RIGHT-TO-CARRY CONCEALED HANDGUNS AND VIOLENT CRIME: CRIME CONTROL THROUGH GUN DECONTROL?*

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Research Summary:

"Right-to-Carry" (RTC) concealed-handgun laws mandate that authorities issue concealed handgun permits to qualified applicants. The supposition by those supporting the laws is that allowing private citizens to carry concealed handguns in public can reduce violent crime by deterring prospective criminals afraid of encountering armed civilians. Critics of the laws argue that violent altercations are more likely to turn deadly when more people carry guns. Whether the laws cause violent crime to increase or to decrease has become an important public policy question, as most states have now adopted such legislation. The present study evaluates Florida's 1987 RTC law, which prior research suggests plays a key role in the RTC debate. Specifically, we use panel data for 58 Florida counties from 1980 to 2000 to examine the effects on violent crime from increases in the number of people with concealed-carry permits, rather than before-after dummy and time-trend variables used in prior research. We also address many of the methodological problems encountered in earlier RTC studies. We present numerous model specifications, and we find little evidence that increases in the number of citizens with concealed-handgun permits reduce or increase rates of violent crime.

Policy Implications:

The main policy implication of this research is that there appears to be little gained in the way of crime prevention by converting restrictive gun carrying laws to "shall-issue" laws, although the laws might still prove beneficial by (1) eliminating arbitrary decisions on gun permit

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applications, (2) encouraging gun safety, (3) making permit holders feel safer when out in public, (4) providing permit holders with a more effective means of self-defense, and (5) reducing the costs to police departments of enforcing laws prohibiting unlicensed gun carrying.

KEYWORDS: Violence, Gun Carrying, Gun Control, Deterrence, Self-Defense

Each year in America a large number of homicides, nonfatal injuries, and nonfatal violent crimes are committed with firearms. In 2000, firearms were involved in 66% of homicides, 26% of robberies, and 6% of aggravated assaults; and 8% of all violent crime victims faced attackers who were armed with firearms (Federal Bureau of Investigation, 2001:12–18; Rennison, 2001:8). An estimated 100,000 to 150,000 individuals are medically treated each year for nonfatal gunshot wounds (Annest et al., 1995; Kleck, 1997:5). Adding to America's gun violence problem is the enormous size of the civilian gun stock, with approximately 250 million firearms in private hands by year-end 1998, about 36% of them handguns (Kleck, 1997:94).

These facts have lead many scholars to conclude that America's high level of violent crime, or at least its high homicide rate, is largely due to the availability of firearms (e.g., Blumstein, 1995; Zimring and Hawkins, 1997), and that increases in gun control, especially laws targeting high-risk subsets of the population such as convicted criminals, can reduce violent crime, especially homicide (Cook and Ludwig, 2000; Loftin et al., 1991; McDowall et al., 1992). Review of studies assessing gun control effectiveness (Kleck and Kovandzic, 2001; see also Kleck, 1997), however, provides little support for the view that gun control laws reduce gun availability or violence rates. Of the 49 studies reviewed, only 7 found a significant beneficial impact of gun laws on violence rates and 12 found mixed support for the laws.

In the past two decades, many states have enacted a radically new policy to address violence problems by making it easier for citizens to carry concealed-handguns in public (Cramer and Kopel, 1995; Lott, 2000; Lott and Mustard, 1997). Commonly referred to as "shall-issue" or "right-to-carry" (hereafter, RTC laws) concealed firearms laws, they mandate that county authorities issue a permit to carry a concealed handgun to anyone who satisfies certain objective criteria, replacing laws that gave local authorities wide discretion to deny permits (Cramer and Kopel, 1995; Lott, 1998b, 2000; Vernick and Hepburn, 2002). By the end of 2001, more than half the states had adopted RTC laws (U.S. Bureau of Justice Statistics, 2001:94–95; Vernick and Hepburn, 2003).

One of the first states to make it easier for citizens to carry concealed handguns in public places was Florida. Prior to 1987, Florida had a county-level "may-issue" carry permit system that allowed county officials to deny carry permits if they believed the applicant lacked "good moral character" (Kleck, 1997:368). As of October 1, 1987, the state law was changed to a state-administered, nondiscretionary permit system with carry permits valid throughout the state. The law allows permit holders to carry concealed handguns anywhere in the state, except in "places of nuisance" such as courtrooms or schools. The applicant must be 21 years old, provide evidence of having satisfactorily completed a gun safety program, and cannot have a felony conviction, history of mental illness, or record of alcohol or drug abuse (Florida Department of State, 2003a). In practice, applicants are rarely denied permits. Of the 837,280 applications received from October 1, 1987 through February 28, 2003, only 3,914 or 0.47% were denied a permit due to a criminal history or incomplete application (Florida Department of State, 2003b).

THEORETICAL ISSUES

Why should increases in legally authorized gun carrying affect rates of crime or violence? And if increases in legally permitted carrying does have crime-reduction effects, how might criminals and citizens respond to such increases? The supposition that increases in the legal carrying of guns by prospective victims can deter criminal behavior is based on the "expected utility" principle of classic and neoclassic theory. Both theories posit that because criminals are rational, utility maximizing individuals, they will be less likely to engage in criminal behavior if the perceived costs of crime outweigh the perceived benefits gained from committing crime (Becker, 1968; Ehrlich, 1973). Similar, although less strict, assumptions can be found in contemporary versions of classic theory, such as the rational choice perspective put forth by Cornish and Clarke (1986). Cornish and Clarke (1986) posit that offenders' rationality is bounded: Their cognitive abilities are limited, they quickly make and revise choices, and they use incomplete or inaccurate information. Thus, increases in legally permitted carrying may lead to reductions in violence rates by raising the expected costs of committing some crimes, due to criminals anticipating greater risks of injury and lower rates of success (Lott, 1998a, 1998b, 2000; Lott and Mustard, 1997). These theoretical arguments apply mainly to violent crimes. Criminals might be deterred from even attempting robberies because they fear the victim is armed. Citizens might be more reluctant to start arguments that would otherwise result in assaults (and homicides) if they fear the arguments might provoke gun attacks by permit holders. Of course, one way that prospective criminals, especially those lacking guns, might respond to the increase in the expected costs of crime would be to displace their criminal activity to a state without an

RTC law (e.g., Bronars and Lott, 1998; Lott, 2000) or switch to other types of crime (e.g., larceny, auto theft) where the likelihood of encountering an armed victim is lower (Lott, 1998b, 2000; Lott and Mustard, 1997).

Opponents of RTC laws advance numerous arguments as to why allowing prospective victims to legally carry guns is unlikely to reduce violent crime. First, it is questionable whether many criminals are aware of the laws or can perceive changes in the amount of gun carrying by prospective victims. Second, even if one assumes, somewhat implausibly, that criminals have accurate information about the laws and their implementation, it is not clear that such knowledge would matter because permitted carrying for self-protection accounts for a relatively small share of all self-protection gun carrying, most of which is probably illegal (Kleck, 1997:193). Although results from the National Self-Defense Survey, which distinguished protection-related gun carrying outside the home from other "carrying," indicated that 8.8% of U.S. adults carried a gun away from home during the last year for self-protection (legally and illegally) (Kleck and Gertz, 1998), only about 1% of the population has a permit to carry a concealed-handgun (Bird, 2000). This suggests that at least 90% of U.S. adults carry guns for protection outside the home in a given year, but without a carry permit. Third, many permit holders probably carried guns illegally before the laws, such that increases in permitted carrying does not necessarily mean an increase at all in the total rate of actual protection carrying or in actual risks to criminals (Kleck, 1997:372; Lott, 1998b:33; Ludwig, 1998). Indeed, results from the 2001 National Gun Policy Survey indicated that among adult gun carriers with permits, 73% reported no change in their level of gun carrying after they obtained a permit (Smith, 2001:15).[1] Finally, permit holders tend to reside in areas where contact with criminals is especially unlikely, such that increases in self-protection carrying by permit holders is unlikely to have any discernible impact on

