Displacement in the Criminal Labor Market: Evidence from Drug Legalizations*

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Abstract

It is widely hypothesized that legalization policies disrupt illicit markets, thereby reducing crime. Little is known, however, about what happens to illegal suppliers displaced from the legalized markets. In this paper, I use comprehensive administrative records on crime to study the effect of marijuana legalization on the subsequent criminal activity of convicted dealers. I find that marijuana legalization increased the 6-month recidivism rate of marijuana offenders by 9.4 percentage points relative to a baseline rate of 10.2 percent. The results are not explained by changes in police enforcement. Rather, the increased recidivism is driven by substitution to the distribution of other drugs, which is consistent with a Becker-style model where individuals develop human capital specific to the drug industry. To learn about the potential mechanisms behind these results, I use transaction and geolocation data to estimate the effect of legalization on price levels and market structure. I provide suggestive evidence consistent with the notion that legalization increased the competition in the marijuana market. Overall, the results in this paper suggest that an unintended consequence of selective legalization is a reallocation of drug criminals to other illicit activities.

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"I've got an out, an amount I'm shooting for, but time is running out. The margins get thinner every year. The shifting legal landscape is destroying the margins."

–Your Friendly Neighborhood Drug Dealer, *The Atlantic*

1 Introduction

Since 1971, the United States government has spent over $1 trillion financing policies designed to eliminate the production, distribution, and use of illicit drugs (Coyne and Hall, 2017). However, despite this massive undertaking, markets for illegal drugs remain pervasive in nearly every American city—motivating a policy debate regarding the fundamental rationale behind these prohibitionist efforts (d'Este, 2019). Much of this ongoing discussion centers on marijuana, which has the most vocal advocates for legalization.

Proponents argue that legalization eliminates the criminal element in drug transactions and reduces the social costs imposed by traffickers or trafficking organizations.[1] For instance, Becker and Murphy (2013) state that the largest costs of a prohibitionist approach to drug policy are "the costs of the crime associated with drug trafficking", predicting that "gangs would be driven out of a decriminalized market". The validity of this argument, however, hinges critically on supply-side responses to policy intervention, which thus far remain poorly understood.

This paper takes a first step toward characterizing how drug dealers respond to the legalization of the drug that they supply. Conceptually, it asks what happens to criminals when their criminal specialization is rendered obsolete. Specifically, I study the causal impact of marijuana legalization on the criminal and labor market behavior of marijuana dealers. I present evidence from the ongoing process of statewide marijuana legalization in the United States. As of 2018, ten states across the United States had fully legalized marijuana. In contrast to previous decriminalization reforms, these are the first attempts to sanction the commercial production and sale of marijuana for recreational purposes, thereby creating competition for illicit suppliers.

To identify the effect of legalization on this population, the paper uses comprehensive administrative data from three states that adopted recreational marijuana legalization relatively early: Colorado, Washington, and Oregon. The data cover the universe of prison admissions and releases in the years immediately preceding and following the policy change. Crucially, the data contain detailed information related to each conviction episode, allowing me to identify marijuana dealers, namely, individuals incarcerated for the sale or manufacture of marijuana, as opposed to the distribution of any other drug or for the commission of any other crime. Unique identifiers are used to link offenders across multiple prison terms, creating a longitudinal dataset that allows me to follow individuals from one criminal activity to the next. While past studies relating legalization to crime only consider aggregate crime rates in states that legalize, the focus on criminals rather than localities as the unit of analysis allows me to

[1] The social costs imposed by the drug trade can be broadly categorized as those related to criminalization and those that stem from psychopharmacological effects of drugs on their users (Donohue III et al., 2010)

provide micro-evidence on legalization-induced displacement and examine otherwise unobserved patterns of substitution, which are critical for evaluating the full welfare implications of the policy.

My research design exploits the sharp timing in offenders' dates of release and, in each state, compares the criminal outcomes of marijuana offenders released prior to legalization with those released immediately thereafter. The key identification challenge is a potential endogeneity problem: changes in the legal status of marijuana may coincide with changes in unobservable contextual factors such as police enforcement, which may independently affect the post-incarceration outcomes of offenders.[2] To overcome this, my main empirical approach consists of a difference-in-differences strategy, employing non-marijuana offenders in legalizing states as a comparison group. The identifying assumption is that absent legalization, the criminal behavior of marijuana and non-marijuana offenders would have evolved along parallel trends. Later in the paper, I provide several pieces of evidence supporting this assumption.

The central findings of the paper are that legalization induced an exit from marijuana distribution and, simultaneously, entry into new criminal opportunities, namely, the distribution of other illicit substances. To arrive at this, I first show that state adoption of marijuana legalization is associated with a significant increase in the risk of recidivism for marijuana dealers. Following legalization, marijuana offenders become 9.4 percentage points more likely to re-enter prison within 6 months of release. The estimated effect is sizable, corresponding to a nearly 95% increase from a baseline rate of 10.2 percent.

When decomposed by crime category, I find that the overall increase masks two countervailing effects. First, marijuana offenders became less likely to commit future marijuana offenses.[3] Second, this reduction is offset by the transition to the distribution of other drugs. As a result, the observed criminality of former marijuana traffickers increased. Because participation in other types of crime did not vary significantly, the revealed patterns are consistent with the importance of drug industry-specific human capital in explaining the persistence of criminal choices.

I take several steps to ensure that my results are not driven by differential selection or police enforcement in states that legalize. First, I observe no discontinuous change in the baseline characteristics of marijuana offenders released within the time window around the policy change. This suggests that changes in the composition of offenders are unlikely to confound my findings. Second, I demonstrate robustness to a specification that relies only on the comparison between marijuana offenders and *non-marijuana drug* offenders, whereby any unobserved changes in drug enforcement would be differenced out in the comparison to the control group.

Third, I provide corroborating evidence using a secondary source of data: the National Longitudinal Survey of Youth 1997 (NLSY97). The NLSY97 is the only dataset, to my knowledge, that contains information on participation in criminal activity independent of arrests for said crimes.[4] I demonstrate that self-reported marijuana dealers in the NLSY97 residing in states that legalize, when compared to their counterparts from other states, became significantly less likely to report selling marijuana but more

[2] This concern is shared by studies such as mine that use crime data as a proxy for underlying criminal behavior, as enforcement could have plausibly changed after legalization. However, previous work such as Gavrilova et al. (2017) indicates that this is not the case.

[3] This effect is not mechanical, as unlicensed and unregulated sale of marijuana remains a crime even in states that legalize.

[4] These data have been widely used in the crime literature. Grogger (1998), for instance, utilizes the NLSY to study the relationship between minimum wage and juvenile crime.

likely to report selling "hard" drugs in years following legalization. These findings mirror those from the administrative data, thereby further removing any concerns that changes in policing or enforcement can explain the results.

Overall, the results suggest that an unintended consequence of selective legalization is a reallocation of drug dealers to other illicit activities. Importantly, however, this reallocation could reflect changes on either the extensive or intensive margin. This means that while it may be intuitive to interpret these findings as former marijuana dealers transitioning into the role of more hardened dealers, they are also compatible with diversified dealers shifting the composition of their drug portfolio without a switch in "occupation" per se. In both cases, however, the legalization of marijuana has the inadvertent effect of intensifying the distribution of non-marijuana-based drugs in the population of individuals previously engaged in marijuana sales.[5]

Having shown that legalization induced a displacement of criminal activity, I conclude by exploring the causal mechanisms underlying the results. Using a combination of transaction and price data, I document two primary sets of facts regarding the post-legalization marijuana market: 1) the street prices of marijuana declined by 20.2-22.7% from pre-legalization levels; 2) legal entrants displaced illegal incumbents by colocating in the same neighborhoods in the city. These findings are broadly consistent with the notion that legalization increased competition in the marijuana market, possibly diverting former marijuana dealers toward the distribution of other drugs.

This paper bridges several different strands of literature. First and foremost, it contributes to the literature on drug legalization and decriminalization. In the wake of the first wave of medical and recreational marijuana legalization in the US, several recent papers examined its effect. Much of the initial emphasis was on evaluating the demand response (Hasin DS, 2015; Choo et al., 2014; Lynne-Landsman et al., 2013; Jacobi and Sovinsky, 2016; Wall et al., 2012; Galenianos and Gavazza, 2017).[6]

The relationship between legalization and crime has also received attention from researchers. For instance, Dragone et al. (2017) and Brinkman and Mok-Lamme (2017) present evidence on aggregate crime rates in states or counties that legalize. However, these papers do not disambiguate the channels through which legalization affects crime. By organizing my analysis at the level of the individual rather than a locality, my paper is able to answer two interrelated questions unaddressed by the existing literature. First, regarding whether legalization disrupts illicit markets, I show using both direct evidence on criminal behavioral response and indirect evidence on market structure that legal entrants associated with legalization competitively displaced illegal incumbents from the market. Second, I answer the logical follow-up question of how these dealers respond to this legalization shock. In other words, what happens to marijuana dealers once marijuana is legalized.

My focus on a specific channel – the connection between legalization and spillovers to other illicit markets – is relatively unique and maps well to models of criminal participation considered in economics. Departing from previous studies, I conceptualize legalization largely as a productivity shock in the

[5] In Section 6, I explore the extent to which the effects are driven by marijuana dealers who also dealt other drugs versus those who specialized in marijuana. I find that the effects are primarily driven by the latter group.

[6] An older literature on the National Prohibition Act also exists, which is primarily concerned with the effectiveness of regulating alcohol consumption (Miron, 2005).

criminal labor sector. I provide evidence that selective drug legalization has the unintended consequence of shifting labor supply to other illicit markets. This highlights a new mechanism linking selective legalization to the production of other illegal substances through an exclusively supply-side channel, absent of any "gateway drug" based general equilibrium considerations. Ultimately, the micro-evidence I uncover for black market participants helps to explain and support the aggregate effects examined in earlier studies.

As a consequence, this paper deepens our understanding of regulatory changes and enriches the policy discussion surrounding it. The results contribute to a growing literature on the consequences of supply-side interventions and drug enforcement policies (Sviatschi et al., 2017; Dell, 2015; Rozo, 2014; Mejia and Restrepo, 2013; Evans et al., 2012; Angrist and Kugler, 2008; Dobkin and Nicosia, 2008). My findings uncover a previously unaccounted for behavioral response to legalization that is crucial for evaluating the costs and benefits of the policy. The results suggest that the perspective that legalization eliminates drug-related crimes may be overly simplistic.

The paper also has implications for our understanding of criminal incentives. It provides evidence on how criminal behavior responds to changes in the private return to committing a crime. In a recent literature review, Draca and Machin (2015) observe that evidence on such a channel is limited, as previous studies focus on how crime is affected by changes in the return to legal labor market opportunities and enforcement (e.g., Buonanno and Raphael, 2013; di Tella and Schargrodsky, 2004; Levitt, 1997). A notable exception is Draca et al. (2019), which reports crime-price elasticities estimated from a comprehensive dataset on stolen goods in London. This study extends this research by exploiting marijuana legalization as a unique window into the economic calculus underpinning criminal decisions. A key takeaway of my analysis is that motives for drug crimes are very sustained, and attempts to disincentivize participation in one crime will incentivize the pursuit of another.

