PRIVATELY PRODUCED GENERAL DETERRENCE*

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Abstract

In this study, we use county data on private security establishments and employment for 1977–92 to test two hypotheses. First, we test whether private security deters crime. Second, we test whether John Lott and David Mustard's estimates of the impact of shall-issue laws on crime are biased because of a lack of controls for private security. We find little evidence that private security reduces the crime rates for assault or larceny. Some estimates suggest murder, robbery, and/or auto theft may be deterred by private security, although these results are not robust. Of all the index crime categories, only rape is estimated to have a consistent negative relationship with private security. In addition, we find little evidence that the Lott and Mustard results are biased because of a lack of controls for the private security measures employed in this study.

I. Introduction

A large empirical literature tests various implications of Gary Becker's path-breaking article "Crime and Punishment," but one aspect of his analysis is largely ignored—his discussion of "private expenditures against crime."[1] Despite the fact that more is spent on private security than on public policing in the United States,[2] the potential for general deterrence from privately produced crime control services has not been considered in empirical studies

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[1] Gary S. Becker, Crime and Punishment: An Economic Approach, 76 J. Pol. Econ. 169, 200–201 (1968).

[2] Bruce L. Benson, To Serve and Protect: Privatization and Community in Criminal Justice 75–93 (1998).

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of crime until recently.[3] For instance, as suggested by the Lott and Mustard[4] (hereafter LM) study of concealed firearms, "general deterrence" implies an overall reduction in a crime rate (or rates). In contrast, "specific" or "localized" deterrence reduces crimes against protected targets, but perhaps not overall crime, because criminals shift to unprotected targets. Furthermore, in regard to LM, Edward Glaeser and Spencer Glendon[5] concluded that gun ownership tends to be most acceptable where there is a mistrust or dissatisfaction with public justice and a "tradition of private retribution." If shall-issue right-to-carry concealed handgun laws tend to be passed in states where citizens generally prefer to take responsibility for their own protection, then these laws might be highly correlated with private investments in crime prevention, other than concealed handguns, which also deter crime. As a consequence, the shall-issue coefficients in LM's study may be larger than they would be if controls for other private security investments were included. Therefore, this study adds controls for the private security industry to the LM data in order to see whether (1) private investments in crime control other than concealed firearms produce general deterrence and/or (2) LM's failure to control for such private investments biased their shall-issue coefficients.[6]

The deterrence model to be tested, data that are employed, and hypotheses are discussed in Section II. Section III reports empirical results. Shall-issue coefficients remain robust, suggesting that LM's conclusions do not suffer from failure to control for those aspects of private security services examined here. More importantly, some types of crime do appear to be deterred by

[3] For instance, Edwin W. Zedlewski, Private Security and Controlling Crime, in Privatizing the United States Justice System: Police Adjudication, and Corrections Services from the Private Sector (Gary W. Bowman *et al.* eds. 1992) used a cross section of 1977 data from 124 Standard Metropolitan Statistical Areas and found a significant negative impact of private security employment on total reported crime.

[4] John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1 (1997).

[5] Edward L. Glaeser & Spencer Glendon, Who Owns Guns? Criminals, Victims, and the Culture of Violence, 88 Am. Econ. Rev. 458, 462 (1998).

[6] The LM conclusions that violent crimes are significantly deterred when citizens are allowed to carry concealed handguns has stimulated a substantial amount of additional research. Several studies are critical, but John R. Lott, Jr., More Guns, Less Crime: Understanding Crime and Gun-Control Laws 128–57 (1998), offers convincing rebuttals to many of them. Some criticisms, such as those of Ian Ayres & John J. Donohue III, Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy, 1 Am. L. & Econ. Rev. 436 (1999), and Mark Duggan, More Guns, More Crime (Working Paper, NBER, October 2000), are more recent than Lott's, but John R. Lott, Jr., More Guns, Less Crime: A Response to Ayres and Donohue (Yale L. & Econ. Res. Paper No. 247, 1999), continues to offer counterarguments to such challenges. Furthermore, some studies, such as Florenz Plassmann & T. Nicolaus Tideman, Does the Right to Carry Concealed Handguns Deter Countable Crimes? Only a Count Analysis Can Say, in this issue, at 771; and Carlisle E. Moody, Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness, in this issue, at 799, also support LM. Thus, the issues they raised are clearly not settled.

private security. Some crimes apparently are not, however, and the conclusions in Section IV explain why this is not surprising.

II. Model, Data, and Hypotheses

A. Model

The first model tested below is

$$C_{it} = f(X_{it}, P_{it}, R_{it}, S_{it}, J_i, T_t), \quad (1)$$

where C_{it} is a crime rate (nine different crime rates are examined, as in LM) in jurisdiction (county) i in year t. The term X_{it} is the vector of variables LM used to control for legal income opportunities (income, unemployment, poverty, and so on) and other sociodemographic factors (such as race, gender, and age distributions and urbanization) that might affect the opportunity cost of crime. The term P_{it} is the LM control for expected punishment, the arrest rate, proxying for the probability of arrest. The LM findings also suggest that the risk of injury or death for criminals in states with shall-issue right-to-carry concealed handgun laws, R_{it}, is a cost of crime that can reduce some crime levels. Finally, we hypothesize that private security, S_{it}, may deter crime. Lott and Mustard also used fixed effects to deal with unobserved heterogeneity in pooled time-series cross-sectional data, so jurisdiction (J_i) and year (T_t) fixed effects are included in the empirical model.

