THE CONCEALED-HANDGUN DEBATE

*JOHN R. LOTT, JR.**

ABSTRACT

Dan A. Black and Daniel S. Nagin state that my article with David Mustard assumes that the effect of concealed-handgun laws is constant over time, that the effect is the same across states, that the article does not control for local time trends, and that we did not investigate whether the results were sensitive to the missing values of the arrest rate. None of these claims are correct, and this is easily verified by anyone who reads the original article. Their statement that the results are sensitive to including Florida applies to fewer than 1 percent of the regressions that I have reported. Using results from previous drafts of Black and Nagin's comment as well as new estimates of my own, I provide additional evidence that allowing law-abiding citizens to carry concealed handguns deters criminals. Violent crime rates were rising before the law was passed and fell thereafter.

I. INTRODUCTION

ALLOWING law-abiding citizens to carry concealed handguns deters violent crime and saves lives. The more permits that are issued over time, the greater the decline in violent crime. The counties and states that issued the most new permits experienced the greatest drops. Violent crime rates rose until the point that nondiscretionary concealed-handgun laws were adopted and then fell after that point. The results are consistent across different samples.

The following sections will first address the claims that Dan A. Black and Daniel S. Nagin make about the assumptions in my article with David Mustard.[1] In Section III, I will also present new evidence on the effect of these laws across states and over time, the results' sensitivity to different

* John M. Olin Visiting Fellow in Law and Economics at the University of Chicago Law School. I would like to thank William Landes for providing an unusually large number of helpful comments. I would also like to thank Cindy Alexander, Gertrud Fremling, and Mark Ramseyer for their helpful comments.

[1] Dan A. Black and Daniel S. Nagin, *Do Right-to-Carry Laws Deter Violent Crime?* in this issue, at 209. I also refer to an October 16, 1996, draft, a December 6, 1996, draft, and a December 18, 1996, draft of the Black and Nagin comment in this issue. Unless noted otherwise, all references are to their comment in this issue. I will provide their drafts to those interested on request. John R. Lott, Jr., and David B. Mustard, *Crime, Deterrence, and Right-to-Carry Concealed Handguns,* 26 J Legal Stud 1 (January 1997).

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samples, the importance of differencing variables, and Black and Nagin's desire to exclude fixed county effects. Because of the public nature of my debate with Black and Nagin, I will not only reply to the criticisms in their comment but also discuss results from the paper they presented at a nationally televised debate that was sponsored by Handgun Control, Inc.[2]

II. What Was Really in the Original Article?

A. *Did Our Article Address the Issue of Missing Observations That Result from Undefined Values of the Arrest Rate?*

Black and Nagin claim that our original article failed to address potential problems that might arise because of missing values for the arrest rate.[3] Yet, in two places our article discussed this problem with county-level data and offered alternative tests to examine its potential effect on our results. For example, we wrote: ''The arrest rate data experience not only some missing observations but also instances where it is undefined when the crime rate in a county equals zero. . . . [In many of these] cases both the numerator and the denominator in the arrest rate are equal to zero, and it is not clear whether we should count this as an arrest rate equal to 100 or 0 percent, neither of which seems very plausible.''[4]

We then offered four solutions. (1) The first stage of the two-stage least squares estimates were used to create ''predicted arrest rates for these [missing] observations.''[5] (2) The results were reported without the arrest rate so that these observations were not excluded.[6] (3) Regressions were run on the larger counties (for example, with more than 10,000, 100,000, or 200,000 people, respectively) since those were less likely to exhibit this problem.[7] And, finally, (4) we argued that this might not be much of a problem because switching from discretionary to nondiscretionary concealed-handgun laws ''largely confirmed the preexisting practice in the lower population counties.''[8] Discussions with state law enforcement officials as well

[2] This exchange is recorded on C-SPAN for anyone who wishes to check. The program was originally broadcast on Monday, December 9, 1996, at 9:00 A.M., 2:30 P.M., and 9:30 P.M. References to the data Black and Nagin presented in the debate are to the December 6, 1996, draft of their paper.

[3] Black and Nagin, in this issue, at 211 (cited at note 1).

[4] Lott and Mustard, at 43, 48 (cited at note 1).

[5] Id at 48.

[6] Id at 18–19 n48.

[7] Id at 48, 35.

[8] While not explicitly discussed in terms of the missing arrest rate observations, other tests in our original article also shed light on the importance of these problems. For example, we discussed and reported using a moving average of the arrest rate over several years (id at 11). This test was motivated by other concerns, but even if a county had a murder or a rape

as the empirical work in our article indicated that in low-population counties discretionary laws (where it is up to either the local police or judges to determine whether someone has a ‘‘need’’ to have a permit) essentially amounted to nondiscretionary laws (where permits are automatically issued once a person meets certain objective criteria).[9] All four ways of dealing with missing observations consistently produced results supporting the hypothesis that concealed-handgun laws deter criminals.

The issue of missing arrest rate information was also not relevant for our state-level regressions.[10] While many counties do not experience a murder or a rape during a particular year, the state-level data, which produced even stronger evidence of deterrence, do not suffer from this problem since even the states with the fewest people experienced at least some crime.[11]

Yet, Black and Nagin claim that ‘‘the Lott and Mustard model excludes observations based on the realization of the dependent variable, potentially creating a substantial selection bias.’’[12] For some estimates this is true, but for others it is definitely not. The bottom line is that we could not find evidence that this affected the results, and since there were different advantages and disadvantages from using the different approaches, we presented them all.

Black and Nagin never acknowledge the solutions we proposed, let alone discuss what is wrong with them. Instead, they focus on only part of one approach (solution 3 above), and they leave a very misleading picture of the trade-offs involved with that approach. Since our regressions are weighted by population and since most of the missing observations are for the smallest counties, the weighted number of missing observations is much smaller than they report.[13]

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every few years, this eliminates the problem of a zero denominator. Indeed, doing this produced very similar evidence for the deterrent effect of concealed handguns.

[9] Id at 8, 31–34.

[10] For example, id at 26–27.

[11] Despite the large number of studies using state-level data, our study found strong evidence that there are serious aggregation problems with state-level data. However, the issue of missing arrest rate data was not a problem with state-level data.

[12] Black and Nagin, in this issue, at 211 (cited at note 1).