Finally, while the empirical setting here pertains to drug regulations, the analysis relates to a substantial literature on the costs and incidence of labor market adjustment to external factors or innovations such as trade, immigration, or innovations in labor demand (e.g., Walker, 2013; Card, 2001; Borjas, 2003; Dell et al., 2018; Autor et al., 2016, 2014). These papers typically follow worker experiences after involuntary job separations to establish short- and long-run consequences of job loss (Farber et al., 1993; Jacobson et al., 1993; Topel, 1990; Davis and von Wachter, 2017). To my knowledge, I am the first to investigate the effect of job displacement in the criminal labor market. By drawing insights from these studies, I show that many of the same forces at work in the formal labor market are also relevant in the informal labor market. The results underscore the close parallels between the legitimate and criminal labor sectors, further substantiating the "crime as work" model.

The paper proceeds as follows: the next section provides an overview of the illicit marijuana industry and the changing regulatory environment. Section 3 describes the individual micro-data. Section 4 outlines the empirical methodology. Section 5 presents the main findings and estimates reoptimization decisions following displacement. Having shown that legalization diverts drug traffic from marijuana to other drugs, the subsequent section explores the mechanisms generating the results as well as heterogeneity in the main estimates. Finally, Section 7 concludes.

2 Background & Setting

Despite longstanding attempts to regulate use, marijuana is the most widely used illicit drug in the world, and markets for it remain pervasive in nearly every country. Globally, the United Nations Office of Drugs and Crimes estimated that there are 119 to 224 million users.

While the nature of the market makes it difficult to determine total sales with certainty, estimates indicate sales in the United States alone are between $15 to $30 billion per year Miron (2005). According to the 2015 National Survey on Drug Use and Health, close to 37 million people in the U.S. used marijuana at least once within the past year. Of those, 22 million used it on a monthly basis, and 15% of the monthly users consumed marijuana more than 20 times per month.[7]

Historically, marijuana enforcement in the United States has been punitive, with an emphasis on "supply reduction". Federal prohibition of cannabis began with the Marijuana Tax Act of 1937, which effectively criminalized possession of the drug except under very specific circumstances.[8] Pursuant to this act, marijuana essentially became illegal. At present, over 500,000 individuals are arrested each year for possession of marijuana. This tough U.S. policy stance is estimated to require billions of dollars in enforcement costs alone and has created a large black market.

In the remainder of this section, I detail what is known regarding the marijuana black market, with an emphasis on informal employment in the industry, and discuss the changing legal environment.

2.1 Marijuana Black Market

The black market for cannabis relies on a sophisticated supply chain. Prior to legalization, production and distribution were not vertically integrated. Until relatively recently, the majority of commercial-grade marijuana consumed in the U.S. was produced in Mexico (Gettman, 2006). Between 2005 and 2011, 13.2 million pounds of marijuana were seized by patrols along the U.S.-Mexico border.[9]

Mexican trafficking organizations are the dominant wholesale drug traffickers in the United States and the only drug trafficking organizations to have a nationwide presence. They are responsible for smuggling the drug into the U.S. and control the wholesale distribution. Kilmer et al. (2010) contend that 20 percent of Mexican drug trafficking organization export revenues come from U.S. marijuana consumption. Mexican drug trafficking organizations' gross revenues from wholesale sales are estimated to be approximately $2 billion annually.

However, much the additional profit in this market is generated is at the retail level, and given the nature of the data used in this paper, this is the primary population studied in this paper. Markups are highest at the retail level, offsetting risks that are also the highest at this point in the supply chain, since street-level dealers are most exposed to law enforcement and interact with a relatively unpredictable and shifting clientele. Prices are estimated to multiply three to five times between wholesale and retail.

[7] Azofeifa (2016) documents considerable variation in the prevalence of marijuana use by geography and demographics.

[8] See Bonnie and Whitebread (1970) for a detailed history of marijuana prohibition in the United States.

[9] However, the flow of Mexican imports started trending downward at the onset of legalization and the subsequent increases in domestic production. In 2016, only 861,231 pounds of marijuana were seized at U.S. ports of entry, compared to 2.4 million pounds in 2013 and 4.3 million pounds in 2009. See https://www.latimes.com/world/mexico-americas/la-fg-mexico-marijuana-20180127-story.html

Wholesale profits account for only 15% of the total retail value (Organization of American States, 2013).

Thus, most of the proceeds from the drug trade are generated domestically and presumably disbursed to participants within the United States. Whereas reasonably reliable statistics on marijuana consumption exist, information on individuals on the supply side is more difficult to ascertain. According to Uniform Crime Reporting data, over 65,000 individuals are arrested for the sale or manufacture of marijuana in the United States each year. This is likely a conservative lower bound on the number of people participating in marijuana distribution on a part- or full-time basis. Estimates from the NLSY79 indicate that 6.7% of young men and 2.2% of young women sold marijuana regularly in 1980 (Fairlie, 2002).[10]

Ethnographic studies indicate that most marijuana sellers are effectively self-employed and function as independent contractors. For example, Adler (1993) notes that "dealing was accomplished during discretionary, or recreational, hours and settings". In a study of drug dealers on probation in Washington, DC, Reuter et al. (1990) find that only 6% of marijuana dealers in their sample were employed by someone else.

Remuneration within the drug trade is not very well understood. Levitt and Venkatesh (2000) find that on average, earnings in a Chicago drug-dealing gang are somewhat above the legitimate labor market alternative. This is corroborated in the NLSY97 data, where, in 2013, marijuana dealers aged 30-33 report approximately $23,704 in annual income, which is slightly higher than the earnings of ex-convicts typically found administrative data (e.g., Grogger, 1995; Kling and Ludwig, 2006). The age-earnings profile created using the NLSY97 (Figure 1b) indicates that earnings from marijuana sales generally increase with age, which likely reflects a combination of selection and returns to experience. These facts highlight that prior to legalization, the illicit marijuana industry offered extensive opportunities for informal employment.

2.2 Legalization in the U.S.

Public attitudes toward marijuana consumption have become more favorable over the past several decades, particularly regarding medical uses of the substance. As a consequence, policy makers are increasingly willing to experiment with legalization, with many countries adopting varying degrees of decriminalization or legalization policies.[11] In the United States, while marijuana is still technically prohibited at the national level, the federal government largely defers to states on local enforcement, and particularly since 2009, 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. In subsequent years, other states followed suit by enacting various reforms. Presently, 20 states have now passed laws allowing its medical use, and 14 others have taken steps to decriminalize consumption

[10] Alternatively, a back-of-the-envelope calculation suggests that the number of marijuana dealers in the United States has to be on the order of 130,000 if consumption figures are taken seriously. This is based on the volume of transactions implied by the number of regular users reported in NSDH 2015. For context, 130,000 is slightly lower than the number of family physicians currently operating in the U.S. See https://fivethirtyeight.com/features/the-number-of-marijuana-dealers-in-the-united-states/

[11] Caulkins et al. (2016), Anderson and Rees (2013), and Miron and Zwiebel (1995) provide more comprehensive summaries of the broad issues surrounding legalization.

to some extent. Since 2012, eight states and the District of Columbia have legalized personal recreational marijuana use.

The structure and implementation of marijuana reform has varied across states. Here, I briefly outline the timing of recreational legalization in the three states studied in this paper: Colorado, Washington, and Oregon.

Colorado and Washington became the first states to legalize marijuana for recreational use in 2012, with sales permitted to anyone over the age of 21 regardless of state of residence. Importantly, the new laws also allowed the legal commercial production of marijuana. Colorado residents are also permitted to cultivate up to six marijuana plants at home.

The legislation was enacted with the intent of bringing marijuana under a tightly regulated, state-licensed system similar to that for controlling hard alcohol. Regulations established three types of licenses: producer, processor, and retailer. Producers are marijuana farmers, while processors include a broad set of businesses that convert marijuana plants into consumable products. Licensing is strictly regulated with an application process. Unlicensed production and sale remain illegal in both states. The first recreational dispensaries began appearing in Colorado in **January 2014**, and retail marijuana sales in Washington began in **July 2014**.

Shortly thereafter, the state of Oregon passed a similar reform in 2014. Then-governor Kate Brown signed an emergency bill declaring marijuana sales legal to recreational users from existing medical dispensaries starting in **October 2015**. Regulatory protocols and structure for licensing, sales of marijuana, and the taxing of such sales by the Oregon Liquor Control Commission (OLCC) would be implemented in the subsequent year.

The comparison of these three states creates a quasi-experimental opportunity to study the effect of marijuana legalization on criminal activity. These are similar states in many respects (Washington and Oregon, in particular, are neighboring states) that legalized cannabis for recreational use sufficiently early to allow the observation of illicit markets using administrative data. The staggered implementation of legalization across these three states provides a rich empirical setting with several sources of identifying variation.

3 Data & Descriptive Statistics

To explore the causal impact of recreational legalization on marijuana dealers, I utilize individual offenders' criminal histories and exploit detailed information on their criminal engagements over time. The data that I use are drawn from multiple sources. This section describes the data and sample construction process in greater detail. I also define several key variables and provide descriptive statistics.

3.0.1 Incarceration & Criminal Data

The three states I study are Colorado, Washington, and Oregon, with effective legalization dates of January 2014, July 2014, and October 2015, respectively.[12] Data for Colorado and Washington are obtained from the National Corrections Reporting Program (NCRP). The NCRP data comprise prison admissions and releases from 2000 to 2016 in the two states and are constructed using administrative records voluntarily provided by corrections departments.[13] For Oregon, I obtain incarceration records directly from the Department of Human Services, and they include all individuals admitted to or released from incarceration between 2007 and 2017.

The resulting data contain the universe of offenders supervised by each state's corrections department, which includes virtually all felony offenders and some misdemeanor offenders. Unique inmate identifiers allow me to link individuals across multiple prison terms. The data also provide information on the exact admission and release dates for each incarceration episode. From these variables, I determine whether offenders returned to prison within a specific time period following release. A limitation of this procedure is that only prison spells within a state can be linked, so reoffending in a different state becomes indistinguishable from an individual who is not readmitted to prison and is, therefore, not captured in the data.

For each prison term, I observe the primary cause of incarceration. The Oregon data provide the most serious statute violated for each incarceration period while the NCRP data include up to three conviction offenses for each prison term. From this information, I identify whether an offender was convicted for the distribution of marijuana. Importantly, I am able to distinguish between the sale and manufacture of marijuana, or attempts to do so, from that of other drugs.

