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Research Paper

Revisiting the effect of recreational marijuana on traffic fatalities

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ABSTRACT

Background: This study examines the effect of retail recreational marijuana legalization on traffic fatalities using the most current data available and recent advancements in difference-in-difference estimation methods proposed by Callaway and Sant'Anna, (2021).

Method: A modified difference-in-difference (CS-DID) is used to estimate the effect of recreational marijuana legalization on traffic fatalities reported in the Fatality Analysis Reporting System (FARS). Difference-in-difference regression models are run at the state-year level, using data from 2007 through 2020, and compared to estimates using traditional two-way-fixed-effects (TWFE) models.

Results: Consistent with past studies, results from conventional TWFE suggest traffic fatalities increase at a rate of 1.2 per billion vehicle miles traveled (BVMT) after retail of recreational marijuana begins. However, using the CS-DID model, we find slightly larger average total treatment effects (~2.2 fatalities per BVMT). Moreover, the size of the effect changes across time, where cohorts "treated" earlier have substantially higher increases than those who more recently legalized.

Conclusion: Traffic fatalities increase by 2.2 per billion miles driven after retail legalization, which may account for as many as 1400 traffic fatalities annually. States who legalized earlier experienced larger traffic fatality increases. TWFE methods are inadequate for policy evaluation and do not capture heterogeneous effects across time.

Introduction

Marijuana is the most commonly used federally illicit drug in the United States, with an estimated 49.6 million people acknowledging use (at least once) in 2020 (SAMHSA 2021). The legality and accessibility of recreational marijuana have evolved substantially in the last decade, starting in 2012 with state-wide referendums by Colorado and Washington to legalize recreational use. Numerous states have followed suit, and legalized recreational cannabis use via their legislature (Vermont, Illinois, New Mexico, Virginia, New York, Connecticut) or through ballot measures (Alaska, Arizona, California, Maine, Massachusetts, Michigan, Montana, Nevada, New Jersey, Oregon, and South Dakota). Given the flurry of recent policy changes, over 91 million Americans now live in states that have legalized recreational marijuana (U.S. Census Bureau 2022).

While marijuana may have recreational and medical benefits, its use has been linked to adverse cognitive and physical consequences (Keyhani et al., 2018). These negative effects include decreased critical thinking, decision-making ability, and motor skills (Leung et al., 2019). While the magnitude and duration of impairment following cannabis use are mostly understood (Eadie et al., 2021), long-term social and health implications are less certain (French et al., 2022). Given its potential for detrimental impacts, the substantial increase in recreational marijuana use over the last decade is likely to have implications for future policy and social outcomes (Cerda´ et al., 2020; Doran et al., 2021). This study adds to this literature by investigating the effect of retail recreational marijuana on traffic and highway safety, the understanding of which is crucial for designing and evaluating policy (Brands et al., 2021).

Before reviewing the current literature, let us first define recreational legalization, and why we investigate it separately from medical marijuana. In this paper legalized recreational marijuana is defined as state law that allows retail recreational sales (without prescription) by a retail outlet. Specifically, we do not count a state as treated until the opening of its first retail outlet, which can lag substantially behind the initial change in law. We do not draw a distinction between decriminalization, tax rates, enterprise structure, home growing restrictions, or licensing requirements.[1]

[1] This binary designation is used to simplify variations across time and state. In reality, marijuana policies can vary on numerous dimensions (e.g. full prohi-

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The AAA Foundation for Traffic Safety found that marijuana-related fatal crashes in Washington increased after legalization of recreational marijuana (Tefft, 2010), though marijuana-related accidents are a problematic metric, since habitual users may test positive even when they are not impaired (Marcotte et al., 2022). Research also suggests a higher frequency of accidents among cannabis users, according to case-control and culpability studies (Hall, 2016), and an increased crash risk under the influence of THC (Burggren et al., 2019; Rogeberg et al., 2018; Windle et al., 2022). It is therefore reasonable to surmise that increased legalization and access to marijuana are likely to impact highway and traffic safety, however the quantitative results of such analyses are mixed (Pearlson et al., 2021). As such, our goal is to further elucidate this relationship using additional data and recent developments in econometric methods.

We use newly released data (through 2020) from the National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS), which captures additional cross-sectional and temporal variation as more states legalized recreational marijuana. Additionally, we use modified difference-in-difference (DID) techniques suggested by Callaway and Sant'Anna (2021) (referred to herein as CS-DID), which are more appropriate, since the staggered timing of marijuana legalization across states likely induces heterogeneous "treatment" effects (Callaway and Sant'Anna, 2021). While variation in the legalization of marijuana across states has created a ripe environment for researchers to examine its effects on a host of welfare and human health indicators, many previous investigations do not properly account for the possibility of heterogeneous treatment effects across time.

