

# Do more guns cause less crime?

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The main argument of a recent book by John Lott is summarized in the title: *More Guns, Less Crime* [26]. There are three parts to this argument:

1. that there were more guns
2. that there was less crime
3. that more guns *caused* less crime

Lott's argument depends on all three parts being true. If any one of the parts is incorrect, the entire argument fails. In fact, as I will show in the next three sections of this document, *all* three parts are wrong:

1. there weren't significantly more guns (see Section 1)
2. there wasn't less crime (see Section 2)
3. even if there was more guns and less crime, more guns did not cause less crime (see Section 3)

## 1 Were there more guns?

Lott's main analysis was of a data set of crime, arrest, income and demographics for each county in the US for the period 1977 to 1992. During that period, several states passed "right-to-carry" laws that made it easier for citizens to obtain permits to carry a concealed weapon. He performed a multivariate regression and concluded that the laws had caused an 8% reduction in murder, a 5% reduction in rape, and a 7% reduction in aggravated assault. He argues that criminals were deterred from committing crimes by

the additional risk they faced in facing permit holders carrying concealed weapons. The “more guns” in the title of Lott’s book mostly refers to more guns being carried in public by permit holders.

However, as the next sections will show, it does not seem that these laws caused there to be significantly more guns, whether you use Lott’s meaning for “more guns”, or some other one.

## 1.1 Was there more gun ownership?

Firstly, the laws did not cause any significant increase in gun ownership. Lott notes that a Texas poll suggests that 97% of first-time applicants for concealed-weapon permits already owned a handgun (page 28).

Lott also presents an analysis (page 114) based on two surveys of gun ownership (conducted in 1988 and 1996) that purports to show that a 1% increase in a state’s gun ownership causes a 4.1% decrease in the violent crime rate and a 3.2% decrease in auto theft.

Lott’s two polls indicate that gun ownership increased by 50% in just eight years, from 26% to 39%. This is contradicted by everything else we know about gun ownership:

Since 1959, there have been at least 86 different surveys on gun ownership [21]. There doesn’t seem to have been in any increase over that period, let alone over 1988–1996. The percentage of the population that declared they were gun owners varied between 25% and 35%, but there was no clear trend. It seems that the changes in the numbers are caused by sampling error, differently worded questions, and changes in the willingness of people to admit to gun ownership. Lott’s apparent increase is an artifact of his having looked at just two polls instead of many.

Data on gun sales in the period 1988–1996 shows that the number of guns per person increased by just 10% [21, 18]. Any increase in ownership rates is going to be less than 10% since the majority of gun purchases are by people who already own guns. Furthermore, gun sales per capita in the period 1988–1996 were only 5% more than in the previous eight year period. This does not seem consistent with the dramatic increase in gun ownership suggested by Lott.

Lott argues (page 36) that a doubling of spending on private security from 1980 to 1996 suggests that more people have been obtaining guns. However, despite an increase in population, gun sales in 1996 (4.8 million) were less than in 1980 (5.8 million).

## 1.2 Were there more guns carried in public places?

Perhaps by “more guns”, Lott means more guns carried in public places. However, surveys indicate that 5–11% of US adults admit to carrying guns for self-protection [21], dwarfing the 1% or so of the population that obtained concealed-weapon permits.

For this reason, Gary Kleck (who strongly believes that armed citizens deter crime) does not think it likely that the carry law caused crime to decrease. He writes [21]

Lott and Mustard argued that their results indicated that the laws caused substantial reductions in violence rates by deterring prospective criminals afraid of encountering an armed victim. This conclusion could be challenged, in light of how modest the intervention was. The 1.3% of the population in places like Florida who obtained permits would represent at best only a slight increase in the share of potential crime victims who carry guns in public places. And if those who got permits were merely legitimating what they were already doing before the new laws, it would mean there was no increase at all in carrying or in actual risks to criminals. One can always speculate that criminals’ perceptions of risk outran reality, but that is all this is—a speculation. More likely, the declines in crime coinciding with relaxation of carry laws were largely attributable to other factors not controlled in the Lott and Mustard analysis.

## 1.3 Were there more guns used to defend against crime?

In response to Zimring and Hawkins, who make a similar argument [47] to Kleck, Lott claims that the people who get permits will tend to be those with a higher risk of being crime victims and hence it is possible for permit holders to use guns for defence in a much larger percentage of crimes than the numbers of permit holders would suggest (page 129). That is, “more guns” means more guns used to defend against crime.

However, his claim is wrong since the group with the highest risk of being a crime victim are those with a criminal record (as Lott himself notes on page 8), and those with a criminal record are not eligible for permits.

Hood and Neeley [17] analyzed permit data for Dallas at the zip-code level and found exactly the opposite pattern from that predicted by Lott, that is,

those zip codes with the highest violent crime rate before Texas passed its carry law had the smallest number of permits per capita.

A similar pattern occurs with seat-belt wearing. People who wear seat belts are less likely to have accidents [14], not more likely.

Lott's error is in making the false assumption that everyone is equally risk averse. People who are risk averse will both wear seat belts and drive more carefully, or get a concealed weapon permit and avoid dangerous situations.

Furthermore, permit holders will not always carry their weapons and will not always get a chance to use them, implying that permit holders will use guns for defense in a much smaller percentage of crimes than the number of permit holders suggests.

There is empirical data on how often permit holders use their weapons:

Dade county police kept records of all arrest and non-arrest incidents involving permit holders in Dade county over a 5 year period [8]. Lott cites this study to show that gun misuse by permit holders is extremely rare (page 11):

A statewide breakdown on the nature of those crimes is not available, but Dade county records indicate that four crimes involving a permitted handgun took place there between September 1987 and August 1992 and none of those cases resulted in injury.

Lott fails to note that the same study shows that defensive gun use by permit holders is also extremely rare (page 692 of [8]):

The Dade police recorded the following incidents involving the defensive use of licensed carry firearms: two robbery cases in which the permit-holder produced a firearm and the robbers fled, two cases involving permit-holders who unsuccessfully attempted to stop and apprehend robbers (no one was hurt), one robbery victim whose gun was taken away by the robber, a victim who shot an attacking pit bull; two captures of burglars, three scaring off of burglars, one thwarted rape, and a bail bondsman firing two shots at a fleeing bond-jumper who was wanted for armed robbery.

There were only 12 incidents where a criminal encountered an armed permit holder. Compare this with the roughly 100,000 violent crimes in Dade county in that period. Clearly the chance of a violent criminal encountering

an armed victim increased by at most 0.012 percentage points. The true figure is considerably less since some permit holders may have carried legally or illegally before the law, only half of the 12 incidents involved defence against a violent crime, and crimes where a gun is used defensively are more likely to be reported to the police than crimes in general. (NCVS data [38] indicates that 65% of crimes where a gun is used defensively are reported to the police, compared with 43% of crimes in general. Kleck's survey [22] indicates that 64% of defensive gun uses are reported to the police.)

Nor was Dade county unusual in that there were very few carry permits issued—at the end of the five year period there were 22,000 permit holders in Dade, about the same percentage as their was in the rest of Florida.

As far as Lott's assertion that permit holders face a higher risk of being attacked than the general population goes, the Dade study shows that the rate at which permit holders in Dade use their weapons for defence against a violent crime is only 12 per 100,000 permit holders per year. Compare this with a violent crime rate of about 1,000 per 100,000 population in Dade county. Needless to say, 12 is not higher than 1,000.

#### **1.4 Does “more guns” mean an increase in the gun stock?**

There is another interpretation of the phrase “more guns”—it could refer to the gun stock (the total number of guns in private hands). This isn't a very useful measure, since if a gun owner purchases another gun the gun stock increases, but there is no increase in the availability of guns. Perhaps for this reason, Lott is not referring to the size of the gun stock when he writes “more guns”.

Since the gun stock can only increase, if by “more guns” you mean an increase in the gun stock, whether “more guns” are associated with “less crime” or “more crime” depends on whether crime rates went up or down. Since the US crime rate has been declining in the last few years, but the long term trend has been upwards, you can have “more guns, more crime” if you look in the long term, or “more guns, less crime” if you look at the short term. This is another reason why it is not useful to have “more guns” refer to the size of the gun stock.

## 1.5 Conclusion

No matter how you interpret the phrase “more guns”, it remains true that there were not a significant number of “more guns”. This fact alone is sufficient to destroy Lott’s case.

However, while this theory is not advanced by Lott, there is a way that carry laws could have caused a reduction in crime without having “more guns”. The publicity about the new law could have caused criminals to overestimate the risks that they faced from permit holders. If this was the case, you would expect to see an immediate decrease in crime followed by a gradual increase as criminals learned that they were not at any greater risk. Lott does not test for this possibility—his models either have an immediate effect or a gradual one, not a combination. However, Ayres and Donahue [2] test such a combination model and find exactly the opposite of what the publicity theory predicts—an immediate increase followed by a gradual decrease.

