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The effect of concealed handgun laws on crime: beyond the dummy variables

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Abstract

So far 33 states have adopted right-to-carry concealed handgun laws. The advocates argue these laws have a deterrent effect on crime, while the opponents believe they facilitate crime by increasing gun availability. Although both sides assume that these laws affect behavior, no attempt has yet been made to model such effects using crime theory. Consequently, the empirical evidence on such effects lack a theoretical basis; for example, a highly publicized study by Lott and Mustard (1997) inappropriately models the effect of the law through a dummy variable (a binary-valued regressor). We extend the economic model of crime to formulate a theoretical basis for empirical examination of the issue. We show that using a dummy variable leads to misspecification, and use an alternative procedure to estimate the effect of concealed handgun laws in 1992 for states which had not yet adopted such laws. Our results show that the expected effect of the law on crime varies across the counties and states and depends on county-specific characteristics in a meaningful way. Such effects appear to be much smaller and more mixed than Lott and Mustard suggest, and are not crime-reducing in most cases.

Keywords: Concealed handgun laws; Crime; Dummy variables; Econometric specification

1. Introduction

The right-to-carry concealed handgun laws—“shall issue” laws—and their possible effects on crime have been the subject of extensive policy and academic debate as more states adopt such laws.[1] From 1977 to 1992, 10 states passed such laws making it much easier to obtain licenses to carry concealed handguns, and 15 states adopted this law between 1992 and 2001. These laws are at odds with the federal Brady Bill, which is restrictive of gun

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[1] Henceforth, we refer to such provisions as “concealed handgun” laws. These laws are also referred to as “shall issue” laws (laws mandating that authorities “shall issue” permits to carry concealed handguns).

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ownership, reflecting the conflict among various levels of government regarding the role of handguns in violence. Such conflict also extends to academic circles. Some argue the concealed handgun laws increase criminals' access to guns through theft, overpowering victims, or the black market. This leads to a civil arms race which can only increase crime (Cook, 1991; Cook & Leitzel, 1996; Cook & Ludwig, 1996; Hemenway, 1997; Kellermann, Westohal, Fischer, & Harvard, 1995; Ludwig, 1998; McDowall, Loftin, & Wiersema, 1995). We call this outcome the "facilitating effect" of concealed handgun laws. The supporters of these laws dispute the facilitating effect, maintaining that the effect is opposite. They argue that allowing citizens to carry firearms will increase criminals' uncertainty regarding an armed response, thus leading to less crime—the "deterrence effect" (Kleck & Patterson, 1993; Lott, 1998; Lott & Mustard, 1997; Polsby, 1994, 1995).

No study has formalized the above arguments theoretically. Such a theoretical basis is necessary for any empirical investigation of the issue. In this paper we formalize these arguments in the context of the economic model of crime. We demonstrate that the direction and magnitude of any resulting change would depend on the parameters of the criminal's optimization function and the characteristics of the individual and his social and economic setting. This means that any change in crime rate induced by concealed handgun laws will depend on demographic, social, and economic specifities of the observation units (e.g. counties). Thus, these laws might lead to increases in crime in some jurisdictions and decreases in others. For example, one would expect the effect of the law on crime to be more pronounced in more populated counties, because authorities who have discretion over issuing handgun-carrying permits in absence of a concealed handgun law are the most restrictive in these counties. The largest changes in handgun density as the result of such laws are therefore expected in populated counties. Moreover, since the law excludes juveniles from receiving gun-carrying permits, the deterrent effect is expected to be smaller in counties with a younger population. Other demographic determinants of propensity to carry a concealed weapon may lead to similar differential effects.

We empirically examine the effect of concealed handgun laws on crime drawing on the aforementioned theoretical considerations. Accordingly, we allow the effect of the law on crime to be a function of population characteristics in a given jurisdiction, so that we can infer how various factors influence the magnitude of the change in crime resulting from these laws. More specifically, we project what the 1992 crime rate for counties without such a law would have been if the county had adopted such a law by 1992. We then compare these projections, which are a function of county characteristics, with actual crime data for each county in 1992 to infer how the absence of the law has affected crime in these counties. We also examine the relationship between these projected changes and county characteristics.[2]

Ignoring specific population characteristics when modeling the effect of the law leads to model misspecification and invalid inference. For example, in a highly publicized study, Lott and Mustard (1997) use a dummy variable to model the effect of the law as a shift in

[2] We use Lott and Mustard's data which covers 3054 counties for the period 1977–1992 and includes series on various categories of crime and arrest rates and economic, demographic, and political variables. The data set allows us to exploit cross-county heterogeneities, while our theory-based empirical procedure allows us to make state level inference about the potential effect of the law.

the intercept of the linear crime equation they estimate.[3] The method is predicated on two assumptions: (1) all behavioral (response) parameters of this equation (slope coefficients) are fixed—unaffected by the law and (2) the effect of the law on crime is identical across counties. We demonstrate that these assumptions can be rejected both on theoretical and empirical grounds.[4] Our procedure is intended to overcome this shortcoming.

The remaining sections are organized as follows: Section 2 elaborates on the stated effects of the concealed handgun laws and extends the economic model of crime to examine such effects. Section 3 discusses the estimation issues involved in measuring the effect of these laws and the problems with using dummy variables for this purpose. This section also presents an alternative estimation procedure that draws on the theoretical considerations discussed in Section 2. Section 4 describes the data and presents and discusses the results. Section 5 contains concluding remarks.

2. Concealed handgun laws and the economic model of crime

Thirty-three states have so far adopted concealed handgun laws.[5] These laws require that permits to carry concealed handguns be granted to any adult applicant unless the individual has a criminal record or a history of serious mental illness. Prior to adopting these laws, local authorities had discretion in granting such permits on a case-by-case basis, and the most populated counties were the most restrictive in issuing such permits (Lott & Mustard, 1997).

The supporters of concealed handgun laws argue that allowing law-abiding citizens to carry concealed handguns increases the overall security by deterring attackers. Since the firearms are concealed, predators do not know a priori which potential victims or bystanders might be armed. The armed citizens, therefore, not only enhance their own security but also provide a positive externality for unarmed citizens. The resulting uncertainty increases the criminal's perceived failure probability, leading to a lower expected net benefit from a criminal act and, therefore, to a lower crime rate.

