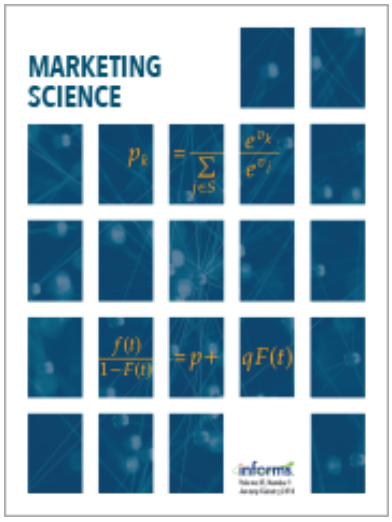

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Frontiers: The Impact of Loosening Concealed Carry Laws on Firearm Demand

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Abstract. Since 2021, 14 U.S. states have loosened Concealed Carry Weapon (CCW) laws. This research investigates the impact of loosening CCW laws on legal firearm purchases. Specifically, we explore CCW Shall Issue adoption, which removes local authority discretion on permit issuance, and CCW Permitless Carry, the least restrictive policy. We construct both state- and county-month panel data sets covering 2010 to 2017, using background checks and online firearm retail purchase data. We find that CCW Shall Issue adoption increases gun purchases, particularly new handguns. Over 70% of this increase is driven by repeat gun buyers. The increase in handguns induced by CCW Shall Issue is substantially greater in high-crime and urban areas. In contrast, CCW Permitless Carry has no effect on gun purchases.

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Keywords: firearm policy • firearm purchases • concealed carry laws

1. Introduction

Firearm purchases and deaths are on the rise. Background checks increased by 95% from 2010 to 2019 (14.3 million to 28 million) and reached an all-time high of 39.7 million in 2020 (The Economist 2021). The National Sports Shooting Foundation (NSSF), an industry lobbying nonprofit organization, estimates that the U.S. firearm industry contributes about 80 billion dollars to the American economy (NSSF 2023). The U.S. gun death rate increased by 17% from 2010 to 2019, with suicides accounting for 60% of all gun deaths in 2019 and homicides accounting for 36% (CDC 2021).

How states should regulate firearm ownership and usage is a key debate. In 2022, the U.S. Supreme Court ruled that people have the right to carry for self-defense in public, garnering increased attention. Since 2000, 11 states and Washington, DC, have adopted Concealed Carry Weapon (CCW) Shall Issue, removing local authority discretion on concealed carry permit issuance. Additionally, 28 states eliminated permit requirements entirely, with 14 states making this change since 2021.[1] Such deregulation on gun usage may motivate more gun purchases, but the extent is unclear across regulation levels.

We investigate the impact of CCW deregulation on legal firearm purchases using data from an online firearm retail platform and background checks. We construct both state- and county-month panel data sets from 2010 to 2017 and use a generalized difference-in-differences approach to account for the staggered policy adoption timing. We find that CCW Shall Issue adoption increases online handgun purchases (+21%) and handgun background checks (+35%) compared with when the law remains more restrictive, with no evidence of substitution from long guns to handguns. There is no statistically significant increase in firearm purchases following CCW Permitless Carry adoption.

We decompose the post-CCW Shall Issue handgun increase by different gun types, buyer characteristics, and county traits. CCW Shall Issue induces new handgun purchases, particularly those with calibers suitable for concealed carrying. CCW Shall Issue has a stronger effect on repeat gun buyers who have purchased three or more guns before and those who purchased both handguns and long guns previously. The handgun increase after CCW Shall Issue adoption is substantially larger in high-crime and urban areas, where even long gun purchases increase. Though CCW Shall Issue increases gun purchases, we do not find conclusive evidence of the law impacting gun mortality. We find modest evidence of CCW Permitless Carry posing long-term risks on gun mortality, particularly suicides, but the findings remain inconclusive because of limited statistical power.

2. Literature Review and Contribution

To the best of our knowledge, this paper is the first to explore CCW policy effects on firearm purchases using actual firearm transaction data covering the entire United States. Other papers use proxy measures. Duggan (2001) uses *Guns & Ammo* magazine sales data and finds no evidence of CCW Shall Issue adoption on sales. Steidley and Kosla (2018) use annual background check data from 1999 to 2010 and do not find a relationship between permissive CCW laws and firearm checks, but when expanding the work to 2016, Steidley (2019) finds that CCW Shall Issue and CCW Permitless Carry are both positively associated with handgun checks. The transaction data in this paper enable investigation of the policy impact on demand by gun type, brands, prices, and buyer characteristics, which is not possible with background check data.