The results from weighted least squares (WLS) regressions may not provide accurate insights regarding the potential general deterrence impact of private security. After all, even if large investments in private security reduce crime, high crime rates may also stimulate demand for more private security. Indeed, simple correlations between private security measures employed below and crime rates reveal a positive relationship, suggesting that the demand-enhancing impact may be quite important. Because coefficients on private security measures in WLS crime rate regressions may suffer from simultaneity bias, two-stage least squares (2SLS) models are estimated treating private security and arrest rates as endogenous. The second-stage regression is given by equation (1), but with both P_{it} and S_{it} estimated in first-stage regressions that include instrumental variables (I_{it} in the regression for P_{it} and V_{it} for S_{it}):[7]

$$P_{it} = f(I_{it}, C_{it}, R_{it}, J_i, T_t) \quad (2)$$

[7] The term R_{it} may be endogenous too, as LM explain. However, there are no changes in the law for most counties. Therefore, a 2SLS fixed-effects model cannot be estimated with this dummy as endogenous, because a probit for the law cannot be estimated with county fixed effects.

and

$$S_{it} = f(V_{it}, C_{it}, R_{it}, J_i, T_t). \quad (3)$$

B. Data

John Lott provided us with the LM data. These data cover the 1977–92 period for each county in the United States. Included are data on both reported violent crime rates (total violent crimes, as well as separate measures for murder, rape, robbery, and aggravated assault) and property crime rates (total property crimes, plus rates for burglary, larceny, and auto theft) to be used as dependent variables, along with variables indicating (with a zero-one dummy) whether the county is in a state with a shall-issue law, the arrest rates for each crime category, county population and population density, various measures of county income, and a large number of variables that classify county population by race, gender, and age.[8] The only systematically collected annual data on private security at the county level is in *County Business Patterns* (hereafter *CBP*), which reports the number of establishments specialized in providing security and detective services and employment by those establishments. These data were obtained (as were other data used as instruments for 2SLS models), and the data for 1977–92 were added to the LM data. They reveal that large numbers of new establishments enter the private security market virtually every year.[9] Between 1964 and 1997 (1977 and 1992), for instance, the number of establishments offering such services within the United States grew by 800.7 (118.8) percent, from 1,988 (6,312) nationally to 17,907 (13,810). Employment by these establishments also increased dramatically over the same period, by 924.4 (92.1) percent, from 62,170 (268,684) to 636,884 (516,236).

Regrettably, both county-level cross-sectional and consistent time-series data on other aspects of private security are simply not available. For instance, while it is widely recognized that markets for security technology are growing rapidly, evidence regarding the increasing use of alarms and other types of security equipment comes from various national surveys rather than from systematically collected data at the county level. There are also no data that can capture the technological advances in security equipment.[10] The same is true for security employment internal to firms and organizations that do not specialize in the security or detective agency markets. Information on this type of security employment is available from the census, but only once a

[8] See Lott & Mustard, *supra* note 4, for details.

[9] Data are for Standard Industrial Classification (SIC) Code 7393 (Detective Agency and Protective Services) for the 1977–87 period; SIC 7393 was split into 7381 (Detective and Armored Car Services) and 7382 (Security Equipment Services) in 1988, so these two classifications are added for 1988–92.

[10] Benson, *supra* note 2, at 75–93.

decade. Therefore, while the census data could be superior to the *CBP* data for exploring the private-security deterrence hypothesis, these data are inadequate for the other purpose of this study: consideration of potential missing-variable bias in LM.

County Business Patterns reports total establishments in each county for a particular Standard Industrial Classification (SIC). It also reports total employees and total payroll as long as the report will not disclose operations for individual establishments. This turns out to be a serious drawback, because the number of establishments in many counties is small and the employment and payroll figures are therefore not reported. Payroll and employment data are missing for so many counties that they are, by themselves, inadequate for the purposes of this study. However, *CBP* also reports the numbers of establishments in all counties in each of 12 different size categories defined by the number of employees in the establishment (1–4, 5–9, 10–19, 20–49, 50–99, 100–249, 250–499, 500–999, 1,000–1,499, 1,500–2,499, 2,500–4,999, and 5,000+). Therefore, we estimate the number of security employees in a county where the number is not reported by multiplying the number of establishments in each size category by the median number of employees in the category (there are no establishments in the 5,000+ category, so the fact that it does not have a median is not a problem). Since the establishment data are consistently reported, but many of the employment observations must be estimated, results using both measures of private security activity are presented below.[11] Additional data are obtained from *CBP* to serve as instruments in the first-stage private security regressions for the 2SLS models (some data from LM are also employed for this purpose). These *CBP* (and LM) variables are listed in the attached Appendix and discussed below where the empirical results are described.

C. Hypotheses

1. Hypothesis 1: Private Security Produces General Deterrence

While there is a good deal of evidence of a specific or localized deterrence impact of private security,[12] this may not translate into general deterrence if it simply induces criminals to substitute unprotected targets for those protected by private security inputs. After all, many investments in private security are effective as specific deterrents because they are very visible. Most of the security officers patrolling malls and mall parking lots and standing in bank lobbies are uniformed so the potential criminal is aware of their

[11] The mean of total security establishments per 100,000 persons is 1.602. The standard deviation is 2.996. Security employment per 100,000 persons mean and standard deviation are 41.172 and 134.408. There are 49,947 observations. See Lott & Mustard, *supra* note 4, at 14–15, for summary statistics on their data.

[12] See Benson, *supra* note 2, at 152–67, for a review.

presence, many homes protected by alarms with response services announce that fact with a sign, the video camera on the automatic teller machine is clearly visible, and so on. Therefore, they may not produce any positive externalities in the form of general deterrence because criminals are simply diverted to alternative targets. Indeed, sellers of security services have strong incentives to exclude nonpayers from consuming benefits, so they should attempt to devise technologies and contractual arrangements that prevent significant spillover benefits. Becker[13] also suggested that many private inputs to crime control are not likely to have large general deterrence impacts.

