American Journal of Epidemiology Advance Access published June 16, 2016

American Journal of Epidemiology


# Letter to the Editor of *Epidemiologic Reviews*

*THE AUTHORS REPLY*

We would like to thank the *American Journal of Epidemiology* and *Epidemiologic Reviews* for the opportunity to respond to the points raised by Drs. Lott, Moody, and Whitley (1) in reference to our paper, "What Do We Know About the Association Between Firearm Legislation and Firearm-Related Injuries?" (2). We will address the critiques by Lott et al. in turn.

First, Lott et al. suggested that we selectively chose data for our figures to fit a predetermined a message. We disagree. Our figures were based on studies for which findings were presented in terms of percent changes in homicide rates because this was the most frequently reported result across studies. This strategy allowed us to present the largest number of studies in the figures. Whenever possible, we also calculated percent changes in homicide rates from papers in which they weren't explicitly stated but that provided enough information for them to be calculated (Web Appendix 1, available at http://aje.oxfordjournals.org/, provides information about estimates presented in figures). Specifically for studies of shall issue laws, we selected estimates from dummy models because most studies provided these estimates. However, we presented results from other models (e.g., trend or state-specific models) in Web Table 1 of our original article (2). The main text of the review was also organized in a way that conveyed the main message of all of the studies. Therefore, the data in the figures were not in any way selected based on a predetermined message, but rather represented a sample of the studies that could be combined in 1 figure.

Second, Lott et al. criticized us for giving equal weight to refereed and nonrefereed studies. Our review did indeed include studies from the published and grey literature, as is standard recommended practice in systematic reviews. We also used a well-validated set of criteria from the Guide to Community Preventive Services (3, 4) to evaluate the design suitability and quality of execution of the studies. This tool allowed us to avoid potential issues of publication bias (which would have arisen had we only examined published papers) while at the same time using an objective set of criteria to evaluate sources of bias in the literature.

Third, Lott et al. also suggested that we omitted papers that had results that did not support gun control. In our systematic literature review, we attempted to include all possible studies in which investigators examined whether firearm laws were associated with changes in overall or firearm-related deaths and injuries; however, studies were not included if they did not focus on our review question of interest or if they did not fit our inclusion criteria. In addition, it is possible that our search strategy failed to detect some studies, because no search is completely exhaustive and such gaps do arise. Further, regarding the papers that Lott et al. claim we did not include, the estimates from Bartley and Cohen (5), Gius (6), and Lott and Whitley (7) were described in both the text and in Web Table 1 of our original article (2).

Fourth, Lott et al. disagreed with our characterization of some studies. We address each critique here.

With regard to the study by Plassmann and Whitley (8) that was included in our review, Lott et al. indicated that we misreported their weighted least square estimate from Table 3a, making it appear as if the results included zero in the 95% confidence interval. This is incorrect: In Figure 2, we showed that the percent change was −6.20 (95% confidence interval: −12.28, −0.12), which is consistent with what Plassmann and Whitley (8) reported in Table 3a and indeed shows that the estimate is significant at the 95% confidence level.

Lott et al. also criticized the focus on dummy model estimates from the studies by Plassman and Whitely (8), Helland and Tabarrok (9), and Moody and Marvell (10) in Figure 2. We reported the dummy model estimates to be consistent with the majority of studies on shall issue law, as explained above. Also, we provided results from trend models for all of these studies (including estimates from Poison models in Plassmann and Whitley) in Web Table 1 of our original article. Further, in the main text, we indicated that these 3 studies (8–10) ". . . supported Lott and Mustard's findings" (2, p. 142), that is, that shall issue laws were associated with lower rates of homicide at the county and state levels.

Lott et al. also criticized us for reporting the results from Table 6.5 in Wellford et al. (11), which do not account for any control variables. We chose to report this estimate (in addition to reporting the adjusted results in Web Table 1 of our original article (2)) to be consistent with the statement by the authors that the results were "sensitive to the inclusion of controls" (11, p. 149), "the laws had no obvious effect in the model without controls (and therefore no clear level effect in the raw data)" (11, p. 149), and it was "not possible to determine that there is a causal link between the passage of right-to-carry laws and crime rates" (11, p. 150).

