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Prevalence of marijuana use does not differentially increase among youth after states pass medical marijuana laws: Commentary on Stolzenberg et al. (2015) and reanalysis of US National Survey on Drug Use in Households data 2002–2011

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Abstract

There is considerable interest in the effects of medical marijuana laws (MML) on marijuana use in the USA, particularly among youth. The article by Stolzenberg et al. (2015) "*The effect of medical cannabis laws on juvenile cannabis use*" concludes that "implementation of medical cannabis laws increase juvenile cannabis use". This result is opposite to the findings of other studies that analysed the same US National Survey on Drug Use in Households data as well as opposite to studies analysing other national data which show no increase or even a decrease in youth marijuana use after the passage of MML. We provide a replication of the Stolzenberg et al. results and demonstrate how the comparison they are making is actually driven by differences between states with and without MML rather than being driven by pre and post-MML changes within states. We show that Stolzenberg et al. do not properly control for the fact that states that pass MML during 2002–2011 tend to already have higher past-month marijuana use before passing the MML in the first place. We further show that when within-state changes are properly considered and pre-MML prevalence is properly controlled, there is no evidence of a differential increase in past-month marijuana use in youth that can be attributed to state MML.

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Conflict of interest

None of the authors (or their family members) have any conflicts of interest with this work. The data being used is publically available.

Keywords

Medical marijuana laws; Pre; post tests; Observational data analysis

Introduction

There is considerable interest in the effects of medical marijuana laws (MML) on marijuana use in the USA, particularly among youth. The article by Stolzenberg, D'Alessio, and Dariano (2015) "*The effect of medical cannabis laws on juvenile cannabis use*" reports an analysis of data from the U.S. National Survey on Drug Use in Households (NSDUH). The article concludes that "implementation of medical cannabis laws increase juvenile cannabis use" and that there is "compelling evidence supporting the assertion that medical cannabis legislation amplifies recreational juvenile cannabis use because the percent of juveniles using cannabis increased substantially following the passage of a medical cannabis law." This conclusion of a substantial increase in marijuana use from pre to post-MML passage in youth is contrary to the findings from other publications analysing similar NSDUH data (Harper, Strumpf, & Kaufman, 2012; Wall et al., 2011, 2012; Wen, Hockenberry, & Cummings, 2015) and is also contrary to additional findings from analyses of other large national sources of data on youth, specifically the Monitoring the Future (Hasin et al., 2015), the Youth Risk Behavior Surveys (YRBS) (Anderson, Hansen, & Rees, 2015; Pacula, David, Paul, & Eric, 2015), and the National Longitudinal Survey of Youth (Pacula et al., 2015). These other studies found that the prevalence of marijuana use was higher in MML states compared to states without MML regardless of when the MML was passed. However, these other studies uniformly found either no difference in the prevalence of adolescent marijuana use comparing before to after the passage of MML (Anderson et al., 2015; Harper et al., 2012; Pacula et al., 2015; Wen et al., 2015), or a decrease in marijuana use comparing before to after the passage of MML (Hasin et al., 2015). Careful assessment of the analytical strategy chosen by Stolzenberg et al. indicates that the statistically significant increased association they report between MML and past-month marijuana use is the result of a biased comparison and thus, the conclusions of Stolzenberg et al. are not supported by the data.

In this commentary, we provide a replication of the Stolzenberg et al. results and demonstrate how the comparison they are making is actually driven by differences between states with and without MML rather than being driven by pre and post-MML changes within states. We show that Stolzenberg et al. do not properly control for the fact that states that pass MML during 2002–2011 (the years of NSDUH data that are available) tend to already have higher past-month marijuana use before passing the MML in the first place. We further show that when within-state changes are properly considered and pre-MML prevalence is properly controlled, there is no evidence of an increase in past-month marijuana use in youth that can be attributed to state MML.

