CRIME, DETERRENCE, AND RIGHT-TO-CARRY CONCEALED HANDGUNS

*JOHN R. LOTT, JR., and DAVID B. MUSTARD**

Abstract

Using cross-sectional time-series data for U.S. counties from 1977 to 1992, we find that allowing citizens to carry concealed weapons deters violent crimes, without increasing accidental deaths. If those states without right-to-carry concealed gun provisions had adopted them in 1992, county- and state-level data indicate that approximately 1,500 murders would have been avoided yearly. Similarly, we predict that rapes would have declined by over 4,000, robbery by over 11,000, and aggravated assaults by over 60,000. We also find criminals substituting into property crimes involving stealth, where the probability of contact between the criminal and the victim is minimal. Further, higher arrest and conviction rates consistently reduce crime. The estimated annual gain from all remaining states adopting these laws was at least $5.74 billion in 1992. The annual social benefit from an additional concealed handgun permit is as high as $5,000.

I. Introduction

Will allowing concealed handguns make it likely that otherwise law-abiding citizens will harm each other? Or will the threat of citizens carrying weapons primarily deter criminals? To some, the logic is fairly straightforward. Philip Cook argues that ''[i]f you introduce a gun into a violent encounter, it increases the chance that someone will die.''[1] A large number of murders may arise from unintentional fits of rage that are quickly regretted, and simply keeping guns out of people's reach would prevent deaths.[2] Us-

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[1] Editorial, Cincinnati Enquirer, January 23, 1996, at A8.

[2] See P. J. Cook, The Role of Firearms in Violent Crime, in Criminal Violence 236–91 (M. E. Wolfgang & N. A. Werner eds. 1982); and Franklin Zimring, The Medium Is the

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ing the National Crime Victimization Survey, Cook further states that each year there are ''only'' 80,000–82,000 defensive uses of guns during assaults, robberies, and household burglaries.[3] By contrast, other surveys imply that private firearms may be used in self-defense up to two and a half million times each year, with 400,000 of these defenders believing that using the gun ''almost certainly'' saved a life.[4] With total firearm deaths from homicides and accidents equaling 19,187 in 1991,[5] the Kleck and Gertz numbers, even if wrong by a very large factor, suggest that defensive gun use on net saved lives.

While cases like the 1992 incident where a Japanese student was shot on his way to a Halloween party in Louisiana make international headlines,[6] they are rare. In another highly publicized case, a Dallas resident recently became the only Texas resident so far charged with using a permitted concealed weapon in a fatal shooting.[7] Yet, in neither case was the shooting

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Message: Firearm Caliber as a Determinant of Death from Assault, 1 J. Legal Stud. 97 (1972), for these arguments.

[3] P. J. Cook, The Technology of Personal Violence, 14 Crime and Justice: Annual Review of Research 57, 56 n.4 (1991). It is very easy to find people arguing that concealed handguns will have no deterrence effect. H. Richard Uviller, Virtual Justice 95 (1996), writes that ''[m]ore handguns lawfully in civilian hands will not reduce deaths from bullets and cannot stop the predators from enforcing their criminal demands and expressing their lethal purposes with the most effective tool they can get their hands on.''

[4] Gary Kleck & Marc Gertz, Armed Resistance to Crime: The Prevalence and Nature of Self-Defense with a Gun, 86 J. Crim. L. & Criminology 150, 153, 180, 180–82 (Fall 1995). Kleck and Gertz's survey of 10 other nationwide polls implies a range of 764,036–3,609,682 defensive uses of guns per year. Recent evidence confirms other numbers from Kleck and Gertz's study. For example, Annest *et al.* estimate that 99,025 people sought medical treatment for nonfatal firearm woundings. When one considers that many criminals will not seek treatment for wounds and that not all wounds require medical treatment, Kleck and Gertz's estimates of 200,000 woundings seems somewhat plausible, though even Kleck and Gertz believe that this is undoubtedly too high given the very high level of marksmanship that this implies by those shooting the guns. Yet, even if the true number of times that criminals are wounded is much smaller, it still implies that criminals face a very real expected cost from attacking armed civilians. See J. L. Annest, J. A. Mercy, D. R. Gibson, & G. W. Ryan, National Estimates of Nonfatal Firearm-Related Injuries: Beyond the Tip of the Iceberg, J. A.M.A. 1749–54 (June 14, 1995); and also Lawrence Southwick, Jr., Self-Defense with Guns: The Consequences (working paper, SUNY Buffalo 1996), for a discussion on the defensive uses of guns.

[5] U.S. Bureau of the Census, Statistical Abstract of the United States (115th ed. 1995).

[6] Japan Economic Newswire, U.S. Jury Clears Man Who Shot Japanese Student, Kyodo News Service, May 24, 1993; and Lori Sharn, Violence Shoots Holes in USA's Tourist Image, USA TODAY, September 9, 1993, at 2A.

[7] Dawn Lewis of Texans against Gun Violence provided a typical reaction from gun control advocates to the grand jury decision not to charge Gordon Hale. She said, ''We are appalled. This law is doing what we expected, causing senseless death.'' Mark Potok, Texan says the concealed gun law saved his life: ''I did what I thought I had to do,'' USA TODAY, March 22, 1996, at 3A. For a more recent evaluation of the Texas experience, see Few Problems Reported after Allowing Concealed Handguns, Officers Say, Fort Worth Star-Telegram, July 16, 1996. By the end of June 1996, more than 82,000 permits had been issued in Texas.

found to be unlawful.[8] The rarity of these incidents is reflected in Florida statistics: 221,443 licenses were issued between October 1, 1987, and April 30, 1994, but only 18 crimes involving firearms were committed by those with licenses.[9] While a statewide breakdown on the nature of those crimes is not available, Dade County records indicate that four crimes involving a permitted handgun took place there between September 1987 and August 1992, and none of those cases resulted in injury.[10]

The potential defensive nature of guns is indicated by the different rates of so-called hot burglaries, where residents are at home when the criminals strike.[11] Almost half the burglaries in Canada and Britain, which have tough gun control laws, are ''hot burglaries.'' By contrast, the United States, with laxer restrictions, has a ''hot burglary'' rate of only 13 percent. Consistent with this, surveys of convicted felons in America reveal that they are much more worried about armed victims than they are about running into the police. This fear of potentially armed victims causes American burglars to spend more time than their foreign counterparts ''casing'' a house to ensure that nobody is home. Felons frequently comment in these interviews that they avoid late-night burglaries because ''that's the way to get shot.''[12]

[8] In fact, police accidentally killed 330 innocent individuals in 1993, compared to the mere 30 innocent people accidentally killed by private citizens who mistakenly believed the victim was an intruder. John R. Lott, Jr., Now That the Brady Law Is Law, You Are Not Any Safer than Before, Philadelphia Inquirer, February 1, 1994, at A9.

[9] Clayton E. Cramer & David B. Kopel, ''Shall Issue'': The New Wave of Concealed Handgun Permit Laws, 62 Tenn. L. Rev. 679, 691 (Spring 1995). An expanded version of this paper dated 1994 is available from the Independence Institute, Golden, Colorado. Similarly, Multnomah County, Oregon, issued 11,140 permits over the period January 1990 to October 1994 and experienced five permit holders being involved in shootings, three of which were considered justified by grand juries. Out of the other two cases, one was fired in a domestic dispute and the other was an accident that occurred while an assault rifle was being unloaded. Bob Barnhart, Concealed Handgun Licensing in Multnomah County (photocopy, Intelligence/Concealed Handgun Unit, Multnomah County, October 1994).

[10] Cramer & Kopel, *supra* note 9, at 691–92.

[11] For example, David B. Kopel, The Samurai, the Mountie, and the Cowboy 155 (1992); and Lott, *supra* note 8.

[12] Wright and Rossi (p. 151) interviewed felony prisoners in 10 state correctional systems and found that 56 percent said that criminals would not attack a potential victim that was known to be armed. They also found evidence that criminals in those states with the highest levels of civilian gun ownership worried the most about armed victims. James D. Wright & Peter Rossi, Armed and Considered Dangerous: A Survey of Felons and Their Firearms (1986).

Examples of stories where people successfully defend themselves from burglaries with guns are quite common. For example, see Burglar Puts 92-Year-Old in the Gun Closet and Is Shot, New York Times, September 7, 1995, at A16. George F. Will, Are We ''a Nation of Cowards''? Newsweek, November 15, 1993, discusses more generally the benefits produced from an armed citizenry.

In his paper on airplane hijacking, William M. Landes, An Economic Study of U.S. Aircraft Hijacking, 1961–1976, 21 J. Law & Econ. 1 (April 1978), references a quote by Archie

The case for concealed handgun use is similar. The use of concealed handguns by some law-abiding citizens may create a positive externality for others. By the very nature of these guns being concealed, criminals are unable to tell whether the victim is armed before they strike, thus raising criminals' expected costs for committing many types of crimes.

Stories of individuals using guns to defend themselves has helped motivate 31 states to adopt laws requiring authorities to issue, without discretion, concealed-weapons permits to qualified applicants.[13] This constitutes a dramatic increase from the nine states that allowed concealed weapons in 1986.[14] While many studies examine the effects of gun control,[15] and a smaller number of papers specifically address the right-to-carry concealed firearms,[16] these papers involve little more than either time-series or cross-sectional evidence comparing mean crime rates, and none controls for variables that normally concern economists (for example, the probability of arrest and conviction and the length of prison sentences or even variables like personal income).[17] These papers fail to recognize that, since it is frequently only the largest population counties that are very restrictive when local authorities have been given discretion in granting concealed handgun permits, ''shall issue'' concealed handgun permit laws, which require permit requests be granted unless the individual has a criminal record or a history of significant mental illness,[18] will not alter the number of permits being issued in all counties.

Bunker from the television show ''All in the Family'' that is quite relevant to the current discussion. Landes quotes Archie Bunker as saying ''Well, I could stop hi-jacking tomorrow . . . if everyone was allowed to carry guns them hi-jackers wouldn't have no superiority. All you gotta do is arm all the passengers, then no hi-jacker would risk pullin' a rod.''

[13] These states were Alabama, Alaska, Arizona, Arkansas, Connecticut, Florida, Georgia, Idaho, Indiana, Kentucky, Louisiana, Maine, Mississippi, Montana, Nevada, New Hampshire, North Carolina, North Dakota, Oklahoma, Oregon, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, and Wyoming.

[14] These states were Alabama, Connecticut, Indiana, Maine, New Hampshire, North Dakota, South Dakota, Vermont, and Washington. Fourteen other states provided local discretion on whether to issue permits: California, Colorado, Delaware, Hawaii, Iowa, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, New Jersey, New York, Rhode Island, and South Carolina.

[15] See Gary Kleck, Guns and Violence: An Interpretive Review of the Field, 1 Soc. Pathology 12–47 (January 1995), for a survey.

[16] For example, P. J. Cook, Stephanie Molliconi, & Thomas B. Cole, Regulating Gun Markets, 86 J. Crim. L. & Criminology, 59–92 (Fall 1995); Cramer & Kopel, *supra* note 9; David McDowall, Colin Loftin, & Brian Wiersema, Easing Concealed Firearm Laws: Effects on Homicide in Three States, 86 J. Crim. L. & Criminology 193–206 (Fall 1995); and Gary Kleck & E. Britt Patterson, The Impact of Gun Control and Gun Ownership Levels on Violence Rates, 9 J. Quantitative Criminology 249–87 (1993).

[17] All 22 gun control papers studied by Kleck, *supra* note 15, use either cross-sectional state or city data or use time-series data for the entire United States or a particular city.

[18] Cramer & Kopel, *supra* note 9, at 680–707.

Other papers suffer from additional weaknesses. The paper by McDowall *et al.*,[19] which evaluates right-to-carry provisions, was widely cited in the popular press. Yet, their study suffers from many major methodological flaws: for instance, without explanation, they pick only three cities in Florida and one city each in Mississippi and Oregon (despite the provisions involving statewide laws), and they use neither the same sample period nor the same method of picking geographical areas for each of those cities.[20]

Our paper hopes to overcome these problems by using annual cross-sectional time-series county-level crime data for the entire United States from 1977 to 1992 to investigate the effect of ''shall issue'' right-to-carry concealed handgun laws. It is also the first paper to study the questions of deterrence using these data. While many recent studies employ proxies for deterrence—such as police expenditures or general levels of imprisonment—we are able to use arrest rates by type of crime and for a subset of our data also conviction rates and sentence lengths by type of crime.[21] We also attempt to analyze a question noted but not empirically addressed in this literature: the concern over causality between increases in handgun usage and crime rates. Is it higher crime that leads to increased handgun ownership, or the reverse? The issue is more complicated than simply whether carrying concealed firearms reduces murders because there are questions over whether criminals might substitute between different types of crimes as well as the extent to which accidental handgun deaths might increase.

II. Problems Testing the Effect of ''Shall Issue'' Concealed Handgun Provisions on Crime

Following Becker (1968), many economists have found evidence broadly consistent with the deterrent effect of punishment.[22] The notion is that the

[19] McDowall *et al.*, *supra* note 16.

[20] Equally damaging, the authors appear to concede in a discussion that follows their piece that their results are highly sensitive to how they define the crimes that they study. Even with their strange sample selection techniques, total murders appear to fall after the passage of concealed weapon laws. Because the authors only examine murders committed with guns, there is no attempt to control for any substitution effects that may occur between different methods of murder. For an excellent discussion of the McDowall *et al.* paper, see Daniel D. Polsby, Firearms Costs, Firearms Benefits and the Limits of Knowledge, 86 J. Crim. L. & Criminology 207–20 (Fall 1995).

[21] Recent attempts to relate the crime rate to the prison population concern us (see, for example, Levitt). Besides difficulties in relating the total prison population with any particular type of crime, we are also troubled by the ability to compare a stock (the prison population) with a flow (the crime rate). Steven Levitt, The Effect of Prison Population Size on Crime Rates: Evidence from Prison Overcrowding Litigation, 144 Q. J. Econ. (1996).

[22] Gary S. Becker, Crime and Punishment: An Economic Approach, 76 J. Pol. Econ. 169-217 (March/April 1968). For example, Isaac Ehrlich, Participation in Illegitimate Activities: A Theoretical and Empirical Investigation, 81 J. Pol. Econ. 521–65 (1973); Michael K.

expected penalty affects the prospective criminal's desire to commit a crime. This penalty consists of the probabilities of arrest and conviction and the length of the prison sentence. It is reasonable to disentangle the probability of arrest from the probability of conviction since accused individuals appear to suffer large reputational penalties simply from being arrested.[23] Likewise, conviction also imposes many different penalties (for example, lost licenses, lost voting rights, further reductions in earnings, and so on) even if the criminal is never sentenced to prison.[24]

While this discussion is well understood, the net effect of ''shall issue'' right-to-carry concealed handguns is ambiguous and remains to be tested when other factors influencing the returns to crime are controlled for. The first difficulty involves the availability of detailed county-level data on a variety of crimes over 3,054 counties during the period from 1977 to 1992. Unfortunately, for the time period we study, the Federal Bureau of Investigation's (FBI) Uniform Crime Report includes only arrest rate data rather than conviction rates or prison sentences. While we make use of the arrest rate information, we will also use county-level dummies, which admittedly constitute a rather imperfect way to control for cross-county differences such as differences in expected penalties. Fortunately, however, alternative variables are available to help us proxy for changes in legal regimes that affect the crime rate. One such method is to use another crime category as an exogenous variable that is correlated with the crimes that we are studying but at the same time is unrelated to the changes in right-to-carry firearm laws. Finally, after telephoning law enforcement officials in all 50 states, we were able to collect time-series county-level conviction rates and mean prison sentence lengths for three states (Arizona, Oregon, and Washington).

The FBI crime reports include seven categories of crime: murder, rape, aggravated assault, robbery, auto theft, burglary, and larceny.[25] Two addi-

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Block & John Heineke, A Labor Theoretical Analysis of Criminal Choice, 65 Am. Econ. Rev. 314–25 (June 1975); Landes, *supra* note 12; John R. Lott, Jr., Juvenile Delinquency and Education: A Comparison of Public and Private Provision, 7 Int'l Rev. L. & Econ. 163–75 (December 1987); James Andreoni, Criminal Deterrence in the Reduced Form: A New Perspective on Ehrlich's Seminal Study, 33 Econ. Inquiry 476–83 (July 1995); Morgan O. Reynolds, Crime and Punishment in America (Policy Report 193, National Center for Policy Analysis, June 1995); and Levitt, *supra* note 21.

[23] John R. Lott, Jr., Do We Punish High Income Criminals Too Heavily? 30 Econ. Inquiry 583–608 (October 1992).

[24] John R. Lott, Jr., The Effect of Conviction on the Legitimate Income of Criminals, 34 Econ. Letters 381–85 (December 1990); John R. Lott, Jr., An Attempt at Measuring the Total Monetary Penalty from Drug Convictions: The Importance of an Individual's Reputation, 21 J. Legal Stud. 159–87 (January 1992); and Lott, *supra* note 23.

[25] Arson was excluded because of a large number of inconsistencies in the data and the small number of counties reporting this measure. Murder is defined as murder and nonnegligent manslaughter.

tional summary categories were included: violent crimes (including murder, rape, aggravated assault, and robbery) and property crimes (including auto theft, burglary, and larceny). Despite being widely reported measures in the press, these broader categories are somewhat problematic in that all crimes are given the same weight (for example, one murder equals one aggravated assault). Even the narrower categories are somewhat broad for our purposes. For example, robbery includes not only street robberies, which seem the most likely to be affected by ''shall issue'' laws, but also bank robberies, where, because of the presence of armed guards, the additional return to having armed citizens would appear to be small.[26] Likewise, larceny involves crimes of ''stealth,'' but these range from pickpockets, where ''shall issue'' laws could be important, to coin machine theft.[27]

This aggregation of crime categories makes it difficult to separate out which crimes might be deterred from increased handgun ownership and which crimes might be increased as a result of a substitution effect. Generally, we expect that the crimes most likely to be deterred by concealed handgun laws are those involving direct contact between the victim and the criminal, especially those occurring in a place where victims otherwise would not be allowed to carry firearms. For example, aggravated assault, murder, robbery, and rape seem most likely to fit both conditions, though obviously some of all these crimes can occur in places like residences where the victims could already possess firearms to protect themselves.

By contrast, crimes like auto theft seem unlikely to be deterred by gun ownership. While larceny is more debatable, in general—to the extent that these crimes actually involve ''stealth''—the probability that victims will

[26] Robbery includes street robbery, commercial robbery, service station robbery, convenience store robbery, residence robbery, and bank robbery. (See also the discussion of burglary for why the inclusion of residence robbery creates difficulty with this broad measure.) After we wrote this paper, two different commentators have attempted to argue that ''[i]f 'shall issue' concealed carrying laws really deter criminals from undertaking street crimes, then it is only reasonable to expect the laws to have an impact on robberies. Robbery takes place between strangers on the street. A high percentage of homicide and rape, on the other hand, occurs inside a home—where concealed weapons laws should have no impact. These findings strongly suggest that something else—not new concealed carry laws—is responsible for the reduction in crime observed by the authors.'' (Doug Weil, Response to John Lott's Study on the Impact of ''Carry Concealed'' Laws on Crime Rates, U.S. Newswire, August 8, 1996.) The curious aspect about the emphasis on robbery over other crimes like murder and rape is that if robbery is the most obvious crime to be affected by gun control laws, why have virtually no gun control studies examined robberies? In fact, Kleck's literature survey only notes one previous gun control study that examined the issue of robberies (see Kleck, *supra* note 15). Yet, more importantly, given that the FBI includes many categories of robberies besides robberies that ''take place between strangers on the street,'' it is not obvious why this should exhibit the greatest sensitivity to concealed handgun laws.

[27] Larceny includes pickpockets, purse snatching, shoplifting, bike theft, theft from buildings, theft from coin machines, and theft from motor vehicles.

notice the crime being committed seems low and thus the opportunities to use a gun are relatively rare. The effect on burglary is ambiguous from a theoretical standpoint. It is true that if ''shall issue'' laws cause more people to own a gun, the chance of a burglar breaking into a house with an armed resident goes up. However, if some of those who already owned guns now obtain right-to-carry permits, the relative cost of crimes like armed street robbery and certain other types of robberies (where an armed patron may be present) should rise relative to that for burglary.