A logical mechanism for the change in traffic fatalities following legalization is an increase in individuals driving under the influence. Interestingly, recreational users are less likely to drive impaired than medical users (Arkell et al., 2020; Brands et al., 2021). There is also some evidence that medical users have significantly lower drug problem severity (Roy-Byrne et al., 2015). Additionally, recreational marijuana increases access and use among the public, both by eliminating the need for a prescription and via increased geographic retail presence (Martins et al., 2021; Zellers et al., 2023). Given this difference, and the recent expansion of recreational marijuana use in the United States, we specifically focus on the effect recreational marijuana retail has on traffic fatalities.

Several studies have identified statistically significant relationships between marijuana legalization and traffic accidents, although mixed results are observed in studies of medical marijuana laws. Some research suggests medical marijuana legalization resulted in fewer fatal car accidents involving people aged 25 to 44, though fatalities varied substantially across state (Santaella-Tenorio et al., 2017). In an analysis of FARS data between 1985 and 2014, states with legal medical marijuana often had lower traffic fatality rates than non-legalized states, and experienced an immediate drop in fatalities following medical legalization (Santaella-Tenorio et al., 2017). In some cases, states that legalized medical marijuana experienced up to an 11% decrease in traffic deaths (Mark Anderson et al., 2013). While the specific mechanism for this decrease is unclear, drivers under the influence of marijuana are likely to drive at reduced speeds, pass less frequently, and make fewer lane changes (Hartman and Huestis, 2013; Mark Anderson et al., 2013; Ronen et al., 2008). As such, even if accidents increase with marijuana legalization, fatalities may not.

Beyond driving behavior changes, there is also an ongoing debate over the possibility that marijuana and alcohol are substitutes (or complements). If the two drugs are substitutes, then increased accessibility to marijuana may reduce drunk driving, thereby decreasing fatalities. We largely ignore this mechanism as research results into the relatedness of these goods are ambiguous (Saffer and Chaloupka, 1999; Subbaraman, 2016; Williams et al., 2004). It is indeed possible that marijuana acts as a substitute for any number of activities that increase road danger, though identifying such substitutions is beyond the scope of our analysis.

Other work suggests a positive relationship between medical marijuana and traffic deaths. While alcohol-positive traffic deaths remained steady between 1999 and 2010, the prevalence of cannabinol increased from 4.2% in 1999 to 12.2% in 2010 among states that legalized medical marijuana (Brady and Li, 2014). This result is consistent with the finding that driving under the influence of cannabis increases after the introduction of medical marijuana laws (Fink et al., 2020), though it does not suggest an aggregate effect of legalization on traffic fatalities. While this study observes an increase in "marijuana-related" fatalities, an overall increase in traffic fatalities in states that legalized medical marijuana is not observed (Mark Anderson et al., 2013). Moreover, marijuana-related accidents are a problematic metric to infer overall changes to road safety, since habitual users may test positive even when they are not impaired. Despite the possible link between legalization and accidents, the effect of retail marijuana legalization on traffic fatalities are less clear, though evidence suggests a positive association (Vingilis et al., 2021).

Following the legalization of retail recreational sales in Colorado and Washington, and the readily available FARS data, several studies were conducted specifically examining the relationship between recreational marijuana laws and traffic fatalities. For example, following legalization, the number of drivers who tested positive for THC after being fatally injured climbed by 92% and 28% in Colorado and Washington, respectively (Hansen et al., 2020). However, the authors of this study are unable to identify a causal link using a synthetic control approach. Instead they find that synthetic control groups saw similar changes in marijuana-related, alcohol-related, and overall traffic fatality rates despite not legalizing recreational marijuana (Hansen et al., 2020). When con-trolling for previous medical marijuana laws (MMLs) Dewey et al. (2021) find no significant change associated with recreational legalization and traffic fatalities (Dewey et al., 2021).

In contrast, several other studies find a significant, causal relationship between traffic fatalities and the legalization of marijuana for recreational purposes, though the magnitude of the effect varies. Aydelotte et al. (2017) find a small average increase of 0.2 fatalities per billion vehicle miles traveled (BVMT) after recreational legalization, though the coefficient is insignificant (Aydelotte et al., 2017). The authors re-conducted the study in 2019, with additional data and specifically focusing on the presence of commercial dispensaries. They find an insignificant effect (1.2 fatalities/BVMT) following legalization but a statistically significant increase 1.8/BVMT following the opening of commercial dispensaries. Their paper is most similar to our own and is in part the justification for using legal recreational retail as the treatment in our DID analysis. A more recent study, suggests the effect is even larger (2.1/BVMT) (Kamer et al., 2020).

