Online Appendix to "Beyond Bruen: The Effect of Concealed Carry Training Requirements on Public Safety"

A. Law coding

When a state offers multiple types or durations of CCW permits, we code the least demanding path to obtaining the level of privilege generally desired and generally thought of as true concealed carry. For example, North Dakota's Class 1 permit offers more reciprocity with other states, but requires a live-fire proficiency test; the Class 2 permit does not and is treated as identical within the state. We assume this is more relevant for resident applicants, such that our coded minimum requirements do not include a live-fire proficiency test. In contrast, Indiana offers a "qualified" handgun carry license solely for hunting and target practice as well as an "unlimited" license for the "purpose of the protection of life and property," with higher fees for the latter, which more closely matches the intent and legal protections offered by a true CCW permit. However, even more expensive are the lifetime versions of these permits, as opposed to the four-year standard which we choose to focus on.

When possible, we add the cost of mandatory fingerprinting and/or background checks to the application or issuance fees. When these procedural charges vary locally or are performed by third parties, we record only the permitting fee: Wyoming's fingerprinting and notarizing fees differ from sheriff to sheriff, but the permit application price is always the same. In some cases, even the permitting fees are not common statewide. Alabama issues through individual counties' sheriffs, who charge from \$5 to \$20; New York does the same, with New York City charging \$340 and Westchester County charging \$10 nominally, with a mandatory \$120 background check. We drop some or all shall-issue years of Arizona, North Dakota, South Dakota, and Utah for similar reasons.

For completeness, students additionally maintained notes on any law changes that did not affect the outcome of the coding, but otherwise relaxed or tightened training requirements. For example, one Missouri bill, effective from August 2014 until the introduction of permitless carry in January 2017, revised the live-fire test to require competency with *either* a revolver or a semiautomatic pistol, rather than both, as had been mandated since Missouri became a shall-issue state in 2003. Also, some states accept prior participation in an organized shooting competition, law enforcement, or military service as proof of competence, but it is unclear how many applicants this affects; besides, we do not expect users of these clauses, who already possess a high level of familiarity with firearms, to respond to training requirements anyway.

B. Total active permits

For completeness, we reached out to the eight states that were *de jure* may-issue before *Bruen*: California, Connecticut, Delaware, Hawaii, Maryland, New

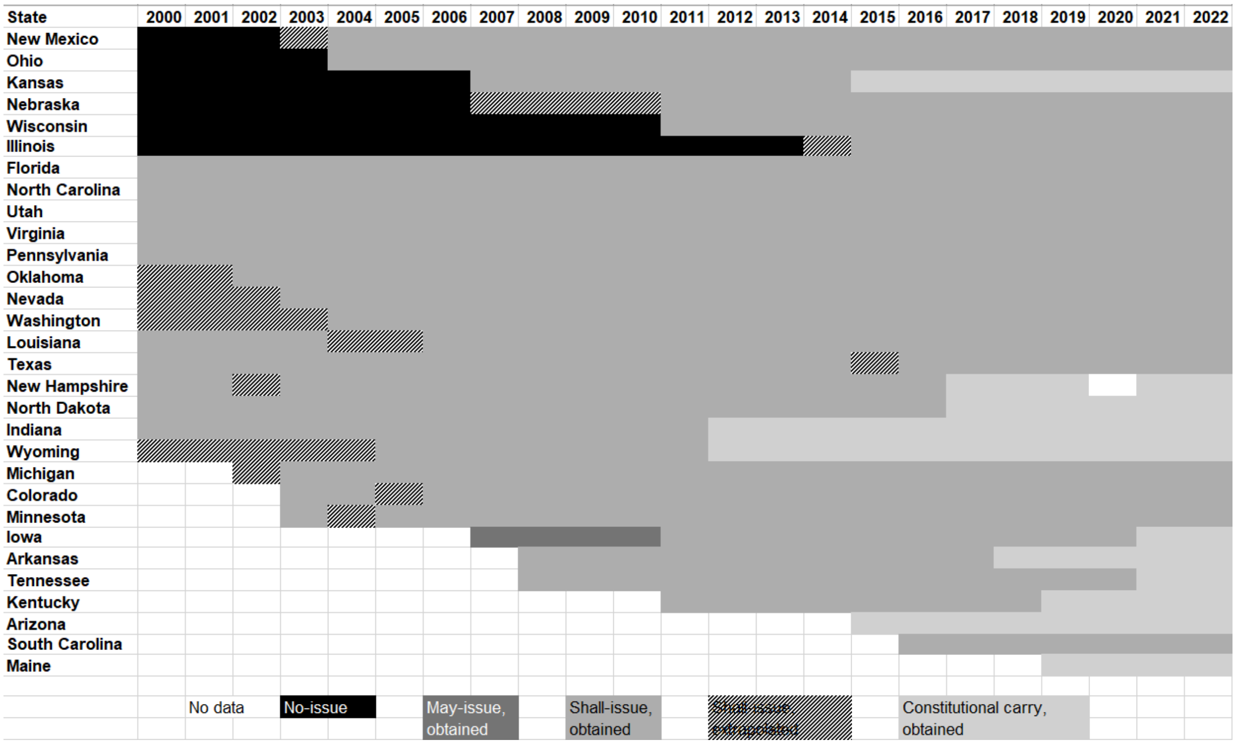

Figure A1. Core panel permit data coverage

Jersey, New York, and Rhode Island. As expected, most issuing agencies reported that up until recently, they saw extremely little activity, or did not collect these numbers due to the piecemeal, case-by-case nature of the process. As a New Jersey officer wrote, "Permits to carry before 2022 were extremely low in number due to a justifiable need being required. Basically, if you didn't need it for an employment than there was a very high bar to receive one. If that helps your mindset in the number of concealed carry permits. My assumption is in the past year there have been more concealed carry permits then [sic] all the years back to 2000." Similarly, the federal GAO report confirms that Hawaii issued 0 permits in 2011. We were, however, able to obtain numbers for 2001-2022 from Rhode Island, while Connecticut's reports were extremely low (less than 1% of the GAO or CPRC figures) and thus we deem them unreliable.

Turning to shall-issue regimes, permitting data from nine states—Alabama, Alaska, Georgia, Idaho, Massachusetts, Mississippi, Missouri, Montana, and Tennessee—were not released because only current numbers were available, or CCW data were specifically exempt from that state's open or public records mandate. In other cases, such as Oregon and South Dakota, we only received total active permit numbers, which do not accurately reflect the direct issuance effects we seek to isolate because they additionally include dynamics of expiration and revocation.

For example, a sudden dip in total active permits may reflect fewer new permits being issued in that year or a no change in new issuance, with a mass expiration of permits from boom in permitting five years before. We were unable to elicit a response from any West Virginia authority, and Vermont has always honored constitutional, or permitless, carry. We procured data from the remaining 30 states as shown in A1; our estimator can handle unbalanced panels.

