)	(0.014)	(0.027)
Pct. White Non-Hispanic	0.515	0.057	0.035	−0.187	0.271	−0.337
	(0.974)	(0.467)	(0.206)	(0.297)	(0.256)	(0.395)
Miles to CBD	−0.209	0.002	0.006	−0.049	0.004	−0.096
	(0.150)	(0.059)	(0.020)	(0.040)	(0.041)	(0.076)
Useable Land/10,000 Res.	0.942	0.008	−0.015	0.158	−0.057	0.439
	(0.801)	(0.302)	(0.119)	(0.219)	(0.219)	(0.402)
N	5112	5112	5112	5112	5112	5112

This table shows the second-stage results for 12-month change in drug crimes (per 10,000 residents) regressed on the 12-month change in store density and our control variables (using month fixed effects and both distance to border and major roadway IVs). Regression (1) shows results for the 12-month change in all drug crimes. In column (2), we show results for marijuana crimes, and in column (3), we show results for marijuana related crimes (data provided by City of Denver). Columns (4), (5), and (6) show results for methamphetamine, cocaine, and heroin crimes. Heteroskedastic robust standard errors are shown in parentheses and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. "Miles to Major Road" is the distance in miles to a major road; "Miles to Border" is the distance in miles to the nearest municipal border; "poverty rate" is the poverty rate as a percentage; "Employment" is total employment in the tract in thousands; "Pct. White" is the percentage of white/non-hispanic population in each tract; "Miles to CBD" is the distance to the central business district of Denver in miles; and "Useable Land" is the the total amount of land per 10,000 residents that is not restricted for use as a dispensary.

passing, public-order crimes,[61] and other criminal mischief.[62] Given that these crime categories could be associated with alcohol abuse, the results support the theory that marijuana use is a substitute for alcohol consumption, corroborating findings by Anderson et al. (2013) and Crost and Guerrero (2012).

We also examine the effects of dispensaries on drug-related crimes; results are shown in Table 8 using the same techniques as above. Column (1) shows that adding a dispensary to a neighborhood of 10,000 residents decreases the change in the drug crime rate by roughly 2.3 crimes per month, but the results are statistically insignificant.[63] Columns (2) and (3) show that dispensaries have almost no effect on the number of marijuana crimes or marijuana-related crimes.[64] Columns (4) through (6) show that, while not significant, we find decreases in the number of methamphetamine, cocaine, and heroin crimes committed in tracts that gain dispensaries. It is possible that increased enforcement near dispensaries could result in offsetting increases in drug arrests, despite lower overall activity, given that arrests for drugs are more discretionary than other crimes.

These results are weakly consistent with the theory that marijuana legalization decreases crime through the displacement of illicit markets. Underlying such a theory is the belief that criminal organizations resort to violence to enforce contracts and that illicit markets for marijuana are horizontally integrated with markets for other types of drugs (methamphetamine, cocaine, and heroin). If increases in legal dispensaries displace illicit markets, then we would expect to see decreases in assaults as well as crimes related to sales of other illegal drugs including methamphetamine, cocaine, and heroin in those neighborhoods.

The results show no evidence supporting theories that marijuana dispensaries increase local cannabis crimes (since we do not find increases in marijuana crimes such as cultivation, possession, or sales nearby) or that dispensaries increase crimes through increased intoxication (since there is essentially no change in the number of crimes with marijuana as a "contributing factor" near locations that gain dispensaries). These findings are consistent with previous research that has found no link between marijuana use and criminal behavior.[65]

7.2. Geographic extent and distribution of effects

Next, we examine the geographic extent of the crime reduction. First, we look at whether or not the opening or closing of a dispensary affects crime in neighboring census tracts. We define the nearest neighbor by comparing the distance between centroids of the treated tract and its neighbors. To test for spillover effects, we use a modified version of the two-stage least-squares approach used previously. We first run the standard first-stage regressions (see equation (3)) for each tract. We then substitute the predicted change in dispensary density for each tract ($\widehat{\Delta disp}_{j,t}$) as well as the predicted change in dispensaries for the tract that is closest to the jth tract ($\Delta \widehat{neighbordisp}_{j,t}$) into the second-stage regression given by the following equation:

$$\Delta crime_{j,t} = \beta_0 + \beta_1 \mathbf{X}_j + \beta_2 \widehat{\Delta disp}_{j,t} + \beta_3 \Delta \widehat{neighbordisp}_{j,t} + \delta_t + \epsilon_{j,t}. \quad (4)$$

We show the results of equation (4) in column (2) in Table 9.[66] For comparison, we show the baseline results in column (1). The coefficient for the 12-month change in dispensary density increases slightly with the inclusion of the nearest neighbor. Importantly, we see that the coefficient on the 12-month change in dispensary density for the neighboring tract (β_3) is positive but not significant. This suggests that the effects of dispensaries on crime are very localized.[67] Note that this

[61] Excluding public fighting or disturbing the peace.

