Crime, Deterrence, and Right-to-Carry Concealed Handguns

John R. Lott, Jr.
School of Law
University of Chicago
Chicago, Illinois 60637

and

David B. Mustard
Department of Economics
University of Chicago
Chicago, Illinois 60637

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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 and it appears to produce no increase in accidental deaths. If those states which did not have right-to-carry concealed gun provisions had adopted them in 1992, approximately 1,570 murders; 4,177 rapes; and over 60,000 aggravate assaults would have been avoided yearly. On the other hand, consistent with the notion of criminals responding to incentives, we find criminals substituting into property crimes involving stealth and where the probabilities of contact between the criminal and the victim are minimal. The largest population counties where the deterrence effect on violent crimes is greatest are where the substitution effect into property crimes is highest. Concealed handguns also have their greatest deterrent effect in the highest crime counties. Higher arrest and conviction rates consistently and dramatically reduce the crime rate. Consistent with other recent work (Lott, 1992b), the results imply that increasing the arrest rate, independent of the probability of eventual conviction, imposes a significant penalty on criminals. The estimated annual gain from allowing concealed handguns is at least $6.214 billion.

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, "If 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] Using the National Crime Victimization Survey (NCVS), Cook (1991, p. 56, fn. 4) further states that each year there are "only" 80,000 to 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 (Kleck and Gertz, 1995, pp. 153, 180, and 182-3).[4] With total firearm deaths from homicides and accidents equaling 19,187 in 1991 (Statistical Abstract of the United States, 1995), 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 (Japan Economic Newswire, May 23, 1993 and Sharn, USA TODAY, September 9, 1993), 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 (Potok, March 22, 1996, p. 3A).[5] Yet, in neither case was the shooting found to be

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1 Editorial, The Cincinnati Enquirer, January 23, 1996, Pg. A8.

2 See Cook (1982) and Zimring (1971) for these arguments.

3 It is very easy to find people arguing that concealed handguns will have no deterrence effect. Uviller (1996, p. 95) writes that, "More handguns lawfully in civilian hands will not reduce deaths froom 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 Kleck and Gertz's survey (1995, pp. 182-3) of 10 other nationwide polls implies a range of 764,036 to 3,609,682 defensive uses of guns per year. Recent evidence confirms other numbers from Kleck and Gertz's (1995) study. For example, Annest et. al. (1995) 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 also Southwick (1995) for a discussion on the defensive uses of guns.)

5 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

unlawful.[6] 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 (Cramer and Kopel, 1995, p. 691).[7] 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 (pp. 691-2).

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 (e.g., Kopel, 1992, p. 155 and Lott, 1994). Almost half the burglaries in Canada and Britain, which have tough gun control laws, are "hot burglaries." By contrast, the U.S., with laxer restrictions, has a "hot burglary" rate of only 13 percent. Consistent with this, surveys of convicted felons in America reveals 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."[8]

The case for concealed handgun use is similar. The use of concealled 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.

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death" (Potok, March 22, 1996, p. 3A). For a more recent evaluation of the Texas experience see Fort Worth Star-Telegram (July 16, 1996). By the end of June 1996, more than 82,000 permits had been issued in Texas.

6 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 (Lott, 1994).

7 Similarly, Multnomah County, Oregon issued 11,140 permits over the period January 1990 to October 1994 and experienced 5 permit holders being involved in shootings, 3 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 assult rifle was being unloaded (Barnhart, 1994).

8 Wright and Rossi (1986, p. 151) interviewed felony prisoners in ten 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.

Examples of stories where people successfully defend themselves from burglaries with guns are quite common (e.g., see "Burglar Puts 92-year-old in the Gun Closet and is Shot," New York Times, September 7, 1995, p. A16). Will (1993) discusses more generally the benefits produced from an armed citizenry.

In his paper on airplane hijacking, Landes (1978, p. 1) references a quote by Archie 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 passangers, then no hi-jacker would risk pullin' a rod."

