ASSESSING THE EFFECT OF FIREARMS REGULATIONS USING PARTIAL IDENTIFICATION METHODS: A CASE STUDY OF THE IMPACT OF STAND YOUR GROUND LAWS ON VIOLENT CRIME*

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I
INTRODUCTION

Empirical research has struggled to reach consensus about the impact of firearms regulations on crime.[1] Consider, for example, the recent research on Stand Your Ground (SYG) laws that allow a person to use lethal force in self-defense in places outside of the home without first attempting to retreat. Using repeated cross-sectional data on annual state crime rates, recent studies have examined the impact of these laws on murder and other violent crimes.[2] Unfortunately, this research has been inconclusive, with some studies finding positive effects, others reporting negligible or insignificant effects, and still others concluding that SYG laws decrease violent crime.[3] Lott, for example, concludes SYG laws reduce murder rates by nine percent and overall violent crime by eleven percent, while Cheng and Hoekstra find that these laws increase the

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1. *See, e.g.*, NAT'L RESEARCH COUNCIL, FIREARMS AND VIOLENCE: A CRITICAL REVIEW 125-51 (Charles F. Wellford et al. eds., 2005); Charles F. Manski & John V. Pepper, *How Do Right-to-Carry Laws Affect Crime Rates? Coping With Ambiguity Using Bounded-Variation Assumptions*, 100 REV. ECON. & STAT. 232–44 (2018) [hereinafter Manski & Pepper, *Right-to-Carry Laws*].

2. *See generally, e.g.*, JOHN R. LOTT, JR., MORE GUNS, LESS CRIME: UNDERSTANDING CRIME AND GUN-CONTROL LAWS (3d ed. 2010); Cheng & Mark Hoekstra, *Does Strengthening Self-Defense Law Deter Crime or Escalate Violence? Evidence from Expansions to Castle Doctrine*, 48 J. HUM. RESOURCES 821 (2013); Chandler B. McClellan & Erdal Tekin, *Stand Your Ground Laws, Homicides, and Injuries*, 52 J. HUM. RESOURCES 621 (2017).

3. Theory provides little guidance. These laws might deter some crimes if potential offenders perceive the costs of committing crimes may be higher. Yet, these laws may increase the lethality of criminal encounters.

murder rate by eight percent.[4] As in many other areas of research on the impact of gun regulations, empirical results on SYG laws are highly variable and sensitive to minor variations in the data or the model.

The fundamental difficulty in drawing inferences on the effects of gun regulations is that the outcomes of counterfactual policies are unobservable. Data alone cannot reveal what the murder rate in a state with a SYG law would have been had the state not adopted the statute. To address this *selection problem*, observed crime data must be combined with assumptions to enable inferences on counterfactual outcomes. Yet, the assumptions needed to identify these counterfactual outcomes cannot be tested empirically, and different assumptions can yield different inferences.

In this setting, where the data alone cannot reveal the effect of firearms regulations on violent crime, it is tempting to impose assumptions strong enough to yield a definitive finding.[5] When this happens, the effect of a firearms regulation is said to be point-identified. Researchers often recognize that these strong assumptions may have little foundation, but defend their strong assumptions as necessary to "provide answers." However, strong assumptions may be inaccurate, yielding flawed and conflicting conclusions. We have seen this repeatedly in the empirical literature on the firearms regulations in general and SYG laws in particular.

To focus attention on the sensitivity of inferences to the underlying identifying assumptions, we make two simplifying restrictions here. First, we examine only the effects of adopting SYG laws in a single year rather than at any point in time. In particular, to simplify the analysis, we draw inferences on the effect of SYG laws on average violent crime rates from 2008–2010 for the thirteen states that adopted these statutes in 2006. By focusing on the impact of adopting a SYG law in 2006, we do not need to make assumptions about how the effect of the statute varies with time.[6] Second, we do not provide measures of statistical precision (for example, standard errors or confidence intervals).[7] Instead, we view the states as the population of interest, rather than as realizations from some sampling process. Thus, imprecision expressed through the width of the bounds only reflects the selection problem, not sampling variability. We do this to focus attention on the selection problem discussed above. However, even if we wanted to provide measures reflecting the

4. *See* LOTT, *supra* note 2, at 333; Cheng & Hoekstra, *supra* note 2, at 849; *see also* McClellan & Tekin, *supra* note 2, at 849 ("[R]esults indicate that Castle Doctrine Laws increase total homicides by around 8 percent.").

5. *See* Manski & Pepper, *Right-to-Carry Laws*, *supra* note 1; Charles F. Manski & John V. Pepper, *Deterrence and the Death Penalty: Partial Identification Analysis Using Repeated Cross Sections*, 29 J. QUANTITATIVE CRIMINOLOGY, 29(1), 123-141, (2013) [hereinafter Manski & Pepper, *Deterrence*].

6. The effects of right-to-carry laws on crime, for example, have been found to vary over time. *See generally* NAT'L RESEARCH COUNCIL, *supra* note 1; *see also* John J. Donohue, Abhay Aneja & Kyle D. Weber, *Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Control Analysis*, 16 J. EMPIRICAL LEGAL STUD. 198 (2019).

7. *See* Manski & Pepper, *Right-to-Carry Laws*, *supra* note 1, at 234–35.

uncertainty generated by sampling variability, it is not clear how to do so in this setting, where the state crime rate is the outcome variable of interest. The conventional assumption of random sampling from an infinite population is not natural when considering states as units of observation, and it is not clear what type of sampling process would be reasonable to assume.[8,9]

With this in mind, we seek to make transparent how assumptions shape inferences on the effects of firearms regulations on violent crime. As noted above, the existing empirical literature provides no clear insight on whether SYG laws increase or decrease violent crime, which can in part be attributed to the varying assumptions made in the literature. This Article highlights the inherent tradeoff between the strength and credibility of assumptions and findings. To do this, we apply the partial identification approach developed by Manski and Pepper to re-examine the empirical analysis of the average effect of SYG laws on violent crime.[10] These weaker, more credible models bound the average effect of SYG laws on violent crime, where the width of estimated bounds reflects the uncertainly resulting from the selection problem.