[62] Other criminal mischief involves the intentional destruction/damaging of property that is not graffiti or damage to a motor vehicle.

[63] Drug crimes include the manufacture, sale, and possession of illegal drugs.

[64] According to the Denver Police Department, marijuana-related crimes are "crimes reported to the Denver Police Department which, upon review, were determined to have clear connection or relation to marijuana."

[65] See White and Gorman (2000) for a review of the literature on the relationship between drug use and crime.

[66] Note that the standard errors generated by this exercise do not include corrections typically conducted in two-stage least-squares estimation, given the complexity of using predicted values for two right-hand variables. Standard errors would be larger if proper adjustments were made. However, the estimates of the coefficient on "nearest neighbor" are not statistically significant, so the different errors would not change the conclusions.

[67] In addition to the nearest neighbor results, we also ran regressions at the county level. These results, shown in Appendix B, provide evidence that increased dispensary density leads to reduced crime at the county level. However, the results are weaker, which would be consistent with the fact that crime effects are localized.

Table 9
Spillover effects for nearest neighbors.

	(1) Base 2SLS	(2) 2SLS w/Neighbor	(3) 2SLS w/2 Neighbors	(4) 2SLS w/3 Neighbors
12-M Chg Store Den.	−17.009**	−18.679***	−19.858***	−19.703***
	(7.035)	(5.437)	(5.736)	(5.691)
12-M Chg Store Den. Nearest Neigh.		1.543	0.990	0.731
		(1.106)	(1.223)	(1.264)
12-M Chg Store Den. 2nd Nearest Neigh.			3.221	3.812*
			(2.046)	(2.080)
12-M Chg Store Den. 3rd Nearest Neigh.				7.495***
				(2.684)
Poverty Rate	79.530**	83.116***	84.689***	86.490***
	(32.448)	(23.893)	(24.257)	(24.320)
Employment (1,000s)	0.732*	0.758**	0.775**	0.766**
	(0.402)	(0.356)	(0.351)	(0.351)
Pct. White Non-Hispanic	0.671	1.211	2.314	8.795**
	(5.472)	(4.315)	(4.549)	(4.077)
Miles to CBD	−1.762***	−1.716***	−1.637***	−1.159**
	(0.659)	(0.644)	(0.634)	(0.502)
Useable Land/10,000 Res.	10.662***	10.892***	11.088***	10.684***
	(3.688)	(3.391)	(3.432)	(3.313)
N	5112	5112	5112	5112

All regressions use both IVs (miles to major road and miles to border) and time fixed effects. For comparison, regression (1) shows the results of the baseline model. Regressions (2)–(4) use fist-stage predictions of changes in dispensary density for each tract and for the change in dispensary density for the nearest 1, 2, and 3 tracts respectively. These regressions do not include standard error corrections. Heteroskedastic robust standard errors are shown in parentheses, and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. "Miles to Major Road" is the distance in miles to a major road; "Miles to Border" is the distance in miles to the nearest municipal border; "poverty rate" is the poverty rate as a percentage; "Employment" is total employment in the tract in thousands; "Pct. White" is the percentage of white/non-hispanic population in each tract; "Miles to CBD" is the distance to the central business district of Denver in miles; and "Useable Land" is the the total amount of land per 10,000 residents that is not restricted for use as a dispensary.

Table 10
Spillovers effects by distance.

	(1) Base 2SLS	(2) <1 mi.	(3) <1.5 mi.	(4) <2 mi.
12-M Chg Store Den.	−17.009***	−27.651***	−19.691***	−20.054***
	(5.685)	(7.834)	(5.716)	(5.816)
Neighbors 12-M Chg Store Den.		14.566***	4.153*	6.110**
		(3.878)	(2.122)	(2.743)
Poverty Rate	79.530***	99.117***	88.438***	90.124***
	(24.334)	(25.686)	(24.817)	(25.125)
Employment (1,000s)	0.732**	0.940***	0.778**	0.795**
	(0.356)	(0.353)	(0.353)	(0.355)
Pct. White Non-Hispanic	0.671	4.498	3.145	3.876
	(4.267)	(4.474)	(4.716)	(4.741)
Miles to CBD	−1.762***	−1.201**	−1.546**	−1.437**
	(0.631)	(0.514)	(0.670)	(0.669)
Useable Land/10,000 Res.	10.662***	15.682***	11.047***	11.332***
	(3.461)	(5.757)	(3.409)	(3.443)
N	5112	4680	5112	5112