The available NSDUH state-level data

A limitation of using the NSDUH data to examine the effects of MML state policy, i.e., whether rates of marijuana use change within states after passage of MML, is that the NSDUH state-level data used by Stolzenberg et al. were only available for a relatively short time frame, 2002–2011, although states have been passing MML since 1996. Of the 23 states that currently (as of 2015) have MML, only eight passed their laws between 2002 and 2011. This leaves 15 states with either no pre-MML or no post-MML data available between 2002 and 2011 for analysing within state pre and post-MML comparisons.

Fig. 1 shows the raw state NSDUH data used by Stolzenberg et al. in the analysis (not shown in the original article). The data are organized into 3 panels to emphasize the 3 different types of states (from left to right): (1) the 34 states that did not pass MML by 2011 (note 7 of these states passed laws after 2011 up until 2015 but are treated as in Stolzenberg et al. as “No Law” states); (2) the 8 states that passed MML between 2002 and 2011, for which NSDUH pre-and post-MML data are available; and (3) the 8 states that already had MML prior to 2002 (in these states, NSDUH data can only inform about post-MML levels of use since no pre-MML data are available). Note that the 5 observations plotted over time for each state are collapsed by NSDUH into 2-year intervals (i.e. 2002–2003–2004–2005–2006–2007–2008–2009–2010–2011). In the second panel, representing the 8 states in which MML was passed between 2002 and 2011, the dots represent the specific year in which the MML was passed, specifically: Arizona (2010), Delaware (2011), Michigan (2008), Montana (2004), New Jersey (2010), New Mexico (2007), Rhode Island (2006), and Vermont (2004). In the Stolzenberg et al. article and also in this re-analysis of data, a state was treated as having the law in the year the law was passed, and in all subsequent years. Because of the two-year collapsing of the data by NSDUH, Rhode Island (2006) and New Mexico (2007) are treated as passing their MML at the same time (i.e. 2006–2007) and Delaware (2011) is treated as passing the MML at the same time as Arizona (2010) and New Jersey (2010) (i.e. 2010–2011), leading to less pre and post-MML passage information.

Descriptive statistics replication and re-analysis

The average past month marijuana use among 12–17 year olds of the 34 states without MML across the 5 time points (10 years) is 6.93% (SD = 1.3%, min = 3.7%, max = 11.8%) (simple unweighted average across states). The average use across the same time period in the 8 states with “MML prior to 2002” is 8.80% (SD = 1.3%, min = 6.8%, max = 12.3%). Importantly, this higher average use of 8.80% in states with MML prior to 2002 cannot be used as evidence of a causal effect of MML on increased use, because the NSDUH dataset does not provide information on the rates of use in these MML states before they passed such laws. In fact, based upon findings from other studies with data from further in the past (back to 1991 in Hasin et al., 2015, back to 1993 in Anderson et al., 2015), these states appear to have already had higher prevalence of marijuana use before they passed MML.

For the states that passed MML during the window from 2002 to 2011, we summarize the pre-MML periods separately from the post MML periods. Table 1 presents the simple descriptive means for the 8 states that changed MML status across the 5 time points (10 years) from 2002 to 2011. Table 1 shows that using the simple within-state differences (for the 8 states with available NSDUH data within this time period), the average rate of marijuana use increased in 3 states (AZ, DE, NJ) but decreased in 5 states (MI, MT, NM, RI, VT). Comparing the simple average of pre-MML marijuana use prevalence (9.44%) to the post-MML prevalence (9.35%), the overall average change pre-MML to post-MML in these eight states is −0.1%, thus, not providing any evidence that could be interpreted as an increase.

