comments were very helpful. I also tried in vain to ask pro-gun-control researchers what variables they wanted me to include in the regressions, but (as discussed in chapter 7) they did not make any suggestions when my initial research was circulated for comments. What comments they made after the publicity broke claimed that I had not controlled for factors that I had indeed accounted for.

Since the original research immediately received a lot of attention, I have let my critics decide for themselves what variables should be included by simply giving them complete access to the data. I know from personal communication that some critics (such as Black and Nagin) did indeed examine numerous different specifications.[15]

A more systematic, if time-consuming, approach is to try all possible combinations of these so-called control variables—factors which may be interesting but are included so that we can be sure of the importance of some other "focus" variables.[16] In my regressions to explain crime rates there are at least nine groups of control variables—population density, waiting periods and background checks, penalties for using guns in the commission of a crime, per-capita income, per-capita unemployment insurance payments, per-capita income maintenance payments, retirement payments per person for those over sixty-five, state poverty rate, and state unemployment rate.[17] To run all possible combinations of these nine groups of control variables requires 512 regressions. The regressions for murder rates also require a tenth control variable for the death-penalty execution rate and thus results in 1,024 combinations of control variables. Given the nine different crime categories, this amounts to 5,120 regressions.

This approach is decidedly biased toward not finding a consistent effect of the right-to-carry laws, because it includes many combinations of control variables that no researcher thinks are correct specifications. Indeed, even the strongest, best-accepted empirical relationships usually fail this test.[18] Since different people will have different preferences for what variables should be included, this massive set of results makes sense only if one knows what variables produce what results. If a range of conflicting estimates are then produced, people can judge for themselves what they think the "true" range of the estimates is.

Two sets of variables have been primarily used to test the impact of right-to-carry laws: crime trends before and after the adoption of right-to-carry laws and the percentage of people with permits. Yet another division is possible by focusing on counties with a large number of people to avoid the difficulty that low-population counties frequently have zero murder or rape rates and thus have "undefined" arrest rates.[19] Eliminating counties with fewer than 20,000 people removes about 70 percent of the missing arrest ratios for murder while sacrificing 20 percent of the observations (the population-weighted frequencies are 23 and 6 percent, respectively). Dropping out more populous counties reduces the sample size but has virtually no impact on further reducing the frequency of missing arrest rates. Even if I limit the estimates to the full sample and counties with more than 20,000 people, combining that with the two other types of specifications results in 20,480 regressions. Because of all the concerns over possible crime trends, all estimates include variables to account for the average differences across counties and years as well as by year within region as well as the thirty-six demographic variables.[20]

Figures 9.10–9.13 present the range of estimates associated with these different combinations of variables and specifications, both in terms of their extreme bounds and their median value. What immediately stands out when one examines all these estimates is how extremely consistent the violent-crime results are. For example, take figure 9.10. A one-percentage-point change in people with permits lowers violent-crime rates by 4.5–7.2 percent. Indeed, all the estimates (over two thousand of them) for overall violent crime, murder, rape, robbery, and aggravated assault indicate that increases in permits reduce crime. All the combinations of the other ten sets of control variables imply that a one-percentage-point increase in the population holding permits reduces murder rates by 2–3.9 percent annually. Compared to the state-level data, the benefits from right-to-carry laws are much smaller for robbery and much larger for aggravated assaults.

Figure 9.11 uses the simple before-and-after trends to examine the impact of the right-to-carry laws, and the results for the violent-crime rates are generally consistent with those shown in figure 9.10. Again, all the violent-crime-rate regressions show the same direction of impact from the concealed-handgun law. The median estimated declines in violent-crime rates are quite similar to those initially reported in table 9. 1. For each additional year that the right-to-carry laws are in effect, violent crimes decline by 2.4 percent, murders by 1.6 percent, rapes and aggravated assaults by over 3 percent, and robberies by 2.7 percent.

With the notable exception of burglaries, which consistently decline, figures 9.10 and 9.11 provide mixed evidence for whether right-to-carry laws increase or decrease other property crimes. Even when one focuses on estimates of one type, such as those using the percentage of the population with permits, the county- and state-level data yield inconsistent results. Yet,

