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

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. This paper estimates the causal impact of permitless carry laws on crime using stacked event study estimators. I find that violent crime increases by 7% in the twelve months following the law's adoption, with increases beginning shortly after passage, or several months before the law formally takes effect. I find no evidence of offsetting deterrence in property crime. The rise in violence is driven by an 12% increase in aggravated assaults involving firearms. These effects are accompanied by a 22% decline in police enforcement of weapons law violations and modest increases in reports of stolen firearms.

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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).

During this timeframe, 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 (S. T. Parker, 2024; Shapira et al., 2022). 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 (Dubois, 2024; Hernández, 2025). 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 (Gates & Glynn, 2021; Reynolds, 2022).

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).

I estimate the effects of adopting a permitless carry law on crime outcomes using a stacked event study framework that uses variation in the timing of state law adoption 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 associated with 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 and may do so in advance of the law's effective date. 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 that adoption of a permitless carry law results in a 7.3% increase in violent crime in the twelve months after the law's implementation. This increase in violent crime is driven by an 11.5% increase in aggravated firearm assaults with no corresponding increase in non-firearm aggravated assaults involving other weapons. These effects start prior to the permitless carry law's effective date and occur during the passage of the law, suggesting a swift behavioral response to the law consistent with recent literature documenting rapid responses to anticipated or actual firearm policy changes (Bollman et al., 2025; Depetris-Chauvin, 2015; Levine & McKnight, 2017). I use the passage date of the permitless carry law as a robustness check, finding that the selection of reference period does not alter main findings. Increases in gun violence occur in public places and are driven from conflicts with friends or acquaintances rather than between family. I find no increase in aggregate property crime, which suggests no net deterrent impact from permitless carry laws; however, I do find suggestive evidence that burglary increases following permitless carry law adoption.

I explore several channels by which permitless carry laws increase violence. First, permitless carry laws also result in changes in enforcement of gun laws. I find substantial reductions in police enforcement of weapons law offenses and arrests. In the twelve months following permitless carry adoption, police reporting of weapons law offenses declines by 13.8% and arrests by 21.6%. I find a modest increase in police ability to solve gun crimes, which may be driven by the public nature of violence associated with permitless carry. I do not find strong evidence that permitless carry increases gun availability through legal gun sales. However, I find suggestive evidence that stolen gun incidents increase, which may increase gun availability to the illicit market for use in crime.

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 handgun possession for individuals under the age of 18 and for disqualifying categories such as a felony conviction or involuntary confinement (*18 U.S.C. § 922(x)(2)*, n.d.).

2.2 The Effects of Concealed Carry Licensing Laws on Crime

A substantial literature focuses on concealed carry licensing laws, beginning with Lott and Mustard (1997). 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; Kyriazis, 2021; Moody & Lott, 2022). Kyriazis (2021) primarily examines changes in crime as a mechanism affecting police shootings after permitless carry adoption and finds increases in 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 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 victims of gun theft.

In addition to the literature on concealed carry laws, this paper contributes to literature that explores how gun carrying and gun laws impact 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.

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

Swift responses to shifts in firearm regulation, both anticipatory and responsive, are increasingly well-documented in the literature. Firearm sales surged in anticipation of Barack Obama's 2008 election, linked to uncertainty about whether stricter future gun policy would be enacted (Depetris-Chauvin, 2015). In advance of regulation in Oregon

[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.

implementing a permit-to-purchase law, firearm background checks rose substantially (Bollman et al., 2025). 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).

In contrast to previous studies of concealed carry laws, this study considers dynamic and rapid impacts of permitless carry laws on crime outcomes. In part, permitless carry laws differ in their implementation. Concealed carry licensing 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 annual 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).[3] In Figure 1, monthly Google Trends data show the increase in searches related to firearm regulation and permitless carry in states that adopted the laws. While search terms such as "constitutional carry" and "concealed carry" increase when permitless carry laws officially take effect, they rise more substantially in the months prior to the law's implementation. Search interest peaks when permitless carry laws are passed, consistent with short-run anticipation.

Finally, many gun owners could respond rapidly to permitless carry laws because most own multiple firearms (Azrael et al., 2017), the majority of which are handguns. Handguns, rather than long guns or rifles, would be eligible for carrying without a license. Further, gun owners are likely to 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 news and increase awareness particularly 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).

4. Data and Descriptive Statistics

4.1 Data Description

[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.).

[3] An exception is Oklahoma, that passed a permitless carry law 8 months prior to implementation.

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 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 FBI-defined crime aggregate measures known as index crimes. Violent index crime includes murder, rape, robbery, and aggravated assault. I additionally examine specific 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 index crime which is an aggregate crime category consisting of burglary, motor vehicle theft, arson and larceny. I additionally use specific property crime types 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.

4.2 Additional Data

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.

4.3 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 ten states passed permitless carry laws 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

I employ a dynamic difference-in-differences or “stacked event study” design to measure the effect of permitless carry laws on crime. Because states adopt permitless carry laws at different times, the staggered nature of treatment requires methods that avoid bias from inappropriate comparisons across treated units (Baker et al., 2022; Cengiz et al., 2019; Deshpande & Li, 2019). Stacked estimators address this by constructing a separate dataset, or "stack," for each treated unit, including only appropriate comparison units that are untreated at that time or never treated. By including fixed effects for each stack, a stacked difference-in-differences estimator avoids concerns

[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, NE, OH, SC, TN, TX, UT.

about biased estimates stemming from inappropriate comparisons between treated and comparison units in the context of staggered policy adoption.

Unlike much of the literature on concealed carry laws, I do not estimate state-year models. Instead, I use monthly or pooled two-month time intervals to detect short-run effects. This high-frequency approach is motivated by the institutional context: permitless carry laws take effect quickly and may produce immediate behavioral responses. Accordingly, I estimate policy effects over the 12 months following the implementation of the law. This narrower window may help reduce violations of the parallel trends assumption, which becomes more plausible over shorter horizons.

A drawback of using short intervals is the increased presence of zero-count outcomes, particularly for rarer crime categories. Rather than log-transforming outcomes, which can introduce bias due to the high proportion of zeros (Chen & Roth, 2023; Mullahy & Norton, 2022), I use Poisson fixed effects estimators, which allow for consistent estimation with count data and fixed effects to estimate average treatment effects (Wooldridge, 2023).

Given emerging evidence of anticipatory behavior in response to changes in firearm policy (Depetris-Chauvin, 2015, Bollman et al., 2025), I use a reference period defined as the twelve months prior to a law's implementation. This choice addresses potential violations of the parallel trends assumption arising from behavioral changes that occur during the post-passage but pre-implementation window. As a robustness check, I also use the passage date itself as the reference period to test the sensitivity of the estimates to alternative treatment timing assumptions.

The main estimating equation is applied to a stacked panel constructed at the police agency level. For each treated agency, I match untreated agencies from states that have not yet (or never) adopted permitless carry laws during the study period. These agency-level stacks are appended, and the stacked panel is centered on the implementation date of the permitless carry law and the reference period is twelve months prior to the implementation month of the permitless carry law. I estimate dynamic effects using the following Poisson fixed effects model:

$$Y_{dim} = exp(\alpha_{di} + \gamma_{dm} + \sum_{\tau} \delta_{\tau}(1[treated]_{di} * D^{\tau}_{dt}) + log(pop_{dim}) + \epsilon_{dim}) \quad (1)$$

Where Y_{dim} is a crime outcome in police agency jurisdiction i in month m in stacked dataset d. The fixed effects α_{di} and γ_{dm} are stack-by-agency and stack-year-by-month fixed effects. The coefficients of interest δ_{τ} capture the average treatment effect of permitless carry law passage in the months following passage of a permitless carry law. Outcomes are pooled into two 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. Event study figures report the percent change in the rate of crime outcomes in treated agencies. To obtain the percent change in the rate of crime outcomes in treated agencies, coefficients are transformed by 100 x (exp(β_1)-1).[6] Data is centered on the twelve months prior to the implementation of the permitless carry law.

To obtain an average treatment effect in the post-period months in the presence of pre-period anticipation, I use a stacked difference-in-differences approach. Using the same stacked panel centered on the implementation of a permitless carry law, I fit a Poisson regression model that takes the following form:

$$Y_{dim} = exp(\beta_1 Treated_{dim} + \beta_2 Passage_{dim} + \gamma_{di} + \phi_{dm} + log(pop_{dim}) + \epsilon_{dim}) \quad (2)$$

The coefficient β_1 captures the average treatment effect of implementing a permitless carry law in the twelve months following the effective date of a permitless carry law. The coefficient β_2 captures the average treatment effect of the passage period, or the period of twelve months to two months prior to the permitless carry law's effective date, during the time window when states passed permitless carry laws but they were not yet effective. The fixed effects α_{di} and γ_{dm} are stack-by-agency and stack-year-by-month fixed effects. Standard errors are clustered at the state level.

This model is similar to the anticipatory effect models used by Alpert (2018) and suggested in Malani and Reif (2015). The intuition for the approach is that if the assumption of no anticipation is correct, then inclusion of a leading indicator variable should have no impact on the estimated treatment effect and the leading indicator variable should be approximately zero.

I also estimate stacked event study models for police behaviors, including weapons law violations and crime clearances, and stolen gun incidents using Equation 1. For background check outcomes, which consist of counts of handgun and long gun background checks, I use state-level observations and state by month and state by stack fixed effects and cluster standard errors at the state level.

If the assumption of parallel trends in the year prior to adoption of a permitless carry law is violated, average treatment effect estimates may be biased. In the appendix, I re-estimate the main event study specification using the passage date of the permitless carry law instead of the twelve months prior to the implementation date as the reference period. I additionally include estimates from alternative comparison groups and

[6] 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.

specifications. I replicate the analysis using only the SRS data, which include additional states that did not report data to NIBRS, and use a comparison group consisting of agencies in states that did not yet enact permitless carry laws during the study timeframe. I estimate log-linear models using the stacked panel aggregated to the state-month level to avoid the large proportion of zero values in outcomes.

6. Results

6.1 Results for Crime Outcomes

Figure 3 presents estimates of the effect of adopting a permitless carry law on violent crime using the event study specification in Equation 1. I estimate that the implementation of a permitless carry law increases violent index crime by 7.3% (panel a) relative to agencies in states that did not adopt a permitless carry law. The increase in violent crime starts to trend upward during the window when states passed permitless carry laws, which occurred typically 4-8 months prior to the law's implementation. The increase in violent crime persists after the implementation period of the law. Estimates translate to approximately 1,765 additional violent index crimes in the twelve months following implementation of a permitless carry law.

Violent index crime increases are driven by aggravated assaults, which rose an estimated 9.8% after permitless carry law passage (Figure 3, panel b). The increase in aggravated assaults occurred prior to the law's effective date, rising a statistically significant 4.4% in the months leading up to the law's effective date in the timeframe of law passage. The effect on simple assaults is null, which suggests that interpersonal conflict is not impacted or deterred by passage of a permitless carry law. Estimates for robbery, the most rare crime outcome, are unreliable due to violations of the parallel trends assumption. While I observe upward movement in robbery following policy implementation, the presence of pre-trends limits a causal interpretation. In robustness checks limited to larger jurisdictions where firearm robberies are more prevalent, I find more consistent patterns by examining firearm vs. non-firearm robbery, detailed below.

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 11.5% in aggravated assaults committed with a firearm (panel a) and a statistically imprecise and modest increase of 2.2% for aggravated assaults not committed with a firearm (panel b). The increase in aggravated gun assaults occurs prior to the law taking effect, rising 6.4% in the passage time window. The increase in aggravated assaults committed with a firearm equates to approximately 408 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. In Appendix Figures A.2-A.4, I estimate effects in agencies serving jurisdictions over 90,000 in population which may be more impacted by firearm crime than communities with lower population density. Violent crime and gun crime increases were higher in magnitude in larger communities with an estimated 13.1% increase for violent index crime and 15.2% increase for aggravated assaults with firearms without a statistically significant increase in non-firearm aggravated assaults. Similarly, robbery with a firearm increases 13.2% with no significant increase or decrease in non-firearm robbery.

In Figure 5, I estimate the effect of permitless carry law adoption on property crimes. In panel a, I estimate a modest increase in property index crime that is not significant at the 5% level. The suggestive increase in property crime is not driven by motor vehicle thefts or larceny-theft (panels c and d) but rather by an increase in burglary.

In Figure 6, 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 15.1% 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 16.6% (panel c). Finally, victims of aggravated gun assaults are increasingly likely to experience injury associated with the gun assault (panel d) which suggests that violence involving firearms is more likely to escalate and result in injury.

6.2 Robustness

In Appendix Figures A.5-A.10, I re-estimate main crime and firearm crime effects using an alternative sample and comparison groups. Appendix Figures A.5-A.7 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.8-A.10, I report main estimates from restricting the NIBRS comparison group to consist only of future adopting states. These estimates are consistent with main crime and gun crime effects.

In Appendix Tables 3-5, I present results for main crime and firearm crime results for alternative specifications and estimators. I include controls for demographics, lagged policing employment and incarceration rates at the agency-month level for the NIBRS and SRS samples using pooled-month Poisson fixed effect models. I aggregate

the panel to the state-pooled month level and use the log crime rate per capita to contrast to linear specifications. In Figures A.11-A.13, I present corresponding event study models for log-linear state-pooled month specifications. Results are broadly similar across specifications and samples. As a whole, I find consistent evidence that permitless carry law adoption increases violent crime with effects driven by aggravated gun assaults that is not a function of sample composition or specification.

Property index crime estimates across specifications are insignificant, but the magnitude of the coefficient ranges differ by sample. The NIBRS estimates range from an increase of 1.03 to 5.6% while the UCR SRS sample estimates are lower in magnitude and occasionally negative. While the estimates for property crime are insignificant regardless of sample composition, the estimates of burglary across samples and specifications are consistently positive and significant. In the SRS, burglary increased a significant 4.34% in the months following permitless carry law implementation (Appendix Figure A.6, panel b) and similarly, an increase of 5.17% in the NIBRS log crime rate model though not significant (Appendix Figure A.12, panel b).

In Appendix Figures A.14-15, 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 16-17, 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 single states or pandemic-related post-period observations.

In Appendix Figures 18-20, I re-estimate main crime and firearm crime effects using alternative reference periods. Using the law's passage date rather than twelve months prior to the implementation date, I reestimate main effects from Equation 1, where the stacked panel is instead centered on the passage date of the permitless carry law. I find that results are robust to using the passage date as a 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.

I begin by focusing on 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 1 to both offenses and arrests. Figure 7 presents estimates of weapons law offenses reported by police, showing a 13.8% reduction in offenses recorded by police (panel a). Similarly, police arrests for weapons

law violations declined by 21.6% following permitless carry law passage. Appendix Figure A.21 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 suspected of 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. It is not possible to disentangle how much of the effect is driven by lessened enforcement due to the law itself or police response to the law.

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, counts of cleared aggravated assault are insignificant, though are positive in magnitude. I find suggestive evidence that clearances of aggravated gun assaults increased as permitless carry laws are enacted (panel d). In part, this may be due to the changing circumstances of firearm crimes that are more likely to occur in public places with witnesses or surveillance available to police. Appendix Figure A.17 presents similar results from the SRS sample. These findings differ 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 in a sample of large cities. While the broader NIBRS sample may differ as it includes smaller police agencies, it is also possible that increased attention to gun crime associated with the permitless carry law's passage or the public nature of aggravated gun assaults, which are more likely to have witnesses and result in injury, may drive short-term increases in gun crime clearance rates.

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 do not find significant increases in background checks for handguns following adoption of a permitless carry law, though a significant spike occurs in the month that a permitless carry law is implemented. This finding is consistent with Kim & Chen (2025) who find no change in gun purchasing following permitless carry law implementation. Taken together, both findings underline that gun purchasing is 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.24, panel c), an age group with greater rates of gun ownership relative to younger groups. Finally, I find a 7.9% increase in stolen gun incidents (Figure 9, panel c) in the twelve months after a permitless carry

law implementation. The post-period increase in the months immediately after the implementation of a permitless carry law suggests a swift response to the law. Robustness checks using the passage date and comparison groups of both the full sample of NIBRS agencies as well as future adopting states find consistent evidence that stolen gun incidents significantly rose after the adoption of a permitless carry law with similar and statistically significant estimates of approximately an 8 percent increase in stolen guns (Appendix Figure A.26).

8. Conclusions

This study examines the impact of permitless carry laws on crime. Permitless carry laws remove the requirement to first obtain a license and undergo a background check before carrying a concealed handgun in public places. Passage of a permitless carry law increases violent index crime by 8.3%, driven by a 15% increase in aggravated gun assaults. Aggravated gun assaults increased 11.5% following adoption of a permitless carry law, with no significant corresponding increase in aggravated assaults that do not involve firearms. Stacked event studies suggest that the increases in violent crime start to occur several months before a permitless carry law takes effect, corresponding to the passage timeframe of the laws. Property index crime does not increase significantly overall, though I find suggestive evidence that burglary increases following permitless carry law adoption.

To my knowledge, this study is the first to find that permitless carry laws increase violence. These findings suggest that removing the need to obtain a license before carrying a firearm in public places results in increased violence, particularly gun violence among strangers in public locations. This finding is robust to a variety of samples, comparison groups, and specifications, including time-varying factors such as economic conditions and policing. In addition, findings are consistent regardless of assumptions when firearm owners may respond to a permitless carry law, whether the passage or adoption timing of the law.

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 aligns 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 though I do not observe gun ownership directly.

Permitless carry laws are accompanied by a substantial reduction in police enforcement of weapons laws violations. The reduction impacts both weapons law offenses, which primarily occur through police reporting of a crime occurrence, as well as

as police arrests, which document the response to an offense. The reduction in arrests is suggestive of a shift in police behavior rather than in the prevalence of public weapons carrying. Police reported difficulty in ascertaining legal and illegal weapon carrying in the months following a permitless carry law (Comber-Wilen, 2023; J. Magdaleno & Lange, 2023). Police further may not expect weapons law violations arrests to result in charged cases and therefore may not be a valuable use of police time. Therefore, the reduction in arrests may be from a reduction in police-public contact for illicit weapons law carrying rather than from actual reductions in illicit weapons carrying. The overall reduction in weapons law offenses and arrests may lower the perceived and real costs of carrying an illegal firearm.