Still, some general deterrence could be evident for at least four reasons. First, if criminals substitute unprotected targets for protected targets, entrepreneurs may recognize the new opportunity and happily offer more security services for other potential targets as well. And if potential victims respond, then private security could spread. If the level of private security becomes sufficiently widespread, the expected cost of searching for targets and/or committing crimes could rise, making potential criminals less likely to become actual criminals. Second, it may be that sellers of security services cannot exclude all nonpayers. If some potential crime victims are able to alter their behavior in ways that take advantage of security service paid for by others, perhaps for other purposes, then general deterrence impacts for some types of crimes may arise as a positive externality. Firms in a shopping or entertainment area may employ security primarily to prevent shoplifting, vandalism, and employee theft, for instance, but a potential victim of robbery or rape may choose to shop or socialize in that area to take advantage of the security presence. If a substantial portion of potential victims behave in this way, robberies and/or rapes could be reduced because the cost for potential criminals of finding an easy target is higher. Third, such external benefits clearly could (and probably will) be internalized as retailers and providers of entertainment services recognize that a relatively secure environment is attractive. They may bundle security with others goods and services, building the cost into the prices they charge for those commodities. The general deterrence impact would still arise, but it would be paid for by those who benefit from it. A fourth potential reason for general deterrence is that some private security investments are not visible. This is the case for concealed handguns, of course, but it also can arise for other security equipment (such as Lojak for cars) and for security personnel (such as plainclothes security guards or detectives). The resulting uncertainty about where such security is present could produce a general reduction in crime similar to what LM conclude arises with the risk of confronting an armed victim. The incentives of the suppliers of security services to internalize benefits suggest that this may not be a large factor, of course, unless the benefits for which

[13] Becker, *supra* note 1, at 201.

suppliers can charge for such unobservable security are large relative to the cost.

2. Hypothesis 2: Lott and Mustard's Coefficients Are Biased

Recall the Glaeser and Glendon[14] conclusion that gun ownership tends to be most acceptable where citizens generally prefer to take responsibility for their own protection. If shall-issue laws are passed in states where such attitudes are prevalent, then these laws might be highly correlated with all sorts of private investments in crime prevention and detection. Indeed, preliminary examination of the data suggests that there may be a relationship between private security and shall-issue laws. Looking at averages over the entire data period is less revealing than considering trends. Nevertheless, the mean number of security establishments per 100,000 persons in counties from states where and when shall-issue laws exist is 1.785 during the 1977–92 period, compared with 1.565 where and when such laws are absent. That is, states with shall-issue laws have approximately 1.14 private security firms for every firm in states without such laws. Similarly, for the same data sample, the mean of estimated security employment per 100,000 population in counties from states with right-to-carry laws is 41.929, while it is 41.022 for states without such laws, suggesting that there are about 1.02 security employees per 100,000 persons for states with such laws for every 1.00 in states without the laws. While these differences may appear to be small, dramatic differences in preferences are not required for the median voter to shift from opposing shall-issue laws to supporting them.

In this context, the differences between the states with shall-issue laws and those without the laws are much greater in recent years than in early years. For instance, if we consider only the 1984–92 period, the mean number of establishments per 100,000 persons in counties from states with shall-issue laws is 2.095, compared with 1.666 for states without the laws. States with shall-issue laws, therefore, have approximately 1.26 private security firms for every firm in states without such laws during this more recent period. For the same 1984–92 period, security employment per 100,000 persons in states with the laws is 53.945, while it is 47.884 for states without the laws, implying that there are about 1.13 security employees in shall-issue states for every security employee in non-shall-issue states. Thus, private security services appear to be increasing faster in states with shall-issue laws than in states without such laws, and the hypothesis follows, if private security services deter crimes, then failure to control for such activity increases the apparent deterrence impact of shall-issue laws.

In this regard, it is also important to note that only 10 states actually changed their laws during the 1977–92 period (eight states passed such laws

[14] Glaeser & Glendon, *supra* note 5, at 462.

before 1977, and the rest do not have such laws for our data period). Therefore, if those that changed their laws during this period have high levels of private security relative to those that do not have such laws, then the LM coefficients on their shall-issue dummy variables could again be biased. The means of security establishments and employment in states that never adopted shall-issue laws during the 1977–92 period are 1.548 and 41.679, respectively, compared with 1.864 and 44.382 for the 10 states that adopted the laws during the time period. Thus, states that adopted the laws had an average of 1.20 establishments and 1.06 employees for every one in states that did not adopt the laws. Finally, note that coefficients on shall-issue laws are significantly positive in regressions explaining private security employment, although they tend to be insignificant in security establishment regressions.[15] Of course, the alternative hypothesis, that the LM coefficients are not biased because of missing private security measures, is possible if private security does not have a general deterrence impact. Even if there is a general deterrence impact, a bias need not arise if, for instance, people who tend to use different security services (for example, business firms and relatively wealthy households) than those who use handguns (for example, relatively poor households) for protection.

III. Empirical Analysis and Results

A. Weighted Least Squares Estimates

The first step in the analysis is to replicate a set of equations from LM with additional controls for private security services. Therefore, the WLS regressions reported in table 3 of LM[16] are replicated with the addition of either security establishments or security employment as measures of private security. The relevant results are reported in Tables 1 and 2. These regressions use county-level data and include both county and time dummies to control for fixed effects, although coefficients for these dummies are not reported. They also include all of the socioeconomic variables used by LM in the regressions reported in their table 3 (population density, various per capita income variables, population, and a large number of variables that control for the portion of the population in various age and race categories). Since the results are virtually identical to those in LM, these coefficients also are not reported. All regressions use WLS, where the weight is each county's population, as in LM.

