**The Effects of Permitless Concealed Carry Laws on Crime and Victimization**

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**Abstract**

In the last ten years, one of the most substantial changes to firearm policy is the widespread adoption of permitless concealed carry laws. Since 2010, more than twenty-two states have adopted a permitless carry law. Permitless carry laws allow firearm owners to carry a concealed handgun in public without first obtaining a license and undergoing a background check. This paper examines the effects of permitless carry laws on crime and gun violence outcomes using data from police agencies. Results from staggered difference-in-differences estimators using agency-month observations show that aggravated assaults increase in the subsequent seven months following policy adoption without reductions in other types of crime. This rapid increase is driven by gun assaults which increase 9.5 percent, constituting over 500 additional firearm assault victims within the sample. Reductions in police enforcement of weapons laws may reduce the barriers to carrying firearms in public.

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## 1. Introduction

Violent crime and gun violence are primary concerns for the majority of Americans. Over 80% of Americans view gun violence reduction as an important priority, placing it above several other issues, including the economy and immigration (AP-NORC, 2022). Almost half of Americans believe it somewhat likely that they will be a victim of gun violence in the next five years, a fear that young Americans are most likely to express. Concerns about gun violence reflect direct experience for many Americans. About a fifth report that they, a family member, or a close friend have experienced gun violence directly in the past five years (AP-NORC, 2022).

Experiences with gun violence reflect an increasing upward trend in the United States (U.S). In 2015, firearm homicide rates rose sharply after reaching a historic low in 2014. Since then, gun violence has risen in nearly every subsequent year. From 2015 to 2020, over 1.7 million victims were assaulted or robbed in incidents involving firearms, and 89,692 of these violent events resulted in a gun homicide (Federal Bureau of Investigation, 2019).

During this timeframe of increasing gun violence, laws regulating carrying concealed handguns in public substantially changed in many states. Since 2015, twenty states have enacted a policy known as permitless concealed carry. Permitless carry is now the most prevalent form of public handgun carrying law in the U.S. In practice, permitless carry removes requirements for firearm owners to obtain a permit and undergo a background check to carry a concealed handgun in public.

On the one hand, permitless carry laws may have a minimal effect on public handgun carrying and therefore on violent crime. Individuals who want to carry a concealed handgun have, in most states, been able to obtain a concealed carry license for decades. Firearm owners with concealed carry licenses have grown substantially over time, and concealed carry licenses applications are rarely denied (Rowhani-Rahbar et al., 2017). Permitless carry may have minimal public safety effects because the vast majority of individuals who want to carry concealed weapons in public are already able to do so.

However, several important features of permitless carry laws imply that they may affect violent crime. First, permitless carry laws expand eligibility to carry a concealed handgun in public to all gun owners. Increasing eligibility may result in more handgun carriers, or an increase to the extensive margin of public handgun carrying. New handgun carriers may differ from existing concealed carry license holders as they may have been previously discouraged by background check policies or the necessity to complete licensing paperwork. Second, permitless carry laws may alter the frequency of public handgun carrying, thereby affecting the intensive margin of public handgun carrying. Because permitless carry laws allow any legal gun owner to carry a concealed handgun, individuals may opt to carry concealed weapons more often. As a result of both shifts, public conflicts between individuals may differ due to the increased likelihood or expectation of encountering an armed individual. Increased public gun carrying may therefore result in deterrence or exacerbation of violent crime in public places. Finally, permitless carry policies may reduce law enforcement focus on preventing illegal handgun carrying. With no permitting requirement, it is more difficult for police to determine if handgun possession is legal and may reduce enforcement aimed at deterring illegal handgun carrying.

The purpose of this paper is to examine the effect of permitless carry policies on crime and victimization to inform ongoing policy debates between those who favor expanding permitless carry and those who seek to repeal it. In the absence of rigorous evaluation of permitless carry laws, public debates lack evidence to support public safety conclusions. Proponents argue that permitless carry is an essential step for maintaining the right to own and carry firearms, and furthermore will deter violent crime due to increased rates of public gun carrying. Opponents argue that removing the background check requirement

to carry a firearm will result in more public violence as higher risk gun carriers are legally allowed to carry a concealed weapon. Among law enforcement stakeholders, police unions and leadership associations argue that efforts to combat illegal gun carrying and possession are hamstrung by the increased difficulty of identifying illegal firearm carrying.

Despite the rapid adoption of permitless carry and widespread public debate, the majority of the existing literature evaluating concealed handgun carrying regulations does not focus on permitless carry laws and therefore has limited relevance to what is now the most prevalent form of concealed carry regulation. Rather, existing research assesses permitless carry laws together with other permissive concealed handgun laws as a single, less restrictive policy often referred to as a "right-to-carry" law. A main contribution of this paper is to extend the existing literature to assess permitless carry as a separate policy.

An emerging literature seeks to address this research gap by studying the effects of increasing permitless carry adoption. The recent work of Moody and Lott (2022) and Doucette et al. (2023) examine adoption of permitless carry laws on yearly measures of homicide and violent crime outcomes in different study time frames with varying levels of methodological rigor, finding that permitless carry laws may result in decreases or no effect on homicide and violent crime outcomes. As Bondy et al. (2023) note, caution is warranted in assessing the effects of concealed carry policies with limited data and demonstrate that the analyses in Moody and Lott (2022) do not identify a causal effect and note substantial model specification concerns.

In this study, I estimate the effects of adopting a permitless carry law on crime outcomes using a staggered difference-in-differences framework that uses variation in the timing of state policy changes from 2015-2020. In contrast to prior studies which focus on long-term effects, I create an original sample of police agencies that consistently report monthly crime data and I primarily focus analysis on the several months before and subsequent to permitless policy adoption. This shorter period is useful for several reasons. First, treatment effects should take effect rapidly over the course of several months to be credibly driven by permitless carry laws as individuals who legally own a are immediately eligible to concealed carry in public. Second, permitless carry laws average 4 months between adoption and implementation which may spur anticipatory effects on the part of both gun owners and law enforcement agents. Third, a short post-period reduces the likelihood that treatment effects are driven by unobservable shifts in gun ownership, for which no data is gathered in the US, and other time-varying confounders. Fourth, as permitless carry laws were enacted recently, a short post-period allows for inclusion of additional states for a more comprehensive estimate of treatment effects while avoiding inclusion of changes in violent crime driven by the Covid-19 pandemic (Abrams, 2021).

I use the nation's crime reporting system, the Federal Bureau of Investigation (FBI) Uniform Crime Reporting Program's National Incident Based Reporting System (NIBRS), which provides rich, detailed information about crime incidents and victims. As NIBRS does not offer complete coverage of all states, I additionally use the FBI's Summary Reporting System (SRS) crime data to ensure that sample composition does not drive results.

I find little evidence of deterrent effects resulting from permitless concealed carry policy adoption. I estimate an approximately 7-8 percent percent increase in aggravated assaults in states that adopted permitless carry in the seven months after policy adoption. This increase in violent crime is driven by gun crime, which rose 9.5 percent. I show that increases in gun violence take place in public and impact male victims. This finding is in line with recent national surveys, which find over the past 5 years, public handgun carrying has almost doubled and is driven by increased carrying among existing

gun owners (Rowhani-Rahbar et al., 2022). Finally, I find substantial reductions in police enforcement of weapons law offenses and arrests both leading up to and after permitless carry law adoption.

## 2. Background

*2.1 Concealed Handgun Carrying in the United States*

Over the course of the last four decades, all states in the U.S. adopted some form of concealed handgun carrying in public. In 2013, Illinois was the sole state in the U.S. that did not allow concealed handgun carrying but its ban on concealed carry of handguns was struck down by the U.S. Seventh Circuit Court of Appeals. In 2014, Illinois became the final state to allow some form of public handgun carrying (Associated Press, 2014; Long et al., 2012). While states may place additional restrictions on public handgun carrying, federal law prohibits firearm possession for individuals convicted of a felony, in cases of an involuntary confinement for mental health, and if an individual is under court order related to domestic violence. Individuals in these disqualifying categories are prohibited from carrying concealed weapons.

States differ in how they regulate public concealed handgun carrying, generally falling into three categories: 1) may issue, 2) shall issue, and 3) permitless carry. These categories vary according to concealed handgun permitting and oversight. The first category, known as "may issue," is least permissive and includes states that both issue and regulate concealed handgun licenses based on law enforcement discretion. In practice, law enforcement discretion can include license denials for misdemeanor (rather than felony) domestic assault, drug, and alcohol abuse (Hemenway & Hicks, 2015). Eight states, including California and New York, remain "may issue" states.

Remaining states differ according to whether concealed handgun carrying in public necessitates a permit or license. States that require a permit or license for legal public concealed carry are known as "shall issue" states, which differ from "may issue" states in that they do not allow for law enforcement discretion. As of 2014, the vast majority of "shall issue" states in the U.S. required gun owners to undergo a background check to carry a concealed weapon (*Gun Law Navigator*, 2022). In most, permits are valid for four to five years with a renewal process including a background check, though states increasingly issue lifetime concealed carry licenses or enhanced licenses which are valid for longer durations (*Handgun Permit Types*, 2020).

Increasingly, shall issue states have adopted permitless or "constitutional carry" policies. By 2020, the majority of U.S. states enacted permitless carry, most of which did so after 2015. Permitless policies remove the requirement that concealed handgun carrying requires a background check and permit, and in some states, the requirement to undergo firearm safety training. Permitless carry states continue licensing programs to enable concealed handgun carriers to travel legally between states while carrying a concealed weapon.

*2.2 The Effects of Concealed Carry Licensing Laws on Crime*

A substantial literature focuses on concealed carry licensing laws. The majority of the evidence on the effects of loosening restrictions on carrying concealed handguns in public show increases in violent crime and homicide (Colmer & Doleac, 2022; Donohue et al., 2017, 2022; M. L. Doucette et al., 2022; Fridel, 2021; Kovandzic & Marvell, 2003; Ludwig, 1998; Siegel et al., 2017). Recent advances to this literature identify mechanisms that increase violence, finding substantial increases in the availability of stolen guns and reductions in police solving gun crimes (Donohue et al., 2022).

A primary limitation of the existing literature is that it does not distinguish between "shall issue" and permitless carry, and therefore has limited policy relevance to the ongoing, rapid adoption of permitless concealed carry. The existing literature assesses "shall issue" and permitless carry laws together as a single, less restrictive policy often referred to as a "right-to-carry" law. In practice, analyses of "right to carry" laws include a substantial and increasing proportion of permitless carry states because all permitless carry states, save Kansas, were previously "shall issue" states.[1]

Recent work examining permitless carry adoption (Moody and Lott 2022; Doucette et al. 2023) finds mixed results concerning permitless carry law adoption. Kyriazis (2022) primarily examines changes in crime as a mechanism affecting police shootings after permitless carry adoption, finding a positive impact on aggravated assaults, and single state research by Smith & Petrocelli (2019) focused on Arizona's 2010 permitless carry policy adoption find null effects on handgun-related firearms offenses in the years following adoption.

## 3. Conceptual Framework

In contrast to previous studies of concealed carry laws, I am able to consider rapid and dynamic impacts on crime outcomes. Given that the nature of a permitless carry law is that a gun owner can instantaneously carry concealed weapons in public without the need to obtain a license, a process which can take several months or even years, it is logical to expect swift policy impacts that take effect over the course of several months after a permitless carry law takes effect.

Firearm owners may increase public firearm carrying if they expect to encounter other armed individuals in public more often, and conflicts that may previously have been settled without access to a gun may be more lethal. Increased public handgun carrying could also result in changes to firearm storage practices both in homes and vehicles as firearms may be more often stored to facilitate more frequent, ready access. Increased general gun availability and carrying may also increase rates of gun theft, which can facilitate gun access to prohibited possessors.

In addition to inducing ownership, the enactment of permitless carry may affect the composition of handgun carriers. Handgun carriers unwilling to complete licensing paperwork and a background check prior to permitless carry enactment are newly eligible to carry a concealed weapon and may elect to. This change in the composition of carriers may be minimal, however, because in most states less than 1 percent of license applications are denied and the majority of individuals who want to carry a handgun may already be able to do so (Phillips 2013; Shapira 2018).

Finally, permitless carry may influence gun ownership and result in gun violence by simply increasing the number of households with firearms. While no data currently exists that comprehensively tracks gun ownership, concealed carry licensing policies are associated with increased firearm background checks, a proxy for firearm sales (Steidley, 2019). However, firearm background checks are at best suggestive evidence of the potential for changing gun ownership because existing gun owners tend to own multiple firearms and most background checks are for additional firearm purchases (Azrael et al., 2017).

[1] Inclusion of permitless carry states in analyses of "right-to-carry" laws may explain some portion of the finding that "right-to-carry" laws differ across the timeframe during which states enacted these laws. The most recent wave of "shall issue" concealed carry adoption, from 2001 to 2014, is associated with higher increases in violent crime and homicide relative to states that changed policies in the mid-1990s (Donohue 2019; Siegel 2020), and include permitless carry state adopters.

In addition to changing behaviors of firearm carriers, permitless carry laws alter the ability of police to ascertain legal gun possession during public encounters such as traffic stops. Police may no longer ask for a permit to ascertain legal gun possession, though some states maintain "duty to inform" laws requiring a gun owner to disclose to police whether or not they are carrying a firearm during encounters with police. Police in permitless carry states rely on alternatives such as, in some cases, searches across multiple databases to determine if a gun possessor is in legal possession of a weapon. Police may therefore elect to reduce resources spent searching for illicit firearms, and procedurally may be more limited.

*3.1 Announcement, Enactment, and Anticipation of Permitless Carry Laws*

Most permitless carry laws are first announced and adopted by state legislatures and signed into law 3-4 months before the law takes effect (Table 1). This relatively short time window may result in anticipatory behavior in advance of regulation or confusion surrounding the announcement versus effective date. Both gun owners and law enforcement may react to future expectations about permitless carry law implementation by altering behavior in advance of when the law takes effect.

