

# SHOOTING DOWN THE MORE GUNS, LESS CRIME HYPOTHESIS

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Abstract: John Lott and David Mustard have used regression analysis to argue forcefully that “shall issue” laws (which give citizens an unimpeded right to secure permits for concealed weapons) reduce violent crime. This article shows that the claim has support from certain facially plausible statistical models, but that these are rejected by a variety of statistical tests. Estimating more statistically preferred disaggregated models on more complete data, we show that in most states shall issue laws have been associated with more crime. Using our expanded data set and our preferred jurisdiction-specific regression model, we show that more states have experienced an upturn in crime than have experienced a downturn in crime after enacting the law and that the apparent stimulus to crime tends to be especially strong for those states that adopted in the last decade. We estimate that on net the passage of the law in 24 jurisdictions has increased the annual cost of crime somewhere on the order of half a billion dollars. We also provide an illustration of how our jurisdiction-specific regression model has the capacity to generate more nuanced assessments concerning which states might profit from a particular legal intervention.

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We thank John Lott for generously sharing his state and county data sets with us. David Autor, David Freedman, and Alan Krueger provided valuable comments. Craig Estes, Nasser Zakariya, Matt Spiegelman, Melissa Ohsfeldt Landman, and Fred Vars provided superb research assistance. We also gratefully acknowledge the research support we have received from Yale and Stanford Universities.

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## Introduction

During the last 15 years, the National Rifle Association (NRA) has conducted a highly successful campaign to encourage states to enact legislation enabling adults without serious criminal records or identified mental illness to carry concealed handguns, and some will cheer as others cringe upon learning that 33 states have now adopted such laws. The NRA campaign has been given a substantial academic boost by an initial paper by John Lott and David Mustard and a subsequent book by Lott arguing that the passage of these so-called “shall issue” laws actually reduces violent crime.<sup>1</sup> Indeed, the basic thesis of the combined oeuvre is well-captured in the title of Lott’s book: More Guns, Less Crime.

Lott and Mustard have certainly unsettled the conventional wisdom that greater gun prevalence leads to more crime – or at least to more deaths and serious injuries. The existence of a widely cited study based on the statistical analysis of a massive dataset that is invoked in both political and popular circles as an argument against most forms of gun control suggests that careful

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<sup>1</sup> A law that allows a citizen to carry a concealed handgun if he or she can demonstrate a need to a government official is a discretionary, or “may issue,” law. The “shall issue” laws are designed to eliminate discretion on the part of governmental officials, by requiring them to issue a permit to carry concealed handguns unless specific and easily verifiable factors dictate otherwise. While there is some uncertainty in how states are categorized, we follow Lott and Mustard’s classification that eight states had shall issue laws prior to 1977: Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington. Between 1977 and 1992, ten more states adopted shall issue laws, and it is this time period and those ten states that form the basis for the initial estimated effects of shall issue laws by Lott and Mustard. John R. Lott, Jr. and David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 Journal of Legal Studies 1 (1997). We have now expanded their data set to include the 13 additional states (plus the city of Philadelphia) that have adopted shall issue laws since 1992, with all of the adopting jurisdictions and years of adoption set forth in Table 1.

scrutiny of the empirical evidence is warranted. Of course, those familiar with the statistical analysis of large data bases that are designed to test the effects of public policy initiatives on complex social phenomena such as rates of crime will recognize that even well-designed and ostensibly unassailable studies can reach the wrong conclusions. Indeed, the probabilistic underpinnings of statistical analysis suggest that running regressions for nine different crime categories to see if there is any measurable impact on crime will, by chance alone, frequently generate estimates that on their face are “statistically significant.” As Milton Friedman stated: “I have long had relatively little faith in judging statistical results by formal tests of statistical significance. I believe that it is much more important to base conclusions on a wide range of evidence, coming from different sources over a long period of time.”<sup>2</sup>

Given the massive extent of gun ownership in this country, coupled with the fact that the United States is exceptional in only one aspect of its crime problem – its high rate of lethal violence – it might at first appear that guns must be a part of the problem. But over the last decade, a number of scholars have offered theoretical and empirical support for the notion that allowing law-abiding citizens to carry concealed handguns (unobservable to potential criminals) can deter criminal behavior.<sup>3</sup> The implicit model has two premises: 1) the world can be divided into those who intend to do bad things with guns and those whose motives for gun possession are pure, and 2) the bad folk will be able to get their hands on guns in any event, so that restrictions on guns will largely limit the gun access of the good citizens. These premises suggest that arming the law abiding may tip the balance of power away from the criminals, thereby reducing crime by elevating the probability that a criminal would face an armed potential victim or passerby. In other words, since criminals are naturally concerned about their own safety, allowing more people to carry concealed weapons is one way to raise the costs of those crimes involving direct contact with potential victims.

Lott and Mustard’s empirical project is grounded in the elegant theoretical insight that hidden precautions by potential victims can generate powerful general deterrence effects. Visible precautions by potential victims may just tend to displace crime toward victims who take less precaution, while unobservable precautions (silent alarms, gasoline kill switches, Lojack) make potential criminals generally more reluctant to commit crime.<sup>4</sup> Thus, while the conventional wisdom focuses on the danger that more guns pose to the citizenry, the new critique emphasizes the protective effect that spills over from those who carry concealed weapons. Because criminals cannot know in advance who is armed with a concealed weapon, their risk goes up in an encounter with any potential victim. Thus, while the open carrying of handguns might only divert criminals from potential victims with guns to those without them, legalizing the concealed carrying of weapons holds out the promise of reducing crime rather than just shifting its incidence.

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<sup>2</sup> Quoted in Daniel Hamermesh, “The Craft of Labormetrics,” 53 *Industrial and Labor Relations Review* 363 (2000).

<sup>3</sup> Kleck, Gary, *Point Blank: Guns and Violence in America*, A. de Gruyter, NY, 1991; Polsby, Daniel D., “The False Promise of Gun Control”, *The Atlantic Monthly*, Vol. 273, No. 3, pg. 57 (March 1994).

<sup>4</sup> The crime displacing qualities of observable precaution is at the heart of the following joke:

Two hikers are walking in the forest and come upon a bear. One hiker stops to put on running shoes. The other says, “That’s ridiculous, you can’t out run a bear.” To which the first responds, “I don’t need to. I only need to out run you.”

In the joke, the running shoes are a precaution that becomes observable to the potential criminal (the bear). The effect of taking the precaution is not to reduce the amount of crime, but to displace it onto the unprotected victim. But as is often the case, the joke does not depict a Nash equilibrium. In equilibrium, both hikers would wear running shoes. But the observable precautions would be an inefficient expenditure that would deter no crime from taking place.

The theory may fail, however, if the categories of the criminal and the law abiding are not impermeable. Indeed, the presence of a gun might cause someone lingering near the border of criminality to move solidly into the realm of criminal conduct -- for example, if angry disputes over small matters evolve into criminal homicides instead of mere black eyes. Moreover, with some estimates suggesting that as many as one million or more guns are stolen each year, we know that more guns initially in the hands of the law-abiding population necessarily means that more guns end up in the hands of criminals. In fact, with guns being a product that can be easily carried away and quickly sold at a relatively high fraction of initial cost, the presence of more guns can actually serve as a stimulus to burglary and theft. It is also conceivable that arming the citizenry can encourage an arms race leading more criminals to carry even higher-powered weapons, and to discharge them more quickly when threatened, given the increased cost of hesitation in choosing whether to fire.<sup>5</sup> Finally, accidental deaths and suicides are obviously aided by the presence of guns, and these costs could conceivably outweigh any benefits of shall issue laws in reducing crime. Extensive empirical study is needed to assess the relative magnitudes of the likely conflicting effects.

In trying to determine the impact of the passage of shall issue laws, one would ordinarily begin by asking whether crime was on average higher or lower after the law went into effect, controlling for all the explanatory variables that are thought to impact on crime in the state. This is the so-called “dummy variable” model, which posits that the law will have a fixed (once-and-for-all) percentage impact on crime—that is, the law will raise or lower crime by, say, 5 percent. Lott and Mustard begin their analysis with this dummy variable model, but also explore a second specification in which they attempt to estimate whether the passage of the law will cause a break in the time path of crime, causing it to tip up or down depending on whether the law raises or lowers crime. In conducting this “trend” (or “spline”) analysis, Lott and Mustard estimate what the average linear time trend of crime is before the law passes, and then probe whether this trend changes after passage.<sup>6</sup>

In response to criticisms about the robustness of the dummy variable (or “static”) specification results,<sup>7</sup> Lott has correctly noted that, as a theoretical matter, shall issue laws could still dampen the trend of crime without showing any effect in the simple dummy regressions if crime were to follow an “inverted V” pattern. For example, Figure A depicts a case in which the

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<sup>5</sup> John Donohue and Steven Levitt, “Guns, Violence, and the Efficiency of Illegal Markets,” 88 *American Economic Review* 463 (May 1998).

<sup>6</sup> Instead of reporting regression coefficients, with regard to his linear specification, Lott only reports the change in the before and after linear trends (see Lott’s original Table 4.8), and with regard to quadratic terms in the time trend, he graphs the before and after quadratic effects (see Lott’s original Figures 4.5-4.9).

Lott in his book and a variety of other articles also supports his “more guns, less crime” thesis with other types of evidence. For example, Lott collected data at the city and state levels to test whether different sized jurisdictions exhibited a reduction in crime when a shall issue law was adopted. See Lott (169 & 190). Lott was also able to secure permit data from ten states that he used to test whether counties with more concealed handgun permits have larger reductions in crime. Lott (178). In addition, Lott has concluded from other work that counties located next to states passing shall issue laws experience an increase in crime just as the passing states experience a decrease. Although our disaggregated analysis below will cast doubt on this finding, it should still be noted that any estimate of the overall effect on crime from these laws will be biased toward making them look more beneficial than they are if they cause crime to shift from passing to non-passing states. John Lott & Steven G. Bronars, *Criminal Deterrence, Geographic Spillovers, and Right-to-Carry Concealed Handgun Laws*, 88 *Am. Econ. Rev.* 246 (1988). We do not have these additional types of data and therefore do not analyze them in this article.

<sup>7</sup> See Ayres, Ian, and Donohue, John, “Nondiscretionary Concealed Weapons Law: A Case Study of Statistics, Standards of Proof, and Public Policy,” 1 *American Law and Economics Review* 436 (1999).

crime rate increased for five years in a state before the law's passage and then symmetrically declined in the five years after the law's passage. In this situation, the static regression would estimate no systematic impact of the law (as depicted by the constant estimated horizontal lines before and after passage) – even though there is a large change in the before and after slopes.<sup>8</sup>

Lott argues that the “inverted V” theory is borne out in the data, and, indeed, his graphs of the trends in crime for the states adopting shall issue laws (in his book) appear to show that the linear trend in robbery and other crime categories were increasing prior to passage of the laws and fell thereafter. But to sort out whether the dummy variable model, the linear trend model, or any other model can support Lott's story that shall issue laws *caused* a downward shift in the trend of violent crime, we examine Lott's estimated static and trend effects using more data (extending Lott's data set through 1997) and less constrained econometric specifications.

This remainder of this article is divided into four parts. The first section discusses Lott's basic methodology in greater detail. Section II shows that neither the dummy variable nor the linear specifications are credible specifications in that the implicit restrictions in these regressions are rejected by less constrained regressions. Section III shows that Lott's results are not robust to estimation over a later period (1991-97) or to the inclusion in his dummy variable model of state specific trends on the entire data set. Section IV explores even less constrained regressions, in particular estimating state specific effects, and finds that the core finding of more guns, less crime is reversed once the statistically preferable state specific regression models are used. Section V discusses a hierarchy of possible conclusions to emerge from our empirical work, and provides an illustration of how state-specific regression models can provide more nuanced policy recommendations across states than are possible with more customary aggregated models.

## I. The Statistical Methodology of Lott and Mustard

The statistical methodology needed to ascertain the impact of a legal change, such as the passage of a shall-issue law, has been developing over the last 25 years. Lott and Mustard adhere to the basic contours of the current gold standard of micro-econometric evaluation – a panel data model with fixed effects. That is, Lott and Mustard begin by collecting data over a period of years (1977-1992) for individual states and counties across the United States, and then used panel data regression techniques to estimate the effect of the adoption of shall-issue laws, controlling for an array of social, economic, and demographic factors.<sup>9</sup> In conducting a major empirical study of this kind, a researcher must confront a large number of choices about data issues, model specification, and control variables, each of which has the potential to influence the outcome of the analysis in ways that are not often predictable *ex ante*. We now address some of the major choices on these matters that Lott and Mustard have made.<sup>10</sup>

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<sup>8</sup> *Id.* at 445.

<sup>9</sup> There are actually two “fixed effects.” The first is a dummy variable for each county or state that is designed to reflect any unvarying trait that influences crime in that county or state yet is not captured by any of the other explanatory variables. The second is a set of “year fixed effects,” which are dummy variables included for each year of the data set to capture any national influence on crime that is not captured in any of the other explanatory variables, but which might be expected to effect all jurisdictions equally. A full list of the variables included in the regressions (other than year and county dummies) and their summary statistics is included in Appendix Table 1. In the second edition, Lott also reports some regressions including region-specific year fixed effects for five regions. Lott (178).

<sup>10</sup> As noted, the initial paper on this topic was by Lott and Mustard and the subsequent book (now in its second edition) is by Lott. As a shorthand, we will at times refer to Lott's work without trying to distinguish between Lott's single-authored contribution versus the combined work of Lott and Mustard.

