**IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MARYLAND**

MARYLAND SHALL ISSUE, INC*., et al.*,

    Plaintiffs,

v.   Civil Case No. 16-cv-3311-ELH

LAWRENCE HOGAN, *et al.,*

    Defendants.

SUPPLEMENTAL DECLARATION OF GARY KLECK

I, Gary Kleck, under penalty of perjury, declare and state:

1.   This supplemental declaration is a response to Daniel W. Webster's Second Supplemental Declaration, in which he cites three additional studies that purportedly buttress his opinion supporting Maryland's Handgun Qualification License ("HQL") law. In Part 1 I explain fundamental problems afflicting Webster's evaluation of background check laws, addressing problems characterizing all of his studies on this topic. Then, in Parts 2-4 I separately examine each of the three studies he newly cites in the Second Supplemental Declaration.

Part 1 - Problems with the Webster Research Program as a Whole

2.   Each of the individual studies on which Webster relies in his Second Supplement is fatally flawed in itself, but the entire series of studies on background check laws co-authored by Webster is misguided and misleading as a whole, showing all the earmarks of data-dredging to obtain chance findings and present them as if they were tests of a single *a priori* hypothesis formulated before looking at the data.

<div style="border:2px solid blue; text-align:center;">EXHIBIT
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3.   "Data dredging" is a misuse of data analysis in which the analyst examines a large, complex body of data (such as the set of all the rates of firearm violence in all states over the past 80 years), identifies some chance (non-causal) associations, formulates a hypothesis after this peek at the data, conducts an analysis rediscovering these associations, then reports the results of the analysis as if it provides a test of a single *a priori* hypothesis, i.e. one formulated before examining the data. The strategy is disreputable partly because it employs tests of significance that rely on the assumption that the analyst was only testing a single hypothesis (such as "firearm purchasing licensing laws that require the applicant to personally appear before law enforcement authorities reduce firearms homicide") when the analyst had actually tested dozens or hundreds of variants of the hypothesis (e.g., "some kind of gun control law (unspecified) reduces some kind of crime or violence"). Tests of statistical significance are supposed to assess the probability that a statistical result could be entirely the result of chance factors (rather than the product of actual causation), but the significance levels yielded when the analyst indulges in data dredging are grossly inaccurate, because the probability of a purely chance finding is far greater when the analyst performs dozens or hundreds of tests rather than the single test assumed by conventional significance computations (for a classic discussion of data dredging, see Selvin and Stuart 1966).

4.   The tactic is notably unreliable because it lends itself to cherry-picking unrepresentative subsets of the available data and reporting misleading results that confirm the researcher's expectations, and not reporting results contrary to those expectations.

5.   There have been thousands of changes in gun control law in the 50 states and the District of Columbia (DC) in past decades. Further, over the past 80 years or so, homicide and suicide rates have increased about half the time and decreased about half the time (U.S. Federal Bureau of Investigation 2017; Kleck 1997, pp. 262-263, 289-290). Thus, at any one time that a

new gun control law is passed or an old one is repealed there is roughly a 50% chance that enactment of the law will *coincide* with a reduction in the homicide or suicide rate – even if the policy has no actual effects. An unscrupulous analyst could dredge through decades of violence statistics, examine data for each of the 50 states and D.C., identify the many time points when firearms violence decreased in this or that state, and then selectively look for the states and years when the decreases coincidentally happened to be preceded by a change in gun law. Even if gun law changes had no actual causal effects on violence rates, there would be hundreds or thousands of such coincidences simply because there were so many changes in gun laws and so many years in which violence decreased. Under a no-effect assumption, it would be a reasonable expectation that roughly half of enactments of new gun laws would be followed by increases in firearm violence and half by decreases.

6. If one were sufficiently selective, one would also be able to identify some very specific subtypes of gun laws for which violence decreases were *more* common in the post-law period than violence increases, just as other subtypes were more often followed by *increases* than decreases. The unethical analyst might be tempted to publish results pertaining to the former type of gun law, while ignoring the latter.

7. Webster and his colleagues do not assert that firearm background check laws in general reduce firearms violence. Quite the contrary, they have explicitly rejected this position (McCourt, Crifasi, Stuart, Vernick, Kagawa, Wintemute, and Webster 2020, pp. 1546-1547). Rather, Webster has claimed that only very specific subtypes of background check laws, incorporating very specific elements, have this beneficial effect, though over time he has changed which elements he thinks are responsible for gun violence reductions.

8.   Webster has offered multiple speculations about which background check subtypes are consequential, and has changed over time which ones he has stressed as beneficial. Thus, he cannot assert that, at the very start of his research on this topic, around 2013, he was going to test for the gun violence-reducing effects of one specific type of background check laws, such as those requiring fingerprinting. Rather, he effectively revised his implied hypothesis as he and his colleagues examined more subsets of the available data, testing multiple specific versions of the general hypothesis that some kind of background check on firearms purchasers reduces gun violence. There is nothing wrong with scholars changing their views about what an evolving body of evidence shows; indeed, flexibility is generally a scholarly virtue. What is not desirable is: (1) selectively presenting only some research findings regarding a wide variety of hypotheses and not others; and (2) reporting erroneous significance tests as if the researcher had tested only a single version of his hypothesis.

9.   Webster's earliest empirical work in this area assessed the effect of repealing Missouri's permit-to-purchase ("PTP") law (which he now relabels a "purchaser licensing law") on firearms violence, finding that the repeal was followed by increases in firearms homicides (Webster, Crifasi, and Vernick 2014). Since the repeal did not eliminate background checks on people trying to get guns from licensed dealers, he had to offer some kind of rationale for why Missouri's repeal of its PTP law would increase firearms violence. The repealed law had to have some additional provisions that reduced gun violence when it was still in effect. At that time (2014) Webster seemed to most strongly emphasize the fact that background checks were required for private transfers as well as dealer transfers, stressing that the repeal "eliminated mandatory background checks for handguns sold by unlicensed sellers" (p. 294; stressed again on p. 298).

