Research Article

Xiuming Dong*

Recreational Marijuana Sales Legalization and Monday Work Injury Claims

https://doi.org/10.1515/bejeap-2021-0105
Received March 26, 2021; accepted November 4, 2021

Abstract: An important stylized fact in the literature is that more Workers' Compensation claims for difficult-to-diagnose injuries are filed on Monday than on any other day of the week. This paper studies the impact of recreational marijuana sales legalization on Monday work injury claims. Using restricted-use Workers' Compensation claim data in Oregon and a Difference-in-Differences (DiD) model, I find the probability of overall Monday injuries increase by 4 percentage points after recreational marijuana sales legalization. The event study graphs suggest the medium-term effects appear to equal the short-term effects. Additionally, I do not find strong evidence to support those difficult-to-diagnose Monday injuries disproportionately increase after recreational marijuana sales legalization, suggesting a limited moral hazard of Monday injury claiming behavior after recreational marijuana sales legalization.

Keywords: recreational marijuana, marijuana legalization, workers' compensation, Monday effect

JEL Codes: I12, I18, J28, K42

1 Introduction

Being one of the oldest social insurance programs, Workers' Compensation insures wage losses and medical costs due to work-related injuries. An important stylized fact from the literature is that a disproportionate number of Workers'

***Corresponding author: Xiuming Dong**, Department of Economics, University of Auckland, Sir Owen G Glenn Building, 12 Grafton Rd, Auckland 1010, New Zealand,
E-mail: audrey.dong@auckland.ac.nz. https://orcid.org/0000-0002-0641-340X

Compensation claims occur on Mondays (i.e. the Monday effect), with the largest number of claims being categorized as difficult-to-diagnose such as lower back pain and sprains (e.g. Smith 1992; Vernon 1921). The disproportionate number of Monday claims suggests that workers may use Workers' Compensation to cover weekend injuries. Other possible explanations include "warm-up" or ergonomic effects, or psychological responses to Monday work (Butler, Kleinman, and Gardner 2014; Campolieti and Hyatt 2006).

As of September 2021, 18 states and the District of Columbia in the United States have passed recreational marijuana law. In other words, nearly 1 in 3 Americans now reside in a state where recreational marijuana is available. A growing literature documents the impact of recreational marijuana laws on social, economic, and public health outcomes (e.g. Anderson and Rees 2014; Hansen, Miller, and Weber 2020; Nicholas and Maclean 2019). Somewhat surprisingly, little is known on how recreational marijuana legalization affects local labor markets, especially through Workers' Compensation claims. The epidemiological literature over the past two decades documents that marijuana use can have negative health effects, such as impaired cognitive, short-term memory, and altered judgment (Hall 2015; Hall and Degenhardt 2009; Volkow et al. 2014). This evidence suggests that marijuana use may increase injuries during non-working days or can create negative externalities in the workplace due to the increase of injury risk (Berning, Compton, and Wochinger 2015; Cawley and Ruhm 2011; Grossman 1972).

This paper further explores the Monday effect by studying the impact of recreational marijuana sales legalization (RML) on Monday work injury claims. After states pass the recreational marijuana law, the recreational marijuana market expands rapidly. In Oregon, monthly recreational marijuana sales exceed $65 million within two years of legalization. Meanwhile, workplace drug test positivity rates have shown a strong increase since the passage of the recreational marijuana law. Figure 1 shows the rise of positive drug tests only holds for marijuana and not for other drug categories, suggesting that workers increase recreational marijuana use in response to RML.[1, 2] To measure the number of claims per day of the week, I obtain Oregon restricted-use administrative Workers' Compensation claims data for 2013–2017.

1 The workplace drug positivity rate trend also holds for other western states, as shown in Figure A1.

2 The author visited numerous recreational marijuana dispensaries in Oregon and asked the staff at the dispensaries, "*When is the busiest time of the day in your store?*". The author received almost the same answer for all the visited dispensaries, "*The busiest time is from 5 pm to 7 pm when workers get off work.*" This anecdotal evidence, combined with Figure 1, suggests that workers increase recreational marijuana use after RML in Oregon.

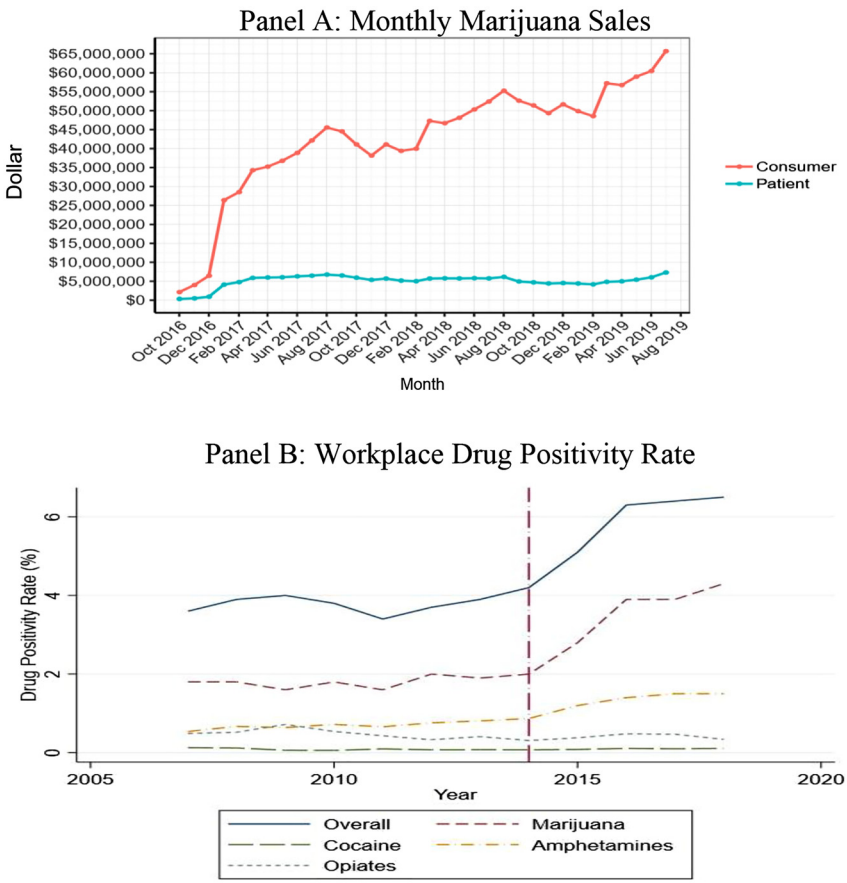

Figure 1: Response to RML in Oregon.
Panel A is the Oregon monthly marijuana sales from the Oregon Liquor Control Commission. The red line indicates recreational marijuana sales, and the blue line represents medical marijuana sales. Panel B is the workplace drug positivity rate by drug category in Oregon from Quest Diagnostics. The vertical dashed red line is the recreational marijuana law passage year.

