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Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data

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A recent study concludes that permissive concealed-handgun-carrying (or "shall-issue") laws have sharply reduced crime rates, including the rate of homicide. The method of the study has been critiqued by several authors. In this paper, I report a quite different approach that exploits the minimum age requirements for concealed-carry permits to more effectively control for unobserved variables that may vary over time. Because even permissive concealed-carry states require permit holders to meet minimum age requirements, any deterrent benefits from these laws should be concentrated among adults and, therefore, should be reflected in the gap between adult and juvenile victimization rates. My results suggest that shall-issue laws have resulted, if anything, in an *increase* in adult homicide rates. © 1998 by Elsevier Science Inc.

I. Introduction

Crime is one of the American public's top priorities,[1] a source of concern and frustration that has translated into individual as well as collective action. Motivated in large part by fear of crime, between 35% and 40% of all American households keep a total of 127 million long guns and 65 million handguns [Cook and Ludwig (1997)], despite uncertainty about whether such widespread gun ownership increases or decreases public safety [Zimring and Hawkins (1997a)]. For the owner, firearms may be used for protection against intruders, yet keeping a gun also seems to be a risk factor for unintentional injury, suicide, and homicide [Vernick et al. (1997)]. Keeping a gun also may impose costs and benefits on others. High rates of gun ownership may produce

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[1]For example, a USA Today/CNN/Gallup poll from January 5 to 7, 1996 ($N = 1000$), found that 66% of voters listed violent crime as an issue that would be a "high priority" in deciding whom to vote for, second only to the quality of public education (67%). (*USA Today*, "Ideal citizens go face to face," by Richard Wolf, January 22, 1996, p. 6D).

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general deterrence effects, for example by reducing the frequency with which burglars rob occupied homes. On the other hand, over 500,000 firearms are stolen each year, and keeping guns out of dangerous hands is made more difficult by over 2 million private transfers of second-hand guns annually [Cook et al. (1995); Cook and Ludwig (1997)]. In a recent survey, 85% of those without guns and 40% of gun owners report that they would feel less safe if more people in their community obtained a gun [Hemenway et al. (1995)].

Given the uncertainty surrounding the benefits and costs of widespread gun ownership, it is noteworthy that many states have responded to the crime problem by expanding the opportunities of private citizens to arm themselves in public. To date, 31 states have enacted "shall-issue" laws, which require local law enforcement authorities to issue concealed-handgun-carrying permits to any applicant who meets a set of specified criteria related to age, criminal history, and mental illness [Jost (1997)]. The number of states with shall-issue laws is likely to increase in the near future, as suggested by the consideration of shall-issue legislation in California and eight other states during 1997 [Hill (1997)].

The net effects of shall-issue laws are as difficult to predict as those of widespread gun ownership, though shall-issue laws have an even greater potential for positive and negative externalities. If gun carrying increases once these laws are passed, homicide rates may increase as guns are substituted for less lethal weapons in hostile confrontations [Zimring (1968); Cook (1991)]. Shall-issue laws also could cause homicides to increase if higher rates of gun carrying among potential victims causes criminals to arm themselves with greater frequency [Cook (1991)]. On the other hand, if shall-issue laws cause more citizens to carry handguns, then the expected costs associated with committing crimes may increase. An increase in the costs of crime may deter some criminal activity [Lott and Mustard (1997)], particularly as the number of permits issued within a state increases over time. It is also possible that the publicity surrounding the passage of the law may be sufficient to cause criminals to revise their perceptions of the costs of crime,[2] in which case any deterrent benefits may surround changes in the legal regime.

Unfortunately, there is currently little empirical evidence on the relationship between shall-issue laws and crime. A recent study by John Lott and David Mustard (1997) analyzes county-level panel data for 1977 through 1992 and finds evidence that shall-issue laws are negatively correlated with crime rates, including homicide. The authors conclude that "concealed handguns are the most cost-effective method of reducing crime thus far analyzed by economists" (p. 65). However, their method has been critiqued by several authors. Their study seems to suffer from model specification problems that will bias their estimates, a point that receives empirical support from Black and Nagin's (1998) reanalysis of the Lott and Mustard data.

In this paper, I present the results of a quite different approach to examining the effects of shall-issue laws on crime that exploits the fact that each shall-issue state enforces a minimum-age requirement for obtaining a concealed-carry permit to help control for the effects of unobserved variables. Because juveniles will not be eligible for concealed-carry permits even after shall-issue laws are passed, any deterrent benefits from these laws should be concentrated among adults. Any deterrent benefits of these laws should, therefore, reveal themselves in the difference in homicide victimization rates between adults and juveniles. My sample includes observations through 1994, an

[2]Zimring and Hawkins (1997b) call this an "announcement effect."

important extension of Lott and Mustard, because some of the shall-issue states studied in their sample enacted these laws as late as 1991. My results suggest that shall-issue laws have resulted, if anything, in an *increase* in adult homicide rates.

The paper is organized as follows. The next section offers a critical review of the available evidence on shall-issue laws. The third section reviews the data and empirical strategy used in this paper, as well as the results of my analysis. The fourth section offers a discussion of my findings.

II. Previous Research

The effects of shall-issue laws on crime will depend, in part, on how concealed-handgun carrying changes when such laws are passed. Although almost nothing is known on this point, most gun carrying in the United States seems to occur without benefit of a concealed-carry permit. Cook and Ludwig (1997) find that 7.5% of American adults carried a firearm on their person or in a motor vehicle at some point during 1994. By way of comparison, a total of 1.4% of adults had obtained a concealed-carry permit in Florida 7 years after that state passed a shall-issue law,[3] and a recent review of other estimates suggests that in 12 of 16 shall-issue states fewer than 2% of adults had obtained permits [Hill (1997)]. Presumably, some fraction of those who apply for permits carried illegally before the shall-issue law was passed, so the number of permits issued may overstate the degree to which gun carrying changes. The effects of shall-issue laws on the prevalence of gun carrying are likely to be small.

