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ABSTRACT

High on Crime? Exploring the Effects of Marijuana Dispensary Laws on Crime in California Counties*

Regulated marijuana markets are more common today than outright prohibitions across the U.S. states. Advocates for policies that would legalize marijuana recreational markets frequently argue that such laws will eliminate crime associated with the black markets, which many argue is the only link between marijuana use and crime. Law enforcement, however, has consistently argued that marijuana medical dispensaries (regulated retail sale and a common method of medical marijuana distribution), create crime in neighborhoods with these store-fronts. This study offers new insight into the question by exploiting newly collected longitudinal data on local marijuana ordinances within California and thoroughly examining the extent to which counties that permit dispensaries experience changes in violent, property and marijuana use crimes using difference-in-difference methods. The results suggest no relationship between county laws that legally permit dispensaries and reported violent crime. We find a negative and significant relationship between dispensary allowances and property crime rates, although event studies indicate these effects may be a result of pre-existing trends. These results are consistent with some recent studies suggesting that dispensaries help reduce crime by reducing vacant buildings and putting more security in these areas. We also find a positive association between dispensary allowances and DUI arrests, suggesting marijuana use increases in conjunction with impaired driving in counties that adopt these ordinances, but these results are also not corroborated by an event study analysis.

JEL Classification: K14, K42, H75

Keywords: crime, marijuana markets, local ordinances

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* This paper was supported by a grant from the National Institute on Drug Abuse to the RAND Corporation (R01DA032693; PI: Rosalie Liccardo Pacula). We are deeply indebted to our colleagues James Anderson, Annie Boustead, Kate Pfrommer and Clinton Saloga who led the efforts in the construction of the ordinance database that was constructed under this grant and being used in this analysis.

1. Introduction

The impact of liberalizing marijuana laws on crime is a subject of great political and scholarly debate. Advocates for policy reform in states considering liberalization laws, both medical marijuana laws (MMLs) allowing for dispensaries as well as policies promoting retail sale for recreational purposes, suggest that bringing marijuana markets out of the shadows of the black market is a clear net public safety gain.[1] Indeed such a position is supported by scholarly work seeking to identify a causal link between marijuana use and violence, but not finding any (Arseneault et al. 2000; Mulvey et al. 2006). Nonetheless, law enforcement agencies in jurisdictions that have already adopted dispensary systems for medical marijuana claim that these dispensaries are inextricably connected to crime (California Police Chiefs Association 2009; Ingold and Lofholm 2016; Powers 2014).

The difficulty in reconciling these two positions can to some extent be comprehended by understanding the different mechanisms through which marijuana liberalization laws might potentially influence crime. First, there is the obvious impact of legitimizing what was previously an illegal market. By transitioning marijuana transactions from illegitimate exchanges that had to be actively enforced to legitimate transactions, there is an immediate reduction in the burden of enforcement assuming the legal market replaces the black market (Miron and Zwiebel 1995; Shepard and Blackley 2005). Law enforcement and the Courts may then transition resources to other, arguably more violent, types of crimes (Huber, Newman, and LaFave 2016; Miron and Zwiebel 1995). Second, there is the potential for liberalization laws to influence crime rates through an increase in marijuana (mis)use (e.g. psychopharmacological crime), to the extent that marijuana use induces criminogenic behavior. While there is no clear causal link between

[1] For example, see the "Issues" webpage for the Marijuana Policy Project: https://www.mpp.org/issues/.

marijuana use and criminogenic behavior, there is suggestive evidence for a positive correlation between use and property crime (Green et al. 2010; Pacula and Kilmer 2003).[2] A third potential mechanism through which liberalization laws could plausibly influence crime, which might also explain the positive correlation between use and property crime, is that these liberalization ordinances enable the creation of new brick and mortar and delivery businesses that, because of the federal prohibition and banking laws that prevent (until recently) debit cards from being accepted in stores, operate entirely on a cash basis, creating strong targets for burglaries or thefts (California Police Chiefs Association 2009).

A whole new body of scholarly work has emerged exploiting the natural experiment created by new state laws that liberalize the sale of medical marijuana to examine this relationship. As of November 2016, medical marijuana laws have been passed by 28 states plus the District of Columbia. The enactment of state laws since 1996 provide an opportunity to empirically test the effect of regulated markets on outcomes of interest. Studies have evaluated outcomes such as marijuana use (Chu 2014; Harper, Strumpf, and Kaufman 2012; Hasin et al. 2015; Lynne-Landsman, Livingston, and Wagenaar 2013; Pacula et al. 2015; Wall et al. 2011; Wen, Hockenberry, and Cummings 2015), crime rates (Chu and Townsend 2017; Gavrilova, Kamada, and Zoutman 2017; Huber, Newman, and LaFave 2016; Morris et al. 2014; Shepard and Blackley 2016), and other health outcomes (Anderson, Rees, and Sabia 2014; Chu 2015; Smart 2015). These studies all use a difference-in-difference methodology where the treatment is a change in a state law that loosens restrictions on the sale of marijuana.

[2] Another important factor to consider is marijuana use and victimization, although any evidence of a causal link pointing to an increase in victimization has been inconclusive (Markowitz 2005; Office of National Drug Control Policy 2013).

The concern with these state-level studies is that many states, particularly the early adopting states, actually defer to local entities when it comes to regulating marijuana supply and production, which leads to variation in treatment within states (Dilley et al. 2017; Freisthler et al. 2013). For example, in Colorado and Washington State, which legalized the sale of recreational marijuana in 2014, various types of policies exist at the community level and a significant portion of the population live in communities where the sale of recreational marijuana is not allowed (Colorado Department of Revenue 2016; Dilley et al. 2017). Moreover, medicinal marijuana laws within a state may differ on important elements, such as bans on dispensaries and cultivation (Pacula, Boustead, and Hunt 2014). Therefore, studies that generalize a MML across the state are ignoring heterogeneity within their treatment sample, possibly leading to the inconsistent findings in the MML literature (Pacula et al. 2015). Given the localized nature of crime and the importance of levels of aggregation, this (mis)measurement of the treatment dosage is especially problematic for estimating effects on crime rates (Freeman, Grogger, and Sonstelie 1996; Hipp 2007).

The objective of this study is to investigate whether a particular element of MMLs, namely allowance for dispensaries, affects local crime and other indicators of marijuana misuse (i.e. driving under the influence). We are also able to identify other dimensions of MMLs, such as allowance for home cultivation, but due to little variation in these other dimensions, we focus on allowances for dispensaries.[3] Moreover, we estimate effects on different types of crime, as well as arrests indicating marijuana use, to better understand the mechanisms driving the results. By utilizing a novel dataset that codifies elements of MMLs across local jurisdictions within California, we capture heterogeneity on the treatment variable that is present in other studies. Moreover, by focusing on local variation within a single state with a long experience with the

[3] We still control for whether a jurisdiction has limitations on home cultivation in all of our models.

policy, we are able to implicitly control for changing state norms and marijuana use that could be independently associated with marijuana-involved crime. Other state-level factors that could bias estimates of crime rates across states, such as depenalization of marijuana, are also implicitly controlled in our analysis (Huber, Newman, and LaFave 2016). This analysis can help inform policies at the local level, where regulation is usually enacted, that better balance safety and access to medical marijuana.

We find no evidence that ordinances allowing for marijuana dispensaries lead to an increase in crime. In fact, we see some evidence of a reduction in property crime along with an increase in DUI and misdemeanor marijuana arrests, pointing to possible increases in misuse of marijuana that do not result in more crime. Supplementary analyses indicate that the significant effects may be driven by pre-existing trends, so we limit our conclusions to the fact that counties allowing dispensaries did not experience an increase in crime. The rest of this paper proceeds as follows: Section 2 provides some background into the literature on regulation of marijuana markets and crime, Section 3 describes the methods used for the analysis, Section 4 provides results, and Section 5 concludes with a discussion of policy implications and limitations.

