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BEYOND *BRUEN*: CAN FIREARM TRAINING REPLACE LOCAL DISCRETION IN CONCEALED CARRY PERMITTING?

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ABSTRACT

The 2022 Supreme Court case *NYSRPA v. Bruen* struck down states' discretion in issuing individuals firearm right-to-carry permits. As the country transitions towards more and more permissive concealed carry regulation, it has remained unclear how permitting processes and requirements affect personal and public safety. Leveraging a novel dataset of state laws spanning 2000- 2022, we find that more stringent concealed carry requirements, such as higher fees or more training hours, do not deter gun owners from obtaining carrying licenses, nor do they alter their behavior substantially enough to impact public safety outcomes including violent crimes, gun theft, or accidental shootings. As such, stricter training requirements are unable to counteract the effects of more permissive concealed carry issuance.

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Beyond Bruen: Can Firearm Training Replace Local Discretion in Concealed Carry Permitting?

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The 2022 Supreme Court case NYSRPA v. Bruen struck down states' discretion in issuing individuals firearm right-to-carry permits. As the country transitions towards more and more permissive concealed carry regulation, it has remained unclear how permitting processes and requirements affect personal and public safety. Leveraging a novel dataset of state laws spanning 2000-2022, we find that more stringent concealed carry requirements, such as higher fees or more training hours, do not deter gun owners from obtaining carrying licenses, nor do they alter their behavior substantially enough to impact public safety outcomes including violent crimes, gun theft, or accidental shootings. As such, stricter training requirements are unable to counteract the effects of more permissive concealed carry issuance.

On June 23, 2022, the United States Supreme Court ruled 6–3 in favor of the New York State Rifle & Pistol Association (NYSRPA), holding that New York was barred by the 2nd Amendment from requiring individuals to show they had a "proper cause" in order to procure a license to carry a concealed weapon outside the home. Besides signaling that "future Second Amendment challenges should be evaluated solely with reference to text, history, and tradition" (Charles, 2023), the landmark *NYSRPA v. Bruen* decision may lead to the replacement of eight states' may-issue permitting protocols with shall-issue mandates, continuing a decades-long trend toward accessible, widespread legal carrying of firearms throughout the United States.

From the 1970s up until the *Bruen* decision, states' approaches to regulating the concealed carry of weapons (CCW) varied along a spectrum. No-issue regimes made it illegal to carry a concealed firearm; Illinois was the last state to abandon this policy officially in 2013, although other states (like Hawaii) have been considered effectively no-issue more recently. May-issue states required applicants to

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satisfy certain requirements of good character, firearm proficiency, or particular need (such as for employment as a private security guard or for self-protection against a violent ex) before the issuing authority (county sheriff, police department, etc.) would grant a permit. Shall-issue or right-to-carry (RTC) states must grant permits to any applicant, unless they are disqualified based on criminal history, mental or physical infirmity, substance dependence, or the like. In other words, may-issue permitting places the burden of proof on the applicant, while shall-issue permitting places the burden of disproof on the authorities. Finally, constitutional (permitless) regimes allow any citizen eligible to own a firearm to freely carry it in all but a few locations, such as polling places, schools, or houses of worship.

The movement in the direction of greater gun carrying corresponds with a growing consensus that this conduct tends to elevate violent crime. Indeed, 19 studies in the last seven years[1] have reached this conclusion, based on evaluations of more RTC adoptions, more years of data allowing for longer panels, and the development of new tools—such as synthetic controls (Donohue, Aneja and Weber, 2019),[2] marginal structural models (Van Der Wal, 2022),[3] and Bayesian methods (Schell et al., 2020).[4] Surveying the entire body of research, the RAND Corporation (2023) has concluded – at its highest level of evidentiary support – that RTC laws increase "total homicides, firearm homicides, and violent crime."[5]

In light of the *Bruen* case and the growing permissiveness of gun laws in the face of concerning social science evaluations of these developments, this paper seeks to identify what can be done to prevent negative firearm-related outcomes, such as gun theft and unintentional injury, when right-to-carry is the new norm. First, we investigate whether firearm training fulfills its intended effects. Curricula are far from standardized: per the survey conducted by Rowhani-Rahbar et al. (2017), topics covered range widely between instructors, but often include proper handling (trigger discipline, how to load and unload the gun, clearing jams) and safe storage (childproofing, theft prevention). More rarely, courses specifically address suicide risk and mental health, or domestic violence; most foster gun enthusiasm and encourage ownership, carrying, defensive gun use, and gun rights advocacy (Hemenway et al., 2017). Next, equipped with this "on paper" or "textbook" understanding, students may progress into acquiring practical experience with the weapon(s) in a supervised range setting.

We observe variation in state legislatures' permit eligibility requirements through

[1] See list in Appendix A

[2] Donohue, Aneja and Weber (2019) find that RTC laws increase violent crime by 13-15 percent after 10 years.

[3] Van Der Wal (2022) finds that "RTC laws significantly increase violent crime by 7.5% and property crime by 6.1%."

[4] Schell et al. (2020) find that firearm homicides increased one year after implementation of RTC laws with probability of .99, but suggest that this effect weakens over time.

[5] RAND Corporation, Effects of Concealed-Carry Laws on Violent Crime, updated July 16, 2024, `https://www.rand.org/research/gun-policy/analysis/concealed-carry/violent-crime.html`. See Table 28.1 of Donohue (2023) summarizing 18 recent papers.

minimum training hours requirements and application fees. More comprehensive mandated training may prevent adverse outcomes through two primary channels: more time-consuming training requirements may deter potentially lower-quality applicants on the extensive margin, while increasing the proficiency of those who successfully obtain concealed carry permits on the intensive margin. To separate these possibilities, we isolate the effects of these two types of permitting requirements on an intermediate measure, the volume of permits issued, to determine the responsiveness of permit demand with regards to administrative barriers. Then, we turn to downstream crime outcomes to assess whether—states where law-abiding armed citizens are more prevalent or more skilled on average see reductions in murder, aggravated assault, and robbery due to the hypothesized "good guys with guns" deterrent effect. Conversely, firearm theft could increase if lax RTC legislation enables negligent handling and storage, as criminals face expanded opportunities, while heightened stakes could cause more encounters to escalate into the aforementioned violent crime categories. Defensive gun use might also increase if would-be victims are armed and prepared or decrease if higher rates of gun carrying preempt criminal attempts altogether.

To construct appropriate counterfactuals for each state that tightens or loosens CCW permitting protocol, we implement the De Chaisemartin and d'Haultfoeuille (2024) estimator (henceforth DCDH), which handles the staggered, non-absorbing, multilevel treatments of changes to minimum hours and licensing fees. We discuss at length the assumptions that underlie DCDH and interpret its abstruse estimand, which capture states' complex and heterogeneous responses. We find that states with stricter training requirements tend to also grant fewer permits, but that this relationship is unlikely to be due to requirements actively deterring gun owners from obtaining concealed carry licenses. Indeed, our DCDH estimator does not indicate any significant trends in the issuance of permits following changes in training requirements for states in a shall-issue regime. Thus, we conclude that training requirements do not have an effect on the extensive margin of concealed carriers; that is, they do not reduce the number of permits being delivered.

We also do not find substantive evidence that training requirements alter gun carriers' competence or behavior. We examine four types of public safety outcomes: rates of several violent crimes from the Uniform Crime Reporting program, defensive gun use from the National Crime Victimization Survey and the Gun Violence Archive, unintentional injuries from the RAND Corporation's hospitalization estimates, and child access from the High School Youth Risk Behavior Surveillance System. While these outcomes are now well-known to react to changes in carry regimes, we do not find any evidence that changes in training requirements *within* a shall-issue regime substantially affect any of our crime or other public safety measures.

