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

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Abstract
It is widely hypothesized that legalization disrupts illicit markets and displaces illegal suppliers, but the consequences for those who are displaced remain poorly understood. In this paper, I use comprehensive administrative data on the universe of offenders in three states that legalized marijuana to study the effect of the policy change on the subsequent criminal and labor activity of convicted dealers. I find that marijuana legalization increased the 9-month recidivism rate of marijuana offenders by 5 percentage points relative to a baseline rate of 11 percent. The results are not explained by changes in 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-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 activity.

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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 are critical public policy concerns (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 (Rolles et al., 2015). Despite this massive effort, 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”. However, the validity of this argument hinges critically on the supply-side consequences of the policy intervention, which to this point, remains poorly understand.

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 specialization is rendered obsolete. Specifically, I study the causal impact of marijuana legalization on the criminal and labor 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 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 in states that legalize,

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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.
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, 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 4 to 5 percentage points more likely to re-enter prison within 9 months of release. The estimated effect is sizable, corresponding to a near 50% increase from a baseline rate of 10 percent. When decomposed by crime categories, I find the overall increase masks two countervailing effects. One, marijuana offenders became less likely to commit future marijuana offenses. Two, 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 within the time window around the policy change. Second, I find police spending did not systematically change following legalization. Lastly, 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 4-5% of former marijuana offenders transitioned to the distribution of other drugs as a consequence of marijuana legalization.

However, the administrative data pertains only to previously convicted marijuana dealers. If incarceration itself leads to heterogenous responses, then the extent to which the effects are generalizable to the overall population of interest remains uncertain. 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

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This 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.
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, 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 activity. 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 large amount of 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-county migration of marijuana offenders upon release. The entry into new criminal sectors possibly precipitated territorial disputes or had otherwise destabilizing effects, as the mortality risk of former marijuana dealers rose sharply post-legalization. I also 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 illicit markets.

With additional administrative data in Oregon and the NLSY, I examine the effect of legalization on participation in the formal labor market. For Oregon offenders, I apply the same identification strategy as earlier and examine the usage of job-search assistance and federal-subsidized work programs, which are important pathways to the labor force for recently released offenders. I document small effect on utilization of these employment services, even amongst sub-populations consisting only of people who are eligible and have not returned to incarceration. With the NLSY data, I fail to detect any increase in weeks worked or income from wages. 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 suggests that legalization presents a critical juncture where marijuana dealers are much more elastic to their local environment: displaced criminals are more within reach of policy and are effectively cheaper to rehabilitate.

In the appendix, I further explore the relevance of the findings in a historical setting – the end of the national Prohibition, through which alcohol became again legalized. Whereas the contemporary analysis centers on changes in criminal “occupations” following legalization, the nature of historical data yields additional insights on the re-allocative costs in terms of criminal productivity and earnings. I show that criminals who switched from bootlegging to another criminal pursuit experienced significant costs in terms of criminal earnings. These findings suggest that the human capital necessary in the commission of crimes can be, in fact, quite specific and partially non-transferable, even between crimes that may appear similar in scope.

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).³

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 do not disambiguate the channels through which legalization affect crime.⁴ Departing from previous studies, my focus on a specific channel – the connection between legalization and spillovers in illicit markets – is relatively unique and maps on well to models of crime considered in economics.

Specifically, I conceptualize legalization largely as a productivity shock in the criminal labor sector. I provide evidence that legalization in one drug 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 (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

³An older literature on the national Prohibition also exists, which is primarily concerned with the effectiveness of regulating alcohol consumption (Miron, 2005).

⁴Propagation 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.
on how criminal behavior responds to changes in the private return to committing a crime. In a recent literature review, Draca and Machin (2015) observe that evidence on such a channel is limited, as previous studies focus on how crime is affected by changes in the return to legal labor market opportunities and enforcement (e.g. Buonanno and Raphael, 2013; di Tella and Schargrodsky, 2004; Levitt, 1997). 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.

The historical episode discussed in the Appendix also relates to a nascent literature that investigates the origins of the organized crime, which often pertains specifically to drug-trafficking organizations. Several recent studies have identified specific features or economic activities whose characteristics and potential profitability may have attracted Mafia activity, shaping its early geographic distribution in Sicily (Bandiera, 2003; Dimico et al., 2017). My results add to this growing line of research by illustrating the evolution of these criminal organizations and their response to policy interventions.

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 outlines the conceptual framework that underpins the study. Section 4 describes the individual micro-data data. Section 5 presents the main findings and estimates re-optimization decisions following displacement. After showing that legalization diverts drug traffic from marijuana to other drugs, Section 6 explores the generalizability of the findings in the Prohibition. Finally, Section 7 offers concluding remarks.

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). Globally, the United Nations Office of Drugs and Crimes (2012) estimated that there are 119 to 224 million users.

Marijuana is actually a common name for the dried leaf and flower of the cannabis genus: composed of cannabis sativa, cannabis indica, and cannabis ruderalis. The slang, derived from the Spanish word ‘marihuana’, was made popular during the onset of the US prohibition and has since become the standard terminology for cannabis in US legislation. I will use the terms marijuana and cannabis interchangeably throughout the the paper.
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.6

Historically, marijuana enforcement in the United States has been punitive, with an emphasis on “supply reduction”. Federal prohibition of cannabis began with the Marijuana Tax Act of 1937, which effectively criminalized possession of the drug except under very specific circumstances.7 Pursuant to 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.8

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 selling to wholesalers 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.

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.

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6Azofeifa et al. (2016) documents considerable variation in the prevalence of marijuana use by geography and demographics.
7See Bonnie and Whitebread (1970) for a detailed history of marijuana prohibition in the United States.
8However, 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.
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 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 24,000 annual income, which is slightly higher than earnings of felons 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. Caulkins et al. (2016), Anderson and Rees (2013), and Miron and Zwiebel (1995) provide more comprehensive summaries of the broad issues surrounding legalization.
ber, 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. The first recreational dispensaries began appearing in Colorado in January 2014 and retail marijuana sales in Washington began July, 2014.

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.\[10\]

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.\[11\]

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

\[10\] As of 2017, there are over 500 active retailers in Coloradan 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.

\[11\] In Ehrlich, for example, the difference between the illegal and legal “wage” determines hours spent generating illegal income versus holding a formal job.
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. 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 Mediating 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 26%, 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 2016).

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

12Displacement of illicit markets for marijuana or other illegal drugs can have important implications given that drug markets often exhibit economies of scale both in supply and demand (Jacobson, 2004). Thereby, spillover from one drug sector to another could have consequences for equilibrium prices.

13The data is described in greater detail in the appendix.
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:

\[ \log(p_{st}) = \beta l_{st} + \sigma_s + \sigma_t + \epsilon_{st} \]  

(1)

where \( p_{st} \) is the average price of marijuana in state \( s \) during quarter \( t \), \( l_{st} \) 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 regressions were run separately for medium and high grades of marijuana. The results are presented in Table 14.

I also estimate a flexible difference-in-differences specification. The leads and lags around the timing of legalization 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 2004, etc.), 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:

\[ \log(p_i) = \sigma_q + \sigma_t + \sigma_c + \epsilon_i \]
\[ \epsilon_i^2 = \delta_t + u_i \]  

(2)

where \( p_i \) is the 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.

Lastly, 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

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\[ ^{14} \] Because the level of the variation is at the state level.

\[ ^{15} \] This 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.
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?

## 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.\(^{16}\) 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 classification distinguishes between the sale and manufacturing of marijuana, or attempt to do so, from the distribution of other drugs. The

\(^{16}\)Several previous studies using the NCRP datasets have validated the reliability of this dataset. Pfaff (2011) compared counts of individuals entering and exiting into state prisons from NCRP (1983-2002) to other official counts such as the National Prisoner Statistics (NPS) Series from the BJS. According to Pfaff, eleven states consistently reported data: California, Colorado, Illinois, Kentucky, Michigan, Minnesota, Nebraska, New Jersey, South Dakota, Virginia, and Washington. Neal and Rick (2014) conduct several checks both internally within the NCRP data and with other data sources such as the NPS using data from 1983 to 2009. After several tests, the authors retain seven states for their analyses: California, Colorado, Michigan, New Jersey, South Carolina, Washington, and Wisconsin.
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 sample consists of individuals released from DOC supervision includes more than 130,000 offenders. Detail on their demographic characteristics and offense history is provided in Table 1. The full population 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, veteran background, health, homelessness, 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 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.

4.0.2 Additional Administrative Data

For the state of Oregon, I supplement the corrections data with additional sources of administrative records. I obtain information on social assistance and employment by merging the offender sample to case files from the Oregon Department of Human Services (DHS).