Previous concealed handgun studies that rely on state-level data suffer from an important potential problem: they ignore the heterogeneity within states.[28] Our telephone conversations with many law enforcement officials have made it very clear that there was a large variation across counties within a state in terms of how freely gun permits were granted to residents prior to the adoption of ''shall issue'' right-to-carry laws.[29] All those we talked to strongly indicated that the most populous counties had previously adopted by far the most restrictive practices on issuing permits. The implication for existing studies is that simply using state-level data rather than county data will bias the results against finding any effect from passing right-to-carry provisions. Those counties that were unaffected by the law must be separated out from those counties where the change could be quite dramatic. Even cross-sectional city data[30] will not solve this problem, because without time-series data it is impossible to know what effect a change in the law had for a particular city.

There are two ways of handling this problem. First, for the national sample, we can see whether the passage of ''shall issue'' right-to-carry laws

[28] For example, Arnold S. Linsky, Murray A. Strauss, & Ronet Bachman-Prehn, Social Stress, Legitimate Violence, and Gun Availability (paper presented at the annual meeting of the Society for the Study of Social Problems, 1988); and Cramer & Kopel, *supra* note 9.

[29] Among those who made this comment to us were Bob Barnhardt, manager of the Intelligence/Concealed Handgun Unit of Multnomah County, Oregon; Mike Woodward, with the Oregon Law Enforcement Data System; Joe Vincent with the Washington Department of Licensing Firearms Unit; Alan Krug, who provided us with the Pennsylvania Permit data; and Susan Harrell, with the Florida Department of State Concealed Weapons Division. Evidence for this point with respect to Virginia is obtained from Eric Lipton, Virginians Get Ready to Conceal Arms; State's New Weapon Law Brings a Flood of Inquiries, Washington Post, June 28, 1995, at A1, where it is noted that ''[a]nalysts say the new law, which drops the requirement that prospective gun carriers show a 'demonstrated need' to be armed, likely won't make much of a difference in rural areas, where judges have long issued permits to most people who applied for them. But in urban areas such as Northern Virginia—where judges granted few permits because few residents could justify a need for them—the number of concealed weapon permits issued is expected to soar. In Fairfax, for example, a county of more than 850,000 people, only 10 now have permits.'' Cramer & Kopel, *supra* note 9. An expanded version of this paper dated 1994, available from the Independence Institute, Golden, Colorado, also raises this point with respect to California.

[30] For example, Kleck & Patterson, *supra* note 16.

produces systematically different effects between the high and low population counties. Second, for three states, Arizona, Oregon, and Pennsylvania, we have acquired time series data on the number of right-to-carry permits for each county. The normal difficulty with using data on the number of permits involves the question of causality: do more permits make crimes more costly or do higher crimes lead to more permits? The change in the number of permits before and after the change in the state laws allows us to rank the counties on the basis of how restrictive they had actually been in issuing permits prior to the change in the law. Of course, there is still the question of why the state concealed handgun law changed, but since we are dealing with county-level rather than state-level data, we benefit from the fact that those counties which had the most restrictive permitting policies were also the most likely to have the new laws exogenously imposed on them by the rest of their state.

Using county-level data also has another important advantage in that both crime and arrest rates vary widely within states. In fact, as Table 1 indicates, the standard deviation of both crime and arrest rates across states is almost always smaller than the average within-state standard deviation across counties. With the exception of robbery, the standard deviation across states for crime rates ranges from between 61 and 83 percent of the average of the standard deviation within states. (The difference between these two columns with respect to violent crimes arises because robberies make up such a large fraction of the total crimes in this category.) For arrest rates, the numbers are much more dramatic, with the standard deviation across states as small as 15 percent of the average of the standard deviation within states. These results imply that it is no more accurate to view all the counties in the typical state as a homogenous unit than it is to view all the states in the United States as one homogenous unit. For example, when a state's arrest rate rises, it may make a big difference whether that increase is taking place in the most or least crime-prone counties. Depending on which types of counties the changes in arrest rates are occurring in and depending on how sensitive the crime rates are to changes in those particular counties, widely differing estimates of how increasing a state's average arrest rate will deter crime could result. Aggregating these data may thus make it more difficult to discern the true relationship that exists between deterrence and crime.

Perhaps the relatively small across-state variation as compared to within-state variations is not so surprising given that states tend to average out differences as they encompass both rural and urban areas. Yet, when coupled with the preceding discussion on how concealed handgun provisions affected different counties in the same state differently, these numbers strongly imply that it is risky to assume that states are homogenous units

TABLE 1

Comparing the Deviation in Crime Rates between States and by Counties within States from 1977 to 1992: Does It Make Sense to View States as Relatively Homogenous Units?

	Standard Deviation of State Means	Mean of Within-State Standard Deviations
Crime rates per 100,000 population:		
Violent crimes	284.77	255.57
Murder	6.12	8.18
Murder with guns (1982–91)	3.9211	6.4756
Rape	16.33	23.55
Aggravated assault	143.35	172.66
Robbery	153.62	92.74
Property crime	1,404.15	2,120.28
Auto theft	162.02	219.74
Burglary	527.70	760.22
Larceny	819.08	1,332.52
Arrest rates defined as the number of arrests divided by the number of offenses:*		
Violent crimes	23.89	112.97
Murder	18.58	88.41
Rape	19.83	113.86
Robbery	21.97	104.40
Aggravated assault	25.30	78.53
Property crimes	7.907	44.49
Burglary	5.87	25.20
Larceny	11.11	71.73
Auto theft	17.37	118.94
Truncating arrest rates to be no greater than one:		
Violent crimes	11.11	25.40
Murder	10.78	36.40
Rape	10.60	31.59
Robbery	8.06	32.67
Aggravated assault	11.14	27.08
Property crimes	5.115	11.99
Burglary	4.63	14.17
Larceny	5.91	12.97
Auto theft	8.36	26.66

* Because of multiple arrests for a crime and because of the lags between when a crime occurs and an arrest takes place, the arrest rate for counties and states can be greater than one. This is much more likely to occur for counties than for states.

with respect to either how crimes are punished or how the laws which affect gun usage are changed. Unfortunately, this focus of state-level data is pervasive in the entire crime literature, which focuses on state- or city-level data and fails to recognize the differences between rural and urban counties.

However, using county-level data has some drawbacks. Frequently, because of the low crime rates in many low population counties, it is quite common to find huge variations in the arrest and conviction rates between years. In addition, our sample indicates that annual conviction rates for some counties are as high as 13 times the offense rate. This anomaly arises for a couple reasons. First, the year in which the offense occurs frequently differs from the year in which the arrests and/or convictions occur. Second, an offense may involve more than one offender. Unfortunately, the FBI data set allows us neither to link the years in which offenses and arrests occurred nor to link offenders with a particular crime. When dealing with counties where only a few murders occur annually, arrests or convictions can be multiples higher than the number of offenses in a year. This data problem appears especially noticeable for murder and rape.

One partial solution is to limit the sample to only counties with large populations. For counties with a large numbers of crimes, these waves have a significantly smoother flow of arrests and convictions relative to offenses. An alternative solution is to take a moving average of the arrest or conviction rates over several years, though this reduces the length of the usable sample period, depending on how many years are used to compute this average. Furthermore, the moving average solution does nothing to alleviate the effect of multiple suspects being arrested for a single crime.

Another concern is that otherwise law-abiding citizens may have carried concealed handguns even before it was legal to do so. If shall issue laws do not alter the total number of concealed handguns carried by otherwise law-abiding citizens but merely legalizes their previous actions, passing these laws seems unlikely to affect crime rates. The only real effect from making concealed handguns legal could arise from people being more willing to use handguns to defend themselves, though this might also imply that they will be more likely to make mistakes using these handguns.

It is also possible that concealed firearm laws both make individuals safer and increase crime rates at the same time. As Peltzman has pointed out in the context of automobile safety regulations, increasing safety can result in drivers offsetting these gains by taking more risks in how they drive.[31] The same thing is possible with regard to crime. For example, allowing citizens to carry concealed firearms may encourage people to risk entering more

[31] Sam Peltzman, The Effects of Automobile Safety Regulation, 83 J. Pol. Econ. 677–725 (August 1975).

dangerous neighborhoods or to begin traveling during times they previously avoided. Thus, since the decision to engage in these riskier activities is a voluntary one, it is possible that society still could be better off even if crime rates were to rise as a result of concealed handgun laws.

Finally, there are also the issues of why certain states adopted concealed handgun laws and whether higher offense rates result in lower arrest rates. To the extent that states adopted the law because crime was rising, ordinary least squares (OLS) estimates would underpredict the drop in crime. Likewise, if the rules were adopted when crime rates were falling, the bias would be in the opposite direction. None of the previous studies deal with this last type of potential bias. At least since Ehrlich,[32] economists have also realized that potential biases exist from having the offense rate as both the endogenous variable and the denominator in determining the arrest rate and because increasing crime rates may lower the arrest rate if the same resources are being asked to do more work. Fortunately, both these sets of potential biases can be dealt with using two-stage least squares (2SLS).

III. The Data

Between 1977 and 1992, 10 states (Florida (1987), Georgia (1989), Idaho (1990), Maine (1985),[33] Mississippi (1990), Montana (1991), Oregon (1990), Pennsylvania (1989), Virginia (1988),[34] and West Virginia (1989)) adopted ''shall issue'' right-to-carry firearm laws. However, Pennsylvania is a special case because Philadelphia was exempted from the state law during our sample period. Eight other states (Alabama, Connecticut, Indiana, New Hampshire, North Dakota, South Dakota, Vermont, and Washington) effectively had these laws on the books prior to the period being studied.[35] Since the data are at the county level, a dummy variable is set equal to one for each county operating under ''shall issue'' right-to-carry laws. A Nexis

[32] Ehrlich, *supra* note 22, at 548–53.

[33] While we will follow Cramer and Kopel's definition of what constitutes a ''shall issue'' or a ''do issue'' state, one commentator has suggested that it is not appropriate to include Maine in these categories (Stephen P. Teret, Critical Comments on a Paper by Lott and Mustard (photocopy, Johns Hopkins University, School of Hygiene and Public Health, August 7, 1996)). Either defining Maine so that the ''shall issue'' dummy equals zero for it or removing Maine from the data set does not alter the findings shown in this paper. Please see note 49 *infra* for a further discussion.

[34] While the intent of the 1988 legislation in Virginia was clearly to institute a ''shall issue'' law, the law was not equally implemented in all counties in the state. To deal with this problem, we reran the regressions reported in this paper with the ''shall issue'' dummy both equal to 1 and 0 for Virginia. The results as reported later in footnote 49 are very similar in the two cases.

[35] We rely on Cramer & Kopel, *supra* note 9, for this list of states. Some states known as ''do issue'' states are also included in Cramer and Kopel's list of ''shall issue'' states though these authors argue that for all practical purposes these two groups of states are identical.

search was conducted to determine the exact date on which these laws took effect. For the states that adopted the law during the year, the dummy variable for that year is scaled to equal that portion of the year for which the law was in effect. Because of delays in implementing the laws even after they go into effect, we also used a dummy variable that equals one starting during the first full year that the law is in effect. The following tables report this second measure, though both measures produced similar results.

While the number of arrests and offenses for each type of crime in every county from 1977 to 1992 were provided by the Uniform Crime Report, we also contacted the state departments of corrections, state attorneys general, state secretaries of state, and state police offices in every state to try to compile data on conviction rates, sentence lengths, and right-to-carry concealed weapons permits by county. The Bureau of Justice Statistics also released a list of contacts in every state that might have available state-level criminal justice data. Unfortunately, county data on the total number of outstanding right-to-carry pistol permits were available for only Arizona, California, Florida, Oregon, Pennsylvania, and Washington, though time-series county data before and after a change in the permitting law were available only for Arizona (1994–96), Oregon (1990–92) and Pennsylvania (1986–92). Since the Oregon ''shall issue'' law passed in 1990, we attempted to get data on the number of permits in 1989 by calling up every county sheriff in Oregon, with 25 of the 36 counties providing us with this information. (The remaining counties claimed that records had not been kept.)[36] For Oregon, data on the county-level conviction rate and prison sentence length were also available from 1977 to 1992.

One difficulty with the sentence length data is that Oregon passed a sentencing reform act that went into effect in November 1989 causing criminals to serve 85 percent of their sentence, and thus judges may have correspondingly altered their rulings. Even then, this change was phased in over time because the law applied only to crimes that took place after it went into effect in 1989. In addition, the Oregon system did not keep complete records prior to 1987, and the completeness of these records decreased the further into the past one went. One solution to both of these problems is to interact the prison sentence length with year dummy variables. A similar problem exists for Arizona, which adopted a truth-in-sentencing reform during the fall of 1994. Finally, Arizona is different from Oregon and Pennsylvania in that it already allowed handguns to be carried openly before passing its concealed handgun law, thus one might expect to find a somewhat smaller response to adopting a concealed handgun law.

[36] The Oregon counties providing permit data were Benton, Clackamas, Coos, Curry, Deschutes, Douglas, Gilliam, Hood River, Jackson, Jefferson, Josephine, Klamath, Lane, Linn, Malheur, Marion, Morrow, Multnomah, Polk, Tillamook, Washington, and Yamhill.

TABLE 2

National Sample Means and Standard Deviations

Variable	*N*	Mean	S.D.
Gun ownership information:			
Shall issue dummy	50,056	.164704	.368089
Arrests rates (ratio of arrests to offenses) for a particular crime category:			
Index crimes	45,108	27.43394	126.7298
Violent crimes	43,479	71.30733	327.2456
Property crimes	45,978	24.02564	120.8654
Murder	26,472	98.04648	109.7777
Rape	33,887	57.8318	132.8028
Aggravated assault	43,472	71.36647	187.354
Robbery	34,966	61.62276	189.5007
Burglary	45,801	21.51446	47.28603
Larceny	45,776	25.57141	263.706
Auto theft	43,616	44.8199	307.5356
Crime rates are defined per 100,000 people:			
Index crimes	46,999	2,984.99	3,368.85
Violent crimes	47,001	249.0774	388.7211
Property crimes	46,999	2,736.59	3,178.41
Murder	47,001	5.651217	10.63025
Murder with guns (1982–91 in counties over 100,000)	12,759	3.9211	6.4756
Rape	47,001	18.7845	32.39292
Robbery	47,001	44.6861	149.2124
Aggravated assault	47,001	180.0518	243.2615
Burglary	47,001	811.8642	1,190.23
Larceny	47,000	1,764.37	2,036.03
Auto theft	47,000	160.4165	284.5969
Causes of accidental deaths and murders per 100,000 people:			
Rate of accidental deaths from guns	23,278	.151278	1.216175
Rate of accidental deaths from sources other than guns	23,278	1.165152	4.342401
Rate of total accidental deaths	23,278	51.95058	32.13482
Rate of murders using handgun	23,278	.444301	1.930975
Rate of murders using other guns	23,278	3.477088	6.115275
Real per capita income data (in real 1983 dollars):			
Personal income	50,011	10,554.21	2,498.07
Unemployment insurance	50,011	67.57505	53.10043
Income maintenance	50,011	157.2265	97.61466
Retirement payments per person over 65	49,998	12,328.5	4,397.49
Population characteristics:			
County population	50,023	75,772.78	250,350.4
County population per square mile	50,023	214.3291	1,421.25
State population	50,056	6,199,949	5,342,068
State NRA membership per 100,000 state population	50,056	1,098.11	516.0701
% of votes Republican in presidential election	50,056	52.89235	8.410228

TABLE 2 (*Continued*)

Variable	*N*	Mean	S.D.
Race and age data (% of population):			
Black male 10–19	50,023	.920866	1.556054
Black female 10–19	50,023	.892649	1.545335
White male 10–19	50,023	7.262491	1.747557
White female 10–19	50,023	6.820146	1.673272
Other male 10–19	50,023	.228785	.769633
Other female 10–19	50,023	.218348	.742927
Black male 20–29	50,023	.751636	1.214317
Black female 20–29	50,023	.762416	1.2783
White male 20–29	50,023	6.792357	1.991303
White female 20–29	50,023	6.577894	1.796134
Other male 20–29	50,023	.185308	.557494
Other female 20–29	50,023	.186327	.559599
Black male 30–39	50,023	.539637	.879286
Black female 30–39	50,023	.584164	.986009
White male 30–39	50,023	6.397395	1.460204
White female 30–39	50,023	6.318641	1.422831
Other male 30–39	50,023	.151869	.456388
Other female 30–39	50,023	.167945	.454721
Black male 40–49	50,023	.358191	.571475
Black female 40–49	50,023	.415372	.690749
White male 40–49	50,023	4.932917	1.086635
White female 40–49	50,023	4.947299	1.038738
Other male 40–49	50,023	.105475	.302059
Other female 40–49	50,023	.115959	.304423
Black male 50–64	50,023	.43193	.708241
Black female 50–64	50,023	.54293	.921819
White male 50–64	50,023	6.459038	1.410181
White female 50–64	50,023	6.911502	1.54784
Other male 50–64	50,023	.101593	.367467
Other female 50–64	50,023	.11485	.374837
Black male over 65	50,023	.384049	.671189
Black female over 65	50,023	.552889	.980266
White male over 65	50,023	5.443062	2.082804
White female over 65	50,023	7.490128	2.69476
Other male over 65	50,023	.065265	.286597
Other female over 65	50,023	.077395	.264319

In addition to using county dummy variables, other data were collected from the Bureau of the Census to try controlling for other demographic characteristics that might determine the crime rate. These data included information on the population density per square mile, total county population, and detailed information on the racial and age breakdown of the county (percentage of population by each racial group and by sex between 10 and 19 years of age, between 20 and 29, between 30 and 39, between 40 and 49, between 50 and 64, and 65 and over).[37] While a large literature

[37] See Table 2 for the list and summary statistics.

discusses the likelihood of younger males engaging in crime,[38] controlling for these other categories allows us to also attempt to measure the size of the groups considered most vulnerable (for example, females in the case of rape).[39] Recent evidence by Glaeser and Sacerdote confirms the higher crime rates experienced in cities and examines to what extent this arises due to social and family influences as well as the changing pecuniary benefits from crime,[40] though this is the first paper to explicitly control for population density. The Data Appendix provides a more complete discussion of the data.

An additional set of income data was also used. These included real per capita personal income, real per capita unemployment insurance payments, real per capita income maintenance payments, and real per capita retirement payments per person over 65 years of age.[41] Including unemployment insurance and income maintenance payments from the Commerce Department's Regional Economic Information System data set was an attempt to provide annual county-level measures of unemployment and the distribution of income.

Finally, we recognize that other legal changes in how guns are used and when they can be obtained can alter the levels of crime. For example, penalties involving improper gun use might also have been changing simultaneously with changes in the permitting requirements for concealed handguns. In order to see whether this might confound our ability to infer what was responsible for any observed changes in crimes rates we read through various editions of the Bureau of Alcohol, Tobacco, and Firearms' *State Laws and Published Ordinances—Firearms* (1976, 1986, 1989, and 1994). Excluding the laws regarding machine guns and sawed-off shotguns, there is no evidence that the laws involving the use of guns changed significantly when concealed permit rules were changed.[42] Another survey which ad-

[38] For example, James Q. Wilson & Richard J. Herrnstein, Crime and Human Nature 126–47 (1985).

[39] However, the effect of an unusually large percentage of young males in the population may be mitigated because those most vulnerable to crime may be more likely to take actions to protect themselves. Depending on how responsive victims are to these threats, it is possible that the coefficient for a variable like the percentage of young males in the population could be zero even when the group in question poses a large criminal threat.

[40] Edward L. Glaeser & Bruce Sacerdote, Why Is There More Crime in Cities? (working paper, Harvard Univ., November 14, 1995).

[41] For a discussion of the relationship between income and crime see John R. Lott, Jr., A Transaction-Costs Explanation for Why the Poor Are More Likely to Commit Crime, 19 J. Legal Stud. 243–45 (January 1990).