Coefficients and *t*-statistics for the shall-issue-law-adopted dummy and the total security establishments per 100,000 population variable from WLS re-

[15] See the first-stage regression for the 2SLS model reported in Table A2 of the Appendix *infra*.

[16] Lott & Mustard, *supra* note 4, at 20–23.

TABLE 1

THE EFFECTS OF SHALL-ISSUE RIGHT-TO-CARRY FIREARMS LAWS AND PRIVATE SECURITY ON THE NATURAL LOG OF VIOLENT CRIME RATES PER 100,000 PERSONS, 1977–92

	DEPENDENT VARIABLES: LOG OF CRIME RATES/100,000 PERSONS				
EXOGENOUS VARIABLES	Total Violent	Murder	Rape	Robbery	Assault
Shall-issue law adopted dummy	−.05329**	−.07402**	−.05223**	−.02253+	−.07372**
	(−5.48)	(−4.68)	(−4.24)	(−1.68)	(−6.47)
Total security establishments per 100,000 persons	.00068	−.00666**	−.00397*	.00201	−.00091
	(.48)	(−2.66)	(−2.13)	(.90)	(−.54)
N	42,580	26,124	33,373	34,494	42,643
F-statistic	114.89	37.16	44.42	129.22	70.27
Adjusted R^2	.8938	.8034	.7998	.9187	.8360
Shall-issue law adopted dummy	−.05199**	−.07530**	−.05134**	−.02197+	−.07375**
	(−5.35)	(−4.76)	(−4.01)	(−1.64)	(−6.47)
Total security employment per 100,000 persons	−.00010**	−.00141	−.00014*	−.00002	−.00002
	(−3.91)	(−.51)	(−4.31)	(−.46)	(−.50)
N	42,580	26,124	33,373	34,494	42,643
F-statistic	114.94	37.15	44.44	129.22	70.27
Adjusted R^2	.8938	.8033	.7999	.9187	.8360

NOTE.—The *t*-statistics are in parentheses. A large number of additional variables are included in these regressions, but coefficients are not reported. These variables include the relevant arrest rate; population per square mile; real per capita income data on personal income, unemployment insurance, income maintenance, and retirement payments per person over 65; population; race, gender, and age data (percent of population) categorizing black males, black females, white males, white females, other (races other than black and white) males, and other females in age groups 10–19, 20–29, 30–39, 40–49, 50–64, and over 65, as in John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1 (1997); county fixed effects dummies; and time fixed effects dummies. All regressions use WLS, where the weight is each county's population.

+ Significant at the 10 percent level, two-tailed test.

* Significant at the 5 percent level, two-tailed test.

** Significant at the 1 percent level, two-tailed test.

TABLE 2

THE EFFECTS OF SHALL-ISSUE RIGHT-TO-CARRY FIREARMS LAWS AND PRIVATE SECURITY ON THE NATURAL LOG OF PROPERTY CRIME RATES PER 100,000 PERSONS, 1977–92

	DEPENDENT VARIABLES: LOG OF CRIME RATES/100,000 PERSONS			
EXOGENOUS VARIABLES	Total Property	Larceny	Burglary	Auto Theft
Shall-issue law adopted dummy	.01970**	.02792**	−.00587	.06806**
	(2.85)	(3.20)	(−.80)	(5.94)
Total security establishments per 100,000 persons	−.00093	−.00355**	−.00137	.00482**
	(−.92)	(−2.80)	(−1.28)	(2.89)
N	44,699	44,584	44,599	43,438
F-statistic	88.93	58.14	83.18	86.32
Adjusted R^2	.8612	.8016	.8531	.8604
Shall-issue law adopted dummy	.01865**	.02869**	−.00520	.06807**
	(2.72)	(2.92)	(−.71)	(5.97)
Total security employment per 100,000 persons	.00002	.00006**	−.00007**	−.00005^{+}
	(.80)	(2.60)	(−3.70)	(−1.69)
N	44,699	44,584	44,599	43,438
F-statistic	90.70	58.88	85.10	87.39
Adjusted R^2	.8637	.8039	.8561	.8620

NOTE.—The *t*-statistics are in parentheses. A large number of additional variables are included in these regressions, but coefficients are not reported. These variables are listed in the note to Table 1. All regressions use WLS, where the weight is each county's population.

$^{+}$ Significant at the 10 percent level, two-tailed test.

** Significant at the 1 percent level, two-tailed test.

gressions explaining logged violent crime rates per 100,000 population are reported in the upper part of Table 1. Estimates of the same models with total security employment per 100,000 population replacing the total security establishment variable are reported in the lower part of Table 1. Table 2 reports the same results from property crime regressions. First note that the coefficients on the shall-issue dummy variables are virtually identical to those in LM's table 3. Therefore, the hypothesis that the LM coefficients suffer from missing-variable bias owing to the lack of control for the private security industry is rejected for the WLS estimates.

Now consider the coefficients on the variables controlling for private security in Tables 1 and 2. Note first that both measures of private security suggest no general deterrence impact for robbery, assault, or total property crime, but some crimes do appear to be deterred by private security. Both measures of private security imply a deterrence impact for rape in Table 1. Similarly, the coefficient on security employment in Table 2 indicates a significant deterrence impact for burglary, while the coefficient for security establishments is also negative, but insignificant. It would be marginally significant at the 10 percent level, using a one-tailed test. Yet given the results in the larceny and auto theft regressions, a one-tailed test does not appear appropriate. Indeed, the remaining results are very inconsistent. The coefficients on total security establishments per capita in Table 1 suggest that private security has a significant deterrence impact for murder, but not total violent crime (which has a positive sign). At the same time, the coefficients for the total security employment variable imply a deterrent impact for total violent crime, but not for murder (although the sign is negative). The differences for property crimes in Table 2 are even more striking. The coefficients on private security establishments suggest that larceny is deterred, while auto theft is significantly increased. In sharp contrast, however, the coefficients on security employment imply that larceny increases because of security employment, while auto theft decreases.

