Comment on Webster et al. (2014) Study of the Impact of the Repeal of Missouri's Handgun Permit Law on Homicide

Gary Kleck

College of Criminology and Criminal Justice

Florida State University

gkleck@fsu.edu

December 1, 2017

Abstract

Daniel Webster and his colleagues (2014) tried to estimate the effect of Missouri (MO) repealing its handgun permit-to-purchase (PTP) law in 2007, using an unsuitable research design, misspecified statistical models, and a biased sample of states. They also examined simple trends in homicide rates in MO and in comparison states. They concluded that the repeal caused an immediate (and astounding) increase of 25% in the firearm homicide rate. They attributed this result at least partially to increased "illegal gun diversion" (a term they never defined). Their results cannot be relied upon to indicate whether the repeal actually caused any homicide increase, or increased movement of guns into criminals' possession. This comment explains why.

Webster and his colleagues claim that repeal of Missouri's (MO) permit-to-purchase law caused an increase in homicide.

Webster et al. Failed to Establish Any a Priori Plausibility for the Hypothesis

Before reviewing the flaws in Webster et al.'s empirical work, it is worth considering the *a priori* plausibility of the claim that repealing MO's PTP could cause a 25% increase in firearm homicide, as Webster et al. hint. They seriously suggest that a single trivial change in the details of MO gun law could, all by itself, cause a 25% increase in gun homicide. The authors use purely associational language in describing their results (e.g., the repeal was "associated with" a 25% increase in firearm homicide), but read in context, the implied meaning of a causal effect is unmistakable.

Here is why the revision was trivial. Repealing the PTP law did not eliminate background checks on firearms; all gun transfers by licensed gun dealers continued to be subject to a background check. Webster et al. argue that the key change produced by repeal of the PTP law was that it "eliminated mandatory background checks for handguns sold by unlicensed sellers." Whether this change was likely to be consequential, however, depends entirely on how often background checks on private transfers were performed before the PTP repeal, and how many blocked a gun transfer. If very few or no such transfers were blocked before the repeal, there is no reason to expect that getting rid of them would have a measurable effect on criminal gun possession, and thus on homicide. Webster et al. did not cite a single scrap of evidence that *any* private gun transfers were blocked under the old PTP system, and show no signs that it even occurred to them that it was important for them to do so. As far as readers can tell, no nondealer transfers of handguns to criminals had been blocked before the PTP repeal, so there is no evidentiary foundation for believing the repeal had any measurable effect on the number of criminals who acquired handguns.

Guns “Diverted to Criminals”

Webster et al. claim that the repeal of the MO PTP law caused increased illegal diversion of guns to criminals, which in turn increased firearm homicides. They do not define what they mean by the term “illegal gun diversion,” but a reasonable guess would be that it simply means *any* illegal movement of guns into criminals’ hands. Thus, it could encompass gun theft, guns purchased from gun traffickers, guns illegally purchased from corrupt licensed dealers, guns acquired through the use of straw purchasers, guns illegally purchased by convicted felons from private parties, guns received as a gift by persons not lawfully entitled to possess guns, and a host of other diverse ways that guns might illegally move into criminal hands. Webster et al. appear to argue that repealing MO’s PTP law caused gun possession to increase among criminals, without being any more specific as to how or why this occurred. In any case, it needs to be stressed that “illegal gun diversion,” as Webster et al. use the term, does not necessarily refer to gun trafficking in particular, and instead may refer to literally any illegal way guns might end up in criminal hands. They provided no specific argument for which of these kinds of illegal movements of guns increased after the repeal. If, for example, the only kind of movement that increased was gun theft, one might reasonably ask “why should the PTP repeal increase gun thefts?” If the authors are correct that repealing PTP provisions made it easier for criminals to *buy* guns, this should have *reduced* the need to steal guns, i.e. reduced one major form of “illegal gun diversion.”

The indicator that Webster et al. used to measure “illegal gun diversion” was “the percentage of guns that had unusually short intervals between the retail sale and the recovery by police" (p. 294). This definition left it unclear which set of "guns" the authors were referring to, but it turns out that the authors meant the set of guns recovered by police in connection with some real or suspected criminal activity and submitted to the Bureau of Alcohol, Tobacco and Firearms (ATF) for tracing. This measure, however, is not a valid measure of either acquisition or possession of guns by criminals, or of gun trafficking, or gun "diversion," and has never passed any check of its validity as a measure of these concepts (Kleck and Wang 2009). Webster et al. did not cite any validation studies. While it is true that many careless analysts have also misinterpreted this indicator as a measure of "firearm diversion or trafficking," it is no such thing, and has failed every test of its validity (Kleck and Wang 2009).

Webster et al. concluded from their analysis of trace data that the increase in recovered crime guns with a short TTC that followed MO's repeal of the PTP law reflected an increase in "illegal gun diversion." Likewise, they speculated that the repeal caused a decrease in guns sold by out-of-state suppliers. In reality, changes in the geographic origins of recovered guns are more likely to reflect patterns in ordinary migration. Noncriminals legally purchase guns in state X, and later move to MO. Some of these migrants have their guns stolen, and some of these guns are subsequently used in crimes, recovered by police, and submitted for ATF tracing. Tracing results would show that the guns originated in state X, but the inference that interstate gun smugglers or weak gun laws in the origin state were responsible for the movement of these guns would, in almost all cases, be unwarranted (Kleck and Wang 2009).

