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Comments on Aneja et al. (2014)

Aneja, Donohue, and Zhang (2014) recently released a report addressing efforts to estimate the impact of right-to-carry (RTC) laws on crime rates. These laws make it easier to acquire permits that allow their possessors to legally carry concealed weapons in public places. They address studies by Lott and Mustard that indicated there were crime-reducing effects, supposedly due to the deterrent effects of potential crime victims possessing guns, as well as reanalyses of Lott and Mustard's data by a panel of the National Research Council (NRC).

While Aneja et al. generally agree with the NRC panel that the data do not permit firm conclusions about the impact of RTC laws, and even make their own contributions to the demonstration of the sensitivity of key estimates to a variety of methodological variations, the authors cannot resist repeatedly suggesting that the passage of RTC laws increases aggravated assaults and possibly other violent crime rates as well. The authors seem to want to eat their cake and have it too – that is, to accurately note how questionable estimates of RTC laws' effects on crime are, yet still tell readers that the laws were a bad idea because they increased violent crime. This note primarily focuses on this aspect of the Aneja et al. report.

This paper serves as an object lesson of what can happen when scholars narrowly focus solely on statistical issues without regard to either theory or prior research, as if statistical estimates could interpret themselves. The authors were solely concerned with refuting the conclusions of Lott and Mustard by noting flaws in their data and statistical analyses, and doing the same to a lesser extent with regard to the NRC report, so they did their own reanalyses of data without reference to a large body of prior research that directly contradicted their premises.

The Bulk of their Estimates Indicate that RTC Laws Do Not Increase Violent Crime Rates

The authors estimated hundreds of models of county and state crime rates, with most of the estimates of the effect of RTC laws indicating no significant (at the conventional 5% level) effect on crime rates. They further show that these estimates change radically when they make methodological changes such as making cluster adjustments to estimates of standard errors or control for state trends (unit trends). Finally, they agree with other critics of the Lott studies that the county-level crime rate data on which Lott primarily relied were essentially useless for judging trends in county crime rates (p. 43; see Maltz and Targonski 2002 for details). Yet despite all this, Aneja and his colleagues nevertheless chose to repeatedly stress their marginally significant estimates supposedly indicating crime increasing effects on aggravated assault (AA), and, to a lesser extent, even weaker findings supposedly indicating positive effects on murder, rape, and robbery (Abstract, p. 2). Regarding AAs, they conclude: “Our analysis of the year-by-year impact of RTC laws also suggests that RTC laws increase aggravated assaults” (Abstract, p. 2). They concede that the findings are only marginally significant (at the 5-10% significance level) in their preferred models and are sensitive to changes in the statistical methods used, yet still chose to repeatedly stress the minority of estimates that indicate crime-increasing effects on AA. Thus, on p. 48 they state that “Table 8b once again shows highly significant evidence ... that RTC laws increase aggravated assault.” The disconnect between their empirical results and their emphases in the text is stark. In their conclusions, the authors even hint that their AA estimates are robust, though without explicitly saying so, by emphasizing that findings indicating AA-increasing effects appear “in different models and different time periods using both state and county data set in different panel data regressions” (p. 82).

This statement is, however, only true because of the authors' willingness to loosen significance standards to the 10% level, to downplay their own doubts about whether the crime rate models were properly specified, and to forget that the entire set of county-level estimates were worthless because they were based on meaningless crime data. Further, one can often produce a large set of statistical estimates supportive of one's preferred conclusions simply by introducing methodological variations that favor those kinds of findings. Thus, the fact that a large subset, even a majority, of the estimates support a given conclusion can be entirely attributable to the analysts' choices as to which methodological variations one chooses to introduce. Aneja et al. do not claim to have tested the effects of all or even most of the variations in methods that have been addressed in the research literature on RTC effects, and indeed they have not. Compare the narrow set of variations in methods they address with the far more extensive tests done by Kovandzic and Marvell (2003) and by Kovandzic, Marvell, and Vieraitis (2005).

If RTC Laws Did Increase Crime Rates, They Would Not Do So in the Patterns Indicated by the Authors' Statistical Results

The authors' position is that, even though there are problems in analyzing the bodies of data that they analyze, there is nevertheless a sound basis ("the strongest evidence of a statistically significant effect..." p. 2) for believing the RTC laws cause increases in the aggravated assault (AA) rate. They do not make equally strong claims regarding the murder rate or other crime rates, but they do point to many of their statistical estimates of RTC laws' effects that seem to indicate increasing effects on other types of crimes (see especially their "preferred" estimates in their Table 8a).