Gun owners may increase carrying in advance of the law implementation, constituting a positive anticipatory response. Approximately forty percent of existing male gun owners report watching gun-related television or videos, or visiting websites about guns (Parker et al., 2017). Gun owners tend to associate with other gun owners, which may also be a pathway for gun-related information sharing or behavior.

Police may also change behavior in advance of the effective date of permitless carry law adoption. Law enforcement responses could be either positive or negative in response to permitless carry. An emerging literature finds that law enforcement respond rationally to changes to punishment of offenders, meaning they could decrease effort enforcing weapons law violations such as improper carrying of a concealed carry weapon if they anticipate that their enforcement will not result in sanction after permitless carry takes effect. Law enforcement could also increase efforts to reduce gun violence by if anticipatory responses by gun owners increase short-term violence.

It is empirically challenging to disentangle anticipatory responses from other, unrelated changes in crime. To test for anticipatory response, I use the fact that different crime types should respond differently to an anticipated permitless carry law. I compare anticipatory responses between aggravated gun assaults, which should be highly impacted, and larceny-theft, which should not be impacted by permitless carry law announcement or enactment. In addition, I examine trends in Internet searches to compare searches for information about permitless carry (or constitutional carry as is more commonly used in searches in most states) in the weeks before and after announcement and effective dates in permitless carry states. I will find that Internet searches for information about permitless carry are concentrated during the weeks when permitless carry is announced or enacted, rather than the law's effective date (Appendix Figure B1).

## 4. Data

I use the RAND State Gun Law Database to identify states that adopted permitless carry policies between 2014 and 2019 and obtain the date each state adopted a permitless carry policy (Cherney et al. 2022). During this timeframe, twelve states adopted a permitless carry policy (Table 1). Due to substantial confusion about the effective date of permitless carry adoption, I omit Arkansas from the analysis sample

(Froelich, 2018). Concurrent gun law passage is another concern for treatment states as other laws may bias estimates of treatment effects. Missouri amended an existing "Stand Your Ground" law in 2016, the year before permitless carry was enacted, removing the duty to retreat requirement. the year before permitless carry was enacted. I elect to include Missouri in main specifications as the law is modified and not newly enacted, and show in robustness checks (Figure 6) that results are not driven by inclusion of Missouri.

For outcome data, I use two sources of police data from the Federal Bureau of Investigation (FBI) Uniform Crime Reporting (UCR) program. The FBI's National Incident Based Reporting System (NIBRS) gathers extensive information about crimes, victims, and offenders from a limited set of police agencies. In 2021, the FBI adopted NIBRS as the nation's sole reporting program without ensuring that law enforcement agencies were able to report data to the new system. Over 60% of the nation, including many large cities and entire states, were unable to make the transition to NIBRS. While this transition has hamstrung U.S. crime data collection, NIBRS does gather data from each state that adopted permitless carry, though not from all agencies (Table 1).

To ensure that results are not driven by the composition of the NIBRS sample, I additionally use the FBI Summary Reporting System (SRS), often referred to as the "UCR Reports." The UCR SRS spans all 50 states and over 90% of the U.S. population. UCR SRS records summary counts of reported crime offenses and does not include the same granular detail as in NIBRS.

Violent crime outcomes include aggravated assaults and robberies, which are serious violent crimes where victims experience the threat of force, physical attack, or both, and can inflict severe injury. Each of these crimes may be committed with a weapon, and additional outcomes include whether each was committed with a firearm or non-firearm weapon, such as a knife or a blunt instrument. Non-violent reported crimes include burglary, motor vehicle theft and larceny. Across both sources, I use monthly counts of reported crime incidents to contrast violent and non-violent reported crime, though in some specifications as violent crime outcomes are often zero, I aggregate monthly measures into 2-month pooled measures.

*Crime Data Quality and Analysis Sample*

Police agencies report data voluntarily to the FBI and many do not consistently and accurately report crime data (Evans & Owens, 2007; M. Maltz, n.d.; M. D. Maltz & Targonski, 2002; Mello, 2019). To address these potential data quality problems, I follow Mello (2019), Weisburst (2019) and Evans and Owens (2007) in determining which police agencies report crime data consistently and reliably. I start by including only agencies that report offenses each month of the year in the sample.

Because I use monthly or pooled 2-month measures of crime outcomes rather than yearly measures, I make some modifications to the regression-based process of detecting outliers employed in the literature. Monthly measures of crime are highly variable time series, often due to high levels of seasonality but also caused by lapses in offense reporting. Local linear regression or quadratic methods appropriate for yearly data are less well-suited to differentiate between unreliable crime reporting and high seasonal variation in monthly data. To identify agencies without drops or lapses in reporting but allowing for seasonal variation, I compare changes in the percentiles of an agency's monthly reported total crimes both within agency and between agencies serving similarly-sized jurisdictions. I remove an additional 4% of the NIBRS sample and approximately 10% of the SRS sample. Appendix A fully documents this process.

For agencies that consistently report accurate monthly crime data, I use monthly summary counts of total reported crimes from January 1, 2014 to February 29, 2020 to construct agency-month panels. I use monthly measures to capture short-term, immediate outcome measures as treatment effects should take place over a relatively short time-frame. Both South Dakota and Kentucky adopted permitless carry effective July 1, 2019 which, omitting the adoption month, permits a seven-month post-period prior to the start of the pandemic in March 2020. Agencies increasingly reported data to NIBRS during the study timeline. I use a balanced panel of agencies who reported during the seven year study timeframe.

As a comparison group, I use agencies that report accurate crime data in states that did not adopt a permitless carry law during the study timeframe. Because many states report to the SRS and not to NIBRS, comparison group states differ between samples. NIBRS includes fewer states in the comparison group, and while the non-reporting states tend to be large, such as California or New York, they also have more restrictive "may issue" firearm regulations in place that may make them less appropriate comparison states. I use primarily "shall issue" states as comparisons in the NIBRS sample. In the SRS sample, I am able to use high regulation "may issue" states, low regulation "shall issue" states and states that are not yet treated but adopt permitless carry after 2020. I find that results are generally robust to composition of the comparison group.

Table 2 presents summary statistics for balanced panels for agencies who report crime consistently. Permitless carry states have lower levels of violent crime, both firearm and non-firearm, compared to the shall issue state comparison group as well as lower levels of property crime.

*NIBRS*

NIBRS offers unique insight into crime by gathering detailed data that extend beyond summary counts of reported crime offenses. NIBRS consists of 4 primary data segments, including crime incidents, victims, offenders and arrests. Each of these segments gather incident-level data including information on: offense location, circumstances, and detailed victim and offender demographics In addition, NIBRS records information about applicable weapons and injuries that occurred in the course of a crime.

NIBRS coverage of states that adopted permitless carry is nearly comprehensive. NIBRS does not have data for earliest states that adopted permitless carry, which include Alaska, Arizona and Wyoming, but it does include nearly full or partial agency coverage for all other state adopters in 2015 and onward.

Several measures of crime are unique to NIBRS. First, NIBRS records the location where a crime occurs. Crimes occurring in homes are an existing field in NIBRS, but to measure crime occurring in public places, multiple locations are combined. These locations include: shopping mall, service/gas station, specialty store, liquor store, highway/road/alley, grocery/supermarket, government/public building, amusement park, air/bus/train terminal, arena/stadium/fairgrounds/coliseum, bank/savings and loan, bar/nightclub, convenience store, department/discount store, drug store/doctor's office/hospital, restaurant. Second, NIBRS records offender and victim demographics, including age, race/ethnicity and sex. While victim demographic information is well-populated, information on offenders is present in only approximately 60% of all violent crimes and in less than 50% of crimes committed with firearms in the sample and is omitted.

*UCR SRS*

The UCR SRS served as the nation's longstanding police data system until 2021. The majority of the nation's approximately 18,000 law enforcement agencies reported data to the UCR SRS for over eight decades. UCR SRS data on counts of main crime outcomes, including aggravated assaults, robberies,

burglaries, motor vehicle thefts and larceny are used alongside whether assaults and robberies are committed with firearms.

## 5. Empirical Strategy

I use a difference-in-differences framework to examine the effect of permitless carry policies on crime and victimization. The model estimates the change in crime outcomes in police agency jurisdictions in states that adopted a permitless carry policy relative to agencies in states that did not adopt a permitless carry law. In contrast to difference-in-differences approaches in the literature, I do not estimate state-year models. Instead, I estimate the effects of permitless carry laws where each observation is at the month or pooled 2-month level for dynamic difference-in-differences specifications or event studies. I use a seven-month post period, as discussed above, as effects should be observed quickly.

As motivated by the conceptual framework and recent advances in differentiation between anticipation effects and endogeneity, I first compare the effects of permitless carry law adoption on two outcomes likely to be highly impacted by anticipatory effects, aggravated assaults with a gun, in contrast to larceny-theft, which is unlikely to be impacted by anticipation of permitless carry laws taking effect. I use strategies proposed in Malani and Reif (2015) that employ leads of the observable treatment variable to account for forward-facing observed treatment effects. Accounting for positive anticipation effects may be particularly important in this setting as it could result in underestimation of the effects of permitless carry laws.

I start with a model that does not incorporate anticipatory effects and add on leads that incorporate anticipation. This baseline model, sometimes referred to as myopic, takes on the following form:

$$Outcome_{imy} = exp(\beta_1 Permitless_{imy} + \gamma_i + \phi_m + \tau_y + log(pop_{iy}) + \epsilon_{imy}) \qquad (1)$$

Where $Outcome_{imy}$ is a crime outcome in police agency jurisdiction *i* in month *m* and year *y*. The coefficient $\beta_1$ is the coefficient of interest that captures the average treatment effect in states that adopted permitless carry policies compared to those that did not during the seven-month post-period. Fixed effects include $\gamma_i$, which is a fixed effect for each agency jurisdiction as well as $\phi_m + \tau_y$ which are fixed effects for month and year to control for seasonal variation in crime. Standard errors are clustered at the level of treatment, the state in which the agency is located. The variable $Permitless_{imy}$ takes on the value of 1 in the month during which a state enacts a permitless carry policy. States whose policies took effect in the middle of a month are classified according to whether the majority of days within the month are under the newly active permitless carry law.

To account for a large proportion of outcomes that are zero-count values, I use a Poisson fixed effects estimator. While much of the literature employs logged or otherwise transformed outcome variables, recent research suggests that choices in how outcomes are logged may result in different treatment effects (Chen & Roth, 2023; Mullahy & Norton, 2022). I include the log of each police agency jurisdiction's population as a continuous control constrained to one. Coefficients approximate the percent change in the rate of crime per agency population in the post-period. To obtain the percent change in the

rate of crime outcomes in treated agencies, coefficients are transformed by 100 x (exp($\beta_1$)-1), which I report as the law effect size.[2]

Recent advances in the literature have raised concerns with two way fixed effect (TWFE) difference-in-difference models used to estimate treatment effects where units are not treated simultaneously (Goodman-Bacon 2021, Callaway and Sant'Anna 2021, Sun and Abraham 2021). In this application, it is possible that treatment unit heterogeneity exists as changes in firearm behaviors resulting from policy enactment take place over the course of several months. Because I estimate short term policy effects at the month-level, I have less potential for time-varying unobserved confounders. However, because gun acquisition is correlated with elections, and this study includes the 2016 presidential election timeframe, temporal heterogeneity in particular may be a concern.

To address these concerns, I use Goodman-Bacon (2021) regression decompositions to investigate the robustness of TWFE models to heterogeneous treatment effects. Goodman-Bacon demonstrates that treatment effects obtained from staggered TWFE estimators are a weighted average of "2x2" comparisons, some of which introduce bias in estimation of treatment effects. I use balanced panels to decompose the proportion of 2x2 comparisons, finding that over 80% of the comparisons made do not introduce bias to treatment effects in Appendix Table C1. Because some crime outcomes are relatively rare, for precision I retain the TWFE specifications and report stacked regression estimates in Appendix C, which do not alter main results.

I next turn to models that account for anticipation, or quasi-myopic models, that include leading indicators for whether the law was yet enacted. These models take the form:

$$Outcome_{imy} = exp(\beta_1 Permitless_{imy} + \sum_{t=-8}^{T} \beta_j (Lead)_{im} + \gamma_i + \phi_m + \tau_y + log(pop_{iy}) + \epsilon_{imy}) \quad (2)$$

The leading indicators, $\beta_j$ take on the value of 1 if the law has been adopted at *j* leading periods before the first period of treatment. I fit models with leads that cover the anticipation dates in all treated states, or from 8 months to 2 months before the law's effective date. The coefficient $\beta_j$ date may be referred to as the ex-ante effect of treatment.

Because I find substantial anticipation effects extending to 4 months prior to treatment for aggravated assaults with firearms, I include leads to present gun violence outcomes (e.g. gun assaults, gun robberies) including 4 months prior to treatment. This is sensible as it corresponds with the law's announcement in the majority of states. I don't find strong evidence of anticipation effects for larceny-theft, and elect to keep event studies for crime outcomes (theft, burglary, motor vehicle theft, robbery, aggravated assault) and present specifications including and omitting leads.

Several identifying assumptions underlie the difference-in-differences strategy. One key identifying assumption is that without a permitless carry policy, crime outcomes would have trended similarly in states that did enact a permitless carry law compared to those that did not. Differentiating between pre-trends, which violate the identifying assumption, and anticipation effects is particularly challenging in this setting. To address these concerns, I use evidence of anticipation effects from gun

[2] Including the log of the population in each agency and constraining its coefficient to be 1 is equal to the crime rate as the dependent variable. In Stata and in R's fixest, this is done using the xtpoisson or fepois() functions and passing in population as the argument to the exposure or log(population) to offset.

assaults and thefts to inform what constitutes months in which treatment and anticipatory effects may occur.