To identify the causal impact of RML on Monday claims, I implement a generalized DiD model that exploits variation in the county level implementation of Oregon's recreational marijuana law. Even though recreational marijuana law is a state policy, regulations are applied at a more local level in Oregon. Specifically, local governments that have less than 45 percent of voters who voted in favor of RML have prohibited the establishment of licensed recreational marijuana producers, processors, wholesalers, and/or retailers. The vote share rule leads to

a substantial amount of variation in recreational marijuana sales and licensed retail stores across Oregon's 36 counties. The empirical strategy compares the probability of Monday claims before and after RML for counties that sell recreational marijuana to the same difference for counties that do not sell recreational marijuana due to the vote share rule.

I find RML increases the probability of Monday injury claims by 4 percentage points in treated counties relative to control counties after RML. For difficult-to-diagnose injuries, such as sprains/strains injuries, they experience a statistically significant increase in the probability of Monday injuries after the implementation of RML. For easy-to-diagnose injuries, such as fractures or cuts/lacerations, the probability of Monday injury claims is statistically insignificant from zero; however, the insignificant results are primarily driven by increases in the standard errors and not changes in the point estimates. To further explore whether the reduced sample size is driving the insignificant results among easy-to-diagnose injuries, I also implement county-level analysis that compares the absolute number of Monday claims before and after RML. The number of Monday claims results confirm the sample size concern and show that the easy-to-diagnose Monday injury numbers go up after RML.

Furthermore, I use an event study design to allow for the effect of RML on Monday claims to vary over time. The individual claim-level and county-level event study graphs show an immediate increase in the fraction of Monday injuries for overall injuries after RML that levels off in subsequent years. This dynamic pattern suggests the medium-term effects are similar to the short-term effects. The dynamic effects of RML on different injury types are mixed. The individual claim-level and the county-level event study results show there is a significant increase in Monday claims for all injury types in some quarters after RML, but not other quarters, indicating mixed short-term and medium-term effects after the implementation of RML.

Overall, the results indicate that RML increases Monday injuries. The results also suggest that RML increases Monday claims for all injury types. In terms of the magnitude, I do not find strong evidence to support those difficult-to-diagnose Monday injuries disproportionately increase after RML, suggesting a limited moral hazard of Monday injury claiming behavior after RML.

This paper relates to an extensive literature on Workers' Compensation insurance Monday claiming behavior. Smith (1990) argues that workers file Workers' Compensation to cover weekend injuries. Using Workers' Compensation claims, they show a greater proportion of sprains and strains relative to fractures and cuts are reported on Monday. However, Card and McCall (1996) and Ruser (1998) do not find evidence of the moral hazard Monday reporting effect with

Workers' Compensation claims. Campolieti and Hyatt (2006) use Canada's universal government-provided medical insurance, which covers soft-tissue injuries, to identify whether the Monday effect is attributable to health coverage differentials across countries. Comparing the Monday effect in Canada with the United States, they find similar Monday effects and conclude their results are consistent with an ergonomic explanation rather than a moral hazard response. Butler, Kleinman, and Gardner (2014) argue that workers simply do not like Monday work.

This paper also relates to a growing literature studying marijuana laws and their effect on social, economic, and public health outcomes. For example, there is evidence that medical marijuana laws (MMLs) increase the rates of Δ-9-tetrahydrocannabinol (THC) positive drivers (Aston et al. 2016; Aydelotte et al. 2017). Meanwhile, Anderson, Rees, and Tekin (2018) find that MMLs have positive impacts on traffic fatalities involving alcohol, indicating that marijuana and alcohol are substitutes. Nicholas and Maclean (2019) provide evidence of the effect of MMLs on the health and labor supply of older adults. McCaffrey et al. (2010) and Pacula, Chriqui, and King (2003) suggest marijuana has impacts on educational outcomes. Ghimire and Maclean (2020) provide the first evidence of the effect of MMLs on Workers' Compensation claiming among adults. Their results suggest that medical marijuana may allow workers to better manage symptoms associated with workplace injuries and illnesses, which in turn, reduces the need for Workers' Compensation claims, although the magnitude of the effect is very modest. Dong (2020) uses the similar administrative claim data and shows that overall workplace injuries increase after RML.

In terms of RMLs, Anderson and Rees (2014) discuss a potential "worst-case scenario" of recreational marijuana legalization through the lens of research documenting effects on marijuana pricing, marijuana and alcohol use, and crime. They conclude that RMLs lead to increases in marijuana usage and decreases in alcohol usage. Abouk et al. (2021) find that Workers' Compensation receipt declines in response to state recreational marijuana law adoption. They offer evidence that the primary driver of these reductions is likely due to access to an additional form of pain management therapy. By utilizing variations in the county-level recreational marijuana law in Oregon and detailed administrative claim data, my paper complements the results from Abouk et al. (2021) and explores another potential moral hazard channel of Monday injury claiming behavior after recreational marijuana legalization.

Finally, my paper relates to the economic literature on the impact of substance use on workplace injuries. Ohsfeldt and Morrisey (1997) show that alcohol taxes are negatively correlated with workdays lost due to industrial injuries. Kaestner and Grossman (1998) use survey data and show that for young adult males,

there is evidence that drug use is significantly and positively related to workplace accidents. In contrast for young adult females, there is no systematic relationship between drug use and workplace accidents.

In what follows, I provide additional background on marijuana policies and variation in RML implementation across counties in Oregon. Section 3 describes the data and construction of the empirical setting. Section 4 presents the research design and identification strategies. After which Section 5 presents the result and Section 6 concludes.

2 Background

2.1 US Marijuana Laws

Marijuana was entered into the United States Pharmacopeia in 1850 as a treatment for pain, some infectious diseases, bleeding, and other conditions. Prior to the passage of the Marijuana Taxation Act of 1937, the consumption of marijuana for both recreational and medical purposes was legal. The Controlled Substance Act of 1970 re-classified marijuana as a Schedule I substance as a drug with "high potential for abuse and little-known medical benefit".