We begin in Part II by demonstrating the sensitivity of inferences to the traditional models which have been used to point-identify the average treatment effect (ATE). As described above, the traditional approach for resolving the selection problem is to impose assumptions strong enough to yield a definitive finding (that is, a *point-identified* average effect). However, these strong assumptions may be inaccurate, yielding flawed and conflicting conclusions; and we have seen this problem repeatedly in the empirical literature on gun regulations. In the analyses of SYG laws in particular, the traditional assumptions asserting that expected counterfactual outcomes are invariant across geography or time lead to qualitatively different empirical findings.

In light of these conflicting findings, what credible conclusions about the effect of SYG statutes can be drawn from the empirical literature? At one extreme, some might take the variability as evidence that empirical results are uninformative. At another, some might argue in favor of a particular model, and draw conclusions based on that model. Yet, the model assumptions cannot

8. *See id.*; *see also* Alberto Abadie et al., *Finite Population Casual Standard Errors* (Nat'l Bureau of Econ. Research, Working Paper No. 20325, 2014). In the setting of a randomized experimental design, Abadie and co-authors develop an alternative conceptualization for drawing inferences when one observes the entire population. However, their modified approach is not applicable in the more general observational data settings where the treatment—for example, right-to-carry laws—may be endogenous.

9. In the panel data literature using annual state or county crime rates to infer the impact of firearms regulations on crime, some researchers report standard errors that allow for arbitrary correlation within at a state or county—so called state/county clustered standard errors—while others do not allow for such correlations. The publications by NAT'L RESEARCH COUNCIL, *supra* note 1, and Alberto Abadie et al., *supra* note 8, show that these clustered sampling standard errors are generally inappropriate in the standard linear panel data models (with fixed state and/or county effects) used in the literature. Manski & Pepper, *Right-to-Carry Laws*, *supra* note 1, argue that random sampling assumptions underlying these conventional approaches may not be justified.

10. *See generally* Manski & Pepper, *Right-to-Carry Laws*, *supra* note 1.

generally be tested empirically and often have little foundation.[11] If the assumptions lack credibility and consensus, picking a "winner" does not resolve the ambiguity.

In Part III, we apply an alternative middle ground approach for drawing inferences under the weaker bounded variation assumptions developed in Manski and Pepper.[12] The basic idea is to replace the assumptions of invariance across geography or time with weaker bounded variation assumptions. For example, rather than assuming that states with and without SYG statutes would otherwise have identical crime rates, we might instead assume these two groups of states are similar to one another. Likewise, one might consider the assumption that, in the absence of a SYG statute, these two groups of states would have experienced similar but not identical trends in crime rates.

Bounded variation assumptions provide a way to relax the traditional invariance assumptions and improve the credibility of the empirical research on the impact of gun regulations. Manski and Pepper[13] show that these assumptions *partially* identify the ATE, yielding bounds rather than point estimates.[14] The basic insight is empirical results need not be an all or nothing undertaking. Available data and credible assumptions may lead to partial conclusions.

In Part IV, we draw conclusions. Under very weak assumptions, the data cannot reveal whether SYG laws increase or decrease crime. However, under our preferred set of bounded variation assumptions, we draw substantive conclusions about the qualitative and quantitative impact of SYG laws on violent crime. In particular, we find SYG laws have modest positive effects on rates of violent crime and murder, and uncertain effects on robbery and assault.

II

INFERENCES UNDER POINT-IDENTIFIED MODELS

In this Part, we demonstrate the sensitivity of inferences on the effect of SYG laws on violent crime, focusing on the types of point-identified models used in the literature. After providing a brief description of the data in Part II.A, in Part

11. *See generally* NAT'L RESEARCH COUNCIL, *supra* note 1.

12. *See generally* Manski & Pepper, *Right-to-Carry Laws*, *supra* note 1.

13. *See generally id*.

14. Partial identification analysis of treatment effects from observational data was initiated in Charles F. Manski, *Nonparametric Bounds on Treatment Effects*, 80 AM. ECON. REV. PAPERS & PROC. 319 (1990). For textbook exposition, see generally CHARLES F. MANSKI, PARTIAL IDENTIFICATION OF PROBABILITY DISTRIBUTIONS (2003), and CHARLES F. MANSKI, IDENTIFICATION FOR PREDICTION AND DECISION (2007). For applications, see generally, for example, Brent Kreider et al., *Identifying the Effects of SNAP (Food Stamps) on Child Health Outcomes When Participation is Endogenous and Misreported*, 107 J. AM. STAT. ASS'N 958 (2012); Charles F. Manski & Daniel S. Nagin, *Bounding Disagreements About Treatment Effects: A Case Study of Sentencing and Recidivism*, 28 SOC. METHODOLOGY 99 (1998); Charles F. Manski & John V. Pepper, *Monotone Instrumental Variables: With an Application to the Returns to Schooling*, 68 ECONOMETRICA 997 (2000); John V. Pepper, *The Intergenerational Transmission of Welfare Receipt: A Nonparametric Bounds Analysis*, 82 REV. ECON. & STAT. 472 (2000). *See also* Manski & Pepper, *Deterrence*, *supra* note 5.

II.B we examine the unconditional crime rates, and in Part II.C we estimate the types of linear regression models applied in the literature.[15]

We consider the problem of drawing inferences on the effect of SYG laws on average violent crime rates in the 2008–2010 period for the thirteen states that adopted these statutes in 2006.[16] The data reveal the annual crime rates from 2008–2010 in the thirteen states that adopted SYG statutes in 2006, but do not reveal the counterfactual crime rate that would have occurred had the thirteen treated states not adopted SYG laws. To address the selection problem, we consider three different invariance assumptions that point-identify the counterfactual crime rate as equal to either: (i) the average crime rate in the thirteen treated states before 2006 (in 2000–2004); (ii) the average crime rate in untreated states in the contemporary post-2006 period (in 2008–2010); or (iii) the pre-treatment rate in treated states adjusted by the trend in crime rates in the untreated states, as would be the case in a difference-in-difference model.