## 2 Was there less crime?

Although Lott found that the carry law was associated with reductions in crime in his multivariate model, whether these reductions are real depends on whether the model is correct. The best test of such a model is its predictive power, unfortunately, as we shall see below, it fails such a test. Other evidence that the model is incorrect is that it throws up many spurious correlations. Other researchers have proposed alternative models that do not have the crime reductions found in Lott’s model.

### 2.1 More Data, More Guns, More Crime

In a critique of econometric studies such as Lott’s[15] Ted Goertzel makes an important point—for any study that involves a multiple regression that finds a significant association, it seems that there is another study that applies a different model to the same data and gets a different answer. There are several examples of this happening with Lott’s study described in section 2.3.

Goertzel argues convincingly that

When presented with an econometric model, consumers should insist on evidence that it can predict trends in data other than

the data used to create it. Models that fail this test are junk science, no matter how complex the analysis.

In the case of Lott's model we are in the fortunate position of being able to test its predictive power. Lott's original data set ended in 1992. Between 1992 and 1996, 14 more jurisdictions (13 states and Philadelphia) adopted carry laws. We can test the predictive power of Lott's model by seeing if it finds less crime in those jurisdictions. Ayres and Donahue [2] have done this test. They found that, using Lott's model, in those jurisdictions carry laws were associated with *more* crime in all crime categories. Lott's model fails the predictive test.

Ayres and Donahue go on to examine all the states adopting carry laws using data up to 1997 and found that carry laws were associated with crime increases in more states than they were associated with decreases. They rather pointedly observe that

Those who were swayed by the statistical evidence previously offered by Lott and Mustard to believe the more guns, less crime hypothesis should now be more strongly inclined to accept the even stronger statistical evidence suggesting the crime-inducing effect of shall issue laws.

## 2.2 Spurious Correlations

As well as finding significant correlations with the carry law Lott found many other correlations. Some of these make sense, for example, the an increase in the percentage of the population that is black, male, and aged 10–19 is correlated with an increase in the property crime rate. Others, however do not, for example, an increase of 1 percentage point in the percentage of the population that is black, female, and 40–49 is associated with a 30% decrease in rape, and a 24% increase in homicide in the average county.<sup>1</sup> These two correlations (and many others) are spurious.

In response to a similar critique by Alschuler [1] Lott argues that these correlations make sense because black females aged 40–49 could be more likely to be crime victims (page 144). It is true that if they were about

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<sup>1</sup>Lott's table A5.1 gives these numbers as 74% and 59%, but because he did not take logs of the population percentages, his numbers are too large. I am indebted to Florenz Plassmann for this observation.

24 times more likely to be homicide victims than the general population, then the correlation would make sense. However, the FBI's *Uniform Crime Reports* [36] shows that black females aged 40–49 made up just 1.3% of murder victims. This is more than the 0.4% of the population that is black female aged 40–49, but clearly not 24 times more. And the association with a 30% decrease in rape makes even less sense. Even if no women in this group were ever rape victims, this would only account for an association with a 1% decrease.

Even more troublesome are the results of the two-stage least squares (2SLS) regressions. The correlations discussed so far were computed assuming that crime rates do not affect arrest rates, which does not seem a reasonable assumption. Table 11 of [31] reports the results of rerunning the regressions using two-stage least squares, which allows arrest rates and crime rates to affect each other. The size of the effect associated with the carry law are spectacularly different from those in table 3 of [31]. For example, the effect on property crime changes from a 3% increase to a 67% decrease and the effect on violent crime changes from 5% decrease to a 72% decrease. The 2SLS regressions are clearly spurious and indicate severe problems with the model.

Dezhbakhsh [9] offers further evidence that Lott's 2SLS model is incorrect:

[Lott,pdftex] obtains mostly negative numbers for arrests. For example, more than 19,000 of approximately 33,000 county-level auto theft arrests are "negative"; the number of negative arrest rates for aggravated assault and property crimes are, respectively, 9,900 and 13,500. What does a negative arrest rate mean? Obviously, the number of individuals arrested for crimes can only be zero or positive.

Black and Nagin [5] report further evidence that Lott's model is not correct. They applied Heckman-Hotz tests [16] which indicated the presence of systematic factors, not modeled by Lott, which significantly affected the crime rate.

### 2.3 Testing alternative models

However, even if the the model is incorrect, it may be that a correct model would still give show a correlation between the carry laws and less crime. A study by Bartley and Cohen [4] sheds some light on this possibility. They

reran the regressions with thousands of different models (formed by deleting variables from Lott's model and by adding a trend term). If the carry law is associated with a reduction in crime in all of these models then we might conclude that is associated with a reduction in the correct model, without us having to identify which of the models is the correct one.

Figures 1–4 of their paper show that the carry laws were not consistently associated with a crime reduction in any crime category: that is, there were some models where the law was associated with an increase for each crime category studied. I should note, however, that if we restrict things to just models that include a trend component, homicide and robbery show consistent reductions. For this reason, Bartley and Cohen argued that Lott's results should not be dismissed as unfounded.

Dezhbakhsh and Rubin [10, 11] re-examined the data using a more general model that allowed the carry law to have different effects in each county and to affect other parameters in the model. With this model they found the carry law did not have any clear effect on rape or assault, that it was associated with a reduction in homicide in six out of 33 states, and with an increase in robbery in 13 out of out of 33 states. The evidence here is stronger for an increase than for a decrease.

Plassmann and Tideman [39] point out that Lott's analysis technique assumes that crime rates are normally distributed and that this is not even close to being true for low crime counties. When they made some plausible changes to the specification, the effects on murder vanished. However, when they did their own analysis assuming that the murder rate was Poisson distributed, they found an even stronger effect (a 12% decrease). They also looked at the effects on each state and found a confusing pattern of results, with the effect varying from a statistically significant increase of 6.5% (Virginia) to a statistically significant decrease of 35% (Montana). While we would not expect the laws to have exactly the same effect in every state, it seems hard to see how the effects could be so radically different.

Duggan [12] points out another problem with Lott's analysis:

One problem with these regression estimates is that Lott and Mustard are implicitly assuming that these laws are varying at the county level, when in fact they are varying only at the state level.

The reason this is a problem is that you would expect crime rates in counties within the same state to be correlated. This problem does not bias the

estimates of the law's effect, but causes the standard errors to be underestimated, so that some results may appear to be statistically significant when they are not.

On page 278, note 3, Lott comments on this problem, but erroneously claims that including dummy variables for all counties solves the problem. This is clearly false. The dummy variables only account for fixed differences between counties and do not address the within-state correlations between counties.

After adjustments to account for this problem, Duggan found that

none of the coefficient estimates on the CCW variable remain statistically significant.

Lott's response to Duggan's paper was to repeat his false claim:

The correlation of the error terms across counties is picked up when one has county fixed effects included in the regression. He does not do the adjustment recognizing that the county fixed effects are already picking up what he wants to adjust for. [25]

Moody [35] noticed the same problem as Duggan:

Merging an aggregate variable with microlevel variables causes ordinary least squares formulas to severely overestimate the t-ratios associated with the aggregate variables. . . . I reestimated the model using the original county-level data set but adjusted the standard errors for clustering within states. The results were somewhat different from the original Lott and Mustard findings. . . . While shall-issue laws reduce violent crime in general in all models, the effects seem to be concentrated in robbery. Murder and rape are significantly reduced in only one version of the model.

In Lott's response to Moody [29] he still did not admit to making a mistake but rather stated that he "had already discussed this issue".

## 2.4 Garbage In, Garbage Out?

Maltz and Targonski [34] have examined the county level UCR statistics used by Lott and found it to have extensive gaps—in many states large numbers

of counties have 30% or more of their population missing coverage. They conclude that

in their current condition, county level UCR crime statistics cannot be used for evaluating the effects of changes in policy.

I should note, however, that state level analyses do not have the same problem and these give somewhat similar results to the county level one.

### 3 Did more guns cause less crime?

Even if there were more guns and less crime, this is a correlation, not causation. Scientists generally accept a replicable controlled experiment as good enough for showing cause and effect for practical purposes. A non-experimental study like Lott's is merely suggestive of causation. When you have a pile of them and well-understood and verified mechanisms (like for the relationship between smoking and cancer) then you can make some causal claims. Although Lott proposes a mechanism; it is not plausible, as we shall see below.

