**Jumping the Gun: The Effects of Permitless Concealed Carry Laws on Crime**

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## Abstract

In the last decade, one of the most substantial changes to firearm policy is the widespread adoption of permitless concealed carry laws. Since 2015, more than twenty 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. Because most permitless carry laws are passed only months before taking effect and immediately repeal licensing requirements, they may elicit swift behavioral responses unlike prior concealed carry laws. Leveraging differential responses to future firearm regulation to empirically identify a reference period, I examine the effects of permitless carry on crime. Results from stacked dynamic difference-in-differences estimators show violent index and property crime increase in the 12 months after the passage of a permitless carry law. This rapid increase is driven by a 15.0 percent increase in aggravated gun assaults, constituting approximately 1,000 additional gun assaults. Concurrent reductions in police enforcement of weapons laws may reduce the barriers to carrying illegal 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. 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, over twenty states have enacted a policy known as permitless carry. Permitless carry laws are 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 one hand, permitless carry laws may have a minimal effect on public handgun carrying and therefore on 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. An estimated 9 million handgun owners carried handguns in the past month in 2015, an estimate that doubled by 2019 to about 16 million (Rowhani-Rahbar et al., 2017). Because in most states less than one percent of license applications are denied, the majority of individuals who want to carry a concealed handgun may already be able to do so (Phillips 2013; Shapira 2018). Therefore, 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 crime. 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. Permitless carry laws may also alter the frequency of public handgun carrying, affecting the intensive margin of handgun carrying, if gun owners 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 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.

This paper examines the effect of permitless carry laws on crime to inform ongoing policy debates between those who favor expanding permitless carry and those who oppose or seek to repeal it. In the absence of rigorous evaluation of permitless carry laws, 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. Recent work examining adoption of permitless carry laws using yearly measures of homicide and violent crime outcomes suggest that permitless carry laws may result in decreases or no significant effect on homicide and violent crime outcomes (Bondy et al., 2023; Doucette, Crifasi, et al., 2023; Moody & Lott, 2022). As Bondy et al. (2023) note, caution is warranted in assessing the effects of permitless carry laws rapidly after adoption due to the shortened post-period of analysis.

Importantly, permitless carry laws differ from other concealed carry laws in that they eliminate existing licensing requirements rather than introduce or modify regulatory processes. As such, they may induce swift behavioral responses, particularly as permitless carry law passage is quickly followed by implementation. The typical permitless carry law takes effect four months after passage. Gun owners may be forward-looking and responsive to anticipated firearm regulation, responding not only to the law's implementation but also to its anticipated passage (Depetris-Chauvin, 2015). This presents a methodological challenge for causal identification by violating the parallel trends assumption in difference-in-differences frameworks.

To overcome this challenge, I test for anticipatory effects to identify the appropriate reference period. I use two empirical strategies. First, I analyze Google Trends data to track public interest in permitless carry (often referred to as "constitutional carry") around the time laws are passed and take effect. Second, I compare trends prior to the implementation of a permitless carry law using theoretically motivated differences in crime types. I compare aggravated gun assaults, which should be highly impacted by changes in carrying behavior, to property crimes such as theft or motor vehicle theft, which are less likely to be affected by public gun carrying. Because I find that aggravated gun assaults increase at the law's passage and implementation without corresponding changes in larceny-theft, I use the passage date as the reference period.

Using the empirically identified reference period, 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. Unlike prior studies that focus on annual effects and assume no behavioral anticipatory responses to permitless carry laws, I focus analysis on dynamic impacts after permitless carry law passage. This approach is well-suited to permitless carry laws, where legal gun owners become eligible to carry handguns in public without delay. I use a sample of police agencies that consistently report monthly crime data from primarily the National Incident Based Reporting System (NIBRS), which provides rich, detailed information about crime incidents. As NIBRS does not fully cover all law enforcement agencies during this time period, I additionally use the FBI's

Summary Reporting System (SRS) crime data to ensure that findings are not driven by sample composition.

I find little evidence of deterrent effects resulting from permitless concealed carry policy adoption. I estimate an 8.3% increase in violent index crime and a 5.2% increase in property index crime in the twelve months after passage of a permitless carry law. This increase in violent crime is driven by gun crime. Aggravated gun assaults rose 15.0% percent without increases in aggravated assaults involving other weapons. I show that increases in gun violence occur in public places. I find substantial reductions in police enforcement of weapons law offenses and arrests. Finally, modest increases in gun sales combined with observed treatment effects for violent crime outcomes shortly after passage suggest that existing gun owners rather than new gun owners may increase concealed handgun carrying.

In addition to the literature on concealed carry laws, this paper contributes to literature that explores how gun carrying and gun laws impact violent crime. Cheng and Hoekstra (2013) find that Stand Your Ground Laws, which remove the duty to retreat before using deadly force during public altercations, do not deter burglary, robbery or aggravated assault but lead to a significant increase in homicide. McClellan and Tekin (2017) expand this analysis by applying event studies to more temporally granular state-month homicide and hospitalization outcomes, finding that the increase in homicides is concentrated among white males.

## 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. 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.

"Shall issue" states do not feature law enforcement discretion to deny a license and adopt a criteria by which licenses are approved or denied. 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).

Permitless carry differs from "may issue" and "shall issue" laws in that it removes the need to obtain a license to carry a concealed handgun in public. 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. Not only do permitless carry policies remove the requirement that concealed handgun carrying requires a background check and permit, in some states it also removes the requirement to undergo firearm safety training. Often, permitless carry states continue licensing programs to enable concealed handgun carriers to travel legally between states while carrying a concealed weapon.

In all states, federal law continues to prohibit firearm possession for individuals under the age of 18 and for disqualifying categories such as a felony conviction or involuntary confinement.

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

A substantial literature focuses on concealed carry licensing laws. The majority of studies on the effects of adopting more permissive concealed carry laws find increases in violent crime and homicide (Colmer & Doleac, 2022; Donohue et al., 2017, 2022; Doucette et al., 2022; Fridel, 2021; Kovandzic & Marvell, 2003; Ludwig, 1998; Siegel et al., 2017; Smart et al., 2024). Effect sizes range from increases of five to fifteen percent in violent crime in the ten years after adoption of a more permissive concealed carry law. Studies focused on homicide find a 6 to 11-percent increase in homicide and firearm homicide rates (Doucette, McCourt, et al., 2023; French & Heagerty, 2008; Gius, 2019).

A key limitation of this literature is its treatment of "shall issue" and permitless carry laws as a single policy type, often referred to as "right-to-carry" laws. As a result, these analyses may have limited relevance to the ongoing, rapid adoption of permitless concealed carry. This classification is especially problematic because nearly all permitless carry states with the exception of Kansas previously operated under shall issue laws, meaning that recent studies may increasingly reflect the effects of permitless carry, even if not identified as such.

More recent studies have begun to examine permitless carry laws directly, finding mixed results on violent crime and homicide (Doucette, Crifasi, et al., 2023; Jorgensen, 2024; Moody & Lott, 2022). Kyriazis (2022) primarily examines changes in crime as a mechanism affecting police shootings after permitless carry adoption, finding a positive impact on aggravated assaults. Single state research by Smith & Petrocelli (2019) focused on Arizona's 2010 permitless carry policy adoption finding null effects on handgun-related firearms offenses in the years following adoption.

Advances in this literature have started to investigate mechanisms linking concealed carry and violence. Donohue et al. (2025) find focus on how concealed concealed carry laws impact policing, finding reductions in police ability to solve violent crimes and substantial increases in stolen gun incidents (Donohue et al., 2022). Similarly, Billings (2023) uses unique individual-level data to establish that concealed handgun permit holders are more likely to be crime victims, particularly having a gun stolen.

## 3. Conceptual Framework

*3.1 Permitless Carry and Crime*

Permitless carry laws may impact crime through several channels. First, by removing licensing requirements, permitless carry laws expand eligibility to carry concealed handguns in public to all legal gun owners. This expansion may increase the extensive margin, or number of individuals carrying handguns in public. New handgun carriers may differ systematically from those who previously obtained permits, as they may have been deterred by background checks, training requirements, or administrative hurdles. Permitless carry laws may also affect the intensive margin, or the frequency with which firearms are carried if existing gun owners choose to carry more often. Together, these changes may alter the nature of public interactions, increasing the likelihood that individuals involved in conflicts are armed. This could lead to either deterrence or escalation of violent encounters in public settings. From a deterrence perspective, the increased prevalence of armed individuals may raise the perceived costs of committing certain crimes, particularly those involving direct interpersonal confrontation, such as robbery or assault.

Permitless carry laws may influence crime by altering police behavior, both in ways common to all concealed carry laws and in ways specific to permitless carry. A key distinction of permitless carry laws is that they eliminate the permitting system, making it more difficult for law enforcement to distinguish between legal and illegal handgun carriers. Law enforcement may respond to changes in the punishment of offenders, particularly if they believe these actions are less likely to result in sanctions once the law is in effect (DeAngelo & Owens, 2017; Soliman, 2022). In the context of permitless carry, officers may reduce their enforcement efforts related to weapons law violations, a crime category driven by illegal carrying of a concealed handgun.[1] These shifts in enforcement behavior may reduce police contact with individuals who are illegally armed, potentially impacting crime.

Permitless carry laws may also affect policing through broader mechanisms shared with other right-to-carry policies. Donohue et al. (2025) find that right-to-carry laws may reduce police ability to solve violent crimes by reducing the clearance rate. If permitless carry laws increase violence, police may face higher workloads related to responding to and investigating serious crimes. Conversely, if permitless carry laws generate deterrence effects and reduce crime, police may be able to reallocate resources toward solving violent crimes, potentially improving clearance rates.

Finally, permitless carry laws may influence crime by increasing the overall availability of handguns. One pathway is through their potential effect on firearm demand. By lowering the barriers to public gun carrying, permitless carry laws may induce more individuals to purchase firearms, particularly handguns suitable for concealed carry. While comprehensive national data on household gun ownership are limited, existing research suggests that concealed carry licensing laws are associated with increases in firearm background checks, a commonly used proxy for gun sales (Kim & Chen, 2025; Steidley, 2019). In addition to lawful purchases, permitless carry laws may affect the availability of firearms through increased theft and these effects may contribute to higher levels of gun availability that impacts crime.

*3.2 Passage, Implementation and Anticipation of Permitless Carry Laws*

In contrast to previous studies of concealed carry laws, this study considers dynamic and rapid impacts of permitless carry laws on crime outcomes. Prior concealed carry laws required individuals to obtain a license to carry a gun, a lengthy process with varying criteria determining if it would be issued.[2] As a result, most prior studies examined long-term trends before and after implementation. Permitless carry laws, however, allow individuals to immediately carry concealed weapons in public without the need to first obtain a license. Typically, permitless carry laws are passed with a relatively short three to four months time window between passage and implementation (Table 1, Appendix C). This timeframe may prompt both gun owners and police to respond rapidly to changing gun laws.

Swift responses to shifts in firearm regulation, both anticipatory and responsive, are well-documented in the literature. Firearm sales surged in anticipation of Barack Obama's 2008 election,

[1] NIBRS and SRS do not tabulate the type of weapons law violation offense, which according to the FBI include: "the violation of laws or ordinances prohibiting the manufacture, sale, purchase, transportation, possession, concealment, or use of firearms, cutting instruments, explosives, incendiary devices, or other deadly weapons." In practice, concealed handgun violations are the majority of weapons law violations. For instance, in 2018, the Louisville, KY Open Crime Data Portal indicates that of the 1,838 weapons law offenses that occurred in the year prior to passing a permitless carry law, 64% took place in public (non-residential) locations with concealed carry or possession of a weapon by a felon charges.

[2] For instance, Florida had a 90 day turnaround time for issuing a license. Other states such as Illinois and Wisconsin faced more substantial delays in setting up permitting programs after the state adopted concealed carry licensing laws (Press, n.d.).

linked to uncertainty about whether stricter future gun policy would be enacted (Depetris-Chauvin, 2015). Demand for firearms is also shaped by responses to social and political events or perceived threats such as such as pandemics, (Crifasi et al., 2021; Lyons et al., 2021; Miller et al., 2022), social protest (Lang & Lang, 2021), and mass shootings (Levine & McKnight, 2017; Pak, 2022; Porfiri et al., 2019; Studdert et al., 2017).

Permitless carry laws may produce similar responses. Gun owners who are likely to already own several handguns may begin carrying more frequently before the law officially takes effect. Many gun owners are well-positioned to respond rapidly because most own multiple firearms (Azrael et al., 2017), and handguns comprise the majority of these. Moreover, gun owners may seek information that facilitates awareness of gun policy changes. Roughly 40% of male gun owners report engaging with gun-related media (K. Parker et al., 2017), which may accelerate the spread of policy news and influence behavior before laws are implemented if permitless carry law passage garners significant media attention. For forward-looking individuals with information about future treatment, anticipation is a reasonable response particularly if there is a perceived cost or benefit to personal safety, the primary reason gun owners report carrying handguns (Rowhani-Rahbar et al., 2017).

However, anticipation poses methodological challenges. It is difficult to rule out whether an observed anticipatory response is endogeneity, or the possibility that crime prompted the adoption of permitless carry laws.[3] To test for anticipatory responses to permitless carry laws, I use two empirical strategies. First, I analyze Google Trends data to track public interest in permitless carry (often referred to as "constitutional carry") around the time laws are passed and take effect. I find that internet searches spike at the time of passage more than implementation, suggesting that responses begin before the law is in effect.

Second, I test for anticipatory effects across crime types that should theoretically respond differently to a permitless carry law. Following Cheng and Hoekstra (2013), who study Stand-Your-Ground laws which also take swift effect, I compare trends in aggravated gun assaults, which should be highly impacted by changes in carrying behavior, to property crimes such as theft or motor vehicle theft, which are less likely to be affected by public gun carrying. Property crimes are not interpersonal and should be less impacted by permitless carry laws. If permitless carry laws encourage early carrying, it may be possible to observe an increase in aggravated gun assaults prior to implementation. Conversely, if the law deters violence, a decline in aggravated gun assaults before the law takes effect may also be observed. Taken together, these tests will form the basis of determining the relevant time point, or reference period, for estimating dynamic effects in an event study or dynamic difference-in-differences framework.

## 4. Data and Descriptive Statistics

*4.1 Data Description*

The primary data source for this paper is the National Incident Based Reporting System (NIBRS) from 2013-2020. NIBRS gathers detailed information about crimes, victims, arrests and property reported stolen at the incident level, including the demographics of individuals involved. However, a limited set of police agencies during the study timeframe report to NIBRS including a subset of agencies from each

[3] With the exception of Missouri, states that initially adopted permitless carry laws had low rates of crime relative to later adopting states. For instance, in 2015, Maine's violent crime rate was 130.1 per 100,000 residents, or the lowest rate in the nation. Similarly, New Hampshire's violent crime rate in 2015 was 199.1 while Kansas' rate was 389.9. (*Crime in the United States, 2015*, n.d.)

state that adopted permitless carry (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") as an alternative source for main crime results. The UCR SRS during the study timeframe spanned all 50 states and over 90% of the U.S. population. UCR SRS records monthly summary counts of reported crime offenses, though it does not include the demographic information or additional granular detail in NIBRS, and may undercount firearm offenses (S. T. Parker, 2022).

While the majority of police agencies report data voluntarily to the FBI, many do not do so consistently and accurately (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 reliably in each month of each year of the sample. However, because I use monthly crime counts 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 include only agencies that report at least one crime offense in each month each year and that, relative to similarly sized jurisdictions, do not report total crimes outside the 90th percentile of their similarly-sized agency peers (See Appendix B). I remove an additional 4% of the NIBRS sample and approximately 10% of the SRS sample. Appendix B fully documents this process.

From this sample, I use violent crime measures including aggravated assaults and robberies, which are serious interpersonal crimes where victims experience the threat of force, physical attack, or both. 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, enabling a falsification test where violent crimes committed with firearms should theoretically drive observed effects. I also examine property crimes including burglary, motor vehicle theft and larceny. I use monthly summary counts of reported crimes from January 1, 2014 to February 29, 2020 to construct agency-month panels. I pool monthly data into 2-months to reduce the proportion of zero observations as crime is infrequent in some jurisdictions.

Additionally, I use Google Trends data which provides a sample of Internet Google search requests. It supplies an index for search intensity by topic over the time period requested in a geographical area. I obtain monthly Google search requests for terms related to permitless carry laws including "constitutional carry", "concealed carry", "gun laws" and "gun control" for each state from 2013 to February 2020. Permitless carry is not included as a search term because it is frequently missing from the Google Trends data as it is not often used in search requests.

I use the RAND State Gun Law Database to identify states that adopted permitless carry policies between 2014 and 2019 and obtain the effective date, or the date the permitless carry law took effect (Cherney et al. 2022). I supplement the effective date with the passage date using news sources (Appendix C). While one state's enactment and passage date is the same (Mississippi), on average states passed permitless carry laws 3-4 months before they took effect. During this timeframe, twelve states adopted a permitless carry law (Table 1), but due to substantial confusion about the effective date of permitless carry adoption, Arkansas is omitted from the sample (Froelich, 2018). I match county-level

time-varying control variables to each agency's county or counties. I use the lagged number of full-time police per 100,000 county residents from the Uniform Crime Reports and the lagged rate of persons incarcerated compiled by the Vera Institute from the Bureau of Justice Statistics and National Corrections Reporting system. From the American Community Survey and Census Bureau, I use county-level population data including the share of the male population in 15–24 and 25–44 age groups as well, the proportion of individuals residing alone in a household, median family income and the poverty rate. Finally, I use handgun and long gun background check data obtained from Small Arms Analytics which gathers monthly data from the FBI's National Instant Criminal Background Check System (NICS), adjusted for checks unlikely to be related to end-user firearms sales.

*4.2 Descriptive Statistics*

In Table 1, descriptive statistics are reported for the NIBRS sample. Columns 3 and 7 present descriptive statistics for treated agencies in both the panel data, consisting of agency pooled-month observations and the stacked panel, where untreated and not-yet-treated agencies are repeated as controls. The stacked panel time period consists of twelve months before and after permitless carry law passage. Data are pooled across 2-months as violence does not occur often in some agencies. Comparison agencies include predominantly shall-issue states, which are more likely to consider adopting a permitless carry law relative to may issue states such as California or New York, neither of which reported data to NIBRS during the study timeframe and are therefore not included in the control group.[4] While agencies increasingly became able to report crime data to NIBRS during the study timeline, I use a balanced panel of agencies who reported during the study timeframe to use a consistent comparison group with evidence of accurate crime reporting rather than agencies newly adopting a complex reporting system.

Violent and property index crime is less prevalent in states that adopted permitless carry laws relative to comparison states in both the panel and stacked panel. Agencies in permitless carry states recorded substantially fewer gun crimes, reporting less than half the average 2-month aggravated assault and robbery with a gun relative to comparison states. Descriptive statistics from the broader SRS sample are consistent with these patterns (Appendix Table A.1). While these differences suggest that states that adopted permitless carry were unlikely to do so because of rising crime relative to neighboring states, the imbalance between groups necessitates the analysis rely on the assumption of parallel pre-trends in the level of crime prior to the passage of a permitless carry law. Additionally, because eleven states passed permitless carry after 2020,[5] these future adopting states are used as an additional alternative comparison group that is less likely to differ in systematic ways from states that have not adopted a permitless carry law.

## 5. Empirical Strategy

There are two key parts to the empirical strategy. In the first part, the goal of the analysis is to test for whether anticipation of permitless carry laws occurs and empirically identify the appropriate reference period to use in an event study framework. This section draws on methodology from literature examining identification of anticipatory effects (Alpert, 2016; Malani & Reif, 2015; McCallum, 1976).

[4] Illinois and Wisconsin are also omitted from the control group due to concurrent concealed carry law changes in 2014 and 2011 respectively. The NIBRS comparison group includes the following comparison states: AL, CO, CT, DE, GA, HI, IA, IN, LA, MA, ME, MI, MN, MT, NC, NE, NM, OH, OR, PA, RI, SC, TN, TX, UT, VA, WA.

[5] These states include: AL, IA, LA, MT, NC, NE, OH, SC, TN, TX, UT.

The second part relies on the identified reference period to introduce the main empirical strategy. Throughout, I implement a stacked difference-in-differences framework (Baker et al., 2022; Cengiz et al., 2019; Deshpande & Li, 2019), which reduces the potential for biased estimates stemming from inappropriate comparisons between treated and comparison units in the context of staggered policy adoption.

Unlike many difference-in-differences studies in the literature, I do not estimate state-year models. Instead, I use monthly or pooled two-month time intervals to detect short-run effects. If permitless carry laws influence crime, these effects should occur rapidly. However, using short temporal intervals increases the prevalence of zero-count outcomes, especially in less common crime types. While much of the literature employs logged or otherwise transformed outcome variables, recent work warns that choices in how outcomes are transformed may bias treatment effects (Chen & Roth, 2023; Mullahy & Norton, 2022). I therefore use Poisson fixed effects estimators (Wooldridge, 2021, 2023) which estimate the average treatment effect under the assumption of parallel trends in the ratio of conditional means.

*5.1. Testing for Anticipation: Google Trends Searches*

Drawing on the conceptual framework, I first use a stacked event study framework to assess search trends for information pertaining to permitless carry laws in the months leading up to the implementation of the law. This time period will include both the passage and implementation dates of the law and enable empirically identifying the timing of spikes in search interest.

To start, I use Google Trends data to provide measures of relative search term volume for Internet searches. I use state-month panel data on the search index measure for terms pertaining to permitless carry laws as outcomes of interest (constitutional carry, gun law, concealed carry, and gun control). I modify the state-month panel data to construct a stacked panel dataset where each stack corresponds to a single treated state. For each treated state, I include as comparison units those states that are either never treated during the study period or are not yet treated relative to the treated state. To avoid contamination of the post-treatment period, I exclude from the comparison group any state that becomes treated and states already treated. I then append all treatment-specific stacks into a single dataset to estimate treatment effects. Data is centered on the month the law takes effect and the pre-period will span the legislative process, from introduction of the law including passage, relative to the implementation date. Models are estimated for the search trend volume in stack *d* in state *s* in month *m*. The linear regression takes the following form:

$$Y_{dsm} = \sum_{t=-36,\, t\neq -36}^{T} \beta_j \, (Lead\ t)_{ds} + \sum_{k=0}^{K} \beta_k \, (Lag\ k)_{dt} + \gamma_{ds} + \tau_{dm} + \epsilon_{dsm}) \qquad (1)$$

The leading indicators $\beta_j$ take on the value of 1 at *j* leading periods before permitless carry law has been implemented in treated states. The leading indicators span 36 months to 1 month before the law's implementation date. The reference period is relative to 36 months prior to the law's effective date. The lag indicators $\beta_k$ take on the value of 1at *k* lagging periods after the permitless carry law has been implemented in treated states.The coefficients $\beta_k$ are indicator variables for each of the months following the implementation of a permitless carry law. Standard errors are clustered at the state-level.

*5.2. Testing for Anticipation: Aggravated Gun Assaults vs. Larceny-theft*

Next, I more formally compare the effects of permitless carry law implementation on two outcomes, one highly likely to be highly impacted by anticipatory effects, aggravated assaults with a gun, and one unlikely to be impacted, larceny-theft. I use strategies proposed in Malani and Reif (2015) that employ leads of the treatment variable to capture forward-facing effects. The intuition for this approach is that if the assumption of no anticipation is correct, then inclusion of leading indicator variables should have no impact on the estimated treatment effect and the leading indicator variables should be approximately zero.

I use a stacked difference-in-differences approach. As described above, I modify panel data to consist of stacks for each treated unit, in this case each police agency. Each stack consists of reporting agencies in a treated state and reporting agencies in comparison group states. Agencies in 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 24-month post-period for a treated unit. No states that are already treated are used in the comparison group. 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 a police agency unit and time fixed effects and a stack-specific indicator variable. I center data on the implementation of a permitless carry law and I start with a stacked Poisson regression model that takes the following form:

$$Y_{dim} = exp(\beta_1 Treated_{dim} + \gamma_{di} + \phi_{dm} + log(pop_{dim}) + \epsilon_{dim}) \qquad (2)$$

The coefficient $\beta_1$ is the average treatment effect of implementing a permitless carry law. I include stack-specific police agency fixed effects $\gamma_{di}$ and stack by month fixed effects $\phi_{dm}$. Because this model does not include specification for leads, it assumes that there are no pre-treatment anticipatory effects.

Next, I compare the baseline model in equation (2) to models that account for anticipation[6] by including a series of leading indicators relative to the implementation of a permitless carry law. These models take the form:

$$Y_{dim} = exp(\beta_1 Treated_{dim} + \sum_{t=-12}^{T} \beta_j (Lead)_{dim} + \gamma_{di} + \phi_{dm} + log(pop_{diy}) + \epsilon_{dimy}) \qquad (3)$$

The leading indicators, $\beta_j$ take on the value of 1 if the law has been implemented at *j* leading periods before the first period of treatment. I estimate six versions of Equation 3 so that the number of leading indicators can range across the time window of staggered adoption that includes passage of permitless carry laws from 12 months to 2 months before the law's effective date though most states passed permitless carry laws 3-4 months prior to implementation. The coefficients $\beta_j$ are indicator variables for each of the pooled 2-month increments leading up to the implementation of a permitless carry law.

I find that the most significant increases in Google Trends searches coincide with the passage rather than the implementation of a permitless carry law. Combined with significant lead effects for

[6] Malani and Reif (2015) refer to these models as "quasi-myopic" models.

aggravated assaults without evidence of anticipation effects for larceny-theft[7], I use the passage date of a permitless carry law as the reference period in main stacked event study specifications.

