

In the Weeds of Traffic Fatalities: Revisiting the Effect of Medical Marijuana Laws

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Abstract

This study re-examines the finding by Anderson, Hansen, and Rees (2013) that medical marijuana laws *decrease* traffic fatality rates by 10.4%. I demonstrate that legalizing states were already experiencing declining fatalities prior to legalization, even after controlling for state-specific linear trends in a Two-Way Fixed Effects model. To address these pre-trends, I apply the Imputation Procedure (IP) by Borusyak, Jaravel, and Spiess (2024), which estimates state-specific trends using only not-yet-treated observations. Depending on the inclusion of potentially confounding covariates, my IP estimates suggest either a 12% *increase* or a zero effect on fatalities. I also show that the average state effect differs substantially from the average individual effect, indicating large heterogeneity across states. Much of the original negative result is driven by California, which accounts for over half of the population-weighted estimate. This state consistently exhibits one of the largest estimated negative effects and one of the steepest negative pre-trends.

Keywords: Policy Evaluation, Traffic Fatalities, Medical Marijuana Laws (MMLs), Two-Way Fixed Effects, Imputation Procedure, Pre-Trends, Heterogeneous Treatment Effects.

JEL: I18, C23.

1 Introduction

Medical marijuana laws (MMLs) make it legal for individuals with certain medical conditions to possess and consume marijuana. These laws are often considered in the broader context of legalization and remain politically contentious. The relation between marijuana and traffic fatalities was brought to public and scientific attention by Anderson et al. (2013), who found that MMLs *decrease* the rate of traffic fatalities by 10.4%.¹ This magnitude is large –comparable to the impact of the 1986 National Minimum Drinking Age Act (Kaestner and Yarnoff, 2011). The public health benefits implied by such a reduction, if taken seriously, have substantial implications for the marijuana legalization debate. However, if the estimated effect of MMLs is misleading (as I show it to be), both past and future policy decisions could be based on faulty evidence.²

The seminal work by Anderson et al. (2013) received hundreds of citations and paved the way for further research on the impact of both MMLs and recreational marijuana laws (RML) on traffic fatalities, including Santaella-Tenorio et al. (2017), Lee et al. (2018), Fink et al. (2020), Hansen et al. (2020), Windle et al. (2022), Adhikari et al. (2023), Chen and French (2023), Smart and Doremus (2023). With the single exception of Smart and Doremus (2023), studies on MMLs consistently find a negative causal effect on traffic fatalities. In contrast, RMLs are generally associated with a *positive* impact. While the documented difference in findings between medical and recreational legalization could reflect differences in consumer populations, this paper shows that the reported negative effect of MMLs may be spurious and becomes positive or zero once appropriate adjustments are made.

I revisit Anderson et al. (2013)’s main estimation and identify concerns regarding both internal and external validity. A formal test shows that, on average, legalizing states experienced declining traffic fatality rates even before legalization. These pre-trends suggest a violation of the parallel trends

¹The authors of the original study, published in the Journal of Law and Economics, were notified about this submission and saw current manuscript. Please see the attached correspondence.

²A decrease of 10.4% implies that medical marijuana legalization saved approximately 36,500 lives between 1997 – when the first law took effect in California – and 2023, accounting for 30% of the national decline in traffic fatalities per capita over that period. In 2023 alone, the estimated impact is 3,175 lives, with an additional 1,400 lives that could have been saved if medical marijuana had been legalized nationally. The total life-saving effect of national legalization in 2023 could be valued at \$60.5 billion or 170,000 years of potential life saved, based on the Department of Transportation’s value of statistical life and life expectancy of 80 years.

assumption and a negative bias in treatment coefficients. Notably, the pre-treatment differences persist even when state-specific linear trends are included in the Two-Way Fixed Effects (TWFE) model, reflecting the fact that those trends are estimated on the entire sample period.

I address this issue with the Imputation Procedure (IP) developed by Borusyak et al. (2024). In the IP, state-specific trends are estimated using only not-yet-treated observations and then subtracted from the post-treatment outcomes, which properly accounts for pre-existing differences. The IP estimates are drastically different from Anderson et al. (2013)’s main result. Under the assumption that state income and other included variables are sufficiently unrelated to the timing of MML adoption, the estimated effect on traffic fatalities is zero. Without this assumption, the estimate suggests that MMLs *increase* the rate of traffic fatalities by 12%. According to the pre-trends test, these two specifications are the only ones in which legalizing states and their control groups exhibit parallel trends in traffic fatalities before treatment.

I also show that the average state effect differs substantially from the average individual effect estimated by the population-weighted regression in Anderson et al. (2013). This indicates a high heterogeneity in treatment effects across states. Ignoring this heterogeneity masks important state-level variation and makes a single aggregate estimate difficult to interpret and potentially misleading. To address this, I explicitly estimate state-specific treatment effects using both TWFE and IP methods and compare these estimates to the measure of pre-trends for each state. I find that states with larger estimated negative effects also tend to exhibit greater pre-existing differences relative to the control group.

California – the first state to implement a MML – plays a particularly important role, accounting for over 50% of the population-weighted estimate. This state simultaneously has one of the largest estimated negative effects on traffic fatalities and one of the largest pre-existing linear differences relative to the rest of the states. California is also responsible for much of the discrepancy between methods, as I show below.

Pinning down the effect of marijuana legalization is important. While 73% of the U.S. population currently lives in states with some legal access (medical or recreational), marijuana remains fully illegal in 12 states and at the federal level. The debates are ongoing, with Nebraska enacting a MML

and three other states rejecting a RML in November of 2024. Figure A1 displays the evolution of the number of states with MML and RML, and Table A1 provides the dates when legalizations went into effect.

While medical laws that legalize acquisition and possession only for select patients can be studied as a policy in itself, much of the literature considers them a scaled-down version of full legalization. First, there is likely to be some bleeding of the medical market into recreational use (Chu, 2014; Pacula et al., 2015). Second, papers such as Anderson et al. (2013) were written before any state introduced a RML and had to rely on MMLs as the best available setting to make empirical conjectures about any liberalization of the access to marijuana. Third, it is probable that MMLs are used as political stepping-stones for full legalization (Kilmer and MacCoun, 2017).

Supporters of increased access cite the beneficial properties of marijuana for pain management (Powell et al., 2018; Abouk et al., 2023) and other conditions (Fraguas-Sánchez and Torres-Suárez, 2018). In the public safety domain, legalization can reduce violent crimes related to illegal markets (Morris et al., 2014; Gavrilova et al., 2019; Dragone et al., 2019) and help control product quality to prevent poisoning (Jameson et al., 2022). Full legalization can also reduce expenses on law enforcement, while both MMLs and RMLs provide additional taxation revenue from newly formed marijuana markets.³

Conversely, opponents of legalization argue that marijuana can be harmful and addictive, and lead to consumption of more dangerous drugs (Secades-Villa et al., 2015). As a mind-altering substance, marijuana can also lead to socially-unacceptable or even destructive behavior, including driving under the influence. Special attention is given to the effect of legalization on teenage use, which is always illegal (Anderson et al., 2015; Smart and Doremus, 2023). Anderson and Rees (2023) provides a comprehensive overview of the literature studying the effect of medical marijuana legalization on various outcomes.

The effect of legalization on traffic fatalities takes a prominent place in this discussion. Motor vehicle accidents are the third leading cause of unintentional injury-related death in the US and the primary

³An estimated \$3.7 billion was collected in taxes in 2021 among states that have some degree of legalization. Source: <https://www.mpp.org/issues/legalization/cannabis-tax-revenue-states-regulate-cannabis-adult-use>

cause for people under 27, with a death toll of 43,273 in 2023. A third of all deaths involves a driver with alcohol present in their blood, while testing for other substances is limited.⁴ A natural concern is that legalization of marijuana, even if only limited to certain patients, exacerbates this issue. Wider availability of the substance may lead to an increase in the prevalence of intoxicated drivers and, therefore, an increase in the number of fatal traffic accidents.