Crime goes up and down, and we do not understand all the reasons for this. Although Lott makes a commendable effort to control for as many factors as he can, there are still significant changes in the crime rate that are not explained by his model. A way to test to see changes in the crime rate were caused by the carry law or one of the other factors is to look for *differential effects*—we would not expect the carry law to affect all crime rates equally. For example, since juveniles are not eligible for the concealed weapon permits, we would not expect the law to affect the juvenile homicide rate as much as it effects the adult rate. With one somewhat problematic exception, we do not find differential effects.

#### 3.1 Where's the mechanism?

Lott believes that the carry laws reduced violent crime by increasing the probability of the criminal encountering an armed victim and thus the cost of the crime to the criminal.

However, the evidence from the Dade county study [8] mentioned in section 1.3 shows that the carry laws did not increase the probability of criminals encountering an armed victim.

Lott reckons that the carry law caused a reduction of 8% in murders, 5% in rapes, 7% in aggravated assaults and 2% in robberies. For Dade county that translates to 1,500 fewer aggravated assaults, 450 fewer robberies, 65 fewer rapes and 30 fewer murders each year, amounting to a total of about 10,000 fewer violent crimes over the five year period. This seems to be far too large an effect to be caused by a mere 12 incidents where a criminal encountered an armed permit holder.

By how much did the carry law increase the cost of crime to criminals? Arrest is 5,000 times more likely than encountering an armed permit holder. If the criminal considers these to be equally bad outcomes, then the carry law increased the cost by no more than 0.02%. It is absurd to expect a 7% decrease in crime from such an insignificant change in the cost.

Anyway, since Lott has estimated the effect of increasing the arrest rate we can do a much more direct calculation. In chapter 4 he reports that concealed carry had an effect of 4.8% reduction in violent crime while a 100 percentage point increase in the arrest rate reduced violent crime by 0.48%. The 12 incidents out of 100,000 violent crimes is equivalent to increasing the arrest rate by 0.01 percentage points. Consequently, Lott's model predicts that this would reduce the violent crime rate by  $0.01 \times 0.48 / 100 = 0.000048\%$ . That is 100,000 (*five* orders of magnitude) times smaller than the effect he actually attributes to carry laws.

To be fair, it looks like Lott has made an error in reporting his results and understated the effect of increasing the arrest rate by a factor of 100. (The numbers seem ridiculously small, and the corresponding numbers in the table giving effects when the data is aggregated by state are about 100 times larger.) Even if we allow for this possible error, the effect is still 1,000 times smaller than the effect he actually attributes to carry laws.

Even if (contrary to what criminals said in Wright and Rossi's study of criminals attitudes to firearms [46]) criminals are not afraid of police guns but are afraid of victim's guns, the change in the cost is still insignificant. If you believe Kleck's survey of defensive gun use [21] there were at least 100,000 DGUs (defensive gun uses) in Dade county over the five year period. If you believe the NCVS [40] there were at least 2,500. Either way, the 12 by permit holders makes no significant difference to the total.

If we decide that the only cost that criminals care about is the chance of getting shot by a victim, then the comparison is even starker. Not one criminal was shot by a permit holder during the entire five year period, while probably around 500 criminals were shot by non-permit holders acting in

self-defence [21].

Nor is it plausible that there were large number of DGUs by permit holders that were not reported to the police. While Kleck's survey and the NCVS give wildly different estimates for the number of DGUs they agree that about half of DGUs are reported to the police.

Some have argued that the publicity about the passing of the law caused criminals to mistakenly believe that the risk they faced increase. This is possible, but Lott didn't test this model (since he didn't have a variable for the publicity). Nor is this consistent with the results of his trend analysis, which shows that the decrease was small at first and gradually increased as time passed. The publicity about the law would have been greatest at the time the law was passed and lessened as time passed.

## **3.2 Where are the differential effects?**

One way to test if the changes in crime rates were caused by the law or by other factors is to look for differential changes in crime. For example, it does not seem likely that the carry law would affect crimes that occur in the home of the victim, since it was already legal to keep a gun in the home for defence before the law.

### **3.2.1 Effect on robbery should be more than on rape**

Lott notes that a Texas poll suggests that 97% of first-time applicants for concealed-weapon permits already owned a handgun (page 28), so the carry law did not result in an increase in gun availability at home. Because the law only affected the carrying of guns in public places we would only expect them to affect crimes carried out in public places.

NCVS statistics indicate 36% of robberies and 10% of rape/sexual assaults occurred "on street other than near home" [37]. If the carry law caused a 20% decline in crimes in public places, you would expect a 7% decline in robbery and a 2% decline in rapes. The effect of the law might be something other than a 20% decline, but in general we should expect the effect on robbery to be about 3 1/2 times as great as that on rape. Instead, Lott found a 5% decline in rape and a 2% decline in robbery.

In response to a similar criticism made by Webster [45] Lott (page 133) offers two arguments:

First, that the effect on the slope was larger for robbery, that the robbery rate was increasing before the law and decreasing afterwards. However, this is undercut by Table 4.13 which shows the result of adding data for 1993 and 1994. This causes the change in robbery rates associated with the law to go from negative to positive. This suggests that robbery rates went back up after 1992.

The robbery rate was changing in a way that was not explained by Lott's model. Since we don't know what was making robbery increase, we have no way of knowing when the increasing trend would end (certainly it couldn't go on increasing forever). Assuming that robbery would have continued to increase for the whole period of the study without the carry law seems a little unwarranted.

There is also an element here of shifting the goal posts. If you can find a decrease in the rate, report that. If that doesn't work, look at the trends.

Second, that some robberies were not street crimes and the laws could cause an increase in other robberies by making them relatively more attractive. This argument seems to miss the point. The carry law could equally well cause an increase by substitution in non-street rapes. Lott offers no evidence at all that this supposed effect was different for rapes than for robberies.

### **3.2.2 Was there substitution from violent crime to property crime?**

Lott found that the laws were associated with an increase in property crime, most noticeably with a 7.1% increase in auto theft. Lott argues that this change occurred because

Criminals respond to the threat of being shot while committing such crimes as robbery by choosing to commit less risky crimes that involve minimal contact with the victim.

Unfortunately for this argument, the law was not associated with a significant decrease in robberies. In fact, when data for 1993 and 1994 was included, it was associated with a small (not statistically significant) increase in robberies.

The law was associated with a significant reduction in assaults, but there does not seem to be any reason why criminals might substitute auto theft for assault.

In the second edition of Lott's book table 9.1 shows the results of his latest analysis, using data up to 1996. This table shows that the effect on violent crime (-2.3%) is very similar to the effect on property crime (-1.6% substitution

effect, but, since it is not plausible that a carry law could cause a greater reduction in auto theft than in murder, there is evidence that both reductions were caused by some other factor. Rather than point this difficulty out to his readers, Lott quietly drops further discussion of the substitution effect.

### **3.2.3 Can different effects in different states be explained by the number of permits issued?**

Table 4.9 shows the effects in each state that adopted a carry law. It is clear that there are some notable differences in the effects, even though each state adopted a similar law. Only murder and rape show a consistent pattern of reduction. The other crime categories show a fairly even split between states showing an increase and states showing a decrease. It does not seem reasonable to argue that the carry law caused any change, except for the case of murder and rape.

In response to a similar argument made by Black and Nagin [5], Lott writes (page 143):

The difference that did exist across states can be explained by differences in the rate at which handgun permits were issued.

Lott has data on handgun permits for three states (table 4.7). This shows that the percentage of the population with permits in 1994 was 1% in Florida, 1.4% in Oregon, and 4% in Pennsylvania. However, this is exactly the opposite ordering of the changes in the violent crime rate: -4% in Florida, -3% in Oregon, and -1% in Pennsylvania. If differences in the rate at which handgun permits were issued explain this, it can only be if more handgun permits cause more violent crime.

Black and Nagin [5] also observe that the effects on murder and rape depend on the inclusion of Florida in data set—if Florida is excluded, the effect on murder changes from a 9% decrease to a 1% decrease, while the effect on rape changes from 3% decrease to 1% increase.

Lott offers several objections to this argument:

Firstly, he objects to Black and Nagin conducting their analysis using only counties with populations of more than 100,000. This is a strange objection to make, since Lott states that restricting the sample in this way makes the effects of shall-issue law stronger and more significant. In any case, it makes no difference whether the sample is restricted in this way since the effects

on murder and rape also vanish when Florida is excluded from a sample containing all counties.

Secondly, Lott reports that when he reran all the regressions in the book without Florida, in only eight out of one thousand did the result change from significant to not significant. This goes some way towards alleviating the concerns about the influence of Florida on the results, but the fact remains that it did make a large difference to the most important regressions.

Thirdly, Lott suggests that Black and Nagin conducted a search for a specification that weakened the results and that traditional statistical tests of significance are based on the assumption that the most favourable (or unfavourable) one out of a number of tests has been chosen. However, it well known that all that is necessary to do in such a situation, is to adjust the level of significance accordingly, and Black and Nagin did so.