The data contains case histories of clients served by most DHS programs. Specifically, it includes information on individuals’ eligibility and enrollment in specific programs. I focus on those programs that are related to job search and labor force participation. The DHS, in partnership the Oregon Employment Department, offers employment services to SNAP recipients. These services are typically delivered via WorkSource centers located across the state. They include a variety of training, preparation, and support services designed to assist job seekers.

Two other programs I consider are the Jobs Opportunity and Basic Skills (JOBS) and JOBS Plus programs. These are more structured programs intended for low-income families on public assistance (receiving TANF) to facilitate the transition from welfare to work. Through JOBS, individuals participate in employment and vocational-training services, receive ongoing screening, assessment and case management, while participating in activities such as life skills, basic education, job readiness or work experience that address barriers to self-sufficiency. JOBS Plus is a subsidized work program administered by the state, participants are placed with private or public employers and receive a subsidy for wages paid.17

Panel B of Table 1 reports participation rates by recently released offenders. At the baseline, these programs are utilized by a significant portion of recently released offenders, indicating they are important.

17If participant income from the JOBS Plus employer is less than one would receive from TANF and SNAP, DHS provides supplement to make up the difference.
pathways to the workforce.

For the Washington state sample, I match offenders to the Social Security Death Index, which includes individuals who either had a Social Security card issued in Washington State or had their last known residence in Washington State prior to their death. The records were linked based on names and date of birth. This provides information on risk of mortality. The unconditional probability of dying within 9 months of release for this sample of offenders is shown in Table 1.

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 2015. 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 with near $9,000 coming 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 a legalization 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 and employment, 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 temporal regression discontinuity design (RDD) and a difference-in-differences strategy, in which I compare marijuana dealers that experience legalization to suitable control groups. I present each in turn.

5.1 Regression Discontinuity Design

The regression discontinuity design compares 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, in specifications where I consider the effect on recidivism, I restrict the pre-legalization sample to offenders released sufficiently early, but still close to the threshold, 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:

\[ y^z_{ist} = \beta_1 \mathbb{1}(rel_t > d_s) + f(rel_t) + \delta_s + \gamma X_i + \varepsilon_{ist} \quad (3) \]

where \( y^z_{ist} \) is the outcome (recidivism and employment) of individual \( i \), who was released from prison at time \( t \) in state \( s \), within \( z \) months following 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 between month \( d_s - z - 12 \) and \( d_s + 12 \) in state \( s \), excluding those released between \( d_s - z \) 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. I estimate the model separately for \( z = 9 \) and \( z = 3 \). The longer time horizon maximizes the amount of variation in the outcome variable, whereas the shorter duration allows me to utilize, as control, marijuana dealers released just 3 months before the policy change.

To extend the regression discontinuity approach to my setting, I allow the effect of the running variable to vary flexibly. The inclusion of a flexible time trend, \( f(rel_t) \), require only an 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 or employment, in the \( d \) months following prison release, between the marijuana offenders who spent \( z \) months under prohibition and \( z \) months under legalization. Standard errors are clustered at the prison jurisdiction level.

The main identifying assumption for the RDD to be valid is that all factors other than treatment vary continuously at the threshold. That is, within the study window, there is no selection with respect to date of release. While the validity of this assumption is ultimately untestable, I show that the pre and post-legalization offenders are similar in observables, which lends credibility to the assumption that the two groups are comparable, except for their different legal status post-release. I also conduct McCrary (2008) tests for discontinuity in the distribution of offenders released around the threshold.

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 null hypothesis of equal means is not rejected for the majority of the characteristics. Thus, the covariates appear largely balanced, the differences are not important enough to explain substantial differences in outcome. In particular, I find
no evidence that offenders released in the post-period have greater propensity to recidivate as predicted by observable. Figure 5 shows that there is also no discontinuous change in the number of releases around the time of legalization.

5.2 Difference-in-Differences Specification

The empirical framework outlined above relies exclusively on variation in the time 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 a difference-in-differences strategy that exploits additional sources of cross-sectional variation for identification. Specifically, I estimate the effect of legalization on marijuana offenders holding constant time-invariant marijuana offender specific characteristics or state-wide time-trends that could bias my estimates.

I estimate both non-parametric and parametric difference-in-difference models. I first estimate a fully flexible specification which captures the dynamics of outcomes relative to the year of release. For individual $i$ who was convicted of crime $j$, the basic non-parametric event study specification is as follows:

$$ y_{ijst} = \sum_{k=1}^{r} \beta_k \mu_k + \delta_s + \delta_j + \delta_t + \gamma X_i + \varepsilon_{ijst} $$

where $s$ and $t$ indexes the state and quarter of release. $\{\mu_k\}$ are the relative event time indicators. Specifically, they take value 1 if individual $i$ is a marijuana dealer that is released in quarter $k$ relative to the date of effective legalization. These indicator variables are always 0 for individuals whose conviction was not for marijuana sales, manufacturing or distribution or whose states never legalized. $\delta_s$, $\delta_t$, and $\delta_j$ are state, time of release, and crime fixed effects, respectively.

I choose the same window 24 months around legalization and as in the regression discontinuity design exclude those who are partially treated (i.e. released within $z$ months of legalization). $\beta_k$ captures the effect of introducing legalization on marijuana offender released $k$ quarters from legalization. As is standard, I make the normalization $\beta_{z} = 0$, so that all coefficients represent differences in outcomes relative to the quarter $z$ months before legalization.

Next, to quantify the magnitude and statistical significance and of the estimates, I also estimate the following parametric specification:

$$ y_{ijst} = \beta (Post_{st} \times Marijuana_i) + Post_{st} + Marijuana_i + \delta_s + \delta_j + \delta_t + \gamma X_i + \varepsilon_{ijst} $$

where $Marijuana_i$ 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. To improve precision, I again control for rich set of offender characteristics, $X_i$. The parameter of interest, $\beta$, measures the change in the outcome of marijuana offenders after legalization, as compared
the control.

The causal interpretation of $\beta$, and $\beta_k$, requires the exclusion restriction that the timing of legal-ization 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 econo-metrician.

For my main results, I consider four 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. Third, I utilized a matched sample of offenders that are comparable to marijuana offenders in term of observable characteristics. Lastly, I use marijuana offenders in non-legalizing states. I separately estimate the model for each of those control groups.

The multiple difference-in-differences specifications and their respective 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 upholding event; not be captured by proxies; and not impact any offender group other than marijuana offenders.

6 Results

The results are reported in the various subsections below. Section 6.1 & 6.2 present the central findings of the paper, consisting 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. I then examine another important margin of adjustment: migration (Section 6.4). The subsequent section (Sections 6.5) explores the mechanisms generating the results as well as heterogeneity in the main estimates. Finally, I present corroborating evidence from the NLSY97 in Section 6.6.

6.1 Impacts on Future Criminality

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

Figure 6a and 6b present the RD graphs for recidivism for the sample of marijuana offenders (graph to the left) and for non-marijuana offender sample (graph to the right). The outcome is a indicator equal to one if an individual returns to incarceration within 3 months of release. The pre-treatment periods exclude individuals who were released within 3 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 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 (9 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. Figure 8a and 8b displays the point estimates of the non-parametric event study over a two-year window around legalization, where we normalize the coefficient in the quarter prior to the legalization.18 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.

To assess the magnitude of the findings, I estimate the regression discontinuity and the parametric difference-in-differences model, equations (3) and (5), using 9-month recidivism as the outcome. Table 4 reports the estimates. 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 RD design using three specifications of $f(\text{rel}_t)$ 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 9 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. Column (6) uses a propensity-score matched sample of offenders as counterfactual. Finally, column (6) considers marijuana offenders in non-legalizing states. The point estimates are largely consistent across the four samples, with estimates based on the marijuana offenders elsewhere being slightly larger.19 Overall, the results are comparable in size and qualitatively similar to the RD results.

Table 5 replicates the analyses separately for each state: Colorado, Washington, and Oregon. Reassuringly, the results are not driven by any individual state in the sample.

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18 Figure 9a and 9b show corresponding figure when the sample includes partially treated offenders.
19 Although not shown, the addition of individual covariates has little effect on the estimates.
6.2 Effects by Crime Category

Contrary to expectations, the above estimates imply 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: drug, property, and violent crimes.\(^{20}\) And estimate the DiD model separately for recidivating in each returning offense.

Within drug offenses, 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).\(^{21}\) However, as the next column shows, this decline in marijuana convictions is completely 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. Property crimes also respond, but by a smaller margin.

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.\(^{22}\) 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 Discussions & Interpretations

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

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\(^{20}\)Drug and property crimes constitute the bulk of illicit income in the criminal sector.

\(^{21}\)Within my dataset, Table 16 in the Appendix reports OLS estimates of regressing recidivism in each crime category on whether the individuals had history in the crime. In each case, 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.

\(^{22}\)As shown in Table 7, these transitions are also associated with longer sentence length conditional on recidivism, indicating the new opportunities pursued are arguably more severe and that these dealers became more hardened criminals.
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 exactly.

