

GVPedia Concealed Carry Literature Review

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Abstract

This report presents an extensive literature review of the existing academic work on the link between concealed carry and crime, as well as a thorough examination of the underlying causal pathways. While a naïve count analysis of studies from 1995-2018 reveals a very divided literature, examining the modern literature (post 2005) – which is significantly more rigorous and methodologically sound on average than older studies – finds that the majority of academic research indicates Right-to-Carry (RTC) Laws increase crime. An in-depth investigation of the causal pathways for whether RTC Laws could increase or decrease crime reinforces the verdict that these laws likely increase crime, primarily through elevating rates of gun theft. These findings highlight the fact that policies such as concealed carry reciprocity, permitless carry, arming teachers, ending gun-free zones, and allowing firearms to be carried in sensitive areas do not make communities safer and are likely to increase gun violence.

Introduction

Over the past four decades there has been a tectonic shift in the content of state laws governing the ability to carry a firearm in public. In [1981](#), nineteen states prohibited carrying firearms, and most of the country had “May Issue” laws, which granted local law enforcement discretion over who was allowed to carry a concealed handgun. Only two states had “Right to Carry” (RTC) laws that allowed individuals to carry concealed handguns as long as they passed a background check and minimum other qualifications such as passing a course. Vermont was the only state with “permitless” carry (which has since been rebranded as “constitutional” carry by gun advocates).

Fast forward to [the present](#): No states prohibit concealed carry, and May Issue laws are rare, with eight states and DC maintaining such a standard. RTC laws are the norm, with twenty-eight states adopting these relatively lax standards for concealed carry. Fourteen states require no permit at all, a list that has rapidly expanded in the past few years.

Undergirding this nationwide reversal in concealed carry laws is a fierce and ongoing academic debate over the impact these laws have on violent crime. Published in 1996, *Crime, Deterrence, and Right-to-Carry Concealed Handguns* by John Lott and David Mustard was the first attempt to quantify whether RTC laws increased or decreased crime on a national scale. Analyzing data from 3054 counties from 1977-1992, the authors concluded that, if RTC laws had been adopted nationally by 1992, 1,500 murders, 4,000 rapes, 11,000 robberies, and 60,000 aggravated assaults would have been avoided annually.

This surprising finding ignited an academic firestorm, with a significant number of studies emerging over the next two decades that both agreed and disagreed with the “more guns, less crime” hypothesis.

This report differs from previous analyses of concealed carry laws in a few important ways. First, it seeks to be comprehensive in cataloging studies and other academic work that focuses on concealed carry. Second, it provides multiple levels of analysis in order to show how the literature has evolved over time. Finally, the report presents an in-depth examination of the causal mechanisms through which concealed carry laws could influence crime.

Methodology

This report utilizes GVPedia's gun study database, a resource with more than 780 academic studies on gun violence. The studies are compiled using Google Scholar searches that query a number of academic databases. Those results are supplemented with the references of previous literature reviews as well as citations in peer review and law review articles.

Studies are classified as peer review, law review, or working papers that run their own regression or some other form of statistical analysis. Working papers that have not passed through formal peer or law review are still included if they are cited by other academic works in the literature, or are written by a scholar with other reviewed publications in the literature, or are highly likely to be published in the near future by a peer or law review journal. Working papers are clearly marked as such.

Studies are only included if the effect of concealed carry laws on crime (or the effect of permit holders on crime) is one of or the only variable of interest (independent variable). Studies that contain a concealed carry law variable as a control in a non-germane study are excluded. Including such studies would distort an otherwise accurate view of how the literature on concealed carry has evolved over time and where it stands today. Studies that combined concealed carry laws in a grouping of other policies and didn't single out Right to Carry laws (and/or May Issue laws) for analysis are also excluded.

Other academic literature falls into one of two basic groups. The first group includes critiques of studies, surveys, and/or literature reviews, all of which are exclusively focused on concealed carry laws. The second group are studies that do not focus directly on the link between concealed carry and crime rates but do examine other aspects of concealed carry. Both of these groups only contain work that is peer reviewed, law reviewed, or is a working paper by publishing scholars.

Academic literature reviews that do not exclusively focus on concealed carry laws are excluded (with the exception of the NRC Report in 2005, which was both a review and a study). A significant number of literature reviews over the past decade cover a wide scope of policies in the same review, and their inclusion would inflate the total number of academic works that find a detrimental or no effect from concealed carry. Books by academics are not included as they were not subject to formal peer or law review. Articles produced by academics in the popular press are also excluded.

Review

Our review found 59 studies from 1995 to 2018, as well as 20 other academic works that met our criteria. Of the studies, 33 were published in 2005 or before, and 26 were published after 2005. The following Tables provide the list of studies and other academic literature (the journal titles for papers not published in peer or law review have an asterisk), which are color coded for their position on concealed carry laws (green for studies finding a crime decrease or literature supporting concealed carry, red for studies finding a crime increase or literature against concealed carry, blue for studies finding no effect, purple for literature that doesn't take a position, and gray for studies that find mixed results):

Studies

Title	Journal	Year
Easing Concealed Firearms Laws: Effects on Homicide in Three States	Journal of Criminal Law and Criminology	1995
Crime, Deterrence, and Right-to-Carry Concealed Handguns	Journal of Legal Studies	1996
Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data	International Review of Law and Economics	1998
Do Right-to-Carry Laws Deter Violent Crime?	The Journal of Legal Studies	1998
Lives saved or lives lost? The effects of concealed-handgun laws on crime	The American Economic Review	1998
Criminal deterrence, geographic spillovers, and the right to carry concealed handguns	The American Economic Review	1998
The Effect of Concealed Weapons Laws: An Extreme Bound Analysis	Economic Inquiry	1998
Nondiscretionary concealed weapons laws; a case study of statistics, standards of proof, and public policy	American Law and Economics Review	1999
Will Rationing Guns Reduce Crime?	Economics Letters	1999
Packin' in the Hood?: Examining Assumptions of Concealed-Handgun Research	Social Science Quarterly	2000
Multiple Victim Public Shootings	SSRN*	2001
Testing for the effects of concealed weapons laws: specification errors and robustness	The Journal of Law and Economics	2001
Right-to-carry concealed weapon laws and homicide in large U.S. counties: the effect on weapon types, victim characteristics, and victim-offender relationship	The Journal of Law and Economics	2001

Does the right to carry concealed handguns deter countable crimes? Only a count analysis can say	The Journal of Law and Economics	2001
The impact of gun laws on police deaths	The Journal of Law and Economics	2001
Privately Produced General Deterrence	The Journal of Law and Economics	2001
More Guns, More Crime	Journal of Political Economy	2001
The Impact of Right-to-Carry Concealed Firearm Laws on Mass Public Shootings	Homicide Studies	2002
A Note on the Use of County-Level UCR Data: A Response	SSRN*	2002
Shooting Down the “More Guns, Less Crime” Hypothesis	Stanford Law Review	2003
The Latest Misfires in Support of the “More Guns, Less Crime” Hypothesis	Stanford Law Review	2003
Confirming "More Guns, Less Crime"	Stanford Law Review	2003
Measurement Error in County-Level UCR Data	Journal of Quantitative Criminology	2003
Right-to-Carry Concealed Handguns and Violent Crime: Crime Control Through Gun Decontrol?	Criminology & Public Policy	2003
The effect of concealed handgun laws on crime: beyond the dummy variables	International Review of Law and Economics	2003
The effect of nondiscretionary concealed weapon carrying laws on homicide	The Journal of Trauma	2004
Using Placebo Laws to Test "More Guns, Less Crime"	Advance in Economic Analysis & Policy	2004
Guns, Crime, and the Impact of State Right-to-Carry Laws	Fordham Law Review	2004
Right-to-Carry Laws and Violent Crime Revisited: Clustering, Measurement Error, and State-by-State Break downs	SSRN*	2004
The Impact of “Shall-Issue” Concealed Handgun Laws on Violent Crime Rates: Evidence From Panel Data for Large Urban Cities	Homicide Studies	2005
FIREARMS AND VIOLENCE: A CRITICAL REVIEW	National Research Council	2005
An evaluation of state firearm regulations and homicide and suicide death rates	Injury Prevention	2005
Systematic Measurement Error with State-Level Crime Data: Evidence from the “More Guns, Less Crime” Debate	Journal of Research in Crime and Delinquency	2005

Should Legal Empiricists Go Bayesian?	American Law and Economics Review	2007
Rebuilding at Gunpoint: A City-Level Re-Estimation of the Brady Law and RTC Laws in the Wake of Hurricane Katrina	Criminal Justice Policy Review	2007
Regression to the Mean, Murder Rates, and Shall-Issue Laws	The American Statistician	2008
The Debate on Shall-Issue Laws	Econ Journal Watch	2008
Analysis of longitudinal data to evaluate a policy change	Statistics in Medicine	2008
More Guns, Less Crime Fails Again: The Latest Evidence from 1977 – 2006	Econ Journal Watch	2009
Yet Another Refutation of the More Guns, Less Crime Hypothesis – With Some Help From Moody and Marvell	Econ Journal Watch	2009
The Debate on Shall Issue Laws, Continued	Econ Journal Watch	2009
Intimate Partner Homicide: Relationships to Alcohol and Firearms	Journal of Contemporary Criminal Justice	2009
The Impact of Right-To-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy	American Law and Economics Review	2011
Revisiting Licensed Handgun Carrying: Personal Protection or Interpersonal Liability?	American Journal of Criminal Justice	2012
The Impact of Right-to-Carry Laws on Crime: An Exercise in Replication	Review of Economics & Finance	2013
An examination of the effects of concealed weapons laws and assault weapons bans on state-level murder rates	Applied Economics Letters	2013
“Gun Control” vs. “Self-Protection”: A Case against the Ideological Divide	Justice Policy Journal	2013
The Impact of Right to Carry Laws and the NRC Report: The Latest Lessons for the Empirical Evaluation of Law and Policy	Stanford Law and Economics Olin Working Paper	2014
The deterrence of crime through private security efforts: Theory and evidence	International Review of Law and Economics	2014
Repeal of the concealed weapons law and its impact on gun-related injuries and deaths	The Journal of Trauma and Acute Care Surgery	2014
Concealed Handgun Licensing and Crime in Four States	Journal of Criminology	2015
Model Uncertainty and the Effect of Shall-Issue Right-to-Carry Laws on Crime	European Economic Review	2016

Firearm Violence and Effects on Concealed Gun Carrying: Large Debate and Small Effects	Journal of Interpersonal Violence	2016
Easiness of Legal Access to Concealed Firearm Permits and Homicide Rates in the United States	American Journal of Public Health	2017
Right-to-Carry Laws and Violent Crime: A Comprehensive Assessment Using Panel Data and a State-Level Synthetic Controls Analysis	NBER Working Paper*	2017
Association between Firearm Laws and Homicide in Urban Counties	Journal of Urban Health	2018
State Level Firearm Concealed-Carry Legislation and Rates of Homicide and Other Violent Crime	Journal of the American College of Surgeons	2018
How Do Right-to-Carry Laws Affect Crime Rates? Coping with Ambiguity Using Bounded-Variation Assumptions	The Review of Economics and Statistics	2018
Do concealed gun permits deter crime? Dynamic insights from state panel data	Working Paper*	2018

