Do Medical Marijuana Laws Increase Marijuana Use? Replication Study and Extension

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PURPOSE: To replicate a prior study that found greater adolescent marijuana use in states that have passed medical marijuana laws, and extend this analysis by accounting for confounding by unmeasured state characteristics and measurement error.
METHODS: We obtained state-level estimates of marijuana use from the 2002-2009 National Survey on Drug Use and Health. We used two-sample t-tests and random-effects regression to replicate previous results. We used difference-in-differences regression models to estimate the causal effect of medical marijuana laws on marijuana use, and simulations to account for measurement error.
RESULTS: We replicated previously published results showing higher marijuana use in states with medical marijuana laws. Difference-in-differences estimates suggested that passing medical marijuana laws decreased past-month use among adolescents by 0.53 percentage points (95% CI: 0.03-1.02) and had no discernible effect on the perceived riskiness of monthly use. Models incorporating measurement error in the state estimates of marijuana use yielded little evidence that passing medical marijuana laws affects marijuana use.
CONCLUSIONS: Accounting for confounding by unmeasured state characteristics and measurement error had an important effect on estimates of the impact of medical marijuana laws on marijuana use. We find limited evidence of causal effects of medical marijuana laws on measures of reported marijuana use.

Introduction

The potential impact of legalizing medical marijuana on both medical and recreational marijuana use has received much popular and legislative attention [1], but little empirical study. In a recent issue of the Annals, Wall and colleagues contributed to this literature by analyzing the prevalence of marijuana use among adolescents in US states that have and have not passed a law legalizing marijuana for medical purposes [2]. They reported evidence that rates of marijuana use were higher in states that had passed medical marijuana laws (MMLs) compared to states that had not passed laws, but concluded that the causal mechanism could not be determined. In this paper we replicate the analyses of Wall and colleagues and, using the same data, we estimate the causal effect of passing medical marijuana laws on measures of marijuana use.

Methods

Wall and colleagues were transparent with respect to both their data and methods, which greatly facilitated replicating their results. We abstracted 2002-2009 data on the state-level prevalence of past-month marijuana use and perceived riskiness of monthly marijuana use from the publicly-available estimates of the National Survey on Drug Use and Health (NSDUH) provided by the US Substance Abuse and Mental Health Survey Administration (OAS) [3]. Additional details on the survey methodology are available on the OAS website (http://www.oas.samhsa.gov/nsduh/methods.cfm). The state-level estimates are two-year averages and are provided for 4 age groups: ages 12 and over, 12-17 years, 18-25 years, and 26 years and over. Since these state-level estimates are derived from Bayesian hierarchical models, they are associated with some uncertainty. We used the published 95% prediction intervals [3] to derive an estimate of the standard error of each state-year estimate by dividing the width of the prediction interval by (2 x 1.96). Wall and colleagues [2] provided the source of data on the dates of enactment of state laws concerning medical marijuana.

We first attempted to replicate the estimates of Wall and colleagues using the same years of data (2002-3 to 2007-8) , then we made adjustments to their assumptions using additional regression models. We also took advantage of another round of NSDUH data (2008-9) that permits evaluating the recent laws passed in New Mexico (2007) and Michigan (2008). Wall and colleagues provided two key pieces of evidence that marijuana laws may be associated with greater marijuana use. Their primary evidence compared the prevalence of marijuana use in states with and without marijuana laws in each year [2, Table 1]. They used two-sample *t*-tests to compare rates of marijuana use in each year in states that did and did not pass medical marijuana laws. These estimates may be derived from a linear regression model with fixed effects for each year, an indicator for whether or not a state had a MML, and an interaction term for each year fixed effect and treatment status. In replicating the results of Wall and colleagues we found no evidence of differential secular trends by MML status for either monthly marijuana use (F statistic = 0.36, p=0.88) or perceived riskiness (F statistic = 0.34, p=0.89), so we dropped these interaction terms. More generally, using this approach one could estimate the effect of the law from a regression model with fixed effects for each year and an indicator for whether or not a state had a MML:

$$Y_{st} = \beta_0 + \beta_1 MML_{st} + \gamma_t + \varepsilon_{st} \quad [1]$$

where Y_{st} is marijuana use in state s in year t, MML_{st} is a dummy variable indicating whether or not a state had a medical marijuana law in place in year t, γ_t is a fixed effect for each year, and ε_{st} is a state-year-specific error term. Under equation 1, if MMLs were randomly assigned to some states in any given year, β_1 would estimate the causal effect of the law on marijuana use. However, states that pass MMLs may differ from those that do not in ways that may also be correlated with marijuana use. For example, if states passing laws tend to have more liberal social norms about drug use, this could be mistaken as the effect of the policy. Without additional control for such factors β_1 does not validly estimate the effect of the law.