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.\(^{23}\) I estimate a dynamic difference-in-differences model assigning narcotics offenders as a ‘treated’ group. Figures 10a and 10b plot these ‘placebo’ event study coefficients together with the actual estimates from earlier. The increase in recidivism is exclusive to marijuana offenders. The absence of an effect for narcotics offenders indicates that policing related concerns are unfounded.

Additional, I directly test whether marijuana legalization affected police expenditure. I collect data on state and local public spending, the share of spending on public safety and police departments by state and year. Figure 12, empirically, the effect of legalization on these measures of law enforcement resources is a precisely estimated zero. Altogether, enforcement driven explanations are not compatible with the totality of my findings and are not supported in the data.

The assumption that enforcement did not systematically change further facilitates interpretation. Specifically, it provides sufficient conditions for my crime-specific estimates from Table 6 to serve as lower bounds on the criminal ‘flow’, that is actual changes in criminal participation. To see this formally, I introduce some helpful notations.

Let \(G = 1\) denote the treated group (e.g. marijuana offenders). The binary treatment (e.g. legalization) is represented by the indicator \(z\). Associated with the treatment alternatives are potential outcomes: \(Y_j(z)\), which in this case indicate whether the individual recidivated with crime \(j\). Define \(P_j(z)\) as the potential probability of arrest faced by individual when committing crime \(j\).\(^{24}\)

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

\[
\beta_j = \mathbb{E}[Y_j(1) \mid G = 1] - \mathbb{E}[Y_j(0) \mid G = 1]
\]

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

\[
\beta_j = P_j(1)\mathbb{E}[Y_j^*(1) \mid G = 1] - P_j(0)\mathbb{E}[Y_j^*(0) \mid G = 1]
\]

where \(Y_j^*(z)\) is a latent variable for participating in the crime \(j\). Imposing the additional assumption that the probability of arrest and conviction for committing crime \(j\) stays the same, this expression then

\(^{23}\)The estimates by crime category indicate that marijuana offenders are recidivating in non-marijuana related offenses so increased marijuana enforcement cannot explain the finding.

\(^{24}\)To simplify notation the discussion without loss of generality, I suppress the individual index \(i\) and implicitly condition on the control set \(X\) throughout.
reduces to:

\[ \beta_j = \mathbb{P}_j(1) \left( \mathbb{E}[Y^*_j(1) | G = 1] - \mathbb{E}[Y^*_j(0) | G = 1] \right) \]

Since the first term is bounded between 0 and 1, the change in participation (e.g. second term) 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.

6.4 Migratory Responses & 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, Table 7 shows that, conditional on re-offending, individuals are more likely to recidivate in a different county. The greater geographic mobility is suggestive of increased search duration. As shown in column (3) of Table 9, I also find in border counties where individuals can migrate more easily to neighboring state, the increase in recidivism was much less pronounced. This is consistent with out-migration substituting for sectoral re-allocation in those counties.

I provide additional evidence that these ventures into new criminal opportunities, whether sectoral or geographic, had potentially violent consequences. Table 7 shows the post-incarceration mortality risk of marijuana offenders increased significantly following legalization, possibly due to an escalation of territorial disputes with incumbents or risks inherent to trafficking harsher substances.

6.5 Heterogeneity and Mechanisms

Previous research have emphasized the central role played by 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 government reports on gang activity, I determine if counties in the sample have Mexican 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 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 Mexican 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 effects vary by age, education, and criminal history. I turn age and education into binary variables based on whether the individual’s level of the given variable is above or below the median at the time of his or her release from prison. I create an indicator for whether an individual’s current conviction was his first.

I find a substantial amount of heterogeneity across these characteristics, most notably the increase in recidivism is concentrated amongst the young and less criminally experienced. Table 8 reports the coefficients. Columns (1) & (2) shows that younger offenders have a significantly larger response than older individuals. This accords with findings of Topel and Ward (1992) which show greater job mobility for youths. Offenders without prior convictions also exhibit comparatively higher post-legalization recidivism. Surprisingly, the increase in recidivism was also more pronounced for individuals with high school education, this contrasts with differences in ex ante probability of recidivism along this dimension. Taken together the results suggest that more adaptable offenders were better able to re-position themselves in the post-legalization landscape.

A question that remains is the underlying mechanisms driving the 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 (ADD CITATIONS), suggesting the effect of marijuana legalization on demand for other drugs is, if anything, weakly negative.

6.6 Individual Panel Results Using NLYS97

My 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 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. As discussed in the data section, respondents are asked questions regarding selling marijuana in each wave of the survey. From the answers, I identify marijuana dealers active prior
to the policy change. The sample is restricted to 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 with different state of residence, some of which experience legalization from 2012 onward. Specifically, I implement the following difference-in-difference strategy:

\[
y_{ist} = \sum_{k=-4}^{3} \beta_k D_k + \delta_i + \delta_s + \delta_t + \epsilon_{ist} \tag{6}
\]

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 11a 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 becoming differentially more disposed to selling “hard” drugs following marijuana legalization in states that legalized marijuana.

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.

Figure 11b 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 selling of “hard” drugs after legalization. If the transition and 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 completely explained by the stigma of incarceration.

To analyze the statistical significance and magnitude 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 in this section demonstrate that marijuana legalization dramatically in-
creased 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.

7 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 linked individual wage data, my ability to detect transitions to formal employment is more limited. To compensate, I present results on labor participation that are based on a variety of complementary sources in lieu of exact information on earnings or hours worked for the full sample.

First, I examine the utilization of employment services provided by WorkSource centers in the state of Oregon. These services are designed to assist job search and include a variety of training or education resources. Second, also in Oregon, I explore the take-up of a subsidized work program, JOBS Plus, that is administered by the Department of Human Services, where individuals receiving Temporary Assistance for Needy Families (TANF) can be placed with private or public employers. Lastly, I refer to the National Longitudinal Survey of Youth 1997 (NLSY97), which provides self-reported annual earnings and the number of hours worked.

These data are not totally ideal. The first two measures do not capture employment per se and may understate actual labor force participation. The NLSY97 could be underpowered to reliably identify impact. Nevertheless, with these caveats in mind, I pursue the same difference-in-differences strategy in order to examine margins of adjustment in the labor market responses.

Table 12 shows the effect of legalization on these outcomes. The dependent variables in columns (1)-(5) are indicators for if an offender utilized the program of interest within 9 months of release. In columns (6) & (7), the outcomes are log hours worked and log hourly wages respectively. Column (1) & (3) include all offenders from the recidivism analysis while column (2) & (4) exclude those that recidivated within the 9 month duration. Column (5) makes an additional restriction of only offenders that are eligible for TANF, and thus eligible for JOBS Plus. The last two columns present results for the NLSY97 sample, consisting of all respondents who reported selling marijuana in the three years preceding legalization.

The baseline estimate in column (1) implies that utilization of employment services increased by 2% in response to legalization. When considering only non-recidivists, the point estimated increases to 2.5%. This magnitude of the response appears to be modest given the baseline utilization rate of 30%. The
small effect size is further reinforced by the results in columns (3)-(5), which show the take-up of work program is basically unchanged, irrespective of the sample. Although precision is limited, employment and earnings exhibit no perceptible increase in the NLSY sample. Columns (6) & (7) report the estimated coefficients and standard errors, which are not statistically significantly different from zero.

For the last two columns, I compute the minimum detectable effect size (MDE), that is, the effect that would have been detectable with 80 percent power at the 5 percent significance level ex post (Haushofer and Shapiro, 2016; Duflo et al., 2008). This approach provides an intuitive measure to distinguish tightly identified null results from those that are not statistically significant but for which we cannot rule out meaningful treatment effects with confidence. Because the NLSY97 has a relative small number of ex-offenders, I am not power to detect relatively small effect sizes. The MDEs for log weeks worked and log earnings are 17.8 and 13.5 percent based on the estimated standard errors in columns (6) and (7).

The null findings mask considerable heterogeneity across the age distribution. Using the administrative data, I separately estimate the model for offenders whose age at release was below and above the median in the sample. Although not shown, I find evidence that, following legalization, older offenders became significantly more likely to utilize job training and employment services upon release.

Stitching together the heterogeneity analysis for recidivism and labor market activity, some broad pattern emerges. The relationship between age and the transition to formal employment is the exact inverse of that between age and subsequent criminality. In the sociological literature there has been extensive focus on the concept of a criminal career and how it develops with age (see Piquero et al., 2003). A criminal career is often characterized by four stages: onset, persistence, escalation/specialization and desistance (Sampson and Laub, 2005). Through this prism, the results suggest the effect of criminal displacement depends crucially on when it occurs along this path: for the not yet specialized, displacement incentivized a switch in their intended specialization, while for the more experienced it played a rehabilitative role, accelerating the end of their criminal trajectory. This has broader implications for labor market studies, which analyze the labor market and the nature of informality. A debate in the literature has centered around the question whether criminal and formal labor markets are segmented or integrated. The results here suggest the answer depend crucially on one’s phase in his or her criminal development.

