Recreational Marijuana Dispensaries and Fatal Car Crashes*

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Abstract

Fatal car crashes are a leading cause of death among younger Americans and have become a central concern in the US marijuana policy debate. I provide new evidence on the effect of marijuana on traffic fatalities by exploiting zip code level data on the opening of recreational marijuana dispensaries in five US states. 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 recreational marijuana dispensary openings increased the rate of fatal car crashes by approximately 6%. These effects are not observed in a placebo analysis of retail pharmacy openings, and the increase in fatal crashes is concentrated at nighttime, after most dispensaries have closed. Collectively, these findings suggest that the effect on fatal car crashes is driven by marijuana impairment rather than increased traffic.

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[1] 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. On the other hand, 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). If individuals substitute from alcohol and other drugs to marijuana, then it could decrease automobile fatalities. Therefore, the relationship between recreational marijuana utilization and driving fatalities is an empirical question.

In this study, I use a differences-in-differences (DD) framework over the timing of marijuana dispensaries opening in zip codes within five RML states (CA, CO, MA, OR, and WA) to estimate the effect of recreational marijuana on fatal car crashes. After controlling for zip-code-by-state and time fixed effects as well as several time-varying demographic and business covariates, I find that opening a recreational marijuana dispensary in a given zip code is associated with a statistically significant 5.9% increase in the rate of fatal car crashes relative to other zip codes in their state. 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.

This analysis is robust to a number of specification checks. An event study confirms the assumption of parallel pre-dispensary trends, addressing concerns of selection bias. An alternative explanation for my result is that areas that introduce a marijuana dispensary may increase commercial activity, which could increase traffic and car crashes. I address this alternative explanation in three steps. First, my results are robust to the inclusion of demographic and business covariates. Second, I show the effect does not exist in a placebo analysis with retail pharmacies. Finally, the increase in traffic fatalities is concentrated at

[1]The CDC website states that motor vehicle traffic is the leading cause of death for Americans age 1 to 54, but I find that it is a close second to unintentional poisonings (CDC, 2020). Nevertheless, motor vehicle crashes are widely recognized as a leading cause of death among younger Americans.

nighttime, after most dispensaries have closed. Collectively, these results suggest that the increase in traffic fatalities is due to marijuana impairment rather than increased traffic.

To date, a handful of 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 imprecision 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. 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 2020, only eight states have opened 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 is small and still growing in RML states, which would make state-wide effects even more difficult to detect.

My approach, which uses dispensary openings at the zip-code-month level, more exactly pins down the relationship between recreational marijuana and traffic fatalities. This methodological approach dramatically improves power relative to state-year analyses. In addition, this approach addresses another major limitation of past studies: the possibility of time-varying, state-level confounding. When studied at the state-year level, it is impossible to disentangle the effect of recreational marijuana laws from confounding state trends and/or concurrent policies (e.g., state-level drug policies, traffic safety interventions). This can be a major concern given the limited number of treated states studied, and some have argued that these confounding trends lead to noisy and counterintuitive results (Caputi, 2019; Shover et al., 2019). I eliminate the possibility of state-level confounding by controlling for state-by-time fixed effects.

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.

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, followed by a conclusion in section 6.

2 Background

2.1 Recreational Marijuana Laws and Dispensaries in the United States

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. Consequently, results from state-level analyses of marijuana policy are unreliable – or even unreasonable. The strength of this criticism is particularly evident in the context of MML research; a very small share (approximately 2.5%) of the overall population in MML states report using marijuana recommended by a physician at all. Therefore, effect estimates reported in past studies, such as a 15% decrease in alcohol sales, were unreasonable (Caputi, 2019). Aggregation bias is likely still a major concern in state-level RML analyses, particularly as only a few states have operationalized RMLs for long enough to collect data. 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 Literature Review

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.[2] These studies can typically be 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

[2]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 other countries have only recently legalized recreational marijuana.

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.[3] 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 travelled) 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 travelled 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 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.

To my knowledge, the only existing study that used individual dispensary opening, thereby leveraging the additional power of local-level variation in marijuana policy, to study the effect of recreational marijuana on automobile crashes is a working paper by Ellis et al. (2019). Ellis et al. (2019) combined a hand-collected dataset of the opening and closing dates of individual medical marijuana dispensaries with annual zip-code-level data on automobile insurance premiums[4] from Nielsen for nine states that began medical marijuana sales between 2014 and 2017. The authors found that zip codes within 25 miles of a medical marijuana dispensary experienced a very small but statistically significant $11.50 decrease in average annual automobile insurance premiums relative to other zip codes in states that had

[3] 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.

