Comment on comments about "Firearm legislation and firearm mortality in the USA: a cross-sectional, state-level study."

We have been pleased to see the attention to our recent paper in The Lancet, seeing it is a clear of the importance of the issue of gun violence in America. We set out to estimate the effects of state gun laws on rates of firearm deaths, both homicide and suicide. We built on prior research that demonstrated a link between a firearm legislative strength score and reduced gun death rates. As with other studies on the effects of gun laws, this is not a randomized trial of the effects of gun laws, nor can it be. States self-select which laws they adopt and states also vary in the designs of specific laws. And since the laws are often changing incrementally in the states, using cross-sectional data on state laws provides at least a baseline estimate. Using these laws at their 2008-10 status, we estimated the effects of state gun laws on firearm mortality one year later. We used both conventional measures of gun laws widely used in other studies, and also constructed an inventory of 25 state laws that were adopted and/or implemented from 2008-10. The effects of state laws pose critical questions in a contentious space in both policy and constitutional law.

We believe that our study improves over previous efforts by examining the simultaneous effects of specific gun laws whose implementation varies from state to state. Most prior work has looked at the cumulative effects of state laws or used *sui generis* metrics of "legislative strength." We focused instead on the role played by *specific* gun laws, seeing this as a key to inform policy by identifying "relevant and effective legislation". We also estimated the effects of gun law *changes* over a three-year period, advancing prior static analyses of state gun laws. We tried to exhaust the potential of this method to estimate the range and magnitude of legal effects.

Our results challenge conventional notions of the range of effects that gun laws can have on firearm deaths, both in their magnitude and in at least one important way, in their substance. If the findings are discomfiting to some, they represent inconvenient evidence that demands interrogation. It is not surprising that some gun laws in some states can have large effects on gun homicides, especially implementation of universal background checks. In fact, our results focusing on state implementation of these laws suggest big effects in some places, well beyond the small and non-significant effect found in earlier work on Brady laws.[1] Although one might question a large effect, evidence of a significant and negative effect should be greeted as good news in reducing deaths. And a spillover – albeit smaller – effect on suicide may also be present in states that better regulate the supply of guns. The precise mechanisms demand explanation, as does the range of plausible effects, topics for future research.

[1] Jens, Ludwig and Philip J. Cook. "Homicide and suicide rates associated with implementation of the Brady Handgun Violence Prevention Act." *Jama* 284.5 (2000): 585-591.

Not surprisingly, some researchers have raised valid questions about the paper. The questions focus primarily on the study design and the large estimates it produced. We address these questions below.

1. *The design, with 50 data points and 25 predictors, can produce unstable results.*

The 25 predictors capture the range of statutes that reflect state efforts to regulate access to firearms and ammunition, as well as a rich set of covariates associated with firearm fatalities. The critiques rest on the limitations of the sample size and on the model robustness. However, as we indicated in the paper, and in the accompanying statistical appendices, we ran an exhaustive set of sensitivity analyses and found the results robust to multiple challenges. In particular, we provide sensitivity analysis by using the *change* in gun death rates from 2008 to 2010 as the outcome and found results similar to the main results (Supplementary Appendix Page 34). We encourage readers to look closely at these appendices to better understand the moving parts of the analysis.

We also present the analysis where we pooled the individual laws as categories (Supplementary Appendix; Page 33) and found that such pooled estimates are much less informative than the assessment of individual laws. In addition, we estimated further sensitivity analyses in response to specific comments raised since publication and continue to find the results robust. We added additional three covariates to our models, as suggested by some comments: urbanicity, poverty and proportion of black population and found that the results did not change (Table 1; at the end of the document).

2. The design ignores the tremendous differences between states--differences that could not be controlled by using a handful of variables like unemployment and gun ownership rates.

We disagree. We used a parsimonious model in our study and used four relevant covariates as potential confounders. Adding multiple covariates may result in over-adjustment or may pose multicollinearity issues, and we tried to stick to covariates that are robust predictors of gun violence. We acknowledged that there could be residual confounding. In some respects, this criticism, coupled with the first criticism, poses for us a double jeopardy. We are being simultaneously criticized for having too many variables and not having enough. We are aware of both challenges, and tried to build an optimal specification to address the research questions about the effects of law. The journal reviewers agreed that this was a reasonable approach, recognizing the limitations that we noted in the paper.

