THE EFFECT OF ACCESS TO CONCEALED CARRY PERMIT DATA: EVIDENCE FROM NORTH CAROLINA

by

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ABSTRACT

Gun regulation in the United States is a contentious political issue. This is exacerbated by the fact that the economics literature has not come to a clear consensus on the effects gun possession has on crime. In this paper, I examine whether access to online gun permit data deters criminal behavior. On July 12th, 2012, WRAL, a Raleigh, North Carolina local television station, published a database containing the number of concealed carry permits held on every street in the station's viewing area. This allowed members of the public to search the database and find the number of permits at the street level in twenty-two of the 100 total counties. This paper studies how public availability of concealed carry permit data affects violent and property crime rates. I use multiple difference-in-differences strategies, exploiting variation in the timing of WRAL's database going online, inclusion in the television station's viewing area, and agency-level permit concentration to examine the effect of a plausibly exogenous shock to crime in North Carolina. My findings indicate that there are no statistically significant changes in property or violent crime rates for counties whose permit data was published relative to those outside WRAL's viewership area. I also find no evidence of crimes shifting between areas of high and low gun concentration. However, an extension of my empirical model suggests that applications for concealed carry permits rise by approximately 18.1% in treated counties after publication of the concealed carry database.

INTRODUCTION

A Gallup survey from October 2015 indicated that when asked "if more Americans carried concealed weapons, would the United States be safer or less safe?", 56 percent of respondents said the U.S. would be safer and 41 percent said it would be less safe.[1, 2] Although many gun rights activists claim that concealed weapons holders are typically responsible citizens who rarely break the law, others are unsure. In a 2011 investigation, the New York Times shed light on several instances of violent crime in North Carolina which were perpetrated by owners of concealed carry permits.[3] Many of the perpetrators used guns and several violent offenders never had their weapons permits revoked. While clearly neither of these claims establish any causality, they are anecdotes which shed light on the controversial nature of whether guns increase or decrease crime. It seems that the sharp divide in public opinion on gun policy runs as deep as questioning the empirical effect of possessing concealed weapons on crime and safety.

Public interest in the gun control debate is certainly one motivation for empirical studies of gun possession. However, the strongest motivation is far simpler: guns kill people, and it is of some importance to determine whether the net effects from gun possession are harmful or save lives. Unfortunately, the quantitative economic literature on the effects of concealed carry weapons on crime rates is relatively ambiguous. This is

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[1] Data from the October 7-11, 2015 Gallup survey are available at https://news.gallup.com/poll/1645/guns.aspx

[2] Cook and Ludwig (2003) use a multivariate regression on General Social Survey data to show that responses to questions about gun control are correlated with smoking and sexual behaviors (i.e. social characteristics). Thus, some empirical work has been done on the nature of these divisions.

[3] Available at: https://www.nytimes.com/2011/12/27/us/more-concealed-guns-and-some-are-in-the-wrong-hands.html

especially true in Right-To-Carry studies where "the empirical research […] has struggled to find consensus on a set of credible assumptions" (Manski and Pepper, 2017). Papers which look at the effects of new concealed carry legislation are notoriously dependent on model specification and sample selection. For example, Lott and Mustard (1997)'s seminal approach to handling state adoption of Right-To-Carry (RTC) laws has been shown to produce inconsistent results when the sample of states is changed, selection bias is accounted for, or different data are used (Black and Nagin, 1998; Ayres and Donohue, 2003; Ludwig, 1998; Wellford, Pepper, and Petrie, 2004).

Furthermore, while much of the existing economic literature examines the effect of states becoming Right-To-Carry, little quantitative research has been done on the effect of publishing data on *who* possesses concealed weapons. As every state in the U.S. currently has some form of legislation allowing concealed carry, it is perhaps helpful to examine the question of the effects of concealed carry on crime from a different angle. This paper attempts to do so by looking at what has happened to crime rates for people who had their permit data published online, and whether that effect changes based on whether the area has a high or low concentration of gun permits per capita.

On July 12, 2012, a local television station in Raleigh, NC called WRAL published a database online of concealed carry permits aggregated to the street level for every county in the station's viewing area. This database was online for just under fifteen months before its removal, but not after harsh criticism from gun rights activists and conservative bloggers. Because publication of the database was plausibly random, this provides an ideal setting to study the effect of published concealed carry data on crime rates.

My research question is thus whether access to data on concealed carry possession changes criminal behavior and subsequently affects property and violent crime rates. To look at the question from a different perspective, I then examine whether crime rates change differently in cities which had differing concentrations of concealed carry permits at the time of database publication. Finally, I examine whether areas which had permit data published changed their behavior in applying for new permits after the database went online.

Data for this study comes from the Federal Bureau of Investigation's Uniform Crime Reporting "Offenses Known" dataset from 2005 through 2013, which is recorded monthly. Additionally, I use panel data on concealed carry permit applications from the North Carolina State Bureau of Investigations, obtained by Depew and Swensen (2018) via a FOIA request. This unique dataset allows me to look at which areas of the state have the highest concentrations of concealed weapons as well as providing the opportunity to use permits as an outcome variable. I then use a difference-in-differences strategy to examine if publication of the database disproportionately affected crime rates for counties within WRAL's viewing area.

To my knowledge, this is the first study to examine the statewide effect of publishing gun ownership data using a quasi-experimental design. Inclusion in WRAL's dataset was determined by county, which allows for a state-wide study of the FBI's UCR data. This allows for a dataset with a large number of observations as well as robust treatment and control groups with heterogenous characteristics. Thus, my paper contributes to the academic literature by both providing a statewide study at the police

agency level and by providing insight into how criminals react to changes in information within a small geographic region.

I find no evidence to suggest that property or violent crime rates change as a result of being in a treatment county after the publication of the WRAL database. Neither are there any effects which appear within the treatment counties that indicate a shifting of criminal behavior between areas of high and low permit concentration. However, an extension of my model shows that treatment counties exhibited a statistically significant increase in applications for gun permits following publication of the WRAL database.

REVIEW OF THE LITERATURE

Public opinion is split on whether guns increase or reduce crime, which furthers contention about gun policy in the United States. As evidenced by the Gallup poll cited above, this also extends to matters of concealed weapons permits. Unfortunately, the academic literature remains divided over such questions as well, particularly because model specification and sample selection are crucial for determining the sign and magnitude of empirical results (Wellford, Pepper, and Petrie, 2004; Durlauf et. al, 2016). There is even little agreement on the number of defensive gun uses per year or how this concept should be defined (Wellford, Pepper, and Petrie, p. 102). The following sections in this chapter outline the existing literature on concealed weapons and examine what consensus appears, if any. I then explain what I believe to be my relevant contributions.

Theory

The bulk of the literature on concealed weapons and the effects of gun possession on crime rest on a foundation built on Becker's (1968) paper. Becker's model of the supply of offenses is based in the idea that "when other variables are held constant, an increase in a person's probability of conviction or punishment if convicted would generally decrease, perhaps substantially, perhaps negligibly, the number of offenses he commits"[4] [5]. In this sense, Becker's theory implies that some criminals make choices

[4] See Becker (1968), page 176
[5] For empirical evidence that supports Becker's (1968) theory of crime, see Ayres and Levitt (1998) on auto theft and Dezhbakhsh et. al (2003) on the deterrent effect of the death penalty on crime, among others

based on their expected utility calculations rather than any underlying sociological or psychological propensity to commit crimes[6].

The existing literature builds off this notion by recognizing that the theoretical casual effect of guns on crime is ambiguous. If there is a "facilitating effect"[7], more potential perpetrators with guns allows for a greater number of crimes to be committed. This would occur when a criminal's expected costs of committing a crime fall, because the cost of inputs in their production function for crime have decreased. Conversely, access to guns may also increase costs for criminals because the probability of being met with a weapon during the commission of a crime is greater. This increases the criminal's marginal cost, and thus would have the effect of decreasing the number of crimes supplied (a "deterrence effect"). It is even possible that both of these instances occur simultaneously, and thus the net effect of guns on crime is theoretically ambiguous in sign.

In a similar vein, Donohue and Levitt (1998) develop a theoretical model which attempts to address when violent conflicts occur over scarce resources in illegal markets. They conclude that fighting is more likely to occur when the outcome of the fight is unpredictable[8]. Although this model is specific in scope, it is plausibly generalizable to gun possession in general, as increased possession of guns could increase the likelihood of extreme violence.

Overall, the theoretical literature points to ambiguity on how guns affect crime, which opens the door for much empirical work to be done.

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[6] Ibid.
[7] This terminology comes from Dezhbakhsh and Rubin (1998)
[8] See Donohue and Levitt (1998)

"More Guns, Less Crime"

Lott and Mustard (1997) is in many ways the seminal paper in the concealed carry literature. This paper was the first to show that concealed carry weapons have a general deterrent effect for both property and violent crimes. Arguments over model specification and sample selection created a subsection of the literature on Right-To-Carry (RTC) laws devoted almost entirely to testing the validity of Lott and Mustard's (1997) results. Lott's work throughout the 1990s and into today[9] has focused a great deal of effort on expanding these results and generalizing them, advocating public policy which allows greater access to concealed weapons (Bronars and Lott, 1998; Lott, 1998; Lott, 2001). Other work by Plassmann and Tideman (2001), Moody (2001), Mustard (2001), and Olson and Maltz (2001) finds evidence of guns having an overall deterrent effect. Moody (2001) tests Lott and Mustard's original model for issues with functional form, omitted variable bias, and simultaneity and finds it to be robust to these concerns.

"More Guns, More Crime"

However, many papers have challenged these original results for various reasons.[10] Ludwig (1998), Black and Nagin (1998), Dezbakhsh and Rubin (1998), Duggan (2001), Ayres and Donohue (2003), and Manski and Pepper (2017) all find null effects or even increasing rates of crime in states which pass RTC laws. Concerns run from selection into the treatment states biasing estimates to suggesting bounded interval

[9] See John Lott's recent appearance regarding mass shootings in a short video by John Stossel. Available at: https://www.youtube.com/watch?v=lXGgI2E5JUw

[10] For a paper which both empirically and rhetorically challenges Lott's "The Bias Against Guns", see Pepper (2003)

estimation rather than a difference-in-differences approach (Ayres and Donohue, 2003; Manski and Pepper, 2017). Even papers that find a general deterrent effect are often based in concerns over potential problems with Lott and Mustard's empirical model[11] or find heterogeneous effects at the state level which average out to an overall fall in crime.[12] Responding to Lott's 2003 book "The Bias Against Guns" which makes a number of strong empirical claims about Child Access Prevention laws and other gun regulation, Pepper (2005) stated "almost any result can be found using the data and methods used by Lott" (p. 935).

It thus seems appropriate to divide the literature down the middle as those aligning with and those opposing Lott and Mustard's finding of a deterrent effect, with perhaps one caveat. If there is indeed any consensus on the issue, it is not over sign or magnitude, but that the literature's original model is flawed.

Contribution to Literature

While Acquisti and Tucker (2011) and Tannenbaum (2017)[13] have similarly dealt with localized publication of gun permit data, my paper expands upon this in a few ways. First, my study examines a natural experiment which takes place at the county level. This allows for a greater number of observations and perhaps a clearer picture of what is occurring. As a result, my counterfactual is relatively robust; since the WRAL database published information for twenty-two of 100 total counties in North Carolina, both my treatment and control groups include large metropolitan and rural areas. Because we

[11] See Moody (2001)
[12] See Plassmann and Tideman (2001)
[13] Both papers cited above are working papers.

know so little about how gun possession changes behavior, this presents a more generalizable framework within which to interpret my results.

With the exception of the above studies, most other papers in the concealed carry literature focus on state adoption of RTC laws. Additionally, the United States currently has a concealed carry law of some form in every one of the fifty states. It may be helpful to review Lott and Mustard's (1997) paper to find further faults in the empirical analysis or methods through which economists can examine the same question. However, as studies of this nature have repeatedly shown, models examining state passage of RTC laws are notoriously dependent on sample selection and model specification (Pepper, 2003; Ludwig, 1998; Plassmann and Tideman, 2001; Manski and Pepper, 2017; Black and Nagin, 1998; Olson and Maltz, 2001; Dezhbakhsh and Rubin, 1998; Wellford, Pepper, and Petrie, 2004). The literature seems to lack studies which use variation at the county level to examine exogenous shocks to crime besides state passage of concealed carry legislation. This is to be expected, as natural experiments with plausibly random variation in gun possession or data on gun permits are relatively rare.

Thus, I view this paper as contributing to the literature in a few different ways. The first is by adding to the empirical literature on guns where RTC laws have already passed. In order to unpack the question of how gun possession affects crime, future empirical studies will need to look at what happens within states that already have these laws, since the model specification problems identified above are now commonly understood. Additionally, how criminals respond to data on gun permits being published is probably a more nuanced decision than the reaction to a state-wide policy. This is because the information available from an interactive database may allow criminals to

target specific areas based on data availability or permit concentration. Examining how information shocks like the WRAL database publication affect crime may be informative about the criminal decision-making process within more granular geographic regions.

BACKGROUND

Gun Permits in North Carolina

During the 1990s, concealed carry legislation passed in a majority of U.S. states, including North Carolina (Depew and Swensen, 2018). The state's "purchase permits", which existed prior to a national concealed carry law, were established to be compliant with the permanent provisions of the 1994 Brady Law passed by the Federal government.[14] The compliance was based in part on usage of the FBI's National Instant Criminal Background Check System.[15] However, a July 1995 state law expanded gun rights and made North Carolina a shall-issue state, overriding the existing policy which outlawed concealed weapons outside of one's home (Depew and Swensen, 7). As a shall-issue state, North Carolina requires that concealed carry permits be granted to applicants who pass a state-standardized application process. In North Carolina, this entails U.S. citizenship, state residency for a minimum of 30 days, a minimum age of 21 years, and completion of an approved gun safety course (Depew and Swensen, 7-8). Additional requirements state that physical and mental limitations which prevent "the safe handling of a handgun" disqualifies an applicant (Depew and Swensen, 7-8).

Since 1995, several gun bills have been passed in North Carolina of varying degree. For example, the 2011 gun bill attempted to allow for concealed weapons to be kept on college campuses in cars with locked boxes and carried into establishments that

[14] See the handbook on North Carolina Firearms Laws from the state Attorney General (revised 2018). Available at https://www.ncdoj.gov/getdoc/241ba1a9-f55f-4517-800a-49a708c4417a/December-2015-Firearms-Publication.aspx

[15] Ibid.

serve alcohol (as long as the owner of the weapon refrains from drinking). It did not pass.[16] The 2013 bill, which effectively removed the WRAL database, passed this provision.[17]

Setting and Institutional Details

On July 12th, 2012, a local television station in Raleigh, North Carolina, WRAL, published a story online about the number of concealed carry permits in the state. The purpose of the article was to use information on concealed carry permits to discuss gun possession in North Carolina. The piece concluded that of the approximately 300,000 guns in the state, the overall number was greater in urban areas, while the concentration was greater in rural areas. Accompanying the article was a database which allowed visitors to the station website to search the aggregate number of permits at the street level for each of the twenty-two counties in WRAL's viewership area. Individual names and addresses were not available. For example, an individual could look up "Fayetteville Street" in Raleigh, and the database would indicate that there were X number of permits on that street as of July 12th, 2012.

Additionally, Mark Binker, the article's author, did a short television news segment on WRAL about the database and his findings.[18] Examining different "captures" of the interactive map webpage from the internet's "Way Back Machine", it appears that the database never updated and was based on July 12th, 2012 data only. Figure 1 below is

[16]Available at https://www.wral.com/committee-passes-measure-to-allow-guns-on-college-campus-in-bars-and-restaurants/12376349/
[17] Ibid.
[18] Available at https://www.wral.com/news/state/nccapitol/video/11311330/

a screenshot of the searchable database from the time it was published, using the "Way Back Machine".

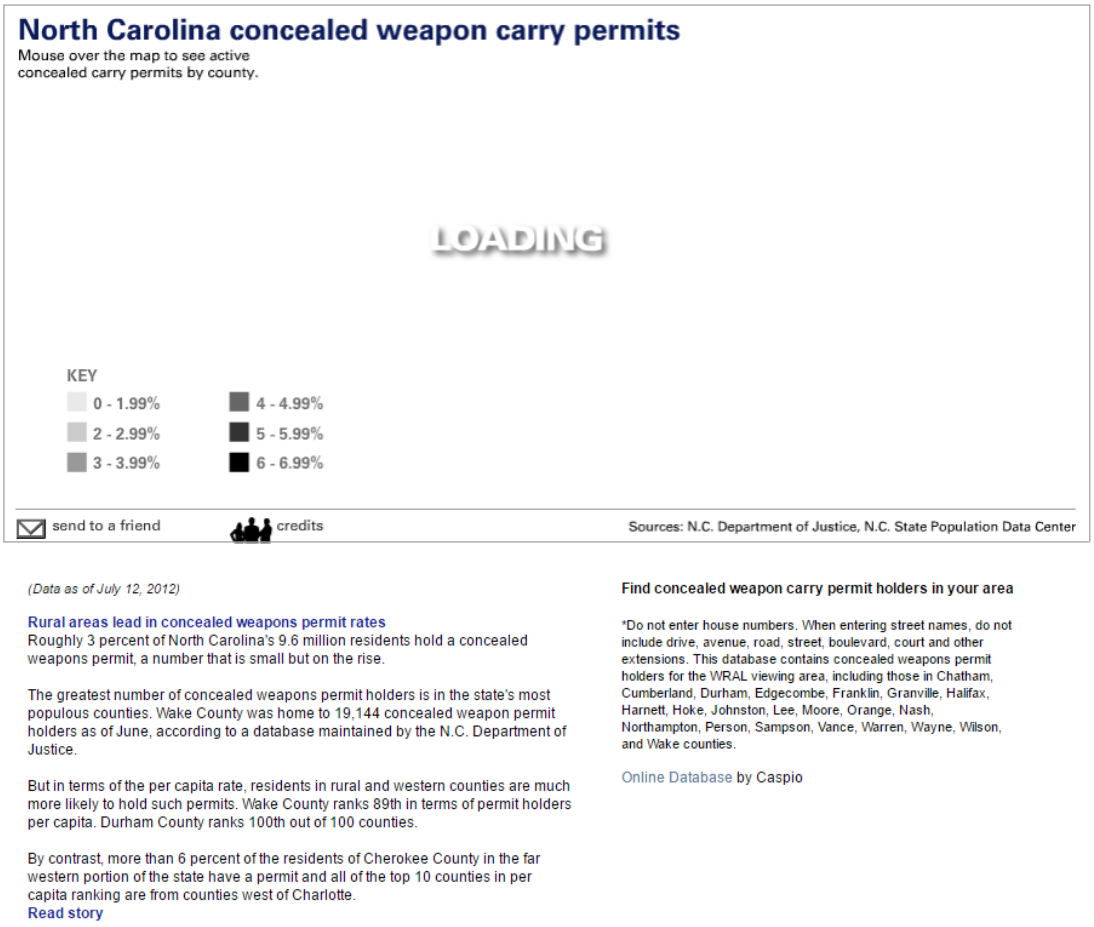

Figure 1. WRAL online gun database search from the original date of publication.

Unfortunately, it is impossible to see the map itself using any archived web captures. However, the legend and phrasing of the interactive map (i.e. "find concealed weapon carry permit holders in your area") are informative about its nature and functioning. The legend appears to indicate the percentage of citizens with permits, which could give potential criminals access to information about which areas within WRAL's viewership are most susceptible to crime. Additionally, the phrasing of the map shows

that the database was indeed searchable, which implies that permit information was more accessible (and thus potentially more useful for criminals and citizens alike).

The database was online from July 12th, 2012 until October 1st, 2013, just under fifteen months. Upon passage of the October 2013 North Carolina State gun bill, concealed carry status, which had initially been public record, was now considered private information.[19] This meant that the WRAL database would be unable to update with current data and was subsequently taken down from the website. Based on archived audio recordings from the North Carolina General Assembly[20], there seems to be no indication that the WRAL database was a motivation for introducing this amendment to the existing gun bill.

Gun rights activists in North Carolina and throughout the nation were thoroughly upset about the publication of the permit data, claiming it highly dangerous. There existed two theories as to why the data should not be published and the database be taken down. The first was that criminals could use the available dataset to target gun owners in burglaries. The justification for this belief was that to a criminal, guns represent high value, easy-to-carry items (Cook and Ludwig, 2003). In this sense, WRAL would be putting gun owners at an increased risk of victimization in a litany of violent and property crimes.[21] The second argument was almost the polar opposite: criminals could target low-gun areas and attempt to take advantage of those unable to protect themselves. In this theory, it is those without guns who have the "target on their back".

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[19] The confidentiality of permit status was included in the Committee Substitute bill adopted on June 11th, 2013. This was exactly two months after the original filing of the proposed gun bill on April 11th, 2013.
[20] Available at https://www.ncleg.gov/Documents/9#2017-2018%20Session\Audio%20Archives
[21] For an explicit statement of concerns, see: https://www.grnc.org/pauls-blog/327-grncs-media-warning-shot-found-its-mark

Loosely, these arguments align with those of Becker (1968).[22] If the supply of criminal offenses is determined by the weighing of the expected utility of committing a crime versus opportunity costs, then the above theories really disagree over what guns represent to criminals (176). If guns represent a high-value item to potential offenders, then publishing the database provides valuable information on where to find guns. In this view, the marginal cost of committing a crime falls, because access to guns makes committing a crime easier. If instead guns represent a deterrent to crime, then potential offenders would see their marginal cost of committing a crime rise in areas with many gun permits. In this view, we would expect to either see crime fall in areas with many permits, or alternatively, criminals may shift those crimes to areas with few guns because they are perceived as safer targets. Thus, both views are consistent with Becker's model of crime because they suggest that changing marginal costs alter the behavior of potential criminals.

Furthermore, concerns over privacy were crucial to the arguments against the database as well. Challengers to the legitimacy of the database held that, although individual names and addresses were not published, privacy remained a major concern. For example, in a rural neighborhood or an urban street with few houses, identifying the residences to which the permits belong is a much simpler task than in a denser urban area which provides an aggregate for many people in a small space.[23] Thus, even without personally identifying information, concern remained over the true anonymity of concealed weapons owners.

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[22] See Becker (1968)

[23] This can be thought of as a corollary to the idea of concealed weapons providing positive externalities. In some sense, the idea of criminals not knowing who has a gun works in larger groups, but fails in rural settings, where it is easier to determine the likelihood that any one person has a concealed weapon.

Tensions arising from unease over second amendment and privacy issues were high among gun rights and political activists. Online blog posts and comments from websites as diverse as "Grass Roots North Carolina", the North Carolina gun rights group; NRA-ILA.com, the legislative wing of the National Rifle Association; and InfoWars.com all expressed deep discontent with the WRAL database. "Grass Roots North Carolina" (hereafter GRNC), which is a significant force in North Carolina state gun rights lobbying, went as far as to post personal information about Mark Binker, the reporter who published the initial article. Such information included his education, dates and places of employment, demographic characteristics, voter registration number, the names of his wife and children (with links to their pictures), descriptions of past family vacations, and his home address excluding two digits to withhold an "exact address". Screenshots from the posted article are included in Appendix G (Figures G.1-G.3). Currently, the original blog post on "GRNC" has 12,028 hits, as indicated by the website counter.[24] The post was also sent as an email alert to what the website currently claims are thousands of subscribers. The post was well-known enough to generate at least some backlash over the publishing of Binker's personal information.[25]

Additionally, commenters from websites like Ammoland.com and AR15.com called for full addresses of WRAL employees, occasionally employing aggressive rhetoric. Some individuals found and commented with full addresses, and a Change.org petition with 522 supporters formed to urge WRAL to remove street level information.[26]

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[24] Available at https://www.grnc.org/grnc-alerts-archive/315-grnc-alert-7-16-12-deliver-a-message-to-wral-tv

[25] See statements on the "BlueNC" blog at https://bluenc.com/content/despicable-behavior-gun-fanatic

[26] Online comments with redacted information (and language) as well as additional screenshots appear in Appendix A.

It seems likely that the availability of the WRAL database was at least recognized enough to create serious discontent, and thus it is plausible that potential criminals would have been knowledgeable about how useful it could be in planning crimes.

The alternatives to these proposed arguments are two-fold. Both also align with Becker's economic theory of crime. First, it is entirely possible that the publication of the WRAL database increases costs for potential criminals. This could happen, for instance, because a criminal learns new information about a high concentration of permits in the area in which they planned to commit a crime. In this case, the supply of crimes falls due to rising marginal costs and thus the total quantity of crimes supplied falls as well. Alternatively, it is possible that the publication of the database has no effect. If criminals are not aware of the database or do not find it useful (either because they do not plan their crimes anyway or because the data are not specific enough to change marginal behavior), there could be no overall shift in the supply of crimes. However, while it is certainly possible that criminals in North Carolina do not plan their crimes, it is highly improbable that this happens systematically. For evidence which indicates that burglars purposefully select targets and plan their crimes, see Cromwell et. al (1991). Thus, from a theoretical perspective, my estimates are ambiguous in sign.

DATA

I use two main sources of data, the Uniform Crime Reporting (UCR) "Offenses Known" data from the United States Federal Bureau of Investigation (FBI) and a panel of concealed carry permit applications from 2005 to February 2013 at the individual level for the state of North Carolina. UCR data includes monthly counts from 2005 through 2013.

I chose to use UCR "Offenses Known" data rather than "Clearances by Arrest" over concerns of endogeneity and accuracy of reporting. If, for example, publication of the WRAL database makes law enforcement more likely to arrest someone for burglary, this could bias my results away from zero. Instead, the "Offenses Known" data are reported at the agency level for every month and "include all reports of Index Crimes received from victims, officers who discovered infractions, or other sources".[27] Thus, the "Offenses Known" data are broader in their interpretation of a crime.

However, this breadth could also bias results if there is reason to believe that an offense not being cleared for arrest is correlated with unsubstantiated claims. That is, if claims which do not make their way into the "Clearances by Arrest" data are more likely to be unsubstantiated, this could bias estimates. Although over-reporting may be a concern for certain crimes such as basic theft, false reports of sexual assault account for between 2 and 10 percent of reported cases.[28] It thus seems plausible that, at least for sexual assaults, additional counts coming from the "Offenses Known" data are

[27] Description taken from https://www.icpsr.umich.edu/icpsrweb/content/NACJD/guides/ucr.html#desc_al
[28] Available at https://www.nsvrc.org/statistics

probabilistically unlikely to arise from false accusations. For more infrequent crimes such as rape, this assumption of unbiased reporting is crucial.

Furthermore, the U.S. Bureau of Justice Statistics' (BJS) 2016 Criminal Victimization report indicated that only 44% of all "violent victimizations were reported to police".[29] Additionally, a U.S. Department of Justice (DOJ) report from 2002 highlights that only 26 to 36 percent of sexual assaults and rapes were reported to police, depending on the specific crime.[30] Finally, as a reported crime does not always lead to an arrest, there is reason to suspect arrest rates for sexual and violent victimization to be even further distorted. Because rates of violent victimization and sexual assault are consistently underreported and unlikely to be unsubstantiated, it is important to use the broadest definition of reported crime available. It is for this reason that I use the UCR "Offenses Known" data.

Specifically, I use data for the crimes of murder, rape, assault, burglary, robbery, theft, and auto theft[31]. These categories are aggregated to create counts for total crimes (all seven crime categories); total property crimes (burglary, theft, and auto theft); and total violent crimes (murder, rape, assault, and robbery).[32] Thus, the number of offenses reported is a measure of all reported crimes committed at the agency level regardless of whether an arrest is made.