1. Background

2.1. Why Might Dispensary Laws Affect Reported Crime Rates and Arrests?

It is clear that explicitly writing into law that entities are permitted to engage in retail distribution of medical marijuana reduces the criminal justice risks of supplying marijuana. Theoretically, we might expect this to increase availability and access to marijuana, which could increase demand at both the extensive, and potentially the intensive margins. Indeed there is consistent evidence that laws on-the-books explicitly permitting entities to produce and distribute medical marijuana increase non-medical use of marijuana among adults (Hasin et al. 2017; Pacula

et al. 2015; Wen, Hockenberry, and Cummings 2015). The laws appear to have no general impact on youth marijuana use (Choo et al. 2014; Hasin et al. 2015; Lynne-Landsman, Livingston, and Wagenaar 2013; Anderson, Hansen, and Rees 2015; Shu-Acquaye 2016), although there is some evidence from studies accounting for the relative size of these evolving marijuana market that larger and/or more competitive markets do in fact influence youth marijuana use (D'Amico et al. 2015; Smart 2015); It is *a priori* unclear, however, what effect this increased use among adults may have on community-level violent and property crime or driving under the influence. Moreover, the replacement of a black market by a new cash-based business may or may not lead to a change in reported robberies, burglaries, and thefts (California Police Chiefs Association 2009). Although we cannot hypothesize on the overall directional change, by applying the Goldstein (1985) typology of drug crime, we consider the potential mechanisms driving changes in crime.

The first set of crimes in the typology is those committed due to intoxication, or psychopharmacological crimes. As was summarized in a recent report by ONDCP, there is little evidence for a causal link between marijuana intoxication and pharmacological crime (Office of National Drug Control Policy 2013, 14). Marijuana has been linked to correlates of violence such as development of psychosis disorders, aggression later in life, and delinquent behavior (Arseneault et al. 2000; Hall and Degenhardt 2008; White and Hansell 1998); but laboratory studies have not found a link between cannabis and violence (Moore and Stuart 2005) and there is reason to believe that marijuana use alone may lower the propensity to commit an aggressive act (Ostrowsky 2011). If marijuana is a substitute for alcohol, then increased availability of marijuana through retail outlets may lead to substitution away from alcohol, thereby reducing crime that would otherwise be associated with alcohol intoxication (Carpenter and Dobkin 2010; Carpenter

2007) and DUIs (Anderson, Hansen, and Rees 2013). However, if alcohol and marijuana are complements (Pacula 1998; Williams et al. 2004), it is possible that their joint consumption could lead to more aggressive behavior than alcohol or marijuana alone. Therefore, on net, we are *a priori* ambivalent towards the expected directional change (if any) in reported crime and DUI arrests due to pharmacological criminality, resulting from legal dispensaries or looser rules on cultivation.

A second type of crime in the typology is "economic-compulsive" crimes caused by those in need of income to pay for a drug habit (Goldstein 1985). We can expect that legalization of marijuana, even for medical purposes, will reduce the price of obtaining marijuana, and indeed there is limited evidence showing that potency has risen while prices for potency-adjusted fixed amounts have fallen (Anderson, Hansen, and Rees 2013; Sevigny, Pacula, and Heaton 2014). Substantially larger price declines have been observed with full legalization (Smart et al. 2017). Overall, we would expect a minimal increase in income-producing property crime driven by economic-compulsive behavior as a result of legalizing dispensaries.

The third category of crime is "systemic crimes," or those associated with the provision and distribution of drugs in black markets. There is very limited evidence of violence attributed to illicit retail marijuana markets, although a recent study has found that counties bordering Mexico in states that passed MMLs have experienced a decrease in violent crime by decreasing the financial incentives of drug trafficking organizations (Gavrilova, Kamada, and Zoutman 2017; Reuter 2009). Nevertheless, any possible violence would have likely been mitigated in California given that home cultivation was allowed in most counties for many years before dispensary laws.[4]

[4] Our models will control for whether the county had any restrictions on self-cultivation.

However, the presence of dispensary store-fronts may, themselves, lead to a change in both violent and property crimes in a given area, although again the direction is theoretically ambiguous. On the one hand the sale of marijuana, even for medicinal purposes, is illegal to the federal government. Therefore, no banks with a national charter are willing to provide credit or regular services to dispensaries that sell these goods. This has meant that most dispensaries must operate on a cash-basis, and they have a lot of cash (California Police Chiefs Association 2009; McDonald and Pelisek 2009). On the other hand, dispensaries have lots of security in and around them because of their cash business and highly desirable product. They often are zoned in areas that previously had empty buildings, and so by moving into these areas and bringing their own security systems they provide more "eyes on the ground" which can deter crime.

Spatial models that measure the density of dispensaries in a given area are an effective way to test the effect of store fronts on crime, but the results from these have been quite mixed. A few correlational studies have found a positive relationship with crime either in the immediate area (Contreras 2016) or in adjacent neighborhoods (Freisthler et al. 2016). A recent study exploiting an exogenous shock that led to closings of dispensaries in Los Angeles County, though, found that these closures actually led to an increase in crime in the immediate vicinity (Chang and Jacobson 2017). The authors argue that the increase in crime was a result of a loss of "eyes on the street" being provided by the dispensaries that were forced to close. Overall, we cannot say whether we expect a change in the distribution of marijuana caused by the legalizing dispensaries to have a positive or negative effect on reported crime in California.

Because theory does not provide any clear guidance on anticipated effects of these laws, it has been viewed an empirical question. A recent set of studies examine the relationship between MMLs and crime by exploiting variation in uptake across states and using Part I reported crimes

at the aggregate level. Morris et al. (2014) and Shepard and Blackley (2016) both use a difference-in-difference methodology, with the former employing a sample of all states in the period between 1990-2006 and the latter a sample of only the eleven states that make up the Western Census Region between 1997 and 2009 .[5] Chu and Townsend (2017) adopt a similar approach while measuring crime at the city policy agency level to improve measurement, but still rely on a state-level classification of MMLs. Huber (2016) add information about whether states have depenalized marijuana to their difference-in-difference model, arguing that depenalization has an effect on non-drug crime by shifting enforcement resources. Finally, Gavrilova, Kamada, and Zoutman (2017) employ a difference-in-difference-in-difference approach to study the added effect of MMLs on crime in counties bordering Mexico compared to inland adopting counties (where MMLs are measured at the state-level). These studies have mostly found very little evidence of a relationship between uptake of medical marijuana laws and reported crime,[6] with the exception of the Huber study that estimated a 5% significant decrease in robberies, larcenies, and burglaries.

The current paper addresses important limitations of the prior studies. First, prior studies that exploit state-level policy variation assume the treatment (exposure to medical marijuana dispensaries) is homogenous across the state. It is clear from recent work that this is not the case, as many local jurisdictions choose to adopt bans on medical marijuana dispensaries (Dilley et al. 2017; Pacula et al. 2015). Crime rates are also not constant across a state, and in fact are very localized, which raises uncertainty as to whether variation in aggregated crime rates observed at the state level are being driven by the same or different jurisdictions in which medical marijuana dispensaries are allowed (Dilley et al. 2017; Freeman, Grogger, and Sonstelie 1996). Our study is

[5] They argue that because up until 2009 only four states outside of the Western Region had passed a MML, a sample of only western states provides a more similar control group.
[6] The Gavrilova study finds a significant decrease in violent crime in Mexico-bordering counties with MMLs, but a negligible insignificant effect on violent crime in inland counties with MMLs.

able to explicitly address this concern by examining more localized measures of crime and dispensaries at the county level. Second, prior state analyses frequently omit other relevant policy variables that are also changing during this time period, such as cannabis depenalization, that might also be important for determining crime and arrest rates (Huber, Newman, and LaFave 2016). Our study overcomes this limitation by exploiting variation within one very large state, thereby holding constant across our treated and control counties changes in other state-specific laws (Shepard and Blackley 2005).

2.2 California Experiment: Medical Marijuana Laws across California Counties

In 1996, California was the first state to pass a law allowing for the legal possession and cultivation of marijuana for medicinal purposes. The initiative changed a section of the Health and Safety code to protect patients who used marijuana with the recommendation from a physician from state prosecution. Passed through a ballot initiative, Proposition 215 (later to be known as the Compassionate Use Act (CUA)), did not address any channels through which marijuana might be supplied or obtained due in large part because of its explicit contradiction with federal law, which still maintained a strict prohibition on marijuana for medicinal purposes by classifying it as a Schedule 1 drug.

A few factors encouraged county and city jurisdictions in California to start crafting their own medical marijuana regulations, creating the variation over time we exploit in this study. First, the lack of specificity in the CUA regarding the production and distribution of marijuana left local governments with the authority to adopt whatever regulations they felt was appropriate, as there was no state pre-emption of any local regulations (Freisthler et al. 2016). Second, the ambiguity of the state law meant that the distribution of marijuana within the state remained illegal unless localities specifically addressed the issue.