We extrapolate the total number of active permits based on each state's concealed carry permit validity period. For most states, permits are valid for five years up to a few exceptions such as Tennessee where they are valid for eight years. This method is not perfect, since we do not have the exact issuance date of each permits but only the total number by year, especially since permits can be renewed early. However, it should provide a rough estimate of the total of active permits, which we do not use in our main analysis and only use to corroborate our numbers. If available, we then remove the number of revoked permits. If reported, we use the actual number of active permits instead of the extrapolated one if the state police shared that information, which only concerns a handful of states.

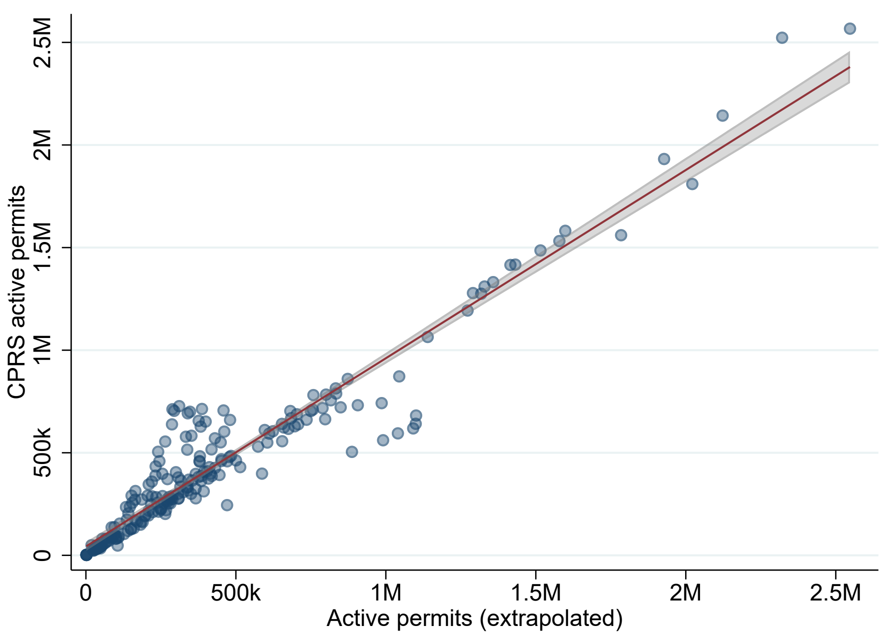

Figure A2. Active permits estimates

Figure A2 plots our estimated number of active permits against the CPRC estimates, as well as a linear regression of the two with 95% confidence intervals. The coefficient of the slope is 0.92 and the figure shows that the two measure are extremely close up to some irregularities. These outliers are most likely due to the extrapolation process being inaccurate rather than a data problem. Some outliers are due to our data being more up-to-date than the CPRC estimates. For instance, Lott (2022) reports 47,674 permits in Maryland in August 2022, while the state police reported 105,855 permits in 2022 to us when asked about the number of active permits in 2023, due to the high number of new permits issued in 2022.

Overall, the closeness of the two measures gives additional confidence in the accuracy of our disaggregated year-specific issuance data.

C. Aggregating UCR data to the state level

The FBI's Uniform Crime Reporting data, while accepted as the nation's leading crime surveillance program, has several well-documented and serious flaws. Notably, reporting is voluntary, so agencies may enter or exit the panel, and reporting some months of the year is also possible. Also, there is no distinction between an agency reporting zero crimes in a year and simply not reporting; see Kaplan (2021) for a thorough discussion. We use the agency-month arrest data from Kaplan (2020) to gather violent crime rates, as well as the agency-month stolen property data from Kaplan (2022) to gather gun theft. While criminologists often argue that such data should only be used at the agency-level, economists often need to aggregate to the county (or state) level to match crime rate to other aggregate datasets, which has given rise to a number of imputing and cleaning procedures.

We deal first with the detection of outliers. It is well-known that some agencies occasionally misreport some of their crimes with implausibly small or large rates. Several outliers detection methods have been used in the literature, most often relying on detecting high standard deviation figures or numbers that deviate from a trend. We use the Chalfin et al. (2022) approach described in their Online Appendix A3. We first divide agencies into five groups based on their population served in 2000: towns with less than 10k inhabitants, towns with 10k to 50k inhabitants, and small (50k to 100k), medium (100k to 250k), and large (more than 250k) cities, analogous to the city division of Chalfin et al. (2022) with the addition of the two categories of less-populous non-urban areas. For each agency and outcome variable, we then regress a polynomial cubic time trend on the outcome plus one, given the large number of zeroes in some variables, and generate the percent deviation of the crime outcome from the trend. We detect an observation as an outlier if the percent deviation from the trend is greater than 50% or in the 99th percentile of the agencies' population-bucket distribution. Contrary to Chalfin et al. (2022), we do not drop outliers since removing outlying years (and not the entire agency) would be problematic when aggregated to the state level, causing a different set of agencies to be aggregated in each period. Instead, we linearly interpolate (or extrapolate with a lower bound at zero if the outlier is at the tails) the outlying years. To ensure we do not excessively interpolate, we fully drop agencies with fewer than 15 years of data free of 1) missing values, 2) fewer than 9 months of reporting, and 3) outliers in the 2000-2021 period.

As it is often the case when using UCR data, we drop any agencies that report serving a population of zero; these are often non-local personnel such as state police, highway patrol units, or state gaming and gambling commissions. These 4,807 unique agencies make up 24.4% of all agencies, yet reported only 0.03% of all aggravated assault arrests in 2020, which is negligible and extremely unlikely to alter our results. We impute missing months following the recommendations of Kaplan (2021), dropping agency-years that report fewer than 9 months (and augmenting the yearly crime figure of those that report 9-12 months by 12 divided by the number of months reported. This approach is of course not perfect, but as we have no reason to suspect that the months unreported are systematically lower or higher in crime, this approximation works in the aggregate and especially at the state level to give a more accurate figure of the number of crimes, dropping agencies that are too problematic.

Overall, the procedure removes about 25% of the agencies reporting to UCR, weighted by population, as detailed on Figure A3. However, three states are known to have close to no accurate historical reporting to UCR: Florida, Alabama, and Illinois. As seen on the maps, these states have 0 or close to 0 coverage (we show a "No Data" value if less than 2% of agencies have data to avoid binning these values with states that report e.g. 40% of their population). This is mostly due to very few agencies consistently reporting more than 9 months of data over the period. Even though coverage has improved in recent years, no agencies have 15 years of clean data. As such, when removing these three well-known problematic states, our procedure only removes about 15% of the agencies reporting to UCR, weighted by population.

We plan to show robustness to alternative UCR aggregations with varying levels of imputation (i.e. only keeping agency-crimes that are reported 19 or more of the 21 panel years).