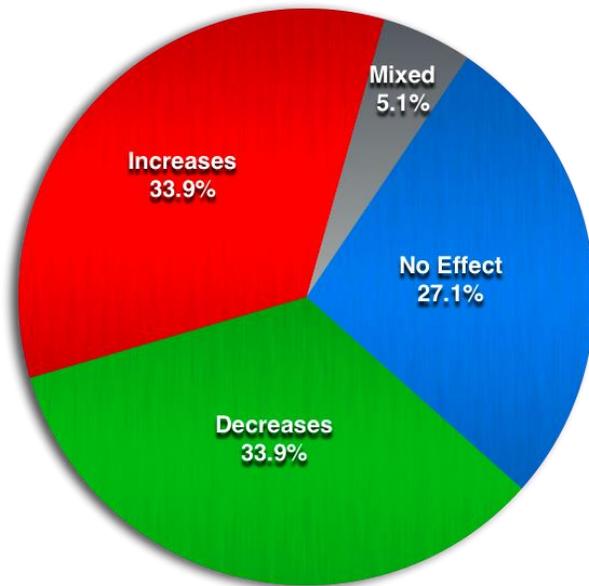
Other Academic Literature

Title	Journal	Year
“Shall issue”: the new wave of concealed handgun permit laws	Tennessee Law Review	1995
Two Guns, Four Guns, Six Guns, More Guns: Does Arming the Public Reduce Crime?	Valparaiso University Law Review	1997
Concealed Handguns: The Counterfeit Deterrent	The Responsive Community	1997
Flawed gun policy research could endanger public safety	American Journal of Public Health	1997
The Concealed-Handgun Debate	The Journal of Legal Studies	1998
Who wants a gun license?	Journal of Criminal Justice	1999
More Guns, Less Crime: A response to Ayres and Donohue	Yale Law & Economics Research Paper	1999
Myths About Defensive Gun Use and Permissive Gun Carry Laws	Berkeley Media Studies Group	2000
Gun Self-Defense and Deterrence	Crime and Justice	2000
A note on the use of county-level UCR data	Journal of Quantitative Criminology	2002
The Final Bullet in the Body of the More Guns, Less Crime Hypothesis	Criminology & Public Policy	2003

Neighborhood Predictors of Concealed Firearm Carrying Among Children and Adolescents: Results From the Project on Human Development in Chicago Neighborhoods	Archives of Pediatrics & Adolescent Medicine	2004
A neighborhood-level analysis of concealed hand-gun permits	Policing: An International Journal of Police Strategies & Management	2008
What a Balancing Test Will Show for Right-to-Carry Laws	Maryland Law Review	2012
Trust But Verify: Lessons for the Empirical Evaluation of Law and Policy	SSRN*	2012
Did J Lott Provide Bad Data to the NRC? A Note on Aneja, Donohue, and Zhang	Econ Journal Watch	2013
When Concealed Handgun Licensees Break Bad: Criminal Convictions of Concealed Handgun Licensees in Texas, 2001–2009	American Journal of Public Health	2013
Not All Right-to-Carry Laws Are the Same, Yet Much of the Literature Keeps Ignoring the Differences	SSRN*	2014
Can Easing Concealed Carry Deter Crime?	Social Science Quarterly	2015
Economists' and Criminologists' Views on Guns: Crime, Suicides, and Right-to-Carry Concealed Handgun Laws	Regulation	2016

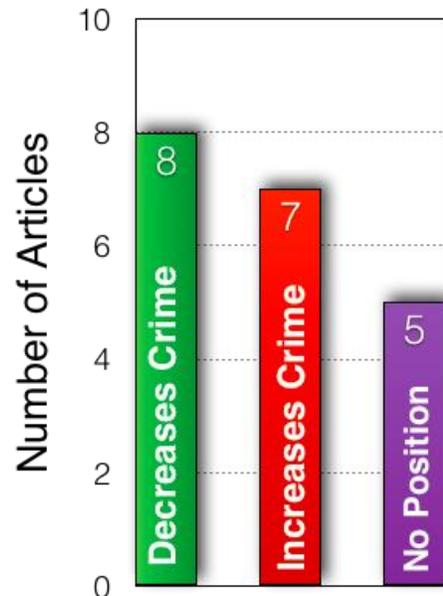
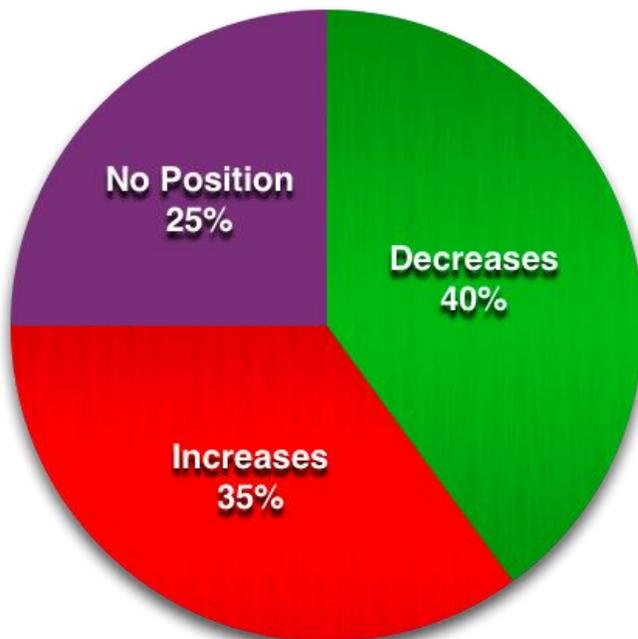
Findings

A naïve count analysis with no quality control indicates a very divided literature. Of the 59 studies identified, twenty find that concealed carry laws increase crime, twenty find a decrease, sixteen find no effect, and three find mixed results.



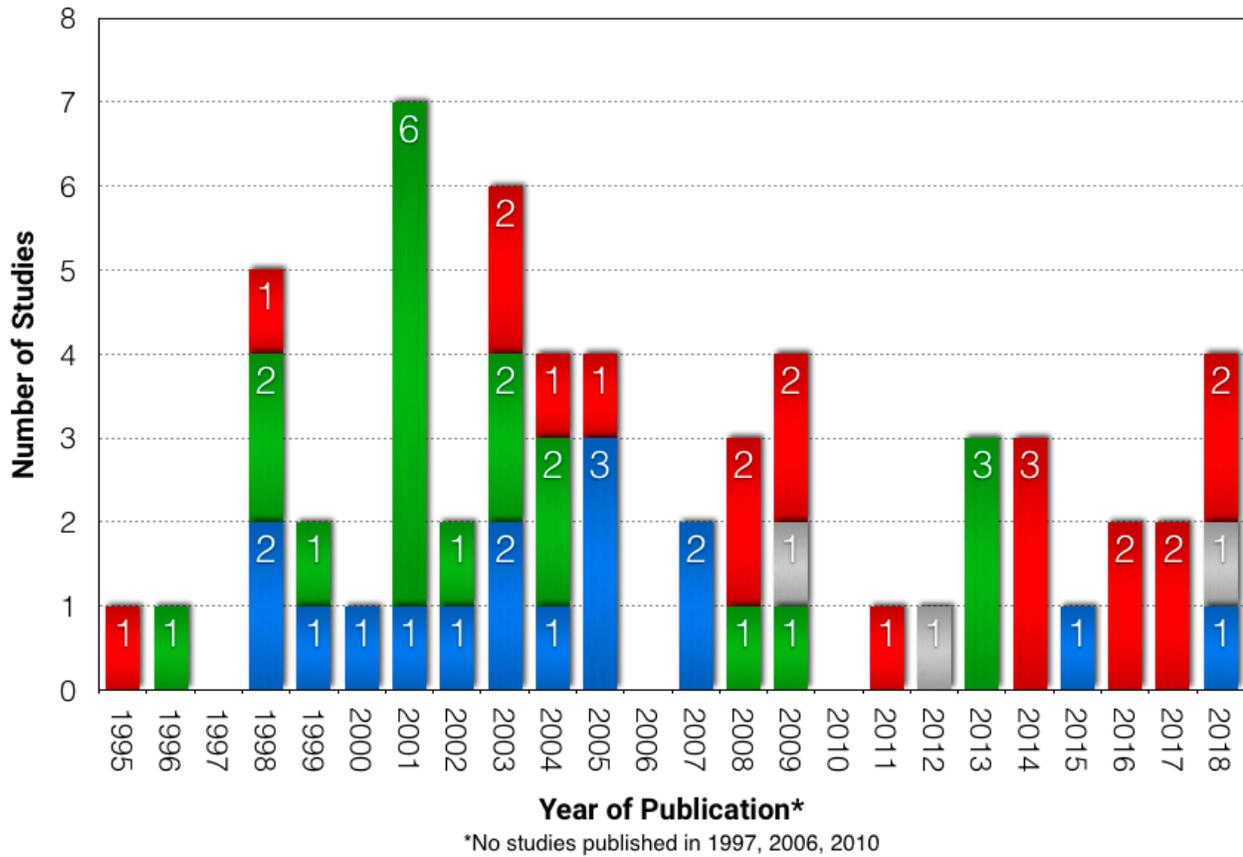
20 studies showed RTC increases crime
20 studies showed RTC decreases crime
16 studies showed RTC has no effect on crime
3 studies showed RTC has mixed results

The “Other Academic Literature” is divided in a very similar fashion. Eight articles generally find that concealed carry laws reduce crime, seven articles find an increase in crime, and the remaining five do not take a position on whether the laws increase or decrease crime rates.



However, this simplistic review obfuscates how the literature has evolved over time and the relative quality of studies on each side of the debate. Let's first examine the timeline of the studies.

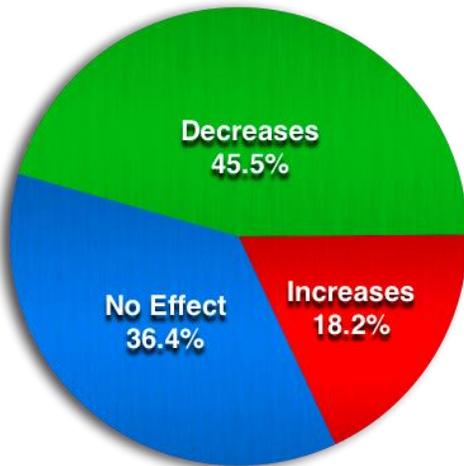
Results of Studies by Year of Publication, Color Coded for Results



20 studies showed RTC increases crime
20 studies showed RTC decreases crime
16 studies showed RTC has no effect on crime
3 studies showed RTC has mixed results

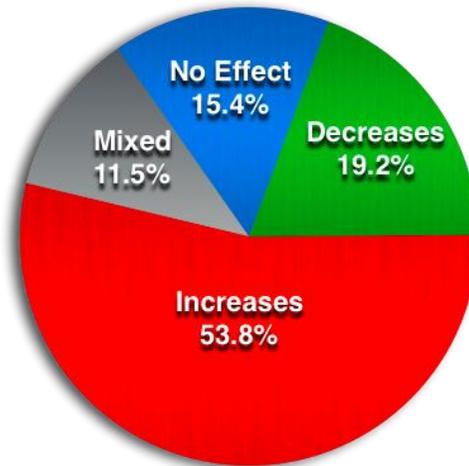
An examination of the timelines is sufficient to reveal that the literature has dramatically evolved over time. Indeed, 2003 was the average year of publication for studies indicating that concealed carry reduced crime; it is the same average year for studies indicating concealed carry had no effect. However, the average year for studies finding a crime increase is 2009. This means that the literature finding a crime increase from concealed carry is significantly more modern, as a whole, than the literature showing a crime decrease. Examining the literature before and after 2005 further highlights this divergence.