Lott and Mustard (1997) examine the effects of shall-issue laws on crime by applying regression models to a panel dataset of all counties in the United States from 1977 through 1992.[4] Their dependent variables include the natural logarithm of several violent and property crime rates. Explanatory variables include age, race, *per capita* income, population, people per square mile, and *per capita* spending on social programs to proxy for poverty, though whether these proxy variables should be positively or negatively correlated with an area's level of material deprivation is not clear.[5] The variables also include year-specific dummy variables to capture changes in the U.S. crime rate over time, county-specific dummy variables to capture unobserved county "fixed effects," and the county's arrest ratio to control for other policy changes that may affect crime.[6] Lott and Mustard find that shall-issue laws are, in general, negatively correlated with violent crimes and are positively correlated with property crimes.

Yet, Lott and Mustard's (1997) analysis may suffer from bias from omitted variables for at least two reasons. First, the Lott and Mustard fixed-effects approach cannot control for unobserved factors that influence county crime trends but are not fixed over time. Crack is one example of a factor that is not explicitly controlled for in the Lott and

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[3]Calculated from permit figures reported in McDowall, et al. (1995, p. 194) together with population estimates from the U.S. Statistical Abstracts (1995, Table 34).

[4]McDowall et al. (1995) estimate the effects of shall-issue laws on crime rates using data from three states. Because their approach is susceptible to the same biases as that of Lott and Mustard, I restrict my attention to the problems with the Lott and Mustard estimates based on national data.

[5]A given level of *per capita* social spending may reflect a large number of pre-government-transfer poor who each receive a relatively meager transfer payment, or a small number of pretransfer poor who each receive a relatively generous transfer payment; the implications for the level of material deprivation are obviously different.

[6]The problems with using arrest ratios in this way have been well known since Blumstein et al. (1978). Yet in practice the Lott and Mustard results do not seem sensitive to the inclusion or exclusion of the arrest ratio [Black and Nagin (1998)].

Mustard study, is likely to be different between shall-issue states such as Idaho and other states such as California and New York, and is unlikely to have fixed effects over time [Zimring and Hawkins (1997b)].[7] Other examples include gang activity [Klein (1995)] and, as noted above, poverty. Second, passage of a shall-issue law presumably reflects a jurisdiction's preferences for anticrime measures, which may manifest themselves in other government anticrime responses beyond passage of shall-issue legislation. Lott and Mustard include policy variables that are likely to capture only a subset of the many possible public-sector responses to crime.[8]

Empirical evidence that Lott and Mustard's (1997) analysis produces biased estimates comes from Black and Nagin (1998). By applying a formal model mis-specification test that exploits the panel structure of the dataset [Heckman and Hotz (1989)], Black and Nagin find evidence to suggest that the Lott and Mustard regression model is unable to control for all of the factors that cause crime rates to differ between shall-issue states and other states *before* these laws are adopted. As a result, Lott and Mustard's estimates for the effects of shall-issue laws will reflect in whole or part the effects of omitted factors that are not captured by their regression model.[9]

Lott and Mustard (1997) present an additional set of regressions that uses two-stage least squares (2SLS) methods in an attempt to control for the omitted variables highlighted by Black and Nagin's analysis. To produce unbiased estimates for the effects of shall-issue laws, their 2SLS approach requires that lagged crime rates (or changes in crime over time), the proportion of a state that belongs to the National Rifle Association or voted Republican in the most recent Presidential election, and *per capita* (and per crime) police resources will only affect a county's crime rate by influencing the state's shall-issue law status. Nagin (1978) offers a relevant discussion of why many of the variables used by Lott and Mustard are unlikely to be valid for this purpose. Unfortunately, Lott and Mustard do not present the results of statistical tests such as those discussed in Hausman (1983) or Newey (1985), which could shed light on the validity of their estimation procedure.

Yet, some evidence that the Lott and Mustard 2SLS estimates are biased comes from their implausibly large magnitudes [Lott and Mustard (1997), Table 11]: The estimates imply that passage of a shall-issue law will reduce homicides by 67%, rapes by 65%, and assaults by 73%.[10] In sum, Lott and Mustard's analysis seems to suffer from bias and, as

[7] How to conceptualize and measure drug market activity is not obvious. Lott and Mustard (1997) experiment with drug prices as an additional covariate, though they ultimately reject this model specification because of missing data problems. Drug prices may be positively correlated with criminal activity if, as Lott and Mustard (1997, note 50) suggest, higher drug prices make addicts more prone to commit crimes to finance their habits. On the other hand, prices could be negatively correlated with criminal activity if low prices reflect the frequency of and (potentially violent) competition among drug suppliers. Unfortunately, as Kleiman and Smith (1990, p. 102) note, "[N]o city has anything resembling a quantitatively accurate description of its own drug problem."

[8] In addition to controlling for arrest ratios and (in some cases) burglary and robbery rates, Lott and Mustard (1997) experiment with including variables for sentencing enhancements for crimes committed with weapons, handgun purchase waiting periods, conviction rates, and sentence lengths (apparently available only for Oregon).

[9] Lott and Mustard (1997) also experiment with a model specification that includes a county's burglary or robbery rate as an additional explanatory variable to control for omitted variables. In unpublished calculations, Black and Nagin find that this model specification is also rejected using the Heckman and Hotz test (Dan Black, personal communication).