7.1 Mitigating Effect of Government Policy

In this section, I study how public policies and labor market opportunities can address the persistence of criminal path. First, I explore whether the effect of legalization depends on the local labor market conditions faced by released offenders. Given the results in the previous section which shows modest changes in employment, 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 unemployment rates in offenders’ county 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

25The sample size of the NLSY97 preclude any credible test for heterogenous effects and do not permit subgroup analysis.
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 quarter of release to account for time-varying conditions.

I study heterogeneous effects using an interacted specification:

$$y_{ijst} = \beta_2 (Post_{st} \times Marijuana_i \times Unemp_{ct}) + \beta_1 (Post_{st} \times Marijuana_i) + Unemp_{ct} + Post_{st} + Marijuana_i + \delta_s + \delta_j + \delta_t + \gamma X_i + \epsilon_{ijst}$$

(7)

where $Unemp_{ct}$ is the rate of unemployment in county $c$ during the quarter of release $t$.

If labor market opportunities affected re-optimization decisions following legalization, we would expect criminality to be more pronounced in high unemployment areas. The results are presented in Table 5, columns 1. We find that offenders released in areas or periods of higher-unemployment experience greater 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. Surprisingly, prior research does not find strong ties between labor market conditions at the time of prison release and recidivism rates (Bolitzer, 2005; Raphael and Weiman, 2003). My results suggest displaced offenders however, are responsive to changes in labor market conditions.

I turn next to investigate the mitigating effects of more targeted interventions which may be cost-effective to devise, and are thus possibly of greater interest to policymakers. Specifically, 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 locally for programs designed to serve a particular community or jurisdiction.

Thus, I exploit the differential rollout of Second Chance Act funding across locations. This second 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 over the period of 2008 to the present. For each quarter, I tabulate the number offenders released in that quarter and compute the per-offender amount of funding allocated in every county. I make a sample restriction by excluding

26 Recent evaluations of re-entry programs in which minimum-wage jobs are randomly assigned to released offenders also produce mixed results as to whether these employment opportunities can reduce recidivism (Jacobs, 2012; Redcross et al., 2011).
27 Source:
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 the grant.

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 county of release. I employ the following estimating equation:

$$y_{ijst} = \beta_2 (Post_{st} \times Marijuana_i \times Fund_{ct}) + \beta_1 (Post_{st} \times Marijuana_i) + Fund_{ct} + Post_{st} + Marijuana_i + \delta_s + \delta_j + \delta_t + \gamma X_i + \epsilon_{ijst}$$ (8)

where the outcome is recidivism and $fund_{ct}$ is the per-offender grant authorized for county $c$ at time $t$.

Table 13 shows the results for the triple interaction model. The triple interaction term has a negative coefficient, significant at the 10 percent level. This captures the fact that the recidivism-reducing effect of the Second Chance Act grant on marijuana offenders is significantly greater after legalization. To interpret the results, coefficients in column (1) imply that a thousand-dollar increase in grant per offender pre-legalization generates an insignificant change in recidivism amongst marijuana offenders, while column (2) indicates that the same amount is associated with a .07 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 within the reach of public policy and become comparatively cost-effective to rehabilitate.

8 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 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 these channels, and establishing their
relative importance, is crucial to devise cost-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 have been staggering and unabated. 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, it is striking, though, how large a portion of the social costs of drug use today arises from a single source with a broad reach: drug-related crime.

Increasingly 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 varying burdens of individual drugs given their different toxicological and inherent criminogenic effects, and their distinct patterns of consumption and distribution.

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: The figure shows the average price level and the extent of price dispersion in the three study states from two years before legalization to two years after. Panel (a) focuses on the mean 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 graph uses the NLSY97 self-reported data on selling of marijuana. In the left figure, the coefficients are estimates from a regression of criminal involvement on age-fixed effects. 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 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

(a) Pre-legalization

(b) Post-legalization

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.

Figure 5: McCrary sorting test

Notes: The figure implements the sorting test suggested by McCrary (2008) and plots the number of offenders released in each month of release bin. The plotted regressions use the number of observations in each bin as the dependent variable on each side of the cut-off to test if there is a discontinuity in the density of offenders released at the time of policy change.
Figure 6: Effect of legalization by crime type (recidivism within 3 months), RD graphs

(a) Marijuana offenders:  
(b) Non-marijuana offenders:

Notes: The graphs show the effect of marijuana legalization on recidivism within 3 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 15 months out to up to 3 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 9 months), RD graphs

(a) Marijuana offenders:  
(b) Non-marijuana offenders:

Notes: The graphs show the effect of marijuana legalization on recidivism within 9 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 9 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: Event study of legalization, marijuana offenders

(a) Recidivism within 3 months of release:

(b) Recidivism within 9 months of release:

Note: The graph shows the effect of marijuana legalization estimated using the event study specification (equation 4) for full sample states on recidivism within 3 months of release (panel a) and on recidivism within 9 months of release (panel b). The sample exploits variation in treatment status based on the quarter of release. The sample includes offenders released in pre-treatment and post-treatment periods but excludes partially treated offenders; namely, offenders released within 3 months before legalization in panel (a) and offenders within 9 months before legalization in panel (b). The point estimates $\beta_k$ from the event study specification with 95% confidence intervals are plotted. Standard errors are clustered at the offense level. State-month and offense fixed effects are included in all columns.

Figure 9: Event study of legalization, marijuana offenders

(a) Recidivism within 3 months of release:

(b) Recidivism within 9 months of release:

Note: The graph shows the effect of marijuana legalization estimated using the event study specification (equation 4) for full sample states on recidivism within 3 months of release (panel a) and on recidivism within 9 months of release (panel b). The sample exploits variation in treatment status based on the quarter of release. The sample includes offenders released in pre-treatment and post-treatment periods and includes partially treated offenders. The point estimates $\beta_k$ from the event study specification with 95% confidence intervals are plotted. Point estimates for partially treated offenders are shown in grey. Standard errors are clustered at the offense level. State-month and offense fixed effects are included in all columns.
Figure 10: Event study for marijuana and narcotics offenders

(a) Recidivism within 3 months:

(b) Recidivism within 9 months:

Figure 11: Event study of legalization on selling “hard” drugs, NLSY sample

(a) Marijuana dealers:

(b) Marijuana dealers (non-incarcerated):

Note: Plotted are the coefficient estimates from a version of equation (6). 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 (6). 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 and excludes respondents with incarceration history. The dashed lines represent 95% confidence intervals.
Figure 12: Public and Police Spending Before and After Legalization

(a) Public Safety Spending

(b) Police Spending

Note: This figure illustrates the change in differences in public spending between legalizing (treated) and non-legalizing (control) states over time. Specifically, I plot (in solid black) the set of coefficients $\beta_k$ for $k \in \{-6, \ldots, 3\}$ from the following specification: $\log y_{st} = \sum_{k=-6}^{3} \beta_k \cdot (\text{treat}_s \times 1 \{t = k\}) + \delta_s + \delta_t + \epsilon_{st}$, where $s$ indexes the state; $t$ represents the year. $\text{treat}_s$ equals 1 if state undergoes legalization during the period. $1(t = k)$ are relative year indicators. The dependent variable is the public safety spending in panel (a) and police spending in panel (b). The dashed gray lines in the figure also outline the 95-percent confidence interval for the year-specific point estimates.
### Table 1: Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>Non-Marijuana Offenders</th>
<th>Marijuana Offenders</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) (2) (3) (4) (5) (6)</td>
<td></td>
</tr>
<tr>
<td><strong>Obs.</strong></td>
<td>Obs.</td>
<td>Mean</td>
</tr>
<tr>
<td><strong>Panel A: Offender characteristics</strong></td>
<td></td>
<td></td>
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<tr>
<td>Age</td>
<td>131177</td>
<td>36.42</td>
</tr>
<tr>
<td>White (%)</td>
<td>131178</td>
<td>.7356</td>
</tr>
<tr>
<td>Black (%)</td>
<td>131178</td>
<td>.1616</td>
</tr>
<tr>
<td>Hispanic (%)</td>
<td>131178</td>
<td>.0670</td>
</tr>
<tr>
<td>Male (%)</td>
<td>131178</td>
<td>.8748</td>
</tr>
<tr>
<td>High school degree (%)</td>
<td>131178</td>
<td>.2233</td>
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<tr>
<td>Sentence length (days)</td>
<td>131178</td>
<td>71.36</td>
</tr>
<tr>
<td>First admission (%)</td>
<td>131178</td>
<td>.6685</td>
</tr>
<tr>
<td># of Priors</td>
<td>35542</td>
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</tr>
<tr>
<td>Violent (%)</td>
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<td>.1225</td>
</tr>
<tr>
<td>Military (%)</td>
<td>35682</td>
<td>.0323</td>
</tr>
<tr>
<td>Addict (%)</td>
<td>42795</td>
<td>.1403</td>
</tr>
<tr>
<td>Homeless (%)</td>
<td>42795</td>
<td>.1269</td>
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<tr>
<td><strong>Panel B: Labor market variables</strong></td>
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<td></td>
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<tr>
<td>Employment services (%)</td>
<td>52701</td>
<td>.2619</td>
</tr>
<tr>
<td>JOBS program</td>
<td>52701</td>
<td>.0749</td>
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</tbody>
</table>

*Note:* This table presents summary statistics on the full sample of released prisoners from 2012-2015 from the three study states. The offender sample contains one observation per prisoner and labor market summary statistics are presented for the quarter of release. The offender-quarter sample contains one observation for each quarter out of prison.

