Gun Control and Crime: Evidence from Concealed Carry Laws in the US

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Abstract

This paper investigates the impact of handgun deregulation on violent crime and firearm mortality. A third of US states have liberalized the concealed carry of handguns, no longer requiring carriers to acquire permits, take safety classes, or undergo background checks. Proponents of Unrestricted Control Carry (UCC) argue that this will reduce crime because criminals will no longer be able to distinguish between armed and unarmed targets. Opponents argue that these changes will increase crime, as criminals will now face no repercussions if they are caught with a concealed weapon. It is also possible that the positive and negative effects of the law offset each other. A final possibility, which I call *neutrality*, is that UCC will have no impact. Handguns are small and easy to conceal, so people need not wait for the state to adopt UCC. I study the impact of this policy by matching counties along the borders of states that liberalized concealed carry with contiguous counties in neighboring states that did not. I construct a county-month panel dataset using death certificate micro-data and crime incident micro-data, and use it to compare differences in crime and mortality between such pairs of counties. In my preferred specification, I find that UCC leads to statistically and economically insignificant 0.15% decrease in homicides. Furthermore, the policy has no impact on other violent crime or overall firearm mortality. Consistent with neutrality, but not offsetting, the policy has no impact on gun ownership or day-to-day usage. I discuss the implications of these results for gun control policy.

JEL classification: K0, K14, K4, K40, K42
Keywords: Concealed Carry, Crime, Gun Control, Handgun Violence, Homicide

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1 Introduction

The United States of America (US) has an unusually high incidence of violent crime, given its per capita GDP. The annual homicide rate is 5.3 per 100,000 residents (Federal Bureau of Investigation 2016) as opposed to the OECD average of 3.2 (OECD 2016). Residents often acquire firearms for self-defense (Pew Research Center 2017). However, the US also experiences 40,000 firearm-related deaths annually. Gun control laws aim to balance the defensive and recreational use of guns with the prevention of violent crime and firearm deaths. These laws include background checks, licensing, mandatory safety classes, and restrictions on what firearms are bought and sold, among others.

This paper analyzes one such regulation in depth: the unrestricted carry of concealed firearms in public. Proponents of concealed carry argue that it reduces violent crime, because it prevents criminals from distinguishing between armed and unarmed victims. The higher expected morbidity and mortality from committing a crime leads to fewer crimes being committed. Opponents argue that UCC increases crime, because criminals themselves can now be legally armed while waiting for an opportunity to commit a crime. If intercepted and searched by the police, they face no repercussions when caught with a concealed weapon. A third possibility is that these two effects offset each other, resulting in no change in crime, but an increase in firearm ownership and usage. A final possibility is that such laws will have no effect at all, because the preceding restrictions were difficult to enforce. A person carrying a concealed handgun is unlikely to be stopped and searched by the police, and therefore has little disincentive to carry. Criminals, and even people with no malicious intent, do not need to wait for deregulation to carry a concealed firearm. I call this argument *neutrality*, and find support for it.

At the federal level, there are no laws governing the carry of firearms in public spaces. At the state level, there are two sets of laws. *Open carry laws* govern the act of publicly carrying a weapon[1] "in plain sight", e.g., in a hip or shoulder holster. The focus of this study are *concealed carry laws*, which govern the public carry of a weapon that is *not* in plain sight e.g. in one's purse, or glove compartment, or waistband. No issue-regimes ban concealed carry altogether. May-issue regimes allow concealed carry with a permit, but give local authorities broad discretion when issuing these permits. Shall-issue regimes do *not* give local authorities any discretion; applicants are entitled to a permit if they meet certain conditions such as background checks. Unrestricted concealed carry regimes (UCC) are the most permissive regimes, and allow residents to carry a concealed firearm without a permit. Table 1 presents the concealed carry regimes in the US and maps them to the states that have adopted them.

The existing literature on concealed carry studies the transition of US states from may-issue to shall-issue regimes. In 1960, the vast majority of US states had may-issue

[1] "Weapon" here includes firearms and knives, but is not limited to them. For example, the state of Florida considers pepper spray to be a weapon.

regimes in place. In response to rising crime in the 1970s, states started adopting shall-issue regimes (Winkler 2011) and by 2011, 36 states had done so. Lott and Mustard (1997) made the claim that, had all states in the US adopted shall-issue laws, "1,570 murders, 4,177 rapes, and over 60,000 aggravate assaults would have been avoided yearly." This claim was challenged by researchers criticizing their data and methodology.[2] In 2005, the National Research Council released a report on gun violence that concluded that there was insufficient evidence to show that shall-issue laws either decreased or increased crime (National Research Council 2005), leading to another round of debate.[3] More recent papers call for caution in analyzing shall-issue laws, finding ambiguous impact.[4]

Unrestricted concealed carry is a relatively new development in gun control. In 2003, Alaska's legislature became the first to adopt unrestricted concealed carry. In 2010, Arizona became the second. As of September 2020, 15 out of 50[5] US states have unrestricted concealed carry regimes in place, and yet little research has been done on the impact of these laws. This paper provides, to the best of my knowledge, the first comprehensive analysis of this unprecedented development in gun control.

The impact of gun control laws can be difficult to identify because of the endogeneity of gun laws, gun ownership, and crime. Perhaps loose gun control laws lead to more criminals acquiring guns. Perhaps high criminal victimization can lead to voters demanding looser gun laws to protect themselves. Perhaps a culture of gun ownership leads to looser laws as well as more crime. Perhaps it's some combination of these factors. It can be difficult to extract causation from correlation.

I tackle endogeneity using a paired border counties difference-in-difference approach. Counties along the borders of US states are similar to contiguous counties in neighboring states. Under this methodology, each county along the border of a treatment state is matched to an adjacent county in a control state, creating a pair. If a county has more than one neighbor in the control state, more than one pair of counties is created. For outcomes of interest, I construct a variable measuring the difference between counties within a pair, and then track this variable after the implementation of UCC. I deploy an OLS regression that includes a full set of county fixed effects, county-trends, and state-month fixed effects.[6] To the best of my knowledge, this is the first paper in the gun control literature to

[2]Black and Nagin (1998) argued that their results were sensitive to small changes in the model and sample. Ayres and J. J. Donohue (2003) argued that "more refined" analysis showed the opposite - that crime rose as a result of shall-issue laws. Maltz and Targonski (2002) pointed out they used imputed data to analyze crime, which it ill-suited for. Helland and Tabarrok (2004), using "placebo laws" find that the impact on crime is not well-estimated. These critiques prompted replies from the authors - see Lott (1998), Lott and Whitley (2003), and Plassmann and Whitley (2003)

[3]See Moody and Marvell (2008), J. Donohue and Ayres (2009), Moody and Marvell (2009), Abhay Aneja, Donohue III, and Zhang (2011), Moody, Lott, and Marvell (2013), J. Donohue, Aneja, and Weber (2017), M. B. McElroy and Wang (2017).

[4]Durlauf, Navarro, and Rivers (2016) explicitly argue that "one should be cautious in using the results from any particular model to inform policy decisions." Gius (2018) found that different empirical methods yielded different conclusions about the link between shall-issue laws and murder rates, reversing Gius (2014). Manski and Pepper (2018) concludes that "[shall-issue] laws increase some crimes, decrease other crimes, and have effects that vary over time for others." Gresenz (2018) reviews the literature and concludes that evidence "that shall-issue concealed-carry laws may increase violent crime is limited. Evidence for the effect of shall-issue laws on total homicides, firearm homicides, robberies, assaults, and rapes is inconclusive."

[5]Figure 1 shows the liberalization of concealed carry in the US.

[6]State-month fixed effects are used to control for seasonal variation

compare border counties in order to identify the impact of a firearm regulation.

Data on mortality are drawn from the Multiple Cause of Death (MCOD) files, maintained by the Centers for Disease Control. These files contain the universe of US death certificates and are made available to researchers upon approval. These data include the date of death, the time of death, county of death, county of residence, the race and gender of the deceased, and the cause of death. For cases in which the cause of death was a firearm, the data include whether the death was a homicide, accident, suicide, or caused by unknown intentions. Access to this micro-data allows me to analyze the impact of UCC with a degree of granularity that previous researchers did not have access to. A summary of mortality in my sample can be seen in Table 7.

Homicide is one of the most important outcomes of interest, and the one most robust to manipulation by law enforcement agencies. This paper analyzes the impact of UCC on homicide using the data from death certificates and the aforementioned methodology. In my preferred specification, there is a statistically and economically insignificant change in homicide, corresponding to an increase of only 0.15%, which is consistent with the neutrality hypothesis. It is also consistent with the offsetting hypothesis but, as we shall see later, this hypothesis is not supported by the data.

I supplement the analysis of mortality data with data on crime. These data come from the incident-level files which are collated by the National Incident Based Reporting System (NIBRS), which is maintained by the Federal Bureau of Investigation (FBI). Law Enforcement Organizations (LEOS) use their own record management systems to send incident reports to the FBI. For the agencies that participate in the program, it collects data for each incident of crime that occurs within the jurisdiction of that LEO, provided the crime falls under the 47 categories that the FBI tracks. The NIBRS includes incident-level data on crimes committed, and these data include the date, time, type of incident, weapon used, and the race and gender of offenders and victims, among other factors. A summary of crime in my sample can be seen in Table 9.

Analyzing these data shows that adopting UCC leads to a statistically insignificant change in homicide, consistent with the neutrality hypothesis. The magnitude corresponds to a statistically insignificant 6.2% decrease in the incidence of this crime, with sufficient power to rule out large effects.[7] Extending this analysis to other violent crimes, I find that UCC has no impact on either aggravated assault or robberies.[8]

It is possible that the policy led to an increase in criminals *and* law abiding citizens carrying weapons, resulting in no overall change in crime. In such a scenario, detecting no impact doesn't mean that the law is truly neutral - merely that the positive and negative effects offset each other. If this were true, the adoption of UCC should lead to an increase in firearm usage and ownership, but no increase in crime.

[7]The difference between this estimate and the estimate generated by the mortality data can be ascribed to the fact that not all LEOS were part of the NIBRS program, making the data noisier and less comprehensive.

[8]Aggravated assault refers to incidents that lead to serious injuries, or incidents where the assailant brandishes a firearm. Robberies include incidents that are colloquially known as muggings.

Niekamp (2018) studies the impact of hunting season on firearm usage by using the population-adjusted number of armed arrestees as a proxy for day-to-day gun usage. The NIBRS data include reports about arrests made, and whether the arrestee was armed or not, even if the arrest was a for a non-violent crime. Following Niekamp's methodology, with sufficient power to detect modest changes, I find no evidence that the adoption of UCC leads to an increase in day-to-day gun usage.[9]

Moody and Marvell (2005) - among others[10] - use the population-adjusted number of firearm suicides as a proxy for gun ownership. The MCOD data contains the universe of suicides, and flags firearm suicides. Using this proxy, I find no evidence that the adoption of UCC leads to an increase in gun ownership.

The absence of changes in gun usage and gun ownership, taken together, support the theory that UCC is truly neutral; it is not the case that negative and positive effects offset each other.

Even if UCC had no effect overall, it may have had an impact on specific demographics. Due to the lack of suitable data, research on heterogeneity in the impact of firearm regulations has been limited. Gun control measures can have differing impacts on people who live within the same regime. For example, some argue that women may benefit from looser gun regulations because guns equalize physical contests between women and men, especially in cases of sexual assault. However, women are only one-fourth as likely as men to own a gun,[11] so they would not benefit from a policy allowing them to covertly carry a weapon that they do not possess.

This paper finds that UCC had no impact on sexual assault. This remains true even when the sample is limited to only female victims. There is also no increase or decrease in homicides, aggravated assaults, and robberies with female victims. In all of these cases, I have sufficient power to rule out modest changes in the incidence of this crime, with the exception of homicide[12].

African-Americans are another group who may experience UCC differently than white men, because they experience higher criminal victimization,[13] are less likely to own firearms than whites,[14] and more likely to be victims of police shootings[15]. I find no change in the number of Black victims of aggravated assault, sexual assault, or robbery. Similarly, I find no differential impact of UCC on crime in urban areas, which have fewer guns and more crimes than rural areas.

Perhaps there were other changes to the legal system that confounded this analysis? To check this, I construct a placebo variable of crimes that are not plausibly affected by

[9] Following Bloom (1995) I define effect sizes as follows: 0-10% are small effect sizes, 10-25% are modest effect sizes, >25% are large effect sizes

[10] Also see Cook and Ludwig (2006) and T. Kovandzic, Schaffer, and Kleck (2013)

[11] Pew Research Center 2017.

[12] While I do not have sufficient power to rule out modest changes, the size of the effect corresponds to a 4% decrease

[13] Bureau of Justice Statistics 2016.

[14] Pew Research Center 2017.

[15] Lott and Moody 2016.

gun laws[16]. With sufficient power to detect modest changes, this paper finds that UCC has no impact on the placebo.

Perhaps this law led criminals to switch to less confrontational crimes? I check the effect of this policy on burglary, theft,[17] and simple assault.[18] With sufficient power to rule out modest effects, this paper finds that the adoption of UCC leads to no changes in these crimes.