D. Comparing stacked and DCDH non-normalized regressions

We estimate the effects of hours and fee changes on permit issuance and crime outcomes via stacked regression and non-normalized DCDH. Both of these differ from our preferred normalized method in that they treat all switches the same, so the outcome's response to a change from a \$100 fee to a \$25 fee and change from \$30 to \$25 are weighted equally in the final aggregated estimate. Stacked also differs from DCDH generally in its selection of controls. Rather than performing principled, if restrictive, selection of control units by matching on prior treatment paths, stacked regression averages treatment effects over several truncated panels, each centered around one observed treatment event. Within each "stack," any state that does not experience a policy change in that window is a valid control unit. Thus, instead of exploiting long-term shared treatment histories, stacked estimators aim to isolate the instantaneous impact of policy adoption by using

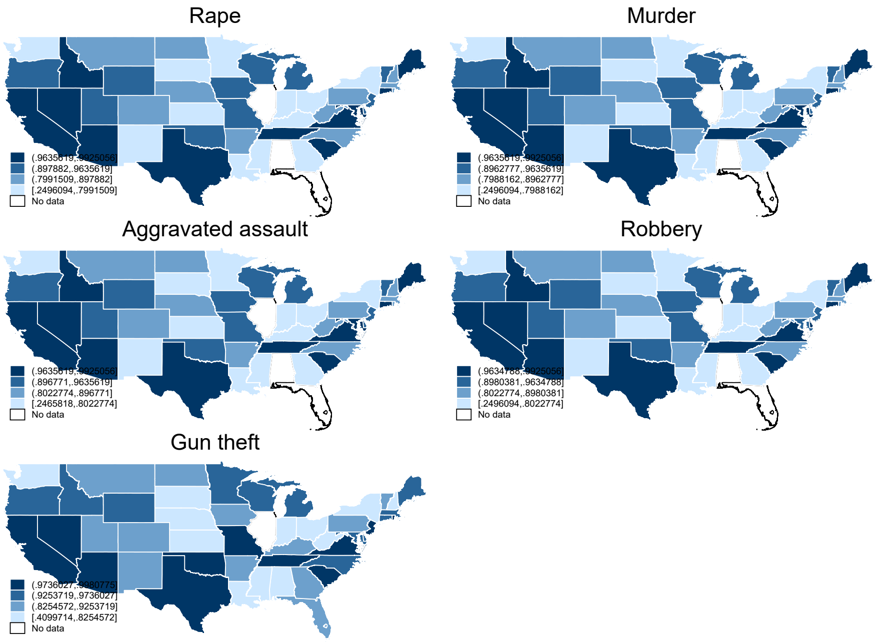

Figure A3. Coverage of cleaning procedure

only the most proximate years. This also achieves the goal—shared with DCDH and other new difference-in-differences approaches—of eliminating the "forbidden comparisons" concerns that arise when standard two-way fixed effects models are applied to staggered treatment settings, where treatment effects may be heterogeneous.

In the stacked results in Figures A4, A6, and A8, note that we have coded the policy change to be a *decrease* in hours or fees, such that the direction of the effects takes the opposite interpretation from their direction in all other figures in the paper, which are produced with DCDH. This is because the estimator allows only a binary treatment, and the vast majority of changes in the data are policies being relaxed, not made more stringent. For the few treatment effects estimated from changes in the other direction, we assume symmetry and reverse their sign before averaging them with the rest. All stacked regressions are weighted by state population.

We begin by discussing the direct effect of stringency on permits issued. The stacked-regression estimates in the top panel of Figure A4 show that decreases in hours produce a borderline-significant increase in newly-issued CCW permits per capita. In line with intuition, then, it appears that lengthier trainings hamper permitting. In the bottom panel, changes in fee do not seem to affect permitting, although the pretreatment confidence intervals in the lower panel of Figure A4 suggest that decreases in fee might be predicted by a steady decrease in issuance

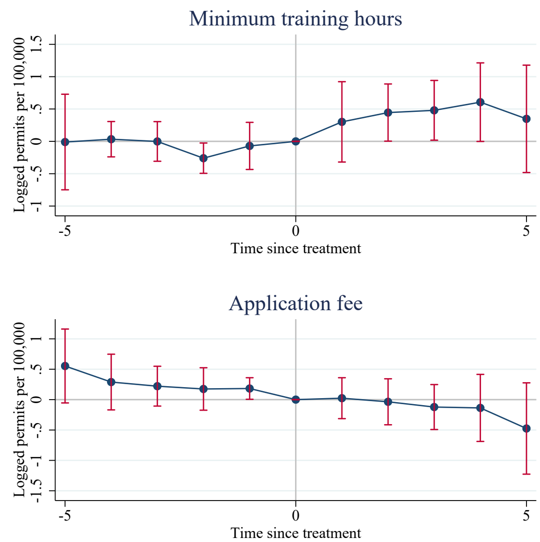

Figure A4. Stacked effect on permits per capita of a decrease in...

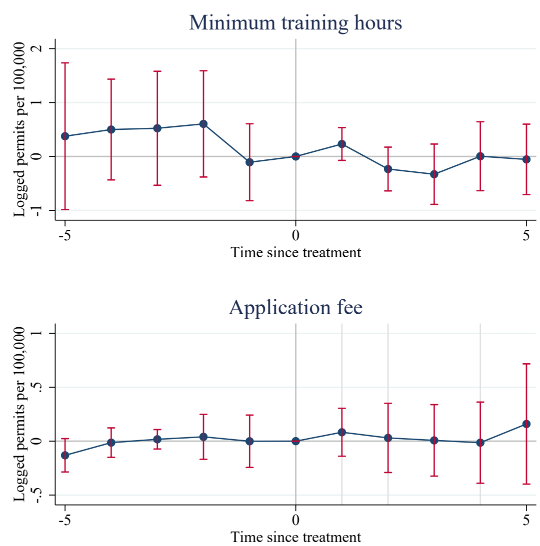

Figure A5. Non-normalized effect on permits per capita of...

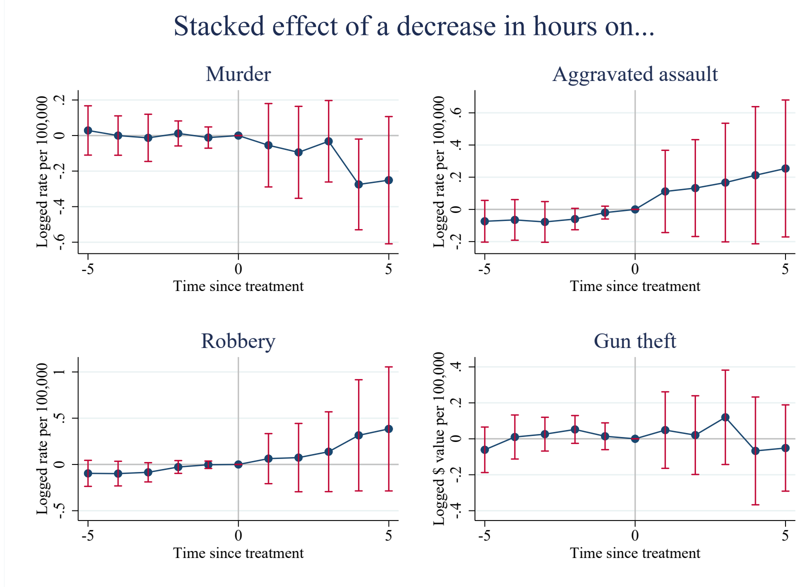

FIGURE A6. STACKED EFFECT OF MINIMUM HOURS ON...

in the years prior. In contrast, the non-normalized DCDH estimator suggests both hours and fees have a null effect on permitting, in Figure A5.