#### **3.2.4 Effect on men should be more than on women**

Only 20% of permits were issued to women, but the male and female homicides rate went down by the same amount and the reduction in the rape rate was similar to the decrease in assaults. Lott speculates that guns are four times as effective for females. While this is not impossible it seems more likely that the decreases were caused by some other factor that applied to males and females equally. Certainly it seems to be a bit much of a coincidence that the supposed extra effectiveness for women just happened to be exactly equal to ratio between the numbers of male and female permit holders.

#### **3.2.5 Effect on strangers should be more than on family members**

As mentioned before, we would expect carry laws to mainly affect crimes committed in public places. Since crimes committed by strangers are more likely to occur in public places than crimes committed by family members, you would expect a greater effect on crimes committed by strangers than on crimes committed by family members. Table 5.2 however, shows just the opposite—the percentage of homicides by strangers went up, while the percentage of homicides by family went down. While these changes were not large enough to be statistically significant, even if there was no change to these percentages, this would still be a failure to find a differential effect, and evidence that the changes in crime rates were caused by something other

than the shall issue laws.

In response to a similar argument made by Alschuler [1], Lott argues (page 148) that concealed handguns could defend against estranged family members. While this is possible, it misses the point of the argument, which is not that carry laws would have no effect on in-family homicides, but that the effect would be less than that on stranger homicides.

### **3.2.6 Effect on adults should be more than on juveniles**

Even more surprising is the fact that the carry laws apparently were just as effective in reducing juvenile homicides as they were in reducing adult homicides, even though juveniles were not eligible for permits.

Ludwig [33] used juvenile homicide rates to control for unobserved variables that may vary over time and found that, if anything, the carry laws resulted in an increase in adult homicide rates.

Lott offers two arguments in response to this criticism (page 147).

First, that “criminals may well tend to leave an area where law-abiding adults carry concealed handguns”. This is contradicted by his own results: a substitution into property crime, and no displacement of violent crime to nearby areas without carry laws.

Second, that gun-carrying adults may be able to protect some youngsters. However, even we make the extremely generous estimate that this happens half the time, the effect of the law on the juvenile homicide rate would only be half that of the adult rate. The effect, as seen above was the same. A more realistic estimate, combined with the substitution effect mentioned on the previous, suggests that there should have been no effect, or a small increase in juvenile homicides.

### **3.2.7 Were crimes displaced to adjacent counties without carry laws?**

Lott claims to have found a differential effect by looking at counties without carry laws that were near counties that adopted such laws. If the carry law is deterring crime in the counties with carry laws, you might expect some criminals to move their activities to nearby counties without carry laws and increase the crime rate there.

His table 4.14 clearly shows that the violent crime in such counties did not change at all when the carry law was passed. However, because three of

the four categories of violent crime showed an increase, and one a decrease, Lott argues that this shows that there was a spillover effect.

### **3.2.8 Did crime reductions exactly coincide with the laws?**

On page 131 Lott writes

To illustrate that the results are not merely due to the “normal” ups and downs for crime, we can look again at the diagrams in chapter 4 showing crime patterns before and after the adoption of the non-discretionary laws. The declines not only begin right when the concealed handgun laws pass, but the crime rates end up well below their levels prior to the law. Even if laws to combat crime are passed when crime is rising, why would one believe that they happened to be passed right at the peak of any crime cycle?

This is wrong. Lott’s diagrams do not show crime rates at all, but rather plot two quadratic curves that he fitted to the data. It is no surprise that there is a peak when the laws were passed—this is one of the few places where it is possible for the fitted curves to peak. Even if the crime rate started to decline before the laws passed, Lott’s diagram could still show a peak coinciding with the law.

I ran some experiments by fitting a similar pair of quadratic curves to a sequence of random numbers. Almost always the curves seemed to show that something had happened at the junction of the two curves, even though nothing had.

Figure 3.2.8 shows two sample results.

### **3.2.9 Were crime reductions greatest in counties which issued more permits?**

Lott found that crime reductions tended to be greater in high population counties and argued that these were the counties that issued the most permits.

There are two problems here: firstly, he is using a counties’ population as a proxy for the number of permits issued without validating that proxy. This is puzzling, since he has county level data for the numbers of permits issued for two states. It would be simple to check to see if relatively more permits were issued in high population counties.

Secondly, high population centres show greater variations in crime rates. The following table shows the standard deviation of the log of the homicide rate for US cities of different sizes over a 20 year period.

City size	s.d. of log homicide rate
One million and over	0.15
500k–999k	0.12
250k–499k	0.15
100k–249k	0.10

It follows that when crime goes down it decreases more in places with higher populations. The next table shows the crime trends in cities of each size for the periods before and after 1991 (when the homicide rate peaked).

City size	annual change	annual change
	in homicide rate	in homicide rate
	1976–1991	1991–1997
One million and over	0.5	-1.7
500k–999k	0.3	-0.8
250k–499k	0.2	0.4
100k–249k	0.2	-0.2

That is, if the reduction in crime rates was just the product of the normal increases and decreases in crime it would also follow the same pattern of being greatest in high population centres.

### 3.2.10 What about multiple victim public shootings?

In his book and in a paper co-authored with Landes [30] Lott argues that carry laws caused an 89% decrease in murders in multiple victim public shootings. For reasons explained in section 3.2.1 we would expect a greater effect of a carry law on crimes that occur in public places.

Lott and Landes also offer another argument why we might expect a greater effect on multiple victim public shootings: since there are more people present in such shootings, the chance of a criminal encountering a permit holder is greater than that for a single-victim crime. However, this argument is flawed, since all the costs of crimes are higher for multiple victim crimes—the criminal is more likely to be caught and convicted, the penalty will be higher, and they are more likely to encounter resistance from non-permit-holders. To put it in economic terms, what is important is not the absolute

increase in cost of a crime, but the relative cost. There is no good reason to expect an increase of \$2 in the price of something costing \$20 to have a greater effect on demand than an increase of \$1 in the price of something costing \$10.

Furthermore, there is an upper limit to the cost of these crimes. Since the criminal dies in many of these crimes it is hard to see how the cost can increase from this.

For these reasons it is unclear whether you would expect to see a greater deterrent effect on multiple victim public shootings than on other crimes.

As to the question of whether “more guns” cause “less multiple victim public shootings”, there are similar problems to the question as to whether “more guns” cause “less crime”.

Firstly, there weren’t significantly more guns (see Section 1).

Secondly, there are doubts about whether there were “less multiple victim public shootings”. Although there was an 89% reduction when comparing before and after rates in states that introduced shall issue laws, (table 2 of [30]) shows that the average murder and injury rate from multiple victim shootings was 0.042 in states without such laws and 0.029 in states with shall issue laws (just 31% lower). The difference between 89% and 31% is enormous and suggests that something is wrong—the only way that the reduction could really have been 89% is if there was some other mysterious factor operating that would have caused the rate of mass public shootings in shall-issue states to have been much higher than in the other states, were it not for the shall-issue law. Lott and Landes do not identify such a factor.

Also, table 3.2 of the book indicates that the overall murder rate was 9.5 in states without such laws and 5.1 in states with shall issue laws (46% lower). That is, multiple victim shootings were actually relatively more common in states with shall issue laws.

Furthermore, it is absurd to suppose that the carry law could cause a decrease as large 89% in multiple victim public shootings. It is possible that some perpetrators might be deterred, but since many of them die from police weapons or their own weapons it is surely less than 89%. Indeed it is unlikely that as much as 89% of adults are even aware of the carry laws.

Lott and Landes found that that neither the frequency nor the severity of punishment had an effect on public shootings, suggesting that these crimes are not easy to deter. Lott believes that carry laws work to reduce public shootings not by increasing the cost of the crime, but by decreasing the value of the crime to the criminal. Lott writes [27]:

What motivates most of these criminals seems to be the desire for publicity. They want to kill as many people as possible. The possible presence of concealed weapons can limit the carnage, and thus the incentive to begin the attack.

However, the average number of deaths per incident in public shootings in states with shall issue laws was 1.7 (my analysis of table 1 of [30]), almost the same as the 1.8 deaths per incident in states without shall issue laws. This very small difference does not seem like anywhere enough to make such shootings worthless to the perpetrators.

Also, out of the hundreds of cases studied, Lott and Landes fail to present a single case where a concealed handgun was used to limit the carnage in a mass public shooting. The closest they come is the description of two cases where a civilian gun was used for defense, one involving a shotgun and one involving a handgun retrieved from a car.

Lott and Landes present an analysis in table 10 that seems to indicate that the shall-issue law reduced the number of deaths per incident by 2.2, a figure much greater than the number of deaths per incident in states without shall-issue laws. This once again suggests that something is wrong with the model.

