Recreational Marijuana Dispensaries and Fatal Car Crashes*

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Abstract

Car crashes are a leading cause of death among younger Americans and have become a central concern in the US marijuana policy debate. I construct a novel dataset of marijuana dispensary openings, which I use to present new evidence on the effect of marijuana on traffic fatalities. My intra-state differences-in-differences approach both increases power relative to past analyses and eliminates the potential of time-varying state-level confounding. I find that marijuana dispensary openings increase the rate of fatal car crashes by approximately 5.7%. I use a series of tests to discern between two plausible mechanisms – increased traffic and increased impairment – and ultimately find that the effect is primarily driven by impairment.

Keywords: health policy, drug policy, marijuana policy, automobile crashes, traffic safety

JEL Codes: I18, I12, I10

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1 Introduction

Since 2012, 18 US states, covering over a third of the US population, have adopted Recreational Marijuana Laws (RMLs). This dramatic policy shift has yielded a contentious policy debate over whether RMLs will improve or worsen public health. A central piece of this debate is the effect that marijuana legalization may have on automobile crashes (Mothers Against Drunk Driving, 2020). Automobile crashes are the second leading cause of death for Americans ages 1 to 54 and have been a driving force behind past drug control policies, such as the minimum legal drinking age.

The effect of recreational marijuana dispensaries on automobile crashes is theoretically ambiguous. On one hand, experimental evidence confirms that marijuana use substantially impairs driving ability (Hartman and Huestis, 2013) and so increased supply could increase traffic fatalities. Further, marijuana could be a complement for alcohol or other drugs, and increased use of these drugs could reduce traffic safety. On the other hand, individuals could substitute to marijuana from alcohol and other drugs. Marijuana appears to have a less clearly detrimental effect on driving ability than alcohol and other drugs (Sewell et al., 2009), which are major drivers of automobile crashes (NHTSA, 2021), and so in that case, increased marijuana access could decrease automobile fatalities. Therefore, the relationship between recreational marijuana utilization and driving fatalities is an empirical question.

In this study, I use a highly granular novel dataset in a differences-in-differences (DD) analysis over the timing of marijuana dispensaries openings within five RML states (CA, CO, MA, OR, and WA) to estimate the effect of recreational marijuana on fatal car crashes. My approach, which controls for both state-by-month and zip code fixed effects as well as a rich array of demographic and business covariates, dramatically improves power relative to past analyses and overcomes concerns of state-level confounding. Ultimately, I find that opening a recreational marijuana dispensary in a zip code increases the rate of fatal car crashes by 5.7%. The magnitude of this effect is in line with other major traffic safety interventions, such as text messaging bans, mandatory seat belt laws, and minimum legal drinking age laws.

I weigh two potential mechanisms for how recreational marijuana dispensaries increase car crashes – increased impairment and increased traffic – and find evidence in favor of the former over the latter. First, I conduct a placebo analysis using retail pharmacies and find that other retail business openings do not meaningfully increase fatal car crashes. Next, I find that the effect is concentrated at nighttime, after most dispensaries and other retail establishments have closed. I find that the effect is significant for initial dispensary openings but not subsequent dispensary openings, suggesting that marijuana access drives the increase in crashes. Finally, I estimate that crashes involving alcohol, a possible complement to marijuana, account for around 23% of the effect.

These results lead to questions about the trade-off between marijuana sales and fatal car crashes. Using dispensary-level sales data from Washington State, I estimate that there is one additional fatal car crash for every $23 million of recreational marijuana sales. Due to data limitations, this finding is more tentative and less precise than my main results. Nonetheless, this estimate provides some indication of the magnitude of the relevant trade-off.

Opening a recreational marijuana dispensary can have a myriad of positive and negative consequences, and this paper provides evidence of one important negative consequence. Precise, causal estimates of effect sizes are useful in the complex policy-making process, and policy-makers can use these estimates to more accurately weigh the benefits and disadvantages of different marijuana policies. Further, an exploration of potential mechanisms may indicate which interventions could be more or less successful in addressing the increase in fatal car crashes.

This research contributes to the long-standing and growing economics literature on the causes of automobile crashes. Car crashes are a major public health concern, both because they have large mortality and morbidity effects – causing over 39,000 deaths and 4.5 million medically consulted injuries in the US in 2019 alone (NSC, 2019) – and because car crashes often affect young, healthy individuals. The total social costs of US motor vehicle crashes in 2000 alone was estimated to exceed $230 billion (Blincoe et al., 2002). Economists have used policy-based natural experiments to estimate the causal effect of several sources of distractions or impairments on automobile crashes, including alcohol use (Carpenter and Dobkin, 2011; Huh and Reif, 2021), cell phone use (Kolko, 2009; Abouk and Adams, 2013), and sleep deprivation (Sood and Ghosh, 2007; Smith, 2016). This study reveals recreational marijuana to be another important factor contributing to automobile crashes.

This paper also contributes to the literature on the public health effects of marijuana legislation. Particularly germane to this paper, a handful of past studies in the public health and economics literatures have examined the effect of recreational marijuana laws on automobile crashes. However, past results have lacked power, largely due to the use of state-year level data. For example, Hansen et al. (2020) found that automobile fatalities increased by 3% in Colorado and 8.4% in Washington State following their RMLs, but neither of these effects were statistically significant at conventional confidence levels. The lack of analytical power in these studies is not surprising given how recently RMLs have come about. The earliest RML states opened dispensaries in 2014, and as of 2019, only seven states have operational recreational dispensaries. Consequently, there is simply a very limited number of post-RML state-years – likely too few to detect meaningful effects. Further, the recreational marijuana market started out small and is still growing in RML states, making state-wide effects even more difficult to detect. Using data disaggregated to the zip code-by-month level and focusing on the effect of dispensary openings on their local areas, my analysis dramatically improves power relative to past analyses.

The paper proceeds as follows. In section 2, I provide institutional details on marijuana laws in the United States and past research on US marijuana policy. In section 3 and section 4, I describe my data sources and empirical approach. My results, as well as a series of specification checks, are presented in section 5. I explore mechanisms in section 6 and quantify the relationship between marijuana sales and car crashes in section 7. I conclude in section 8.

2 Background

2.1 Recreational Marijuana Laws and Dispensaries

In 1979, marijuana was classified as a Schedule I drug under the US Controlled Substances Act, making all marijuana sales, possession, and use federally illegal in all US states and territories. However, since then, individual states have adopted policies (both through legislation and ballot initiative) allowing for the production and use of marijuana for specific purposes. Consequently, marijuana policy today varies markedly by state.

Starting in 1996, states began adopting medical marijuana laws (MMLs), which legalized certain proscribed supply chains to produce marijuana and allowed for eligible patients to use marijuana for medical purposes (with physician permission). These laws led to a small industry of "medical marijuana dispensaries", where patients with certain health conditions and a recommendation from a medical professional could purchase marijuana. Importantly, the specific provisions and strictness of medical marijuana laws varied substantially across states (Pacula et al., 2015; Kim et al., 2021). In some states, lenient regulations and enforcement made it easy for individuals to purchase medical marijuana and potentially for some to use medical marijuana for recreational purposes. Other states adopted stricter regulatory schemes, for example by limiting the patients able to qualify for medical marijuana or by requiring patients to cultivate their own marijuana rather than purchase it from a dispensary.

In 2012, Colorado and Washington became the first two states to adopt recreational marijuana laws (RMLs; also known as retail marijuana laws or adult-use marijuana laws), which allowed adults over the age of 21 to purchase and consume marijuana regardless of their medical status. The nation's first "recreational marijuana dispensaries" opened their doors in 2014. All adults with identification can freely purchase marijuana (typically with some modest quantity restrictions) at these dispensaries. As of November 2021, 36 US states have adopted medical marijuana laws and 18 have additionally adopted recreational marijuana laws.

Unsurprisingly, researchers have been eager to use the staggered policy adoption of both MMLs and RMLs across US states in order to study the effects of marijuana policy. To date, at least one hundred published studies have examined the relationship between state-level marijuana policy and youth drug use, alcohol consumption, tobacco use, obesity, opioid overdose, and healthcare expenditures, among other outcomes. A review of this literature is well beyond the scope of this paper (see Anderson and Rees (2021) for a recent review), but it is worthwhile to highlight a recurring methodological theme and criticism. The majority of these papers focus on state-level policies (either RML adoption or the date that dispensaries first opened) and state-level outcomes, typically the easiest data for researchers to capture and analyze.