Oregon became the first state in the United States to decriminalize the possession of small amounts of marijuana in 1973, although cultivation and distribution remained felony offenses. As of July 2021, 36 states and the District of Columbia allow the cultivation, possession, and use of marijuana by doctor recommendation for patients with certain medical conditions. Since 2012, 18 states and the District of Columbia have legalized personal recreational marijuana use. Despite the increase in marijuana laws for a number of states, marijuana is still illegal under federal law.[3]

2.2 Oregon Recreational Marijuana Law

Measure 91 was approved by the Oregon General Election on November 4th, 2014. It has two main components. The first component is what I refer to as "demand-side legalization", which took effect on July 1st, 2015. This allows non-medical

3 The federal government regulates drugs through the Controlled Substances Act (21 U.S.C. § 811), which does not recognize the difference between the medical and recreational use of marijuana.

cultivation and the possession of small amounts of marijuana for adults over the age of 21. Specifically, Oregonians are allowed to grow up to four plants on their property, possess up to eight ounces of usable marijuana in their homes, and up to one ounce on their person.

The second component is what I refer to as "supply-side legalization", which took effect on October 1st, 2015, after Oregon Governor Kate Brown signed an emergency bill. It allows the manufacture and sale of marijuana by/to adults, which is subject to state licensing, regulation, and taxation. State officials began working on establishing a regulatory structure for the sales of marijuana and taxing such sales, with the Oregon Liquor Control Commission (OLCC) to oversee it.

This paper focuses on recreational marijuana sales legalization. One reason is that supply-side interventions often dominate the discussion surrounding drug policy (Alpert, Powell, and Pacula 2018; Dobkin and Nicosia 2009; Dobkin, Nicosia, and Weinberg 2014; Miron and Zwiebel 1991). The other reason is that the scope of demand-side legalization's impact on marijuana usage is limited with only small possession of marijuana allowed. Lastly, there is a time lag in the demand side legalization. Even though Measure 91 was approved at the end of 2014, it did not enter into force until July 2015. Moreover, by the time that sales were legalized the Legislature passed four bills, which made comprehensive reforms to Measure 91 and addressed issues of local control, taxation, and early sales. Therefore, by exploiting the supply-side regulation on recreational marijuana facilities across counties, it better approximates how market participants actually interact with the law.

Important to the identification strategy, even though RML is a state policy, regulations are applied at a more local level. The law provides cities and counties the opportunity to prohibit recreational marijuana businesses in their jurisdiction. Counties that have less than 45 percent of voters who voted in favor the Measure 91 prohibited the establishment of Licensed Recreational Marijuana producers, processors, wholesalers, and/or retailers. Due to rounding, the final decision cutoff is 46 percent. 16 out of the 36 counties have banned recreational marijuana businesses. Prior to December 2015, cities could also implement local bans following the same vote share rules without referring the ordinance to the voter. One caveat is that the vote share rule is not a mandate. For example, Marion County has more than 46 percent of voters who supported RML and decided to restrict marijuana businesses on their unincorporated land.

3 Data and Empirical Setting

3.1 Claim Data

The Workers' Compensation data comes from the Oregon Department of Consumer and Business Services, Workers' Compensation Division.[4] It is restricted use accepted disabling Workers' Compensation claims from 2013 to 2017. These are work-related injuries, where the worker gets medical care and misses more than 3 days of work.[5]

For each claim, I have information on the claimant's demographic characteristics, date and day-of-week of injury, county of injury, occupation, industry, job tenure, and detailed information on the cause of injury, including body part. It also has detailed employer information such as the employer's name, address, and whether the employer company is publicly or privately owned. Since the interest of this study is on the Monday effect, I follow the literature and focus on three primary causes of injuries: (i) sprains/strains, (ii) fractures, and (iii) cuts/lacerations injuries. Together these three injury causes represent over 50 percent of all claims. The remaining dropped claims are a mix of stroke, inflammatory diseases, exposure to chemicals, and other injuries that occur more rarely.

3.2 Recreational Marijuana Sales

The data of monthly administrative marijuana sales records are from the "traceability" system maintained by the OLCC. The system was designed to track each step in the marijuana supply chain, enabling state officials to collect taxes and enforce regulations. To ensure accurate data OLCC employees conduct random in person audits. Violators face penalties that include inventory seizure and destruction. The data set has monthly county level recreational marijuana sales in dollars by product type and quantity of sales. The tracking system records the date, quantity, and price of the transaction and generates a unique identifier.

The local government vote share rule leads to a substantial amount of variation in recreational marijuana sales across counties. For example, up to December

4 Oregon requires nearly all business with one or more employees to carry Workers' Compensation coverage. Sole proprietors without employees are not required to carry coverage for themselves but can optionally purchase coverage. Approved business may self-insure.

5 One limitation of the data is that since the data cannot observe the change of the number of claim applications and denial rate by the day-of-the-week before and after RML, the result using only accepted claims may underestimates the true effect of RML on Monday claims.

Table 1: Tests of excess fraction of Monday injuries.

Type of injury	Observations	Mean (%)	Test statistics
All	46,755	21.14	6.06
Sprains/strains	32,454	21.37	6.00
Fractures	5519	20.35	0.64
Cuts/lacerations	8782	20.83	1.91

The estimation tests whether the proportion of Monday injuries exceeds 20% in a one-tailed test.

2017, total recreational marijuana sales ranged from $0 to $183 million across counties. Hence, I use total sales of recreational marijuana in each county as the recreational marijuana exposure measure. This measure serves as a proxy for the interaction of actual demand and supply of recreational marijuana.[6]

3.3 Measuring the Monday Effect

Before examining whether RML leads to changes in Monday filings I document overall Monday filing trends in Oregon. If work hours are evenly spread over the week, one test of the Monday effect is to determine whether the proportion of injures reported on Mondays is greater than 20 percent. I first present *t*-tests of whether the mean proportion of Monday claims is greater than 20 percent in Table 1.[7] When including all injury types, I find that an excess proportion of total injuries are reported on Mondays (t-statistics = 6.06). For sprains/strains there is also an excess proportion on Monday claims (t-statistics = 6.00). Consistent with the literature, injuries involving fractures (t-statistics = 0.64) and cuts/lacerations (t-statistics = 1.91) are not disproportionately reported on Mondays.

Next, I examine the distribution of work week claims to see how Mondays compare to other days. Figure 2 presents the distribution of several types of injuries. The figure shows consistent evidence with the *t*-tests in Table 1. Relative to all injuries, difficult-to-diagnose injuries (i.e. sprains/strains) are more frequently reported on Mondays than on other days of the week. There is also a large drop

6 Note that the OLCC starts to collect administrative sales data from July 2016, so the total sales are the sum of the sales (in dollars) from July 2016 to December 2017.