Despite prolific research in this area, decisive results from which we can draw conclusions remain elusive. We suggest several reasons for seemingly contradictory evidence. First, the dependent variable (metric of interest) is not consistent across studies; for example, Kamer et al. (2020) and Aydelotte et al. (2017) use fatalities per vehicle miles traveled whereas Lane and Hall (2019) use fatalities per total population of the state (Aydelotte et al., 2017; Kamer et al., 2020; Lane and Hall, 2019). Second, determining the "treatment" of a policy is not entirely clear. For example, the date of legislation is rarely the date of implementation. Additionally, marijuana legalization may affect total number of accidents, impaired driving prevalence, and traffic fatalities differently.

The second explanation for these contradictory findings is one of sample size, timing, and potential bias. Studies conducted shortly after legalization have fewer states and years included in their data set and may provide misleading results if the treatment effect changes across time. These early studies may not appropriately account for the temporal aspects of sales, accessibility and legal status given the con-

bition, decriminalization, if and how much cannabis adults can legally grow for person use, etc.) [14].

trolled opening of dispensaries after legalization. Indeed, this explanation is why Kamer et al. (2020) suggest "changes may not be detected immediately after legalization, but only after a longer time period or after commercial sales begin" (Kamer et al., 2020). For example, Aydelotte et al. (2017) were initially limited by the number of treated states and number of years post-treatment, since their analysis ran from 2009 to 2015.

Several studies using "marijuana-related" accidents (e.g. Lee et al., 2018) or fatalities may draw different conclusions because they are answering slightly different questions. We avoid this metric based on the following logic. First, states that have legalized recreational marijuana may have unobserved changes in enforcement and testing policies, that confound researchers' ability to measure the metric of interest. Second, traffic incidents may be flagged as "marijuana-related" even when drivers are no longer impaired by its use. THC remains detectable in blood past the duration of impairment. Thus, accidents and fatalities involving regular cannabis users may be labeled "marijuana-related", even in the absent of impairment. While these studies are still useful, they may overestimate the number of crashes in which THC is a causal factor.

Yet another explanation is the experimental design, model choice, and confounding factors. Some of the aforementioned studies are simply time series descriptions, which do little to identify causal effects. Of those studies that use DID, synthetic controls, and other methods that allow causal inference, there is little consistency in the covariates included in their regression analyses. Additionally, the appropriate "treatment" determination is also unclear, since there is a considerable lag between marijuana legislation and its accessibility (e.g. the opening of retail stores). Previous studies have handled these nuances differently.

Further, recent developments in econometrics and statistical analysis suggest traditional DID methods may not allow for proper inference given the staggered timing of treatment across states. Conventional DID methods generally use two-way-fixedeffects (TWFE) when treatment varies across time, but these models do not allow for proper inference in the case of treatment heterogeneity (De Chaisemartin and D'Haultfoeuille, 2022; Goodman-Bacon, 2021). This, and other issues, are expounded in the methods section.

Methods

This study uses panel data from 50 states and Washington DC to estimate the effect of recreational marijuana laws on traffic fatalities. We are the first to include the 2020 data from the National Highway Traffic Safety Administrations FARS such that the years of our study include 2007 through 2020. Additionally, we highlight specific model specification choices and conduct substantial sensitivity analysis to determine how such choices affect our results.

Difference-in-difference advancements

Traditional DID methods are used prolifically in quasi-experimental contexts–evaluating everything from parking policy to Medicaid (Maas and Watson, 2018; Yarbrough, 2020). This method has even been referred to as the most widely applicable design-based estimators in the field of economics (Angrist and Pischke, 2008). In its basic form, DID has two periods and two groups. Neither group is treated in the first period, and one group is treated in the second period, with the underlying presumption that the average outcome for both groups would have followed parallel paths if the treatment never occurred. In practice, researchers have modified the canonical DID to identify treatment effects across time and groups by using two-way-fixed-effects (TWFE).

Recent developments in the field highlight key methodological issues with using the TWFE-DID estimator to draw policy conclusions with staggered treatment panel data sets. If treatment effects are constant across time, the general TWFE-DID estimator is a variance-weighted average of all possible two-group/two periods DID estimators. However, if the treatment effect varies across time, some of these estimates are averaged with negative weights because already-treated units act as controls, which changes the proportion of their treatment effect across time (Baker et al., 2022; De Chaisemartin and D'Haultfoeuille, 2022; Goodman-Bacon, 2021). A Bacon decomposition is presented in the supplemental material, which further motivates the need to account for treatment timing.[2] These heterogeneous effects across cohorts suggest that TWFE may be insufficient to determine causal treatment effects when treatment timing is staggered and treatment effects are heterogeneous, which is likely the case in the context of marijuana legalization. For example, a set number of recreational stores do not blip into existence immediately following legalization. Rather, stores are opened over the course of several years, which has implications for marijuana accessibility across time after the treatment occurs. A similar heterogeneity may exist due to behavior changes since consumers of recreational marijuana learn over time. Much of the existing research into the effects of marijuana legalization use TWFE, which is problematic in the presence of such heterogeneity.