Next, we consider the response of various crimes to these policy changes, starting with training length in hours. Figure A6's stacked approach suggests a substantial, but noisy, decrease in murder 4-5 years after a decrease in minimum hours. This is consistent with the "good guys with guns" argument–that arming law-abiding citizens may save the lives of would-be victims. However, the non-normalized DCDH estimates in Figure A7 all appear insignificant. In both figures, upticks in nonfatal violent crime (aggravated assault and robbery) seem to precede the implementation of a new training length. Otherwise, the pretrends are fairly null; combined with the apparent impact of hours on number of permits issued above, this lends some credibility to the suggestive changes in both figures. It is interesting to note that Figure A6 displays increases in robbery and assault once hours have decreased, while Figure A7 shows the opposite: that robbery increases when hours increase.

Finally, unlike the event studies of hours changes, the stacked event studies of fee changes in Figure A8 yield consistently problematic pretrends. We thus do not interpret any of the post-treatment estimates. The DCDH analog in figure A9 shows null effects of fees. These results are unsurprising, since permit issuance itself does not seem to respond to the permit fee (Figures A8 and A9).

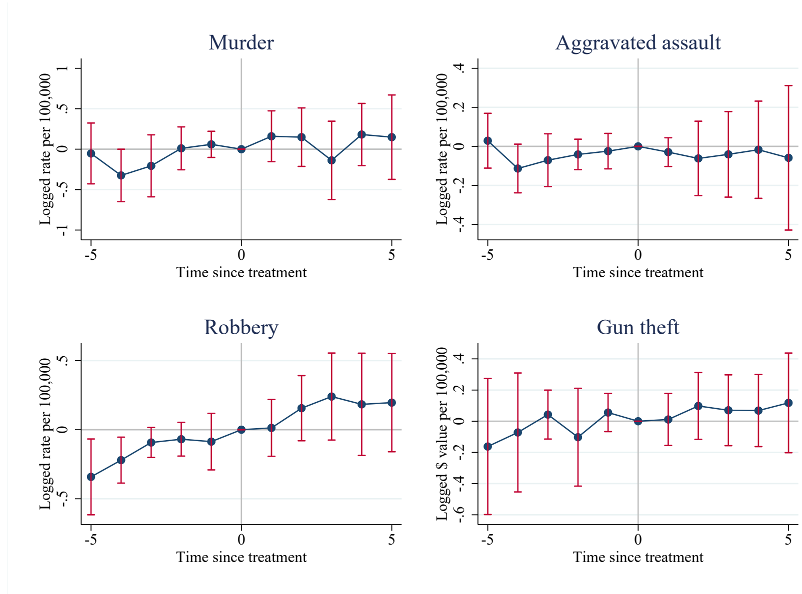

FIGURE A7. NON-NORMALIZED EFFECT OF MINIMUM HOURS ON...

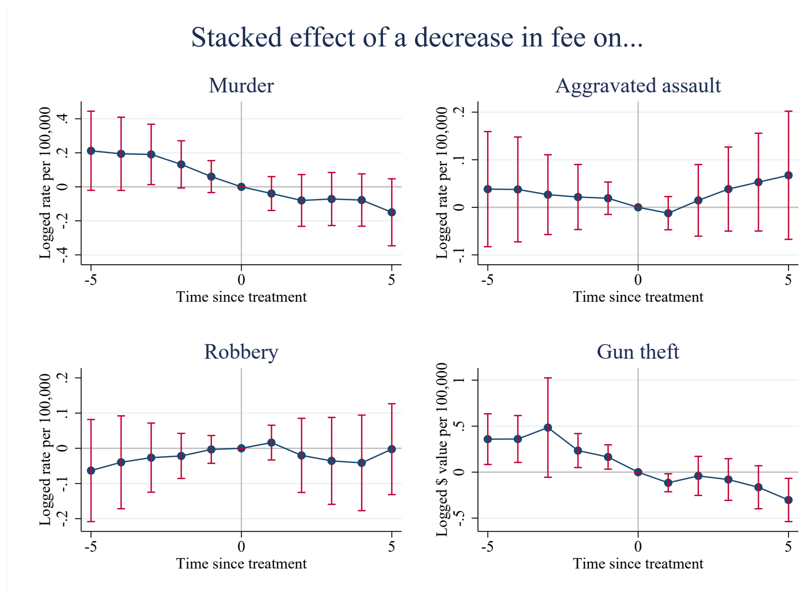

FIGURE A8. STACKED EFFECT OF APPLICATION FEE ON...

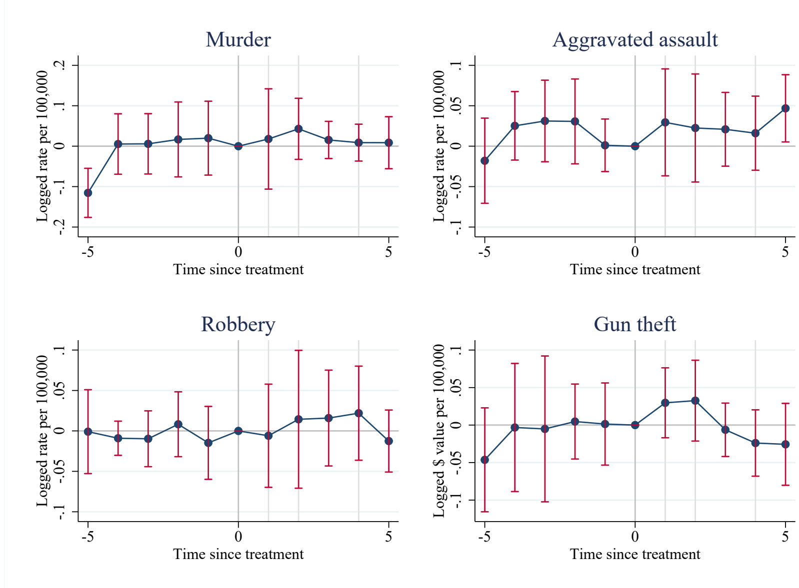

Figure A9. Non-normalized effect of application fee on...