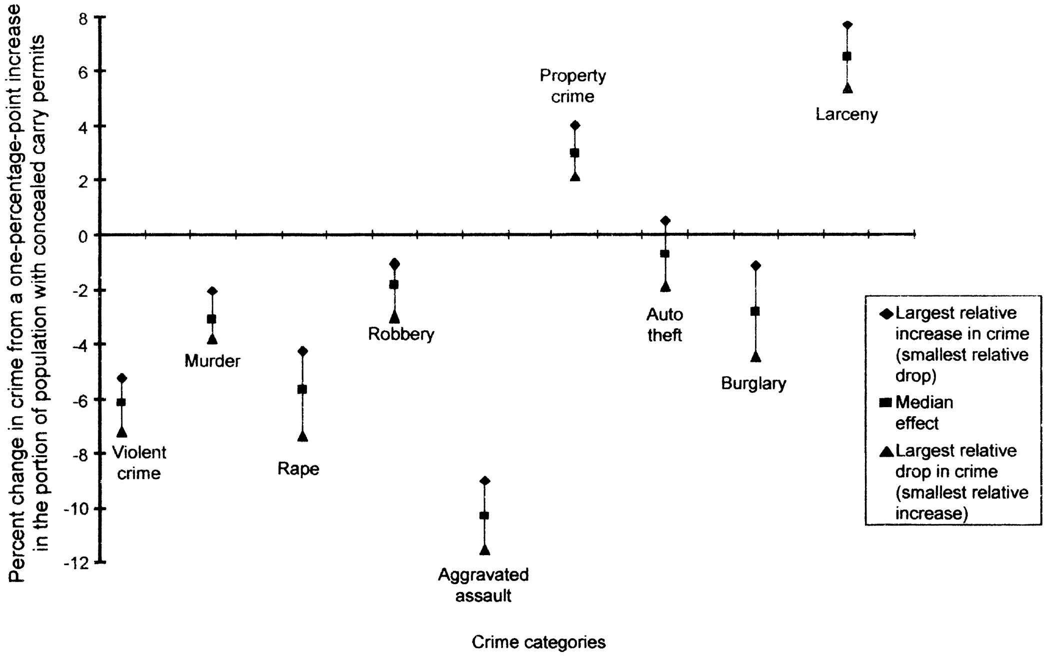

Figure 9.10. Sensitivity of the relationship between the percentage of the population with permits and annual changes in crime rates: data for all counties

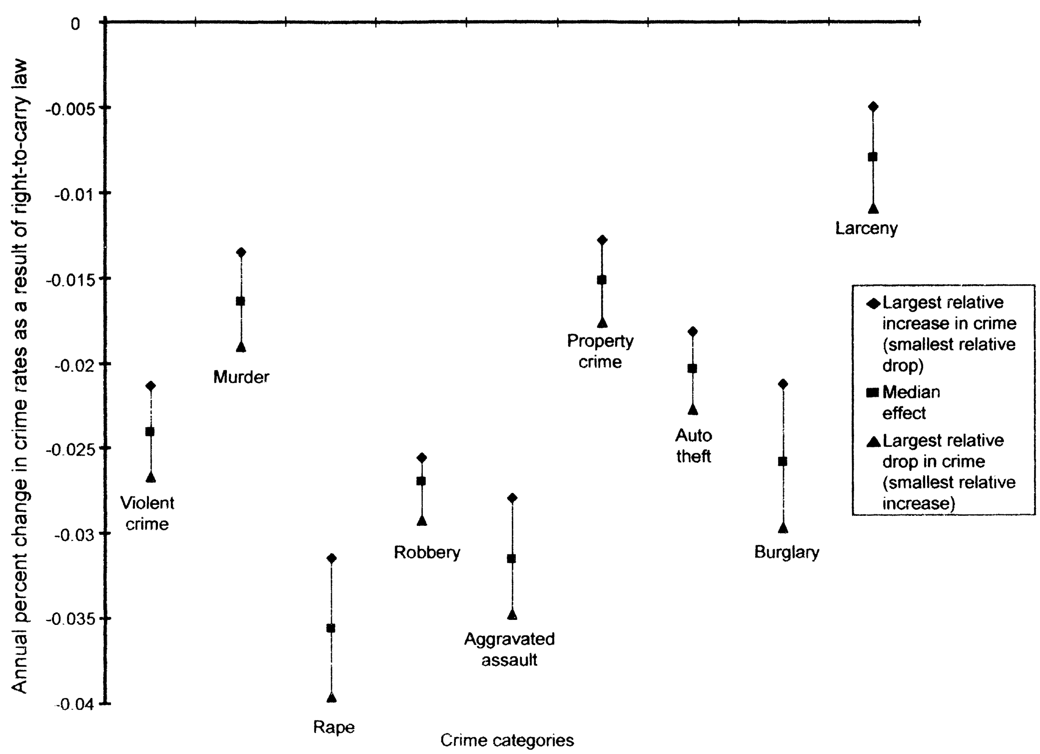

Figure 9.11. Sensitivity of the relationship between right-to-carry laws and annual changes in crime rates: data for all counties