### Table 11: NLSY Summary Statistics by Incarceration

<table>
<thead>
<tr>
<th></th>
<th>Marijuana Dealers</th>
<th>Marijuana Dealers (Incarcerated)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Obs</td>
<td>Mean</td>
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<tr>
<td>Education</td>
<td>152</td>
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<tr>
<td>Black</td>
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<td>0.2775</td>
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<tr>
<td>Annual Earnings</td>
<td>95</td>
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<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>
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<tr>
<td>Sold hard drugs</td>
<td>153</td>
<td>0.135</td>
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37
Table 2: Evidence of geographical displacement

<table>
<thead>
<tr>
<th></th>
<th>County-Level Analysis</th>
<th>Neighborhood-Level Analysis</th>
</tr>
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<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td></td>
<td>Denver</td>
<td>Portland</td>
</tr>
<tr>
<td>Log Marijuana Arrests</td>
<td>0.580***</td>
<td>0.246**</td>
</tr>
<tr>
<td></td>
<td>(0.060)</td>
<td>(0.096)</td>
</tr>
<tr>
<td>Demographics controls:</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Other crime controls:</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>States in sample</td>
<td>Wa, Co, &amp; Or</td>
<td>Wa, Co, &amp; Or</td>
</tr>
<tr>
<td>Observations</td>
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<td>126</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.640</td>
<td>0.691</td>
</tr>
<tr>
<td>Median # of retailers</td>
<td>22</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 3: Covariate balance

<table>
<thead>
<tr>
<th>Time of release:</th>
<th>Pre-Legalization</th>
<th>Post-Legalization</th>
<th>Differences</th>
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<tbody>
<tr>
<td></td>
<td>Means</td>
<td>Means</td>
<td></td>
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<tr>
<td><strong>Demographic</strong></td>
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<tr>
<td>White</td>
<td>0.639</td>
<td>0.631</td>
<td>-0.008</td>
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<tr>
<td>Black</td>
<td>0.188</td>
<td>0.209</td>
<td>0.021</td>
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<td>Hispanic</td>
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<tr>
<td>Age</td>
<td>34.19</td>
<td>35.14</td>
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<tr>
<td><strong>Criminal</strong></td>
<td></td>
<td></td>
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<tr>
<td>Sentence length (days)</td>
<td>471.2</td>
<td>482.3</td>
<td>11.13</td>
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<td>First admission</td>
<td>0.683</td>
<td>0.667</td>
<td>-0.016</td>
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<td>Priors</td>
<td>1.795</td>
<td>1.771</td>
<td>-0.024</td>
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<td>Violent crimes</td>
<td>0.025</td>
<td>0.027</td>
<td>0.002</td>
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<tr>
<td>Robbery crimes</td>
<td>0.029</td>
<td>0.042</td>
<td>0.013</td>
</tr>
<tr>
<td>Property crimes</td>
<td>0.151</td>
<td>0.166</td>
<td>0.016</td>
</tr>
<tr>
<td><strong>Socio-economic</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High school graduation</td>
<td>0.262</td>
<td>0.277</td>
<td>0.015</td>
</tr>
<tr>
<td>Military service</td>
<td>0.010</td>
<td>0.033</td>
<td>0.023**</td>
</tr>
<tr>
<td>Addiction</td>
<td>0.221</td>
<td>0.180</td>
<td>-0.040</td>
</tr>
<tr>
<td>Homeless</td>
<td>0.102</td>
<td>0.083</td>
<td>-0.019</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>795</td>
<td>522</td>
<td>1307</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 12 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). Column 3 shows the difference in means and the t-test for significance. $p < .10$, $** p < .05$, $*** p < .01$
Table 4: Effect of legalization on the risk of recidivism

<table>
<thead>
<tr>
<th>Control group:</th>
<th>Recidivism within 9 months of release</th>
<th>RDD</th>
<th>Difference-in-Differences</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>None</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>None</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post</td>
<td></td>
<td>0.056***</td>
<td>0.066***</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.015)</td>
<td>(0.018)</td>
</tr>
</tbody>
</table>

Time of release:

- **Linear** trend: X
- **Quadratic** trend: X
- **Cubic** trend: X
- Fixed effects: X X X X X
- Offender characteristics: X X X X X X X X
- County F.E.: X X X X
- Offense category F.E.: X X X

- States in sample: 3 3 3 3 3 3 3 30
- Observations 1307 1307 1307 128380 27452 6801 43846
- $R^2$ 0.204 0.204 0.204 0.145 0.111 0.202 0.099
- Mean of dep. var .111 .111 .111 .139 .124 .143 .131
Table 5: Estimating effects separately for individual state

<table>
<thead>
<tr>
<th>Control group:</th>
<th>None</th>
<th>None</th>
<th>None</th>
<th>Other Offenders</th>
<th>Drug Offenders</th>
<th>Matched Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Washington</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post</td>
<td>0.059***</td>
<td>0.107***</td>
<td>0.107***</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.028)</td>
<td>(0.028)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.042***</td>
<td>0.032***</td>
<td>0.031**</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.009)</td>
<td>(0.014)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>659</td>
<td>659</td>
<td>659</td>
<td>43367</td>
<td>8648</td>
<td>3578</td>
</tr>
<tr>
<td><strong>Panel B: Colorado</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post</td>
<td>0.116**</td>
<td>0.096*</td>
<td>0.091</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.030)</td>
<td>(0.032)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.118***</td>
<td>0.065***</td>
<td>0.138***</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.014)</td>
<td>(0.039)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>246</td>
<td>246</td>
<td>246</td>
<td>31788</td>
<td>12013</td>
<td>1382</td>
</tr>
<tr>
<td><strong>Panel B: Oregon</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post</td>
<td>0.039***</td>
<td>0.036**</td>
<td>0.036**</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.013)</td>
<td>(0.013)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>0.025*</td>
<td>0.018</td>
<td>0.035*</td>
</tr>
<tr>
<td></td>
<td>(. )</td>
<td>(. )</td>
<td>(. )</td>
<td>(0.014)</td>
<td>(0.017)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Observations</td>
<td>311</td>
<td>311</td>
<td>311</td>
<td>53231</td>
<td>8109</td>
<td>2432</td>
</tr>
</tbody>
</table>

* p < .10, ** p < .05, *** p < .01
Table 6: Decomposing effects by crime categories

<table>
<thead>
<tr>
<th>Recidivism in:</th>
<th>Drug Offenses</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Marijuana Distribution</td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td>-0.034***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
</tr>
</tbody>
</table>

Offender characteristics: X X X X X X
County F.E.: X X X X X X
Offense category F.E.: X X X X X X
Time of release F.E.: X X X X X X

Observations 128380 128380 128380 128380 128380 128380
$R^2$ 0.049 0.092 0.176 0.085 0.177 0.131
Baseline participation 0.05 0.01 0.01 0 0 0.01

Table 7: Evidence and cost of re-allocation in criminal labor sector

<table>
<thead>
<tr>
<th>Dep. Var:</th>
<th>Sentence Length</th>
<th>Different County</th>
<th>Mortality Risk</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post × Marijuana</td>
<td>0.386**</td>
<td>0.025***</td>
<td>0.012***</td>
</tr>
<tr>
<td></td>
<td>(0.164)</td>
<td>(0.008)</td>
<td>(0.003)</td>
</tr>
</tbody>
</table>

States in Sample: WA, CO, OR WA, CO WA only
Observations 17,745 17,745 27,880
$R^2$ 0.713 0.037 0.016
Table 8: Heterogeneous Effects by Age, Education, & Criminal Experience

