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IN THE UNITED STATES DISTRICT COURT

CENTRAL DISTRICT OF CALIFORNIA

WESTERN DIVISION

MICHELLE FLANAGAN, et al., **Plaintiffs,** **v.** **CALIFORNIA ATTORNEY GENERAL XAVIER BECERRA, in his official capacity as Attorney General of the State of California, et al.,** **Defendants.**	Case No.: 2:16-cv-06164-JAK-AS **DECLARATION OF JOHN J. DONOHUE III REGARDING DEFENDANT'S REPLY IN SUPPORT OF MOTION FOR SUMMARY JUDGMENT** Date: November 6, 2017 Time: 8:30 a.m. Courtroom: 10B Judge: Hon. John A. Kronstadt Action Filed: August 17, 2016

I, John J. Donohue III, declare as follows:

1. I have personal knowledge of the following facts, except where my knowledge is based on information and belief, as indicated. If called as a witness at a relevant proceeding, I could and would testify competently to the following facts.

2. I am a testifying expert witness for the defense in the lawsuit entitled Flanagan v. Becerra, U.S. District Court, Central District of California, Case No. 2:16-cv-6164-JAK-AS. I am the C. Wendell and Edith M. Carlsmith Professor of Law at Stanford Law School, in Stanford, California. I have been a law professor for more than 30 years. I have a law degree from Harvard University (1977), and a Ph.D. in economics from Yale University (1986). My scholarship focuses on performing empirical analysis to determine the impact of law and public policy in a wide range of areas, including but not limited to the impact of so-called "right-to-carry" firearms laws on rates of violent crime.

3. I submitted an expert report, and was deposed, in the Flanagan matter. I also have been informed of the nature of some of the deposition testimony given last month by Prof. Gary Kleck, a rebuttal testifying expert witness for the plaintiffs in the Flanagan matter. I understand that Prof. Kleck cited a 2005 research paper titled "The Impact of 'Shall-Issue' Concealed Handgun Laws on Violent Crime Rates," by Prof. Tomislav Kovandzic and two other scholars (the "Kovandzic paper"), as the best study of the question of the relationship between right-to-carry laws and rates of violent crime. I have read and am familiar with the Kovandzic paper. Attached hereto as Exhibit A is a true and correct copy of the Kovandzic paper.

4. I have determined that the Kovandzic paper contains a math error in the way that the authors interpret their findings concerning the impact of right-to-carry (RTC) laws on aggravated assault, which has a significant effect on their reported results on this issue. I am submitting this declaration to point out the error and correct it, because I believe that the actual regression estimates in the Kovandzic paper corroborates my own research findings about the relationship between RTC laws and rates of violent crime, as contained in the 2017 research paper titled "Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Controls Analysis," which I co-authored with two other scholars, and in additional, follow-up work that I have done on that research paper since it was published as a working paper of the National Bureau of Economic Research, Inc. Attached hereto as Exhibit B is a true and correct copy of the research paper.

5. Kovandzic, Marvell, and Vieraitis (KMV) estimate the impact of RTC laws on violent crime using a panel data analysis on 189 cities with a population of 100,000 or more in 1990 and find that RTC laws lead to a highly statistically significant increase in the rate of aggravated assault

(using data from 1980-2000).[1] The relevant estimate is shown to be .019 in a trend model in which the dependent variable is the natural logarithm of the aggravated assault rate.[2]

6. To compute the exact effect of RTC laws on aggravated assault, one uses this formula, derived from the KMV paper:

1) $\boldsymbol{ln(crime\ rate) =}$ **.019 * Years Since Passage of the RTC Law**

To precisely estimate the impact of RTC laws on aggravated assault, one exponentiates (takes the anti-log of) both sides of the above equation, which generates the following equation:

2) $\boldsymbol{crime\ rate = Exp(}$ **.019 * Years Since Passage of the RTC Law)**

7. Equation 2 implies that one year after adoption of a state RTC law, the aggravated assault rate in a city in that state would be X times the aggravated assault rate had the state not adopted a RTC laws, where X is given by this formula:

3) $\boldsymbol{Exp(}$ **.019 * 1) =** $\boldsymbol{Exp(}$ **.019) = 1.019182**

Equation 2 indicates that after one year in effect, the impact of a RTC law would be to raise the rate of aggravated assault by somewhat more than 1.9 percent.

8. To see the estimated effect after ten years, one simply replaces the 1 in the first part of Equation 3 with a 10, generating the following increase in aggravated assault:

4) $\boldsymbol{Exp(}$ **.019 * 10) = Exp (.19) = 1.20925**

In other word, the KMV estimate is that the aggravated assault rate increase attributable to the adoption of a right to carry law is roughly 20.9 percent after 10 years.

9. The mistake that appears on page 309 in the KVM paper is in their sentence describing their estimated effect of RTC laws on aggravated assault, which states: "… the results for the aggregate SI law time-trend variable imply an average increase of 0.2% in aggravated assault for each additional year SI laws are in effect, for a net effect of 1% higher aggravated assault rates after 5 years." The error in this statement is that KVM have apparently misplaced a decimal point in their estimate, which should show that RTC laws lead to a roughly 2 percent increase per year in aggravated assault, and hence a roughly 10 percent increase five years after adoption of a RTC law and 20 percent 10 years after adoption. This finding exactly corresponds with the estimated tenth year effect I present in equation 4 above.

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[1] In addition to controlling for city and year fixed effects, KMV control for an array of other factors specified on p. 306 as: the percentage African American; percentage Hispanic; percentage ages 18 to 24 and 25 to 44; percentage households headed by females; percentage persons living below the poverty line, per-capita income; percentage population living alone, per-capita income; and percentage state prison population.
[2] KMV at 308, table 1, column 5 (titled "assault coefficient").

I declare under the penalty of perjury, under the laws of the United States of America, that the foregoing is true and correct, and that I signed this declaration on September 1, 2017 at Stanford, California.

John J. Donohue III

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John J. Donohue III

The Impact of "Shall-Issue" Concealed Handgun Laws on Violent Crime Rates

Evidence From Panel Data for Large Urban Cities

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What happens when states ease access to permits to carry concealed handguns in public places? Supporters maintain the laws can reduce violent crime rates by raising the expected costs of crime, because of criminals anticipating greater risks of injury and lower rates of success completing their crimes. Opponents argue that the laws are likely to increase violent crime, especially homicide, as heated disputes involving permit holders are more likely to turn deadly because of the greater lethality of firearms. This study uses panel data for all U.S. cities with a 1990 population of at least 100,000 for 1980 to 2000 to examine the impact of "shall-issue" concealed handgun laws on violent crime rates. The authors measure the effects of the laws using a time-trend variable for the number of years after the law has been in effect, as opposed to the dummy variable approach used in prior research. They also address many of the methodological problems encountered in previous studies. The results provide no evidence that the laws reduce or increase rates of violent crime.