These two factors paved the way for subsequent policy decisions since the passage of the CUA in 1996 that have affected medicinal marijuana regulation in California and solidified the role of counties and cities to create their own laws related to medical marijuana dispensaries. Statute SB 420, which provided legal protection to marijuana dispensaries operating within the state as of January 1, 2004, gave local jurisdictions the autonomy to decide whether and how to permit dispensaries. While it exempted the "collective or cooperative cultivation" from prosecution, it left it to local jurisdictions the authority to implement and regulate them (State of California. October 12, 2003, §1(b), (c)). The "Ogden memo," published in October, 2009, strengthened the incentive localities had to develop clear regulations over dispensaries, as it specified that the Federal government would not prioritize prosecuting patients or caregivers that were acting in clear compliance with state laws (Ogden 2009). As California law delegated these authorities to local jurisdictions, this memo signaled to city and county governments that local ordinances regarding dispensaries would be binding.[7] As demonstrated by the increase in counties after 2004 and 2009 with dispensary laws shown in Figure 1, the evolution of these rulings and decisions has spurred the variation in local policy that is currently observed within the state of California today.

[7] The authority of local governments to regulate dispensaries in their jurisdiction was reinforced in 2013 after the Supreme Court of California ruled in the case of *Riverside vs. Inland Empire Patients Health and Wellness Center* (56 Cal. 4th 729 [2013]) that the city of Riverside had the right to abolish marijuana dispensaries through zoning laws.

2. Data and Methods

3.1. Data

City and County Ordinance Data

This study uses a database of medical marijuana legal provisions adopted across all 58 counties of California as well its most populous 14 cities (those with a population exceeding 200,000 residents). The database is based on legal analysis of the language in the public law versions of county ordinances that were adopted between the period January 1, 1997 through December 31, 2014. The year 1997 is used as a starting point because California's statewide policy passed in November of 1996.

The legal database includes jurisdictions' (dis)approval on provisions related to the distribution of cannabis supply-related products. By December 31, 2014, 28% (16 out 58) of jurisdictions had made legally effective a county ordinance permitting co-operatives or dispensaries.[8] In order to limit the subjectivity of the database associated with subsequent implementation of the provisions and to ensure every jurisdiction's county ordinances were assessed along the same criteria, e.g. as written in public version, this study does not include successive interpretations of courts or policies established by regulatory bodies.

An indicator variable was created that is defined as "1" for counties that explicitly allow dispensaries in a given year and "0" otherwise. This is a reasonable definition because none of the state-level statutes or court rulings explicitly allow for dispensaries. Inevitably, some counties changed their policy throughout the year and we have created an annual dataset, so we use the law

[8] The peak number of dispensaries open in one year is actually 18 during 2013, but two counties stopped allowing for dispensaries the following year.

in place for the majority of the year.[9] Additionally, because home cultivation is allowed by the Compassionate Use Act, we assume that home cultivation is allowed with no limits unless explicitly stated. We create a variable that identifies whether the county has placed any limits[10] on cultivation for the given year. To make it easier to interpret along with the dispensary variable, the indicator variable is defined as "1" when there are no limits placed by the county on home cultivation and "0" when the county either explicitly prohibits or places any sort of limit on home cultivation.

A significant complication to the analysis is that a county ordinance applies to the unincorporated part of a county, which is the area of a county that does not pertain to an incorporated city. Incorporated cities may create different laws than the county they are nested in, which apply to residents within the city limits. Estimation of impacts of just county ordinances ignoring the specific ordinances of the cities incorporated within them could therefore lead to biased results. This is why in addition to the county ordinances, the research team also completed the same categorization procedure of ordinances for the 14 incorporated cities in California with a population larger than 200,000. Doing so meant that in most counties we would capture the laws that applied to the greatest share of the county population in each county.

While crime offense data are available at the level of police agencies within counties, our main analysis will be conducted at the county level because it mitigates the problem of agency jurisdiction borders and because more control variables are available at the county level. The distinction between counties and cities, however, means we have to be careful to account for the

[9] The data on ordinances is at the monthly level, but because crime offense data should be interpreted at the annual level we collapse the ordinance data annually (very little variation is lost by collapsing the ordinance data to the annual level). We also show as a robustness check the results from estimating the treatment variable as the fraction of the year in which the policy was in place (for example, 0.5 if dispensaries were allowed for 6 months.

[10] We categorize this variable as "any limits" because it is rare for counties to place limits on cultivation. Counties that place any limits, then, should be different than those that do not regulate cultivation.

treatment of cities that had laws different than that of the county (and for the possibility of differences for those cities in which we do not have information). Therefore, we construct a county-level policy indicator using the following rules: (1) the county unincorporated area policy is used if we don't have information on any city within a county, or if the city information is consistent with the county; (2) when information on a city within a county is available and contradicts that of the county policy, we use the county or city policy that applies to the larger share of the full county population.[11] As a sensitivity analysis, we collect data on police agency-level reported crime for the 14 cities and the unincorporated areas of each of the 58 counties; and run a similar analysis using ordinances for these 72 independent jurisdictions to ensure that our results are not driven by different laws within jurisdictions in a county. We also conduct robustness checks using other methods of classifying the treatment variable.

Figure 1 displays the distribution of counties in each year that have a policy that allows for dispensaries, using the policy definition described above. It shows that the change in this element of the MMLs was a gradual process that, with the exception of 2 counties (San Francisco and Santa Clara), starts in 2004 after SB420 passed. By 2010, 12 of the 58 counties allow dispensaries and in 2013 18 counties do so. Finally, by the end of our sample period in 2014, 16 of the 58 counties allow for dispensaries. One may note that the number of counties allowing for dispensaries can decrease, and this is a function of the fact that counties that allow dispensaries may either (a) include sunset provisions, or (b) subsequently pass ordinances that disallow them. Our indicator variable reflects these subsequent changes as well.[12]

[11] There are 3 counties (Alameda, San Diego, and Los Angeles) that have 2 cities within its boundaries that fit the criteria of a population of 200,000 or more. In these cases, we use the city with the larger population.
[12] This is complicated in our event study analyses, but we account for it by adding a dummy variable to the model indicating years post-treatment when the county no longer allows for dispensaries.

Crime Incident Data

The second source of data we have compiled for use in this study are the number of total offenses reported to police by type of crime, for each county and each year in our sample period. The data on reported offenses for the seven types of index crimes are pulled from the State of California Department of Justice (Criminal Justice Statistics Center 1997-2014) website. The California DOJ publishes raw county-level data[13] from the information it receives from each police agency. We also create a variable for violent offenses that corresponds to the Uniform Crime Report (UCR) Part I violent crimes: homicide, rape,[14] robbery, and aggravated assault; and property offenses refers to UCR Part I property crimes: burglary, larceny/theft,[15] and motor vehicle theft.

Since the UCR is based on the Hierarchy Rule, only the most severe crime is counted per incident. The importance of this for our purposes is that if marijuana has an effect on the severity of crimes, we may observe this as a change in crime; although no change in the actual number of incidents. For example, where two offenses (e.g. aggravated assault and theft) occurred during an incident; this incident will be recorded as an aggravated assault. If marijuana results in a fall in pharmacological crime (such as aggravated assault), but still affects economic-crimes (theft), we would observe a decrease in aggravated assault and an increase in thefts. While in this scenario the former is true (there is indeed a fall in assault), the latter is not true; the offense of theft occurred in both incidents.

[13] Raw data means that no imputation procedures are used to account for possible missing values. California does not conduct a state-wide version of the National Crime Victimization Survey, meaning that reported crime-offense reports is the only source for measuring the level of crime.

[14] We don't show results for effects on rape because there is no reason to believe dispensaries would have an effect and agencies in California were allowed to start using the new expanded definition in January 2014.

[15] Larceny/theft includes both felony and misdemeanor crimes. The classification for felony theft in California changed in 2011 and it was not possible to go back and re-classify all previous felony offenses into misdemeanors, so the state decided to include all larceny and theft crimes, regardless of monetary value, under felony property crime.

