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Evaluation of the causal impact of recreational marijuana legalisation on traffic safety in the US

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ABSTRACT

Since the legalisation of recreational marijuana in certain US states, traffic fatalities involving drivers testing positive for marijuana have markedly increased, thereby prompting the need to understand how this policy change affects road safety. While marijuana is well-known to impair driving, determining if its recreational use directly causes more traffic fatalities remains contentious due to challenges in roadside impairment testing. Additional challenges arise because (i) Simulations may not accurately replicate driver impairment and road conditions, (ii) Estimation based on observational data must adjust for (unobserved) confounding factors, requiring an innovative model to generate causal inference, and (iii) The dynamic, evolving nature of the process requires capturing temporal relationships. This paper contributes by employing a rigorous study design based on an augmented synthetic control method to assess the causal impact of recreational marijuana legalisation on traffic fatalities. It identifies a consistent but lagged pattern of increased fatality rates in several states post-legalisation, with the effect primarily linked to the drug's retail availability. These findings disprove any prevailing conjectures that dismiss the link between recreational marijuana use and fatal traffic crashes, highlighting the need for informed policy responses.

1. Introduction

Recreational marijuana, or *adult-use cannabis*, refers to the non-medical or therapeutic use of marijuana for relaxation, socialisation, or to experience its psychoactive effects, akin to alcohol consumption in social settings. Recreational marijuana remains legally regulated in most regions around the world, with guidelines typically governing its distribution, possession, and consumption. In 2012, Colorado and Washington became the first two states in the United States to legalise recreational marijuana for adult use. Since then, twenty-one other states, the District of Columbia, and three permanently inhabited US territories have enacted laws to legalise recreational marijuana as of August 1, 2023, as shown in Fig. 1.[1] These states comprise over 50 percent of the US population. Results from the 2022 National Survey on Drug Use and Health suggest that the estimated number of past-month marijuana users in the US aged 12 and older increased from 18.9 million in 2012 to 42.3 million in 2022 (Abuse and Administration, 2023).

With the expansion of marijuana access and use across a substantial share of the US population; and with further liberalisation under consideration in other states; policymakers and researchers face an increasingly urgent need to understand the broader public health and safety implications of these legal changes. Notably, the public discourse surrounding marijuana use is marked by both promise and concern. On one hand, proponents of legalisation cite its potential to reduce criminal justice burdens and serve as a substitute for more harmful substances like alcohol. On the other, critics highlight marijuana's psychoactive effects, its potential impact on young people, and its possible threats to public safety (Chiu et al., 2021; Anderson and Rees, 2023). This paper contributes to one of these debated areas by evaluating the road safety impacts of recreational marijuana in the US.

It is well-established that marijuana contains tetrahydrocannabinol (THC), a psychoactive ingredient which impairs cognitive abilities required for driving by reducing visual and motor coordination, slowing reaction time, and distorting multitasking abilities that require split attention (Sewell et al., 2009; Veldstra et al., 2015; Bondallaz et al., 2017). Yet the role of marijuana in fatal traffic crashes remains unclear because of the following key reasons. First, there are no reliable tests for detecting marijuana intoxication at the time of a crash. Delayed blood tests can miss THC's mind-altering effects, while urine tests detect non-psychoactive carboxy-THC, possibly mislabelling sober drivers as

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[1] Source: Wikipedia — By Lokal Profil, CC BY-SA 2.5, https://tinyurl.com/yc5v4efr.

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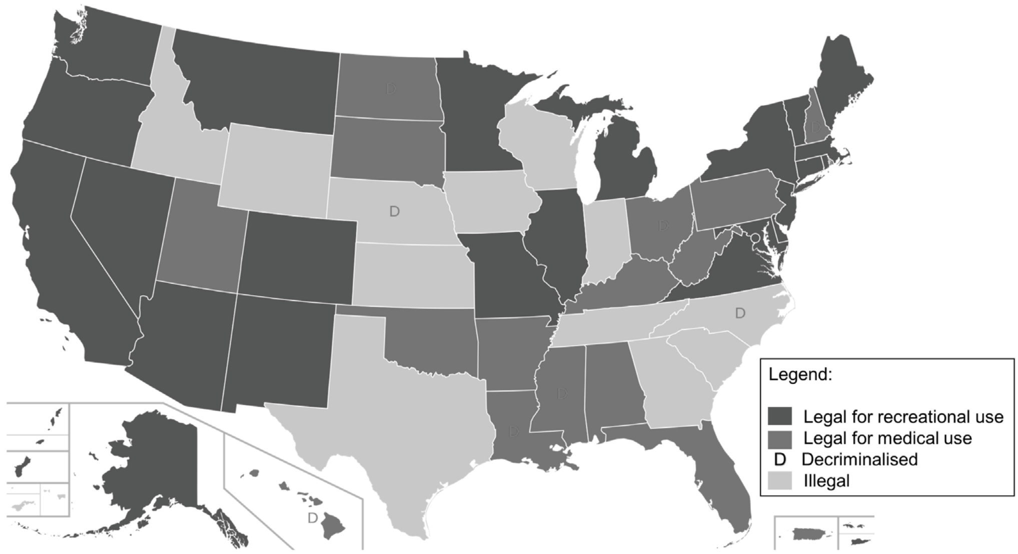


Fig. 1. Legality of recreational marijuana in the United States.

intoxicated (Sewell et al., 2009; Terhune et al., 1992). This undermines epidemiological studies that assess crash risks for drivers under the influence of the drug (Li et al., 2013). Second, the understanding of the driving behaviours of marijuana users is limited. Evidence suggests they may adopt compensatory actions like driving at reduced speeds or steering clear of hazardous manoeuvres, unlike alcohol-impaired drivers who take more risks (Sewell et al., 2009; Kelly et al., 2004; Ronen et al., 2008). However, this evidence is mostly from laboratory studies, not real-world driving. Further, another pool of driving studies shows that marijuana increases body sway, impairs essential driving functions, and reduces control over speed and lane position, leading to more lane weaving and closer vehicle spacing (Smiley et al., 1981; Liguori et al., 1998; Lenné et al., 2010; Preedy, 2016). The reasons stated above have tainted the findings of previous studies, leaving the impact of marijuana on traffic safety ambiguous. Some reviews and meta-analyses find that marijuana use increases crash risk, consistent with driving simulations (National Academies of Sciences, Engineering, and Medicine, 2017; Hartman and Huestis, 2013; Ramaekers et al., 2009; Asbridge et al., 2012; Li et al., 2012; Elvik, 2013; Pearlson et al., 2021). Conversely, studies by the National Highway Traffic Safety Administration and others show insignificant or negative effects (Terhune et al., 1992; Lacey et al., 2016; Hostiuc et al., 2018). Additionally, a recent review noted publication bias towards studies linking marijuana to traffic crashes (Hostiuc et al., 2018).

The literature indicates that the relationship between marijuana and alcohol consumption is central to understanding marijuana's impact on traffic fatalities (Cole, 2018). Alcohol-impaired drivers are at a high risk of fatal crashes (Williams et al., 1985; Martin et al., 2017), and studies have suggested that combining alcohol and marijuana significantly increases impairment (Li et al., 2013; Robbe, 1998; Hartman et al., 2016). If marijuana availability reduces alcohol consumption, traffic fatalities might decrease. Conversely, if marijuana increases alcohol use, fatalities could rise. However, evidence on whether marijuana and alcohol are complements or substitutes is inconclusive. A meta-analysis found support for both complementary and substituting relationships between marijuana and alcohol (Guttmannova et al., 2016). For instance, a study on French accidents showed that half of the drivers who tested positive for marijuana also tested positive for alcohol (Martin et al., 2017). Similarly, other studies based on FARS data from 1991 to 2018 indicated that more drivers tested positive for both substances than for THC alone (Dubois et al., 2015; Lira et al., 2021). These findings suggest a complementary relationship. Conversely, some studies argue that marijuana and alcohol are substitutes (Anderson et al., 2013; Santaella-Tenorio et al., 2017). Building upon this hypothesis, their research on medical marijuana laws in the US found a 10 percent decrease in traffic fatalities in states with such laws. However, another study found statistically insignificant effects of medical marijuana legalisation on traffic fatalities and alcohol-related crashes (Sevigny, 2018). Similarly, a recent study (Brubacher et al., 2022) found a statistically insignificant effect of marijuana legalisation on alcohol-related crashes in British Columbia, Canada. A review of recent studies on marijuana liberalisation laws in the US and Canada concluded that the evidence on the relationship between marijuana and alcohol consumption remains inconclusive (Pacula et al., 2022). Overall, the debate on this theme continues in the research community.

Following the liberalisation of recreational marijuana laws in Washington and Colorado, many studies have examined recreational marijuana's impact on fatal traffic crashes. However, researchers face consistent methodological challenges. Some studies (Lee et al., 2018; Windle et al., 2021; Farmer et al., 2022) explored the association between marijuana laws and traffic fatalities, but their results may be biased by temporal trends and effects unrelated to the change in law. Studies using causal inference methods like differences-in-differences (DID) aim to address this limitation, but their findings vary. A few report insignificant change (Aydelotte et al., 2017; Gunadi, 2022), while others suggest increases in fatalities ranging from 8% to 17% (Cole, 2018; Lane and Hall, 2019; Vogler, 2017; Aydelotte et al., 2019; Kamer et al., 2020; Adhikari et al., 2023). In a similar vein, another study has found a statistically insignificant effect of recreational cannabis dispensary sales on traffic crashes in Colorado counties (Gunadi, 2022). González-Sala et al. (2023) reviewed several recent studies on this theme and suggested a negative effect of the law change on road safety. Interestingly, a recent study suggests that the impact varies over time, with earlier legalising states seeing greater increases (Adhikari et al., 2023). The key idea underlying these causal inference approaches is to understand the impact of recreational marijuana legalisation on traffic fatality rates (the outcome) by comparing the average outcome in states that passed the law with the estimated counterfactual outcomes — what those outcomes would have been had the law not been enacted. In the DID approach, the counterfactual outcomes are constructed by considering the average changes in traffic fatality rates in states that did not pass the law. Crucially, the DID approach relies on the assumption that, in the absence of the law change, the changes in traffic fatality rates would have been similar in both the states with and without the law. Nevertheless, some studies rightly argue that DID estimates may be biased in this context due to state-level reporting differences, regional consumption preferences, and spillover effects from legalisation efforts, complicating the identification of a true counterfactual trend for the states passing the law (Hansen et al., 2017; Romano et al., 2017).

To address these methodological challenges, we estimate the causal impact of recreational marijuana legalisation on traffic fatalities using an augmented synthetic control method (ASCM) proposed by Ben-Michael et al. (2021). The method allows us to construct the counterfactual by assigning weights for states that have not legalised marijuana to match the pre-legalisation trends of traffic fatalities. The closest precedents to our analysis, Hansen et al. (2020) and Santaella-Tenorio et al. (2020), use the original synthetic control method (SCM) to evaluate the impact of recreational marijuana legalisation on traffic fatalities in Washington and Colorado. They consider data until 2016 and 2017, respectively. While the former study found a statistically insignificant effect in both states, the latter observed a statistically significant increase in Colorado but an insignificant impact in Washington. These differing results suggest that (i) the effect of the law may vary across states due to contextual differences, such as variations in population, travel behaviour and traffic enforcement, and (ii) the effect may change over time. Nevertheless, we note that these analyses are limited to a couple of states and only cover a few years post-legalisation, where attitudes towards marijuana use and associated driving preferences may be highly transitory. Moreover, their studies do not test for the year-on-year variation in the statistical significance of the estimated effect of the law. We argue that the process of interest is dynamic, evolving over time, and it is therefore of vital importance to capture the temporal phasing of the underlying relationship. Furthermore, there is a noticeable imbalance between the outcome trajectories of the states with the law and the synthetic control counterparts in their studies, which, as suggested by Abadie et al. (2015), may undermine the validity of results from the SCM. This imbalance can lead to biased estimates and unreliable conclusions.

Our study makes critical contributions to the growing literature on the causal impact of recreational marijuana on traffic fatalities by:

1. Providing novel estimates of the heterogeneity in the effect of recreational marijuana legalisation across different states where the law change was introduced before 2019, which includes new individualised insights from states such as Alaska, California, Oregon, Maine, Massachusetts, Nevada and Michigan.
2. Quantifying how the effect of the legalisation and its statistical significance varies over the years until 2019 in these states.
3. Delivering new insights on whether marijuana and alcohol are substitutes or complements in consumption.
4. Finally, yet importantly, developing a rigorous study design to understand the link between recreational marijuana and traffic fatalities, which can be used for further research.

Our study uses data on state-wise annualised traffic fatality rates over the period 1994–2019 as reported in the Fatality Analysis Reporting System (FARS). It is worth noting that marijuana and alcohol-related fatalities are explicitly recorded in the FARS data. However, as pointed out by previous studies (Cole, 2018; Hansen et al., 2020), such data may suffer from a sampling error because there may be cases where individuals involved in traffic accidents are not tested for drugs or alcohol. We, therefore, do not consider marijuana and alcohol-related fatalities but rather focus on total traffic fatalities and (weekend) night-time fatalities. Data on alcohol-related fatalities is used to validate the estimated marijuana-alcohol consumption relationship.

2. Model and data

We aim to study the causal impact of recreational marijuana legalisation on fatal traffic crashes using an augmented synthetic control method (SCM). The data we have collated for estimation is for forty-eight US states; thirty-nine states without liberalised recreational marijuana laws, equivalently, the control states, and nine states with the law, that is, the treated states; for the period 1994–2019. The treated states include Washington, Colorado, Alaska, Oregon, California, Maine, Massachusetts, Nevada, and Michigan. We exclude Hawaii from the control group due to its unique archipelagic topography, which limits the extent and type of its road network (Hawaii has very few highways and major roads) compared to other US states. Moreover, the District of Columbia (DC) and Vermont are omitted from the treated group, for which we were unable to obtain ideal synthetic matches. This is due to DC's unique size and the lack of rural roads. For Vermont, a substantially higher proportion of rural roads and lower traffic volumes results in disproportionately lower traffic fatality rates compared to the rest of the US.

It is worth emphasising that we restrict our study period to 2019 because using traffic safety data from both pre-2019 and post-2019 periods within the same SCM model can be challenging due to significant disruptions and changes in travel behaviour and traffic patterns caused by the COVID-19 pandemic. Conducting separate analyses for pre-2019 and post-2019 periods, where covariates such as lockdown measures and traffic volume changes are included in the post-2019 model, may appear as a solution. However, note that SCM inherently assumes that the covariates used to estimate the synthetic control weights remain stable and do not change substantially between pre-intervention and post-intervention periods. This stability is crucial for ensuring that the synthetic control accurately represents what the treated unit would have experienced in the absence of the intervention. Pertinent to the COVID-19 pandemic, given the significant disruptions and rapidly evolving changes in travel behaviour, particularly in the first two years, the assumption of covariate stability is likely violated. The pandemic caused dramatic changes in many factors that could affect the outcomes of interest, such as traffic patterns, enforcement practices, and general travel behaviour. This instability complicates the use of SCM in a straightforward manner when using post-2019 data.

2.1. Data and relevant variables

The three main categories of variables relevant to this analysis are traffic safety outcomes, treatment status, and state-wise baseline characteristics (or auxiliary covariates). The data used to represent variables in each category are as follows.

2.1.1. Traffic safety outcomes

We consider three traffic safety outcomes: (i) log-transformed total traffic fatalities per 100,000 population, (ii) log-transformed night-time (0:00 - 3:00 h) traffic fatalities per 100,000 population, and (iii) log-transformed weekend night-time traffic fatalities per 100,000 population.