However, state-level approaches have several noteworthy limitations. State-level approaches typically have low power to evaluate even economically significant results. Marijuana policies – particularly RMLs – are a new policy innovation, and with only a small number of states adopting and implementing RMLs, state-level analyses lack a large sample size. Further, this level of aggregation can yield biased results as it is difficult to identify the correct set of potential confounders or control for concurrent state-level policies. Consequently, results from state-level analyses of marijuana policy are unreliable – or even unreasonable. Analyses that exploit within-state variation, allowing for better control of unobserved, time-varying state-level policies and trends, reduce the potential biases of state-level analysis but are still rare within the literature on RMLs.

2.2 Marijuana Laws and Car Crashes

In line with the rest of the RML literature, most of the existing work relating marijuana to car crashes have studied state-level correlations and have consequently focused on state-level policies rather than local-level dispensary openings.[1] These studies can typically be

[1]This discussion largely ignores the literature on recreational marijuana policies in other countries such as Canada, Georgia, Malta, Mexico, South Africa, and Uruguay. This is because US-based studies are more common in the literature and more relevant to the current study and because, other than Uruguay, these

separated into two categories: studies of state-level medical marijuana laws and studies of state-level recreational marijuana laws.

The existing literature has generally found that medical marijuana laws were negatively associated with automobile crashes. Using Fatality Analysis Reporting System (FARS) data from 1990-2010, Anderson et al. (2013) found that state-level medical marijuana laws were associated with an 8-11 percent decrease in traffic fatalities, concentrated in alcohol-related crashes. Santaella-Tenorio et al. (2017) updated Anderson et al.'s work using FARS data from 2000 to 2014, finding that medical marijuana laws reduced traffic fatalities by approximately 10.8%. Cook et al. (2020) affirmed this finding among US cities with over 100,000 residents.[2] While state-level analyses of fatal crashes have been relatively consistent, some surveys present conflicting evidence. For example, Fink et al. (2020) found a near doubling in self-reported driving under the influence of marijuana in medical marijuana states, with no significant effect on self-reported driving under the influence of alcohol.

State-level evidence on the effect of recreational marijuana laws on automobile crashes have been much more mixed and tend to be imprecise. For example, Aydelotte et al. (2017) at first found no evidence of a significant increase in fatal automobile crashes after Colorado and Washington legalized recreational marijuana but later found a large (1.8 crashes/billion vehicle miles traveled) increase in fatal crashes in Colorado and Washington after the first opening of recreational dispensaries (Aydelotte et al., 2019). Lane and Hall (2019) found that recreational cannabis sales in Colorado, Oregon, and Washington were associated with an immediate increase and then a trend decrease in traffic fatalities. Santaella-Tenorio et al. (2020) used a synthetic controls approach with FARS data from 2005 to 2017 and found that recreational marijuana laws were associated with an increase of 1.46 traffic deaths per billion vehicle miles traveled per year in Colorado (but no effect in Washington). On the other hand, Hansen et al. (2020) used similar data (FARS 2000-2016) and methods (a synthetic

other countries have only recently legalized recreational marijuana.

[2]The authors also found that city-level marijuana decriminalization laws were associated with an increase in fatal automobile crashes, concentrated in 15-24 year old male drivers.

controls approach) to Santaella-Tenorio et al. (2020) but found that recreational marijuana laws in Colorado and Washington were not associated with an increase in marijuana-related, alcohol-related, or overall traffic fatality rates.

3 Data

3.1 Dispensary Data

In order to capture where and when recreational marijuana dispensaries opened and closed, I construct a novel dataset of recreational marijuana dispensary licenses in five US states (CA, CO, OR, MA, WA) that opened dispensaries before 2020 using public records and Freedom of Information Act requests.[3] All states require licensing for recreational marijuana dispensaries,[4] so this dataset represents the near universe of recreational dispensaries in those states. I then use dispensary zip codes to geolocate the dispensary and licensure issuance and expiration dates to proxy for when dispensaries opened and (if applicable) closed.[5] Using these dates, I compiled a month-by-zip-code panel dataset for every month from January 2005 to December 2019 and every zip code in each of the five states, tagging months when each zip code had a dispensary license active in any part of the month. The dataset is visualized in Figure A.1.

It is important to differentiate between the two major forms of marijuana policy being considered and adopted by states: medical marijuana laws (MMLs) and recreational marijuana laws (RMLs). Medical marijuana laws typically allow for individuals to use marijuana

[3] Two additional states also opened dispensaries before 2020 (AK and NV). However, Alaska does not maintain historical licensure records and Nevada did not respond to my request with complete, historical records of marijuana licenses.

[4] Oregon allowed some recreational sales by medical dispensaries without a recreational license for a short "early sales" period in 2015-2016. Results are robust to excluding observations from Oregon during this period.

[5] License issuance and expiration does not necessarily correspond perfectly to the opening and closing of dispensaries. It is possible that some licenses go unused (at least for some time), that some licenses are assigned to a zip code different from the actual dispensary, or that some dispensaries operate without a license. From several spot checks, these misclassifications appear uncommon. Further, measurement error would bias my result towards the null, making my estimates conservative.

for a set of predetermined medical conditions. Patients must receive a recommendation (analogous to a prescription) from a qualifying medical professional (e.g., a doctor) that they have one of the approved medical conditions and that marijuana may help. These patients can then purchase marijuana from medical marijuana dispensaries by presenting this recommendation. Recreational marijuana laws, on the other hand, allow for all individuals over a certain age (so far, it has always been 21) to purchase and use marijuana, regardless of their medical status.

In this study, I focus on recreational marijuana rather than medical marijuana for two reasons. First, only about 2.5% of people in MML states report any use of marijuana recommended by a health care professional, and about 1.7% of people in non-MML states report using marijuana recommended by a health care professional (Caputi, 2019). Therefore, medical marijuana dispensaries are unlikely to be a substantial enough policy intervention to bring about an observable effect in automobile crashes or most other health outcomes. In contrast, while federal drug surveys do not capture the share of respondents who have purchased marijuana from recreational dispensaries, it is clear from sales figures that recreational marijuana dispensaries are serving a substantial portion of the population. For example, Colorado reported $1.4 billion in recreational marijuana sales in 2019, which is over $240 per capita. Second, state licensure of medical marijuana dispensaries is very inconsistent. In the earlier years of this study, some states did not require medical marijuana dispensaries to register with the state at all. Even today, some states (notably California) do not maintain a central registry of medical marijuana dispensaries. Therefore, it is not possible to construct a comprehensive dataset of medical marijuana dispensaries for the entire study period using administrative licensure data.

3.2 Fatality Data

My fatality data comes from the Fatality Analysis Reporting System (FARS), which is collected by the National Highway Traffic Safety Administration. FARS is a publicly available national registry of all fatal car crashes in the United States occurring since 1975 and has formed the basis for virtually all existing US traffic fatality research. FARS reports the date, time, and state of each crash, and since 2001, it also reports the latitude and longitude for virtually all automobile fatalities. I use FARS data from 2005 to 2019, the most current available data. I use the latitude and longitude to geolocate each automobile crash to 2019 zip codes.[6] I sum the number of fatal automobile crashes for each month for each zip code.

3.3 Demographic and Business Data

I control for zip-code level demographics and business activity using annual data retrieved from the Census Bureau. I retrieve demographic data from the 2000 and 2010 Decennial Census and the 2011-2019 5-Year American Community Survey. I use business activity data (e.g., number of employees and number of business establishments) from the 2005-2019 Zip Codes Business Patterns dataset to control for business activity. I control for unemployment at the county level using Bureau of Labor Statistics data. I use linear interpolation to complete the panel of controls between 2005 and 2009, as well as any missing individual years.

