

# GUNS, CRIME, AND THE IMPACT OF STATE RIGHT-TO-CARRY LAWS

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## INTRODUCTION

Since the United States is a large country with nearly 200 million guns in circulation,<sup>1</sup> there is plenty of, shall we say, ammunition for those seeking anecdotal evidence to support their views about guns. These views are largely polarized between those who believe that guns are primarily used to protect law-abiding citizens and those who believe that guns mainly serve to exacerbate lethal violence—both intentional and accidental—and encourage more frequent suicides. Every day is likely to produce some gripping anecdote about how a gun can be used defensively to thwart criminals, as well as some wrenching account reflecting the frightening misuse of guns. A major goal of scholarly research in this area is to move beyond mere tendentious recitation of anecdotes to establish the true costs and benefits of guns.

Those who have followed the troubling tales of alleged misconduct in academic research by gun researchers Michael Bellesiles (from the left)<sup>2</sup> and John Lott (from the right)<sup>3</sup> may draw the conclusion that in America the topic of guns is so ideologically charged that no researcher can be believed. While Gresham's Law may be operating in the public's attitude toward gun research, this is unfortunate because there are indeed some very serious researchers in this domain, of whom Phil Cook and Jens Ludwig are among the most important. It would be a sad fact if the cloud that Bellesiles and Lott have cast over all gun research limited the influence of Cook and Ludwig's careful and measured scholarly work.

For those who missed the contretemps, the left-leaning historian Bellesiles wrote a book suggesting that the gun culture was far less

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1. Philip J. Cook & Jens Ludwig, *Guns in America: Results of a Comprehensive Survey of Gun Ownership and Use* (1996).

2. Michael A. Bellesiles, *Arming America: The Origins of a National Gun Culture* (2000); *see also infra* note 5.

3. *See infra* note 6 and accompanying text.

prevalent in Colonial America than previously believed.<sup>4</sup> Bellesiles's book received widespread praise in certain circles, but allegations soon surfaced that Bellesiles had manufactured some of his data.<sup>5</sup> After Emory University, where he was a tenured professor, convened a committee to look into the allegations, Bellesiles ended up losing his job (although he continues to insist that at most he was sloppy in his historical work). John Lott, who has championed the view that more guns lead to less crime, was also accused—by one of Bellesiles' major accusers, Northwestern Law Professor James Lindgren, as well as by the eminent sociologist Otis Dudley Duncan—of manufacturing data to support his claim that 98% of the time, merely brandishing the gun was enough to terminate a violent attack.<sup>6</sup> The point was important because critics of the “more guns, less crime hypothesis” noted that if defensive gun use were as prevalent as Lott has claimed—he accepts claims that there are roughly 2.5 million defensive gun uses per year—one would expect that there would be far more dead criminals lying around than the relatively meager number (less than 170 per year) that the federal government identifies in the Uniform Crime Reports.<sup>7</sup> Cook and Ludwig find that a more reasonable estimate is “that there are about 100,000 instances per year in which someone uses a gun to defend against an assault or break-in.”<sup>8</sup>

Interestingly, while Bellesiles has been severely penalized, Lott has avoided any negative repercussions, perhaps because his employer, the American Enterprise Institute, has steadfastly refused to accede to suggestions—such as that made by the editor of the prestigious journal *Science*—that it impanel a committee of scholars to investigate Lott's behavior.<sup>9</sup> Indeed, if Lott were an academic, one would assume that his school would follow Emory's lead and convene an investigative committee to sort through the charges of academic misconduct and evaluate Lott's denials that he manufactured the

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4. Bellesiles, *supra* note 2.

5. See Stanley N. Katz et al., Report of the Investigative Committee in the Matter of Professor Michael Bellesiles (July 10, 2002), available at [http://www.emory.edu/central/NEWS/Releases/Final\\_Report.pdf](http://www.emory.edu/central/NEWS/Releases/Final_Report.pdf). The report was prepared by an independent committee appointed by Emory University, which includes a summary of, and investigation into, the major criticisms. *Id.*

6. The details of the charges against Lott raised by Lindgren and Duncan are recounted in exhaustive detail in a web page by the highly talented Australian professor Tim Lambert. Tim Lambert, John Lott's Unethical Conduct, at <http://www.cse.unsw.edu.au/~lambert/guns/lott98update.html> (last modified Sept. 20, 2004). Lambert also notes that Lott has repeatedly generated estimates based on data sets flawed by coding errors and refused to acknowledge these problems when the errors were brought to his attention. *Id.*

7. FBI, Crime in the United States 2002: Uniform Crime Reports 28. Justifiable homicide by private citizens using all types of firearms averaged almost 170 during that period. *Id.*

8. Philip J. Cook and Jens Ludwig, *Principles for Effective Gun Policy*, 73 *Fordham L. Rev.* 589 (2004).

9. Donald Kennedy, *Research Fraud and Public Policy*, 300 *Science* 393 (2003).

survey allegedly supporting the 98% brandishing figure. The episode may suggest that the opinions of those laboring at institutions that are unwilling to enforce the highest standards of scholarly integrity should trade at somewhat of a discount.<sup>10</sup>

But whether or not some of Lott's questionable practices in support of his "more guns, less crime" hypothesis are unethical,<sup>11</sup> he has still raised a serious academic question that needs to be resolved: What is the effect of a state's adoption of so called "right to carry" ("RTC") laws?<sup>12</sup> While I completely agree with Cook and Ludwig that "the best empirical evidence does not support"<sup>13</sup> the "more guns, less crime hypothesis," I thought it might be useful to highlight some of the issues in this debate and provide some new evidence that further strengthens the Cook and Ludwig conclusion. I therefore begin with a few comments that illuminate some of the key theoretical points involving an evaluation of RTC laws, and then demonstrate the fragility of some of the econometric models used to support Lott's thesis, before offering an alternative econometric approach that may yield better estimates of the impact of RTC laws.

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10. As Steven Shapin has written:

[W]e are told things about the world [today] by people whom we do not know, working in places we have not been. Trust is no longer bestowed on familiar individuals; it is accorded to institutions and abstract capacities thought to reside in certain institutions . . . We trust the truth of specialized and esoteric scientific knowledge without knowing the scientists who are the authors of its claims.

Steven Shapin, *A Social History of Truth: Civility and Science in Seventeenth-century England* 411 (1994).

11. Lott also used one or more pseudonyms to anonymously attack his critics and buttress his own arguments and standing—a practice that Kennedy considered to be fraudulent. *See* Kennedy, *supra* note 9, at 393.

12. RTC laws are also called "shall-issue" laws in that they require government officials to issue concealed weapons permits to all applicants of a certain age, who have no felony criminal record and no recognized record of serious mental illness. Indeed, federal law bans the mentally ill from purchasing guns if they have been adjudicated to be mentally deficient or incompetent, or if they have been involuntarily committed to a mental institution. *See* 18 U.S.C. § 922(g) (2000). But the recent case of Charles McCoy, the alleged Ohio highway shooter, shows that even those who are barred by these federal prohibitions from legally obtaining guns can still purchase guns freely in the thirty-three states—including Ohio—"that have no mechanism to enter court records concerning mental illness into the National Instant Check System, the computer system that checks the background of gun purchasers." Steve Stephens, *Mentally Ill Can Pass Background Checks*, *The Columbus Dispatch*, March 17, 2004, available at LEXIS, News Library, Coldis File. Despite the fact that he was a paranoid schizophrenic, McCoy would simply buy another gun when his father would take away his other weapons, such as a 9mm semiautomatic pistol that police tests indicated was used in nine of the twenty-four shootings. Having no felony conviction, McCoy was able to easily buy guns at various retail gun outlets. *Id.*

13. Cook & Ludwig, *supra* note 8, at 595.

## I. SOME INSIGHTS INTO THE THEORY OF THE IMPACT OF RTC LAWS

Increasing the number of guns in circulation could conceivably deter crime (good), shift its incidence from gun toters to non-gun toters (bad), or increase crime, accidental death, and suicide (horrible). John Lott and David Mustard have argued that the first of these—crime deterrence—is the dominant influence of laws allowing citizens to carry concealed handguns.<sup>14</sup> One possible consequence of the carrying of a gun by a potential target of crime (even if illegally carried) is that some crime will be deterred. Any such deterrence is a clear social benefit, unless the carrying (whether legal or illegal) facilitates other criminal conduct on the part of the gun toter (for example, by a drug dealer packing heat for protection). A second possible consequence, though, is that gun carrying may only shift the burden of crime from a target likely to be able to use a gun to someone who does not possess or who will not be in a position to use a gun defensively at the moment of criminal attack. As an initial approximation, this transfer rather than deterrence of criminal victimization is not a social gain, and if one factors in the cost of the gun, it becomes a net social cost. Moreover, it is clear that there are times when having a gun would be helpful for self-defense. But the benefit of having a gun available for the relatively rare times when self-defense is needed comes at a price because the gun is constantly subject to theft, accidental discharge, or the facilitation of suicidal impulses. Even those who happen to resort to ostensibly legitimate defensive gun use may find that (1) they are mistaken (for example, the terrible killing of the Japanese exchange student in Baton Rouge for which the gun-toting homeowner was found civilly liable)<sup>15</sup> or (2) they are overwhelmed by their attacker in a way that makes the possession of the gun contribute to one's loss from victimization rather than reduce or avoid it.

An analysis of two horrible crimes in Parts I.A and I.B—the Dartmouth College murders and the Oklahoma City bombing—reveals how the complexity of the world has a way of defying the simple lessons drawn from anecdotal evidence or predictions about the consequences of either defensive gun use or gun control. Given this complexity, only statistical analysis can hope to ascertain whether the net effects of gun ownership and carrying—and the laws that encourage or discourage such behavior—are positive or negative. Part II will then respond to some of the claims that allowing law abiding citizens to carry guns ensures that only benign consequences will flow from adopting RTC laws, and Part III will evaluate some

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14. John R. Lott, Jr. & David B. Mustard, *Crime, Deterrence, and Right-to-Carry Concealed Handguns*, 26 J. Legal Stud. 1, 26 (1997).

15. Adam Nossiter, *Judge Awards Damages in Japanese Youth's Death*, N.Y. Times, Sept. 16, 1994, at A12.

new econometric evidence concerning the net effect of RTC laws on crime.