Stories of individuals using guns to defend themselves has helped motivate thirty-one states to adopt laws requiring authorities to issue, without discretion, concealed-weapons permits to qualified applicants.[9] This constitutes a dramatic increase from the nine states that allowed concealed weapons in 1986.[10] While many studies examine the effects of gun control (see Kleck, 1995 for a survey), and a smaller number of papers specifically address the right-to-carry concealed firearms (e.g., Cook, et al., 1995; Cramer and Kopel, 1995; McDowall, et. al., 1995; and Kleck and Patterson, 1993), 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 (e.g., the probability of arrest and conviction and the length of prison sentences or even variables like personal income).[11] 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 (Cramer and Kopel, 1995, pp. 680-707), will not alter the number of permits being issued in all counties.

Other papers suffer from additional weaknesses. The paper by McDowall, et. al. (1995), 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 neither use the same sample period nor the same method of picking geographical areas for each of those cities.[12]

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

10 These states were Alabama, Connecticut, Indiana, Maine, New Hampshire, North Dakota, South Dakota, Vermont, and Washington. Fourteen other states provide 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.

11 All 22 gun control papers studied by Kleck (1995) use either cross-sectional state or city data or use time-series data for the entire US or a particular city.

12 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 Polsby (1995).

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 impact of "shall issue" right-to-carry firearm 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 (Levitt, 1996) —, 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.[13] 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 Impact of "Shall Issue" Concealed Handgun Provisions on Crime

Starting with Becker (1968), many economists have found evidence broadly consistent with the deterrent effect of punishment (e.g., Ehrlich (1973), Block and Heineke (1975), Landes (1978), Lott (1987), Andreoni (1995), Reynolds (1995), and Levitt (1996)). The notion is that the 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 (Lott, 1992b). Likewise, conviction also imposes many different penalties (e.g., lost licenses, lost voting rights, further reductions in earnings, etc.) even if the criminal is never sentenced to prison (Lott, 1990b, 1992a and b).

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 3054 counties during the period from 1977 to 1992. Unfortunately, for the time period we

[13] Recent attempts to relate the crime rate to the prison population concern us (Levitt, 1996). 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).

study, the FBI's Uniform Crime Report only includes 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.[14] Two additional 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 (e.g., 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 the additional return to having armed citizens would appear to be small.[15] Likewise, larceny involves crimes of "stealth," but these range from pick pockets, where "shall issue" laws could be important, to coin machine theft.[16]

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 increasing 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,

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[14] Arson was excluded because of a large number of inconsistencies in the data and the small number of counties reporting this measure.

[15] Robbery includes street robbery, commercial robbery, service station robbery, convenience robbery, residence robbery, and bank robbery.

[16] Larceny includes pick pockets, purse snatching, shoplifting, bike theft, theft from buildings, theft from coin machines, and theft from motor vehicles.

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 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 (e.g., Linsky, et. al., 1988 and Cramer and Kopel, 1995). 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.[17] 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 impact 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 (e.g., Kleck and Patterson, 1993) will not

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[17] Among those who made this comment to us were: Bob Barnhardt, Manager of the Intelligence/Concealed Handgun Unite of Multinomah County, Oregon; Mike Woodward, with the Oregon Law Enforcemnt 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 (Lipton, 1995, p. A1) where it is noted that, "Analysts 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." The Cramer and Kopel (1994) piece also raises this point with respect to California.

solve this problem, because without time series data it is impossible to know what impact 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 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 upon 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 upon 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 could produce widely differring estimates of how increasing a state's average arrest

rate will deter crime. 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 risky to assume that states are homogenous units 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 couple 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 upon 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 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 (1975) 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. The same thing is possible with regard to crime. For example, allowing citizens to carry concealed firearms may encourage people to risk entering more 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 were rising, ordinary least squares estimates would underpredict the drop in crime. Likewise, if the rules were adopted when crimes 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 (1973, pp. 548-553), economists have also realized that potential biases exist from having the offense rate as both the endogenous variable and as the denominator in determining the arrest rate and because increasing crime rates may lower the arrest 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.

