Criminal Deterrence, Geographic Spillovers, and the Right to Carry Concealed Handguns

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Increased law enforcement or penalties may deter crime, but they may also cause criminals to move to other crimes or other areas. When a car owner uses "The Club," auto thieves respond by moving on to other cars unprotected by such a lock.[1] Auto theft should decline if more car-owners lock their cars as the cost of searching for easily stolen cars rises, and the extent of the decline is an empirical question. The movement of criminals between jurisdictions is similar, though more difficult to predict. Greater law enforcement may cause criminals to migrate to bordering areas. However, if criminals were using the original location as a "staging" area for crime into surrounding communities, crime should decline elsewhere (e.g., clamping down on automotive "chop shops" or fences thus reduces the return to theft in surrounding communities).[2] Despite the potential importance for evaluating law enforcement, spillovers have not been investigated empirically. Existing studies of local law enforcement might either over- or underestimate the overall benefits from deterrence.

This paper examines whether the adoption of a shall-issue concealed-weapons law in one state alters crime in neighboring areas. The shall-issue law guarantees a citizen who meets certain objective criteria the right to carry a gun. Lott and David B. Mustard (1997) found a strong local deterrent effect of the law, but we wish to examine whether much of this merely represents crime moving elsewhere (see also William Alan Bartley and Mark A. Cohen [1998] and Lott [1998a, b]). Alternatively, simultaneously passing shall-issue laws in a number of states might reduce a state's crime rate by even more than simply adopting the law on its own. By incorporating spillover effects into our analysis, we estimate the aggregate effect of shall-issues laws on crime rates. To our knowledge, this paper constitutes the first attempt to include spillover effects in estimating criminal deterrence. The methodology used here could be extended to other crime-deterrence policies.

I. Data and Methodology

This paper uses annual cross-section time-series county-level crime data for the continental United States from 1977 to 1992. We weigh all regressions by county population, and we include county-fixed effects and year-fixed effects in each of the regressions reported below. Therefore, all the estimated effects of concealed-weapons law on crime are derived from changes within a given county, relative to the year-to-year changes in the overall U.S. crime rate.

For each county-year we employ a set of demographic characteristics: the distribution of the county's population by age, race, sex, average income, welfare, and population density. We include this vector of demographic characteristics in all the regression characteristics below (see Lott and Mustard [1997] for further discussion of these variables). The crime data are from the FBI's Uniform Crime Report. Our primary dependent variables are the reported crime rates per 100,000 population per county per year for nine different

[1] For a survey of the theoretical literature on protective spillovers see Kermit Daniel and Lott (1995).

[2] "Thriving" criminal communities may also facilitate people becoming criminals. If there are fences on every street corner in a city, it is much easier for thieves from outlying areas to come to the city to sell their stolen goods. Penalties that affect the number of "fences" (either by directly punishing the fences or reducing the number of criminals who seek them out) could reduce crime in surrounding areas.

crime categories: overall violent crimes, murders, rapes, robberies, aggravated assaults, overall property crimes, burglaries, auto thefts, and larcenies.

We extend the analysis of Lott and Mustard (1997) to incorporate the impact on neighboring counties. A neighboring county is here defined as any county with a geographic center within 50 miles of the geographic center of the home county.[3] We compare variation in crime rates across counties within the same state over the same time period. A change in concealed-weapons laws in a given state is assumed to affect crimes in neighboring counties. For example, when Georgia introduced a shall-issue law in 1989, it likely affected neighboring counties in Florida, Tennessee, Alabama, and South Carolina. The effects of the Georgia law on neighboring counties in Tennessee was estimated by comparing time-series fluctuations in crime for these counties to other counties in Tennessee. Further, some states bordering Georgia had already passed these laws, and we assume that, for this reason, the spillover effects could also differ across states.

While our initial specifications correspond to Lott and Mustard's simplest estimates, our other specifications are different. First, we control for either the violent or property-crime arrest rate depending upon whether the crime rate being studied is that of violent or property crime. This mitigates any spurious relationship between crime and arrest rates that might arise because the arrest rate is a function of the crime rate. It also helps solve the missing-value problem arising because the arrest rate is undefined when no crime occurs,[4] but it still allows us to control for changes over time in the effectiveness of law enforcement. Including the arrest rate for the detailed crime category or omitting the arrest rate from the

regression produces similar results. Second, we use the crime rate, rather than the log of the crime rate, as the dependent variable to test whether the results are sensitive to examining absolute rather than percentage changes in the crime rate.