Final Dataset

We have created a dataset of aggregate crime by year and county and merged it with the ordinance data to create a panel dataset from 1997 to 2014. Figure 2, Panels A, B, and C, track how reported violent offense, property offense, and DUI arrest rates, respectively, have changed over our sample time period for counties that started allowing dispensaries at any period between 1997 and 2014 and those that never allowed dispensaries during that same period. The vertical axes signify the two years (2004 and 2009) that begin an "episode" when more counties start entering the treatment group, as well as a year (2011) when various important state criminal justice policies are passed.[16] The first important pattern to note is that crime has decreased in the state as a whole since 1997, regardless of whether dispensaries were allowed. This downward trend in crime per capita is consistent with the decline in crime that has been observed throughout the United States. Secondly, it appears that there are parallel trends throughout most of the pre-treatment time period between the two groups of counties, with the exception that counties that never allowed dispensaries had a larger decline in violent and property offense rates between 1999 and 2001 than counties that allowed dispensaries at any point in time, and DUI arrests between the two groups start to converge around 2009. Nevertheless, because jurisdictions start allowing dispensaries at different times, it is difficult to draw conclusions about the relationship between dispensaries and crimes from these broad state trends.

We also collect data on variables at the county level that have been shown in the literature to influence the crime rate. These variables include the one-year lagged unemployment rate (Raphael and Winter-Ebmer 2001), the average per capita income, the density of alcohol outlets per capita (Gruenewald and Remer 2006), and the county population density (Shepard and

[16] These include decriminalization of marijuana as well as AB 109, a major policy that led to a shift in resources among all law enforcement agencies.

Blackley 2005). Lastly, we include an indicator for 2011 and later, the year that California both decriminalized recreational marijuana use and substantially changed its criminal justice system through a process that has been termed "Public Safety Realignment." The unemployment rate comes from the Bureau of Labor Statistics, the per capita income from the Bureau of Economic Analysis, the alcohol outlets from the California Department of Alcohol and Beverage Control, and the land area and population from the United States Census Bureau.

Table 1 shows the summary statistics for the different types of crime we are analyzing and the independent variables used in our model. One will note that most of the total property crime is made up of larceny/thefts and most of the total violent crime is made up of aggravated assaults.

3.2 Empirical Strategy

To test whether allowing dispensaries affects aggregated criminal activity, we analyze the impact of local dispensary laws on UCR reported violent and property crime rates as well as DUI and marijuana-related arrest rates.[17] Using the county-year as our unit of analysis, we will capture the effect from a change in dispensary allowance through a staggered difference-in-difference approach. Counties become part of the treated group at different times as they adopt laws throughout our sample period, and the changes resulting from adoption are compared to a control group that never adopts. All our model specifications include county fixed effects, as we are confident that there are unique unobservable county characteristics, which may cause a spurious correlation between crime rates and policy adoption. To account for the fact that there are trends in crime and arrest rates that are common across counties, we also include in the model a continuous (annual) time variable and a second order term. We choose this specification, over the

[17] DUIs include driving under the influence of any substance that may impair driving, so driving under the influence of marijuana is included in these figures..

more common method of including year dummy variables, to preserve more power after observing a clear quadratic trend in all crime.[18] Finally, we control for various time-variant county characteristics, described in the previous section, that may be correlated with both changes in crime and a county's propensity to adopt an ordinance allowing for dispensaries.

Our preferred specification is one that also adds county-specific time trends to the model. If counties across the state had differing pre-treatment trends, this specification helps create a better fit of the data. Studies that examine crime as an outcome across states, including in the MML literature, have argued for the inclusion of these jurisdiction-specific trends (Chu and Townsend 2017; Gavrilova, Kamada, and Zoutman 2017; Raphael and Winter-Ebmer 2001). California is very diverse with counties that differ in economic, political, and demographic characteristics; creating differences in crime trends one would usually associate with states. Moreover, there were differential impacts of the Great Recession and Public Safety Realignment across counties because of these different characteristics, resulting in differential crime trends that we can see when we look at counties individually.

The model specification is represented by the equation

$$\log(y_{ict}) = \alpha + \delta D_{ct} + \rho Cult_{ct} + \beta X_{ict} + \alpha_c + \omega Time_t + \tau Time^2{}_t + f_{ct} + \varepsilon_{ict}$$

where y_{ict} represents the logarithm of the reported crimes per 100,000 residents[19] of crime type *i* for county *c* in year *t.* Our main treatment variable is represented by D_{ct}, an indicator for whether county *c* in year *t* allows for dispensaries. α_c controls for the county-specific variation, *Time* and *Time*^{2} control for state trends over the study's time-period, and f_{ct} accounts for the county-specific trend (we will show results based on different functional forms used to model the trend). *Cult*

[18] We run all the models including year dummies as well to ensure that the coefficients are not affected by this choice.

[19]We ran a variety of tests for model fit and found that this model best described the data generating process. Additional models were also tested and available upon request.

controls for whether the county had a restriction in place on amount or location with regards to cultivation and X_{ict} represents a vector of county time-varying covariates that have been shown to be associated with crime rates in the literature.[20] The coefficient of interest, δ, estimates the average effect in reported offenses for counties that allowed dispensaries compared to those that did not.[21] Finally, our models are robust to clustered standard errors.

A primary assumption in the difference-in-difference methodology is that of pre-policy parallel trends in outcomes, or that there are no variables in the error term correlated to the outcome as well as the decision for a jurisdiction to adopt a dispensary policy. If this type of policy endogeneity were occurring or if pre-policy trends in crime between the treated and untreated groups differed for other reasons, we'd expect the trend for policy-adopting jurisdictions to change before the passage of an ordinance, leading to a biased coefficient of the treatment variable. One advantage from our technique is that the treatment is staggered over time, mitigating the probability that something happened at the state level that affected both crime and county-specific entry into treatment. Moreover, as ordinances are legislative processes, it is likely that many factors are attributable to the passage that have nothing to do with changes in crime (Williams and Bretteville-Jensen 2014). Finally, dispensaries were adopted by large and small, urban and rural counties, which mitigates the concern that counties adopting dispensaries are inherently different.

As a check that the parallel trends assumption holds and to explore possible dynamic effects of treatment, we complement our average effect model with an event study. The event study

[20] The covariates used are: An indicator for when California decriminalized marijuana starting in 2011, the density of alcohol outlets in the county, log of per capita income for the county, the lagged unemployment rate in the county, and the log of the population density in the county.

[21] While there is a wide range in populations and urban density in counties across California, which could lead to variance in the error term that is not constant across observations, we decided not to incorporate a weighted least squares regression. Models incorporating a WLS regression, testing various weights, did not improve estimates for homoskedasticity, so we did not feel that we fully understood the structure of the variance component to properly adjust for it. Results from these tests are available upon request.

disaggregates D_{it} into a set of dummy variables indicating whether a county-year observation represents a certain number of years before or after treatment. The model specification will be the same as that shown above, to account for other state and county characteristics and trends that affect county-specific crime rates.

The event study allows for identifying potential endogeneity if there are significant effects in the years leading up to policy adoption. For example, it picks up effects from the years preceding the passage of the law if suppliers sense that the county legislature or law enforcement are amenable to dispensaries and start to operate before an ordinance is officially put in place. The event study model also addresses a limitation from our model in which the average effect may mask differences in the development stage of dispensary operations after implementation (Meer and West 2015). It may be the case that there is a lag in observed effects as development of dispensaries takes place in the first few years.

3.3 Local Jurisdictions and Sensitivity Checks

As mentioned in the section describing the data, the analysis incorporates a measure of crime and arrests at the county level even though it is not always the case that a dispensary ordinance applies throughout the entire county. We address this issue by also running our difference-in-difference model at the ecological level of police-agency jurisdictions, where the reported crime rates should reflect 100% of the geographical area defined by our treatment variable. We collected agency-level data on the reported offenses by crime type from the UCR database for each of the 14 cities for which we have ordinance information and for the unincorporated areas of each of the 58 counties. For the cities, we use offense data that are reported by the police department of the city (e.g. reported offenses according to the Los Angeles Police Department to measure crime in Los Angeles City). For the unincorporated parts of each county,

we use offense data reported by the Sheriff's department of the county. This leaves us with crime rates from a total of 72 independent jurisdictions, which match the 72 jurisdictions for which we have information on ordinances regarding allowance for dispensaries.