There are various potential explanations for the differences that arise from the two security measures (and for the insignificant coefficients for robbery, assault, and total property crime). First, it must be noted that the security establishment data are all reported observations, while the employment data consist of a combination of reported and estimated observations. Perhaps the estimates do not accurately reflect the real data.[17] The security employment measure should produce more accurate indications of deterrence if the estimated observations are reasonable, because employment is a direct measure of resources used in security, while the number of firms is much less direct

[17] Census data indicate that security employment in nonsecurity firms has expanded at a slower rate than employment by security establishments. Perhaps contracting out is being substituted for directly employing security personnel, and if so, the results presented here could be biased.

given that firm sizes vary considerably. Moreover, licensing requirements and other barriers to entry could limit the number of firms. Existing firms, however, could still respond to increased demand by expanding employment, unless there are also barriers to such expansion, such as training requirements that limit the pool of employees. That is, security employment may be more responsive to crime than security establishments. This brings us to what could be the most important explanation for the various results for the security coefficients.[18] While the WLS estimates in Tables 1 and 2 assume that private security is exogenous, it may in fact be endogenous. If private security deters crimes but higher crime rates simultaneously increase the use of private security, then the insignificant private security coefficients in Tables 1 and 2 could reflect the offsetting effects of these two causal relationships, while the negative (positive) coefficients could reflect the dominance of the deterrence (demand increasing) relationship. Thus, all of the coefficients on security measures could suffer from simultaneity bias. Therefore, 2SLS models with measures of private security (and arrest rates) treated as endogenous are estimated.

B. Two-Stage Least Squares Estimates

A number of potential instrumental variables were obtained from both the LM data set and from *CBP*. Instruments drawn from the LM data for the arrest rate equation are measures of policing and lags of various crime rates.[19] In addition, LM data on the percent of the state population belonging to the National Rifle Association (NRA) and on the percent of the state voting Republican for 1983–86, 1987–90, and 1991–92 are used as instruments for private security. Membership in the NRA may reflect what Glaeser and Glendon[20] refer to as "a mistrust of public justice" that leads to gun ownership, but also perhaps to a general reliance on private alternatives. Republican vote could also reflect a relative mistrust of government. Neither of these

[18] The models may also be inappropriately specified. For example, Plassmann & Tideman, *supra* note 6, argue that the relationships should be specified as Poisson models. Models using security employment were therefore reestimated, specifying the left-hand variables as integers and the right-hand continuous variables as logs. Results support significant private security deterrence for rape and burglary. The coefficient for assault is also negative, but auto theft, murder, robbery, and violent crime all appear to significantly increase with increases in private security (larceny and total property crime also have positive signs). These results are not reported, in part because Plassmann & Tideman, *supra* note 6, point out that if the data are overdispersed (the variance is greater than the mean), then assumptions of the classical Poisson model are violated as well. Thus, they wrote programs to use the Gibbs sampler. Furthermore, these models do not deal with simultaneity bias.

[19] Such measures are commonly used in the economics of crime literature. The actual variables are changes in logs of property crime arrest rates, larceny arrest rates, and violent crime arrest rates; lagged property crime, larceny crime, and violent crime arrest rates; and state-level per capita, per violent, and per property crime measures of police employment and payroll for police with and without arrest power.

[20] Glaeser & Glendon, *supra* note 5, at 462.

variables are likely to be determinants of crime rates, so they are potentially attractive instruments. In addition, we hypothesize that several types of industries are likely to demand private security services, while the presence of other types of industries is likely to be associated with less use of such services. Of course, the presence of some types of industries may also "cause" crime in a jurisdiction, for example, by attracting criminals. Therefore, data drawn from *CBP* on numbers of establishments and numbers of employees in 23 SIC categories to serve as instruments are all lagged for 1 year.

As with the security employment data, total employees are reported by *CBP* only as long as the report will not disclose operations for individual establishments. However, *CBP* also reports the numbers of establishments in all counties in various size categories defined by the number of employees in the establishment. So we estimate the number of employees in a county where the number is not reported by multiplying the number of establishments in each size category by the median number of employees in the category. Since the establishment data are consistently reported, but many of the employment observations must be estimated, both measures of industry size are used in the first-stage regressions. The industry categories are agriculture, mining, construction, manufacturing, transportation (divided into bus terminal, airport, warehouse, and other transportation), wholesale, retail (divided into department store, grocery, and other retail), financial (divided into banking and other financial), and services (divided into hotel/tourist, parking, commercial sports, amusement park, hospital, college/university, library, museum, and other service). Summary statistics are reported in the Appendix. Among these industries, agriculture probably does not use private security, but most of the others employ security personnel. The mix of internal security employment and contractual relationships with specialized security firms is not known, however, so the individual coefficients cannot be predicted. Table A2 of the Appendix reports a sample of first-stage private security regressions (note that the first-stage regressions for the various crime rate models differ because of differences in N) in order to illustrate the potential inferences that might be drawn from a detailed examination of such models. Any conclusions are tentative of course, since there clearly is considerable multicollinearity in the data. Indeed, the correlation between security establishments and security employment in many industries is .90 or greater. For 2SLS, collinearity among instruments does not matter, of course, and if including the additional instruments improves the model fit, it is better to include them.[21] However, collinearity does mean that the individual coefficients should not be taken very seriously, as both sign switching and large

[21] For the security employment (establishments) regression reported in Table A2 of the Appendix, the F-tests for joint significance of the LM instruments (variables listed in note 18 and the Appendix) and *CBP* instruments were $F(22, 29{,}245) = 4.83$ (4.93) and $F(46, 29{,}245) = 15.41$ (17.09), respectively, all significant at the .01 level.

standard errors are anticipated. Nevertheless, a substantial majority of the instruments are at least marginally significant in at least one of the two regressions. It appears that many of the industries are employers of security services and that states with relatively large recent Republican votes are more likely to employ private security.

