Appendix A

Dissent

James Q. Wilson

The thrust of Chapter 6 of the committee's report is that studies purporting to show a relationship between right-to-carry (RTC) laws and crime rates are fragile. Though I am not an econometrician, I am struck by the fact that most studies of the effect of policy changes on crime rates are fragile in this sense: Different authors produce different results, and sometimes contradictory ones. This has been true of studies of the effect on crime rates of incapacitation (that is, taking criminals off the street), deterrence (that is, increasing the likelihood of conviction and imprisonment), and capital punishment. In my view, committees of the National Research Council that have dealt with these earlier studies have attempted, not simply to show that different authors have reached different conclusions, but to suggest which lines of inquiry, including data and models, are most likely to produce more robust results.

That has not happened here. Chapter 6 seeks to show that fragile results exist but not to indicate what research strategies might improve our understanding of the effects, if any, of RTC laws. To do the latter would require the committee to analyze carefully not only the studies by John Lott but those done by both his supporters and his critics. Here, only the work by Lott and his coauthors is subject to close analysis.

If this analysis of Lott's work showed that his findings are not supported by his data and models, then the conclusion that his results are fragile might be sufficient. But my reading of this chapter suggests that some of his results survive virtually every reanalysis done by the committee.

Lott argued that murder rates decline after the adoption of RTC laws even after allowing for the effect of other variables that affect crime rates.

The committee has confirmed this finding as is evident in its Tables 6-1, 6-2, 6-5 (first row), 6-6 (first row), and 6-7 (first two rows). This confirmation includes both the original data period (1977-1992) used by Lott and data that run through 2000. In view of the confirmation of the findings that shall-issue laws drive down the murder rate, it is hard for me to understand why these claims are called "fragile."

The only exceptions to this confirmation are, to me, quite puzzling. Tables 6-5 and 6-6 suggest that RTC laws have no effect on murder rates when no control variables are entered into the equations. These control variables (which include all of the social, demographic, and public policies other than RTC laws that might affect crime rates) are essential to understanding crime. Suppose Professor Jones wrote a paper saying that increasing the number of police in a city reduced the crime rate and Professor Smith wrote a rival paper saying that cities with few police officers have low crime rates. Suppose that neither Jones nor Smith used any control variables, such as income, unemployment, population density, or the frequency with which offenders are sent to prison in reaching their conclusions. *If* such papers were published, they would be rejected out of hand by the committee for the obvious reason that they failed to supply a complete account of the factors that affect the crime rate. One cannot explain crime rates just by observing the number of police in a city any more than one can explain them just by noting the existence of RTC laws.

It is not enough to say that it is hard to know the right set of control variables without calling into question the use of economics in analyzing public policy questions. All control variables are based on past studies and reasonable theories; any given selection is best evaluated by testing various controls in one's equations.

In addition, with only a few exceptions, the studies cited in Chapter 6, including those by Lott's critics, do not show that the passage of RTC laws drives the crime rates up (as might be the case if one supposed that newly armed people went about looking for someone to shoot). The direct evidence that such shooting sprees occur is nonexistent. The indirect evidence, as found in papers by Black and Nagin and Ayres and Donohue [cited in Chapter 6], is controversial. Indeed, the Ayres and Donohue paper shows that there was a "statistically significant downward shift in the trend" of the murder rate (Chapter 6, page 135). This suggests to me that for people interested in RTC laws, the best evidence we have is that they impose no costs but may confer benefits. That conclusion might be very useful to authorities who contemplate the enactment of RTC laws.

Finally, the committee suggests that extending the Lott model to include data through 2000 may show no effect on RTC laws on murder rates if one analyzes the data on a year-by-year basis (Table 6-7, rows three and four). I wish I knew enough econometrics to feel confident about this

argument, but I confess that at first blush it strikes me as implausible. To me, Lott's general argument is supported even though it is hard to assign its effect to a particular year. Estimating the effects of RTC laws by individual years reduces the number of observations and thus the likelihood of finding a statistically significant effect. It is possible that doing this is proper, but it strikes me that such an argument ought first to be tested in a peer-reviewed journal before it is used in this report as a sound strategy.

Even if the use of newer data calls into question the original Lott findings, a more reasonable conclusion is that Lott's findings depend on crime rate trends. The committee correctly notes that between 1977 and 1992 crime rates were rising rapidly while between 1993 and 1997 they were declining. Lott's original study was of the first time period. Suppose that his results are not as robust for the second period. The committee concludes that this shows that his model suffers from "specification errors" (page 141). Another and to me more plausible conclusion is that the effect of RTC laws on some crime rates is likely to be greater when those rates are rising than when they are falling. When crime rates are rising, public policy interventions (including deterrence, incapacitation, and RTC laws) are likely to make a difference because they create obstacles to the market and cultural forces that are driving crime rates up. But when crime rates are falling, such interventions may make less of a difference because they will be overwhelmed by market and cultural changes that make crime less attractive. This may or may not be a reasonable inference, but it is worthy of examination.

In sum, I find that the evidence presented by Lott and his supporters suggests that RTC laws do in fact help drive down the murder rate, though their effect on other crimes is ambiguous.

Appendix B
Committee Response to Wilson's Dissent

This response addresses Professor Wilson's dissent from one aspect of the committee report. It is important to stress at the outset that his dissent focuses on one part of one chapter of the report. Except for the effects of right-to-carry laws on homicide, the entire committee is in agreement on the material in Chapter 6 and the report overall. In particular, the committee, including Wilson, found that "it is impossible to draw strong conclusions from the existing literature on the causal impact" of right-to-carry laws on violent and property crime in general and rape, aggravated assault, auto theft, burglary, and larceny in particular.