We follow the same empirical model from the main analysis, where the explanatory variable of interest is now an indicator for whether dispensaries are allowed in each jurisdiction, for the 72 independent jurisdictions over the 18-year sample period. One complication of running the analysis at the level of individual police-agency jurisdictions is that the covariates used in the regressions from the previous section are not available at this geographic level. Nevertheless, this should not affect the results because the variations across years within jurisdictions for variables that affect crime rates are minimal and are mostly absorbed by the controls that exploit the panel data structure. In the current model, we incorporate dummies indicating the independent jurisdictions to control for unobserved heterogeneity across individual jurisdictions and the same continuous time variables as above to control for state-level changes. Lastly, we estimate standard errors robust to clustering at the county-level, as even across two independent agencies, there may be correlation within the same county.

We also apply other sensitivity checks that address less serious, but important, concerns. First, we present results removing certain counties that may be different than the rest because they adopted a dispensary policy very early, even before the passage of SB 420. Second, we conduct robustness checks related to the issue of differential city ordinances contained within a county by estimating the model using other methods to choose the applicable ordinance for the jurisdiction. Third, we address the difficulty in properly identifying the amount of time during a year in which the policy was active by presenting results of a model measuring the main treatment variable using fractional years based on the month that the policy went into effect. Finally, we estimate a model

with a sample consisting of only control counties and treated counties with a policy in place five or more years. Similar results to the main analysis would point to consistent effects on crime over the treatment period.

3. Results

4.1 County Level Crime and Arrests

We present in Table 2 the results of the average effect on overall violent and property crime based on specifications with no county-specific time trend (columns (1) and (4)), as well as with county-specific trends using linear (columns (2) and (5)) and quadratic functional forms (columns (3) and (6)).[22] The first important result to observe is that our estimates are sensitive to an inclusion of the county-specific time trend, as it leads to an increase in the magnitude of the coefficients for both overall violent and property crimes. For property crimes, it changes a roughly zero effect size to at least a partially significant coefficient. The choice of functional form for the county-specific trends is less important, with coefficients that are roughly similar across the different specifications. As we found differential property crime trends in some counties in supplemental analyses (not reported here), we have greater confidence in models that adjust these series for the county-specific time trend.

We find no significant impact of dispensaries on violent crime in any of our models. Table 3 shows that even when we disaggregate by crime type, none of the violent crimes (columns 1-3) are affected by dispensary laws. The consistency of findings regardless of inclusion or exclusion

[22] We also ran the model using a cubic functional form for the county-specific time trend, but don't show the results for simplicity, as they are very similar to the quadratic functional form model.

of the county-specific time trend is reassuring, but not surprising in light of the more consistent trends observed across counties in these measures.

For property crimes, we see no effect from adopting dispensaries in the model excluding county-specific time trends. However, the model incorporating a linear trend shows a 5.1% statistically significant drop in reported property offenses during the years in which counties allowed for dispensaries, while the quadratic specifications shows a 6.3% decrease that is statistically significant. Further decomposing these results, Table 3 shows that the effect on property crime appears to be driven by a decrease in thefts.

Only a brief discussion of the other covariates is warranted. As previously mentioned, due to very lax regulations on cultivation, the variable for cultivation regulations only measures whether there were any explicit limits set by a county. While Table 2 does show a sharp drop in violent crime of almost 10% in counties that didn't restrict cultivation, when county-specific trends are not included, this relationship becomes insignificant with the inclusion of time trends. The effects from the other covariates included in the model are difficult to interpret due to limited variation once the fixed effects and time trends are controlled for.[23]

Table 4 shows results for the effects on variables that may be informative with regards to marijuana (mis)use. We see a very strong and robust effect on DUI arrests, as adopting dispensary laws was associated with at least a statistically significant 7.7% increase in DUI arrests. [24] This effect increases when county-specific time trends are included in the model, with the preferred specification indicating a significant increase in DUIs of 9.1%. As DUIs in California (at least

[23] Supplemental analyses not shown here reveal that nearly all of the variation in our other descriptors (more than 90%), with the notable exception of unemployment, can be captured by fixed effects and county-specific time trends.

[24] While the increase in DUIs may be a result of changes in enforcement in counties that allowed for dispensaries, it is unlikely that there is a high correlation between the timing of dispensary laws and changes in DUI enforcement. Many factors impact enforcement, and cultivation of marijuana was allowed in almost all counties well before dispensaries opened (Williams and Bretteville-Jensen 2014).

during the study period) apply to any substance use, this increase may have been a result of more marijuana-impaired driving arrests. This is equivalent to 65 more DUI arrests per 100,000 residents on average per year,[25] as a result of dispensaries. Arrests for felony and misdemeanor marijuana arrests are noisy due to important changes across the state that led to an overall large drop in both types of arrests statewide. Our results demonstrate a significant increase in misdemeanor arrests with our preferred specification, though, which does reinforce the evidence of possible increases in marijuana misuse.

The event study analysis results, demonstrated in the panels in Figure 3 where the graphs show the effect of each individual year relative to the passage of a law, can help in interpreting the results described above. Note that the sample is not perfectly balanced; many counties adopted dispensary laws later in the sample period so they did not have as many years of post-treatment observations. The tails in the figures below, the values -3 and 4 on the x-axis, represent dummy variables that incorporate all the years before or after, respectively, relative to the year of adoption (0 value on the x-axis).

Panels A of Figure 3 show that for overall violent crime, the failure to observe an effect is not due to a violation of the parallel trends assumption. The effect sizes of for violent crime consistently include 0 in both the pre- and post-policy periods and do not demonstrate any clear trends. Panels B, C, and D, on the other hand, demonstrate pre-existing trends for property crime and DUI and misdemeanor marijuana arrests. Moreover, it appears from the left tail of the figures that, historically, counties that adopt dispensary laws have higher property crime rates and lower DUI and misdemeanor arrests than non-adopting counties, and that regression towards the mean was occurring before dispensaries were allowed. Possible policy endogeneity makes it difficult to

[25] We took the average across non-adopting years for counties that would eventually adopt dispensaries for this calculation because counties that adopted dispensary laws had lower DUIs on average (see Figure 2).

measure the magnitude of any possible overall and dynamic effects, though the trends continuing past zero, even if not significant, point to the significant effects shown in Table 2.

4.2 Sensitivity Checks

In this section, we show the results from a variety of sensitivity checks that account for limitations to our main analysis. Each row in Table 5 shows the coefficient for the "allows dispensary" variable of a different analysis, with regressions run for property and violent crime, as well as DUIs, presented in the columns. We show these three outcomes because our main analysis has not demonstrated any significant effects on specific types of these crimes, with the exception of theft, which seems to track the property crime variable.[26][27]

The results for our first sensitivity check, shown in the first row of Table 5, represent the average effect of allowing dispensaries when variables are measured at the police-agency jurisdiction level. We see that the coefficient magnitudes are similar to those of the main analysis, even though the DUI arrests and property crime variables are no longer significant. This may occur because our new unit of analysis is smaller, leading to more variation from year to year and noisier data. Overall, these results do not contradict those of the main analysis.

In the next two rows, we check for whether how we define the treatment variable changes our findings. "Unincorporated County" means that we identify the treatment based only on the county (i.e unincorporated part of the county) law even if a city exists within the county with a different law, and "City Always" defines a variable that uses the city law (if available) to identify treatment regardless of whether the unincorporated population is larger. The following row shows the results of a model allowing for the treatment variable to be a fraction if an ordinance was passed

[26] We also ran these models on theft crimes and find similar results to those shown for property crime.
27 We use county-specific time trends instead of agency-specific because county rates should have smoother trends. This decision has no impact on the results shown.

after January of that year. The two rows labelled "No San Francisco" and "No Santa Clara" show the results of analyses that exclude each of these counties. These two counties adopted dispensaries very early on, even before the enactment of SB 420, which might indicate something unique about them.[28] Moreover, given the changing trend in crime over our sample period, the timing of their "post-intervention" may impact the results (even after adjusting for county-specific linear trends) in addition to the higher leverage demonstrated by San Francisco due to it experiencing more years of treatment. Finally, the last row presents the effects of dispensary laws when we restrict the treatment sample to counties with laws for five or more years. All of these sensitivity checks point to the same findings as our main analysis, indicating a significant increase in DUI arrests and decrease in reported property crime offenses. While the analysis excluding San Francisco leads to an insignificant coefficient for property crime, it is still negative and similar in magnitude to the other models.