Permitless carry law adoption increases the number of stolen gun incidents, which may increase the supply of illicit firearms for use in crime. This finding is consistent with Billings (2022) who finds that concealed handgun permit holders are more likely to be victims of property crime and specifically, more likely to have a firearm stolen from them. The short term effects in this analysis may therefore not generalize to the ongoing process of diverting firearms to the illicit market and future research should be undertaken to document the longer-term impacts of permitless carry laws.

Policy options that promote or mandate safe firearm carrying and storage may be impactful for reducing gun assaults in permitless carry states. In many states, restrictions on where handguns may be carried in public include bars, stadiums or and other public locations where the majority of the violence related to permitless carry laws is likely to occur. Location restrictions on public gun carrying typically have broad support among those who do and do not own guns alike (Crifasi, Ward, et al., 2021; Wolfson et al., 2017). Policies encouraging safe storage practices, not only in the home but also during transit and in vehicles, may further reduce the likelihood of theft and downstream availability for use in crime.

Tables

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	3/28/2017	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	Yes
South Dakota	1/31/2019	7/1/2019	6	Yes	Yes	Yes
West Virginia	3/5/2016	5/24/2016	3	Yes	Yes	Yes

Notes: Effective 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.

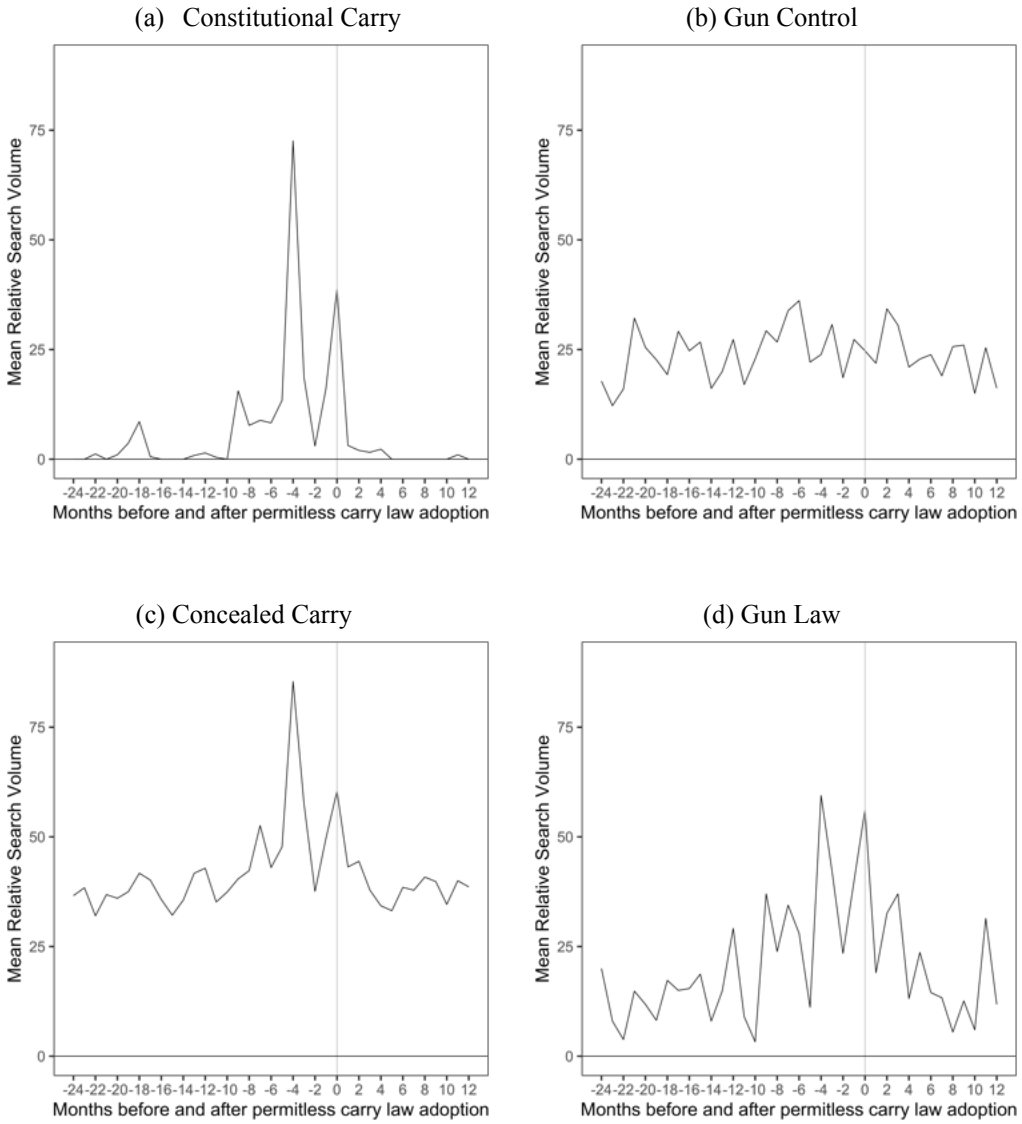
Table 2: Descriptive Statistics of NIBRS Crime Outcomes

	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. The control group consists of agencies in states including AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

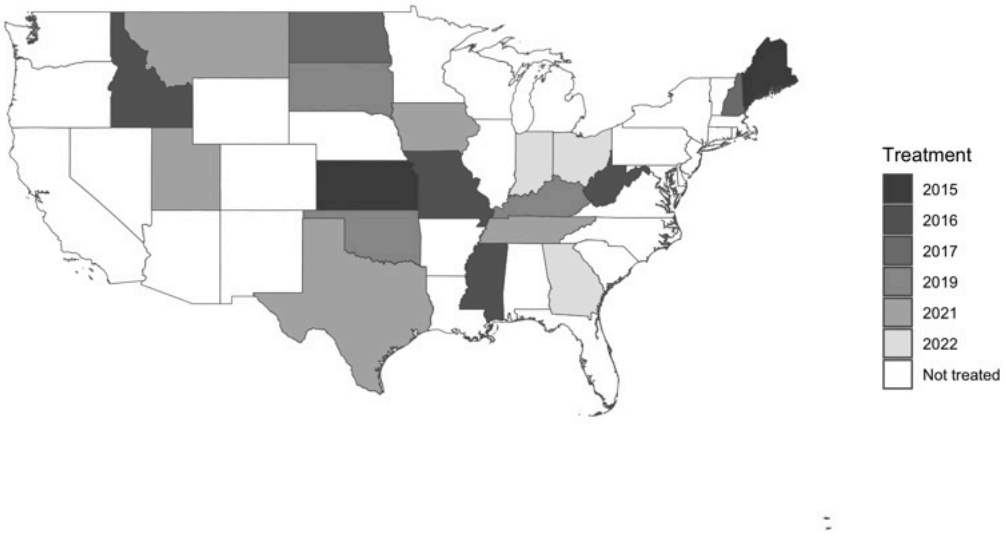
Figures

Figure 1: Google Trends Searches for Permitless Carry-Related Search Terms Before and After Permitless Carry Law Adoption in Treated States



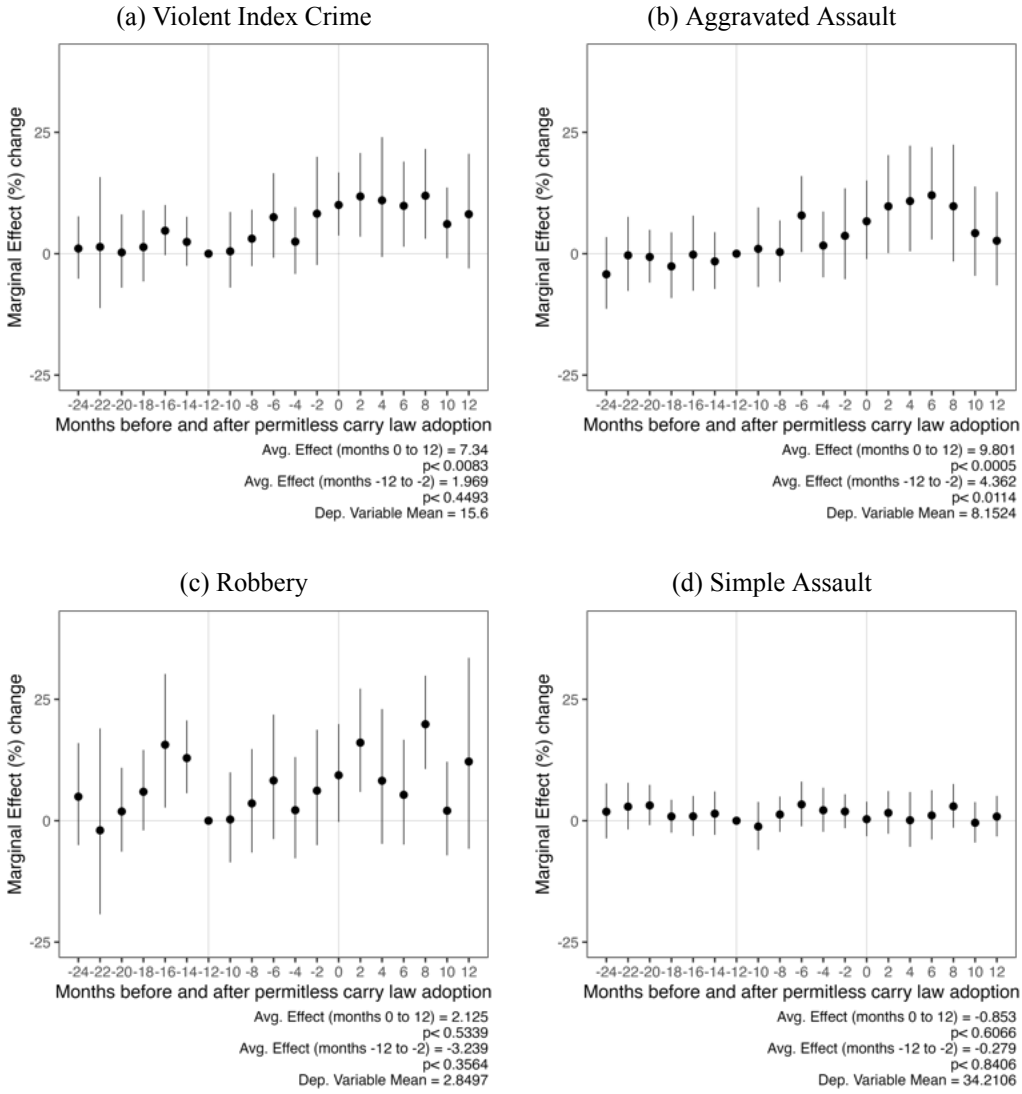
Notes: Each figure presents a time-series of mean monthly search volume from Google Trends search data from search terms related to permitless carry laws and firearm regulation in states that adopted permitless carry during the study timeframe (including ID, KS, KY, ME, MO, MS, OK, SD and WV; ND and NH do not have non-zero search term volume during this timeframe for constitutional carry search terms). Search trends measures are scaled from 0-100 and measure relative interest. The vertical line represents the month a permitless carry law takes effect.

Figure 2: States by Permitless Carry Adoption Status



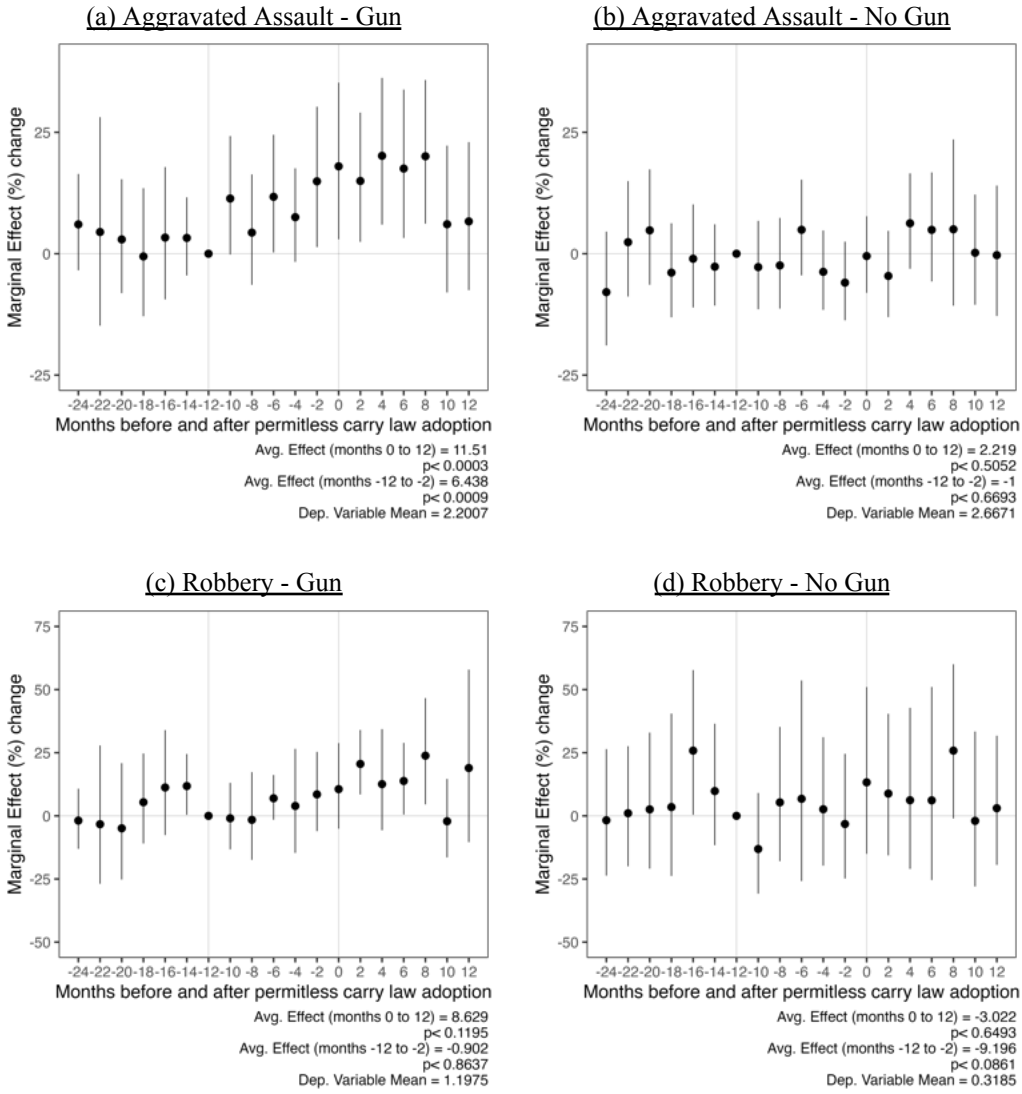
Note: Permitless carry adoption in Vermont occurred in 1903 and is grouped with Alaska (2003) as treated before 2004. Wyoming (2010) and Arizona (2011) fall outside of the sample timeframe. States that have yet to adopt permitless carry include: AL, IA, LA, MT, NE, OH, SC, TN, TX, UT. The analytic sample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

Figure 3: Effect of Permitless Concealed Carry Laws on Violent Crime, NIBRS



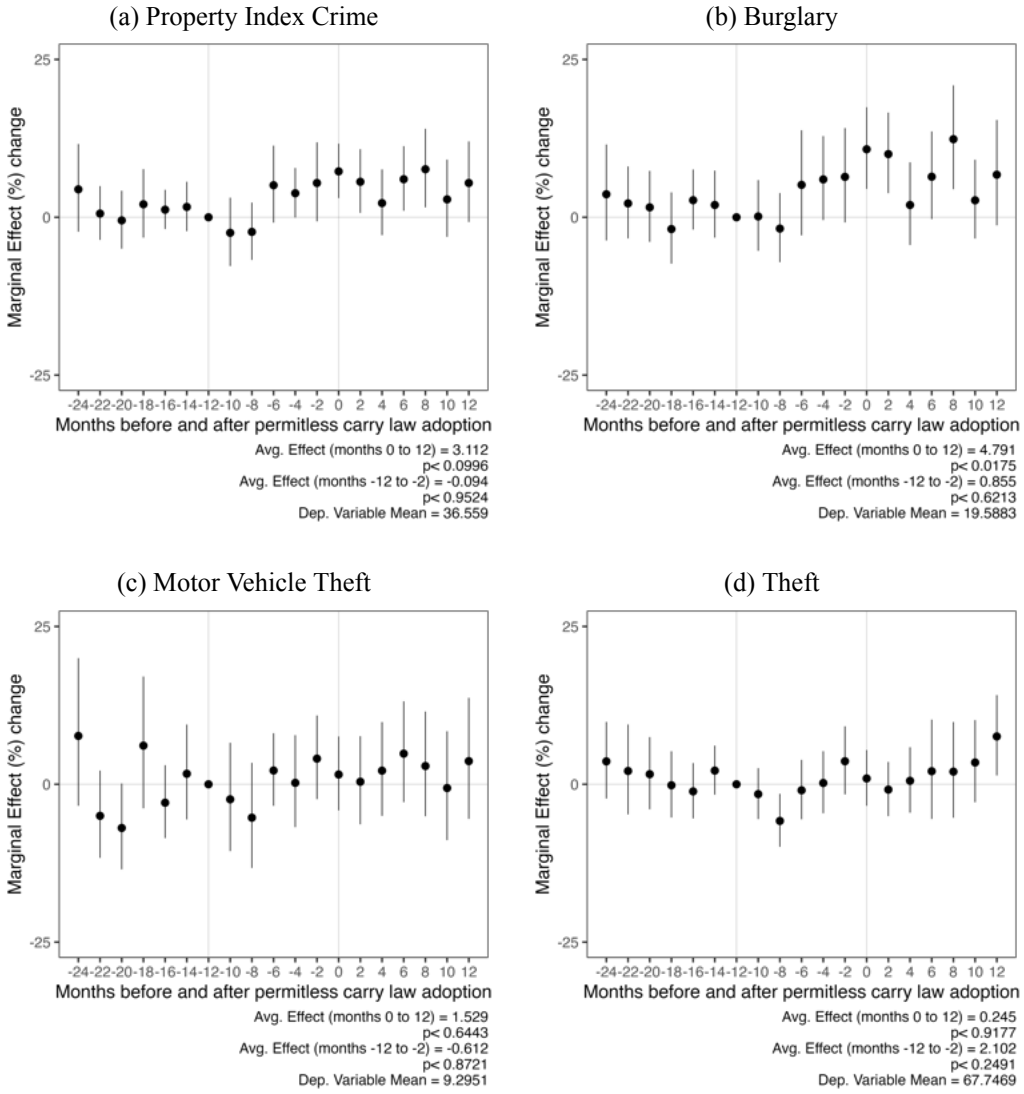
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 232,733 observations pooled into 2-month groups at the agency level. Stack by 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. The panel consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