*5.3. Baseline Model: Stacked Event Study*

Using the empirically-identified reference period of the passage date of a permitless carry law, I estimate the effects of permitless carry laws on crime outcomes using a stacked event study. I construct the stacked regression panel at the agency-level as described in the prior section, where each stack consists of reporting agencies in a treated state combined with reporting agencies in comparison states that are not yet or never treated during the study timeframe. Treatment-specific stacks are appended together and a Poisson fixed effect dynamic 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. The stacked panel is centered on the passage date of the permitless carry law. The event study or dynamic difference-in-differences model takes the following form:

$$Y_{dim} = exp(\sum_{t=-12;\, t\neq-2}^{T} \beta_j (Lead)_{dim} + \sum_{k=0}^{K} \beta_k (Lag\ k)_{dim} + \gamma_{dm} + \phi_{dm} + log(pop_{dim}) + \epsilon_{dim}) \quad (4)$$

Where $Y_{dim}$ is a crime outcome in police agency jurisdiction *i* in month $m$. The coefficients of interest $\beta_j$ and $\beta_k$ capture the average treatment effect of permitless carry law passage in the 12 months proceeding and following passage of a permitless carry law. Outcomes are pooled over 2 months as some months and agencies have sparse outcome data. Standard errors are clustered at the agency level. Models 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. Event study figures report the percent change in the rate of crime outcomes in treated agencies, coefficients are transformed by 100 x (exp($\beta_1$)-1).To obtain the percent change in the rate of crime outcomes in treated agencies, coefficients are transformed by 100 x (exp($\beta_1$)-1).[8]

## 6. Results

I start the analysis by examining search volume for permitless carry-related terms using Google Trends data relative to 36 months prior to the implementation of the law. Because most permitless carry laws are passed 3-4 months prior to the law's implementation, this enables me to determine if search volume is concentrated around the passage or implementation of the law. I use search terms pertaining to permitless carry, including "constitutional carry", "concealed carry", "gun laws" and "gun control" as the search tem "permitless carry" is not regularly used in searches.

Figure 2 plots event study coefficients from Equation 1, where data is centered on the implementation date of a permitless carry law. Panel A shows a significant spike in search volume for "constitutional carry" four months prior to the implementation of the law, intersecting with the law's passage date in the majority of states. Searches for "concealed carry" demonstrate similar patterns with

[7] I additionally examine motor vehicle theft which may be better-reported due to insurance claims and find similar effects. This includes police weapons law arrests.

[8] 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.

significant increases around the timing of a permitless carry law passage rather than when the law takes effect (panel c). While there is a significant increase in at the month of implementation for constitutional carry searches, the break larger in magnitude occurs four months prior for all search terms pertaining to gun laws with the exception of gun control, which is consistent with searches for information about permitless carry specifically rather than broad searches for impending gun regulations. Immediately following the law's implementation, there is a downward trend in search interest for constitutional carry and concealed carry search terms which suggests focus declines after implementation, making responses months after implementation unlikely.

While the search trend evidence is suggestive of possible positive anticipatory response on the part of individuals interested in information about permitless carry laws, it is possible that search behaviors may not strongly correlate to changes in crime. To more formally test for anticipatory responses, I examine effects leading up to permitless carry law implementation using crimes types more likely to be impacted by permitless carry laws (aggravated gun assaults) in contrast to crime types less likely to be impacted (property crimes including larceny-theft and motor vehicle theft). Table 2 presents results from variants of Equation 2 which create leads for each pooled 2-month period up until 12 months prior to the implementation of a permitless carry law. If the assumption of no anticipatory effects is correct, adding indicators for months prior to implementation should not change estimates of $\beta_1$. Across the first row of Table 2, the estimated effect increases from an increase in aggravated assaults with a firearm of 8.45% in the months following the implementation of a permitless carry law to 11.9% with the inclusion of leading indicators for the duration of the staggered passage of the permitless carry laws from months 10.[9] In Table 3, effects for larceny theft are presented with as would be theoretically anticipated, no significant treatment effect shifts with the inclusion of leading indicators. Appendix Table A.2. repeats this analysis for motor vehicle theft, which may be more well-reported to police relative to theft.

Taken together, the significant increase in search volume intersecting with the passage rather than the implementation of permitless carry laws combined with significant leads for aggravated gun assaults is suggestive that the passage date of a permitless carry law rather than the implementation date may be the more salient start of treatment. In the subsequent analysis, the passage date rather than the implementation date will be used for the reference period in event study specifications.

### *6.1 Results for Crime Outcomes*

Figure 3 presents estimates of the effect of passing a permitless carry law on crime using the event study specification in Equation 3. I estimate that passage of a permitless carry law increases violent index crime by 8.3% (panel a) and property index crime by 5.2% (panel b) relative to agencies in states that did not pass a permitless carry law. These effects occur fairly quickly after the passage of the permitless carry law and persist into the implementation period of the law, which is staggered between pooled months 4 and 8 in the post-period. Estimates translate to approximately 3,546 additional violent index crimes and approximately 9,540 additional property crimes in the twelve months following passage of a permitless carry law. Violent index crime increases are driven by aggravated assaults, which rose an estimated 11.2% after permitless carry law passage (panel c) without significant increases in robbery (panel g). The effect on simple assaults is a somewhat precise null estimate, which suggests that interpersonal conflict is not impacted or deterred by passage of a permitless carry law. While estimates for

[9] Oklahoma passed permitless carry eight months prior to implementation.

larceny-theft and motor vehicle theft are statistically imprecise and close to zero in magnitude, burglary increased an estimated 6.78% (panel e).

Figure 4 compares estimates of firearm and non-firearm crimes, specifically aggravated assaults and robbery. For effects to be credibly driven by permitless carry laws, the majority of the increase in aggravated assaults should result from assaults involving firearms rather than knives or other weapons. I estimate an increase of 15.0% in aggravated assaults committed with a firearm (panel a) and a statistically imprecise and modest increase of 2.6% for aggravated assaults not committed with a firearm (panel b). The increase in aggravated assaults committed with a firearm equates to approximately 998 additional firearm assaults in the twelve months following passage of a permitless carry law. I find imprecise but positive estimates suggestive of increases in robbery committed with a gun, and no significant increase in non-firearm robberies. The increase in aggravated assaults committed with a firearm occurs shortly following the passage of the permitless carry law. In Appendix Figures A.2-A.3, I estimate effects in agencies serving jurisdictions over 90,000 in population which may be more impacted than communities with lower population density. Violent crime and property crime increases were higher in magnitude in larger communities (15.2% increase for violent index crime and 9.0% increase for property index crime).

In Figure 5, I focus on the circumstances of aggravated firearm assaults as a falsification exercise to examine whether changes in aggravated gun assaults are consistent with permitless carry laws which should alter encounters that occur in public. Consistent with permitless carry taking effect through altering gun carrying in public places, aggravated firearm assaults between victims and perpetrators whose relationship is that of acquaintances or strangers increase 16.4% after a permitless carry law is enacted (Figure 5, panel b). I do not find a significant increase in domestic violence, or conflicts that occur between family or intimate partners (panel a) that would most likely occur in residential locations. Aggravated firearm assaults increase in public locations by 13.5% (panel c). Finally, victims of aggravated gun assaults are increasingly likely to experience injury associated with the gun assault (panel d) which suggests an escalation of public disputes between strangers.

*6.2 Robustness*

In Appendix Figures A.4-A.7, I re-estimate main crime and firearm crime effects using an alternative sample and comparison group. Appendix Figures A.4-A.5 report qualitatively similar main effects on property and violent crime using a more comprehensive agency sample from the UCR Summary Reporting System (SRS). Appendix Figures A.6-A.7, I report main estimates from restricting the NIBRS comparison group to consist only of future adopting states. On the whole, these estimates are consistent with main crime and gun crime effects.

In Appendix Tables A.5-A.8, I present results for main crime and firearm crime results for alternative specifications and estimators using controls for demographics and lagged policing employment and incarceration rates. In Tables A.6-A.7, I estimate Poisson models with controls for both the NIBRS and SRS sample. In Tables A.8-A.9, I estimate log-linear models with the same controls. I find a smaller percent decline in violent crime and do not find statistically significant declines in property crime. Because a log transformation places more weight on smaller counts, it is possibly suggestive of treatment effect heterogeneity where places with higher levels of crime have larger effects. This is consistent with results from agencies serving jurisdictions over 90,000 in population, where the magnitude of treatment effects was larger than the main sample (Appendix Figures A.2-A.3).

In Appendix Figures A.8-9, I include only agencies in states that passed a permitless carry law prior to 2019, as post-period treatment effects may be influenced by the start of the Covid-19 pandemic

(Abrams, 2021). These models exclude 2019 passage states including Oklahoma, Kentucky and South Dakota. In Appendix Figures 10-11, I use the same sample of early adopters to extend the post-period to 18 months after passage of a permitless carry law. I find consistent increases in violent index crime with significant increases in aggravated assaults and aggravated assaults with a gun. I find suggestive evidence of increases in gun robbery. In Appendix Figures 12-13, I exclude Missouri, which in 2016, the year before permitless carry was enacted, removed the duty to retreat requirement from its Stand-Your-Ground Law . Results in magnitude remain consistent and main effects are not driven by large impacts from Missouri. Finally, in Appendix Figures 14-15, I estimate main crime and firearm crime effects using the law's effective date rather than passage date relative to the year prior to passage and use one year prior to treatment as the reference period.

*6.3 Mechanisms*

In this section, I explore mechanisms discussed earlier that may account for policy effects, including changes in policing and shifts in firearm availability. The post-period in the main analysis does limit understanding in particular of policing and outcomes whose impacts may take effect over a longer time window.[10]

I begin by focusing on short-term policing outcomes, specifically changes in police enforcement targeted at illegal public weapons law carrying, known as weapons law violations. I fit event study models in Equation 3 to both offenses and arrests. Because I find that police recorded substantially fewer weapons law arrests in advance of treatment (Appendix Figure A.3), I again employ the passage date as the reference period. Figure 7 presents estimates of weapons law offenses reported by police, showing a 17.8% reduction in offenses recorded by police (panel a). Similarly, police arrests for weapons law violations declined by 22.9% following permitless carry law passage. Appendix Figure A.16 presents violent and property index arrests which do not show comparable declines to rule out a secular decline in police arrests. The reduction in offenses, which may be initiated by a police motorist or pedestrian stop, and not just in arrests, is suggestive of a decline in contact between police and individuals police suspect to be carrying an illegal handgun. Lessened police focus on weapons law violations may therefore contribute to changes in public behaviors of gun carriers if they are less likely to encounter police. This shift may account for additional burglaries, as I do not observe an increase in guns stolen in the course of a burglary.

Figure 8 presents results from crime clearances, showing no significant increase or decrease in violent or property crime clearance in the 12 months following the passage of a permitless carry law (panels a and b). In panel c and d, counts of cleared aggravated assaults and aggravated gun assaults are insignificant, though are positive in magnitude. Appendix Figure A.17 presents results from the SRS sample with similar findings for violent crime but a modest increase in property crime clearance. While these findings are differs from Donohue et al. (2025) who find that the passage of right-to-carry laws result in substantial long-term declines in police ability to clear violent crime, it is possible that the additional attention to gun violence that occurs alongside permitless carry passage or the public nature of aggravated gun assaults, which may be more likely to have witnesses and result in injury, may in the short-term counteract longer term declines in clearance rates.

[10] For instance, Dononue et al. (2025) find significant decreases in clearance rates following adoption of right-to-carry laws. Because I use NIBRS and UCR to calculate clearance rates, for NIBRS, I am able to observe that X% of aggravated assaults are cleared within a year of the initial incident report. This is consistent with a literature examining time to clearance using both NIBRS (*Clearing Murders*, n.d.) data.

I next turn to analysis that focuses on gun availability by examining outcomes including background checks for firearm purchases and a measure of stolen gun incidents recorded in NIBRS. Figure 9 presents estimates of handgun and long gun background checks. I find a fairly small but significant increase in background checks for handguns following permitless carry law passage. While this finding differs somewhat from Kim & Chen (2025) who find no change in gun purchasing following permitless carry law implementation, both findings underline that gun purchasing is at most a modest pathway by which permitless carry laws increase crime. Results from changes in the composition of gun violence victims by age provide further suggestive evidence that permitless carry laws may take effect through existing rather than new gun owners. The largest increase in victimization occurs among individuals above the age of 40 (Appendix Figure A.20, panel c), the age group most likely to already own guns. Finally, I find a 9.8% increase in stolen gun incidents (Figure 9, panel c) which suggests an increase in public carrying.

## 8. Conclusions

This study examines the impact of permitless carry laws on crime. Passage of a permitless carry law increases violent index crime by 8.3%, driven by a 15% increase in aggravated gun assaults. Property index crime increased 5.2%, driven by increases in burglary without evidence of decreases in other types of property crime. Permitless carry laws are accompanied by a substantial reduction in police enforcement of weapons laws violations which may lower the perceived and real costs of carrying an illegal firearm.

Rapid responses permitless carry law passage combined with a modest increase in handgun background checks suggest existing gun owners alter their carrying behavior. This evidence is consistent with survey data showing that Americans doubled the rate at which they carry concealed handguns in public during this time period (Rowhani-Rahbar et al., 2017). It is likely that the frequency of gun carrying on the part of existing owners drives a substantial part of the observed policy impacts rather than increases in gun ownership which is unobservable.

Policy options that promote safe firearm carrying, storage and reduce illegal market availability may be most impactful for permitless carry states as they engage existing firearm carriers. 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.

## TABLES AND FIGURES

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

| State | Passage Date | Effective Date | Months Between Announcement and Effective Date (rounded) | Included in Study Timeframe | Included in NIBRS Sample | Included in SRS 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 | 0 | 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 | No |
| 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: Treatment dates are obtained from the RAND Gun Law Database and passage dates from Google news searches. States that reported to NIBRS but that already enacted permitless carry including AZ and VT are excluded.

**Table 2:** Descriptive Statistics of NIBRS Crime Outcomes

| | Panel (1/1/2013 - 2/1/2020) | | | | Stacked Panel (12 months Pre/Post Treatment) | | | |
|---|---|---|---|---|---|---|---|---|
| | Control | | Treatment | | Control | | Treatment | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Violent Index Crime | 16.7 | 87.2 | 7.6 | 57.7 | 34.5 | 128.4 | 16.5 | 77.2 |
| Aggravated Assault | 8.6 | 42.6 | 3.9 | 25.8 | 17.6 | 63.2 | 7.6 | 33.8 |
| Robbery | 3.2 | 22.3 | 1.2 | 12.5 | 6.9 | 32.4 | 3.9 | 21.5 |
| Simple Assault | 37.4 | 127.1 | 17.6 | 71.9 | 74.3 | 179 | 35.7 | 99.8 |
| Property Index crime | 40.8 | 156.4 | 20.6 | 107.2 | 81.6 | 225.6 | 50.1 | 184 |
| Burglary | 22.1 | 84.8 | 11.7 | 52.6 | 43.9 | 122.2 | 30.3 | 95.3 |
| Motor Vehicle Theft | 9.6 | 48.4 | 4.7 | 33.2 | 20.1 | 71.3 | 12.5 | 56.4 |
| Larceny-theft | 56 | 178.6 | 25.7 | 111.8 | 56.4 | 155.8 | 25.8 | 108.8 |
| Aggravated Assault - Gun | 2.2 | 16 | 0.9 | 10 | 4.7 | 24.7 | 1.8 | 13.1 |
| Aggravated Assault - Non Gun | 2.9 | 13.9 | 1.2 | 7.5 | 6 | 20.1 | 2.4 | 8.1 |
| Robbery - Gun | 1.4 | 11.4 | 0.5 | 6.5 | 3 | 16.9 | 1.8 | 11.3 |
| Robbery - Non Gun | 0.4 | 2 | 0.2 | 1.3 | 0.8 | 3 | 0.5 | 2.2 |
| Unique Observations | 91422 | 91422 | 35122 | 35122 | 287784 | 287784 | 29553 | 29553 |
| Agencies | 2483 | 2483 | 954 | 954 | 1334 | 1334 | 278 | 278 |

Notes: The NIBRS sample includes 3,437 agencies that reported crime data consistently and accurately in each year with a sample of 1,612 in the stack that report at least one crime in the months preceding and following passage of a permitless carry law. Crime outcomes are reported at the agency-pooled month level.

**Table 3:** Effects of Permitless Carry Law Enactment on Aggravated Assaults with a Firearm

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Agg. Assault - Gun | Agg. Assault - Gun | Agg. Assault - Gun | Agg. Assault - Gun | Agg. Assault - Gun | Agg. Assault - Gun | Agg. Assault - Gun |
| Permitless Carry Law | 0.0812** (0.0280) | 0.0901** (0.0299) | 0.0926** (0.0289) | 0.1004*** (0.0303) | 0.1008** (0.0323) | 0.1125*** (0.0341) | 0.1071*** (0.0317) |
| Lead 2 | | 0.0886* (0.0396) | 0.0911* (0.0377) | 0.0987** (0.0376) | 0.0991* (0.0414) | 0.1107** (0.0410) | 0.1053** (0.0380) |
| Lead 4 | | | 0.0248 (0.0355) | 0.0324 (0.0355) | 0.0328 (0.0341) | 0.0444 (0.0352) | 0.0390 (0.0376) |
| Lead 6 | | | | 0.0707* (0.0298) | 0.0710* (0.0323) | 0.0826* (0.0352) | 0.0772* (0.0344) |
| Lead 8 | | | | | 0.0028 (0.0317) | 0.0144 (0.0331) | 0.0090 (0.0317) |
| Lead 10 | | | | | | 0.0793* (0.0369) | 0.0739* (0.0368) |
| Lead 12 | | | | | | | -0.0337 (0.0457) |
| Stack by Agency FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stack by Year by Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 |

Notes: Permitless carry law coefficients are from Equation 2 and 3 which estimate the change in crime outcomes following enactment of a permitless carry law.. 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 12 months in the post period only. Standard errors are clustered at the state level.

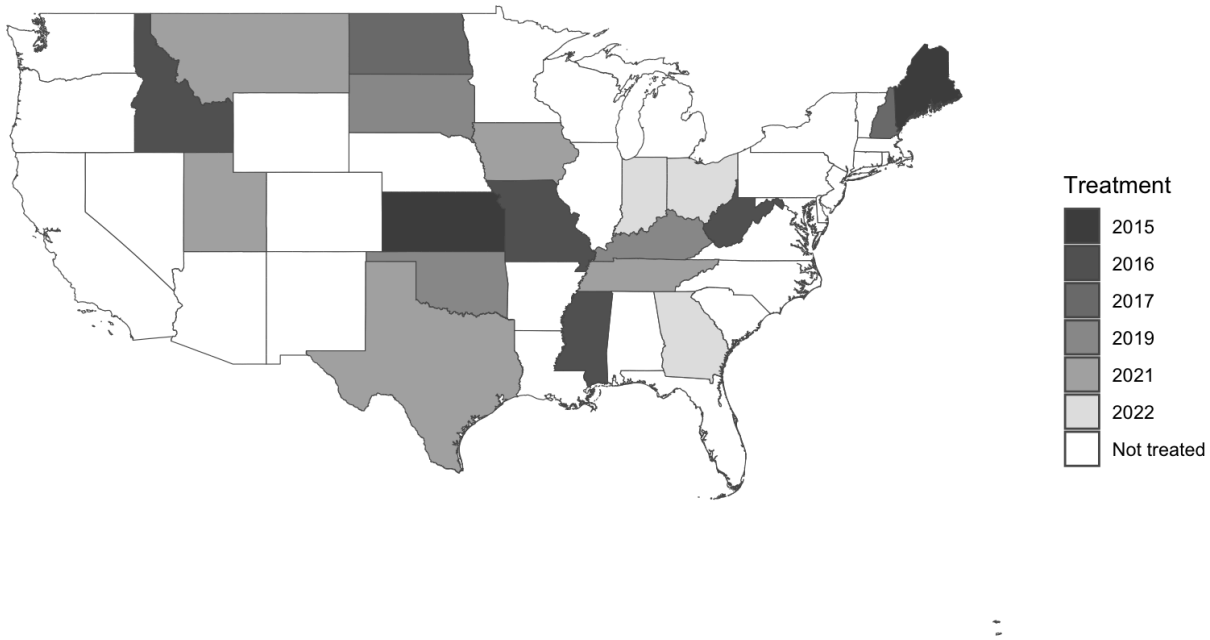
**Table 4:** Effects of Permitless Carry Law Enactment on Theft

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Theft | Theft | Theft | Theft | Theft | Theft | Theft |
| Permitless Carry Law | 0.0132<br>(0.0307) | 0.0171<br>(0.0325) | 0.0176<br>(0.0334) | 0.0201<br>(0.0360) | 0.0117<br>(0.0382) | 0.0031<br>(0.0395) | -0.0035<br>(0.0413) |
| Lead 2 | | 0.0435<br>(0.0387) | 0.0440<br>(0.0404) | 0.0464<br>(0.0427) | 0.0381<br>(0.0465) | 0.0296<br>(0.0497) | 0.0231<br>(0.0517) |
| Lead 4 | | | 0.0054<br>(0.0312) | 0.0078<br>(0.0327) | -0.0004<br>(0.0368) | -0.0089<br>(0.0395) | -0.0154<br>(0.0409) |
| Lead 6 | | | | 0.0242<br>(0.0345) | 0.0160<br>(0.0377) | 0.0075<br>(0.0398) | 0.0011<br>(0.0413) |
| Lead 8 | | | | | -0.0717*<br>(0.0322) | -0.0802*<br>(0.033) | -0.0867*<br>(0.0338) |
| Lead 10 | | | | | | -0.0620<br>(0.0491) | -0.0686<br>(0.0530) |
| Lead 12 | | | | | | | -0.0398<br>(0.0318) |
| Stack by Agency FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stack by Year by Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 |

Notes: Permitless carry law coefficients are from Equation 2 and 3 which estimate the change in crime outcomes following enactment of a permitless carry law.. 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 12 months in the post period only. Standard errors are clustered at the state level.

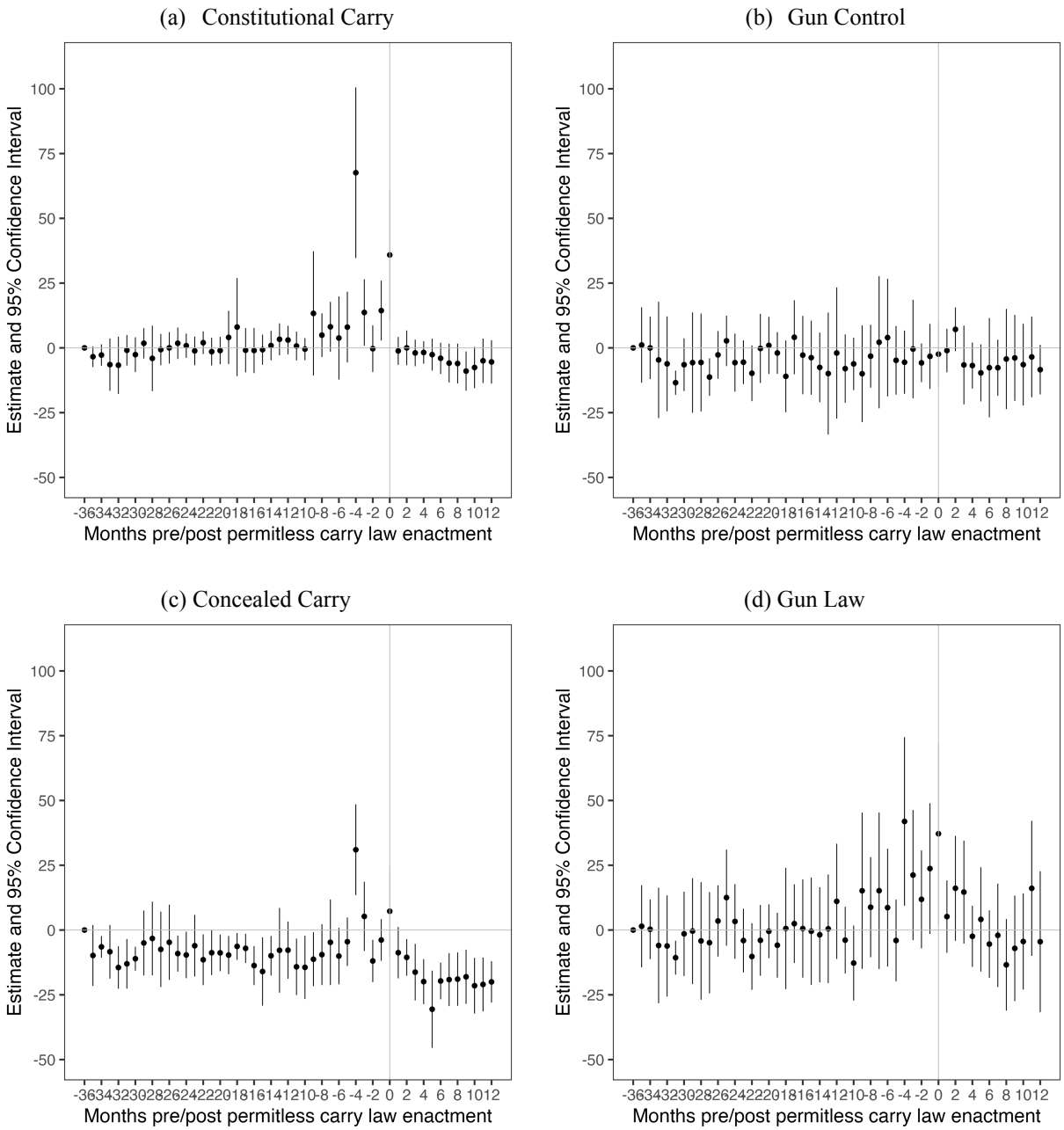
**Figures**

**Figure 1:** States by Permitless Carry Adoption Status




Note: Permitless carry adoption in Vermont occurred in 1903 and is grouped with Alaska (2003) as treated before 2004. Wyoming (2010) and Arizona (2011) fall outside of the sample timeframe. States that have yet to adopt permitless carry include: AL, IA, LA, MT, NC, NE, OH, SC, TN, TX, UT. The analytic sample includes states shaded up until 2019.

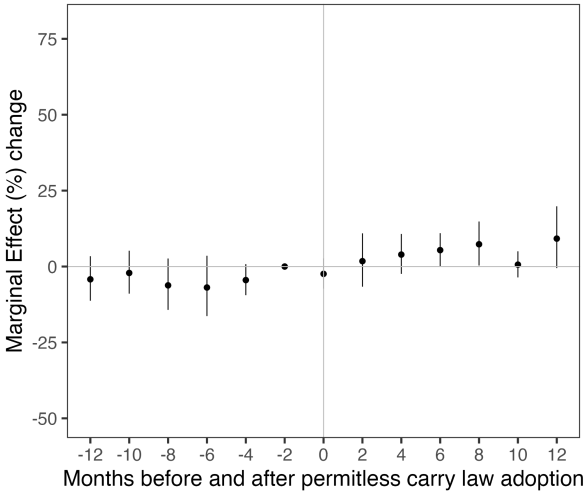
**Figure 2:** Google Trends Searches for Permitless Carry-Related Search Terms Before and After Permitless Carry Law Adoption




**Notes:** The vertical axis shows the coefficient from Equation 1 for the change in the search index (on a scale from 0 to 100) relative to the 36 months before (negative values) and one year after (positive values) the permitless carry law was implemented. Models are fit with a linear specification. Standard errors are clustered at the state level.