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[29] Available at https://www.bjs.gov/content/pub/pdf/cv16re_sum.pdf
[30] Available at https://www.bjs.gov/content/pub/pdf/rsarp00.pdf
[31] Here, the Total Theft category does not include thefts of motor vehicles.
[32] Legally, burglary, robbery, and theft (larceny) are classified as different crimes. Burglary and robbery are specific types of theft, which on its own is simply the taking of another's property. Burglary occurs when a criminal enters into a structure (a person's residence or business) and steals, while robbery occurs when force or threat of force is used to take another's property. A summary can be found at https://www.carneydefense.com/blog/2017/06/what-is-the-difference-between-burglary-and-robbery.shtml

A further concern with using agency-level monthly data from UCR is the "hierarchy rule" discussed in the UCR Handbook accompanying each year of data. This is the method by which the FBI only assigns a value to the highest-ranking offense on a hierarchical list of offenses for a single incident. As Tabarrok et. al (2009) discuss, this presents an issue for agency-level data especially, as it is the lowest available level of aggregation. For instance, if a burglar breaks into a home, steals goods from the residence, and in an altercation kills someone, this is reported as a murder. Thus, the additional charges like breaking and entering and burglary, although brought as charges in real life, are not recorded in the UCR dataset. However, the empirical strategy in this paper examines individual crime rates and not simply aggregates for total crimes. Thus, any change in multiple offense incidents should be picked up by my estimates in some category.

From this UCR dataset, some restrictions are made to the sample. First, to examine agency-level changes in crime, but eliminate overlap from various jurisdictions, my sample includes only municipal police agencies and sheriff's offices. I will refer to these collectively as "agencies" unless context dictates specificity. These two agency types constitute both the incorporated and unincorporated areas of a county. To get more precisely at the issue of double-counting, agencies are dropped when they are indicated to have their reports "covered by other agencies". Furthermore, as incomplete year reports may result in lumping of data or lead to biased estimates, the sample is restricted to all agency-years which report all twelve months. Although "lumpy" reporting is a common problem in UCR data, it is less common in North Carolina. Figure 2 below shows the average total crime rates by month in my sample.

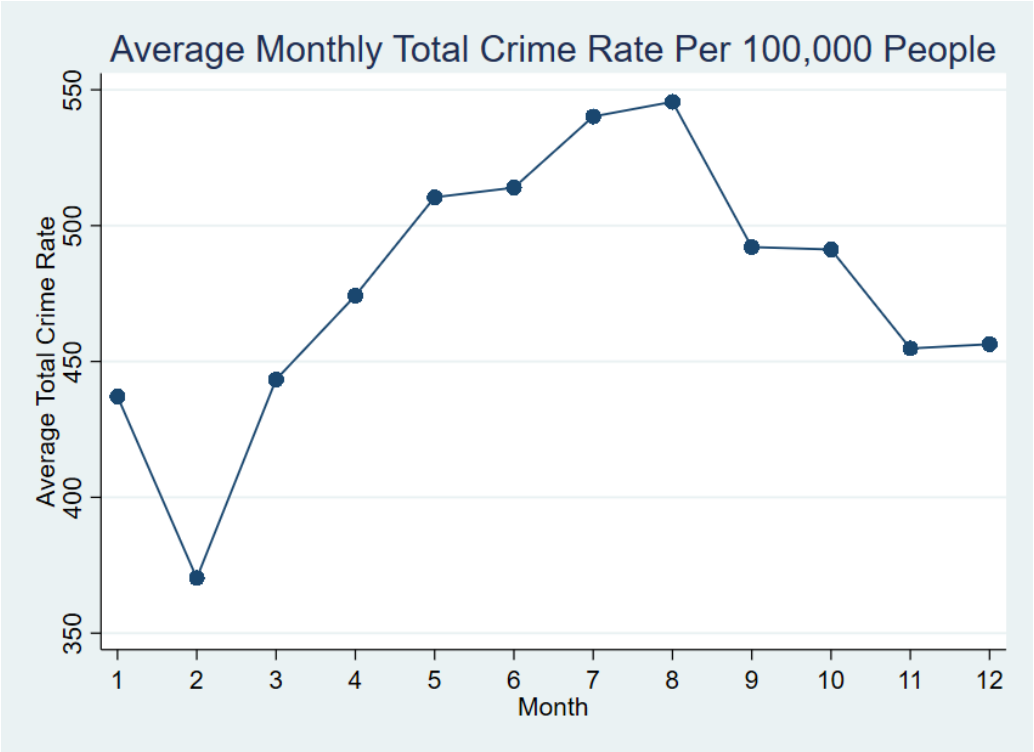

Figure 2. Average Monthly Total Crime Rates Per 100,000 People

As it is well-established that crimes tend to spike in the summer months, Figure 2 indicates that the North Carolina data do align with the spikes expected to see during this season (months 6-8). This, coupled with the moderate value of total crimes in December, indicates North Carolina does not suffer greatly from "lumping" of the data.

The sample is additionally restricted to agencies which have at least one year of data in both the pre and post-treatment period. This eliminates some issue of agencies reporting inconsistently over my panel and helps to make it slightly more balanced[33]. Similarly, 180 observations are dropped which report a population of zero for the

[33] Main results have also been run with a balanced panel. The results from the first D-D remain insignificant. While murder increases significantly in low permit concentration areas in the second D-D model, I do not interpret this as a significant change in my results due to the infrequency of murder and little theoretical intuition to explain this change. Additionally, these regressions identify estimates from approximately half the observations as my preferred specification.

calendar year of 2012. When completed, my sample restrictions leave me with at least one agency in 97 of 100 total counties in the state of North Carolina, with 352 of 491 total municipal and sheriff's agencies. A map below, which utilizes a shapefile from the North Carolina state government, shows counties which remain in the dataset after cleaning and whether they were within the WRAL viewership area:

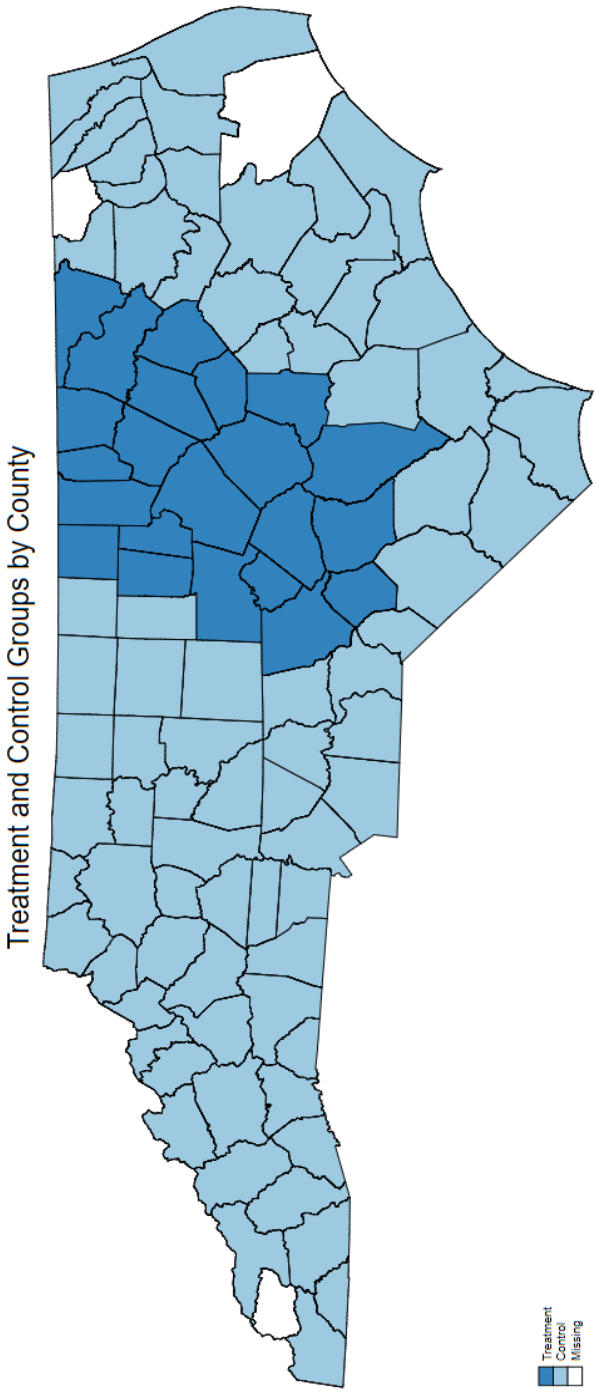

Figure 3. Treatment and control counties (Counties within WRAL viewership area and those outside)

Data on concealed carry permit applications comes from the North Carolina State Bureau of Investigations and have observations at the individual level. These data identify the applicant's city, gender, age, and race, as well as dates of application and permit issue. I keep data on race and gender, while using only active permits in July 2012, the month during which the WRAL database came online. These data come from a Freedom of Information Act request by Depew and Swensen (2018), who were generous enough to share their data. When looking at permit applications as a dependent variable, I use the full panel. As the UCR data is at the agency level, the concealed carry observations must be collapsed to the city-level, which is then matched with the name of the agency reported in the UCR database. After careful matching, I lose only 180 of 25,728 total observations for local police departments.

Additionally, my data set includes county-level monthly unemployment rates, obtained from the U.S. Bureau of Labor Statistics (BLS). As demographic data are scarce at the county-level, especially monthly data, I am restricted to using unemployment as the only covariate in my regressions. However, demographic data from the U.S. Census Bureau 2010 census allows for basic insight into the demographic characteristics of North Carolina. Median income data by county is from the 2012 U.S. Census.

EMPIRICAL STRATEGY

The initial approach taken in this paper is to look at whether crime rates change as a result of having concealed carry data published on WRAL's website. The WRAL database coming online can be considered plausibly random because the general public presumably did not know of its existence prior to publication. This means that aside from variation in whether an agency was a part of a county whose data was published in WRAL's database, there is also variation in time to exploit because the data include crimes in both the pre-publication period and post-publication. It follows that the most relevant empirical strategy will employ some form of difference-in-differences approach, while accounting for fixed effects related to agency and time. This approach takes advantage of a natural experiment which closely aligns with the "ideal experiment": namely, publishing street-level permit data randomly.

However, producing estimates which accurately reflect causal relationships in this context is difficult for a few reasons. First, as North Carolina has a relatively heterogeneous population, there are concerns about cyclicality in the data and of properly accounting for vastly different populations between agencies. Both of these issues will be addressed directly in my econometric model.

More abstractly, one obstacle to identifying the impact of concealed carry data availability on criminal behavior would be an underlying cultural trend in North Carolina. For instance, if individuals in the state of North Carolina become more or less tolerant of concealed weapons during the time of this study's panel, it is possible that changes in criminal behavior observed in the forthcoming regression analysis are really reflections

of criminals reacting to existing changes in concealed gun possession. The question, stated simply, is whether observed estimates should be interpreted as causal or rather as reflections of a change in cultural opinions towards guns.

There are two main reasons why this objection is likely not a concern to my identification strategy. First, unobservable characteristics at the agency level which vary by month or are trending over the timeframe of my panel should be taken care of by fixed effects and time trends, respectively. Second, the institutional context surrounding the publication of the WRAL database suggests this is not an issue. The 2011 gun bill, effective December 1, was the last major act of gun legislation prior to the July 2012 publishing of the WRAL database.[34] If anything, the cultural trend towards guns in North Carolina throughout the history of concealed carry and through the timeframe of my panel has trended in favor of gun possession. Although it is possible that this played some role in the removal of the database, there seems to be no evidence that the WRAL article motivated the amendment which effectively required the database's removal.[35]

A final reason this is not concerning is that my empirical strategy aims to look at the availability of the information "coming online" and not also the effect of the database "turning off". This means the effect examined is limited to the clean variation from the data coming online, and not from its removal, which could have been influenced by unknown factors.

Now that general concerns about how the empirical strategy will identify causal estimates are addressed, we turn to the econometric model.

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[34] Date effective comes from https://www.ncdoj.gov/getdoc/19be6294-bfbf-4875-bbef-ac2ebb6f47b2/2-6-3-6-3-Concealed-Weapon-Reciprocity.aspx

[35] This was determined through research of archived audio recordings from the North Carolina General Assembly. Available at https://www.ncleg.gov/Documents/9#2017-2018%20Session\Audio%20Archives

First Difference-in-Differences

My empirical approach exploits variation in database publication and whether a crime was committed before or after July 2012 to examine whether crime rates change systematically based on inclusion in the database. This difference-in-differences is modeled by the following equation:

$$IHS(CrimeRate_{acmy}) = \beta_0 + \beta_1 Treat_{ac} * Post_{my} + \beta_2\, Unemployment_{cmy} + t_{my} + \delta_y + \theta_{am} + \varepsilon_{acmy} \quad (1)$$

where $CrimeRate_{acmy}$ is a crime rate for agency a in county c during month m of year y, normalized per 100,000 population. The left-hand side of this equation is a transformation of the crime rate, namely the inverse hyperbolic sine function.[36] This function is used rather than a natural log function because it is known that small agencies will report zeros for relatively infrequent crimes like rape and murder. The interpretation of coefficients is the same as that of the natural log function: a percentage change in the normalized crime rate.

$Treat_{ac}$ is a dummy variable for whether agency a in county c was included in the WRAL database. $Post_{my}$ is a dummy for whether or not the observation occurred during or after July 2012 when the data became available. The interaction of the two is the

[36] While it is possible to use crime counts or logged crime counts, the issue of recorded zero observations could pose a threat to identification, especially as it is not always clear for what reason an observation in UCR is recorded as zero (see Tabarrok et. al (2009)). The issue would be the same if attempting to use logged crime rates. For more on this specific issue in the literature, Plassmann and Tideman (2001) speak at length about the improper dropping of zero observations in Lott and Mustard (1997) and Black and Nagin (1998). The quartic root transformation is a good alternative to my specification, but it is not clear that the interpretation is useful for my model. I include a Poisson regression as a robustness check later.

difference-in-differences term, and thus β_1 is the coefficient of interest. $Unemployment_{cmy}$ is the unemployment rate in county c during month m of year y. County-level covariates are scarce, and therefore unemployment is the only such variable in this regression. Finally, t_{my} is a vector of agency-specific linear time trends, while δ_y is a vector of year fixed effects, and θ_{am} is a vector of agency by month fixed effects. Standard errors are clustered at the county level to account for the possibility that monthly crime observations within counties are correlated, as this is the level at which treatment is assigned.

Yearly dummies allow each regression to have a different y-intercept based on yearly averages. This ensures that estimates control for yearly shocks to crime which could otherwise bias my estimates. Additionally, the agency-specific linear time trends take out trends in crime over the length of my panel. This is important because the heterogeneity of agencies in my sample implies that there will likely be agencies which have differing trends in crime during this period, making estimates a reflection of averaged trends. The model also weights results by average population of the agency over the panel to give more weight to areas which are more representative of the overall state population. In this sense, estimates reflect changes in crime rates for the average agency in the sample.

Agency-by-month fixed effects handle issues of cyclicality in the data: because urbanicity and season are likely highly correlated with crime rates, I allow dummies for agency to vary at the month level. This interaction eliminates some variation from cyclicality and nets out the average effect of being, for example, in Charlotte during the month of July (when murder rates tend to spike).

Addressing Concerns of Policy Endogeneity

Ideally, the experiment which I am performing would assign treatment status, the availability of street-level concealed carry permit data, randomly. However, because treatment status is determined by WRAL's twenty-two county viewing area, this introduces potential endogeneity into my experimental design. Although unlikely, it is of some importance to identify whether assignment of treatment group status is correlated in any way with crime rates. That is, if the WRAL database was published because the viewership area was experiencing a systematically different change in crime rates, this could invalidate my findings.

In order to address this issue of potential endogeneity, it is important to state explicitly what I must assume to be true in order to make a causal statement. The identifying assumption of my empirical strategy rests on the parallel trends assumption: crime rates in control counties acted how they would have even if the WRAL database had never been published. Assuming this statement is valid allows me to assign a causal interpretation to deviations in crime rates in the treatment group. Clearly, this assumption cannot be proven, as it tests a counterfactual which never actually existed. Nonetheless, there exist methods which can aid in examining systematic differences in crime rates between treatment and control groups.

Initially, one method of investigating such systematic differences comes from looking at the relevant descriptive statistics. Table 1 shows unweighted means and standard deviations for different normalized crime rates, as well as average population over the panel and monthly unemployment rates. The large standard errors (especially on population) indicate that treatment and control counties appear quite heterogeneous,

which should alleviate some concerns of any policy endogeneity. However, looking at pre-period statistical differences between counties in and out of the WRAL coverage area will be more helpful for informing the decision of whether to adopt the parallel trends assumption.

Table 2 shows unweighted means and standard deviations for different normalized crime rates by treatment and control groups in the pre-treatment period (2005 through June of 2012). These statistics are also available for unemployment and average population of an agency over the length of the panel. Also included are the mean of the full sample, and the level and normalized differences between the means. The normalized difference simply adjusts the difference between mean values for the treatment and control groups by altering the standard t-statistic to not depend on the sample size of the underlying distribution.[37]

It seems apparent that there are indeed level differences between treatment and control groups, but there is no reason to suspect differing trends in the pre-treatment period. The rule of thumb established by Imbens and Rubin (2007) suggests that linear regression will be sensitive to specification when the normalized difference is greater than 0.25. As the largest normalized difference returned is -0.109 for Unemployment (and even less for crime categories), this suggests that none of the unweighted rates in the pre-treatment period are statistically different from each other. This is evidence towards the legitimacy of adopting the parallel trends assumption.

Additionally, Table 3 reports similar statistics, but for pre-treatment trend differences for the treatment and control groups (rather than a simple comparison of pre-

[37] This is reiterated in the notes which accompany Table 2.

period means). For example, the mean for Total Crime in the control group (the first table entry), indicates that the mean increase for control counties in the total crime rate during the pre-period is 17.052 per 100,000 people. Again, the normalized differences indicate that mean pre-period trends for the treated and control counties are not statistically different from one another.

Furthermore, as a way to observe trending crime rates graphically, Appendix C consists of residual plots for every crime rate examined in this paper. These graphs represent residuals saved after regressing crime rates on the D-D term from Equation (1), including year and month fixed effects, with separate graphs for treatment and control groups. These simple regressions attempt to net out some cyclicality from the data and provide a visualization for whether the data is clearly trending in some way over the time frame of my panel. The figure below represents the residual plot for the total crime rate in both treatment and control groups.

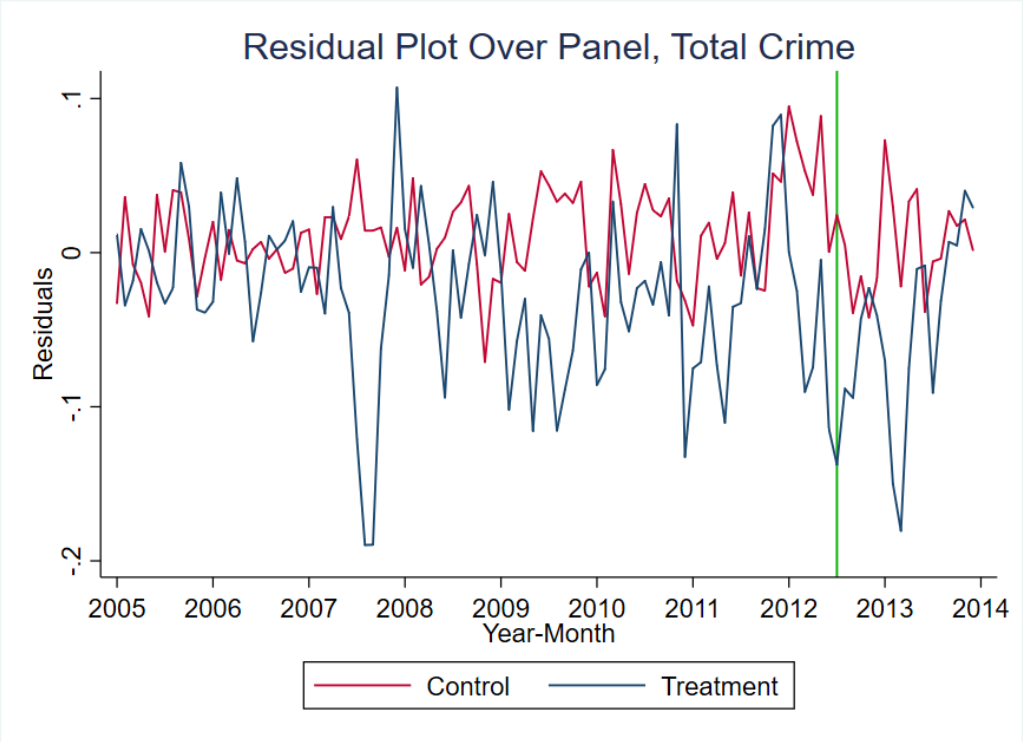

Figure 4. Total Crime residual plot 2005-2013, year and month fixed effects

In the above residual plot, control counties are indicated by the red line, while treatment counties are in blue. The green vertical line represents July 2012, when the WRAL database was published. It seems clear that, while the treatment counties may have a greater variation in crime rates, both treatment and control groups have noisy estimates. Additionally, the graph indicates that after accounting for year and month fixed effects, there do not appear to be noticeable differences in the trends of total crime rates between treatment and control counties. This is another (lesser) justification for adoption of the parallel trends assumption necessary for causal identification. As most of the remaining residual plots are similar to the above, they are reserved for Appendix C.

Second Difference-in-Differences

In my previous specification, my empirical strategy involves using a difference-in-differences approach to examine whether treatment counties experienced statistically significant differences in crime rates relative to the control group after the publication of the WRAL database. While I believe this approach is informative, it may not accurately describe the entirety of what is going on during my panel.

Thus, a potential criticism of my initial difference-in-differences approach is that it does not ask the correct question. In light of the deterrence versus displacement literature, the relevant question may not be whether there are different changes in crime rates between treatment and control groups. Instead, it may be more appropriate to examine whether there was a displacement effect between areas of high and low gun concentration. Like Acquisti and Tucker (2011) find, it is possible that crimes are displaced from areas of high to low gun concentration within the treatment counties. This can also be examined with a difference-in-differences approach, instead exploiting variation in concentration of permits rather than whether a county's permit data was published.

This model uses FOIA-requested data containing the number of concealed carry permits by address, collapsed to the city-level. These concealed carry data are a static measure of the number of permits by city in July of 2012, when the WRAL database was first published. As a result of the FOIA request data on concealed carry possession only running through December 2012, I have six months of post-treatment data in my regressions involving concealed carry rates. Cities are matched with the UCR dataset,

excluding sheriff's offices, whose unincorporated jurisdictions do not belong to cities which match with the concealed carry data.

Dropping sheriff's offices should be of little concern, as most populations covered by a sheriff's office are relatively rural. The larger numbers in average population are driven by counties having relatively large unincorporated areas which include many small towns.[38] Because I suspect criminal displacement, if it exists, to be a largely urban phenomenon, I argue this does not constitute a great threat to identification. Therefore, my sample in the following model consists of local police departments which reside in a county that received treatment group status in July 2012.

This alternative difference-in-differences approach can be modeled by the following equation:

$$IHS(CrimeRate_{acmy}) = \beta_0 + \beta_1 HighConcentration_{ac} * Post_{my} +$$

$$\beta_2 LowConcentration_{ac} * Post_{my} + \beta_3\, Unemployment_{cmy} + t_{my} + \delta_y + \theta_{am} + \varepsilon_{acmy} \quad (2)$$

where $HighConcentration_{ac}$ and $LowConcentration_{ac}$ are dummies for being in the top third or lower third of concealed carry permits per capita, calculated as the city total divided by the normalized population.[39] Generally, this specification bins concentrations of permits per capita into thirds, with middle concentration cities (33rd to 67th percentile)

[38] Additionally, this is why I argue that sheriff's offices are largely inappropriate for this model. A large amount of unincorporated area in a geographically large county will have many citizens when aggregated. However, as my data is weighted by population, this means hundreds of square miles of unincorporated area with few residents in each town is weighted the same as an urban city with the same total population. Such areas are unlikely to be representative of incorporated areas where I expect most of my effect to occur. As sheriff's offices in treated counties tend to follow this description (large in landmass with several small populations aggregated together), I omit them from the second model.

[39] The relevant population statistic here is the normalized population in 2012, the year in which the treatment occurred. Therefore, my binning of high, medium, and low concentration permit groups is based on the population in the year the WRAL database was published.

as the omitted category.[40] This helps establish a more convenient interpretation taken independently as well as alongside Acquisti and Tucker's (2011) findings, which also bins by thirds. Thus, both the high and low concentration dummies represent variables of interest, interpreted as percentage changes in the crime rate relative to mid-concentration areas.

As $HighConcentration_{ac}$ and $LowConcentration_{ac}$ are dummies for different permit concentrations and $Post_{my}$ is a dummy for whether the observation occurred during or after July 2012, the interactions are my difference-in-differences terms, making β_1 and β_2 the coefficients of interest. As in equation (1), $CrimeRate_{acmy}$ is a crime rate for agency a in county c during month m of year y, normalized per 100,000 population; $Unemployment_{cmy}$ is the unemployment rate in county c during month m of year y; t_{my} is a vector of agency-specific linear time trends; δ_y is a vector of year fixed effects; and θ_{am} is a vector of agency by month fixed effects. For reasons discussed above, I continue to transform my left-hand side using the inverse hyperbolic sine function. In this model, standard errors are clustered at the agency (or equivalently, city) level to account for possible correlation in monthly permit counts within agencies.

Similar to the first D-D above, Tables 6 and 7 represent normalized difference statistics which help understand pre-period trends in crime rates, population, and unemployment between areas with heterogeneous permit concentrations. Tables 6 and 7

[40] I have also run specifications with a two-bin model that simply groups agencies by whether or not they were above or below the median concentration of gun permits per capita at the time the database was published. I find no significantly different results. Additionally, a model which bins permit concentration by quartiles reveals that there may be significant decreases in Murder and increases in Total Theft for highest quartile agencies relative to lowest quartile agencies (the omitted group). However, what constitutes a relevant omitted group with quartile bins is unclear and there is no relevant theoretical reason to find significant changes in Murder and Total Theft. For these reasons, I omit regression results from the quartile bins specification.

indicate that there are no statistically different trends in crime rates between high and mid-concentration agencies or low and mid-concentration agencies prior to the publication of the WRAL database[41]. The only variable for which trends are statistically different is population when comparing low and mid-concentration agencies (Table 7). This is a further justification for keeping population weighting a focus of my preferred specification.

[41] This difference is calculated by subtracting the crime rate in a category for January 2005, the first month in the pre-period, from the rate in June 2012, the last month in the pre-period. This gives a rough estimate of the change in the crime categories over the pre-treatment period. These differences are then averaged for all agencies by concentration of permits. Thus, the normalized differences are an indication of whether the average difference in trends over the pre-period is statistically different between groups with varying permit concentrations at the time of database publication.

RESULTS

Effect on Crime in Treated Counties

Column (4) of Table 4 shows the estimated effect of treatment group status on different crime rates, as specified by Equation (1). Each column in Table 4 represents a different model specification, all of which have standard errors clustered at the county level. Column (1) represents a model with basic year, month, and agency fixed effects, which additionally weights by average population of the agency over the panel. Column (2) represents a model with the preferred year and agency-by-month fixed effects, and weights by population, but does not include time trends. Column (3) includes the preferred fixed effects and time trends but does not weight by population. Each model is run over the full set of 352 agencies in my sample, which include local police departments and sheriff's offices.

The coefficient estimates from Column (4) indicate that there is no evidence of an observable, significant effect of WRAL data publication on crime rates. As explained in Chapter V, the Empirical Strategy section, my left-hand side is the inverse hyperbolic sine transformation of the crime rate, which is normalized per 100,000 people. This means that my estimates can be interpreted as a percentage change in the normalized crime rate. While the effect on Total Rape is significant at the 10% level, inconsistency across models, weak significance, and low frequency of rapes reported[42] suggest that this estimate may not be causal.