Studies by Result 1995-2005



6 studies showed RTC increases crime
 15 studies showed RTC decreases crime
 12 studies showed RTC has no effect on crime
 No study showed RTC has mixed results

Studies by Result 2007-2018



14 studies showed RTC increases crime
 5 studies showed RTC decreases crime
 4 studies showed RTC has no effect on crime
 3 studies showed RTC has mixed results

The publication of the National Research Council (NRC) report in 2005 marked a key turning point in the literature on concealed carry. After reviewing the existing literature (through 2003) and conducting their own statistical analysis, the panel concluded:

“The literature on right-to-carry laws summarized in this chapter has obtained conflicting estimates of their effects on crime. Estimation results have proven to be very sensitive to the precise specification used and time period examined. The initial model specification, when extended to new data, does not show evidence that passage of right-to-carry laws reduces crime. The estimated effects are highly sensitive to seemingly minor changes in the model specification and control variables. No link between right-to-carry laws and changes in crime is apparent in the raw data, even in the initial sample; it is only once numerous covariates are included that the negative results in the early data emerge. While the trend models show a reduction in the crime growth rate following the adoption of right-to-carry laws, these trend reductions occur long after law adoption, casting serious doubt on the proposition that the trend models estimated in the literature reflect effects of the law change. Finally, some of the point estimates are imprecise. Thus, the committee concludes that with the current evidence it is not possible to determine that there is a causal link between the passage of right-to-carry laws and crime rates.”

The NRC chapter on concealed carry laws stands out from the rest of the report in a number of aspects. First, the findings on concealed carry had one dissenter out of a panel of sixteen academics. The lone dissenter argued that RTC laws might reduce murder. The remaining fifteen empaneled academics, in their own summary, strongly disputed the conclusion of the lone dissenter. Second, the panel used their own regression analysis to analyze the effect of RTC laws, which was in stark contrast to the remainder of the report which operated as a broad literature review, tepidly weighing in on studies concerning gun violence and various policies. Third, perhaps in part because the fifteen academics employed regression analysis of their own, they were quite pessimistic about future research finding conclusive results if the same methodology was employed as in the existing literature.

It is important to recognize that statistical best practices have significantly improved since the publication of the NRC Report in 2005. For example, earlier studies often: did not use clustered standard errors, were significantly biased by the crack epidemic, failed to properly consider the problems with multi-collinearity when choosing control variables, did not properly select appropriate criminal justice variables, and did not properly account for major structural flaws in underlying county-level data. Given these facts, and the skepticism of the NRC in the existing literature’s ability to identify the causal impact of concealed carry laws, the remainder of this review will focus on studies published after 2005.

Focusing on the literature after 2005 leaves 26 studies. While a simple count analysis is useful, further filtering studies based on scope and quality should yield better insights. Studies that don’t have a national scope (only focusing on a handful of states or cities) and don’t provide estimates for concealed carry’s impact on violent crime or overall homicide are excluded. This removes the following 8 studies:

Title	Journal	Year
Rebuilding at Gunpoint: A City-Level Re-Estimation of the Brady Law and RTC Laws in the Wake of Hurricane Katrina	Criminal Justice Policy Review	2007
Intimate Partner Homicide: Relationships to Alcohol and Firearms	Journal of Contemporary Criminal Justice	2009
“Gun Control” vs. “Self-Protection”: A Case against the Ideological Divide	Justice Policy Journal	2013
Repeal of the concealed weapons law and its impact on gun-related injuries and deaths	The Journal of Trauma and Acute Care Surgery	2014
Concealed Handgun Licensing and Crime in Four States	Journal of Criminology	2015
Firearm Violence and Effects on Concealed Gun Carrying: Large Debate and Small Effects	Journal of Interpersonal Violence	2016
Association between Firearm Laws and Homicide in Urban Counties	Journal of Urban Health	2018
How Do Right-to-Carry Laws Affect Crime Rates? Coping with Ambiguity Using Bounded-Variation Assumptions	The Review of Economics and Statistics	2018

This now leaves 18 qualifying studies. Ignoring the important question about the strength of differing methodological choices and academic paradigms, one study included in the 18 published after 2005, *The Impact of Right-to-Carry Laws on Crime: An Exercise in Replication*, suffers from a grievous error that, when corrected, changes the results of the paper. The authors claim that "...the most robust result, confirmed using both county and state data, is that RTC laws significantly reduce murder. There is no robust, consistent evidence that RTC laws have any significant effect on other violent crimes, including assault." However, the study's authors misread and incorrectly label their own results. On page 7 of the study, they present the following table (red box added):

Table 3. Alternative models for murder and assault, county data

	Murder				Assault			
	General Model		Specific Model		General Model		Specific Model	
	Coeff	Stat	Coeff	Stat	Coeff	Stat	Coeff	Stat
RTC dummy	-0.0213	1.40	-0.0218	1.46	0.0164	0.47	0.0224	0.87
post-law trend	-0.0220	7.36	-0.0216	8.37	0.0153	2.80	0.0128	2.25
yt-1	0.0691	3.14	0.0692	3.14	0.5990	7.16	0.5991	7.17
Ratrob	0.0009	2.81	0.0009	2.82				
Rataga	0.0011	5.53	0.0011	5.57				
prison_1	-0.0004	-1.94	-0.0004	-2.05	0.0000	0.20		
lpolicerate2	-0.0003	-0.43			0.0008	0.77		
Rpcpi	-0.0056	-2.66	-0.0056	-2.65	-0.0022	-1.14	-0.0023	-1.19
Rpcui	0.0001	0.85			0.0000	-0.03		
Rpcim	0.0000	-0.30			0.0000	0.59		
Rpcrpo	-0.0013	-0.25			0.0001	0.02		
Unemp	-0.0147	-1.53	-0.0149	-1.63	0.0006	0.08		
Povrate	-0.0052	-1.10	-0.0053	-1.16	0.0091	1.17	0.0096	1.15
Density	0.0183	1.48	0.0183	1.48	-0.0165	-4.26	-0.0164	-4.39
Test		P-value		P-value		P-value		P-value
demographics		0.0000		0.0000		0.0000		0.0000
state trends		0.0000		0.0000		0.0000		0.0000
equal(dummies)		0.0000		0.0000		0.0000		0.0000
equal(trends)		0.0000		0.0000		0.0000		0.0000
ADZ spec		0.0000		0.0000		0.0000		0.0000
PET				0.8415				0.9370

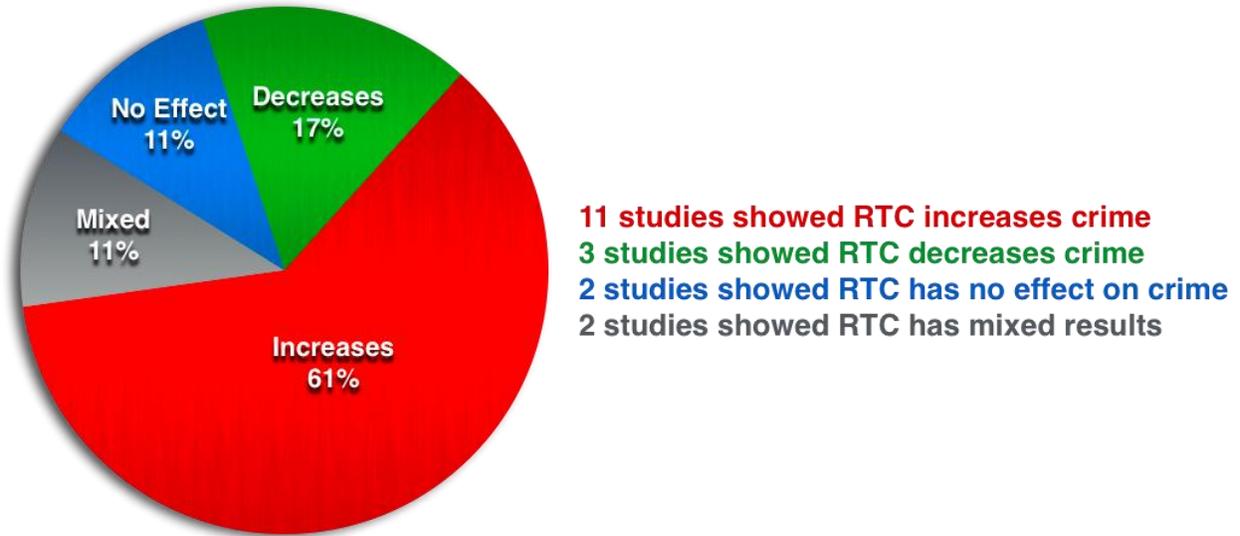
Notes: (1) See notes to Table 2 above; (2) Ratrob and rataga are robbery and assault rates.

³ As t goes to infinity the crime rate goes to positive infinity if the coefficient on the trend is positive or zero if the coefficient is negative.

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As the table reveals, there is a statistically significant increase in aggravated assault for county level data. Yet it is not bolded by the authors like all the other statistically significant findings. The "stat" for the "post-law trend" for "Assault" (highlighted with a red box) has t-statistics of 2.8 and 2.25 for the general and specific model respectively. Further, the result itself is a positive number, indicating an increase in assault. That result leads to the conclusion that the conflicting results between state and county levels are in error and that both models are actually in agreement. This significant error changes the study's reported findings from concealed carry laws reducing crime to instead having a mixed effect.

The remaining eighteen studies published after 2005 that are national in scope and provide an estimate for the impact of concealed carry on violent crime and/or homicide contain 11 studies showing that concealed carry increases crime, three find a decrease, two find no effect, and two find mixed results.



The results are clear. Examining the most recent literature reveals a majority of studies find that concealed carry increases crime, even after controlling for scope and quality. In contrast, the literature finding concealed carry laws reduce crime is antiquated. While more research should be conducted to examine the link between RTC laws and crime, the existing literature paints a troubling picture with this nationwide experiment in the widespread carrying of firearms.

Causality

“Correlation is not causation.”

The concept of correlation and causation raises an important point: sophisticated studies can find statistically significant correlations that are the product of random chance alone, not any underlying causal mechanism. One [classic study](#) in the 1990s found, for example, that the best predictor of S&P 500 stock index returns, with 99% accuracy, was a model with three simple variables: Butter production in Bangladesh, US cheese production, and the sheep population in both Bangladesh and the US. Of course, none of these variables have any plausible causal connection with S&P 500 returns – which was the point of the study. No matter how sophisticated the data analysis and regression techniques are, statistically significant results without a plausible causal pathway should not be considered valid.

In the case of concealed carry, both sides of the debate can point to a number of potential causal pathways through which Right to Carry (RTC) laws can influence crime rates. Debating these theoretical pathways provides yet another avenue to scrutinize the RTC debate.

How RTC Laws could reduce crime: Deterrence Theory

Deterrence Theory provides the main causal mechanism through which RTC laws could reduce crime. The theory holds that, if the consequences of committing a crime (being shot, for example) become too severe, would-be criminals will refrain from committing the kinds of crime which expose them to a potentially armed civilian. While a severe consequence won't stop all people who want to commit a crime (no potential cost may be high enough for some driven criminals), it will stop many.