I present event study estimates to examine for evidence of differing trends and anticipation effects for treatment and comparison states. The event study or dynamic difference-in-differences model takes the following form:

$$Outcome_{imy} = exp(\sum_{t=-24;\, t\neq -2}^{T} \beta_t \, (Lead\ t)_{im} + \sum_{k=0}^{K} \beta_k \, (Lag\ k)_{im} + \gamma_i + \phi_m + \tau_y + log(pop_{iy}) + \epsilon_{imy}) \quad (3)$$

Where $Outcome_{imy}$ is a crime outcome in police agency jurisdiction *i* in month *m* and year *y*. The variables *k* and *j* assume a value of one in the corresponding month prior to or post treatment. The coefficients of interest, $\beta_k$ , capture the average treatment effect in states that adopted permitless carry policies compared to those that did not in each of the *k* months after treatment, relative to the month prior to policy adoption. I estimate event studies with outcomes pooled over 2 months as some months and agencies have sparse outcome data available. Informed by anticipatory effects, I estimate gun crimes relative to 4 months prior to treatment and other crime types relative to the standard time before treatment takes effect as I do not observe significant larceny-theft anticipation effects.

Finally, limited research into state gun policy adoption suggests that if bordering states adopt a gun law, its neighboring states are more likely to adopt the law as well (Clark et al. 2022). This is the case for both permissive policies, such as permitless carry, as well as restrictive policies. State political alignment is also predictive of state policy adoption, particularly for state policy changes in the last two decades, which characterizes the permitless carry adoption period (DellaVigna et al. 2022). Exogenous treatment is another underlying assumption, which would be violated if states enacted permitless carry to address rising rates of gun crime. While the majority of permitless carry states fall among the lowest in the nation for rates of firearm homicide and violent crime making this scenario less likely, several rank among the highest, including Missouri. To address the potential that adopting states differ from those that did not enact a permitless carry law, I additionally compare adopting states to only future adopting states through the present day (e.g. 2020-2023 adopting states) in robustness checks.

## 6. Results

I first report estimates from a baseline model that does not account for anticipatory effects for both aggravated assaults with a gun and larceny theft, two outcomes with a high and low likelihood of being affected by anticipatory behavior changes resulting from permitless carry laws. Column 1 of Table 3 estimates that that post period coefficient with no leading indicators is 0.090 or an approximately 9% increase in aggravated assaults following the effective date of a permitless carry law in SRS agencies in adopting states relative to agencies in states that did not adopt a permitless carry law. In columns 2-5, treatment effects increase from 0.09 to .109 as leads are added to the specification (Equation 2). Leads in periods 2 and 6 are significant, corresponding to anticipatory effects approximately 2-6 months prior to the effective date of the permitless carry law. Effects are similar in NIBRS outcomes in Panel B. In contrast, larceny theft remains insignificant across specifications with significant leads in lead period 2.

As a result, I opt to include alongside baseline specifications leads extending to 2 months for crimes that do not pertain to gun crime and 4-6 months in specifications for gun-related crime outcomes.

The short-term effect of adopting a permitless carry policy in baseline models (Equation 1) is a 7.4 to 8.4 percent increase in aggravated assaults that is significant in both the SRS and NIBRS samples (Table 4). This implies that in the seven months following permitless policy adoption, at least 519 additional assault victimizations occurred due to policy implementation within the sample.[3] The effects of permitless carry policy adoption on robbery is less clear between samples. Robbery does not increase substantially in the larger SRS sample but does increase significantly using the NIBRS sample. Simple assault, which measures assaults that do not involve a serious weapon such as a knife or gun, does not significantly increase in either sample. This suggests that the increase in aggravated assaults is not the result of simple assaults that escalated, but rather of additional conflict resulting in serious violence. Taken together, this suggests no evidence that permitless carry policies decrease or deter interpersonal crime.

To explore whether permitless carry laws may reduce or deter other types of crime, I estimate the effects of permitless carry adoption on burglary, motor vehicle theft, and theft. The short-term effects of adopting a permitless carry policy on property crimes imply no significant decrease in crime. Burglary in baseline specifications does not increase significantly though does so with the addition of a leading indicator for 2 months prior to treatment.

Figure 3 presents event study results from Table 3 across SRS and NIBRS for the two years prior to permitless carry law enactment and 7 months after using agency-month observations aggregated to the 2-month level. Aggravated assault coefficients trend upward in each of the post-period treatment months relative to the month prior to policy adoption without significant pre-trends. Broadly, burglary and motor vehicle theft suggest little evidence of treatment effects and remain relatively flat. Larceny-theft deviates substantially in the two years prior to permitless carry enactment and less substantially closer to enactment. Robbery suggests some possible positive treatment effects in the post-period but no consistent pattern.

To assess whether results are driven by increases in violence are the result of gun violence, I compare aggravated assaults and robberies that are committed with a non-firearm weapon (e.g. blunt object, knife) to those committed with a firearm. Results suggest that anticipatory effects are likely to be more significant for gun-related crimes. In Table 5, column 1, gun assaults increased 9.46 to 10.1 percent in SRS and NIBRS samples without accounting for the potential for anticipatory effects. Controlling for leads at t=-2 and t=-4 suggest more substantial increases in gun assaults. Figure 4 estimates equation 3 for gun assaults relative to t-4, with significant increases in gun assaults in the following periods even before the law took effect. Gun robbery increases 8.1% in the SRS sample but not in NIBRS, which may be driven by a greater proportion of Missouri police agencies included in the sample. While assaults that do not involve firearms remain flat in the SRS, estimates using NIBRS suggest that non-firearm assaults increased.

Next, I examine whether the increase in aggravated assaults is driven by firearm assaults that occur in public places. If permitless carry policies, which remove licensing restrictions on public gun carrying, affect gun violence, the treatment effects should be concentrated in public places. These detailed

[3] This is calculated as follows using the guidance in Falarco et al. (2022) to account for seasonality in the month-level model: a predictive value is created for treatment groups pre- and post-intervention that is summed to generate the predicted number of events under the counterfactual. The two totals are subtracted to obtain the total number of events, or in this case, assaults.

outcomes are only available in the NIBRS sample. I find that for both aggravated assaults and robberies with guns, the increase is driven by public violence among male victims (Figure 5).

**6.1 Heterogeneity**

Violent crime and in particular gun crime is generally concentrated in dense, more populated communities. It is reasonable to expect that policy effects may therefore differ by community population. Table 6 presents violent crime, gun crime and non-gun crime victimization effects for permitless carry adoption among agencies that serve populations greater than 90,000 in the SRS and NIBRS samples. Column 1 suggests that violent crime increases in communities with a greater population are higher in magnitude than in the overall sample including smaller communities. In larger communities, aggravated assaults increased between 9 and 13% in the SRS and NIBRS.

**6.2 Robustness**

To ensure that results are not driven by sample composition or selection of comparison group, I estimate main effects using different comparison groups for both in Figure 6. I consider three comparison groups. First, I consider all agencies that consistently report data in the comparison group. In the SRS, this constitutes nearly nationwide coverage with the exclusion of Florida for problematic reporting practices. Second, as a reference, I include the main specification which employs a comparison group consisting of only shall issue states, or states with less restrictive handgun laws that may be most likely to consider and adopt a permitless carry law. Finally, I include a comparison group composed of states that, during my study timeframe, had not yet adopted permitless carry but would do so after 2020. These states include large adopters such as Texas, Indiana, and Ohio, each of whom report both to NIBRS and the SRS. I additionally estimate an OLS TWFE model with a logged dependent variable. I include only those agencies with a population greater than 20,000 to decrease bias stemming from small agencies with large proportions of zeros in outcome variables. Broadly, I find that treatment effects of permitless carry policy enactment on crime outcomes are similar across different comparison groups for both SRS and NIBRS.

*Stacked Difference-in-differences estimator*

I implement a stacked difference-in-differences model (Baker et al., 2022; Cengiz et al., 2019; Deshpande & Li, 2019), which reduce the potential for biased estimates stemming from inappropriate comparisons between treated and comparison units. Comparisons between states undergoing treatment during the study timeframe and states that have already been treated can introduce bias to estimates of treatment effects. Because stacked regression models allow researchers to specify comparisons for each treated unit, stacked regression can omit problematic comparisons between treated units and already-treated units.

To estimate the stacked difference-in-differences model, I modify the NIBRS and UCR datasets to consist of stacks for each treated state. Each stack consists of the treated state and comparison group states. Comparison states are included in the stack if they are not ever treated during the study timeframe, or if, relative to the treated unit, the comparison state is not yet treated. I allow not-yet-treated units to enter into the comparison group if they do not undergo treatment during the 7-month post-period for a treated unit. No states that are already treated are used in the comparison group. I also create a stack-specific identifying variable.

These treatment unit-specific stacks are then stacked, or appended, together and a difference-in-differences estimator is applied to the stacked dataset. Each stack is fitted with unit and time

fixed effects and a stack-specific indicator variable. Average treatment effects are aggregated from each stack by averaging individual stack treatment effects. Ongoing research suggests that clusters at the state-cluster level are appropriate for policy changes at the state level (Wing et al., 2021).

The stacked difference-in-differences model specifically takes on the following form:

$$Outcome_{dimy} = exp(\beta_1 Permitless_{dimy} \times Stack_{dimy} + \gamma_{di} + \phi_{dm} + \tau_{dy} + log(pop_{diy}) + \epsilon_{dimy}) \quad (3)$$

Where $d$ is a stack-specific identifier and $\gamma_{di}$, $\tau_{dy}$, $\phi_{dm}$ are agency, month, year fixed effects. The coefficient $\beta_1$ captures the average treatment effect across treated states in stack $d$ in the 7-month post-period. Standard errors are clustered at the state-stack level.

I find that stacked difference-in-differences estimates are similar in magnitude and significance to TWFE specifications.

### 6.3 Mechanisms

In this section, I explore mechanisms that may account for policy effects. I fit Poisson models with the difference-in-differences framework outlined in Equation 1. I use additional outcomes from NIBRS and UCR SRS to compare states outcomes in states that adopted a permitless carry policy compared to those that did not to examine changes in police enforcement targeted at public firearm carrying. Police recorded substantially fewer weapons law violations in advance of treatment (Figure 6). Police enforcement of weapons law violations arrests decreased by 14.9 percent in the months following permitless carry adoption. Less police focus on weapons law violations may change public behaviors of gun carriers.

## 7. Discussion

Adoption of a permitless carry law increases violent interpersonal assaults, particularly assaults with firearms, without evidence of decreases in other types of crime. Firearm violence increases in public places among males, suggesting that the increase in violence is not caused by changes in gun ownership but rather by changes to carrying frequency or behaviors while carrying a concealed handgun. Permitless carry policies are accompanied by a substantial reduction in police recording of crimes and arrests for weapons law violations, which may reduce the risks associated with high risk or illegal gun carrying.

Understanding whether treatment effects are driven by the number of handgun carriers or the frequency of carrying, the extensive and intensive margins of carrying, has direct relevance to public policy. Importantly, this research finds rapid increases in violence resulting from permitless carry in a seven-month time period. This evidence, taken together with survey data showing that Americans doubled the rate at which they carry concealed handguns in public, suggests that observed increases in gun violence stemming from permitless carry occurred without substantial increases in gun ownership. It is likely that the frequency of gun carrying on the part of existing owners drives a substantial part of the observed policy impacts. There may therefore exist policy options to reduce gun violence as other factors including carrying behaviors, firearm storage, illegal market availability, and policing may be more salient than legal ownership in influencing observed outcomes. Policy interventions that reduce carrying

during high risk times and places may be particularly effective, such as location restrictions in sensitive public areas or options to store firearms safely in public.

Future research should assess to what extent individuals who were previously unable to obtain concealed carry licenses are increasing carrying behavior. If individual carriers drive some component of the observed increase in violence, then policy mechanisms focusing on prohibited firearm possessors may be effective. However, this path may be difficult for law enforcement agencies, which preliminarily report that their ability to identify illegal gun possession is made more difficult under permitless carry laws.

## TABLES AND FIGURES

**Table 1: Permitless Carry Adoption States (2015-2019)**

| State | Announcement Date | Effective Date | Months Between Announcement and Effective Date (rounded) | Included in Study Timeframe | Included in NIBRS Sample | Included in Analysis Sample |
|---|---|---|---|---|---|---|
| Arkansas | -- | unclear | -- | No | No | No |
| Idaho | 3/38/2017 | 7/1/2016 | 3 | Yes | Yes | Yes |
| Kansas | 4/2/2015 | 7/1/2015 | 3 | Yes | Yes | Yes |
| Kentucky | 4/11/2019 | 6/26/2019 | 4 | Yes | Yes | Yes |
| Maine | 7/8/2015 | 10/15/2015 | 4 | Yes | Yes | Yes |
| Missouri | 9/11/2017 | 1/1/2017 | 4 | Yes | Yes | Yes |
| Mississippi | 4/6/2016 | 4/15/2016 | >1 | Yes | Yes | Yes |
| North Dakota | 4/23/2017 | 8/1/2017 | 4 | Yes | Yes | Yes |
| New Hampshire | 2/9/2017 | 2/22/2017 | 7 | Yes | Yes | Yes |
| Oklahoma | 2/27/2018 | 11/1/2019 | 8 | Yes | Yes | Yes |
| South Dakota | 1/31/2019 | 7/1/2019 | 6 | Yes | Yes | Yes |
| West Virginia | 3/5/2016 | 5/24/2016 | 3 | Yes | Yes | Yes |

Notes: The NIBRS sample includes the following comparison states: CO, CT, DE, GA, HI, IA, IN, LA, MA, ME, MI, MN, MT, NE, NM, OH, OR, PA, RI, SC, TN, TX, UT, VA, WA. States that reported to NIBRS but that already enacted permitless carry including AZ and VT are excluded. Additionally, IL and WI are omitted from all comparison groups as both transitioned from not allowing concealed handguns in public to issuing licenses during the study timeframe.

