The Partisan Effects of Firearm Access and Media*

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Abstract

Political identity shapes what Americans buy. We ask whether the reverse also holds, using firearms, the consumer good most tightly bound to partisan identity in the United States. Exploiting the staggered arrival of the first retail firearms dealer in 2,381 ZIP code areas that had none in January 2018, and comparing them with 12,255 areas that never gain one, we find that dealer entry raises Republican political giving by local men. The response begins within three to four months of entry; a year after entry, male Republican contributions are about 0.13 log points higher and the Republican share of men's two-party giving is 3 percentage points higher. The increase comes from new donors rather than larger gifts, and Democratic giving does not rise. Women respond later, toward the end of the first year. A survey experiment with equal samples of gun owners and non-owners ($N = 2{,}000$) shows that a brief, organic firearm video raises Republican affect among men who do not personally own guns by 10 points on a 100-point thermometer and moves non-owning men and women apart in their support for less strict gun laws. Together, the evidence indicates that access to firearms shifts partisan allegiance, and that the effect is not confined to people already invested in guns.

JEL codes: D12, D72, D91, L81, Z13

Keywords: firearms, partisanship, identity goods, campaign contributions, media, survey experiment

*We thank Verasight for fielding the survey experiment. The experiment was approved by the Case Western Reserve University Institutional Review Board and pre-registered, with a pre-analysis plan, on the AEA RCT Registry.

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1 Introduction

Few consumer goods are as entangled with American political identity as firearms. About a third of U.S. adults personally own a gun, but ownership is sharply partisan: 45 percent of Republicans and Republican-leaning independents own one, compared with 20 percent of Democrats and Democratic leaners (Pew Research Center, 2023). Political elites have sorted even more sharply. Among House and Senate candidates, the share of Democrats who support stricter gun control rose from about 70 percent in 2008 to 97 percent in 2022, while the share of Republicans fell to about 4 percent (Figure 1). A gun is no longer only a tool for hunting, sport or self-defense. It has become a marker of who one is and which side one is on (Lacombe, 2021; Lacombe et al., 2019).

A large literature in economics and political science documents that political identity shapes consumption. Americans renamed French fries and poured out French wine when France opposed the invasion of Iraq (Chavis and Leslie, 2009; Pandya and Venkatesan, 2016); partisans boycott and "buycott" brands whose executives take political stands (Liaukonyte et al., 2023; Endres and Panagopoulos, 2017); and brand preferences, media diets and even food choices now predict partisanship with remarkable accuracy (Bertrand and Kamenica, 2023; Schoenmueller et al., 2023). The reverse direction has received far less attention. Does acquiring, or simply gaining access to, a politically coded good change a person's political allegiance? If it does, consumption is not only an expression of identity but one of its causes, and the diffusion of a product can reshape the political landscape.

Firearms are a natural setting for this question, and the answer bears on a long-standing puzzle in American politics. Guns are associated with more than 40,000 deaths a year (Centers for Disease Control and Prevention, 2025), and mass shootings recur with grim regularity, yet federal gun policy has been remarkably stable and public support for gun rights remarkably resilient (Goss, 2006; Luca et al., 2020; Barney and Schaffner, 2019). One explanation, long suggested by political scientists, is that gun ownership itself creates a constituency: owning a gun draws people into a community and an identity that the gun-rights movement has tied to the Republican Party (Lacombe, 2021). If that is correct, the spread of guns could be self-reinforcing politically, and each new owner could be a potential recruit to the coalition opposing regulation. The potential for this to be the case is of particular importance given the surge in gun buying that occurred starting in 2020 during the Covid-19 pandemic—a surge that included many first–time buyers—as well evidence indicating shifts in the demographics of gun ownership over the last 10 years (Miller et al., 2022). Testing this idea is difficult, however, because people who choose to buy guns differ in many ways from people who do not, and because one's political views

shape one's decision to buy.

This paper provides causal evidence that access to firearms shifts partisan political behavior, using two complementary research designs. The first, which is our primary design, exploits the opening of retail gun stores. Federal Firearms Licensees (FFLs) are the legal gateway to the firearms market: every sale by a licensed dealer requires a federal background check, handguns may cross state lines only through a dealer in the buyer's state, and firearms bought online must be shipped to a dealer for transfer.[1] We assemble monthly data on the location of every licensee in the United States from 2018 to 2025 and link them to the universe of itemized individual political contributions reported to the Federal Election Commission, aggregated to ZIP Code Tabulation Areas (ZCTAs). We then study 14,636 ZCTAs that had no retail dealer in January 2018, comparing the 2,381 that gain a first dealer during the panel with the 12,255 that never do, month by month around the date of entry. Restricting attention to true first entries into places with no dealer gives treated and comparison units a common baseline and isolates the margin of interest: the arrival of access where there was none.

The arrival of a first dealer is followed by a sharp increase in Republican giving by local men. No changes occur in the month of entry. Within three to four months, the number of male Republican donors is about 3 percent higher and the Republican share of men's two-party giving is 2 to 3 percentage points higher. The effect persists and grows over the first year: at twelve months, male Republican dollars are 0.13 log points higher, the number of male Republican donors 0.05 log points higher, and the Republican share of two-party giving, whether measured in dollars or in donors, 3 percentage points higher. Averaged over all post-entry months, the effects are slightly larger still. Two-way fixed-effects and heterogeneity-robust estimators (Callaway and Sant'Anna, 2021) give nearly identical answers at every post-entry event time. Because the dollar-weighted and donor-weighted shares move by the same amount, the increase comes from new Republican donors rather than larger contributions by existing ones. Democratic giving in the same places does not rise.

Several features of these results support a causal reading. Within the year before entry, event-study coefficients are small and their average is statistically insignificant, and the break at entry is sharp. The response is specific to Republican giving. Above all, the timing is hard to reconcile with the most natural confounder, a change in the composition of the local population. Partisan residential sorting operates slowly and is limited in magnitude (Mummolo and Nall, 2017; Martin and Webster, 2020), and in-migration of

[1] Rosenberg (2026) shows that the entry of a first gun store into a California ZIP code raises local handgun purchases by about 30 percent one year after entry.

Republican-leaning households would raise giving by men and women at the same time. Instead, the male response materializes within three months, while the response among female (or unclassified) donors is flat for the first half-year and appears mostly in months 10 to 12. By the second year, the two converge. We view this gender gap in timing as evidence of a genuine behavioral response that reaches men first, and it motivates the gender-specific analysis of our experiment. Scaled by Rosenberg's first-stage estimate, our results imply that a 10 percent increase in local gun purchases is associated with a rise of about 1 percentage point in the Republican share of men's two-party political giving.

Observational evidence of this kind leaves two questions open. The first is identification. Dealers choose where to open, and although the within-window pre-trends are flat, political giving in entrant ZCTAs had already stepped up, for both parties, more than a year before the first dealer arrived. A skeptic might worry that some unobserved local shock both attracts dealers and moves politics. The second question concerns mechanism. A new store could change politics by intensifying the partisanship of people who already own guns, or by bringing people who did not previously own guns into contact with firearms and the identity attached to them. The two channels have different implications: the first describes a reinforcement of existing divisions, the second the recruitment of new partisans through access.

We designed a randomized survey experiment to address both questions. Verasight recruited 2,000 U.S. adults, stratified by personal gun ownership into 1,000 owners and 1,000 non-owners. Within each stratum, respondents were randomly assigned to watch either a popular, roughly two-minute video showcasing an AR-15-pattern rifle being handled and fired at an outdoor range, or a similar video showcasing Apple consumer products. The treatment is organic: it is an existing YouTube video with approximately six million views, not a stimulus constructed for the study, and the gun video contains no political message. Random assignment and outcomes measured minutes after exposure rule out selection and compositional change by design, and the equal strata let us compare the response of people who already own guns with that of people who do not.

The experiment reproduces the key features of the observational results. Pooling all respondents, the gun video raises the Republican feeling thermometer by 4.1 points (standard error 1.5). The response is strongly gendered among non-owners, and the largest effects are for non-owning men. Among men who do not personally own a gun, exposure raises the Republican thermometer by 10.2 points ($p = 0.003$) and net partisanship, the Republican minus the Democratic thermometer, by 12.1 points. Among non-owning women, the Republican thermometer does not move and net partisanship falls by 10.0 points, so that the same video pulls non-owning men and women 22 points apart. Among

owners, the estimated effects on the Republican thermometer are smaller and imprecise, 5.5 points for men ($p = 0.054$) and 4.3 for women ($p = 0.23$), and the gender difference is small and insignificant. Gun-policy views show the same gendered pattern among non-owners, in their general orientation more than in their views on specific policies. The video moves non-owning men toward favoring less strict gun laws and non-owning women toward favoring stricter ones, a gender difference of 0.31 points on a five-point scale ($p = 0.02$), or about 0.3 standard deviations; an index of views on specific policy proposals moves in the same direction but by less and not significantly. Owners' gun-policy views do not move. Exposure to guns thus moves partisan attachments, and general orientation toward gun rights, more than views on particular gun laws.

Taken together, the two designs tell a consistent story. Access to firearms moves partisan allegiance toward the Republican Party—although this is partially offset by a partial backlash effect among women—and the movement is not confined to people who already own guns—indeed, it is driven mostly by non-owners. Among men, the point estimates are larger for those who do not personally own a gun—who in the observational data would be the new customers a store attracts—than for existing owners, although the difference between the two is not statistically significant. This is unsurprising given that existing owners are, in a sense, already treated. That exposure moves partisan affect more than views on specific policies, and moves owners' partisanship without moving their policy views, suggests that the channel runs through identity and group affiliation rather than persuasion about the merits of particular regulations. And both settings point to a gender difference: in the contribution data, men respond months before women, and in the experiment the same video moves non-owning men toward the Republicans and non-owning women away from them.

These findings suggest a feedback loop at the heart of American gun politics. Political factors shape who buys guns, but guns in turn shape individuals' political views and actions. The diffusion of firearms therefore generates its own political support, which may help explain why public opinion and policy on guns have proven so inelastic to events, such as mass shootings, that might be expected to move them. More broadly, the results point to what might be called a political endowment effect: goods that carry social meaning do not merely reflect preferences, they help form them.

1.1 Related Literature

Our paper contributes to five literatures.

Identity goods and the politics of consumption. A growing body of work documents that political and social identities shape consumption. In the economics of identity, people derive utility from acting in accordance with the norms of the group they identify with, and consumption is one way of doing so (Akerlof and Kranton, 2000; Bénabou and Tirole, 2011; Shayo, 2009, 2020); Atkin et al. (2021) show, for example, that food consumption in India responds to the salience of religious identity. Political identity in particular has become predictive of a remarkably wide range of consumer choices, from brands and media to everyday products (Bertrand and Kamenica, 2023; Schoenmueller et al., 2023). Partisanship shapes economic behavior more generally, including where people choose to work and shop (McConnell et al., 2018) to how they respond to elections and public-health guidance (Gerber and Huber, 2009; Mian et al., 2023; Allcott et al., 2020a). The most direct evidence comes from boycotts. The 2003 French opposition to the Iraq War led to lower sales of French wine and even of brands that merely sounded French (Chavis and Leslie, 2009; Pandya and Venkatesan, 2016); calls to boycott Goya Foods in 2020 were met by a larger conservative "buycott" (Liaukonyte et al., 2023); and politically motivated purchasing is common and partisan in the United States (Endres and Panagopoulos, 2017). This literature studies how politics shapes consumption. We study the reverse direction, about which much less is known: whether access and exposure to a politically coded good change political identity. Guns are the quintessential identity good in contemporary American politics, and our setting lets us trace the effect of the good on partisan behavior, holding fixed the political environment that makes the good meaningful.

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Ownership, endogenous preferences, and beliefs. Our results relate to work showing that owning things shapes individuals' preferences and beliefs. The endowment effect implies that people value goods more once they own them (Thaler, 1980; Kahneman et al., 1990), and psychologists have long documented that ownership and choice change evaluations of the objects and attributes associated with them (Brehm, 1956; Festinger, 1957; Beggan, 1992). Possessions become part of the extended self (Belk, 1988), and holding beliefs can itself be valuable (Akerlof and Dickens, 1982; Bénabou and Tirole, 2011). A smaller body of causal evidence shows that ownership shapes political attitudes: squatters randomly granted land titles in Argentina became more individualist and market-oriented (Di Tella et al., 2007); Israeli voters randomly given stock portfolios shifted their votes toward parties favoring peace negotiations (Jha and Shayo, 2019); stock trading and ownership shift economic values and political behavior (Margalit and Shayo, 2021; Kaustia et al., 2016); and homeownership and house prices shape support for social insurance and local policy (Ansell, 2014; Fischel, 2001). More broadly, economic institutions can shape the preferences of those who participate in them (Bowles, 1998). In these studies, ownership creates a material stake that plausibly moves political preferences. We show that exposure to a good—in this case, firearms—moves partisan identity more than it moves views on the specific policies that govern the good, which suggests that identity rather than material interest is the primary channel.