A. *Gun Brandishing Saves Lives—Or Does It?*

In 2001, two boys aged sixteen and seventeen killed two popular Dartmouth College professors in Hanover, New Hampshire.<sup>16</sup> Since their assault was with hunting knives, guns cannot be blamed for the murders. Indeed, there is reason to credit guns with saving two lives in the period prior to the Dartmouth killings, since the two boys had previously planned on killing the inhabitants of a cabin in Vermont. But when the boys knocked late one night, the Vermont cabin owner refused to open the door to them and showed them a handgun through a window. No gun was fired, but the mere brandishing of a handgun certainly may have discouraged the two aspiring criminals from trying to force their way into the house (they had already cut the phone lines to the cabin).<sup>17</sup> It is not unreasonable to think that gun ownership by the Vermont cabin owner did save him and his son, which at first sounds like this was a case of crime deterrence.<sup>18</sup>

But the ostensibly beneficial use of guns in this case might not show up in any reduction in crime because the defensive gun use may have saved two specific lives, without reducing the total number of killings. In other words, this may be a case not of crime reduction, but rather a case of crime transfer, since the gain of the two in Vermont was the loss of the two in New Hampshire. Thus, a successful defensive use of a gun through brandishing—as opposed to the killing of an offender—may not reduce the total crime count if the offender just moves on to the next target.

Of course, one might legitimately inquire whether the New Hampshire murders could have been avoided if the Dartmouth professors had had their own guns. Here, one can only speculate, but it seems unlikely guns would have helped the Dartmouth professors since the boys did not approach their New Hampshire house in the way that prompted the Vermont cabin owner to reach for his gun (with sudden, loud door knocks in the middle of the night), but rather

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16. Lawrence J. Scholer, *Zantop Murderers Sentenced: Tulloch Gets Life*, *The Dartmouth Review*, April 9, 2002, available at [http://dartreview.com/archives/2002/04/09/zantop\\_murderers\\_sentenced\\_tulloch\\_gets\\_life.php](http://dartreview.com/archives/2002/04/09/zantop_murderers_sentenced_tulloch_gets_life.php).

17. *See id.*

18. *Id.* All we know is that the cabin owner would have been viciously attacked if he had opened the door to his cabin and that things worked out well for the cabin owner when he showed a gun and refused to open the door. It is also possible that things might have ended happily if he didn't have a gun and had just refused to open the door. Still, without the brandishing, they might have (we will never know for sure) tried to knock his door in, which would certainly have been a criminal violation regardless of the outcome, and the gun was almost certainly a comfort for the cabin owner who had to spend the night in the cabin with his phone lines cut. *Id.*

through the disarming artifice of telling an environmental professor that they were working on an environmental project. This ploy led the professors to invite the boys into their house and converse with them until the boys unexpectedly launched their murderous assault. Even here, a gun *might* have helped save the wife if it had been in the kitchen (or on her person) since the wife was there when her husband was attacked in the living room and she was killed when she ran to the living room after hearing his cries. If her instinct had been to run to her husband first, having a gun somewhere in the house would not have saved either of them. While deterring two murders generally would end up saving lives, in this case, the amateurish criminal conduct of the young killers suggests that they were going to be caught if they succeeded in killing anyone. Consequently, the Vermont cabin owner's gun changed the names but in all likelihood not the number of victims.

*B. An Unpredictable Response to the Federal Assault Weapon Ban*

On September 13, 1994, President Clinton signed the federal assault weapon ban (which incidentally has just lapsed after 10 years).<sup>19</sup> That very day, Timothy McVeigh decided that he must respond violently to this perceived insult to the Second Amendment.<sup>20</sup> Ultimately, McVeigh decided that the appropriate response to Clinton's action was to bomb the federal building in Oklahoma City, which killed 168 people, including nineteen children in a federal day care center. McVeigh was part of the dangerous, extremist wing of the American gun culture that uses the Second Amendment as a rallying cry in its anti-government crusade.<sup>21</sup> Thus, McVeigh, who was moved by the right wing diatribe denouncing the deaths of children at the Branch Davidian compound in Waco, Texas, voiced no remorse over the deaths he caused of the nineteen children in the federal building bombing. The initial irony that a federal gun control measure stimulated 168 murders is further compounded by the fact that state gun control law led to McVeigh's capture. After McVeigh was stopped for driving a car with no license plates (he later claimed that he wanted to be caught so he would be put to death by the federal

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19. Pub. L. No. 103-322, 108 Stat. 1796 (codified in scattered sections of U.S.C.); see also William Douglas & Martin Kasindorf, *Clinton Signs \$30.2 Billion Crime Bill*, Denver Post, Sept. 14, 1994, available at LEXIS, News Library, Dpost File; Rachel L. Swarns, *Clock Ticks on Extension of Gun Ban*, N.Y. Times, July 24, 2004, at A9; see also Tamara Lytle & Henry Pierson Curtis, *Weapons Ban Expires, Assault-Style Guns Will Go Back on Sale*, Orlando Sentinel, Sept. 14, 2004, available at LEXIS, News Library, Orsent File.

20. Frontline provided a detailed historical account of McVeigh's plot. Frontline, PBS, *McVeigh Chronology*, at <http://www.pbs.org/wgbh/pages/frontline/documents/mcveigh/> (last visited Sept. 23, 2004).

21. *Id.*

government), it was his possession of an illegal firearm that led to his detention long enough for federal agents to connect him to the Oklahoma City bombing.<sup>22</sup> If he had lived in a state such as Alaska, which allows anyone to carry a gun without a permit, the police would not have detained him, and one can only speculate as to whether this determined killer would have done more harm before being stopped. The case does illustrate how gun restrictions can at times aid law enforcement efforts by making it easier to identify and apprehend certain bad guys.

## II. HOW CAN SOMETHING GO WRONG IF ONLY LAW-ABIDING CITIZENS CAN GET HANDGUNS?

In November 2000 a forty-three-year-old man went to see the movie *102 Dalmatians* with a Glock pistol in his jacket.<sup>23</sup> When he went to don his jacket at the end of the movie, “something hung on the trigger of his gun, causing it to discharge” according to the police<sup>24</sup> report. Although the moviegoer had a valid Alabama pistol permit, he ended up in critical condition in intensive care when a bullet ripped into his abdomen. When I recounted this anecdote while giving a lecture on RTC laws at Stanford University’s Alumni Weekend, a person in attendance insisted that nothing like that had ever happened. But not surprisingly in a big country with lots of guns, such events are not infrequent since every day of the year someone in America is accidentally shooting himself or someone else (or just missing).<sup>25</sup> Some pro-gun advocates are at times unwilling to recognize that guns can lead to some bad outcomes just as anti-gun advocates have at times been unwilling to recognize some of the beneficial uses.

For example, Herbert Collins, 63, of Montgomery, Texas accidentally shot himself in the ankle when his gun fell, causing the trigger to engage.<sup>26</sup> Collins doesn’t sound like someone who simply didn’t know how to use guns or who was unusually reckless. He was a special adviser to the Kendleton Police Department and a pastor at a church in Montgomery. At a recent college basketball tournament, an off-duty police officer brought new meaning to March Madness when he accidentally shot himself in the buttocks during the Atlantic Coast

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22. *Id.*

23. Jon Anderson, *Man Shot in Theater Wasn’t Holding Pistol*, Birmingham News, Nov. 28, 2000, available at LEXIS, News Library, Birmnw File.

24. *Id.*

25. See Ctr. for Disease Control and Prevention, WISQARS Injury Mortality Reports, 1999-2000, available at <http://webapp.cdc.gov/sasweb/ncipc/mortrate10.html>. According to the CDC, accidental gun deaths in the U.S. are roughly 800 per year, and it is safe to assume that injuries and near misses are many times that number. *Id.*

26. *Accidental Shooting*, The Courier (Texas), March 9, 2004.

Conference quarterfinal game between Maryland and Wake Forest in Greensboro, North Carolina.<sup>27</sup>

Moreover, because guns are awkward and heavy, they are sometimes left behind in dangerous areas such as planes and schools. For example, just this month, an airline employee found a loaded 9mm handgun left on the seat of a Continental Airlines flight after it landed in Newark.<sup>28</sup> The gun had belonged to a Secret Service agent traveling on the flight after declaring his gun possession.<sup>29</sup> When one adds in the roughly 1.5 million guns stolen every year, it is clear that legally possessed guns frequently end up in the hands of criminals and others who were not their intended users.<sup>30</sup>

There can be little doubt that—other things being equal—more carrying of guns contributes to more gun accidents. In a recent book, David Hemenway presents evidence from various studies to argue convincingly that the rate of suicide is elevated by the presence of guns.<sup>31</sup> But not all of the things that go wrong with guns carried by lawful permit holders are mishaps or self-induced injuries. Consider the case of Minnesotan Damian Peterson, who got into a heated argument with his brother eight days after getting a concealed handgun permit: Peterson “fired 11 bullets into a car, continuing to fire as the car drove away through a residential neighborhood.”<sup>32</sup> Similarly, a good Samaritan named Louis Mockewich, who went to the aid of a woman whose car was stuck in a snowbank in Philadelphia during a bad winter storm in January 2000, precipitated a quarrel with a neighbor when he shoveled some snow around another parked car,

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27. *Officer Hit In Buttocks When Own Gun Goes Off During ACC Game*, Associated Press, Mar. 12, 2004.

28. Brock N. Meeks, *Airline Employee Finds Loaded Gun on Plane* (May 3, 2004) at <http://www.msnbc.msn.com/id/4894528>.

29. The news story continued:

“This is not the first time this has happened,” said an administration official while confirming the incident to NBC News. “There have been one or two other occasions where federal agents have left their firearms on board planes,” the administration official said, though he couldn’t name which agencies the officers worked for. The administration official also noted that a federal air marshal recently left his weapon in an airport restroom. Another recent incident involved a commercial airline pilot who was qualified to carry a gun in the cockpit under the new Federal Flight Deck Officer Program. During a shuttle ride from the aircraft the pilot left the gun—which was inside a lockbox—behind on the shuttle. Another pilot recognized the lockbox and retrieved it, according to a federal security employee.

*Id.*

30. George J. Benston, *The Supply of Handguns to Criminals: Evidence from Firearms Tracing Data 9* (2004) (unpublished manuscript, on file with the *Fordham Law Review*).

31. David Hemenway, *Private Guns, Public Health* 35-45 (2004).

32. Luis Tolley, *Concealed and Carried; Proposed Legislation Puts Police and the Public at Risk*, Milwaukee Journal Sentinel, Sept. 20, 2003, available at LEXIS, News Library, Miljnl File.



whose elderly owner complained. When the good Samaritan, who had no criminal record but did have a permit to carry a concealed handgun, yelled at the elderly man, the man's son—a well-liked youth football coach—came out to complain. Mockewich killed him with a single bullet fired into his chest. Mockewich then uttered “self-defense” and walked away from the dying man and his father.<sup>33</sup> Mockewich, an NRA member whose truck had a bumper sticker “Armed with Pride,” was sentenced to thirty years in prison for third-degree murder.<sup>34</sup> “If only the victim had been armed, this might not have happened,” a gun zealot might protest. In fact, though, the victim had just put on his gun as he planned to take his mother to the store. He never got it out of his holster before he was struck down.<sup>35</sup> Clearly, the ability to qualify for a concealed gun permit is no guarantee that the permit holder will never misuse a gun, nor does it give any assurance that the permit holder will be able to protect himself or keep the gun from falling into the wrong hands via accidental loss or theft.