The first thing to note about the set of guns submitted for ATF tracing is that it is not a representative sample of all crime guns, or even of all crime guns recovered by police, or any subset of these populations of guns. There can be no honest doubt about this point among users of ATF trace data, as ATF issues a quite explicit disclaimer on this point: "Firearms selected for tracing … do not constitute a random sample and should not be considered representative of the larger universe of all firearms used by criminals, or any subset of that universe" (U.S. ATF 2013). This disclaimer only confirms the same conclusions that had been previously drawn by the Congressional Research Service (1992) and the National Research Council (2005, p. 40).

The National Research Council panel also concluded that "trace data cannot show whether a firearm has been illegally diverted from legitimate firearms commerce" (p. 40) and that "trace data analyses cannot describe the illegal pathways thorough which crime guns travel from legal commerce to its ultimate recovery by law enforcement" (pp. 80-81). Thus, Webster et al. were wrong to believe that they could use trace data to measure illegal diversion of guns to criminals. These facts about the trace data have been well-known for decades, suggesting that Webster et al. are either remarkably ignorant of the basic facts about the data on which they rely, or they were knowingly misleading their readers as to what could be inferred from changes in time-to-recovery among ATF-traced guns.

Webster et al.'s indicator of diversion has been tested for validity as a measure of gun trafficking (a specific subtype of illegal gun diversion), and it was found that it had no validity whatsoever. In fact, the share of recovered "crime guns" with short times to recovery actually has a weak *negative* association with a widely accepted indicator of trafficking, the share of recovered guns that had an obliterated serial number (Kleck and Wang 2009, p. 1283). That is, Webster et al.'s indicator actually tends to be *lower* where gun trafficking is higher.

Thus, Webster et al.'s analyses of traced gun data can tell us nothing about trends in gun trafficking or illegal gun diversion. They misused trace data for a purpose that they cannot legitimately serve. One cannot legitimately use trace data to infer anything about the guns used to commit crimes, including the interval from their first retail sale to their recovery by police (better known as “time-to-crime” (TTC) or “time-to-recovery”), and this time interval is not an indicator of gun trafficking. Likewise, one cannot use trace data to track trends in gun trafficking or “illegal gun diversion.”

And even if short time-to-crime *were* a valid indicator of illegal gun diversion, and the guns chosen for tracing by police *were* a random sample of “crime guns” in general, analysis of general samples of traced guns still could not tell us anything about the guns used in homicides, since only a tiny fraction of traced firearms are recovered in connection with homicides. For example, among the 4,341 guns submitted for tracing in Missouri in 2011, only 134, or *3.1 percent* were linked with homicide (ATF 2013). Indeed, few of these guns had been used to commit *any* violent crime – only 534, or 12 percent were linked with homicide, aggravated assault, or robbery. Instead, traced guns were most commonly linked with violations of gun control laws, such as unlawful “Possession of Weapon.” Thus, even if one assumed, contrary to empirical evidence, that short TTC among all traced “crime guns” was an indicator of illegal gun diversion, it would not be an indicator of diversion of guns used in homicide or in violent crime. In principle, Webster et al. might have used trace data to measure the average time-to-crime among MO guns recovered in connection with homicides, since those data are available, but chose not to do so.

In sum, Webster et al. had no scientific foundation for drawing even the weakest conclusions about trends in gun trafficking or “illegal gun diversion” after MO repealed its PTP law.

Webster et al.'s Simple Comparisons of Homicide Trends in MO with Trends in Other States

Webster et al. reported simple comparisons of trends in age-adjusted firearm homicide rates in MO over the period 1999-2010 with trends in other states and with the U.S. as a whole (Figure 1 and Table 1). This is an unusually short period of time to analyze in this type of panel design, and is prone to extremely unstable results with respect to exactly what set of years one happens to analyze. Certainly data availability cannot explain the authors' decision to study so few years, since state firearms homicide data are available from at least as far back as 1968. The authors offer a different, bizarre explanation for why they used such an unusually short time series, arguing that 1999-2012 was a period of stable homicide rates, which they speculate means that an analysis of homicide rates will be less subject to omitted variable bias. There is no justification of this type for short time series in the statistical literature, and the single source they cite in support (see their source 16) does not in fact support the use of such a short time series. A longer time period would have provided a more stable set of estimates of the repeal's effects.

The results of longitudinal analyses can be radically manipulated simply by analyzing arbitrarily selected subsets of the total set of time points for which data are available (see Britt, Kleck, and Bordua 1996 for a direct demonstration of radical changes in estimates of the impact of a gun law change when different sets of years were analyzed). Likewise, if we repeat the authors' simple before-and-after comparisons of firearm homicide rates, but use different sets of years to compare, we arrive at quite different results. One could argue that 2007 should not have been treated as a pre-repeal year since part of the year (after 8-28-07) was after the repeal was in effect. Using the last two years before 2007 as the pre-repeal baseline, the firearm homicide rate was 5.2. And one could argue that the lasting effects of the repeal would be better observed in a later set of post-repeal years, 2011-2013. The mean firearm homicide rate in those years was also 5.2, indicating no lasting increase in firearm homicide after the PTP repeal (CDC WONDER 2015).