Tables 3 and 4 report the second-stage regression coefficients on the shall-issue law and private security (either security establishment or security employment), where the private security (and unreported arrest rate) variables are now predicted values.

The security establishment coefficients suggest potential deterrence relationships for murder and rape (coefficients are significant at the 10 percent level of confidence), both of which had significantly negative coefficients in the WLS models (Table 1). Larceny also had a significantly negative security establishment coefficient in the WLS equation (Table 2), but it is insignificant in Table 4, although still negative. Furthermore, the insignificant and positive security establishment coefficient in the WLS robbery regression (Table 1) turns negative in the 2SLS model (Table 3), although it remains insignificant, and the significantly positive coefficient in the WLS auto theft model (Table 2) turns insignificant in Table 4 (although it remains positive). However, the coefficient on security establishments is marginally positive in the 2SLS total violent crime model (Table 3) compared with the insignificant but positive relationship implied by the WLS regression (Table 1), and the insignificant but negative coefficient in the WLS assault regression (Table 1) becomes positive and marginally significant in the 2SLS model (Table 3).

In the models using security employment as the measure, significant deterrence relationships that appeared for rape and auto theft in the WLS regressions are supported in the 2SLS models. Furthermore, the 2SLS robbery results suggest a significant deterrence relationship that did not appear in the WLS results. However, what appeared to be significant deterrence impacts for total violent crime and burglary in the WLS models disappear in the 2SLS models, where the coefficients are insignificant. The security employment coefficient for murder is also negative, but insignificant in both WLS and 2SLS models, and the coefficients in the assault and total property crime regressions also are insignificant in both the single-equation and simultaneous-equation models. Finally, the positive and significant coefficient in the WLS larceny regression also appears in the 2SLS results. Thus, although the simultaneous equation models do eliminate some of the inconsistencies implied by the WLS results, some remain. This may reflect the effort to consider both the potential bias in LM coefficients and the private security deterrence hypothesis in the same model, thus constraining the model to a replication of LM with their data. An alternative data set, such as census data that include security employment by security firms and by nonsecurity firms, might yield clearer deterrence implications. Nonetheless, these results do suggest that some general deterrence is probably produced by private

TABLE 3

THE EFFECTS OF SHALL-ISSUE RIGHT-TO-CARRY FIREARMS LAWS AND PRIVATE SECURITY ON THE NATURAL LOG OF VIOLENT CRIME RATES PER 100,000 PERSONS, 1982–92, IN TWO-STAGE LEAST SQUARES MODELS TREATING PRIVATE SECURITY MEASURES AND ARREST RATES AS ENDOGENOUS

	SECOND-STAGE DEPENDENT VARIABLES: LOG OF CRIME RATES/100,000 PERSONS				
VARIABLES	Total Violent	Murder	Rape	Robbery	Assault
Shall-issue law adopted dummy	−.07108**	−.08010**	−.00034	−.00912	−.07628**
	(−3.26)	(−3.33)	(−.01)	(−.44)	(−3.29)
Total security establishments per 100,000 persons	.03940+	−.04204+	−.04549+	−.02030	.03888+
	(1.81)	(−1.86)	(−1.92)	(−1.00)	(1.68)
N	28,576	17,034	22,718	22,527	28,132
Shall-issue law adopted dummy	−.06924**	−.08103**	.00272	−.00418	−.07723**
	(−3.16)	(−3.33)	(.11)	(−.20)	(−3.30)
Total security employment per 100,000 persons	.00029	−.00050	−.00102*	−.00087*	.00062
	(.70)	(−1.27)	(−2.27)	(−2.28)	(1.39)
N	28,576	17,034	22,718	22,527	28,132

NOTE.—The *t*-statistics are in parentheses. A large number of additional variables are included in these regressions, but coefficients are not reported. Many of these variables are listed in the note to Table 1, and others are listed in the Appendix. All regressions use WLS, where the weight is each county's population.

+ Significant at the 10 percent level, two-tailed test.

* Significant at the 5 percent level, two-tailed test.

** Significant at the 1 percent level, two-tailed test.

TABLE 4

THE EFFECTS OF SHALL-ISSUE RIGHT-TO-CARRY FIREARMS LAWS AND PRIVATE SECURITY ON THE NATURAL LOG OF PROPERTY CRIME RATES PER 100,000 PERSONS, 1982–92, IN TWO-STAGE LEAST SQUARES MODELS TREATING PRIVATE SECURITY MEASURES AND ARREST RATES AS ENDOGENOUS

	SECOND-STAGE DEPENDENT VARIABLES: LOG OF CRIME RATES/100,000 PERSONS			
VARIABLES	Total Property	Larceny	Burglary	Auto Theft
Shall-issue law adopted dummy	−.01774	−.01146	−.00345*	.01890
	(−1.16)	(−.64)	(−2.30)	(1.20)
Total security establishments per 100,000 persons	.00409	−.01972	.02261	.01230
	(.27)	(−1.10)	(1.52)	(.78)
N	29,366	29,295	29,309	28,523
Shall-issue law adopted dummy	−.01740	−.03227^{+}	−.03377*	.03134^{+}
	(−1.14)	(−1.75)	(−2.25)	(1.94)
Total security employment per 100,000 persons	.00001	.00240**	.00021	−.00144**
	(.04)	(6.92)	(.74)	(−4.75)
N	29,366	29,295	29,309	28,523

NOTE.—The *t*-statistics are in parentheses. A large number of additional variables are included in these regressions, but coefficients are not reported. Many of these variables are listed in the note to Table 1, and others are listed in the Appendix. All regressions use WLS, where the weight is each county's population.

