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Medical Marijuana Laws and Suicide

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In the current study we use a synthetic control group design to estimate the causal effect of a medical marijuana initiative on suicide risk. In 1996, California legalized marijuana use for medical purposes. Implementation was abrupt and uniform, presenting a "natural experiment." Utilizing a panel dataset containing annual frequencies of Total, gun, and non-gun suicides aggregated by state for the years 1970–2004, we construct a control time series for California as a weighted combination of the 41 states that did not legalize marijuana during the analysis period. Post-intervention differences for California and its constructed control time series can be interpreted as the effects of the medical marijuana law on suicide. Significance of the effects were assessed with permutation tests. Our findings suggest that California's 1996 legalization resulted in statistically significant ($p<.05$) reductions in suicides and gun suicides, but only a non-significant reduction in non-gun suicides (p?.488). Since the effect for non-gun suicides was indistinguishable from chance, we infer that the overall causal effect was realized through gun suicides. The mechanism could not be determined, however. Participation in the medical marijuana program legally disqualifies participants from purchasing guns. But since most suicides involve guns, it is possible that the effect on total suicide is driven by gun suicide alone.

INTRODUCTION

In 1996, California became the first state to legalize medical marijuana. Known officially as the Compassionate Use Act, Proposition 215 allowed patients and caregivers to cultivate and possess marijuana for medical use. The campaign in favor of Proposition 215 focused on the benefits for seriously ill patients. Claiming that the Proposition "sends our children the false message that marijuana is safe and healthy," the campaign against the Proposition focused on anti-drug education (Legislative Analyst's Office, 1996, p. 4). Neither side addressed potential public health consequences. If Proposition 215 led to an increase in marijuana use, for example, might it also lead to higher rates of all injury deaths (Gerberich et al.,

2003), including deaths from assault (Resnick, Ireland, & Borowsky, 2004), deaths from motor vehicle crashes (Li et al., 2012), and—the subject of the present study—deaths from suicide (Petronis, Samuels, Moscicki, & Anthoney, 1990)?

Such consequences assume that medical and recreational users are similar. With one exception, the evidence supports this assumption. Since most California medical users were introduced to marijuana as recreational users, for example, it is reasonable to assume that the user-types have similar socioeconomic backgrounds (Grella, Rodriguez, & Kim, 2014). Compared with recreational users, however, California's medical users were more likely to report early health problems or disabilities that would warrant medical use (Lankenau et al., 2018). Although Proposition 215 was drafted so loosely that it effectively legalized all uses of marijuana (Vitiello, 1998), marijuana use by California juveniles, who were not eligible for medical marijuana certificates, did not increase following Proposition 215 (Khatapoush & Hallfors, 2004).

Nevertheless, at the national level, during a 15-year period when a majority of states loosened their control of medical marijuana, the U.S. suicide rate rose by 24 percent (Curtin, Warner, & Hedegaard, 2016), prompting many to question how legalization and suicide might be linked. The systematic evidence connecting this trend to the availability of medical marijuana is ambiguous, however. Rylander, Valdez, and Nussbaum (2014), for example, find no correlation between a state's suicide rate and the number of medical marijuana cardholders in the state. Similarly, comparing suicide before and after a state enacts a medical marijuana law, Grucza et al. (2015) find no change in a state's suicide rate. In contrast, Anderson, Rees, and Sabia (2014) report a 10.8 percent reduction in suicides averaged across all medical marijuana states.

Attributing a suicide trend to the availability of medical marijuana raises questions about the potential mechanisms at play. What theoretical mechanisms could lead us to expect a relationship between the availability of medical marijuana and suicide? Could these mechanisms be more salient for certain types of suicides than others? If the expected relationship is observed, what methodological rules could be used to support a causal interpretation of the relationship? We address these questions in order.