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 232,733 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. The panel consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

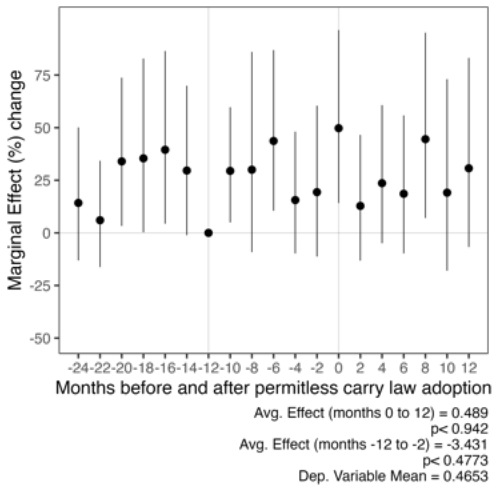
Figure 5: Effect of Permitless Concealed Carry Laws on Property Crime, NIBRS



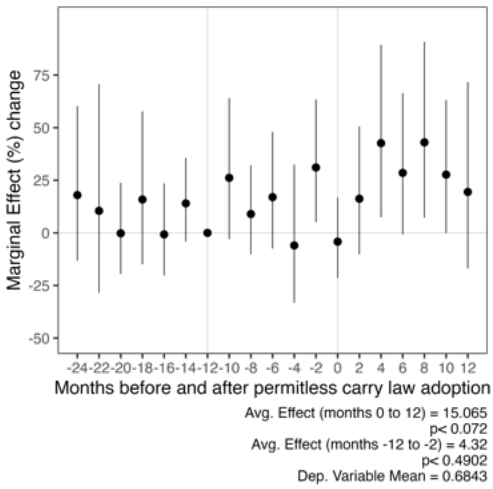
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 232,733 observations pooled into 2-month groups at the agency level. Stack by 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. The panel consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

Figure 6: Effect of Permitless Concealed Carry Laws on Aggravated Gun Assault Circumstances, NIBRS

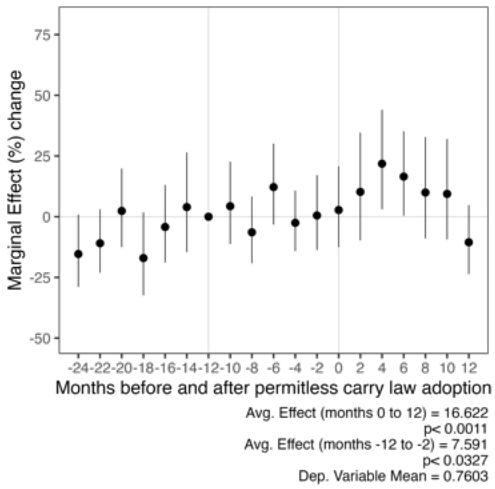
(a) Domestic Violence Victim-Suspect Relation



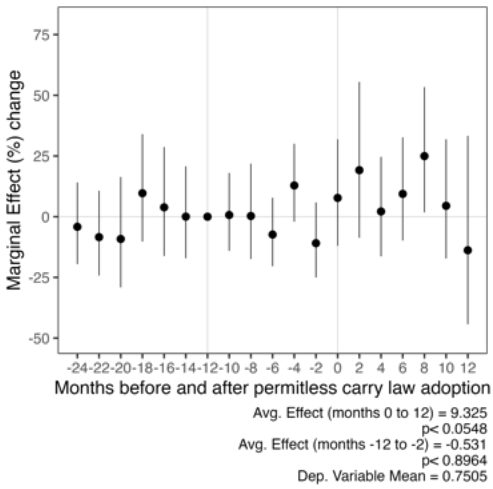
(b) Acquaintance or Stranger Victim-Suspect Relation



(c) Public Place Location



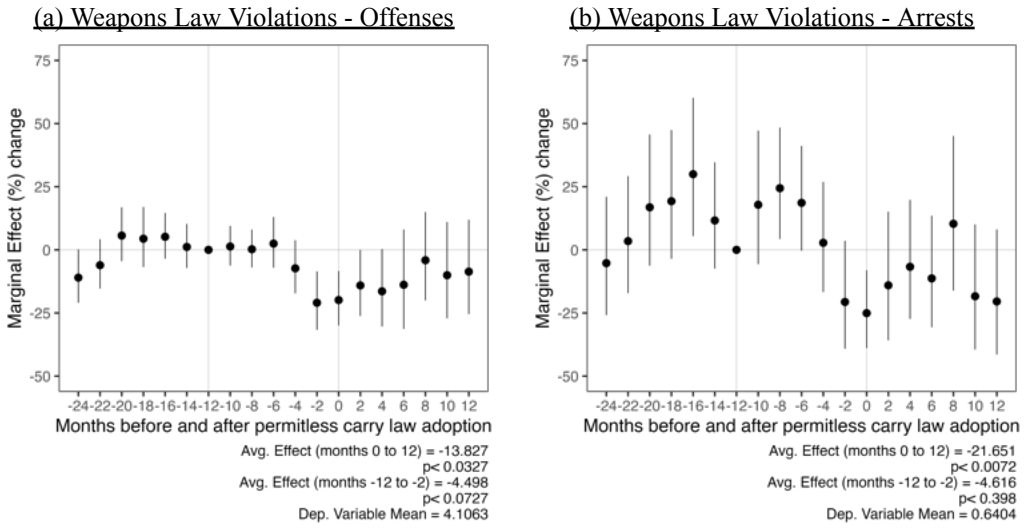
(d) Major or Minor Injury Incurred



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 232,733 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. The panel

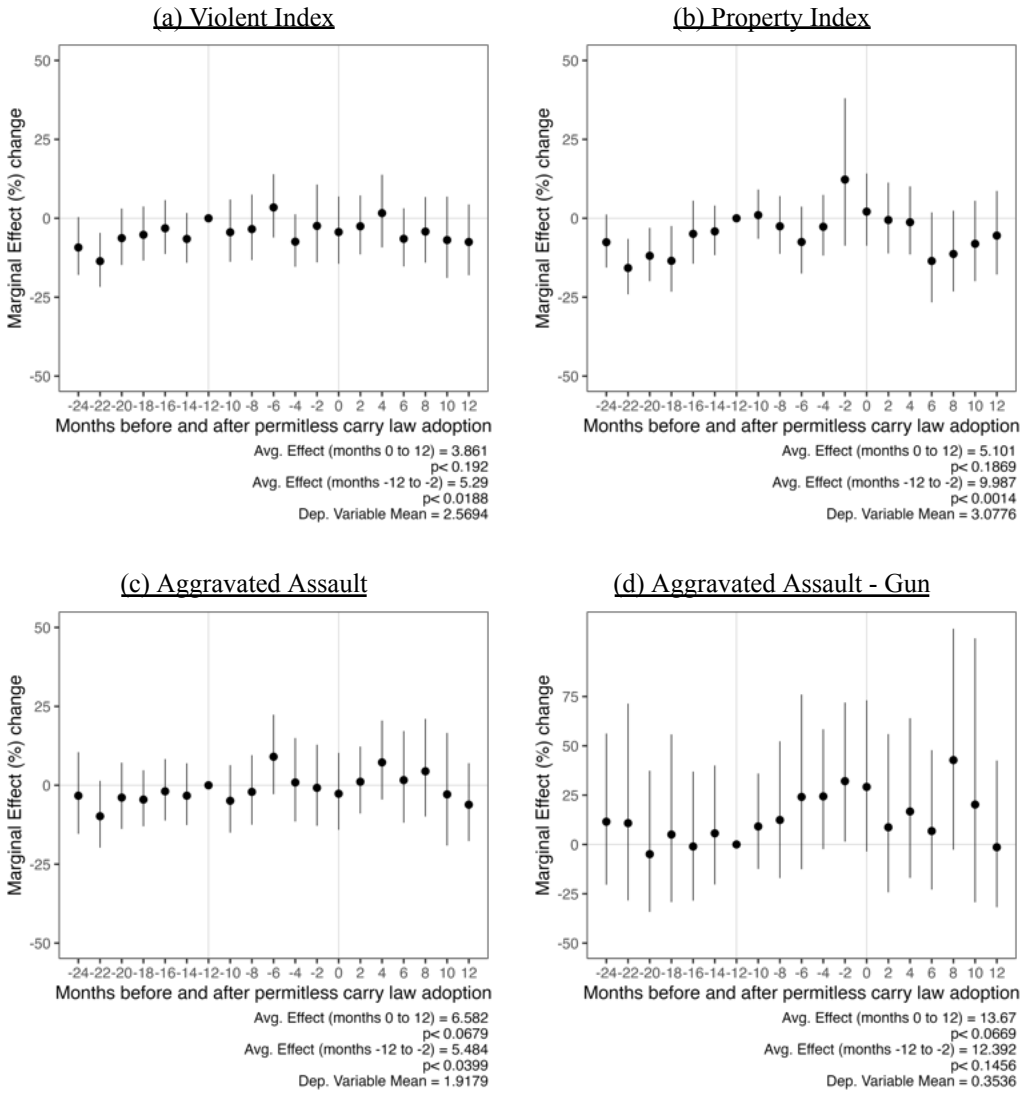
consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

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



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 232,733 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. The panel consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

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

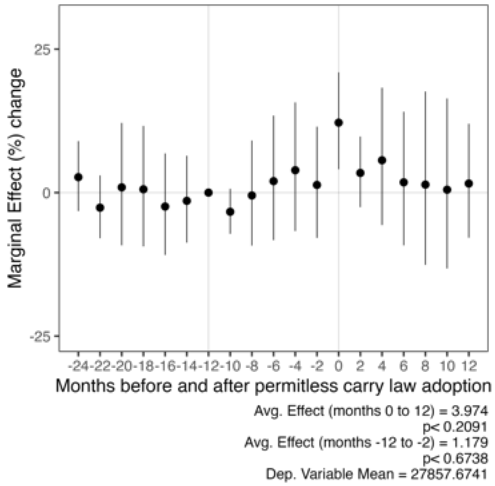


Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 232,733 observations pooled into 2-month groups at the agency level. Stack by 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. The panel

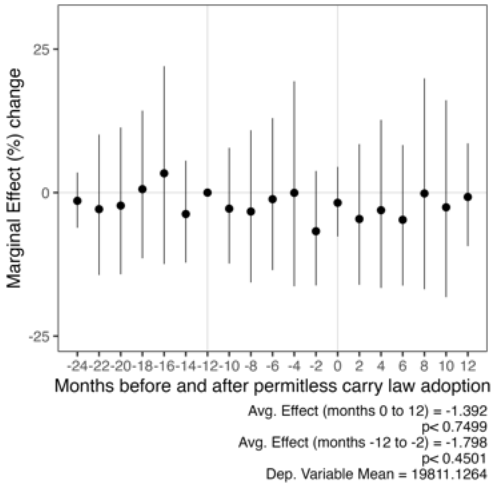
consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

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

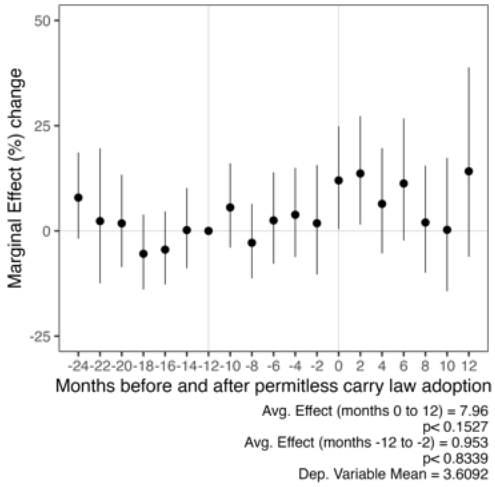
(a) Handgun Background Checks



(b) Long Gun Background Checks



(c) Stolen Gun Incidents



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. Background checks data span 43 states and the District of Columbia from 2013-2020. Stolen guns data come from NIBRS and 1607 agencies in the sample that reported a stolen gun incident during the study timeframe.

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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	NIBRS Agencies in Sample	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 on Violent Index Crime

	Poisson	Poisson + controls	Poisson State Level	OLS Log Linear - Unweighted	OlS Log Linear - Weighted
Panel A: NIBRS					
Permitless Carry (months 0 to 12)	7.2526** (2.9124)	9.1996*** (2.2556)	6.8017*** (2.3445)	7.2933** (2.7720)	5.9533 (4.1504)
Passage (months -12 to -2)	7.2933** (2.7720)	5.9533 (4.1504)	7.2526** (2.9124)	9.1996*** (2.2556)	6.8017*** (2.3445)
Stack by Agency	Yes	Yes	No	No	No
Stack by State	No	No	Yes	Yes	Yes
Stack by Year by Month	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	No	No
Observations	232733	214865	5069	5068	5068
Panel B: UCR SRS					
Permitless Carry (months 0 to 12)	7.6427*** (1.8534)	4.3691 (4.6706)	3.6246 (3.6309)	7.8489*** (1.8153)	6.7365* (3.3832)
Passage (months -12 to -2)	7.8489*** (1.8153)	6.7365* (3.3832)	7.6427*** (1.8534)	4.3691 (4.6706)	3.6246 (3.6309)
Stack by Agency	Yes	Yes	No	No	No
Stack by State	No	No	Yes	Yes	Yes
Stack by Year by Month	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	No	No
Observations	386666	378528	7029	7029	7029

Notes: Columns estimate separate difference-in-differences estimators that compare estimates across specifications and aggregation level. Columns 1 and 2 are Poisson fixed effect estimators fit on panel data at the agency-pooled month level. Controls include median household income, percent of households in poverty, proportion male, Black and white population aged 18-29, and lagged police officers per capita. Column 3 is a Poisson fixed effect estimator fit on panel data aggregated to the state level. Columns 4 and 5 are log-linear OLS models where the outcome is the log rate of crime per capita in each state from reporting agencies. Column 5 estimates are weighted by state population. Estimates are presented as a marginal percent change.

Appendix Table A.4: Effects of Permitless Carry on Property Crime

	Poisson	Poisson + controls	Poisson State Level	OLS Log Linear - Unweighted	OlS Log Linear - Weighted
Panel A: NIBRS					
Permitless Carry (months 0 to 12)	3.1803 (3.0287)	1.0363 (6.7618)	3.4402 (3.3484)	5.6337 (5.2769)	4.2365 (3.8117)
Passage (months -12 to -2)	-0.3669 (2.2190)	-1.1320 (3.8882)	-0.0721 (2.0439)	1.1523 (2.1887)	0.5112 (2.1504)
Stack by Agency	Yes	Yes	No	No	No
Stack by State	No	No	Yes	Yes	Yes
Stack by Year by Month	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	No	No
Observations	232733	135734	5069	5069	5069
Panel B: UCR SRS					
Permitless Carry (months 0 to 12)	-3.7249 (4.1164)	-0.3515 (4.2846)	2.2225 (2.0827)	0.5728 (3.1896)	0.9210 (2.5946)
Passage (months -12 to -2)	-6.7337* (3.795)	-4.7153 (3.8374)	-0.7921 (1.3104)	-2.0434 (1.8841)	-1.3307 (1.5617)
Stack by Agency	Yes	Yes	No	No	No
Stack by State	No	No	Yes	Yes	Yes
Stack by Year by Month	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	No	No
Observations	385139	245014	6816	6816	6816

Notes: Columns estimate separate difference-in-differences estimators that compare estimates across specifications and aggregation level. Columns 1 and 2 are Poisson fixed effect estimators fit on panel data at the agency-pooled month level. Controls include median household income, percent of households in poverty, proportion male, Black and white population aged 18-29, and lagged police officers per capita. Column 3 is a Poisson fixed effect estimator fit on panel data aggregated to the state level. Columns 4 and 5 are log-linear OLS models where the outcome is the log rate of crime per capita in each state from reporting agencies. Column 5 estimates are weighted by state population. Estimates are presented as a marginal percent change.

Appendix Table A.4: Effects of Permitless Carry on Firearm and Non-Firearm Crime

	Poisson	Poisson + controls	Poisson State Level	OLS Log Linear - Unweighted	OlS Log Linear - Weighted
Panel A: NIBRS Aggravated Assault - Gun					
Permitless Carry (months 0 to 12)	11.3046*** (2.9150)	15.0910*** (3.1903)	10.4636** (4.5685)	12.6744*** (3.3513)	10.5968 (6.3572)
Passage (months -12 to -2)	12.6744*** (3.3513)	10.5968 (6.3572)	11.3046*** (2.9150)	15.0910*** (3.1903)	10.4636** (4.5685)
Stack by Agency	Yes	Yes	No	No	No
Stack by Year by Month	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	No
Observations	232733	213721	5069	5069	5069
Panel B: UCR SRS Aggravated Assault - Gun					
Permitless Carry (months 0 to 12)	14.4514*** (2.4630)	14.1413*** (2.5712)	10.2020* (5.7329)	14.5741*** (2.0957)	5.0826 (8.6256)
Passage (months -12 to -2)	14.5741*** (2.0957)	5.0826 (8.6256)	14.4514*** (2.4630)	14.1413*** (2.5712)	10.2020* (5.7329)
Stack by Agency	Yes	Yes	No	No	No
Stack by Year by Month	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	No
Observations	386666	344639	6567	6519	6519
Panel C: NIBRS Agg. Assault - Non Gun					
Permitless Carry (months 0 to 12)	2.0193 (3.9883)	3.4655 (3.7970)	1.9361 (3.1762)	2.4411 (3.9181)	10.1791 (6.9956)
Passage (months -12 to -2)	2.4411 (3.9181)	10.1791 (6.9956)	2.0193 (3.9883)	3.4655 (3.7970)	1.9361 (3.1762)
Stack by Agency	Yes	Yes	No	No	No
Stack by State	No	No	Yes	Yes	Yes

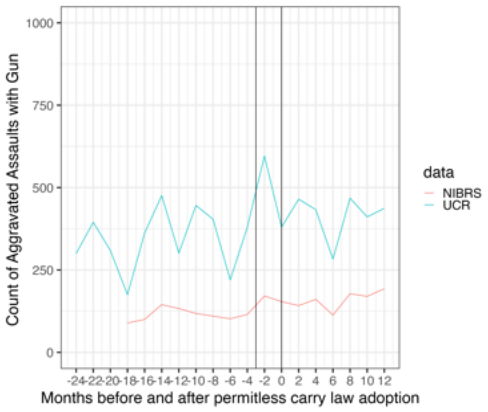
Stack by Year by Month	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	No
Observations	232733	214606	5069	5069	5069
Panel D: UCR SRS Agg.Assault - Non-Gun					
Permitless Carry (months 0 to 12)	5.2280 (3.5902)	2.8472 (3.5906)	6.8888 (4.7414)	3.8977 (3.4750)	12.2882** (4.8964)
Passage (months -12 to -2)	3.8977 (3.4750)	12.2882** (4.8964)	5.2280 (3.5902)	2.8472 (3.5906)	6.8888 (4.7414)
Stack by Agency	Yes	Yes	No	No	No
Stack by State	No	No	Yes	Yes	Yes
Stack by Year by Month	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	No
Observations	386666	344639	6567	6519	6519

Notes: Columns estimate separate difference-in-differences estimators that compare estimates across specifications and aggregation level. Columns 1 and 2 are Poisson fixed effect estimators fit on panel data at the agency-pooled month level. Controls include median household income, percent of households in poverty, proportion male, Black and white population aged 18-29, and lagged police officers per capita. Column 3 is a Poisson fixed effect estimator fit on panel data aggregated to the state level. Columns 4 and 5 are log-linear OLS models where the outcome is the log rate of crime per capita in each state from reporting agencies. Column 5 estimates are weighted by state population. Estimates are presented as a marginal percent change.