Media and political persuasion. A large literature estimates the political effects of media, from Fox News and cable news (DellaVigna and Kaplan, 2007; Martin and Yurukoglu, 2017) to television, newspapers and social media (Gentzkow, 2006; Gentzkow et al., 2011; Enikolopov et al., 2011; Levy, 2021; Allcott et al., 2020b); see DellaVigna and Gentzkow (2010) and Zhuravskaya et al. (2020) for reviews. Most of this work studies explicitly political content. A smaller set of papers shows that entertainment and other non-political media can shift political attitudes and behavior (Durante et al., 2019) and social outcomes more generally (Jensen and Oster, 2009; La Ferrara et al., 2012; Kearney and Levine, 2015), including in the domain of violence (Dahl and DellaVigna, 2009). Our experimental

treatment is a consumer product video with no political content, of the kind that circulates widely on social media and plays on screens in gun stores. That such content moves partisan affect illustrates how political identity can be transmitted through commercial culture rather than political communication. Methodologically, we combine a large-scale natural experiment with a pre-registered survey experiment in which the treatment is organic content with millions of views, which speaks to concerns about the artificiality and external validity of survey treatments (Coppock, 2022; Haaland et al., 2023).

The economics and politics of firearms. A substantial literature in economics studies the consequences of gun ownership and gun regulation for crime, suicide and accidental death (Lott and Mustard, 1997; Duggan, 2001; Cook and Ludwig, 2006; Luca et al., 2017; Levine and McKnight, 2017; Edwards et al., 2018; Donohue et al., 2019; Studdert et al., 2020; Koenig and Schindler, 2023), as well as the cross-jurisdiction externalities of gun laws (Knight, 2013; Dube et al., 2013) and the functioning of legal and underground gun markets (Cook et al., 2007; Rosenberg, 2026). We borrow from Rosenberg (2026) the insight that dealer entry is a shock to local gun access and use his estimates to scale our results. A parallel literature in political science studies the politics of guns: the construction of gun-owner identity by the National Rifle Association (Lacombe, 2021), gun ownership as a social identity (Lacombe et al., 2019), its growing association with Republican voting (Joslyn et al., 2017), the anti-system beliefs of recent gun buyers (Lacombe et al., 2024), and the cultural and racial correlates of gun attitudes (Kahan and Braman, 2003; Filindra and Kaplan, 2016). Work on the politics of gun policy finds that mass shootings have at most modest effects on public opinion (Barney and Schaffner, 2019; Newman and Hartman, 2019), can mobilize local voters (Hassell et al., 2020), and lead Republican-controlled legislatures to loosen rather than tighten gun laws (Luca et al., 2020). We provide what is, to our knowledge, the first causal evidence that access to firearms itself shifts partisan behavior, which offers a micro-foundation for the resilience of gun-rights politics.

Polarization, affective partisanship and the gender gap. Finally, we contribute to work on the sources of political polarization. Affective polarization, the tendency of partisans to dislike the other party, has risen sharply in the United States (Iyengar et al., 2012, 2019; Boxell et al., 2024), as social identities have aligned with partisanship (Mason, 2018) and as people adjust their identities to fit their politics (Egan, 2020). Economic research has linked polarization to media, trade shocks and changes in elite behavior (Martin and Yurukoglu, 2017; Autor et al., 2020; Gentzkow et al., 2019; Canen et al., 2020). We identify a consumption-based source of partisan sorting and show that it operates more

through affect than through positions on specific policies. Our finding that men respond more quickly than women, and that non-owning women may even move in the opposite direction, relates to work on the partisan gender gap, which has widened largely because men have moved toward the Republican Party (Kaufmann and Petrocik, 1999; Edlund and Pande, 2002; Box-Steffensmeier et al., 2004).

The rest of the paper proceeds as follows. Section 2 describes the partisan realignment of the gun issue and the regulatory role of FFLs. Section 3 describes the data and the empirical strategy for the observational analysis, and Section 4 presents its results. Section 5 presents the design and results of the experiment. Section 6 discusses mechanisms and implications and concludes.

2 Background

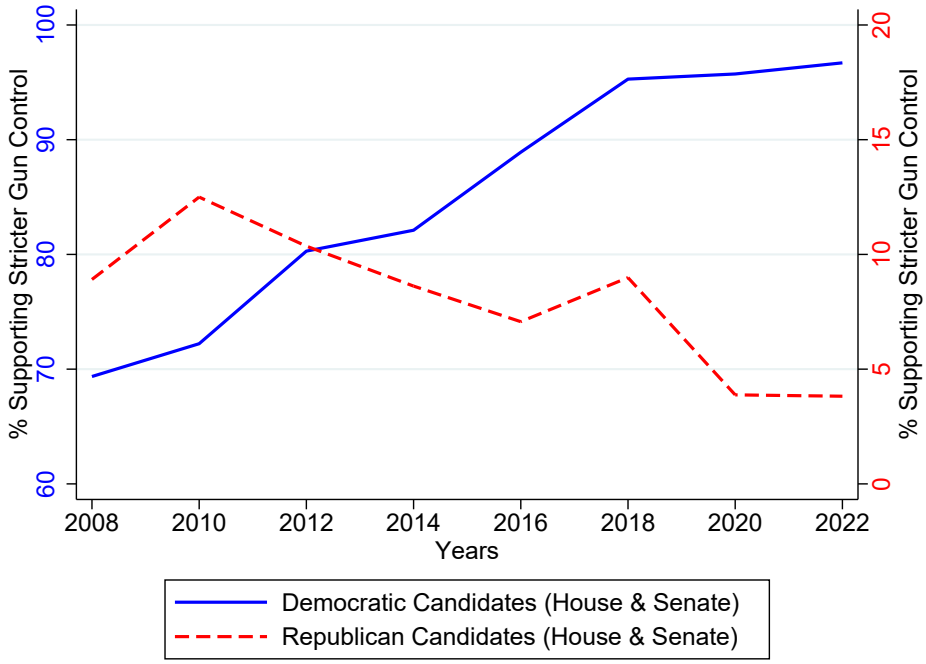
2.1 The Partisan Realignment of Guns

Gun ownership has not always been a partisan marker. The National Rifle Association spent decades cultivating a politically engaged gun-owner identity, but the alignment of that identity with the Republican Party and its fusion with a broader conservative worldview developed largely from the late 1970s onward (Lacombe, 2021). Into the 1990s, prominent members of both parties still supported some forms of gun regulation. Gun owners now share a politically meaningful social identity (Lacombe et al., 2019), and gun ownership has become an increasingly strong predictor of Republican voting in presidential elections (Joslyn et al., 2017).

Figure 1 documents the consequence at the level of political elites. Using candidate issue positions compiled by Vote Smart (2025) and assembled as in Longuet-Marx (2026), it plots the share of House and Senate candidates who support stricter gun control, separately by party. In 2008, about 70 percent of Democratic candidates supported stricter gun control; by 2022, 97 percent did. Among Republicans, support was low throughout and fell further, from 9 to 12 percent in 2008 to 2010 to about 4 percent in 2020 and 2022. The two parties now offer voters an almost perfectly sorted choice on guns, so that a shift in a person's attachment to the gun issue maps directly into a shift in partisan alignment.

Mass opinion has followed. In 2023, 45 percent of Republicans and Republican-leaning independents reported personally owning a gun, against 20 percent of Democrats and Democratic leaners (Pew Research Center, 2023). Ownership is also sharply gendered: 40 percent of men report owning a gun compared to only 25 percent of women. The pool of owners has, however, been changing. An estimated 7.5 million U.S. adults became new

Figure 1. Partisan Divergence on Gun Control among Congressional Candidates



Notes: The figure plots the share of House and Senate candidates who support stricter gun control, by party and election year. Democratic candidates are plotted on the left axis (solid line) and Republican candidates on the right axis (dashed line); the two axes have different scales. Data are candidate issue positions from Vote Smart, as used in Longuet-Marx (2026).

gun owners between January 2019 and April 2021; about half were women, and most had previously lived in homes without guns (Miller et al., 2022). Whether these new owners adopt the political identity historically associated with gun ownership is an important (and open) question, which we speak to by examining whether gun access and marketing shapes individuals' political preferences and partisanship.

2.2 Federal Firearms Licensees and the Regulation of Gun Sales

The commercial market for firearms in the United States is organized around federal licensing. The Gun Control Act of 1968 requires anyone engaged in the business of dealing in, manufacturing or importing firearms to hold a Federal Firearms License issued by the Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF). Licenses come in several types, including dealers, pawnbrokers, collectors of curios and relics, manufacturers and importers. At the end of 2023 there were about 132,000 active licensees, down gradually from a peak of about 139,000 in 2013 (Bureau of Alcohol, Tobacco, Firearms and Explosives, 2024).

Licensed dealers are the legal gateway to the firearms market. Since the implementation of the Brady Handgun Violence Prevention Act, every sale by a licensed dealer requires a background check through the National Instant Criminal Background Check System (Ludwig and Cook, 2000). Handguns may be transferred to a resident of another state only through a licensed dealer in the buyer's state, and firearms bought online or from out-of-state sellers must be shipped to a local dealer, who completes the transfer and the background check. Federal law exempts sales between unlicensed private individuals residing in the same state from the background-check requirement. Twenty-one states and the District of Columbia require background checks for at least some private sales, which routes most legal transactions there through a licensed dealer; in the remaining 29 states, private sales offer an alternative channel of acquisition. For people without firearms, and in particular for first-time buyers without networks of gun-owning acquaintances, a local retail dealer is the most direct point of access.

A retail gun store is also more than a point of sale. It is a place where customers handle firearms, encounter gun-rights messaging and gun media, sign up for training and concealed-carry classes, and meet other gun owners. Qualitative work describes such settings as sites where the political meanings of gun ownership are learned and reinforced (Carlson, 2023, 2015; Stroud, 2012). The arrival of a store therefore changes both the cost of acquiring a gun and exposure to the culture that surrounds it. Rosenberg (2026) provides direct evidence on the first of these: in California, where virtually all legal transfers go

through dealers, the entry of a first firearm retailer into a ZIP code raises local handgun purchases by about 30 percent one year after entry, with a confidence interval of roughly 15 to 45 percent.

2.3 From Access to Allegiance

There are several ways a new dealer could change local political behavior. Access could *intensify* the partisanship of existing gun owners, by making the gun issue more salient to them, by increasing their engagement with gun culture, or by adding to their stock of firearms. Access could also *mobilize* people who did not previously own guns, by lowering the cost of a first purchase and exposing them to firearms and the identity attached to them. We refer to this second channel as the access channel. Finally, a new store might not change anyone's behavior at all but instead coincide with, or respond to, a change in the local population, such as the in-migration of gun-owning Republicans, or a local shock that also moves politics. The observational design in Sections 3 and 4 estimates the combined effect of the first two channels and uses the timing of the response to argue against the third. The experiment in Section 5 isolates the effect of exposure by randomization and separates the intensification and access channels by comparing owners with non-owners.

3 Data and Empirical Strategy

3.1 Firearms Dealers

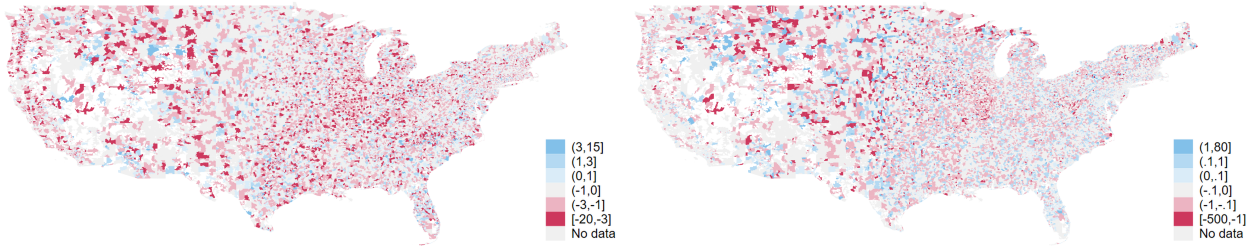
Our data on firearms dealers come from the ATF's monthly Federal Firearms Listings (Bureau of Alcohol, Tobacco, Firearms and Explosives, 2025), which report every active licensee, its license type and its premises address. We assign licensees to ZCTAs by premises ZIP code and construct a balanced panel of 32,989 ZCTAs observed monthly from January 2018 to March 2025, a total of 87 months and 2,870,043 ZCTA-months. For each ZCTA-month we count licensees of each type.