This paper also relates to the literature at the intersection of marketing and policy that explores the impact of regulation on demand in various markets. We contribute by exploring the effects of deregulation on firearm demand. Moshary et al. (2023) estimate firearm demand based on choice-based conjoint analysis survey data and run simulations to estimate how firearm demand may shift for restrictive policies such as assault weapon bans, handgun bans, and a firearm tax. Other research scholars explore demand shifts induced by a soda tax (Seiler et al. 2021) and tobacco regulation like place-based smoking bans and taxation (Evans et al. 1999, Wang et al. 2016). This paper examines the effects of deregulation on various types of gun demand.

3. Empirical Context and Data

3.1. Concealed Carry Laws

CCW laws regulate the concealed carrying of handguns. Four types of regulation are possible across states:

- **CCW Complete Prohibition:** Restricts all private citizens from concealed carrying.
- **CCW May Issue:** Selectively grants permits at the discretion of local law authorities who evaluate the specific need for firearm use.
- **CCW Shall Issue:** Grants permits if one meets all objective state requirements (e.g., no criminal record, proof of residency, no mental illness, background check). Removes local authority discretion in permit issuance.
- **CCW Permitless Carry:** Allows concealed carrying without a permit.

CCW Shall Issue and CCW Permitless Carry make concealed carrying easier. The rise in these laws is linked to gun rights mobilization and a gun culture centered on self-defense, encouraging legal protective firearm ownership (Steidley 2019). Permissive CCW laws motivate ordinary citizens to see themselves as social actors of protection against crime (Carlson 2014). Consequently, these laws may increase demand for handguns (pistols and revolvers), with the extent of the increase varying based on the degree of permissiveness. Intuitively, more permissive laws could lead to greater demand. However, in states with strong gun culture and higher gun ownership, the permissive laws may induce existing gun owners to carry their current firearms rather than purchase new ones. Therefore, the effects of the laws on gun purchases remain uncertain. Permissive CCW laws may also decrease long guns (shotguns and rifles) if consumers substitute them with handguns.

During the sample period (2010 to 2017), no state adopted a more restrictive CCW policy. Many states adopted CCW Permitless Carry toward the end of the sample period, which is a limitation for estimating long-term effects. See Web Appendix Table A.1.1 for each state's law adoption date, retrieved from the RAND State Firearm Law Database.

Some states adopt Stand Your Ground (SYG; also known as No Duty to Retreat) during the sample period. SYG allows a person in imminent danger to use lethal force at home or in public without first retreating (Light 2017). States without SYG have the Castle Doctrine, in which one has a duty to retreat in public. We remove states that adopt both CCW and SYG during the sample period if the laws are adopted within two years of each other and then remove all states that only adopt SYG during the sample period. As a result, we remove six states from the analysis and control for SYG existence in the model estimation.

3.2. Firearm Demand

Measuring firearm demand is difficult because of lack of data available across all states. We use transaction data from an online firearm retail platform and background checks.

3.2.1. Online Firearm Purchases. Firearm transaction data are from an online U.S. firearm retail platform that connects buyers and sellers of new and used guns. Both federal firearm licensed dealers (FFLs) and private sellers can sell on this platform. Once a buyer purchases a firearm from the platform, the seller ships the firearm to a local FFL who runs a background check on the buyer. This platform has national coverage and is one of the largest online firearm marketplaces. Its sales account for roughly 2% of the average estimated annual gun sales (13.9 million per year) for the period of 2010 to 2017 (Gabriele 2023). We acknowledge the representativeness as a limitation, but we note that these data are highly correlated with background checks as found in Kim and Wilbur (2022). We compare the analysis results with those of background checks.

Another key advantage of this data is the detailed transaction information including firearm type, price, product characteristics, purchase date, buyer purchase history, and zip code. Given the data granularity, we analyze the data by county-month to investigate heterogeneity in policy effects across buyer types and locations (e.g., crime levels, urban population, and political affiliation).

3.2.2. Background Checks. A background check is required before gun purchase from an FFL. Therefore, background checks (BGCs) are a well-known metric for aggregate measures of firearm prevalence. The Federal Bureau of Investigation's National Instant Criminal Background Check System (NICS) publishes the number of BGCs by state-month. We use BGCs as an alternative measure to complement the online purchase measure.

We note some data weaknesses. BGCs are not required when a buyer purchases from a private seller. The number of BGCs indicates the number of checks conducted, not the number of people seeking a check or the number of checks passed. Not all BGCs indicate a firearm purchase, and one person can have multiple BGCs in a given month. We only focus on BGCs conducted by FFLs for gun purchases only. Also, states manage BGCs differently. Point-of-Contact (POC) states conduct all BGCs through their local agencies, whereas partial POC states manage handgun checks locally but require FFLs to contact NICS for long gun checks (FBI 2024). We account for the differences in POC status by controlling for whether the state is full POC, partial POC, or non-POC in the analysis.