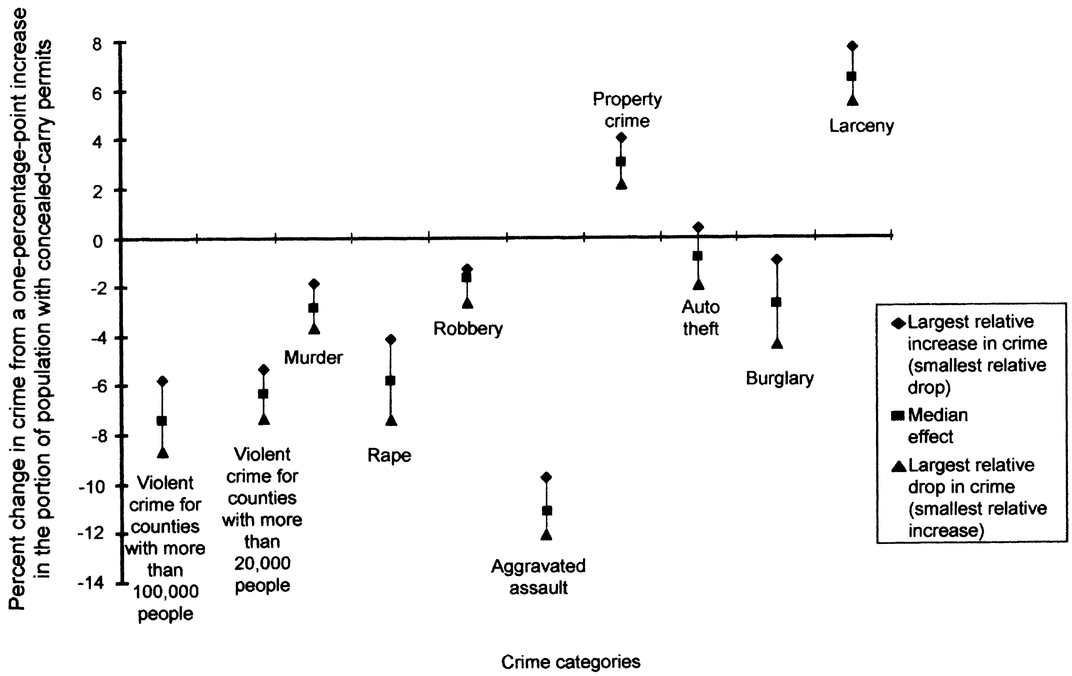

Figure 9.12. Sensitivity of the relationship between the percentage of the population with permits and annual changes in crime rates: data for counties with either more than 20,000 people or more than 100,000 people (all individual crime categories—that is, all categories except "violent crime"—are for counties with more than 20,000 people)