This paper contributes to the large literature on firearm and public safety by providing the first quasi-experimental estimates of the effects of firearm training requirements, as well as by estimating their effects beyond gun storage practices. Most studies on firearm training, such as Weil and Hemenway (1992), Hemenway, Solnick and Azrael (1995), Cook and Ludwig (1997), or Nordstrom et al. (2001), use surveys of gun owners to assess whether trained firearm possessors are more likely to store their guns safely (i.e. locked and unloaded), which has been shown to reduce firearm suicides and unintentional injuries (Grossman et al., 2005; Conwell et al., 2002; Cummings et al., 1997). Most results are inconclusive, with little evidence that firearm training leads to safer storage. However, it is possible that *(i)* surveys may fail to capture the causal effect of firearm training requirements because of social desirability bias, selection bias, and other endogeneity concerns linked to the use of observational studies, and *(ii)* the effects of training requirements may extend well beyond the safe storage of guns. By design, surveys of gun owners cannot measure the deterring impact such requirements would have on prospective gun buyers who do not end up purchasing a firearm (the extensive margin), and they cannot measure how better training might affect the behavior of gun owners and its impact on adverse outcomes (the intensive margin). Conversely, existing observational studies of licensing approval rates such as Shapira, Jensen and Lin (2018) draw upon limited states and years and cannot determine whether denials are primarily discriminatory and/or unfounded, or whether they are principled enough to actually improve public safety. In contrast, by zooming out to include a broader range of outcomes, the difference-in-differences approach of this paper estimates the global effect of concealed carry training requirements on public safety through several causal channels, both direct and indirect. Moreover, while Depew and Swensen (2019) examines recent local crime as a motivating factor in permit applications, there exists little research on the opposing force of the administrative burdens of the application process. Finally, we contribute to the large literature on the effects of right-to-carry laws (see, e.g., Donohue et al. 2022; Schell et al. 2020*a*; Donohue, Aneja and Weber 2019 for recent examples) by highlighting the mediating role that training requirements, both in hours and fee, may play in the effects of such policies.

Considering that about 61% of gun owners currently receive formal firearm training (Rowhani-Rahbar et al., 2017) and the sizable economic costs associated with them, assessing the relevance and desirability of such programs is of considerable importance for future firearm policies.Our study provides suggestive evidence that current efforts to limit negative firearm-related outcomes have relatively little impact. Most importantly, it appears that stricter training requirements cannot substitute nor compensate for changes in licensing regime.

The remainder of this paper is organized as follows. In section I, we present our two novel datasets and covariate sources. Section II presents associations between training requirements and permitting volume. In section IV, we describe our casual strategy to leverage changes in training requirements, benchmark it against stacked regression, and summarize our estimated effects on permit issuance and downstream public safety outcomes. Section V concludes.

I. Data

Very little centralized data exists on gun ownership, carrying, or usage in the United States. There is no national firearms registry, and surveys of individuals and households who own guns differ widely in their estimates of prevalence. Homicides, aggravated assaults, and robberies committed with a gun appear in the FBI's National Incident-Based Reporting System (NIBRS) and its predecessor, the Uniform Crime Reporting (UCR) program, alongside unlawful possession and gun theft. Current best practice involves piecemealing together many disparate datasets and proxying for values of interest. To this end, we introduce two novel datasets that provide concrete numbers where before data did not exist and had to be approximated with existing already messy data or simply ignored. First, we use various legal databases to compile all state-level changes to the requirements to obtain a concealed carry permit since 2000. Second, we collect CCW permit issuance numbers from 30 states over the same period through public record requests. Taken together, these contributions not only allow us to examine the impact of states' concealed carry training demands on permitting in the current paper, but significantly advance researchers' ability to separate trends in lawful firearm access from trends in overall crime or criminal firearm activity using "upstream" variables.

A. Training Requirements

We hired Stanford Law students to parse concealed carry permit statutes available through Westlaw, Lexis, and/or state legislature websites for the years 2000-2023. In each year that a law change went into effect, they recorded the minimum legally-mandated hours of firearm training as well as the presence of live-fire and accuracy demonstrations for those with a positive number of minimum hours. Furthermore, the permissiveness of the law around online curricula took on one of three values: online learning could be explicitly allowed, explicitly disallowed, or not mentioned explicitly—which must be distinguished from the other two categories as this may indicate either a lack of supply or an implicit assumption that online classes are comparable to in-person, as in the explicit approval case.

We reviewed their work and augmented this dataset with several additional state-level measures derived from the students' findings. First, we uncovered considerable variations in the cost of applying for or renewing a permit, and thus recorded changes in these financial barriers well. We also added an indicator for whether the National Rifle Association was mentioned by name as an acceptable sponsor or certifier of instructors, given the group's significant lobbying power in relaxing states' laws. Finally, we recorded an indicator for whether the recognition of what constitutes a minimal curriculum can be delegated to another state regulatory agency or left up to individual sheriffs' offices entirely. This simultaneous review-and-expansion concluded an equally extensive and detailed data collection

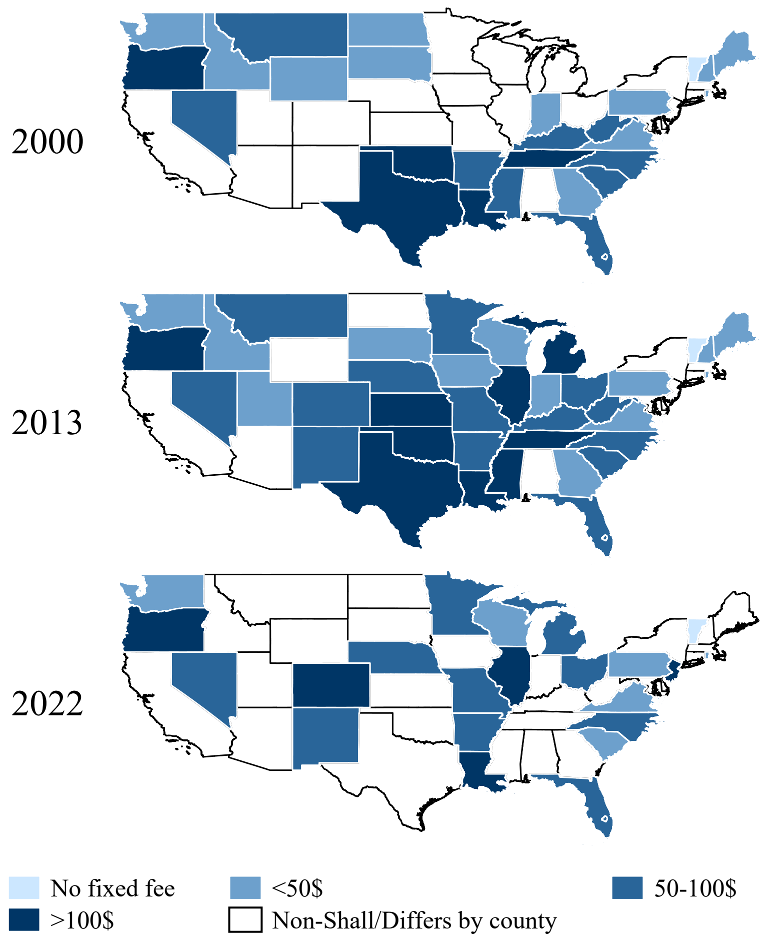

Figure 1. CCW permit fee

Notes: "No fixed fee" states did not have specific fee barriers to licensing in those years. Alaska is omitted for plotting purposes but enters the no fixed fee category.

process. More details and all choices regarding ambiguous training specifications are carefully outlined in Section A of the Online Appendix.

As shown in Figures 1 and 2, there exists meaningful geographic variation in the most stringent training requirement observed in each state. In 2022, fees range from as little as $5 in South Carolina to as much as $200 in New Jersey, while hours range from no fixed number of hours in most states to 16 hours in New York, Maryland, and Illinois. We also observe some temporal variation: for instance, Texas went from a fee of $140 and 10 hours of training in 2000 to 4 hours

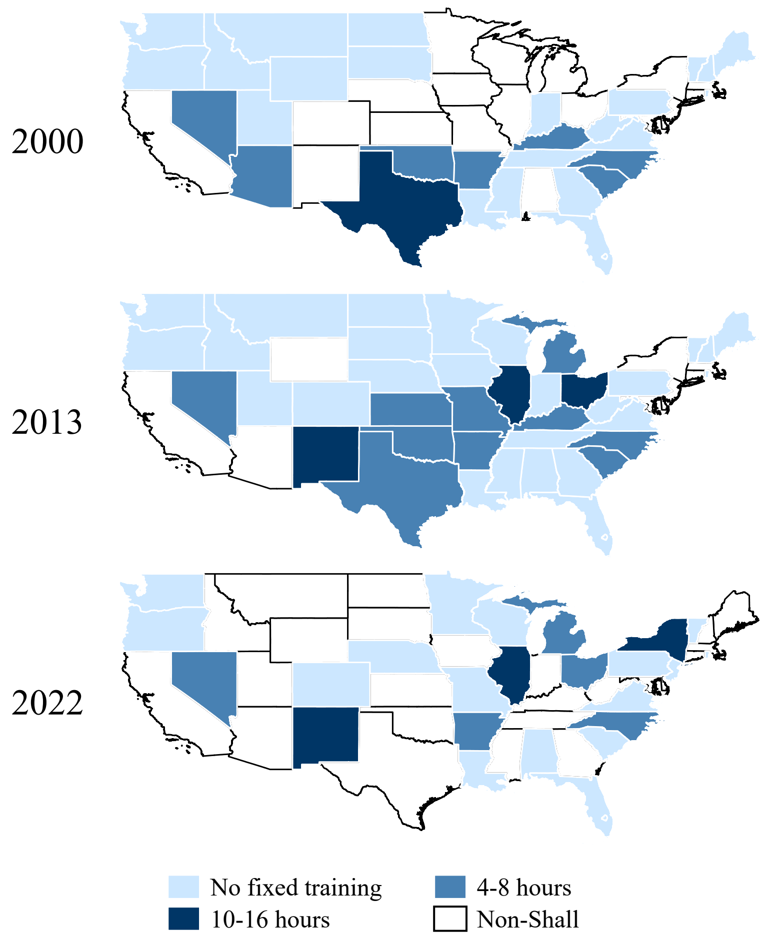

Figure 2. CCW training requirements

Notes: "No fixed training" states did not have specific hours barriers to licensing in those years. Omitted for plotting purposes, Alaska always has no fixed training and Hawaii is always non-shall-issue.