We conclude this Part by examining the pre-2006 data when the thirteen treated states did not have SYG statutes. Although the identifying assumptions cannot be empirically tested in the post-2006 period when the crime rates that would have occurred without SYG laws are counterfactual, they can be assessed in the pre-2006 period. For example, one identifying assumption holds that the effect of a SYG law can be identified as the difference between the crime rate in treated states post-2006 and untreated states post-2006. If this invariance model is valid in the pre-2006 period, the average homicide rate in states that adopt SYG laws should equal the average rate in states that do not adopt. After all, before 2006, none of the states used in our analysis had adopted a SYG statute. Yet, the homicide rate in states that adopt SYG laws in 2006 exceeds the rate in non-adopting states by as much as one and a half-points in the pre-2006 period.[17] Thus, this contemporaneous invariance model is rejected in the pre-2006 period. In fact, all three invariance model assumptions are violated during this period.

A. Data

The crime data used in our analysis comes from the Federal Bureau of Investigation's Uniform Crime Reports.[18] We use state-level data on annual crime rates (per 100,000 residents) from 1977 to 2010. For each state and year, we observe the overall violent crime rate and crime rates separately for murder,

15. *See* sources cited *supra* note 2.

16. The thirteen states are Alabama, Alaska, Arizona, Georgia, Indiana, Kansas, Kentucky, Louisiana, Michigan, Mississippi, Oklahoma, South Carolina, and South Dakota.

17. *See infra* Table 5.

18. These panel data were originally assembled by John Lott and have subsequently been modified, corrected, and updated several times. Our analysis uses the iteration assembled and evaluated by Abhay Aneja, John J. Donohue & Alexandria Zhang, *The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy*, 13 AM. L. & ECON. REV. 565 (2011).

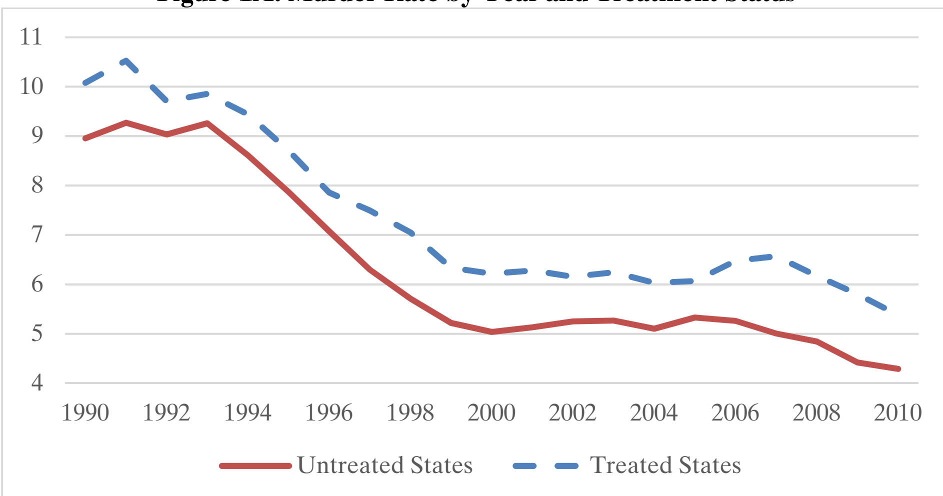
assault, and robbery. We also observe whether a SYG statute is in place.[19] Our analysis uses the SYG adoption date data from Cheng and Hoekstra.[20]

To illustrate the basic counterfactual outcomes problem, we focus on the impact of SYG laws in the thirteen states that adopted them in 2006. We refer to the states adopting SYG statutes in 2006 as "treated" states, and those that did not adopt them as "untreated" states. The eight other states that adopted SYG laws between 2005 and 2009 are excluded from the analysis.[21]

Figures 1A and 1B display the annual time series of murder and robbery rates in treated and untreated states from 1990 to 2010. The figures reveal several interesting characteristics of the crime rates.

First, murder rates in the treated states exceed the analogous rates in the untreated states. The opposite pattern exists for robbery. Second, the annual time series variation from 1990 to 2010 in crime rates for the two groups of states is similar, but not identical. For example, crime rates in untreated states have a more pronounced drop during the 1990s than those in treated states.