[4] By using auto insurance premiums, the authors claim to account for all automobile crashes rather than just fatal crashes, a major limitation in much of the existing literature.

also legalized medical marijuana.[5] The dearth of studies exploiting intra-state variation to investigate the relationship between recreational marijuana dispensaries and traffic fatalities motivates the empirical design of this study.

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.[6] 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.[7] 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 individual zip codes had an operational dispensary on the first day of the month.

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 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. Some individuals undoubtedly use recreational marijuana to self-medicate for certain medical conditions, but adults are permitted to purchase and use marijuana even if the only goal is to achieve impairment.

In this study, I focus on recreational marijuana rather than medical marijuana for two

[5] The average annual premium in their full dataset was \$3743.00, so an \$11.50 decrease is small in percentage terms.

[6] 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.

[7] License issuance and expiration does not necessarily correspond perfectly to the opening and closing of dispensaries. However, dispensaries are unlikely to operate without an active license (low Type 2 error) and counting some zip codes as having an operational dispensary when they do not (Type 1 error) is likely to bias my results towards the null.

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. One study of medical marijuana dispensaries (Ellis et al., 2019) hand-collected dispensary locations, opening dates, and closing dates by performing Google searches, though it is difficult to evaluate the accuracy or completeness of this dataset.

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.[8] 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

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

(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. My main specification is summarized in the following Poisson regression equation:

$$log(E[y_{zst}]) = \gamma 1(\text{Dispensary Open}) + X_{zt}\gamma + \alpha_z + \alpha_t + \alpha_t \times \alpha_s + \epsilon_{zst} \quad (1)$$

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, α_s, and αt are zip code, state, and month fixed effects; and ϵ_{zst} is a stochastic error term. 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 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:

$$log(E[y_{zst}]) = \sum_{k=-10;k\neq-1}^{10} \gamma_k D_k + X_{zt}\gamma + \alpha_z + \alpha_t + \alpha_t \times \alpha_s + \epsilon_{zst} \tag{2}$$

This event study is a fully dynamic version of the DD model in Equation 1. D_k is a series of lags and leads for halfyears (i.e., 6 month periods) before and after the recreational dispensary first opens in a given zip code.[9] The γ_k coefficients are normalized such that the base period is the halfyear 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 $k \leq 0$. 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).

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.

[9] As is common in event studies with long panels, the lags and leads are truncated. γ_{-10} corresponds to all halfyears at least 5 years before the dispensary first opens and γ_{10} corresponds to all halfyears at least 5 years after the dispensary first opens.

5 Results

5.1 Baseline Results

I begin with a description of the summary statistics in Table 1. 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 are county unemployment and median household income; treated zip codes have much higher median household income ($63200 vs. $53420) and lower (county-level) unemployment rates after the dispensary opens (4.3% vs 7.3%) than before. 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 controls address this issue.

Next, I provide evidence that dispensary openings increase fatal car crashes. Table 2 show the results of a Poisson estimate of equation 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 effectively 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 month-by-state 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.[10] 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 5.7% 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

[10]Recreational marijuana laws could affect marijuana-related stigma at the state-level, but because I include month-by-state 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.

controls for demographics (Column 3) increases the estimate by around 0.1%, and adding controls for business covariates (Column 4) changes the estimate by only another 0.1%. 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 month-by-state fixed effects and adjusting for the full set of zip-by-month varying covariates, introducing a marijuana dispensary is associated with a 5.9% increase in automobile fatalities (Column 4).[11] 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 barely affects the estimate: opening an RML increases the rate of car crash deaths by 5.5%. In absolute terms, in just the treated zip codes in the five US states over the length of the study period, recreational marijauana dispensaries caused approximately 494 excess fatal car crashes and 508 excess car crash deaths.

This effect size is comparable in magnitude 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%.[12] 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). The effect of opening a recreational marijuana dispensary is equivalent to the effect of a large decrease in beer taxes: one study focused on youth drivers found that an increase of 10% in beer taxes reduces motor vehicle fatalities among drivers aged 15-24 by only 1.3% (Morrisey and Grabowski, 2011).

