THE EFFECT OF CONCEALED WEAPONS LAWS: AN EXTREME BOUND ANALYSIS

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Lott and Mustard [1997] provide evidence that enactment of concealed handgun ("right-to-carry") laws deters violent crime and induces substitution into property crime. A critique by Black and Nagin [1998] questions the particular model specification used in the empirical analysis. In this paper, we estimate the "model uncertainty" surrounding the model specified by Lott and Mustard using an extreme bound analysis (Leamer [1983]). We find that the deterrence results are robust enough to make them difficult to dismiss as unfounded, particularly those findings about the change in violent crime trends. The substitution effects are not robust with respect to different model specifications. (*JEL* K42)

I. INTRODUCTION

In a recent paper, Lott and Mustard [1997] provide evidence on the relationship between concealed handgun laws (often called "right-to-carry" or "shall issue" laws) and crime. They examine 16 years (1977–1992) of county-level crime data from the FBI's Uniform Crime Reports (UCR) and compare crime rates before and after the introduction of right-to-carry laws, controlling for demographic factors and county-level arrest rates. They find that introducing right-to-carry laws resulted in significant reductions in the violent crimes of murder, rape, aggravated assault and robbery, and increases in some property crimes such as burglary, larceny and auto theft. The latter increase is attributed to a "substitution effect," suggesting that potential criminals will (at the margin) switch from violent crimes that have now become more risky to them, to property crimes where the probability of encountering an armed victim is much lower. Comparing these two effects (coupled with a slight increase in accidental gunshots from individuals owning concealed handguns), Lott and Mustard conclude that the social benefits of these laws exceed their costs.

Economists who present one set of empirical results are always vulnerable to criticism that they could have selected another group of right hand side variables or modeled them in a different way. Lott and Mustard's analysis has been criticized by Black and Nagin [1998], who re-examine the data set choosing a different set of controls and a different model and reach different conclusions about the effects of the right-to-carry laws. Uncertainty about what to control for and how to do it has been called "model uncertainty" by Leamer [1983] who suggests estimating the size of model uncertainty by testing the sensitivity of the particular results to many different possible model specifications. This methodology is called "extreme bound analysis."

In this paper, we report extreme bounds from nearly 20,000 estimated regressions that vary the modeling choices made by Lott and Mustard. We find support for the deterrence result from a broad set of specifications. In

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ABBREVIATION
UCR: Uniform Crime Report

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particular, we find strong support for the hypothesis that the right-to-carry laws are associated with a decrease in the trend in violent crime rates. We do not find much support for a substitution effect into property crimes. In what follows, we first describe some econometric issues related to the problem of estimating the effect of the right-to-carry laws on crime rates. We then report our extreme bound analysis and offer concluding remarks.

II. EXTREME BOUND ANALYSIS

Although behavioral theories can provide general guidance about the kinds of factors that should influence the crime rate, they do not specifically determine which variables should be included in a regression analysis. In addition, conflicting theories typically suggest alternate model specifications.

In the context of crime rates, for example, we might expect higher crime rates in counties with higher unemployment and a larger percentage of young men age 18–30. Following convention, we call this a "supply" effect (Nagin [1978]). However, it is also possible that higher unemployment leads to the passage of right-to-carry laws, or that right-to-carry laws are less likely to be enacted in areas with high percentages of young men age 18–30. We call this a "demand" effect. Omitted or unobserved demand or supply variables may induce a spurious correlation between the adoption of the right-to-carry laws and crime rates that has nothing to do with the supply of crime.

Leamer [1983] suggests a formal specification search, or "extreme bound" analysis, to estimate the size of such "model uncertainty" surrounding certain regression results. For example, by including different right hand side variables in a supply-of-crime equation for murder, Leamer estimates an extreme bound interval for the deterrent effect of capital punishment that ranges from +29 to −12 murders prevented for every execution. Our goal is to put similar extreme bound intervals around the deterrence results of Lott and Mustard.