[13] In the December 18 draft of their comment, Black and Nagin have a footnote that admits this point: ‘‘Lott and Mustard weight their regression by the county’s population, and smaller counties are much more likely to have missing data than larger counties. When we weight the data by population, the frequencies of missing data are 11.7% for homicides, 5.6% for rapes, 2.8% for assaults, and 5% for robberies’’ (Black and Nagin, December 18, at 5 n4 (cited at note 1)). In discussing the sample with only counties over 100,000 people, they write in the same draft that ‘‘the (weighted) frequency of missing arrest ratios are 1.9% for homicides, 0.9% for rapes, 1.5% for assaults, and 0.9% for robberies’’ (id at 6). Restricting the sample to only those counties with more than 100,000 people thus reduces the portion of the remaining sample that is plagued by the missing observation by less than they suggest in their comment. While Black and Nagin do not use the violent crime category in their com-

Restricting the sample size also involves costs because it loses a lot of information. For example, dropping counties with fewer than 100,000 people reduces the number of observations for aggravated assault from 43,445 to 6,109 (an 86 percent drop and a 29 percent drop in the weighted frequency), even though the weighted frequency of missing arrest ratios is almost identical in the two samples. Even for murder, the sample is reduced from 26,458 to 6,009 counties, a 21 percent drop in the weighted sample size to obtain a less than 10 percent reduction in the weighted frequency of missing arrest rates. Why Black and Nagin focus on the 100,000 cutoff is neither explained nor obvious. The current cost-benefit ratio appears rather lopsided. Their additional step of dropping Florida, which has many large counties and relatively few missing observations, only exaggerates this problem. Eliminating counties with fewer than 20,000 people would have removed 70 percent of the missing arrest ratios for murder and lost only 20 percent of the observations (the weighted frequencies are 23 and 6 percent, respectively).

In contrast with their approach presented in their comment, Black and Nagin argued earlier at the Handgun Control, Inc., sponsored debate that the proper solution to the missing data problem was to ''concentrate our efforts on equations without the arrest ratio so that we may use all of the data.''[14] Indeed, although they claim that their current results are sensitive to the inclusion of the arrest rate, in the debate, they justified dropping the arrest rate because ''the absence or presence of the arrest ratio has little impact on the coefficient estimates in the model. Consequently, the inclusion of the arrest ratio in the model only has the undesirable effect of excluding values based on the realization of the dependent variable.''[15] Further, so as to differentiate their results from mine, they labeled the estimates with the arrest rate removed the ''full sample'' estimates. Of course, this ignores the fact that we also reported results without the arrest rate.[16]

ment, it is the one county-level measure of violent crime that is the least effected by this missing observation problem.

[14] Black and Nagin, December 6, at 5 (cited at note 1).

[15] Id at 5. This claim about arrest rates can be consistently found in Black and Nagin's various drafts previous to the Handgun Control, Inc., debate. See, for example, Black and Nagin, October 16, at 5 (cited at note 1): ''[T]he inclusion of the arrest rate variable itself has very little impact on the coefficient estimates of the right-to-carry laws. In what follows, we shall present results for two specifications. First we include the arrest ratio, which uses the same sample as Lott and Mustard. Second, we also exclude the arrest ratio, which allows us to use the full sample.'' The evidence excluding the arrest ratio is very similar to the graphs that I report in Figure 2 of this article.

[16] Lott and Mustard, at 18–19 n48 (cited at note 1).

B. ''The Lott and Mustard model makes two restrictive identification assumptions. . . . the model assumes that [right-to-carry] laws have an impact on crime rates that is constant over time.''[17]

With the exception of one footnote, Black and Nagin's current discussion of our ''model'' focuses solely on table 3 in the original article. The same can be said for their equation 1, which they claim identifies the approach we used. While table 3 does indeed rely on a simple dummy variable to measure whether the law is in effect, we also considered other more complicated specifications. Indeed, we emphasized that table 3 was biased toward not finding an effect from discretionary concealed-handgun laws because the law dummy variable implied that the law affected all counties in the state equally, even though there was other evidence indicating that the laws did not alter the issuing of permits in the more rural, lower-population counties.

If criminals respond to the probability that a potential victim is carrying a concealed handgun, the deterrent effect of concealed-handgun laws should be related to the number of concealed handguns being carried. While data on the actual rate of carrying guns are not available, it takes many years before the number of permits reaches its long-run level. To correct any misimpressions that Black and Nagin had about our specifications, at the Handgun Control, Inc., sponsored debate I read the following quote from the article: ''Perhaps not surprisingly, the results imply that an additional change was spread out over time, possibly because concealed-handgun use did not instantly move to its new steady-state level (for example, in 1994, Oregon permits increased by 50 percent and Pennsylvania's by 16 percent even though both ordinances had been in effect for at least 4 years).''[18]

Because the actual number of permits issued was not available for most states over time, two alternative approaches were possible. First, we included time and time-squared variables for the number of years after the law had been in effect, along with a similar set of time trends for before the law went into effect, and replicated the regressions reported in tables 3 and 7 in our original article. Besides controlling for the various demographic and income variables, arrest rates, unemployment, population density, and so forth, these regressions also accounted for fixed county and year effects.[19] As demonstrated in figure 1 in our original article, these estimated

[17] Black and Nagin, in this issue, at 213 (cited at note 1).

[18] Lott and Mustard, at 34 (cited at note 1).

[19] Other regressions controlled for cocaine prices and other types of gun laws, such as waiting periods, background checks, and penalties for using guns in a commission of a crime.

time trends confirmed that crime rates were rising before the law went into effect and falling afterward, with the effect increasing as more years went by.[20]

Second, for two states, Oregon and Pennsylvania, it was possible to obtain the number of permits issued by county over many years.[21] Running the regressions for each state separately, the results indicated that the size of the drop in the murder rates was closely related to the number of permits issued, though the results for Oregon were only statistically significant at the 11 percent level for a two-tailed *t*-test. The more permits issued, the greater the deterrence effect from concealed-handgun laws. If the data for the two states had been pooled, the effect on murder would have been statistically significant at the 5 percent level.

Black and Nagin take a different approach to examining the effect of the law over time. They use a series of dummy variables: one that examines the crime rate 5 years before the law goes into effect, another dummy for 4 years before the law goes into effect, and so on until a dummy variable for 5 or more years after the law goes into effect is reached. Their earlier estimates differ from those produced in their comment in two ways: they used what they termed the ''full sample'' by excluding the arrest rate from the regressions and ran the regression on levels with fixed county effects.