Again, following Lott and Mustard, our regressions capture the effects of concealed-weapons laws over time by including linear trend variables for the years before and after the change in the law, along with a shall-issue dummy variable.[5] Theoretically, the long-run impact ought to be larger as law-abiding citizens and criminals alike adapt to changes in the weapons laws. This simple parametric specification can accommodate a once-and-for-all change as well as cumulative changes which may reinforce or mitigate the immediate impact. For many of the jurisdictions in our sample only a few years elapse after passage of the law, which means that we cannot confidently evaluate the cumulative impact beyond four years.

II. Empirical Results

Part A of Table 1 reports murder, rape, and robbery regression results similar to those in Lott and Mustard's (1997) table 3, with the addition of spillover effects for shall-issue laws in neighboring counties (results that are not shown are available from the authors upon request). We use the log of the crime rate as the dependent variable. Independent variables include the arrest rate for the corresponding crime category, a dummy variable for a state's own shall-issue law, a variable measuring the (population-weighted) fraction of neighbors with shall-issue laws, and an interaction between the own and neighboring shall-issue variables as explanatory variables. The own effects of the law reduce violent crimes and increase property crimes, especially auto theft and larceny. The neighboring shall-issue coefficient measures the effect of a neighbor's law on a home county without a shall-issue law. Except for assaults, these spillover effects

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[3] To test for sensitivity, we tried two other definitions of neighbors: counties that were adjacent to each other or were within 100 miles of the geographic center of the home county. Neither definition altered the results shown here, though the spillovers were about 15-percent larger for counties within 100 miles than for those within 50 miles.

[4] This is most important for murder and rape. See Lott and Mustard (1997) and Lott (1998a) for other approaches to deal with the problem of missing observations.

[5] Trends are used for counties in states that enacted the law during the period studied. Prior to 1977 the last state to enact a shall-issue law was Washington in 1961.

TABLE 1—EFFECTS OF OWN AND NEIGHBORING
SHALL-ISSUE LAWS ON CRIME RATES

Independent variable	Regression coefficients		
	Murder	Rape	Robbery
Part A:[a]			
Own shall-issue dummy	−6.57	−4.15	−2.76
	(3.848)	(3.131)	(1.908)
Neighboring shall-issue dummy	4.5	7.45	4.2
	(1.676)	(3.59)	(1.851)
Own shall-issue × neighboring shall-issue	−8.7	−7.29	0.41
	(2.383)	(2.580)	(0.131)
N:	25,931	33,121	34,271
Part B:			
Own shall-issue dummy	−1.401	−1.848	−15.00
	[−16.1]	[−5.2]	[−6.8]
	(8.07)	(3.80)	(3.83)
Neighboring shall-issue dummy	0.816	−0.348	15.72
	[+9.4]	[−1.0]	[+7.1]
	(3.68)	(0.56)	(3.14)
Own shall-issue × neighboring shall-issue	−0.815	0.321	−26.80
	[−9.4]	[+0.9]	[−12.1]
	(2.71)	(0.38)	(3.94)
Difference in trends before and after law	−0.124	0.264	−4.81
	[−1.4]	[+0.7]	[−2.2]
	(5.22)	(3.97)	(8.99)
Part C:			
Effect of own law in fourth year of law	−1.774	−1.011	−28.95
	[−20.4]	[−2.8]	[−13.1]
	(8.37)	(1.70)	(6.05)
Effect of neighbor's law in fourth year, own law = 0	0.952	2.352	35.87
	[+11.0]	[+6.6]	[+16.2]
	(2.45)	(2.16)	(4.09)
F test: own effect + neighbor's effect = 0	p = 0.0076	p = 0.2940	p = 0.5011
Effect of neighbor's law in fourth year, own law = 1	0.140	2.713	9.34
	[+1.6]	[+7.6]	[+4.2]
	(0.33)	(2.24)	(0.96)

Notes: The regressions in part A use the log of the crime rate and the arrest rate that corresponds directly to the crime rate being evaluated. Parts B and C were run on the actual crime rate, and the implied percentage change in crime rates as evaluated at the mean is reported in brackets. Because parts B and C use the violent-crime arrest rates for all the violent-crime categories and the property-crime arrest rates for all the property crime arrest rates, the sample sizes for those regressions are 44,445 for violent-crime categories and 42,326 for property-crime categories. The absolute values of *t* statistics are shown in parentheses.