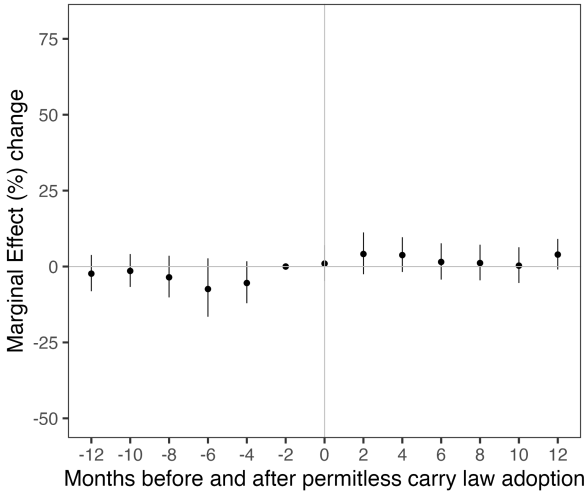
**Figure 3:** Effect of Permitless Concealed Carry Laws on Crime

(a) Violent Index Crime



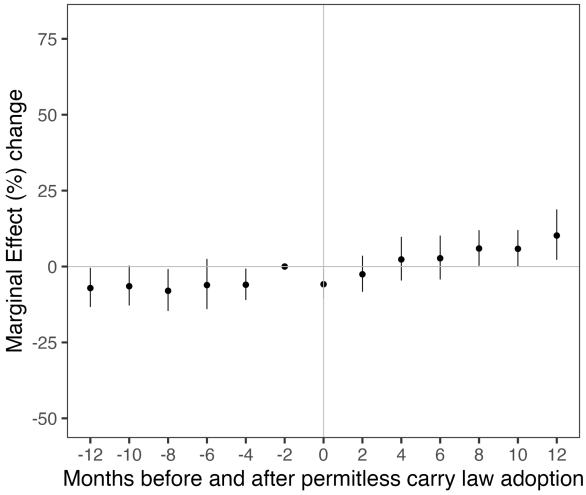

Avg. Effect, months 0 to 12 (%) = 8.729
p< 0.0016
Agency N = 1612
Dep. V. Mean = 25.238

(b) Property Index Crime



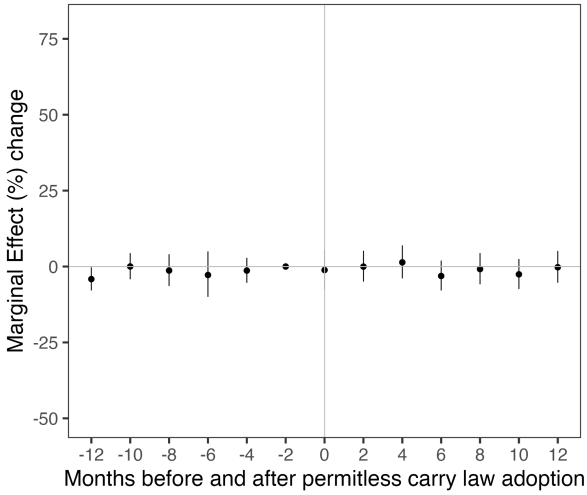

Avg. Effect, months 0 to 12 (%) = 5.197
p< 0.0002
Agency N = 1612
Dep. V. Mean = 114.048

(c) Aggravated Assault




Avg. Effect, months 0 to 12 (%) = 11.206
p< 0.0001
Agency N = 1612
Dep. V. Mean = 17.277

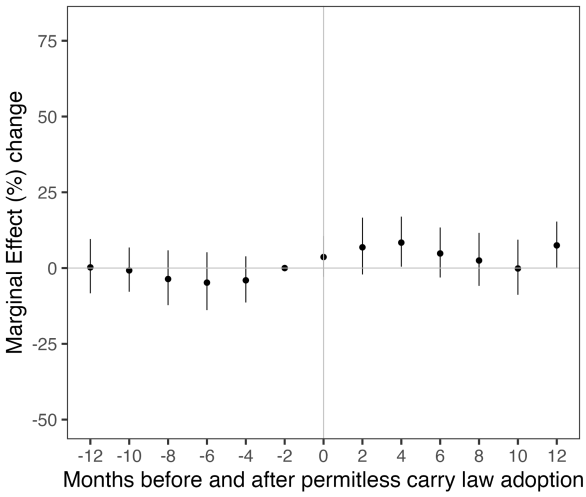
(d) Simple Assault



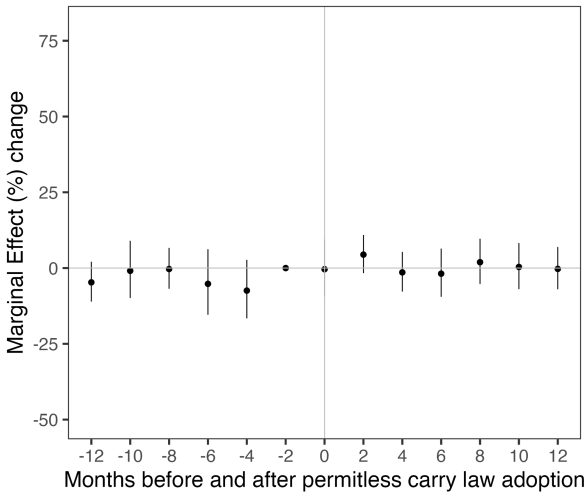

Avg. Effect, months 0 to 12 (%) = 0.734
p< 0.5922
Agency N = 1612
Dep. V. Mean = 74.785

(e) Burglary

(f) Motor Vehicle Theft

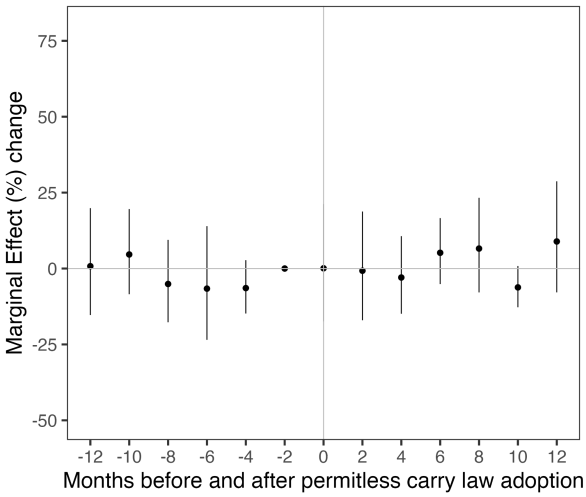



Avg. Effect, months 0 to 12 (%) = 6.783
p< 0.0001
Agency N = 1612
Dep. V. Mean = 46.865



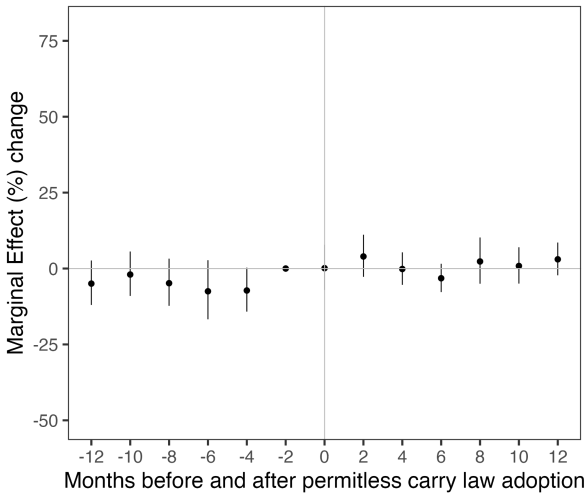

Avg. Effect, months 0 to 12 (%) = 4.088
p< 0.0582
Agency N = 1612
Dep. V. Mean = 19.552

(g) Robbery




Avg. Effect, months 0 to 12 (%) = 4.172
p< 0.2314
Agency N = 1612
Dep. V. Mean = 7.171

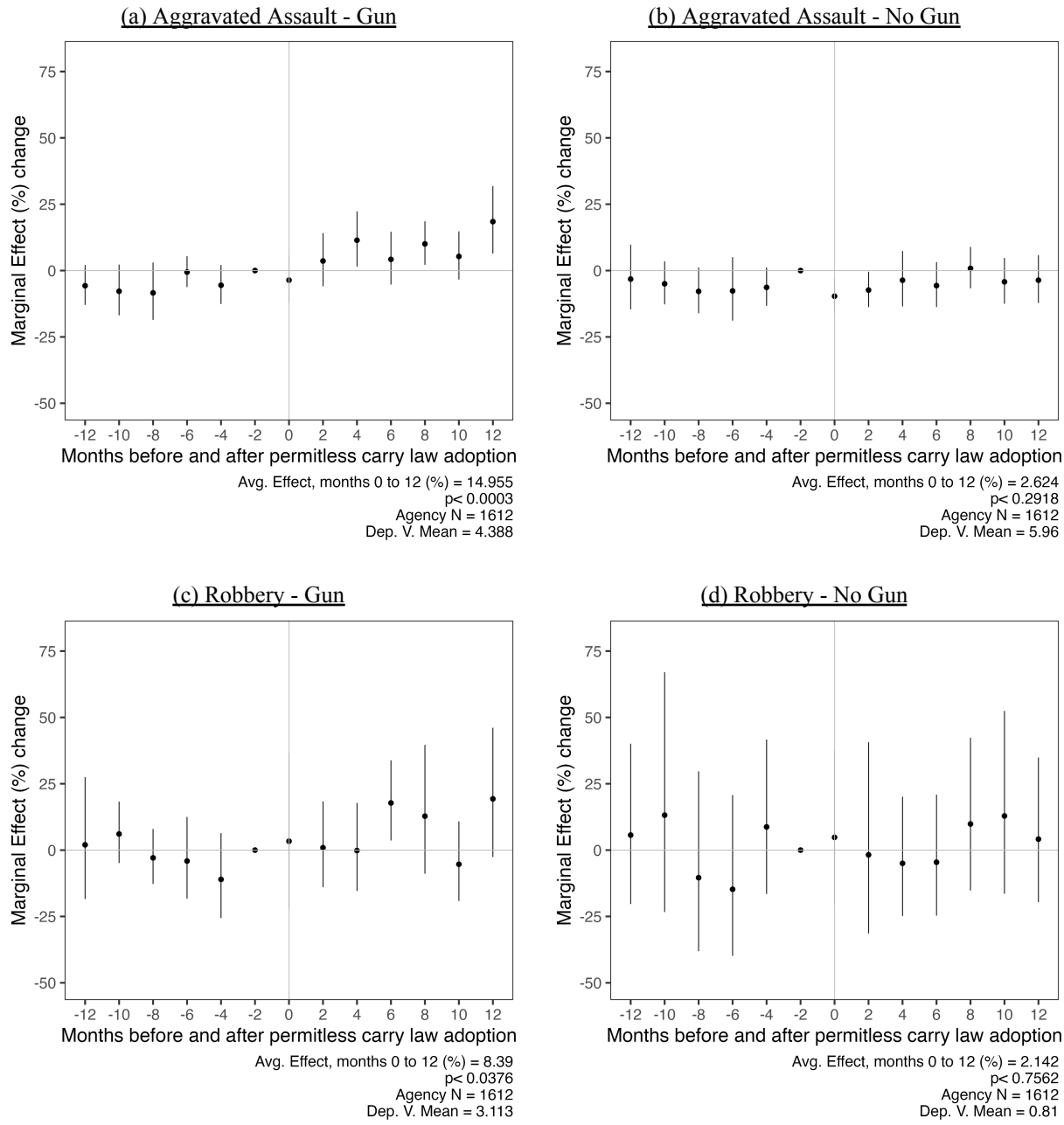
(h) Larceny-theft




Avg. Effect, months 0 to 12 (%) = 5.733
p< 0.0444
Agency N = 1612
Dep. V. Mean = 57.81

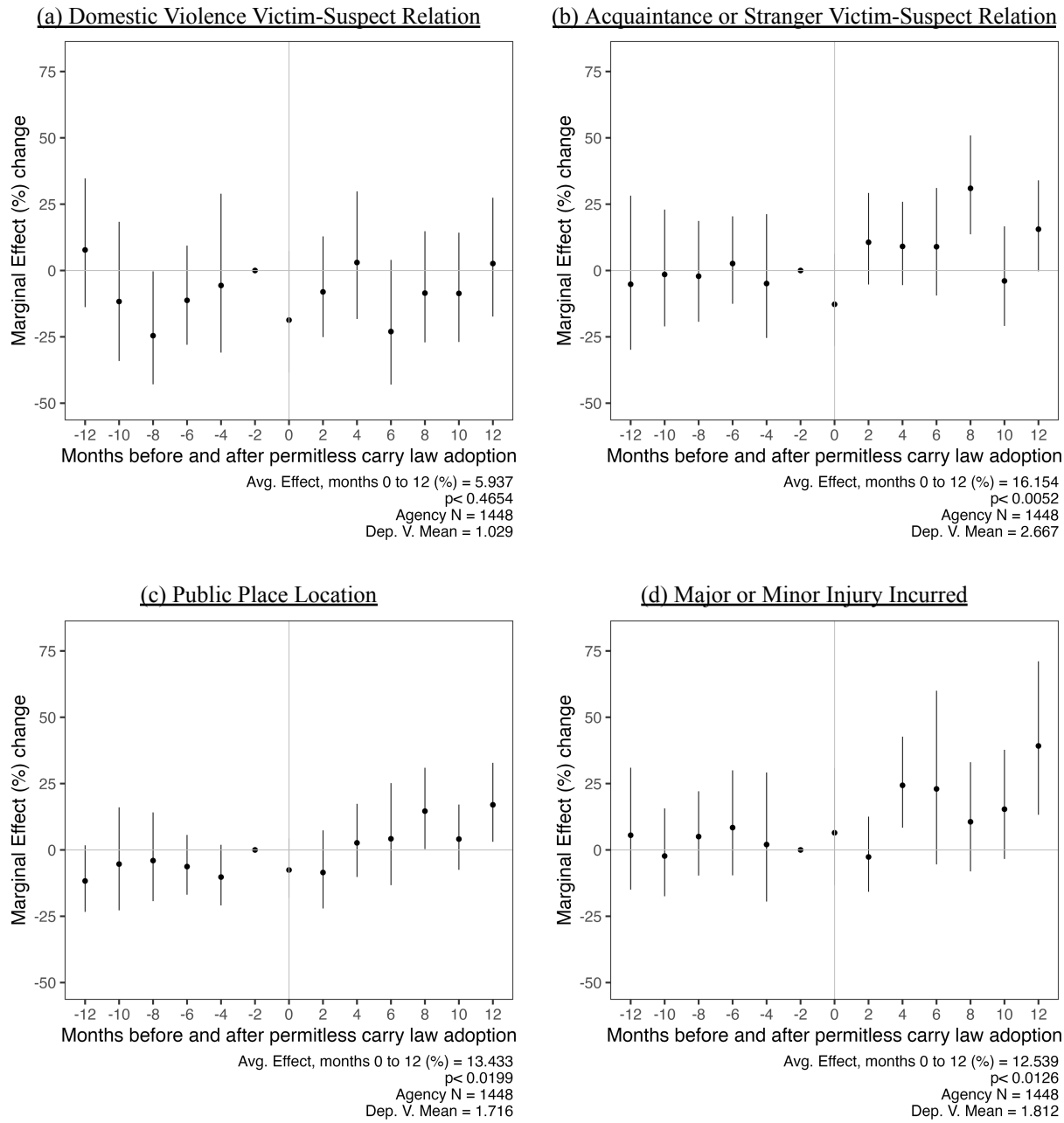
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 2,067,573 observations pooled into 2-month groups at the agency level. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments.

**Figure 4:** Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 2,067,573 observations pooled into 2-month groups at the agency levelStack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments.

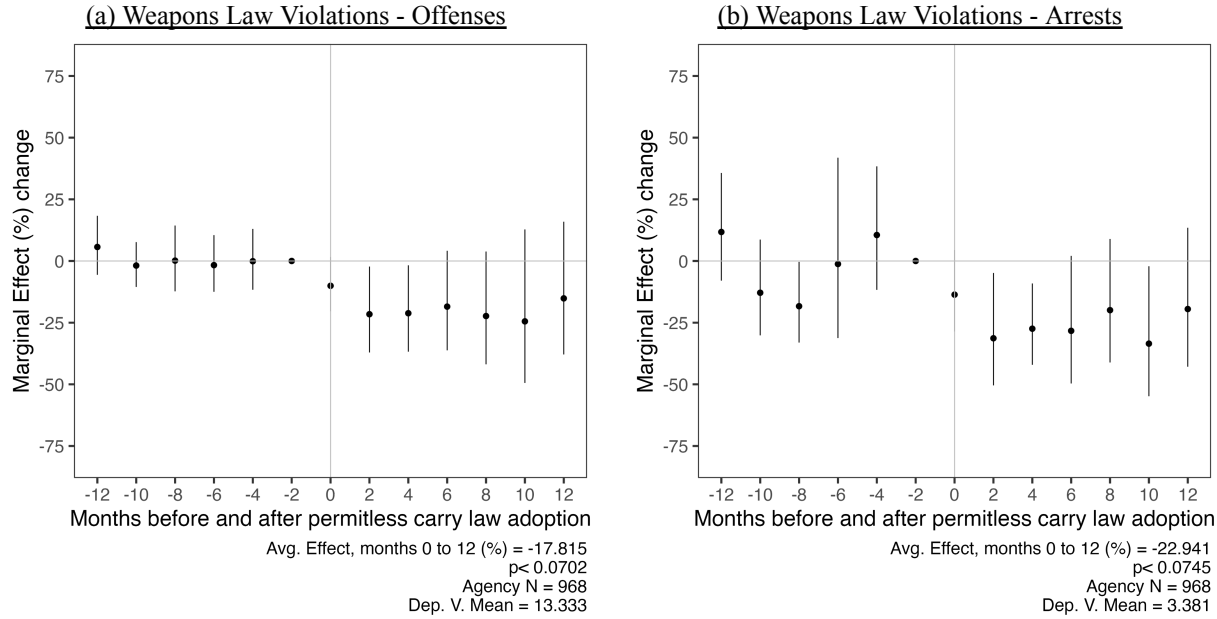
**Figure 5:** Effect of Permitless Concealed Carry Laws on Aggravated Gun Assault Circumstances




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. 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,

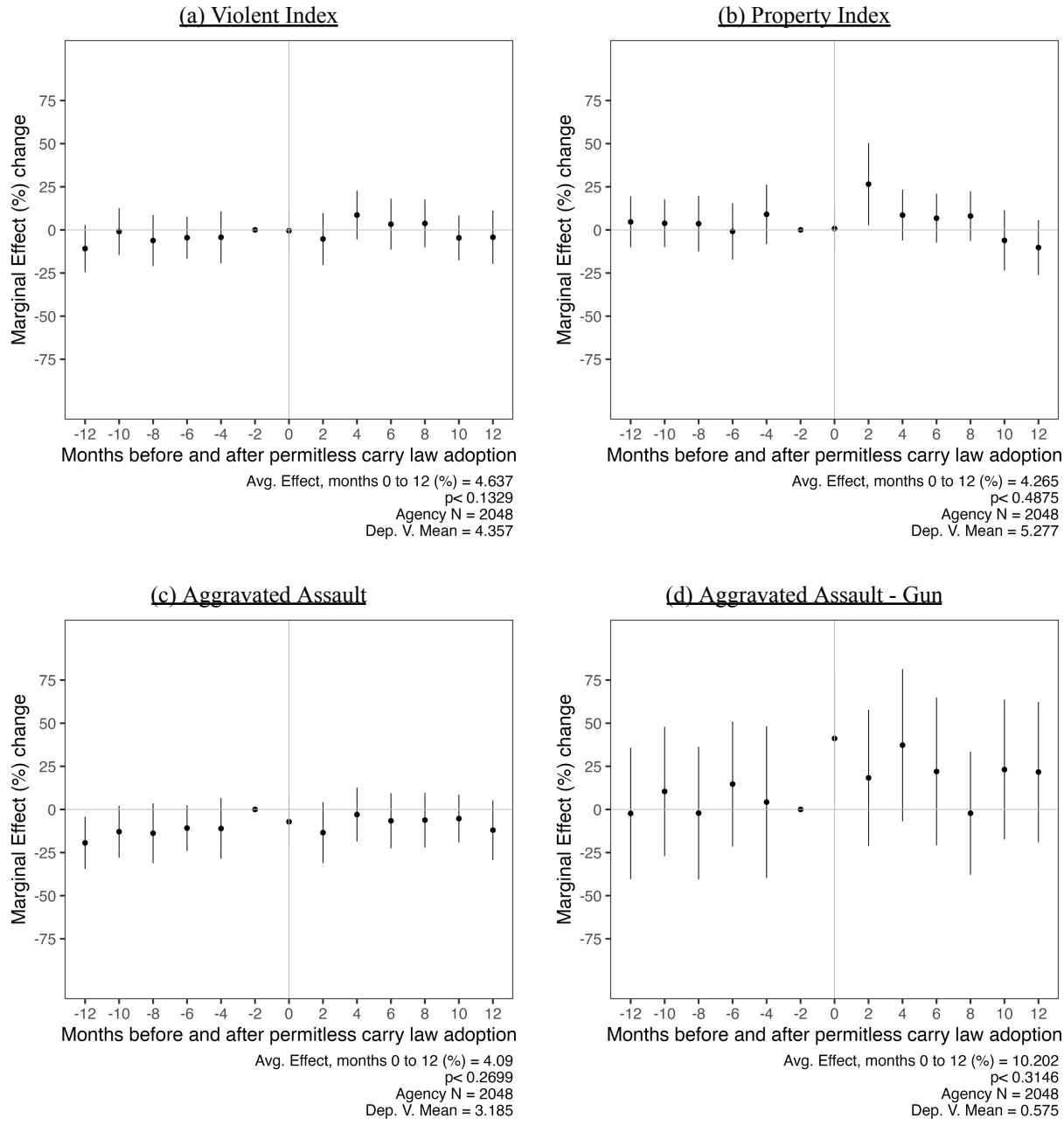
bank/savings and loan, bar/nightclub, convenience store, department/discount store, drug store/doctor's office/hospital, restaurant. To measure domestic violence-related crime, victim suspect relationships pertaining to family or dating relationships are combined, including: spouse, parent, sibling, child, grandparent/grandchild, in-law/step-parent, or other family member. To measure acquaintance / stranger relationships, relationships including victim was an acquaintance, friend, neighbor or stranger were combined.

**Figure 7:** Effect of Permitless Concealed Carry Laws on Policing Weapons Law Violations



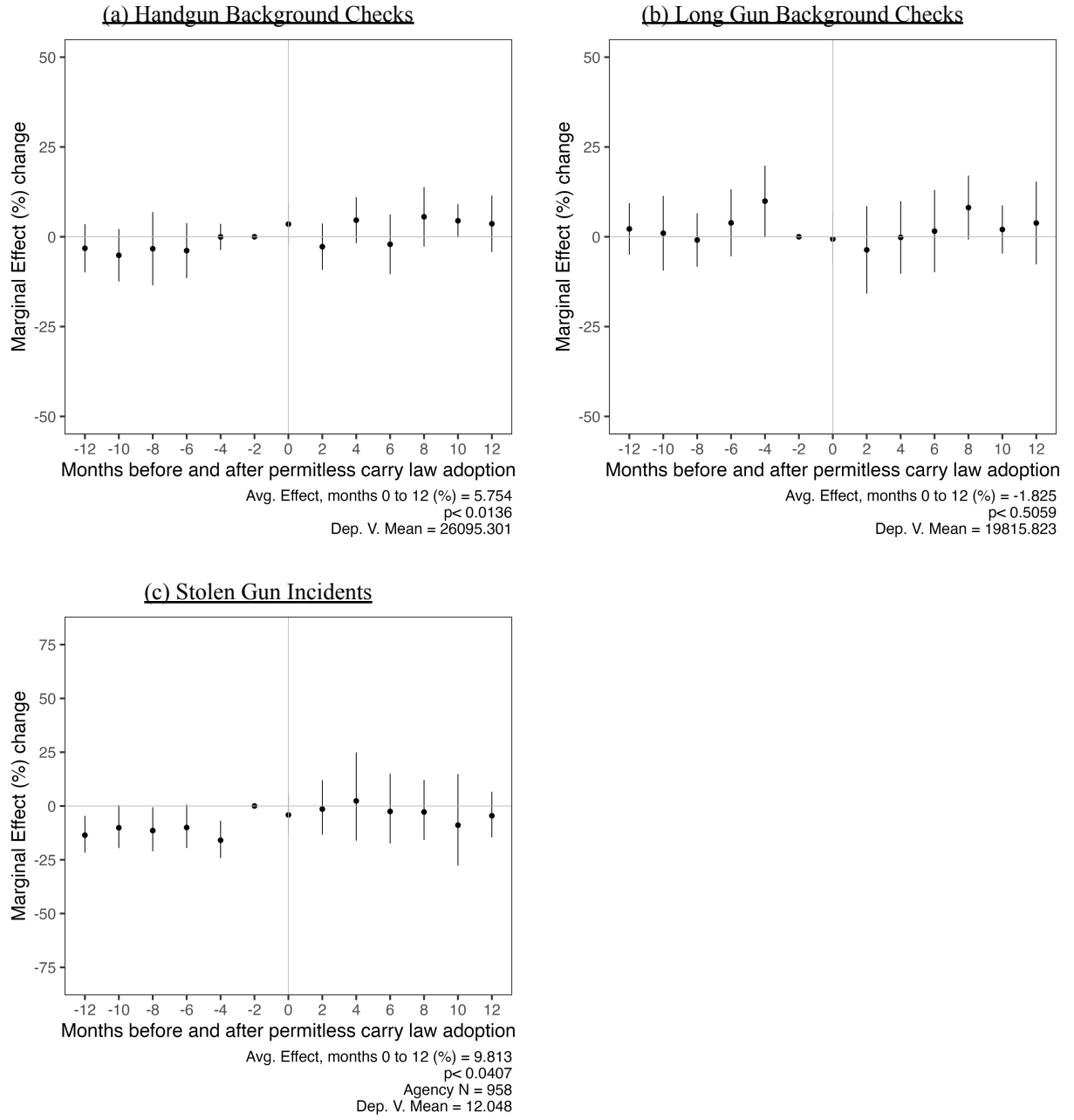

Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments.

**Figure 8:** Effect of Permitless Concealed Carry Laws on Crime Clearance by Arrest




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments.

**Figure 9:** Effect of Permitless Concealed Carry Laws on Gun Availability




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments.