Appropriate analysis of changes in adolescent marijuana use after passage of MML should compare the mean marijuana use prevalence in states before the passage of MML to the mean of those **same** states after the passage of MML. Stolzenberg et al. did not make this comparison in the descriptive statistics used for Fig. 1 of their manuscript. Their Fig. 1 does not compare marijuana use in the same states pre and post-MML, but instead compares different states. Each bar for before and after MML passage in the Stolzenberg et al. paper represents a different set of states (Supplemental Table S1 in this commentary replicates the results and shows the sample size for each bar). The states that passed MML early during 2002–2011 (VT, MT, RI) already had higher prevalence of marijuana use even before they passed MML. Therefore, creating means of post-MML marijuana use by combining these states with the states that passed MML later leads to an artificial appearance that marijuana use increased post-MML passage because the means are increased through inclusion of states that were higher even before they passed MML. Unfortunately, Stolzenberg et al. did not recognize this and use their Fig. 1 to make the following conclusion:

> Although it is certainly plausible that the prevalence of cannabis use in a state is a causal factor in whether or not a state implements a medical cannabis law, Fig. 1 also shows that with the exception of the 2002–2003 period juvenile cannabis use was consistently higher in states following the passage of a medical cannabis law than in states that would eventually pass such a law. This finding suggests that the implementation of medical cannabis laws increase juvenile cannabis use rather than cannabis use influencing the passing of medical cannabis laws.

Contrary to their conclusion, the evidence in Table 1 is much more consistent with Stolzenberg et al.'s other plausible conclusion, i.e. that states with higher use were the ones that passed MML in the first place, with no evidence of an increase post-MML passage.

Regression modeling replication and re-analysis

Stolzenberg et al. used the panel regression procedure in the LIMDEP software to estimate the effect of medical cannabis laws and the control variables on the prevalence of past month marijuana use. For our replication and re-analysis we did not have access to the 11 control variables used by Stolzenberg et al., but, given that only five control variables were statistically significant and all of those variables had small coefficients, suggesting that they had little actual effect on the results, we proceeded with re-analyses without them. Moreover, our replication of Stolzenberg et al.'s primary regression result yields an estimated coefficient similar to their result that adjusts for control variables. Stolzenberg et al. describe using a two-way random effects model (commonly applied in econometrics when time series and cross-sectional data are combined, e.g. Greene, 1990). Although not explicitly shown, the authors appear to have used the following, where Y_{it} is the prevalence of past-month marijuana use in state i (i = 1, …, 50) in the 5 time periods t, (t = 2002–2003–2004–2005–2006–2007–2008–2009–2010–2011)

Model 1

$$Y_{\mathrm{it}} = \beta_0 + \beta_1 * \mathrm{MML}_{\mathrm{it}} + b_i + \tau_t + e_{\mathrm{it}} \quad (1)$$

$$b_i \sim N(0, \sigma_b), \quad \tau_t \sim N(0, \tau_t), e_{\mathrm{it}} \sim N(0, \sigma) \quad (2)$$

$\mathrm{MML}_{\mathrm{it}}$ is a dichotomous indicator of whether state *i* has a MML in time period *t* or not, and b_i and τ_t are random intercepts for state and time respectively. In Stolzenberg et al. there were 11 additional continuous covariates in Eq. (1), which included state summaries such as availability of cannabis and census variables such as percent of fathers in households.

Fitting Model 1 in SAS Proc Mixed (without covariates, code will provided upon request) to the NSDUH data from 2002 to 2011, we obtain coefficients β_0 = 7.35% with standard error (s.e.) = 0.34%, and β_1 = 0.93% s.e. = 0.25%, *p*-value = 0.0002. The positive and significant MML effect for β_1 corresponds to the major finding in the Stolzenberg article (Table 2 in Stolzenberg et al. “Medical Cannabis State Coefficient = 0.861%, s.e. = 0.298%”). This positive statistically significant coefficient of 0.861% (0.93% in our replication without covariates) was used by Stolzenberg et al. to conclude there was evidence of an increase in marijuana use after the passage of MML.