Wall and colleagues provide some evidence that such unmeasured confounding may in fact be operating in this case. They used random-effects regression analysis that accounted for a common linear time trend and a random state intercept to compare "the prevalence of marijuana use and perceived riskiness in the years prior to MML passage (data available for 8 states prior to MML) to that of: 1) post-MML years in states that passed MML and 2) all years for states that did not pass MML by 2011." [2, p.715]. In this model the time trend controls for a common linear trend affecting all states, and the random effect allows for a state-specific intercept, assumed to be constant over time. Based on the above description we fit the following model to replicate Wall and colleagues' work:

$$Y_{st} = \beta_0 + \beta_1 Year + \beta_2 PreMML_{st} + \beta_3 PostMML_{st} + u_s + \varepsilon_{st} \quad [2]$$

where Y_{st} is marijuana use in state s in year t, *Year* captures any linear change in marijuana use common to all states, *PreMML* and *PostMML* refer to the state contrasts described by Wall and colleagues above, u_s is a state-specific random deviation from the overall mean, and ε_{st} is a state-year-specific error term. Similar to equation 1, in this case neither β_2 nor β_3 estimates the effect of interest. β_2 represents the difference in marijuana use between states passing laws and states never passing laws in the years before the law is in place, whereas β_3 captures the difference in marijuana use between states that have

already passed laws and states that never passed a law. Thus, the difficulty with equation 2 is that it is unable to validly isolate the causal effect of interest, which is the estimated difference in marijuana use that we would observe if we randomly assigned some states to pass a law. Moreover, random effects models like equation 2 also assume that any unobserved state-level factors are uncorrelated with measured state characteristics [4], which, for reasons noted above, may not be satisfied in the case of marijuana use and beliefs about its risks.

The major limitations of equations 1 and 2 are that neither allows one to isolate the causal effect of the policy. Wall and colleagues suggest that "A longer time window of pre/post data would be needed to provide enough information both before and after passage of MML for each state" [2, p.716] in order to investigate whether MMLs cause changes in marijuana use. We agree that more policy changes over this period would likely increase the power to detect any casual effect of MMLs on marijuana use. However, even with the existing data it is possible to estimate the causal effect of MMLs under some additional assumptions. One well-established method for estimating causal effects of policy changes is difference-in-differences estimation [5]. Using the same data, we fit a model similar to that of equation 1 above, but slightly modified:

$$Y_{st} = \beta_0 + \beta_1 MML_{st} + \gamma_t + \delta_s + \varepsilon_{st} \quad [3]$$

where Y_{st} is marijuana use in state s in year t, MML_{st} is a dummy variable indicating whether or not a state had a medical marijuana law in place in year t, γ_t is a fixed effect for each year, δ_s is a fixed effect for each state, and ε_{st} is a state-year-specific error term. Year fixed effects control for any secular trend affecting marijuana use that is common to all states (not constrained to be linear). More importantly, state fixed effects control for any time-invariant characteristics of states. States that passed laws are our treatment group, and we use states that did not pass laws as a control group to estimate the counterfactual trend that treatment states would have demonstrated, had we been able to observe them [6]. Thus, under this specification, the effect of the law is identified by comparing *within-state* changes in marijuana use before and after the passage of a law in states passing laws to states whose law status does not change. This controls for any differences in state characteristics that do not change over time, similar to the case-crossover design [7] commonly used in epidemiologic studies. It should be noted that whereas Wall and colleagues include states passing laws *after* 2008 (Arizona, Delaware, New Jersey) in some models as being "treated", in our model they are "control" states since during the period of observation they did not pass a law. However, we conducted sensitivity analyses using only these states as a control group since, as they eventually passed laws, they could be considered most similar to states that passed laws during our period of observation.

We also attempted to account for measurement error in the state-level estimates of marijuana use using simple simulations. We used the mean and standard error for each state-year estimate of marijuana use to randomly draw an estimate for each outcome variable (assuming a normal distribution). For each resulting estimate we fit a difference-in-differences model (as in equation 3 above) to estimate the effect of passing a medical marijuana law. We collected the coefficients on the law term in each of 5000 simulations and used the mean to estimate the effect of the law and the 2.5th and 97.5th percentiles to estimate the 95% confidence interval. All analyses were conducted with Stata (version 12), and standard errors for the difference-in-differences models were clustered at the state level [8].