Lott et al. suggested that the study by Black and Nagin (12) implied a negative relationship between right-to-carry laws and homicide and that we reported the only national estimate that implied a positive effect. We disagree. Different results reported in the paper showed no significant association between shall issue laws and homicide (e.g., Table 1 showed no association when 1 state was excluded from analysis; Table 2 showed no difference between the sum of coefficients in years prior and after adoption of laws; and moreover, this is how authors refer to results in Table 2: "We find no statistically significant evidence that [right-to-carry] laws have an impact on any of the crime rates" (12, p. 218)). Lott et al. further criticized the focus on the findings of Black and Nagin that were based on a quadratic specification of time. We focused on these results because according to the authors, they

adequately captured time trends in a way that linear trends did not. In the words of the authors, "The results suggest that the Lott and Mustard model, which includes only a single national trend, does not adequately capture local time trends in crime rates . . . In Table 2, we report the results for models with a quadratic time trend. The only significant impact estimate is for assaults, and its sign is positive, not negative" (12, p. 218). In addition, Black and Nagin concluded, "Our reanalysis of Lott and Mustard's data provides no basis for drawing confident conclusions about the impact of [right-to-carry] laws on violent crimes . . . Finally, a more general model based on year-to-year differences yields no evidence of significant impact" (12, pp. 218–219).

With regard to the study by Ayres and Donohue (13) on shall issue laws, Lott et al. suggested that we should have reported estimates from Table 8b, which showed the homicide rate trends before and after legislation, because they showed a statistically significant downward trend in rates. In fact, none of the results from the models presented in Table 8b by Ayres and Donohue show a significant relationship between shall issue laws and homicides at the 95% confidence level. Indeed, Ayres and Donohue's point is that shall issue laws are not robustly associated with changes in crime, including homicide rates, and that estimates vary according to different model specifications.

Lott et al. also criticized us for reporting an estimate from a dummy model in Figure 2 of Lott's book (14). Lott et al. stated that the simple before and after averages provided an underestimation of the true benefit of these laws. As we mentioned before, in Figure 2 of our original article (2), we presented estimates from dummy models to be consistent across studies of shall issue laws; however, we also provided estimates from trend models in Web Table 1 of our article and indicated in the main text that the overall message from this publication was that shall issue laws were associated with reductions in homicide rates.

Lott et al. further claimed that "another error" in our article related to the book by Lott (15) was the decision to report results that related to the castle doctrine laws as though they applied to right-to-carry laws. This is not an error. As it was clearly shown in our Figure 2, the value we reported for "Lott, 2010 (47)" was for castle doctrine laws.

Finally, Lott et al. criticized the estimate we provided for the study by Koper and Roth (16) because we reported a point estimate with perfect certainty. In our original Figure 3, we indicated that 95% confidence intervals were not provided, which means that confidence intervals are not shown, not that we are reporting the estimate with perfect certainty. Moreover, to be consistent with what Koper and Roth discussed in their paper, we described in Web Table 1 of our original article (2) that the federal law was associated with a nonsignificant reduction (6.7%) in firearm homicide rates. In the text, we also mentioned that "Koper and Roth . . . using [Uniform Crime Reports] data (1980–1995) found no association between the law and homicide rates in 15 states" (2, p. 149).

At a broader level, Lott et al. implied that we chose to exclude findings from papers because they did not fit our perspective on firearms. This is wrong. Many of the studies included a vast number of estimates. However, given the guidelines of *Epidemiologic Reviews*, we were constrained by space and had to make a choice as to which estimates to report. To avoid bias, we made a clear and systematic decision to focus on the primary results from each paper we reviewed, as stated by the authors of each paper. Hence, secondary findings from papers were not included because of the space limitations and not because of a particular worldview. Further, we believe that the decision to not report secondary findings does not dramatically impact the overall findings that emerged from our literature review. Our systematic literature review included the work from 130 national and international studies, providing a useful resource for those interested in research on firearm regulations and their associations with overall and firearm-related injuries and deaths. We hope researchers can use it to constructively move forward and identify new directions to further our understanding of the impact of firearm policies on injuries.

## Acknowledgments

Conflict of interest: none declared.

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DOI: 10.1093/aje/kww052