ETIOLOGY OF SUICIDE

Sociological theories of suicide follow Durkheim's (1951[1897]) dictum that "suicide varies inversely with the degree of integration of the social groups of which the individual forms a part" (p. 209). Institutions that successfully integrate the individual, providing a sense of belonging to the community, inhibit suicide. When institutions break down, so do the community ties that might otherwise inhibit suicide atrophy; and suicide increases. Durkheim used cross-sectional correlations between suicide and the strength of religious, familial, and socioeconomic institutions to demonstrate his theory. The theory has been used to investigate relationships between suicide and unemployment, (Almgren, Guest, Immerwahr, & Spittel, 1998; Coope et al., 2014; Phillips & Nugent, 2014), poverty and income inequality (Burr, Hartman, & Matteson, 1999; Curtis, Curtis, & Fleet, 2013; Kubrin, Wadsworth, & DiPietro, 2006), divorce and family structure (Baller & Richardson, 2002; Stockard & O'Brien

2002; Sun & Zhang, 2016), immigration and cultural assimilation (Wadsworth & Kubrin, 2004), and cohort size (Stockard & O'Brien, 2002). Regardless of focus, research largely advances a motivational argument for understanding variation in suicide rates across place or time.

Although legalization of medical marijuana is likely to affect a range of societal institutions, the indirect effects through these institutions are expected to accrue gradually. Individual-level direct effects, in contrast, are expected to be realized abruptly. A more appropriate individual-level theory for explaining the relationship between medical marijuana and suicide posits suicide risk as the product of motivation and opportunity factors. That is,

Risk ¼ Motivation Opportunity

Holding motivation constant, suicide risk responds to changes in opportunity. Holding opportunity constant, risk responds to changes in motivation. Chew and McCleary (1994) use motivation/opportunity mechanisms to explain life-course changes in suicide. Kubrin and Wadsworth (2009) use motivation/opportunity mechanisms to explain the effects of socioeconomic factors and firearms availability on race-specific suicide. Wadsworth, Kubrin, and Herting (2014) use motivation/opportunity mechanisms to explain suicide trends for young Black males. Consistent with this literature, we argue that if medical marijuana affects suicide risk, it must do so through one or both pathways.

Mental health theories operate through a motivation pathway. The psychiatric consensus is that suicide is related to depression, anxiety, and other treatable disorders (Mann et al., 2005; Turecki & Brent, 2016; Zalsman et al., 2016). If marijuana alleviates the acute stress associated with these disorders, then we expect suicide risk to decrease following legalization of medical marijuana. The evidence for this is mixed, however (Anderson, Rees, & Sabia, 2014; Calabria, Degenhardt, Hall, & Lynskey, 2010; Moore et al., 2007; Rylander et al., 2014). Whereas medical users report that alleviation of acute symptoms of these disorders was a primary motivation for permit applications (Grella et al., 2014), a systematic review by Walsh et al. (2017) reported that this was not consistently observed across credible studies. Of course, marijuana use itself may constitute a risk factor for suicide apart from alleviating symptoms related to depression and anxiety, at least among some populations and for some levels of suicidality (Felts, Chenier, & Barnes, 1992; Stack, 2014; Van Ours & Williams, 2011).

The relationship between alcohol and suicide also operates through a motivation pathway. A meta-analysis by Darvishi, Farhadi, Haghtalab, and Poorolajal (2015, p. 1) supports the strong consensus that alcohol use disorder "significantly increases the risk of suicidal ideation, suicide attempt, and completed suicide." With respect to medical marijuana, of course, the theoretical prediction depends on whether marijuana is used *in addition to* or *instead of* alcohol. If marijuana and alcohol use are combined, one might expect no change in suicide risk or even an increase in suicide following legalization. If marijuana replaces alcohol, on the other hand, one might expect a decrease in suicide risk following legalization. Self-reports by medical users in California (Reiman, 2009) and Canada (Lucas et al., 2013) indicate that a substantial proportion substitute marijuana for alcohol and other drugs. Since the instruments included items about patients' potentially criminal behaviors, self-reports are potentially biased.

With a few exceptions, opportunity pathways have received less attention in the suicide literature. Noting that suicide risk is highest when the victim is alone in the absence of guardians who would otherwise intervene (Wasserman & Stack, 2008, p. 759), Chew and McCleary use the routine activity theory (Cohen & Felson, 1979) to explain why risk is relatively lower on weekend days, when other household members are more likely to be present, and relatively higher on Mondays, when other household members are likely to be out of the home at school or work. By analogy, if access to medical marijuana obviates the need to leave home, one might expect a lower suicide risk following legalization. If medical users substitute marijuana for alcohol, moreover, legalization may result in less time spent in licensed alcohol establishments. Anderson, Hansen, and Rees (2013) use this argument to explain their finding of a reduction in motor vehicle fatalities following legalization.