In light of prior knowledge about guns and violence, what set of results would it be reasonable to expect, if one assumed that RTC laws *do* cause crime increases? The authors suggest that RTC laws could increase violence by increasing the prevalence or carrying of guns (p. 3), so any crime-increasing effects should be observed among crime types that are most affected by firearms prevalence. The full body of prior research directly estimating the effects of gun prevalence on crime rates consistently indicates that gun availability has no measurable effect on any crime rate, with the possible exception of murder (Kleck 2015). Between 1969 and 2014 there were ten tests of the impact of gun prevalence levels on AA rates, and not a single one indicated a significant positive effect on total AA rates. Only three of the associations were even positive; the most common finding was a nonsignificant *negative* association of firearm availability and AA rates (six findings) (Kleck 2015, pp. 42-43). Gun availability might affect weapon choice, and thus the fraction of AAs involving guns, but it does not appear to increase how many total AAs are committed. Indeed, if gun prevalence has any effect on the AA rate, the evidence is more supportive of it being an *AA-reducing* effect than an *AA-increasing* effect.

Findings pertaining to robbery rates have been similarly unresponsive. From 1969 through 2014 there were 11 independent tests of the hypothesis that gun prevalence increases total robbery rates. Only one of these yielded a positive association that was significant at the .05 level, and this was balanced out by a single finding of a significant negative association. The other nine findings indicated no significant association. Thus, even if RTC laws did increase gun prevalence, there is no sound empirical foundation for expecting that it would increase robbery rates.

On the other hand, some empirical findings have indicated a significant positive effect of gun prevalence on the murder rate. Of 40 separate tests of this effect, a slight majority (21)

yielded significant positive associations. The appearance of support, however, was built entirely on poor-quality research that failed to use valid measures of gun availability, to distinguish an effect of homicide rates on gun levels from an effect of gun levels on homicide rates, and/or to control for more than a handful of confounding variables. None of the technically sound studies found support for a positive effect on murder (Kleck 2015, p. 46).

Nevertheless, the mixed findings regarding homicide do indicate that *if* any crime were affected by increased gun availability, it would be the murder rate. Conversely, the full body of prior research indicates that AA rates would *not* be affected, since gun levels have no net effect on AA rates. Thus, if RTC laws did somehow increase gun availability (more on this later), the one crime that would be most likely to be increased is the murder rate. Conversely, RTC would *not* increase AA rates.

So what do the authors' preferred state-level estimates - those in Table 8a - indicate? Precisely the opposite of what prior research would lead one to expect. The one crime they do *not* find to be affected by RTC laws is the murder rate, while their estimates indicate positive effects of RTC laws on virtually every *other* crime type!

If one takes their Table 8a findings at face value, RTC laws caused increases in rape and larceny that were significant at the 5% level – their strongest findings - despite the fact that neither crime involves offender use of guns. By definition, larceny does not involve a threat or attack with a gun; if an incident involved such elements it would be defined as a robbery rather than a larceny. And while rapes theoretically can involve offender gun use, in practice they virtually never do. For example, in the National Crime Victimization Surveys covering the U.S. for 2000-2005 inclusive, only 2.6% of rapes involved offenders who even *possessed* guns; fewer still involved offenders actually using them to threaten or attack the victims (Bureau of Justice

Statistics 2007). Likewise, burglaries and auto thefts do not involve offender use of guns since they are crimes of stealth that do not involve direct contact of offenders with victims – if they did, they would be classified as robberies. Yet, the authors’ preferred estimates also indicated marginally significant positive effects of RTC laws on burglary and auto theft rates.

How could hypothesized increases in gun possession among offenders – assuming that RTC laws did somehow produce them – cause increases in rates of rape, auto theft, burglary, or larceny even though offenders do not use guns in these crimes? Even the authors seemed to regard the results regarding property crimes as anomalous (p. 2; fn. 63, p. 81), but appear to regard offense-increasing effects on violent crimes – including offenses that virtually never involve offender use of guns – as perfectly plausible.