$^{+}$ Significant at the 10 percent level, two-tailed test.

* Significant at the 5 percent level, two-tailed test.

** Significant at the 1 percent level, two-tailed test.

security firms (for example, in cases of rape) and therefore that the issue deserves more attention in the literature on crime control.

Some of the shall-issue dummy coefficients change when the private security measures are treated as endogenous. For example, the coefficients of the shall-issue dummy in the models for rape and robbery become much smaller and insignificant in the 2SLS models. However, while LM estimate 2SLS models by treating the arrest rate and shall-issue dummy as endogenous, we do not treat the shall-issue dummy as endogenous.[22] Therefore, our 2SLS estimates are not directly comparable with the 2SLS estimates of LM.

IV. Conclusions

Our results provide at least tentative insights regarding the potential general deterrence impact of private security. We also find little evidence that the LM findings regarding the deterrence impacts of right-to-carry laws are biased by a lack of control for at least some other private investments in protection or detection (this analysis does not address any of the other alleged problems with LM, of course; it simply clarifies the potential missing-variable issue addressed here).

If private security has a general deterrence impact, it appears to be strongest for rape, as the negative relationship between measures of private security and rape rates tends to be robust in WLS regressions and in 2SLS models that treat private security and arrest rates as endogenous. Apparent deterrence relationships for other types of crime are simply not robust enough across different measures of security and/or model specifications to warrant strong support of the general deterrence hypothesis. Even so, deterrence relationships could hold for robbery, murder, and/or auto theft. This is not a particularly surprising result, however, for at least two reasons.

First, as noted above, criminals deterred from victimizing one individual or location because of visible private security may simply find another victim. Second, firms selling such services have strong incentives to develop products and contractual arrangements that allow them to exclude nonpayers. Therefore, while investments in private security produce large deterrence benefits, many are specific to the individual or location being protected. Such investments may produce some spillover benefits for others in the vicinity if they raise the cost of searching for crime targets, as suggested above, but apparently this has not occurred to a sufficient degree to be detected for some types of crime.

The apparent deterrence impact on rape also makes sense in this context. A potential rape victim can take advantage of private security paid for by others by choosing to shop and socialize where there is a security presence, thereby reducing opportunities for victimization and raising the overall cost

[22] See *supra* note 7.

of committing rapes. Potential victims of some other types of crime are of course also able to make choices similar to the potential rape victim. This may be the case for potential robbery and car theft (and perhaps some potential murder) victims who can choose to shop at malls with security patrols in parking lots, for instance, rather than in downtown areas that do not have such patrols.

In contrast, a potential burglary victim does not have the same type of mobility. There are growing numbers of privately protected residential communities,[23] but they are still limited relative to the population. In fact, the positive (although insignificant) relationship between private security and burglary in 2SLS regressions suggests that criminals may actually respond to private security by shifting into burglary (potential burglary victims can purchase alarm services, of course, but these are generally accompanied by notices that alarms are present, so the cost of burglars finding an unprotected victim is not increased very much). The relationships for larceny are not as clear, and this is the type of crime where results are the least consistent across models and security measures. Although there are also opportunities for some potential victims to choose relatively secure environments (for example, by shopping where security is present and thereby reducing the opportunity for purse snatchers and pickpockets), many potential victims of larceny may not be able to take advantage of security services paid for by others.

Clearly, additional research is required before strong conclusions about general deterrence from private security can be drawn. Nevertheless, these results suggest that concealed handguns may not be the only private sector option for reducing crime rates.

APPENDIX

TABLE A1

First-Stage Regressions for Two-Stage Least Squares Models Treating Private Security and Arrest Rates as Endogenous Sample Sizes, Means, and Standard Deviations, 1977–92 Data Period

Instruments for Private Security First-Stage Regressions	Sample Mean	Standard Deviation
National Rifle Association membership per 100,000 persons[a] ($N = 49{,}983$)	1,095.98	512.54
Percentage of 1984 state population voting Republican for 1983–86 ($N = 49{,}983$)[a,b]	15.18	26.41
Percentage of 1988 state population voting Republican for 1987–90 ($N = 49{,}983$)[a,b]	13.76	23.94
Percentage of 1992 state population voting Republican for 1990–92 ($N = 49{,}983$)[a,b]	4.96	13.21

[23] Benson, *supra* note 1, at 84–85 & 90–93.

One-year lag per 100,000 persons (N = 46,810), by type of employment:[c]		
Agriculture	152.00	237.58
Mining	674.73	2,558.97
Construction	1,221.21	1,232.39
Manufacturing	4,335.64	5,592.10
Bus terminal	.60	6.55
Airport	11.58	89.44
Warehouse	27.80	106.28
Other transportation	1,209.99	1,162.52
Wholesale	1,448.02	1,103.43
Department Store	308.44	416.80
Grocery	920.52	387.15
Other retail	3,917.96	2,273.43
Bank	553.21	333.05
Other financial	652.91	938.83
Hotel/tourist	437.67	1449.87
Parking	2.41	15.91
Commercial sports	11.78	100.37
Amusement park	7.23	96.40
Hospital	752.07	1,148.84
College/university	172.25	632.78
Library	3.57	14.78
Museum	8.11	136.05
Other service	3,486.76	2,831.74
One-year lag per 100,000 persons (N = 46,810), by type of establishment:[c]		
Agriculture	28.80	24.98
Mining	29.42	75.46
Construction	199.04	111.61
Manufacturing	84.81	84.43
Bus terminal	.60	6.55
Airport	.97	2.83
Warehouse	2.93	6.69
Other transportation	101.11	95.90
Wholesale	163.02	100.52
Department store	2.87	3.57
Grocery	75.97	35.39
Other retail	548.10	215.29
Bank	30.95	21.25
Other financial	123.45	73.60
Hotel/tourist	34.25	60.43
Parking	.50	21.25
Commercial sports	.76	3.04
Amusement park	.25	1.25
Hospital	3.57	6.14
College/university	.74	2.17
Library	.76	3.39
Museum	1.02	4.25
Other service	505.39	239.30