[10] Lott and Mustard (1997) report that the "percent of a standard deviation change in the endogenous variable [logged crime rate] that can be explained by a 1 standard deviation change in the exogenous variable [predicted probability of enacting a shall-issue law]" (p. 47). The implied effects on crime rates from passing a shall-issue law can

a result, is unlikely to provide reliable information about the effects of shall-issue laws on crime.

III. Empirical Methods and Results

This section presents the results of a new test for the causal effects of shall-issue laws using state homicide data disaggregated by age. After reviewing the data, I discuss why my estimation approach may help control for the omitted variables problems that seem to plague Lott and Mustard (1997). Then, I show that there is little evidence to suggest that shall-issue laws have reduced homicide victimization rates for adults.

Data

The dataset used in this paper contains information for each state in the United States from 1977 through 1994. Of the various crime rates that may be used in assessing the effects of shall-issue laws, homicide is widely considered to be measured most accurately [Cook and Laub (1997)] and, as such, is the focus of the analysis presented here. Annual state-by-state homicide counts are taken from vital statistics reports compiled by the U.S. Department of Health and Human Services. State population data are taken from the Statistical Abstracts for the United States, while data on the age distribution within each state are from the Census Bureau's Population Estimates and Population Distribution Branches.[11] Descriptive statistics for these data can be found in Table 1.

Lott and Mustard (1997) classify the following states as having enacted shall-issue laws between 1987 and 1991: Florida (1987); Georgia (1989); Idaho (1990); Maine (1985); Mississippi (1990); Montana (1991); Oregon (1990); Pennsylvania (1989); Virginia (1988); and West Virginia (1989). As Lott and Mustard note, whether Virginia and Maine should be included in this list is unclear, because Maine passed a series of modifications to its concealed-carry laws starting in 1981, and Virginia enacted additional shall-issue legislation on July 1, 1995, that eliminated the previous law's "need-to-carry" requirement and greatly increased the rate at which permits were issued [Hill (1997); Webster et al. (1997)]. The appropriate treatment of Pennsylvania in my sample is also complicated, because the shall-issue law exempts Philadelphia [Lott and Mustard (1997)].

Several additional states had shall-issue laws in place at the start of my sample period (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington). Although I present descriptive statistics for these states in what follows, identification of the effects of shall-issue laws using estimation approaches that control for state fixed effects (as does my empirical strategy) will rest on the states that change their laws during the sample period.

The minimum age requirement for obtaining a concealed-carry permit in those states that changed their laws from 1987 to 1991 is 18 in Maine, Montana, and West Virginia and is 21 in the others. For my empirical analysis, I define juvenile homicide victimization rates as those involving victims between the ages of 12 and 17. I exclude homicides to younger children because they tend to have characteristics that are quite different from those involving older children or adults, though replicating the analysis presented

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be calculated as $e^{\beta} - 1$ for the coefficient β on the shall-issue variable, because the dependent variable is the logarithm of the crime rate [for example, see Kennedy (1993), p. 106]. Thanks to Daniel Nagin for this point.

[11] Annual state population estimates taken from the U.S. Department of Commerce web page, http://www.census.gov/population/www/estimates/statepop.html.

TABLE 1. Descriptive statistics for state data

	Homicide rate (per 100,000 population)	*Adult (21+) homicide rate (per 100,000 adults)*	*Youth (12–17) homicide rate (per 100,000 youth)*
U.S., 1977–1994	9.35	11.17	5.93
Non-shall-issue states, 1977–1994	9.75	11.73	6.07
Rates for states with concealed-carry laws before 1977,* for 1977–1994	6.68	8.18	3.68
Rates for states that implemented concealed-carry laws between 1987–1991,† for the period before these laws went into effect	10.96	13.89	4.08
Rates for states that implemented concealed-carry laws between 1987–1991,† for the period after these laws went into effect	9.95	11.48	7.62

Notes: All means were calculated using state population figures as weights. Homicide counts taken from U.S. Vital Statistics, population counts taken from U.S. Census Bureau.

*States with shall-issue laws before 1977: Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington.

†States that implemented shall-issue laws between 1987 and 1991: Florida, Georgia, Idaho, Mississippi, Montana, Oregon, and West Virginia. Pennsylvania, Maine, and Virginia are excluded from the sample for reasons discussed in the text.

below using victimization rates for all those under 18 years of age produces qualitatively similar results.[12]

Estimation Strategy

Of primary concern with previous research such as Lott and Mustard (1997) are the difficulties involved in controlling for unobserved or difficult-to-measure factors that influence local crime rates but change over time. One way to address the problem of unobserved, time-varying factors is suggested by the requirement in each shall-issue state that permit holders be at least 18, or more typically 21, years of age. As a result, the probability of encountering an armed juvenile (the costs of committing crime against juveniles) should be largely unaffected by shall-issue laws. Any deterrent benefits from these laws thus should be concentrated among adults and should be reflected by a decrease in the difference between adult and juvenile victimization rates (that is, adult rates should decrease relative to juvenile rates).

Both the standard fixed-effects approach and the empirical strategy used here can be illustrated using Table 2, adapted from Joyce and Kaestner (1996). The standard fixed-effects approach consists of comparing the rate of change in adult homicide victimization rates in shall-issue states (*a-b*) with the change in non-shall-issue states (*e-f*) to control for unobserved state fixed effects that cause crime rates to differ between

[12] Thanks to an anonymous referee for this suggestion.