Firearms access is a relevant opportunity pathway. The positive correlation between firearms access and suicide risk has been demonstrated for U.S. metropolitan areas and counties (Miller, Barber, White, & Azrael, 2013). These correlations are subject to the ecological fallacy, however. At a disaggregated level, compared with matched controls who live in non-gun households, individuals who live in gun households are 3.4 times more likely to die of suicide (Wiebe, 2003). Comparable risks have been reported for subpopulations, including middle-aged and older adults (Conwell et al., 2002), adolescents (Brent et al., 1991), and women (Bailey et al., 1997).

The 1993 *Brady Handgun Violence Prevention Act* prohibits the purchase of guns by individuals who are addicted to controlled substances. Though used for legal medical purposes, marijuana remains a controlled substance under U.S. law.[1] Since California medical marijuana users were not allowed to purchase firearms in 1997, and since California firearms regulations are relatively strict, we expect a reduction in suicide risk following legalization.

In sum, in 1996, California legalized marijuana use for medical purposes. Implementation was abrupt and uniform, presenting a natural experiment that we take advantage of in order to estimate the causal effect of a medical marijuana initiative on suicide risk. In the current study, we aggregate total, gun, and non-gun suicides by state for the years 1970–2004. Using a Synthetic Control Group quasi-experimental design (Abadie, Diamond & Hainmueller, 2010, 2015), we construct a control unit for California from time series of the 41 states that did not legalize marijuana during the time frame. We interpret post-intervention differences for California and its synthetic control time series as the effects of the medical marijuana law on suicide. Significance of the effects is assessed with permutation tests.

DATA AND METHODS

Ideal counterfactual control units seldom exist in nature (McCleary, McDowall, &

[1] In 2011, a Nevada medical user attempted to purchase a firearm and was refused. Claiming a violation of Second Amendment rights, the user sued. A trial court dismissed the suit and in 2016, the dismissal was affirmed by an appeals court. Wilson *v.* Lynch *et al.*, U.S. Court of Appeals for the Ninth Circuit, No. 14-15700.

Bartos, 2017; Rubin, 1974). However, given an ensemble of less-than-ideal untreated units, an ideal counterfactual can often be approximated as a weighted sum of the untreated units. In order to create Counterfactual California, our control unit, suicide death certificates from the National Center for Health Statistics were aggregated by year, state, and firearm *vs.* non-firearm cause.[2] Years prior to 1970 were excluded from the analytic time frame due to a revision in the International Classification of Diseases (ICD).[3] Since our constructed synthetic control unit is meant to reflect California's suicide time series had the 1996 medical marijuana law not been enacted, years after 2004 were also excluded due to the proliferation of medical marijuana laws across the U.S. threatening to contaminate our "donor pool" of untreated states. These time frame exclusions left pre- and post-intervention time series segments of 27 and eight annual observations, respectively.

States that had not enacted medical marijuana laws during the analytic time frame constitute a "donor pool" of states for the construction of a counterfactual control time series. Powell, Pacula, and Jacobson (2018) report that Washington, Oregon, Alaska, Maine, Hawaii, Colorado, Nevada, Maryland, Vermont, and Montana enacted marijuana laws during the 1997–2004 time period. Excluding these states leaves a donor pool of 40 states and the District of Columbia for construction of the counterfactual control unit. To facilitate comparisons across states of different sizes, the time series were scaled as proportionate changes from their 1968 values.

With the time frame and donor pool defined, if a suitable synthetic control state exists, it can be constructed from a constrained factor analysis of predictors (Abadie et al., 2010, 2015). To introduce this method, suppose that an intervention breaks a time series of T observations, $Y_{0,t}$, into pre- and post-intervention segments of T_0 and T-T_0 years, respectively. If $Y_{j,t}$ is the t^{th} pre-intervention year ($t=1, \ldots, T_0$) for the j^{th} donor state ($j=1, \ldots, J$), then the corresponding observation of the synthetic control state is the sum,

$$Y_{Syn,t} = w_1 Y_{1,t} + \cdots + w_J Y_{J,t}$$

The $w_1, \ldots, w_J$ weights are selected under two constraints.

$$w_1, \ldots, w_J \geq 0 \quad \text{and} \quad w_1 + \cdots + w_J = 1$$

Within these constraints, the w_j minimize the pre-intervention residual mean-square prediction error (*RMSPE*) defined as

$$RMSPE = \sqrt{\frac{1}{T_0}\sum_{t=1}^{T_0}\left(Y_{0,t} - \sum_{j=1}^{J} w_j Y_{jt}\right)^2}$$

Since the internal sum is equivalent to $Y_{syn,t}$, minimizing the *RMSPE* statistic optimizes the "fit" between the affected time series, $Y_{0,t}$, and the corresponding synthetic control time series.