Keywords: *gun control; right-to-carry laws; homicide; violent crime; concealed-carry laws; handguns*

By 2001, at least 33 states had adopted "right-to-carry" or "shall-issue" concealed firearms laws (SI laws), which require authorities to issue concealed handgun permits to adult residents meeting specified objective criteria (U.S. Bureau of Justice Statistics, 2001, pp. 94-95). The laws replaced earlier locally administered, highly discretionary, "may issue" carry permit laws in which

Exhibit A

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local authorities could issue a carry license but were not required by law to do so. Supporters of SI laws maintain that allowing citizens to carry guns legally reduces crime, especially those committed in public places such as robbery, because prospective criminals fear encountering armed victims (Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997). This position is based on theories of economic choice which posit that "a person commits an offense if the expected utility to him exceeds the utility he can get by using his time and other resources at other activities" (Becker, 1968). Specifically, proponents argue that SI laws can reduce levels of violence by deterring prospective criminals from even attempting crimes, presumably because would-be criminals perceive an increased risk of injury to themselves and a reduction in the rate of success in completing crimes (Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997).

SI laws, however, do not automatically increase criminals' fear that victims might be armed. They might not know about the law. The actual increase in self-protection gun carrying might be, or might be perceived as, slight in comparison with normal rates of self-protection gun carrying., most of which is probably done in violation of concealed weapons carrying laws (Kleck, 1997; Kovandzic & Marvell, 2003). And some newly licensed gun carriers probably carried illegally before the new laws (Kleck, 1997; Kovandzic & Marvell, 2003; Lott, 1998; Ludwig, 1998).

Opponents of SI laws argue that "threatening situations" (when someone is attacked or fears an attack) are more likely to turn fatal when more people carry guns (Cook, 1991; Ludwig, 1998; McDowall, Loftin, & Wiersema, 1995b; Webster, Vernick, Ludwig, & Lester, 1997; Zimring, 1968).[1] Other critics speculate that higher levels of self-protection gun carrying by permit holders might prompt criminals to carry guns more often (Ayres & Donohue, 2003a; Cook, 1991; Green, 1987; Ludwig, 1998; McDowall et al., 1995a; but see Kleck, 1997, pp. 204-205).

The present study examines the impact of SI laws on the four major forms of violent crime, using panel data from 1980 to 2000 for U.S. cities with a 1990 population of 100,000 or more. In the next section of the article, we examine the extensive prior research on SI laws and suggest procedures to mitigate methodological problems encountered there. We then describe our data and methods and present our results. In the final section, we consider the theoretical and policy implications of our findings.

PREVIOUS RESEARCH[2]

The first evaluation of SI laws was Kleck and Patterson (1993), using cross-sectional data for 170 U.S. cities with a population greater than 100,000 in 1980. They separately assessed the effects of 19 different types of state and city gun controls, including those SI laws passed before the post-1986 wave of SI laws on rates of homicide, robbery, assault, rape, suicide, and fatal gun accidents, as well as the impact on gun ownership levels. The authors found no evidence that cities in states with SI laws have lower or higher rates of violence compared to cities in states without SI laws. There was also no evidence of higher rates of gun ownership in cities that reside in SI states, undercutting the idea by many that SI laws might lead to increases in gun ownership levels (Ayres & Donohue, 2003a; Lott, 2000). Because few SI laws existed in 1980, however, this evaluation is incomplete.

The next study (McDowall et al., 1995a) used ARIMA time-series analyses with monthly homicide mortality data (during 1973 to 1992) from five counties in Mississippi, Oregon, and Florida. They found positive, and usually significant, impacts on gun homicides, whereas the impacts on nongun homicide were mixed. The authors concluded that, at the least, there was no evidence that SI laws reduce homicide. Several have criticized this study for failing to justify the selection of the five counties (Kleck 1997; Polsby, 1995). In response to Poslby's (1995) criticism that deterrence theory suggests that nongun homicides are also likely to be reduced by more gun carrying, McDowall et al. (1995a) examined annual total homicide data for all of Florida and found an overall decline following the passage of Florida's SI law (see second panel of their Table 2).

The most publicized and controversial study of SI legislation is by Lott and Mustard (1997) in the *Journal of Legal Studies* and subsequent follow-ups to that work, especially two books by Lott titled *More Guns, Less Crime* (Lott, 1998b, 2000). The initial study by Lott and Mustard (1997) evaluated SI laws in 10 states using county panel data for 1977 to 1992. The SI laws were entered as before-after 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 alternate analyses, such as with differenced variables, with individual state trends, and with laws represented by linear and nonlinear trends and permits issued in a single year. In general, they concluded that SI laws reduce violent crime, including homicide, by some 4% to 7%, but increased property crimes. Follow-up studies by Lott (1998a, 1998b, 2000), which added later years of data and new SI laws, largely confirmed the negative correlations between enactment of SI laws and violent crimes observed in the original Lott and Mustard (1997) study.

Given the obvious policy implications of Lott and Mustard's findings for the regulation of concealed gun carrying in public places, numerous academics have reanalyzed the Lott and Mustard data, at least 15 by our count. Of these 15 studies, 8 of them found SI laws to be significantly and negatively correlated with violent crime in at least half of the model specifications presented (Benson & Mast, 2001; Bronars & Lott, 1998; Donohue, 2003; Duggan, 2001; Marvell, 1999; Moody, 2001; Olson & Maltz, 2001; Plassmann & Tideman, 2001; Plassmann & Whitley, 2003). Five studies generally found nonexistent effects of SI laws on violent crime rates (Black & Nagin, 1998; Dezhbakhsh & Rubin, 1998; Harrison, Kennison, & Macedon, 2000; Marvell, 2001), whereas the remaining three studies generally found SI laws in more than half of all model specifications presented to be, if anything, positively related to violent crime rates (Ayres & Donohue, 2003a, 2003b; Ludwig, 1998).

Especially important is Black and Nagin (1998), who relaxed the assumption of uniform effects in the Lott and Mustard (1997) model by entering separate dummy variables for each state SI law. With respect to homicide and rape, the number of negative coefficients, significant and nonsignificant, only slightly outnumbered their positive counterparts. Florida's large negative coefficients stood out, and without Florida the apparent impact of the laws when using an aggregate law dummy disappeared for murder and rape.

Another reanalysis of Lott and Mustard's (1997) data was conducted by Ludwig (1998). Ludwig suggests Lott and Mustard's results may be attributed to omitted variable bias because the fixed-effects approach cannot control for unobserved factors (e.g., crack markets, gang activity, poverty) that influence county crime rates but are not fixed across time. Ludwig argues that these factors may have influenced SI and non-SI states differently, resulting in spurious or partially spurious findings for the SI law variable. To address the problem of omitted variable bias, Ludwig uses the difference-in-difference-in-difference (DDD) estimator, which takes advantage of the fact that juveniles cannot obtain carry permits because of minimum age requirements. Ludwig argues juveniles serve as a natural control group for estimating the impact of SI laws on adult homicide victimization rates (i.e., the treatment group). According to Ludwig, the difference between the change in the adult and juvenile homicide victimization rate eliminates the effects of both fixed and time-varying factors that cause both homicide series to vary across time and isolates those factors that impact the difference between adult and juvenile homicide victimizations. Ludwig also accounts for the possibility that nationwide factors may have influenced changes in adult and juvenile homicide victimization rates differently by comparing differences in the adult-juvenile trends in SI states with the difference in adult-juvenile homicide rates in non-SI states. As a result, the DDD estimator is able to isolate those factors that are unique to states passing SI laws that will cause adult homicide rates to increase or decrease compared to juvenile homicide rates. Using state panel data for 1977 through 1994, Ludwig found that adult homicide rates have increased, albeit nonsignificantly, in states passing SI laws. More specifically, Ludwig reports an increase of .16 homicides per 100,000 adults, implying an increase in adult homicide rates in SI states of roughly 1.4%. Consistent with the findings of Black and Nagin (1998), Ludwig also finds Florida to be a key player in the SI-crime debate. When excluding Florida from the sample, the estimated impact of SI laws on adult homicide rates become even greater in the positive direction (.76 homicides per 100,000 adult population, which equates to a 6.8% increase in the adult homicide rate in SI states).[3]