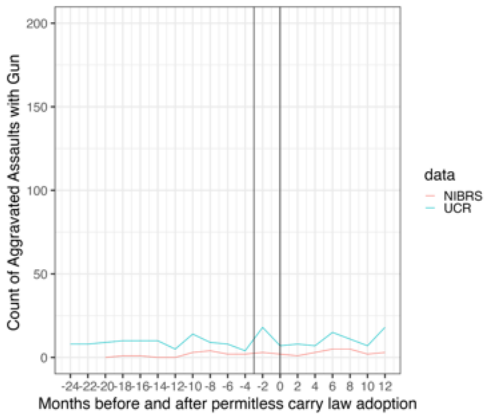
Figures

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

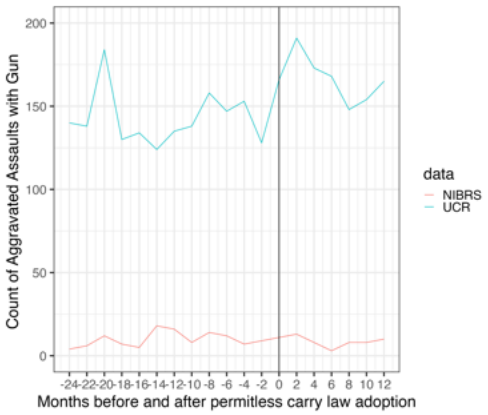
Kansas - 2015**



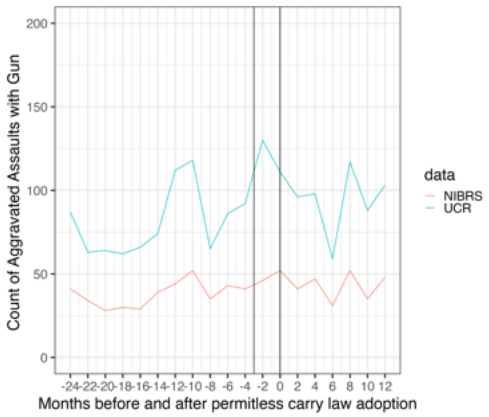
Maine - 2015*



Mississippi - 2016*

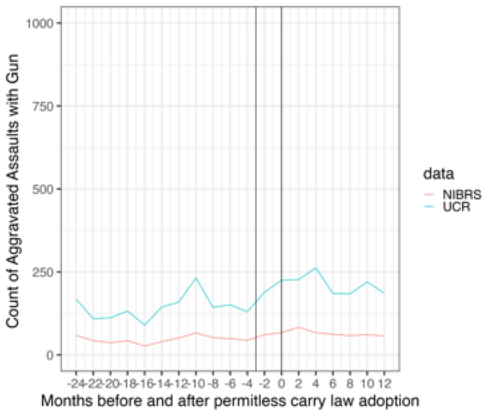


Idaho - 2016*

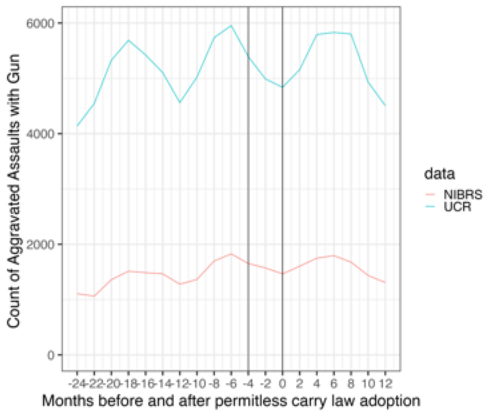


West Virginia - 2016*

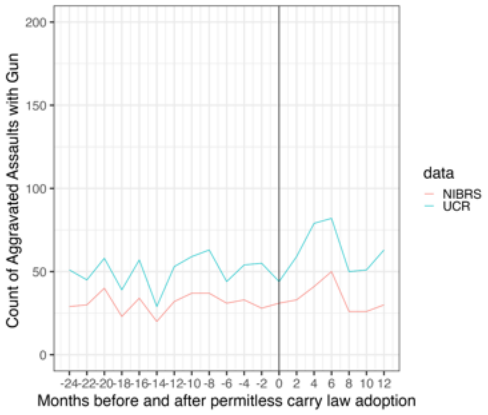
Missouri - 2017***



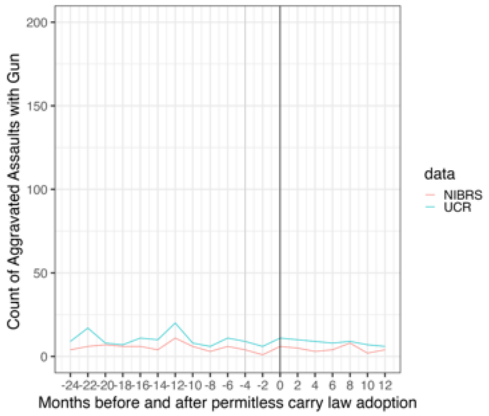
New Hampshire - 2017*



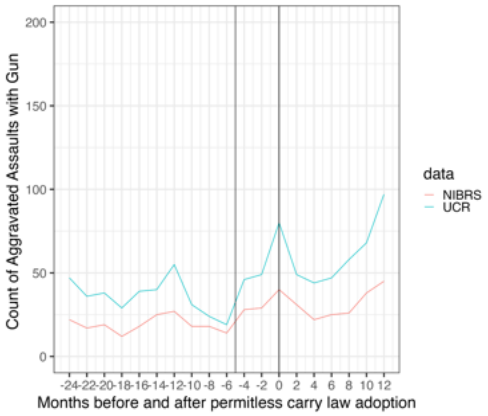
North Dakota - 2017*



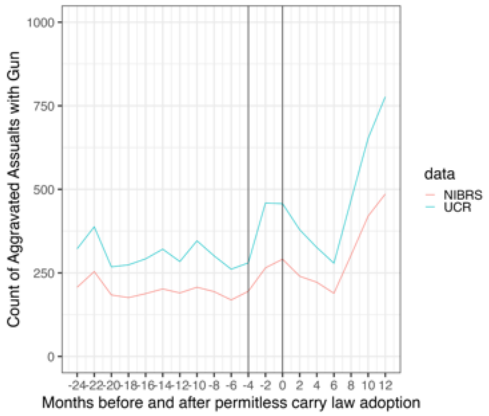
South Dakota - 2019*

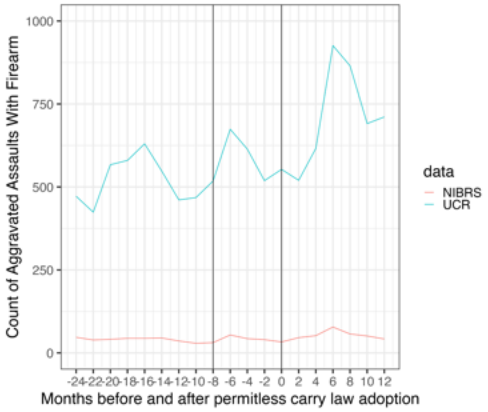


Kentucky - 2019**



Oklahoma - 2019**

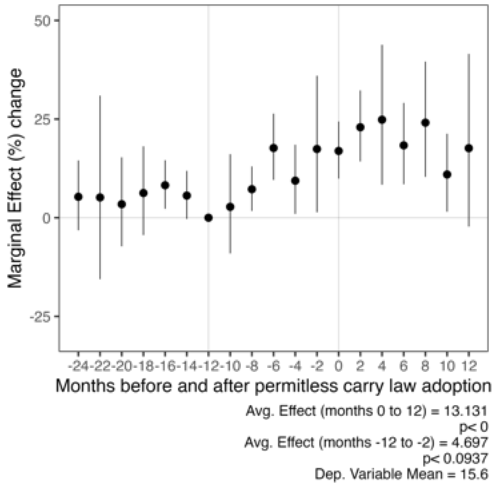




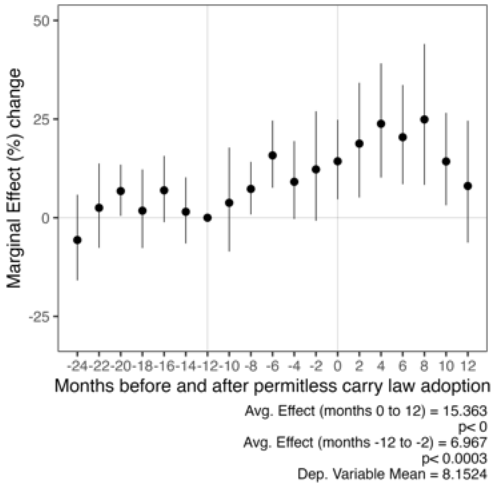
Notes: Counts of pooled 2-month aggravated gun assaults for both NIBRS and SRS samples are plotted. Horizontal reference lines mark the passage of the permitless carry law and the second centered at t=0 corresponds to the implementation date.

Appendix Figure A.2: Effect of Permitless Concealed Carry Laws on Violent Crime, NIBRS, Agencies over 90,000 Population

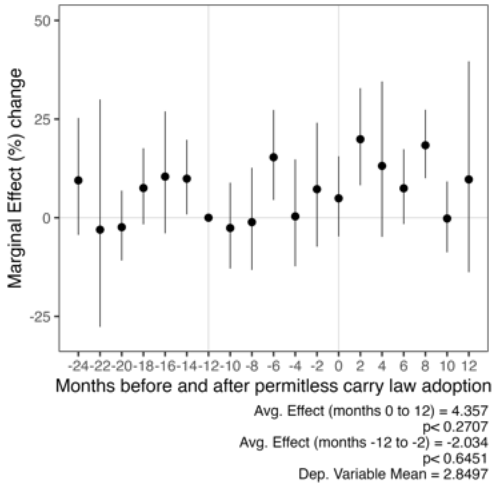
(a) Violent Index Crime



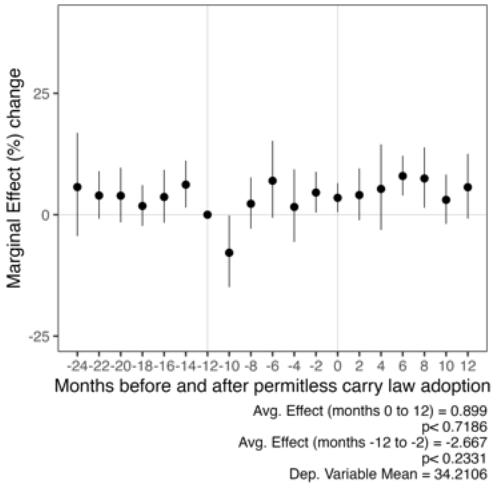
(b) Aggravated Assault



(c) Robbery



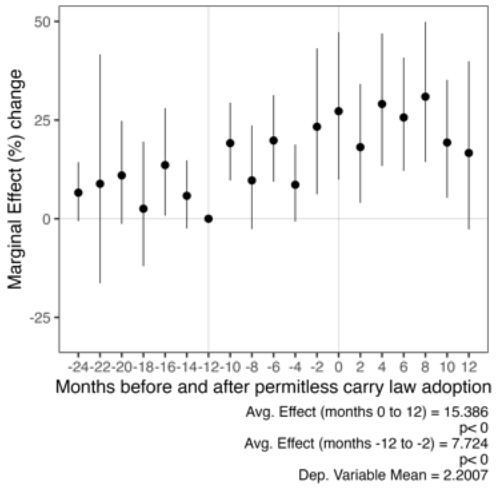
(d) Simple Assault



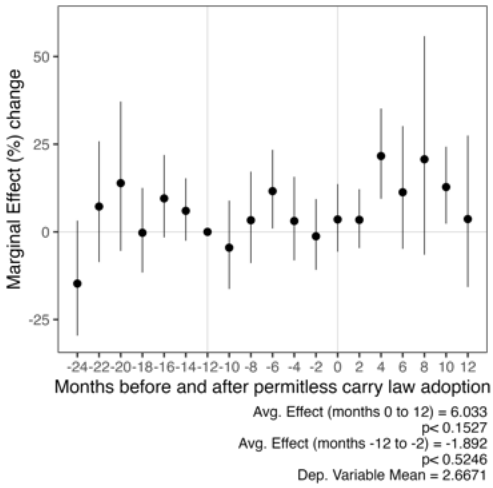
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the volume of crime outcomes in the stacked panel consisting of 27496 observations from 142 agencies serving populations greater than 90,000 pooled 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.

Appendix Figure A.3: Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS, Agencies over 90,000 Population

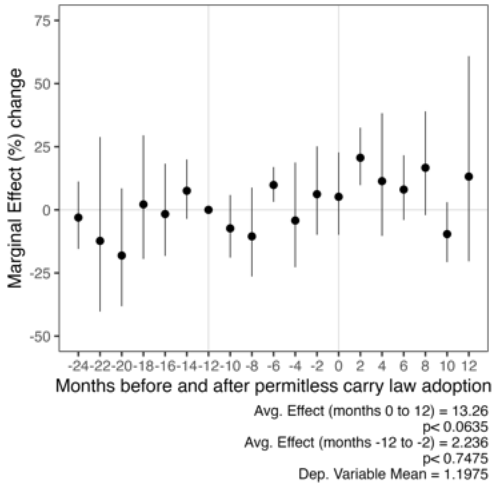
(a) Aggravated Assault - Gun



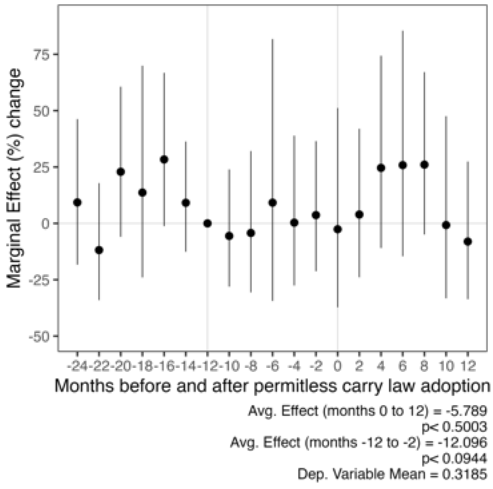
(b) Aggravated Assault - No Gun



(c) Robbery - Gun



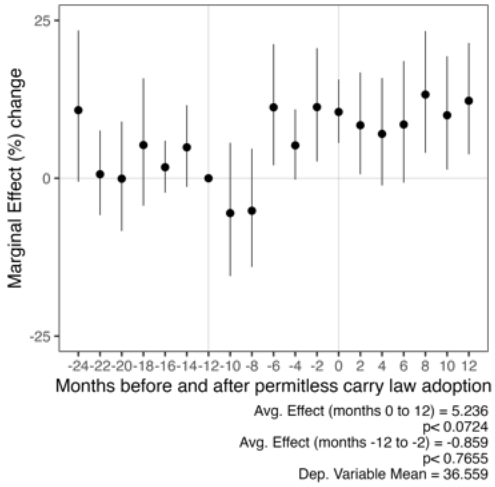
(d) Robbery - No Gun



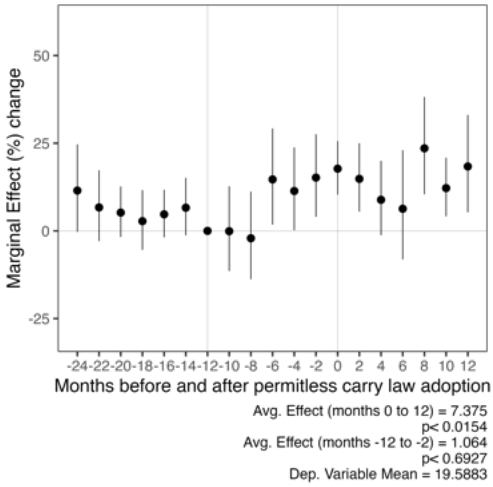
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the volume of crime outcomes in the stacked panel consisting of 27496 observations from 142 agencies serving populations greater than 90,000 pooled 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.

