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

SAM HARPER, PhD, ERIN C. STRUMPF, PhD, AND JAY S. KAUFMAN, PhD

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.

Ann Epidemiol 2012;22:207–212.

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

From the Department of Epidemiology, Biostatistics and Occupational Health (S.H., E.C.S., J.S.K.); and Department of Economics (E.C.S.), McGill University, Montreal, Canada.

Address correspondence to: Sam Harper, PhD, Department of Epidemiology, Biostatistics & Occupational Health, McGill University, 1020 Pine Avenue West, Room 34B, Montreal, QC H3A 1A2, Canada. Tel.: 514-398-2856; Fax: 514-398-4503. E-mail: sam.harper@mcgill.ca.

Received October 12, 2011. Accepted December 29, 2011. Published online January 29, 2012.

1047-2797/$ - see front matter
doi:10.1016/j.annepidem.2011.12.002

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:

$$Y_{st} = \beta_0 + \beta_1 MML_{st} + \gamma_t + \varepsilon_{st} \tag{1}$$

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:

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

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:

$$Y_{st} = \beta_0 + \beta_1 MML_{st} + \gamma_t + \delta_s + \varepsilon_{st} \tag{3}$$

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

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

		Wall et al. (2)		Replication estimates*			
Year	No. of states with laws	With law	Without law	With law	Without law	t-statistic	p value
Prevalence of past-month marijuana use (%)							
2002–3	8	9.67	8.33	9.67	8.31	–2.24	.029
2003–4	10	9.84	7.66	9.84	7.62	–4.38	<.0001
2004–5	10	8.95	7.12	8.95	7.12	–4.00	<.0001
2005–6	11	8.57	6.63	8.57	6.63	–5.25	<.0001
2006–7	12	8.40	6.45	8.40	6.46	–5.37	<.0001
2007–8	13	8.27	6.40	8.27	6.42	–5.56	<.0001
Prevalence of perceived riskiness of monthly marijuana use (%)							
2002–3	8	29.13	33.84	29.13	33.71	2.75	.008
2003–4	10	30.82	35.44	30.82	35.29	3.06	.004
2004–5	10	30.39	35.13	30.39	34.97	3.35	.002
2005–6	11	30.00	35.09	30.00	34.95	4.04	<.0001
2006–7	12	30.02	36.01	30.02	35.78	4.50	<.0001
2007–8	13	29.53	36.17	29.53	35.93	4.94	<.0001

*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.

over time, similar to the case-crossover design (7) commonly used in epidemiologic studies. It should be noted that, whereas Wall et al. include states passing laws after 2008 (Arizona, Delaware, New Jersey) in some models as being "treated," in our model they are "control" states because during the observation period they did not pass a law. However, we conducted sensitivity analyses using only these states as a control group; because 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) to estimate the effect of passing a MML. We collected the coefficients on the law term in each of 5,000 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 (CI). All analyses were conducted with Stata (version 12; StatCorp, College Station, TX), 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 et al. (2) 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 MMLs have higher average rates of marijuana use and lower perceived riskiness of monthly use than states without laws in each data year. As Wall et al. reported, from 2002 to 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% CI, 1.4%–2.4%). Wall et al. 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%; $P = .25$), 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 with states that never passed a law (1.65%; 95% CI, 1.0–2.3). Replication estimates for perceived riskiness were similar to Wall et al., and full results from these replication models are given in the web appendix.

Table 2 shows the ordinary least-squares regression estimates with year fixed effects (equation 1) and difference-in-differences estimates (equation 3) for the impact of passing MMLs 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 et al., 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-

TABLE 2. Regression estimates of the effect of passing medical marijuana laws on marijuana use among 12- to 17-year-olds, 2003 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 to 2.2	−0.59	−1.1 to −0.1	−5.19	−6.3 to −4.1	0.72	−1.1 to 2.5
Constant	8.23	7.9 to 8.6	8.62	8.4 to 8.8	33.8	32.7 to 34.9	32.88	32.4 to 33.3
Year fixed effects	Yes		Yes		Yes		Yes	
State fixed effects	No		Yes		No		Yes	

CI = confidence interval.

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 an 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, although the latter estimate is not statistically different from zero (95% CI, –1.1 to 2.5). Perhaps more important, for both outcomes the difference-in-differences estimate was considerably different from the estimate controlling only for year fixed effects (both $P < .0001$ for a test that 2 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 ($\beta = -0.41$; 95% CI, –1.1 to 0.2) and perceived riskiness of monthly use ($\beta = 1.13$; 95% CI, –0.4 to 2.7), and in both cases the 95% CIs showed no overlap with the estimates controlling only for year fixed effects.