**Table 2:** Descriptive Statistics of NIBRS Agency-Month Crime Offenses and Victims

| | Permitless Carry | | Shall Issue | | All States | |
|---|---|---|---|---|---|---|
| | Mean | Sd | Mean | Sd | Mean | Sd |
| **Panel A: UCR SRS** | | | | | | |
| *Interpersonal Crime* | | | | | | |
| Aggravated Assault | | | | | | |
| All | 5.1 | 37.3 | 8.2 | 55.3 | 10.0 | 82.5 |
| Gun | 1.5 | 18.0 | 2.5 | 22.5 | 2.5 | 22.3 |
| Non-gun | 2.2 | 15.7 | 3.8 | 28.4 | 5.0 | 43.8 |
| Robbery | | | | | | |
| All | 1.3 | 13.5 | 2.9 | 30.3 | 4.1 | 44.8 |
| Gun | 0.7 | 7.6 | 1.4 | 16.7 | 1.6 | 17.2 |
| Non-gun | 0.2 | 1.9 | 0.5 | 4.4 | 0.7 | 8.5 |
| Simple Assault | 18.9 | 79.2 | 33.9 | 177.8 | 35.5 | 181.2 |
| *Property Crimes* | | | | | | |
| Burglary | 10.1 | 52.0 | 16.1 | 83.7 | 17.9 | 88.0 |
| Motor Vehicle Theft | 4.8 | 34.8 | 7.9 | 56.1 | 9.9 | 66.8 |
| Larceny-theft | 35.1 | 169.7 | 61.2 | 297.1 | 69.3 | 370.7 |
| *Observations* | 60,865 | 60,865 | 242,036 | 242,036 | 307,415 | 307,415 |
| Agencies | 1,645 | 1,645 | 6,542 | 6,542 | 8,309 | 8,309 |
| **Panel B: NIBRS** | | | | | | |
| *Interpersonal Crime* | | | | | | |
| Aggravated Assault | | | | | | |
| All | 5.7 | 40.4 | 11.3 | 60.8 | 11.2 | 58.0 |
| Gun | 1.4 | 17.4 | 3.2 | 25.0 | 2.9 | 23.6 |
| Non-gun | 1.4 | 8.8 | 3.0 | 15.5 | 3.0 | 14.9 |
| Gun - Public | 0.5 | 6.4 | 1.0 | 8.4 | 1.0 | 8.0 |
| Gun - Male | 0.8 | 9.6 | 1.9 | 14.0 | 1.7 | 13.3 |
| Robbery | | | | | | |
| All | 1.9 | 18.3 | 4.6 | 31.7 | 4.5 | 30.2 |
| Gun | 0.9 | 10.0 | 2.1 | 17.4 | 2.0 | 16.5 |
| Non-gun | 0.2 | 1.8 | 0.5 | 2.8 | 0.5 | 2.8 |
| Simple Assault | 18.0 | 66.5 | 34.7 | 118.9 | 33.8 | 113.9 |

| | | | | | | |
|---|---|---|---|---|---|---|
| *Property Crimes* | | | | | | |
| Burglary | 13.2 | 49.4 | 24.3 | 91.5 | 23.3 | 87.2 |
| Motor Vehicle Theft | 5.2 | 31.4 | 10.7 | 53.0 | 10.0 | 50.3 |
| Larceny-theft | 13.1 | 63.9 | 29.6 | 116.0 | 28.6 | 110.8 |
| *Observations* | 25,049 | 25,049 | 82,214 | 82,214 | 92,167 | 92,167 |
| Agencies | 677 | 677 | 2,222 | 2,222 | 2,491 | 2,491 |

Notes: The NIBRS sample includes 3,836 agencies that reported crime data in each year of the sample. The SRS sample includes 8,385 agencies that similarly reported in all years. Crime outcomes are reported at the agency-month level.

**Table 3:** Estimates of Anticipatory Effects: Aggravated Assaults with Guns and Larceny-theft

| | Aggravated Assault - Gun | | | | | Theft | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (1) | (2) | (3) | (4) | (5) |
| **Panel A: UCR SRS** | | | | | | | | | | |
| Post Period effect (t-0) | 0.090*** | 0.098*** | 0.098*** | 0.105*** | 0.109*** | 0.016 | 0.021 | 0.023 | 0.024 | 0.019 |
| | (0.019) | (0.021) | (0.021) | (0.022) | (0.024) | (0.020) | (0.020) | (0.020) | (0.021) | (0.022) |
| Lead effect (t-2) | | 0.082 | 0.082 | 0.0888* | 0.0926* | | 0.062* | 0.064* | 0.065* | 0.060 |
| | | (0.045) | (0.045) | (0.045) | (0.046) | | (0.030) | (0.030) | (0.032) | (0.031) |
| Lead effect (t-4) | | | 0.001 | 0.008 | 0.012 | | | 0.020 | 0.021 | 0.016 |
| | | | (0.036) | (0.038) | (0.041) | | | (0.020) | (0.022) | (0.022) |
| Lead effect (t-6) | | | | 0.057* | 0.061* | | | | 0.009 | 0.003 |
| | | | | (0.027) | (0.031) | | | | (0.027) | (0.027) |
| Lead effect (t-8) | | | | | 0.028 | | | | | -0.049 |
| | | | | | (0.041) | | | | | (0.027) |
| Number of Agencies | 2,523 | 2,523 | 2,523 | 2,523 | 2,523 | 2,330 | 2,330 | 2,330 | 2,330 | 2,330 |
| Observations | 90,831 | 90,831 | 90,831 | 90,831 | 90,831 | 83,881 | 83,881 | 83,881 | 83,881 | 83,881 |
| **Panel B: NIBRS** | | | | | | | | | | |
| Post Period effect (t-0) | 0.091* | 0.109** | 0.114** | 0.123** | 0.121** | -0.050 | -0.018 | -0.020 | -0.021 | -0.070 |
| | (0.040) | (0.042) | (0.041) | (0.042) | (0.041) | (0.038) | (0.040) | (0.041) | (0.044) | (0.045) |
| Lead effect (t-2) | | 0.178*** | 0.182*** | 0.191*** | 0.190*** | | 0.014 | 0.013 | 0.011 | -0.029 |
| | | (0.032) | (0.032) | (0.033) | (0.033) | | (0.036) | (0.037) | (0.040) | (0.037) |
| Lead effect (t-4) | | | 0.042 | 0.051 | 0.050 | | | -0.019 | -0.020 | -0.033 |
| | | | (0.043) | (0.044) | (0.046) | | | (0.027) | (0.028) | (0.035) |

| | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Lead effect (t-6) | | | | 0.0720* | 0.071 | | | | -0.015 | -0.063 |
| | | | | (0.036) | (0.037) | | | | (0.041) | (0.035) |
| Lead effect (t-8) | | | | | -0.011 | | | | | -0.102* |
| | | | | | (0.038) | | | | | (0.044) |
| Number of Agencies | 1,146 | 1,146 | 1,146 | 1,146 | 1,146 | 1,185 | 1,185 | 1,185 | 1,185 | 1,185 |
| Observations | 40,110 | 40,110 | 40,110 | 40,110 | 40,110 | 41,475 | 41,475 | 41,475 | 41,475 | 41,475 |
| Agency, Month Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Poisson regression estimates from Equation 2. Each column comes from a separate regression with increasing inclusion of lead indicators. Crime outcomes are counts of crimes in each agency in the NIBRS and SRS sample that report at least 1 crime. The level of observation is the agency-2-month level. Agencies are considered treated when treatment takes effect for the majority of the month observation. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the state level.

**Table 3:** Estimates of the Effects of Permitless Carry Adoption on Crime Outcomes

| | Interpersonal Crime | | | Property Crime | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Aggravated Assault | Robbery | Simple Assault | Burglary | Motor Vehicle Theft | Larceny-theft |
| **Panel A: UCR SRS** | | | | | | |
| Permitless Carry Law Effect Size (%) | 6.727 | 1.979 | 0.249 | 4.352 | 4.415 | 1.979 |
| Permitless Carry Law | 0.065** | 0.020 | 0.002 | 0.043 | 0.043 | 0.020 |
| | (0.023) | (0.032) | (0.015) | (0.024) | (0.028) | (0.024) |
| Permitless Carry Law - Lead effect (t-2) | 0.065** | 0.025 | 0.004 | 0.043** | 0.038 | 0.020 |
| | (0.024) | (0.030) | (0.012) | (0.016) | (0.026) | (0.013) |
| Number of Agencies | 4,267 | 4,267 | 4,267 | 4,267 | 4,267 | 4,267 |
| Observations | 149,345 | 149,345 | 149,345 | 149,345 | 149,345 | 149,345 |
| **Panel B: NIBRS** | | | | | | |
| Permitless Carry Law Effect Size (%) | 8.470 | 6.556 | 0.136 | 1.582 | -2.596 | -2.566 |
| Permitless Carry Law | 0.0859*** | 0.064 | 0.001 | 0.028 | 0.016 | -0.026 |
| | (0.025) | (0.038) | (0.013) | (0.017) | (0.022) | (0.035) |
| Permitless Carry Law - Lead effect (t-2) | 0.099*** | 0.069 | -0.003 | 0.047** | 0.017 | -0.018 |
| | (0.028) | (0.041) | (0.016) | (0.018) | (0.025) | (0.040) |
| Number of Agencies | 1,970 | 1,970 | 1,970 | 1,970 | 1,970 | 1,970 |
| Observations | 68,950 | 68,950 | 68,950 | 68,950 | 68,950 | 68,950 |
| Agency, Month Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Permitless carry law effect sizes are estimates of $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 1 which estimate the percent change in crime outcomes following enactment of a permitless carry law. Poisson regression estimates from Equation 1 for Permitless carry law rows and Equation 2 for Permitless carry law - lead effect rows. Crime outcomes are counts of crimes in each agency in the NIBRS and SRS sample that report at least 1 crime. The level of observation is the agency-2-month level. Agencies

are considered treated when treatment takes effect for the majority of the month observation. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the state level.

**Table 4:** Estimates of the Effects of Permitless Carry on Gun Crime vs. Non Gun Crime Outcomes

| | Aggravated Assault | | | Robbery | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | All | Gun | Non-Gun | All | Gun | Non-Gun |
| **Panel A: UCR SRS** | | | | | | |
| Permitless Carry Law Effect Size (percent) | 6.737 | 9.461 | 4.488 | 2.501 | 8.166 | -1.222 |
| Permitless Carry Law | 0.0652** | 0.0904*** | 0.0439 | 0.0247 | 0.0785* | -0.0123 |
| | (0.024) | (0.019) | (0.032) | (0.030) | (0.033) | (0.049) |
| Permitless Carry - Lead Effect (t-4) | 0.0632* | 0.105*** | 0.0405 | 0.0262 | 0.0873* | -0.0268 |
| | (0.031) | (0.022) | (0.040) | (0.038) | (0.041) | (0.062) |
| Number of Agencies | 2,205 | 2,205 | 2,205 | 2,205 | 2,205 | 2,205 |
| Observations | 82,817 | 82,817 | 82,817 | 82,817 | 82,817 | 82,817 |
| **Panel B: NIBRS** | | | | | | |
| Permitless Carry Law Effect Size (percent) | 8.470 | 10.164 | 5.622 | 3.676 | 8.448 | 1.461 |
| Permitless Carry Law | 0.0813*** | 0.0968** | 0.0547*** | 0.0361 | 0.0811 | 0.0145 |
| | (0.022) | (0.034) | (0.016) | (0.064) | (0.078) | (0.098) |
| Permitless Carry - Lead Effect (t-4) | 0.108*** | 0.123** | 0.0508 | 0.0676 | 0.0944 | 0.0542 |
| | (0.032) | (0.042) | (0.047) | (0.049) | (0.061) | (0.087) |
| Number of Agencies | 1,491 | 1,491 | 1,491 | 1,491 | 1,491 | 1,491 |
| Observations | 26,129 | 24,937 | 25,672 | 25,415 | 23,552 | 25,010 |
| Month, Agency Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Permitless carry law effect sizes are estimates of $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 1 which estimate the percent change in crime outcomes following enactment of a permitless carry law. Poisson regression estimates from Equation 1 for Permitless carry law rows and Equation 2 for Permitless carry law - lead effects for t-2 and t-4 for gun-related crime outcomes. Crime outcomes are counts of crimes in each agency in the NIBRS and SRS sample that report at least 1 crime. The level of observation is the agency-2-month level. Agencies are considered treated when treatment takes effect for the majority of the month observation. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the state level.