### **3.3 Conclusion**

Even if we decide that there were more guns and less crime it is wrong to conclude that “more guns caused less crime”. The mechanism for causation proposed by Lott is not consistent with the empirical evidence on how often criminals encounter armed permit holders. The observed changes in crime rates are consistent with past changes in crime rates caused by factors other than concealed weapons and are inconsistent with the changes we would expect concealed carry to cause.

## **4 Conclusion**

It would be nice if there was a simple and inexpensive way to reduce crime significantly, but unfortunately the world does not usually work that way and we should be careful to avoid wishful thinking on this subject.

Lott is undoubtedly sincere in his belief that more guns caused less crime and one cannot be other than impressed by the energy he has devoted to

marshaling the evidence in favour of his proposition and dismayed by the tactics of some of his opponents that he describes in chapter 7. However, he is none the less completely wrong—there weren't significantly more guns, there wasn't less crime, and the mechanism for causation just isn't there.

There may be good reasons for a state to introduce “right-to-carry” laws but reducing crime is not one of them.

## **Lott's responses to criticisms**

In chapter 7, Lott responds to many of the criticisms of his original study and in chapter 9 of the second edition he responds to criticisms of the first edition of his book. In this section, rather than just comment on the responses that I disagree with, I will also note where Lott has adequately dealt with the criticism so that readers can get an overall impression of how well Lott has responded to his critics.

In the list below, “7.1” refers to criticism number 1 in chapter 7.

### **In Chapter 7**

#### **7.1 Is the scale of the effect realistic?**

Lott's response is completely inadequate. As shown in section 1.3 permit holders are much less, not more, likely to encounter violent criminals.

His argument here is not even internally consistent. In his last paragraph he argues that if permit holders face the same risk of being attacked as everyone else, only 0.65% of permit holders need to thwart an aggravated assault to account for the observed drop in the assault rate. But in the previous paragraph he stated that only 0.18% of the population are victims of aggravated assault, so if permit holders face the same risk as everyone else, only 0.18% of them will even have a chance to thwart an aggravated assault. This is much less than 0.65%, so if permit holders face the same risk, it is not possible for 0.65% of them to thwart an aggravated assault.

The fact that permit holders are less likely to be crime victims makes this comparison even worse for Lott.

## **7.2 The importance of “crime cycles”**

Lott is correct that he controls for national crime cycles, but the state time trends do not control for cycles at the state level.

Lott claims that the reductions in crime begin right when the carry laws were passed and that it too much of a coincidence to expect a crime cycle to have peaked exactly when the law was passed. However, as shown in section 3.2.8 the fact that the declines in Lott’s graphs begin at the time of the law is an artifact of the way the graphs were created—it is not easy for a decline to begin anywhere else in his graphs.

## **7.3 Did Lott assume that there was an immediate and constant effect from these laws and that the effect should be the same everywhere?**

While it is true that Lott also included models where the effect increased with time, the model given greatest prominence in his paper is the one with an immediate and constant effect. The abstract of that paper [31] states:

If those states without right-to-carry concealed gun provisions had adopted them in 1992, county- and state-level data indicate that approximately 1,500 murders would have been avoided yearly. Similarly, we predict that rapes would have declined by over 4,000, robbery by over 11,000, and aggravated assaults by over 60,000.

These numbers are based on the constant-effect model, so it does not seem unreasonable for critics to concentrate on that model.

## **7.4 When were these concealed-handgun laws adopted in different states?**

While there do seem to be some problems in classifying when Maine and Virginia passed their laws, showing that the results do not depend on Maine or Virginia is a satisfactory response.

## **7.5 Should robbery be the crime most affected by the adoption of the non-discretionary law**

This is discussed in detail in section 3.2.1.

### **7.6 Do concealed-handgun laws cause criminals to substitute property crime for rape?**

Lott does not seem to have even understood the criticism. Webster wrote “theft is the motive for only a small fraction of the violent crimes for which Lott and Mustard find shall-issue effects”. Lott responds that robberies made up 34% of violent crimes, missing Webster’s point that in his main analysis (table 3 of the paper and the results reported in the abstract), Lott and Mustard did not find a statistically significant effect on robbery.

### **7.7 Comparing crime rates for two to three years before non-discretionary laws go into effect with crime rates for two to three years after the passage of such laws**

This criticism is from a draft of Black and Nagin’s paper [5] and does not appear in the final version. It does not seem necessary for Lott to reply.

### **7.8 The impact of including Florida in the sample**

This is discussed in detail in section 3.2.3.

### **7.9 The impact of including Maine in the sample**

Lott shows that excluding Maine has very little effect. I suspect that the critic has confused Maine with Florida.

### **7.10 How much does the impact of these laws vary across states?**

This is discussed in detail in section 3.2.3.

### **7.11 Do the coefficient estimates for the demographic variables make sense?**

This is discussed in detail in section 2.2.

### **7.12 Can we compare counties with discretionary and non-discretionary concealed-handgun laws?**

Lott is correct in noting that it is the change in the number of permits that matters. However, he has that data for only a few states, which does not

show consistent effects, possibly because there is so little data.

### **7.13 Should changes in the arrest rate be accounted for when explaining changes in the crime rate?**

Lott argues that he accounted for the fact the crime rate and the arrest rate mutually effect each other with a two stage least squares analysis (2SLS). He claims that the 2SLS estimates provide “even stronger evidence that concealed handguns deter crime”. This claim is wrong. It is true that the crime reductions associated with the laws are larger in the 2SLS analysis, but they are so much larger (a 67% reduction in homicide and a 65% reduction in rape) that they suggest that the model is incorrect.

Fortunately for Lott, his second argument is a better one—excluding arrests from the analysis has little effect on the results.

### **7.14 Are the graphs in Lott’s book misleading?**

Webster writes “What is not obvious to the casual observer of the graphs is that each data point represents an aggregate average for states that liberalized their gun-carrying laws, but the states that make up the average are not the same each year.” Lott denies that the graphs are misleading, but the test for whether the graphs are misleading is whether people have been misled, and it is clear that Webster has been misled by Lott’s graphs. The data points on the graphs do not represent averages for states that liberalized their laws. Rather, the points are not data points at all, but just show where the curve fitted through the data points goes.

More about the problems with Lott’s graphs is in section 3.2.8.

### **7.15 Should concealed-handgun laws have differential effects on the murder rates of youths and adults?**

This is discussed in detail in section 3.2.6.

### **7.16 Are changes in the characteristics of victims consistent with the theory?**

Lott’s counterargument is that the observed changes in victim characteristics is not statistically significant. However, if the observed decrease in murder was caused by the laws, you would expect a change in the characteristics of

victims. Failure to observe such a change is evidence that the change in the homicide rate was not caused by the gun laws.

### **7.17 Do non-discretionary concealed-handgun laws only affect crimes that occur in public places?**

Lott argues that concealed carry laws could cause an increase in gun ownership and hence deter crimes in homes and other private places. However, Lott himself noted (page 28) that a Texas poll suggests that 97% of first-time applicants for concealed-weapon permits already owned a handgun, that is, concealed carry laws do not significantly increase gun ownership.

### **7.18 Is it reasonable to make comparisons across states?**

Lott argues that while this objection was plausible when only a few states had implemented the laws, now that there is data from many states, the objection is now longer plausible.

The problem here is that while there is now data from many states, there is not much data from those states that have only recently changed their laws, and the results are dominated by the few states that have had their laws for a long time. When more data becomes

### **7.19 Does Lott’s discussion provide a “theory” linking concealed-handgun ownership to reductions in crime?**

Lott writes “The theory is obvious: A would-be criminal is deterred by the risk of being shot.” However, as discussed in section 3.1, the change in that risk caused by concealed-carry law is negligible.

Lott goes on to argue that he can link the laws to the crime reductions because:

- “Not only does the drop in crime begin when non-discretionary laws are adopted.” This is wrong, as shown in section 3.2.8
- “the extent of the decline is related to the number of permits issued in the state.” This is also wrong, as shown in section 3.2.3
- “Non-discretionary laws reduce crime most in the areas with the greatest increase in the number of permits.” Lott has not shown this at all. See section 3.2.9.

- “crimes that involve criminals and victims in direct contact [consistently decrease the most, pdftex]” This is not true. See section 3.2.2.
- “crimes occurring in places where the victim was previously unable to carry a gun are the ones the consistently decrease the most” Nor is this true. See section 3.2.1.

## 7.20 What can we infer about causality?

The question at issue is the possibility that some other factor than the gun law caused the reduction in crime.

Lott does have a good point when he argues that it would be better if critics who argue that some other factor caused the reduction in crime would specify what that factor was, however, as Lott concedes, it remains possible that some unknown factor caused the crime changes. Lott’s own analysis that found crime trends before the laws were passed demonstrates that there were factors operating that were not explained in his model.