A possible shortcoming of the border counties approach is that the relatively small number of border counties and the relatively short time-span that this policy has been in existence for may lead to insufficient power for analysis. It is therefore supplemented - at each step - with a traditional all-county difference-in-difference approach, with the increased power compensating for the lack of plausible identification. I use an OLS regression with a full set of county fixed effects, county-trends, and region-month fixed effects.[19] The results from this analysis are, overall, consistent with the results from the border counties analysis.

These conclusions must be interpreted carefully. This paper does *not* claim that concealed carry is harmless or completely lacking in impact. All states studied in this paper transitioned from a shall-issue regime to an unrestricted regime. It is possible that *this* transition is harmless while the transition from may-issue to shall-issue is harmful (or beneficial, or harmless.)

What are the implications of these results for policy?

First, UCC is not a successful crime-fighting tool. This is unfortunate, as it requires no taxation or public funding, and would have been a cost-effective tool had it succeeded. Policy-makers who wish to reduce the incidence of violent crime will need to look elsewhere.

Second, UCC is not a dangerous form of deregulation. This is fortunate, as a third of US states have transitioned into UCC regimes and it is comforting to know that the residents of these states have not been subject to increased violence as a result.

Third, the enforceability of laws should be a major concern while drafting gun control legislation or prioritizing gun control activism. Laws governing the carry of weapons may be impractical, and so activists may wish to turn their attention to, for example, universal background checks, or legislation that ensures that people with domestic violence records are restricted from purchasing firearms via the NICS. Siegel et al. (2019) provides a overview of gun control measures that do affect mortality, and is an excellent resource for gun control activists.

[16] This include blackmail, counterfeiting, fraud, embezzlement, pornography, gambling, sex work, bribery, bad checks, loitering, vagrancy, and driving under the influence.

[17] Incidents that do not involve a confrontation with a victim e.g. the theft of an unlocked bicycle.

[18] Assault that did not happen with a weapon, nor resulted in serious injury

[19] I divide the US into 6 climatic regions: the north-east, the north, the south, the south-west, California, the northwest. Region-month fixed effects are used to control for seasonal variation

2 Context

From 1976 to 2008, Washington DC banned the purchase of handguns by residents. In 2008, the Supreme Court of the United States ruled this to be a violation of the Second Amendment of the US Constitution, asserting that it guaranteed the right of citizens to possess firearms[20]. However, the ruling also gave states the right to impose "reasonable restrictions", such as banning convicted felons from possessing a firearm. Most importantly, for the purpose of this project, the ruling explicitly did not guarantee the right of citizens to carry a concealed firearm. Supreme Court Justice Antonin Scalia wrote:

> Like most rights, the Second Amendment right is not unlimited. It is not a right to keep and carry any weapon whatsoever in any manner whatsoever and for whatever purpose: For example, *concealed weapons prohibitions have been upheld under the Amendment or state analogues* [Emphasis added]. The Court's opinion should not be taken to cast doubt on longstanding prohibitions on the possession of firearms by felons and the mentally ill, or laws forbidding the carrying of firearms in sensitive places such as schools and government buildings, or laws imposing conditions and qualifications on the commercial sale of arms.

There are currently no federal laws governing the possession a weapon in public - all such laws are made by local or state legislatures.

The concealed carry of a weapon refers to the practice of carrying a weapon, such as a handgun or a knife, in a public place in such a way that it is not "in plain sight". It could be on one's person (e.g. in a purse or one's waistband) or in close proximity (e.g. in a glove compartment.) While the exact definition of "plain sight" varies from jurisdiction to jurisdiction, researchers recognize 4 types of concealed carry regimes:

- *No Issue:* Residents are not allowed to carry a concealed handgun in a public place. American Samoa is one such place.
- *May-Issue:* These are jurisdictions that allow the concealed carry of a handgun, but local authorities have discretion over whether permits are granted or not. Many counties and cities in may-issue regimes are no-issue regimes in practice, as law enforcement agencies are often reluctant to issue such permits[21].
- *Shall-Issue:* These are jurisdictions that require a permit to carry a concealed gun, and local authorities have little or no discretion over who gets a permit. Authorities are required to issue a permit if the applicant has fulfilled conditions determined by the law. These conditions can include background checks, minimum ages, safety and training classes, safety tests, and proficiency tests, amongst other things. The

[20]Supreme Court of the United States 2008.
[21]Cramer and Kopel 1995.

laws that create such regimes are referred to as “Right-To-Carry" laws or “Shall-Issue” laws.

- *Unrestricted Concealed Carry*: A permit is not needed to carry a concealed handgun. This term is a slight misnomer - there are still restrictions on who can carry a concealed weapon (e.g. people with felony convictions and underage people are not allowed to) and where (e.g. guns are banned in courthouses and schools.)

Table 1 presents the 4 categories from least-to-most permissive and enumerates the states that fall under these policies.

In 1813, Kentucky and Louisiana banned concealed carry, and were the first states to do so. Over the course of the nineteenth century, an increasing number of US states followed suit and by the middle of the twentieth century, most states legislated concealed carry restrictions[22]. A notable exception is Vermont. In 1903, the city of Rutland in Vermont attempted to regulate the carry of weapons in public spaces. However, the Vermont Supreme court ruled it a violation of the state constitution[23]. For this reason unrestricted concealed carry is also known as Vermont carry.

In 1961, Washington adopted a shall-issue concealed carry permitting regime. In 1980, responding to a sharp increase in crime, Indiana became the second US state to do so[24]. This process accelerated in the 1980s and 1990s, as an increasing number of states transitioned from no-issue or may-issue regimes to shall-issue regimes. By 2011, 36 states had enacted such laws. The bulk of the empirical literature on concealed carry examines the transition from may-issue regimes to shall-issue regimes[25]

In 2003, Alaska became the first state to switch from a shall-issue regime to an unrestricted concealed regime. In 2010 Arkansas became the second. This process accelerated in the 2010s and, at the time of publication, 15 out of 50 US states have UCC regimes in place. Figure 1 shows the change in policy in US States over time, while Table 2 shows the dates at which unrestricted concealed carry went into effect in various states. Every treatment state in the sample is a state that transitioned from shall-issue to unrestricted concealed carry.

Closely related to concealed carry laws are open carry laws, which determine whether or not citizens are allowed to carry a visible weapon in plain sight. As with concealed carry laws, states adopt one of 4 types of open carry regimes:

- *Banned:* These are jurisdictions the effectively outlaw open carry e.g. the District of Columbia.
- *May-Issue:* These are states that allow open carry with a permit, but give local authorities substantial discretion in issuing permits, making them *de-facto* non-permissive regimes. New Jersey is one such state.

[22]Winkler 2011.
[23]Supreme Court of Vermont 1903.
[24]Winkler 2011.
[25]Gresenz 2018.

- *Shall-Issue:* These are states that allow open carry with a permit, and do not give local authorities any discretion over who may be issued a permit e.g. Minnesota.
- *Permissive:* These are states that allow the open carry of weapons without a permit. e.g. Kansas

While a history of open carry is beyond the scope of this paper, it should be noted that all treatment states in the sample have had permissive open carry regimes in place for the entirety of the treatment period.

3 Conceptual Framework

In this section, I will outline an informal theory of how the incidence of crime might be affected by concealed carry laws, as well as a formal mathematical model of concealed carry and violence. This model will not be estimated; its purpose is solely to illustrate the incentives facing criminals and non-criminals under different carry regimes.

3.1 How Might Concealed Carry Affect Crime?

I follow the long economic tradition established by Becker (1968) and treat the criminal as an agent weighing the costs and benefits of committing a crime. The benefit is the material or immaterial gain from successfully committing a crime. The costs are many; among others, there is the guilt or shame from having committed a crime, the opportunity cost of committing a crime, the social costs that arise if one's community discovers one's wrongdoing, the legal penalties that arise if one is caught and convicted, and the physical danger from a target who resists. It is that last two of these that deregulating concealed carry affects, in opposite directions.

Criminals who confront their victims, such as muggers, have to consider whether their potential victims are armed or not. Visibly armed targets are easy to avoid. For the sake of argument, let us put aside the possibility that people using concealed carry defensively will do so regardless of the law. Under no-issue or may-issue concealed carry regimes, targets are likely unarmed. Under shall-issue regimes, targets are more likely to be invisibly armed, but statutory and bureaucratic hurdles may keep the probability low. The likelihood of an encounter with an invisibly armed victim is highest under unrestricted concealed carry regimes, and this may discourage criminals who fear for their health or safety.

However, now criminals themselves may be armed without fear of reprisal. Criminals motivated by material gain are often opportunistic, waiting for the right moment and target to enact a crime (Felson and Clarke 1998). A mugger who openly carries a weapon can be avoided by potential victims. In a shall-issue concealed carry regime, a mugger who applies for a permit leaves a paper trail, which they may wish to avoid. A mugger who carries a concealed weapon without a permit risks being intercepted by law enforcement officials who can arrest them under any regime other than unrestricted concealed carry. Under unrestricted concealed carry, criminals without a felony record can carry a concealed weapon without fear of being arrested by law enforcement. Thus potential legal penalties are reduced.

Neutrality can arise under UCC if potential criminals (or law-abiding citizens) are unlikely to be stopped and searched by police officers, making the cost of covertly carrying a weapon negligible. A change in the costs of carrying a weapon in this situation would therefore have no effect.

It is important to consider how gun control affects different sub-populations. For example, proponents of concealed carry claim that women will be beneficiaries of such a policy, since a gun acts as an equalizer in a physical contest with men. However, women are only half as likely to own guns, and four times more likely to live with someone who owns a gun, possibly because of the belief that guns are a "masculine" hobby(Pew Research Center 2017). They might not benefit from a policy that allows them to carry a weapon if they don't own one. Furthermore, if this policy disproportionately encourages men to purchase guns, it might put women at greater risk if a domestic conflict escalates. Finally, if men become more likely to carry a concealed weapon, criminals may redirect their attention to women, increasing their vulnerability to crime.

African-Americans may experience this policy differently from white people do, as they are unusually vulnerable to crime, as well as to police shootings (Lott and Moody 2016). An unrestricted concealed carry regime might make police officers fearful for their safety, which could lead to an increased willingness to open fire in uncertain situations. African-Americans might disproportionately be victims of such a dynamic. Furthermore, they are also less likely to own a gun, and therefore might benefit less from such a policy (Pew Research Center 2017).

Finally, it is difficult to say whether such laws would have more of an impact on cities, where the incidence of crime is higher, or in rural areas, whose residents are more likely to own a firearm.

3.2 Formal Model

In my formal conceptual framework, individuals must choose between committing an armed crime and not committing an armed crime. If they choose not to commit an armed crime, they must choose between not carrying a weapon, openly carrying a weapon, and covertly carrying a weapon.

Let us first consider the decision to commit a crime. The benefit of committing an armed crime to person i is the gain if they are successful, π_i. Committing an armed crime carries numerous costs. Let L be the legal penalties faced by the individual if caught with an illegal firearm by the police with some probability p_L. Let M measure the cost of being injured if the victim is also armed and fights back with some probability p_{armed}. Finally let C_i be measure all other costs of committing the crime: guilt, forgone wages etc. C_i

also includes the expected costs that arise if caught e.g. jail time, the social and economic burden of a criminal record, the disapproval of one's community, and so forth.

I assume that if the target is armed, the criminal accrues no gain. The crime is worth committing if :

$$(1 - p_L - p_{armed})\pi_i \geq C_i + p_L L + p_{armed} M$$

where $p_L - p_{armed} < 1$. Crime is decreasing in (a) the legal penalties faced if caught with a firearm L and (b) the probability of a victim being armed and fighting back p_{armed}. Heterogeneity in the values of π_i and C_i serve to prevent the fraction of people committing a crime from collapsing to 0% or 100% in this model.

The net benefit of committing a crime is

$$B_i = (1 - p_L - p_{armed})\pi_i - C_i - p_L L - p_{armed} M$$

which implies $\delta B_i / \delta L = -p_L$. Thus if p_L is small, the overall response to the deregulation of concealed carry will be as well, consistent with neutrality.

The overall effect of adopting unrestricted concealed carry regimes in this model is ambiguous. UCC can decrease L, because criminals can now legally carry guns undetected until they see an opportunity to commit a crime. However, unrestricted concealed carry laws can increase p_{armed}. Under no-carry, potential victims are disarmed. Under the most permissive open-carry regimes, potential victims have to be *visibly* armed, and criminals can avoid them. In both cases $p_{armed} = 0$. A state that transitions from a may-issue to a shall-issue regime may see an increase in p_{armed}, but the cost of dealing with a bureaucracy may keep p_{armed} low. Under unrestricted concealed-carry regimes, anyone can carry concealed weapons, potentially increasing p_{armed} substantially. Thus, the overall impact of UCC on crime is ambiguous, because it decreases p_L but increases p_{armed}.

Consider a potential victim j who must choose between not carrying, openly carrying, and covertly carrying a handgun. Let $K \cdot \pi_j$ be their loss from an incident, where $K \geq 1$ is a cost multiplier which reflects the fact that the suffering experienced by the victim might be greater than the gain to the criminal. Let $p_{j,victimized}$ be the probability that they will be victimized, g_j be the cost of acquiring a handgun, O_j be cost of openly carrying a handgun (e.g. people may avoid you socially), and κ_j be the cost of covertly carrying a handgun (which could be zero). j will covertly carry a handgun if

$$K \cdot \pi_j \cdot p_{j,victimized} \geq g_j + O_j \geq g_j + \kappa_j$$

Note that the cost of acquiring a gun $g_j = 0$ if j already owns a gun.