**Table 5:** Estimates of the Effects of Permitless Carry Law Adoption on Agencies Serving Populations>90,000

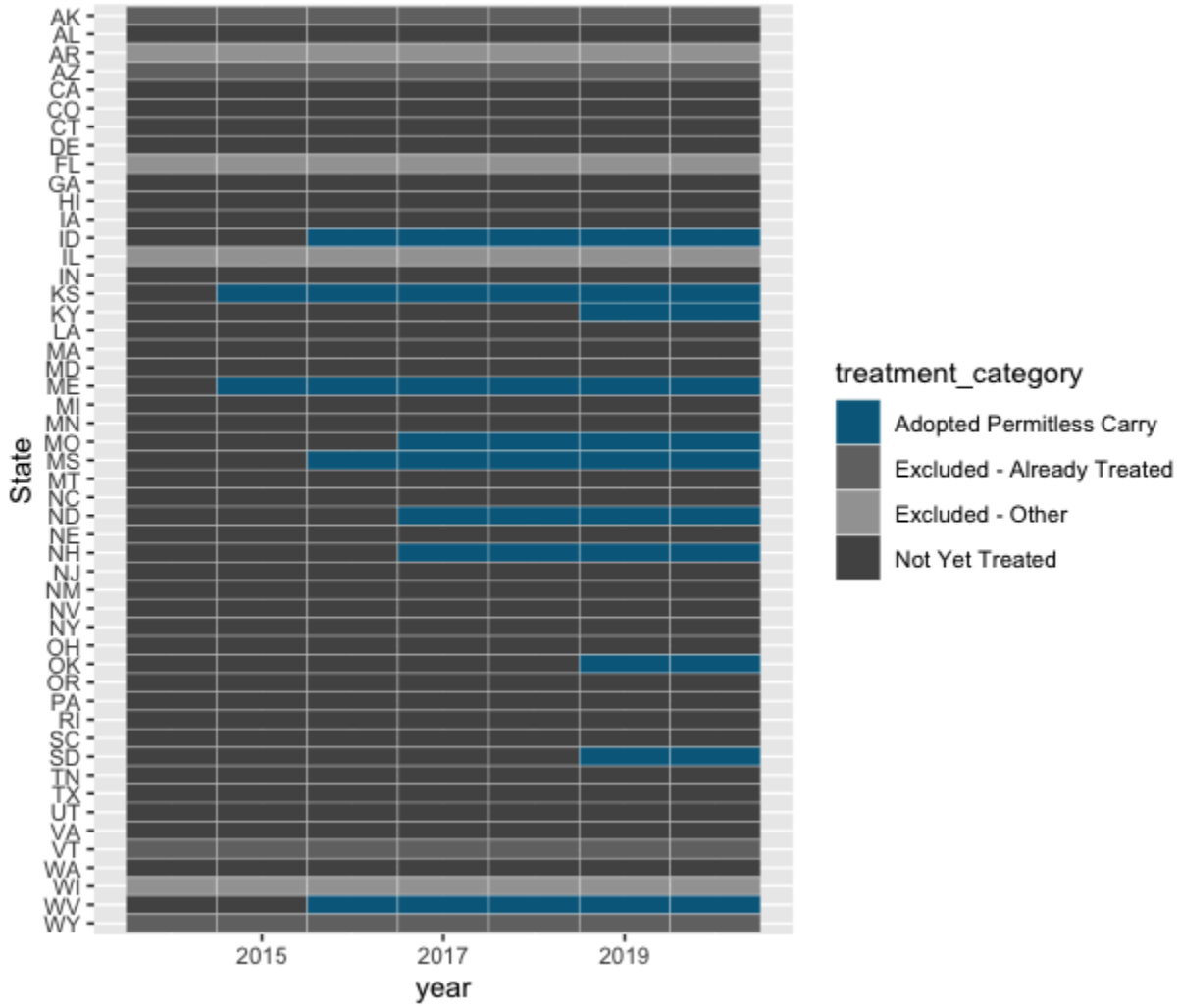
| | Aggravated Assault | | | Robbery | | |
|---|---|---|---|---|---|---|
| | All | Gun | Non-Gun | All | Gun | Non-Gun |
| **Panel A: UCR SRS** | | | | | | |
| Permitless Carry Law Effect Size (percent) | 9.133 | 10.849 | 5.760 | 3.221 | 8.752 | -3.950 |
| Permitless Carry Law | 0.0874** | 0.103*** | 0.056 | 0.032 | 0.0839* | -0.040 |
| | (0.032) | (0.021) | (0.048) | (0.041) | (0.041) | (0.070) |
| Permitless Carry - Lead effects | 0.0871* | 0.117*** | 0.051 | 0.037 | 0.095 | -0.058 |
| | (0.043) | (0.024) | (0.062) | (0.050) | (0.050) | (0.090) |
| Number of Agencies | 231 | 231 | 231 | 231 | 231 | 231 |
| Observations | 8,056 | 8,056 | 8,056 | 8,056 | 8,056 | 8,056 |
| **Panel B: NIBRS** | | | | | | |
| Permitless Carry Law Effect Size (percent) | 13.655 | 7.810 | 10.960 | 11.516 | 15.488 | 12.075 |
| Permitless Policy Law | 0.128*** | 0.0752 | 0.104 | 0.109* | 0.144* | 0.114 |
| | (0.037) | (0.063) | (0.065) | (0.049) | (0.067) | (0.089) |
| Permitless Carry - Lead effects | 0.157*** | 0.108 | 0.114 | 0.117* | 0.15 | 0.103 |
| | (0.045) | (0.065) | (0.075) | (0.059) | (0.081) | (0.117) |
| Number of Agencies | 115 | 115 | 115 | 115 | 115 | 115 |
| Observations | 4,049 | 4,049 | 4,049 | 4,049 | 4,049 | 4,049 |
| Month, Agency Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Permitless carry law effect sizes are estimates of $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 1 which estimate the percent change in crime outcomes following enactment of a permitless carry law. Poisson regression estimates from Equation 1 for Permitless carry law rows and Equation 2 for Permitless carry law - lead effects for t-2 and t-4 for gun-related crime outcomes. Crime outcomes are counts of crimes in each agency in the NIBRS and SRS sample that report at least 1 crime. The level of observation is the agency-2-month level. Agencies are considered treated when treatment takes effect for the majority of the month observation. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the state level.

**Table 6:** Estimates of the Effects of Permitless Carry on Weapons Offenses and Weapons Offenses Arrests **Forthcoming.**

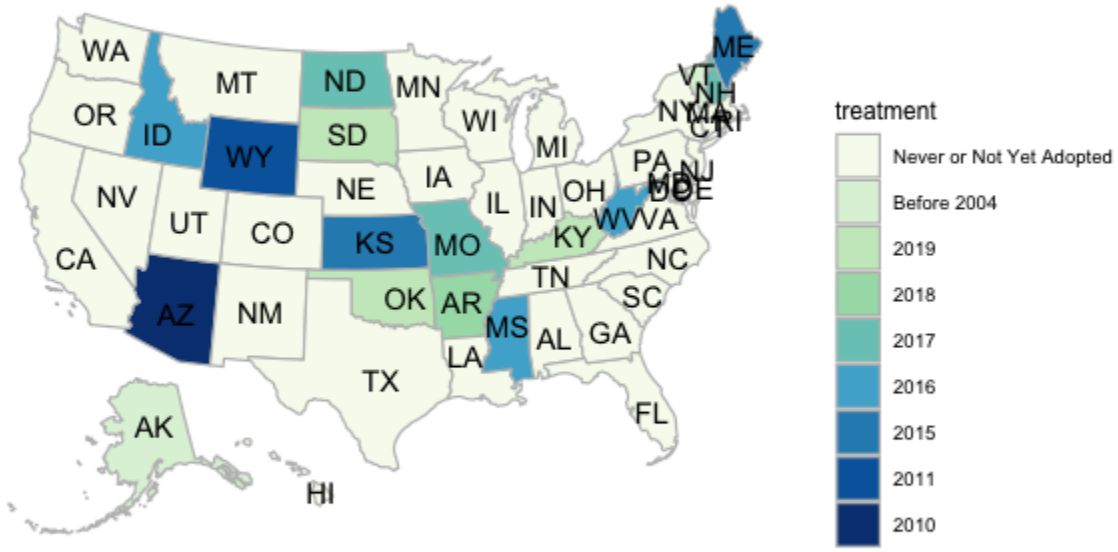
**Figures**

**Figure 1:** Permitless Carry Treatment Timing



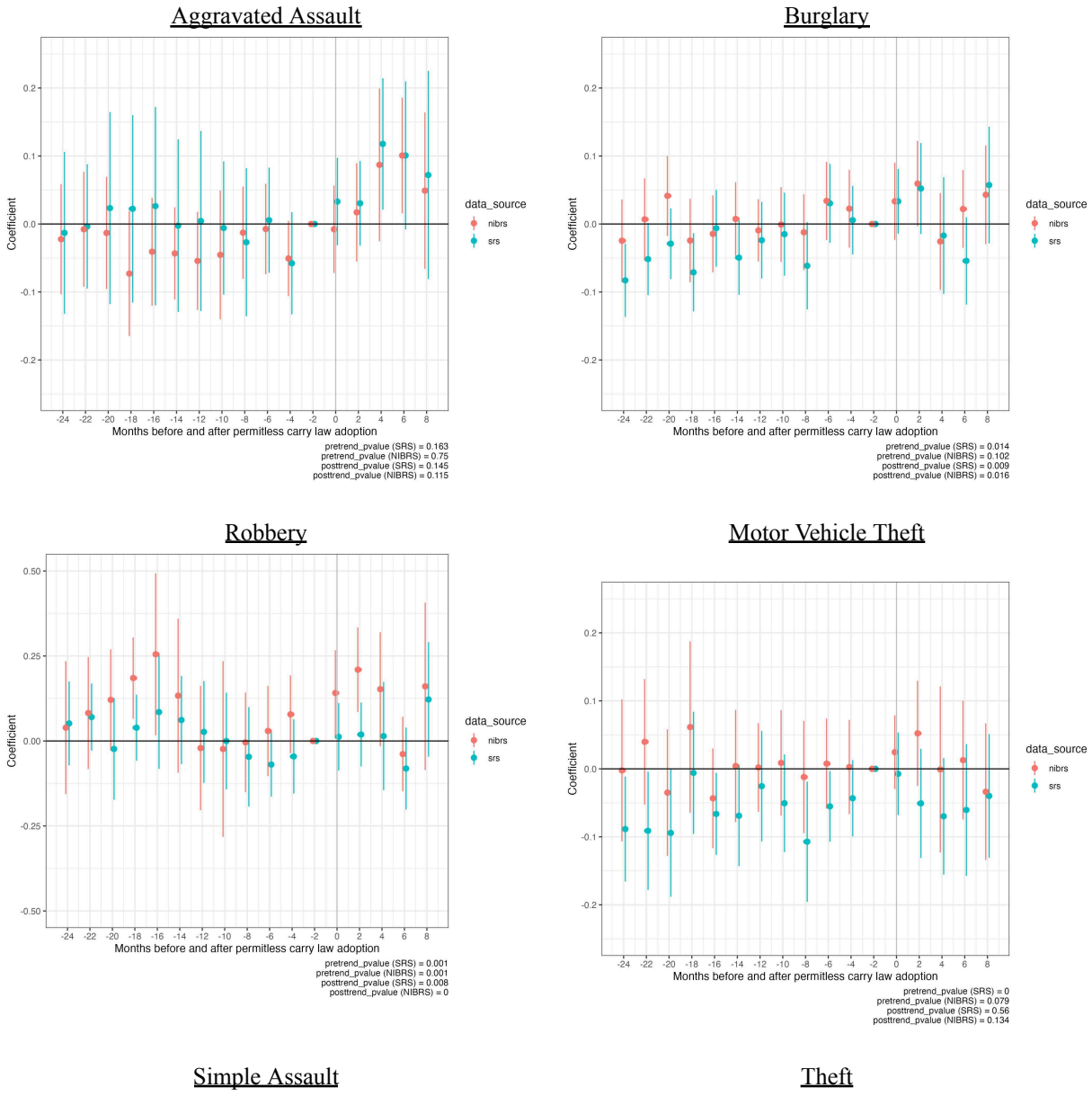

Notes: Permitless carry adoption and comparison state aggregated to the year-level. Excluded - other states are omitted as they were undergoing a concurrent policy change (IL, WI) or had data quality issues (FL) or did not have a clear treatment date (AR).

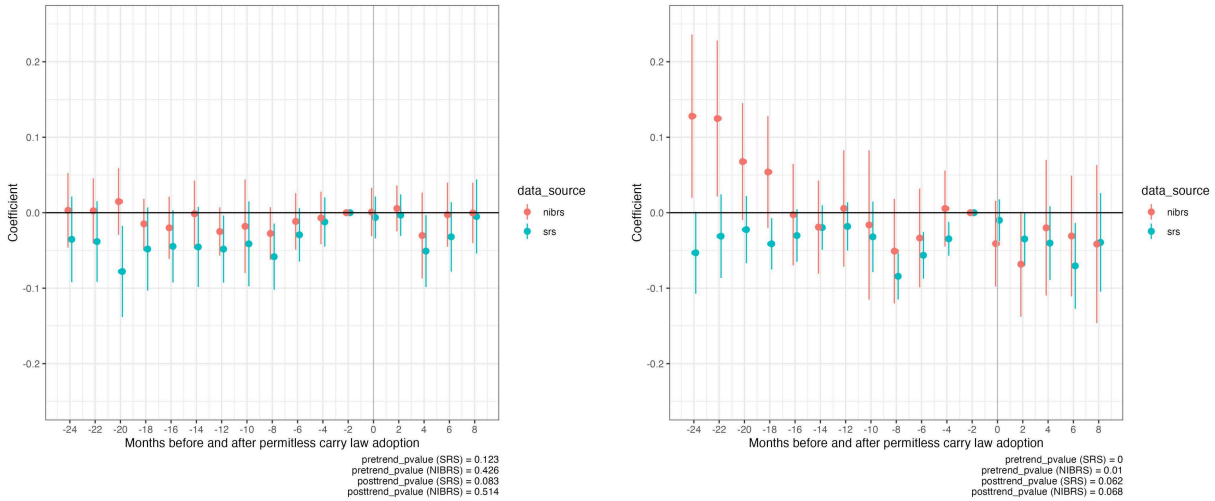
**Figure 2:** States by Permitless Carry Adoption Status, 1903-2019




Note: Permitless carry adoption in Vermont occurred in 1903 and is grouped with Alaska (2003) for visualization. Of states visualized, Wyoming (2010) and Arizona (2011) fall outside of the sample timeframe. States that have yet to adopt permitless carry are not included in the Figure.

