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 12-month recidivism rate of marijuana offenders by 7-8 percentage points relative to a baseline rate of 13 percent. The results are not explained by changes in police enforcement. Rather, the increased recidivism is driven by substitution to the trafficking of other drugs, which is consistent with a Becker-style model where individuals develop human capital specific to the drug industry. Using the NLSY97, I show evidence of legalization-induced displacement even amongst non-previously convicted dealers. In contrast, the transition to formal employment appears much more modest. To learn about potential mechanisms behind these results, I use transaction-level data to estimate the effect of legalization on average prices and price dispersion. I provide suggestive evidence that both the price level and residual variance declined following legalization, consistent with legalization eroding rents earned in the illicit marijuana market. Overall, the results in this paper suggest that an unintended consequence of selective legalization is a re-allocation 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

Illicit markets are estimated to represent a fifth of global economic activity and the criminal opportunities associated with them constitute a critical policy concern (Hsiang and Sekar, 2016). Over the past four decades, the U.S. federal and state governments have spent over $1 trillion in financing drug enforcement policies (d’Este, 2019). Against this backdrop, markets for illicit drugs remain pervasive in nearly every American city, motivating an ongoing policy-debate that questions the fundamental rationale behind the ‘War on Drugs’. Much of the present 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. For instance, Becker and Murphy (2013) state the largest costs of a prohibitionist approach to drug policy are “the costs of the crime associated with drug trafficking”, predicting “gangs would be driven out of a decriminalized market”. The validity of this argument, however, hinges critically on the supply-side responses to the policy intervention, which to this point, remains poorly understood.

This paper takes a first step towards characterizing how drug traffickers 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 have fully legalized marijuana. In contrast to previous decriminalization reforms, these are the first policies to sanction the commercial production of marijuana for recreational sales, thereby creating competition for the incumbent 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 covers the universe of prison admissions and releases in the years immediately preceding and following the policy change. Crucially, the data contains detailed information related to each conviction episode, allowing me to identify marijuana dealers; namely individuals incarcerated for the sale or manufacturing 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 longitudinal dataset that allows me to follow individuals from one criminal activity to the next. While past studies relating legalization to crime look only at aggregate crime rates

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1The social cost imposed by drug trade can be broadly categorized as those related to criminalization and costs that stem from psychopharmacological effects of drugs on their users.
in states that legalize, the focus on criminals rather than localities as the unit of analysis allows me to provide micro-evidence on legalization-induced displacement and examine otherwise unobserved patterns of substitution, which are critical to evaluating the full welfare implications of the policy.

My research design exploits the sharp timing in offenders’ dates of release and, in each respective state, compare criminal outcomes of marijuana offenders released prior to legalization with those released just after. 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 post-incarceration outcomes of offenders. To overcome this, my main empirical strategy consists of a difference-in-differences approach, employing non-marijuana offenders in legalizing states as a comparison group. The identifying assumption is that absent legalization, 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 is that legalization induced an exit from marijuana trafficking and, simultaneously, entry into new criminal opportunities, namely the distribution of other illicit substances. To arrive at this, I first show that that the state adoption of marijuana legalization is associated with a significant increase in the risk of recidivism for marijuana dealers. Following legalization, marijuana offenders become 7 to 8 percentage points more likely to re-enter prison within 12 months of release. The estimated effect is sizable, corresponding to a near 70% increase from a baseline rate of 13 percent. When decomposed by crime categories, I find the overall increase masks two countervailing effects. First, marijuana offenders became less likely to commit future marijuana offenses. Second, this reduction is offset by the transition to the trafficking of other drugs. As a result, the observed criminality of former marijuana traffickers increased. Because participation in other type of crimes 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 my results are not driven by differential selection or police enforcement in states that legalize. First, I observe no discontinuous change in baseline characteristics of marijuana offenders released within the time window around the policy change. Second, I find police spending did not systematically change following legalization. Finally, 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. Removing these concerns, the crime-specific treatment effect I estimate has a straightforward and policy-relevant interpretation: at least 7-8% of former marijuana offenders transitioned to the distribution of other drugs as a consequence of marijuana legalization.

However, the administrative data used in this analyses pertains only to a specific subset of marijuana dealers: those who were previously convicted. If incarceration itself leads to heterogenous responses, then the extent to which the effects are generalizable to the overall population of interest remains uncertain.

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2This concern is especially pressing and is shared by studies such as mine which 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 this is not the case.

3This effect is not mechanical as unlicensed and unregulated sale of marijuana remains a crime even in states that legalize.
I confront this challenge by leveraging a supplementary source of data. I turn to a restricted-use version of the National Longitudinal Survey of Youth 1997 (NLSY97). It is the only dataset to my knowledge which allows me to directly assess participation in criminal activity independent of arrests for said crimes. I show NLSY97 respondents who report selling marijuana and reside in states that legalize, are significantly more likely to report selling “hard” drugs in years following legalization compared to their counterparts from other states, irrespective of whether they were previously convicted or not. This provides corroborating evidence that legalization-induced crime displacement is not limited to convicted dealers.

Overall, the evidence from two different datasets identifying the effect of legalization on two distinct sub-populations of interest suggest that an unintended consequence of selective legalization is a re-allocation of drug criminals to other illicit activities. To shed light on the causal mechanisms underlying the results, I provide indirect evidence that legalization lowered the profit margins and returns for participants in the illicit marijuana market. Using crowd-sourced data containing over 300,000 individual purchases, I show that the retail prices of marijuana dropped significantly following legalization. Additionally, owing to the large-scale legal entry and lower search frictions, much of the within-state price dispersion disappeared. These findings are consistent with the anecdotal evidence suggesting that legalization have reduced profits for traffickers in the marijuana market. I argue that this environment of lower markup and increased competition induced dealers to leave the market and diverted them towards the production of other drugs. In other words the legalization of marijuana inadvertently turned marijuana dealers into more hardened traffickers.

The search for more lucrative opportunities is evidenced by the increased geographic mobility and cross-state migration of marijuana offenders upon release post-legalization. This entry into new criminal sectors may precipitate territorial disputes or otherwise destabilizing effects, I document that the transition away from marijuana and to the distribution of other drugs was concentrated in locations with transnational cartel presence. This underscores the role of criminal organizations in reducing search cost and facilitating matches in circumstances where explicit markets do not exist.

With additional data from the NLSY, I examine the effect of legalization on participation in the formal labor market. Following same identification strategy, I fail to detect any increase in weeks worked or income from wages, even amongst sub-populations consisting only of dealers who have never been incarcerated. Altogether, the transition to legitimate employment resulting from criminal displacement is evidently low.

I conclude by investigating how government policies can address the persistence of criminal behavior. Since the Second Chance Act in 2007, the federal government has spent more than $475 million on reentry programs aimed at reducing recidivism. The grants are locally distributed and the amount allocated vary significantly between municipalities over time. Using a triple differences strategy, I evaluate the effectiveness of the implemented programs before and after legalization. I find while the programs were effective in lowering recidivism prior to legalization, the per-dollar effectiveness of the allotted funds became significantly higher for marijuana dealers post-displacement. This suggest that legalization presents a critical juncture where marijuana dealers are more responsive to incentives and are more
within the reach of policy.

This paper bridges several 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 have examined its effect. Much of the initial emphasis has been evaluating the demand response (Anderson et al., 2014; Hasin DS, 2015; Choo et al., 2014; Lynne-Landsman et al., 2013; Jacobi and Sovinsky, 2016; Wall et al., 2012).

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 crimes rates in states or counties that legalize. However, these papers does not disambiguate the channels through which legalization affect crime. By organizing my analysis at the level of the individual rather than a locality, my paper is able to answer two inter-related question unaddressed by be the existing literature First, whether legalization disrupt illicit makers: 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 which is how do these dealers respond to this legalization shock. In other words what happens to marijuana dealers now that marijuana is legalized.

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

As a consequence, this paper deepens our understanding of the regulatory change 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 behavioral response to legalization that is a previously unaccounted but crucial for evaluating the costs and benefits of the policy. The results suggest 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

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4An older literature on the national Prohibition also exists, which is primarily concerned with the effectiveness of regulating alcohol consumption (Miron, 2005).