10.   Recent research on states with universal background checks, however, indicates that it is highly unlikely that this element of Missouri law had any measurable pre-repeal effect on gun violence or criminal acquisition of guns, since few private sellers obey this legal requirement, and virtually none of the few prospective private transferees who do submit to background checks are denied. Data from Colorado, Oregon, and California indicate that only about $1/10^{th}$ of 1% of private transfers of guns in those states were both: (1) subjected to the legally required background check; and (2) resulted in a denial of the transfer (Kleck 2020). In this light, it is implausible that eliminating this feature of Missouri gun law could have had any measurable effect on rates of firearms violence. Indeed, Webster's own recent research concludes that universal background checks - which he has relabeled "comprehensive background checks" ("CBC") - do not reduce firearms violence (McCourt et al. 2020).

11.   Webster also stressed that applicants for PTP permits in Missouri had to appear in person at some law enforcement agency to apply, asserting (though not documenting) that "most states with PTP handgun licensing require applicants to apply for the license directly at a law enforcement agency" (Webster, Crifasi, and Vernick 2014, p. 294). He speculated that this might deter some criminal applicants who would have passed the checks (those who would not have passed the checks are irrelevant to this claim since they would have been denied anyway).

12.   By 2018 Webster and his colleagues (Crifasi et al. 2018) had obtained findings of *higher* firearms homicide rates in places with CBC laws, suggesting that violence-reducing effects of background check laws, including Missouri's pre-repeal law, were *not* due to background checks covering private (nondealer) transfers. Webster then argued that CBCs alone do not reduce firearms homicides, but that other elements of the permitting process do. He and his colleagues then speculated that these crucial elements were either: (1) the longer times that permit laws often

allow for authorities to conduct the background check (p. 387); or (2) the requirement for a personal appearance of the applicant at a law enforcement agency.

13.   By 2020, however, Webster had become more ambivalent about the value of the personal appearance requirement for reducing gun violence. The background check laws that he and his colleagues claimed reduce firearms violence were those that required *either*: (1) an in-person application; *or* (2) fingerprinting (Webster, McCourt, Crifasi, Booty, and Stuart 2020, p. 187). In other places in that article, he alludes only to fingerprinting (p. 171). Strictly speaking, by this point Webster was unwilling to unambiguously commit himself to the value of personal appearance of applicants, effectively hedging his bets by claiming it *might* reduce gun violence, or it might not, and that it may instead be fingerprinting requirements that account for the purported benefits of permit laws.

14.   Webster has never offered a credible explanation of why fingerprinting would strengthen the ability of background check laws to block criminals and other high-risk persons from getting guns. A fingerprinting requirement does not increase the comprehensiveness of criminal background databases' coverage, nor does it widen the scope of persons who fall into a disqualified category. Rather, the standard rationale for requiring applicants to be fingerprinted is simply to ensure that applicants really are who they claim to be, and minimize the use of fake documents to claim the identity of a person qualified to receive a gun. Whether a fingerprinting requirement has a measurable effect on gun acquisition by disqualified persons is, then, a function of how often such persons use false ID to impersonate a qualified person.

15.   This tactic, however, appears to be extremely unusual. According to a 2016 national survey of 24,848 prison inmates, most criminals who possessed a gun during the offense for which they were incarcerated did not get the gun from a licensed source of the type required to

perform a background check, and among those who use such sources, most used their own name. Only about 1.3% of gun-armed criminals got their gun from a retail source and used a false name (Alper and Glaze 2019, p. 8). A fingerprinting requirement therefore seems to be a solution to an extremely rare problem, and therefore unlikely to produce the enormous effects on firearms homicides claimed by Webster.

16.   In his most recent article on background check laws, Webster no longer stresses the personal appearance requirement at all - or even mentions it. In that article's conclusions, he and his co-authors primarily emphasize the purported benefits of authorities being allowed more time to conduct background checks (McCourt et al. 2020, p. 1550), though they also make a single brief allusion to "mandated fingerprinting" at the beginning of the article (p. 1546). Having dropped the stress on either universal background checks or personal appearance requirements (and possibly fingerprinting), he seems to now stress the waiting period element of the permitting process. The problem with this emphasis is that virtually all technically sound research indicates that waiting period laws have no measureable effect on homicide rates (Loftin and McDowall 1983; Kleck and Patterson 1993; Ludwig and Cook 2000; Lott and Whitley 2001; Makarios and Pratt 2012; Kleck, Kovandzic, and Bellows 2016). The authors cite a *single* study to support the contrary position (Luca, Malhotra, and Poliquin 2017), which supposedly showed that "longer waiting periods between applying to purchase firearms and receiving the firearms are associated with lower rates of firearm homicides and suicides," but even this study's strongest findings did not find an association between waiting periods and either total homicide or total suicide that was significant at the conventional 5% level (Kleck 2017, pp. 8-9).

17.   Thus, Webster did not have, back in 2013, a single specific *a priori* hypothesis about a specific kind of background check law that he believed reduced gun violence. Rather,

over the years he tested for effects of many different variants of such laws, repeatedly changing

which elements of those different laws were claimed to be responsible for supposed effects. He

was testing multiple hypotheses, not just one. And the results of these multiple hypothesis test do

not consistently support any one specific hypothesis about the benefits of permit laws.

18.   They certainly do not consistently support *Maryland's* HQL law. Maryland

requires fingerprinting of handgun applicants, yet the results of Webster's research do not

consistently support the effect of fingerprinting of permit applicants. His most recent research

concludes that the law in Connecticut, which requires fingerprinting, reduces gun violence, but

that Maryland's law, which also requires fingerprinting, does not reduce gun violence (McCourt

et al. 2020, pp. 1548-1549).

19.   Nor are his results consistent regarding the view that allowing more time for

background checks reduces gun violence. Maryland allows 30 days for the check to be completed,

but Webster and his colleagues concluded that its law had no effect on firearms homicide, while

concluding that Missouri's repealed permit law, which involved no wait at all, *did* reduce gun

homicide (McCourt et al. 2020, pp. 1548-1549).

Cherry-picking States to Study

20.   At least 11 states plus DC have laws requiring a permit to purchase firearms (CT,

HA, IA, MD, MI, NE, NJ, NC) or a license to own or purchase them (IL, MA, NY) (Giffords Law

Center to Prevent Gun Violence 2021)(collectively referred to as "Purchaser Licensing" or "PL"

laws). Why, then, did Webster study just four of these 12 jurisdictions? And if only four, why

CT, MD, MO, PA in particular? These questions are crucial because if researchers decide to study

just a few instances of a policy that has been implemented in many jurisdictions, there is a risk that

researchers will cherry-pick one or two unrepresentative examples that appear to support a

preferred finding, even if analysis of all instances would have indicated that the average effect of the policy was zero.