in 2013, then decreased both to $40 and 5 hours in 2017, while finally switching to a constitutional carry regime in 2021. While the most significant change in most states is to pivot in and out of a shall-issue system, a small number of states introduced variations in training requirements while maintaining a shall-issue regime. These space-time variations in training requirements form the basis of our identification strategy.

Specifically, we observe four changes in minimum training hours required over our panel. They are all decreases: Texas moves from more than 8 hours to 8 or fewer hours required in 2013, with Ohio following suit in 2014. South Carolina moves from 8 or fewer hours to no minimum length of training in 2014, and Missouri does the same in 2016. We observe seven changes in permitting fee—four increases and three decreases. Ohio increases its fee in 2012 from less than \$50 to \$50-\$100, while Michigan (2003), Mississippi (2004), and Colorado (2022) increase their fees from the second bin to the highest, \$100-\$150. South Carolina decreases its fee from \$50-\$100 to less than \$50 in 2021; Texas decreases its fee from \$100-\$150 to less than \$50 in 2017. Finally, Indiana eliminates its less-than-\$50 fee entirely in 2021.

It is important to note that most states' handling and/or shooting competence demonstrations are considered (by gun bloggers, forum users, and even certified instructors' websites) too easy to discriminate meaningfully between skill levels, serving instead as simply another administrative hurdle.[6] Furthermore, the vast majority of these tests are conducted during training by private instructors, rather than by a government official—as in a DMV-administered behind-the-wheel driving exam. Thus, permit holders within the same state sometimes report variation in the attentiveness or leniency of their evaluator. It is conceivable that these *de jure* firing accuracy minimums show an attenuated effect in our data because of their spotty *de facto* application or inherently uninformative nature.

B. Permit Issuance

We systematically solicited annual data on permit applications received and final permits issued through public records requests made to appropriate contacts in each state when figures were not available online. Ultimately, this effort culminated in a core panel of 30 states spanning 2000 to 2022, as shown in Figure 3. Of these, twelve states were always-treated under a shall-issue law: Florida, Indiana, Louisiana, Nevada, New Hampshire, North Carolina, North Dakota, Oklahoma, Pennsylvania, Texas, Utah, and Virginia. Six states—Illinois,[7] Kansas, Nebraska, New Mexico, Ohio, and Wisconsin—switch from a true no-issue to a shall-issue policy. The other four switchers (Colorado, Iowa, Michigan, Minnesota) had may-issue laws before switching to shall-issue. However, gun rights and gun control activists alike tend to consider the "demonstrated need" standard for issuance prohibitively rigorous in may-issue states (Hoober, 2016). The Government Ac-

[6] The one exception is *de facto* may-issue state New Jersey, whose law requires 40 out of 50 rounds to land within an FBI-style target starting at a distance of 25 yards and including shots fired offhand (non-dominant) and kneeling, compared to the typical 3, 5, and 7-yard bullseye progression with little restriction on form. New Jersey's shooting qualification has been criticized as outdated (drawing upon 90s police training) and prohibitively technical for civilians, for whom self-defense encounters rarely occur at such distance.

[7] We were denied access to the number of permits issued in Illinois and use instead the number of applications received, which is publicly available on the State Police's website. We found in other states that the number of applications and permits granted are usually extremely close, as only a handful of applicants get denied. Moreover, while this change might slightly inflate their absolute number of permits, it should not affect the temporal trends and relative response to the treatment, making it perfectly valid in our difference-in-differences setting.

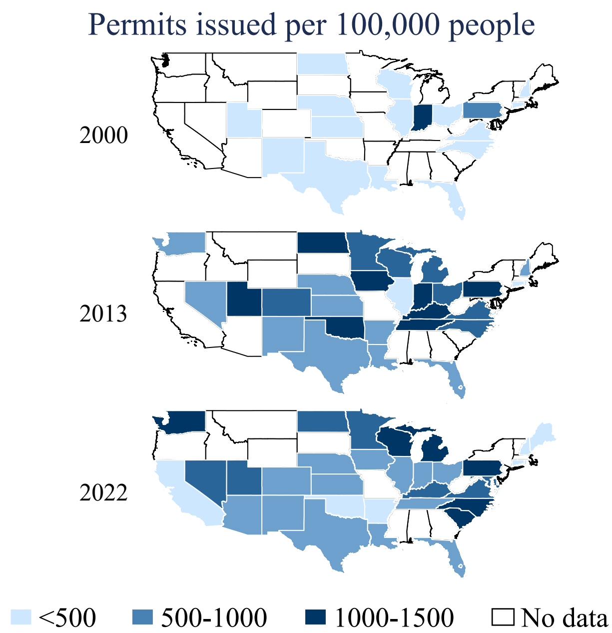

FIGURE 3. CCW PERMIT PREVALENCE OVER TIME

Notes: States with no data either did not respond to our inquiry altogether or did not report that year's permit numbers. Alaska and Hawaii are omitted for plotting purposes but have no data.

countability Office (GAO) also reports tedious evaluations in may-issue states, including in-person interviews and multiple reference letters (Cha and Larence, 2012). Because we expect these may-issue procedures to have more of a screening-out effect than codified training requirements, we focus our analysis on shall-issue state years.

Because of the vagaries of the state-level records, such as system changeovers and missing pages, we linearly impute up to four idiosyncratic years for several states. Many are well into their shall-issue regimes, when permitting numbers should be relatively predictable; details on state-by-state coverage and other data decisions may be found in Section B of our Data Appendix. Ultimately, we opt to prioritize the cleanliness of our data over its comprehensiveness, so unlike English (2021) we do not impute observations beyond single missing state-years.

As summarized in the left panel of Figure 4, the nationwide trend towards more permissive concealed carry is confirmed by the total number of licenses issued per year. This effect is driven by both new shall-issue adopters and existing shall-issue states delivering more permits over time, as shown in the right panel which shifts focus from calendar time to time since treatment (passage of a shall-issue law).

We corroborate these data against reports by the Crime Prevention Research

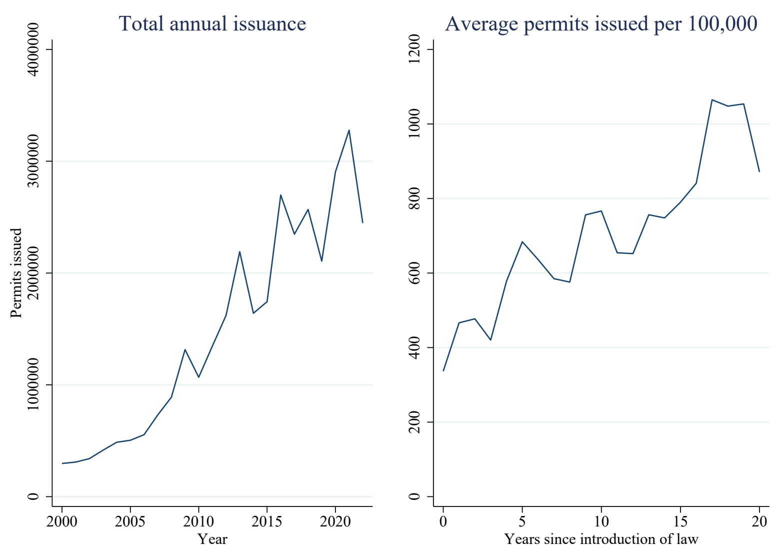

FIGURE 4. TRENDS IN PERMITTING VOLUME, 2000-2022

Notes: Left panel shows the cumulative number of new permits issued in all 27 states we recorded. Right panel shows the number of permits issued per 100,000 population, in the years following the introduction of an RTC/shall-issue law.