The study population consists of individuals released from the corrections system in a relatively small time window around legalization. I retain all prison releases up to 3 years prior and 1 year following the policy change in each state. Specifically, all individuals released between 1/1/2011 and 1/1/2015 in Colorado; 7/1/2011 and 7/1/2015 in Washington; and 10/1/2012 and 10/1/2016 in Oregon.

This provides me a total of 92,587 prison releases. The number of unique offenders is 76,710, because some offenders were released, recommitted, and then released again from custody within the study period. Within this sample, the number of prison releases for marijuana distribution is small, numbering only 860, introducing potential challenges for statistical inference.[14]

Summary statistics for the marijuana and non-marijuana prison releases are separately reported in Table 1. Demographic characteristics for each offender at the time of release include age, gender, race, ethnicity, education, and whether the individual was previously incarcerated. Overall, offenders are predominately white and male. On average, non-marijuana offenders are approximately 36 years old and have 1.6 prior felonies. More than 60% of releases are from the offender's first prison term. Marijuana offenders are, on average, younger, less likely to have committed violent offenses, less likely to be Hispanic,

[12] I consider the effective date of legalization when recreational sales began. As discussed in the background section, these dates correspond to the first day that recreational sales took place in each state.

[13] The NCRP dataset has been widely used in research on crime. The reliability of the data is validated in several studies, including Pfaff (2011), Neal and Rick (2014), and Yang (2017)

[14] In Appendix D, I use bootstrapping to evaluate the extent to which the small sample size may bias my results.

and more likely to be African American. Marijuana offenders also serve, on average, significantly shorter sentences compared to the other offenders.

3.1 National Longitudinal Survey of Youth 1997

I augment the analysis using a restricted-use version of the NLSY97. The NLSY97 consists of a nationally representative random sample of approximately 9,000 youths who were twelve to sixteen years old as of December 31, 1996. The survey was administered annually from 1997 to 2011 and biannually from 2011 onward. To focus on a time window around legalization in the study states, I restrict my attention to Rounds 11-17 of the NLSY97 (years 2009-2015).

The dataset is exceptionally detailed and contains self-reports of criminal involvement (property, drug, assault and theft offenses) in the past twelve months. In particular, each wave asks separate questions on selling marijuana and selling "hard" drugs, as well as income derived from these activities. Whereas the administrative data are informative only about convictions, the NLSY97 allows me to assess direct participation in criminal activities independent of arrest or incarceration. To my knowledge, it is the only dataset that asks questions about selling marijuana specifically. For this reason, the NLSY97 data are uniquely suited to address my research question. The main limitation of the data, however, is the sample size. With observations on only a few thousand individuals, NLSY97 contains a relatively small number of marijuana dealers.

Figure 1a shows the fraction of respondents who report selling marijuana by age group. Participation peaks in late adolescence, although approximately 1% of respondents continue selling marijuana at the ages of 34 and 35. The data are also informative about the probability of incarceration conditional on selling marijuana. I observe that 43.39% of self-reported marijuana dealers above age 30 were incarcerated at some point.

I identify individuals who sold marijuana in 2011 or 2013 (the last two waves of the survey prior to the start of recreational legalization) and construct an individual-level panel dataset that tracks them both retrospectively and prospectively. Table 2 summarizes selected variables for this group in 2013. In this table, each observation is a respondent.[15] On average, individuals who sell marijuana on a regular basis report an annual income of $23,704 in 2013, of which $14,477 from marijuana sales.

4 Empirical Strategy

This section introduces the research design. I overview the main estimating equations and discuss the validity of the identifying assumptions. My goal is to understand how illicit producers or distributors respond to legalization and to quantify the extent of adjustment along several different margins.

The ideal experiment for causal identification would be to randomly assign legalizations to marijuana dealers and observe the subsequent impacts on criminal and labor engagements. To approximate this ideal experiment, I adopt an event study strategy using the effective dates of regulatory changes in Colorado, Washington, and Oregon as thresholds.

[15] In the panel data, each observation is a respondent-year pair.

My identification strategy exploits the fact that offenders with otherwise similar criminal histories are released from prison at different points in time. Depending on the timing of release relative to the implementation of legalization, offenders will experience distinct legal environments upon release. I then test whether post-incarceration outcomes, such as recidivism, differ between those who are"treated" and those who are not.

The empirical setting yields three useful sources of variation: i) temporal variation in date of release from prison, ii) cross-state variation in marijuana legality, and iii) differential exposure within location-time cells based on individual criminal specialization (whether the offenders dealt marijuana). I incorporate these components to study the causal effect of legalization using two methods: a pre-post analysis, in which I compare marijuana dealers released before and after legalization, and a difference-in-differences strategy, where I compare the changes for marijuana offenders to those of suitable control groups. I present each in turn.

4.1 Event-Study Analysis

The event study design compares the post-incarceration outcomes of established marijuana dealers released prior to legalization with those released immediately thereafter. To the extent that the date of release within a sufficiently narrow window around legalization is "as good as" random, each prison release defines a separate experiment, and the comparison is informative about the effect of the policy change.

As a result, the strategy divides marijuana offenders into treatment and control groups based on their time of release. However, because outcomes are measured over specific periods of time following release, I require the duration, on which the outcomes are based, to be spent entirely under a single legal regime to ensure appropriate comparisons. For this reason, when I consider the effect on recidivism, I restrict the pre-legalization sample to offenders released sufficiently early, so I can observe their risk of recidivism under prohibition. I pool offender-release events across the three study states and estimate the model below:

$$y(z)_{ist} = \beta \mathbb{1}\{rel_t > d_s\} + f(rel_t) + \delta_s + \gamma X_{it} + \varepsilon_{ist} \tag{1}$$

where $y(z)_{ist}$ equals 1 if individual i, who was released from prison at time t in state s returns to prison within z months following release from incarceration. rel_t is the month-year of release (i.e. the running variable). d_s is the month-year of effective legalization in state s. $f(rel_t)$ is a higher-order polynomial in the month-year of release. $\mathbb{1}\{rel_t > d_s\}$ is an indicator variable that equals one if the prison release occurs after legalization in state s.

δ_s are state fixed effects. X_{it} is a vector of offender i's characteristics at the time of prison release t, including: race, age, education, types of previous convictions, and whether this was the individual's first incarceration.[16] These controls should allow me to more precisely pinpoint the effect of the treatment.

[16] For observations where the highest grade is missing, I impute the education value to be -1 to include these observations in the regression. I then create a variable that is 1 if education was imputed and control for it flexibly in the specification by interacting it with other covariates.

The sample consists of offenders convicted of the sale, manufacture or distribution of marijuana released up to 3 years prior to legalization and 1 year following legalization in the three states, excluding those released z months before the policy change. The value of z leads to a tradeoff between the credibility of the research design and the amount of variation present in the outcome variable. A longer time horizon associated with larger values of z, maximizes the amount of variation in the outcome, whereas a shorter duration allows me to utilize, as control, marijuana dealers released closer to the policy change. In practice, I estimate the model separately for $z = 12\ months$ and $z = 6\ months$.

This research design exploits the timing of release as a source of treatment. For this to be valid, I must assume that unobservables are uncorrelated with the timing of release. To relax this assumption and extend a regression discontinuity-like approach to my setting, I allow the effect of the release date to vary flexibly. The inclusion of a flexible time trend, $f(rel_t)$, requires only the assumption that nothing changes discontinuously across the threshold date d_s, so that the impact of legalization—local to the date of the regulatory change—can be identified. Specifically, the coefficient β measures the average treatment effect, namely the difference in the probability of recidivism, in the z months following prison release, between the marijuana offenders who spent z months under prohibition and z months under legalization.

The main identifying assumption for this first-difference estimate to be causal is that all factors other than treatment vary continuously at the threshold. That is, within the study window, treated offenders have similar pre-treatment propensities to reoffend as their control counterparts. While the validity of this assumption is ultimately untestable, I show that the pre- and post-legalization offenders are similar on observables, which lends credibility to the assumption that the two groups are comparable, except for their different legal status post-release.

Table 3 shows the observable characteristics of offenders released before and after the legalization dates, along with the results of t-tests for differences in means. The table reveals small differences in observables and that the covariates appear largely balanced. With the sole exception that post-legalization marijuana releases were more likely to take place in Colorado, the null hypothesis of equal means is not rejected for any other variables. The differences in observables are not large enough to explain substantial differences in outcomes. Therefore, any findings in actual recidivism cannot be explained on the basis of compositional changes in the offender population.

To further test that the pre- and post-legalization samples are comparable, I also conduct McCrary (2008) tests for discontinuity in the distribution of offenders released around the policy change. Figure 2 shows that there is also no discontinuous change in the number of releases around the time of legalization. Ultimately, there is no evidence of selection with respect to date of release, suggesting that the pre- and post-legalization samples of marijuana offenders are observationally similar.

4.2 Difference-in-Differences Specification

The empirical framework outlined above relies exclusively on variation in the temporal dimension. A key identification concern is that unobserved confounding factors may be correlated with the timing of marijuana legalization. For instance, changes to law enforcement or economic conditions could coincide

with the start of legalization. Under these scenarios, marijuana offenders released at earlier points in time may not represent a valid counterfactual for marijuana offenders released later.

I address these concerns by introducing additional sources of cross-sectional variation for identification. I estimate the effect of legalization on marijuana offenders using a difference-in-differences strategy that holds constant time-invariant marijuana offender-specific characteristics or statewide time trends that could bias my estimates.

For offender i who was released from prison at time t (this pair of information uniquely identifies an observation in my dataset), I estimate the following model:

$$y(z)_{ijst} = \beta\left(Post_{st} \times Marijuana_{it}\right) + Post_{st} + \delta_s + \delta_j + \delta_t + \gamma X_{it} + \varepsilon_{ijst} \tag{2}$$

where s and j index the state and primary conviction associated with each prison term. $Marijuana_{it}$ equals 1 if individual i released at time t was convicted of marijuana sales, manufacturing or distribution. $Post_{st}$ is a dummy variable that indicates whether the individual was released after legalization in his or her state. δ_s, δ_t, and δ_j are state, time of release, and crime fixed effects, respectively. To improve precision, I again control for a rich set of offender characteristics, X_{it}. Standard errors are clustered at the offense category level.

I choose the same time window around legalization and, as in the first-difference design, exclude those who are partially treated (i.e. released within z months of legalization). The parameter of interest, β, measures the change in the outcome of marijuana offenders after legalization compared to the control group. It captures and quantifies the effect of introducing legalization on marijuana offenders.

The causal interpretation of β requires the exclusion restriction that the timing of legalization is uncorrelated with shocks that differentially affect marijuana offenders relative to the control group, irrespective of policy adoption. Therefore, credible estimates require the identification of a group of offenders who are similar to marijuana offenders in ways observable and unobservable to the econometrician.

