Letter to the Editor of *Epidemiologic Reviews*

RE: "WHAT DO WE KNOW ABOUT THE ASSOCIATION BETWEEN FIREARM LEGISLATION AND FIREARM-RELATED INJURIES?"

In their article, Santaella-Tenorio et al. (1) repeated that they provided a summary of results from studies in which researchers investigated at the impact of various gun control laws on crime rates. In legends of their Figures 2–4, they stated that they presented only a single estimate from each study because of space limitations. The Discussion section of their article reads as though the authors were providing a representative result. Instead, from papers that provide hundreds of results, they picked the most extreme result time after time and misreported others.

There are 5 problems with the way Santaella-Tenorio et al. created their figures: 1) They consistently picked results that were the most favorable single result for gun control in the papers they surveyed; 2) they picked results that the authors of those papers rejected; 3) they gave equal weight to refereed and nonrefereed papers; 4) they left out papers from their surveys that have results that do not support gun control; and 5) they inaccurately reported some results. The errors here also apply to all the tables in the article by Santaella-Tenorio et al.; however, because of space considerations, we will focus only on some of the errors in their figure about right-to-carry laws (Figure 2 in their original paper (1)). We also feel strongly that our findings in previous works (2–6) have been misreported.

In the articles by Plassmann and Whitley (2) and Plassmann and Tideman (7), the authors argued that weighted least square estimates bias "will bias the estimated benefit of the concealed handgun law towards finding an increase in crime" (2, p. 14). They argued that the estimates should be determined using a count data approach. Yet, Santaella-Tenorio et al. only reported their simple dummy variable estimates using weighted least squares. Santaella-Tenorio et al. misreported the weighted least square estimate from Table 3a of the article by Plassmann and Whitley (2), making it appear that the results included zero in the 95% confidence interval even though the result was statistically significantly different from zero at the 5% level for a 2-tailed *t*-test. However, Plassmann and Whitley argue that the Poisson estimate in Table 8, which was statistically significant at better than the 1% level, should have been reported.

Of the estimates in the article by Wellford et al. (8), Santaella-Tenorio et al. reported only the result in Table 6.5 without any control variables other than fixed effects and the law dummy. That estimate is also biased toward zero because the endogenous variable in that case had a high percentage of observations that had zero values and the truncation bias was not taken into account. Note that all of the other results in which the authors accounted for any combination of other control variables showed that right-to-carry laws reduced murder rates.

In the article by Black and Nagin (9), there is an implied negative relationship between right-to-carry laws and homicide rates in all but 1 of their specifications. However, Santaella-Tenorio et al. picked the only national estimate that implied a positive association. The aberrant result is easily explained: It is because that specification included state-specific quadratic time trends. Suppose that homicide rates were rising before the law and then began to fall afterward. In that case, the state-specific quadratic time trends would account for the entire reduction in crime resulting from the right-to-carry law and there would be nothing left for the dummy variable for that law to detect.

For the book by Lott (10), Santaella-Tenorio et al. reported 1 estimate from Table 4.1 out of the more than 25,000 estimates provided in the book (specification bounds on all possible combinations of the control variables are reported on pages 184–190). Lott clearly argued that simple before and after averages were very misleading and in this case provided an underestimation of the true benefit from these laws (10, pp. 212–215). The dummy variable estimate assumes that the crime levels are at constant levels before and after implementation of the law. The point of the dummy test is to see whether the constant level before implementation of the law differs from the constant level after. Yet, the trends estimates before and after the law reject this assumption (see also Bronars and Lott (3)).

The problems with using the dummy variable can be illustrated using results of 3 other papers. Santaella-Tenorio et al. reported the dummy model from Table 8b of the article by Ayres and Donohue (11). Had they reported the other specification in Table 8b (or other tables) that showed the trends before and after implementation of the law (specifications that reject the assumptions behind the simple dummy approach), they would have shown the statistically significant downward trend in murder rates that indicated that the longer the right-to-carry laws were in effect, the greater the drop in murder rates was.

The results from Helland and Tabarrok (12) depended on whether they used the simple dummy variable or the trends before and after the legislation. Santaella-Tenorio et al. again only reported the simple dummy estimate. In addition, Helland and Tabarrok noted that the issue was how violent crime rates changed relative to property crimes. They wrote that, after taking this into account, "we estimate that the probability of this [relative reduction in murder] occurring by chance is less than one in 1,000" (12, p. 5).

Santaella-Tenorio et al. also reported a dummy variable estimate from Table 5a of the article by Moody and Marvell (4). When Moody and Marvell corrected the data for the trends (as they did in Table 7a), which were highly significant, they found a statistically significant drop in murder rates after right-to-carry laws are enacted.

There were other errors as well. For example, Santaella-Tenorio et al. (1) used data from Table 10.14 in the book

by Lott (5), but that table shows the impact on violent crime rates of the castle doctrine laws, not right-to-carry laws.

Santaella-Tenorio et al. did not include 7 peer-reviewed papers that were published during the time period that they covered and that showed statistically significant benefits from right-to-carry laws: Bartley and Cohen (13), Lott (14), Benson and Mast (15), Gius (16), Lott and Whitley (17), Lott and Whitley (18), and Moody et al. (19). On the other hand, they include nonpublished (Aneja et al. (20)) and nonrefereed papers (Ayres and Donohue (11), Donohue (21), and Ayres and Donohue (22)).

Santaella-Tenorio et al. also omitted papers that did not fit their views in other areas. For example, Lott (5) and Moody and Marvell (6) examined the simultaneous impact of multiple laws (indeed, more laws than cited by Santaella-Tenorio et al.), but these papers were excluded. All of the above types of problems apply to their discussions of the assault weapon ban, Brady background checks, background checks on private transfers of guns, the impact of preventing access to guns by children on accidents and suicides, and prevention of suicides more generally.

We conclude with a final example. Figure 3 in the article by Santaella-Tenorio et al., which includes data about assault weapons, shows a point estimate demonstrating that the ban reduced the number homicides and appears to indicate the statistical significance is not reported; however, the paper by Koper and Roth (23) clearly reports that estimate is not statistically significant at even the 10% level.

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