[42] Table 1 indicates that mean rates of rape is around 2 per 100,000 people for both treatment and control counties in the pre-period.

Aside from Total Rape and the slight change in significance for Total Auto Thefts when agency-specific linear time trends are included[43], the preferred model in Column (4) is highly robust to specification. Although point estimates change in sign depending on model specification, there does not appear to be any clear pattern as to which models are affecting sign or which variables are positive or negative[44]. This is largely because the standard errors across models are relatively homogeneous and are often larger than the estimates. The other models included in Table 4 indicate that my initial results are not significantly changed by the inclusion of time trends, population weighting, or a dynamic fixed effect specification.[45]

However, my estimates do not fully reflect the dynamics of the post period. This is because the post period indicator will mathematically average the treatment effects in all post period months (of which there are 18). The figures in Appendix D represent graphs of the dynamic estimates of my model for different crime rates. These dynamic plots are event study graphs which plot the coefficients of the estimated effect of treatment group status on crime rates by individual months. The bars surrounding the coefficient estimates represent ninety-five percent confidence intervals. The running variable is months surrounding the publication of the WRAL database which is represented by the vertical, green, dashed line labeled "Treatment".[46] To examine a more

[43] This may be an indication that auto thefts are trending upward over the time frame of my panel, in which case inclusion of time trends is likely the correct specification.
[44] Total Theft is positive across all specifications. It is the only dependent variable which does not change in sign in Table 4.
[45] Although one could apply an "Occam's Razor" argument here, I choose to retain my previously established preferred model. This is because other questions are examined later and concerns remain about identifying coefficients based on cyclical variation, agency populations, and time trends over the panel.
[46] A minor point that should be mentioned concerns the nature of constructing an event study plot. As the UCR data used is based on monthly crime reports, the treatment effect of the WRAL dataset coming online does not happen "cleanly" on July 1st, but rather July 12th, and thus the vertical "Treatment" may be misleading. The coefficient in this month is the reported effect of being in the treated counties in July 2012

complete picture of the trending estimates, I change the original counterfactual slightly to include the three months prior to publication of the gun database.

To start with the most obvious analysis, standard errors on many estimates are rather large. This is to be expected, as the initial residual plots from Appendix C indicated a large amount of variation, even when accounting for year and month fixed effects. Seemingly, the only exceptions are the plots for Total Crime, Total Property Crime, and estimates for Total Theft soon after treatment. Again, large standard errors on every category of violent crime are not surprising; the frequency with which murders, rapes, and robberies occur is very low, even without correcting for agency-specific seasonality.

Estimates in the dynamic plots in Appendix D do not seem to have any clear trend over the pre-period. However, it can be noted that for Total Property Crime, Total Robbery, Total Theft, and Total Burglary, there appear to be three to four-month trends of increasing rates starting approximately eight months after the WRAL database is published (March of 2013). The same trend occurs with Total Rape but begins ten months after database publication. However, these coefficients are not statistically significant or very different from each other.

Furthermore, although the point estimates listed in Column (4) of Table 4 do not seem large, they are not insignificant. For example, the coefficient on Total Rape shows that the percentage of rapes per 100,000 people falls by 6.24% for treated counties after the publication of the WRAL database. While the estimate is not statistically significant,

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overall, which averages both pre- and post-treatment periods. Thus, the remainder of my months surrounding treatment, because they are arbitrarily assigned by the first of the month, are slightly off from an actual "month from" the treatment effect. Therefore, estimates should be interpreted as average effects on crime rates in the month following the treatment month, not following the actual day of treatment.

a "back-of-the envelope" calculation suggests that costs averted could be as high as $55,565.[47]

Overall, when examining the effect of an agency's inclusion in the WRAL gun database on crime rates, there does not appear to be an observable effect. As discussed in the theoretical section at the beginning of this paper, it is possible that this arises from criminals not using or not knowing how to use the database. Another explanation for the results, which seems more plausible, is that the level of aggregation at the street level does not disseminate useful enough information for criminals to change their behavior. If indeed most crime is to occur in urban areas, the opposite of the NRA's privacy concern could be true: in an area with a high population density, such as a large apartment complex with several permits, the database could represent potentially useless information for the planning criminal. It is possible, then, that when given access to this very general information, criminals do not change their behavior on the margin. Overall, I fail to find any convincing evidence that publication of the WRAL database had any significant impact on crime rates in counties whose gun permit data was made public.

Effect on Crime in High and Low Concentration Agencies

Tables 8 through 10 show the estimated effect of being in a city/agency of high or low gun concentration on different crime rates relative to mid-concentration areas.[48] Each

[47] In terms of state budgets this may seem small, but this cost may be large for victims. This estimate is calculated using the estimated lifetime cost of a rape at $122,461, which comes from the National Sexual Violence Resource Center. Based on the mean 1.695 rapes per 100,000 population in treated counties, which have a mean population of 138,485.6, my calculation yields $17,937.29. However, estimates may range as high as $55,565.13, which is calculated based on Raleigh's total population of 428,993 in 2013. Estimated lifetime cost information available at https://www.nsvrc.org/blogs/cost-rape

[48] In these regressions, the omitted group is the set of counties after the post-period which have a concentration of permits per capita in the middle third of the distribution. Thus, "High Concentration" and

point estimate reflects the coefficient from a regression with dynamic fixed effects, population weighting, and agency-specific linear time trends, as specified in Equation (2). In these regressions, the sample is restricted to local police departments in treated counties, which reduces the number of agencies to 67.

In general, the coefficients from Tables 8 through 10 indicate that there is no observable significant effect on crime resulting from being in an agency of high or low gun concentration relative to a mid-concentration agency when the WRAL database was published. Although the point estimate for Total Property Crime in high concentration agencies is significant at the 10% level, this is being driven by an increase in Total Theft in these agencies, also significant at the 10% level. The other property crime categories, Total Burglary and Total Auto Theft, are highly statistically insignificant. Furthermore, it is well documented that motor vehicle theft is consistently one of, if not the most frequently reported crime.[49] Thus, there is reason to believe the point estimates for Total Auto Theft are some of the most precise for offenses reported, and the large standard errors indicate no observable effect of gun concentration on crime rates. Additionally, estimates for Murder increasing in low concentration agencies is likely not causal due to weak significance and the frequency of murders reported.

"Low Concentration" variables are interpreted as percentage changes of the relevant crime rate relative to "mid-concentration" agencies. Tables 8 through 10 are separated by groups of crimes: Table 8 represents estimates for aggregate rates of total crimes, total property crimes, and total violent crimes; Table 9 represents estimates for individual rates of violent crimes; Table 10 represents estimates for individual rates of property crimes.

[49] The 2017 Criminal Victimization Report, released by the Bureau of Justice Statistics, indicates that an estimated 79% of all motor vehicle thefts were reported to police in 2017. This figure seems to be consistent from year to year. For use of another similar report and a more detailed discussion of auto theft in general, see Ayres and Levitt (1998).

Appendix E includes plots of dynamic estimates comparable to those found in Appendix D for the first differences-in-differences estimates. Here, coefficients represent the estimated effect of being in a high concentration group relative to the middle third of the distribution for three months prior to treatment and for every month in the post period.[50] Again, bars represent 95 percent confidence intervals around the point estimates.

As in the first difference-in-difference plots from Appendix D, there do not seem to be clear trends in the monthly estimates for high concentration agencies. Estimates for several variables (most notably Total Theft, Total Assault, and the aggregate categories) are on average higher towards the end of the post-treatment period, while other variables, namely, Total Rape, have lower estimates further along in the panel. Another upward trend can be seen around months 4 through 8 for the aggregate categories as well as a number of other variables. However, like the plots in Appendix D, the estimates tend to have large standard errors and are not statistically different from zero, nor from each other.

Overall, there appears to be no evidence of an observable significant effect of existing gun concentration on crime rates within the treated counties. While the estimates are not precise, these results, in conjunction with those from the section above, indicate that that there is no convincing evidence of any changes in crime as a result of WRAL publishing their online gun database.

[50] Thus, coefficients are the monthly estimates which relate to the "High Concentration" variable in Tables 8 through 10. This was chosen rather than "Low Concentration" because the infrequent significance which appears comes from high concentration agencies.

Permit Applications as an Outcome Variable

One benefit of the gun permit data from the North Carolina State Bureau of Investigations is that enough data exists to construct a panel of permit applications which matches the UCR crime data. However, the number of active permits at any one time is a function of the number of permits applied for, granted, and not expired. Thus, it is very difficult to track the actual number of permits in each agency over the panel. Instead, I use a panel of *applications* for concealed carry permits as an outcome variable. This allows me to examine whether the behavior of private citizens in the treatment counties changed significantly in response to publication of the WRAL database. Because North Carolina is a Right-To-Carry state, there is sufficient reason to believe that a significant change in permit status would be highly correlated with actual increases in the number of concealed weapons[51].

Table A.14 shows results from regressing permit applications on the treatment status difference-in-differences variable from Equation 1. Besides the variable being transformed on the left-hand side, my model is the same: a monthly unemployment covariate, agency-specific linear time trends, population weighting, year and agency-by-month fixed effects, and standard errors clustered at the county level. The regression includes a placebo for the three months prior to the July 2012 publication of the database, but coefficient estimates are identical to the regression without pre-treatment periods.

[51] More explicitly, this is because Right-To-Carry states must issue concealed weapons permits to those within the state who apply for them, as long as they meet a minimum standard of scrutiny (which in North Carolina includes a search in the National Instant Criminal Background Check System). Therefore, the assumption that a change in permits actually correlates to an increase in concealed weapons is likely valid.

The “Treatment Post Period” point estimate indicates that inclusion in the treated counties results in an 18.1% increase in applications for concealed carry permits after the WRAL database is published. As the pre-period mean for treated counties is approximately 10.41 applications per month, this estimate suggests an increase in the mean treated county to 12.29 applications per month, an increase of approximately 1.88 additional permit applications. Figure 5 below represents the dynamic estimates of this regression.

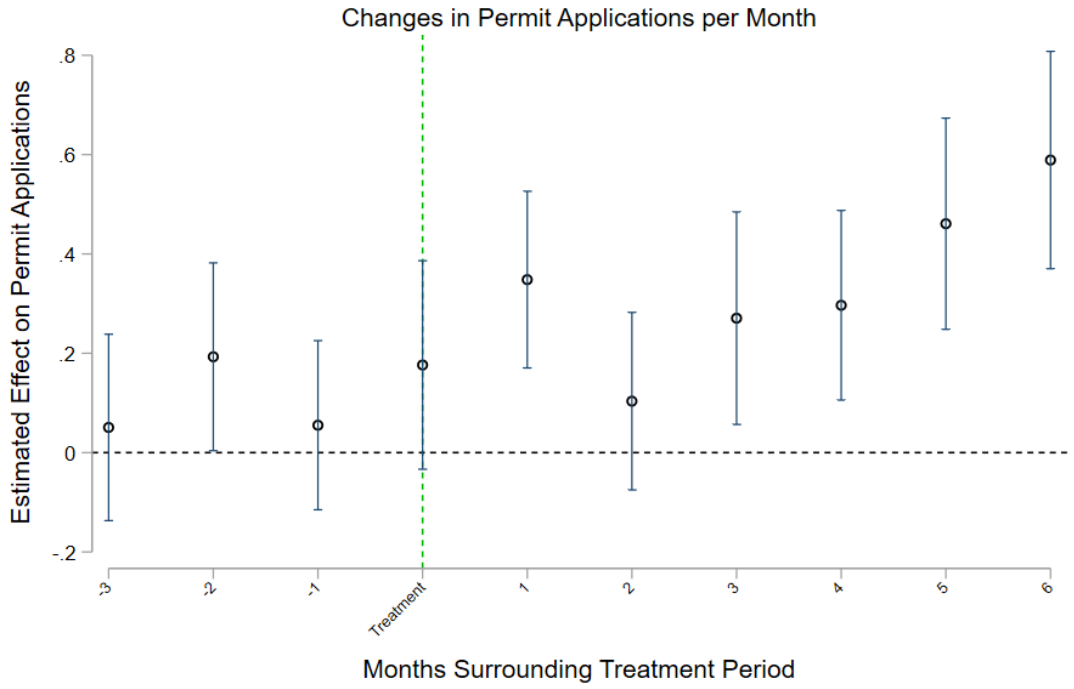

Figure 5. Dynamic estimate plot of total permit applications in treated counties

Here, there is a clear upward trend for approximately six months following the publication of the WRAL database. This indicates that rather than a level jump in the percentage of permits applied for, this increase in applications rose from no significant change to an approximately 50% increase between September 2012 and January 2013.

Not only is this estimate robust to the included placebo test, but an additional Poisson specification of the same model indicates that the estimate may be conservative.[52] Table A.15 shows an increase in permit applications by 46.2%[53], or an increase in the mean treated county to 15.219 applications per month. This represents an increase of an estimated 4.8 additional applications total per month. The Poisson, too, is robust to placebo testing.

Further breakdowns of this result into race and gender categories indicate that the effects are relatively homogenous. Appendix F presents dynamic estimates by race and gender similar to the above figure. Additionally, Table A.16 presents estimates from regressing the rates of these total permit subcategories on the treatment status difference-in-differences term. Across almost every demographic characteristic, magnitudes are very similar; there is a consistent increase in permit applications of approximately 18.5% from black, white, and male citizens. The notable exceptions are females, whose applications increase by almost 20%, and "other race" citizens, who show no statistically significant increase in applications[54].

The dynamic estimates in Figure F.5 (located in Appendix F) indicate that the point estimates for increases in permit applications by women are the highest of all subgroups in the post period. These estimates, coupled with the total post period effect found in Table A.16, are suggestive of a story that centers on safety concerns. If women were perceived to be more at risk than men following the publication of gun permit data

[52] It is important to note that this Poisson regression uses the same weighting, etc. as the IHS model, but is unable to account for time trends due to convergence issues.

[53] This is calculated by using the traditonal $((e^{.38} - 1)$ X $100)$.

[54] However, while the standard error is larger here than for the other total permit subcategories, it is worthwhile to note that the magnitude of the effect is relatively similar to the other estimates. This may arise as a result of few "other race" permit applications generating noisy estimates.

by WRAL and subsequently applied for additional permits, this would explain the difference in estimate magnitude between female and male applicants.

Finally, using quantile regression methods and code from Cameron and Trivedi (2005), I examine whether this behavior of applying for new permits changes when examining different quantiles of the distribution of permits per capita. Figure 6 below shows the results from this quantile regression:

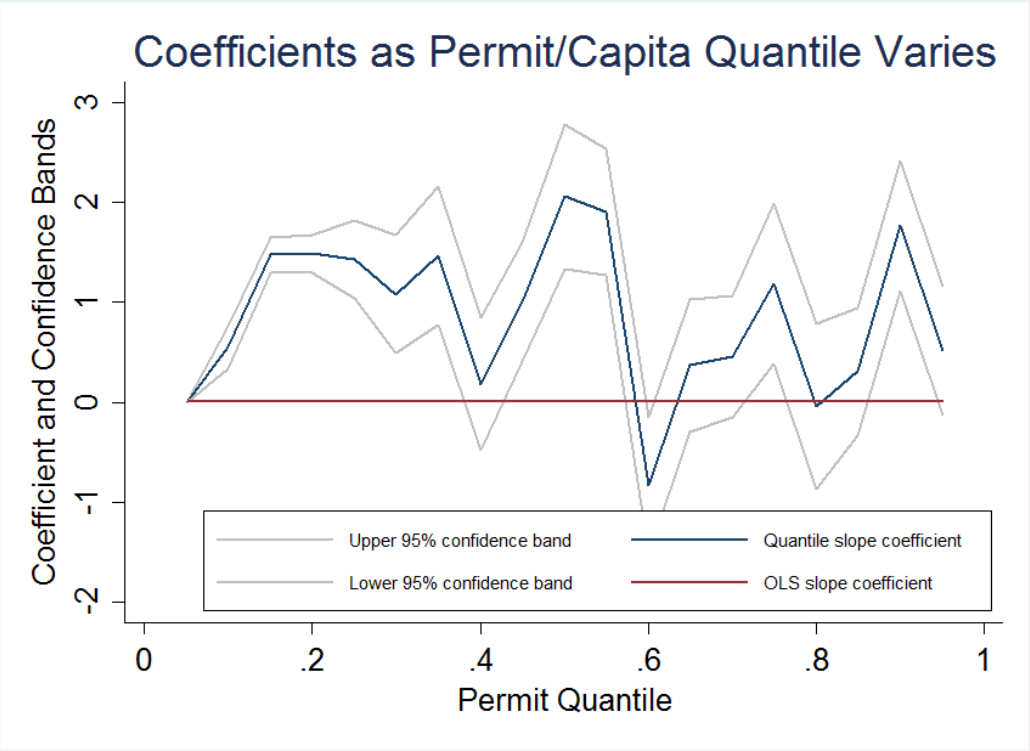

Figure 6. Quantile regression graph showing the effect of different quantiles of permits per capita with 95 percent confidence intervals

Here, the coefficient estimates are shown with 95 percent confidence intervals, and indicate the effect of regressing various quantiles of the distribution of permits per capita on the inverse hyperbolic sign of total permits applied for (per 100,000 people). Fixed effects are year, month, and agency, and standard errors are based on 200 bootstrap

samples. While most quartile estimates are above zero, there seems to be no indication that coefficients vary consistently by permit status.

Robustness Checks

Accompanying both sets of main results tables are the dynamic estimates for crime rates by treatment status and permit concentration located in Appendices D and E. A quick check of the pre-period estimates, which double as placebo tests, indicate that only 4 of 30 monthly estimates in Appendix D and 2 of 30 in Appendix E are statistically significant. As we can expect roughly ten percent of these estimates to be significant based purely on probability, this is a good indication that my specification is generally robust in both models to placebo testing.

Additionally, both difference-in-difference specifications for the main results were run with Poisson regressions rather than using the inverse hyperbolic sine. The left-hand side of the resulting Poisson regressions are total crime counts for every agency-year-month. Coding issues prevented interacting agency and month fixed effects, but otherwise the models in Equations (1) and (2) remain the same[55]. Tables A.17 and A.18 indicate that for the aggregated total crime categories, there is similarly no effect when running Poisson regressions rather than the inverse hyperbolic sine specification. Issues of overfitting the model resulted when regressing the other crime categories, but with the exception of violent crimes in the first D-D, results generally remain statistically insignificant.

[55] The similar point estimates from Columns (1) and (4) of Tables 4 and A.4-A.13 indicate that the more basic fixed effects specification yields values very close to those with dynamic fixed effects. Thus, this coding issue is unlikely to be a concern.

CONCLUSION

Gun policy remains an issue of great debate in American politics. Both the general population and those in academia have thus far been unable to agree on whether the net externalities of concealed weapons possession are positive or negative. Most empirical work on concealed carry in the United States focuses on the adoption of concealed carry legislation at the state level. My paper is the first to examine the statewide effect of publishing gun ownership data using a quasi-experimental design.

Using data from the FBI's Uniform Crime Reporting datasets, this study employs multiple difference-in-differences approaches to examine what effect, if any, the publication of street-level aggregates of concealed carry permits had on crime rates in North Carolina. The first D-D exploits variation in whether a county's data was published and if a crime occurred before or after publication, showing no evidence of an observable effect of permit publication on crime rates. The second D-D uses FOIA requested data from the North Carolina State Bureau of Investigations, exploiting variation in time and concentrations of gun permits within treated counties at the time of database publication. I find no evidence of any observable effect which would indicate displacement of crimes between areas of high and low gun concentration. The general robustness of my models to multiple specifications and placebo tests leads me to conclude that I fail to find any evidence of a significant effect on crime as a result of WRAL publishing street-level aggregations of concealed carry permits. However, it is possible that criminal decisions change at the street-level, displacing crime between high and low permit streets rather than across cities. As street-level crime incident data do not exist, this limitation means I

am unable to say precisely that there is no effect on crime rates, but rather that there is no evidence of an effect at the agency level.

Finally, extending my model, I use the same North Carolina state data on gun permit applications and find a statistically significant increase in applications within treated counties relative to the control group following database publication. These effects do not appear to be consistently different based on quartiles of permits per capita.

When the main results from this study are coupled with the result from the model extension, a clearer picture is painted. Although I fail to find evidence that criminals change their behavior with access to granular geographic data on gun permits, the strong evidence that citizens apply for gun permits as a result of database publication suggests that externalities are probably non-zero, on net. In this sense, I view my research as contributing to the literature on gun possession and criminal behavior with a statewide natural experiment, but also as providing insight into citizen responses to perceived changes in personal safety.

On the one hand, the economic literature from Lott and Mustard (1997) and others suggests that more guns in the hands of "responsible citizens" reduces crime. Alternatively, the literature also suggests that violence may increase as a result of gun possession, turning otherwise non-violent conflicts deadly (Donohue and Levitt, 1998). It is therefore unclear whether WRAL's gun database had net positive externalities through increased safety, or net negative externalities through increased violent confrontation. This gets at the heart of the "more guns, less crime" debate, and should be the focus of future empirical research.

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TABLES

Table 1. Descriptive Statistics by Treatment Group Status

	Mean		Standard Deviation	
Total Crime	444.122	**402.791**	265.472	**237.999**
Total Violent Crime	127.182	**106.074**	93.851	**73.409**
Total Property Crime	316.939	**296.717**	194.586	**177.655**
Murder	0.463	**0.526**	1.586	**1.568**
Total Rape	2.067	**1.695**	3.333	**2.829**
Total Assault	200.253	**187.722**	142.247	**125.593**
Total Robbery	113.897	**92.997**	85.804	**65.781**
Total Theft	10.755	**10.856**	13.854	**11.828**
Total Burglary	95.198	**91.023**	58.498	**56.850**
Total Auto Theft	59.063	**27.543**	132.687	**31.144**
Average Population	165,169.4	**138,485.6**	240,976.2	**125,381.7**
Unemployment	8.087	**7.235**	3.010	**2.769**
Median Income	45,942.30	**52,295.66**	7875.856	**10,763.85**
% White	71.454	**61.081**	15.307	**10.465**
% Black	18.979	**27.5**	11.963	**11.03**
% Asian	1.914	**3.083**	1.510	**2.241**
% Hispanic	7.877	**9.745**	3.298	**2.913**
% Minority (Total Population Non-White)	31.565	**42.595**	15.601	**10.158**
No. Agencies	262	**90**	262	**90**
N	24,516	**8400**	24,516	**8400**

Notes: Counties whose data was published on July 12th, 2012 have their relevant statistics in bolded font.

The above table shows means and standard deviations for crime rates, average agency population over the panel, and unemployment rates (measured at the county-year-month level), for treatment (bolded) and control groups. Crime rates are normalized for a population of 100,000 people and created from the FBI's UCR crime data (2005 through June 2012). All statistics are weighted by an agency's average population over the panel. Race and income data are at the county-level and comes from the U.S. Census Bureau's 2010 census. Unlike tables that follow, these descriptive statistics indicate means for the entire sample rather than just the pre-period.

Table 2. Descriptive Statistics by Treatment Group Status, Pre-Period

	Mean *Published=0*	**SD** *Published=0*	**Mean** *Published=1*	**SD** *Published=1*	**Sample Mean**	**Normalized Difference**	**Level Difference**
Total Crime	498.899	(416.788)	457.230	(289.549)	488.240	-0.082	-41.668
Total Violent Crime	116.735	(117.868)	107.780	(96.005)	114.445	-0.059	-8.955
Total Property Crime	382.163	(354.967)	349.450	(229.359)	373.796	-0.077	-32.713
Murder	0.488	(3.756)	0.577	(4.463)	0.511	0.015	0.089
Total Rape	2.295	(9.044)	2.107	(10.224)	2.247	-0.014	-0.188
Total Assault	105.463	(106.845)	94.559	(88.071)	102.674	-0.079	-10.903
Total Robbery	8.490	(37.675)	10.537	(20.425)	9.013	0.048	2.047
Total Theft	256.184	(282.614)	227.938	(175.138)	248.959	-0.085	-28.246
Total Burglary	108.644	(136.009)	104.805	(92.328)	107.662	-0.023	-3.838
Total Auto Theft	17.336	(31.140)	16.707	(24.529)	17.175	-0.016	-0.629
Average Population	25141.320	(59611.840)	32250.230	(58750.890)	26959.730	0.085	7108.914
Unemployment	8.289	(3.410)	7.777	(3.173)	8.158	-0.109	-0.513
No. Agencies	263	263	90	90	353	353	353
N	20232	20232	6954	6954	27186	27186	27186

Notes: *Published* is a dummy variable equal to one if the county in which the agency resides was ever included in the WRAL database. Categories are crime rates, normalized for a population of 100,000 people, created from the FBI's UCR crime data (2005 through June 2012). All statistics are unweighted, and the table reports means of each variable. Standard deviations are in parentheses. "Normalized difference" adjusts the difference between mean values for the treatment and control groups by altering the standard t-statistic to not depend on the sample size of the underlying distribution (for a more detailed explanation, see Imbens and Wooldridge (2008)).

Table 3. Difference in Mean Pre-Period Trends by Treatment Group Status

	Mean *Published=0*	**SD** *Published=0*	**Mean** *Published=1*	**SD** *Published=1*	**Sample Mean**	**Normalized Difference**	**Level Difference**
Total Crime	17.052	385.247	-31.012	202.38	4.564	-0.11	-48.065
Total Violent Crime	8.975	136.032	5.285	71.683	8.016	-0.024	-3.69
Total Property Crime	8.077	309.101	-36.297	187.879	-3.452	-0.122	-44.375
Murder	-0.596	6.296	0.158	1.961	-0.4	0.114	0.754
Total Rape	-0.188	8.212	-1.407	9.889	-0.505	-0.094	-1.219
Total Assault	12.581	127.608	9.623	61.864	11.812	-0.021	-2.957
Total Robbery	-2.822	25.681	-3.089	23.772	-2.891	-0.008	-0.267
Total Theft	-6.798	135.151	-11.772	94.183	-8.09	-0.03	-4.973
Total Burglary	21.29	236.247	-17.542	146.698	11.201	-0.138	-38.832
Total Auto Theft	-6.414	36.51	-6.984	41.295	-6.562	-0.01	-0.569
Total Population	2861.787	10646.7	4749.722	17709.32	3352.308	0.091	1887.935
Unemployment	4.021	1.31	4.282	1.342	4.089	0.138	0.261
No. Agencies	218	218	76	76	294	294	294
N	17640	17640	6192	6192	23832	23832	23832

Notes: *Published* is a dummy variable equal to one if the county in which the agency resides was ever included in the WRAL database. Categories are crime rates created from the FBI's UCR crime data (2005 through June 2012). All rates are per 100,000 people, and the table reports means of each variable. Standard deviations are in parentheses. "Normalized difference" adjusts the difference between mean values for the treatment and control groups by altering the standard t-statistic to not depend on the sample size of the underlying distribution (for a more detailed explanation, see Imbens and Wooldridge (2008)). The "Mean" category should be interpreted as the mean change in the crime rate between the first and last month of the pre-period for treatment and control groups. For instance, the first entry on the top left indicates that the mean change over the pre-period in total crimes for all control agencies was an increase by approximately 17.052 crimes per 100,000 people. Number of observations and agencies differ from similar tables because the sample from which this table is drawn includes only agencies who report in both 2005 and 2012 (as this is how the difference over the pre-period is calculated).