The CS-DID model allows for heterogeneous treatment effects across time/cohort, which is likely necessary to estimate the effect of legalization across time. CS-DID avoids the problem of the negative weighting of TWFE by avoiding bad comparisons between already treated units (late treatment and early treatment) (Roth et al., 2022).As such, we use both the traditional TWFE model and the modified CS-DID approach, and compare their results.

Data and covariates

In addition to model selection, previous TWFE work has been inconsistent in the inclusion of covariates, partially because TWFE models in most DID setups reduce concern over confounding and omitted variables (Mummolo and Peterson, 2018) and concerns that changes to time-varying covariates are potentially results of the treatment, and thus inappropriate to use as control variables (Wooldridge, 2005). For example, if GDP increases in states across time, one can reasonably assume additional traffic that may influence road fatalities. However, Colorado's marijuana industry contributes substantially to the state-GDP. Thus, some portion of the increase in GDP is itself due to a change in marijuana policy.

In the CS-DID model, covariates serve a different function. Covariates in this model are used to determine something akin to a propensity score, by which units can be determined more or less likely to fall in the treatment group. Their theoretical approach is clear, but in practice there is little direction on which (if any) covariates should be included in the model. Further, the CS-DID model relies on time-invariant covariates, of which there are very few in the current setting. As such, both the TWFE and the CS-DID model are specified in a parsimonious form (no covariates) and a full form (all relevant covariates). The list of possible covariates was based on previous literature, and a summary of the most relevant articles is presented in Table 1. Unfortunately, many of these covariates are time-variant, such that in the full CS-DID specification, they are assigned the value of the most recent year before treatment.

Distracted driving is one of the primary factors contributing to traffic fatalities in the US (National Center for Analysis and Statistics 2021, (Strayer et al., 2006)), yet laws around mobile phone use have largely been omitted in previous studies addressing marijuana policies' effect on traffic safety (Aydelotte et al. are notable exceptions). Including these laws in this analysis is critical in developing unbiased point estimates since they are (weakly) correlated with marijuana policies, and directly affect traffic fatalities. In particular, research suggests that bans on handheld devices result in a short-term decrease in traffic fatalities (36%), but

[2] Figure A1 suggests that most of the negative values on the relative weight in the overall beta are close to zero, except for one within treatment timing group that has a significant weight above 10%. A large negative weight dilutes the estimation of the beta.

Table 1
Summary of recreational Marijuana studies.

Study	Model type	Covariates	Effect on Fatalities
Dewey et al. (2021)	Poisson TWFE with timing considerations	Medical marijuana laws (MMLs), ln (Population), VMT (per capita), seat belt laws	Controlling for MMLs, no statistically significant change
Kamer et al. (2020)	DID with selective control states	Unemployment rate, speed limits, seat belt laws	Statistically significant increase of 2.1 per BVMT
Aydelotte et al. (2019)	TWFE with selective control states	Population density, GDP per capita, seat belt laws, texting ban, VMT per lane, highway spending	Statistically significant increase of 1.8 per BVMT (post commercial sales)
Aydelotte et al. (2017)	TWFE with selective control states	Population density, GDP per capita, seat belt laws, texting ban, VMT per lane, highway spending	Statistically insignificant increase of 0.2 per BVMT

have minimal long-term effects (Abouk and Adams, 2013). In fact, any reduction in accidents brought on by texting bans is transient, returning to pre-ban levels within a few months (Wright and Dorilas, 2022). Thus, ignoring the timing of distracted driver laws may result in the attribution of the fatality rebound to marijuana laws. That said, studies that have included distracted driver laws have generally found them to be highly insignificant (Aydelotte et al., 2019). Handheld and texting ban data were obtained from Bureau of Transportation Statistics (BTS 2022) and was verified from the Insurance Institute for Highway Safety (IIHS 2022).

Several other covariates are included as possible explanatory variables. These include primary and secondary seat belt laws given their intuitive relationship with highway safety. Seat belt laws were extracted from the Governors' Highway Safety Association (GHSA 2022) and the years of initial enforcement were obtained from Centers for Disease Control and Prevention (CDC) and the Insurance Institute for Highway Safety (IIHS 2022). In general seatbelt and texting laws enter our model as 0 or 1, but we do allow for values between 0 and 1, to represent the proportion of the year in which the law existed. For example, a state that implements a texting ban in July, would receive a 0.5 value for that year.