Figure 1 shown here uses their results to clearly demonstrate that violent crime rates were rising or flat until the point where the law went into effect and then falling after that point. The figures graph out the coefficient values for murder, rape, aggravated assault, and robbery as shown in table 3 in Black and Nagin's December 6, 1996, draft. (Black and Nagin do not report the violent crime rate, and I have been unable to completely replicate their results, so the violent crime graph is the weighted average of the other crime regression coefficients, where the weight is the different crime categories share of total violent crimes.) The largest drops occurred after the laws had been in effect for a while, which is consistent with our explanation of how the issuing of permits changes over time. Despite the change in sample size and the exclusion of the arrest rate variable, the results are very similar to what we obtained. Figure 2 in this article provides the graphs using the before and after time trends that were described but not shown in our original article (the interested reader is referred to fig. 1 in our original article for the graph for violent crimes).

Before I had been shown the time pattern that was produced by Black and Nagin's December 6, 1996, estimates, they claimed that the results pre-

[20] Lott and Mustard, at 35 (cited at note 1).

[21] Id at 51–59.

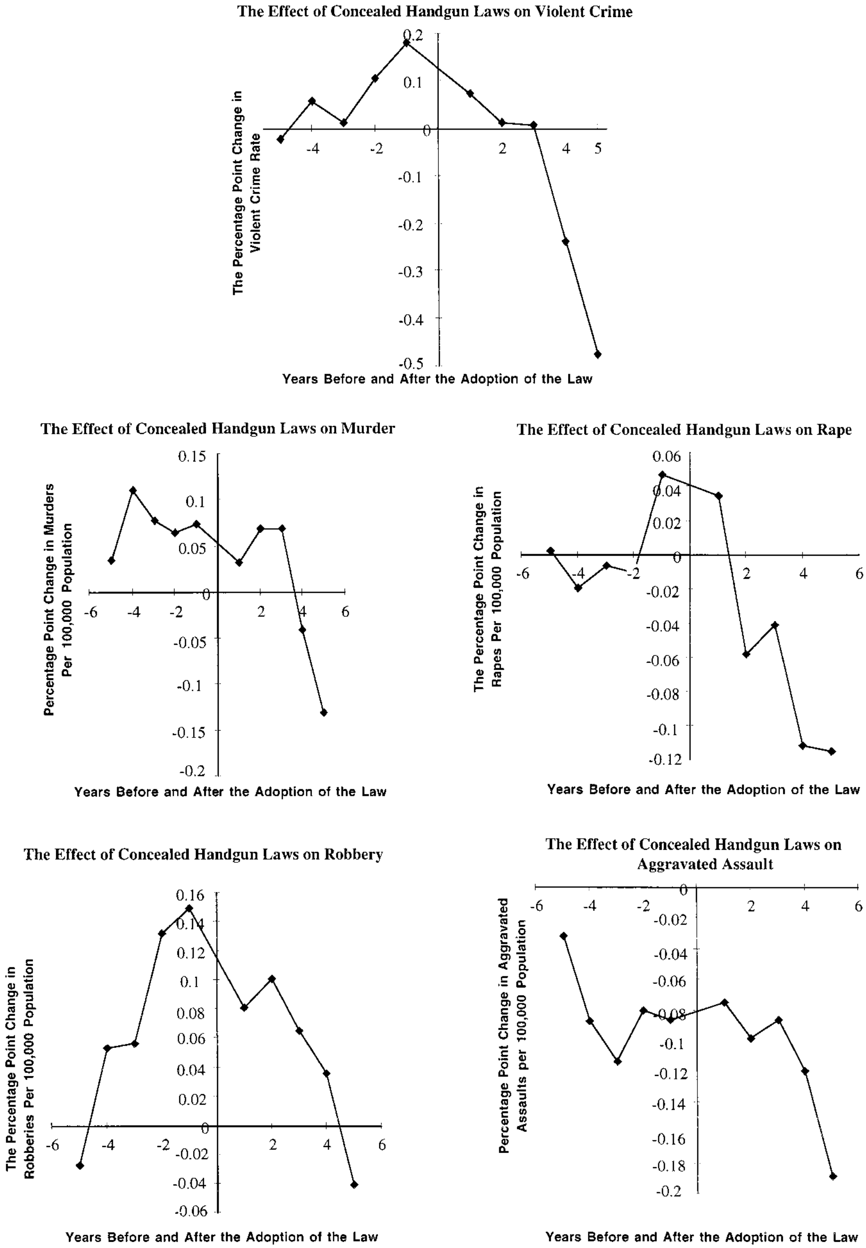

FIGURE 1.—Using Black and Nagin's coefficient estimates from their December 6, 1996, draft (cited at note 1) to study the effect of concealed-handgun laws over time. Regressions exclude the arrest rate to avoid missing observations. Year 1 is the first year that the law goes into effect. Year −1 is the year immediately before the law goes into effect. There is no year between years −1 and 1, and thus there is no year ''zero'' in their setup.