Table 4. Main Results by Treatment Group Status and Model Robustness

	(1)	(2)	(3)	(4)
Total Crime	0.0208	0.0169	-0.0107	0.0210
	(0.0318)	(0.0324)	(0.0420)	(0.0389)
Total Violent Crime	0.00316	-0.000203	-0.00722	0.0336
	(0.0453)	(0.0464)	(0.0545)	(0.0688)
Total Property Crime	0.0370	0.0339	-0.00929	0.0193
	(0.0358)	(0.0362)	(0.0437)	(0.0366)
Murder	-0.000525	0.0123	-0.00521	-0.00950
	(0.0469)	(0.0508)	(0.0149)	(0.0304)
Total Rape	-0.00924	-0.00682	0.000841	-0.0624*
	(0.0357)	(0.0363)	(0.0180)	(0.0326)
Total Assault	-0.00834	-0.0102	-0.000353	0.0399
	(0.0504)	(0.0515)	(0.0551)	(0.0737)
Total Robbery	0.00714	0.00661	-0.00666	-0.0224
	(0.0630)	(0.0642)	(0.0329)	(0.0526)
Total Theft	0.00988	0.00613	0.0256	0.0299
	(0.0350)	(0.0348)	(0.0394)	(0.0360)
Total Burglary	0.0699	0.0684	-0.0245	0.0371
	(0.0546)	(0.0560)	(0.0549)	(0.0602)
Total Auto Theft	0.112*	0.110	-0.0653	-0.0582
	(0.0652)	(0.0665)	(0.0435)	(0.0597)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	352	352	352	352
N	32916	32916	32916	32916

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each entry represents the coefficient from a separate OLS regression, which regresses the IHS of various crime rates on the difference-in-differences term explained in Equation (1). All models control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the county-level in all models, are in parentheses.

Table 5. Descriptive Statistics by Permit Concentration

	Means and SD's: Low Concentration	Means and SD's: Mid-Concentration	Means and SD's: High Concentration
Total Crime	480.869	619.328	533.846
	(225.305)	(278.312)	(311.134)
Total Violent Crime	130.382	135.664	135.216
	(63.511)	(80.872)	(117.515)
Total Property Crime	350.487	483.665	398.630
	(170.775)	(221.811)	(232.088)
Murder	0.541	0.890	0.415
	(0.949)	(3.070)	(4.706)
Total Rape	2.094	2.009	2.278
	(2.041)	(5.390)	(9.710)
Total Assault	111.132	116.481	119.121
	(56.174)	(73.664)	(108.886)
Total Robbery	16.615	16.284	13.402
	(10.987)	(16.297)	(21.244)
Total Theft	236.140	333.922	269.407
	(108.869)	(155.697)	(170.730)
Total Burglary	92.999	128.017	112.731
	(56.754)	(83.462)	(103.087)
Total Auto Theft	48.220	4.387	0.969
	(34.816)	(4.838)	(1.272)
Average Population	213,831.700	19,144.55	7,103.964
	(138,953.800)	(11,365.520)	(5277.896)
Unemployment	6.690	7.944	7.220
	(2.471)	(3.083)	(2.628)
No. Agencies	23	21	23
N	2148	2052	1992

Notes: The above table shows means and standard deviations for crime rates, average agency population over the panel, and unemployment rates (measured at the county-year-month level), for agencies with low, mid, and high concentrations of concealed carry permits as of July, 2012. Crime rates are normalized for a population of 100,000 people and created from the FBI's UCR crime data (2005 through June 2012). All statistics are weighted by an agency's average population over the panel. Unlike tables that follow, these descriptive statistics indicate means for the entire sample rather than just the pre period. Standard deviations are in parentheses.

Table 6. Difference in Mean Pre-Period Trends for High versus Mid-Concentration Agencies

	Mean *permit_HC=0*	SD *permit_HC=0*	Mean *permit_HC=1*	SD *permit_HC=1*	Sample Mean	Normalized Difference	Level Difference
Total Crime	-49.689	227.205	-73.209	288.988	-61.313	-0.064	-23.52
Total Violent Crime	-9.751	83.38	1.833	87.598	-4.026	0.095	11.584
Total Property Crime	-39.938	205.669	-75.043	279.517	-57.286	-0.101	-35.104
Murder	0.375	1.789	0	0	0.19	-0.205	-0.375
Total Rape	-0.51	12.112	-5.162	14.46	-2.809	-0.239	-4.652
Total Assault	-0.835	71.372	7.365	74.936	3.217	0.079	8.2
Total Robbery	-8.781	26.264	-0.37	31.015	-4.624	0.203	8.411
Total Theft	-1.767	96.983	-11.471	126.819	-6.562	-0.061	-9.704
Total Burglary	-34.197	161.5	-42.22	229.205	-38.162	-0.029	-8.023
Total Auto Theft	-3.975	33.664	-21.352	71.369	-12.562	-0.215	-17.377
Total Population	1157.563	3508.749	550.016	1031.382	857.322	-0.164	-607.548
Unemployment	4.23	1.287	4.422	1.306	4.325	0.104	0.192
No. Agencies	21	21	23	23	44	44	44
N	1566	1566	1530	1530	1566	1530	1530

Notes: *permit_HC* is a dummy variable equal to one if the city in which the agency resides is in the highest third of the distribution for gun permits per capita at the time of treatment. Otherwise, the observations are for the middle third of this distribution (low concentration agencies are omitted for this table). Categories are crime rates created from the FBI's UCR crime data (2005 through June 2012). All rates are per 100,000 people, and the table reports means of each variable. Standard deviations are in parentheses. "Normalized difference" adjusts the difference between mean values for the treatment and control groups by altering the standard t-statistic to not depend on the sample size of the underlying distribution (for a more detailed explanation, see Imbens and Wooldridge (2008)). The "Mean" category should be interpreted as the mean change in the crime rate per 100,000 people between the first and last month of the pre-period for treatment and control groups. Number of observations and agencies differ from similar tables because the sample from which this table is drawn includes only agencies who report in both 2005 and 2012 (as this is how the difference over the pre-period is calculated).

Table 7. Difference in Mean Pre-Period Trends for Low versus Mid-Concentration Agencies

	Mean *permit_LC=0*	**SD** *permit_LC=0*	**Mean** *permit_LC=1*	**SD** *permit_LC=1*	**Sample Mean**	**Normalized Difference**	**Level Difference**
Total Crime	-49.689	227.205	-25.883	143.300	-37.972	0.088	23.806
Total Violent Crime	-9.751	83.380	15.164	64.807	2.513	0.230	24.914
Total Property Crime	-39.938	205.669	-41.047	126.256	-40.484	-0.005	-1.109
Murder	0.375	1.789	0.224	2.277	0.301	-0.052	-0.151
Total Rape	-0.510	12.112	-0.622	3.589	-0.565	-0.009	-0.112
Total Assault	-0.835	71.372	17.952	55.239	8.412	0.204	18.787
Total Robbery	-8.781	26.264	-2.391	23.645	-5.636	0.178	6.390
Total Theft	-1.767	96.983	-28.011	93.899	-14.684	-0.191	-26.244
Total Burglary	-34.197	161.500	-13.259	67.234	-23.891	0.119	20.938
Total Auto Theft	-3.975	33.664	0.223	15.980	-1.909	0.112	4.198
Total Population	1157.563	3508.749	16419.700	26747.500	8669.862	0.492	15262.140
Unemployment	4.230	1.287	4.008	1.415	4.120	-0.115	-0.222
No. Agencies	21	21	23	23	44	44	44
N	1566	1566	1518	1518	3084	3084	3084

Notes: *permit_LC* is a dummy variable equal to one if the city in which the agency resides is in the lowest third of the distribution for gun permits per capita at the time of treatment. Otherwise, the observations are for the middle third of this distribution (high concentration agencies are omitted for this table). Categories are crime rates created from the FBI's UCR crime data (2005 through June 2012). All rates are per 100,000 people, and the table reports means of each variable. Standard deviations are in parentheses. "Normalized difference" adjusts the difference between mean values for the treatment and control groups by altering the standard t-statistic to not depend on the sample size of the underlying distribution (for a more detailed explanation, see Imbens and Wooldridge (2008)). The "Mean" category should be interpreted as the mean change in the crime rate per 100,000 people between the first and last month of the pre-period for treatment and control groups. Number of observations and agencies differ from similar tables because the sample from which this table is drawn includes only agencies who report in both 2005 and 2012 (as this is how the difference over the pre-period is calculated).

Table 8. Main Results by Permit Concentration, Total Crime Categories

	(1) Total Crime	(2) Total Violent	(3) Total Property
High Concentration	0.152	0.0703	0.175*
	(0.102)	(0.144)	(0.0934)
Low Concentration	0.0786	0.0154	0.0902
	(0.0988)	(0.124)	(0.0914)
Agency-Month F.E.'s	X	X	X
Population Weighted	X	X	X
Time Trends	X	X	X
No. Agencies	67	67	67
R-Squared	0.310	0.175	0.296
N	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of total crime rates categories on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics, include agency-by-month fixed effects as well as agency-specific linear time trends, and are weighted by population. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table 9. Main Results by Permit Concentration, Violent Crime Categories

	(1) Murder	(2) Total Rape	(3) Total Assault	(4) Total Robbery
High Concentration	0.000513	-0.0432	0.0708	0.00259
	(0.0388)	(0.0535)	(0.152)	(0.112)
Low Concentration	0.0731*	-0.0736	0.0140	-0.0140
	(0.0429)	(0.0973)	(0.130)	(0.0955)
Agency-Month F.E.'s	X	X	X	X
Population Weighted	X	X	X	X
Time Trends	X	X	X	X
No. Agencies	67	67	67	67
R-Squared	0.0932	0.0880	0.145	0.221
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of different property crime rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics, include agency-by-month fixed effects as well as agency-specific linear time trends, and are weighted by population. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table 10. Main Results by Permit Concentration, Property Crime Categories

	(1) Total Burglary	(2) Total Theft	(3) Total Auto Theft
High Concentration	0.130	0.178*	-0.0159
	(0.119)	(0.0984)	(0.122)
Low Concentration	0.0749	0.0763	0.0930
	(0.114)	(0.0984)	(0.0987)
Agency-Month F.E.'s	X	X	X
Population Weighted	X	X	X
Time Trends	X	X	X
No. Agencies	67	67	67
R-Squared	0.161	0.272	0.279
N	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of different property crime rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics, include agency-by-month fixed effects as well as agency-specific linear time trends, and are weighted by population. Standard errors, clustered at the agency level in all regressions, are in parentheses.

APPENDICES

APPENDIX A

SUPPLEMENTARY TABLES

Table A.1 Descriptive Statistics by Concealed Carry Permit Concentration, Pre-Period

	Mean	SD	Mean *permit_HC=1*	SD *permit_HC=1*	Sample Mean	Normalized Difference	Level Difference
Total Crime	508.181	277.498	533.200	328.076	520.617	0.058	25.020
Total Violent Crime	126.011	91.647	117.605	113.225	121.833	-0.058	-8.406
Total Property Crime	382.169	217.746	415.595	265.737	398.785	0.097	33.426
Murder	0.506	3.282	0.704	6.467	0.605	0.027	0.198
Total Rape	2.087	6.744	2.726	15.282	2.404	0.038	0.639
Total Assault	110.215	84.442	101.156	104.011	105.712	-0.067	-9.058
Total Robbery	13.204	18.982	13.019	26.600	13.112	-0.006	-0.184
Total Theft	261.934	158.728	279.799	204.497	270.814	0.069	17.865
Total Burglary	103.316	87.441	117.908	116.323	110.570	0.100	14.592
Total Auto Theft	16.919	19.625	17.888	34.183	17.401	0.025	0.969
Average Population	44506.210	82649.470	5322.245	5862.172	25028.540	-0.427	-39183.970
Unemployment	7.324	3.012	8.024	3.214	7.672	0.157	0.699
No. Agencies	33	33	34	34	67	67	67
N	2586	2586	2556	2556	5142	5142	5142

Notes: Permit_HC is a dummy variable equal to one if the city in which the agency resides is above the median for gun permits per capita at the time of treatment. Otherwise, the observations are for agencies below the median. Categories are crime rates, normalized for a population of 100,000 people, created from the FBI's UCR crime data (2005 through June 2012). All statistics are unweighted, and the table reports means of each variable. Standard deviations are in parentheses. "Normalized difference" adjusts the difference between mean values for the treatment and control groups by altering the standard t-statistic to not depend on the sample size of the underlying distribution (for a more detailed explanation, see Imbens and Wooldridge (2008
)).

Table A.2 Descriptive Statistics for High versus Mid-Concentration Agencies, Pre-Period

	Mean	SD	Mean *permit_HC=1*	SD *permit_HC=1*	Sample Mean	Normalized Difference	Level Difference
Total Crime	588.133	(295.770)	503.682	(322.183)	545.908	-0.190	-84.451
Total Violent Crime	139.809	(97.033)	109.240	(117.191)	124.524	-0.197	-30.569
Total Property Crime	448.325	(236.159)	394.442	(263.908)	421.383	-0.150	-53.883
Murder	0.787	(4.626)	0.604	(7.160)	0.695	-0.021	-0.182
Total Rape	2.477	(7.942)	2.693	(17.751)	2.585	0.011	0.216
Total Assault	121.536	(88.861)	93.872	(108.035)	107.704	-0.194	-27.664
Total Robbery	15.010	(20.782)	12.071	(28.979)	13.540	-0.082	-2.939
Total Theft	296.179	(170.226)	271.395	(212.330)	283.787	-0.091	-24.784
Total Burglary	132.503	(101.631)	105.803	(118.366)	119.153	-0.169	-26.700
Total Auto Theft	19.643	(23.419)	17.244	(38.136)	18.443	-0.054	-2.399
Average Population	10701.320	(9556.780)	3363.437	(3527.910)	7032.377	-0.584	-7337.881
Unemployment	8.105	(3.248)	7.647	(3.025)	7.876	-0.103	-0.458
No. Agencies	21	21	23	23	44	44	44
N	1698	1698	1698	1698	3396	3396	3396

Notes: *Permit_HC* is a dummy variable equal to one if the city in which the agency resides is in the top third of the distribution for gun permits per capita at the time of treatment. Otherwise, the observations are for the middle third of this distribution (low concentration agencies are omitted for this table). Categories are crime rates, normalized for a population of 100,000 people, created from the FBI's UCR crime data (2005 through June 2012). All statistics are unweighted, and the table reports means of each variable. Standard deviations are in parentheses. "Normalized difference" adjusts the difference between mean values for the treatment and control groups by altering the standard t-statistic to not depend on the sample size of the underlying distribution (for a more detailed explanation, see Imbens and Wooldridge (2008)).

Table A.3 Descriptive Statistics for Low versus Mid-Concentration Agencies, Pre-Period

	Mean	SD	Mean *Permit_LC=1*	SD *Permit_LC=1*	Sample Mean	Normalized Difference	Level Difference
Total Crime	588.133	(295.770)	471.428	(281.025)	528.967	-0.275	-116.706
Total Violent Crime	139.809	(97.033)	116.598	(90.866)	128.041	-0.172	-23.211
Total Property Crime	448.325	(236.159)	354.830	(219.424)	400.926	-0.279	-93.494
Murder	0.787	(4.626)	0.428	(2.548)	0.605	-0.068	-0.358
Total Rape	2.477	(7.942)	2.053	(6.451)	2.262	-0.041	-0.423
Total Assault	121.536	(88.861)	101.837	(83.896)	111.549	-0.159	-19.699
Total Robbery	15.010	(20.782)	12.279	(18.106)	13.625	-0.099	-2.731
Total Theft	296.179	(170.226)	245.582	(159.691)	270.528	-0.212	-50.597
Total Burglary	132.503	(101.631)	93.875	(82.804)	112.920	-0.283	-38.628
Total Auto Theft	19.643	(23.419)	15.373	(18.038)	17.478	-0.143	-4.269
Average Population	10701.320	(9556.780)	60031.370	(96525.840)	35710.110	0.453	49330.050
Unemployment	8.105	(3.248)	7.275	(3.070)	7.684	-0.182	-0.829
No. Agencies	21	21	23	23	44	44	44
N	1698	1698	1698	1698	3396	3396	3396

Notes: *Permit_LC* is a dummy variable equal to one if the city in which the agency resides is in the lowest third of the distribution for gun permits per capita at the time of treatment. Otherwise, the observations are for the middle third of this distribution (high concentration agencies are omitted for this table). Categories are crime rates, normalized for a population of 100,000 people, created from the FBI's UCR crime data (2005 through June 2012). All statistics are unweighted, and the table reports means of each variable. Standard deviations are in parentheses. "Normalized difference" adjusts the difference between mean values for the treatment and control groups by altering the standard t-statistic to not depend on the sample size of the underlying distribution (for a more detailed explanation, see Imbens and Wooldridge (2008)).

Table A.4 Model Robustness by Permit Concentration, Total Crime

	(1)	(2)	(3)	(4)
High Concentration	0.137	0.153	0.220**	0.152
	(0.112)	(0.115)	(0.0878)	(0.102)
Low Concentration	0.0196	0.0252	0.0627	0.0786
	(0.108)	(0.112)	(0.0833)	(0.0988)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.296	0.215	0.148	0.310
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of total crime rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.5 Model Robustness by Permit Concentration, Total Violent Crime

	(1)	(2)	(3)	(4)
High Concentration	0.0745	0.0793	0.0545	0.0703
	(0.131)	(0.136)	(0.119)	(0.144)
Low Concentration	-0.0313	-0.0327	0.0342	0.0154
	(0.127)	(0.132)	(0.116)	(0.124)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.150	0.109	0.0898	0.175
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of violent crime rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.6 Model Robustness by Permit Concentration, Total Property Crime

	(1)	(2)	(3)	(4)
High Concentration	0.157	0.172	0.250***	0.175*
	(0.108)	(0.112)	(0.0863)	(0.0934)
Low Concentration	0.0326	0.0383	0.0666	0.0902
	(0.105)	(0.108)	(0.0777)	(0.0914)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.281	0.193	0.139	0.296
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of property crime rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.7 Model Robustness by Permit Concentration, Murder

	(1)	(2)	(3)	(4)
High Concentration	-0.0241	-0.0169	-0.0237	0.000513
	(0.0602)	(0.0589)	(0.0226)	(0.0388)
Low Concentration	-0.0790	-0.0731	0.0232	0.0731*
	(0.0750)	(0.0742)	(0.0318)	(0.0429)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.0929	0.0689	0.155	0.0932
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of murder rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.8 Model Robustness by Permit Concentration, Total Rape

	(1)	(2)	(3)	(4)
High Concentration	-0.0373	-0.0237	-0.0288	-0.0432
	(0.0632)	(0.0643)	(0.0323)	(0.0535)
Low Concentration	-0.118*	-0.120*	-0.0184	-0.0736
	(0.0625)	(0.0672)	(0.0398)	(0.0973)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.0918	0.0615	0.129	0.0880
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of rape rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.9 Model Robustness by Permit Concentration, Total Assault

	(1)	(2)	(3)	(4)
High Concentration	0.0905	0.0865	0.0467	0.0708
	(0.138)	(0.142)	(0.119)	(0.152)
Low Concentration	-0.0312	-0.0374	0.0472	0.0140
	(0.134)	(0.139)	(0.122)	(0.130)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.120	0.0747	0.0868	0.145
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of assault rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.10 Model Robustness by Permit Concentration, Total Robbery

	(1)	(2)	(3)	(4)
High Concentration	-0.0520	-0.00550	-0.0154	0.00259
	(0.122)	(0.123)	(0.0813)	(0.112)
Low Concentration	-0.0894	-0.0776	0.0156	-0.0140
	(0.0795)	(0.0735)	(0.0750)	(0.0955)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.208	0.181	0.114	0.221
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of robbery rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.11 Model Robustness by Permit Concentration, Total Burglary

	(1)	(2)	(3)	(4)
High Concentration	0.157	0.155	0.199	0.130
	(0.114)	(0.115)	(0.129)	(0.119)
Low Concentration	0.0941	0.101	0.0441	0.0749
	(0.0940)	(0.0944)	(0.124)	(0.114)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.161	0.0940	0.0873	0.161
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of burglary rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.12 Model Robustness by Permit Concentration, Total Theft

	(1)	(2)	(3)	(4)
High Concentration	0.159	0.175	0.202**	0.178*
	(0.117)	(0.121)	(0.0920)	(0.0984)
Low Concentration	0.00170	0.00357	0.0543	0.0763
	(0.116)	(0.119)	(0.0931)	(0.0984)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.226	0.149	0.142	0.272
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of theft rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.13 Model Robustness by Permit Concentration, Total Auto Theft

	(1)	(2)	(3)	(4)
High Concentration	0.0677	0.105	-0.0547	-0.0159
	(0.0771)	(0.0782)	(0.0853)	(0.122)
Low Concentration	0.0686	0.0775	0.0804	0.0930
	(0.0568)	(0.0552)	(0.0869)	(0.0987)
Basic F.E.'s	X			
Agency-Month F.E.'s		X	X	X
Population Weighted	X	X		X
Time Trends			X	X
No. Agencies	67	67	67	67
R-Squared	0.258	0.239	0.158	0.279
N	6192	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. Each column represents a separate regression for different crime rates, all of which regress the IHS of auto theft rates on the difference-in-differences term explained in Equation (2). The omitted category is the middle third of the distribution of permits per capita. All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. The first column represents a model with basic year, month, and agency fixed effects, weighted by population. The second column represents a model with the preferred year and agency-by-month fixed effects as in Equation (1), weighted by population, but with no time trends. The third column represents a model that shows my preferred specification, but without weighting by population. The fourth column represents estimates from my preferred specification. Standard errors, clustered at the agency level in all regressions, are in parentheses.

Table A.14 Permit Applications by Treatment Status

	(1) Preferred Specification
Three Months Pre-Treatment	-0.0427
	(0.0510)
Two Months Pre-Treatment	0.0915*
	(0.0508)
One Month Pre-Treatment	-0.0509
	(0.0390)
Treatment Post Period	0.181***
	(0.0618)
R-Squared	0.533
N	24744

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the North Carolina State Bureau of Investigations on applications for concealed carry permits. The model above represents a regression of my preferred specification (year and agency-by-month fixed effects, weighting by population, and agency-specific linear time trends) with the normalized rate of total permits applied for as the dependent variable. This regression controls for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. Standard errors, clustered at the county level, are in parentheses.

Table A.15 Permit Application by Treatment Status, Poisson

	(1) No Time Trends
Three Months Pre-Treatment	0.0248
	(0.114)
Two Months Pre-Treatment	0.157*
	(0.0922)
One Month Pre-Treatment	0.0717
	(0.0597)
Treatment Post Period	0.380***
	(0.107)
R-Squared	0.916
N	23382

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the North Carolina State Bureau of Investigations on applications for concealed carry permits. The model above represents a Poisson version of the regression run in Table 19, with total permits applied for as the dependent variable. Due to convergence issues, this Poisson model does not include agency-specific linear time trends. This regression controls for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. Standard errors, clustered at the county level, are in parentheses.

Table A.16 Permit Application by Demographic Characteristics

	(1) Black	(2) White	(3) Other	(4) Male	(5) Female
Treatment Post Period	0.184**	0.188***	0.140	0.184***	0.199**
	(0.0719)	(0.0591)	(0.102)	(0.0547)	(0.0825)
R-Squared	0.316	0.539	0.385	0.491	0.597
N	24744	24744	24744	24744	24744

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the North Carolina State Bureau of Investigations on applications for concealed carry permits. The model above represents a regression of my preferred specification (year and agency-by-month fixed effects, weighting by population, and agency-specific linear time trends) with the normalized rates of various subcategories of total permits applied for as the dependent variables. Thus, each column represents a separate regression with the same model, but different dependent variables. Total permits can either be calculated as the sum of the race categories (black, white, and other) or the gender categories (male and female). All regressions control for county-level monthly unemployment, obtained from the Bureau of Labor Statistics. Standard errors, clustered at the county level, are in parentheses.

Table A.17 Total Crime Categories, First D-D Poisson Robustness Check

	(1) Total Crime	(2) Total Violent	(3) Total Property
Treatment Post Period	-0.0238	-0.00903	-0.0279
	(0.0251)	(0.0328)	(0.0257)
R-Squared	0.994	0.992	0.991
N	33024	33024	33024

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. The above represents results from three separate regressions on total crime categories. The model is a Poisson version of Equation (1), which includes agency-specific linear time trends, but with basic agency, year, and month fixed effects. Standard errors, clustered at the county level in all regressions, are in parentheses.

Table A.18 Total Crime Categories, Second D-D Poisson Robustness Check

	(1) Total Crime	(2) Total Violent	(3) Total Property
High Concentration	0.0201	-0.0472	0.0383
	(0.0525)	(0.0936)	(0.0494)
Low Concentration	-0.00400	-0.0278	-0.00199
	(0.0386)	(0.0554)	(0.0395)
R-Squared	0.993	0.988	0.990
N	6192	6192	6192

Notes: * Indicates a coefficient is statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Based on data from the FBI's *Uniform Crime Reporting* datasets. The above represents results from three separate regressions on total crime categories. The model is a Poisson version of Equation (2), which includes agency-specific linear time trends, but with basic agency, year, and month fixed effects. Standard errors, clustered at the agency level in all regressions, are in parentheses.