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## Appendix A

### Tables

**Appendix Table A.1:** Agency Coverage by State, NIBRS and SRS

| State | Included in Main Specification | Reason For Exclusion | Number of NIBRS Agencies in Sample | Number of SRS Agencies in Sample | Total Crime Reporting Agencies | Total Reporting Agencies |
|---|---|---|---|---|---|---|
| Treatment States | | | | | | |
| Arkansas | No | 1 | -- | -- | -- | -- |
| Idaho | Yes | | 15 | 15 | 39 | 128 |
| Kansas | Yes | | 25 | 20 | 75 | 404 |
| Kentucky | Yes | | 105 | 48 | 139 | 395 |
| Maine | Yes | | 4 | 13 | 73 | 147 |
| Missouri | Yes | | 5 | 54 | 132 | 653 |
| Mississippi | Yes | | 3 | 12 | 15 | 691 |
| North Dakota | Yes | | 9 | 9 | 29 | 131 |
| New Hampshire | Yes | | 20 | 18 | 62 | 219 |
| Oklahoma | Yes | | 56 | 48 | 150 | 413 |
| South Dakota | Yes | | 9 | 6 | 31 | 172 |
| West Virginia | Yes | | 27 | 14 | 26 | 258 |
| Comparison States | | | | | | |
| Alabama | | | 1 | 1 | 1 | 413 |
| Alaska | No | 2 | -- | -- | -- | -- |
| Arizona | No | 2 | -- | -- | -- | -- |
| California | Yes | | 0 | 401 | 482 | 545 |
| Colorado | Yes | | 67 | 42 | 78 | 264 |
| Connecticut | Yes | | 45 | 61 | 83 | 102 |
| Delaware | Yes | | 23 | 8 | 23 | 48 |
| Florida | No | 3 | -- | -- | -- | -- |
| Georgia | Yes | | 0 | 59 | 84 | 571 |
| Illinois | No | 4 | -- | -- | -- | -- |
| Indiana | Yes | | 0 | 19 | 31 | 292 |
| Iowa | Yes | | 48 | 34 | 72 | 271 |
| Louisiana | Yes | | 12 | 52 | 71 | 265 |
| Maryland | Yes | | 0 | 2 | 3 | 123 |
| Massachusetts | Yes | | 146 | 135 | 177 | 337 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Michigan | Yes | | 163 | 138 | 268 | 691 |
| Minnesota | Yes | | 0 | 102 | 160 | 447 |
| Montana | Yes | | 23 | 12 | 38 | 122 |
| Nebraska | Yes | | 15 | 11 | 18 | 227 |
| Nevada | Yes | | 0 | 10 | 18 | 33 |
| New Jersey | Yes | | 0 | 181 | 322 | 519 |
| New Mexico | Yes | | 0 | 4 | 5 | 112 |
| New York | Yes | | 0 | 94 | 136 | 667 |
| North Carolina | Yes | | 0 | 59 | 89 | 494 |
| Ohio | Yes | | 147 | 114 | 195 | 839 |
| Oregon | Yes | | 38 | 26 | 48 | 151 |
| Pennsylvania | Yes | | 6 | 1 | 2 | 1377 |
| Rhode Island | Yes | | 23 | 22 | 31 | 40 |
| South Carolina | Yes | | 96 | 66 | 108 | 260 |
| Tennessee | Yes | | 168 | 118 | 201 | 364 |
| Texas | Yes | | 41 | 260 | 424 | 1022 |
| Utah | Yes | | 34 | 41 | 64 | 149 |
| Virginia | Yes | | 116 | 104 | 150 | 260 |
| Washington | Yes | | 102 | 100 | 142 | 273 |
| Wisconsin | No | 4 | -- | -- | -- | -- |
| Wyoming | Yes | | 0 | 9 | 27 | 77 |

Notes: States are listed according to main sample inclusion. Exclusion reasons are: 1-Unclear treatment date and status, 2 - Treated before the study timeframe, 3 - Does not report monthly data to NIBRS or UCR, and 4 - Passage of a concurrent concealed carry law. Number of SRS and NIBRS agencies are agencies that are in the analytic sample. Total crime reporting agencies are agencies that do not report 0 total crimes in a month to the SRS during the study time period. Total reporting agencies exclude police agencies serving a population listed as 0, which means the agency may be a statewide law enforcement agency, campus or university police or other specialized agency functions without a set population.

**Appendix Table A.2:** Summary Statistics, UCR SRS Alternative Sample

| | Panel (1/1/2013 - 2/1/2020) | | | | Stacked Panel (12 months Pre/Post Treatment) | | | |
|---|---|---|---|---|---|---|---|---|
| | Control | | Treatment | | Control | | Treatment | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Violent Index Crime | 20.3 | 153.7 | 7.7 | 53 | 49.6 | 196.4 | 36.7 | 134.5 |
| Aggravated Assault | 12.6 | 92.4 | 5.1 | 35.2 | 30.5 | 115.3 | 24.6 | 91.8 |
| Robbery | 5.6 | 52.6 | 1.5 | 13.8 | 13.5 | 68.4 | 7.3 | 32.7 |
| Simple Assault | 45.3 | 215.5 | 20 | 78.5 | 111.6 | 343.5 | 84.6 | 172.2 |
| Property Index Crime | 128.2 | 631.4 | 55.4 | 259.6 | 302.7 | 862.3 | 227.6 | 525.7 |
| Burglary | 24.9 | 117.9 | 11.5 | 55.4 | 56.2 | 163 | 42.2 | 105.1 |
| Motor Vehicle Theft | 12.2 | 76 | 4.9 | 34.1 | 33.3 | 127.3 | 23.1 | 76.7 |
| Theft | 90.5 | 453.1 | 38.7 | 172.5 | 211.4 | 587 | 160.9 | 347.5 |
| Clearance All Crime | 57.4 | 261.2 | 25.1 | 102.2 | 128.7 | 320 | 105.7 | 198.5 |
| Clearance Property Index Crime | 23.8 | 106.1 | 11.1 | 47.4 | 49.5 | 100.4 | 45 | 81.7 |
| Clearance Violent Index Crime | 9.4 | 77 | 3.5 | 21.4 | 21.3 | 72.3 | 15.8 | 49.2 |
| Clearance Agg. Assault with a Gun | 1.1 | 7.2 | 0.5 | 4.7 | 2.9 | 11.4 | 2.8 | 12 |
| Clearance Agg. Assault with a Gun | 6.9 | 54.7 | 2.7 | 15.8 | 15.6 | 50.1 | 12 | 37.4 |
| Unique Observations | 276435 | 276435 | 64042 | 64042 | 528472 | 528472 | 3101 | 3101 |
| Agencies | 7551 | 7551 | 1762 | 1762 | 2287 | 2287 | 257 | 257 |

Notes: The SRS sample includes 9,313 agencies that reported crime data consistently and accurately in each year with a sample of 2,544 in the stack that report at least one crime in the months preceding and following passage of a permitless carry law. Crime outcomes are reported at the agency-pooled month level.

**Appendix Table A.3:** Effects of Permitless Carry Law Enactment on Motor Vehicle Theft

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Motor Vehicle Theft | Motor Vehicle Theft | Motor Vehicle Theft | Motor Vehicle Theft | Motor Vehicle Theft | Motor Vehicle Theft | Motor Vehicle Theft |
| Permitless Carry Law | 0.0196<br>(0.0224) | 0.0236<br>(0.0232) | 0.0242<br>(0.0240) | 0.0272<br>(0.0252) | 0.0210<br>(0.0253) | 0.0175<br>(0.0295) | 0.0170<br>(0.0324) |
| Lead 2 | | 0.0436<br>(0.0374) | 0.0442<br>(0.0383) | 0.0472<br>(0.0404) | 0.0411<br>(0.0407) | 0.0376<br>(0.0442) | 0.0372<br>(0.0476) |
| Lead 4 | | | 0.0067<br>(0.0403) | 0.0096<br>(0.0411) | 0.0035<br>(0.0436) | 1.58e-5<br>(0.0504) | -0.0004<br>(0.0539) |
| Lead 6 | | | | 0.0290<br>(0.0311) | 0.0228<br>(0.0321) | 0.0193<br>(0.0350) | 0.0189<br>(0.0381) |
| Lead 8 | | | | | -0.0531<br>(0.0393) | -0.0566<br>(0.0417) | -0.0570<br>(0.0430) |
| Lead 10 | | | | | | -0.0263<br>(0.0600) | -0.0267<br>(0.0636) |
| Lead 12 | | | | | | | -0.0026<br>(0.0309) |
| Stack by Agency FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stack by Year by Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 | 232,733 |

Notes: Permitless carry law coefficients are from Equations 2, 3. 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 12 months in the post period only. Standard errors are clustered at the state level.

**Appendix Table A.4:** Effects of Permitless Carry Law Enactment on Weapons Law Arrests

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Weapons Law Arrests | Weapons Law Arrests | Weapons Law Arrests | Weapons Law Arrests | Weapons Law Arrests | Weapons Law Arrests | Weapons Law Arrests |
| Permitless Carry Law | -0.2198* (0.0859) | -0.2460** (0.0912) | -0.2552** (0.0918) | -0.2497** (0.0948) | -0.2365** (0.0901) | -0.2275** (0.0824) | -0.2437** (0.0855) |
| Lead 2 | | -0.3464* (0.137) | -0.3555** (0.1375) | -0.3501* (0.1399) | -0.3371* (0.1382) | -0.3282* (0.133) | -0.3443* (0.1369) |
| Lead 4 | | | -0.0968 (0.0744) | -0.0914 (0.0773) | -0.0784 (0.0785) | -0.0696 (0.0767) | -0.0857 (0.0779) |
| Lead 6 | | | | 0.0519 (0.0718) | 0.0648 (0.0725) | 0.0737 (0.0688) | 0.0576 (0.0731) |
| Lead 8 | | | | | 0.1123 (0.0765) | 0.1212 (0.0811) | 0.1051 (0.0865) |
| Lead 10 | | | | | | 0.0669 (0.0982) | 0.0508 (0.1017) |
| Lead 12 | | | | | | | -0.1133 (0.0902) |
| Stack by Agency FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stack by Year by Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 218,359 | 218,359 | 218,359 | 218,359 | 218,359 | 218,359 | 218,359 |

Notes: Permitless carry law coefficients are from Equations 2, 3. 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 12 months in the post period only. Standard errors are clustered at the state level.

**Appendix Table A.5:** Effects of Permitless Carry on Crime, Poisson Estimator with Controls

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Violent Index | Agg. Assault | Robbery | Simple Assault | Property Index | Burglary | MVT | Larceny-theft |
| Panel A: NIBRS | | | | | | | | |
| Permitless Carry Law | 0.0436*** (0.0124) | 0.0567*** (0.0208) | 0.0060 (0.0222) | -0.0115 (0.0157) | 0.0425 (0.0486) | 0.0345 (0.0406) | 0.0066 (0.0546) | 0.0745 (0.0627) |
| Stack by Agency | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stack by Year by Month | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 170317 | 170317 | 170317 | 170317 | 170317 | 170317 | 170317 | 170317 |
| Agencies | 1612 | 1612 | 1612 | 1612 | 1612 | 1612 | 1612 | 1612 |
| Panel B: UCR SRS | | | | | | | | |
| Permitless Carry Law | 0.0490*** (0.0120) | 0.0570*** (0.0165) | 0.0330* (0.0174) | -0.0143 (0.0179) | 0.0499*** (0.0164) | 0.0240* (0.0126) | 0.1189*** (0.0321) | 0.0457*** (0.0168) |
| Stack by Agency | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stack by Year by Month | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 311214 | 311214 | 311214 | 311214 | 311214 | 311214 | 311214 | 311214 |
| Agencies | 2544 | 2544 | 2544 | 2544 | 2544 | 2544 | 2544 | 2544 |

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 4 which estimate the percent change in crime outcomes following passage of a permitless carry law, omitting the months immediately pre- and post- law enactment, or months -2 to 2. Crime outcomes are counts of crimes in each agency in the NIBRS. 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 12 months in the post period. Standard errors are clustered at the state level.

**Appendix Table A.6:** Effects of Permitless Carry on Gun Crime, Poisson Estimator with Controls

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Agg. Assault (Gun) | Agg. Assault (No Gun) | Robbery (Gun) | Robbery (No Gun) |
| Panel A: NIBRS | | | | |
| Permitless Carry Law | 0.1332*** (0.0292) | 0.0192 (0.0215) | 0.0821** (0.0359) | 0.0181 (0.0640) |
| Stack by Agency | Yes | Yes | Yes | Yes |
| Stack by Year by Month | Yes | Yes | Yes | Yes |
| Observations | 292123 | 292123 | 292123 | 292123 |
| Agencies | 1612 | 1612 | 1612 | 1612 |
| Panel B: UCR | | | | |
| Permitless Carry Law | 0.1175*** (0.0252) | 0.0142 (0.0253) | 0.1038*** (0.0213) | -0.0341 (0.0397) |
| Stack by Agency | Yes | Yes | Yes | Yes |
| Stack by Year by Month | Yes | Yes | Yes | Yes |
| Observations | 306109 | 306109 | 306109 | 306109 |
| Agencies | 2544 | 2544 | 2544 | 2544 |

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 4 which estimate the percent change in crime outcomes following passage of a permitless carry law, omitting the months immediately pre- and post- law enactment, or months -2 to 2. Crime outcomes are counts of crimes in each agency in the NIBRS. 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 12 months in the post period. Standard errors are clustered at the state level. Controls include demographic and lagged crime (incarceration and police officers per capita) controls merged to each agency according to each agency's county.

**Appendix Table A.7:** Effects of Permitless Carry on Crime, Log-Linear Model with Controls

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Violent Index | Agg. Assault | Robbery | Simple Assault | Property Index | Burglary | MVT | Larceny-theft |
| Panel A: NIBRS | | | | | | | | |
| Permitless Carry Law | 0.0453*** (0.0141) | 0.0708* (0.0368) | 0.0230 (0.0541) | -0.0494*** (0.0175) | -0.0437 (0.0320) | -0.0853* (0.0424) | -0.0046 (0.0531) | 0.0276 (0.0639) |
| Stack by Agency | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stack by Year by Month | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 162590 | 162590 | 162590 | 162590 | 162590 | 162590 | 162590 | 162590 |
| Agencies | 1612 | 1612 | 1612 | 1612 | 1612 | 1612 | 1612 | 1612 |
| Panel B: UCR | | | | | | | | |
| Permitless Carry Law | 0.0437** (0.0181) | 0.0232 (0.0148) | 0.0190 (0.0409) | -0.0412* (0.021) | 0.0221 (0.0153) | 0.0138 (0.0161) | 0.0514* (0.0289) | 0.0164 (0.0162) |
| Stack by Agency | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Stack by Year by Month | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 305288 | 305288 | 305288 | 305288 | 305288 | 305288 | 305288 | 305288 |
| Agencies | 2544 | 2544 | 2544 | 2544 | 2544 | 2544 | 2544 | 2544 |

Notes: Permitless carry law effect sizes are OLS estimates from log outcomes with the inclusion of log jurisdiction population, which estimate the percent change in crime outcomes following passage of a permitless carry law. Crime outcomes are log counts of crimes in each agency in the NIBRS. 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 12 months in the post period. Standard errors are clustered at the state level.

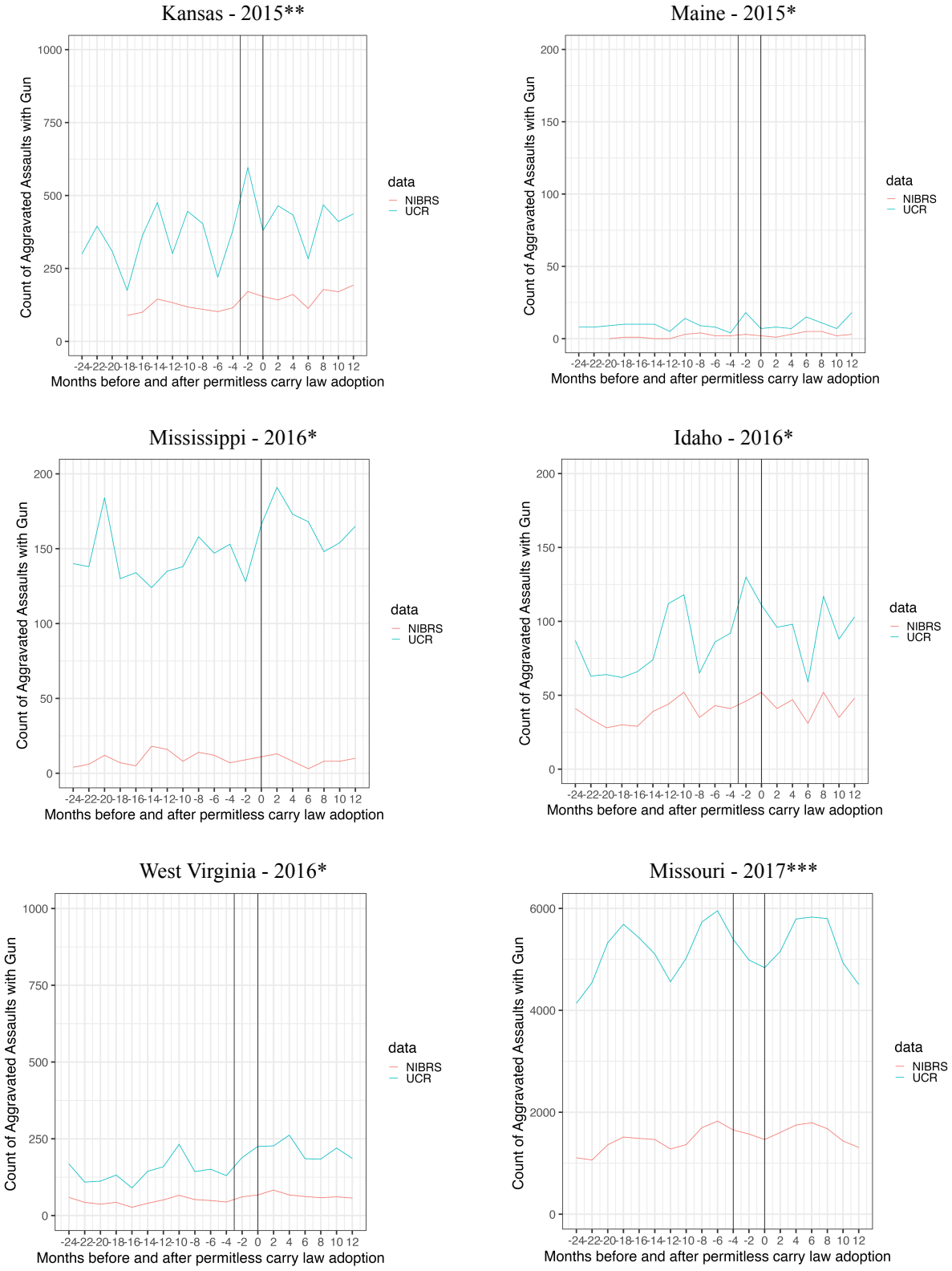
**Appendix Table A.8:** Effects of Permitless Carry on Gun Crime, Log-Linear Model with Controls

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Agg. Assault (Gun) | Agg. Assault (No Gun) | Robbery (Gun) | Robbery (No Gun) |
| Panel A: NIBRS | | | | |
| Permitless Carry Law | 0.6227*<br>(0.3059) | 0.1229<br>(0.1052) | 0.2326**<br>(0.1020) | 0.0069<br>(0.0494) |
| Stack by Agency | Yes | Yes | Yes | Yes |
| Stack by Year by Month | Yes | Yes | Yes | Yes |
| Observations | 293279 | 293279 | 293279 | 293279 |
| Agencies | 1612 | 1612 | 1612 | 1612 |
| Panel B: UCR | | | | |
| Permitless Carry Law | 1.8747*<br>(1.0682) | -0.0289<br>(0.2561) | 0.6158***<br>(0.1985) | -0.0935<br>(0.0571) |
| Stack by Agency | Yes | Yes | Yes | Yes |
| Stack by Year by Month | Yes | Yes | Yes | Yes |
| Observations | 311218 | 311218 | 311218 | 311218 |
| Agencies | 2544 | 2544 | 2544 | 2544 |

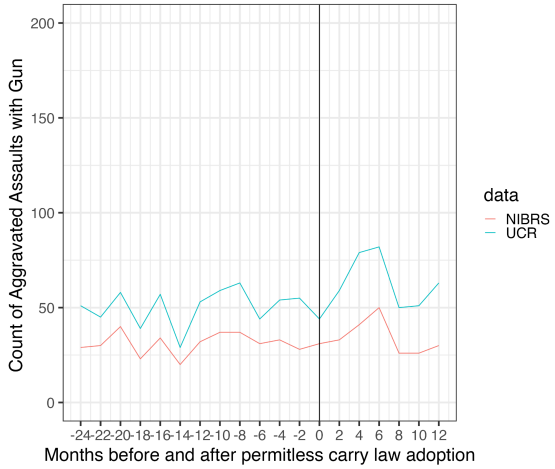
Notes: Permitless carry law effect sizes are OLS estimates from log outcomes with the inclusion of log jurisdiction population, which estimate the percent change in crime outcomes following passage of a permitless carry law. Crime outcomes are log counts of crimes in each agency in the NIBRS. The level 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 12 months in the post period. Standard errors are clustered at the state level.

## Figures

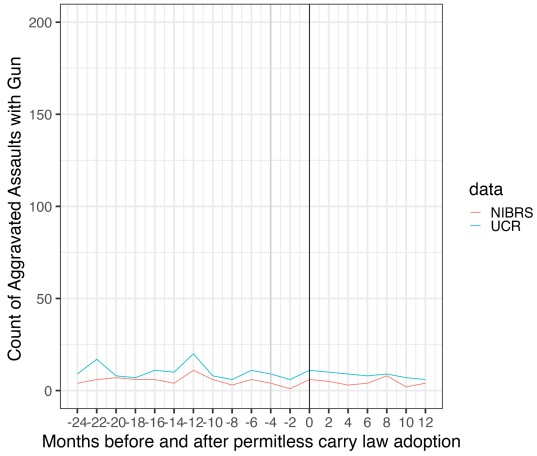
**Figure A.1:** Aggravated Gun Assaults in Treatment States, NIBRS and SRS



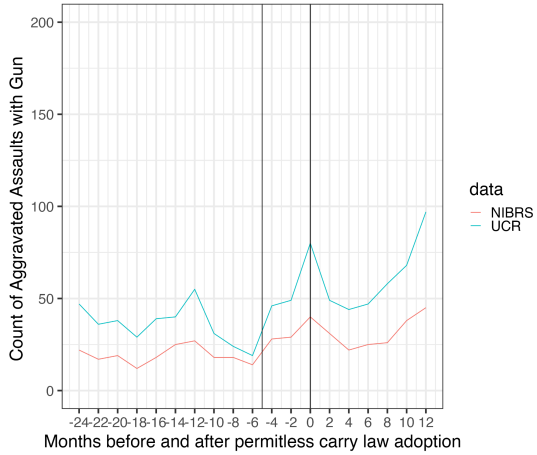
New Hampshire - 2017*



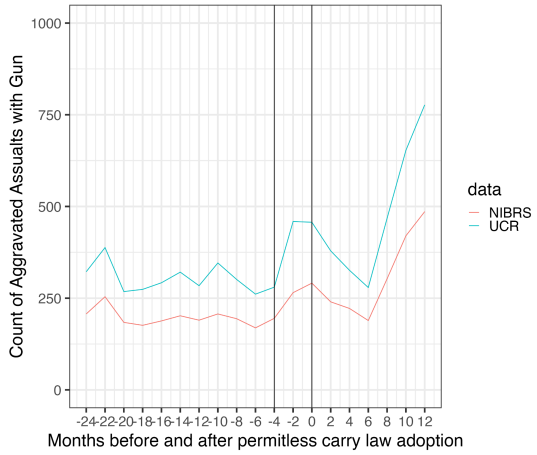

North Dakota - 2017*



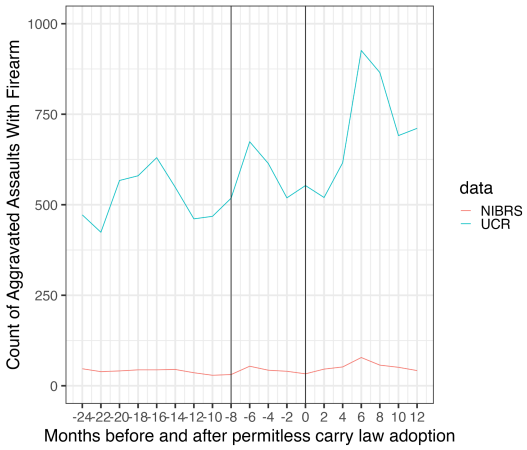

South Dakota - 2019*




Kentucky - 2019**



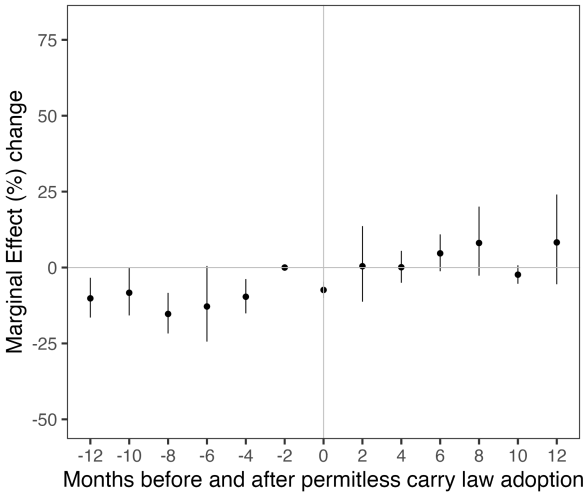

Oklahoma - 2019**



Notes: Counts of pooled 2-month aggravated gun assaults for both NIBRS and SRS samples are plotted. Horizontal reference lines mark the passage of the permitless carry law and the second centered at t=0 corresponds to the implementation date.