1. To our knowledge, there is only one study that attempts to estimate the impact that RTC laws have on gun ownership, but it is severely flawed. Duggan (2001) uses subscriptions to *Guns & Ammo* magazine as a proxy for gun ownership, and he uses state panel data to estimate a fixed-effects model with this variable as the dependent variable and an RTC law dummy variable as the independent variable. He concludes that RTC laws have not lead to significant increases in gun ownership because the coefficient for the law dummy variable was small and statistically insignificant. Recent research by Kleck (2003), however, concludes that most of the gun proxies used in cross-sectional research and all of those used in longitudinal studies, including subscriptions to *Guns & Ammo* magazine, have poor validity. For example, the correlation between the change in the rate of subscriptions to *Guns & Ammo* and the change in the General Social Survey measure of gun prevalence at the national level between 1972 and 1999 was small and nonsignificant ($r = 0.14$). Thus, the finding by Duggan (2001) of a nonsignificant relationship between RTC laws and gun ownership is inconclusive due to the simple fact that gun levels were not actually measured. Duggan also used the wrong years for many of the RTC laws (see Ayres and Donohue, 2003).

criminals' perception of risk of encountering an armed victim (Hood and Neeley, 2000).[2]

Critics of RTC laws further maintain that the laws might increase violence, especially homicide. The most common explanation is based on the Zimring-Cook hypothesis: if more people carry guns, more interpersonal conflicts (e.g., bar fights) will result in death or major injuries because gun shot injuries are more likely to result in death than those inflicted by other weapons (Kleck and McElrath, 1991; Newton and Zimring, 1969).[3] Others speculate that RTC laws might stimulate more criminals to use guns, countering the threat from armed victims (Ayres and Donohue, 2003; Cook, 1991; Green, 1987; Ludwig, 1998; McDowall et al., 1995; but see Kleck, 1997:204–205). Because robbers are more likely to complete their crimes if they use guns, this additional gun carrying by criminals could result in more robberies, especially opportunistic ones, with greater likelihood of injury (Kleck, 1997:385). Ayres and Donohue (2003:10) add that more carrying might prompt criminals to shoot quicker, again resulting in more injury. Finally, some criminals, without felony records, might find it useful to obtain concealed-handgun permits (Ayres and Donohue, 2003:10).

PREVIOUS RESEARCH

There have been at least two dozen empirical evaluations of RTC laws, with most studies using a multiple time-series research design with state and county panel data.[4] The studies reach a wide range of conclusions concerning the impact on violent crimes, but most conclude that the laws

2. Hood and Neely (2000) argue that research estimating the impact of RTC laws using county-level data suffers from aggregation bias because there is important variation in permit and crime rates within counties. To illustrate this, they point to a strong negative association between permit rates and violent crime rates in Dallas. Permit holders are mostly whites males living in affluent, low crime neighborhoods. This association leads the authors to conclude that RTC laws are unlikely to influence violent crime rates. However, it is weak evidence because (1) as the authors admit, people living in low crime neighborhoods are likely to get permits because they work in, or otherwise travel in, high crime neighborhoods, and (2) the authors compute permit rates and violent crime rates based on resident population; this is a poor measure because it ignores the large numbers of persons traveling into areas where they do not reside, thereby inflating crime rates for commercial neighborhoods.

3. Analysis of revocation data in Florida provides little support for the Zimring-Cook hypothesis; i.e., gun violence among permit holders is nearly nonexistent. Of the 829,334 permits issued as of February 28, 2003, only 1,584 were revoked for conviction of a crime committed after licensure, 156 of whom were convicted of perpetrating crimes with firearms. This represents an average of 10 gun crime convictions per year (Florida Department of State, 2003b).

4. A summary of the studies can be found on the Internet at http://mmarvell.com/data.html. The summary includes the type of research design employed by the authors,

reduce homicide, rape, and assault (although not robbery). Most also find that the laws increase property crime. John Lott and his colleagues typically find the largest reductions in violent crimes (Bronars and Lott, 1998; Lott 1998a, 1998b, 2000; Lott and Mustard, 1997; Plassmann and Whitley, 2003; Plassmann and Tideman, 2001). Several others find modest reductions (Duggan, 2001; Harrison et al., 2000; Marvell, 1999; Moody, 2001; Olson and Maltz, 2001). Still others find at most slight reductions (Bartley and Cohen, 1998; Black and Nagin, 1998; Dezhbakhsh and Rubin, 1998; Marvell, 2001). Finally, a few suggest no impacts or even increases in violent crime (Ayres and Donohue, 1999, 2003a; Donohue, 2003; Kleck and Kovandzic, 2001; Ludwig, 1998; McDowall et al., 1995).

In the earliest study, McDowall and his colleagues (1995) studied gun and non-gun homicides separately using five county-level time series with monthly homicide data for 1973–1992. Three counties were in Florida (Dade, Duval, and Hillsborough). The RTC laws were entered as binary dummy variables scored 1 starting the year after a law went into effect, and 0 otherwise. The law dummy coefficients were usually positive and significant in the gun homicide regressions, but not the non-gun homicide regressions. The authors concluded that, at the least, there was no evidence that RTC laws reduce homicide. Critics (Kleck, 1997:370–372; Lott and Mustard, 1997; Polsby, 1995) argue that the authors did not control for potential confounding factors, they did not study total homicides (deterrence theory does not distinguish between gun and non-gun homicides in this situation), and they did not explain why they selected only 3 of Florida's 67 counties, despite the law applying statewide.

The most publicized study, Lott and Mustard (1997), evaluated RTC laws in ten states using fixed-effects models with both state and county panel annual data for 1977 to 1992 (extended to 1994 in Lott, 1998b, and to 2000 in Plassmann and Whitley, 2003). The RTC laws were entered as binary dummy variables scored 1 starting the year after a law went into effect, and 0 otherwise. Control variables included age structure, economic trends, and arrest rates. They conducted numerous alternative analyses, such as with differenced variables, with individual state trends, without arrest rates (which are missing for a number of counties and which induce simultaneity), and with laws represented by linear and nonlinear trends and permits issued in a single year. In general, they concluded that RTC laws deter violent crimes, except robbery, but increase property crimes (because, they claim, criminals substitute property for violent crimes).

The Lott and Mustard (1997) paper sparked considerable debate and

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unit of analysis, time period covered, measure used to assess the impact of RTC law on crime rates, regression procedures used, and the results for each crime type.

empirical investigation. The most recent substantial criticism is by Ayres and Donohue (2003a), who present numerous regression results, using both state- and county-level data. It is difficult to summarize their work because results differ greatly from specification to specification. They stress the importance of separate trend variables for each state; otherwise, when the RTC law is represented by a binary dummy variable, its coefficient reflects the extent to which general crime trends in the state differ from trends nationwide (which are captured by the year effects), and the coefficient is not an estimate of the law's impact. In what seems to be their most credible state-level models (see Table 5b especially), they find that RTC laws reduce rape and, in some cases, assault. Robbery and property crimes generally increase. With county data (through 1997), their analysis suggests that the laws increase assault, larceny, and auto theft, but have little impact on other crimes (Table 11). When entering state-specific law dummies, positive coefficients outnumber negative ones for all crimes. Results with post-law trends, rather than dummies, are roughly the same.[5]

Plassmann and Whitley (2003) present a wide-ranging rebuke to Ayres and Donohue and increase the county data set to the years 1977–2000. When using a single dummy variable, they find modest reductions in homicide, rape, and robbery, but again increases in property crimes. They emphasize a model in which separate dummy variables are used for each year before the law and each year afterward (out to eight years); these show irregular declines in the same three violent crimes, as well as steady increases in property crimes. Ayres and Donohue (2003b) argue that these results largely disappear when the RTC law variable is coded correctly and state trend variables are added.

This line of research, which includes many other studies, suffers from numerous problems, which the current research is designed to mitigate. The following paragraphs discuss what we consider to be the major problems and our attempts to deal with them.[6]

5. Ayres and Donohue (2003a) also use a "hybrid" model, and Lott uses a similar "spline" model. We believe that these specifications have little merit, as discussed below.