It is useful to compare the effect size estimated here (5.9%) to previous estimates of the effect of RMLs on fatal car crashes computed 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

[11] A robustness check additionally accounting for zip-specific linear trends yields an effect estimate of 4.8% (SE: 0.0301). It is unsurprising that the effect estimate is no longer statistically significant, as the addition of zip-specific linear trends substantially reduces the power of the analysis. Further, it is unsurprising that this effect estimate is somewhat smaller than the preferred estimate, as accounting for group-specific linear trends in differences-in-differences analyses biases effect estimates towards 0 (Wolfers, 2006). Nevertheless, it is comforting that the point estimate after accounting for zip-specific linear trends is largely in line with my preferred estimate and contained in my preferred estimate's 95% confidence interval.

[12] 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.

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 travelled, p=0.674) in Washington. Using a differences-in-differences approach, Aydelotte et al. (2019) found that RMLs were associated with an increase of 1.8 (95%CI 0.4-3.7) crashes per billion vehicle miles travelled. Given that the average in Colorado and Washington was around 9.3 crashes per billion vehicle miles travelled, 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 10.4%. 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.

5.2 Specification Checks

The main identifying assumption of my DD analysis is that of parallel trends, i.e., that the treated group would have continued on a parallel trajectory to the controls had it not been for the treatment (in this case, opening of a marijuana dispensary). Because I use a Poisson regression, this parallel trends assumption corresponds to a common trajectory in log growth rates rather than parallel trends in levels. This identifying assumption cannot be positively confirmed because we only observe each zip code with or without a marijuana dispensary at a given point in time. However, an event study can be used to evaluate whether parallel trends existed in the data before the dispensary opened. If the treatment and control groups experience substantially similar trends prior to the dispensary opening, this constitutes evidence that the parallel trends assumption may be valid. If trends prior to the dispensary opening are substantially dissimilar, however, then the parallel trends assumption is unlikely to hold and DD is not a valid, causal approach.

The event study shown in Figure 1 displays the γ_k coefficients from Equation 2. These coefficients are adjusted for zip code and state-by-month fixed effects as well as the full array of covariates, analogously to Column 4 in Table 2. Treated zip codes had a relatively parallel fatal car crash rate compared to control zip codes before the entrance of a recreational marijuana dispensary, but the rates gradually diverged in the years following. These results provide strong evidence in support of the DD's parallel trends assumption and the positive effect of the policy. Further, this fully dynamic model of the differences-in-differences design reveals that effects do not come about immediately but emerge over time, suggesting that shorter-term analyses may not capture the full effect of recreational marijuana dispensaries. These findings are robust to whether I choose the number of fatal car crashes or the number of car crash deaths as the dependent variable.

Next, I test whether the effect estimate from my preferred specification is robust to heterogeneous treatment effects, using the procedure recommended by De Chaisemartin and d'Haultfoeuille (2020). I find that none (0%) of the 27,191 comparisons in my preferred specification 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 least 76% untreated zip codes, and no zip code is treated for more than 29% of the length of the panel. Overall, because my regression generates no negative weights, the effect estimated by γ in Equation 1 is robust to heterogeneous effects and definitively not biased by negative weights.

5.3 Mechanisms

It is possible that the estimated effect is due to confounding trends at the zip code level that occur alongside the entry of the recreational marijuana dispensary as opposed to marijuana use. Specifically, it is possible that zip codes that open a commercial area may be more likely to attract traffic (and hence car crashes) as well as a new recreational marijuana dispensary. I address this concern, in part, by adjusting for demographic and economic controls, but it is nevertheless possible that residual confounding biases my estimates. I test for this possibility using a placebo: the entrance of retail pharmacies.

Like recreational marijuana dispensaries, retail pharmacies are likely to open in new and existing commercial areas and attract retail customers. Further, retail pharmacies are licensed by the state, and licensure data is publicly available in California and Colorado, allowing me to compile a panel dataset analogous to the marijuana dispensary panel dataset. In the first column of Table 4, I show that the opening of retail pharmacies in California and Colorado is largely unrelated to fatal car crashes. This finding supports the notion that opening a marijuana dispensary has an effect on fatal car crashes to an extent that opening other licensed businesses does not.