To address this concern, we re-estimated our core models using a null model with no covariates, then added our four covariates, and then a panel of alternate three social structural covariates (1) urbanicity, (2) poverty rates and (3) proportion of black population (Table 1). We found no major differences between the original and alternate models (MV 1 and MV 2). The absence of change in these models reinforces the point made here: many of the potential confounders are highly collinear and do not add much to the estimates; rather, adding

additional variables would introduce model instability. For these reasons, we conclude that the approach we took, using a trimmed set of predictors, does not overtax the model. In addition, readers can assess on their own whether we have stretched the model too far by looking at the model diagnostics in the Supplementary appendix (page 18).

Table 1: Association of firearm laws with firearm-related deaths

	Crude		MV- manuscript		MV- 2	
	IRR (95%CI)	P	IRR (95%CI)	P	IRR (95%CI)	P
Firearm dealer regulations						
Gun Dealer License	0.73 (0.60-0.89)	0.002	0.91 (0.85-0.97)	0.007	0.88 (0.83-0.94)	<0.0001
Record Keeping/ Retention	0.77 (0.65-0.92)	0.003	0.79 (0.74-0.85)	<0.0001	0.76 (0.70-0.83)	<0.0001
Report Records to State	0.70 (0.57-0.85)	<0.0001	1.04 (0.97-1.11)	0.23	1.07 (1.00-1.13)	0.038
Mandatory Theft Reporting	0.68 (0.56-0.84)	<0.0001	1.64 (1.26-2.13)	<0.0001	1.78 (1.52-2.08)	<0.0001
Gun Store Security Precaution	0.76 (0.63-0.95)	0.016	0.84 (0.76-0.92)	<0.0001	0.82 (0.75-0.90)	<0.0001
Police Inspection	0.78 (0.65-0.95)	0.011	1.20 (1.10-1.30)	<0.0001	1.21 (1.13-1.30)	<0.0001
Owner purchase regulations						
Bulk Purchases Limitation	0.74 (0.62-0.89)	0.002	1.54 (1.39-1.70)	<0.0001	1.62 (1.50-1.74)	<0.0001
Firearm identification	0.65 (0.52-0.82)	<0.0001	0.16 (0.09-0.29)	<0.0001	0.12 (0.08-0.17)	<0.0001
Owner Theft Reporting	0.63 (0.45-0.87)	0.006	0.54 (0.40-0.74)	<0.0001	0.54 (0.46-0.63)	<0.0001
Background checks or additions						
Universal BC	0.72 (0.60-0.86)	<0.0001	0.39 (0.23-0.67)	0.001	0.34 (0.25-0.47)	<0.0001
Fingerprinting	0.60 (0.47-0.76)	<0.0001	1.00 (0.69-1.47)	0.98	0.91 (0.70-1.18)	0.46
Safety Training	0.71 (0.57-0.87)	0.001	0.57 (0.45-0.73)	<0.0001	0.55 (0.47-0.65)	<0.0001
Extension of BC Limit	0.61 (0.50-0.75)	<0.0001	1.33 (1.09-1.63)	0.005	1.39 (1.25-1.55)	<0.0001
Permit Law Involvement	0.64 (0.46-0.88)	0.006	0.70 (0.61-0.80)	<0.0001	0.60 (0.54-0.67)	<0.0001
Closure of Gun Show Loophole	0.66 (0.55-0.79)	<0.0001	1.09 (1.03-1.15)	0.004	1.13 (1.07-1.18)	<0.0001
Ammunition Purchaser Records	0.72 (0.61-0.84)	<0.0001	1.04 (0.82-1.31)	0.37	1.02 (0.90-1.16)	0.70
Ammunition BC	0.60 (0.42-0.85)	0.005	0.18 (0.09-0.36)	<0.0001	0.13 (0.08-0.20)	<0.0001
Child access prevention						
Firearm locks	0.64 (0.53-0.76)	<0.0001	3.90 (2.12-7.15)	<0.0001	4.62 (3.22-6.65)	<0.0001
Child Handgun Restrictions	0.50 (0.46-0.56)	<0.0001	1.16 (0.68-1.98)	0.59	1.20 (0.87-1.65)	0.27
Child Access	0.79 (0.64-0.98)	0.031	0.98 (0.94-1.02)	0.29	0.94 (0.91-0.98)	0.002
Juvenile Handgun Purchases	0.60 (0.51-0.71)	<0.0001	1.03 (0.93-1.15)	0.53	1.07 (1.01-1.14)	0.024
Assault weapon laws						
Assault weapon ban	0.57 (0.45-0.72)	<0.0001	1.70 (1.11-2.59)	0.015	1.95 (1.50-2.53)	<0.0001
Large Magazine Ban	0.56 (0.43-0.73)	<0.0001	0.97 (0.88-1.08)	0.61	1.12 (1.03-1.22)	0.007
Conceal carry "may" issue	0.62 (0.52-0.75)	<0.0001	1.20 (1.11-1.29)	<0.0001	1.22 (1.16-1.29)	<0.0001
Stand your ground	1.49 (1.27-1.74)	<0.0001	1.07 (1.03-1.12)	0.001	1.07 (1.04-1.09)	<0.0001