Results

Table 1 shows comparisons between the estimates presented by Wall and colleagues, and our own estimates using the same data. We find nearly identical prevalence estimates of past month marijuana use and the perceived riskiness of monthly marijuana use. Our replication of their 2-sample t-tests provides similar evidence that states with medical marijuana laws have higher average rates of marijuana use and lower perceived riskiness of monthly use than states without laws in each data year. As Wall and colleagues reported, over the period 2002-2008 we estimated higher monthly marijuana use in the 16 states that ever passed a MML (8.68%) than in states that never passed a law (6.94%, prevalence difference = 1.74%, 95% confidence interval [CI]: 1.4%, 2.4%). Wall and colleagues also reported that, in the years before passing laws, states eventually passing laws had similar prevalence of past month use to states that had already passed laws (8.88% vs. 8.58%, $p = 0.25$), but greater prevalence than states that had not passed a law by 2011 (6.94%, $p < .0001$). We find the same prevalence estimates for these state comparisons (8.88, 8.58, and 6.94%, respectively), and similarly find a higher prevalence of reported use in states passing laws since 2004 compared to states that never passed a law (1.65%, 95% CI: 1.0, 2.3). Replication estimates for perceived riskiness were similar to Wall and colleagues, and full results from these replication models are given in the web appendix.

Table 2 shows the ordinarly least squares (OLS) regression estimates with year fixed effects (i.e., equation 1 above without interaction terms) and difference-in-differences estimates (i.e., equation 3 above) for the impact of passing medical marijuana laws on adolescent marijuana use and the perceived riskiness of monthly use. The first column contains the estimates that control only for any common time trends and include a dummy variable for whether or not a state had enacted a law in a given year. The year fixed effects generally showed an overall decline of about 2 percentage points in past month use, and an increase of about 2 percentage points in the perceived riskiness of monthly use (see Appendix for estimates). Similar to Wall and colleagues, these estimates suggest that, controlling for common secular trends in use, states with a MML tended to have a 1.9 percentage point higher prevalence of past month marijuana use (95% CI 1.5, 2.2), and a 5.2 percentage point lower perceived riskiness of monthly marijuana use (95% CI: 4.1, 6.3) than states without a MML. However, additionally controlling for state fixed effects suggests precisely the opposite: relative to states not passing laws, passing a law actually reduced reported adolescent marijuana use by 0.6 percentage points on average (95% CI: 0.1, 1.1) and raised the perceived riskiness of monthly use by 0.7 percentage points, though the latter estimate is not statistically different from zero (95% CI: -1.1, 2.5). Perhaps more importantly, for both outcomes the difference-in-differences estimate was considerably different from the estimate controlling only for year fixed effects (both p-values < 0.0001 for a test that two estimates were equal). This suggests there may be considerable confounding of the effect of MMLs by unmeasured, time-invariant state characteristics. Difference-in-differences estimates based on using only the 3 states that passed laws after 2008 as a control group were less precise but similar to the full-sample estimates for past month use (β = -0.41, 95% CI: -1.1, 0.2) and perceived riskiness of monthly use (β = 1.13, 95% CI: -0.4, 2.7), and in both cases the 95% confidence intervals showed no overlap with the estimates controlling only for year fixed effects.

Table 3 shows regression estimates based on data through 2008-9 for 4 age groups using 4 different estimation strategies: 1) OLS with year fixed effects; 2) difference-in-differences; 3) difference-in-differences accounting for measurement error in the state-level estimates of marijuana use; and 4) difference-in-differences with the restricted set of control states. For each set of outcomes, the models that estimate the impact of laws controlling only for year fixed effects consistently find that laws are associated with increased past-month marijuana use (ranging from 2.2 to 4.7 percentage points) and decreased perceived riskiness of monthly marijuana use (from 5.2 to 7.6 percentage points) across all age groups. The difference-in-differences estimates, however, show few consistent effects, and most are not statistically distinguishable from zero. For those ages 12-17, there is still some evidence that passing a law decreases the rate of past month marijuana use by half a percentage point (95% CI -1.0, -0.0), but accounting for measurement error in the state-level estimates

Table 1: Prevalence of marijuana use by year and medical marijuana status among 12-17 year-olds.