The most recent analysis of the Lott and Mustard (1997) data is by Ayres and Donohue (2003a, 2003b). Similar to Black and Nagin (1998), the authors found SI laws to be negatively and significantly related to most violent crimes when using an aggregated "hybrid model," which includes a dummy variable and a linear trend variable in the model specifications to capture any immediate and long-term effects of the laws on crime (see Tables 10 and 11 in Black & Nagin, 1998). However, when the authors used a separate dummy and time-trend variable for each state to estimate a state-specific effect for each of the 24 adopting states, they found every crime type in more states where SI laws were positively and significantly related to crime than in more states where SI laws were negatively and significantly related to crime. Of the 216 estimated impacts reported (24 states by 9 crime types), 150 were in the positive direction and 59 of them were statistically significant, whereas only 17 were statistically significant in the negative direction. More important, there were 6 states which witnessed a statistically significant increase in violent crime, whereas only one state (Florida) experienced a statistically significant decrease. The authors attributed the differences between the aggregated and disaggregated hybrid models to two factors. First, weighting the regressions by population in the aggregated hybrid model gives undue influence to states with a large number of high population counties like Florida and Texas—both of which witnessed statistically significant decreases in crime after they passed SI laws. Second, the aggregated model gives early-adopting states greater impact in the estimation than late-passing states. Because early- and large-passing states such as Florida and Georgia witnessed drops in crime following the passage of SI laws, they had a greater impact on the estimated aggregate impact.

A study not based on the Lott and Mustard (1997) data set is by Kovandzic and Marvell (2003). It evaluated Florida's SI law's impact using county-level Uniform Crime Report (UCR) data from Florida authorities. As discussed above, previous studies of SI laws have suggested that Florida plays a pivotal role in the SI law-crime debate. McDowall et al. (1995b) found that the Florida law, if anything, is associated with more gun homicides, whereas Ayres and Donahue (2003a), Lott and Mustard (1997), Lott (1998b, 2000), and Ludwig (1998) found that it reduced homicides. More important, Black and Nagin (1998) and Marvell (1999) argue that the Lott and Mustard (1997) and Lott (1998b, 2000) results for homicide and rape are entirely driven by the inclusion of Florida in their sample. Kovandzic and Marvell (2003) used panel data for 58 Florida counties from 1980 to 2000. The impact of SI laws on violent crime was measured using data on carry permits issued per 100,000 population rather than the dummy variable and time-trend variable approach used in earlier evaluations. They controlled for numerous confounding factors including age structure, economic deprivation, and prison population. The authors also addressed potential simultaneity problems between permit issuance rates and violent crime using the Granger causality test. The authors found little evidence of a relationship between permit-issuing rates and violent crime. They also found no evidence of a deterrent or homicide-promoting effect of permit rate growth when using homicide victimization data from the Centers for Disease and Control (CDC) or when modeling UCR and CDC homicide victimization rates as a Poisson distribution. Results from the Granger causality test also found little evidence that increases in violent crime lead to increases in permit-issuance rates.

Methodological Shortcomings of Previous Research

Although previous evaluations of SI laws and crime have attempted to address the various methodological shortcomings typically associated with macro-level evaluations of policy interventions, they have done so in a piecemeal fashion. It is important that research address all these shortcomings at once. We believe the major methodological deficiencies are the following: (a) the use of dummy variables to measure the treatment effects of SI laws on crime; (b) the use of aggregate law variables, which assume that SI law impacts are similar in all states; (c) the inability to address potential simultaneity problems between passage of SI laws and crime; (d) measurement problems surrounding the dates of passage of state SI laws; (e) the use of county-level UCR data, which is unreliable because of incomplete crime reporting and inadequate procedures to impute missing crime data; and (f) the overestimation of significance levels in county-level studies because of "clustering" of error terms at the state level. We discuss each of these problems below and discuss how we attempt to address them in our research.

Using dummy variables to measure the treatment effects of shall-issue laws. With several exceptions (e.g., Ayres & Donohue, 2003a, 2003b; Kovandzic & Marvell, 2003; Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997), analysts have relied solely on before-after dummy variables to measure the "treatment effects" of SI laws on violent crime. This assumes unrealistically that SI laws have a once-and-for-all impact on crime. More specifically, this dummy variable approach implies that criminals know when SI laws go into effect, do not forget about them, and believe the chance of encountering an armed victim varies little across time. Although it is entirely plausible that the mere passage of a SI law could lead to immediate reductions in crime because of publicity campaigns and news coverage attendant to the passage of the laws (often referred to as *announcement effects*), it is unlikely that such effects would remain static across time (Ayres & Donohue, 2003a; Kovandzic & Marvell, 2003). Perhaps crime levels would have to return to normal as publicity fades. Perhaps the crime-reduction impact of SI laws is lagged for a year or so as the criminal population learns about the laws via word of mouth (Kleck, 1997; Kovandzic & Marvell, 2003). Quite likely the laws act as a deterrent according to the extent they increase the number of permits and adults carrying guns (Kovandzic & Marvell, 2003; Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997). Because the number of adults with carry permits grows in approximately a linear fashion (Kovandzic & Marvell, 2003, p. 377; Lott, 2000, p.75), one might expect any deterrent impacts of SI laws on violent crime to increase across time as criminals respond to the increased risk of coming into contact with armed victims (Lott, 1998a, 1998b, 2000; Lott & Mustard, 1997).

Data on the number of persons with carry permits is only available in a few states such as Florida (see Kovandzic & Marvell, 2003), therefore we rely primarily on time trend variables to model the impact of the laws. This procedure is not without precedent. Lott and Mustard (1997), for example, presented results using time and time-squared variables for the number of years before and after the law went into effect, and the results suggest that deterrent effects of SI laws increase across time, presumably because of increased self-protection carrying by prospective victims. Ayres and Donohue (2003a) also found evidence of growing deterrent effects of SI laws on violent crime when using an aggregated time-trend model (referred to as the Lott-spline model) and the hybrid model which we described earlier, but they discount these results because they are not based on their preferred model with disaggregated SI law variables. Black and Nagin (1998) also examined whether SI laws become more effective over time. They used a series of dummy variables indicating the number of years before and after the enactment of a SI law. Results indicated that homicide, rape, and assault were declining in counties residing in

SI states prior to the adoption of the SI law and continued to decline thereafter. With respect to robbery, they found increases prior to and after of the adoption of a SI law, although the postintervention increase was at a much slower rate (Black & Nagin, 1998, p. 215).

Assuming uniform effects of SI laws on violent crime. A second problem is that most studies assume that SI law effects are homogeneous. As noted above, Black and Nagin (1998), Marvell (1999), Ayres and Donohue (2003a) found substantial differences between states when the SI law variable is disaggregated and a tendency for positive coefficients to outnumber negative ones. This work is consistent with recent econometric research by Pesaran and Smith (1995) and Baltagi and Griffin (1997), which concludes that the assumption in panel studies of homogeneous impacts across jurisdictions is probably not justified. In the present analysis, we conduct the main analysis with an aggregated SI variable, and then use state-specific SI law variables to see if the results are consistent.