## **A. Coding the Timing and Status of Shall Issue Laws**

The data set that Lott and Mustard initially constructed covered the years from 1977 – 1992. Because of their fixed effects estimation technique, their analysis is able to measure the effect of the law only for those states that changed their legal status over this period. Hence, the coding of any state as either a shall issue or non-shall issue state will not influence the estimated effect on crime as long as the legal status persisted over the entire sample time period.<sup>11</sup> Perhaps surprisingly, there are conflicts among the supporters and opponents of gun control legislation about whether various jurisdictions even have a shall issue law or not. For example, the National Rifle Association characterizes Alabama and Connecticut as having shall issue laws while the Brady Campaign to Prevent Gun Violence treat both states as having a more discretionary system of providing permits to carry concealed weapons (a so-called “may issue” law).<sup>12</sup> Since neither of these laws officially changes status after 1977 (although query whether administrative enforcement patterns as well as citizen behavior concerning the purchase and carrying of handguns may have changed over this period), this dispute will have no bearing on the estimated effect of shall issue laws. Nonetheless, there are numerous disagreements among different scholars about the timing of adoption of shall issue laws that can influence these estimates.<sup>13</sup>

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<sup>11</sup> Since the data from these states will influence the year effects and the estimated coefficients for the various explanatory variables, their inclusion in the analysis – as opposed to the coding of their shall issue laws -- will have an indirect influence on the estimated effect of shall issue laws.

<sup>12</sup> See <http://www.handguncontrol.org/b-main.htm> The NRA’s description of the relevant Alabama law suggests that demonstrating need is in fact a requirement for obtaining a concealed-carry permit (<http://www.nraila.org/research/riflaws.html>), which would seemingly support the coding advanced by the Brady Campaign. On the other hand, it might well be the case that the law which on the surface seems to be a “may issue” law was always, or came to be, administered as a shall issue law. Indeed, between 1985 and the present, every Southern state from Texas to Virginia – with the single exception of Alabama -- adopted a shall-issue law, and it is possible that this lone exception to a universal Southern trend of adoption reflects the NRA’s recognition that in practice Alabama had indeed become a shall issue state in practice even if it had not originally been one by virtue of statutory language.

The case of Connecticut is more complicated, and since no state bordering on Connecticut has enacted the law, one cannot draw inferences about the enforcement of the law from geography as we suggested for Alabama. In January 1978, there was debate over whether local police chiefs (who first need to approve any application to carry a pistol before state-wide approval can be sought) were being too stringent in rejecting the applications. At that point, nearly 50,000 Connecticut residents held state pistol permits, up from 27,628 in 1973. In reflecting on this increase, the president of the Connecticut Chiefs of Police Association stated, “We are concerned about the increasing availability of handguns and the ease with which a person can get a pistol permit. [A] permit is dangerous in the hands of a neophyte who goes to a bar and shows off his phallic symbol to the boys.” As a result, local police chiefs began setting their own ground rules for determining who should get permits. Lincoln Millstein, “Police Toughen Criteria for Getting Gun Permit,” *Hartford Courant*, January 15, 1978, at 1. Indeed, there was a bit of a scandal in Connecticut in 1977 when it was revealed that Michael O’Brien, deemed by the federal organized crime strike force special prosecutor as one of the “two most important criminals in the Hartford area” and convicted for racketeering, extortion, and gambling, had obtained a right to carry a concealed weapon with the support of letters of recommendation from certain major political figures in the state. Andrew Kreig, “Pair Gets 3 to 10 Years in Prison in Racketeering Case,” *Hartford Courant*, July 6, 1977 at 3. This suggests that those who are able to secure handgun permits are not always model citizens, and that at least that criminal thought it would be useful to have the legal right to carry weapons.

<sup>13</sup> While conceding that there are different interpretations of which states have shall issue laws, Lott and Mustard indicate in footnote 32 that they follow the shall issue law classification found in Clayton E. Cramer & David B. Kopel, “Shall Issue”: The New Wave of Concealed Handgun Permit Laws, 62 *Tenn. L. Rev.* 679, 691 (Spring 1995). Lott and Mustard cite two states in particular—Maine and Virginia—as potentially not “true” shall issue states, though they state that their results are not affected by either redefining or dropping these states altogether (see Lott and Mustard, footnotes 33, 34, 35 and 49). Indeed, other scholars provide different dates of passage of the shall issue laws for these two states than those offered by Cramer and Kopel. See Jon Vernick and Lisa Hepburn, “Description and

Choices not only have to be made about identifying when and if a state adopts a shall issue law, but how to begin modeling its effect. Lott states that he assumes that the effect of the shall issue law would emerge in the first year after the law takes effect. However, Lott coded the shall-issue law dummy in that fashion only for Florida and Georgia, with all other states being coded so that the effect of the law begins in the year of passage.<sup>14</sup> Table 1 shows the passage dates of the various shall issue laws that we employ and our differences with Lott and Mustard (column D) and their own inconsistencies across models (column E). Our post-passage dummy, as well as our post-passage trends, are coded to begin in the year following the passage dates indicated in the first column of Table 1.

Note that there is imprecision in these dates both because the statutes are not entirely clear about the precise legal status and because adoption of a shall issue law does not perfectly equate with the actual enforcement of the law either within the state or over time, since enforcement could be quite different county-by-county and year-by-year. This problem will exist whenever one must characterize imprecisely defined statutes into sharply delineated discrete categories so that one can say when a state changes from one category to another. The difficulty is greatest when the statutory language of the shall issue law invokes the command "shall" but then includes inherently discretionary criteria (such as a requirement for the "good moral character" of the permit recipient), or where the law says "may," but some counties or issuing authorities make it quite easy to obtain a permit. In either of these circumstances, there may be important differences between the law in practice and the law on the books, yet none of those nuances will be captured in our statistical analysis.<sup>15</sup>

## ***B. The Use of County Data***

Lott and Mustard decided to rest their analysis most heavily on county crime data rather than state crime data (although they do present some state data results as well). The advantages of this approach are that (1) with over 3000 counties in the country as opposed to only 50 states, there is far more data with a county data set than with a state data set, which, *ceteris paribus*, should improve the precision of the estimates; and (2) the county fixed effects will explain a great deal of the fixed cross-sectional variation in crime across the country, thereby diminishing the inevitable problem of omitting some appropriate, but possibly unavailable, time-invariant explanatory variables.

As we will see, however, using county data has some disadvantages. First, the intervention of interest is generally a statewide phenomenon.<sup>16</sup> Using county data under these circumstances will likely exaggerate the amount of independent data available to the researcher, thereby possibly

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Analysis of State and Federal Laws Affecting Firearm Manufacture, Sale, Possession, and Use, 1970-1999. (Brookings Paper, forthcoming 2002), see Table 5.

<sup>14</sup> Ayres & Donohue (1999 at 449 note 21). Lott made different coding choices in his linear trend analysis, coding the enactment dates in Oregon, Pennsylvania, Virginia, and Philadelphia earlier than was proper.

<sup>15</sup> See Jon Vernick and Lisa Hepburn, "Description and Analysis of State and Federal Laws Affecting Firearm Manufacture, Sale, Possession, and Use, 1970-1999. (Brookings Paper, forthcoming 2002). While there are enough classification discrepancies among the different authors that have tried to determine the presence of shall issue laws that it becomes burdensome to probe all of the possible permutations, our efforts suggested that the aggregated results (which are weighted by state population, thereby sharply reducing the impact of small states on the analysis) were not highly sensitive to these classification issues. Of course, the estimated effect of the shall issue laws for individual states that we provide below will be far more sensitive to the classification issues for these small states.

<sup>16</sup> But not uniformly. Pennsylvania initially excluded Philadelphia from its 1989 shall-issue law. In 1995, the law was extended to include Philadelphia.

creating the appearance of statistical significance when in fact none exists.<sup>17</sup> Second, many of the explanatory variables are only measured on the state level and thus again the county data analysis may be giving a false sense of precision. Third, Lott uses arrest rates (the ratio of arrests to crime in a county) as an explanatory variable, which leads to many counties being dropped from the analysis. This occurs because of missing arrest data in some cases, and also because the arrest rate is undefined for any county that experiences no crime in a particular category in a particular year (since the rate would have zero in the denominator in such cases). Thus, a substantial number of counties are thrown out of the Lott analysis by virtue of the realization of the dependent variable (if it is zero in a given year, that county is dropped from the analysis), which can potentially bias the results of the regression estimation.<sup>18</sup> Fourth, Maltz and Targonski consider the quality of UCR county-level data to be so poor that they dismiss Lott's work on that basis alone (at least if the data extends beyond 1992).<sup>19</sup>

### **C. Model Specification**

We have already noted that the regression model employed by Lott and Mustard is for a panel data set with both county and year fixed effects. Even within this structure, though, a number of decisions need to be made concerning how one models the impact of the law. First, note that the initial assumption is that each adoption of a shall-issue law can be treated as an exogenous event, which means that changes in crime in various jurisdictions do not influence the decision to adopt the law. While it makes sense to begin with this assumption, we will address evidence in section III, that suggests that the assumption may be unwarranted (as Lott and Mustard recognize). Second, the passage of a shall issue law could generate a number of different effects on crime, which call for different types of model specification:

The Dummy Variable Model: Assuming Lott is correct that the passage of the shall-issue law reduces crime, one might see a sudden and persistent drop in crime that would be captured by a post-passage dummy in the panel data regression. Since typically the dependent variable in these regressions will be the natural log of the crime rate, the coefficient on the post-passage dummy variable can be interpreted as the percentage change in crime associated with the adoption of the law. Lott interprets negative coefficients on the post-passage dummy variable to imply that prospective criminals anticipate the dangers they would face in trying to prey upon a more armed population and that they would reduce their criminality in response. Of course, one way that prospective criminals could reduce any increased personal risk to them from such a law would be

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<sup>17</sup> Indeed, when we reran both Lott's and our own specifications clustering on the state level we found dramatically lower statistical significance for the estimated impacts. The cluster procedure allows the regression to relate the variability across counties within a state for more refined estimation of the variance-covariance matrix. See Stata User's Guide (Release 6, 258-9). However, to be both conservative and consistent with Lott, all the regressions reported in this paper do not cluster by state.

<sup>18</sup> The percentage of dropped observations (because of either missing numerators or zero denominators) vary for the individual crime categories – from a low of about 9% for auto-theft to a high of about 48% for murder. 57% of county/year observations have at least one of the arrest rates undefined or missing.

<sup>19</sup> Maltz, Michael, and Targonski, Joseph, "A Note on the Use of County-Level UCR Data," 2001. In this paper, we rely on the county data set to maintain greater comparability with the Lott results, but, with some noted exceptions, we generally find broadly similar results when we either use the state data set or confine the county results to the period not beyond 1992. See Appendix Table 3. We also note that to the extent – however unlikely -- that the break in the county crime data series is relatively uniform across all counties, the year effects would control for this change.

to continue their life of crime in a more hospitable jurisdiction (presumably one without a shall-issue law) to pursue their criminal objectives.<sup>20</sup>

The Lott Spline Model: Again assuming Lott is correct that the law reduces crime, one might observe crime falling as individuals applied to the relevant state officials to secure the right to carry concealed weapons and then in fact began carrying them. Since this process might unfold gradually, one might expect to see a gradual and continuing decrease in crime – at least until the increase in the number of citizens carrying lawful concealed weapons came to an end. In this model (the trend or “Lott spline model”), then, a time trend would emerge after passage reflecting a dampening effect on crime that grew as the number of concealed handguns being carried increased.

The Hybrid (or Main Effect Plus Trend) Model: While Lott employs both of the preceding models, a third more general model is actually a hybrid of the preceding two in that it allows a post-passage dummy to capture the main effect of the law but also allows the law to change the linear trend in crime for adopting states. This model could be important if an announcement effect initially scares some criminals into fearing possible victim or bystander retaliation, but that the ultimate effect is that more guns leads to more serious criminal acts – perhaps as fist fights end with someone dead or seriously injured instead of with a bloodied nose.<sup>21</sup> Under this scenario, one might even see an initial drop in crime followed by a subsequent turn around as the number of concealed guns being carried and crime increase in tandem. Note that, although Lott does not employ this model,<sup>22</sup> it can be used to test whether one or both of the first two models is appropriate.<sup>23</sup>

The hybrid model will generate two estimated effects that could either be reinforcing (both the dummy and trend have the same sign), or in conflict in that one effect is positive and the other

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<sup>20</sup> Note that the simple panel data results cannot distinguish between an effect of a shall-issue law that reduces crime overall versus one which only shifts it to another jurisdiction. Obviously, crime transfers are much less desirable from a policy perspective than crime reductions. Lott & Bronars, *supra* note 6, claim to have found such geographic substitution, but our disaggregated analysis below casts doubt on their findings.

<sup>21</sup> While Lott peppers his work with anecdotes of gun-toting citizens who apprehend or thwart criminals, the nightmare scenario for those asserting the value of defensive use of guns is not mentioned: the case of the Japanese exchange student, Yoshihiro Hattori, on his way to a Halloween party in October of 1992 who mistakenly approached the wrong house and was killed by the gun-toting homeowner Rodney Peairs. Mr. Peairs, who was found civilly liable for the boy’s wrongful death, was at home with his family when the student and an American companion mistakenly rang his doorbell in search of the party. Mr. Peairs’s wife answered and, apparently frightened by the costume, yelled to her husband to get his gun. Mr. Peairs shot Mr. Hattori dead after warning him to “freeze,” a phrase the young man apparently did not understand. A Baton Rouge, Louisiana judge awarded more than \$650,000 in damages and funeral costs to the parents of the Japanese exchange student, saying there was “no justification whatsoever” for the killing of the 16-year-old boy. Adam Nossiter, “Judge Awards Damages In Japanese Youth’s Death,” *The New York Times*, September 16, 1994, Section A; Page 12; Column 1. Although Mr. Peairs was acquitted of manslaughter, there is little doubt that if he hadn’t owned a gun, the 16 year-old boy would not have been killed. At the memorial service held for her dead son, Mrs. Hattori called the man who killed her boy “a victim of the American gun culture.”