Webster et al. never explain why they chose to make crude comparisons of MO with the entire U.S. in Figure 1, rather than only making more refined comparisons with specific states that were demonstrably similar to MO. When Webster et al. did compare MO with specific states (Table 1), their results directly contradicted their interpretation of the drops in firearm homicide in MO. The data showed that Nebraska experienced a much larger percentage drop in firearm homicide rates, even though it neither repealed a PTP system nor weakened its gun laws in any other way. Furthermore, the authors' choice of years to compare before and after the PTP repeal concealed that fact that Iowa also experienced a larger percent drop in firearms homicide rates that MO experienced. Just the year before MO dropped the PTP element of their gun laws, from 2006 to 2007, Iowa's firearm homicide rate had dropped by 52.9% (from CDC WOMDER website). The authors carry out meaningless significance tests to provide a spurious pretext for ignoring these results. Significance tests are meaningless with population data, since they test for random sampling error. Vital statistics homicide rates are based on complete counts of all homicides occurring in a given location, as the authors themselves note, and thus are not subject to sampling error. The tests did, however, provide a pretext for ignoring the results in Iowa and Nebraska – they were not statistically "significant" (p. 297). What the results in fact indicate is that huge drops in firearm homicide rates of individual states in the 2006-2008 period were not uniquely confined to MO or even especially unusual. MO was merely one of at least three

Midwest states that experienced such huge drops, and it was not even the one that experienced the biggest drop.

These simple comparisons are of little value because they merely establish *when* changes in homicide rates occurred, but cannot tell us *why* they changed. If the PTP provisions had been keeping down firearm homicide before its repeal, its continuing absence throughout the post-2007 period should have continued to contribute to higher gun homicide rates for years after 2007. In fact, all of the post-repeal increase in MO homicide occurred in just a single year, from 2007 (4.6 firearms homicides per 100,000) to 2008 (6.2). After 2008, MO experienced no further homicide increases, contrary to an interpretation that the PTP repeal was what was responsible for the post-2007 increase in homicide. So the issue boils down to why homicide increased in this single year. The authors' use of multi-year averages in Table 1 concealed the extreme instability of single-year state homicide rates, the fact that all of MO's homicide decline occurred in a single year, and that other states experienced even larger one-year increases during this period.

Webster et al. noted other states or areas where no such homicide increases occurred, suggesting that MO's increase was unique or at least extremely unusual, and therefore must be due to something peculiar to MO. The reality is that both Iowa and Nebraska experienced jumps in their homicide rate during this same period, and their increases were *bigger* than that of MO. Thus, Webster et al. were flat wrong to claim that "Missouri's sharp increase in firearm homicides was unique within the region" (p. 299). Quite the contrary, big single-year percent changes in firearm homicide were commonplace in the region. Iowa's firearm homicide rate increased by 88% from 0.60 in 2007 to 1.13 in 2008, even though Iowa did not repeal its PTP law, which was on the books in both 2007 and 2009 (see ATF 2005, p. 183 and ATF 2010, p.

202). Likewise, Nebraska experienced a 65% increase in age-adjusted firearm homicide rate from 1.7 in 2006 to 2.8 in 2008 (CDC WONDER 2015), even though it did not repeal any of its gun control laws either. By comparison, the firearm homicide rate in MO increased by only 35% from 2007 to 2008, its sharpest rate of increase following the PTP repeal. The only reason Webster and his colleagues could make it appear that big gun homicide increases were unique to MO was by highly selective reporting of the trend data available to them.

The point is not that we can tell anything useful about the reasons for homicide changes in either Iowa or Nebraska or MO from these kinds of simplistic comparisons. Rather, the Iowa and Nebraska data demonstrate that single states can easily experience year-to-year homicide increases just as large as that observed in MO without it being due to the repeal of a PTP law or any other gun control measure, that it could happen at the roughly the same time as it happened in MO, and in the same part of the country. Thus, this simple comparison does definitively establish the simple point that MO's homicide increase could easily be entirely due to other factors, like those operating in neighboring Iowa or Nebraska, besides the repeal of a PTP law.

Multivariate Homicide Analysis

The Near-complete Failure to Control for Actual Confounders

Webster et al. appear to address the possibility that other factors were responsible for the MO homicide increase with their multivariate analysis of homicide rates in 43-51 states (including the District of Columbia), over the period 1999 through 2010 (or 2012 in some analyses). They controlled for changes in some other factors that might have affected changes in homicide rates, and still found a significant association between MO's PTP law and homicide rates. That is, homicide was higher in 2008-2010 in MO (post-repeal) than in other states and years, controlling for the other factors that Webster et al. included in their statistical models.