5Propagation of drug markets is linked to criminal activity through four main channels: systemic, economic, lifestyle, and pharmacologic. Legalization can affect equilibrium quantities which may result in economic oriented crime amongst users depending on the elasticity of demand. These channels are largely orthogonal to the scope of my paper.
enforcement (e.g. Buonanno and Raphael, 2013; di Tella and Schargrodsky, 2004; Levitt, 1997). The two legalization episodes provide 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 dis-incentivize 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 experience after involuntary job separations in order 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 many of the same forces at work in formal labor market are also relevant in the informal. The results underscore the close parallels between the legitimate and criminal labor sector, 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 discusses the policy implications of legalization and the conceptual framework that underpins the study. Section 4 describes the individual micro-data data. Section 5 outlines the empirical methodology. Section 6 presents the main findings and estimates re-optimization decisions following displacement. The subsequent section explores the mechanisms generating the results as well as heterogeneity in the main estimates. Having shown that legalization diverts drug traffic from marijuana to other drugs, Section 8 explores the external validity amongst non-convicted dealers and also potential substitution to formal employment. Finally, Section 9 concludes.

2 Background & Setting

Despite long-standing attempts to regulate use, marijuana is the most widely used illicit drug in the world and markets for it remain pervasive in nearly very country (Office of National Drug Control Policy 2004).6 Globally, the United Nations Office of Drugs and Crimes (2012) 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 Stats 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

6Marijuana is actually a common name for the dried leaf and flower of the cannabis genus. I will use the terms marijuana and cannabis interchangeably throughout the paper.
7Azofeifa (2016) documents considerable variation in the prevalence of marijuana use by geography and demographics.
8See Bonnie and Whitebread (1970) for a detailed history of marijuana prohibition in the United States.
this act, marijuana essentially became illegal. To date, over 500,000 individuals are arrested each year for possession of marijuana. This tough US policy stance is estimated to require billions of dollars in cost of enforcement 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 emphasis on the 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, the production and distribution was not vertically integrated. Until relatively recently, majority of the commercial grade marijuana consumed in the US was produced in Mexico (Gettman, 2006). Between 2005 and 2011, 13.2 million pounds of marijuana seized by border patrol along the U.S.-Mexico border.

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) claim that 20 percent of Mexican drug-trafficking organization export revenues come from U.S. marijuana consumption. Mexican DTOs’ gross revenues from wholesale sales is estimated to be around $2 billion annually.

However, it is at the retail level where much of the additional profit in this market is generated. Markups are highest at this level, offsetting risks which are also the highest at this point in the supply-chain, since retailers 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. When added together, wholesale profit accounts for only 15% of the total retail value (Organization of American States, 2013).

Thus, most of the proceeds from the drug trade is generated domestically and presumably disbursed to participants within the United States. Whereas reasonably reliable statistics on marijuana consumption exist, information on individuals in the supply-side is more difficult to ascertain. According to Uniform Crime Reporting data, over 65,000 individuals are arrested for sales or manufacturing 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-time 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).

Alternatively, a back of the envelope calculation suggests that the number of marijuana dealers in the United States have 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. To put this in context, 130,000 is slightly lower than the number of family physicians currently operating in the U.S.

Ethnographic studies indicate most marijuana-sellers are effectively self-employed and function as

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9However, the flow of Mexican import has started trending downward since 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, as compared to 2.4 million pounds in 2013 and 4.3 million pounds in 2009.

10See: [https://fivethirtyeight.com/features/the-number-of-marijuana-dealers-in-the-united-states/]
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 full-time marijuana dealers report around $22,000 annual income, which is slightly higher than earnings of ex-convicts typically found administrative data (e.g., Grogger, 1995; Kling and Ludwig, 2006).

These facts highlight that, prior to legalization, the illicit marijuana industry provided extensive informal employment opportunities.

### 2.2 Legalization in the U.S.

Public attitudes towards marijuana consumption have become more favorable over the past several decades, particularly with regards to 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. In the United States, while marijuana is still technically prohibited at the national level, the federal government largely defers to states with regard to 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 reforms in varying forms. Presently, 20 states have now passed laws allowing its medical use, and 14 others have taken steps to decriminalize consumption by some degree. Since 2012, eight states and the District of Columbia have legalized personal recreational marijuana use.

The structure and implementation of marijuana reform varied across respective 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 November, 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 home-cultivate up to six marijuana plants.

The legislations were enacted with 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. The licensing is strictly regulated with an application process. Unlicensed production and sale remains illegal in both states.

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

At the November 2014 general election ballot, Oregon voters approved a cannabis law reform that is similar to the one passed in Washington in terms of taxing sales and subjecting them to regulation and licensing by the Liquor Control Commission, but is less restrictive in terms of possession and cultivation. Legalization of possession, use and home cultivation went into effect in July 2015, recreational sales through medical dispensaries started in October 2015, and retail store licenses began in October 2016. The supply responses were overwhelming.\textsuperscript{12}

The comparison between these three states offers an experimental opportunity to study the effect of such legalization on criminal displacement because these are similar states in many respects (Washington and Oregon, in particular, are neighboring states), that legalized cannabis for recreational use at about the same time, but with a 2-year time lag that induces a quasi-experiment, and sufficiently early to allow the observation of illicit markets for at least two years from administrative data.

3 Conceptual Framework

To motivate the empirical analysis, I describe a simple conceptual framework that illustrate how legalization affects black market participants using key considerations emphasized in existing models of crime. The simplest possible rational model of crime — as examined in the seminal Becker (1968) — treats crime as a substitute for labor and frames the decision to commit crime as a choice between illegal employment and a legal alternative.

As such, an individual will undertake criminal activity only if the expected benefit exceed the costs. This model predicts that crime should increase in the return to criminal activity and decrease in the probability of apprehension, the severity of punishment, or the value of the outside option, which is typically thought of as legal wage. More complicated models, such as the time allocation model in Ehrlich (1973) or the dynamic model in Lochner (2004), generally yield similar predictions.

To begin mapping the legalization policy onto this framework, we can extend the standard model by introducing occupational choices within the broad choice of crime. For instance, through the prism of a partial-equilibrium Roy (1951) model, prospective criminals would self-select into the criminal sector for which they have comparative advantage. Within this framework, legalization can be conceptualized as a shock to the expected payoff of engaging in a specific sector of crime.

In particular, as next section will show, legalization disrupts illicit markets by eliminating key sources of rent. This reduces the direct returns to participation in the legalized sector and distorts the relative returns between criminal specialization. The Roy intuition suggests that individuals will re-allocate to criminal or legitimate sectors in pursuit of their comparative advantage, which is partially determined by human capital. Ethnographic studies on crime treat seriously the notion that there is human capital in the successful commission of crime and this human capital can be quite specialized Fairlie (2002). For instance, drug dealing is frequently characterized as requiring risk tolerance and entrepreneurial acumen.

\textsuperscript{12}As of 2017, there are over 500 active retailers in Colorado and Oregon; 334 retailers in Washington. In 2016 recreational marijuana generated over $1.8 billion in sales. Washington state realized over $264 million in tax revenue.
At the same time the criminal human capital may not readily transfer into the legalized regime. The skills requisite to compete in a legal environment is not necessarily the same skills that allow one to navigate the illicit one.

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 and Ayres, 2009), the welfare implication is ex ante ambiguous and depends crucially on what displaced workers transition to. The reduced form effects estimated in this paper speak precisely to these underlying cross-elasticities and substitutability.

3.1 Mechanisms

The relevance of the above-discussed framework turns on the competitive displacement of illegal supplies by legal production. Here I provide indirect evidence that legalization depressed illicit profits by showing its net effect on transaction prices and market structure. I document three sets of facts regarding the legalized landscape: i.) the street-prices of marijuana declined by over 22%, ii.) within-state price dispersion lessened significantly, iii.) legal entrants geographically displaced illicit retailers by locating where illicit exchange took place. These findings are broadly consistent with a market environment characterized by increased competition and lowered search cost (Barron et al., 2004).

To measure equilibrium prices in the black market, I use crowd-sourced transaction data from the website priceofweed.com. As the domain name suggest, priceofweed.com was designed to gather price information directly from the consumers and increase transparency in an otherwise opaque market. Visitors to the site can either view previously submitted prices or anonymously submit a price themselves.

When submitting an entry, users are required to provide the quantity purchased, the price paid, and the quality, choosing from low, medium or high, as well as the location (state and city) where the purchase happened. The website launched in 2010 and as of 2016, there are over 300,000 total transactions. The validity of data is attested to in studies by Lutz (2016) and Davis et al. (2016).