Despite this intuition, Anderson et al. (2013) and subsequent studies report a negative effect of MMLs on the rate of traffic fatalities. The proposed mechanism suggests that consumers of marijuana switch away from alcohol and take fewer trips or drive more safely under the influence. This mechanism is supported by a decrease in alcohol sales, also documented in Anderson et al. (2013). The only previous paper contradicting this finding is Smart and Doremus (2023), which shows a significant positive effect for young drivers and for all age groups during nighttime and weekends. The effect on alcohol-involved fatalities remains negative, consistent with Anderson et al. (2013). Unlike Smart and Doremus (2023), I utilize the same time period and identification strategy as Anderson et al. (2013) to demonstrate that the effect of MMLs on traffic fatalities is either positive or zero when a more robust empirical model is used. These findings help reconcile conflicting estimates of the effect of MMLs in the prior literature, as well as the opposing conclusions regarding medical and recreational marijuana laws. My analysis suggests that both types of laws may increase traffic fatalities, and that the negative estimate reported by Anderson et al. (2013) is likely spurious.

The rest of the paper is organized as follows. Section 2 presents the empirical framework. Section 3 introduces the data I use. Section 4 presents the result of my estimation. Section 5 concludes.

⁴The total number of unintentional injury-related deaths in 2023 was 222,698, of which the leading causes were drug poisoning (97,231) and falls (47,026). Source: <https://wisqars.cdc.gov/explore>. Alcohol statistics are from own calculations and <https://www-fars.nhtsa.dot.gov/>.

2 Estimation

2.1 Two-Way Fixed Effects

I begin by replicating Anderson et al. (2013)’s main specification that estimates the effect of MMLs on traffic fatalities. It is done with a standard Two-Way Fixed Effects (TWFE) model:

$$y_{st} = \beta^{TWFE} MML_{st} + \gamma_x X_{st} + \gamma_s + \gamma_t + \varepsilon_{st}, \quad (1)$$

where y_{st} is the logarithm of the number of traffic fatalities per 100,000 people in state s in year t ; MML_{st} is a continuous indicator of “treatment”, equal to the share of year y when medical marijuana was legal in state s ; X_{st} is a set of covariates that may include state-specific linear trends; and γ_s, γ_t are state and year fixed effects. In most of my analysis beyond replication, I use a binary version of MML_{st} .

There are two sets of potential issues with equation (1). For a causal interpretation of β^{TWFE} , the conditional parallel trends and no-anticipation assumptions need to hold. A direct anticipatory effect of legalization on traffic fatalities through the mechanism proposed by Anderson et al. (2013) seems unlikely. While the parallel trends assumption is fundamentally non-verifiable, its plausibility can be assessed indirectly by testing for “pre-trends” – violations of parallel trends in the pre-treatment period. I show that large pre-trends are an issue in this case, even after the inclusion of state-specific linear trends in (1). The implied violation of the conditional parallel trends assumption suggests that the TWFE estimates might be spurious. A proper accounting for the pre-trends makes the negative effect disappear.⁵

Additionally, the inclusion of time-varying covariates in (1) requires the exogeneity assumption to hold for each of them (Caetano et al., 2022; Powell, 2022). For a large set of variables, as in Anderson et al. (2013), this assumption is implausible. If, for example, a state tightens traffic laws concurrently with legalizing medical marijuana, this assumption is likely violated. I show that in

⁵Figures 1 and 2 in Anderson et al. (2013) show that MML states have decreasing traffic fatalities relative to non-MML states even before the legalization date. While Table 6 of the same paper shows the insignificance of placebo coefficients for three years before legalization, this is done only for the state-specific trends equation, which produces an insignificant treatment effect.

most cases, the addition of covariates to the TWFE or the alternative estimate, which requires a weaker assumption of unconfoundedness, does not change the result significantly. However, in the specification that appropriately controls for pre-trends, the inclusion of covariates is influential.⁶

The other set of issues has to do with the weighting of the underlying estimates and the interpretation of the “average” treatment effect. It is well known that the TWFE may fail to recover a meaningful aggregate metric when treatment effects are heterogeneous and timing is staggered, due to implicit weighting and the inclusion of “forbidden comparisons”.⁷ In the context of MMLs, this problem was acknowledged by Chen and French (2023). They apply a more sophisticated estimator and find a substantially smaller negative effect of MMLs on traffic fatalities over the period 1990–2019. For the period used in Anderson et al. (2013), I find that implicit weighting and forbidden comparisons have little impact on the results.

A separate issue is how underlying treatment effects are aggregated across states. Any aggregate estimate masks the underlying heterogeneity of unit-specific treatment effects. Large differences in legal provisions across state MMLs were first noted by Pacula et al. (2015) and later used by Smart and Doremus (2023) in their identification strategy. Discrepancies in enforcement, compliance, and other factors could further exacerbate the differences in state responses to MMLs. My state-by-state estimation explicitly shows the substantial heterogeneity across state treatment effects.⁸

Anderson et al. (2013) weight observations by population, which places more importance on larger states in the empirical estimation. This is commonly used to estimate the effect of interest on an *individual* in a legalizing state, whereas an unweighted regression can be used to estimate the average effect in a legalizing state ((Angrist and Pischke, 2009, Chapter 3.4); Solon et al. (2015)). With high heterogeneity across states, population weighting effectively selects specific states, such

⁶Anderson et al. (2013) partially address this concern by reporting several regressions of treatment on covariates. Most of the coefficients turn out to be insignificant, although this is reported only with the inclusion of state-specific trends, which makes the main effect insignificant as well. The outcome without covariates is not reported.

⁷Forbidden comparisons arise when earlier-treated units are used as controls for later-treated ones, causing their treatment effects to enter the aggregate estimate with a negative weight. More broadly, the weights on unit-specific treatment effects are determined implicitly by treatment timing, rather than being explicitly specified. For detailed discussions of these problems, see, for example, de Chaisemartin and D’Haultfœuille (2020); Callaway and Sant’Anna (2021); Goodman-Bacon (2021); Sun and Abraham (2021); Wooldridge (2021); Borusyak et al. (2024)

⁸Using data from 1985 to 2014, Santaella-Tenorio et al. (2017) report state-by-state effects of MMLs on traffic fatalities. Their findings are consistent with mine, including the substantial heterogeneity across state-specific estimates. However, they do not discuss the implications of this heterogeneity for the aggregate effect.

as California, to drive the aggregate estimate. For this reason, the choice of weighting is very influential for the resulting estimate. While this does not necessarily invalidate Anderson et al. (2013)’s main finding, the interpretation of the weighted average is complicated and might not be policy-relevant.

2.2 Imputation Procedure

As an alternative to the TWFE, I use the Imputation Procedure (IP) developed by Borusyak et al. (2024). It was primarily devised to overcome the issue of forbidden comparisons but has other attractive properties as well. The idea behind the IP is very intuitive. The first stage imputes counterfactual behavior of each unit using only information from untreated observations. In the second stage, the treatment effect for each unit-by-period pair is defined as the difference between the observed outcome and the counterfactual prediction from the first stage. Finally, the average treatment effect is computed by averaging over all unit-by-period treatment effects, ensuring only valid comparisons drive the estimate:

$$(s, t : MML_{st} = 0) : y_{st} = \gamma_x X_{st} + \gamma_s + \gamma_y + \varepsilon_{st} \quad (2a)$$

$$(s, t : MML_{st} = 1) : \tau_{st} = y_{st} - \hat{\gamma}_x X_{st} - \hat{\gamma}_s - \hat{\gamma}_y \quad (2b)$$

$$\beta^{IP} = \sum_{s,t} \tau_{st} / \sum_{s,t} \mathbb{1}\{MML_{st} = 1\}. \quad (2c)$$

Borusyak et al. (2024) provide an inference method for this estimator and show that, given conditional parallel trends and no anticipation assumptions, it is unbiased and efficient under staggered timing and heterogeneous treatment effects.⁹

Compared to the TWFE, the IP changes weighting of the underlying period-by-unit treatment effects. This resolves the issue of forbidden comparisons and makes the aggregate estimate mean-

⁹Wooldridge (2021) and Lee and Wooldridge (2023) independently develop similar estimators called the Extended TWFE (ETWFE) and the Rolling Method. With common timing and time-constant covariates, all three methods are algebraically equivalent. With staggered timing and time-constant covariates, ETWFE is equivalent to the IP. In all other cases, they are numerically close according to previous evidence. I chose the IP over the other two estimators primarily because it allows for time-changing covariates used in Anderson et al. (2013). On the other hand, Wooldridge (2021) more explicitly discusses the inclusion of state-specific trends, which are important for my analysis.