6. Other problems include the following: (1) Almost all researchers use incorrect dates for many of the state RTC laws (see, Marvell, 1999, 2001; Ayres and Donohue, 2003a). (2) The county-level crime data used by Lott and Mustard and others are unreliable in some states due to the incomplete crime reporting and inadequate attempts to impute missing data (see Lott and Whitley, 2002; Maltz and Targonski, 2002; Marvell, 1999). This problem is especially damaging for arrests, which Lott and Mustard use as a control variable. (3) Most county-level studies overstate the significance levels on the RTC law coefficients because of clustering (see Duggan, 2001; Harrison et al., 2000; Moody, 2001). (4) The impact of violent crimes tends to disappear when control variables are dropped, although there apparently is no evidence that the control variables bias the results (see Ayres and Donohue, 2003a; Duggan, 2001). (5) Lott and Mustard's

INADEQUATE MEASUREMENT OF RTC LAWS

Perhaps the most controversial issue in the RTC debate concerns how researchers operationalize the "treatment" effects of RTC laws. Numerous measures have been used to assess the impact of RTC laws on crime rates: binary (i.e., before-after) dummy variables, post-law linear trend variables, a "hybrid" approach that enters the two previous measures in the same regression, before-and-after trend variables, separate dummies for each year before and after the passage of the law, and the number of RTC permits issued each year. The most common measure is a binary dummy variable for all laws (scored 1 in years after the law), and the coefficient on that variable estimates the impact of the law on crime rates. This assumes that the impact starts at or close to the law's effective date and continues at a fairly regular level. The initial impact, presumably, is based on news accounts and publicity campaigns. If this pattern is a poor approximation of the actual causal process by which RTC laws deter or enhance crime rates, regression results will produce inaccurate estimates of the impact. Although it is often difficult to state, *a priori*, when the effect of a legal intervention should become evident, we question the assumption underlying the use of binary dummy variables that RTC laws have a once-and-for-all impact on violent crime. One might hypothesize, for example, that the "treatment" effect of RTC laws on violence rates is greatest when the laws are first publicly proposed, introduced into the legislature, enacted by the legislature, or have reached their peak level of publicity. One could also argue that criminals only get information about the laws after word spreads among the criminal population, such that the full effects of the laws might not become evident for several years (Kleck, 1997:353). In any event, even if researchers using the dummy variable approach are correct that announcement effects surrounding the passage of RTC laws are enough to cause prospective criminals to desist from committing crime, it is unlikely that these effects would remain fixed over time. Instead, one might expect an initial drop in crime due to announcement effects to be followed by a subsequent return to normal crime levels, as publicity and memories of the new laws fade from the minds of criminals (Ayres and Donohue, 2003a:16).

Although it is facially plausible that RTC laws have a fixed impact on crime through announcement effects, we agree with Lott and Mustard (1997) and others (e.g., Ayres and Donohue, 2003a; Black and Nagin,

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results defy common sense in that robbery is not reduced, whereas other violent crimes are, and most property crimes are increased (see Ayres and Donohue, 2003a; Black and Nagin, 1998). Lott and Mustard (1997) argue that criminals might substitute property crimes for violent crimes. (6) The post-intervention periods are too short for proper estimates of the impact of later laws (Ayres and Donohue, 2003a).

1998; Lott, 1998a, 1998b, 2000), that a more plausible theoretical mechanism by which RTC laws might work to reduce or enhance violence rates is by increasing the number of citizens obtaining and carrying concealed-handguns in public. Studies that attempt to link the deterrent or criminogenic effects of RTC laws to increases in legally permitted carrying by prospective victims using dummy variables do not effectively determine how a criminals' perception of risk from armed victims is affected by increases in legally permitted gun carrying, especially because it takes many years before the number of permits reaches its long-run level (Lott, 1998b:225). Rather, this theoretical model assumes that the exact "dose" experienced by states adopting RTC laws will grow gradually over time as the number of persons with permits reaches levels high enough to produce sharp changes in criminals' awareness and experiences with those legally carrying guns.

In order to examine the possibility that the effects of RTC laws grows over time, Lott and Mustard (1997) and others (e.g., Ayres and Donohue, 2003a; Black and Nagin, 1998; Lott, 1998a, 1998b, 2000) have modeled the effects of RTC laws using time trend variables. These, however, necessarily assume that permit rate growth in each geographical area is constant and identical. Such an assumption is probably unwarranted, however, because permit rates vary between counties and over time. Also, pre-law permit rates differ between counties. As a general rule, local authorities issue permits less often in urban areas than in rural areas (Kleck, 1997:368; Lott and Mustard, 1997:8). In Hillsborough county (Tampa), for example, a county of more than 800,000 people, only 25 permits were in effect before Florida enacted its RTC law (St. Petersburg Times, 1988:1A).

Recent research has used more elaborate procedures to model the impact of laws. Ayres and Donohue (2003a) emphasize a "hybrid" model, using both a binary dummy variable and a post-law trend variable in the same regression. This model is flawed because collinearity between the two variables, together with influence problems, creates artificially large coefficients in opposite directions. The typical result is highly significant positive coefficients on the dummy variable and negative coefficients on the post-law trends. Likewise, Plassmann and Whitley (2003) use a "spline" model, in which there is a trend variable before the law and another after. They then measure the impact of the law by comparing the trends. This is not helpful, because the impact of the law involves what happened afterward only, and an upward trend before the law, more than anything else, is evidence of a regression to the mean (which would suggest that any apparent crime-reduction impact of the law is an artifact of legislative timing). Plassmann and Whitley (2003) also enter separate dummies for each year before and after laws, as discussed above, but these encounter extreme collinearity problems because the dummies have different values in a small number of observations (Lott and his colleagues do not give *t*-ratios for these variables, in contrast to other results).

The obvious way to address these measurement problems is to operationalize the effects of RTC laws using data on the number of permits issued, a procedure advocated by Lott and Mustard (Lott, 1998b, 2000; Lott and Mustard, 1997). This provides for a more direct measure of the expected costs criminals face when attacking people, and it allows one to calculate the benefit of issuing permits (Lott, 1998:103). Lott and Mustard (1997) were able to obtain county-level data for the number of permits issued in Arizona, Oregon, and Pennsylvania. They found no evidence that permits reduce crime in Arizona, but some partial evidence that they reduce violent crime in Oregon and Pennsylvania. There are negative coefficients on the permit rate variable for murder, rape, and assault in both states, as well as for robbery and larceny in Oregon. Only two of these coefficients, however, are significant at the 0.05 level (murder in Pennsylvania and larceny in Oregon). A possible explanation for the null findings is how the authors constructed the permit variable, using the number of permits issued each year (less the number of permits issued in the year before the law), rather than the number of concealed-handgun permits outstanding. If criminals are deterred by the perception that more people are carrying guns, however, the important information is the number of permits outstanding. (permits are valid for four, two, and five years in Arizona, Oregon and Pennsylvania, respectively).

For most researchers examining RTC laws, the presumed reason for any effects on crime is that they increase the actual number of prospective victims with concealed-handgun permits, which in turn affects criminals' perception of risk of encountering an armed victim. To our knowledge, no studies explicitly measure the effects of RTC laws using data on the number of persons with valid concealed-handgun permits. The present study, therefore, is the first to use data on the number of persons in each county with valid concealed-handgun permits.[7] Data on the number of concealed-handgun permits issued and renewed in each county for the years 1987 to 2000 were obtained from the Florida Department of State, Division of Licensing (FDL). The construction of the permit rate variable is described in detail below.

7. A partial exception is that Lott (1998b:107–108) was able to obtain state-level data on the number of persons with valid permits in Florida. He compared graphs of permits and murder rates from 1987, when the law was passed, to 1992. Permits rose steadily, and homicides dropped about 20%, mostly after 1990. This analysis is too crude to be useful.

SIMULTANEOUS RECIPROCAL CAUSATION

A second problem with past studies of RTC laws is that they either ignore or fail to adequately address potential simultaneity problems between the passage of RTC laws and crime/violence rates. Simultaneity is clearly possible because states might respond to growing violent crime problems by passing legislation making it easier for civilians to carry guns in public places for self-defense. In such a situation, the coefficients on the law variables would be biased, most likely in the positive direction, the opposite of any violence reduction impact due to the laws. Lott and Mustard (1997) and Lott (1998b, 2000) attempt to address potential simultaneity bias using 2SLS regression, but their crime models were almost certainly underidentified. In order for the authors to use 2SLS, they must find at least one instrumental variable, which is known to affect, and not be affected by, the passage of RTC laws and is known not to be affected by crime. Their instruments include (1) National Rifle Association membership per capita, (2) percent Republican vote in the most recent Presidential election, and (3) per-capita police expenditures. However, there is no evidence that these variables are related to the passage of RTC laws, and they are likely to be affected by crime rates. Unfortunately, Lott and Mustard do not present results of standard statistical tests, such as those presented in Bound et al. (1995), Hausman (1978), and Basmann (1960), which could shed light on the adequacy of their identification restrictions. In all, it is probably impossible to satisfy the requirements for 2SLS; so we use an alternative procedure, the Granger causality test, as discussed later.