21.   The danger can be illustrated by a simple example. Suppose gun control policy X had no effect whatsoever on homicide rates, but a hypothetical researcher wanted to create the false impression that X was effective. This is easy to do with any policy that has been implemented in numerous states. In the long run, over the past 80 years or so, homicide rates have increased about half the time and decreased about half the time (U.S. Federal Bureau of Investigation 2017). Thus, at any one time that a new state gun control law is passed, there is roughly a 50% chance that its introduction will be followed by a reduction in the homicide rate, even if the law has no effect on violence. All the researcher would need to do to create the false impression that some kind of gun control law was effective in reducing homicide would be to dredge through data on homicide rates in the 50 states and DC, looking for declines in state firearms homicide rates occurring in any of the 80-some years for which state homicide statistics are available, and to then search for instances of new gun laws that happened to have been introduced just before the homicide declines began. Each of the 4,000-plus state-years (51 x 80 = 4,080) would represent a potential opportunity to observe a homicide decline that began just after a new gun law was enacted, and roughly half of these state-years would be cases in which the homicide rate was lower than it had been the year before. Consequently, there would be hundreds of instances where introduction of a new gun law was coincidentally followed by a drop in the firearms homicide rate. The researcher could simply pick a few of them that happened to coincide with an especially strong drop in the firearm homicide rate to analyze and publish the results for these few nonrandomly selected states, as if they were the specific states that the researcher wanted to study all along.

22.   Studying just a small minority of a larger number of implementations of a given type of policy is prone to yielding misleading results for the foregoing reasons, and consequently is not accepted as a method by knowledgeable researchers. The more accepted procedure is to study *all* implementations of a given type of gun law, estimating the average treatment effect of the full set, in either a cross-sectional analysis of states, counties, or cities (e.g. Kleck and Patterson 1993; Kleck, Kovandzic, and Bellows 2016) or a panel design in which all state-years are coded as to which ones had a given type of law in operation (e.g. Lott and Whitley 2001; Marvell and Moody 1995). Either way, no one could claim that researchers using these methods had cherry-picked an unrepresentative subset of the instances of a given type of law being implemented.

23.   It is unhelpful to phrase research results in associational language, saying that changes in handgun purchasing licensing laws were "associated with" changes in firearms homicide or suicide. This could charitably be interpreted as a sign of scientific caution, the authors refraining from making unwarranted claims about cause-and-effect. Less charitably, it serves to obscure the actual meaning the authors were clearly trying to convey – that changes in gun law *caused* changes in firearms violence. Webster and his colleagues conveyed their actual intended meaning in their abstract (and other places) when they admitted that they were trying to estimate "the *effects* of these laws on homicide and suicide rates" (p. 1546, emphasis added). The word "effects" plainly denotes causal effects. But, policy cannot be based on merely coincidental statistical *associations* between violence rates and changes in law. Accordingly, for purposes of this supplemental report, I treat the authors' conclusions as if they pertained to the purported causal effects of changes in gun laws.

Part 2 - Critique of the Hasegawa et al. (2019) Study

24.   In his Second Supplement, Webster does not explain how the Hasegawa study adds anything to our understanding of the impact of Missouri's repeal of its PTP law or in what specific ways it improves on his original Missouri study (Webster et al. 2014). It fails to address the most glaring problems with that prior study.

25.   Hasegawa et al. appeared to believe that a serious problem with the original 2014 research was that it did not address "concerns about history interacting with group" (p. 371). As applied to this study, "history interacting with group" refers to the possibility that unmeasured confounding variables had different effects on firearms homicide rates in Missouri in the post-repeal period than in other states that did not change their purchase permit laws. It is crucial to stress what "interacting" means in this context. A confounder (or an historical event) "interacting with group" means that the confounding factor has different effects in one group (e.g., Missouri) than in another (e.g. a bordering control state such as Iowa). It does not mean the level of the variable changed over time more in one group than another. Rather, it means that the *degree of effect* differs across groups, e.g. the amount of change in firearms homicide caused by a one unit change in the confounder (or the historical event) differs between the groups.

26.   This is not a problem that afflicted Webster's 2004 study, and thus the solution is irrelevant to any of the actual problems with the earlier study (summarized in Kleck 2017). The most important problem with that study was uncontrolled confounders – variables beside the PTP repeal that affected firearms homicide rates, that were not controlled by Webster, and that changed over time more in Missouri than in control states. The problem was not "history interacting with group," but rather simple omitted variables bias. These "omitted variables" were uncontrolled variables that might well exert the same magnitude of effect, unit-for-unit, in both Missouri and

control states (and thus did *not* "interact with group"), both before and after the repeal, but simply changed more in Missouri than in the control states. This is not the problem addressed in the Hasegawa et al. study, and the procedures they employ do not solve it. The only way to solve it would be to measure and explicitly control for the omitted confounding variables, and this was not done in either the original Webster et al. (2014) study or in the Hasegawa et al. (2019) study.

27.   Suppose, for example, that a spate of street gang violence occurred in Missouri for reasons completely unrelated to the PTP repeal, and resulted in more firearms homicides in Missouri in 2008, just after the repeal. The unit-for-unit impact on the firearms homicide rate of a given number of gang combats might be identical in both Missouri and in other states, and identical in both the pre-repeal and post-repeal period, so their effect does not interact with either group or period. Nevertheless, if the level of gang violence increased in Missouri more than in the control states, it would cause a larger increase in firearms homicide in Missouri than in other states. Since Webster did not control for the level of gang violence (or any other known confounders), he had no basis for attributing post-repeal homicide increases in Missouri to the PTP repeal (Kleck 2017).

28.   In that earlier study, Webster did at least acknowledge in principle the need to control for confounders, and facially appeared to do that. A confounder in that study would be a variable that both affects the rate of firearms homicide and is correlated with the existence of a PTP law. The variables actually controlled by Webster, however, were not confounders, either because they showed no significant association with firearms homicide rates, or had no known association with the existence of a PTP law. For example, while Webster and his colleagues claimed to control for at least eight variables or sets of variables, results buried in their Supplemental Tables show that five of these showed no significant association with firearms

homicide rates (and thus could not be confounders), while two others showed nonsensical associations (they implied that more poverty *reduces* homicide, and that bans on Saturday Night Special handguns *increase* homicide) (Kleck 2017).