To examine the effect of legalization on price levels, I aggregate the data to the quarterly level to create a state-quarter panel of marijuana prices. The result is a state-quarter panel dataset of marijuana prices. I estimate the following model:

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

where $p_{qst}$ is the average price of marijuana of quality $q$ in state $s$ during quarter $t$. $l_{qst}$ equals 1 if marijuana is legal in state $s$ during quarter $t$. Included is a set of state and quarter fixed effects. The coefficient of interest is $\beta$. The results are presented in Table 12, where the coefficients suggest that prices declined by 21% following the policy change.

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13 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). Thereby, spillover from one drug sector to another could have consequences for equilibrium prices.  
14 Because the level of the variation is at the state level.
I also estimate a flexible difference-in-differences specification. The leads and lags around the timing of legalization for medium quality marijuana are plotted in Figure 1a. This shows the dynamic effect of legalization. Overall, the patterns are consistent with the timing of the legalization and the coefficients reveal little evidence of an anticipatory effect.

Next, I consider the second moment of the price distribution. To examine within-state price variance, I use dis-aggregate the data using each transaction as a separate observation. Following established methods in the empirical industrial organization literature (Barron et al., 2004), I measure price dispersion as the squared residuals from a hedonic price regression. This captures the unexplained variance in prices accounting for market characteristics.

Formally, I separately estimate the system of equations below for Colorado, Washington and Oregon:

\[
\log(p_i) = \sigma_q + \sigma_s + \sigma_c + \sigma_t + \epsilon_i
\]

where \(p_i\) is the sale price of transaction \(i\); \(q\) denotes the quality; \(t\) and \(c\) indexes the month and county of sale respectively. \(\epsilon_i^2\) is the squared residual term.

The \(\delta_t\) coefficients in the second equation measure the average level of price dispersion each month. I verify whether the magnitude of the coefficients changed with legalization policies. This is corroborated in Figures 1b, which shows that legalization is associated with a structural break in the time trend. The results in Table 12 imply that over 50% of the within-state variation in prices disappeared following legalization.

Finally, I investigate if legal entrants dislodged illegal suppliers. Using data on business licenses, I present evidence that, at least geographically, this is true.

I observe that counties with higher instances of marijuana arrests in the year preceding legalization experienced greater retailer entry. Table 2 shows that a 1% increase in marijuana crime within the county corresponds to a 2% increase in number of establishments. Surprisingly this relationship persists even at a very fine geographic level. Neighborhoods in Denver, Portland, and Seattle where more illegal marijuana sales occurred received more legal retailers post-legalization.

The spatial pattern of entry reveal that legal dispensaries entered precisely in locations where illegal dealers operated. Given their close proximity, this implies that legal entrants directly competed with the incumbent illegal dealers. Hence, illegal retailers was supplanted by legitimate trade.

Altogether, the findings suggest an environment of increased competition and diminished rents following legalization. This raises the question of how did marijuana dealers respond to this shock. I find, as a first pass, evidence of a significant exit from the illicit marijuana industry. Figure 3 shows that the number of arrests and incarcerations for marijuana sales or manufacturing dropped precipitously in legalizing states following the policy change. In the remaining sections, I turn to the follow-up question at the heart of the study: what happened to these would-be dealers?

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15This may not be necessarily true. For instance, in the case of ivory markets, Hsiang and Sekar (2016) find the legal and illegal versions of otherwise physically identical goods are treated by consumers as different products that are not perfect substitutes, leading to segregated black and white markets that coexist.
4 Data & Descriptive Statistics

To explore the causal impact of recreational legalization on marijuana dealers, I need criminal histories for individual offenders and 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 more detail. I also define several key variables and provide descriptive statistics.

4.0.1 Incarceration & Criminal Data

The three states I study are Colorado, Washington, and Oregon. Data for Colorado and Washington are obtained from the National Corrections Reporting Program (NCRP). The NCRP data comprise of prison admissions and releases from 2000-2016 in the two states and is constructed using administrative records voluntarily provided by corrections departments. For Oregon, I obtain incarceration records directly from the Department of Corrections (DOC) and they include all individuals sentenced to incarceration or probation between 2007 to 2017.

For each state, the resulting data contains the universe of offenders supervised by the 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 provides information on the exact admission date and release date for each custody event. From these variables, I determine if offenders returned to prison in a specific time duration following release. I treat recidivism as measure of subsequent engagements with the criminal labor market. A limitation of this procedure is that only prison spells within a state can be linked, so any reoffending in a different state is not captured and is indistinguishable from an individual who is not recommitted in the same state.

For each prison term, I observe the exact cause of incarceration. The NCRP data includes up to three conviction offenses. From offense types provided by each of the participating states, the BJS created a uniform classification of 171 offense types. Importantly, the data allows me to discern possession from distribution. Furthermore, the classification distinguishes between the sale and manufacturing of marijuana, or attempt to do so, from the distribution of other drugs. The Oregon data provides the exact statutes violated in each conviction, which I then harmonize with the BJS classification and use to identify if the offender was involved in marijuana distribution.

The study population consists of individuals released from DOC supervision in a 3 year window around legalization in each respective state. It includes more than 50,000 offenders. Detail on their demographic characteristics and offense history is provided in Table 1. The full sample of offenders is presented in columns (1)-(3), whereas column (4)-(6) are restricted to only marijuana offenders. The amount of demographic information varies depending on the state. Age, race, ethnicity, gender, and whether the individual has previously been convicted and incarcerated of a felony are available for all three states. Additional information on offender’s education and county of release are observed for the Colorado and Washington data.

Overall, offenders are predominately white and male. On average offenders are 36 years old and

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16 The NCRP dataset has been widely used in research on crime. The reliability of data is validated in several studies including Pfaff (2011), Neal and Rick (2014), and Yang (2017)
appeared for a total of 1.9 separate supervision spells when they first come under DOC supervision. More than half of these spells are for new offenses many represent probation violations and parole revocations. With most state prison systems, majority of individuals are never incarcerated and serve probation sentences only. When they do serve time, their sentences are typically longer than a year. Marijuana offenders are less likely to have committed violent offense and more likely to have a high school degree.

4.1 National Longitudinal Survey of Youth 1997

I augment the analysis using a restricted-use version of the National Longitudinal Survey of Youth 1997 (NLSY97). The NLSY97 consists of a nationally representative sample of approximately 9,000 youths who were twelve to sixteen years old as of December 31, 1996. The dataset is exceptionally detailed and contains self-reports of criminal involvement (property, drug, assault and theft offenses) in the preceding twelve months for each year between 1997 and 2016. 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 is 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 ask questions about selling marijuana specifically. For this reason, the NLSY97 data is uniquely suited to address my research question. The main limitation of the data, however, is its sample size. With observations on only a few thousand individuals, the NLSY97 contains a relative small number of marijuana dealers.

Figure 2a shows the fraction of respondents who report selling marijuana by age group. Participation peaks in late adolescence, although around 1% of respondents continue selling marijuana at the age of 34 and 35. The data is also informative about the probability of incarceration conditional on selling marijuana. The figure reveals that the likelihood of conviction is high, indicating the sub-population of dealers studied using administrative data is empirically relevant.

I identify individuals who sold marijuana between 2009 and 2012 (three years prior to legalization) and construct an individual-level panel dataset which tracks them over time. Table 11 summarizes selected variables for this group. Each observation is a respondent-year pair. On average, individuals who sell marijuana on a regular basis report an annual income of $22,000 and near $9,000 earnings from marijuana sales. To examine how criminal earnings vary with criminal experience, I create age-earnings profiles for marijuana dealers. Figure 2b shows that earnings generally increase as individuals accumulate criminal experience.

4.1.1 Descriptive Evidence

To motivate the baseline empirical framework, I document patterns in the administrative data consistent with legalization inducing changes to criminal participation. Figure 4a shows the cumulative hazard of returning to prison by month since release for marijuana offenders in the sample. I find that the unconditional hazard rate is significantly higher for marijuana dealers post-reform. In other words, contrary to expectations that legalization lowers criminal involvement, marijuana dealers are more likely
to return to prison under legalization relative to prohibition. Figure 4b demonstrates this difference is absent for non-marijuana offenders over the same time frame.

In the next section, I pursue strategies to pin down the causal link between this increase in observed recidivism and the policy change. To do so, I will consider the timing of reform more carefully and show it coincides precisely with an abrupt increase in the risk of new criminal charges. The ensuing methodology clarifies the assumptions required for causal inference and allows us to assess their plausibility.

5 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. In order 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.

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 “treated” and those who are not.

The empirical setting yields three useful sources of variation: i) over-time 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 that of suitable control groups. I present each in turn.