Criticism of the extreme bound analysis has focused on the possibility of excluding a variable even though it really is a significant explanatory factor (McAleer, Pagan and Volker [1985]). As Ehrlich and Liu [1997] note, one could inadvertently exclude a variable that was "jointly significant statistically and highly correlated" with the variable of interest (in this case, the right-to-carry law). This would lead to overly wide extreme bounds around the factor and a mistaken inference that the deterrent effect is "fragile" even when it is not. This occurs when a researcher mistakenly classifies a variable as "doubtful" instead of "important" or "free" (McAleer, Pagan and Volker [1985]). Without trying to enter the Bayesian/Classical debate, we acknowledge these difficulties, but suggest that a systematic specification search, like extreme bound analysis, can at least help put debates about model specification into perspective. This specification search is meant to be as exhaustive as possible, but as noted above, it can lead to overly wide extreme bound intervals if some of the variables are misclassified. In addition, we note the obvious criticism that extreme bound analysis only deals with model specification and is not designed to address many of the potential violations of the classical regression model assumptions.

We classify all right hand side variables as doubtful, except for the county and time dummies and the variable of interest—"right-to-carry" laws. We always include the dummies in the extreme bound analysis because the fixed effects regression approach mitigates the bias problems caused by omitted variables correlated with the passage of gun laws. The within-county estimator, which follows from the use of county dummies, uses each county as a control for itself (before and after the gun laws) which eliminates the bias induced by between-county variation in omitted or unobservable factors (e.g., Mundlak [1961]; Hausman, [1978]). To control for nationwide trends that could drive the results, we also always include year dummies. Note that the use of county and year dummies prevents us from testing some of the alternate specifications used by Black and Nagin [1998].

III. RESULTS

We begin with Lott and Mustard's original county-level data set for the time period 1977–1992, and replicate their main deterrence result (Lott and Mustard [1997, Table 3]) for the crimes of murder, rape, aggravated assault, robbery, burglary, larceny, auto theft, and the combined categories of violent crime and property crime. The dependent variables are the natural log of the county-level crime rates. Right hand side variables include a series of demographic variables (age, gender, income, etc.), the arrest rate (as a proxy for the level of expected punishment), year and county-level dummies, and a dummy variable to account for time periods when the right-to-carry laws were in force. A complete listing of the variables used can be found in Lott and Mustard [1997, Table 2].

Some of the right hand side variables may be endogenous. Right-to-carry laws may be enacted in states that have had a recent growth in crime and where other attempts to reduce crime have simultaneously been instituted (e.g., increased police hiring or higher arrest rates). Arrest rates—which are included partly to overcome this problem—might also be endogenous for the same reason. Lott and Mustard use instrumental variables techniques to examine this issue. Since the instrumental variables estimates also find a deterrent effect, we restrict our attention to the modeling choices implied by the choice of various right hand side variables using the simpler OLS specification.

Each of Lott and Mustard's nine regression equations (taken from their Table 3) are re-estimated with different right hand side variables by systematically removing and adding groups of right hand side variables thought to have an effect on crime rates. We group Lott and Mustard's right hand side variables (see Lott and Mustard's Table 2 for summary statistics on these variables) into several overlapping categories based on a common demographic factor, like age, race or gender. We choose ten categories of variables in the hope of identifying a variable or set of related variables whose inclusion or exclusion can account for the different results found by researchers studying this question:

(1) county population;

(2) population density per square mile;

(3) arrest rate;

(4) a set of poverty variables (e.g., income, unemployment, income maintenance expenditures, and retirement payments);

(5) percentage of population black;[1]

(6) percentage of population white;

(7) percentage of population under age 30;

(8) percentage of population over age 30;

(9) percentage of population male; and

(10) percentage of population female.

The ten variable groups lead to 1024 different model specifications ($2^{10} = 1024$) for each crime category, a total of 9,216 regressions.