The traffic fatalities data are obtained from the accident files of the Fatality Analysis Reporting System (FARS),[2] a public repository maintained by the US Department for Transportation. The accident file contains a record of every accident that resulted in at least one fatality. The data entries corresponding to each record include the total number of fatalities, the time of the crash, whether any drivers tested positive for alcohol and additional indicators like road type and weather conditions. We translate the data into unique state-years via aggregating relevant crash statistics. Note that while it is possible to use higher granularity time units (such as monthly or quarterly) with the available data, we focus on annualised crash statistics for two key reasons. Firstly, the weight of evidence in the literature (refer to Section 1) corresponds to changes in annualised traffic safety performance indicators. Using a similar time unit of aggregation ensures the comparability of our findings with previous studies. Secondly, and more critically, monthly or quarterly traffic safety records often exhibit strong seasonal variations

[2] Available at https://tinyurl.com/sf3754s3.

due to weather, holidays, and school schedules, introducing noise that can complicate the analysis. Additionally, such data might be disproportionately affected by reporting errors, inconsistencies, or anomalous events like extreme weather. Using annual data alleviates these issues by reducing the impact of noise and outliers, thereby providing a more stable and reliable foundation for long-term trend analysis and policy evaluation. While weather normalisation techniques, such as those proposed by Grange and Carslaw (2019), can help mitigate the influence of extraneous factors, these methods are data-intensive and require millions of data points for precise implementation. The data in hand, owing to its smaller size, limits the applicability of such methods in our analysis.

Data on annual state-wise population is obtained from the United States Census Bureau website.[3]

One of the key objectives of this study is to deliver an understanding of whether marijuana and alcohol are supplements or complements in consumption. The FARS data reports whether one or more drivers involved in any reported incident were under the influence of alcohol or not. However, as mentioned in the Introduction, we do not rely on this data as our primary source of information as there may be sampling bias in the data across states and years, driven by the non-requirement for the responding officers to administer a breath analyser. Instead, in line with previous studies (Cole, 2018; Dee, 1999; Ruhm, 1996; Dee, 2001; Fell and Nash, 1989), we consider two proxy variables for drunk driving: (i) night-time (midnight to 3:00 h) traffic fatalities per 100,000 population, and (ii) weekend night-time traffic fatalities per 100,000 population. The proxy is reasonable as the proportion of drunk drivers at night is substantially larger compared to the rest of the day, with weekend nights being the most likely time for drivers to be drunk (Dee, 1999; Ruhm, 1996; Portman et al., 2013), while the proportion of drivers under the influence of marijuana tends to remain constant from day to night (Berning et al., 2015). However, we note that the definition of night-time varies across studies. For instance, Dee (1999) and Cole (2018) use midnight to 4:59 am, Ruhm (1996) uses midnight to 3:59 am, Dee (2001) uses 6 pm to 5:59 am, and Fell and Nash (1989) uses 8 pm to 3:59 am. We adopt a definition of night-time from midnight to 3:00 am, which intersects all these definitions while also representing the typical range of bar closing (last call) times across US states. Additionally, we validate the insights provided by these proxies using the FARS data on alcohol-related fatalities.

2.1.2. Treatment status

We classify US states into control and treatment groups based on whether the state enacted recreational marijuana legalisation at any point during the study period (1994–2019). Accordingly, for each treated state, the treatment year corresponds to the *effective enactment year*, that is, the year in which the law came into effect and legal possession or use was permitted, rather than the date of ballot passage or legislative approval. To ensure accuracy and transparency, policy dates were initially compiled using publicly available sources and then cross-validated using multiple authoritative datasets, including the Prescription Drug Abuse Policy System (PDAPS) dataset on Recreational Marijuana Laws[4] and the National Conference of State Legislatures (NCSL) State Cannabis Legislation Database.[5] Table 1 summarises, for each treated state, the year the law was enacted (effective date), the year retail sales commenced, and the corresponding pre- and post-treatment periods used in our analysis.

2.1.3. Auxiliary covariates

To aid the matching of pre-treatment trends in the traffic safety outcomes of the treated unit and its synthetic control, we consider several auxiliary covariates that could be relevant to the traffic safety outcomes of each state. Following previous studies (such as Cole, 2018; Lee et al., 2018; Smart and Doremus, 2023; Fowles and Loeb, 2021; Das et al., 2021; Cook et al., 2020), we gather state-wise data on annual vehicle miles travelled (VMT), rural percentage of VMT, length of rural and urban roads, speed limits (rural and urban), and number of licenced drivers per population (aged 14 and above) from the Highway Statistic series published by the Federal Highway Administration,[6] employment data published by the US Bureau of Labour Statistics,[7] real GDP published by the US Bureau of Economic Analysis,[8] and average annual temperature and precipitation published by the National Centers for Environmental Information.[9] We consider the logarithms of these covariates within our model.

It is worth noting that a key principle underlying covariate selection for causal inference models is that covariates should remain unaffected by the intervention. This is because conditioning on covariates that change in response to the intervention can obscure important causal pathways through which the intervention can affect the outcome, leading to biased estimates of the impact of the intervention. This issue, extensively discussed in previous studies such as Wooldridge (2005), has been referred to in the recent causal inference literature as *collider bias* (Munafò et al., 2018; Holmberg and Andersen, 2022; Tönnies et al., 2022). Accordingly, we exclude covariates related to impaired driving enforcement (for instance, number of patrol officers, number of DUI arrests, or marijuana or alcohol testing rates as in Hansen (2015)) from our model. Although these factors might initially seem to be confounders, they are actually colliders, as traffic enforcement levels seem to have increased across the US, although not uniformly, in response to the legalisation of recreational marijuana (see, for instance, Wiens et al., 2018). A recent review (Hasan et al., 2022) on the effectiveness of various enforcement approaches to drug driving across several jurisdictions around the world also suggests that changes in drug-related laws can influence the number of roadside enforcement activities. Including these covariates could, therefore, introduce bias by blocking important causal pathways.

2.2. Statistical analysis

2.2.1. The synthetic control method

Our analysis uses the potential outcomes framework for causal inference (Rubin, 1976). We have longitudinal data on N states indexed with $i = 1, \ldots, N$, each of which has T_i observations made over years t, $t = 1, \ldots, T_i$, giving aggregate of $n = \sum_{(i=1)}^{N} T_i$ samples. Data available for estimation is treated as realisations of a random vector, $\Psi_{it} = (Y_{it}, W_{it}, Z_{it})$, where, Y_{it} denotes the traffic safety outcome of interest, for instance, log-transformed fatal traffic crashes per 100,000 population, in state i and year t. W_{it} signifies the treatment status and Z_{it} a vector of covariates. As discussed before, the treatment corresponds to the enactment of the recreational marijuana legalisation law. W_{it} defined in binary form.

The key quantity of interest in our calculations is the average treatment effect (ATE, denoted by τ), or in other words, the difference in outcome that would occur under treatment status (with the law) $(W = 1)$ relative to control status (without the law) $(W = 0)$.

$$\tau_{it} = E[Y_{it}(1) - Y_{it}(0)],$$

where $Y_{it}(1)$ and $Y_{it}(0)$ are the outcomes for state i under treatment and control status respectively. Based on the above equation, the causal

[3] Available at https://tinyurl.com/s8mum77j.
[4] Available at https://tinyurl.com/544bws5p.
[5] Available at https://tinyurl.com/prbavdk3.
[6] Available at https://tinyurl.com/yfezptbp.
[7] Available at https://tinyurl.com/2p9h7ce9.
[8] Available at https://tinyurl.com/y6s4usev.
[9] Available at https://tinyurl.com/55xk7e4b.

Table 1
Treated states and the intervention years.

Treated state	Year law enacted (effective date)	Year retail sales began	Pre-treatment period	Post-intervention period
Washington	2012	2014	1994–2011	2012–2019
Colorado	2012	2014	1994–2011	2012–2019
Alaska	2015	2016	1994–2014	2015–2019
Oregon	2015	2015	1994–2014	2015–2019
California	2016	2018	1994–2015	2016–2019
Massachusetts	2016	2018	1994–2015	2016–2019
Maine	2017	2020	1994–2016	2017–2019
Nevada	2017	2017	1994–2016	2017–2019
Michigan	2018	2019	1994–2017	2018–2019

estimate of the impact of the treatment for state i can be obtained by comparing the average outcome in treated units and the average counterfactual outcome in those units if untreated. The fundamental challenge herein is that the counterfactual outcomes for treated units remain unobserved. Nevertheless, the potential outcomes approach suggests that causal effects can still be validly identified if the focus remains on estimating average causal effects (for a conceptual review, refer to Graham, 2022). This can be achieved by comparing the mean outcomes across treated and potential control units, netting out any confounding influences that create differences between the mean outcomes of the treated and control units beyond those from the treatment. However, in the context of our study, achieving explicit adjustment for all confounding factors may be challenging. This is because mean outcomes in treated and control states could differ due to unobserved factors, such as variations in driver behaviour across populations and differences in state-wise traffic fatality reporting.

The synthetic control method (SCM), originally pioneered by Abadie and Gardeazabal (2003), offers a means to estimate the missing average counterfactual outcomes, $E[Y_{it}(0)]$, by constructing a synthetic unit that is designed to closely match the averages and trajectories of key variables observed in each treated unit. By design, the SCM does not rely on explicit adjustment for all confounding factors, making it particularly useful when several confounders remain unobserved. SCM has been applied to investigate various policy interventions, particularly in the fields of labour, development, and health economics (see, for instance, Cavallo et al., 2013; Kreif et al., 2016; Johnston and Mas, 2018).

In our study, we seek to assess the impact of recreational marijuana legalisation (the treatment) on traffic safety outcomes in various treated states. Ideally, for each treated state, we would find a state in the US that did not undergo legalisation but closely matches the treated state in various aspects, such as traffic safety outcomes, traffic infrastructure, and macroeconomic indicators, among others. However, in reality, it is unlikely to find an exact match for each treated state. To address this challenge, we adopt the SCM, a data-driven method that calculates a weighted average of potential control states to construct a synthetic or an artificial version of each treated state. The goal of this synthetic unit is to replicate the trajectory of traffic safety outcomes in the actual treated state before the intervention (that is, the legalisation of recreational marijuana). By comparing the trajectories of the synthetic and real treated state after the intervention, we can determine the causal impact of the intervention on the outcome of interest. Essentially, the synthetic unit serves as a counterfactual representation of how traffic fatalities in the treated state would have evolved if it had not been subjected to the intervention (Abadie et al., 2015; Athey and Imbens, 2017).

More specifically, consider a pool of J potential control states that did not receive the treatment. The SCM estimates the causal impact of the treatment for treated state i at time t as:

$$\tau_{it} = Y_{it} - \sum_j \gamma_j Y_{jt}$$

where γ_j represents the estimated weight for control unit j. The weights γ_j are chosen to minimise the distance between the treated unit and a weighted average of the control units in terms of pre-treatment characteristics. Specifically, the SCM selects weights such that the weighted combination of control units most closely replicates the treated unit's trajectory during the pre-treatment period. This is achieved by solving the following optimisation problem:

$$\min_{\gamma} \sum_{t\in \mathrm{Pre}} \left(Y_{it} - \sum_j \gamma_j Y_{jt} \right)^2 + \lambda \cdot \mathrm{Penalty}(\gamma)$$

where the first term represents the mean squared prediction error between the treated unit and its synthetic control over the pre-treatment period, and the second term is a regularisation penalty (for instance, ridge penalty, as used in ASCM). The minimisation is typically subject to the constraints $\sum_j \gamma_j = 1$ and, in the original SCM, $\gamma_j \geq 0$ for all j. However, the ASCM used in our study relaxes the non-negativity constraint to allow for negative weights, improving the fit when exact matches are difficult to obtain (see Appendix A for details). In our implementation, the distance is computed using lagged values of the outcome variable (traffic fatalities) and a subset of auxiliary variables described in Section 2.1.3, ensuring that the synthetic unit closely mimics the treated state's pre-treatment trends.

Adopting the SCM approach offers several advantages. Firstly, there is no need for extrapolation, and the synthetic weights are determined without using post-intervention data, which eliminates the risk of cherry-picking or manipulating specifications. Additionally, the explicit presentation of each control unit's contribution to the overall synthetic unit enhances transparency, enabling experts to validate the weights using their knowledge (Abadie, 2021). However, as cautioned by Abadie et al. (2015), the SCM may not yield meaningful estimates if the outcome trajectory of the synthetic unit does not closely align with the outcome trajectory of the treatment unit before the intervention. In such cases, the reliability of the results may be compromised.

Ben-Michael et al. (2021) present a potential solution to address concerns regarding outcome trajectories through an augmented synthetic control method (ASCM). The ASCM is an extension of the original SCM designed to handle situations where obtaining a suitable pre-intervention match between the treatment and synthetic unit is challenging. In such cases, the ASCM employs an outcome model to estimate the bias resulting from the mismatch and subsequently corrects the original SCM estimate for this bias. The approach proposed by Ben-Michael et al. (2021) involves using a ridge-regularised linear regression model, which relaxes the non-negative weights constraint of the original SCM. This allows for incorporating negative weights within the Ridge ASCM, offering more flexibility in the selection and assignment of weights to control units.

Other notable extensions to the original SCM include the demeaned or intercept shift SCM (Doudchenko and Imbens, 2016), the synthetic DID (Arkhangelsky et al., 2021), the generalised SCM (Xu, 2017), the matrix completion method (Athey et al., 2021), the micro SCM (Robbins and Davenport, 2021) and the Bayesian SCM (Kim et al., 2020). The first two approaches, the demeaned SCM and the synthetic DID focus on balancing the outcomes after removing unit-specific and both unit-specific and time-specific effects, respectively, thus focusing on the deviations from these averages. In contrast, the adopted approach, the ASCM, balances the raw outcomes, thus providing a better match to the pre-treatment outcome data by design. The next two approaches, the generalised SCM and the matrix completion method focus on outcome modelling rather than SCM-style weighting. On the contrary, the ASCM begins with the original SCM estimate, uses an outcome model to estimate the bias due to imperfect pre-treatment fit, and then uses this to de-bias the SCM estimate. This makes them analogous to standard doubly robust estimators which are more robust to model misspecification and better suited for small sample (small N and small T) settings similar to this study. The micro SCM offers extensions for granular, individual-level data analysis, rather than aggregated groups (for instance, states). Finally, the Bayesian SCM extends the original SCM with Bayesian methods to allow for probabilistic inferences and a more nuanced understanding of the uncertainty surrounding the estimates. The extension, however, relies on the availability of prior information on the distribution of weights. By contrast, the ASCM does not require the specification of prior distributions, which can be subjective and challenging to determine. It relies purely on the observed data, avoiding potential biases introduced by incorrect priors. Technical details of the ASCM are attached in Appendix A.

The analysis is conducted in R using the `augsynth` package.

2.2.2. Model validation

To ensure that the estimated ASCM weights do not overfit to noise, we apply extensive in-time placebo checks as recommended in Abadie et al. (2015) and adopted by Ben-Michael et al. (2021). These in-time placebo tests validate the estimated ATE by applying the ASCM to pre-treatment periods assuming the treatment occurred earlier. The process involves constructing synthetic controls for these placebo periods and comparing the estimated placebo effects to the actual ATE. If statistically insignificant effects are observed in the placebo periods, it suggests the estimated ATE is credible; significant effects indicate potential model issues or spurious results.