<table>
<thead>
<tr>
<th></th>
<th>Age</th>
<th>Education</th>
<th>Criminal Exp.</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>&lt; 27</td>
<td>≥ 27</td>
<td></td>
</tr>
<tr>
<td></td>
<td>No H.S.</td>
<td>H.S. Degree</td>
<td>First Offense</td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td></td>
<td>0.092***</td>
<td>0.038</td>
<td>0.040</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.026)</td>
<td>(0.043)</td>
</tr>
<tr>
<td>SUR (p-value)</td>
<td>0.099</td>
<td>0.124</td>
<td>0.053</td>
</tr>
<tr>
<td>Observations</td>
<td>20782</td>
<td>74965</td>
<td>20822</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.195</td>
<td>0.128</td>
<td>0.177</td>
</tr>
</tbody>
</table>

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

Table 9: Heterogeneous Effects by County Characteristics

<table>
<thead>
<tr>
<th>Heterogeneity by:</th>
<th>Border Cnty Release</th>
<th>Labor Market Condition</th>
<th>DTO Presence</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Baseline Model</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td>0.053**</td>
<td>0.060***</td>
<td>0.014</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.017)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>× Border County</td>
<td>–</td>
<td>-0.062**</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>–</td>
<td>(0.027)</td>
<td></td>
</tr>
<tr>
<td>× Unemployment Rate</td>
<td>–</td>
<td>–</td>
<td>0.051**</td>
</tr>
<tr>
<td></td>
<td>–</td>
<td>–</td>
<td>(0.024)</td>
</tr>
<tr>
<td>× Cartel Presence</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td>–</td>
<td>–</td>
<td></td>
</tr>
<tr>
<td>Baseline Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Observations</td>
<td>128380</td>
<td>128380</td>
<td>128380</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.120</td>
<td>0.108</td>
<td>0.123</td>
</tr>
</tbody>
</table>

*p < .10, ** p < .05, *** p < .01
Table 10: NLSY Results

<table>
<thead>
<tr>
<th>Dep. Var: Selling “hard” drugs</th>
<th>Sample</th>
<th>All</th>
<th>Non-incar.</th>
<th>Dealers</th>
<th>Dealers</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Post × Legal State</td>
<td>0.230*</td>
<td>0.238*</td>
<td>0.201</td>
<td>0.181*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.121)</td>
<td>(0.140)</td>
<td>(0.154)</td>
<td>(0.105)</td>
<td></td>
</tr>
<tr>
<td>Individual F.E.</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>State by Year trend:</td>
<td>–</td>
<td>X</td>
<td>–</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>896</td>
<td>890</td>
<td>487</td>
<td>482</td>
<td></td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.435</td>
<td>0.441</td>
<td>0.504</td>
<td>0.527</td>
<td></td>
</tr>
<tr>
<td>Baseline mean</td>
<td>.148</td>
<td>.148</td>
<td>.121</td>
<td>.121</td>
<td></td>
</tr>
</tbody>
</table>

Table 12: Labor Market Activity

<table>
<thead>
<tr>
<th>Dep. Var: Employment, JOBS, Log Week, Log Earnings</th>
<th>Sample: All Non-Recid.</th>
<th>All Non-Recid.</th>
<th>NLSY</th>
<th>NLSY</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Employment, Services</td>
<td>0.020*</td>
<td>0.025**</td>
<td>-0.004</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td>(0.111)</td>
<td>(0.011)</td>
<td>(0.018)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>JOBS Program</td>
<td>-0.036</td>
<td>0.040</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.119)</td>
<td>(0.381)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post × Marijuana</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post × Legal State</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>37091</td>
<td>34253</td>
<td>37091</td>
<td>34253</td>
</tr>
<tr>
<td></td>
<td>896</td>
<td>896</td>
<td></td>
<td></td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.100</td>
<td>0.102</td>
<td>0.127</td>
<td>0.126</td>
</tr>
<tr>
<td></td>
<td>0.611</td>
<td>0.336</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 13: Mitigating Effect of the Second Chance Act Grant

<table>
<thead>
<tr>
<th></th>
<th>Pre Legalization (1)</th>
<th>Post Legalization (2)</th>
<th>Triple Differences (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Marijuana</td>
<td>-0.038 (0.042)</td>
<td>0.078 (0.055)</td>
<td>-0.046 (0.041)</td>
</tr>
<tr>
<td>Funding</td>
<td>-0.020* (0.011)</td>
<td>-0.011 (0.018)</td>
<td>-0.017 (0.010)</td>
</tr>
<tr>
<td>Marijuana × Funding</td>
<td>0.021 (0.029)</td>
<td>-0.070* (0.036)</td>
<td>0.022 (0.030)</td>
</tr>
<tr>
<td>Post</td>
<td>–</td>
<td>–</td>
<td>0.054 (0.048)</td>
</tr>
<tr>
<td>Post × Marijuana × Funding</td>
<td>–</td>
<td>–</td>
<td>-0.130** (0.060)</td>
</tr>
</tbody>
</table>

Observations: 8087 3323 11410

$R^2$: 0.199 0.178 0.192

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


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 14: Effect of legalization on price levels & price dispersion

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>( \log ) prices&lt;sub&gt;st&lt;/sub&gt;</th>
<th>( \log (\text{prices}<em>{st} - \text{prices}</em>{ct})^2 )</th>
<th># of submissions</th>
<th>log STRIDE</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Legalization</td>
<td>-0.360***</td>
<td>-0.735***</td>
<td>22.935</td>
<td>0.899</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.231)</td>
<td>(63.747)</td>
<td>(48.906)</td>
</tr>
<tr>
<td>( \text{prices}_{st} )</td>
<td>–</td>
<td>–</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Quality of Marijuana</td>
<td>Medium</td>
<td>High</td>
<td>All</td>
<td>All</td>
</tr>
<tr>
<td>States in Sample</td>
<td>All</td>
<td>All</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>Observations</td>
<td>1145</td>
<td>1146</td>
<td>2211</td>
<td>2211</td>
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<tr>
<td>( R^2 )</td>
<td>0.720</td>
<td>0.945</td>
<td>0.239</td>
<td>0.302</td>
</tr>
</tbody>
</table>

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

#### Table 15: Changes in enforcement

<table>
<thead>
<tr>
<th>Public Expenditure</th>
<th>Public Safety Expenditure</th>
<th>Police Spending</th>
<th># of Parolees</th>
<th># of Arrests</th>
<th># of Prisoners</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Legalization</td>
<td>0.011</td>
<td>-0.000</td>
<td>0.001</td>
<td>-0.017</td>
<td>-0.024</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.082)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Observations</td>
<td>569</td>
<td>569</td>
<td>569</td>
<td>497</td>
<td>547</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.995</td>
<td>0.888</td>
<td>0.826</td>
<td>0.982</td>
<td>0.989</td>
</tr>
<tr>
<td>Dep var mean</td>
<td>16.963</td>
<td>0.036</td>
<td>0.007</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* * p < .10, ** p < .05, *** p < .01
Table 16: Evidence of Specialization and Persistence in Criminal Choice

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full Sample</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Probability of committing the same offense</td>
<td>0.529***</td>
<td>0.569***</td>
<td>0.655***</td>
<td>0.820***</td>
<td>0.678***</td>
<td>0.544***</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.009)</td>
<td>(0.013)</td>
<td>(0.025)</td>
<td>(0.018)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Age</td>
<td>–</td>
<td>0.000***</td>
<td>-0.005***</td>
<td>-0.007***</td>
<td>-0.005***</td>
<td>-0.001***</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Black</td>
<td>–</td>
<td>0.035***</td>
<td>0.044***</td>
<td>0.054***</td>
<td>0.025***</td>
<td>0.030***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.009)</td>
<td>(0.006)</td>
<td>(0.011)</td>
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<td>-0.001</td>
<td>-0.002**</td>
<td>-0.001</td>
<td>-0.002</td>
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<tr>
<td>Male</td>
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<td>-0.091***</td>
<td>-0.088***</td>
<td>-0.105***</td>
<td>-0.114***</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.013)</td>
<td>(0.009)</td>
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<td></td>
<td>0.032***</td>
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<td></td>
<td></td>
<td>(0.001)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sentence</td>
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<td></td>
<td></td>
<td>-0.000***</td>
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<td>(0.000)</td>
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<td></td>
</tr>
<tr>
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<td>Yes</td>
<td>Yes</td>
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<td>Yes</td>
</tr>
<tr>
<td>Quarter of year FE</td>
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<td>–</td>
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</tr>
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<td>81302</td>
<td>81302</td>
<td>19647</td>
<td>37088</td>
<td>11929</td>
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<tr>
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<td>0.009</td>
<td>0.060</td>
<td>0.069</td>
<td>0.064</td>
<td>0.056</td>
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</tbody>
</table>

* p < .10, ** p < .05, *** p < .01
B  Historical Episode: the Repeal of National Prohibition

The above analysis demonstrates the effect of legalization on black market participants in a modern setting. In the last part of the paper, I explore the external validity of findings in a historical context and show the insights are generalizable. Specifically, I focus on a distinct policy experiment from US history: the end of national Prohibition, through which alcohol is decriminalized and legalized.