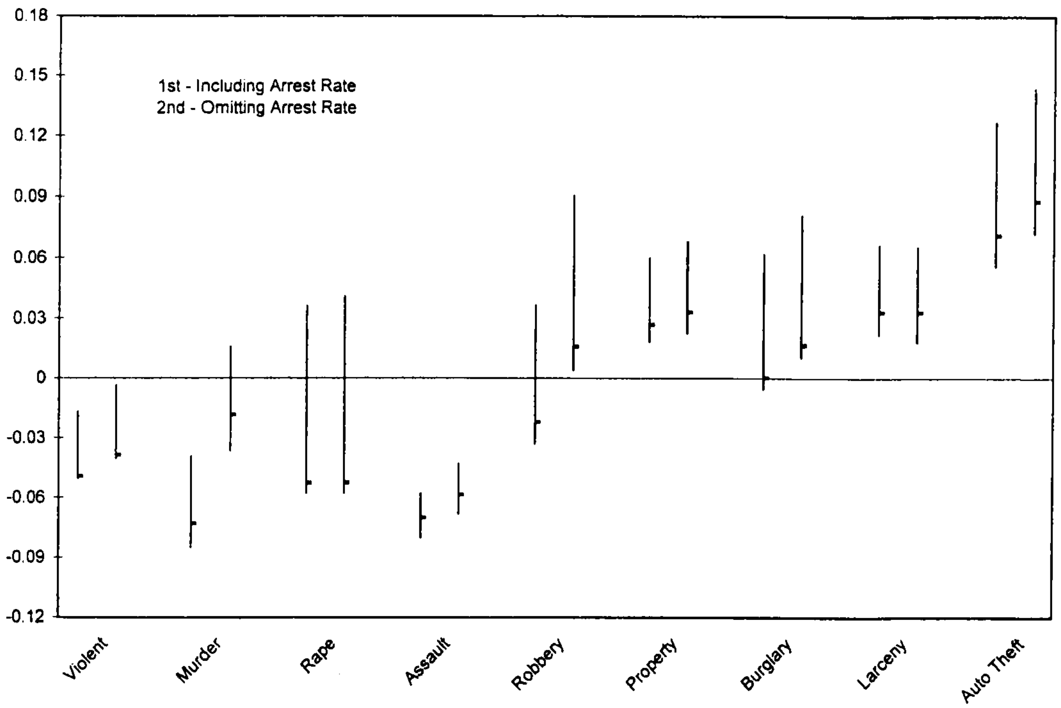
We report these results graphically in Figure 1. The original Lott and Mustard results (Lott and Mustard [1997, Table 3]) are shown as a small rectangle inside an estimated extreme bound interval indicating the maximum and minimum coefficients for the "right-to-carry" dummy. The extreme bound interval for all 1024 regressions is the union of the two smaller extreme bound intervals, computed by including and excluding the arrest rate, for each crime type. The passage of a right-to-carry law coincides with a decrease in the categories of violent crime and assault and increase in property crime, larceny and auto theft. The extreme bound interval includes zero (no effect) for murder, robbery and rape. Only the results for aggravated assault (and the combined category of violent crimes)[2] are less than zero for all models. For property crimes, the extreme bound interval for burglary includes zero.

One concern with the original Lott-Mustard results is that an important explanatory variable—arrest rate—is likely to be endogenous and is missing in counties where there are no crimes. Lott and Mustard [1997] and Lott [1998] address this issue by limiting their sample to larger counties (where the arrest rates are usually positive), and by replacing the arrest rate with instrumental variables. Black and Nagin [1998] also suggest eliminating small counties. We investigate the effects

1. The last six demographic variables are each composites of several other variables in Lott-Mustard. For example, the percent population black includes 12 combinations of male/female by age category.