Figure 1A. Murder Rate by Year and Treatment Status

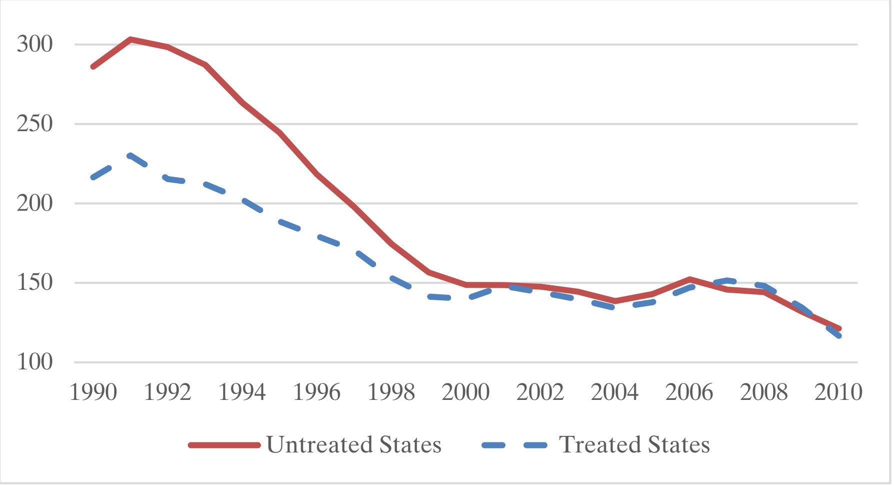


19. There are significant differences in the definition of a SYG law within the literature. In particular, LOTT, *supra* note 2, studies primarily "Castle Doctrine" laws, which remove the "duty to retreat" from the home before using lethal force. Once these laws are passed, people are allowed to use force to defend their homes even if they could retreat from the home to safety. More recent papers consider a SYG law to be a law that removes the duty to retreat from somewhere outside of the home. McClellan & Tekin, *supra* note 2, consider any law that removes the duty to retreat from a public space, while Cheng & Hoekstra, *supra* note 2, consider laws that remove the duty to retreat from someplace outside of the home, which might include private property such as a vehicle. We use the classification reported in Cheng & Hoekstra, *supra* note 2. Thus, the laws passed in thirteen states in 2006 that expanded the "Castle Doctrine" to someplace outside of the home constitute SYG laws for our purposes in this Article.

20. *See generally* Cheng & Hoekstra, *supra* note 2.

21. These states are Florida (2005), Missouri (2007), Montana (2009), North Dakota (2007), Ohio (2008), Tennessee (2007), Texas (2007), and West Virginia (2008).

Figure 1B. Robbery Rate by Year and Treatment Status



B. Unconditional Analysis

Consider the problem of using these data to draw inferences on the impact of adoption of a SYG law on the average murder rate in the 2008–2010 period for the thirteen states that adopted these statutes in 2006. Table 1 displays the average murder rate per 100,000 residents from 2000–2004 and from 2008–2010 in the states that did and did not adopt SYG statutes in 2006.

Table 1. Average Murder Rates per 100,000 Residents by Period and Treatment Status[22]

	2000–2004	**2008–2010**
Treated (in 2006)	6.94	6.17
Untreated	5.16	4.52

What do these data reveal about the effect of SYG laws on the 2008–2010 murder rate in treated states? The average murder rate in the 2008–2010 period in the thirteen states that adopted SYG statute in 2006 equals 6.17. However, the data do not reveal the murder rate that would have been observed in the treated states had the statutes not been adopted in 2006, which is necessary to assess the impacts of these statutes on the murder rate.

These counterfactual average murder rates can be estimated by comparing the 6.17 value to other rates presented in Table 1. Each comparison represents a different assumption and results in a different estimated ATE. Thus, our assumptions provide three different estimates of the counterfactual average murder rate:

i. A before-after assumption uses the pre-period rates (2000–2004) among the treated states, 6.94, as the point of comparison;

22. The treated group includes the thirteen states stated *supra* note 16. The untreated group includes twenty-nine states and the District of Columbia. The eight states that adopted the SYG statutes between 2005 and 2009 (not including 2006), stated *supra* note 21, are not included.

ii. A contemporaneous assumption uses the contemporaneous rate in the untreated states, 4.52, as the point of comparison; and

iii. A difference-in-difference assumption (DnD) adjusts the pre-period rate in the treated states by the time-series variation in the untreated state, 6.29 (=6.94-(5.16-4.52)).

These data and assumptions result in estimated ATEs of -0.77, 1.66, and -0.12, respectively, and are summarized in Table 2. Table 3 displays estimates of the ATE of SYG laws on violent crime, murder, robbery, and assault under the same modeling assumptions. All of the estimates in Tables 2 and 3 are highly sensitive to the different modeling assumptions: depending on the model used, the estimates imply that SYG laws decrease, increase, or have almost no effect on murder rates, and have similarly variable impacts on other crime rates.

Given certain assumptions, each estimate measures the effect of SYG laws on the murder or other crime rates. The before-after estimate is correct under the assumption that, except for the enactment of a SYG law, no determinant of criminal behavior changed in treated states between the early and late 2000s. The contemporaneous comparison estimate of the 2008–2010 rates is correct under the assumption that, except for the presence of the SYG statutes in the treated states, the populations of the treated and untreated states had the same propensities for criminal behavior and faced the same environments. The DnD estimate is correct under the assumption that, in the absence of a SYG statute, the treated and untreated states would have experienced the same change in murder and crime rates between the early and late 2000s. Thus, each of the three estimates can be justified by specific invariance assumptions. However, it may be that none of the assumptions hold and the variation in empirical findings shows that these invariance assumptions cannot jointly hold.

Table 2. 2008–2010 Murder Rate for the Treated States Under Different Models: Observed, Counterfactual, and the ATE

Model Employed	Observed Murder Rate	Counterfactual Murder Rate	ATE
Before-After	6.17	6.94	-0.77
Contemporaneous	6.17	4.52	1.65
DnD	6.17	6.30	-0.12

Table 3. Crime Rate ATEs Under Different Invariance Models

Model Employed	Murder	Robbery	Assault	Violent Crime
Before-After	-0.8	-4	-27	-35
Contemporaneous	1.7	-14	56	53
DnD	-0.1	9	18	27

Importantly, these are more than contrived illustrations of the sensitivity of inferences to different assumptions. All three of these research designs have been applied in the literature on the impact of SYG statutes, especially the before-after and DnD models.[23]

While commonly used, these invariance assumptions may not credibly address the selection problem. SYG statutes are not likely to be randomly assigned, as would be the case in a randomized control trial, and any imaginable comparison group is likely to differ in ways that may lead to spurious correlations in the observed data and biased inferences on the impact of firearms regulations.

To address this concern, many of these studies (especially those using DnD models) use multiple regression models to statistically account for observed factors such as state demographics, socioeconomics, policing, other firearms laws, and so forth. In this case, researchers employ the conditional analysis approach, which assumes that an invariance assumption conditionally applies when statistically controlling for the set of observed covariates even if it may not apply when excluding such control variables from the analysis. Yet, the fact that states or time-periods with the same observed covariates have different firearms regulations suggests that confounding unobserved factors may play a role in the selection process even after controlling for observed variables. We consider the conditional analysis approach below in Part II.C.

