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ABSTRACT

Despite dozens of studies, research on crime in the United States has struggled to reach consensus about the impact of right-to-carry (RTC) gun laws. Empirical results are highly sensitive to seemingly minor variations in the data and model. How then should research proceed? We think that policy analysis is most useful if researchers perform inference under a spectrum of assumptions of varying identifying power, recognizing the tension between the strength of assumptions and their credibility. With this in mind, we formalize and apply a class of assumptions that flexibly restrict the degree to which policy outcomes may vary across time and space. Our bounded variation assumptions weaken in various respects the invariance assumptions commonly made by researchers who assume that certain features of treatment response are constant across space or time. Using bounded variation assumptions, we present empirical analysis of the effect of RTC laws on violent and property crimes. We allow the effects to vary across crimes, years and states. To keep the analysis manageable, we focus on drawing inferences for three states –Virginia, Maryland, and Illinois. We find there are no simple answers; empirical findings are sensitive to assumptions, and vary over crimes, years, and states. With some assumptions, the data do not reveal whether RTC laws increase or decrease the crime rate. With others, RTC laws are found to increase some crimes, decrease other crimes, and have effects that vary over time for others.

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1. INTRODUCTION

Research on crime in the United States has commonly used data on county or state crime rates to evaluate the impact of laws allowing individuals to carry concealed handguns – so called right-to-carry (RTC) laws. Theory alone cannot predict even the direction of the impact. The knowledge or belief that potential victims may be carrying weapons may deter commission of some crimes but may escalate the severity of criminal encounters (see Donohue and Levitt, 1998).[1] Ultimately, how allowing individuals to carry concealed weapons affects crime is an empirical question.

Lott (2000) describes this empirical research to a lay audience in a book with the provocative and unambiguous title *More Guns, Less Crime*. Yet, despite dozens of studies, the research provides no clear insight on whether "more guns" leads to less crime. Some studies find that RTC laws reduce crime, others find that the effects are negligible, and still others find that such laws increase violent and/or property crime. In a series of papers starting in 1997, Lott and coauthors have argued forcefully that RTC laws have important deterrent effects which can play a role in reducing violent crime. Lott and Mustard (1997) and Lott (2000), for example, found that RTC laws reduce crime rates in every violent crime category by between 5 and 8 percent. Using different models and revised/updated data, however, other researchers have found that RTC laws either have little impact or may increase violent crime rates.[2] Consider, for example, Aneja,

[1] Donohue and Levitt (1998) formalize a simple stylized model showing that in illegal markets with scarce resources, firearms (or lethality more generally) do not have a clear impact on violence. This model also implies that the impact of firearms might vary across markets and over time, a finding which is inconsistent with standard models used in empirical analysis. We elaborate in Section 3.

[2] See, for example, Black and Nagin (1998), Ludwig (1998), Ayres and Donohue (2003), and Aneja *et al.* (2011).

Donohue and Zhang (2011), who make seemingly minor modifications to the basic model and data (e.g., reducing the number of demographic covariates) used by Lott (2000). Whereas Lott (2000) found that RTC laws decrease the different violent crime rates by between 5 and 8 percent, Aneja *et al.* (2011) found that RTC laws have a negligible impact on murder and increase other violent crime rates by 20 to 30 percent. Aneja *et al.* (2011) also report that many of the point estimates are statistically insignificant.

This ambiguity may seem surprising. How can researchers using similar data draw such different conclusions? In fact, it has long been known that inferring the magnitude and direction of treatment effects is an inherently difficult undertaking. Suppose that one wants to learn how crime rates would differ with and without a RTC law in a given place and time. Data cannot reveal counterfactual outcomes. That is, data cannot reveal what the crime rate in a RTC state would have been if the state had not enacted the law. Nor can data reveal what the crime rate in a non-RTC state would have been if a RTC law had been in effect. To estimate the law's effect, one must somehow "fill in" the missing counterfactual observations. This requires making assumptions that cannot be tested empirically. Different assumptions may yield different inferences.

Yet the empirical research on RTC laws has struggled to find consensus on a set of credible assumptions. Reviewing the literature, the National Research Council (NRC) Committee to Improve Research Information and Data on Firearms concluded that it is not possible to infer a credible causal link between RTC laws and crime using the current evidence (National Research Council, 2005). Indeed, the Committee concluded that (p. 150): "additional analysis along the lines of the current literature is unlikely to yield results that will persuasively demonstrate" this link. The Committee found that estimates are highly sensitive to model specification. Yet there is no solid foundation for particular assumptions and, as a result, no obvious way to prefer specific results. Hence, drawing credible point estimates that lead to consensus about the impact of RTC laws has thus far proven to be impossible.

How then should research proceed? We think that analysis of treatment response is most useful if researchers perform inference under a spectrum of assumptions of varying identifying power, recognizing the tension between the strength of assumptions and their credibility. Research on RTC laws has commonly made *invariance* assumptions asserting that specified features of treatment response are constant across space or time. These assumptions may be too strong to be credible, but weaker assumptions asserting bounded variation may be credible.

For example, an invariance assumption may assert that neighboring states such as Virginia and Maryland have identical environments and propensities for criminality. This assumption may seem attractive because it enables contemporaneous comparison of the two states, but it may not be credible. Yet it may be credible to assume that these states are similar to one another. Likewise, an invariance assumption may assume that, in the absence of RTC statutes, Virginia and Maryland would have experienced the same change in murder rates between two periods, say 1988 and 1990. This assumption may also seem attractive because it enables difference-in-differences analysis of treatment response but, again, it may not be credible. On the other hand, it may be credible to assume that the two states would have experienced similar changes.

With this in mind, we formalize and apply a class of *bounded-variation* assumptions that flexibly restrict the variation of treatment response across states and years. Throughout, we allow for the possibility that the effects of RTC laws vary across years, states, and crimes. To keep our task manageable, we mainly focus on drawing inferences on the impact of RTC laws in a single state, Virginia, but also further illustrate the approach by examining what can be inferred about the impact of RTC laws in Maryland and Illinois, two states that did not adopt RTC statutes during the period we study.

The bounded variation assumptions we use generally do not point-identify the effects of RTC laws on crime rates. Instead, they partially identify them, yielding bounds rather than point estimates. Partial identification analysis of treatment effects from observational data was initiated in Manski (1990) and has developed subsequently. Manski (2007) provides a textbook exposition. Some applications to evaluation of criminal-justice policy include Manski and Nagin (1998), Manski and Pepper (2013), and Siddique (2013). The closest methodological precedent to the present study is the Manski and Pepper (2000) study of monotone instrumental variable assumptions. The closest applied precedent is the Manski and Pepper (2013) analysis of the deterrent effect of the death penalty, which applied some simple forms of the bounded-variation assumptions used here.[3]

The paper is organized as follows: After providing a brief overview of the data in Section 2, Section 3 formally defines the empirical question and the selection problem, and then introduces invariance and bounded variation assumptions. In Section 3, we illustrate the sensitivity of inferences to different identifying restrictions but we do not argue in a favor of any particular

[3] The only other related methodological work of which we are aware is Athey and Imbens (2006). They too study inference on treatment response with data on repeated cross-sections. Their *changes-in-changes* model weakens the assumption of homogeneous treatment effects made in many studies by making treatment response a nonseparable function of the treatment and a scalar person-specific unobserved variable. They assume that this response function is homogeneous across persons and monotone in the unobserved variable. They combine their response model with distributional invariance assumptions that enable one to infer certain counterfactual outcomes from observations of realized outcomes. We share with Athey and Imbens the broad objective of offering empirical research assumptions of varying strength and identifying power. We do not assume their changes-in-changes model, and our assumptions that restrict variation in treatment response differ from their distributional invariance assumptions.

assumption. In Section 4, we motivate particular bounded variation assumptions and present results on the impact of RTC laws under these assumptions. The results are nuanced and not amenable to a simple punch line conclusion. Inferences on RTC laws are sensitive to assumptions and, even under relatively strong assumptions, the results vary across crimes, states and years. RTC laws appear to reduce some crimes, increase others, and for some the results vary over time. Section 5 draws conclusions.

2. DATA

To evaluate the impact of RTC laws on crime, we use state level data on annual crime rates (per 100,000 residents) from 1970 to 2007. We do not normalize crime rates to lie in the unit interval or take the natural log of the crime rate, as is done in much of the literature. Rather we are interested in directly studying state-year crime rates: 100,000 times the ratio of the number of reported crimes to the population of the state. We mostly focus on crime rates in Virginia and Maryland. To further illustrate the bounded variation assumptions, we briefly examine crime rates in Illinois and Indiana in Section 4.3.

The crime data, obtained from the FBI's Uniform Crime Reports (UCR), were originally assembled by Lott and Mustard and have subsequently been modified, corrected, and updated several times. Our analysis uses the iteration assembled and evaluated by Aneja, Donohue and Zhang (2011).[4] For each state and year, we observe crime rates separately for murder, rape,

[4] The data were downloaded from http://works.bepress.com/john_donohue/89/ in June 2012.

assault, robbery, auto theft, burglary, and larceny. For each state-year, we observe whether a RTC statute is in place.[5]

Figures 1A and 1B display the annual time series of murder and robbery rates in Virginia and Maryland over the period 1970–2007. The figures reveal several interesting characteristics of the crime rates. First, notice that except for murder rates in a few years during the 1970s, crime rates in Maryland exceed the analogous rate in Virginia, in many cases by a substantial margin. Second, the figures show well-known temporal patterns in crime: crime rates rose in the 1980s and then declined sharply beginning in the mid-1990s. Crime rates in Maryland, which did not adopt a RTC statute over this period, have more pronounced changes than those in Virginia, rising faster in the 1980s and dropping faster in the 1990s. Virginia enacted a RTC statute in 1989.