Center (CPRC) starting in 2014, such as Lott (2022). One major difference is that we collect the number of permits issued each year where the CPRC reports the total number of permits active in a given year. The vast majority of states do not report this number publicly, nor did they provide it upon request. However, it is easy to approximate such figures by summing the number of permits delivered in recent years (based on each state permit validity period, usually five years) and removing the corresponding number of licenses revoked. As detailed in Online Appendix B, we are able to match 225 state-year active permits numbers with CPRC estimates and find that our figures are extremely close to theirs, with the added benefit of being able to study the number of new permits delivered each year and not only the total number of active permits. We were unable to obtain the similar incomplete panel used in English (2021) for comparison after requesting it.

Data from 19 of the remaining 20 states were not released for a variety of reasons. Most commonly, the tracking system only stored current or very recent numbers, or CCW data were specifically exempt from that state's open or public records mandate. On one occasion, we were unable to elicit a response even after contacting several relevant departments. The last state, Vermont, has always honored constitutional, or permitless, carry.

C. *Public Safety Outcomes and Covariates*

While permitting data are available only for the limited subset of states in our core panel, the FBI's Uniform Crime Reports (UCR) collect monthly data from all 50 states for 1974-2021 at the level of the law enforcement agency, allowing us to investigate the impact of stringent training on violent crimes (homicides, rape, robbery, and aggravated assault) as well as gun-involved property crimes (gun theft) for every single shall-issue state. Despite being the main source of crime data in the United States, this data source is known to contain a sizeable number of non-random non-reporting agencies, missing values for some months, cross-county double-counting issues, and outliers which impede its use on the year-county level (see, e.g., Maltz and Targonski 2002).

We thus subset UCR to agencies that report more than 9 months of data per year, and report at least 15 years of data without outliers in the 2000-2021 period.[8] We then impute missing months and linearly interpolate missing values at the agency level for agencies that are clean enough to report 15 years but contain a small number of outliers or missing values. Doing so yields a conservative panel of agencies that includes about 85% of the population covered by the UCR program, which we aggregate to the state level.[9] This conservative panel costs us some potentially useful data but buys us confidence in our identification strategy and that any changes in crime rates represent actual crime trends rather than misreporting issues. As such, our resulting crime estimates are not fully representative of the U.S. population since *(i)* agencies reporting to UCR is done on a voluntary basis and thus does not cover the whole universe of law enforcement agencies as discussed in Kaplan (2021), and *(ii)* we employ a cleaning procedure that eliminates about 15% of the remaining panel of agencies (in terms of population covered). We detail our cleaning procedure in Online Appendix C and show that our results are qualitatively robust to more conservative cleaning strategies.

To examine the effect of permitting laws on defensive gun use, which is very poorly measured and understood, we splice together two imperfect, complementary sources. First, the National Crime Victimization Survey (NCVS), administered annually to a random sample of 150,000 American households by the Bureau of Justice Statistics, covers the first portion of our study period, spanning 2000-2015. However, public-use data only cover the 52 largest metropolitan statistical areas (MSAs) due to suppression and privacy limitations. MSAs are often quite unrepresentative of their counties, and many straddle multiple counties or even states. As such, unlike the state-level analyses in the rest of the paper, the NCVS regression omits the firearm suicide ratio and Republican vote share, as these co-

[8]We specifically follow Chalfin et al. (2022) for the detection of outliers, which are defined as values deviating from an agency-specific cubic time trend at the 99th percentile of the distribution of similarly sized agencies. We detail the procedure in Online Appendix C.

[9]This figure is computed excluding Illinois, Alabama, Florida, and the District of Columbia, which are known to have close to no basic UCR coverage and never consistently report 12 months of data. Including these, the figure becomes 75% of the U.S. population.

variates are unavailable at the MSA level. Also, rates per 100,000 are calculated using the NCVS-provided survey population (individuals over age 12) rather than the MSA's total population available in ACS demographics. Finally, note that the relatively low number of respondents means the NCVS suffers from yearly fluctuations and nonrepresentativeness concerns, worsened by the fact that criminal victimization is already a rare event, of which encounters leading to defensive gun use are a vanishingly small subset. As most of our fee changes happen within this window, we use the NCVS to study their effect on defensive gun use.

Next, the independent Gun Violence Archive (GVA) scrapes police and media reports of gun-involved violent incidents from 2014 to 2022 for a rich set of descriptors, including whether and how a firearm was used in self-defense. While there may be systematic underreporting that differs by state, we expect the nature of these discrepancies to be largely time-invariant and thus generally absorbed by our unit fixed effects. Most adoption of new minimum hours requirements takes place in the later portion of our panel, so we use the GVA outcomes to study effects of these policies. Thus, although neither the NCVS nor the GVA alone suffice to answer the elusive question of how access to lawful concealed carry affects defensive gun use, we take their results together as suggestive evidence.

Finally, while firearm mortality data is well-reported by the CDC, we do not expect safety training to significantly impact rates of intentional harm—firearm homicide and suicide—which make up the vast majority of these incidents. Firearm accidents, which are likely quite responsive to adequate training, are rarely fatal. Gani, Sakran and Canner (2017) find that 35%, or nearly 250,000, of the firearm injury patients in the Nationwide Emergency Department Sample from 2006 to 2014 were unintentionally injured. The State Emergency Department Databases (SEDD) available through the US Department of Health & Human Services' Healthcare Cost and Utilization Project (HCUP) provide micro-level, rich information on *all* emergency visits in a state-year. However, the SEDD are unfortunately least complete in the shall-issue states that make up our sample, often including only a single year or nothing at all. Not a single switch to a new hours or fee requirement was represented in the data, rendering causal inference impossible, so we turn to the RAND Corporation's estimates of state-level firearm injury hospitalization data from 2000-2016 (Smart et al., 2022). They are imputed from the similarly-structured, and similarly incomplete, State Inpatient Databases (SID), and come with three caveats. First, as patients with minor injuries are treated and released from the emergency department without reaching inpatient status, this means our conclusions are limited to relatively severe firearm injuries. Second, we cannot distinguish between changes in severity (intensive margin) and frequency (extensive margin) of injury. For example, if increasing hours of training appears to reduce SID hospitalizations coded as unintentional and inflicted by a firearm, that could be due to 1) fewer people getting injured by firearms, 2) a lower proportion of those injured being sent to inpatient, or 3) both at the same time. Third, as they do not separate by intent, changes in assault and self-harm rates may confound clear identification of changes in unintentional injury rates.

For our final public safety outcome, we obtain estimates of high school weapon carrying from the CDC (2021) High School Youth Risk Behavior Surveillance System, which combines the results of multiple surveys of youth in 45 states from 1991 to 2021. This collection of cross-sectional surveys measures students' health from grades 9 through 12, and has asked more than 5 million students a large set of questions surrounding their health behaviors and experiences. We analyze the students' responses to "how many days did [they] carry a weapon such as a gun, knife, or club on school property" in the previous month. While this question is an imperfect proxy for gun carrying since it is a survey and it includes other types of personal weapons, it remains the best available measure and should nevertheless provide an accurate representation of gun carrying habits.[10] We linearly interpolate all missing years since the survey is conducted once every two years, and aggregate the responses to get the percentage of students who reported carrying a weapon for one or more days in each state. We additionally control for the stringency of gun safe laws in each state, which is likely to impact children's access to weapons. We use the Everytown (2024) compilation of child access prevention policies which we group into three categories: no specific policy (24 states), weak policy that penalizes adults in case a child gets access to a gun (16 states), and strong policy where adults may be penalized for not properly securing a gun regardless of whether a child actually gains access to it (10 states). After controlling for differing child-access prevention laws across states, we expect that more stringent CCW training will encourage parents to rethink their storage practices within the home, limiting minors' exposure and weapon carrying at school.

In all our analyses, we control for states' population density, median household income adjusted to 2022 dollars, percent of the population aged 18 to 65 living in poverty, and unemployment rate. These demographics are drawn from the American Community Surveys for the years 2006-2022 and the 2000 Census, with missing years linearly interpolated. Additionally, we control for the state's Republican vote share, from the MIT Election Data and Science Lab (2020), with non-election years linearly interpolated. Finally, we proxy for firearm prevalence and general attitudes towards guns with the RAND household firearm ownership estimates, which improve upon the firearm suicide to total suicide ratio, another widely-used and well-accepted measure, by overcoming some of its biases (Schell et al., 2020*b*).