Because $Y_{syn,t}$ is unobserved, the optimization relies on a factor analysis of variables that predict $Y_{0,t}$. Abadie and Gardeazabal (2003) describe two approaches to the optimization. The first approach uses known causal predictors of $Y_{0,t}$. This method necessarily assumes that

[2]The ICD 8 protocol does not distinguish between firearms and explosives.

[3]Prior to the ICD-8a revision adopted in 1968, physicians did not report whether external causes of death were intentionally self-inflicted, accidental, or malicious. Under the ICD-7, a firearm suicide would have been coded as a general explosive fatality, with no indication of intentional self-harm, accidental injury, or intentional suicide (Gjertsen, 2015).

the causes of $Y_{0,t}$ are known, measured, available, and stable across units and time. If the causes are unknown, unmeasured, or unavailable, the observed values of $Y_{0,t}$ can be substituted. McCleary et al. (2017) extend this second approach by including first-differences. The causal predictors of suicide are unknown, unmeasured, or unavailable, so we follow this data-driven approach.

To evaluate the significance of the 1996 intervention on the suicide outcomes, we adopt the permutation inference framework proposed by Abadie and Gardeazabal (2003) and extended by Abadie et al. (2010). By iteratively reassigning the treatment condition to untreated donor pool units, this "in-sample placebo test" generates a distribution of observed placebo effects among the untreated donor pool units. If the estimated effect for California is larger than the effect for any of the untreated donor pool states, we can infer that the estimated impact of California's 1996 intervention is significant, thereby rendering spuriousness implausible.

RESULTS

Figures 1–3 plot California's total, gun, and non-gun suicide rates against their synthetic control states. Visual comparisons of the pre-intervention time series reinforces our interpretation of the *RMSPE* statistics. Post-intervention differences are more difficult to interpret. While it appears that rates of total suicides and gun suicides drop significantly in the aftermath of Proposition 215, the impact of medical marijuana on rates of non-gun suicides is ambiguous.

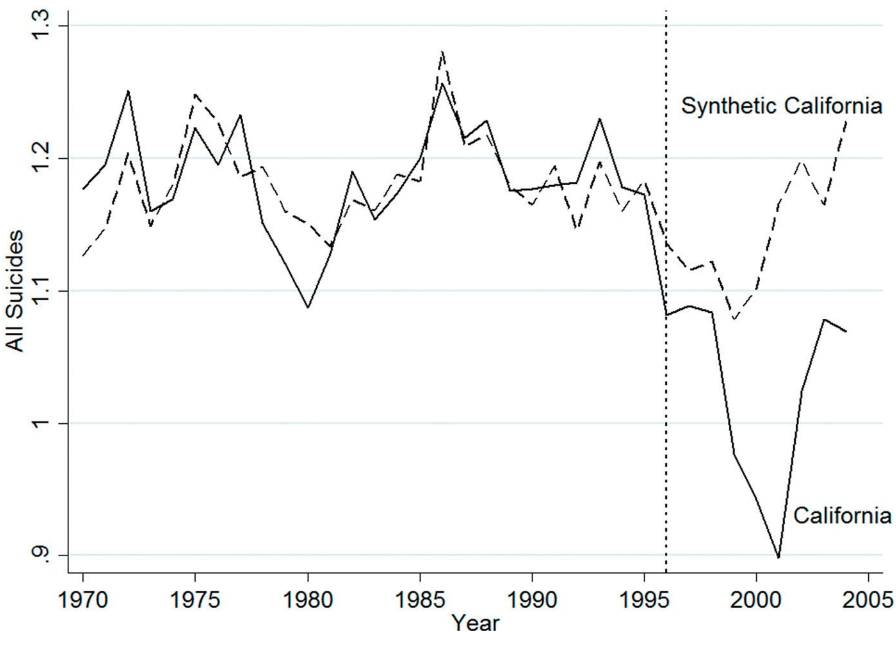

FIGURE 1. Total suicide rate (proportion of 1968 rate).