4. Discussion

California is experimenting with opening recreational marijuana retail stores, which will make it the largest state (in population and size) to do so. Again, localities will get to decide where and how many stores are allowed to open in each of their jurisdictions. Insights from the opening of medical marijuana dispensaries may be useful for better understanding the likely impacts of opening these recreational stores, and could serve to help police agencies and the courts and correctional systems prepare.

This study improves upon the work conducted thus far evaluating the impact of retail medical marijuana stores on crime. We use a novel longitudinal local ordinance database that allows us to assess the extent to which types of violent, property, and substance abuse crime rates

[28] In fact, Santa Clara County is unique in that it stops allowing dispensaries to operate after 3 years and then adopts a new ordinance allowing for dispensaries in 2011.

are impacted over time with the decision by local jurisdictions to allow dispensaries to open. Consideration of local variation within a state where substantial differences exist in allowances is crucial but had been previously ignored in the literature. Moreover, by examining variation within a single state, we can account for important statewide changes that are also important for driving marijuana use and potentially crime, including rules related to cultivation and decriminalization.

Evidence from our statistical analysis of a quasi-experimental setting finds no impacts on any type of violent crime, although counties adopting local ordinances did potentially experience a small decrease in property crime and increase in DUI arrests. Due to evidence of pre-existing trends, it is not possible to make a conclusive statement about the magnitude of these effects.

Our study is not without its own limitations, however. A clear problem is that our policy indicator is not capturing the actual exposure to the law for the residents in a county, since cities within counties can adopt conflicting ordinances. Our analysis at the police-agency level suggests that, at the very least, we are not missing increases to reported crime due to incongruence in treatment exposure. It also does not provide enough evidence to refute our findings of increases in DUI arrests. Moreover, when we measure our treatment variable using two alternative methods, we find similar results.

Second and relatedly, our study does not empirically assess the impact of having many versus few dispensaries within a jurisdiction (i.e. the "intensive margin"). Studies focusing on dispensary density and crime in the immediate vicinity, though, have not been much more definitive, finding no effect on any crime (Kepple and Freisthler 2012), a negative relationship with property crimes (Chang and Jacobson 2017), and small increases on property and violent crimes in adjacent areas (Freisthler et al. 2016). We do know that within California, counties differed substantially in their approach to dispensary allowances, with some jurisdictions

significantly limiting the total number of dispensaries allowed from the beginning and others not imposing any thresholds until much, much later. The lack of annual store-front data (pertaining to density) makes it more difficult to interpret dynamic effects, as there is scant research on the length of time we should expect for dispensary laws to be fully implemented within a jurisdiction and whether there are threshold effects in terms of total number of open dispensaries. There is also little to no information about delivery services, and laws associated with delivery services. The impact of delivery services may cause property crimes to rise in areas outside of the immediate vicinity of the dispensaries, thereby influencing property theft crimes in jurisdictions outside of those choosing to adopt the policy.

Third, a significant limitation in all difference-in-difference analyses is that there is no direct mechanism to test whether the treatment variable is correlated to an unobserved variable that affects the outcome, leading to a violation of the parallel trends assumption. We performed an event study analysis that did not refute our conclusions in the case of violent crimes, although there was evidence of policy endogeneity for property crime and DUI arrests. Until the policy endogeneity is explicitly addressed, the magnitude of the true effect on these outcomes cannot be easily determined.

Our study appears to reinforce the conclusions from other studies that fail to find an increase in the type of crime predicted by law enforcement. We find no effects on burglary, robberies, or assaults, which are the types of crimes one would expect if dispensaries were prime targets as a result of their holding large amounts of cash. It is important to note, though, that it may merely be the case that crime is such a localized effect that there is too much variation even within our treatment exposure aggregated to the city or county level (Hipp 2007). Pertaining to our findings of potentially decreasing property crime rates, there is a theoretical reason for why

dispensary store-fronts may decrease crime. Dispensaries may open in otherwise desolate areas, creating foot traffic, or "eyes on the street," that makes these areas safer (Chang and Jacobson 2017).

We do find some interesting preliminary results with respect to the relationship between dispensaries and DUIs. Anderson, Hanson, and Rees (2013) find that MMLs in Colorado led to a substitution away from alcohol use, but the potential positive relationship between dispensaries and DUI arrests we find in our analysis suggests that either increases in marijuana-impaired driving exceeded reductions in alcohol impaired driving (a hypothesis we find highly unlikely) or that the opening of dispensaries induced use of both substances among those who were willing to drive impaired (more likely). The latter interpretation would have important ramifications for crime rates, given the known association between using alcohol together with other illicit substances and violent behavior (Office of National Drug Control Policy 2013). It is possible that our null results mask an increase in violent crime due to concurrent use of marijuana and alcohol, which is being offset by other mechanisms such as a decrease in pharmacological crimes due to an increase in marijuana use alone.

As we can only measure an aggregate effect, future research should attempt to tease out the effects on crime due to different mechanisms and actions of local actors. While some attributes of dispensaries may have led to a reduction in crime compared to the status quo, other aspects may have promoted crime. Moreover, the effect on crime rates will depend on other actions taken on by the local policymakers, dispensary owners, and law enforcement. For example, dispensaries may have adopted home delivery methods, which would reduce the potential number of victims near dispensaries. There may have also been specific actions taken by police that prevented an increase in crime rates, and these should be identified. Further research that identifies elements of

MMLs along with more specific aspects of implementation can help policymakers respond with actions that address crime-promoting aspects of allowing for retail dispensaries.

Our findings indicate that policymakers should be careful in how they regulate the presence of dispensaries, while not jumping to the conclusion that dispensaries are clearly crime generating hot-spots. Similarly, while police are right to be wary about potential crime effects from the introduction of cash-dependent businesses, our results demonstrate that current policy has not led to a wave in crime (even if this may be due to actual police practices). Our findings suggest that it is possible to regulate these markets and find a common ground between safety and access to medical marijuana. Natural experiments like the one being undertaken in California will only further help researchers better understand exactly how to find this ideal common ground.

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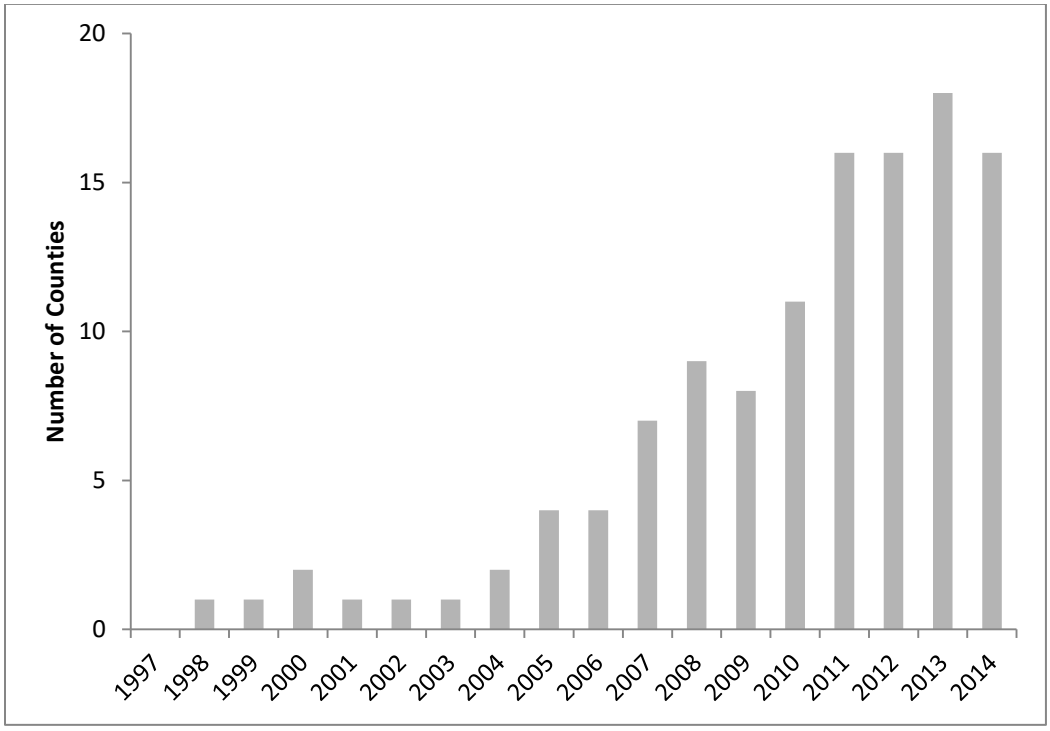
6. Figures