5.1 Pre/Post Analysis

The event study design compares the post-incarceration outcomes of established marijuana dealers released prior to legalization with those released immediately after. To the extent that the date of release within a narrow enough 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:

\[
y(z)_{ist} = \beta \mathbb{1}(rel_t > d_s) + f(rel_t) + \delta_s + \gamma X_i + \varepsilon_{ist}\n\]

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

\(X_i\) is a vector of demographic and criminal history information, including: race, age, education, types of previous convictions, and whether this was the individual’s first incarceration. These controls should absorb a large share of the variation in the risk of recidivism and allow me to more precisely pinpoint the effect of the treatment.

The sample consists of offenders convicted of sales, manufacturing or distribution of marijuana released within a 3-year window of legalization in state \(s\), excluding those released between months \(d_s - z\) and \(d_s\). The value of \(z\) leads to a trade-off 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 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 the legalization — local to the date of the regulatory change — can be identified. Specifically, the coefficient \(\beta\) 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 reoffended 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 smaller differences in observables and that covariates appear largely balanced. The null hypothesis of equal means is
not rejected for any of the variables. Thus, differences in observables are not large enough to explain substantial differences in outcome. So 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 5 shows that there is also no discontinuous change in the number of releases around the time of legalization. Altogether, there is no evidence of selection with respect to date of release, suggesting that the pre- and post-legalization samples of marijuana offenders are extremely similar.

5.2 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 hold constant time-invariant marijuana offender specific characteristics or state-wide time-trends that could bias my estimates. For individual $i$ who was convicted of crime $j$, I estimate the following model:

$$y(z)_{ijst} = \beta (Post_{st} \times Marijuana_i) + Post_{st} + Marijuana_i + \delta_s + \delta_j + \delta_t + \gamma X_i + \varepsilon_{ijst} \quad (4)$$

where $s$ and $t$ indexes the state and quarter of release. $Marijuana_i$ equals 1 if individual $i$ was convicted for marijuana sales, manufacturing or distribution. $Post_{st}$ is a dummy variable that indicates whether the individual was released after legalization in his state. $\delta_s$, $\delta_t$, and $\delta_j$ are state, time of release, and crime fixed effects, respectively. To improve precision, I again control for a rich set of offender characteristics, $X_i$. Standard errors are clustered at the offense category level.

I choose the same window 3-years 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, $\beta$, measures the change in the outcome of marijuana offenders after legalization, as compared the control group. It captures and quantifies the effect of introducing legalization on marijuana offender.

The causal interpretation of $\beta$ 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 that 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 comprising of only drug offenders. Lastly, I utilized a matched sample of offenders that are comparable to marijuana offenders in term of observable characteristics.

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 legalization event; not be captured by controls; and not impact any offender group other than marijuana offenders.

6 Main Results

The results are reported in the various subsections below. Section 6.1 & 6.2 present the central findings of the paper, comprising of the criminal response to the change in regulations by marijuana dealers. I first show changes to the overall probability of recidivism. Next, I decompose the effect by examining various categories of offending separately. Section 6.3, addresses potential threats to identification and offer robustness checks.

6.1 Impact on Future Criminality

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.

Figure 6a and 6b present the RD event-study graphs for recidivism for the sample of marijuana offenders (figure to the left) and for non-marijuana offender sample (figure to the right). 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 average value of the outcome for offenders released at different dates (i.e. the running variable). The figures provide an important visual test for the identifying assumptions. Reassuringly, we observe no differential pre-trend. Recidivism across comparison groups appear to be flat and not statistically 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 threshold for non-marijuana offenders, who exhibit no perceptible change. Figure 7a and 7b show analogous results for recidivism over a longer time window (12-months after release), where more variation in the outcome exists. As evidenced by graphs, no difference exists for non-marijuana offenders, but marijuana offenders released in the post-period are significantly more likely to commit new offenses. Many studies use even longer time horizons to measure recidivism, but it is clear, from the figures, 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 model, equations (3) and (5), using 6-months and 12-months recidivism as outcomes. Table 4 reports the estimates in two panels respectively. The table corroborate the results suggested by patterns revealed in the graphs: marijuana offenders released post-legalization became differentially more susceptible to recidivism.

Columns (1)-(3) show results of the pre/post design using three specifications of $f( rel_t)$ with different ordered polynomials in the month of release: linear, quadratic, and cubic. Across all specifications, marijuana offenders released in the post-period are on average 6% more likely to return to prison within 12 months of release with respect to those released prior. The coefficients are statistically significant at the 5% level, and the result is robust to different parametrization of the time trend. The magnitude of the effect is large, corresponding to a 60% increase from the baseline recidivism rate for this population (10%).

In the remaining columns, I present estimates from the difference-in-differences specifications. Column (4) compare the change in recidivism amongst 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 counterfactual. Overall, the results are comparable in size and qualitatively similar to the first-difference results.

### 6.2 Effects by Crime Category

The above results suggest that legalization significantly increased the subsequent criminality of marijuana dealers. To unpack this puzzling finding, I investigate more closely the exact crimes that are committed and decompose the overall effect on recidivism by crime type. I focus on three broad categories: marijuana distribution, non-marijuana drug distribution, and other offenses.\(^{17}\) I estimate the DiD model separately for recidivating in each category.

In particular, 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 distribution or not. The former suggests that legalization reinforces an intensification of existing criminal activities. While 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 column (1), 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).\(^{18}\) However,

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\(^{17}\) Drug and property crimes constitute the bulk of illicit income in the criminal sector.

\(^{18}\) 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 effect of generic criminal experience.
as the next column shows, this decline in marijuana convictions is more than offset by the increase in new criminal charges related to distribution of other drugs. The coefficient in column (2) corresponds to nearly the entire increase in overall recidivism and represents nearly six-times the pre-legalization mean. Notably, outside of non-marijuana drug sales, other crime categories do not respond.

The contrasting signs for marijuana and non-marijuana related drug offenses provide support that legalization incentivized marijuana dealers to leave their specialization in search of new criminal opportunities. The crime-specific estimates add context to the first result and show it is driven primarily by the transition to the production of other prohibited substances. Evidently, the degree of substitutability between criminal sectors is high and the legalization of one illicit market has the unintended consequence of increasing the labor supply in others. The degree of mobility within the drug sector, I argue, indicates dealers develop human capital specific to the industry.

6.3 Discussion & Interpretation

I interpret the results on recidivism as evidence that legalization shifted criminal participation away from marijuana and towards other drugs. The greatest challenge to this interpretation is the concern 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 crime statistics and not the underlying incidents. Because criminal activity is not directly observable, this is a concern shared by many studies such as this 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. To start with, any changes in general enforcement should be differenced out, in the difference-in-differences strategy, by the 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 is placed on drug enforcement. However, specifications using non-marijuana drug offenders as the control are designed to address this exact point.

Changes in drug enforcement would affect drug offenders presumed to be unaffected by the direct regulatory variation, which are marijuana specific. This lends itself to a falsification exercise. To drive this point home, I estimate a dynamic event-study model that considers narcotics offenders as a ‘treated’ group. Figures 8a and 8b plot these ‘placebo’ regression discontinuity style figures. We note the absence of a post-legalization effect for non-marijuana drug offenders. The increase in recidivism is exclusive to marijuana offenders only, indicating that drug policing related concerns are unfounded.

Additional, I directly test whether marijuana legalization affected police expenditure. I collect data on total state expenditure, public safety spending, and police department spending by state and year. Table 7 shows, empirically, the effect of legalization on these measures of law enforcement resources is insignificant. The absence of police response is partially corroborated by the findings of Gavrilova et al. (2017), which found no evidence that medical marijuana legalization induced changes in policing strategies. Altogether, enforcement driven explanations are not compatible with the totality of my

19The estimates by crime category indicate that marijuana offenders are recidivating in non-marijuana related offenses so increased marijuana enforcement cannot explain the finding.
findings and are not supported in the data.

The lack of any systematic changes in enforcement facilitates interpretations. Specifically, the crime-specific estimates from Table 6 act as lower bounds on the criminal ‘flow’, that is, the actual changes in criminal participation. Because $\beta_j$ captures 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 $\beta_j$. Using conservative estimates of the probability of arrest and conviction found in Lochner (2007) and coefficients from Table 6, a back of the envelope calculation suggests that 22% of former marijuana dealers transitioned to the supply of other drug products.

A question that remains is the underlying mechanisms driving these changes in criminal behavior. The equilibrium effects discussed in Section 3.2 suggest a “supply-push” explanation. Whereby, legalization disrupts the profitability of the marijuana trade and displace individuals who in absence of the policy change would have dealt marijuana, driving them toward other illicit opportunities.