Simultaneity problems. With the possible exception of Kovandzic and Marvell (2003), previous studies of SI laws have not adequately addressed simultaneity problems, which might arise because growing crime rates might prompt states to pass SI laws and prompt citizens to obtain permits. Such an effect would bias the SI law coefficients in a positive direction, understating any deterrent effect. Lott and Mustard (1997) and Lott (1998b, 2000) address potential simultaneity bias using two-stage least squares regressions but do not present the results of any standard diagnostic tests to ensure their excluded instrumental variables are reliable (i.e., the excluded instruments are correlated with the endogenous explanatory variable, passage of SI laws) and valid (i.e. the excluded instruments are uncorrelated with the error terms in the violent crime equations). Davidson and Mackinnon (1993) maintain that "tests of overidentifying restrictions should be calculated routinely whenever one computes 2SLS estimates" (p. 236). Sargan takes it a step further and argues that studies using 2SLS regression procedures without testing for overidentifying restrictions is a "pious fraud" (as cited in Godfrey, 1988). In this article, we follow the lead of Kovandzic and Marvell (2003) and use the Granger causality test to address the possible reciprocal relationship between the passage of SI laws and violent crime.

Incorrect dates for passage of SI laws. Lott and Mustard (1997) coded the effective dates of SI laws based on a compilation of passage dates provided in Cramer and Kopel (1995). As Kleck (1997) notes, relying on a single source of information for coding of gun laws often leads to measurement error for the gun law variables. In Lott and Mustard's case, they used the incorrect effective date for 5 of the 10 laws studied. The correct effective dates of the laws are given in Marvell (2001, p. 707; see also Vernick & Hepburn, 2003).

County-level UCR data problems. Most research on SI laws uses county-level UCR data, archived and produced by the National Archive of Criminal Justice Data (NACJD). These data are highly suspect because reporting is spotty, especially in small counties, and attempts by NACJD to estimate missing data are incomplete and change across time (Maltz & Targonski, 2002; Marvell, 1999). NACJD obtains from the FBI the raw UCR figures that are sent by police agencies to the FBI, and it combines agencies within each county to develop county-level crime data. However, NACJD has to deal with missing data to make reasonable county level estimates of crime and permit year-to-year comparisons in crime. NAJCD imputed crime data for counties during the years 1977 to 1993 as follows: Within each county, any agency submitting less than 6 monthly reports is excluded when calculating the county's total crime and population counts. If, however, the agency submitted 6 to 11 monthly reports, the crime data were weighted to produce 12 monthly equivalents. As a result, crime rate calculations derived from the NACJD county crime dataset implicitly assumes that excluded law enforcement agencies have a crime rate that is identical to the rest of the county (Maltz & Targonski, 2002, p. 308). Lott and Mustard (1997), moreover, did not rely on population figures from NACJD when calculating county crime rates, instead using countywide population counts from the U.S. Census Bureau, such that they assume that agencies with missing data have no crime.[4]

In the present study, we use cities as our unit of analysis, and UCR city data does not suffer from the data-reporting problems described above for county-level crime data. Because the FBI only reports crime counts for a particular city in their annual report if the individual law enforcement agency responsible for that jurisdiction submits 12 complete monthly reports, there is no need to impute missing crime data because of incomplete agency reporting. In addition, cities exhibit greater per-capita variation in crime rates than do large urban counties or states, which is exactly what SI law-crime research is trying to explain. Finally, cities are more internally homogenous than counties or states and thus are less likely to be susceptible to aggregation bias (see also Lott, 2000, p. 30-33).

Overestimation of significance levels. Finally, Lott and Mustard (1997), Lott (1998a, 1998b, 2000), and those revisiting the SI law-crime question using county-level data have overestimated the statistical significance of their findings because of correlation of variables within states (Harrison, Kennison, & Macedon, 2000; Moody, 2001). In such a situation, standard errors can be seriously biased downward, leading to inflated *t* ratios for the SI law variable (Greenwald, 1983; Moulton, 1990). Using Lott and Mustard's county-level data and robust Huber-White standard errors, which do not require independence of observations within "clusters" (i.e., SI states), both Harrison et al. (2000) and Moody (2001) found that the robust standard errors for the SI law dummy variables in the homicide regressions were much larger than the conventional standard errors. Coefficients on the dummy variables in the homicide regressions were rarely significant at the .05 level.

DATA AND METHOD

Research Design and Sample

The present study examines the potential deterrent effects of SI laws using panel data for the period 1980 to 2000 from 189 cities with a population of 100,000 or more in 1990 for which there were Uniform Crime Reports data. Of the 189 cities with populations greater than 100,000 in 1990, 77 resided in states passing SI laws between 1980 and 2000. If SI laws have any deterrent impact, it is most likely to show up in cities, because the cities had more restrictive permit practices under pre-SI laws then rural areas, such that the SI laws probably had a larger impact on self-protection gun carrying (Lott, 1998b, 2000; Lott & Mustard, 1997).

Panel data have distinct advantages over more commonly used time-series or cross-sectional data. The most important is the ability to enter proxy variables for omitted variables that cause crime rates to vary across time and space. The proxy variables, which number more than 200 here, are discussed further below. Second, the high number of degrees of freedom provides greater statistical power and permits numerous control variables, which gives us more confidence that nonsignificant coefficients indicate the absence of an impact.

Methods for Panel Data

We follow conventional strategies for the statistical modeling of panel data by using a fixed-effects model, in which there is a dummy variable for each city and year, except the first year and city to avoid perfect collinearity (Hsiao, 1986, p. 41-58; Pindyck & Rubinfeld, 1991, p. 224-226).[5] Specifically, the city dummies control for unobserved (and unmeasurable) city-specific factors whose values remained approximately stable during the study period (i.e., time-invariant factors) that caused rates of violent crime to differ across cities (Hsiao, 1986). Examples of these factors might include demographic characteristics, political orientation of city, urbanity, climate, drug and gang-related activities, and deeply embedded cultural and social norms. The city dummies also control for differences in city-level crime reporting practices that remained approximately stable during the study period. The year dummies control for unobserved time-varying factors that could affect all cities in a given year in the same fashion. An example of a national event that may have affected violent crime throughout the nation would be the 1994 Crime Control and Law Enforcement Act, which contained several major crime-reduction programs including truth-in-sentencing, the federal version of a three-strikes law; funds for 100,000 new officers; expansion of the death penalty; ban on possession of guns by juveniles; and enhanced penalties for drug offenses and for using firearms in crimes. Because the analysis includes fixed effects for both years and cities, the coefficient estimates for the SI law time-trend variable and specific control variables (discussed below) are based solely on within-city changes across time. Finally, we follow the recommendation of Ayres and Donohue (2003a) and Marvell and Moody (1996, 2001) and include separate linear trend variables for each city.[6] These control for unobserved factors that affect the time-series behavior of crime that can differ from city to city and depart from the nationwide trends captured by the year dummies. Without them, the coefficient on the SI law time-trend variable would simply measure whether crime rates are higher or lower for years after the law (relative to national trends captured by the year dummies), even if the change occurred before or well after the law went into effect.