3.3. Summary Statistics

To estimate the impact of the laws separately, we split the sample into two conditions as shown in Table 1. The CCW Shall Issue condition includes treated states that adopt CCW Shall Issue and control states that remain more restrictive. The CCW Permitless condition contains treated states that adopt CCW Permitless Carry and control states that remain CCW Shall Issue. No state adopted CCW Permitless Carry directly from CCW May Issue. Although the sample is split at the state level, we analyze online gun purchases at both state and county level. The latter allows better investigation of the heterogeneity across buyer types and locations (e.g., crime levels, urban population). BGC data are only available by state, so we analyze BGCs only at the state level.

Table 2 compares prepolicy characteristics of law-adopting and nonadopting counties and states within each policy condition mentioned in Table 1. In both conditions, the treated states have more firearms (both online purchases and BGCs) than the control states. About half of the gun buyers in the sample have purchased multiple guns in both conditions (see Web Appendix Section A.2). Significant differences in gun purchases and county traits like population size and political affiliation are evident, even before policy adoption. More strictly regulated counties lean more Democrat. Although our identification strategy does not require balanced pretreatment outcomes or covariates, the intrinsic differences between the treated and untreated counties emphasize the importance to control for these differences in the analysis.

Our main analysis leverages the timing of the policy adoptions to estimate the impact of loosening concealed carry laws on firearm purchases. Figure 1 shows model-free plots comparing average online firearm purchases and BGCs for the treatment and control groups,

Table 1. Sample Groups

CCW Shall Issue condition	CCW Permitless condition
We compare treated states that switch from CCW Prohibited/CCW May Issue to CCW Shall Issue with control states that remain CCW Prohibited/CCW May Issue.	*We compare treated states that switch from CCW Shall Issue to CCW Permitless Carry with control states that remain CCW Shall Issue.*
Treated states: DC, IA, WI, IL	Treated states: NH, ID, ME, ND, WY, AZ, KS, MS, WV
Control states: CA, DE, HI, MD, MA, NY, RI, NJ	Control states: AR, MN, NE, CO, CT, NM, OH, OR, VA, WA, AL, FL, GA, IN, KY, LA, MI, MT, OK, SC, SD, TN, TX, UT

Notes. Sample period: 2010–2017. We split the data to estimate the effects of CCW Shall Issue and CCW Permitless Carry, ensuring treated and control states share the same CCW status before the policy change. We remove states that adopt both SYG and CCW laws within two years of each other during the sample period along with states that only adopt SYG during the sample period. This removes six states (MO, AK, VT, NV, NC, and PA) from the analysis.