3. BASIC ISSUES IN INFERENCE ON AVERAGE TREATMENT EFFECTS, WITH AN ILLUSTRATIVE APPLICATION

In this section, we formally define the empirical question and the selection problem, and introduce invariance and bounded variation assumptions. We begin by defining the average

[5] As of 2014, all 50 states have passed laws allowing citizens to carry concealed firearms. Not all states, however, have RTC laws as defined in the empirical research. The research has classified a state as having a RTC law when legal gun owners are allowed to carry concealed firearms, perhaps after certification and training. These are often referred to as “shall issue” provisions, in that the state shall issue a permit subject to the applicant meeting basic determinate criteria. Other states require applicants to demonstrate “good cause” before granting a concealed carry permit. These so called “may issue” states are not classified as having RTC laws. Today, ten states and the District of Columbia have “may issue” provisions and forty-one states have “shall issue” provisions. A recent wave of high profile state legislation allows citizens to openly carry firearms in public, enacting so called “open carry” laws. While not considered in this analysis, the same identification problems creating uncertainty in the research on RTC laws apply to any evaluation of the impact of “open carry” laws on crime.

treatment effect and then assess the effect of a RTC law on the 1990 murder rate in Virginia. We focus on this somewhat narrow question to clearly illustrate the utility of bounded variation restrictions. In particular, we show how bounded variation restrictions provide an intuitive and simple way to improve the credibility of empirical research. We also illustrate the sensitivity of inferences to different identifying restrictions, without arguing in favor of any particular set of assumptions. In Section 4, we use the observed data on crime rates to motivate a particular set of bounded variation assumptions and explore, in some detail, the inferences that arise from this set of assumptions.

In this paper, we do not provide measures of statistical precision when presenting our empirical findings on the impact of RTC laws. That is, we perform a finite-population analysis that views the states as the population of interest, rather than as realizations from some sampling process. One reason is expositional, being that we want to focus attention on the identification problem arising from the unobservability of counterfactual outcomes. Concerns with statistical precision arise only when interpreting the available data on crime rates across states and years.

Turning to the available data, a fundamental reason for not performing statistical inference is that measurement of statistical precision requires specification of a sampling process that generates the data. Yet we are unsure what type of sampling process would be reasonable to assume in this application. One would have to view the existing United States as the sampling realization of a random process defined on a super-population of alternative nations.[6] That is, one would have to pose a random process generating actual American history, with its division of the country into states with their populations of persons, as one among a set of possible histories that

[6] See Cochran (1977) and Deaton (1997), among others, for expositions of the distinction between finite-population and super-population analysis.

could have generated alternative state-year crime rates. But what random process should be assumed to have generated the existing United States, with its realized state-year crime rates? There is no obvious answer and the literature analyzing RTC laws has not engaged the question.

Existing methods for computing confidence intervals in partial identification analysis assume that the data are a random sample drawn from an infinite population; see, for example, Imbens and Manski (2004), Chernozhukov, Hong, and Tamer (2007), and Chernozhukov, Lee and Rosen (2013). Likewise, random sampling assumptions are implicit in the inferential methods used to estimate the standard errors of parameter estimates in the linear panel data models prevalent in the existing literature on RTC laws.[7] Random sampling assumptions, however, are not natural when considering states or counties as units of observation.

A similar concern has been raised by Abadie *et al.* (2014), who note that state level analysis

> "creates a problem for interpreting the uncertainty in the parameter estimates. If the parameters of interest are defined in terms of observable variables defined on units in the population, then if the sample is equal to the population there should be no uncertainty and standard errors should be equal to zero."

In the setting of a randomized experimental design, Abadie *et. al*. (2014) develop an alternative conceptualization for drawing inferences when one observes the entire population. However, their approach is not applicable in observational settings where the treatment, such as RTC laws, may be endogenous.

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[7] There is an ongoing debate about the appropriate method of estimating standard errors for the linear models used in the RTC literature (see Aneja *et al.*, 2011, and NRC, 2005). Many researchers report standard errors that allow for arbitrary correlation within a state or county – so called state/county clustered standard errors – while others do not allow for such correlations. The NRC report explains that these clustered sampling standard errors are inappropriate in the standard linear panel data models (with fixed county effects) used in the literature. Our concerns are distinct from these issues. We find no basis for reporting standard errors using any of the conventional methods, allowing for clustering or not.

3.1 Average Treatment Effects and the Selection Problem

Consider the problem of inferring the average treatment effect (ATE) of a RTC statute on the rate of commission of a specified crime in a specified year in a group of states that share specified observed covariates:

$$(1) \qquad ATE_{dx} = E[Y_d(1)|X] - E[Y_d(0)| X].$$

Outcome $Y_d(1)$ denotes the crime rate if a state were to have a RTC statute in year d, $Y_d(0)$ denotes the analogous outcome if the state were not to have a RTC statute, X denotes the specified covariates, and d indicates the specified year. The average treatment effect measures how the crime rate would differ if all states with covariates X were to have a RTC statute in year d versus the rate if all such states did not have a RTC law. The ATE can vary with d and X. We follow the literature by assuming that treatment response is individualistic; that is, a RTC law in state j may impact crime in state j, but not elsewhere. Thus, there are no spillover effects to other states.

For each state j and year d, there are two potential outcomes, $Y_{jd}(1)$ and $Y_{jd}(0)$. The former outcome is counterfactual if state j did not have a RTC statute in year d while the latter is counterfactual if the state did have a RTC statute. The observed crime rate is $Y_{jd}(Z_{jd}) = Y_{jd}(1) Z_{jd} + Y_{jd}(0) (1 - Z_{jd})$, where $Z_{jd} = 1$ if state j has a RTC statute in year d and $Z_{jd} = 0$ otherwise. We henceforth write $Y_{jd} = Y_{jd}(Z_{jd})$ for short.

The fact that the data only reveal one of the two mutually exclusive outcomes constitutes the selection problem. The implications for identification of the average treatment effect can be seen by using the law of iterated expectations to decompose the two conditional expectations on the right-hand side of (1) as follows:

$$(2) \qquad ATE_{dx} = E[Y_d(1)|X, Z_d = 1]P(Z_d = 1|X) + E[Y_d(1)|X, Z_d = 0]P(Z_d = 0|X) - E[Y_d(0)|X, Z_d = 1]P(Z_d = 1|X) - E[Y_d(0)|X, Z_d = 0]P(Z_d = 0|X).$$

Observation of the realized crime rate, treatment (RTC statute), and covariates in each state j reveals (Y_{jd}, Z_{jd}, X_{jd}). Hence, the data identify $P(Z_d|X)$, $E[Y_d(1)|X, Z_d = 1]$, and $E[Y_d(0)|X, Z_d = 0]$. However, the data do not reveal the counterfactual means $E[Y_d(1)|X, Z_d = 0]$ and $E[Y_d(0)|X, Z_d = 1]$. Thus, ATE_{dx} is not point-identified by the data alone.

3.2. Invariance Assumptions

The conventional practice used to address the selection problem has been to invoke assumptions that are strong enough to point-identity counterfactual mean outcomes and, hence, the average treatment effect. These assumptions typically assert invariance of some kind. One might, for example, assume that the ATE is constant across X and/or d.

To illustrate in a simple setting, consider inference on the impact of adoption of a RTC law on the 1990 murder rate in Virginia (VA). Virginia enacted a RTC statute in 1989. In this case, d = 1990, X = VA, and $Z_{VA,1990} = 1$. There exists only one state with X = VA, so $P(Z_{1990} = 1|X = VA) = 1$. Hence, (2) reduces to

$$ATE_{1990,VA} = Y_{VA,1990}(1) - Y_{VA,1990}(0).$$

The available data reveal that $Y_{VA,1990}(1) = Y_{VA,1990}$, but they do not reveal the counterfactual quantity $Y_{VA,1990}(0)$.

Table 1 displays the murder rates per 100,000 residents in Virginia and Maryland in 1988 and 1990. Neither state had an RTC statute in 1988. Virginia enacted one in 1989 but Maryland did not. Thus, $Y_{VA,1990}(1) = Y_{VA,1990} = 8.81$.

These data may be used to compute three simple estimates of the counterfactual 1990 murder rate in Virginia, $Y_{VA,1990}(0)$, under alternative invariance assumptions:

(3) Time invariance: $Y_{VA,1990}(0) = Y_{VA,1988}(0) = Y_{VA,1988} = 7.75$;

(4) Interstate invariance: $Y_{VA,1990}(0) = Y_{MD,1990}(0) = Y_{MD,1990} = 11.55$;

(5) Difference-in-difference invariance: $Y_{VA,1990}(0) = [Y_{MD,1990}(0) - Y_{MD,1988}(0)] + Y_{VA,1988}(0) = 9.67$.

These, in turn, imply ATE estimates of 1.06, -2.74, and -0.86 respectively. Thus, the three estimates yield empirical findings that differ in direction and magnitude.

Given certain invariance assumptions, each estimate appropriately measures the effect of the RTC law on the 1990 murder rate in Virginia. The time invariance estimate is correct under the assumption that no determinant of criminal behavior changed in Virginia between 1988 and 1990 except for enactment of the RTC statute. Interstate invariance of the 1990 murder rates is correct under the assumption that the populations of Virginia and Maryland had the same propensities for criminal behavior and faced the same environments except for the presence of the RTC statute in Virginia. The difference-in-difference (DID) estimate is correct under the assumption that, in the absence of RTC statutes, Virginia and Maryland would have experienced the same change in murder rates between 1988 and 1990. Thus, each of the three estimates can be justified by specific invariance assumptions. However, the variation in empirical findings shows that these invariance assumptions cannot hold jointly. Indeed, it may be that none of the assumptions holds. Thus, the ATE may equal none of the values 1.06, -2.74, and -0.86.