[42] A more detailed survey of the state laws is available from the authors. The findings of a brief survey of the laws excluding the permitting changes are as follows: Alabama: No significant changes in these laws during period. Connecticut: Law gradually changed in wording from criminal use to criminal possession from 1986 to 1994. Florida: Has the most

dresses the somewhat broader question of sentencing enhancement laws for felonies committed with deadly weapons (firearms, explosives, and knives) from 1970 to 1992 also confirms this general finding, with all but four of the legal changes clustered from 1970 to 1981.[43] Yet, controlling for the dates supplied by Marvell and Moody still allows us to examine the deterrence effect of criminal penalties specifically targeted at the use of deadly weapons during this earlier period.[44]

States also differ in terms of their required waiting periods for handgun purchases. Again using the Bureau of Alcohol, Tobacco, and Firearms' *State Laws and Published Ordinances—Firearms,* we identified states with waiting periods and did a Lexis search on those ordinances to determine exactly when those laws went into effect. Thirteen of the 19 states with waiting periods had them prior to the beginning of our sample period.[45]

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extensive description of penalties. The same basic law (790.161) is found throughout the years. An additional law (790.07) is found only in 1986. Georgia: A law (16-11-106) that does not appear in the 1986 edition appears in the 1989 and 1994 issues. The law involves possession of a firearm during commission of a crime and specifies the penalties associated with it. Because of the possibility that this legal change might have occurred at the same time as the 1989 changes in permitting rules, we used a Lexis search to check the legislative history of 16-11-106 and found that the laws were last changed in 1987, 2 years before the change in permitting rules (O.C.G.A. 16-11-106 (1996)). Idaho: There are no significant changes in Idaho over time. Indiana: No significant changes in these laws during the period. Maine: No significant changes in these laws during the period. Mississippi: Law 97-37-1 talks explicitly about penalties. It appears in the 1986 version, but not in the 1989 or the 1994 versions. Montana: Some changes in punishments related to unauthorized carrying of concealed weapons laws, but no changes in the punishment for using a weapon in a crime. New Hampshire: No significant changes in these laws during the period. North Dakota: No significant changes in these laws during the period. Oregon: No significant changes in these laws during the period. Pennsylvania: No significant changes in these laws during the period. South Dakota: Law 22-14-13, which specifies penalties for commission of a felony while armed appears in 1986, but not 1989. Vermont: Section 4005, which outlines the penalties for carrying a gun when committing a felony, appears in 1986, but not in 1989 or 1994. Virginia: No significant changes in these laws during the period. Washington: No significant changes in these laws during the period. West Virginia: Law 67-7-12 is on the books in 1994, but not the earlier versions. It involves punishment for endangerment with firearms. Removing Georgia from the sample, which was the only state that had gun laws changing near the year that the ''shall issue'' law went into affect, so that there is no chance that the other changes in gun laws might affect our results does not appreciably alter our results.

[43] Thomas B. Marvell & Carlisle E. Moody, The Impact of Enhanced Prison Terms for Felonies Committed with Guns, 33 Criminology 247, 258–61 (May 1995).

[44] Using Marvell and Moody's findings shows that the closest time period between these sentencing enhancements and changes in concealed weapon laws is 7 years (Pennsylvania). Twenty-six states passed their enhancement laws prior to the beginning of our sample period, and only four states passed these types of laws after 1981. Maine, which implemented its concealed handgun law in 1985, passed its sentencing enhancement laws in 1971.

[45] The states with a waiting period prior to the beginning of our sample include Alabama, California, Connecticut, Illinois, Maryland, Minnesota, New Jersey, North Carolina, Pennsylvania, Rhode Island, South Dakota, Washington, and Wisconsin. The District of Columbia

IV. The Empirical Evidence

A. Using County Data for the United States

The first group of regressions reported in Table 3 attempts to explain the natural log of the crime rate for nine different categories of crime. The regressions are run using weighted ordinary least squares. While we are primarily interested in a dummy variable to represent whether a state has a ''shall issue'' law, we also control for each type of crime's arrest rate, demographic differences, and dummies for the fixed effects for years and counties. The results imply that ''shall issue'' laws coincide with fewer murders, rapes, aggravated assaults, and rapes.[46] On the other hand, auto theft and larceny rates rise. Both changes are consistent with our discussion on the direct and substitution effects produced by concealed weapons.[47] Rerunning these specifications with only the ''shall issue'' dummy, the ''shall issue'' dummy and the arrest rates, or simply just the ''shall issue'' dummy and the fixed year effects produces even more significant effects for the ''shall issue'' dummy.[48]

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also had a waiting period prior to the beginning of our sample. The states which adopted this rule during the sample include Hawaii, Indiana, Iowa, Missouri, Oregon, and Virginia.

[46] One possible concern with these initial results arises from our use of an aggregate public policy variable (state right-to-carry laws) on county-level data. See Bruce C. Greenwald, A General Analysis of the Bias in the Estimated Standard Errors of Least Squares Coefficients, 22 J. Econometrics 323–38 (August 1983); and Brent R. Moulton, An Illustration of a Pitfall in Estimating the Effects of Aggregate Variables on Micro Units, 72 Rev. Econ. & Stat. 334 (1990). As Moulton writes: ''If disturbances are correlated within the groupings that are used to merge aggregate with micro data, however, then even small levels of correlation can cause the standard errors from the ordinary least squares (OLS) to be seriously biased downward.'' Yet, this should not really be a concern here because of our use of dummy variables for all the counties, which is equivalent to using state dummies as well as county dummies for all but one of the counties within each state. Using these dummy variables thus allows us to control for any disturbances that are correlated within any individual state. The regressions discussed in footnote 53 rerun the specifications shown in Table 3 but also include state dummies that are interacted with a time trend. This should thus not only control for any disturbances that are correlated with the states, but also for any disturbances that are correlated within a state over time. Finally, while right-to-carry laws are almost always statewide laws, there is one exception. Pennsylvania exempted its largest county (Philadelphia) from the law when it was passed in 1989, and it remained exempt from the law during the rest of the sample period.

[47] However, the increase in the number of property crimes is larger than the drop in the number of robberies.

[48] While we believe that such variables as the arrest rate should be included in any regressions on crime, one concern with the results reported in the tables is whether the relationship between the ''shall issue'' dummy and the crime rates still occurs even when all the other variables are not controlled for. Using weighted least squares and reporting only the ''shall issue'' coefficients, we estimated the following regression coefficients (absolute *t*-statistics are shown in parentheses):

The results are large empirically. When state concealed handgun laws went into effect in a county, murders fell by 7.65 percent, and rapes and aggravated assaults fell by 5 and 7 percent.[49] In 1992, there were 18,469 murders, 79,272 rapes, 538,368 robberies, and 861,103 aggravated assaults in counties without ''shall issue'' laws. The coefficients imply that if these counties had been subject to state concealed handgun laws, murders in the United States would have declined by 1,414. Given the concern that has been raised about increased accidental deaths from concealed weapons, it is interesting to note that, for the most recent year that such a breakdown is available, the entire number of accidental handgun deaths in the United States in 1988 was 200. Of this total, 22 accidental deaths were in states with concealed handgun laws and 178 were in those without these laws. The reduction in murders is as much as eight times greater than the total number of accidental deaths in concealed handgun states. Thus, if our results are accurate, the net effect of allowing concealed handguns is clearly to save lives. Similarly, the results indicate that the number of rapes in

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Endogenous Variables	Shall Issue Dummy Only	Shall Issue Dummy and Year Effects Only
Violent crimes	−.335 (22.849)	−.449 (30.092)
Murder	−.394 (19.095)	−.419 (19.829)
Rape	−.147 (8.030)	−.248 (13.34)
Aggravated assault	−.322 (21.932)	−.448 (30.356)
Robbery	−.485 (19.522)	−.561 (22.110)
Property crime	−.1603 (18.030)	−.186 (20.605)
Auto theft	−.268 (7.793)	−.358 (23.407)
Burglary	−.247 (26.381)	−.217 (22.739)
Larceny	−.101 (10.288)	−.136 (13.640)

Regressing the crime rates on only the ''shall issue'' dummy and the year and county dummies produces a ''shall issue'' coefficient that equals −.021 (*t*-statistic = 1.66) for violent crimes and .051 (*t*-statistic = 6.52) for property crimes. The other estimates discussed in the text produce similar results and are available on request from the authors.

[49] While we adopt the classifications used by Cramer and Kopel (*supra* note 9), some are more convinced by other classifications of the states (for example, Weil, *supra* note 26; and Teret, *supra* note 33). Setting the ''shall issue'' dummy for Maine to zero and rerunning the regressions shown in Table 3 results in the following ''shall issue'' coefficients (*t*-statistics in parentheses): −.0295 (2.955) for violent crimes, −0.813 (5.071) for murder, −.0578 (4.622) for rape, −.0449 (3.838) for aggravated assault, −.0097 (0.714) for robbery, .029 (3.939) for property crimes, .081 (6.942) for automobile theft, .0036 (0.466) for burglary, and .0344 (3.790) for larceny. Similarly, setting the ''shall issue'' dummy for Virginia to zero results in the following ''shall issue'' coefficients (*t*-statistics in parentheses): −.0397 (3.775) for violent crimes, −0.868 (5.138) for murder, −.0527 (4.007) for rape, −.05426 (4.410) for aggravated assault, −.0011 (0.076) for robbery, .0334 (4.326) for property crimes, .091 (7.373) for automobile theft, .0211 (2.591) for burglary, and .0348 (3.646) for larceny. As a final test, dropping both Maine and Virginia from the data set results in the following ''shall issue'' coefficients (*t*-statistics in parentheses): −.0233 (2.117) for violent crimes, −0.9698 (5.519) for murder, −.0629 (4.589) for rape, −.0313 (2.436) for aggravated assault, 0.006 (0.400) for robbery, .0361 (4.436) for property crimes, .0977 (7.607) for automobile theft, .0216 (2.526) for burglary, and .03709 (3.707) for larceny.

TABLE 3

The Effect of ''Shall Issue'' Right-to-Carry Firearms Laws on the Crime Rate: National County-Level Cross-Sectional Time-Series Evidence

Exogenous Variables	Endogenous Variables (Natural Logs of the Crime Rate per 100,000 People)								
	ln (Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate)	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Burglary Rate)	ln (Larceny Rate)	ln (Auto Theft Rate)
Shall issue law adopted dummy	−.0490	−.0765	−.0527	−.0701	−.0221	.0269	.00048	.03342	.0714
	(5.017)	(4.660)	(4.305)	(6.137)	(1.661)	(3.745)	(.063)	(3.763)	(6.251)
	1%	2%	1%	1%	.3%	1%	.02%	1%	1%
Arrest rate for the crime category appropriate endogenous variable	−.00048	−.00139	−.00081	−.000896	−.00057	−.000759	−.0024	−.00018	−.00018
	(77.257)	(37.139)	(47.551)	(69.742)	(88.984)	(96.996)	(90.189)	(77.616)	(74.972)
	9%	7%	4%	9%	4%	10%	11%	4%	3%
Population per square mile	.00006	−.00002	−.00002	5.76E−06	.000316	4.83E−06	−.00007	.000037	.00048
	(3.684)	(.942)	(1.022)	(.320)	(15.117)	(.428)	(5.605)	(2.651)	(26.722)
	5%	1%	1%	.4%	17%	1%	9%	4%	36%
Real per capita income data:									
Personal income	7.92E−06	0.0000163	−5.85E−06	4.71E−06	4.73E−06	−.0000102	−.0000184	−.0000123	.000015
	(2.883)	(3.623)	(1.669)	(1.467)	(1.244)	(5.118)	(8.729)	(4.981)	(4.689)
	1%	2%	1%	1%	1%	3%	4%	2%	2%
Unemployment insurance	−.00022	−.00046	−.00047	−.00019	.00007	.00038	.00060	−.00019	.00021
	(3.970)	(5.260)	(6.731)	(2.904)	(.898)	(9.468)	(14.003)	(3.706)	(3.316)
	.07%	1%	1%	.05%	.01%	2%	3%	.08%	.06%
Income maintenance	−.0000699	.00025	−.00017	.000139	−.00032	.00019	.00039	.00002	.00033
	(.841)	(1.928)	(1.634)	(1.438)	(2.840)	(3.107)	(6.219)	(.320)	(3.452)
	.3%	1%	.7%	.7%	1%	2%	4%	.1%	2%
Retirement payments per person over 65	−1.97E−06	−.000013	−2.37E−06	−6.81E−06	−5.50E−06	−8.65E−06	−.0000106	−6.34E−06	−9.27E−06
	(.895)	(3.713)	(.861)	(2.651)	(1.835)	(5.371)	(6.273)	(3.186)	(3.613)
	.5%	3%	.4%	2%	1%	4%	7%	2%	2%

Population	8.59E−08	−3.44E−08	−2.94E−07	4.54E−08	−6.10E−08	−2.18E−07	−2.14E−07	−3.10E−07	−4.06E−09
	(4.283)	(1.109)	(11.884)	(1.947)	(2.271)	(15.063)	(14.060)	(17.328)	(.177)
	1%	.4%	3%	.06%	.06%	6%	5%	6%	.05%
Race and age data (% of population):									
Black male 10–19	.05637	.1134	.04108	.0900695	.10548	.1287	.074	.1710	.0513
	(1.293)	(1.515)	(.722)	(1.767)	(1.752)	(4.068)	(2.214)	(4.366)	(1.007)
	5%	8%	3%	7%	5%	22%	11%	22%	4%
Black male 20–29	.0009	.0663	.0794	−.0528	−.0060	−.0143	−.0203	−.0057	.00665
	(.035)	(1.514)	(2.366)	(1.749)	(.168)	(.759)	(1.022)	(.245)	(.220)
Black male 30–39	.0419	.1085	−.0832	.2024	.0061	.04126	−.0074	.0044	.14955
	(1.063)	(1.640)	(1.617)	(4.424)	(.111)	(1.445)	(.246)	(.124)	(3.254)
Black male 40–49	−.0243	−.33549	.9029	−.3654	−.00867	−.02391	−.03132	.18939	−.6846
	(.300)	(2.498)	(8.562)	(3.860)	(.077)	(.406)	(.506)	(2.601)	(7.235)
Black male 50–64	.1816	−.34753	−.1509	.2861	−.00706	−.0519	.09135	−.1318	.05626
	(2.159)	(2.518)	(1.381)	(2.889)	(.060)	(.843)	(1.409)	(1.730)	(.569)
Black male over 65	.12165	−.14275	.4373	.1053	.17053	−.0367	.06132	−.0965	−.3384
	(1.337)	(.971)	(3.742)	(1.014)	(1.379)	(.567)	(.900)	(1.204)	(3.254)
Black female 10–19	−.00394	.0374	.0368	−.0692	−.18307	.0836	.0217	.1564	−.1766
	(.088)	(.490)	(.630)	(1.321)	(2.957)	(2.570)	(.631)	(3.883)	(3.372)
Black female 20–29	−.0993	−.2247	.1751	−.1938	−.2167	−.0996	−.1688	−.0075	−.2481
	(3.094)	(4.312)	(4.280)	(5.219)	(4.986)	(4.307)	(6.936)	(.264)	(6.711)
Black female 30–39	.1218	−.0828	.1489	.0947	.3808	.13409	.2721	.0944	.1701
	(3.383)	(1.409)	(3.228)	(2.265)	(7.691)	(5.137)	(9.909)	(2.923)	(4.072)
Black female 40–49	.0107	.59197	−.7396	.26946	−.06891	.05958	−.05022	−.0342	.4816
	(.158)	(5.321)	(8.431)	(3.387)	(.738)	(1.213)	(.970)	(.562)	(6.093)
Black female 50–64	−.2105	.20188	.1044	−.0532	.07078	−.0241	−.21799	.0100	.1153
	(2.826)	(1.648)	(1.076)	(.612)	(.684)	(.443)	(3.817)	(.149)	(1.321)
Black female over 65	−.2035	.3071	−.5164	−.1557	−.36915	−.2035	−.3877	−.1234	.2433
	(3.229)	(2.969)	(6.278)	(2.104)	(4.212)	(4.406)	(7.968)	(2.160)	(3.283)
White male 10–19	−.0060	−.0271	.0056	.03998	.00219	−.0066	−.0062	.00027	−.0568
	(.382)	(.935)	(.265)	(2.208)	(.098)	(.593)	(.523)	(.020)	(3.152)
White male 20–29	.00842	.0598	.03779	.0219	.0426	.00456	.01738	.00377	−.0200
	(.729)	(3.023)	(2.528)	(1.623)	(2.636)	(.542)	(1.958)	(.362)	(1.487)
White male 30–39	−.006	−.01289	−.0376	.0739	−.0706	−.0520	−.0268	−.0579	−.0592
	(.322)	(.371)	(1.444)	(3.206)	(2.507)	(3.633)	(1.779)	(3.268)	(2.583)

TABLE 3 (*Continued*)

Exogenous Variables	ln (Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate)	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Burglary Rate)	ln (Larceny Rate)	ln (Auto Theft Rate)
	Endogenous Variables (Natural Logs of the Crime Rate per 100,000 People)								
White male 40–49	−.0095	−.02078	.0898	−.0406	−.11188	−.14626	−.0995	−.1271	−.0962
	(.375)	(.462)	(2.685)	(1.369)	(3.099)	(7.981)	(5.147)	(5.600)	(3.265)
White male 50–64	−.00575	−.0458	.0397	−.0904	−.14195	−.1282	.0729	−.1071	−.2749
	(.236)	(1.074)	(1.237)	(3.184)	(4.104)	(7.309)	(3.942)	(4.929)	(9.771)
White male over 65	−.1291	.02336	.0441	−.1651	.0421	−.1442	−.1194	−.13975	−.1104
	(6.065)	(.618)	(1.547)	(6.627)	(1.370)	(7.635)	(8.887)	(6.264)	(5.651)
White female 10–19	.02346	.0452	.0741	−.00863	.0561	.0824	.0816	.0865	.0866
	(1.410)	(1.473)	(3.307)	(.448)	(2.359)	(6.907)	(6.474)	(5.863)	(4.513)
White female 20–29	.0128	−.0405	.0551	.03926	.01327	−.0086	−.0421	.02928	−.0289
	(.896)	(1.673)	(2.999)	(2.348)	(.669)	(.828)	(3.832)	(2.272)	(1.739)
White female 30–39	.01878	.0447	.14127	.0299	−.0079	.0388	.0171	.06611	−.1017
	(.890)	(1.209)	(5.092)	(1.215)	(.265)	(2.545)	(1.065)	(3.502)	(4.165)
White female 40–49	−.0901	−.00077	−.0689	−.0031	−.02258	.0584	−.0354	.0741	−.0172
	(3.553)	(.017)	(2.061)	(.106)	(.626)	(3.193)	(1.833)	(3.270)	(.585)
White female 50–64	.00332	.0119	.0213	.07882	.03094	.1044	.06396	.1100	.10687
	(.163)	(.335)	(.794)	(3.313)	(1.072)	(7.103)	(4.126)	(6.042)	(4.534)
White female over 65	.0558	−.0681	.0578	.0836	−.0870	.02027	.0483	.03631	−.0459
	(3.719)	(2.588)	(2.904)	(4.761)	(4.046)	(1.867)	(4.218)	(2.701)	(2.636)
Other male 10–19	.2501	.6624	.5572	.1872	.5360	.1587	.2708	.1487	.6039
	(2.179)	(3.022)	(3.546)	(1.389)	(3.124)	(1.917)	(3.100)	(1.451)	(4.532)
Other male 20–29	−.1229	.14495	−.1656	−.0573	.0129	.0786	.0007	.2037	−.4066
	(1.966)	(1.367)	(2.065)	(.794)	(.149)	(1.748)	(.015)	(3.661)	(5.667)