For my main results, I consider three distinct sets of control groups, each of which is chosen to address a possible competing explanation. First, I employ all non-marijuana offenders in the states that legalize. Second, I restrict the control to comprise only drug offenders. Last, I utilized a matched sample of offenders who are comparable to marijuana offenders in terms of observable characteristics.[17]

The multiple difference-in-differences specifications and sample restrictions narrow the possible variation that could violate the exclusion restriction. Such variation would have to produce an immediate break from trend; occur with precise time lag to the legalization event; not be captured by controls; and not impact any offender group other than marijuana offenders.[18]

[17] The propensity score model uses age, a polynomial term in age, gender, prior offenses, education, race, ethnicity as matching variables.

[18] In Appendix B, I use a fourth comparison group, which consists of marijuana offenders in non-legalizing states. For reasons I discuss in detail the appendix, I believe the cross-state variation to be less well identified and less compelling relative to the within-state comparisons.

5 Main Results

This section presents the central findings of the paper. First, Section 6.1 shows the response to legalization by marijuana offenders in terms of future criminality. Next, Section 6.3 decomposes the overall changes in recidivism by crime category. Finally, in Section 6.2, I address potential threats to identification and conduct falsification analysis.

5.1 Impact on Recidivism

The first set of outcomes I consider is the impact on criminality. I proxy for criminal participation and behavior on the basis of new charges and adjudications after release from incarceration. I begin by providing graphical evidence on the impact of legalization on recidivism for established marijuana offenders.

Figures 3a and 4a present regression discontinuity (RD)-style event-study graphs for recidivism for marijuana offenders (figure on the left) and for a propensity-score matched sample of non-marijuana offenders (figure on the right).[19] The outcome is an indicator equal to one if an individual returns to incarceration within 6 months of release. The pre-treatment periods exclude individuals who were released within 6 months of release as they were partially treated. I estimate a locally linear regression (Gelman and Imbens, 2016) separately on each side of the policy change. The figure plots the fitted line from that regression and shows the average value of the outcome for offenders released at different dates (i.e. the running variable).

The figures provide an important visual test of the identifying assumptions. Reassuringly, I observe no differential pre-trend. Recidivism among marijuana offenders appears to be flat and not significantly different from zero prior to legalization. The effect materializes only after the introduction of legalization starting with the first cohort fully exposed to the policy change. The figures show a large discontinuous increase in the likelihood of recidivism for marijuana dealers released just after the adoption of legalization. By contrast, there is no discontinuity at the threshold for non-marijuana offenders, who exhibit no perceptible increase in recidivism after legalization.

Figures 4a and 4b show analogous results for recidivism over a longer time window (12 months after release), where more variation in the outcome exists. As evidenced by the graphs, marijuana offenders released in the post-period are significantly more likely to commit new offenses, whereas such differences do not exist for non-marijuana offenders. Many studies use even longer time horizons to measure recidivism, but it is clear from the figures that the effect of legalization is more or less immediate.

Next, I formally compare the evolution of recidivism for non-marijuana offenders to that of marijuana offenders in a difference-in-differences framework. To assess the magnitude of the findings, I estimate the regression discontinuity and the parametric difference-in-differences models, equations (3) and (5), using 6-month and 12-month recidivism as outcomes. Table 4 reports the estimates in the two panels: Panel A and Panel B show the effect on 6-month and 12-month recidivism, respectively.

[19] Because the population of non-marijuana offenders far exceeds that of marijuana offenders, a graphical comparison of the two groups may be misleading. To ensure a more appropriate comparison of two groups containing approximately the same number of observations, I used propensity score matching to select a sample of 800 non-marijuana offenders who are similar in terms of observable characteristics to marijuana dealers.

Columns (2)-(3) show the results of the event-study design using two different ordered polynomials in the month of release: linear and quadratic. In the remaining columns, I present estimates from the difference-in-differences specifications. Column (4) compares the change in recidivism among marijuana offenders against the same changes in the rest of the offender population. Column (5) restricts the comparison to only other drug offenders. Finally, column (6) uses a propensity-score matched sample of offenders as a counterfactual.

The table corroborates the results suggested by patterns revealed in the graphs: marijuana offenders released post-legalization became differentially more susceptible to recidivism. Across all specifications, marijuana offenders released in the post-period are on average 9.4 to 14.7 percentage points more likely to return to prison within 6 months of release than those released prior. The coefficients are statistically significant at the 5% level, and the result is robust to different parameterizations of the time trend. The magnitude of the effect is large, corresponding to a near twofold increase from the baseline recidivism rate for this population (7.4%). For 12 month recidivism, the point estimates are between 7.4 and 11.5 percentage effects. Overall, the difference-in-differences results are comparable in size and qualitatively similar to the first-difference results.

Because my analysis relies on variation in timing, it is difficult to ascertain whether the changes in the recidivism of marijuana offenders result from legalization or differential trends. This is especially concerning given the small number of marijuana offenders in the study population and the inherent variability that may arise due to the sample size. To address these concerns, I perform a series of earlier-in-time "placebo" regression tests. I define a series of placebo legalization experiments in a systematic fashion, shifting the temporal windows backward in time 1 year at a time. Specifically, I create three placebo marijuana reforms, occurring at 2, 3, and 4 years prior to the actual effective date in each state.[20] My placebo tests use the main difference-in-differences specification, corresponding to column (4) of the main table. The results are shown in Table 5. Reassuringly, in each of the placebo tests, the difference-in-differences coefficient remained insignificant and small in magnitude, suggesting that the effect of real policy change is unlikely to be attributable to cyclical trends or fluctuations that occur naturally over time.

5.2 Effects by Crime Category

The above results suggest that legalization significantly increased the subsequent criminality of marijuana dealers. To unpack this finding, I investigate more closely the exact crimes that are committed and study whether the rise in crime was driven by any particular offense type. I focus on three broad categories: marijuana distribution, non-marijuana drug distribution, and other offenses.[21] I estimate the difference-in-differences model separately for each outcome variable.

To be clear, I examine whether individuals recidivate by committing these types of crimes after being released from prison. I distinguish between the distribution of marijuana and that of other drugs. This distinction is important because it is informative of whether marijuana offenders returned to marijuana

[20] The exact dates and period for each placebo are described in detail in Appendix A.
[21] Drug and property crimes constitute the bulk of illicit income in the criminal sector.

distribution. The former suggests that legalization reinforces an intensification of existing criminal activities. The latter is consistent with legalization disrupting illicit markets and causing individuals to branch out into new areas.

Table 6 shows regression estimates of equation (3) for the different crime types. In columns (1) & (2), where the dependent variable is recidivating with a marijuana offense, the coefficient is negative. This indicates that former marijuana dealers became less likely to continue trafficking marijuana. This exit from the illicit marijuana industry is interesting as previous work has emphasized that offenders develop tendencies to specialize —i.e., recidivate in a crime category in which they already have a criminal history (Bayer et al., 2009; Bursik, 1980; Rojek and Erickson, 1982; Cohen, 1986; Farrington et al., 1988).[22]

However, as columns (3) & (4) show, this decline in marijuana convictions is more than offset by the increase in new criminal charges related to the distribution of other drugs. The coefficient in column (3) represents nearly the entire increase in overall recidivism. Notably, outside of non-marijuana drug sales, recidivism in other crime categories does not change.

The crime-specific estimates from Table 6 serve as lower bounds on the criminal 'flow', that is, the actual changes in criminal participation. Because β_j captures the changes in recidivism, which reflects both arrest and participation, and the probability of arrest is bounded between 0 and 1, the change in participation must exceed β_j.[23] Using estimates of the probability of arrest and conviction calculated using the NLSY97 data and coefficients from Table 6, a back-of-the-envelope calculation suggests that $11\% \div 43.39\% = 25.35\%$ of former marijuana dealers transitioned to the supply of other drug products.

5.3 Identification Concerns & Discussion

I interpret the results on recidivism as evidence that legalization shifted criminal participation away from marijuana and toward other drugs. The main challenge to this interpretation is the possibility that the result may be confounded by differential policing or reporting at the time of the policy change, whereby the estimates reflect only changes in reporting statistics and not the underlying criminal incidents. Because criminal activity is not directly observable, this is a concern shared by many studies that use data generated by law enforcement agencies.

However, I argue that my results are unlikely to be an artifact of differential enforcement in states that legalize. First, any changes in general enforcement should be differenced out in the difference-in-differences strategy by comparison with non-marijuana offenders in the same state. A more pointed critique is that overall policing could have remained the same, but greater emphasis was placed on drug enforcement. However, specifications using non-marijuana drug offenders as the control are designed to address this exact point.

Importantly, any change in drug enforcement would affect non-marijuana drug offenders, who are presumably unaffected by the marijuana-specific regulatory change per se. This lends itself to a falsification exercise.[24] To reinforce this point, I estimate a dynamic event-study model that considers

[22] Within my dataset, when regressing recidivism in each crime category on whether the individuals had history in the crime, experience in a particular crime is a significant predictor of recidivating with that crime. The magnitudes of these specialization coefficients are greater than the effect of generic criminal experience.

[23] A more in-depth discussion of the interpretation of the magnitudes is provided in Appendix C.

[24] The estimates by crime category indicate that marijuana offenders are recidivating in non-marijuana-related offenses,

narcotics offenders as a 'treated' group. Figures 5a and 5b plot these 'placebo' regression discontinuity-style figures. I note the absence of a post-legalization effect for non-marijuana drug offenders. The increase in recidivism is exclusive to marijuana offenders, indicating that drug-policing-related concerns are unfounded.[25]

To emphasize this point, I further validate my findings using an alternative source of data, where the data generating process is unaffected by possible changes in police enforcement. Specifically, I turn to the NLSY97, which contains self-reported information on participation in crime. In particular, individuals are asked questions regarding the sale of marijuana and/or hard drugs in each wave of the survey. From these answers, I identify marijuana dealers active prior to the start of legalization. The sample consists of individuals who self-reported selling marijuana in 2011 or 2013, which gives me a national sample of 121 marijuana dealers.

I examine the effect of regulatory changes on this group through a simple difference-in-differences design that compares the evolution of criminal behavior across marijuana dealers residing in different states, some of whom experience legalization from 2014 onward. Formally, I estimate the following regression specification:

$$Y_{ist} = \beta Legal_{st} + \delta_i + \delta_s + \delta_t + \epsilon_{ist} \tag{3}$$

where i, s, and t index the individual, state of residence, and year, respectively. Y_{ist} represents the two outcomes of interest: whether individual i living in state s reported selling marijuana and "hard drugs" in year t. $Legal_{st}$ is a dummy equaling 1 if marijuana was legalized in state s in year t. To flexibly control for unobserved heterogeneity across individuals, place or time, the regression includes a set of respondent, state, and year fixed effects.

The results are shown in Table 9. I find that marijuana dealers became less likely to sell marijuana after legalization and simultaneously more likely to sell "hard drugs". The effects are sizable and statistically significant. These patterns coincide with the findings from the administrative data, further lending credibility to the results.