The literature evaluating RTC laws has analyzed crime data across many states and years rather than two states and two years. Having more data, however, does not reduce the dependence of empirical findings on the assumptions that researchers maintain. It has been common, for example, to assume a linear model with a homogeneous treatment effect and state-year fixed effects.[8] This model has the form

$$(6) \quad Y_{jd}(t) \;=\; \theta\cdot t + X_{jd}\beta + \alpha_j + \gamma_d + \varepsilon_{jd}.$$

Here treatment t = 1 denotes the presence of an RCT law and t = 0 otherwise. The parameter θ is the treatment effect, which does not vary with j and d. Thus, the model assumes that right-to-carry laws have the same effect on crime rates, θ, in all states and years. Model (6) permits variation in crime rates across states and years only through the composite additive intercept $X_{jd}\beta + \alpha_j + \gamma_d + \varepsilon_{jd}$. Here β is a parameter vector, while α_j and γ_d are state and year fixed effects. The unobserved

[8] The existing research on RTC laws does not formally model the selection process by which states adopt RTC statutes or, in general, apply an invariance assumption that the response functions are mean independent of observed instrumental variables. Notable exceptions are Lott and Mustard (1997) and Lott (2000). While their main focus is on standard linear panel data models like (6), they also present results under a variety of instrumental variable invariance assumptions. The instruments include, among other variables, the levels and changes in property and violent crime rates, the percent of the population in the National Rifle Association, and the percent voting for the Republican presidential candidate. The resulting estimates imply that RTC laws reduced violent and property crimes by nearly seventy percent. Other researchers have thought these estimates to be implausibly large and, more importantly, the instrumental variable assumptions to be invalid (Donohue, 2003).

variable ε_{jd} is a random state-year interaction assumed to have mean zero conditional on each realized value of RCT and X.

This model relies on the invariance assumption that the effect of RTC laws, θ, is the same for all states and all years. While this homogeneity assumption is convenient and has substantial identifying power when combined with certain other assumptions, there is little support for the notion that the effects of RTC laws are identical across states and time. In fact, the empirical literature provides some evidence to the contrary; some estimated effects of RTC laws are found to vary over time and across states (see Black and Nagin, 1988; NRC, 2005, Aneja *et al.*, 2011).

The VA and MD crime rates displayed in Figure 1 provide direct evidence that the invariance assumptions in (3), (4) and (5) do not hold across all years and states. Figure 1 displays the annual murder rates in Maryland and Virginia, from 1970 to 2007. While neither state had a RTC statute prior to 1989, crime rates vary over time and across states from 1970 to 1988. For example, in 1988, the murder rate in Virginia was 1.89 less than the rate in Maryland in that year and 0.39 greater than the Virginia rate in 1987.[9] Thus, the invariance assumptions are violated in the years before VA adopted a RTC statute, and the signs and magnitudes of these violations differ over time and across assumptions.

While all three of these invariance restrictions are rejected in the pre-1989 period, the existing literature on the effects of RTC laws on crime consistently applies these types of invariance restrictions, especially variations of the DID model in (5) or the related model in (6). In this setting, researchers implicitly assume that the invariance restrictions apply when outcomes

[9] Also notice that the time invariance assumption does not hold in Maryland, where the murder rate increased from 9.63 in 1988 to 11.55 in 1990.

are counterfactual even though they are rejected in periods where outcomes are observed. This is hard to motivate and the literature using these invariance assumptions fails to do so.

3.3. Bounded Variation Assumptions

3.3.1. The Assumptions

Invariance assumptions have a sharpness that often makes them suspect. Consider each of the three equalities (3), (4), and (5) that point-identify the impact of the RCT law in Virginia in 1990. Why should any of them hold exactly? Why should the treatment effect be exactly constant across states and years as assumed in the linear model (6)? Empirical researchers often say that such assumptions are "approximations," but they do not formalize what this means. Instead, they perform analyses that use some invariance assumptions as if they are truth and that entirely dismiss other assumptions. For example, DID estimation maintains assumption (5) but places no restrictions on response levels.

A simple way to improve the credibility of empirical research is to weaken invariance assumptions to assumptions of bounded variation. In this paper we report empirical findings under bounded-variation assumptions that weaken assumptions (3), (4), and (5) for a specified treatment t as follows:

(7) ***Bounded Time Variation***: $|Y_{jd}(t) - Y_{je}(t)| \le \delta_{j(de)}$,

(8) ***Bounded Interstate Variation***: $|Y_{jd}(t) - Y_{kd}(t)| \le \delta_{(jk)d}$,

(9) ***Bounded DID Variation***: $|[Y_{jd}(t) - Y_{je}(t)] - [Y_{kd}(t) - Y_{ke}(t)]| \le \delta_{(jk)(de)}$,

where (j, k) are specified states, (d, e) are specified years, and ($\delta_{j(de)}$, $\delta_{(jk)d}$, $\delta_{(jk)(de)}$) are specified positive constants. To simplify the notation, we suppress the dependence of the δ values on the treatment t. We also suppress for simplicity the logical requirement that crime rates must take non-negative values. This requirement does not bind in the empirical analyses that we report.

The bounded time variation assumption restricts the absolute difference in treatment response in state j between two years, say 1988 and 1990, to be less than $\delta_{j(de)}$. Letting $\delta_{j(de)} \geq 0$ weakens the traditional time invariance assumption by supposing that response may differ over time by no more than $\delta_{j(de)}$. The larger the selected value of $\delta_{j(de)}$, the weaker the assumption. Similarly, the bounded interstate variation assumption restricts the contemporaneous absolute difference at year d in response between two states, say Virginia and Maryland, to be less than $\delta_{(jk)d}$. Letting $\delta_{(jk)d} \geq 0$ weakens the traditional invariance assumption by supposing that response across the two states may differ by no more than $\delta_{(jk)d}$. The larger the selected value of $\delta_{(jk)d}$, 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. The degree of similarity is determined by the bound parameters ($\delta_{j(de)}$, $\delta_{(jk)d}$, $\delta_{(jk)(de)}$). These assumptions resemble the smoothing assumptions made in kernel nonparametric regression analysis, with δ acting as the bandwidth. An important technical difference is that the asymptotic theory for nonparametric regression makes smoothing go to zero with sample size, so smoothing is performed to achieve desired asymptotic statistical properties rather than identification. In contrast, we impose smoothing assumptions that relate counterfactual quantities to observed ones and thus provide identifying power.

It is also of interest to compare the bounded variation assumptions to those made in the linear homogeneous model. These assumptions are not nested. The bounded variation assumptions are weaker in two respects. First, they permit the ATE to vary across states and years, whereas model (6) restricts the ATE to have the constant value θ across states and years. Second, assumptions (7)-(9) do not impose a condition on mean variation in treatment response akin to the restriction of model (6) that ε_{jd} has mean zero conditional on each realized value of RCT and X. On the other hand, assumptions (7)-(9) restrict the potential magnitudes of time, interstate, and DID variation in outcomes under a given treatment, whereas model (6) does not.

3.3.2. Findings for Virginia in 1990

Figure 2 shows bounds on $ATE_{1990,VA}$ for different values of $\delta_{j(de)}$, where d = 1990, e = 1988, and t = 0. The traditional time invariance assumption ($\delta_{j(de)} = 0$) point identifies the ATE, revealing that enactment of the RTC statute increases the murder rate by 1.06. Ambiguity about the ATE increases with $\delta_{j(de)}$. The figure shows that the bound on the ATE is entirely positive when $\delta_{j(de)}$ is less than one, but any value of $\delta_{j(de)}$ larger than one renders it impossible to sign the ATE. For example, when $\delta_{j(de)} = 2$, we know that $Y_{VA,1990}(1) = 8.81$ and $Y_{VA,1990}(0) \in [5.75, 9.75]$. Hence, $ATE_{1990,VA} \in [-0.94, 3.06]$.

Figure 2 also shows bounds on $ATE_{1990,VA}$ for different values of $\delta_{(jk)d}$, where k = Maryland. The traditional interstate invariance assumption ($\delta_{(jk)d} = 0$) point identifies the ATE, revealing that the RTC statute decreases the murder rate by -2.74. Ambiguity about the ATE increases with $\delta_{(jk)d}$. The figure shows that the bound is entirely negative when $\delta_{(jk)d} < 2.74$ but values in excess of 2.74 make it impossible to sign the ATE. For example, when $\delta_{(jk)d} = 3$, we know that $Y_{VA,1990}(1) = 8.81$ and $Y_{VA,1990}(0) \in [8.55, 14.55]$. Hence, $ATE_{1990,VA} \in [-5.74, 0.26]$.

Bounded variation assumptions can be easily adapted to fit particular features of the application. For example, Figure 1 reveals that crime rates in MD generally exceed the contemporaneous crime rates in VA, even in the years before 1989 when neither state had adopted a RTC statute. One might therefore think it reasonable to make the bounded variation assumption one sided; that is,

$$(10) \qquad 0 \leq Y_{MD,1990}(0) - Y_{VA,1990}(0) \leq \delta_{(MD.VA)1990}$$

Assumption (10) makes the lower bound on the ATE equal -2.74, the point estimate under invariance assumption (4). The upper bound is identical to the one displayed in Figure (2).

Finally, letting $\delta_{(jk)(de)} \geq 0$ weakens the traditional DID invariance assumption. The traditional assumption ($\delta_{(jk)(de)} = 0$) point identifies the ATE, revealing that the RTC statute decreases the mean murder rate by -0.86. Ambiguity about the ATE increases with $\delta_{(jk)(de)}$, and for values in excess of 1 it impossible to sign the ATE.

3.4. Joint Bounded Geographic and Time Variation Assumptions

Rather than use a single invariance assumption in isolation, one may want to combine assumptions. For example, one might simultaneously assume that Virginia and Maryland are similar to one another and that Virginia had similar characteristics in 1988 and 1990. Here we evaluate bounds on $ATE_{1990,VA}$ under joint interstate and time bounded variation assumptions, and also under joint DID and time bounded variation assumptions.