The opponents argue that these laws are likely to increase the crime rate. For example, Cook and Leitzel (1996) note that only a small percentage of felons and youths use the primary market to acquire their handguns; the rest rely on friends, theft, or on street transactions. Through these channels, concealed handgun laws may increase the number of guns available to criminals. Criminals can also use their victims' guns against them, when victims are not able to use their guns effectively (Kellermann et al., 1995). Overall, these authors believe that increased gun availability lowers the criminals' cost of illegally obtaining firearms, prompting their substitution for less lethal weapons in hostile confrontations. This, in turn, leads to an increase in crime rates (Ludwig, 1998).

Both arguments imply that the net change in expected benefit from committing crime is the causal link between concealed handgun laws and crime rates. The direction and

[3] Lott and Mustard report that passage of concealed handgun laws by a state causes a significant reduction in violent as well as property crime rates (Lott and Mustard, Table 11). They attribute their results to a deterrent effect.

[4] Ayres and Donohue (1999) and Zimring and Hawkins (1997) raise a similar concern about Lott's work.

[5] States are adopting these laws at an increasing rate. Only 8 states had adopted such laws by 1986. By 1992, another 10 states had adopted them, and since then 13 more states have joined the group.

magnitude of such change depends on the relative strength of the hypothesized forces. In the rest of this section we formalize these arguments, in the context of the economic model of crime, to examine theoretically the effect of these laws. The analysis provides a basis for empirical examination of the issue.

2.1. Modeling the effect of the law

We extend an economic model of crime to incorporate the effect of the gun laws. The economic models of criminal behavior—Becker (1968), Block and Heineke (1975), Ehrlich (1975), Fleisher (1966), and Sjoquist (1973)—are formulated within the framework of the theory of choice under uncertainty. The basic model we consider assumes an optimizing agent who allocates time between legal and/or illegal activities in such a way as to maximize expected utility. The times allocated to these activities are denoted by T_l and T_i, respectively. So, $T_l + T_i = \bar{T}$, where $\bar{T}$ is net of non-market activities (e.g. leisure).

To obtain the agent's optimal supply of illegal (and legal) activities, we assume he maximizes the following expected von Newmann–Morgenstern utility function:

$$\max_{T_i} \int U[T_l, T_i, W_0 + RT_l + (B - xP)C(T_i)]\, \mathrm{d}F(x), \tag{1}$$

subject to $T_l \geq 0$, $T_i \geq 0$, and $T_l + T_i = \bar{T}$. The third argument in the utility function is wealth which includes the individual's assets (net of expected current earnings) W_0, return on legal activities R (i.e. wage rate), number of criminal offenses $C(T_i)$, benefit per offense B, punishment (if arrested) per offense P, and a random variable x, representing the stochastic failure (arrest) rate. The number of criminal offenses is assumed to increase with the amount of time devoted to illegal activities—$C'(T_i) > 0$. The function $F(x)$ denotes the individual's subjective probability distribution of x. Following Block and Heineke (1975), we assume that random variable x can take any value in the interval [0, 1].[6] Also, note that B, P, and other components of wealth incorporate pecuniary as well as non-pecuniary (psychic) values.

We introduce concealed handgun laws through an index variable H defined on [0, 1] interval, where $H = 0$ means no law (no concealed handgun-carrying) and a larger H value indicates a more permissive handgun law. To incorporate the deterrent and the facilitating effect of these laws, we allow some of the variables of the basic model to change with H. The deterrent effect is captured by increasing the perceived probability of the failure rate x as well as the possible punishment P. We model the probability change by augmenting the failure rate to $x + \alpha H$, where α is a shift parameter and the added term αH increases with H in such a way that $x + \alpha H$ remains within the [0, 1] range. This increases the expected failure rate in the presence of a concealed handgun law to $E(x) + \alpha$. Moreover, the prospect

[6] This approach is more general than Becker, Ehrlich and Sjoquist's approaches that assume x is either 1 or 0 and $F(x)$ follows a Bernoulli distribution, and, therefore, encompasses those as special cases. Moreover, the binary formulation assumes that the individual makes his allocative decision believing that he either succeeds in all offenses he plans or fails them all. This is unrealistic because the individual may fail on all, none, or a fraction of the attempted offenses; the formulation we adopt allows the individual to be confronted with a continuum of failure possibilities.

of an armed response from a civilian increases the possible punishment. We model this by changing P to $P(H)$, where $P'(H) > 0$.

The facilitating effect of the law manifests itself through an increase in the net benefit per offense B, and an increase in the number of offenses committed under any given time allocation C. Since B is net of any expense related to implementing a crime, the reduction in the cost of acquiring handguns that results from these laws increases B. So, we change B to $B(H)$, where $B'(H) > 0$. Moreover, the substitution of handguns for less lethal weapons increases the efficiency of the committed offenses. Accordingly, we change the offense function to $C(T_i, H)$, where $C_H = \partial C(\cdot)/\partial H > 0$.

The wealth function obtained by incorporating the above effects of concealed handgun laws is

$$W(x, H) = W_0 + RT_l + [B(H) - (x + \alpha H)P(H)]C(T_i, H). \tag{2}$$

And the individual's optimization problem in the extended framework is given by

$$\max_{T_i} \int U[T_l, T_i, W_0 + RT_l + (B(H) - (x + \alpha H)P(H))\, C(T_i, H)]\, \mathrm{d}F(x), \tag{3}$$

subject to $T_l \geq 0$, $T_i \geq 0$, and $T_l + T_i = \bar{T}$. The first-order condition for maximization requires that

$$A = E[U_i - U_l + U_W((B(H) - (x + \alpha H)P(H))C_i(T_i, H) - R)] = 0, \tag{4}$$

where the first three terms denote the derivatives of U with respect to T_i, T_l, and W, respectively, C_i denotes the derivative of C with respect to T_i, and $U_i - U_l$ is referred to as the individual's preference for honesty. The second-order condition requires that $\Delta = \partial A/\partial T_i < 0$.[7]

The effect of concealed handgun laws on the time allocated to criminal activities and the number of crimes committed is analytically derived by differentiating Eq. (4). The effect of a more permissive handgun law on T_i is

$$E\left[\frac{\partial T_i}{\partial H}\right] = -\frac{1}{\Delta} E\left[(U_{iW} - U_{lW} + U_{WW}G)D + U_W \frac{\partial G}{\partial H}\right], \tag{5}$$

where D is change in wealth resulting from a more permissive handgun law,[8] Δ is from the second-order condition above, and G is the expression which is multiplied by U_W in Eq. (4). Since the ratio outside the bracket is positive the sign of the derivative depends on the expectation of the bracketed term. This term cannot be signed without a detailed knowledge of the individual's preference structure and the magnitudes of D and G. However, Eq. (5)

[7] In the rest of this analysis we include both effects, although the effects can be isolated by setting either C_H and B' or a and P' equal to zero.