2.2.3. Model selection and assessment of model fit

Further, to assess the quality of the ASCM fit, we use a normalised L^2 imbalance score, referred to as the fit index. Here, L^2 imbalance signifies the Euclidean distance between the pre-treatment outcome vectors of the treated and its synthetic counterparts.

$$L^2 \text{ imbalance} = \sqrt{\sum_{t=1}^{T_0} (Y_{treated,t} - Y_{synthetic,t})},$$

where $t \in [1, T_0]$ represents the pre-treatment period. This measure is further normalised by the norm of a zero fit model to obtain the fit index.

$$\text{Fit index} = \frac{\sqrt{\sum_{t=1}^{T_0} (Y_{treated,t} - Y_{synthetic,t})^2}}{\sqrt{\sum_{t=1}^{T_0} (Y_{treated,t})^2}} \times 100\%.$$

The above index intuitively evaluates the overall quality of the fit by calculating the percentage deviation of the predicted synthetic outcomes from the actual outcomes during the pre-treatment period. Following Adhikari and Alm (2016), we use a 5% threshold for the fit index, indicating a 95% match between the synthetic and actual paths of the outcome variable in the pre-treatment period.

We also use the L^2 imbalance score as the criterion for the inclusion of auxiliary covariates into our ASCM models. For each treated unit, we start with a baseline ASCM model that comprises an outcome model with only pre-treatment outcomes as predictors (see Equation 4 attached in Appendix A). We record the deviation between the pre-treatment outcomes of the treated and synthetic units using the L^2 imbalance score discussed. Next, we introduce various combinations of auxiliary covariates alongside the pre-treatment outcomes into the outcome model underlying the ASCM, re-generate the synthetic units using Equation 6 attached in Appendix A, and record the L^2 imbalance scores. The final model is chosen based on two criteria: (i) it must minimise the L^2 imbalance score without leading to overfitting, which we check using in-time placebo tests in Appendix F, and (ii) the estimated ATE curve must not differ substantially from the baseline ASCM model, ensuring our estimates are not sensitive to the choice of covariates. If these criteria are not met, we retain the baseline ASCM model estimates.

It is important to clarify that the inclusion of auxiliary covariates is not required for estimation in ASCM. The method can validly proceed using only lagged outcome variables in the outcome model. In fact, as noted by Ben-Michael et al. (2021), this approach is often preferred when covariates do not improve the pre-treatment match or introduce instability. Our key motivation behind using this iterative process is to improve the precision of our ATE estimates while also avoiding the issue of cherry-picking for specification in SCM models highlighted by Ferman et al. (2020). Technical details on the formulation and estimation of the ASCM model; with and without covariates; are provided in Appendix A.

3. Results

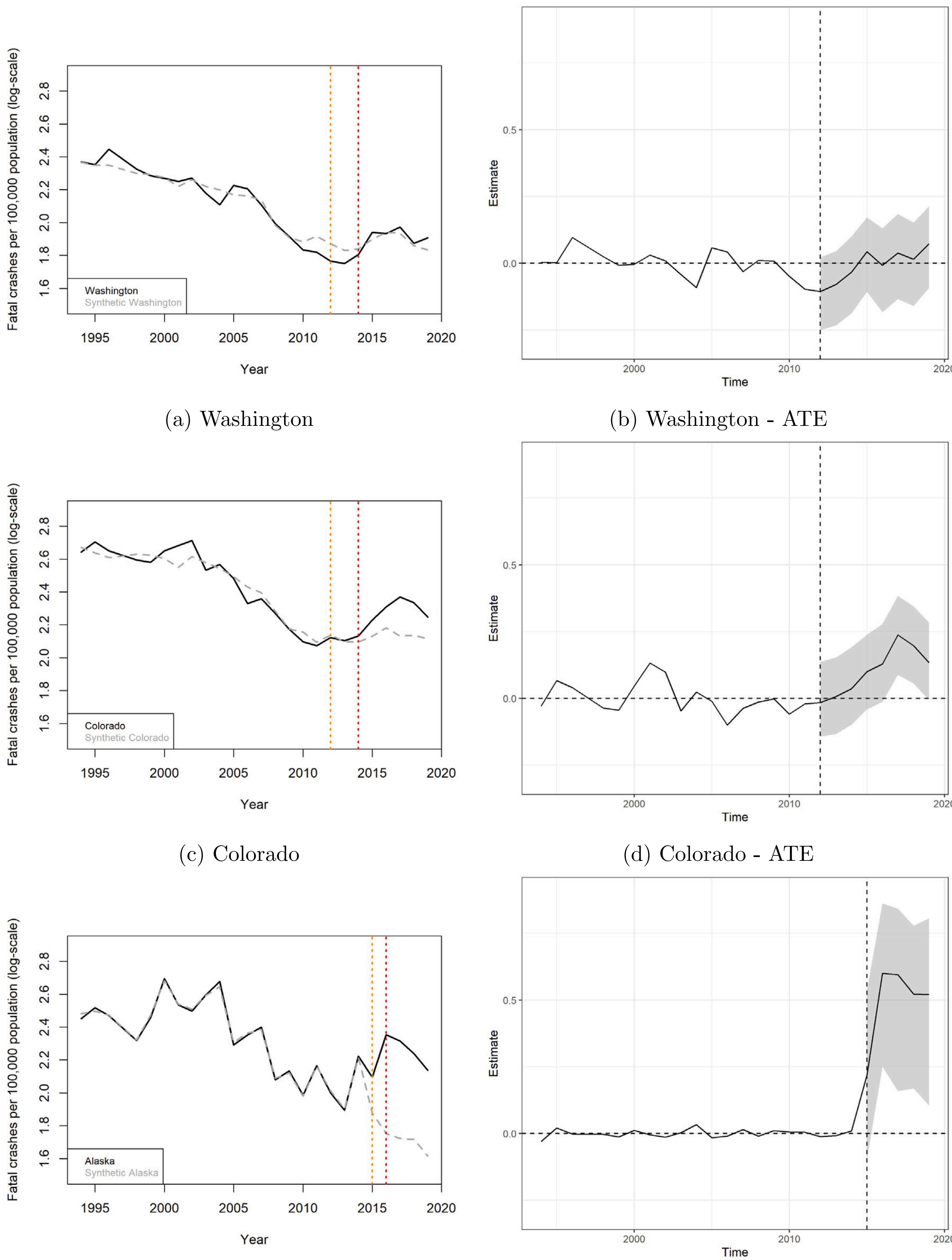
We first discuss our estimates of the year-on-year impact of recreational marijuana legalisation on traffic fatalities in nine states with the law, or in other words, the treated states. Thereafter, we present our results on the marijuana-alcohol consumption relationship. These results are supplemented with appendices F to I that present a series of in-time placebo tests to validate our results. For these tests, we consider two scenarios. In the first scenario, we assume that the legalisation law was enacted in the treated states four years earlier than the actual year of the intervention, and in the second scenario, we pre-pone the intervention by three years.

3.1. Evolution of the causal effect of the law change on total traffic fatalities

We use our ASCM models to infer the impact of recreational marijuana on our primary traffic safety outcome of interest, traffic fatalities per 100,000 population, in the nine treated states: Washington (WA), Colorado (CO), Alaska (AK), Oregon (OR), California (CA), Maine (ME), Massachusetts (MA), Nevada (NV), and Michigan (MI).

Fig. 2 (left) illustrates the evolution of log-transformed traffic fatalities per 100,000 population in the nine treated states and in their respective synthetic control units. The figure (right) also shows the corresponding estimates of the average treatment effect, ATE, (in log points) in each year, that is, the estimated year-on-year change in the log-transformed traffic fatalities per 100,000 population due to the intervention. The corresponding 95-percent confidence intervals are given by the shaded grey area in the figure. The orange and red vertical dotted lines mark the year of enactment of the recreational marijuana legalisation law (as in Table 1) and the year of the beginning of retail sales in the state, respectively. Consistent with the discussion in Section 2.1.2, the former line represents the treatment (intervention) year used to develop the synthetic control units, whereas the latter line serves merely as an illustration. Table 2 tabulates the yearly estimates of the ATE in percentage points. Further, the auxiliary covariates and the estimated weights for the generation of the synthetic control units are summarised in Table 3 and Appendix B, respectively. The set of auxiliary covariates relevant to each synthetic unit, as shown in Table 3, is identified through an iterative process described in Section 2.2.3.

Fig. 2(b) suggests that the enactment of the recreational marijuana legalisation law had a statistically insignificant impact on traffic fatalities in Washington in the entire post-treatment period under study, that is,2012–2019. Overall, the ATE curve shows an increasing trend, but the values remain statistically insignificant until the end of our study period, that is, 2019. The obtained fit index (discussed in Section 2.2.2, threshold for acceptance = 5%) of 2.2%, see Table 3, and the statistically insignificant placebo effects (see Figures F.1a and F.2a) support the credibility of the estimated ATE. These findings align with Hansen et al. (2020) and Santaella-Tenorio et al. (2020), who also reported a statistically insignificant impact of recreational marijuana

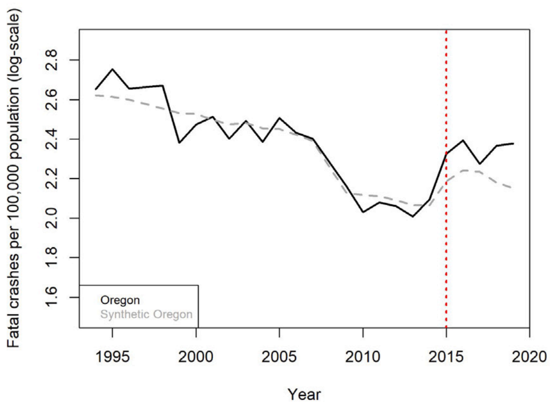


(a) Washington (b) Washington - ATE

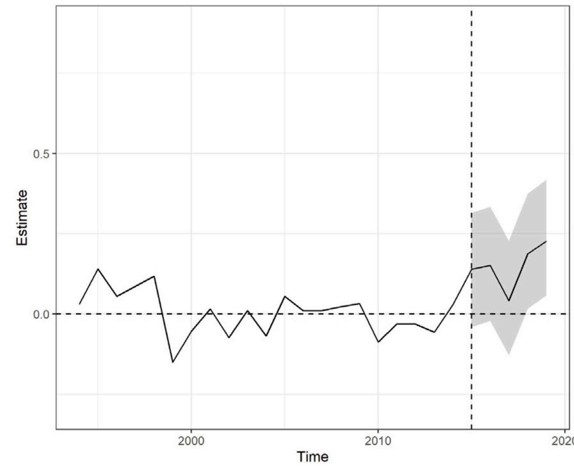
(c) Colorado (d) Colorado - ATE

(e) Alaska (f) Alaska - ATE

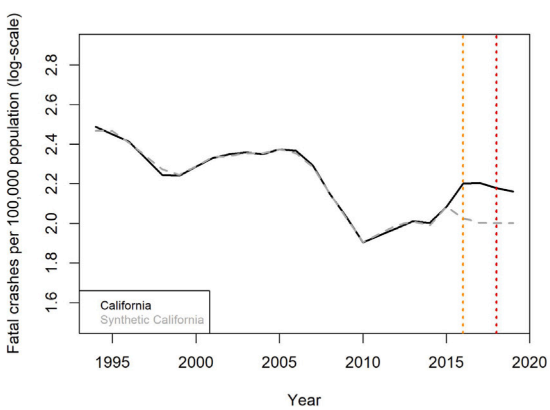
Fig. 2. Figures on the left present a comparison of traffic fatalities per 100,000 population (in log-scale) in different states with their synthetic counterparts. The orange line indicates the year of intervention, that is, the enactment of the recreational marijuana legalisation law and the red line indicates the year when retail sales of recreational marijuana began. Figures on the right show the corresponding estimates of the average treatment effect. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)



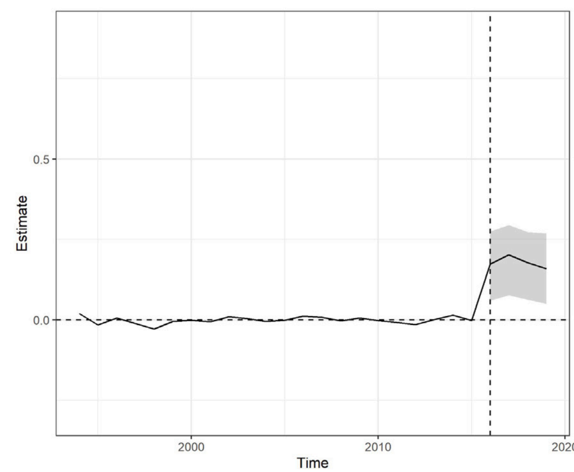
(g) Oregon



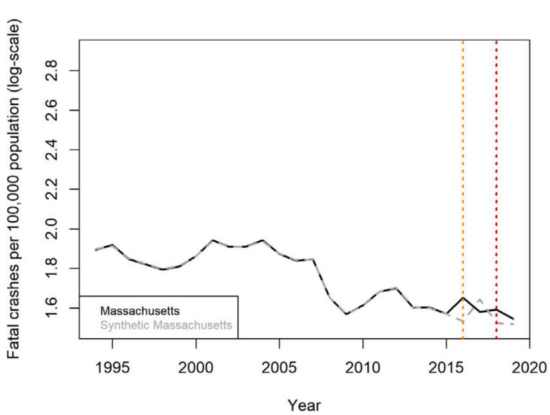
(h) Oregon - ATE



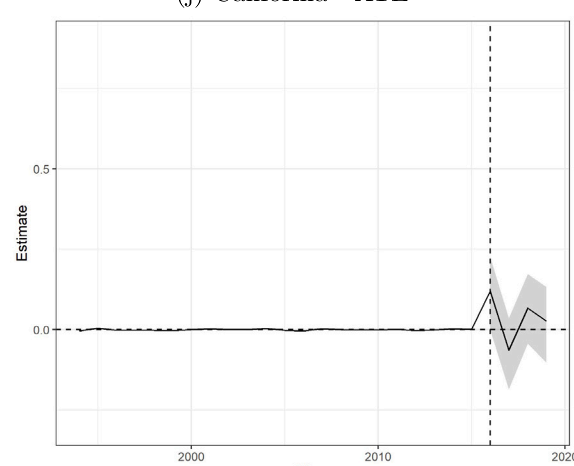
(i) California



(j) California - ATE

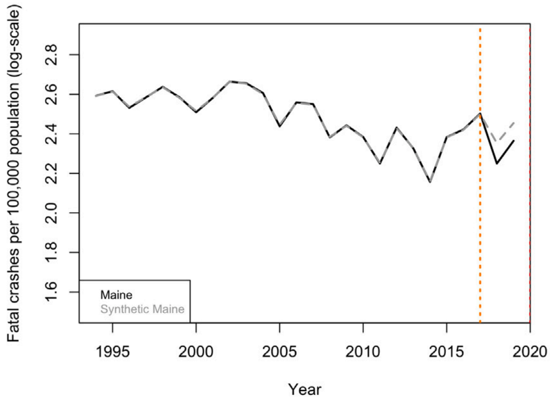


(k) Massachusetts

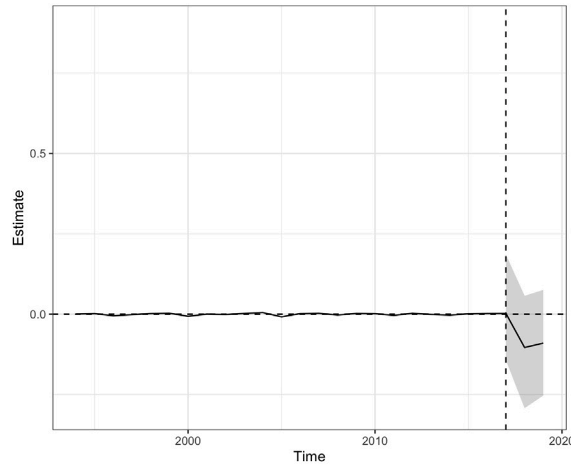


(l) Massachusetts - ATE

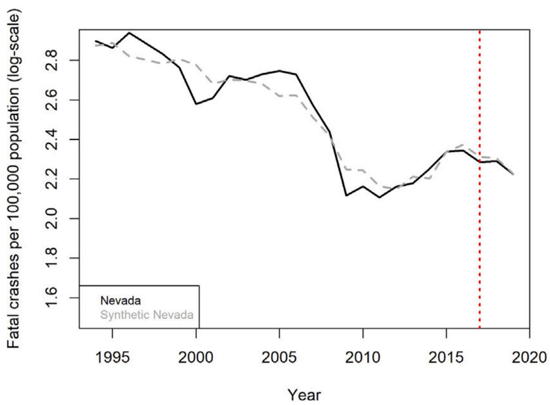
Fig. 2. (*continued*).