6 Heterogeneity & Mechanisms

6.1 Heterogeneity

To understand the full distributional implications of the policy change, I perform a series of tests to examine whether the effect on recidivism varied by age, education, and criminal history. I find a substantial amount of heterogeneity across these characteristics.

Most notably, the average treatment effect masks considerable nonlinearities along the age distribu-

so increased marijuana enforcement cannot explain the finding.

[25] The absence of police response is partially attested to by the findings of Gavrilova et al. (2017), who find no evidence that medical marijuana legalization induced changes in policing strategies. On the other hand, Adda et al. (2014) find that marijuana depenalization caused the police to reallocate effort toward non-drug crime; however this type of police response would go against the finding of increased recidivism. Altogether, enforcement driven explanations are not compatible with the totality of my findings and are not supported by the data.

tion. I transform age into binary variables based on whether the individual's level of the given variable is above or below the cutoffs of 25 and 35 at the time of his or her release from prison. Table 7 reports the coefficients. Columns (1),(3) and (5) show that younger (< 26) and older (> 35) offenders exhibited significantly greater responses than those between the ages of 25 and 35, for whom the increase in recidivism was not significant. The increase in recidivism is simultaneously concentrated among the youngest and oldest offenders.

For each age group, I also estimate the following interacted specification to examine the joint effect of education and age:

$$y(z)_{ist} = \beta(\mathbb{1}\{rel_t > d_s\} \times HS_i) + f(rel_t) + \delta_s + \gamma X_{it} + \varepsilon_{ist} \tag{4}$$

where HS_i is a dummy indicating whether individual i graduated high school.[26] In columns (2), (4), and (6), within each age group, I find that increase in recidivism was less pronounced for individuals with a high school education, but the difference is only significant for those aged 35 or above. The findings by education and age are consistent with the notion that prime working-age individuals (age 26-35) with better employment prospects may have responded to legalization by shifting to the legal sector.[27]

The last dimension of heterogeneity I consider is that of criminal experience. I begin by investigating whether the length of criminal history matters. In Table 8, I separately estimate the first-difference model for marijuana offenders with prior convictions and those without. I find that the effect size did vary significantly with the extent of an offender's criminal history but, surprisingly, the increase in recidivism is statistically significant only for those without prior incarceration history.

Building on this, I find that the effect also differed systematically depending on the individual's degree of criminal specialization. Specifically, I split the sample of marijuana dealers into two groups: 1) marijuana offenders who were simultaneously convicted of distributing other drugs and 2) marijuana dealers who were not. The former subgroup represents marijuana dealers who also sold other drugs, while the latter represents more specialized marijuana distributors. I find that the rise in recidivism was driven primarily by marijuana offenders who did not deal other drugs whereas the effect was notably absent for marijuana offenders who sold other illicit substances. This suggests that marijuana legalization was less disruptive to drug dealers who dealt a greater variety of drug products.

6.2 Mechanisms

The results in the preceding sections underscore the effect that legalization has on criminal behavior. A question that remains is what are the causal channels driving these changes? One possibility is a "supply-push" explanation. Legalization disrupts the profitability of illicit marijuana distribution and disincentivizes participation in trade. This displaces individuals who, in the absence of the policy change, would have dealt marijuana, driving them toward other illicit opportunities.[28] In this section, I provide

[26] I focus on the pre/post model for heterogeneity as the sample contains only marijuana offenders and it is apparent the number of marijuana belonging to each subgroup.

[27] This result is consistent with the findings of Lochner (2004) showing that education reduces participation in criminal activity.

[28] An alternative hypothesis is a demand-driven hypothesis pursuant to the "gateway drug" theory, where the liberalization of marijuana use promotes additional drug consumption. This increased demand for other drugs pulls offenders from

some suggestive evidence consistent with this idea by exploring the changes in transaction prices and market structure after legalization.

First, I show that the retail prices of marijuana declined significantly following legalization. To measure the street prices of marijuana before and after the policy change, I utilize data from the website priceofweed.com.[29] The data on the website are crowdsourced, and the information is user submitted. Each entry is supposed to represent an individual transaction. When making a submission, users are required to provide the quantity purchased, total price paid, and quality (low, medium or high) as well as the location (state and city) where the purchase happened.[30] The website launched in 2010, and as of 2016, there were over 300,000 total submissions.

From these data, I create a state-quarter panel (all fifty states, plus the District of Columbia) of average marijuana prices from 2011 to 2016.[31] The average prices were calculated as the sale-weighed per-unit (ounces) price. Summary statistics by year and quality are presented in Table A2.

To examine the effect of legalization on price levels, I estimate the following model:

$$log(p_{qst}) = \beta Legal_{st} + \sigma_s + \sigma_q + \sigma_t + \epsilon_{st} \tag{5}$$

where p_{qst} is the average per-unit marijuana price of quality q in state s during time t. $Legal_{st}$ equals 1 if marijuana is legal in state s during time t. A set of state and quarter fixed effects is included. The coefficient of interest is β, which captures the relationship between legalization and marijuana prices.

The results are presented in Table A3, where the coefficients suggest that prices declined by ∼21% following the policy change. In the same table, columns (3)-(4) show that the number of user-submitted purchases remained relatively stable after legalization. This addresses the possible concern that the change in prices is driven by changes in submission behavior or composition of visitors to the website rather than reflecting actual price differences.

Finally, I investigate whether recreational dispensaries dislodged illegal suppliers. I present evidence that legal entrants geographically displaced illicit incumbents by locating where illegal exchange took place. To arrive at this, I exploit marijuana licensing records from Denver, Portland, and Seattle. This information was retrieved from each city's respective Open Data Portal and is accurate as of June 2017. From these data, I geocode the locations of all dispensaries and calculate the number of dispensaries established in each neighborhood. Table A4 examines the relationship between illicit marijuana activity and the entry of legal dispensaries. I regress the number of marijuana businesses on the number of marijuana-related arrests prior to legalization at the neighborhood level. I observe that neighborhoods with higher instances of marijuana arrests in the year preceding legalization experienced greater entry

the marijuana sector, resulting in the observed patterns of substitution. However, there is no compelling evidence that the consumption of marijuana and that of "hard" drugs are in fact complements. The existing literature is broadly consistent in showing the opposite (Powell et al., 2018), suggesting that the effect of marijuana legalization on demand for other drugs is, if anything, weakly negative.

[29] As the domain name suggests, priceofweed.com was designed to gather price information directly from consumers and increase transparency in an otherwise opaque market.

[30] The low-quality designation was discontinued from 2015 onward.

[31] This was accomplished by scraping timestamped snapshots of the priceofweed website from the Internet Archive (Wayback Machine). The priceofweed website shows aggregate price and submission summaries for each state at any given point in time. I collect information from the earliest date of each quarter that an archived snapshot of the website is available on the Internet Archive from 2011 to 2016.

of legal dispensaries. These patterns of entry suggest that legal retailers entered in close proximity to where illegal dealers operated.

Overall, the above findings are consistent with a market environment characterized by increased competition from legal entrants. I argue that these factors possibly induced dealers to exit the marijuana market and diverted them towards the distribution of other drugs.

7 Conclusion

This paper focuses on the supply side of the illicit drug trade. As policymakers increasingly turn to legalization as a possible remedy for the failures of the 'War on Drugs', I study criminal responses to these policies at an individual level. In summary, my analyses demonstrate that marijuana legalization dramatically increased the risk of recidivism for recently released marijuana offenders. The results are not explained by differential policing or enforcement in states that legalize. Instead, they reflect changes in underlying criminal behavior. The positive estimates on recidivism mask two countervailing phenomena: legalization induced an exit from illicit marijuana sales, but the effect was offset by entry into new criminal opportunities, mainly concentrated in the drug industry. Hence, legalization led to a sizable shift in illicit employment at the intensive margin, implying that the labor supply between different illegal sectors is responsive to changes in relative wages.

The results shed light on the effect of selective legalization on black market participants. I provide evidence that marijuana legalization incentivized illicit marijuana suppliers to substitute to the distribution of other prohibited substances. As a result, liberalization in one drug market has the unintended consequence of increasing the labor supply in other illicit markets absent more targeted interventions. These findings contribute to the current policy debates on the drug-crime nexus.

Whereas existing research has emphasized the effect of legalization on aggregate crime rates, the present work provides micro-evidence using individual-level data. The disruption of illicit drug markets can affect criminal activity through several channels. Establishing the relative importance of these channels is crucial for policymakers. The targeted scope of the analysis in this paper is an attempt to investigate a chief mechanism, namely, its impact on supply-side actors.

The social costs of recreational drug use in America are striking. According to the Office of National Drug Control Policy in 2002, the economic cost of illegal drug use in the United States was $180.9 billion. When disaggregated into its component parts, a large portion of the social costs of drug use today arise from a single source: drug-related crime (Donohue III et al., 2010).

Increasingly, the evidence suggests that the cost of prohibition exceeds its benefit. The results in this paper do not contradict this logic. Rather, it contributes to a more nuanced understanding of the regulatory change and shows, at least in the short run, that the problems caused by prohibition cannot be easily solved by partial legalization. Individual labor supply is highly elastic across criminal sectors, and once criminal careers are established, they are difficult to eradicate. Any changes to the drug regime would likely affect the magnitude and composition of the supply of different drugs rather than overall activity.

Therefore, marijuana legalization affects criminal decisions through at least two channels. First, it displaces individuals from the illicit marijuana market. Second, it can divert individuals who otherwise would have dealt marijuana to other crimes. To the degree that different choices of crime vary in their severity or social cost (Donohue III et al., 2010), the welfare implication is ex ante ambiguous and depends crucially on what displaced workers transition to.[32] The reduced-form effects estimated in this paper speak precisely to these underlying cross-elasticities and substitutability.

From a broader perspective, this paper suggests that enforcement in targeted drug markets should not be considered in isolation. Labor supply across separate illicit markets and territories is fundamentally linked. Major producers and dealers – such as those trafficking marijuana, cocaine, or heroin – respond to changing legal environments by intensifying the level of systemic violence Dell (2015) or, as this paper shows, by repositioning themselves in other illegal industries. If the social costs of these different drugs differ, as the evidence suggests, then the analysis of illegal drug policy from the perspective of minimizing social costs requires greater focus on the interaction between drug markets.

Overall, this paper takes a first step toward understanding how legalization affects criminal incentives. The crime-specific effects I estimate have a straightforward and policy-relevant interpretation: at least 7-11% of former marijuana offenders transitioned to the distribution of other drugs as a consequence of marijuana legalization. The displacement effects that I document can have important implications for equilibrium consumption given that drug markets are often subject to returns to scale (Jacobson, 2004). Deepening the understanding of these spillovers represents a promising and policy-relevant direction for future research.