7 To further explore the change of the absolute number of claims by each day-of-the-week, Table A1 presents the summary statistics on claim numbers over time and weekdays.

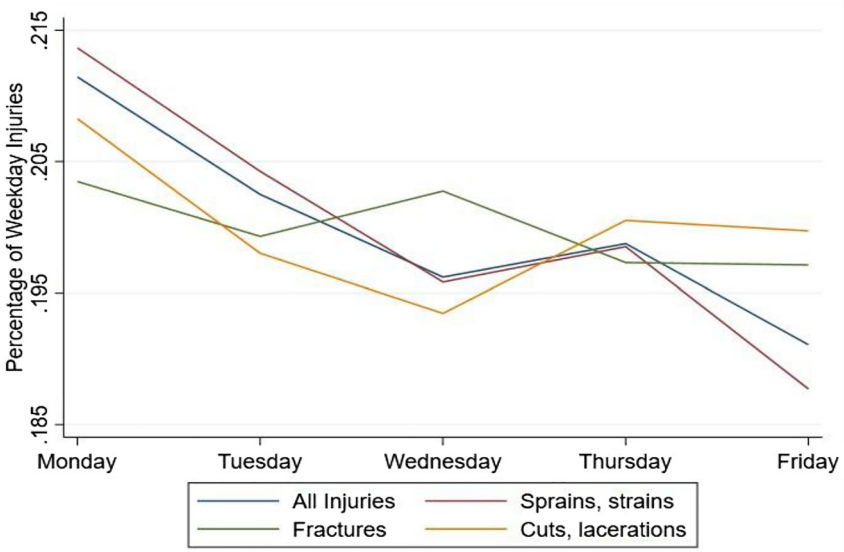

Figure 2: Distribution of injuries by day of the work week.

in the proportion of injuries reported on Friday.[8] This is less the case for blunt trauma injuries such as fractures and cuts/lacerations.

4 Empirical Strategy

4.1 Difference-In-Differences

To estimate the causal impact of RML on Monday injury claims I follow Hansen (2016) and combine a linear probability model with a DiD empirical strategy as shown in Eq. (1):

$$\text{Monday}_{\text{ioct}} = \gamma \text{Treat}_{\text{c}} \times \text{After}_{\text{t}} + \theta_{\text{o}} + \theta_{\text{t}} + \theta_{\text{c}} + X_i' \alpha + \varepsilon_{\text{ioct}}. \tag{1}$$

Where $\text{Monday}_{\text{ioct}}$ is a binary variable that equals 1 when worker i is injured on Monday, and 0 otherwise. Treat_{c} equals 1 when county c has positive recreational marijuana sales over the analysis period. After_{t} equals 1 when the monthly

8 The large drop of sprains/strains injuries on Friday may also indicate that the distribution of work effort is not likely to be evenly distributed across the days of the week.

time period t occurs when recreational marijuana sales passed state legalization, starting from October 2015. θ_{o}, θ_{c} and θ_{t} are occupation, county, and time fixed effects. This accounts for a wide range of unobservable that could potentially be correlated with Monday injury trends. I also include additional worker-specific controls with the vector X_i (e.g. age range, gender, etc.) that are readily available in the restricted-use data. $\varepsilon_{\mathrm{ioct}}$ is the error term. The standard errors are two-way clustered by county and year.[9] Therefore, the coefficient γ provides the reduced form causal estimate of RML on Monday work injury claims.

To further explore the mechanism of the impact, I offer additional evidence by estimating Eq. (1) separately for different injury types. Intuitively, if moral hazard is present then the relative Monday injury claims should be higher for injuries or situations that exhibit greater asymmetries of information (i.e. harder or more difficult to diagnose). If this holds, we expect RML increases Monday sprains/strains injuries, rather than blunt trauma injuries such as cuts/lacerations or fractures.

4.2 Event Study

I also estimate and visually illustrate the effect of RML on Monday injuries using an event study model. Specifically, I decompose the binary $\mathrm{After}_{\mathrm{t}}$ time indicator in Eq. (1) into a series of quarterly leads and lags around the effective date of RML (i.e. October 2015).[10] To do this I construct indicators for eight quarters or more through one quarter in advance of the RML, the effective quarter of RML, and one through eight quarters or more following the RML. I center the data around the implementation of RML, with the quarter prior to the passage as the reference time. The event study equation is as follows:

$$\mathrm{Monday}_{\mathrm{ioct}} = \sum_{s=-8}^{8} \gamma_s \mathrm{Treat}_{\mathrm{c}} + \theta_{\mathrm{o}} + \theta_{\mathrm{t}} + \theta_{\mathrm{c}} + X_i'\alpha + \varepsilon_{\mathrm{ioct}}. \tag{2}$$

The explanatory variables include 17 policy dummies, $\sum_{s=-8}^{8} \gamma_s \mathrm{Treat}_c$, which denote eight quarters before RML to eight quarters after. Quarter 0 denotes RML implementation quarter (fourth quarter of 2015). The coefficients of interest, γ_s, measure how the effect of RML on Monday injuries evolves over time. In the regression analysis, γ_{-1} is omitted and serves as the baseline. All other controls are the same as the previous DiD analysis.

9 Standard errors are similar when using bootstrap clustering (Cameron and Miller 2015; Cameron, Gelbach, and Miller 2008).

10 An alternative strategy is to allow the treatment effect to vary by month. However, monthly level analysis yields more imprecise estimates given the sample includes 36 counties per month.

The event study model offers two important extensions to the DiD model. First, visual examination of the normalized pre-RML trends allows for the assessment of the plausibility of the common time trends assumption that is necessary for DiD models to recover estimates of causal effects. Second, by including the lag variables the event study allows for treatment effects to vary over time. This allows observation of the dynamic impact of RML and avoids functional form assumptions on the effects. For example, if employees are only using recreational marijuana immediately after the RML, differentiating between short- and medium-term effects may be crucial.

5 Results

5.1 DiD Results

Table 2 presents the estimated coefficients DiD interaction term from Eq. (1) for a number of alternative model specifications. Column (1) omits the individual characteristics and occupation fixed effects. The estimate is positive and significant at the 5 percent level, suggesting that the probability of Monday injury claims is about 4 percentage points higher for treated relative to control counties after the implementation of RML. The estimates are insensitive to the inclusion of individual characteristics and occupation fixed effects as shown in columns (2) and (3).

Table 2: The effect of recreational marijuana sales legalization on probability of Monday claim.