Whether these analyses improved their ability to estimate the effect of the PTP repeal, however, depends entirely on the degree to which Webster et al. controlled specifically for *confounding variables*. A confounding variable is a variable that has *both* of two properties: (1) it has a causal effect of its own on the dependent variable (in Webster et al.'s research, the state homicide rate), *and* (2) is associated with the principle independent variable of interest (the existence or absence of the MO PTP law). If a variable lacks the first property it is not a confounder because it does not affect the homicide rate, i.e., it is an "irrelevant variable." If it lacks the second property, it does not matter whether the variable is statistically controlled, since estimates of the impact of the PTP law will be the same regardless of its inclusion in statistical models of homicide rates. Its inclusion is simply inconsequential.

Controlling for a confounding variable serves to rule out an alternative explanation of why homicide changed in MO after the repeal. Thus, if one controls for confounding factor X, one rules out the possibility that changes in X caused the homicide increases rather than the PTP repeal. The more genuine confounding variables one controls, the more confidence one can have in the analyst's estimate of the effect of the key independent variable.

Unfortunately, we can tell from Webster et al.'s own statistical results that they controlled for virtually no genuine confounders, and thus did virtually nothing to rule out any specific variables as being responsible for the MO homicide increase. They report that only three of their control variables were significantly related to homicide rates (p. 298), and thus might be confounders. All the rest of Webster et al.'s control variables lacked the first necessary property of a genuine confounder, that they have a causal effect on the homicide rate.

Further, even the findings regarding two of Webster et al.'s three significant control variables were perverse and contrary to theory and prior research. The significant coefficient for poverty was negative, indicating that higher poverty rates cause *lower* homicide rates! This is contrary to a mountain of prior (and more sophisticated) research indicating that greater economic deprivation causes higher homicide rates (for classic reviews, see Kovandzic, Vieraitis, and Leisley 1998; and Land, McCall, and Cohen 1990). This bizarre finding is itself strong reason to believe there is something seriously wrong with Webster et al.'s statistical models. Likewise, Webster et al.'s analysis yielded a significant positive coefficient for bans on "Saturday night special" handguns, indicating that these bans significantly *increases* the homicide rate. While the National Rifle Association might welcome this finding, it is doubtful that Webster et al. themselves would regard it as a sensible finding. Again, this dubious finding points to the likelihood of errors in Webster et al.'s specification of their models, in particular the omission of confounding variables.

In any case, it is clear the Webster et al. did virtually nothing to rule out any specific variables as confounders, so virtually any factors that affect homicide rates and that changed over time might be the actual causes of MO's post-repeal homicide increases. They did control for state and year dummy variables, which helps the estimates in a nonspecific way, but these controls cannot rule out any specific alternative explanations of the post-repeal increase in MO homicide.

Perhaps what is most conspicuous about Webster et al.'s statistical models, then, is the completely arbitrary character of their choice of control variables. There is no evident rhyme or reason to their choices. They include as controls variables that have been found in most prior research to have no effect on crime rates (e.g., the number of law enforcement officers, official unemployment rates, and bans on so-called "Saturday Night Specials"), while excluding variables consistently found in prior research to affect homicide rates, such as the divorce rate, the percent of the population African-American, and the percent living in urban or metropolitan areas. Note that Webster et al. did not say that they tested for effects of these variables and found them unrelated to homicide rate; rather, there is no evidence that they ever included them in their models in the first place. They did not even control for effects of other gun control laws, even though Webster's prior writings make it amply clear that he believes many such laws reduce gun crime, including not only PTP laws but also "assault weapon" bans, and gun registration laws (see Webster, Vernick, McGinty, and Alcorn 2013).

It is almost as if Webster et al. were picking and choosing control variables on some basis other than one grounded in their own empirical evidence, theory, or prior research. This is especially worrisome, because it is possible to manipulate the estimated effect of a given variable simply by failing to control for confounders. Confounders are, by definition, variables whose control will affect estimates of the variable with which they are associated. That is, failing to control for a genuine confounder will distort the estimate of the variable with which the confounder is correlated. For example, Kleck (2017) reanalyzed the data underlying a study in which the authors had found a large significant positive association between gun rates and suicide rates (Miller et al. 2007), and showed that when five genuine confounders were controlled that had not been controlled in the original analysis, the association initially observed between guns and suicide disappeared. The original analysis had only controlled for, at most, a single genuine confounder.

In the conclusions to their report, Webster et al. give the impression that they had ruled out a substantial number of plausible alternative explanation of the MO post-repeal homicide increase, listing no less than eight variables or categories of variables that could not explain this increase. This listing is deceptive because few of these implied alternative explanations were plausible in the first place, so ruling them out was a largely pointless exercise. Further, of the eight, they could in fact rule out just three of them. None of their analyses rule out the possibility that there was an outbreak of homicide in MO in 2008-2010 that was entirely caused by factors other than the PTP repeal. The other factors that Webster et al. claimed to have ruled out would not be considered by knowledgeable scholars to be likely alternative explanations of this short-term homicide increase anyway, either because the variables do not in general affect homicide rates (e.g., unemployment rates, as officially measured; policing levels; MO's Stand Your Ground Law) or because they do not change enough over short periods of time to cause large short-term homicide increases (incarceration rates).

