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 (MMLs), 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 through 2009 National Survey on Drug Use and Health. We used 2-sample *t*-tests and random-effects regression to replicate previous results. We used difference-in-differences regression models to estimate the causal effect of MMLs on marijuana use, and simulations to account for measurement error.

RESULTS: We replicated previously published results showing higher marijuana use in states with MMLs. Difference-in-differences estimates suggested that passing MMLs decreased past-month use among adolescents by 0.53 percentage points (95% confidence interval [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 MMLs affects marijuana use.

CONCLUSIONS: Accounting for confounding by unmeasured state characteristics and measurement error had an important effect on estimates of the impact of MMLs on marijuana use. We find limited evidence of causal effects of MMLs on measures of reported marijuana use.
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KEY WORDS: Adolescents, Medical Marijuana Law, Medical Marijuana, National Survey on Drug Use and Health, Quasi-Experiments.

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 et al. 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 with 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 et al. and, using the same data, we estimate the causal effect of passing MMLs on measures of marijuana use.

METHODS

Wall et al. were transparent with respect to both their data and methods, which greatly facilitated replicating their results. We abstracted data from 2002 through 2009 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 (3). Additional details on the survey methodology are available on the US Substance Abuse and Mental Health Survey Administration website (available: http://www.oas.samhsa.gov/nsduh/methods.cfm). The state-level estimates are 2-year averages and are provided for 4 age groups: Ages 12 and over, and 12 to 17, 18 to 25, and 26 years and over. Because 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 (2×1.96). Wall et al. (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 et al. using the same years of data (2002–2003 to 2007–2008),then we made adjustments to their assumptions using additional regression models. We also took advantage of another round of NSDUH data (2008–2009) that permits evaluating the recent laws passed in New Mexico (2007) and Michigan (2008). Wall et al. provided 2 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

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Selected Abbreviations and Acronyms

CI = confidence interval
MML = medical marijuana law
NSDUH = National Survey on Drug Use and Health

each year (Table 1; 2). They used 2-sample *t*-tests to compare rates of marijuana use in each year in states that did and did not pass MMLs. 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 et al., we found no evidence of differential secular trends by MML status for either monthly marijuana use (F statistic $= 0.36$; $P = .88$) or perceived riskiness (F statistic $= 0.34$; $P = .89$), 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 MML 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 et al. 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 before MML passage (data available for 8 states before MML) to that of (i) post-MML years in states that passed MML and (ii) 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 this description, we fit the following model to replicate Wall et al.'s 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 et al. 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. Here, 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 the reasons we noted, 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 et al. 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) 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 MML 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 important, 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