Our treatment is defined from the count of retail licensees: dealers in firearms and pawnbrokers in firearms (ATF license types 01 and 02), which sell firearms to walk-in customers.[2] Figure 2 maps the change in licensee counts between the start and end of the panel, in absolute terms (panel a) and per 1,000 residents (panel b), and Appendix

[2]Broader counts that add manufacturer-type licenses contain the retail count in every ZCTA-month. The additional licenses account for about 20,000 monthly increases nationally over the panel, almost all of them manufacturer-type licenses. Section 4.3 shows that our results are unchanged when treatment is defined from these broader counts.

Figure A1 maps their distribution at the start of the panel. Licensees are present throughout the country and are densest per capita in rural areas of the Mountain West and Great Plains. The change maps show substantial changes in both directions. Declines are common, consistent with the gradual national decline in the number of licensees, while gains are scattered across rural and ex-urban areas in every region.

Figure 2. Change in the Number of Federal Firearms Licensees, January 2018 to March 2025



(a) Change in count

(b) Change per 1,000 residents

Notes: Each map shows ZCTAs in the contiguous United States. Panel (a) shows the change in the number of Federal Firearms Licensees between January 2018 and March 2025; panel (b) shows the same change per 1,000 residents, using 2020 Census population. Blue shades indicate increases and red shades decreases; the lightest bins include no change, and gray also marks ZCTAs with no data. Source: ATF Federal Firearms Listings.

3.2 Political Contributions

We measure political behavior with individual campaign contributions, built from the Federal Election Commission's records of itemized contributions by individuals (Federal Election Commission, 2025). Contribution records are a revealed-preference measure of political engagement and partisan allegiance (Bonica, 2014; Hill and Huber, 2017), observed at a fine geographic and temporal resolution, and have been used to measure the local political effects of economic shocks (Autor et al., 2020). Each record reports the contributor's name and ZIP code, the date and amount of the contribution, and the recipient committee. We assign contributions to ZCTAs by the donor's ZIP code and to parties by the party of the recipient candidate or committee, and aggregate them to ZCTA-months. We infer donor gender from first names with the `genderit` Stata package, which applies the World Gender-Name Dictionary (Lax Martinez et al., 2021).[3]

For each gender we construct four outcomes. *Log Republican dollars* is $\ln(1 + R^{\$}_{it})$, where $R^{\$}_{it}$ is the sum of contributions by residents of ZCTA i in month t to Republican candidates and committees. *Log Republican donors* is $\ln(1 + R^{n}_{it})$, where R^{n}_{it} is the number of distinct

[3]Contributions by donors whose gender cannot be classified from their first name are included in all-donor totals but not in male-donor totals. We construct female-donor outcomes as the difference between all-donor and male-donor quantities, so "female" throughout means female or unclassified.

residents who gave to Republican recipients. The *Republican share of two-party dollars* is $R^{\$}_{it}/(R^{\$}_{it} + D^{\$}_{it})$, where $D^{\$}_{it}$ is Democratic dollars, and the *Republican share of two-party donors* is defined analogously from donor counts. We set the shares to zero when the two-party total is zero, which keeps every ZCTA-month in the sample and folds the extensive margin of whether anyone gives into the outcome. Since about three quarters of ZCTA-months have no two-party giving by men, and single-month giving in small ZCTAs is usually all of one party, a share effect of 0.03 is best read as a 3-percentage-point increase in the probability that a ZCTA-month's two-party giving is Republican.

We focus on men as the primary population. Gun ownership is concentrated among men (Pew Research Center, 2023), the retail channel we study runs through customers who are disproportionately male, but we report parallel results for women.

3.3 Sample: First Entry

We define treatment as the arrival of the first retail dealer in a ZCTA that had none. Accordingly, our sample consists of the 14,636 ZCTAs with no retail licensee in January 2018, which we call the zero-baseline sample. Within it, an *entrant* is a ZCTA whose retail count becomes positive during the panel, and its *entry month* is the first month with a positive count. Treatment is absorbing: an entrant is treated from its entry month onward, whatever happens to its count afterward. *Never-entrants*, whose count remains zero in all 87 months, form the comparison group. There are 2,381 entrants spread across 71 monthly entry cohorts between February 2018 and March 2025, and 12,255 never-entrants. Appendix Table A1 summarizes the sample.

The first-entry design addresses problems associated with using always treated or earlier treated as control groups in event-study designs at the source. By restricting our attention to the sample of ZCTAs without FFL access, every unit starts genuinely untreated, so no earlier entry is still playing out when the event we study occurs. Treated and comparison units share the same baseline, which makes parallel trends plausible. The estimand is also more interpretable: the effect of the arrival of access where there was none, rather than an average effect of an incremental increase in the number of dealer across a varying baseline ranging from zero to forty.

We define entry from retail licensees because the mechanism we have in mind runs through customers. A retail dealer creates a place where residents buy firearms, encounter gun culture and meet other gun owners. A manufacturer, collector, or importer license, often held by a small workshop or home-based operation, does none of this and would not be noticed by residents. Counting such licenses would add events that cannot plausibly

affect political behavior and would attenuate the estimates.

Entrant ZCTAs differ from never-entrants in levels. Across the panel, 30 percent of entrant ZCTA-months have at least one male Republican donor, compared with 16 percent of never-entrant ZCTA-months (Appendix Table A1), a gap far larger than any effect of entry we estimate. Our design allows for such level differences and requires only that, absent entry, giving in entrant and never-entrant ZCTAs would have evolved in parallel around the entry date. One third of entrants (784) see their retail count return to zero at some point after entry; we keep them in the treated group, and dropping them changes the estimates only by hundredths.

3.4 Estimation

Two-way fixed-effects event study. For ZCTA i in month t, with entry month E_i, we estimate

$$y_{it} = \alpha_i + \gamma_t + \sum_{\substack{k=-12 \\ k\neq -1}}^{12} \beta_k \, \mathbf{1}\left[t - E_i = k\right] + \varepsilon_{it}, \tag{1}$$

where α_i and γ_t are ZCTA and calendar-month fixed effects. The month before entry is the omitted category, so each β_k measures the change in the outcome relative to that month. The endpoint indicators are binned: $k = -12$ includes all months twelve or more before entry and $k = 12$ all months twelve or more after. Never-entrants have all indicators equal to zero. To summarize the effect with a single number, we also estimate a pooled specification with one post-entry indicator and a windowed specification with separate indicators for months 0 to 12 and for months 13 and later, so that later months do not enter the first-year estimate. Standard errors are clustered by ZCTA throughout.[4]

Heterogeneity-robust estimator. With staggered entry, two-way fixed-effects estimators implicitly compare later entrants with earlier ones, and under heterogeneous effects some of these comparisons receive negative weights (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Roth et al., 2023). We therefore also use the estimator of Callaway and Sant'Anna (2021), which estimates the average effect for each entry cohort g in each month t,

$$ATT(g,t) = \mathbb{E}\left[y_{it} - y_{i,g-1} \mid E_i = g\right] - \mathbb{E}\left[y_{it} - y_{i,g-1} \mid i \text{ never enters}\right], \tag{2}$$

[4] We estimate equation (1) with `reghdfe` (Correia, 2016).

using only never-entrants as controls and the month before entry as the base period for all pre- and post-entry months. This puts the pre-entry estimates on the same footing as the leads in equation (1). We aggregate the $ATT(g,t)$ into event-time effects, weighting cohorts by their share of entrants observed at each event time, and into a simple ATT that averages all post-entry cohort-by-month effects, including those beyond twelve months. We use no covariates, so the regression, inverse-probability-weighting and doubly robust versions coincide, and we compute analytical standard errors clustered by ZCTA.[5]

Identification. The key assumption is that, absent entry, outcomes in entrant ZCTAs would have evolved in parallel with those in never-entrant ZCTAs from the month before entry onward. Three threats deserve attention. First, dealers may open where demand for guns, and perhaps Republican politics, is already rising. We examine pre-entry dynamics directly, and we use Democratic giving as a placebo outcome: a general rise in political engagement would raise giving to both parties. Second, entry may coincide with changes in the composition of the population. Here the timing of the response is informative, because residential sorting operates slowly, while a behavioral response to a new store can be immediate. Third, entry could coincide with other local shocks. The month fixed effects absorb national shocks, including the election cycle, and the staggered timing of entry across 71 cohorts makes it unlikely that a common shock drives the results. The experiment in Section 5 complements this analysis with a setting in which exposure is randomly assigned and composition is held fixed by design.

4 The Effect of Dealer Entry on Political Giving

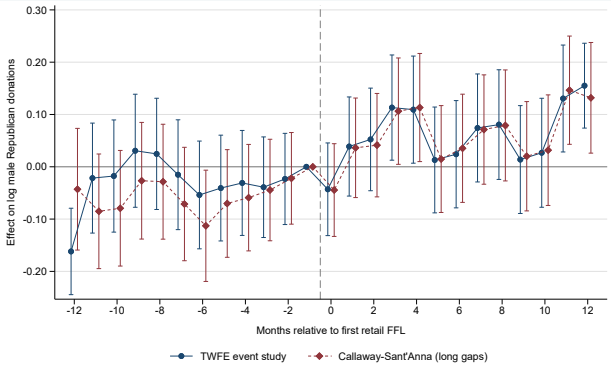
4.1 Men

Figure 3 presents the event-study estimates for the four male outcomes, overlaying the two-way fixed-effects estimates (circles) and the Callaway and Sant'Anna (2021) estimates (diamonds) with 95 percent confidence intervals. Appendix Table A5 reports every coefficient. The two estimators agree closely at every post-entry event time, which is what one expects when never-entrants are the comparison group and cohorts are many and small.

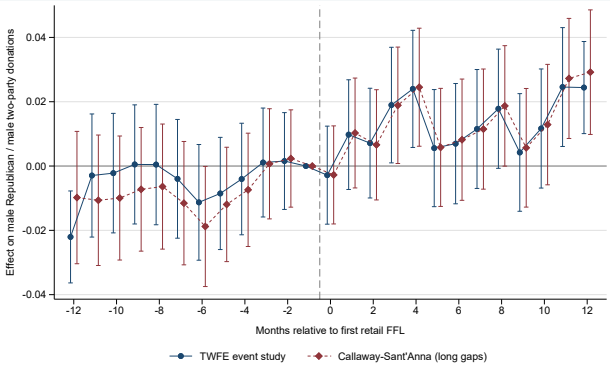
Dynamics. The four outcomes tell one story. No change occurs in the month of entry: the Callaway and Sant'Anna (2021) estimates at $k = 0$ are -0.045 (standard error 0.045) for

[5] We use `csdid2` (Rios-Avila et al., 2023). The interaction-weighted estimator of Sun and Abraham (2021) coincides with the Callaway and Sant'Anna (2021) estimator when never-treated units are the controls and there are no covariates.

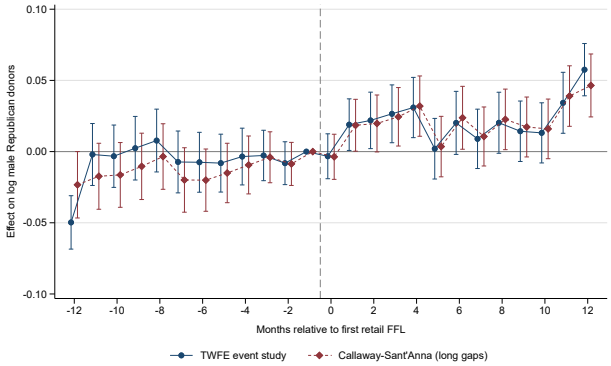
Figure 3. Event-Study Estimates of the Effect of a First Retail Dealer: Male Donors



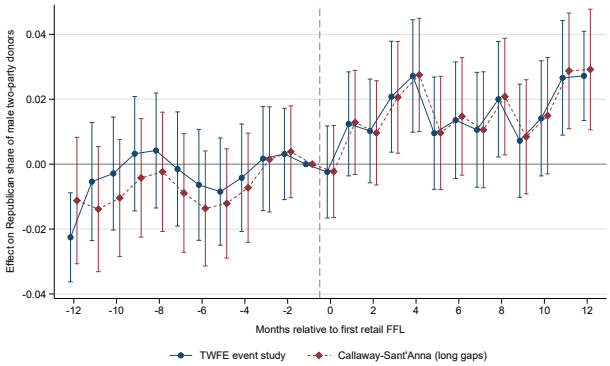
(a) Log Republican dollars



(b) Republican share of two-party dollars



(c) Log Republican donors



(d) Republican share of two-party donors

Notes: Each panel plots event-study estimates of the effect of the arrival of a first retail firearms dealer on giving by male donors resident in the ZCTA, relative to the month before entry. Navy circles are two-way fixed-effects estimates from equation (1), with ZCTA and month fixed effects; the endpoints are binned indicators for twelve or more months before or after entry. Maroon diamonds are Callaway and Sant'Anna (2021) estimates with never-entrant controls and the month before entry as the base period. Whiskers show 95 percent confidence intervals based on standard errors clustered by ZCTA. The sample is the 14,636 ZCTAs with no retail dealer in January 2018, of which 2,381 gain one during the panel. Appendix Table A5 reports the coefficients.

log dollars, -0.003 for the dollar share, -0.004 for log donors and -0.002 for the donor share. After that point, gving rises, with log male Republican donors 0.018 higher in the first month after entry and 0.024 to 0.032 higher in months 3 and 4, each significant at the 5 percent level. By months 3 and 4, log dollars are 0.11 higher, and the Republican shares of two-party dollars and donors are 1.9 to 2.8 percentage points higher. The effects dip in month 5, recover over the following months, and reach their largest values at the end of the first year. At month 12, log dollars are 0.132 higher (standard error 0.054), log donors 0.046 higher (0.011), and the Republican shares of two-party dollars and donors both 0.029 higher (0.010 and 0.009).