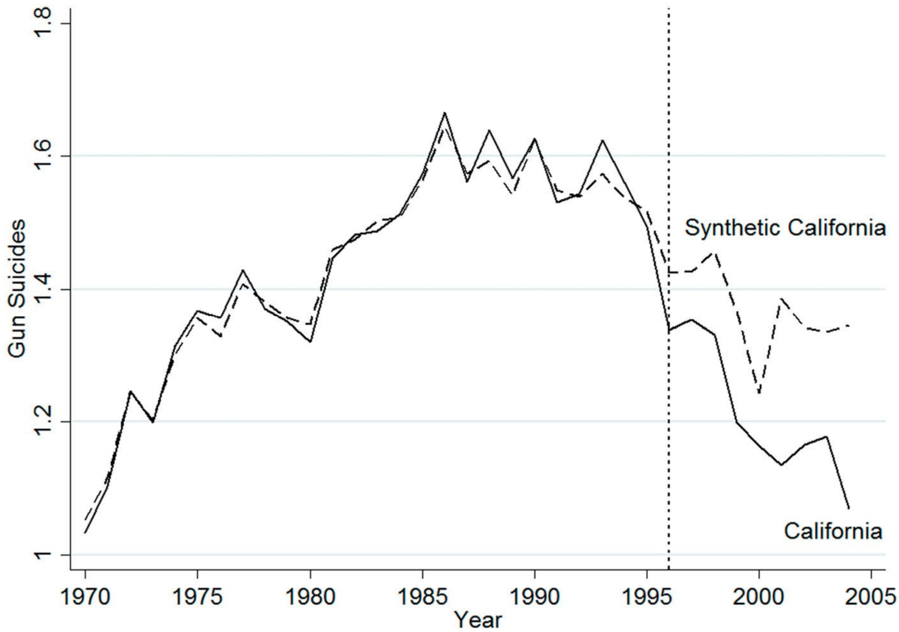

FIGURE 2. Gun suicide rate (proportion of 1968 rate).

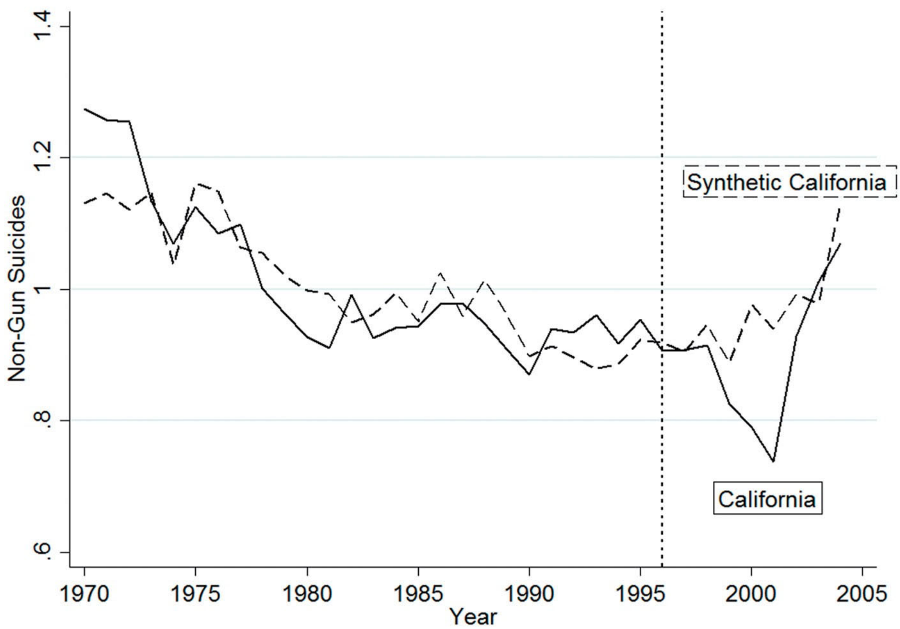

FIGURE 3. Non-gun suicide rate (proportion of 1968 rate).