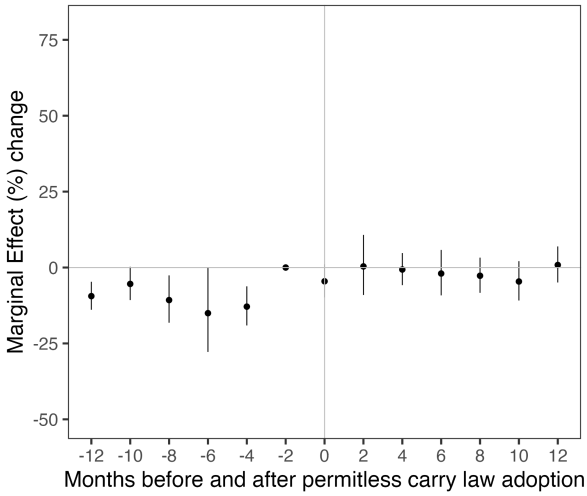
**Appendix Figure A.2:** Effect of Permitless Concealed Carry Laws on Crime, NIBRS Agencies Serving Populations Over 90,000

(a) Violent Index Crime



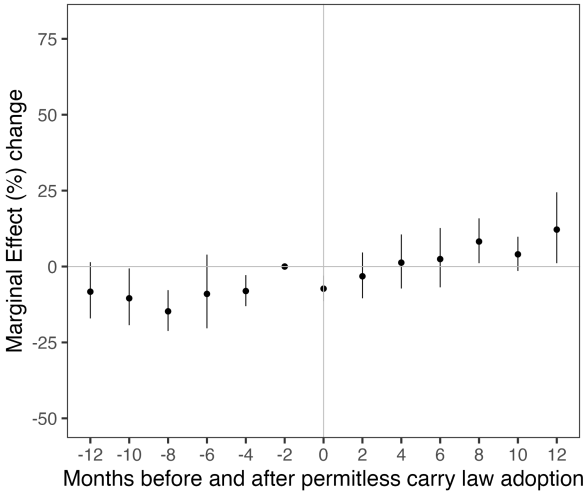

Avg. Effect, months 0 to 12 (%) = 15.295
p< 0.0016
Agency N = 142
Dep. V. Mean = 276.249

(b) Property Index Crime



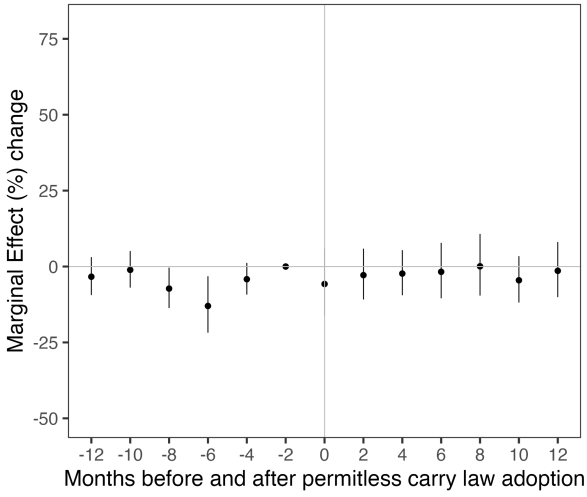

Avg. Effect, months 0 to 12 (%) = 9.346
p< 0
Agency N = 142
Dep. V. Mean = 715.525

(c) Aggravated Assault




Avg. Effect, months 0 to 12 (%) = 15.152
p< 0.0011
Agency N = 142
Dep. V. Mean = 117.316

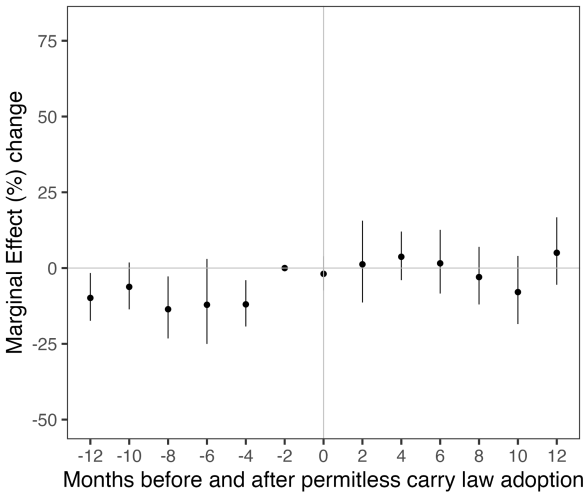
(d) Simple Assault



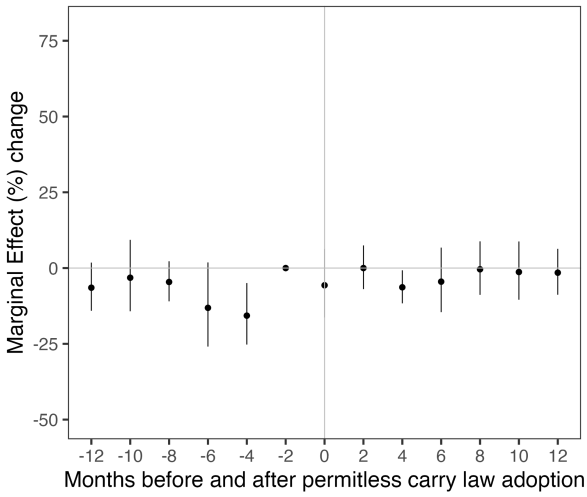

Avg. Effect, months 0 to 12 (%) = 3.713
p< 0.0662
Agency N = 142
Dep. V. Mean = 398.661

(e) Burglary

(f) Motor Vehicle Theft

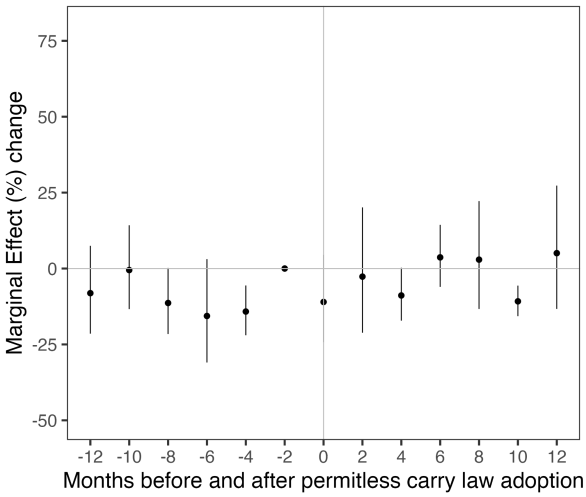



Avg. Effect, months 0 to 12 (%) = 10.56
p< 0
Agency N = 142
Dep. V. Mean = 393.446



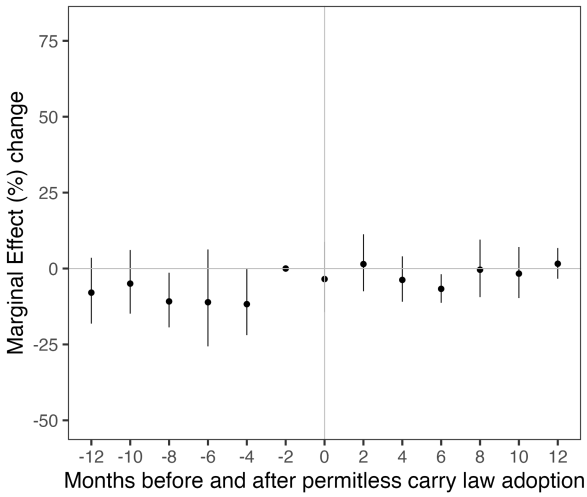

Avg. Effect, months 0 to 12 (%) = 6.601
p< 0.0198
Agency N = 142
Dep. V. Mean = 213.335

(g) Robbery




Avg. Effect, months 0 to 12 (%) = 8.737
p< 0.048
Agency N = 142
Dep. V. Mean = 78.659

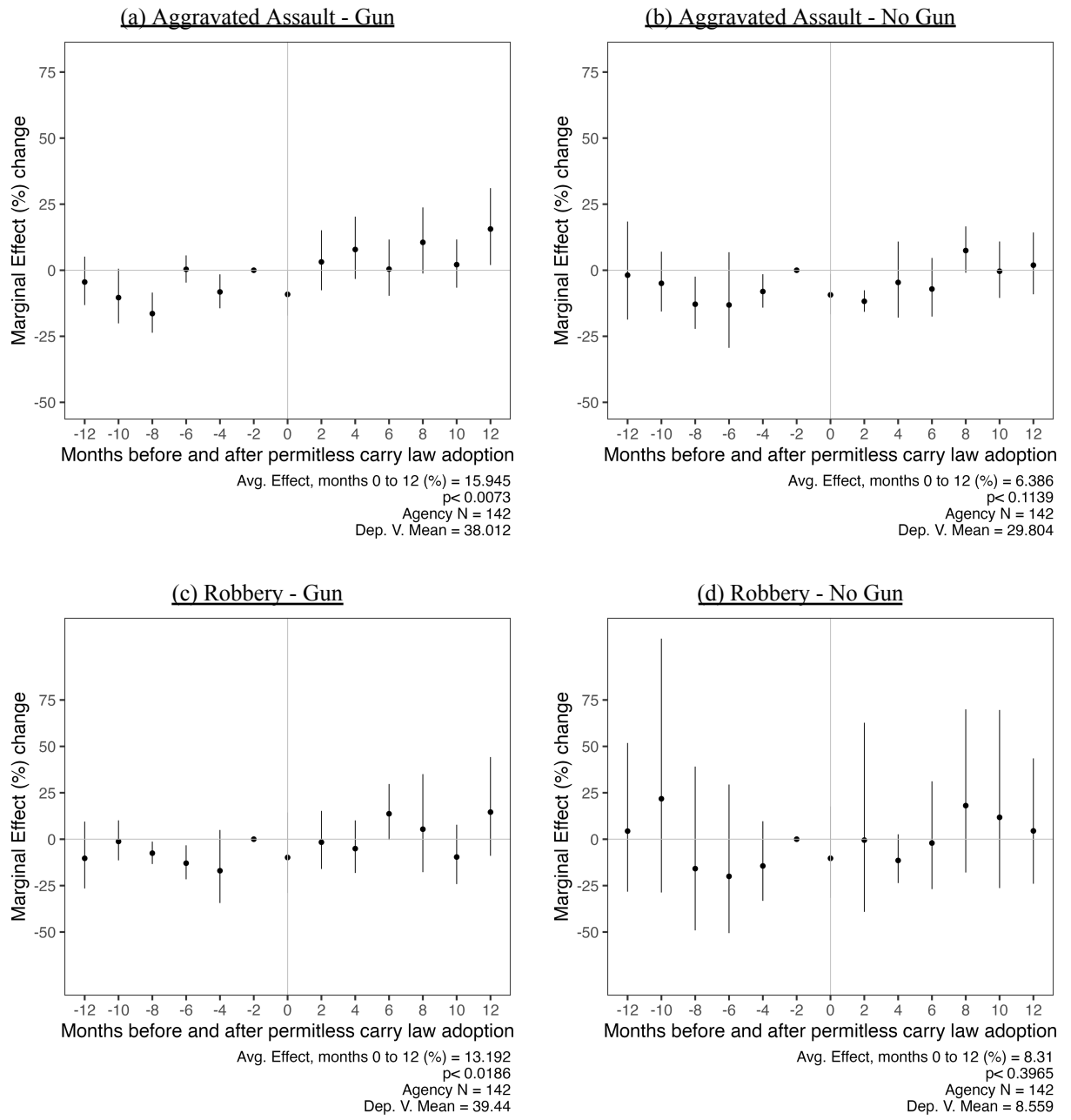
(h) Larceny-theft




Avg. Effect, months 0 to 12 (%) = 7.364
p< 0.0834
Agency N = 142
Dep. V. Mean = 425.955

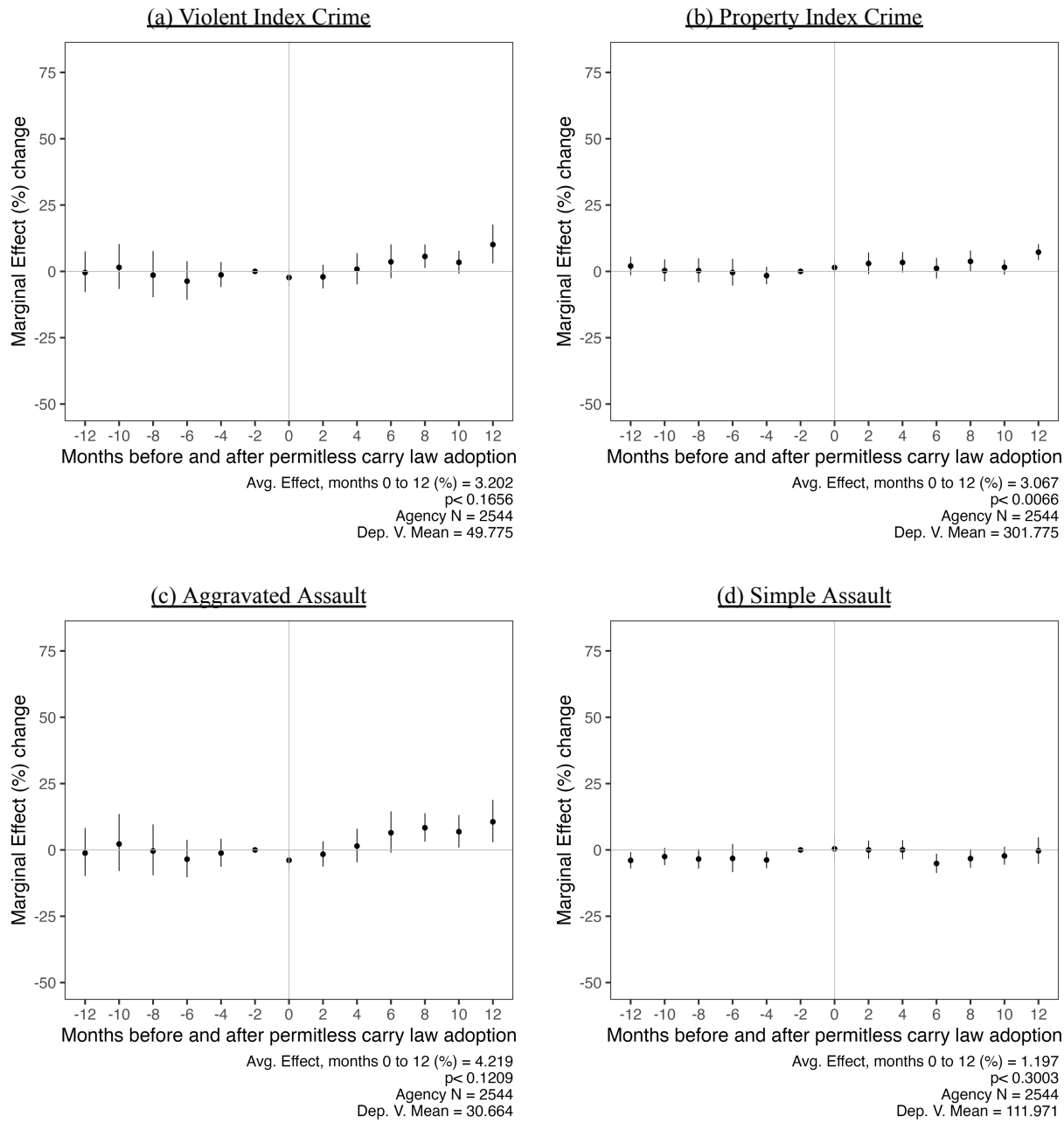
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked UCR NIBRS panel restricted to agencies serving populations greater than or equal to 90,000 population. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

**Appendix Figure A.3:** Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS Agencies Serving Populations Over 90,000

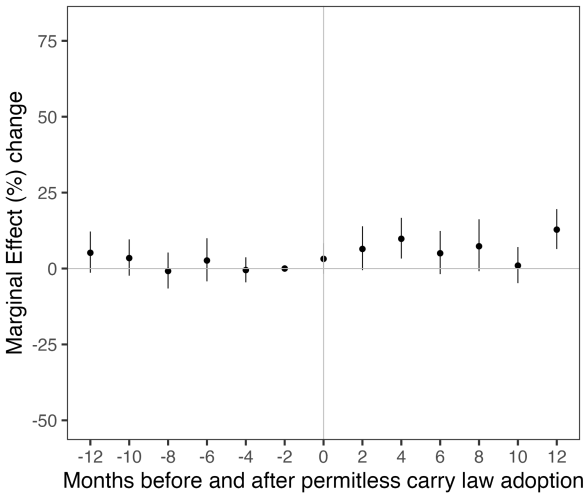



Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked UCR NIBRS panel restricted to agencies serving populations greater than or equal to 90,000 population. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

**Appendix Figure A.4:** Effect of Permitless Concealed Carry Laws on Crime, Alternative UCR Summary Reporting System (SRS) Sample

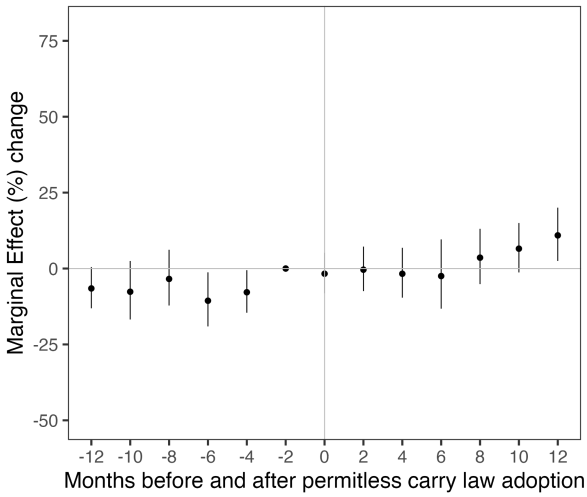


(e) Burglary



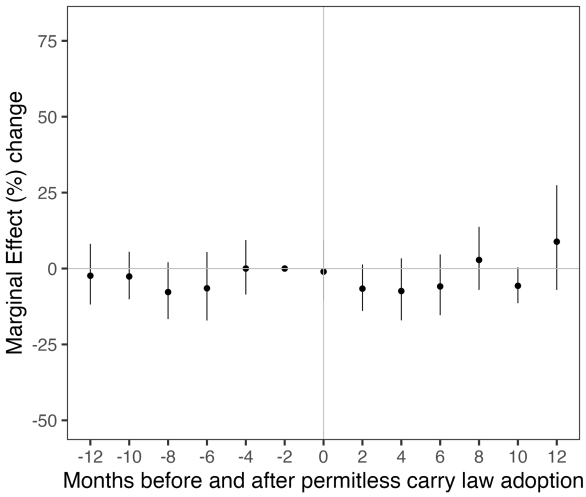

Avg. Effect, months 0 to 12 (%) = 5.205
p< 0.0024
Agency N = 2544
Dep. V. Mean = 55.363

(f) Motor Vehicle Theft



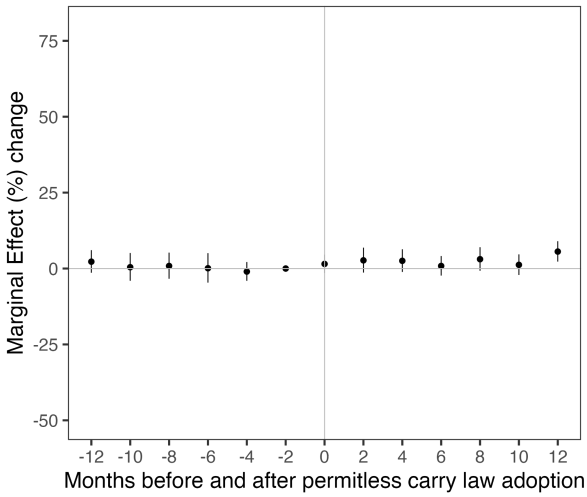

Avg. Effect, months 0 to 12 (%) = 7.728
p< 0.0038
Agency N = 2544
Dep. V. Mean = 33.3

(g) Robbery




Avg. Effect, months 0 to 12 (%) = -0.098
p< 0.9751
Agency N = 2544
Dep. V. Mean = 13.529

(h) Larceny-theft

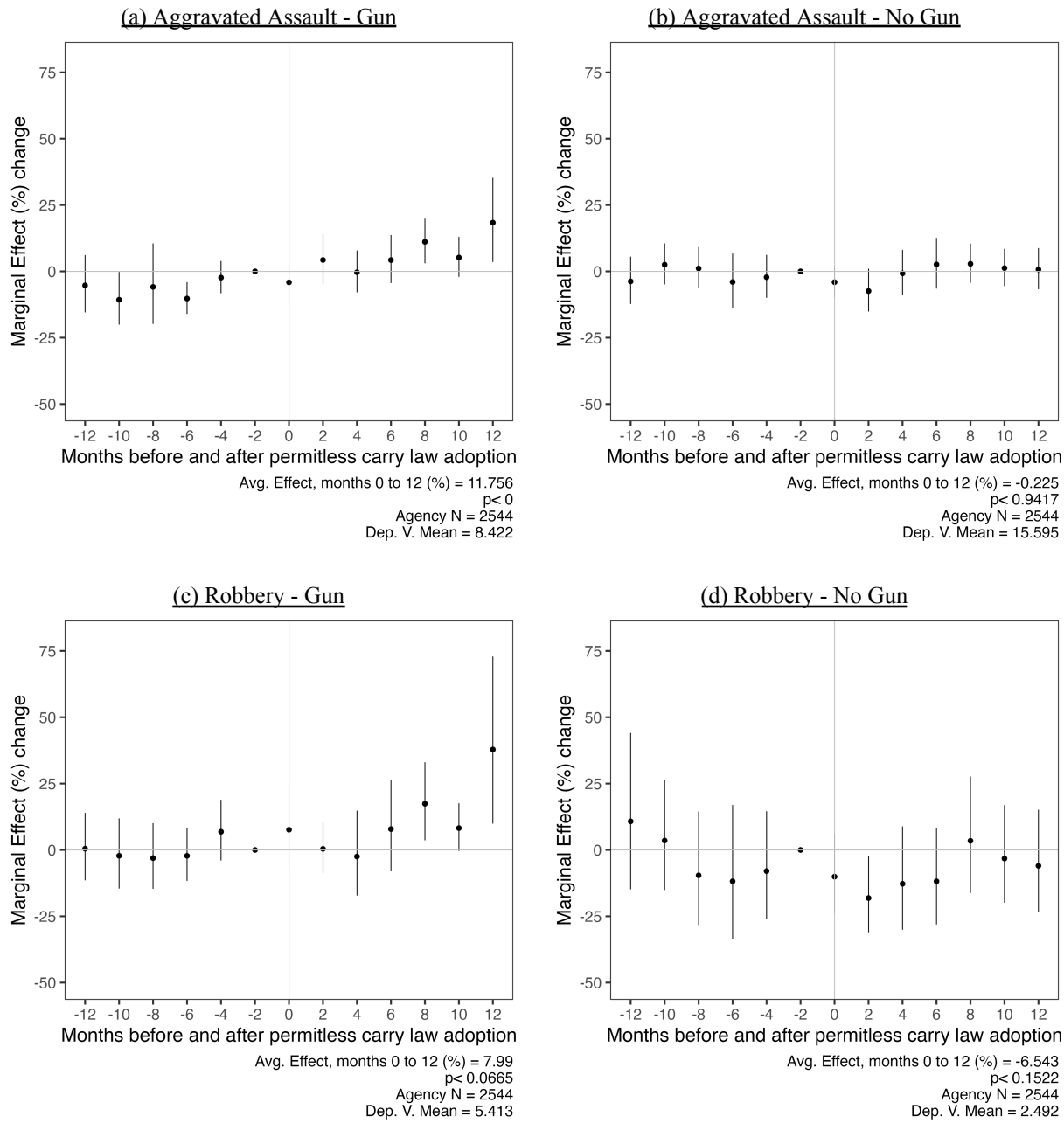



Avg. Effect, months 0 to 12 (%) = 2.149
p< 0.0869
Agency N = 2544
Dep. V. Mean = 211.264

Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked UCR SRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as 100 ×

(exp(β ) − 1) or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

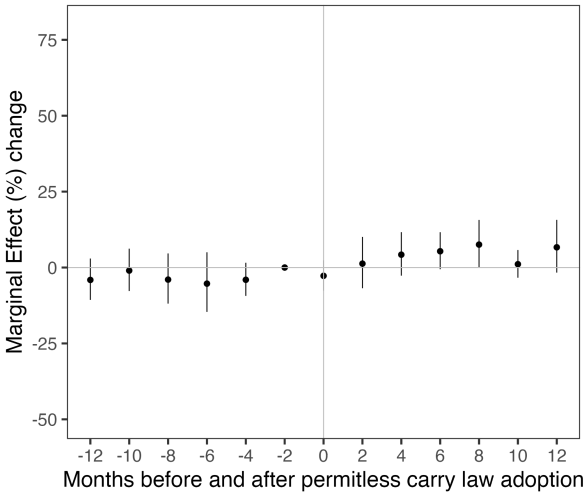
**Appendix Figure A.5:** Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, UCR Summary Reporting System (SRS) Sample




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked UCR SRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

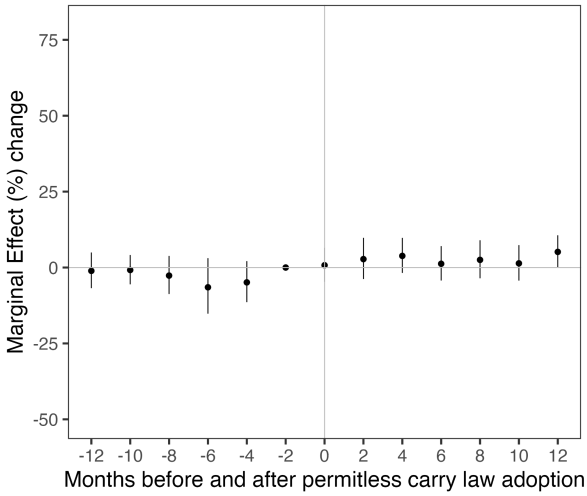
**Appendix Figure A.6:** Effect of Permitless Concealed Carry Laws on Crime, NIBRS Alternative Comparison Group, Future Adopters

(a) Violent Index Crime



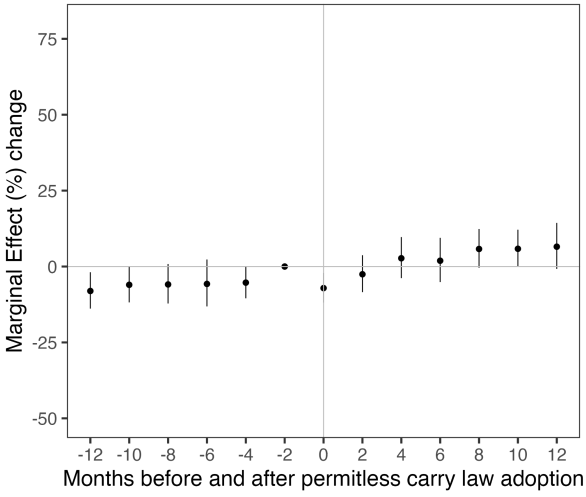

Avg. Effect, months 0 to 12 (%) = 8.148
p< 0.0023
Agency N = 890
Dep. V. Mean = 25.238