In the appendix, I explore the validity and relevance of the findings in a historical setting: the end of the national Prohibition. Under Prohibition, criminal organizations monopolized liquor production and distribution. The repeal of the Volstead Act and the legalization of alcohol shut off this key source of revenue available to organized crime. Whereas the contemporary analysis characterized responses in criminal “occupations” following legalization, the nature of historical data yields additional insights on the re-allocative costs in terms of criminal productivity and earnings. Using cross birth-cohort variation in exposure to the Prohibition, I first replicate the basic intuition at the organizational level: following the end of Prohibition, the Mafia re-allocated personnel from bootlegging to narcotics trafficking.

I then show that “switchers” in the Mafia who switched from bootlegging to another criminal pursuit bore a significant transitional cost in terms of criminal earnings. They earned less from their subsequent criminal occupation in 1940 than individuals who started there originally and the loss is increasing in the years spent as a bootlegger. The difference in income has implications for lifetime wealth accumulation as evidenced by homeownership and house values. These findings suggest that the human capital necessary in the commission of crimes can be, in fact, quite specific and partially non-transferable, even between crimes that may appear similar in scope.

Whereas the contemporary analysis pertains to individuals, the historical analysis centers on one particular criminal organization borne out of the illicit liquor trade: the Italian-American Mafia, which profited extensively from bootlegging during the Prohibition. I provide evidence on how the organization responded to alcohol legalization on both the extensive and intensive margin. Specifically, I examine changes in the overall growth of the Mafia following the end of Prohibition as well as changes in the allocation of personnel within the organization.

The research draws upon declassified biographical information on 712 Italian-American Mafia members, active between the 1930s and the 1960s, compiled by the Federal Bureau of Narcotics. The data contains detailed information on criminal history that describes their specific areas of operation within the syndicate. I then match this list of members to the 1940 Census of Population, which gives information on incomes and places of residence. Using cross birth-cohort variation in exposure to the Prohibition, I first replicate the basic intuition at the organizational level: following the end of Prohibition, the Mafia re-allocated personnel from bootlegging to narcotics trafficking.

The historical results add to our understanding in the following important respects. First, by analyzing decision-process at an organizational level, I show how drug-trafficking organizations evolve in response to policy interventions, which we have limited evidence on. Second, the nature of historical data shed light on re-allocative cost in terms of criminal earnings for the displaced individuals. Lastly, the empirical results contribute to a fuller understanding of an important episode from American history.
and its relationship to organized crime, a topic which has received substantial attention in narrative history.

I will briefly overview the historical background of the Prohibition and discuss features of the episode critical to my research design.

**B.1 Historical Background**

Nationwide alcohol Prohibition in the United States was ratified via a constitutional amendment in January 1919. It was the culmination of a prohibitionist wave with origins dating back as far as the 1870s, during the Temperance Crusade. Under this amendment and the Volstead Act, which provided for the enforcement of Prohibition, the manufacture, transportation, and sale of alcohol were prohibited by federal law. Just fourteen years later, in December 1933, Prohibition was repealed in a striking policy reversal.

The near decade and half long experiment facilitated the growth of a sizable black market in alcohol and created demand for a large-scale criminal organization to regulate those markets. In both academic and popular literature, the underground market for illegal alcohol during the Prohibition movement is viewed as a catalyst for the highly organized, geographically diffuse, criminal organization known as La Cosa Nostra, or the American Mafia (Reuter, 1994).

When state and federal governments criminalized alcohol markets, producers operated in absence of legally enforceable contracts. This increased demand for informal contract enforcement among market participants. Street gangs, particularly Italian and Jewish street gangs, are alleged to have stepped into this role. Gangs quickly capitalized and profited from the illegal alcohol market. With their expertise in working outside the institutional boundaries of traditional economic activity, they supplied speakeasies and underground establishments with large quantities of beer and liquor. These complex bootlegging operations used rivers and waterways to smuggle alcohol across state lines. As the illegal market for alcohol grew, so did the influence of these street gangs.

By early 1930s, these gangs consolidated their gains and merged into an organized, multilevel, hierarchical syndicate that is a precursor to modern drug-cartels and trafficking organizations. This conglomerate maintained certain level of decentralization. Each cell or family, comprised of a 10-15 person crew, operate on a semi-autonomous basis and often ran his own business (distillery, distribution ring, etc.) independent of that of other members of the same gang. The Mafia commission, in turn, adjudicated territorial disputes and negotiated agreements between competitors in absence of state-protection or public law-enforcement. The viability of this organizational structure was predicated on the unusually high profits acquired from bootlegging, which far exceeded that of traditional criminal opportunities. Thus, Prohibition was not only a promoting factor in the development of organized crime but an enabling factor.

Yet the end of the Prohibition did not spell the end of the Mafia, which would persist long after alcohol was made legal again. Eventually, other criminal enterprises expanded from the bootlegging profits. When alcohol ceased to be prohibited in 1933, the Mafia diversified its money-making activities to include (both old and new): illegal gambling operations, loan sharking, extortion, protection rackets,
drug trafficking, fencing, and labor racketeering through control of labor unions. In the mid-20th century, the Mafia was reputed to have infiltrated many labor unions in the United States, most notably the Teamsters and International Longshoremen’s Association. This allowed crime families to make inroads into very profitable legitimate businesses such as construction, demolition, waste management, trucking, and in the waterfront and garment industry.

The remaining section establishes empirical support for the historical account above. It investigates the effect of the end of the Prohibition on the evolution of the Italian-American Mafia and provides empirical evidence on the pattern of diversification that allowed the organization to accommodate the elimination of such a key source of revenue. In particular, I analyze the allocation of personnel within the Mafia in order to quantify the extent to which the changing market-structure affected overall Mafia activity.

B.2 Data

To do so, I utilize a unique dataset on criminal profiles of Mafia members collected by the Federal Bureau of Narcotics, a predecessor to the modern FBI.\textsuperscript{28} The data comes from an exact facsimile of a de-classified report, of which fifty copies were circulated within the Bureau starting in the 1950s. The information is compiled from more than 20 years of investigations, and several successful infiltrations by undercover agents (McWilliams, 1990).

The FBN file represents a snapshot of what the authorities knew by 1960. It identifies a total of 832 Mafia members. Of these, 726 were based in the United States. Given that there were an estimated 5,000 members active in the US during those years, the list represents a non-random sample of Cosa Nostra members. More active and more connected mobsters were certainly more likely to be noticed and tracked. I follow Mastrobuoni (2013) and re-weight the data, based on the number of connections each member had, to address this concern.

Figure 13 shows a sample page from the file. Fairly rich biographical information is provided for each Mafia member. Table [TO BE ADDED] summarizes key characteristics. The average age is 48 years, which means that the average year of birth is 1912, right in the middle of the Italian migration wave. The distribution of the year of birth has almost full support within the range 1885 and 1933. Half of the mobsters reside in either New York, or in New Jersey. Most mobsters are either first or second generation immigrants with 29 percent were born in Sicily and another 10 percent in other regions of Italy. The average height is 5.6 feet, the average weight is 176 pounds, irrespective of the weighting.

The data also contains reliable information on the legal and illegal activities that each individual engaged in. Mobsters’ criminal career starts early. They are on average 22 years old when they end up in jail for the first time, and the majority has committed some violent crime. Only 16 percent do not have an arrest record. Table 2 shows the list of legal activities that at least 5 percent of members were involved in. Weighting does little to the distribution of legal activities. Mobsters were involved in racketeering, drug trafficking, gambling and bootlegging, etc. The average mobster specialized in 2.58

\textsuperscript{28}In the 1930s and up to the 1950s the FBN, which later merged with the Bureau of Drug Abuse Control to form the Bureau of Narcotics and Dangerous Drugs, was the main authority to pursue organized crime.
Figure 13: Sample page from FBN file

types of crimes.

Mobsters were also involved in legitimate businesses. They owned restaurants, drugstores or were otherwise involved in the food sector. Real estate, casinos, car dealerships, loan-sharking and import-export were also common businesses. According to the FBN files, by 1960 only 32 percent of gangsters had no businesses, while 43 percent had one, 19 percent had two, and the remaining 5 percent had 3, 4, or 5 different businesses.

We then link these records based on a multitude of variables (name, surname, city of birth, names of family members, the residence address, the year of birth, arrests, etc.) by hand to the 1940 Decennial Census using the genealogical website ancestry.com. I successfully match 489 individuals to their Census record, achieving a high match rate of almost 59%. This compares favorably with match rates from other studies searching for individuals in historical Censuses using ancestry.com, the relative success is likely attributed to the amount of information in the FBN records that we could match to the Census.