4 Empirical Strategy

I use a DD framework to estimate the effect of recreational marijuana dispensaries on local traffic fatalities, exploiting variation in the introduction of recreational marijuana dispensaries to different zip codes. I use a Poisson model because my outcome (number of fatal car crashes) is a non-negative, count variable (for robustness, I also present OLS estimates using number of fatal car crashes as the dependent variable). My main specification is summarized in the following Poisson regression equation:

[6]Zip codes occasionally change over time, so I geolocate points to their 2019 zip code. I am able to geolocate 99% of fatal crashes in the five US states studied here to their zip code.

$$log(E[Y_{zst}]) = \gamma 1(\text{Dispensary Open})_{zt} + X_{zt}\beta + \alpha_z + \alpha_{st} + \epsilon_{zst} \quad (1)$$

In this model, Y_{zst} refers to the count of fatal car crashes in zip code z in state s in month t; X_{zt} is a matrix of time-varying zip-level covariates; α_z and α_{st} are zip code and state-by-month fixed effects; and ϵ_{zst} is a stochastic error term. The $1(\text{Dispensary Open})_{zt}$ term is an indicator variable that takes the value 1 when there is at least one recreational marijuana dispensary open in zip code z in month t and 0 otherwise. The parameter of interest is γ. To the extent that the treated zip codes would have followed a parallel trajectory of the rate of fatal car crashes compared to untreated units (conditional on the fixed effects), γ identifies the average treatment effect on the treated (ATT) of having at least one recreational marijuana dispensary open within the given zip code. Note that this model is estimated using a Poisson regression, and an estimate for γ corresponds to the effect of opening a recreational marijuana dispensary on the rate of fatal car crashes within the state rather than a raw number. I control for population per square mile, the natural logarithm of the population, the number of employees, the number of business establishments, the natural logarithm of the median household income, the median age, the share of the population that is male, the average household size, and the share of the population that is between 21 and 39 years of age at the zip code level, as well as the unemployment rate for the county the zip code is situated in. Standard errors for all regressions were clustered at the zip-code level (Bertrand et al., 2004).

The aforementioned differences-in-differences approach relies upon the parallel trends assumption that, were the treated units to not open a recreational marijuana dispensary, they would have continued along a parallel trajectory to the untreated units. In this setting, one may worry about the plausibility of parallel trends. For example, it is possible that, even after adjusting for zip code, time, and state-by-month fixed effects and demographic and economic covariates, areas that open dispensaries were following a different traffic safety trajectory than areas that did not open dispensaries. I validate this hypothesis by evaluating whether the trajectories were parallel in periods before the dispensary opened. To conduct this evaluation, I report estimates from an event study model:

$$log(E[Y_{zst}]) = \sum_{k=-10;k\neq-1}^{5} \gamma_k D_k + X_{zt}\beta + \alpha_z + \alpha_{st} + \epsilon_{zst} \quad (2)$$

In this model, D_k is a series of lags and leads for halfyears (i.e., six month periods) before and after the recreational dispensary first opens in a given zip code.[7] The γ_k coefficients are normalized such that the base period corresponds to months 1-6 before the zip code opens its first recreational marijuana dispensary. In this model, periods before the introduction of the dispensary are the pre-trends, and the parameters of interest are those γ_k for $k < 0$. If the estimates for these parameters are close to 0, then this suggests that, conditional on the fixed effects and other controls in the model, treated zip codes were on a parallel trajectory to untreated zip codes before the dispensary opened (Perez-Truglia, 2018).

Another assumption of my preferred specification is that there are no spillovers across zip codes, e.g., a zip code is not affected by a dispensary in a neighboring zip code. This assumption is unlikely to hold exactly, as some zip codes are small and have meaningful through-traffic. However, the average zip code in my dataset has over 90 square miles of land area and are large enough to contain meaningful markets. Further, as long as neighboring zip codes are affected in the same direction as treated zip codes, spillovers would only attenutate my results, making my estimates overly conservative.

Nevertheless, to better understand spillovers, I complement my main specification from Equation 1 with a model using distance to a recreational marijuana dispensary. Specifically, I analyze whether having a recreational marijuana dispensary closer to a given zip code (even if not in that zip code) leads to an increase in fatal car crashes. This specification is summarized in the following Poisson regression equation:

[7] To improve power, the lags and leads are binned at the endpoints. γ_{-10} corresponds to all halfyears at least 5 years before a dispensary opens and γ_5 corresponds to all halfyears at least 2.5 years after a dispensary opens.

$$log(E[Y_{zst}]) = \gamma \text{Distance to Dispensary}_{zt} + X_{zt}\beta + \alpha_z + \alpha_{st} + \epsilon_{zst} \quad (3)$$

In this specification, Distance to Dispensary$_{zt}$ refers to the distance between zip code z and the nearest zip code with a recreational marijuana dispensary (scaled to 10 miles), and γ, the parameter of interest, identifies the effect of having a recreational marijuana dispensary 10 miles further from a zip code's centroid. Zip-to-zip distances are defined by the distance between the centroids of each zip code. This approach accounts for spillovers across neighboring zip codes; if a recreational dispensary opens in a neighboring zip code, that would affect Distance to Dispensary$_{zt}$ in Equation 3 but not 1(Dispensary Open)$_{zt}$ in Equation 1. However, there are several disadvantages to this approach. First, I do not have access to all marijuana dispensaries, and so I can only use distance to dispensaries identified in this study's five states. For example, if the nearest dispensary to a California zip code is located in Nevada, I would not correctly capture that zip code's Distance to Dispensary$_{zt}$. Second, before 2014, there were no recreational marijuana dispensaries in the United States, and so distance to a dispensary is infinite for all zip codes in all months before 2014. To address this issue, I cap distance at a maximum of 50 miles, such that Distance to Dispensary$_{zt}$ takes values in $[0, 5]$. A zip code with a dispensary has a Distance to Dispensary$_{zt}$ equal to 0, and any zip code without a dispensary within 50 miles has a Distance to Dispensary$_{zt}$ equal to 5.

4.1 Differences-in-Differences Weights

Recently, many econometricians have raised concerns that two-way fixed effects DD models may generate misleading estimates if treatment effects are heterogeneous (Roth et al., 2022). Under the common trends assumption, the DD estimator identifies the weighted sum of the treatment effect in each group (i.e., zip code) and at each time period (i.e., month), computed by comparing the outcome between consecutive time periods across pairs of groups. For comparisons between a group that switches from untreated to treated and a group that is treated in both periods, the assigned weights can be negative. Negative weights can be problematic when the treatment effect is heterogeneous because the DD estimator need not be in the convex set of the group-level effects; indeed, in some cases, negative weights can make the DD estimator the opposite sign of all the group-level treatment effects. To address this concern, I follow the recommendation by De Chaisemartin and d'Haultfoeuille (2020) to estimate the weights attached to each pairwise comparison in my regression and evaluate whether negative weights are likely to affect my results. The De Chaisemartin and d'Haultfoeuille (2020) method is currently only available for linear models, so I estimate these weights for an OLS version of my preferred specification using the count of fatal car crashes as the dependent variable.

5 Results

5.1 Main Results

Table 1 shows summary statistics for both treated and untreated units at the zip-code-month level. I consider 3375 standard, 2019 zip codes in the five RML states. Of these, no zip code had a recreational dispensary at the beginning of my study period, 813 opened a recreational dispensary during my study period, and 2562 never had a recreational dispensary during my study period. Never-treated zip codes were less populated and had fewer employees than treated zip codes, both before and after the first dispensary opens. Importantly, treated zip codes tended to be relatively similar on many covariates before and after the dispensary opens. The main exceptions is county unemployment; treated zip codes have much lower (county-level) unemployment rates after the dispensary opens than before (4.3% vs 7.4%). This is likely a consequence of the sample period and the time that recreational dispensaries opened. I examine zip codes from 2005 to 2019, and the first recreational dispensaries opened in January 2014. Therefore, observations in the "before" period necessarily include the 2008 global financial crisis, and observations in the "after" period occurred during the US economic boom from 2014 to 2019. Time fixed effects in my preferred specification (Equation 1) address this issue.

Table 1: Summary Statistics

	After Dispensary		Before Dispensary		Never Dispensary	
	Mean	SD	Mean	SD	Mean	SD
Fatal Crashes	0.15	0.41	0.14	0.40	0.11	0.36
Dispensary Open	1.00	0.00	0.00	0.00	0.00	0.00
Population	24 872.39	17 672.21	25 032.20	18 178.98	15 399.72	18 330.86
Population Per Sq. Mi.	3368.50	5591.03	3720.96	6051.24	2204.03	4458.56
Land Area in Sq. Mi.	104.34	187.68	90.96	171.09	94.52	197.40
Share Male	0.50	0.03	0.50	0.03	0.50	0.05
Share Aged 21-39	0.27	0.09	0.27	0.08	0.22	0.09
Median Age	39.53	7.23	38.44	6.99	41.55	8.72
Median Household Income (2010 Dollars)	55 235.37	18 728.21	52 477.17	17 410.73	60 844.40	26 749.34
Avg. Household Size	2.52	0.45	2.57	0.51	2.66	0.56
No. Employees	10 492.11	12 316.05	9689.20	11 517.63	5365.78	9451.63
No. Establishments	733.12	595.23	684.22	578.07	362.08	491.89
County Unemployment	4.30	1.44	7.37	2.96	7.06	3.36
Year	2017.54	1.42	2010.69	3.69	2012.00	4.32

This table provides the mean and standard deviation for several covariates in the analytical sample. The data is a month-by-zip-code panel (N=606504) for all standard, 2019 zip codes from CA, CO, MA, OR, and WA in each month between January 2005 and December 2019 with complete information. There are 28150 after dispensary month-zips, 119090 before dispensary month-zips, and 459264 never dispensary month-zips. Over 99% of the zip codes (3357 zip codes of 3375) have complete information for the entire panel. The first four columns refer to zip codes that had dispensary open during the study period. Columns under "After Dispensary" correspond to observations after the zip had a dispensary open. Columns under "Before Dispensary" correspond to observations before the zip had a dispensary open. The last two columns refer to the zip codes that never had a dispensary open during the study period.