[a] Source: Data provided by John R. Lott, Jr. See John R, Lott, Jr., More Guns, Less Crime: Understanding Crime and Gun-Control Laws (1998), or John R. Lott, Jr., & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J. Legal Stud. 1 (1997), for details regarding sources.

[b] These variables are interacted with a time dummy.

[c] Source: Bureau of Census, County Business Patterns (Washington, D.C.: Bureau of Census, various years).

TABLE A2

First-Stage Regressions from Two-Stage Least Squares Models of the Natural log of Property Crime Rates per 100,000 Persons, 1982–92

Instrumental Variables (plus Shall-Issue Law)	First-Stage Dependent Variables	
	Total Security Establishments	Total Security Employment
Shall-issue law adopted dummy	.00640 (.17)	5.03527* (2.48)
Lags by type of employment per 100,000 persons:		
Agriculture	−.00038** (−3.71)	−.01474** (−2.62)
Mining	−.000008 (−.42)	.00020 (.19)
Construction	−.00004* (−2.10)	−.00114 (−.70)
Manufacturing	.00001** (2.89)	−.00079** (−3.91)
Bus terminal	−.00035 (−.17)	.38503** (3.56)
Airport	−.00003 (−.19)	−.00545 (−.62)
Warehouse	−.00005 (−.28)	.04128** (3.88)
Other transportation	−.00002 (−1.04)	.00742** (5.52)
Wholesale	−.00004 (−1.35)	.01325 (.76)
Department store	−.00009 (−1.23)	−.01750** (−4.51)
Grocery	−.00005 (−.77)	−.00973** (−2.82)
Other retail	−.00003 (−.12)	−.00028 (−.22)
Bank	.00025** (4.72)	.01760** (6.08)
Other financial	.00016** (5.85)	.02175** (14.76)
Hotel/tourist	−.00012** (−3.70)	.00072 (.39)
Parking	−.00130 (−1.52)	−.12603** (−2.71)
Commercial sports	.00025 (1.23)	.39656** (3.61)
Amusement park	.00010 (.61)	−.00958 (−1.03)
Hospital	.00013** (6.54)	−.00147 (−1.35)
College/university	−.00002 (−.49)	.01058** (4.30)
Library	−.00133 (−1.34)	.20847** (3.85)
Museum	−.00059** (−4.14)	−.00896 (−1.15)
Other service	.00002 (1.53)	.00245** (3.49)
Lags by type of establishment per 100,000 persons:		
Agriculture	−.00073 (−.47)	−.30635** (−3.64)
Mining	.00090 (.69)	.09660 (.14)
Construction	.00119** (3.24)	.00756 (.38)
Manufacturing	−.00032 (−1.21)	.03530* (2.48)
Bus terminal	−.02539 (−.86)	−3.17218* (−2.23)
Airport	−.00128 (−1.50)	−.42663 (−.92)
Warehouse	.00969^{+} (1.73)	−.89051** (−2.92)
Other transportation	.00027 (.80)	−.03489^{+} (−1.92)
Wholesale	.00412** (5.84)	.15599** (4.07)
Department store	.00287 (.32)	.52333 (1.09)
Grocery	−.00141 (−1.23)	.06558 (1.06)
Other retail	.00072* (2.08)	.01518 (.80)
Bank	−.00698** (−4.69)	−.09274 (−1.15)
Other financial	.00076 (1.23)	−.17230** (−5.14)
Hotel/tourist	−.00165 (−1.25)	−.01432 (−.20)
Parking	.01121* (1.99)	.72930* (2.39)
Commercial sports	.01982* (2.24)	−1.02222* (−2.12)
Amusement park	−.01026 (−.67)	−.74605 (−.90)
Hospital	−.00241 (−.33)	.37841 (.96)
College/university	.00867 (.50)	.21562 (.23)
Library	−.00718 (−.57)	−1.96661** (−2.98)
Museum	.00393 (.47)	.24974 (.55)

Other service	.00339** (11.75)	.05961** (3.80)
National Rifle Association membership per 100,000 persons	−.00010 (−.83)	.00656 (.22)
Percentage of 1984 state population voting Republican for 1983–86	−.00264 (−.64)	.23793 (1.07)
Percentage of 1988 state population voting Republican for 1987–90	−.00911* (−2.13)	.68881** (2.96)
Percentage of 1992 state population voting Republican for 1990–92	.01191* (2.16)	1.17818** (3.94)
F-statistic	68.25	91.79
Adjusted R^2	.8790	.9175

NOTE.—The *t*-statistics are in parentheses. A large number of additional variables (those listed in the note to Table 1 and in note 18) are included in these regressions, but coefficients are not reported. All regressions use WLS, where the weight is each county's population. $N = 29{,}366$.

+ Significant at the 10 percent level, two-tailed test.

* Significant at the 5 percent level, two-tailed test.

** Significant at the 1 percent level, two-tailed test.

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