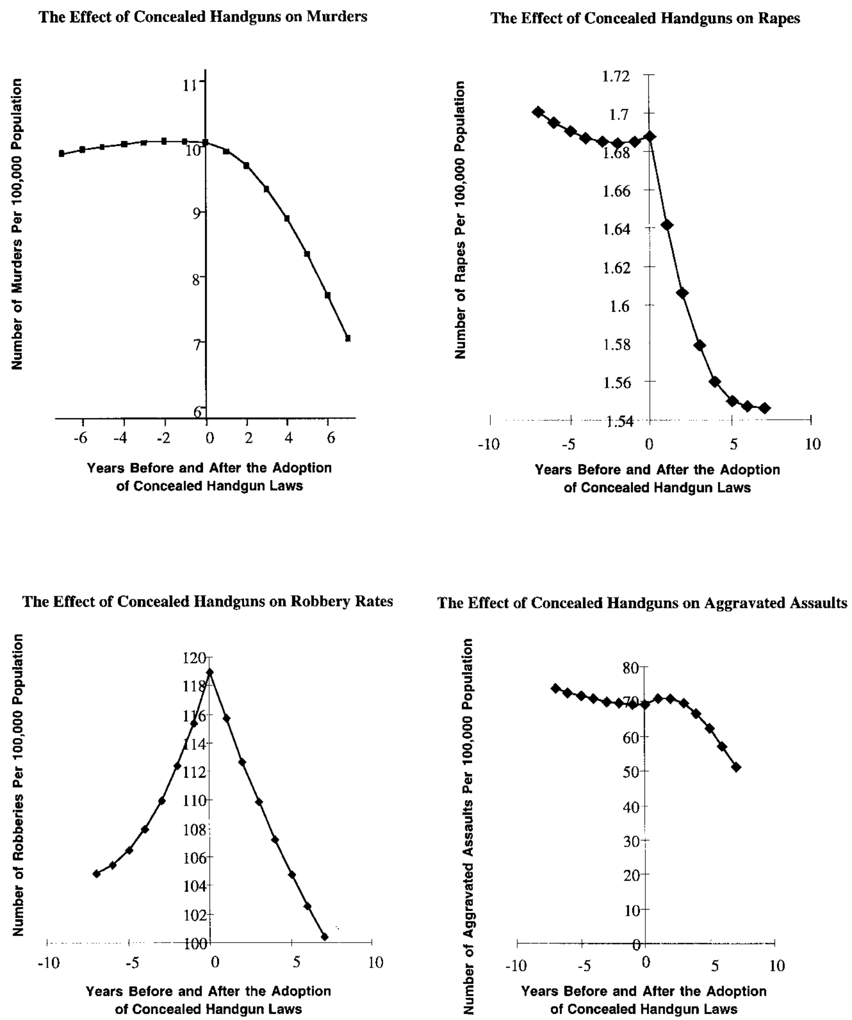

FIGURE 2.—The results from our original article (Lott and Mustard (cited at note 1)). The violent crime graph was already reported in the original article.

sented in Figure 1 of this article proved that concealed-handgun laws had no effect on crime because they were insistent on comparing the coefficients 2–3 years after the law went into effect with those 2–3 years before the law went into effect. Not surprisingly, if crime is rising before the enactment of a law and falling thereafter, it is possible to pick years on either side of the enactment that have similar mean values.

C. ''The results suggest that the Lott and Mustard model, which includes only a single national trend, *does not adequately capture local time trends in crime rates'' (emphasis added).*[22]

In fact, different attempts were made to control for individual state and county time trends, and they resulted in similar or even stronger estimates of the deterrence effect of concealed handguns. For example, during the Handgun Control, Inc., debate I pointed out that the original article states that ''[w]e reran the specifications shown in Table 3 by also including state dummies which were each interacted with a time trend variable. . . . Under this specification, adopting concealed handgun laws in those states currently without them would have reduced 1992 murders by 1,839, rapes by 3,727.''[23]

In our original article, we also studied the effect of differencing all the variables and at the same time controlling for county fixed effects.[24] The regressions thus allowed for a separate time trend for each county (the county fixed effect measures the average rate of change for a county during the sample). The two different types of dummy variables measured whether the concealed-handgun law had either a temporary or a permanent effect in the rate that crime rates changed over time. For violent crimes and for murder, the initial effect of the law reduced crime, but the long-term change was even greater. Black and Nagin accuse me of not controlling for ''local time trends,'' yet their own differences regressions do not include county fixed effects.

Our third approach used crime rate categories to help explain the changes over time in the other violent or property crime rates in a county.[25] These crime rates were used to proxy for changes in the criminal justice system that were not being picked up by our other measures. The results shown in table 8 of our original article produced among the strongest evidence of the deterrent effect from concealed handguns.

After claiming that we had not dealt with state-specific time trends, Black and Nagin include a quadratic time trend for each state. Unlike our use of a state-specific linear time trend, there are problems with using state-specific quadratic trends. Suppose, for example, that the crime rate for each state followed the pattern that Black and Nagin found in their December 6, 1996, draft and that I found in our original article (see the figures in this article),

[22] Black and Nagin, in this issue, at 218 (cited at note 1).

[23] Lott and Mustard, at 25 n53 (cited at note 1). The use of state-specific time trends was also mentioned. See id at 18 n46.

[24] Id at 34–37 & table 9.

[25] Id at 31–34.

where crime rates were rising until the law went into effect and falling thereafter. Allowing a separate quadratic time trend for each state results in the time trend picking up *both* the upward path before the law and the downward path thereafter. If the different state crime patterns all peaked in the year that their state law went into effect, the state-specific quadratic trends would account for all the effect of the law. Rather than interpreting the law dummy as picking up whether the law raised or lowered the crime rate, the dummy must be interpreted as whether the law raised or lowered the crime rate as quickly as that implied by the quadratic time trend.

D. ''The Lott and Mustard model . . . assumes the impact is the same across all 10 states that passed [right-to-carry] laws in the period from 1977 to 1992.''[26]

As with Black and Nagin's other claims, the above statement would only have been true if our original article had ended with table 3. However, just as we did not expect the effect of the laws to remain constant over time as citizens obtained more permits, we did not expect the effect to be the same across all states or even all counties. The reason why most of our regressions interacted the law dummy with the county population was because state law enforcement officials had continually stated that the law made the biggest difference in the largest population counties, which had been the most restrictive on gun permits. The individual state data for Oregon and Pennsylvania were also used for this same reason, and these individual state data sets were run separately so as to allow for any possible differences across states.

E. Should Florida Be Excluded from the Sample Because of the Mariel Boat Lift of 1980, Florida's Volatile Crime Rates, and the Passage of Several Other Gun Laws in 1991?

It can be interesting to see if the results are driven by an individual state, and indeed many gun control advocates claimed (incorrectly) that our results were sensitive to the inclusion of Maine and/or Virginia in the sample.[27] The same is also true for the effect of excluding Florida. Figure 3 in this article regraphs figure 1 from the original article, showing the relationship between violent crime rates and concealed-handgun laws, but this time excludes Florida from the sample. A careful comparison with our earlier graph produces only a few, very small differences.

[26] Black and Nagin, in this issue, at 213 (cited at note 1).

[27] For example, see Lott and Mustard, at 19 n49, 12 nn33 & 34 (cited at note 1).