2. Although one might only be interested in violent crime as a category (since many robberies and assaults end up as murders), the violent crime category is dominated by aggravated assaults. Thus, to the extent that the causes of robbery, assault and murder differ, it is worthwhile to look at individual crime types.

FIGURE 1
Range of Coefficients of Right-to-Carry Dummy for Full Sample

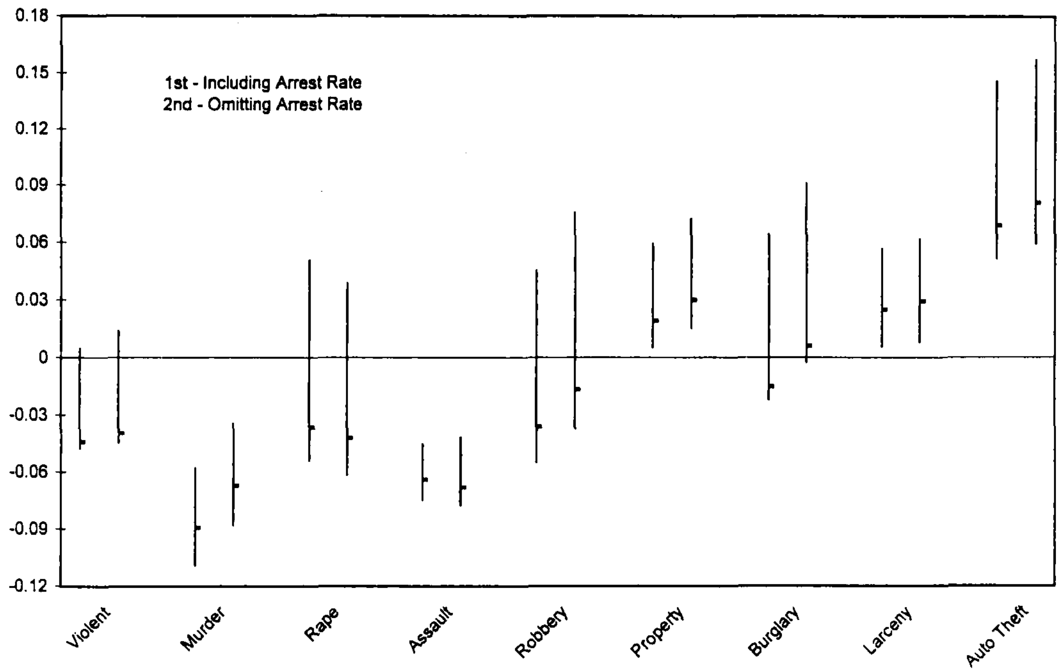


of the arrest rate variable by conducting sensitivity analysis on the arrest rate variable, and by limiting attention to a restricted sample, those counties with population over 100,000, where the arrest rate is almost always defined.

In Figures 1 (all counties) and 2 (large counties only), the estimated extreme bound interval for the full set of 1024 different specifications is plotted on the vertical axis. The units measure the percentage change in the crime rates following enactment of the laws. In addition, the extreme bound interval is "split" into two pieces, 512 regressions that include the arrest rate and 512 that exclude the arrest rate. The full extreme bound interval is the union of the two smaller extreme bound intervals. Inclusion of the arrest rate has an effect only in the case of murder in the full sample (with the smaller counties). In Figure 1, including the arrest rate in the murder regressions always results in a negative right-to-carry coefficient, while excluding it reduces the magnitude of the right-to-carry coefficient and causes it to cross zero. The big effect of the arrest rate on the murder coefficient can be explained by the large increase in sample size, from about 26,000 cases to 47,000 cases when the arrest rate is omitted. More counties experience zero murders, and thus have an undefined arrest rate, than for any other crime. The effect of a change in sample size is confirmed in Figure 2 (large counties), where the extreme bound interval for murder lies below zero, regardless of whether the arrest rate is included or not. The rest of the results in Figure 2 are qualitatively similar to those in Figure 1. As before, the extreme bound interval for aggravated assault lies below zero.