[a] Main table entries in part A are percentages.

are either deleterious or insignificant. Some spillover effects are substantial: rapes increase by 7.45 percent, robberies by 4.2 percent, and murder by 4.5 percent. Only for assaults is the

estimated spillover effect beneficial, reducing neighboring assault rates by 3.6 percent.

Adding the coefficients for the neighboring shall-issue variable and the interaction effect provides the effect of a neighbor's law if the home county already has a shall-issue law. In crime categories where the neighboring effect is either economically important or even marginally significant (murder, rape, robbery, assault, property crime, and burglary) the interaction effect is always of the opposite sign. Therefore the magnitude of spillover effects are mitigated for counties that have already enacted shall-issue legislation.

Part B allows for spillover effects and trend-rate effects of the own-county law, as well as the changes described in Section I, using the aggregate arrest rates and the crime rate instead of the logarithm of the crime rate as the endogenous variable. Consider the estimates for the violent crime rate. The coefficient on the own shall-issue dummy is −34.16, implying a decrease in the violent crime rate of 34.16 per 100,000 residents, relative to what would have occurred in the absence of the law (a decrease of 5.9 percent). The trend coefficient is reduced by 3.46 after passage of the law, so that in the second and third years of the law the violent crime rate is reduced by 37.62 (34.16 + 3.46) and 41.08 (34.16 + [2 × 3.46]) per 100,000 residents, respectively. In every crime category except larceny, the short-run impact of the own shall-issue law significantly reduces crime rates. For murder, robbery, property crime, burglary, and auto theft, the deterrent effect of the law grows over time, while it gradually diminishes for assaults and rapes.[6] Except larceny, the own effect of the law reduces all crimes over the first seven years.

The second row in part B indicates that spillover to neighbors tends to be deleterious: the murder rate, robbery rate, overall property crime rate, and burglary rate all significantly increase (only assaults decline). Some of these changes are quite large: murders increase by 9.4 percent, and robberies increase by 7.1

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[6] In part B, if the dependent variables are instead the logs of the crime rates, the trend effects of the own law are significantly negative for all crime categories, including rape and assaults.
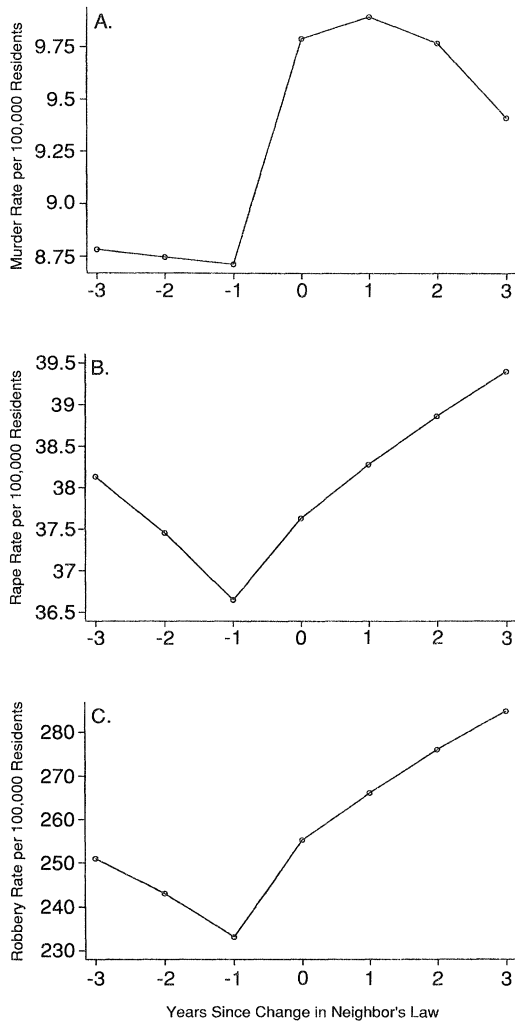
FIGURE 1. THE EFFECT OF A NEIGHBOR'S SHALL-ISSUE
LAW ON CRIME: (A) EFFECT ON OWN MURDER RATE;
(B) EFFECT ON OWN RAPE RATE; (C) EFFECT
ON OWN ROBBERY RATE