Deterrence is a concept that has been around long before the concealed carry debate and is widely used in geopolitics as well as the legal system. Mutually Assured Destruction (MAD) during the Cold War, for example, was based on the idea that the prospect of a devastating nuclear retaliation would preclude the possibility of a first strike – assuming, of course, that nation-state actors were rational and committed to their own survival. In the legal system, punishments ranging from prison time to hefty fines are designed to curtail deleterious behavior by substantially increasing the costs of committing a crime. Given deterrence theory's widespread applications, it is intuitively appealing for gun advocates to utilize the same framework for concealed carry.

To this end, the two possible avenues through which deterrence may be realized in the context of the concealed carry debate are direct deterrence or indirect deterrence.

Direct Deterrence: Defensive Gun Use

Direct Deterrence theory postulates that RTC laws reduce crime by increasing the number of defensive gun uses (DGUs) outside the home. A DGU occurs when a citizen either fires, brandishes, or reveals a firearm in an attempt to stop an assailant from committing or completing a crime. This action can be in defense of oneself, others, or even property. Unfortunately, rigorous empirical work on the relationship between DGUs and crime is fraught with definitional

challenges, as it is unclear what kind of behaviors are sufficient to constitute a genuine defensive gun use. For example, if a potential victim merely announces that they have a gun (without physically brandishing it), and this results in the perceived deterrence of crime, does this count as a DGU? If a legitimate gun holder misperceives a situation as dangerous – such as a few teenagers crowding around his car – and brandishes his weapon, does this count as deterrence?

Regardless of how generally or specifically one defines a DGU, it is highly unlikely that these borderline cases are sufficient to have a material impact on actual crime rates. As far as direct deterrence is concerned, the most conservative definitions of DGU are more meaningful, as they represent cases that, had they not occurred, would have actually contributed to crime rates.

The three major estimates of the yearly frequency of defensive gun use (DGU) are:

- Approximately 2.5 million (according to Gary Kleck and other private surveys)
- Fewer than 100,000 (according to the National Crime Victimization Survey)
- Approximately 2,000 (according to the Gun Violence Archive, based on police and media reports)

While the FBI reported [331 justifiable homicides](#) in 2016, this number is necessarily a subset of overall DGUs (which include cases where the attacker was not killed), and is therefore of limited use in determining the overall prevalence of DGUs. Further, given that FBI data is incomplete for many states due to voluntary reporting requirements, this number is likely an underestimate of the true justifiable homicide total. The Gun Violence Archive records 611 total deaths from DGUs in 2016 (of which 460 were the defender killing the attacker), which is a more accurate assessment. All of these numbers involve civilian DGUs, not law enforcement shootings.

Gary Kleck and 2.5 million

The most frequently cited figure by those advancing the direct deterrence hypothesis is 2.5 million DGUs annually. That number comes from Dr. Gary Kleck and Mark Gertz's "[Armed Resistance to Crime: The Prevalence and Nature of Self-Defense with a Gun](#)" published in 1995. The survey itself was administered in 1993 to 4,977 individuals who were directly asked whether they had used a firearm in self-defense over the past year. The survey also asked whether such a use had occurred over the past 5 years as well, but the main focus of the study and subsequent articles has been on the yearly total. Of these roughly 5,000 individuals, 66 indicated they had indeed used a firearm in self-defense in the past year (1.33% of the sample), which extrapolated to 2.5 million for the entire country. Similar one-shot telephone surveys produce DGU estimates ranging from [760,000 to 3.6 million](#).

Such telephone surveys have a number of advantages. Assuming the participants accurately describe their experiences, the primary benefits are allowing people to report legitimate incidents that for whatever reason were not reported to the police or media (unlike the National Crime Victimization Survey which asks a broad, open-ended question about what action the participants took in response to an attempted or completed crime, these one-shot surveys directly ask respondents about defensive gun use). In addition, their anonymity (and no connection to the

government) allows respondents to relate incidents they would otherwise be hesitant to report out of fear of repercussions.

The primary disadvantages of these surveys are a lack of verification, relatively small size, and a high risk of false positives. Due to the anonymity these surveys provide participants, it is impossible to find police, media reports, or any witnesses to verify respondents' claims. Kleck makes much of the fact that if participants respond in the affirmative to whether they had a DGU, they then go on to answer 19 more questions (whereas those who answer "no" don't have follow-ups). However, researchers did not make the requisite follow-ups that are necessary to filter out false stories. Furthermore, these questions are not independent of each other, so it is not difficult for a respondent to identify the intent of a researcher's questions and thereby manufacture and embellish stories to satisfy research demands. Compared with the NCVS that surveys 135,000 households twice a year, the one-shot surveys have sample sizes ranging from fewer than 1,000 to 5,000 participants. Finally, in surveys of statistically rare events (even the most aggressive DGU survey estimate still represents less than 2% of the US adult population in a year), false positives are likely to dominate false negatives.

A false positive occurs when a respondent claims that he or she had a defensive gun use, but in fact did not during the time period allotted by the survey. A false negative occurs when a respondent claims that he or she did not have a defensive gun use during the allotted time period, but did have one in reality.

False positives could arise as a result of social desirability bias, strategic answering, lying, or telescoping. Social desirability bias occurs when respondents want to portray themselves positively to the interviewer, or justify their purchase of their firearms. Given there are 5 million members of the National Rifle Association, and several million more who would consider themselves pro-gun, the potential exists that some of these respondents would exaggerate or fabricate incidents in a survey to move the DGU tally upwards. Finally, telescoping occurs when a respondent reports a DGU that was actually outside the time-frame of the survey.

False negatives could arise as a result of social undesirability, not wanting to report criminality, and forgetting. Some respondents could feel that the interviewer would look negatively on a DGU incident, and therefore decide to not report it. Some gun owners could be fearful that the interviewer or the government would find out that they own firearms, and seek to hide the fact that they had used a firearm at all. Some respondents may fear that their DGU was in fact an illegal action (or that they were carrying the firearm illegally at the time) and therefore not want to disclose the incident. Further, the longer the time horizon, the more likely it will be that someone could forget a DGU, particularly if the respondent wasn't directly assaulted.

Kleck [contends](#) respondents have stronger incentives to give false negatives than false positives, and that his and similar surveys are therefore likely to underestimate DGUs. He estimates that anywhere from 36-64% of the respondents were either illegally using or carrying the firearm during their DGU, and that surveys of illegal activity consistently provide underestimates of said activity. For example, surveys on illicit drug use on juvenile arrestees and individuals using

walk-in clinics find that they markedly underreport their own drug usage; false negatives that are subsequently revealed with medical tests.

The connection, however, between admitting cocaine use and disclosing a defensive gun use strains credulity. For starters, cocaine use among arrestees is not a rare event, which is the most important aspect when considering whether a survey might have a false positive problem. While using cocaine is obviously illegal in any context, Kleck's survey on DGUs states: "We made no effort to assess either the lawfulness or morality of the respondents' defensive actions." Kleck consistently downplays any suggestion that respondents might think their DGU was heroic, yet 46.1% of the respondents indicate that their action at least "might have" saved someone's life, with 15.7% saying it "almost certainly would have" saved someone's life. It is not hard to imagine that at least some survey respondents would think saving someone's life would appear heroic and socially desirable, thereby leading to potential false positives.

Even without contesting Kleck's core points about incentives, his dismissal of false positives as an overriding concern commits the base rate fallacy. In reply to a critique by Dr. David Hemenway of Harvard, Kleck wrote the following:

"In our view, a more realistic version of Hemenway's hypothetical scenarios, one more in tune with research on errors in surveys of illegal behavior, might have 48 true positives, 48 false negatives (and thus 96 persons with a genuine DGU), 18 false positives, and 4,886 true negatives in a sample of 5,000 cases, implying 50% test sensitivity (the percent of true positives accurately detected) and 99.6% test specificity. Under this alternative set of hypothetical assumptions, the true DGU prevalence would be 1.92%, while the measured rate would be 1.32%, as was obtained in the NSDS, implying that the true DGU rate was actually 45% higher than the one estimated."

Kleck's postulation that the false negative rate might be **135 times** larger than the false positive rate is completely unrealistic. Furthermore, this example unintentionally highlights just how unrealistic assumptions of false positive and negative rates must be for Kleck's survey not to suffer from a false positive problem. Even assuming a true DGU incidence rate of 2% (or approximately 5 million DGUs at the time of the survey, which is higher than even the most aggressive estimate), the false negative rate would have to be at least **50 times** larger than the false positive rate for the survey not to overestimate DGUs. Assuming a true DGU incidence the exact same as Kleck's results (2.5 million annually), the false negative rate would need to be nearly **100 times** larger than the false positive rate for the survey not to overestimate DGUs. For the NCVS estimate of roughly 50,000 DGUs, the false negative rate would have to be nearly **5,000 times** larger than the false positive rate to not overestimate.

There is no defensible justification available to explain a differential this extreme between the false positive and negative rates. Indeed, the difference between the Gun Violence Archive's finding of 2,000 verified DGUs annually and Kleck's 2.5 million DGUs can be explained by a false positive rate of just slightly more than 1%. With 2,000 verified DGUs, an accurate survey of 5,000 individuals would find 0 true positives on average. This would mean that 66 DGU cases out of that 5,000 that Kleck's survey found would all be false positives, which would mean a

false positive rate of 1.32% (66/5000). Meanwhile, police and media reports would have to miss more than 99.9% of all DGUs for Kleck's survey to be accurate.

External validation checks of the survey reveal the false positive problem is more than hypothetical:

According to Kleck's estimates, guns were used defensively in 845,000 burglaries. We know, however, from reliable victimization surveys that there were fewer than 1.3 million burglaries in the United States at the time of the survey; 33% of the burglaries had occupants in the home who were awake at the time of the burglary; and of those households broken into, only 42% owned firearms. Even if burglars only targeted households that owned firearms, and those gun owners used guns in self-defense every time they were awake, the number would still be impossible to obtain. At most, only 182,000 defensive uses in response to burglaries would be even possible.

Kleck's analysis also implies hundreds of thousands of criminals are getting shot every year, yet medical records do not support this claim. While Kleck has claimed the majority of criminals simply never seek medical attention, [data indicates](#) more than 90% of criminals who have been wounded prior to incarceration seek medical attention.

Respondents indicate that police were informed of or found out about 64.2% of their DGUs. This would mean empirical evidence of 1,605,000 DGUs exists. Yet police and media reports collected by the Gun Violence Archive find fewer than 2,000 verified DGUs. Even assuming that only the 23.9% of cases where shots were fired (according to Kleck's survey) are recorded, that would still mean 597,500 cases, yet empirical evidence for only 0.3% of those actually exists.

In Kleck's survey, 73.4% of the reported DGU incidents were allegedly against strangers. Yet according to the [NCVS](#): "In 2010, strangers committed about 38% of nonfatal violent crimes, including rape/sexual assault, robbery, aggravated assault, and simple assault."

And these are just a few of the many nonsensical results that Kleck's survey and other one-shot surveys produce.

In addition, Kleck argues that crime estimates from the NCVS cannot be used as a validity check to his own results. Yet in his own [study](#), when trying to make the point for why the NCVS is not well equipped to measure DGUs, Kleck admits that: "Its survey instrument has been carefully refined and evaluated over the years to do as good a job as possible in getting people to report illegal things which *other* people have done *to* them."