Appendix Figure A.4: Effect of Permitless Concealed Carry Laws on Property Crime, NIBRS, Agencies over 90,000 Population

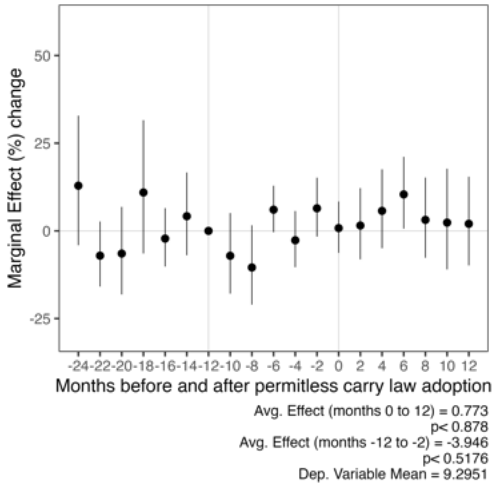
(a) Property Index Crime



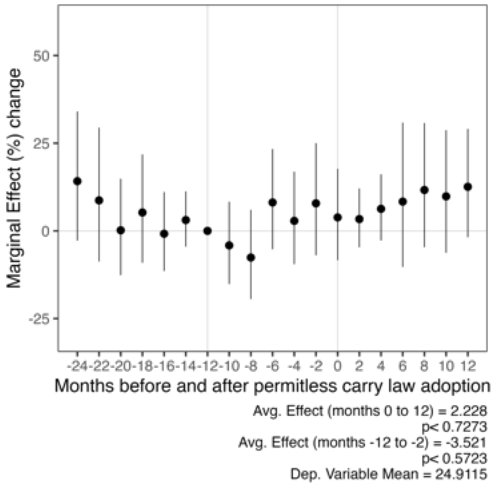
(b) Burglary



(c) Motor Vehicle Theft



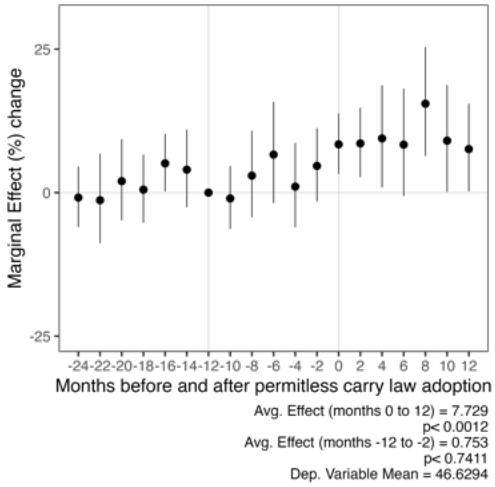
(d) Theft



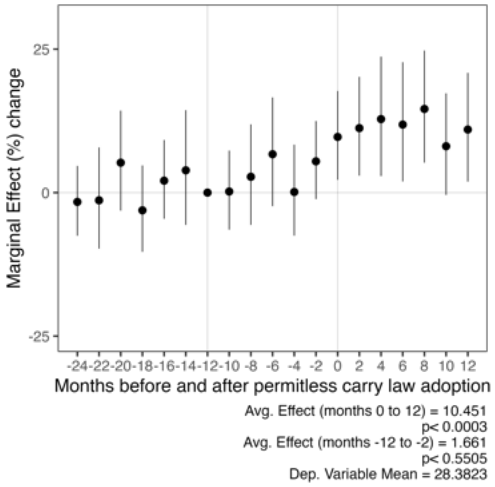
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the volume of crime outcomes in the stacked panel consisting of 27496 observations from 142 agencies serving populations greater than 90,000 pooled 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.

Appendix Figure A.5: Effect of Permitless Concealed Carry Laws on Violent Crime, UCR Summary Reporting System (SRS) Sample

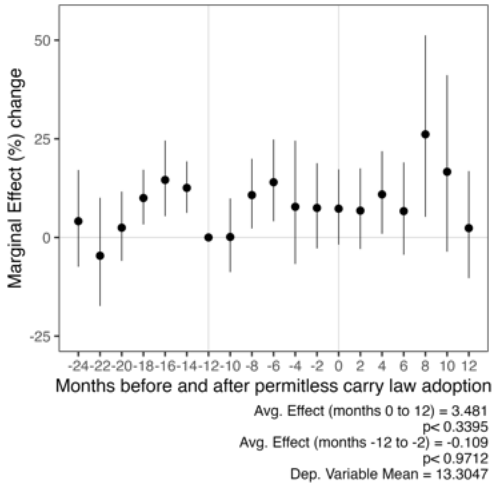
(a) Violent Index Crime



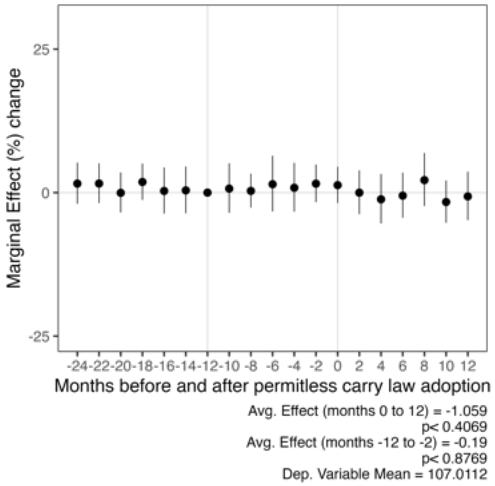
(b) Aggravated Assault



(c) Robbery

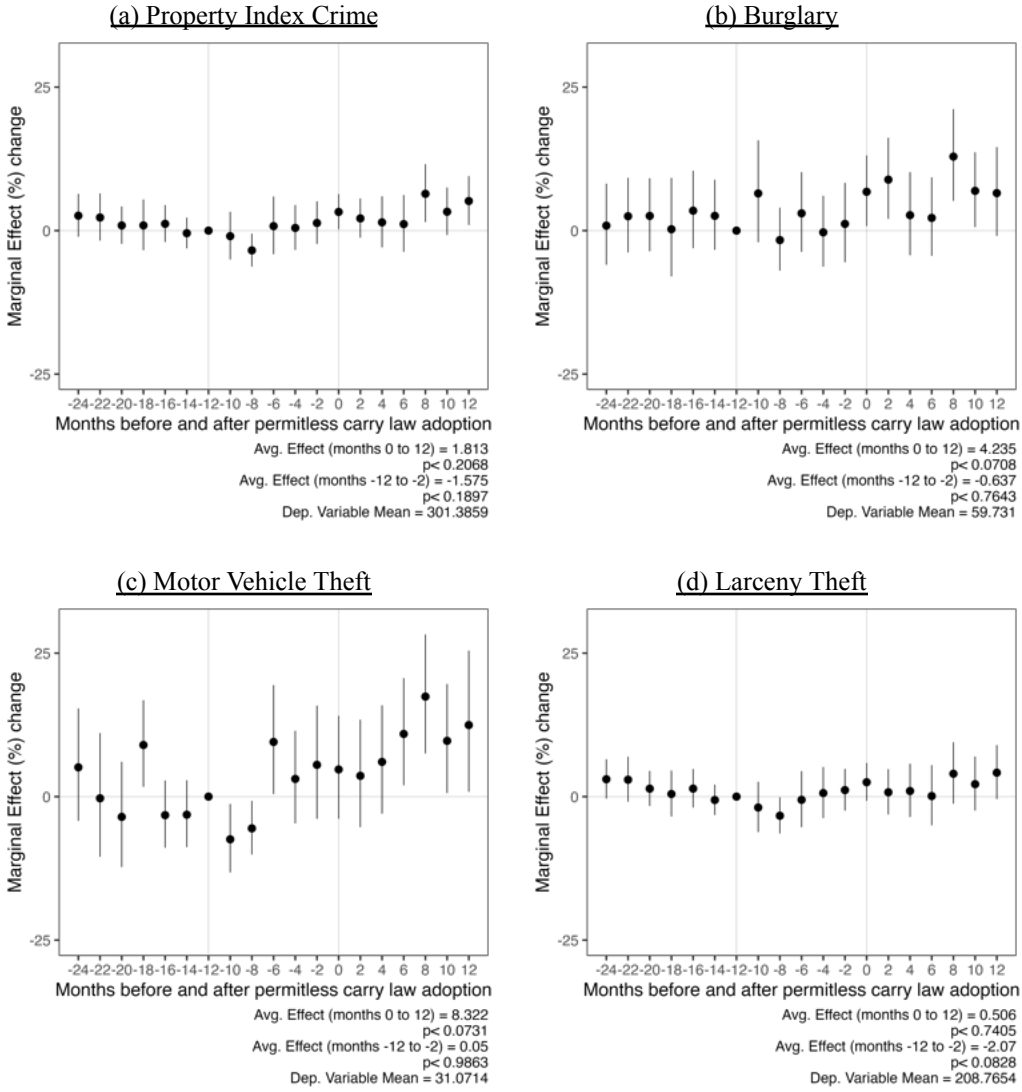


(d) Simple Assault



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 418,308 observations from 2,614 agencies 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.

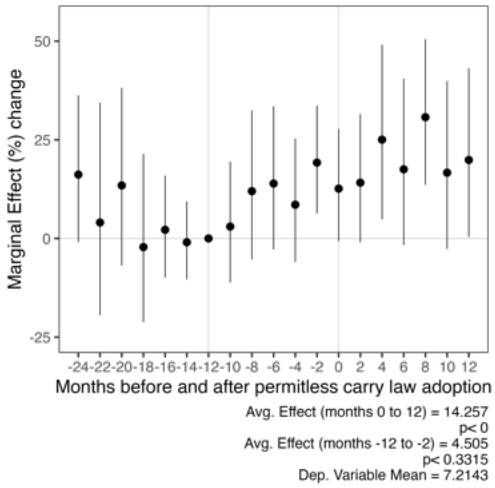
Appendix Figure A.6: Effect of Permitless Concealed Carry Laws on Property Crime, UCR Summary Reporting System (SRS) Sample



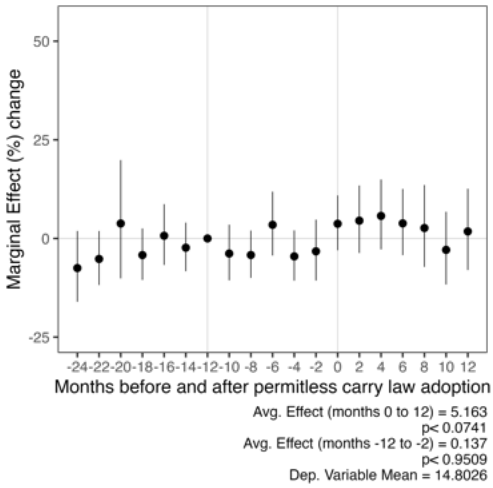
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 418,308 observations from 2,614 agencies pooled into 2-month groups at the agency 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.

Appendix Figure A.7: Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, UCR Summary Reporting System (SRS) Sample

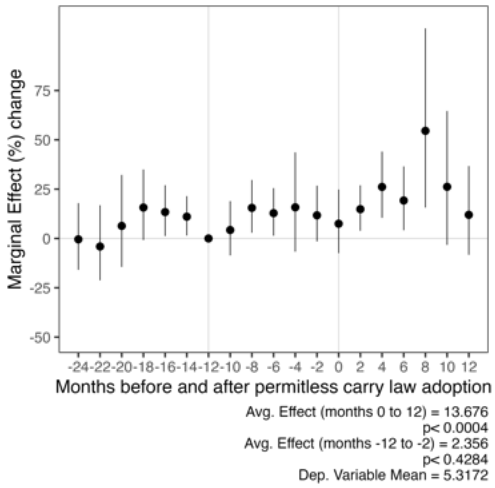
(a) Aggravated Assault - Gun



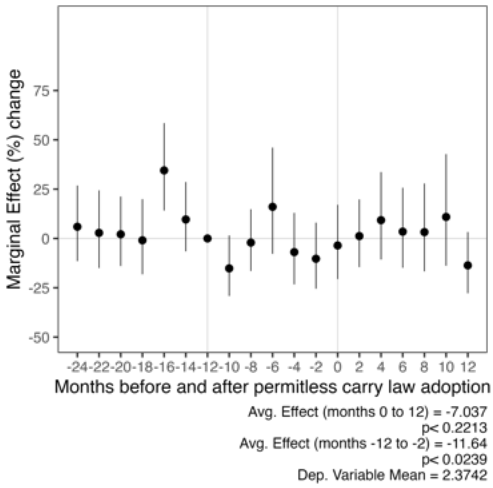
(b) Aggravated Assault - No Gun



(c) Robbery - Gun



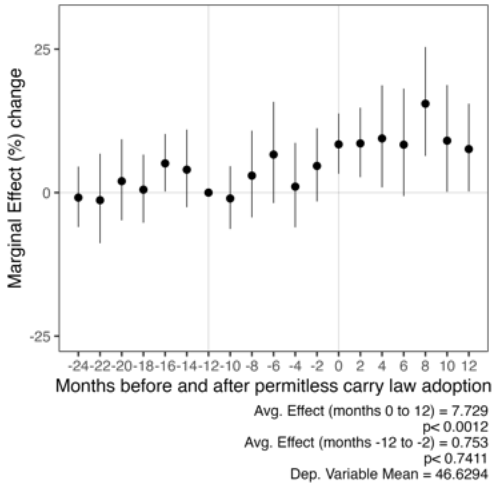
(d) Robbery - No Gun



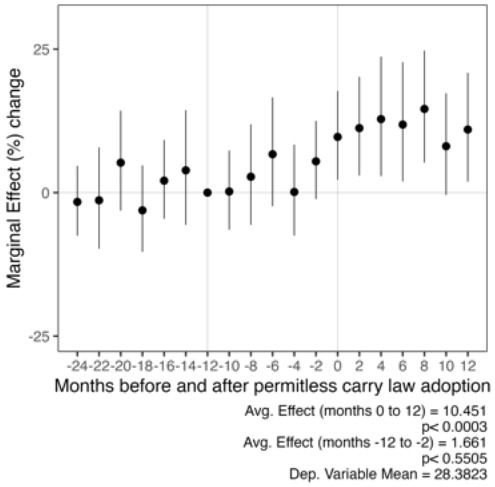
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 418,308 observations from 2,614 agencies pooled into 2-month groups at the agency 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.

Appendix Figure A.8: Effect of Permitless Concealed Carry Laws on Violent Crime, NIBRS Sample with Future Adopting States Comparison Group

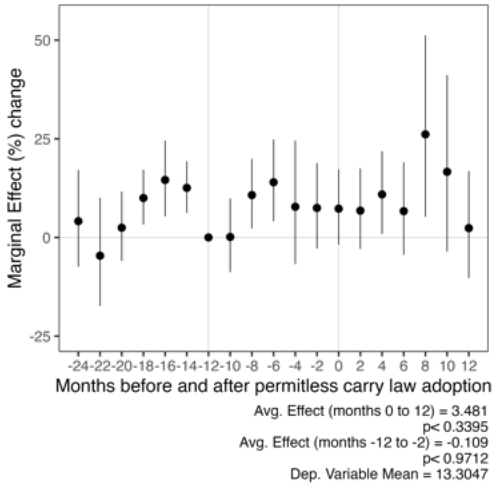
(a) Violent Index Crime



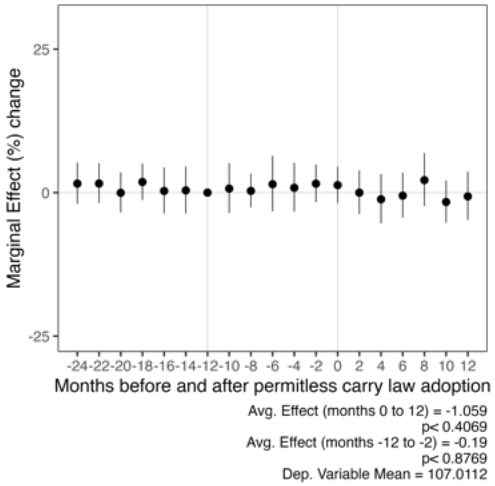
(b) Aggravated Assault



(c) Robbery

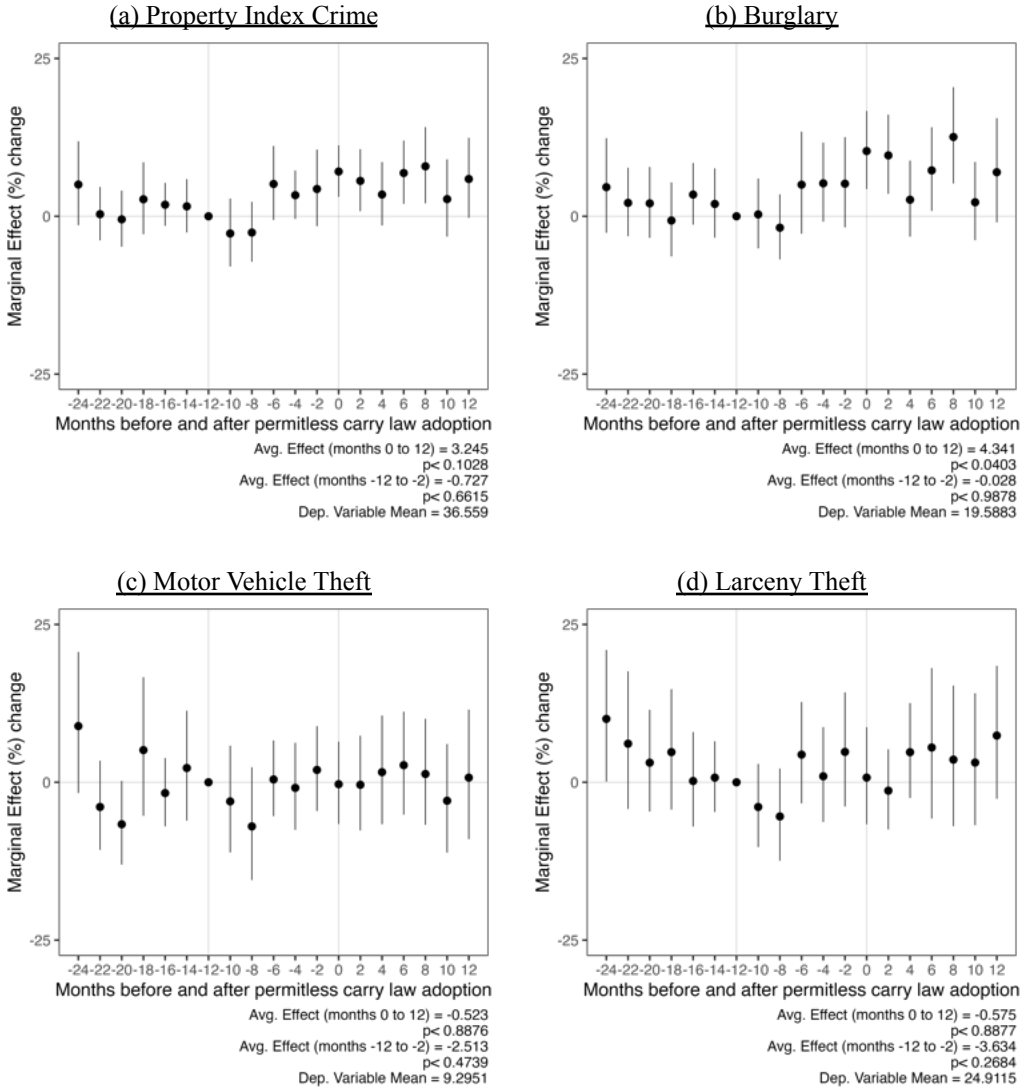


(d) Simple Assault



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 117,980 observations from 899 agencies 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.