Right-to-Carry Law Variables

Between 1980 and 2000, 24 states switched to a nondiscretionary permit system allowing applicants, who meet certain objective criteria, to obtain a permit to carry a concealed handgun. The 24 states and the years they began issuing permits on a nondiscretionary basis were obtained through statutory research conducted by Marvell (2001). They are as follows: Alaska (1994), Arizona (1994), Arkansas (1995), Florida (1987), Georgia (1989), Idaho (1990), Kentucky (1996), Louisiana (1996), Maine (1980), Mississippi (1990), Montana (1991), Nevada (1995), New Hampshire (1994), North Carolina (1995), Oklahoma (1995), Oregon (1990), Pennsylvania (1989), South Carolina, (1996), Tennessee (1994), Virginia (1995), Texas (1995), Utah (1995), West Virginia (1988), and Wyoming (1994). Seven states had SI laws or their equivalents prior to 1980 (Alabama, Connecticut, Indiana, North Dakota, South Dakota, Vermont, and Washington).[7] The SI laws include only those that did not give local authorities discretion to reject applications; they do not include laws that state that authorities "shall issue" permits but then proceed to give the issuing authority discretion to reject the application because, for example, the authority deems the applicant to lack "good moral character."

As discussed above, the impact of SI laws on violent crime are measured using a time-trend variable, which is coded as zeroes for all the years up to and including the year the SI law was passed in each particular city and the values 1, 2, 3, and so forth for the following years. For example, consider a city located in Florida, which passed its SI law in 1987. In this case, in 1990, the time-trend variable is equal to 3. Again, measuring the effects of SI laws in this manner allows us to test whether the impacts of the laws are more closely linked to the number of people carrying guns in public, which grows across time as more people obtain permits. Because it is possible, albeit unlikely, that the full deterrent impacts of the laws occur immediately (if prospective shooters quickly learn about the laws through "announcement effects" discussed earlier), we also present results of estimations in which the effects of SI laws are measured using a before-after dummy variable. Similar to prior SI law studies (e.g., Lott & Mustard, 1997), the dummy variable is scored 1 the year after a law went into effect and 0 otherwise.[8]

Violent Crime

Violent crime is measured by the four offenses in the UCR Crime Index involving force or threat of force: homicide, forcible rape, robbery, and aggravated assault (Federal Bureau of Investigation, 1981-2001). Rape and assault data are probably less reliable than homicide and robbery data, because reporting rates for assault and rape have changed within the past couple of decades because new laws encourage women to report domestic violence and because police are more likely to record assaults (Reiss & Roth, 1993, pp. 407-414). To the extent these reporting changes occurred nationwide, they would be captured by the year dummies, but we cannot be sure that is the case. Consequently, results for these two crimes should be interpreted with caution. Seven cities were dropped from the sample because they failed to report crime data to the FBI for more than half of the years studied: Moreno Valley, CA; Rancho Cucamonga, CA; Santa Clarita, CA; Overland Park, KS; Kansas City, KS; Cedar Rapids, IA; and Lowell, MA.

Specific control variables. In addition to the year dummies, city dummies, and city-trend variables, we include eight specific control variables. These are selected based on a review of previous macro-level studies linking violence rates to the structural characteristics of geographical units (Byrne, 1986; Kovandzic, Vieraitis, & Yeisley, 1998; Land, McCall, & Cohen, 1990; Parker, McCall, & Land, 1999; Sampson, 1986; Vieraitis, 2000, and the studies reviewed therein); they are percentage African American; percentage Hispanic; percentage ages 18 to 24 and 25 to 44; percentage households headed by females; percentage persons living below the poverty line, per-capita income; percentage population living alone, per-capita income; and percentage state prison population. Data for the first six are from the U.S. Census Bureau (1983, 1994), except that 2000 data were obtained from the U.S. Census Bureau Web site using American Fact Finder. These measures are only available for decennial census years, and we estimate data between decennial census years via linear interpolation. Given the small changes in these variables between decennial census years, a linear trend is justified. Income data for 1980 to 2000 are from the U.S. Bureau of Economic Analysis Web site. The income data are county-level estimates, and we use these values as imperfect substitutes for city-level income. Personal income data are converted from a current dollar estimate to a constant dollar 1967 basis by dividing personal income by the consumer price index. Prison population is the number of inmates sentenced to state institutions for more than a year, available annually at the state level,[9] using data from the U.S. Bureau of Justice Statistics Web site. Because prison populations are year-end estimates, we take the average of the current year and prior year to estimate mid-year prison population.

Continuous variables are expressed as natural logs to reduce the impact of outliers. Heteroscedasticity was detected using the Breusch-Pagan test, mainly because violent crime rate variation is greater across time in the smaller cities. To avoid inefficient and biased estimated variances for the parameter estimates, we weighted the violence regressions by amounts determined by the test. Panel unit root tests (Levin & Lin, 1992; Wu, 1996) indicate that the violent crime data are stationary (i.e., the unit root hypothesis is rejected, suggesting that the analysis be conducted in levels and not first differences). Autocorrelation is mitigated by including a 1-year lag of the dependent variable in each violent crime regression (Hendry, 1995). The lagged dependent variable also has the added benefit of controlling for omitted lagged effects

(Moody, 2001; Wooldridge, 2000). Examination of collinearity diagnostics developed by Belsley, Kuh, and Welsh (1980) revealed no serious collinearity problems for the SI law time-trend variable. Although there were collinearity problems among the proxy variables, they did not substantively alter the coefficients or the statistical significance of the SI law time-trend variable, and we only measured the significance of proxy variables as groups using the *F* test. Perfect collinearity among each set of proxy variables was avoided by dropping one year dummy (i.e., 1980), one city dummy (Birmingham, AL), and one city trend variable (Birmingham, AL).

RESULTS

Table 1 presents the results for each violent crime type, using regression procedures described above. Specifically, we estimate the aggregate impact of SI laws on violent crime with the following model:

$$y_{it} = \alpha_t \mathbf{year}_t + \Phi_i \mathbf{D}_i + \gamma(Shall_{it}\text{*trend}) + \Psi_i(\mathbf{D}_i\text{*}\mathbf{trend}) + \beta \mathbf{x}_{it} + u_{it}$$

where y_{it} is the natural logarithm of a particular violent crime per 100,000 people in city i in year t, $\mathbf{year}_t$ is a vector of year dummies, $\mathbf{D}_i$ is a vector of city dummies, $\mathbf{D}_i$***trend** is a vector of individual city trends (equal to 1 in 1980, 2 in 1981, and 21 in 2000), $\mathbf{x}_{it}$ is a vector of demographic and economic controls and u_{it} is an error term. The variable $Shall_{it}$*trend is a time-trend variable equal to the number of years after the law had been in effect and equal to 0 for the years before the law had been in effect. Additional analyses explore potential simultaneity bias problems using the Granger causality test and potential "announcement effects" of SI laws on violent crime using the dummy variable approach.