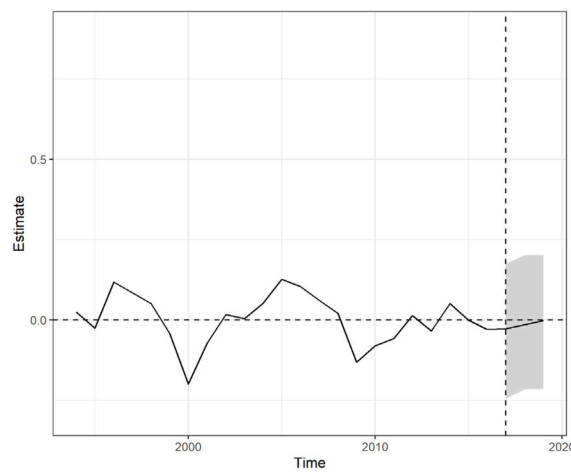
(m) Maine



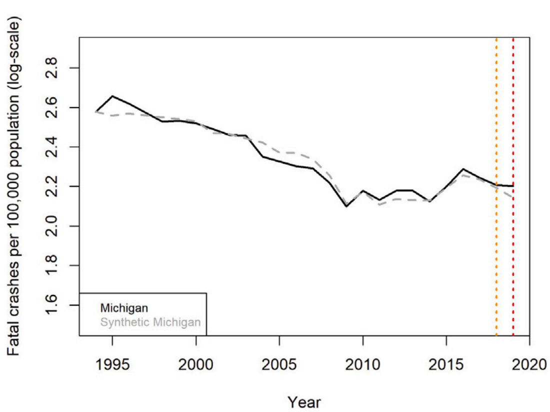
(n) Maine - ATE



(o) Nevada



(p) Nevada - ATE



(q) Michigan

(r) Michigan - ATE

Fig. 2. (*continued*).

legalisation on traffic fatalities in Washington for the post-treatment periods 2012–2016 and 2012–2017, respectively.

For Colorado (see Fig. 2)(d), the estimated impact on traffic fatalities remains statistically insignificant for the first five years post-intervention (2012–2016). However, the ATE estimates become statistically significant in 2017 and 2018 before turning insignificant again in 2019. Notably, while the aggregated ATE in the pre-retail sales period (2012–2013) is statistically insignificant (estimate = −0.44, 95% confidence intervals (CI) = [-15.16, 14.27]), the period following the commencement of retail sales (2014–2019) shows a statistically significant aggregated ATE estimate of +15.16 (95% CI = [0.15, 30.16]) at the 95% confidence level. The aggregated post-retail ATE estimate implies an average increase of 15 percent in traffic fatality rates in the years 2014 to 2019 due to the law change. Similar to Washington, the

Table 2
The estimated average treatment effect in percentage points.

State	2012	2013	2014	2015	2016	2017	2018	2019	Average ATE	Pre-retail ATE	Post-retail ATE
WA	−10.06	−7.69	−3.25	4.39	−0.80	3.87	1.51	7.57	−0.55	−8.87	2.22
	[−24.13,4.02]	[−22.08,6.71]	[−18.18,11.68]	[−10.05,18.84]	[−17.13,15.54]	[−12.73,20.48]	[−14.71,17.74]	[−8.33,23.47]	[−15.95,14.84]	[−23.11,5.36]	[−13.54,17.98]
CO	−1.59	0.70	3.67	10.52	13.77	26.87	21.90	14.22	11.26	−0.44	15.16
	[−16.14,12.97]	[−14.17,15.58]	[−11.37,18.7]	[−4.04,25.07]	[−1.32,28.86]	[11.56,42.18]	[6.97,36.83]	[−0.87,29.32]	[−3.68,26.19]	[−15.16,14.27]	[0.15,30.16]
AK				24.48	82.21	81.30	68.54	68.37	64.98	24.48	75.11
				[−9.75,58.72]	[49.15,115.28]	[44.06,118.54]	[35.59,101.49]	[30.06,106.68]	[29.75,100.21]	[−9.75,58.72]	[39.63,110.58]
OR				15.03	16.30	4.19	20.56	25.48	16.31		16.31
				[−3.59,33.64]	[−2.32,34.92]	[−14.38,22.75]	[1.84,39.29]	[6.59,44.37]	[−2.37,34.99]		[−2.37,34.99]
CA					19.01	22.38	19.36	17.35	19.53	20.70	18.36
					[7.96,30.05]	[11.13,33.64]	[8.58,30.14]	[6.15,28.55]	[8.45,30.6]	[9.54,31.85]	[7.36,29.35]
MA					12.64	−6.20	6.93	2.63	4.00	3.22	4.78
					[0.96,24.32]	[−17.56,5.16]	[−4.17,18.03]	[−9.52,14.79]	[−7.58,15.58]	[−8.30,14.74]	[−6.86,16.42]
ME						0.30	−9.79	−8.61	−6.03	−6.03	
						[−16.84,17.45]	[−28.02,8.45]	[−25.75,8.54]	[−21.20,9.14]	[21.20,9.14]	
NE						−2.66	−1.49	−0.20	−1.45		−1.45
						[−24.75,19.42]	[−23.46,20.49]	[−22.12,21.72]	[−23.45,20.54]		[−23.45,20.54]
MI							1.41	6.08	3.74	1.41	6.08
							[−9.27,12.08]	[−4.60,16.75]	[−6.93,14.42]	[−9.27,12.08]	[−4.6,16.75]
All									13.40	6.18	17.07
									[−5.57,32.37]	[−12.07,24.44]	[−1.70,35.84]

*Figures in square brackets denote the 95 percent confidence intervals (CI).

Table 3
Auxiliary variables for the development of synthetic control units for the analysis of total fatalities.

Covariates	WA	CO	AK	OR	CA	ME	MA	NE	MI
Real GDP	*	*			*		*	*	
Population	*	*	*			*	*	*	
Area		*	*					*	
No. of employees	*	*	*		*	*	*	*	
Length of all roads					*	*	*	*	
Length of rural roads			*		*	*	*	*	
licenced driver per unit population					*				
Total vehicle miles travelled	*	*			*	*	*	*	
L^2 imbalance	0.20	0.24	0.06	0.30	0.05	0.02	0.01	0.36	0.19
Fit Index	2.20	2.35	0.61	2.85	0.48	0.14	0.14	3.02	1.65

ATE curve for Colorado shows an increasing trend until 2017, after which it declines, although the rise is steeper for Colorado. With a fit index of 2.35% (see Table 3) and statistically insignificant placebo effects (see Figures F.1b and F.2b), our estimates are credible. Our findings align with Hansen et al. (2020), who found a statistically insignificant impact from marijuana legalisation on traffic fatalities in Colorado during 2012–2016, and with Santaella-Tenorio et al. (2020), who found a significant impact when extending the period to 2017. Nevertheless, unlike these studies, we do not focus on the statistical significance of ATE estimates averaged over the entire post-treatment period. Instead, we examine the variation in ATE estimates and their significance across the post-treatment years to better understand the long-term effects of the law change.

Similar to Colorado, we observe a statistically insignificant effect of the law in Alaska (Fig. 2(f)) in the pre-retail sales period, that is, 2015 (aggregated ATE estimate = +24.48, 95% CI = [−9.75,58.72]), and a statistically significant effect during the study period when the drug is commercially available, that is,2016–2019 (aggregated ATE estimate = +75.11, 95% CI = [39.63,110.58]). The aggregated post-retail ATE estimate implies an average increase of +75 percent in traffic fatality rates in the years 2016 to 2019 due to the law change. Nevertheless, unlike Colorado, the ATE in Alaska is statistically significant over the entire post-retail study period (2016–2019) and is substantially higher, being roughly five times that of Colorado. Again, the obtained fit index of 0.61% (see Table 3) and the statistically insignificant placebo effects (see Figures F.1c and F.2c) support the credibility of the estimated ATE.

The estimated ATE curve for Oregon (Fig. 2(h)) exhibits statistically insignificant effects of recreational marijuana legalisation on traffic fatalities in Oregon in the initial three years post-intervention, that is, until 2017. However, the estimated ATE becomes statistically significant and positive for 2018 and 2019. The legalisation law's enactment and the commencement of retail sales of recreational marijuana coincide, with the aggregated post-retail ATE estimate being +16.31 (95% CI = [−2.37, 34.99]). This estimate suggests an average increase of 16 percent in traffic fatality rates from 2015 to 2019 due to the law change, similar in magnitude to Colorado's experience. Additionally, the ATE curve indicates an increasing trend similar to that of Washington. With a fit index of 2.85% (see Table 3) and statistically insignificant placebo effects (see Figures F.1d and F.2d), our estimates are once again credible.

Interestingly, according to Fig. 2(j), the estimated ATE in California is positive and statistically significant over the entire post-treatment study period (that is,2016–2019). The aggregated pre-retail and post-retail ATE estimates are roughly similar in magnitude, +20.70 (95% CI = [9.54,31.85]) and +18.36 (95% CI = [7.36,29.35]), respectively. These estimates suggest an average increase of +20 percent and +18 percent in traffic fatality rates due to the legalisation in the post-intervention periods 2016–2017 and 2018–2019, respectively. The obtained fit index of 0.48% (see Table 3) and the statistically insignificant placebo effects (see Figures F.1e and F.2e) again support the credibility of the estimated ATE.

Further, according to our estimates, the legalisation of recreational marijuana had a statistically insignificant effect on traffic fatalities in the remaining four treated states, Massachusetts (Fig. 2(l)), Maine (Fig. 2(n)), Nevada (Fig. 2(p)) and Michigan (Fig. 2(r)). For Maine, we note that although recreational marijuana was legalised in Maine in January 2017, the legislature made many changes and compromises and had to overcome vetoes to pass an amended law. Among the changes was reducing the number of marijuana plants a recreational consumer can grow at home from six to three. The legislature also postponed consideration of marijuana social clubs until 2023, leaving private property as the only place where recreational marijuana can be consumed. Given such restrictions on marijuana use, people may have had less incentive to drive when high. Furthermore, recreational marijuana did not become commercially available during the post-treatment period under study. The year-on-year effect in Massachusetts is highly variable, with no discernible trend in the ATE over the years. Further, similar to Washington, the estimated ATE curves for Nevada and Michigan show an increasing trend, but the values remain statistically insignificant until the end of our study period, that is, 2019. Likewise, the obtained fit indices of 0.14%, 0.14%, 3.02% and 1.65%, respectively (see Table 3) and the statistically insignificant placebo effects (see Figures F.1f - F.1i and F.2f - F.2i) support the credibility of the estimated ATE for the four states.

Overall, our findings indicate that the strong temporal trend in the ATE is primarily driven by the retail availability of marijuana. However, variations in impact over time may also reflect shifting attitudes towards marijuana use and its perceived safety while driving. For instance, many Americans, especially young adults, increasingly believe that driving under the influence of marijuana is safe (McCarthy et al., 2007; Cavazos-Rehg et al., 2018; Greene, 2018; Keyhani et al., 2018). In terms of regional differences in ATE, our results align with those of Adhikari et al. (2023), showing that states that legalised recreational marijuana earlier experienced larger increases compared to those that did so more recently. Washington, however, is an exception. Notably, Washington is the only treated state among the nine studied that did not decriminalise marijuana before legalising its recreational use, which may influence these differing results.

Additionally, factors such as traffic enforcement intensity, the perceived likelihood of apprehension, and the chances of avoiding punishment (Hasan et al., 2022) could also contribute to the observed state-level variations. Among the nine treated states, Washington, Colorado, and Nevada implemented per se driving laws, while Michigan introduced a zero-tolerance law after legalising recreational marijuana. Per se drugged driving laws, similar to per se alcohol laws, establish a specific impairment threshold, making it easier to prosecute impaired drivers (DuPont et al., 2012). Zero-tolerance laws, which set the threshold at zero, mean any detectable amount of the drug can lead to charges of intoxication. Although per se alcohol laws have been effective in reducing traffic fatalities (Dee, 2001; Freeman, 2007; Hansen, 2015), evidence regarding the impact of per se drugged driving laws on traffic fatalities remains limited (Anderson and Rees, 2015). The lack of statistical significance in the ATE for Washington, Nevada, and Michigan, along with the decline in ATE for Colorado in later years, may suggest that these drugged driving laws are influencing the observed trends.

In the analyses presented above, we define the intervention as the enactment of recreational marijuana laws, that is, the point at which possession and personal use become legal because this represents the earliest legally sanctioned change that could shift public norms, risk perceptions, and enforcement practices. Moreover, our results indicate that in states such as California (Fig. 2(j)), there are significant changes in traffic safety outcomes occurring prior to the commencement of retail sales. Nonetheless, to evaluate the potential distinct effects of cannabis commercialisation, we have conducted a supplementary sensitivity analysis (see Appendix J) where the intervention is re-defined as the year when retail sales began. Importantly, in this alternative specification the previously significant results for California (Figure J.1h) are no longer statistically significant. This finding underscores the challenges associated with framing commercialisation as the primary intervention and reaffirms our choice to focus on legalisation enactment as the core treatment point. We present the commercialisation analysis as exploratory and as a means to assess effect heterogeneity, without altering our central identification strategy.

3.2. The relationship between marijuana and alcohol consumption

Next, we use our ASCM models to infer the impact of recreational marijuana on proxy outcome measures for drunk driving: (i) log-transformed night-time traffic fatalities per 100,000 population, and (ii) log-transformed weekend night-time traffic fatalities per 100,000 population, in the nine treated states. We also investigate the impact on log-transformed traffic fatalities per 100,000 population where one or more drivers tested positive for alcohol, but only with the view of validating these proxy measures. Before proceeding, we emphasise that our interpretation of the results presented below relies on the assumption that legalising recreational marijuana led to an increase in adult marijuana use. This assumption is supported by previous research showing a positive link between such legal changes and increased adult marijuana consumption (Cerdá et al., 2020; Hall and Lynskey, 2020; Zellers et al., 2023).

Figs 3 (left), 4 (left), and 5 (left) illustrate the evolution of log-transformed night-time fatalities per 100,000 population, log-transformed weekend night-time fatalities per 100,000 population, and log-transformed alcohol-related fatalities per 100,000 population, respectively, in the nine treated states and in their respective synthetic control units. The figures (right) show the corresponding estimates of the ATE (in log points) in each year for the three outcomes, where the ATE signifies the estimated year-on-year change in the outcome due to the intervention. The corresponding 95-percent confidence intervals are shown by the shaded grey area in the figures. The orange and red vertical dotted lines mark the year of enactment of the recreational marijuana legalisation law (as in Table 1) and the year of the beginning of retail sales in the state, respectively. As before, the former line represents the treatment year used to develop the synthetic control units, whereas the latter line serves merely as an illustration. Further, the auxiliary covariates and the estimated weights for the generation of the synthetic control units are summarised in Tables 4–6 and Appendices C - E, respectively. The set of auxiliary covariates relevant to each synthetic unit, as shown in Tables 4–6, are identified using the iterative process described in Section 2.2.3. Note that we do not present the ATE estimates in percentage points, as the exact magnitude of the impact is not central to our discussion. Instead, Figs. 3–5 sufficiently convey the nature of the impact — whether it indicates an increase, decrease, or no change.