29.   The Hasegawa study likewise did nothing to correct any other serious defects of the 2014 study. One fundamental problem was simply the decision to assess the impact of just one state's change in PTP law, rather than studying the average impact of all state PTP laws. Focusing on a single state lends itself to cherry-picking an unrepresentative state, and ignoring the more typical effects in other states, or the average effect across all states with a PTP law. At the time this study was done there were at least nine states with PTP laws, raising the question: "Why study just Missouri?" (Kleck 2017). The Hasegawa study is also confined to just Missouri, so it simply repeats this problem.

30.   Another problem in the 2014 study left unsolved by the Hasegawa study was the extremely short pre-repeal time series, 1998-2007. Webster et al. had limited the pre-repeal period to just nine years, even though there were data for many times that many years. This decision to needlessly restrict the pre-intervention sample guaranteed more unstable results, particularly regarding how much post-repeal Missouri firearms homicide rates changed compared to the rates prevailing prior to the repeal. Hasegawa's analysis used exactly the same needlessly truncated pre-repeal time period (p. 375).

31.   Yet another problem with the 2014 study of Missouri was that Webster and his colleagues could not explain why repeal of the PTP law appeared to have all of its "effect" in a single year. All of the post-2007 increase in the firearms homicide rate in Missouri occurred from 2007 to 2008. Thereafter, there was no further increase during the period studied by Webster et al. The Missouri firearm homicide rates jumped from 4.6 per 100,000 in 2007 to 6.2 in 2008, but

by 2011 had returned to its pre-repeal 2005 rate of 5.2. (Kleck 2017). If eliminating the PTP

elements of background checks had actually caused a gun homicide increase, the effects should

have persisted as long as those elements continued to be absent, i.e. right up through the end of the

study period. They did not. Why would a persisting set of conditions for buying a gun have effects

lasting only a year?

32.   More likely causes of this very short-lived jump in Missouri gun homicide would

be short-lived developments in Missouri, such as a brief spate of inter-gang violence in which

killings by one gang triggered retaliatory killings by another. A similar development might be a

brief elevation of homicide linked with conflicts over drug dealing. Since nearly all homicides

linked with street gang conflict or drug dealing are committed with firearms (U.S. FBI 2007,

Expanded Homicide Data Table 10), one would expect the impact to be largely limited to the rate

of firearms homicide. This is precisely the pattern observed in Missouri, but one that Webster et

al. touted as evidence that the increase was due to the PTP repeal.

33.   Nothing in either the original 2014 study or in the 2019 Hasegawa study even

establishes that more Missouri criminals purchased guns after the 2007 repeal, which is clearly the

reason why Webster et al. thought that eliminating the PTP law would cause increased gun

homicide (2014, p. 294). They claimed to have had measures of what they vaguely described as

"illegal diversion" of guns to criminals (p. 299), a term they never defined, and asserted that this

increased after the PTP law was repealed. Their indicator of "illegal diversion" was the share of

guns recovered by police that: (1) had been first sold at retail a relatively short time before

recovery; and (2) came from a state outside of Missouri. Neither is a valid indicator of gun

trafficking or of criminals' gun acquisition (Kleck and Wang 2009).

34.   The repeal of the PTP law only changed one mechanism for acquiring guns - purchase. Webster later concluded that extending background checks to cover private transfers does not affect gun homicide (McCourt et al. 2020), so repealing this element of Missouri's PTP law should not have affected criminal gun acquisition via purchases from private transfers. Thus, he must believe criminal purchases of guns from dealers must have increased. Since Missouri continued to have federally mandated background checks on purchases from gun dealers, one would expect that Webster might have checked whether an increased share of these checks in Missouri resulted in denials due to criminal records. He did not - or at least did not report the results of such an inquiry. Hasegawa et al. contribute nothing on this score – they provide no basis for believing that criminal purchases of guns increased after Missouri repealed its PTP law.

35.   The Hasegawa study repeats the critical error pervading all of the studies Webster has advanced addressing the purported impact of gun control laws. He reports analyses confined to *firearms* violence. Nothing in either Missouri study even addresses whether Missouri's PTP law saved lives while it was in effect, i.e. reduced the total number of homicides. There is no public safety benefit in merely inducing criminals to murder people with different weapons, if there is no decrease in the total number murdered. Webster, in both the 2014 study and in his 2019 study with Hasegawa, simply ignores this problem. Consequently, nothing in either study – even if taken at face value – actually supports the view that PTP laws save any lives.

36.   In sum, Webster's new reliance on the Hasegawa study does not strengthen his opinion that PTP laws in general or the Maryland HQL provisions in particular reduce firearms violence.

Part 3 - Critique of the Webster et al. (2020) Study

37.   Webster, McCourt, Crifasi, Booty, and Stuart (2020) concluded, based on a panel study of annual state-level data, that the incidence of mass shooting incidents and the total number of fatalities linked with such incidents are reduced by purchaser licensing laws that require applicants to personally appear at a public safety agency or that require them to be fingerprinted, as well as bans on large-capacity magazines (LCM). The conclusion regarding purchaser licensing is not supported by any technically sound methods, and is highly sensitive to exactly how a mass shooting is defined.

38.   Webster cites this study to support the claim that "handgun purchaser licensing laws requiring either in-person application with law enforcement or fingerprinting (of applicants) were associated with incidents of fatal mass shootings 56 percent lower than that of other states" (Webster Second Supplement, p. 3). Webster in this Supplement, and in the cited article, used associational language, but in the Abstract of that article advanced the meaning that he and his colleagues actually intended to convey: causation. They asserted that their findings indicate that "laws requiring firearms purchasers to be licensed through a background check supported by fingerprints and laws banning LCMs [large-capacity magazines] are the most *effective* gun policies for reducing fatal mass shootings" (Webster et al. 2020, p. 171, emphasis added).