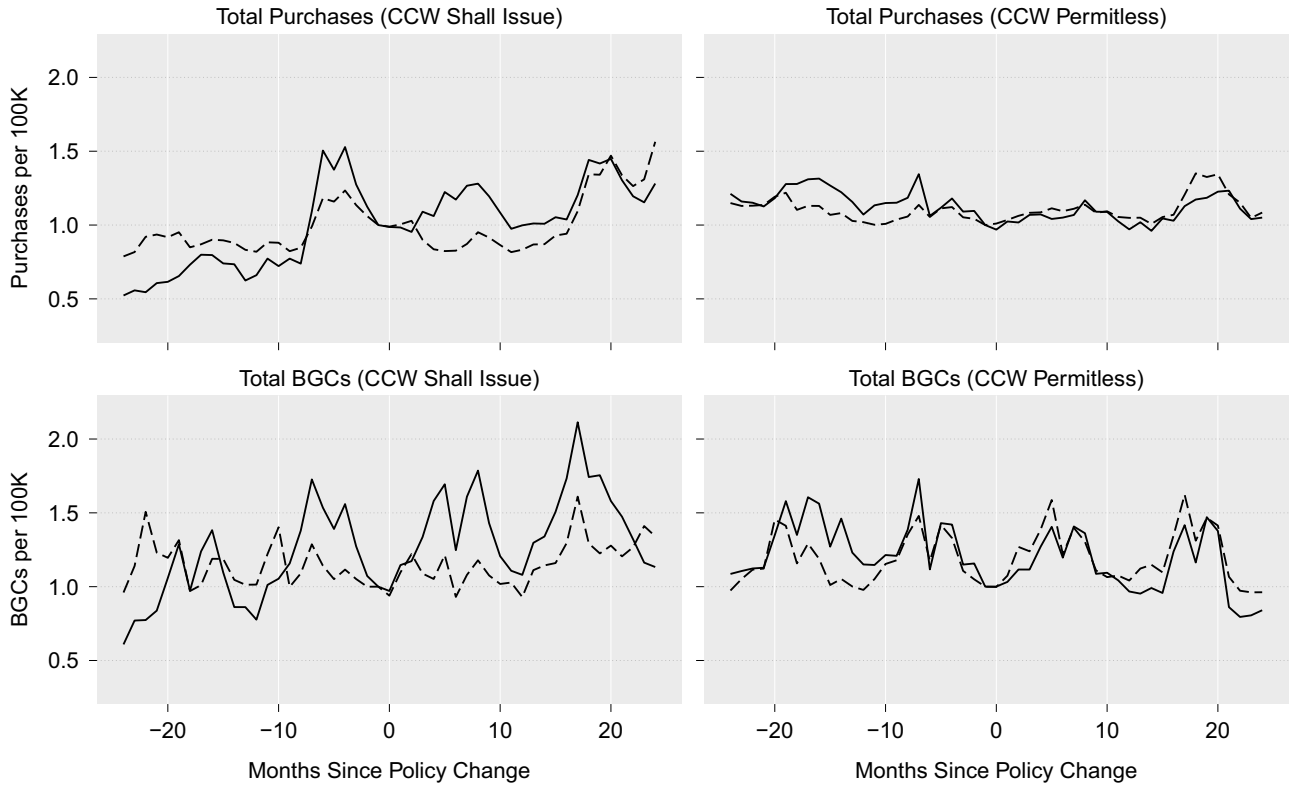
Table 2. Summary Statistics

	CCW Shall Issue			CCW Permitless		
Variable	Treatment	Control	*p*-value	Treatment	Control	*p*-value
Panel A: Purchases per 100K						
Total	12.39	4.41	1.02×10^{-43}	20.24	14.61	2.00×10^{-22}
Handgun	6.41	1.97	1.24×10^{-53}	10.36	8.06	2.88×10^{-14}
Long gun	5.92	2.42	1.24×10^{-31}	9.81	6.50	3.94×10^{-28}
Panel B: BGCs per 100K[a]						
Total	225.31	144.80	1.86×10^{-8}	516.17	414.50	6.88×10^{-3}
Handgun	112.01	65.67	5.89×10^{-5}	255.98	224.64	1.24×10^{-1}
Long gun	138.25	73.23	2.17×10^{-10}	239.58	170.93	1.84×10^{-4}
Panel C: County traits						
Counties (*n*)	274	192		403	1,932	
Population (1,000)	80.90	422.17	7.03×10^{-215}	41.26	79.58	4.93×10^{-65}
Gun deaths per 100K	0.72	0.56	4.64×10^{-7}	1.19	1.13	5.84×10^{-2}
Democrats	58.05%	61.33%	5.03×10^{-3}	40.35%	45.94%	2.23×10^{-49}
Urban	80.44%	96.58%	1.07×10^{-26}	64.32%	82.44%	4.31×10^{-19}

Notes. Panels A and B show the average per 100K of the corresponding measure a year before the policy adoption date for treated and control counties/states in each condition from Table 1. Panel C presents population-weighted county traits using 2010 census data. Democrats indicate the Democratic vote share in the nearest presidential election term. The *p*-values indicate significant differences between treatment and control groups. See Web Appendix Section A.3 for data sources.

[a]BGC data are only available at the state level.

Figure 1. Online Purchases/BGCs per 100K 24 Months Before and After Policy Adoption



Notes. Graphs in the first column from top to bottom show total online purchases and BGCs per 100K for the CCW Shall Issue condition. Graphs in the second column are for the CCW Permitless condition. The solid line represents the treated group, and the dashed line represents the corresponding control group. For the treated group, we calculate the weighted average purchases for each event time across all counties (states for BGCs) in the treated states; for the control group, we calculate the weighted average purchases across all control counties (states for BGCs) across all of the different treated states' law adoption timelines. The lines are normalized by their period −1 values (one period before the policy change).

covering 24 months before and 24 months after for CCW Shall Issue and CCW Permitless Carry adoption. We show model-free plots across gun types and by state in Web Appendix Sections A.4.1 and A.4.2. Overall, we do not observe parallel trends between treated and control regions before the policy adoption.[2] As such, we use an estimation method that allows for more flexibility on parallel trends after conditioning on covariates.

4. Empirical Strategy and Results

4.1. Identification Strategy

Identifying the impact of loosening CCW laws on gun demand presents some challenges. First, the decision to relax concealed carry regulations could potentially correlate with local conditions such as crime rates and political affiliation. Second, states have adopted permissive CCW laws in a staggered fashion, making conventional methods like the two-way fixed effects (TWFE) approach inappropriate (Goodman-Bacon 2021).

To address these identification challenges, we employ the following strategies. First, we view CCW Shall Issue and CCW Permitless Carry as separate treatments. We split the sample and define the control groups separately for each law as in Table 1. This ensures comparability between the treatment and control groups in terms of prevailing CCW law status. Second, to account for the staggered law adoption timing, we use a generalized difference-in-differences (DiD) estimator introduced by Callaway and Sant'Anna (2021), noted going forward as CS.