From Fig. 3, we observe a statistically significant increase in night-time traffic fatality rates in Colorado, Oregon, and California (see Figs. 3(d), 3(h), and 3(j) following the enactment of recreational marijuana legalisation in these states. In contrast, Washington (Fig. 3(b) experienced a statistically significant decrease in night-time traffic fatality rates in the early years after the law was passed, though this effect became statistically insignificant in later years. The ATE estimates for other treated states remain statistically insignificant. Washington is unique among the nine treated states in that it did not decriminalise marijuana before legalising its recreational use. The initial decline in nighttime fatal crashes in Washington, a pattern that may reflect reduced alcohol-impaired driving, is consistent with a possible substitution away from alcohol use following marijuana legalisation. However, this substitution effect diminishes over time. Conversely, the evidence from Colorado, Oregon, and California indicates that marijuana and alcohol may be complementary substances. It is important to note that night-time fatalities represent a restricted subset of total incidents, which reduces the effective sample size and increases data noise. This can limit our ability to obtain precise synthetic matches for the treated states, even though the number of aggregate observations remains unchanged. For this reason, our best-fitting models fail to meet the fit index threshold of 5% for most states (see Table 4). This limitation also affects the placebo tests presented in Appendix C, which, though statistically insignificant, do not meet the fit index criterion.

The estimates presented in Fig. 4, which pertain to weekend night-time traffic fatality rates, reveal similar trends in ATE for Washington and California. In Washington, there is evidence of a substitutionary relationship between alcohol and marijuana in the early years following recreational marijuana legalisation, with no significant relationship observed in later years. Conversely, the estimates for California suggest a complementary relationship between alcohol and marijuana throughout most of the post-legalisation study period. Estimates from other treated states remain statistically insignificant, indicating no clear relationship between alcohol and marijuana consumption. Additionally, with further restrictions on effective sample size as noted above, fit indices (see Table 5) generally fall short of acceptable thresholds, and placebo tests (Appendix D) also remain limited in their ability to justify the credibility of our estimates.

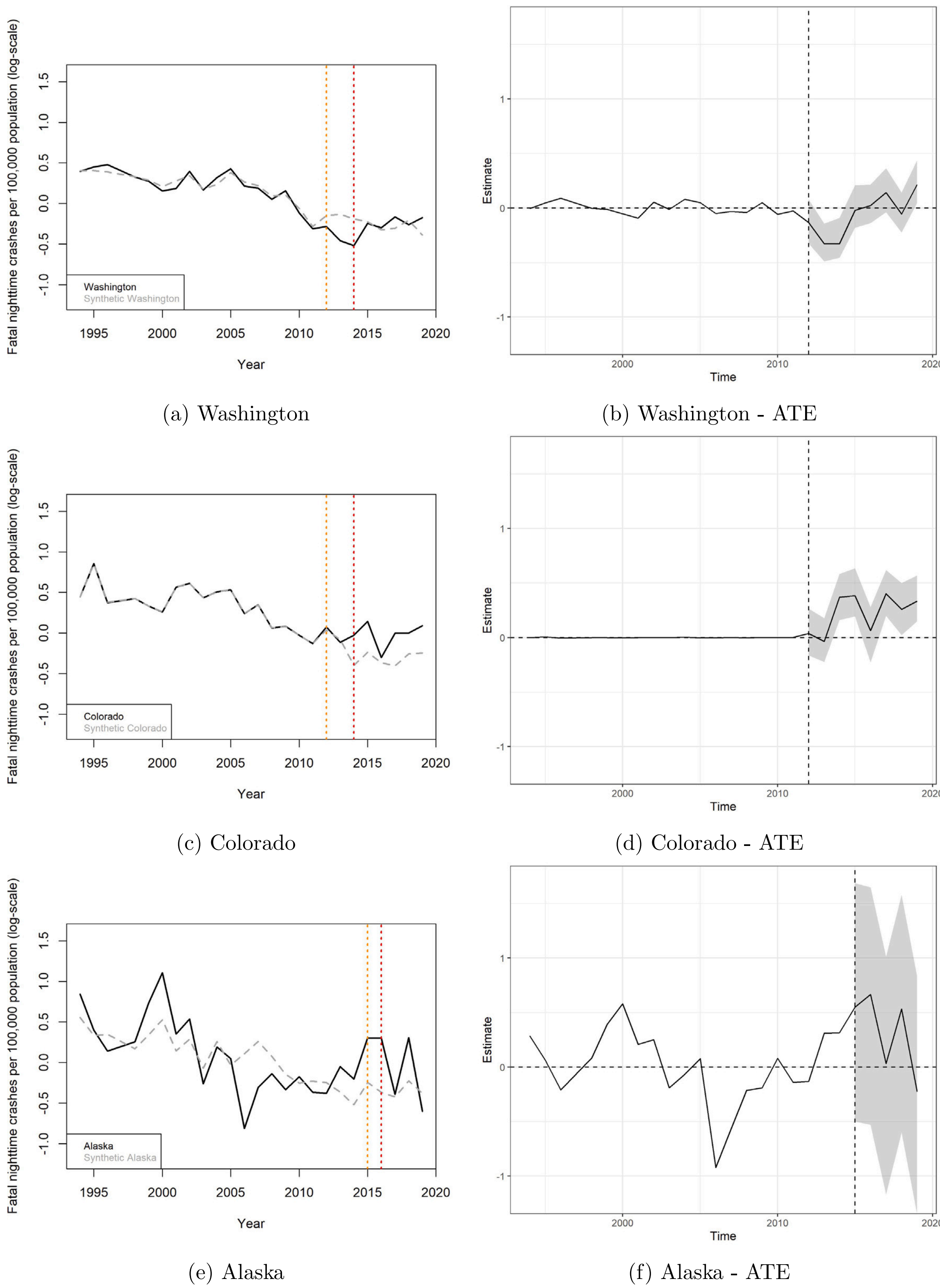
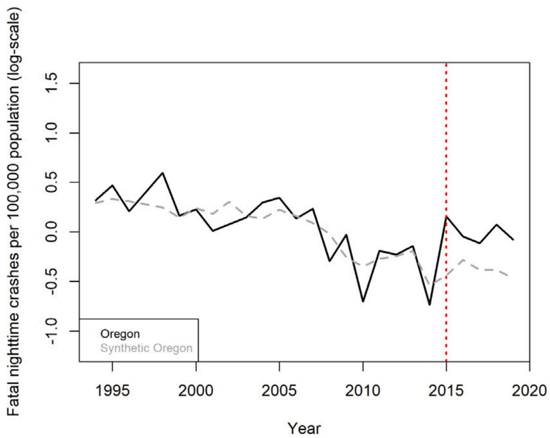
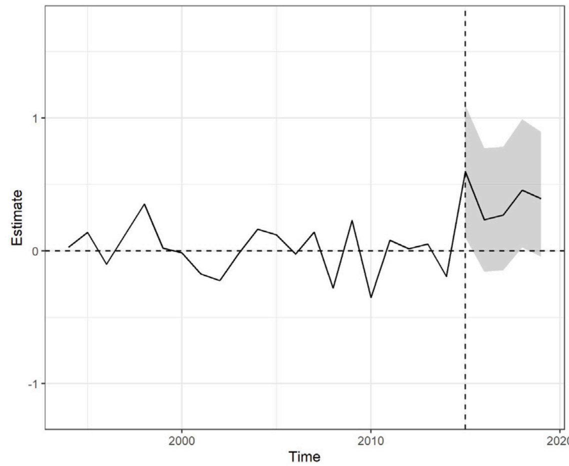


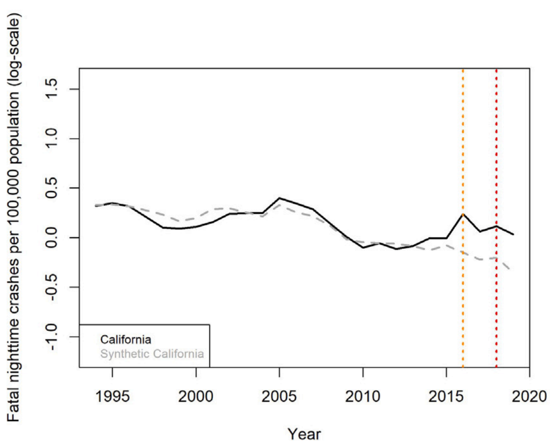
Fig. 3. Figures on the left present a comparison of night-time traffic fatalities per 100,000 population (in log-scale) in different states with their synthetic counterparts. The orange line indicates the year of intervention, that is, the enactment of the recreational marijuana legalisation law and the red line indicates the year when retail sales of recreational marijuana began. Figures on the right show the corresponding estimates of the average treatment effect. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)



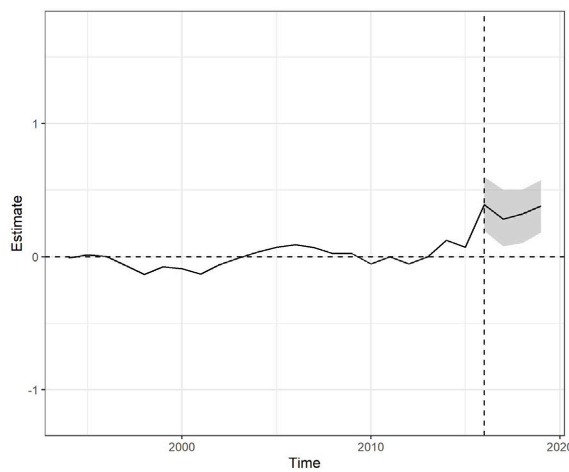
(g) Oregon



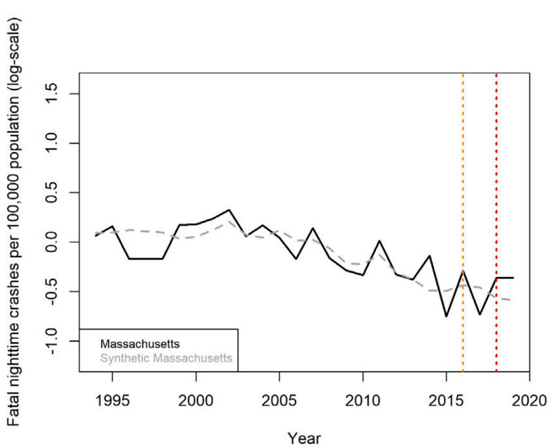
(h) Oregon - ATE



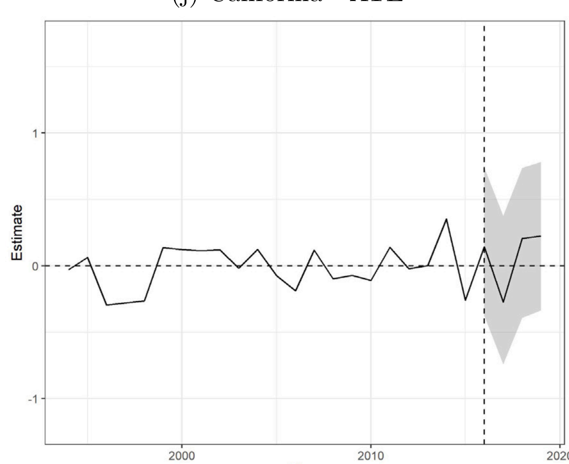
(i) California



(j) California - ATE

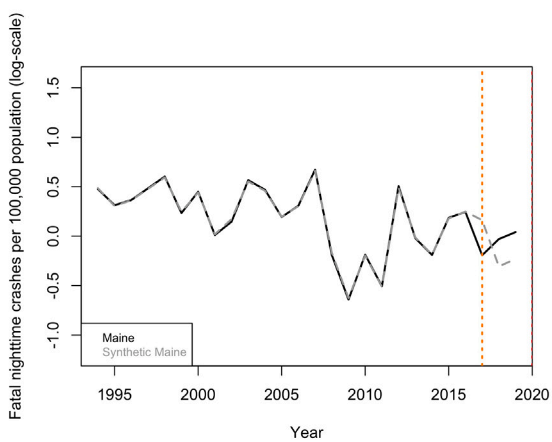


(k) Massachusetts

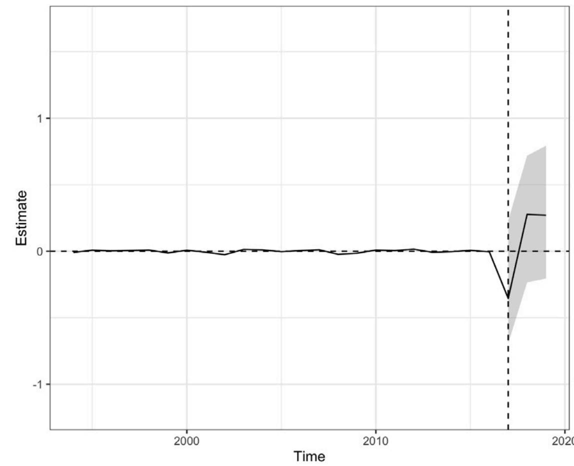


(l) Massachusetts - ATE

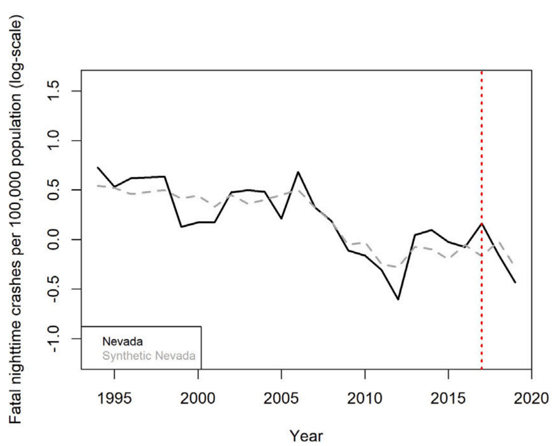
Fig. 3. (*continued*).