E. Additional path-specific event studies

Here we include the underlying estimated effects particular to each observed combination of requirements before and after a law change, whose aggregation makes up the normalized results of the De Chaisemartin and d'Haultfoeuille (2024) estimator presented in the main paper. Figures A10, A11, A12, and A13 each contain two paths, comprised of two switchers each. 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. The upper panel of main-text Figure 5, depicting the effect of hours changes on permitting, draws only upon the former path, as we do not have South Carolina and Missouri permit data.

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

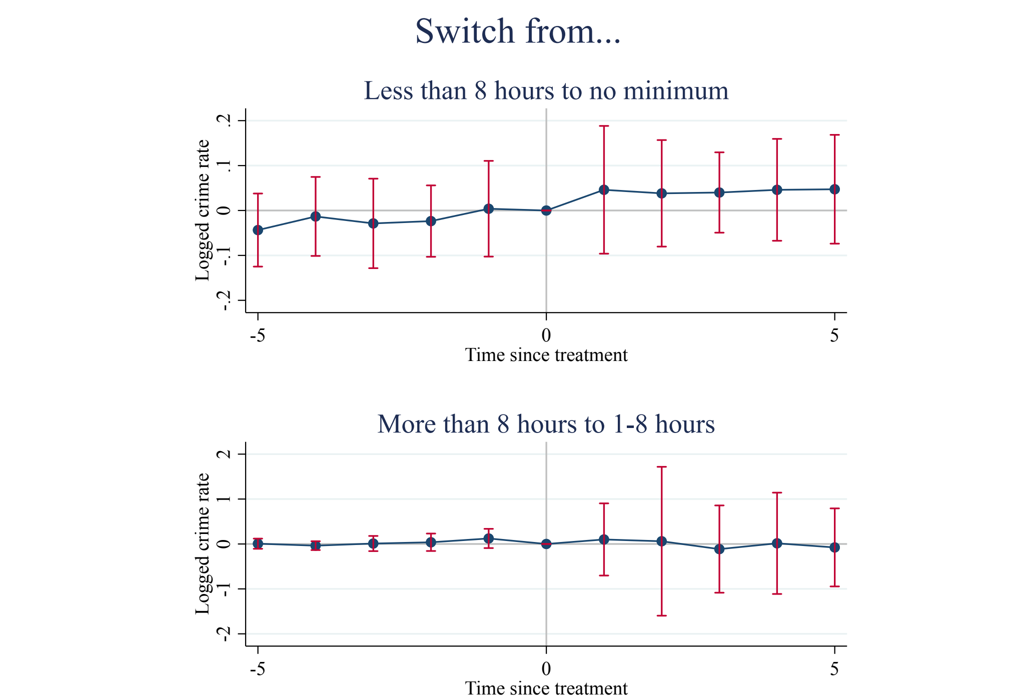

Figure A10. Treatment path-specific estimated effect of training hours requirement on murder

These paths comprise the estimates in the top-left panel of Figure 6.

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 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 A14 shows the response of permitting volume to four distinct fee-change paths of differing lengths due to data limitations. The upper-left panel displays the effect of Ohio increasing its permitting fee in 2012, from less than \$50 to the second nonzero bin, \$50-\$100. South Carolina performs the reverse operation in 2021, in the bottom left. In the upper-right, Michigan (2003) and Colorado (2022) increase their fees from the second bin to the highest, \$100-\$150. Finally, in the bottom-right panel, Texas decreases its fee from the highest bin to the lowest in 2017.

Because we have slightly more crime data than we do permitting data, two

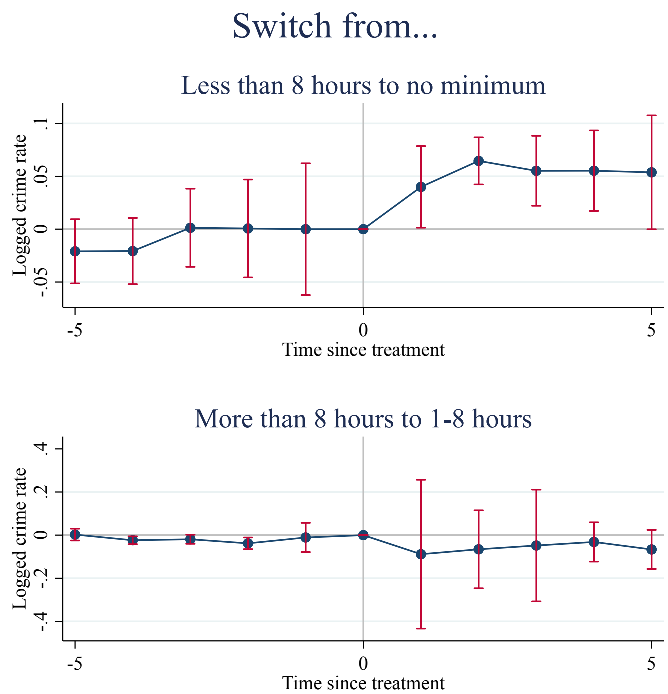

Figure A11. Treatment path-specific estimated effect of training hours requirement on aggravated assault

These paths comprise the estimates in the top-right panel of Figure 6.

additional treatment paths are available to calculate the crime-response estimates. First, Mississippi increases its fee from \$50-\$100 to \$100-\$150 in 2004, joining Michigan and Colorado in the upper-right panel. Second, Indiana decreases its fee from less than \$50 to \$0 in 2021. This is a new path, the only time we see a fee eliminated, and shown in the bottom rows of Figures A15, A16, A17, and A18.

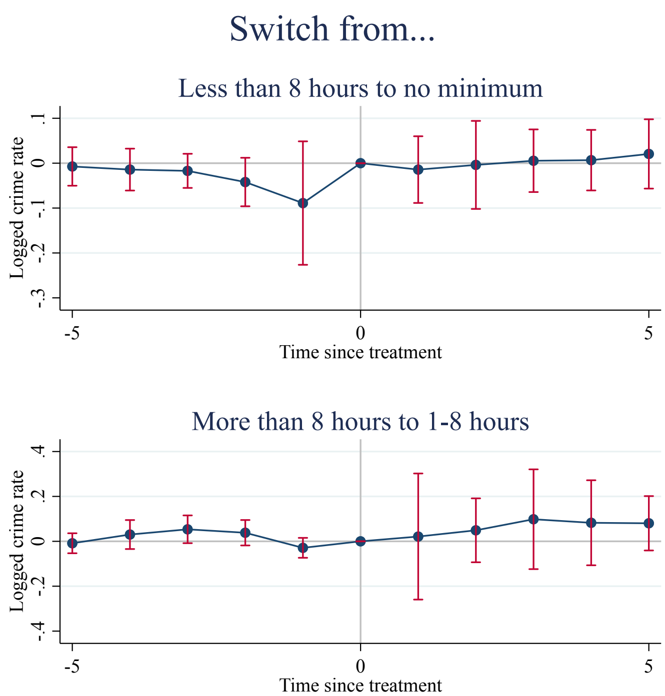

Figure A12. Treatment path-specific estimated effect of training hours requirement on robbery

These paths comprise the estimates in the bottom-left panel of Figure 6.