III. The Data

Between 1977 and 1992, 10 states (Florida (1987), Georgia (1989), Idaho (1990), Maine (1985), Mississippi (1990), Montana (1991), Oregon (1990), Pennsylvania (1989), Virginia (1988), 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. Nine other states (Alabama, Connecticut, Indiana, Maine, New Hampshire, North Dakota, South Dakota, Vermont, and

Washington) effectively had these laws on the books prior to the period being studied.[18] 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 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.

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 department of corrections, State Attorney Generals, State Secretary 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 was only available for Arizona (1994 to 1996), Oregon (1990 to 1992) and Pennsylvania (1986 to 1992). 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.)[19] For Oregon, data on the county level conviction rate and prison sentence length was 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 only applied 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

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[18] We rely on Cramer and Kopel (1994 and 1995) 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.

[19] 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.

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.

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 (percent 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). (See Table 2 for the list and summary statistics.) While a large literature discusses the likelihood of younger males engaging in crime (e.g., Wilson and Herrnstein, 1985, pp. 126-147), controlling for these other categories allows us to also attempt to measure the size of the groups considered most vulnerable (e.g., females in the case of rape).[20] Recent evidence by Glaeser and Sacerdote (1995) 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, 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.[21] Including unemployment insurance and income maintenance payments from the Commerce Department's Regional Economic Information System (REIS) data set were attempts to provide annual county level measures of unemployment and the distribution of income.

Finally, we recognize that other legal changes in penalties involving improper gun use might also have been changing simultaneously with changes in the permitting requirements for concealed handguns.

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[20] 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 upon how responsive victims are to these threats, it is possible that the coefficient for a variable like the percent of young males in the population could be zero even when the group in question poses a large criminal threat.

[21] For a discussion of the relationship between income and crime see Lott (1990a)

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.[22] Another survey which addresses the somewhat boarder question of sentencing enhancement laws for felonies committed with deadly weapons (firearms, explosives, and knives) from 1970-1992 also confirms this general finding with all but four of the legal changes clustered from 1970 to 1981 (Marvell and Moody, 1995, pp. 258-261). 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.[23]

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[22] A more detailed survey of the state laws is available from the authors, a brief survey of the laws excluding the permitting changes finds: 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 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 198 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, two 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 period. Maine: No significant changes in these laws during 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 period. North Dakota: No significant changes in these laws during period. Oregon: No significant changes in these laws during period. Pennsylvania: No significant changes in these laws during 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 period. Washington: No significant changes in these laws during 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 guns laws might effect our results does not appreciably alter our results.

[23] Using Marvell and Moody's findings show that the closest time period between these sentencing enhancements and changes in concealed weapon laws is 7 years (Pennsylvania). 26 states passed their enhancement laws prior to the beginning of our sample period and only 4 states passed these types of laws after 1981. Maine which implemented its concealed handgun law in 1985 passed its sentencing enhancement laws in 1971.

IV. The Empirical Evidence

A. Using County Data for the United States

The first group of regressions reported in Table 3 attempt 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 the 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.[24] 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.[25] Rerunning these specifications with only the "shall issue" dummy, the arrest rates, and the fixed year and county effects produces even more significant effects for the "shall issue" dummy and the arrest rates.

The results are large empirically. When state concealed handgun laws went into effect in a county, murders fell by 8.5 percent, and rapes and aggravated assaults fell by 5 and 7 percent. 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 1,570. Given the concern that has been raised about increased accidental deaths from concealed weapons, it is interesting to note that the entire number of accidental gun deaths in the United States in 1992 was 1,409. Of this total, 546 accidental deaths were in states with concealed handgun laws and 863 were in those without these laws. The reduction in murders is as much as three times greater than the total number of

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[24] One possible concern with these initial results raises from our use of an aggregate public policy variable (state right-to-carry laws) on county level data (Greenwald, 1983 and Moulton, 1990). As Moulton (p. 334) 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 allow us to control for any disturbances that are correlated within any individual state. The regressions discussed in fn. 26 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 it 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.