Other male 30–39	.23126	−.2958	−.1907	.4015	−.1021	−.1779	−.4257	−.0415	.64667
	(1.866)	(1.370)	(1.161)	(2.777)	(.572)	(1.996)	(4.532)	(.376)	(4.525)
Other male 40–49	.12678	−.35775	−.2406	−.1903	.77753	.0287	.2356	−.2320	.4640
	(.824)	(1.341)	(1.180)	(1.060)	(3.538)	(.261)	(2.027)	(1.700)	(2.620)
Other male 50–64	−.0904	−.1572	.2403	−.2829	−.39616	−.0211	.2676	−.1952	−.4198
	(.605)	(.623)	(1.240)	(1.612)	(1.869)	(.194)	(2.330)	(1.449)	(2.411)
Other male over 65	.3469	−.2585	.8709	1.0193	−.267	−.0785	.1863	−.2342	−.1792
	(2.222)	(1.019)	(4.389)	(5.566)	(1.237)	(.688)	(1.549)	(1.659)	(.985)
Other female 10–19	−.0303	−.7299	−.1095	.1207	−.3461	−.1769	−.2861	−.2304	−.2739
	(.253)	(3.185)	(.670)	(.857)	(1.936)	(2.049)	(3.140)	(2.155)	(1.971)
Other female 20–29	−.1323	−.3293	.2093	.0933	−.3033	−.1464	−.3243	−.3334	−.5646
	(1.253)	(2.145)	(1.670)	(.557)	(1.535)	(1.849)	(3.366)	(2.435)	(4.768)
Other female 30–39	−.2187	−.1103	.1556	−.1674	−.2158	−.0874	.2703	−.2838	−.7516
	(1.823)	(.531)	(.988)	(1.189)	(1.253)	(1.005)	(2.949)	(2.638)	(5.395)
Other female 40–49	−.1413	.56562	.07877	.1831	−.48132	.2452	−.2767	.6971	−.1461
	(1.011)	(2.343)	(.429)	(1.116)	(2.407)	(2.432)	(2.600)	(5.574)	(.901)
Other female 50–64	−.0972	.4354	−.6588	−.2700	.36585	−.0491	−.4901	.1615	.3078
	(.607)	(1.612)	(3.184)	(1.439)	(1.620)	(.424)	(4.006)	(1.125)	(1.659)
Other female over 65	−.4376	.0569	−.3715	−.4428	−.3596	−.1052	−.1408	−.0478	−.587
	(3.489)	(.277)	(2.324)	(3.012)	(2.058)	(1.148)	(1.458)	(.422)	(4.020)
Intercept	5.8905	2.0247	.4189	4.2648	5.4254	9.1613	8.7058	7.596	8.332
	(15.930)	(3.326)	(.890)	(9.857)	(10.623)	(33.945)	(30.614)	(22.751)	(19.372)
N	43,451	26,458	33,865	43,445	34,949	45,940	45,769	45,743	43,589
F-statistic	115.11	37.95	44.93	70.47	131.75	87.22	82.16	59.33	116.35
Adjusted R^2	.8925	.8060	.8004	.8345	.9196	.8561	.8490	.8016	.8931

Note.—The absolute t-statistics are in parentheses, and the percentage reported below that for some of the numbers is the percent of a standard deviation change in the endogenous variable that can be explained by a 1 standard deviation change in the exogenous variable. Year and county dummies are not shown. All regressions use weighted least squares where the weighting is each county's population.

states without ''shall issue'' laws would have declined by 4,177, aggravated assaults by 60,363, and robberies by 11,898.[50]

On the other hand, property crime rates definitely increased after ''shall issue'' laws were implemented. The results are equally dramatic. If states without concealed handgun laws had passed such laws, there would have been 247,165 more property crimes in 1992 (a 2.7 percent increase). Thus, criminals respond substantially to the threat of being shot by instead substituting into less risky crimes.[51]

A recent National Institute of Justice study[52] estimates the costs of different types of crime based on lost productivity; out-of-pocket expenses such as medical bills and property losses; and losses for fear, pain, suffering, and lost quality of life. While there are questions about using jury awards to measure losses such as fear, pain, suffering, and lost quality of life, the estimates provide us one method of comparing the reduction in violent crimes with the increase in property crimes. Using the numbers from Table 3, the estimated gain from allowing concealed handguns is over $5.74 billion in 1992 dollars. The reduction in violent crimes represents a gain of $6.2 bil-

[50] Given the possible relationship between drug prices and crime, we reran the regressions in Table 3 by including an additional variable for cocaine prices. One argument linking drug prices and crime is that if the demand for drugs is inelastic and if people commit crimes in order to finance their habits, higher drug prices might lead to increased levels of crime. Using the Drug Enforcement Administration's STRIDE data set from 1977 to 1992 (with the exceptions of 1988 and 1989), Michael Grossman, Frank J. Chaloupka, & Charles C. Brown, The Demand for Cocaine by Young Adults: A Rational Addiction Approach (working paper, National Bureau of Economic Research, July 1996), estimate the price of cocaine as a function of its purity, weight, year dummies, year dummies interacted with eight regional dummies, and individual city dummies. There are two problems with this measure of predicted prices: (1) it removes observations during a couple of important years during which changes were occurring in concealed handgun laws and (2) the predicted values that we obtained from this ignored the city-level observations. The reduced number of observations provides an important reason why we do not include this variable in the regressions shown in Table 3. However, the primary impact of including this new variable is to make the ''shall issue'' coefficients in the violent crime regressions even more negative and more significant (for example, the coefficient for the violent crime regression is now −.075, −.10 for the murder regression, −.077 for rape, and −.11 for aggravated assault, with all of them significant at more than the .01 level). Only for the burglary regression does the ''shall issue'' coefficient change appreciably: it is now negative and insignificant. The variable for drug prices itself is negatively related to murders and rapes and positively and significantly related to all the other categories of crime at least at the .01 level for a one-tailed *t*-test. We would like to thank Michael Grossman for providing us with the original regressions on drug prices from his paper.

[51] By contrast, if the question had instead been what would the difference in crime rates have been between either all states or no states adopting right-to-carry handgun laws, the case of all states adopting concealed handgun laws would have produced 2,020 fewer murders, 5,747 fewer rapes, 79,001 fewer aggravated assaults, and 14,862 fewer robberies. By contrast, property crimes would have risen by 336,409.

[52] Ted R. Miller, Mark A. Cohen, & Brian Wiersema, Victim Costs and Consequences: A New Look (February 1996).

lion ($4.28 billion from murder, $1.4 billion from aggravated assault, $374 million from rape, and $98 million from robbery), while the increase in property crimes represents a loss of $417 million ($343 million from auto theft, $73 million from larceny, and $1.5 million from burglary). However, while $5.7 billion is substantial, to put it into perspective, it equals only about 1.23 percent of the total aggregate losses from these crime categories. These estimates are probably most sensitive to the value of life used (in the Miller *et al.* study this was set at about $3 million in 1992 dollars). Higher estimated values of life will increase the net gains from concealed handgun use, while lower values of life will reduce the gains.[53] To the extent that people are taking greater risks toward crime because of any increased safety produced by concealed handgun laws,[54] these numbers will underestimate the total savings from concealed handguns.

The arrest rate produces the most consistent effect on crime. Higher arrest rates imply lower crime rates for all categories of crime. A 1 standard deviation change in the probability of arrest accounts for 3–17 percent of a 1 standard deviation change in the various crime rates. The crime most responsive to arrest rates is burglary (11 percent), followed by property crimes (10 percent); aggravated assault and violent crimes more generally (9 percent); murder (7 percent); rape, robbery, and larceny (4 percent); and auto theft (3 percent).

For property crimes, a 1 standard deviation change in the percentage of the population that is black, male, and between 10 and 19 years of age explains 22 percent of these crime rates. For violent crimes, the same number is 5 percent. Other patterns also show up in the data. For example, more black females between the ages of 20 and 39, more white females between the ages of 10 and 39 and those over 65, and other race females between 20 and 29 are positively and significantly associated with a greater number of rapes occurring. Population density appears to be most important in ex-

[53] We reran the specifications shown in Table 3 by also including state dummies which were each interacted with a time trend variable. In this case, all of the concealed handgun dummies were negative, though the coefficients were not statistically significant for aggravated assault and larceny. Under this specification, adopting concealed handgun laws in those states currently without them would have reduced 1992 murders by 1,839, rapes by 3,727, aggravated assaults by 10,990, robberies by 61,064, burglaries by 112,665, larcenies by 93,274, and auto thefts by 41,512. The total value of this reduction in crime in 1992 dollars would have been $7.02 billion. With the exceptions of aggravated assault and burglary, violent crimes still experienced larger drops from the adoption of concealed handgun laws than did property crimes. Rerunning the specifications in Table 3 without either the percentage of the populations that fall into the different sex, race, and age categories or without the measures of income tended to produce similar though somewhat more significant results with respect to concealed handgun laws. The estimated gains from passing concealed handgun laws were also larger.

[54] Again see Peltzman, *supra* note 31.

plaining robbery, burglary, and auto theft rates, with a 1 standard deviation change in population density being able to explain 36 percent of a 1 standard deviation change in auto theft. Perhaps most surprising is the relatively small, even if frequently significant, effect of income on crime rates. A 1 standard deviation change in real per capita income explains no more than 4 percent of a 1 standard deviation change in crime, and in seven of the specifications it explains 2 percent or less of the change. If the race, sex, and age variables are replaced with variables showing the percentage of the population that is black and the percent that is white, 50 percent of a standard deviation in the murder rate is explained by the percentage of the population that is black. Given the high rates at which blacks are arrested and incarcerated or are victims of crimes, this is not unexpected.

Given the wide use of state-level crime data by economists and the large within-state heterogeneity shown in Table 1, Table 4 provides a comparison by reestimating the specifications reported in Table 3 using state-level rather than county-level data. The only other difference in the specification is the replacement of county dummies with state dummies. While the results in these two tables are generally similar, two differences immediately manifest themselves: (1) all the specifications now imply a negative and almost always significant relationship between allowing concealed handguns and the level of crime and (2) concealed handgun laws explain much more of the variation in crime rates while arrest rates (with the exception of robbery) explain much less of the variation.[55] Despite the fact that concealed handgun laws appear to lower both violent and property crime rates, the results still imply that violent crimes are much more sensitive to the introduction of concealed handguns, with violent crimes falling three times more than property crimes. These results imply that if all states had adopted concealed handgun laws in 1992, 1,592 fewer murders and 4,811 fewer rapes would have taken place.[56] Overall, Table 4 implies that the estimated gain from the lower crime produced by handguns was $8.3 billion in 1992 dollars (see Table 5). Yet, at least in the case of property crimes, the concealed handgun law coefficients' sensitivity to whether these regressions are run at the state or county level suggests caution in aggregating these data into such large units as states.

[55] Other differences also arise in the other control variables such as those relating the percentage of the population of a certain race, sex, and age. For example, the percentage of black males in the population between 10 and 19 is no longer statistically significant.

[56] By contrast, if the question had instead been what would the difference in crime rates have been between either all states or no states adopting right-to-carry handgun laws, the case of all states adopting concealed handgun laws would have produced 2,286 fewer murders, 9,630 fewer rapes, 50,353 fewer aggravated assaults, and 92,264 fewer robberies. Property crimes would also have fallen by 659,061.

TABLE 4

Questions of Aggregating the Data: National State-Level Cross-Sectional Time-Series Evidence

Exogenous Variables	ln (Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate)	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Auto Theft Rate)	ln (Burglary Rate)	ln (Larceny Rate)
Shall issue law adopted dummy	−.1011	−.0862	−.0607	−.1090	−.1421	−.0419	−.0088	−.0825	−.0314
	(3.181)	(2.297)	(1.955)	(3.365)	(3.071)	(1.907)	(.206)	(3.146)	(1.452)
	5.8%	5.0%	4.7%	6.5%	5.7%	4.8%	.43%	7.6%	3.8%
Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.000802	−.00073	−.000205	−.00153	−.0105	−.00599	−.00145	−.00715	−.00657
	(2.920)	(3.979)	(1.823)	(4.230)	(21.030)	(4.591)	(3.727)	(3.772)	(6.257)
	1.5%	5.3%	.69%	3.9%	14.4%	8.1%	6.5%	7.6%	10.4%
Intercept	2.093	−.2715	−1.2892	1.4156	−1.4719	8.5370	8.5195	7.6149	7.7438
	(1.089)	(.121)	(.686)	(.728)	(.531)	(6.502)	(4.687)	(4.847)	(5.985)
N	804	809	804	811	811	811	811	811	811
F-statistic	139.45	103.83	76.44	132.60	126.64	80.25	174.63	85.06	76.83
Adjusted R^2	.9490	.9322	.9103	.9461	.9437	.9135	.9586	.9181	.9100

Note.—Except for the use of state dummies in place of county dummies, the control variables are the same as those used in Table 3 including year dummies, although they are not all reported. Absolute t-statistics are in parentheses, and the percentage reported below that for some of the numbers is the percentage of a standard deviation change in the endogenous variable that can be explained by a 1 standard deviation change in the exogenous variable. All regressions use weighted least squares where the weighting is each state's population.

Table 6 examines whether changes in concealed handgun laws and arrest rates have differential effects in high- or low-crime counties. To test this, the regressions shown in Table 3 were reestimated first using the sample above the median crime rate by type of crime and then separately using the sample below the median. High crime rates may also breed more crime because the stigma from arrest may be less when crime is rampant.[57] If so, any change in apprehension rates should produce a greater reputational effect and thus greater deterrence in low-crime than high-crime counties.

The results indicate that the concealed handgun law's coefficient signs are consistently the same for both low- and high-crime counties, though for two of the crime categories (rape and aggravated assault) concealed handgun laws have only statistically significant effects in the relatively high-crime counties. For most violent crimes such as murder, rape, and aggravated assault, concealed weapons laws have a much greater deterrent effect in high-crime counties, while for robbery, property crimes, auto theft, burglary, and larceny the effect appears to be greatest in low-crime counties. The table also shows that the deterrent effect of arrests is significantly different at least at the 5 percent level between high- and low-crime counties for eight of the nine crime categories (the one exception being violent crimes). The results do not support the claim that arrests produce a greater reputational penalty in low-crime areas. While additional arrests in low- and high-crime counties produce virtually identical changes in violent crime rates, the arrest rate coefficient for high-crime counties is almost four times larger than it is for low-crime counties.

One relationship in these first three sets of regressions deserves a special comment. Despite the relatively small number of women using concealed handgun permits, the concealed handgun coefficient for explaining rapes is consistently comparable in size to the effect that this variable has on other violent crime rates. In the states of Washington and Oregon in January 1996, women constituted 18.6 and 22.9 percent of those with concealed handgun permits for a total of 118,728 and 51,859 permits, respectively.[58] The time-series data which are available for Oregon during our sample period even indicates that only 17.6 percent of permit holders were women in 1991. While it is possible that the set of women who are particularly likely to be raped might already carry concealed handguns at much higher rates

[57] Eric Rasmusen, Stigma and Self-Fulfilling Expectations of Criminality, 39 J. Law & Econ. 519 (1996).

[58] The Washington State data were obtained from Joe Vincent of the State Department of Licensing Firearms Unit in Olympia, Washington. The Oregon state data were obtained from Mike Woodward, with the Law Enforcement Data System, Department of State Police, Salem, Oregon.

TABLE 5

The Effect of Concealed Handguns on Victim Costs: What If All States Had Adopted ‘‘Shall Issue’’ Laws?

Crime Category	Change in Number of Crimes If the States without ‘‘Shall Issue’’ Laws in 1992 Had Adopted the Law		Change in Victim Costs If the States without ‘‘Shall Issue’’ Laws in 1992 Had Adopted the Law (in 1992 Dollars)	
	Estimates Using County-Level Data	Estimates Using State-Level Data	Estimates Using County-Level Data	Estimates Using State-Level Data
Murder	−1,414	−1,592	−4,281,608,125	−4,820,594,155
Rape	−4,177	−4,811	−374,277,659	−431,086,861
Aggravated assault	−60,363	−93,860	−1,405,042,403	−2,184,737,007
Robbery	−11,898	−62,852	−98,033,414	−517,868,225
Burglary	1,052	−180,813	1,516,890	−260,716,190
Larceny	191,743	−180,261	73,068,706	−68,693,188
Auto theft	89,928	−11,084	342,694,264	−42,236,828
Total change in annual victim costs			−5,741,681,741	−8,325,932,454

Note.—The table uses 1996 estimates of the costs of crime in 1992 dollars from Ted R. Miller, Mark A. Cohen, & Brian Wiersema, Victim Costs and Consequences: A New Look (February 1996).

TABLE 6

Questions of Aggregating the Data: Do Law Enforcement and "Shall Issue" Laws Have the Same Effect in High and Low Crime Areas?

Exogenous Variables	ln (Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate)	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Burglary Rate)	ln (Larceny Rate)	ln (Auto Theft Rate)
A. Sample where county crime rates are above the median:									
Shall issue law adopted dummy	−.0597	−.0988	−.0719	−.04468	−.0342	.0161	.0036	.0296	.0524
	(7.007)	(7.173)	(7.415)	(4.411)	(3.012)	(2.943)	(.533)	(5.474)	(5.612)
Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.000523	−.00049	−.000326	−.00063	−.00294	−.005354	−.00565	−.00596	−.00133
	(−17.661)	(11.472)	(3.8130)	(18.456)	(9.381)	(33.669)	(27.390)	(41.585)	(11.907)
B. Sample where county crime rates are below the median:									
Shall issue law adopted dummy	−.0369	−.0436	−.0304	−.0025	−.0787	.0881	.0297	.0874	.07226
	(1.934)	(1.938)	(.978)	(.013)	(2.978)	(5.801)	(2.110)	(5.246)	(3.276)
Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.0005242	−.00123	−.000656	−.00068	−.0003699	−.001354	−.0027135	−.000998	−.0001412
	(30.302)	(25.43)	(31.542)	(37.306)	(9.018)	(39.101)	(41.603)	(37.559)	(62.596)

Note.—The control variables are the same as those used in Table 3 including year and county dummies, although they are not all reported. Absolute t-statistics are in parentheses. All regressions use weighted least squares where the weighting is each county's population.

than the general population of women, the results are at least suggestive that rapists are particularly susceptible to this form of deterrence. Possibly this arises since providing a woman with a gun has a much bigger effect on her ability to defend herself against a crime than providing a handgun to a man. Thus even if relatively few women carry handguns, the expected change in the cost of attacking women could still be nearly as great. To phrase this differently, the external benefits to other women from a woman carrying a concealed handgun appear to be large relative to the gain produced by an additional man carrying a concealed handgun. If concealed handgun use were to be subsidized to capture these positive externalities, these results are consistent with efficiency requiring that women receive the largest subsidies.[59]

As mentioned in Section II, an important concern with these data is that passing a concealed handgun law should not affect all counties equally. In particular, we expect that it was the most populous counties that most restricted people's ability to carry concealed weapons. To test this, Table 7 repeats all the regressions in Table 3 but instead interacts the shall issue law adopted dummy with county population. While all the other coefficients remain virtually unchanged, this new interaction retains the same signs as those for the original shall issue dummy, and in all but one case the coefficients are more significant. The coefficients are consistent with the hypothesis that the new laws produced the greatest change in the largest counties. The larger counties have a much greater response in both directions to changes in the laws. Violent crimes fall more and property crimes rise more in the largest counties. The bottom of the table indicates how these effects vary for different size counties. For example, passing a concealed handgun law lowers the murder rate in counties 2 standard deviations above the mean population by 12 percent, 7.4 times more than a shall issue law lowers murders for the mean population city. While the law enforcement officers we talked to continually mentioned population as being the key variable, we also reran these regressions using population density as the variable that we interacted with the shall issue dummy. The results remain very similar to those reported.

Admittedly, although arrest rates and county fixed effects are controlled for, these regressions have thus far controlled for expected penalties in a limited way. Table 8 reruns the regressions in Table 7 but includes either

[59] Unpublished information obtained by Kleck and Gertz, *supra* note 4, in their 1995 National Self-Defense Survey implies that women were as likely as men to use handguns in self-defense in or near their home (defined as in their yard, carport, apartment hall, street adjacent to home, detached garage, and so on), but that women were less than half as likely to use a gun in self-defense away from home.