Kleck further [contends](#) that the overall DGU number should not be judged by subsets of DGUs due to their size in the survey: "Our estimates of total DGUs are likely to be fairly reliable partly because they are based on a very large sample (n=4977), while any estimates one might derive pertaining to one specific crime type are necessarily less reliable because they rely partly on a far smaller subsample, i.e., the 194 reported DGU incidents, of which about 40 were linked to burglaries." However, it would be rather highly unlikely for a survey to fail every attempt at external validation ranging from empirical data to other larger criminological surveys, and yet still have its total number be accurate. Furthermore, any survey plagued by even a small risk of

false positives, becomes less (not more) accurate and more error-prone when implemented on a larger population – the base rate of false positives simply scales with the sample size.

This problem of false positives is not limited only to surveys of DGUs. Surveys ranging from measuring NRA membership, the number of people abducted by UFOs, people reporting rare diseases, and so on are all plagued by false positives due to their very nature of attempting to measure a statistically rare event. It would be highly unusual to think that one-shot surveys are the singular exception to the false positive problem, particularly when all of the available empirical data indicates that their numbers are not accurate.

NCVS and 50,000

The National Crime Victimization Survey ([NCVS](#)) is conducted semi-annually by the Bureau of Justice Statistics and surveys a nationally representative sample of 135,000 households “on the frequency, characteristics, and consequences of criminal victimization in the United States.” From 2007-2011 NCVS [extrapolated](#) 235,700 DGUs in response to violent nonfatal victimizations, which translates to slightly more than 47,000 annually. During the same period, NCVS estimated 103,000 victims of property crime used a firearm in self-defense, or approximately 20,000 annually. These figures represent approximately 1% of all violent non-fatal victimizations and 0.1% of property crime victimizations (86% of property crime victims were not there during the incident, which indicates 0.7% of victims who were present during the property crime used a firearm defensively). At the time of Kleck’s survey (1993), NCVS [estimated](#) 83,000 DGUs annually.

The primary advantages of the NCVS are its size, frequency, and sophistication. With 135,000 households being surveyed, the NCVS dwarfs one-shot surveys like Kleck’s with a sample size of merely 5,000. Unlike one-shot surveys that are only done once (hence the name), the NCVS has been conducted since 1973, usually semi-annually. This provides researchers a significant opportunity to analyze and revise methodology to enhance the survey’s accuracy.

The primary disadvantage of the NCVS survey with regard to DGUs is that the survey does not explicitly ask about DGUs, asking participants instead about attempted or completed crimes against them more generally. Unlike Kleck’s survey which directly asks participants whether they used a firearm in self-defense, the NCVS [asks](#) the open ended “What did you do?” and “Anything else?” to determine whether the participants took any defensive action. Without a direct prompt, participants might fail to specifically disclose that a firearm was used. Participants also would not have the opportunity to disclose a DGU if they used their firearm to stop a crime happening to someone else, leaving these types of incidents uncounted.

Kleck and others postulating widespread defensive gun use also argue that the ordering of NCVS questions is problematic. Respondents must first indicate they were the victim of a crime before they disclose a DGU. Hence if a participant doesn’t think they were the victim of a crime (i.e. they stopped the perpetrator before a crime was committed), they won’t have an opportunity to report their firearm usage.

This concern, however, is not as problematic as it first appears. The NCVS is careful to ask about attempted crimes and threats as well as completed crimes. If a respondent wasn't at least on the receiving end of a threat, justifying the resulting firearm use as "defensive" is difficult. Instead, it is more methodologically coherent to first ask whether the participant was the victim of an attempted or completed crime (or threat) in order to counter false positives and socially undesirable firearm uses (namely aggressive rather than defensive gun use).

Despite measures to combat false positives, the NCVS still faces the same structural problems that plague Kleck and other's one-shot surveys of Kleck and others. As stated in the discussion of Kleck's results, assuming a true DGU incidence of roughly 50,000 (in line with NCVS estimates), the false negative rate would have to be nearly **5,000 times** larger than the false positive rate to not overestimate. The order of the questions (and not asking a direct priming question on firearm use) is unlikely sufficient to combat such a differential, especially in light of strong social incentives to embellish, fabricate, or mis-remember defensive gun use details..

While NCVS defensive gun use estimate totals are within the realm of plausibility, the foundational problem of false positives in surveys of rare events, combined with the best available empirical evidence, indicate that the NCVS also produces an overestimate.

Gun Violence Archive and 2,000

The Gun Violence Archive (GVA) was founded in 2012 with the purpose of comprehensively tracking gun violence incidents. Utilizing more than 2,000 media and police sources across the country, GVA tracks, in near real-time, all forms of gun violence except suicides. Suicide is tallied after the CDC publishes its data and has a lag of more than a year. GVA classifies defensive gun uses as: "The reported use of force with a firearm to protect and/or defend one's self or family. Only verified incidents are reported." The totals, for each year since GVA's founding:

2014: 1,561

2015: 1,381

2016: 1,993

2017: 2,111

2018: 1,798

These verified defensive gun use totals are necessarily an undercount of the actual prevalence of overall DGUs. As GVA itself notes under its methodology: "There are sometimes questions about Defensive Gun Uses which are not reported to police. GVA can ONLY list incidents which can be verified. Our policies do not take into account stories not reported, 'I can't believe this happened to me' scenarios or extrapolations from surveys. Our position is that if an incident is significant enough that a responsible gun owner fears for their life and determines a need to threaten lethal force it is significant enough to report to police so law enforcement can stop that perpetrator from harming someone else."

Proponents of the widespread DGU theory could contend that police and media reports won't capture the overwhelming majority of DGUs. Therefore the total verified DGUs count is just as misleading and limited as the FBI's count of justifiable homicides when trying to determine the number of overall DGUs. While this explanation directly contradicts the results contained within Kleck's survey and the NCVS – both of which find that more than 60% of respondents claim that police found out about their DGU – but if those surveys are decisively flawed, then the ratio of reported to non-reported DGUs is unknowable.

However, unless police reports miss more than 99.9% (which is necessary for Kleck's estimate to be accurate) or 96% (for the NCVS estimate to be accurate) of DGUs, the widespread DGU theory lacks any empirical support. Under reasonable assumptions about the ratio of unreported to reported DGUs, GVA data will provide a better benchmark than surveys.

It is important to note that GVA's tally is not a total of socially desirable gun uses. Many of the cases involve shootouts where neither side can reasonably claim the mantle of "good guy," or cases where one drug dealer may break into another drug dealer's home and is fought off with a gun.

However, it is dubious to suggest there are significant numbers of socially desirable DGUs that aren't captured by this tally. Failure to report incidents to the police suggest one of two scenarios: either the DGU was probably illegal or the DGU was justified but the defendant was irresponsible by not reporting a dangerous criminal to police. Therefore, arguing that a massive number of DGUs aren't reported is synonymous with an admission that defensive gun use, in the aggregate, is likely not beneficial for society. Indeed, Kleck's own research finds that between 36-64% of the DGUs were likely illegal. This concession is mirrored in [research](#) by Harvard researchers, who found in a series of surveys that a majority of reported DGUs are likely illegal (a finding we will delve more deeply into in the section on Direct Escalation).

GVA's data represents the first comprehensive attempt to track DGUs nationally. An earlier [study](#) in 2004 attempted to track DGU incidents in the Phoenix metropolitan area over the course of three and a half months using newspaper reports, supplemented by police and court records. The results: "Two DGUs involving killing assailants and one involving firing at an assailant were found. The three DGUs stemmed from cases of 'mutual combat' or exposed bystanders to gunfire." As the authors concluded: "These findings cast doubt on rates of DGUs reported in an influential study by Kleck and Gertz, which predict that the police should have known about 98 DGU killings or woundings and 236 DGU firings at adversaries during the time the newspaper was surveyed. The findings reported here were closer to predictions based on the National Crime Victimization Survey, which suggest that the police should have known about eight DGU killings or woundings and 19 DGU firings at adversaries."

GVA's verified DGU numbers are a significant improvement over this early attempt both in scope and sophistication, but confirm the same hypothesis: both one-shot surveys and NCVS overestimate DGU prevalence.

Direct Deterrence Conclusion

When survey data and empirical data fundamentally contradict each other, the best course is to rely on the hard data, particularly when there is significant evidence that the surveys in question suffer from foundational flaws. While GVA's verified DGU count of approximately 2,000 annually is an undercount of overall DGUs, the best available evidence and logic dictates that it is closer to the true number than one-shot surveys and the NCVS. Further, the higher the ratio of unreported to reported DGUs, the more likely it is that the sum of these firearm incidents are actually detrimental to society.

This analysis significantly undermines the argument that concealed carry laws reduce violent crime through direct deterrence. Only one-shot surveys' estimates of millions of DGUs provide a sufficient number of defensive incidents to support crime reduction numbers of the magnitude John Lott postulates, and these surveys are fatally flawed.

Indeed, even Lott's own research points to the implausibility of direct deterrence being a sufficient causal force for crime decreases. At the time of Lott's data in *More Guns, Less Crime*, the general population faced an aggravated assault rate of 0.18%. Assuming as Lott does that 2% of the population obtained concealed carry permits (and were consistently carrying their firearms), 0.65% of those permit holders would need to stop aggravated assaults annually to reduce crime by the amount projected in his own models. This means permit holders would have to stop 3.6 times the number of aggravated assaults they would be expected to encounter. In other words, there isn't sufficient opportunity for permit holders to use their firearms defensively to reduce crime by the degree Lott's models require.

Unless superior empirical data is collected that bolsters the NCVS or Kleck's DGU estimates, it is safe to conclude that Direct Deterrence is not a causal pathway that concealed carry laws could be using to reduce violent crime.

Indirect Deterrence

Indirect Deterrence theory postulates that RTC laws reduce crime by increasing the perceived cost of committing a crime. Under this framework, criminals weigh the costs and benefits of committing a crime. If the costs (in this case the risk of being shot) are too high, criminals will either not commit the crime or will attempt to do so elsewhere. By its very nature, it is extremely difficult (if not impossible) to tell who is carrying a concealed firearm. Therefore, rather than risk encountering an armed individual, a criminal will decide against the action and is deterred from crime even though there was never a defensive gun use. Indirect Deterrence theory contends that the mere fact that some people in a given population likely have firearms on their person is enough to decrease crime, even if they those firearms are never used. In other words, "an armed society is a polite society."

Indirect deterrence can work in two ways: either criminals know the relative likelihood of encountering a "good guy with a gun" during their crime and are deterred, or they hear about the concealed carry law being passed and decide crime is no longer worth the risk (announcement effect).

While the announcement effect is possible, it is highly unlikely as it would require criminals to be savvy about statewide legislation that was not heavily advertised, and then have their unsubstantiated fears over that legislation significantly influence their behavior. Indeed, John Lott himself endorses the causal framework of permit holders being the primary source of crime reduction (through direct and indirect deterrence), not the announcement of the law itself. Therefore, we will focus on the relative likelihood of indirect deterrence vis-à-vis the permit holders themselves as it is much more in line with the academic literature on both sides of the debate.

Support for indirect deterrence comes from an extensively cited 1985 [survey](#) of prison inmates by James Wright and Peter Rossi. Of a sample of slightly more than 1,600 inmates, 56% agreed with the statement that: "a criminal is not going to mess around with a victim he knows is armed with a gun." On whether "A smart criminal always tries to find out if his potential victim is armed," 81% agreed. And 57% of the respondents agreed that "Most criminals are more worried about meeting an armed victim than they are about running into the police."