All regressions use both IVs (miles to major road and miles to border) and time fixed effects. For comparison, regression (1) shows the results of the baseline model. Regressions (2)–(4) use fist-stage predictions of changes in dispensary density for each tract and for the change in dispensary density for tracts within 1, 1.5, and 2 miles respectively. These regressions do not include standard error corrections. Heteroskedastic robust standard errors are shown in parentheses, and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. "Miles to Major Road" is the distance in miles to a major road; "Miles to Border" is the distance in miles to the nearest municipal border; "poverty rate" is the poverty rate as a percentage; "Employment" is total employment in the tract in thousands; "Pct. White" is the percentage of white/non-hispanic population in each tract; "Miles to CBD" is the distance to the central business district of Denver in miles; and "Useable Land" is the the total amount of land per 10,000 residents that is not restricted for use as a dispensary.

result does not provide insight on whether or not crime is displaced into nearby neighborhoods, as the estimates only uncover relative changes in crime.

In addition to looking at the nearest neighboring tract, we use several additional specifications to test the robustness of this result. First, we look at the effect on not just the nearest neighbor, but also the second and third closest tracts. These results are shown in columns (3) and (4) in Table 9. Again we find a reduction in crime only in the tract that receives the dispensary. In Table 10 we show the results of a specification where we include the treated tract as well as all tracts within a given radius. The results for a radius of 1, 1.5, and 2 miles are shown in columns (2)–(4). Again, there is only a reduction in crime in the treated tract.

The lack of spillover effects into neighboring tracts is consistent with the theory that dispensaries decrease crime in their neighborhood through increased private surveillance and the theory that law

enforcement may change their enforcement behavior in the direct vicinity of dispensaries. Our findings are related to those of Freisthler et al. (2016) who find no effect in the neighborhoods with dispensaries but increased crime in nearby neighborhoods. The differences in the conclusions may be due to the fact that the current analysis considers the effects of both openings and closures and accounts for selection bias arising from nonrandom location of dispensaries. These results are also consistent with the literature on crime and place reviewed by Eck and Weisburg (2015) that finds that criminal activity is very localized, often at particular addresses or businesses. In addition, displacement of crime or spillover benefits from place based policies are often small and geographically concentrated.

Finally, it is useful to think about the distributional effects of dispensary locations on crime. As discussed in Section 3.3, neighborhoods with higher poverty rates, larger Hispanic populations, and more employment on average saw substantially larger increases in dispensary density compared with other tracts. Neighborhoods with these characteristics also tend to have higher initial crime rates. Since we find the increases in dispensary densities have a depressive effect on crime, then the net result of the heterogeneous location of dispensaries is that crime distributions are more equitably distributed across these neighborhood characteristics compared with a counterfactual without changes in dispensary density.

8. Conclusion

We use a novel identification strategy to show significant crime reductions in neighborhoods that receive marijuana dispensaries. To our knowledge, our research is the first research to use exogenous variation in dispensary locations to identify local crime effects of marijuana dispensaries. We find that adding a dispensary to a neighborhood (of 10,000 residents) decreases changes in crime by 19 percent relative to the average monthly crime rate in a census tract. These results are robust to many alternative specifications, are unique to time periods after legalization, and diminish quickly over space. Our results are consistent with theories that predict that marijuana legalization will displace illicit criminal organizations and decrease crime through changes in security behaviors or substitution toward more harmful substances.

These neighborhood results are important to policy makers in states that recently legalized marijuana, as law makers consider regulating the location of dispensaries. Our research can also inform local public hearings about dispensary locations and the decisions of future voters in states and municipalities that have ballot initiatives regarding marijuana legalization. If it is the case that municipal-level changes in crime are an aggregation of neighborhood effects on crime, then our research would suggest that the legalization of legal marijuana markets would decrease crime at the municipal level.

There are questions about the external validity of this study, given that data come from a single municipality and are therefore vulnerable to idiosyncrasies specific to Denver. Future research could use similar methods to analyze the effects of legalization in other municipalities and states as more data become available. While the single municipality of study limits the external validity of our research, it improves the internal validity of our results, since we do not compare differences between municipalities that have different regulatory environments. Our research is also only applicable to short-term outcomes, and in future years more research should be done to determine the long-run general equilibrium effects of legalization on crime. Other opportunities for future research include examining the effects of legal marijuana distribution on other neighborhood amenities and further dissecting the underlying mechanisms that lead to reduced crime.

A. Change in crime tracking methods

"In May 2013 the Denver Police Department implemented the Unified Summons and Complaint (US and C) process. This process unifies multiple types of paper citations, excluding traffic tickets, into an electronic process. That information is transmitted to the Denver Sheriff, County Court, City Attorney and District Attorney through a data exchange platform as needed" (Denver Police Department, 2013). As a result of this change in tracking, certain crime categories (such as curfew, gambling, or disturbing the peace) show large increases in the number of crimes reported under the new system. For instance, in 2013 there was a total of 1,811 "Disorderly Conduct/Disturbing the Peace" incidents in the Denver crime database of which 798 incidents would not have been reported under the old system.