We additionally collect county-level permit issuance data from Colorado, Iowa, Indiana, Michigan, Minnesota, Mississippi, Missouri, Montana, Ohio, Oklahoma, Pennsylvania, Tennessee, and Texas. Across these 10 states, we observe an unbal-

[10]The survey started asking for gun-only carrying habits in 2017, but it unfortunately contains a large number of missing values and is not available for a large enough number of years to be usable in our study.

anced panel of 981 counties spanning 2000-2021, with a median of 650 observations per year. These county-year observations are the sample in the first column of Table A1. Covariate sources are the same as in our state-level analyses, except we retrieve county-level voting (MIT Election Data and Science Lab, 2018) and do not control for the firearm suicide ratio or gun prevalence estimates, as these measures do not exist below the state level.

II. Descriptive statistics

Before pursuing causal inference, we deemed it prudent to descriptively explore our newly compiled dataset, searching to validate existing trends and uncover new ones. First, which states tend to pass the most stringent training requirements? Second, do these states actually issue fewer permits? We study the relationship between training requirements and permit delivery rates at the state level, using the following fixed effects regression:

$$\ln(\text{PPC}_{it}) = \alpha + \beta_1 \text{Fee}_{it} + \beta_2 \text{Hours}_{it} + \mathbf{X}'_{it}\mathbf{\Gamma} + \alpha_t + \alpha_i + \varepsilon_{it}\,, \tag{1}$$

where PPC_{it} is the number of permits delivered per 100,000 inhabitants in state i in year t, Fee is the fee required to obtain a new concealed carry permit in the same state-year, Hours is the number of training hours required to obtain the permit, and $\mathbf{X}'_{it}$ is a vector of covariates including state density, median household income, adult poverty rate, unemployment rate, firearm suicide ratio, and share of Republican votes. We examine \$50 categories of fees, and four categories of hours: no required training at all, no fixed hours (training mentioned but not of a specified length), 1-8 hours, and more than 8 hours (in our data, 15 or 16).[11] We include fixed effects for states and years (α_i, α_t) and cluster our standard errors at the state level. As discussed previously, we run our analysis solely on shall-issue states since permit-granting in may-issue states is at the discretion of the issuing authority, which limits the bite of training requirements.

Table 1 details the coefficients under different specifications. Columns (1) and (2) regress each type of training requirement separately, (3) combines them, (4) adds controls, (5) adds time fixed effects, and (6) adds state fixed effects. Without controlling for time fixed effects—that is, up until specification (4)—more expensive permitting is associated with lower issuance, as might be expected. In

[11]The cutoff at 8 hours is motivated by the length of a full workday, since more than 8 hours of training would require permit-seekers to return for a second day of classes. Note also that we distinguish legislation that does not mention or explicitly does not require training ("0 hours") from that which mandates training without any further detail ("unfixed hours") for thoroughness in this descriptive table, but not in our causal analyses. First, license-issuing authorities with completely open-ended "requirements" seem far less likely to enforce them (through i.e. maintaining an approved instructor list, collecting certificates of completion, or offering state-proctored proficiency tests) than those that call for a minimum length or format. Thus, the effect of unfixed training on permitholder composition and/or behavior may be quite comparable to the effect of no training. Second, this coarser bin structure allows us to pool our limited number of observed switchers, striking a balance between specificity and precision.

particular, the highest statewide fees we observe come in at over \$100, a substantial investment above and beyond the cost of obtaining and maintaining a firearm, and they are consistently correlated with up to 1% fewer permits per capita. Adding year and state fixed effects reverses the relationship, perhaps suggesting that issuing authorities are able to adjust "prices" in response to trends in demand. Minimum length of training required, by contrast, appears to have a more consistent inverse correlation with permit popularity, particularly in states with the most demanding requirement of over a full day of training. Overall, although we find that states with more stringent training requirements, especially training hours, also usually deliver fewer permits, there remains some unexplained variation in the two policy intensities after controlling for state and year fixed effects in (6).

We additionally run the same analysis at the county level for robustness. While we were only able to obtain granular permit data for 10 states,[12] and we are unable to use the firearm suicide ratio at that level, the results nevertheless support the same conclusions. Interestingly, permits fees seem to have a more sizable and significant effect in those counties than in the state-level regression. All coefficients are reported in Online Appendix Table A1.

III. Identification

When standard two-way fixed effect models are applied to staggered adoption designs with dynamic and/or heterogeneous treatment effects, estimates can range from being slightly biased to displaying the wrong sign (e.g. positive treatment effects in every 2x2 differences-in-differences comparison may result in a negative average treatment effect). A swath of recent literature, including Goodman-Bacon (2021), Sun and Abraham (2021), Callaway and Sant'Anna (2021), De Chaisemartin and d'Haultfoeuille (2020), and Borusyak, Jaravel and Spiess (2024), has explored the precise nature of this issue and offered solutions to the problems posed by this setting. On top of staggered policy changes, many of the treatments explored in this paper are non-absorbing and exhibit multiple potential dosage levels. Thus, we choose the extremely general method developed in De Chaisemartin and d'Haultfoeuille (2024), which nests several other estimators, as our primary approach. In exchange for flexibility, DCDH demands a number of assumptions and admits limited and specific interpretations. We first justify these assumptions before explaining precisely what our main results we present actually estimate.

The non-binary, non-absorbing general treatment structure of DCDH produces an unusual situation where the estimand is not dictated by the researcher, but is instead a function of the treatment paths of each switcher group. The estimand of the ℓth lag is a combination of that lag from all treated units, regardless of

[12] Colorado, Iowa, Indiana, Louisiana, Michigan, Minnesota, Oklahoma, Pennsylvania, Tennessee, and Texas.

Table 1—Training requirements and permits delivered

	Log permits delivered per 100,000 capita					
	(1)	(2)	(3)	(4)	(5)	(6)
Permit fees:						
$0				Baseline		
Less than $50	-0.539***		-0.522**	-0.373***	0.606*	0.530**
	(0.012)		(0.006)	(0.066)	(0.334)	(0.221)
$50-$100	-0.198		-0.302	-0.283*	0.552	0.628***
	(0.422)		(0.231)	(0.153)	(0.443)	(0.197)
$100-$150	-1.005***		-0.828***	-0.791***	0.506	0.489
	(0.386)		(0.232)	(0.223)	(0.450)	(0.300)
Varies by county	-1.060		-1.041	-0.872	0.726	0.358
	(1.228)		(1.240)	(0.682)	(0.583)	(0.696)
Training hours:						
No hours				Baseline		
Some unfixed hours		-0.495	-0.495	-0.261	-0.728**	0.946
		(0.331)	(0.391)	(0.690)	(0.297)	(2.203)
1 to 8 hours		-0.245	-0.279	-0.191	-0.693**	-0.685
		(0.263)	(0.321)	(0.524)	(0.347)	(1.97)
more than 8 hours		-1.336***	-1.222***	-0.941**	-0.245	-0.948
		(0.269)	(0.304)	(0.507)	(0.341)	(1.860)
Density				-0.000	0.000	-0.000
				(0.000)	(0.000)	(0.000)
HH income (in 1000s)				0.000***	-0.000	0.000
				(0.000)	(0.000)	(0.000)
Poverty Rate				44.500***	-25.547**	7.567
				(8.542)	(11.679)	(8.962)
Unemployment				-8.895***	9.505	-7.350
				(2.761)	(7.401)	(6.489)
Firearm suicide ratio				-5.386	7.255**	0.054
				(5.113)	(3.276)	(13.265)
Republican votes				-2.645	0.555	-2.828
				(2.185)	(3.215)	(3.082)
Constant	6.941***	6.865***	7.380***	-1.482	4.621	3.725
	(0.537)	(0.093)	(0.228)	(3.556)	(3.875)	(7.496)
State fixed effect	no	no	no	no	no	yes
Year fixed effect	no	no	no	no	yes	yes
Observations	479	479	479	479	479	479
R^2	0.001	0.053	0.046	0.000	0.589	0.847

Note: Univariate (columns 1 and 2), multivariate (columns 3 and 4), and fixed effect (columns 5 and 6) linear regressions of permits per capita on training requirements, with controls. The data covers our entire sample of 27 states over 23 years (2000-2022) but we only have 479 observations (out of a possible 621) because not all states delivering permits every year. Data and sources are described in the text. Standard errors in parenthesis and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

path. For example, if $\ell = 2$, and we have two treatment paths of $1 \to 1 \to 2 \to 8$ and $0 \to 1 \to 1 \to 1$, we estimate both the effect of the first group going from 1 to 2 to 8 and the second group going from 0 to 1 to 1. In this unique setting, the first assumption needed is that each group's treatment dosage is either weakly greater than or weakly less than the initial dosage, which, as demonstrated above, need not be 0. This is not an issue for us, since we do not observe any states pass new legislature that moves in the opposite direction of previous changes to CCW permit training requirements. Additionally, the standard difference-in-difference identifying assumptions—no anticipation and parallel trends—also apply. It does not make any sense for criminals to be anticipating these minor training changes and shifting their crime temporally. More realistically, permit-seekers may rush to apply or hold off depending on whether the rules will become laxer or more stringent. However, as people get CCW permits for self-defense, it seems unlikely that anyone would try and save at most $100 or a couple hours of their time if they believe that their life is at stake. In Appendix Section F.F.1, Figures A19 and A20, we find no empirical evidence of anticipation in permit issuance.