ingful. The IP also differs in how the effect of covariates is estimated. Using only not-yet-treated observations to estimate γ_x avoids the treatment effect contamination. If, for example, the evolution of state income affects both driving and the likelihood of marijuana legalization, the inclusion of income in the TWFE will bias treatment estimate. In contrast, if income itself is not affected by MML, its inclusion in the IP is valid. This allows one to relax exogeneity of X_{st} in favor of unconfoundedness, which requires only random treatment assignment conditional on pre-treatment observable variables (Powell, 2022; Caetano et al., 2022). Since this assumption might still be too strong for a large number of covariates included, I report estimates with and without the inclusion of controls.¹⁰

An important application of this is the inclusion of state-specific linear trends $\gamma_s \times t$ in X_{st} . When these trends are included in the TWFE, they are estimated from the whole sample, made up of both control and treated periods for each state. With a non-constant treatment effect, the slope in outcomes changes with treatment. Thus, $\gamma_s \times t$ captures a weighted average of pre- and post-treatment slopes, and as a result both fails to control for pre-treatment differential trends and biases the estimated average treatment coefficient. With the IP, it is possible to control for pre-existing linear differences exactly. State-specific linear trends $\gamma_s \times t$ are estimated using only untreated observations, thus their inclusion captures any linear differences in outcomes before treatment. The treatment effect is estimated on the residuals, after these trends are taken into account. While the TWFE and the IP differ by both the underlying weighting and the inclusion of controls, it is how they deal with that pre-trends that will drive the difference in the estimates between them.

2.3 Testing for Pre-Trends

The validity of the parallel trends assumption, necessary for the causal interpretation of a Difference-in-Differences estimate, is often associated with the absence of a pre-trend. If the treated and control groups exhibit parallel trends in the outcome variable before treatment, it is plausible they would have continued to do so without an intervention. Conversely, if the gap between the groups was already changing before treatment, there is little reason to expect it would have stabilized

¹⁰This idea of estimating the effect of covariates on untreated observations only predates the Imputation Procedure. See, for example, the Regression Adjustment in (Wooldridge, 2010)

thereafter.

A standard way of formally asserting the absence of a pre-trend is through individual or joint testing of pre-treatment dummy coefficients. In addition to statistical power issues (Roth, 2022; Borusyak et al., 2024), this approach ignores the order of pre-treatment periods when considering their pairwise differences. It is possible that none of the pre-treatment dummy coefficients is significant, but their magnitudes are changing at a statistically-significant rate.

When estimating an aggregate effect of a staggered and heterogeneous treatment, an additional issue arises. Earlier-treated cohorts receive greater weight in the estimation of post-treatment effects but lower weight in the estimation of pre-treatment coefficients. As a result, a scenario in which all post-treatment differences are driven by pre-treatment trends can still pass the test for zero pre-treatment coefficients, if earlier-treated cohorts exhibit pre-trends of larger magnitude. In my testing for pre-trends, I therefore explicitly estimate the difference in linear trends between treatment and control for each legalizing state.

Borusyak et al. (2024) propose a simple framework that can be used, in particular, to test for linear differential pre-trends. Testing for whether state i exhibits a pre-trend relative to the control group boils down to a simple regression on the not-yet-treated:

$$(s, t : MML_{st} = 0) : y_{st} = \gamma_x X_{st} + \gamma_s + \gamma_y + \kappa_i \mathbb{1}\{s = i\} \times t + \varepsilon_{st}. \quad (3)$$

Here, κ_i identifies the slope of the differential linear pre-trend. If it is significantly different from zero, the existing differential trend likely biases the estimate of the treatment effect. Note that mechanically, κ_i is exactly zero if state-specific linear trends are included in X_{st} . This underscores that in the IP framework, state-specific linear trends are able to control for pre-trends.

For the TWFE, testing on not-yet-treated observations only is not informative. Pre-trends are identified simultaneously with the treatment effect in the main regression and depend on the post-treatment period via the coefficients on covariates, γ_x . For a comparable testing, I modify Borusyak et al. (2024)’s procedure. First, the residuals from the state-specific TWFE regression are calcu-

lated: $\tilde{y}_{st} = \varepsilon_{st}$. Second, a pre-treatment differential trend for state i is estimated:

$$(s, t : MML_{st} = 0) : \tilde{y}_{st} = \gamma_y + \gamma_s + \kappa_i \mathbb{1}\{s = i\} \times t + \varepsilon_{st}. \quad (4)$$

Without covariates in the TWFE estimation, this procedure is equivalent to testing via Borusyak et al. (2024). With covariates or state-specific linear trends, it allows one to examine whether their inclusion in the main regression eliminates the pre-trends.

3 Data

My data follows Anderson et al. (2013) as closely as possible, using only the period from 1990 to 2010. The dependent variable is the logarithm of traffic fatalities (defined as deaths involving at least one motor vehicle) per 100,000 population, aggregated at the state-by-year level. Treatment is represented by the medical marijuana legalization variable (MML). For replication, I use a continuous version of this variable, equal to the share of the year during which medical marijuana was legal in a given state. In all other models, this is replaced with a binary indicator equal to 1 if MML was in effect for the majority of the year. Legalization dates are reported in Table A1, of which only those occurring before 2011 are included in the analysis.

Covariates used in Anderson et al. (2013) can be grouped into three categories. The demographic group includes mean age, the unemployment rate, and the logarithm of real income per capita. Driving-related variables include annual miles traveled per licensed driver and indicators for the following policies: graduated driver licensing law, primary and secondary seat belt laws, the existence of speed limits above 70 mph, texting and handheld device bans, .08 BAC law, administrative license revocation, zero-tolerance underage drunk-driving law, and drug per se law. The substance-related category includes the state-level beer tax and an indicator for marijuana decriminalization. A full list of variables and their data sources is provided in Table A2 in the Appendix. All data and code used in this analysis are publicly available in the replication package [dataset]Braslavskiy (2025).

Including a large number of covariates is uncommon, as any one of them may violate the exogeneity

assumption. When estimating equation (1) with the full set of controls, only four covariates have statistically significant coefficients: the logarithm of real income per capita, miles traveled per licensed driver, speed limit above 70 mph, and the hands-free law. Among these, income, miles traveled, and the hands-free law have opposite signs of correlation with the treatment and the outcome at the state-by-year level (all correlation coefficients are in Table A2). This suggests that including these variables may introduce negative bias and threaten the causal interpretation of the treatment effect. For example, states that see a persistent increase in income relative to the average might be more likely to legalize medical marijuana and simultaneously see a decrease in traffic fatalities due to higher urbanization. Alternatively, states that legalize medical marijuana may be more likely to implement a handheld ban to thwart a potential increase in fatalities. Since the exogeneity or unconfoundedness assumptions cannot be verified, I report specifications both with and without the inclusion of covariates.

4 Results

4.1 Aggregate Estimates

Panel A of Table 1 presents my replication of Anderson et al. (2013). Despite independently collecting all covariates and having to resolve several ambiguities, Columns (2) and (4) closely replicate their main results: $-.112$ (.030) and $-.090$ (.063) in my replication, compared to $-.110$ (.030) and $-.098$ (.065) in Anderson et al. (2013). To distinguish more clearly between pre- and post-MML periods in each state, in subsequent analysis I transform the MML variable into a binary indicator. This modification lowers the number of legalizing states by two, as D.C. and New Jersey enacted their MMLs in November of 2010 and thus are not treated in the binary sense.¹¹

Panel B of Table 1 differs from Anderson et al. (2013) only in the use of the binary treatment indicator. This does not change the results drastically, but decreases their magnitude somewhat. As in Anderson et al. (2013), the inclusion of state-specific linear trends both attenuates the estimates

¹¹D.C. and New Jersey do not hold a large weight in the original estimation due to a short (1 year out of 21) post-treatment period. Unsurprisingly, their exclusion from the estimation in (1) does not change the treatment estimate in any substantial way.

and substantially increases standard errors, rendering the coefficients insignificant in Columns (3) and (4). To the extent that state-specific trends make the parallel trends assumption more plausible, these estimates are less biased.

Table 1: Aggregate Effect of Medical Marijuana Laws on Traffic Fatalities.