C. Conditional Analysis

Some evaluations of SYG laws analyze crime data across many states and years and statistically control for a large set of observed covariates.[24] Although these studies use additional data on SYG statutes, annual crime rates, and covariates, the empirical findings rest on similar if not stronger identifying assumptions. Data alone cannot resolve the selection problem.

Consider, for example, the linear panel data models that rely on the strong invariance assumption that, after controlling for a set of covariates, SYG laws have the same effect on crime rates in all states and years. When combined with certain other assumptions, this homogeneity assumption point-identified the impact of SYG laws on crime. However, the assumption that the effects of firearms regulations are identical across states and time is not credible. For

23. The before-after invariance model is applied in Mitchell B. Chamlin, *An Assessment of the Intended and Unintended Consequences of Arizona's Self-Defense, Home Protection Act*, 37 J. CRIME & JUST. 327 (2014); Mitchell B. Chamlin & Andrea E. Krajewski, *Use of Force and Home Safety: An Impact Assessment of Oklahoma's Stand Your Ground Law*, 37 DEVIANT BEHAV. 237 (2015); David K. Humphreys, *Evaluating the Impact of Florida's "Stand Your Ground" Self-Defense Law on Homicide and Suicide by Firearm: An Interrupted Time Series Study*, 177 JAMA INTERNAL MED. 44 (2017); Ling Ren et al., *The Deterrent Effect of the Castle Doctrine Law on Burglary in Texas: A Tale of Outcomes in Houston and Dallas*, 61 CRIME & DELINQ. 1127 (2015).

DnD invariance models are also applied in a number of papers. *See generally* sources cited *supra* note 2; *see also generally* Vincent Ferraro & Saran Ghatak, *Expanding the Castle: Explaining Stand Your Ground Legislation in American States, 2005–2012*, 62 SOC. PERSP. 907 (2019); Mark Guis, *The Relationship Between Stand-Your-Ground Laws and Crime: A State-level Analysis*, 53 SOC. SCI. J. 329 (2016); Daniel Webster et al., *Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides*, 91 J. URB. HEALTH 293 (2014).

24. *See*, *e.g.*, sources cited *supra* note 2.

example, the empirical literature on right-to-carry (RTC) laws finds that the effects of these laws vary over time and across states.[25]

To illustrate how estimates from the linear panel data models can vary with data and assumptions, we use repeated cross-sectional crime data from 1977–2010 to estimate a set of linear panel data models that differ in the underlying invariance restrictions, the time period used in the evaluation, the set of covariates, and the classification of SYG statutes. For each model, we present estimates for the log crime rate and the level crime rate. Much of the literature studies the natural log of the crime rate, where the estimates are interpreted as the effect of a SYG law on the percent change in the average crime rate. Following Manski and Pepper, our analysis here using the bounded variation assumption focuses directly on the state-year crime rates in which case the estimates are interpreted as the effect of SYG law on the average crime rate.[26] We also present results using two different sets of covariates applied in the literature on RTC laws. First, we use the control variables from Lott including detailed state race and age demographics, the arrest rate, the incarceration rate, median per-capita income, the poverty and unemployment rate, an RTC indicator, and state and year fixed effects.[27] Second, we use the more parsimonious set of controls from Donohue and co-authors.[28] In particular, this specification does not include measures of the arrest rate or the full set of age and race demographic control variables, but does include variables measuring the size of the police force.

As with the unconditional analysis in Part II.B, the estimates from these linear panel data models vary with the particular form of the invariance assumption. Table 4A presents results restricting the treatment group to the same thirteen "treated" states that adopted SYG statutes in 2006 and the control group to the same "untreated" states that did not adopt a SYG statute used in Parts II.A and B.[29] Most of the point estimates imply that SYG laws increase violent crime, but the estimated magnitudes are sensitive to the underlying identifying assumption. The first two rows in Table 4A present basic linear DnD results with different sets of covariates. The estimates displayed in Row 1, which use the Lott specifications,[30] suggest that SYG laws increased the average murder rate by 9%, the robbery rate by 2%, and overall violent crime rate by 1%, but decreased the average assault rate by 1%. The second row, which reports results using the specifications from Donahue and co-authors,[31] suggests substantially larger effects of SYG laws; SYG laws increased murder rates by 12%, and increased robbery, assault, and violent crime rates by about 5%.

25. *See generally, e.g.*, Donohue, Aneja & Weber, *supra* note 6.

26. *See generally* Manski & Pepper, *Right-to-Carry Laws*, *supra* note 1.

27. *See generally* LOTT, *supra* note 2.

28. *See generally* Donohue, Aneja & Weber, *supra* note 6. This is similar to the set of controls used in Cheng & Hoekstra, *supra* note 2.

29. The eight states that adopted SYG laws in other years, identified *supra* note 21, are dropped.

30. *See generally* LOTT, *supra* note 2.

31. *See generally* Donohue, Aneja & Weber, *supra* note 6.

Table 4A. Linear Panel Model Estimates, 13 Treated States that Adopted in 2006

	Murder		Robbery		Assault		Violent crime	
	Log	Level	Log	Level	Log	Level	Log	Level
DnD, Lott (2010) controls	0.09	0.75	0.02	8.31	-0.01	3.68	0.01	12.13
DnD, Donohue (2019) controls	0.12	0.39	0.05	17.15	0.03	-4.11	0.05	13.42
Before/After, Lott controls	0.12	0.79	0.09	11.04	-0.01	-3.45	0.03	11.85
Contemporaneous, Lott controls	0.06	0.32	0.21	9.39	0.17	48.13	0.15	60.91

The final two rows in Table 4A present estimates for a before-after and contemporaneous identification strategy for the effect of SYG laws on crime rates. The before-after comparison uses only states that passed the law in 2006 and compares the crime rates in those states before and after 2006, while the contemporaneous estimate compares crime rates after 2006 in states that passed a SYG law with the crime rates in states that did not. While these estimates generally imply SYG laws increase violent crime, the magnitudes vary across the two models. For example, SYG laws are estimated to increase the robbery rate by 21% under the contemporaneous invariance model versus a 9% increase under the before-after model.