An alternative hypothesis is a demand driven one, 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 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. Existing literature are broadly consistent in showing the opposite (Powell et al., 2018), suggesting the effect of marijuana legalization on demand for other drugs is, if anything, weakly negative.

7 Heterogeneity & Additional Results

The results in the preceding section underscore the effect legalization has on criminal incentives. Thus far the analysis has focused on sectoral spillovers, but the policy change may also have motivated traffickers to relocate their operations.

While migratory responses to job displacements are frequently documented in studies of the formal labor market, its importance in the informal is much less certain. Local knowledge and network are requisite inputs for professional traffickers, and hence their expertise may not readily translate between localities. At the same time, the severity of the disruption may nevertheless have necessitated spatial adjustments. While limitations in the data makes it difficult to track cross-state migration, column (2) of Table 9 shows that in border counties, where individuals can more easily migrate to a neighboring state, the increase in recidivism was much less pronounced and not significant. This is consistent with out-migration substituting for sectoral re-allocation in those counties.

Previous research have emphasized the central role of transnational drug-trafficking organizations (DTOs) in the drug trade (Dell, 2015). Most prominently, Mexican DTOs operate in transit and producing countries and dominate the U.S. wholesale markets. While my analysis pertains fundamentally to individuals working further downstream, I highlight a key source of heterogeneity that points to the significance of organizations in the story.

Using the 2013 National Gang Intelligence Center report, I determine if counties in the sample
have DTO presence and categorize them accordingly. I then evaluate the extent to which the results are explained by cartel activities. Specifically, I examine whether the effect of legalization differed depending on where the offender was released. The pattern of heterogeneity in column (2) of Table 9 suggests greater transitional dynamics in localities where centralized cartels operate.

These heterogenous responses can be rationalized by the more efficient re-allocation of personnel and resources in areas controlled by large-scale organizations. Because DTOs are much more diversified in their holdings and have vertically-integrated supply routes, retailers with relationships with those suppliers plausibly find it easier to switch product lines or to secure new clientele. The evidence suggests that transition costs between crimes are significantly reduced by the presence of criminal infrastructure.

To understand the full distributional implications of the policy change, I examine whether the effect on recidivism varied by age, education, and criminal history. I turn 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. I create an indicator for whether the current conviction was an individual’s first offense as well as if the offender completed high school.

I find a substantial amount of heterogeneity across these characteristics. Most notably, the average effect masks considerable non-linearities across the age distribution. The increase in recidivism is simultaneously concentrated amongst the youngest and oldest offenders. Table 8 reports the coefficients. Columns (1)-(3) show that younger (< 25) and older (> 35) offenders exhibited significantly greater response than those between the age of 25 and 35, for whom the increase in recidivism was not significant.

Surprisingly, effect size did not vary significantly with the extent of an offender’s criminal experience. As shown in columns (6) & (7), the point estimate was largely comparable between offenders without prior convictions and those with a longer criminal history. However, the increase in recidivism was significantly more pronounced for individuals without high school education. The findings by education and age are consistent with the notion that criminal displacement was absent amongst prime working age individuals with greater prospects for employment. Building on this I investigate how labor market opportunities shape the results.

Specifically, I explore whether the effect of legalization depended on the local labor market conditions faced by released offenders. Given the difficulty of securing legitimate employment for those with felony convictions, one might expect this not to matter. However, ex ante differences in job prospects may affect decision to pursue crime even if the realized rate of employment is low, ex post.

Following Raphael and Weiman (2003), I utilize the unemployment rate in offenders’ county of release during his year of release as a proxy for local labor market demand. Offenders entering into parole are generally required by statute to remain in the county of conviction or last county of residence, with over 90 percent of offenders residing in the county of conviction post-release. Nevertheless, this assignment of labor market conditions may introduce measurement error and bias. The values are assigned to each individual based on the county and year of release to account for time-varying conditions.

If labor market opportunities affected re-optimization decisions following legalization, we would expect criminality to be more pronounced in high unemployment areas areas. The results are presented in Table 9, columns 4. I find that offenders released in areas or periods of higher-unemployment experienced
greater post-legalization increase in recidivism, as shown by the positive and significant interaction term in the column. This is consistent with the hypothesis that improved employment or earnings prospects increased the opportunity cost of crime and reduced economic incentives for crime. This contrasts with some prior work, which does not find strong association between labor market conditions and the likelihood of recidivism (Bolitzer, 2005; Raphael and Weiman, 2003). My results suggest displaced offenders are responsive to changes in labor market conditions.

7.1 Mitigating Effect of Government Policy

In this section, I study how public policy can address the persistence of criminal path following legalization. I investigate the mitigating effects of targeted interventions which may be cost-effective to devise, and are thus possibly of interest to policymakers. In particular, I focus on the Second Chance Act (SCA), which was adopted in 2008 as a comprehensive legislation focusing on employment assistance and job-skills training. The act expanded the federal government’s role in the provision of reentry services by creating grants for states to implement prisoner reentry programs. Understanding the effectiveness of these programs and their interaction with criminal displacement is of growing importance given the resources spent on rehabilitation efforts.

The Second Chance Act is designed to address economic incentives that can steer individuals towards formal labor rather than criminal alternatives. The granted projects often focuses on general workforce ‘job-readiness’ skills which, if successfully learned, put the inmate in competition for low-skilled jobs and employment. Additional services provided to inmates after release also include referrals to assisting agencies and more specific vocational programing. Since 2008, more than $475 million have been authorized for prisoner re-entry program.

These grants are typically allotted for local programs designed to serve a particular community or jurisdiction. I exploit the differential rollout of Second Chance Act funding across locations. This experiment helps further disentangle the mechanisms underlying criminal path dependency and sheds light on the economic calculus underpinning criminal decisions. I obtain the date and site of programs funded by the Second Chance Act in Colorado, Washington and Oregon from the period of 2008 to the 2018. For each year, I tabulate the number offenders released in that year and compute the per-offender amount of funding allocated in every county. I make a sample restriction by excluding counties which never receive any Second Chance Act funding in the study period to account for possible selection in program site. The resulting variation I exploit comes only from the timing of grants and funding.

To examine how Second Chance Act and legalization jointly determine the subsequent recidivism of marijuana offenders, I implement a triple-differences design: the first two differences are the post legalization dummy and the marijuana offender indicator; and the third interaction is the normalized measure of Second Chance Act grant in the offender’s county during the year of release.

Column 5 of Table 9 shows the results for the triple interaction model. The triple interaction term

20However, a recent study Yang (2017) show strong and robust relationship between labor market conditions and criminal recidivism. Program evaluations where released offenders were randomly assigned to jobs also produce mixed evidence on whether employment opportunities reduce recidivism (Jacobs, 2012; Redcross et al., 2011).

21Source: https://csgjusticecenter.org/nrrc/national-criminal-justice-initiatives-map/
has a negative coefficient, significant at the 10 percent level. This captures the fact that the recidivism-reducing effectiveness of the Second Chance Act grant on marijuana offenders is significantly greater after legalization. To interpret the results, the coefficients suggest that a thousand-dollar increase in grant per offender pre-legalization is associated with a .14 percentage point decrease in recidivism after legalization.

Overall, while legalization in itself is insufficient to disrupt the path dependency of criminal behavior, the results in this section suggest that it does displace marijuana offenders sufficiently so that they become more responsive to their local environment. The empirical findings showing increased response to labor market conditions and targeted interventions support the notion legalization effectively altered the crime-labor tradeoff and lowered the threshold for desistance. To conclude, legalization presents a unique opportunity where criminals displaced by the regulation change actively re-optimize and, as a result, come within the reach of public policy and become comparatively cost-effective to rehabilitate.

8 Individual Panel Results Using NLYS97

The findings so far show the causal effect of legalization on a specific sub-population of marijuana dealers, namely, those who have been arrested and convicted of the crime. Given considerable evidence on the dis-employment effect of incarceration, one may be concerned that this group is not representative of the overall intended population. To investigate whether the results are externally valid, I turn to the National Longitudinal Survey of Youth 1997.

This longitudinal data set initially surveyed a random sample of American youth aged 12-16 in 1997 and has followed them since. To focus on a time window around legalization, I restrict my attention to Rounds 11-17 of the NLSY97 (years 2009-2016). As discussed in the data section, individuals are asked questions regarding selling marijuana and/or hard drugs in each wave of the survey. From these answers, I identify marijuana dealers active prior to the policy change. The sample consists of individuals who self-reported selling marijuana in the three years preceding 2012, regardless of whether they have been incarcerated or not.