	(1)	(2)	(3)	(4)
Continuous Treatment (Replication)				
<i>A. Two-Way Fixed Effects, Weighted by Population</i>				
β^{TWFE}	-.119*** (.023)	-.112*** (.030)	-.073 (.048)	-.090 (.063)
Binary Treatment				
<i>B. Two-Way Fixed Effects, Weighted by Population</i>				
β^{TWFE}	-.113*** (.023)	-.106*** (.029)	-.063 (.046)	-.079 (.060)
<i>C. Imputation Procedure, Weighted by Population</i>				
β^{IP}	-.123*** (.017)	-.105*** (.018)	.113*** (.044)	.004 (.047)
<i>D. Two-Way Fixed Effects, No Weighting</i>				
β^{TWFE}	-.074** (.031)	-.051* (.026)	.022 (.035)	.007 (.042)
Covariates		X		X
State-specific linear time trends			X	X

Panels A, B, and D implement a standard Two-Way Fixed Effect design with state and year fixed effects. Panel C uses the Imputation Procedure from Borusyak et al. (2024). The dependent variable is the logarithm of the number of fatalities per 100,000 population. In Panel A, the independent variable is the share of each state-year when medical marijuana is legal. In Panels B through D, the independent variable is equal to one if medical marijuana was legalized before June 30 of the current year, and zero otherwise. Years 1990-2010 and 50 states plus D.C. are included. Standard errors clustered at the state level are in parenthesis: * p<0.05, ** p<0.01, *** p<0.001

Panel C of Table 1 presents comparable aggregate estimates using the Imputation Procedure (IP) by Borusyak et al. (2024). The estimates in Columns (1) and (2) – which exclude state-specific linear trends – are very similar across the two methods. This suggests two things. First, in this setting, forbidden comparisons and other weighting issues associated with the TWFE estimator are negligible. Second, estimating the effect of covariates using only not-yet-treated observations and

thus relaxing the exogeneity assumption does not have a substantial effect, at least in the absence of state-specific trends.¹²

In contrast, including state-specific linear trends in the IP leads to dramatically different conclusions. Without covariates (Column 3), the estimated effect is strongly positive, suggesting a 12% *increase* in traffic fatalities following MML adoption. When both covariates and state-specific trends are included (Column 4), the estimate is a precisely estimated zero. The key distinction between the TWFE and IP estimators in Columns (3) and (4) is in whether the included state-specific trends are estimated on the whole period or on not-yet-treated observations only. While differences in weighting could also contribute to the divergence between the TWFE and IP results, this is not supported by the similarity of estimates without state-specific trends (Columns 1 and 2).

When different specifications (both between Panels B and C and between columns in Panel C) lead to such drastically different results, it is reasonable to trust the ones with the more plausible underlying assumptions. As I will show below, TWFE specifications are not able to deliver parallel trends between treatment and control before MML, meaning that the parallel trends assumption in latent outcomes is unlikely to hold either. The same is true for the IP without state-specific trends. In the TWFE with state-specific trends it is a result of the trends being identified from the entire sample period, effectively controlling for a combination of pre- and post-treatment slopes. In contrast, when linear trends are included in the IP, they are estimated on the not-yet-treated observations only and thus control for the pre-trends.

Therefore, judging by the plausibility of the parallel trends assumption, the estimates in Columns (3) and (4) of Panel C are the preferred estimates. To determine which one of these is closer to the true effect, the assumptions behind covariates inclusion must be considered. Income is the most influential in driving the difference between these estimates: when only log real income and state-specific trends are included, the coefficient on MML is 0.045 (0.035). If income satisfies unconfoundedness, the effect of MMLs on traffic fatalities is zero. If, for example, economic growth

¹² As a robustness check, I restrict the sample to bordering states only and apply the synthetic control method of Arkhangelsky et al. (2021), but neither approach improves upon the TWFE. Using bordering counties with pair fixed effects (Dube et al., 2010) produces a better pre-treatment fit but drives the estimates toward zero, possibly due to cross-border contamination.

leads to adoption of marijuana laws or legalization causes higher growth, the effect is more likely to be large and positive, as in Column (3).¹³

While the discussion so far dealt with the internal validity of Anderson et al. (2013)’s estimate, there are reasons to doubt its external validity as well. Under weighting by population, as in Panels A through C of Table 1, more importance is assigned to larger states. This is justified if the interest is in the ex-post effect on the average *person* affected by the law, but might be less policy-relevant than the average state effect.

In panel D of Table 1, I report unweighted TWFE estimates. These differ substantially from the weighted results, ranging from a 5% decrease in fatalities to a statistically insignificant increase when state-specific linear trends are included. This sensitivity to weighting suggests heterogeneity in state-specific treatment effects. Heterogeneity is important both for interpreting the aggregate effect and assessing its validity. As shown below, one state – California – has an outsized influence on the weighted estimate and accounts for much of the difference between the TWFE and the IP results.

4.2 State-by-State Estimation

The aggregate estimation using either method masks the underlying heterogeneity of state-specific treatment effects of MMLs on traffic fatalities. To examine this heterogeneity, I estimate each model on a restricted sample that includes all not-yet-treated states and one treated state at a time, without applying population weights. For each of the 13 regressions, I also conduct an empirical test for differential pre-trends, as described in the Estimation section. The full set of state-specific coefficients is reported in Tables B1 and B2 in the Appendix, with their averages presented in Table B3. Averages weighted by mean population over 1990-2010 closely approximate population-weighted aggregate effects, suggesting that differences between Panels B and D in Table 1 arise from the relative importance of state-specific coefficients, not differences in the underlying estimates themselves. The similarity between average and aggregate estimates also suggests that forbidden

¹³This assumes that economic growth causes a decline in the rate of traffic fatalities, as found in Kopits and Cropper (2005).

comparisons do not substantially bias the aggregate TWFE estimates, as across-state averages are free from them.

State-specific treatment effects together with differential pre-trends are depicted in Figure 1a. Without the inclusion of covariates or state-specific trends, MMLs in most states are estimated to have a negative effect. However, most of them also exhibit a negative differential trend before the treatment. In fact, there is a substantial correlation between β_i and κ_i , suggesting that some of the estimated effects might be spurious and driven by differences before legalization. The inclusion of covariates and trends improves the measure of fit, manifested by a smaller horizontal dispersion of estimates. It also drives the simple average of the effects close to zero.

Yet, the inclusion of state-specific trends estimated on the whole period does not fully remedy pre-treatment differences, even in the linear sense. For instance, the population-average of κ_i from Figure 1a with covariates and state-specific trends included, is equal to -0.011 . This implies an 11% decline in traffic fatalities over ten years following legalization based solely on pre-treatment trends.¹⁴ Such decline is larger than the population-weighted average of estimated legalization effects, -0.096 . The presence of pre-trends reflects a key limitation of the TWFE: state-specific trends estimated on the full sample fail to fully account for pre-treatment differences if treatment affects the slope of the outcome.

Figure 1b shows the analogous estimation using the IP. The state-by-state IP without covariates or trends exactly corresponds to the TWFE and serves as a basis of comparison. The version with the inclusion of additional controls, however, is drastically different, as foreshadowed by the aggregate results. Out of 13 states, 8 are estimated to have a positive effect of MML on traffic fatalities. The simple average of their effects is positive, while the population-weighted one is close to zero. The pre-trends are exactly zero for each state, as a result of using linear trends estimated from not-yet-treated observations only. Judging by the pre-trends testing as an indicator of the parallel trends assumption validity, the IP specifications with state-specific linear trends are the most preferred estimators.