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

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 states without "shall issue" laws would have declined by 4,177; aggravated assaults by 60,363; and robberies by 11,898.[26]

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.[27]

A recent National Institute of Justice study (Miller, Cohen, and Wiersema, 1996) provides estimates the costs of different types of crime based upon 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 $6.214 billion in 1992 dollars. The reduction in violent crimes represents a gain of $6.6 billion ($4.75 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

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[26] 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), Grossman et. al. (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 (e.g., 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 at least at the .01 level for a one-tailed t-test to all the other categories of crime. We would like to thank Michael Grossman for providing us with the original regressions on drug prices from his paper.

[27] By contrast, if the question had instead been what would the difference in crime rates have been between either have all states or no states adopting right-to-carry handgun, 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.

million ($342 million from auto theft, $73 million from larceny, and $1.5 million from burglary). However, while $6.2 billion is substantial, to put it into perspective, it equals only about 1.33 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 $1.84 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.[28] To the extent that people are taking greater risks towards crime because of any increased safety produced by concealed handgun laws (again see Peltzman (1975)), 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 one standard deviation change in the probability of arrest accounts for 3 to 17 percent of a one 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 (both 3 percent).

For property crimes, a one standard deviation change in the percent 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 explaining robbery, burglary, and auto theft rates, with a one standard deviation change in population density being able to explain 36 percent of a one standard deviation change in auto theft. Perhaps most surprising is

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[28] 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.

the relatively small, even if frequently significant, effect of income on crime rates. A one standard deviation change in real per capita income explains no more than 4 percent of a one 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 percent 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 percent of the population that is black. Given the high rates that blacks are arrested and incarcerated or are victims of crimes, this is not unexpected.

Rerunning the regressions by adding a dummy variable to control for state laws that increase sentencing penalties when deadly weapon are used (Marvell and Moody, 1995, pp. 259-260) has no noticeable effect on the concealed handgun coefficients. The enhanced sentencing law dummy is negative and statistically significant only for aggravated assaults, with the coefficient implying that adopting this type of law reduces aggravate assaults by 4 percent. Otherwise these laws generally appear to have little effect on crime rates.

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.[29] 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, 1,777 fewer murders and 7,000 fewer rapes would have taken place.[30] Overall, Table 4 implies

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[29] 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 percent of black males in the population between 10 and 19 is no longer statistically significant.

[30] By contrast, if the question had instead been what would the difference in crime rates have been between either have all states or no states adopting right-to-carry handgun, the case of all states adopting concealed handgun laws would have

that the estimated gain from the lower crime produced by handguns was $10.3 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.

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 (Ramusen, 1996). If so, any change in apprehension rates should produce a greater reputational impact 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 aggravate 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 bigger 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 crimes rates. In Washington and Oregon states in January 1996, women constituted 18.6 and 22.9

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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.

percent of those with concealed handgun permits for a total of 118,728 and 51,859 permits respectively.[31] 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 than the general population of women, the results are at least suggestive that rapists are particularly susceptable to this form of deterrence. Possibly this arises since providing a woman with a gun has a much bigger affect 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 women 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.[32]

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 cities two standard deviations above the mean population by 12

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[31] 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.

[32] Unpulished information obtained by Kleck and Gertz 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, stree adjacent to home, detached garage, etc.), but that women were less than half as likely to use a gun in self-defense away from home.

percent, 7.4 times more than a shall issue laws 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 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 affect 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 only statistically significant 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 impact 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. The annual decrease in violent crimes averaged about 2 percent, while the annual increase in property crimes average about 5 percent.

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 100,000 and then 200,000 people. Consistent with the evidence reported in Table 7, the more the sample was limited to 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 three years divided by the total number of offenses over the last three years. Despite the reduced sample size, the results remained similar to those already reported.