The Authors Present Little Explanation of How or Why the Passage of RTC Laws Would

Increase AA Rates

The only thing the authors have to say regarding why RTC laws might increase the AA rate can be found on their p. 36: “Certainly an increase in gun carrying and prevalence induced by a RTC law could well be thought to spur more aggravated assaults.” Thus, Aneja et al. suggest that (1) RTC laws increase gun availability among offenders, and that (2) greater gun prevalence would cause a higher AA rate. They appear to be either innocent of any knowledge of the prior research that directly tested the latter proposition, or chose not to share their knowledge with readers. The findings in those studies were uniformly unresponsive of the proposition that higher gun prevalence will produce higher AA rates. In any case, this one sentence provides the reader’s only clue as to how the authors think that RTC laws could increase AA rates.

The authors appear to consider it self-evident that more guns must cause more AAs (as distinct from merely causing a larger share of them to involve guns). Prior research, however, has uniformly failed to find any significant positive effect of gun prevalence on AA rates. If gun availability does not increase AA rates, how else might RTC laws increase AA rates? The question apparently was not something the authors thought they needed to address, perhaps because they were not aware of just how little support there was for the proposition that more guns will produce more AAs.

The Authors Present No Evidence that Gun Prevalence or Gun Carrying Increased After RTC Laws Were Passed

The authors do nothing to support the first position, that RTC laws increase gun availability, gun ownership, or gun carrying. They do not show this with regard to the subset of the population that would be most relevant to a claim that RTC laws increase violence, i.e. the violence-prone subset, nor do they show or cite prior evidence that indicates it occurred in the population as a whole. Perhaps they considered it so self-evident that they did not need to document these intermediate effects of RTC laws. Focusing narrowly on technical statistical issues of how to analyze their panel crime data, their atheoretical inquiry was divorced from the wider issues of how or why RTC laws might increase crime.

As it happens, the best available evidence indicates that gun carrying did *not* increase after RTC laws were passed. The claim that RTC laws increased rates of gun carrying, accepted by both supporters and opponents of RTC laws (Lott 2000; Donahue 2003), relies on the assumption that when people acquired carry permits, allowing them to legally carry guns in public places, they must have increased their rates of carrying. Those who were rigorously law-

abiding did not carry guns before they got carry permits, and only began carrying once they had the permits. Since the RTC laws increased the number of people who had carry permits, it would therefore necessarily increase overall rates of carrying, it is argued.

This assumes that people who eventually got carry permits were not carrying before it was legal to do so, or at least not doing so as often. Another possibility, however, is that people getting permits were merely legitimating what they had already been doing illegally (Kleck and Gertz 1998, p. 220). Among people doing this, their frequency of carrying would not necessarily increase at all. In May of 2001 the National Opinion Research Center fielded its 2001 National Gun Policy Survey, asking a sample of self-reported carry permit holders: “Since you’ve obtained the permit (to carry a handgun), has your frequency of gun carrying increased, decreased, or stayed the same?” Only 8% responded that they increased the frequency of their carrying, 72% said their carrying remained the same, and 11% reported that it *decreased* (the rest refused to answer or responded “don’t know”). (Roper iPoll 2012). Since there were slightly more permit holders who decreased their carrying frequency than there were who increased it (though the difference in percentages is not significant), overall gun carrying appears to have either declined slightly among those who acquired permits, or did not change at all. The legality of carrying among the remainder of the population, i.e. those who did not get a carry permit, was unaffected by the passage of the RTC laws, so there is no strong reason to expect their carrying rates to be affected one way or the other. In sum, the evidence contradicts the assumption that passing RTC laws produced an overall increase in rates of carrying guns in public places.

Aneja et al. also do not explain why laws that allowed only adult residents *without criminal records* to legally carry guns would increase gun availability among persons who commit serious violent crimes like AAs (though they insert a half-hearted, evidence-free

speculation that RTC laws might increase gun thefts into footnote 63). They do not show that any significant fraction of AAs are committed by persons without criminal records, or by persons with carry permits.

One might, however, argue that crimes among carry permit holders were the problem, that carry permit holders do commit significant numbers of AAs. Even though applicants for permits had to pass background checks showing they had no criminal convictions, perhaps violence-prone people without such a record nevertheless got permits, and because it enabled them to carry legally, they increased their frequency of carrying.