Additional covariates include GDP in 2022 dollars (BEA 2022), highway spending (USDoT FHA 2022) (FHA, 2022), and population (US Census) (U.S. Census Bureau, 2022). Lastly, we include a covariate to capture political affiliation. This metric is calculated as the percent of total votes for a democratic candidate in the most recent presidential election. While there is limited theoretical basis for this variable, states that ultimate legalize have a higher percent of democratic voters. Medical marijuana is also included as an explanatory factor, though there is a confounding issue with medical and recreational legalization. The effect of medical marijuana can be estimated, since some states only have legalized medical use and states that have legalized recreational use have years in the data set with only medical legalization. However, the effect of recreational legalization in the absence of medical marijuana laws cannot be estimated, since every state with that legalized recreational use had previously legalized medical use.

Result from the TWFE and CS-DID models are presented in the next section. Results from two specifications are reported in the next section: 1) a parsimonious specification with no additional covariates), and 2) a full model, including all covariates listed above. Numerous additional specifications of each model are included in the appendix so the reader can evaluate the effect including or excluding covariates has on the coefficient of interest (the legalization of recreational marijuana).Generally, results from the TWFE are very stable regardless of covariate choice, while results from the CS-DID model are qualitatively consistent but vary in magnitude base.

Dependent variable

To quantify the effects of retail recreational marijuana legalization, we select annual traffic fatalities per billion vehicle miles driven from 2007 to 2020 as the variable of interest. Traffic fatality data are available through the FARS. We obtained vehicle miles travelled data for each state per year from the Policy and Governmental Affairs Office of Highway Policy Information and calculate fatalities per billion vehicle miles traveled (BVMT) as a simple ratio. Given concerns about pseudo-replication and to drastically improve fit and normality assumptions, we elect the annual time-step over monthly. For completeness, results from the TWFE models run at the monthly step are included in supplemental material. Using the monthly time-step, point estimates remain similar, significance slightly increases, model residuals lose normality, and overall model fit decreases substantially.

Model specification

The parsimonious and full TWFE are presented in the results; results from additional model specifications are included in supplemental material. The TWFE models estimated are consistent with previous research efforts, where the treatment variable takes the value of 0 before recreational stores first opened in the state, and 1 afterwards. Standard errors are clustered at the state level.

The second model we estimate using Callaway and Sant'Anna's suggested specification in the presence of staggered treatment, implemented using the *csdid* in Stata (Rios-Avila et al., Aug. 2021). This method breaks the single group-time DID estimate into many 2 × 2 DID estimates and aggregates each DID to calculate the average total treatment effect across the full sample.[3] The model is estimated using an improved doubly-robust DID estimator based on inverse probability of tilting and weighted least squares derived by Sant 'Anna and Zhao (Sant'Anna and Zhao, 2020), though results are consistent regardless of estimation method. Average Total Treatment (ATT) is calculated by averaging the effect of participating treatment groups with the same number of periods in which they were treated.

A complicating factor of the CS-DID model is the choice of time-invariant explanatory variables to be used as the base-period covariates to estimate the propensity score and outcome regressions. We therefore include both the full model (all covariates and the parsimonious model (no covariates) as our main specification. In the full CS-DID, time-variant covariates are included, and take the value associated with the year before treatment. While this choice is not ideal, the substantial cross-sectional variation likely overcomes issues of within variation. For example, the population of California changes from year to year, but those variations are small compared to the population difference between California and Wyoming. The parsimonious CS-DID model includes no covariates and therefore avoids this issue.

While the CS-DID model does allow for heterogeneous treatment effects by cohort, it imposes the assumption of irreversibility in treatment. This is not a problem as no states in our sample that have legalized recreational retail marijuana have rescinded the policy.

Results

All 50 states (and Washington D.C.) are included in the sample, across 14 years for a total of 714 observation. By the end of 2020, ten

[3] An intuitive, graphical presentation of this method can be found here: https://www.stata.com/symposiums/economics21/slides/Econ21.Rios-Avila.pdf.