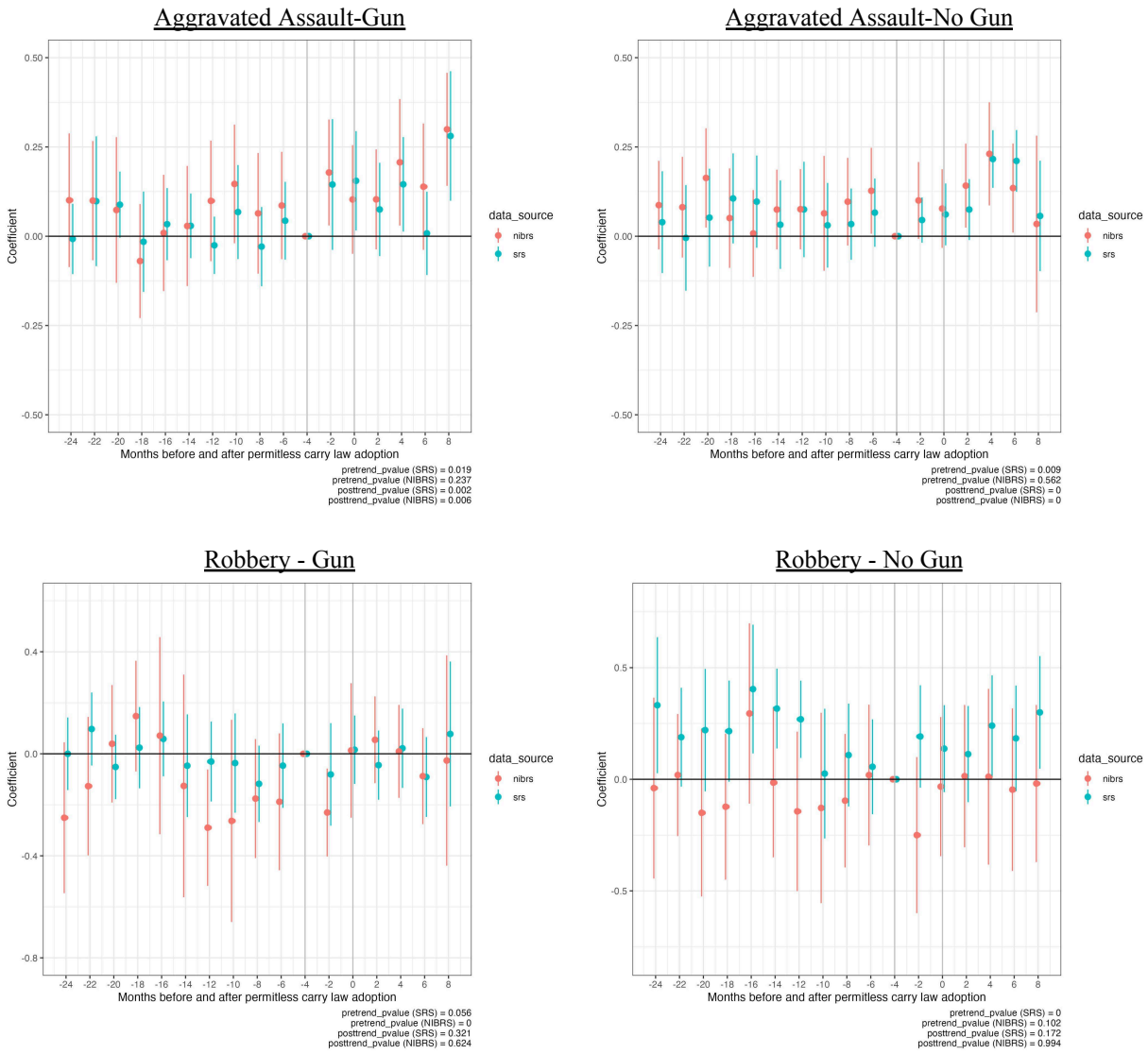
**Figure 3:** Effect of Permitless Concealed Carry Law Implementation on Crime: Event Study





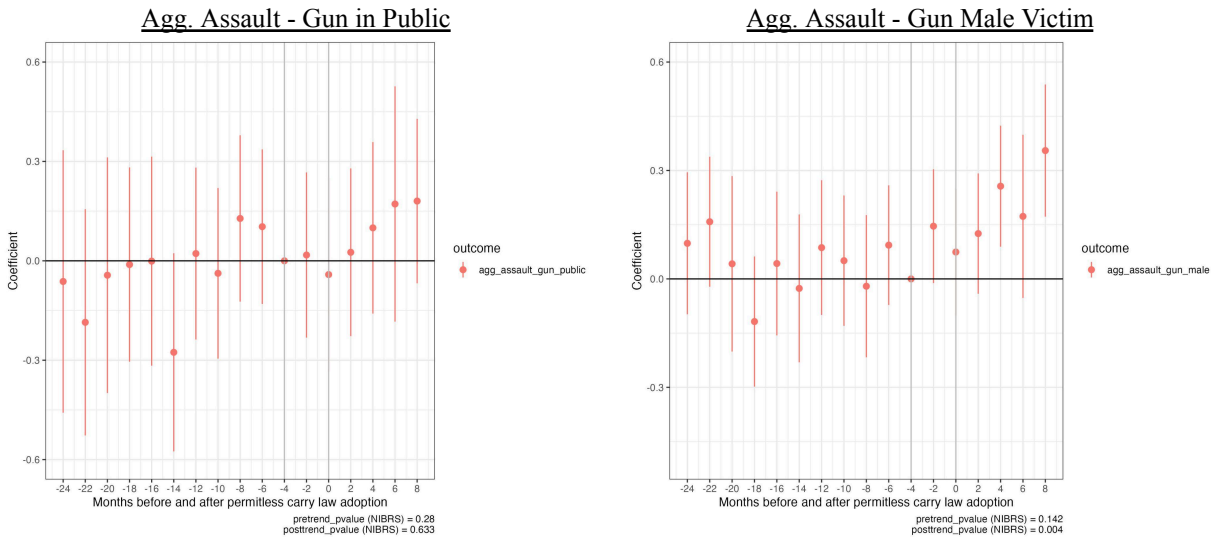

Notes: Crime outcomes are counts of crimes in each agency in the NIBRS and SRS sample that report at least 1 crime. The level of observation is the agency-2-month level. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the agency level. Pre-trend and post-period Wald tests are reported for both NIBRS and SRS at the bottom right of each panel.

**Figure 4:** Effect of Permitless Concealed Carry Laws on Weapon Crimes



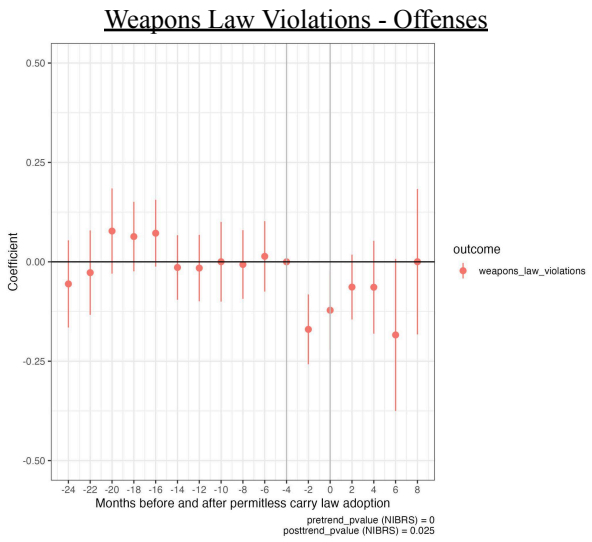

Notes: Crime outcomes are counts of crimes in each agency in the NIBRS and SRS sample that report at least 1 crime. The level of observation is the agency-2-month level. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the agency level. Pre-trend and post-period Wald tests are reported for both NIBRS and SRS at the bottom right of each panel. For gun-related outcomes, the relative time is t-4 to account for anticipation effects in advance of treatment at t=0.

**Figure 5:** Effect of Permitless Concealed Carry Laws on Gun Violence Outcomes (NIBRS only)




Notes: Crime outcomes are counts of crimes in each agency in the NIBRS sample that report at least 1 crime. The level of observation is the agency-2-month level. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the agency level. Pre-trend and post-period Wald tests are reported at the bottom right of each panel. For gun-related outcomes, the relative time is t-4 to account for anticipation effects in advance of treatment at t=0.

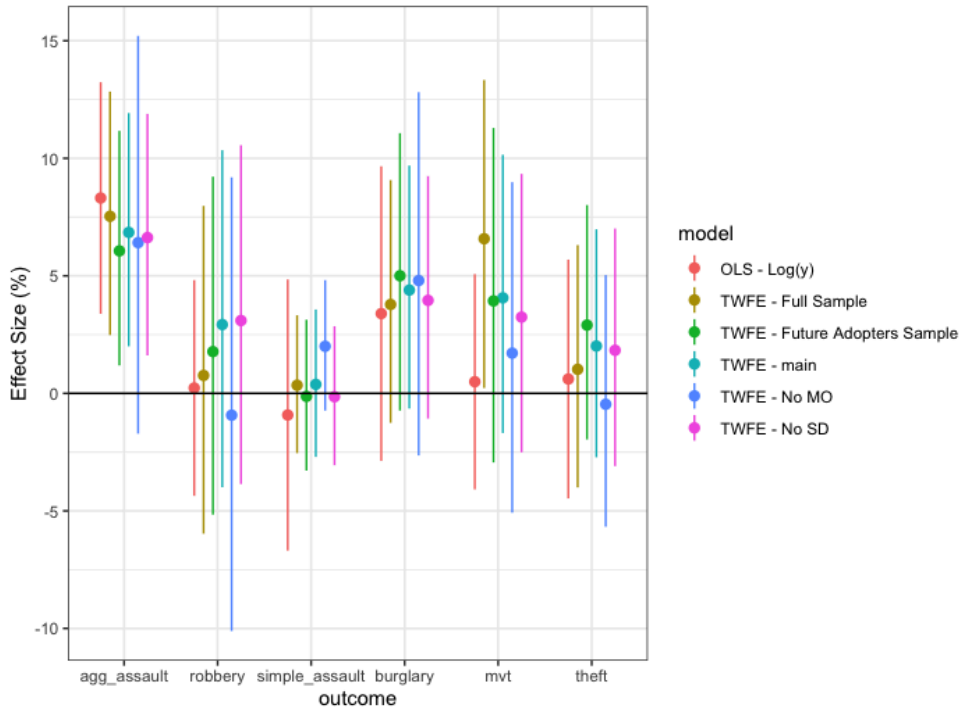
**Figure 6:** Effect of Permitless Concealed Carry Law Enactment on Policing Weapons Law Violations (NIBRS)



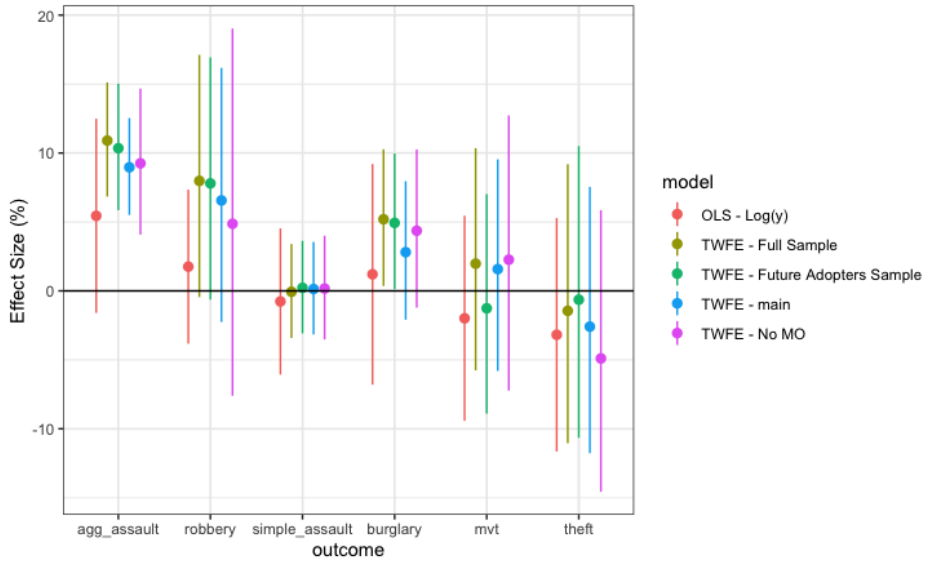

Notes: Crime outcomes are counts of crimes in each agency in the NIBRS sample that report at least 1 crime. The level of observation is the agency-2-month level. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the agency level. Pre-trend and post-period Wald tests are reported at the bottom right of each panel. For gun-related outcomes, the relative time is t-4 to account for anticipation effects in advance of treatment at t=0.

**Figure 8:** Effect of Permitless Concealed Carry Laws on Crime by Sample and State Inclusion

**Panel A: UCR SRS**




**Panel B: NIBRS**




Notes: Effect size estimates from Poisson and OLS TWFE estimations with different samples and exclusion of different states transformed by 100 x (exp(1)-1), which I report as the effect size. The full sample includes all states (may issue, shall issue, future adopters). Future adopting states include: TX, IN, AL, GA, IA, MT, NE, OH, TN, UT. Standard errors across models are clustered at the state level and the specification corresponds to Equation 1 and includes no lead effects.

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# APPENDIX

**Appendix A:** Data Appendix

The Uniform Crime Reporting (UCR) Program crime data that the FBI gathers from law enforcement agencies undergoes minimal data quality and validation procedures. UCR data have been gathered in two forms: 1) the Summary Reporting System (SRS) and 2) the National Incident Based Reporting System (NIBRS). The SRS, which was retired in 2021, gathered monthly counts of crime data and served as the nation's primary crime reporting program for over nine decades. The SRS suffered from substantial data quality problems. Agencies frequently do not report data, or only report partially, making missing data a common occurrence (Maltz and Targonski, 2003). These reporting problems and resulting imputation have been used to substantiate incorrect conclusions around the effects of gun laws (Maltz and Targonski, 2002).