The percent increase of incidents caused by the change in reporting in decreasing order are curfew (97%), liquor law/drunkenness (93%), disorderly conduct/disturbing the peace (44%), criminal trespassing (40%), drug/narcotics violations (29%), gambling (25%), prostitution (17%), weapons laws (13%), intimidation (10%), larceny (10%), simple assault (8%), and embezzlement (8%). Categories with less than 8% change include counterfeiting/forgery, criminal mischief/damaged property, fraud, family offenses(nonviolent), violation of a restraining/court order, and harassment. Categories not affected by the change in the tracking system were murder, aggravated assault, kidnapping/abduction, arson, bribery, burglary, theft from motor vehicle, motor vehicle theft, robbery, and stolen property.

We control for changes in crime reporting levels across Denver by using time fixed effects. As long as the increases in crime incidents caused by the new reporting system are uncorrelated with our IVs, then the changes in the reporting system do not jeopardize our results. A check, we divide crimes into two categories: those that show large changes in the number of incidents reported from the new system (more than a 15% difference) and those that show small changes in the number of crimes reported from the new system (less than a 15% difference). Table 11 shows the IV results using each set of crimes. The coefficient on the 12-month change in store density is statistically significant for those crimes that show only small changes under the new reporting system (column (2)) and is consistent with our other results. The coefficient for crimes largely affected by the change in reporting systems (column (3)) is also significant and negative. The consistently negative and statistically significant coefficients on the 12-month change in store density across both crime groupings suggest that the new reporting system did not substantially change the results.

Table 11
Regressions by crimes minimally vs. substantially affected by changes in crime tracking.

	(1) Base	(2) Minimally Effected†	(3) Substantially Effected‡
12-M Chg Store Den.	−17.009**	−10.401**	−6.608*
	(7.035)	(4.866)	(3.539)
Poverty Rate	79.530**	51.558**	27.972*
	(32.448)	(23.170)	(15.178)
Employment (1,000s)	0.732*	0.532**	0.200
	(0.402)	(0.213)	(0.236)
Pct. White Non-Hispanic	0.671	−1.201	1.873
	(5.472)	(3.909)	(2.486)
Miles to CBD	−1.762***	−1.069**	−0.693**
	(0.659)	(0.450)	(0.345)
Useable Land/10,000 Res.	10.662***	6.423**	4.239**
	(3.688)	(2.512)	(1.913)
N	5112	5112	5112

For comparison, our base IV results for the change in 12-month crime on 12-month changes in dispensary density (using month fixed effects and both distance to border and major roadway IVs) are shown in column (1). The City of Denver changed its crime tracking system in May of 2013 (the crime levels of some categories were minimally effected by the change while other categories of crimes showed substantial increases in levels). We divide all crimes into two categories: those minimally affected (†), defined by crime categories effected less than 15% by the change in tracking, and those substantially effected (‡) by the change (if crime levels were effected by more than 15%). We then run our main model on each subset of crimes (shown in columns (2) and (3)). Heteroskedastic robust standard errors are shown in parentheses, and significance is denoted by * = 0.01, ** = 0.05, and *** = 0.01. Crime data provided by the City of Denver.

B. County-level results

While this paper is primarily about neighborhood-level effects of dispensaries, it is useful to consider the effects of crime at the county level. While we would expect external demand to also be important in location of dispensaries among counties, the instruments used in our tract-level regressions are not useful at the county level given the concentration of dispensaries in urban areas and the coarseness of the geography. Instead we use ex-ante employment in industries that serve tourist populations to instrument for dispensary locations.[68] We separate these into two standard industry categories, defined by the economic census: employment in entertainment, recreation, and art; and employment in accommodation and food services. There are concerns that these instruments suffer from endogeneity, in that they may be correlated with changes in crime during the sample period, and thus these results should be considered with some skepticism. For this analysis we use crime data from the Federal Bureau of Investigations' National Incident-Based Reporting System accessed through the National Archive of Criminal Justice Data (University of Michigan). Data on dispensaries are the same as the tract-level aggregated at the county level. Table 12 shows the county-level results along with the tract-level results presented earlier. The OLS results are shown in column (3), while the IV results using the two instruments both separately and together are shown in columns (4) through (6). The OLS estimates show no significant correlation between changes in dispensary density and changes in crime at the county level. The IV estimates are consistent with the tract-level results, showing some evidence of reduced crime in counties that received more dispensaries. The results, however, are weaker both economically and statistically, which is to be expected given that we have shown that the effects tend to be contained at the neighborhood level where the dispensaries are located.