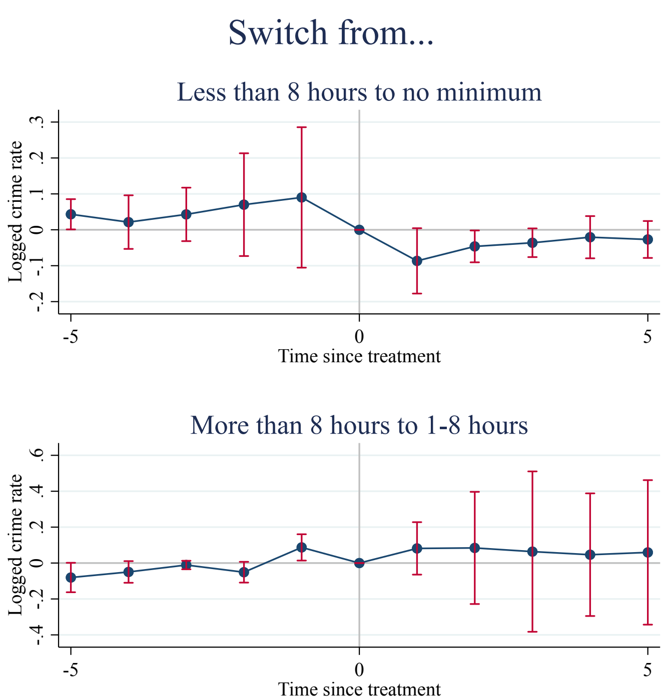

Figure A13. Treatment path-specific estimated effect of training hours requirement on gun theft, in USD

These paths comprise the estimates in the bottom-right panel of Figure 6.

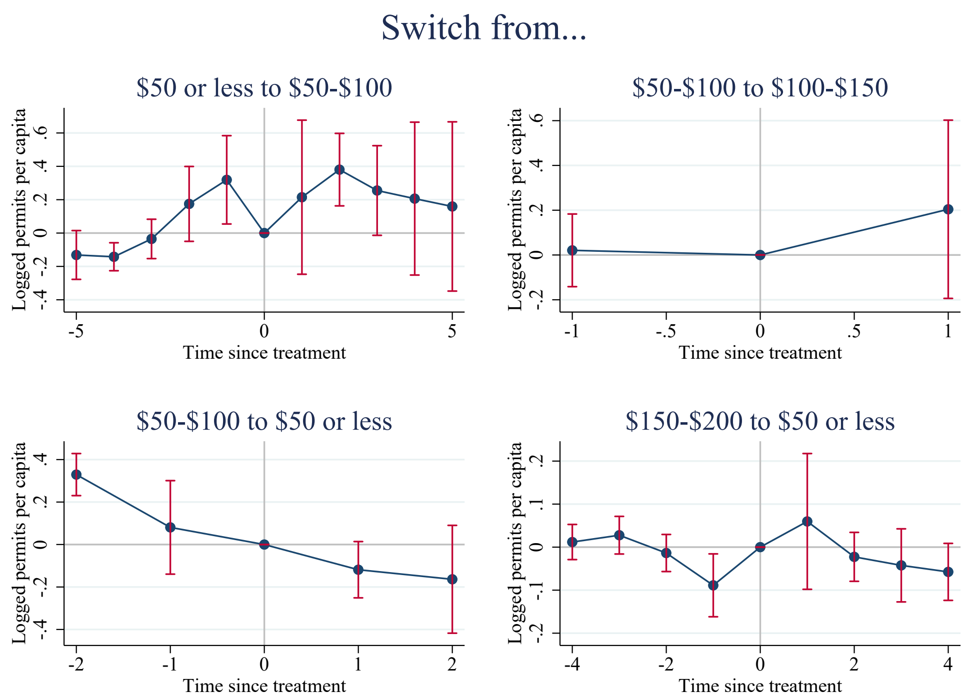

Figure A14. Treatment path-specific estimated effect of application fee cost on permit issuance

These paths comprise the estimates in Figure A14.

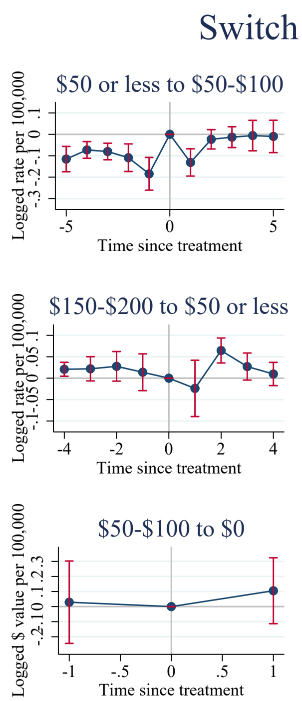

Figure A15. Treatment path-specific estimated effect of application fee on murder

These paths comprise the estimates in the top-left panel of Figure 7.

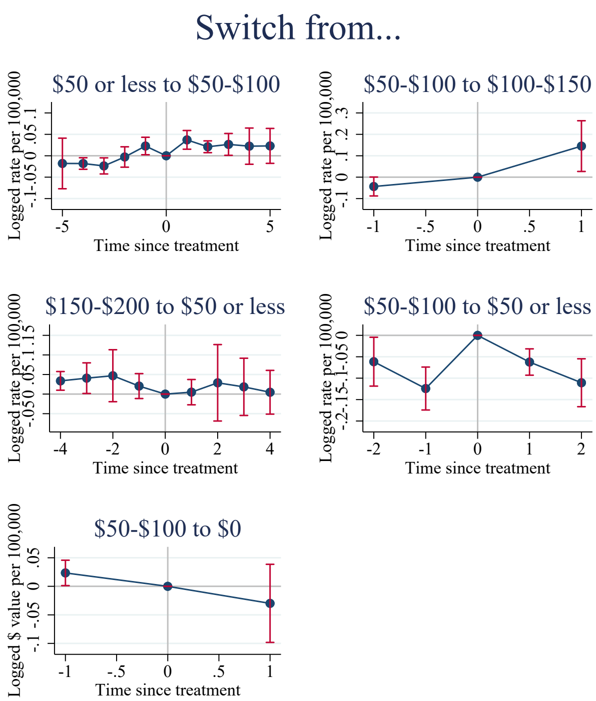

Figure A16. Treatment path-specific estimated effect of application fee on aggravated assault

These paths comprise the estimates in the top-right panel of Figure 7.