The CS estimator identifies the policy effect by deconstructing the staggered design into separate pieces of two-period comparisons between the treatment and control groups. Suppose that we want to identify the policy effect at calendar time t for observations that adopt the treatment at time $g < t$ (in the Callaway and Sant'Anna (2021) terminologies, these treated units are labeled as $\{G = g\}$, where G is defined to be the period of policy change). The CS estimator starts with estimating the common trend from the reference period $g-1$ (one period before the policy change) to calendar time t as a function of the covariate X

$$m_{g,t}(x) = E_{Y_{it}(0), Y_{i(g-1)}(0)}[Y_{it}(0) - Y_{i(g-1)}(0) \mid X_i = x],$$

using observations from the control group. If the researchers suspect that periods closer to the policy change are subject to anticipation bias, it is also possible to use other pretreatment periods as the reference period to estimate the common trend $m_{g,t}(\cdot)$. In Web Appendix Section B.3, we test if our results are robust to anticipation effects by replacing the reference point with the month before the bill announcement.

The CS estimator estimates the "group-time average treatment effect on the treated" (ATT) (Callaway and Sant'Anna 2021) by subtracting the common trend from the temporal changes in Ys among the treatment group $\{G = g\}$:

$$\mathrm{ATT}(g,t) = E_{Y_{it}, Y_{i(g-1)}, X_i}[Y_{it} - Y_{i(g-1)} - m_{g,t}(X_i) \mid G_i = g],$$

which captures the effect heterogeneity across both g and t.

For concise presentation of the effects, Callaway and Sant'Anna (2021) suggest several ways to aggregate the group-time specific ATT. In the result section, we show event-study plots that aggregate the ATT by the event time $e = t - g$, and we report the "1-year ATT," which is the average of the 12-month-postadoption ATTs, in the regression table. More detail on the CS estimator is provided in Web Appendix Section B.1.

Our specification for the common trend is as follows:

$$Y_{it}(0) = \theta_t + \eta_i + X_{it}'\beta_t + v_{it},$$

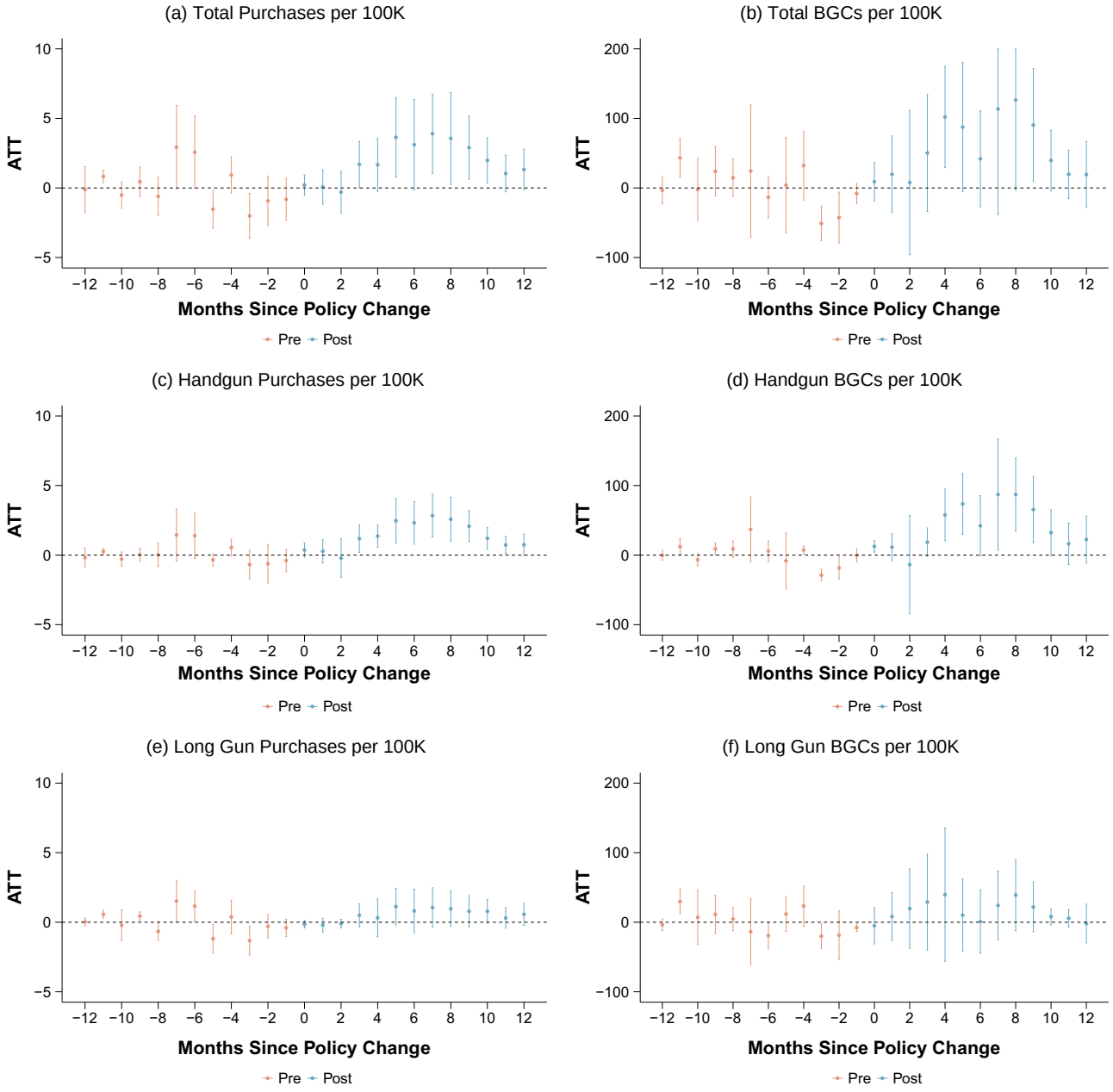
where Y_{it} is the outcome variable per 100K, θ_t is the time fixed effect, η_i is the state/county fixed effect, and X_{it}s are the covariates. Note that the effect of covariates on the untreated outcome, β_t, is allowed to vary across time in the CS framework.[3] In our main specification, we control for SYG existence, lagged gun deaths per 100K, and the vote share of Democrats in the nearest presidential election. For BGCs, we additionally control for the differences in collection and reporting of BGCs across states. We discuss the rationale for the choice of control variables and perform robustness checks with alternative specifications in Web Appendix Sections B.2 and D.1. We weight the observations by state/county population to account for the fact that larger states/counties have more absolute amounts of the outcomes. Standard errors are bootstrapped at the state level to allow for correlations between counties in the same state.