Table 12
County-level regressions.

	Tracts in Denver		Counties in Colorado			
	(1) OLS	(2) ALL IV	(3) OLS	(4) Ent/Rec/Art IV	(5) Acomed/Food IV	(6) BOTH IV
12-M Chg Store Den.	1.924***	−17.009**	0.891	−3.121**	−6.878	−3.002*
	(0.459)	(7.035)	(0.570)	(1.588)	(5.236)	(1.564)
Poverty Rate	14.427	79.530**	−1.839	5.788	12.929	5.562
	(13.247)	(32.448)	(7.111)	(7.985)	(12.603)	(7.947)
Employment (1,000s)	0.403	0.732*	−0.002	0.002	0.005	0.001
	(0.370)	(0.402)	(0.003)	(0.003)	(0.005)	(0.003)
Pct. White Non-Hispanic	0.579	0.671	−0.319	−0.597	−0.857	−0.589
	(3.799)	(5.472)	(3.071)	(2.961)	(3.060)	(2.962)
Miles to CBD	−1.302**	−1.762***	−0.013*	−0.005	0.003	−0.005
	(0.579)	(0.659)	(0.007)	(0.007)	(0.011)	(0.007)
Useable Land/10,000 Res.	5.390**	10.662***				
	(2.225)	(3.688)				
N	5112	5112	768	768	768	768
Weak IV Test Statatistic♯		10.415		120.599	9.569	62.104

For comparison, our base OLS and IV results for the 12-month change in crime date (for Denver) are included in (1) and (2) (our IV results use both miles to major road and miles to border as IVs for change in dispensary density and use time fixed effects). In regressions (3) through (6) we show county-level regression results of 12-month changes in crime rates (per 10,000 residents) on 12-month changes in dispensary density (per 10,000 residents). Regression (3) shows OLS results. In regression (4), we use the number of employees in entertainment, recreation, or art industries as an IV for changes in dispensary density, and regression (5) uses the number of employees in accommodation or food industries as an IV. In regression (6) we use both the IVs in the first stage. Denver regressions use crime data provided by City of Denver (between 2013 and 2016) and county-level regressions use crime data provided by the FBI (for between 2013 and 2014). Heteroskedastic robust standard errors are shown in parentheses and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. The bottom row shows the Kleibergen-Paap Wald F statistic for week IV.

[68] Anecdotally, there is a clear concentration of recreational dispensaries near ski resorts in the Rocky Mountains.

C. Police districts

Fig. 6 shows a map of the police districts for the City of Denver. In Table 6 we showed the results of a specification where police district fixed effects are included in the regression.

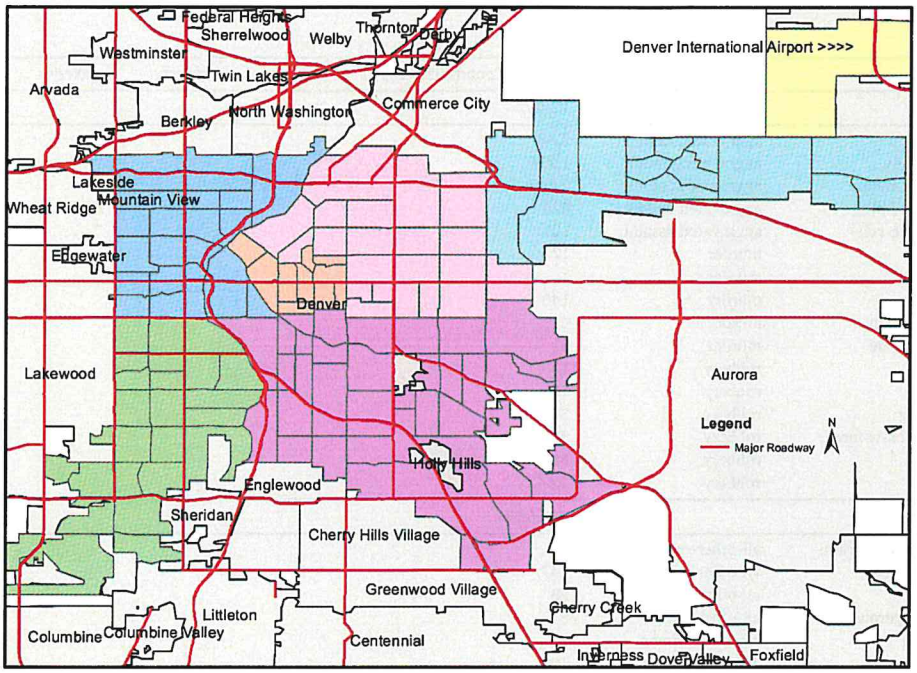


Fig. 6 Police Districts. Each police district is shown in a different color (major roadways shown in red). Police district data are provided by the City of Denver. (For interpretation of the references to color in this figure legend, the reader is referred to the Web version of this article.)