[32] 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 (Jacobson, 2004). Therefore, spillovers from one drug sector to another could have consequences for equilibrium prices.

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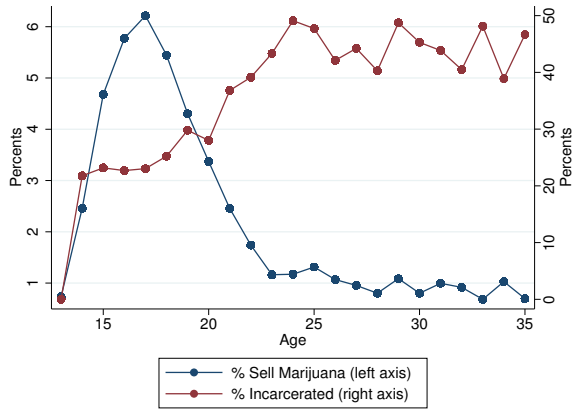
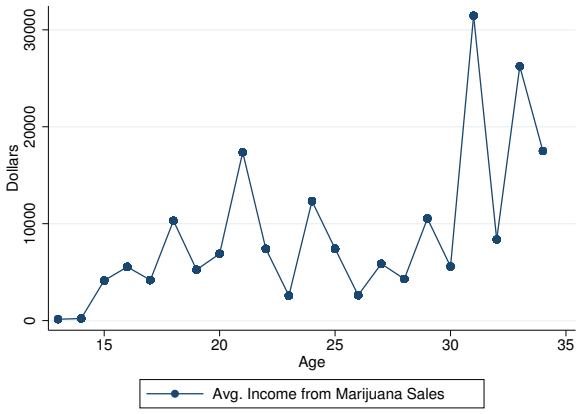
Figures

Figure 1: Marijuana dealers in the NLSY

(a) % of respondents selling marijuana by age

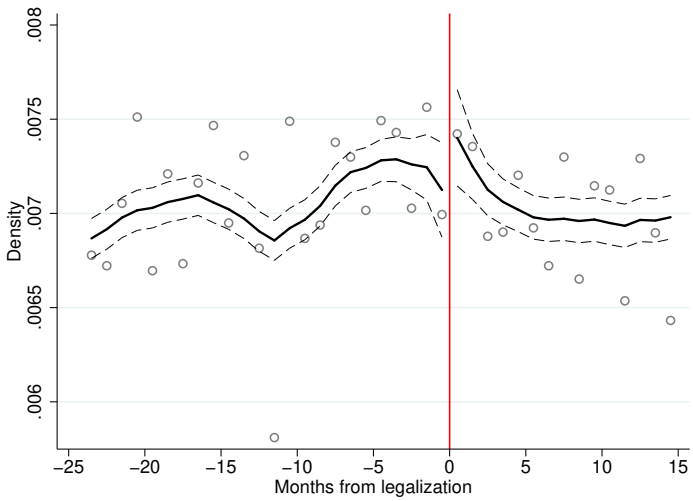


(b) Earnings from marijuana sales by age



Notes: This left figure shows the share of NLSY97 respondents who self-report selling marijuana at each age and the probability of having been incarcerated conditional on selling marijuana at each age. The figure on the right plots the average income from marijuana sales at each age.

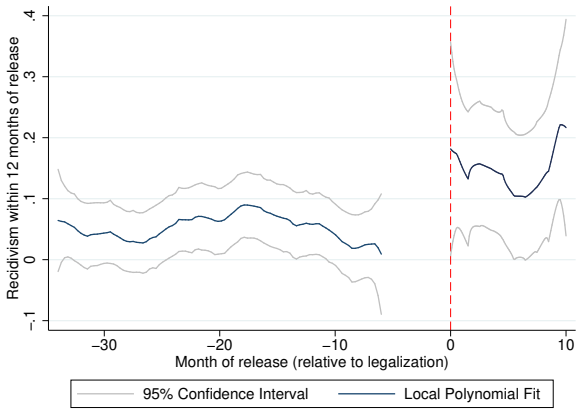
Figure 2: McCrary sorting test



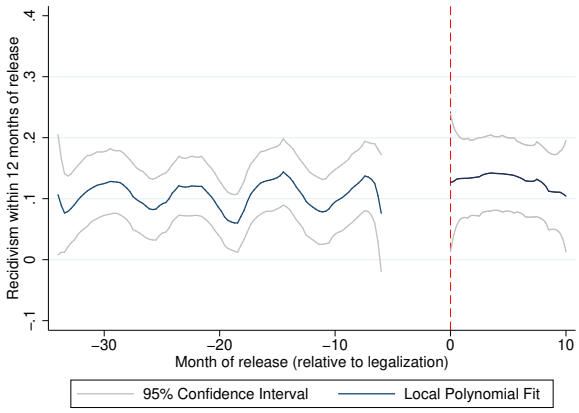
Notes: The figure implements the sorting test suggested by McCrary (2008) and plots the number of offenders released in each month of release bin. The plotted regressions use the number of observations in each bin as the dependent variable on each side of the cut-off to test if there is a discontinuity in the density of offenders released at the time of policy change.

Figure 3: Effect of legalization by crime type (recidivism within 6 months), RD graphs

(a) Marijuana offenders:



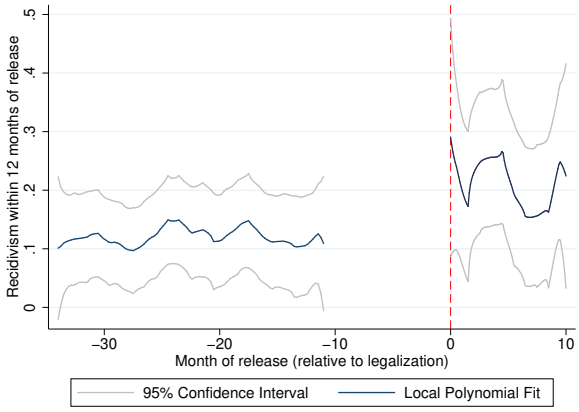
(b) Non-marijuana offenders:



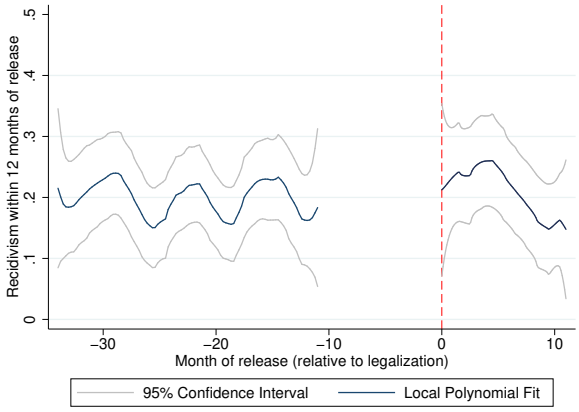
Notes: The graphs show the effect of marijuana legalization on recidivism within 6 months of release for the sample of marijuana offenders (on the left) and non-marijuana offenders (on the right). The sample exploits variation in treatment status based on the month of release in the three states. Pre-treatment period spanned from offenders released 24 months out to up to 6 months until legalization in the respective state. Post-treatment period spanned from the date of legalization to 12 months later. The the line plots a linear fit estimated separately on each side of the discontinuity and the 95% confidence interval.

Figure 4: Effect of legalization by crime type (recidivism within 12 months), RD graphs

(a) Marijuana offenders:



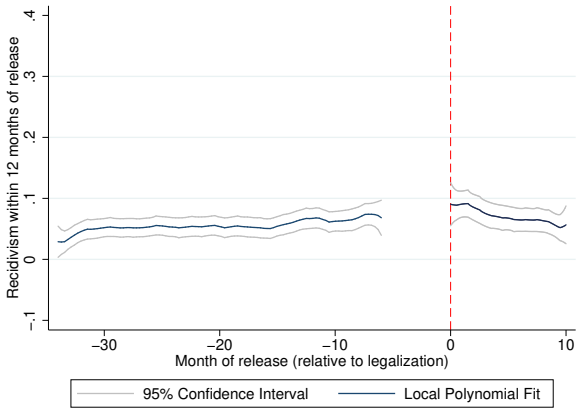
(b) Non-marijuana offenders:



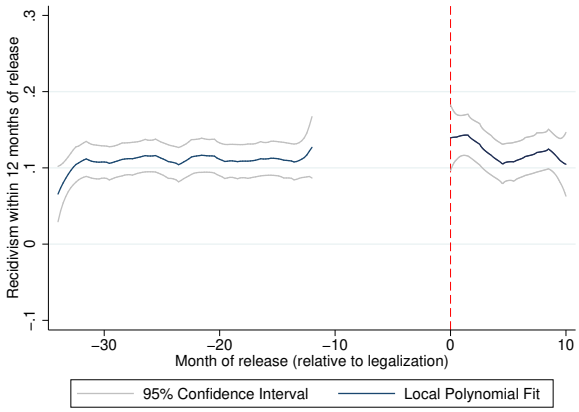
Notes: The graphs show the effect of marijuana legalization on recidivism within 12 months of release for the sample of marijuana offenders (on the left) and non-marijuana offenders (on the right). The sample exploits variation in treatment status based on the month of release in the three study states. Pre-treatment period spanned from offenders released 24 months out to up to 12 months until legalization in the respective state. Post-treatment period spanned from the date of legalization to 12 months later. The the line plots a linear fit estimated separately on each side of the discontinuity and the 95% confidence interval.

Figure 5: RD-style figures for non-marijuana drug offenders

(a) Recidivism within 6 months:



(b) Recidivism within 12 months:



Notes: The graphs show the placebo effect of marijuana legalization on recidivism for non-marijuana drug dealers and traffickers. Pre-treatment period spanned from offenders released 24 months out to up to 6 months until legalization in the respective state. Post-treatment period spanned from the date of legalization to 12 months after. The the line plots a linear fit estimated separately on each side of the discontinuity and the 95% confidence interval.

Tables

Table 1: Summary Statistics: Offender Characteristics

	Non-Marijuana Prison Releases			Marijuana Prison Releases		
	(1) Obs.	(2) Mean	(3) Std. Dev.	(4) Obs.	(5) Mean	(6) Std. Dev.
Age	92,587	35.77	10.80	860	33.11	10.12
White	92,587	0.63	0.48	860	0.61	0.49
Black	92,587	0.15	0.36	860	0.18	0.38
Hispanic	92,587	0.19	0.39	860	0.18	0.38
Male	92,587	0.88	0.33	860	0.94	0.23
Highest grade	92,587	4.32	2.78	860	3.86	2.95
Sentence length (days)	92,587	789.17	1135.78	860	504.20	551.14
First admission	92,587	0.63	0.48	860	0.74	0.44
# of Priors	71,678	1.61	0.49	610	1.71	0.45
Violent history	92,587	0.15	0.35	860	0.02	0.15
Non-marijuana drugs	–	–	–	860	0.35	0.48

Note: This table presents summary statistics on the full sample of released prisoners from a three year window of effective legalization date in the three study states. Missing values for the education variable (highest grade) was imputed to be -1.