We conduct analyses to address potential identification threats such as spillovers and price effects. A violation of the Stable Unit Treatment Value Assumption (SUTVA) could occur if the change in demand by the policy shift in the treated states affects the gun prices in untreated states. We find that treated states only account for a small portion of all purchases (less than 5%), so a general equilibrium effect is unlikely. We also do not find any policy impact on gun price after controlling for gun characteristics like caliber size and brand, suggesting that price changes are not likely to be the driver of our results. Detailed evidence of these analyses is documented in Web Appendix Sections B.4 and B.5.

4.2. Policy Effects on Gun Purchases

4.2.1. State-Month. Figure 2 shows the CCW Shall Issue adoption's effect on online purchases and BGCs over time. Postpolicy, total and handgun purchases and BGCs gradually increase and then decline. The pretrend estimates do not significantly deviate from zero, so we do not observe a violation of the parallel trends

Figure 2. CCW Shall Issue ATT Over Time—Online Purchases vs. BGCs (State-Month)



Note. The error bars are the pointwise 95% confidence intervals with bootstrapped standard errors clustered by state.

assumption. Table 3 compares the average ATT between online purchases and BGCs. Total online purchases and total BGCs both increase, but the latter is less precise. There is an average increase of 1.37 online handgun purchases per 100K, and 39.2 handgun BGCs per 100K. The growth in online handgun purchases relative to the pre-policy benchmark is smaller than that of handgun BGCs (+21.37% versus +34.99%). We do not observe a substitution from long guns to handguns, as we see neither an increase or decrease in long gun purchases/BGCs.

In Table 3, we show the difference in percentage growths between online purchases and BGCs for each gun type (see column labeled "Comparison"). Assuming the online purchase and BGC estimates are independent, we conduct a *t*-test to assess whether the percentage growths are significantly different between online purchases and BGCs. Overall, the *p*-values show that the percentage growth differences are not statistically significant, partially because the BGC estimates are less precise.

Unlike CCW Shall Issue, CCW Permitless Carry has no effect on gun demand as shown in Table 3 (see Web Appendix Figure C.1.1 for event-study plots). The change from CCW Shall Issue to CCW Permitless Carry

Table 3. Online Purchases vs. BGCs (State-Month)

	Online purchases		BGCs		Comparison	
Firearm type	1-year ATT	Δ_1%	1-year ATT	Δ_2%	Δ_1% − Δ_2%	*p*-value
Panel A: CCW Shall Issue						
Total	1.89 (0.73)***	15.29%	55.68 (37.96)	21.81%	−6.52%	0.68
Handgun	1.37 (0.28)***	21.37%	39.20 (18.58)**	34.99%	−13.62%	0.43
Long gun	0.52 (0.39)	8.71%	15.24 (9.95)	11.02%	−2.31%	0.81
Panel B: CCW Permitless Carry						
Total	0.23 (0.68)	1.13%	−8.72 (11.07)	−1.69%	2.82%	0.49
Handgun	−0.04 (0.64)	−0.37%	−6.84 (8.76)	−2.67%	2.31%	0.75
Long gun	0.24 (0.42)	2.46%	−2.01 (13.82)	−0.84%	3.30%	0.65