D. Spatially clustered errors

A potential concern is that errors may be spatially correlated. Recently, a preferred method for dealing with this issue is to estimate standard errors using a spatial HAC correction, as proposed by Conley (1999). However, these methods are not well developed for panel IV settings. Table 13 presents results with standard errors at the police district level. The estimated standard errors remain similar in this case, with only slight loss of precision.

Table 13
Results with standard errors clustered at by police district.

	(1) OLS	(2) m2b - IV	(3) m2r - IV	(4) ALL IV
12-M Chg Store Den.	1.924***	−20.575**	−10.980	−17.009**
	(0.501)	(9.400)	(7.551)	(7.731)
Poverty Rate	14.427	91.791**	58.798*	79.530**
	(13.649)	(41.261)	(33.740)	(35.923)
Employment (1,000s)	0.403	0.794*	0.627	0.732*
	(0.375)	(0.415)	(0.405)	(0.406)
Pct. White Non-Hispanic	0.579	0.689	0.642	0.671
	(4.393)	(5.942)	(4.987)	(5.552)
Miles to CBD	−1.302**	−1.848***	−1.615***	−1.762***
	(0.538)	(0.688)	(0.619)	(0.652)
Useable Land/10,000 Res.	5.390**	11.655***	8.983**	10.662***
	(2.373)	(4.088)	(3.720)	(3.773)
[1em] Weak IV Test Statatistic		20.327	16.246	12.723
Hansen J Statistic				1.230
Hansen J p-value				0.267

This table shows the second-stage results for the 12-month change in crime rate (per 10,000 residents) using time fixed effects. Standard errors are clustered at the police district level. For comparison, we include OLS regression results in regression (1). In regression (2), we use only the miles to border IV, in regression (3) we use just the miles to roads IV, and in regression (4), we use both variables as IVs. Heteroskedastic robust standard errors are shown in parentheses and significance is denoted by * = 0.10, ** = 0.05, and *** = 0.01. The bottom rows show the Kleibergen-Paap Wald F statistic for weak IV, as well as the results of the Hansen J overidentification test. "Miles to Major Road" is the distance in miles to a major road; "Miles to Border" is the distance in miles to the nearest municipal border; "poverty rate" is the poverty rate as a percentage; "Employment" is total employment in the tract in thousands; "Pct. White" is the percentage of white/non-hispanic population in each tract; "Miles to CBD" is the distance to the central business district of Denver in miles; and "Useable Land" is the the total amount of land per 10,000 residents that is not restricted for use as a dispensary.

E. Crime categories

Table 14 show all crimes and crime categories in the data. The third column shows the number of incidents for each individual offense from 2013 to 2016 as well as the aggregate counts for violent and nonviolent crimes. In addition, the categories used in the analysis in Table 7 are shown in the last four columns.

Table 14
Crime categories.

Offense	Offense Category	Incident Count(2013–2016)	persons	property	society	other
violent crimes		**12,097**				
aggravated-assault	aggravated-assault	3,587	*			
aggravated-assault-dv	aggravated-assault	1,216	*			
menacing-felony-w-weap	aggravated-assault	2,082	*			
weapon-fire-into-occ-bldg	aggravated-assault	424			*	
weapon-fire-into-occ-veh	aggravated-assault	101			*	
homicide-family	murder	17	*			
homicide-negligent	murder	1	*			
homicide-other	murder	148	*			
homicide-police-by-gun	murder	0	*			
traf-vehicular-homicide	murder	21	*			
robbery-bank	robbery	135		*		
robbery-business	robbery	969		*		
robbery-car-jacking	robbery	373		*		
robbery-purse-snatch-w-force	robbery	267		*		
robbery-residence	robbery	314		*		
robbery-street	robbery	2,442		*		
nonviolent crimes		**295,928**				
accessory-conspiracy-to-crime	all-other-crimes	78				*
agg-aslt-police-weapon	all-other-crimes	147	*			
animal-cruelty-to	all-other-crimes	58				*
animal-poss-of-dangerous	all-other-crimes	8				*
aslt-agg-police-gun	all-other-crimes	5	*			
bomb-threat	all-other-crimes	103				*
bribery	all-other-crimes	8				*
contraband-into-prison	all-other-crimes	82				*
contraband-possession	all-other-crimes	32				*
criminal-trespassing	all-other-crimes	7,697				*
disarming-a-peace-officer	all-other-crimes	2				*
eavesdropping	all-other-crimes	1				*
escape	all-other-crimes	121				*
escape-aiding	all-other-crimes	0				*
explosive-incendiary-dev-pos	all-other-crimes	13			*	
explosive-incendiary-dev-use	all-other-crimes	8			*	
extortion	all-other-crimes	52		*		
failure-to-appear	all-other-crimes	279				*
false-imprisonment	all-other-crimes	71	*			
fireworks-possession	all-other-crimes	75			*	
gambling-device	all-other-crimes	1			*	
gambling-gaming-operation	all-other-crimes	6			*	
homicide-solicitation	all-other-crimes	2	*			
illegal-dumping	all-other-crimes	72				*
intimidation-of-a-witness	all-other-crimes	97	*			
kidnap-adult-victim	all-other-crimes	79	*			
kidnap-dv	all-other-crimes	39	*			
littering	all-other-crimes	80				*
obstructing-govt-operation	all-other-crimes	26				*
other-environment-animal-viol	all-other-crimes	111				*
parole-violation	all-other-crimes	10				*
pawn-broker-viol	all-other-crimes	27				*
police-disobey-lawful-order	all-other-crimes	165				*
police-false-information	all-other-crimes	1,904				*
police-interference	all-other-crimes	746				*
police-making-a-false-rpt	all-other-crimes	19				*
police-obstruct-investigation	all-other-crimes	6				*
police-resisting-arrest	all-other-crimes	57				*
probation-violation	all-other-crimes	12				*
property-crimes-other	all-other-crimes	93		*		
public-fighting	all-other-crimes	443				*
public-order-crimes-other	all-other-crimes	4,689				*
pub-peace-desecrate-symb	all-other-crimes	2				*
reckless-endangerment	all-other-crimes	51				*
sex-off-fail-to-register	all-other-crimes	884				*
sex-off-registration-viol	all-other-crimes	3				*
stolen-property-buy-sell-rec	all-other-crimes	54		*		