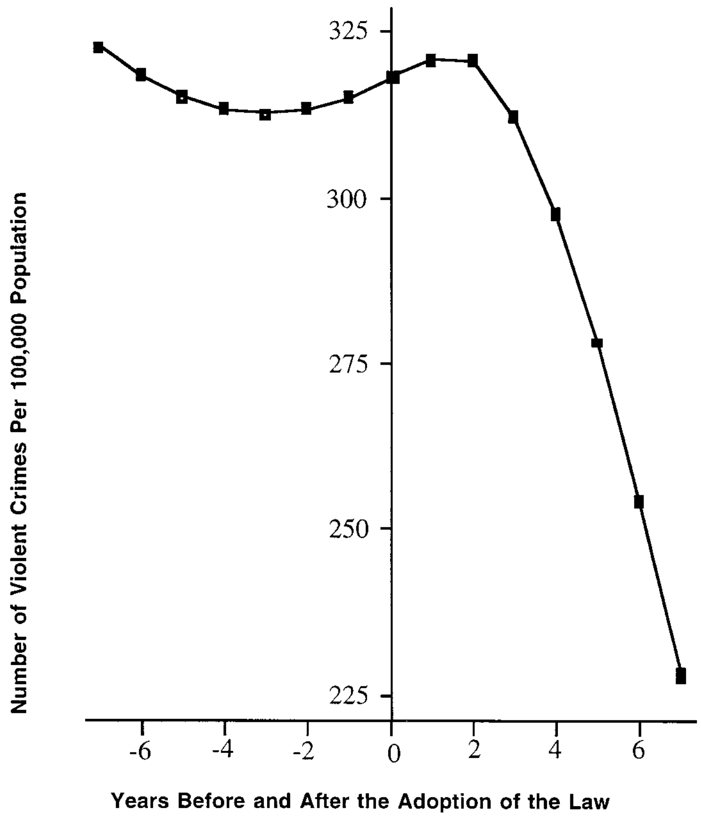

FIGURE 3.—The effect of concealed-handgun laws on violent crimes excluding Florida

As a more systematic response to this concern, I excluded Florida and reestimated all the regressions shown in the original article and in my forthcoming book.[28] In only eight out of the nearly 1,000 regressions discussed did the exclusion of Florida cause the coefficient for the nondiscretionary variable to remain negative but to lose its significance. The regressions that were most sensitive to deleting Florida were the murder and rape regressions in table 3 and the murder regressions that corresponded to table 7 as well as the corresponding regressions in a couple of related footnotes in our original article. The rest of the regression estimates either remained unchanged or (especially for aggravated assault and robbery) became larger and more statistically significant. Yet, even if a result is statistically sig-

[28] John R. Lott, Jr., *More Guns, Less Crime: Understanding Crime and Gun Control Laws* (1998, in press).

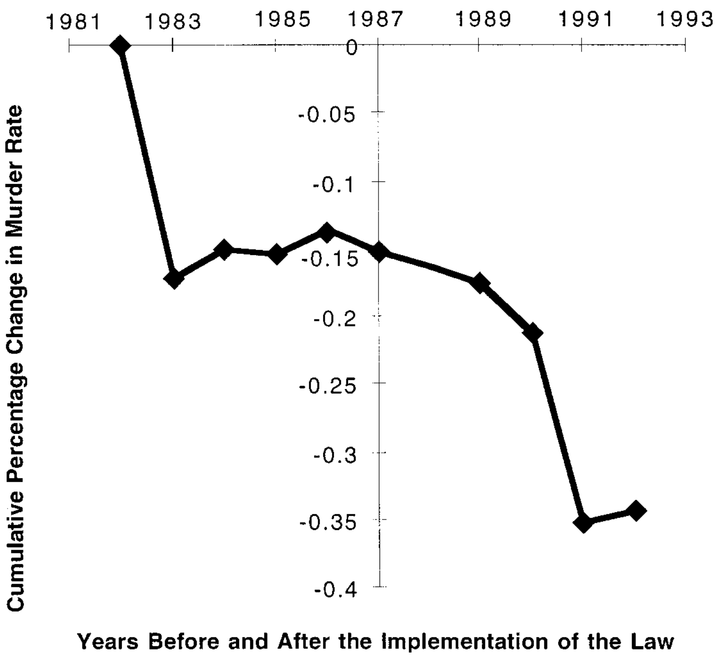

FIGURE 4.—Cumulative percentage change in Florida's murder rate, examining the explanation for removing Florida from the data set.

nificant at the 1 percent level, one would expect that one out of every 100 regressions would not find a statistically significant result—out of 1,000 regressions one would expect to find at least 10 where the effect from nondiscretionary concealed-handgun laws was not statistically significant.

However, despite the legitimate interest in seeing whether the results are sensitive to inclusion of a single state, the reasons given by Black and Nagin for excluding Florida are factually wrong. Figure 4 in this article depicts the murder rate in Florida from the early 1980s until 1992. The Mariel boat lift did dramatically raise violent crime rates like murder, but these rates had returned to their pre-Mariel levels by the early 1980s. For murder, the rate was extremely stable until the concealed-handgun law passed in 1987, when it began to drop dramatically.

The claim that Florida should be removed from the data because a waiting period and a background check went into effect in 1992 is even weaker. If this were a valid reason for exclusion, why not exclude other states with these laws as well? Why only remove Florida? For example, 17 other states had waiting periods in 1992. An even more valid response would be to try to account for the effect of these other laws—as we did in table 10 in our original article.[29] Indeed, accounting for these other laws slightly strengthens the evidence that concealed handguns deter crime.

The diagram for Florida, Figure 4 in this article, also produces other interesting implications. The murder rate declined in each consecutive year following the implementation of the concealed-handgun law until 1992, the first year that these other much touted gun control laws went into effect.[30] I am not claiming that these other laws caused murder rates to rise, but Figure 4 makes it more difficult to argue that the drop in Florida's murder rate was due to restrictions on the ability of law-abiding citizens to own guns. In any case, Black and Nagin's concern that our previous results may have been accidentally picking up a drop in Florida's crime rates that was really due to these other laws seems dubious. This is especially true since we directly controlled for these other laws.

III. The Evidence

Black and Nagin conclude that allowing law-abiding citizens to carry concealed handguns has ''no significant impact for any type of violent crime.''[31] Before I proceed further here though, a couple of general observations should be made about their approach. Their first notable omission is that, despite their title's emphasis on ''violent crime,'' the overall violent crime category is absent from any of their results. Their only justification for this omission is to cite one critical argument in our article, even though we included this to measure changes in the total amount of crime and because the violent crime rate category is missing very few observations for the arrest rate. When I have tried to replicate their work using the violent crime rate, the findings are dramatic: for example, in the state-specific effect of right-to-carry law regressions in table 1 of their comment, nine of the 10 states experienced declines in total violent crimes as a result of the law, and six of these reductions were statistically significant. It would have been a little difficult to answer ''no'' to the question, ''Do right-to-carry laws deter

[29] Lott and Mustard, at 38–41 (cited at note 1).