Unfortunately, for addressing the present research question, Model 1 is estimating only two means across all states and years, one corresponding to $\mathrm{MML}_{\mathrm{it}}$ = 0 which is β_0 = 7.35% and one corresponding to $\mathrm{MML}_{\mathrm{it}}$ = 1 which is $\beta_0 + \beta_1$ = 7.35% + 0.93% = 8.28%. The two fitted means of Model 1 are superimposed in red in Fig. 1. Model 1 rests on two highly questionable assumptions: (1) that pre-MML marijuana use in the MML states that passed laws prior to 2002 was equal to marijuana use in the non-MML states during 2002–2011 and (2) that in the future when non-MML states eventually pass laws, their marijuana use post-MML will be equal to the post-MML marijuana use in the states that already passed MML prior to 2011. More simply, the assumption is that a difference between non-MML and MML states exist, but is only attributable to the enactment of the law in the second group, and not to other differences between the states. This can be seen because the 34 No MML states are the primary contributors to the before passage (i.e. $\mathrm{MML}_{\mathrm{it}}$ = 0) mean estimate and the 8 states with MML prior to 2002 are the primary contributors to the after passage (i.e. $\mathrm{MML}_{\mathrm{it}}$ = 1) mean estimate. The 8 states that changed from $\mathrm{MML}_{\mathrm{it}}$ = 0 to $\mathrm{MML}_{\mathrm{it}}$ = 1 during the period contribute to both the pre-MML and post-MML mean estimates, but their contribution is swamped by the other states with no change. Hence the β_1 effect, interpreted by Stolzenberg et al. as indicating the effect on prevalence in a state due to enacting the MML, is greatly confounded by between-state differences. We know that states are not interchangeable and that laws were not passed randomly. Indeed evidence from the NSDUH indicates that the pre-passage years in states that passed laws in 2002–2011 already had higher mean marijuana use in youth than the “No Law” states before passing their laws (see Table 1, before MML prevalence 9.44% compared to 6.93% for No Law states). Moreover, the seven states that passed laws after 2011 up to 2015 also had higher mean MJ use than the “No Law” states before passing laws. Specifically, the 7 states which passed laws after 2011 had mean prevalence before passing laws of 8.2% (SD = 1.5%, min = 5.6%, max 11.8%) across 2002–2011 compared to the average use of the 27 which had still not passed laws by

2015 which was only 6.6% (SD = 1.0%, min = 3.7%, max = 9.6%). Simply stated, the "No Law" states do not make a good (or even reasonable) comparator for estimating pre and post-MML changes. The regression model used by Stolzenberg et al. (Model 1) unjustifiably extrapolates by using the "No Law" states to estimate the pre-passage mean prevalence and compares it to post-passage mean prevalence for other states which do not have pre-passage data available.

A simple addition to Model 1 improves this particular confounding problem. By adding one more predictor variable indicating whether a state is one that passed a MML or not up to 2011, we can effectively separate the No Law states out of the estimation of the pre-MML prevalence estimate. Define $\mathrm{NoMML}_i = 0$ for the 34 states that did not have laws prior to 2011, and $\mathrm{NoMML}_i = 1$ for the 16 states that did have laws (regardless of what year it was passed). Thus we have:

Model 2

$$Y_{\mathrm{it}}=\beta_0+\beta_1 * \mathrm{MML}_{\mathrm{it}}+\beta_2 * \mathrm{NoMML}_i+b_i+\tau_t+e_{\mathrm{it}} \quad (3)$$

$$b_i \sim N(0,\sigma_b), \quad \tau_t \sim N(0,\tau_t), \quad e_{\mathrm{it}} \sim N(0,\sigma) \quad (4)$$

Fitting this model in SAS Proc Mixed we obtain: β_0 = 6.94% s.e. 0.34%, β_1 = 0.43%, s.e. 0.28% p-value = 0.123, and β_2 = 1.65%, s.e. 0.39% p-value < 0.001. After controlling for NoMML_i, the β_1 effect of passing MML is *not* significant and does not indicate a pre–post change in the laws. Instead, the large, highly significant effect of β_2 indicates there is a significantly higher mean in MML states before they pass the laws than in the No MML states. Model 2 yields three means: β_0 = 6.94%, indicating the mean in the No Law states, $\beta_0 + \beta_2$ = 6.94% + 1.65% = 8.59% indicating the mean in the states with MML before they pass the law, and $\beta_0 + \beta_1 + \beta_2$ = 9.02% indicating the mean in the states with MML after they pass the law. The three fitted means of Model 2 are superimposed in blue in Fig. 1.