Or consider the story of Skip Olson, 58, and his roommate and friend of 25 years, Michael Jurisin, 50. The two Palo Alto, California residents were fighting about rent payments when Jurisin took out a handgun. Olson grabbed Jurisin’s gun and shot him in the back of the head on February 17, 1998. Olson was later convicted of second-degree murder. One suspects that if neither man had owned a gun, no one would have been murdered. Emily Richmond, “Roommate Found Guilty of Second-Degree Murder, Palo Alto Daily News, May 27, 1999 at 3.

<sup>22</sup> Lott, however, did use the hybrid specification in his analysis of geographic substitution. See Lott & Bronars, *supra* note 6 at 250.

<sup>23</sup> If the estimated coefficient on the post-passage dummy were virtually zero, one would reject the first model, and if the estimated coefficient on the time trend were virtually zero, one would reject the second model. If they were both virtually zero, one would conclude that the law had no effect on crime.

is negative. It is theoretically difficult to tell a story in which the main effect of the law would be pernicious while the trend effect is benign, so if we were to see such a pattern, it would probably be suggestive of some model mis-specification rather than evidence that the law actually generated this pattern.<sup>24</sup> The opposite pattern – a negative dummy and a positive time trend -- could occur in a number of ways. For example, this pattern would emerge if the announcement effect dampens crime more powerfully than any effect initially generated by the actual higher risk to the potential lawbreakers of confronting an armed citizen, but over time this effect is overwhelmed by the stimulus to crime that greater gun prevalence generates.<sup>25</sup> Similarly, this pattern would be seen if an initial announcement effect led to a drop in crime, followed by a return to previous levels of crime as the salience of the new law recedes from the consciousness of criminals. The Lott spline model, in contrast, essentially posits no announcement effect and assumes a gradually growing risk to lawbreakers generates a reduction in crime (the Lott thesis) or that the gradual pernicious influence of more guns in the hands of hotheads or criminals leads to more shootings and opportunistic criminal acts. Of course, if both influences are operating – an announcement effect dampening crime and an increased risk effect (reducing crime if the risk is to lawbreakers and increasing it if the risk is to the public) – then the Lott spline model would be mis-specified.

#### ***D. Control Variables***

Lott tries to control for an array of measurable factors that might influence crime rates, such as the arrest rate for a particular crime category in a county, the level of income, and various demographic measures. Since many of these variables make little difference to the analysis it is not worth arguing about whether they should be included or not.<sup>26</sup> The most important potentially problematic variable employed by Lott and Mustard is the arrest rate, which is used to capture any changing deterrence that might be resulting from alterations in the intensity or effectiveness of police or prosecutorial resources. As mentioned in subsection B above, reliance on the arrest rate (measured as the ratio of total arrests for any particular crime to the total number of occurrences of that crime) can lead to a large amount of data being excluded because of the realization of the dependent variable, which is always problematic. Accordingly, we examined the sensitivity of the Lott results to using the state incarceration rate as an alternative and, in some ways, preferable

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<sup>24</sup> Lott does suggest a way in which a pernicious main effect could be followed by a benign long-term trend effect, but this argument is unconvincing. In discussing his findings that public shootings increase for a few years after passage of nondiscretionary handgun laws, Lott suggests that there might have been a type of temporal substitution: people planning such shootings might “do them sooner than they otherwise would have, before too many citizens acquire concealed-handgun permits.” p. 102. But we find temporal substitution to be an unlikely explanation for the aggregate behavior of violent criminals.

<sup>25</sup> Zimring and Hawkins first characterized the announcement effect and the increased-risk effect in Franklin Zimring and Gordon Hawkins, “Concealed Handguns: The Counterfeit Deterrent,” *The Responsive Community* 7(2) (Spring 1997): 46-60. Duggan (2001: 1094) reports that 500,000 guns are stolen in the United States every year (others estimate the figure to be as much as 3 times that number), which means that even if the intended effect of concealed handgun laws is to put arms into the hands of the law abiding, a huge number of them will end up in the hands of criminals. Note that these thefts could undo any initial benign effect of the law in a way that leads to a negative post-passage dummy (as crime initially dropped) and a positive time trend (in light of the increased arming of criminals).

<sup>26</sup> A minor variable that attempts to measure the amount of income maintenance per capita given to those over age 65 under certain social welfare programs is problematic in that the reason for inclusion is uncertain and attempts to replicate and extend this data beyond 1992 to 1997 were unsuccessful. Since its inclusion or omission matters little to the analysis before 1992, we just excluded it in all reported regressions.

measure of the likelihood of punishment.<sup>27</sup> To dampen problems of endogeneity, we used the lagged value of the incarceration rate. But whether one uses arrest rate or incarceration rate one must acknowledge the possible problem that instead of being a truly independent variable, either of these measures will in fact be influenced by the trends in crime. Therefore, we will also estimated models in which both of these explanatory variables are omitted as not being truly exogenous controls.

## **II. The Basic Findings Of Lott and Mustard**

### **A. Lott's County Data Analysis for 1977-92**

#### **1. The Dummy Variable Model**

Lott began his analysis by examining county level data over the period from 1977-92. Line 1 of Table 2 shows the predicted effect on nine crime categories using the dummy variable model and his data.<sup>28</sup> A quick examination of the line 1 results reveals 1) four of the five categories of violent crime (the exception is robbery) have negative and statistically significant coefficients, suggesting that shall-issue laws reduce these types of violent crime by from 4-7 percent; and 2) all four property crimes have positive and statistically significant coefficients, suggesting that the laws increase property crime by from 2-9 percent. Lott accepts the regression results at face value and concludes that the passage of these laws causes criminals to shift from committing violent crime to committing property crime where, he argues, they are less likely to be shot since the victim is frequently not present when the crime occurs. Thus, we see violent crime decreasing by 3.5 percent and murders falling by over twice that percentage, while property crime rises by over 5 percent. As Ayres and Donohue (1999) stressed, however, the fact that robbery is not dampened by the adoption of a shall-issue law constitutes a major theoretical problem for Lott's interpretation of the results of the dummy variable model. If there is to be the type of substitution away from violent crime that Lott predicts, one would expect that the new law would induce potential robbers to avoid confronting victims and shift to more stealthy property crime yet, as Table 2 reveals, we see no evidence of this effect. Hence, the dummy variable model undermines a key prediction that Lott offers to explain the line 1 regression results for the period 1977-92.

In Table 4.1 of Lott's book, he presents a version of the line 1 robbery regression that shows that robbery reduces crime by 2.2 percent, which is indicated to be statistically significant at the .10 level (considered marginally significant). But Donohue and Ayres (1999) reveal that this – 2.2 percent figure is an error that results from a miscoding of the effect of the shall-issue laws. The problem was that, instead of following his articulated strategy of assuming that the effect of the law would emerge in the first year after passage, Lott coded the shall-issue law in that fashion only for Florida and Georgia, with all other states being coded so that the effect of the law begins

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<sup>27</sup> The disadvantage of the incarceration rate data is that it is only available on a statewide (as opposed to countywide) basis, although this is not a concern in the analysis of the state data set.

<sup>28</sup> In this table and through out the rest of this paper, in the interests of space we report only coefficients of interest relating to the impact of the shall issue law. But the interested reader can find the complete regression output for all the regressions in this paper (as well as the underlying STATA do files and data sets for independent verification) on the internet at [www.yale.law.edu](http://www.yale.law.edu).

in the year of passage. Correcting this error to consistently adhere to the articulated Lott protocol wipes out the size and significance of the estimated effect on robbery.<sup>29</sup>

## 2. Lott's Trend (or Spline) Model

Lott responds to the point that shall-issue laws seem not to dampen robberies in the dummy variable model by saying that a model that captures the change in the linear trend of crime – the Lott spline model -- provides a better picture of the effect of the passage of a shall-issue law in this case. The only numbers that Lott reports in his book concerning his trend analysis are found in a single row of numbers representing the difference between the before-passage linear trend and after-passage linear trend for the states that passed shall-issue laws (appearing in the book's Table 4.8 and reprinted at the top – Panel A1 – of Appendix Table 2 here). Lott's regressions include year effect dummies, so the pre-and post-passage trend coefficients would capture linear movements in crime in the 10 passing states apart from the general movements in crime for the nation as a whole (which would be captured by the general year dummies). Lott's message in his Table 4.8 is that a trend analysis shows that shall-issue laws lower all crime categories – both violent and property – and in all cases but one (larceny) the reduction is statistically significant.

Panel A2 of Appendix Table 2 reproduces the methodology (although not the reported results) of the Lott trend analysis and reports both the before and after linear time trends (as well as their difference, which is taken as the estimated effect of the law). The discrepancies between Lott's Table 4.8 and Appendix Table 2.A2 result from two different Lott errors. First, Lott has informed us that he mistakenly wrote down the "Shall Trend After" coefficient for violent crimes instead of reporting the difference between the before and after coefficients. Second, Lott has erred in his reporting of the statistical significance of these effects, perhaps because he did not correct the standard errors for the presence of heteroscedasticity in the panel data. Lott reported that the change in trend owing to the shall-issue law for violent crime, aggravated assault, and auto theft was statistically significant at the level shown in the top row of Appendix Table 2. But Appendix Table 2.A2 shows, however, that the differences for these three crime categories were all insignificant.

This Appendix Table 2.A2 replication of Lott still ostensibly finds that the law generates a statistically significant reduction in the time trends (at, at least, the 5% significance level) in 5 of the 9 crime categories tested. But Lott's regressions also incorrectly identify the passage date of three jurisdictions that adopted shall-issue laws,<sup>30</sup> which makes the laws look more effective than they are. The corrected numbers are presented in Appendix Table 2.A3, which shows that the

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<sup>29</sup> Ayres and Donohue (1999) replicate Lott precisely with the coding error and then show how the correction eliminates the robbery effect. The line 1 regressions in Table 2 of this paper are identical to Lott's Table 4.1 results with three exceptions, which are maintained in all the regressions presented here: 1) the coding error is corrected, 2) standard errors are corrected to adjust for heteroscedasticity, and 3) one explanatory variable – a measure of the real per capita income maintenance, SSI and other, for those over 65 – was dropped. One can compare the results in Table 1 of Ayres and Donohue (1999) with those of Table 2 here to see that the only one of these changes that influences the basic story is the correction for the coding error. The explanatory variable of real per capita income maintenance for the elderly was omitted for the reasons noted in subsection D above.

<sup>30</sup> Lott coded the enactment dates in Oregon, Pennsylvania, and Virginia earlier than was proper. Table 1 shows discrepancies between our coding dates and those used by Lott (column D), as well as discrepancies in coding dates between Lott's own dummy variable and trend models (column E).

shall-issue laws statistically significantly reduce crime in only three of the nine categories (murder, rape and robbery).<sup>31</sup>

Line 2 of Table 2 yields essentially identical results to those in Appendix Table 2.A3 in a spline format that is somewhat easier to interpret because the statistical significance of any induced change in trend can be ascertained directly from the regression output (the t-statistic for the post-passage linear trend coefficient). We call this the Lott-spline model even though he did not use a spline himself because this spline is the equivalent of the methodology of the linear trend model that Lott did employ in which he estimates pre- and post-passage trends and then subtracts them. Note that the story in line 2 is changed in a number of respects from that of line 1 (the dummy variable model). Instead of all violent crime (but robbery) falling and property crime rising, line 2 suggests that shall-issue laws have no effect on property crime (or overall violent crime and aggravated assault), but dampen murder, rape, and the heretofore unaffected robbery.

### **3. The Hybrid Model Testing for Main and Trend Effects**

The different results between the dummy and trend models suggests the advisability of employing a more general model that will enable us to test whether either of the more constrained models is statistically preferable. Consequently, we estimate regression 3 in Table 2, which is the generalized model of regressions 1 and 2. Our hybrid model is a less constrained specification than either the dummy variable or the linear specification because it allows (and tests) for the existence of both a once-and-for-all announcement effect as well as a changed (linear) trend effect.<sup>32</sup> But for the four violent crime categories, we see a pattern that is the exact opposite of what one might expect – the main effect of the shall-issue laws is positive but over time this effect gets overwhelmed as the linear trend turns crime down. In other words, according to the hybrid model, in the year after passage the main effect of the shall-issue law is a 6.7 percent increase in violent crime, which is dampened by the 2 percent drop associated with the negative trend variable, for a net effect of 4.7 percent higher crime. After 3.5 years, the conflicting effects cancel out at which point crime begins to fall. This particular result of a positive main effect and a negative trend effect is inconsistent with any plausible theoretical prediction of the impact of a shall-issue law, since it is not clear why the law should initially accelerate crime and then dampen it.<sup>33</sup> The anomalous results suggest that the even the most general form of the three crime models is still mis-specified and hence that its results are unreliable.

### ***B. Extending Lott's County Data Through 1997***

Lott's initial analysis using 1977-92 data captured the period in which only 10 states newly adopted shall-issue statutes, and therefore Lott's regression results should be taken as the predicted effect of the adoption of the law in these ten states. Between 1992 and 1996, however, 14 more jurisdictions (13 states and Philadelphia) have adopted the law, and therefore one might hope to

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<sup>31</sup> Lott's discussion of the impact of shall-issue laws causing criminals to shift from committing violent to committing property crime is no longer central if the Lott spline analysis (regression 2 in Table 2) is the appropriate estimation approach.

<sup>32</sup> Regression 3 confirms the prediction of regression 2 and contradicts that of regression 1 that the shall-issue laws have virtually no effect on property crime (although we will soon see that this finding breaks down when we extend the analysis through 1997 in regression 6 of Table 2).