APPENDIX B

DEMOGRAPHIC MAPS

Figure B.1 Median Income by County (2012 Census)

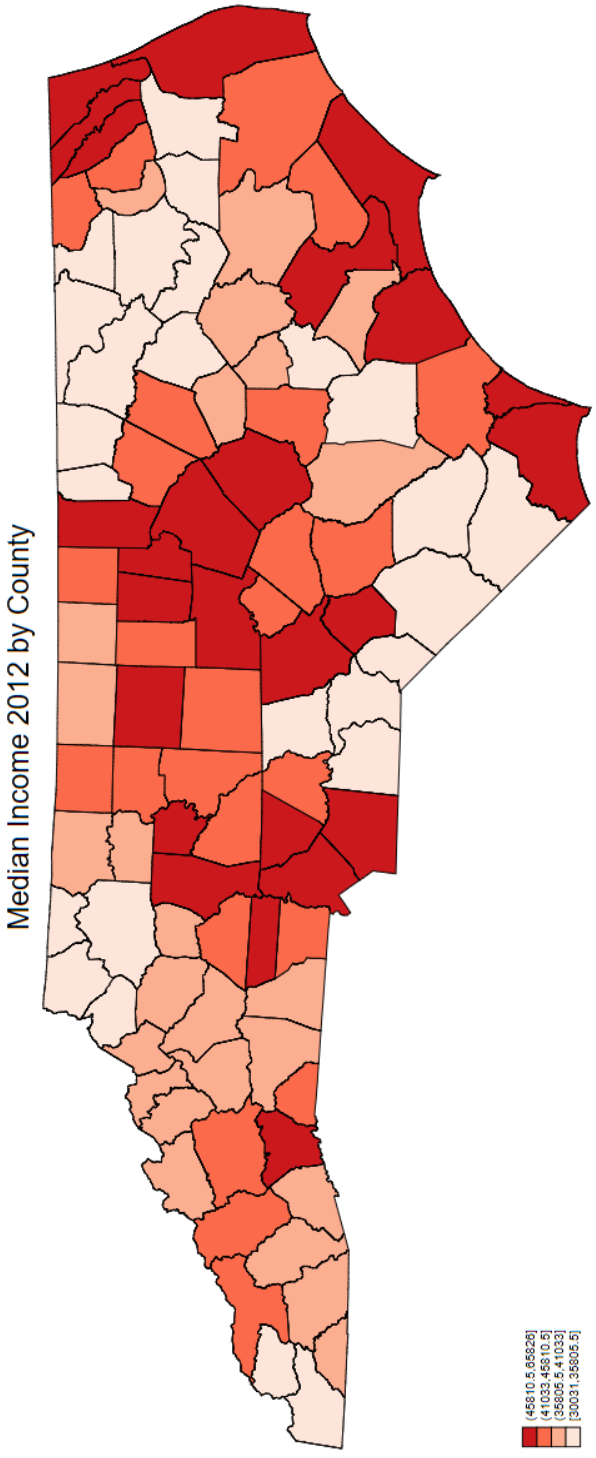


Figure B.2 Mean Population over Panel (2005-2013) by County

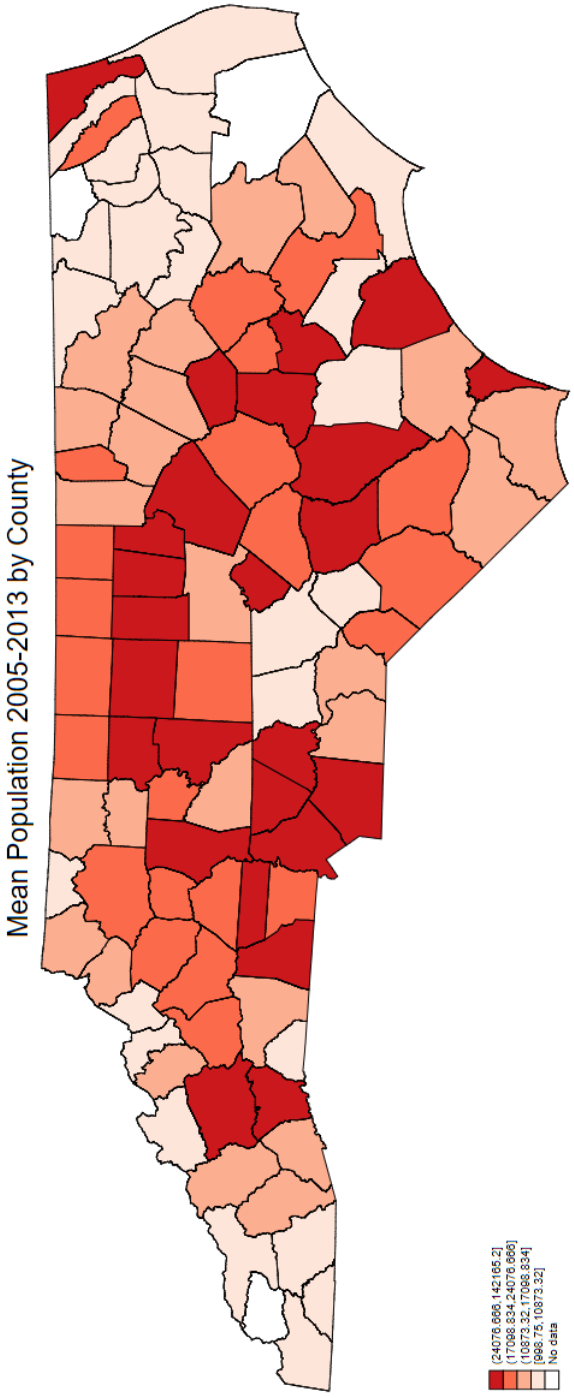


Figure B.3 Percent of White Citizens by County (2010 Census)

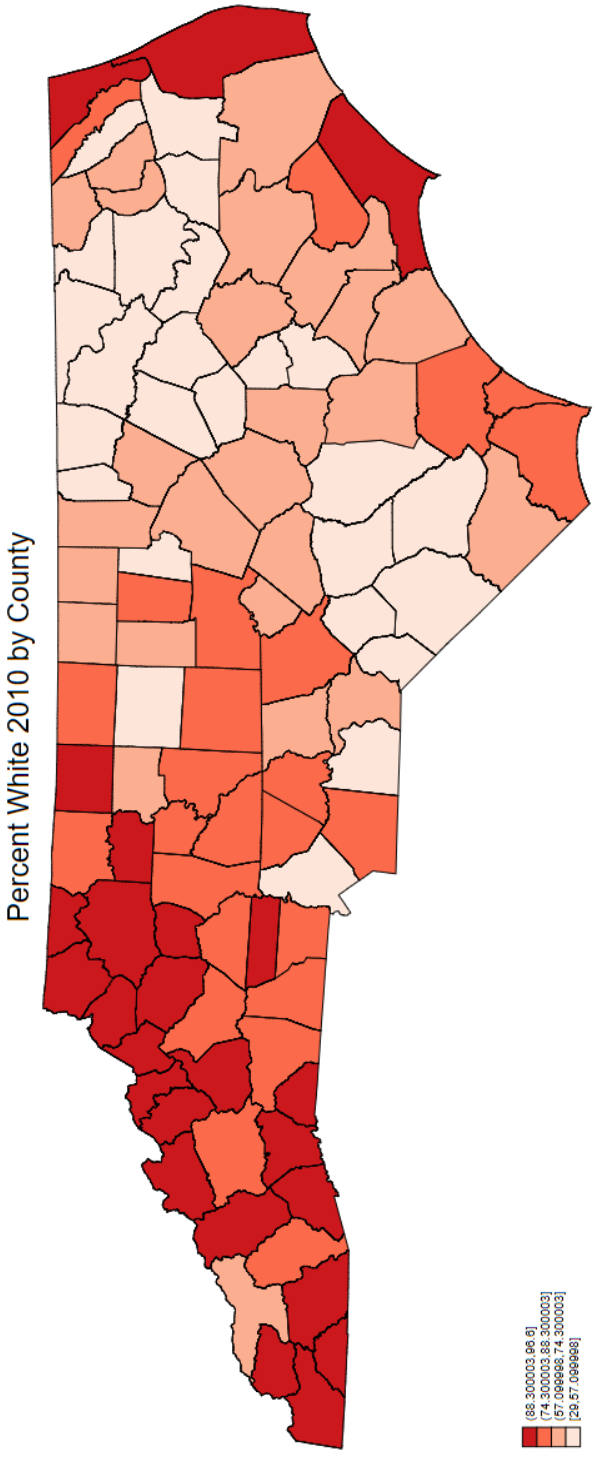


Figure B.4 Percent of Black Citizens by County (2010 Census)

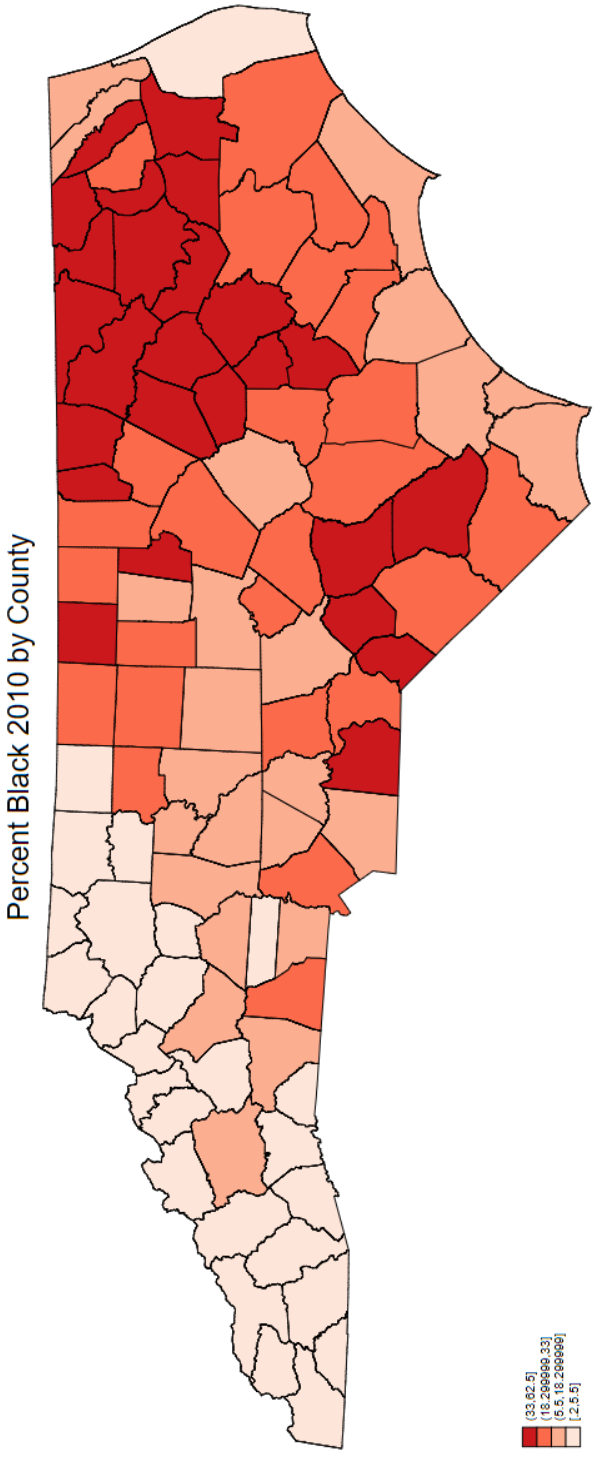
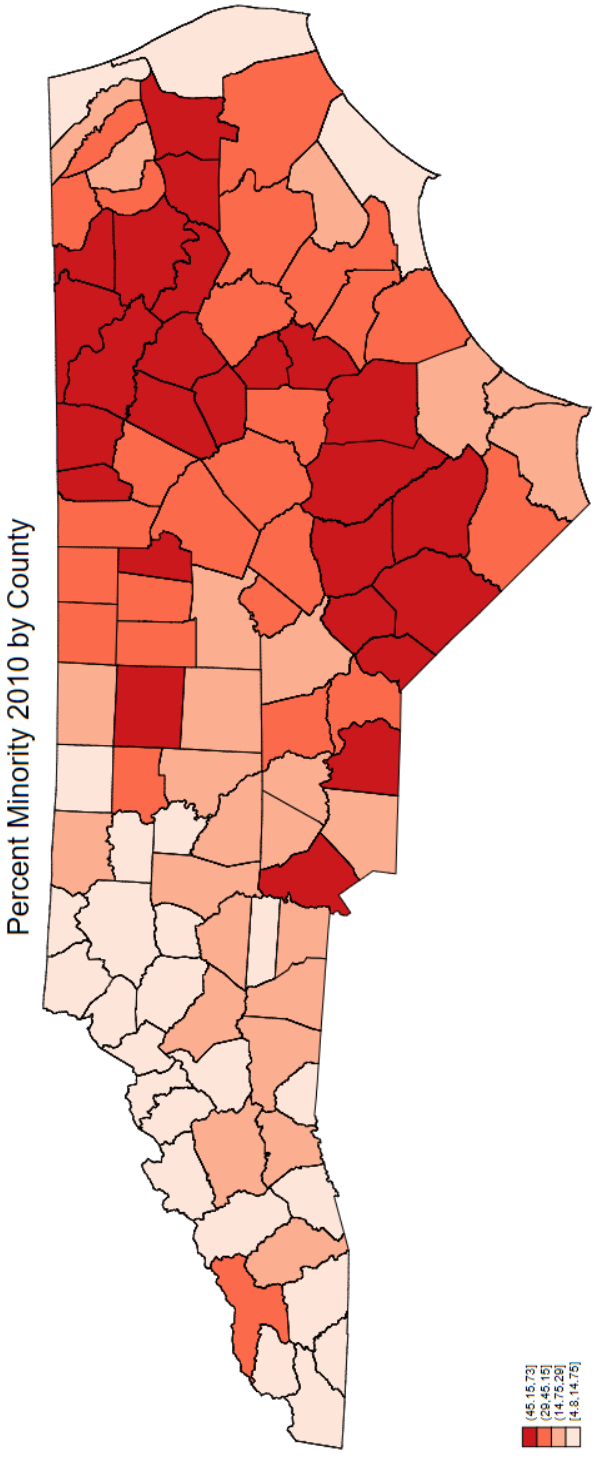


Figure B.5 Percent of All Minority (Non-White) Citizens by County (2010 Census)