[8] For example, $D = -[(\alpha P + (x + \alpha)P')C(\cdot)] + [B'C(\cdot) + (B - (x + \alpha)P)C_H]$, where the first bracketed term captures the deterrent effect and is negative and the second term captures the facilitating effect and is positive. Note that B must exceed the expected punishment $[E(x) + \alpha]P$ for any offense to take place.

clearly indicates that the effect of concealed handgun laws on criminal activities does indeed depend on several variables, some individual specific and others more general.[9]

Also, since the number of criminal offenses increases with T_i, the law has a similar effect on the number of crimes:

$$E\left[\frac{\partial C(\cdot)}{\partial H}\right] = E\left[C_i \frac{\partial T_i}{\partial H}\right] + E(C_H), \tag{6}$$

where this effect cannot be signed either. Given the sign-ambiguity, the issue has to be settled empirically. The above results, however, should influence the empirical examination of the effect of the law, as will be discussed below.

2.2. *Empirical implications*

Eqs. (5) and (6) suggest that the effect of concealed handgun laws on the crime rate is not fixed, because it depends on behavioral parameters as well as the exogenous variables of the underlying model. This theoretical finding is also consistent with other observations reported in the literature. Ludwig (1998), for example, argues that because juveniles are not eligible to carry concealed weapons, any deterrent benefit from such laws will be limited to the non-juvenile population. Therefore, counties with a younger population may not experience the full deterrent effect of these laws. Other demographic determinants of the propensity to carry a concealed handgun, for example, age or gender, may also lead to a similar differential effect. Moreover, the effect of the law on crime should be more pronounced in the more populated counties, because authorities who have discretion over issuing handgun-carrying permits in absence of a concealed handgun law are the most restrictive in using such discretion in populated counties. Finally, Black and Nagin's (1998) time-specific dummies also point to the variability of the effect of these laws.

Using the county as the basis for aggregation, behavioral Eqs. (5) and (6) can be written in the following general form:

$$E\left[\frac{\partial C(\cdot)}{\partial H}\right]_{jt} = EK\left[W_0, R_{jt}, B_{jt}, P_{jt}, \alpha_{jt}C_{jt}(T_i), x_{jt}, g_{jt}(U), \eta_{jt}\right], \tag{7}$$

where j and t denote county and time, $K[\cdot]$ is a general function, $g(U)$ is a function denoting higher derivatives of the utility function, and η is a portmanteau variable capturing higher derivatives of the terms in the above expression as well as influences which are unaccounted for.

The heterogeneity indicated by the above equations implies that the effect of concealed handgun laws on crime varies across counties. Moreover, the testing procedure should allow the behavioral (response) parameters of the model to change. In fact, the effect may vary with the age and gender composition of the population, population density, characteristics of police, and economic conditions of the counties, among other things. Finally, variations across counties within a state in terms of how easily permits were issued prior to adoption

[9] These include, for example, the individual's attitude toward risk U_{WW}, the effect of increased wealth on his preference for honesty $U_{iW} - U_{lW}$, his perceived failure rate, the perceived benefits and costs associated with concealed handgun laws, and return on legal market activities.

of concealed handgun laws provide additional justification for allowing the effect of these laws to be heterogeneous across counties. For example, the most pronounced changes are expected in counties with the most restrictive licensing practice prior to the enactment of the law. Ignoring such heterogeneity and assuming that $E[\partial C(\cdot)/\partial H]_{jt}$ is a fixed quantity leads to estimation bias due to imposing an incorrect restriction. We report empirical evidence to support this point.

Also, note that a crime equation in implicit form can be derived from the first-order condition, Eq. (4). The right hand side variables and parameters of this equation are the same as the variables and parameters that appear on the right hand side of Eq. (7) which captures the effect of the concealed handgun law on crime. We maintain the same parallel between our crime equation and the equation we propose for measuring the effect of the law.

3. Estimation methods and issues

3.1. Shortcomings of dummy variable approach

In regression analysis an intercept-shifting dummy variable is often used to estimate the effect of an institutional change. The statistical and conceptual ramifications of this practice is seldom examined, particularly when the empirical analysis is not predicated on economic theory. To better motivate our procedure, which is intended to overcome the shortcomings of this approach, we elaborate on this issue using Lott and Mustard's (1997) study.[10] Lott and Mustard use county level panel data to estimate several linear crime equations. The dependent variable in each equation is one of several crime rates—murder, rape, aggravated assault, robbery, burglary, larceny, and auto theft. The regressors include the arrest rate corresponding to that crime category, a host of economic and socio-demographic factors, and a binary variable measuring the status of the concealed handgun law. This variable equals 1 if a county has such a law in place in a given period and 0 otherwise. The other regressors serve as control variables. The model they estimate is therefore

$$C_{jt} = \alpha + \gamma H_{jt} + \beta A_{jt} + \delta X_{jt} + \varepsilon_{jt}, \tag{8}$$

where H is the binary variable, A is the arrest rate, X includes the economic and demographic variables and a set of time and county dummies (one for each sampling year or county), ε is the regression error, and j and t denote counties and time periods, respectively.

Lott and Mustard's inference about the effect of concealed handgun laws on various categories of crime is based on the sign and statistical significance of the estimated coefficient of the binary variable—estimate of γ. A positive (negative) and significant estimate suggests that concealed handgun provisions would increase (decrease) the crime rate. Note that Lott and Mustard use γ in place of the expression in the right hand side of Eq. (7). This expression clearly depends on county-specific exogenous variables as well as the behavioral parameters of the model. Ignoring the heterogeneity of the effect of the law on various

[10] Lott and Mustard use the most comprehensive data set to examine this issue. There are several other useful but smaller studies that examine the effect of gun availability on crime; See Kleck (1995) and Lott and Mustard (1997) for a review.

counties and parameterizing the effect as a fixed parameter γ leads to biased estimation. The 2SLS estimate of γ reported by Lott and Mustard is negative, substantially large, and significant for all crime categories, further supporting their deterrence hypothesis.[11] The aforementioned bias can perhaps explain these unusually large negative estimates.[12]