Figure 1: Number of Counties that Allow Medical Marijuana Dispensaries, by Year



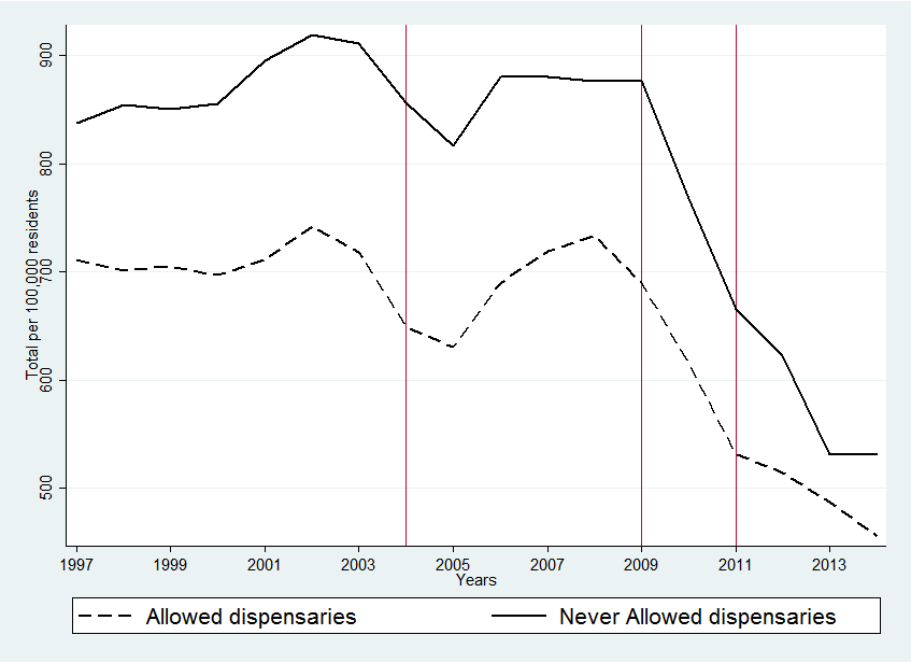
Note: This figure represents the method of using a county's ordinance unless there is a city in the county that has the largest share of the population in the county.

Figure 2: Crime Rates per 100,000 residents, by Whether County Ever Allows Dispensaries

Panel A: Total Violent

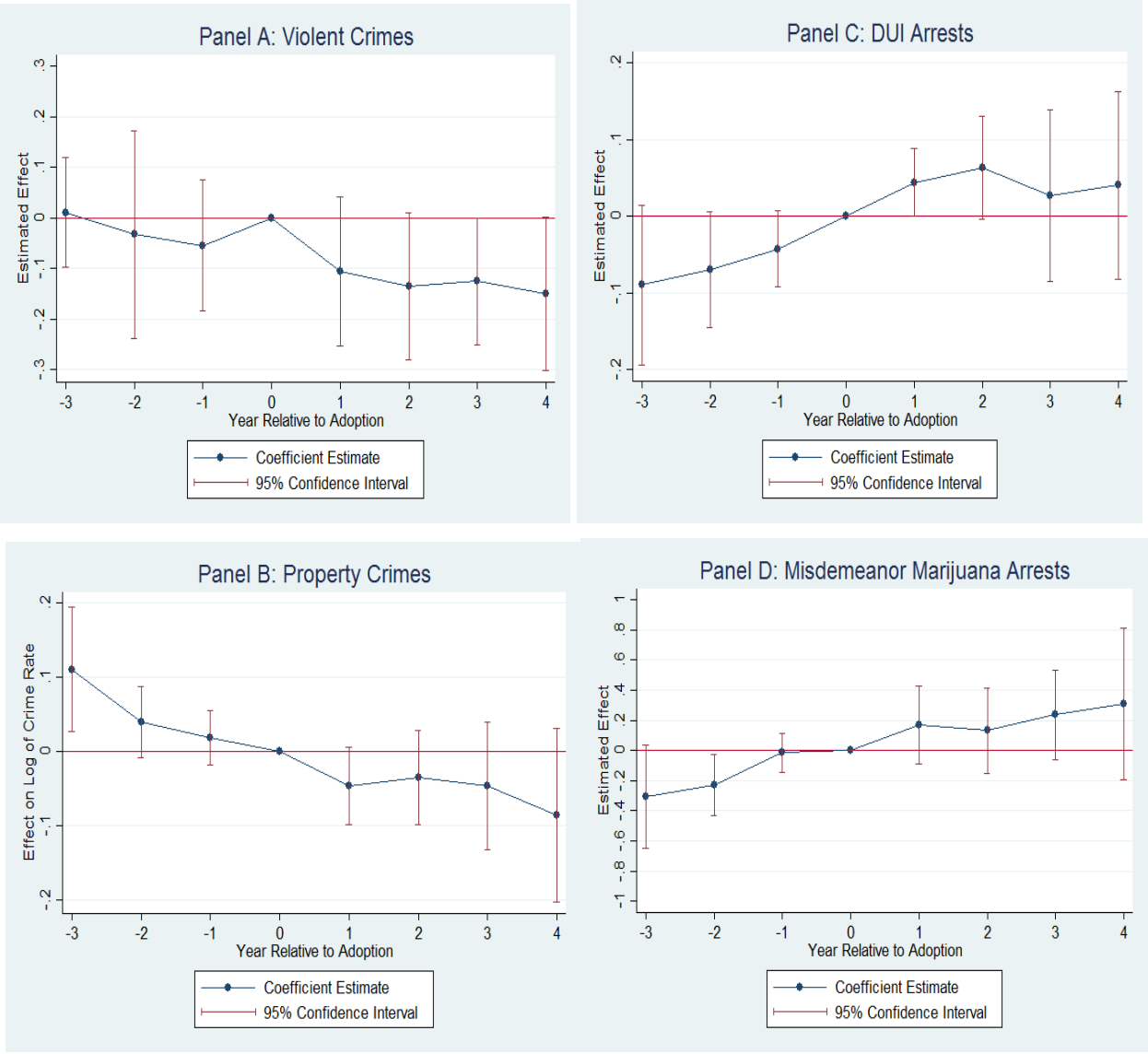
Panel B: Total Property

Panel C: DUI Arrests



Notes: The dashed line represents the counties that ever allow dispensaries in the sample period. The solid line represents counties that up until the end of the sample period, had never allowed dispensaries. The vertical lines represent the years 2004 and 2009 because these were important transition years.

Figure 3: Event Study



Notes: "Year 0," indicating year when policy adopted, is omitted from the regression to provide an excluded category. Counties never adopting a law have a 0 for all indicator variables. Dummy variables for 3+ years pre- and 4+ years post-intervention are represented by -3 and 4, respectively. Regressions also include the covariates from the model described in Section 3.2. An indicator variable in the model controls for county-year observations for periods after an adopting county no longer allows for dispensaries.

7. Tables

Table 1: Summary Statistics

	Mean	Standard Deviation
All Violent Crime Rate	433.3	156.3
Homicide Crime Rate	4.3	2.2
Robbery Crime Rate	85.7	82.7
Assault Crime Rate	312.7	109.3
All Property Crime Rate	2,866.1	980.0
Burglary Crime Rate	735.8	225.8
Theft Crime Rate	1,768.1	695.8
Motor Vehicle Theft Crime Rate	362.3	219.1
DUI Arrests	749.4	346.1
Felony Marijuana Arrests	59.5	61.0
Misdemeanor Marijuana Arrests	140.6	110.7
Alcohol Outlet Density	32.4	27.6
Per Capita Income	34,852.5	11,069.5
Unemployment Rate	9.0	3.1
Population Density	659.1	2,298.9

All rates are calculated per 100,000 residents. Alcohol outlet density is calculated as the number of outlets per 10,000 people in the county. Population density is calculated as the number of people per square mile of land area in the county. All violent crime rate includes rape crimes even though we do not study the effect on rape crimes alone.