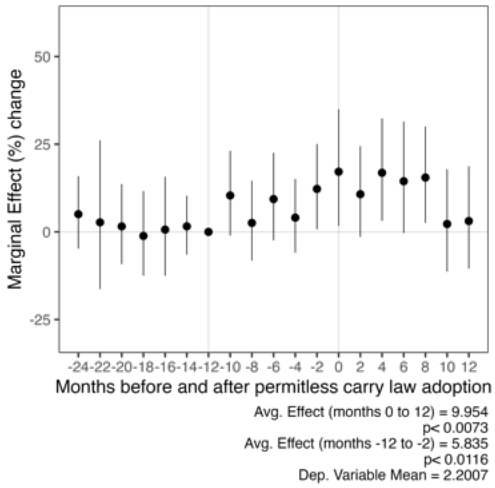
Appendix Figure A.9: Effect of Permitless Concealed Carry Laws on Property Crime, NIBRS Sample with Future Adopting States Comparison Group



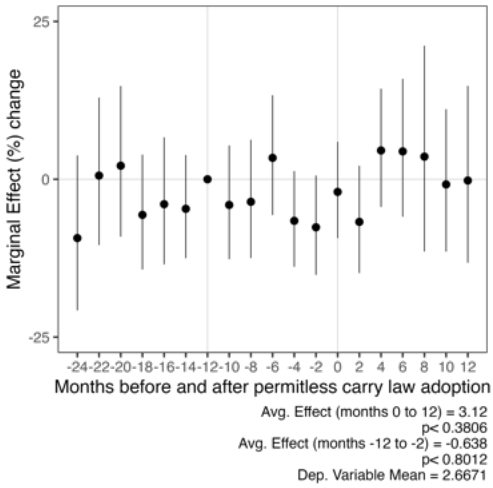
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 117,980 observations from 899 agencies pooled into 2-month groups at the agency 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.

Appendix Figure A.10: Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS Sample with Future Adopting States Comparison Group

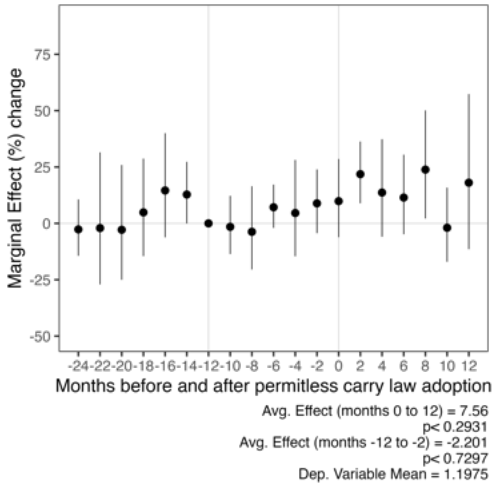
(a) Aggravated Assault - Gun



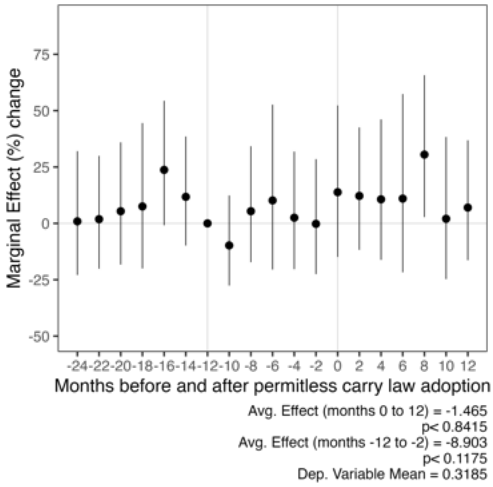
(b) Aggravated Assault - No Gun



(c) Robbery - Gun



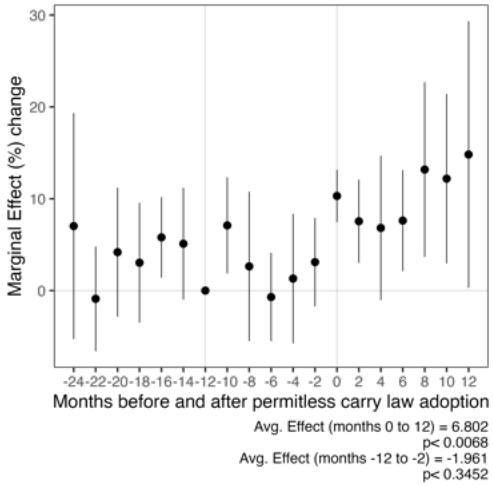
(d) Robbery - No Gun



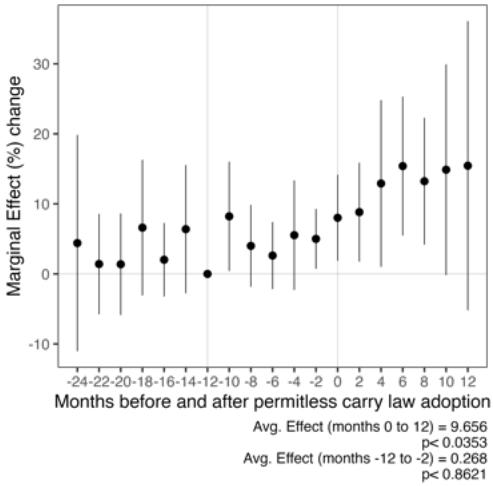
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 117,980 observations from 899 agencies pooled into 2-month groups at the agency 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.

Appendix Figure A.11: Effect of Permitless Concealed Carry Laws on Violent Crime, NIBRS State-Month Log-Linear Model

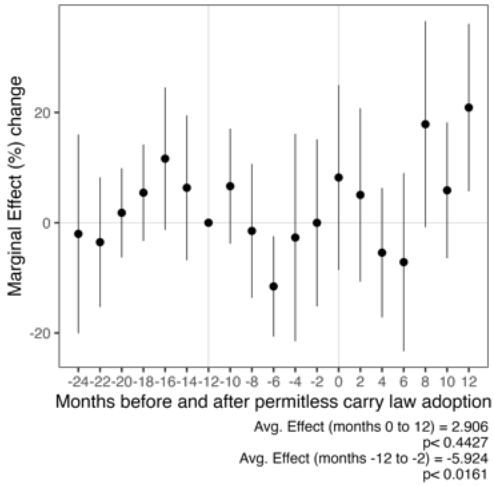
(a) Violent Index Crime



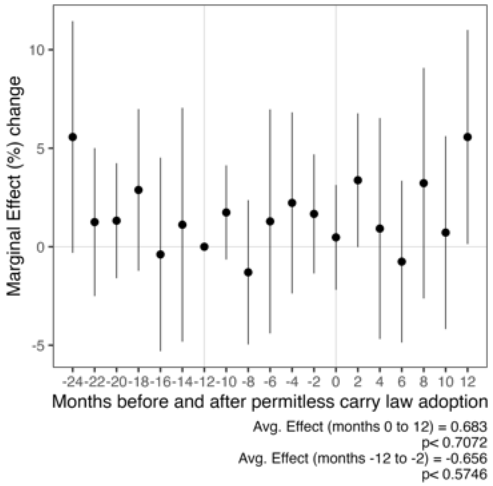
(b) Aggravated Assault



(c) Robbery

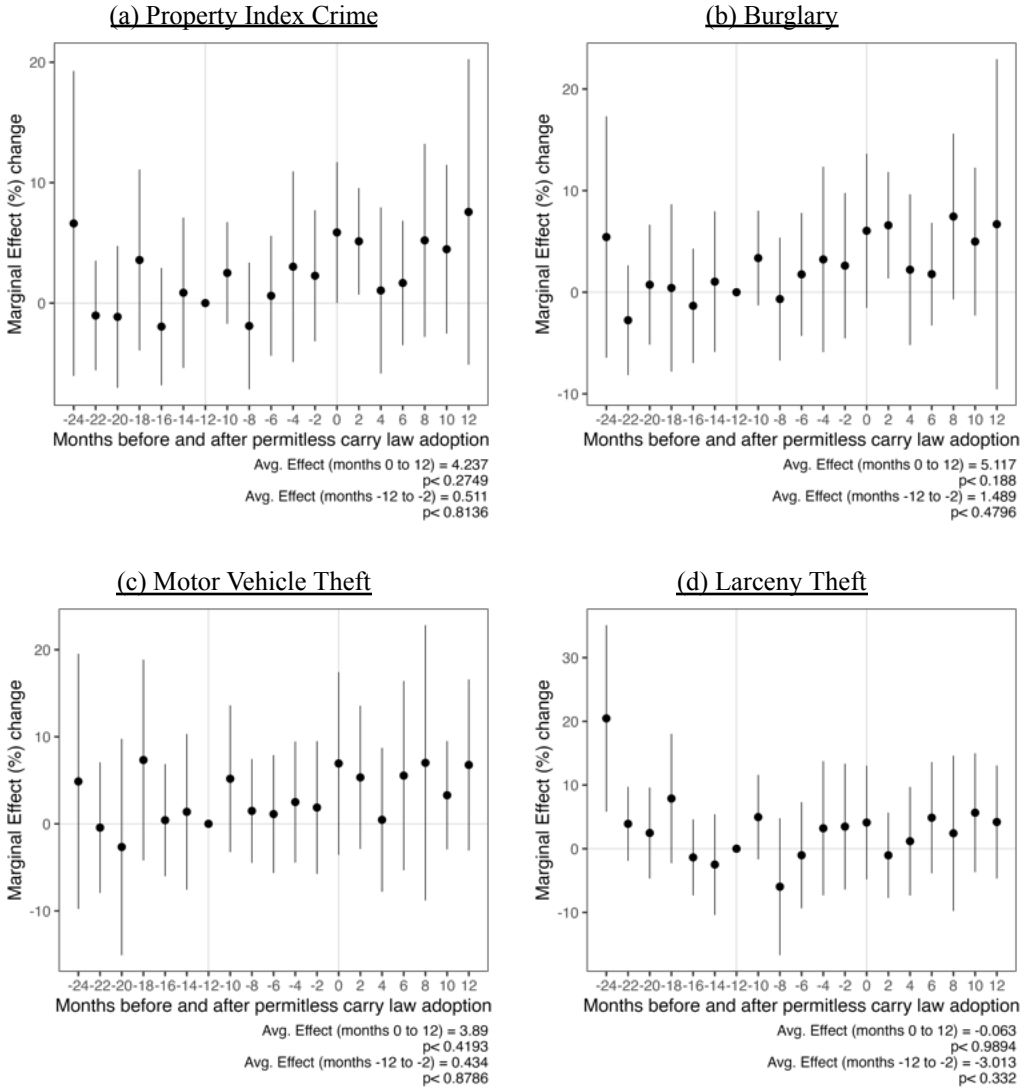


(d) Simple Assault



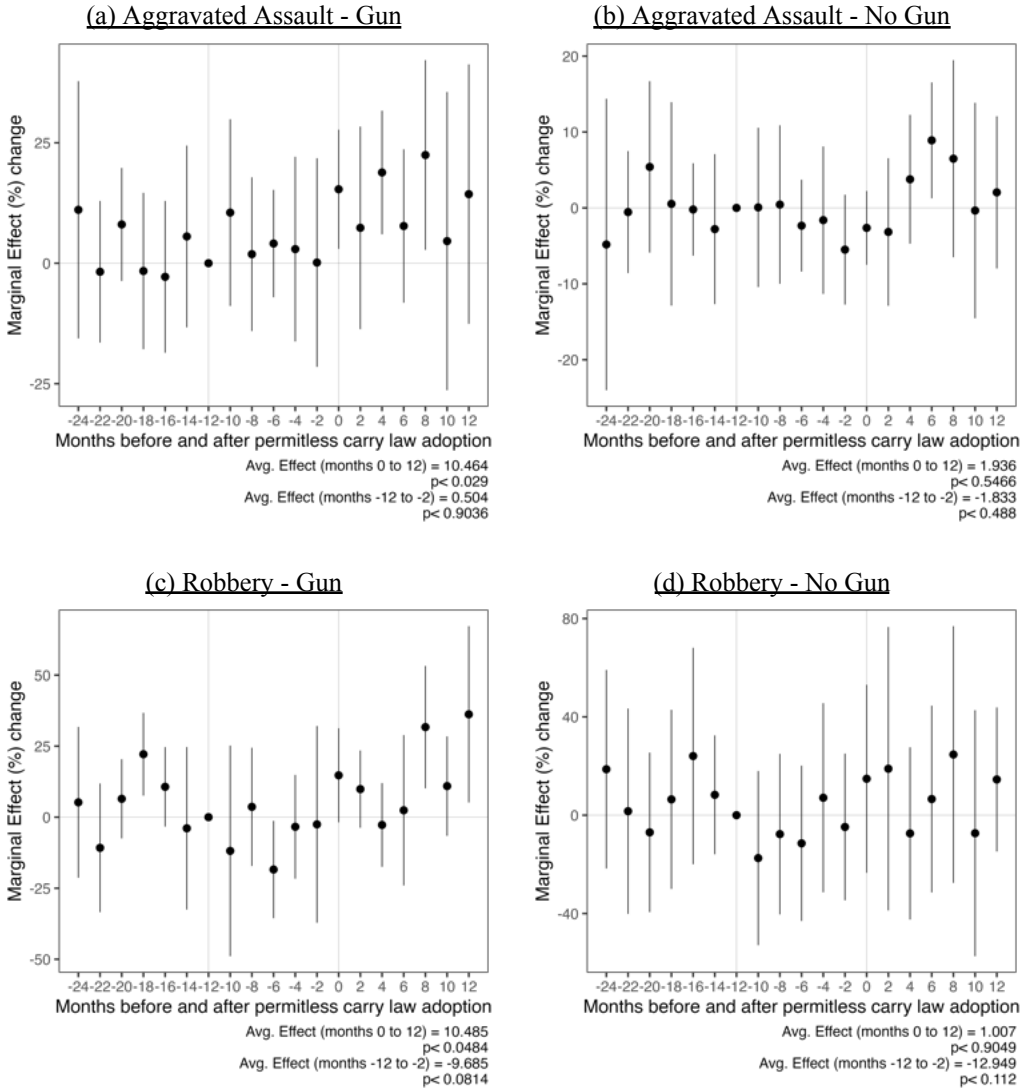
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the log rate of crime outcomes in the stacked panel consisting of 5,069 observations from 32 states pooled into 2-month groups at the state level. Stack by time and state by stack fixed effects are included. Standard errors are clustered on the state. 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.

Appendix Figure A.12: Effect of Permitless Concealed Carry Laws on Property Crime, NIBRS State-Month Log-Linear Model



Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the log rate of crime outcomes in the stacked panel consisting of 5,069 observations from 32 states pooled into 2-month groups at the state level. Stack by time and state by stack fixed effects are included. Standard errors are clustered on the state. 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.

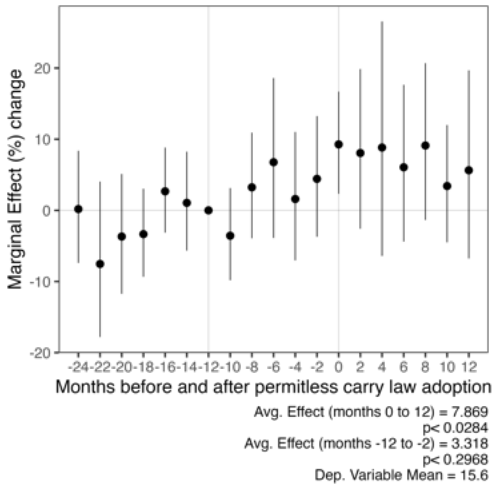
Appendix Figure A.13: Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS State-Month Log-Linear Model



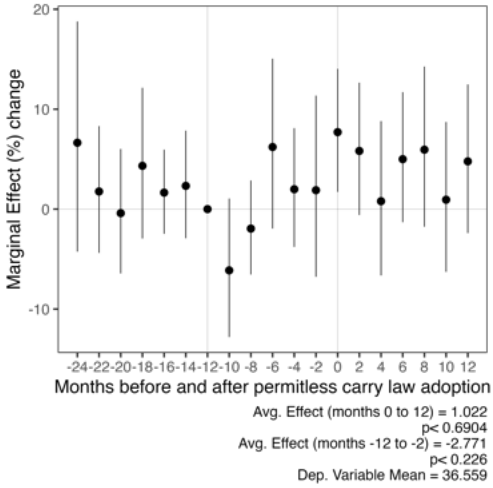
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the log rate of crime outcomes in the stacked panel consisting of 5,069 observations from 32 states pooled into 2-month groups at the state level. Stack by time and state by stack fixed effects are included. Standard errors are clustered on the state. 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.

Appendix Figure A.14: Effect of Permitless Concealed Carry Laws on Crime, NIBRS, Adoption Prior to 2019

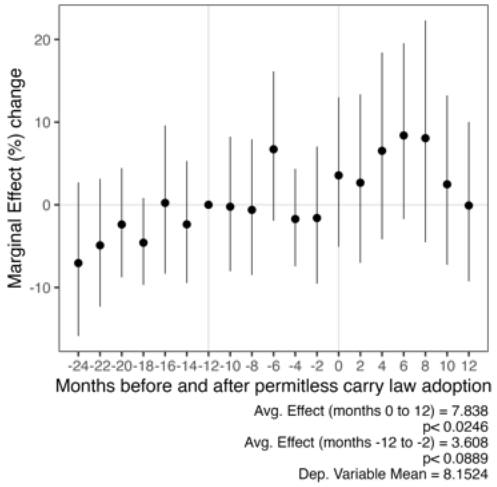
(a) Violent Index Crime



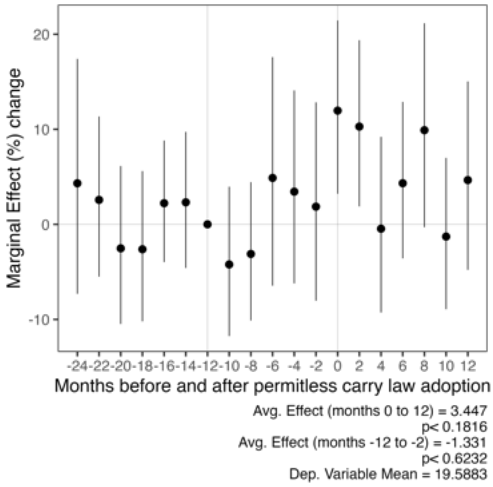
(b) Property Index Crime



(c) Aggravated Assault



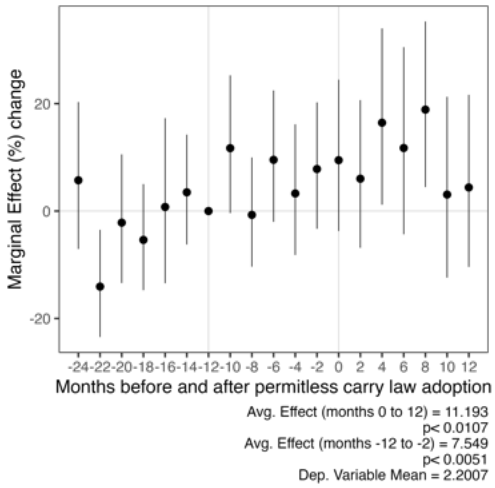
(d) Burglary



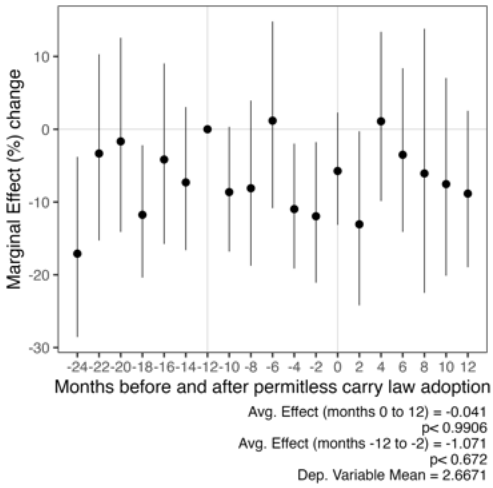
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 183,419 observations from 1,533 agencies pooled into 2-month groups at the agency 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.