California plays an outsized role in the weighted average, and its inclusion is crucial for producing a

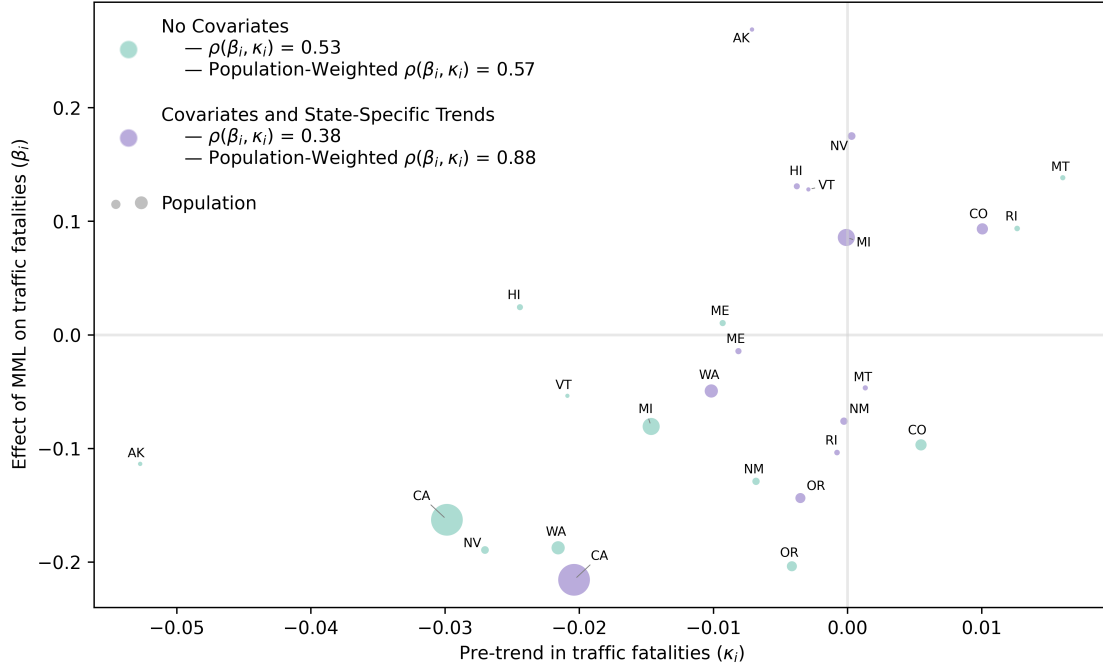
¹⁴The population-weighted average of the after-treatment length is 9.57.

negative aggregate effect. California’s population weight is over 50%, meaning that in 2010, about half of all individuals with access to medical marijuana lived in the state. Its estimated impact is among the largest in magnitude under both TWFE specifications. Consequently, when California is excluded, the aggregate population-weighted TWFE estimate with covariates yields a much smaller MML coefficient of -0.059 (0.025), or 0.031 (0.033) when state-specific linear trends are included.

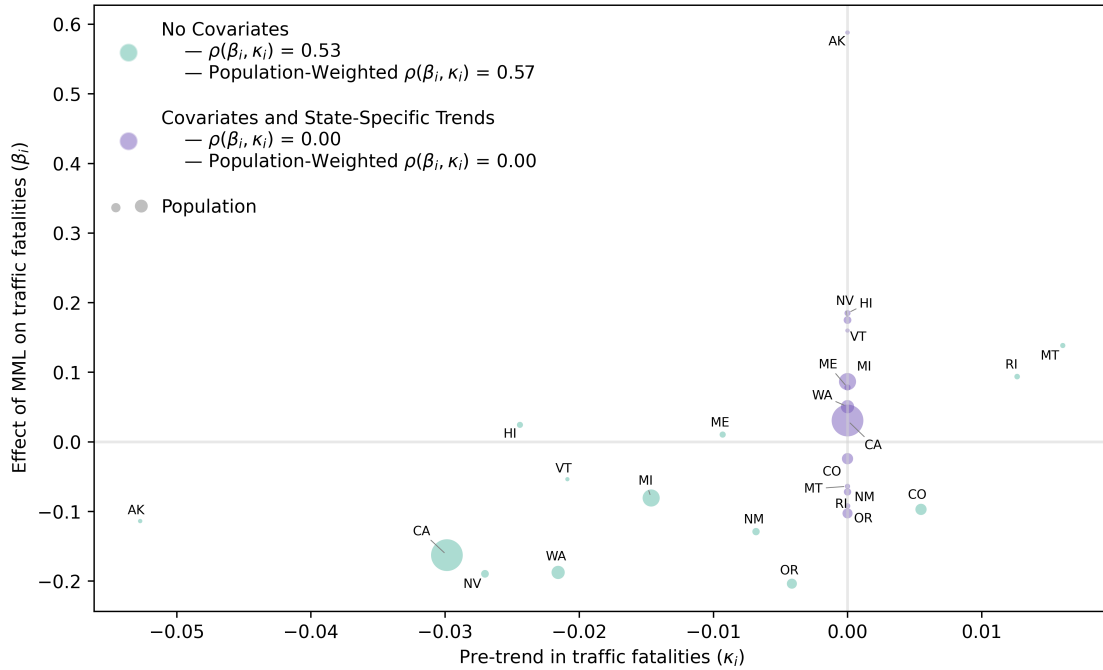
From an external validity perspective, placing so much weight on a single state is problematic: California is an outlier in many economic and demographic respects. It was the first state to legalize medical marijuana, and its MML remains one of the most lenient in the country (Smart and Doremus, 2023). There are also internal validity concerns in California’s case: it exhibits one of the largest negative pre-trends, suggesting downward bias in its treatment effect estimate. When this pre-trend is addressed using the IP specification, the effect of legalization on traffic fatalities in California becomes positive. I examine in detail why different specifications yield different conclusions for California in Section C of the Appendix.

Figure 1: State-by-State Estimation.

(a) Two-Way Fixed Effects



(b) Imputation Procedure



Each circle in the figure represents a pair of pre-trend (κ_i) and treatment (β_i) coefficients from a state regression using the TWFE or the IP. Circle size is proportional to the average population of that state over 1990-2010. The underlying numbers are in Tables B1 and B2.

5 Conclusion

Replicating a previously documented fact that medical marijuana legalization reduces traffic fatality rates, I show that this estimate is biased by the presence of pre-trends and averages highly heterogeneous underlying state effects. The Imputation Procedure developed by Borusyak et al. (2024), which accurately accounts for pre-trends, reveals either a positive or a precisely estimated zero effect. More than half of each population-weighted estimate is driven by the singular experience of California, which the original method estimates to have the largest negative treatment effect but also the steepest negative pre-trend. This study highlights the importance of accounting for pre-treatment differences and heterogeneity across adopters in policy evaluation. The nature of pre-legalization trends in the case of medical marijuana should be the subject of future analysis. States experiencing declining traffic fatalities may have been more likely to legalize medical marijuana, or a third factor may have influenced both the timing of MML adoption and fatality rates.

6 Notes

6.1 Glossary

Borusyak et al. (2024) Imputation Procedure (IP): A recent method for difference-in-differences analysis that uses explicit weighting of unit-by-time treatment effects and is able to control for linear pre-trends.

Medical Marijuana Laws (MMLs): State-level laws that legalize the use of marijuana for medical purposes.

Pre-Trends: Relative trends in the outcome variable prior to treatment adoption that can confound causal inference if not accounted for.

Two-Way Fixed Effects (TWFE): A regression method controlling for unit and time fixed effects, commonly used in policy evaluation.

6.2 Acknowledgments

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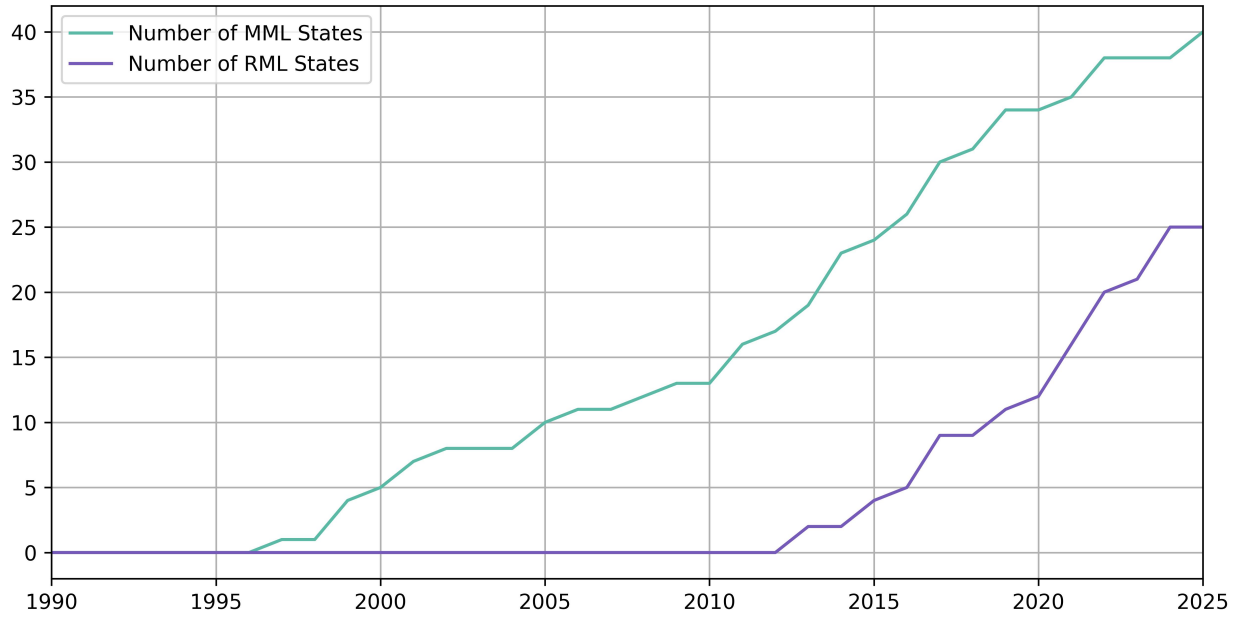
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A Data Description

Figure A1: History of Medical (MML) and Recreational (RML) Marijuana Laws.