In this model, heterogeneity in carry strategies arises from these costs varying from person to person. For a woman j, the cost of being victimized π_j might be higher than for men, since women are disproportionately victims of sexual assault (Bureau of Justice Statistics 2016). Furthermore, if guns as seen as "masculine", the cost of openly carrying a gun O_j might be higher than the cost of covertly carrying κ_j. However, the cost of acquiring a gun g_i is likely higher for women - they are 75% less likely to own a firearm (Pew Research Center 2017).

African-Americans are more likely to be victims of crimes than whites (Bureau of Justice Statistics 2016), so the odds of being a victim $p_{j,victimized}$ might be higher for African-Americans than for whites. However, so might the costs of carrying a gun - O_j and κ_j - if they are likely to be racially profiled by the police.

Finally, urban areas tend to have more crime per capita, and so people that live in these areas may face a higher likelihood of being victimized $p_{j,victimized}$. However, rural areas tend to have more guns per capita, which means they may already have a gun at home i.e. $g_j = 0$.

Thus it is important to check the heterogeneity in the impact of these policies.

4 Data

4.1 Data on Mortality

When a US resident dies, their death is registered using the US Standard Certificate of Death which is filled out by a medical examiner. The causes of death are classified according to the International Classification of Diseases (10th Revision). These death certificates are collected as part of the National Vital Statistics Systems (NVSS), an inter-governmental system for the sharing of data on births and deaths in the US. The NVSS is the product of coordination between state health departments and the National Center for Health Statistics, a division of the Centers for Disease Control and Prevention.

Since 2005, death certificate data has not made publicly available due to concerns about privacy. However, approved researchers are given access to the Multiple Cause of Death (MCOD) files, which contain anonymized data on each death that occurs in the US. The observational unit in these data is a death. These data include the date and time of death, the county of residence, the county of death, race, sex, age, the cause of death, and so on.

For this paper, I use data from 2008 to 2015, the last year for which these data were made available. For all US counties, I construct a panel of population-adjusted monthly deaths caused by firearm homicide, firearm suicide, firearm accidents, and firearm deaths where the intent is unknown. The number of non-firearm homicides is also used, as a placebo. To remain consistent with the crime analysis, these data are aggregated as deaths per 100,000 residents instead of the per million residents that is standard in the public health literature. Table 5 shows the control and treatment states in the all county sample and the number of counties in each state that are part of the panel. Table 6 shows the treatment states that are in the border county sample, the number of pairs in them, the control states with whom they share a border, and the number of pairs of border counties the treatment and control states have. For example, Arizona has a total of 22 border county pairs. It's border with California generates 5 pairs, it's border with Colorado generates 1 pair and so on. Note that each county can be part of more than one pair.

Table 7 shows summary statistics for mortality in the pre-treatment year 2010, and also compares the level of firearm mortality in the treatment and control counties. For the all county sample I use a two-sample t-test, and for the border county sample I use a paired t-test. For both samples the differences between the level of firearm homicide in control and treatment counties are statistically insignificant.

Suicides committed with a firearm are used as a proxy for firearm ownership. This follows the literature established by researchers such as Moody and Marvell (2005), Cook and Ludwig (2006), and T. Kovandzic, Schaffer, and Kleck (2013). For the border county sample, the differences between firearm suicides in the control and treatment counties are statistically insignificant. I observe a statistically significant difference in the level of firearm suicides in the all-county sample, suggesting that the treatment states tend to have higher levels of gun ownership than the control states. This is one of the reasons I eschew a spatial regression discontinuity approach in favor of a difference-in-difference approach.

4.2 Data on Crime

Data on crime come from the National Incident Based Reporting System (NIBRS), which collects incident-level data on violent (and some non-violent) crimes that occur in a law-enforcement agency's jurisdiction. Law enforcement agencies generate NIBRS incident data via their own record management systems, which relay the data to the Federal Bureau of Investigation who makes the data publicly accessible. The data include the incident date, time, offenses committed, whether these offenses were completed or attempted, the type of location (restaurant, residence etc), and the type of weapon used. Of particular importance to this project is that the data records the demographic characteristics of the both the victim and offender - including age, ethnicity, gender - and what relationship (if any) they had to each other.

It should be noted that the researchers studying shall-issue concealed carry (SICC) regimes did not have access to incident-level data, because relatively few police departments were part of the NIBRS system at the time states transitioned into SICC regimes. Researchers relied on the Summary Reporting System in which agencies reported their crime statistics to state agencies, which then conveyed them to the FBI.[26] Maltz and Targonski (2002) point out that these data are not appropriate for county-level analysis, since the FBI often imputed data when it was not available. Aggregating incident data myself using the NIBRS allows me to avoid this problem. Furthermore, I can aggregate the data at

[26] See, for example, Lott and Mustard (1997), Black and Nagin (1998) , Ayres and J. J. Donohue (2003), Maltz and Targonski (2002), and Helland and Tabarrok (2004)

the level of the month, an option not available to researchers of the shall-issue laws, who relied on annual data.

Crimes in a county are tabulated by the number of incidents of the crime per month, per 100,000 residents. The crimes that the FBI categorizes as violent crimes - homicide, sexual assault, aggravated assault, and robberies - are also the crimes most frequently studied in the literature on gun control.[27] I also look at burglaries (also known as breaking and entering) and thefts. A placebo crime variable is constructed by adding up the number of incidents of crimes that are plausibly unaffected by UCC laws. These include, and are limited to, blackmail, counterfeiting, fraud, embezzlement, pornography, gambling, sex work, bribery, bad checks, loitering, vagrancy, and driving under the influence.

The NIBRS data is tabulated from 2008-2016. 2016 is the last year for which the data was available at the time of my analysis. The further back one goes, the fewer law enforcement agencies participate in the NIBRS, and so I chose 2008 as an (unavoidably arbitrary) cutoff. Table 4 shows the states that are in the border-county sample, and Table 3 shows the states that are in the all-county sample. Figure 2 maps the counties that are in our border county sample. The NIBRS also includes data on the arrests made by law enforcement officers. These data include the demographic characteristics of the arrestee, a cross-reference to the crime committed, and information about whether or not the arrestee was armed and, if so, with what. This is true for all arrests, not just those made for violent or gun related crimes; a person arrested for driving under the influence of alcohol will be recorded as being armed if they are carrying a handgun, even if that handgun was legal.

A county-month panel dataset was constructed with 15,600 observations for 182 pairs of border counties spread across 5 treatment states and 14 control states, tracking the number of incidents of crime (per 100,000 residents) for the time period 2008-16. Due to limitations on the availability of data, this does not include all possible border county pairs. Figure 2 shows the counties that are included in the paired border county sample. For the all-county method, a monthly panel data was constructed for 1740 counties spread over 5 treatment states and 35 control states for the period 2008-2016. When deploying the border county method, the data has a total of 160,000 observations.

I use the number of arrests as a proxy for day-to-day usage of guns. In this, I am following Niekamp (2018), who found that gun usage sharply rose at the beginning of hunting seasons in US states using this measure. Since the population of people being arrested is likely dissimilar to the general population, this should not be though of as a precise measure of the general level of day-to-day gun usage, but only as a proxy useful for measuring changes in usage.

[27]See, for example, the first paper on concealed carry written by an economist, Lott and Mustard (1997)

4.3 Miscellaneous Data

This section discusses the data used to determine balance in the border county panels.

4.3.1 Law enforcement agency characteristics

Data on Law Enforcement Organizations come from the Law Enforcement Management and Administrative Statistics (LEMAS) survey. These data are used to check the similarity of law enforcement in treatment counties and their paired control counties. They include responsibilities, expenses, salaries and special pay, the demographic characteristics of their officers, and so on. The survey is conducted irregularly by the Department of Justice. I use the 2013 survey, which is in the pre-treatment period for all treatment counties, except those in Arkansas.

4.3.2 County characteristics

Data on county characteristics are from the US Census Bureau's Statistical Compendia program, and are used to check the similarity of border counties along demographic characteristics. While the program is now defunct, it was active in the pre-treatment period of this paper's analysis. The fraction of adults with at least a high school education is used as a measure of education. The fraction of voters voting for the Republican party (in the 2008 election) is used as a measure of voting patterns. Other measures include the headcount ratio to measure poverty, median individual income as a measure of income, people per square mile as a measure of density, and the unemployment rate.

5 Empirical Strategy

In this section, I discuss my identification strategy, show that the panels are balanced, and address issues relating to the validity of the methodology.

5.1 Border County Difference-in-Difference

For the crime data, I construct a county-by-month panel dataset for the time period 2008-16, match neighboring border counties to each other, and then construct a variable

$$Y'_{csc's'my} = Y_{csmy} - Y_{c's'my}$$

where Y_{csmy} is the outcome Y for county c in treatment state s in month m and year y. $Y_{c's'my}$ is the outcome Y for adjacent county c' in control state s' such that $s \neq s'$ and s' has never had concealed carry over this period.

I then estimate

$$Y'_{csc's'my} = \alpha + \beta D_{smy} + \lambda_{csc's'} + \lambda_{my} + \lambda_{sm} + u_{csmy} \tag{1}$$

where $Y'_{csc's'my}$ is the difference between the outcome of interest in a county c in treatment state s and c' in control state s' where c' is contiguous to c. D_{smy} is a policy indicator that takes the value 1 if the policy is active in state s in month-year my. $\lambda_{csc's'}$ is a full set of county pair fixed effects, while λ_{my} is a full set of time period fixed effects. λ_{sm} is a set of state-specific month-wise fixed effects, meant to capture seasonal effects. Note that each pair of counties is treated as if it were a separate geographical unit, so if a county c in state s neighbors two contiguous counties c^1 and c^2 in state s', they will be a part of two border pairs: csc^1s' and csc^2s'. Standard errors are clustered by treatment state and use wild cluster bootstrapping to estimate confidence intervals.

The coefficient on the policy dummy is interpreted as the change in crime in the treatment county relative to it's neighbor. A positive coefficient indicates an increase in crime or mortality, while a negative coefficient indicates a decrease in the same.

The crime data has 15,663 units spread over 5 treatment states and 182 pairs. Table 4 shows the treatment and control states in the crime sample, and the number of border county pairs corresponding to each state. 2016 is the last year for which data is available. 2008 precedes the earliest instance of legalization by three years. Participation in the NIBRS has risen over time, an implication of which is that the further one goes back in the dataset, the fewer agencies are a part of it. I judged 2008 to be a reasonable cut-off point.

The mortality data is a sample of 488 border county pairs spread over 12 treatment states and 26 control states, for the years 2008-2015[28], for a total of approximately 52,500 observations. Table 6 shows the treatment and control states in the mortality sample and the number of border county pairs corresponding to each state.

5.2 All County Difference-in-Difference

For the NIBRS data, I construct a county-by-month panel dataset for the time period 2008-16, and then estimate

$$Y_{csrmy} = \alpha + \beta D_{smy} + \lambda_{cs} + \lambda_{my} + \lambda_{rm} + u_{csrmy} \tag{2}$$

where Y_{csrmy} is the variable of interest in county c, state s, climatic region r, month m, and year y. D_{smy} is a binary which takes the value 1 if state s had adopted UCC before month m and year y and is 0 otherwise. λ_{cs} is a full set of county fixed effects, λ_{my} is a full set of month-year fixed effects, and λ_{rm} is a set of region-specific month-wise fixed-effects to capture seasonal variation. Standard errors are clustered by state.

We are interested in the coefficient on D_{smy}. A negative coefficient indicates that crime has fallen in treatment states relative to control states, while a positive coefficient would indicate the opposite. The size of the coefficients estimates the change in crime or mortality per 100,000 residents.

Table 3 shows the states in the crime data. There is a total of 160,511 observations

[28]Data for the years 2016-17 have been applied for and will be included in future versions of this paper

spread over 40 states for the years 2008-2016.

Table 5 shows the states in the mortality data. I construct a county-month panel dataset for 3,102 counties spread over 12 treatment states and 39 control states for the years 2008-2015, for a total of approximately 372,000 observations.

5.3 Validity

In this section, I check that the panel dataset is suitable for the difference-in-difference analysis.

5.3.1 Parallel Trends

It is important that trends in mortality and crime before the implementation of a policy are similar between control and treatment groups in the border counties sample. To that end, I estimate the following equation:

$$Y'_{csc's'my} = \alpha + \sum_{j=-24}^{24} \beta_j D_{smy}(my = k + j) + \lambda_{csc's'} + \lambda_{my} + \lambda_{sm} + u_{csc's'my} \tag{3}$$

where $Y'_{csc's'my}$ is the difference between outcomes in a county c in treatment state s and its neighbor c' in control state s' during month m of year y. Here, k is the month and year at which a policy actually goes into effect. $D_{smy}(my = k + j)$ is a set of policy indicators that take the value 1 if the UCC has been adopted in state s in month m and year y with lags and leads. There are 24 policy indicators that lead the treatment effect, and 24 indicators that lag behind it. $\lambda_{csc's'}$ is a full set of county fixed effects, while λ_{my} is a full set of time period fixed effects. λ_{sm} is a full set of state-month fixed effects meant to control for seasonal variation in crime. For the parallel trends assumption to be considered valid, $\beta_j = 0$ for all $j < 0$. If the impact of the law were neutral, $\beta_j = 0$ for all $j \geq 0$ as well.