[30] Black and Nagin write that ''further, 4 years after its 1987 passage of the [right-to-carry] law, Florida passed several other gun-related measures, including background checks of handgun buyers and a waiting period for handgun purchases'' (Black and Nagin, in this issue, at 214 (cited at note 1)). While it is true that these laws were passed in 1991, they did not go into effect until 1992. In addition, the Florida law was not a pure waiting period in the normal sense. The only time restriction mentioned in the law was that the background check was to take ''no more than 3 days.'' In practice, this background check regularly takes less than a day to complete, and as county police departments are becoming computerized, the length of time for the check is becoming even shorter.

[31] Black and Nagin, in this issue, at 209 (cited at note 1).

violent crime?'' when their own regressions implied that overall violent crime was declining.[32]

A. Different Effects across States

The first type of evidence presented by Black and Nagin involves the differential effect that concealed-handgun laws have across states. Table 1 presented here reexamines the many regressions in our original article where we took into account that concealed-handgun laws have different effects across counties on the basis of how lenient officials were in issuing permits under a previous discretionary system. The one change from our original article is that a different dummy variable is used for the counties in each of the 10 states that changed their laws during the 1977 to 1992 period. At least for violent crimes, the results indicate a very consistent effect of nondiscretionary concealed-handgun laws across states. This is true for both the first column of Table 1, which uses only counties with more than 100,000 people during the sample period, and the second column, which runs the estimates over the entire sample for which data is available. In both of the first two columns, nine of the 10 states saw violent crimes decline as a result of these laws, and the effect is statistically significant for six states in the over 100,000 population counties and for eight states when using the sample employed in our original article's table 3.

Using the table 3 sample, the next four columns of Table 1 show that eight of the 10 states experienced falls in murder rates, six experience drops in rapes and robberies, and nine had drops in aggravated assaults. The increases were very small in the states where violent crimes, murders, or robberies rose. In fact, the largest increases were smaller than the smallest declines in the states where those crime rates fell. Similar results were obtained for the various violent crime categories when only the over 100,000 population counties were used.

In general, the states with the largest decreases in any one category tended to have relatively large decreases across all the violent crime categories, although the ''leader'' in each category varied across all the violent crime categories. Likewise, the states with relatively small crime decreases (for example, Georgia, Oregon, Pennsylvania, and Virginia) tended to ex-

[32] A second very minor point is that in their comment Black and Nagin (contrary to their previous work) set the standard so that something is only considered statistically significant at the 5 percent level for a two-tailed *t*-test. Their previous drafts used the 10 percent level for a two-tailed *t*-test. In their table 1, if the 10 percent level were adopted as the standard, five of the negative coefficients would now be listed as statistically significant, and this change would not effect any of the positive coefficients (Black and Nagin, in this issue, at table 1 (cited at note 1)).

hibit little change across all the categories. (The main exception was West Virginia, which showed large changes in murder, but not in other crime categories.)

Column 7 of Table 1 examines the violent crime category omitted from Black and Nagin's analysis of violent crimes. When only counties over 100,000 people and the simple nondiscretionary concealed-handgun-law dummy were used, nine of the 10 states experienced a drop in violent crimes, and in six of these cases, the drop was statistically significant at least at the 6 percent level for a two-tailed *t*-test. Column 8 shows the same test for the larger sample and finds that total violent crime declined in eight states after the law went into effect, and seven of these cases were statistically significant at the 10 percent level.

Property crime statistics (not shown but available from me on request) exhibited no clear pattern. In five states property crimes fell, in five states they increased, and the size of any decrease or increase was quite small and unsystematic.

One cautionary point should be raised about estimates relying only on these large-population counties. Counties with more than 100,000 people are rare in some states, so it can be misleading to label estimates from these counties as representing what is happening in these states. For example, Black and Nagin discuss the results for West Virginia, yet in West Virginia they have examined only one single county—Kanawha. The other 54 counties in West Virginia, with 89 percent of the state's population, were excluded from their estimates.[33]

B. ''Intertemporal Aggregation''

Figure 5 in this article illustrates earlier estimates provided by Black and Nagin for counties with over 100,000 people when the regressions are run on levels, with fixed county effects.[34] Figure 5 is calculated using the weighted average of the coefficients for the different crime categories (as was done for the violent crime category illustrated in Figure 1 in this article).[35] Despite excluding most of the sample, the pattern is still similar to what we reported in our original figure 1 and the other figures shown in

[33] It is surely more dramatic to claim that murder rates went up in an entire state than to claim that they have identified one county out of 54 where it rose. Black and Nagin are quoted in the *Washington Post* as claiming that they ''found the annual murder rate did go down in six of the 10 states—but it went up in the other four, including a 100 percent increase in West Virginia'' (Richard Morin, *Do Guns Prevent Crime? Another Look,* Wash Post (March 23, 1997), at C5).

[34] Black and Nagin, December 6 (cited at note 1).

[35] They did not run the regressions for violent crime, and the estimates that I obtain more strongly support the deterrence hypothesis.

TABLE 1

STATE-SPECIFIC EFFECT OF RIGHT-TO-CARRY LAWS ON VIOLENT CRIME RATES

	Percent of Specifications Interacting the Nondiscretionary Right-to-Carry Law Dummy with County Population						Percent of Specifications Using a Simple Dummy Variable	
State	Violent Crimes following Black and Nagin—Only Using Counties with More than 100,000 People (1)	Violent Crime (2)	Murder (3)	Rape (4)	Aggravated Assault (5)	Robbery (6)	Violent Crimes following Black and Nagin—Only Using Counties with More than 100,000 People (7)	Violent Crimes Based on Lott and Mustard's Table 3 (8)
Florida	−10	−4	−10	−8	−4	.3	−1.5	−1
	(2.219)	(3.914)	(8.959)	(3.967)	(4.956)	(.771)	(.489)	(.480)
Georgia	.2	.2	−2	.5	−.2	0	.02	10
	(.487)	(.792)	(.373)	(1.364)	(.421)	(.107)	(.451)	(4.00)
Idaho	−3.6	−3	−1	.1	−3	−7	−34	−17
	(1.923)	(2.582)	(.676)	(.099)	(1.856)	(4.063)	(1.932)	(2.78)
Maine	−15.5	−17	−5	1	−24	−8	−42	−42
	(4.779)	(8.005)	(1.439)	(.372)	(9.166)	(2.561)	(5.162)	(9.63)