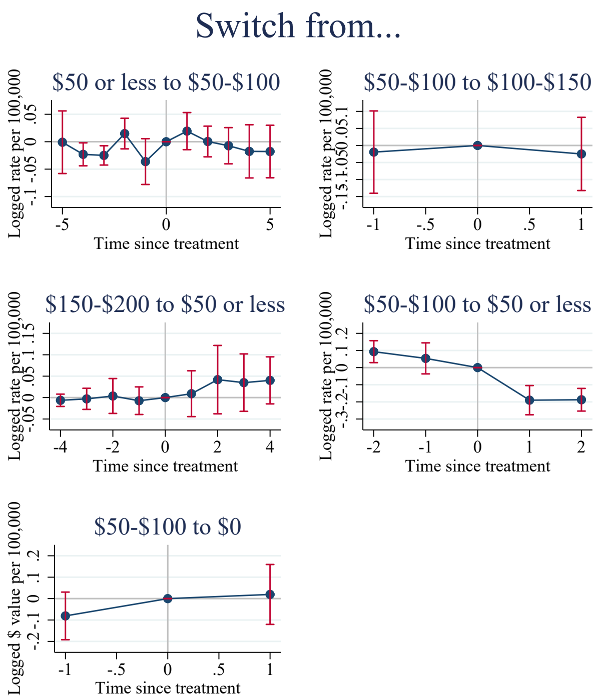

Figure A17. Treatment path-specific estimated effect of application fee on robbery

These paths comprise the estimates in the bottom-left panel of Figure 7.

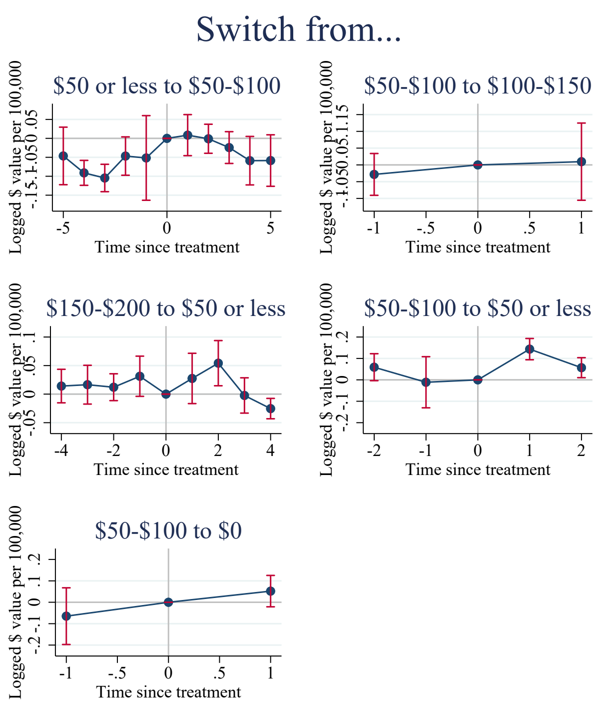

FIGURE A18. TREATMENT PATH-SPECIFIC ESTIMATED EFFECT OF APPLICATION FEE ON GUN THEFT, IN USD

These paths comprise the estimates in the bottom-right panel of Figure 7.

F. Additional tables and figures

TABLE A1—TRAINING REQUIREMENTS AND PERMITS DELIVERED (COUNTY-LEVEL)

	Log permits delivered per 100,000 capita					
	(1)	(2)	(3)	(4)	(5)	(6)
Permit fees	-0.006***		-0.006***	-0.008***	-0.005***	-0.003***
	(0.002)		(0.001)	(0.001)	(0.001)	(0.000)
Training hours:						
No hours				Baseline		
Some unfixed hours		-0.442***	0.004	0.162**	-0.092	Omitted
		(0.063)	(0.088)	(0.080)	(0.081)	(.)
1 to 8 hours		-0.333***	0.060	-0.149**	-0.187**	Omitted
		(0.061)	(0.074)	(0.071)	(0.073)	(.)
more than 8 hours			No data			
Density				-0.000***	-0.000***	-0.000
				(0.000)	(0.000)	(0.000)
HH income				-0.000***	-0.000***	0.000
				(0.000)	(0.000)	(0.000)
Poverty				-3.235***	-3.367***	-0.061
				(0.658)	(0.661)	(0.549)
Unemployment				1.023	-0.448	-0.387
				(0.988)	(1.072)	(0.581)
Firearm suicide ratio			No data			
Republican votes				4.618***	1.713***	1.232**
				(0.326)	(0.390)	(0.575)
Constant	7.611***	7.391***	7.604***	6.275***	8.275***	6.362***
	(0.143)	(0.045)	(0.056)	(0.341)	(0.375)	(0.473)
County fixed effect	no	no	no	no	no	yes
Year fixed effect	no	no	no	no	yes	yes
Observations	11,416	12,336	11,416	11,416	11,416	11,416
R^2	0.064	0.035	0.064	0.230	0.370	0.825

Note: Univariate, multivariate, and fixed effect linear regressions of permits per capita on training requirements, with controls. The data covers our whole county-level sample of 981 counties over 10 states over 23 years (2000-2022), with not all counties delivering permits every year. Data and sources are described in the text. Standard errors in parenthesis and clustered at the county level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F.1. No anticipation

We find no evidence that changes in hours or fees are anticipated by CCW applicants, as there is no systematic bunching in permitting per capita in the years preceding or following policy adoption.

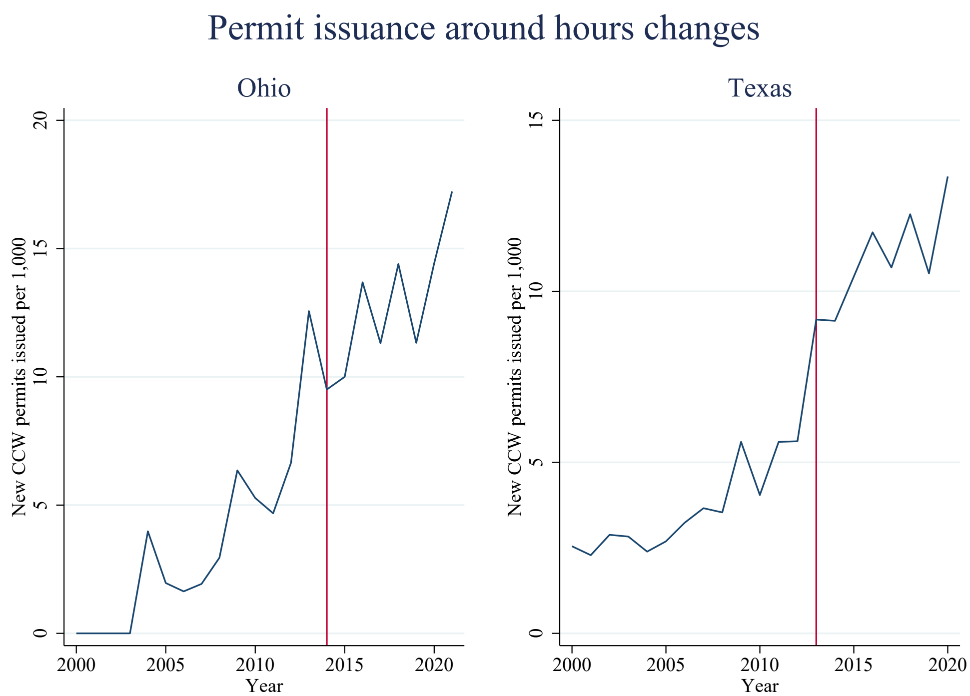

Figure A19. No anticipation of hours changes

Both Ohio and Texas switch from requiring more than 8 hours of training to 8 or fewer hours, in the years indicated by the vertical red lines.