Table 2 shows the results of a Poisson estimate of Equation 1. The dependent variable is a count of fatal car crashes, and the independent variable of interest is whether a recreational dispensary was open in a given zip code-month. In the first column, I control only for zip and month fixed effects. This specification accounts for all zip-code-invariant and time-invariant confounders, as in a typical two-way fixed effects model. In my baseline model (Column 2), I additionally control for state-by-month fixed effects, which flexibly account for any confounders that vary at the state-level, which may include state-level drug control policies, highway safety measures, or criminal justice reforms.[8] This is particularly important as state policies may play a major role in determining traffic safety. In this baseline model, introducing a marijuana dispensary is associated with a statistically significant 5.5% increase in automobile fatalities.

In my preferred specification, I additionally adjust for a rich set of demographic and business controls. These changes do not meaningfully affect my effect estimate. Adding controls for demographics (Column 3) increases the estimate from 5.5% to 5.6%, and additionally controlling for business covariates (Column 4) increases the estimate to 5.7%. That controlling for observed confounders does not substantially change the effect estimate provides some confidence that the estimates are not affected by omitted variables bias.

Ultimately, after controlling for zip code and state-by-month fixed effects and adjusting for the full set of zip-by-month varying covariates, introducing a marijuana dispensary is associated with a 5.7% increase in automobile fatalities (Column 4). This relatively large and statistically significant effect indicates that recreational marijuana dispensaries substantially increase the rate of fatal car crashes.

A single car crash can cause multiple fatalities, and some past studies have investigated the number of car crash deaths rather than the number of fatal car crashes. My result is robust to this choice. Replacing the dependent variable with the number of car crash deaths

[8] Recreational marijuana laws could affect marijuana-related stigma at the state-level, but because I include state-by-month fixed effects, that state-level effect would be differenced out of my estimate. However, as the stigma effect would almost certainly be in the same direction as the dispensary effect, this would only bias my effect estimates towards 0, making my estimates overly conservative.

Table 2: Effect of Recreational Marijuana Dispensaries on Fatal Car Crashes

Dependent Variables:			Fatal Crashes		Deaths
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Dispensary Open	0.0800***	0.0551**	0.0562**	0.0572**	0.0526**
	(0.0203)	(0.0228)	(0.0228)	(0.0228)	(0.0239)
log(1+Population)			0.0559	0.0632	0.0570
			(0.0426)	(0.0424)	(0.0451)
Share Male			0.0371	0.0427	-0.0279
			(0.1866)	(0.1870)	(0.1991)
Share Aged 21-39			-0.3365*	-0.3589*	-0.3931*
			(0.1946)	(0.1945)	(0.2164)
Population Per Sq. Mi.			5×10^{-6}	6.72×10^{-6}	6.27×10^{-6}
			(1.26×10^{-5})	(1.3×10^{-5})	(1.25×10^{-5})
Median Age			-0.0013	-0.0013	-0.0010
			(0.0024)	(0.0024)	(0.0025)
Avg. Household Size			-0.0179	-0.0185	-0.0072
			(0.0337)	(0.0337)	(0.0367)
log(1+Median Household Income)			-0.0882*	-0.0865*	-0.1163**
			(0.0464)	(0.0464)	(0.0485)
County Unemployment			-0.0484***	-0.0484***	-0.0519***
			(0.0064)	(0.0064)	(0.0068)
No. Employees				1.12×10^{-6}	3.87×10^{-7}
				(2.37×10^{-6})	(2.34×10^{-6})
No. Establishments				-0.0001	-9.66×10^{-5}
				(9.36×10^{-5})	(0.0001)
Fixed-effects					
Zip Code	Yes	Yes	Yes	Yes	Yes
Month (180)	Yes	No	No	No	No
Month-State (900)	No	Yes	Yes	Yes	Yes
Fit statistics					
# Zip Code	3,476	3,476	3,375	3,375	3,375
Observations	625,680	625,680	606,504	606,504	606,504
Squared Correlation	0.12801	0.13040	0.12842	0.12844	0.11834
Pseudo R^2	0.15154	0.15397	0.14749	0.14749	0.15014
BIC	446,757.3	455,226.8	452,096.9	452,121.6	484,028.6
Dependent variable mean	0.11557	0.11557	0.11880	0.11880	0.12958

Clustered (Zip Code) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents results from five separate Poisson difference-in-difference regressions. In each regression, the independent variable of interest is "Dispensary Open", which is an indicator that at least one recreational marijuana dispensary was open in that zip code-month. The first column is the standard two-way fixed effects model, which controls for zip-code and month fixed effects, for the effect of having a recreational marijuana dispensary open ("Dispensary Open") on the rate of fatal car accidents. The second column additionally accounts for state-by-month fixed effects, which eliminates all state-level confounding. The third column controls for the fixed effects in the second column, as well as zip-code level demographic variables. The fourth column is the preferred specification and controls for zip code and state-by-month fixed effects, as well as demographic and business covariates. The fifth column replicates the fourth but replacing the independent variable with the rate of automobile accident deaths instead of the rate of fatal car accidents.

barely affects the estimate: introducing a recreational marijuana dispensary into a zip code increases the rate of car crash deaths by 5.3%. In absolute terms, in just the treated zip codes in the five US states over the length of the study period, recreational marijuana dispensaries caused approximately 240 excess fatal car crashes.

I repeat the analysis using distance to a recreational marijuana dispensary, instead of a binary indicator, as the independent variable, as shown in Equation 3. Again, recreational marijuana dispensaries increase fatal car crashes (Table 3). After controlling for all business and demographic controls, as well as zip code and state-by-month fixed effects, being 10 miles closer to (further from) a dispensary increases (decreases) fatal car crashes by a statistically significant 2.5% (Column 4). Each new treated zip code reduces distance to a dispensary for that zip code and has the potential to reduce distance to a dispensary for untreated, nearby zip codes. In my data, the average treated zip code reduces aggregate distance to a dispensary across all zip codes by approximately 33 miles. This implies that, with spillovers to other zip codes included, each recreational dispensary entering a new zip code increases fatal car crashes by approximately 8.5%. This estimate accounts for spillovers and is therefore larger than the 5.7% estimate from Equation 1.

5.2 Robustness

The main identifying assumption of my DD analysis is that of parallel trends, i.e., that the treated group would have continued along a parallel trajectory to the controls had it not been for the treatment (in this case, the introduction of a marijuana dispensary). Because I use a Poisson regression, this assumption corresponds to a common trajectory in log growth rates rather than parallel trends in levels. The results from my event study specification (Equation 2), shown in Figure 1, confirms relatively parallel trends in fatal car crash rates before the entrance of the dispensary and supports the validity of my main results.