This figure is constructed using legalization dates from Table A1. The year of legalization is determined as the same calendar year if legalization happened before June 30th, or the next year otherwise. This corresponds to the binary treatment indicator used in most specifications.

Table A1: Dates of MML and RML laws.

	MML Date	RML Date
AL	May 17, 2021	
AK	March 4, 1999	February 24, 2015
AZ	April 14, 2011	November 30, 2020
AR	November 9, 2016	
CA	November 6, 1996	November 9, 2016
CO	June 1, 2001	December 10, 2012
CT	October 1, 2012	July 1, 2021
DE	July 1, 2011	April 23, 2023
DC	July 27, 2010;	February 26, 2015
FL	January 3, 2017	
HI	December 28, 2000	
IL	January 1, 2014	January 1, 2020
KY	January 1, 2025	
LA	May 19, 2016	
ME	December 22, 1999	January 30, 2017
MD	June 1, 2014;	July 1, 2023
MA	January 1, 2013	December 15, 2016
MI	December 4, 2008	December 6, 2018
MN	May 30, 2014	August 1, 2023
MS	February 2, 2022	
MO	December 6, 2018	December 8, 2022
MT	November 2, 2004	January 1, 2021
NE	December 12, 2024	
NV	October 1, 2001	January 1, 2017
NH	July 23, 2013	
NJ	October 1, 2010	January 1, 2021
NM	July 1, 2007	June 29, 2021
NY	July 5, 2014	March 31, 2021
ND	April 18, 2017	
OH	September 8, 2016	December 7, 2023
OK	August 25, 2018	
OR	December 3, 1998	July 1, 2015
PA	May 17, 2016	
RI	January 3, 2006	May 25, 2022
SD	July 1, 2021	
UT	December 3, 2018	
VT	July 1, 2004	July 1, 2018
VA	July 1, 2021	July 1, 2021
WA	November 3, 1998	December 6, 2012
WV	July 5, 2017	

Dates prior to 2020 are from Chen and French (2023) and verified against Anderson et al. (2013). Dates from 2020 to March, 2025 are added using state websites and the Marijuana Policy Project.

Table A2: Description of Variables Used in the Analysis.

	Mean	SD	ρ_{MML}	ρ_{fat}
Log Fatalities per 100,000	2.620	0.351	-0.264	1.000
MML[continuous]	0.130	0.334	1.000	-0.264
MML[binary]	0.129	0.335	0.998	-0.258
Mean Age	35.934	1.666	-0.101	-0.203
Unemployment	5.874	1.875	0.188	-0.239
Log Real Income per Capita	10.282	0.156	0.274	-0.775
Miles Driven per Licensed Driver	14.128	2.046	-0.076	0.573
Graduate Driver-Licensing Law	0.557	0.490	0.273	-0.197
Primary Seat Belt Law	0.461	0.494	0.301	-0.287
Secondary Seat Belt Law	0.518	0.494	-0.284	0.270
≥ 70 mph Speed Limit	0.481	0.500	0.312	0.262
Texting Ban Law	0.045	0.199	0.201	-0.309
Hands-Free Law	0.063	0.237	0.147	-0.460
.08 BAC Law	0.584	0.485	0.312	-0.148
Administrative License Revocation Law	0.722	0.446	0.204	0.178
Zero-Tolerance Drunk-Driving Law	0.763	0.417	0.221	-0.222
Drug Per Se Law	0.131	0.334	-0.086	-0.093
Beer Tax	0.254	0.203	0.017	0.375
Marijuana Decriminalization	0.342	0.475	0.387	-0.277

Correlations with the dependent variable and the continuous version of the treatment indicator are calculated at the state-by-year level and weighted by population. All law indicators (except for the binary version of MML) are continuous variables representing the share of the year during which the law was in effect. Sources are as follows. Fatalities are from the Fatality Analysis Reporting System (FARS). Mean age is calculated from Census data; the unemployment rate comes from the Bureau of Labor Statistics; and income is obtained from the Bureau of Economic Analysis and converted to 2000 dollars. Annual vehicle-miles per licensed driver are calculated using data from the Federal Highway Administration. Information on graduated driver licensing laws (whether a required learning stage is present) and on primary and secondary seat belt laws comes from Dee et al. (2005) and the Insurance Institute for Highway Safety (available via this archived link: https://web.archive.org/web/20120229201440/http://www.iihs.org:80/laws/pdf/gdl_effective_dates.pdf). The indicator for speed limits above 70 mph is based on FARS. Data on texting and handheld device bans are from McCartt (2014) and HandsFreeInfo.com. Information on .08 BAC laws is from Freeman (2007); on administrative license revocation laws, from Fell and Scherer (2017); on zero-tolerance underage drunk-driving laws (whether underage drivers are permitted any measurable blood alcohol content), from the National Highway Traffic Safety Administration (NHTSA); and on drug per se laws, from norml.org. Beer tax is calculated using the Brewers Almanac by the Beer Institute, and marijuana decriminalization status is sourced from norml.org.

B Additional Results

To recover state-specific treatment effects, I perform a separate estimation for each legalizing state, denoted by i , restricting the sample to not-yet-treated states and the state i itself. While state-specific treatment coefficients are unbiased under standard assumptions, the statistical inference with only one treatment unit is invalid (Borusyak et al., 2024). I therefore abstain from reporting significance levels for these coefficients, since standard errors are likely biased downwards. The magnitude of the coefficients, however, is still useful for understanding the components of the aggregate estimate.

In addition to analyzing the heterogeneity of the whole set of state treatment effects β_i^{TWFE} and β_i^{IP} , I estimate the average of these effects:

$$\begin{aligned}\beta_a^{TWFE} &= \left(\sum_{i=1}^{13} p_i \beta_i^{TWFE} \right) / \left(\sum_{i=1}^{13} p_i \right), \\ \beta_a^{IP} &= \left(\sum_{i=1}^{13} p_i \beta_i^{IP} \right) / \left(\sum_{i=1}^{13} p_i \right),\end{aligned}$$

where p_i are either average population of state i over 1990-2010 or equal to one for an unweighted estimate. The numerical difference between these averages and aggregate estimates, both presented in Table B3, turns out to be minimal. This relation is not guaranteed ex-ante. The absence of substantial wedge is a result of several elements that could have been driving the difference not being influential in this application. First, state-specific regressions are not population-weighted, even when the average is. Second, weighting of state-by-year treatment effects is different. For the IP, state-by-state approach is equivalent to changing (2c) to averaging first across periods for each state and then across states, which this does not place more weight on early adopters. For the TWFE, the explicit weighting additionally resolves the issue of forbidden comparisons. Finally, for the TWFE, the effect of covariates is estimated on a different sample between the aggregate estimate and the state average. For the IP, in both cases it is estimated on not-yet-treated only. Note that β_a^{TWFE} and β_a^{IP} have the same weighting and only differ with respect to how the effect of covariates is estimated.

Table B1: Two-Way Fixed Effects State-by-State Estimation.