ASSUMING THE IMPACT OF RTC LAWS IS THE SAME ACROSS ALL JURISDICTIONS

A third problem is that most studies make the unrealistic assumption that the effects of RTC laws on crime are the same in each state and county. As a general rule, one cannot assume that there is a uniform impact across ecological units in panel studies (Baltagi and Griffin, 1997; Pesaran and Smith, 1995). A reanalysis of Lott and Mustard's data by Black and Nagin (1998) revealed large variations in state-specific estimates of RTC laws impacts on crime rates. They broke out the law dummy variable into separate variables for each of the ten states that adopted RTC laws between 1977 and 1992 (as opposed to a single dummy variable pertaining to all adopting states), and the number of negative coefficients, and the number significant, only slightly outnumbered their positive counterparts. Florida's large negative coefficients stood out, and without Florida, the apparent impact of the laws when using a single dummy variable for all adopting states declined greatly. Ayres and Donohue (2003a) also found substantial heterogeneity across states when estimating state-specific impacts of 23 RTC laws on violence rates, again with Florida as an outlier with large estimated crime-reduction impacts.

In the present study, our main model specification uses an aggregated permit rate variable, which allows us to estimate a rough statewide average impact. However, we also explore the possibility that permit rates have differential effects across counties by creating separate permit rate variables for each county in the same manner that Ayres and Donohue (2003a) and Black and Nagin (1998) used separate law dummy variables for each RTC state.

SUMMARY

In this paper, we employ the procedures discussed above to mitigate the methodological shortcomings of previous evaluations of RTC laws. We examine the impact of Florida's 1987 RTC law using panel data for 58 counties from 1980 to 2000. Prior research suggests Florida plays a pivotal role in the RTC debate. McDowall and his colleagues (1995) found that the Florida law, if anything, is associated with more gun homicides, whereas Ayres and Donohue (2003a), Lott and Mustard (1997), Lott (1998b, 2000), and Ludwig (1998) found that it reduced homicides. More importantly, Black and Nagin (1998) and Marvell (1999) argue that the Lott and Mustard (1997) and Lott (1998b, 2000) results for homicide and rape, which by the Lott's calculations account for 80% of the social benefit of RTC laws, are driven by the inclusion of Florida in their sample. Essentially, the conclusions of Lott and Mustard (1997) and those replicating their findings stand or fall on whether Florida's law actually reduces violent crime.

Likewise, past research has not actually addressed the theoretically important issue, whether more gun carrying by perspective victims deters criminals from attempting crimes, because the law is measured by dummy or trend variables. Most permits are not issued until many years after the law goes into effect. We should emphasize, however, that our study cannot assess whether the laws have an immediate and fixed impact due to announcement effects.

The next two sections explain the regression procedures and the data used. The last two sections discuss the findings and policy implications.

DATA AND METHODS

The present study examines the impact of permit rate growth on crime rates using panel data for 58 Florida counties for the period 1980 to 2000.[8] The primary model specifications are conducted with the variables in their original levels. As seen later in rows 5 and 6 of Table 5, the results are essentially the same when we use first-differences.[9] All continuous variables (e.g., permit rates, crime rates) are divided by population and are expressed as natural logs to reduce the excessive influence of outliers, such that coefficients are elasticities, the percent change in the dependent variable expected from a 1% change in the independent variable (Greene, 1993). The Breusch-Pagan test indicated heteroscedasticity, which we corrected by weighting the regressions by a power of population (usually 0.8 or 0.9) as determined by the test (Greene, 1993). Autocorrelation is mitigated by including a one-year lag of the dependent variable (Hendry, 1995). The inclusion of a lagged dependent variable causes us to lose the first year in the time series, which means the analysis covers the time period 1981 to 2000.

To estimate the impact of permit rates on crime rates, we follow conventional strategies for panel data and estimate a fixed-effects model. The fixed-effects model requires adding a binary dummy variable for each county and year, except the first year and county to avoid perfect collinearity (Hsiao, 1986:41–58; Pindyck and Rubinfeld, 1998:252–253). The county dummies are an integral part of the fixed-effects approach because they allow us to control for the collective effect of stable, unobserved county-specific factors that do not trend upward or downward over the study period, which caused crime rates to differ from county to county. In addition to mitigating omitted variable bias, the county dummies control

8. Data for the remaining nine counties (Franklin, Gilchrist, Glades, Gulf, Hamilton, Holmes, Jefferson, Lafayette, and Suwanee counties) were dropped from the analysis because of severe crime reporting problems, and data for Okeechobee county after 1998 were dropped for the same reason. Reestimating the crime models using data for all counties does not substantively alter the findings reported in Table 3A (see row 2 of Table 5).

9. One reason for estimating the model specifications in first-differences is that we cannot rule out the possibility that some key variables have unit roots. The standard test is the IPS test (Baltagi and Kao, 2000; Im et al., 1997); it showed that homicide and rape are stationary, but the results are inconclusive for the other crimes and for the permit rate variable. This test assumes independence between counties, which might not be the case because missing variables might have similar effects in various counties, and a variable in one county might affect crime in others. Any lack of independence is more likely to occur with respect to nearby counties. We correlated the error terms for the various counties (a major task, with eight crimes and 58 counties), and found that the mean correlation is essentially zero, with nearly equal numbers of significant positive and negative correlations. Only 8.5% overall are significant, not much more than expected by chance. This suggests that any departures from the assumption of independence are minor. In this light, Phillips and Moon (1999) argue that panel data can be conducted in levels even if variables are nonstationary, assuming independence.

for measurement error in crime due to reporting differences at the county level. It is necessary to control for such time-stable omitted variables because if they are correlated with any of the time-varying factors such as permit rates, then the parameter estimates for those time-varying predictors will be biased. Entering county fixed effect means that the regression coefficients are based solely on within-county variation over time. The year dummies control for statewide events that could raise or lower crime rates in a given year across the entire state (Hsiao, 1986:41–58; Pindyck and Rubinfeld, 1998:252–253). Examples of events that may have affected crime rates statewide include the passage of laws requiring background checks and waiting periods for handgun purchases in 1991 and laws implemented in 1988, which substantially revised sentencing provisions for repeat offenders.

RIGHT-TO-CARRY CONCEALED HANDGUN PERMITS

As mentioned above, data on the number of concealed-handgun permits issued and renewed in each county from 1987 to 2000 were obtained from the Florida Department of State, Department of Licensing (hereafter FDL). The data are for the fiscal year ending June 30th. It is not difficult to calculate the number of outstanding permits for each county from the issuance and renewal figures. Because permits under the RTC law are valid for three years, we added permits granted and renewed in the current year to those issued and renewed in the two prior years. This estimates the number of outstanding concealed-handgun permits in the middle of the year.

We are able to test the accuracy of this estimation because the FDL compiles yearly data on the number of outstanding permits for the entire state. Table 1 shows the number of permits issued and renewed statewide for 1988 to 2000. Our estimated statewide figures, which are the sum of the countywide estimates, are only 2.2% higher in 2000 than the actual figure, a slight discrepancy.[10]

We do not have data on the number of persons with permits before the law. The number outstanding in each county just prior to the law are estimated from FDL county-level data on the number of people who converted their county-issued permits to statewide permits during the first two years of the law (October 1, 1987-October 1, 1989), and that number is assumed to approximate the levels in prior years. The pre-RTC permits, issued by counties, had to be renewed every two years. Some 2,660 persons with county-issued permits obtained statewide permits during the two years. Almost all were granted in 1988. These figures are probably close

10. Much of this is due to permit revocations, which totaled 2,239 by February 30, 2003 (Florida Department of State, 2003b).