Sample Bias in the Analyses of Age-adjusted Homicide Rates

Webster et al.'s reliance on age-adjusted firearms homicide rates derived from vital statistics mortality data resulted in a biased sample of states, one systematically slanted to favor the proposition that gun laws reduce homicide rates, or conversely that the absence or repeal of gun laws increase homicide rates. The vital statistics data on which Webster et al. relied can be obtained from the WONDER website of the Centers for Disease Control and Prevention (CDC 2014). Unfortunately, the CDC suppresses reporting of homicide data when there were fewer than ten homicides in a given year in a given state. This had the effect of systematically excluding as many as nine low homicide states from Webster et al.'s firearm homicide analyses: Delaware, Hawaii, Maine, Montana, New Hampshire, North Dakota, South Dakota, Vermont, and Wyoming (though Webster et al. claim to have used Delaware and Montana – see p. 295). These nine states, not surprisingly, all have rates of total homicide and firearms homicide that are lower than average (see, e.g. U.S., FBI 1998, pp. 76-87, 207). Further, with the exception of

Hawaii and Delaware, they also have less gun control than average (Brady Campaign 2013). States that have little gun control yet nevertheless also have little homicide contradict the hypothesis that less gun control causes more homicide. By excluding these contradictory states, Webster et al. slanted their sample in favor of finding a negative association between the presence of gun laws and homicide rates. From a statistical standpoint, it is totally irrelevant *why* they omitted those states.

This sample bias was most serious for firearms homicide because state counts of gun homicides are lower than total homicide counts, and thus more likely to be suppressed by CDC policies. Unfortunately, Webster et al. chose not to make use of UCR data to measure firearms homicide, even though the requisite data are reported for nearly all states in nearly all years in the period 1999-2012 (see, e.g., U.S. FBI, 1998, pp. 68-74, 207). Their (p. 295) claims as to why the UCR data cannot be used are without merit. They note that the UCR homicide rates are not age-adjusted, but do not acknowledge that they did not need age-adjusted rates for their purposes, since (a) they are nearly identical to non-adjusted homicide rates, and (b) state age distributions do not change enough from year to year to have any detectable effect on homicide rates. They accurately noted that the FBI has to perform interpolations for missing data from nonreporting law enforcement agencies, but did not present any evidence that these procedures introduce any significant errors in state homicide rates. And they certainly did not explain why it makes sense to introduce massive sample bias into the study by omitting the 7-9 suppressed CDC states altogether, all for the sake of avoiding purely hypothetical, and probably minor, measurement flaws in the UCR homicide rates. In sum, Webster et al. needlessly used a severely biased sample to analyze homicide rates when a relatively unbiased sample was available. They did not say a word about the pronounced differences between the omitted states and those included in their study sample regarding levels of gun control and homicide rates, or how their use of vital statistics data biased their study sample.

Note that the authors vaguely allude to use of UCR data for 1999-2012, but this was done to add two years to the time series (p. 295), a relatively trivial improvement. There is no indication that they used UCR data so as to cover the entire U.S. and eliminate the severe sample bias in their dataset based on vital statistics age-adjusted homicide data.

Use of an Inappropriate Research Design

Webster et al.'s entire strategy for estimating an effect of MO's PTP repeal on homicide is both inappropriate for this purpose and contrary to customary scholarly practice in the field. Two broad categories of research design are used to evaluate the impact of gun control measures. First, studies of gun control impact attempt to estimate the effect of a given type of gun control (such as PTP laws) on violence rates *across the full set of jurisdictions that implemented that control*, comparing violence in these multiple areas with violence in multiple areas without such controls (see Kleck and Patterson 1993 for an example, and a review of similar prior studies). These studies use either a pure cross-sectional research design – studying many different areas at a single point in time – or a panel design that studies multiple areas in multiple time periods. With either design, the analyst assesses multiple implementations of a given type of gun control, in multiple jurisdictions.

Alternatively, other studies evaluate the impact of a specific gun control measure in a single specific area, using an interrupted time series design (see Britt, Kleck, and Bordua 1996 for a review and critique). Webster et al. uses neither of these approaches, adopting the unique strategy of applying a panel design to multiple states, observed over multiple years, to estimating the effect of a *single* change in a *single* type of gun control (a PTP law) in a *single* jurisdiction at a *single* point in time. To my knowledge, Webster and his colleagues are the only researchers in the history of gun control research that have ever adopted this curious research strategy.

There is good reason why previous researchers have not adopted this approach. Researchers studying the impact of a given type of gun control such as PTP laws in multiple states are, in effect, estimating the average effect of these laws across the multiple states that have such laws. Thus, if twelve states have adopted PTP laws, the analyst has, in a sense, twelve opportunities to detect the effects of PTP laws. As a result, the possibility that some other attribute of PTP states, besides the presence of the PTP law itself, might account for their lower homicide rates becomes less likely, since this factor would have to characterize to some degree the full set of twelve PTP states, or at least a large share of them. Put another way, simultaneously studying many implementations of a PTP law helps rule out a large number of alternative explanations of lower homicide rates in PTP laws, because it is less likely that all or most of the twelve PTP states also share some other trait that actually causes their lower homicide rates. In contrast, if one studies only the implementation of a single PTP law (or its repeal) in a single jurisdiction, literally every homicide-related factor that changed in that one jurisdiction might cause its change in homicide rates. The analyst then faces the hopeless task of trying to control for an immense number of likely confounding factors.