Certainly if large numbers of AAs were committed by carry permit holders, it should have been fairly easy to document this, since carry permit holders who are convicted of violent crimes have their permits revoked, and states maintain records of permit revocations. Evidence on revocations of carry permits, however, indicates that permit holders virtually never commit violent crimes with their guns. Data from Florida covering 24 years when the state's RTC law was in operation indicate that the state issued 2,047,928 concealed weapon licenses between October 1, 1987 (when the RTC law went into effect) and August 31, 2011 (when the state ceased keeping track of revocations due to crimes committed with guns), and that there were 853,272 active licenses as August 31, 2011. Yet, over this entire period, the state revoked a grand total of just 168 carry licenses due to licensees committing a crime in which a firearm was utilized – an average of just *seven gun crime convictions per year*, including gun crimes that did not involve violence (Florida Department of Agriculture and Consumer Services, 2011). Even if there were five total gun crimes actually committed by permit holders for every one that resulted in a criminal conviction and permit revocation, this would still imply only 35 gun crimes per year, not all of them violent, in a state in which there 113,641 violent crimes known to the police

in 2009 (U.S. FBI 2010). Thus, carry permit holders could not have committed more than 3/100ths of one percent of Florida's violent crimes. And even if these 35 gun crimes were spread out over 35 different permit holders, it would still mean that less than 4/1000th of one percent of active Florida carry permit holders committed any kind of gun crime. In sum, gun crimes committed by carry permit holders are so extremely rare that it is virtually impossible that they could exert a measurable effect on rates of AAs or other violent crimes.

Macro-Level Studies of Counties or States Do Not Directly Test Deterrent Effects of RTC Laws

Aneja et al. obviously disagree with Lott's contention that RTC laws had a net negative effect on crime rates, but they do appear to accept that *if* there were such an effect it would be due to the deterrence mechanism, i.e. to increased offender fears of confronting an armed criminal. It is worth noting, however, that macro-level analyses do not actually test the deterrent effects of RTC laws. Deterrence of crime requires an increase in perceived risk of committing crimes (in this case, the risk of being shot or threatened with a gun by the crime victim), and none of these studies have measured prospective offenders' perceptions of risk in any way. The very best a macro-level study might accomplish would be to produce a very rough estimate of the net *overall* effect of enacting the laws, however it was produced. Those who believe that RTC laws reduce crime necessarily assume (usually implicitly) that the variables that they *can* measure, such as whether a time period was before or after the enactment of an RTC law, can effectively serve as proxies for offenders' perceptions of risk. That is, they assume the these perceived risks of harm from gun-wielding victims *must* increase after the laws are passed.

There is no evidence whatsoever for this assumption, and considerable reason to doubt it. Research on the risks of legal punishment for doing crime indicate that perceptions of those risks

have no association with actual levels of risk (Kleck et al. 2005). There is no evidence indicating any more of an association between prospective offenders' perceptions of the risks of confronting a gun-wielding victim and actual levels of the risk. This does not mean that criminals are not deterred by the possibility that their victims might possess guns. Rather, it suggests that whatever deterrent effect victim gun possession might have exerted on offenders was not likely to have increased after RTC laws were passed.

The Supposed Improvements in Statistical Methods in the County-level Analysis

The authors' main claim to have improved statistical methods for analyzing the county-level data, relative to the National Research Council (NRC) and/or Lott and Mustard studies, is that they adjusted for the clustering of counties with states. Counties within the same state tend to have similar errors in predictions of their crime rates, which leads to underestimation of standard errors, and thereby makes associations appear more statistically significant than they actually are. Whether it is worth making this kind of subtle refinement in estimation procedures, however, depends on whether the county-level analyses were worth doing in the first place.

Aneja et al. were well aware of the problems in the county-level crime data used for these analyses, since they cite (p. 43) an article that documented grave errors that made the data essentially useless for judging the impact of RTC laws on crime rates. Many local law enforcement agencies, in many counties, for many time periods, did not report their crime figures to the Uniform Crime Reporting program, and the compilers of the county UCR crime dataset did nothing to adjust for these missing data problems (unless the agency reported 6 or more months of data for a given year). As a result, it often appears that crime went down in various counties when in fact the drop in crime counts was merely an artificial product of these missing

crime counts. Aneja et al. likewise do nothing to correct for these crippling missing data problems, yet nevertheless devote half their estimation efforts (Tables 1a through 6b) to analyzing the meaningless county-level “crime rates.”