	Population Weight	(1)		(2)		(3)		(4)	
		β_i	κ_i	β_i	κ_i	β_i	κ_i	β_i	κ_i
AK	0.010	-0.114	-0.053	0.095	-0.036	0.258	-0.019	0.269	-0.007
		(0.016)	(0.002)	(0.051)	(0.002)	(0.012)	(0.002)	(0.020)	(0.002)
CA	0.507	-0.163	-0.030	-0.139	-0.013	-0.159	-0.032	-0.216	-0.020
		(0.015)	(0.003)	(0.024)	(0.003)	(0.012)	(0.003)	(0.023)	(0.003)
CO	0.064	-0.097	0.005	-0.105	-0.006	0.040	0.016	0.093	0.010
		(0.016)	(0.002)	(0.018)	(0.002)	(0.015)	(0.002)	(0.033)	(0.001)
HI	0.019	0.024	-0.024	0.115	-0.000	0.170	-0.012	0.131	-0.004
		(0.016)	(0.002)	(0.033)	(0.002)	(0.015)	(0.002)	(0.026)	(0.001)
ME	0.019	0.010	-0.009	-0.002	-0.009	0.023	-0.011	-0.014	-0.008
		(0.015)	(0.002)	(0.040)	(0.002)	(0.015)	(0.002)	(0.019)	(0.002)
MI	0.147	-0.081	-0.015	-0.036	-0.012	0.077	-0.000	0.086	-0.000
		(0.023)	(0.001)	(0.033)	(0.001)	(0.017)	(0.000)	(0.019)	(0.000)
MT	0.014	0.138	0.016	0.026	0.014	-0.003	0.001	-0.047	0.001
		(0.018)	(0.002)	(0.025)	(0.001)	(0.016)	(0.001)	(0.026)	(0.001)
NM	0.027	-0.129	-0.007	-0.156	-0.005	-0.052	-0.000	-0.076	-0.000
		(0.019)	(0.002)	(0.030)	(0.001)	(0.017)	(0.001)	(0.025)	(0.001)
NV	0.030	-0.190	-0.027	-0.054	-0.029	0.153	0.004	0.175	0.000
		(0.016)	(0.002)	(0.031)	(0.001)	(0.013)	(0.001)	(0.039)	(0.001)
OR	0.051	-0.204	-0.004	-0.101	-0.002	-0.161	-0.003	-0.144	-0.004
		(0.016)	(0.003)	(0.028)	(0.002)	(0.012)	(0.002)	(0.030)	(0.002)
RI	0.016	0.094	0.013	0.100	0.017	-0.040	-0.001	-0.104	-0.001
		(0.019)	(0.002)	(0.020)	(0.001)	(0.017)	(0.001)	(0.028)	(0.001)
VT	0.009	-0.054	-0.021	-0.092	-0.026	0.134	-0.004	0.128	-0.003
		(0.018)	(0.002)	(0.037)	(0.001)	(0.018)	(0.001)	(0.026)	(0.001)
WA	0.088	-0.188	-0.022	-0.105	-0.013	-0.022	-0.009	-0.049	-0.010
		(0.016)	(0.003)	(0.032)	(0.002)	(0.012)	(0.002)	(0.039)	(0.002)
Covariates				X				X	
St. Trends						X		X	

Each row is a state-specific estimate of the treatment effect β_i and the linear pre-trend κ_i using the TWFE. The way pre-trends are estimated is described in the Estimation section. In each of these regressions, the dependent variable is the logarithm of the number of fatalities per 100,000 population. The independent variable is equal to one if medical marijuana was legalized before June 30 of the current year, and zero otherwise. Observations from years 1990-2010 and all states are included in each regression as long as a) MML indicator is zero in that year (not-yet-treated), or b) it is the one legalizing state whose effect is being estimated. Population weight is calculated using the average population over 1990-2010. Standard errors clustered at the state level are in parenthesis. They are likely biased downward and thus significance levels are not reported.

Table B2: Imputation Procedure State-by-State Estimation.

	Population Weight	(1)		(2)		(3)		(4)	
		β_i	κ_i	β_i	κ_i	β_i	κ_i	β_i	κ_i
AK	0.010	-0.114	-0.053	0.256	-0.039	0.462	0.0	0.588	0.0
		(0.015)	(0.002)	(0.119)	(0.006)	(0.028)		(0.119)	
CA	0.507	-0.163	-0.030	-0.136	-0.014	0.177	0.0	0.031	0.0
		(0.015)	(0.003)	(0.023)	(0.006)	(0.036)		(0.076)	
CO	0.064	-0.097	0.005	-0.107	-0.005	-0.128	0.0	-0.024	0.0
		(0.015)	(0.002)	(0.017)	(0.003)	(0.022)		(0.039)	
HI	0.019	0.024	-0.024	0.116	-0.001	0.301	0.0	0.185	0.0
		(0.015)	(0.002)	(0.031)	(0.007)	(0.022)		(0.049)	
ME	0.019	0.010	-0.009	0.001	-0.010	0.135	0.0	0.078	0.0
		(0.014)	(0.002)	(0.039)	(0.005)	(0.024)		(0.036)	
MI	0.147	-0.081	-0.015	-0.036	-0.014	0.078	0.0	0.086	0.0
		(0.021)	(0.001)	(0.031)	(0.002)	(0.016)		(0.017)	
MT	0.014	0.138	0.016	0.025	0.016	-0.015	0.0	-0.064	0.0
		(0.017)	(0.002)	(0.024)	(0.003)	(0.017)		(0.027)	
NM	0.027	-0.129	-0.007	-0.156	-0.006	-0.050	0.0	-0.072	0.0
		(0.018)	(0.002)	(0.028)	(0.004)	(0.016)		(0.023)	
NV	0.030	-0.190	-0.027	-0.056	-0.030	0.110	0.0	0.175	0.0
		(0.015)	(0.002)	(0.029)	(0.003)	(0.019)		(0.040)	
OR	0.051	-0.204	-0.004	-0.099	-0.002	-0.128	0.0	-0.103	0.0
		(0.015)	(0.003)	(0.027)	(0.003)	(0.028)		(0.034)	
RI	0.016	0.094	0.013	0.102	0.018	-0.027	0.0	-0.092	0.0
		(0.018)	(0.002)	(0.019)	(0.002)	(0.017)		(0.028)	
VT	0.009	-0.054	-0.021	-0.093	-0.029	0.176	0.0	0.160	0.0
		(0.017)	(0.002)	(0.035)	(0.003)	(0.019)		(0.027)	
WA	0.088	-0.188	-0.022	-0.077	-0.009	0.067	0.0	0.050	0.0
		(0.015)	(0.003)	(0.034)	(0.006)	(0.028)		(0.034)	
Covariates		X				X			
St. Trends						X		X	

Each row is a state-specific estimate of the treatment effect β_i and the linear pre-trend κ_i using the IP. The way pre-trends are estimated is described in the Estimation section. In each of these regressions, the dependent variable is the logarithm of the number of fatalities per 100,000 population. The independent variable is equal to one if medical marijuana was legalized before June 30 of the current year, and zero otherwise. Observations from years 1990-2010 and all states are included in each regression as long as a) MML indicator is zero in that year (not-yet-treated), or b) it is the one legalizing state whose effect is being estimated. Population weight is calculated using the average population over 1990-2010. Standard errors clustered at the state level are in parenthesis. They are likely biased downward and thus significance levels are not reported.

Four estimates of the average treatment effect – $\beta^{TWFE}, \beta_a^{TWFE}, \beta^{IP}, \beta_a^{IP}$ – form a compelling set for analyzing the influence of different estimation choices on the outcome. β^{TWFE} and β^{IP} differ in two major ways: weighting of state-by-year treatment effects, including forbidden comparisons, and the set of observations used to estimate the effect of covariates. The difference between β^{TWFE} and β^{IP} can be decomposed into three parts: 1) the difference between β^{TWFE} and β_a^{TWFE} due to weighting, 2) the difference between β_a^{TWFE} and β_a^{IP} due to the way covariates are included, and 3) the difference between β_a^{IP} and β^{IP} due to slightly different weighting of state-by-year treatment effects. In this case, only the second component matters.

Table B3: Aggregate and Average Effect of Medical Marijuana Laws on Traffic Fatalities Using Different Methods.