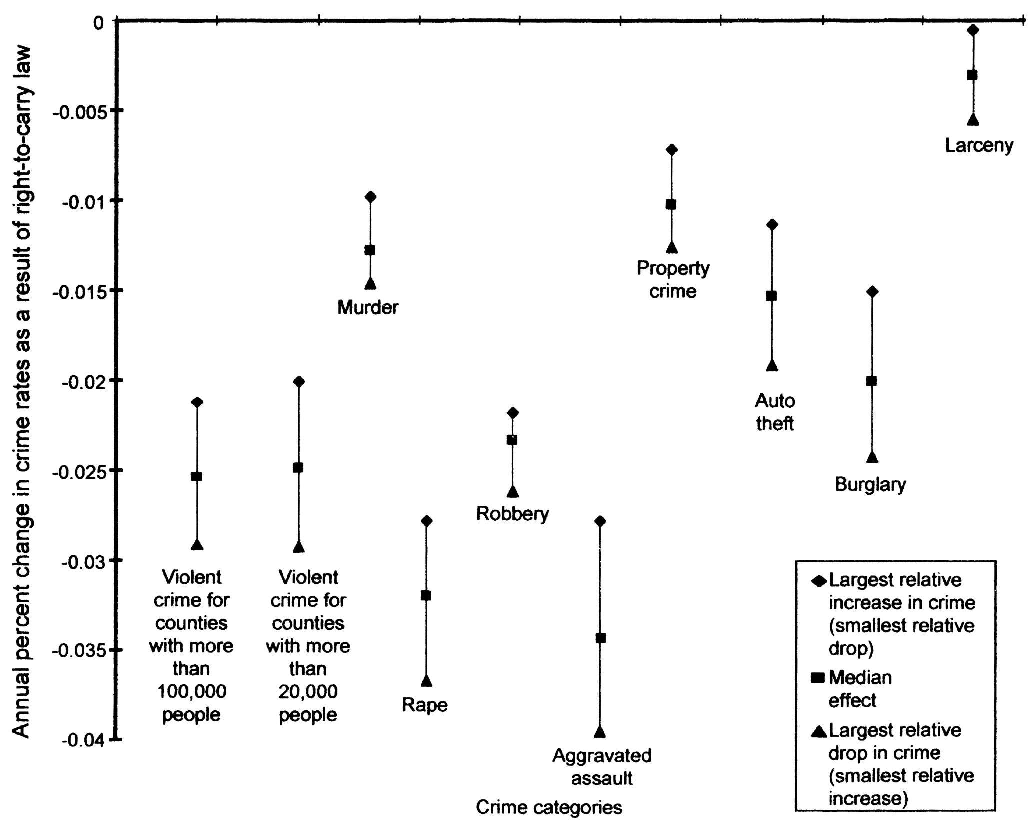

Figure 9.13. Sensitivity of the relationship between right-to-carry laws and annual changes in crime rates: data for counties with either more than 20,000 people or more than 100,000 people (all individual crime categories—that is, all categories except "violent crime"—are for counties with more than 20,000 people)

while the net effect of right-to-carry laws on larceny and auto theft is not clear, one conclusion can be drawn: the passage of right-to-carry laws has a consistently larger deterrent effect against violent crimes than property crimes and may even be associated with increases in property crimes.

Figures 9.12 and 9.13 limit the sample to the more populous counties and continue reaching very similar results. For counties with more than 20,000 people, the estimate ranges are always of the same sign and have magnitudes similar to those results which examined all the counties. Both figures also looked at the sensitivity of the overall violent-crime rate for counties over 100,000. The range of estimates was again very similar, though they implied a slightly larger benefit than for the more populous counties. For example, figure 9.12 shows that in counties with more than 20,000 people, violent crime declines by between 5.4 and 7.4 percentage points for each additional 1 percent of the population with permits, while the analogous drop for counties with more than 100,000 people is between 5.8 and 8.7 percentage points.

A total of 13,312 regressions for the various violent-crime categories are reported in this section. The evidence clearly indicates that right-to-carry laws are always associated with reductions in violent crime, and 89 percent of the results are statistically significant at least at the 1 percent level. The results are not sensitive to including particular control variables and always show that the benefits from these laws increase over time as more people obtain permits. The 8,192 regressions for property crime imply a less consistent relationship between right-to-carry laws and property crime, but even when drops in property crime are observed, the declines are smaller than the decrease in violent crime.

While limiting the sample size to only larger-population counties provides one possible method of dealing with "undefined" arrest rates, it has a serious drawback—information is lost by throwing out those counties with fewer than 20,000 people. Another approach is to control for either the violent- or property-crime arrest rate depending upon whether the crime rate being studied is that of violent or property crime. Even if a county has zero murders or rapes in a particular year, virtually all counties have at least some violent or property crime, thus eliminating the "undefined" arrest rate problem and still allowing us to account for county-level changes over time in the effectiveness of law enforcement. This approach also helps mitigate any spurious relationship between crime and arrest rates that might arise because the arrest rate is a function of the crime rate. Reestimating the 4,096 regressions in figure 9.10 for murder, rape, robbery, aggravated assault, auto theft, burglary, and larceny with this new measure of arrest rates again produces very similar results.

City Crime Data

County data, rather than city data, allow the entire country to be examined. This is important, since, obviously, not everyone lives in cities. Such data further allow us to deal with differences in how permits are issued, such as the discretion states grant to local law enforcement. Relying on county data allows a detailed analysis of many important factors, such as arrest and conviction rates, the number of police, expenditures on police, (sometimes) prison sentences, and proxies for policing policies like the so-called broken-windows strategy (according to which police focus on less serious property crimes as a means of reducing overall violent crime). Yet a drawback with county data is that policing policies cannot be dealt with well, for such policy decisions are made at the level of individual police departments—not at the county level.[21] With a few exceptions such as San Francisco, Philadelphia, and New York, where county and city boundaries coincide, only city-level data can be used to study these issues.

The focus of my research is guns and crime, but I had to make sure that I accounted for whatever policing policies are being employed.[22] Three policing strategies dominate the discussion: community-oriented policing, problem-oriented policing, and the broken-windows approach. While community-oriented policing is said to involve local community organizations directly in the policing effort, problem-oriented policing is sometimes viewed as a less intrusive version of the broken-windows policy. Problem-oriented policing began as directing patrols on the basis of identified crime patterns but nowadays involves the police in everything from cleaning housing projects and surveying their tenants to helping citizens design parking garages to reduce auto theft.[23] An extensive Westlaw database search was conducted to categorize which cities adopted which policing strategies as well as their adoption and rescission dates.[24]

Other recent research of mine demonstrates the importance of racial and gender hiring decrees on the effectiveness of police departments.[25] When hiring rules are changed so as to create equal pass rates on hiring exams across different racial groups—typically by replacing intelligence tests with what some claim are arbitrary psychological tests—the evidence indicates that the quality of new hires falls across the board. And the longer these new hiring policies are in place, the more detrimental the effect on police de-