TABLE 1. CONCEALED-HANDGUN PERMITS ISSUED, RENEWED, AND VALID IN FLORIDA, 1988 TO 2000

End of Fiscal Year (June 30th estimates)	Concealed-Handgun Permits Issued	Concealed-Handgun Permits Renewed	Actual Number of Valid Concealed-Handgun Permits	Estimated Number of Valid Concealed-Handgun Permits	Percent of Adult Florida Residents with Concealed-Handgun Permit
1988	33,414	0	N/A	33,414	0.37
1989	17,824	0	N/A	51,238	0.55
1990	14,294	7	N/A	65,539	0.69
1991	12,476	22,403	65,497	67,004	0.67
1992	14,173	12,247	74,259	75,600	0.75
1993	24,052	9,859	93,923	95,210	0.94
1994	45,040	28,643	132,150	134,014	1.30
1995	36,142	20,023	161,756	163,759	1.56
1996	33,754	28,607	N/A	192,209	1.83
1997	30,129	60,781	204,695	209,436	1.92
1998	28,561	41,970	221,446	223,802	2.04
1999	26,958	51,480	235,532	239,879	2.14
2000	30,837	73,883	248,049	253,556	2.11

NOTES: Data on the number of concealed-handgun permits issued and renewed each year from 1988 to 2000 were provided by the Florida Department of State (FDS). Figures on the number of valid permits statewide were taken from old FDS concealed weapons/firearms license statistical reports and are mid-year estimates (Florida Department of State 1991–1995, 1997–2000). The number of valid permits statewide in 1988, 1989, 1990, and 1996 were not provided in FDS statistical reports for these years. Florida permits were initially valid for three years and were later increased to five years after July 1999. See Data and Methods Section for a complete description of the procedures used to calculate the number of valid permits (column 5). The percentage of the adult population (ages 21 and older) with permits (column 6) was calculated using actual permit levels (column 4). We use our estimates (column 5) when these figures are not available. State population estimates for Florida were obtained from the U.S. Bureau of Census.

to the actual number of outstanding permits prior to the RTC law because individuals with county-issued permits who carry guns, or plan to, would have converted to state-issued permits to avoid not having a permit. Permits before the law numbered less than 10% of the number issued in the first year of the laws and only slightly more than 1% of the permits outstanding in 2000 (see Table 1). Therefore, our estimates for pre-law outstanding permits would have to be extremely inaccurate to affect our results. For all practical purposes, the number of pre-law permits is nearly zero, and as shown later in Table 5 the results remain virtually unchanged if the analysis only uses post-1988 data.

CRIME RATES

Crime is measured using the Uniform Crime Report (UCR) index offenses for the period 1980 to 2000. The FBI crime reports include seven categories of crime: murder, rape, aggravated assault, robbery, burglary, larceny, and auto theft. Crime data were obtained from the Florida Department of Law Enforcement on computer disk. Assault and rape data are suspect; police are increasingly more likely to record these crimes; and new laws encourage women to report domestic violence (Reiss and Roth, 1993). Year dummies absorb such changes to the extent that they are statewide.

Crime data for 1988 are very incomplete because many agencies were unable to comply with format changes made to the state's incident-based UCR reporting system that year (Florida Department of Law Enforcement, 1999). Rather than estimate crime data for 1988 by taking the average of 1987 and 1989, we decided to drop 1988 from the analysis.[11] This procedure has the drawback of shortening the time series by 1 year, but the 20 years remaining is easily sufficient for a panel study. In partial mitigation, we conducted a separate analysis using vital statistics counts of homicide victims derived from Centers of Disease Control and Prevention (CDC) Part III Mortality Detail Files, provided by Professor Gary Kleck (see Table 3B). The homicide counts allow us to study the effects of permit rate growth on homicide rates in all of Florida's 67 counties covering the time period 1980 to 1998.

Several low population counties reported zero homicides for some of the years during the study period. Specifically, there were 106 and 157 observations, respectively, where the number of UCR and CDC homicides reported was zero. Because the logarithm of zero is undefined, a one was added to all homicide counts before we computed the rates and took the natural logs of those values. As Osgood (2000) notes, however, the choice of adding a one is highly arbitrary and results can be highly sensitive to the constant chosen by the researcher. Consequently, we reran the homicide models in Tables 3A and 3B using Poisson-based regression methods standardized for the size of the population at risk, which are designed to handle the discrete and skewed nature of homicide rates for small population counties. As seen in Table 3C, the results for homicide are not qualitatively different when modeling the homicide rate as a Poisson distribution.

SPECIFIC CONTROL VARIABLES

In addition to the proxy variables for unknown factors, we include five specific control variables that theory and prior research suggest are potentially causally antecedent to both crime rates and permit rates. Failing to

11. We reran the specifications shown in Table 3A by estimating crime data for 1988 by taking the average of 1987 and 1989. The results, as reported later in Table 5, were very similar to each other.

control for factors that have opposite or same sign effects on both permit rates and crime rates could suppress (i.e., mask any negative impact of permit rates on crime) or lead to spurious or partially spurious results for the permit rate variable, respectively. Additional covariates would have been desirable, but few useful violence-related variables (e.g., poverty rates) are available at the county-level between decennial census years.

The first set of control variables we enter into the crime models are economic variables. The economic variables include unemployment and real per capita income (measured in 1992 dollars). A number of criminological theories, including strain/deprivation and social disorganization theory contend that economic distress has a positive impact on crime, and extant research provides support for the effects of economic deprivation on crime, especially homicide (see reviews in Chiricos, 1987; Land et al., 1990). The underlying theme in strain/deprivation theory is that individuals lacking legitimate or limited opportunities for economic gain may become frustrated by their inability to attain, through lawful means, the material goods that others around them possess. This frustration or strain, which is often accompanied by feelings of injustice, and resentment, could manifest itself in the form of expressive violence, as those in the lower class respond to the unfulfilled expectations of justice and equity or instrumental violence, as individuals attempt to acquire the material goods they have been unable to attain through legitimate means (Bernard, 1990; Messner and Rosenfeld, 2000). For social disorganization theory, adverse economic conditions impacts crime indirectly by weakening networks of informal social control, and diminishing a community's ability to regulate its members and to solve crime problems (Bursik, 1988; Sampson, 1986, Sampson and Groves, 1989). With respect to permit rates, recent research by Hood and Neeley (2000) indicates that permit holders predominantly reside in areas with higher per-capita incomes. Thus, if counties with lower per-capita incomes generally have higher levels of violent crime but lower permit rates, then failing to control for the economic well-being of counties might lead to a spurious negative relationship between the permit rate variable and crime. Unemployment data were provided by the Florida Department of Labor and Employment Security on computer disk. Personal income data were downloaded from the U.S. Bureau of Economic Analysis website (2002).

The second set of control variables we enter into the crime models are age structure variables. Age structure is important because it affects both crime and permits rates in the opposite direction; thus, without it, the results might lead to a spurious negative permit-crime relationship. The age structure variables include the percentage of the population aged 15 to 19, and 21 to 24 years. These age groups are consistently those with the highest arrest rates for crime (Federal Bureau of Investigation, 2001:227).

This suggests that crime rates should increase as the size of these age cohorts increases, although prior research generally finds limited impacts of age structure on crime rates (Cohen and Land, 1987; Marvell and Moody, 1991). Age structure is also an important determinant of concealed-handgun permits. An examination of the ages of permit holders in Florida indicates that the majority of permit holders are in their middle ages. For example, 40.4% of those currently licensed to carry a concealed-handgun are 51 and older, whereas less than a quarter (22.7%) are between the ages of 21 and 35 (Florida Department of State, 2003c). Thus, one might expect counties with larger numbers of older residents to contain greater numbers of concealed-handgun permits. Data on county population by age were obtained directly from the Florida Legislature, Office of Economic and Demographic Research.

Finally, we enter prison population in the crime models because research suggests that this variable is related to crime reduction at the national (e.g., Devine et al., 1988; Marvell and Moody, 1997) and state level (Levitt, 1996; Marvell and Moody, 1994). We use a county-level variable, the number of prisoners (counted at mid-year) who had been sentenced in the county (regardless of where the offender is imprisoned). Prison population data for each year were taken from Annual Reports prepared by the Florida Department of Corrections (Florida Department of Corrections, 1980–2001).

Table 2 lists each of the variables that have been included in the crime models. In addition, the means and standard deviations are also shown.[12] The crime models are estimated using weighted ordinary least squares. Specifically, we estimate the impact of concealed-handgun permit rates on crime rates with the following model:

$$y_{it} = \alpha YEAR_t + \phi_i D_i + \gamma(PERMIT_{it}) + \beta x_{it} + u_{it,}$$

where y_{it} is the natural log of crime per 100,000 people in county i in year t; $YEAR_t$ is a vector of year dummies; D_i is a vector of county dummies; x_{it} is a vector of demographic, economic, and deterrence/incapacitation controls; and u_{it} is an error term. The variable $PERMIT_{it}$ is the natural log of concealed-handgun permits per 100,000 population in county i in year t. Estimation was carried out in SAS, version 8.2. The data and the programs used here are available on the Internet at http://mmarvell.com/data.html.

12. The standard deviation reflects variation between counties, whereas only within-county variation is used in the analysis.