The Aggregate Impact of Shall-Issue Laws on Violent Crime

The results in Table 1 provide no support for Lott and Mustard's (1997) and Lott's (1998a, 1998b, 2000) thesis that the longer SI laws are in place, the greater their deterrent effect on violent

TABLE 1
The Estimated Impact of Shall-Issue Laws on Violent Crime

	Dependent Variable: Natural Log of the Corresponding Violent Crime Type Per 100,000 Resident Population							
	Homicide		*Robbery*		*Assault*		*Rape*	
Target Independent Variable	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*
SI law time-trend variable	.011	0.80	.010	0.91	.019	2.59	.012	1.33
Control variables (in natural logs)								
Percentage 18 to 24 years old	**1.55**	4.13	**.532**	2.22	–.333	–1.59	–.097	–0.39
Percentage 25 to 44 years old	**–.867**	–0.58	–.086	–0.17	–.379	–0.84	.824	1.59
Percentage Black	.264	1.18	**.276**	2.45	.042	0.47	.071	0.30
Percentage Hispanic	.085	0.97	.045	0.86	–.008	–0.15	**–.105**	–2.03
Percentage female-headed households	**.311**	2.68	–.030	–0.58	.028	0.46	.005	0.05
Percentage persons < poverty line	–.033	–0.11	.014	0.09	–.190	–1.22	**.335**	2.23
Percentage persons living alone	–.737	–1.28	**–.670**	–2.48	.189	0.70	.558	1.09
Per-capita income, county	.753	1.98	.177	0.92	–.008	–0.06	**.479**	3.56
Prison population, state	**–.298**	–3.57	**–.212**	–3.78	.013	0.29	–.074	–1.39
Violent crime type, 1-year lag	**.070**	1.97	**.558**	23.61	**.565**	17.77	**.409**	7.62
Sample size	3,863		3,863		3,863		3,773	
Adjusted R^2	.897		.971		.941		.907	

NOTE: The violent crime regressions encompass 189 cities (in 43 states) during 1980 to 2000. The dependent variables are listed at the top of each column. To conserve space results for city dummies, year dummies and city trend variables are not shown. The shall-issue law is represented by a time-trend variable as described above. All continuous variables are divided by population and logged. All regressions are weighted by a function of population as determined by the Breusch-Pagan Test. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

crime. The coefficient on the aggregate SI law time-trend variable is in the unexpected positive direction for each of the four violent crime regressions and is significant in the positive direction for aggravated assault. The *t* ratio for aggravated assault, however, is somewhat small given the large sample size and, as discussed above, the assault data are somewhat suspect. In any event, the results for the aggregate SI law time-trend variable imply an average increase of 0.2% in aggravated assault for each additional year SI laws are in effect, for a net effect of 1% higher aggravated assault rates after 5 years. Perhaps the most damaging finding in Table 1 to the more guns–less crime thesis, however, is the fact that robbery is not reduced by the increased presence of SI laws. If prospective criminals afraid of encountering armed victims in public places are deterred from even attempting crimes in the first place, then robbery should be the crime most likely to decline because it is committed in public more than homicide, rape, and assault.

Examining Robustness of Findings Using Alternate Model Specifications

Additional analyses, which are not reported in the interest of space, indicate that the lack of deterrent effects of SI laws on violent crime rates revealed in Table 1 do not appear to be sensitive to model specification.[10] The results are similar with a distributed lag (a trend that plateaus after 5 years), with first-differenced variables, dropping the city trend variables, without logging variables, without weighting the regressions, and without the lagged dependent variables. In contrast to Table 1, the SI law coefficient is not significant in the assault regressions. When we reestimated the regressions in Table 1 using robust standard errors without clustering by state, *t* ratios were greater than 2 in the robbery, assault, and rape regressions.

Addressing Potential Simultaneity Problems

One possible explanation for the lack of a negative and significant coefficient for the SI law variables is simultaneity, which can happen if citizens respond to increases in violent crime by applying for and obtaining permits to carry guns or if state governments enact SI laws in response to high-crime rates. It does not help to lag the independent variable because serial correlation between current and prior year crime rates can lead to simultaneity with the lagged dependent variable. If there is simultaneity, the SI variable coefficient might be biased in the positive direction—the opposite of any deterrent impact on violent crime. We explore this issue in two ways. The first is the Granger causality test, which entails regressing the SI law time-trend variable on one and 2-year lags of itself and 1- and 2-year lags of violent crime (Granger, 1969; Pindyck & Rubinfeld, 1991). The Granger test has a drawback in that it misses purely contemporaneous (same year) causation (Wooldridge, 2000, p. 98). In the present situation, however, if violent crime has a contemporaneous impact on permit laws and permit use, it must also have an impact lagged 1-year, because it takes time for legislatures and citizens to learn of crime trends and act on them. In addition, serial correlation of current and lagged crime rates would probably produce a significant coefficient on the lagged crime variable even if causation is completely contemporaneous. Thus, the absence of a lagged impact implies the absence of a current-year impact. The results of the Granger test showed no evidence of reverse causation. The lagged homicide variables in the SI time-trend variable regression were far from significant, small in size, and in the unexpected negative direction.

The second procedure, which only addresses possible simultaneity involved in enacting the law (i.e., that the legislature might act in response to high crimes rates, as opposed to simultaneity because of citizens getting more permits), is to drop from the analysis observations occurring just before and just after the law was passed (i.e., three observations for each state with SI laws). This analysis produces results very similar to those in Table 1. In sum, there is no evidence that individuals respond to increases in violent crime by acquiring concealed carry permits and, presumably, begin lawfully carrying guns in public for purposes of self-protection.

Models With Shall-Issue Law Dummy Variable

As discussed above, estimating the impact of SI laws on homicide by the number of years the law is in existence might miss an impact that is due solely to the existence of the law or to "announcement" effects when the law went into effect. This is the traditional before-and-after model, operationalized by a dummy variable scored 1 for all years after the law went into effect. Although the coefficients on this SI law dummy variable are generally in the negative direction, they are extremely small and far from significant (homicide, $b = -.001$, $t = -.03$; robbery, $b = .009$, $t = .30$; assault, $b = -.021$, $t = -.94$; rape, $b = -.005$, $t = -0.23$). The results do not differ substantially when using the alternate regression procedures listed above in reference to the regressions with SI trend variables. These "null" results for the SI law dummy variables differ from much previous work, which generally find a deterrent effect (e.g., Lott, 1998b, 2000; Lott & Mustard, 1997) or "homicide promoting effect" (e.g., McDowall et al., 1995b) for SI laws.

To test the possibility of announcement effects (i.e., a short term impact resulting from publicity given the law when first enacted), we constructed a dummy variable that is scored one only in the first 2 years after a SI law is enacted. Again, coefficients are small and far from significant, with the exception of the assault regression, where the coefficient is $-.041$ ($t = -2.71$). Although this suggests a small announcement effect that deters assaults, it is not evidence that SI laws reduce assault because in the long run, SI laws appear to increase assault (see Table 1).

Estimating the State-Specific Impacts of Shall-Issue Laws on Violent Crime

Based on the results in Table 1, there is no evidence to support the thesis that the longer SI laws are in place, the greater their deterrent effect on violent crime. However, the regressions in Table 1 estimated an aggregated effect for the laws across all cities residing in adopting SI states. If, for example, the impact of the laws on violent crime rates varies significantly across states then the models in Table 1 are misspecified. Moreover, as noted above, the dangers of estimating a single aggregated effect are particularly acute because of differences in (a) permit fees and training requirements for a concealed handgun permit and where concealed handguns can be taken (Lott, 2000), (b) publicity and news coverage surrounding passage of the laws, and (c) the number of persons in the adult population with concealed handgun permits.