		Wall et al.[2]		Replication estimates[a]			
Year	No. states with laws	With Law	Without Law	With Law	Without law	*t*-stat	*p*-value
Prevalence of past-month marijuana use (%)							
2002-3	8	9.67	8.33	9.67	8.31	-2.24	0.029
2003-4	10	9.84	7.66	9.84	7.62	-4.38	<0.0001
2004-5	10	8.95	7.12	8.95	7.12	-4.00	<0.0001
2005-6	11	8.57	6.63	8.57	6.63	-5.25	<0.0001
2006-7	12	8.40	6.45	8.40	6.46	-5.37	<0.0001
2007-8	13	8.27	6.40	8.27	6.42	-5.56	<0.0001
Prevalence of perceived riskiness of monthly marijuana use (%)							
2002-3	8	29.13	33.84	29.13	33.71	2.75	0.008
2003-4	10	30.82	35.44	30.82	35.29	3.06	0.004
2004-5	10	30.39	35.13	30.39	34.97	3.35	0.002
2005-6	11	30.00	35.09	30.00	34.95	4.04	<0.0001
2006-7	12	30.02	36.01	30.02	35.78	4.50	<0.0001
2007-8	13	29.53	36.17	29.53	35.93	4.94	<0.0001

[a] *t*-statistic and *p*-value for a test of whether the replication study prevalence difference between states with and without laws is different from zero.

Table 2: Regression estimates of the effect of passing medical marijuana laws on marijuana use among 12-17 year-olds, 2002-3 to 2007-8.

	Past month marijuana use rate (%)				Perceived riskiness of monthly use (%)			
	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Passed law	1.87	(1.5, 2.2)	-0.59	(-1.1, -0.1)	-5.19	(-6.3, -4.1)	0.72	(-1.1, 2.5)
Constant	8.23	(7.9, 8.6)	8.62	(8.4, 8.8)	33.80	(32.7, 34.9)	32.88	(32.4, 33.3)
Year fixed effects		Yes		Yes		Yes		Yes
State fixed effects		No		Yes		No		Yes

N=306 for all models.

Table 3: Regression estimates of the effect of passing medical marijuana laws on marijuana use among different age groups, 2002-3 to 2008-9.

	Past month marijuana use rate (%)							
	12+ years		12-17 years		18-25 years		26+ years	
Effect of law, controlling for:	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Year fixed effects only	2.18	(1.9, 2.5)	1.92	(1.6, 2.2)	4.67	(3.7, 5.7)	1.86	(1.6, 2.1)
Difference-in-differences	0.05	(-0.6, 0.7)	-0.53	(-1.0, -0.0)	0.65	(-0.8, 2.1)	0.03	(-0.5, 0.6)
Difference-in-differences + measurement error	0.04	(-0.5, 0.6)	-0.52	(-1.3, 0.3)	0.65	(-0.6, 1.9)	-0.01	(-0.6, 0.6)
Difference-in-differences, restricted control group[a]	-0.16	(-0.8, 0.5)	-0.42	(-1.0, 0.2)	0.18	(-1.6, 2.0)	-0.20	(-0.8, 0.4)
	Perceived riskiness of monthly use (%)							
Effect of law, controlling for:	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Year fixed effects only	-0.87	(-8.1, -5.7)	-5.23	(-6.2, -4.2)	-4.44	(-5.6, -3.3)	-7.61	(-8.9, -6.3)
Difference-in-differences	0.24	(-1.4, 1.9)	0.84	(-0.9, 2.5)	-0.09	(-1.2, 1.0)	0.23	(-1.4, 1.9)
Difference-in-differences + measurement error	0.24	(-0.9, 1.4)	0.85	(-0.5, 2.2)	-0.09	(-1.2, 1.0)	0.24	(-1.2, 1.7)
Difference-in-differences, restricted control group[a]	-0.14	(-2.0, 1.7)	1.43	(-0.7, 3.6)	-0.30	(-1.5, 0.9)	-0.34	(-2.3, 1.7)

N=357 for all models. Estimates of β for models that account for measurement error are derived from the mean, and the 95% CI limits are the 2.5th and 97.5th percentiles of 5000 simulations.
[a]N=112. Control states are AZ, DE, NJ

of marijuana use leads to a less-precise estimate (95% CI -1.3, 0.3). None of the difference-in-differences estimates suggest any positive or negative impact of passing MMLs on perceived riskiness.