Appendix Figure A.15: Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS, Adoption Prior to 2019

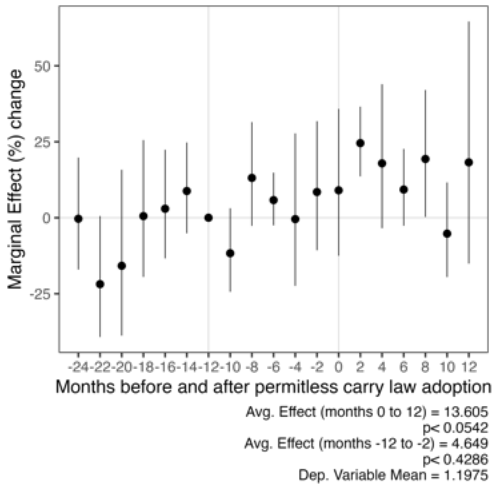
(a) Aggravated Assault - Gun



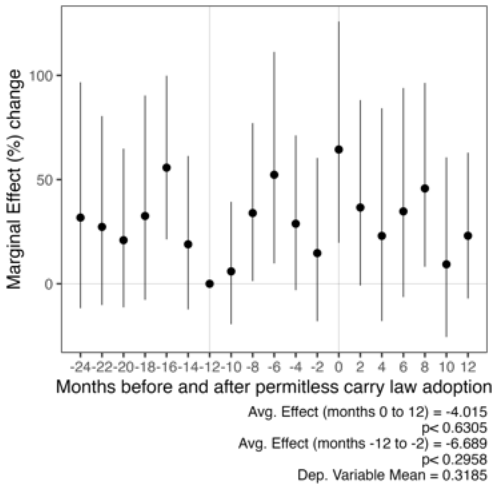
(b) Aggravated Assault - No Gun



(c) Robbery - Gun



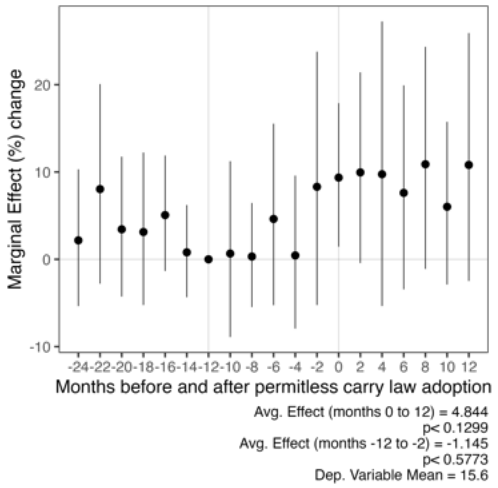
(d) Robbery - No Gun



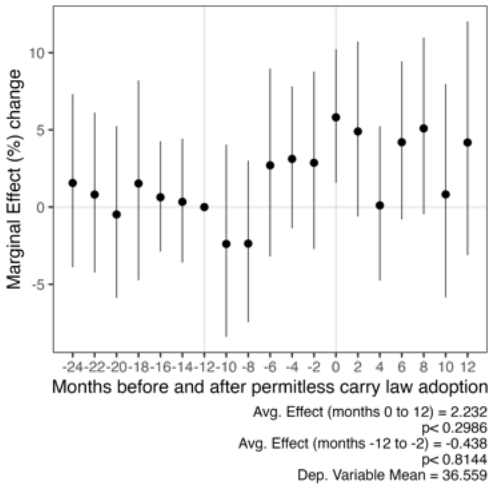
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 183,419 observations from 1,533 agencies pooled into 2-month groups at the agency 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.

Appendix Figure A.16: Effect of Permitless Concealed Carry Laws on Crime, NIBRS, Omit Missouri

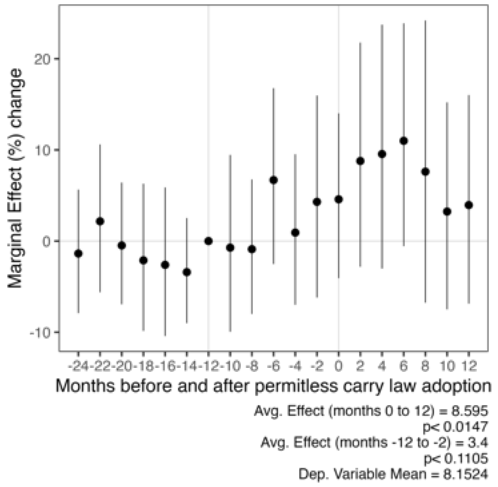
(a) Violent Index Crime



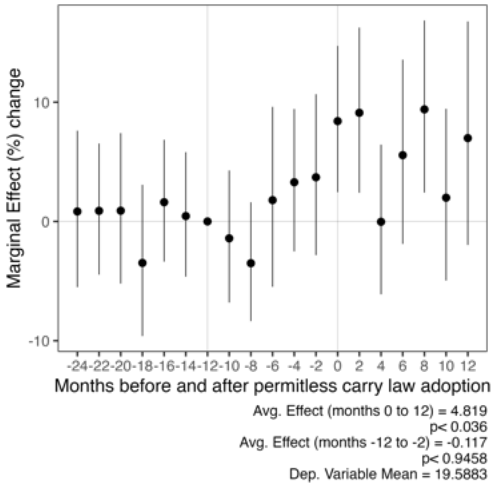
(b) Property Index Crime



(c) Aggravated Assault



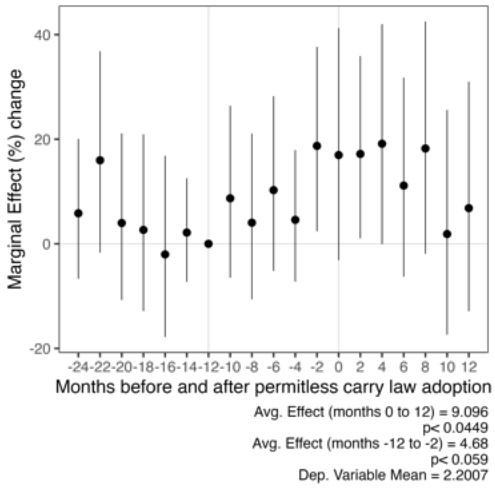
(d) Burglary



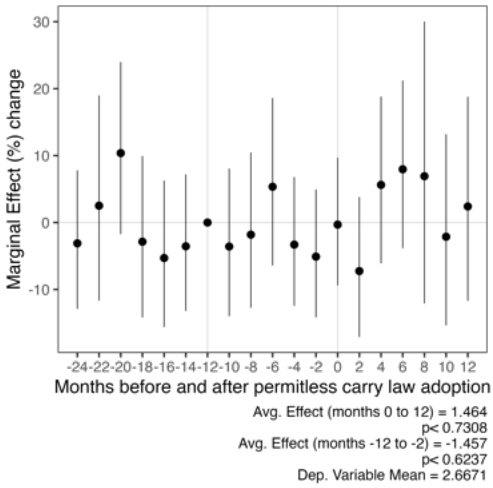
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 210,054 observations from 1,607 agencies pooled into 2-month groups at the agency 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.

Appendix Figure A.17: Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes, NIBRS, Omit Missouri

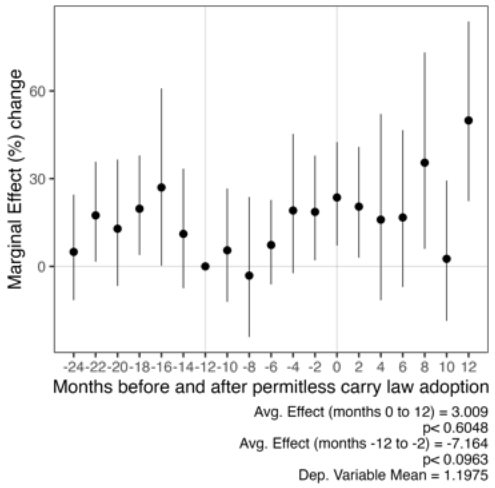
(a) Aggravated Assault - Gun



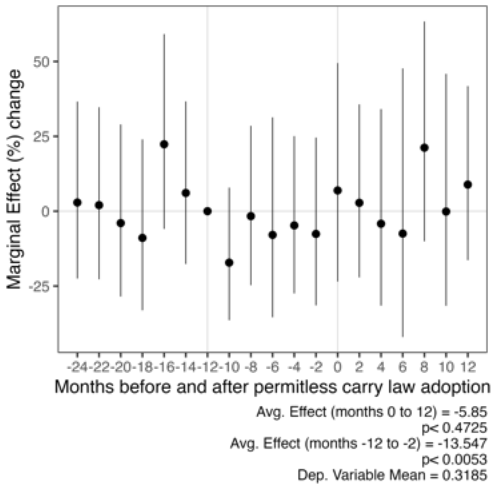
(b) Aggravated Assault - No Gun



(c) Robbery - Gun

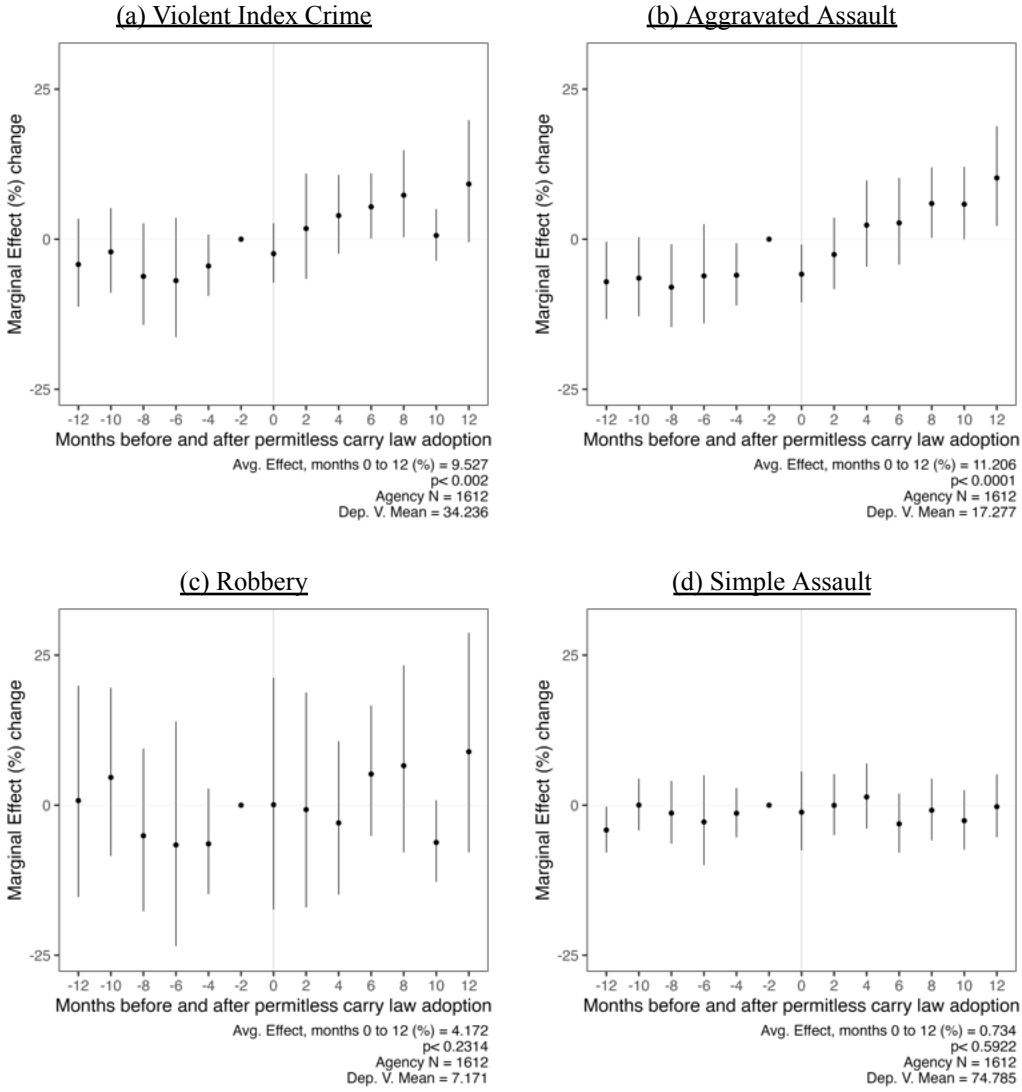


(d) Robbery - No Gun



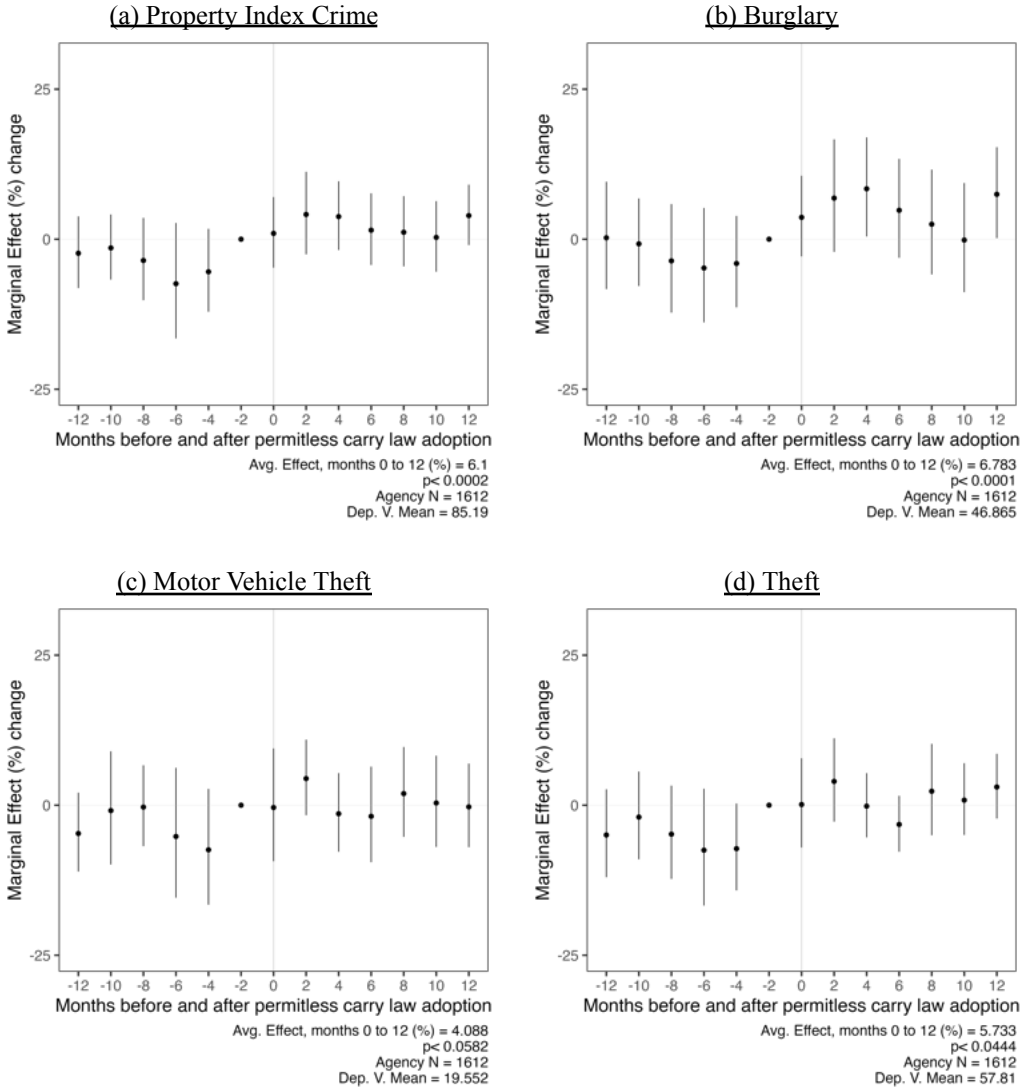
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 210,054 observations from 1,607 agencies pooled into 2-month groups at the agency 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.

Appendix Figure 18: Effect of Permitless Concealed Carry Laws on Violent Crime Relative to Passage Date, NIBRS



Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the volume of crime outcomes in the stacked panel consisting of observations from 1,612 agencies pooled into 2-month groups at the agency level. Data is centered on the passage date of the permitless carry law. 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.