TABLE 2. Differences-in-differences-in-differences model

	Pre-shall-issue	*Post-shall-issue*	*Difference*
Shall-issue states			
Adults ("treatment")	b	a	$(a\text{-}b)$
Juveniles ("control")	d	c	$(c\text{-}d)$
Difference in differences			$(a\text{-}b) - (c\text{-}d)$
Non-shall-issue states			
Adults ("treatment")	f	e	$(e\text{-}f)$
Juveniles ("control")	h	g	$(g\text{-}h)$
Difference in differences			$(e\text{-}f) - (g\text{-}h)$
DDD			$[(a\text{-}b) - (c\text{-}d)] - [(e\text{-}f) - (g\text{-}h)]$

Source: Joyce and Kaestner (1996). Cell entries represent homicide rates per 100,000 for the group defined at left.

shall-issue states and other states by the same amount each period. Yet, the fixed-effects approach will not address the effects of unobserved variables that differ between shall-issue states and other states and that vary over time. For example, suppose that crack use and gang activities have increased more substantially during the sample period in states without shall-issue laws relative to states that have such laws. Fixed-effects comparisons will reveal that adult homicide rates have grown more slowly in shall-issue states $[(a\text{-}b) < (e\text{-}f)]$, even if shall-issue laws have no effect on crime.

The "difference-in-difference-in-difference" (DDD) estimation strategy exploits the fact that juveniles are not eligible to obtain gun-carrying permits after shall-issue laws are passed but will still be affected by other fixed and time-varying state-specific factors that influence crime victimization rates. Juveniles thus provide a natural "control group" for examining the effects of shall-issue laws (the "treatment") on adults who are 21 years of age and older (the "treatment group"). The difference between the change in adult homicide victimization rates and the change in juvenile rates $[(a\text{-}b) - (c\text{-}d)]$ differences out the effects of both fixed and time-varying factors that cause both adult and juvenile rates to change over time, and it will reflect only those factors that act on the difference between adult and juvenile homicides. To control for the possibility that there are nationwide changes in the differences between adult and juvenile homicide victimization rates that are independent of the shall-issue laws, the difference in the adult-juvenile trends in shall-issue states are compared with the difference in the adult-juvenile trends in other states $[(a\text{-}b) - (c\text{-}d) - (e\text{-}f) - (g\text{-}h)]$. The DDD estimator thus isolates those factors that are unique to shall-issue states (such as shall-issue laws) that will cause adult homicide rates to decrease relative to the rates for juveniles.[13]

More formally, the proposition that shall-issue laws reduce adult homicide victimization rates suggests that $[(a\text{-}b) - (c\text{-}d) - (e\text{-}f) - (g\text{-}h)]$ will be negative, which can be tested by estimating the following regression model:

$$y_{it} = \theta_0 + \theta_1(Exper_i) + \theta_2(Adult_i) + \theta_3(Post_t) + \theta_4(Exper_i{*}Adult_i) + \theta_5(Adult_i{*}Post_t) + \theta_6(Exper_i{*}Post_t) + \theta_7(Exper_i{*}Post_t{*}Adult_i) + v_{it} \quad (1)$$

The sample used to estimate equation (1) will include two observations for each state (i) for each period (t); one corresponds to the state's juvenile homicide victimization

[13]The DDD estimator is discussed further in Card (1992), Gruber (1994), and Joyce and Kaestner (1996).

TABLE 3. Differences-in-differences-in-differences regression model*

	Pre-shall-issue	*Post-shall-issue*	*Difference*
Shall-issue states			
Adults ("treatment")	$(\theta_0 + \theta_1 + \theta_2 + \theta_4)$	$(\theta_0 + \theta_1 + \theta_2 + \theta_3 + \theta_4 + \theta_5 + \theta_6 + \theta_7)$	$(\theta_3 + \theta_5 + \theta_6 + \theta_7)$
Juveniles ("control")	$(\theta_0 + \theta_1)$	$(\theta_0 + \theta_1 + \theta_2 + \theta_3 + \theta_4 + \theta_5 + \theta_6 + \theta_7)$	$(\theta_3 + \theta_6)$
Difference in differences			$(\theta_5 + \theta_7)$
Non-shall-issue states			
Adults ("treatment")	$(\theta_0 + \theta_2)$	$(\theta_0 + \theta_2 + \theta_3 + \theta_5)$	$(\theta_3 + \theta_5)$
Juveniles ("control")	(θ_0)	$(\theta_0 + \theta_3)$	(θ_3)
Difference in differences			(θ_5)
DDD			$(\theta_5 + \theta_7) - (\theta_5) = (\theta_7)$

Source: Modification of Joyce and Kaestner (1996). Cell entries represent homicide rates per 100,000 for group defined at left.

**Regression model*:

$$y_{it} = \theta_0 + \theta_1(Exper_i) + \theta_2(Adult_i) + \theta_3(Post_t) + \theta_4(Exper_i{*}Adult_i) + \theta_5(Adult_i{*}Post_t) + \theta_6(Exper_i{*}Post_t) + \theta_7(Exper_i{*}Post_t{*}Adult_i) + v_{it}$$

y_{it} = homicide victimization rate for observation (either adult or juvenile) in state (i), period (t)
$Exper_i$ = 1 if state (i) enacts shall-issue law during sample period, 0 otherwise
$Adult_i$ = 1 if observation corresponds to adult victimization rate, 0 if juvenile rate
$Post_t$ = 1 if observation occurs in post-shall-issue law period, 0 if pre-shall-issue law period

rate in period (t), whereas the other corresponds to the adult rate in period (t). That is, with a data sample consisting of N states in the panel, with observations on the states for T periods that span changes in shall-issue law status in a subset of states, then equation (1) is estimated using $2NT$ observations. The variable y_{it} represents a homicide rate measure for state (i) in period (t), whereas $Adult_i$ equals 1 if the observation is for adult homicide rates (zero otherwise), $Exper_i$ is equal to 1 if state (i) adopts a shall-issue law during the sample period (zero otherwise), and $Post_t$ equals 1 if the period is after the shall-issue laws have been enacted (zero otherwise). Equation (1) is estimated using state populations as weights to control for heteroskedasticity in the regression residuals [Greene (1993)].