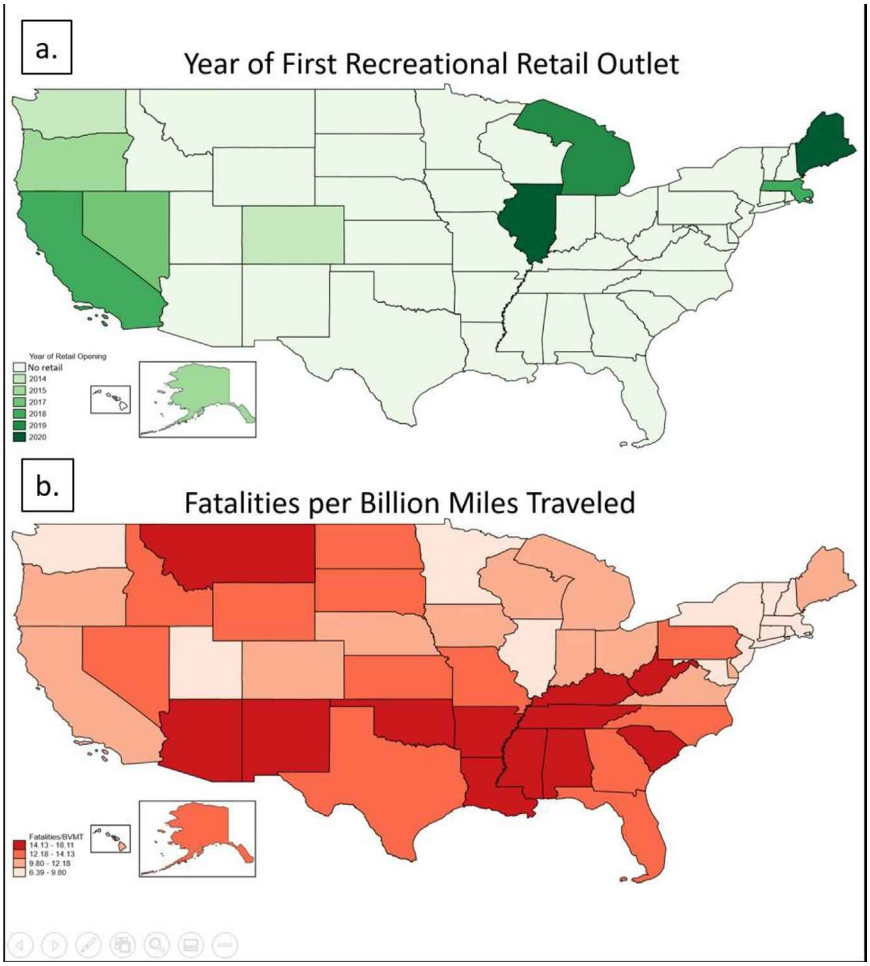

Fig. 1. Recreational Retail and BVMT. Fatalities per BVMT are averaged across the entire sample period, thus include both pre and post retail openings.

Table 2
Summary statistics.

	Mean	Std. dev	Min	Max
Fatalities	700.2	746.3	15	3995
Vehicle miles travelled (billions)	59.6	62.3	2.3	366.4
Population (millions)	6.19	6.96	0.51	39.5
GDP (trillion)	0.34	0.43	0.02	3.05
Highway spending (billion)	3.27	3.74	0.26	23.59
Primary seat belt	0.61	.47	0	1
Primary texting ban	0.61	.48	0	1
% Democratic vote	49.8	12.0	24.3	95.7

Table 3
Primary TWFE results.

Variables	(1) Parsimonious	(2) Full
Rec. Legalization	1.18**	0.89*
	(0.44)	(0.45)
GDP		1.06
		(0.86)
Population (million)		0.08
		(0.17)
Texting Ban		-0.04
		(0.28)
Primary Seatbelt		-0.30
		(0.44)
Highway Spending (billion)		0.08**
		(0.038)
% Dem. Vote		0.06*
		(0.030)
R-squared	0.455	0.487
Observations	714	714
Number of states	51	51

Year-dummies coefficients are omitted from this table robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

states had open recreational retail outlets (Fig. 1a). Summary statistics for dependent and independent variables are presented in Table 2. On average in each state, 59.6 billion miles are driven, and 700 traffic fatalities occur each year, though there is large variation from state to state. States with the highest fatalities per BVMT are generally located in the Southeast (Fig. 1b). Most states have both primary seat belt laws and texting bans in place for the years in our sample.

While the specific requirements vary slightly (see Callaway and Sant'Anna (2021)) for a discussion of parallel trends conditional on covariates), both the TWFE and CS-DID models rely on pre-treatment parallel trends to ensure validity of the estimate. While not a formal test, Fig. 2 presents the trends of traffic fatalities/BVMT for two groups: 1) States that will eventually legalize retail recreational marijuana, and 2) states where recreational marijuana remains illegal. While the baseline differs across the groups, trends across years are nearly identical, providing plausibility to our parallel trend's assumption.

Table 3 presents the results from the parsimonious and full TWFE model specification.[4] Curiously, but consistent with previous work, we

[4] Regression results using the full set of explanatory variables are presented in supplemental. AIC, BIC and overall fit (adjusted R^2) suggest that the preferred model includes GDP as the sole covariate in the TWFE model. Additional models