percent in nearby counties without a shall-issue law.

If the home county already has a shall-issue law, the impact of the neighbor's law is the sum of the neighbor's law and interaction coefficients. In every crime category (other than larceny) the own-county × neighboring-county interaction has the opposite sign of the neighboring-county effect. Thus the home county appears largely insulated from the del-

eterious effects of a neighbor's shall-issue law if the home county already allows citizens to carry concealed weapons. If the home county has a right-to-carry law, the only spillover effects that can be statistically determined from a neighbor's shall-issue law are beneficial: decreases in larceny and overall property-crime rates.

Parts A and B of Table 1 do not allow for any trend effects of the neighbor's shall-issue law on the home county. We now modify the specification in part B by including time-trend variables before and after the neighbor's right-to-carry legislation was enacted and present results from these regressions in part C of Table 1. Rather than reporting all the shall-issue variables, we summarize the results by only presenting estimates for the impact of shall-issue laws in their fourth year. We consider three types of changes in shall-issue laws: the own-county effect alone, the neighbor or spillover effect if the home county does not have a shall-issue law, and the spillover effect if the home county has a shall-issue law. Consider first the impact of a change in own-county right-to-carry laws. The own-law effects in part C are virtually identical to those in part B: allowing citizens to carry concealed weapons leads to a significant reductions in crime rates in every crime category (other than larceny). Murder and robbery respectively decline by 20 percent and 13 percent.

The second row of part C measures spillovers to counties without a right-to-carry law. A shall-issue law in neighboring counties leads to significant increases in murders, rapes, robberies, property crimes, burglaries, and larcenies. Many of these deleterious spillover effects are quite large. For rapes, property crimes, burglary, and larceny, these spillover effects are even larger than the own shall-issue effect. The only evidence of beneficial spillover effects is for assaults and auto theft.

The final row of part C presents estimated spillover effects for a home county with the shall-issue law. In general, the presence of an own shall-issue law appears to mitigate the deleterious spillover effects from a neighbor's shall-issue law. In particular the harmful spillover effects for murder, robbery, property crimes, and burglary are eliminated by the presence of an own-county shall-issue law.

Harmful spillover effects remain for rapes and larceny, and there are beneficial spillover effects for larceny.

The spillover effects are vividly illustrated in Figure 1, which is based on part C of Table 1 and the inclusion of additional squared time trends for the periods before and after the adoption of the law. Year 0 is the year when a neighbor's shall-issue law takes effect. All four of the violent-crime categories show dramatic crime spillovers precisely at year 0.

We also investigated whether changing arrest rates in neighboring areas similarly affect crime in the home county. Although we consistently find a strong negative relationship between the arrest rate and crime for the home county, we cannot detect any spillover effects for arrest rates. With one exception, the coefficients on neighboring arrest rates are at most one-eighth as large as the coefficient on the own arrest rate, and the *t* statistics are never significant.

III. Conclusion

The benefits that a county obtains from its state passing a shall-issue concealed-handgun law are generally stronger than those found in previous work. Spillover effects on neighboring areas are almost always deleterious. Criminals tend to move across communities more readily in response to changes in concealed-handgun laws than in response to changes in arrest rates. The spillover effects are surprisingly large, especially for property crimes, thus challenging existing research which ignores these considerations. The spillovers are immediate and increase over time (with the exception of assaults and auto theft). Except for rapes, the negative effects of a neighbor's law are mitigated by adoption of the law by one's own state. Taken together, these results imply that concealed handguns deter criminals and that the largest reductions in violent crime will be obtained when all the states adopt these laws.

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