Notes. Coefficients represent the policy effects on the corresponding outcomes per 100K with bootstrapped standard errors clustered by state in parentheses. Observations are weighted by state population. Δ_1% and Δ_2% represent the percentage growth in online purchases and BGCs, respectively, compared with the corresponding benchmark (the average per 100K one year before the policy adoption for the treated group). Δ_1% − Δ_2% indicates the difference in these percentage growths. We conduct a *t*-test on whether the percentage growths in online purchases and BGCs are the same, assuming that the two estimates are independent. The *p*-values indicate the statistical significance of the *t*-statistic.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

may not induce more purchases as those legally eligible for concealed carrying may already have purchased guns before.

4.2.2. County-Month. We find similar results at the county-month level for online purchases as in the state-month results, where CCW Shall Issue increases total and handgun purchases, whereas CCW Permitless Carry has a null effect. See Web Appendix Figure C.1.2 for event-study plots. There is an average increase of 1.48 handguns per 100K in one year post-CCW Shall Issue, a 23.09% increase relative to the prepolicy benchmark as shown in Table 4. We cannot analyze BGCs at the county-month level because the NICS only provides state-level data.

4.3. Exploring CCW Shall Issue Effects on Gun Purchases

In this section, we decompose the gun purchase ATT estimate further into different gun and buyer characteristics. Panel A in Table 4 shows that 70% of the county-level one-year ATT estimate for total gun purchases post-CCW Shall Issue consists of handguns. In panel B, we compare the composition of handgun subcategories against that of the benchmark, which are the proportions one year before the law adoption for the counties in law-adopting states. We find that CCW Shall Issue induces purchases of new, popular-brand handguns. New handgun purchases account for 60% of the policy-induced handgun purchases, which is a notable shift from the benchmark (49%). More postpolicy purchases are of handguns with caliber sizes suitable for concealed carrying (ATT versus Benchmark: 75% versus 65%).[4]

In Web Appendix Section C.2, we decompose the post-CCW Shall Issue handgun increase by buyer and county traits. About half of the purchases come from repeat buyers who previously purchased more than three guns (ATT versus Benchmark: 51% versus 40%) and those who previously purchased both handguns and long guns (ATT versus Benchmark: 45% versus 39%). The post-CCW Shall Issue handgun increase is substantially larger in high-crime (+27.72%) and urban areas (+25.30%). The results are similar after adjusting for multiple testing. We also observe some increase in long guns in these areas, where loosening CCW laws may make guns more valuable. Further details on the

Table 4. ATT Decomposition by Gun Type (County-Month)

Firearm type	1-year ATT	%	Benchmark	%
Panel A: CCW Shall Issue				
Total	2.12 (1.02)**	100%	12.39	100%
Handgun	1.48 (0.47)***	70%	6.41	52%
Long gun	0.64 (0.54)	30%	5.92	48%
Panel B: CCW Shall Issue—by handgun types				
All handguns	1.48 (0.47)***	100%	6.41	100%
Top 5 brand	0.75 (0.29)***	51%	3.20	50%
New	0.88 (0.36)**	60%	3.16	49%
Expensive	0.59 (0.37)	40%	3.39	53%
CC*	1.10 (0.38)***	75%	4.17	65%
Panel C: CCW Permitless Carry				
Total	0.23 (1.65)	100%	20.24	100%
Handgun	0.02 (1.74)	11%	10.36	51%
Long gun	0.20 (1.81)	89%	9.81	49%

Notes. Coefficients represent the policy effects on the corresponding purchase outcome per 100K with bootstrapped standard errors clustered by state in parentheses. The benchmark indicates the average purchases one year before the policy adoption for the treated group. % indicates the proportion of the gun type within each panel. The top five brands include Smith & Wesson, Ruger, Colt, Sig Sauer, and Glock. CC* indicates handguns with caliber sizes suitable for concealed carrying. Expensive handguns are those priced higher than the median ($499). Observations are weighted by county population.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

high-crime and urban county analysis are in Web Appendix Section C.3.

5. CCW Policy Effects on Gun Mortality

We explore how loosening CCW laws affects gun mortality. We focus on gun homicides, gun suicides, and unintentional gun deaths, given their prevalence in American society. Fewer restrictions on concealed carrying could enable more people to defend themselves, but their impact on gun violence remains debated. Increased concealed carrying could deter criminals and reduce gun homicides, or it might escalate conflicts and increase gun homicides. Permissive CCW laws may allow easier gun access for those at risk for suicide, which constitutes the majority of gun deaths. These laws may encourage gun use over storage, raising the risk of unintentional gun deaths, such as from mishandling loaded firearms.