Sample	**(1) All injuries**	**(2) All injuries**	**(3) All injuries**	**(4) Sprains, strains**	**(5) Fractures**	**(6) Cuts, lacerations**
Treat*after	0.038**	0.037**	0.037**	0.036**	0.028	0.049
	(0.016)	(0.016)	(0.017)	(0.018)	(0.057)	(0.046)
Individual characteristics		Y	Y	Y	Y	Y
Occupation dummies			Y	Y	Y	Y
N	46,755	46,755	46,755	32,454	5519	8782

The table reports the DiD estimates from Eq. (1). The dependent variable in all regression is whether a claim occurred on a Monday or not. Standard errors are two-way clustered at the county and year level in parentheses. $^{*}p < 0.10$, $^{**}p<0.05$, $^{***}p < 0.01$.

Columns (4) to (6) of Table 2 offer additional evidence by estimating Eq. (1) for different injury types. The results imply that RML increases the probability of Monday sprains/strains injuries by about 4 percentage points in treatment counties relative to the control counties after the implementation of RML. The probability of Monday injury claims for easy-to-diagnose injuries, such as fractures or cuts/lacerations, does not statistically significant change after RML, although the point estimates are all positive with similar magnitudes. Given the strongly reduced sample sizes for factures and cuts/lacerations, interpretation should be careful with the easy-to-diagnose injuries. Moreover, Eq. (1) only looks at the probability of Monday claims but not the absolute number of claims, while it may be that the absolute Monday claim numbers went down but the relative numbers increased after RML. To address these concerns, Table A2 adds a specification that examines the impact of RML on the number of Monday injury claims, as well as estimating the equation separately for different injury types. The results are consistent results with the linear probability model in Table 2 and imply that RML increases the number of Monday injuries by about 0.410 in treatment counties relative to the control counties. More interestingly, columns (3) and (4) of Table A2 show that RML significantly increases the number of Monday easy-to-diagnose claims as well, providing some support that the insignificant results in the main specification are due to decreases in the sample sizes.

Overall, the results indicate that RML increases the Monday injuries. The results also suggest that RML increases Monday claims for all injury types. In terms of the magnitude, I do not find strong evidence to support those difficult-to-diagnose Monday injuries disproportionately increase after RML, suggesting a limited moral hazard of Monday injury claiming behavior after RML.

Another related concern with the main results is that if the fraction of Monday injuries is going up after RML, mechanically the proportion of injuries on other days of the week must decrease. In other words, we may be concerned the increases in Monday injuries are driven by changes in one particular day of the week. To investigate this Table 3 examines the effect of RML on the fraction of claims for each day of the week. Column (1) reproduces the Monday results from Columns (3) to (6) of Table 2 to aid comparisons. The remaining columns consider Tuesday through Thursday with the majority yielding statistically insignificant estimates. This implies a shift in the workplace injuries distribution towards Monday injuries after RML and not the decrease of injuries on only a single day.

Finally, Table A3 presents several additional robustness checks. I first add a placebo test into the main specification. For this test, I perform an additional DiD estimation using a "fake" treatment time that is one quarter before the recreational marijuana sales legalization. Additionally, I exclude the post-RML period for the new estimation sample. Panel A presents this test and shows that for all injury

Table 3: The effect of recreational marijuana sales legalization on claim day of week.

Variables	(1) Monday	(2) Tuesday	(3) Wednesday	(4) Thursday	(5) Friday
			All injuries		
Treat*after	0.037**	−0.000	−0.022	−0.013	−0.001
	(0.017)	(0.013)	(0.014)	(0.015)	(0.020)
			Sprains, strains		
Treat*after	0.036**	0.002	−0.030	−0.029	0.021
	(0.018)	(0.024)	(0.022)	(0.021)	(0.019)
			Fractures		
Treat*after	0.028	−0.012	0.014	−0.009	−0.026
	(0.057)	(0.046)	(0.053)	(0.046)	(0.046)
			Cuts, lacerations		
Treat*after	0.049	−0.003	−0.002	0.027	−0.070*
	(0.046)	(0.042)	(0.038)	(0.049)	(0.036)

The table reports the DiD estimates from Eq. (1). The dependent variable in all regression is whether a claim occurred on a day of the week. Standard errors are two-way clustered at the county and year level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

types when using non-treatment time, the effect of RML on the probability of Monday claim goes away, which is consistent with the expectation. The result is also not sensitive when changing the "fake" treatment time. Secondly, in Oregon there are several counties that have populations less than 2000 people. To explore whether the less populated counties are driving the results, I drop these counties that have less than 2000 people from the sample and re-estimated the DiD model. The results are consistent with the main results in Table 2, and indicate they are not driven by the "outlier" counties. Finally, I observe that the western Oregon border counties are more likely to legalize marijuana sales. As another robustness check, in panel C I drop these counties from the sample and re-estimated the DiD model and find consistent results.

5.2 Event Study Results

Figure 3 presents the event study results from Eq. (2). The four outcomes of Monday injury claims that I considered with the DiD analysis are shown in four separate panels. The x-axis shows the normalized time dimension of quarters since RML. The y-axis shows the treatment effect in each unit. I also plot the point estimates with 95 percent confidence intervals. All four panels confirm the

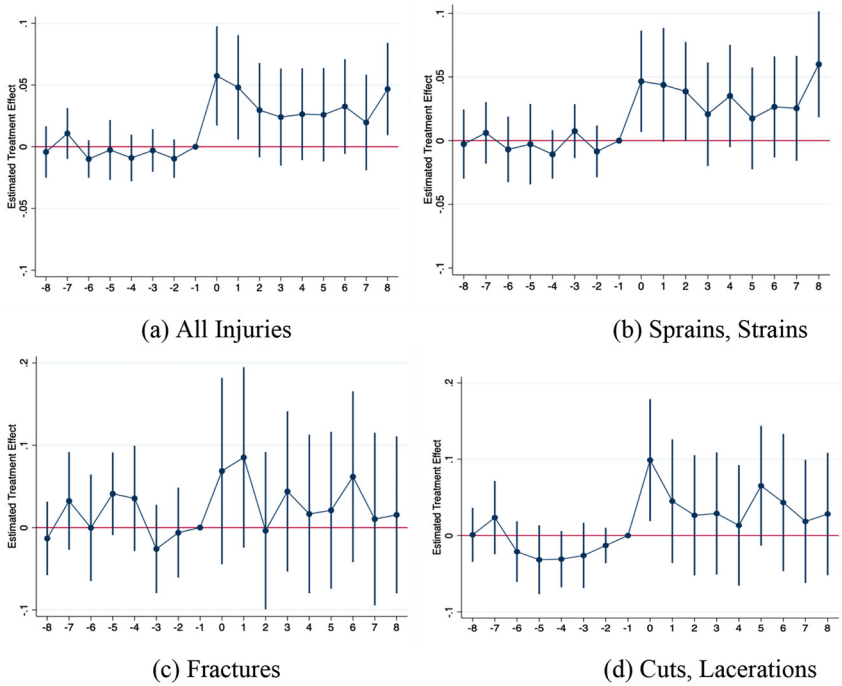

Figure 3: Event studies from difference-in-differences models.
The graphs show the event study based on the DiD model as in Eq. (2). The errors terms are two-way clustered at the county and year level and the gray bars depict 95% confidence intervals.

findings in Table 2 and additionally illustrate how the treatment effects evolve over time. More importantly, all estimates for the time prior to RML are small in magnitude and statistically insignificant, suggesting there are no differential trends between treatment and control group before RML. The full results are in Appendix Table A4.