The parameters in this regression model will capture fixed factors that reflect differences between shall-issue states and other states during the sample period (θ_1), differences between adult and juvenile homicide rates (θ_2), trends over time in homicide rates (θ_3), differences in the effects of fixed-state factors on adults versus juveniles (θ_4), differences in the trends of adult versus juvenile homicide rates over time (θ_5), and differences in homicide trends over time between shall-issue states and other states (θ_6). The key parameter of interest is θ_7, which represents $[(a\text{-}b) - (c\text{-}d) - (e\text{-}f) - (g\text{-}h)]$, the effects of the shall-issue law on the difference between adult and juvenile homicide rates in states that do adopt a shall-issue law during this period versus those that do not.

That the estimate for θ_7 from equation (1) represents an estimate for the quantity $[(a\text{-}b) - (c\text{-}d) - (e\text{-}f) - (g\text{-}h)]$ can be seen with the help of Table 3, which is identical to Table 2 except that the homicide rates are now expressed in terms of the parameters underlying equation (1). For example, the expected value of adult homicide victimization rates in shall-issue states after these laws are passed is given by [$a = (\theta_0 + \theta_1 + \theta_2 +$

$\theta_3 + \theta_4 + \theta_5 + \theta_6 + \theta_7)]$, because each of the dummy variables underlying equation (1) will be equal to one in this case. The expected value of juvenile homicide victimization rates in states that never pass these laws, during the period before the adoption of shall-issue laws by the shall-issue states, is equal to $[h = (\theta_0)]$, because none of the dummy variables are "switched on" in this case. Taking the difference between adult and juvenile homicide trends over time in shall-issue states, and subtracting from this the difference between adult and juvenile homicide trends in non-shall-issue states, leaves us with θ_7.

Note also that the DDD approach differs in important ways from that used in Section IV-C of Lott and Mustard (1997), in which they apply their standard regression model to data for 1977 through 1992 to examine whether shall-issue laws change the age composition of homicide victimizations. They find a negative, but not statistically significant, relationship between shall-issue laws and the proportion of murder victims above some age level, though the specific age cutoff and regression coefficients are not reported. Yet, the strategy of using the ratio of adult to total homicides will not help control for unobserved state factors that vary over time.[14]

Empirical Results

Figure 1 provides a graphical representation of my results. The graph shows trends in the difference between adult (age 21 and over) and juvenile (ages 12 to 17) homicide victimization rates over time for those states that passed a shall-issue law during the period 1977 to 1994 (Florida, Georgia, Idaho, Mississippi, Montana, Oregon, and West Virginia), those that did not have a shall-issue law in effect during this period, and those that had enacted a shall-issue law before the sample period (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington). The sample excludes Pennsylvania, Virginia, and Maine because of the uncertainty surrounding how these states should be classified.

As noted above, any deterrent benefits of shall-issue laws should manifest themselves as a decrease in the difference between adult and juvenile homicide rates. Moreover, any change in the adult-juvenile difference should be greater in shall-issue states than in other states if the shall-issue laws themselves exert any influence on adult homicide rates beyond those factors that affect adult homicide nationwide. However, as seen in Figure 1, adult and juvenile homicide rates converged throughout the United States during the 1980s, and the rate of this convergence in shall-issue states after these laws were passed (1987–1991) does not seem to be noticeably different than the rates observed in other states. Figure 1 thus presents informal evidence that shall-issue laws did not serve to reduce adult homicide rates.

The results of testing this proposition more formally by estimating regression equation (1) are shown in Table 4. The one complication is the proper definition of the pretreatment and posttreatment periods. Because the states that passed shall-issue laws between 1987 and 1991 passed these laws in different years, "the" treatment period actually consists of a several-year window. In my preferred regressions, I define the 10

[14]This can be seen by imagining two separate regression equations with adult and juvenile homicide rates as the dependent variables of interest and the various explanatory variables on the right-hand side. The regression equation with the ratio of adult to total homicides can be written as the ratio of the adult equation divided by the adult plus juvenile equations, with a residual term that still includes unobserved, time-varying state effects that influence adult and juvenile rates equally. These terms will be purged with my differencing strategy.