The Failure to Control Confounding Variables Means the Results Cannot Be Used to Support any Causal Effects of Purchaser Licensing Laws

39.   Accurately inferring causation in this case would have required the authors to control for as many confounding variables as possible. In this context, confounding variables would be other factors besides the two supposedly effective gun laws (PL laws and LCM bans)

that had both of two properties: (1) they affected the frequency or seriousness of mass shootings; and (2) they were correlated with the presence of absence of those two gun laws.

40.   As far as can be determined from the authors' published findings, they did not actually control for *any* confounding variables, which are the only kind of controls that help establish causal effects of one's focal variables.

41.   In the analysis reported in their Table 3, only two control variables (i.e. variables other than the two gun control laws) were even significantly related to incidence of fatal mass shootings, neither of which is known to be correlated with the presence/absence of purchaser permit laws or LCM bans. In the Table 3 analysis pertaining to number of victim deaths, *none* of the control variables were related to the outcome variable, and thus none could be regarded as confounders. In their Table 4 analyses limited to "domestic-linked" mass shootings, not a single control variable was significantly related to either outcome variable, and thus none could be regarded as confounders. Finally, in their Table 5 analyses, pertaining to mass shootings not linked to domestic violence, just *one* of the control variables was significantly related to either outcome variable, and the authors did not show this control variable to be correlated with their two preferred gun control laws. Thus, the authors did not control for a single known confounder in this analysis either. In particular, although they controlled for a few gun control laws unlikely to affect mass shootings, they did not even control for the one that would seem to be most likely to affect killings by mentally ill killers – bans on gun purchases by mentally ill persons. In sum, the authors simply did not control for variables that were actually confounders. The controls that they did introduce could not help isolate the effect of purchaser licensing laws because the variables they did control were not confounding variables, but rather were either irrelevant variables (i.e. variables that do

not affect the outcome variable) or variables that were relevant but uncorrelated with gun laws and consequently could not bias estimates of gun law effects.

The Misleading Effects of an Ambiguous Definition of the Gun Law Variable

42.   If one ignores the authors' failure to control for confounders, and takes at face value their estimates of the effect of firearm Purchaser License ("PL") laws, what do their findings mean? The key to understanding these findings lies in the curiously ambiguous way they defined PL laws, as "handgun purchaser licensing laws that require *either* in-person application *or* fingerprinting" (p. 174, emphasis added). Any sensible analyst would obviously want to know which of these elements of PL laws reduce violence – it might be only the fingerprinting requirement, it might be only the personal appearance requirement, or it might be both. The ambiguous way that Webster and his colleagues chose to define their PL variable makes it impossible to establish which element has violence-reducing effects. Given that Maryland's PL law requires fingerprinting, but not a personal appearance, it would be especially important in the current case to know which element improves the law's potential for reducing violence.

43.   The authors could easily have created two separate variables, one of which measured whether a state had a PL law requiring fingerprinting (without regard to whether it also required a personal appearance), and another that measured whether a state had a PL law requiring a personal appearance (without regard to whether it also required fingerprinting). This approach could have revealed which one worked. From the standpoint of which approach gives better guidance as to policy makers in crafting better public policy, the separate variables approach is obviously superior, but the authors did not utilize this strategy.

44.   Since the authors do not report any results of analyses using the superior strategy, it cannot be known for certain what those results would have been. Nevertheless some relevant

statistical insights can be confidently stated. First, whether the PL/mass shooting association is statistically significant is a function of the standard error of the coefficient measuring this association. The standard error is a measure of the instability of estimates of the coefficient. The bigger the standard error, the less likely it is that a given estimate of the PL/mass shooting association is statistically significant.

45.   Second, the size of a standard error is a function of, among other things, the variation in the variables involved in the association – in this case, PL laws and mass shootings. The more variation, the smaller the standard error. Variation in a binary variable that merely measures the presence or absence of a PL law, as with any other binary variables, is a function of how common the thing being measured is. If only two or three states have a specific kind of PL law, there is little variation, since nearly all states are the same, i.e. nearly all do *not* have the law. Conversely, if nearly all states had that type of PL law, there would also be little variation since nearly all states would be the same in that they *did* have the law. The greatest amount of variation, as with any binary variable, would be if half the states had the law and half did not.

46.   Consequently, the standard error of the coefficient for a specific PL law would be larger if few states had that law, smaller if the share was closer to half. The rarer the specific type of PL law being tested, the bigger its coefficient's standard error would be, other things being equal, and the less likely the coefficient would be significant. By definition, the number of states with a type of PL law that required fingerprinting would have to be smaller than the number of states that had *either* a fingerprinting requirement *or* a personal appearance requirement, unless all states with the former requirement also had the latter – something we know is not true. Thus, there is less variation in a variable that specifically measures the presence of a PL law with a fingerprinting requirement, or a variable that specifically measures the presence of a PL law with

a personal appearance requirement, than there is in a variable that ambiguously measures whether a state has *either* provision. Consequently, the standard error will be smaller, other things being equal, using the ambiguous formulation used by the authors.

47.   This means that by choosing to use the more ambiguous way of defining their PL variable, the authors artificially increased their chances of getting a significant association between the PL variable and the incidence or seriousness of mass shootings. Webster of all people should have been especially desirous of establishing which specific elements of a background check law reduce violence, since his own research indicates that some variants appear to be effective, while others do not.

48.   Webster's ambiguity about which elements of PL laws help reduce violence is especially problematic in connection with Maryland's HQL law. It requires fingerprinting of applicants but does not require in-person application at a law enforcement agency. Therefore it is critical to know which of these two provisions reduce violence. If it is fingerprinting that matters, then Webster's results may support Maryland's HQL law as he claims. If only the personal appearance requirement matters, his results do not support Maryland's HQL law. As things stand, given the inherent ambiguity of Webster's definition of his PL law variable, it is impossible to tell whether the results of the Webster et al. (2020) study provide any support for Maryland's PL law.

The Results of this Study Are Inconsistent and Dependent on Arbitrary Decisions as to How a Mass Shooting is Defined

49.   Tables A14 and A15 of the appendix include findings based on analyses using different cut-offs for the minimum number of victims that must be killed in an incident for it to be defined as a mass shooting. In the analysis reported in the main body of the article, there was a significant association between PL laws and the incidence of mass shootings, interpreted by the authors to means that PL laws "were associated with incidents of fatal mass shootings 56% lower

than that of other states" (p. 181). This was based on a definition of mass shooting as an incident with more than three victims killed. Since the exact numerical cut-off used is necessarily somewhat arbitrary, it is important to test whether the results are consistent if different cut-offs are used.