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 discussion on potential aggregation problems with state level data. County level data implies 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 towards 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 Endogeniety 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 (1973, pp. 548-551), 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 arrest); 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.[33] In an attempt to control for political influences, we also included the percent of a state's population that are members of the National Rifle Association and the percent of the vote received by the Republican presidential candidate at the state level. Because presidential candidates and issues vary between elections, the percent voting Republican is undoubtedly not directly comparable across years. To account for these difference across elections, we interacted the percent voting Republican with dummy variables for the years immediately next to the relevant elections. Thus, the percent of the vote obtained in 1980 is multiplied by a year dummy for the years from 1979 to 1982, the percent of the vote obtained in 1984 is multiplied by a year dummy for the years from 1983 to 1986, and so on through the 1992 election. A second set of regressions explaining the arrest rate also include the change in the natural log of the crime rates to proxy for the difficulty police forces face in adjusting to changing circumstances.[34] 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 Justices' Expenditure and Employment data for the Criminal Justice System from 1982 to 1992.

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[33] See also McCormick and Tollision (1985) for an novel article testing the endogeniety of the "arrest rate" in the context of basketball fowls.

[34] We would like to thank Phil Cook for suggesting this addition to us. In a sense, this is similar to Ehrlich's (1973, p. 557) specification except that the current crime rate is broken down into its lagged value and the change between the current and previous periods.

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, ordinary least squares estimates underpredict the drop in crime. Similarly, if these rules were adopted when crimes rates were falling, a 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 the National Rifle Association membership as a percent of a state's population; the Republican presidential candidate's percent of the statewide vote; the percentage a state's population that is black and the percent 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 10 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 study, five of the estimates imply that a one 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 explains 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 10), 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 two-stage least squares 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.

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 11 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 realtively less attractive.

There is also the question of what effect does conceal 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 percent of victims by sex and race as well as information on the whether the victim 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).[35] Table 12 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 victims know their nonfamily offenders 2.6 percentage points more frequently and that the percent 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 they carry concealed handguns.

The arrest rate for murder variable produces more interesting results. The percent of white victims and the percent of victims killed by family members both declined when states passed concealed handgun laws, while the percent of black victims and the percent that 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 a 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. 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

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[35] While county level data were provided in the Supplementary Homicide Report, matching these county observations with those used in the Uniform Crime Report 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.

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 14 for Pennsylvania and Table 15 for Oregon provide a couple of consistent patterns. The most economically and statistically important relationship involves the arrest rate: higher arrest rates consistently 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 20 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, rape is the one crime where a one 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 handguns usage explains more than 10 percent of a standard deviation change in murder, rape, aggravated assualt, 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 significantly lowers crime rates, while property crimes exhibit the opposite tendency. However, concealed handgun use only explains about half the variation for property crimes that it explains for violent ones.[36] The regressions for Oregon weakly imply a similar relationship

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[36] Running the regressions for all Pennsylvania counties (and not just those over 200,000 population) produced similar coefficients 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 occuring in the larger counties.

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 one standard deviation change in conviction rates explains 4 to 20 percent of a one 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.[37] The biggest differences between the deterrence effects of arrest and conviction rates produce an interesting pattern. For rape, increasing the arrest rate by one percentage point produces more than ten times the deterrent effect of increasing the conviction rate conditional on arrest by one percent. The reverse is true for auto theft where a one percentage point increase in reduces crime by about ten times more than the same increase in convictions. These results are consistent with arrests producing large shaming or reputational penalties (e.g., see Kahan 1996). In fact, the existing evidence shows that the reputational penalties from arrest and conviction can dwarf the other legally imposed penalties (Lott, 1992a and b). However, while the literature has not separated out whether these drops are occurring due to arrest or conviction, these results are consistent with the reputational penalties for arrests alone being significant for at least some crimes.

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 upon 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 16 produces no significant relationship between the change in concealed handgun permits and the various measures of crime rates.

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

In fact, the coefficient signs themselves indicate no consistent pattern with the fourteen coefficients being equally divided between negative and positive signs, though six of the specfications imply that a one standard deviation change in the concealed handgun permits explains at least 8 percent of a one standard deviation change in the corresponding crime rates. The results involving either the mean 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.