Both [John Lott](#) and [Gary Kleck](#) cite these findings as significant evidence in favor of private citizens indirectly deterring criminals. However, the evidence is not as clear cut as it initially appears. The authors of the survey penned this lengthy footnote regarding the above findings:

“All the data reported in this section are ambiguous in the sense that we do not know directly just who the armed victims were that our sample reported encountering. One very potent and oft exploited image of the "armed victim" is that of the hard-bitten, generally law abiding home owner valiantly defending self and family from the incursions of the predatory criminal class. This, for example, is the image one obtains from "The Armed Citizen" column in the NRA's American Rifleman, where accounts of these kinds of incidents are collected and printed. Such incidents doubtlessly occur, perhaps with considerable frequency; national surveys suggest that some 2-6% of the adult US population have at some time actually fired a gun in their own self-defense (Wright et al, 1983: Ch. 7).

On the other hand, one must also keep in mind in reviewing our materials on armed victim encounters that felons prey, for the most part, on others much like themselves, and that in many of these encounters, the question, Who is victim and who is perpetrator?, is often a judgment call. That the predatory felons in this sample hung around with other men who owned and carried guns has already been reported; that felons prey on the people they associate with and on others in the immediate environment is confirmed in details in the criminal victimization surveys (see, e.g., Hindelang et al., 1978). Given these points, one would have to expect that the rate at which these men encountered armed victims would be rather high, which, indeed, it is (see text). To emphasize, some of these encounters would involve encounters between perpetrators and total innocents; others – perhaps the larger share – would involve their own friends and associates.”

Concealed carry laws are ostensibly designed to increase the number of individuals **legally** carrying firearms in public. Those legal permit holders, in turn, would be the ones deterring

criminals under the indirect deterrence framework. However, if a large portion of the felons surveyed by Wright and Rossi are referring to their interactions with other criminals and their concerns about them being armed, then it is not feasible for concealed carry laws to have an influence on interactions between individuals who are armed regardless of its legality. It is telling that despite the concerns outlined by the felons in their interviews, they still committed felonies in a heavily armed population, which indicates that their concerns didn't translate to altered behavior. Finally, the sample includes burglars, and concealed carry laws are only going to increase deterrence outside the home (if at all).

In addition to significant questions over whether a survey conducted more than three decades ago is still applicable today, the authors' footnote makes it clear that their survey does not provide strong evidence for the indirect deterrence framework pertinent to concealed carry laws.

A more direct test of whether or not Lott's proposed mechanism for indirect deterrence is plausible would be to simply ask people questions about the rates of gun ownership and concealed carry in their area. For indirect deterrence to occur, individuals looking to commit a crime would need to roughly know the likelihood of encountering an armed individual, and then be deterred by that risk. However, if the individual is unaware of the distribution of firearm carriers in the general public, then it is highly unlikely that they would be deterred by a factor they are unaware of.

A [2015 survey](#) by David Fortunato tested this exact question, asking 1,000 participants across the US to estimate the percentage of people in their home state carrying firearms. Fortunato then cross-referenced those responses with the concealed carry policy in their respective states (may-issue, shall-issue, permitless) and the number of active permits in the state. The paper also controlled for a number of factors that could influence the perception of how many people were carrying firearms, including measures of legal and illegal firearms markets, state ideology, and levels of firearm violence.

The paper found no statistically significant relationship between firearm policies and people's perception of the number of firearm carriers in the state. The paper also found no relationship between the actual proportion of firearm carriers and perceptions of how many people were carrying. In other words, people were profoundly ignorant about the likelihood of encountering somebody who was carrying a concealed firearm in their state – which is the very statistic that would need to be well-known in order for indirect deterrence to make sense. As Fortunato states: “Because beliefs over the distribution of firearm carriers are impervious to permitting policies and do not respond positively to the true distribution of carriers,” increasing the number of concealed carry permits in a state “cannot deter crime.” Fortunato concludes that by passing concealed carry laws and increasing the number of permit holders in a state: “at best, we increase the probability of accidental discharge. At worst, these policies open the door for more violent, potentially deadly, escalations of altercations — altercations that may have ended peacefully if not for the presence of a firearm.”

While further research should be conducted to directly test the indirect deterrence framework – which is a central component of whether concealed carry laws can be reducing crime –

Fortunato's paper is a key first step in determining the framework's plausibility. His results cast significant doubt on whether indirect deterrence is a valid causal pathway for concealed carry laws to reduce violent crime, and is bolstered by other suggestive evidence.

Another important aspect of whether concealed carry laws can reduce crime is the location of permit holders, which has significant implications for both direct and indirect deterrence. While people are unaware of the density of permit holders in their state, the number of firearm carriers is not evenly distributed throughout the state. Therefore, it could be the case that there are significant numbers of permit holders in areas criminals are more likely to operate in, and criminals are aware of and deterred by that increased local presence of firearm carriers.

However, county and zip-code level permit data paints the opposite picture. In 2015, the *Chicago Sun-Times* released a [report](#) analyzing concealed-carry permit data at a zip-code level in Chicago and Illinois. Initially, the *Sun-Times* reported that the zip codes with the highest number of permits were those with the highest crime and police presence. Yet after adjusting for the population, the results were the opposite. Those zip codes with the highest permit rates per-capita had the lowest amount of crime and police presence. In other words, the individuals getting permits tended to live in areas where they were the least likely to suffer from a crime where they might use their firearm.

This Illinois data is confirmed by previous studies. An examination of Dallas permit-holders by [Hood and Neely](#) revealed that: “[p]ermit holders were overwhelmingly white males and resided in areas with little violent crime. Those areas with high violent-crime rates were the least likely to contain a high number of residents with concealed-handgun permits.” Another [analysis](#) on North Carolina permit holder data found that such individuals were concentrated in: “rural and suburban areas where crime rates are already relatively low, among people who are at relatively low risk of victimization — white, middle-aged, middle-class males.”

It is important to note that this is not evidence of concealed carry reducing crime. These areas already had significantly lower violent crime rates **before** people living there started obtaining permits to carry concealed firearms. Therefore, obtaining permits could not have had a causal impact on the already existing crime rate. Further, there is extensive evidence that any increase in gun ownership resulting from concealed carry in these areas would likely lead to more [homicides](#), [suicides](#), [fatal domestic violence](#), [accidental shootings](#), and [firearm theft](#).

Instead, the location of permit holders casts further doubt on both the direct and indirect causal mechanisms through which concealed carry laws could potentially reduce crime. If permit holders are typically not in the same geographical vicinity as those who would commit crimes, it becomes less feasible for there to be enough defensive gun uses to reduce violent crime rates by the amount suggested by Lott. Furthermore, even if criminals are largely aware of the geographical distribution of permit holders, it is less likely to influence their decision making as to whether or not to commit a crime. In other words, a high concentration of permit holders 30 miles away from a city with high rates of violent crime is not going to have any impact on said crime rates.

The final consideration in determining the validity of the indirect deterrence framework are the types of crimes that are being influenced by the adoption of concealed carry laws. Criminal recidivists are the *most likely* group to be deterred by concealed laws, given the fact that they come into contact with citizens on a more regular basis than non-repeat criminals, and are thus more likely to confront a gun-owning citizen. Ayres and Donohue [write](#), “If 2 percent of the population carries concealed weapons, then a criminal who robs 100 people a year faces an 86.7 percent chance of encountering a concealed weapon over the course of the year. A 2 percent chance of encountering an armed victim may not be sufficient to deter a one-time criminal, but it may be sufficient to deter someone from making a profession out of robbery.”

Furthermore, we would expect to see stronger decreases in robbery than for other crimes such as murder because there’s at least some non-trivial risk that RTC laws might induce ordinary citizens to become murderers (one can imagine a hostile argument becoming lethal in the presence of a gun), but no such effect occurs to balance out robberies. As Ayres and Donohue explain, “But while law abiding citizens with concealed weapons may get angry and commit murder, it is less likely that they will get angry and commit robbery, which is primarily an economically motivated crime.”

If RTC laws have any effect at all on crime, then we should see the effect most prominently in crimes that are committed by recidivists, of which robbery is the most likely candidate. Instead, Lott finds effects for non-repeat crimes like murder and rape, but not for robbery.

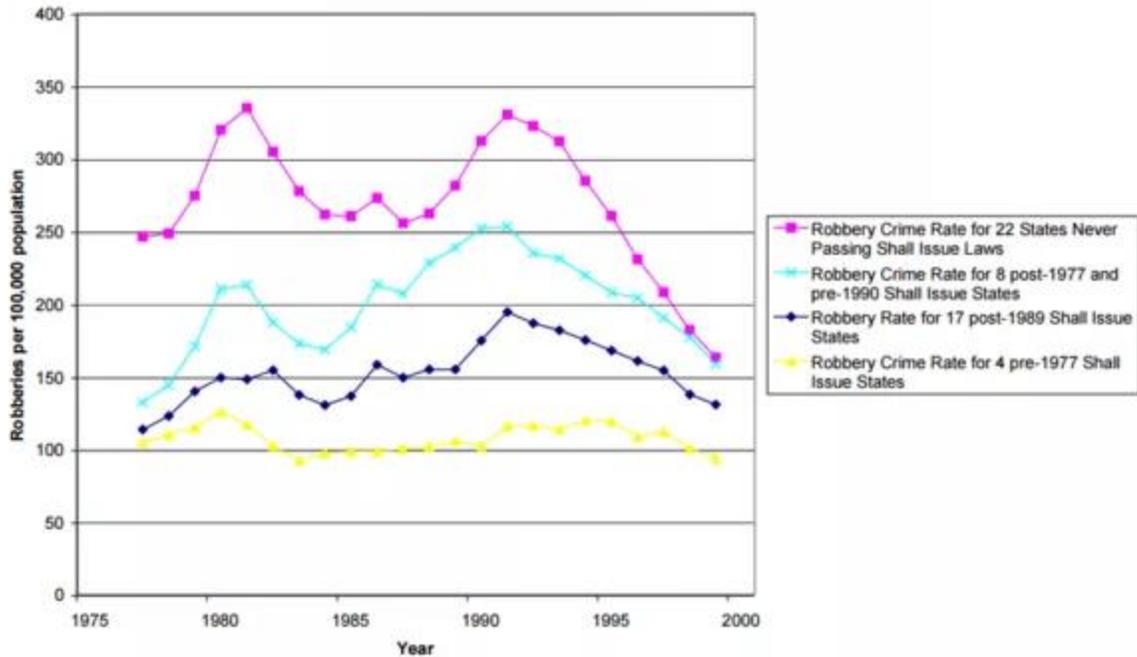
Lott has [argued](#): “it is not clear that robbery should exhibit the largest impacts primarily because “robbery” encompasses many crimes that are not street robberies. For instance, we do not expect bank or residential robberies to decrease, and, in fact, they could even rise. Allowing law-abiding citizens to carry concealed handguns makes street robberies more difficult, and thus may make other crimes like residential robbery relatively more attractive.”

Lott also [contends](#): “...as anyone who has carefully reads my work will know, it is simply not true that the results show “little or no effect on robbery rates.” Whether the effect was greater for robbery or other violent crimes depends on whether one simply compares the mean crime rates before and after the laws (in which case the effect is relatively small for robbery) or compares the slopes before and after the law (in which case the effect for robbery is the largest).”

Yet from [1973-2011](#), by far the most common type of burglary was street / highway burglaries, which accounted for between 42% - 56% of all robberies. At the same time, residential burglaries accounted for only 10-17% of robberies, while the percentage of bank robberies never exceeded 2%. Street/highway robberies account for such a large percentage of overall robberies, that Lott should have been able to find an effect of RTC laws on robberies if there was one.