For the all county sample, I estimate

$$Y_{csrmy} = \alpha + \sum_{j=-24}^{24} \beta_j D_{smy}(my = k + j) + \lambda_{cs} + \lambda_{my} + \lambda_{rm} + u_{csrmy} \tag{4}$$

where Y_{csrmy} is the variable of interest in county c in state s in region r for month m in year y. k is the month and year at which a policy actually goes into effect. $D_{smy}(my = k+j)$ is a set of policy indicators that take the value 1 if the UCC has been adopted in state s in month m and year y with lags and leads. There are 24 policy indicators that lead the treatment effect, 24 policy indicators that lag behind it. λ_{cs} is a full set of county fixed effects, while λ_{my} is a full set of time period fixed effects. λ_{rm} is a full set of region-month fixed effects meant to control for seasonal variation in crime. For the parallel trends assumption to be considered valid, $\beta_j = 0$ for all $j < 0$. If the impact of the law were neutral, $\beta_j = 0$ for all $j \geq 0$ as well.

The parallel trends assumption holds for the NIBRS crime data. Figure 3 plots the β_j

coefficients of border county Equation 3, and Figure 4 plots the β_j coefficients of all counties Equation 4. For homicide, rape, aggravated assault, and robbery, these coefficients are close to zero from month to month, and are statistically insignificant.

The parallel trends assumption also holds for the MCOD mortality data. Figure 5 plots the β_j coefficients of border county Equation 3, and Figure 6 plots the β_j coefficients of all counties Equation 4.

5.3.2 Balance

I check that the panel dataset is balanced along various dimensions by comparing pre-treatment levels of demographic characteristics, geographical characteristics, crime, and mortality in the treatment and control counties. The paired two-sided t-test is used to perform this comparison for the border county sample, and a regular two-sided t-test for the all-county sample.

Table 10 compares the county characteristics of control and treatment counties in the border county sample in 2010, before the implementation of UCC. It shows the results of a paired t-test done on various measures of interests. All figures are normalized for population. The treatment and control counties have similar levels of sworn and non-sworn personnel, full-time and part-time personnel, similar operating budgets, and equipment. Furthermore, treatment and control counties have similar high school graduation rates, Republican vote shares, unemployment, poverty, and population density. They differ in following respects: median income is lower in treatment counties (38k vs 40k), and the racial composition skews more white (89% vs 85%).

Data from the NIBRS is used to average monthly incidence of crimes (normalized for population) between treatment and control counties. Table 9 shows that treatment and control counties have similar pre-treatment levels of homicide, sexual assault, and robbery, though treatment counties have significantly more aggravated assault. For this reason, I eschew a spatial discontinuity approach in favor of a difference-in-difference approach that requires similar pre-treatment trends, but not levels, in the outcomes of interest.

Table 7 compares 2010 mortality in the control and treatment counties. In the all county sample, there are similar levels of pre-treatment homicide, but significantly more firearm suicides, suggesting that the treatment states have similar levels of crime, but higher levels of initial gun ownership. However, this problem does not exist in the border counties sample, where control and treatment counties show similar initial levels of both firearm homicides and firearm suicides, suggesting that the border county panel is better balanced than the all county panel.

5.3.3 Power

As most of the resulting coefficients are statistically insignificant, I try to show that there is a well-identified zero instead of a sample size that is too small to detect an effect. I follow Bloom (1995) and report the minimum detectable effect (MDE) for each estimate. I set $\alpha = 0.05$ and $\beta = 0.2$. The minimum detectable effects are discussed using the following, unavoidably arbitrary, terminology: 1-10% are small effects, 10-25% are modest effects, and over 25% are large effects. The MDE will be reported along with the estimates. Ideally, the sample should have sufficient power to rule out even small effects, though that is rarely the case.

6 Results

In this section I discuss the impact of UCC on firearm mortality, violent crime, gun usage, and gun ownership. Special attention is paid to heterogeneity - the impact of this policy on African-Americans and women.

6.1 Impact on Homicide

Homicide is an important measure for many reasons. First, it is a serious crime, and carries steep legal penalties. Second, it is important to the discourse on firearm regulation; gun rights activists and gun control activists discuss it at length, though they have differing beliefs about the direction of the impact of firearms. Third, it is a reliable indicator of the level of crime in a jurisdiction. Police departments sometimes attempt to improve their statistics by downplaying serious crimes e.g. downgrading aggravated assault to simple assault. It is difficult to do this for homicide.

The most complete source of data on homicide is the mortality data drawn from the universe of death certificates. My preferred specification is Equation 1, which analyzes the sample of paired border counties, which are demographically very similar to each other. Panel A of Table 11 presents the coefficient estimates. The increase in the incidence of firearm homicide of only 0.15%, which is both statistically and economically insignificant. The small coefficient estimate strongly supports the neutrality hypothesis.

Panel B shows the estimates of Equation 2, which is a difference-in-difference analysis of firearm mortality for all US counties. It shows a statistically insignificant 5.4% increase in firearm homicides in the all-county sample, consistent with neutrality.

Perhaps the results are being confounded by an overall change in criminal law, rather than merely gun laws? To check that this is not the case, I apply the above methodology to non-firearm homicides. The coefficient estimates show a statistically insignificant decrease of 5%, which supports neutrality.

Turning to the crime data, Panel A and Panel B of Table 12 shows the estimates of Equation 2 - the all country difference-in-difference analysis - for violent crimes. Column

1 shows the coefficients of the policy dummy for this equation. Unrestricted concealed carry had little to no effect on homicides. Looking at all homicides, there is a statistically insignificant coefficient of -0.015, corresponding to a decline of 6.2%. Results for firearm homicides are starker: the coefficient is -0.003, corresponding to a decline of only 2.3%. These estimates mirror the mortality estimates.

Panel C and Panel D of Table 12 shows the estimates of Equation 1 for violent crimes. Column 1 shows the coefficients of the policy dummy for this equation. Once again there is a statistically insignificant decrease in the incidence of homicides, consistent with neutrality.

A note of caution is in order when discussing homicide in the crime data. Homicides are relatively rare, and the number of counties covered by the crime data is significantly lower than that covered by the mortality data. As a result, the all county analysis has an MDE of 23.91%, just short of the 25% threshold for large effects. Thus, small effects cannot be ruled out, based on the crime data alone. These shortcomings are even more pronounced when it comes to the border county data where the MDE exceeds 50%. Analysis of the homicide incident data must be considered in tandem with the mortality data, as well as the incidence of the other crimes.

6.2 Impact on Other Violent Crimes

Next, consider the impact of deregulating concealed carry on violent crimes that do not result in a fatality. My conclusions are consistent with the results for homicide - deregulating concealed carry does not seem to reduce the incidence of violent crimes such as sexual assault, aggravated assault, or robbery. This is true even if the analysis were to be restricted to crimes committed with a firearm.

Sexual assault is analyzed in Column 2 of Table 12. The estimates show that, after the adoption of concealed carry, sexual assault declined a statistically insignificant 10% in the all county sample. The border county regression estimate finds a decrease of 3% which is also statistically insignificant. The all-county sample has an MDE of 5.9% while the border county sample has an MDE of 8.7%, indicating that I have the power to detect modest effects. Deregulating concealed carry doesn't seem to discourage rapists, nor does it embolden them.

A glance at our summary statistics reveals this to be unsurprising - less than 1% of sexual assaults are committed with a firearm. Since such incidents are so rare, the coefficients for the crime are noisy, with MDE of 67% and 83% in the border county and all-county panels respectively. Thus, the coefficients are large, but statistically insignificant in the all-county panel and only borderline significant in the border county panel.

Aggravated assault refers to either a physical altercation in which serious injuries are sustained, or in which a weapon is brandished. Column 3 of Table 12 shows that the adoption of UCC resulted in a statistically insignificant 6% decrease in overall aggravated assault in the all-county sample. There is sufficient power to rule out this modest change, as the data have an MDE of 4.36%. When I restrict the analysis to only those incidents that involve firearms, the estimate shows a 14% *increase* in aggravated assault, which is also statistically insignificant, and larger than the MDE of 8.35%.

The directions of these coefficient estimates are mirrored in the border county regressions, which yield a statistically insignificant decrease in aggravated assaults coupled with a statistically insignificant increase in aggravated assaults committed with a firearm. The coefficient estimate is 30%, which is larger than our MDE of 8.06%. The coefficient estimate for firearm-related aggravated assaults in border counties is 30% which is larger than our MDE of 16.3%.

Robberies are analyzed in Column 4 of Table 12. In the border counties sample, there is statistically insignificant increase in overall robberies. The size of this coefficient corresponds to a 20% increase in robberies, which is greater than the MDE of 9.6%. In the all-county sample, we see a statistically and economically insignificant 0.9% increase in robberies. Restricting the analysis to robberies with firearms, I see an economically more significant 12% increase that is, nonetheless, statistically insignificant, and the MDE is 14%.

All of these results, taken together, support neutrality - deregulating concealed carry does not lead to any detectable change in the level of violent crime.

6.3 Impact on Women and African Americans

Consider next crimes against women and African-Americans. Here, no restriction has been placed on the gender or race of the perpetrator. This analysis is, to the best of my knowledge, unprecedented in the gun control literature, and is enabled by access to fine-grained data that previous researchers did not have access to. Table 13 shows the estimates for Equations 1 and 2 .

As discussed earlier, there are those who argue that women benefit from the deregulation of gun control, as it empowers them in physical contests. Panel A1 shows the estimators for Equation 1 when incidents are limited to those with female victims, while Panel A2 shows the estimators for Equation 2 under the same restrictions. Once again, the analysis generates statistically insignificant estimated coefficients for homicides, sexual assaults, aggravated assaults, and robberies. For both equations, there is sufficient power to detect at least a modest MDE, though unsurprisingly this is lower than in the general analysis, which includes both men and women.

As discussed earlier, African-Americans suffer disproportionately from criminal victimization and policing. Panel B1 shows the estimated coefficients of Equation 1 when incidents are limited to those with Black victims, while Panel B2 shows the estimates for Equation 2 under the same restrictions. The analysis generates statistically insignificant estimated coefficients for homicides, sexual assaults, and aggravated assaults. There appears to be a statistically significant increase in robberies committed against African-Americans in the all-county sample, though not in the border county sample. African-Americans constitute only 13% of the US population, and thus the analysis has significantly less power when it is restricted, leading to higher MDE than in the general case.

6.4 Impact on Rural and Urban Areas

Residents of rural areas are more likely to own a firearm, and less likely to be the victim of a crime. It is entirely possible for rural and urban areas to be affected differently by firearm and criminal legislation.

I classify incidents of crime as being committed in a rural area if they are recorded by a county sheriff department, and as being committed in an urban area if they are recorded by a municipal police department. (State and federal organizations have been excluded from this paper.) Equation 1 and Equation 2 are estimated while restricting the analysis to crimes committed in these areas.

Table 14 presents the coefficient estimates of this analysis. Consistent with the neutrality hypothesis, the coefficient estimates are statistically insignificant. An exception is aggravated assault, which is negative and statistically significant in the all county urban sample, but negative and only borderline statistically significant in the border county sample. For all of these crimes, there is sufficient power to rule out modest effects if they existed, with the unsurprising exception of homicide, a high-variance crime.

6.5 Impact on Other Crimes

A concern in the literature on law and economics is that criminals may respond to increased penalties for crime A by switching to crime B, leaving the overall level of crime unchanged. Consider, for example, burglary. A criminal may prefer to be a burglar rather than a mugger if their potential victims are completely disarmed. If their victims can have a firearm at home but not in public, the criminal may opt to be a mugger instead, since their victims are likely to be disarmed. If concealed carry in public is liberalized as well, then burglary becomes relatively more attractive again.

This does not appear to be the case for concealed carry. Table 15 shows the impact of UCC on burglary and theft. The coefficient for the policy dummy is negative and statistically insignificant for each crime in the border county sample as well as the all county sample. For each crime, there is sufficient power to rule out modest changes in the incidence of these crimes.

7 Is the Policy Neutral, or Are Effects Offsetting Each Other?

It is possible that the adoption of UCC would lead to both criminals and non-criminals acquiring firearms, with the two effects cancelling each other out. If this theory is true, there should be an increase in ownership and usage, even in the absence of an increase or decrease in crime. In this section, I test the possibility that effects are offsetting each other by looking at the impact of UCC on gun ownership rates and day-to-day gun usage.

Gun suicides are a commonly used proxy for gun ownership, because people who wish to kill themselves and own one or more firearms are likely to use a gun for that purpose. Column 3 of Table 11 shows the impact of the legalization of UCC on firearm suicide in the mortality data. The coefficient estimates are negative and statistically insignificant in both the border county sample and the all-county sample, suggesting that UCC did not lead to more people acquiring guns for the purpose of carrying them.