The matched data provides me unusually detailed information on the criminal career of the Cosa Nostra members, along with their early life history and background, as well as their socio-economic status in 1940, presumably in part from their criminal endeavors. The 1940 Census was the first U.S. Census to ask questions about highest grade of schooling attained, wage income, whether any non-labor income was earned in the previous year, migration in the past five years at the individual level and it also provides information on homeownership and the house value or rent paid for each household. Campaniello et. al (2015) show mobsters are considerably more likely to report no income, and more likely to report income incompatible with the value of the house where they live, suggesting underreporting. Mobsters are more likely to own a house (33 percent vs. 31 percent), and their house is on average worth about 10
percent more compared to those of their neighbors. Since under or mis-reporting might bias my results I will conduct our analysis considering income, whether they own the house, the value of their home, and their monthly rent payments, as four different measures of their economic status.

The following sections will outline my methodology and results, facilitating an analysis of how the end of Prohibition affected the organization as a whole as well as individual bootleggers within the Mafia.

### B.3 Results

First, I investigate the effect of alcohol legalization on the overall growth of the Italian-American Mafia. To provide evidence on this, I leverage the information on Mafia members’ year of birth and examine the evolution in the flow of entrants into the organization across different cohorts. Specifically, I rely on cross-time variation in exposure to the illegal alcohol markets and compare the number of Mafia members in cohorts that came of age during the Prohibition with later cohorts that missed it. Intuitively, given that Prohibition ended in 1933, individuals born closer to that year would have had less opportunity to profit from illicit liquor markets. To the extent that this loss of profitable opportunities affected incentive to join or pursue criminal careers, this should be reflected in the number of Mafia members observed.

Figure 14a shows the total number of U.S. based Mafia members identified in the file by year of birth. We find that the number of Mafia members born each year is increasing and relatively stable before 1914 and declines sharply thereafter. Consistent with the notion that alcohol prohibition was a prerequisite for the explosion of the Mafia, the growth in membership slowed significantly amongst cohorts that could not sufficiently capitalize on it. The timing largely coincides with what the data reveals about criminal development, individuals born after 1914 would be only 19 by the year national Prohibition ended, younger than when individuals typically begin their criminal career. The sharp break from trend starting at this cohort suggests that the the end of Prohibition disrupted Mafia operations and decelerated the expansion of organized crime.

However, a cohort of individuals shares a large number of experiences, and it remains possible that the change in trend is unrelated to the Prohibition. In order to convince ourselves that the slowdown in
membership growth was truly precipitated by the regulatory change, I delve further into the allocation of personnel and resources within the Mafia. I examine closely the distribution of criminal activities across cohorts. Figure 14b shows that concomitant with the reduction in number of Mafia members per cohort was a simultaneous drop in the share of members who specialized in bootlegging. The fact that the timing coincides with a decline in the importance of bootlegging within Mafia signifies that the decline in growth was driven by the end of Prohibition, which eliminated bootlegging profits. Any violation of the exclusion restriction would require confounding factors that affect participation in the Mafia and specialization within the Mafia, which is independent of alcohol prohibition. Such omitted variables seem implausible.

Figure 14b further shows that Mafia shifted focus towards narcotics trafficking. Prior to the end of Prohibition, members of each cohort was evenly split between bootlegging and drug-trafficking and a smaller and less-diversified criminal organization was left in the wake of its repeal. By showing that the abolition of illicit markets decreased participation in organized crime, my findings capture the flip-side of what has been documented in the previous literature, which shows that childhood exposure to lucrative illegal markets lead to the development of criminal life paths (Sviastschi, 2017).

Having shown the organizational consequences, I turn next to examine the costs of alcohol legalization on bootleggers within the Mafia. I identify bootleggers based on the criminal profile of the Mafia members. Because Prohibition ended in 1933, this represents past involvement in bootlegging and liquor trafficking, rather than contemporaneous participation during the 1950s, when the records was compiled. Nearly all gangsters who were said to be bootleggers also had a secondary or tertiary criminal occupation. I consider these mobsters “switchers” as they transitioned from bootlegging to other criminal activity. I begin by comparing the economic status of these “switchers” to mobsters who were not primarily involved in bootlegging in 1940.

I regress the economic outcome \( y_i \) (income, housing value, and rent) of mobsters in 1940 on a indicator variable for having been a bootlegger, \( \text{Liquor}_i \), controlling for other observable characteristics.

\[
\log y_i = \beta \text{Liquor}_i + \theta X_i + \delta_s + \epsilon_i \tag{9}
\]

where \( \delta_s \) is a set of fixed effects for the mobsters’ state of residence in 1940. And \( X_i \) is a vector of individual level controls that include the criminal activity that mobsters specialized in, age, place of birth, size of criminal network, position within the Mafia hierarchy, arrest record, family composition and structure, body-mass index, and the number of legal business that gangsters own. By controlling for criminal activity, the regressions facilitate a comparison between economic outcomes of previous bootleggers engaged in one criminal task with non-bootleggers engaged in the same task. I weight all regression by the inverse probability weights to account for non-random sampling.

The results are presented in Table 17. As discussed earlier, I examine four separate economic outcomes: i) reported income in 1940, ii) home ownership, iii) rental rate, iv) housing value. Across all four measures of economic status, I detect a negative coefficient on the liquor term. This indicates that mobsters who previously trafficked or distributed liquor had lower earnings and wealth in 1940, seven
Table 17: Mafia Earnings and Wealth in 1940

<table>
<thead>
<tr>
<th>Owns Income</th>
<th>Rent Home Value</th>
<th>Home Residence Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Bootlegging</td>
<td>-0.163**</td>
<td>-328.773*</td>
</tr>
<tr>
<td></td>
<td>(0.067)</td>
<td>(194.756)</td>
</tr>
<tr>
<td>Observations</td>
<td>252</td>
<td>234</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.358</td>
<td>0.344</td>
</tr>
</tbody>
</table>

years after the end of the Prohibition. Previous bootleggers earned less from their future criminal employment after being involuntarily separated from bootlegging due to the change in policy. The difference translates to $300 fewer income annually and house values that were on average $3,000 less than their peers who were never substantially involved in bootlegging.

There are two ways to interpret these findings. First, one can view them as corroborating evidence on the transitional costs of sectoral reallocation in the Mafia, reflecting the long run earnings losses associated with the transition from bootlegging. Second, the differences in earnings profiles between these two groups can also be explained by selection, whereby previous bootleggers would have earned less in irrespective of whether they started in bootlegging or not.

To disentangle these two stories, I explore the dynamics of the economic disparity between bootleggers and non-bootleggers across different cohorts. I estimate a set of Mincer-type regressions for these two groups I follow the long tradition of Mincer regressions and use linear models, where the log of economic outcome is regressed on years of education and potential experience (age).

Table 18: Mincer Regressions

<table>
<thead>
<tr>
<th>Dep var:</th>
<th>Income</th>
<th>Rent</th>
<th>Home Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bootlegger</td>
<td>Non-Boot.</td>
<td>Bootlegger</td>
<td>Non-Boot.</td>
</tr>
<tr>
<td>age 0.012</td>
<td>0.033***</td>
<td>-0.124</td>
<td>0.437*</td>
</tr>
<tr>
<td>(0.014)</td>
<td>(0.008)</td>
<td>(0.340)</td>
<td>(0.255)</td>
</tr>
<tr>
<td>Observations</td>
<td>61</td>
<td>174</td>
<td>42</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.012</td>
<td>0.089</td>
<td>0.333</td>
</tr>
</tbody>
</table>

The coefficients on the age term from the Mincer regressions on income are shown in Column 1 and 2 of Table 18 for bootleggers and non-bootleggers respectively. The returns to experience are equal to about
4 percent for non-bootleggers, but are a precise zero for bootleggers. This results are quantitatively very similar for the other economic outcomes. This implies that the loss in economic status for bootleggers was higher for individuals with longer tenure in bootlegging. Note that this cannot be explained by selection unless older bootleggers were more negatively selected. In absence of that, the results suggest that the transition from bootlegging is associated with a deterioration in the returns to experience, consistent with human capital being partially untransferable across criminal specializations.

Several recent papers have pinpointed the initial conditions that facilitate the rise of organized crime, the results in this section extends this growing line of research by providing evidence on how, once established, organized crime adapt to policy interventions. Overall, I replicate and extend the main insights from the previous section at an organizational level. I show that following Prohibition, the Italian-American Mafia transitioned to being a primarily narcotics trafficking organization. I also show that bootleggers in the Mafia derive less income and have lower economic status in 1940 from the criminal activity they transitioned to. Whereas there are strong returns to experience for non-bootleggers, Mincer regressions this is absent for bootleggers. This suggest that in accordance with the effect of job displacement in the formal labor market, there was significant earnings loss associated with changes in criminal specialization. Human capital accumulated in bootlegging did not transfer to subsequent criminal pursuits and mobsters experienced significant transitional costs associated with reallocation.