The only substantive issue on which the committee differed is whether the existing research supports the conclusion that right-to-carry laws substantially reduce murder. The report suggests that the scientific evidence is inconclusive. Wilson disagreed, arguing that virtually every estimate shows a substantial and statistically significant negative effect of right-to-carry laws on murder.

While it is true that most of the reported estimates are negative, several are positive and many are statistically insignificant. In addition, when we use Lott's trend model but restrict the out years to five years or less (Table 6-7), the trends for murder become positive and those for other crimes remain negative. Therefore, the key question is how to reconcile the contrary findings or, conversely, how to explain why these particular positive, or negative, findings should be dismissed. Three sets of results discussed more fully in Chapter 6 provide support for the committee's conclusion: Published studies, the committee's analysis of control variables, and the committee's analysis extending the time period.

1. **Published studies.** There is no question that the empirical results on the effects of right-to-carry laws on murder (and other crimes) are sensitive to seemingly small variations in data and specification. Indeed, Wilson agrees that a few studies find positive effects of right-to-carry laws on murder. We cite four studies in Tables 6-3 and 6-4: Ayres and Donohue, Black and Nagin, Moody, and Plassmann and Tideman (cited in Chapter 6). There are almost certainly others not reported in these tables.

The rest of the committee and Wilson agree that fragility does not prove that the results of any specific paper are incorrect. However, some of the published results must be incorrect because they are inconsistent with one another. The important question, therefore, is whether the correct results can be identified. The rest of the committee thinks that they cannot. Contrary to Wilson's claim, the committee did assess the existing body of empirical literature on right-to-carry laws (see the section beginning on page 127 and Tables 6-3 and 6-4). As described in the report, all of the empirical research on right-to-carry laws relies on the same conceptual and methodological ideas (page 121). Relative to the basic models estimated by Lott, some researchers used data from more counties and some from fewer; some used hybrid linear models while others used nonlinear specifications; some provide state-specific estimates while most provide a single national estimate; some added control variables while others used relatively parsimonious specifications; and so forth. All of the studies described in the literature review made plausible cases for their choices of models and data. Wilson seems to argue that a careful evaluation of the literature would reveal which paper or papers obtained correct results, but he does not suggest the evaluation criteria. The rest of the committee does not think that application of any scientific criteria to existing papers would identify the effects of right-to-carry laws on crime.

2. Committee control variable analysis. Chapter 6 shows that when the trend and dummy variable models do not include demographic and socioeconomic covariates (but do include year and county dummy variables) the estimates are relatively small, positive in one case (Table 6-6, Row 3), and statistically insignificant in all cases. Contrary to Wilson's assertion, the chapter does not claim that this or any other specification is correct. Rather, this finding simply reveals that "detecting the effect, if any, of right-to-carry laws requires controlling for appropriate confounding variables." In light of the fragility revealed in the literature, the fundamental issue is which set of covariates is sufficient to identify the effects of right-to-carry laws on homicide and other crimes. The importance of controlling for the correct set of covariates is well known. In fact, much of the debate between Lott and his statistically oriented critics focuses on determining the correct set of control variables. Everyone (including Wilson and the rest of the committee) agrees that control variables matter, but there is disagree-

ment on the correct set. Thus, the facts that there is no way to statistically test for the correct specification and that researchers using reasonable specifications find different answers are highly relevant. Given the existing data and methods, the rest of the committee sees little hope of resolving this fundamental statistical problem.

Furthermore, the example of the relationship between crime rates and policing in the dissent raises another problem. The usual way one proceeds in research is to estimate the relationship between two variables and if a significant relationship is found controls are introduced to test the relationship. As the dissent notes, these controls are selected based on reasonable theories and research. In this case, the bivariate relationship (between right to carry laws and crime) is small, positive in one case, and insignificant in all. This is not like the hypothesized conflicting bivariate findings in Wilson's police example. Thus the selection of controls in the analysis of right-to-carry laws is as difficult as the committee contends

3. Committee trend model analysis. Wilson states that the trend model analysis in Table 6-7 estimates the effects of right-to-carry laws on a yearly basis, rather than a single trend.[1] This is incorrect. The estimates reported in Table 6-7 are found using Lott's trend model with restrictions on the number of postadoption years used in the analysis. If the model is correctly specified, this restriction should be inconsequential. However, we find substantial differences, especially for murder. In fact, when we restrict the number of postadoption years to five or fewer, the estimates switch from negative to positive. Thus, Model 6.2 appears to be misspecified. Moreover, despite Wilson's assertion, these types of sensitivity test are commonly used in peer-reviewed journals and are suggested by Rosenbaum (2001) as a way to assess the robustness of an empirical model. Of course, results like those reported in Chapter 6 might often lead a paper to be rejected from a peer-reviewed journal.

Wilson further suggests that Lott's findings may depend on the crime rate trends that changed dramatically over the course of the 1990s. All of the studies in this literature, however, attempt to control for trends in crime, and thus purport to reveal a time invariant effect of right-to-carry laws. If the effects vary by time, all of the existing models are misspecified.

In sum, we are encouraged that Professor Wilson agrees with the rest of the committee except for the specific conclusion regarding the effects of right-to-carry laws on murder. On this point, we find his arguments to be unconvincing and his summary of some parts of the chapter inaccurate. In our view the evidence on homicide is not noticeably different from that on other crimes evaluated in this literature and cannot be easily separated. If

[1]Contrary to Wilson's claim, the results in Table 6-7 all rely on models with covariates.

the effects of right-to-carry laws on violent and property crimes are ambiguous, as argued in Chapter 6, we see no reason why the same is not true of homicide. Professor Wilson may be correct on this matter—it is theoretically possible—but we maintain that the scientific evidence does not support his position.

REFERENCE

Rosenbaum, P.R.
2001 Replicating effects and biases. *American Statistician* 55(3):223-227.