BC: background check; IRR: incidence rate ratio; CI: confidence intervals. IRR (95%CI) and p-values derived from poisson regression with population of 2010 as offset. Homicide rates exclude firearm homicides. MV-1: Adjusted for unemployment rate, non-firearm homicide rate, firearm ownership rate, firearm export rate and 2009 firearm mortality rate. MV-2: Adjusted for unemployment rate, non-firearm homicide rate, firearm ownership rate, firearm export rate, 2009 firearm mortality rate, urbanicity, poverty and proportion of black population.

3. *'The law on firearm identification requirements wasn't actually being implemented in any of the states at the time of the study, so it was implausible to attribute the decrease in firearm mortality to that type of law.'*

This is a recurring challenge to this type of work. We recognize that a law may be passed and fully implemented well after enactment. However, the passage of a law is associated with a range of hard-to-measure actions on the part of law enforcement, many in anticipation of the law being fully implemented, that in and of themselves may well result in changes, even if not directly linked to the law itself. [2]

In this case, at issue has been firearm identification laws. In the absence of a national firearm registry, the only way to trace assault related gun deaths is the use of forensic testing of ammunition. In 2013, CA became the first state to bar retailers from selling new models of semiautomatic guns without micro-stamping when a bullet is fired; this measure has been strongly supported by law enforcement. Since the law does not ban firearm sales, it is rather an indicator of law enforcement efforts on crime. The promise of enhanced technological advance may also deter trafficking of such weapons, preventing illegal transactions.

This is very much the case in many other documented examples (see footnote). In other areas, such as marijuana legalization studies, it is understood that there is substantial heterogeneity in the meaning of 'law' and how laws need to be considered analytically. The following taxonomy for marijuana laws has been proposed: (1) the type of provisions that enable use (in this case the intent of injury, assault, suicide, unintentional, undetermined and legal), (2) specific conditions identified in state laws that identify the type of use (in our case purpose of guns, i.e., hunting, protection etc.) and (3) identified distribution mechanism (in our case the different type of gun sales, i.e., licensed dealers, unlicensed dealers, pawn shops, gun shows and in some states like AZ which is attempting to not require background checks at all)[3]. The core question is how do these state policy dimensions hold up within each state and across states (considering reciprocity laws) and to federal challenges? Rosalie Pacula and her colleagues suggest that simple dichotomous indicators of the implementation of a law is a substantial simplification, and that policy endogeneity has to be considered when attempting to understand the impact of laws.[4]

We argue that this is the case with firearm-related laws. The dimensions of each of these policies vary from state to state, introducing several layers of complexity, and at core, an

[2] One recurring example is in capital punishment: more than half the states retain the death penalty, but fewer than 10% either sentence people to death or execute them. Yet, most research on deterrence, however flawed, still considers the presence of a death statute as a signal of risk, and includes those states in the panel models.