The outcome of interest is whether the marijuana dealers reported selling “hard” drugs in the year surveyed. The empirical strategy in this section consists comparing the evolution of this outcome across marijuana dealers residing in different states, some of which experience legalization from 2012 onward. Specifically, I implement the following difference-in-difference strategy:

\[ Y_{ist} = \sum_{k=-4}^{3} \beta_k D_k + \delta_s + \delta_t + \epsilon_{ist} \]  

where \( Y_{ist} \) is a binary variable that equals 1 if respondent \( i \) sold “hard” drugs in state \( s \) during year \( t \). Define \( D_k \) as relative year indicators, with respect to marijuana legalization in each state. To controls flexibly for unobserved heterogeneity across individuals, place or time, the regression includes a set of respondent, state, and year fixed effects.
Under common-trends assumption, we would expect to see no effect of legalization on the years before its adoption, with $\beta_k$ being statistically indistinguishable from 0. On the other hand, estimates of increasingly large in the years before policy implementation could indicate changes in the outcome attributed to legalization are due to confounding factors.

Figure 9a presents the coefficient estimate. The parallel trends are validated. As one can see, the effect of legalization in years prior to implementation is not statistically different from zero. And the trends evolve smoothly except through the change in policy. The pattern is consistent with marijuana dealers, in states that legalized marijuana, becoming differentially more disposed to selling “hard” drugs following marijuana legalization.

Next, I stratify the sample into those dealers who have been previously convicted and those who have not. Comparison across the two groups is informative about the importance of incarceration in generating the changes. In particular, the non-incarceration sample allows me to draw inference about a sub-population that is excluded from the analysis using administrative data. Revisiting the summary statistics of the two groups reported in Table 11, interestingly, we observe that marijuana dealers who have never been caught report higher earnings from sales and greater volume of transaction, suggesting there is negative selection with respect to incarceration.

Figure 9b plot the estimates of $\beta_k$ for the non-conviction sample. The visual patterns are qualitatively similar to the results for the overall group. Marijuana dealers without any criminal convictions, nevertheless, transitioned to the selling of “hard” drugs after legalization. If the escalation of criminal activity is dictated entirely by barriers to formal employment, then we would expect significant disparity on the basis of criminal record. The lack of discrepancy between the two groups indicate the displacement in criminal activity cannot be solely explained by the stigma of incarceration.

To quantify magnitudes and assess the statistical significance of the estimates, I present the results using parametric specifications in Table 10. Non-convicted marijuana dealers became 20 percentage points more likely to sell “hard drugs” following legalization, this represents a near twofold increase from a baseline of 13%. The effect sizes are comparable to that of dealers with conviction records.

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 the 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 the labor supply between different illegal sectors is highly responsive to changes in relative wages. The transition patterns are consistent with the formation of crime-specific human capital being important for understanding the perpetuation of illegal industries. Corroborating evidence from the NLSY97 suggests the findings are generalizable to non-convicted marijuana dealers.
8.1 Formal Labor Market Analysis

Having shown that legalization drives marijuana dealers toward other illicit activities, I turn next to investigate its effect on engagement with the legitimate labor market. Owing to the lack of wage information for the administrative sample, I leverage the NLSY97 to detect transitions to formal employment. This is not totally ideal as the NLSY97 could be underpowered to reliably identify impact. Nevertheless, with this caveat in mind, I pursue the same difference-in-differences strategy in order to examine margins of adjustment in the labor market responses.

The NLSY97 provides data on self-reported employment and earnings. Columns (5)-(8) of Table 10 show the effect of legalization on these outcomes. I limit my attention to the same sample used for the crime analysis, consisting of all respondents who reported selling marijuana in the three years preceding first state wide legalization in 2012. Although precision is limited, log weeks worked and log weekly wages exhibit no significant increase in the difference-in-differences analysis. Columns (5)–(8) report the estimated coefficients and standard errors which are not statistically significantly different from zero.

Because the NLSY97 could be underpowered to reliably identify impact, I compute the minimum detectable effect size (MDE). The MDE is what would have been detectable with 80 percent power at the 5 percent significance level (Haushofer and Shapiro, 2016; Duflo et al., 2008). This measure provides an intuitive guideline to distinguish tightly estimated null findings from statistically insignificant results which we can nevertheless not rule out meaningful treatment effects with confidence. Because the NLSY97 has a relative small number of ex-offenders, I am not powered to detect moderate effect sizes. The MDEs for log weeks worked and log weekly earnings are 28.8 and 46.1 percent based on the estimated standard errors in columns (5) and (7).

9 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. 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 add to the current policy debates in the drug-crime nexus. Whereas existing research has emphasized the effect of legalization on aggregate crime rates, the present work provides micro-evidence on patterns of substitution which underly the aggregate effects. The disruption of illicit drug markets can affect criminal activity through several channels. Quantifying the relative importance of these channels is crucial to the planning and implementation of effective policies. The targeted scope of the analysis in this paper makes 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 ONDCP’s most
recent estimate, the economic cost of illegal drug use in the United States in 2002 including lost productivity, health effects, and crime-related costs such as policing expenditures and incarceration was $180.9 billion. When disaggregated into its component parts, a large a portion of the social costs of drug use today arises 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 show, at least in the short-run, the problems caused by prohibition cannot be easily solved by partial legalization. The elasticity of substitution between drug trafficking and other criminal activity is significantly higher than the elasticity between it and formal labor. Once criminal careers are established, they are difficult to eradicate. Any changes to drug regime would likely affect the magnitude and composition of the social costs of different drugs.

On 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 are fundamentally linked. Major producers and dealers – such as those trafficking marijuana, cocaine, or heroin – respond to changing legal environment by intensifying the level of systemic violence Dell (2015) or, as this paper shows, by re-positioning themselves in other illegal industries. And if the social costs of these different drugs differ, as the evidence suggests, then the analysis of illegal drug policy from a perspective of minimizing social costs requires greater focus on the interaction between drug markets.

Overall, this paper provides a first step at understanding how legalization affect criminal incentives. 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 represent a promising, and policy-relevant, direction for future research.
Figures

Figure 1: Price level and dispersion

(a) Event-study: average state prices

(b) Variance of residuals

Notes: These figures visualize the average price level and the extent of price dispersion in the three study states before and after legalization. Panel (a) focuses on the average transaction price of marijuana. Panel (b) focuses on the residual price variation within each state. See text for details on methodology and data.

Figure 2: 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 for at each age.
Figure 3: Marijuana sales, trafficking, or manufacturing activity over time

Notes: This figure shows the evolution in the number of yearly arrests and incarcerations for marijuana sales, distribution, or manufacturing in the three study states before and after legalization. The arrests information comes from Uniform Crime Reports and incarceration totals are compiled using corrections records.

Figure 4: Recidivism hazard by crime type

Notes: This figure calculates the unconditional probability of returning to prison in each month post-release conditional on having not yet returned to prison. Data are from the three study states (i.e. Colorado, Washington, and Oregon). Panel (a) includes prisoners released 24 to 12 months before legalization (who will spend 12 months post release under legalization). Panel (b) include prisoners who were released 0 to 12 months after legalization.
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 6: 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 study states. Pre-treatment period span from offenders released 24 months out to up to 6 months until legalization in the respective state. Post-treatment period span from date of legalization to 12 months after. The line plots a linear fit estimated separately on each side of the discontinuity and the 95% confidence interval.