3.2. Alternative approach

Following our theoretical results, we allow all behavioral parameters of the regression model to change with the law, and thus allow the effect of the law on crime rates to be heterogeneous across counties. The data will then show which of these parameters the law indeed affects. We implement this parameter flexibility by first estimating two separate crime equations, one for counties in states with a concealed handgun law and the other for the remaining counties:

$$C_{l,jt} = \alpha_l + \beta_l A_{l,jt} + \delta_l X_{l,jt} + \varepsilon_{l,jt}, \tag{9a}$$

$$C_{nl,jt} = \alpha_{nl} + \beta_{nl} A_{nl,jt} + \delta_{nl} X_{nl,jt} + \varepsilon_{nl,jt}, \tag{9b}$$

where l and nl indicate the presence or the absence of the concealed handgun law, respectively. Accordingly, the data used to estimate Eq. (9a) include all counties that have adopted a concealed handgun law for the period after the adoption of the law. The remaining data are used to estimate Eq. (9b); these include data for all counties that never adopted a concealed handgun law during our sample period as well as data for the adopting counties over the period before they adopt the law.

We then examine whether the law affects the response parameters by using an asymptotic Wald test of the null hypothesis $H_0 : \Theta_l = \Theta_{nl}$ against the alternative $H_0 : \Theta_l \neq \Theta_{nl}$, where Θ denotes (β, δ).[13] This hypothesis implies that the effect of the law on crime is a constant parameter γ (or $\alpha_l - \alpha_{nl}$) which does not change across county or over time. This of course is at odds with Eq. (7). A rejection of the null implies that the law affects the response (slope) parameters of the model, thus rejecting a simple intercept change formulation such as the one used by Lott and Mustard. As we report in the next section the above null is rejected strongly in all cases, making it necessary to use a less restrictive procedure.

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[11] The 2SLS that treats the arrest rate as an endogenous variable which is itself affected by the crime rate is the appropriate method for estimating Eq. (8). In addition to 2SLS, Lott and Mustard use OLS method, which ignores the simultaneity between crime and arrest, to project the expected reduction in the number of murders, rapes, robberies, and aggravated assaults for 1992 through 1995 if those states without right-to-carry concealed handgun provisions had adopted them in 1992. Much of the public attention that Lott and Mustard have received centers on these OLS based projections and not the more appropriate 2SLS results; see, for example, the article by Richard Morin, in *The Washington Post*, Sunday, 23 March 1997, page 5; also, see Black and Nagin (1998) and Ludwig (1998) who criticize Lott and Mustard on methodological grounds. These authors all focus on the inappropriate OLS results rather than the 2SLS results.

[12] Such specification bias also makes the coefficient estimates fragile with respect to small change in the model such as inclusion or exclusion of various control variables. Bartley and Cohen (1998) use the method suggested by Leamer and Herman (1983) to examine the range of estimates of the coefficient of the binary variable in Lott–Mustard specification and find it to be quite wide in many cases.

[13] The Wald statistic is the quadratic form constructed on the estimate of the difference $(\Theta_l - \Theta_{nl})$. The statistic is asymptotically distributed as a χ^2 variate with degrees of freedom equal to the number of parameters tested. Godfrey (1988, chap. 4) and Lo and Newey (1985), see also Pesaran, Smith, and Yeo, 1985.

We estimate, accordingly, the direction and extent of the change in crime rate that may result from introducing the concealed handgun law. More specifically, we determine how different the crime rate would have been during 1992 in the counties that did not have the concealed handgun law in place, had they adopted the law by 1992. We obtain these estimates, which are useful for policy purposes, simply by replacing the estimates of the behavioral parameters in Eq. (9b) with those in Eq. (9a) and computing the resulting predicted values for the crime rate in 1992. For example, we estimate $\hat{C}_{j92} = \hat{\alpha}_l + \hat{\Theta}_l Z_{nl,j92}$, where Z_{nl} denotes the regressors in Eq. (9b), Θ denotes (β, δ), 92 is year, and j is restricted to the aforementioned group of counties. These are simply predicted crime rates conditional on adopting the concealed handgun law. The difference between the predicted and actual crime rates measures the effect of concealed handgun laws on crime.

We emphasize that our interest is to estimate the expected 1992 crime rates conditional on the law being in place in a county that did not have it in 1992. Since the adoption of the law changes the regression coefficients, we must use the coefficients estimated for the subsample of counties with a concealed handgun law when estimating this conditional expectation. The conditional prediction so obtained is then compared with the county's actual 1992 crime rates to estimate the expected change resulting from adoption of the law. It is important to note that in the above comparison, one should not use the county's predicted crime rate without the law in 1992, $\hat{\alpha}_{nl} + \hat{\Theta}_{nl} Z_{nl,j92}$, instead of the observed (actual) crime rate C_{nl}. This is because the former does not have any information that is useful for our inference but is not contained in the county's observed 1992 crime rate. Therefore, if we used the predicted crime rate instead of the actual crime rate, we would just be adding extra noise (residual), thus reducing the accuracy of the inference. Also, note that all the information relevant to adopting the law is incorporated in $\hat{\Theta}_l$ which is estimated using counties with the law.[14]

To see how our procedure relates to the theoretical Eq. (7) and also to formally contrast this procedure with the intercept-shifting dummy variable procedure, consider the following. The latter procedure parameterizes the law-induced crime change as an intercept-shifting parameter $\alpha_l - \alpha_{nl}$ (or γ in Eq. (8)), implying the law does not affect any of the behavioral parameters of the model. We, on the other hand, parameterize the change as

$$(\alpha_l + \Theta_l Z_{nl,92}) - C_{nl,j92},$$

which after substitution from (9b) and setting the random error ϵ_{nl} equal to its expected value which is zero yields

$$(\alpha_l - \alpha_{nl}) + (\Theta_l - \Theta_{nl}) Z_{nl,92}.$$

Note that the first term in the above expression is the intercept change, used, for example, by Lott and Mustard, while the second term is our addition which varies with county characteristics and is a function of model parameters. This expression is the empirical

[14] Lott (1998, p. 304) claims that our approach throws out useful information. In making this claim, he ignores basic statistical concepts such as efficiency and sufficiency. Statistical inference involves extracting useful data information by means of efficient and unbiased coefficient estimators and conditional predictors. And that is what we do here. All the data pertaining to concealed handgun regimes (counties and time periods) are used to estimate the coefficient estimates, and these estimates are then used along with the data for non-concealed handgun regimes to obtain conditional predictions.

counterpart of the law-induced crime change given by Eq. (7). Also, note the similarities between the parameters and variables in this expression and those in the crime Eqs. (9a) and (9b). We documented a similar parallel between the theoretical counterparts of crime and the change in crime.