### III. STATISTICAL ANALYSIS OF THE IMPACT OF RTC IS THE WORST FORM OF EVALUATION—EXCEPT FOR ALL THE OTHERS

#### A. *Using the Standard Panel Data Approach*

With so many factors cutting in so many conflicting ways, the issue of the impact of RTC laws on crime is a tricky empirical question that can only be resolved through statistical analysis. Much ink has been spilled in the effort to answer this question using panel data econometric techniques.<sup>36</sup> This statistical approach may be the best hope for ascertaining the impact of the law, but, unfortunately, small changes in the statistical modeling can have large effects on the estimated effects. To illustrate this fact, I present a number of different specifications in Figures 1 to 9, with each Figure estimating the effect on each of the nine FBI Index I crime categories.<sup>37</sup> Because of the powerful ideological motivations of many gun researchers, a legitimate fear is that an analyst trying to prove a certain point might

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33. See Elisa Ung, *Man Faces Trial Over Shooting*, Phila. Inquirer, Feb. 3, 2000, at B1; see also Robert Moran & Thomas J. Gibbons, Jr., *Man Shot, Killed In Snow Dispute*, Phila. Inquirer, Jan. 27, 2000, at A1.

34. Jacqueline Soteropoulos, *Killer in Snow Dispute Gets 30-Year Term*, Phila. Inquirer, Dec. 8, 2000, at B1.

35. See Ung, *supra* note 33.

36. In this Essay, the panel data is data collected for all fifty states over the period 1977-1999. Looking at this combined cross-section and time series data is much more powerful than simply examining a single time series (the before and after effect of a law adopted in one state) or at a single cross-section (looking, say, at all fifty states at only one point in time).

37. See *infra* figs. 1-9.

choose among a vast array of possible statistical models simply to generate a desired result. To address this concern, I report not only a modified version of Lott's original model (called the "Modified Lott" set of explanatory variables),<sup>38</sup> but also the results of three other models that were developed by researchers to answer questions having nothing to do with RTC laws—one by Wentong Zheng (developed to look at the impact of lotteries on crime),<sup>39</sup> one by William Spelman (developed to look at the impact of incarceration on crime),<sup>40</sup> and one by John Donohue and Steve Levitt (developed to look at the impact of abortion legalization on crime).<sup>41</sup> These four different sets of explanatory variables are set forth in Table 1.<sup>42</sup> Whatever infirmities these last three models have, we know that they were created by serious academics without any intention of skewing the estimates of the impact of RTC laws. When we add a variable identifying the date of adoption of the RTC laws to these pre-existing statistical models, we can see if the results support—or refute—the more guns, less crime hypothesis.<sup>43</sup>

Let's start with the one crime—robbery in Figure 4<sup>44</sup>—for which we would expect to see a *reduction* in crime if the "more guns, less crime"

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38. The "Modified Lott" model starts with Lott's original set of explanatory variables and replaces one particularly questionable variable—the arrest rate—with a lagged incarceration rate. The problems with the arrest rate as Lott used it are numerous: the variable is poorly measured and not exogenous (that is, it is not fair to assume that arrest rates influence crime since crime in year *t* will also influence the number of arrests in year *t* divided by the amount of crime in year *t*—thus, the contemporaneous crime rate ends up on both sides of the regression equation).

39. Computer program supplied to author by Wentong Zheng, graduate student in Economics, Stanford University.

40. William Spelman, *The Limited Importance of Prison Expansion, in The Crime Drop in America* 97 (Alfred Blumstein & Joel Wallman eds., 2000).

41. John J. Donohue and Steven D. Levitt, *The Impact of Legalized Abortion on Crime*, 116 Q. J. of Economics 379 (2001).

42. All of the estimates presented in this Essay are based on state crime data, which is the approach used in the work by Zheng, Spelman, and Donohue & Levitt. While Lott does present estimates of the impact of RTC laws using state data, he prefers to use county data. I have been persuaded, though, that the county crime data is considerably less accurate than the state data, and since the intervention of interest—the adoption of a state RTC law—applies at the state level, I am more comfortable with the state data than with the county data.

43. Interestingly, one might think that identifying the date of adoption of an RTC law would be easy, but even on this issue there is disagreement. It is not always straightforward to identify the precise date among a number of competing statutory enactments, when citizens of a state had a right to carry concealed handguns without demonstrating the need for a gun to a governmental official. Table 2 presents a number of different codings of RTC adoptions, and one can see that there are a number of differences across the various authors. *See infra* tbl. 2. This Essay will rely on my latest coding scheme identified in the fourth column of Table 2. *Id.* Luckily, since most of the disputed dates are in small states whose impact in a population-weighted regression will necessarily be smaller, the coding issues do not seem to influence the results too strongly. Nonetheless, the coding issue illustrates how even simple issues become complex and disputed in empirical work.

44. *See infra* fig. 4.

hypothesis were true. The dummy model is the simplest statistical model, which simply tries to determine whether on average, and controlling for the various explanatory variables of Table 1, crime was higher or lower after RTC adoption. These before and after estimates can be generated in either of two ways: as a single estimate for the aggregate of all adopting states, or as multiple individual estimates for each adopting state (which can then also be averaged). Beginning with this dummy model and using the Modified Lott set of explanatory variables that are set forth in column 1 of Table 1, we see four columns on the far left of Figure 4. The first thick column in Figure 4 shows that four more states had positive estimates—implying that the RTC law *increased* crime—than had negative estimates (suggesting crime decreases).<sup>45</sup> One might argue that the first column numbers are less meaningful because they count positive and negative estimates that are not statistically significant in calculating the overall difference. The last of the four columns above “Dummy” in the “Modified Lott” portion of Figure 4 limits the analysis to statistically significant estimates and finds that three more of the state estimates are positive than are negative.<sup>46</sup> Column 3 takes a population weighted average of all the estimated state-specific effects and highlights whether the result is statistically significant by using a bright (dark) color.<sup>47</sup> Column 2 avoids getting individual state estimates and simply generates an aggregated statistical estimate from the overall model. For this model (“Dummy” using “Modified Lott”), neither of the estimates of the aggregated effects of the RTC laws is negative or statistically significant, and thus there is no support in that model for the “more guns, less crime hypothesis” with respect to robbery. Indeed, the aggregate of the state specific effects (column 3) is close to 4%, which, if true, would indicate a rather substantial *increase* in robbery.

The same four column estimates are generated for the second model, which replicates the first, with one exception—rather than just looking at whether crime is lower or higher, the “dummy with state trends model” tries to see whether crime simply followed a pre-existing trend at the time of adoption.<sup>48</sup> Here the results are terrible for the Lott model: columns 1 and 4 reveal that most states experienced crime jumps (whether one looks at all states or only those with statistically significant estimates) and columns 2 and 3 suggest

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45. The numbers associated with the first and fourth columns should be read from the index on the right hand side of the Table. *See infra* tbl. 4

46. *Id.*

47. *Id.*

48. *Id.*

that robbery increased by over 5% (with both estimates being statistically significant).<sup>49</sup>

Looking to the third set of “Modified Lott” models—the spline model—we see that one estimate (in column 2) suggests that crime goes down while the other estimates suggest the opposite.<sup>50</sup> The spline model attempts to ascertain whether there is a change in the trend of crime after adoption.

These figures were constructed so that visually one would get a sense of whether the estimated crime effects were positive (in which case the RTC laws would increase crime and the columns would go upward) or negative (in which case the opposite would be true). The overwhelming impression from Figure 4 is that RTC laws *increase* crime. Yet, despite the fact that only 8 of the 48 columns are negative, one can immediately see the danger from someone who wishes to portray the data tendentiously. All one has to do is cherry-pick the estimates and present the aggregate spline model (as opposed to the weighted mean of the individual state estimates) using the “Modified Lott” or Zheng variables, which generates statistically significant negative estimates in the neighborhood of 1-2%. Of course, such conduct would violate statistical conventions, but this showing does reveal that even when the evidence points strongly in one direction, there are often scattered estimates that cut in the other direction against the weight of the evidence.

If one looks at the assault and even more powerfully at the violent crime figures, the evidence again supports—if anything—a positive relationship between RTC laws and crime. But again, a tendentious presentation of the data could find negative estimates, including a negative and statistically significant estimate of the impact of RTC laws on assault. On the four property crime categories, the estimates are overwhelmingly in the direction of suggesting *increases* in crime, but even there one could find two negative and statistically significant estimates for burglary.<sup>51</sup>

In fact, the only two crimes for which the visual impact of the corresponding Figure is not overwhelmingly suggestive of a crime *increase* rather than a decrease are murder<sup>52</sup> and rape.<sup>53</sup> My basic read of the evidence on murder is that it provides no evidence of a drop in murder resulting from the adoption of the RTC law, despite the one statistically significant negative estimate. Given that there are 24 different estimates, the fact that only 1 of 24 is negative and significant, while 13 of the 24 estimates are *positive*, is more suggestive

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49. *Id.*

50. *Id.*

51. *See infra* figs. 6-9.

52. *See infra* fig. 2.

53. *See infra* fig. 3.

of a random influence rather than a true impact of the law.<sup>54</sup> Indeed, looking at the state-specific estimates, one finds that 11 of the 12 sets of estimates show more states have crime *increases* (and 8 of 12 have more states with statistically significant positive estimates).

For the crime of rape, however, the visual impression is that RTC laws are associated with *lower* rates of crime. This is not to suggest that there are no estimates suggesting crime increases, but the weight of the statistical evidence in Figure 3 seems to be suggestive of crime reduction.<sup>55</sup>

For the four property crimes (although least powerfully for burglary), the data is suggestive only of crime increases associated with the adoption of the law.<sup>56</sup> At this point, if one were to rely on the statistical evidence from these four sets of standard fixed effects panel data models—about which we will raise some concerns presently—one would probably reach the following conclusion: judging impressionistically, adoption of RTC laws seems to be associated with crime *increases* in all crime categories except murder, where the mixed evidence is probably most consistent with there being no impact, and rape, where the evidence is suggestive of crime decreases.

#### B. *An Alternative to the Standard Panel Data Model*

Before we pause to reflect on whether such a pattern seems plausible as the causal outcome of the adoption of RTC laws, we should consider an alternative approach to the standard fixed-effects, panel data estimation. My work with Ian Ayres presented evidence that the aggregated specifications employed by Lott are unreliable because they assign more weight to early-adopting states (states adopting RTC laws in the late 1980s and early 1990s) than later-adopting states.<sup>57</sup> The problem stems from the fact that the standard models of the type shown in Figures 2 through 10 can be heavily influenced by changes that occur long after the adoption of the RTC law, which in itself is problematic, but is even more troubling since this effect can only occur for early adopting states (since there is no evidence beyond three years after adoption for some of the late-adopting states). We examined the effect of shall-issue law adoption on a year-by-year basis and discovered that the model estimated very large swings in crime 10+ years post-adoption.<sup>58</sup> These large swings occur, not because shall-issue laws suddenly deter crime over 10 years after adoption, but because some of the late-adopting states dropped out of the analysis at that point. These large swings, which tend to

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54. See *infra* fig. 2.