Given the magnitude of these problems and minimal FBI attention[4], it is reasonable to expect that NIBRS, which replaces the SRS, has similarly limited data quality and validation procedures. NIBRS likely shares some of the SRS problems, but may differ due to its unique data gathering guidelines and submission processes. Like the SRS, agencies are not mandated to report crime data via NIBRS, and so may not report consistently. Agencies may also not report comprehensively, and may omit offenses even in months in which some records are submitted. NIBRS uses automatic "edit checks" to flag records as needing review, and it is possible to change records needing review to "null" in order for the system to accept the record (National Academies Press, 2018).[5] NIBRS data quality may be particularly affected by partially submitted records. Finally, as NIBRS is a new reporting system, agencies may improve reporting over time. Coding improvements may be likely for the many additional NIBRS fields that were not required under the SRS. These fields include associated details about each offense, ranging from the time of day the offense occurred to its location and detailed demographic information for victims and offenders.

To assess these potential data quality problems, I follow Mello (2019), Weisburst (2019) and Evans and Owens (2007) in determining which police agencies report crime data consistently and reliably. I start by including only agencies that report offenses each month of the year included in the sample. I further exclude agencies that are state police, hospital, campus, university police or transit police.[6]

Evans and Owens (2007) and Weisburst (2019) fit quadratic time trends to yearly crime data to identify potential outliers for each agency's reported crimes. Mello (2019) uses local linear regression to approximate expected yearly crime reporting. Because I use monthly rather than yearly measures, I make some modifications to the process of detecting outliers and misreporting. Monthly agency-level measures are highly variable time series data, often due to high levels of seasonality but as well caused by lapses in reporting. Local linear regression or

[4] Maltz and Targonski were funded by the National Institutes of Justice to address simplistic imputation that the FBI applies to crime data, which they note were created by a consultant in the 1960s. The FBI has never implemented the suggested changes and its imputation strategy "seems to be heavily if not exclusively based on stratification along a single variable—population of the agency's service population—which is overly simplistic; partial-year counts are commonly weighted up to a full year without reflecting any kind of seasonal component. (Modernizing Crime Statistics: Report 2: New Systems for Measuring Crime. Washington, DC: The National Academies Press. https://doi.org/10.17226/25035.)."

[5] The edit checks are described by stakeholders as 'a curiously unforgiving "all or nothing" approach under which entire NIBRS incident records may be discarded if a relatively minor edit check is failed' (Modernizing Crime Statistics: Report 2: New Systems for Measuring Crime. Washington, DC: The National Academies Press. https://doi.org/10.17226/25035.).

[6] In practice, I join NIBRS agency ORI identifiers to the FBI's Agency Participation List to obtain Population Groups and population served information. Agencies who serve a population of zero are omitted, which excludes most state police agencies. However, other campus, hospital, and state police agencies or drug task forces are assigned a population. I identify agencies using regular expression searches for "hospital," "university", "univ" and "campus" to identify agencies omitted under these categories. Because some state police agencies and drug task forces report substantive numbers of crimes each month, I elect to keep them if their reported offenses and victim measures are not flagged by the data cleaning process.

quadratic methods appropriate for yearly data are less well-suited to differentiate between unreliable crime reporting and high seasonal variation in monthly data.

To differentiate between true seasonal variation and drops or lapses in reporting, for each agency, I create three measures of agency reporting which I compare relative to agencies serving similar jurisdictions by population. First, I calculate the percentile of each agency's reported crime offenses by month relative to agency population groups in each year. For each agency *a,* I calculate the minimum and maximum percentile of reported offenses in each month *m* of each year *y* relative to a group *g* of similar agencies. To identify agencies with large jumps in reported offenses, each agency's percentile range $PctRange_{agy}$ is calculated as:

$$PctRange_{agy} = max_{agy}\left(Percentile_{agym}\right) - min_{agy}\left(Percentile_{agym}\right) \qquad (1)$$

Following Mello (2019), I calculate the reported offenses percentile for each agency-month observation in a year relative to population group *g* to compare agencies with similar jurisdictions and levels of crime reporting. I use population groupings from 1-10,000, 10,001-15,000, 15,001-25,000, 25,001-50,000, 50,001-100,000, 100,001 to 250,000 and agencies serving populations greater than 250,000. Because small agencies serving populations under 2,500 have high levels of variation in reported offenses, I elect to combine agencies serving populations less than 10,000. These categories are somewhat in line with the FBI's own population group descriptions, but I do not differentiate between city, Metropolitan Statistical Area (MSA), or non-MSA jurisdictions as these distinctions are not particularly correlated with levels of crime.[7] Agencies that fall in multiple categories across time are assigned to their most frequent category.

While $PctRange_{agy}$ measures agencies whose variation is unusual compared to similar agencies, it does not measure whether the variation is atypical for the agency. To differentiate between seasonal variation in crime and drops in reporting, I create the $MinPctAvg_{aym}$ which is calculated as:

$$MinPctAvg_{agy} = \sum_{y=1;\, y \neq y^*}^{7} min_{agy}\left(Percentile_{agym}\right) \qquad (2)$$

In this measure, the average minimum percentile of reported crime is calculated for all other years excluding the observation year, y*. This measure creates a baseline for each agency's typical minimum within-year percentile of reported crime compared to similar agencies. To formalize a reporting drop that is not typical to an agency's yearly variation, I calculate the difference between the observed minimum percentile in year y* and the $MinPctAvg_{agy}$ as:

$$MinPctDiff_{agy} = min_{agy}\left(Percentile_{agym}\right) - MinPctAvg_{agy} \qquad (3)$$

Agencies whose reported offenses are seasonal with high variation will have a lower $MinPctDiff_{agy}$ relative to agencies whose minimum is characterized by a percentile drop not reflected in any other years of reporting. This measures both lapses in reporting as well as incomplete reporting.

To establish a rejection threshold among agency population groups, I employ a two step process to exclude agencies whose time series of reported offenses is driven only by misreporting. I exclude agencies whose $PctRange_{agy}$ exceeds 95 and whose $MinPctDiff_{agy}$ exceeds 50. I establish these thresholds following the

[7] For instance, the average yearly crime for agencies serving the smallest FBI categories of "Cities under 2,500" and "Non MSA " is X and Y respectively.

procedure outlined in Mello (2019) by manually reviewing distributions of agencies. I identify thresholds that flag agencies with lapses or no monthly crime reporting that is not seasonal, and I review the totality of agencies selected under the threshold for false positives. I then review agencies not selected by the threshold rule for false negatives. Next, agencies whose $min_{agy}\left(Percentile_{agym}\right)$ is less than 2, but whose within-year percentile increase exceeds 40 are excluded. This exclusion rule captures agencies that may have a lower $PctRange_{agy}$ but still demonstrate lapses in reporting. I review all agencies excluded under these thresholds to ensure that poor crime reporting is accurately flagged without excluding false positives.[8] For NIBRS, this first step excludes 192 agencies and for the SRS, 375 agencies.

Second, after omitting agencies using the first rejection thresholds, I recalculate all three measures to make comparisons between agencies with now more consistent reporting. I find thresholds where the $MinPctDiff_{agy}$ exceeds 40 percentiles relative to other years observed combined with $min_{agy}$ less than 20 in the year of observation identifies agencies whose drops in reporting relative to similar agencies is due to misreporting. I again confirm this with manual review of all agencies flagged by the threshold. This second step excludes an additional 81 agencies for NIBRS and 512 for the SRS. I repeated the process of recalculating all three measures without the newly excluded agencies, and identified only 5 in NIBRS that did not meet the decision rule. Three of these agencies had problematic reporting patterns and were excluded. In total, I exclude 276 agencies, or 4% of the NIBRS sample and 887 or 9.95% of the SRS sample.[9]

I test the validity of this procedure in two ways. First, I select a random sample of 250 agencies whose $PctRange_{agy}$ is greater than 50 to manually review whether agencies with higher variation are anomalous or likely true reporting. Second, I test agencies with high seasonal variance that are most likely to be erroneously flagged by this process for the variation in their reported offenses. Agencies that serve tourism destinations often demonstrate patterns of highly seasonal crime variation. I identified several test cases including: Breckenridge, Colorado, Nantucket, Massachusetts, Dewey Beach Police in Delaware. These agencies are not flagged by the data cleaning process.

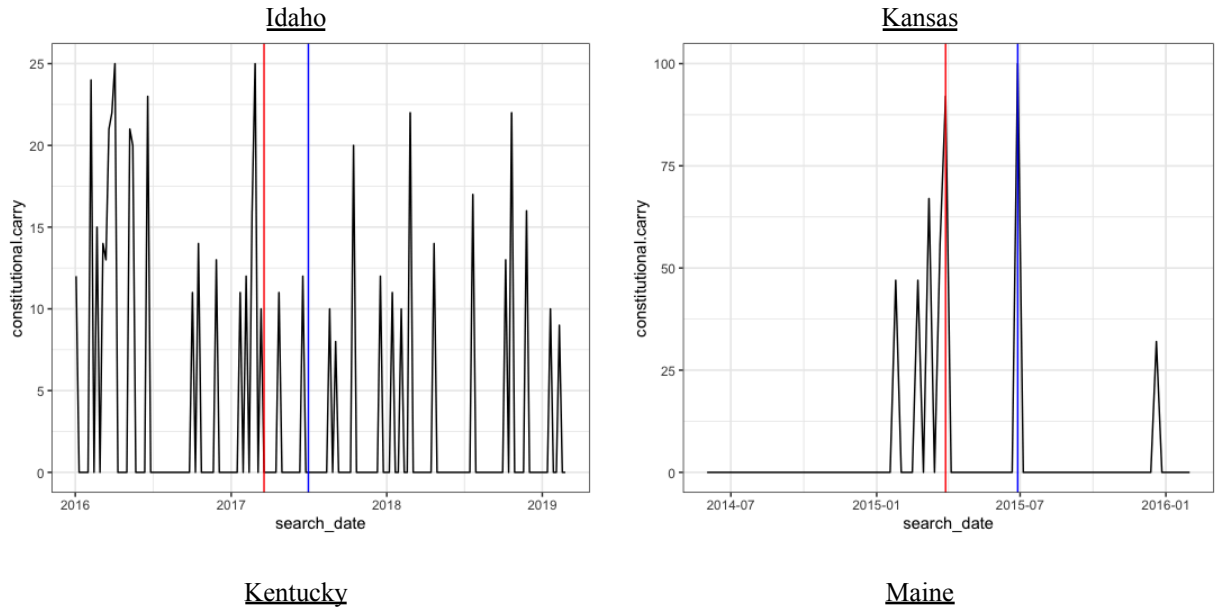
[8] In this review, I find that 4 agencies whose variation in reporting could possibly be reported correctly and could be considered false positives. I elect to omit these agencies as the error rate at this threshold is less than 2% (4 agencies / 192 agencies).

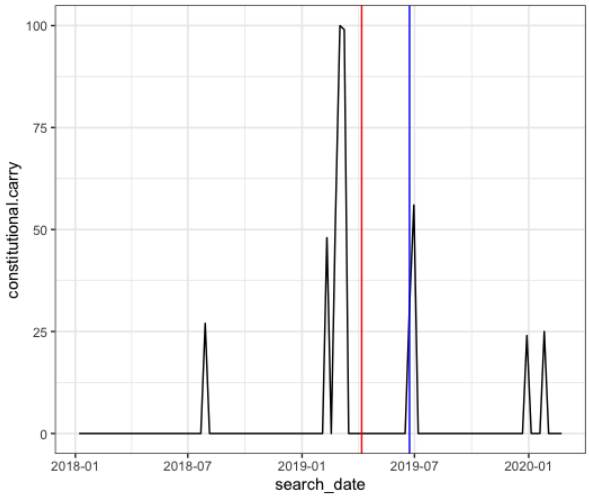
[9] Mello (2019) finds that approximately 2.5% of crime observations appear to be mistakes. While Maltz and Targonski (2002) report that as many as 19% of the agencies in the SRS do not report crime data accurately, this sample of NIBRS agencies is composed of agencies who are voluntarily transitioning before the national deadline and may therefore have more robust crime data reporting programs.

**Appendix B:** Anticipatory effects

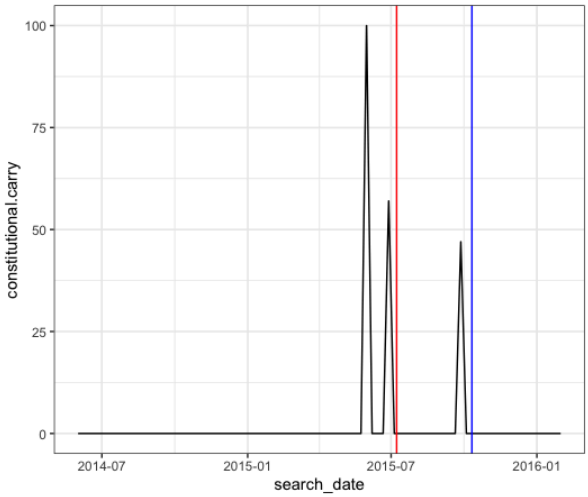
An identifying assumption of a difference-in-differences framework is that no anticipation of trends exists. I examine whether in the time between the passage of the permitless carry law and when the law takes effect if there is anticipatory searching for policy information using Google Trends data. Using Google Trends, I examine search terms including: permitless carry, Vermont carry, and constitutional carry. Vermont carry is a term for permitless carrying after the state of Vermont which has since 1903 not required a license to carry a concealed handgun. I use searches for the term "constitutional carry" which are most prevalent in all states. Appendix Figure B1 shows that searches around announcement or adoption date are higher relative to effective date for all permitless carry states with the exception of Missouri that does not have substantial time between adoption and effective dates.