We summarize the predictions we so obtain to generate an inference about the potential influence of the law in each state which did not have a concealed handgun law in 1992. The predictions are further analyzed to determine factors that influence their direction or magnitude. This approach allows the effect of permissive handgun laws to vary with population density, racial and gender characteristics, income, and so forth. At the same time, it exploits the variation in the timing of these state laws to investigate their impact.

4. Data and results

4.1. Data

We use the data provided to us by Lott and Mustard. The data set covers 3054 counties for the period 1977–1992. However, since several series are only reported for 1982 through 1992, the effective time span is shorter. The data set includes the FBI's crime data for murder, rape, aggravated assault, and robbery which comprise "violent crime" and auto theft, burglary, and larceny which comprise "property crime". The series also include the corresponding arrest rate for these nine crime categories, population, population density, real per capita personal income, real per capita unemployment insurance payments, real per capita income maintenance payments, real per capita retirement payments per person over 65 years of age, and population characteristics for 36 age and race segments (black, white and other; male and female; and age divisions). The data set also includes state level observations on police employment and payroll, percentage of votes received by the Republican presidential candidate, and the percentage of each state's population that are members of the National Rifle Association.

The primary sources of data include FBI's Uniform Crime Report (for crime and arrest data), Cramer and Kopel, 1995 (for states with shall issue laws), the Bureau of the Census (for demographic data), Commerce Department's Regional Economics Information System and Statistical Abstract of the United States (for economic data), U.S. Department of Justice's Expenditure and Employment Data for the Criminal Justice System (for police employment and payroll), and National Rifle Association (for NRA membership data).[15]

We use the percentage of statewide vote received by the Republican presidential candidate in the most recent election as a proxy for partisan influence on the process that we estimate. Partisan influence is expected to capture any political pressure to adopt more permissive gun legislations—a stand which is more popular with Republican candidates.

4.2. Results

Following Ehrlich (1973), in all our estimation we treat the arrest rate, A, as an endogenous variable. A first stage equation then specifies arrest as a function of a set of independent

[15] See, also, Lott and Mustard's (1997) data descriptions on pages 6, 7, 12–17, 42, 43, and 66–68.

variables that include lagged crime rate, economic and demographic variables in the crime equation, time and county-specific dummies, police employment and payroll, and a set of variables to control for political influences. These latter variables include percentage of votes received by the Republican presidential candidate, and the percentage of a states' population that are members of the National Rifle Association.

We estimate Eqs. (9a) and (9b) along with the corresponding arrest equations via 2SLS, allowing the concealed handgun law to also shift the coefficients of the arrest equation in the first stage of estimation; such shifts are incorporated in cases where the Wald test applied to an arrest equation suggested such a change is warranted. This ensures the consistency of the second stage estimates. In all our estimations, we correct the residuals from the second stage least square to account for using predicted arrest rather than the actual arrest rate in estimation of crime equation; see for example, Davidson and MacKinnon (1993, chap. 7).

As indicated earlier, our empirical strategy starts with testing whether the data supports modeling the effect of the law through an intercept-shifting dummy variable—the hypothesis of no slope change due to the law. Using an asymptotic Wald test for all nine categories of crimes, we find that this hypothesis is rejected strongly for all categories of crime. The statistics for various crime equations are 131.2 (murder), 152.5 (rape), 395.3 (aggravated assault), 194.2 (robbery), 451.2 (burglary), 323.7 (larceny), 479.3 (auto theft); all statistics have P-values which are close to zero. This suggests that there are significant changes in slope coefficients in all cases, so the assumption that all changes are embedded in the intercept is invalid. The Lott–Mustard results are, therefore, biased by misspecification.[16]

Similar results for the arrest equation, used in the first stage of the 2SLS estimation, indicate the coefficients of these equations also change with the law. In fact, we incorporate these changes when obtaining the predicted arrest rates. A comparison of our predicted arrest rates to that of Lott and Mustard's reveal the inaccuracy introduced by limiting the change to the intercept term. For example, depending on the crime category, the mean square error of Lott and Mustard's predicted arrest rates is from 1.5 to 5.2 times larger than ours. Their predicted arrest rates also include a large number of negative values; for example, more than 19,000 of the 33,000 predicted arrest rates for auto theft are negative; the number of negative arrest rates for aggravated assault and property crimes are, respectively, 9900 and 13,500.[17]

We use the two-stage procedure described earlier to estimate the hypothetical effect on crime in each county in states that did not have a concealed handgun law in place if such a law had been in effect in 1992. We examine these effects in two ways, both on a county-by-county basis. First, we examine for each crime and for each county the predicted effect of changing the law. Table 1 contains summary statistics derived from these county level conditional predictions. Second, we examine the effect of county characteristics on predicted change in crime rates for each aggregated crime category (violent, property). Table 2 reports results of regressing these predictions on various county characteristics.

[16] We reported some of the empirical results in Dezhbakhsh and Rubin (1998).

[17] Obviously, any prediction has a range that may include undesirable values (e.g. negative estimates for a positive-valued variable such as arrest rate). The problem here is that a large number of such values are obtained by Lott and Mustard, which makes us suspect that their predicted arrest rates are biased downward.

Table 1
The predicted effect of adopting concealed handgun laws on crimes in states without such laws in 1992