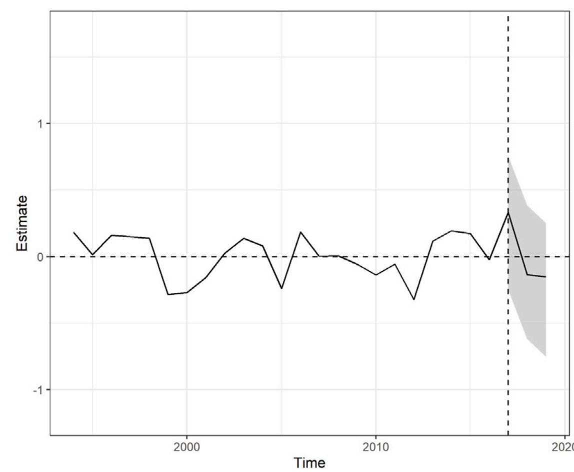
(m) Maine



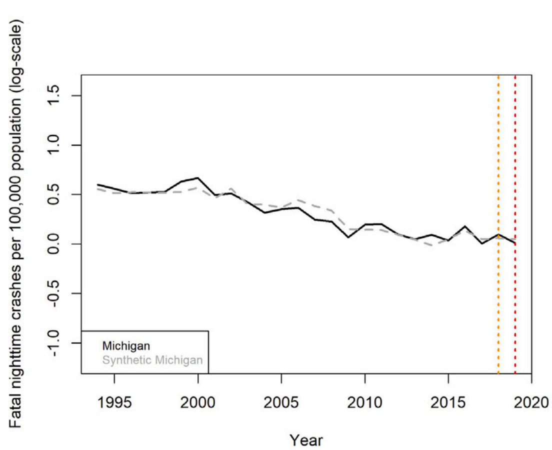
(n) Maine - ATE



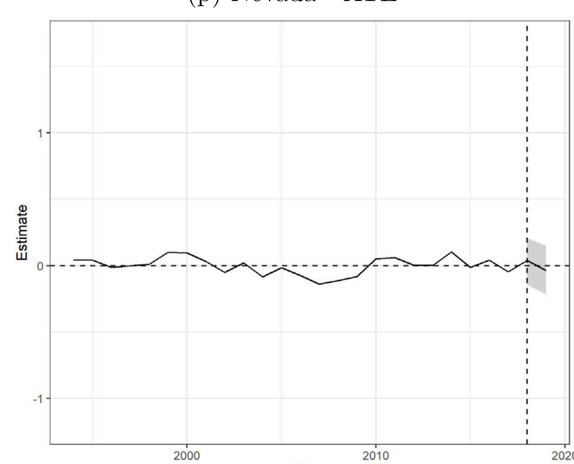
(o) Nevada



(p) Nevada - ATE

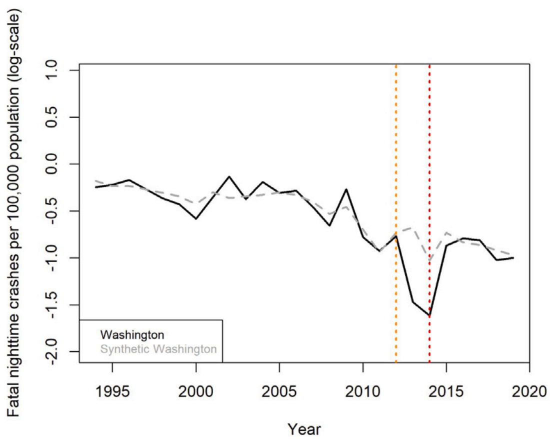


(q) Michigan

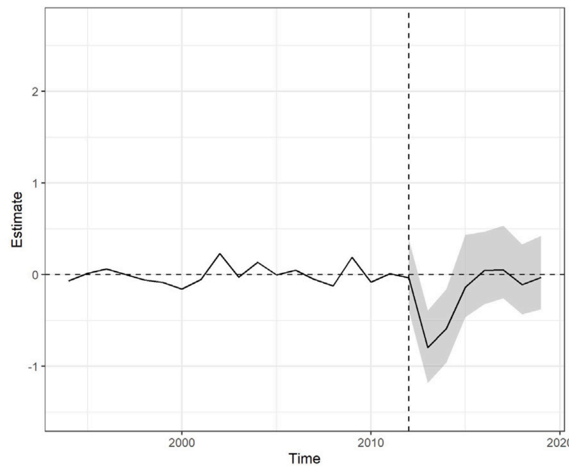


(r) Michigan - ATE

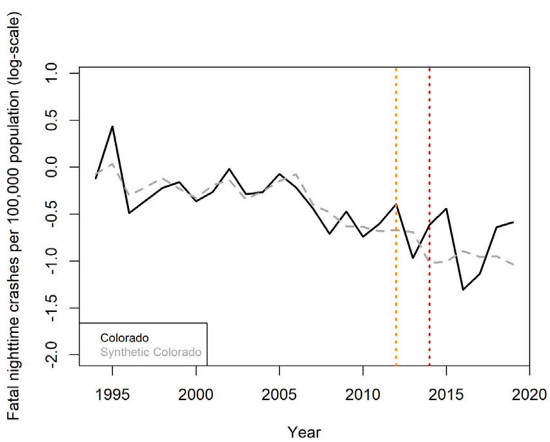
Fig. 3. (*continued*).



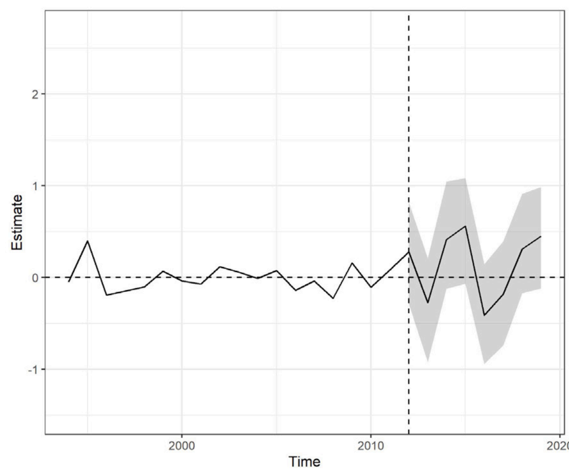
(a) Washington



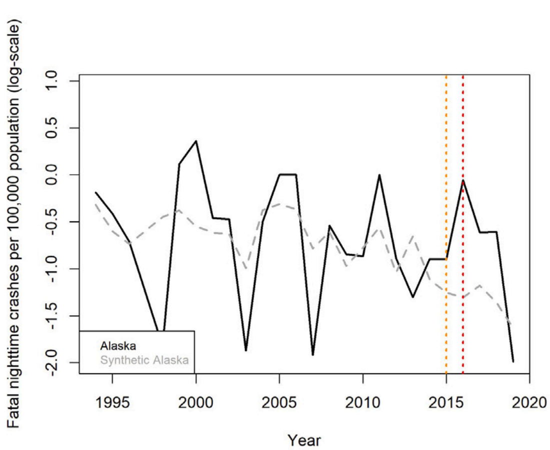
(b) Washington - ATE



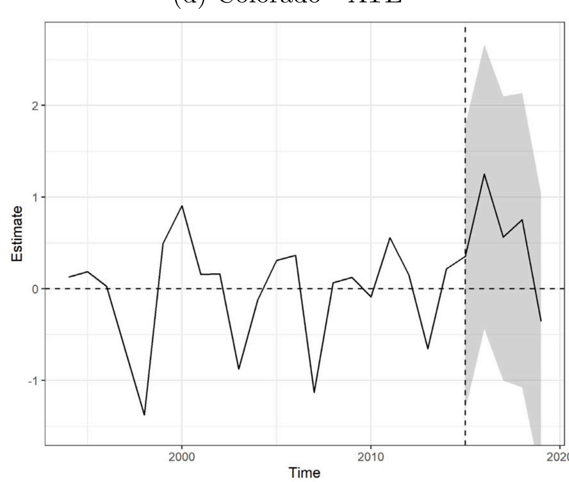
(c) Colorado



(d) Colorado - ATE

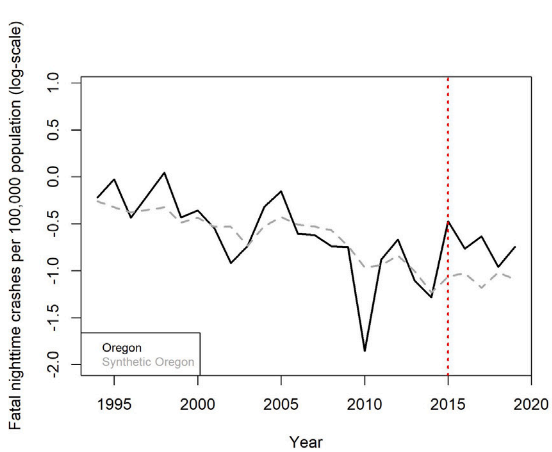


(e) Alaska

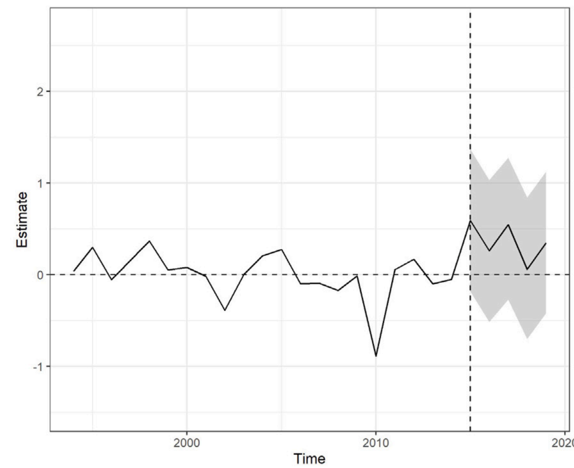


(f) Alaska - ATE

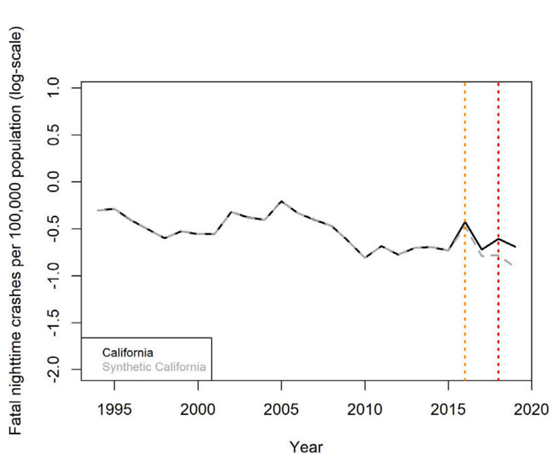
Fig. 4. Figures on the left present a comparison of weekend night-time traffic fatalities per 100,000 population (in log-scale) in different states with their synthetic counterparts. The orange line indicates the year of intervention, that is, the enactment of the recreational marijuana legalisation law and the red line indicates the year when retail sales of recreational marijuana began. Figures on the right show the corresponding estimates of the average treatment effect. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)



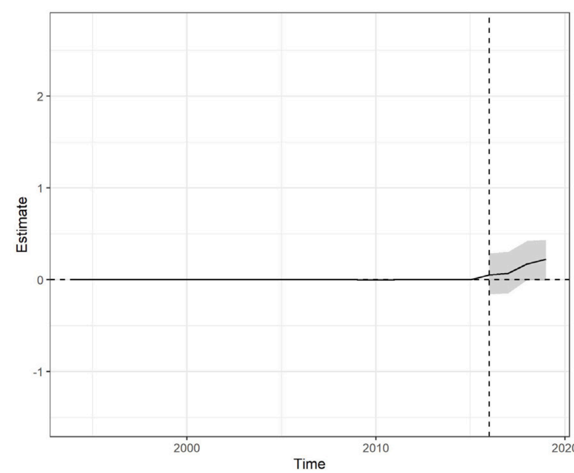
(g) Oregon



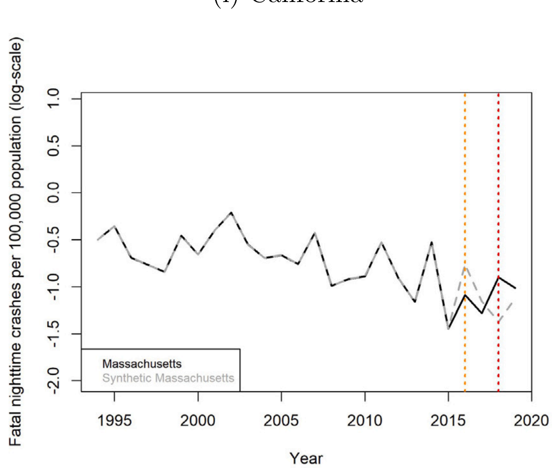
(h) Oregon - ATE



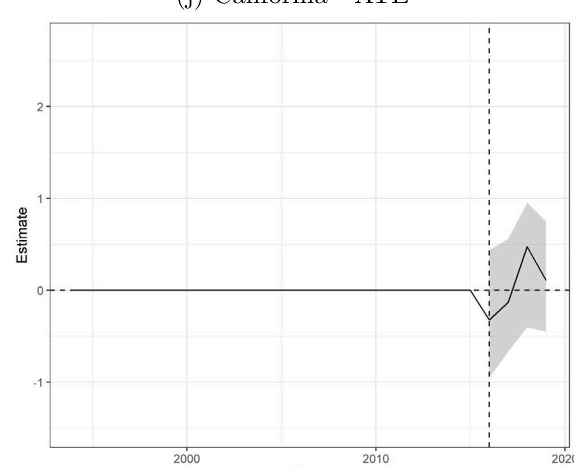
(i) California



(j) California - ATE

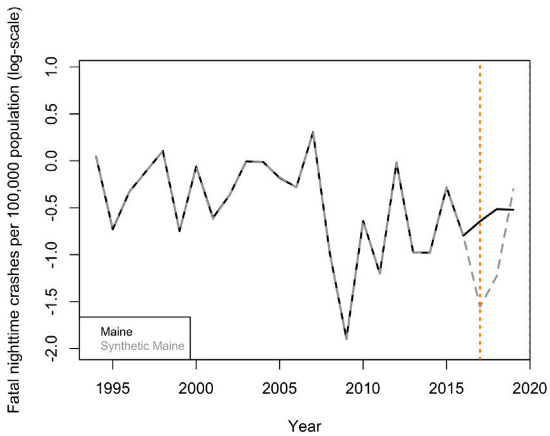


(k) Massachusetts

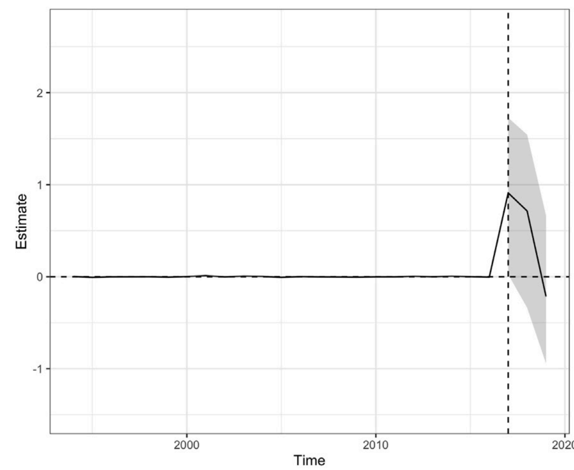


(l) Massachusetts - ATE

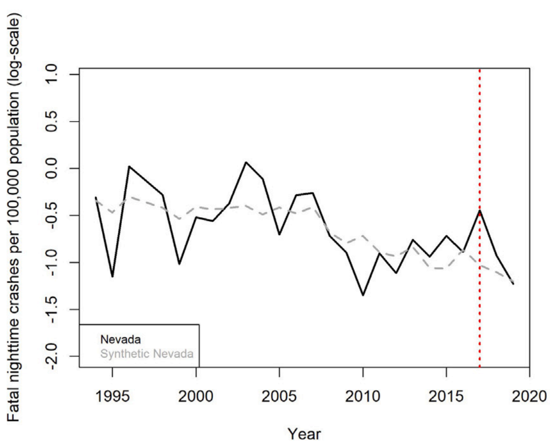
Fig. 4. (*continued*).