Further, between 1977 and 1992 (the time period analyzed by Lott’s initial study), street/highway robberies actually increased from 46% to 56%. At that same time, residential robberies remained constant. Clearly, criminals were not being deterred by the prospect of meeting armed resistance.

Figure 1a: Robbery Rates for States by Passage of Shall Issue Law, Weighted by State Population (Vernick coding)



A [study](#) by Ayres and Donohue raises yet another problem for the indirect deterrence framework and its expected impact on robbery. The 22 states that never adopted concealed carry laws (the pink line) had substantially higher initial rates of robbery than the states that eventually adopted shall-issue laws. These differences preceded the widespread adoption of concealed carry laws, and so were caused by external factors unrelated to gun carrying in public. Ayres and Donohue point out that, from graphical inspection alone, the only group that experienced significant downturns in robbery were the 22 states that **never adopted shall-issue laws**. If the “change in slope” matters at all, as Lott contends, then it’s apparent you can get significant decreases in the robbery rate by **not** adopting shall issue laws. Furthermore, by the same logic, concealed carry laws appear to increase robbery, as the teal line changes slope after 1985 and steadily increases until the 90s, while the blue line sharply increases after 1989.

Unless new research is conducted that reverses the existing findings on awareness of permit holder distribution, the location of said permit holders in already low crime areas, and the lack of a significant effect of concealed carry laws on robbery, it is safe to conclude that indirect deterrence is also not a viable causal pathway for concealed carry laws to reduce crime.

Deterrence Theory Conclusion

When applied to the concealed carry debate, Deterrence Theory requires a significant level of direct or indirect deterrence in order to reduce crime. However, the best available evidence reveals that neither of these crime reducing causal pathways are open. For Direct Deterrence, there are an insufficient number of defensive gun uses for a significant impact on crime. For Indirect Deterrence, it is highly unlikely that criminals know the actual distribution of permit

holders in the population, the actual distribution is predominantly in areas that already had low levels of crime before RTC laws were passed, and the crimes most likely to be prevented by the passage of RTC laws are not reduced. Therefore, there is no plausible mechanism through which concealed carry laws could be reducing crime.

Next, we will examine the causal pathways through which concealed carry laws could increase crime.

How RTC laws could increase crime: Escalation Theory

Escalation Theory provides the main causal mechanism through which concealed carry laws could increase crime. While RTC deterrence theories posit that the carrying of firearms de-escalates and reduces crime, Escalation Theory holds that the presence of firearms will, on average, lead to conflicts becoming more dangerous. Escalation does not require an increase in the number of overall crimes, but simply a heightening of the severity of pre-existing crimes. With the presence of a firearm, what could have been an argument and simple assault becomes aggravated assault, and aggravated assault can quickly turn into homicide.

To this end, there are several pathways for concealed carry laws through escalation theory to increase crime:

- Permit holders escalate otherwise normal activities into potentially lethal situations.
- Criminals arm themselves in response to the higher risk of encountering “good guys with guns.”
- Criminals steal more firearms from “good guys with guns,” which leads to a criminal arms race.

The foundation of escalation theory is that access to lethal means such as a firearm significantly increases the likelihood of death or severe injury. While someone committing a criminal act might substitute different weaponry if a firearm is not available, the very act of substituting would reduce the severity of the crime, and vice versa. A [plethora](#) of research outside of the concealed carry literature highlights the increased lethality resulting from easy access to firearms. For example, a [2014 meta-analysis](#) of 16 studies found that the presence of a firearm in the home doubled the risk of homicide and tripled the risk of suicide.

Direct Escalation

The first mechanism through which concealed carry laws could increase crime is directly from permit holders themselves. By carrying firearms in places or situations they didn't before, permit holders risk escalating arguments and other interactions into violent confrontations. For example, cases of [road rage](#) where a person brandishes a firearm have been increasing over the past few years. Indeed, a [2006 survey](#) by Harvard researchers found that road rage was more common when a firearm was present in the vehicle. This is either because a firearm emboldens drivers to pursue aggressive confrontation they would otherwise not engage in, or because people with aggressive predispositions are more likely to secure a firearm. Both possibilities offer a rather stark and pessimistic account of the public health risks of armed populations. Indeed, a [2015 study](#) found that approximately 10.6% of the population has significant anger traits and access to

a firearm; 1.6% carry a firearm in public and have anger traits. While no single survey should be taken as definitive and further study definitely should be done before firm conclusions are drawn, it is certainly feasible that anger in combination with legally carrying firearms could lead to an increase in crime.

The counter to this hypothesis is the low recorded crime rates of permit holders and very few permit revocations. As John Lott states in a [2017 report](#) on concealed carry permit rates: “A new, unpublished study by John Donohue, Abhay Aneja, and Kyle Weber has received a lot of attention for supposedly finding some evidence that right-to-carry laws increase overall violent crime rates. But the bottom line is pretty clear: Since permit holders commit virtually no crimes, right-to-carry laws can’t increase violent crime rates. You can't get the increases in violent crime rates that a few of their estimates claim with only thousandths of one percent of permit holders committing violent crimes (see appendix). To get their results, state police agencies would have to be missing around 99.4% to 99.83% of violent crimes committed by permit holders.”

Lott also compares permit holders and police officers in terms of how law-abiding each group is: “Even given the low conviction rate for police, concealed carry permit holders are even more law-abiding than police. Between October 1, 1987 and June 30, 2017, Florida revoked 11,189 concealed handgun permits for misdemeanors or felonies. This is an annual revocation rate of 10.4 permits per 100,000. In Texas in 2016 (the last year for which data is available), 148 permit holders were convicted of a felony or misdemeanor – a conviction rate of 12.3 per 100,000. Combining Florida and Texas data, we find that permit holders are convicted of misdemeanors and felonies at less than a sixth of the rate for police officers.”

However, comparing crime rates among permit holders to those of the general public and even police officers is highly misleading in relation to whether concealed carry laws increase crime. All of the evidence indicates that permit holders commit fewer crimes per capita than the general population, but this is a byproduct of the fact that permit holders are required to pass a background check in the first place. A population of individuals who can pass a criminal background check is always going to be more law-abiding than a population that includes individuals who have a criminal history and therefore could not have passed that background check. People with clean criminal records are more law-abiding than people who have committed crimes. The real debate is whether permit holders are more or less law-abiding than the rest of the population who could pass a background check, but don’t obtain a permit. And the comparison with police officers is a tangent, as law enforcement frequently encounters situations where the opportunity for criminal conduct by officers such as the misuse of force is a potential, whereas permit holders as a group will almost never face similar situations.

Nevertheless, Lott raises an interesting point that leads to three important questions:

- 1) Does revocation data accurately reflect whether permit holders are committing serious crimes?
- 2) Are permit holders more law-abiding than other citizens who could also pass a background check?
- 3) Are there a sufficient number of cases to explain a statistically significant crime increase?

Multiple in-depth investigations have uncovered that permit revocation data is notoriously inaccurate. A [2011 report](#) by MLive analyzed revocation data in Michigan and found that in two large counties, 77% and 79% of the convictions of permit holders were unreported, meaning many permit holders who should have had their permits revoked did not. Many counties turn in incomplete reports, or none at all, making statewide revocation data all but useless in determining crime rates among permit holders. Frequently the boards overseeing permits aren't even notified when a permit holder is convicted.

A [2011 investigation](#) by The New York Times examined concealed carry permit data in North Carolina. They found:

“More than 2,400 permit holders were convicted of felonies or misdemeanors, excluding traffic-related crimes, over the five-year period, The Times found when it compared databases of recent criminal court cases and licensees. While the figure represents a small percentage of those with permits, more than 200 were convicted of felonies, including at least 10 who committed murder or manslaughter. All but two of the killers used a gun....

More than 200 permit holders were also convicted of gun- or weapon-related felonies or misdemeanors, including roughly 60 who committed weapon-related assaults.

In addition, nearly 900 permit holders were convicted of drunken driving, a potentially volatile circumstance given the link between drinking and violence.

The review also raises concerns about how well government officials police the permit process. In about half of the felony convictions, the authorities failed to revoke or suspend the holder's permit, including for cases of murder, rape and kidnapping. The apparent oversights are especially worrisome in North Carolina, one of about 20 states where anyone with a valid concealed handgun permit can buy firearms without the federally mandated criminal background check. (Under federal law, felons lose the right to own guns.)

...The Times also found scores of people who kept their permits after convictions for violent misdemeanors. They included more than half of the roughly 40 permit holders convicted in the last five years of assault by pointing gun and nearly two-thirds of the more than 70 convicted of a common domestic violence charge, assault on a female.”

It is worth mentioning that John Lott cites the revocation data from both states as evidence to bolster his claims about permit holders being extremely law abiding.

It is safe to conclude that any analysis that relies on permit revocation data without significant caveats is fundamentally flawed. And this is before considering other issues with the concealed carry permitting system, including cases of [not properly](#) conducting background checks on permits, states such as Florida [pushing](#) to allow for incomplete background checks (or none in the case of “Constitutional Carry” states), the many states relying on an [honor system](#) for permit revocation (physical confiscation of licenses is rare), and a [majority](#) of states do not have permit holder data available to the public (therefore making it impossible to tell how law-abiding permit holders actually are). Further, Gary Kleck and Brion Sever in their 2017 book [Punishment and](#)

[Crime](#) indicate that only approximately one in five violent crimes result in the offender's arrest and conviction. If this ratio is applied to permit revocations, which are already a substantial undercount of crime, that would mean revocations could be capturing less than a tenth of permit holder crime. Until additional higher-quality data becomes available, revocation data should not be used to determine criminality among permit holders without significant caveats.

The question of whether concealed carry permit holders are more law-abiding than their peer citizens who could also pass a background check is murkier. Not only is there no accurate measure of criminality among permit holders currently, but there is also no real data on the entire subset of the population who could pass a background check. More data is needed before any concrete conclusions are drawn.

That being said, there is some suggestive evidence that permit-holders may engage in more firearm-related crime than other law-abiding citizens. A [2013 study](#) analyzed conviction rates of permit holders in Texas for a variety of crimes relative to the general population from 2001 to 2009. However, rather than a direct comparison, the authors examined the ratio of crimes within each group. The underlying assumption of this research is that permit holders and law-abiding citizens should have the same crime rates, if obtaining a permit has no effect on criminality (and that firearm related crimes should not be disproportionately large for permit holders). Yet 22.3% of convictions of permit holders involved "weapons offenses" and 13.3% were for "deadly conduct." For the general population, those percentages were 9% and 2.8%. It is important to note that the general population had a higher rate of both crimes than permit holders, but that when permit holders were convicted, the odds were greater that the conviction was for a weapons related charge or deadly conduct.

Further, permit holders had a disproportionate rate of gun related crimes relative to other non-gun related crimes. While this difference could purely be a result of demographics in the state of Texas, the evidence tentatively points in the direction that concealed carry causes otherwise law-abiding citizens to engage in more firearm related crime.

It is important to again stress that the conviction data is likely an undercount (due to the reporting errors investigations have found in other states), and that no good data yet exists for a comparison group of individuals who could pass a background check but don't carry firearms.

The final question of whether there are enough permit holder crimes to generate a significant crime increase has a much more straightforward answer: almost certainly not. While revocation data is highly flawed, it takes extreme assumptions about the number of cases missed to approach the levels necessary for a crime increase to be caused directly by permit holders. The best available evidence, while flawed, does show that permit holders are more law-abiding than the general population, and the possible increase in firearm-related crime caused by concealed carry laws is likely only a fraction of the potential overall increase in crime.