<sup>33</sup> As noted above, if the results had been flipped with the main effect dampening crime and the time trend suggesting a longer term increase, one could interpret those results in a straightforward manner: the announcement of the law scared potential criminals, thereby dampening crime initially, but as more guns got out on the street and/or as the fear subsided, crime ultimately turned up.

gain more accurate results by extending the period over which the effect of the law is estimated. Regressions 4-6 in Table 2 simply repeat the models of regressions 1-3 but now estimating them over the longer time period from 1977-1997 (and thus measure the effect of adoption of the law in 23 states and the city of Philadelphia).<sup>34</sup> Comparing (the dummy variable model in) lines 1 and 4 of Table 2, we see that adding more years of data weakens Lott's story. Importantly, violent crime is no longer negative, so the basic story that the prospect of meeting armed resistance shifts criminals from violent crime to property crime is undermined. Lott might respond that murders fall by nearly 8 percent and rape by almost 3 percent, as murderers and rapists shifted over to committing property crime, thereby raising its prevalence by 8 percent. But the suggestion that this pattern could be explained by the changed behavior of would-be murderers and rapists is not compelling.<sup>35</sup>

Indeed, the idea that a thwarted rapist would decide to switch to property crime because rape had become more dangerous (to the perpetrator) seems rather fanciful. The issue is important for the following reason. The dummy variable model regression on the full 1977-97 period gives very strong significant results on a number of crimes – murder and rape being negative and property crime, auto theft, and larceny being positive. Yet if the theoretical explanation for this substitution pattern in crime is flawed, then the fact that we cannot believe the regression finding of a large jump in property crime as an effect of the shall-issue law suggests we should not believe the accompanying regression finding of a substantial drop in murders and rapes. Again, the possibility of model mis-specification seems to be a serious concern.

Just as adding five years of data weakens Lott's story based on the dummy model (line 1 versus line 4), so too does it weaken the story using Lott's spline analysis (compare lines 2 and 5 in Table 2). Thus, we see in line 5 that violent crime and property crime both become positive and

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<sup>34</sup> In the second edition of his book, Lott analyzes 4 additional years of data that allow him to test the effects of shall issue laws in 13 additional states. But he only reports results for this data set from tests of the trend specification. It is important to emphasize that combining these later years of data with the original data set is potentially problematic. The 1994 codebook for the (NACJD) crime data that both Lott and we use explicitly notes under a major heading, "Break in Series," that describes a new imputation procedure it will use from 1994 on and cautions:

These changes will result in a break in series from previous UCR county-level files. Consequently data from earlier year files should not be compared to data from 1994 and subsequent years . . . .

Michael D. Maltz & Joseph Targonski, A Note on the Use of County-Level UCR Data (unpublished manuscript 2000). If the break in series caused a uniform jump up or down in crime that applied to all jurisdictions, then our year dummies would control for this problem. Unfortunately, it is generally unlikely that errors in crime data would be uniform across the country (or even random across the country), so the break in the series is a concern. See *supra* note 19 (discussing newer data).

<sup>35</sup> Consider the case of Florida -- one of the states that statistically is most conducive to the Lott story in that murders fell after the passage of a shall-issue law in 1987. If the law caused the predicted drop in murders and rape and accompanying rise in property crime from the 1987 level, then one would expect to see 106 fewer murders and 176 fewer rapes in the state, and an increase in property crime of 68,590. At the least, this would imply that every murderer and rapist thwarted by a shall-issue law went out and committed over 240 property crimes instead -- a number that would be even higher if any of the murders or rapes had been committed by multiple offenders. Assume that every murder and rape that would have occurred but for the shall-issue law would have been committed by a different individual (that is, there were no multiple offenses for either of these crimes). This would imply that a total of 282 individuals did not commit a murder or rape because of the law but that they each committed over 240 property crimes instead, thereby elevating property crime by 68,590. If a number of these rapes and murders represented multiple crimes by the same individual, then the number of property crimes that would have had to have been committed per former rapist/murderer to generate this large property crime drop would rise commensurately. It seems unlikely that the shall-issue law could explain an increase in the amount of property of this magnitude.

significant (even though murder, rape, and robbery still remain negative and significant). In contrast to both dummy variable models, the Lott spline estimated effect for robbery for both time periods is negative and significant – an almost indispensable finding if the Lott deterrence story is in fact true.

The added five years of data has perhaps the greatest impact on the estimates generated by the hybrid model. For Lott's initial time period, the hybrid model (line 3 of Table 2) basically suggested that shall-issue laws were not affecting property crime or robbery but were having unexpected conflicting effects on four other violent crimes. For the full time period, however, the hybrid model's unexpected conflicting results become more prevalent and generally larger (line 6 of Table 2). For example, in the year after passage the line 6 regressions indicate that shall-issue laws increase robbery by over 6 percent (the main effect of 9.5 percent less the trend effect of 3.3 percent). Rather than seeing shifting from violent to property crime, we observe similar patterns for a number of crimes in which early increases in crime are followed by subsequent drops after a number of years. Notice also the very strong *increase* in violent crime and aggravated assault associated with passage of the shall-issue law in regression 6 – a picture that is completely at odds with the dummy variable analysis for 1977-92 (regression 1) showing a strong *decrease* in crime and for 1977-97 (regression 4) showing no effect for these laws. The instability in these models to changes in the five extra years of data or the inclusion of both a dummy variable and a time trend effect is striking.

Because the hybrid model nests both the dummy variable and the spline models, it is possible to test whether the data rejects the implicit constraints imposed by these more-restrictive specifications. Specifically, we can test the spline specification's implicit assumption that there is no announcement effect by looking to see whether the dummy variable coefficient in the hybrid model is statistically different than zero. The data strongly rejects the spline specification in 8 of the 9 hybrid regressions (in specification 6). The dummy variable model analogously assumes that there is no spline effect. This assumption is also rejected in 5 of the 9 hybrid regressions.<sup>36</sup>

### **C. Focusing on the More Recent Adopters – 1991 - 1997**

Table 2 illustrates that extending the data beyond the original Lott time period ending in 1992 weakens Lott's findings. Another way to see this is to limit our focus to the period from 1991 – 1997, during which 14 states and one city (Philadelphia) adopted shall issue laws (as shown in Table 2).<sup>37</sup> In Table 3, lines 1 - 3 report the dummy, spline and hybrid

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<sup>36</sup> Since we have already noted that criticism has been leveled at the use of county-wide data, we also explored in Appendix Table 3 whether the estimated effects of the passage of the shall-issue law continue to hold up when the analysis proceeds using state law data for the three different models and the two different time periods. Again, the striking finding is how sensitive the results are in the nine different regressions presented. The dummy variable model gives results that are strongly corroborative of Lott's thesis, but the two hybrid models reject that specification because the post-passage dummy is virtually never significant. Still, in some ways, the state data results in Appendix Table 3 are stronger for the Lott argument than the county data results in Table 2. In this table, the estimated effect of the law on robbery for all six regressions is always negative and significant (as it is almost always for murder) and the general effect on property crime is insignificant, which is more plausible than the initial argument that shall-issue laws induced massive shifts by thwarted murderers and rapists toward property crime. Although it is disconcerting that the results of the hybrid model of Table Appendix Table 3 follow the pattern of Table 2 in being quite sensitive to the addition of five years of added data, if one just looked at the most complete data period and credited only the state data analysis, one might conclude that the laws appear to be dampening murder and robbery (although this story is weakened somewhat by the inclusion of fixed state trends, as noted in footnote 47 below).

<sup>37</sup> Michigan and New Mexico adopted shall issue laws after 1999, and therefore the impact of these laws cannot be tested with our pre-1998 data. Jon Vernick and Lisa Hepburn, "Description and Analysis of State and

specifications for 15 jurisdictions that passed between 1991 and 1997. For this later passing cohort, we see that, instead of the shall issue law exhibiting a “more guns less crime” effect, both the static and the spline specifications suggest that passage of the shall issue law led to statistically significant increases in virtually every crime category. All nine crime categories of the dummy specification were statistically significant at at least the 5% level, and 8 of the nine crime categories in the spline model were positive and statistically significant at at least the 10% level. Moreover, unlike in the previous hybrid regressions of Table 2, the hybrid model of Table 3 does not clearly reject either the dummy variable or trend models, so we can repose greater confidence in their estimated crime-increasing effect of the shall issue law.<sup>38</sup>

The dramatically different impact of the shall-issue laws on the later passing states also explains why we see the implausible combination of effects in the hybrid specification applied to all years (Table 2, line 6). The early passing states have many more years of post-passing data (relative to the late passing states) to define the trend line. Since the shall issue law seems to have produced more beneficial effects in the early years (relative to the late years), these beneficial impacts find their way into the trend estimate, while the deleterious impact of the law in later passing states finds its way into the dummy estimate of the hybrid specification. In this way, the hybrid specification can indirectly reproduce the cohort story that the data wants to tell by predicting beneficial impacts for the early passing states after a few years (as reflected in the negative trend effect), while predicting deleterious impacts for the later passing states (as reflected in the positive post-passage dummy effect).<sup>39</sup> We will return again to this “cohort” effect when we later estimate jurisdiction-specific impacts of the law and attempt to assess why different jurisdictions experienced markedly different impacts.<sup>40</sup>

#### ***D. Summary of County Data Results in Tables 2 and 3***

The foundation of the Lott thesis essentially is captured in regressions 1 (dummy variable model) and 2 (spline model) of Table 2. While these results are not identical to those presented in Lott’s book, these regressions are probably more authoritative because some apparent coding errors by Lott have been corrected. The results are not as stable as one might like, but if one were to examine only those two regressions, the evidence would tend to support Lott’s thesis. Obviously, the analyst’s task would be easiest if the regressions generated by three different models (dummy, spline, hybrid), for three different time periods (1977-92, 1977-97, and 91-97) all conveyed essentially the same picture. Unfortunately, they do not. Importantly, both the dummy variable and spline models are essentially rejected by the data by virtue of the large and

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Federal Laws Affecting Firearm Manufacture, Sale, Possession, and Use, 1970-1999. (Brookings Paper, forthcoming 2002), see footnotes to Table 5.

<sup>38</sup> The individual dummy and trend effects in the hybrid specification are also overwhelming positive. And while the coefficients in the hybrid specification are rarely significant individually, they are often jointly significant as both effects push in the same direction. The lack of significance of the individual terms suggests (counter to the foregoing hybrid specifications) that the data for the later passing cohort fail to reject the implicit constraints of the static and spine specification.

<sup>39</sup> This same bifurcation may explain the perverse patterns of coefficients in even the 77-92 hybrid specification (line 3). In separate regressions, we have found that the law is associated with dramatically greater benefits in the two jurisdictions that passed the law between 77-87, Maine and Florida, than in the eight states that passed the law between 88 and 92. To fit the different cohort impacts, the hybrid model is likely to funnel the ostensible beneficial impact (whether causal or spurious) in Maine and Florida into the trend effect because they have relatively more years of post-passage data.

<sup>40</sup> We also estimated these 1991-97 regressions on state data and got virtually identical results: over this period, the shall issue laws are uniformly associated with *higher* crime rates.

statistically significant positive effects on both terms in the hybrid models (lines 3 and 6) – particularly for the full data set. But the hybrid model’s prediction of initial jumps in crime followed by subsequent declines in response to the adoption of a shall-issue law seems to conflict with any plausible story of how the laws might influence criminal conduct. For example, it is simply not plausible that the announcement effect caused potential rapists to substitute so strongly toward earlier rapes to avoid the coming prevalence of concealed weapons. This pattern again suggests the likelihood of model mis-specification, perhaps resulting from some omitted variable generating a drop in crime – relative to non-passing states -- that is being spuriously attributed to the shall-issue law.

The confirmation that the Lott model is mis-specified can be seen in the Table 3 regressions restricting attention to states adopting the shall issue laws over the period from 1991-1997. Here we find dramatically different impacts of the shall issue laws. For the later passing jurisdictions, the shall issue laws systematically increased both violent and property crime. While for the entire twenty-year period (1977-97) there is still some evidence of crime reducing impacts for some crime categories (particularly murder), the multiple inconsistencies between these results and the “more guns, less crime” thesis drive us to further explore alternative – even less constrained – specifications.<sup>41</sup>

### III. Probing Robustness

#### ***A. Estimating Passage Effects With State-Specific State Trends***

Researchers use the types of panel data with fixed state and year effects that we presented in Tables 2 and 3 because the included explanatory variables will not fully capture all of the differences in crime across states or the changes in crime over time within states. Using fixed state and year effects corrects for a certain amount of omitted variable bias, and if the remaining excluded effects are random, then we should be able to determine the impact of shall-issue laws if we have the correct model.<sup>42</sup> If there are pre-existing county or state trends in crime that are persistent and not explained by the included independent variables, though, these regression models can give misleading results. To address this issue we added state fixed trends to the regressions presented in Tables 2.<sup>43</sup> This approach parallels a specification that Lott reports in his

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<sup>41</sup> As noted above, we were concerned with Lott’s use of the arrest rate as a right-hand-side control variable because it leads to the exclusion of such a large number of observations. As Ayres and Donohue (1999) emphasized, the incarceration rate may be a useful proxy in its place, and we separately estimated the regressions in Tables 2 to illustrate the effect of eliminating the arrest rate problem by (1) including the state incarceration rate as a control variable, and (2) excluding both the incarceration and arrest rates. The bottom line is that in most ways the analysis changes little from these alterations, although if anything the Lott story is weaker still in both these alternative models. Indeed, a general conclusion from the work of Bartley and Cohen analyzing the 1977-92 Lott county data is that dropping the arrest rate tended to slightly weaken Lott’s findings across all crime categories. William Bartley and Mark Cohen, “The Effect of Concealed Weapons Laws: An Extreme Bound Analysis,” 36 *Economic Inquiry* 258 (1998), see Figures 1 and 2.

<sup>42</sup> The fixed county or state effects essentially imply that crime rates are always higher by a fixed percentage in New York than in, say, Vermont unless some included explanatory variable explains the difference. Similarly, the fixed year effects imply that there are national influences that will operate proportionally on all state and/or counties.