At this late date, there is no justification for analyzing the unmodified county crime data that Lott and Mustard, the NRC, and Aneja et al. studied, for any purpose, as they cannot tell us whether crime rates increased or decreased after RTC laws were enacted. The data are worthless for estimating the effects of RTC law (Maltz and Targonski 2002), but are equally worthless for assessing the impact of introducing various changes in how the data are analyzed. Thus, they can no more use these data to establish which variations in statistical analytic technique distort estimates of RTC effects than they can use them to tell how big RTC effects are. Therefore, all the results of Tables 1a-6b can be simply ignored without loss. We focus instead on the state-level analyses in Tables 7-13, since missing values are estimated for the state crime data.

The Supposed Improvements in the Analyses of State-Level Data

How did Aneja et al. improve the state-level analyses? A number of their changes were probably appropriate. It was certainly a good idea for them to add additional years of data (thereby extending the post-law follow-up periods), to use robust estimation of standard errors (which the NRC panel did not do), to apply cluster adjustments to those estimates, and to reduce the number of highly collinear demographic variables that Lott and Mustard used.

On the other hand, it is highly questionable whether the remaining set of control variables that the authors include are effective in eliminating or even significantly reducing the omitted variables problem. The authors’ biggest claim to having usefully expanded the set of potential confounders controlled is that they controlled for police rates (lagged one year), a variable that

they assert is an “important explanatory factor” affecting crime rates (p. 7). In fact, the best available evidence indicates that the “level of police force” does not affect crime rates. Having more police per capita does not affect perceptions of the risk of being arrested and punished for crimes, and therefore cannot increase the deterrent effect of punishment. Further, the police rate cannot affect the number of criminals incarcerated because even small police forces arrest far more offenders for imprisonable offenses than can be absorbed by prisons and jails, and the criminal justice system is always able to fill the jails and prisons to capacity, regardless of how many police officers there are (Kleck and Barnes 2014). Consequently, controlling for police rates will not improve estimates of RTC effects because police rates do not affect crime rates.

The authors did add a control for incarceration rates, which do affect crime rates, but this control helps isolate the effect of RTC laws only if the enactment of those laws is correlated with trends in incarceration rates – something the authors do not document. Consequently, it is questionable whether this addition improves estimates of RTC effects.

The authors also try out different ways of modeling the impact of RTC laws, including the dummy variable approach, spline approach, and a hybrid approach that combines both dummy variable and spline approaches (see p. 6 and fn. 3 for details). They end up being ambivalent as to whether any of the approaches is clearly superior, noting limitations in all of them, and concluding that none of them is totally satisfactory (pp. 14-15). Thus, by their own judgements, it is not clear that the use of these alternative techniques can even be regarded as improvements.

Further, the authors did nothing to address the possible endogeneity of RTC laws, i.e. the possibility that crime rate trends might have influenced the enactment of RTC laws. Any positive association they found between RTC laws and crime rates therefore might reflect a

positive effect of pre-law crime rates (which are strongly correlated with post-law crime rates) on passage of RTC laws. This possibility was acknowledged by the authors (e.g., p. 11, and fn. 63), but they did nothing to address it, and they also ignore empirical evidence that it is more than a mere theoretical possibility – the Granger analysis performed by Kovandzic and Marvell (2003) supported a positive effect of crime rates on carry permit rates.

The Price of a Narrow Focus on the NRC Report and the Lott Analyses

One reasonable response to the Aneja et al. paper might be: “Who cares?” Why should anyone care about what the group of nonspecialist scholars who worked on the NRC report, none of whom were experts on the effects of firearms and gun control laws on violence, said about RTC laws? And if it has already been amply documented why Lott’s data and analysis methods were fatally flawed, why does this dead horse need to be beaten any further? Authors are, of course, entitled to choose what they like as the focus of their work, but their audience does not have to accept that the topic is worth addressing. Whether RTC laws affect crime rates is a moderately important topic; the specific ins and outs of the NRC and Lott analyses are not.

This tunnel-vision focus on just these two bodies of research unfortunately leaves out other, better research approaches. For all their challenges to the Lott/NRC approach, Aneja et al. stick with the basic model: try to relate state laws to trends in the crime rates of large aggregates. This approach has either of two serious problems that Aneja and his colleagues never fixed. First, after noting that the county-level crime data were useless for judging trends in crime, they nevertheless wasted their efforts estimating hundreds of models based on these fatally flawed county crime data (Tables 1a to 6b). In response to this data problem, they then switched to analyzing state-level data, but this only introduced another problem, that of aggregation bias.