Magnitudes. Table 1 summarizes the effects. Panel A reports the effect over the first year. The Callaway and Sant'Anna (2021) average of the event-time effects over months 0 to 12, which is measured relative to the month before entry and includes the flat entry month, is 0.021 for log donors ($p = 0.007$), 0.014 for the dollar share and 0.016 for the donor share (both $p < 0.05$), and 0.060 for log dollars ($p = 0.12$). The windowed two-way fixed-effects estimates are larger, 0.052 for log donors and 0.03 for the two shares, because their comparison period is the whole pre-entry period, which includes the lower level of giving more than a year before entry discussed below. Panel B reports effects averaged over all post-entry months. The simple ATT, which includes months beyond the first year, is 0.184 for log dollars, 0.062 for log donors, 0.031 for the dollar share and 0.034 for the donor share, each significant at the 1 percent level; the pooled two-way fixed-effects estimates are similar. Because the level outcomes are $\ln(1 + y)$, these estimates translate into proportional changes in one plus the level: a year after entry, one plus male Republican contributions is about 14 percent higher (20 percent over all post-entry months), one plus the number of male Republican donors is about 5 to 6 percent higher, and the probability that a ZCTA-month's two-party male giving goes to Republicans is about 3 percentage points higher. Relative to the sample mean of the donor share, 0.13, the long-run effect is an increase of about a quarter.

Extensive versus intensive margins. The dollar-weighted and donor-weighted Republican shares move by almost exactly the same amount at every horizon (Figure 3, panels b and d). If existing Republican donors were giving more, the dollar share would rise while the donor share stayed put. The two shares move together because the shift is in who gives: entry brings new male Republican donors and tilts the two-party balance of men's giving toward Republicans. The parallel rise in dollars and donors points the same way. This extensive-margin pattern is what the access channel predicts, since it implies that

Table 1. Effect of a First Retail Firearms Dealer on Republican Giving: Men

	Log dollars		Dollar share		Log donors		Donor share	
	TWFE (1)	CS (2)	TWFE (3)	CS (4)	TWFE (5)	CS (6)	TWFE (7)	CS (8)
Panel A. First year after entry								
Months 0–12 after entry	0.168***	0.060	0.028***	0.014**	0.052***	0.021***	0.030***	0.016**
	(0.022)	(0.039)	(0.004)	(0.007)	(0.006)	(0.008)	(0.004)	(0.007)
Months 13+ after entry	0.259***		0.038***		0.089***		0.041***	
	(0.023)		(0.004)		(0.007)		(0.004)	
Panel B. All months after entry								
All months after entry	0.226***	0.184***	0.034***	0.031***	0.075***	0.062***	0.037***	0.034***
	(0.020)	(0.044)	(0.004)	(0.007)	(0.006)	(0.009)	(0.004)	(0.007)
Mean of dep. var.	1.030		0.129		0.215		0.130	
SD of dep. var.	2.289		0.314		0.521		0.307	
ZCTA and month FE	✓		✓		✓		✓	
Never-entrant controls		✓		✓		✓		✓
Base period $t = -1$		✓		✓		✓		✓
ZCTA-months	1,272,368		1,273,332		1,273,332		1,273,332	
ZCTAs	14,636		14,636		14,636		14,636	
Entrant ZCTAs	2,381		2,381		2,381		2,381	

Notes: Each column reports a separate estimate for the outcome in the column heading, measured for male donors resident in the ZCTA. Log dollars is $\ln(1 + \text{Republican dollars})$; dollar share is Republican dollars divided by Republican plus Democratic dollars; log donors is $\ln(1 + \text{Republican donors})$; donor share is Republican donors divided by Republican plus Democratic donors. Shares are set to zero when the two-party total is zero. The sample is the 14,636 ZCTAs with no retail firearms dealer in January 2018, observed monthly from January 2018 to March 2025; 2,381 gain a first dealer during the panel and are treated from the month of entry onward, and the 12,255 never-entrants form the comparison group. TWFE columns report ordinary least squares estimates with ZCTA and calendar-month fixed effects. In Panel A they are the coefficients on two indicators entered jointly, one for months 0 to 12 after entry and one for months 13 and later, so that the first-year coefficient excludes later months; the comparison is the entire pre-entry period. In Panel B the TWFE column is the coefficient on a single post-entry indicator. CS columns report the Callaway and Sant'Anna (2021) estimator with never-entrants as the comparison group and each cohort's month before entry as the base period. In Panel A the CS column is the average of the event-time effects for months 0 to 12; in Panel B it is the simple average of all post-entry cohort-by-month effects. Standard errors clustered by ZCTA in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

entry draws new people into Republican politics rather than deepening the commitment of those already engaged.

4.2 Credibility of the Design

Pre-entry dynamics. Within the twelve months before entry, the estimates are small and statistically indistinguishable from zero. Appendix Table A2 reports the average of the Callaway and Sant'Anna (2021) pre-entry estimates for months -12 to -2: -0.058 (standard error 0.040) for log dollars, -0.008 (0.007) for the dollar share, -0.013 (0.008) for log donors and -0.007 (0.007) for the donor share, none significant at the 10 percent level. The donor-share outcome has the cleanest pre-period, with no individual Callaway and Sant'Anna (2021) coefficient in the window more than 1.6 standard errors from zero. For log donors, the coefficients for months -8 to -4 are all slightly negative, so a gentle upward drift of about one percent into entry cannot be excluded; but the estimates for months 3 and 4 and months 11 and 12 exceed every pre-entry estimate in absolute value, the month-12 estimate is twice the largest lead, and the break at entry is sharp.[6]

Effect Timing. The speed of the response is, in our view, the strongest evidence that it reflects a genuine effect of entry rather than a change in the local population. The number of male Republican donors rises within one month, and all four outcomes are significantly higher within three to four months. A compositional explanation would require that Republican-leaning donors move into entrant ZCTAs in the months immediately after a dealer opens, in numbers large enough to shift the local balance of giving. Partisan residential sorting, however, is slow and limited in magnitude: even partisans who prefer politically congenial neighborhoods rarely move for that reason (Mummolo and Nall, 2017), and sorting explains little of the geographic pattern of polarization (Martin and Webster, 2020; Brown and Enos, 2021). The gender pattern discussed below reinforces the point, since households that move bring men and women with them.

[6]The binned two-way fixed-effects coefficient for twelve or more months before entry is negative and significant for every outcome (Appendix Table A2, column 2). Giving in entrant ZCTAs stepped up at some point more than a year before the first dealer arrived and then held roughly flat until entry. The same step-up appears in Democratic giving in the same ZCTAs and under every definition of a license. It therefore reflects a general rise in political engagement that precedes dealer entry rather than a Republican-specific anticipation of entry. Because the event-study and Callaway and Sant'Anna (2021) estimates are measured relative to the month before entry, the step-up does not enter them directly; it would bias them only if the trend it reflects continued into the window, which the flat within-window leads argue against (Rambachan and Roth, 2023). The windowed and pooled two-way fixed-effects estimates in Tables 1 and 2, by contrast, compare post-entry months with the entire pre-entry period, and the lower level of giving more than a year before entry inflates them; for this reason we treat the Callaway and Sant'Anna (2021) estimates as our preferred measures.

4.3 Robustness & Additional Results

The results are robust to a range of alternative choices. Defining entry from the broader license counts that add manufacturer-type licenses adds about 350 entrants and, for the three outcomes we examine in this check, changes the estimates by a fraction of a standard error at every event time. Dropping the 784 entrants whose dealer later exits changes the estimates by hundredths. The imputation estimator of Borusyak et al. (2024) produces the same post-entry profile. Outcomes defined for all donors, rather than male donors, show the same dynamics, with male effects about three quarters of the all-donor effects for dollars and donors.

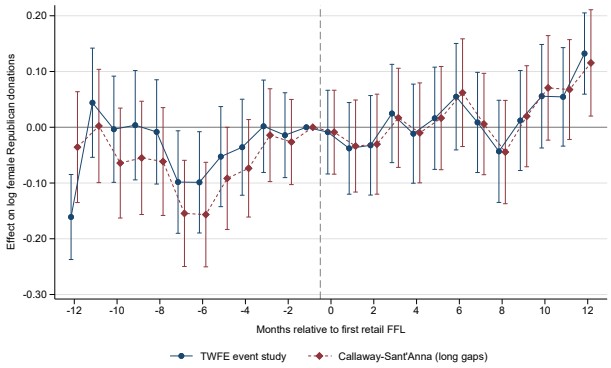
Appendix Table A4 splits the sample by whether the state requires background checks for private sales. One might expect effects to be larger where private sales are regulated, since a dealer is then necessary for nearly every legal acquisition. The Callaway and Sant'Anna (2021) simple ATTs are instead practically identical in the two groups: 0.18 and 0.21 for log dollars, 0.031 and 0.031 for the dollar share, 0.063 and 0.067 for log donors, and 0.036 and 0.033 for the donor share, with differences far from significant. The pooled two-way fixed-effects estimates differ somewhat more across groups, and in a specification that interacts the post-entry indicator with the state group, the difference is significant at the 5 percent level only for log donors. The effect of a store thus does not depend in any systematic way on the legal ease of acquiring guns elsewhere. This is consistent with the view that a store matters as a point of contact with guns and gun culture, especially for people with no other access, and not only as a legal requirement for purchase.

4.4 Women and the Gender Gap in Timing

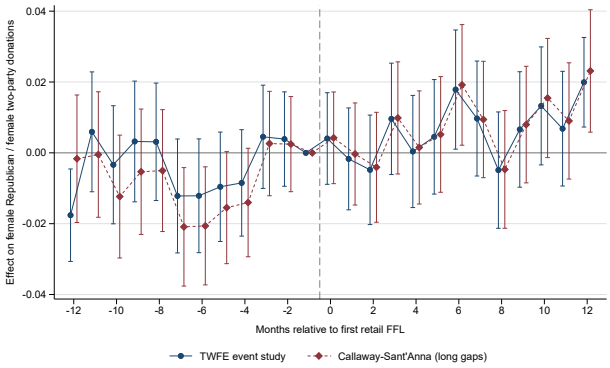
Figure 4 and Table 2 present the same analysis for female (or unclassified) donors. Female Republican giving is rarer than male: 21 percent of entrant ZCTA-months and 11 percent of never-entrant ZCTA-months have a female Republican donor, compared with 30 and 16 percent for men. The pre-entry estimates are noisier than for men, with dips in months -7 and -6 for all four outcomes; the average of the pre-entry estimates is marginally significant for dollars (-0.066, standard error 0.036) and insignificant for the other three outcomes (Appendix Table A2).