Under a joint interstate and time variation assumption, the counterfactual murder rate $Y_{VA,1990}(0)$ is bounded as follows:

$$\begin{aligned} &\max(Y_{MD,1990} - \delta_{(MD,VA)1990},\ Y_{VA,1988} - \delta_{VA(1990,1988)}) \\ (11) \qquad &\leq Y_{VA,1990}(0) \leq \\ &\min(Y_{MD,1990} + \delta_{(MD,VA)1990},\ Y_{VA,1988} + \delta_{VA(1990,1988)}). \end{aligned}$$

A necessary condition for this assumption to be valid is that $\delta_{VA(1990,1988)} + \delta_{(MD,VA)1990} \geq |Y_{MD,1990} - Y_{VA,1988}|$. Otherwise, the lower bound exceeds the upper bound.

Tables 2A and 2B display the upper and lower bound on the ATE under joint bounded interstate and time variation assumptions, and under joint bounded DID and time variation assumptions, respectively. Estimates highlighted in grey are not valid; the lower bound exceeds the upper bounds. Estimates in pink (bold) identify the sign as negative, whereas those in green (italics) identify the sign of the ATE as positive. Estimates not highlighted are partially identified, but do not reveal whether adoption of the RTC law in 1989 increased or decreased the 1990 murder rate in VA.

Focusing on the joint interstate and time variation assumptions, there are several interesting findings. First, the necessary condition for validity of assumptions rules out a range of small values of $\delta_{VA(1990,1988)}$ and $\delta_{(MD,VA)1990}$ as infeasible. Thus, it cannot jointly be true that (Virginia, Maryland) are highly similar in 1990 and that (1988, 1990) are highly similar years in Virginia.

Second, the assumptions point identify the ATE for a variety of feasible values (i.e., $\delta_{VA(1990,1988)} + \delta_{(MD,VA)1990} = 4$), and they identify the sign of the ATE for others. For example, when $\delta_{VA(1990,1988)} = \delta_{(MD,VA)1990} = 2$, the ATE is identified to equal -1.2. When $\delta_{va(1990,1988)} = 0.5$ and $\delta_{(MD,VA)1988} = 3.5$, the ATE is identified to equal 0.3. The sign of the ATE is identified to be negative for all feasible $\delta_{(MD,VA)1990} \leq 3$, and greater than zero for all feasible $\delta_{VA(1990,1988)} \leq 0.5$. For larger values of $\delta_{VA(1990,1988)}$ and $\delta_{(MD,VA)1990}$, the assumptions do not identify the sign of the ATE.

Likewise, under joint DID and time variation assumptions, the sign of the ATE is identified to be negative for all feasible $\delta_{(MD,VA)(1990,\ 1988)} \leq 1$, and positive for all feasible $\delta_{VA(1990,1988)} \leq 0.5$. For larger values of $\delta_{VA(1990,1988)}$ and $\delta_{(MD,VA)(1990,\ 1988)}$, the assumptions do not identify the sign of the ATE.

To summarize, in this section we have examined the sensitivity of inferences to different bounded variation assumptions, focusing on the effect of the 1989 adoption of an RTC law on the 1990 murder rate in Virginia. We find that the sign of the ATE is identified for some assumptions, but not others. Especially interesting findings emerge when different bounded variation assumptions are combined. In particular, we find that the strongest assumptions are ruled out, including the traditional strict invariance assumptions. Less strong assumptions either point identify the ATE or identify the sign of the ATE, while weaker ones do not identify whether adoption of an RTC law increased or decreased murder in Virginia in 1990.

4. EVALUATING THE IMPACT OF RTC LAWS USING BOUNDED VARIATION ASSUMPTIONS

Whereas Section 3 assessed the sensitivity of inference to different assumptions, in this section we evaluate the impact of RTC laws under particular bounded variation assumptions. We begin in Section 4.1 by developing an approach for selecting the parameters δ based on observed data. Then, using these parameters, we present findings for Virginia in Section 4.2 and for Maryland and Illinois in Section 4.3. The latter states did not adopt RTC statutes prior to 2007. In Section 4.4 we discuss and summarize the results.

4.1. Selecting the Bound Parameters

In Section 3, we assessed how inferences vary across different values of the bound parameters δ. In this section we use observed data in VA and MD to develop sensible data-based parameter values. In particular, we use data prior to 1989, when VA did not have a RTC statute, to determine the minimum parameter values that would be required to make bounded variation assumptions consistent with the observed data. While this approach does not ensure the validity of the bounded variation assumptions, it provides a starting point for our analysis.

To illustrate the idea, consider the murder rates for Maryland and Virginia in 1987 and 1988, displayed in Table 3. The observed murder rates vary over time and across states: the 1988 murder rate in VA is 7.75 and the rate in MD is 9.63, while the analogous rates for 1987 are 7.36 and 9.55, respectively. Neither state had a RTC statute in these years, so these data can be used to test whether invariance and bounded variation assumptions are valid. In fact, the data are inconsistent with the time invariance assumption (3), the interstate invariance assumption (4) and the DID assumption (5).

While the invariance assumptions are inconsistent with the observed pre-1989 data, the bound parameters in assumptions (7), (8) and (9) can be chosen to ensure internal consistency. The bounded interstate variation assumption in (8), for example, is valid in 1988 if $\delta_{(VA,MD)1988} \geq 1.88$ (= 9.63 − 7.75). Likewise, the bounded time variation assumption is consistent with the VA 1988 murder rate if $\delta_{VA(1988,1987)} \geq 0.39$ (= 7.75 − 7.36), and the DID assumption is valid if $\delta_{(VA,MD)(1988,1987)} \geq 0.31$. These restrictions on the bound parameters make the bounded variation assumptions consistent with the 1987 and 1988 data.

Extending this approach to perform comparisons from 1970 to 1988, we find the minimum values of the bound parameters required for the assumptions to be consistent with the data prior to 1989. To do this, we compute the minimum valid parameter value for each pair of adjacent years from 1970 to 1988 (i.e., 1970 to 1971, 1971 to 1972, and so forth), and then select the maximum of these values. Table 4 displays the minimum parameters that ensure the bounded variation assumptions are consistent with the observed pre-1989 data on various crime rates. For murder, the time variation parameter needs to be at least 2.0, the interstate variation parameter at least 2.7 and the DID variation parameter at least 2.3. For robbery, the analogous parameters are 30, 292, and 55.

We use the parameter values displayed in Table 4 as a starting point for implementing the bounded variation assumptions in (7), (8), and (9). Of course, whether these particular assumptions are valid is unknown, and one must invariably make judgements about the tradeoff between the strength and credibility of assumptions and findings. Using these parameters as an anchor, one might assess the sensitivity of findings by using some multiple of the maximum value or some quantile of the distribution. Table 4, for example, shows the 0.75-quantile of the minimum valid parameters for the DID assumption (DID_0.75). In this case, the DID parameter for murder falls from 2.3 to 1.2, and for robbery falls from 55 to 27.

4.2. The Impact of a RTC Statute in VA

In this section, we use bounded variation assumptions with the parameter values displayed in Table 4 as a starting point for drawing inferences on the impact of an RTC law on crime in VA. Tables 5, 6, and 7 display bounds on the ATE of a RTC statute in VA by year, from 1990 to 2006, and for seven different crimes. Bounds highlighted in green (italics) identify the sign of the ATE as positive and those in pink (bold) identify the sign as negative. Bounds highlighted in grey are infeasible, the lower bound being above the upper bound. Bounds that are not highlighted do not identify the sign of the ATE. Each set of results applies a different assumption.

We begin by first considering estimates under the joint time variation assumption (7) and the one-sided interstate variation assumption (10) given the parameter values displayed in Table 4. For the time variation assumption, we use 1988 as the anchor year. That is, the counterfactual crime rate is identified to lie between the 1988 crime rate plus or minus $\delta_{j(d,88)}$. The estimates are displayed in Table 5. Most results are inconclusive about the sign of the ATE but there are a few notable exceptions. For murder, the bounds are consistently negative, suggesting that having an RTC statute reduced the murder rate in VA in most years. For rape and assault, the bounds are positive in some years. For auto-theft, burglary, and larceny the bounds are negative in some years, especially post-2000. There are also a number of years where the bounds are infeasible.

These conclusions rely on a bounded time variation assumption in (7) that may not be flexible enough to account for the large temporal changes in crime rates over the period 1970 to 2006. As noted above, to identify the counterfactual crime rate that would have occurred had VA not adopted a RTC law, the time variation assumption in (7) adds and subtracts the $\delta_{j(d,88)}$ to the observed crime rate in 1988. This bound on the counterfactual crime rate does not depend on the year under consideration, d – the bound is the same for 1989 and 2006 – even though there were significant changes in observed crime rates over this period.

Consider, for example, the problem of inferring the effect of a RTC statute on the 2005 burglary rate in VA. From 1988 to 2005, burglary rates fell by nearly 50% in MD, from 1175 in 1988 to 644 in 2005. Yet, using the static bounded time variation assumption, we infer that had the RTC statute not been adopted, the burglary rate in VA would have fallen by no more than 206 points or 33%, from 813 to 607. While the bounded variation parameter of 206 may be sensible when evaluating the counterfactual crime rate in 1989, it is hard to defend when evaluating the rate in 2005. Arguably, the static bounded time variation parameter developed in Section 4.1 does not properly reflect the dramatic reduction in crimes rates in the 2000s.

Using this bounded time variation model implies that the RTC statute reduced the 2005 burglary rate by at least 212, from 607 to 395. Another option is to use a DID assumption to simultaneously account for time series and geographic variation. Continuing with the example above, the DID invariance assumption would identify the counterfactual 2005 VA burglary rate by subtracting the observed time series difference in the MD crime rates, (1175 − 644), from the observed VA burglary rate in 1988, 813. Thus, this DID assumption accounts for time series variation using the observed changes in the MD crime rate.

Table 6 presents results for all seven crimes and all years under the DID invariance assumption. As with the time variation assumption, we use 1988 as the anchor year for the DID invariance assumption. For murder, the DID assumption estimates are strictly negative. For the other crimes, the estimates are negative in earlier years, but tend to be positive in later years. For example, the RTC statute is estimated to have increased the 2005 burglary rate by 112.4.