Figures 1 and 2 also include small rectangles indicating the location of the original Lott-Mustard [1997, Table 3] results within the extreme bound interval. In the first subset of 512 regressions, the rectangle represents the actual Lott-Mustard specification, while in

FIGURE 2
Range of Coefficients of Right-to-Carry Dummy for Sample Restricted to Counties with 100,000+ Population



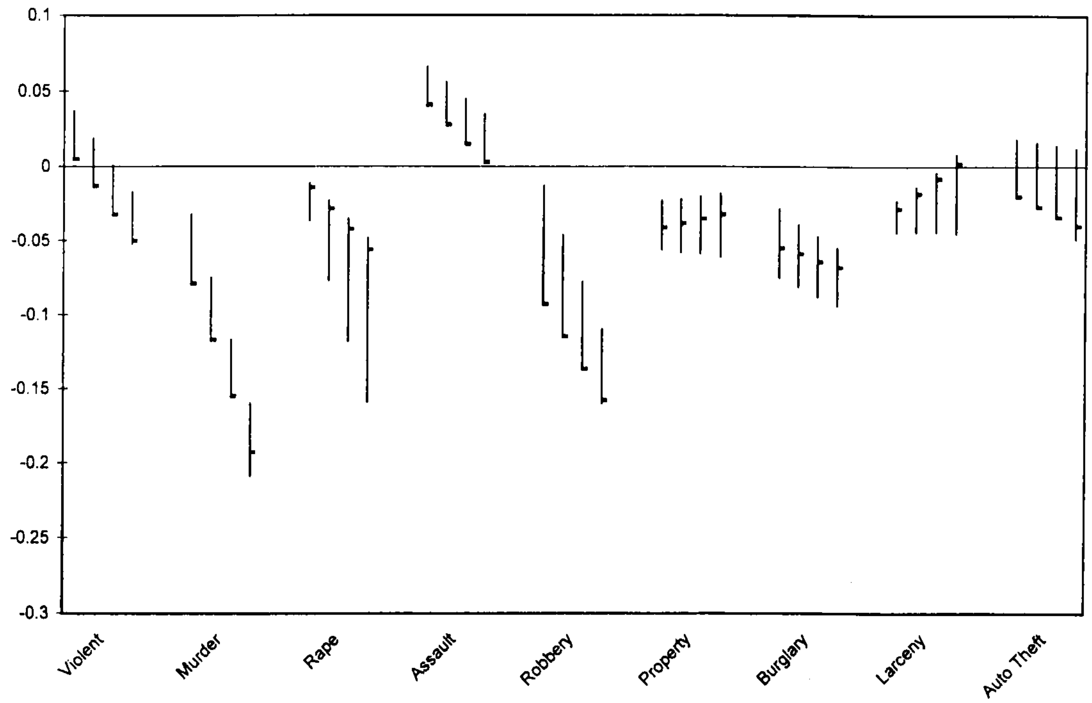
the second subset, it is a model that excludes only the arrest rate variable.

Another important modeling choice concerns the timing of the crime reduction benefits following adoption of the right-to-carry laws. Although Table 3 of Lott and Mustard [1997] restricts the model to a one-time change in the crime rate (a shift in the intercept), further refinements in Lott and Mustard [1997] and Lott [1998] allow the effects of right-to-carry laws to vary over time, with the full effect not being realized for several years. Assuming that the enactment of these laws deters criminals (especially violent offenders with the greatest probability of encountering armed victims), we might expect the effects to be magnified over time as more permits are issued. Black and Nagin [1998] also use a model with a time-varying impact.