Still, it may be possible that recreational marijuana dispensaries attract a specific clientele that may be more likely to be involved in fatal car crashes. In this case, the increase in traffic fatalities could be due to the type of traffic marijuana dispensaries increase (e.g., perhaps more reckless drivers) rather than marijuana impairment. To investigate this possibility, I examine the relationship between recreational marijuana dispensaries and both daytime (8AM to 7PM) and nighttime (7PM to 8AM) traffic fatalities. Daytime automobile fatalities may be caused by increased traffic to the dispensary, while nighttime fatalities -– after most retail stores have closed — are less likely to show this effect. I find my primary results are largely driven by nighttime car crashes: daytime automobile fatalities increase only 1.7% while nighttime automobile fatalities increase by 8.4% (Table 3). Figure 2 shows the effects on fatal car crashes occurring in different 4-hour time blocks throughout the day. Again, the effect appears strongest at nighttime, after most dispensaries are open. This suggests that the observed effect is driven by impaired driving rather than increased traffic to the dispensary, supporting the hypothesis that marijuana dispensaries increase car crashes via increased impairment.

6 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 opening a recreational marijuana dispensary caused a 5.9% increase in the rate of fatal car crashes. This effect is not found for other retail store openings, suggesting that marijuana is key to the effect. Further, these results are concentrated in nighttime fatalities, when most retail stores are closed. This suggests 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 opening dispensaries instead of states adopting RMLs, I dramatically improve the power of the analysis and have far more power in precisely estimating 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 time-by-state 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 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.

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. Further, policy-makers may wish to enhance traffic safety measures in areas that open a recreational marijuana dispensary. Which programs could mitigate this effect — marijuana sale restrictions, educational campaigns, or deterrence through penalties – is fodder for future research.

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
Nighttime Crashes	0.08	0.32	0.08	0.30	0.06	0.27
Daytime Crashes	0.07	0.28	0.06	0.28	0.05	0.25
Dispensary Open	1.00	0.00	0.00	0.00	0.00	0.00
Population	24 892	17 656.20	24 993	18 204.95	15 427	18 333.18
Population Per Sq. Mi.	3364.26	5606.79	3731.16	6061.97	2203.87	4455.48
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	39.56	7.23	38.47	6.99	41.54	8.72
Median Household Income	63 186	21 799.33	53 420	18 198.64	63 351	28 609.77
Avg. Household Size	2.52	0.45	2.56	0.51	2.66	0.56
No. Employees	10 492.34	12 334.42	9724.87	11 550.98	5366.66	9444.78
No. Establishments	732.20	594.80	686.88	579.49	362.18	491.58
County Unemployment	4.31	1.44	7.33	2.96	7.06	3.36
Year	2017.58	1.40	2010.73	3.71	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 27191 after dispensary month-zips, 119149 before dispensary month-zips, and 460164 never dispensary month-zips. Over 99% of the zip codes 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: Effect of Recreational Marijuana Dispensaries on Fatal Car Crashes

Dependent Variables:	Fatal Crashes				Deaths
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Dispensary Open	0.0816***	0.0566**	0.0578**	0.0588**	0.0550**
	(0.0207)	(0.0232)	(0.0232)	(0.0232)	(0.0244)
log(1+Population)			0.0558	0.0631	0.0568
			(0.0426)	(0.0424)	(0.0451)
Share Male			0.0370	0.0427	-0.0277
			(0.1867)	(0.1871)	(0.1992)
Share Aged 21-39			-0.3366*	-0.3590*	-0.3933*
			(0.1947)	(0.1945)	(0.2165)
Population Per Sq. Mi.			4.97×10^{-6}	6.69×10^{-6}	6.13×10^{-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			-0.0177	-0.0183	-0.0070
			(0.0338)	(0.0337)	(0.0368)
log(1+Median Household Income)			-0.0890*	-0.0873*	-0.1161**
			(0.0464)	(0.0464)	(0.0484)
County Unemployment			-0.0484***	-0.0484***	-0.0519***
			(0.0064)	(0.0064)	(0.0068)
No. Employees				1.11×10^{-6}	3.84×10^{-7}
				(2.37×10^{-6})	(2.34×10^{-6})
No. Establishments				-0.0001	-9.65×10^{-5}
				(9.36×10^{-5})	(0.0001)
Fixed-effects					
ZIP	Yes	Yes	Yes	Yes	Yes
Month (180)	Yes	Yes	Yes	Yes	Yes
Month-State (900)	No	Yes	Yes	Yes	Yes
Fit statistics					
# ZIP	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	446,757.0	457,615.7	454,480.1	454,504.8	486,411.7
Dependent variable mean	0.11557	0.11557	0.11880	0.11880	0.12958

Clustered (ZIP) 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 month-by-state 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, year, and month-by-state 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.