Panels (a) show after RML there is an immediate increase in the fraction of Monday injuries that level off quickly for all injuries. This dynamic pattern of RML effects is important. In particular, it suggests significant increases in the fraction of Monday injuries during the first couple of quarters after RML. In subsequent quarters, the fraction of Monday injuries stays relatively constant although it is not possible to reject null effects. In fact, no further significant increases in the following quarters except after 8 quarters and more after implementation of RML. Thus, the medium-term effects appear to equal the short-term effects.

Panels (b), (c) and (d) show the dynamic effects of RML on Monday injuries for sprains/strains, fractures and cuts/lacerations injuries. In all cases, there are no observed substantial pre-RML differential trends. After RML there is no significant increase in Monday injuries for fractures, while sprains/strains and cuts/lacerations experience an instant increase that lasts one quarter that returns to insignificant levels afterward. This is in line with the previous DiD results. However, the large standard errors for fracture and cuts/lacerations injuries confirms that the statistically insignificant results might be due to the small sample size.[11]

6 Conclusions

The landscape of marijuana policies is changing rapidly. This has led to a heated discussion in its impact on social, economic, and public health outcomes in both positive and negative aspects. While the RML has received much attention in the literature, little is known how RML affects local labor markets, especially through workplace injuries. To the best of my knowledge, it is the first study to analyze the relationship between RML and Monday work injury claims. Using administrative Workers' Compensation claims data in Oregon from 2013 to 2017, I find RML increases the probability of Monday injury claims by 4 percentage points in treated counties relative to control counties after the implementation of RML.

The event study graphs show an immediate increase in the fraction of overall Monday injuries. This dynamic pattern suggests the medium-term effects are similar to the short-term effects. The dynamic effects of RML on different injury types are mixed. The individual claim-level and the county-level event study results show there is a significant increase in Monday claims for all injury types in some quarters after RML, but not other quarters, indicating mixed short-term and medium-term effects after the implementation of RML. In terms of the magnitude, I do not find strong evidence to support those difficult-to-diagnose (i.e. sprain/strains) Monday injuries disproportionately increase after RML, suggesting a limited moral hazard of Monday injury claiming behavior after RML.

11 Similar to the DiD analysis, Figure A2 shows the event study graphs using the county-level DiD model, where the dependent variable is the number of Monday claims in county c and month t. Panel (a) of Figure A2 shows the consistent results with the linear probability model in the main specification. It suggests significant increases in the number of Monday injuries during the first quarter after RML, indicating a short-term effect. Panels (b)–(d) show there is a significant increase in the number of Monday injuries for all injury types in some quarters after RML, but not in the other quarters, indicating mixed short-term and medium-term effects.

It is important to mention the limitations of this paper. One is that there are different forms of recreational marijuana sales legalization in the United States and other countries, future research on the impact of sales legalization on Workers' Compensation claiming behavior in different legalization contexts is crucial to understand the generalized sales legalization impact on Monday injury claims. Third is that some research, such as Hansen, Miller, and Weber (2020) and Hao and Cowan (2020), has shown that there are spillover effects of recreational marijuana sales legalization across state borders. This evidence indicates that the intent to treat effect identified in this paper serves as the lower bound of the true impact of RML. Last but not the least, the effect identified in this paper is the short/medium-term effect, based on the results, future research on the long-term impact of sales legalization on other economic agents' behaviors, such as firm, workers, the insurance company, is crucial to understand the full labor market impact of recreational marijuana sales legalization.

Acknowledgement: I thank Gary Engelhardt, Jeffrey Kubik, and Ross Jestrab for their helpful comments. I also thank the editor and the anonymous referee for their valuable suggestions. I appreciate the assistance of the Oregon Department of Consumer and Business Services, Oregon Workers' Compensation Department, and Oregon Liquor Control Commission for their data support. The statements, findings, conclusions, views, and opinions contained and expressed herein are not necessarily those of Oregon Workers' Compensation Department, Oregon Occupational Safety and Health Administration or any of their affiliated or subsidiary entities. All errors are my own.

Appendix

Table A1: Summary statistics on claim numbers over time and weekdays.

Year	Monday	Tuesday	Wednesday	Thursday	Friday
2013	1916	1826	1741	1722	1765
2014	1924	1774	1816	1825	1733
2015	1992	1896	1749	1846	1661
2016	2030	2011	1969	1908	1873
2017	2024	1961	1899	1992	1902

The table reports the claim numbers by each day-of-the-week from 2013 to 2017.

Table A2: The effect of recreational marijuana sales legalization on number of Monday claim.

Sample	(1) All injuries	(2) Sprains, strains	(3) Fractures	(4) Cuts, lacerations
Treat*after	0.410***	0.112	0.146***	0.152**
	(0.134)	(0.109)	(0.054)	(0.062)
Control mean	4.468	3.197	0.464	0.807
N	2160	2160	2160	2160

The table reports the DiD estimates using county-level analysis, where the dependent variable is the number of Monday claims in county *c* and month *t*. Standard errors are two-way clustered at the county and year level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Robustness checks: difference-in-difference regression.

Sample	(1) All injuries	(2) Sprains,strains	(3) Fractures	(4) Cuts,lacerations
		Panel A: placebo test		
Treat*after	−0.012	0.009	−0.118	0.019
	(0.038)	(0.031)	(0.117)	(0.111)
N	25,735	18,193	2931	4611
		Panel B: dropping small counties		
Treat*after	0.038**	0.042**	0.029	0.046
	(0.018)	(0.022)	(0.044)	(0.037)
N	46,677	32,402	5507	8768
		Panel C: dropping border counties		
Treat*after	0.035*	0.043**	0.027	0.027
	(0.019)	(0.021)	(0.063)	(0.049)
N	32,892	22,621	4076	6195

Panel A reports the placebo test of the DiD estimates in Eq. (1) but defines After = 1 if it is July 2015, which is one quarter before the recreational marijuana sales legalization. Panel A also excludes the post-treatment period (After October 2015) from the original estimation sample. Panel B excludes the 5% less populated counties (i.e. 2000 people) in Oregon into the analysis. Panel C excludes the western Oregon border counties into the analysis. Standard errors are two-way clustered at the county and year level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A4: Event study analysis.