[3] See, for example, Rosalie Liccardo Pacula, et al. "State medical marijuana laws: Understanding the laws and their limitations." *Journal of Public Health Policy* (2002): 413-439.

[4] Rosalie Liccardo Pacula, et al. "Assessing the effects of medical marijuana laws on marijuana use: the devil is in the details." *Journal of Policy Analysis and Management* 34.1 (2015): 7-31.

analytic approach needs to take a pre-specified approach, understanding the limitations of the approach, but also reflecting the likeliest most meaningful measures that capture the state-level influences, linked to legislation, that may (or may not) influence firearm-related mortality. When considering how best to deal with this problem, we followed the lead of Eric Fleegler and colleagues, whose 2013 *JAMA Internal Medicine* analysis used a cumulative legislative strength score.[5] While this paper is not without limitations, it used a score that also included *all* the laws that had been passed, regardless of their enforcement; we used the same in our paper including the firearm identification. The Fleegler paper served as the foundation for how we considered laws in our study and is cited as such in our paper. Our paper provides a first step forward towards assessing the effectiveness of each law as opposed to a cumulative legislative strength score that offers limited solutions without considering the impact of reciprocity laws.

4. *The data suggest that some controversial gun laws have not reduced gun deaths, and may have even slightly increased the likelihood of gun-related deaths, such as Assault Weapons Bans and firearm locks.*

The observation that some firearms laws on the books are ineffective or have inconclusive findings is not new; several studies have found similar results including most recently Prickett in AJPH in 2014.[6] They conclude that "… that the important point of this debate is not the number of legislations and formulating new legislations but understanding the true impact of the current legislations and how these legislations can safeguard the people they intended to protect."

That position informed our design. We think the scientific and policy community needs to be open to the possibility that some laws that might intuitively seem to make sense may not in fact make any difference or even have unintended and perverse consequences. One such example is gun safety lock laws that are child access prevention laws. We explain why this law may not in fact have the desired effect. "The results of our analysis suggests that CAP laws are ineffective, which are in line with conflicting results on the effect of CAP laws available up to now. In a nationally representative study that used the *Brady* legislative score, a protective effect of CAP laws was shown with a differential according to rearm storage characteristics. By contrast, we showed that requirements for firearm locks, one of the CAP laws, to be ineffective, which was similar to the null effect reported in a study assessing the effect of firearm dealer regulations on firearm homicides.

On one hand, the increased risk attributed to firearm locks in our study could be explained by the results of a longitudinal study for which the presence of CAP laws was associated with an

[5] Eric W. Fleegler, Lois K. Lee, Michael C. Monuteaux, David Hemenway, and Rebekah Mannix. "Firearm legislation and firearm-related fatalities in the United States." *JAMA Internal Medicine* 173, no. 9 (2013): 732-740.

[6] Kate C. Prickett, Alexa Martin-Storey, and Robert Crosnoe. "State firearm laws, firearm ownership, and safety practices among families of preschool-aged children." *American Journal of Public Health* 104.6 (2014): 1080-1086.

increased likelihood of unsafe rearm storage in states with fewer firearm policies. On the other hand, we showed the permissive stand-your-ground law to be associated with an increased risk in rearm mortality, which was similar to the results of another analysis in which stand-your-ground was associated with an increase in accidental firearm injuries.

We also find that the assault weapons ban is ineffective, as have others.[7] That paper notes the ambiguity in the effectiveness of the 1994 Federal assault weapons ban. This law banned the sales and ownership of semiautomatic fire- arms and large-capacity ammunition magazines. Koper and Roth, using UCR data (1980–1995), found no association between the law and homicide rates in 15 states after adjusting for the presence of other firearm laws and crime laws in New York and California.[8] Similarly, Gius showed that the federal assault weapons ban was associated with higher rates of firearm homicides.[9] We think that highlighting the laws that do, and the ones that likely do not work, in combination in each state, is the central contribution of our paper.