(continued on next page)

Table 14 *(continued)*

Offense	Offense Category	Incident Count(2013–2016)	persons	property	society	other
traf-habitual-offender	all-other-crimes	2,164				
traf-impound-vehicle	all-other-crimes	13				
traf-other	all-other-crimes	17,908				
traf-vehicular-assault	all-other-crimes	138	*			
vehicular-eluding	all-other-crimes	96				*
vehicular-eluding-no-chase	all-other-crimes	1,670				*
violation-of-court-order	all-other-crimes	1,205				*
violation-of-custody-order	all-other-crimes	22				*
violation-of-restraining-order	all-other-crimes	1,398				*
weapon-altering-serial-number	all-other-crimes	8			*	
weapon-by-prev-offender-powpo	all-other-crimes	531			*	
weapon-carrying-concealed	all-other-crimes	230			*	
weapon-carrying-prohibited	all-other-crimes	264			*	
weapon-flourishing	all-other-crimes	139			*	
weapon-other-viol	all-other-crimes	401			*	
weapon-poss-illegal-dangerous	all-other-crimes	286			*	
weapon-unlawful-discharge-of	all-other-crimes	731			*	
wiretapping	all-other-crimes	2				*
arson-business	arson	36		*		
arson-other	arson	101		*		
arson-public-building	arson	10		*		
arson-residence	arson	98		*		
arson-vehicle	arson	142		*		
theft-of-motor-vehicle	auto-theft	15,514		*		
theft-stln-veh-const-eqpt	auto-theft	30		*		
theft-stln-vehicle-trailer	auto-theft	283		*		
burg-auto-theft-busn-no-force	burglary	31		*		
burg-auto-theft-busn-w-force	burglary	88		*		
burg-auto-theft-resd-no-force	burglary	285		*		
burg-auto-theft-resd-w-force	burglary	127		*		
burglary-business-by-force	burglary	3,634		*		
burglary-business-no-force	burglary	1,110		*		
burglary-poss-of-tools	burglary	141		*		
burglary-residence-by-force	burglary	6,436		*		
burglary-residence-no-force	burglary	6,643		*		
burglary-safe	burglary	54		*		
drug-barbiturate-mfr	drug-alcohol	2			*	
drug-barbiturate-possess	drug-alcohol	31			*	
drug-barbiturate-sell	drug-alcohol	6			*	
drug-cocaine-possess	drug-alcohol	2,005			*	
drug-cocaine-sell	drug-alcohol	828			*	
drug-fraud-to-obtain	drug-alcohol	97			*	
drug-hallucinogen-mfr	drug-alcohol	1			*	
drug-hallucinogen-possess	drug-alcohol	89			*	
drug-hallucinogen-sell	drug-alcohol	31			*	
drug-heroin-possess	drug-alcohol	1,411			*	
drug-heroin-sell	drug-alcohol	449			*	
drug-make-sell-other-drug	drug-alcohol	27			*	
drug-marijuana-cultivation	drug-alcohol	77			*	
drug-marijuana-possess	drug-alcohol	2,405			*	
drug-marijuana-sell	drug-alcohol	213			*	
drug-methampetamine-possess	drug-alcohol	2,028			*	
drug-methampetamine-sell	drug-alcohol	288			*	
drug-methamphetamine-mfr	drug-alcohol	9			*	
drug-opium-or-deriv-possess	drug-alcohol	66			*	
drug-opium-or-deriv-sell	drug-alcohol	25			*	
drug-pcs-other-drug	drug-alcohol	430			*	
drug-poss-paraphernalia	drug-alcohol	3,345			*	
drug-synth-narcotic-possess	drug-alcohol	49			*	
drug-synth-narcotic-sell	drug-alcohol	35			*	
liquor-manufacturing	drug-alcohol	2				*
liquor-misrepresent-age-minor	drug-alcohol	5				*
liquor-other-viol	drug-alcohol	8				*
liquor-possession	drug-alcohol	6,510				*
liquor-sell	drug-alcohol	173				*
burglary-vending-machine	larceny	199		*		
theft-bicycle	larceny	5,740		*		
theft-from-bldg	larceny	4,137		*		
theft-from-mails	larceny	281		*		
theft-gas-drive-off	larceny	23		*		
theft-of-services	larceny	654		*		
theft-other	larceny	13,498		*		
theft-pick-pocket	larceny	130		*		