These results, however, rely on a strict invariance assumption. If we use the parameter values in Table 4 to relax this invariance restriction to a bounded variation DID assumption, nearly all of the estimated bounds except those for larceny include zero. That is, under the bounded DID variation assumption, we cannot generally identify the sign of the ATE. To see this, add and subtract the DID parameters in Table 4 to the point estimates in Table 6. For example, the impact of RTC laws on the 2000 murder rate is bounded between [-0.6 − 2.3, -0.6 + 2.3] = [-2.9, 1.7]. The bound on the impact on the 2005 burglary rate is [-24, 248]. Interestingly, while this bound includes zero it does not intersect with the estimate found under the joint time and interstate variation model displayed in Table 5, and therefore implies that the two bounded variation models cannot both be valid.

The results from these two DID assumptions provide a stark illustration of the tradeoff between the strength of assumptions and conclusions. Under the strong invariance assumptions the ATE is point identified while under the weaker bounded variation assumption one cannot generally infer the sign of the ATE.

Partial identification methods allow us to formally bridge the gap between these two extremes by considering middle ground assumptions that yield information on the sign of the ATE. To do this, we continue to focus on applying DID bounded variation assumptions and also introduce weak time and interstate variation assumptions that seem well suited for this application.

In particular, we impose the following three assumptions:

a. $|[Y_{jd}(0) - Y_{je}(0)] - [Y_{kd}(0) - Y_{ke}(0)]| \leq \delta_{(jk)(de)}$, where $\delta_{(jk)(de)}$ = DID_0.75 in Table 4 for $d \in [1990, 2006]$ and $e = 1988$;

b. $Y_{VA,d}(0) \leq Y_{MD,d}(0)$ for $d \in [1990, 2006]$;

c. $Y_{VA,d}(0) \geq Y_{VA,88}(0)$ for $d \in [1990, 1996]$;

$Y_{VA,d}(0) \leq Y_{VA,88}(0)$ for $d \in [1999, 2006]$.

Assumption (a) is the DID bounded variation assumption where the bound parameter equals the 0.75 quantile of the distribution of the minimum bound parameters required for the assumptions to be consistent with the data in each year prior to 1989 (see the DID_0.75 column in Table 4). This assumption relaxes the strict invariance assumption where $\delta = 0$ but strengthens the bounded variation assumption using the DID parameters in Table 4. Assumption (b) restricts the crime rate in VA to be no greater than the crime rate in MD, a modified version of the one-sided bounded interstate variation assumption in (10). Finally, assumption (c) is a bounded time variation assumption that operationalizes the idea that crime rates rose in the early 1990s and fell in the 2000s.

Table 7 displays the estimates using these bounded variation assumptions. The results vary by crime and year. We find that the RTC law in VA reduced murder and larceny rates in nearly every year, but increased assaults after 1997. For other crimes, the sign of the bounds is generally negative in the 1990s but positive or indeterminate in the 2000s.

Interestingly, these results are generally inconsistent with the standard assumption that the ATE does not vary over time (see model 6). To see this, note that under the assumption that the ATE is the same in every year, one can take the intersection of the bounds from each year to derive a refined tighter bound. This is an instrumental variable bound, as defined in Manski (1990). Yet, except for murder, the intersection bounds are empty; the lower bound exceeds the upper bound. For example, the lower and upper intersection bounds for robbery are 23.2 and -78.2. Thus, this bounded variation assumption is inconsistent with the homogeneous treatment effect assumption in model (6).

4.3. The Impact of a RTC Statute in MD and IL

Finally, we consider using these bounded variation assumptions to infer crime rates in Maryland (MD) and Illinois (IL). Inferring the impact of RTC in these states presents a unique methodological challenge in that neither state adopted a RTC statute during the observed sample period. Thus, bounded time and DID variation assumptions are not informative in this setting. Invariance assumptions used in the literature, such as the homogeneous treatment effect model (6), identify the ATE in states that have not adopted RTC statutes by assuming that the ATE does not vary across states. One can use results from VA to infer the impact of RTC laws in MD, but this invariance assumption does not seem credible.

Instead, we use a modified interstate variation assumption to provide information on the counterfactual crime rate that would have been realized had a RTC statute been adopted. For MD, the post-1988 crime rates in VA (recall that VA adopted a RTC statute in 1989) are used to bound the counterfactual. In particular, let

$$(12) \qquad Y_{VA,d}(1) \leq Y_{MD,d}(1) \leq Y_{VA,d}(1) + \delta_{(Va,MD)d}.$$

The lower bound reflects the assumption that crime rates in VA are no greater than crime rates in MD and the upper bound follows from the bounded interstate variation assumption.

For IL, the post-1988 crime rate in Indiana (IN adopted a RTC statute in 1981) is used to bound the counterfactual crime rate. In particular, let

$$(13) \quad Y_{IN,d}(1) \leq Y_{IL,d}(1) \leq Y_{IN,d}(1) + \delta_{(IL,IN)d}.$$

The lower bound reflects the assumption that crime rates in IN are no greater than crime rates in IL and the upper bound follows from the bounded interstate variation assumption.

Table 8 displays the estimated bounds for murder, robbery and burglary. Reflecting, in part, the fact that there is only one source of identifying information, most of the bounds are very wide and do not identify the sign of the ATE. However, there are a few exceptions: the estimated effect is negative for murder in MD for most years and for robbery in IL from 1990-1994. It is positive for burglary in IL from 1999-2005.

The mostly inconclusive findings about the effect of RTC laws on crime in MD and IL reflects the fact that these two states did not adopt a RTC statute. Bounded variation assumptions have little identifying power in years when neighboring states have the same realized treatment and when a particular state does not switch treatments across years. In the extreme, when all states have the same policy that does not vary over time, these assumptions are uninformative. Thus, without strong assumptions, it may be difficult to draw precise inferences on treatment effects in states that do not vary treatment over time.

4.4 Discussion of Results

Our findings about the impact of RTC laws on crime are nuanced and not amenable to a simple punch line conclusion. Inferences are sensitive to assumptions. Even under strong assumptions, the results vary across crimes, states, and time. Under a middle ground DID bounded variation assumption, we find that the RTC statute in VA reduces some crimes, increases others, and has ambiguous sign effects on others. In MD and IL, we generally cannot identify the sign of the ATE.

When the signs of the ATE are identified for VA, the magnitudes are generally large. For example, using the middle ground DID assumption (see Table 7), RTC laws are estimated to reduce the VA murder rate in 1995 by at least 14% (from 8.8 to 7.6), the rape rate by at least 6%, the robbery rate by at least 37%, and the larceny rate by at least 13%. In contrast, in 2005 RTC laws are estimated to reduce the murder rate by at least 12%, but increase the rape rate by at least 47%, the assault rate by at least 8%, the robbery rate by at least 3%, and the burglary rate by at least 13%.

These findings of heterogeneous effects across years may partially explain why the estimates obtained with homogenous response assumptions like (6) are sensitive to the years of data included in the sample. Models estimated using more recent data tend to find that RTC laws have negligible or even positive effects on violent crime, whereas the same models estimated using data through the early 1990s tend to find that RTC laws decrease violent crime rates. See Aneja *et al.*, (2011) and the National Research Council (2005, Chapter 6, Tables 6-5 and 6-6).

Given that the estimated effects vary over time and across crimes, and in many cases do not reveal the sign of the ATE, it is difficult to provide a simple assessment of the overall efficacy of the RTC law in VA. One's perspective will necessarily depend both on the assumption that seems most plausible, if any, as well as how to weigh results that vary over time and across crimes. Under the weaker assumptions, the estimated bounds generally do not identify the sign of the ATE and thus provide little guidance about whether RTC laws increase or decrease crime. Under the stronger DID invariance assumption (Table 6), RTC laws are found to have reduced crime in the early 1990s and have mixed effects a decade later, in the 2000s. Similar but less definitive results, are found under the middle ground assumption used to generate the results displayed in Table 7.

To assess the overall benefit of a RTC statute, one must somehow aggregate the effects across the different crimes. Obviously, the decision of how to weight the effects for different crimes is a complex and subjective undertaking. A simple metric might be to count the aggregate change in crimes. In this case, using the results from Table 7, one would conclude that RTC laws tended to decrease the number of crimes in the 1990s but increase the total number of crimes in the 2000s.

Instead, one might adjust the raw counts for the fact that some crimes are more costly than others. A number of researchers have estimated the average costs of different crimes.[10] By combining the estimated effects of RTC laws on crime rates with a set of costs estimates, one can compute how the average costs of crime would change if a RTC statute were to be adopted in VA. While admittedly a tenuous exercise, this might provide a rough snapshot of the aggregate impact of RTC laws on crime.

[10] As might be expected, there is a great deal of variation in these cost of crime estimates, but they all imply that murder is many times more costly than any other crime. For example, McCollister et al. (2010) estimate that the average costs (in 2008 dollars) of a murder is nearly $9 million, whereas the average costs of a rape, the next most costly offense, is $241,000.

A final complication in evaluating the impact of RTC laws arises because the results reported are retrospective, not prospective. The two forms of analysis differ in their objectives. Researchers performing retrospective analysis aim to learn past treatment effects in a study population, asking questions such as: What do we know about the effects of RTC laws in Virginia from 1990 to 2006? A central objective of prospective analysis is to inform treatment choice in a future population, asking questions of the form: What would happen if a state were to adopt a RTC statue in 2016?

Prospective analysis is more difficult than retrospective analysis. Empirical evidence on treatment response is entirely absent prospectively, but it is partially present retrospectively after the outcomes of realized treatments are observed. Under a time invariance assumption, as applied in Assumption (6), the answer is straightforward. The impact of RTC laws does not vary over time. Yet, this assumption is not tenable. One must address the forecasting problem inherent in prospective inference.

In this paper, we have not considered prospective analysis. The literature on RTC laws, including this paper, may provide policymakers useful retrospective information. It is not, per se, informative about prospective questions, and in particular about what would happen if a RTC statute were to be adopted in a state that does not currently have such a law.