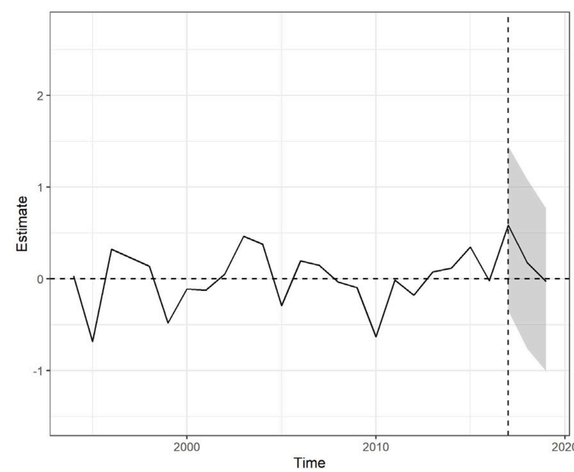
(m) Maine



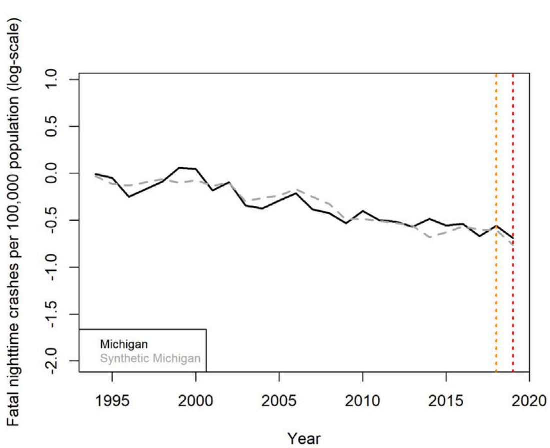
(n) Maine - ATE



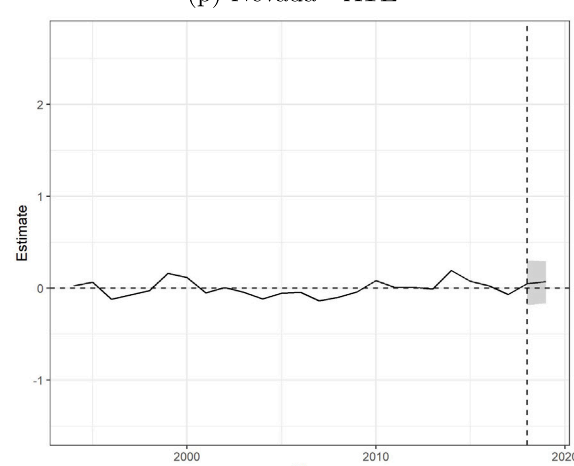
(o) Nevada



(p) Nevada - ATE



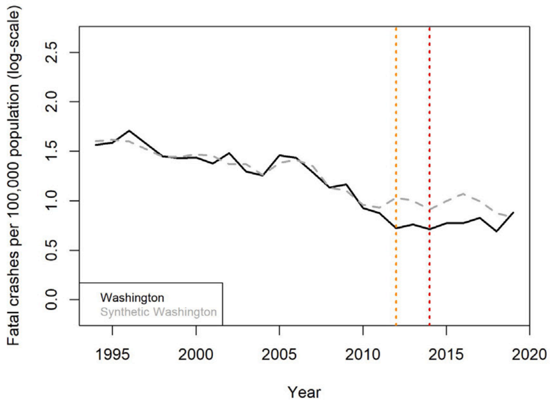
(q) Michigan



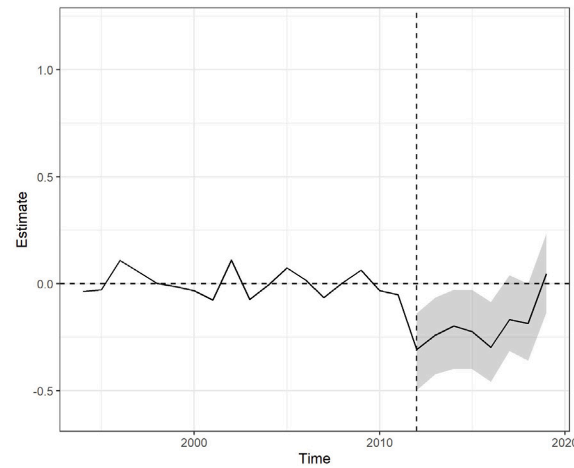
(r) Michigan - ATE

Fig. 4. (*continued*).

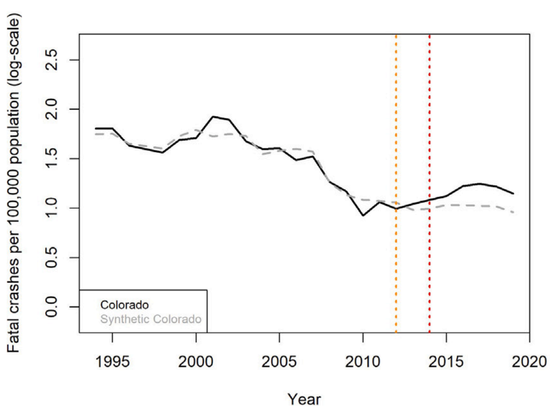
Given the limitations discussed earlier, we further validate these findings using alcohol-related crash data. Fig. 5 shows the results from the corresponding ASCM models. In Washington, the trends in ATE are consistent with our previous results, indicating a substitutionary relationship between alcohol and marijuana in the early years following the law change, with no discernible relationship thereafter. Additionally, we observe a statistically significant increase in alcohol-related traffic fatalities in Colorado, Oregon, and California, though this effect is present only in certain years. Estimates for other treated states remain statistically insignificant. Most fit indices (see Table 6) meet the acceptable threshold, and placebo effects are statistically insignificant (Appendix E), supporting the reliability of these estimates. In sum, the



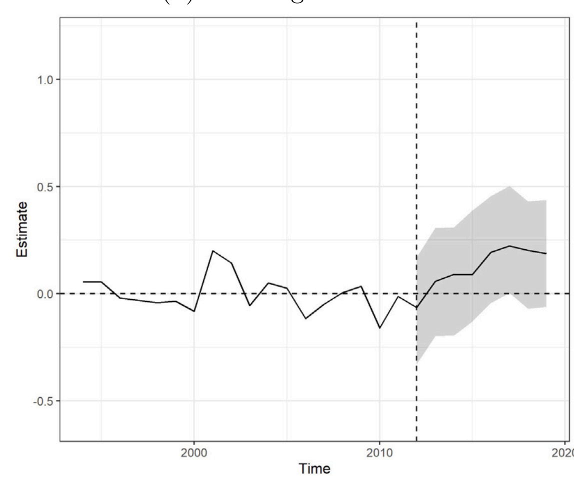
(a) Washington



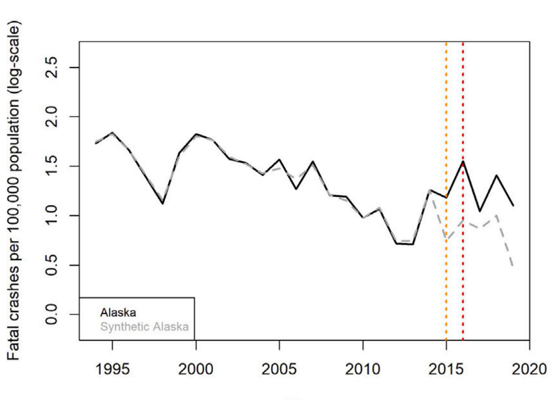
(b) Washington - ATE



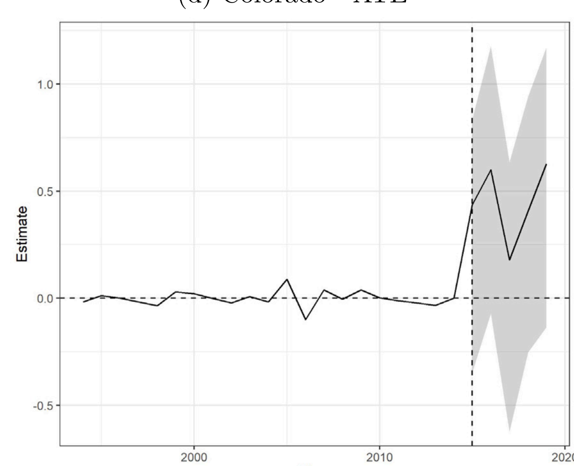
(c) Colorado



(d) Colorado - ATE

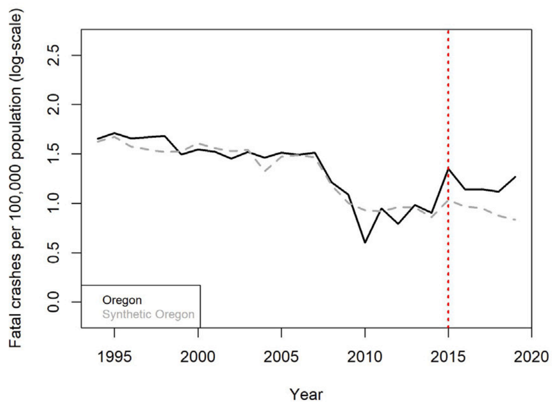


(e) Alaska

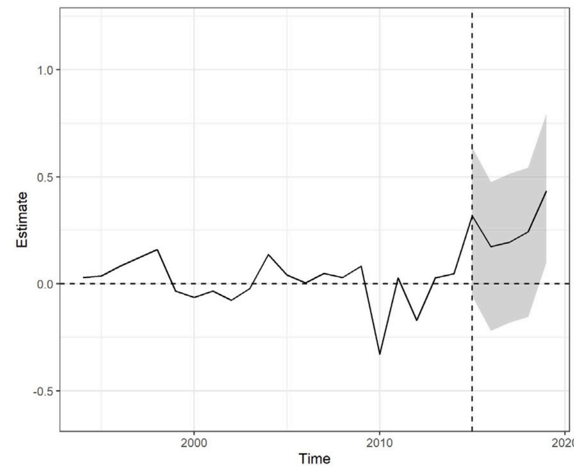


(f) Alaska - ATE

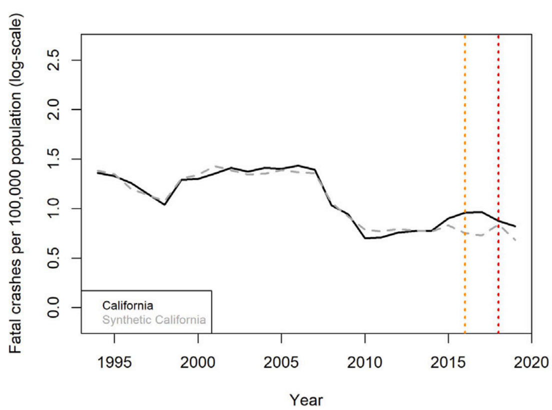
Fig. 5. Figures on the left present a comparison of alcohol-related traffic fatalities per 100,000 population (in log-scale) in different states with their synthetic counterparts. The orange line indicates the year of intervention, that is, the enactment of the recreational marijuana legalisation law and the red line indicates the year when retail sales of recreational marijuana began. Figures on the right show the corresponding estimates of the average treatment effect. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)



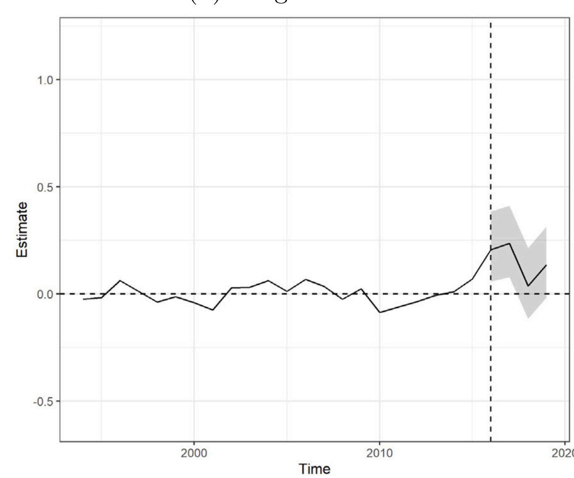
(g) Oregon



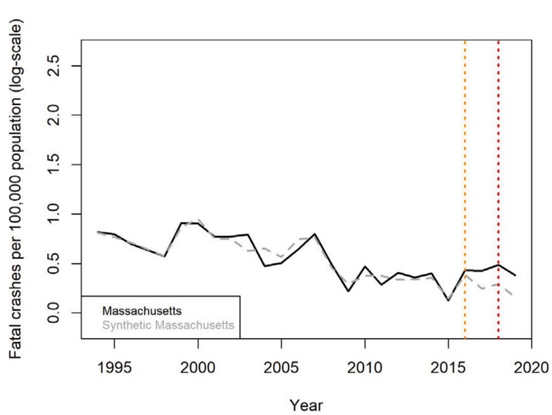
(h) Oregon - ATE



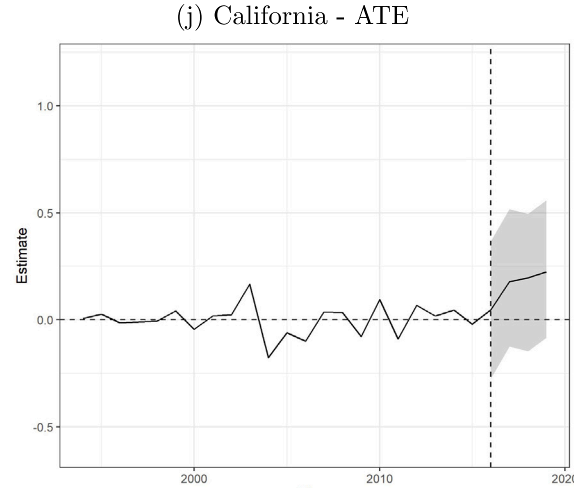
(i) California



(j) California - ATE

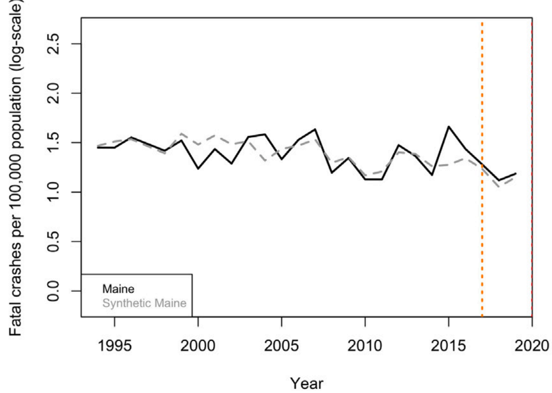


(k) Massachusetts

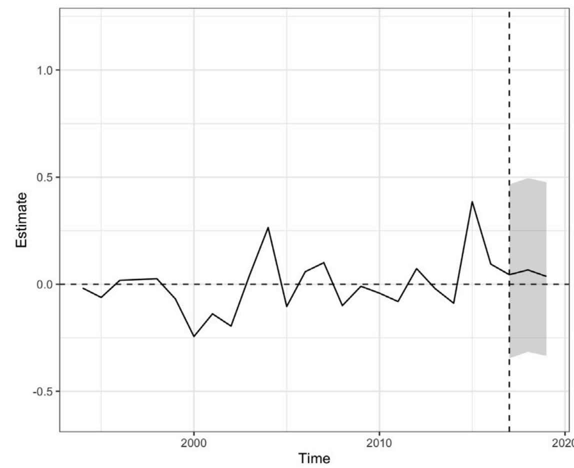


(l) Massachusetts - ATE

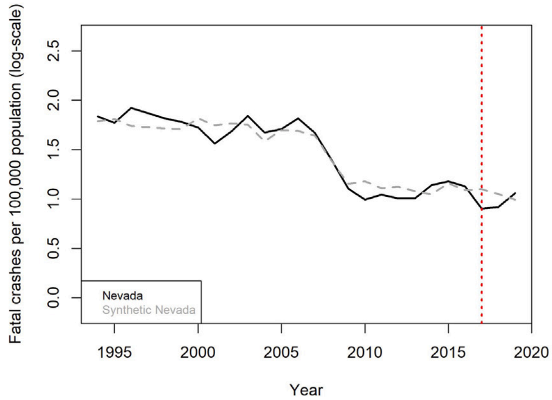
Fig. 5. (*continued*).