The female response is weaker in the first year and arrives later. The Callaway and Sant'Anna (2021) estimates for months 0 to 5 are all within 0.035 of zero for dollars and within 0.01 for the other outcomes, with mixed signs. The response appears from month 6 and mostly in months 10 to 12. Averaged over months 0 to 12, the female effects are a quarter to a half of the male effects and none is significant, whereas three of the four male

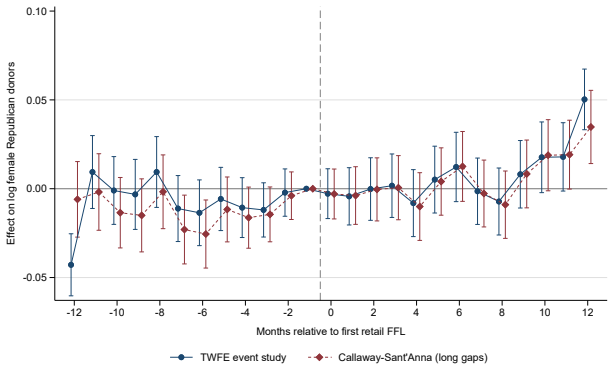
Figure 4. Event-Study Estimates of the Effect of a First Retail Dealer: Female Donors



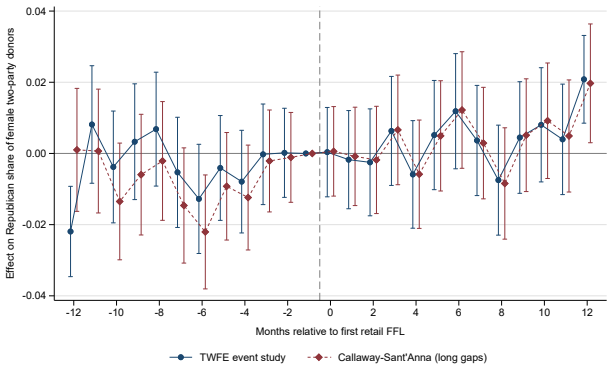
(a) Log Republican dollars



(b) Republican share of two-party dollars



(c) Log Republican donors



(d) Republican share of two-party donors

Notes: Each panel plots event-study estimates of the effect of the arrival of a first retail firearms dealer on giving by female (or unclassified) donors resident in the ZCTA, relative to the month before entry. Female outcomes are computed as all-donor minus male-donor quantities. Estimators, sample and confidence intervals as in Figure 3. Appendix Table A6 reports the coefficients.

Table 2. Effect of a First Retail Firearms Dealer on Republican Giving: Women

	Log dollars		Dollar share		Log donors		Donor share	
	TWFE (1)	CS (2)	TWFE (3)	CS (4)	TWFE (5)	CS (6)	TWFE (7)	CS (8)
Panel A. First year after entry								
Months 0–12 after entry	0.127***	0.019	0.018***	0.007	0.034***	0.005	0.018***	0.004
	(0.021)	(0.034)	(0.004)	(0.006)	(0.006)	(0.007)	(0.004)	(0.006)
Months 13+ after entry	0.238***		0.031***		0.078***		0.034***	
	(0.022)		(0.004)		(0.007)		(0.004)	
Panel B. All months after entry								
All months after entry	0.197***	0.139***	0.026***	0.023***	0.062***	0.049***	0.028***	0.023***
	(0.019)	(0.039)	(0.003)	(0.007)	(0.006)	(0.008)	(0.003)	(0.006)
Mean of dep. var.	0.723		0.091		0.131		0.087	
SD of dep. var.	1.896		0.268		0.388		0.258	
ZCTA and month FE	✓		✓		✓		✓	
Never-entrant controls		✓		✓		✓		✓
Base period $t = -1$		✓		✓		✓		✓
ZCTA-months	1,272,582		1,273,332		1,273,332		1,273,332	
ZCTAs	14,636		14,636		14,636		14,636	
Entrant ZCTAs	2,381		2,381		2,381		2,381	

Notes: Each column reports a separate estimate for the outcome in the column heading, measured for female (or unclassified) donors resident in the ZCTA. Log dollars is $\ln(1 + \text{Republican dollars})$; dollar share is Republican dollars divided by Republican plus Democratic dollars; log donors is $\ln(1 + \text{Republican donors})$; donor share is Republican donors divided by Republican plus Democratic donors. Shares are set to zero when the two-party total is zero. The sample is the 14,636 ZCTAs with no retail firearms dealer in January 2018, observed monthly from January 2018 to March 2025; 2,381 gain a first dealer during the panel and are treated from the month of entry onward, and the 12,255 never-entrants form the comparison group. TWFE columns report ordinary least squares estimates with ZCTA and calendar-month fixed effects. In Panel A they are the coefficients on two indicators entered jointly, one for months 0 to 12 after entry and one for months 13 and later, so that the first-year coefficient excludes later months; the comparison is the entire pre-entry period. In Panel B the TWFE column is the coefficient on a single post-entry indicator. CS columns report the Callaway and Sant'Anna (2021) estimator with never-entrants as the comparison group and each cohort's month before entry as the base period. In Panel A the CS column is the average of the event-time effects for months 0 to 12; in Panel B it is the simple average of all post-entry cohort-by-month effects. Standard errors clustered by ZCTA in parentheses. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

averages are (Table 2, Panel A; Appendix Table A3). By month 12, however, female effects are two thirds to nine tenths of the male effects and significant, and beyond the first year the two are close: the ratio of the female to the male windowed estimate for months 13 and later ranges from 0.81 to 0.92. A stacked regression that tests the male–female difference in the first-year windowed estimate finds a significant gap for log donors ($t = 2.4$) and the donor share ($t = 2.2$), and a marginal one for the dollar share ($t = 1.8$).

The gender difference is thus mainly one of timing rather than eventual magnitude. Men respond within a few months of a dealer opening, and women only toward the end of the first year. We read this as further evidence against a compositional explanation, since in-migrating households would raise male and female giving at the same time. The pattern is also informative about mechanism. Men, who are more likely to become customers, respond first; the delayed female response may reflect a slower adoption of gun ownership among women, spillovers from male household members, or the lower power that comes with female Republican giving being less common. We cannot distinguish these explanations with the contribution data, but the existence of a genuine gender gap in the immediate response motivates the gender-specific analysis of the experiment below.

4.5 Scaling by the First Stage

Our estimates are reduced-form effects of dealer entry on political giving. To interpret their magnitude, we scale them by the effect of entry on gun purchases estimated by Rosenberg (2026): a first firearm retailer raises handgun purchases in a California ZIP code by about 30 percent one year after entry. Taking this as the first stage, the ratio of our month-12 estimates to his implies that a 10 percent increase in local gun purchases is associated with an increase of about 1.5 percent in the number of male Republican donors ($0.046/0.30 \times 0.10$) and of about 1 percentage point in the Republican share of men's two-party giving ($0.029/0.30 \times 0.10$). Using the simple ATTs instead gives 2.1 percent and 1.0 to 1.1 percentage points. Put differently, the implied elasticity of male Republican donors with respect to local gun purchases is about 0.15 to 0.2, or 0.18 to 0.24 if the 30 percent first stage is expressed in log points ($\ln 1.3 = 0.26$).

Three caveats apply. First, Rosenberg's estimate is for handguns in California, where virtually all legal transfers go through dealers; the first stage may be smaller in states where private sales are an alternative, in which case the per-purchase effects would be larger. Second, his confidence interval spans roughly 15 to 45 percent, so the implied scaling is correspondingly uncertain. Third, and most important, the ratio is a per-purchase effect only if a store affects politics solely through the guns it sells. A store also exposes residents

to firearms and gun culture, and the experiment below shows that exposure alone moves partisan affect. The scaled estimates are therefore best read as the political response per unit of increased activity in the local gun market, not as the effect of a single additional gun.

5 Experimental Evidence

The observational results leave two questions that an experiment can answer. The first concerns identification. Dealer entry is not randomly assigned, and the step-up in political engagement more than a year before entry shows that entrant ZCTAs were changing before the dealer arrived. The timing and specificity of the response argue against selection and compositional change, but they cannot rule them out entirely. In a randomized experiment, exposure is independent of respondents' characteristics and outcomes are measured minutes after exposure, before any change in the composition of the sample could occur. If exposure to guns moves partisanship in the experiment, the mechanism underlying the observational estimates is at least causally possible, and the observational estimates are harder to attribute to confounding.

The second question concerns mechanism. The contribution data cannot tell whether the response comes from existing gun owners whose partisanship intensifies or from people without guns who are drawn in by access. Our experiment is designed to separate the two. We stratify the sample by gun ownership, with equal numbers of owners and non-owners, and compare the response to the same treatment across the two groups. The gender gap in the timing of the observational response, and the gender gap in ownership itself, call for making this comparison within gender. Because the observational effects appear first among men, the most informative comparison is between men who own guns and men who do not: if the observational effect reflects intensification, owning men should respond more strongly; if it reflects access, the response should extend to non-owning men.

5.1 Design

Sample. The survey was fielded by Verasight to its national online panel of U.S. adults from September 1 to 8, 2026. Verasight delivered 2,000 completed interviews, quota-stratified on personal firearm ownership: 1,000 respondents who reported personally owning a handgun, rifle, shotgun or other firearm, alone or jointly with someone else in the household, and 1,000 who did not. Non-owners include respondents who live in a

household in which only another member owns a firearm. Verasight removed speeders, straight-liners, respondents who failed attention checks, and duplicate and non-human responses before delivery; we make no further exclusions. The gender composition of the two strata reflects the gender gap in ownership: 614 of the 1,000 owners and 357 of the 1,000 non-owners are men.

Treatment. Within each stratum, respondents were individually randomized by the survey software, after all pre-treatment questions had been answered, into one of three arms with fixed probabilities of 40, 30 and 30 percent. The *control* arm watched a popular, approximately two-minute social-media video showcasing Apple consumer products. The *gun video* arm watched a popular video of similar length in which an AR-15-pattern rifle is presented, handled and fired at an outdoor shooting range. The video depicts lawful recreational use, and contains neither violence against people nor political messaging. The *gun video plus partisan cue* arm watched the same gun video and then read two short paragraphs of roughly equal length summarizing the Democratic and Republican parties' positions on gun policy, worded neutrally and presented without evaluation. Videos were embedded with skipping disabled and had to play to completion; the video page held respondents for a median of 131 to 135 seconds in every arm. After the video, respondents in all arms answered factual comprehension questions about its content, with a small bonus for correct answers, which mirrors the attentive engagement of real product research and serves as a manipulation check. Comprehension performance is not used to exclude anyone.

Outcomes. Before treatment, respondents answered the ownership screener, seven-point party identification and ideology scales, and two filler items. After the video they answered, in a fixed order: the preferred strictness of laws covering the sale of firearms; support for six gun-policy proposals, four restrictions (among them a ban on assault-style weapons and background checks for private and gun-show sales) and two expansions (allowing concealed carry in more places and shortening waiting periods); the importance of gun policy in vote choice; perceived threat to the right to own guns; political-action intentions; and future purchase intentions. The partisan cue appeared next, for the cue arm only, followed by 0–100 feeling thermometers for the Democratic and Republican parties and demographic questions. Appendix B describes the instrument in more detail.

Our main outcomes are the Republican feeling thermometer and *net partisanship*, the Republican minus the Democratic thermometer, which ranges from -100 to 100 and is robust to respondent-specific use of the thermometer scale. Net partisanship captures the

relative partisan affect that underlies vote choice and political contributions, the outcome of our observational design. For gun attitudes, we examine two measures. *Support for less strict gun laws* is the preferred strictness of laws covering the sale of firearms, coded from 1 (much more strict) to 5 (much less strict), and measures general orientation toward gun regulation. The *gun-attitude index* is the first principal component of the six policy items and perceived threat to gun rights, and summarizes views on specific proposals. The index is oriented toward gun rights: it loads positively on opposing each of the four restrictions and on perceived threat, and negatively on opposing the two expansions. The strictness item is not among its components; the two measures are strongly correlated ($r = 0.77$) but distinct.

A natural treatment. The treatment video was not constructed for the experiment. It is a pre-existing video on YouTube, where it has been viewed approximately six million times, and it is representative of the gun content that circulates widely on social media. Content of this kind is also what a customer encounters in and around a gun store, where firearms are displayed and handled, product videos play on screens, and staff and other customers discuss the merits of particular guns. The treatment is therefore a natural, if brief, version of the exposure that a new store creates. Because the control video is also popular consumer-product content of similar length and format, the comparison holds fixed the experience of watching and answering questions about a product video.

Interpretation. Our experiment is a priming design: outcomes are measured within minutes of a single two-minute exposure. Such effects can decay quickly (Hill et al., 2013; Gerber et al., 2011), and we do not claim that one video permanently changes a person's partisanship. The relevance of the experiment for the observational results comes from the comparison of treatment conditions. Real-world exposure to guns through a local store is sustained, repeated and of much larger dosage: customers return, acquire firearms, take classes, and join the networks and media ecosystems of gun owners. If a single brief exposure moves partisan affect measurably, it is plausible that sustained exposure has larger and more durable effects, and could account for the persistent effects we observe in the contribution data.