50.   When the authors changed their cut-off by just one, to "more than four victims," there was no longer any significant association between PL laws and mass shootings (Table A14). When the cut-off was changed to "more than five victims," the association was not only insignificant; it almost completely disappeared (Table A15). The authors gloss over this glaring inconsistency by claiming that the magnitude of the association did not change much when the cut-off was changed (p. 187), but this is inaccurate. The estimated association changed from one implying 56% fewer mass shootings in states with PL laws when the cut-off was more than three (Table 3) to a nonsignificant *13%* lower when the cut-off was more than five (Table A15). Describing these results as "similar," as the authors did (p. 187) is misleading.

**The Authors Analyzed a Biased Sample of Mass Shootings that Artificially Inflated
        Support for an Impact of Purchaser Licensing Laws**

51.   Background check laws of all types are most likely to affect gun acquisition by persons willing to submit to checks when trying to get a gun, i.e. the law-abiding. Conversely, the people least likely to get guns from a source that would require them to submit to a background check would be hard-core criminals. Surveys of prison inmates confirm that few serious criminals get guns from sources that require a background check, such as licensed gun dealers (Alper and Glaze 2019, p. 7). Thus, PL laws are least likely to influence the kinds of repeat offenders who deal drugs or belong to gangs, and when addressing mass shootings, one would expect that PL laws would be least likely to affect mass shooters who commit massacres connected to gangs or drug dealing.

52.   This means that one could bias results in favor of the view that PL laws reduce mass shootings by simply not counting the kinds of massacres least likely to be affected. This is precisely what the authors did. They frankly admitted that "we excluded any case that was coded as having a connection to gang or narcotic activity" (p. 174). They did not acknowledge the biasing effects of this exclusion. Their justification for introducing this sample bias was that other researchers had altered their samples in the same way (p. 174).

53.   They further biased their sample by non-randomly excluding five states from their analyses (p. 174). Their justification for these exclusions was that there were "Uniform Crime Reports (UCR)-SHR [Supplementary Homicide Reports] reporting issues over multiple years" (p. 374). A justification based on problems with UCR/SHR data is particularly implausible given that the authors did not need to use these sources for counting up either the number of mass shootings in a given state or the number of deaths linked with these incidents (or for any other purpose). They could rely on either the "Stanford Mass Shootings in America" dataset or the data in the Gun Violence Archive for producing these counts, and indeed they did use these very sources to "remedy" the deficiencies in UCR/SHR data (p. 374). Significantly, their "sensitivity analyses" (pp. 183-187) did not include any checks to see if their estimates of gun law effects were distorted by excluding these particular five states.

54.   In sum, the Webster et al. (2020) study does not provide a scientifically credible basis for estimating the effect of handgun purchaser license laws on mass shootings, and does not strengthen Daniel Webster's support for Maryland's Handgun Qualifying License law.

Part 4 - Critique of the McCourt et al. (2020) Study

The Results Pertaining to Maryland

55.   Before addressing why the McCourt et al. study cited by Webster is not credible, it is first important to note what its results bearing on Maryland, if taken at face value, imply for this case. Webster claimed that the McCourt study showed "that State handgun purchaser licensing laws such as the Maryland law at issue in this case—which require a prospective buyer to apply for a license or permit from state or local law enforcement—are highly effective at reducing firearm homicide and suicide rates." (Webster Second Supplement, pp. 3-4).

56.   Webster uses the key phrase "laws *such as* the Maryland law," as opposed to simply "the Maryland law." Webster stresses the McCourt et al. study's findings regarding Connecticut and Pennsylvania, but is silent on what that study found specifically regarding the purchaser licensing law of Maryland - the only state whose law is at issue in this case. McCourt et al. only studied Maryland's "implementation of a CBC [comprehensive background check] law (1996-2013) (p. 1548). That is, they studied Maryland's pre-exiting and continuing background check law, and NOT the HQL that was adopted in 2013. And despite the fact they studied through the period of 2017, they failed to report what happened to the firearms homicide in Maryland after it implemented the HQL in 2013. Instead they compare Maryland's pre-existing CBC with Connecticut's PL and failed to report the effects if any of Maryland's comparable law, the HQL. In sum, Webster's own most recent study does not support a claim that Maryland's gun law reduced firearms homicide.

The Essential Analysis the Authors Failed to Do

57.   There is no public health benefit in reducing the number of firearms homicides (or firearms suicides) if the number of *non-firearms* homicides (or *non-firearms* suicides) increases

by an equal or larger amount, so that the total number of people who are murdered or commit suicide is unchanged. If such an outcome did result from a change in gun law, however, it would be impossible to detect if analysts never analyzed the impact of the change on the *total* (firearms and non-firearms homicides combined) homicide rate or the *total* suicide rate (firearms and non-firearms suicides combined). For reasons the authors never explain, they never analyzed either total homicide rates or total suicide rates – or at least did not report the results of such an analysis.

58.   It is possible the authors fell prey to a fallacy widespread among scholars who publish in public health journals. They accept, consciously or unconsciously, the following fallacious logic: If X is: (1) significantly associated with the rate of *firearms* homicide (or suicide); and (2) X has *no* significant association with the rate of *non-firearms* homicide (or suicide); then (3) X must have a significant association with the *total* homicide (or suicide) rate. In that case, the reasoning goes, it is unnecessary to actually show that X has a significant association with the total homicide/suicide rate.

59.   We can be certain this logic is fallacious because numerous empirical studies have obtained results directly contradicting the logic. For example, many studies obtain findings of: (1) a significant positive association of gun ownership rates with *firearm* suicide rates; and (2) no significant association of gun ownership rates with *non-firearm* suicide rates, yet also find *no* significant association of gun ownership rates with *total* suicide rates. For a sample of examples displaying this pattern, see Smith and Stevens (2003, p. 37), Miller et al. (2002, p. 32), Markush and Bartolucci (1984, p. 126), Lester (1987, p. 288), and Killias (1993, p. 294).