Lott goes on to argue that the reductions were not caused by “other factors” because:

- “States that were expected to issue the greatest number of new permits . . . observed the largest decreases in crime.” Of more relevance is the fact that states that in fact issued the smallest number of new permits observed the largest decrease in crime. See section 3.2.3.
- “the number of concealed-handgun permits in a state rises over time, so we expect to see a greater reduction in crime after [ . . . , pdftex ] several years” This is also consistent with normal up and down trends in crime rates.
- “where data on the actual number of permits at the county level are available, we find that the number of murders declines as the number of permits increases” Since the number of permits just increased with time, this is also consistent with a crime trend.

## 7.21 Concerns about the arrest rate due to missing observations

Lott devotes a whole page to criticism of of Black and Nagin [5] conducting their analysis using only counties with populations of more than 100,000,

implying that they were searching for a subset of the data to show that the laws had no effect.

This is a very strange criticism, since Lott and Mustard did exactly the same the thing: Page 35 of [31]

We reran all the regressions in this section first by limiting the sample to those counties over 10,000, 100,000, and then 200,000 people. Consistent with the evidence reported in Table 7, the more the sample was limited to larger population counties the stronger and more statistically significant was the relationship between concealed handgun laws and the previously reported effects on crime.

That is, according to Lott and Mustard's original paper, Black and Nagin's analysis was biased *towards* finding a beneficial effect for the gun laws.

Lott then writes:

Despite ignoring all these observations, it is only when they *also* remove the data for Florida that they weaken my results for murder and rape.

This statement is false. Black and Nagin report that removing Florida makes the effects on murder and rape not statistically significant whether or not the analysis is restricted to large counties.

## **7.22 What can we learn about the deterrent effects of concealed handguns from this study?**

Lott doesn't really address the criticism in his answer. The criticism is directed at something Lott writes at the end of the chapter—that even his critics are correct and the models misspecified, the gun laws have no effect.

As Zimring and Hawkins note, this is not correct. If the critics are correct and the models wrong, you cannot draw any conclusion about the effect of the gun laws on crime. It would be possible for the laws to cause an increase, a decrease or to have no effect.

## **7.23 Summarizing the concerns about the evidence that concealed-handgun laws deter crime.**

Lott's answer is about the state of the academic debate. This answer is updated in section 9.16, so it will be dealt with there.

He also complains about “the reluctance of gun-control advocates to release their data”, mentioning Kellermann’s study (data available from the ICPSR, study 6898) and the Police Foundation study on gun ownership and use (data available from the ICPSR, study 6955). While Lott may have had trouble getting this data, it is publicly available on the Internet.

## **In Chapter 9**

This chapter only appears in the second edition of “More Guns, Less Crime”.

### **9.1 How do we know that these findings are not a result of the normal ups and downs in crime rates?**

Lott starts with a false claim “Even my most determined critics concede one point: violent-crime rates fell at the point on time that the right-to-carry laws went into effect.” As far as I know **none** of Lott’s critics have conceded this point, possibly because there is no good evidence that it is true (as shown in section 3.2.8).

Lott goes on to argue that the reductions were not just the result of crime trends because

- “the size of the drop is closely related to the number of permits issued” Since the number of permits just increased with time, this is also consistent with a crime trend.
- “it is not just the number of permits, but also the type of people who obtain permits is important” Lott doesn’t have any data on the type of people who obtain permits. What he does is fit a model that predicts the number of people that obtain permits in a state. (Incidentally, this model predicts that Texas issued a negative number of permits in the years after it passed its shall-issue law.) He then finds that the number of permits predicted by this model is correlated with crime decreases. He then infers that since this model contains a term that the number of permits issued is reduced by an increased cost of a permit, and since poor people might be expected to be more deterred by higher fees that this means that poor people getting permits has a greater effect on crime than others.

However, because the model he fits contains a term that includes the number of years since the carry law was passed, this result is also consistent with a crime trend.

- “why do violent-crime rates start rising in adjacent counties in states without right-to-carry laws?”. The answer is that they don’t (see Section 3.2.7).
- “as the period of time studied gets progressively longer, the results are less likely to be due to crime cycles, since any possible crime ‘cycles’ involve crime not only going down but also up.” I reviewed Lott’s results to see how they changed as the time period got longer. Unfortunately, since the specification of the model in table 9.1 is different from that in tables 4.8 and 4.13 the only comparison that can be made is that between the results in table 4.8 and 4.13. This shows the effect on violent crime lessening from -0.9% to -0.5% when the period of time studied increased by two years. This seems more consistent with a crime cycle than with Lott’s theory.

## **9.2 Does it make sense to control for nonlinear time trends in every state?**

Here Lott makes a very strange argument—that including nonlinear trends for regions is reasonable but including nonlinear trends for individual states makes no sense. If it reasonable for crime rates to follow nonlinear trends in regions, then surely it is reasonable for crime rates to follow nonlinear trends in individual states. Lott argues that a crime rate that was rising before the carry law and as a result of the law started falling after the law would be fitted by a nonlinear trend, making it look like the law had no effect. Equally, you could argue that if a crime rate that following a nonlinear trend which peaked before the carry law, Lott would fit it with a model that showed an increase until the law was passed and then a decrease afterwards, making it look like the law had an effect when nothing special happened to crime rates when the law was passed.

To resolve the situation it would be necessary to fit a model with both individual nonlinear trends and a parameter that allowed the trend to change at the time of the carry law. We could then test to see if the change in the trend was statistically significant.

Unfortunately, while some of the models considered by Lott contain a parameter that measures the change in a trend at the time of the carry law, none of them contain nonlinear trends for individual states (and so would be expected to fail the Heckman-Hotz tests for misspecification). And while the model considered by Black and Nagin contains nonlinear trends for individual states it only allows the carry law to change the crime rate, and not the crime trend.

To summarize, Lott is wrong when he claims that nonlinear trends in each state make no sense, and we don't know whether his results on changes in crime trends associated with the carry laws would still be significant if nonlinear trends in each state were controlled for.

### **9.3 Should one expect an immediate and constant effect from right-to-carry laws with the same effect everywhere?**

In the original paper written with David Mustard, the model with an immediate and constant effect was clearly the primary one—the results from it were the ones given in the abstract and the other models considered were variations on it. Black and Nagin were responding to that paper, so it was quite reasonable for them to focus on that model. They found that that model failed the Heckman-Hotz tests for misspecification.

Now that Lott has moved to a different preferred model, the appropriate way to answer their objections would be test to see if the new model satisfies the Heckman-Hotz tests. Lott has not done this.

### **9.4 Can changes in illegal drug use explain the results?**

Lott argues that the changes in crime rates are unlikely to have been caused by changes in illegal drug use because:

- “Neighboring counties without right-to-carry laws directly on the other side of the border experienced an increase in violent crime precisely when the counties adopting the law were experiencing a drop.” This is incorrect. Neighboring counties did not have an increase in violent crime (see Section 3.2.7).
- “The timing of changes in right-to-carry laws also makes their argument less plausible.” However, the changes in crime rates caused by drug use do not have to exactly coincide with the carry laws do make it look as

if the carry laws reduced crime in the states which did not experience an increase in drug use.

### **9.5 Do right-to-carry laws significantly reduce the robbery rate?**

This is part of Lott's response to this document. My rebuttal is in section 4.

### **9.6 Is the way criminals learn about victims' ability to defend themselves inconsistent with the results?**

Lott argues that the data strongly suggests that criminals respond to the actual increased risk rather than the announcement of the carry laws. However, all of the data he mentions is also consistent with a crime trend and Lott does not mention the data that shows that the increased risk was negligible (see Section 1.3).

### **9.7 Have prominent "pro-gun" researchers questioned the findings in Lott's book?**

Lott actually responds again to the same Kleck quote in section 9.14, except that in that section he mistakenly attributes the quote to me. Lott writes "Let me try to explain the meaning of Kleck's quote", and then fails to explain the meaning of the quote.

The explanation of the quote is simple. As the quote from Kleck inside the front cover of "More Guns, Less Crime" indicates, Kleck has no problem with the design or methodology of Lott's study. Kleck disagrees with Lott's conclusions because Kleck's own research indicates that a significant number of people carry guns for protection even without carry laws. The number of carry permits issued represents a relatively small increase in the number of people carrying, insufficient to cause the crime decreases that Lott claims the carry laws caused.

### **9.8 Do concealed-handgun permit holders pose a risk to others?**

It is true that permit holders are less likely to commit crimes than the average person. This is because a people with criminal records are not eligible for permits. However, Lott understates the risk they pose to others. In Texas, three permit holders have been convicted of murder, while another permit holder used his gun to kill himself after committing a murder [44].