5.2 Balance and Exposure

Table 3 reports the means of 18 pre-treatment covariates by arm. The arms are well balanced. Two differences are significant at the 5 percent level, residence in a metropolitan

area ($p = 0.002$) and moderate ideology ($p = 0.04$), about what one expects from 18 tests. Joint tests that the full covariate vector predicts assignment do not reject for either treated arm ($p = 0.59$ for the gun video and $p = 0.20$ for the gun video with the partisan cue). Exposure was uniform across arms: no respondent spent less than about 120 seconds on the video page, and the median time was 131 to 135 seconds in every arm.

Table 3. Covariate Balance across Experimental Arms

	Control (1)	Gun video (2)	Gun video + partisan cue (3)	p-value (4)
Gun ownership				
Owns a firearm	0.49	0.49	0.52	0.650
Firearm in household	0.66	0.64	0.69	0.310
Political predispositions				
Democrat (incl. leaners)	0.43	0.45	0.40	0.418
Independent	0.12	0.12	0.12	0.919
Republican (incl. leaners)	0.46	0.43	0.47	0.385
Liberal	0.32	0.27	0.29	0.119
Moderate	0.28	0.34	0.32	0.039
Conservative	0.40	0.39	0.39	0.866
Voted for Trump in 2024	0.51	0.52	0.54	0.756
Demographics				
Male	0.50	0.49	0.46	0.158
Age (years)	49.7	51.0	49.0	0.093
Bachelor's degree or higher	0.43	0.43	0.42	0.978
White	0.75	0.73	0.73	0.712
Black	0.11	0.13	0.13	0.296
Hispanic	0.14	0.14	0.17	0.181
Family income (1–7)	3.87	3.88	3.81	0.692
South	0.38	0.40	0.41	0.527
Metropolitan area	0.82	0.85	0.89	0.002
Joint test vs. control, p-value		0.594	0.204	
Observations	789	602	609	

Notes: Columns (1)–(3) report means by experimental arm; binary variables are shares. Column (4) reports the p-value from a joint test that both treated-arm coefficients are zero in a regression of the covariate on arm indicators and the ownership-stratum indicator, with HC2 robust standard errors. The joint-test row reports the p-value of an F-test that all covariates are jointly zero in a regression of the treated-arm indicator on the full pre-specified covariate vector and the stratum indicator, estimated on the subsample of that arm and the control group. Covariates are measured before treatment and taken from the Verasight panel profile where available. Party identification includes leaners. The 2024 vote is among respondents reporting a vote choice.

5.3 Estimation

For each ownership stratum $s \in \{\text{owners}, \text{non-owners}\}$, we estimate separately

$$y_i = \alpha + \beta_1 T_i + \beta_2 M_i + \beta_3 (T_i \times M_i) + X_i'\gamma + \varepsilon_i, \tag{3}$$

where T_i equals one if respondent i was shown the gun video (in either treated arm), M_i equals one for men, and X_i is a vector of controls: age, age squared, income, an indicator for religious affiliation, and fixed effects for census region, Hispanic ethnicity, race and education. The coefficient β_1 is the effect of the gun video on women in the stratum, β_3 is the difference between the effects on men and women, and $\beta_1 + \beta_3$ is the effect on men. Estimating the model separately by stratum is equivalent to a fully interacted model and lets every coefficient, including those on the controls, differ by ownership. We report estimates with and without controls and HC2 robust standard errors.

We pool the two treated arms throughout. The gun-attitude outcomes were measured before the partisan cue appeared, so for them the two treated arms had received identical treatment when respondents answered. For the thermometers, which were measured after the cue, the arm-versus-arm comparison is underpowered: the minimum detectable effect of the cue relative to the video alone is 5.4 thermometer points, larger than the pooled effect of the video itself, 4.1 points. We therefore treat the cue as part of the treatment rather than as a separate factor.[7]

5.4 Results: Partisan Affect

Table 4 reports the effects on partisan affect, and Appendix Table A7 reports the implied effects for each gender and ownership group from the controlled specifications. Pooling all respondents, the gun video raises the Republican thermometer by 4.1 points (standard error 1.5). The pooled effect masks a sharp pattern of heterogeneity.

Among non-owners (Panel B), the effect of the gun video is strongly gendered. The interaction with male is large and significant for both outcomes, and slightly larger with controls: 10.5 thermometer points (standard error 4.3) and 22.1 points of net partisanship (7.4). Non-owning men warm to the Republican Party by 10.2 thermometer points ($p = 0.003$), about 30 percent of the outcome's standard deviation, and move 12.1 points toward

[7] In our specification, the difference between the two treated arms is $+3.7$ points on the Republican thermometer (standard error 1.9) and -1.4 points on net partisanship (3.4). Together these imply that the cue raises the Democratic thermometer by about 5.1 points relative to the video alone, so information about the parties' positions appears to warm respondents toward both parties. Under the pre-specified covariate vector, the Republican thermometer difference is $+1.3$ (1.2).

Table 4. Effect of Gun-Video Exposure on Partisan Affect

	Republican thermometer		Net partisanship (Rep. − Dem.)	
	(1)	(2)	(3)	(4)
Panel A. Gun owners				
Treated	2.76	4.34	0.62	3.99
	(3.71)	(3.57)	(6.11)	(5.67)
Treated × Male	1.48	1.15	7.70	6.06
	(4.76)	(4.57)	(7.90)	(7.39)
Mean of dep. var.	52.53	52.52	14.32	14.25
SD of dep. var.	36.02	35.99	60.72	60.70
Controls		✓		✓
Observations	1,000	996	1,000	996
Panel B. Non-owners				
Treated	0.51	−0.30	−8.97*	−10.00**
	(2.74)	(2.55)	(4.73)	(4.46)
Treated × Male	9.57**	10.48**	19.88**	22.11***
	(4.56)	(4.29)	(7.78)	(7.38)
Mean of dep. var.	36.88	36.91	−10.96	−10.92
SD of dep. var.	34.26	34.30	59.15	59.22
Controls		✓		✓
Observations	1,000	997	1,000	997

Notes: OLS estimates with HC2 robust standard errors in parentheses. Treated equals one for respondents shown the gun video (both treated arms pooled) and zero for the control group. Each panel is estimated on its own ownership stratum: Panel A on respondents who personally own a firearm, Panel B on those who do not. The coefficient on Treated is the treatment effect for women in the stratum; the coefficient on Treated × Male is the difference between the effects for men and women. Every specification includes the male indicator; columns (2) and (4) add controls for age, age squared, income and a religious-affiliation indicator, together with fixed effects for census region, Hispanic ethnicity, race and education. The Republican thermometer ranges from 0 to 100; net partisanship is the Republican minus the Democratic thermometer and ranges from −100 to 100. Mean and SD of the dependent variable are computed over the estimation sample of the column. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the Republicans on net partisanship ($p = 0.04$). Non-owning women do not warm to the Republicans; if anything, they move away, by 10.0 points on net partisanship ($p = 0.03$). The same two-minute video thus pulls non-owning men and women 22 points apart.

Among owners (Panel A), the response is more muted and not distinctly gendered. The effects on the Republican thermometer are positive for both women (4.3 points) and men (5.5 points, $p = 0.054$), and the interaction with male is small and insignificant. Owners are far more Republican on average, with a mean net partisanship of $+14.3$ against -11.0 among non-owners, which leaves less room to move. The one owner estimate that is significant at the 5 percent level is the effect on net partisanship for men, 10.1 points ($p = 0.03$), which is similar to the effect for non-owning men.

Three features of these results bear on the interpretation of the observational evidence. First, the response among men is not confined to those who already own guns. The point estimates for men who do not personally own a gun exceed those for men who do: 10.2 against 5.5 points on the Republican thermometer, and 12.1 against 10.1 points on net partisanship. Exposure to guns thus moves the partisanship of men who do not own them, which is what the access channel requires, and not only that of men already invested in guns. Second, the gendered divergence is concentrated among non-owners, the group whose political response to new access the observational design cannot observe directly. Third, the effects among owners are smaller or less precisely estimated on the thermometer, as one would expect if owners are already treated: they have already been exposed to guns and gun culture, so one more video adds little. The differences across strata are, however, imprecisely estimated. Among men, the difference between non-owners and owners is 4.7 thermometer points (standard error 4.5) and 2.1 points of net partisanship (7.5). The difference between the gender interactions in the two strata, from the controlled specifications, is 9.3 thermometer points (6.3) and 16.1 points of net partisanship (10.4). The evidence therefore establishes that the response extends to men who do not own guns, but it does not establish that they respond more than owners.

Comparisons within gender are the relevant ones for the observational design, whose primary outcomes are for male donors. Stratum-level averages that pool men and women mix treatment effects with the different gender composition of the two strata: 61 percent of owners but only 36 percent of non-owners are men. Weighting the gender-specific effects by stratum composition, the average effect on the Republican thermometer is about 5.0 points among owners and 3.4 among non-owners, and on net partisanship about $+7.7$ and -2.1. The lower non-owner averages reflect the negative response of non-owning women, who make up nearly two thirds of that stratum, rather than a weaker response among non-owning men.

5.5 Results: Gun Attitudes

Table 5 reports the effects on support for less strict gun laws and on the gun-attitude index. Among non-owners (Panel B), the gendered pattern of Table 4 reappears in general orientation toward gun regulation. With controls, the interaction with male is 0.31 points on the five-point strictness scale (standard error 0.14, $p = 0.02$), about 0.3 standard deviations. The video moves non-owning women toward favoring stricter laws, by 0.14 points ($p = 0.08$), and non-owning men toward favoring less strict ones, by 0.18 points ($p = 0.11$); it is the difference between the two, rather than either effect on its own, that is precisely estimated. The gun-attitude index moves in the same direction but by less: the interaction is 0.32 index points ($p = 0.13$), about 0.19 standard deviations, and the implied effect for non-owning men is 0.30 points ($p = 0.08$), with essentially no effect for non-owning women. Among owners (Panel A), neither measure moves, for men or for women (Appendix Table A7).

The contrast between Tables 4 and 5 is itself informative. Among owners, exposure moves partisan affect, significantly so for men's net partisanship, while leaving gun-policy views unchanged. Among non-owners, it moves partisan affect and general orientation toward gun regulation in opposite directions for men and women, but moves views on specific proposals by less. This pattern is hard to reconcile with persuasion on the merits of particular regulations, which would move views on specific policies most. It points instead to an identity or group-affiliation channel: exposure to guns makes the social group associated with guns, and the party that represents it, more attractive to men and, among non-owners, less attractive to women, and people align their general stance on guns with that attachment more readily than their views on particular laws (Egan, 2020). The pattern is also hard to reconcile with experimenter demand, which would most naturally push all respondents toward more pro-gun views after a gun video rather than produce opposite responses among non-owning men and women (Mummolo and Peterson, 2019; de Quidt et al., 2018). Finally, because the owner–non-owner and male–female comparisons difference out any effect of the control video that is common across groups, the heterogeneity we emphasize does not depend on the control video being politically neutral.

5.6 Discussion

The experiment strengthens the causal interpretation of the observational results and clarifies their mechanism. Random exposure to a common piece of gun content moves partisan affect toward the Republican Party among men, in a setting where selection and

Table 5. Effect of Gun-Video Exposure on Gun-Policy Attitudes

	Support for less strict gun laws		Gun-attitude index	
	(1)	(2)	(3)	(4)
Panel A. Gun owners				
Treated	−0.107	−0.098	−0.064	−0.019
	(0.108)	(0.107)	(0.182)	(0.180)
Treated × Male	0.168	0.175	−0.037	−0.061
	(0.146)	(0.144)	(0.251)	(0.243)
Mean of dep. var.	2.647	2.646	0.749	0.746
SD of dep. var.	1.171	1.171	2.031	2.029
Controls		✓		✓
Observations	1,000	996	1,000	996
Panel B. Non-owners				
Treated	−0.117	−0.136*	0.027	−0.021
	(0.081)	(0.078)	(0.134)	(0.125)
Treated × Male	0.256*	0.313**	0.194	0.320
	(0.144)	(0.135)	(0.233)	(0.211)
Mean of dep. var.	1.978	1.981	−0.749	−0.746
SD of dep. var.	1.050	1.050	1.712	1.713
Controls		✓		✓
Observations	1,000	997	1,000	997

Notes: OLS estimates with HC2 robust standard errors in parentheses. Specifications as in Table 4. Support for less strict gun laws is the response to whether laws covering the sale of firearms should be made more strict, kept as they are, or made less strict, coded from 1 (much more strict) to 5 (much less strict). The gun-attitude index is the first principal component of the six gun-policy proposal items and perceived threat to gun rights, oriented so that higher values are more supportive of gun rights; its scale is arbitrary, with mean zero over the full sample and a standard deviation of about 2. The strictness item is not a component of the index, but the two are strongly correlated ($r = 0.77$), so columns (1)–(2) and (3)–(4) are not independent tests. All items were answered before the partisan cue was displayed. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

compositional change are ruled out by design. Perhaps surprisingly, the results is driven, in part, by men who do not personally own a gun. This is precisely the population whose potential firearm access is affected most by a new gun store, and their point estimates are the largest in the experiment. Interestingly, the response is concentrated in partisan attachment rather than views on specific policies, consistent with the effect operating on an emotive rather than an informational channel.