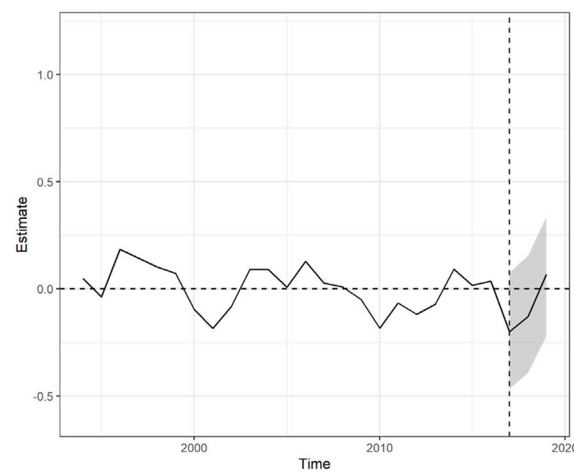
(m) Maine



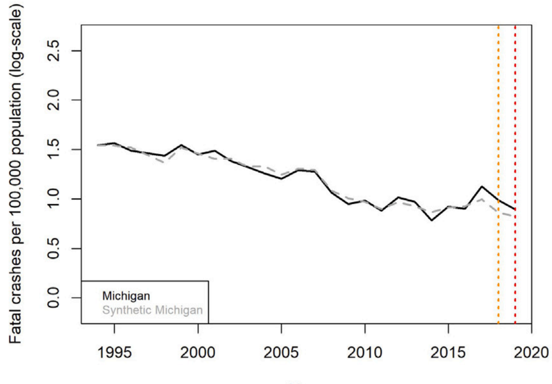
(n) Maine - ATE



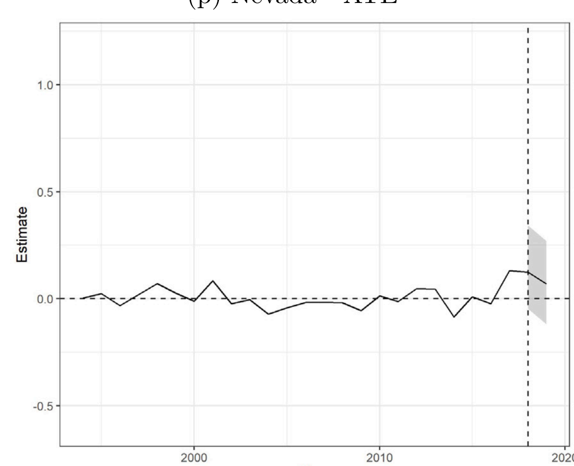
(o) Nevada



(p) Nevada - ATE



(q) Michigan



(r) Michigan - ATE

Fig. 5. (*continued*).

Table 4
Auxiliary variables for the development of synthetic control units for the analysis of night-time fatalities.

Covariates	WA	CO	AK	OR	CA	ME	MA	NE	MI
Real GDP		*							*
Population		*							*
Area		*				*			*
No. of employees		*				*			*
Length of all roads		*				*			
Length of rural roads		*				*			*
licenced driver per unit population									
Total vehicle miles travelled						*			*
L^2 imbalance	0.21	0.01	1.51	0.78	0.31	0.05	0.73	0.77	0.32
Fit index	17.27	0.607	1.275	0.633	1.78	2.926	0.624	0.70	17.28

Table 5
Auxiliary variables for the development of synthetic control units for the analysis of weekend night-time fatalities.

Covariates	WA	CO	AK	OR	CA	ME	MA	NE	MI
Real GDP	*					*	*		*
Population	*				*	*	*		*
Area						*	*		*
No. of employees	*				*	*	*		*
Length of all roads					*	*	*		*
Length of rural roads					*	*	*		*
licenced driver per unit population					*	*	*		
Total vehicle miles travelled	*				*	*	*		*
L^2 imbalance	0.43	0.60	2.49	1.18	0.00	0.02	0.00	1.39	0.41
Fit index	22.90	36.01	60.16	34.33	0.16	0.62	0.06	40.40	22.10

Table 6
Auxiliary variables for the development of synthetic control units for the analysis of alcohol-related fatalities.

Covariates	WA	CO	AK	OR	CA	ME	MA	NE	MI
Real GDP					*		*		*
Population			*		*				
Area			*		*				*
No. of employees			*		*		*		*
Length of all roads		*			*		*		*
Length of rural roads		*			*				*
licenced driver per unit population					*		*		
Total vehicle miles travelled		*			*		*		*
L^2 imbalance	0.24	0.36	0.16	0.47	0.49	0.64	0.33	0.46	0.24
Fit index	4.23	5.49	2.57	7.60	3.92	9.67	11.69	6.35	3.99

weight of the evidence presented in this section points towards no clear relationship between marijuana use and alcohol consumption.

4. Discussion

4.1. Principal findings

Based on our results, we observe the following trends: Washington, which was the first state to legalise recreational marijuana in 2012, did not experience a statistically significant change in fatal traffic crashes due to the law. The subsequent states – Colorado (2012), Alaska (2015), and Oregon (2015) – did show a statistically significant increase in traffic fatality rates, but this effect was only evident once the drug became commercially available. Among these states, Alaska experienced an immediate rise in fatalities following the start of retail sales, while Colorado and Oregon saw a delayed increase over a couple of years. By the end of our study period, that is,1994–2019, the increase in traffic fatalities in Colorado had become statistically insignificant. In contrast, California, which legalised recreational marijuana a year after Alaska and Oregon, also saw an immediate rise in traffic fatality rates. The four other states, Maine, Massachusetts, Nevada, and Michigan, that enacted legalisation in 2016, 2016, 2017, and 2018, respectively, did not experience statistically significant changes in traffic fatality rates. However, year-on-year changes in traffic fatalities in Washington, Nevada, and Michigan, while not statistically significant, showed an upward trend. Furthermore, we find no discernible link between marijuana and alcohol consumption.

4.2. Strengths and weaknesses of the study

Our study employs a rigorous framework that uses observational (non-experimental) data on traffic fatality rates and an augmented synthetic control method to assess the causal impact of recreational marijuana legalisation on fatal traffic crashes in the US. This innovative causal inference framework is a key strength of our study for several reasons:

1. It is not hindered by the lack of accurate roadside tests for marijuana impairment or sampling errors arising from drivers involved in accidents not being tested for the drug.
2. Unlike driving simulation studies, it does not rely on experimental conditions, which may not accurately replicate real-world driver impairment and traffic conditions.
3. Synthetic control methods do not require explicit adjustment for all potential confounding factors, some of which may be unobserved. Instead, these methods determine causal effects by comparing the post-treatment trajectories of traffic fatality rates in each treated state with their synthetic counterparts, developed to match the pre-treatment trajectory of the treated state.
4. The adopted approach captures the temporal dynamics of the relationship, offering a more accurate characterisation of the evolving process over time.

Using this framework, we demonstrate a consistent pattern of increased traffic fatality rates across several US states following their legalisation of recreational marijuana. Our analysis provides a robust look at the causal linkages between the law and traffic fatality rates across different geographical regions and across time.

Regardless, we recognise that our analysis may suffer some general limitations in drawing causal inferences from observational data. Our findings may lack generalisability, potentially not applying to different regional settings or populations beyond the studied sample. Moreover, measurement errors, such as inaccurate or incomplete data on traffic fatalities, may distort the true relationship we aim to understand. There may also be residual confounding or unaccounted temporal trends. Overall, we emphasise that our findings are based on an ecological study design and should be interpreted accordingly.

Furthermore, while the use of night-time and weekend night-time traffic fatalities as proxies for alcohol impairment allows us to overcome limitations in direct testing data – such as inconsistent enforcement or reporting across states – these proxies are not without shortcomings. Specifically, they may capture fatalities not directly attributable to alcohol or drug impairment, potentially inflating raw fatality rates relative to the actual number of impairment-related incidents. Although this trade-off is acknowledged in prior literature (Dee, 1999; Ruhm, 1996), and we validate our proxy-based insights using FARS data on alcohol-related fatalities, it remains important to interpret these results with this caveat in mind.

4.3. Comparisons with other studies

Since the liberalisation of recreational marijuana laws in Washington and Colorado, several studies have adopted causal inference approaches, especially differences-in-differences (DID), to understand the impact of the change in law on traffic fatality rates in the US (Cole, 2018; Aydelotte et al., 2017; Gunadi, 2022; Lane and Hall, 2019; Vogler, 2017; Aydelotte et al., 2019; Kamer et al., 2020; Adhikari et al., 2023). The findings from these studies vary — some have reported a statistically insignificant change, while others have suggested increases in fatalities ranging from 8% to 17%. Some studies (Hansen et al., 2017; Romano et al., 2017) have suggested that DID estimates may be biased in this context due to challenges pertaining to the identification of the true counterfactual trend for the states passing the law.

Accordingly, two studies, Hansen et al. (2020) and Santaella-Tenorio et al. (2020), have applied the original synthetic control method to investigate the impact of the law on traffic fatality rates in both Washington and Colorado for the post-treatment periods of 2012–2016 and 2012–2017, respectively. Both studies have found a statistically insignificant impact of the intervention in Washington. However, their estimates for Colorado differ — the former study has found a statistically insignificant impact, while the latter has estimated an increase in traffic fatality rates. In contrast to these studies that have focused on aggregated effects of the intervention over the entire post-treatment study period, we leveraged the synthetic control method to understand year-on-year impacts and their statistical significance. We also expanded the study period up to 2019 and included additional states; Alaska, California, Oregon, Maine, Massachusetts, Nevada and Michigan; for no individualised insights were available in the literature. Our findings reveal a consistent, albeit delayed, increase in traffic fatality rates across many treated states. Further, similar to recent studies such as Hansen et al. (2020) and Cole (2018), this study found no clear link between marijuana and alcohol consumption, refuting the idea that marijuana and alcohol serve as substitutes.

In sum, our study complements and extends (with new spatial and temporal insights) existing studies that have found evidence of a negative impact of recreational marijuana on traffic safety. The findings from this study disproved prevailing conjectures that have dismissed the link between recreational marijuana and fatal traffic crashes (see, for instance, McCarthy et al., 2007; Cavazos-Rehg et al., 2018; Greene, 2018; Keyhani et al., 2018).

4.4. Meaning of the study for clinicians and policy makers

States and countries considering the legalisation of recreational marijuana must carefully evaluate its impact on traffic safety. While legalising commercial marijuana may reduce illicit market activity and generate state tax revenue, our study presents compelling evidence of a significant increase in traffic fatalities associated with the drug's greater availability. We also find indicative evidence that per se drugged driving laws may help mitigate some of these negative effects. However, enforcing such laws remains challenging due to the absence of a reliable roadside test for THC, analogous to alcohol breathalysers. Identifying marijuana-impaired drivers typically requires resource-intensive training of officers in drug recognition techniques and introduces subjectivity relative to alcohol enforcement. Although blood tests for THC levels are currently the most accurate option, they are invasive, time-sensitive, and logistically difficult to implement during traffic stops.

Given these enforcement limitations, public education is a critical policy lever. The evolving legal and cultural status of marijuana has created mixed perceptions about its risks, especially in comparison to alcohol. Education campaigns can play an important role in promoting awareness about the real dangers of driving under the influence. In addition to broad-based public service announcements, states should also consider complementary policies such as: (i) stricter labelling requirements for commercial cannabis products to indicate THC and other cannabinoid concentrations clearly; (ii) guidance on safe waiting periods before driving after cannabis use, based on available evidence; and (iii) improved systems for measuring drug-impaired driving, including more consistent post-accident drug testing protocols across states. These measures, in combination with well-designed education strategies, can help reduce the unintended road safety consequences of marijuana legalisation.

4.5. Unanswered questions and future research

Our study employed a causal inference approach on observational data to assess the impact of recreational marijuana legalisation on traffic safety outcomes, such as traffic fatality rates. By design, this approach limits the inclusion of certain factors – such as traffic enforcement measures and public awareness campaigns – that influence traffic safety but have themselves changed in response to the legalisation. This is because conditioning the estimated average treatment effect (ATE) on such variables risks obscuring critical causal pathways through which legalisation may affect the outcomes of interest. Consequently, while we find preliminary evidence suggesting that per se drugged driving laws may have helped mitigate some negative impacts of legalisation on traffic safety, we cannot isolate the independent effects of legalisation and these enforcement responses. In states such as Washington, Colorado, Nevada, and Michigan, where such laws were introduced in response to legalisation, our estimates reflect the combined package of effects arising from both interventions.

At the same time, it is important to acknowledge that these post-treatment factors, though excluded from estimation, may partially explain the heterogeneity in estimated treatment effects across states and over time. For instance, variation in how states implemented traffic enforcement measures, public messaging, or law enforcement capacity could mediate the observed effects. As such, even if excluded from the formal estimation process, they remain critical to interpreting the differences in outcomes across treated cases. Future research should explore methods for separately identifying and quantifying the mediating roles of such policy responses, which could help guide more targeted strategies for mitigating adverse effects of marijuana legalisation on traffic safety.

Further, this study is limited to examining the effects of legalisation up to 2019. While states such as Washington, Nevada, and Michigan exhibit an upward trend in the estimated treatment effect on traffic fatalities, these effects remain statistically insignificant within the observed window. The exclusion of post-2019 data reflects a methodological choice rather than a data availability constraint. The onset of the COVID-19 pandemic introduced major disruptions to traffic volumes, travel behaviour, enforcement patterns, and broader socioeconomic conditions. These disruptions represent a structural break in the data-generating process that undermines the assumptions required for causal identification in this context. In particular, when a large, exogenous shock coincides with or closely follows the treatment, and its effects cannot be separately identified or appropriately controlled for, it becomes fundamentally difficult, regardless of the chosen causal inference framework, to isolate the treatment effect of interest. This is not a limitation of any one method, but a general challenge inherent to causal inference: if credible counterfactual outcomes cannot be constructed due to overlapping influences, no estimation strategy can recover unbiased effects without invoking strong and often untestable assumptions. Given that our intervention predates the pandemic and that the pandemic's effects are both widespread and heterogeneous, we judged that including post-2019 data would risk confounding the policy signal with unrelated behavioural shocks.

Recent evidence suggests that global work and travel patterns are only now converging towards a new post-pandemic equilibrium.[10] As such, the years immediately following 2020 may reflect a period of adjustment rather than stability. Future research should revisit this

[10] See, for instance, the WFH Research Project (https://wfhresearch.com), that presents a long-running international survey and data initiative led by Professor Nicholas Bloom (Stanford University). The project has collected monthly survey data since May 2020 on changes in working arrangements, commuting patterns, and related economic behaviours across major global economies. Their findings consistently show that the early post-COVID years represent a transitional phase, with structural changes in work and travel habits stabilising only in recent periods.

question once a longer post-pandemic time series becomes available, ideally using methods that can explicitly account for structural change, such as dynamic causal models. This would allow researchers to examine whether the effects of marijuana legalisation persist, intensify, or shift in a post-pandemic policy and behavioural landscape.

4.6. Summary

We studied the causal impact of recreational marijuana legalisation on traffic fatality rates in various US states that passed the law by 2019. Our study found that the change in law caused an increase in fatal traffic crashes in many of these states until 2019. This increase is particularly observed in the years following the start of retail sales. Given the widespread push for recreational marijuana legalisation in the US and several other countries, including Canada, New Zealand, Germany, Switzerland, and the UK, and the significant rise in the proportion of traffic fatalities involving US drivers testing positive for marijuana in recent years (Banta-Green et al., 2016), the question of how recreational use of marijuana affects traffic safety is particularly pressing. Overall, our study contributes to understanding the link between recreational marijuana and traffic fatalities and its implications for public policy and safety.

CRediT authorship contribution statement

Anupriya: Writing – original draft, Visualization, Software, Resources, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Emma McCoy:** Writing – review & editing. **Daniel J. Graham:** Writing – review & editing, Supervision.

Declaration of competing 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.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.aap.2025.108106.

Data availability

All relevant data sources have been acknowledged in the manuscript.

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