Police arrest reports note whether arrestees were armed at the time of arrest, even if they were not being arrested for a violent or firearm-related crime. I use this variable - constructed with the NIBRS data - as a proxy for day-to-day gun usage. Table 16 shows the impact of UCC on the number of armed arrestees. The coefficient estimates are statistically insignificant and have opposite signs, with the border county estimate showing a 10% decrease and the all county estimate showing a 5% increase. In both cases, there is insufficient power to rule out these modest effect sizes.

Overall, the data on suicides and arrests do not support the theory that UCC led to an increase in gun usage by criminals and law abiding citizens alike, with these effects canceling each other out. This, in turn, lends weight to the theory that neither group was waiting on the state government to deregulate concealed carry.

8 Other Robustness tests

In this section I test the robustness of the conclusions drawn to alternative specifications.

8.1 The Entry of Law Enforcement Organizations into the Data

The NIBRS is a relatively recent program, and has expanded considerably over the period of time covered by this analysis. The expansion allows me to conduct analysis that previous researchers were not able to do, but carries a drawback: if agencies that joined late had relatively fewer incidents than the average agency that could bias the coefficient estimates downwards. Alternatively, if such agencies had more incidents than the average, it could bias the coefficient estimates upwards.

To test this possibility, I restrict the analysis to agencies that were present for the entirety of the period 2008-16. These results are presented in Table 17. Consistent with neutrality, the coefficient estimates are statistically insignificant, negative for three of the crimes and positive for the fourth.

8.2 Spillovers

It is possible that criminals in border counties, now faced with invisibly armed residents, will travel to neighboring counties without UCC regimes to commit crimes. Thus, crime would rise in the neighboring county, leading to erroneous conclusions being drawn from the analysis, which focuses on the difference between crime in treatment and control counties. Specifically, the coefficient estimates will be biased downwards.

To test this possibility, I estimate

$$Y_{c's't} = \alpha + \beta D_{st} + \lambda_{c's'} + \lambda_t + u_{c's't}$$

where $Y_{c's't}$ is the outcome of interest in control county c' in state s'. D_{st} is a policy indicator that takes the value 1 if the policy is active in state s at time period t. $\lambda_{c's'}$ is a full set of county fixed effects, while λ_t is a full set of time period fixed effects. As before, standard errors are estimated using the wild cluster bootstrapping method.

Table 18 shows the results of this analysis. The adoption of UCC in treatment states had no impact on trends in crime in the control counties that border them.

8.3 Placebo Tests

It is possible that such laws are adopted along with other changes to policing or the judicial system - ones that negate the non-zero impact of this law - resulting in misleading conclusions being drawn from difference in difference analysis. To test this theory I construct a placebo variable consisting of crimes that are plausibly unaffected by gun control measures, including and limited to blackmail, counterfeiting, fraud, embezzlement, pornography, gambling, sex work, bribery, bad checks, loitering, vagrancy, and driving under the influence. The impact of legalizing unrestricted concealed carry on this placebo variable is then analyzed using the same tools as before.

Table 15 shows the impact of UCC on this placebo variable. In the all county sample, there is a 13% decline significant at the 5% level. However, in the border counties sample, there is no statistically significant decline. Table 11 shows that UCC did not lead to a decline in non-firearm homicides, which would plausibly be affected by changes to the legal system. There exists sufficient power to rule out modest changes in this composite variable.

8.4 Synthetic Controls

A shortcoming of the data is that most of the states that deregulated concealed carry did so quite recently, creating a relatively brief post-treatment window. It is possible that the law does not have an immediate impact but does have some sort of long term impact.

An exception to this is Arkansas, which legalized unrestricted concealed carry in 2013 and therefore has a relatively long post-treatment period. However, it is but one state, and it would be unwise to draw any strong conclusions from it. To address this problem, I follow Abadie, Diamond, and Hainmueller (2010) in applying the synthetic control method to this problem. I construct a state by applying weights to a control group of states such that the resultant "state" is similar to Arkansas along various demographic dimensions. I then check that there was no divergence between these two "states" after the implementation of unrestricted concealed carry.

After excluding the other treatment states in the data, a synthetic control for Arkansas was created using pre-intervention state-level demographic data to assign weights to the remaining US states, including the fraction of Whites, median income, high school graduation rates, Republican vote share, the unemployment rate, and the poverty rate. The analysis was performed using the `synth` program in STATA.

Figure 7 shows the impact of the unrestricted concealed carry in Arkansas vs it's synthetic control. Before and after the implementation of the policy, trends in both "states" closely matched each other, which supports neutrality.

9 Discussion

Proponents of concealed carry argue that it will decrease crime as it would lead to more law-abiding citizens carrying firearms. Opponents argue that it will increase crime, as it would lead to more criminals carry firearms. However, the data contradicts both of these theories, and supports the idea that the law had no impact at all. Part of this conclusion comes from specific results, such as the 0.15% increase in homicides that was estimated using the death certificate data. Part of the conclusion comes from looking at the evidence as a whole: in specification after specification the coefficient estimates are statistically insignificant, sometimes positive and sometimes negative. The analysis shows that that UCC has no effect on homicide, sexual assault, aggravated assault, or robbery. This is true for the overall sample, women and African-Americans, for rural and urban areas, for non-violent crimes, and for placebo crimes.

Of course, this alone does not imply that the law had no impact - it could have led to criminals and law-abiding citizens carrying more firearms but these two effects cancelling each other out. However, the data shows that UCC has no effect on gun ownership, using suicides as a proxy, or on day-to-day usage, using arrestee reports as a proxy. These conclusions contradict the "offsetting" theory. This leaves us with only one theory: that the law did not influence people's decision to carry concealed firearms, and so had no effect - positive or negative - on crime. In other words, the analysis supports neutrality.

Perhaps this should not come as a surprise. A concealed firearm - by definition - is difficult to spot, and it is unlikely that a person would be stopped and searched. There is little reason for someone to *not* be armed should they so wish, whether they had malicious intent or not. This paper analyzes the transition from permit-based concealed carry to unrestricted concealed carry. The impact of such laws comes not from a previously-forbidden action becoming legal, but from a change to costs. It is possible that the costs of acquiring a permit were not prohibitive, so law-abiding citizens were not particularly empowered by this law. Criminals likely banked on not being stopped and searched before they committed their crime, and simply continued their previous behavior.

None of this is to say that concealed carry is harmless or completely lacking in impact. All states studied in this paper transitioned from shall-issue to unrestricted regimes. It is possible that *this* transition is harmless while the transition from may-issue to shall-issue is harmful (or beneficial, or harmless.) Nonetheless, the neutrality of UCC is consistent with the recent literature or shall-issue that finds that the impact of these regimes is ambiguous.

That said, the neutrality of UCC offers three lessons.

First, UCC is not a successful crime-fighting tool. This is unfortunate, as it requires no taxation or public funding, and would have been a cost-effective tool had it succeeded. Policy-makers who wish to reduce the incidence of violent crime will need to look elsewhere.

Second, UCC is not a dangerous form of deregulation. This is fortunate, as a third of US states have transitioned into UCC regimes and it is comforting to know that the residents of these states have not been subject to increased violence as a result.

Third, the enforceability of laws should be a major concern while drafting gun control legislation or prioritizing gun control activism. Laws governing the carry of weapons may be impractical, and so activists may wish to turn their attention to other measures such as universal background checks, or legislation that ensures that people with domestic violence records are restricted from purchasing firearms via the NICS. Siegel et al. (2019) provides a overview of the effect of gun control measures affect mortality, and is an excellent resource for gun control activists.

10 Conclusion

This paper hits many firsts in the economic analysis of gun control policy. It is the first to use a border county difference-in-difference approach to identify the impact of such a policy. It is the first to use granular death certificate data, and the first to extend this analysis to marginalized groups such as women and African-Americans. It is also one of the few to comprehensively analyze this relatively new gun control policy, as an increasing number of states adopt it.

Overall, it appears that few people, criminal or not, wait on the state to deregulate concealed firearms to carry one, as evidenced by the lack of change in day-to-day gun usage or ownership. This lack of change in behavior results in the policy neither increasing nor decreasing the incidence of violent crimes. This conclusion is quite robust - it applies to homicide, aggravated assault, sexual assault, robbery, theft, and burglary. It does not change when I look at demographic subgroup such as women, African-Americans, city-dwellers, or the residents of rural areas.

Perhaps this conclusion should not come as a surprise. Criminals are, by definition, the group least likely to constricted by the law. Conscientious citizens who wish to carry a firearm probably feel the urge strongly enough that they will acquire whatever permits are necessary. Less-than-conscientious but non-criminal citizens who do not want to jump through bureaucratic hurdles simply won't - concealed handguns are small, easily secreted, and unlikely to be intercepted.

These conclusions must not be read as support for, or opposition to, concealed carry, let alone firearm deregulation as a whole. All states involved moved from shall-issue to unrestricted regimes, and this analysis tells us nothing about, for example, the impact of moving from may-issue to shall-issue regimes. This paper should be understood as a very deep dive into one specific policy.

Overall, the important contribution of this paper is methodological - to highlight how large datasets and the tools available to modern economists and criminologists can be used to answer important questions related to public policy, especially important and difficult to answer questions such as the impact of a specific gun control policy.