<sup>43</sup> In contrast to the earlier spline and hybrid specifications (which estimated a single pre-passage time-trend for the states which passed the shall issue law), the regressions discussed in this section estimate a separate pre-passage time-trend for each passing jurisdiction (since we control for the crime trend in each passing state and then estimate a single post-passage spline effect for all passing states).

second edition.<sup>44</sup> These new regressions, which we present in Table 4, allow each state to have its own time trend and then enable us to probe whether shall-issue laws cause departures from these state trends.

Comparing the dummy variable models for 1977-92 (line 1) in Tables 2 and 4, one sees that including state trends leads to some mixed results for Lott and causes some strange shifts from the basic Lott estimates.<sup>45</sup> The really bad news for the “more guns, less crime” thesis comes in the dummy model for the entire 1977-97 period (line 3 of Table 4). Here we see virtually all the coefficients turn positive, although only aggravated assault and auto theft are highly significant, with rape being marginally significant.

Once again, however, Table 4 reveals the familiar but unsettling pattern of strong positive main effects and strong negative time trend results in the hybrid regressions 2 and 4, again confirming the Table 2 finding that the Lott dummy and trend models are rejected by the data.<sup>46</sup> One might try to parse the results more finely by arguing that the early period hybrid model of line 2 was better for Lott’s thesis than the hybrid model of line 4 (where violent crime is positive and insignificant and aggravated assault is positive and significant), but we are not inclined to do so. Instead, we take this Table to show that the hybrid model (which rejects the dummy and trend models in both Tables 2 and 4) continues to generate results that are flawed and inconclusive whether or not one controls for state trends in crime.<sup>47</sup>

## ***B. Estimating Less Structured Pre- and Post- Passage Effects***

While our introduction of the hybrid model as a supplement to the dummy variable and trend models used by Lott and Mustard provided a more flexible regression framework with which to estimate the effect of shall issue laws, all three models still impose a considerable degree of structure on how the law’s effect will be estimated. We can further relax the basic Lott and Mustard restrictions by estimating models with a series of pre- and post-passage dummies that can enable us to more closely examine the behavior of crime in a state prior to and immediately following the adoption of a shall issue law.

Our interest in probing the *pre-passage* pattern of crime is to examine the implicit assumption that the passage of the shall-issue law is an exogenous event. This assumption is necessary if, for example, the estimated coefficient on a post-passage dummy is to be interpreted as a measure of the impact of the law. Including a series of *pre-passage* dummies can tell us whether crime is changing in unexpected ways *before* the shall-issue laws are passed. As Autor, Donohue and Schwab have indicated in analyzing the impact of state laws involving exceptions to

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<sup>44</sup> See Lott (172) & Lott (209), “it is reasonable to include individual linear state trends”). See also Dan A. Black & Daniel S. Nagin, “Do Right-To-Carry Law Deter Violent Crime?” 27 *Journal of Legal Studies* 209 (1998) (estimating a static model that includes state-specific linear trends).

<sup>45</sup> Specifically, this change leads to the following shifts: 1) the positive effects on property crime are wiped out (although the “more guns, less crime” thesis is not undermined by this effect); 2) robbery goes from utterly insignificant to importantly negative, while murder remains significant and negative (which supports the Lott thesis); and 3) violent crime, rape, and aggravated assault all go from significant and negative to either insignificant or positive (albeit only marginally significant in the case of aggravated assault).

<sup>46</sup> Because this finding rejects the appropriateness of the Lott spline model, those regressions are not presented in Table 4 (nor were they run).

<sup>47</sup> While we had earlier suggested that state results (reported in Appendix Table 3) were probably the strongest in favor of Lott’s thesis, these results are weakened by the inclusion of state fixed trends. In other words, part of what might look to have been caused by the shall-issue law may have only been a state-specific downward time trend that started before the passage of the law but in the earlier specifications was improperly attributed to the shall-issue law.

employment at will: “Ideally, from the perspective of getting a clean estimate of the impact of the [relevant state laws], the lead dummies would be close to zero and statistically insignificant.”<sup>48</sup> There is, however, reason to believe that the timing of the adoption of shall-issue laws might be influenced at least in part by changes in crime levels, thereby undermining the assumption that the causal arrow points only from law to crime rather than vice versa. For example, one consequence of the terrorist attacks of September 11 is that gun sales rose sharply.<sup>49</sup> It might not be surprising then that in states where crime is suddenly spiking, one would see a similar jump in demand for guns – and possibly for the right to carry concealed handguns, leading to passage of shall-issue laws.<sup>50</sup> In this event, the following scenario is a real concern: (1) crime would be elevated from some extraneous circumstance, encouraging adoption of a shall-issue law; and (2) when crime returned to normal levels the regressions shown in Tables 2 and 4 would erroneously attribute the crime drop to the shall-issue law. This phenomenon would then bias our estimates of the effect of shall-issue laws by making them seem to reduce crime even if they did not. To explore the possibility of this endogeneity, we estimated the impact of shall-issue laws while introducing three lead dummies, one estimating the crime rate four to five years prior to adoption, the second estimating the crime rate three to four years prior to adoption, and the third estimating the situation one to two years prior to adoption.

Similarly, by including a series of *post-passage* dummies, we can allow the data to reveal the pattern in crime change (if any) that follows the adoption of the shall-issue laws. Consequently, we use additional time dummies to estimate the crime level in the year of and after adoption, two to three years after adoption, four to five years after, six to seven years after, and eight or more years after.

There is another major advantage from using the series of time dummies as opposed to the trend estimation that is conducted in both the spline and hybrid models. These trend specifications give inordinate weight to early passing states in estimating the post-passage trends (because these states have a disproportionate number of post-passage observations) and give inordinate weight to the late passing states in estimating the pre-passage trends (because these states have a disproportionate number of the pre-passage observations). In contrast, the less-constrained dummy variable approach allows the regression to decouple the more remote pre- and post-passage effects (which are respectively identified by a relatively small number of states) from the 2 and 3 year effects which are identified by a larger set of data. For example, it is useful to look at the estimated effect for the second or third year after adoption on the grounds that any impact of the law is likely to show up by this point and that we still have a significant number of states included in this estimation (a total of 20 of the 24 adopting jurisdictions).<sup>51</sup> For the next 2 dummies (“4 and

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<sup>48</sup> Autor, David, Donohue, John and Schwab, Stewart, “The Costs of Wrongful Discharge Laws,” (2001).

<sup>49</sup> Unless one thinks that the terrorists are planning to open fire on Americans in public places or to conduct other terrorist operations in the U.S. in ways that will enable them to be thwarted by gun-toting citizens, the post-September 11 gun-buying spree would seem to contradict the essence of the Lott hypothesis – that individuals are motivated to purchase and carry guns because of their proper appreciation of how such acts can increase their safety, as opposed to some attempt to create the mere illusion of heightened safety.

<sup>50</sup> Indeed, Bice and Hemley find that the demand for handguns is sensitive to the lagged violent crime rate. Bice, Douglas and Hemley, David, “The Market for New Handguns: An Empirical Investigation” (2001).

<sup>51</sup> For example the number of observations used to estimate the “year of or year after” dummy is 3314 (county-year observations) for the county data regressions for the 1977-97 dataset. For the “2 or 3 years after” dummy the number of observations drops to 2309 and for the “4 or 5 years after” dummy the number drops to 1436. The reason for this is that states that pass the law in 1996, say, will contribute data to the “year of or year after” dummy in both 1996 and 1997, but will never contribute to the successive dummies. Lott, himself, commented upon how the unbalanced nature of the data could bias the estimated impacts of the law. Lott (216).

5 years after” and “6 and 7 years after,” only the 10 original states that Lott evaluated for the 1977-92 period are included in the estimation.

The results from this estimation of lead and lag dummies are shown in Table 5 (which corresponds to Table 2). To unravel the meaning of these regressions, let’s begin with the lead dummy results for the entire 1977-97 period, which are about as far from the ideal mentioned above as possible. Rather than the lead dummies being close to zero and statistically insignificant, they are often quite large and highly significant. For every crime category except murder, the lead dummies are very large positive and statistically significant coefficients in the three dummies prior to passage. This implies that in the years *before* adoption, crime was higher than average in the adopting states, controlling for national effects occurring each year, the average rate of crime in each county overall, and an array of explanatory variables. Just as one would never make the mistake of attributing the large positive *pre-passage* coefficients to a subsequently adopted shall-issue law, one must be very careful in attributing any negative coefficients in the post-passage period to the shall-issue law.<sup>52</sup> But in fact, for the 1977-97 period, the effect for the “2 or 3 years after” dummy is seen to be highly positive and statistically significant in seven of the nine categories. The other two categories are insignificant, with one negative (murder) and one positive (rape).

If one compares these post-passage effects with the first pre-passage time trend (“1 or 2 years prior”) for the 1977-97 period, there is no statistically significant evidence of any drop in crime. This same pattern holds up if one compares the “1 or 2 years prior” dummy with the “2 or 3 years after” dummy for the 1977-92 period.<sup>53</sup> This is certainly not what one would expect if shall-issue laws reduced crime.

The regressions in Table 5 also give insight into why the constrained regressions run by Lott and Mustard might appear to suggest that shall-issue laws reduced crime. Note that for a number of the violent crime categories, very large negative estimated coefficients are found on some of the dummies for more than 6 years after passage. As noted, only a small portion of the entire array of shall issue states contribute to these estimates, thereby allowing a substantial drop in crime in an early passing state (whether caused by the shall issue law or not) to have a disproportionate effect in estimating a post-passage dummy or linear trend. This finding gives further reason to suspect that a few possibly unrepresentative early adopters may be skewing the

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<sup>52</sup> At the very least, one must acknowledge the possibility that high crime levels induce passage of shall-issue laws and the subsequent return to more normal crime levels is now being incorrectly attributed to the laws.

Lott and Mustard recognize the potential problem of the endogeneity of adoption of shall issue laws, and tries to address it using the theoretically appropriate two-staged least squares approach (2SLS). This approach requires the use of instruments that are correlated with the adoption of a shall issue law while not influencing crime (except through the effect of the shall issue law itself). Unfortunately, a good instrument is hard to find. As Black and Nagin as well as Jens Ludwig have stressed, the Lott and Mustard 2SLS regressions yield such implausibly high estimates for the crime reduction generated by a shall issue law – reductions in homicides of 67 percent, in rapes of 65 percent, and in assaults of 73 percent – that one is forced to accept the a priori conclusion that their instruments, and hence their 2SLS estimates, are not valid. . Dan A. Black and Daniel S. Nagin, “Do Right-To-Carry Law Deter Violent Crime?” 27 *Journal of Legal Studies* 209, 211 (January 1998); Jens Ludwig, “Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data,” 18 *International Review of Law and Economics* 239, 242 (1998).

<sup>53</sup> For the 1977-92 regressions, the pre- and post-passage analysis was carried out in two different ways. First, we defined the pre-passage dummies so that a state passing after 1992 would have non-zero pre-passage values. For example, Alaska passed shall-issue laws in 1994, so the dummy estimating the crime rate 5 to 6 years prior to adoption is non-zero for Alaskan counties in the years 1988-89. The results of this analysis are reported in Table 5. Second, we defined all pre-passage dummies to be identically zero for any states adopting shall-issue laws after 1992. Similar patterns are found in both analyses.

overall estimates of the effect of shall issue laws. This suggests the need to look at state-specific effects in a more focused way – a topic to which we now turn.

#### ***IV. Estimating State-Specific Passage Effects***

On the surface, the panel data regressions in Table 2 for the initial Lott and Mustard time period ending in 1992 appeared to establish a prima facie case that shall-issue laws reduce crime (or, at least in the dummy variable model, reduce violent crime while increasing property crime). This story begins to break down as the data is extended through 1997 (for example, in Table 2 we see the shall law is associated with *increased* violent crime and assault, although the violent crime effect loses significance when state trends are used in Table 4). Indeed, if one just looks at the 1991-97 data, the shall issue law is associated with increases in *all* crimes.

All of the regressions presented so far have estimated an aggregated effect for the laws across all adopting states. But it is well known that aggregation can at times lead to misleading conclusions in statistical studies.<sup>54</sup> For example, the model would be mis-specified if one tried to estimate a uniform effect from the shall issue law while the law had systematically different effects across states. Indeed, we know that this heterogeneity is a problem since the Table 3 results suggest that the adoption of shall issue laws in later passing states produced (or at least is associated with) more pernicious impacts on crime than adoption by earlier passing states. Moreover, the dangers of estimating a single aggregated effect are particularly acute in this case because a state that adopts a shall issue law early in the data period will contribute fully to the estimated post-passage effect, while a state that adopts near the end of the period will have little weight. Since we know from Table 3 that the late adopters tended to experience crime increases, the aggregated analysis will give less weight to these states in estimating the overall effects of shall issue laws. For example, when the Table 5 regressions were run, 14 out of a possible 24 states were not included in the dummy coefficients designed to capture the effect of the shall issue law after more than three years since passage. Thus, what might look like a changing effect over time from the passage of the law may simply be a compositional effect as certain states drop out of the analysis.

One way to avoid these aggregation and compositional biases is to change the specification to estimate a state specific effect for each state that adopts a shall issue law.<sup>55</sup> In other words, we include in our regression for each crime category a separate post-passage dummy for each adopting state (as opposed to a single post-passage dummy pertaining to all adopting states). Building on our previous dummy variable model with state fixed trends, we now use the full 1977-1997 data set to estimate the effect on 9 crime categories for 24 jurisdictions that adopted shall issue laws – a total of 216 estimates. Table 6 presents all of these estimates for all 9 crime categories, while Figures 1 - 3 graphically depict the results for violent crime, murder, and

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<sup>54</sup> Simpson's Paradox, also sometimes referred to as aggregation bias, is just one such example. See Bickel, Hammel, and O'Connell, "Sex Bias in Graduate Admissions: Data From Berkeley," 187 Science 398 (1975) (showing that while aggregate data suggested bias against female graduate applicants, the disaggregated data by department reversed this conclusion).