APPENDIX C

RESIDUAL PLOTS FOR NORMALIZED CRIME RATES, YEAR AND MONTH FIXED EFFECTS

Figure C.1 Total Violent Crime Residual Plot

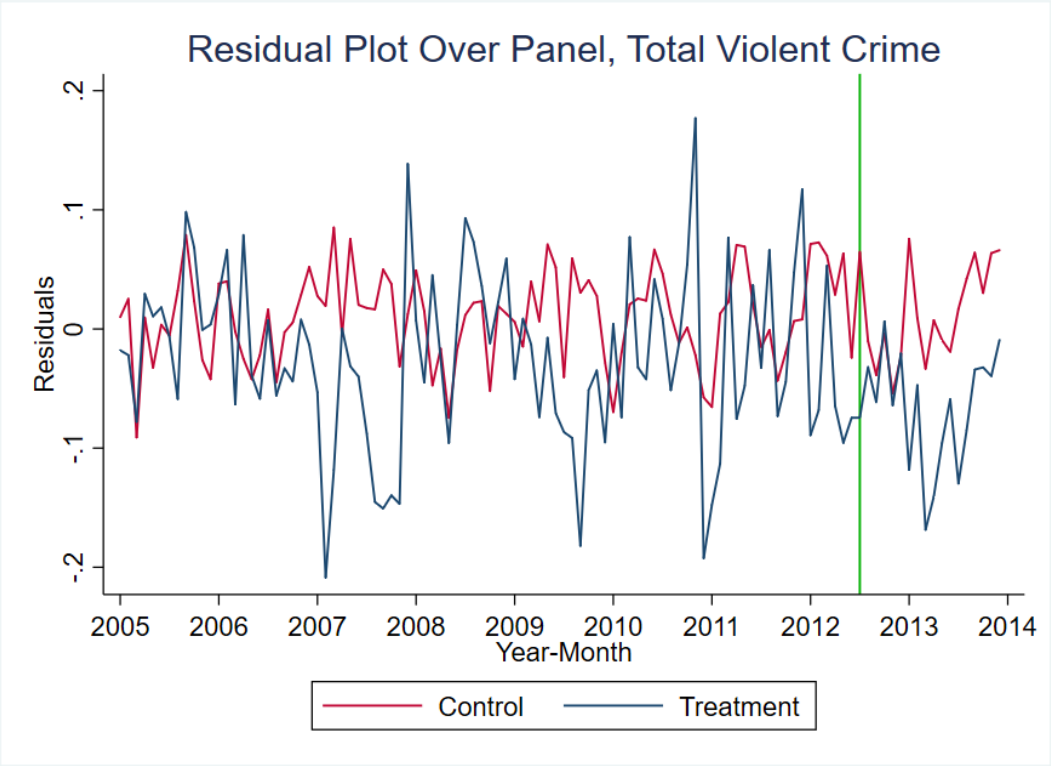


Figure C.2 Total Property Crime Residual Plot

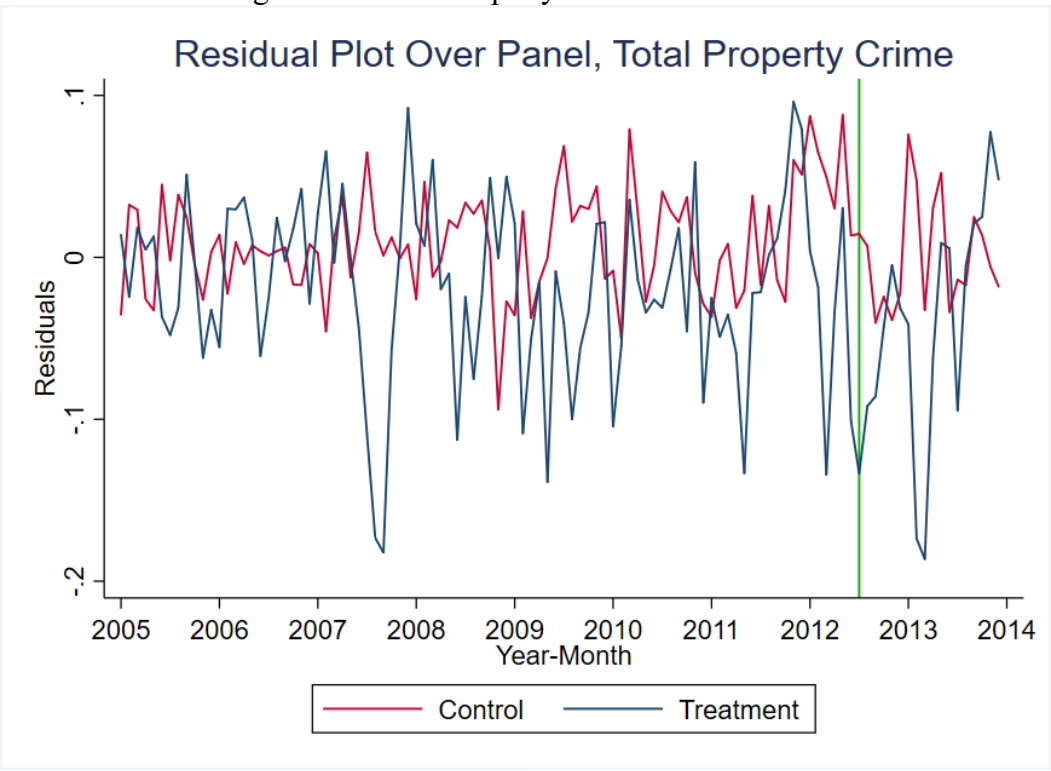


Figure C.3 Murder Residual Plot

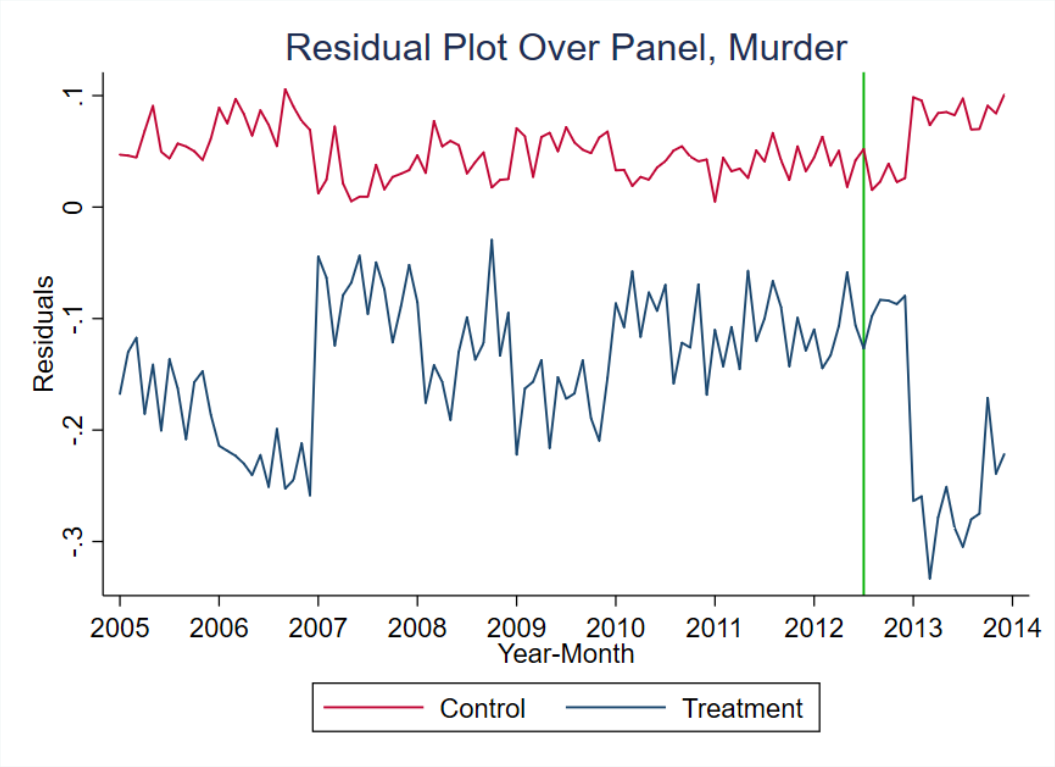


Figure C.4 Total Rape Residual Plot

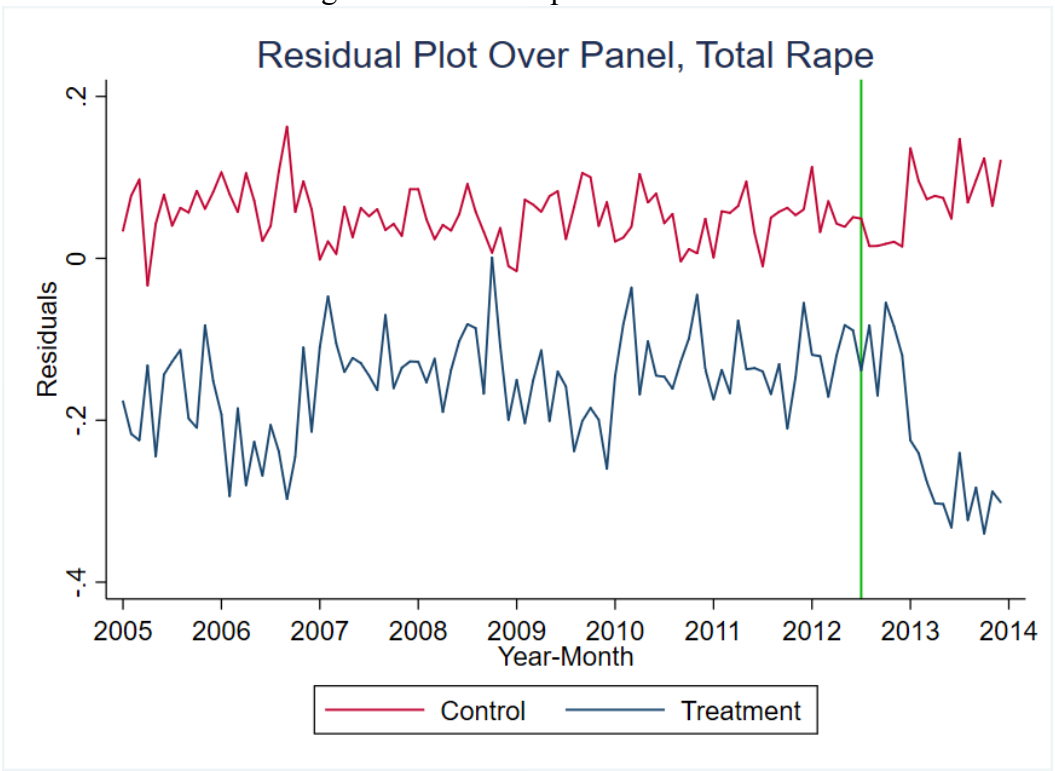


Figure C.5 Total Assault Residual Plot

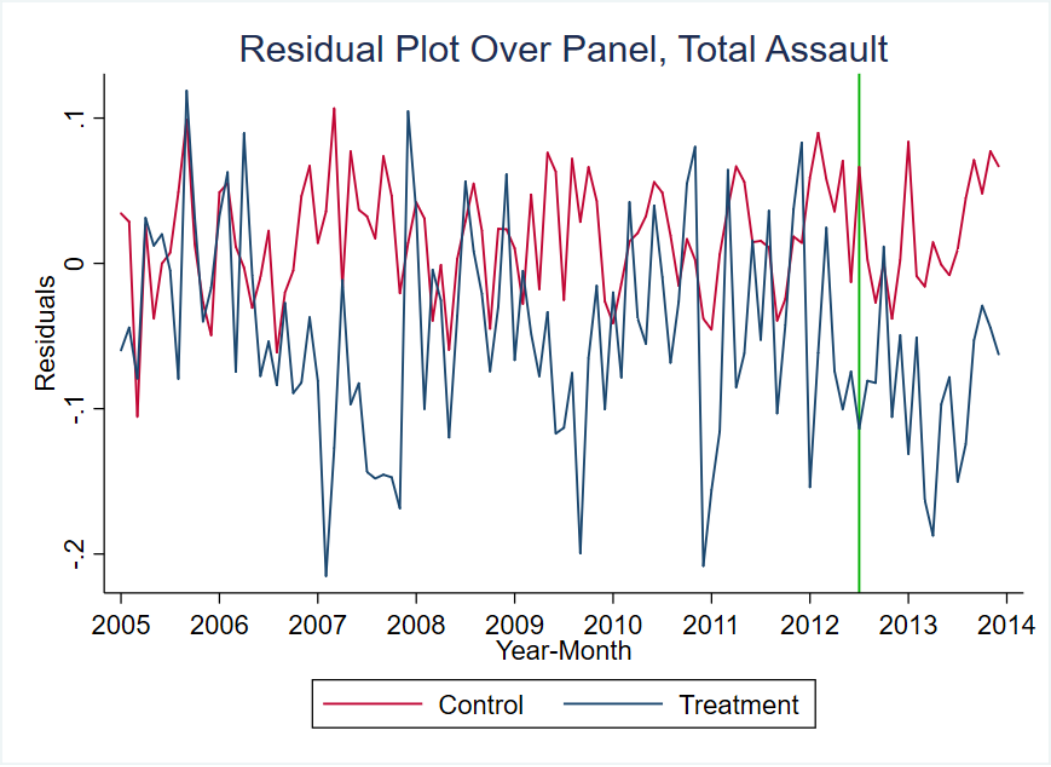


Figure C.6 Total Robbery Residual Plot

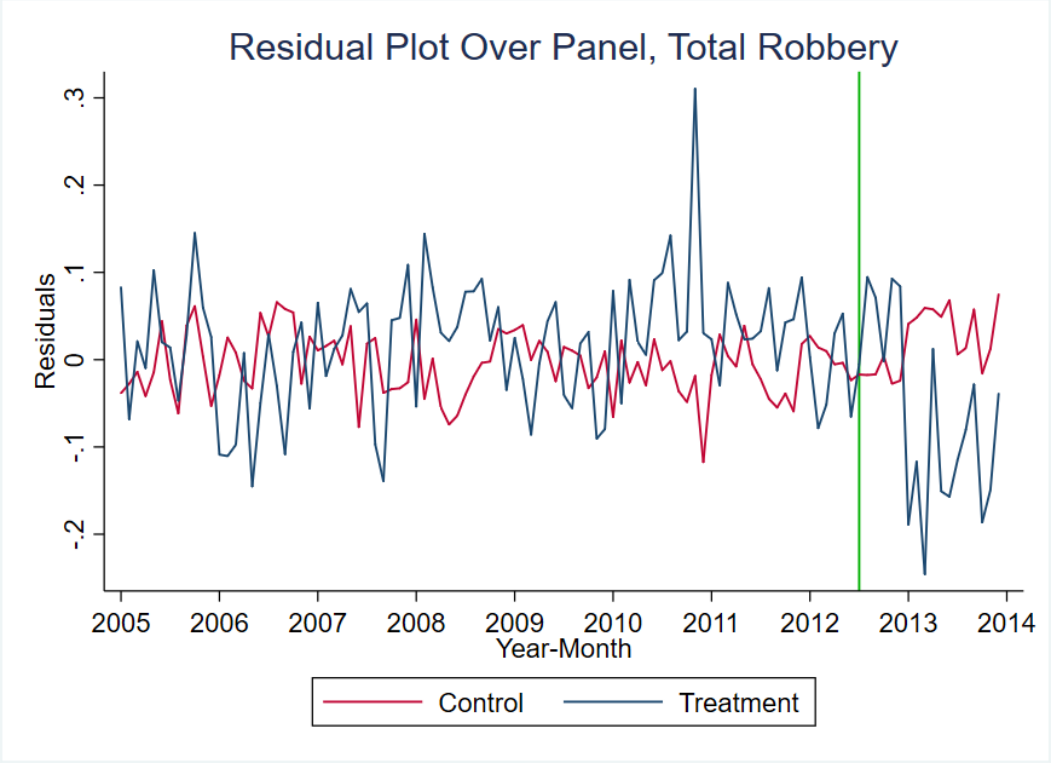


Figure C.7 Total Theft Residual Plot

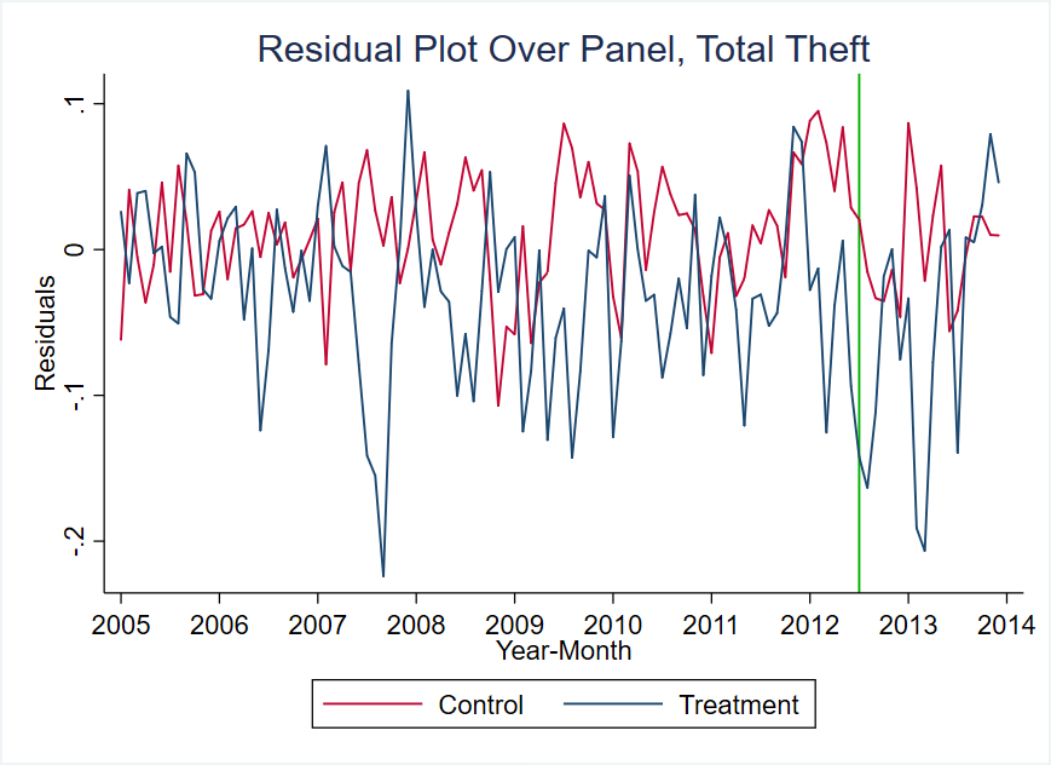


Figure C.8 Total Burglary Residual Plot

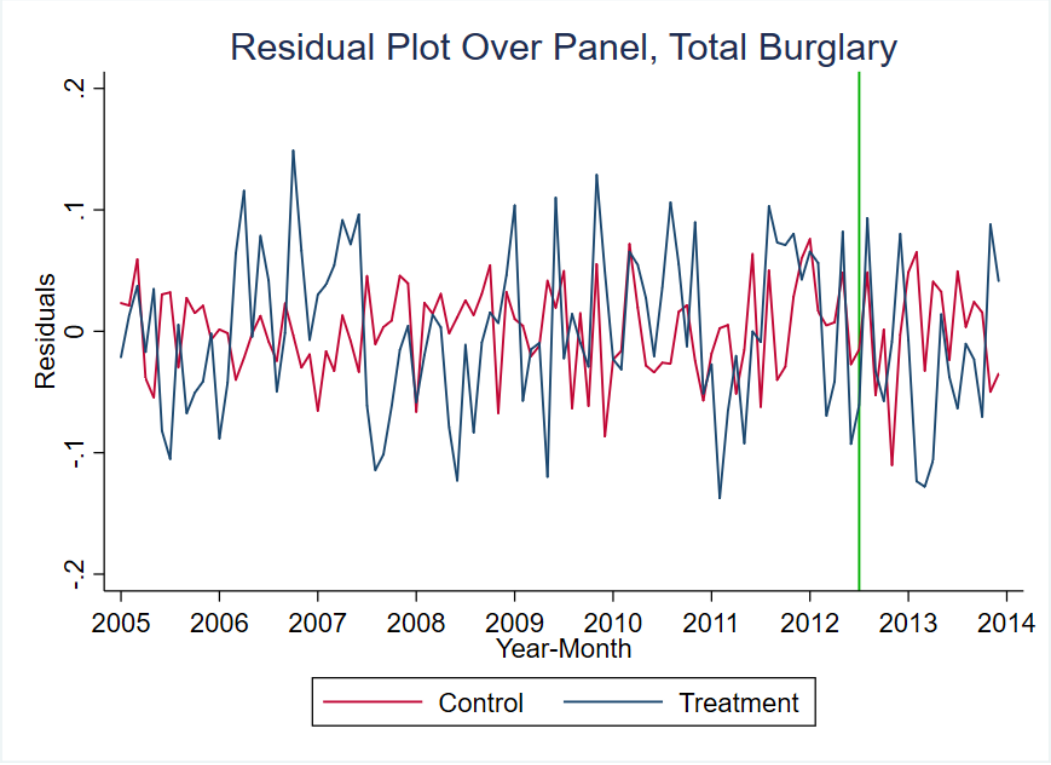
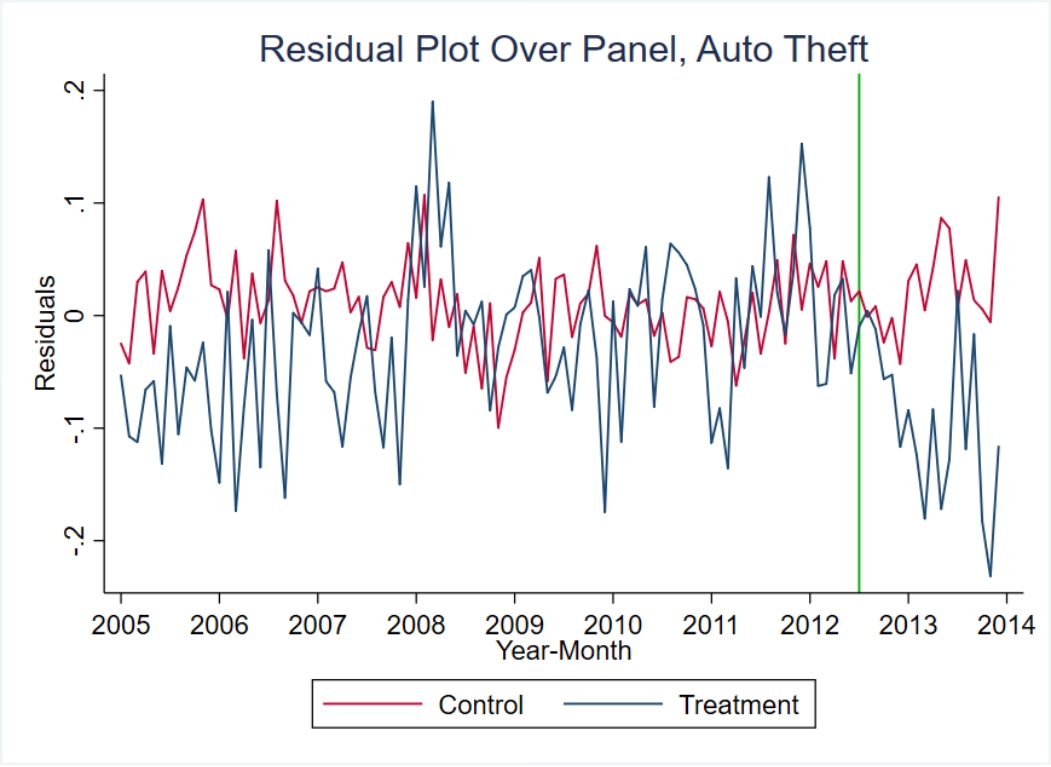