Figure 7: 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 span from offenders released 24 months out to up to 12 months until legalization in the respective state. Post-treatment period span from date of legalization to 12 months after. The line plots a linear fit estimated separately on each side of the discontinuity and the 95% confidence interval.
Figure 8: 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 span from offenders released 24 months out to up to 6 months until legalization in the respective state. Post-treatment period span from date of legalization to 12 months after. The line plots a linear fit estimated separately on each side of the discontinuity and the 95% confidence interval.
Figure 9: Event study of legalization on selling “hard” drugs, NLSY sample

(a) Marijuana dealers:

(b) Marijuana dealers (w/ no incarceration history):

Note: Plotted are the coefficient estimates from a version of equation (5). Specifically, the two figures plot event time indicators for marijuana offenders in legalizing states, which correspond to the interaction terms from the difference-in-difference specification in equation (5). The dependent variable in both panels is an indicator variable for selling “hard” drugs in the corresponding year. The first year of the legalization adoption corresponds to year 0 in the graph. Panel (a) includes all respondents who self-report selling marijuana between the year 2009 and 2012. Panel (b) makes a further sample restriction to respondents without incarceration history. The dashed lines represent 95% confidence intervals.
Table 1: Summary Statistics: Offender Characteristics

<table>
<thead>
<tr>
<th></th>
<th>Non-Marijuana Offenders</th>
<th>Marijuana Offenders</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Obs. (2) Mean (3) Std. Dev.</td>
<td>(4) Obs. (5) Mean (6) Std. Dev.</td>
</tr>
<tr>
<td>Age</td>
<td>57,900 35.58 10.73</td>
<td>635 33.36 10.26</td>
</tr>
<tr>
<td>White (%)</td>
<td>57,900 62.42 48.43</td>
<td>635 63.77 48.10</td>
</tr>
<tr>
<td>Black (%)</td>
<td>57,900 15.26 35.96</td>
<td>635 19.52 39.67</td>
</tr>
<tr>
<td>Hispanic (%)</td>
<td>57,900 19.06 39.27</td>
<td>635 12.91 33.56</td>
</tr>
<tr>
<td>Male (%)</td>
<td>57,900 87.73 32.80</td>
<td>635 89.29 30.94</td>
</tr>
<tr>
<td>Highest grade</td>
<td>40,025 5.59 0.92</td>
<td>475 5.59 0.70</td>
</tr>
<tr>
<td>Sentence length (days)</td>
<td>57,900 717.92 1017</td>
<td>635 457.45 378.09</td>
</tr>
<tr>
<td>First admission (%)</td>
<td>57,900 62.65 48.37</td>
<td>635 67.24 46.96</td>
</tr>
<tr>
<td># of Priors</td>
<td>26,335 1.68 .46</td>
<td>157 1.88 31.96</td>
</tr>
<tr>
<td>Violent history (%)</td>
<td>57,900 14.11 34.82</td>
<td>635 2.20 14.69</td>
</tr>
</tbody>
</table>

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.
Table 2: Evidence of geographical displacement

<table>
<thead>
<tr>
<th>Dep Var:</th>
<th>Log Number of Retailers</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>County-Level Analysis</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td></td>
<td>Denver</td>
</tr>
<tr>
<td></td>
<td>Analysis</td>
</tr>
<tr>
<td>Log Marijuana Arrests</td>
<td>0.580***</td>
</tr>
<tr>
<td></td>
<td>(0.060)</td>
</tr>
<tr>
<td>Demographics controls:</td>
<td>X</td>
</tr>
<tr>
<td>Other crime controls:</td>
<td>X</td>
</tr>
<tr>
<td>States in sample</td>
<td>Wa, Co, &amp; Or</td>
</tr>
<tr>
<td>Observations</td>
<td>126</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.640</td>
</tr>
<tr>
<td>Median # of retailers</td>
<td>22</td>
</tr>
</tbody>
</table>

Notes: This table shows the spatial distribution of legal dispensaries at the state and city level. A unit of observation in each of the first three columns is a county and, in the latter three, a neighborhood. Each column represents a different specification which is estimated by OLS, where the dependent variable is the log number of marijuana retailers opened in the county or 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. * $p < .10$, ** $p < .05$, *** $p < .01$

Table 11: NLSY Summary Statistics by Incarceration History

<table>
<thead>
<tr>
<th></th>
<th>Self-Reported Marijuana Dealers</th>
<th>Marijuana Dealers w/ No Criminal Record</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Obs</td>
<td>Mean</td>
</tr>
<tr>
<td>Education</td>
<td>152</td>
<td>12.58</td>
</tr>
<tr>
<td>Black</td>
<td>152</td>
<td>0.2775</td>
</tr>
<tr>
<td>Annual Earnings</td>
<td>95</td>
<td>22304</td>
</tr>
<tr>
<td>Marijuana Income</td>
<td>131</td>
<td>8739</td>
</tr>
<tr>
<td># times sold marijuana</td>
<td>138</td>
<td>132.1</td>
</tr>
<tr>
<td>Sold hard drugs</td>
<td>153</td>
<td>0.135</td>
</tr>
</tbody>
</table>
Table 3: Covariate balance

<table>
<thead>
<tr>
<th>Time of release:</th>
<th>Control Group</th>
<th>Treatment Group</th>
<th>Difference</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>Mean</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Demographic</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>White</td>
<td>0.62</td>
<td>0.65</td>
<td>-0.03</td>
<td>0.35</td>
</tr>
<tr>
<td>Black</td>
<td>0.21</td>
<td>0.16</td>
<td>0.04</td>
<td>0.15</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.11</td>
<td>0.14</td>
<td>-0.02</td>
<td>0.30</td>
</tr>
<tr>
<td>Age</td>
<td>32.91</td>
<td>34.01</td>
<td>-1.09</td>
<td>0.18</td>
</tr>
<tr>
<td>Highest grade</td>
<td>5.58</td>
<td>5.60</td>
<td>-0.02</td>
<td>0.71</td>
</tr>
<tr>
<td>Criminal</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sentence length (days)</td>
<td>450.43</td>
<td>467.52</td>
<td>-17.0</td>
<td>0.57</td>
</tr>
<tr>
<td>First admission</td>
<td>0.66</td>
<td>0.67</td>
<td>-0.00</td>
<td>0.79</td>
</tr>
<tr>
<td>Priors</td>
<td>1.87</td>
<td>1.89</td>
<td>-0.01</td>
<td>0.76</td>
</tr>
<tr>
<td>Violent crimes</td>
<td>0.02</td>
<td>0.02</td>
<td>0.001</td>
<td>0.89</td>
</tr>
<tr>
<td>Observations</td>
<td>374</td>
<td>261</td>
<td>635</td>
<td></td>
</tr>
</tbody>
</table>

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 24 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). Columns 3 & 4 shows the difference in means and the t-test for significance respectively. p < .10, ** p < .05, *** p < .01

Table 4: Effect of legalization on the risk of recidivism

<table>
<thead>
<tr>
<th>Dep. Var:</th>
<th>Recidivism within 6 months of release</th>
<th>Difference-in-Differences</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pre/Post (1) (2) (3)</td>
<td>(4) Other offenders (5) Drug offenders (6) Matched sample</td>
</tr>
<tr>
<td>Control group: None</td>
<td>None</td>
<td>None</td>
</tr>
<tr>
<td>Post 0.050** (0.023)</td>
<td>0.052** (0.021)</td>
<td>0.052** (0.021)</td>
</tr>
<tr>
<td>Post x Marijuana – – –</td>
<td>0.057*** (0.022)</td>
<td>0.045* (0.024)</td>
</tr>
<tr>
<td>Time of release: X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Linear trend: X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Quadratic trend: X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cubic trend: X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Year of release F.E.: X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Offender characteristics: X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>County F.E.: X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Offense category F.E.: X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations 635</td>
<td>635</td>
<td>635</td>
</tr>
<tr>
<td>$R^2$ 0.175</td>
<td>0.176</td>
<td>0.176</td>
</tr>
<tr>
<td>Mean of dep. var .050</td>
<td>.050</td>
<td>.050</td>
</tr>
</tbody>
</table>

Notes: Columns (1)-(3) present estimates from regression equation (3) and columns (4)-(6) provide estimates from regression equation (4). The dependent variable is a binary indicator for recidivism within 6 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 6 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. Robust standard errors are presented in columns (1)-(3) and standard errors are clustered at the offense category level in subsequent columns. * p < .10, ** p < .05, *** p < .01
### Table 5: Effect of legalization on the risk of recidivism

<table>
<thead>
<tr>
<th>Control group:</th>
<th>Recidivism within 12 months of release</th>
<th>Difference-in-Differences</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pre/Post</td>
<td>None</td>
</tr>
<tr>
<td>Post</td>
<td>(1)</td>
<td>0.086*</td>
</tr>
<tr>
<td></td>
<td>(2)</td>
<td>0.093**</td>
</tr>
<tr>
<td></td>
<td>(3)</td>
<td>0.093**</td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td>(4)</td>
<td>0.070**</td>
</tr>
<tr>
<td>Time of release:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Linear trend:</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Quadratic trend:</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Cubic trend:</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Year of release F.E.:</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Offender characteristics:</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>County F.E.:</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Offense category F.E.:</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Observations</td>
<td>518</td>
<td>518</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.223</td>
<td>0.224</td>
</tr>
<tr>
<td>Mean of dep. var</td>
<td>.120</td>
<td>.120</td>
</tr>
</tbody>
</table>

**Notes:** Columns (1)-(3) present estimates from regression equation (3) and columns (4)-(6) provide estimates from regression equation (4). 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. Robust standard errors are presented in columns (1)-(3) and standard errors are clustered at the offense category level in subsequent columns. * $p < .10$, ** $p < .05$, *** $p < .01$.