TABLE 7

Controlling for the Fact That Larger Changes in Crime Rates are Expected in the More Populous Counties Where the Change in the Law Constituted a Bigger Break with Past Policies

Exogenous Variables	ln (Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate)	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Burglary Rate)	ln (Larceny Rate)	ln (Auto Theft Rate)
Shall issue law adopted dummy *county population	−9.41E−08	−2.07E−07	−7.83E−08	−1.06E−07	−2.29E−08	5.18E−08	6.96E−09	4.90E−08	1.40E−07
	(6.001)	(7.388)	(4.043)	(5.784)	(1.295)	(4.492)	(.572)	(3.432)	(7.651)
Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.000475	−.00139	−.000807	−.000895	−.000575	−.000759	−.002429	−.000177	−.0001754
	(77.222)	(37.135)	(47.535)	(69.663)	(88.980)	(97.027)	(90.185)	(77.620)	(75.013)
N	43,451	26,458	33,865	43,445	34,949	45,940	45,769	45,743	43,589
F-statistic	115.15	38.02	44.92	70.46	131.74	87.23	82.16	59.33	116.41
Adjusted R^2	.8925	.8062	.8004	.8345	.9196	.8561	.8490	.8016	.8931

	Violent Crimes	Murder	Rape	Aggravated Assault	Robbery	Property Crimes	Auto Theft	Burglary	Larceny
Implied percent change in crime rate: The effect of the ''shall issue'' interaction coefficient evaluated at different levels of county populations:									
1/2 Mean = 37,887	−.36	−.78	−.3	−.4	−.1	.2	.03	.2	.5
Mean = 75,773	−.71	−1.6	−.6	−.8	−.2	.4	.05	.4	1.1
Plus 1 SD = 326,123	−3.1	−6.8	−2.6	−3.5	−.7	1.7	.23	1.6	4.6
Plus 2 SD = 576,474	−5.4	−11.9	−4.5	−6.1	−1.3	2.99	.4	2.8	8.1
% of a 1 standard deviation change in corresponding crime rate that can be explained by a 1 standard deviation change in the arrest rate for that crime	9	7	4	9	4	10	11	4	3

Note.—The control variables are the same as those used in Table 3 including year and county dummies, although they are not reported since the coefficient estimates are very similar to those reported earlier. Absolute *t*-statistics are in parentheses. All regressions use weighted least squares where the weighting is each county's population.

TABLE 8

Using Other Crime Rates That Are Relatively Unrelated to Changes in ''Shall Issue'' Rules as a Method of Controlling for Other Changes in the Legal Environment: Controlling for Robbery and Burglary Rates

Exogenous Variables	Endogenous Variables								
	ln (Net Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate)	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Burglary Rate)	ln (Larceny Rate)	ln (Auto Theft Rate)
Controlling for robbery rates:									
Shall issue law adopted dummy *county population	−1.03E−07	−1.72E−07	−7.73E−08	−1.03E−07	. . .	5.61E−08	−3.50E−09	5.35E−08	1.47E−07
	(6.318)	(7.253)	(4.049)	(5.777)		(5.206)	(.304)	(3.911)	(8.844)
Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.0003792	−.0013449	−.00073	−.000776	. . .	−.0006448	−.0020339	−.0001547	−.0001382
	(57.644)	(36.240)	(42.672)	(60.834)		(86.517)	(77.992)	(69.968)	(63.888)
ln(Robbery Rate)	.1083118	.116406	.0983088	.1196466	. . .	.1176149	.1135451	.1164045	.2173908
	(46.370)	(24.616)	(30.363)	(47.469)		(78.825)	(70.826)	(61.762)	(92.212)
N	43,197	26,458	33,865	43,445	. . .	45,940	45,769	45,743	43,589
F-statistic	81.93	39.19	46.55	75.09	. . .	101.83	93.39	65.82	143.54
Adjusted R^2	.8555	.8111	.8062	.8433	. . .	.8744	.8649	.8179	.9117
Controlling for burglary rates:									
Shall issue law adopted dummy *county population	−9.52E−08	−1.73E−07	−8.03E−08	−1.03E−07	−1.47E−08	7.23E−08	. . .	5.50E−08	1.45E−07
	(6.937)	(7.434)	(4.356)	(6.072)	(.759)	(6.854)		(4.769)	(8.943)
Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.00026	−.00128	−.00051	−.00054	−.000429	−.000469	. . .	−.000102	−.000116
	(44.982)	(35.139)	(30.010)	(42.883)	(69.190)	(61.478)		(53.545)	(53.961)
ln(Burglary Rate)	.5667123	.4459916	.4916113	.5302516	.6719892	.5773792	. . .	.6009071	.6416852
	(110.768)	(37.661)	(56.461)	(83.889)	(78.531)	(155.849)		(150.635)	(106.815)
N	43,451	26,458	33,865	43,445	34,949	45,813	. . .	45,743	43,589
F-statistic	154.04	40.78	50.59	84.97	159.18	123.99	. . .	98.08	152.82
Adjusted R^2	.9176	.8173	.8191	.8591	.9327	.8949	. . .	.8706	.9167

Note.—While not all coefficient estimates are reported, all the control variables are the same as those used in Table 3 including year and county dummies. Absolute t-statistics are in parentheses. All regressions use weighted least squares where the weighting is each county's population. Net violent and property crime rates are respectively net of robbery and burglary crime rates to avoid producing any artificial collinearity. Likewise, the arrest rates for those values subtract out that portion of the corresponding arrest rates due to arrests for robbery and burglary.

the burglary or robbery rates to proxy for other changes in the criminal justice system. Robbery and burglary are the violent and property crime categories that are the least related to changes in concealed handgun laws, but they are still positively correlated with all the other types of crimes. One additional minor change is made in two of the earlier specifications. In order to avoid any artificial collinearity either between violent crime and robbery or between property crimes and burglary, violent crimes net of robbery and property crimes net of burglary are used as the endogenous variables when robbery or burglary are controlled for.

Some evidence that burglary or robbery rates will proxy for other changes in the criminal justice system can be seen in their correlations with other crime categories. The Pearson correlation coefficient between robbery and the other crime categories ranges between .49 and .80, and all are statistically significant at least at the .0001 level. For burglary the correlations range from .45 to .68, and they are also equally statistically significant. The two sets of specifications reported in Table 8 closely bound our earlier estimates, and the estimates continue to imply that the introduction of concealed handgun laws coincided with similarly large drops in violent crimes and increases in property crimes. The only difference with the preceding results is that they now imply that the effect on robberies is statistically significant. The estimates on the other control variables also essentially remain unchanged.

We also reestimated the regressions in Table 3 using first differences on all the control variables (see Table 9). These regressions were run using a dummy variable for the presence of ''shall issue'' concealed handgun laws and differencing that variable, and the results consistently indicate a negative and statistically significant effect from the legal change for violent crimes, rape, and aggravated assault. Shall issue laws negatively affect murder rates in both specifications, but the effect is statistically significant only when the shall issue variable is also differenced. The property crime results are also consistent with those shown in the previous tables, showing a positive effect of shall issue laws on crime rates. Perhaps not surprisingly, the results imply that the gun laws immediately altered crime rates, but that an additional change was spread out over time, possibly because concealed handgun use did not instantly move to its new steady-state level (for example, in 1994, Oregon permits increased by 50 percent and Pennsylvania's by 16 percent even though both ordinances had been in effect for at least 4 years). The annual decrease in violent crimes averaged about 2 percent, while the annual increase in property crimes averaged about 5 percent.

The short and long term effects of these legal changes were further examined by reestimating the regressions in Tables 3 and 7 with a time trend for the number of years after the law has been in effect and that time trend

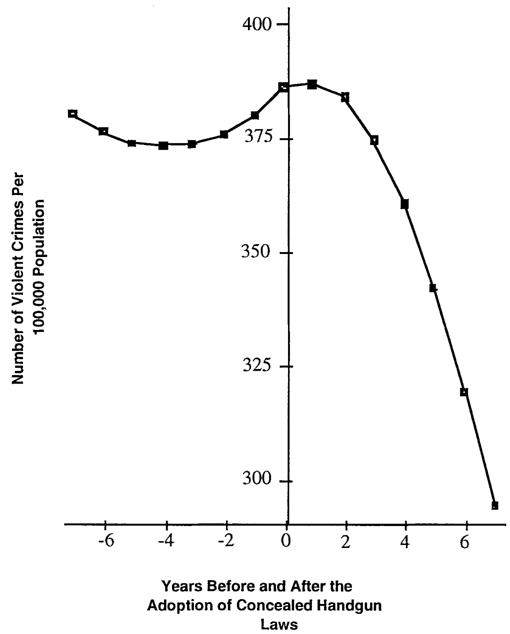

Figure 1.—The effect of concealed handguns on violent crimes

squared. A similar set of time trends were also added for before the law went into effect to test whether there were systematic changes in crime leading up to the passage of the law. While not shown, these regression results provide consistent strong evidence that the deterrent impact of concealed handguns increases with time. For most violent crimes, the time trend leading up to the adoption of the laws indicates that crime was rising prior to the laws being enacted. Figure 1 shows how the violent crime rate varies before and after the implementation of these nondiscretionary permit laws. Using restricted least squares to compare whether the crime rate trends before and after the enactment of the laws were the same, *F*-tests reject that hypothesis at least at the 10 percent level for all the crime categories except aggravated assault and larceny, where the *F*-tests are only significant at the 20 percent level.

All the results in Tables 3, 6, and 7 were reestimated to deal with the concerns raised in Section II over the ‘‘noise’’ in arrest rates arising from the timing of offenses and arrests and the possibility of multiple offenders. We reran all the regressions in this section first by limiting the sample to those counties over 10,000, 100,000, and then 200,000 people. Consistent with the evidence reported in Table 7, the more the sample was limited to

TABLE 9

Rerunning the Regressions on Differences

Exogenous Variables	Endogenous Variables (in Terms of First Differences)								
	Δln (Violent Crime Rate)	Δln (Murder Rate)	Δln (Rape Rate)	Δln (Aggravated Assault Rate)	Δln (Robbery Rate)	Δln (Property Crime Rate)	Δln (Burglary Rate)	Δln (Larceny Rate)	Δln (Auto Theft Rate)
All variables except for the ‘‘shall issue’’ dummy differenced:									
Shall issue law adopted dummy	−.021589	−.025933	−.052034	−.0456251	−.0331607	.0526532	.0352582	.0522435	.128475
	(1.689)	(.841)	(2.761)	(2.693)	(1.593)	(4.982)	(3.16)	(4.049)	(5.324)
First differences in the arrest rate for the crime category corresponding to the appropriate endogenous variable	−.0004919	−.0015482	−.0008641	−.0009272	−.0005725	−.0007599	−.0024482	−.0001748	−.0001831
	(75.713)	(25.967)	(46.509)	(67.782)	(82.38)	(91.259)	(88.38)	(75.969)	(53.432)
Intercept	−.073928	−.0402018	−.014342	−.0522417	−.1203331	−.0952347	−.0770997	−.1062443	−.2604944
	(6.049)	(1.554)	(.904)	(3.68)	(6.925)	(10.8)	(8.312)	(9.872)	(13.009)
N	37,611	20,420	26,269	37,694	27,999	40,901	40,686	40,671	37,581
F-statistic	3.80	.69	2.56	4.03	4.05	4.36	6.62	3.1	10.34
Adjusted R^2	.1867	−.0379	.1389	.1972	.2283	.2047	.3018	.1386	.4338

All variables differenced:									
First differences in the shall issue law adopted dummy	−.026959	−.0363798	−.0394318	−.0540946	.0071132	.0481937	.0072487	.0623146	.2419118
	(2.57)	(1.826)	(2.887)	(4.414)	(.471)	(6.303)	(.898)	(6.676)	(13.884)
First differences in the arrest rate for the crime category corresponding to the appropriate endogenous variable	−.0004919	−.0015481	−.0008642	−.0009275	−.0005724	−.0007598	−.002448	−.0001748	−.0001829
	(75.728)	(25.968)	(46.519)	(67.819)	(82.371)	(91.266)	(88.362)	(75.978)	(53.495)
Intercept	−.0758797	−.042305	−.0188927	−.056264	−.1176478	−.0907433	−.0742121	−.1016434	−.248623
	(6.241)	(1.642)	(1.196)	(3.983)	(6.801)	(10.341)	(8.038)	(9.494)	(12.506)
N	37,611	20,420	26,269	37,694	27,999	40,901	40,686	40,671	37,581
F-statistic	3.8	.69	2.56	4.04	4.05	4.37	6.62	3.11	10.45
Adjusted R^2	.1868	−.0378	.1389	.1975	.2282	.205	.3016	.1393	.4365

Note.—The variables for income, population, racial, sex, and age compositions of the population and density are all in terms of first differences. While not all the coefficient estimates are reported, all the control variables used in Table 3 are used here, including year and county dummies. Absolute t-statistics are in parentheses. All regressions use weighting where the weighting is each county's population.

larger population counties the stronger and more statistically significant was the relationship between concealed handgun laws and the previously reported effects on crime. The arrest rate results also tended to be stronger and more significant. We also tried rerunning all the regressions by redefining the arrest rate as the number of arrests over the last 3 years divided by the total number of offenses over the last 3 years. Despite the reduced sample size, the results remained similar to those already reported.

Two of the most common laws affecting the use of handguns are increased sentencing penalties when crimes are committed using a gun and waiting periods before a citizen can obtain a gun. To test what role these two types of laws may have played in changing crime rates, we reran the regressions in Tables 3 and 4 by adding a dummy variable to control for state laws that increase sentencing penalties when deadly weapons are used and variables to measure the impact of waiting periods.[60] Because we have no strong prior beliefs about whether the effect of waiting periods on crime is linear with respect to the length of the waiting period, we included not only a dummy variable for when the waiting period is in effect but also variables for the length of the waiting period in days and the length in days squared. In both sets of regressions, the dummy variable for the presence of ''shall issue'' concealed handgun laws remains generally consistent with the results reported earlier, though the ''shall issue'' coefficients for robbery in the county-level regressions and for property crimes using the state levels are no longer statistically significant. While the coefficients for arrest rates are not reported, they remain very similar to those shown previously.

With respect to the other gun laws, the pattern shown in Table 10 is less clear. The county-level data imply that increased sentencing penalties when deadly weapons are used reduce violent crimes (particularly, aggravated assault and robbery), but this effect is not statistically significant for violent crimes using state-level data. The state-level data also indicate no statistically significant nor economically consistent relationship between either the presence of waiting periods or their length and crime. While the county-level data frequently imply a relationship between murder, rape, aggravated assault, and robbery, the coefficients imply quite inconsistent effects for these different crimes. For example, simply passing the law appears to raise murder and rape rates but lower aggravated assaults and robbery. These differential effects also apply to the length of the waiting periods, with longer periods at first lowering and then raising the murder and rape rates; the reverse is true for aggravated assaults. However, these results make it very

[60] Marvell & Moody, *supra* note 43, at 259–60. With the exception of only one state, the adoption of waiting periods corresponds to the adoption of background checks.

difficult to argue that waiting periods (particularly long ones) have an overall beneficial effect on crime.

In concluding this section, not only does this initial empirical work provide strong evidence that concealed handgun laws reduce violent crime and that higher arrest rates deter all types of crime, but the work also allows us to evaluate some of the broader empirical issues concerning criminal deterrence discussed in Section II. The results confirm some of our earlier discussions on potential aggregation problems with state-level data. County-level data imply that arrest rates explain about six times the variation in violent crime rates and eight times the variation in property crime rates that arrest rates explain when we use state-level data. Breaking the data down by whether a county is a high- or a low-crime county indicates that arrest rates do not affect crime rates equally in all counties. The evidence also confirms the claims of law enforcement officials that ''shall issue'' laws represented more of a change in how the most populous counties permitted concealed handguns. One concern that was not borne out was over whether state-level regressions could bias the coefficients on the concealed handgun laws toward zero. In fact, while state- and county-level regressions produce widely different coefficients for property crimes, seven of the nine crime categories imply that the effect of concealed handgun laws was much larger when state-level data were used. However, one conclusion is clear: the very different results between state- and county-level data should make us very cautious in aggregating crime data and would imply that the data should remain as disaggregated as possible.

B. The Endogeneity of Arrest Rates and the Passage of Concealed Handgun Laws

The previous specifications have assumed that both the arrest rate and the passage of concealed handgun laws are exogenous. Following Ehrlich,[61] we allow for the arrest rate to be a function of the lagged crime rates; per capita and per violent and property crimes measures of police employment and payroll at the state level (these three different measures of employment are also broken down by whether police officers have the power to make arrests); the measures of income, unemployment insurance payments, and the percentages of county population by age, sex, and race used in Table 3; and county and year dummies.[62] In an attempt to control for political influences,

[61] Ehrlich, *supra* note 22, at 548–51.

[62] See also Robert E. McCormick & Robert Tollison, Crime on the Court, 92 J. Pol. Econ. 223–35 (April 1984), for a novel article testing the endogeneity of the ''arrest rate'' in the context of basketball fouls.

TABLE 10

Controlling for Other Laws Regulating Gun Use

Exogenous Variables	ln (Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate)	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Burglary Rate)	ln (Larceny Rate)	ln (Auto Theft Rate)
A. County-level regressions:									
Shall issue law adopted dummy	−.04171	−.08747	−.06113	−.05462	−.01817	.03633	.0133	.045018	.08206
	(3.976)	(5.173)	(4.660)	(4.452)	(1.272)	(4.717)	(1.636)	(4.723)	(6.695)
Enhanced sentencing law dummy	−.04171	−.00284	.01128	−.01528	−.028832	−.0000151	−.01992	.01219	−.0182
	(3.976)	(.230)	(1.165)	(1.680)	(2.694)	(.003)	(3.340)	(1.733)	(2.021)
Waiting law dummy	.02297	.23386	.2534	−.0937	−.09307	.02023	.02012	−.003398	−.08302
	(.601)	(3.663)	(5.213)	(2.071)	(1.704)	(.718)	(.679)	(.098)	(1.853)
Waiting period in days	−.000829	−.0943	−.1363	.06447	−.1121	−.01477	−.04533	−.011885	−.0100
	(.075)	(5.112)	(9.726)	(4.966)	(7.349)	(1.812)	(5.279)	(1.175)	(.772)
Waiting period in days squared	−.0008046	.00546	.00802	−.00498	.00731	.0001884	.002268	−.001706	.0009851
	(1.182)	(4.864)	(9.363)	(6.248)	(7.836)	(.376)	(4.297)	(2.751)	(1.237)
N	43,451	26,458	33,865	43,445	34,949	45,940	45,769	45,743	43,589
F-statistic	115.06	37.96	45.24	70.51	132.58	87.30	84.99	59.34	116.32
Adjusted R^2	.8926	.8062	.8018	.8348	.9202	.8564	.8499	.8018	.8932

B. State-level regressions:									
Shall issue law adopted dummy	−.1005	−.0810	−.05746	−.10189	−.1332	−.0342	−.0761	−.0219	−.0079
	(3.030)	(2.068)	(1.799)	(3.013)	(2.770)	(1.499)	(2.785)	(.976)	(.178)
Enhanced sentencing law dummy	.0347	.0303	.02725	−.0283	.0073	.0287	.0054	.0369	.0175
	(1.491)	(1.103)	(1.209)	(1.192)	(.217)	(1.798)	(.282)	(2.354)	(.564)
Waiting law dummy	.1010	.0684	.2173	.02613	.1524	.0325	.0647	.0233	−.0307
	(.809)	(.464)	(1.805)	(.205)	(.842)	(.378)	(.628)	(.276)	(.184)
Waiting period in days	−.02988	−.03066	−.1049	−.0065	−.1000	−.0095	−.0220	−.0053	−.0238
	(.854)	(.744)	(3.109)	(.183)	(1.978)	(.397)	(.765)	(.223)	(.509)
Waiting period in days squared	.00117	−.00132	.0059	−.00041	.0059	−.000207	.0005	−.00059	−.00248
	(.576)	(.553)	(3.004)	(.200)	(2.017)	(.149)	(.302)	(.435)	(.921)
N	804	809	804	811	811	811	811	811	811
F-statistic	134.75	100.20	76.15	127.93	123.66	78.29	82.33	75.57	168.47
Adjusted R^2	.9491	.9322	.9129	.9461	.9443	.9144	.9183	.9116	.9586

Note.—The control variables are the same as those used in Table 3 including year and county dummies. Absolute t-statistics are in parentheses. All regressions use weighting where the weighting is each county's population.

we also included the percentage of a state's population that are members of the National Rifle Association and the percentage of the vote received by the Republican presidential candidate at the state level. Because presidential candidates and issues vary between elections, the percentage voting Republican is undoubtedly not directly comparable across years. To account for these differences across elections, we interacted the percentage voting Republican with dummy variables for the years immediately next to the relevant elections. Thus, the percentage of the vote obtained in 1980 is multiplied by a year dummy for the years 1979–82, the percentage of the vote obtained in 1984 is multiplied by a year dummy for the years 1983–86, and so on, through the 1992 election. A second set of regressions explaining the arrest rate also includes the change in the natural log of the crime rates to proxy for the difficulty police forces face in adjusting to changing circumstances.[63] However, the time period studied in all these regressions is more limited than in our previous tables because state-level data on police employment and payroll are only available from the U.S. Department of Justice's Expenditure and Employment data for the Criminal Justice System from 1982 to 1992.