States without such laws in 1992 (no. of counties)	No. of counties (population & crime as % of state population & crime) to experience					
	Murder		Rape		Robbery	
	Crime increase	Crime decrease	Crime increase	Crime decrease	Crime increase	Crime decrease
Alaska* (26)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Arizona* (15)	0 (0%, 0%)	0 (0%, 0%)	1 (3.0%, 0.2%)	0 (0%, 0%)	1 (3.0%, 0.1%)	0 (0%, 0%)
Arkansas* (75)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	3 (1.7%, 8.0%)	0 (0%, 0%)	0 (0%, 0%)
California (58)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Colorado (63)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Delaware (3)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Dist. of Col. (1)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Hawaii (5)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Illinois (102)	0 (0%, 0%)	3 (2.1%, 15.8%)	0 (0%, 0%)	0 (0%, 0%)	1 (0.1%, 0.1%)	1 (1.5%, 7.3%)
Iowa (99)	0 (0%, 0%)	0 (0%, 0%)	5 (14.2%, 3.3%)	1 (0.7%, 8.6%)	8 (18.1%, 5.7%)	0 (0%, 0%)
Kansas (105)	0 (0%, 0%)	1 (0.2%, 7.5%)	0 (0%, 0%)	1 (0.1%, 2.2%)	1 (0.6%, 0.2%)	0 (0%, 0%)
Kentucky* (120)	0 (0%, 0%)	3 (0.8%, 7.1%)	1 (0.6%, 0.5%)	9 (3.7%, 32.9%)	8 (4.8%, 4.2%)	0 (0%, 0%)
Louisiana* (64)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	1 (1.1%, 0.1%)	0 (0%, 0%)
Maryland (24)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	2 (14.7%, 8.6%)	0 (0%, 0%)
Massachusetts (14)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Michigan (83)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	2 (1.0%, 0.2%)	0 (0%, 0%)
Minnesota (87)	0 (0%, 0%)	1 (0.3%, 9.6%)	0 (0%, 0%)	0 (0%, 0%)	1 (0.5%, 0.3%)	0 (0%, 0%)
Missouri (115)	0 (0%, 0%)	0 (0%, 0%)	2 (1.6%, 1.4%)	2 (3.7%, 4.3%)	3 (1.8%, 1.4%)	1 (0.6%, 1.0%)
Nebraska (93)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Nevada* (17)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%%)	1 (3.2%, 6.3%)	0 (0%, 0%)
New Jersey (21)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
New Mexico (33)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	1 (3.2%, 0.4%)	0 (0%, 0%)
New York (62)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
N. Carol.* (100)	0 (0%, 0%)	0 (0%, 0%)	2 (1.9%, 0.3%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Ohio (88)	0 (0%, 0%)	1 (1.3%, 3.2%)	2 (2.6%, 0.4%)	1 (1.2%, 1.8%)	5 (3.9%, 1.2%)	0 (0%, 0%)
Oklahoma* (77)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Rhode Island (5)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
S. Carolina* (46)	0 (0%, 0%)	0 (0%, 0%)	1 (1.6%, 0.3%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Tennessee* (95)	0 (0%, 0%)	0 (0%, 0%)	2 (10.9%, 8.1%)	5 (5.2%, 13.8%)	6 (12.7%, 17.3%)	0 (0%, 0%)
Texas* (254)	0 (0%, 0%)	7 (0.4%, 9.4%)	0 (0%, 0%)	8 (0.6%, 6.4%)	2 (0.3%, 0.1%)	1 (0.01%, 2.0%)
Utah* (29)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Wisconsin (72)	0 (0%, 0%)	0 (0%, 0%)	1 (0.7%, 0.2%)	0 (0%, 0%)	2 (1.6%, 0.3%)	0 (0%, 0%)
Wyoming* (23)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)

	Aggravated assault		Burglary		Auto theft	
Alaska* (26)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Arizona* (15)	1 (3.0%, 0.6%)	(0%, 0%)	1 (3.0%, 1.0%)	0 (0%, 0%)	2 (3.2%, 1.9%)	0 (0%, 0%)
Arkansas* (75)	0 (0%, 0%)	5 (7.5%, 6.2%)	2 (0.9%, 1.4%)	8 (4.9%, 7.8%)	5 (2.3%, 0.8%)	4 (2.2%, 4.0%)
California (58)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	1 (0.01%, 0.2%)
Colorado (63)	3 (5.7%, 2.0%)	0 (0%, 0%)	9 (6.6%, 6.2%)	3 (1.3%, 2.7%)	1 (0.1%, 0.8%)	4 (5.4%, 4.9%)
Delaware (3)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Dist. of Col. (1)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Hawaii (5)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
Illinois (102)	8 (5.3%, 2.8%)	2 (0.1%, 0.9%)	9 (1.8%, 2.6%)	13 (5.1%, 11.8%)	15 (2.7%, 3.9%)	6 (6.2%, 12.7%)
Iowa (99)	8 (13.2%, 1.7%)	1 (0.4%, 2.0%)	23 (36.6%, 13.8%)	4 (1.8%, 4.7%)	19 (33.8%, 12.4%)	5 (2.4%, 6.9%)
Kansas (105)	1 (0.3%, 0.3%)	0 (0%, 0%)	10 (2.0%, 3.1%)	19 (24.6%, 16.8%)	16 (5.1%, 10.4%)	15 (4.0%, 4.9%)
Kentucky* (120)	0 (0%, 0%)	12 (6.4%, 8.2%)	50 (36.3%, 41.4%)	6 (1.2%, 2.9%)	23 (9.0%, 13.2%)	14 (4.7%, 6.9%)
Louisiana* (64)	0 (0%, 0%)	1 (0.2%, 2.5%)	2 (1.3%, 0.9%)	5 (6.4%, 8.5%)	4 (3.2%, 2.3%)	4 (2.3%, 5.4%)
Maryland (24)	1 (14.4%, 7.0%)	1 (1.5%, 4.4%)	3 (15.3%, 11.3%)	2 (2.6%, 5.2%)	3 (15.3%, 12.3%)	0 (0%, 0%)
Massachusetts (14)	0 (0%, 0%)	0 (0%, 0%)	1 (0.1%, 6.6%)	0 (0%, 0%)	1 (0.1%, 1.2%)	0 (0%, 0%)
Michigan (83)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	1 (0.2%, 1.4%)	0 (0%, 0%)	6 (1.0%, 4.2%)
Minnesota (87)	1 (0.2%, 0.2%)	2 (0.6%, 7.9%)	8 (2.1%, 3.3%)	5 (1.2%, 4.6%)	3 (0.6%, 0.9%)	12 (4.5%, 9.2%)
Missouri (115)	4 (2.0%, 1.2%)	6 (4.7%, 11.2%)	18 (6.9%, 16.6%)	8 (6.4%, 9.4%)	18 (6.1%, 7.4%)	5 (4.8%, 3.5%)
Nebraska (93)	2 (0.6%, 0.8%)	3 (1.9%, 3.2%)	12 (3.6%, 11.6%)	8 (5.2%, 8.0%)	12 (4.2%, 8.1%)	6 (4.6%, 10.9%)
Nevada* (17)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	2 (0.7%, 5.4%)	0 (0%, 0%)
New Jersey (21)	1 (3.2%, 5.0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
New Mexico (33)	1 (3.2%, 0.5%)	0 (0%, 0%)	2 (3.5%, 5.0%)	0 (0%, 0%)	2 (6.5%, 4.4%)	0 (0%, 0%)
New York (62)	1 (3.9%, 0.8%)	2 (0.4%, 4.2%)	0 (0%, 0%)	3 (0.5%, 3.0%)	0 (0%, 0%)	4 (3.4%, 2.2%)
N. Carol.* (100)	2 (0.3%, 0.1%)	0 (0%, 0%)	2 (0.5%, 0.4%)	8 (4.0%, 5.0%)	0 (0%, 0%)	5 (3.0%, 3.0%)
Ohio (88)	8 (7.3%, 4.4%)	3 (2.0%, 9.6%)	14 (9.0%, 10.1%)	2 (0.4%, 1.5%)	9 (5.2%, 3.5%)	3 (1.7%, 2.9%)
Oklahoma* (77)	2 (0.6%, 0.3%)	1 (0.3%, 7.1%)	0 (0%, 0%)	4 (0.6%, 3.2%)	3 (0.9%, 1.6%)	1 (0.1%, 0.3%)
Rhode Island (5)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)	0 (0%, 0%)
S. Carolina* (46)	0 (0%, 0%)	0 (0%, 0%)	10 (39.1%, 21.7%)	0 (0%, 0%)	3 (9.6%, 7.6%)	0 (0%, 0%)
Tennessee* (95)	7 (13.2%, 7.8%)	6 (9.4%, 13.8%)	16 (16.1%, 12.6%)	3 (1.2%, 3.9%)	15 (16.1%, 15.6%)	0 (0%, 0%)
Texas* (254)	3 (0.1%, 0.1%)	17 (1.6%, 8.1%)	13 (0.5%, 2.4%)	20 (1.0%, 5.9%)	37 (2.2%, 7.2%)	24 (2.4%, 7.3%)
Utah* (29)	1 (0.6%, 0.2%)	0 (0%, 0%)	2 (0.9%, 3.2%)	4 (4.1%, 14.5%)	2 (0.5%, 3.7%)	5 (5.6%, 12.8%)
Wisconsin (72)	0 (0%, 0%)	2 (1.1%, 2.9%)	0 (0%, 0%)	14 (9.0%, 17.6%)	2 (0.9%, 0.1%)	16 (16.9%, 23.8%)
Wyoming* (23)	0 (0%, 0%)	0 (0%, 0%)	2 (2.7%, 3.2%)	1 (1.1%, 2.1%)	4 (5.9%, 4.6%)	1 (1.1%, 3.5%)