Overall, the Pennsylvania results provide more evidence that concealed handgun ownership reduces violent crime, murder, rape, aggravated assault, and burglary; and in the case of Oregon larceny decreases as well. While the Oregon data implies 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.

V. Accidental Deaths from Handguns

Even if “shall issue” hand gun 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. As Table 2 showed, while only a small portion of either 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 17 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 either a measure of the number of 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 implies that accidental handgun deaths rose by about .5 percent when concealed handgun laws were passed. With only 156 accidental handgun deaths occurring in counties over 100,000 population (27 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 (.645 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 17 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,[38] 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 1,561 to 1,767.

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[38] 182 million people lived in states without these laws in 1991 so the Tobit regressions would have also implied 9 more accidental handgun deaths in that year.

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 country had adopted right-to-carry concealed handgun provisions in 1992, at least 1,570 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 are minimal. The largest population counties where the deterrence effect on violent crimes is the greatest is also where the substitution effect into these property crimes is the highest. The estimated annual gain in 1992 from allowing concealed handguns was over $6.21 billion.

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 (Kahan, 1996 and Lott, 1992b), 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 deterrence effect of a one percentage point increase in arrest rates is much larger than the same increase in the probability of conviction. Also surprising was 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 (e.g., low crime rates) which lead to the adoption of these laws. Equally importantly, 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 its population.

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Data Appendix

The number of arrests and offenses for each crime in every county from 1977-1992 were provided by the Uniform Crime Report. 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% of the total population. The coverage amounted to 97% of the U.S. population living in Metropolitan Statistical Areas (MSAs) and 86% of the population in non-MSA cities and in rural counties.[39] 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 andthe offender.[40]

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 effect of concealed handgun laws on murder were greater than what is shown in this paper and the effects on rape and aggravated assult were smaller. Observations were eliminated because of changes in reporting practices or definitions of crimes (see ***Crime in the United States*** (1977 to 1992)). 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 droped from the data set include: Florida (1988 to 1992); Georgia (1980); Kentucky (1988); Hawaii (1982); Iowa (1991); Oakland, Ca. (1991 to 1992). The counties with the following cities were also eliminated: aggravated assult for Steubenville, OH. (1977 to 1990); aggravated assult for Youngstown, OH (1977 to 1988); aggravated assult and burglary for Mobile, Al. (1977 to 1985); aggravated assult for Milwaukee, WI (1977 to 1985); Glendale, AZ (1977 to 1984); aggravated assult for Jackson, MS (1982 and 1983); aggravated assult for Aurora, CO (1982 and 1983); aggravated assult for Beaumont, TX (1982 and 1983); aggravated assult for Corpus Cristi, TX (1982 and 1983); rape for Macon, GA (1977 to 1981); robbery and larceny for Cleveland, OH (1977 to 1981); aggravated assult for Omaha, NE (1977 to 1981); Little

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39 *Crime in the United States 1994*.

40 The ICPSR number for this data set was 6387 and the principle investigator was James Alan Fox of Northeastern University College of Criminal Justice.

Rock, Ark. (1977 to 1979); burglary and larceny for Eau Claire, WI (1977 to 1978); Green Bay, WI. (1977); and Fort Worth, TX (1977). For all of the different crime rates, if the true rate equals zero, we added .1 before we took the natural log of those values. For the accident rates, if the true rate equals zero, we added .01 before we took the natural log of those values.[41]

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 to 1992 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 five 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.[42] 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.[43] 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 1990-1991 growth rate of each county's populations.

Data on income, unemployment, income maintenance and retirement were obtained by the Regional Economic Information System (REIS). Income maintenance includes Supplemental Security Insurance

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41 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.

42 For further descriptions of the procedures for calculating intercensus estimates of population see ICPSR (8384): "Intercensal Estimates of the Population of Counties by Age, Sex and Race" (United States): 1970-1980. US Department of Commerce, Bureau of the Census. Winter 1985. ICPSR, Ann Arbor, MI 48106. Also, see "Intercensal Estimates of the Population of Counties by Age, Sex and Race: 1970-1980 Tape Technical Documentation." US 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." US Bureau of the Census, Census of Population, 1980: "County Population by Age, Sex, Race and Spanish Origin" (Preliminary OMB-Consistent Modified Race).