55. See *infra* fig. 3.

56. See *infra* figs. 6-9.

57. Ian Ayres & John J. Donohue III, *Shooting Down the "More Guns, Less Crime" Hypothesis*, 55 *Stan. L. Rev.* 1193, 1245 (2003).

58. *Id.*; see also *infra* figs. 2-10.

make the RTC laws look much better than they really are, are probably the result of the failure of the crime models to control for the influence of crack on crime, which was substantial in the late 1980s and early 1990s.<sup>59</sup>

Autor, Donohue, and Schwab (“ADS”) examined the effect of wrongful discharge laws on employment and developed a method to avoid the problem of inadvertently weighting early-adopting states disproportionately.<sup>60</sup> The basic idea of this model is to limit the number of years pre- and post-adoption that can affect the estimates of the shall-issue dummy. This limited window is centered around the shall-issue adoption data and will be referred to as the “treatment period.” The model, then, compares the change in the crime rate from the pre- to the post-period (within the treatment period) to the change in the crime rates of the states that did not adopt a shall-issue law during that period. Thus, a state can be both part of the control group and the treatment group, just not at the same time. The model also controls for other covariates, state-fixed effects, and year-fixed effects. A dummy variable is also included for the post-treatment period. This specification is shown below, where  $s$  indexes the state and  $t$  indexes the year:

$$\text{Ln}C_{st} = \alpha + \beta_0 (\text{Treatment}_{st}) + \beta_1 (\text{Post}_{st}) + \beta_2 (\text{Postpost}_{st}) + \delta X_{st} + \gamma_s + \varphi_t + \varepsilon_{st}$$

Treatment is a dummy variable equal to 1 if the year is within  $z$  years of the shall-adoption year where  $z$  is varied. Post is equal to 1 starting the year after shall-issue adoption and ending  $z$  years after adoption. Postpost is equal to 1 starting  $z+1$  years after adoption.  $\beta_1$  is the coefficient of interest.  $X$  represents the covariates included in the Modified Lott specification seen previously.

Importantly, the ADS specification allows for an “adjustment period,” by excluding some observations.<sup>61</sup> In this case, I have always excluded the year that a shall-issue law was adopted since it cannot be cleanly placed in the pre- or post-period. I have also varied this model by sometimes also excluding the year immediately after adoption to allow even more time for the crime rate to adjust.

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59. See Bruce D. Johnson et al., *The Rise and Decline of Hard Drugs, Drug Markets, and Violence in Inner-City New York*, in *The Crime Drop in America*, *supra* note 40, at 164.

60. See Nat'l Bureau of Econ. Research, *The Costs of Wrongful-Discharge Laws*, NBER Working Paper #9425 (Dec. 2002), available at <http://www.nber.org/papers/w9425>.

61. The ADS specification uses Huber-White standard errors, which allow for arbitrary error correlations within states. In addition, the ADS specification weights by population shares instead of just population counts. This technique avoids weighting later observations simply due to national population increases. This final change is inconsequential to the results.

Figure 10 shows the results using four different specifications that vary the treatment period between five and seven years and the adjustment period between one year and two years.<sup>62</sup> These choices seem reasonable to understand the effects of shall-issue laws, given both the likely time of adjustment and the limitations imposed by data. Again, these estimates are unaffected by arbitrary fluctuations in crime more than three years after the year of adoption. Notice that there are *no* statistically significant negative coefficients, and the RTC laws are associated with *increased* larceny and auto theft (and, at a lower level of significance, overall property crime). Figure 10 again shows that there is absolutely no support for the more guns, less crime hypothesis.<sup>63</sup> The violent crime and burglary estimates are all statistically significant and the other property crimes seem to experience significant crime *increases*. If one preferred this approach over the earlier standard panel data model results presented in Figures 1 to 9,<sup>64</sup> one would essentially discard the previous suggestion that RTC laws reduce rape, and conclude that RTC laws appear to have no effect on violent crime and increase property crime (except for burglary) during the first two or three years following adoption.

In evaluating the plausibility of this conclusion, one must consider the following issues. One, we know there is a substantial omitted variable bias lurking in all of these regressions because of the inability to control carefully for the influence of crack on crime. How does this skew the analysis? The Ayres and Donohue article showed that the initial Lott and Mustard estimates, which used data from 1977-1992, were badly skewed because in the 1985-1992 period the adopting states did not have big crack-induced crime increases, while the non-adopting states like New York, California, Illinois, and Michigan did have big crime increases.<sup>65</sup> This explains why crime rose so much faster in the non-adopting states during the 1985-1992 period (even though Lott and Mustard attributed this disparity to their failure to adopt RTC laws), and also why crime fell so much more in the non-adopting states in the post-crack period after 1992.<sup>66</sup> Thus, the early adopters appear to be much better at reducing crime than they likely are, and the late adopters appear to be much worse in increasing crime. Two possibilities exist: the biases cancel each other out and the resulting Figure 10 estimates can be taken as reasonably accurate, or the biases cut one way or the other.<sup>67</sup> It is at least possible that the

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62. I also extended the period of examination to four years on either side of the adopting year and got similar results. I do not report those results, however, because they are still somewhat affected by the concern that one does not have four years of post-passage data on the 1996 RTC adopters.

63. *See infra* fig. 10.

64. *See infra* figs. 1-9.

65. Ayres & Donohue, *supra* note 57, at 1217-18.

66. Lott & Mustard, *supra* note 14, at 18-19.

67. *See infra* fig. 10.

Figure 10 results are biased in favor of the more guns, less crime hypothesis, particularly for violent crime, which rose so sharply during the crack epidemic. If this were true, then the apparent finding of no effect on violent crime but increases in property crime may be inaccurate and the true effect might be that RTC laws yield across-the-board crime increases.<sup>68</sup>

But are RTC laws likely to cause any property crime increases? It is certainly not clear what the causal mechanism behind such an increase might be. Lott and Mustard suggest a substitution effect as robbers try to avoid the newly armed populace and thus resort to property crimes of stealth.<sup>69</sup> This might not be a bad tradeoff—if it were true. But since we saw no evidence of any decline in robbery—Figures 4 and 10 actually show *increases* in robbery, although the latter figures are not statistically significant—the empirical support for this thesis is nonexistent. Also, as a theoretical matter, it is not clear how dissuading a criminal from rape or assault could encourage the thwarted criminals to steal cars and hubcaps.

Where does this leave us? Based on the current evidence, it would seem that one needs to make a judgment about whether the crime models presented in this Essay are working well. If the judgment is that they are, then one would have to embrace the finding that property crime will be *higher* when RTC laws are adopted. Conversely, some might contend that the fact that the models suggest that RTC laws increase property crime is itself the best evidence that the statistical models are not working well. In any event, there is *no* evidence here that would support the more guns, less crime hypothesis.

#### CONCLUSION

With cable TV and talk radio degrading the quality of policy discourse, and with the public being exposed to a seemingly endless array of lies, distortions, and frauds from both the government and the corporate and retail sectors of the economy, it is alarming to see a similar pattern demonstrated in the academic realm. As I have shown in this Essay, there are enough opportunities for ideologically driven analysts to cherry-pick their preferred model to reach results that they find congenial, even if in so doing, the truth is the first casualty. All we can really say is that we know that there is no evidence of reduction in violent crime when RTC laws are passed, and that, although there is evidence of increases in property crime, the theoretical basis for such a finding is weak. We do know that anything

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68. I would expect, though, that the Figure 10 estimates are less likely to be biased than those presented in Figures 1 to 9 because the Figure 10 estimates are generated over a shorter time frame of five or seven years. *See infra* figs 1-10.

69. Lott & Mustard, *supra* note 14, at 64.



that increases the number of guns in circulation will increase the number of guns in the hands of criminals, since about 1.5 million guns are stolen every year.<sup>70</sup> It may be that concealed carry laws spur some crime in that way (offsetting any benefits the law might generate). It also may be the case that as potential victims arm themselves, some criminals are deterred while others simply increase their armament and shoot faster. There can also be an emboldening effect that makes some who did not intend to become criminals lapse into that camp when they get stressed in some way and now have a gun that can turn a shouting match into an aggravated assault or death.

This Essay has tried to illustrate the net effects of RTC laws by presenting causal models from which one can determine whether the gun law in question causes crime to rise, fall, or remain unchanged. While developing this factual information is undoubtedly important, it is possible that an ultimate evaluation of RTC laws, as well as other forms of gun control, should pursue a different objective. An alternative approach would first identify the best possible global set of laws, practices, and institutions, and then evaluate RTC laws by the extent to which their adoption will succeed in moving society in the direction of the globally best solution. If the socially desirable solution is to impose greater controls on guns, then RTC laws probably can be viewed as harmful in that they entrench more gun owners, thereby strengthening the NRA while undermining the prospects of greater future gun control legislation. Under this approach, one might deem the Brady Bill<sup>71</sup> to be valuable legislation even if it has yielded no discernible crime-reduction benefits, because it represents a step in the right direction designed to help move society closer to a social welfare optimum, such as the registration of all handguns. In determining the contours of the socially optimal solution, I would trust Cook and Ludwig far more than John Lott and the NRA.

A final concern is that our statistical models are simply too blunt an instrument to ascertain the likely modest impact of RTC laws on overall crime. This does not mean that their effect is zero, but only that if crime rose or fell by 1-2%, our models might not be able to accurately detect such a change given all the problems of omitted variables (for example, the crack cocaine epidemic) and endogenous state adoption (these laws are adopted in part in response to crime

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70. See Benston, *supra* note 30.

71. The Brady Handgun Violence Prevention Act, Pub. L. No. 103-159, 107 Stat. 1536 (1993) (codified at 18 U.S.C. § 921 (2000)), also known as the "Brady Bill," was enacted on November 30, 1993 and imposed (as an interim measure) a waiting period of five days before a licensed importer, manufacturer, or dealer could sell, deliver, or transfer a handgun to an unlicensed individual. The waiting period was replaced in November 1998 by the National Instant Criminal Background Check System (NICS), which verifies available records on persons who may be disqualified from receiving firearms under 18 U.S.C. § 922(t) (2000).

rather than in the random fashion that would help ascertain their true impact). But what if it turned out that the impact of these laws on crime were *truly zero*? In this event, society would be confronted with an interesting choice: Should we allow the 2 or 3% of the population who wish to secure concealed carry permits to expend their money on a net useless product, or should we let the much larger segment of the population deprive the small minority of their intense preference for guns? Tradeoffs between the intense preferences of a small minority versus less intense preferences of a larger population raise difficult issues for democracies. One suspects that intensity of preferences tends to count for a great deal, and since it would appear that a lot more Americans are willing to commit violence against gun control advocates than are willing to attack the NRA leadership, it is clear that on intensity of preference grounds, the pro-gun forces will likely enjoy greater electoral success. Indeed, under current conditions, it is impossible to achieve truly effective gun legislation, because the NRA forces will always manage to eviscerate any truly effective gun bill. We must look to scholars such as Cook and Ludwig to help us discern the marginal steps that can be taken to reduce gun violence in a politically acceptable fashion.