Fig. 2 shows the residuals from the two models that clearly indicate the biased results that arise from Model 1. In Model 1, the "No law" states mean prevalence is overestimated, while the means for the states with MML before 2002 and those that passed between 2002 and 2011 are underestimated. This occurs primarily because the pre-passage observed values in the states that passed between 2002 and 2011 are constrained in Model 1 to equal the "No Law" mean (because Model 1 only has one fixed parameter for MML). Yet the observed prevalence in the 8 states which changed MML during 2002–2011 is already higher before passing the laws than the "No law" states. Model 2, on the other hand, shows little or no bias because it separates the estimate for the No law states from the pre–post estimates for the MML states provided by the states that have passed MML.

Finally, we demonstrate how a fixed effects model can also be used to properly examine the pre and post-MML effect. Stolzenberg et al. 2015 dismiss the possibility of using a fixed effect model "…because the dummy coded medical cannabis law variable has reduced variability". The minimal variability in the $\mathrm{MML}_{\mathrm{it}}$ variable is a symptom of the limited number of states (only 8 states) for which both pre and post (i.e. within state) information is available. But lack of variability does not preclude using fixed effects, and other groups have analysed data pertaining to the MML question using fixed effects models (Wen et al., 2015 and Harper et al., 2012 for the NSDUH, and Anderson et al., 2015 and Pacula et al., 2015 for the YRBS). The fixed effects model is identical to Eq. (1), but rather than assuming random distributions for b_i and τ_t as in Eq. (2), they are estimated as separate fixed parameters (49 estimates for b_i with one state taken as the reference, and 4 estimates for τ_t with 2002–2003 taken as the reference). A Hausman test (Greene, 1990) rejects Model 1 in favour of the fixed effects model (m-value = 17.5, df = 1, p-value < .001). Methodological arguments in the literature can be found comparing and contrasting the virtues of using fixed effects versus random effects, for a recent review see (Clarke, Crawford, Steele, & Vignoles, 2015). For the present data, the results from using a fixed effect model yield consistent results to those of Model 2. Specifically, the coefficient for MML_{it} from the fixed effects model is β_1 = 0.33%, s.e. 0.29%, p-value = 0.254 indicating no significant change in marijuana use post-MML, as was concluded from Model 2. It is beyond the scope of this commentary to repeat all the pros and cons of random versus fixed effects models, but a well-known problem of the random effects approach is that bias can occur when there is correlation between the predictor of interest and the random effects. By not properly controlling for the large mean difference in No MML states versus MML states, the model used by Stolzenberg et al. (Model 1) suffers from this bias-inducing correlation, whereas the random effects Model 2 which adds one fixed state covariate NoMML alleviates the problem. The fixed effects model avoids such bias by taking each state with a change in MML as its own control.

Conclusion

There is enormous interest in evidence-based information about the effects of MML on U.S. marijuana use, particularly in youth. The problem is that limited information is available that can address whether MML are truly *causally* related to changes in rates of marijuana use. NSDUH data clearly illustrate that states with MML had higher average prevalence of marijuana use between 2002 and 2011 than states without MML. However, as is well known, an association does not necessarily indicate a causal relationship. To build a case for a causal relationship between MML and use would require there to be evidence of a change in use after passage compared to before. Longitudinal data can be useful to address this (repeated cross-sectional data is longitudinal for states), however, longitudinal datasets cannot address this question if they do not go back far enough in time to provide information on those early adopting states (the 8 with MML prior to 2000) prior to passing the MML. A limitation of NSDUH data for addressing the relationship of MML to marijuana use is that state-level data are not available prior to 2002. Stolzenberg et al. mistakenly used information from the No Law states to substitute for the lack of information in pre-passage period for states that now have laws. This biased comparison contributed to a conclusion of increased marijuana use among youth due to MMLs. To the uncritical eye, this conclusion seems plausible (and potentially worthy of media coverage and political sound bites), but it is simply not substantiated by appropriate analysis of the data. When appropriately analysed, there is no evidence of a significant differential increase in youth marijuana use post-MML based on the available NSDUH data. While states with MML feature higher rates of adolescent marijuana use, to date, no major U.S. national dataset, including the NSDUH, supports that MML are a cause of these higher use levels. An important and challenging question for future research is to determine the reason(s) that marijuana use is higher in states before they pass MML.