In the case of Webster et al.'s MO homicide study, the authors were, in effect, trying to estimate the effect of a single repeal of a single type of gun control (a PTP law) in a single jurisdiction (MO) in a single very brief time period (2008-2010). It must be stressed that they were *not* assessing the impact of PTP laws in general, though that might have been a quite reasonable course of action. Nor were they assessing the impact of PTP laws that required fingerprinting of applicants in general, which they could also have done. They were instead applying a panel design to the assessment of a single change in gun law in a single place at a single time.

As a result, a change in virtually *any* homicide-related factor that occurred in MO around 2007-2010 could account for the state's homicide increase. The only specific confounding factors that Webster et al. can rule out as providing alternative explanations of MO's post-repeal homicide increase are those they explicitly controlled. Unfortunately, the only potentially confounding factor that Webster et al. explicitly controlled in the UCR homicide rate analysis was the poverty rate; all the rest of their control variables were not confounders, so controlling them did not help isolate the effect of the PTP repeal. And in the analyses based on vital statistics data from CDC, Webster et al. likewise controlled for, at most, three variables that had a significant effect on homicide rates. Even these three, however, may not have been confounders because Webster et al. present no evidence that they were correlated with the repeal of MO's PTP law.

The units of analysis in Webster et al.'s multivariate analysis of state homicide rates are the 50 states as they were observed in each of twelve years from 1999 to 2010 (or 1999-2012 in the UCR-based analyses). That is, each case for which they measured homicide rates and other variables is a "state-year" such as Missouri in 1998 or Florida in 2006. The single statistic on which they relied to draw their conclusions was the coefficient for a variable that is coded 1 for MO in 2008, MO in 2009 and MO in 2010, while it is coded 0 for all other state-years. Thus, the coefficient for this variable represents the average difference in homicide rates between (1) MO in the period 2008-2010 and (2) all other state-years, controlling for the other variables that Webster et al. included in their multivariate models.

The estimated value of this coefficient, then, is entirely dependent on just how high homicide rates were in just *three* of the 600-700 state-years in the sample. Worse still, the change in homicide that Webster et al. attributed to the PTP repeal actually occurred in just one year, 2008. The MO total age-adjusted homicide rate increased from 6.6 in 2007 to 8.3 in 2008, but after 2008 the rate declined to 7.3 in 2009 and then leveled off to 7.4 in 2010 (CDC Wonder). Further, UCR-based data indicate that the homicide rate declined from 7.0 in 2010 to 6.1 in 2011 and 6.5 in 2012. Thus, there was no lasting increase in total homicide after 2008, even though the PTP repeal remained in effect and presumably should have continued elevating the homicide rate, if it actually had the detrimental effects that Webster et al. attributed to it. Only the single increase from 2007 to 2008 supports Webster et al.'s conclusions.

Thus, Webster et al.'s conclusion ultimately stands or falls on the basis of the size of a *single* data point, the homicide rate in MO in 2008. If they were wrong about why homicide was higher in MO in 2008, their entire case for a homicide-elevating effect of the PTP repeal collapses. Thus, their conclusion rests on an extremely fragile foundation. We have shown that they have no reliable foundation for their claim that the repeal caused an increase in gun trafficking, illegal gun diversion, or gun possession among criminals, since they was not able to measure any of these things. Consequently, they had no basis for claiming that the repeal put more guns in criminal hands, regardless of the mechanism by which this might have occurred. We have also shown that Webster et al. ruled out only a single alternative explanation of the post-repeal homicide increase (changes in poverty) in his analyses of the less biased UCR-based sample, so they had no sound basis for seizing on the PTP repeal as being responsible for the 2008 jump in MO homicide.

As far as Webster et al. can demonstrate, it is just coincidence that this increase happened to follow the PTP repeal. There is also nothing special about a homicide increase this large, since even larger ones occurred in Nebraska and Iowa. And there is nothing special about it occurring around 2008, as the Iowa and Nebraska homicide increases occurred in the same period, even though neither state repealed a PTP law.

Webster and his colleagues insist that there is something significant about the fact that this large homicide increase occurred specifically in the *firearm* homicide category. They appear to be unaware that, when homicide in general is increasing, for whatever reason, gun homicide always shows proportionally larger increases than nongun homicide. Even when gun law is unchanged and gun ownership levels are stable, one can still routinely expect changes (upward or downward) to be proportionally larger in the gun homicide category than in the nongun homicide category (Britt, Kleck, and Bordua 1996).

Flaws in Statistical Procedures

Webster et al.'s description of their statistical procedures is sketchy, so it is hard to be certain about what they did or did not do. Nevertheless, a number of problems are evident. First, and probably most important, Webster et al. did not report that they weighted the state-years by population. This means that every state-year has equal influence on statistical estimates regardless of population size. Thus, data for South Dakota, with an estimated 2008 resident population of about 0.8 million were given as much weight as data for California, with a 2008 population of 36.8 million (U.S. Bureau of the Census 2014). This does not make common sense, and is not customary scholarly practice. Cases in macro-level studies are supposed to be weighted by some function of population (such as the square root of population), so as to give larger aggregates (states, cities, etc.) an appropriately larger influence on estimates.