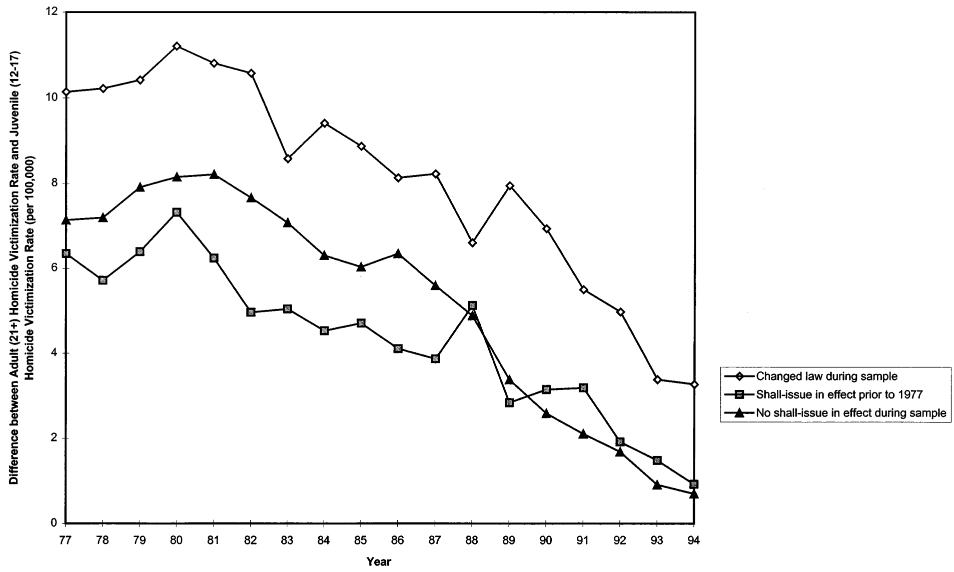

FIG. 1. Difference Between Adult (21+) and Juvenile (12–17) Homicide Victimization Rates, 1977–1994. States that enacted shall-issue laws during the sample period are as follows: Florida (1987), Georgia (1989), Idaho (1990), Mississippi (1990), Montana (1991), Oregon (1990), and West Virginia (1989). States with shall-issue laws in effect during entire sample period are: Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington. Excluded from the sample are Maine, Virginia, and Pennsylvania (see text).

years before Florida's implementation of its shall-issue law as the "pretreatment" period (1977 through 1986) and the 3 years after Montana's shall-issue law as the "posttreatment" period (1992 through 1994).

Because this analysis compares homicide rates that are averaged over several pretreatment and posttreatment years, the method is not well suited for determining whether shall-issue laws have immediate versus gradual effects on crime. If the effects of shall-issue laws change over time, for example because the number of concealed-carry permits issued within a state increases, then the posttreatment effect will reflect the average treatment effect for states with these laws in place for different lengths of time. Any bias that may arise from time-varying treatment effects will be exacerbated by including those states that enacted shall-issue laws before 1977 in the comparison (no change in shall-issue regime) group, because the change in the comparison-group homicide rates in this case may in part reflect changes in the shall-issue "dose" in some comparison-group states. As a result, these states are excluded from the my analytic sample, though below I examine the sensitivity of my estimates to the treatment of these states.

The regression results shown in Table 4 reveal that parameter θ_7, which captures the effects of shall-issue laws on adult homicide rates, is slightly positive, implying an increase of around one-sixth of a homicide per 100,000 adults. With an average adult homicide victimization rate of 11.17 per 100,000 in the United States for 1977 through 1994, this implies an increase of 1.4%. Because the sample of states that change their laws from 1977 to 1994 is relatively small, the standard errors around this point estimate

TABLE 4. Differences-in-differences-in-differences regression results

Explanatory Variable†	Coeff.	Controls for	Estimate (standard error)
Exper = 1 if state ever passes shall-issue law (=0 else)	θ_1	Fixed factors which differ between shall-issue and other states	−1.53 (0.53)*
Adult = 1 if observation is for adult homicide rates (=0 if observation is for juvenile homicide rate)	θ_2	Differences in levels between adult and juvenile homicide rates	7.19 (0.26)*
Post = 1 if period is after implementation of shall-issue laws	θ_3	Trends over time in homicide rates	4.80 (0.44)*
Adult × *Exper*	θ_4	Differences in shall-issue state fixed-effects on adult versus juvenile homicide rates	2.62 (0.73)*
Adult × *Post*	θ_5	Differences in trends of adult versus juvenile homicide rates over time	−6.10 (0.52)*
Exper × *Post*	θ_6	Differences in homicide trends in shall-issue v. other states over time	−1.43 (1.01)
Exper × *Post* × *Adult*	θ_7	Effects of shall-issue laws on adult homicide rates relative to juvenile homicide rates	0.16 (1.42)
N			1,039
Adjusted R^2			0.64

Notes: Preprogram years included in the model are 1977 through 1986. Postprogram years included in the model are 1992 through 1994. The regression model also includes a constant term, the percentage of state population living in poverty, the percentage of state that is African-American, the state *per capita* personal income (measured in 1987 constant dollars), and the percentage of the state population living in urban areas, and it is estimated using state population counts as weights. Shall-issue states are Florida, Georgia, Idaho, Maine, Mississippi, Montana, Oregon, Virginia, and West Virginia. The sample excludes states with shall-issue laws enacted before the sample period (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington), as well as Maine, Pennsylvania, and Virginia (see text). "Pretreatment" period is defined as 1977 to 1986, "posttreatment" period is defined as 1992 to 1994.

* = significant at 1%.

†Dependent variables: Adult (21 and older) and juvenile (12–17) homicide victimization rates per 100,000 population.

are somewhat large. The standard errors imply that the point estimate is not statistically significant, with a 95% confidence interval of −2.68 to 3.00 homicides per 100,000. Yet even fairly small standard errors (such as those produced by Lott and Mustard's county-level ordinary least squares analysis) would imply that these estimates are consistent with positive, negative, or nonexistent effects of shall-issue laws on adult homicides.

As shown in Table 5, the results are not qualitatively different when states with shall-issue laws enacted before 1977 are included in the comparison group for the analysis, when the natural logarithm of the adult and juvenile victimization rates are

TABLE 5. Sensitivity analysis of DDD regression results

Difference in regression model from that used in Table 4	*Estimated effect (standard error) of shall-issue laws on adult homicide rates (per 100,000)*
Alternative weighting variable	
Use adult (21+) rather than total population as weighting variable	0.15 (1.62)
Alternative functional form	
Use natural logarithm of homicide victimization rates	−0.04 (0.19)
Alternative definitions of "pre" and "post treatment" periods	
"Pre-law" period defined as 1982–1986	0.35 (1.65)
"Pre-law" period defined as 1980–1986	0.24 (1.55)
"Post-law" period defined as 1992	0.67 (1.98)
"Post-law" period defined as 1992–1993	0.26 (1.62)
"Post-law" period defined as 1993–1994	−0.09 (1.63)
Alternative "comparison state" groupings	
Include states with shall-issue laws on books before 1977 in comparison group	−0.05 (1.33)
Alternative "shall-issue" state groupings	
Include Pennsylvania as shall-issue state	1.20 (1.23)
Include Virginia as shall-issue state	0.53 (1.30)
Include Maine as shall-issue state	0.43 (1.39)
Drop Florida	0.76 (1.86)
Drop Georgia	1.18 (1.60)
Drop Idaho	0.05 (1.46)
Drop Mississippi	0.11 (1.48)
Drop Montana	−0.01 (1.45)
Drop Oregon	−0.29 (1.50)
Drop West Virginia	−0.29 (1.47)