There is also the question of why some states adopted concealed handgun laws while others did not. As noted earlier, to the extent that states adopted the law because crime was either rising or was expected to increase, OLS estimates underpredict the drop in crime. Similarly, if these rules were adopted when crime rates were falling, the bias is in the opposite direction. Thus, in order to predict whether a county would be in a state with concealed handgun laws we used both the natural logs of the violent and property crime rates and the first differences of those crime rates. To control for general political differences that might affect the chances of these laws being adopted, we also included National Rifle Association membership as a percentage of a state's population; the Republican presidential candidate's percentage of the statewide vote; the percentage of a state's population that is black and the percentage white; the total population in the state; regional dummy variables for whether the state is in the South, Northeast, or Midwest; and year dummy variables.

While the 2SLS estimates shown in the top half of Table 11 again use the same set of control variables employed in the preceding tables, the results differ from all our previous estimates in one important respect: concealed handgun laws are associated with large significant drops in the levels of all nine crime categories. For the estimates most similar to Ehrlich's

[63] We would like to thank Phil Cook for suggesting this addition to us. In a sense, this is similar to Ehrlich's specification, *supra* note 22, at 557, except that the current crime rate is broken down into its lagged value and the change between the current and previous periods.

study, five of the estimates imply that a 1 standard deviation change in the predicted value of the shall issue law dummy variable explains at least 10 percent of a standard deviation change in the corresponding crime rates. In fact, concealed handgun laws explain a greater percentage of the change in murder rates than do arrest rates. With the exception of robbery, the set of estimates using the change in crime rates to explain arrest rates indicates a usually more statistically significant but economically smaller effect from concealed handgun laws. For example, concealed handgun laws now explain 3.9 percent of the variation in murder rates compared to 7.5 percent in the preceding results. While these results imply that even crimes with relatively little contact between victims and criminals experienced declines, the coefficients for violent crimes are still relatively more negative than the coefficients for property crimes.

For the first-stage regressions explaining which states adopt concealed handgun laws (shown in the bottom half of Table 11), both the least square and logit estimates imply that the states adopting these laws are relatively Republican with large National Rifle Association memberships and low but rising violent and property crime rates. The other set of regressions used to explain the arrest rate shows that arrest rates are lower in high-income, sparsely populated, Republican areas where crime rates are increasing.

We also reestimated the state-level data using similar 2SLS specifications. The coefficients on both the arrest rates and concealed handgun law variables remained consistently negative and statistically significant, with the state-level data again implying a much stronger effect from concealed handguns and a much weaker effect from higher arrest rates. Finally, in order to use the longer data series available for the nonpolice employment and payroll variables, we reran the regressions without those variables and produced similar results.

Ehrlich also raises the concern that the types of 2SLS estimates shown in Table 11, part A, might still be affected by spurious correlation if the measurement errors for the crime rate are serially correlated over time. (The potential difficulties for part B are much more serious.) To account for this, we reestimated the first stage regressions predicting the arrest rate without the lagged crime rate. Doing this makes the estimated results for the Shall Issue Law dummy even more negative and statistically significant than those already shown.

Finally, using the predicted values for the arrest rates allows us to investigate the significance of another weakness with the data. The arrest rate data experience not only some missing observations but also instances where it is undefined when the crime rate in a county equals zero. This last issue is really only a concern for murders and rapes in low population counties. In

TABLE 11

Regression Estimates of the Causes and Effects of the Adoption of Concealed Handgun Laws

A. Allowing the Change in the ''Shall Issue'' Law and the Arrest Rate to Be Endogenous Using Two-Stage Least Squares (2SLS)*

	Endogenous Variables (in Crimes per 100,000 Population)								
Exogenous Variables	ln (Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate)	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Auto Theft Rate)	ln (Burglary Rate)	ln (Larceny Rate)
1. Using the predicted values of arrest rates similar to Ehrlich's 1973 study:									
Shall issue law adopted dummy	−1.262	−1.1063	−1.059	−1.3192	−.8744	−1.1182	−.7668	−.7603	−1.122
	(21.731)	(5.7598)	(−4.4884)	(18.5277)	(7.4979)	(15.3716)	(11.435)	(19.328)	(25.479)
	10.5%	7.5%	6.4%	10.1%	4.9%	7.67%	11.4%	10.6%	13.5%
Arrest rate for the crime category corresponding to the appropriated endogenous variable	−.002324	−.00094	−.0359	−.002176	−.00241	−.01599	−.002759	−.01783	−.0124
	(9.6892)	(1.8436)	(9.667)	(7.1883)	(4.481)	(33.26)	(2.989)	(14.36)	(31.814)
	60.7%	5.2%	60.1%	44.6%	36.9%	80.1%	21.3%	79.6%	80.6%
N	31,129	31,129	31,129	31,129	31,129	31,129	31,129	31,129	31,129
F-statistic	61.97	19.07	22.3	39.81	63.71	60.78	84.21	46.48	38.37
Adjusted R^2	.8592	.644	.6807	.7953	.8626	.8568	.8893	.8199	.7891
2. Including the change in crime rates when estimating the predicted values of the arrest rates:									
Shall issue law adopted dummy	−.26104	−.5732	−.1992	−.29881	−.0054	−.20994	−.2774	−.1153	−.2623
	(20.12)	(18.21)	(9.6317)	(15.4465)	(.2935)	(29.4242)	(32.5051)	(13.397)	(32.4253)
	2.2%	3.9%	1.2%	2.3%	0.3%	3.3%	2.1%	1.6%	3.2%

Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.007827	−.024	−.02626	−.01028	−.00716	−.00933	−.01233	−.03839	−.0101
	(746.74)	(687.7)	(1,047)	(582)	(901.8)	(820.7)	(1,242.7)	(796.8)	(956.14)
	104%	95%	117%	88%	109%	95%	95.1%	71%	101%
N	31,129	31,129	31,129	31,129	31,129	31,129	31,129	31,129	31,129
F-statistic	1,723	1,260.9	4,909.6	797.5	3,614.86	1,671.49	6,424	1,389	1,625.8
Adjusted R^2	.9942	.9921	.9980	.9876	.9972	.9941	.9984	.9929	.9939

B. First-Stage Estimates of Shall Issue Law†

	Exogenous Variables										
Endogenous Variables	ln (Violent Crime Rate)	Δ ln (Violent Crime Rate)	ln (Property Crime Rate)	Δ ln (Property Crime Rate)	NRA Membership as % of State Population	% Rep. Pres. in State Vote 80 * Year Dummy 79–82	% Rep. Pres. in State Vote 84 * Year Dummy 83–86	% Rep. Pres. in State Vote 88 * Year Dummy 87–90	% Rep. Pres. in State Vote 92 * Year Dummy 91–92	% Population Black for State	% Population White for State
Least squares estimate:											
Shall issue law	−.01817	.00825	−.02889	.0094	.000107	.0061	.0034	.01702	.0299	.00518	.0031
	(9.710)	(5.031)	(8.748)	(2.577)	(19.383)	(5.485)	(4.986)	(22.844)	(27.317)	(13.06)	(8.470)
N						31,137					
F statistic						209.85					
Adjusted R^2						.1436					
Logit:											
Shall issue law	−.0797	.038249	−.2095	.08119	.0004344	.0567	.01456	.09976	.12249	.0409	.0364
	(6.003)	(3.294)	(8.657)	(3.121)	(10.329)	(6.227)	(2.437)	(16.203)	(16.273)	(10.090)	(9.131)
N						31,137					
χ^2						5,007.44					
Pseudo-R^2						.1687					

TABLE 11 (*Continued*)

C. First-Stage Estimates of the Probability of Arrest: Violent and Property Crime Rates‡

Endogenous Variables	Exogenous Variables: ln (Violent Crime Rate Lagged)	ln (Property Crime Rate Lagged)	No. of Police in State Employed with Power of Arrest/State Population	No. of Police in State Employed without Power of Arrest/State Population	NRA Membership as % of State Population	Population Density per Square Mile	% Rep. Pres. in State Vote 80 * Year Dummy 79–82	% Rep. Pres. in State Vote 84 * Year Dummy 83–86	% Rep. Pres. in State Vote 88 * Year Dummy 87–90	% Rep. Pres. in State Vote 92 * Year Dummy 91–92
1. The predicted values of arrest rates that most closely correspond to Ehrlich's 1973 2SLS estimates:										
Arrest rate for violent crimes	−2.224	. . .	−14,093.61	95.085	.01463	.0739	−6.936	−4.293	−3.3467	−3.4316
	(1.441)		(3.065)	(2.206)	(1.940)	(6.418)	(9.975)	(8.270)	(5.865)	(4.967)
N					28,954					
F-statistic						1.83				
Adjusted R^2						.0814				
Arrest rate for property crimes	. . .	.90203	−2,805.2	−1.3057	.01045	.00415	−1.5931	−.9155	−1.1778	−1.2009
		(.738)	(1.173)	(.059)	(1.305)	(.697)	(4.434)	(3.420)	(4.004)	(3.416)
N					30,814					
F-statistic						1.08				
Adjusted R^2						.0084				

	Exogenous Variables								
	ln (Violent Crime Rate Lagged)	Δ ln (Violent Crime Rate)	ln (Property Crime Rate Lagged)	Δ ln (Property Crime Rate)	No. of Police in State Employed with Power of Arrest/State Population	No. of Police in State Employed without Power of Arrest/State Population	NRA Membership as % of State Population	Density per Square Mile	County Population
2. Including the change in crime rates in addition to those already noted when estimating the predicted values of arrest rates (the coefficients on the percentage of the state voting Republican in presidential elections is similar to those reported above):									
Arrest rate for violent crimes	−128.4 (39.86)	−123.64 (44.17)	. . .	. . .	−12,194 (2.750)	96.3244 (2.317)	.0009 (.060)	.0646 (5.284)	−.0000726 (4.877)
N					28,954				
F-statistic					2.59				
Adjusted R^2					.1458				
Arrest rate for property crimes	. . .	. . .	−109.69 (49.342)	−106.92 (58.21)	−1,394 (.618)	−1.9891 (.095)	−.0072 (.949)	.0083 (1.473)	−.0000111 (1.522)
N					30,814				
F-statistic					2.30				
Adjusted R^2					.1165				

Source.—Isaac Ehrlich, Participation in Illegitimate Activities: A Theoretical and Empirical Investigation, 81 J. Pol. Econ. 521–65 (1973).

* While not all coefficient estimates are reported, all the control variables are the same as those used in Table 3, including year and county dummies. Absolute t-statistics are in parentheses, and the percentage reported below that for some of the numbers is the percent of a standard deviation change in the endogenous variable that can be explained by a 1 standard deviation change in the exogenous variable.

† Absolute t-statistics are in parentheses. The sample is limited because the data on police employment used in producing the predicted arrest rates were available only for 1982–92. While the estimates from the first specification were used in the above regressions, the logit estimates are provided for comparison. Not all the variables that were controlled for are shown. These additional variables included year and regional dummies (South, Northeast, and Midwest) and the state's population. NRA = National Rifle Association. % Rep. Pres. = percentage of the vote received by the Republican presidential candidate.

‡ Absolute t-statistics are in parentheses. The sample is limited because the data on police employment were available only for 1982–92. Not all the variables that were controlled for are shown. These additional variables included the number of police with arrest powers divided by the number of violent crimes; the number of police with arrest powers divided by the number of property crimes; the number of police without arrest powers divided by the number of violent crimes; the number of police without arrest powers divided by the number of property crimes; these preceding variables using payrolls; the breakdown of the county's population by age, sex, and race used in Table 3; year and county dummies; the measures of income reported in Table 3; and the state's population. The estimates also using the change in crime rates are available from the authors. NRA = National Rifle Association. % Rep. Pres. = percentage of the vote received by the Republican presidential candidate.

these cases both the numerator and denominator in the arrest rate are equal to zero, and it is not clear whether we should count this as an arrest rate equal to 100 or 0 percent, neither of which seems very plausible. The previously reported evidence where regressions were run only on the larger counties sheds some light on this question since these counties do not exhibit this problem. In addition, if the earlier reported evidence that the movement to nondiscretionary permits largely confirmed the preexisting practice in the lower population counties, one would expect relatively little change in these counties with the missing observations.

However, the analysis presented in this section also allowed us to try another approach to deal with this issue. We created predicted arrest rates for these observations using the regressions that explain the arrest rate in Table 11, and then we reestimated the second-stage relationships shown there for murder and rape with the new larger samples. While the coefficient on murder declines, implying a 5 percent drop when ''shall issue'' laws are adopted, the coefficient for rape increases, now implying over a 10 percent drop. Both coefficients are statistically significant. The effect of arrest rates also remains negative and statistically significant.

C. Concealed Handgun Laws, the Method of Murder, and the Choice of Murder Victims

Do concealed handgun laws cause a substitution in the methods of committing murders? For example, it is possible that the number of gun murders rises after these laws are passed even though the total number of murders falls. While concealed handgun laws raise the cost of committing murders, murderers may also find it relatively more dangerous to kill people using nongun methods once people start carrying concealed handguns and substitute into guns to put themselves on a more even basis with their potential prey. Using data on the method of murder from the Mortality Detail Records provided by the United States Department of Health and Human Services, we reran the murder rate regression from Table 3 on counties over 100,000 during the period from 1982 to 1991. We then separated out murders caused by guns from all other murders. Table 12 shows that carrying concealed handguns appears to have been associated with approximately equal drops in both categories of murders. Carrying concealed handguns appears to make all types of murders relatively less attractive.

There is also the question of what effect concealed handgun laws have on determining which types of people are more likely to be murdered? Using the Uniform Crime Reports Supplementary Homicide Reports we were able to obtain annual state-level data from 1977 to 1992 on the percentage of victims by sex, age, and race as well as information on whether the vic-

TABLE 12

Changes in Murder Methods for Counties over 100,000, 1982–91

Exogenous Variables	Endogenous Variables (in Murders per 100,000 Population) ln(Total Murders)	ln(Murder with Guns)	ln(Murders by Nongun Methods)
Shall issue law adopted dummy	−.09074	−.09045	−.08854
	(3.183)	(1.707)	(1.689)
Arrest rate for murder	−.00151	−.00102	−.00138
	(26.15)	(6.806)	(7.931)
Intercept	.63988	−8.7993	−7.51556
	(.436)	(2.136)	(2.444)
N	12,740	12,759	8,712
F-statistic	21.40	6.60	4.70
Adjusted R^2	.8127	.5432	.5065

Note.—While not all the coefficient estimates are reported, all the control variables are the same as those used in Table 3, including year and county dummies. Absolute *t*-statistics are in parentheses. All regressions use weighting where the weighting is each county's population. The first column uses the Uniform Crime Reports numbers for counties over 100,000, while the second column uses the numbers on total gun deaths available from the Mortality Detail Records, and the third column takes the difference between the Uniform Crime Report's numbers for total murders and Mortality Detail Records of gun deaths.

tim and the offender knew each other (whether they were members of the same family, knew each other but were not members of the same family, strangers, or the relationship is unknown).[64] Table 13 implies no statistically significant relationship between the concealed handgun dummy and the victim's sex, race, or relationships with offenders. However, while they are not quite statistically significant at the .10 level for a two-tailed *t*-test, two of the point estimates appear economically important and imply that in states with concealed handgun laws the percent of victims who know their nonfamily offenders rose by 2.6 percentage points and that the percentage of victims where it was not possible to determine whether a relationship existed declined by 2.9 percentage points. This raises the question of whether concealed handguns cause criminals to substitute into crimes against those whom they know and presumably are also more likely to know whether

[64] While county-level data were provided in the Supplementary Homicide Report, matching these county observations with those used in the Uniform Crime Report (UCR) proved unusually difficult. A unique county identifier was used in the Supplementary Homicide Report, and it was not consistent across years. In addition, some caution is suggested in using both the Mortality Detail Records and the Supplementary Homicide Report since the murder rates reported in both sources have relatively low correlations of less than .7 with the murder rates reported in Uniform Crime Reports. This is especially surprising for the Supplementary Report, which is derived from the UCR.

TABLE 13

Changes in Composition of Murder Victims Using Annual State-Level Data from the Uniform Crime Reports Supplementary Homicide Reports, 1977–92

	Endogenous Variables (in Percentage Points)									
	By Victim's Sex			By Victim's Race			By Victim's Relationship with Offender			
Exogenous Variables	Male	Female	Unidentified	White	Black	Hispanic	Offender Is Known to Victim but Is Not in Family	Offender Is in the Family	Offender Is a Stranger	Relationship Is Unknown
Shall issue law adopted dummy	.3910	−.4381	.0476	.0137	.7031	−.8659	2.5824	−.2503	.5438	−2.8755
	(.388)	(.439)	(.399)	(.017)	(.575)	(.609)	(1.567)	(.210)	(.459)	(1.464)
Arrest rate for murder	.00068	−.001385	.000703	−.0202	.0132	.00327	.0174	−.0145	.0079	−.0108
	(.141)	(.289)	(1.227)	(2.316)	(2.244)	(.478)	(2.198)	(2.541)	(1.394)	(1.141)
Intercept	102.20	−3.2763	1.0558	152.19	−30.948	−7.7863	−73.4677	165.1719	89.843	−81.55
	(1.718)	(.056)	(.150)	(1.418)	(.428)	(.093)	(.755)	(2.345)	(165.17)	(.703)
N	804	804	804	804	804	804	804	804	804	804
F-statistic	14.27	14.51	1.06	45.47	125.09	35.94	14.96	12.87	7.84	26.06
Adjusted R^2	.6409	.6450	.0077	.8568	.9435	.8245	.6525	.6150	.4790	.7712

Note.—While not all the coefficient estimates are reported, all the control variables are the same as those used in Table 4, including year and state dummies. Absolute t-statistics are in parentheses. All regressions use weighting where the weighting is each state's population.

they carry concealed handguns. While the effect of age (not shown in Table 13) is negative (consistent with the notion that concealed handguns deter crime against adults more than young people because only adults can legally carry concealed handguns), the effect is statistically insignificant. Possibly some of the benefits from adults carrying concealed handguns are conferred to younger people who may be protected by these adults.

The arrest rate for murder variable produces more interesting results. The percentage of white victims and the percentage of victims killed by family members both declined when states passed concealed handgun laws, while the percentage of black victims and the percentage of victims killed by nonfamily members that they know both increased. The results imply that higher arrest rates have a much greater deterrence effect on murders involving whites and family members. One explanation is that whites with higher incomes face a greater increase in expected penalties for any given increase in the probability of arrest.

D. Arizona, Pennsylvania, and Oregon County Data

One problem with the preceding results was the use of county population as a proxy for how restrictive counties were in allowing concealed handgun permits before the passage of ''shall issue'' laws. Since we are still going to control county-specific levels of crime with county dummies, a better measure would have been to use the actual change in gun permits before and after the adoption of a concealed handgun law. Fortunately, we were able to get that information for three states: Arizona, Oregon, and Pennsylvania (see Table 14). Arizona and Oregon also provided additional information on the conviction rate and the mean prison sentence length. However, for Oregon, because the sentence length variable is not directly comparable over time, it is interacted with all the year dummies so that we can still retain any cross-sectional information in the data. One difficulty with the Arizona prison sentence and conviction data is that they are available only from 1990 to 1995 and that since the shall issue handgun law did not take effect until July 1994, it is not possible for us to control for all the other variables that we control for in the other regressions. Unlike Oregon and Pennsylvania, Arizona did not allow private citizens to carry concealed handguns prior to July 1994, so the value of concealed handgun permits equals zero for this earlier period. Unfortunately, however, because Arizona's change in the law is so recent, we are unable to control for all the variables that we can control for in the other regressions.