If treatment and control units are equivalent prior to the intervention, the causal validity of a synthetic control inference can be justified with a difference-in-difference rationale. To estimate the unemployment effects of an increase in the minimum wage, for example, Card and Krueger (1993) observe 400 fast-food stores in New Jersey and Pennsylvania before and after a minimum wage increase in New Jersey. Because the stores were made equivalent prior to the intervention, and because the minimum wage rose in New Jersey but not in Pennsylvania, the before/after differences could be interpreted causally (Rubin, 2008).

Table 1 reports the non-zero wj weights for synthetic total suicides, synthetic gun suicides, and synthetic non-gun suicides. The values of the wj weights are interpreted relative to the set of non-zero weights. In this instance, 23 of $J = 41$ states have weights of zero and, hence, contribute nothing to the constructions. The non-zero weights for Connecticut, the District of Columbia, and Ohio contribute to all three synthetic controls; Kentucky and Texas contribute to two of the three synthetic controls; Alabama, Idaho, Illinois, Kansas, Louisiana, Massachusetts, North Dakota, New Hampshire, New Jersey, Pennsylvania, South Carolina, Wisconsin, and Wyoming make unique contributions to a single synthetic control. Contributions range from very small ($w_j = 0.013$) to moderately large ($w_j = 0.324$). No state makes an overwhelming contribution to any of the synthetic control states. The gun suicide synthetic control "fits" best ($RMSPE = 0.02597$), the non-gun suicide synthetic control "fits" worst ($RMSPE = 0.06219$) statistics. The "fit" of the total suicide synthetic control falls midway ($RMSPE = 0.03125$) between the best and worst.

In this particular instance, $California_t$ and $Synthetic\ California_t$ follow the same path prior to the 1997 intervention,

$$E(California_t - SyntheticCalifornia_t) = 0 \quad \text{for} \quad t = 1970, \ldots, 1996$$

Beginning in 1997, however, the expected difference is the quantity a_t.

$$E(California_t - SyntheticCalifornia_t) = a_t \text{ for } t = 1997, \ldots, 2004$$

The quantity a_t is not necessarily zero nor constant for different values of t. Allowing for approximate equivalence, estimates of a_t will be tested for significance.

The first two columns of Table 2 report the values of a_t for $= 1997, \ldots,$ 2004, as well as the difference for each post-intervention year, the mean difference, and the cumulative difference. For all suicides, our results demonstrate that California's 1996 intervention led to an average reduction of 398.9 suicides per year and a cumulative reduction of approximately 3,191 suicides during 1997–2004. Similarly, California's 1996 law led to a reduction in gun suicides of 208 per year on average and a cumulative reduction of approximately 1,668 fewer gun suicides between 1997–2004. The impact on non-gun suicides is considerably smaller, and arguably no different than what would be expected to occur by chance.

Results from the in-sample placebo tests for all suicides, gun suicides, and non-gun suicides are reported in the last column of Table 2. For all suicides, California's mean post-intervention effect was 4.6 times greater than its pre-intervention $RMSPE$, ranking second among 41 donor pool states ($p < .05$). For gun

TABLE 1. Non-Zero Weights for Synthetic Control States

	Total	Gun	Non-Gun
Alabama	0	0	0.029
Connecticut	0.226	0.014	0.324
District of Columbia	0.116	0.022	0.089
Idaho	0	0.063	0
Illinois	0	0.059	0
Kansas	0.120	0	0
Kentucky	0.062	0.042	0
Louisiana	0	0.180	0
Massachusetts	0	0.119	0
North Dakota	0.087	0	0
New Hampshire	0	0.073	0
New Jersey	0	0.137	0
Ohio	0.274	0.171	0.310
Pennsylvania	0	0	0.223
South Carolina	0	0	0.013
Texas	0.087	0.119	0
Wisconsin	0.029	0	0
Wyoming	0	0	0.013
N of $w_j > 0$	8	11	7
RMSPE, 1970-96	0.031	0.026	0.062

suicides, California's mean post-intervention effect was 6.8 times greater than its pre-intervention *RMSPE*, ranking first among 41 donor pool states ($p < .05$). In contrast, for non-gun suicides, California's mean post-intervention effect was only 1.7 times greater than its pre-intervention *RMSPE*, ranking 20th among 41 donor pool states ($p = .488$). While the in-sample placebo tests for all suicides and gun suicides suggest that the estimated impacts are unlikely to reflect spurious artifacts and are larger than the idiosyncratic shocks observed among the untreated donor states, the in-sample placebo test for non-gun suicides cannot be interpreted with the same confidence. Since gun suicides and non-gun suicides are the two nested subtypes of suicide, the effect of medical marijuana laws for all suicides appears to be driven by the reduction in gun suicides.