Mississippi	−1.5	−3	.6	3	−8	0	−12	−19
	(.849)	(2.367)	(.334)	(2.663)	(5.838)	(.018)	(2.034)	(4.365)
Montana	−7.4	−10	−5	−10	−12	−6	−61	−52
	(2.155)	(6.073)	(1.495)	(4.818)	(5.978)	(2.33)	(2.187)	(6.397)
Oregon	−2.2	−3	−1	−1	−3	−4	−13	−22
	(1.901)	(3.127)	(.678)	(1.318)	(3.032)	(.370)	(2.13)	(6.043)
Pennsylvania	−1.2	−1	−3	−1	1	−2	−1	3
	(1.548)	(2.283)	(2.413)	(1.348)	(1.278)	(2.364)	(.466)	(1.497)
Virginia	−1.7	−2	−3	−1	−2	−2	−9	−8
	(3.969)	(6.136)	(3.178)	(2.847)	(6.453)	(5.442)	(2.052)	(3.449)
West Virginia	−.2	−1	11	−5	−1	1	−2	−7
	(.085)	(.599)	(4.356)	(2.646)	(.035)	(.479)	(.108)	(1.712)
Number of observations	6,628	43,445	26,457	33,862	43,440	34,947	6,628	43,445
Summary of the coefficients' signs:								
Number negative	9	9	8	6	9	6	9	8
Number of these that are significant	(6)	(8)	(3)	(4)	(7)	(5)	(6)	(7)
Number positive	1	1	2	4	1	4	1	2
Number of these that are significant	(0)	(0)	(1)	(1)	(0)	(0)	(0)	(1)

NOTE.—I used arrest rates and adjusted for which of those counties adopting nondiscretionary right-to-carry laws are most likely to represent a real change from past practice by multiplying the shall issue law variables with the population in each county. The percentages are evaluated at the mean county population. Absolute *t*-statistics are shown in parentheses.

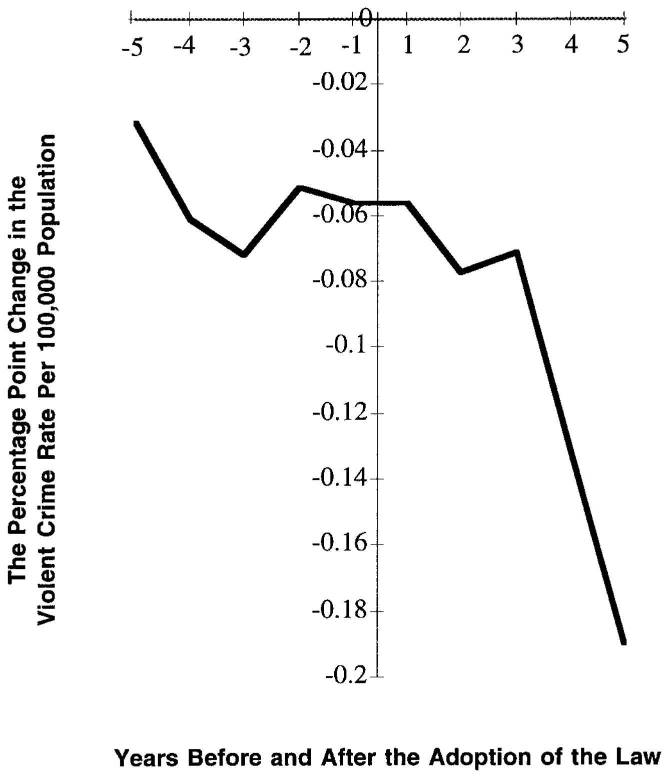

FIGURE 5.—The effect of concealed-handgun laws over time, using Black and Nagin's December 6, 1996, estimates for counties with over 100,000 population. Year 1 is the first year that the law goes into effect. Year −1 is the year immediately before the law goes into effect. There is no year between years −1 and 1, and thus there is no year ''zero'' in their setup.

this article. The violent crime rate appears to rise briefly immediately before the implementation of the law and then—after momentarily hesitating—it declines dramatically. The figures for the individual crime categories display a similar pattern.[36]

As noted earlier, our original article also studied the effect of differencing all the variables. The results all confirmed the deterrence effect of concealed handguns on crime. Unlike Black and Nagin's regressions, our regressions in the original article controlled for county fixed effects and used simple dummy variables for the immediate and long-term changes in the

[36] I am happy to provide these figures for those who are interested.

various crime rates. We also did not limit ourselves to only counties with over 100,000 people.

Table 2 in this article provides another simple test of the trends in violent crime rates before and after the implementation of the nondiscretionary concealed-handgun laws. As was done in table 9 in our original article, the endogenous and exogenous variables are all differenced. The primary change from my previous estimates using differences is that these regressions employ a separate time trend for the years immediately before and after the implementation of the law. In addition, three different sample sizes are examined: the complete sample using the arrest rate, the sample using the arrest rate and counties with over 100,000 people, and the so-called full sample with the arrest rate excluded.

It is incorrect for Black and Nagin to claim that our original article ''does not adequately capture local time trends in crime rates'' when our regressions on differences control for fixed county effects while theirs do not.[37] The county dummy variables account for the different average crime rate changes across counties over time. Yet, to accommodate Black and Nagin's desire not to use the fixed county effects in these regressions, Table 2 reports separate estimates with and without the county fixed effects. The evidence provided in Black and Nagin's earlier drafts of their comment that they claimed rejected the deterrence hypothesis is very similar to what we had already reported. The patterns that they found in their December 6, 1996, paper (reported in Figure 1 here) and what I show in Figure 2 in this article are difficult to distinguish.

All six specifications imply that concealed-handgun laws deter violent crime. While the results for counties with more than 100,000 people are similar in size to the other estimates, possibly because of the much smaller sample size, they are only statistically significant at the 20 percent level when county fixed effects are used and the 37 percent level without county fixed effects. When county fixed effects are used, the results consistently imply a one percentage point difference in violent crime growth rates before and after the passage of the law. The regression that most closely corresponds to Black and Nagin's specification is the only estimate where the coefficients imply a slight downward trend in violent crime for the years before the law goes into effect.

Figure 6 in this article graphs the results for these three different samples and specifications when the individual year dummy variables are used in the manner by Black and Nagin. The results vary little across samples, though the much smaller sample for the over 100,000 people county sample

[37] Black and Nagin, in this issue, at 218 (cited at note 1).