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Appendix: Figures

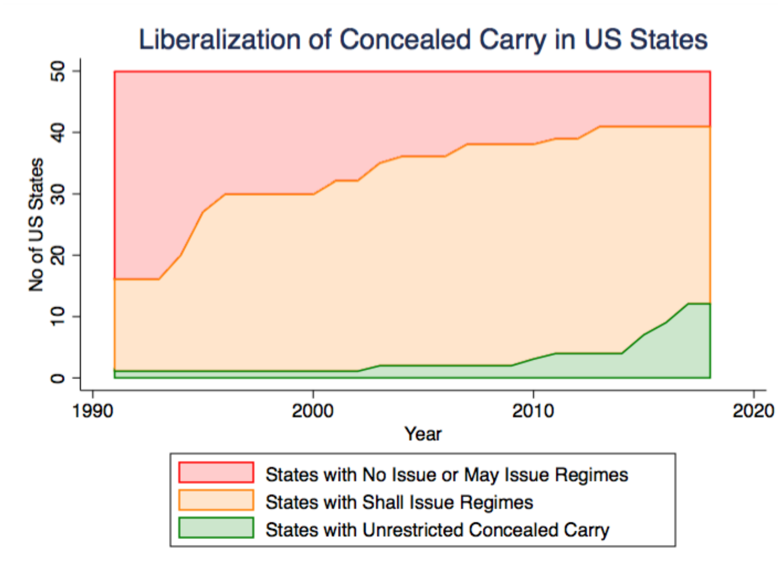


Figure 1: Trends in Concealed Carry

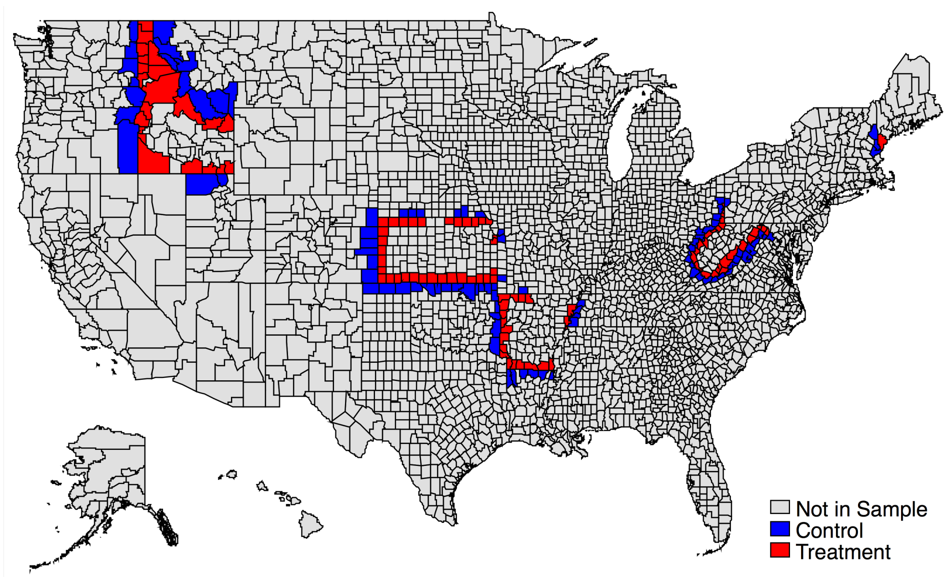


Figure 2: Counties in Border Counties Sample

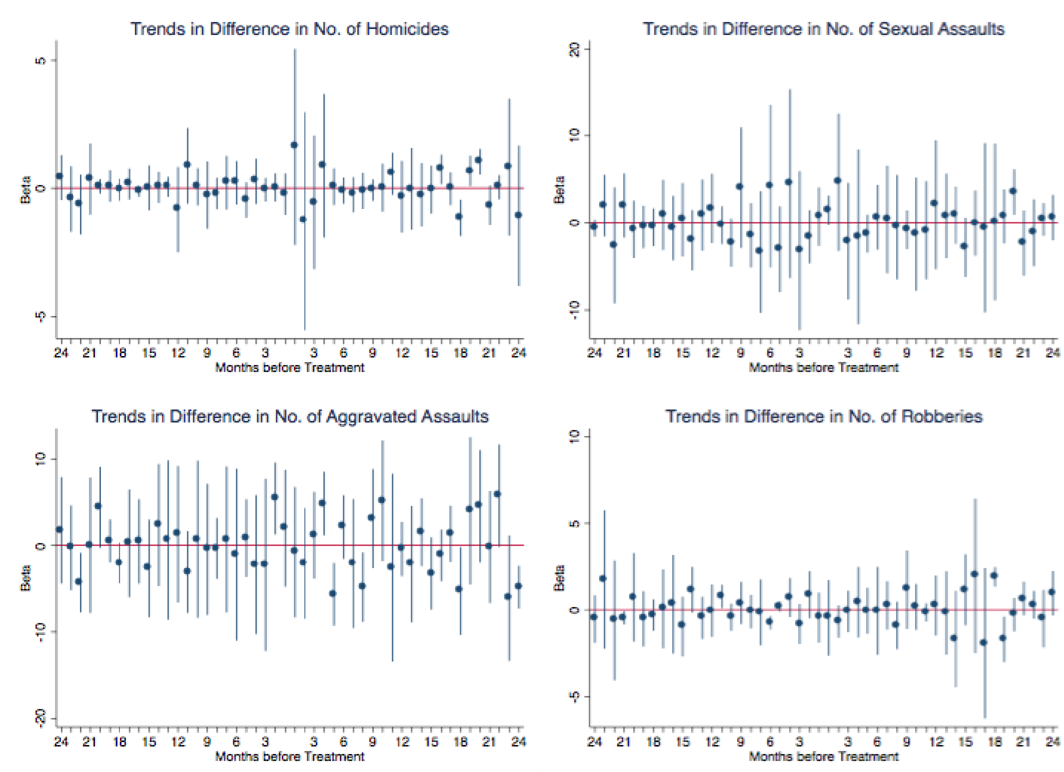


Figure 3: Pretrends in Crime for Border Counties

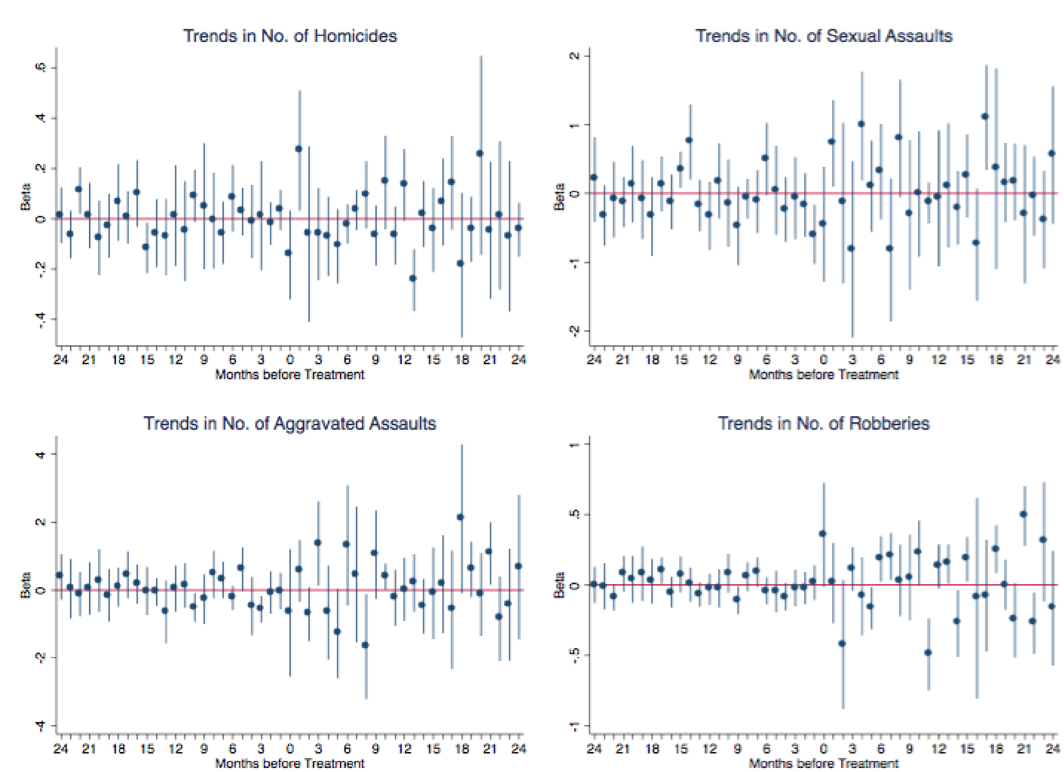


Figure 4: Pretrends in Crime for All Counties

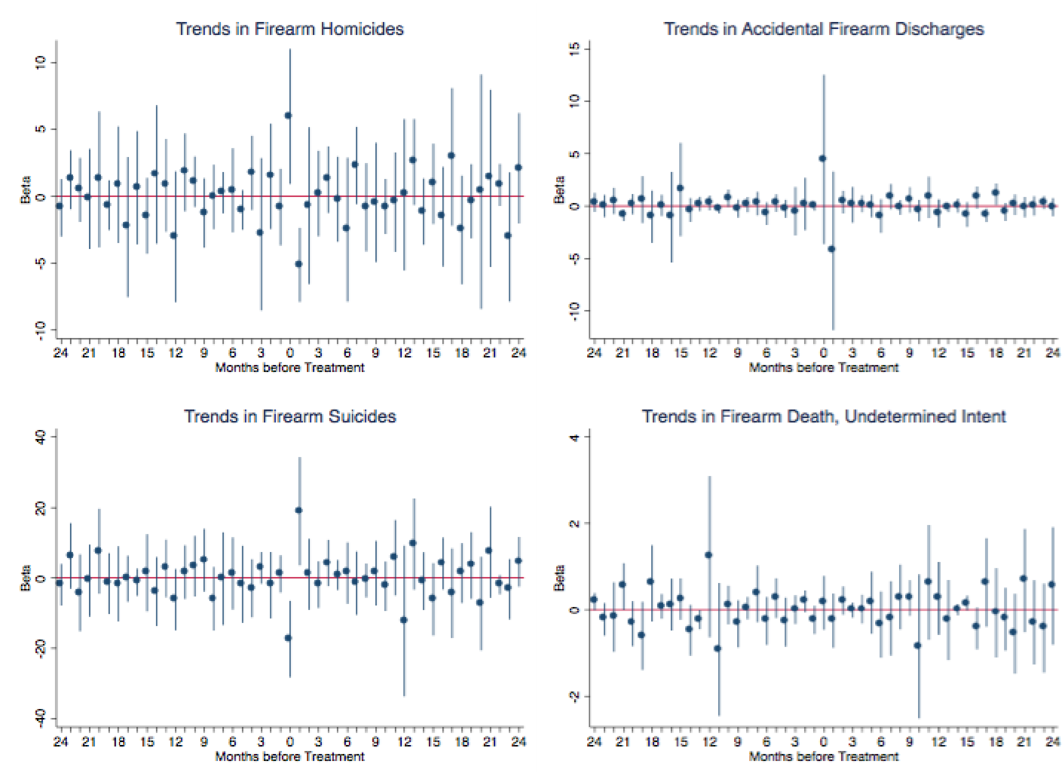


Figure 5: Pretrends in Mortality for Border Countes

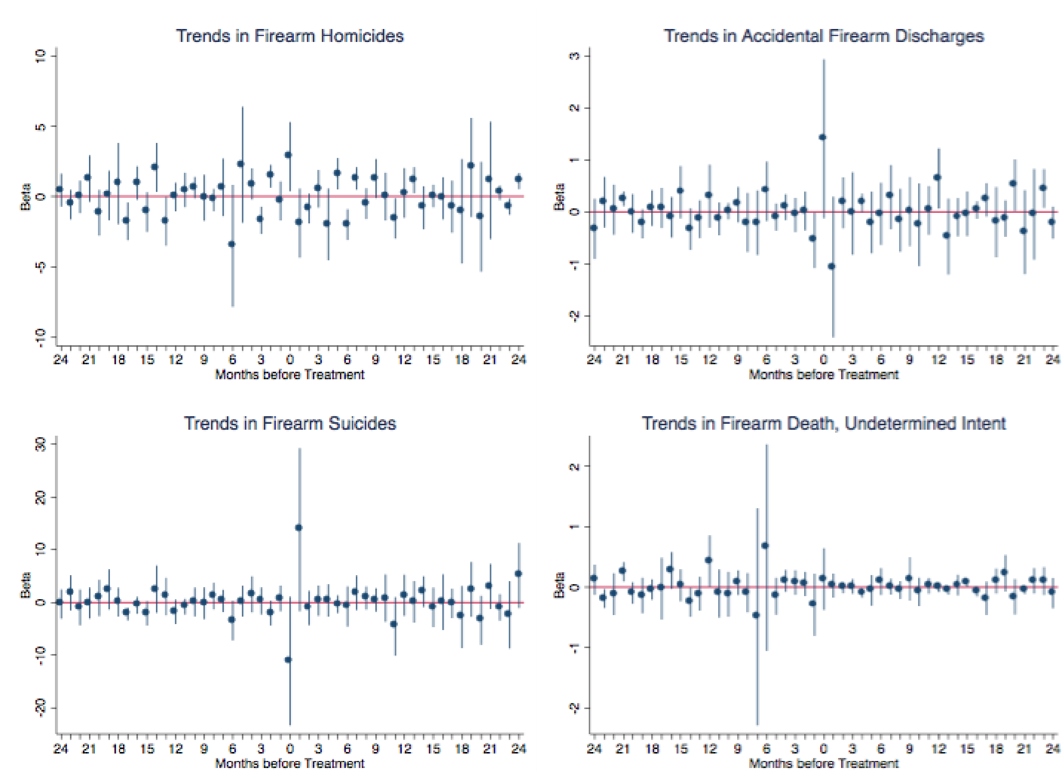
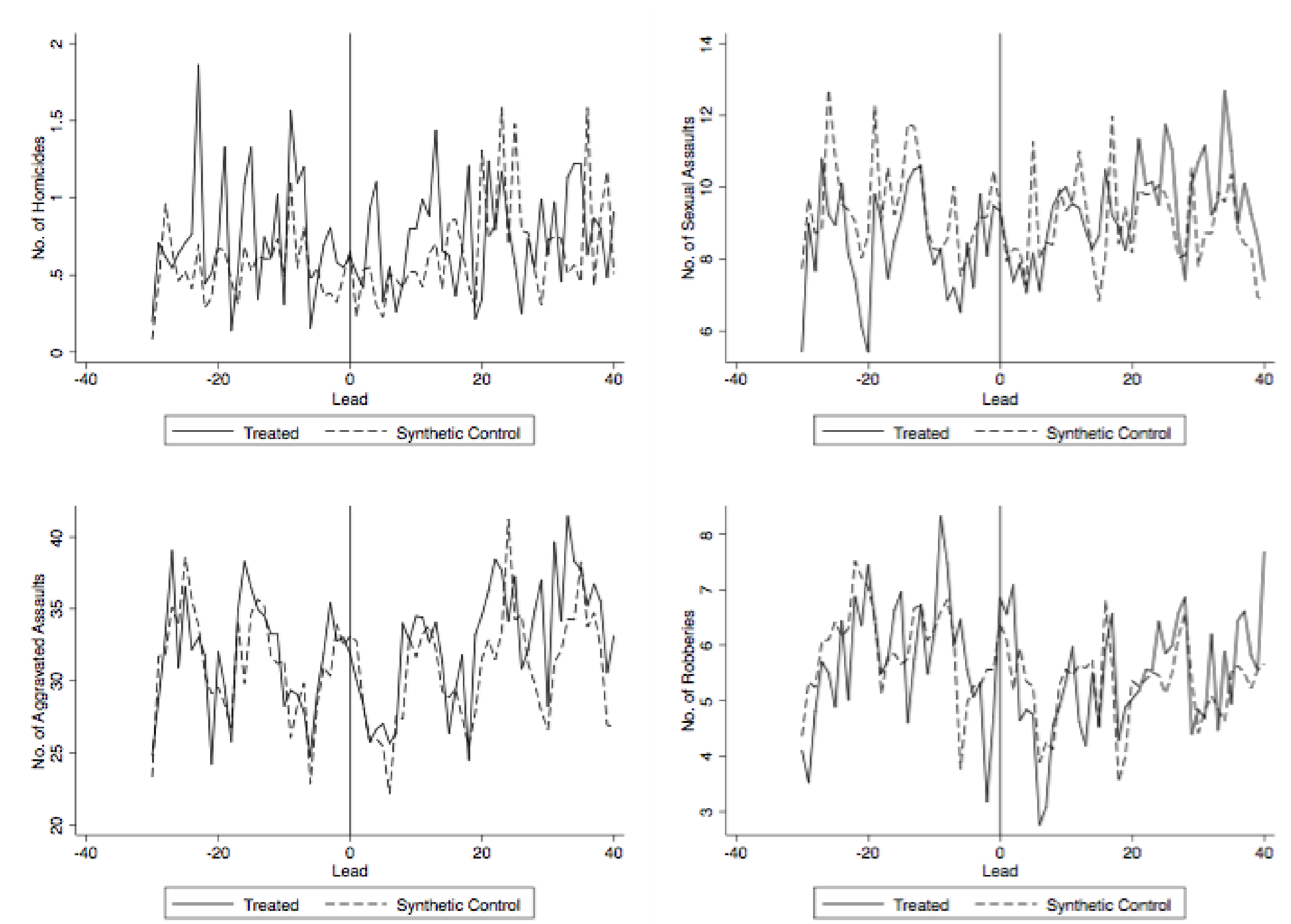