5. CONCLUSION: INCREDIBLE CERTITUDE

Given that research on RTC laws is often inconclusive, contradictory, and confusing, the research community has been largely marginalized in this important policy debate. Why?

Researchers are rewarded for producing simple findings leading to definitive policy prescriptions – e.g., more guns leads to less crime – yet generating such findings requires strong assumptions that cannot be persuasively defended. In this setting, researchers report findings with "incredible certitude" rather than expressing due caution (Manski, 2013). Drawing inferences about the effects of RTC laws, or guns policy more generally, is an inherently difficult undertaking: conclusions are highly sensitive to the data and assumptions, the available data are limited, and a wide range of assumptions, and thus conclusions, are consistent with the data. Researchers combining data with different maintained assumptions reach different logically valid conclusions, yet fail to adequately express sensitivity of the findings to untestable assumptions.

For empirical research on complex policies such as gun laws, and RTC laws in particular, to be informative to the policy debate, we believe it is critical that the discussion move away from this paradigm of incredible certitude towards an honest portrayal of partial knowledge (Manski, 2013). Adequate expression of caution goes beyond using temperate language, replicating results under marginally different assumptions, or reporting confidence intervals. Although helpful, these means of expressing caution do not go nearly far enough. Drawing inferences under a variety of assumptions that are not credible does not resolve the problem. Adequate expression of caution requires formal methods to perform empirical research under assumptions that are weak enough to be credible.

In this paper, we develop and apply one such set of assumptions, namely bounded variation assumptions. These assumptions, which relax the traditional invariance assumptions applied in the literature, provide an intuitive, simple, and flexible way to improve the credibility of empirical research, assess the sensitivity of inferences to different identifying restrictions, and apply middle ground assumptions that sometimes identify the sign of the ATE.

The results reveal the inherent tradeoff between the strength of assumptions and findings. Even under the strongest invariance models, the ATE of a RTC statute varies across crimes, states, and time. Estimates found under the more credible bounded variation models suggest even greater degrees of ambiguity with many results not identifying the sign of the ATE, and others varying over time and across states. Certainly, our results are inconsistent with the notion that RTC laws uniformly increase or decrease crime. In this light, we do not report findings with incredible certitude: there are no simple conclusions. Still, our findings may inform the policy debate by providing credible albeit ambiguous information and by constraining the resulting discussion to lie within a set of credible bounds.

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Figure 1A: Murder Rate by Year and State

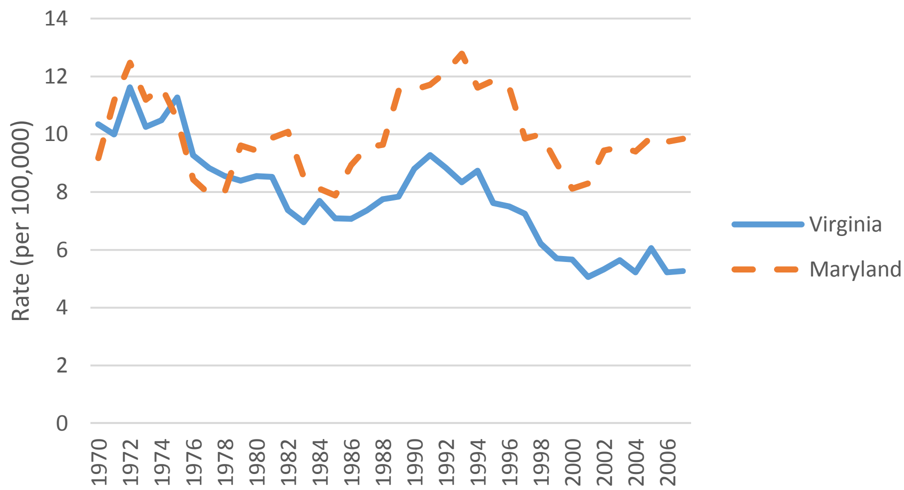

Figure 1B: Robbery Rate by Year and State

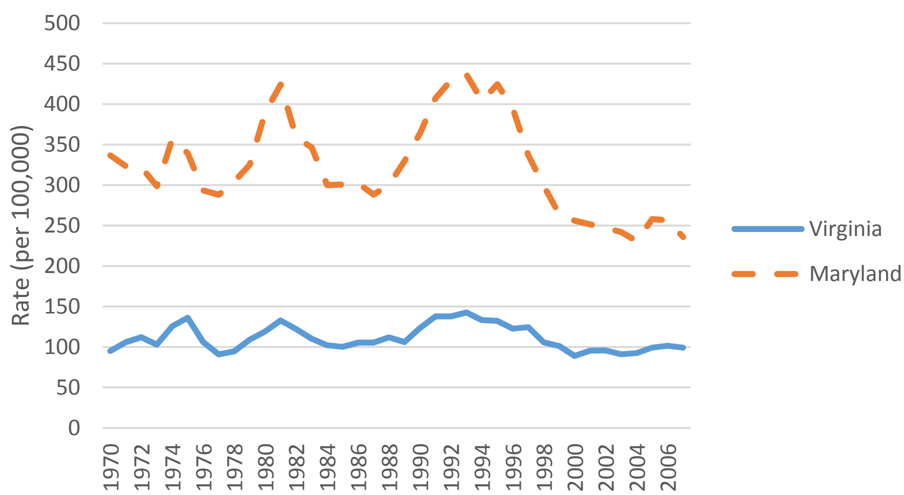

Figure 2: Bounds on the ATE as a Function of δ

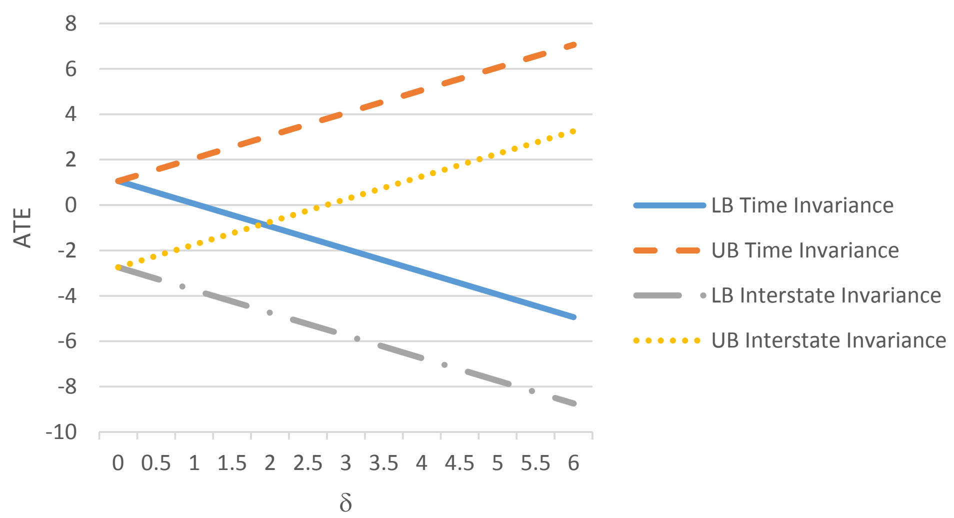

Table 1: Murder Rates per 100,000 Residents by Year and State

Year	Maryland	Virginia
1988	9.63	7.75
1990	11.55	8.81

Table 2A: **Bounds on the Average Treatment Effect Given Bounded Interstate and Time Variation Assumptions**
Virginia, 1990

	δ_(jk)d																									
	0		0.5		1		1.5		2		2.5		3		3.5		4		4.5		5		5.5		6	
δ_j(de)	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB
0	0.8	-3.2	0.8	-2.7	0.8	-2.2	0.8	-1.7	0.8	-1.2	0.8	-0.7	0.8	-0.2	0.8	0.3	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8
0.5	0.3	-3.2	0.3	-2.7	0.3	-2.2	0.3	-1.7	0.3	-1.2	0.3	-0.7	0.3	-0.2	0.3	0.3	0.3	0.8	0.3	1.3	0.3	1.3	0.3	1.3	0.3	1.3
1	-0.2	-3.2	-0.2	-2.7	-0.2	-2.2	-0.2	-1.7	-0.2	-1.2	-0.2	-0.7	-0.2	-0.2	-0.2	0.3	-0.2	0.8	-0.2	1.3	-0.2	1.8	-0.2	1.8	-0.2	1.8
1.5	-0.7	-3.2	-0.7	-2.7	-0.7	-2.2	-0.7	-1.7	-0.7	-1.2	-0.7	-0.7	-0.7	-0.2	-0.7	0.3	-0.7	0.8	-0.7	1.3	-0.7	1.8	-0.7	2.3	-0.7	2.3
2	-1.2	-3.2	-1.2	-2.7	-1.2	-2.2	-1.2	-1.7	-1.2	-1.2	-1.2	-0.7	-1.2	-0.2	-1.2	0.3	-1.2	0.8	-1.2	1.3	-1.2	1.8	-1.2	2.3	-1.2	2.8
2.5	-1.7	-3.2	-1.7	-2.7	-1.7	-2.2	-1.7	-1.7	-1.7	-1.2	-1.7	-0.7	-1.7	-0.2	-1.7	0.3	-1.7	0.8	-1.7	1.3	-1.7	1.8	-1.7	2.3	-1.7	2.8
3	-2.2	-3.2	-2.2	-2.7	-2.2	-2.2	-2.2	-1.7	-2.2	-1.2	-2.2	-0.7	-2.2	-0.2	-2.2	0.3	-2.2	0.8	-2.2	1.3	-2.2	1.8	-2.2	2.3	-2.2	2.8
3.5	-2.7	-3.2	-2.7	-2.7	-2.7	-2.2	-2.7	-1.7	-2.7	-1.2	-2.7	-0.7	-2.7	-0.2	-2.7	0.3	-2.7	0.8	-2.7	1.3	-2.7	1.8	-2.7	2.3	-2.7	2.8
4	-3.2	-3.2	-3.2	-2.7	-3.2	-2.2	-3.2	-1.7	-3.2	-1.2	-3.2	-0.7	-3.2	-0.2	-3.2	0.3	-3.2	0.8	-3.2	1.3	-3.2	1.8	-3.2	2.3	-3.2	2.8
4.5	-3.2	-3.2	-3.7	-2.7	-3.7	-2.2	-3.7	-1.7	-3.7	-1.2	-3.7	-0.7	-3.7	-0.2	-3.7	0.3	-3.7	0.8	-3.7	1.3	-3.7	1.8	-3.7	2.3	-3.7	2.8
5	-3.2	-3.2	-3.7	-2.7	-4.2	-2.2	-4.2	-1.7	-4.2	-1.2	-4.2	-0.7	-4.2	-0.2	-4.2	0.3	-4.2	0.8	-4.2	1.3	-4.2	1.8	-4.2	2.3	-4.2	2.8
5.5	-3.2	-3.2	-3.7	-2.7	-4.2	-2.2	-4.7	-1.7	-4.7	-1.2	-4.7	-0.7	-4.7	-0.2	-4.7	0.3	-4.7	0.8	-4.7	1.3	-4.7	1.8	-4.7	2.3	-4.7	2.8
6	-3.2	-3.2	-3.7	-2.7	-4.2	-2.2	-4.7	-1.7	-5.2	-1.2	-5.2	-0.7	-5.2	-0.2	-5.2	0.3	-5.2	0.8	-5.2	1.3	-5.2	1.8	-5.2	2.3	-5.2	2.8