Table 2: Descriptive statistics of NLSY97 marijuana dealers in 2013

	Obs.	Mean	Std. Dev.
Education	121	12.62	2.57
Black	121	0.26	0.44
Annual Earnings ($)	74	14,477.02	46,618.97
Marijuana Income ($)	64	23,704.73	18,843.14
# times sold marijuana	72	198.89	354.80
Sold hard drugs	93	0.47	0.50

Notes: This table provides descriptive statistics of the 121 self-reported marijuana dealers from the NLSY97 in 2013.

Table 3: Covariate balance

Time of release:	Control Group Mean	Treatment Group Mean	Difference	p-value
Demographic				
White	0.609	0591	0.018	0.677
Black	0.178	0.170	0.009	0.800
Hispanic	0.175	0.182	-0.007	0.836
Age	33.09	33.18	0.111	0.925
Highest grade	3.054	2.943	-0.14	0.698
Oregon	0.298	0.252	0.046	0.243
Washington	0.345	0.289	0.056	0.178
Colorado	0.357	0.459	-0.102	0.0160**
Criminal				
Sentence length (days)	499.71	523.96	-24.25	0.617
First admission	0.743	0.704	0.039	0.317
Priors	1.711	1.712	-0.000	0.992
Violent crimes	0.019	0.038	-0.019	0.137
Non-marijuana drugs	0.362	0.295	0.066	0.111
Observations	701	159		

Notes: The table shows balance tests for marijuana offenders' covariates based on the timing of release. Column 1 reports the mean of the covariate in the control group, namely marijuana offenders released in the pre-legalization period (0 to 36 months prior to legalization). Column 2 reports the mean of the covariates in the treatment group, namely marijuana offenders released in the post-legalization period (0 to 12 months following legalization). Missing values for the education variable (highest grade) was imputed to be -1. Columns 3 & 4 shows the difference in means and the p-value of its significance. $p < .10$, ** $p < .05$, *** $p < .01$

Table 4: Effect of legalization on the risk of recidivism

	Event-Study			Difference-in-Differences		
	(1)	(2)	(3)	(4)	(5)	(6)
Control group:	None	None	None	Other offenders	Drug offenders	Matched sample
Panel A: recidivism within 6 months of release						
Post	0.126**	0.107**	0.147***	–	–	–
	(0.048)	(0.044)	(0.051)	–	–	–
Post × Marijuana	–	–	–	0.094**	0.098**	0.109**
				(0.040)	(0.043)	(0.050)
Age	-0.007	-0.008	-0.007	-0.004**	0.000	-0.004
	(0.006)	(0.006)	(0.006)	(0.002)	(0.002)	(0.003)
Age^2	0.000	0.000	0.000	0.000**	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	795	795	795	83539	10020	2648
R^2	0.138	0.142	0.146	0.083	0.082	0.137
Mean of dep. var	0.074	0.074	0.074	0.102	0.060	0.096
Panel B: recidivism within 12 months of release						
Post	0.115***	0.109**	0.103*	–	–	–
	(0.036)	(0.051)	(0.050)	–	–	–
Post × Marijuana	–	–	–	0.074*	0.079*	0.097*
				(0.040)	(0.041)	(0.051)
Age	-0.018***	-0.018***	-0.019***	-0.008***	-0.002	-0.012***
	(0.004)	(0.004)	(0.004)	(0.002)	(0.004)	(0.003)
Age^2	0.000***	0.000***	0.000***	0.000***	-0.000	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	715	715	715	75401	9034	2381
R^2	0.181	0.181	0.183	0.122	0.109	0.167
Mean of dep. var	0.145	0.145	0.145	0.187	0.115	0.180
Year of release F.E. :	X	X	X	X	X	X
Offense category F.E. :				X	X	X
Linear time-of-release trend:		X				
Quadratic time-of-release trend:			X			
Offender controls:	X	X	X	X	X	X
State F.E. :	X	X	X	X	X	X

Notes: Each observation is a prison release event. In columns (1)-(3), each observation represents a prison term associated with marijuana distribution convictions. The comparison group indicated in the column header are included in the regression in columns (4)-(6). In the top panel, the dependent variable is a binary indicator for recidivism within 6 months of release from prison. In the bottom panel, the dependent variable is a binary indicator for recidivism within 12 months of release from prison. The sample contains prison releases occurring up to 3 years before marijuana legalization in each state and up to 1 year after legalization. In the top panel, prison releases within 6 months prior to legalization is excluded. In the bottom panel, prison releases within 12 months piror to legalization is excluded. Offender controls include age, race, education, gender, ethnicity, sentence length, and criminal history at the time of each prison release. Standard errors are clustered at the county level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 5: Placebo Experiments

	Recidivism within 6 months			Recidivism within 12 months		
	Placebo 1	Placebo 2	Placebo 3	Placebo 1	Placebo 2	Placebo 3
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Marijuana	0.008	-0.024	-0.028	0.009	0.004	-0.021
	(0.013)	(0.023)	(0.019)	(0.030)	(0.029)	(0.033)
Offender controls:	X	X	X	X	X	X
Crime category F.E. :	X	X	X	X	X	X
State F.E. :	X	X	X	X	X	X
Year of release F.E. :	X	X	X	X	X	X
Observations	83745	84365	84726	76303	76593	76630
R^2	0.083	0.082	0.080	0.123	0.127	0.125

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

Notes: Columns (1)-(3) present estimates from regression equation (1) and columns (4)-(6) provide estimates from regression equation (6). The dependent variable is a binary indicator for recidivism within 12 months of release from prison. In columns (1)-(3), the sample includes marijuana offenders released up to 2 years prior to legalization and up to a year following legalization, excluding those released 12 months prior to legalization. The comparison group indicated in the column header are included in the regression In columns (4)-(6). Each column considers different groups of controls in the specification. Standard errors are clustered at the county level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 6: Offense Type Specific Estimates

Dep Var:	Marijuana Recidivism		Non-MJ Drug Recidivism		Other Recidivism	
	6 mos	12 mos	6 mos	12 mos	6 mos	12 mos
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Marijuana	-0.023*	-0.032*	0.110***	0.086*	-0.007	-0.002
	(0.013)	(0.018)	(0.041)	(0.050)	(0.012)	(0.015)
Offender characteristics:	X	X	X	X	X	X
State F.E. :	X	X	X	X	X	X
Offense category F.E. :	X	X	X	X	X	X
Time of release F.E.:	X	X	X	X	X	X
Observations	46450	53530	46450	53530	46450	53530
R^2	0.057	0.032	0.087	0.066	0.174	0.119
Mean of dep. var	0.145	0.145	0.145	0.187	0.115	0.180

Notes: This table estimates the model from column (4) of Table 4 for different crime-specific outcome variables. The dependent variable in each column is recidivating in the offense category indicated in the column header within the months indicated. Standard errors are clustered at the county level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 7: Heterogeneous Effects by Age and Education

Age:	≤ 25		26 to 34		≥ 35	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.255**	0.271**	0.036	0.045	0.116***	0.137***
	(0.102)	(0.099)	(0.042)	(0.042)	(0.036)	(0.044)
Post ×High School	–	-0.067	–	-0.158	–	-0.227**
		(0.124)		(0.142)		(0.109)
Observations	188	188	283	283	244	244
R^2	0.217	0.222	0.240	0.241	0.235	0.241

Notes: Columns (1), (3), and (5) report regression results from estimating equation 1 for the age group indicated in the column header. Columns (2), (4), and (6) provide estimates of equation 4 separately for each age group. Each observation is a prison release event. Sample includes Standard errors are clustered at the county level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 8: Heterogeneous Effects by Criminal Experience

Dep. Var.:	Recidivism within 6 months				Recidivism within 12 months			
Sub-sample:	Prior Incarcerations		Sells Non-MJ Drugs		Prior Incarcerations		Sells Non-MJ Drugs	
	Yes	No	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)				
Post	0.064	0.147***	0.110	0.124***	0.112	0.124***	0.025	0.138***
	(0.096)	(0.022)	(0.063)	(0.035)	(0.115)	(0.037)	(0.070)	(0.025)
Observations	210	585	272	523	184	531	243	472
R^2	0.164	0.179	0.060	0.159	0.181	0.212	0.062	0.164

Notes: This table presents the estimates of 1 for different subsamples indicated in the column header. The outcome is recidivism within 6 months and 12 months in columns (1)-(4) and (5)-(8), respectively. All specifications include the baseline controls and fixed effects corresponding to column (2) from Table 4. Standard errors are clustered at the county level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 9: NLSY Results

Dep. Var:	Selling "hard" drugs			Selling marijuana		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Legal State	0.225***	0.262***	0.118***	-0.166***	-0.098***	-0.388***
	(0.025)	(0.031)	(0.027)	(0.030)	(0.035)	(0.035)
Individual F.E.	–	X	X	–	X	X
Year F.E.	X	X	X	X	X	X
State F.E.	X	X	–	X	X	–
State by Year trend:	–	–	X	–	–	X
Observations	603	603	603	603	603	603
R^2	0.183	0.441	0.396	0.228	0.406	0.290
Mean of dep. var.	0.191	0.191	0.191	0.398	0.398	0.398

Notes: The sample comprises of all respondent-years from 2009 to 2015 if the respondent reported selling marijuana in 2011 or 2013. Each observation is an individual-year. The outcome in columns (1)-(2) is an indicator for selling marijuana. The outcome in columns (3)-(4) is an indicator for if an individual self-report selling "hard" drugs. Post × Legal State is an indicator variable that takes a value of one if the individual is residing in a legalizing state after the policy change has taken place. Standard errors are clustered at the level of the state. * $p < .10$, ** $p < .05$, *** $p < .01$

Appendix

A Placebo Legalization Experiments

As discussed in the main text, I perform a series of "placebo" regression tests to ensure the validity of my findings. I construct three placebo earlier-in-time treatments occurring prior to the actual legalization dates in each state. These "placebo" dates were systematically chosen to be 2, 3, and 4 years prior to the effective dates of legalization in the corresponding states. I show that difference-in-differences analyses over these "placebo" dates produce statistically insignificant results. This demonstrates that the significant results of the real policy change on recidivism are unlikely to be generated at random, thereby adding credibility to my empirical methods. Here, I briefly outline the exact timing and definition of these placebo treatments. For each placebo test, the sample consists of offenders released up to 3 years prior to the placebo dates in each state and up to 1 year after the dates in each state.

Placebo #1: Colorado - January 1st, 2012; Washington - July, 2012; Oregon - October, 2014.

Placebo #2: Colorado - January 1st, 2011; Washington - July, 2011; Oregon - October, 2013.

Placebo #3: Colorado - January 1st, 2010; Washington - July, 2010; Oregon - October, 2012.