It is our hope that this commentary will not only clarify why the conclusion by Stolzenberg et al. is problematic, but also demonstrate that careful modeling consideration is needed when using observational data to examine the impact of policy changes. Finally, herein we only examined the effect of a binary indicator of MML status, but tremendous variability in the implementation of MML exists within and across states (Pacula et al., 2015; Smart, 2015) which presents further modeling challenges but also important opportunities for understanding the potential impact of these laws on use in adolescents.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.drugpo.2016.01.015.

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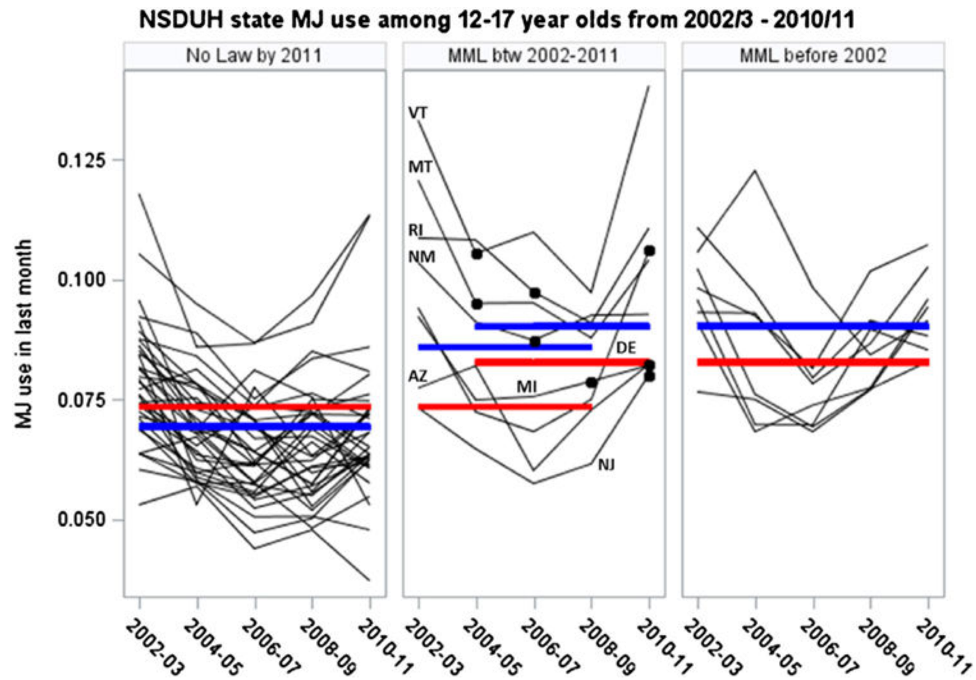

Fig. 1.

State level NSDUH prevalence of past month marijuana (MJ) use for 12–17 year olds in all 50 states from 2002 to 2011 stratified by MML status with overlay of fitted means from regression Model 1 (red) and Model 2 (blue). *Dots in middle panel indicate year MML passed for those 8 specific states (abbreviation of states shown) that passed during 2002–2011. The two fitted regression lines for each Model in the middle panel indicate pre and post-MML passage fitted mean values. Pre is the line staggered to the left, post is the line staggered to the right. Note: State-level NSDUH data used herein for 2002–2003, 2004–2005, 2006–2007, 2008–2009 were compiled at an earlier date from SAMHSA http://www.samhsa.gov/samhda. As of 11/9/2015 the web archive is unavailable. Data for 2010–2011 are available from http://archive.samhsa.gov/data/NSDUH/2k11State/NSDUHsae2011/ChangeTabs/NSDUHsaeChangeTabs2011.htm. (For interpretation of the references to colour in this legend, the reader is referred to the web version of the article.)