States are extremely heterogeneous units. Most are mixtures of primarily low crime areas and a few very high crime areas, rural areas and urban areas, high gun-ownership areas and low gun-ownership areas. Generally speaking, gun ownership rates are lowest in the urban areas where crime rates are highest (Kleck 1997). The larger the units analyzed, the greater the heterogeneity, and the greater the potential for aggregation bias. Aneja et al. believe that RTC laws could increase the AA rate by increasing the rate of gun carrying. Suppose, however, that in states that passed these laws the increases in gun carrying occurred largely in suburban, small town, and rural areas, while the increases in AA rates occurred in big cities. Surely this would cast doubt on the notion that the RTC laws, and the increases in gun carrying that they supposedly produced, were responsible for the AA increases - *if* the analyst knew of these patterns. State-level analysis makes it impossible to detect them.

Thus, it would be ideal to study smaller aggregates, as Lott and Mustard originally did, but using better quality crime data. Aneja et al. did not do this, since their narrow definition of their task confined them to merely fiddling with details of the Lott/NRC analyses, such as whether one should make cluster adjustments to estimates of standard errors or include unit trend variables in their models.

Likewise, since RTC laws supposedly increase crime by increasing gun carry rates, it would have been constructive if Aneja and his colleagues had directly tested the relationship between the number of carry permit holders and crime rates, as Lott (2000) and Kovandzic and Moody (2003) did. Aneja et al. did not do this either.

It is not as if these alternative approaches are impossible. We know they are feasible because scholars have in fact implemented them. Kovandzic and Marvell (2003) analyzed good quality county-level crime data and directly measured carry permit rates. They analyzed Florida

counties because all of them had complete crime reporting of all of their constituent local law enforcement agencies, thereby avoiding the crippling missing-data problems afflicting the county-level analyses of Aneja et al., the NRC and Lott and Mustard. They found no consistent evidence of a significant positive effect of carry permit rates on any violent crime rate, including the AA rate. While some estimates indicated such an effect, others indicated a significant negative effect, and most indicated no significant effect in either direction. They also improved on previous research by addressing the causal order problem – the possibility that higher crime rates could cause more people to get carry permits. They found evidence of such reverse causation, implying that positive associations between carry permit rates and crime may actually be reflecting an impact of crime on carry rates rather than the reverse.

Kovandzic, Marvell, and Vieraitis (2005) further contributed to the literature by analyzing city-level data. Since a single city police force is responsible for reporting crime counts for each city, the problem of nonreporting law enforcement agencies was avoided. (Lott [2000, pp. 190-194] reported a city-level analysis, but his description of the study is too sketchy to know what he did. He concluded that RTC laws reduce violent crime, though the effect is not significant for the AA rate.) And by studying units of analysis that were far more homogenous than states, and even somewhat more homogenous than counties, the authors minimized the potential for aggregation bias. Applying a panel design to all U.S. cities with a population of 100,000 or larger, they concluded that: “the results provide no evidence that the [RTC] laws reduce or increase rates of violent crime” (p. 292).

In sum, when one studies smaller and more homogenous units of analysis, and uses crime data of acceptable quality, there is no support for the claim of Aneja et al. that RTC laws increase AA rates or any other violent crime rate. Significantly, Aneja et al. do not cite either of

the studies by Kovandzic and his colleagues, even though both of the Kovandzic studies used better crime data and were considerably more thorough in testing the robustness of their findings than the Aneja et al. study was.

Conclusions:

To summarize:

- (1) The topic as Aneja et al. have defined it is trivial and should be of no interest to anyone with a serious interest in the effects of gun control laws on violence. It does not matter how results in the Lott and NRC analyses would have turned out had analysts varied the analyses in this or that minor way, since the whole approach was misguided.
- (2) Their findings make no sense in the context of prior research on the effects of gun levels on crime rates, since they find “effects” of RTC laws for crimes that are not affected by gun levels (including crimes in which offenders never even use guns), while finding no effect (in their preferred models) on the one crime that might be affected by gun levels, murder.
- (3) Their approach to the topic, using either unduly aggregated units of analysis or fatally flawed crime data for smaller units, is misguided.
- (4) Better studies, using fresher approaches and superior data, find that RTC laws do not affect crime rates one way or the other. The Aneja et al. study does not provide any serious basis for reversing this conclusion.

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