Findings from both settings point to a gendered impact and differential behavioral response. In the experiment, the immediate effect of exposure on women is small and insignificant among owners and, among non-owners, zero on the Republican thermometer and negative on net partisanship. This pattern is consistent with the absence of a short-run female response in the contribution data, where female Republican giving is both weaker and delayed. Altogether the results suggest that exposure to guns and gun media may lead to an escalation of partisan gender gap.

6 Conclusion

This paper provides evidence that access to firearms shifts partisan allegiance. The arrival of the first retail gun store in a ZIP code raises Republican political giving by local men within a few months, through new donors rather than larger gifts, and without any increase in Democratic giving. A randomized survey experiment shows that brief exposure to ordinary gun content moves men toward the Republican Party, that the effect extends to men who do not own guns, and that it moves partisan affect more than views on specific gun policies. Politics shape who buys guns; our results show that guns, in turn, shape politics.

We close with a more speculative discussion of mechanisms and implications. The pattern of our results, in which partisan affect moves more than views on specific gun policies, and owners' partisanship moves while their policy views do not, suggests that the operative mechanism is closer to identity formation than to persuasion. One interpretation is a political analogue of the endowment effect (Kahneman et al., 1990). Owning, or merely handling and contemplating, a good that carries a strong social meaning makes a person more invested in the group and the ideology associated with it. Guns are an extreme case of such an identity good: they are durable, visible, associated with a distinctive culture and set of institutions, and mapped by the party system onto a partisan divide. Consumption of such goods may do more than reveal preferences. It may play a determinative role in forming them, much as ownership of land or financial assets has been shown to shape beliefs about markets and politics (Di Tella et al., 2007; Jha and Shayo, 2019). The gender

asymmetry we find, with men drawn toward the Republican Party and non-owning women pushed away from it, is consistent with the view that the meaning of guns is itself gendered, bound up with norms of masculinity and protection (Stroud, 2012; Carlson, 2015).

These mechanisms shed light on the politics of gun policy. The United States has seen no comprehensive change in federal gun law for decades despite the scale of gun violence, and support for gun ownership appears remarkably inelastic, resilient even to highly salient mass shootings (Barney and Schaffner, 2019; Luca et al., 2020). Our results offer one explanation for this resilience. If gun ownership and exposure to guns generate partisan attachment, the diffusion of firearms creates its own political constituency: each new owner is a potential recruit to the coalition opposed to regulation, and the stock of guns becomes a stock of political support. Surges in gun purchasing, such as the one that followed the onset of the COVID-19 pandemic and brought millions of new owners into the market (Miller et al., 2022), may therefore have political consequences that outlast them. The feedback also runs through the marketplace: retailers and manufacturers of firearms, and the media that promote them, may shape the political landscape as much as political organizations do.

Our analysis has limitations that suggest directions for future work. The contribution data measure political behavior at the level of places rather than people, and itemized donors are a selected group, so we cannot observe who among the residents of an entrant ZCTA changes their behavior. Linking individual gun purchases to individual political behavior, for instance through voter files, would allow a direct test of the access channel. Our experiment measures immediate responses to a single exposure; studying the durability of these effects and the effect of repeated exposure, including through algorithmically curated social media, is a natural next step. Finally, the logic of our argument extends beyond firearms. Many goods now carry partisan meaning, from vehicles and energy technologies to food and media. Whether the diffusion of such goods reshapes political identity, and through what mechanisms, is a promising question for research on the political economy of consumption.

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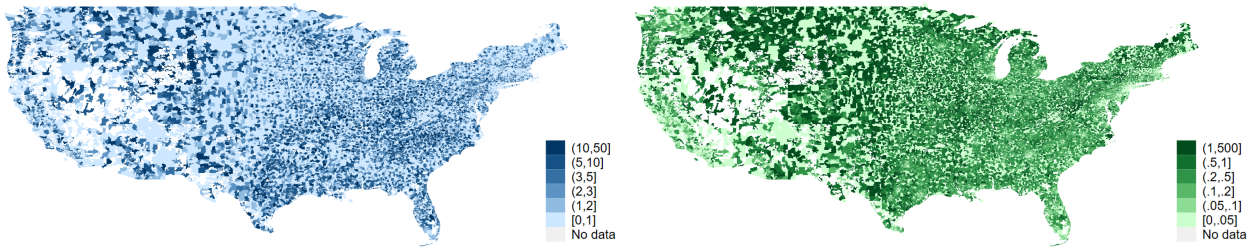
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A Additional Figures and Tables

Figure A1. Federal Firearms Licensees at the Start of the Panel, January 2018



(a) Number of licensees

(b) Licensees per 1,000 residents

Notes: Each map shows ZCTAs in the contiguous United States. Panel (a) shows the number of Federal Firearms Licensees in January 2018; panel (b) shows licensees per 1,000 residents, using 2020 Census population. Gray indicates no data. Source: ATF Federal Firearms Listings.

Table A1. Summary Statistics, ZCTA Panel

Panel A. Sample construction				
ZCTAs in the national panel	32,989			
Months (January 2018 to March 2025)	87			
ZCTAs with no retail dealer, January 2018	14,636			
Entrants (gain a first retail dealer)	2,381			
Never-entrants (comparison group)	12,255			
Entrants whose dealer later exits	784			
Distinct entry months (cohorts)	71			
ZCTA-months, zero-baseline sample	1,273,332			
	Men		Women	
	Mean	SD	Mean	SD
Panel B. Outcomes, zero-baseline sample				
Log Republican dollars, $\ln(1+\$)$	1.030	2.289	0.723	1.896
Republican share of two-party dollars	0.129	0.314	0.091	0.268
Log Republican donors, $\ln(1+n)$	0.215	0.521	0.131	0.388
Republican share of two-party donors	0.130	0.307	0.087	0.258
	Entrants	Never	Entrants	Never
Panel C. Extensive margin, share of ZCTA-months				
Share with a Republican donor	0.30	0.16	0.21	0.11

Notes: Unit of observation is the ZCTA-month. In Panel C, "Never" refers to never-entrant ZCTAs. Retail dealers are Federal Firearms Licensees that sell to the public, from the monthly ATF listings. Political giving is built from itemized individual contributions in FEC records, assigned to ZCTAs by the donor's ZIP code and to parties by the recipient candidate or committee. "Women" refers to female or unclassified donors, computed as all-donor minus male-donor quantities; gender is inferred from first names. Shares are set to zero when the two-party total is zero; about 74 percent of ZCTA-months have no two-party giving by men. Where the share is defined, the Republican share of two-party giving averages 0.50 for men and 0.38 for women. ZCTA-months with negative net Republican dollars (refunds exceeding contributions) are excluded from the log-dollar outcome only (964 for men, 750 for women).

Table A2. Event-Study Summary: Pre-Entry Coefficients and Post-Entry Aggregates

	Pre-entry		Post-entry			
	CS average -12 to -2 (1)	TWFE bin ≤ -12 (2)	CS average 0 to 12 (3)	CS month 12 (4)	CS simple ATT (5)	TWFE bin ≥ 12 (6)
Panel A. Male donors						
Log dollars	-0.058	-0.162^{***}	0.060	0.132^{**}	0.184^{***}	0.155^{***}
	(0.040)	(0.042)	(0.039)	(0.054)	(0.044)	(0.041)
Dollar share	-0.008	-0.022^{***}	0.014^{**}	0.029^{***}	0.031^{***}	0.024^{***}
	(0.007)	(0.007)	(0.007)	(0.010)	(0.007)	(0.007)
Log donors	-0.013	-0.050^{***}	0.021^{***}	0.046^{***}	0.062^{***}	0.058^{***}
	(0.008)	(0.010)	(0.008)	(0.011)	(0.009)	(0.009)
Donor share	-0.007	-0.023^{***}	0.016^{**}	0.029^{***}	0.034^{***}	0.027^{***}
	(0.007)	(0.007)	(0.007)	(0.009)	(0.007)	(0.007)
Panel B. Female (or unclassified) donors						
Log dollars	-0.066^{*}	-0.161^{***}	0.019	0.115^{**}	0.139^{***}	0.132^{***}
	(0.036)	(0.039)	(0.034)	(0.049)	(0.039)	(0.037)
Dollar share	-0.008	-0.018^{***}	0.007	0.023^{***}	0.023^{***}	0.020^{***}
	(0.006)	(0.007)	(0.006)	(0.009)	(0.007)	(0.006)
Log donors	-0.012	-0.043^{***}	0.005	0.035^{***}	0.049^{***}	0.050^{***}
	(0.007)	(0.009)	(0.007)	(0.011)	(0.008)	(0.009)
Donor share	-0.007	-0.022^{***}	0.004	0.020^{**}	0.023^{***}	0.021^{***}
	(0.006)	(0.006)	(0.006)	(0.009)	(0.006)	(0.006)

Notes: Sample, outcomes and estimators as in Tables 1 and 2. All estimates are relative to the month before entry ($t = -1$). Column (1) is the unweighted mean of the Callaway and Sant'Anna (2021) (CS) event-time effects for months -12 to -2; column (2) is the coefficient on the binned TWFE lead indicator for twelve or more months before entry, which captures the level of giving more than a year before entry relative to $t = -1$. Column (3) is the unweighted mean of the CS event-time effects for months 0 to 12; column (4) is the CS event-time effect at month 12; column (5) is the CS simple ATT, which averages all post-entry cohort-by-month effects including those beyond month 12; column (6) is the coefficient on the binned TWFE lag indicator for twelve or more months after entry. Standard errors clustered by ZCTA in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3. The Gender Gap in the Timing of the Response

	Relative to month before entry				Windowed DiD	
	CS month 12 (1)	CS simple ATT (2)	CS average 0 to 12 (3)	TWFE average 0 to 11 (4)	Months 0 to 12 (5)	Months 13+ (6)
Panel A. Log Republican dollars						
Men	0.132**	0.184***	0.060	0.053	0.168***	0.259***
	(0.054)	(0.044)	(0.039)	(0.038)	(0.022)	(0.023)
Women	0.115**	0.139***	0.019	0.008	0.127***	0.238***
	(0.049)	(0.039)	(0.034)	(0.034)	(0.021)	(0.022)
Ratio, women/men	0.88	0.75	0.32	0.15	0.75	0.92
Difference, men − women					0.041	
					(0.029)	
Panel B. Republican share of two-party dollars						
Men	0.029***	0.031***	0.014**	0.012*	0.028***	0.038***
	(0.010)	(0.007)	(0.007)	(0.007)	(0.004)	(0.004)
Women	0.023***	0.023***	0.007	0.005	0.018***	0.031***
	(0.009)	(0.007)	(0.006)	(0.006)	(0.004)	(0.004)
Ratio, women/men	0.79	0.73	0.54	0.44	0.66	0.81
Difference, men − women					0.009*	
					(0.005)	
Panel C. Log Republican donors						
Men	0.046***	0.062***	0.021***	0.017**	0.052***	0.089***
	(0.011)	(0.009)	(0.008)	(0.008)	(0.006)	(0.007)
Women	0.035***	0.049***	0.005	0.003	0.034***	0.078***
	(0.011)	(0.008)	(0.007)	(0.007)	(0.006)	(0.007)
Ratio, women/men	0.75	0.79	0.26	0.19	0.66	0.88
Difference, men − women					0.018**	
					(0.007)	
Panel D. Republican share of two-party donors						
Men	0.029***	0.034***	0.016**	0.014**	0.030***	0.041***
	(0.009)	(0.007)	(0.007)	(0.006)	(0.004)	(0.004)
Women	0.020**	0.023***	0.004	0.002	0.018***	0.034***
	(0.009)	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)
Ratio, women/men	0.67	0.67	0.24	0.15	0.60	0.84
Difference, men − women					0.012**	
					(0.005)	