Notes: The entries in each crime category are the number of counties in each states that would have experienced a statistically significant change in their 1992 crime rates, had they adopted a concealed handgun law by 1992. The numbers in parentheses are the respective population of these counties as a percent of the state population and their crime rates as a percent of the state total crimes in that category. In 1992 Philadelphia was the only county in Pennsylvania that was exempt from Pennsylvania's 1989 concealed handgun law. Entries for Philadelphia, not reported, are all zero. An asterisk (*) indicates that the state adopted a handgun law between 1992 and 1996.

Table 2
Determinants of the magnitude of the change in crime induced by concealed handgun laws

Characteristics	Violent crimes	Property crimes
Arrest rate	+0.002765** (0.00060)	+0.009427** (0.00285)
Police payroll	−0.031942** (0.00857)	−0.060959** (0.01340)
Population density	−8.03e−07 (4.55E−06)	+0.000028** (8.51E−06)
NRA membership	+0.000150** (0.00003)	−0.000250** (0.00006)
Income	+0.000030** (7.54E−06)	+6.38E−06 (0.00001)
Unemployment insurance	+0.000103 (0.00031)	+0.000057 (0.00038)
Retirement payment	−0.000016** (3.49E−06)	+0.000022** (9.84E−06)
Black males (10–29)	−0.085921* (0.05122)	+0.007725 (0.05856)
Black females (10–29)	+0.105542** (0.04713)	+0.071520 (0.05375)
Non-black males (10–29)	+0.054673** (0.01562)	−0.000593 (0.02344)
Non-black females (10–29)	−0.069583** (0.02009)	−0.053331* (0.02962)
Population over 65	−0.012907* (0.00671)	−0.016139* (0.00897)
R^2	0.276	0.272

Notes: Asymptotic heteroskedasticity-robust standard errors are in parentheses. (*) and (**) indicate significance at the 10% and 5% levels, respectively.

The interpretation of Table 1 is as follows: In 1992, there were thirty-three states without such laws, excluding Pennsylvania where Philadelphia county was given exemption from the law passed in 1989. Consider, for example, murder in Texas. Since Texas is in our sample, this indicates that in 1992 this state did not have a concealed handgun law in place, although the asterisk (*) indicates that it had adopted such a law by 1996. There are 254 counties in Texas as shown in (column 1). Had the concealed handgun law been in effect in Texas in 1992, then in seven of those counties, which include 0.4% of the population in the state and account for 9.4% of the state murders, murder rates would have decreased by a statistically significant amount.[18] Thus, for counties in six states a concealed handgun law would have reduced murder rates and for all counties in the other 27 states it would have been ineffective. Overall, the results indicate a relatively small, and crime-reducing, effect of concealed handgun laws on murder rates. Moreover, it appears that there would have been little effect on rape with 21 states unaffected, 4 states with unambiguous increases, and 2 states with unambiguous decreases.

The effect on robbery would have been an increase in crime for many states. For counties in 13 states, there would have been an unambiguous increase in robbery; there would have been mixed effects (increase in some counties and decrease in some) in counties in only three states. The overall increase in robbery is not surprising. As discussed earlier, the sum of the facilitating and deterrent effects determine how crime changes as the result of these laws. For robbery, the facilitating effect is crime-inducing but the deterrent effect is not necessarily crime-reducing. While some of the robberies such as street stick-ups are deterrable by concealed handguns, many potential robbery targets such as banks and various shops already have armed protection. Concealed firearm laws, therefore, do not provide the

[18] If the actual 1992 crime rate for a county falls short of (exceeds) the confidence interval for the projected crime rate conditional on the law being in place, then we infer that the law would have increased (decreased) the crime rate for that county.

latter group with additional deterrence benefits. The low deterrence and the apparently large crime-facilitating effect for robbers implies a net crime enhancing effect in this case.

For aggravated assault 11 states would have been unaffected, 7 states adversely affected, and 4 states would have observed a drop in crime. The result for the remaining states is mixed. For the three categories of property crime (only two reported in the table) the effect would have been more mixed. Altogether there were 33 states containing 2074 counties that did not have shall issue laws in 1992, so the largest percentage of counties predicted to be affected in one direction by changing the law would have been the 15% of counties predicted to experience an increase in larceny; all other predicted percentage changes in any direction are less than 10%.