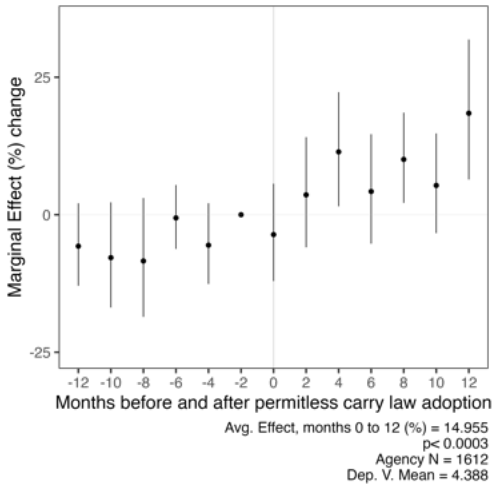
Appendix Figure 19: Effect of Permitless Concealed Carry Laws on Property Crime Relative to Passage Date, NIBRS



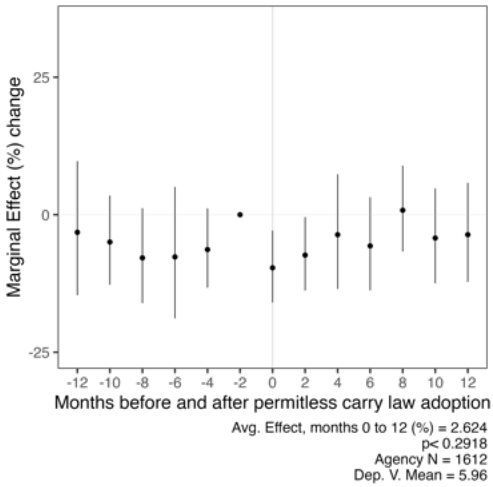
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the volume of crime outcomes in the stacked panel consisting of observations from 1,612 agencies pooled into 2-month groups at the agency level. Data is centered on the passage date of the permitless carry law. 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.

Appendix Figure 20: Effect of Permitless Concealed Carry Laws on Gun vs. Non-Gun Crimes Relative to Passage Date, NIBRS

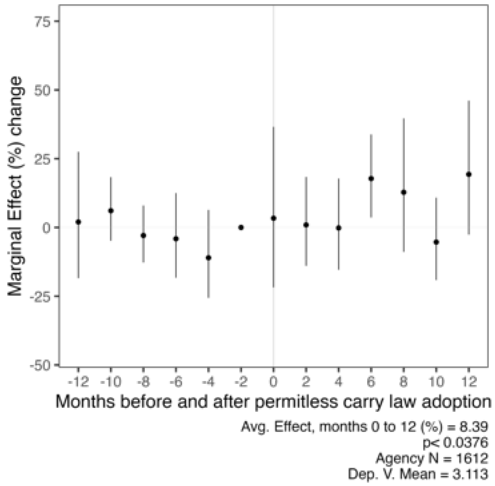
(a) Aggravated Assault - Gun



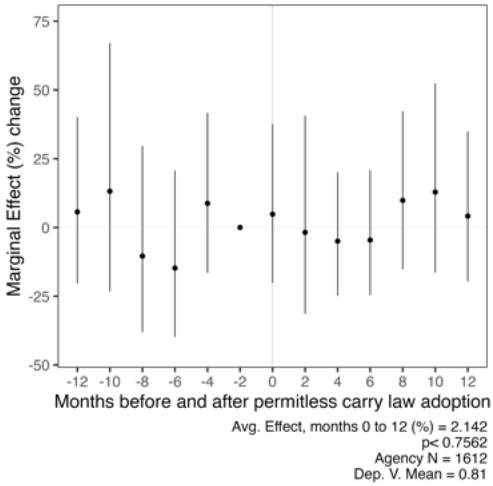
(b) Aggravated Assault - No Gun



(c) Robbery - Gun

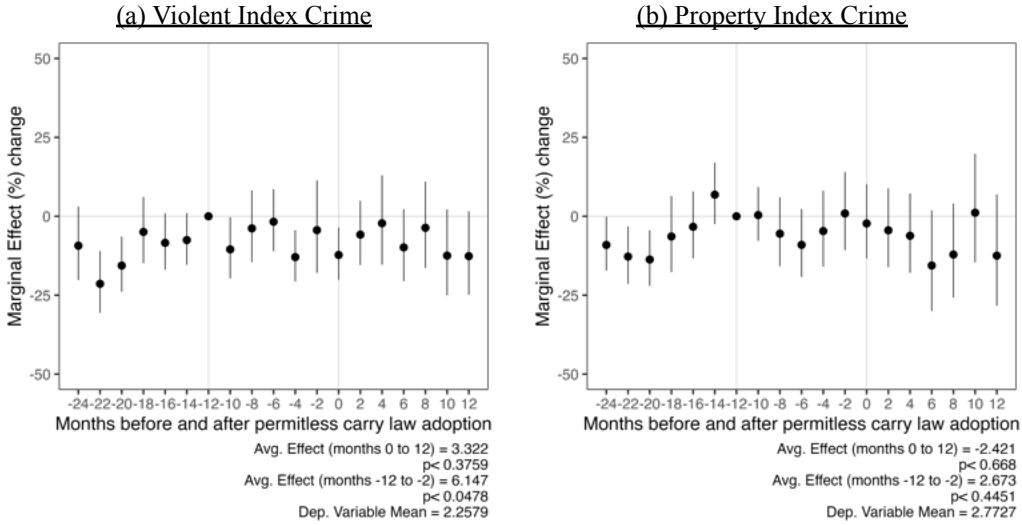


(d) Robbery - No Gun



Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 1 on the volume of crime outcomes in the stacked panel consisting of observations from 1,612 agencies pooled into 2-month groups at the agency level. Data is centered on the passage date of the permitless carry law. 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.

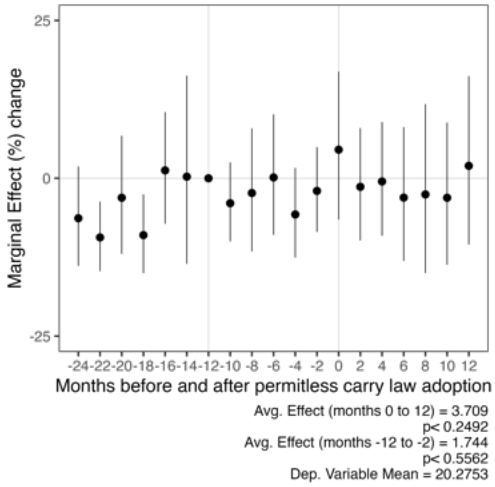
Appendix Figure A.21: Effect of Permitless Concealed Carry Laws on Arrests, NIBRS Sample



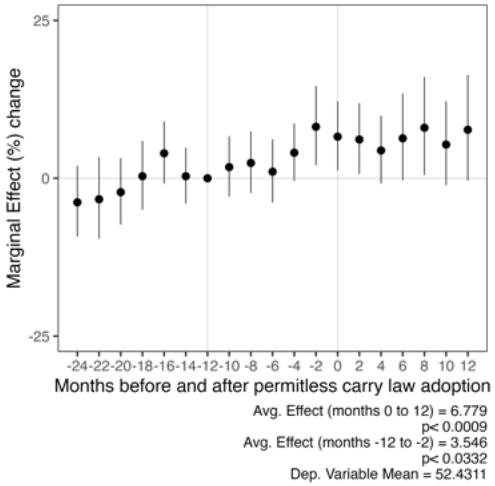
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 232,733 observations pooled into 2-month groups at the agency level. Stack by 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. The panel consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

Appendix Figure A.22: Effect of Permitless Concealed Carry Laws on Crime Clearance, UCR SRS Sample

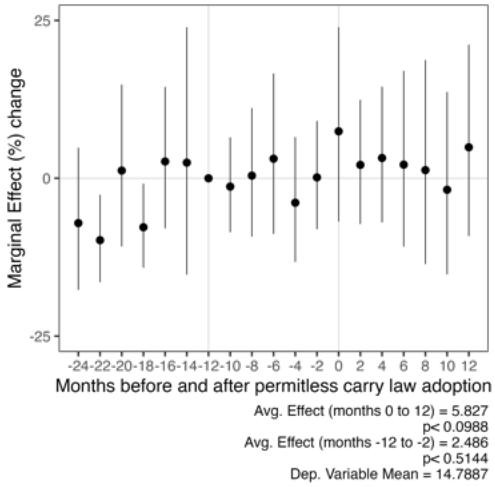
(a) Part I Violent Crime



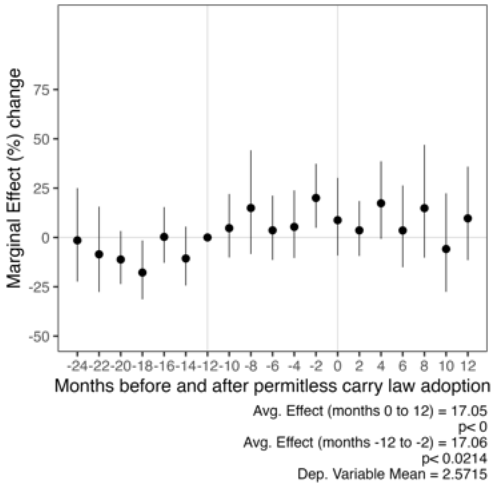
(b) Part I Property Crime



(c) Aggravated Assault

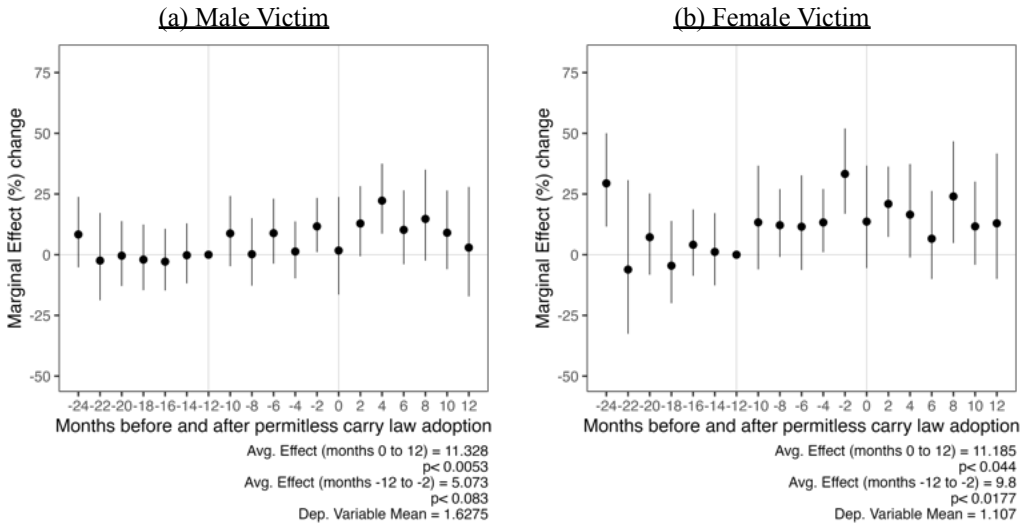


(c) Aggravated Assault with a Firearm



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.

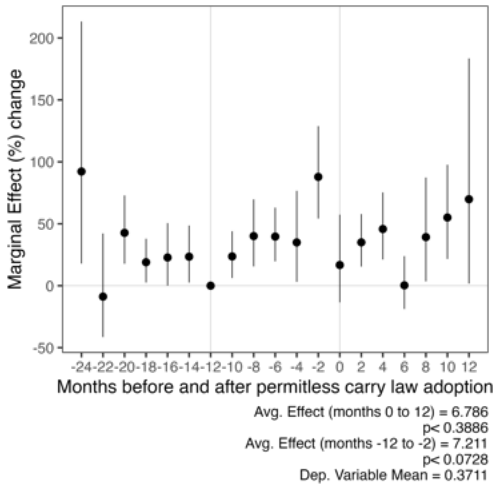
Appendix Figure A.23: Effect of Permitless Carry Laws on Aggravated Gun Assault Victim by Sex



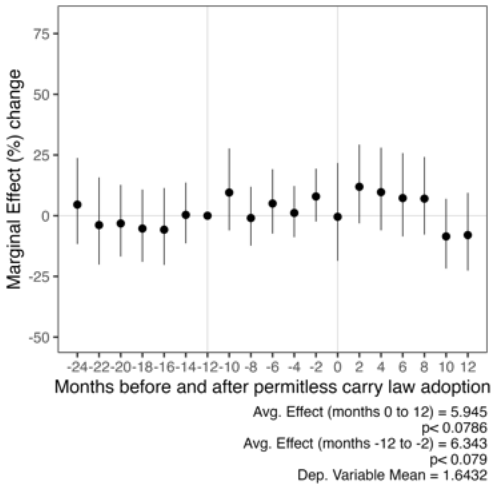
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 232,733 observations pooled into 2-month groups at the agency level. Stack by 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. The panel consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

Appendix Figure A.24: Effect of Permitless Carry Laws on Aggravated Gun Assault Victim by Age Groups

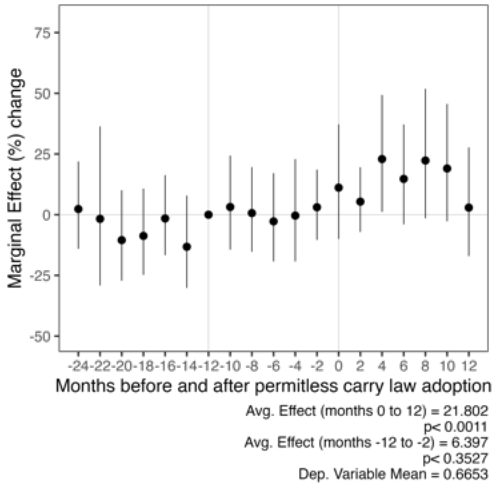
(a) Under Age 17



(b) Age 18-39



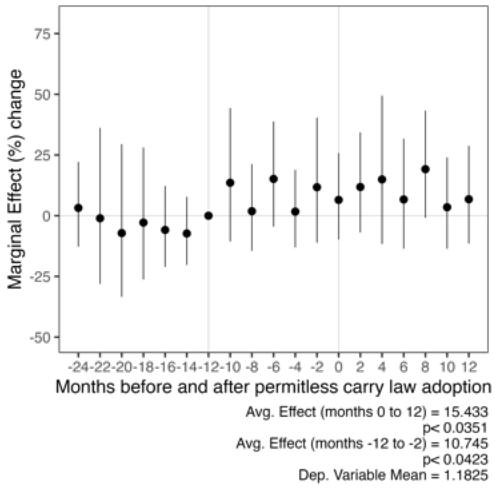
(c) Age Over 40



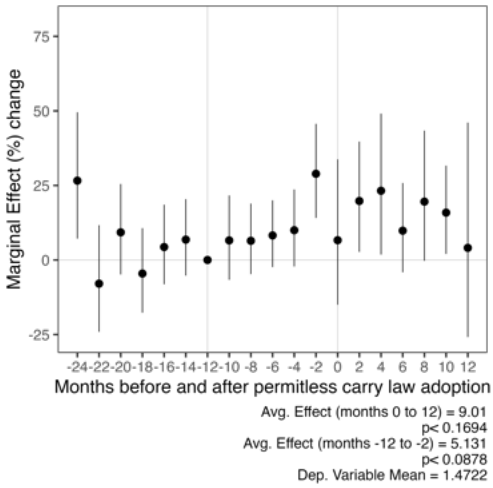
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 232,733 observations pooled into 2-month groups at the agency level. Stack by 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. The panel consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

Appendix Figure A.25: Effect of Permitless Concealed Carry Laws on Aggravated Gun Assault Victim by Race

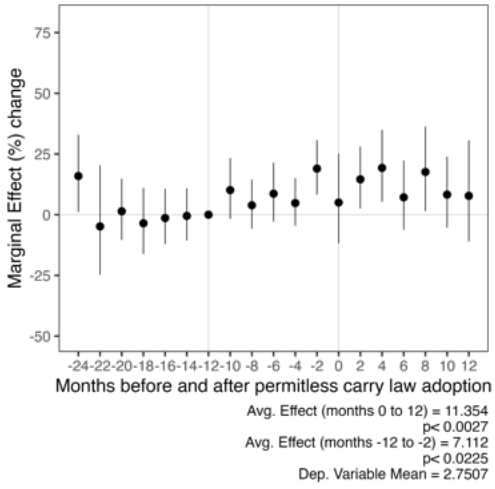
(a) White



(b) AA/Black



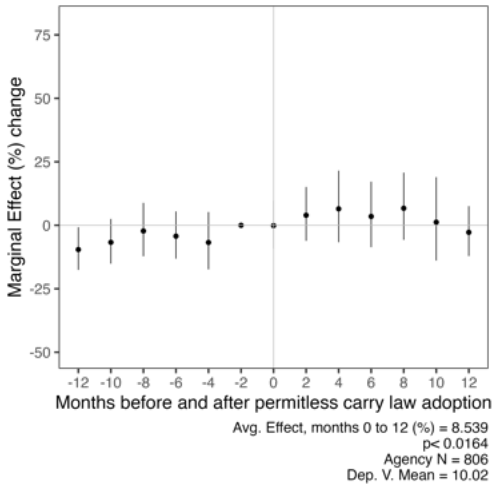
(c) Non-White



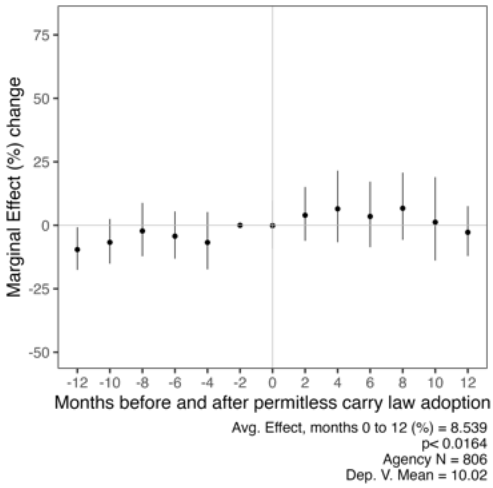
Notes: 95 percent confidence intervals are plotted around the coefficient using the point estimates and standard errors obtained from estimating Equation 4 on the volume of crime outcomes in the stacked panel consisting of 232,733 observations pooled into 2-month groups at the agency level. Stack by 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. The panel consists of 1612 agencies. The comparison group consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

Appendix Figure A.26: Effect of Permitless Carry Law Passage on Stolen Gun Incidents, NIBRS

(a) Full sample



(b) Future Adopters



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. Stolen guns data come from NIBRS and 1607 agencies in the sample that reported a stolen gun incident during the study timeframe. The comparison group for the full sample consists of agencies in states including: AL, CO, CT, DE, IA, LA, MA, MI, MT, NE, OH, OR, PA, RI, SC, TN, TX, UT, VA, VT, WA.

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 (M. D. Maltz & Targonski, 2002). These reporting problems and resulting imputation have been used to substantiate incorrect conclusions around the effects of gun laws.

Given the magnitude of these problems,[7] 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).[8] 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,

[7] 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).

[8] 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.).

campus, university police or transit police by including agencies with a population served greater than zero.[9]

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

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

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.

[9] 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.

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.[10]

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.

[10] 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