(b) Property Index Crime



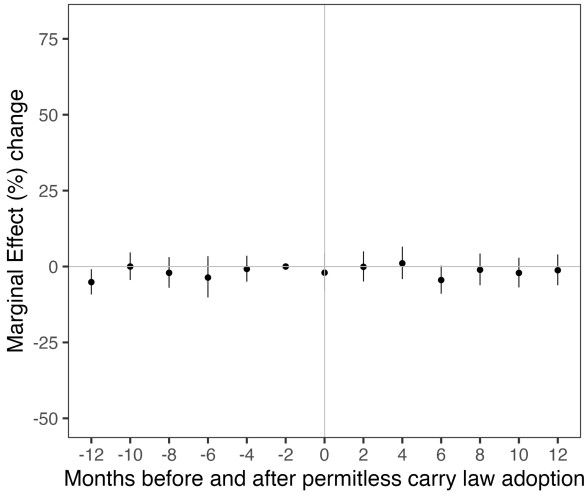

Avg. Effect, months 0 to 12 (%) = 5.694
p< 0.0001
Agency N = 890
Dep. V. Mean = 114.048

(c) Aggravated Assault




Avg. Effect, months 0 to 12 (%) = 10.115
p< 0
Agency N = 890
Dep. V. Mean = 6.429

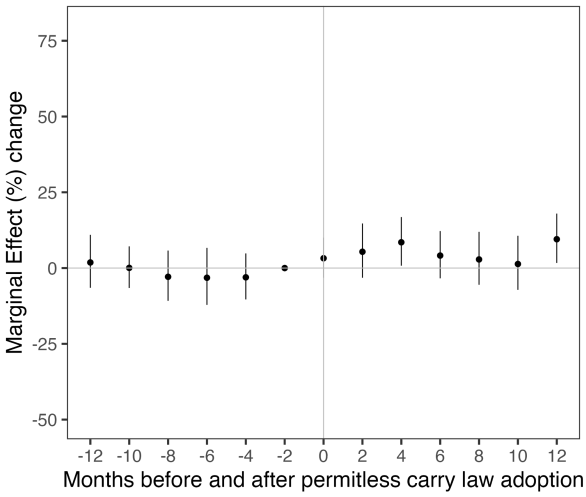
(d) Simple Assault



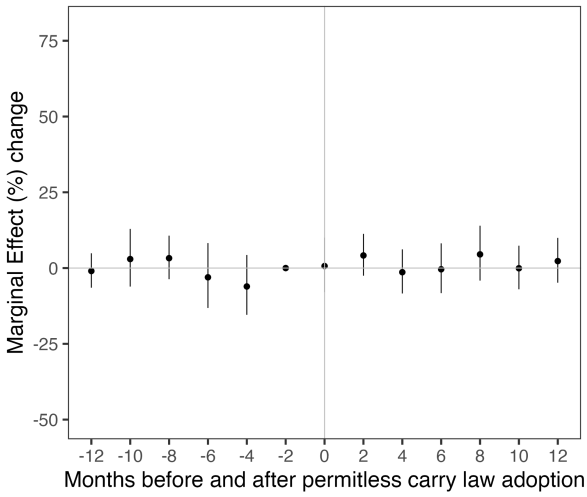

Avg. Effect, months 0 to 12 (%) = 0.819
p< 0.5519
Agency N = 890
Dep. V. Mean = 184.81

(e) Burglary

(f) Motor Vehicle Theft

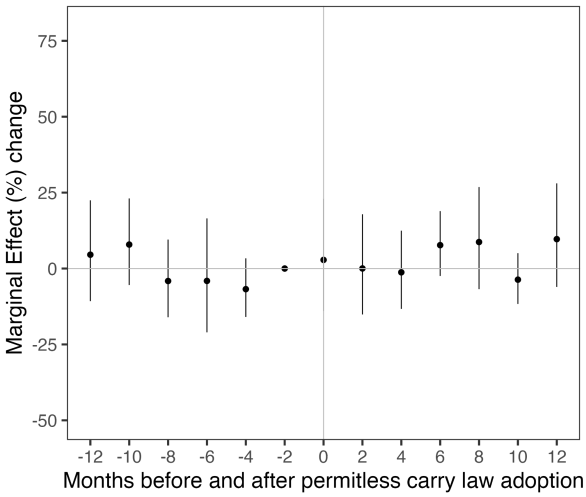



Avg. Effect, months 0 to 12 (%) = 6.046
p< 0.0002
Agency N = 890
Dep. V. Mean = 92.381



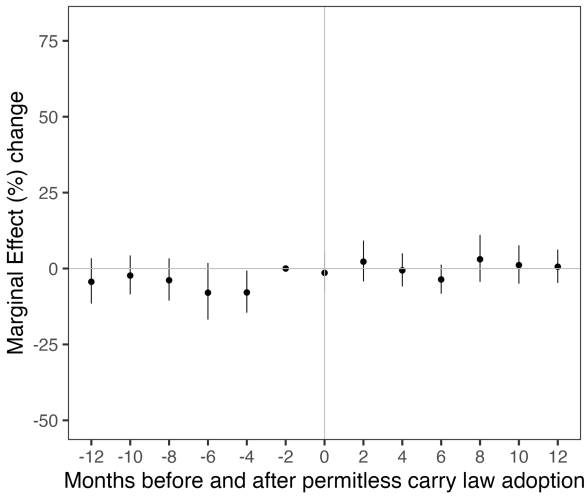

Avg. Effect, months 0 to 12 (%) = 2.38
p< 0.2425
Agency N = 890
Dep. V. Mean = 16.143

(g) Robbery




Avg. Effect, months 0 to 12 (%) = 3.826
p< 0.2514
Agency N = 890
Dep. V. Mean = 9.381

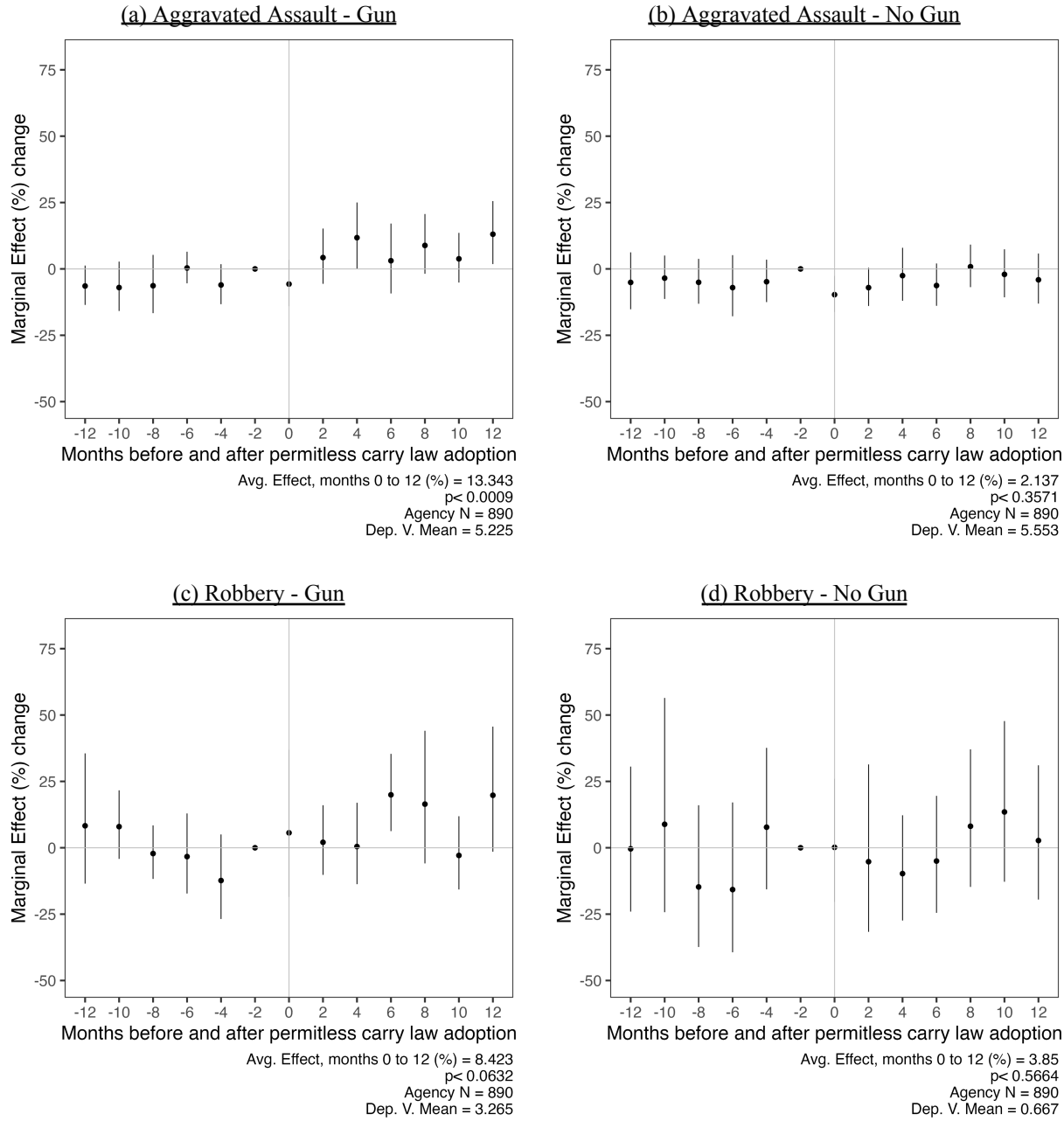
(h) Larceny-theft




Avg. Effect, months 0 to 12 (%) = 5.273
p< 0.0535
Agency N = 890
Dep. V. Mean = 43.19

Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked UCR NIBRS panel. The comparison group consists of future adoption states (AL, IA, LA, MT, NE, OH, SC, TN, TX, UT , NC). Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

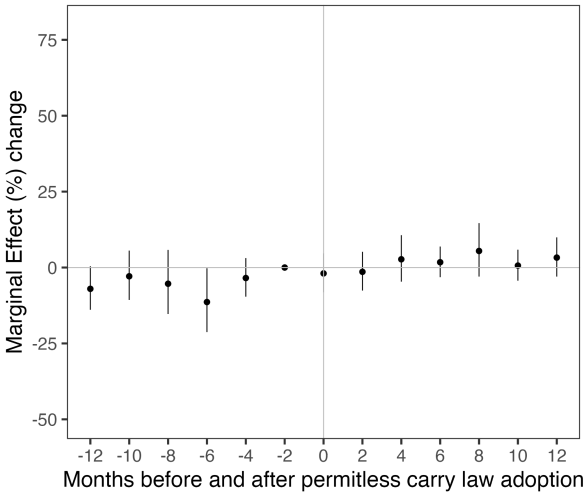
**Appendix Figure A.7:** Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS Future Adopters Alternative Comparison Group




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked UCR NIBRS panel. The comparison group consists of future adoption states (AL, IA, LA, MT, NE, OH, SC, TN, TX, UT , NC). Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

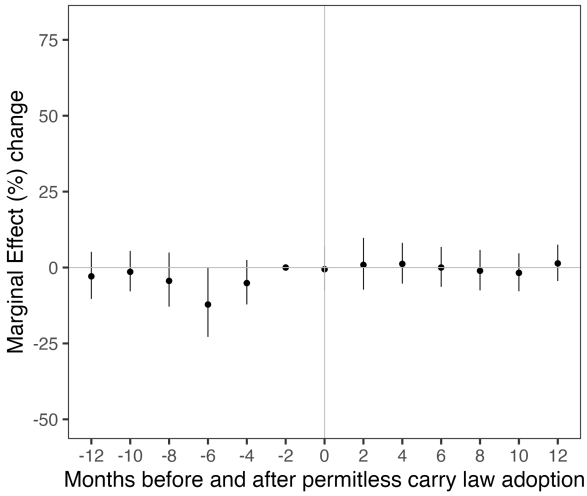
**Appendix Figure A.8:** Effect of Permitless Concealed Carry Laws on Crime, Treated Prior to 2019

(a) Violent Index Crime



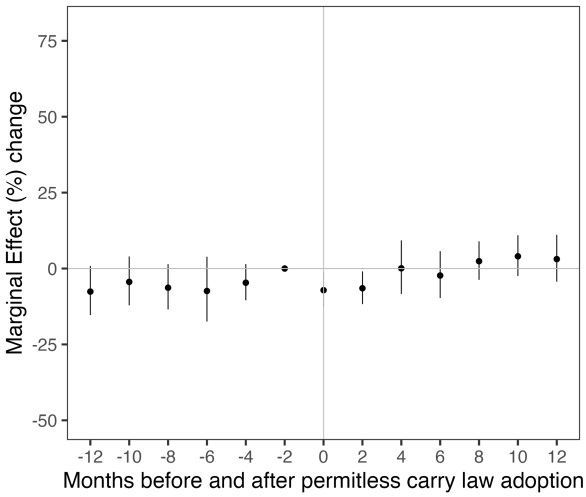

Avg. Effect, months 0 to 20 (%) = 7.797
p< 0.0202
Agency N = 1563
Dep. V. Mean = 25.238

(b) Property Index Crime



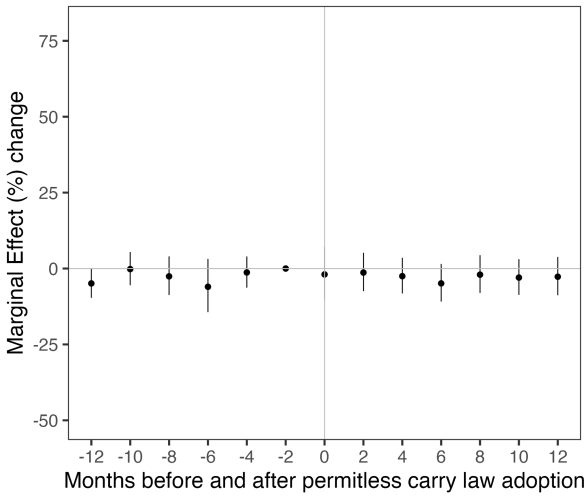

Avg. Effect, months 0 to 20 (%) = 5.042
p< 0.0347
Agency N = 1563
Dep. V. Mean = 114.048

(c) Aggravated Assault




Avg. Effect, months 0 to 20 (%) = 6.562
p< 0.0211
Agency N = 1563
Dep. V. Mean = 6.429

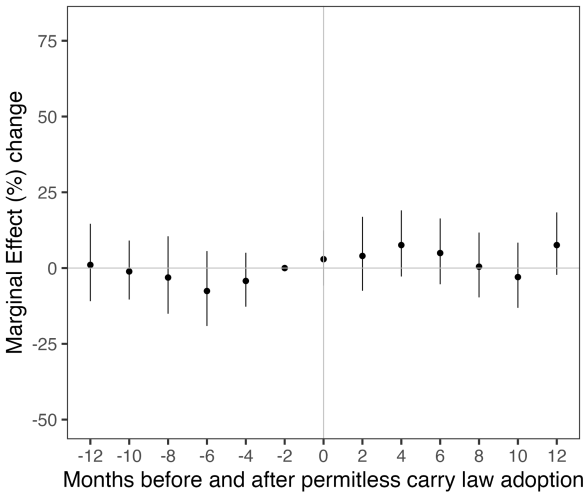
(d) Simple Assault

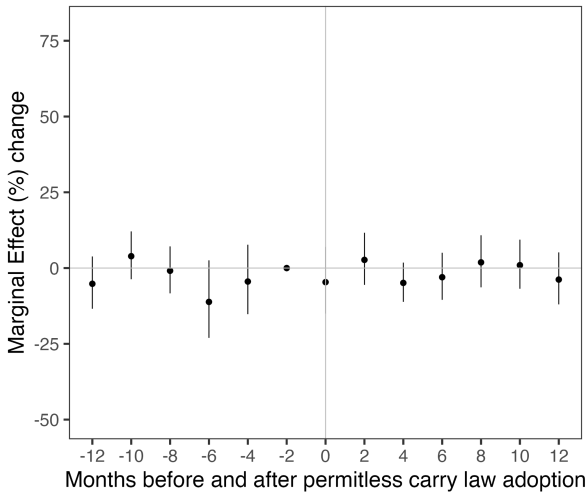



Avg. Effect, months 0 to 20 (%) = -0.155
p< 0.9279
Agency N = 1563
Dep. V. Mean = 184.81

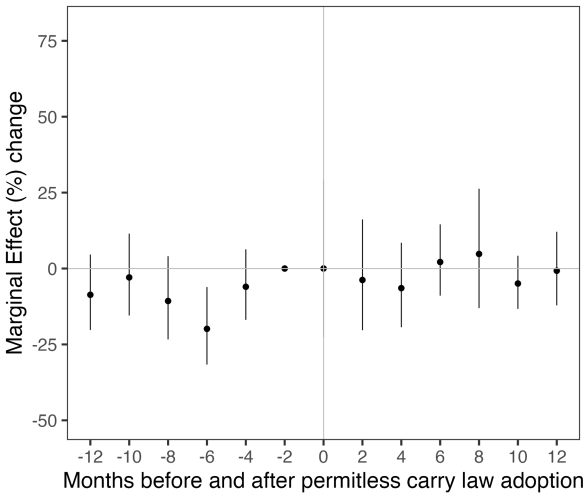
(e) Burglary

(f) Motor Vehicle Theft

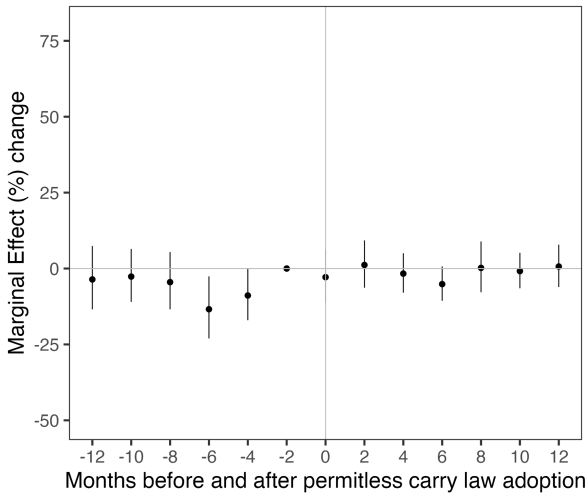



(g) Robbery

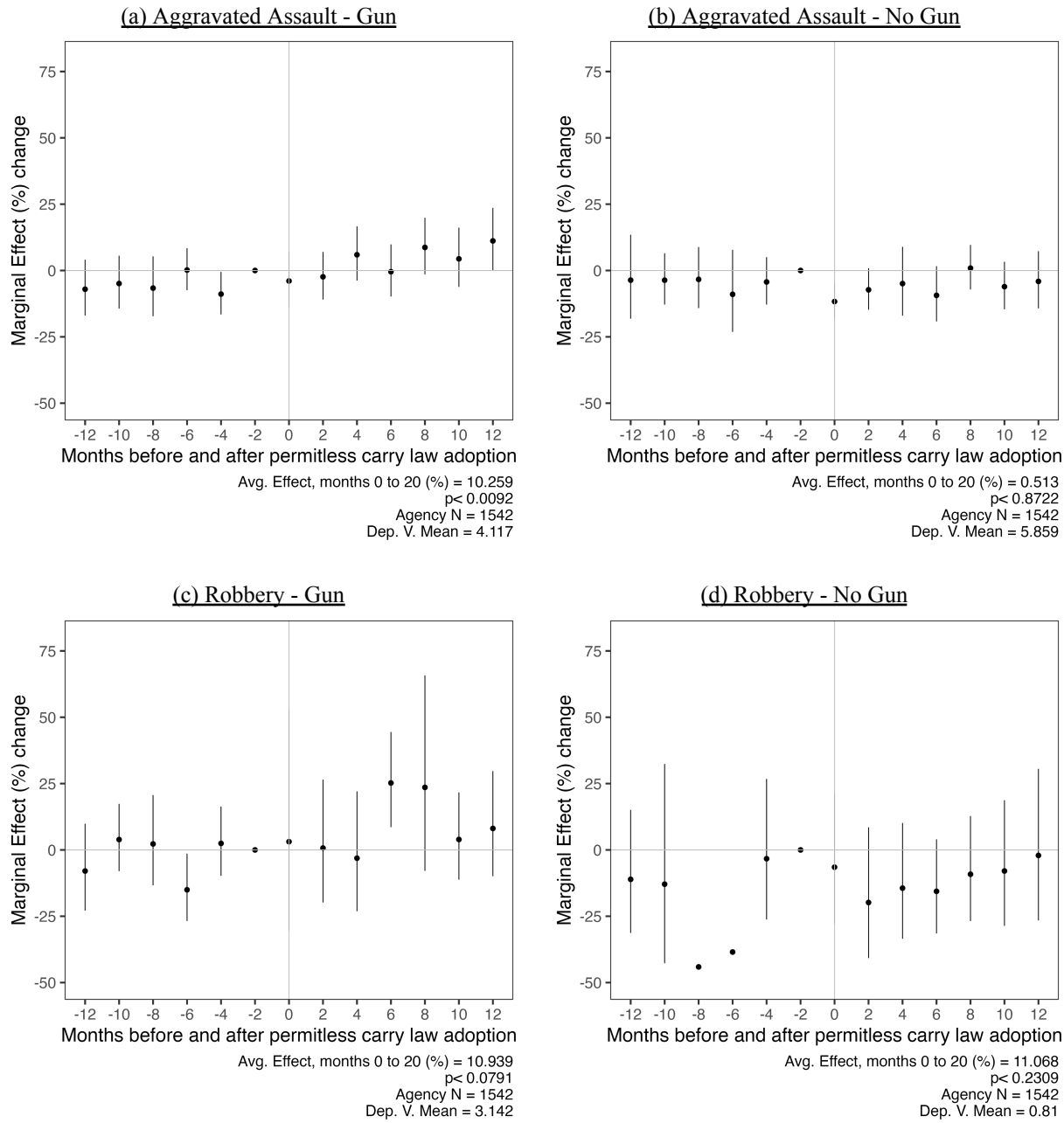



(h) Larceny-theft




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked UCR NIBRS panel. The panel is restricted to states treated prior to 2019 (omitting stacks for OK, KY, SD and including KS, ME, NH, MO, MS, ND, WV, ID). Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

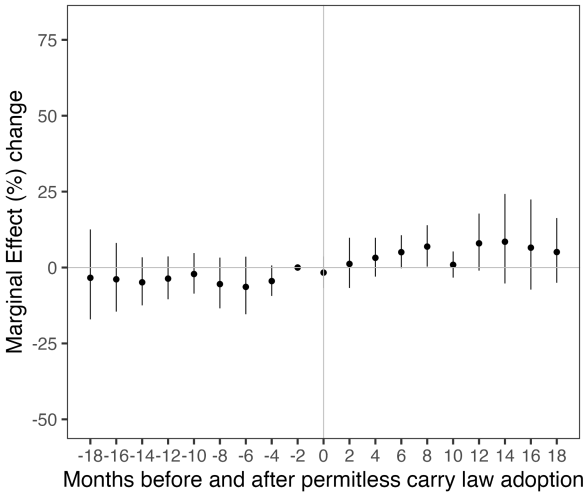
**Appendix Figure A.9:** Effect of Permitless Concealed Carry Laws on Gun Crime, Treated Prior to 2019




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked UCR NIBRS panel. The panel is restricted to states treated prior to 2019 (omitting stacks for OK, KY, SD and including KS, ME, NH, MO, MS, ND, WV, ID). Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

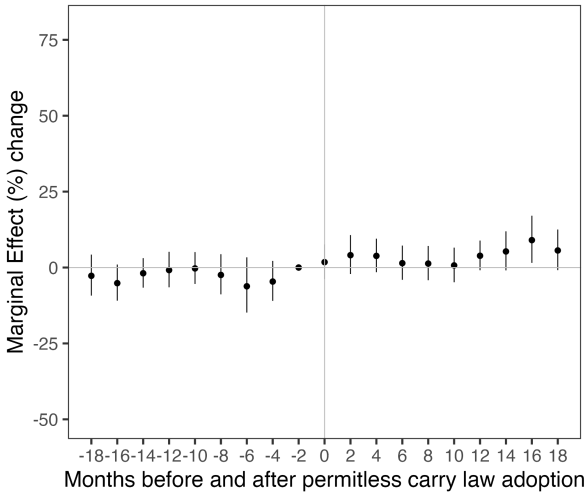
**Appendix Figure A.10:** Effect of Permitless Concealed Carry Laws on Crime, NIBRS Sample, Extended Period

(a) Violent Index Crime



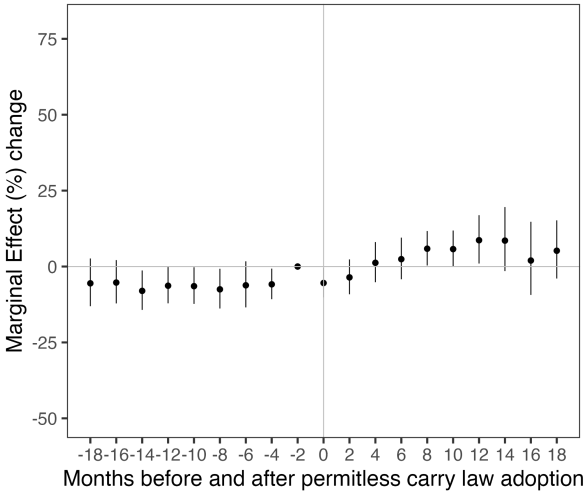

Avg. Effect, months 0 to 18 (%) = 9.469
p< 0.0038
Agency N = 1659
Dep. V. Mean = 32.261

(b) Property Index Crime



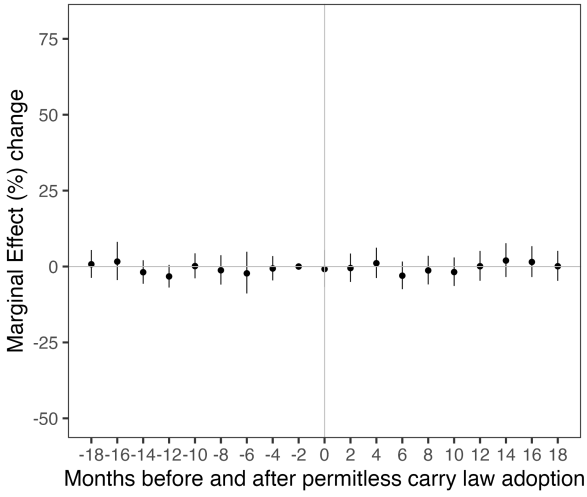

Avg. Effect, months 0 to 18 (%) = 6.657
p< 0.0001
Agency N = 1659
Dep. V. Mean = 81.672