We can also derive policy implications from these ex post predictions for particular states which had not adopted the law by 1996 (states without an identifying asterisk (*)). Maryland would expect increases in robbery, assault, burglary, and auto theft, and so probably should not adopt the law. Similarly New Mexico would expect small increases in robbery and all three categories of property crime, and so also should not adopt the law. In Iowa, rape, robbery, assault, burglary and auto theft would increase, if the law is adopted. On the other hand, were Illinois to adopt a handgun law, then we would expect decreases in murder, robbery, burglary, and auto theft, but an increase in assault. Kansas could expect reductions in murder, rape, and burglary, and increases in auto theft and a small increase in assault. Minnesota might also benefit from the law. For most other states that had not adopted the law by 1996, effects would be small and mixed.

We next examine how various county characteristics affect the magnitude of the law-induced changes in aggregate (violent and property) crimes. We do this by regressing the predicted (law-induced) change in crime rates for each of the counties without the law in 1992 on a set of demographic and economic variables for the county. The economic variables, all measured per capita, are personal income, unemployment insurance, and retirement payments per person over 65. We also include (predicted) arrest rates, population density, and demographic variables. Since most crime is committed by young males, we include number of black and non-black males 10–29 years old, and similarly for females. We include persons 65 and over, who are perhaps more likely to be victims than perpetrators of crimes. Finally, we include per capita measures of police payroll and the number of NRA members in the state. In all cases, we measure the effect of the relevant variable on predicted changes in crime resulting from the existence of a concealed handgun law in the county.

Regression results are summarized in Table 2. A positive and significant coefficient suggests that the characteristic is expected to have a positive influence on the change in crime that results from right-to-carry concealed handgun laws; “positive” means that crime increase will become larger (stronger facilitating effect) and crime decrease will become smaller (weaker deterrence effect) when the characteristic takes a larger value. A negative and significant coefficient suggests that the characteristic is expected to have a negative influence on the change in crime that results from right-to-carry concealed handgun laws; “negative” means that crime increases will become smaller (weaker facilitating effect) and crime decrease will become larger (stronger deterrence effect) when the characteristic takes a larger value.

For example, the positive and statistically significant coefficient of arrest rate suggest that concealed handgun laws have a stronger facilitating effect and a weaker deterrence effect in counties with a relatively higher arrest rate, holding other county characteristics constant. Since in these counties police force is more proactive (efficient)—clearing a higher percent of crimes by arrest—citizens do not have as much need to carry guns for deterrence. So the criminals receive much of the gain from these laws as they need to better arm themselves to face the efficient police. Such asymmetrical needs lead to a weak deterrence effect and a strong facilitating effect. Table 2 results also suggest that for counties that spend relatively more on police the laws are expected to have a stronger deterrence effect and a weaker facilitating effect, holding other county characteristics constant. This is plausible: Higher spending on police may not effect the deterrent benefit of handguns, but will reduce the facilitating effect of handguns that benefits criminal activity. It may also be that higher police expenditures enable more effective screening of would-be gun carriers. This implies that states contemplating passage of handgun laws should increase expenditure on enforcement.

The other results that are statistically significant and consistent across the two crime categories pertain to likely victims: more elderly people and more young (10–29-year-old) non-black females are associated with reduced crime as a result of passage of gun laws. This may represent evidence of the deterrent effect in some cases, with these variables contributing to such an effect. Experiments with other specifications indicates that this specification provides most of the useful information in the data, and is sufficiently aggregated so that the results are easily interpreted.

5. Concluding remarks

The role of handguns in violent crimes is a hotly debated public policy issue. Recently many states have adopted right-to-carry concealed handgun laws. The advocates argue these permissive laws have a deterrent effect on crime, while the opponents point to their potential crime-facilitating effects through increased gun availability. These arguments imply that concealed handgun laws may cause a change in the behavior of criminals either directly or as the result of a change in the behavior of their potential victims. No attempt has yet been made to model the effect of such laws in the context of the economic theory of crime. The existing empirical work, therefore, uses methods that are not based on the behavioral implications of such laws, although such a theoretical basis is necessary for any credible examination of the issue. For example, the highly publicized Lott and Mustard (1997) study that suggests these laws have a strong crime-reducing effect estimates such effects through a regression dummy variable. This method assumes that the effect of the law on crime is identical across all counties and independent of any county characteristics, an assumption contradicted by both the theory and evidence presented in this paper. Research purporting to demonstrate statistically that handgun related laws have important impacts on crime rates are of direct relevance to policy debates as well as to legislation. It is imperative, therefore, that such debates and subsequent legislation rely on solid empirical findings.

In this paper we extend the economic model of crime to incorporate the effect of concealed handgun laws. We demonstrate that the direction and magnitude of any resulting change would depend on the parameters of the criminal's optimization problem and thus the characteristics of the individual and his (her) social and economic setting. This means that any change in crime rate induced by concealed handgun laws will depend on demographic, social, and economic specifities of the observation units. So these laws might lead to increases in crime in some jurisdictions and decreases in others. The results that assume these effects are identical across all counties are, therefore, invalid. We present empirical evidence supporting this assertion.

We propose, alternatively, an empirical procedure to examine the effect of concealed handgun laws on crime rates. Our procedure draws on the theoretical considerations resulting from the extended crime model, therefore allowing us to assess the full implications of the right-to-carry gun provisions. We find that the results of concealed weapons laws are much smaller than suggested by Lott and Mustard and by no means crime-reducing across all categories. For murder, for example, there is only a small reduction. For robbery, many states experience increases in crime. For other crimes, results are ambiguous, with some states showing predicted increases and some predicted decreases. We identify states (Illinois, Kansas, Minnesota) that might benefit from passage of these laws, and states (Maryland, New Mexico, and Iowa) that probably would not.

We also examine demographic and other influences on the likely effect of passage of laws on crime rates. We find that there are predictable patterns on the effect of shall issue laws on crime. For example, counties spending more on police could expect a larger decrease in crime from the passage of a law, or smaller increases where the law leads to an increase in crime. Our theoretical and subsequent empirical work points to the inadequacy of testing the effect of concealed handgun laws without considering their theoretical implication. The sort of analysis developed here could be used to enable policy makers to more carefully tailor laws to particular conditions in a jurisdiction.

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