Table 3 shows regression estimates based on data through 2008–2009 for 4 age groups using 4 different estimation strategies: (i) ordinary least-squares with year fixed effects, (ii) difference-in-differences, (iii) difference-in-differences accounting for measurement error in the state-level estimates of marijuana use, and (iv) 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 (range, 2.2–4.7 percentage points) and decreased perceived riskiness of monthly marijuana use (range, 5.2–7.6 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 to 17 years, there remains some evidence that passing a law decreases the rate of past-month marijuana use by half a percentage point (95% CI, –1.0 to –0.0), but accounting for measurement error in the state-level estimates of marijuana use leads to

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

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

CI = confidence interval.
n = 357 for all models. Estimates of 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 5,000 simulations.
*n = 112. Control states are AZ, DE, NJ.

a less-precise estimate (95% CI, –1.3 to 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 et al. suggested that states passing MMLs since 2004 had higher prevalence of marijuana use than states not passing laws. They gave 3 possible explanations for this finding: (i) States with higher use are more likely to enact laws, (ii) laws increase use, and (iii) unmeasured factors (e.g., social norms about drug use) that affect marijuana use and affect the likelihood of passing a law may be more common in states that passed laws. Our analysis, using the same data, replicated Wall et al.'s results and attempts to provide some additional evidence as to which of these 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 after passing MMLs. 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 (9) recently found some evidence of increased validity (based on urinalysis) of self-reporting among arrestees in states with MMLs (which would seem to work against this interpretation), but this result seems unlikely to be generalizable. Our analyses are most consistent with the third proposed explanation of Wall et al.: Unmeasured confounding. U.S. states differ in many respects that may not be easily measured in standard epidemiologic 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 MMLs increases reported use, among adolescents or any other age group.

Although our analysis controls for unmeasured, time-invariant state characteristics, it also has limitations. As Wall et al. noted, the limited number of policy changes over the period from 2002 to 2009 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 (although 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, because 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, because the NSDUH estimates are 2-year averages, there is uncertainty about in which year of data the law should be considered as being enacted. We followed Wall et al., 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, although the effect on monthly use among 12- to 17-year-olds (shown in Table 3) 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 MMLs 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, although that seems 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 (available: http://dvn.iq.harvard.edu/dvn/dv/samharper).

CONCLUSION

We replicated the findings of Wall et al. (2) that marijuana use was higher in states that have passed MMLs, and our analysis suggests this is unlikely to be a causal association. Our difference-in-differences estimates suggest little detectable effects of passing MMLs 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.

Sam Harper was supported by a Chercheur Boursier Junior 1 from the Fonds de la Recherche en Santé du Québec. Erin Strumpf was supported by a Research Scholars career award funded by the Quebec Research Fund in Health and the Quebec Ministry of Health and Social Services. Jay Kaufman was supported by a Canada Research Chair in Health Disparities.

SUPPLEMENTARY DATA

Supplementary data related to this article can be found online at doi:10.1016/j.annepidem.2011.12.002.

REFERENCES

1. Eddy M. Medical marijuana: Review and analysis of federal and state policies. CRS Report for Congress RL33211; 2010. Available from: http://assets.opencrs.com/rpts/RL33211_20100402.pdf. Accessed August 16, 2011.
2. Wall MM, Poh E, Cerdá M, Keyes KM, Galea S, Hasin DS. Adolescent marijuana use from 2002 to 2008: higher in States with medical marijuana laws, cause still unclear. Ann Epidemiol. 2011;21:714–716.
3. U.S. Department of Health and Human Services, Substance Abuse and Mental Health Services Administration, Office of Applied Studies. State level data on alcohol, tobacco, and illegal drug use; 2010. Available from: http://www.oas.samhsa.gov/states.cfm. Accessed August 16, 2011.
4. Rabe-Hesketh S, Skrondal A. Multilevel and longitudinal modeling using Stata. 2nd ed. College Station, TX: Stata Press; 2008.
5. Meyer BD. Natural and quasi-experiments in economics. Journal of Business & Economic Statistics. 1995;13:151–161.
6. Angrist JD, Pischke JS. Mostly harmless econometrics: an empiricist's companion. Princeton: Princeton University Press; 2008.
7. Maclure M. The case-crossover design: A method for studying transient effects on the risk of acute events. Am J Epidemiol. 1991;133:144–153.
8. Bertrand M, Duflo E, Mullainathan S. How much should we trust differences-in-differences estimates? The Quarterly Journal of Economics. 2004;119:249–275.
9. Miller RN, Kuhns JB. Exploring the impact of medical marijuana laws on the validity of self-reported marijuana use among juvenile arrestees over time. Criminal Justice Policy Review. 2011. Available from: http://cjp.sagepub.com/content/early/2011/01/10/0887403410392026.abstract
10. Dee T. Does setting limits save lives? The case of 0.08 BAC laws. J Pol Anal Manage. 2001;20:111–128.
11. Chaloupka FJ, Grossman M, Saffer H. The effects of price on alcohol consumption and alcohol-related problems. Alcohol Res Health. 2002;26:22–34.
12. Carpenter C, Harris K. How do "point oh-eight" (.08) BAC laws work? The BE Journal of Economic Analysis & Policy. 2005;5:6.
13. Cohen PJ. Medical marijuana 2010: It's time to fix the regulatory vacuum. J Law Med Ethics. 2010;38:654–666.
14. King G. Replication, replication. PS: Political Science & Politics. 1995;28:444–452.
15. Freese J. Replication standards for quantitative social science: Why not sociology? Sociological Methods & Research. 2007;36:153–172.
16. Hernán MA, Wilcox AJ. Epidemiology, data sharing, and the challenge of scientific replication. Epidemiology. 2009;20:167–168.
17. Gorman DM, Charles Huber J Jr. Do medical cannabis laws encourage cannabis use? Int J Drug Policy. 2007;18:160–167.