**Table 1: Different Models Used to Estimate the Impact of RTC Laws**

	Stanford Law Review (Modified Lott)	Zheng	Donohue Levitt	Spelman
<b>Control Variables:</b>				
<b>Demographic:</b>	36 race-age categories	% black % 15-17 % 18-24 % 25-34		% black % 0-14 % 15-17 % 18-24 % 25-34
	Population size	Population size		
	Population density			
		% metro		% metro
<b>Economic:</b>	Personal income per capita	Income per capita	log (per capita income)	log (per capita income)
	Unemployment insurance per capita			log (unemployment rate)
	Per Capita income maintenance			
		Poverty rate	Poverty rate	
<b>Criminal:</b>	Lagged incarceration rate	Lagged prisoners per capita	log (lagged prisoners per capita)	log (lagged incarceration rate)
		Lagged police per capita	log (lagged police per capita)	log (police per capita)
<b>Other:</b>		Alcohol consumption per capita	Effective abortion rate	
		Governor party affiliation dummies		
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Regression Type:	OLS	OLS	OLS, adjusted for serial correlation with fixed effects (Bhargava)	OLS, standard errors adjusted for clustering at state-level
Additional notes:		No D.C.		

Table 2: Differences in Coding of Passage of Shall-Issue Laws

State	(A) Lott	(B) Vernick	(C) Marvell	(D) Donohue 2004
New Hampshire	Prior	Prior	1994	1994
North Dakota	Prior	1985		1985
South Dakota	Prior	1985		1985
Maine	1985	1981	1980	1981
Florida	1987	1987	1987	1987
Virginia	1988	1995	1995	1988
Georgia	1989	1989	1989	1989
Pennsylvania**	1989	1989	1989	1989
West Virginia	1989	1989	1988	1989
Idaho	1990	1990	1990	1990
Mississippi	1990	1990	1990	1990
Oregon	1990	1990	1990	1990
Montana	1991	1991	1991	1991
Alaska	1994	1994	1994	1994
Arizona	1994	1994	1994	1994
Tennessee	1994	1994	1994	1994
Wyoming	1994	1994	1994	1994
North Carolina	1995	1995	1995	1995
Nevada	1995	1995	1995	1995
Oklahoma	1995	1995	1995	1995
Texas	1995	1996	1995	1995
Utah	1995	1986	1995	1995
Philadelphia**	1995			
Kentucky	1996	1996	1996	1996
Louisiana	1996	1991	1996	1991
South Carolina	1996	1996	1996	1996

Both Lott/Mustard and Vernick considered the following states to have adopted shall issue laws prior to 1977: Indiana, New Hampshire, and Washington. Lott and Mustard also considered Alabama, Connecticut, North Dakota, South Dakota, and Vermont as having adopted shall issue laws prior to 1977. Of those states, Vernick listed both North Dakota and South Dakota as states adopting shall issue laws in 1985, while he considered Alabama and Connecticut as states that never adopted shall issue laws. Finally, Vernick pointed out that in Vermont, no permit is necessary to carry a concealed weapon.

\*\*Pennsylvania initially excluded Philadelphia from its 1989 shall-issue law. In 1995, the law was extended to include Philadelphia.

Note: the shaded lines highlight instances of conflicts between the Lott coding and the Donohue 2004 coding.

\*\*\*The Donohue 2004 Coding supports Vernick's coding in all cases except for the following: For Virginia, I endorsed the Ayres/Donohue and Lott 1988 date of passage of what appears to be a shall-issue law; For Utah, I accepted the Ayres/Donohue and Lott 1995 coding since up to 1994 the statutory language casts doubt that the law was shall-issue; For New Hampshire, I endorsed Marvell's 1994 date due to the addition of "self-defense" to the causes of carrying a concealed weapon; and for Texas, I followed Ayres/Donohue and Lott 1995 coding because the law's effective date was September 1995.

Figure 1: Estimating the Impact of RTC Laws on Violent Crime with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

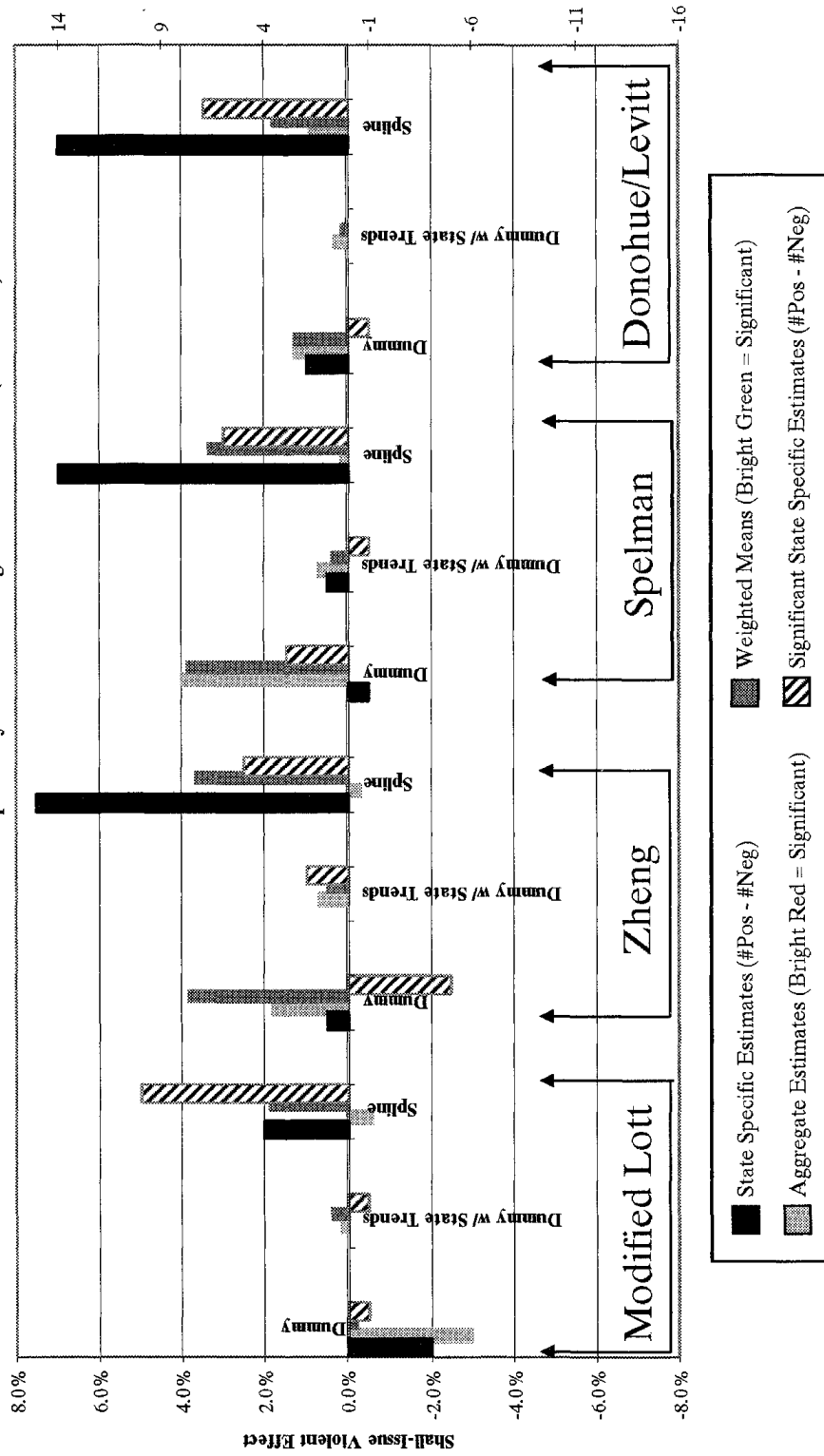


Figure 2: Estimating the Impact of RTC Laws on Murder with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

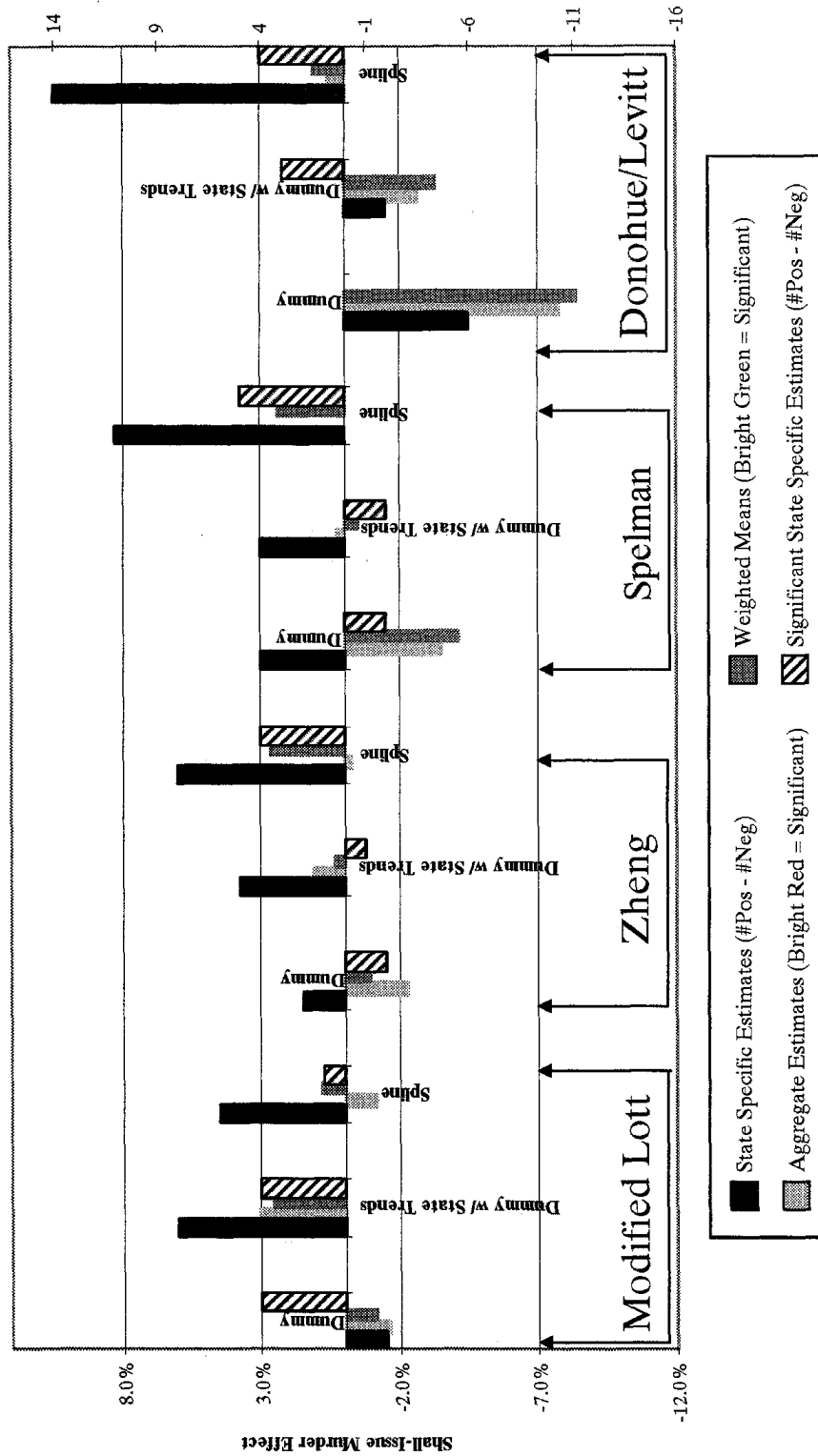


Figure 3: Estimating the Impact of RTC Laws on Rape with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