Overall, these results suggest that SYG laws increase violent crime rates for the thirteen treated states in the 2008–2010 period. However, there is substantial variation in the magnitude of the estimated effects. Depending on the identifying assumptions, the log crime rate regression estimates imply that SYG laws increase murder rates between 6% and 12%, robbery between 2% and 21%, the assault rate between -1% and 17%, and the violent crime rate between 1% and 15%.

Table 4B expands the treatment group to all states that adopted SYG statutes between 2005 and 2010.[32] This replicates the basic models and time period examined by Cheng and Hoekstra.[33] As in Cheng and Hoekstra's article, the resulting estimates imply that SYG laws increase some crimes but decrease others. In particular, when using the covariates employed by Donahue and co-authors,[34] SYG laws are estimated to increase the murder rates by 4% but decrease the robbery and assault rates by 6% and 2%, respectively.

Finally, Table 4C uses data from 1977–2005 to examine the effect of the "Castle Doctrine" laws, which remove the "duty to retreat" from the home

32. These additional states are identified *supra* note 21.

33. *See generally* Cheng & Hoekstra, *supra* note 2.

34. *See generally* Donohue, Aneja & Weber, *supra* note 6.

before using lethal force.[35] As with Lott's findings, the resulting estimates imply that these laws decrease violent crime. In particular, the murder rate is estimated to fall by 4%, robbery by 2%, and assault by 10%. Finally, under this specification, SYG laws are estimated to decrease the overall violent crime rate by 6%.

Table 4B. Linear Panel Data Model Estimates 2005–2010, Cheng and Hoekstra (2013) Replication[36]

	Murder		**Robbery**		**Assault**		**Violent crime**	
	Log	**Level**	**Log**	**Level**	**Log**	**Level**	**Log**	**Level**
DnD, Lott (2010) controls	0.03	0.13	-0.0001	-2.67	-0.005	-16.06	-0.003	-19.40
DnD, Donohue (2019) controls	0.04	0.10	-0.06	-13.59	-0.02	-34.40	-0.03	-48.01

Table 4C. Linear Panel Data Model Estimates 1977–2005, Lott (2010) Replication[37]

	Murder		**Robbery**		**Assault**		**Violent crime**	
	Log	**Level**	**Log**	**Level**	**Log**	**Level**	**Log**	**Level**
DnD, Lott (2010) controls	-0.04	-0.49	-0.02	5.27	-0.10	-44.37	-0.06	-40.32

D. Assessing the Invariance Assumptions Using Pre-Treatment Data

One question to consider in light of the different invariance models' estimates is whether a data-driven approach can determine if any of the identifying assumptions are valid. Although the validity of the different invariance models cannot be empirically tested, pre-treatment data can be used to assess the credibility of the assumptions. Here, we can compare the pre-treatment estimates of the crime rate implied by the invariance model with the observed crime rate in the thirteen states that adopt SYG laws in 2006. The invariance model, if correct, point-identifies the crime rate that would be realized if the treated states did not have a SYG statute. Pre-2006, the treated states did not have a SYG statute. Thus, if the invariance assumptions are valid in the pre-2006 period, the estimated rate of crime found under the invariance assumption will equal the observed crime rate in the thirteen treated states. If so, this finding may provide some heuristic support in favor of the invariance assumption.

The crime rates displayed in Figures 1A and 1B provide direct evidence that the invariance assumptions do not hold across all years. All three of the invariance model assumptions are inconsistent with crime rate data pre-2006,

35. These are the object of Lott's analysis. *See generally* LOTT, *supra* note 2. The treated states for this analysis include Florida (2005), Illinois (2004), Colorado (1995), New Mexico (1978), North Carolina (1993), Utah (2003), and Washington (1999).

36. *See generally* Cheng & Hoekstra, *supra* note 2.

37. *See generally* LOTT, *supra* note 2.

before any state in our data set adopted a SYG statute. For example, consider the annual murder rates in treated and untreated states displayed in Figure 1A. While these states did not have SYG statutes prior to 2006, the murder rates vary over time and across the groups of states from 1990 to 2006. For example, in 2000, the average murder rate in the thirteen treated states is 1.2 points higher than the average rate in untreated states, nearly four points less than the average murder rate in treated states in 1990, and 0.03 points less than the rate found using the DnD invariance model. Thus, the strict invariance assumptions are violated in 2000, before these states adopted SYG statutes. Moreover, the signs and magnitudes of these violations differ over time and across models.

Table 5 provides additional evidence on these violations. For each pair of adjacent years from 1990 to 2006, we compute the differences between the observed crime rate in the treated states and the analogous rate found under three different invariance models. In Table 5, we display the maximum of the absolute values of these differences. So, for example, the before-after assumption is inconsistent with the observed crime rate in treated states by as much as eighty-five crimes per 100,000 people for violent crime, one for murder, fifteen for robbery and sixty for assault. Thus, the invariance models evaluated in Part II are inconsistent with the observed rates in the pre-treatment periods.