Figure C.9 Total Auto Theft Residual Plot



APPENDIX D

FIRST DIFFERENCE-IN-DIFFERENCES DYNAMIC ESTIMATES

Figure D.1 Dynamic Total Crime Estimates

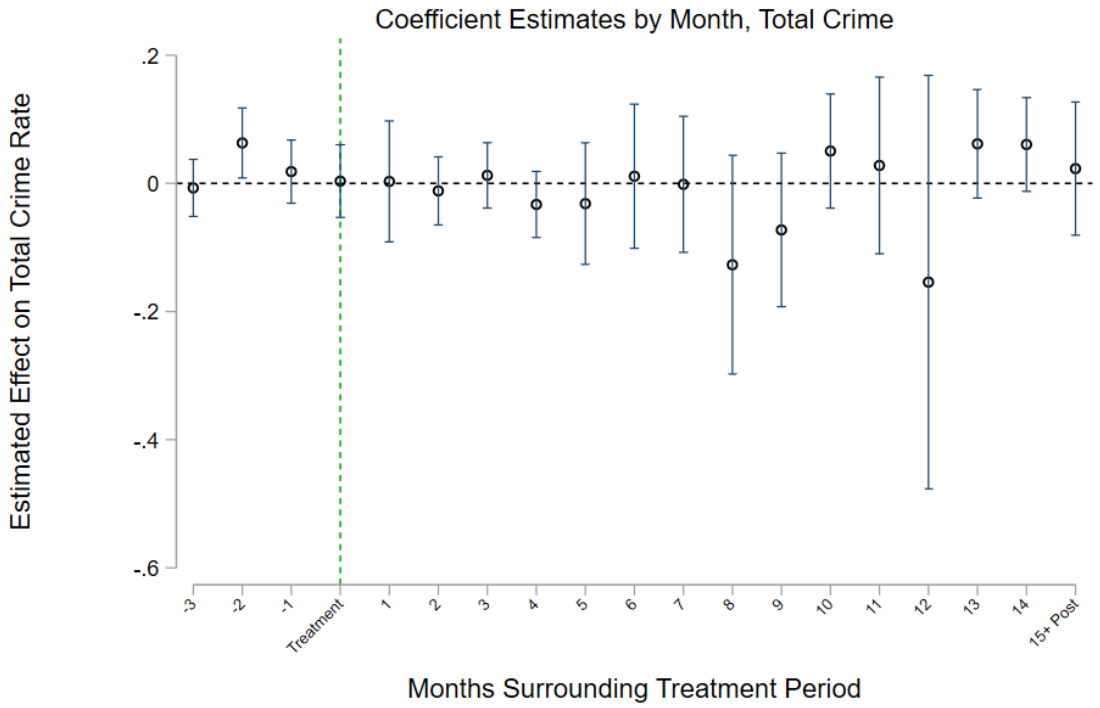


Figure D.2 Dynamic Total Violent Crime Estimates

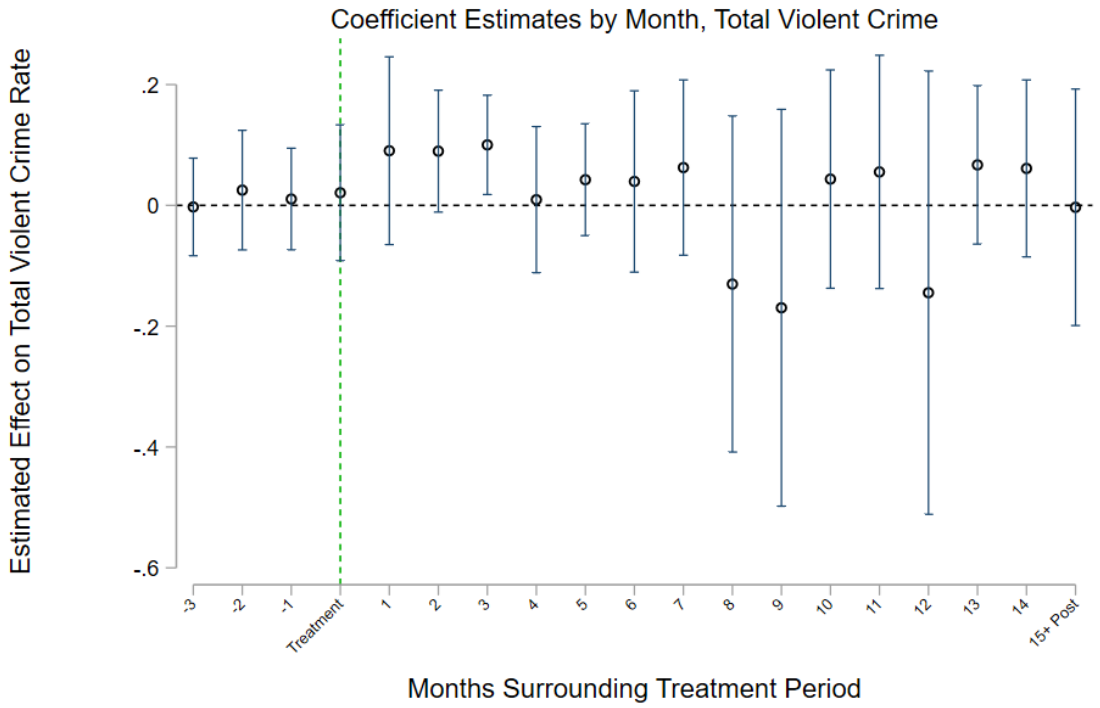


Figure D.3 Dynamic Total Property Crime Estimates

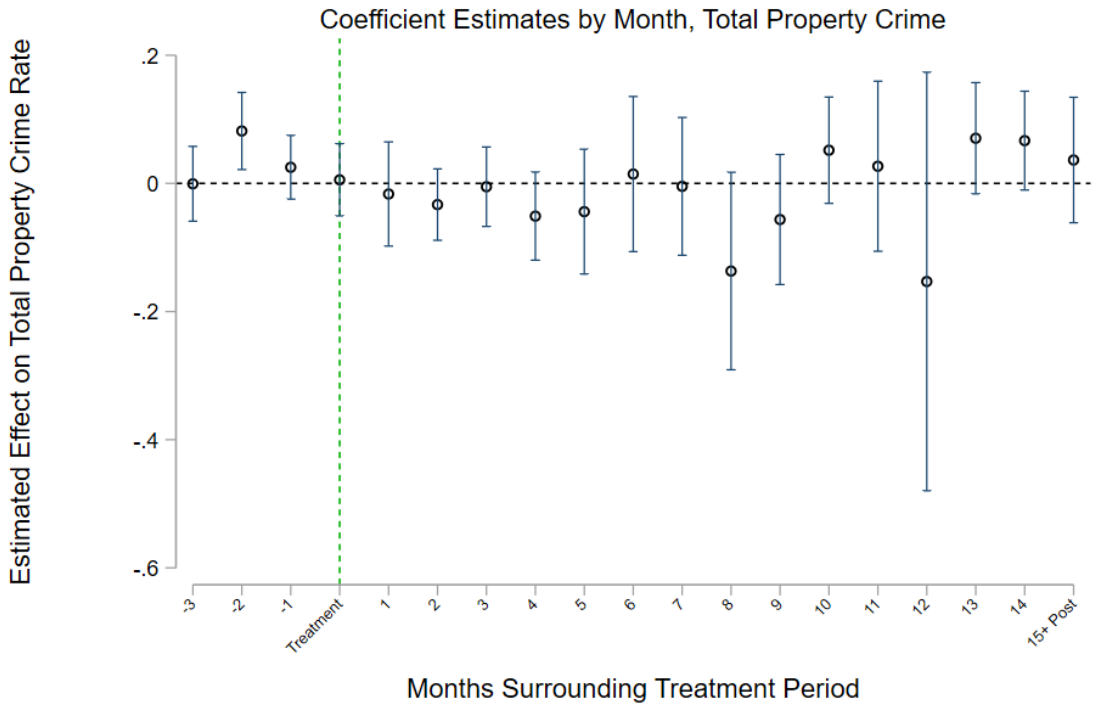


Figure D.4 Dynamic Murder Estimates

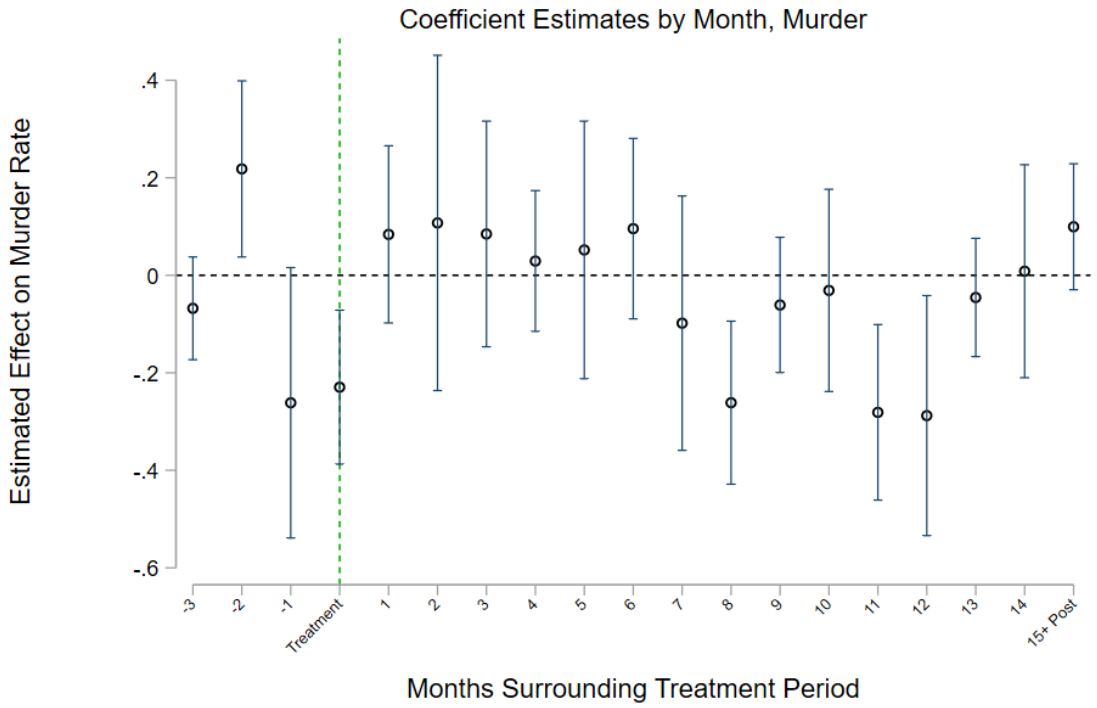


Figure D.5 Dynamic Total Rape Estimates

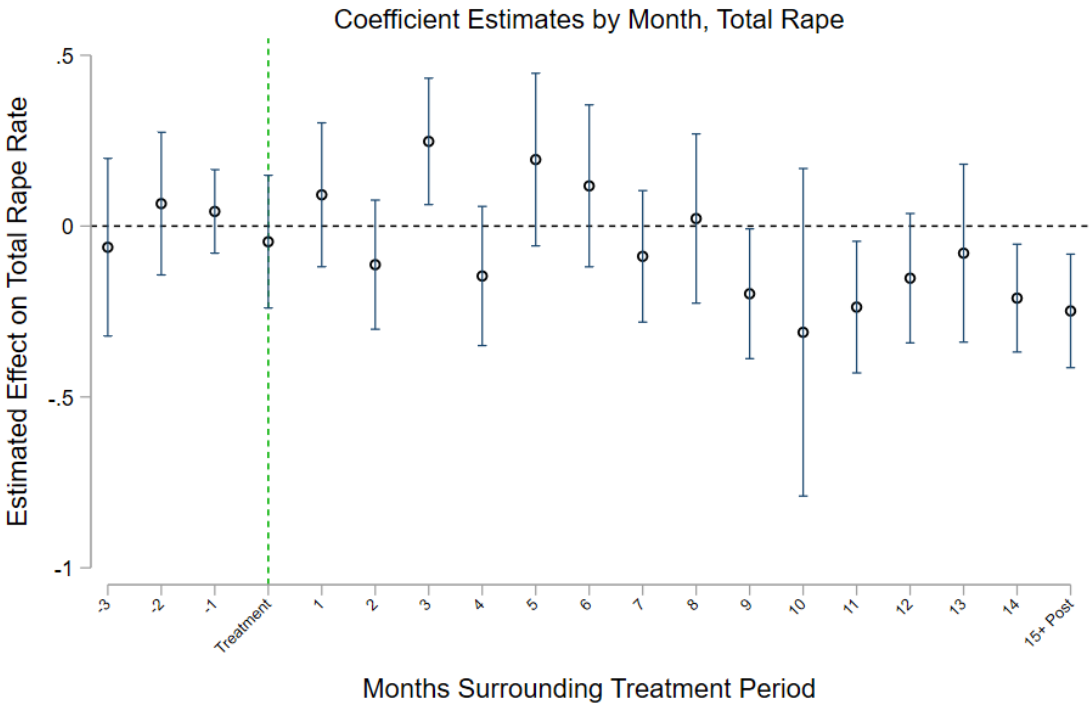


Figure D.6 Dynamic Total Assault Estimates

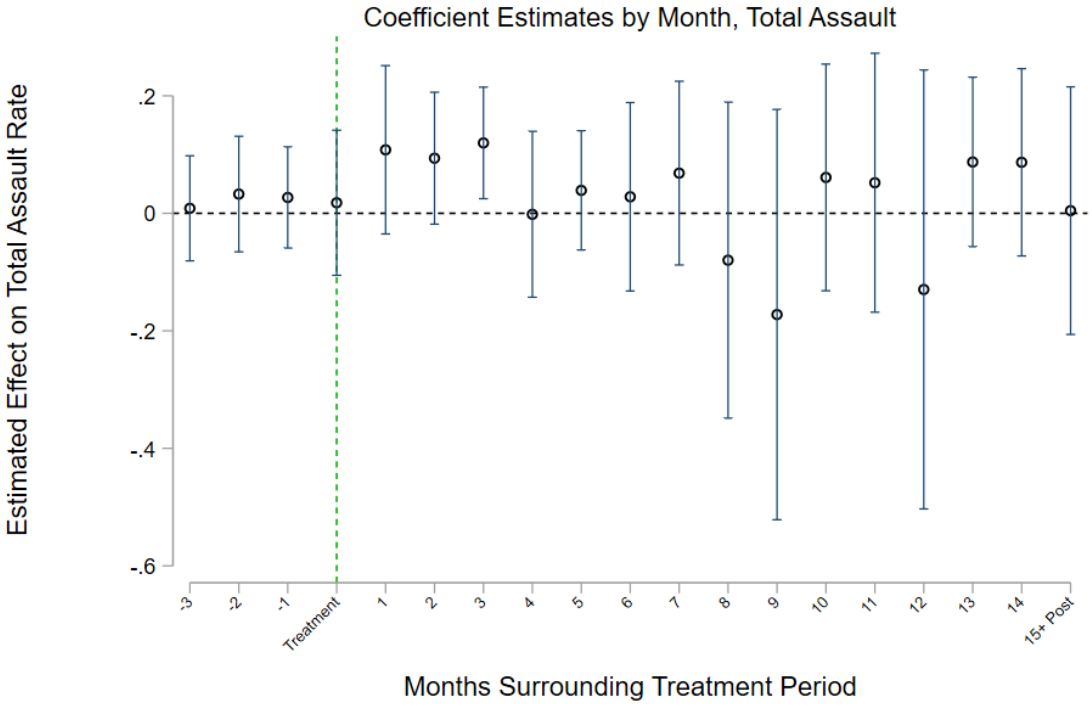


Figure D.7 Dynamic Total Robbery Estimates

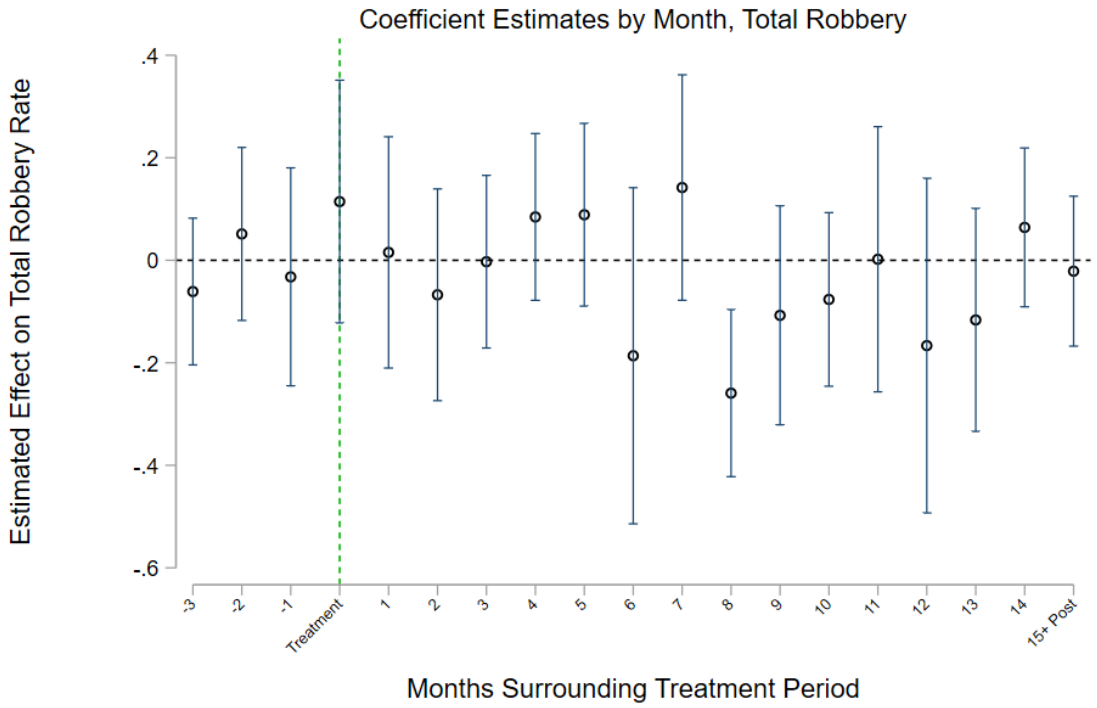


Figure D.8 Dynamic Total Theft Estimates

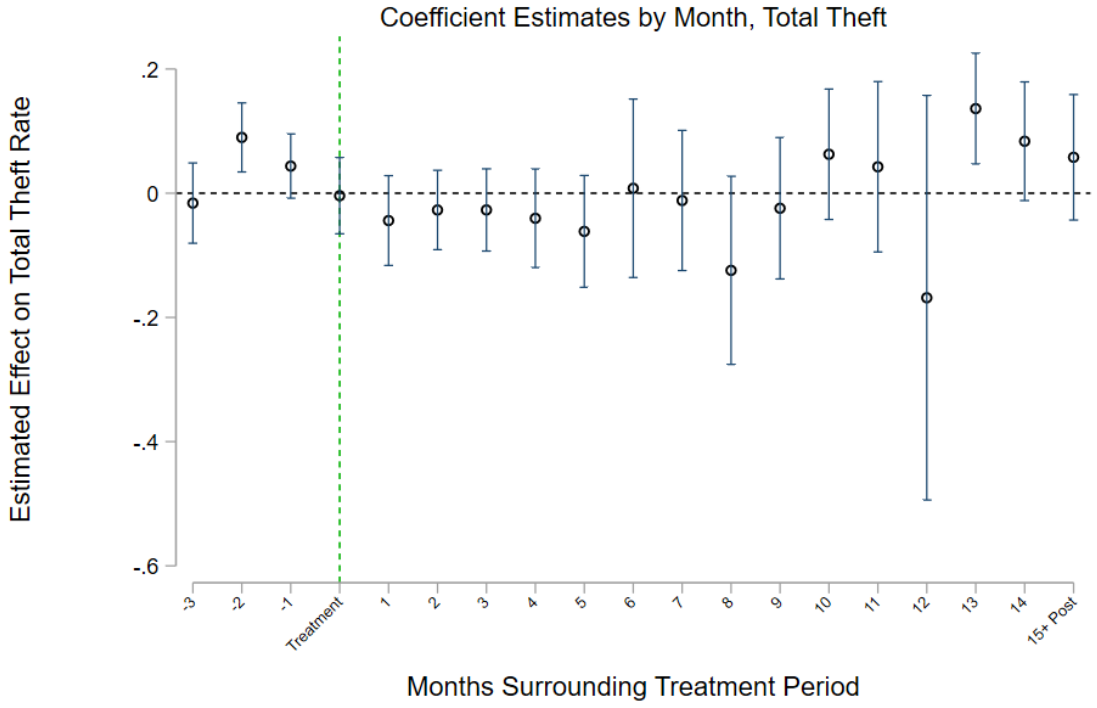


Figure D.9 Dynamic Total Burglary Estimates

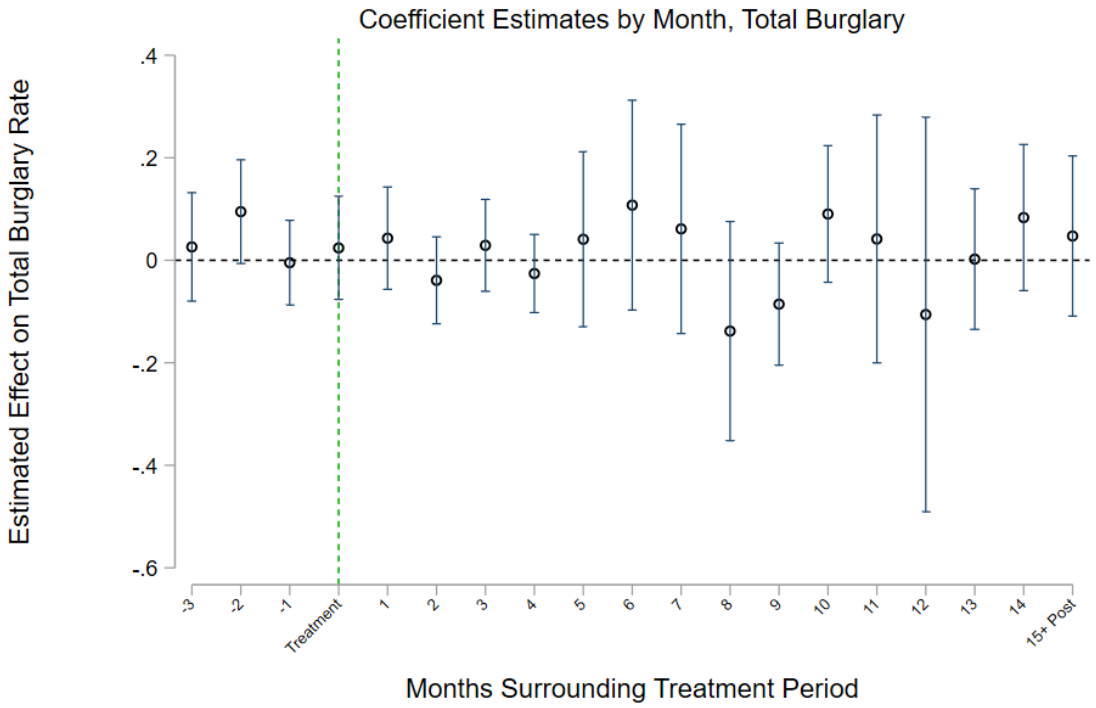
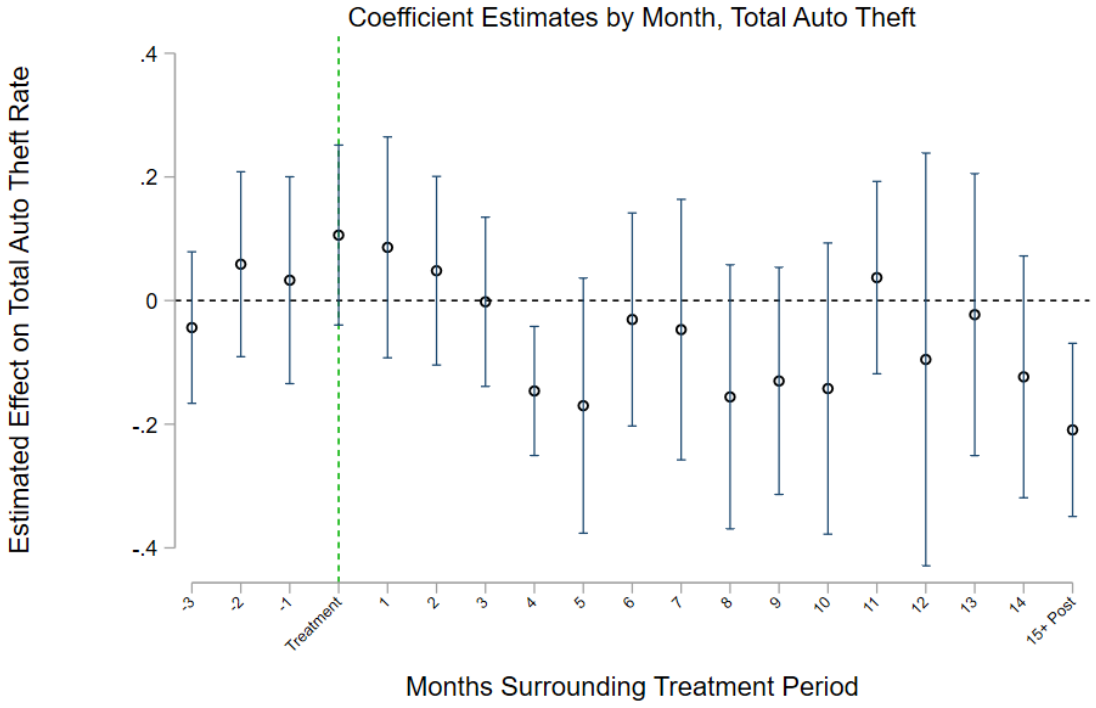


Figure D.10 Dynamic Total Auto Theft Estimates



APPENDIX E

SECOND DIFFERENCE-IN-DIFFERENCES DYNAMIC ESTIMATES

Figure E.1 Dynamic Total Crime Estimates

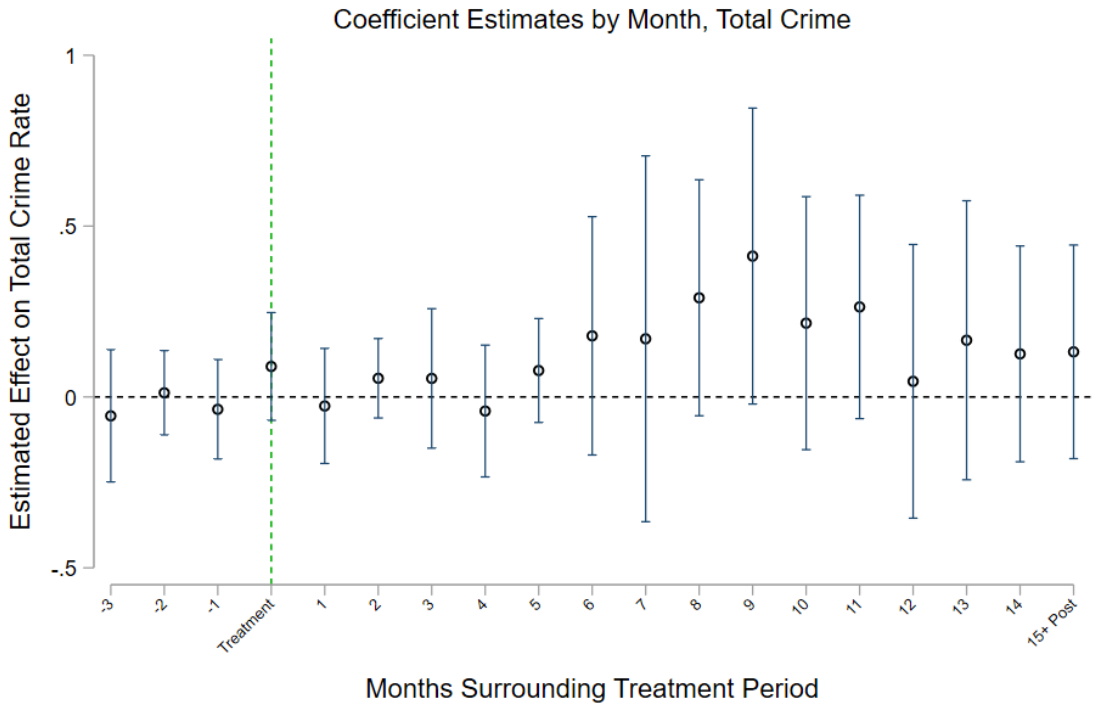


Figure E.2 Dynamic Total Violent Crime Estimates

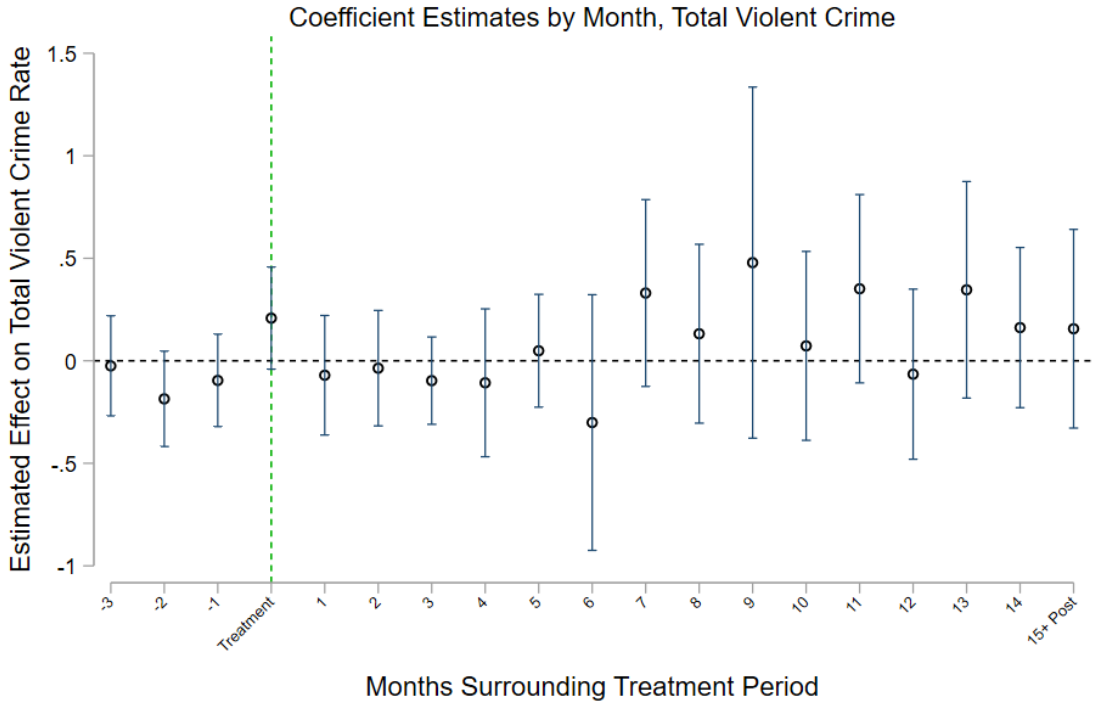


Figure E.3 Dynamic Total Property Crime Estimates

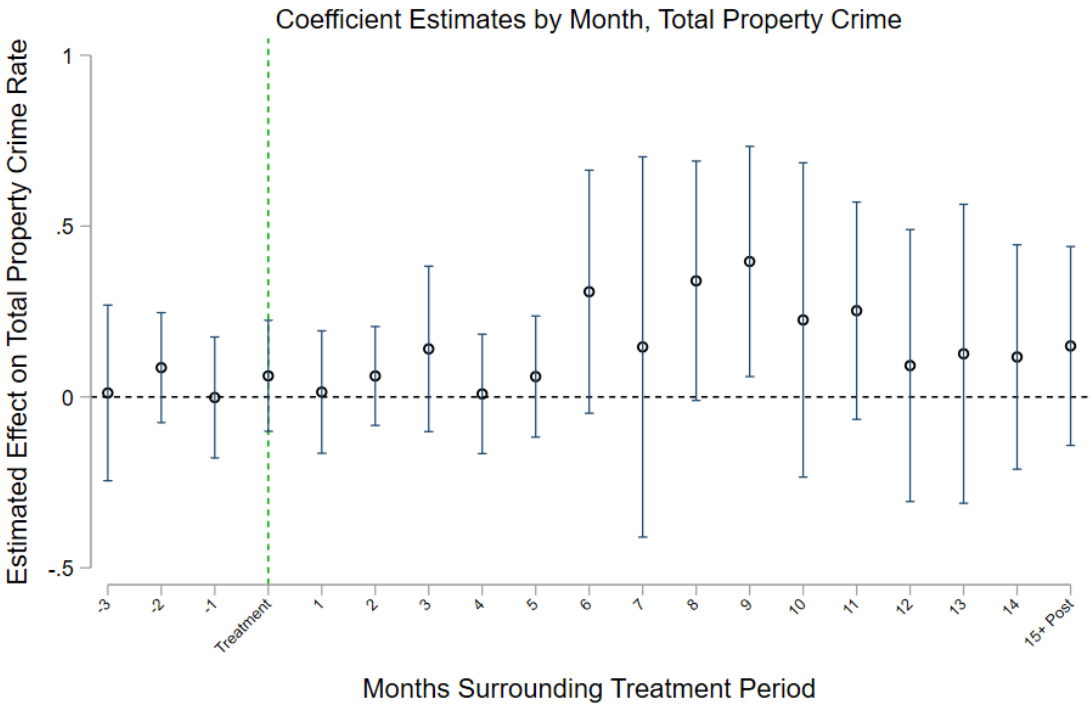


Figure E.4 Dynamic Murder Estimates

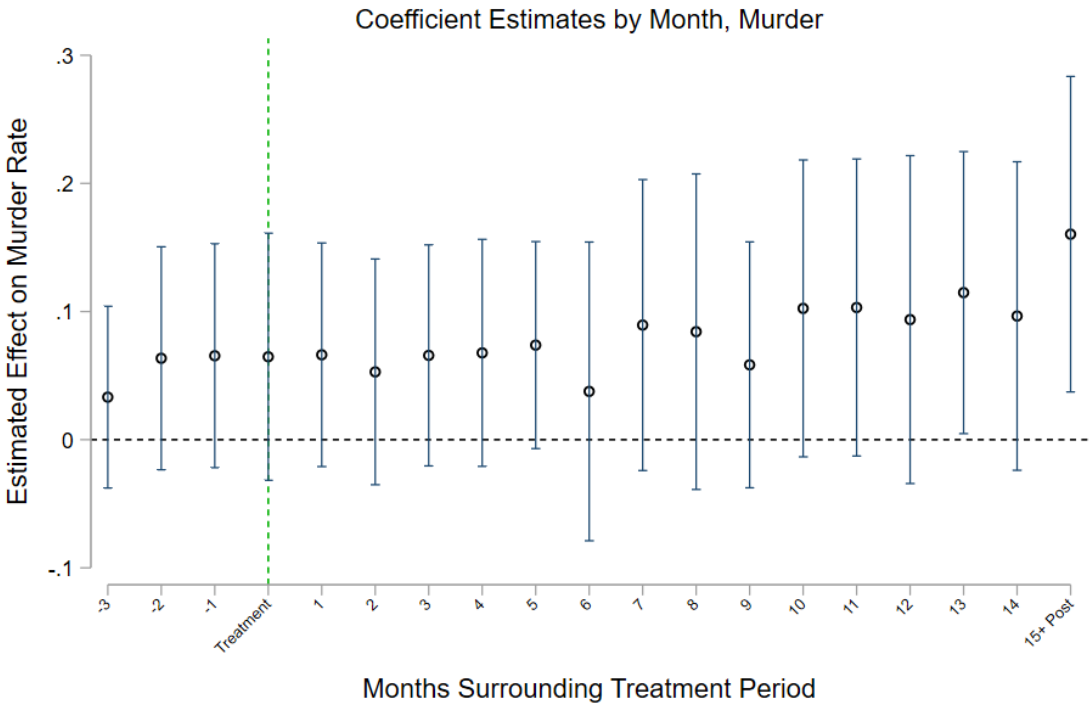


Figure E.5 Dynamic Total Rape Estimates

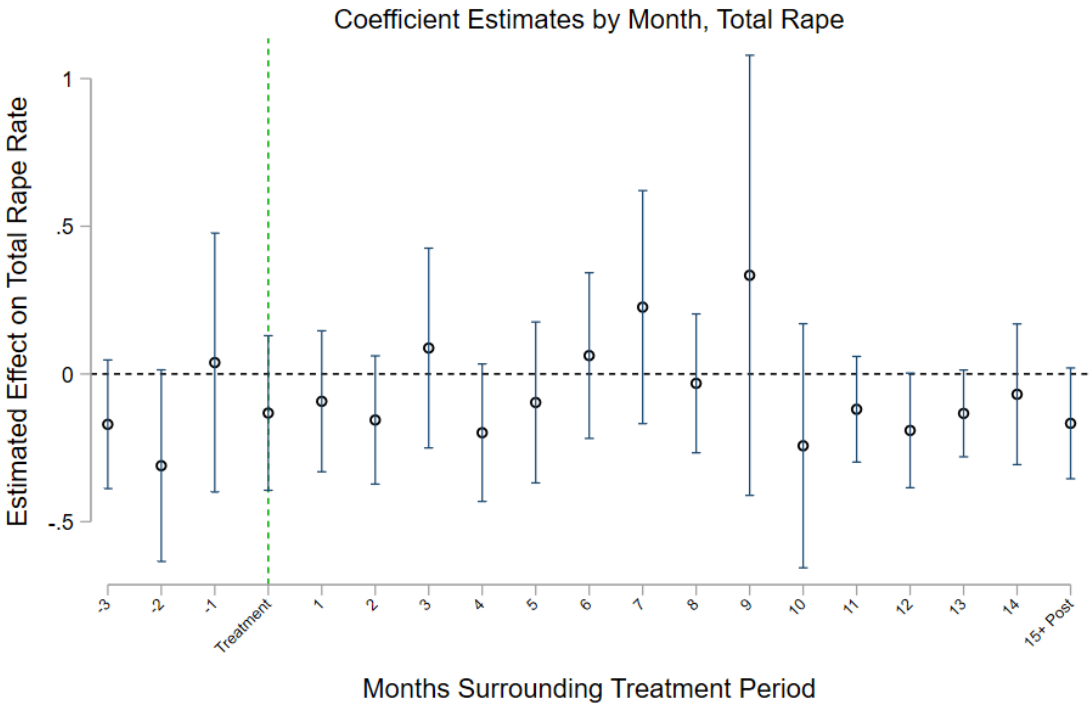


Figure E.6 Dynamic Total Assault Estimates

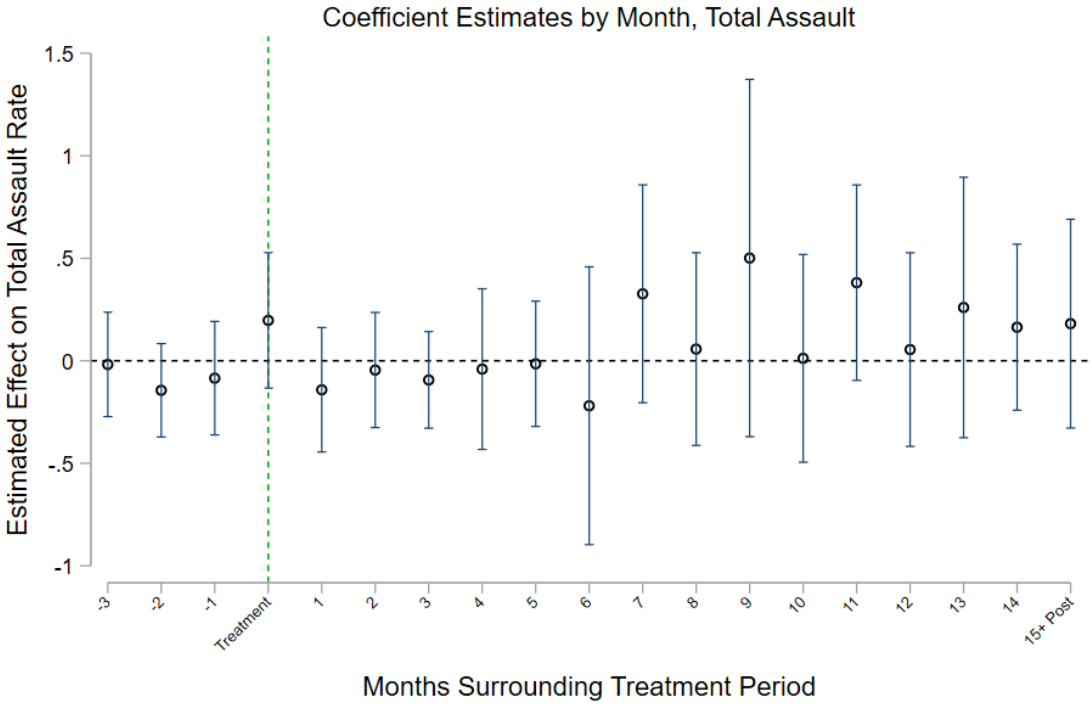


Figure E.7 Dynamic Total Robbery Estimates

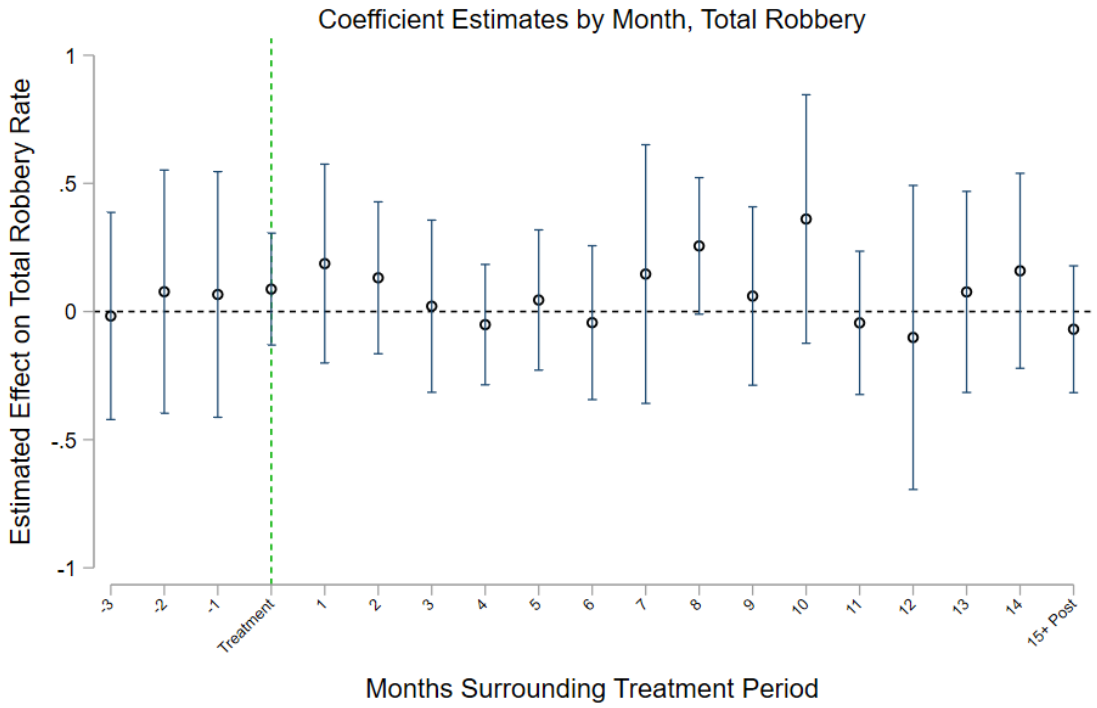


Figure E.8 Dynamic Total Theft Estimates

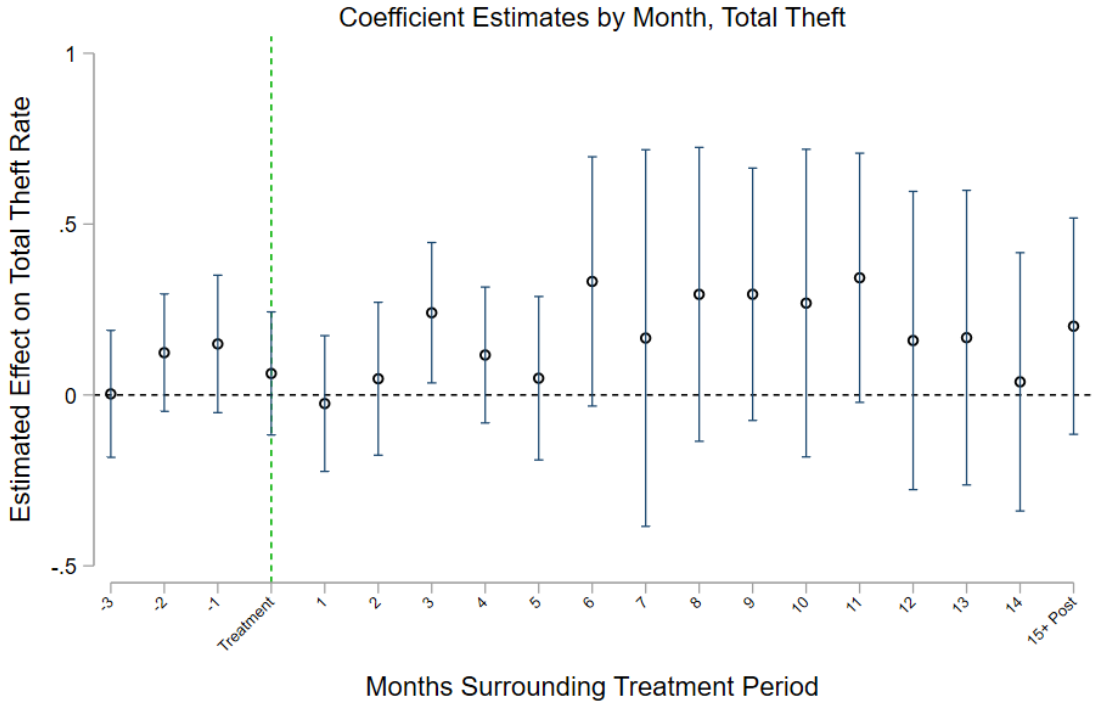


Figure E.9 Dynamic Total Burglary Estimates

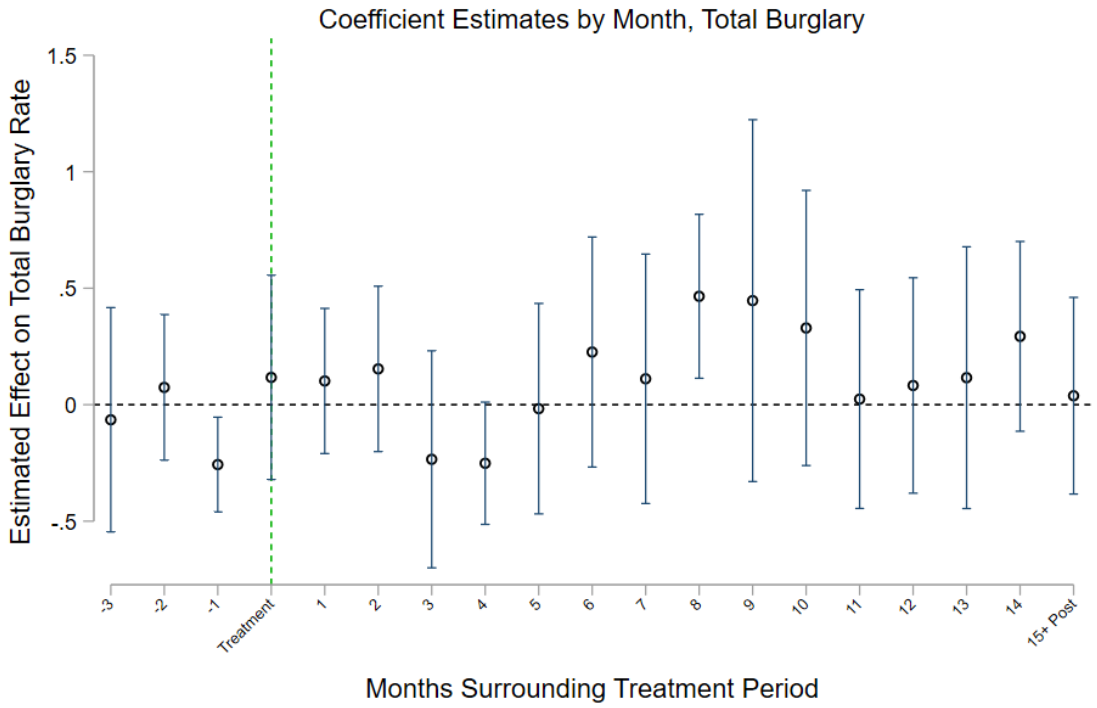
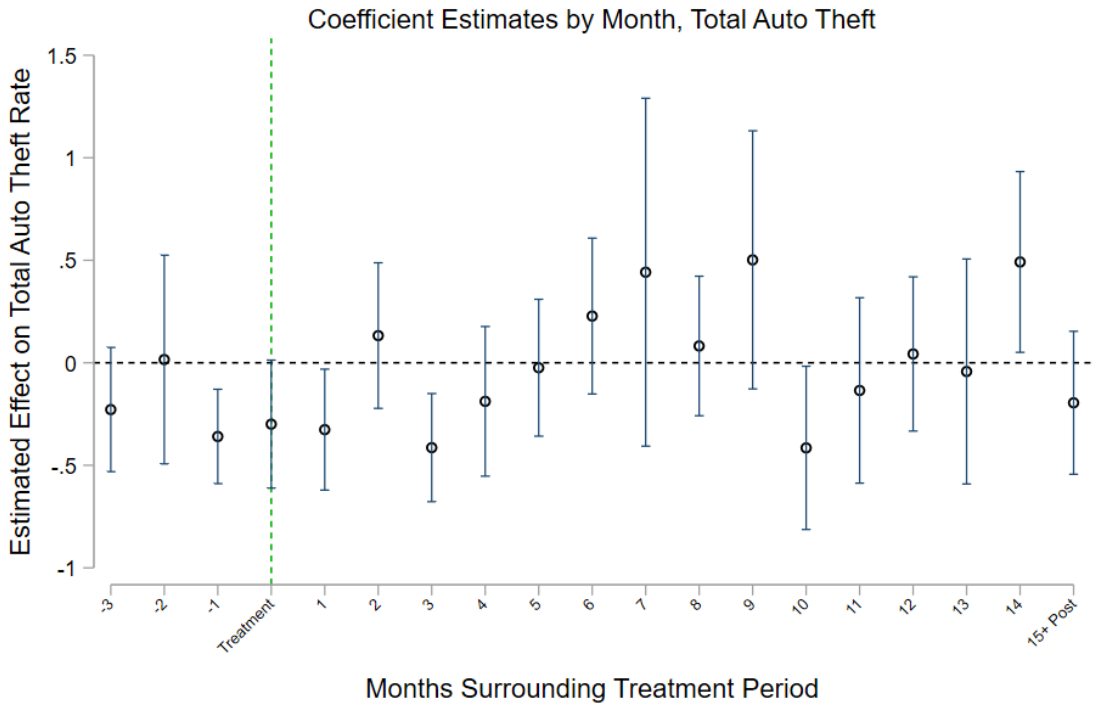