<sup>55</sup> Black & Nagin, *supra* note 44, were the first to run a disaggregated regression estimating state specific impacts of the shall issue law. However, their analysis was limited to Lott's initial data set, which only allowed them to test for the impacts on ten passing states. They reported substantial heterogeneity in the law's impact for a sample limited to large counties, but their results differ from ours in that (for their limited data set) the state specific impacts were more beneficial, but the impacts tended to be less statistically significant. They also limited their analysis to the dummy specification – which may mis-estimate the true impact of the law (because of Lott's inverted V argument or some other misspecification). For example, in Table 6, we include state fixed trends as an added control.

property crime.<sup>56</sup> These state-specific “dummy variable” coefficients represent an even less constrained specification than we saw in line 3 of Table 4 in that they separately estimate the impact of shall issue laws for each of the 24 jurisdictions that passed the laws between 1977 and 1997. The Table 6 results reject the more constrained specifications of the aggregate regressions which implicitly assumed that the impact of the shall issue law was constant across jurisdictions. More importantly, the state specific estimates frequently undercut Lott’s “more guns, less crime” thesis.

For every crime type there are more states where shall issues laws produce a positive and statistically significant coefficient than states that produce a negative and statistically significant coefficient. For example, as Figure 1 reveals, while there are three states that experience a statistically significant drop in violent crime upon passage of the law, there are five states that experience a statistically significant increase. Overall, there are twice as many jurisdictions (16) that have an estimated increase in violent crime as those that have an estimated decrease (8).<sup>57</sup>

The second column of Table 6 and Figure 2 both reveal the even more dramatic disparity for murder: there are 8 states with a statistically significant *increase* in murder, while only 4 states exhibit a statistically significant decrease. Of the 12 jurisdictions that experienced a statistically significant effect on property crime from the passage of a shall issue law, Figure 3 shows that 8 experienced an *increase* in crime.

Stepping back, we see that out of the 216 estimated impacts in Table 6 (24 jurisdictions by 9 crime categories), 69 exhibited statistically significant increases in crime while only 25 exhibited statistically significant decreases. Overall, Table 6 shows 142 increases in crime versus only 74 decreases. The striking implication from this disaggregated analysis is that shall issue laws increased crime in substantially more jurisdictions than it decreased crime.<sup>58</sup>

But if this is so why would the corresponding aggregate analysis of Table 4 (line 3) suggest that shall issue laws had a negative impact on crime? The reason for this apparent anomaly is worth exploring. First, note that weighting by population in the regression gives far greater influence in the regression to large states and that Texas and Florida (the two largest states) had large and statistically significant estimated drops in violent crime after they passed shall-issue laws.<sup>59</sup> As Table 2 indicated, the estimated aggregated effect on murder in the dummy variable

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<sup>56</sup> As noted above, Philadelphia is treated as a separate jurisdiction, because the law became effective in the city of Philadelphia at a different time than for the rest of Pennsylvania. For convenience, we will still refer to state-specific estimates in referring to these 24 jurisdictions.

<sup>57</sup> Figure 1 also shows the estimated effect for the corresponding aggregated model (in line 3 of Table 4), which is 1.7 percent (albeit statistically insignificant). The population-weighted average of the 24 jurisdiction-specific effects is also shown to be 1.2 percent.

<sup>58</sup> The same story comes through if one uses the Table 6 analysis on state data. In that event, there are only 207 effects (9 regressions x 23 jurisdictions) because we don't treat Philadelphia separately in the state data regressions. We find that there are 39 significant *increases* in crime versus 9 significant decreases. Overall, 124 of the 207 effects were positive versus 83 negative.

<sup>59</sup> Even though one can interpret the coefficients on the individual state post-passage dummies as the percentage effect of the law on crime, one has to at least entertain the prospect that these estimates are picking up other changes in the states in question that happened to coincide with the passage of the shall issue laws. This could happen for any state, but one that has been singled out on this ground is Florida because of the influx of roughly 125,000 largely male, uneducated, and young refugees from Cuba from 1980 through 1981 in the Mariel boatlift, which swelled crime in Florida in the early 1980s, presumably followed by at least some crime decline once the refugees had been fully integrated into the community (or removed from it). David Card, *The Impact of the Mariel Boatlift on the Miami Labor Market*, 43 *Industrial and Labor Relations Review* 245, No. 2 January 1990. Any such crime decline occurring after 1987 from this factor would be captured in the Lott analysis as the result of shall issue law. Moreover, even if the effect of the Mariel boatlift had been completely dissipated by the time of passage as Lott has argued, the

model is a drop in crime of 7.8 percent. Running the aggregated regression without weighting by population lowers the estimated effect on murder from -7.8 percent to -5.1 percent. Hence, weighting clearly increases the apparent murder reducing capacity of shall-issue laws in the aggregated dummy variable model, but it is not the entire story.

Second, as we have seen, the fact that a state adopts a shall-issue law earlier means that it will have a greater impact in the estimation of any post-passage dummy in the aggregated analysis. Thus, imagine a scenario under which only two states (with equal populations) adopt shall-issue laws -- one in 1987 and another in 1996. Assume the effect in the two states is exactly opposite, in the early adopter crime *drops* by 10 percent in the first year after passage and stays at that lower level through 1997, while in the late adopter crime *increases* by 10 percent and will stay that way for ten years. In the disaggregated analysis, one will see equal and opposite impacts, suggesting no overall net effect on crime. This is what the aggregated dummy variable analysis would show if the laws had been adopted at the same time. But the later adoption in the second state means that its impact will be diminished when the aggregated dummy variable model is estimated. Indeed, the aggregated effect in this hypothetical will be a drop in crime of 9 percent because the 10 years of a crime drop of 10 percent will be averaged with the 1 year of the crime increase of 10 percent.<sup>60</sup> As it turns out, two (large) early passing states (Florida and Georgia) experienced drops in murder -- thus inordinately dragging down the estimated aggregate impact. But when we decouple the impact of the law on individual jurisdictions, a much different picture emerges.<sup>61</sup>

Lott might respond that these jurisdiction-specific dummy effects could understate the true impact of the law because his “inverted V” concern might operate on an individual state by state basis. While this specific concern is dampened somewhat by the inclusion of state-specific trends in our regressions, there is value in exploring whether the hybrid analysis is superior to the dummy variable model for the disaggregated analysis as it was for the aggregated analysis. Accordingly, we employed a disaggregated version of the hybrid specification, which estimates for each jurisdiction both an intercept effect and a trend effect. While only 30% of the estimated state-specific spline effects were statistically different than zero, we were able to reject in each of the 9 crime type regressions the hypothesis that the 24 disaggregated spline effects were jointly equal to

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very sharp spike in crime that can be seen for Florida in Figure 1(a) of Ayres and Donohue (1999) reveals that the pre-passage fixed effect for Florida would be artificially elevated, biasing downward any estimated effect on the post-passage dummy. See Lott, “the Concealed Handgun Debate,” 27 *Journal of Legal Studies* 221, 232 (1998).

Of course, one cannot simply select the states that seemed to do well under the law for further evaluation, but it does suggest that some examination of whether there were any identifiable factors influencing crime in any of the states that appear to have large crime changes – whether positive or negative – at around the time of the adoption of a shall issue law might be worthwhile.

<sup>60</sup> Lott includes graphs in his second edition showing the distorting impacts of unbalanced data sets in estimating the impact of the law. Lott (216).

<sup>61</sup> Our disaggregated results also substantially weaken the power of Lott and Bronars’s geographic substitution result. Lott and Bronars use an aggregate specification to show that passage of the law caused crime to decrease in the passing states but increase in adjoining states – because, they argue, of geographic substitution. Lott & Bronars, *supra* note 6. But this purported spillover result could simply be a byproduct of aggregation bias. Our disaggregated analysis demonstrates that passage of the law was likely associated with increases in many metro areas and with decreases in others. The Lott and Bronars story would only be true if crime fell on the shall issue side of the metro border and rose on the no-shall issue side, but nothing in their aggregated analysis would ensure this was the case. If the spillover regression were re-estimated on a more disaggregated basis, we predict that most metro areas would show similar movements in crime in both the areas that were covered and uncovered by the law, which would be the exact opposite of the Lott and Bronars hypothesis of crime fall on one side of the border (in response to the shall issue law) and rising on the other.

zero. Thus, the regressions suggest that the implicit constraints of the disaggregated static model are once again too restrictive.

While we report the raw coefficients of these hybrid regressions in an appendix,<sup>62</sup> Table 7 reports the net 5-year impact of the law annualized in order to facilitate comparison with the static model.<sup>63</sup> Turning to the substance of the disaggregated hybrid specification captured in Table 7, we find a remarkably consistent pattern to that presented in Table 6. Just as in the static model, for the disaggregated hybrid specification more states experienced statistically significant increases in crime after the shall issue law than experienced statistically significant decreases in crime. Overall, 17 of the 24 states report a net increase in violent crime, and 20 out of 24 showed an increase in assault. Only one state (Florida) showed a statistically significant drop in violent crime, while 5 states showed statistically significant increases in violent crime. Similarly, while 8 states experienced a statistically significant increase in assault, not one experienced a statistically significant decrease. In fact, as before, every crime category reports more increases than decreases in crime. For example, there are 6 states showing a statistical increase in murder while only 4 report a statistical decrease. And the disparity is even greater for rape with 5 states displaying a statistical increase, while only 1 state reports a statistical decrease. Overall 148 (of the 216) tests indicate that the shall issue law increased crime and 58 of these estimates were statistically significant (at a 5% level), while only 17 states report a statistically significant decrease – a ratio of more than 3 to 1.<sup>64</sup>

Indeed, for the clear majority of states for *all crime types*, shall issue laws are associated with *increases* in crime, and the statistically significant impacts are more than twice as likely to exhibit increases in crime. While the story of murder or robbery dropping can be found in the aggregated analysis with the linear spline model, it is purely an artifact of the happenstance of early adoption that weights a few large states most heavily.<sup>65</sup> If one takes the population-weighted

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<sup>62</sup> See Appendix Table 4.

<sup>63</sup> To calculate the 5-year impact of the shall issue law under the hybrid specification it is necessary to add together the impacts of the intercept and trend terms for individual years and then sum the yearly impacts. For example, the predicted impact of a law for individual years is:

Year 1:	$1*\text{beta}(\text{shall dummy in state X}) + 1*\text{beta}(\text{spline trend in state X})$
Year 2:	$1*\text{beta}(\text{shall dummy in state X}) + 2*\text{beta}(\text{spline trend in state X})$
Year 3:	$1*\text{beta}(\text{shall dummy in state X}) + 3*\text{beta}(\text{spline trend in state X})$
Year 4:	$1*\text{beta}(\text{shall dummy in state X}) + 4*\text{beta}(\text{spline trend in state X})$
Year 5:	$1*\text{beta}(\text{shall dummy in state X}) + 5*\text{beta}(\text{spline trend in state X})$

where  $\text{beta}(\text{shall dummy})$  and  $\text{beta}(\text{spline trend})$  represent the estimated coefficients on the intercept and trend variables. Summing these individual year impacts together, we were able to calculate a net annualized 5-year impact as:

$$\text{beta}(\text{shall dummy in state X}) + 3*\text{beta}(\text{spline trend in state X}).$$

We also tested whether this linear combination of regression coefficients was statistically different than zero and report the results of this testing above in Table 7.

<sup>64</sup> These results were qualitatively unaffected when we instead calculated the 4-year and the 6-year annualized impact of the law. We also estimated the Table 7 results for state data, which generated 32 positive and statistically significant 5-yr annualized effects versus 18 negative and statistically significant effects. Overall, there were seven more positive effects than negative effects, 107 vs. 100.

<sup>65</sup> The disaggregated analysis is also amenable to the same kinds of test of internal theoretical consistency that we undertook earlier with respect to the aggregate analysis. Looking again at property crimes and robbery, we see in Table 7 on a disaggregated basis that 16 (of 24) states experienced an increase in property crime. And that 14 of those 16 states also experienced an increase in violent crime of which 5 were statistically significant increases in violent crime. Of the other two states, that experienced an increase in property crime but a decrease in violent crime, in only one was the decrease statistically significant. Once again, the strong conclusion is that shall-issue laws are associated

average effect for the 24 passing jurisdictions in Table 6, shall issue laws are associated with *more* crime in all 9 crime categories. For table 7, this is true for 8 of the 9 categories, the sole exception being a -.5 percent weighted average for murder, which is statistically insignificantly different from zero.

We take these disaggregated (state-specific) hybrid regressions to be our most definitive results. In a sense, this paper has been an exercise in testing and rejecting a series of progressively less constrained specifications. We began by rejecting the simple aggregate dummy variable and spline models in favor of the aggregate hybrid specification. We next rejected the constraint that the law had the same impact on early and late passing jurisdictions. We then rejected the decision to exclude state-specific trends. And finally we rejected the disaggregated dummy variable specification. The disaggregated hybrid model that we have finally settled on allows the data to reveal a variety of different impacts of the law – allowing separate intercept and trend effects for each of the 24 passing jurisdictions. And while we might have concerns that estimating this many impacts would rob the regressions of the statistical significance (as we eat up degrees of freedom), we still find that over one-third of the state/crime type tests (75 out of 216) are statistically significant.<sup>66</sup>

To get a better handle on the net impact of the law on all crime, we have estimated the dollar impact of the law on particular crime categories – using the same dollar value per crime that Lott used in his initial study.<sup>67</sup> Table 8 reports the annualized dollar impact of crime for each of the 24 jurisdictions and for each of the 9 crime categories – as well as various aggregations of these amounts. 18 of the 24 states have estimated increased dollar harms, as shown in the “Total” column, which is also depicted in Figure 5. Viewed in aggregate the Table suggests that the net annual impact of the law was to increase the dollar harm of crime by approximately \$1.2 billion. This represents a “harm weighted” annual increase in crime of 2.5% that amounts to an annual burden of \$11.64 on the average citizen in the passing states.