Table 5. Maximum Absolute Difference between the Observed Annual Crime Rate in the Treated States and the Counterfactual Estimate of the Crime Rate, 1990–2000[38]

	Murder	**Robbery**	**Assault**	**Violent Crime**
Before-After	1.0	15	60	85
Contemporaneous	1.5	80	60	50
DnD	0.5	15	35	25

While all three of these invariance restrictions are rejected in the pre-2006 years, the existing literature consistently applies these models, especially the DnD and before-after invariance models. Thus, researchers are assuming that the invariance restrictions apply when the outcomes are counterfactual even though they are rejected in periods when the outcomes are observed. While assumptions that are rejected in 2000 may be valid in 2010, the literature using these invariance models does not explain why this might be the case when evaluating SYG laws. Rather, as noted above in Part I, researchers often recognize that these strong assumptions may have little foundation, but defend their strong assumptions as necessary to "provide answers." The problem is that the strong invariance assumptions appear to be inaccurate and yield conflicting conclusions.

38. The maximum differences are rounded to the nearest 0.5 for murder and nearest 5 for all other crimes.

III
BOUNDED VARIATION ASSUMPTIONS

In this Part, we use bounded variation assumptions developed by Manski and Pepper to relax the three invariance models evaluated above.[39] For example, rather than assuming the counterfactual murder rate equals the contemporaneous rate from the untreated states, an assumption that is violated in the pre-2006 period, we will instead assume that the counterfactual rate is bounded between the contemporaneous rate plus and minus a constant. That is, treated and untreated states are assumed to be similar but not identical to each other. A bounded before-after variation assumption restricts the absolute difference in mean treatment response between two periods to be less than some constant. We refer to this constant as the degree of similarity and label it as "S." The larger the selected value of the constant, S, the weaker the assumption. These bounded variation assumptions have identifying power because they imply that counterfactual state-year murder rates are similar to observed rates in other states and years.

In Part III.A, we examine the sensitivity of inferences to different identifying restrictions without taking a stand on any particular bounded variation model or the value of the associated degree of similarity, S. Then, in Part III.B, we apply a specific set of bounded variation models that are consistent with the pre-2006 crime rates. In particular, we use the results in Table 5 to define the degree of similarity. Under these bounded variation models, we find that SYG statutes increase murder and violent crimes rates, but have uncertain effects on robbery and assault.

A. Sensitivity of Inferences to the Bounded Variation Assumption

We begin by examining the sensitivity of inferences to different bounded variation assumptions without taking a stand on the particular value of the degree of similarity, S. This allows us to illustrate the sensitivity of inferences to different assumptions.

Figure 2 traces out the effect of SYG statutes on average murder rates in the thirteen treated states for different values of S. The traditional contemporaneous invariance assumption, where S = 0, point-identifies the ATE, revealing that SYG statutes increase the average murder rate by 1.65. However, ambiguity about the ATE increases with S, and any value of S larger than two renders it impossible to sign the ATE. For example, when S = 2, the counterfactual murder rate is estimated to lie between [2.5, 6.5], or the counterfactual murder rate from Table 2 +/- S. The observed murder rate for the thirteen treated states is 6.2, and the ATE is estimated to lie between [-0.3, 3.7].

Figure 2 also traces out the ATE of SYG statutes under the bounded before-after invariance assumption. The traditional before-after invariance assumption (S = 0) point-identifies the ATE, revealing that the SYG statutes decrease the

39. *See generally* Manski & Pepper, *Right-to-Carry Laws*, *supra* note 1.

average murder rate by -0.77. Uncertainty about the ATE increases with S, and for values of S in excess of 1, the sign of the ATE is not identified.[40] Finally, relaxing the DnD invariance restriction even by small amounts (S ≥ 0.15) makes it impossible to sign the ATE.

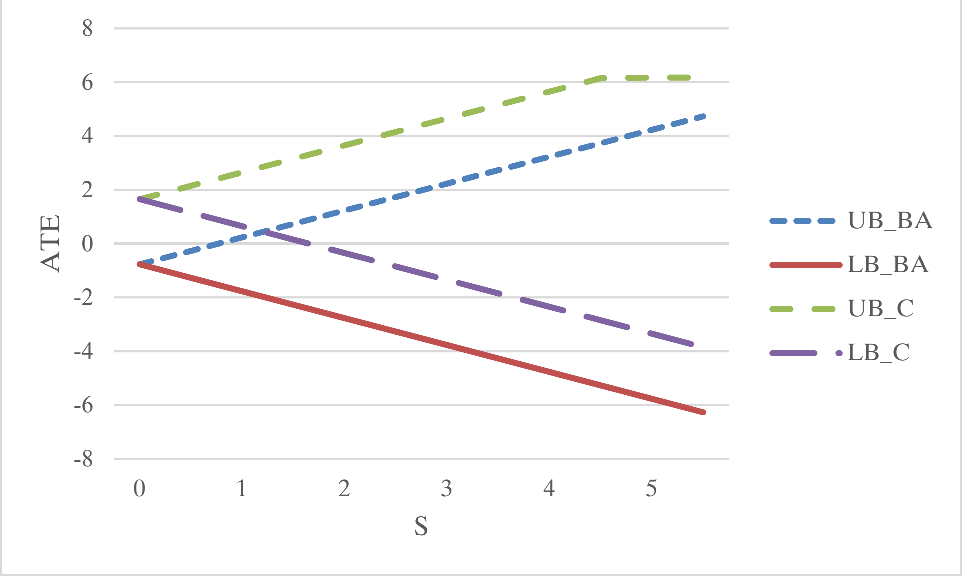
Rather than consider a single invariance assumption in isolation, it may be sensible to combine different sets of bounded variation assumptions. For example, one might simultaneously assume that the treated and untreated states are similar and that treated states had similar characteristics in the 2000–2004 and 2008–2010 periods. Focusing on this joint model, some restrictions on the degree of similarity are rejected while others lead to point-identification. For example, it cannot be that S = 0 for both the contemporaneous and before-after bounded variation assumptions. To see this, examine Figure 2, which shows that the point estimate when S = 0 for the contemporaneous model differs from the point estimate when S = 0 for the before-after model. The models point-identify the ATE for a variety of parameter values, and identify the sign of the ATE for others.[41] The ATE is identified to be negative for any feasible values of the degree of similarity such that S < 0.8 for before-after models, and greater than zero for any feasible value of the parameters where S < 1.7 for contemporaneous models. For other values of the degree of similarity, the bounds do not identify the sign of the ATE.