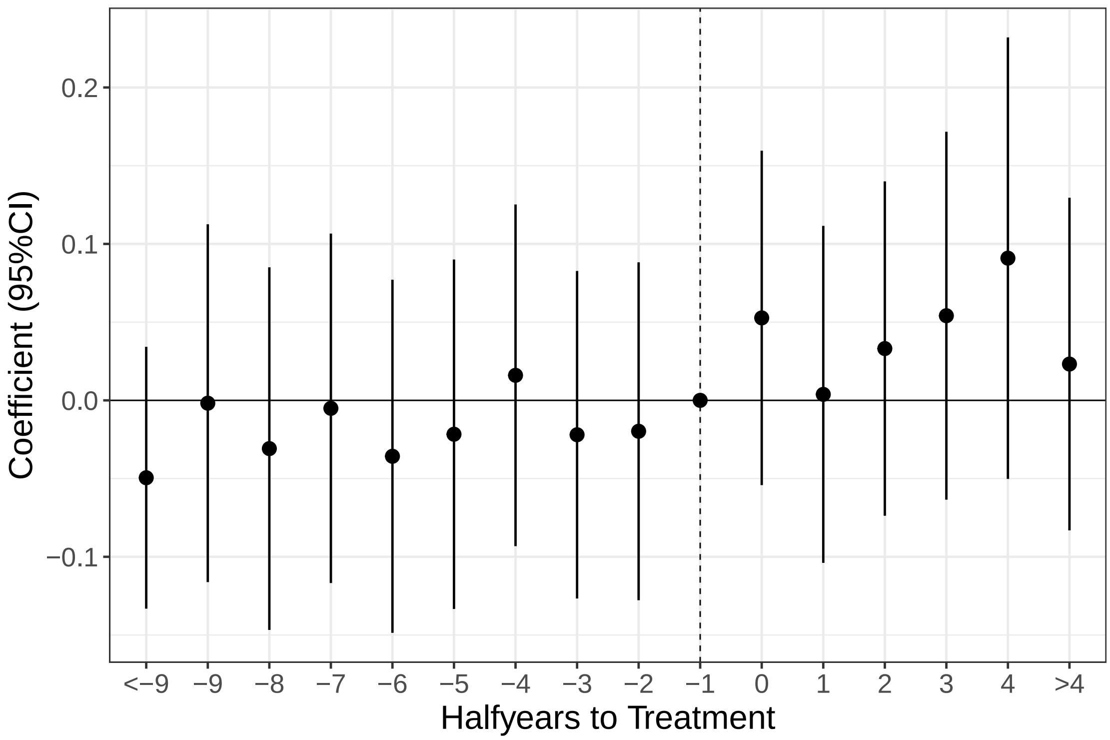
Table 3: Effect of Distance to Recreational Marijuana Dispensaries on Fatal Car Crashes

Dependent Variables:	Fatal Crashes				Deaths
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Distance to Dispensary (10mi)	-0.0172***	-0.0238***	-0.0243***	-0.0253***	-0.0226***
	(0.0043)	(0.0077)	(0.0077)	(0.0077)	(0.0082)
log(1+Population)			0.0505	0.0588	0.0529
			(0.0428)	(0.0425)	(0.0454)
Share Male			0.0347	0.0414	-0.0296
			(0.1862)	(0.1867)	(0.1990)
Share Aged 21-39			-0.3232*	-0.3490*	-0.3840*
			(0.1954)	(0.1952)	(0.2173)
Population Per Sq. Mi.			3.76×10^{-6}	5.77×10^{-6}	5.41×10^{-6}
			(1.24×10^{-5})	(1.29×10^{-5})	(1.24×10^{-5})
Median Age			-0.0014	-0.0014	-0.0011
			(0.0024)	(0.0024)	(0.0025)
Avg. Household Size			-0.0193	-0.0202	-0.0087
			(0.0333)	(0.0333)	(0.0364)
log(1+Median Household Income)			-0.0917**	-0.0900*	-0.1192**
			(0.0462)	(0.0463)	(0.0483)
County Unemployment			-0.0484***	-0.0484***	-0.0519***
			(0.0064)	(0.0064)	(0.0068)
No. Employees				1.08×10^{-6}	3.6×10^{-7}
				(2.37×10^{-6})	(2.35×10^{-6})
No. Establishments				-0.0001	-0.0001
				(9.37×10^{-5})	(0.0001)
Fixed-effects					
Zip Code	Yes	Yes	Yes	Yes	Yes
Month (180)	Yes	No	No	No	No
Month-State (900)	No	Yes	Yes	Yes	Yes
Fit statistics					
# Zip Code	3,476	3,476	3,375	3,375	3,375
Observations	625,680	625,680	606,504	606,504	606,504
Squared Correlation	0.12801	0.13041	0.12843	0.12846	0.11835
Pseudo R^2	0.15154	0.15398	0.14750	0.14750	0.15015
BIC	446,755.4	455,222.5	452,092.6	452,116.8	484,024.5
Dependent variable mean	0.11557	0.11557	0.11880	0.11880	0.12958

Clustered (Zip Code) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents repeats the exercise from Table 2 with one key difference: in each regression, the independent variable of interest is now "Distance to Dispensary", which is a continuous variable indicating the number of miles to the nearest zip code with an open recreational marijuana dispensary. The distance is capped at 50 miles and scaled by 10 miles, so the variable takes values in the range $[0, 5]$; distance is 0 when there is a dispensary in the zip code and 5 when there is no dispensary within 50 miles. Distance only refers to the recreational dispensaries in the five states studied (CA, CO, MA, OR, WA), and so it is possible there is a closer dispensary in another state. A negative coefficient on this distance metric means that having a dispensary further away is associated with a decrease in fatal car crashes.

Figure 1: Event Study Estimates



This plot shows the results of a single regression: the event study estimated using Equation 2. Each dot represents a coefficient estimate, and the error bars represent 95% confidence intervals. Each coefficient corresponds to an indicator variable set equal to the number of halfyears before or after a recreational marijuana dispensary opens in a given zip code. The coefficients are normalized so that the halfyear immediately before the dispensary opens (-1) is 0. The coefficient on < -9 corresponds to all observations at least 10 halfyears (i.e., 5 years) before the dispensary opened, while the coefficient on > 4 corresponds to all observations at least 5 halfyears (i.e., 2.5 years) after the dispensary opened. Standard errors are clustered at the zip code level.

I test whether this effect estimate is robust to heterogeneous treatment effects, using the procedure recommended by De Chaisemartin and d'Haultfoeuille (2020). First, I estimate an OLS version of my preferred specification using the count of fatal car crashes as the dependent variable and find a similar, statistically significant, positive effect of recreational marijuana dispensaries on fatal car crashes (Table A.2): having a recreational marijuana dispensary open increases the number of monthly fatal car crashes by 0.0138. Then, I find that none (0%) of the 28,150 comparisons in this specification's regression has a negative weight. This is, perhaps, unsurprising. De Chaisemartin and d'Haultfoeuille (2020) find that

DD models are more likely to assign a negative weight to periods where a large fraction of groups are treated and to groups treated for many periods. In my dataset, each month has at most 22.6% treated zip codes, and no zip code is treated for more than 40% of the length of the panel. Overall, because my regression generates no negative weights, the estimated effect is robust to heterogeneous effects and not biased by negative weights.

5.3 Magnitude

The effect size of dispensary entry is comparable (though in the opposite direction) to that of other major traffic safety interventions. For example, Carpenter and Stehr (2008) found that mandatory seat belt laws reduced deaths from fatal car crashes by 8% and reduced serious injuries from car crashes by 9%. Abouk and Adams (2013) found that weak and strong text messaging bans (temporarily) reduced fatal crashes by 4%.[9] US states that moved to a minimum legal drinking age of 21 – recognized as a highly successful traffic safety intervention – reduced youth traffic fatalities by 9-11% (Dee, 1999). Morrisey and Grabowski (2011) found that an increase of 10% in beer taxes reduces motor vehicle fatalities among drivers aged 15-24 by only 1.3%, suggesting that opening a recreational marijuana dispensary has an effect on fatal car crashes in line with a large beer tax increase.

It is useful to compare the effect size estimated here to previous estimates of the effect of RMLs on fatal car crashes computed in past studies using state-year level data. Hansen et al. (2020) used synthetic controls and found that RMLs were associated with a 3% and 8.4% increase in fatal car crashes in Colorado and Washington, respectively. However, neither of these estimates were statistically significant. Santaella-Tenorio et al. (2020) used a synthetic controls approach and found that RMLs were associated with a 13% increase in fatal car crash deaths (an increase of 1.46 deaths per billion vehicle miles traveled, p=0.046) in Colorado but no significant effect (an increase of 0.08 deaths per billion vehicle miles traveled, p=0.674) in Washington. Using a differences-in-differences approach, Aydelotte et al. (2019) found that

[9] Abouk and Adams (2013) found a 4% effect using state and month fixed effects, but this effect diminished to 0 after additionally accounting for state-specific linear trends.

RMLs were associated with an increase of 1.8 (95%CI 0.4-3.7) crashes per billion vehicle miles traveled. Given that the average in Colorado and Washington was around 9.3 crashes per billion vehicle miles traveled, this represents an approximately 19% increase in fatal car crashes, with a confidence interval from 4% to 40%. My point estimates, computed using local-level variation in recreational marijuana dispensaries, are mostly in line with (perhaps slightly more modest than) these past results. However, my estimates are much more precise; the 95% confidence interval from my preferred specification ranges from 1.3% to 10.2%. This demonstrates that my approach significantly improves the power and precision of the analysis. Further, these past studies represent effects only in Washington and Colorado, while my results represent effects in five RML states, encompassing a much larger population. In addition, my results flexibly account for all state-level confounding.