(c) Aggravated Assault




Avg. Effect, months 0 to 18 (%) = 10.917
p< 0.0017
Agency N = 1659
Dep. V. Mean = 16.213

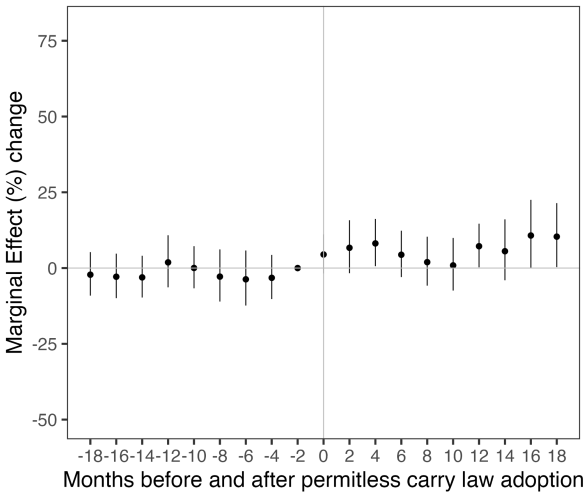
(d) Simple Assault



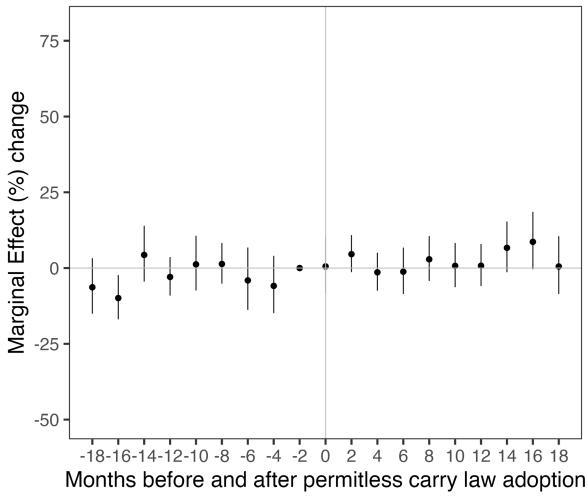

Avg. Effect, months 0 to 18 (%) = 0.652
p< 0.6064
Agency N = 1659
Dep. V. Mean = 70.746

(e) Burglary

(f) Motor Vehicle Theft

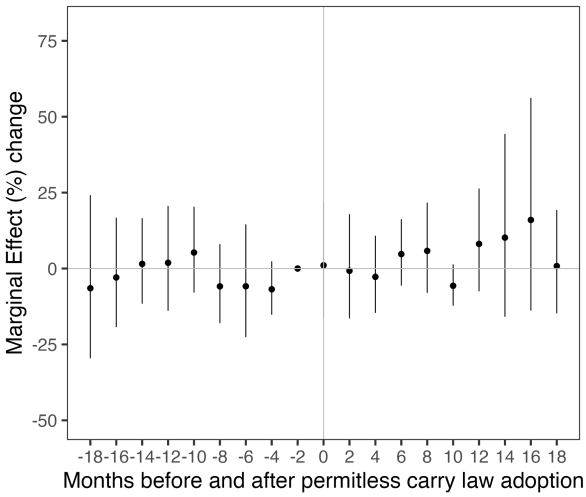



Avg. Effect, months 0 to 18 (%) = 7.62
p< 0
Agency N = 1659
Dep. V. Mean = 45.378



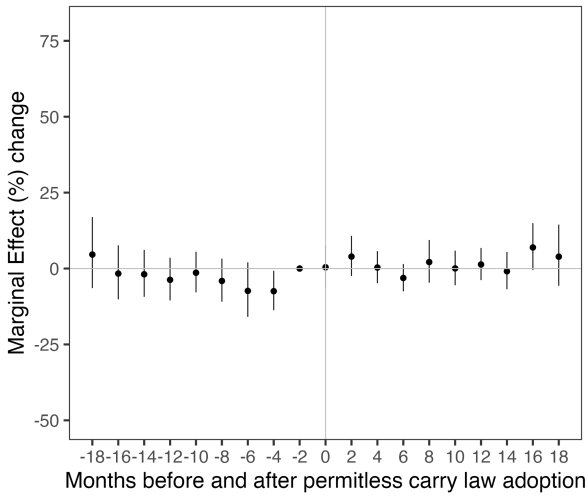

Avg. Effect, months 0 to 18 (%) = 5.075
p< 0.026
Agency N = 1659
Dep. V. Mean = 18.537

(g) Robbery




Avg. Effect, months 0 to 18 (%) = 6.265
p< 0.0733
Agency N = 1659
Dep. V. Mean = 6.826

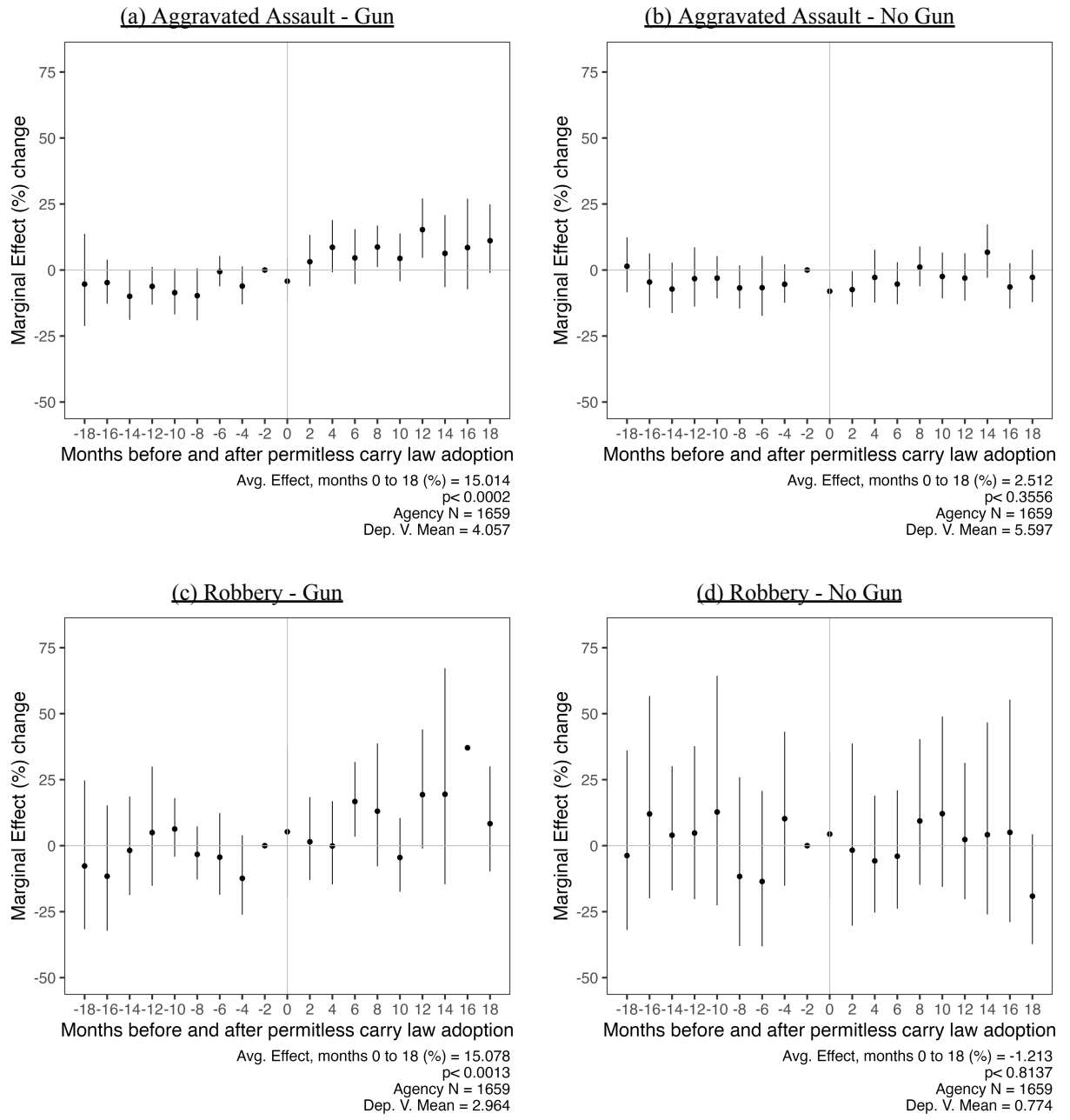
(h) Larceny-theft




Avg. Effect, months 0 to 18 (%) = 4.404
p< 0.1155
Agency N = 1659
Dep. V. Mean = 54.37

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 12 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.

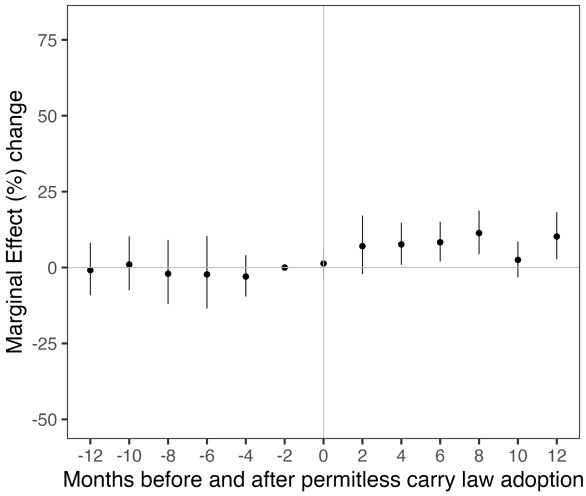
**Appendix Figure A.11:** Effect of Permitless Concealed Carry Laws on Gun Crime, NIBRS Sample, Extended Period




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

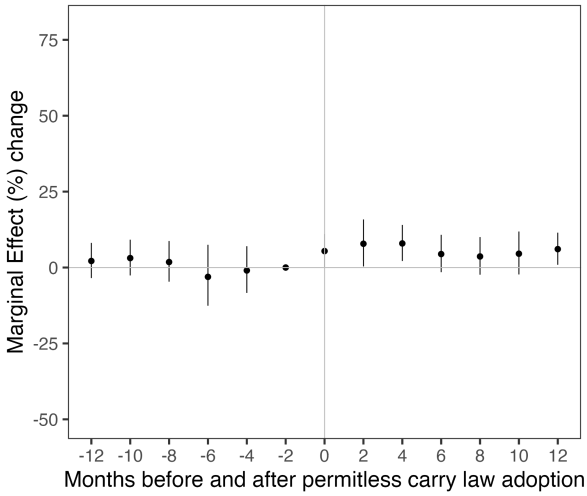
**Appendix Figure A.12:** Effect of Permitless Concealed Carry Laws on Crime, NIBRS Sample, Excluding Missouri

(a) Violent Index Crime



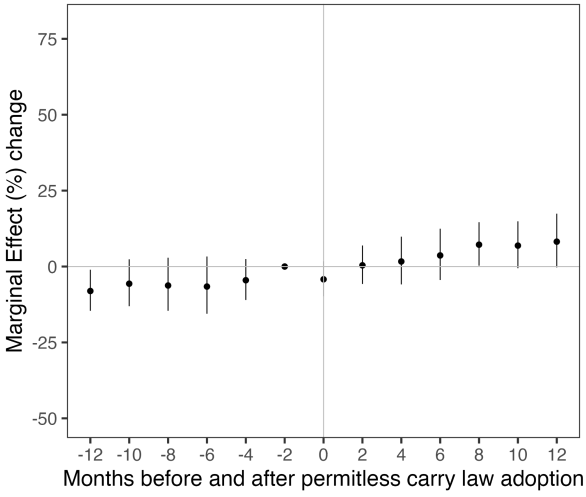

Avg. Effect, months 0 to 20 (%) = 9.227
p< 0.0079
Agency N = 1631
Dep. V. Mean = 25.238

(b) Property Index Crime



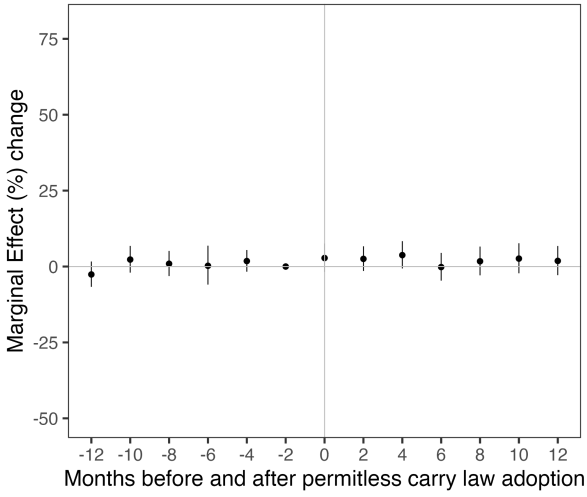

Avg. Effect, months 0 to 20 (%) = 4.266
p< 0.0019
Agency N = 1631
Dep. V. Mean = 114.048

(c) Aggravated Assault




Avg. Effect, months 0 to 20 (%) = 10.841
p< 0.0004
Agency N = 1631
Dep. V. Mean = 6.429

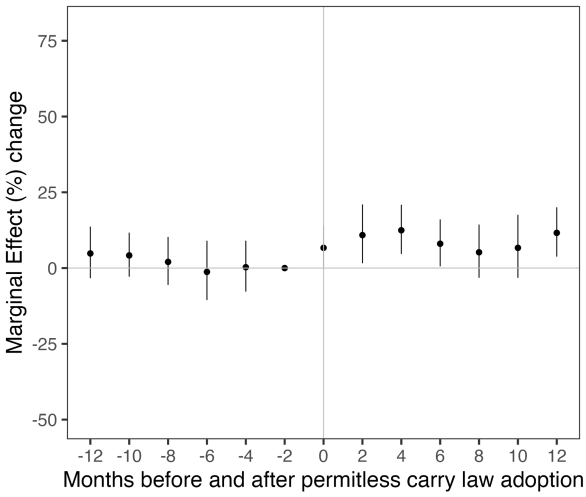
(d) Simple Assault



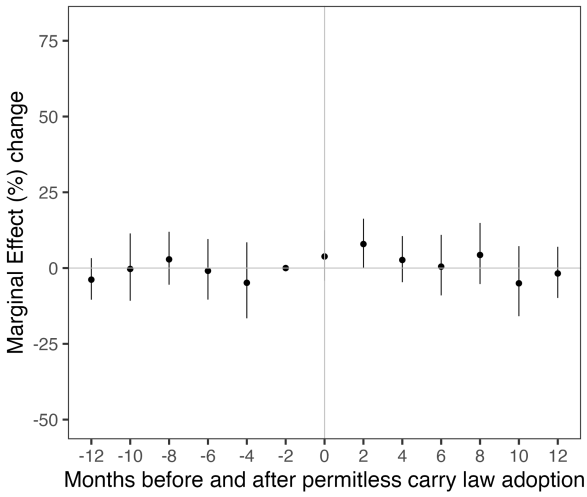

Avg. Effect, months 0 to 20 (%) = 0.774
p< 0.5758
Agency N = 1631
Dep. V. Mean = 184.81

(e) Burglary

(f) Motor Vehicle Theft

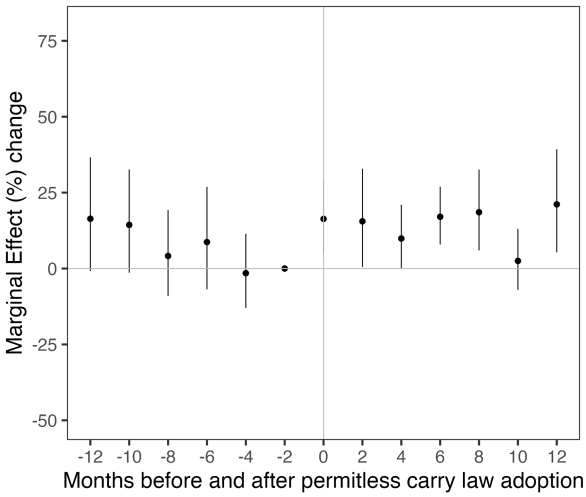



Avg. Effect, months 0 to 20 (%) = 6.076
p< 0.0003
Agency N = 1631
Dep. V. Mean = 92.381



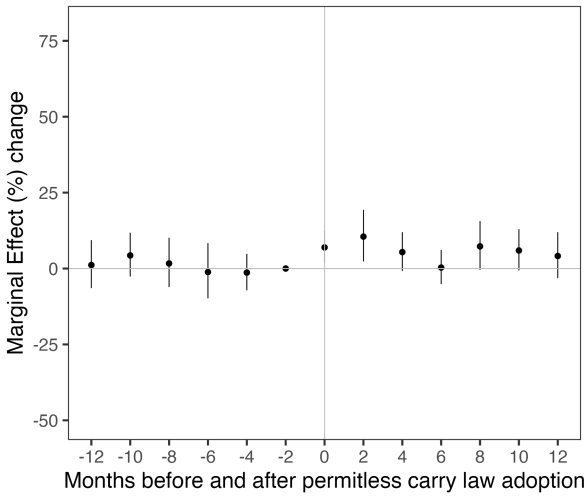

Avg. Effect, months 0 to 20 (%) = 2.433
p< 0.2644
Agency N = 1631
Dep. V. Mean = 16.143

(g) Robbery




Avg. Effect, months 0 to 20 (%) = 3.91
p< 0.3716
Agency N = 1631
Dep. V. Mean = 9.381

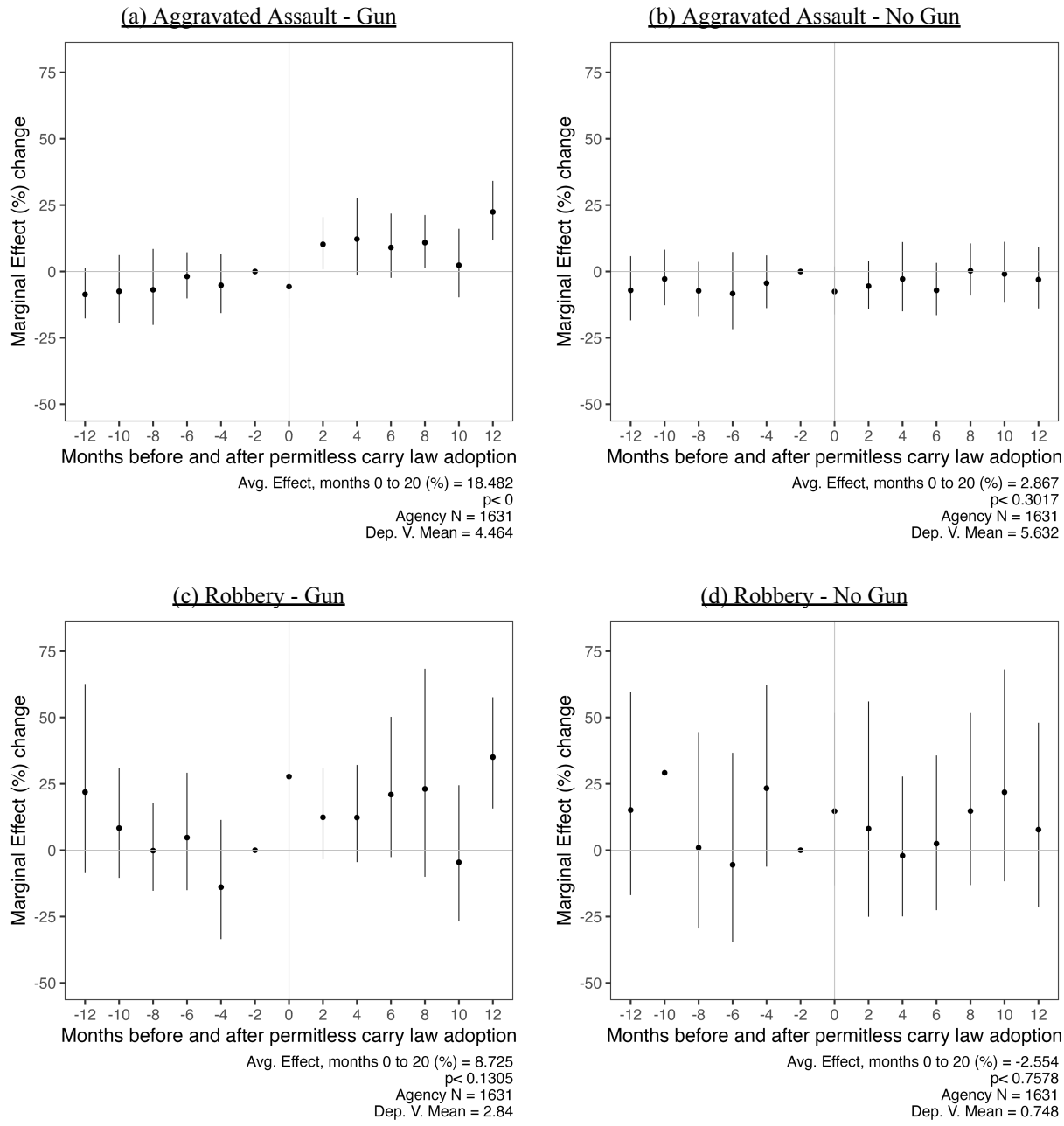
(h) Larceny-theft




Avg. Effect, months 0 to 20 (%) = 2.639
p< 0.2474
Agency N = 1631
Dep. V. Mean = 43.19

Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel, excluding the Missouri stack and treated state. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

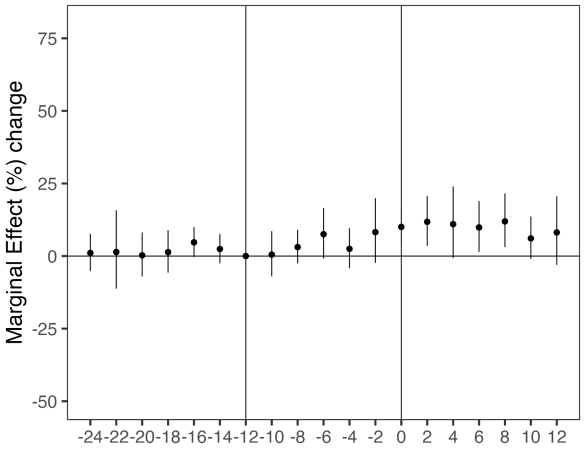
**Appendix Figure A.13:** Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS Sample, Excluding Missouri




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel, excluding the Missouri stack and treated state. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

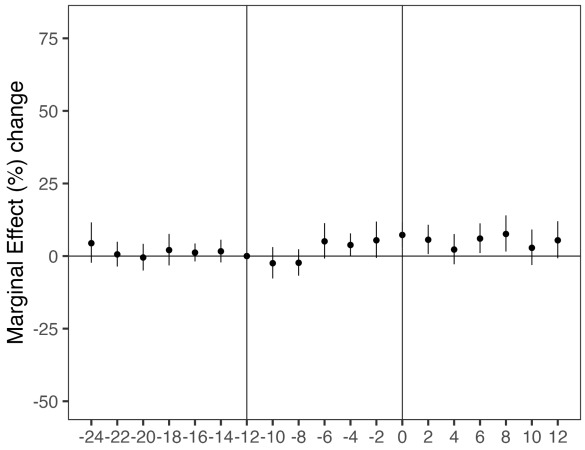
**Appendix Figure A.14:** Effect of Permitless Concealed Carry Laws on Crime, NIBRS, Relative to One-Year Prior to Treatment Date

(a) Violent Index Crime



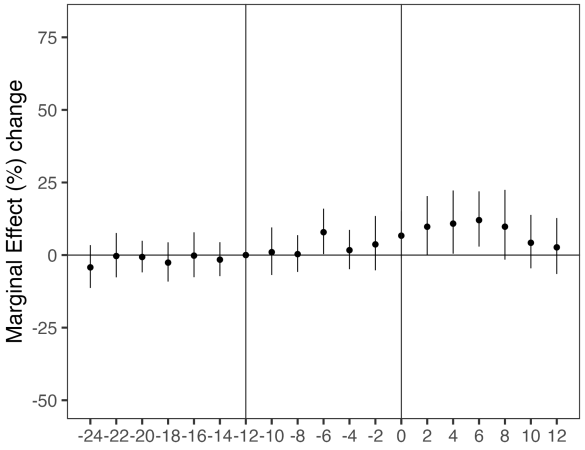

Avg. Effect (months 0 to 12) = 7.34
p< 0.0083
Avg. Effect (months -12 to -2) = 1.388
p< 0.4493
Dep. V. Mean = 16.35

(b) Property Index Crime



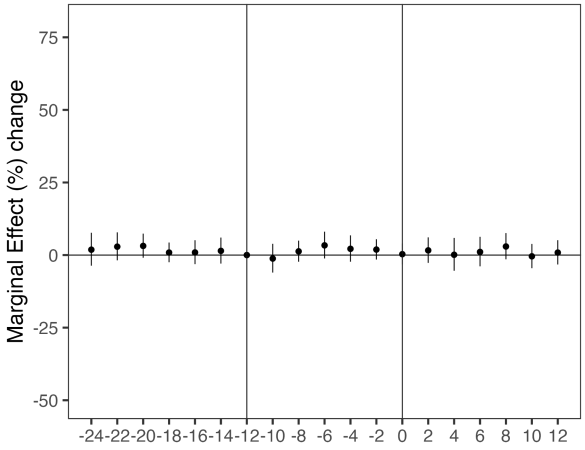

Avg. Effect (months 0 to 12) = 3.112
p< 0.0996
Avg. Effect (months -12 to -2) = 0.586
p< 0.9524
Dep. V. Mean = 51.51

(c) Aggravated Assault




Avg. Effect (months 0 to 12) = 9.801
p< 0.0005
Avg. Effect (months -12 to -2) = -0.329
p< 0.0114
Dep. V. Mean = 7.351