### Table 6: Decomposing effects by crime categories

<table>
<thead>
<tr>
<th>Offense type:</th>
<th>Marijuana distribution</th>
<th>Non-marij. drug distribution</th>
<th>Other offenses</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>12 mos</td>
<td>6 mos</td>
<td>12 mos</td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td>(1)</td>
<td>-0.025****</td>
<td>(0.007)</td>
</tr>
<tr>
<td></td>
<td>(2)</td>
<td>-0.008***</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Offender characteristics:</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>County F.E.:</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Offense category F.E.:</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Time of release F.E.:</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Observations</td>
<td>46450</td>
<td>53530</td>
<td>46450</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.057</td>
<td>0.032</td>
<td>0.087</td>
</tr>
<tr>
<td>Baseline participation</td>
<td>.053</td>
<td>.021</td>
<td>.030</td>
</tr>
</tbody>
</table>

**Notes:** Estimates from the difference-in-difference model (4) using all non-marijuana offenders as the control group. The dependent variable is recidivating in the offense category indicated in the column header within the months indicated. Baseline participation denotes mean of dependent variable in the pre-legalization period. Standard errors are clustered at the offense category level in subsequent columns. * $p < .10$, ** $p < .05$, *** $p < .01$.
Table 7: Public and Police Spending Before and After Legalization

<table>
<thead>
<tr>
<th></th>
<th>Log Public Expenditure</th>
<th>Log Public Safety Expenditure</th>
<th>Log Police Dept. Expenditure</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Marijuana Legalization</td>
<td>0.026</td>
<td>0.013</td>
<td>-0.020</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.039)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>Time trend:</td>
<td>X</td>
<td>–</td>
<td>X</td>
</tr>
<tr>
<td>State by year trend.:</td>
<td>–</td>
<td>X</td>
<td>–</td>
</tr>
<tr>
<td>State F.E.:</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Observations</td>
<td>519</td>
<td>519</td>
<td>519</td>
</tr>
<tr>
<td>R²</td>
<td>0.995</td>
<td>0.997</td>
<td>0.993</td>
</tr>
</tbody>
</table>

Notes: Difference-in-difference estimation of the effect of legalization on log public expenditure, log public safety expenditure, and log policy spending. The sample comprises of an balanced panel of states 2006 to 2016. Observations are at the state by year level throughout. Marijuana Legalization variable equals 1 during post legalization years in states that legalize. Standard errors clustered on state. * p < .10, ** p < .05, *** p < .01

Table 8: Heterogeneous Effects by Experience, Education & Criminal History

<table>
<thead>
<tr>
<th>Age</th>
<th>Education</th>
<th># of Priors</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>No H.S.</td>
<td>H.S. Degree</td>
</tr>
<tr>
<td></td>
<td>Degree</td>
<td></td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>7414</td>
<td>19322</td>
</tr>
<tr>
<td>R²</td>
<td>0.241</td>
<td>0.136</td>
</tr>
</tbody>
</table>

Notes: Estimates from the difference-in-difference model (4) using all non-marijuana offenders as the control group for different sub-populations of marijuana offenders as indicated in the column header. The outcome is recidivism within 12 months of release. Standard errors are clustered at the offense category level in subsequent columns. * p < .10, ** p < .05, *** p < .01
Table 9: Heterogeneous Effects by County Characteristics

<table>
<thead>
<tr>
<th>Heterogeneity by:</th>
<th>Baseline Model</th>
<th>DTO Activity</th>
<th>Border Cnty</th>
<th>Labor Market</th>
<th>Second Chance Act</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post × Marijuana</td>
<td>0.070**</td>
<td>0.057**</td>
<td>0.087**</td>
<td>0.094**</td>
<td>0.383*</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.026)</td>
<td>(0.041)</td>
<td>(0.037)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>× Cartel Presence</td>
<td>–</td>
<td>0.064</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.047)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>× Border County</td>
<td>–</td>
<td>–</td>
<td>-0.095</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.130)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>× Unemployment Rate</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.074*</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.040)</td>
<td></td>
</tr>
<tr>
<td>× Second Chance Fund</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>-0.142*</td>
</tr>
<tr>
<td>($ per inmate)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.082)</td>
</tr>
</tbody>
</table>

Baseline Controls: Yes Yes Yes Yes Yes
Observations: 46450 35299 35299 46450 7815
R²: 0.139 0.134 0.134 0.139 0.137

Notes: Column (1) replicates baseline specification from column (4) of Table 5. Subsequent columns provide estimates of triple-differences models which interacts the main treatment variable with heterogeneity variables. In column (2), the heterogeneity variable is an indicator for cartel activity in an offender’s county of release. In county (3), it is an indicator for if the county of release is a border county. In columns (3) & (4), the heterogeneity variables are unemployment rates and Second Chance Act Funds respectively. The sample is the same as the baseline unless the heterogeneity variable is not available. The outcome is recidivism within 12 months of release. Standard errors are clustered at the offense category level.

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

Table 10: NLSY Results

<table>
<thead>
<tr>
<th>Dep. Var:</th>
<th>Selling “hard” drugs</th>
<th>Log Week Worked</th>
<th>Log Weekly Earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sample:</td>
<td>Dealers:</td>
<td>Dealers:</td>
<td>Dealers:</td>
</tr>
<tr>
<td></td>
<td>All</td>
<td>No Criminal Hist.</td>
<td>All</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td></td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td></td>
<td>(7)</td>
<td>(8)</td>
<td></td>
</tr>
<tr>
<td>Post × Legal State</td>
<td>0.230*</td>
<td>0.238*</td>
<td>0.201**</td>
</tr>
<tr>
<td></td>
<td>(0.136)</td>
<td>(0.140)</td>
<td>(0.096)</td>
</tr>
</tbody>
</table>

Individual F.E. | X | X | X | X | X | X | X | X |
Year F.E. | X | X | X | X | X | X | X | X |
State F.E. | X | X | X | X | X | X | X | X |
State by Year trend: | – | – | – | – | – | – | X | – |

Observations: 890 890 482 482 880 880 462 462
R²: 0.431 0.441 0.503 0.527 0.620 0.629 0.554 0.584
Mean of dep. var. | .149 | .149 | .121 | .121 | 2.72 | 2.72 | 6.16 | 6.16 |

Notes: The sample comprises of all individual-years in the dataset where the individual self-reported selling marijuana during the period from 2009 to 2012. The outcome in columns (1)-(4) is an indicator for if an individual self-report selling “hard” drugs. Log earnings are the annual gross income from work, in logs and in 2015 US dollars. Log weeks worked is the number of weeks respondent reported working. Post × Legal State is an indicator variable that takes a value of one if the individual is residing in legalizing state after the policy change has taken place. All specifications include as control variables individual fixed effects, dataset-year fixed effects, and state fixed effects or state by year trends. Standard errors are clustered at the level of the state. * p < .10, ** p < .05, *** p < .01
References


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Raphael, S. and Weiman, D. (2003). The impact of local labor market conditions on the likelihood that parolees are returned to custody.


## A Appendix Tables

Table 12: Effect of legalization on price levels & price dispersion

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>log $price_{ct}$</th>
<th>log $(price_{ct} - \bar{price}_{ct})^2$</th>
<th>log # of purchases</th>
</tr>
</thead>
<tbody>
<tr>
<td>Legalization</td>
<td>-0.237***</td>
<td>-0.556***</td>
<td>0.064</td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.195)</td>
<td>(0.140)</td>
</tr>
<tr>
<td>Time trend:</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Time F.E.:</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>State F.E.:</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>City F.E.:</td>
<td>–</td>
<td>X</td>
<td>–</td>
</tr>
<tr>
<td># of States</td>
<td>49</td>
<td>3</td>
<td>49</td>
</tr>
<tr>
<td>Observations</td>
<td>3,110</td>
<td>2,805</td>
<td>3,110</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.680</td>
<td>0.302</td>
<td>0.935</td>
</tr>
</tbody>
</table>

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

Notes: Difference-in-difference estimation of the log(sales price), price dispersion, and log number of purchases on a post-legalization indicator. Observations are state by quarter pairs in columns (1)-(2) & (5)-(6) and city by quarters in columns (3)-(4). Standard errors clustered on city level in columns (3)-(4) and state level in remaining columns.