5. *That three gun laws implemented at the federal level could reduce the American gun death rate by 90 percent is "implausible."*

The question of what is or is not "plausible" is ultimately an empirical question. No group has tried to address the specific effects of a large set of specific gun laws; we have taken a first stab at doing so. Considering that there are as many guns in this country as there are residents, it strikes us as plausible indeed that background checks for purchase of ammunition—effectively making sure that we limit ammunition in the wrong hands— may in fact substantially reduce the gun death rate. In line with this, we found a somewhat smaller reduction in gun death rates for background checks for firearm purchases. The cumulative effect of three different laws, where we currently have at least effective federal laws, may very well be possible. And when those effects are multiplied across states, large local effects may predict – if adopted elsewhere – significant reductions in gun deaths. We note that we predicted the possible discrete change in firearm mortality associated with *federal level implementation* of three most effective laws. We note that, at core, the driver of the US disproportionately high firearm-mortality rate, both homicide and suicide, is the widespread and ready availability of guns. Our analysis then is demonstrating the laws that may most effectively reduce the widespread availability of guns towards both lethal ends.

We also ran extensive sensitivity analyses including using the change in firearm mortality rate

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[7] Julian Santaella-Tenorio, et al. "What Do We Know About the Association Between Firearm Legislation and Firearm-Related Injuries?." *Epidemiologic Reviews* 38.1 (2016): 140-157.

[8] Christopher S. Koper and Jeffrey A. Roth. "The impact of the 1994 federal assault weapon ban on gun violence outcomes: An assessment of multiple outcome measures and some lessons for policy evaluation." *Journal of Quantitative Criminology* 17.1 (2001): 33-74.

[9] Mark Gius, "An examination of the effects of concealed weapons laws and assault weapons bans on state-level murder rates." *Applied Economics Letters* 21.4 (2014): 265-267.

per 100 000 people from 2008 to 2010 as the outcome, and assessing the effectiveness of laws after combining them into different classifications that do not change the core findings. We also provide the pre and post comparison of gun death rates (2008 to 2010) in the supplementary appendix (Supplementary Table 4, page 34) as a sensitivity analysis; once again we found the results of this sensitivity analysis to be similar to the main results indicating that the results presented are robust. However, we were careful in the paper to note that we believe that this is a long-term effect and thus will take several years to occur after passing and implementation of the laws. We note in the paper: "Assessment of the effect of legislative policies is akin to assessment of the effect of natural experiments or real-world data. We expect the fall in mortality to be a long-term effect and might take years to occur."

6. *There are changes in the univariate and multivariate estimates in Figure 1; these changes in both magnitude and direction of effect suggest major instabilities in the model.*

We used a cross section of the data from 2009 and 2010. Although there are some changes between models, they are very few in number and, given the large number of predictors, what one would expect to see given chance alone. And, these shifts are entirely to be expected given the data limitations, and is entirely consistent with any number of analyses using comparable data in the field.

Most important, though, we found all restrictive laws in the unadjusted analysis to be associated with reduced gun death rates. This strongly suggests that our findings are robust and replicable across states. Although we look at these laws' effects by disaggregating existing laws, unlike most prior work, there is actually nothing in our findings that is inconsistent with a careful read of the literature that examines similar statute effects. For example, the recent review and meta-analysis by Santaella-Tenorio shows that some of the laws that were intended to be restrictive were reported to be inconclusive or associated with an increased gun death rate.[10] We provided preliminary evidence regarding the association of different gun laws taken together, fully expecting that our results will have to be validated using larger studies.

7. The effect sizes obtained in this study are divergent from studies in other areas. For example, the impact of tobacco control from legislation is estimated to be ~ 2-10% change in prevalence, smoking initiation, or units used. http://www.ncbi.nlm.nih.gov/pubmed/15235381.

In many ways, gun control legislation provides a classic example of vector control in public health, echoing the previous calls to consider the gun epidemic as a public health concern by Hemenway and Miller.[11] Effective gun control legislation would be akin to effective vector control. Simply put, if there were fewer guns, there will be fewer gun deaths. This is both

[10] E.g., the assault weapon ban was shown to be inconclusive in Koper, while Gius showed an association with higher rates of gun homicides, similar to our results.