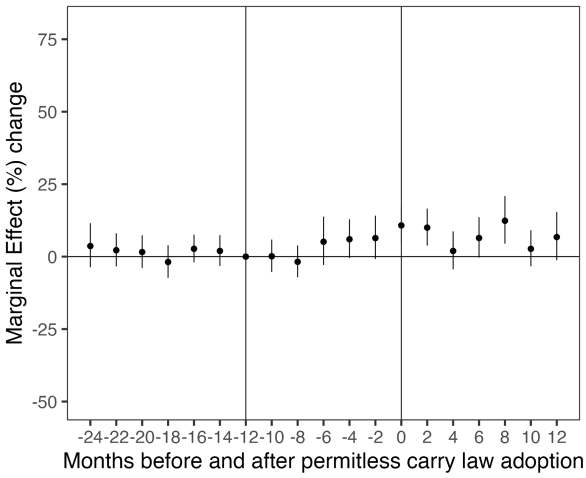
(d) Simple Assault



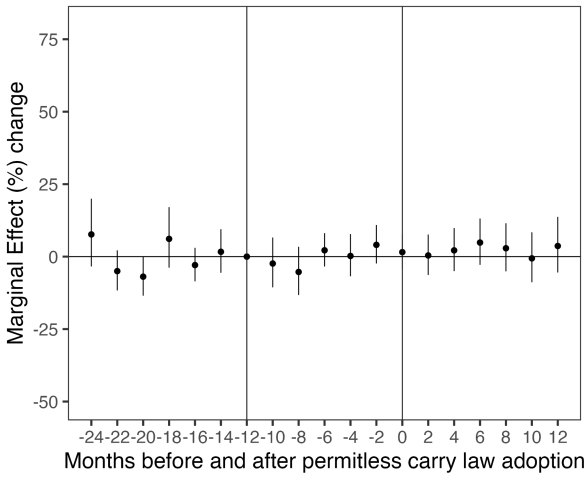

Avg. Effect (months 0 to 12) = -0.853
p< 0.6066
Avg. Effect (months -12 to -2) = 2.892
p< 0.8406
Dep. V. Mean = 35.46

(e) Burglary

(f) Motor Vehicle Theft

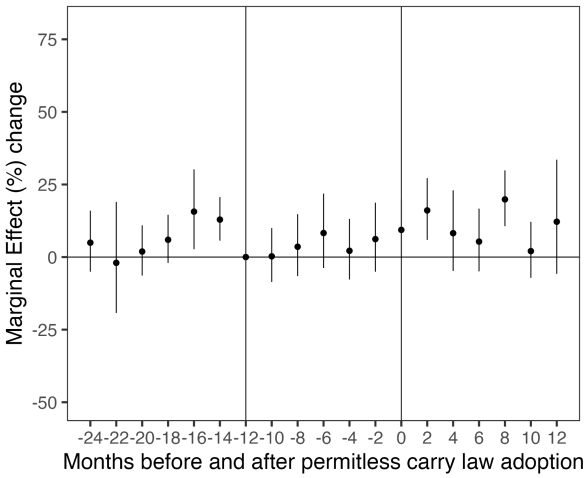



Avg. Effect (months 0 to 12) = 4.791
p< 0.0175
Avg. Effect (months -12 to -2) = 2.199
p< 0.6213
Dep. V. Mean = 32.082



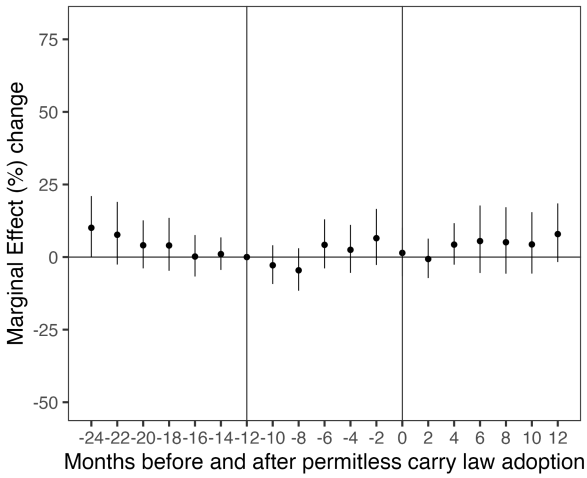

Avg. Effect (months 0 to 12) = 1.529
p< 0.6443
Avg. Effect (months -12 to -2) = -5.001
p< 0.8721
Dep. V. Mean = 12.17

(g) Robbery




Avg. Effect (months 0 to 12) = 2.125
p< 0.5339
Avg. Effect (months -12 to -2) = -1.987
p< 0.3564
Dep. V. Mean = 4.194

(h) Larceny-theft

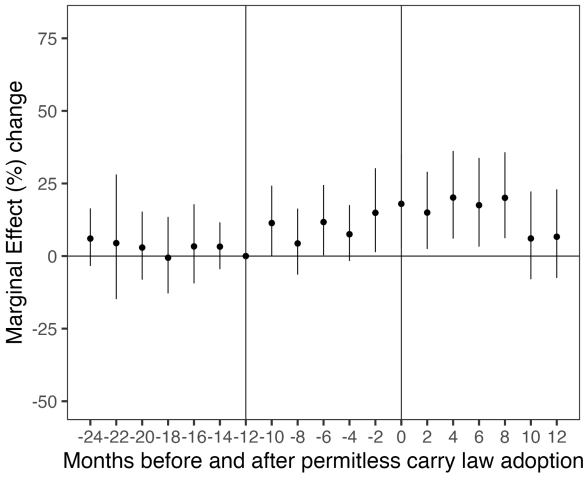



Avg. Effect (months 0 to 12) = -0.376
p< 0.925
Avg. Effect (months -12 to -2) = 7.677
p< 0.3849
Dep. V. Mean = 26.251

Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -24 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

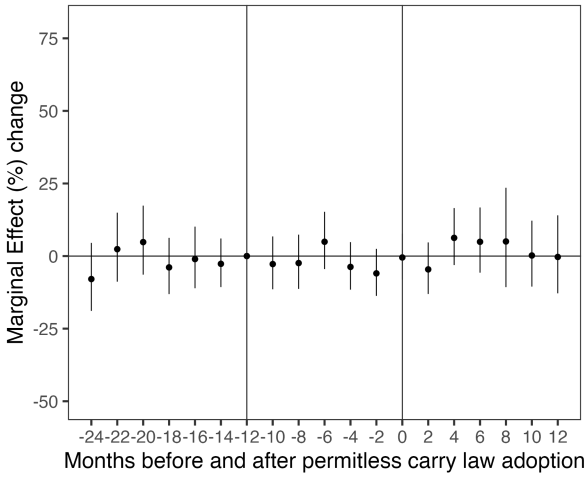
**Appendix Figure A.15:** Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS, Relative to One-Year Prior to Treatment Date

(a) Aggravated Assault - Gun



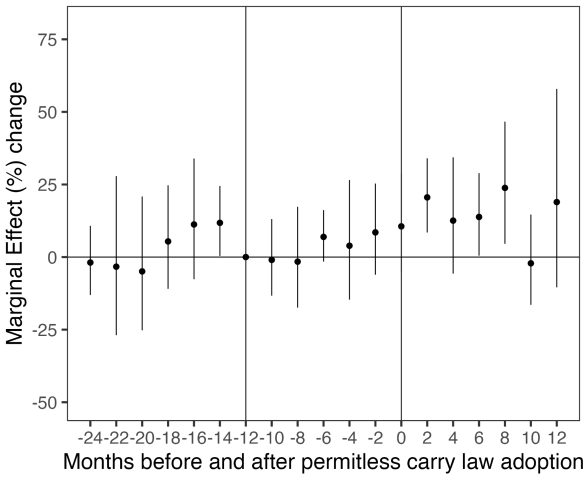

Avg. Effect (months 0 to 12) = 11.51
p< 0.0003
Avg. Effect (months -12 to -2) = 4.462
p< 0.0009
Dep. V. Mean = 1.668

(b) Aggravated Assault - No Gun



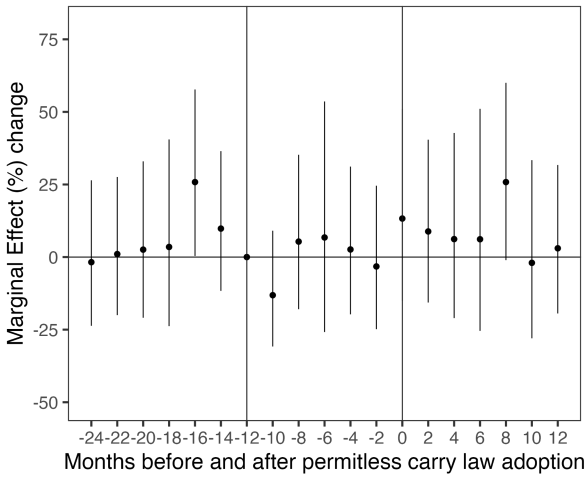

Avg. Effect (months 0 to 12) = 2.219
p< 0.5052
Avg. Effect (months -12 to -2) = 2.376
p< 0.6693
Dep. V. Mean = 2.319

(c) Robbery - Gun




Avg. Effect (months 0 to 12) = 8.629
p< 0.1195
Avg. Effect (months -12 to -2) = -3.314
p< 0.8637
Dep. V. Mean = 1.955

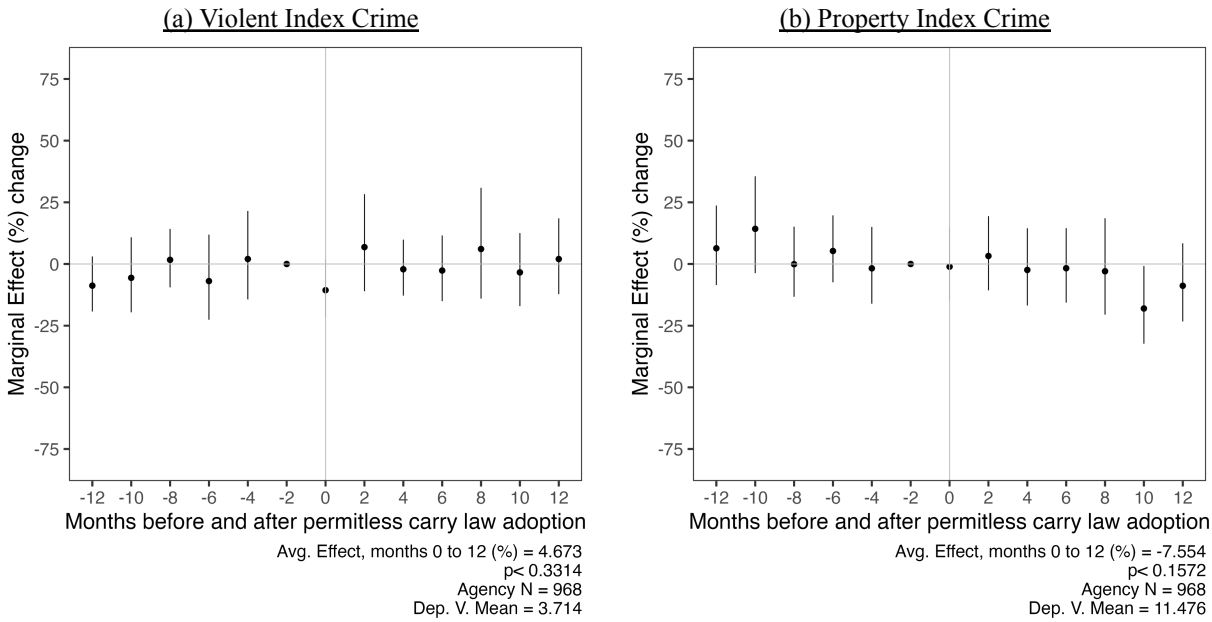
(d) Robbery - No Gun




Avg. Effect (months 0 to 12) = -3.022
p< 0.6493
Avg. Effect (months -12 to -2) = 1.033
p< 0.0861
Dep. V. Mean = 0.501

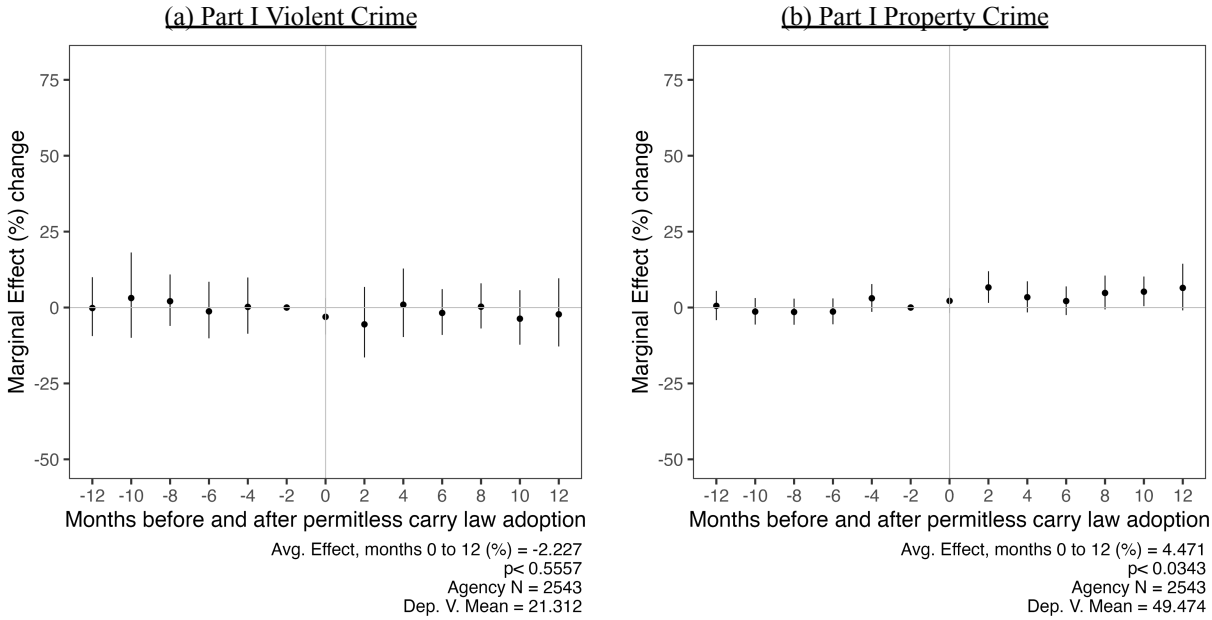
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -24 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

**Appendix Figure A.16:** Effect of Permitless Concealed Carry Laws on Arrests, NIBRS Sample



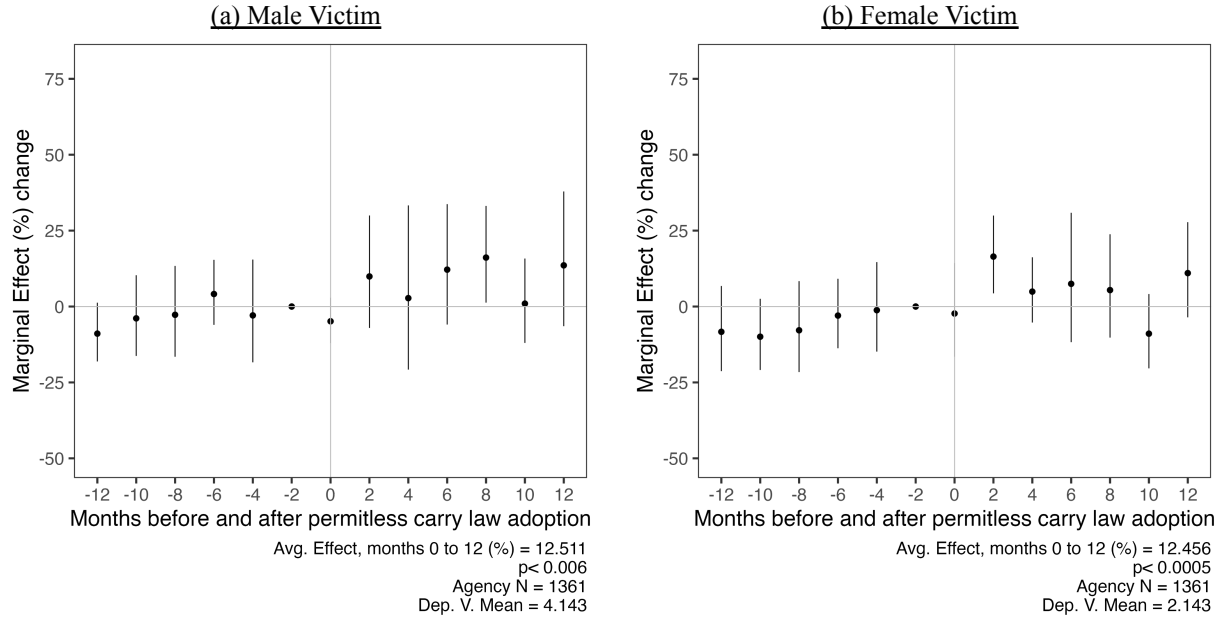

Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

**Appendix Figure A.17:** Effect of Permitless Concealed Carry Laws on Crime Clearance, UCR SRS Sample



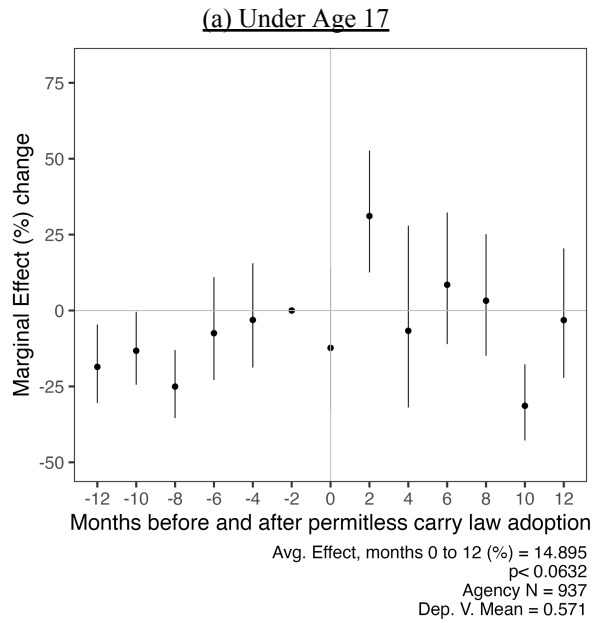

Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked SRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

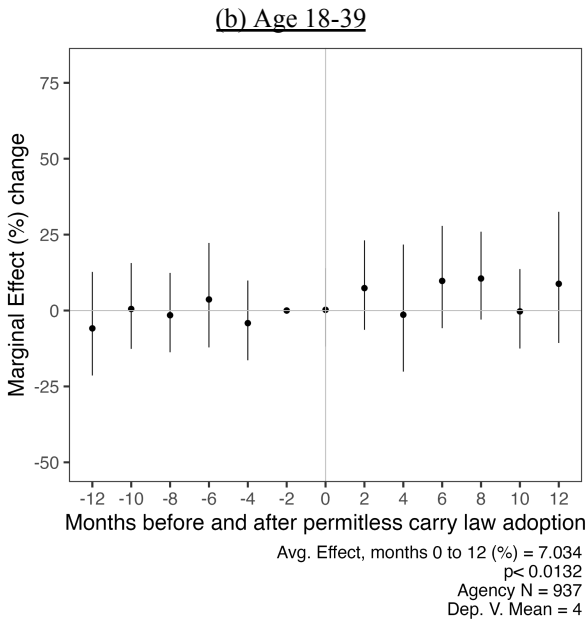
**Appendix Figure A.18:** Effect of Permitless Carry Laws on Aggravated Gun Assault Victim by Sex

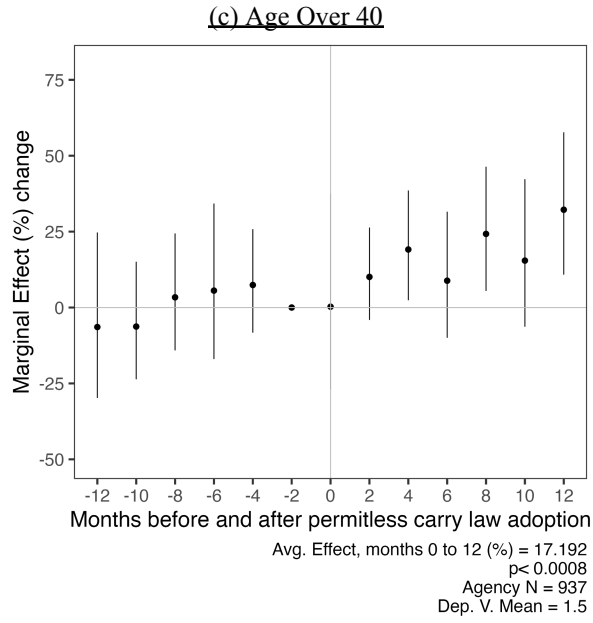



Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

**Appendix Figure A.19:** Effect of Permitless Carry Laws on Aggravated Gun Assault Victim by Age Groups

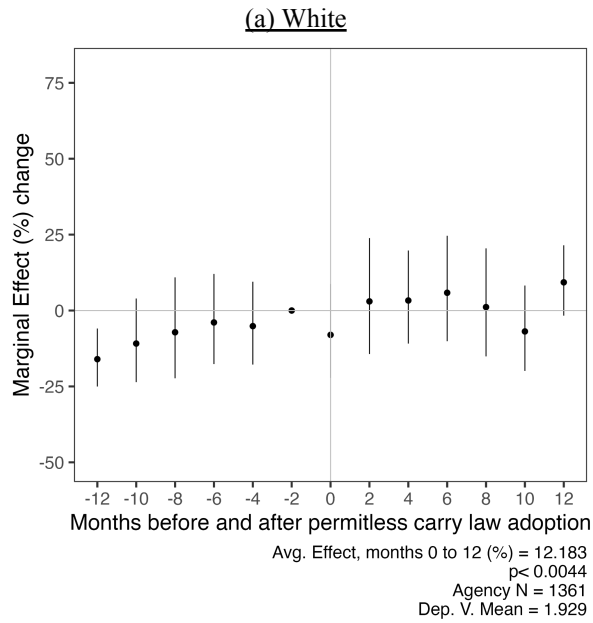


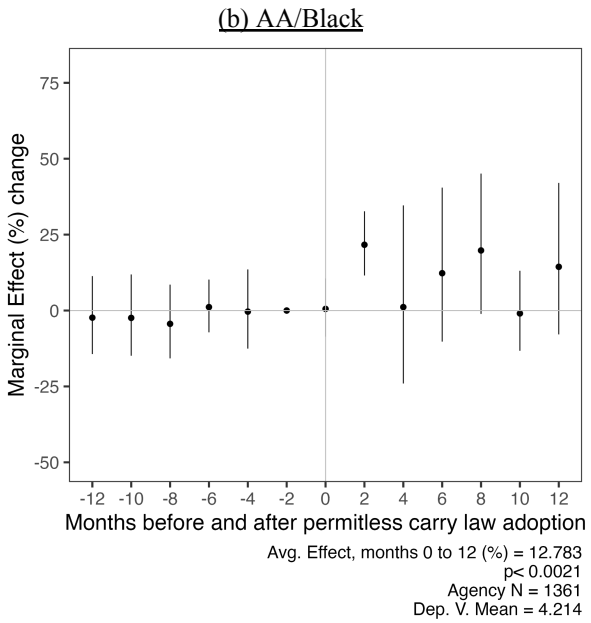



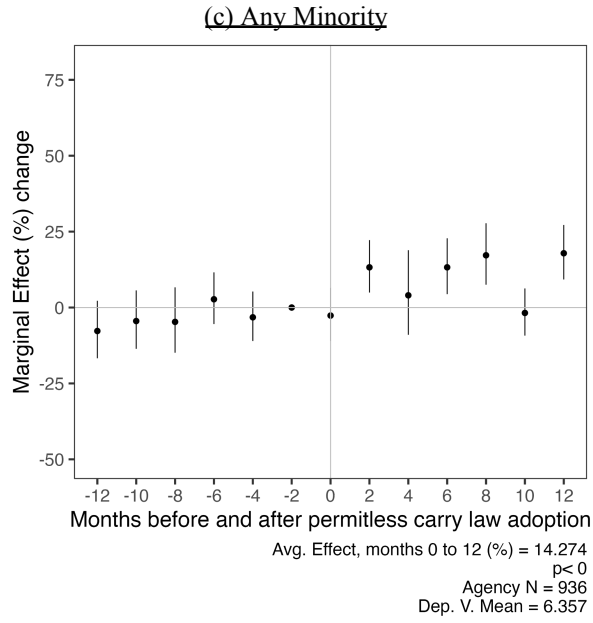




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

**Appendix Figure A.20:** Effect of Permitless Concealed Carry Laws on Aggravated Gun Assault Victim by Race




Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked NIBRS panel. Stack by time and department by time fixed effects are included. Standard errors are clustered on the agency. Avg. Effect, months 0 to 12 is the point estimate from a difference-in-differences model that compares pooled months -12 to -2 months before the passage of a permitless carry law relative to periods 0 to 12 months after the passage relative to control departments. Marginal effect sizes are reported as $100 \times (\exp(\beta) - 1)$ or transformed coefficients from the permitless carry regression equation in Equation 4 which estimate the percent change in crime outcomes following passage of a permitless carry law.

## Appendix B: Data and Sample

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,[11] it is reasonable to expect that NIBRS, which replaces the SRS, has similarly limited data quality and validation procedures. However, NIBRS 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).[12] 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 hospital, campus, university police or transit police by including agencies with a population served greater than zero.[13]

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

[11] 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 (National Academies of Sciences, Engineering, and Medicine, 2018).

[12] 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.).

[13] 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.

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 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. 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 90 and whose $MinPctDiff_{agy}$ exceeds 30. I establish these thresholds following the procedure outlined in Mello (2019) by manually reviewing distributions of

agencies. I identify thresholds that flag agencies with lapses in crime reporting that is not seasonal. I review all agencies excluded under these thresholds to ensure that poor crime reporting is accurately flagged without excluding false positives. For NIBRS, this first step excludes 31 agencies and for the SRS, 20 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 65 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 48 agencies from NIBRS and 68 for the SRS. In total, I exclude agencies for reporting zero crimes in a month and 59 agencies for misreporting from the NIBRS sample. I exclude 8,029 agencies for reporting zero total crimes in a month and 88 agencies for misreporting and 502 agencies in Florida for not reporting monthly crimes, or 10% of the total SRS sample.[14]

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.

[14] 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 C: Permitless Carry Law Passage Timing

**Appendix Table C.1:** Permitless Carry States by Passage and Effective Date

| State | State Law Passage Date | State Law Effective Date | Difference (rounded) |
|---|---|---|---|
| Kentucky | March 11, 2019 | June 26, 2019 | 4 months |
| Oklahoma | February 27, 2019 | November 1, 2019 | 8 months |
| South Dakota | January 31, 2019 | July 1, 2019 | 6 months |
| North Dakota | Mar 23, 2017 | August 1, 2017 | 4 months |
| West Virginia | March 5, 2016 | May 24, 2016 | 3 months |
| Idaho | March 28, 2016 | July 1, 2016 | 3 months |
| Kansas | Apr 2, 2015 | July 1, 2015 | 3 months |
| Maine | Jul 8, 2015 | October 15, 2015 | 4 months |
| New Hampshire | Feb 9, 2017 | February 22, 2017 | less than 1 month |
| Missouri | September 11, 2017 | January 1, 2017 | 7 months |
| Mississippi | April 6, 2016 | April 15, 2016 | less than 1 month |