To capture this effect, we permit both the intercept and trend of the supply of crime equation to shift after enactment of the right-to-carry laws. In particular, we introduce two new variables—"before" and "after" trends that are measured relative to the year of enactment—in addition to the right-to-carry dummy. This approach is similar—but not identical to—Black and Nagin [1998], who test the Lott-Mustard findings by utilizing an additional set of year dummies corresponding to the number of years either before or after enactment of the laws. In this way, we go beyond the simple model of Figures 1 and 2, where dynamic trends are ignored. As Lott [1998] notes, if crime was increasing prior to enactment of the right-to-carry laws and they have a deterrent effect reducing crime, then a model that includes only a shift parameter might fail to pick up this effect.

For these regressions, we consolidate the demographic variables into one group to include or remove from the regressions. The individual demographic variable groups used in Figures 1 and 2 had virtually no impact on the estimated extreme bound. This consolidation allows us to conduct our sensitivity analysis

FIGURE 3
Simulated Differences in Crime Rates for Four Years After Enactment of Right-to-Carry for Full Sample



with only five groups of variables to be included or omitted, for a total of 32 (2^5) regressions for each crime category.[3] We investigate the model uncertainty around the two new time trend variables.

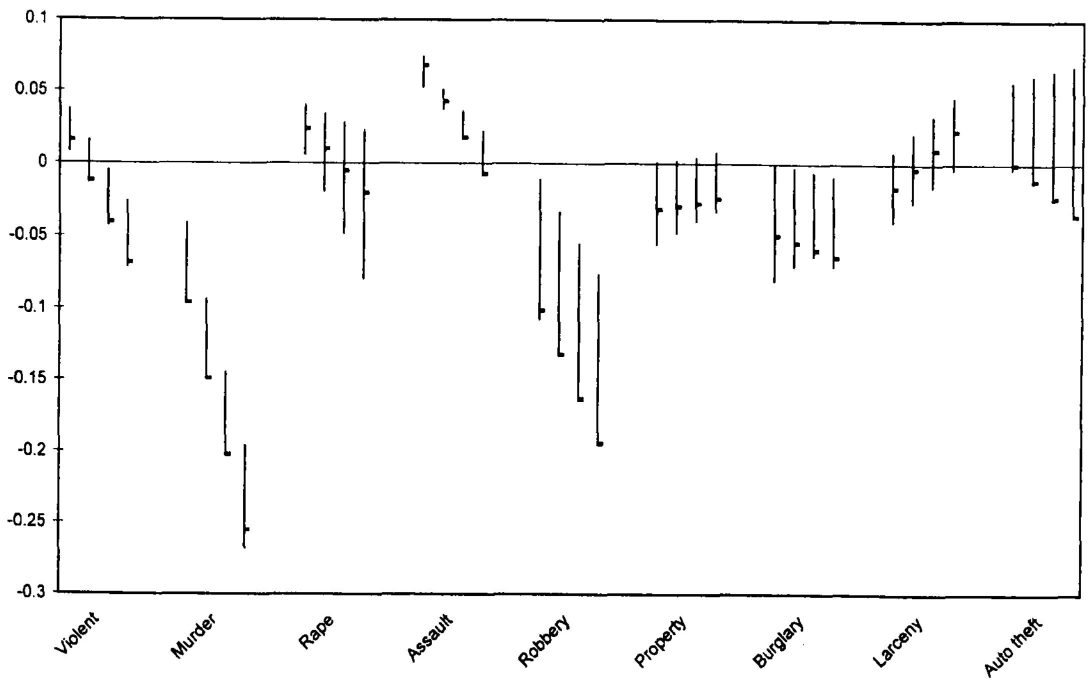
Figures 3 (full sample) and 4 (large counties only) illustrate the results for violent and property crimes using the time trend variables. We simulate changes in the crime rates following enactment of the laws by comparing predicted crime rates if no law is enacted to predicted crime rates when right-to-carry laws are in effect. We plot extreme bound intervals around these simulated differences. The shaded rectangles in Figures 3 and 4 correspond to the year-by-year difference in trends implied by the original Lott-Mustard Table 3 specification. We follow this trend from year +1 to year +4 after enactment.[4] The units are the simulated percentage changes in crime rates following enactment. Note that these are not cumulative results, like those reported in Black and Nagin [1998].