TABLE 2. DESCRIPTIVE STATISTICS

Variable	Mean	S.D.
CDC Homicides per 100k	9.240	7.793
UCR Homicides per 100K	7.882	6.578
Rapes per 100K	42.265	25.394
Robberies per 100K	140.531	137.788
Aggravated assaults per 100K	492.678	241.398
Burglaries per 100K	1401.240	664.807
Larcenies per 100K	2899.720	1414.300
Auto thefts per 100K	324.567	269.088
Concealed-handgun permits per 100K	508.223	553.558
Percent 15 to 19	6.948	1.514
Percent 20 to 24	6.707	2.383
Per-capita income	12319.370	3617.190
Percent unemployed	6.493	2.636
Prison population per 100K	348.604	198.244

RESULTS

ESTIMATING THE STATE-WIDE IMPACT OF PERMIT RATES ON CRIME RATES

Estimates of the effects of permit rates on crime rates are presented in Table 3A, using procedures described above. The major features are using an aggregated permit rate variable (as opposed to separate permit rate variables for each county), logarithmically transformed rates for all continuous variables, county dummies, and year dummies. Because the variables are logged, the coefficient on the permit rate variable is an elasticity, the percent change in the crime rate due to a 1% increase in the permit rate. Additional analyses explore the potential two-way relationship between permit rates and crime rates (spikes in crime rates might encourage citizens to obtain concealed-handgun permits) using the Granger causality test and potential differential effects of permit rates across counties by creating a separate permit variable for each county. We also examine the robustness of our results by varying the model specifications in Table 3A.

The results in Table 3A provide little evidence that permit rate growth reduces crime, and there is some evidence that it increases robbery and auto theft. We also found no evidence that permit rate growth reduces homicide when using homicide victimization data from the CDC (Table 3B) or when modeling UCR and CDC homicide rates as a Poisson distribution (Table 3C). The coefficients for the permit rate variable in all four homicide models are small and far from significant. Given the large number of degrees of freedom, any consistent impact of permit rate growth on homicide rates should produce a significant negative coefficient for the permit rate variable. Permit rates are positively associated with rates of robbery, although the magnitude of the effect appears to be small. Nevertheless, the finding that permit rate growth is associated, if anything, with increases in robbery rates decisively undercuts Lott and Mustard's thesis that criminals are deterred from attacking victims in public places because they fear confronting armed victims. The reason for the significant positive coefficient in the auto theft model is not entirely clear. It is probably not a "substitution effect," as described by Lott and Mustard (1997) and Lott (2000), in which criminals substitute violent crimes for crimes of stealth, because a substitution effect implies a reduction in a violent crime category, something that does not show up in our findings.

ADDRESSING POTENTIAL SIMULTANEITY BIAS

One possible threat to the results reported in Table 3A is simultaneity, which can happen if citizens respond to crime problems by obtaining and begin carrying concealed-handguns. This possibility is supported by individual-level survey evidence (not afflicted by simultaneity problems) that people buy guns in response to higher crime rates (summarized in Kleck, 1997:74–79). In such a situation, the coefficients on the permit rate variable would be biased, most likely in the positive direction, the opposite of any crime-reduction impact due to increases in lawful gun carrying. Thus, the positive, albeit weak, association between permit rate growth and robbery rates could simply reflect the immediate effect of higher robbery rates on citizens getting concealed-handgun permits. One would expect this positive effect to prevail for robbery more than for other crime types because robbery involves direct victim-offender contact and is usually committed in public locations where a permit would authorize carrying guns, and this fact is widely known in the general public. As discussed above, Lott and Mustard (1997) and Lott (1998b, 2000) addressed potential simultaneity bias using 2SLS, but provided no rationale for their selection of identifying restrictions and did not report any tests of the adequacy of their identification restrictions.

We explore the possibility of simultaneity bias using the Granger causality test (Granger, 1969; Pindyck and Rubinfeld, 1998:242–246; Wooldridge, 2000). The fundamental notion underlying the test is that if X causes Y, then lagged values of X will be significant in a regression of Y on its own lagged values and lagged values of X. Likewise, if Y causes X, then lagged values of Y should be significant in a similar regression of X on its own history and the history of Y. In the present study, X is crime and Y is permit rates. We use the model specifications in Table 3A, except with permit rates as the dependent variable, and with two lags of permit rates and crime rates. The Granger test has a drawback in that it cannot rule out the possibility of a purely contemporaneous (same-year) relationship

TABLE 3A. THE ESTIMATED IMPACT OF CONCEALED-HANDGUN PERMIT RATES ON CRIME RATES

	Dependent Variables: Natural Logs of the Crime Rate per 100,000 people													
	UCR Homicide		Rape		Robbery		Assault		Burglary		Larceny		Auto Theft	
Main Independent Variable	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*
Concealed-Handgun Permit Rate	−0.004	−0.22	−0.017	−1.38	**0.021**	1.97	0.005	0.60	*−0.011*	1.67	0.002	0.26	**0.031**	2.80
Control Variables														
Percent 15 to 19	0.175	0.53	*−0.006*	−0.02	0.232	1.21	−0.094	−0.64	−0.020	−0.16	**−0.232**	−2.08	0.089	0.46
Percent 20 to 24	−0.130	−0.54	*0.314*	1.77	*0.259*	1.83	−0.050	−0.45	**0.183**	1.97	***0.303***	3.61	**0.308**	2.16
Per-Capita Income	0.062	0.26	−0.176	−1.03	**0.287**	2.06	***−0.227***	−2.17	−0.006	−0.06	0.064	0.80	−0.110	−0.75
Percent Unemployed	**−0.167**	−1.99	**−0.121**	−2.03	−0.066	−1.35	***−0.103***	−2.81	**−0.079**	−2.46	−0.021	−0.76	**−.120**	−2.37
Prison Population	0.056	0.79	−0.061	−1.19	−0.016	−0.38	−0.023	−0.71	***−0.038***	−1.42	−0.032	−1.32	0.037	0.90
Crime Rate, Lagged One-Year	***0.091***	2.97	***0.436***	15.71	***0.470***	17.02	***0.591***	23.53	***0.611***	24.29	***0.581***	22.41	***0.561***	21.78
F Values for Variable groups														
County dummies	***8.20***		***3.90***		***5.86***		***3.72***		***3.53***		***3.93***		***4.72***	
Year dummies	***4.85***		***2.46***		***12.65***		***6.39***		***16.69***		***10.58***		***8.24***	
N	1,099		1,099		1,099		1,096		1,098		1,098		1,090	
D.F.	1,017		1,017		1,017		1,014		1,016		1,016		1,008	
Adjusted R^2	.72		.72		.95		.91		.92		.93		.93	

TABLE 3B. THE ESTIMATED IMPACT OF CONCEALED-HANDGUN PERMIT RATES ON HOMICIDE VICTIMIZATION RATES USING VITAL STATISTICS DATA FROM CDC (ONLY RESULTS FOR THE PERMIT RATE VARIABLE ARE SHOWN)

TABLE 3C. THE ESTIMATED IMPACT OF CONCEALED-HANDGUN PERMIT RATES ON HOMICIDE RATES USING POISSON REGRESSION PROCEDURES (ONLY RESULTS FOR THE PERMIT RATE VARIABLE ARE SHOWN)

	Table 3B: CDC Homicide		Table 3C: UCR Homicide		Table 3C: CDC Homicide	
Main Independent Variable	Coef.	*t*	Coef.	*z*	Coef.	*z*
Concealed-Handgun Permit Rate	−0.005	−0.31	−0.006	−0.18	0.104	1.04

NOTES: The time period covered in Table 3A and C for UCR crimes, excluding the one year lost because of the lagged dependent variable, is 1981 to 2000, with data missing for 1988. The time period covered in Table 3B and C for the homicide analyses using CDC homicide victimization data is 1980 to 1998. The regressions in Table 3A are weighted by a function of county population as determined by the Breusch-Pagan test. Continuous variables are divided by 100,000 persons and logged, except in Table 3C where the homicide variables are counts. Only the results for the permit rate variable are shown in Table 3B and C. We would be happy to provide full estimates for the homicide models in Table 3B and C with all variables on request. Although not shown, county and year dummies were included in all specifications. Coefficients that are significant at the 0.10 level are in italics. Coefficients that are significant at the 0.05 level are displayed in bold. Coefficients that are significant at the 0.01 level are both in italics and displayed in bold.