Figure E.10 Dynamic Total Auto Theft Estimates



APPENDIX F

PERMIT APPLICATIONS BY DEMOGRAPHICS, DYNAMIC ESTIMATES

Figure F.1 Dynamic Estimates for Black Permit Application Rates

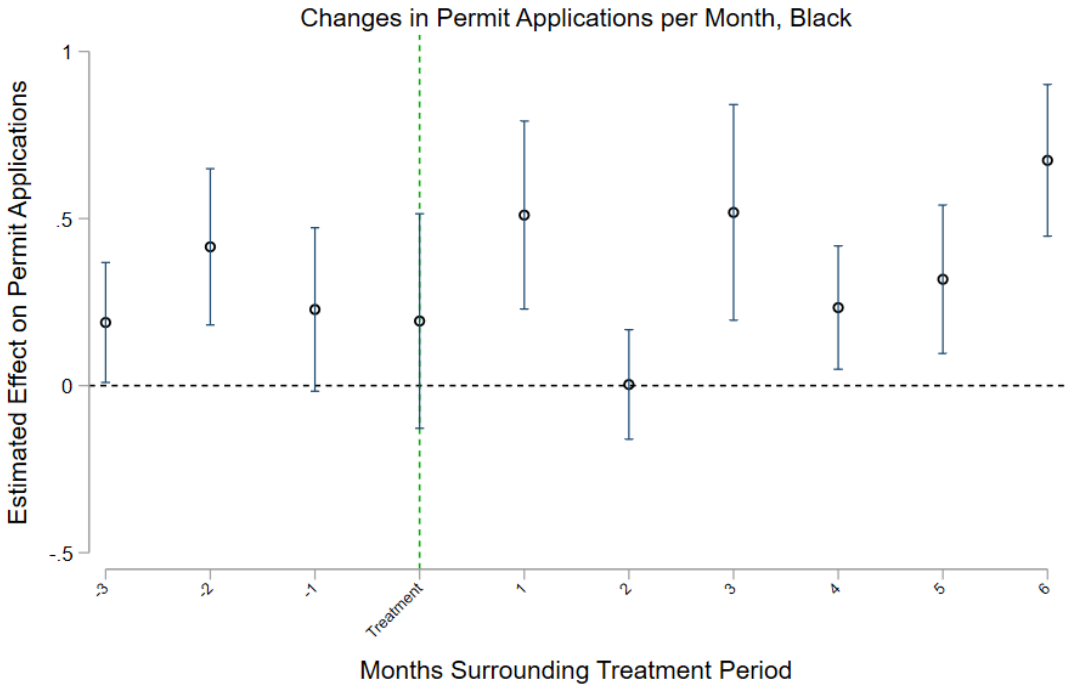


Figure F.2 Dynamic Estimates for White Permit Application Rates

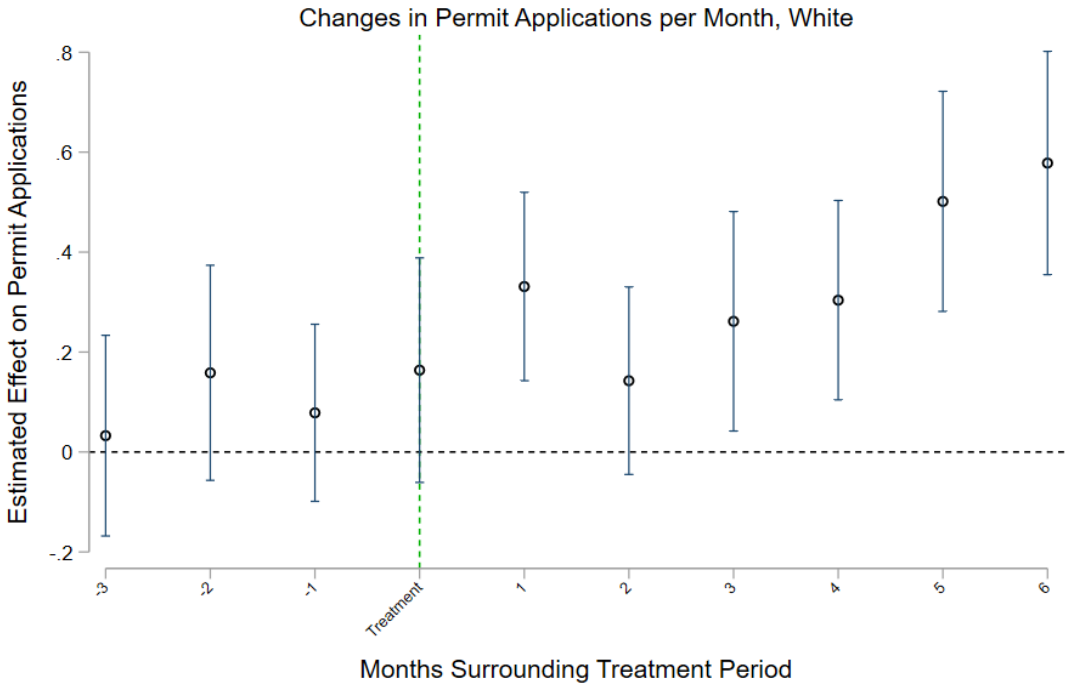


Figure F.3 Dynamic Estimates for Other Race Permit Application Rates

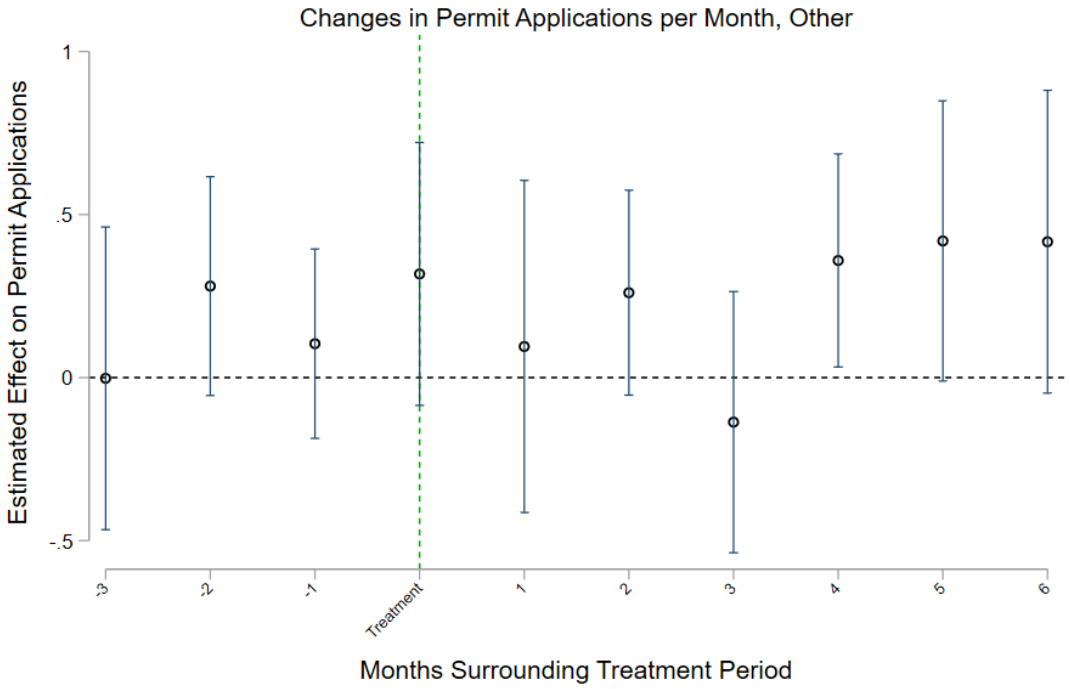


Figure F.4 Dynamic Estimates for Male Permit Application Rates

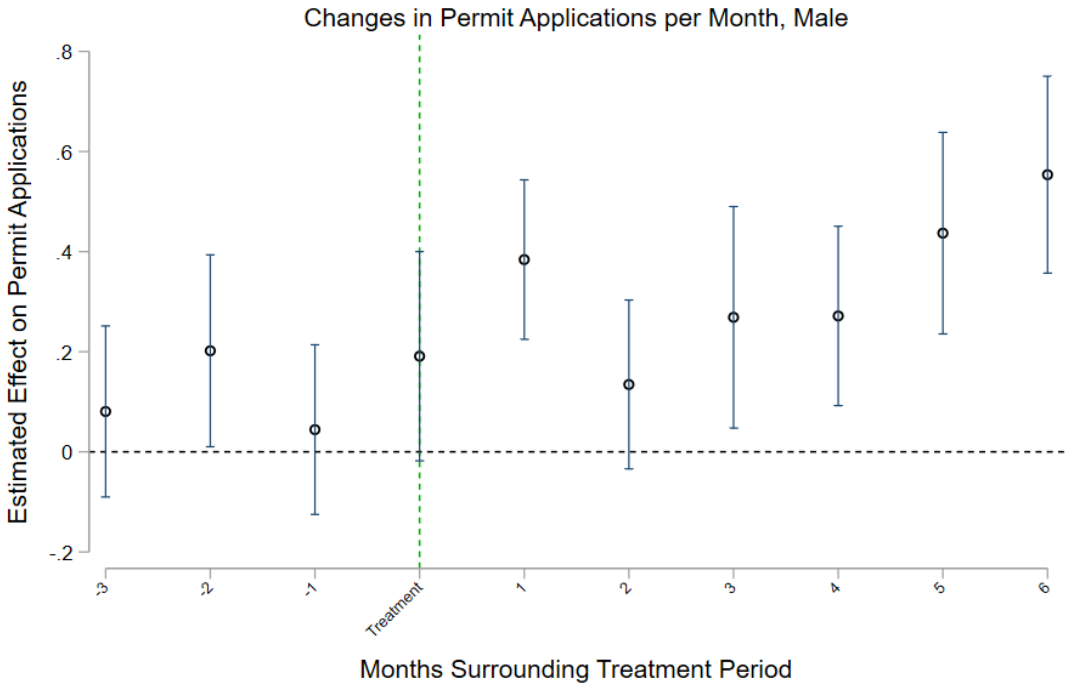
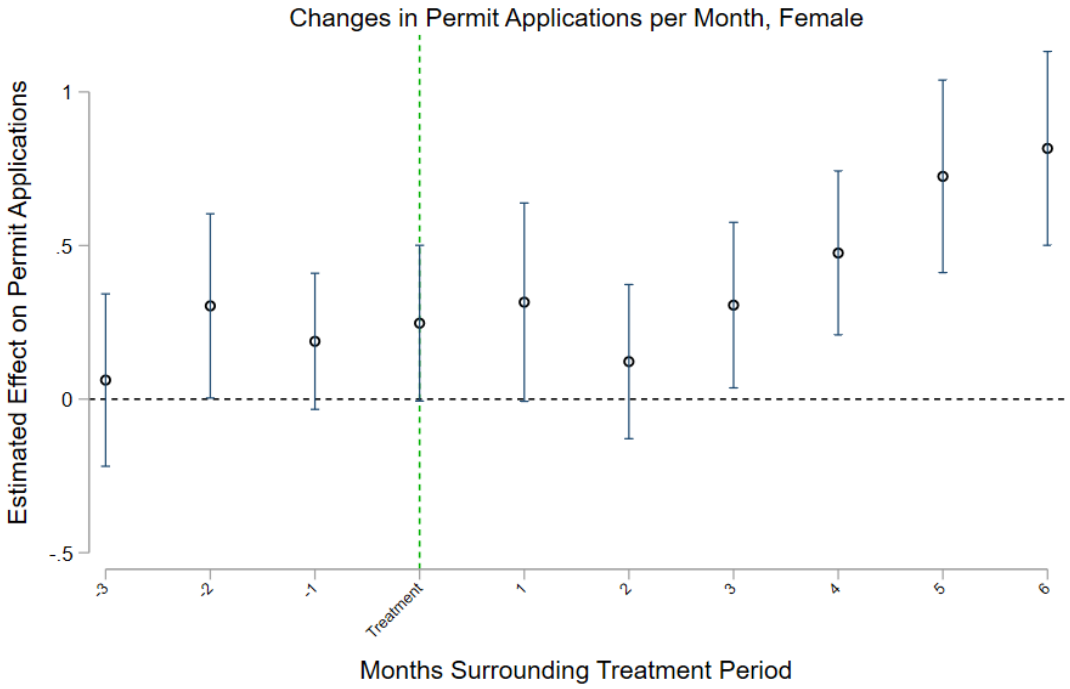


Figure F.5 Dynamic Estimates for Female Permit Application Rates



APPENDIX G

ARCHIVED RESPONSES TO WRAL DATABASE PUBLICATION

Figure G.1 Screenshot of GRNC article which published Mark Binker's personal information (1/3)

Grass Roots North Carolina for Firearms Education

North Carolina –-(Ammoland.com)-On Thursday, WRAL-TV published an article on its site entitled *"Rural areas lead in concealed weapons permit rates."* (http://tiny.cc/42klhw)

Unfortunately, within the article a section entitled *"Find concealed carry permit-holders in your area"* contained a database of permit-holders searchable by address, potentially revealing to criminals the homes from which firearms may be stolen.

WRAL web editor Kelly Hinchcliffe responded to complaints by insisting that permit information is public record, ignoring the fact that obtaining it requires a written request to the State Bureau of Investigation. Moreover, although the names of permit-holders were not disclosed, the partial address information *(including apartment numbers)*, particularly on small streets, makes it possible for criminals to target specific homes for theft.

A lengthy conversation with WRAL General Manager Steve Hammel, while pleasant, produced no significant movement on the part of the station. Their position is that they have sufficiently de-identified the address information. When it was noted that street numbers had appeared initially for some records, he noted that it had been corrected by Friday. He also said they would *"look at"* trailing apartment numbers which are still part of the addresses. But the bottom line is that the station refuses to pull the link.

TIME FOR ACTION

It has been longstanding policy at GRNC that any media attempt to invade the privacy of either handgun purchase permit-holders or concealed handgun permit-holders would be met with economic action against the media outlet and its advertisers. It is now time for you to act: Help GRNC make an example that will leave an impression on media outlets to discourage them from further acts of journalistic irresponsibility!

Figure G.2 Screenshot of GRNC article which published Mark Binker's personal information (2/3)

GLASS HOUSES?

In an apparent attempt to shame gun owners, some media outlets have a history of publishing the names of gun purchasers. But that was many years ago, before the advent of the Internet. Things are now far more reciprocal. So let's talk a bit about reporter Mark Binker, the apparent engineer of the piece to reveal concealed handgun permit-holders.

> *But first, an admonition: DO NOT THREATEN OR HARASS MARK BINKER. If you contact him to express your disappointment with the article … BE POLITE, DO NOT THREATEN, and CALL OR EMAIL ONLY ONCE!*

MEET MARK BINKER

Mark Gerald Binker is an up-and-coming reporter. After making an excellent career move in leaving the Greensboro News & Record just three months ago, he landed a job with WRAL-TV. Mark and his lovely wife, Marla J. Binker, reside at 17XX Chadstone Ct., Raleigh, NC 27615-7403. His cell number is 919-621-0970 and his work email is mbinker@wral.com.

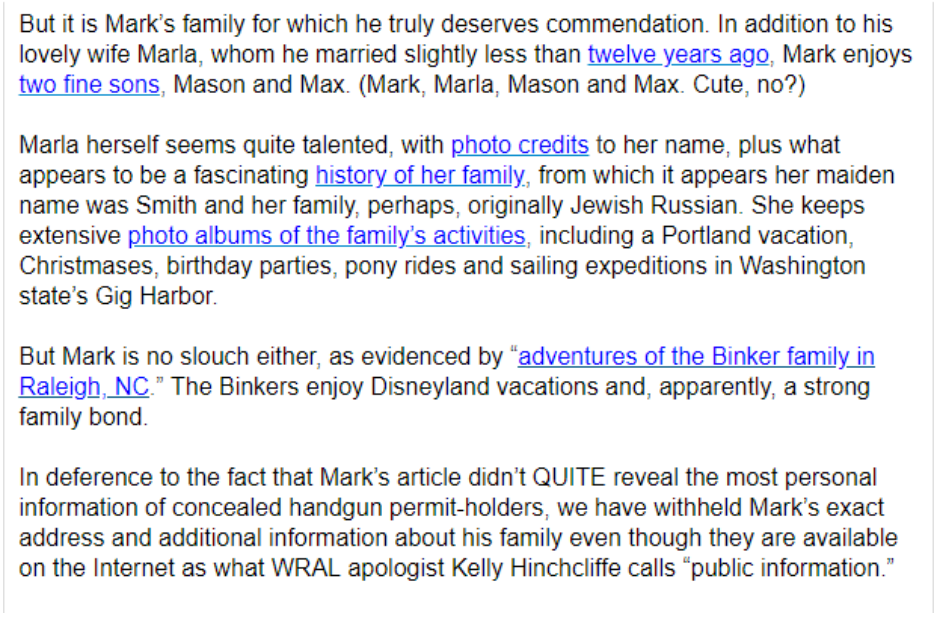
Mark has exceptional media experience, including:

- 1987-1991, 1997: Interim Reporter, Capital News Service, College Park, MD.
- 1996-1997: Associate Editor, CD Publications, Silver Springs, MD.
- 1998-2000: Suburban Staff Writer, Philadelphia Enquirer, Philadelphia, PA
- 2000-2005: Business/ local/ government Reporter, News & Record, Greensboro, NC
- 2005-March 2012: State Government & Politics Reporter, News & Record, Greensboro, NC
- March 2012- Present: Multimedia Investigative Reporter, WRAL TV, Raleigh, NC

Mr. Binker is also very well educated, having received his Bachelor of Arts in Psychology (1995) at The Johns Hopkins University, Baltimore, MD; and his Masters of Arts in Journalism (1997 University of Maryland, College Park, MD).

Happily, Mark is one of the few reporters who is not a registered Democrat. According to the State Board of Elections, he is registered as Unaffiliated, with a gender of male, a race of white, and an ethnicity of "*undesignated*." His voter registration number is 000099949361, his registration date is 08/29/2005, and his precinct is 01-42. For better or worse, he is, in fact, eligible to vote.

Figure G.3 Screenshot of GRNC article which published Mark Binker's personal information (3/3)



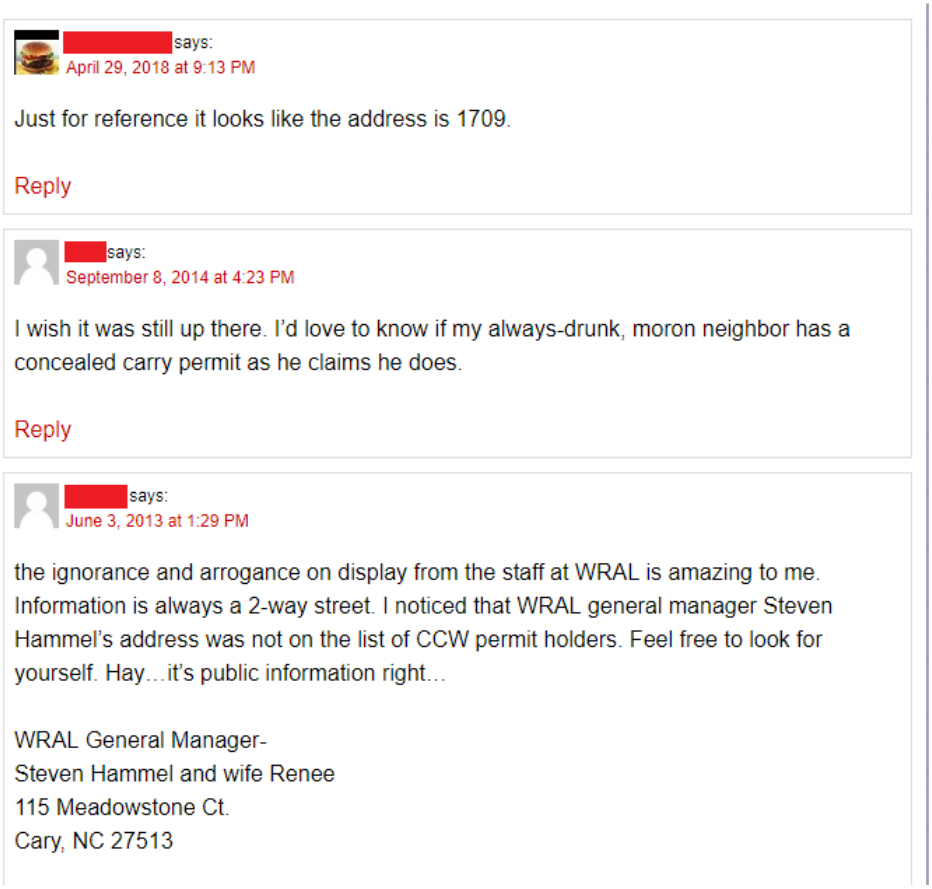
But it is Mark's family for which he truly deserves commendation. In addition to his lovely wife Marla, whom he married slightly less than twelve years ago, Mark enjoys two fine sons, Mason and Max. (Mark, Marla, Mason and Max. Cute, no?)

Marla herself seems quite talented, with photo credits to her name, plus what appears to be a fascinating history of her family, from which it appears her maiden name was Smith and her family, perhaps, originally Jewish Russian. She keeps extensive photo albums of the family's activities, including a Portland vacation, Christmases, birthday parties, pony rides and sailing expeditions in Washington state's Gig Harbor.

But Mark is no slouch either, as evidenced by "adventures of the Binker family in Raleigh, NC." The Binkers enjoy Disneyland vacations and, apparently, a strong family bond.

In deference to the fact that Mark's article didn't QUITE reveal the most personal information of concealed handgun permit-holders, we have withheld Mark's exact address and additional information about his family even though they are available on the Internet as what WRAL apologist Kelly Hinchcliffe calls "public information."

Figure G.4 Comments on Ammoland.com, responding to the above GRNC article. Usernames redacted



[redacted] says:
April 29, 2018 at 9:13 PM

Just for reference it looks like the address is 1709.

Reply

[redacted] says:
September 8, 2014 at 4:23 PM

I wish it was still up there. I'd love to know if my always-drunk, moron neighbor has a concealed carry permit as he claims he does.

Reply

[redacted] says:
June 3, 2013 at 1:29 PM

the ignorance and arrogance on display from the staff at WRAL is amazing to me. Information is always a 2-way street. I noticed that WRAL general manager Steven Hammel's address was not on the list of CCW permit holders. Feel free to look for yourself. Hay…it's public information right…

WRAL General Manager-
Steven Hammel and wife Renee
115 Meadowstone Ct.
Cary, NC 27513

Figure G.5 AR15.com forum comments about WRAL database, username redacted

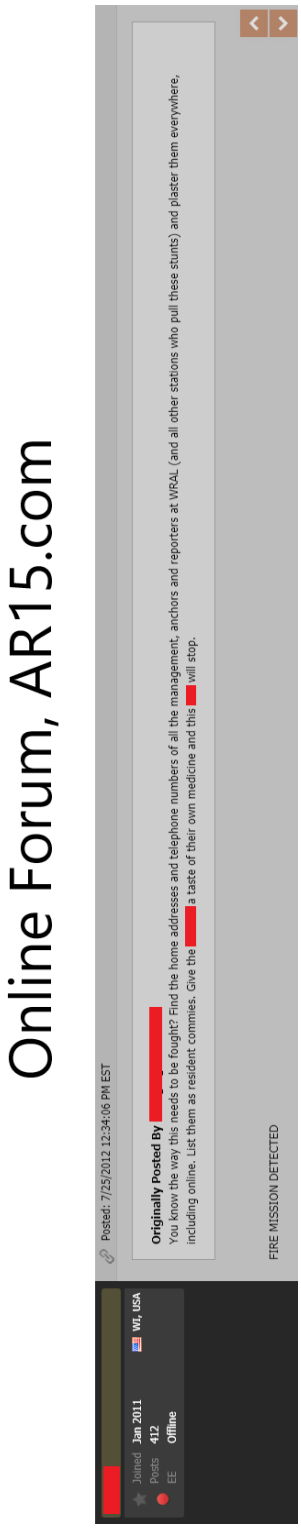


Figure G.6 Change.org petition Asking WRAL to remove street-level aggregates

Figure G.7 WRAL message indicating removal of the gun database

Map: North Carolina concealed weapon carry permits

Editor's Note: Effective Oct. 1, 2013, North Carolina state law requires that county sheriffs keep confidential the list of those who apply for and are granted permits to carry a concealed weapon.

In the spirit of that law, and because the law would preclude the ability to keep any data on concealed carry permits current, WRAL has removed data related to concealed handgun permits from this page.