### **9.9 Are the CBS and Voter News Service polls accurately reflecting how gun ownership rates vary across states?**

Lott completely fails to address any of the criticisms of his use of these surveys. For details, see Section 1.1).

In a later paper on safe storage laws [32] Lott and Whitley do not use these exit polls. Instead they use GSS surveys to measure how gun ownership changes as a result of a state passing a safe storage law. Using these polls they find that gun ownership declined by one percentage point per year in the states with the laws and argue that the laws caused increases in crime rates.

Lott does not explain why, after stoutly defending his use of the exit polls to measure changes in gun ownership at the state level he abandoned them for his later paper. One possible explanation is that the exit polls say the opposite thing to the GSS surveys. The exit polls show substantial **increases** in gun ownership in the states that passed safe storage laws. I computed a regression relating the change in gun ownership as measured by the exit polls to the number of years that a safe storage law had been in place and found that the laws were associated with a 0.06 percentage point per year **increase** in gun ownership rates. This increase is not statistically significant, but it is the opposite sign to Lott's result using the GSS surveys.

Since the exit polls give the opposite result from the GSS surveys it is quite possible that if Lott has conducted his analysis in chapter 3 using the GSS surveys he would have gotten the opposite result and found that more guns were associated with more crime.

It is not good practice to choose your data source to get the result you desire.

### **9.10 Has Lott ignored the costs of gun violence?**

Lott answers this criticism satisfactorily, since, as he points out, his models include the cost of violence.

### **9.11 What happens to the evidence when Florida and counties with fewer than 100,000 people are removed from the sample?**

The results of excluding Florida were discussed see Section 3.2.3).

### **9.12 Are the results valid only when Maine and Florida are included?**

While Lott shows that if you look at changes in the trends rather than the changes in the rates there is still a significant change associated with the law in the states other than Maine and Florida, his results also show that issuing a permit in Maine and Florida had four times the effect on violent crime as issuing a permit in the other states. They also show that issuing a permit in Maine and Florida reduced property crime just as much as it reduced violent crime. This suggests that the results in those states were the product of general decreases in crime relative to other states and not the carry laws.

### **9.13 Was it proper to assume that more permits were issued in the more populous counties after right-to-carry laws were adopted?**

Since Lott has county level data on permits for some states, an appropriate response here would have been to have used this data to test to see if permits were actually issued at a greater rate in more populous counties. It is puzzling that he did not do this. The one extra piece of information that he provides in his answer seems to be evidence against his theory—he finds bigger drops associated with a change from discretionary to non-discretionary permits than from no permits to non-discretionary permits, even though you would expect a greater change in the number of permits in a change from no permits than in a change from discretionary permits.

### **9.14 Did the passage of right-to-carry laws result in more guns being carried in public places?**

This is part of Lott's response to this document. My rebuttal is in section 4.

### **9.15 Shouldn't permit holders be required to have the same type of training as police officers?**

I agree that because of different circumstances citizens do not necessarily require the same training as police, but Lott contradicts himself in his comments here, stating both that "Training requirements improve the deterrence effect for concealed-handgun laws" and that the "effect of increased training is clearly to reduce the deterrent effect".

## 9.16 Where does the academic debate stand?

### Errors in *More Guns, Less Crime*

Unless otherwise specified, the page numbers below are the same in first and second editions of Lott's book. Unfortunately, Lott has corrected only one of these errors in the second edition, and that "correction" turns an error into what is probably a lie.

**Page 3, pdftex** Lott writes "If national surveys are correct, 98 percent of the time that people use guns defensively, they merely have to brandish a weapon to break off an attack." In fact, Kleck's survey [22] indicates that 24% fired the weapon and the NCVS indicates that 38% fired the weapon [40]. Five other surveys give numbers between 34% and 67% [21].

In the second edition Lott changes "national surveys" to "a national survey that I conducted". This is a fabrication. There is no evidence that Lott ever conducted such a survey. Lott provides some details about what he claims to have done in a letter to *The Criminologist* [28] and a phone conversation with James Lindgren [23].

- The survey was conducted over three months in early 1997.
- 2,424 people were surveyed. (This would have required well over 10,000 phone calls and about five full-time interviewers over the three months).
- The interviewers were students and Lott does not remember any of their names.
- Lott did not retain any records of reimbursing the students for their phone calls.
- The data was lost in a computer crash.
- Lott does not remember what happened to the tally sheets or what the questions were.

Unfortunately for Lott, he first made the claim about 98% brandishing on Feb 6, 1997 [24] *before* the survey he alleges it came from was completed. Even if he did conduct a survey (unlikely, given the lack of evidence that he did), he has not told the truth about the origin of the 98% figure.

found that the probability of serious injury from an attack is 2.5 times greater for women offering no resistance than for women resisting with a gun. In contrast, the probability of women being seriously injured was almost 4 times greater when resisting without a gun than with resisting with a gun.

...

Men also fare better with guns, but the differences are significantly smaller. Behaving passively is 1.4 times more likely to result in serious injury than resisting with a gun. Male victims, like females also run the greatest risk when they resist without a gun, yet the difference is again much smaller: resistance without a gun is only 1.5 times as likely to result in serious injury than resistance with a gun. The much smaller difference for men reflects the fact that a gun produces a smaller change in a man's ability to defend himself than it does for a woman.

Southwick's description of his findings is very different from Lott's (page 362 of [43])

Table 6 also reports the number of serious injuries received by those who choose each set of actions. The only significant difference here is in the likelihood of receiving an injury if one takes no action.

What Southwick noted and Lott failed to report to his readers is that *none* of the numbers Lott reported are statistically significant.

Here are 95% confidence intervals (calculated from the data in table 6 of [43]) for the injury rate ratios mentioned by Lott:

Ratio	value	lower limit	upper limit
female: no resist/gun	2.5	0.35	17.4
female: resist/gun	4.0	0.57	28.1
male: no resist/gun	1.4	0.85	2.3
male: resist/gun	1.5	0.91	2.4

*All* of the confidence intervals include the number 1. This means that resisting with a gun is not associated with a statistically significant lower injury rate for women or men. Nor are the differences between the ratios for men and women statistically significant.

Even if the ratios were statistically significant they would not prove that resisting a criminal attack with gun is a safer choice than not offering resistance. Correlation is not the same as causation. The correlation in the NCVS data could equally well come about if serious injury made with-gun resistance less likely or if there were other differences between with-gun resisters and others. The BJS warns against drawing the conclusions that Lott does [40].

**Page 5, pdftex** The “hot burglary” rate in Canada is 9% [41] which is less than than that for the US. The “hot burglary” rate in England is indeed higher than that for the US, but if that is enough to conclude that guns cause this difference, we should also conclude that guns cause the US homicide rate to be far higher than that of England. Lott also claims that American burglars spend more time than their foreign counterparts “casing” a house. This is pure speculation—he has no evidence at all for this claim.

Cook and Ludwig conducted a multi-variate analysis of gun prevalence and burglary in the US [7] and found that where there were more guns the burglary rate was actually higher (that is, more guns, more burglary). Moreover, the “hot burglary” rate was not lower where there were more guns.

**Page 10, pdftex** Lott states that child-resistant bottle caps actually have resulted in “3,500 additional poisonings of children under age 5 annually from [aspirin-related drugs, pdftex] . . . [as] consumers have been lulled into a less-safety-conscious mode of behavior by the existence of safety caps.” I reviewed the literature on this topic and found that the only study to conclude that child-resistant packaging caused harm is the one cited by Lott. All the other studies (none of them mentioned by Lott) found substantial reductions in poisonings [42]. The most recent and sophisticated study [42] found a 45% reduction in deaths. Furthermore, no evidence for the claimed “lulling” effect exists—studies have found that people do not store medicines with child-resistant caps differently from medicines without them.

**Page 11, pdftex** Lott claims that the National Crime Victimization Survey does not weight regions by population and relies too heavily on urban data. Lott offers no evidence for this claim and apparently would have us believe that the NCVS has been conducted incompetently for over 25 years and no-one has noticed and made the trivial fix to the problem. He also falsely claims that a law-enforcement agency asks the NCVS questions. In fact, the NCVS is conducted by the Census Bureau, which is not a law-enforcement agency.

**page 11, pdftex** Lott claims that “fifteen national polls. . . imply that there are 760,000 defensive handgun uses to 3.6 million defensive uses of any type of gun per year”. However, the reference he cites [22] gives a table containing data from thirteen polls. However, three of the polls are not national polls, but are confined to a single state and two of the polls do not yield an estimate at all. Two later surveys [22, 6] could be added to this yielding a total of ten “national polls”.

In a reply [28] to an article by Otis Dudley Duncan [13] that pointed out this error, Lott compounds his error. Rather than admit to making a mistake, he falsely claims that the table contains fifteen polls. It doesn’t—there are thirteen polls listed in that table [22].