Second, Webster et al. did not include the lagged homicide rate as a predictor of the current homicide rate. This procedure, in this instance, would control for homicide in the previous year to estimate the effect of the other variables on homicide in the current year. It serves to indirectly control for variables not explicitly measured that may nonetheless affect homicide rates, and thus better separate out the effect of the PTP repeal. Thus, an analyst could control for other factors that might have influenced the MO homicide trends in 2008-2010 besides the repeal of the PTP law without actually measuring them, based on the simple assumption that factors the affected homicide rate before 2008 probably continued to affect them after 2008. The lagged homicide rate, then, serves as a proxy for these unknown or unmeasured confounding variables.

Why Study This Particular Change in Gun Laws?

Over the past 40 years, homicide and other violent crime rates have increased about half of the time and decreased the other half (U.S. FBI 2014). Thus, if one randomly selected a change in gun control law, no matter how inconsequential it may have been, there is roughly a 50% chance that its implementation happened to coincide with a violence increase and a 50% chance that it coincided with a decrease. If one wanted to make it appear that a weakening of gun controls had caused an increase in violence, one need only identify one of the numerous instances of violence rates happening to increase just after an instance of gun laws being weakened.

And there are an extremely large number of changes to gun control laws to choose from. An analysis of legislation in Florida found that the legislature passed an average of 2.45 gun control bills per year over the period from 1973 to 1992 – a total of 49 changes in gun control in a single 20-year period in a single state (Etten 2002). Across all 50 states, and the entire 1973-2014 period, the number of changes in gun control law an analysts would have to choose from would certainly number at least in the low thousands. Given that violence was increasing in about half of those years, the number of instances in which changes that weakened gun control coincided with violence increases would probably number in the hundreds. Of course, the other side of the coin is that there could also be an equal number of instances of weakenings of gun control that coincided with *decreases* in violence. Regardless of scholars' biases, they could easily find an ample number of instances to support their preferred conclusions.

The challenge in evaluating the effect of gun law changes is to separate the impact of the specific change targeted by the research from the effects of changes in other variables, including other gun law changes, other changes in criminal law occurring around the same time that the target gun law change occurred, and changes in all the other factors that affect violence rates. The researcher must convincingly rule out changes in other crime-related factors, other than the legal change under study, as alternative explanations for observed shifts in crime trends (Campbell and Stanley, 1963), known to methodologists as the problem of "history." This problem is especially acute in time series studies of gun laws due to the fact that state legislatures are almost continuously making large numbers of changes in the criminal law, often for the express purpose of reducing crime.

An examination of one common form of gun control illustrates this point. Twenty-two states enacted up to four different gun laws in the same year during the period 1977 to 2000

(Marvell and Moody, 2006, Table 3). Likewise, over the period 1973 to 1992, the Florida legislature alone passed an annual average of 381 general bills (excluding resolutions), including an average of 2.45 gun control bills per year (Etten, 2002). Thus, almost every enactment of a new gun law is accompanied by dozens or hundreds of other changes in criminal law passed during the same legislative session, making it virtually impossible to separate the effects of a single legal change law from those of others, enacted at the same time, and also intended to reduce crime. Even with regard to only gun laws, there are commonly multiple changes enacted in a given legislative session. This is not surprising since same political conditions that favor repealing a PTP system would also favor other kinds of weakening of gun laws, as well as conservative measures like add-on penalties for crimes committed with a deadly weapon, and would also discourage passage of measures favored by liberals, such as gun registration or waiting periods. Webster and his colleagues controlled for a grand total of just three of the dozens of major types of gun control law ("Stand Your Ground" laws are not gun controls laws), one of which the authors admitted does not affect homicide rates (Right to Carry Laws). Regarding the two remaining gun control measures that they controlled, the authors' estimates of the effect of SNS bans were perverse, suggesting that they cause significant *increases* in firearms homicides – another indication there is something seriously wrong with the authors' models. These results were contrary to the best available prior research, which indicated no effect of SNS bans on homicide rates (Kleck and Patterson 1993). The only potentially relevant gun law the authors controlled was an obscure measure vaguely described as "firearm prohibitions [on purchase? possession?] for young adults resulting from convictions for serious crimes adjudicated in juvenile courts" (p. 295). This gun control measure also showed no significant effect on homicide rates, since the authors reported that only three of their control variables were significantly related to homicide rates, and this law was not among them (p. 298). In sum, the authors did literally nothing to control for other legal changes known to affect homicide rates, and virtually nothing to control for anything else that affects them (only poverty and burglary rates were significantly related to homicide, leaving aside the dubious SNS results).

Since the authors did virtually nothing to control for confounding variables, this means that their conclusions rely almost entirely on the simple fact that this particular weakening in gun control law coincided with a short-term increase in MO firearm homicide rates from 4.6 in 2007 to 6.2 in 2008. By 2011 MO's firearm homicide rate had returned to its pre-repeal 2005 rate of 5.2.