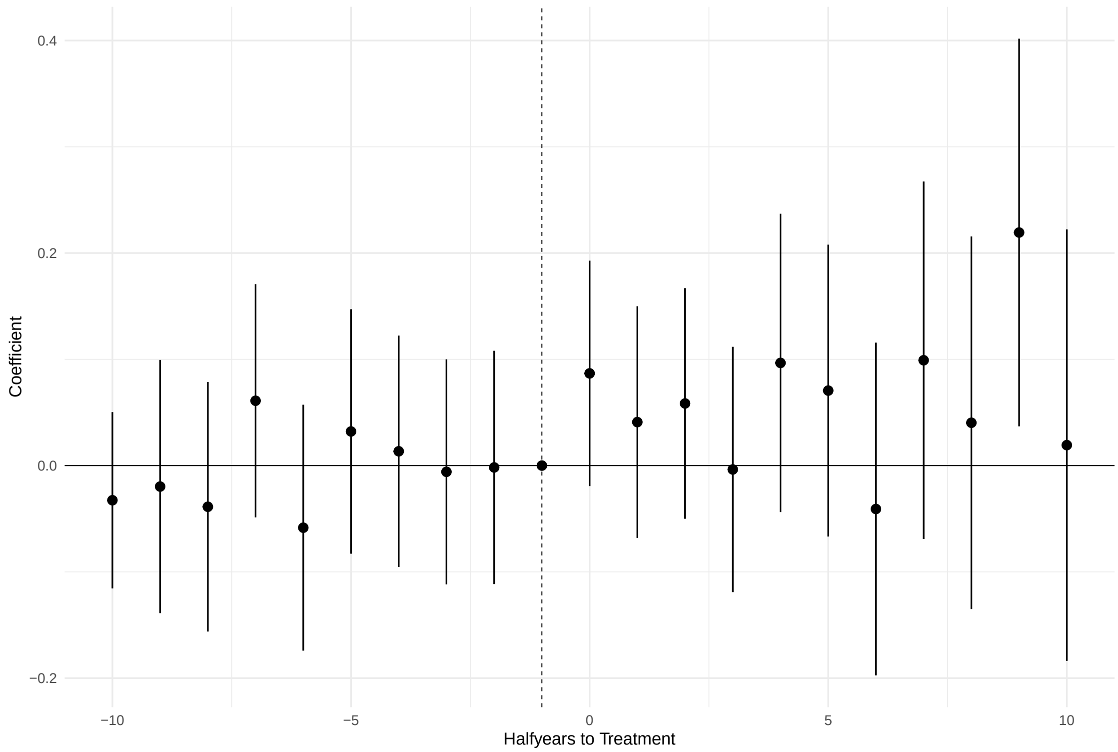
Table 3: Effects of Recreational Marijuana Dispensaries on Different Traffic Outcomes

Dependent Variables:	Fatal Crashes	Deaths	Daytime Crashes	Nighttime Crashes
Model:	(1)	(2)	(3)	(4)
Variables				
Dispensary Open	0.0588**	0.0550**	0.0177	0.0841**
	(0.0232)	(0.0244)	(0.0350)	(0.0332)
Fixed-effects				
ZIP (3,375)	Yes	Yes	Yes	Yes
Month (180)	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	0.08410
Pseudo R^2	0.14749	0.15014	0.11470	0.15431
BIC	454,504.8	486,411.7	291,056.7	308,669.9
Dependent variable mean	0.11880	0.12958	0.05387	0.06308