[11] David Hemenway and Matthew Miller. "Firearm availability and homicide rates across 26 high-income countries." *Journal of Trauma and Acute Care Surgery* 49.6 (2000): 985-988.

rational and expected. The legislation that we show to be effective would substantially limit the availability of guns and ammunition. It would be surprising if such an effort did *not* result in substantial reductions in mortality. We do not think that this compares in any way to tobacco 'control' that really is an issue of pricing and availability restriction to younger people but in no way limits adults' access to tobacco. The opposition to this legislation at the federal level is, in and of itself, evidence of the suspicion by those forces aligned against gun control, that these policies may indeed make a big difference. It also is worth noting that the prevalence of tobacco use in 2009-10 years of our study is well below the levels of firearm ownership in the U.S. in the same period.

8. The finding that some types of legislation may have a contrary effect of increasingly the risk of firearm mortality is surprising.

We have addressed this in our response to point number 3 above. And iatrogenic results from treatment interventions are nothing new in legal or social policy experiments.[12] While we did explain some of these findings in the paper, space constraints obviously limit how much we could say. We agree that a complex set of factors are likely interact with each other and with the design of specific statutes to shape the consequence of gun control legislation. Our core observation, that three forms of legislation are associated with reduction of gun-related deaths, seems to us robust in our analysis, and raises questions about other forms of legislation that should be examined in other studies.

9. *Results suggest that federal implementation of universal background checks for firearm purchase could reduce national firearm mortality per 100000 people from 10·35 to 4·46 deaths (57% reduction) and background checks for ammunition purchase could reduce it to 1·99 per 100 000 (81% reduction).*

Using our data and the model used in the final analysis, we computed estimates and confidence intervals for linear combinations of coefficients.[13] In the supplementary appendix (Page 16), we describe the methods in detail. Predictions are generally based on the final model. We first ran the final model and then predicted the discrete change. Discrete change is the difference in the predicted value as one independent variable changes values while all others are held constant at specified values. The discrete change is computed when a variable changes from its minimum to its maximum (Min->Max), from 0 to 1 (0->1), from its specified value minus .5 units to its specified value plus .5 (-+1/2), and from its specified value minus .5 standard deviations to its value plus .5 standard deviations (-+sd/2).

In the "Final model" column of Table 3, using the final model, we held the covariate "firearm mortality rate in 2009" at the national rate (10.22 per 100,000) to assess the firearm mortality

[12] See, e.g., Brandon C., Welsh and David P. Farrington. "Conclusions and directions from evidence-based crime prevention." In *Preventing Crime*. New York: Springer (2007): 227-237. See also

[13] David W. Hosmer, et al. (2013). Applied Logistic Regression. 3rd ed. Hoboken, NJ: Wiley.

rate in 2010 in the presence of each law. We also provide information on the predicted probability of firearm mortality in the presence of each law, for example the predicted probability of firearm mortality will drop by 0.91 in the presence of gun dealer license. Lack of federal laws is the main source of heterogeneity in firearm laws in the country.