B Alternative Difference-in-Differences Specification

The central research design in this paper relies on a comparison of the changes in marijuana offenders before and after legalization with those of other offenders in the same states. This difference-in-differences analysis utilizes non-marijuana offenders belonging to several different categories (all offenders, drug offenders, observably comparable offenders) within the same states as a counterfactual for marijuana offenders.

An alternative approach instead compares marijuana offenders in legalizing states to those in non-legalizing states. This is encapsulated in the following regression framework:

$$y(z)_{ist} = \beta Legal_{st} + \delta_s + \delta_j + \delta_t + \gamma X_{it} + \varepsilon_{ijst} \tag{6}$$

where $y(z)_{ijst}$ is a binary variable equaling 1 if individual i released in time t in state s recidivates within z months of release. $Legal_{st}$ equals 1 if marijuana has been effectively legalized in the state at the time of release. δ_s, δ_t, and δ_j are state, time of release, and crime fixed effects.

The control group consists of marijuana offenders released between 2013 and 2016 in 23 states that submit data to the NCRP that did not undergo any legalization change during this time period. This includes the following states: Alabama, Arizona, California, Delaware, Florida, Georgia, Illinois, Iowa, Kentucky, Maryland, Michigan, Minnesota, Mississippi, Missouri, Nebraska, Nevada, New Hampshire, New Mexico, New York, North Carolina, North Dakota, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, and Utah. The sample include marijuana offenders released in these control states and the three legalizing states I study.

Table A1 shows the resulting estimates. While, the results are comparable in magnitude and qualitatively similar to the main difference-in-differences models, I believe that my within-state design is more credible in the context of the question studied. Given that legalization policies are decided and approved on a state-by-state basis, it is reasonable to suspect that the passage of marijuana legalization may coincide with other changes in state laws (or bundles of changes) that may affect criminal behavior within that state. Therefore, marijuana offenders in other states may not serve as a reasonable control group. This may contaminate my estimates.

C Interpretation of Crime-Specific Results

This section presents a more detailed discussion of the interpretation of the coefficients from Table 6 . The assumption that police enforcement did not systematically change facilitates interpretation. Specifically, it provides sufficient conditions for my crime-specific estimates to serve as lower bounds on the criminal 'flow', that is, actual changes in criminal participation. To see this formally, I introduce some helpful notations.

Let $G = 1$ denote the treated group (e.g., marijuana offenders). Binary treatment (e.g., legalization) is represented by the indicator z. Associated with the treatment alternatives are potential outcomes: $Y_j(z)$, which in this case indicates whether the individual recidivated with crime j. Define $\mathbb{P}_j(z)$ as the potential probability of arrest faced by an individual when committing crime j.[33]

Under the common-trends assumption, the DiD estimate, β_j, identifies the average treatment effect on the treated (ATET), that is:

$$\beta_j = \mathbb{E}\left[\mathrm{Y}_j(1) \mid \mathrm{G} = 1\right] - \mathbb{E}\left[\mathrm{Y}_j(0) \mid \mathrm{G} = 1\right]$$

Incorporating the probability of arrest, the potential outcomes can be written explicitly as:

$$\beta_j = \mathbb{P}_j(1)\mathbb{E}[\mathrm{Y}_j^*(1) \mid \mathrm{G} = 1] - \mathbb{P}_j(0)\mathbb{E}[\mathrm{Y}_j^*(0) \mid \mathrm{G} = 1]$$

where $Y_{jt}^*(z)$ is a latent variable for participating in crime j. Imposing the additional assumption that the probability of arrest and conviction for committing crime j remains unchanged, this expression then reduces to:

$$\beta_j = \mathbb{P}_j(1)\ \Big(\mathbb{E}[\mathrm{Y}_j^*(1) \mid \mathrm{G} = 1]\ -\ \mathbb{E}[\mathrm{Y}_j^*(0) \mid \mathrm{G} = 1]\Big)$$

Since the first term is bounded between 0 and 1, the change in participation (i.e., the second term) must exceed β_j. Using estimates of the probability of arrest and conviction calculated from NLSY97 data and coefficients from Table 6, a back-of-the-envelope calculation suggests that 21.66% of former marijuana dealers transitioned to the supply of other drug products.

[33] To simplify notation in the discussion without loss of generality, I suppress the individual index i and implicitly condition on the control set X throughout.

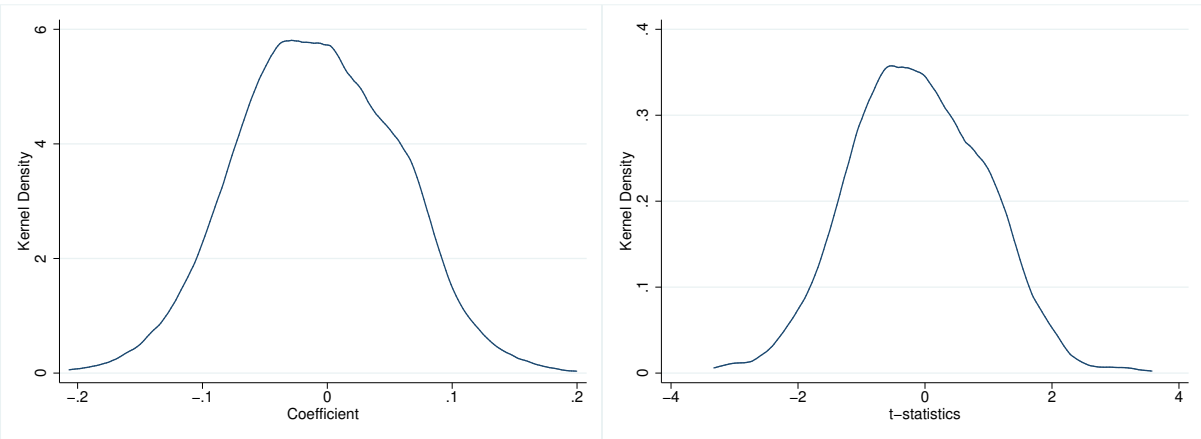
D Evaluating Sampling Bias

Because there are relatively few marijuana offenders in the data, the sample size in the pre/post analysis and in the treatment group of the difference-in-differences model is small. One might be concerned that the effect I estimate are noise driven by sampling variability due to the limited number of observations. In this section, I employ resampling techniques to evaluate the bias that might come from results using small samples. Specifically, I draw a random sample of non-marijuana offenders of approximately equal size to the population of marijuana offenders—800. I then proceed to estimate equation 1 using the random sample. I repeat this process 1000 times, resampling with replacement each time.

In Figure A1, I present two density plots depicting the distribution of the estimated coefficient of *Post* and that of the corresponding t-statistics. The outcome variable is recidivism within 12 months of release. As the figures show, both parameters are centered at approximately 0. This suggests that it is unlikely that sampling variability can result in the positive and statistically significant estimates I find using the marijuana offenders. This mitigates concerns that my results simply capture idiosyncratic shocks, or small sample errors.

Figure A1: Bootstrapped Impact of Legalization on Non-Marijuana Offenders

(a) Coefficient (b) T-statistics



Notes: This density plot depicts the distribution of estimated impacts on random samples of non-marijuana offenders. The left figure shows the distribution of the point estimate of the *Post* variable on recidivism within 12 months of release. The right figure shows the distribution of the corresponding t-statistics.

E Additional Tables & Figures

Table A1: Cross-State Difference-in-Differences

	Recidivism within 6 months (1)	Recidivism within 12 months (2)
Legal	0.095**	0.091***
	(0.036)	(0.021)
Year of release F.E. :	X	X
Offender characteristics:	X	X
State F.E. :	X	X
Observations	27163	27085
R^2	0.099	0.123

Notes: Each observation is a prison release for marijuana distribution. The sample consists of prison releases in Colorado, Washington, Oregon, and 30 control states occurring up to 3 year prior to legalization and up to 1 year post legalization. Legal is an indicator variable equalling 1 if a prison release occurs in a legalizing state after the policy change. Column (1) excludes prison releases up to 6 months before legalization and column (2) exclude releases up to 12 months before legalization. Standard errors are clustered at the state level. * $p < .10$, ** $p < .05$, *** $p < .01$

Table A2: Average Marijuana Prices by Year and Quality ($ per Ounce)

	Marijuana Quality			
	Low	Medium	High	Total
2010:	147.91	246.36	409.07	272.25
	(67.93)	(101.8)	(50.99)	(131.9)
Observations	93	103	104	300
2011:	147.91	246.36	409.07	272.25
	(67.93)	(101.8)	(50.99)	(131.9)
Observations	93	103	104	300
2012:	167.77	242.27	371.38	260.47
	(98.95)	(66.61)	(49.96)	(112.4)
Observations	208	208	208	624
2013:	193.71	245.14	341.91	260.25
	(115.6)	(52.50)	(44.83)	(99.01)
Observations	208	208	208	624
2014:	201.89	246.64	330.33	267.63
	(105.5)	(44.83)	(40.89)	(82.23)
Observations	132	208	208	548
2015:	–	245.49	323.20	284.34
	–	(39.96)	(40.22)	(55.83)
Observations	–	204	204	408
2016:	–	249.46	323.46	286.46
	–	(42.83)	(38.72)	(55.07)
Observations	–	52	52	104

Notes: Observations are at the state by year by quality level. The mean by year and quality are shown with corresponding standard deviation in parentheses below. Prices are quoted as dollars per ounce values. Number of observations are shown.

Table A3: Effect of legalization on marijuana prices

Dependent variable:	Per Unit Price		# of User Submissions	
	(1)	(2)	(3)	(4)
Legal	-0.227***	-0.202***	162.462	-145.648
	(0.036)	(0.031)	(586.911)	(647.380)
# of States	51	51	51	51
Observations	3171	3171	3171	3171
R^2	0.666	0.669	0.582	0.596

Notes: In this table, I regress the log per-unit price of marijuana and the number of user submissions on `priceofwood` on a post-legalization indicator along with a set of quality, state, and quarter-year fixed effects. Each observation is a state by quarter pair. Standard errors clustered on city level in columns (3)-(4) and state level in remaining columns.

Table A4: Evidence of geographical displacement at the neighborhood-level

Dep Var:	Log Number of Retailers		
	(1)	(2)	(3)
City	Denver	Portland	Seattle
Log Marijuana Arrests	0.294***	0.235***	0.293***
	(0.104)	(0.049)	(0.106)
Observations	60	77	52
R^2	0.223	0.238	0.451
Median # of retailers	5	2	1

Notes: This table shows the spatial distribution of dispensary entry in Denver, Portland, and Seattle. A unit of observation in a neighborhood. Each column represents a univariate OLS regression, where the dependent variable is the log number of marijuana retailers opened in the neighborhood. The explanatory variable of interest is the log number of arrests for marijuana sales in the year immediately prior to legalization. Robust standard errors are presented in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$