Table 2: Effect of Allowing Dispensaries on Violent and Property Crime Rates

	Violent Offenses			Property Offenses		
	(1)	(2)	(3)	(4)	(5)	(6)
Allows Dispensaries	0.006	-0.020	-0.013	0.008	-0.051	-0.063*
	(0.065)	(0.071)	(0.064)	(0.029)	(0.025)	(0.019)
Cultivation-No Limits	-0.096	-0.047	-0.048	-0.044	-0.056	-0.049
	(0.043)	(0.054)	(0.055)	(0.037)	(0.038)	(0.037)
Alcohol Outlet Density	-0.000	-0.000	0.000	0.001	-0.000	-0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Post CJ Reforms	-0.137*	-0.154**	-0.151**	0.081*	0.084*	0.072*
	(0.042)	(0.044)	(0.043)	(0.026)	(0.025)	(0.026)
Population Density	-0.871	1.495	1.161	-0.072	0.912	1.591**
	(0.432)	(1.292)	(0.894)	(0.197)	(0.630)	(0.416)
Per Capita Income	-0.300	0.043	-0.139	-0.209	-0.122	-0.026
	(0.364)	(0.267)	(0.301)	(0.296)	(0.182)	(0.158)
Unemployment Rate	0.003	0.004	0.006	0.002	0.001	0.004
	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
R^2	0.22	0.43	0.41	0.52	0.66	0.67
County-specific trends	None	Linear	Quadratic	None	Linear	Quadratic

+ $p<0.0167$; * $p<0.0083$; ** $p<0.00167$ (p-values are adjusted for multiple testing using the Bonferroni adjustment for 6 tests)

Outcome variable is in log scale of the per/100,000 population. All models include county fixed effects and a continuous time trend (including 2nd order term). Standard errors (in parentheses) are adjusted for clustering within counties. The county ordinance is based on the ordinance of the major city when that city has a population greater than 200,000 and a population greater than the unincorporated area. Each regression has 1,044 observations and covers the period from 1997-2014.

Table 3: Effect from Allowing Dispensaries, by Crime Type

	Homicide (1)	Robbery (2)	Assault (3)	Burglary (4)	Theft (5)	MV Theft (6)
Allows Dispensaries	0.249	0.299	-0.047	-0.016	-0.060*	-0.098
	(0.185)	(0.247)	(0.077)	(0.026)	(0.029)	(0.075)
Cultivation-No Limits	-0.449	-0.064	-0.027	-0.033	-0.078+	-0.081
	(0.330)	(0.167)	(0.063)	(0.045)	(0.041)	(0.121)
Alcohol Outlet Density	-0.016	-0.001	-0.000	0.000	-0.000	-0.004
	(0.013)	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)
Post CJ Reforms	0.129	-0.090	-0.134*	0.002	0.104**	0.263
	(0.292)	(0.180)	(0.054)	(0.035)	(0.031)	(0.176)
Population Density	6.447	8.287+	1.681	-0.650	1.176*	-2.146
	(4.161)	(4.306)	(1.501)	(0.829)	(0.508)	(3.986)
Per Capita Income	-0.263	-1.616	-0.045	0.016	-0.282	0.120
	(3.372)	(1.061)	(0.298)	(0.221)	(0.258)	(1.274)
Unemployment Rate	-0.001	-0.006	0.003	0.012**	-0.002	0.001
	(0.050)	(0.020)	(0.005)	(0.004)	(0.003)	(0.020)
R^2	0.08	0.11	0.46	0.51	0.64	0.09

+ $p<0.1$; * $p<0.05$; ** $p<0.01$

Outcome variable is in log scale of the per/100,000 population. All models include county-specific linear time trends, as well as fixed effects and a continuous time trend (including 2nd order term). Standard errors (in parentheses) are adjusted for clustering within counties. The county ordinance is based on the ordinance of the major city when that city has a population greater than 200,000 and a population greater than the unincorporated area. Years since adoption is calculated from January of the first year dispensaries were explicitly allowed. Each regression has 1,044 observations and covers the period from 1997-2014.

Table 4: Effect of Allowing Dispensaries on Marijuana-Related Arrests

	DUI Arrests			Felony Marijuana			Misdemeanor Marijuana		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Allows Dispensaries	0.077*	0.091*	0.088**	-0.022	-0.015	-0.049	0.087	0.215	0.125
	(0.024)	(0.026)	(0.025)	(0.092)	(0.083)	(0.084)	(0.120)	(0.106)	(0.080)
Cultivation-No Limits	0.034	0.070	0.085	-0.113	-0.135	-0.090	-0.383	-0.241	-0.095
	(0.032)	(0.039)	(0.038)	(0.106)	(0.100)	(0.092)	(0.174)	(0.133)	(0.098)
Alcohol Outlet Density	0.001	0.001	0.000	0.002	-0.000	0.001	0.003	-0.002	-0.003
	(0.001)	(0.001)	(0.001)	(0.004)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Post CJ Reforms	-0.198**	-0.195**	-0.204**	-0.337**	-0.434**	-0.429**	-1.628**	-1.666**	-1.714**
	(0.030)	(0.032)	(0.033)	(0.061)	(0.089)	(0.102)	(0.163)	(0.187)	(0.161)
Population Density	-0.657*	-1.715	-1.701**	-0.064	-5.463	-1.413	2.343	-0.341	-2.521
	(0.226)	(0.817)	(0.463)	(0.622)	(2.955)	(1.560)	(1.217)	(1.704)	(1.522)
Per Capita Income	-0.177	-0.151	0.042	1.075	2.611	2.608	-3.052	-2.206	-0.679
	(0.146)	(0.206)	(0.231)	(0.785)	(1.570)	(1.773)	(2.784)	(2.377)	(1.599)
Unemployment Rate	-0.003	-0.003	-0.002	0.033	0.053*	0.048	-0.010	-0.001	0.003
	(0.004)	(0.005)	(0.005)	(0.014)	(0.018)	(0.019)	(0.018)	(0.019)	(0.015)
R^2	0.47	0.58	0.57	0.03	0.16	0.15	0.55	0.66	0.68
County-specific trends	None	Linear	Quadratic	None	Linear	Quadratic	None	Linear	Quadratic

+ $p<0.0111$; * $p<0.0055$; ** $p<0.0011$ (p-values are adjusted for multiple testing using the Bonferroni adjustment for 9 tests)
Outcome variable is in log scale of the per/100,000 population. All models include county fixed effects and a continuous time trend (including 2nd order term). Standard errors (in parentheses) are adjusted for clustering within counties. The county ordinance is based on the ordinance of the major city when that city has a population greater than 200,000 and a population greater than the unincorporated area. Each regression has 1,044 observations and covers the period from 1997-2014.

Table 5: Sensitivity Checks

	Violent Offenses (1)	Property Offenses (2)	DUI Arrests (3)
City Agencies	-0.036	-0.042	0.160
	(0.067)	(0.026)	(0.145)
Unincorporated County	-0.027	-0.068+	0.056
	(0.085)	(0.031)	(0.038)
City Always	-0.034	-0.054	0.090**
	(0.075)	(0.026)	(0.028)
Partial Year	-0.022	-0.052	0.099**
	(0.088)	(0.031)	(0.030)
Removing San Francisco	-0.008	-0.036	0.097**
	(0.074)	(0.022)	(0.027)
Removing Santa Clara	-0.033	-0.059	0.087*
	(0.081)	(0.028)	(0.030)
Only Counties 5+ years	-0.170+	-0.088	0.089
	(0.069)	(0.044)	(0.041)

+ $p<0.0333$; * $p<0.0067$; ** $p<0.0033$ (p-values are adjusted for multiple testing using the Bonferroni adjustment for 3 tests)

City Agencies refers to using city agency crime rates and laws. Unincorporated County refers to analysis using laws according to county, even when larger cities exist. City Always refers to analysis always choosing law applicable to city within county if available. Partial Year refers to using fractions for treatment variable, in case a law was passed after January. Removing San Francisco, Removing Alameda, and Removing Santa Clara refers to excluding each of these counties, individually, from the analysis. Only Counties 5+ Years refers to analysis with sample of non-adopters and counties that had law in place 5+ years.

Outcome variable is in log scale of the per/100,000 population. All models include county fixed effects, a continuous time trend (including 2nd order term), and county-specific linear time trends. Standard errors (in parentheses) are adjusted for clustering within counties. Each regression (except City Agencies) has 1,044 observations and covers the period from 1997-2014