	(1) All injuries	(2) Sprains, strains	(3) Fractures	(4) Cuts, lacerations
−8	−0.004	−0.003	−0.013	0.001
	(0.011)	(0.013)	(0.023)	(0.018)
−7	0.011	0.006	0.032	0.024
	(0.010)	(0.011)	(0.030)	(0.024)
−6	−0.010	−0.007	−0.000	−0.021
	(0.008)	(0.011)	(0.033)	(0.020)
−5	−0.003	−0.003	0.041	−0.032
	(0.012)	(0.014)	(0.025)	(0.023)
−4	−0.009	−0.011	0.035	−0.031*
	(0.010)	(0.010)	(0.032)	(0.019)
−3	−0.003	0.007	−0.026	−0.026
	(0.009)	(0.010)	(0.027)	(0.022)
−2	−0.010	−0.008	−0.006	−0.013
	(0.008)	(0.010)	(0.028)	(0.012)
0	0.057***	0.047*	0.069	0.099**
	(0.020)	(0.024)	(0.057)	(0.040)
1	0.048**	0.044*	0.085	0.045
	(0.021)	(0.025)	(0.056)	(0.041)
2	0.030	0.039	−0.004	0.027
	(0.019)	(0.024)	(0.048)	(0.040)
3	0.024	0.021	0.044	0.029
	(0.020)	(0.024)	(0.049)	(0.041)
4	0.026	0.035	0.016	0.013
	(0.019)	(0.023)	(0.049)	(0.040)
5	0.026	0.017	0.021	0.065
	(0.019)	(0.023)	(0.048)	(0.040)
6	0.033*	0.027	0.062	0.043
	(0.019)	(0.023)	(0.053)	(0.046)
7	0.020	0.025	0.010	0.019
	(0.020)	(0.024)	(0.053)	(0.041)
8	0.047**	0.060**	0.015	0.028
	(0.019)	(0.024)	(0.048)	(0.041)
Observations	46,755	32,454	5519	8782

The table reports the event study estimates from Eq. (2). The dependent variable in all regression is whether a claim occurred on a Monday or not. Standard errors are two-way clustered at the county and year level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

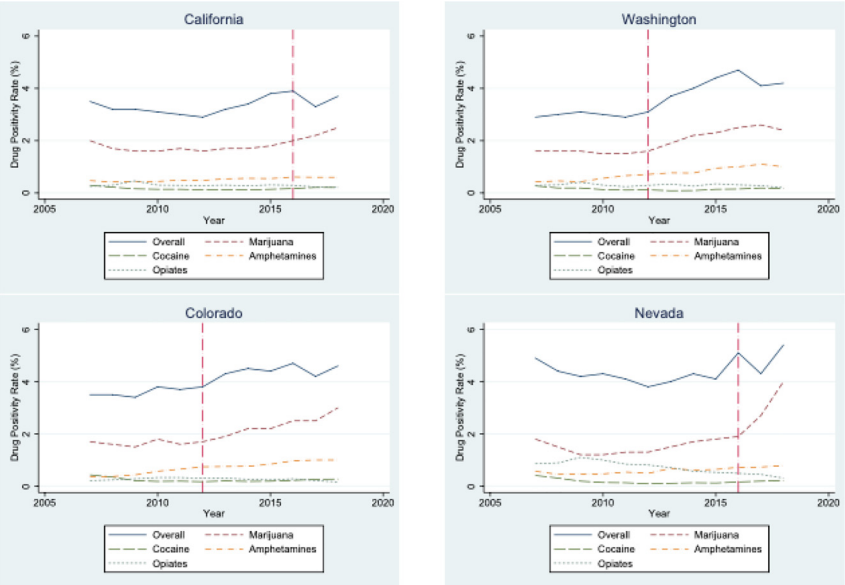

Figure A1: Workplace drug positivity rate by drug category and state.
Data is from Quest Diagnostics. The vertical dashed red line is the recreational marijuana law passage year for each western state.

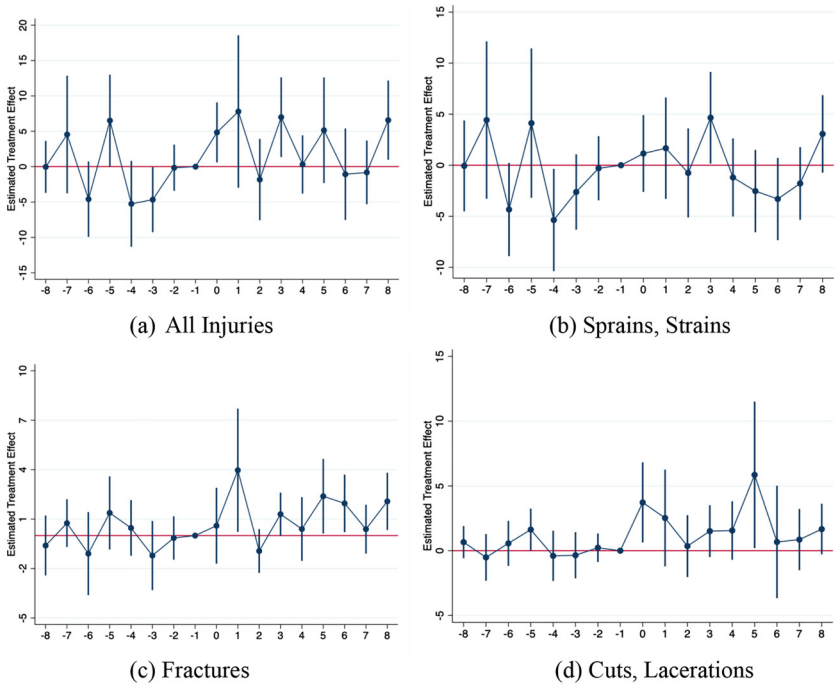

Figure A2: Event study for number of Monday claims models.
The graphs show the event study based on the DiD model as in Table A2. Standard errors are two-way clustered at the county and year level, and the gray bars depict 95% confidence intervals.