6 Mechanisms

I explore two potential mechanisms that could explain the increase in fatal car crashes following the opening of a recreational marijuana dispensary: (A) increased impairment, which could come from increased marijuana consumption or increased consumption of complement drugs, and (B) increased traffic, which could be increased traffic to the dispensary or increased traffic to a new shopping center where the dispensary opened. If the increase is primarily driven by increased impairment, then restricting marijuana dispensaries could avert fatal car crashes. If, instead, the increase is driven by increased traffic, then restricting marijuana dispensaries would only avert fatal car crashes to the extent that other businesses may not drive similar traffic.

First, I conduct a placebo analysis using the openings of retail pharmacies. Like recreational marijuana dispensaries, retail pharmacies are likely to open in new and existing commercial areas and attract retail traffic. If the relationship between marijuana dispensaries and fatal car crashes is driven by traffic to new commercial development, then we would expect a positive effect of pharmacy openings on car crashes. Retail pharmacies are licensed by the state, and licensure data is publicly available in four of the five states analyzed in this study (all except Massachusetts), allowing me to compile and analyze a pharmacy panel dataset analogous to the marijuana dispensary panel dataset.

I estimate Equation 1 using the introduction of a retail pharmacy rather than a dispensary, excluding Massachusetts zip codes. In the first column of Table 4, I show that introducing retail pharmacies is largely unrelated to fatal car crashes. The point estimate for the effect of pharmacy openings on fatal car crashes is -0.7% with a 95% confidence interval of -6.4% to 5.0%, thereby ruling out the effect size I estimate for marijuana dispensaries. This confidence interval also rules out the effect size I estimate for marijuana dispensaries in a comparable sample (i.e., excluding Massachusetts; Table A.1). My placebo estimates for car crash deaths and nighttime crashes are also statistically insignificant and much smaller than my dispensary estimates. These null findings for pharmacies support the notion that introducing a marijuana dispensary has an effect on fatal car crashes to an extent that other retail businesses do not.

Table 4: Placebo Effect of Retail Pharmacy Openings

Dependent Variables:	Fatal Crashes	Deaths	Daytime Crashes	Nighttime Crashes
Model:	(1)	(2)	(3)	(4)
Variables				
Pharmacy Open	-0.0072	0.0138	-0.0388	0.0364
	(0.0290)	(0.0295)	(0.0440)	(0.0389)
Fixed-effects				
Zip Code (2,890)	Yes	Yes	Yes	Yes
Month-State (720)	Yes	Yes	Yes	Yes
Fit statistics				
Observations	519,204	519,204	519,204	519,204
Squared Correlation	0.12953	0.11858	0.05249	0.08519
Pseudo R^2	0.14442	0.14658	0.11061	0.15193
BIC	407,615.0	437,637.2	259,797.4	275,581.4
Dependent variable mean	0.12922	0.14124	0.05854	0.06863

Clustered (Zip Code) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents four separate Poisson regressions that vary only by their dependent variable. In each model, the independent variable of interest is an indicator for whether at least retail pharmacy is open in a given zip-month ("Pharmacy Open"). Pharmacy opening is used as a placebo in place of recreational marijuana dispensary opening. Each model controls for zip code and state-by-month fixed effects, as well as business and demographic covariates. The columns present the DD estimate of opening a dispensary on the rate of (1) fatal car accidents; (2) automobile deaths; (3) fatal accidents occurring in the daytime (8AM to 7PM); (4) fatal accidents occurring at nighttime (8PM to 7AM); and (5) fatal accidents that involved alcohol. These regressions are only conducted for California, Colorado, Oregon, and Washington zip codes.

Second, I examine the differential impact on car crashes of first and subsequent dispensary openings within a zip code. If the observed effect is driven by impairment and not just commercial development, we would expect that the first marijuana dispensary in a zip code to have a larger effect on fatal car crashes than subsequent dispensary entries, as some business for the subsequent entrants will come from existing customers. I define a zip code's first dispensary entry as the month that the first dispensary opened in a zip code and subsequent entry as the first month with more dispensaries than the first month. I then estimate a regression including both indicators to identify the effect of a first dispensary openings and the net effect of a subsequent dispensary opening (Equation 4).

$$log(E[y_{zst}]) = \gamma 1(\text{Date} \geq \text{Initial Entry}) + \delta 1(\text{Date} \geq \text{Subsequent Entry}) + X_{zt}\gamma + \alpha_z + \alpha_{st} + \epsilon_{zst} \tag{4}$$

As shown in column (3) of Table 5, I confirm that the first marijuana dispensary significantly increases fatal car crashes but subsequent dispensary openings do not have a significant marginal effect on fatal car crashes. Specifically, the first dispensary opening increases fatal car crashes by a statistically significant 8.4%, but the coefficient for subsequent dispensary openings is statistically insignificant. This finding provides further evidence that the car crash effect is due to marijuana access rather than commercial development.

Next, I examine the relationship between recreational marijuana dispensaries and traffic fatalities separately for crashes that occur in the daytime (8AM to 7PM) and the nighttime (7PM to 8AM). Daytime automobile fatalities may occur because of traffic to the dispensary or a new commercial area, but nighttime fatalities -– which occur after most retail stores have closed — are less likely to reflect commercial traffic. I find my primary results are largely driven by nighttime car crashes: daytime automobile fatalities increase only by a statistically insignificant 1.3% while nighttime automobile fatalities increase by a significant 8.6% (Table 6). Figure 2 shows the effects on fatal car crashes occurring in different 6-hour

Table 5: First and Subsequent Dispensary Openings

Dependent Variable:	Fatal Crashes		
Model:	(1)	(2)	(3)
Variables			
First Dispensary	0.0680***		0.0836***
	(0.0225)		(0.0281)
Subsequent Dispensary		0.0340	-0.0296
		(0.0283)	(0.0353)
Fixed-effects			
Zip Code (3,375)	Yes	Yes	Yes
Month-State (900)	Yes	Yes	Yes
Fit statistics			
Observations	606,504	606,504	606,504
Squared Correlation	0.12845	0.12843	0.12844
Pseudo R^2	0.14750	0.14748	0.14750
BIC	452,118.8	452,126.6	452,131.4
Dependent variable mean	0.11880	0.11880	0.11880

Clustered (Zip Code) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents results from three separate Poisson difference-in-difference regressions. In each regression, the dependent variable is the count of fatal car crashes. The independent variable in column (1) is an indicator for any date after a zip code opened an initial recreational marijuana dispensary. The independent variable in column (2) is an indicator for any date after a zip code opened at least one more dispensary after the initial entry. In column (3), both an indicator for initial and subsequent recreational dispensaries are included in the regression. All three regressions include demographic and business controls, as well as zip and state-by-month fixed effects.

time blocks throughout the day. Again, the effect appears strongest at nighttime, after most dispensaries and other commercial businesses are closed. This suggests that the observed effect is driven by marijuana access rather than increased traffic, supporting the hypothesis that marijuana dispensaries increase car crashes via impairment.

Table 6: Effects of Recreational Marijuana Dispensaries on Different Traffic Outcomes