60.   Thus, if this is the logic the authors were relying on to believe that it was unnecessary to analyze total homicide or suicide rates, they were wrong. Based on the findings the authors did report, even if one took these dubious findings at face value, *there was no*

foundation in this study for believing that the changes in the four gun laws studied had any effect at all on either the total number of homicides or the total number of suicides. All of their results are completely consistent with the interpretation that these changes, if they had any actual impact at all, merely induced some people to change the weapons they used to kill others, or the methods they used to kill themselves, without any effect on the total number who died.

61.   This issue is critical to understanding the extremely misleading summary of previous studies the authors provide on p. 1547. In study after cited study, the authors report that previous research found that purchaser licensing laws were significantly associated with rates of *firearm* homicide or *firearm* suicide (see their cited studies 11, 12, 14, and 15). The crucial information the authors omitted, however, was that none of these four studies showed any impact of such laws on either *total* homicide or *total* suicide. Three of the studies did not even address this crucial issue (or at least did not report the relevant findings), and the one that did (study 12, p. 48) found *no* significant association of the gun law with total suicide – a finding McCourt et al. did not feel obliged to share with readers. In sum, as far as the authors knew, all four of these studies supported the view that these laws were useless for reducing either total homicides or total suicides.

The Authors' Arbitrary Truncation of the Time Period Studied

62.   The results of any statistical analysis can always be manipulated simply by arbitrarily picking unrepresentative subsets of the available data – in this case, unrepresentative sets of years – to analyze. The authors cannot justify their truncation of their study period by claiming the necessary data were not available. Official statistics on firearm and non-firearm homicides, and firearm and non-firearm suicides, have been available for every state and every year since at least as far back as 1933, in a volume titled Vital Statistics of the United States (year).

For example, the easily available online version of the 1933 data (and corresponding data for later years) may be found at https://www.cdc.gov/nchs/data/vsushistorical/mortstatsh_1933.pdf.   By arbitrarily starting their study period at 1985, leaving out 1933-1984 (52 years), the authors were omitting over 61% of the available data (1933-2017 – 85 years).

63.   This is especially harmful to their efforts to identify states that could effectively serve as components of the synthetic control because the efforts then rely on a needlessly reduced number of data points, which increases the probability that any correlation of pre-intervention trends found between the prospective control state and the target state is the mere result of a short-term coincidence prevailing only in the very brief 10-year pre-intervention period they chose to study. More generally, using smaller samples leads to less stable statistical results, regardless of the topic studied or the statistical techniques employed.

64.   The authors themselves admit that their estimates of effects of the Connecticut law were smaller when they changed the end point of their study period from 2017 to 2012. Deleting just five years from their time series reduced the estimated effect of the law on firearm homicide by 40% (compare their Table 2 with Table I in Appendix A). In short, the results are extremely sensitive to exactly which set of years were analyzed.

65.   The data for some of the predictor variables the authors incorporated in their synthetic controls (listed on p. 1547) would not be available for some earlier years, but this is irrelevant to whether it was legitimate to exclude the earlier years. The authors do not provide any evidence, in either their main article or the online supplement Appendix A, that these variables are essential or even helpful in predicting trends in homicide or suicide rates. Therefore, there is no reason to believe that the absence of such data in earlier years would justify excluding most of the years for which data on homicide and suicide were available.

66.   The end points for some of the authors' study periods are also arbitrary. The end point for their Maryland analysis is 2013 even though the authors had data for years at least up through 2017. Having fewer post-intervention years makes their results more unstable and subject to chance findings, so one should have a very strong justification for this truncation, something the authors lack. They say they truncated their study periods because another change in gun law occurred after their end-point year (2013 for the Maryland analysis, 2017 for Missouri). If enactment of new laws really did mean that an analyst could not use years after such laws had been implemented, it would mean one could not use data for *any* year. *Every* state legislature makes multiple changes in the criminal law that could affect violence rates in *every* year. For example, over the period 1973 to 1992, the Florida legislature passed an average of 381 general bills (this total excludes resolutions), including 2.45 gun control bills, *per year* (Etten, 2002). A cursory glance at the Session Laws of other states, including MO, MD, PA, and CT, supports the same general point - almost every enactment of a change in gun law is preceded or followed by numerous other changes in criminal law, many also intended to reduce crime. If the occurrence of such changes were accepted as a legitimate reason for truncating a time series, every researcher would be entitled to trim their time series down to whatever subset of history generated results supporting a favored hypothesis.

67.   Here the reason given for cutting off the study at 2013 in Maryland was enactment of a criminal law, but they do not reveal that the law was the HQL, similar to Connecticut's. Nor do they reveal that the homicide rates in Maryland increased during their study period – including 2017 – after that law's enactment. *See* Everytown for Gun Safety, Gun Violence in Maryland, at

p. 1[1]; Daniel W. Webster, *et al*., Reducing Violence and Building Trust, at p. 12[2]; Violent Crime

& Property Crime Statewide Totals: 1975 to Present.[3]

The Synthetic Control Method is Unlikely to be Useful for Assessing Policy Impact

68.   The authors tested the impact of changes in purchaser license laws on firearm

homicide and firearm suicide rates using the "synthetic control" (SC) methodology. This method

itself theoretically might be useful for evaluating the impact of a policy, but only in extraordinarily

rare circumstances.

69.   The basic logic of the design is that the researcher looks for areas (besides the target

area that implemented the policy being evaluated) that had similar trends in the outcome variable

(the firearms homicide or suicide rate in this case) as well as correlates of the outcome variable

prior to the implementation of the new policy. These areas are then combined into a single

"synthetic control" unit whose trends in the outcome variable are used to simulate how that

outcome variable would have trended in the intervention area during the post-intervention period,

had that policy not been implemented. The areas that more closely mirror the pre-treatment trends

of the outcome variable are assigned greater weight in the computation of the synthetic control

(SC). If post-intervention trends in the outcome variable are more favorable (more of a decrease

or less of an increase in violence) in the area with the new policy than in the synthetic control, the

analyst tentatively concludes that the intervention was effective.