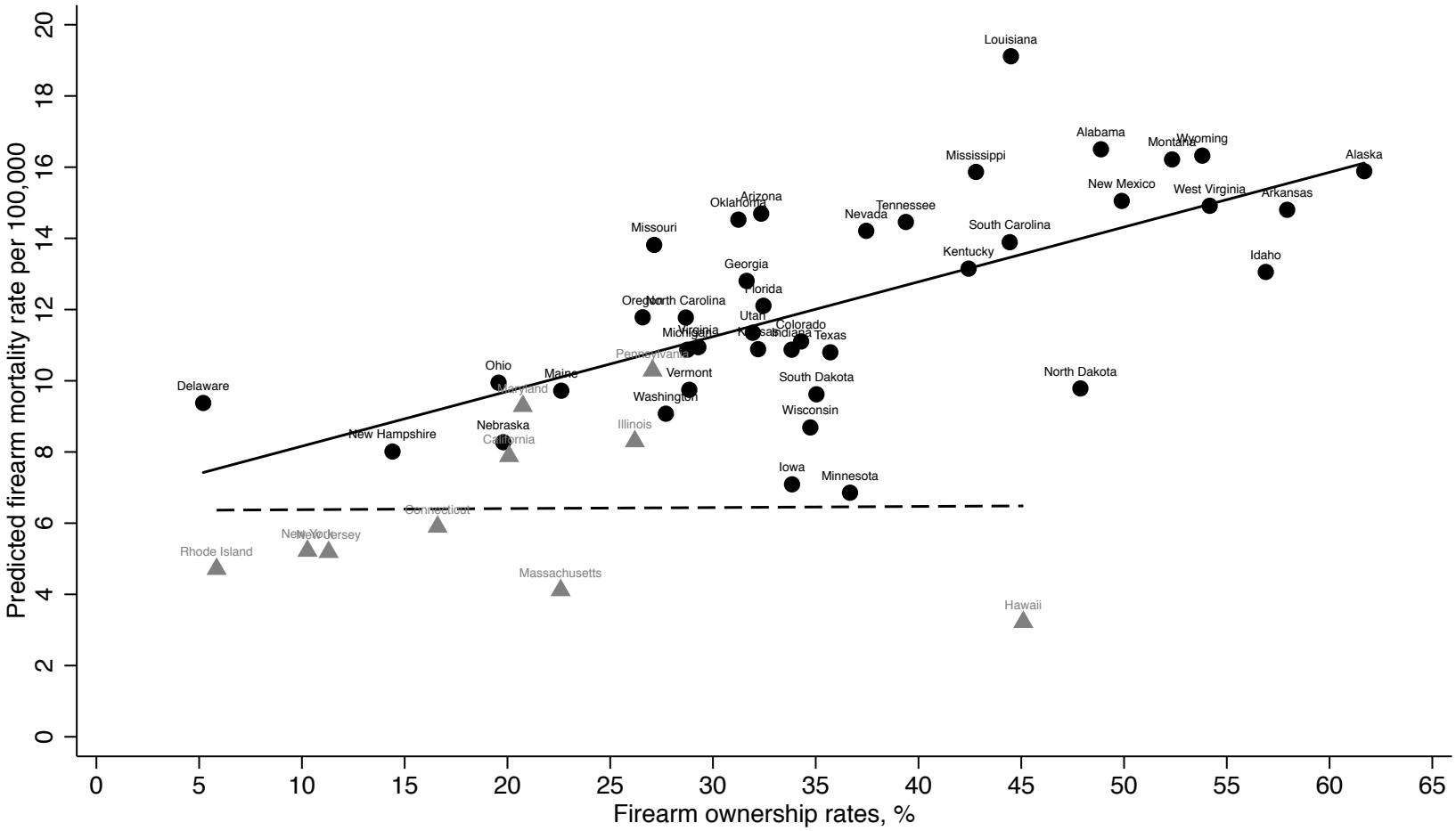
Accordingly, it is helpful to understand how much the risk estimates would decrease assuming federal level implementation of three of the most effective laws (universal background checks for firearm purchase, background checks for ammunition and firearm identification). We found that if a background check law were implemented, the firearm mortality rate would decrease from 10·35 to 4·46 per 100,000. Similarly, in the presence of ammunition background checks, the rates would decrease to 1·99 per 100,000 and with firearm identification requirements the rates would be 1·81 per 100,000. Based on the current model, on a national level, implementation of background checks for firearm purchases, for ammunition purchases and firearm identification could reduce firearm mortality rates by 57%, 81% and 83% respectively.

Additionally, we have included a new figure (Figure 1 below), showing the predicted firearm mortality rates according to gun ownership rates in states that have at least one of the three key laws and in states that do not. Figure 1 indicates that having at least one of the three laws is associated with reduced gun death rates even in the presence of increased gun ownership rates.

We are always glad to see discussions in science, and are deeply committed to ensuring that our work is as robust as possible, moving the field forward. We think disagreements about approaches and interpretation of data can indeed move fields forward. Our intent in this work was to answer a difficult question, using the best possible available data. We think we have done just that, and progress in the field will depend on other scholars building on our work, and using other empiric evidence to replicate or refute what we found.

Bindu Kalesan, Jeffrey Fagan, Sandro Galea

Figure 1: Firearm Mortality Rates and Firearm Ownership Rates by Three Key State Gun Laws



States with any of the three laws

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