	(1)	(2)	(3)	(4)
<i>A. Two-Way Fixed Effects, Weighted by Population</i>				
β^{TWFE}	-.113*** (.023)	-.106*** (.029)	-.063 (.046)	-.079 (.060)
β_a^{TWFE}	-0.134	-0.099	-0.067	-0.096
κ_a^{TWFE}	-0.021	-0.012	-0.017	-0.011
<i>B. Imputation Procedure, Weighted by Population</i>				
β^{IP}	-.123*** (.017)	-.105*** (.018)	.113*** (.044)	.004 (.047)
β_a^{IP}	-0.134	-0.093	0.108	0.039
κ_a^{IP}	-0.021	-0.012	0.000	0.000
<i>C. Two-Way Fixed Effects, No Weighting</i>				
β^{TWFE}	-.074** (.031)	-.051* (.026)	.022 (.035)	.007 (.042)
β_a^{TWFE}	-.073	-0.035	0.032	0.018
κ_a^{TWFE}	-0.014	-0.009	-0.005	-0.003
<i>D. Imputation Procedure, No Weighting</i>				
β^{IP}	-.091*** (.033)	-.018 (.046)	.109* (.060)	.097 (.068)
β_a^{IP}	-0.073	-0.020	0.089	0.077
κ_a^{IP}	-0.014	-0.010	0.000	0.000
Covariates		X		X
State-specific linear time trends			X	X

Each β_a and κ_a is an average of 13 coefficients. The dependent variable is the logarithm of the number of fatalities per 100,000 population. The independent variable is equal to one if medical marijuana was legalized before June 30 of the current year, and zero otherwise. Observations from years 1990-2010 and all states are included in each regression as long as a) MML indicator is zero in that year (not-yet-treated), or b) it is the one legalizing state whose effect is being estimated. The way pre-trends are estimated is described in the Estimation section. Panels A and B use average state population over the period to weight state treatment effects. Standard errors are not reported for the average estimates because they are likely biased for state-by-state estimates. All underlying estimates with standard errors can be found in Tables B1 and B2. For aggregate estimates, standard errors clustered at the state level are in parenthesis: * p<0.05, ** p<0.01, *** p<0.001.

C Fatalities in California

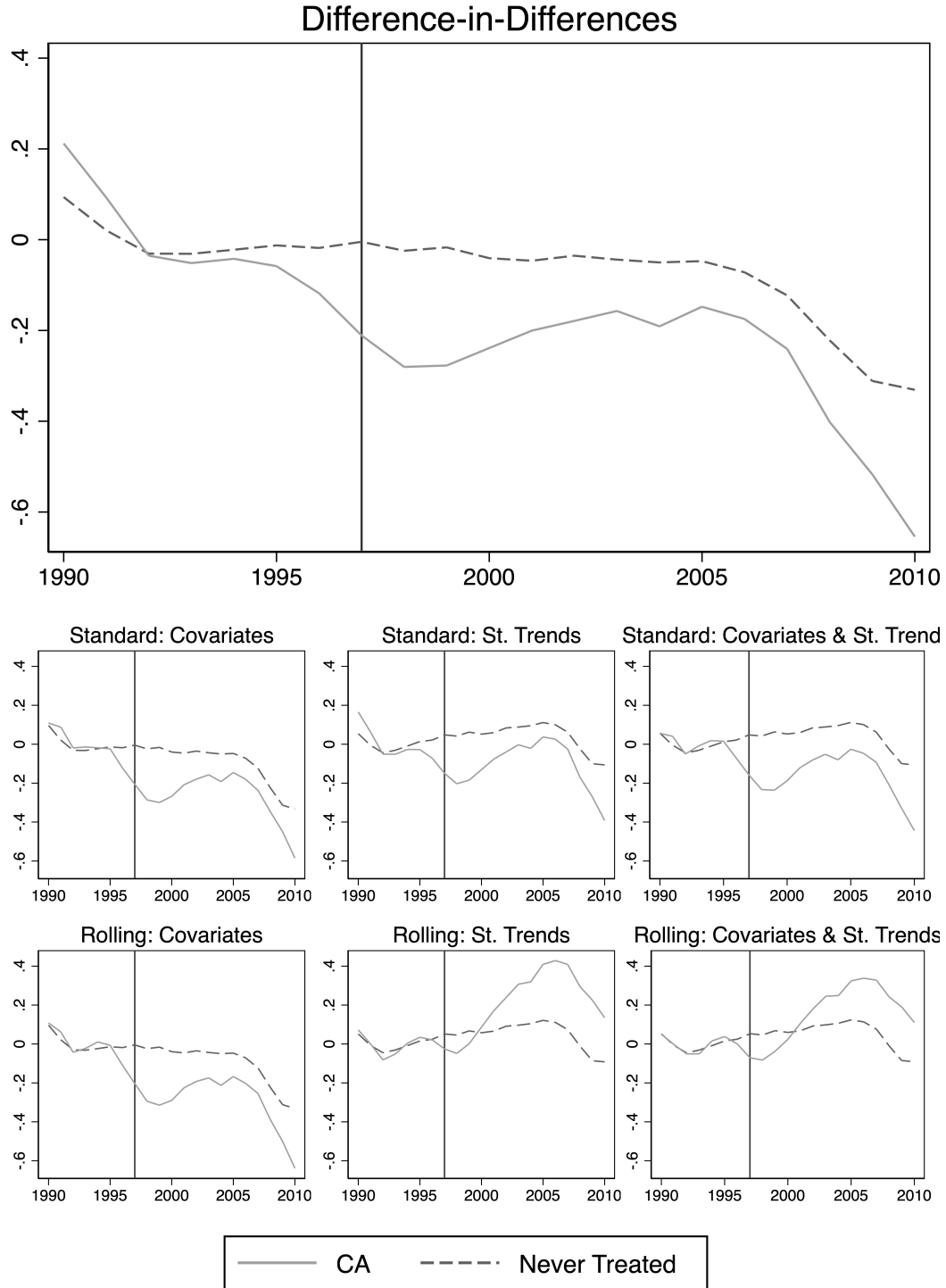
Using California as an example, I illustrate how the TWFE and IP transform the data, whether they adequately account for pre-existing differences, and why they may arrive at different conclusions. Figure C1 presents the residualized logarithm of traffic fatalities under each method, with the data transformed to resemble a standard Difference-in-Differences plot. For the TWFE, after estimating the regression (with or without covariates and state-specific trends), the treatment coefficient is added back to the post-treatment residuals. No such adjustment is needed for the IP, as the estimation procedure itself allows plotting of the residuals from (2a) estimated on not-yet-treated units. In each case, the estimated treatment effect β_i corresponds to the average of post-treatment points in the figure, while the pre-trend estimate κ_i is reflected in the slope of the line during the pre-treatment period.

The upper part of Figure C1 displays a plain Difference-in-Differences, which can be equivalently estimated using either the TWFE or the IP without covariates or state-specific trends. The figure shows that traffic fatalities in California were already declining relative to other states as early as 1990. This negative post-treatment difference can therefore be at least partially attributed to the continuation of this pre-existing trend. The TWFE model with only state-specific trends included does not properly adjust for this. This problem is especially pronounced for California, where the post-treatment period is much longer than the pre-treatment period, causing the estimated trend to be driven mostly by post-treatment outcomes.

The two specifications that include a full set of covariates do a better job of ensuring comparability between California and not-yet-treated states. Nonetheless, the pre-trend estimate κ_i remains negative and large, driven by a sharp decline in fatalities immediately before legalization. While this drop may have been indirectly related to the upcoming legalization—for instance, through stricter enforcement of traffic laws or informational campaigns—it cannot be attributed to the effect of the MML itself.

The introduction of state-specific trends in the IP, by contrast, produces a nearly perfect pre-treatment fit. Although these trends only match the first moment (the slope) of the pre-treatment

Figure C1: Illustration of Different Specifications for the State of California.



Each figure depicts the residualized logarithm of the number of fatalities per 100,000 population. For the TWFE, a regression on California and all not-yet-treated states is used to estimate β_i . This treatment coefficient is then added back to the residuals in the post-treatment period. For the IP, a regression is estimated using only not-yet-treated observations and residuals are plotted. Treatment effect β_i estimated from each method is always equal to the average of post-treatment values of the solid line.

relationship, the overall fit is very close. Both versions, with and without covariates, show a period of relatively lower traffic fatalities in California following MML adoption. After several years, this turns into an increase. This pattern is consistent with stricter enforcement of traffic laws in anticipation of legalization, followed by a positive effect of legalization on traffic fatalities as marijuana use became more widespread.

While both IP specifications with state-specific trends indicate a positive effect of MML on traffic fatalities in California, their magnitudes differ substantially. This difference has a decisive impact on the aggregate result since California determines more than half of the population-weighted estimate. Which estimate is closer to the true effect depends on whether the unconfoundedness assumption holds for the included covariates. In particular, income – one of the key covariates – was rising slightly faster in California than in other states after 1996. The unconfoundedness would require that this increase did not affect traffic fatality rates. The specification without covariates avoids this additional assumption and, in addition, produces a better pre-treatment fit between California and the rest of the states before treatment.