But the Table also reveals substantial variation in the impact of the law. In Texas and Florida alone, the law is estimated to have *reduced* the annual cost of crime by almost \$3 billion, while in Louisiana and Tennessee the law is estimated to have *increased* the annual cost of crime

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with *increased* property crime (whether looking at a disaggregated or aggregated analysis), yet we have no plausible story to back up this effect. Table 7 shows neither a general shift from violent to property crime, nor a more nuanced shift from robbery to property crime, as none of the 16 states that have estimated increases in property crime reported statistically significant decreases in robbery.

<sup>66</sup> It is possible of course to estimate even less constrained specifications that admit the possibility of higher order impacts. And indeed, we estimated a disaggregated quadratic hybrid – which is identical to the disaggregated hybrid discussed above, but which includes a pre-passage quadratic term and a post-passage quadratic spline term. Estimating this quadratic hybrid specification allowed us to test (1) whether the implicit restrictions of the (linear) hybrid are rejected by the less constrained specification and (2) whether the results of the (linear) hybrid were robust to the less constrained specification. We found that the (linear) hybrid’s implicit assumption of no quadratic post passage effect was not decisively rejected in that only 49 of the 216 coefficients were statistically different than zero (although the quadratic spline effects were jointly different than zero in eight of the nine regressions). But the basic results of the (linear) hybrid analysis discussed in the text remain unaltered: calculating the net annualized 5-year impact, we continued to find that the vast majority of the statistically significant impacts were positive (45 vs. 17).

Lott has sharply criticized Black and Nagin’s decision to estimate state-specific quadratic time trends in an aggregated dummy variable model. See Lott (209) & Black & Nagin, *supra* note 44. We agree that such a specification can understate the law’s impact if the impact comes primarily through a kink or bend in the time trend. But Lott’s criticism is not relevant to our quadratic hybrid specification which allows the law’s impact to come through an intercept effect, a linear spline effect, or a quadratic spline effect.

<sup>67</sup> In 1997 dollars, the harm of the following crimes was assumed to be: murder - \$3,092,804; rape - \$91,522; aggravated assault - \$25,247; robbery - \$8416; auto theft - \$3,892; burglary - \$1472; larceny \$389.

by \$1.7 billion. In the 24 adopting jurisdictions that we examined, the mean impact of the law was a \$50 million increase in crime, but the standard deviation was more than ten times this amount (\$574 million).<sup>68</sup>

These dollar figures price all of the estimated impacts of the law (reported above in Table 7) regardless of their statistical significance. An alternative way to estimate the aggregate impact of the law is to put a zero dollar value on all the impacts that are not sufficiently statistically significant. Table 9 reports the aggregate dollar impact of the law for alternative levels of significance – for both the dummy variable specification (originally reported in Table 6) and our preferred hybrid specification (of Table 7). The table shows that the law continues to display a pernicious dollar effect even when we limit our focus to 5-year impacts that were statistically significant at the 10% or 5% level. At these levels, the static specification estimates a net annual impact on the order of half a billion dollars, while the less-restrictive hybrid specification estimates an increased cost ranging between \$143 and \$760 million.<sup>69</sup>

We take these results to be generally devastating to Lott’s “More Guns, Less Crime” hypothesis. Estimating a less constrained specification with more data, we find that more jurisdictions experience an increase in crime than a decrease and that the statistically significant increases outpace the statistically significant decreases by more than a two to one rate in county data (and still by a substantial amount on state data).

## V. What Should Be Done?

Our effort to find the statistically most appropriate model with which to assess the impact on crime of shall issue laws has involved an extended odyssey as our testing, on a more comprehensive data set, has constantly pushed us towards more disaggregated and less restrictive models than the more aggregated and highly constrained models employed by Lott and Mustard. The lesson has been a sobering one in that the facially plausible models relied upon by Lott and Mustard that we present in Table 2 could well encourage a researcher or policymaker into believing that shall issue laws reduce crime. Yet when we reached the end of the journey with our more complete data, the use of a statistically superior model that estimates jurisdiction-specific effects while estimating both main and trend effects (our “hybrid”) and controlling for state fixed effects reduces the initial conclusion to ashes. The best evidence suggests that, for the majority of states, shall issue laws are associated with higher levels of crime.

It is important, though, to be clear about the degree of confidence that we can repose in any particular interpretation of the evidence. In the end, we are left with a hierarchy of 3 conclusions, that we will discuss in turn below.

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<sup>68</sup> One can see from Figure 5 that the two states showing the biggest dollar impact on crime are Texas (a crime decline of over \$2 billion per year) and Louisiana (a crime increase of over \$1 billion) both adopted the law late in data period, which implies that there is relatively little post-passage data with which to estimate these figures. It is quite likely that with more years of data, one would see the estimated effects for those two states to move closer to the mean.

<sup>69</sup> Only if we restrict attention to 5-year impacts in the hybrid specification that were statistically significant at the 1% level do we find a net benefit of the law – with an estimated reduction in crime of \$639 million. But here it is important to note again that the result is driven by just 2 states – Texas and Florida (with an estimated combined benefit of more than 2.5 billion) – which more than offset the estimated crime-increasing impact of Louisiana and Tennessee (approximately \$1.5 billion).

1. There remains no credible statistical evidence that the adoption of shall issue laws will generally lower crime, and indeed the best statistical evidence to be presented thus far points in the opposite direction that the adoption of shall issue laws will generally increase crime.

We are quite confident in this conclusion. While Lott and Mustard have been energetic in trying to offer other types of evidence that can bolster their core statistical findings, if the foundation falls, the entire edifice will crumble. We believe we have shown the foundation has collapsed. Whether further advances in statistical modeling or additional years of data analyzing more state adoptions (or repeals) of shall issue laws will be able to resurrect the structure remains to be seen.

We hasten to add, though, that showing that superior statistical modeling on more complete data reverses the Lott and Mustard conclusion does not necessarily resolve the debate, since “better” doesn’t always imply “good enough.” Another plausible conclusion from the evidence that we have presented is:

2. While the best evidence suggests shall issue laws generally tend to increase crime, there is still too much uncertainty to make strong claims about their effects.

The dramatic reversal in findings in moving from Table 2 to Table 7 certainly reveals that many conclusions about the impact of shall issue laws will be dependent on the particular statistical model that is employed. Some will be convinced that our model is superior and therefore the conclusions of the state-specific analysis can be accepted. More cautious analysts will be concerned that the problems we have highlighted of data accuracy, model misspecification, endogeneity, and lack of robustness are too severe to confidently assert whether shall issue laws dampen crime, increase crime, or have no overall effect on crime. We share these concerns, especially since the theoretical argument for rising crime across the board in response to shall issue laws is not particularly compelling.<sup>70</sup> Thus, a plausible interpretation of the existing evidence would be that shall issue laws generally appear to increase crime, but that the uncertainty about whether the statistical models are working properly makes it difficult to make any strong claim about the impact, other than to say that it is not so huge that it can overwhelm any defects in the model.

At the end of the day, it is still possible that shall issue laws have no effect – positive or negative – on crime. As discussed above, one reason states like Florida and Texas may have an estimated negative impact is simply that they passed the law because crime was increasing and as crime reverted of its own accord to its normal levels the regression inappropriately attributed the mean reversion to the passage of the law.

An alternative reason why even the best regression results may not be believable is that some states may have by happenstance adopted the law in the mid 1980’s when their crime rate was about to increase by less than the national crime rate (with the introduction of crack pushing up crime sharply in many jurisdictions), while other states adopted the law in the early 90’s when their crime rate was about to decrease by less than the national crime rate (as the criminogenic influence of crack subsided). Essentially, Lott and Mustard are aware that there are unexplained variations each year in the overall crime rate, which are to be controlled for by including a year

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<sup>70</sup> The figures we provide show property crime rising as much in response to shall issue laws as violent crime does (with virtually no evidence of the shifting from violent crime to property crime that was initially posited by Lott and Mustard).

dummy for each year of his model. The idea behind this explanatory variable is that if crime rises X percent in every county in the country owing to some factor that is not picked up by the other explanatory factors, we don't want to attribute this effect to the presence (or absence) of a shall issue law. Instead, we want to find the effect of a shall issue law net of the national influence.<sup>71</sup>

But is there a constant national influence on crime? Figure 4 compares the pattern of murder rates for all 20 states that had not adopted a shall issue law by 1996 with that of the eight states that had such laws before 1977. Prior to 1985, the two patterns move in lock step, even though the shall issue states have a substantially lower level of crime. This portion of the graph supports Lott's use of the year dummies. But after 1985, note how crime rose in the 20 non-shall issue states at a far more precipitous rate than in the shall issue states. The assumption of the constant national effect on crime can cause problems if two conditions apply: 1) the mean national effect varies substantially across counties, and 2) the shall issue laws tend to get passed in states that are less effected by the national upturn in crime that began around 1985.

Figure 4 suggests that both of these factors may hold true.<sup>72</sup> The late 1980s and early 1990s were a time in which crime rose dramatically in areas in which the crack trade proliferated. Other areas where crack did not penetrate saw no such surge in violent crime. One can get Lott-like results in estimating a crime regression if the states that passed the shall issue laws once the crack epidemic got underway were not the states that had the severe crack-induced problem of increased violence. Since crack is an omitted variable that affects crime, its omission from the crime regression will bias the estimated effect of shall issue laws if states where crack was prevalent shunned the shall issue laws. In his book, Lott reported that the states adopting shall issue laws tend to be Republican and have high NRA membership and low crime rates. That doesn't sound like the sort of place where one finds the worst problem with crack. Since the inclusion of the year dummies are premised on an identical nationwide effect on crime in any year, the inability to control for the violence-inducing influence of the local crack trade may well create a serious omitted variable bias problem in all these regressions.<sup>73</sup>

Mis-specifications of this type make it difficult for the researcher to distinguish between the possibility that the law has no effect and the possibility that any effect of the law is beyond the current ability of researchers to identify. Ironically, however, either a "no effect" or "don't know the effect" assessment might be enlisted to argue normatively for adoption of the shall issue law.

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<sup>71</sup> In his second edition, Lott admirably included region-specific fixed year effects for five regions to allow for more heterogeneous year effects for different parts of the country. Lott (170). But Lott's less constrained approach would not be sufficient to solve the crack problem (outlined above) which may very well have played out within the regions.

<sup>72</sup> The graph is merely suggestive since we are really interested in comparing the crime pattern of the rest of the country versus the 24 adopting jurisdictions under the counterfactual that they had not adopted a shall issue law. Moreover, even if we could do the relevant counter-factual comparison, which we can't, there would be nothing wrong with Lott's regression if any different observed crime pattern could be explained by the included explanatory variables.

<sup>73</sup> Indeed, in Ayres & Donohue, *supra* note 7, Figure 1, we graphed the murder rates for all 10 states that adopted shall issue laws during the period from 1977 through 1992, and showed that in only two (Florida and Georgia) of the ten states was there anything like a sustained drop in crime following -- although not necessarily because of -- the passage of the shall issue law. The whole effect of Lott and Mustard's regression thus comes from the fact that the national time dummies are high for the late 1980s and early 1990s and the shall issue dummy reveals that violent crime did not rise as substantially in the shall issue states. If crack caused crime to grow in selected areas of the country and legislatures in states that did not have the crack problem passed shall issue laws (while those with the problem did not pass such laws), then Lott and Mustard's results could simply be the product of omitted variable bias (a view that is buttressed by our Table 3 findings for the period 1991-97).

While assessment 1 would tend to lead consequentialists to oppose the law,<sup>74</sup> the second assessment of (no effect or) ignorance might provide a libertarian ground to support the law.<sup>75</sup>

Those who were swayed by the statistical evidence previously offered by Lott and Mustard to believe the more guns, less crime hypothesis should now be more strongly inclined to accept the even stronger statistical evidence suggesting the crime-inducing effect of shall issue laws. For these individuals, there is still one last assessment that needs to be explored. The last approach takes the state-specific estimates at face value as being the most authoritative and concludes that while shall issue laws generally increase crime, there are some states for which the estimates go the other way. We will now evaluate the normative implications of this heterogeneity assessment. While the first and second assessments militate toward across the board policies, the heterogeneity assessment might counsel toward a more nuanced, piece-meal adoption of the statute.

3. We should simply accept the 24 different jurisdiction-specific estimates, and conclude that shall issue laws increase crime in most states but reduced it in other states.

This is the most speculative of the three conclusions we discuss in that it violates Milton Friedman admonition against accepting statistically significant findings too readily. Nonetheless, anyone who was willing to accept the Lott and Mustard evidence should feel on at least as strong grounds in accepting conclusion 3. One interesting consequence of this conclusion is that it invites the researcher to investigate whether there may be some particular attributes about the small number of states for which crime drops were estimated that diminish the generally harmful effects of shall issue laws enough so that they may actually dampen crime.

On the face of the evidence presented in Table 7, there are a number of problems with accepting that crime fell with the adoption of shall issue laws in even a limited number of states. There is only one state that shows a statistically significant decrease in at least two of the five violent crime categories without showing a statistically significant increase in another violent crime category (Georgia, showing drops in rape and robbery). Florida shows substantial drops in violent crime and murder, but a statistically significant increase in aggravated assaults (all against the background of the potentially confounding influence of the Mariel boatlift), while Oregon shows drops in murder and robbery, but a huge and statistically significant increase in aggravated assaults. Only two other states show a statistically significant drop in any other violent crime category (Texas and Montana saw enormous drops in murder). In other words, it is rare (only 5 out of 24) to see *any* statistically significant evidence of declines in any violent crime category from the adoption of a shall issue law. Certainly, there is nothing to give one confidence that an overall drop in violent crime is likely to be spawned anywhere by the adoption of a shall issue law. Indeed, the rare and seemingly haphazard pattern of statistically significant drops across isolated violent crime categories makes one think of these drops as more random noise than estimates upon which much confidence can be reposed that real effects have been identified. This is underscored

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<sup>74</sup> Lott, himself, is clearly in the consequentialist camp. See Lott (21) (“the ultimate test: does it save lives?”).