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[1] https://maps.everytownresearch.org/wp-content/uploads/2020/04/Every-State-Fact-Sheet-2.0-042720-Maryland.pdf

[2] https://www.jhsph.edu/research/centers-and-institutes/johns-hopkins-center-for-gun-policy-and-research/_docs/reducing-violence-and-building-trust-gun-center-report-june-4-2020.pdf

[3] https://opendata.maryland.gov/Public-Safety/Violent-Crime-Property-Crime-Statewide-Totals-1975/hyg2-hy98

70.   The utility of the SC method, then, relies entirely on the coincidence of there being other areas whose trends in the outcome variable closely mirror those prevailing in the area in which the policy was implemented. In the present case, if there are no states whose trends in firearms homicide or suicide rates in the years prior to changes in purchaser licensing law happened to closely parallel those trends in the states experiencing such changes, the SC method cannot predict post-intervention trends and thus cannot generate an accurate estimate of the impact of the gun law change. This is true regardless of what weights are attached to each state – if none of the states are much good for predicting trends in firearms homicide or suicide rates, the differing weights can only reflect the fact that some states are even worse than others.

71.   As it happens, there were no states whose trends in firearms homicide or firearms suicide closely matched those prevailing in the pre-intervention periods in the four states evaluated by the authors.

The Authors' Synthetic Controls Were Not Effective in Tracking Gun Violence Trends

72.   The authors' conclusion that the changes in gun laws caused changes in firearms homicide or suicide rates was entirely dependent on a single assumption: that their synthetic controls could accurately predict how these rates would have trended in the target states, had those states not changed their gun laws. The empirical support for this assumption in turn consists entirely of the temporal correspondence of pre-intervention trends in the synthetic control and those trends in the target state.

73.   The authors' own results, however, uniformly indicate that their synthetic controls had a very poor pre-intervention correspondence with actual trends in rates of firearms violence. Consider, for example, Figure 1 (p. 1550), focusing on pre-intervention trends in the firearm homicide rate (to the left of the dashed vertical line). In the figure pertaining to the Missouri

analysis (Figure 1b), the synthetic Missouri increases from 1997 to 1998, in effect predicting that Missouri's gun homicide rate would increase as well. In reality, the actual rate (solid line) *decreased*. Not only did the authors' synthetic Missouri fail to predict the magnitude of the actual change in firearm homicide, it did not even get the *direction* of change correct – something one could guess correctly 50% of the time by flipping a coin. One might think this was just an isolated failure, but the next year's change (1998-1999) indicates the same thing – the SC predicted a decline in firearms homicide, but Missouri actually experienced another increase. Then the SC predicted a reversal of trend from increases to decreases between 1999 and 2000, but actual gun homicides did precisely the opposite of what the SC predicted. Indeed, in *every single year from 1997 to 2002,* actual changes in firearms homicide rates were exactly the opposite of what the authors' SC predicted. In the last year before Missouri changed its law, from 2006 to 2007, the SC again failed to predict the direction of the change in firearms homicide. The same unsupportive patterns can be found in the figure pertaining to Connecticut (Figure 1a) – the direction of change predicted by the Connecticut synthetic control was wrong for 1986-1987, 1987-1988, 1989-1990, and 1991-1992. Even when the synthetic control got the direction of change correct, the magnitude of change was often wrong. For example, the SC predicted a sharp decline from 1993 to 1994, but Connecticut actually experienced only a mild decline.

74.   Results in the online supplement Appendix A[4] regarding Maryland (see their figures A and B) and Pennsylvania (see their figures J, K, and L) likewise indicate that the authors' synthetic controls for those two states do a poor job of tracking pre-intervention trends in firearm and non-firearm homicides and suicides, and thus provide no sound basis for forecasting post-intervention trends, or judging the impact of the changes in gun laws. In sum, the authors did not

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[4] https://ajph.aphapublications.org/doi/suppl/10.2105/AJPH.2020.305822

have effective synthetic controls for any of the four states they studied, and thus no scientifically

valid basis for judging the effects of changes in purchasing licensing laws. The statistical method

the authors contend to be "rigorous" (p. 1551) is anything but.

The Authors' Interpretation of their Findings is Unwarranted

75.   The authors claimed that, because post-intervention trends in homicide or suicide

rates deviated from what their synthetic controls predicted, the change in gun laws that they

happened to be studying caused the deviation. This interpretation is unwarranted for two reasons.

First, even if the authors' synthetic controls were effective in predicting post-intervention trends

(something we know is not true), their interpretation of the results would still be unwarranted. At

best, the SC method can only establish that *something* happened around the time of the intervention

to change firearms homicide or suicide rates. It cannot establish what specific factor (or, more

likely, factor*s*) changed at that time to produce the change. The authors' opinion that it was

changes in firearms purchaser license laws that caused the change is little more than speculation

based on the temporal coincidence of the law change and the shift in gun violence trends.

However, as previously noted, virtually every drop in violence will *coincide* with some change in

law simply because of the frequency of law making and the frequency of violence declines, so this

coincidence is essentially meaningless.

76.   Second, there is an obvious alternative explanation for the deviation of (a) post-

intervention trends in homicide or suicide in the target state from (b) post-intervention trends in

the synthetic control. Trends in the synthetic control's homicide or suicide are used as predictions

of future trends in homicide or suicide in the treated state, had no gun law changed. Predictions

of future trends, however, tend to get less and less accurate the further into the future they are

projected. For example, the weatherman can predict fairly well what the daily high temperature

will be tomorrow, and his predictions for a few days after that may be moderately accurate, but his predictions for two or three weeks into the future are usually much less accurate. Correspondingly, predictions of homicide or suicide for future years get worse and worse the more one tries to make predictions for times many years into the future. Thus, even if the purchaser licensing laws had no effect at all on homicide or suicides, one would still expect target states' trends in homicide or suicide to deviate more and more from what the synthetic control predicted the homicide or suicide would be, simply because the synthetic control's ability to predict future levels of violent crime degrades the further into the future the one goes.

77.   To summarize, the McCourt et al. study does not provide any credible evidence on the effects of background check laws. None of the three Webster-coauthored studies increases the scientific strength of Webster's support for Maryland's Handgun Qualifying License.

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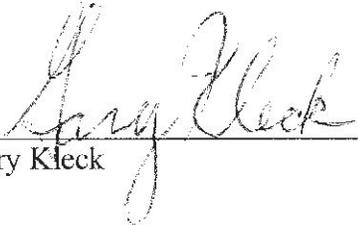https://www2.fbi.gov/ucr/cius2007/offenses/expanded_information/data/shrtable_10.html

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I hereby declare under penalty of perjury that the foregoing is true and correct.

Date: 1-26-21

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