DISCUSSION

In 1996, California voters passed an initiative Proposition 215, which legalized marijuana use for medical purposes. Because the Proposition was implemented in an abrupt and uniform manner, legalization presented a "natural experiment." To estimate the causal impact of legalization on suicide, annual time series of total, gun, and non-gun suicides were analyzed by comparing California with an estimated

TABLE 2. Annual Reduction Estimates

Total Suicide			
	a_t	Deaths	
1997	0.026353	83.037	
1998	0.038627	121.713	Post/Pre RMSPE ¼4.619
1999	0.101714	320.501	
2000	0.159679	503.148	Rank ¼2/41
2001	0.267134	841.739	(p <.05)
2002	0.173985	548.228	
2003	0.086504	272.574	
2004	0.158786	500.336	
P	1.012782	3,191.277	
Mean	0.126597	398.910	
Gun Suicide			
	a_t		
1997	0.072398	92.525	
1998	0.125547	160.449	Post/Pre RMSPE ¼6.856
1999	0.167173	213.647	
2000	0.078568	100.409	Rank ¼1/41
2001	0.251268	321.120	(p <.05)
2002	0.176556	225.639	
2003	0.157505	201.291	
2004	0.276803	353.754	
P	1.305817	1,668.834	
Mean	0.163227	208.604	
Non-Gun Suicide			
	a_t		
1997	0.002641	4.946	
1998	0.031605	59.196	Post/Pre RMSPE ¼1.702
1999	0.063671	119.255	
2000	0.185592	347.613	Rank ¼20/41
2001	0.202892	380.017	(p ¼.488)
2002	0.064828	121.423	
2003	0.031651	59.283	
2004	0.061108	114.455	
P	0.575404	1,077.731	
Mean	0.071925	134.706	

counterfactual state in a Synthetic Control Group design. The synthetic control time series for California were constructed as a weighted combination of 41 states that did not legalize medical marijuana during the time frame. Post-intervention differences between California and its constructed control time series were interpreted as the causal effect of the medical marijuana law on suicide. The statistical significance of these effects was assessed with permutation tests.

Findings reveal that rates of total suicide and gun suicide dropped significantly in the aftermath of Proposition 215. Findings also reveal, however, that legalization's impact on non-gun suicides is considerably smaller, and arguably no different than what would be expected to occur by chance. Confidence in these findings is underscored by the methodological approach undertaken in the study. A strength of the Synthetic Control Group Design is that it allows us to examine the net effect of medical marijuana legalization on suicide.

Despite the strengths of this design, important limitations remain, many of which present opportunities for future directions in research. Because we examine suicide trends over eight post-intervention years, we are fairly confident that the effects are permanent. Because our time series end in 2005, on the other hand, it is difficult to generalize our theoretical result to subsequent years. We are limited by the fact that medical marijuana laws began to proliferate across the U.S. after 2005, threatening to contaminate the "donor pool" of untreated states. In virtually all the states that legalized medical marijuana after 2005, moreover, reforms were not implemented abruptly or uniformly, making confident causal interpretations more difficult.

Another limitation that presents a future direction relates to the mechanisms that may account for the findings of the study. What are the mechanisms responsible for the sharp decline in total, but especially gun, suicides following medical marijuana legalization in California? We proposed mechanisms related to the substitution of marijuana for alcohol and other related substances; marijuana use itself, which may reduce actual motivation for suicide; the inability of medical marijuana patients to purchase firearms; and changes in the culture of recreational substance use, leading to fewer unsupervised opportunities to commit suicide in the home. Each of these pathways should be tested, although many will require additional data collection. For example, one likely fruitful research direction would be to collect annual data on alcohol consumption in California and assess whether it is a plausible mechanism by which medical marijuana legalization could cause a reduction in gun suicides. Beyond adjudicating these various pathways, testing mechanisms could yield insight into why we do not find the expected reduction in non-gun suicides following legalization. Unfortunately, we do not have the data to test these mechanisms, yet it will be essential for future researchers to do so.

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