The results in Table 15 for Pennsylvania and Table 16 for Oregon provide a couple of consistent patterns. The most economically and statistically important relationship involves the arrest rate: higher arrest rates consis-

TABLE 14

Oregon, Pennsylvania, and Arizona Sample Means and Standard Deviations

Variable	Oregon			Pennsylvania			Arizona		
	N	Mean	S.D.	*N*	Mean	S.D.	*N*	Mean	S.D.
Gun ownership information:									
Shall issue dummy	576	.1875	.39065	1,072	.24627	.4310	90	.33333	.47404
Change in the (number of right-to-carry pistol permits/population 21 and over) between 1988 and each year since the law was implemented, otherwise zero	576	.02567	.13706	1,072	.46508	1.2365	90	2.1393	15.02066
Arrest rates are the ratio of arrests to offenses for a particular crime category:									
Violent crimes	576	66.17437	49.2031	1,072	55.0738	21.1293			
Murder	368	100.8344	97.2253	801	92.2899	64.0169			
Rape	507	37.80920	37.8298	1,031	52.5967	32.8287			
Aggravated assault	558	76.37541	62.5568	1,070	57.4422	25.6491			
Robbery	490	50.98248	53.2559	999	53.5970	49.3320			
Property crimes	576	21.95107	7.90548	1,072	21.0539	7.12458			
Auto theft	566	57.17941	99.6343	1,069	36.6929	63.9266			
Burglary	576	18.99394	11.0296	1,072	18.8899	8.50639			
Larceny	576	21.71564	8.21388	1,072	22.0378	7.47778			
Conviction rates are the ratio of convictions to arrests for a particular crime category (for Arizona it is the ratio of convictions to offenses):									
Violent crimes	542	25.93325	40.5691				90	16.0757	33.85482
Murder	358	94.42969	107.128				90	111.8722	107.9311
Rape	444	161.7508	215.635				90	47.4365	81.42314
Aggravated assault	536	2.505037	5.61042				90	9.204778	13.66225
Robbery	420	38.51352	49.9308				90	17.09185	39.17454
Property crime	555	6.530883	13.8484				90	1.370787	1.432515
Auto theft	539	10.1805	14.3673				90	1.175114	3.671085
Burglary	544	15.56064	17.7937				90	2.534157	3.4627
Larceny	552	2.577337	11.3266				90	1.070667	1.308081

Prison sentence in months (Oregon) or years (Arizona):									
Murder	327	301.6697	164.55				90	16.0557	7.31179
Rape	443	103.2212	50.4662				90	8.761905	5.974623
Aggravated assault	241	154.4647	79.7893				90	4.28876	1.874496
Robbery	364	106.8709	55.4847				90	6.852239	3.108169
Auto theft	405	43.40494	20.7846				90	1.415	.3308054
Burglary	489	65.17791	32.2003				90	3.937647	1.03187
Larceny	424	46.42925	19.0075				90	66.64444	145.6599
Crime rates defined per 100,000 people:									
Violent crimes	576	4079.07	1621.53	1,072	2281.56	967.430	90	429.2972	254.1692
Murder	576	4.52861	6.67245	1,072	3.01319	4.12252	90	5.7787778	4.413259
Rape	576	31.4474	25.4623	1,072	15.9726	11.6156	90	23.5	18.90888
Aggravated assault	576	196.192	152.965	1,072	107.332	78.5966	90	339.2977	200.0264
Robbery	576	50.5625	89.5707	1,072	45.2030	86.7830	90	60.72056	71.75822
Property crimes	576	282.666	230.421	1,072	171.485	156.683	90	4,147.692	2,282.633
Auto theft	576	228.403	157.204	1,072	160.831	162.572	90	351.3749	339.0281
Burglary	576	1,089.5	495.926	1,072	753.668	535.022	90	950.7187	563.3711
Larceny	576	2,761.17	1,098.06	1,072	1,367.06	569.563	90	2,845.597	1,569.837
Real per capita income data (in real 1983 dollars):									
Personal income	576	11,389.39	1,630.47	1,072	11,525	2,099.44			
Unemployment insurance	576	108.8037	45.9864	1,072	130.560	64.0694			
Income maintenance	576	131.4323	40.3703	1,072	149.652	69.5516			
Retirement payments per person over 65	576	12,335.17	1,278.18	1,072	13,398.9	2,253.29			
Population characteristics:									
County population	576	74,954.98	112,573.3	1,072	177,039	274,289.9			
County population per square mile	576	77.46861	219.7100	1,072	453.549	1,516.16			
Race and age data (% of population):									
Black male under 10	576	.051847	.092695	1,072	.2089	.439286			
Black female under 10	576	.049275	.089665	1,072	.2018	.434456			
White male under 10	576	7.367641	.683587	1,072	6.7258	.808574			

TABLE 14 (*Continued*)

Variable	Oregon			Pennsylvania			Arizona		
	N	Mean	S.D.	*N*	Mean	S.D.	*N*	Mean	S.D.
White female under 10	576	7.012212	.649409	1,072	6.3567	.761709			
Other male under 10	576	.322532	.437321	1,072	.0525	.040573			
Other female under 10	576	.307242	.402487	1,072	.0536	.039637			
Black male 10–19	576	.052283	.084658	1,072	.2515	.468536			
Black female 10–19	576	.047129	.088479	1,072	.2276	.473586			
White male 10–19	576	7.603376	.952584	1,072	7.7274	1.155154			
White female 10–19	576	7.140808	.895257	1,072	7.37287	1.158130			
Other male 10–19	576	.308009	.348147	1,072	.05396	.040844			
Other female 10–19	576	.295728	.286703	1,072	.05141	.038375			
Black male 20–29	576	.064034	.087570	1,072	.24866	.439191			
Black female 20–29	576	.042044	.082821	1,072	.22014	.497373			
White male 20–29	576	6.918945	1.613700	1,072	7.53233	1.416936			
White female 20–29	576	6.767993	1.485155	1,072	7.56037	1.094322			
Other male 20–29	576	.280987	.322992	1,072	.05412	.078002			
Other female 20–29	576	.273254	.287497	1,072	.05431	.060281			
Black male 30–39	576	.048262	.073100	1,072	.19163	.354741			
Black female 30–39	576	.032534	.071081	1,072	.17443	.419096			
White male 30–39	576	7.363739	.883651	1,072	6.81373	.850949			
White female 30–39	576	7.333140	.845647	1,072	6.87622	.837649			
Other male 30–39	576	.227610	.215892	1,072	.04737	.050606			
Other female 30–39	576	.248852	.221020	1,072	.05518	.045324			
Black male 40–49	576	.030101	.044355	1,072	.12300	.244123			
Black female 40–49	576	.022872	.043869	1,072	.12520	.311716			
White male 40–49	576	5.506716	.817220	1,072	5.27656	.727481			
White female 40–49	576	5.456938	.760387	1,072	5.43223	.650546			
Other male 40–49	576	.148190	.127731	1,072	.03571	.030029			
Other female 40–49	576	.157778	.121413	1,072	.03901	.030711			
Black male 50–64	576	.028558	.045301	1,072	.13316	.305455			
Black female 50–64	576	.024530	.050093	1,072	.15634	.404990			
White male 50–64	576	7.123300	1.164997	1,072	7.27097	.814601			
White female 50–64	576	7.396392	1.084129	1,072	8.08559	1.031230			
Other male 50–64	576	.135419	.115337	1,072	.02496	.021059			
Other female 50–64	576	.158164	.126546	1,072	.03093	.021638			

TABLE 15

Using Pennsylvania Data on the Number of Permits Issued to Measure the Differential Impact of Pennsylvania's 1989 "Shall Issue" Law on Different Counties: Data for Counties with Populations over 200,000

Exogenous Variables	Endogenous Variables (in Crimes per 100,000 Population)								
	ln (Violent Crime Rate)	ln (Murder Rate)	ln (Rape Rate	ln (Aggravated Assault Rate)	ln (Robbery Rate)	ln (Property Crime Rate)	ln (Auto Theft Rate)	ln (Burglary Rate)	ln (Larceny Rate)
Change in the (number right-to-carry pistol permits/population over 21) between 1988 and each year since the law was implemented	−.0527	−.267	−.0567	−.0481	.0124	−.00116	.0146	−.0140	.0073
	(1.653)	(2.759)	(1.725)	(1.656)	(.265)	(.060)	(.337)	(.562)	(.37)
	10%	21%	6%	9%	2%	1%	2%	4%	2%
Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.00785	−.00365	−.000804	−.00763	−.000836	−.0041	−.00065	−.0112	.00126
	(7.371)	(6.364)	(.668)	(6.413)	(7.031)	(2.057)	(1.185)	(5.138)	(.641)
	25%	15%	2%	28%	24%	8%	4%	25%	2%
Population per square mile	−.000386	.00262	.000987	−.00039	.0005395	.00037	−.000171	.000518	.00077
	(.832)	(1.991)	(1.087)	(.600)	(.835)	(1.283)	(.275)	(1.442)	(2.601)
Real per capita personal income	.0000376	−.000016	.000066	.0000197	.000047	−.0000485	−.000067	−.000034	−.00004
	(1.074)	(.156)	(1.071)	(.452)	(1.055)	(2.611)	(1.599)	(1.396)	(2.025)
Intercept	−15.352	118.93	−67.015	34.752	−52.529	−10.31	27.816	−29.40	6.2484
	(.348)	(1.069)	(.889)	(.671)	(.993)	(.467)	(.557)	(1.016)	(.269)
N	264	264	264	264	264	264	264	264	264
F-statistic	219.4	38.70	42.49	75.00	227.51	111.04	225.8	87.43	83.19
Adjusted R^2	.9841	.9150	.9221	.9549	.9848	.9691	.9846	.9609	.9591

Note.—Absolute t-statistics are in parentheses, and the percentage reported below is the percent of a standard deviation change in the endogenous variable that can be explained by a 1 standard deviation change in the exogenous variable. While not all the coefficient estimates are reported, all the control variables are the same as those used in Table 3, including year and county dummies. All regressions use weighted least squares where the weighting is each county's population. The use of SHALL∗POPULATION variable that was used in the earlier regressions instead of the change in right-to-carry permits variable was tried here and produced very similar results. We also tried controlling for either the robbery or burglary rates, but we obtained very similar results.

TABLE 16

Oregon Data on the Number of Permits Issued, the Conviction Rate, and Prison Sentence Lengths

Exogenous Variables	Endogenous Variables (in Crimes per 100,000 Population)						
	ln(Murder Rate)	ln(Rape Rate)	ln(Aggravated Assault Rate)	ln(Robbery Rate)	ln(Auto Theft Rate)	ln(Burglary Rate)	ln(Larceny Rate)
Change in the (number right-to-carry pistol permits/population over 21) between 1988 and each year since the law was implemented	−.3747	−.0674	−.0475	−.04664	.1172	.02655	−.0936
	(1.598)	(.486)	(.272)	(.385)	(1.533)	(.536)	(2.328)
	3%	1%	.5%	.28%	1%	1%	3%
Arrest rate for the crime category corresponding to the appropriate endogenous variable	−.00338	−.00976	−.00442	−.00363	−.00036	−.00679	−.00936
	(6.785)	(9.284)	(7.279)	(4.806)	(1.481)	(4.458)	(6.764)
	17%	19%	19%	9%	3%	16%	16%
Conviction rate conditional on arrest for the crime category corresponding to the appropriate endogenous variable	−.00208	−.00093	−.01511	−.00190	−.00373	−.00274	−.00859
	(6.026)	(7.668)	(2.150)	(4.465)	(3.031)	(4.297)	(3.140)
	11%	10%	6%	4%	4%	10%	20%
Population per square mile	−.00333	.0063	.01177	.0079	.00062	.00425	−.00030
	(.415)	(.059)	(2.430)	(2.551)	(.367)	(3.937)	(.319)
Real per capita personal income	−.000138	−.000038	−.000162	−.000108	.000037	.000021	8.29E−6
	(.769)	(.463)	(1.301)	(1.542)	(.965)	(.816)	(.407)
Intercept	6.1725	8.2432	84.464	−16.303	2.6213	−11.2489	20.047
	(.342)	(.496)	(3.131)	(1.114)	(.326)	(2.169)	(4.748)
N	250	393	239	337	403	487	422
F-statistic	5.74	16.61	38.79	97.94	156.02	89.90	86.81
Adjusted R^2	.6620	.8113	.9439	.9677	.9766	.9522	.9569

Note.—Absolute *t*-statistics are in parentheses, and the percentage reported below that is the percent of a standard deviation change in the endogenous variable that can be explained by a 1 standard deviation change in the exogenous variable. We also controlled for prison sentence length, but the different reporting practices used by Oregon over this period makes its use somewhat problematic. To deal with this problem the prison sentence length variable was interacted with year dummy variables. Thus while the variable is not consistent over time it is still valuable in distnguishing penalties across counties at a particular point in time. While not all the coefficient estimates are reported, all the remaining control variables are the same as those used in Table 3, including year and county dummies. The categories for violent and property crimes are eliminated because the mean prison sentence data supplied by Oregon did not allow us to use these two categories. All regressions use weighted least squares where the weighting is each county's population.

tently imply lower crime rates, and in 12 of the 16 regressions the effect is statistically significant. Five cases for Pennsylvania (violent crime, murder, aggravated assault, robbery, and burglary) show that arrest rates explain more than 15 percent of a standard deviation change in crime rates. Automobile theft is the only crime for which the arrest rate is insignificant in both tables.

For Pennsylvania, murder and rape are the only crimes where a 1 standard deviation change in per capita concealed handgun permits explains a greater percentage of a standard deviation in crime rates than it does for the arrest rate. However, increased concealed handgun usage explains more than 10 percent of a standard deviation change in murder, rape, aggravated assault, and burglary rates. For six of the nine regressions, the concealed handgun variable for Pennsylvania exhibits the same coefficient signs that were shown for the national data. Violent crimes, with the exception of robbery, show that higher concealed handgun use lowers crime rates, while property crimes exhibit very little relationship. Concealed handgun use only explains about one-tenth the variation for property crimes that it explains for violent ones.[65] The regressions for Oregon weakly imply a similar relationship between concealed handgun use and crime, but the effect is only statistically significant in one case: larceny, which is also the only crime category where the negative concealed handgun coefficient differs from our previous findings.

The Oregon data also show that higher conviction rates consistently result in significantly lower crime rates. A 1 standard deviation change in conviction rates explains 4–20 percent of a 1 standard deviation change in the corresponding crime rates. However, increases in conviction rates appear to produce a smaller deterrent effect than increases in arrest rates for five of the seven crime categories.[66] The biggest differences between the deterrent effects of arrest and conviction rates produce an interesting pat-

[65] Running the regressions for all Pennsylvania counties (and not just those over 200,000 population) produced similar coefficients and signs for the change in concealed handgun permits coefficient, though the coefficients were no longer statistically significant for violent crimes, rape, and aggravated assault. Alan Krug, who provided us with the Pennsylvania handgun permit data, told us that one reason for the large increase in concealed handgun permits in some rural counties was because people used the guns for hunting. He told us that these low population rural counties tended to have their biggest increase in people obtaining permits in the fall around hunting season. If people were in fact getting a large number of permits in low population counties which already have extremely low crime rates for some reason other than crime, it will make it more difficult to pick up the deterrent effect on crime from concealed handguns that was occurring in the larger counties.

[66] We reran these regressions taking the natural logs of the arrest and conviction rates, and they continued to produce statistically larger and even economically more important effects for the arrest rates than they did for the conviction rates.

tern. For rape, increasing the arrest rate by 1 percentage point produces more than 10 times the deterrent effect of increasing the conviction rate conditional on arrest by 1 percent. The reverse is true for auto theft, where a 1 percentage point increase in arrests reduces crime by about 10 times more than the same increase in convictions. These results are consistent with arrests producing large shaming or reputational penalties.[67] In fact, the existing evidence shows that the reputational penalties from arrest and conviction can dwarf the other legally imposed penalties.[68] However, while the literature has not separated out whether these drops are occurring because of arrest or conviction, these results are consistent with the reputational penalties for arrests alone being significant for at least some crimes.

One possible explanation for these results is that Oregon simultaneously passed both the ''shall issue'' concealed handgun law and a waiting limit. Given the very long waiting period imposed by the Oregon law (15 days), the regressions in Table 10 imply that such a waiting period increases murder by 4.8 percent, rape by 2 percent, and robbery by 5.9 percent. At least in the case of murder, which is almost statistically significant in any case, combining the two sets of regressions implies that the larger drop in murder that would have been observed in the absence of the Oregon waiting period would have produced a *t*-statistic for murder of 1.8.

The results for the prison sentences are not shown, but the *t*-statistics are frequently near zero and the coefficients indicate no clear pattern. One possible explanation for this result is that all the changes in sentencing rules produced a great deal of noise in this variable not only over time but also across counties. For example, after 1989 whether a crime was prosecuted under the pre- or post-1989 rules depended on when the crime took place. If the average time between when the offense occurred and when the prosecution took place differs across counties, the recorded prison sentence length could vary even if the actual time served was the same.

Finally, the much more limited data set for Arizona used in Table 17 produces no significant relationship between the change in concealed handgun permits and the various measures of crime rates. In fact, the coefficient signs themselves indicate no consistent pattern, with the 14 coefficients being equally divided between negative and positive signs, though six of the specifications imply that a 1 standard deviation change in the concealed handgun permits explains at least 8 percent of a 1 standard deviation change in the corresponding crime rates. The results involving either the mean

[67] For example, see Dan M. Kahan, What Do Alternative Sanctions Mean? 63 U. Chi. L. Rev. 591–653 (1996).

[68] Lott, *supra* note 23; Lott, The Effect of Conviction; and An Attempt at Measuring the Total Monetary Penalty from Drug Convictions, both *supra* note 24.

prison sentence length for those sentenced in a particular year or the actual time served for those ending their sentences also imply no consistent relationship between prison and crime rates. While the coefficients are negative in 11 of the 14 specifications, they provide weak evidence of the deterrent effect of longer prison terms: only two coefficients are negative and statistically significant. Since the Brady Law also went into effect during this sample period, we reran Table 17 using a dummy variable for the Brady Law. Both the coefficients for the change in permits and the Brady Law dummy variable are almost always insignificant, except for the case of aggravated assault, where the Brady Law is both positive and significant, implying that it increased the number of aggravated assaults by 24 percent.

Overall, the Pennsylvania results provide more evidence that concealed handgun ownership reduces violent crime, murder, rape, and aggravated assault, and in the case of Oregon larceny decreases as well. While the Oregon data imply that the change in handgun permits is statistically significant at 11 percent level for a one-tailed *t*-test, the point estimate is extremely large economically, implying that a doubling of permits reduces murder rates by 37 percent. The other coefficients for Pennsylvania and Oregon imply no significant relationship between the change in concealed handgun ownership and crime rates. The evidence from the small sample for Arizona implies no relationship between crime and concealed handgun ownership. All the results also support the claim that higher arrest and conviction rates deter crime, though, possibly in part due to the relatively poor quality of the data, no systematic effect appears to occur from longer prison sentences.

Combining these individual state estimates with the National Institute of Justice's measures of the losses that victims bear from crime allows us to attach a monetary value to the marginal social benefit from an additional concealed handgun permit and to compare this with the private costs of gun ownership. While the results for Arizona imply no real savings from reduced crime, the estimates for Pennsylvania indicate that potential victims' costs are reduced by $5,079 for each additional concealed handgun permit, and for Oregon the savings are $3,439 per permit. As with the discussion in Table 5, the results are largely driven by the effect that concealed handguns have in lowering the murder rate (with savings of $4,986 for Pennsylvania and $3,202 for Oregon).