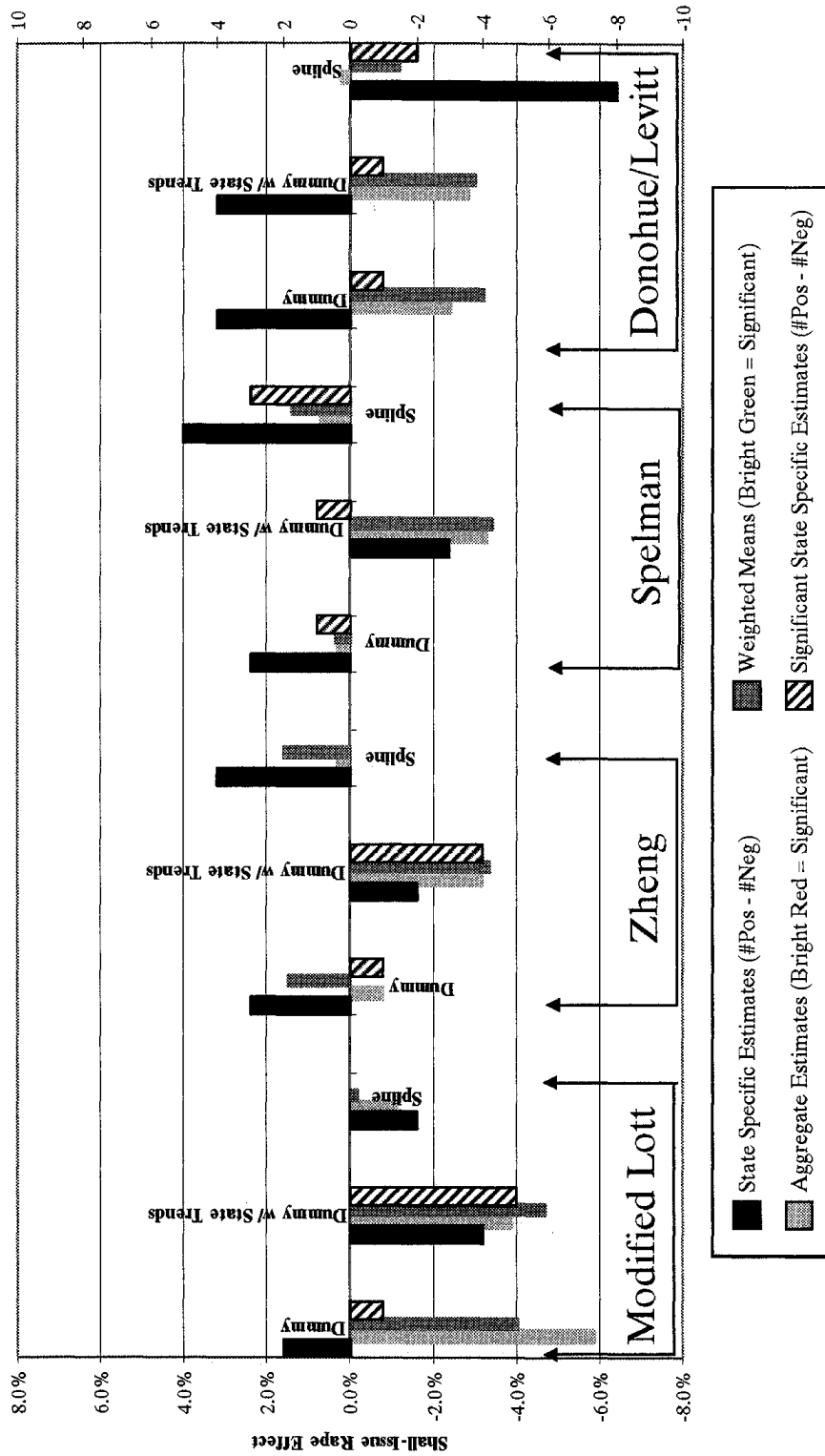


Figure 4: Estimating the Impact of RTC Laws on Robbery with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

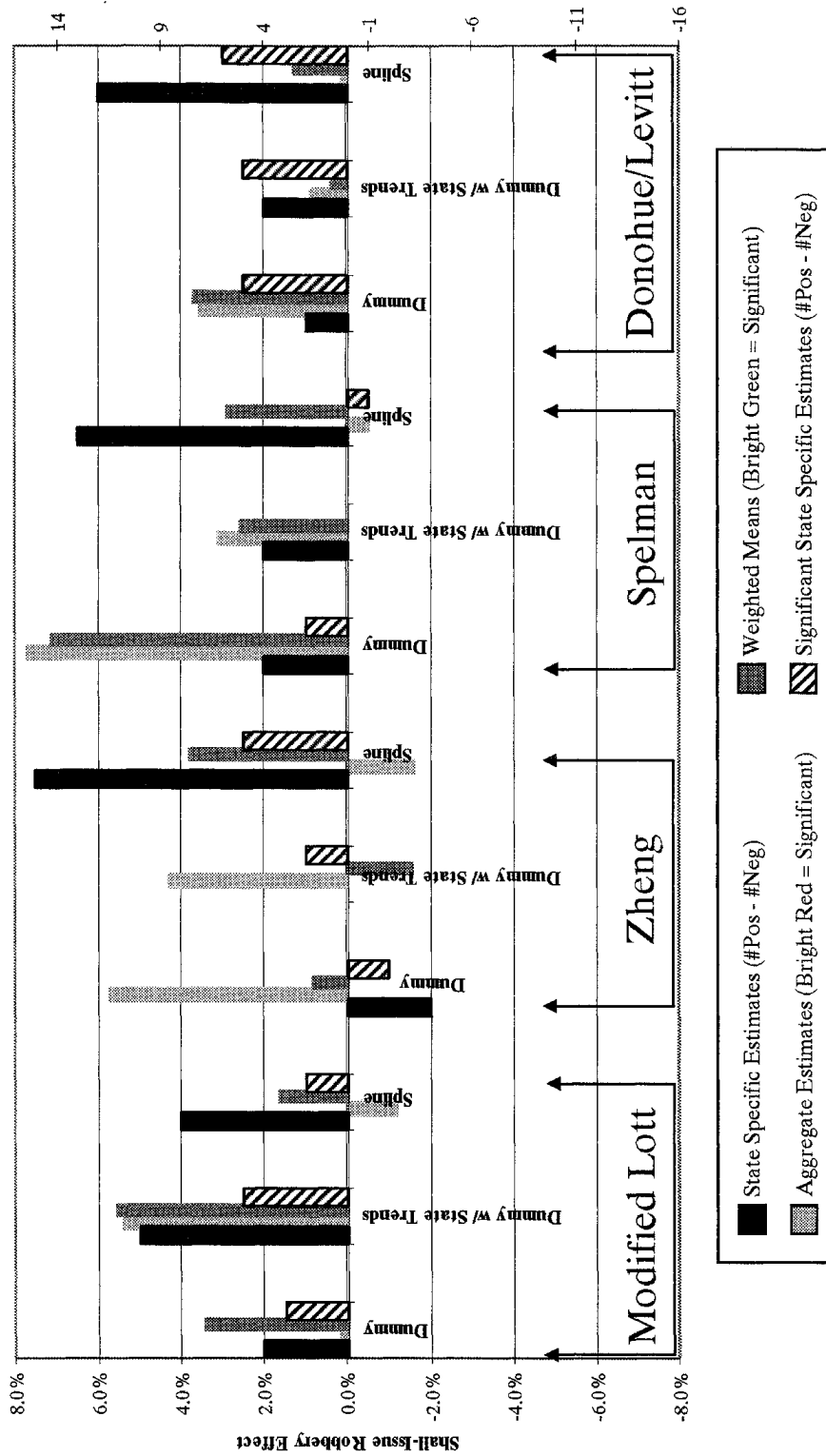




Figure 5: Estimating the Impact of RTC Laws on Aggravated Assault with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