Notes: Standard errors in parentheses. Results presented above taken from estimating regression equations similar to those underlying Table 4; coefficients presented above correspond to the variable in the last row of Table 4.

used rather than the raw values,[15] or when the adult (rather than total) populations are used as regression weights. The results are also generally not sensitive to the choice of pretreatment and posttreatment periods, though the exclusion of data from 1993 and 1994 causes the estimated positive effect of shall-issue laws on homicide to become even larger. When Pennsylvania, Virginia, or Maine are included, in turn, as shall-issue states, the estimated effect of shall-issue laws on adult homicides becomes even more positive, though the idiosyncracies in how these laws were enacted makes interpretation of these results difficult.

Previous research has found that the shall-issue "treatment effects" implied by the Lott and Mustard model vary quite substantially across states [Black and Nagin (1998)].

[15]Using the natural logarithm for the homicide victimization rates is complicated somewhat by the fact that several states reported no homicides to victims ages 12 to 17 for some of the years between 1977 and 1994. Because the logarithm of 0 is undefined, I substitute the logarithm of (0.1) in these cases. Substitution of the logarithm of yet smaller values will increase the implied difference between adult and juvenile homicides when there are no juvenile homicide cases and will serve to make the shall-issue coefficient more negative.

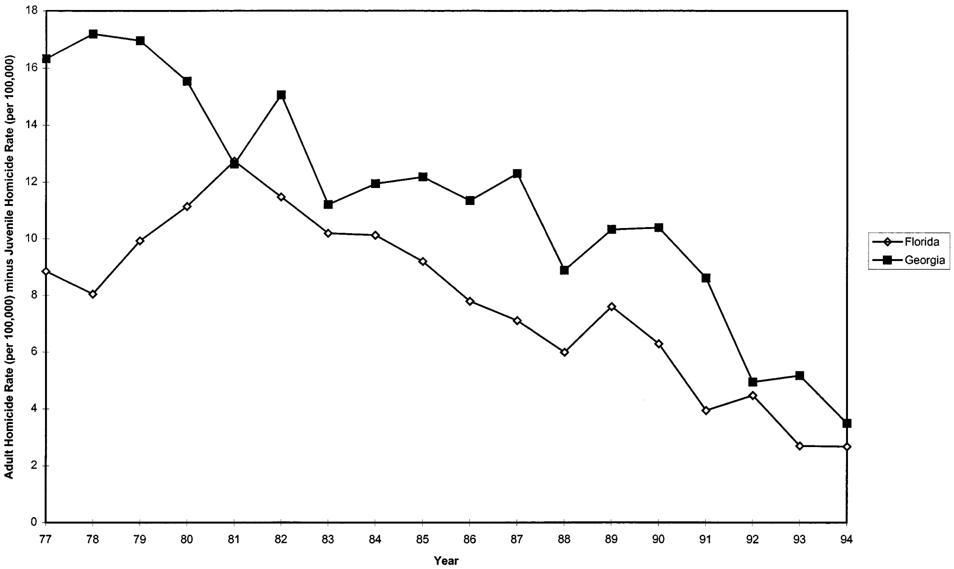

FIG. 2. Difference in Adult (21+) minus Juvenile (12–17) Homicide Victimization Rates in Florida and Georgia, 1977–1994. Florida enacted shall-issue law in 1987, while Georgia enacted shall-issue law in 1989.

This finding may reflect heterogeneity across states that is not captured by the Lott and Mustard regression model, including differences in the way that shall-issue laws are written or enacted and the rate at which citizens within a state obtain concealed-carry permits. For example, state shall-issue laws vary with respect to fingerprint and safety training requirements, as well as to permit application fees, and even to the degree to which carrying privileges are restricted within some counties in a state [National Rifle Association (1998)]. Estimates for the proportion of adults who have been issued permits range from 0.2 percentage points in Mississippi to as high as 6.0% in South Dakota [Hill (1997)]. Although most of the permit holders in shall-issue states seem to be middle-aged white men, there does seem to be some variation across states in the age distribution of those holding permits [Hill (1997)].

Figures 2 and 3 provide informal evidence that the effects of shall-issue laws may vary across states. Figure 2 presents trends in the difference between adult and juvenile homicide victimization rates in Florida and Georgia, those states with the most noticeable changes in the difference between adult and juvenile homicide rates. However, as seen in Figure 3, even after enacting shall-issue laws the remaining states reflect the kind of cyclicality in homicide rates that is typical in the United States [Blumstein (1995)].