As shown in Figure 3, the effect of enactment of right-to-carry laws on murder, rape and robbery is negative. Particularly evident is the shift in the trend variable for the violent crimes (the extreme bound intervals shift down following enactment). For aggravated assault, the net effect (the effect of the intercept shift plus the trend shift) is slightly positive in the first few years following enactment, but not significantly different from zero after the fourth year. For violent crimes as a whole, there is a slight jump in year one and then a net decline after four years when the extreme bound interval does not contain zero. In Figure 4, which presents results for the re-

3. We recreated the original sets of regressions with this reduced set and find no significant difference in the maximum and minimum coefficients compared to those in Figures 1 and 2.

4. Our data span a time period as long as 14 years prior to and seven years following enactment of the "right-to-carry" laws. Only a few observations, however, exceed four years following enactment.

FIGURE 4
Simulated Differences in Crime Rates for Four Years After Enactment of Right-to-Carry for Restricted Sample



stricted sample, the net effect of enactment is negative for murder and robbery, but not for aggravated assault or rape. The effect is negative for violent crimes as a whole only after the third year following enactment. Although all crimes exhibit a shift in the trend rate of crime following enactment, the net effect is significantly negative (the extreme bound interval does not include zero) for only murder and robbery.

In Figures 3 and 4, we also present the results for property crimes on both the full and restricted samples. Unlike violent crimes, there is no discernible shift in crime trends. The trend variables seem weak compared to the shift in the intercept, but the property crimes exhibit no consistent substitution effect. In the full sample, property crimes as a whole and burglary rates shift down following enactment, but there is no net effect for auto theft and larceny. In the restricted sample, only burglary has an extreme bound interval that excludes zero.

Results using the trend specification suggest that enactment of the right-to-carry laws is associated with a shift in violent crime trends. There is no corresponding positive shift in property crime trends or levels. The shift in crime trend leads to an immediate reduction in murder and robbery rates, but the extreme bound intervals on the net effect (shift in intercept plus shift in trend) includes zero for the other violent crime categories. This lag is consistent with the reported timing of concealed handgun purchases following enactment of right-to-carry laws (see Lott and Mustard [1997]).

IV. CONCLUDING REMARKS

We have systematically estimated the model uncertainty surrounding the effects of the passage of right-to-carry laws on crime rates (Leamer [1985]). Our study has paid particular attention to the concerns raised by Black and Nagin [1998] surrounding large vs.

small counties, inclusion of the arrest rate, and the timing of the effects of right-to-carry laws. Although the extreme bound approach has its limitations—it is only dealing with model specification and is probably biased towards finding no effects of the laws—it can help frame the debate surrounding model specification.

In the case of the "right-to-carry" concealed handgun laws, we show that model uncertainty does exist, but the deterrence results are robust enough to make them difficult to dismiss as unfounded or contrived, particularly those findings about the change in violent crime trends. Thus, we cannot rule out the possibility that potential offenders are deterred by the prospect of confronting a victim who has a concealed handgun. The substitution results, i.e. the increase in property crimes, are not robust with respect to different model specifications.

Our analysis ignores many of the other modeling and data availability issues surrounding the right-to-carry debate. Lott and Mustard [1997] deal with many of these issues including the potential endogeneity of arrest rates, missing observations, and confounding events such as other gun-related laws in individual states. Others will no doubt comment on these refinements and provide alternative data sets to analyze. The debate over the effect of right-to-carry laws on crime has become a heated policy issue and will continue to foster more research in this area. As in most areas of empirical research, one study is seldom adequate to draw strong policy implications. Over time, we expect a body of literature to develop and ultimately lead to some resolution of which side of the debate is correct. Our piece of this puzzle, however, suggests that the model specified in the original Lott-Mustard paper cannot be dismissed outright.

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