43 US 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." US Bureau of the Census, Census of Population, 1980: "County Population by Age, Sex, Race and Spanish Origin" (Preliminary OMB-Consistent Modified Race), pp. 19-23.

(SSI), Aid to Families with Dependent Children (AFDC), 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 survivor and disability payments, federal civil employee retirement payments, 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.[44] The index uses the average consumer price index for July 1983 as the base period.

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 NRA 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 (1995, pp. 259-260). The first year where the dummy variable comes on is weighted by the portion of that first year that the law was in effect.

The Bureau of the Census provided data on the latitude, longitude and area in square kilometers 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.

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[44] Statistical Abstract of the United States, 114th Edition, Table No. 746, page 487.

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 Crime Rate	284.77	255.57
Murder Rate	6.12	8.18
Murder Rate for Guns (from 1982 to 1991)	3.9211	6.4756
Rape Rate	16.33	23.55
Aggravate Assault Rate	143.35	172.66
Robbery Rate	153.62	92.74
Property Crime Rate	1404.15	2120.28
Auto Theft Rate	162.02	219.74
Burglary Rate	527.70	760.22
Larceny Rate	819.08	1332.52
Arrest Rates Defined as the Number of Arrests Divided By the Number of Offenses[45]		
Arrest Rate for Violent Crimes	23.89	112.97
Arrest Rate for Murder	18.58	88.41
Arrest Rate for Rape	19.83	113.86
Arrest Rate for Robbery	21.97	104.40
Arrest Rate for Aggravated Assault	25.30	78.53
Arrest Rate for Property Crimes	7.907	44.49
Arrest Rate for Burglary	5.87	25.20
Arrest Rate for Larceny	11.11	71.73
Arrest Rate for Auto Theft	17.37	118.94
Truncating Arrest Rates to be no greater than one		
Arrest Rate for Violent Crimes	11.11	25.40
Arrest Rate for Murder	10.78	36.40
Arrest Rate for Rape	10.60	31.59
Arrest Rate for Robbery	8.06	32.67
Arrest Rate for Aggravated Assault	11.14	27.08
Arrest Rate for Property Crimes	5.115	11.99
Arrest Rate for Burglary	4.63	14.17
Arrest Rate for Larceny	5.91	12.97
Arrest Rate for Auto Theft	8.36	26.66

[45] 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 much more likely to occur for counties than for states.

Table 2: National Sample Means and Standard Deviations

Variable	Obs.	Mean	Standard Dev.
Gun Ownership Information:			
Shall Issue Dummy	50056	0.164704	0.368089
Arrests Rates are the ratio of arrests to offenses for a particular crime category:			
Arrest Rate for Index Crimes	45108	27.43394	126.7298
Arrest Rate for Violent Crimes	43479	71.30733	327.2456
Arrest Rate for Property Crimes	45978	24.02564	120.8654
Arrest Rate for Murder	26472	98.04648	109.7777
Arrest for Rape	33887	57.8318	132.8028
Arrest for Aggravated Assault	43472	71.36647	187.354
Arrest Rate for Robbery	34966	61.62276	189.5007
Arrest Rate for Burglary	45801	21.51446	47.28603
Arrest Rate for Larceny	45776	25.57141	263.706
Arrest Rate for Auto Theft	43616	44.8199	307.5356
Crime Rates are Defined per 100,000 People:			
Crime Rate for Index Crimes	46999	2984.99	3368.85
Crime Rate for Violent Crimes	47001	249.0774	388.7211
Crime Rate for Property Crimes	46999	2736.59	3178.41
Crime Rate for Murder	47001	5.651217	10.63025
Murder Rate for Guns (from 1982 to 1991 in counties over 100,000)	12759	3.9211	6.4756
Crime Rate for Rape	47001	18.7845	32.39292
Crime Rate for Robbery	47001	44.6861	149.2124
Crime Rate for Aggravated Assault	47001	180.0518	243.2615
Crime Rate for Burglary	47001	811.8642	1190.23
Crime Rate for Larceny	47000	1764.37	2036.03
Crime Rate for Auto Theft	47000	160.4165	284.5969
Causes of Accidental Deaths and Murders per 100,000 People:			
Rate of Accidental Deaths from Guns	23278	0.151278	1.216175
Rate of Accidental Deaths from Sources Other than Guns	23278	1.165152	4.342401
Rate of Total Accidental Deaths	23278	51.95058	32.13482
Rate of Murders Using Handgun	23278	0.444301	1.930975
Rate of Murders Using Other Guns	23278	3.477088	6.115275
Income Data (All $ Values in Real 1983 dollars):			
Real Per Capita Personal Income	50011	10554.21	2498.07
Real Per Capita Unemployment Insurance	50011	67.57505	53.10043
Real Per Capita Income Maintenance	50011	157.2265	97.61466
Real Per Capita Retirement Per Over 65	49998	12328.5	4397.49