Indeed, the only way to have a sufficient number of permit holder crimes to generate an increase is if Gary Kleck's defensive gun use survey is accurate. As mentioned in the section on Direct Deterrence, Kleck's and other private surveys estimate millions of defensive gun uses annually. While Kleck states that he "made no effort to assess either the lawfulness or morality of the

respondents' defensive actions," his survey does estimate that 36-64% of the DGUs were likely illegal.

A series of two similar [surveys](#) published in 2000 by Harvard researchers further explored how many of the reported DGUs would likely be illegal by submitting responses to a panel of 5 judges. In a majority of those cases, the panel concluded that the DGU was likely illegal, assuming both the respondent's story was 100% accurate and that they were legally carrying the firearm at the time.

This means that if one-off survey estimates of millions of DGUs are accurate, it actually re-opens the causal pathway for concealed carry laws to increase crime through direct escalation. Lott emphasizes that "...state police agencies would have to be missing around 99.4% to 99.83% of violent crimes committed by permit holders" to see crime increases of a sufficient magnitude. Yet Kleck's estimates require those very agencies to be missing 99.9% of DGUs, more than half of which are likely illegal based on a review of survey participants' responses. However, there is no feasible reason at this point to believe Kleck's estimates are accurate, so just as direct deterrence is not a viable causal pathway, neither is direct escalation.

While it is possible that there is some crime increase directly caused by permit holders, the best available data indicate that cases of permit holder crime are too scarce to generate significant increases. Unless future data and research is produced that reveals significantly more recorded crime by permit holders than currently exists, it is safe to conclude that direct crime escalation is not a sufficiently wide causal pathway for concealed carry laws to significantly increase crime.

Indirect Escalation

The secondary mechanism through which concealed carry laws could increase crime is through indirect escalation. Under this framework, criminals recognize that they are more likely to encounter armed individuals, and therefore arm themselves in order to avoid being outgunned. The resultant criminal arms-race can be caused by three possible pathways: (1) criminals encounter more "good guys with guns" and arm themselves in response; (2) criminals have easier access to firearms due to stolen guns flooding the market; and/or (3) criminals encounter an increased number of other "bad guys with guns." While it is technically possible that the announcement effect of a concealed carry law could spark an arms-race, it is just as unlikely responsible for indirect escalation as it is responsible for indirect deterrence.

Criminals arming themselves in response to "good guys with guns" hinges on the prerequisite that they either directly encounter armed good guys on a regular basis, or know the density of permit holders in their area. However, the evidence that indicates that direct and indirect deterrence are not viable causal pathways for crime reduction also apply here, with one exception.

The same [1985 survey](#) of prison inmates that John Lott and Gary Kleck use to argue the viability of indirect deterrence also contains evidence that many criminals maintain a "better safe than sorry" approach to carrying their own firearms. More than 70% of the surveyed inmates who carried firearms said they did so in order to not hurt the victim by maintaining control over the

situation, more than 60% indicated the chance their victim might be armed played a deciding role in carrying, and a little under 70% wanted to be “prepared for anything” (the respondents could choose more than one answer). Unlike deterrence theory where a criminal needs to have some idea of the likelihood of encountering armed resistance when choosing a target, in deciding to carry their own firearm, criminals would not need to estimate such a distribution. The firearm is a hedge just in case the perpetrator needs additional force (no matter how small that probability may be). John Donohue’s 2018 iteration of his study on concealed carry laws provides some evidence for this form of escalation, with armed robberies significantly increasing in states that pass RTC laws.

The primary pathway through which concealed carry could lead to an increase in violent crime via indirect escalation is widespread firearm theft that leads to an arms race. A [2003 study](#) by Phillip Cook and Jens Ludwig found that communities with higher levels of gun ownership had significantly higher rates of burglary, indicating that firearms in the home are an inducement, not a deterrent, to crime (after controlling for a number of factors including per capita income). Carrying a firearm as an [estimated](#) 3 million Americans do every day, and 9 million at least once a month, changes behavior both outside and in the home. People who carry a concealed firearm do so for self-defense, which typically means more emphasis is placed on easy access to those firearms than secure storage. Indeed, a [2017 survey](#) by Harvard researchers found “...that owning many guns, owning guns for protection, carrying guns, and storing guns unsafely are associated with having guns stolen.”

Indeed, firearm theft is widespread. In 2016, more than [237,000 firearms](#) were reported stolen according to an FBI database, an increase from under 150,000 in 2005. This result is actually an underestimate, as it only counts reported thefts. The 2017 Harvard survey estimated 250,000 gun theft incidents which amount to a total of 380,000 stolen firearms annually. One reason for the substantial increase in thefts is due to unsecured [firearms left in cars](#), which is highly related with concealed carry rates in a population. People who carried firearms at least once in the past month were [three times](#) more likely to have had a firearm stolen than other gun owners.

It is important to note, unlike with indirect deterrence, criminals need not know the distribution of people carrying firearms in their area. Instead, burglars can operate off a trial and error basis. If they don’t find firearms in one home or car, they can just move to the next. Once they do find a community with poorly stored firearms, it is likely there are [more easy targets](#) in the vicinity. Stolen firearms are easy to transport, very hard to trace, and are quite lucrative to sell with little risk of being caught thanks to the large private sales loophole.

Once firearms enter the [vast gray market](#) for guns, they can spend years drifting from one owner to another before finally ending up at a crime scene. There is some academic debate over how much of a role gun theft plays in other criminal activity. A [2018 study](#) by Phillip Cook found that fewer than 1% of firearm transactions were theft, and most criminals do not steal the firearm they used in their most recent crime. Indeed, most criminals [obtain firearms](#) from their social network, and rarely buy from a gun store or personally steal the firearms. That being said, simply because the most recent criminal to possess a firearm didn’t steal that firearm doesn’t mean it wasn’t stolen at some point. Every firearm in the US starts out legal (with a few exceptions), and

either enters the criminal market directly from gun dealers, or from gun owners. Indeed, the fact that many criminals don't personally steal their firearm reveals just how large the gray market for those guns is, a market directly facilitated by weak gun laws on private sales as well as irresponsible gun owners not storing their firearms properly. Further, even if the firearm never ends up at a crime scene, it is still financially enabling more crime.

Significant empirical and academic evidence indicates concealed carry can increase crime through indirect escalation, particularly due to gun thefts that further facilitate violent criminal activity. More research should definitely be conducted to probe the linkage between concealed carry, firearm theft, a culture of self-defense, and the ripple effect those have violent crime. That being said, based on the best available data it is safe to conclude that the indirect escalation causal pathway is robust and viable.

Causality Conclusion

It is important to note that this causality discussion employed the "good guy" vs "bad guy/criminal" dichotomy in order to parallel arguments in favor of deterrence theory. However, while deterrence theory requires such a stark dualistic framework (good guys stopping bad guys), escalation theory allows for a more nuanced and realistic view of the world, where criminality is on a spectrum. While someone with a violent criminal history is clearly different from someone who could easily pass a background check, a significant amount of gray area exists in the middle.

For example, is a permit holder with a history of violent misdemeanors who can still obtain a permit a "good guy with a gun" or a bad guy? What about permit holders who frequently flout laws and seek to circumvent rules that don't allow them to carry in certain locations? Or a legal gun owner conducting a private sale of a firearm to a complete stranger for whom there is no way to tell whether he is a criminal or not? Or a permit holder who uses his firearm to intimidate a significant other?

The ability for a gun owner to pass a background check to obtain a firearm is not synonymous with being a "good guy." Not breaking the law and acting responsibly are not necessarily the same, and sometimes perfectly legal actions can be detrimental for societal safety. Even "good guys" can lose their temper and escalate a situation that wouldn't have absent the presence of a firearm. Or in a moment of forgetfulness leave a firearm on the counter where a child could find it. Or think that having a gun in every room of the home makes the family safer, when in fact it at best makes it easier for burglars to steal those firearms, and at worst puts his family in mortal danger.

Even within the "good guy vs bad guy" framework, the best available data indicate that the direct and indirect deterrence causal pathways lack the explanatory power necessary for concealed carry laws to reduce crime. There are not enough defensive gun uses to provide for a crime decrease under the direct deterrence framework. The average person's lack of knowledge surrounding the distribution of permit holders, the actual location of permit holders, and a lack of a significant reduction in robbery all undercut the indirect deterrence mechanism for decreasing crime.

Meanwhile, though the phenomenon of permit holders directly escalating crime is too rare to explain a substantial increase in crime from concealed carry laws on its own, an indirect escalation of crime is quite probable. A substantial number of gun thefts every year provide for a large gray market for firearms. These firearms then facilitate the escalation of crime.

However, simply because a causal pathway is viable does not guarantee that it is being used. More research is definitely needed to directly examine the causal links between concealed carry laws and crime. These studies could reveal that the causal pathway is not actually available and that concealed carry laws have no discernible impact on crime. Yet, unless future groundbreaking research unveils new data on deterrence, any correlational studies showing an alleged crime decrease resulting from concealed carry laws can reasonably be labeled as the product of poor modeling and statistical noise.

Given the existing body of academic literature as well as an examination of the various forms of causation between concealed carry and crime, one may conclude that at best RTC laws have no effect on crime, and at worst substantially increase violent crime rates.

Implications

This report finds the following:

- A naïve count analysis of academic studies from 1995-2018 indicates an evenly split literature on whether RTC Laws reduce, increase, or have no effect on crime.
- Focusing on modern studies (post 2005) with a national scope reveals that a majority find that RTC Laws increase crime.
- There are an insufficient number of defensive gun uses for RTC laws to reduce crime through direct deterrence.
- Indirect deterrence is highly implausible due to a lack of general knowledge about the distribution of permit holders.
- There are an insufficient number of permit holder crimes for RTC laws to increase crime through direct escalation.
- An increase in crime through indirect escalation is highly likely given the more than 200,000 gun thefts every year.
- The modern academic literature and investigation of causal pathways both reinforce the conclusion that RTC laws likely increase crime, or at best have no net beneficial effect.

These results provide a number of insights regarding state and federal policy on gun violence. Efforts such as Concealed Carry Reciprocity, which would allow individuals from states with looser restrictions to obtain a concealed carry permit to carry their firearms in states with stronger laws, are deeply misguided unless paired with measures that substantially increase training requirements and the strength of background checks. The growing trend of permitless carry (otherwise known as “constitutional carry”) that has already spread to fourteen states removes the two most beneficial aspects of the permitting process: passing a background check and undergoing at least some training. As such, permitless carry is likely to exacerbate the crime inducing effects of concealed carry. Allowing concealed firearms into more sensitive areas such as schools and places of worship is also likely to have a similar detrimental effect on community safety.

Public policy should be evidence based and rely on solid, academic studies. As discussed in this paper, replacing RTC laws with May Issue standards provides local law enforcement discretion to double check whether a permit applicant is truly responsible before granting the permit. Increased training with an emphasis on deescalating conflicts and how to safely store firearms to avoid unintentional shootings and theft could also help reduce gun violence. Furthermore, the training should provide a clear-eyed assessment of the dangers that accompany owning and carrying a firearm. Studies clearly show that gun violence can be reduced by adopting an evidence-based approach to firearm policy and shifting towards proposals with significant academic evidence support such as Permit to Purchase, Extreme Risk Protection Orders, and funding community-led programs aimed at reaching at-risk individuals.