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

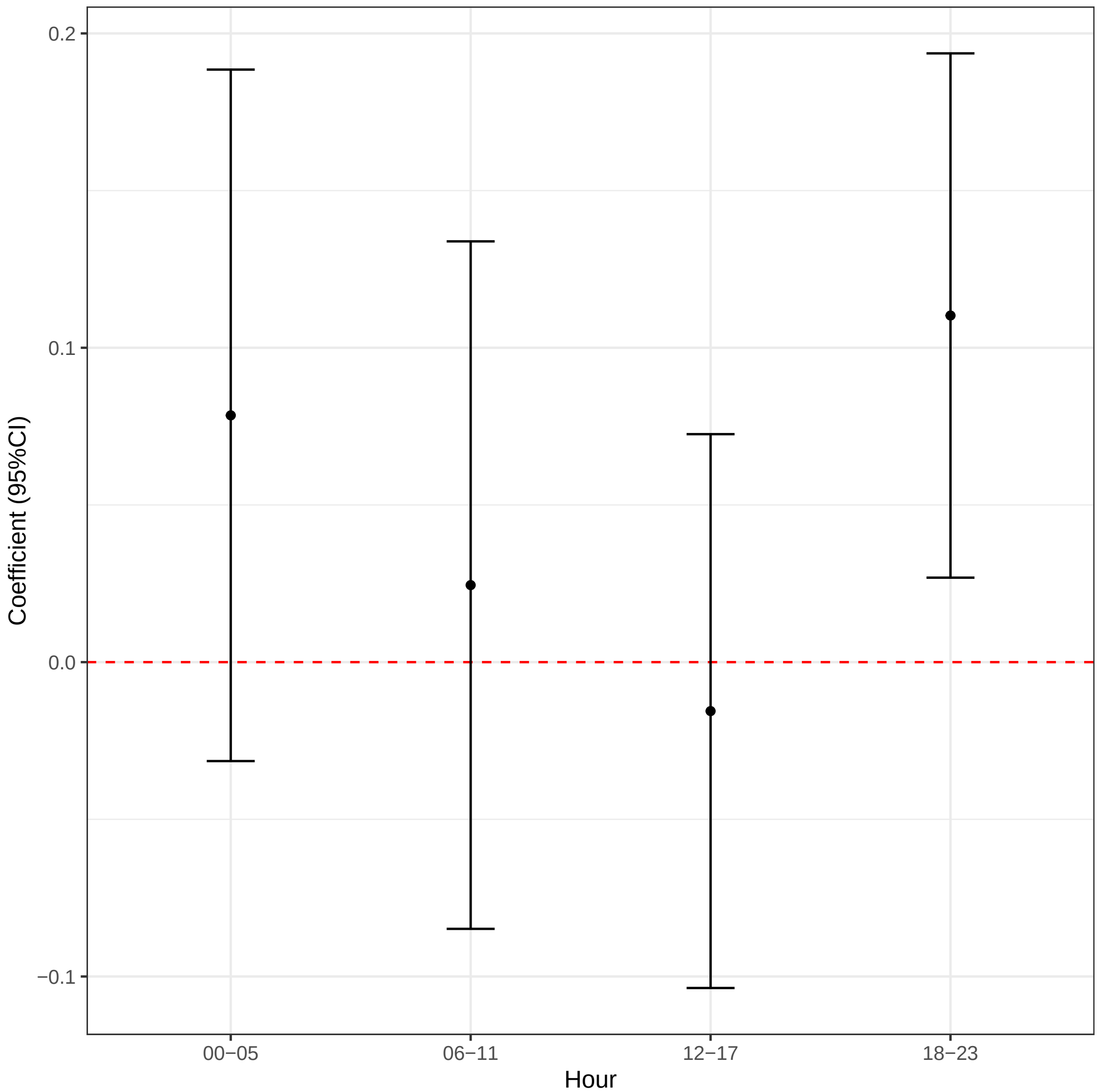
This table presents five separate Poisson regressions that vary only by their dependent variable. In each model, the independent variable of interest is an indicator for whether at least recreational marijuana dispensary is open in a given zip-month ("Dispensary Open"). Each model controls for month, zip code, and month-by-state 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.

Figure 1: Event Study Estimates of Recreational Marijuana Dispensary Opening on Fatal Car Crashes



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 -10 corresponds to all observations at least 10 halfyears (i.e., 5 years) before the dispensary opened, while the coefficient on 10 corresponds to all observations at least 10 halfyears after the dispensary opened. Standard errors are clustered at the zip code level.

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, month, and state-by-month fixed effects, as well as demographic and business covariates. Standard errors are clustered at the zip code level.

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	-0.0179	0.0057	-0.0432	0.0229
	(0.0294)	(0.0300)	(0.0458)	(0.0397)
Fixed-effects				
ZIP (1,986)	Yes	Yes	Yes	Yes
Month (180)	Yes	Yes	Yes	Yes
Month-State (360)	Yes	Yes	Yes	Yes
Fit statistics				
Observations	356,712	356,712	356,712	356,712
Squared Correlation	0.13144	0.12024	0.05192	0.08574
Pseudo R^2	0.13515	0.13720	0.10124	0.14120
BIC	313,845.8	337,355.9	196,787.5	215,567.4
Dependent variable mean	0.15344	0.16775	0.06769	0.08336

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

This table presents five 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 month, zip code, and month-by-state 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 and Colorado zip codes.

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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.