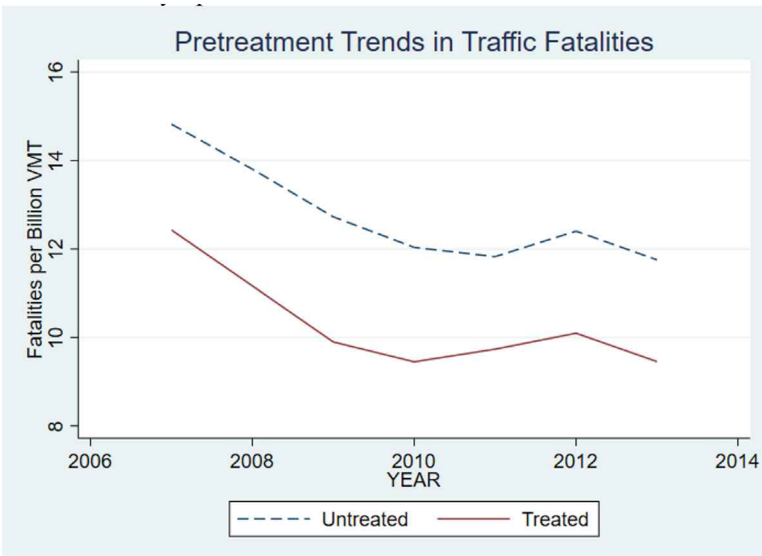

Fig. 2. Average Pre-treatment Fatality Trends. Treated refers to all states in our sample that will eventually open a retail outlet.

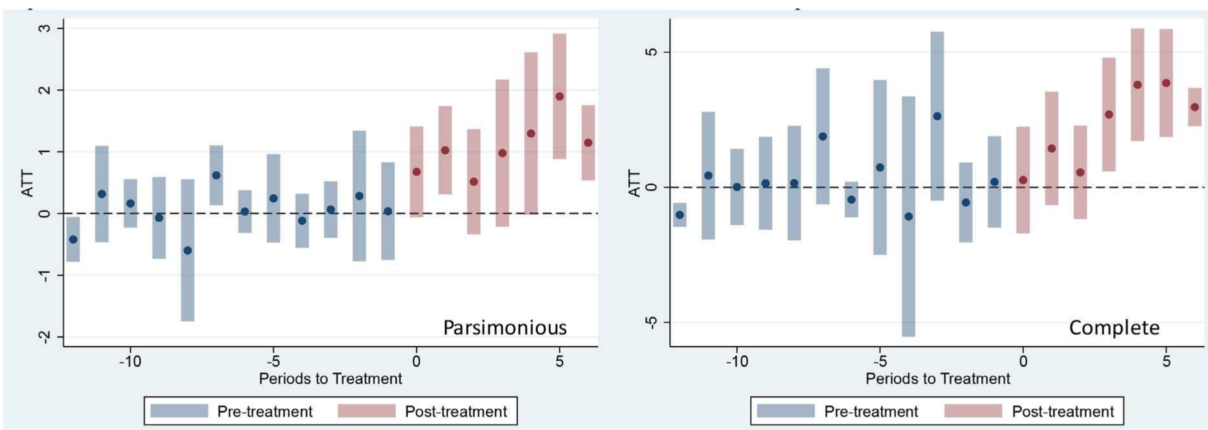

Fig. 3. Treatment effect by cohort of treatment. Left Panel uses results from the simple CS-DID while the right panel presents results from the Full (all covariates) specification. Bars represent 95% confidence intervals. Y-axis scales differ across panels.

Table 4
Average total treatment effect.

Model/Control	(1) Parsimonious	(2) Full
ATT	1.08***	2.23***
	(0.30)	(0.69)

Estimates represent the Average Total Treatment effect across all years (0-6) after treatment, aggregated by cohort.
Standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

find intuitively reasonable explanatory variables do little to improve model fit and are generally insignificant. For example, despite the established link between distracted driving and traffic accidents, texting bans appear to have no statistical effect. The same is true for seat belt laws and population. Although insignificant, coefficients for each carry the correct sign. Traffic fatalities appear to increase with Highway Spending, though this result may be reasonable since states with higher fatality rates may be willing to spend more of traffic safety. Traffic fatalities also appear positively associated with voting for Democratic presidential candidates, though we have no prior on what is driving this phenomenon.

Regardless of model choice, the coefficient of interest remains relatively stable, although loses some significance in the full model. While both specifications are presented for completeness, including many time-variant covariates in TWFE, can be problematic. As such, we prefer the parsimonious estimate, which suggests that the legalization of recreational retail marijuana has an average treatment effect of 1.2 traffic fatalities per BVMT. If we consider that roughly 860 billion miles were driven in states with recreational marijuana in 2020, excess fatalities due to recreational marijuana laws exceed 1000 annually. For context, traffic fatalities in 2020 totaled approximately thirty-nine thousand nationally (FARS) (Table 4).