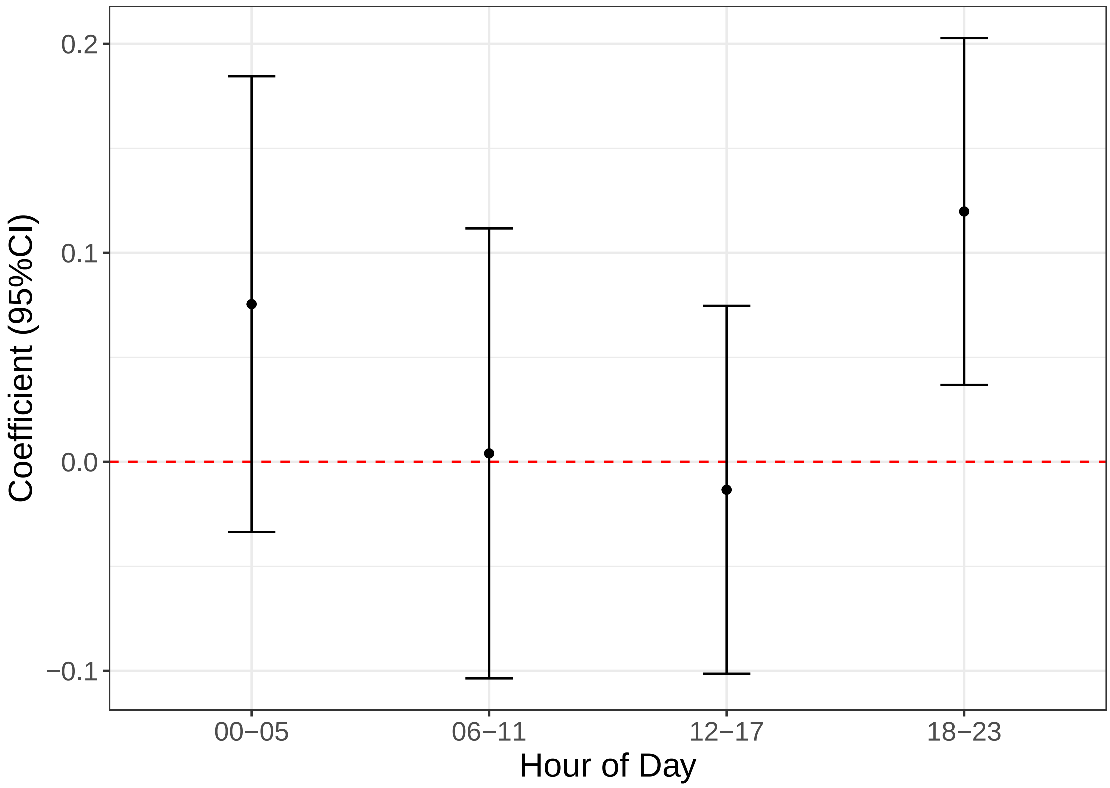
Dependent Variables:	Fatal Crashes	Deaths	Daytime Crashes	Nighttime Crashes
Model:	(1)	(2)	(3)	(4)
Variables				
Dispensary Open	0.0572**	0.0526**	0.0133	0.0857***
	(0.0228)	(0.0239)	(0.0345)	(0.0326)
Fixed-effects				
Zip Code (3,375)	Yes	Yes	Yes	Yes
Month-State (900)	Yes	Yes	Yes	Yes
Fit statistics				
Observations	606,504	606,504	606,504	606,504
Squared Correlation	0.12844	0.11834	0.05262	0.08411
Pseudo R^2	0.14749	0.15014	0.11470	0.15431
BIC	452,121.6	484,028.6	288,673.4	306,286.0
Dependent variable mean	0.11880	0.12958	0.05387	0.06308

Clustered (Zip Code) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents four separate Poisson regressions that vary only by their dependent variable. In each model, the independent variable of interest is an indicator for whether at least recreational marijuana dispensary is open in a given zip-month ("Dispensary Open"). Each model controls for zip code and state-by-month fixed effects, as well as business and demographic covariates. The columns present the DD estimate of opening a dispensary on the rate of (1) fatal car accidents; (2) automobile deaths; (3) fatal accidents occurring in the daytime (8AM to 7PM); and (4) fatal accidents occurring at nighttime (8PM to 7AM).

Figure 2: Effect on Fatal Car Crashes by Time of Day



This plot shows the results of four separate Poisson regressions. Each dot refers to the coefficient on an indicator for whether a recreational marijuana dispensary was open in the zip-month, and the error bars represent 95% confidence intervals. The dependent variable is the rate of fatal car crashes occurring between (1) 12:00AM and 5:59AM; (2) 6:00AM and 11:59AM; (3) Noon and 5:59PM; (4) 6:00PM and 11:59PM. Each Poisson regression controls for zip code and state-by-month fixed effects, as well as demographic and business covariates. Standard errors are clustered at the zip code level.

Finally, I investigate the effect of recreational marijuana dispensaries on alcohol-involved fatal car crashes. Increased traffic to the dispensary or a new shopping area may affect fatal car crashes but should not affect drunk driving. On the other hand, an increase in drunk driving would indicate that impairment is driving the effect.

There are several data limitations to consider. Whether alcohol was the cause of a crash is not directly reported. Instead, the FARS includes data on whether alcohol was "involved" for each participant in the crash. This reflects the judgement of law enforcement, which could be biased, particularly if officers' opinions change around the time of a dispensary opening. Even then, alcohol involvement data is missing for many participants, and for some car crashes, alcohol involvement data is missing for all drivers. In light of these limitations, I exclude Massachusetts zip codes from my analyses because less than 25% of fatal car crashes have data for any driver in the crash. The other states have alcohol-involvement data for at least one driver in at least 50% of fatal car crashes, though all still have some car crashes with missing data. I define an alcohol-involved fatal car crash as a fatal car crash where a police officer reported that alcohol was involved for at least one driver in the crash. I estimate Equation 1 using the number of alcohol-involved fatal car crashes as the dependent variable.

This approach yields that alcohol-related fatal car crashes increased by a statistically significant 10.1% after recreational marijuana dispensary openings (Table 7). An analogous OLS analysis shows that having a marijuana dispensary open increases monthly alcohol-involved fatal car crashes by a statistically significant 0.0032 fatal crashes per zip-month. Excluding Massachusetts, recreational marijuana dispensaries increase total fatal car crashes by 0.0142 per zip-month (Table A.1), and so approximately 23% (0.0032/0.0142) of the total effect of recreational marijuana dispensaries on fatal car crashes is driven by an increase in alcohol-impaired driving. While these estimates should be interpreted cautiously due to data limitations, this finding again suggests that the dispensary effect is driven by impairment.

Collectively, these results imply that the dispensary effect on fatal car crashes is primarily driven by increased impairment rather than increased traffic. This conclusion suggests that interventions targeting impaired driving may be more successful than interventions targeting traffic volume.

7 Marijuana Sales

Quantifying the relationship between marijuana sales and fatal car crashes could be useful evaluating policy trade-offs. Unfortunately, I do not have zip code level data on recreational marijuana sales from all five states, and so I cannot estimate the quantity of marijuana sales

Table 7: Police-Reported Alcohol Involvement

Dependent Variable:	Alcohol-Involved Crash	
Model:	(1)	(2)
	Poisson	OLS
Variables		
Dispensary Open	0.1014**	0.0032**
	(0.0468)	(0.0014)
Fixed-effects		
Zip Code (2,890)	Yes	Yes
Month-State (720)	Yes	Yes
Fit statistics		
Observations	519,204	519,204
Squared Correlation	0.04090	0.03845
Pseudo R^2	0.12550	-0.05914
BIC	168,292.6	-316,888.4
Dependent variable mean	0.02916	0.02916

Clustered (Zip Code) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents two separate regressions: the first column is estimated by Poisson and the second by OLS. In each model, the independent variable of interest is an indicator for whether at least recreational marijuana dispensary is open in a given zip-month ("Dispensary Open"), and the dependent variable is the number of fatal car crashes with at least one driver with police-reported alcohol involvement (DRINKING in the FARS data). Each model controls for zip code and state-by-month fixed effects, as well as business and demographic covariates. These regressions exclude data from Massachusetts, where less than 25% of fatal car crashes have at least one individual with non-missing data for alcohol involvement. Over 50% of fatal car crashes in each of the remaining states (CA, CO, OR, and WA) have at least one driver with non-missing data for alcohol involvement.

per car crash directly. However, using dispensary-level sales data from Washington, I can provide some informal estimates on the effect of marijuana sales on fatal car crashes.

I use a two-sample instrumental variables approach to estimate the effect of marijuana sales on fatal car crashes. I use the count of fatal car crashes as the dependent variable, marijuana sales (in 2010USD) as the endogenous variable, and having a dispensary open as the instrument. I estimate the reduced form moment on the entire analytical sample and the first stage moment on only Washington zip codes. I control for business and demographic variables, as well as zip code and state-by-month fixed effects. Introducing recreational marijuana dispensaries in a Washington zip code is associated with $320,000 in monthly recreational marijuana sales, and introducing dispensaries is associated with 0.0138 more monthly fatal car crashes. Combining these two, $10 million in total sales is associated with a statistically significant increase of 0.44 fatal car crashes (95%CI 0.127-0.743). This implies that there is an additional fatal car crash for each $23 million of marijuana sales. For reference, with around 11,000 alcohol-related fatal car crashes[10] and around $220 billion of alcohol sales[11] in the United States each year, there is an alcohol-related fatal car crash for approximately every $20 million of alcohol sales.

It is comforting that these back-of-the-envelope analyses suggest a reasonable amount of marijuana would have to be sold to cause an additional fatal car crash. However, this result should be interpreted with caution. Marijuana sales per zip code with a dispensary may vary substantially across states, and if they do, then the two-sample instrumental variable estimate could be substantially biased.