<sup>75</sup> There are, however, many reasons why reasonable people who embrace conclusion 2 might nevertheless oppose the statute. Non-consequentialists and expressivists may oppose the law notwithstanding lack of evidence that the law increases crime. Risk aversion or a concern that the law moves society away from a more global maximum might also ground opposition. Finally, if the realization that thousands of citizens were carrying around concealed handguns generated fear or apprehension in the community, one might oppose shall issue laws even if one could not prove that they increased crime. In a world where NRA members have bumper stickers stating “Keep Honking, I’m Reloading,” the costs of intimidation of law-abiding citizens may become intolerable.

by the fact that finding 8 statistically significant drops in crime across the 120 estimates (24 jurisdictions times 5 violent crime categories) in Table 7 looks to be only modestly more than one would expect from a purely random process.

Still, there may be more information captured in the harm-weighted estimate of the total effect on crime for a state, and by assuming that these measures are picking up real information, we can investigate whether there is any pattern that explains which states show overall crime increases and which show decreases. Unfortunately, we have very little data to make this assessment. The county fixed-effects model which we have followed Lott in using to test for the law's impact only allows us to test for the impact of changes in the law within particular counties.<sup>76</sup> But now we are called upon to assess the determinants of the law's impact across the passing states. In essence, we have just 24 observations (23 passing states plus Philadelphia) on which to try to disentangle what caused the law to increase crime in some jurisdiction and decrease it in others.

We regressed various measures of the impact of shall issue laws on a variety of state characteristics. But before reporting the results, let us emphasize that with just 24 observations we are certainly pushing the limits of data. The results that we report are not nearly as robust as our prior findings to the inclusion or exclusion of other variables.<sup>77</sup> Indeed, the data's resistance to explanation can be taken as evidence that notwithstanding our best efforts that the underlying model (the disaggregated hybrid version) is still mis-specified.

With these several caveats about the substantial limits in our data and the residual concerns with mis-specification, we proceed to discuss the impact regressions of Table 10. The dependent variable in the first row regression is the harm weighted percentage impact of the hybrid regression (reported above in Table 8), which ranges from a 23.8 percent *drop* in crime in Montana to a 37.5 percent *jump* in crime in Nevada.<sup>78</sup> We use the following 6 jurisdiction characteristics as explanatory variables: the year the law was adopted, population, log of violent crime rate, density (measured in population per square mile), and two regional dummies (South and West).<sup>79</sup> Looking at the estimated coefficients, we see that later passing states are predicted to have a more deleterious impact – with each additional year adding more than 1.6 percentage points to the estimated impact (prob. = .07). This is huge effect (a law passing 10 years later would be expected to have the crime impact by 16 percentage points higher), and is qualitatively consistent with the aggregate result reported in Table 3 that the law led to an increase in crime in later passing jurisdictions.

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<sup>76</sup> Lott attempted to assess what characteristics were associated with larger or smaller impacts of the law by interacting the law dummy variable with various demographic characteristics of the county (its density, its income, etc.). But this interaction specification in a fixed-effects model only allowed Lott to assess the impact of, say, changes of density *within* particular counties that had passed the law. Since, on average, there is very little within-county variation in density over a twenty-year period, this specification can tell us very little about impact of different densities across different counties or states.

<sup>77</sup> And the fact that we ran a number of alternative specifications that we did not report suggest a “pre-testing” effect that should lead us to discount the nominal levels of significance reported in the regression.

<sup>78</sup> The dispersion in the estimated effects of the 24 shall issue laws is quite wide, which in itself shows that there is a considerable degree of noise in the estimate of any particular jurisdiction. As Black and Nagin comment in finding substantial dispersion in state-specific estimates for the 10 states adopting shall issue laws during the 1977-92 period: “Widely varying estimates such as these are classic evidence that, even beyond the assumption of homogeneous impacts across states, the model is misspecified.” Dan A. Black & Daniel S. Nagin, “Do Right-To-Carry Law Deter Violent Crime?” 27 *Journal of Legal Studies* 209, 214 (1998).

<sup>79</sup> There were no Midwest states that passed the law during this twenty-year period (and the Northeast region was the excluded attribute).

The harm-weighted estimates that we use as regressors have the advantage of aggregating the individual crime category impacts in a natural way that weights the individual crime estimates commensurate with their underlying importance. But a disadvantage of the harm-weighted estimates is that they ignore the varying significance of the individual crime impact estimates. Returning to Figure 2 (which depicts the disaggregated dummy variable model estimates for murder from Table 6), one can readily identify the positive correlation between the estimated impact of the law on the murder rate and the year in which the law was adopted. We see that 13 of the final 14 passing states had estimated increases in murder (and 7 of these were statistically significant), while only 3 out of the first 10 passing states posted estimated increase in murder (and only 1 of these was statistically significant).<sup>80</sup> More generally, Table 8 shows that 13 of the last 14 passing jurisdictions experienced *increases* in violent crime.

If one accepts the accuracy of the jurisdiction-specific estimates, one might interpret this cohort result as a kind of “Peter Principal” applied to law. As a pop theory of job advancement, the “Peter Principal” asserts that employees are promoted to jobs requiring successively higher skills until they reach a job where they are relatively incompetent. Analogously, the data indicates that the law has performed less well – rising to the level of its incompetence – as successive states have chosen to adopt it. On the other hand, the temporal pattern that states that adopted shall issue laws in the late 1980s while those adopting in the 1990s did worse may simply reflect the influence of a time-varying factor (perhaps the crack trade) that caused sharp rises in crime for many states in the late 1980s and then greater than average price declines in the 1990s.

The regressions, however, do at least suggest some non-temporal traits that are associated with the estimated impact. Passing jurisdictions with larger populations had more beneficial impacts and this correlation was strongly significant (prob. = .000), while less dense jurisdictions and jurisdictions starting with higher base levels of violent crime had more pernicious impacts (and these effects were significant at the 10% level).<sup>81</sup> The negative association between state population and the estimated dollar impacts (estimated in Table 8) is dramatically illustrated in Figure 5 – with the two largest states (Texas and Florida) having by far the largest harm-weighted dollar drops in crime. This should not be surprising, though, since a given percentage change in crime will have a bigger dollar impact in a large (or higher-crime) state. Finally, western states tended to be associated with better crime outcomes, and this effect was statistically significant.<sup>82</sup> While there are always some “cultural” rationales that we could offer ex post for these effects, we should emphasize that these results are suspect as they are the byproduct of reduced form regressions rather than growing out of a priori theory.

Note that if the data were rich enough, it would be possible to use the results of these regressions to predict out of sample the expected impact of the law on the jurisdictions that either have never passed a shall issue statute or had already passed the statute before our data began in

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<sup>80</sup> In his second edition, Lott attributes the diminishing beneficial impact of the law in later passing jurisdiction to heightened fees and training requirements that were imposed on permit applicants in the later passing states. But while the imposition of greater obstacles could have explained a diminished beneficial trend, it does not explain why we find that later passing states generally experienced increases in crime. It also does not explain why Texas (which Lott notes requires 10 hours of training and charges the highest fee in the sample) was one of the great outliers in generating a beneficial impact. We did not have available the fee and training data that Lott used, and hence did not control for these attributes in the regressions reported in Table 10.

<sup>81</sup> While potentially interesting, it is not evident that any theory could support this empirical finding.

<sup>82</sup> In the remaining columns of Table 10, we replicate the regression of the first column by regressing alternative measures for the impact of crime coming from the murder category and from the static regressions. One sees that the patterns of sign and statistical significance are generally consistent.

1977.<sup>83</sup> Indeed, Appendix Table 5 reports the predicted impact (as well as the standard deviation of the prediction) of passing the law today in all 52 jurisdictions (including Philadelphia and the District of Columbia) -- given the jurisdiction's underlying characteristics. While this predictive process is based on extremely limited data, it may be useful to illustrate the possibility of more nuanced state-specific policy recommendations in contrast to the across the board recommendations to adopt or repeal that are the only possible product of the type of aggregated analysis that Lott championed.

As one might suspect the predictions produced heterogeneous results – which we break into four categories:

Jurisdictions in which adoption is strongly counterindicated. We found 34 jurisdictions in which adoption of the law predicted an annual percentage increase in the harm-weighted measure of crime that was more than twice the prediction's estimated standard deviation. 17 of these jurisdictions had already adopted the law and thus would need to repeal the law to avoid the deleterious effect, while 15 of these states would merely need to refrain adopting the law in the future.

Jurisdictions in which adoption is strongly indicated. We found just two states (Texas and California) in which adoption of the law predicted an annual percentage *decrease* in the law that was more than twice the prediction's estimated standard deviation. One of these jurisdictions (California) would need to adopt the law to secure its benefits, while the other (Texas) would need to merely retain the law that it had previously adopted.<sup>84</sup>

Jurisdictions in which adoption is weakly counterindicated. We identified 13 jurisdictions in which adoption of the law predicted an annual percentage increases in crime, but were not statistically significant at the 5% level. Risk-aversion would probably counsel against passing (or retaining) shall issue laws in these jurisdictions. Under this normative reckoning, 9 of these jurisdictions had previously passed the law and thus would be targets for repeal, while 4 should simply continue to refrain from adopting the law.

Jurisdictions in which adoption is weakly indicated. Finally, we identified 5 jurisdictions in which the predicted impact of the law on a harm weighted measure of crime was beneficial but not statistically so. A libertarian would argue that these five states – which all previously passed the law – should retain them.

On net, this analysis suggests the following legislative (in)action: 6 states should retain their current shall issue laws; 1 state (California) should adopt the law for the first time; 26 states should repeal the law; and 19 states should continue to refrain from adopting.<sup>85</sup>

For all of the reasons set forth above, this analysis can only be taken as suggestive of the type of nuanced policy recommendations that are possible from statistical analysis of state-specific

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<sup>83</sup> Lott undertook an analogous prediction procedure when he uses a first-stage regression run on ten states to predict how many permits would be issued in other states. Lott (176).

<sup>84</sup> While crime did drop as the Texas shall-issue law went into effect on January 1, 1996, California has never adopted a shall issue law, so the prediction is based on our weakly predictive model with 24 data points. But while California is shown by these tentative impact regressions to be a possible candidate for enactment of a shall issue law, its experience of showing dramatic crime declines in the 1990s, both absolutely and vis a vis shall issue states, suggests another reason for caution before accepting the predictions of these regressions.

<sup>85</sup> Lott's own analysis suggests that issuing an unlimited number of permits may produce a pernicious impact on crime. In his second edition, Lott estimated, in an aggregate specification, that when concealed handgun permits exceeded a maximum percentage of the population, additional permits would be predicted to increase crime. Lott (178-80). If Lott is right on this point, a state that passed the law under the foregoing normative account might be advised to cap the maximum number of permits issued.

estimates of the impact of the law. Nonetheless, we think it can provide a useful blueprint for researchers who have rich enough data and well-specified state-specific regression results to be able to make more nuanced policy recommendations than are possible from the typical aggregated analysis.

## Conclusion

Judge Richard Posner has recently criticized moral philosophy for failing to persuade on any contentious issue.<sup>86</sup> But a similar criticism might be made of quantitative empiricism. Readers might tend to only accept quantitative analyses that resonate with their prior normative beliefs. Indeed, Judge Stephen Reinhardt famously proclaimed at a Yale seminar that social science had never affected his judicial decisionmaking. And Donald Braman and Dan Kahan have recently called upon econometricians like Lott and us to put away our statistical packages. In a piece, provocatively titled, “More Statistics, Less Persuasion,” Braman and Kahan argue that rather than “quantifying the impact of gun control laws on crime,” academics “should dedicate themselves to constructing a new expressive idiom that will allow citizens to debate the cultural issues that divide them.”<sup>87</sup>

We disagree. A body of empiricism can over time disentangle thorny issues of causation and lead toward consensus. We view this paper as playing a role in this process (not ending the conversation). On net, we believe that Lott and Mustard’s initial efforts made an important contribution to the literature. They amassed an important new panel data set and subjected it to state-of-the-art modeling (and indeed their data set, which we know from experience was quite costly to construct, has been used by many researchers to explore this and other questions about crime). Unfortunately, their results have not withstood the test of time. When we added five years of data, allowing us to test an additional 14 jurisdictions that adopted shall issue laws, the previous Lott and Mustard findings proved not to be robust. Indeed, if one only looked at the 15 jurisdictions that adopted shall issue laws between 1991 and 1997, the statistical estimates suggest uniform *increases* in crime. Looking at the entire period from 1977-97 yields less clear results for the aggregated data, yet shall issue laws are uniformly associated with higher violent crime and aggravated assault in county data (Tables 2 and 4), although not in state data (Appendix Table 3). Importantly, we show that the Lott and Mustard results collapse when subjected to less constrained jurisdiction-specific specifications estimated on the more complete data: no longer can any plausible case be made on statistical grounds that shall-issue laws are likely to reduce crime for all or even most states. How much farther one can go in arguing that shall issue laws likely increase crime across the board or have heterogeneous effects across states (albeit most commonly pernicious) will be matters about which various analysts will differ.

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<sup>86</sup> Richard A. Posner, *The Problematics of Moral and Legal Theory* (1999).

<sup>87</sup> Donald Braman & Dan M. Kahan, *More Statistics, Less Persuasion: A Cultural Theory of Gun-Risk Perceptions at 1* (Working Paper 2002).