F.2. Different transformations of NCVS outcomes

Rates of NCVS defensive action are highly skewed, but cannot simply be log-transformed because they are frequently zero in less populous states—particularly the more-specific defensive gun use response. While our main results in Figure 8 are calculated under the inverse hyperbolic sine transform, applying the negative logarithm and cubic root transforms yield extremely similar causal pictures.

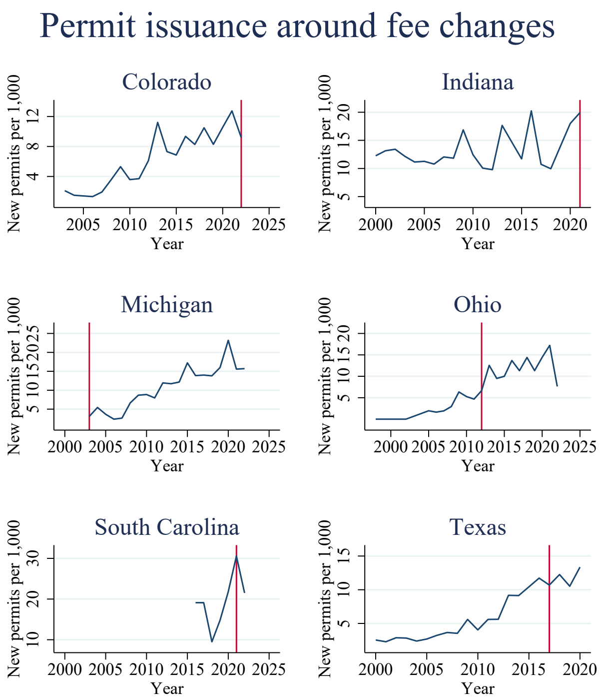

Figure A20. No anticipation of fee changes

Colorado, Indiana, Michigan, and Ohio increase their fees by one bin (up to \$50), while South Carolina and Texas decrease their fees by one and two bins (up to \$100), respectively, in the years indicated by the vertical red lines.

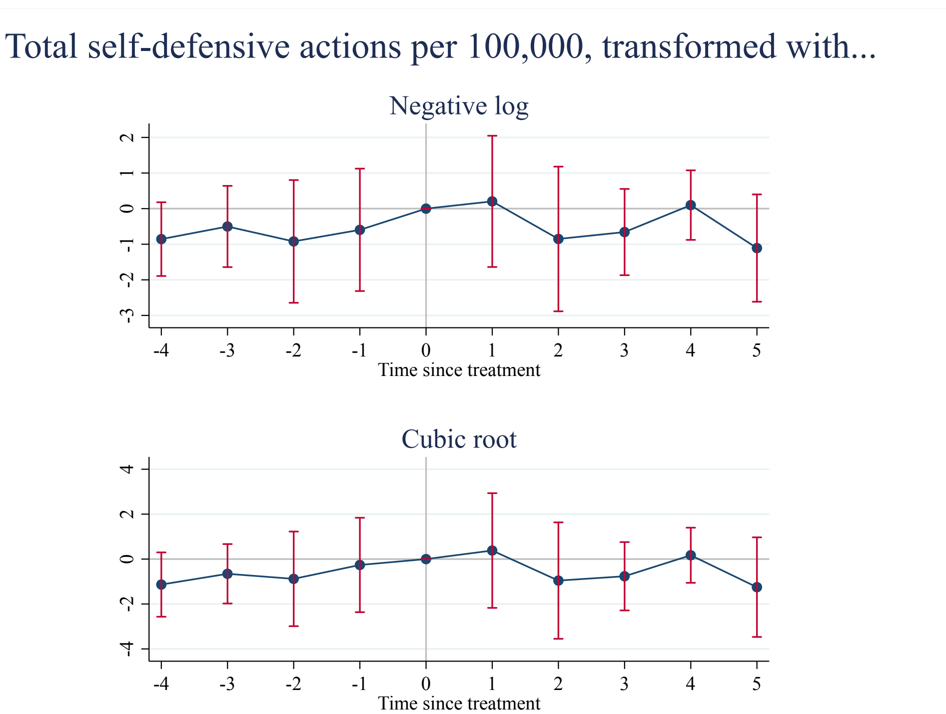

Figure A21.

Compare this plot to the upper panel of Figure 8.

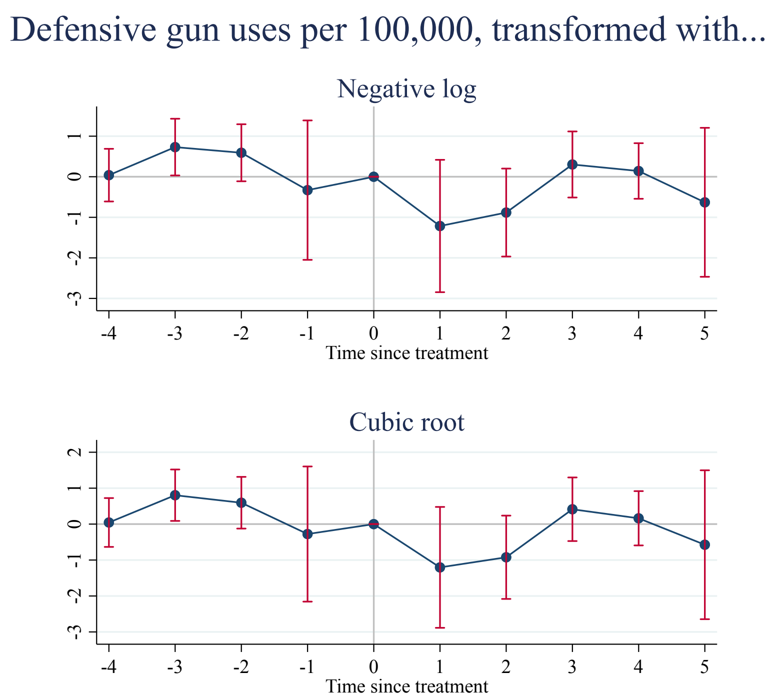

FIGURE A22.

Compare this plot to the lower panel of Figure 8.