These estimated gains appear to far exceed the private costs of owning a concealed handgun. The purchase price of concealed handguns ranges from $25 for the least expensive .25-caliber pistols to $719 for the newest ultracompact 9 millimeter models; the permit filing fees can range from $19 every 5 years in Pennsylvania to a first-time $65 fee with subsequent 5-year renewals at $50 in Oregon; and several hours of supervised safety training are required in Oregon. Assuming a 5 percent real interest rate and the abil-

TABLE 17

Arizona Data on the Number of Permits Issued, the Conviction Rate, and Prison Sentence Lengths, 1990–95

Exogenous Variables	Endogenous Variables (in Crimes per 100,000 Population)													
	ln(Murder Rate)		ln(Rape Rate)		ln(Aggravated Assault Rate)		ln(Robbery Rate)		ln(Auto Theft Rate)		ln(Burglary Rate)		ln(Larceny Rate)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Change in the (number right-to-carry pistol permits/population) from the zero allowed before the law and each year since the law was implemented; the numbers for 1994 were multiplied by .5	.0016 (.209) 1.7%	.0025 (.311) 2.7%	−.0803 (1.397) 8%	−.0095 (.334) 2%	.0051 (1.265) 9%	−.00516 (1.291) 9%	.0037 (.574) 3%	.0039 (.551) 3%	−.0019 (.222) 2%	−.0076 (.940) 9%	.0006 (.210) 8%	.0007 (.225) 9%	−.0003 (.094) 1%	−.0005 (.185) 1%
Conviction rate for the crime category corresponding to the appropriate endogenous variable	−.0039 (7.677) 29%	−.00399 (6.798) 30%	−.0055 (7.558) 27%	−.0053 (7.014) 26%	−.0453 (13.51) 72%	−.0429 (12.18) 67%	−.0111 (9.553) 21%	−.0110 (9.391) 20%	−.1373 (1.678) 37%	−.1605 (1.879) 43%	−.10032 (14.44) 28%	−.1037 (14.62) 29%	−.325 (12.1) 60%	−.3298 (13.80) 60%

Mean prison sentence length for those sentenced to prison in that year	−.01033	. . .	.0052	. . .	−.0261	. . .	−.0095	. . .	−.0087	. . .	−.0084	. . .	−.018	. . .
	(1.457)		(.364)		(1.155)		(.629)		(.055)		(1.759)		(.936)	
	5%		2%		6%		1%		.2%		.7%		3%	
Time served for those ending their prison terms in that year	. . .	.0041	. . .	−.0178	. . .	−.0170	. . .	−.0221	. . .	.0317	. . .	−.0119	. . .	−.0952
		(.18)		(.602)		(.464)		(.871)		(.463)		(.405)		(3.479)
		4%		2%		2%		2%		2%		.8%		11%
Population per square mile	−.1014	−.0791	−.4748	−.4459	−.1424	−.1361	−.1411	−.1514	−.413	−.4019	−.0835	−.0798	−.0313	−.00030
	(.826)	(.569)	(3.595)	(3.274)	(2.164)	(1.942)	(1.288)	(1.477)	(2.603)	(2.433)	(1.759)	(1.670)	(.631)	(.319)
Intercept	1.208	.926	1.4750	1.477	4.341	4.365	1.838	1.753	3.432	2.5099	5.467	5.4296	6.621	6.873
	(3.594)	(1.765)	(5.095)	(5.262)	(28.46)	(26.30)	(5.157)	(4.203)	(5.061)	(7.094)	(38.66)	(5.430)	(53.03)	(57.475)
N	74	70	78	75	89	86	64	68	60	89	84	84	85	84
F-statistic	17.26	14.50	27.64	24.86	56.48	38.79	81.33	76.67	32.12	39.60	109.61	101.18	99.75	118.24
Adjusted R^2	.8367	.8182	.8925	.8856	.9380	.9439	.9656	.9629	.9239	.9330	.9691	.9666	.9658	.9713

Note.—Absolute t-statistics are in parentheses, and the percentage reported below that is the percent of a standard deviation change in the endogenous variable that can be explained by a 1 standard deviation change in the exogenous variable. All variables, except for the county's population and the year and county dummies, have been reported. The categories for violent and property crimes are eliminated because the mean prison sentence data supplied by Oregon did not allow us to use these two categories. All regressions use weighting where the weighting is each county's population. Odd-numbered columns control for mean prison sentence, while even-numbered columns control for time actually served for those leaving prison.

ity to amortize payments over 10 years, purchasing a $300 handgun and paying the licensing fees every 5 years in Pennsylvania implies a yearly cost of only $43, excluding the time costs incurred. The estimated expenses for Oregon are undoubtedly higher because of both the higher fees and the time costs and fees involved in obtaining certified safety instruction, but even if these annual costs double, they are still quite small compared to the social benefits. While any ammunition purchases and additional annual training would increase annualized costs, the very long life span of guns and the ability to resell them work to reduce the above estimate. The results imply that permitted handguns are being obtained at much lower than optimal rates, perhaps because of the important externalities not directly captured by the handgun owners themselves.

V. Accidental Deaths from Handguns

Even if ''shall issue'' handgun permits lower murder rates, the question of what happens to accidental deaths still remains. Possibly, with more people carrying handguns, accidents may be more likely to happen. Earlier we saw that the number of murders prevented exceeded the entire number of accidental deaths. In the case of suicide, carrying concealed handguns increases the probability that a gun will be available to commit suicide with when an individual feels particularly depressed, and thus it could conceivably increase the number of suicides. As Table 2 showed, while only a small portion of accidental deaths are attributable to handgun laws, there is still the question whether concealed handgun laws affected the total number of deaths through their effect on accidental deaths.

To get a more precise answer to this question, Table 18 uses county-level data from 1982 to 1991 to test whether allowing concealed handguns increased accidental deaths. Data are available from the Mortality Detail Records (provided by the United States Department of Health and Human Services) for all counties from 1982 to 1988 and for counties over 100,000 population from 1989 to 1991. The specifications are identical to those shown in all the previous tables with the exceptions that we no longer include variables related to arrest or conviction rates and that the endogenous variables are replaced with a measure of the number of either accidental deaths from handguns or accidental deaths from all other nonhandgun sources.

While there is some evidence that the racial composition of the population and the level of income maintenance payments affect accident rates, the coefficient of the shall issue dummy is both quite small economically and insignificant. The point estimates for the first specification imply that accidental handgun deaths rose by about .5 percent when concealed hand-

TABLE 18

Did Carrying Concealed Handguns Increase the Number of Accidental Deaths? County-Level Data, 1982–91

Exogenous Variables	Endogenous Variables (in Deaths per 100,000 Population)			
	Ordinary Least Squares		Tobit	
	ln(Accidental Deaths from Handguns)	ln(Accidental Deaths from Nonhandgun Sources)	Accidental Deaths from Handguns	Accidental Deaths from Nonhandgun Sources
Shall issue law adopted dummy	.00478	.0980	.574	1.331
	(.096)	(1.706)	(.743)	(.840)
Population per square mile	−.0007	.000856	−.0000436	−.0001635
	(6.701)	(7.063)	(.723)	(1.083)
Real per capita personal income	.0000267	−.000057	.0000436	−.009046
	(1.559)	(2.882)	(1.464)	(6.412)
Intercept or ancillary parameter	−3.376	−8.7655	7.360841	29.36
	(1.114)	(2.506)	(44.12)	(201.7)
N	23,271	23,271	23,271	23,271
F-statistic	3.98	3.91		
Adjusted R^2	.2896	.2846		
Log likelihood			−7,424.6	−109,310.6
Left-censored observations			21,897	680

Note.—While not all the coefficient estimates are reported, all the control variables are the same as those used in Table 3, including year and county dummies. Absolute t-statistics are in parentheses. All regressions weight the data by each county's population.

gun laws were passed. With only 156 accidental handgun deaths during 1988 (22 accidental handgun deaths occurred in states with ''shall issue'' laws), this point estimate implies that implementing a concealed handgun law in those states which currently do not have it would produce less than one more death (.851 deaths).

Given the very small number of accidental handgun deaths in the United States, the vast majority of counties have an accidental handgun death rate of zero, and thus using ordinary least squares is not the appropriate method of estimating these relationships. To deal with this, the last two columns in Table 18 reestimate these specifications using Tobit procedures. However, because of limitations in statistical packages we were no longer able to control for all the county dummies and opted to rerun these regressions with only state dummy variables. While the coefficients for the concealed handgun law dummy variable is not statistically significant, with 186 million people living in states without these laws in 1992,[69] the third specification implies that implementing the law across those remaining states would have resulted in about 9 more accidental handgun deaths. Combining this finding with the earlier estimates from Tables 3 and 4, if the rest of the country had adopted concealed handgun laws in 1992, the net reduction in total deaths would have been approximately from 1,405 to 1,583.

VI. Conclusion

Allowing citizens without criminal records or histories of significant mental illness to carry concealed handguns deters violent crimes and appears to produce an extremely small and statistically insignificant change in accidental deaths. If the rest of the country had adopted right-to-carry concealed handgun provisions in 1992, at least 1,414 murders and over 4,177 rapes would have been avoided. On the other hand, consistent with the notion that criminals respond to incentives, county-level data provides evidence that concealed handgun laws are associated with increases in property crimes involving stealth and where the probability of contact between the criminal and the victim is minimal. The largest population counties where the deterrence effect from concealed handguns on violent crimes is the greatest also experienced the greatest substitution into property crimes. The estimated annual gain in 1992 from allowing concealed handguns was over $5.74 billion.

The study provides the first estimates of the annual social benefit from private expenditures on crime reduction, with an additional concealed hand-

[69] In 1991, 182 million people lived in states without these laws, so the Tobit regressions would have also implied nine more accidental handgun deaths in that year.

gun permit reducing total victim losses by up to $5,000. The results imply that permitted handguns are being obtained at much lower than optimal rates in two of the three states for which we had the relevant data, perhaps because of the important externalities that are not captured by the individual handgun owners. Our evidence implies that concealed handguns are the most cost-effective method of reducing crime thus far analyzed by economists, providing a higher return than increased law enforcement or incarceration, other private security devices, or social programs like early educational intervention.[70]

The data also supply dramatic evidence supporting the economic notion of deterrence. Higher arrest and conviction rates consistently and dramatically reduce the crime rate. Consistent with other recent work,[71] the results imply that increasing the arrest rate, independent of the probability of eventual conviction, imposes a significant penalty on criminals. Perhaps the most surprising result is that the deterrent effect of a 1 percentage point increase in arrest rates is much larger than the same increase in the probability of conviction. Also surprising is that while longer prison lengths usually implied lower crime rates, the results were normally not statistically significant.

This study incorporates a number of improvements over previous studies on deterrence, and it represents a very large change in how gun studies have been done. This is the first study to use cross-sectional time-series evidence for counties at both the national level and for individual states. Instead of simply using cross-sectional state- or city-level data, our study has made use of the much bigger variations in arrest rates and crime rates between rural and urban areas, and it has been possible to control for whether the lower crime rates resulted from the gun laws themselves or other differences in these areas (for example, low crime rates) which led to the adoption of these laws. Equally important, our study has allowed us to examine what effect concealed handgun laws have on different counties even within the same state. The evidence indicates that the effect varies both with a county's level of crime and with its population.

[70] For a comparison with the efficiency of other methods to reduce crime, see John Donohue and Peter Siegelman, Is the United States at the Optimal Rate of Crime? Stanford University School of Law (1996); and Ian Ayres and Steven Levitt, Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of Lojack (Yale University working paper, October 1996). For a discussion of what constitutes true externalities (both benefits and costs) from crime, see Kermit Daniel and John R. Lott, Jr., Should Criminal Penalties Include Third-Party Avoidance Costs? 24 J. Legal Stud. 523–34 (June 1995).

[71] Kahan, *supra* note 67; and Lott, The Effect of Conviction; and An Attempt at Measuring the Total Monetary Penalty from Drug Convictions, both *supra* note 24.

DATA APPENDIX

The number of arrests and offenses for each crime in every county from 1977 to 1992 were provided by the Uniform Crime Report (UCR). The UCR program is a nationwide, cooperative statistical effort of over 16,000 city, county, and state law enforcement agencies to compile data on crimes that are reported to them. During 1993, law enforcement agencies active in the UCR Program represented over 245 million U.S. inhabitants, or 95 percent of the total population. The coverage amounted to 97 percent of the U.S. population living in metropolitan statistical areas (MSAs) and 86 percent of the population in non-MSA cities and in rural counties.[72] The Uniform Crime Reports Supplementary Homicide Reports supplied the data on the victim's sex and race and whatever relationship might have existed between the victim and the offender.[73]

The regressions report results from a subset of the UCR data set, though we also ran the regressions with the entire data set. The main differences were that the effects of concealed handgun laws on murder were greater than what is shown in this paper and the effects on rape and aggravated assault were smaller. Observations were eliminated because of changes in reporting practices or definitions of crimes (see *Crime in the United States* (1977–92)). For example, from 1985 to 1994 Illinois adopted a unique ''gender-neutral'' definition of sex offenses. Another example involves Cook County, Illinois, from 1981 to 1984 where there was a large jump in reported crime because there was a change in the way officers were trained to report crime. The additional observations that either were never provided or were dropped from the data set include Arizona (1980), Florida (1988), Georgia (1980), Kentucky (1988), and Iowa (1991). The counties with the following cities were also eliminated: violent crime and aggravated assault for Steubenville, Ohio (1977–89); violent crime and aggravated assault for Youngstown, Ohio (1977–87); violent crime, property crime, aggravated assault, and burglary for Mobile, Alabama (1977–85); violent crime and aggravated assault for Oakland, California (1977–90); violent crime and aggravated assault for Milwaukee, Wisconsin (1977–85); all crime categories for Glendale, Arizona (1977–84); violent crime and aggravated assault for Jackson, Mississippi (1977–83); violent crime and aggravated assault for Aurora, Colorado (1977–82); violent crime and aggravated assault for Beaumont, Texas (1977–82); violent crime and aggravated assault for Corpus Cristi, Texas (1977–82); violent crime and rape for Macon, Georgia (1977–81); violent crime, property crime, robbery, and larceny for Cleveland, Ohio (1977–81); violent crime and aggravated assault for Omaha, Nebraska (1977–81); all crime categories for Little Rock, Arkansas (1977–79); all crime categories for Eau Claire, Wisconsin (1977–78); all crime categories for Green Bay, Wisconsin (1977).

For all of the different crime rates, except for the Supplementary Homicide Data, if the true rate equals zero, we added .1 before we took the natural log of those

[72] Federal Bureau of Investigation, Crime in the United States (Uniform Crime Reports 1994). We also wish to thank Tom Bailey at the FBI and Jeff Maurer at the U.S. Department of Health and Human Services for answering questions concerning the data used in this article.

[73] The Intercensal Estimates of the Population of Counties by Age, Sex and Race (ICPSR) number for this data set was 6,387, and the principal investigator was James Alan Fox of Northeastern University College of Criminal Justice.

values. For the accident rates and the Supplementary Homicide Data, if the true rate equals zero, we added .01 before we took the natural log of those values.[74]

The original Uniform Crime Report data set did not have arrest data for Hawaii in 1982. These missing observations were supplied to us by the Hawaii Uniform Crime Report program. In the original data set, a few observations also had two listings for the same county and year identifiers. The incorrect observations were deleted from the data.

The number of police in a state, which of those police have the power to make arrests, and police payrolls for a state by type of police officer are available for 1982–92 from the U.S. Department of Justice's Expenditure and Employment Data for the Criminal Justice System.

The data on age, sex, and racial distributions estimate the population in each county on July 1 of the respective years. The population is divided into 5-year segments, and race is categorized as white, black, and neither white nor black. The population data, with the exception of 1990 and 1992, were obtained from the Bureau of the Census.[75] The estimates use modified census data as anchor points and then employ an iterative proportional fitting technique to estimate intercensal populations. The process ensures that the county-level estimates are consistent with estimates of July 1 national and state populations by age, sex, and race. The age distributions of large military installations, colleges, and institutions were estimated by a separate procedure. The counties for which special adjustments were made are listed in the report.[76] The 1990 and 1992 estimates have not yet been completed by the Bureau of the Census and made available for distribution. We estimated the 1990 data by taking an average of the 1989 and 1991 data. We estimated the 1992 data by multiplying the 1991 populations by the 1990–91 growth rate of each county's populations.

Data on income, unemployment, income maintenance, and retirement were obtained by the Regional Economic Information System. Income maintenance includes Supplemental Security Insurance, Aid to Families with Dependent Children, and food stamps. Unemployment benefits include state unemployment insurance compensation, Unemployment for Federal Employees, unemployment for railroad employees, and unemployment for veterans. Retirement payments include Old Age, Survivors, and Disability Insurance, federal civil employee retirement payments,

[74] Dropping the zero crime values from the sample made the shall issue coefficients larger and more significant, but doing the same thing for the accident rate regressions did not alter those shall issue coefficients. (See also the discussion at the end of Section IV*B.*)

[75] For further descriptions of the procedures for calculating intercensus estimates of population, see U.S. Department of Commerce, Bureau of the Census, Intercensal Estimates of the Population of Counties by Age, Sex, and Race (United States): 1970-1980 (ICPSR No. 08384, ICPSR, Ann Arbor, Mich., Winter 1985); also see U.S. Department of Commerce, Bureau of the Census, Intercensal Estimates of the Population of Counties by Age, Sex and Race: 1970–1980 Tape Technical Documentation. U.S. Bureau of the Census, Current Population Reports, Series P-23, No. 103, Methodology for Experimental Estimates of the Population of Counties by Age and Sex: July 1, 1975. U.S. Bureau of the Census, Census of Population, 1980: County Population by Age, Sex, Race and Spanish Origin (Preliminary OMB-Consistent Modified Race).

[76] U.S. Bureau of the Census, Current Population Reports, Series P-23, No. 103, Methodology for Experimental Estimates of the Population of Counties by Age and Sex: July 1, 1975. U.S. Bureau of the Census, Census of Population, 1980: County Population by Age, Sex, Race and Spanish Origin (Preliminary OMB-Consistent Modified Race), at 19–23.

military retirement payments, state and local government employee retirement payments, and workers compensation payments (both federal and state). Nominal values were converted to real values by using the consumer price index.[77] The index uses the average consumer price index for July 1983 as the base period. There were 25 observations whose county codes did not match any counties listed in the ICPSR code book. Those observations were deleted from the sample.

Data concerning the number of concealed weapons permits for each county were obtained from a variety of sources. The Pennsylvania data were obtained from Alan Krug. Mike Woodward of the Oregon Law Enforcement and Data System provided the Oregon data for 1991 and after. The number of permits available for Oregon by county in 1989 was provided by the sheriffs' departments of the individual counties. Cari Gerchick, deputy county attorney for Maricopa County in Arizona, provided us with the Arizona county-level conviction rates, prison sentence lengths, and concealed handgun permits from 1990 to 1995. The National Rifle Association provided data on their membership by state from 1977 to 1992. Information on the dates at which states enacted enhanced sentencing provisions for crimes committed with deadly weapons was obtained from Marvell and Moody.[78] The first year where the dummy variable comes on is weighted by the portion of that first year that the law was in effect.

For the Arizona regressions, the Brady Law dummy for 1994 is weighted by the percentage (83 percent) of the year that it was in effect.

The Bureau of the Census provided data on the area in square miles for each county. The number of total and firearm unintentional injury deaths was obtained from annual issues of *Accident Facts* and *The Vital Statistics of the United States.* The classification of types of weapons is in *International Statistical Classification of Diseases and Related Health Problems, Tenth Edition, Volume 1.* The handgun category includes guns for single-hand use, pistols, and revolvers. The total includes all other types of firearms.

Finally, while our regressions use the ICPSR's estimates of arrest rates, after this paper was accepted we discovered that the ICPSR may have accidentally recorded some missing data on the number of arrests as zero. Working with the ICPSR and the FBI we attempted to correct this problem, and doing so tends to usually increase the significance and size of the shall issue dummies.

[77] U.S. Bureau of the Census, Statistical Abstract of the United States, Table No. 746, at 487 (114th ed. 1994).

[78] Marvell & Moody, *supra* note 43, at 259–60.