Now consider the hundreds of instances of this sort of coincidence researchers would have to choose from in picking a gun law change to study. Unscrupulous researchers might be tempted to cherry-pick the thousands of gun law changes that have occurred in recent decades to selectively study only those changes that misleadingly suggested causal effects of gun control laws.

Since there were at least nine states with PTP systems in place c. 2000, one might ask, why did the authors choose to estimate the effect of only Missouri's PTP law? After all, focusing on just one specific instance of this gun control measure guarantees that results will be more unstable and sensitive to controls for confounding variables, compared to assessing the average effect of all available PTP systems. The authors rather disingenuously wonder whether their findings can be generalized to the PTP laws of other states (p. 300), without telling readers that they could easily have resolved the issue by simply altering how they coded their main independent variable. Instead of coding this binary variable 1 only for the absence of a PTP law in *MO*, they could have done the same thing for the absence of a PTP law in *all* of the state-years in their sample, thereby covering the entire U.S. The coefficient for this variable would then have represented the average treatment effect of the absence of a PTP law in all states, and there would have been no issue of generalizability. The authors do not provide any explanation of why MO's PTP law is any more important than other PTP laws, so it remains unclear why they focused on this particular state.

The approach used by these authors is useless for assessing the impact of changes in gun laws, but can easily be used to generate results that appear to support the researchers' policy preferences, whatever they might be, and regardless of the actual effects of gun law changes. Thus, I suspect that we have not seen the last of this methodology.

References

Brady Campaign to Prevent Gun Violence. 2013. "2013 State Scorecard." Web page providing overall ratings of gun control strictness at http://bradycampaign.org/?q=2013-state-scorecard. Accessed February 2, 2014.

Britt, Chester, Gary Kleck, and David Bordua. 1996. "A reassessment of the D.C. gun law: some cautionary notes on the use of interrupted time series designs for policy impact assessment." Law & Society Review 30(2):361-380.

Centers for Disease Control and Prevention (CDC). 2014. WONDER website at http://wonder.cdc.gov/mortSQL.html.

Kleck, Gary. 2017. "Macro-level research on the effect of firearms prevalence on suicide rates: a systematic review and new evidence." Paper presented at the annual meetings of the American Society of Criminology, in Philadelphia, PA on November 15, 2017.

Kleck, Gary, and Shun-yung Wang. 2009. "The myth of big-time gun trafficking." UCLA Law Review 56(5):1233-1294.

Kovandzic, Tomislav V., Lynne M. Vieraitis, and Mark R. Yeisley. 1998. "The structural covariates of urban homicide." Criminology 36: 569-599.

Land, Kenneth C; McCall, Patricia L; Cohen, Lawrence E. 1990. "Structural covariates of homicide rates." American Journal of Sociology 95: 922-963.

Miller, Matthew, Steven J. Lippman, Deborah Azrael, and David Hemenway. 2007. "Household firearm ownership and rates of suicide across the 50 United States." Journal of Trauma 62:1029-1035.

National Research Council. 2005. Firearms and Violence: A Critical Review. Washington, D.C.: The National Academies Press.

U.S. Bureau of Alcohol, Tobacco and Firearms (ATF). 2005. State Laws and Published Ordinances – 2005. Washington, D.C.: U.S. Government Printing Office.

U.S. Bureau of Alcohol, Tobacco and Firearms (ATF). 2010. State Laws and Published Ordinances – 2010-2011. Available at http://www.atf.gov/publications/firearms/state-laws/31st-edition/index.html.

U.S. Bureau of Alcohol, Tobacco and Firearms (ATF). 2013. ATF Missouri. Available online at https://www.atf.gov/sites/default/files/assets/statistics/tracedata-2012/2012-trace-data-missouri.pdf. Retrieved February 2, 2014.

U.S. Bureau of the Census. 2012. Statistical Abstract of the United States – 2012. Washington, D.C.: U.S. Government Printing Office.

U.S. Bureau of the Census. 2014. State population estimates for 2000-2008, found at http://www.census.gov/popest/data/historical/2000s/vintage_2008/index.html. Accessed February 4, 2014.

U.S. Congressional Research Service. 1992. Assault Weapons: Military-style Semi-automatic Firearms Facts and Issues. Report 92-434. Washington, D.C.: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation (FBI). 1998. Crime in the United States 1997 – Uniform Crime Reports. Washington, D.C.: U.S. Government Printing Office.

U.S. Federal Bureau of Investigation (FBI). 2014. National Instant Criminal Background Check System. "Total NICS Firearm Background Checks by State, Nov. 30, 1998 – Dec. 31, 2013. http://www.fbi.gov/about-us/cjis/nics/reports/1998_2013_state_monthly_totals-123113.pdf. Accessed 2-6-14.

Webster, Daniel, CK Crifasi, and JS Vernick. 2014. "Effects of the repeal of Missouri's handgun purchaser licensing law on homicides." *Journal of Urban Health* 91:293-302.

Webster, Daniel, Jon S. Vernick, Emma E. McGinty, and Ted Alcorn. 2013. "Preventing the diversion of guns to criminals through effective firearm sales laws." Pp. 109-121 in *Reducing Gun Violence in America*, edited by Daniel W. Webster et al. and Jon S. Vernick. Baltimore: Johns Hopkins University Press.