References

Abouk, R., K. M. Ghimire, J. C. Maclean, and D. Powell. 2021. "Does Marijuana Legalization Affect Work Capacity?" In *Evidence from Workers' Compensation Benefits. No. w28471*. National Bureau of Economic Research.

Alpert, A., D. Powell, and R. L. Pacula. 2018. "Supply-Side Drug Policy in the Presence of Substitutes: Evidence from the Introduction of Abuse-Deterrent Opioids." *American Economic Journal: Economic Policy* 10 (4): 1–35.

Anderson, D. M., and D. I. Rees. 2014. "The Legalization of Recreational Marijuana: How Likely Is the Worst-Case Scenario." *Journal of Policy Analysis and Management* 33 (1): 221–32.

Anderson, D. M., D. I. Rees, and E. Tekin. 2018. "Medical Marijuana Laws and Workplace Fatalities in the United States." *International Journal of Drug Policy* 60: 33–9.

Aston, E. R., J. E. Merrill, D. M. McCarthy, and J. Metrik. 2016. "Risk Factors for Driving after and during Marijuana Use." *Journal of Studies on Alcohol and Drugs* 77 (2): 309–16.

Aydelotte, J. D., L. H. Brown, K. M. Luftman, A. L. Mardock, P. G. Teixeira, B. Coopwood, and C. V. Brown. 2017. "Crash Fatality Rates after Recreational Marijuana Legalization in Washington and Colorado." *American Journal of Public Health* 107 (8): 1329–31.

Berning, A., R. Compton, and K. Wochinger. 2015. "Results of the 2013–2014 National Roadside Survey of Alcohol and Drug Use by Drivers." *Journal of Drug Addiction, Education and Eradication* 11 (1): 47.

Butler, R. J., N. Kleinman, and H. H. Gardner. 2014. "I Don't like Mondays: Explaining Monday Work Injury Claims." *Industrial and Labor Relations Review* 67 (3_suppl): 762–83.

Cameron, A. C., J. B. Gelbach, and D. L. Miller. 2008. "Bootstrap-based Improvements for Inference with Clustered Errors." *The Review of Economics and Statistics* 90 (3): 414–27.

Cameron, A. C., and D. L. Miller. 2015. "A Practitioner's Guide to Cluster-Robust Inference." *Journal of Human Resources* 50 (2): 317–72.

Campolieti, M., and D. E. Hyatt. 2006. "Further Evidence on the 'Monday Effect' in Workers' Compensation." *Industrial and Labor Relations Review* 59 (3): 438–50.

Card, D., and B. P. McCall. 1996. "Is Workers' Compensation Covering Uninsured Medical Costs? Evidence from the 'Monday Effect'." *Industrial and Labor Relations Review* 49 (4): 690–706.

Cawley, J., and C. J. Ruhm. 2011. "The Economics of Risky Health Behaviors." In *Handbook of Health Economics*, Vol. 2, pp. 95–199. Elsevier.

Dobkin, C., and N. Nicosia. 2009. "The War on Drugs: Methamphetamine, Public Health, and Crime." *The American Economic Review* 99 (1): 324–49.

Dobkin, C., N. Nicosia, and M. Weinberg. 2014. "Are Supply-Side Drug Control Efforts Effective? Evaluating Otc Regulations Targeting Methamphetamine Precursors." *Journal of Public Economics* 120: 48–61.

Dong, X. 2020. "The Economic Impact of Government Regulations on Marijuana and Immigration." PhD Dissertation. Syracuse University.

Ghimire, K. M., and J. C. Maclean. 2020. "Medical Marijuana and Workers' Compensation Claiming." *Health Economics* 29 (4): 419–34.

Grossman, M. 1972. "On the Concept of Health Capital and the Demand for Health." *Journal of Political Economy* 80 (2): 223–55.

Hall, W. 2015. "What Has Research over the Past Two Decades Revealed about the Adverse Health Effects of Recreational Cannabis Use?" *Addiction* 110 (1): 19–35.

Hall, W., and L. Degenhardt. 2009. "Adverse Health Effects of Non-Medical Cannabis Use." *The Lancet* 374 (9698): 1383–91.

Hansen, B. 2016. "California's 2004 Workers' Compensation Reform: Costs, Claims, and Contingent Workers." *Industrial and Labor Relations Review* 69 (1): 173–98.

Hansen, B., K. Miller, and C. Weber. 2020. "Federalism, Partial Prohibition, and Cross-Border Sales: Evidence from Recreational Marijuana." *Journal of Public Economics* 187: 104159.

Hao, Z., and B. W. Cowan. 2020. "The Cross-Border Spillover Effects of Recreational Marijuana Legalization." *Economic Inquiry* 58 (2): 642–66.

Kaestner, R., and M. Grossman. 1998. "The Effect of Drug Use on Workplace Accidents." *Labour Economics* 5 (3): 267–94.

McCaffrey, D. F., R. Liccardo Pacula, B. Han, and P. Ellickson. 2010. "Marijuana Use and High School Dropout: The Influence of Unobservables." *Health Economics* 19 (11): 1281–99.

Miron, J. A., and J. Zwiebel. 1991. "Alcohol Consumption during Prohibition." Technical Report. National Bureau of Economic Research.

Nicholas, L. H., and J. C. Maclean. 2019. "The Effect of Medical Marijuana Laws on the Health and Labor Supply of Older Adults: Evidence from the Health and Retirement Study." *Journal of Policy Analysis and Management* 38 (2): 455–80.

Ohsfeldt, R. L., and M. A. Morrisey. 1997. "Beer Taxes, Workers' Compensation, and Industrial Injury." *The Review of Economics and Statistics* 79 (1): 155–60.

Pacula, R. L., J. F. Chriqui, and J. King. 2003. "Marijuana Decriminalization: What Does it Mean in the United States?" Technical Report. National Bureau of Economic Research.

Ruser, J. W. 1998. "Does Workers' Compensation Encourage Hard to Diagnose Injuries?" *Journal of Risk & Insurance* 65: 101–24.

Smith, R. S. 1990. "Mostly on Monday: Is Workers' Compensation Covering Off-The-Job Injuries?" In *Benefits, Costs, and Cycles in Workers' Compensation*, 115–27. Dordrecht: Springer.

Smith, B. D. 1992. "Benefits, Costs, and Cycles in Workers' Compensation." *Journal of Risk & Insurance* 59 (1): 162–5.

Vernon, H. M. 1921. *Industrial Fatigue and Efficiency*. London: G. Routledge.

Volkow, N. D., R. D. Baler, W. M. Compton, and S. R. Weiss. 2014. "Adverse Health Effects of Marijuana Use." *New England Journal of Medicine* 370 (23): 2219–27.