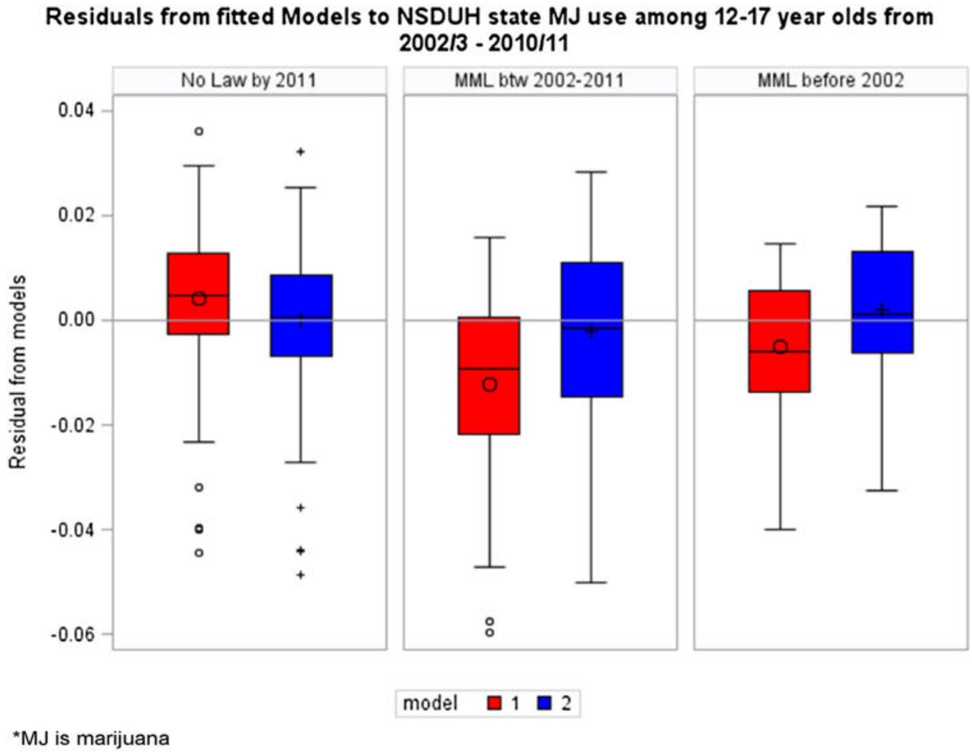

Fig. 2.
Residuals from Models 1 and 2. *MJ is marijuana.

Table 1

Mean prevalence of past-month marijuana use in 12–17 year olds for the 8 states that passed MML between 2002 and 2011.

State	Year MML passed	Period	# of years[b]	Average prevalence of marijuana use	Change (post–pre)
AZ	2010	Pre	4	7.3%	
		Post	1	8.2%	0.9%
DE	2011	Pre	4	7.7%	
		Post	1	10.6%	2.9%
MI	2008	Pre	3	8.1%	
		Post	2	8.0%	−0.0%
MT	2004	Pre	1	12.1%	
		Post	4	9.5%	−2.5%
NJ	2010	Pre	4	6.4%	
		Post	1	8.0%	1.6%
NM	2007	Pre	2	9.7%	
		Post	3	9.1%	−0.6%
RI	2006	Pre	2	10.8%	
		Post	3	10.0%	−0.9%
VT	2004	Pre	1	13.3%	
		Post	4	11.3%	−2.0%
Aggregated[a]		Pre		9.44%	
		Post		9.35%	−0.1%

[a]Aggregated results average over all 8 states which passed MML during 2002–2011 by taking simple average of pre use and simple averages of post use.

[b]#of years is number of NSDUH datapoints available pre or post-MML passage. Each NSDUH datapoint represents aggregation of two years.