8 Conclusion

In this study, I investigated the local-level effect of opening a recreational marijuana dispensary on automobile fatalities in five US states that adopted RMLs. I found that introducing

[10] https://www.nhtsa.gov/risky-driving/drunk-driving

[11] https://www.statista.com/statistics/233699/market-share-revenue-of-the-us-alcohol-industry-by-beverage

a recreational marijuana dispensary caused a 5.7% increase in the rate of fatal car crashes. I provide several pieces of evidence that this effect is due to impairment and not traffic. The effect is robust to controlling for business and demographic controls; it is not present for other retail business openings; it is stronger for the first dispensary than subsequent dispensaries; and it is concentrated at nighttime, after most retail stores have closed. Altogether, my analyses suggest that the effect is due to an increase in impaired driving instead of increased traffic. In context, these effects on fatal car crashes have a magnitude somewhere between that of text messaging bans (4%) (Abouk and Adams, 2013) and mandatory seat belt laws (8%) (Carpenter and Stehr, 2008).

To my knowledge, this is the first study to estimate the effect of recreational marijuana on fatal car crashes using within-state variation in dispensary openings. Some previous studies have used state-level analyses. By focusing on zip codes introducing dispensaries instead of states adopting RMLs, I dramatically improve the power of the analysis and am able to much more precisely estimate the effect of recreational marijuana on fatal car crashes. Further, the existing, state-level literature is prone to bias from confounding trends at the state-level. In my approach, I use state-by-month fixed effects to flexibly account for unobserved time-varying, state-level confounding and eliminate this bias. Further, this study uses more states and a longer time period than past published studies.

There are limitations to my study design. First, when using zip-code level data instead of state-level data, aggregation bias is mitigated but not entirely eliminated. Unfortunately, individual level data is not available, and zip codes appear to be the smallest feasible level of aggregation for analysis. Second, it is not possible to entirely rule out the possibility of confounding trends at the zip-code level. However, I have conducted several tests suggesting that the effect is, indeed, driven by impaired driving. Third, my preferred DD approach implies no spillovers between treated and untreated zip codes (e.g., increased car crashes in zip codes neighboring a new marijuana dispensary). This assumption is unlikely to hold, as some zip codes are small geographic areas with frequent through-traffic. However, these zip code spillovers would only attenuate my results, making the estimate overly conservative, and I show in a distance model that accounts for cross-zip code spillovers that recreational marijuana dispensaries still significantly increase fatal car crashes.

These results provide a key insight to local, state, and federal policy-makers considering different marijuana policies. Recreational marijuana dispensaries likely have a variety of effects, and policy-makers will have to carefully weigh the costs against the benefits of expanding access to recreational marijuana. Precise estimates like those presented in this paper can help policy-makers navigate these decisions. Further, policy-makers may wish to enhance traffic safety measures in areas that open a recreational marijuana dispensary. I find that impairment is the driving force behind the increase in fatal car crashes, which suggests that interventions targeting impaired driving may be successful. Which specific programs could mitigate the effect of dispensaries on fatal car crashes — marijuana sale restrictions, educational campaigns, or deterrence through penalties – is fodder for future research.

Funding

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Declaration of Interest

The author reports an equity interest in Data Science Solutions LLC, a public health consulting firm, outside the submitted work.

Appendix

Table A.1: Preferred Specification Excluding MA

Dependent Variable:	Fatal Crashes	
Model:	(1)	(2)
	Poisson	OLS
Variables		
Dispensary Open	0.0590***	0.0142***
	(0.0229)	(0.0033)
Fixed-effects		
Zip Code (2,890)	Yes	Yes
Month-State (720)	Yes	Yes
Fit statistics		
Observations	519,204	519,204
Squared Correlation	0.12956	0.12593
Pseudo R^2	0.14444	0.14528
BIC	407,608.0	458,772.2
Dependent variable mean	0.12922	0.12922

Clustered (Zip Code) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents results two regressions, estimated on the analytical sample excluding Massachusetts. Column (1) is estimated by Poisson regression, using the rate of fatal car crashes as the dependent variable. Column (2) is estimated by OLS, using the count of fatal car crashes as the dependent variable. In each regression, the independent variable of interest is "Dispensary Open", which is an indicator that at least one recreational marijuana dispensary was open in that zip code-month. Each regression controls for zip code and state-by-month fixed effects, as well as demographic and business covariates.

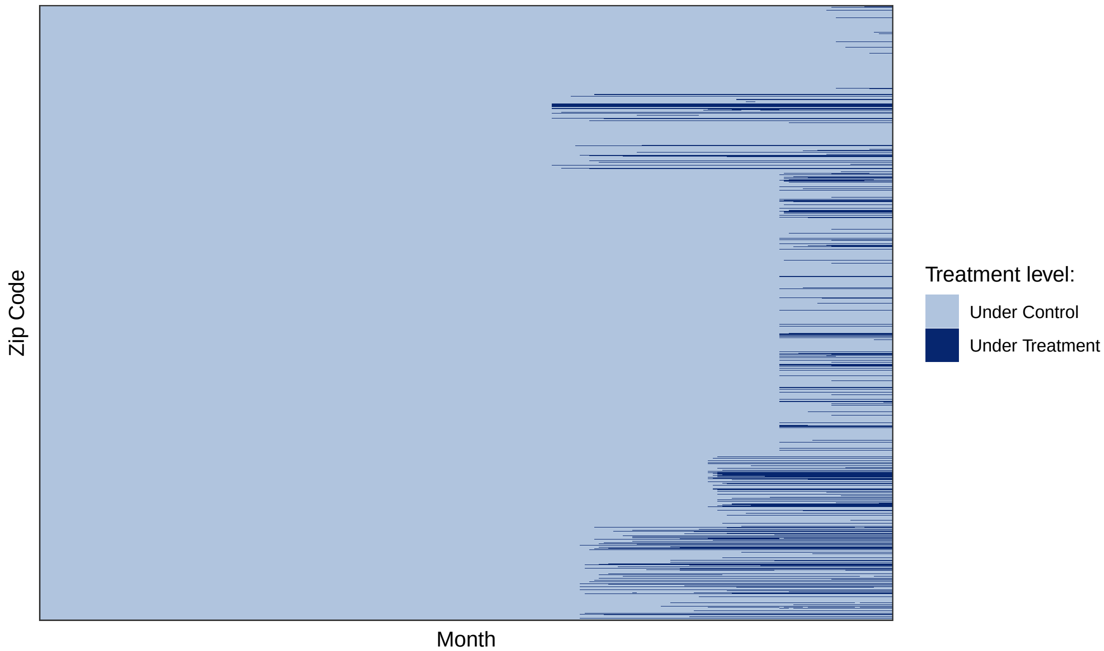
Table A.2: OLS Estimates for Effects of Recreational Marijuana Dispensaries

Dependent Variables:	Fatal Crashes	Deaths	Daytime Crashes	Nighttime Crashes
Model:	(1)	(2)	(3)	(4)
Variables				
Dispensary Open	0.0138***	0.0135***	0.0029	0.0105***
	(0.0033)	(0.0037)	(0.0021)	(0.0026)
Fixed-effects				
Zip Code (3,375)	Yes	Yes	Yes	Yes
Month-State (900)	Yes	Yes	Yes	Yes
Fit statistics				
Observations	606,504	606,504	606,504	606,504
R^2	0.12489	0.11482	0.04976	0.08063
Within R^2	0.00023	0.00024	8.25×10^{-5}	0.00014
Dependent variable mean	0.11880	0.12958	0.05387	0.06308

Clustered (Zip Code) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents four separate OLS regressions that vary only by their dependent variable. In each model, the independent variable of interest is an indicator for whether at least recreational marijuana dispensary is open in a given zip-month ("Dispensary Open"). Each model controls for zip code and state-by-month fixed effects, as well as business and demographic covariates. The columns present the DD estimate of opening a dispensary on the rate of (1) fatal car accidents; (2) automobile deaths; (3) fatal accidents occurring in the daytime (8AM to 7PM); and (4) fatal accidents occurring at nighttime (8PM to 7AM).

Figure A.1: Treated and Untreated Zip Codes over Sample Period



This figure shows all zip codes in the sample (CA, CO, MA, OR, WA) across the entire sample period (2005-2019). Zip codes are represented on the Y axis, and months are represented on the X axis. The light blue areas indicate zip code months that do not have a dispensary, while the dark blue areas indicate zip code months that have at least one dispensary.

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