TABLE 2

RERUNNING THE VIOLENT CRIME RATE REGRESSIONS ON DIFFERENCES EXAMINING VIOLENT CRIME RATES

	Sample Corresponding to That Used in Table 3 in Lott and Mustard		Sample Using Only Counties with More than 100,000 People		So-Called Full Sample Where the Arrest Rate is Excluded	
	County Fixed Effects (1)	No County Fixed Effects (2)	County Fixed Effects (3)	No County Fixed Effects (4)	County Fixed Effects (5)	No County Fixed Effects (6)
Time trend for years after the right-to-carry law is in effect (negative values imply that crime was falling after the law went into effect)	−.0081 (2.246)	−.0061 (2.302)	−.0079 (1.272)	−.0048 (1.086)	−.0087 (1.646)	−.00499 (1.311)
Time trend for years before the right-to-carry law is in effect (positive values imply that crime was rising until the law went into effect)	.00182 (1.202)	.0011 (1.312)	.00145 (.859)	−.00056 (.344)	.00384 (1.727)	.00199 (1.544)
F-statistic for whether the before and after time trends are different (probabilities are in parentheses)	4.79 (.029)	6.72 (.01)	1.45 (.201)	.80 (.370)	3.54 (.060)	3.00 (.083)
Adjusted R^2	.1261	.1753	.3331	.3684	.0485	.0089
Number of observations	38,675	38,675	6,160	6,160	42,037	42,037

NOTE.—I used different sample sizes and examined the effect of county fixed effects. Regressions control for all the variables controlled for in the regressions shown in table 3 in our original article (see Lott and Mustard (cited at note 1)). With the exception of the before and after time trends, all the endogenous and exogenous variables are differenced. The absolute *t*-statistics for the time-trend variables are reported in parentheses.

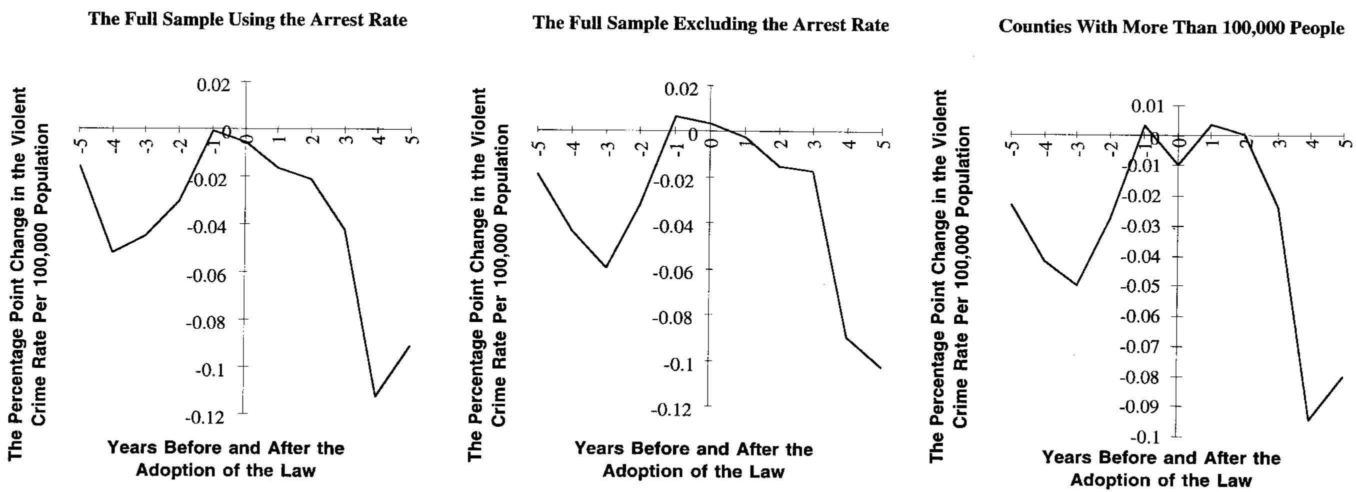

FIGURE 6.—Using differences to study the effect of concealed-handgun laws, examining different sample sizes

produces less significant results. I also obtain very similar results when I use separate time and time-squared terms for both the prelaw and postlaw periods. Similar figures with the county fixed effects again indicate an even larger deterrence effect of concealed-handgun laws. The general patterns for the individual violent crime categories are similar to those reported in Figures 1 and 2 in this article.

Black and Nagin's results produce something of a puzzle. One would think that differences without fixed county effects would produce similar results to levels with fixed county effects. Indeed, the results in Figure 6 in this article are very similar to the earlier Black and Nagin estimates reported in Figures 1 and 5 in this article.

Finally, my forthcoming book extends the sample period to 1994 for the county-level data and to 1995 for the state-level data and finds even stronger evidence of the deterrent effect of concealed handguns.

IV. Conclusion

So what is the final outcome of this exchange? On the basis of Black and Nagin's comment and our original article, the choice is between concealed handguns either producing a deterrent effect or having no effect (one way or the other) on murders and violent crime generally. Even if this were the state of the current debate, it represents a big change in the bounds of the debate, where many academics have argued that more guns lead to more violence. Their regressions that examine the different state-level effects fail to account for whether the law affected all counties equally and fail to report the effect of these laws on violent crime. Black and Nagin's use of quadratic individual state time trends makes it impossible for their reported estimates to test any individual state-level effects from the concealed-handgun laws. The evidence provided in Black and Nagin's earlier drafts of their comment, which they claimed rejected the deterrence hypothesis, is very similar to what we had already reported and what I report here. The patterns that they found in their December 6, 1996, paper (reported in Figure 1 in this article) and what I show in Figure 2 in this article are difficult to distinguish.

Measures of statistical significance depend on the reported tests being random draws. My article with David Mustard and my forthcoming book report nearly 1,000 regressions that implied a very consistent effect of concealed-handgun laws on crime. The very public nature of this debate allows us also to document many tests that Black and Nagin ran but did not report in their comment. The evidence in my Figure 1 can be evaluated on its own merit.

Black and Nagin claim that our original article assumes that the effect of concealed-handgun laws is constant over time, that the effect is the same across states, that our article does not control for local time trends, and that we did not investigate whether the results were sensitive to the missing values of the arrest rate. They are wrong. We raised these issues ourselves and then suggested tests for them. In fact, we went much further in investigating the effect of local time trends on crime rates than they did. Normally, a comment would explain why the approaches that we used to solve these problems were inadequate and then offer alternative approaches. Instead, Black and Nagin claim that we completely ignored these issues and contend that they are raising them for the first time. The most surprising aspect of this whole exchange is that Black and Nagin's claims of what is or is not included in our article are so easily verified by the reader.