We address this problem by using separate SI law variables for each state. The variable is a postlaw trend for cities in a particular state and 0 for cities elsewhere. Table 2 presents these estimates for all four violent crime categories and shows that the coefficients on the SI law time-trend variable for each of the 19 states that switched to a nondiscretionary carry permit system between 1980 and 2000—a total of 76 estimates.

Similar to Ayres and Donohue (2003a), we are leery of the more constrained specifications of the aggregate regressions, which implicitly assumed that the impact of SI laws is uniform across states. Indeed, for each violent crime type, we were able to reject the hypothesis that the 19 SI law time-trend variables were jointly equal. But this heterogeneity does not lead us to revise the Table 1 results because for each violent crime category, there are more states where passage of SI laws lead to statistically significant increases in violent crime rates than states with statistically significant decreases. For example, although there are two states that experienced significant declines in homicide, five states experienced significant increases. Of the 76 estimated impacts of SI laws on violent crime rates presented in Table 2, 13 exhibited statistically significant decreases in violent crime upon passage of the laws, whereas 23 exhibited significant increases. Overall, Table 2 shows 33 decreases in violent crime and 43 increases. In sum, the results of the state-specific effects of SI law suggests that for most states, the passage of SI laws are positively associated with violent crime rates.

Examination of the SI law time-trend variables for individual states reveals that cities in two states (Arkansas and Louisiana) show a statistically significant decrease in at least three violent categories without showing a significant increase in any category. This result differs from Ayres and Donohue (2003a), who found a positive association between passage of SI laws and violent crime rates in these states. On the other hand, the significant increases for cities in Pennsylvania and Nevada are similar to Ayres and Donohue's findings. Perhaps the most important finding in Table 2 is the lack of a significant relationship between passage of SI laws and homicide rates in Florida. As noted above, the

TABLE 2
The State-Specific Impact of Shall-Issue Laws on Violent Crime

	Dependent Variable: Natural Log of the Corresponding Violent Crime Type Per 100,000 Resident Population							
	Homicide		*Robbery*		*Assault*		*Rape*	
State	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*
Alaska	−.021	−1.31	−.042	−4.42	−.001	−0.06	.009	0.51
Arizona	.042	3.66	.022	2.91	.015	1.92	.039	3.34
Arkansas	−.046	−3.21	−.049	−5.98	−.065	−6.07	−.009	−0.75
Florida	−.008	−0.81	−.020	−3.26	.013	2.10	.017	2.76
Georgia	.020	1.37	.011	1.29	.034	5.61	−.005	−0.61
Idaho	−.010	−0.59	.070	5.38	.058	8.60	.017	1.85
Kentucky	.059	2.84	.017	1.43	−.016	−2.17	−.040	−4.10
Louisiana	−.045	−2.06	−.041	−3.60	−.050	−5.96	.001	0.12
Mississippi	−.023	−1.94	−.007	−0.91	−.002	−0.33	.038	4.78
Nevada	.116	8.19	.078	9.08	.023	4.13	.064	8.35
North Carolina	.010	0.58	.002	0.23	.022	2.15	−.004	−0.31
Oklahoma	−.014	−0.97	−.027	−3.07	−.010	−1.61	−.020	−2.14
Oregon	−.007	−0.56	.002	0.35	.047	9.55	−.001	−0.26
Pennsylvania	.060	4.83	.035	4.33	.058	7.22	.045	6.55
South Carolina	−.032	−0.96	.019	1.25	−.019	−1.08	−.107	−4.13
Tennessee	.039	2.30	.019	1.82	.001	0.15	.016	1.75
Texas	−.014	−0.96	.026	2.93	.006	0.94	−.003	−0.43
Utah	.004	0.07	.035	1.71	.079	2.79	.009	0.19
Virginia	−.024	−0.83	.044	2.48	.034	1.96	−.009	−0.50

(continued)

TABLE 2 (continued)

	Dependent Variable: Natural Log of the Corresponding Violent Crime Type Per 100,000 Resident Population							
	Homicide		*Robbery*		*Assault*		*Rape*	
State	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*	*Coefficient*	*t ratio*
Summary								
Negative and significant	2		5		3		3	
Negative and not significant	9		1		4		6	
Positive and significant	5		6		7		5	
Positive and not significant	3		7		5		5	

NOTE: This table presents violent crime regressions similar to those reported in Table 1 except that state-specific SI law time-trend variables are entered instead of the aggregate SI law time-trend variable. Coefficients that are significant at the .05 level are displayed in bold. Coefficients that are significant at the .01 level are both underlined and displayed in bold.

disaggregated SI law analyses conducted by several researchers (e.g., Ayres & Donohue, 2003a; Black & Nagin, 1998; Marvell, 1999) revealed large drops in homicide rates for Florida counties after its SI law, and they concluded that Florida is largely responsible for the negative correlations observed between passage of SI laws and homicide when using aggregate law variables. The reason for the disparate findings between those and the present study might be because there was a decline limited to rural areas or because of problems with the NACJD county data.

Results for Specific Control Variables in Table 1

Finally, the results for the control variables in Table 1 yield several key findings for future macro-level studies attempting to explain temporal variation in violent crime. First, increases in the number of African Americans and persons living below the poverty line do not appear to increase violent crime, except that the former may increase robbery and the latter may increase rape. These results contradict the findings of most cross-sectional studies, which typically find both of these structural covariates to be positively associated to violent crime rates, especially homicide (Kovandzic et al., 1998; Land et al., 1990; Parker et al., 1999). The most likely explanation for the disparate findings is that cross-sectional studies are reproducing cross-sectional variation patterns established at some point in the distant past. That is, at some point in time increases in the size of the African American and the number of persons living in poverty lead to increases in violent crime rates, and a subsequent pattern of cross-sectional variation was established, but this pattern was established well before the study period examined here. Second, increases in state imprisonment rates are associated with lower homicide and robbery, although the elasticities are somewhat smaller than those found in state- and national-level studies (Levitt, 1996; Marvell & Moody, 1997). As expected, increases in the number of persons between ages 18 to 24 are systematically related to increase in homicide and robbery. Finally, the number of families headed by females appears to be positively related to homicide rates. Although a common finding in macro-level cross-sectional studies, to our knowledge, this is the first time this variable has been related to cross temporal changes in homicide rates.

SUMMARY AND DISCUSSION

Our results provide little support for the findings by Lott and Mustard (1997) and Lott (1998b, 2000) that SI laws reduce violent crime. This does not automatically refute the theory that criminals are deterred by a greater possibility that victims are armed, because it is possible that this occurs but is counterbalanced by the theorized criminogenic effects of increased gun carrying that we discussed earlier. It seems unlikely, however, that the two would happen to balance so precisely for most violent crimes. More likely there is no deterrent effect. A likely reason is that the laws do not significantly alter rates of civilian gun carrying for self-protection and thus do not increase actual risks to criminals (Kleck, 1997, p. 372; Kovandzic & Marvell, 2003). Only about 1% of the adult population has concealed handgun permits (Kovandzic & Marvell, 2003), whereas survey research, such as the National Self-Defense Survey (Kleck & Gertz, 1998), indicate that at least 8% of adults carry a gun for protection each year. This suggests that upward of 90% of all self-protection carrying is done in violation of concealed weapon laws. To the extent that jurisdictions with higher levels of permitted gun carrying also have higher rates of total self-protection carrying, it seems unlikely that such a modest increase in the number of prospective victims carrying guns in public places is perceptible to criminals (Kleck, 1997, p. 372). Also, the National Gun Policy Survey found that 73% of adult gun carriers with permits reported no change in their level of gun carrying after they obtained a carry permit (Smith, 2001, p. 15). Most of the permits issued under SI laws, therefore, do not represent additional gun carrying. It is important to stress, however, that the essential factor, according to the deterrence hypothesis, is criminals' perception of the laws' impacts. To our knowledge, there is no information on this topic, and it is a prime candidate for further research.