**Appendix Figure B1:** Google Trends Searches by Adoption State for Permitless Carry: Adoption (red) vs. Effective Date (blue)

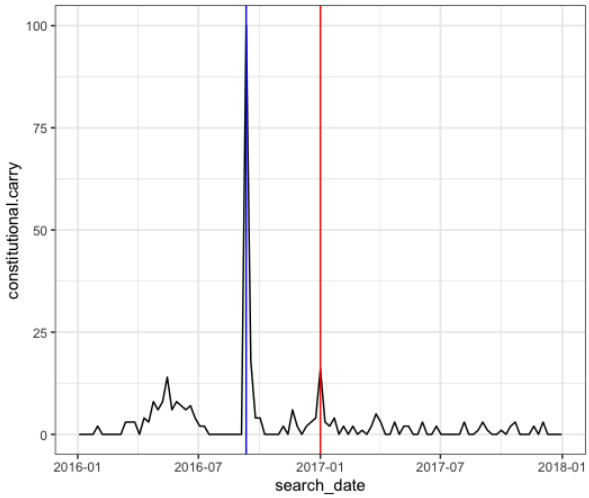




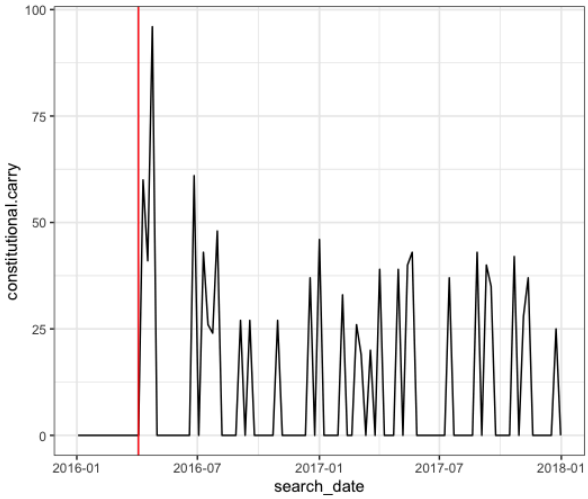

Missouri



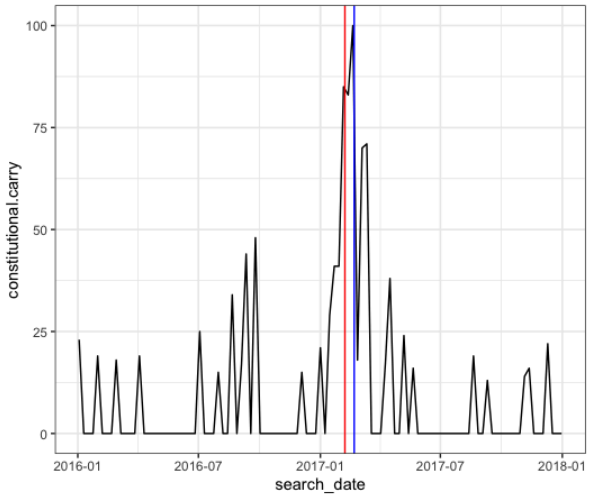

Mississippi

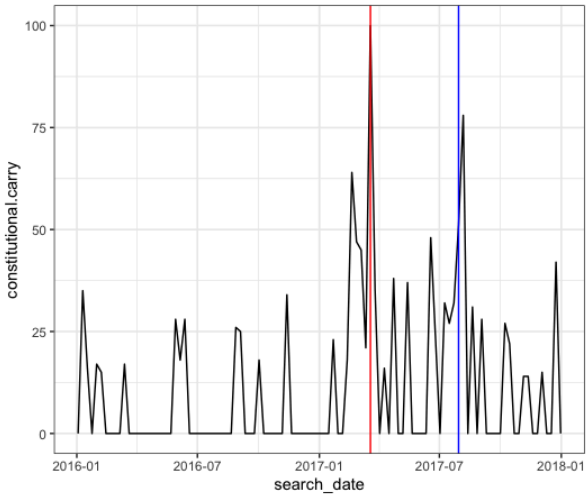



New Hampshire

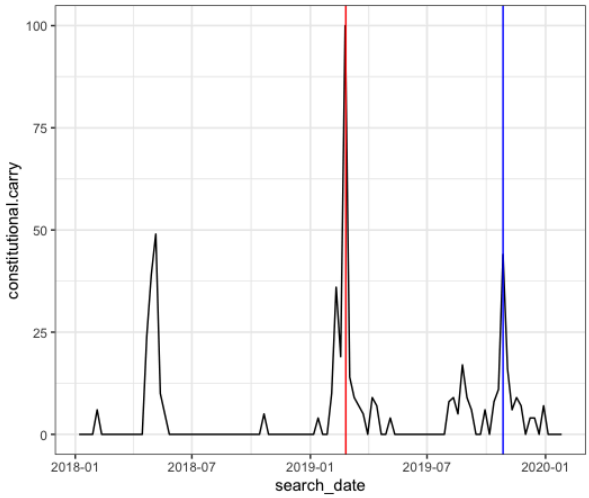



North Dakota

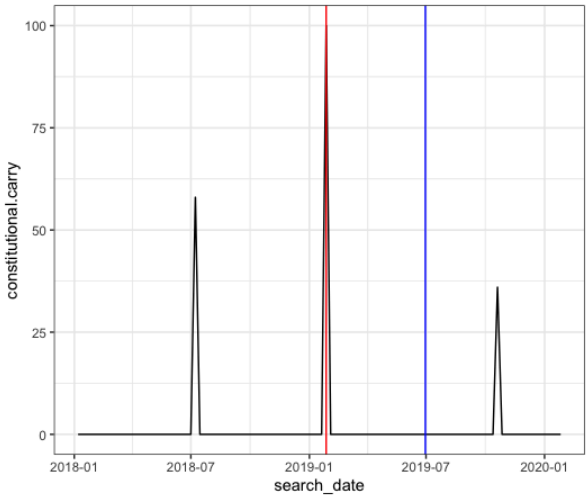


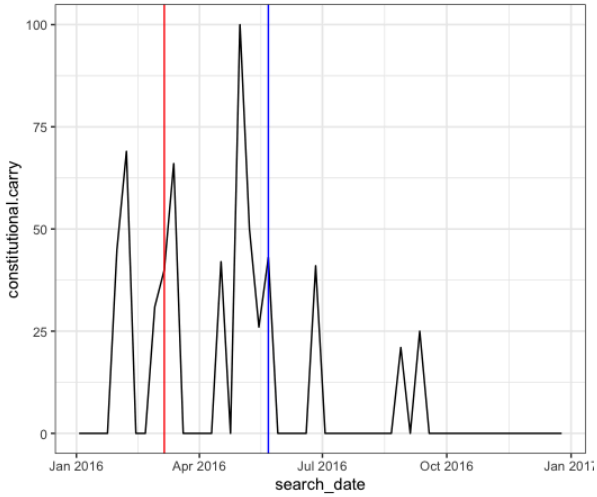

Oklahoma




South Dakota




West Virginia



Notes: Google Trends data are at the week-level of observation for each state. Enactment and effective dates are denoted with horizontal lines in red and blue respectively. States including Missouri have no time difference between the announcement and enactment dates.

**Appendix C:** Robustness

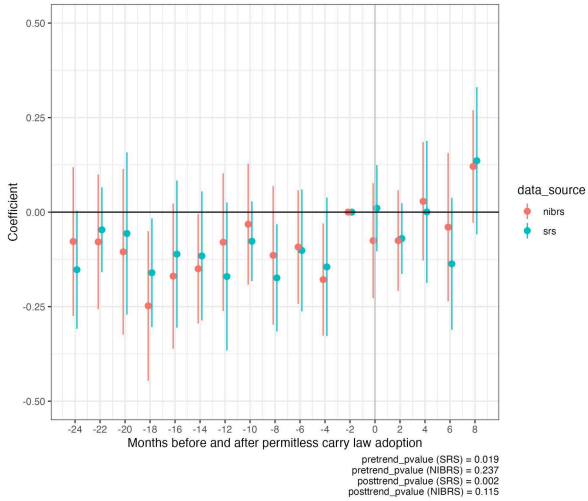
**Appendix Figure C1:** Goodman-Bacon Decomposition of Treatment Effects into Timing Groups

| Group | Weight (%) |
|---|---|
| Early vs. Late | 12.9 |
| Late vs. Early | 4.6 |
| Never vs. Timing | 82.5 |

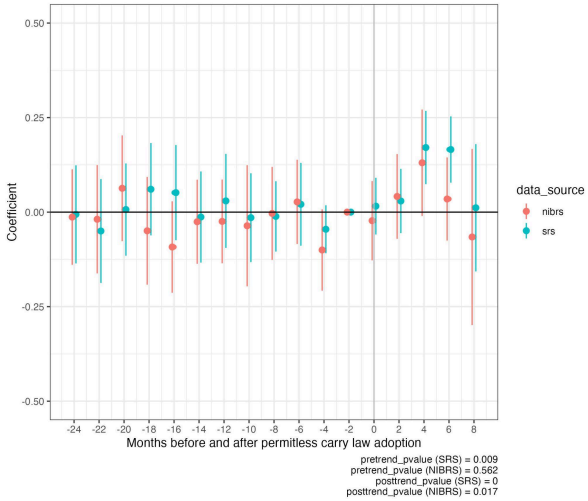
Notes: Weights are obtained using the Stata package BaconDecomp (Goodman-Bacon et al., 2022).

**Appendix Figure C4:** Effect of Permitless Concealed Carry Law Enactment on Weapon Crimes - Relative to 2 Months prior to treatment

Aggravated Assault-Gun

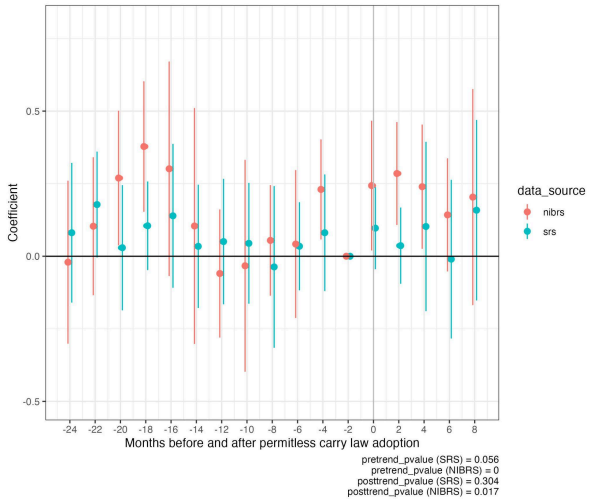


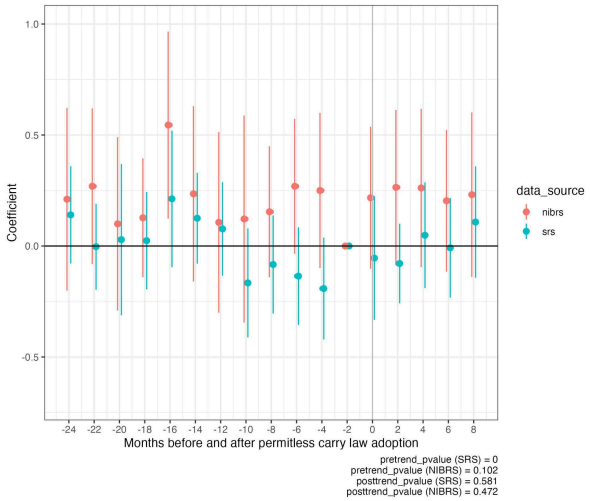

Aggravated Assault-No Gun




Robbery - Gun

Robbery - No Gun


Coefficient
0.5
0.0
-0.5
-24 -22 -20 -18 -16 -14 -12 -10 -8 -6 -4 -2 0 2 4 6 8
Months before and after permitless carry law adoption
data_source
nibrs
srs
pretrend_pvalue (SRS) = 0.056
pretrend_pvalue (NIBRS) = 0
posttrend_pvalue (SRS) = 0.304
posttrend_pvalue (NIBRS) = 0.017


Coefficient
1.0
0.5
0.0
-0.5
-24 -22 -20 -18 -16 -14 -12 -10 -8 -6 -4 -2 0 2 4 6 8
Months before and after permitless carry law adoption
data_source
nibrs
srs
pretrend_pvalue (SRS) = 0
pretrend_pvalue (NIBRS) = 0.102
posttrend_pvalue (SRS) = 0.581
posttrend_pvalue (NIBRS) = 0.472

**Appendix Table C1**:Stacked Regression Estimates of the Effects of Permitless Carry Law Adoption

| | Interpersonal Crime | | | Property Crime | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Aggravated Assault | Robbery | Simple Assault | Burglary | Motor Vehicle Theft | Larceny-theft |
| **Panel A: UCR SRS** | | | | | | |
| Permitless Carry | 0.072** | 0.001 | 0.015 | 0.0566 | 0.0516 | 0.0191 |
| | (0.027) | (0.047) | (0.017) | (0.032) | (0.030) | (0.024) |
| Number of Agencies | 4,267 | 4,267 | 4,267 | 4,267 | 4,267 | 4,267 |
| Observations | 1,719,671 | 1,719,671 | 1,719,671 | 1,719,671 | 1,719,671 | 1,719,671 |
| **Panel B: NIBRS** | | | | | | |
| Permitless Carry | 0.092* | 0.050 | -0.009 | 0.026 | 0.023 | -0.035 |
| | (0.036) | (0.062) | (0.019) | (0.022) | (0.030) | (0.022) |
| Number of Agencies | 1,461 | 1,461 | 1,461 | 1,461 | 1,461 | 1,461 |
| Observations | 315,131 | 315,131 | 315,131 | 315,131 | 315,131 | 315,131 |
| Agency, Month Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Crime outcomes are counts of crimes in each agency in the NIBRS and SRS sample that report at least 1 crime. The level of observation is the agency-month level. Agencies are considered treated when treatment takes effect for the majority of the month observation. The comparison group includes shall issue states and omits may issue states. Each treated state's agency observations appear for 7 months in the post period only. Standard errors are clustered at the state level. Coefficients correspond to Equation 3.