Table 2B: **Bounds on the Average Treatment Effect Given Bounded DnD and Time Variation Assumptions**

Virginia, 1990

	δ_(jk)(de)																									
	0		0.5		1		1.5		2		2.5		3		3.5		4		4.5		5		5.5		6	
δ_j(de)	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB
0	0.8	-1.2	0.8	-0.7	0.8	-0.2	0.8	0.3	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8
0.5	0.3	-1.2	0.3	-0.7	0.3	-0.2	0.3	0.3	0.3	0.8	0.3	1.3	0.3	1.3	0.3	1.3	0.3	1.3	0.3	1.3	0.3	1.3	0.3	1.3	0.3	1.3
1	-0.2	-1.2	-0.2	-0.7	-0.2	-0.2	-0.2	0.3	-0.2	0.8	-0.2	1.3	-0.2	1.8	-0.2	1.8	-0.2	1.8	-0.2	1.8	-0.2	1.8	-0.2	1.8	-0.2	1.8
1.5	-0.7	-1.2	-0.7	-0.7	-0.7	-0.2	-0.7	0.3	-0.7	0.8	-0.7	1.3	-0.7	1.8	-0.7	2.3	-0.7	2.3	-0.7	2.3	-0.7	2.3	-0.7	2.3	-0.7	2.3
2	-1.2	-1.2	-1.2	-0.7	-1.2	-0.2	-1.2	0.3	-1.2	0.8	-1.2	1.3	-1.2	1.8	-1.2	2.3	-1.2	2.8	-1.2	2.8	-1.2	2.8	-1.2	2.8	-1.2	2.8
2.5	-1.2	-1.2	-1.7	-0.7	-1.7	-0.2	-1.7	0.3	-1.7	0.8	-1.7	1.3	-1.7	1.8	-1.7	2.3	-1.7	2.8	-1.7	3.3	-1.7	3.3	-1.7	3.3	-1.7	3.3
3	-1.2	-1.2	-1.7	-0.7	-2.2	-0.2	-2.2	0.3	-2.2	0.8	-2.2	1.3	-2.2	1.8	-2.2	2.3	-2.2	2.8	-2.2	3.3	-2.2	3.8	-2.2	3.8	-2.2	3.8
3.5	-1.2	-1.2	-1.7	-0.7	-2.2	-0.2	-2.7	0.3	-2.7	0.8	-2.7	1.3	-2.7	1.8	-2.7	2.3	-2.7	2.8	-2.7	3.3	-2.7	3.8	-2.7	4.3	-2.7	4.3
4	-1.2	-1.2	-1.7	-0.7	-2.2	-0.2	-2.7	0.3	-3.2	0.8	-3.2	1.3	-3.2	1.8	-3.2	2.3	-3.2	2.8	-3.2	3.3	-3.2	3.8	-3.2	4.3	-3.2	4.8
4.5	-1.2	-1.2	-1.7	-0.7	-2.2	-0.2	-2.7	0.3	-3.2	0.8	-3.7	1.3	-3.7	1.8	-3.7	2.3	-3.7	2.8	-3.7	3.3	-3.7	3.8	-3.7	4.3	-3.7	4.8
5	-1.2	-1.2	-1.7	-0.7	-2.2	-0.2	-2.7	0.3	-3.2	0.8	-3.7	1.3	-4.2	1.8	-4.2	2.3	-4.2	2.8	-4.2	3.3	-4.2	3.8	-4.2	4.3	-4.2	4.8
5.5	-1.2	-1.2	-1.7	-0.7	-2.2	-0.2	-2.7	0.3	-3.2	0.8	-3.7	1.3	-4.2	1.8	-4.7	2.3	-4.7	2.8	-4.7	3.3	-4.7	3.8	-4.7	4.3	-4.7	4.8
6	-1.2	-1.2	-1.7	-0.7	-2.2	-0.2	-2.7	0.3	-3.2	0.8	-3.7	1.3	-4.2	1.8	-4.7	2.3	-5.2	2.8	-5.2	3.3	-5.2	3.8	-5.2	4.3	-5.2	4.8

Note: To simply the presentation of Tables 2a and 2b, the murder rates reported in Table 1 are rounded to the nearest whole number (except for the 1990 murder rate in VA). Estimates highlighted in grey are infeasible, estimates in pink (bold) identify the sign to be negative, and estimates in green (italics) are identified to be positive (dark green are point identified, light green are partially identified).

Table 3: Murder Rates per 100,000 Residents by Year and State

Year	Maryland	Virginia
1988	9.63	7.75
1987	9.55	7.36

Table 4: Bounded Variation Parameters

	Interstate	Time	DID	DID_0.75
Murder	2.7	2.0	2.3	1.2
Rape	17	4	6	3
Aggravated Assault	324	39	60	34
Robbery	292	30	55	27
Auto Theft	371	46	61	43
Burglary	496	206	136	67
Larceny	775	410	175	93

Note: These parameters are found by taking the maximum of the minimum parameter value for the models to be consistent with the data in each year prior to 1989. The DID_0.75 parameter is the 0.75 quantile.

Table 5: Bounds on the ATE of a RTC Law in VA Given the Joint Interstate and Time Bounded Variation Model, by Year and Crime

	Murder		Rape		Assault		Robbery		Auto Theft		Burglary		Larceny	
	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB
1990	**-0.9**	**0.0**	-0.2	2.2	-1.8	13.1	-18.5	41.4	**-18.5**	**-10.9**	-287.5	107.0	-52.8	701.8
1991	-0.5	0.3	-1.3	0.9	*7.1*	*28.1*	-4.1	21.8	-7.2	-22.1	-235.0	121.0	-35.4	521.0
1992	**-0.9**	**-0.6**	*0.3*	*1.9*	7.2	7.0	**-4.1**	**-0.2**	-40.6	-51.4	-310.0	72.1	-242.9	310.3
1993	-1.4	-1.7	*1.0*	*5.0*	*1.3*	*5.5*	0.8	-2.5	**-59.2**	**-29.3**	-348.0	28.5	-347.6	268.7
1994	**-1.0**	**-0.2**	-2.6	4.7	-1.3	16.3	-8.6	20.6	-66.1	-115.8	-378.1	33.1	-370.6	171.2
1995	**-2.1**	**-1.5**	-3.9	1.8	*6.1*	*7.5*	**-9.7**	**-1.1**	-52.6	-56.3	**-421.7**	**-10.6**	**-402.1**	**-24.1**
1996	**-2.3**	**-1.4**	-4.4	4.2	-4.5	18.1	-19.0	19.9	**-66.7**	**-63.8**	**-406.3**	**-18.3**	-385.0	101.7
1997	-2.5	0.1	-4.2	4.5	-2.8	45.6	-17.2	42.7	-65.3	27.4	**-369.4**	**-35.8**	-469.5	189.8
1998	**-3.5**	**-1.1**	-4.5	4.1	-2.1	55.9	-36.1	23.8	-75.5	17.1	**-362.7**	**-46.1**	-595.6	175.4
1999	**-3.3**	**-0.6**	-5.0	2.5	-6.5	65.7	-40.7	19.3	-84.7	8.0	**-364.3**	**-135.7**	**-521.7**	**-2.2**
2000	**-2.5**	**-0.1**	-6.3	0.3	**-25.0**	**-5.5**	-52.8	7.1	**-94.2**	**-1.6**	**-314.4**	**-177.3**	**-680.8**	**-263.9**
2001	**-3.2**	**-0.7**	-2.3	2.1	**-23.2**	**-6.5**	-46.3	13.6	-83.8	8.8	**-333.6**	**-167.7**	**-530.8**	**-144.4**
2002	**-4.1**	**-1.4**	*0.0*	*2.7*	**-23.6**	**-2.3**	-46.1	13.8	**-92.1**	**-1.8**	**-295.7**	**-171.1**	**-473.9**	**-165.1**
2003	**-3.9**	**-1.2**	-0.2	2.0	-32.6	44.6	-50.7	9.3	**-102.0**	**-48.2**	**-308.4**	**-212.4**	**-364.9**	**-247.9**
2004	**-4.2**	**-1.5**	*0.5*	*1.8*	-35.2	38.0	-49.3	10.6	**-112.0**	**-43.6**	**-277.5**	**-221.9**	**-280.2**	**-263.4**
2005	**-3.7**	**-1.1**	*0.6*	*0.8*	-34.3	42.9	-42.6	17.4	**-134.3**	**-41.6**	**-249.9**	**-212.6**	-260.1	-283.3
2006	**-4.5**	**-1.8**	2.4	0.9	-37.2	40.0	-40.3	19.6	**-152.0**	**-59.3**	**-250.9**	**-189.5**	-408.4	-461.3

Note: Estimates highlighted in grey are infeasible, estimates in pink (bold) identify the sign to be negative, and estimates in green (italics) are identified to be positive.