**Page 24, pdftex** Lott grossly misrepresents Kellermann’s study. He states that “they fail to report that in only 8 of these 444 homicide cases could it be established that the gun involved had been kept in the home.” Kellermann et al do indeed fail to report that, but that is because it is not true. They do note that in 8 out of a subset of 14 cases the police report stated that the gun involved had been kept in the home. Needless to say, 14 is not equal to 444. Lott goes on to claim that “all or virtually all the homicide victims were killed by weapons brought into their homes by intruders”. This claim is also false. Table 1 of Kellermann’s paper [19] shows that only 14% of the homicide victims were killed by intruders. My analysis of Kellermann’s data shows only 8% of the homicide victims were killed with guns by intruders.

This is not the only way that Lott has misrepresented Kellermann’s study. He claims that the case-control method, as used by Kellermann, was not designed to study these sort of issues because other factors could cause a correlation between gun ownership and homicide. This claim is also false. Lott fails to tell his readers that Kellermann did

a multivariate analysis, controlling for dozens of other factors. Earlier (page 4), a simple correlation without controlling for other factors was enough for Lott to conclude causation when it suited his purposes.

**Page 52–53, pdftex** In table 4.1 he claims that carry laws cause a 7.7% decrease in murder. However, in table 3 of the Lott-Mustard paper the result of the same regression is shown as a decrease of -0.0765 in the natural log of the murder rate, which is a decrease of  $1 - e^{-0.0765} = 7.4\%$ . Lott has obtained his percent changes by multiplying the change in the natural log of the crime rates by 100. This is only an approximation to the correct value, and for large changes, not a good one. Every single percent change given in this table and the other tables giving percent changes (tables 4.3, 4.4, 4.5, 4.6, 4.8, 4.9, 4.10, 4.11, 4.13, 4.14, 5.1, 5.4, 5.5, 5.6, 5.7, 9.1, 9.3, 9.4, 9.6 and A5.1) is calculated incorrectly.

**Page 54, pdftex** Lott claims that there were only 200 accidental handgun deaths in the US in 1988. While it may be true that there were 200 gun deaths where a handgun was identified as the gun type involved, in most accidental deaths the gun type was not identified, and it is incorrect to assume, as Lott does, that in none of these a handgun was involved. If we assume that the handgun percentage for these was the same as for the ones where the gun type was identified then there were 632 accidental handgun deaths [21].

**Page 68, pdftex** Lott claims that blacks benefit more than other groups from concealed-handgun laws. However, his own table 5.2 shows that blacks did not benefit more.

**Page 113, pdftex** Lott's claim that there is no worldwide relationship between gun ownership and crime rates is false. Using gun ownership data from the International Crime Victimization Survey Killias found significant correlations between gun ownership and homicide rates [20].

**Page 160 (161 in 2nd edition), pdftex** Lott falsely claims that women and blacks benefit the most from concealed-handgun laws in terms of reduced rates of murder. His own table 5.2 shows that the change in murder rates was the same for blacks and whites and for women and men.

**Page 160 (161 in 2nd edition),pdf** Lott claims that the “halo” effect from shall issue laws also protects juveniles who are not eligible for concealed weapon permits. As discussed in Section 3.2.6 the fact that juvenile homicides declined as much as adult homicides is evidence that the crime decreases were not caused by the shall issue laws, not evidence for extra benefits from such laws. It seems as if Lott can interpret any outcome at all as evidence for benefits from shall issue laws.

**Page 32 of [27],pdf** Lott claims that “Guns are used for defensive purposes about five times as often as they are used for crimes.” In fact, the National Crime Victimization Survey indicates that the number of gun crimes (about 850,000 in 1996 [38]) is about twelve times as much as the number of defensive gun uses (about 72,000 in 1996 [38]). This is surely not surprising—criminals are more likely to be involved in a situation where a gun might be useful, and so have more incentive to carry a gun. They can also only choose to commit crimes on the occasions when they are carrying a gun.

Lott arrives at his claim by taking the lowest available estimate for gun crimes (430,000 from the FBI’s UCR) and a high estimate for defensive gun uses (An average of the estimates computed by Kleck [21], omitting the NCVS estimate). While that produces a ratio favourable to Lott’s position, it is impossible for both estimates to be correct. According to the respondents in Kleck’s survey (which is the basis for all the estimates computed in [21]) one fifth of his estimated 2.5 million defensive gun uses were against gun crimes, implying that every single time a criminal committed a gun crime, they encountered an armed victim. This is clearly impossible.

## Comments on John Lott’s response

John Lott has been kind enough to make a response to this document.

### Do I misquote?

Lott claims that much of what I write “involves misquoting of different sources”. Unfortunately, he does not provide any examples of these alleged

misquotes, so it is not possible for me to correct these errors, if in fact there are any.

The only clues Lott gives me is references to two papers, one by Bartley and Cohen, and one by Plassmann and Tideman. I don't include any quotations from these papers, so perhaps Lott is trying to say that I didn't accurately report their conclusions.

Bartley and Cohen [4] summarize their conclusions in their abstract:

We find that the deterrence results are robust enough to find them difficult to dismiss as unfounded, particularly those findings about the change in violent crime trends. The substitution effects are not robust with respect to different model specifications.

I wrote in section 2.3:

if we restrict things to just models that include a trend component, homicide and robbery show consistent reductions. For this reason, Bartley and Cohen argued that Lott's results should not be dismissed as unfounded.

Lott also claims:

Bartley has another piece in Economic Letters where he describes how his paper with Cohen provides "strong support" for the deterrence hypothesis. [3]

What Bartley actually says is:

Bartley and Cohen (1998) find that some evidence does exist from extreme-bounds analysis that violent crimes, such as rape and murder, may be reduced by the passage of these laws.

The words "strong support" do not appear anywhere in Bartley's article.

Plassmann and Tideman [39] summarize their conclusions in their abstract:

John Lott and David Mustard have argued that their county-level weighted least-squares analysis shows that the right to carry concealed handguns has a statistically significant deterrent effect on crime. However, the number of crimes committed in a county in a year is a non-negative integer that is zero or one in most

cases for some important crimes, which makes the estimates of an analysis that assumes a normal distribution unreliable. In a weighted least-squares analysis the conclusions of Lott and Mustard with respect to murders vanish when some plausible changes are made in the specification that is estimated. However, when the data are analyzed as the product of a generalized Poisson process, the average effect of shall-issue laws on the number of murders is even stronger than Lott and Mustard estimated, and the effect is estimated with much greater precision.

I wrote in section 2.3:

Plassman and Tideman [39] point out that Lott's analysis technique assumes that crime rates are normally distributed and that this is not even close to being true for low crime counties. When they made some plausible changes to the specification, the effects on murder vanished. However, when they did their own analysis assuming that the murder rate was Poisson distributed, they found an even stronger effect (a 12% decrease).

Readers can judge for themselves whether my summaries are accurate.

## **Was there substitution from violent crime to property crime?**

I'm afraid that Lott has missed the point of the passage he quotes (in Section 3.2.2) As indicated by the title, I was considering Lott's claim there had been substitution from violent crime to property crime (page 54). The basis for this claim is that table 4.1 shows a 4.9% decrease in violent crime and a 2.7% increase in property crime associated with the carry law. Lott can argue that looking at before-and-after averages can be misleading, but before-and-after averages is exactly what his claim about substitution is based on. If instead we look at changes in crime trends, table 4.8 shows that the law was associated with a decrease in the trend in the violent crime rate of 0.9% and a decrease in the trend in the property crime rate of 0.6%—not only was there no increase in the property crime trend (contrary to the predictions of the substitution hypothesis), but the change in the trends was similar for property and violent crime, more consistent with a general decrease in crime trends rather than a decrease just in violent crimes that might be caused by the carry law.

## Did the passage of right-to-carry laws result in more guns being carried in public places?

Firstly, the passage Lott quotes (in Section 1.2) and attributes to me was mostly written by Kleck.

Secondly, the survey results did not include transportation of guns and hunting, but only counted guns carried for self-protection. The details of the survey are found in chapter 6 of *Targeting Guns* [21] which is about the carrying of guns for self-protection. I am surprised to find that Lott is unaware of current research on the frequency with which guns are carried for self-protection.

Thirdly, Lott argues that the percentage of people who get permits will increase in the future. However, Lott's thesis is that the relatively small number of people that got permits in the present caused a relatively large decrease in crime. It is not, I hope, that permits granted in the future are causing crime decreases in the past.

Finally, Lott asks “why did violent crime rates in neighboring counties without the law increase at the same time that they were falling in neighboring counties with the right-to-carry law?” The answer is that they didn't (see Section 3.2.7).

## Acknowledgments

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