The parallel trends assumption in DCDH is an extension of the standard 2x2 assumption. To explain it, we must establish some terminology. First, we refer to the treatment dosage of a group in the first period of observation as its *status-quo treatment*. Similarly, we call the potential outcome at the ℓth period for a group the *status-quo potential outcome* if that group were to maintain its status-quo treatment at every time period up to and including the ℓth period. Under parallel trends, groups with the same observed status-quo treatment should experience the same expected evolution of their status-quo potential outcomes. In other words, two groups with the same initial treatment should see identical-in-expectation changes in status-quo potential outcomes when moving from time ℓ to $\ell+1$. This period-by-period restriction allows common trends to change over time, as long as they move in lockstep, and only compares each switcher group to stayers that share its initial treatment dose; no structure is imposed upon the trajectories of groups with other status-quo treatments regardless of shared adoption times, identical levels or changes in dosage, or other commonalities.

Interpreting the output of the DCDH method can be tricky due to the estimator's incredible flexibility. In Appendix Section D, we present the non-normalized results alongside those of the stacked regression approach for transparency. However, these can only be interpreted as the treatment effect of being exposed to a weakly higher treatment for ℓ periods after initial treatment.[13] Such a vague interpretation is not particularly meaningful, but the basic estimator produces this interpretation because we are aggregating distinct treatment paths over many possible dosages. Thankfully, there are two more-easily interpretable DCDH outputs: path-specific estimates and normalized ATEs, our preferred coefficients.

Following one solution proposed in De Chaisemartin and d'Haultfoeuille (2024), we estimate treatment-path-specific effects instead of taking a weighted average

[13]We assume symmetry of an increase or decrease somewhat arbitrarily. We do not think there are any particularly principled reasons to either believe in symmetry or asymmetry. Furthermore, when disaggregated to path-specific effects, a finer level than increase and decrease, all results still look to be null.

of paths for each treatment lag as above. That is, we display treatments unique to each path $\mathbf{D} = \{D_1, ..., D_{t-1}\}$ shared by multiple treatment groups. There must be very few treatment paths relative to the number of groups in order for this approach to aid interpretation; fortunately, our count of paths for fees and hours are both quite low. We present some highlights of these alongside our main findings in Sections IV.A and IV.B.

The second strategy the authors propose is to normalize the estimates by treatment intensity. Our main result of interest, the normalized estimated treatment effect, is an excess-dosage-weighted average of lag-specific effects that can be considered *temporally* marginal, yet more average with respect to treatment *dosage*. These values are defined for a particular fixed lag ℓ of an earlier period which is being altered, k. The ℓth lag and kth period *DCDH "marginal" effect*, $M_{\ell,k}$, is the expected difference between two outcomes, whose treatment dosages are fixed for some pretreatment period determined by the researcher (and constrained by the data) up to and including the ℓth lag, divided by an incremental dosage term. The first term in the difference is the potential outcome under the actual treatment path up to the $\ell - k$th lag, followed by the status quo dosage until the ℓth lag. The second term is another potential outcome under the same path as the first term, except the $\ell - k$th lag itself is also the status quo dosage. This difference in potential outcomes is then divided by the difference between the actual treatment dosage at the $\ell - k$th lag and the status quo dosage. This is a marginal effect with respect to units of time, since it captures the move from period $\ell - k - 1$ to $\ell - k$ along the treatment path, but it is an average effect of the units of *dosage* change at that time.

For each group and specified ℓ lag, these $M_{\ell,k}$s are averaged across all $k \in \{0, ..., \ell - 1\}$ weighted by their share of excess treatment dosage. In this estimate, we are measuring how much each step along the dosage path up until the target ℓth lag impacts the outcome, inversely scaled by how much dosage was applied in that step. Each of these effects are weighted by the percent of *total* excess treatment dosage that group received in the ℓth lagged period and then summed to get the normalized single-group ℓth lagged effect. This does collapse to the raw treatment effect from DCDH, divided by the total excess dosage up to the ℓth lag. This quantity does not have any real intuitive meaning without unreasonably strong additional functional form assumptions. Instead, this estimator ought to be interpreted as the sum of interpretable normalized average effects—the path-specific results we discuss—under mechanically-dictated weights. Once this quantity is computed for each path, it is then aggregated together into the ℓth period normalized effect where each path is weighted by its total excess dosage.

It is worth noting, once more, that the normalized results in Section IV are excess-dosage-weighted averages of treatment effects, where the status quo treatments are different, the paths are different, and the total dosages are different, so it is not an intuitively interpretable result. Rather, it is better to think of each coefficient as a dosage-weighted average of the interpretable values that compose

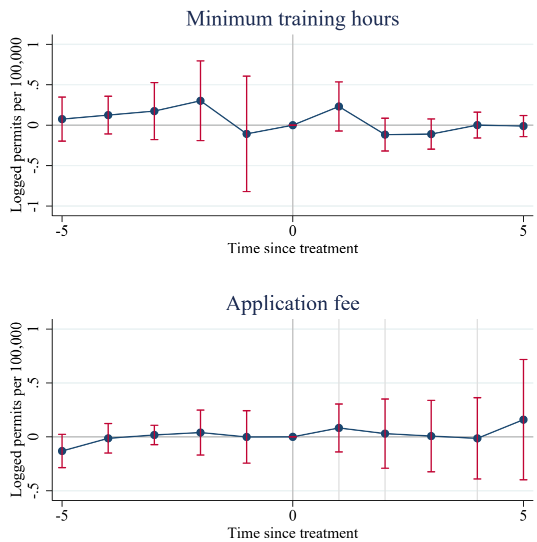

Figure 5. Normalized effect on permits per capita of...

The pale vertical lines indicate where paths exit the event study plot due to a lack of pre- or post-periods (being treated close to the beginning or end of the panel, respectively). The period-one effect is calculated from all four paths (five switchers). The second effect is calculated from three paths/switchers. Effects 3 and 4 are calculated from two paths/switchers. Finally, the fifth effect is calculated only from one path/switcher. See Appendix Figure A14 for more details.

it, and look at it in conjunction with the path-specific treatment effects. The normalized aggregated results trade off interpretability for power, and the path-specific results are interpretable but lose power.

IV. Results

We compare stacked regression event studies to those produced by the DCDH method without dosage normalization in Appendix Section D. These approaches share a binary treatment structure, but are less interpretable than the main results in this section (IV.A and IV.B), the DCDH normalized estimates. We also explore the heterogeneity present in our setting by discussing path-specific treatment estimates, but further detail along with complete path-by-path event study plots, may be found in Appendix Section E.

A. *Permits Issued: Volume and Stringency*

As previously discussed, permit-seekers in states that were may-issue prior to the *Bruen* decision were bound not by training requirements, but by the high standard of proof of demonstrated need. Only under shall-issue regimes do states' codified, standardized licensing procedures have enough bite to be directly comparable to one another. Thus, we restrict our sample to examine only the roughly 700 shall-issue state-years. In this section, we present our DCDH estimation results for each of these outcome variables in more detail. All event-study plots are produced with 200 bootstrap replications. Also note that period-one estimates are biased by the fact that law changes go into effect at different points in the calendar year and should be interpreted cautiously.[14]

Surprisingly, under the DCDH estimates, neither more time-consuming nor more expensive processes seem to dissuade citizens from obtaining their CCW licenses, as shown in Figure 5. When turning to path-specific estimates, we do not see any particular nuance. Hours only has a single path for permits due to data limitations. Fees has four paths, but none of them provide convincing results.

B. *Crime, Defensive Gun Use, Unintentional Injury, and Child Access*

Turning to our downstream outcome variables, we detect imprecise null effects of increased training hours as well as higher application fees under DCDH across a variety of crime rates. Figure 6 illustrates the normalized causal effect of a one-bin increase in minimum training length on murder, aggravated assault, robbery, and value of guns stolen.[15] Our estimates are not precise due to the few hours changes in shall-issue states observed during our study period. No conclusions can be drawn from aggregated results, but there are more interesting results when disaggregated by paths.