(continued on next page)

Table 14 *(continued)*

Offense	Offense Category	Incident Count(2013–2016)	persons	property	society	other
theft-purse-snatch-no-force	larceny	258		*		
theft-shoplift	larceny	10,415		*		
assault-dv	other-crimes-against-persons	5,719	*			
assault-police-simple	other-crimes-against-persons	426	*			
assault-simple	other-crimes-against-persons	8,524	*			
indecent-exposure	other-crimes-against-persons	484			*	
obscene-material-mfr	other-crimes-against-persons	3				*
window-peeping	other-crimes-against-persons	39				*
criminal-mischief-graffiti	public-disorder	2,796				*
criminal-mischief-mtr-veh	public-disorder	10,215				*
criminal-mischief-other	public-disorder	8,000				*
curfew	public-disorder	996				*
disturbing-the-peace	public-disorder	2,626				*
harassment	public-disorder	774			*	
harassment-dv	public-disorder	328			*	
harassment-sexual-in-nature	public-disorder	168			*	
harassment-stalking-dv	public-disorder	103			*	
loitering	public-disorder	3				*
prostitution-aiding	public-disorder	4			*	
prostitution-engaging-in	public-disorder	343			*	
prostitution-pimping	public-disorder	37			*	
public-peace-other	public-disorder	433				*
riot	public-disorder	0				*
riot-incite	public-disorder	0			*	
threats-to-injure	public-disorder	2,368				*
sex-aslt-fondle-adult-victim	sexual-assault	0				
sex-aslt-non-rape	sexual-assault	0				
sex-aslt-non-rape-pot	sexual-assault	0				
sex-aslt-rape	sexual-assault	0				
sex-aslt-rape-pot	sexual-assault	0				
sex-aslt-w-object	sexual-assault	0				
theft-items-from-vehicle	theft-from-motor-vehicle	17,935		*		
theft-parts-from-vehicle	theft-from-motor-vehicle	6,020		*		
traffic-accident	traffic-accident	62,092				
traffic-accident-dui-duid	traffic-accident	2,598				
traffic-accident-hit-and-run	traffic-accident	22,376				
altering-vin-number	white-collar-crime	0		*		
drug-forgery-to-obtain	white-collar-crime	76			*	
forgery-checks	white-collar-crime	415		*		
forgery-counterfeit-of-obj	white-collar-crime	74		*		
forgery-other	white-collar-crime	165		*		
forgery-posses-forge-device	white-collar-crime	25		*		
forgery-poss-of-forged-ftd	white-collar-crime	12		*		
forgery-poss-of-forged-inst	white-collar-crime	77		*		
fraud-by-telephone	white-collar-crime	258		*		
fraud-by-use-of-computer	white-collar-crime	443		*		
fraud-criminal-impersonation	white-collar-crime	229		*		
fraud-identity-theft	white-collar-crime	1,448		*		
fraud-nsf-closed-account	white-collar-crime	61		*		
gambling-betting-wagering	white-collar-crime	4			*	
impersonation-of-police	white-collar-crime	41		*		
theft-confidence-game	white-collar-crime	34		*		
theft-embezzle	white-collar-crime	108		*		
theft-fail-return-rent-veh	white-collar-crime	196		*		
theft-of-rental-property	white-collar-crime	22		*		
theft-unauth-use-of-ftd	white-collar-crime	357		*		
obscene-material-possess		6				*

Appendix F. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.regsciurbeco.2019.103460.

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