As expected, results from our TWFE models are similar to those from Aydelotte et al. (2019). However, the primary motivation for this paper is implementing new DID methods to appropriately account for staggered treatment timing and potential for heterogeneous treatment effects. Using the CS-DID model and post-estimation aggregation method we calculate an average total treatment effect of 1.08 (p-value<0.001) if the model is run without additional covariates and 2.25 (p-value<0.000) if we condition on the full set of covariates. Using the CS-DID model

are also included using several transformations of the dependent variable. Based on the ability to explain within-state variation and error normality assumption, the preferred dependent variable is fatalities per vehicle miles driven (q-norm plots are also presented in supplemental material). Key results are qualitatively similar and robust to all model specifications.

therefore suggests a positive and slightly larger impact of recreational retail marijuana than previously estimated by Aydelotte et al. (2019). Interestingly, the average total treatment effect across all cohorts obfuscates the potential policy-relevant dynamics.

While Average Total Treatment (ATT) effect is perhaps the most relevant estimate for policy evaluation, we are also interested in the progression of heterogeneous treatment effects across time. Thus, Fig. 2 presents the Average Total Treatment by periods before and after treatment.

Results clearly suggest the use of traditional DID methods does not capture the dynamics of the treatment effect, which appear to increase over time. Conventional TWFE methods would provide accurate estimates for interpretation if the treatment effect were homogeneous or a one-time "shock" to the dependent variable. Clearly, this is not the case. The CS-DID method suggests that early adopters of recreational retail experience substantially larger increases in fatalities than in those states who have more recently legalized. It is worth noting that, while the CS-DID results suggest some heterogeneity in treatment effect, it does not allow us to differentiate between the underlying mechanisms driving this difference. For example, early adopters may see higher rates because the population is inherently different than more recent adopters, but it may also be the result of increased THC potency or access as across time (Fig. 3).

The qualitative interpretation of this result is not sensitive to the inclusion of co-variates, but the magnitude of the effect increases substantially when covariates are used to estimate propensity toward treatment in the CS-DID approach. While there is some variation in fatalities based on pre-treatment cohorts, most of the confidence intervals overlap with 0, which can be interpreted as having no effect based on the temporal distance from the decision to legalize. By comparison, post-treatment years have a positive slope and overlap with 0 less often. This result suggests that postlegalization has a positive effect on traffic fatalities and the effect may grow with time.

Conclusion

This study adds to the existing literature, where the majority of studies investigating similar phenomenon find positive and significant effects (Vingilis et al., 2021). Results from our work are qualitatively similar to previous findings, although we observe a larger effect size using recent advancements in DID methodology (1.08 to 2.22 fatalities per billion vehicle miles driven). Based on these estimates, traffic fatalities attributable to retail recreational marijuana legalization may account for 1400 deaths per year.

While our analysis suffers from the similar drawbacks of most quasi-experimental settings, our approach reduces concerns over the non-equivalency of intervention and comparison groups, pseudo-replication (by using an annual time-step), and heterogeneity in treatment effects based on timing. However, there is still a need for future work in this area. Specifically, our results further support the idea that marijuana availability, peer effects, norms, and other factors evolved overtime, and may be key in understanding this phenomenon. Geographic availability in particular requires further investigation. Gunadi (2022) for example, uses county-level retail openings in an attempt to capture a more geographically precise effect of retail outlets on traffic fatalities, though spill-over effects and other endogeneity concerns make such analyses difficult (Gunadi, 2022; Lane and Hall, 2019). Indeed a plausible explanation for the larger effect retail stores have on early-adopting states, may be a story of spillovers. For example, Oregon may not have experienced an increase in traffic fatalities when they began retail sales because residents were already buying retail marijuana from across the border in Washington.

Another area of concern is the use of states who have never legalized cannabis as the control group for states that have. DID methods require a parallel trend assumption, which often relies on treatment and control groups being otherwise comparable. This issue has received attention in both methodological papers and in articles investigating the effect of marijuana laws specifically (Besley and Case, 2000; Hansen et al., 2020; Wing et al., 2018; Wu et al., 2021). In the current context, states that have never legalized marijuana may differ in ways systematically affecting fatality trends across time and their choice to legalize. Although, we do observe very similar trends before legalization in 2014 for states that will and will not legalize recreational marijuana by 2020, we cannot rule out possible that states with legal recreational retail are inherently different than other states.

It is worth noting that our analysis does not evaluate such policies' total welfare or economic efficiency. Tax revenues have been significant, and are generally used to fund public services like education, which may improve other welfare metrics. For example, Colorado has cumulatively earned 2.25 billion in tax revenue since legalization, and these funds are exclusively used for health care, education, law enforcement, and substance abuse prevention and treatment programs. The comprehensive (net) effects of legalization are beyond the scope of this paper but an area where future work is needed.

Declarations of interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Ethics approval

The authors declare that they have obtained ethics approval from an appropriately constituted ethics committee/institutional review board where the research entailed animal or human participation.

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Data availability

All processed data used in this analysis can be accessed at: DATA DIRECTORY TO BE ADDED AFTER ACCEPTANCE.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.drugpo.2023.104000.

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