The only (marginally) significant impacts of hours on crime outcomes appear in the top panels of Figure A11 and Figure A13. Eliminating training requirements altogether seems to instantly increase aggravated assault rates by about 5%, while decreasing the total value of guns stolen by about the same proportion, with some die-out by five years post-adoption. On the other hand, we find no detectable effect of relaxing training from more than 8 minimum hours to 1-8. As is the case in many instructional contexts, perhaps the first few hours are the most crucial, especially if instructors prioritize student interests, such as de-escalation and nonviolent self-defense technique upfront. It is also possible that state legislatures that mandate more than 8 hours of training may be viewed as overly demanding by permit-seeking civilians, instructors, and "on-the-ground" issuing authorities alike, increasing noncompliance and producing diminishing safety returns to stringency. The relative effect strengths between the two paths are thus plausible, and the increase in assault is predicted by our theory that undertrained

[14]In more words, we do not account for the policy change occurring in the middle of the year; we code the dosage level at the end of the year. Thus, the first-period effect is a Frankenstein estimation of sorts, as it aggregates across paths that differ by unobserved length of exposure to the observed dosage. We choose to still show this effect to capture the impact of the partial year of treatment and do not worry about later effects since the initial partial year carries less weight after more lags.

[15]Our buckets are 0 hours, 1-8 hours, and 9-16 hours.

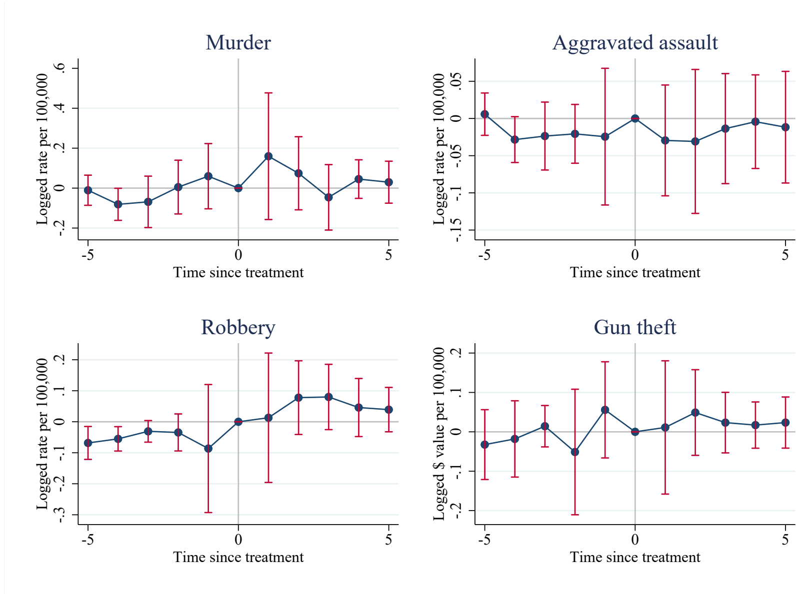

Figure 6. Normalized effect of minimum hours on...

gun carrying tends to inflame adversarial situations. However, it remains unclear why dropping training requirements would decrease gun theft; further research is sorely needed.

Figure 7 shows the causal effect of an application fee being about \$50 more expensive, corresponding to our bin width. In the upper-left panel, a higher fee may elevate the murder rate by 2-5%, with smaller and less significant increases in other violent crime categories. See Figures A15, A16, A17, and A18 for a decomposition of these aggregate effects; Ohio drives the increase in aggravated assault, while Texas drives the others. Overall, most paths are null but there are some modest changes although they vary in direction such that there is no consistent conclusion on if harsher restrictions increase or decrease crime generally.

Next, we turn to estimating the effects of law changes on defensive gun use, widely considered the main desirable outcome of more permissive concealed carry legislation. Responses from the National Crime Victimization Survey (NCVS) allow us to trace defensive gun use for the earlier portion of our study period, from 2000 to 2015, but only for the 52 largest metropolitan statistical areas (MSAs) due to privacy restrictions. Further restricting our sample in terms of both states and years prevents us from estimating the effect of hours on any NCVS outcomes, as we no longer observe enough law changes. Also, in these specifications, we do not include the firearm suicide ratio and Republican vote share, as these covariates are unavailable at the MSA level. Although we are unable to condition on these characteristics, we also avoid introducing measurement error, since MSAs are of-

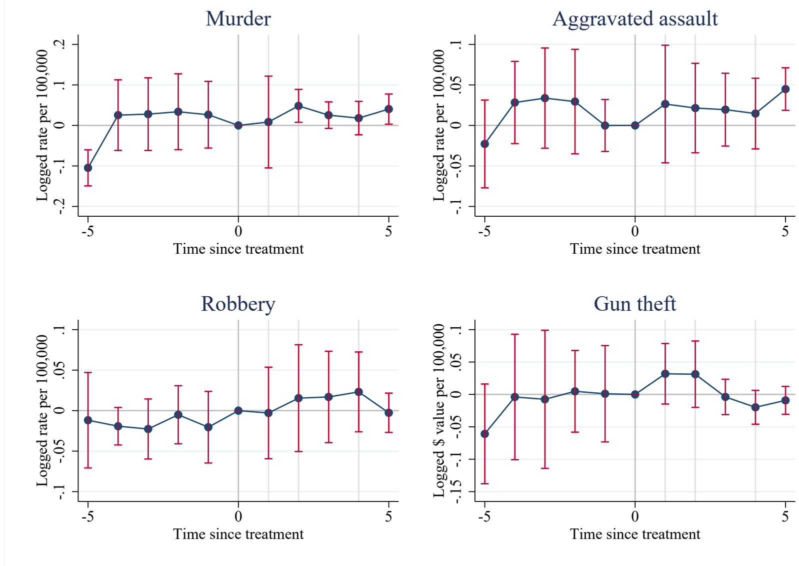

Figure 7. Normalized effect of application fee on...

The pale vertical lines indicate where paths exit the event study plot due to a lack of pre- or post-periods (being treated close to the beginning or end of the panel, respectively). The period-one effects are calculated from all five paths (seven switchers). The second effect is calculated from three paths/switchers. Effects 3 and 4 are calculated from two paths/switchers. Finally, the fifth effect is calculated only from one path/switcher. See Appendix Figure A14 for more details.

ten quite unrepresentative of their counties and many straddle multiple counties (or even states). Further, including an MSA fixed effect should suffice, as gun ownership and political leanings are likely relatively stable over this short 15-year period within each MSA. Finally, due to the prevalence of state-years in which no respondents reported taking defensive action with a gun, where zeroes would preclude us from using a logged dependent variable as in our other specifications, we transform the rate per 100,000 using the inverse hyperbolic sine. This means the event-study coefficients shown in Figure 8 are not directly comparable to others, but they can be interpreted qualitatively: making permits more expensive *might* hamper civilians' ability to interrupt crime, without quite reaching conventional levels of significance.[16]

To study the latter portion of our study period, we use logged rates per 100,000 from Gun Violence Archive (GVA) data for 2014-2022. Again, we are unable to measure the effect of fee changes because of the short panel. In the upper-left plot in Figure 9, we see that increasing the minimum hours of training required does not appear to have an impact on defensive gun use in these data. Additionally, we also do not detect changes in preventable negative outcomes, as shown in the

[16]See substantively similar findings under other dependent variable transformations in Figures A21 and A22.