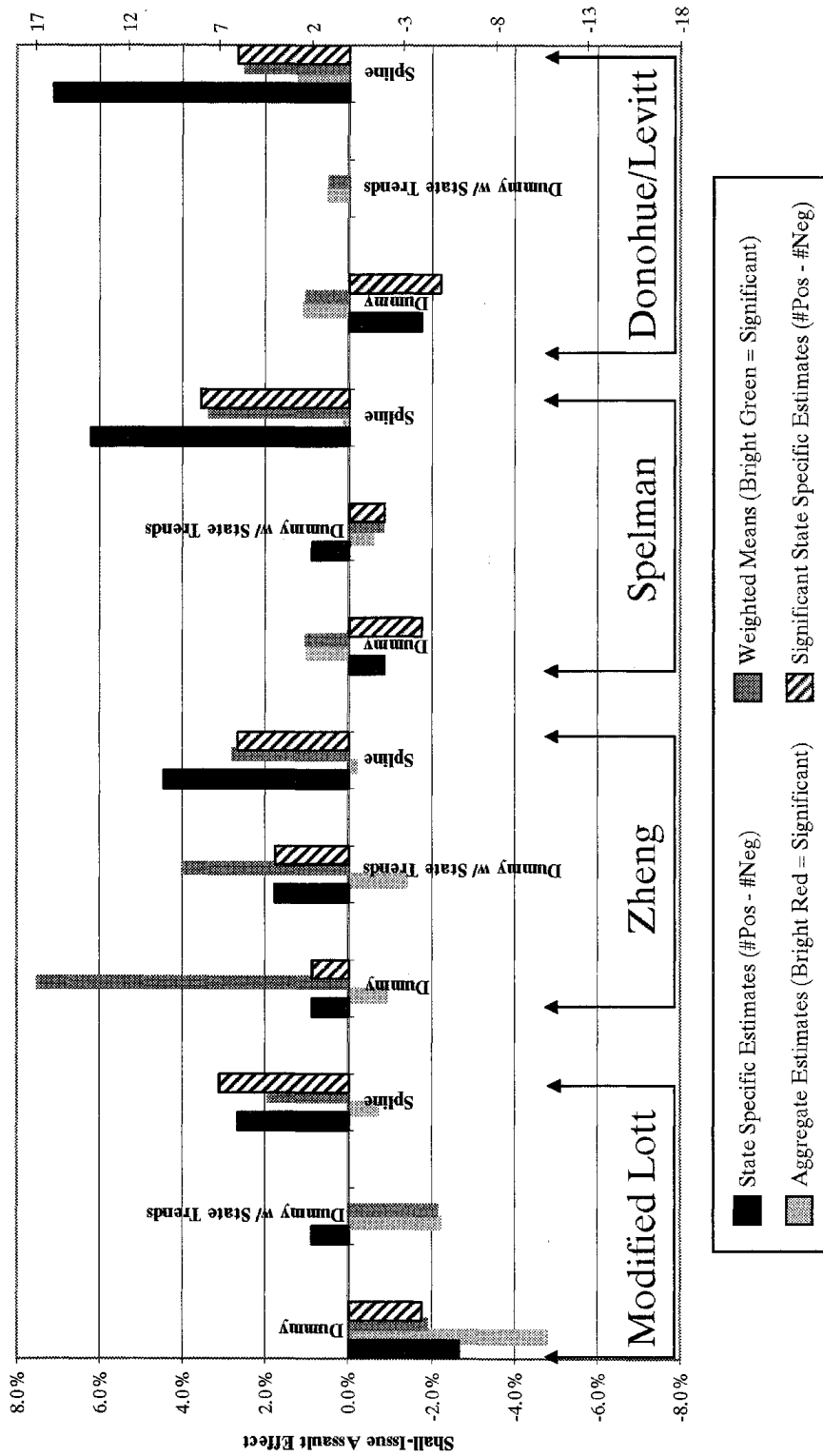


Figure 6: Estimating the Impact of RTC Laws on Property Crime with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

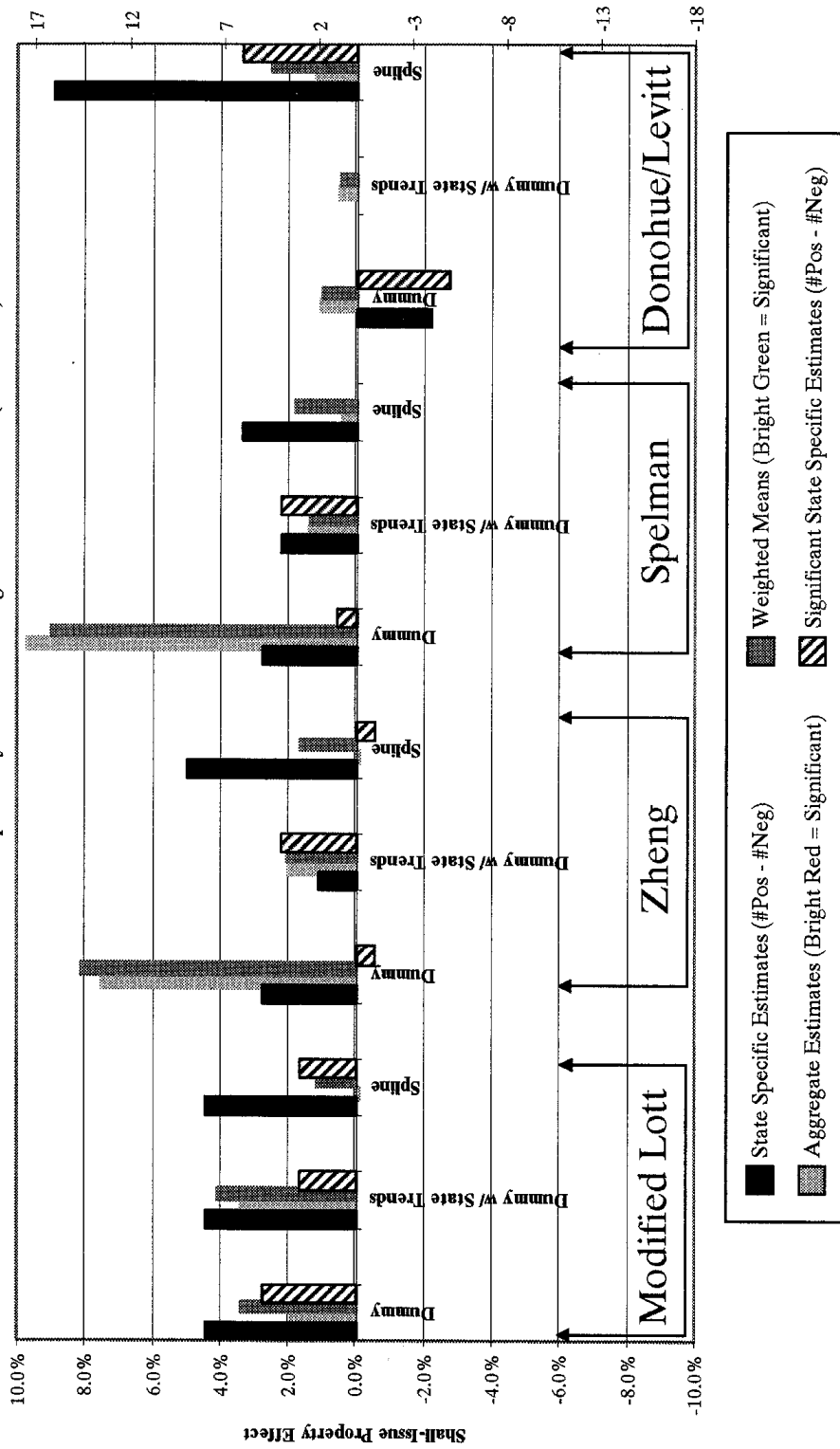


Figure 7: Estimating the Impact of RTC Laws on Burglary with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

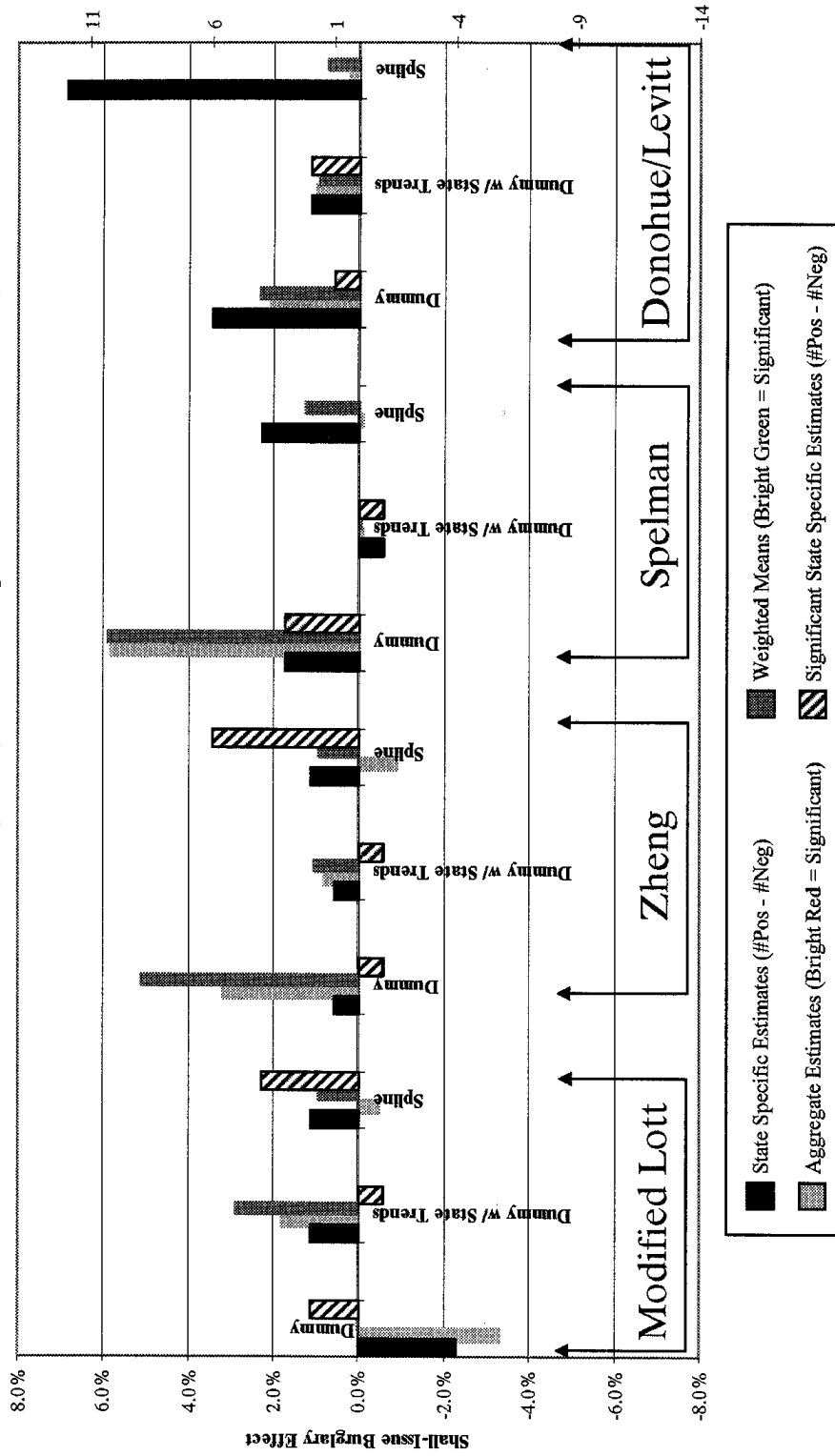


Figure 8: Estimating the Impact of RTC Laws on Larceny with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