Although the problems with prior research on SI laws have largely been methodological, the impetus for increasing support for such laws is based on a simplistic view of criminal behavior. Proponents of SI laws have relied on early versions of rational choice theory, put forth by economists, but contemporary versions posit more complex explanations for criminal behavior. The basic idea that criminals make choices based on an analysis of the perceived costs and benefits remains; however, we recognize that offenders' rationality is "bounded" or "limited" (Clarke & Cornish, 2002, p. 25). Offenders do not simply add and subtract the perceived costs and benefits of crime as efficiently as economic theory suggests. The context in which they make their choices, including background factors and situational opportunities, is given greater consideration and specification in contemporary rational choice theories.

In addition, although economic theories of choice assume individuals use similar cost-benefit analyses, criminological rational choice theories consider a wider range of costs and benefits and explore in greater detail individual differences in the criminal decision-making process (Cornish & Clarke, 1986; Paternoster & Bachman, 2002; Tittle, 2000). Even if criminals have timely information regarding the passage of SI laws and the number of people lawfully carrying guns in public, such information is unlikely to have a significant impact on their behavior and violent crime rates. According to ethnographic research on active offenders, most crime is opportunistic and does not involve elaborate planning and potential costs are given relatively little consideration (Jacobs, 2000; Jacobs, Topalli, & Wright, 2003; Shover, 1996; Wright & Decker, 1994, 1997). Even when offenders do calculate the costs, they also factor in their ability to manage or eliminate these potential costs (Hochstetler & Copes, 2003; Miller & Jacobs, 1998). Research suggests that criminals are extremely confident about their abilities to control a situation and deal with whatever may arise, including encountering an armed victim (Jacobs, 2000; Wright & Decker, 1997).

Although the focus of the rational choice perspective as delineated by Cornish and Clarke (1986) concentrates on the impact of decision making on individual criminal behavior, the perspective has also been applied at the macro level. Routine activity theory explains variations in crime rates over time and place. Cohen and Felson (1979) contend that crime rates will be higher in the presence of motivated offenders, suitable targets, and in the absence of capable guardians and that the convergence of these three elements is dependent on the routine activities of persons in everyday life. The presence of motivated offenders is assumed to be a constant; but the number of young males, particularly those residing in poor urban areas, is probably a better measure of the number of motivated offenders. Depending on the type of crime to be studied, definitions of "suitable" targets vary, but for violent crime, the profile of victims mirrors that of offenders (i.e., young, poor, non-White males residing in urban areas). Guardianship concerns any measure—human or nonhuman—which would make a target difficult if not impossible to access. In this case, a gun serves as a capable guardian over a person. Theoretically, violent crime rates should decline with an increase in guardianship (i.e., potential targets are armed), regardless of levels of motivated offenders or suitable targets. However, because the ability of everyday routines to impact violent crime rates is dependent on the convergence of all three elements in time and space, it is unlikely that the passage of SI laws would significantly reduce violent crime rates because permit acquisition, much like gun ownership in general, is higher among Whites, middle-aged persons, richer people, and in rural and suburban areas—patterns that are all the reverse of the way in which criminal victimization is distributed (Hood & Neeley, 2000).

We should point out, however, that neither the present study nor previous evaluations of SI laws have explicitly measured total rates of civilian gun carrying. Consequently, conclusions regarding the net effect of civilian gun carrying on violent crime rates based on this body of research are not warranted.[11] That is, the lack of a negative correlation between passage of SI laws and violent crime rates observed in the present study tells us nothing about the broad effects of civilian gun carrying rates on violent crime, especially homicide. Moreover, if "citizens arming" did reduce violent crime, much of the effect may have nothing to do with gun-carrying rates. The best documented effect of citizen arming on crime is the effect of actual defensive use of guns on whether crime victims are injured. Because homicide, by definition, requires that a victim be injured, anything that reduces injury is very likely to also reduce fatal injury. The evidence on the effects of actual defensive gun use uniformly indicates that it significantly reduces the likelihood of victim injury (see Kleck, 1997, chap. 5, for a review of the literature). Neither the possible, albeit undocumented, effects of civilian gun carrying rates nor the documented effects of actual defensive gun use in any way require that states adopt SI laws for these effects to occur.

NOTES

1. Analysis of revocation data by Lott (2000, p. 221-222) provides little support for the Zimring-Cook hypothesis (i.e., gun violence among permit holders is nearly nonexistent), with less than 0.5% of permits issued being revoked for any type of firearms-related violations.

2. A summary of macro-level studies examining the impact of SI laws on crime rates by Kovandzic and Marvell (2003) can be found on the Internet at http://www.mmarvell.com/data.html. Studies examining the impact of SI laws on mass public shootings (Duwe, Kovandzic, & Moody, 2002) and police deaths (Mustard, 2001) are not included.

3. Lott and Mustard (1997) also examined the possibility that passage of SI laws would have differential effects on homicide rates for adults and juveniles. They find that passage of SI laws leads to reductions in homicide rates for both adults and juveniles. The authors argue that this evidence is not contradictory to the SI law efficacy hypothesis because (a) criminals may leave areas where adults carry concealed handguns, and thus all age groups benefit from the increase in permitted gun carrying by adults, and (b) gun-carrying adults can protect juveniles in violent confrontations when they are physically present. We are not persuaded by either of these claims.

4. An extensive examination of the county-level crime datasets by Marvell (1999) also revealed extreme measurement problems with the county-level crime datasets produced by the NACJD. When comparing the sum of the county crime data in states as compiled by the NACJD to the state totals reported in the FBI's *Crime in the United States*, which adjusts estimates when agencies fail to report, Marvell found the NACJD totals in 16 states to be off by at least 50% from 1982 to 1985 and off by 25% after 1985.

5. Because the coefficients for the city and year dummies are uninterpretable (i.e., they merely denote the presence of some unobserved time-stable feature of cities or unobserved factors affecting all cities equally in a given year), we do not include them in Table 1.

6. Each city has its own trend variable, which equals 1 in 1980, 2 in 1981, and 20 in 2000.

7. Because Maine, Montana, New Hampshire, West Virginia, and Wyoming did not have a city with a population of 100,000 or more in 1990, these laws were not evaluated.

8. The seven states that had SI laws or their equivalent prior to 1980 were coded 0 because the effect of the law is captured by the city dummy variable.

9. We realize that some readers might be uncomfortable with including prison population in the homicide regression because it induces simultaneity bias—that is, homicide rates might affect prison population levels and be affected by them. As Marvell and Moody (2001) note, however, this is unlikely to be the case because murderers make up only 14.6% of the overall prison population (U.S. Bureau of Justice Statistics, 2003). In any event, deleting prison population from the homicide regressions has no impact on the results presented in Table 1.

10. Results of these alternate model specifications are available upon request from the senior author.

11. We thank one of the anonymous reviewers for pointing this out to us.

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