Table 6: The ATE of a RTC Law in VA Given the DID Invariance Model, by Year and Crime Type

	Murder	Rape	Assault	Robbery	Auto Theft	Burglary	Larceny
1990	**-0.9**	**-4.7**	**-3.9**	**-52.0**	**-10.9**	**-26.9**	*246.3*
1991	**-0.6**	**-5.9**	*11.1*	**-81.1**	**-22.1**	**-12.9**	*45.4*
1992	**-1.4**	**-4.9**	**-10.0**	**-103.1**	**-51.4**	**-61.8**	**-165.3**
1993	**-2.6**	**-1.9**	**-11.5**	**-105.4**	**-29.3**	**-105.4**	**-206.9**
1994	**-1.0**	**-2.2**	**-0.7**	**-82.3**	**-115.8**	**-45.2**	**-304.4**
1995	**-2.4**	**-5.1**	**-9.5**	**-104.0**	**-56.3**	**-102.4**	**-499.7**
1996	**-2.2**	**-0.8**	*1.1*	**-83.0**	**-63.8**	**-44.0**	**-373.8**
1997	**-0.7**	*1.5*	*28.6*	**-23.7**	*49.0*	**-7.1**	**-285.8**
1998	**-1.9**	*3.3*	*38.9*	**-4.7**	*91.0*	**-0.5**	**-296.4**
1999	**-1.4**	*5.1*	*48.6*	*26.1*	*139.7*	**-2.0**	**-222.5**
2000	**-0.6**	*3.8*	**-22.5**	*21.6*	*82.7*	*47.9*	**-381.7**
2001	**-1.3**	*7.7*	**-23.5**	*32.4*	*36.8*	*28.7*	**-231.6**
2002	**-2.2**	*10.1*	**-19.4**	*37.3*	**-1.8**	*66.6*	**-174.8**
2003	**-2.0**	*9.9*	*33.9*	*37.6*	**-48.2**	*53.9*	**-65.8**
2004	**-2.3**	*10.6*	*20.9*	*50.4*	**-43.6**	*84.8*	*18.9*
2005	**-2.0**	*10.7*	*45.8*	*29.8*	**-29.2**	*112.4*	*39.1*
2006	**-2.6**	*12.5*	*65.8*	*33.5*	*19.6*	*111.3*	**-109.3**

Note: Estimates highlighted in grey are infeasible, estimates in pink (bold) identify the sign to be negative, and estimates in green (italics) are identified to be positive.

Table 7: Bounds on the ATE of a RTC Law in VA Given the Joint DID, Interstate and Time Bounded Variation Model, by Year and Crime Type

	Murder		Rape		Assault		Robbery		Auto Theft		Burglary		Larceny	
	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB
1990	-2.0	0.3	**-7.9**	**-1.4**	-38.0	30.2	**-79.2**	**-24.7**	-53.6	27.8	**-94.3**	**-81.9**	*153.2*	*291.6*
1991	-1.7	0.6	**-9.2**	**-2.7**	-23.0	45.2	**-108.3**	**-53.8**	-64.9	20.6	**-83.4**	**-29.5**	-47.8	138.6
1992	**-2.6**	**-0.3**	**-8.2**	**-1.7**	-44.1	24.1	**-130.4**	**-75.9**	**-94.1**	**-8.7**	**-132.4**	**-104.5**	**-258.5**	**-72.1**
1993	**-3.7**	**-1.4**	-5.2	1.4	-45.6	22.6	**-132.7**	**-78.2**	**-72.0**	**-12.9**	**-175.9**	**-142.4**	**-300.1**	**-113.7**
1994	-2.1	0.2	-5.4	1.1	-34.8	33.3	**-109.6**	**-55.1**	**-158.6**	**-73.1**	-115.7	-172.5	**-397.6**	**-211.3**
1995	**-3.5**	**-1.2**	**-8.3**	**-1.8**	-43.6	24.5	**-131.2**	**-76.7**	**-99.0**	**-13.6**	-173.0	-216.2	**-592.9**	**-406.5**
1996	**-3.4**	**-1.1**	**-4.1**	**-0.1**	-33.0	34.0	**-110.2**	**-55.7**	**-106.5**	**-21.0**	-114.6	-223.9	**-467.0**	**-280.7**
1997	-1.9	0.4	-1.8	4.7	-5.4	62.7	-51.0	3.6	*6.2*	*91.7*	-74.5	60.3	**-379.0**	**-192.6**
1998	**-3.0**	**-0.7**	*0.1*	*6.6*	*4.8*	*73.0*	-32.0	22.5	*48.2*	*133.7*	-71.0	67.0	**-389.6**	**-203.3**
1999	**-2.0**	**-0.2**	*1.9*	*8.4*	*32.1*	*82.7*	-1.2	53.3	*97.0*	*182.5*	-69.5	65.4	**-315.7**	**-129.4**
2000	-1.7	0.6	*0.5*	*7.0*	13.5	11.6	-5.7	48.8	*40.0*	*125.5*	-19.6	115.3	**-474.8**	**-288.5**
2001	**-2.5**	**-0.2**	*4.5*	*11.0*	15.4	10.6	*5.2*	*59.7*	-6.0	79.5	-38.7	96.1	**-324.8**	**-138.5**
2002	**-2.4**	**-1.1**	*6.9*	*13.4*	15.0	14.7	*10.1*	*64.6*	-44.5	41.0	-0.8	134.0	**-267.9**	**-81.6**
2003	**-2.1**	**-0.9**	*6.6*	*13.1*	*6.0*	*68.0*	*10.4*	*64.9*	**-55.7**	**-5.4**	-13.6	121.3	-158.9	27.4
2004	**-2.5**	**-1.1**	*7.4*	*13.9*	*3.3*	*55.0*	*23.2*	*77.7*	**-65.7**	**-0.8**	*17.4*	*152.2*	-74.2	112.1
2005	**-1.7**	**-0.8**	*7.4*	*14.0*	*11.7*	*79.9*	*2.6*	*57.1*	-71.9	13.6	*45.0*	*179.8*	-54.1	132.2
2006	**-2.5**	**-1.5**	*9.2*	*15.8*	*31.8*	*99.9*	*6.2*	*60.7*	-23.1	62.3	*43.9*	*178.7*	*-202.4*	*-16.1*

Note: Estimates highlighted in grey are infeasible, estimates in pink (bold) identify the sign to be negative, and estimates in green (italics) are identified to be positive.

Table 8: Bounds on the ATE of a RTC Law in MD and IL given the Interstate Bounded Variation Assumption, by Crime and Year

	Maryland						Illinois					
	Murder		Robbery		Burglary		Murder		Robbery		Burglary	
	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB	LB	UB
1990	**-2.7**	**0.0**	-240.6	83.0	-389.2	107.0	-4.2	0.6	**-293.7**	**-69.8**	-122.6	142.6
1991	-2.4	0.3	-269.7	53.9	-375.1	121.0	-3.8	1.0	**-341.7**	**-117.8**	-145.7	119.4
1992	**-3.3**	**-0.6**	-291.8	31.8	-424.1	72.1	-3.2	1.6	**-291.0**	**-67.1**	-125.9	139.3
1993	**-4.4**	**-1.7**	-294.1	29.5	-467.6	28.5	-3.8	0.9	**-261.0**	**-37.1**	-161.6	103.6
1994	**-2.9**	**-0.2**	-271.0	52.6	-407.4	88.7	-3.8	1.0	**-240.7**	**-16.8**	-149.5	115.6
1995	**-4.2**	**-1.5**	-292.6	31.0	-464.7	31.5	-2.2	2.5	-193.5	30.4	-89.1	176.1
1996	**-4.1**	**-1.4**	-271.6	51.9	-406.3	89.9	-2.6	2.1	-151.6	72.2	-116.9	148.3
1997	-2.6	0.1	-212.4	111.2	-369.4	126.7	-1.8	2.9	-143.8	80.1	-42.4	222.8
1998	**-3.8**	**-1.1**	-193.4	130.2	-362.7	133.4	-0.6	4.1	-136.3	87.6	-20.2	245.0
1999	**-3.3**	**-0.6**	-162.6	161.0	-364.3	131.8	-1.2	3.6	-108.6	115.3	*1.2*	*266.3*
2000	-2.5	0.3	-167.1	156.5	-314.4	181.7	-1.5	3.3	-103.5	120.4	*16.0*	*281.2*
2001	**-3.2**	**-0.5**	-156.2	167.3	-333.6	162.6	-1.1	3.6	-81.9	142.0	*67.3*	*332.5*
2002	**-4.1**	**-1.4**	-151.3	172.3	-295.7	200.5	-1.7	3.0	-93.4	130.5	*46.3*	*311.5*
2003	**-3.9**	**-1.2**	-151.0	172.5	-308.4	187.7	-1.6	3.1	-85.3	138.6	*50.6*	*315.8*
2004	**-4.2**	**-1.5**	-138.2	185.4	-277.5	218.7	-1.1	3.7	-75.3	148.6	*78.9*	*344.1*
2005	**-3.8**	**-1.1**	-158.8	164.8	-249.9	246.3	-0.4	4.4	-74.3	149.6	*88.0*	*353.2*
2006	**-4.5**	**-1.8**	-155.2	168.4	-250.9	245.2	-0.2	4.5	-70.9	153.0	*128.9*	*394.1*

Note: For Maryland, the comparison state is Virginia. For Illinois, the comparison state is Indiana. Estimates highlighted in grey are infeasible, estimates in pink (bold) identify the sign to be negative, and estimates in green (italics) are identified to be positive.