Using mortality data from the Centers for Disease Control and Prevention (CDC), we analyze gun mortality by state-month for comparability with the literature. We use the CS estimator as we did for gun purchases, but we change the covariates to include lagged total BGCs per 100K (to control for gun demand), lagged unemployment, and alcohol consumption levels. We do not observe evidence of a violation of the parallel trends assumption. See Web Appendix Section C.4 for more details on the data, method, event-study plots, and robustness checks.

The one-year ATT for each CCW Shall Issue and CCW Permitless Carry is not statistically different from zero, and the 95% confidence intervals are wide as shown in Table 5. Therefore, we cannot provide conclusive evidence on each law's short-term effects. In Web Appendix Section C.4, we extend the analysis to 24 months considering that it may take longer for the proportion of concealed carriers induced by deregulation to stabilize (Schell et al. 2020). We find modest evidence of a possible increase in total gun deaths and gun suicides after CCW Permitless Carry adoption in the long run as the average two-year ATT for each is statistically significant, but the 95% confidence intervals barely exclude zero. The significance is also not robust to adjustment for multiple testing. Overall, we find inconclusive evidence of the impact of loosening CCW laws on gun mortality because of limited statistical power.

Table 5. ATTs for Gun Deaths

Mortality	1-year ATT	95% CI	Benchmark
Panel A: CCW Shall Issue			
Total gun deaths	−0.006 (0.069)	[−0.141, 0.129]	0.718
Gun homicides	−0.024 (0.029)	[−0.081, 0.033]	0.305
Gun suicides	0.021 (0.058)	[−0.093, 0.135]	0.388
Unintentional gun deaths	−0.008 (0.008)	[−0.024, 0.008]	0.010
Panel B: CCW Permitless Carry			
Total gun deaths	0.099 (0.077)	[−0.052, 0.250]	1.192
Gun homicides	0.030 (0.051)	[−0.070, 0.130]	0.291
Gun suicides	0.062 (0.046)	[−0.028, 0.152]	0.847
Unintentional gun deaths	0.010 (0.010)	[−0.010, 0.030]	0.022

Notes. Coefficients represent the policy effects on the corresponding death outcome per 100K with bootstrapped standard errors clustered by state in parentheses. The benchmark indicates the average deaths per 100K one year before the policy adoption for the treated group. Total gun deaths also include gun deaths with undetermined intent. Observations are weighted by the state population.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

6. Discussion and Conclusion

Our findings on purchases have three key implications. First, the regulation in CCW Shall Issue does not appear burdensome to concealed carriers, as we observe increased purchases only with CCW Shall Issue adoption and not when states shift from CCW Shall Issue to CCW Permitless Carry. CCW Shall Issue particularly induces more handguns suitable for concealed carry and the increase is notable in high-crime areas, suggesting that CCW Shall Issue induces self-defensive gun use.

Second, handguns and long guns are not always substitutes. Post-CCW Shall Issue, handgun purchases rise without reducing long gun purchases. Many gun owners own both gun types. In high-crime, urban areas, permissive CCW laws may heighten the value of both handguns and long guns because of increased salience of gun usage, with gun dealerships facilitating purchases of both types.

Third, this paper demonstrates the consistency in findings using NICS background checks and proprietary online gun transaction data. This is noteworthy given the proprietary data's limitation in representativeness. Our findings align across both measures, suggesting robustness.

Although CCW Shall Issue increases gun purchases, especially in high-crime areas, we find no conclusive evidence of immediate effects on gun mortality. CCW Shall Issue, which still requires a permit, may attract more law-abiding gun owners. Conversely, CCW Permitless Carry does not affect purchases but may influence mortality directly because the law deregulates usage. Modest evidence suggests unrestricted carrying could pose long-term safety risks, though our mortality findings remain inconclusive because of substantial uncertainty in the estimates. With firearm regulation remaining controversial, continued research on firearm policies is important.

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Endnotes

[1] Source: RAND State Firearm Law Database and Brownlee (2024).

[2] See Web Appendix Section A.4.1 for more detail on the temporal spike for CCW Shall Issue in Figure 1 around event time −6. Our estimation method uses the period immediately before the policy change as the reference point, so the spike does not affect the effect estimates.

[3] In the state-month specification, we have to restrict $\beta_t = \beta$ because of the limited sample size. If we do not restrict $\beta_t = \beta$, the trend $Y_{it'}(0) - Y_{it}(0)$ has to be estimated using the subsample that consists of control states from period t' and t. As we split states into CCW Shall Issue and CCW Permitless conditions, each subsample would only consist of about 10 control states, and the coefficient estimate of β_t would be rather unreliable. By contrast, if we impose $\beta_t = \beta$, we can pool observations from multiple periods to estimate β.

[4] See Web Appendix Section C.2 for caliber size information.

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