Discussion

The evidence presented by Wall and colleagues suggested that states passing medical marijuana laws since 2004 had higher prevalence of marijuana use than states not passing laws. They gave 3 possible explanations for this finding: 1) states with higher use are more likely to enact laws; 2) laws increase use; and 3) unmeasured factors (e.g., social norms about drug use) that affect marijuana use and af-

fect the likelihood of passing a law may be more common in states that passed laws. Our analysis, using the same data, replicated Wall and colleagues' results and attempts to provide some additional evidence as to which of the above explanations may be most plausible. Our results are consistent with their first explanation since the descriptive data and random effects models do find elevated use in states that passed laws. We find little evidence for the second explanation. If anything, our estimates suggest that reported adolescent marijuana use may actually decrease following the passing of medical marijuana laws. This could be plausibly explained by social desirability bias or greater concern about enforcement of recreational marijuana use among adolescents after the passage of laws. Miller and Kuhns recently found some evidence of increased validity (based on urinalysis) of self-reporting among arrestees in states with medical marijuana laws (which would seem to work against this interpretation), but this result seems unlikely to be generalizable [9]. Our analyses are most consistent with the third proposed explanation of Wall and colleagues: unmeasured confounding. US states differ in many respects that may not be easily measured in standard epidemiological or social surveys, and analyses of the health impacts of policies or exposures often find that controlling for state fixed effects substantively change the magnitude and possibly the direction of estimates [10, 11]. Once we control for any unmeasured state characteristics that do not change over time, we find very little evidence that passing medical marijuana laws increases reported use, among adolescents or any other age group.

While our analysis controls for unmeasured, time-invariant state characteristics, it also has limitations. As Wall and colleagues noted, the limited number of policy changes over the period 2002-9 make it difficult to precisely estimate the effect of passing laws on marijuana use. Nevertheless, the NSDUH data have been used previously to study the impact of blood alcohol content laws on alcohol use over a short time widow (though with more policy changes) [12]. A more important limitation is that the difference-in-differences estimates assume that there are no unmeasured policy or behavioral changes that may differentially affect marijuana use in states adopting laws versus those not adopting laws at the time of adoption [5, 6]. This may not be a tenable assumption, as it is possible that states implementing laws could increase efforts to reduce recreational marijuana use among adolescents or young adults or to enforce existing federal marijuana policy law. In addition, since the NSDUH estimates are two-year averages, there is uncertainty about in which year of data the law should be considered as being enacted. We followed Wall and colleagues, assuming that any effect of a law passed in 2004 would be observed in the 2003-2004 data, but one could argue that any effects of a 2004 law might be observed only in the 2004-2005 data. Our results were generally not sensitive to this choice and we found little evidence of an effect of laws on marijuana use using this specification, though the effect on monthly use among 12-17 year olds reported in Table 4 was diminished (tests of equality between the law coefficients for these models are shown in the web appendix). Finally, our analysis assumes that the implementation of medical marijuana laws is similar across states, but the lack of federal regulations on medical marijuana means this may not be a tenable assumption [1, 13]. For example, in California and Colorado the definitions of what constitutes "appropriate" or "necessary" medical conditions that may be treated with marijuana are vague and may not be similar to other states, and levels of enforcement may also differ across states [13].

As a final point, our study demonstrates the value of replication studies for epidemiology and health policy. Replication has received a good deal of attention in the broader social science literature [14, 15], but has been less enthusiastically received in epidemiology, though that appears to be changing [16]. Replication studies aim to highlight the importance of asking different but related questions, changing assumptions or applying alternative analytic techniques to the same data, and have the potential to provide more robust evidence on causal questions relevant to epidemiology. King [14] argues that the replication standard holds if authors provide sufficient information with which to understand, evaluate, and build upon a prior work (i.e., if a third party could replicate the results without any additional information from the authors). Adhering to the replication standard, regardless of whether or not any replication is actually conducted, should make it easier for new research to build on prior work, and will hopefully enhance the transparency and credibility of future work. In the spirit of good practice for replication efforts, replication materials containing the data and statistical code used to generate our estimates are available as an online supplement or at the corresponding author's replication website (http://dvn.iq.harvard.edu/dvn/dv/samharper).

In summary, while we replicated the findings of Wall and colleagues that marijuana use was higher in states that have passed medical marijuana laws, our analysis suggests this is unlikely to be a causal association. Our difference-in-differences estimates suggest little detectable effects of passing medical marijuana laws on marijuana use or perceived riskiness of use among adolescents or adults, which is consistent with some limited prior evidence on arrestees and emergency department patients [17]. Future analyses that take advantage of additional policy changes may provide further evidence on this question, but our results suggest that such analyses should adequately control for potential confounding by unmeasured state characteristics.

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