The sensitivity of my estimates to the exclusion of each shall-issue state in turn is shown in Table 5. As suggested by Figures 2 and 3, evidence for any crime-reducing benefits are concentrated in Florida and Georgia: The exclusion of these states causes the estimated effect of shall-issue laws on adult homicides to become even more positive. This finding is consistent with Black and Nagin (1998), who note that many of the negative shall-issue effects estimated by Lott and Mustard (1997) disappear once Florida is excluded from the sample. The results are generally not sensitive to excluding any of the other shall-issue states from the sample, or even to excluding such atypical

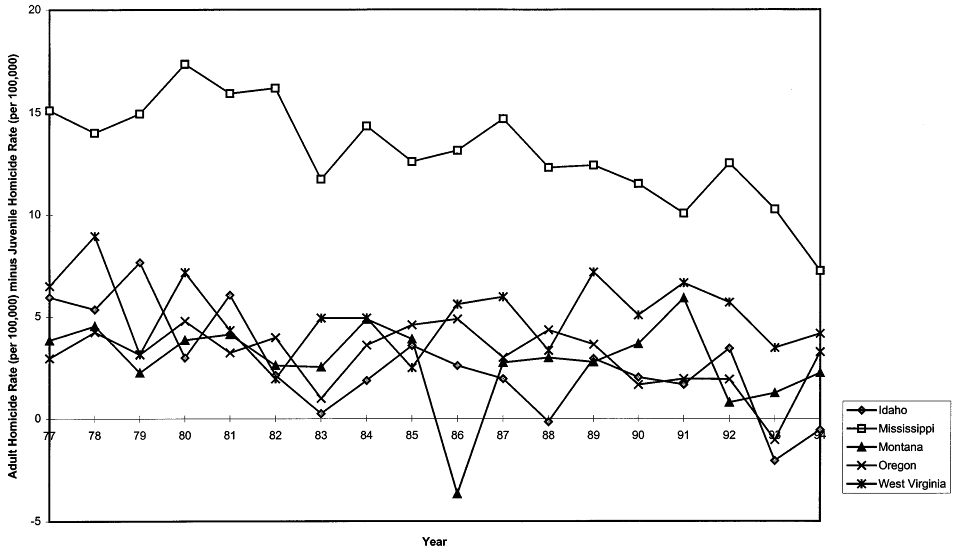

FIG. 3. Difference between Adult (21+) and Juvenile (12–17) Homicide Victimization Rates for Idaho, Mississippi, Montana, Oregon, and West Virginia, 1977–1994. Idaho, Mississippi, and Oregon enacted shall-issue laws in 1990, whereas Montana enacted a shall-issue law in 1991 and West Virginia in 1989.

non-shall-issue states as California or New York, with estimated effects that are consistently no larger than one-third of a homicide in absolute value. Taken together, this analysis produces little evidence that shall-issue laws reduce crime and suggests that these laws are as likely to cause crime to increase as to decrease.

IV. Discussion

Whether "shall-issue" laws that liberalize concealed-handgun-carrying requirements cause crime rates to increase or to decrease has become an increasingly important public policy question, as a growing number of states adopt or consider such legislation. The widely publicized study of Lott and Mustard (1997) suggests that shall-issue laws reduce crime and save lives and money. However, as I have argued above, the Lott and Mustard study does not seem to have controlled adequately for omitted variables and other problems and, as a result, is unlikely to provide reliable information about the effects of shall-issue laws on crime.

In this paper, I present the results of an alternative test for the effects of shall-issue laws on homicide rates that exploits the fact that juveniles are not eligible for concealed-carry permits to control for time-varying unobserved state factors. The results of my analysis suggest that shall-issue laws have resulted, if anything, in an increase in adult homicide rates.

What explains the difference between the findings in Lott and Mustard (1997) and those presented here? My use of state-level rather than county-level data is unlikely to explain the difference, inasmuch as Lott and Mustard's analysis of state-level data using their fixed-effects regression approach produces results that are similar to their county-level analysis. The additional 2 years of data that I use (1993 and 1994) also do not seem to explain the difference across studies, because excluding data from 1993 and 1994 in my analysis causes the estimated positive effect of shall-issue laws on adult homicides to become even larger.

I believe that the most compelling explanation for the differences between the results in Lott and Mustard (1997) and those presented here is that my estimation strategy is able to more adequately control for unobserved state variables that vary over time. Lott and Mustard's (1997) analysis is susceptible to bias from any unobserved state or county factor that varies over time, which in fact seems to be the case on the basis of Black and Nagin's (1998) analysis and the implausibility of Lott and Mustard's 2SLS results. In contrast, only social or public policy changes that are unique to shall-issue states, concurrent with the implementation of these laws, and that affect the difference between adult and juvenile homicide rates may impart bias to the estimates presented here. It is also possible that some criminals change their behavior after shall-issue laws are passed and now either victimize juveniles instead of adults or leave crime altogether, in which case my estimates may be subject to a slight negative or positive bias, respectively. The possibility of some unmodeled heterogeneity in my estimates is suggested by the sensitivity of the estimates to the exclusion of Florida and Georgia from the sample; these sample restrictions cause the estimated positive effect of shall-issue laws on homicide to become even larger. My results are generally robust to dropping other shall-issue and non-shall-issue states from the analytic sample.

The omitted variables problems highlighted in this paper are of general concern in evaluating the effects of anticrime efforts and seem even more severe than the problems involved in evaluating other areas of public policy such as education. In both crime and education, many of the important factors that influence policy outcomes vary at the local level. In the area of education policy, the government has invested substantial resources to collect rich data at levels as disaggregated as the school or student. In contrast, many of the important factors that influence crime are not measured, are not systematically compiled by government agencies, or are unusually difficult to measure. Even sophisticated measurement techniques such as fixed-effects or 2SLS models may produce biased estimates in the face of these problems, given that many of the unmeasured factors that cause crime are likely to vary over time and that valid instrumental variables are difficult to find. Public policymakers should be made aware of the unique identification problems in evaluating anticrime policies such as concealed-carry laws and should recognize that even elaborate studies such as Lott and Mustard (1997) may not provide reliable information. There may be many reasons for state and federal legislators to support shall-issue laws, but the belief that these laws reduce crime should not be one of them.

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