Population Characteristics:

County Population	50023	75772.78	250350.4
County Population per Square Mile	50023	214.3291	1421.25
State Population	50056	6199949	5342068
State NRA membership per 100,000 State Population	50056	1098.11	516.0701
% of votes Republican in Pres. Election	50056	52.89235	8.410228
% of Pop. Black Male Between 10-19	50023	0.920866	1.556054
% of Pop. Black Female Between 10-19	50023	0.892649	1.545335
% of Pop. White Male Between 10-19	50023	7.262491	1.747557
% of Pop. White Female Between 10-19	50023	6.820146	1.673272
% of Pop. Other Male Between 10-19	50023	0.228785	0.769633
% of Pop. Other Female Between 10-19	50023	0.218348	0.742927
% of Pop. Black Male Between 20-29	50023	0.751636	1.214317
% of Pop. Black Female Between 20-29	50023	0.762416	1.2783
% of Pop. White Male Between 20-29	50023	6.792357	1.991303
% of Pop. White Female Between 20-29	50023	6.577894	1.796134
% of Pop. Other Male Between 20-29	50023	0.185308	0.557494
% of Pop. Other Female Between 20-29	50023	0.186327	0.559599
% of Pop. Black Male Between 30-39	50023	0.539637	0.879286
% of Pop. Black Female Between 30-39	50023	0.584164	0.986009
% of Pop. White Male Between 30-39	50023	6.397395	1.460204
% of Pop. White Female Between 30-39	50023	6.318641	1.422831
% of Pop. Other Male Between 30-39	50023	0.151869	0.456388
% of Pop. Other Female Between 30-39	50023	0.167945	0.454721
% of Pop. Black Male Between 40-49	50023	0.358191	0.571475
% of Pop. Black Female Between 40-49	50023	0.415372	0.690749
% of Pop. White Male Between 40-49	50023	4.932917	1.086635
% of Pop. White Female Between 40-49	50023	4.947299	1.038738
% of Pop. Other Male Between 40-49	50023	0.105475	0.302059
% of Pop. Other Female Between 4049	50023	0.115959	0.304423
% of Pop. Black Male Between 50-64	50023	0.43193	0.708241
% of Pop. Black Female Between 50-64	50023	0.54293	0.921819
% of Pop. White Male Between 50-64	50023	6.459038	1.410181
% of Pop. White Female Between 50-64	50023	6.911502	1.54784
% of Pop. Other Male Between 50-64	50023	0.101593	0.367467
% of Pop. Other Female Between 50-64	50023	0.11485	0.374837
% of Pop. Black Male Over 65	50023	0.384049	0.671189
% of Pop. Black Female O65	50023	0.552889	0.980266
% of Pop. White Male Over 65	50023	5.443062	2.082804
% of Pop. White Female Over 65	50023	7.490128	2.69476
% of Pop. Other Male Over 65	50023	0.065265	0.286597
% of Pop. Other Female Over 65	50023	0.077395	0.264319