Figure 6: Pretrends in Mortality for All Counties

Figure 7: Arkansas vs Synthetic Control



Appendix: Tables

Table 1: Concealed Carry Regimes in the USA, from Least to Most Permissive

Name	Allows Concealed Carry?	Mandates Issue of Permit?	States
No Issue	No	No	American Samoa, Northern Mariana Islands
May Issue	Yes	No	CA, CT, DE, HI, MA, MD, NJ, PR, RI
Shall Issue	Yes	Yes	AL, CO, DC, FL, GA, Guam, IL, IN, IA, LA, MI, MN, MN, NE, NV, NM, NC, OH, OR, PN, SC, TN, TX, UT, VA, WA, WI
Unrestricted	Yes	N/A	AK, AR, AZ, ID, KS, KY, ME, MO, MS, ND, NH, OK, SD, VT, WV, WY

Table 2: Dates for the Legalization of Unrestricted Concealed Carry

State	Date effective	Note
Vermont	n/a	Ruled a constitutional right by the Vermont State Supreme Court in 1903
Alaska	September 9, 2003	
Arizona	July 1, 2011	
Wyoming	July 1, 2011	
Arkansas	August 16, 2013	
Idaho	July 1, 2015	Legalized outside cities
	July 1, 2016	Legalized within cities
Kansas	July 1, 2015	
Mississippi	July 1, 2015	
Maine	July 1, 2015	
West Virginia	May 24, 2015	
Missouri	January 1, 2017	
New Hampshire	February 22, 2017	
North Dakota	August 1, 2017	
Kentucky	June 26, 2019	
South Dakota	July 1, 2019	
Oklahoma	November 1, 2019	

TABLE 3: STATES IN ALL-COUNTY CRIME SAMPLE

Control		Treatment	
Statename	Counties	Statename	Counties
Alabama	1	Arizona	2
Colorado	38	Arkansas	69
Connecticut	8	Idaho	35
Delaware	3	Kansas	66
District Of Columbia	1	Maine	4
Illinois	1	West Virginia	49
Iowa	71		.
Kentucky	15		.
Louisiana	13		.
Massachusetts	13		.
Michigan	82		.
Montana	31		.
Nebraska	26		.
New Hampshire	10		.
North Dakota	21		.
Ohio	76		.
Oklahoma	43		.
Oregon	13		.
Rhode Island	5		.
South Carolina	46		.
South Dakota	27		.
Tennessee	94		.
Texas	23		.
Utah	12		.
Virginia	131		.
Washington	7		.
Wisconsin	17		.
Total	828	*Total*	225

Notes: Crime incidence data drawn from National Incident Based Reporting System files, maintained by the Federal Bureau of Investigation

Table 4: States in Border County Crime Sample

Treatment	Total	Control	Pairs
Arkansas	32	Louisiana	11
		Missouri	3
		Oklahoma	12
		Tennessee	6
Idaho	38	Montana	15
		Oregon	9
		Utah	5
		Washington	9
Kansas	56	Colorado	12
		Missouri	5
		Nebraska	13
		Oklahoma	26
Maine	3	New Hampshire	3
West Virginia	53	Kentucky	5
		Ohio	20
		Pennsylvania	1
		Virginia	27
Total	182		.

Notes: Crime incidence data drawn from National Incident Based Reporting System files, maintained by the Federal Bureau of Investigation

Table 5: States in All-County Mortality Sample

Control		Treatment	
Statename	Counties	Statename	Counties
Alabama	67	Alaska	25
California	58	Arizona	15
Colorado	62	Arkansas	74
Connecticut	8	Idaho	44
Delaware	2	Kansas	102
District Of Columbia	1	Maine	15
Florida	67	Mississippi	81
Georgia	158	Missouri	115
Hawaii	3	New Hampshire	9
Illinois	102	North Dakota	48
Indiana	91	Vermont	13
Iowa	98	West Virginia	54
Kentucky	119	Wyoming	23
Louisiana	63		.
Maryland	25		.
Massachusetts	13		.
Michigan	82		.
Minnesota	86		.
Montana	52		.
Nebraska	79		.
Nevada	18		.
New Jersey	21		.
New Mexico	32		.
New York	62		.
North Carolina	99		.
Ohio	88		.
Oklahoma	76		.
Oregon	35		.
Pennsylvania	67		.
Rhode Island	4		.
South Carolina	46		.
South Dakota	64		.
Tennessee	94		.
Texas	244		.
Utah	28		.
Virginia	132		.
Washington	38		.
Wisconsin	71		.
Total	2455	*Total*	618

Notes: Mortality data drawn from Multiple Cause of Death files, maintained by the Centers for Disease Control.

Table 6: States in Border County Mortality Sample

Treatment	Total	Control	Pairs
Arizona	22	California	5
		Colorado	1
		Nevada	2
		New Mexico	8
		Utah	6
Arkansas	32	Louisiana	14
		Oklahoma	9
		Tennessee	6
		Texas	3
Idaho	42	Montana	15
		Nevada	4
		Oregon	7
		Utah	7
		Washington	9
Kansas	61	Colorado	11
		Nebraska	26
		Oklahoma	24
Mississippi	54	Alabama	21
		Louisiana	23
		Tennessee	10
Missouri	64	Illinois	27
		Iowa	18
		Kentucky	5
		Montana	4
		Nebraska	4
		Oklahoma	3
		Tennessee	3
New Hampshire	6	Massachusetts	6
North Dakota	35	Minnesota	11
		Montana	8
		South Dakota	16
Vermont	11	Massachusetts	3
		New York	8
West Virginia	71	Kentucky	5
		Maryland	9
		Ohio	20
		Pennsylvania	10
		Virginia	27
Wyoming	37	Colorado	8
		Montana	11
		Nebraska	6
		South Dakota	7
		Utah	5
Total	435		.

Notes: Mortality data drawn from Multiple Cause of Death files, maintained by the Centers for Disease Control.

Table 7: Comparison of Mortality in Treatment and Control Counties, Pre-Treatment

	Treatment		Control		Difference
	Mean	Obs	Mean	Obs	
Border Counties (Paired t-test)					
Firearm Homicides	1.925	5,244	1.969	5,244	-0.045
	(0.150)		(0.228)		(0.270)
Accidental Firearm Discharges	0.282	5,244	0.466	5,244	-0.184
	(0.056)		(0.111)		(0.125)
Firearm Suicides	10.147	5,244	9.404	5,244	0.743
	(0.556)		(0.508)		(0.752)
Firearm Death, Undetermined Intent	0.059	5,244	0.017	5,244	0.042*
	(0.024)		(0.007)		(0.025)
Homicide by Unspecified Means	1.593	5,244	0.822	5,244	0.771***
	(0.184)		(0.080)		(0.197)
All Counties (Two sample t-test)					
Firearm Homicides	1.980	7,512	1.874	29,688	0.106
	(0.132)		(0.067)		(0.148)
Accidental Firearm Discharges	0.388	7,512	0.327	29,688	0.061
	(0.074)		(0.035)		(0.079)
Firearm Suicides	9.519	7,512	7.729	29,688	1.790***
	(0.436)		(0.167)		(0.398)
Firearm Death, Undetermined Intent	0.098	7,512	0.088	29,688	0.010
	(0.032)		(0.014)		(0.032)
Homicide by Unspecified Means	1.114	7,512	0.977	29,688	0.137
	(0.112)		(0.043)		(0.103)

Notes: Crime data drawn from National Incident Based Reporting System, maintained by the Federal Bureau of Investigation. Mortality data drawn from Multiple Cause of Death files, maintained by the Centers for Disease Control. All statistics are for year 2010. The p-values assigned to the difference in the all county sample are drawn from a two-sample ttest. The p-values assigned to the difference in border county sample are drawn from a paired ttest. All figures are per 100,000 deaths. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 8: Comparison of Treatment and Control Counties, Pre-Treatment

	Treatment	Control	Difference	N
Law Enforcement Characteristics				
No. of Full-Time Sworn Personnel	37.853	28.792	9.061	182
	(5.351)	(5.037)	(7.682)	
No. of Unpaid Sworn Reserve Officers	6.818	3.649	3.169*	182
	(1.592)	(0.903)	(1.897)	
No. of Paid Full-Time Nonsworn Personnel	12.892	13.738	-0.846	182
	(2.070)	(3.116)	(3.865)	
No. of Paid Part-Time Nonsworn Personnel	1.755	1.862	-0.107	182
	(0.446)	(0.459)	(0.624)	
Total Operating Budget (in millions)	3.408	3.133	0.275	182
	(0.493)	(0.590)	(0.812)	
No. of Marked Vehicles Operated	21.406	17.241	4.165	182
	(3.026)	(2.865)	(4.334)	
No. of Unmarked Vehicles Operated	7.255	7.439	-0.184	182
	(1.010)	(1.474)	(1.872)	
Pre-treatment County Characteristics				
High School Graduation Rate in County	83.073	83.562	-0.489	182
	(0.481)	(0.497)	(0.437)	
Percent of Votes Cast for Republicans	63.812	62.629	1.184	182
	(0.844)	(0.877)	(0.767)	
Unemployment Rate in County	8.724	8.426	0.298	182
	(0.236)	(0.235)	(0.194)	
Percent of Population Below Poverty	15.104	15.715	-0.611	182
	(0.416)	(0.450)	(0.440)	
Population per Sq Mile	82.482	88.877	-6.395	182
	(11.947)	(13.873)	(11.579)	
Median Income of County	38468.077	40806.852	-2338.775***	182
	(530.799)	(732.314)	(666.551)	
Percentage of County that is White	88.587	85.341	3.246***	182
	(0.893)	(1.070)	(0.746)	

Notes: Law Enforcement Data is drawn from the 2013 Law Enforcement Management and Administrative Statistics (lemas) survey. County characteristic data drawn from the US Census Bureau's Statistical Compendia program. Test of equivalence is paired t-test. Figures are per 100,000 residents. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 9: Comparison of Crime in Treatment and Control Counties, Pre-Treatment

	Treatment		Control		Difference
	Mean	Obs	Mean	Obs	
Border Counties (Paired t-test)					
No. of Homicides	0.118	134	0.154	134	-0.035
	(0.051)		(0.073)		(0.084)
No. of Sexual Assaults	5.303	134	4.704	134	0.599
	(0.523)		(0.553)		(0.754)
No. of Aggravated Assaults	12.515	134	6.068	134	6.447***
	(1.479)		(0.932)		(1.310)
No. of Robberies	2.084	134	1.442	134	0.642
	(0.407)		(0.374)		(0.467)
All Counties (Two sample t-test)					
No. of Homicides	0.197	295	0.255	1,125	-0.058
	(0.060)		(0.041)		(0.085)
No. of Sexual Assaults	5.470	295	6.098	1,125	-0.628
	(0.402)		(0.340)		(0.695)
No. of Aggravated Assaults	11.625	295	8.963	1,125	2.662***
	(0.836)		(0.369)		(0.837)
No. of Robberies	1.462	295	2.336	1,125	-0.874***
	(0.210)		(0.142)		(0.297)

Notes: Crime data drawn from National Incident Based Reporting System, maintained by the Federal Bureau of Investigation. Mortality data drawn from Multiple Cause of Death files, maintained by the Centers for Disease Control. All statistics are for year 2010. The p-values assigned to the difference in the all county sample are drawn from a two-sample ttest. The p-values assigned to the difference in border county sample are drawn from a paired ttest. Firgures are per 100,000 residents. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 10: Comparison of Treatment and Control Counties, Pre-Treatment

	Treatment	Control	Difference	N
Law Enforcement Characteristics				
No. of Full-Time Sworn Personnel	37.853	28.792	9.061	182
	(5.351)	(5.037)	(7.682)	
No. of Unpaid Sworn Reserve Officers	6.818	3.649	3.169*	182
	(1.592)	(0.903)	(1.897)	
No. of Paid Full-Time Nonsworn Personnel	12.892	13.738	-0.846	182
	(2.070)	(3.116)	(3.865)	
No. of Paid Part-Time Nonsworn Personnel	1.755	1.862	-0.107	182
	(0.446)	(0.459)	(0.624)	
Total Operating Budget (in millions)	3.408	3.133	0.275	182
	(0.493)	(0.590)	(0.812)	
No. of Marked Vehicles Operated	21.406	17.241	4.165	182
	(3.026)	(2.865)	(4.334)	
No. of Unmarked Vehicles Operated	7.255	7.439	-0.184	182
	(1.010)	(1.474)	(1.872)	
Pre-treatment County Characteristics				
High School Graduation Rate in County	83.073	83.562	-0.489	182
	(0.481)	(0.497)	(0.437)	
Percent of Votes Cast for Republicans	63.812	62.629	1.184	182
	(0.844)	(0.877)	(0.767)	
Unemployment Rate in County	8.724	8.426	0.298	182
	(0.236)	(0.235)	(0.194)	
Percent of Population Below Poverty	15.104	15.715	-0.611	182
	(0.416)	(0.450)	(0.440)	
Population per Sq Mile	82.482	88.877	-6.395	182
	(11.947)	(13.873)	(11.579)	
Median Income of County	38468.077	40806.852	-2338.775***	182
	(530.799)	(732.314)	(666.551)	
Percentage of County that is White	88.587	85.341	3.246***	182
	(0.893)	(1.070)	(0.746)	