between permit and crime rates (Wooldridge, 2000:98). This problem can be addressed in the present case, following the analysis in Marvell and Moody (1996). If crime rates affect permit rates, the impact would not occur solely within a calendar year, and the absence of a lagged impact implies the absence of a same-year impact. There are two reasons why there must be a lagged impact. First, crime figures are for the calendar year, whereas permit numbers are for mid-year, such that approximately half the crime trend in a year could only contribute to permit numbers in the following year. Second, a crime rise cannot lead to more permits immediately. It probably takes several months, on average, for citizens to learn about crime trends, to take the required gun safety course, and to apply for permits. And the Florida Department of Licensing estimates that it takes 90 days to process permit applications. Therefore, if crime rates do affect permit rates, a substantial amount of the impact must be lagged (that is, crime rates in one year affecting permits rates in the next year), and simultaneity bias is unlikely if there is no lagged impact. The issue is complicated, however, by the fact that, although the absence of a significant positive coefficient on the lagged crime rate in the Granger test means we cannot argue that "reverse causation" exists, it does not rule it out. Therefore, to argue that more crime does not lead to more permits, the coefficient on lagged crime must be negative or, if positive, very small and far from significant. As seen in Table 4, that is true for all crimes except robbery and auto theft. The coefficients are positive and fairly large, adding up to 0.062 for robbery and 0.052 for auto theft. The coefficients are not significant, but the probabilities of 0.22 and 0.26 for the *F*-test suggest that the positive impact is still likely.[13] In sum, there is little evidence that individuals respond to increases in reported crime rates by acquiring concealed-handgun permits and, presumably, begin carrying guns in public for purposes of self-protection.

ESTIMATING COUNTY-SPECIFIC PERMIT RATE EFFECTS

As discussed above, by aggregating the permit rate variable, into a single variable, we were assuming that all counties would exhibit similar changes in crime rates in response to permit rate growth. To explore the

13. We also conducted the Granger using as dependent variables (1) the number of new permits issued and (2) the number of new permits plus the number of renewals. In both cases, the results did not differ substantially from those in Table 4, except that there is a hint that more larceny increases the number of permits issued (the coefficients are 0.078 and 0.002 on the two lags of larceny, with an *F* statistic of 0.29). An anonymous reviewer correctly noted that permit acquisition might be more heavily influenced by prior statewide crime trends than by prior crime trends in the acquirer's county. Even if this was true, however, it would not lead to simultaneity at the county level.

TABLE 4. GRANGER ANALYSIS OF THE IMPACT OF CRIME RATES ON CONCEALED-HANDGUN PERMIT RATES

Independent Variable	Coefficients on Crime Rates Lagged One and Two Years (Independent Variables)					
	One-year lag		Two-year lag		*F* Value	
	Coef.	*t*	Coef.	*t*	Value	Prob.
Homicide	–0.016	–0.76	0.013	0.61	0.44	0.64
Rape	–0.008	–0.35	0.008	0.35	0.09	0.91
Robbery	0.016	0.50	0.046	1.44	1.49	0.22
Assault	0.003	0.09	0.030	0.85	0.50	0.61
Burglary	0.015	0.35	0.007	0.15	0.13	0.88
Larceny	0.020	0.47	0.011	0.26	0.28	0.76
Auto Theft	0.048	1.41	0.004	0.13	1.36	0.26

NOTES: Permit rates are regressed on permit rates lagged one and two years, the crime rate lagged one and two years, and the control variables listed in Table 3A. Only the results for lags of crime are presented, and the *F* value is for the two lags. Coefficients that are significant at the 0.10 level are in italics. Coefficients that are significant at the 0.05 level are displayed in bold. Coefficients that are significant at the 0.01 level are both in italics and displayed in bold.

issue of coefficient heterogeneity, we created separate permit rate variables for each county, in the same way that others have created separate dummies for each state (Ayres and Donohue, 2003a; Black and Nagin, 1998). Unfortunately, the separate permit rate variables were highly colinear with the county and year dummies. In any event, analysis with the disaggregated permit rate variables produced coefficients on the county permit rate variables that are almost exclusively positive.[14]

ROBUSTNESS CHECKS

Any study is subject to the criticism that results might depend on which of various reasonable options are selected, primarily options regarding model specification and sample selection. We believe we selected the best options, but others may not be completely unjustified. As seen in Table 5, by and large, the results are similar to those reported above, with virtually no evidence that permit rate growth significantly reduces crime rates.

First, the analyses in Tables 3A-C and 4 do not control for omitted factors that make crime rates in one county grow faster or slower than statewide trends, which are captured by the year dummies. This is a special problem when using dummy variables to measure laws, because if crime is trending upward (or downward), the coefficient on the law dummy might

14. To conserve space, we do not report the estimates of this analysis. We will send the full results upon request.

simply reflect the overall trends (see Ayres and Donohue, 2003a; Marvell, 2001:700). Although there is little reason to expect this problem in the present research because we do not use a law dummy, we conduct a separate analysis with separate trend variables for each county (see row 1 of Table 5). The results are similar to those in Table 3A for violent crime, but there is strong suggestion that permit rate growth is associated with more property crime. In another attempt to control for trends, we conducted the regressions in Table 3A, adding two lags and two leads of the permit rate variable. With the partial exception of the motor vehicle theft regression, the coefficients are very small and far from significant.

Second, as discussed above, we deleted nine counties because of severe crime reporting problems over the study period, and dropped 1988 from the analysis because most counties were unable to comply with format changes made to the states incident-based UCR reporting system that year. We reran the model specifications in Table 3A using crime data for all counties (see row 2 of Table 5), and with estimated crime data for 1988 (see row 3 of Table 5). In both cases, the results are very similar to those in Table 3A.

Third, we had to estimate permit rate data prior to 1988 based on the number of people who converted their county-issued permits to statewide permits during the first two years of the laws implementation. When data prior to 1988 are dropped from the analysis (see row 4 of Table 5), the estimated impact of permit rates on violent crime rates are stronger, in a positive direction, than in Table 3A.

Fourth, the regressions in Tables 3A were reestimated using year-to-year changes on all explanatory variables (called differencing the variable) because the results of the stationarity tests were inconclusive for the permit rate variable and most crime types. Granger and Newbold (1974) demonstrated that two independent random walks, when regressed on each other, tend to yield significant regression coefficients and high R^2 values. The authors also show that if one detrends the variables by taking first-differences, the spurious regression disappears. As seen in rows 5 and 6 of Table 5, using differenced rates produces results similar to those obtained in levels, except that the coefficient on the permit rate for robbery is far from significant. This might indicate that the positive impact of permit rates on robbery rates suggested in Table 3A is spurious, due to the use of nonstationary variables.

Fifth, we estimated the regressions in Table 3A with variables converted to natural logs. When the variables are not logged, the estimated impact of permit rates on violent crime changes dramatically, especially for robbery (see row 7 in Table 5). The results imply increases in permit rates lead to significant drops in both homicide and robbery, although the coefficients here are not elasticities. However, when we added county linear trend dummies to the unlogged model (see row 8 in Table 5), which control for omitted factors that make crime grow faster or slower than statewide trends (which are captured by the year dummies), permit rates no longer have significant negative impacts on homicide and robbery. The most likely explanation for these findings is that several outlier observations for the permit rate variable, a problem mitigated when using natural logs, were correlated with omitted factors causing homicide and robbery rates to decline.

Sixth, we weighted the regressions in Tables 3A-B and 4 by a function of county population to correct for heteroscedasticity problems and added a one-year lag of the dependent variable to mitigate autocorrelation and omitted variable bias. The results of the unweighted regression, which are dominated by small counties, are roughly similar to those in Table 3A-B. Dropping the lagged dependent variable leads to significant negative coefficients in the rape and burglary regressions and positive coefficients for robbery and assault. These results are probably due to missing variable bias, because the lagged dependent variable is an important control for unmeasured factors that affect crime in a consistent manner across years (Moody, 2001:806).

Finally, we examined Lott and Mustard (1997) and Lott's (2000) claim that allowing citizens to carry concealed-handguns in public places would produce greater deterrent effects in the more populous counties that had been the most restrictive in allowing citizens to carry concealed-handguns. To test this, the model specifications shown in Table 3A were reestimated by breaking down the sample into two equal groups: (1) counties with above 100,000 population in 1990 and (2) counties with below 100,000 population in 1990. Each set of counties were reexamined separately. As seen in the bottom two rows of Table 5, permit rate growth appears to affect high- and low-population counties similarly. The coefficient signs are generally in the same direction for both, although permit rate growth has statistically significant effects only in the relatively high-population counties. In contrast, for robbery, the effect appears to be greatest in the low-population counties.