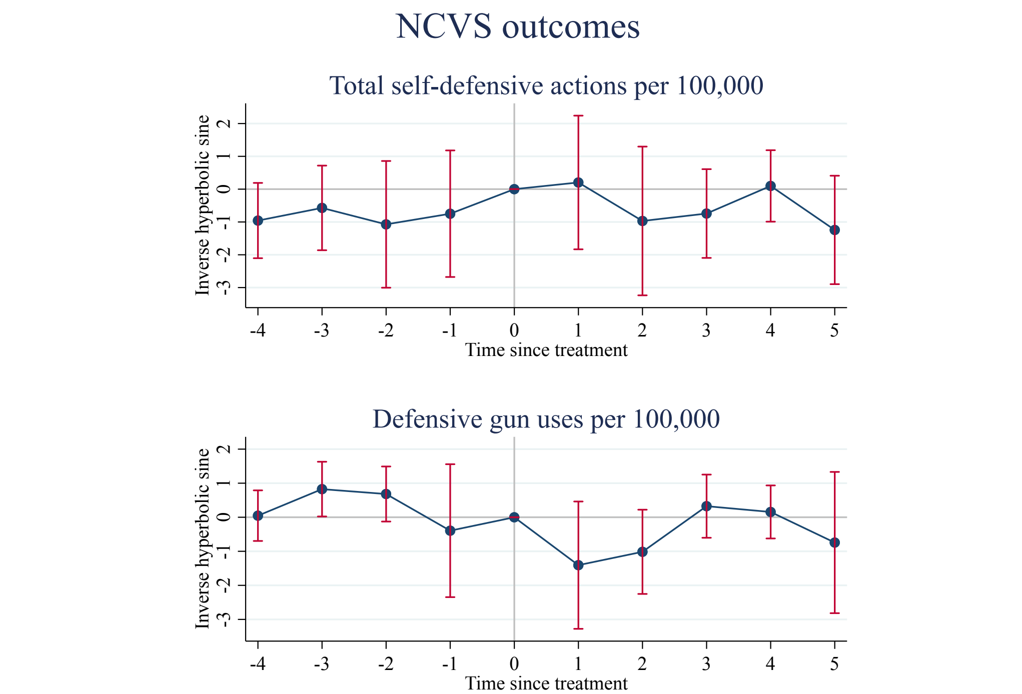

Figure 8. Normalized effect of application fees on...

other three panels.

Ultimately, interpretation of both the NCVS and GVA results is limited by coverage concerns. The NCVS is still a relatively small survey, while the GVA draws upon police and media reports and thus mainly captures high-stakes adversarial encounters, which may not represent all cases of self-defense. The large confidence intervals in these regressions combined with our data concerns suggest that further research is of great importance.

Figure 10 shows that inpatient admissions for firearm-inflicted wounds may decrease by around 10% in the first full year after a one-bin increase in training hours (i.e. period 2), although this effect disappears in period 3 and may reverse in period 4. Increasing fees appears to insignificantly and very slightly elevate injuries as well. However, the concerning pretrends in both panels of the figure suggest that these findings must be interpreted with a grain of salt. Indeed, the RAND report we use (Smart et al., 2022) does not separate unintentional injuries from self-inflicted injuries and those that result from assault, so state trends in mental health or crime may contaminate the data making this estimation

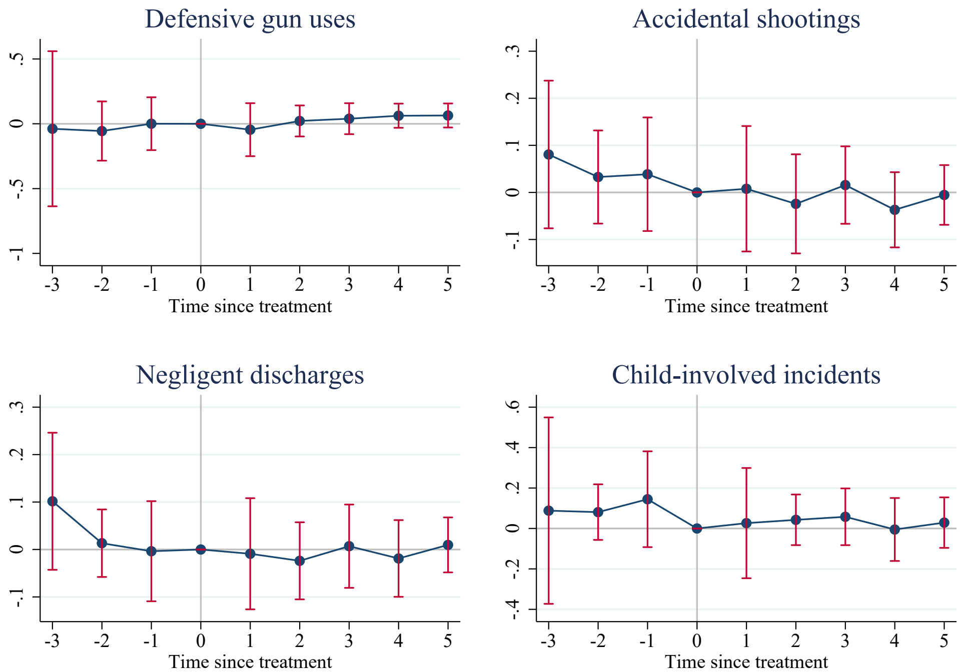

FIGURE 9. NORMALIZED EFFECT OF MINIMUM HOURS ON LOGGED GVA OUTCOMES

extremely fuzzy.

Last, Figure 11 demonstrates that hours of training have a quite precisely-estimated null effect on the percentage of high schoolers who report carrying a weapon in the last month. Surprisingly, making permitting more expensive leads to a slight, one-time increase that remains stable over time. Again, we are unable to distinguish between guns and other types of weapons in these survey data.

V. Conclusion

Most studies on the consequences of the concealed carry of firearms have focused on states' steady shift from no-issue and may-issue laws towards shall-issue and permitless carry, while the impact of varying the difficulty of obtaining a concealed carry permit under shall-issue regimes has not been examined. We introduce a nuanced legal dataset that enables this research, but also reveals that these subtler changes are few and far between. Furthermore, they generally appear highly correlated with existing "gun culture" and regional attitudes, confounding causal

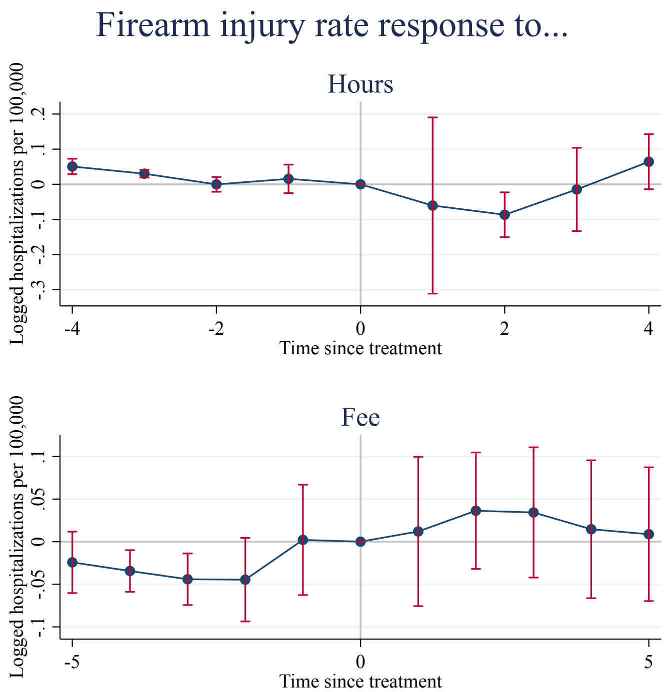

FIGURE 10. NORMALIZED EFFECT ON INJURIES OF...

inference. We use the strategy introduced in De Chaisemartin and d'Haultfoeuille (2024) to conservatively estimate treatment effects via closely matching switching states to controls that otherwise follow the same exposure path. With only four changes in minimum hours of training and seven changes in fees—some of which are modest in size—we cannot identify an effect of stringency on CCW permit issuance volume nor downstream outcomes (crime, unintentional injury, child access, and defensive gun use).

While more evidence may unfold over the next decade, as the repercussions of *Bruen* stabilize in affected states, it is also possible that the gradual switchover to right-to-carry has already had an outsize criminogenic effect that dwarfs any further changes in public safety. Stripping issuing authorities of their case-by-case discretion by rigidly codifying license application and acquisition leaves the right to concealed carry open to abuse and recklessness, at the cost of innocent lives.

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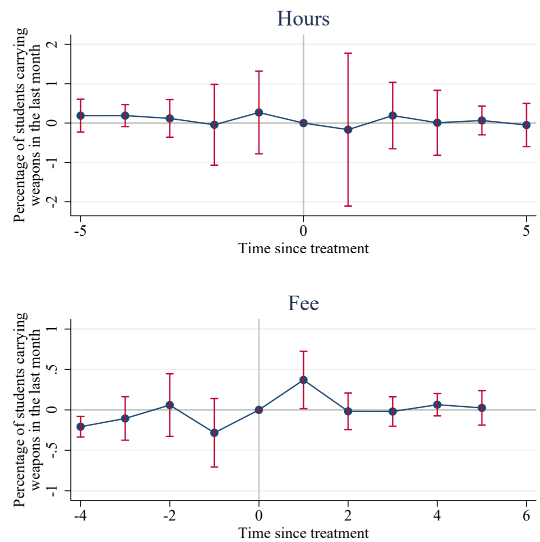

Figure 11. Normalized effect on child access of...

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