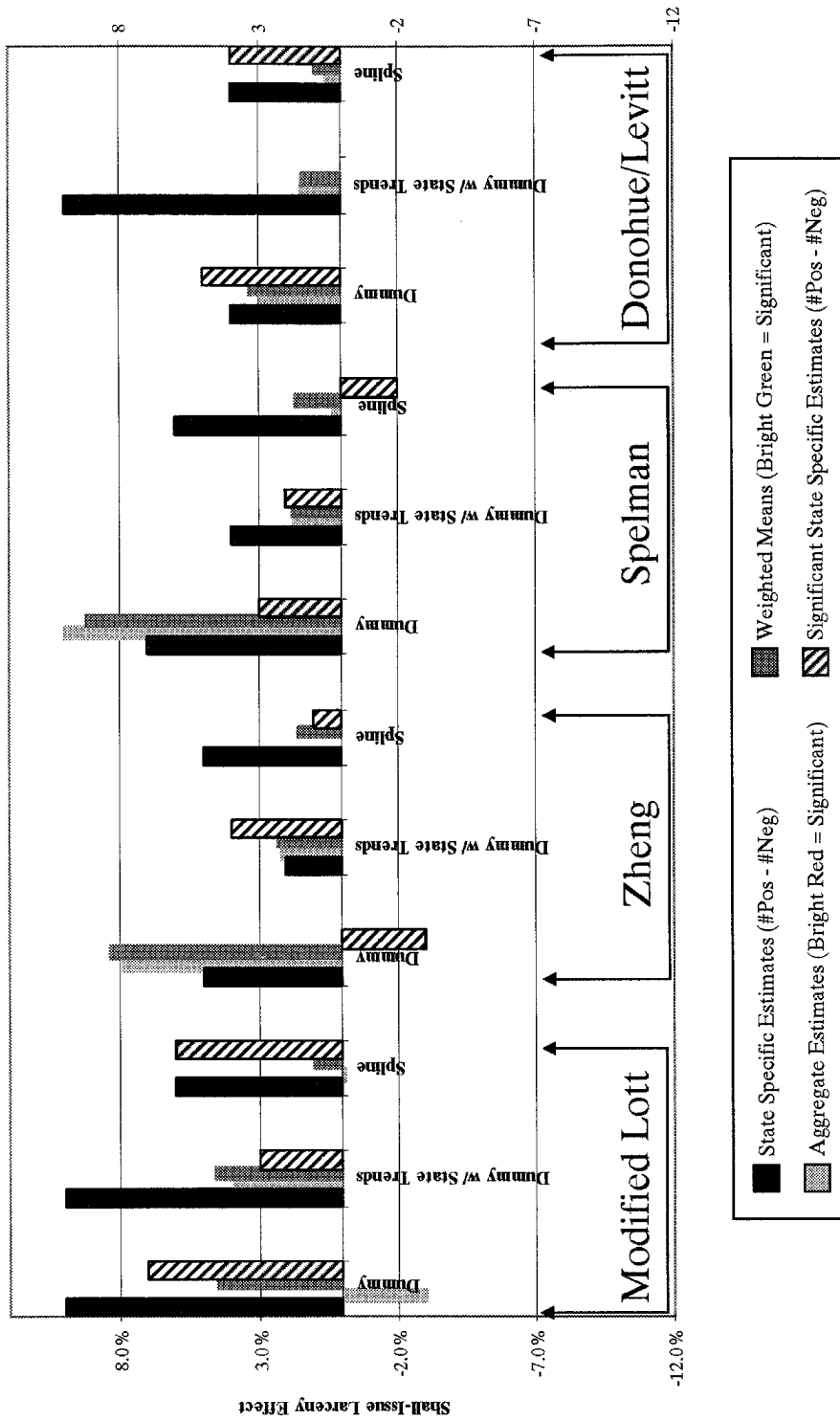


Figure 9: Estimating the Impact of RTC Laws on Auto Theft with Different Models and 4 Sets of Explanatory Variables Using State-Level Data (1977-1999)

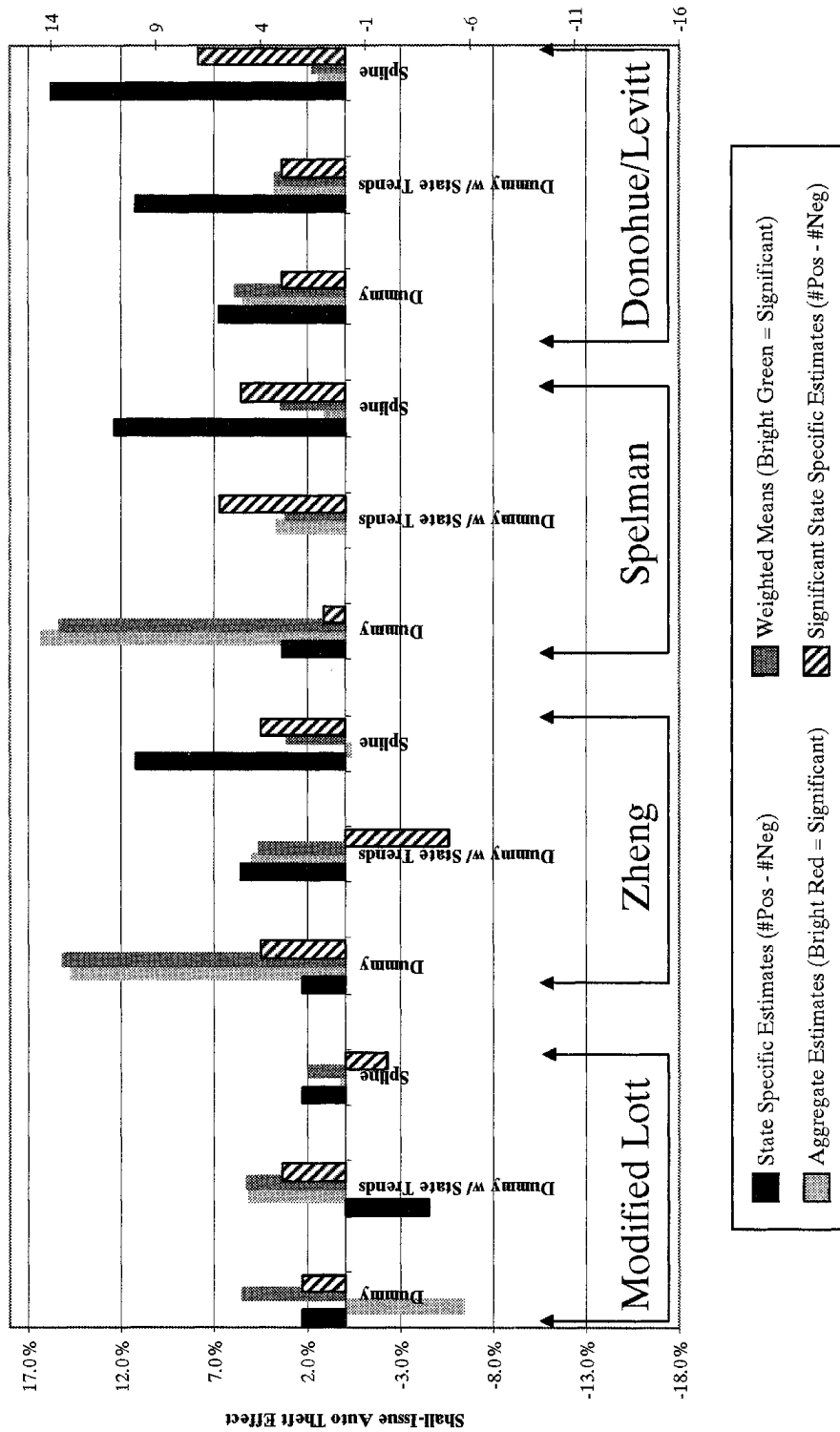
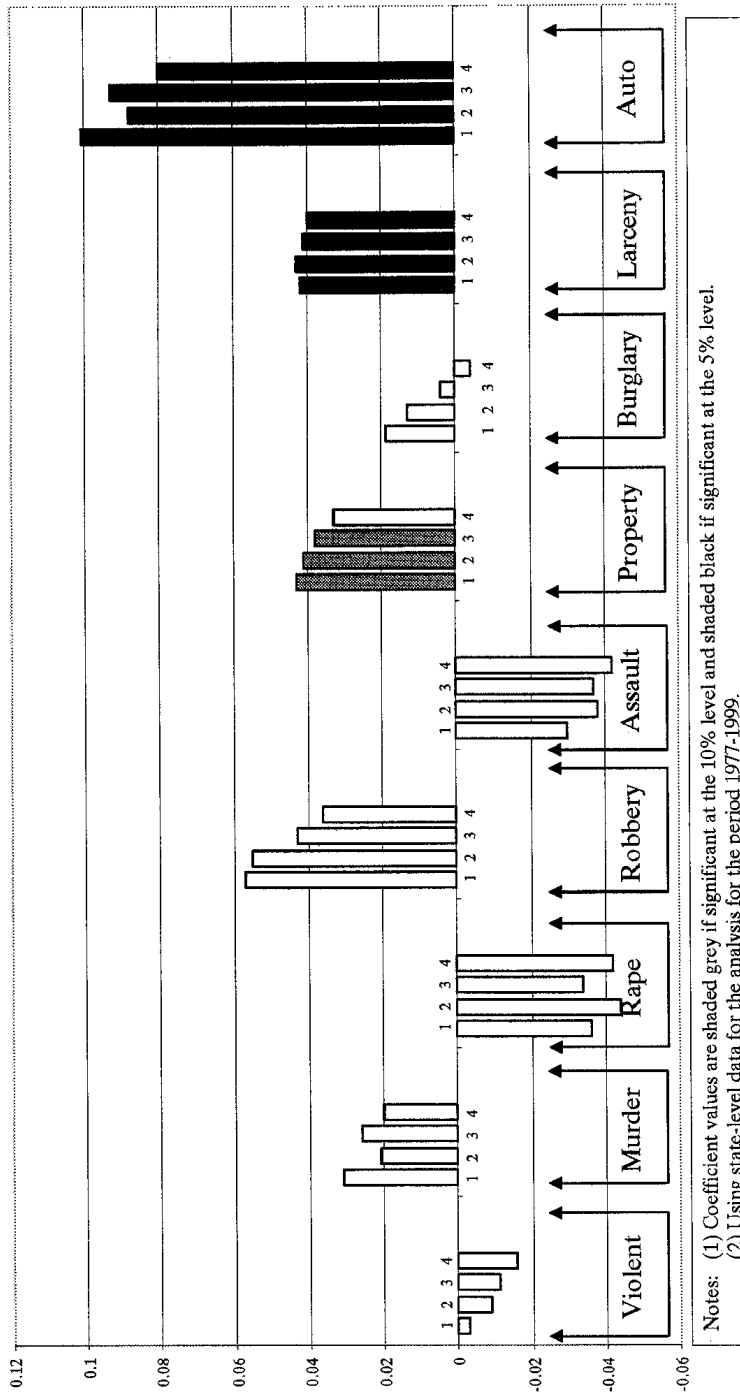


Figure 10: Estimating the Impact of RTC Law Using the ADS Specification, with Modified Lott Explanatory Variables and Donohue (2004) Coding of Laws



Notes: (1) Coefficient values are shaded grey if significant at the 10% level and shaded black if significant at the 5% level.  
 (2) Using state-level data for the analysis for the period 1977-1999.

Key:  
**Specification 1:** Dummy, 5 year period, drop year 0  
**Specification 2:** Dummy, 5 year period, drop year 0 and year 1  
**Specification 3:** Dummy, 7 year period, drop year 0  
**Specification 4:** Dummy, 7 year period, drop year 0 and year 1