Overall, this sensitivity analysis traces out ambiguity resulting from the selection problem. We find that the sign of the ATE is identified for some parameter values, but not others. When different bounded variation models are simultaneously applied, the strict invariance models are ruled out, but other models either point-identify the ATE or identify the sign of the ATE. Finally, for some parameter values, the joint model does not identify whether SYG laws increase or decrease murder. Clearly, as with the invariance models, the results are sensitive to the underlying assumptions. Next, we evaluate the impact of SYG laws under a set of particular bounded variation models that are based on the pre-2006 data.

40. For example, when S = 2, the counterfactual murder rate is estimated to lie between [4.9, 8.9], the observed crime rate for the thirteen treated states is 6.2, and the ATE is estimated to lie between [-2.7, 1.3].

41. For example, when S = 1.2 for both models, the ATE is identified to equal 0.4, and when S = 0.1 for the bounded time variation models and 2.3 for the geographic variation models, the ATE is identified to equal -0.7.

Figure 2. Bounds on the ATE of Murder Rates Under Different Models and S[42]



B. Estimates of the Effect of SYG Laws on Bounded Variation Assumptions

Following Manski and Pepper,[43] we use the pre-treatment period to generate data-based degree of similarity parameters, S, for each bounded variation model. In particular, we use the estimates derived using the pre-2006 data displayed in Table 5. These values of S ensure the bounded variation models are consistent with the observed pre-2006 data. So, for murder, a before-after parameter S of 1.0 ensures that the bounded invariance model is consistent with the pre-2006 murder rate data. The analogous parameters for the contemporaneous model is 1.5 and for the DnD model is 0.5.

Table 6 displays the results for the three bounded-variation models discussed in this Article, first considering the three assumptions separately and then combining the assumptions. When the bounded variation models are applied separately, the estimates do not generally identify the sign of the ATE. There are, however, several notable exceptions. For violent crime rates, the ATE is estimated to be positive under the contemporaneous and DnD bounded variation models. Under these models, we estimate that the SYG statutes increase average violent crime rates by at least two and by as much as 103. For murder, the ATE is estimated to increase by at least 0.2 and at most 3.2 under the contemporaneous model.

To narrow the bounds, we combine the three bounded variation assumptions. Under this joint bounded variation model, the sign of the ATE is not identified for robbery and assault. The estimated bounds imply that SYG laws might lead

42. In this Figure, LB ≡ lower bound; UB ≡ upper bound; C ≡ contemporaneous restriction; and BA ≡ before-after restriction.

43. *See generally* Manski & Pepper, *Right-to-Carry Laws*, *supra* note 1.

the robbery rate to fall by as much as -6 or increase by as much as 11. For the assault rate, the data and models imply that effect of SYG laws lies between -4 and 33. Without stronger assumptions, we cannot determine whether SYG laws increase or decrease the expected rates of robbery or assault.

However, under this joint bounded variation model, we estimate that SYG laws increase violent crime and murder. In particular, SYG are estimated to increase the expected murder rate by 0.2, and the violent crime rate by between 3 and 50. This implies that SYG laws increase the murder rate by 3 percent (from 6.0 to 6.2) and the violent crime rate by at least 1 percent and as much as 13 percent. Thus, under this weak bounded variation model, SYG laws are estimated to have a modest positive effect on the average murder and violent crime rates. Cheng and Hoekstra, who use a DnD invariance assumption, draw similar conclusions.[44] In particular, they find SYG laws do not have a statistically significant effect on burglary, robbery, and aggravated assault, but do have a positive effect of 8% on murder.[45]

Table 6. Estimated Treatment Effect Under Different Bounded Variation Models

	Murder	**Robbery**	**Assault**	**Violent Crime**
Before-After	[-1.8, 0.2]	[-19. 11]	[-87, 33]	[-120, 50]
Contemporaneous	[0.2, 3.2]	[-94, 67]	[-4, 116]	[3, 103]
DnD	[-0.6, 0.4]	[-6, 24]	[-17, 53]	[2, 52]
DnD + Contemporaneous	[0.2, 0.4]	[-6, 24]	[-4, 53]	[3, 52]
DnD + Before-After	[-0.6, 0.2]	[-6, 11]	[-17, 33]	[2, 50]
All Three Models	[0.2, 0.2]	[-6, 11]	[-4, 33]	[3, 50]

IV

CONCLUSION

Providing credible estimates of the impact of gun laws on crime has proven to be a difficult undertaking. Despite a large empirical literature, research has failed to reach consensus about the impact of different gun laws on crime.[46] Empirical results vary with the data and are highly sensitive to minor variation in the model assumptions.

In this Article, we make transparent how assumptions shape inference, focusing on the impact of SYG laws on crime rates. These assumptions can affect the credibility of studies claiming an impact on violence relating to SYG laws. After illustrating how the empirical findings are sensitive to commonly used invariance assumptions, we then apply the recent methods developed by Manski and Pepper to assess what can be inferred under relatively weak assumptions restricting variation in treatment response across geography and time. These partial identification models highlight the inherent tradeoff between the strength and credibility of assumptions and findings. Strong invariance assumptions lead

44. *See generally* Cheng & Hoekstra, *supra* note 2.

45. *Id.* at 839.

46. *See supra* note 1.

to definitive findings that may lack credibility. Weaker bounded variation assumptions can lead to uncertain but credible findings.

By assessing the effect of SYG laws using the bounded variation assumptions, we illustrate the sensitivity of inferences to underlying assumptions. Under the weakest assumptions, the bounds are wide and cannot reveal whether SYG laws increase or decrease violent crimes. But under our preferred joint bounded invariance model, we find evidence that SYG laws have uncertain effects on assault and robbery but lead to a modest increase in violent crime and murder.