DISCUSSION AND CONCLUSIONS

Using the most direct measure of lawful gun carrying readily available, the number of citizens with valid concealed-handgun permits, and over a dozen plausible adjustments to the model specifications in Table 3A, we find no credible statistical evidence that permit rate growth (and presumably more lawful gun carrying) leads to substantial reductions in violent crime, especially homicide.

The fact that permit rate growth had no deterrent effect on violence

TABLE 5. ALTERNATE MODEL SPECIFICATIONS (ONLY COEFFICIENT FOR PERMIT RATE VARIABLE IS SHOWN)

	Homicide		Rape		Robbery		Assault		Burglary		Larceny		Auto Theft	
	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*	Coef.	*t*
(1) With County Trend Variables	–0.007	–0.22	0.018	0.75	**0.048**	2.44	0.023	1.57	**0.029**	2.30	**0.025**	2.16	***0.059***	2.83
(2) With all Counties	–0.003	–0.21	–0.018	–1.46	*0.020*	1.90	0.005	0.61	–0.010	–1.15	0.004	0.69	***0.039***	3.38
(3) With Estimated Crime for 1988	–0.005	–0.31	–0.016	–1.29	*0.019*	1.93	0.004	0.51	–0.010	–1.56	0.001	0.23	***0.028***	2.72
(4) Only Years 1989–2000	0.172	1.58	0.094	1.19	***0.218***	3.51	*0.085*	1.92	0.050	1.19	0.056	1.47	–0.006	0.09
(5) Regression in Differences	–0.015	–0.34	–0.034	–1.11	0.026	1.09	0.012	0.65	–0.001	–0.07	0.005	0.32	***0.072***	2.83
(6) Regression in Differences, with county trends)	–0.026	–0.53	–0.034	–1.06	0.022	0.85	0.009	0.44	0.004	0.25	0.006	0.41	***0.072***	2.69
(7) All Variables Unlogged	***–0.003***	–3.79	–0.002	–0.88	**–0.015**	–2.57	–0.006	–0.35	***–0.158***	–4.03	–0.067	–1.02	0.002	0.12
(8) All variables unlogged, with county trends	0.001	0.86	0.000	0.07	0.007	0.52	**0.079**	2.58	–0.102	1.32	0.145	0.97	0.001	0.03
(9) Regressions are Unweighted	–0.017	–0.80	–0.014	–0.72	***0.040***	2.72	–0.017	–1.23	**–0.029**	–2.50	0.013	1.16	*0.024*	1.71
(10) Without Lagged DV	0.003	0.17	**–0.030**	–2.12	***0.038***	3.28	**0.020**	2.08	***–0.030***	–3.56	0.006	0.85	***0.069***	5.33
(11) Large Urban Counties	0.000	0.01	–0.016	–1.05	*0.020*	1.66	0.004	0.38	–0.001	0.19	0.001	0.24	**0.025**	2.06
(12) Small Counties	–0.023	–0.61	–0.049	–1.35	*0.052*	1.91	–0.019	–0.82	–0.025	1.35	0.001	0.04	0.022	0.83

NOTES: Coefficients that are significant at the .10 level are in italics. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both in italics and displayed in bold. All model specifications are the same as those in Table 3A, except as noted.

Alternative Model Specifications:

(1) Includes county linear trend dummies, which control for county-specific trends.
(2) With all 67 counties, including those with incomplete data.
(3) 1988 crime is estimated, the average of 1987 and 1989 figures.
(4) Regression without pre-1989 data.
(5) Regression in first-differences.
(6) Regression in first-differences, with county linear trend dummies.
(7) Variables are not logged (coefficients are not elasticities).
(8) Variables are not logged, with county linear trend dummies.
(9) The regressions are not weighted (and thus encounter heteroscedasticity problems).
(10) Without the lagged dependent variable (therefore, with one additional year).
(11) Counties with over 100,000 population in 1990.
(12) Counties with less than 100,000 population in 1990.

rates is likely due to one of three reasons. First, few people wanted to obtain concealed-handgun permits. Despite millions of Floridians being eligible for permits, apparently only a handful of people were willing to go through the hassle of applying for one. By mid-year 2000, some 12 years after the law was in effect, there were only 248,049 valid concealed weapons permits in Florida, representing 2.1% of the Florida adult population. Given the small number of persons with concealed-handgun permits, coupled with the fact that most gun carrying by prospective victims in Florida is probably done illegally, the premise that increases in lawful gun carrying would even be perceptible to criminals seems implausible. Second, as discussed above, the law might have had little impact on rates of gun carrying among prospective victims—people already carrying merely legitimated what they were doing by obtaining concealed-handgun permits. Third, it may be that noncriminal gun carrying actually did increase, but the crime-increasing effects of a few violent people getting permits balanced out the crime-decreasing effects of many nonviolent people getting permits. Such an explanation implies that rates of lawfully gun carrying may have no net effect on crime rates for the same basic reasons that gun ownership levels in general have no net effect (summarized in Kleck, 1997; Kleck and Kovandzic, 2001), guns among criminals may increase violence whereas guns among noncriminals decrease it, with the two opposite-sign effects canceling each other out.

The policy implications of this research depend somewhat on how much weight one attaches to the robbery and auto theft results. Permit rate growth is associated with more of these crimes in Table 3A and in most of the alternative regressions in Table 4. There are three problems, however, that lead us to believe that these findings should not be taken at face value. First, there is no convincing rationale for them. The theory developed in the vast literature on RTC laws does not explain the findings (Lott and Mustard [1997] posit a substitution effect to explain positive coefficients on RTC variables in property crime regressions, but substitution is unlikely here because there is little evidence that the RTC laws reduce other crimes.) Second, because the robbery regression in first-differences does not suggest that permit rates increase robbery, there is a chance that the positive coefficient in Table 3A is due to nonstationarity. This problem, however, does not apply to auto thefts. Third and most important, the Granger analysis suggests that reverse causation is possible for robbery and auto theft, such that the positive coefficients on the permit rate variables might result from citizens applying for permits when these two crimes rise. They are the only two crimes with positive significant coefficients on the permit rate variable in Table 3A and the only two crimes with possible simultaneity problems in Table 4. This is only suggestive, however, and in the future, it would be helpful if researchers could locate valid instrumental variables, enabling one to model reciprocal relationships between permit rates and these two crimes.

Neither the present study nor recent research by Ayres and Donohue (2003a; 2003b) offers much support for the view that RTC laws have any net negative effect on the rate of any major category of violence. Nevertheless, policymakers might still support the enactment of RTC laws, even if they do not deter violent crimes, because they eliminate arbitrary decisions on gun permit applications and encourage gun safety training. They also provide permit holders with a very effective means of self-defense; the literature is nearly unaninimous in that when crime victims use guns, they are less likely to be injured or lose property (see Kleck 1997:147–190). Also, it is possible that permit holders enjoy the psychological benefits of carrying guns—even people who never actually use them for self-defense will enjoy greater feelings of security, and this applies to far more people than the first benefit. This benefit would occur only to the extent that there are people who will carry guns for self-protection if they can get a permit, but would not do so without a permit—the more such people there are, the more extensive these benefits would be.[15] It is important to note, however, that other citizens might feel less safe in pubic places if RTC lead to more people carrying guns in their communities (Smith, 2001). Finally, passage of RTC laws would increase the share of noncriminal gun carriers who have carry permits, thereby reducing the costs to police departments of enforcing laws prohibiting unlicensed carrying (most of which is done by otherwise law-abiding citizens), and the more enforcement aimed at unlicensed carriers can be concentrated on persons who represent a significant threat to public safety (see Kleck, 1997:211). In sum, we agree with Ludwig's (1998) admonition that there may be numerous reasons for state policymakers to support RTC laws, but the belief that these laws reduce crime should not be one of them.

15. Future research might consider empirically testing these other potential benefits of RTC laws. Surveys, for example, could ask people if they would be willing to carry, even if it was unlawful, to establish some rough ideas of how many people fit into the carry-only-if-permitted category. And researchers could examine police department records in cities in RTC states, before and after, to see if (1) the percent of incidents in which victims used guns defensively increased and (2) the percent of victims injured or losing property declined after the laws (information not available in the Uniform Crime Reports). This would test whether more victims were enabled to use guns defensively and whether they were doing so effectively. Interpreting results of such research, however, would have to be tempered by the cautions raised by Kleck (1997, Chapter 5), that incidents reported to police are likely to overrepresent failures of victim self-defense efforts. The absolute level of victim success would not be the focus; rather, changes in the level of success over time would be of interest.

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