Notes: Law Enforcement Data is drawn from the 2013 Law Enforcement Management and Administrative Statistics (lemas) survey. County characteristic data drawn from the US Census Bureau's Statistical Compendia program. Test of equivalence is paired t-test. Figures are per 100,000 residents. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 11: Impact of Unrestricted Concealed Carry on Mortality

	Firearm Mortality				Non-Firearm Mortality
	Homicide	Accident	Suicide	Unk Intent	Other Homicide
	Coef/CI	Coef/CI	Coef/CI	Coef/CI	Coef/CI
Panel A: Border Counties					
Post Treatment	0.003	0.678	-0.254	-0.022	-0.078
	(-0.778,0.783)	(-0.058,1.413)	(-1.776,1.268)	(-0.257,0.213)	(-0.596,0.439)
Observations	52551	52551	52551	52551	52551
Avg Trt Mort	1.925	.282	10.147	.059	1.593
MDE (%)	17.97	46.19	12.67	94.95	26.69
No. of Clusters	12	12	12	12	12
Panel B: All Counties					
Post Treatment	0.096	0.130	-0.862	-0.009	-0.231**
	(-0.486,0.677)	(-0.129,0.389)	(-3.240,1.515)	(-0.081,0.062)	(-0.459,-0.003)
Observations	371965	371965	371965	371965	371965
Avg Trt Mort	1.98	.388	9.519	.098	1.114
MDE (%)	16.67	46.14	11.45	82.34	25.22
No. of Clusters	51	51	51	51	51

P-Values estimated via wildcluster bootstrapping. Data is from the National Vital Statistics System's Multiple Cause of Death Files. Estimates are presented per month, per 100,000 residents. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

Table 12: Impact of Unrestricted Concealed Carry on Violent Crimes

	Homicide Coef/CI	Sx. Assault Coef/CI	Ag. Assault Coef/CI	Robbery Coef/CI
Panel A: Violent Crimes, Border Counties				
Post Treatment	-0.166	-0.796	-4.222	0.424
	[-0.249,-0.082]	[-1.871,0.279]	[-6.892,-1.552]	[-0.092,0.940]
Observations	15663	15663	15663	15663
Mean in Trt Cnty	.315	5.308	14.017	2.112
MDE (%)	50.11	8.71	8.06	12.93
No. of Clusters	5	5	5	5
Panel B: Firearm Crimes, Border Counties				
Post Treatment	-0.049	-0.077*	0.804	0.377
	[-0.235,0.137]	[-0.149,-0.005]	[-0.083,1.691]	[-0.134,0.888]
Observations	15663	15663	15663	15663
Mean in Trt Cnty	.206	.042	2.619	.762
MDE (%)	55.75	67.37	16.23	17.97
No. of Clusters	5	5	5	5
Panel C: Violent Crimes, All Counties				
Post Intervention	-0.015	-0.559	-0.752	0.014
	[-0.019,0.049]	[-1.609,0.491]	[-2.452,0.948]	[-0.222,0.25]
Observations	160511	160511	160511	160511
Mean in Trt Cnty	.242	5.438	12.529	1.485
MDE (%)	23.91	5.9	4.36	9.6
No. of Clusters	40	40	40	40
Panel D: Firearm Crimes, All Counties				
Post Intervention	-0.003	-0.008	0.281	0.067
	[-0.017,0.011]	[-0.018,0.002]	[-0.451,1.013]	[-0.031,0.165]
Observations	160511	160511	160511	160511
Mean in Trt Cnty	.131	.028	2.19	.534
Max ES	32.97	83.41	8.35	14.24
No. of Clusters	40	40	40	40

Notes: Confidence intervals are reported at the 95% level. Confidence intervals for Panels C and D obtained via wildcluster bootstrapping. Data is from the National Vital Statistics System's Multiple Cause of Death Files. All estimates include a year fixed effect to capture overall trends, and month fixed effects to capture seasonal variation. Estimates are presented per month, per 100,000 residents $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

Table 13: Demographic Heterogeneity in the Impact of Unrestricted Concealed Carry on Violent Crimes

	Homicide Coef/CI	Sx. Assault Coef/CI	Ag. Assault Coef/CI	Robbery Coef/CI
Crimes against women				
Panel A1: Border counties				
Post Treatment	-0.103	-0.515	-2.750	0.212
	[-0.170,-0.036]	[-1.392,0.362]	[-4.629,-0.870]	[-0.205,0.629]
Observations	15663	15663	15663	15663
Mean in Trt Cnty	.052	4.454	7.199	.652
Max ES (%)	50.69	9.19	9.89	15.5
No. of Clusters	5	5	5	5
Panel A2: All counties				
Post Intervention	-0.003	-0.477	-0.508	0.054
	[-0.025,0.019]	[-1.363,0.409]	[-1.288,0.272]	[-0.138,0.03]
Observations	160511	160511	160511	160511
Mean in Trt Cnty	.079	4.747	6.36	.502
Max ES	39.12	6.31	5.44	12.83
No. of Clusters	40	40	40	40
Crimes against African-Americans				
Panel B1: Border counties				
Post Treatment	-0.044	-0.174	-1.383	0.292
	[-0.108,0.019]	[-0.387,0.038]	[-2.624,-0.141]	[-0.172,0.755]
Observations	15663	15663	15663	15663
Mean in Trt Cnty	.112	.570	3.937	.46
Max ES (%)	35.44	20.04	18.76	26.54
No. of Clusters	5	5	5	5
Panel B2: All counties				
Post Intervention	0.001	0.011	-0.128	0.067^{*}
	[-0.023,0.025]	[-0.055,0.077]	[-0.576,0.32]	[-0.013,0.147]
Observations	160511	160511	160511	160511
Mean in Trt Cnty	.045	.347	2.01	.297
Max ES	37.08	17.59	13.09	20.19
No. of Clusters	40	40	40	40

Notes: Confidence intervals are reported at the 95% level. Confidence intervals obtained via wildcluster bootstrapping. Data is from the National Vital Statistics System's Multiple Cause of Death Files. All estimates include a year fixed effect to capture overall trends, and month fixed effects to capture seasonal variation. Estimates are presented per month, per 100,000 residents
$^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$

Table 14: Geographical Heterogeneity in the Impact of Unrestricted Concealed Carry on Violent Crimes

	Homicide Coef/CI	Sx. Assault Coef/CI	Ag. Assault Coef/CI	Robbery Coef/CI
Crimes committed in rural areas				
Panel A1: Border counties				
Post Treatment	-0.052	-0.255	-1.128	0.081*
	[-0.224,0.119]	[-1.210,0.700]	[-3.154,0.898]	[-0.075,0.237]
Observations	15663	15663	15663	15663
Mean in Trt Cnty	.168	1.941	4.743	.242
Max ES (%)	90.65	15.74	12.78	23.41
No. of Clusters	5	5	5	5
Panel A2: All counties				
Post Intervention	-0.006	-0.334	0.108	0.003
	[-0.046,0.034]	[-0.848,0.180]	[-0.916,1.132]	[-0.049,0.055]
Observations	160511	160511	160511	160511
Mean in Trt Cnty	.113	2.013	4.819	.226
Max ES	42.12	11.45	7.28	23.38
No. of Clusters	40	40	40	40
Crimes committed in urban areas				
Panel B1: Border counties				
Post Treatment	-0.065	-0.452	-3.101*	0.400
	[-0.222,0.093]	[-1.027,0.122]	[-4.151,-2.051]	[-0.151,0.951]
Observations	15663	15663	15663	15663
Mean in Trt Cnty	.121	2.564	7.965	1.773
Max ES (%)	26.75	11.79	11.18	14.41
No. of Clusters	5	5	5	5
Panel B2: All counties				
Post Intervention	-0.001	-0.227	-0.846**	0.004
	[-0.029,0.027]	[-0.757,0.303]	[-1.584,-0.108]	[-0.214,0.206]
Observations	160511	160511	160511	160511
Mean in Trt Cnty	.095	2.826	6.614	1.19
Max ES	26.91	7.57	6.2	10.61
No. of Clusters	40	40	40	40

Notes: Confidence intervals are reported at the 95% level. Confidence intervals obtained via wildcluster bootstrapping. Data is from the National Vital Statistics System's Multiple Cause of Death Files. All estimates include a year fixed effect to capture overall trends, and month fixed effects to capture seasonal variation. Estimates are presented per month, per 100,000 residents
$^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 15: Impact of Unrestricted Concealed Carry on Non-Violent Crimes

	Burglary Coef/CI	Theft Coef/CI	Simple Assault Coef/CI	Placebo Coef/CI
Panel A: Border Counties				
Post Treatment	-12.534	-16.048*	-0.855	0.882
	[-19.523,-5.545]	[-22.900,-9.197]	[-8.698,6.988]	[-4.757,6.521]
Observations	15663	15663	15663	15663
Mean in Trt Cnty	40.622	106.013	47.554	19.135
MDE (%)	6.23	5.11	4.59	6.21
No. of Clusters	5	5	5	5
Panel B: All Counties				
Post Intervention	-5.256	-8.482	-2.804	-2.592**
	[-12.888,2.376]	[-15.541,-1.423]	[-6.996,1.388]	[-3.787,-1.397]
Observations	160511	160511	160511	160511
Mean in Trt Cnty	40.84	110.014	46.775	21.757
Mean in Con Cnty	35.408	110.2	46.471	20.059
Power	1	1	1	1
Clustered At	State	State	State	State
No. of Clusters	40	40	40	40

Notes: Confidence intervals are reported at the 95% level. Confidence intervals obtained via wild-cluster bootstrapping. Data is from the Mational Incident Based Recording System, maintained by the Federal Bureau of Investigation. All estimates include a year fixed effect to capture overall trends, and month fixed effects to capture seasonal variation. Estimates are presented per month, per 100,000 residents.$^{*}p < 0.10$,$^{**}\ p < 0.05$,$^{***}\ p < 0.01$

Table 16: Impact of Unrestricted Concealed Carry on Usage

	Armed with Firearm	Armed with Knife
Panel A: Border Counties		
Post Treatment	-0.148	-0.343
	[-0.943,0.648]	[-0.681,-0.005]
Observations	15663	15663
Mean in Trt Cnty	1.652	.973
MDE (%)	24.63	15.22
No. of Clusters	5	5
Panel B: All Counties		
Post Intervention	0.082	-0.075
	[-0.159,0.323]	[-0.291,0.142]
Observations	160511	160511
Mean in Trt Cnty	1.662	1.448
Max ES	11.45	9.66
No. of Clusters	40	40

P-Values estimated via wildcluster bootstrapping. Data is from the Mational Incident Based Recording System, maintained by the Federal Bureau of Investigation. Estimates are presented per month, per 100,000 residents. $^{*}p < 0.10$,** $p < 0.05$,*** $p < 0.01$

Table 17: Restricting Analysis to LEOs that were Always Present

	Murder Coef/CI	Sexual Assault Coef/CI	Aggravated Assault Coef/CI	Robbery Coef/CI
Panel A: Border Counties				
Post Treatment	-0.100	-0.112	-2.212*	0.640
	[-0.192,-0.009]	[-1.011,0.787]	[-3.603,-0.821]	[-0.053,1.334]
Observations	13892	13892	13892	13892
Mean in Trt Cnty	.274	5.299	14.069	2.146
MDE (%)	-47.81	-22.47	-49.85	-34.06
No. of Clusters	5	5	5	5
Panel B: All Counties				
Post Intervention	-0.001	-0.385	-0.539	0.070
	[-0.031,0.029]	[-1.387,0.616]	[-2.140,1.061]	[-0.155,0.295]
Observations	150094	150094	150094	150094
Mean in Trt Cnty	.239	5.4	12.494	1.476
MDE	23.06	5.95	4.39	9.69
No. of Clusters	37	37	37	37

P-Values estimated via wildcluster bootstrapping. Data is from the Mational Incident Based Recording System, maintained by the Federal Bureau of Investigation. Estimates are presented per month, per 100,000 residents. $^{*}p < 0.10$,** $p < 0.05$,*** $p < 0.01$

Table 18: Spillovers

	Homicide Coef/CI	Sexual Assault Coef/CI	Aggravated Assault Coef/CI	Robbery Coef/CI	Burglary Coef/CI	Theft Coef/CI	Placebo Coef/CI
Post Treatment	0.062	0.035	-1.067	-0.418	1.739	0.558	-2.021
	[-0.108,0.233]	[-0.834,0.905]	[-3.656,1.521]	[-1.191,0.354]	[-5.314,8.793]	[-10.366,11.483]	[-5.617,1.575]
Observations	13892	13892	13892	13892	13892	13892	13892
Mean in Cntrl Cnty	.143	4.108	7.055	1.415	26.47	85.206	15.1
No. of Clusters	5	5	5	5	5	5	5

P-Values estimated via wildcluster bootstrapping. Data is from the Mational Incident Based Recording System, maintained by the Federal Bureau of Investigation. Estimates are presented per month, per 100,000 residents. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$