Notes: Sample and outcomes as in Tables 1 and 2; "women" refers to female or unclassified donors. Columns (1)–(3) are Callaway and Sant'Anna (2021) (CS) aggregates relative to the month before entry: the event-time effect at month 12, the simple ATT over all post-entry months, and the mean of the event-time effects for months 0 to 12. Column (4) is the mean of the TWFE event-study coefficients for months 0 to 11 (month 12 is a binned endpoint). Columns (5) and (6) are from a TWFE regression with separate indicators for months 0 to 12 and months 13 and later after entry; their comparison period is the entire pre-entry period rather than $t = -1$. The ratio row divides the estimate for women by that for men. The difference row reports the male-minus-female difference in the column (5) coefficient from a regression that stacks the two outcomes with ZCTA-by-gender and month-by-gender fixed effects. Standard errors clustered by ZCTA in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4. Heterogeneity by State Regulation of Private Firearm Sales, Male Donors

	Private sales exempt from background checks		Universal background checks		Difference in CS ATT
	CS ATT (1)	TWFE (2)	CS ATT (3)	TWFE (4)	(1) − (3) (5)
Log dollars	0.177***	0.240***	0.208***	0.224***	−0.031
	(0.054)	(0.024)	(0.073)	(0.034)	(0.091)
Dollar share	0.031***	0.039***	0.031***	0.028***	0.000
	(0.009)	(0.004)	(0.012)	(0.006)	(0.015)
Log donors	0.063***	0.077***	0.067***	0.084***	−0.003
	(0.010)	(0.007)	(0.016)	(0.010)	(0.019)
Donor share	0.036***	0.041***	0.033***	0.031***	0.003
	(0.009)	(0.005)	(0.011)	(0.006)	(0.014)
ZCTAs	7,612		7,024		
Entrant ZCTAs	1,446		935		

Notes: The zero-baseline sample is split by whether the ZCTA's state requires a background check for private (non-dealer) firearm sales. Columns (1) and (2) cover the 29 states in which private sales between residents are exempt from background checks, so that a buyer can acquire a firearm without visiting a dealer; columns (3) and (4) cover the remaining states and the District of Columbia. CS ATT is the Callaway and Sant'Anna (2021) simple ATT with never-entrants from the same group of states as the comparison group; TWFE is the coefficient on a single post-entry indicator with ZCTA and month fixed effects. Column (5) is the difference between columns (1) and (3), with a standard error that treats the two estimates as independent, since the groups share no ZCTAs. Outcomes are for male donors and defined as in Table 1. Standard errors clustered by ZCTA in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5. Event-Study Coefficients, Male Donors

Months	Log dollars		Dollar share		Log donors		Donor share	
since entry	TWFE (1)	CS (2)	TWFE (3)	CS (4)	TWFE (5)	CS (6)	TWFE (7)	CS (8)
≤ -12	-0.162^{***}	-0.043	-0.022^{***}	-0.010	-0.050^{***}	-0.023^{*}	-0.023^{***}	-0.011
	(0.042)	(0.059)	(0.007)	(0.010)	(0.010)	(0.012)	(0.007)	(0.010)
-11	-0.022	-0.085	-0.003	-0.011	-0.002	-0.017	-0.005	-0.014
	(0.054)	(0.056)	(0.010)	(0.010)	(0.011)	(0.012)	(0.009)	(0.010)
-10	-0.018	-0.079	-0.002	-0.010	-0.003	-0.016	-0.003	-0.010
	(0.055)	(0.056)	(0.009)	(0.010)	(0.011)	(0.012)	(0.009)	(0.009)
-9	0.031	-0.027	0.001	-0.007	0.002	-0.010	0.003	-0.004
	(0.055)	(0.057)	(0.009)	(0.010)	(0.011)	(0.012)	(0.009)	(0.009)
-8	0.025	-0.029	0.000	-0.006	0.008	-0.003	0.004	-0.002
	(0.054)	(0.056)	(0.010)	(0.010)	(0.011)	(0.012)	(0.009)	(0.009)
-7	-0.015	-0.071	-0.004	-0.012	-0.007	-0.020^{*}	-0.001	-0.009
	(0.054)	(0.055)	(0.009)	(0.010)	(0.011)	(0.012)	(0.009)	(0.009)
-6	-0.054	-0.113^{**}	-0.011	-0.019^{**}	-0.008	-0.020^{*}	-0.006	-0.014
	(0.053)	(0.054)	(0.009)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)
-5	-0.041	-0.070	-0.009	-0.012	-0.008	-0.015	-0.008	-0.012
	(0.052)	(0.053)	(0.009)	(0.009)	(0.010)	(0.011)	(0.008)	(0.009)
-4	-0.031	-0.059	-0.004	-0.007	-0.003	-0.009	-0.004	-0.007
	(0.051)	(0.052)	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)	(0.009)
-3	-0.039	-0.044	0.001	0.001	-0.003	-0.004	0.002	0.001
	(0.049)	(0.050)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
-2	-0.023	-0.022	0.002	0.002	-0.008	-0.009	0.003	0.004
	(0.044)	(0.045)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
-1	*Reference period*							
0	-0.043	-0.045	-0.003	-0.003	-0.003	-0.004	-0.002	-0.002
	(0.045)	(0.045)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
1	0.039	0.036	0.010	0.010	0.019^{**}	0.018^{**}	0.012	0.013
	(0.048)	(0.049)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
2	0.052	0.041	0.007	0.007	0.022^{**}	0.020^{*}	0.010	0.010
	(0.050)	(0.050)	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)	(0.008)
3	0.113^{**}	0.106^{**}	0.019^{**}	0.019^{**}	0.027^{**}	0.024^{**}	0.021^{**}	0.021^{**}
	(0.051)	(0.052)	(0.009)	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)
4	0.109^{**}	0.113^{**}	0.024^{***}	0.025^{***}	0.031^{***}	0.032^{***}	0.027^{***}	0.028^{***}
	(0.052)	(0.053)	(0.009)	(0.009)	(0.011)	(0.011)	(0.009)	(0.009)
5	0.013	0.015	0.006	0.006	0.002	0.004	0.010	0.010
	(0.052)	(0.052)	(0.009)	(0.009)	(0.011)	(0.011)	(0.009)	(0.009)
6	0.024	0.035	0.007	0.008	0.020^{*}	0.024^{**}	0.014	0.015
	(0.052)	(0.053)	(0.010)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)
7	0.074	0.071	0.012	0.011	0.009	0.011	0.011	0.011
	(0.053)	(0.053)	(0.009)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)
8	0.081	0.079	0.018^{*}	0.019^{*}	0.020^{*}	0.023^{**}	0.020^{**}	0.021^{**}
	(0.053)	(0.054)	(0.009)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)
9	0.014	0.020	0.004	0.006	0.014	0.017	0.007	0.008
	(0.053)	(0.053)	(0.009)	(0.009)	(0.011)	(0.011)	(0.009)	(0.009)
10	0.027	0.032	0.012	0.013	0.013	0.016	0.014	0.015
	(0.053)	(0.054)	(0.009)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)
11	0.131^{**}	0.146^{***}	0.025^{***}	0.027^{***}	0.034^{***}	0.039^{***}	0.027^{***}	0.029^{***}
	(0.052)	(0.053)	(0.009)	(0.010)	(0.011)	(0.011)	(0.009)	(0.009)
≥ 12	0.155^{***}	0.132^{**}	0.024^{***}	0.029^{***}	0.058^{***}	0.046^{***}	0.027^{***}	0.029^{***}
	(0.041)	(0.054)	(0.007)	(0.010)	(0.009)	(0.011)	(0.007)	(0.009)

Notes: Coefficients plotted in Figure 3. TWFE columns are from an event-study regression with ZCTA and month fixed effects and indicators for 12 leads and 12 lags of entry, with the month before entry omitted; the first and last rows of the TWFE columns are binned endpoints (twelve or more months before or after entry). CS columns are Callaway and Sant'Anna (2021) event-time aggregates with never-entrant controls and the month before entry as the base period; their first and last rows are the effects at exactly -12 and $+12$ months. Sample and outcomes as in Tables 1 and 2. Standard errors clustered by ZCTA in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6. Event-Study Coefficients, Female (or Unclassified) Donors

Months	Log dollars		Dollar share		Log donors		Donor share	
since entry	TWFE (1)	CS (2)	TWFE (3)	CS (4)	TWFE (5)	CS (6)	TWFE (7)	CS (8)
≤ -12	-0.161^{***}	-0.036	-0.018^{***}	-0.002	-0.043^{***}	-0.006	-0.022^{***}	0.001
	(0.039)	(0.051)	(0.007)	(0.009)	(0.009)	(0.011)	(0.006)	(0.009)
-11	0.044	0.002	0.006	0.000	0.009	-0.002	0.008	0.001
	(0.050)	(0.052)	(0.009)	(0.009)	(0.010)	(0.011)	(0.008)	(0.009)
-10	-0.004	-0.064	-0.003	-0.012	-0.001	-0.013	-0.004	-0.013
	(0.049)	(0.050)	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)	(0.008)
-9	0.004	-0.055	0.003	-0.005	-0.003	-0.015	0.003	-0.006
	(0.050)	(0.052)	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)	(0.009)
-8	-0.008	-0.061	0.003	-0.005	0.009	-0.002	0.007	-0.002
	(0.048)	(0.049)	(0.008)	(0.009)	(0.010)	(0.011)	(0.008)	(0.009)
-7	-0.098^{**}	-0.155^{***}	-0.012	-0.021^{**}	-0.011	-0.023^{**}	-0.005	-0.015^{*}
	(0.047)	(0.049)	(0.008)	(0.009)	(0.009)	(0.010)	(0.008)	(0.008)
-6	-0.099^{**}	-0.157^{***}	-0.012	-0.021^{**}	-0.014	-0.025^{***}	-0.013	-0.022^{***}
	(0.046)	(0.048)	(0.008)	(0.009)	(0.009)	(0.010)	(0.008)	(0.008)
-5	-0.053	-0.092^{**}	-0.010	-0.015^{*}	-0.006	-0.012	-0.004	-0.009
	(0.046)	(0.047)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)
-4	-0.036	-0.074^{*}	-0.008	-0.014^{*}	-0.011	-0.016^{*}	-0.008	-0.012^{*}
	(0.044)	(0.045)	(0.008)	(0.008)	(0.009)	(0.009)	(0.007)	(0.008)
-3	0.002	-0.014	0.005	0.003	-0.012	-0.014^{*}	0.000	-0.002
	(0.042)	(0.042)	(0.007)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
-2	-0.014	-0.026	0.004	0.002	-0.002	-0.004	0.000	-0.001
	(0.039)	(0.039)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
-1				*Reference period*				
0	-0.009	-0.009	0.004	0.004	-0.003	-0.003	0.000	0.001
	(0.038)	(0.038)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)
1	-0.038	-0.034	-0.002	0.000	-0.004	-0.004	-0.002	-0.001
	(0.042)	(0.042)	(0.007)	(0.007)	(0.008)	(0.008)	(0.007)	(0.007)
2	-0.032	-0.030	-0.005	-0.004	0.000	0.000	-0.002	-0.002
	(0.046)	(0.046)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)
3	0.025	0.017	0.010	0.010	0.002	0.001	0.006	0.007
	(0.045)	(0.045)	(0.008)	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)
4	-0.011	-0.010	0.000	0.002	-0.008	-0.010	-0.006	-0.006
	(0.045)	(0.046)	(0.008)	(0.008)	(0.010)	(0.010)	(0.008)	(0.008)
5	0.016	0.016	0.005	0.005	0.005	0.004	0.005	0.005
	(0.047)	(0.047)	(0.008)	(0.008)	(0.010)	(0.010)	(0.008)	(0.008)
6	0.055	0.062	0.018^{**}	0.019^{**}	0.012	0.013	0.012	0.012
	(0.049)	(0.049)	(0.009)	(0.009)	(0.010)	(0.010)	(0.008)	(0.008)
7	0.009	0.006	0.010	0.009	-0.001	-0.003	0.004	0.003
	(0.046)	(0.046)	(0.008)	(0.008)	(0.010)	(0.010)	(0.008)	(0.008)
8	-0.043	-0.044	-0.005	-0.005	-0.007	-0.009	-0.007	-0.008
	(0.047)	(0.047)	(0.008)	(0.008)	(0.010)	(0.010)	(0.008)	(0.008)
9	0.012	0.020	0.007	0.008	0.008	0.008	0.004	0.005
	(0.046)	(0.046)	(0.008)	(0.008)	(0.010)	(0.010)	(0.008)	(0.008)
10	0.056	0.071	0.013	0.015^{*}	0.018^{*}	0.019^{*}	0.008	0.009
	(0.047)	(0.048)	(0.008)	(0.009)	(0.010)	(0.010)	(0.008)	(0.008)
11	0.055	0.068	0.007	0.009	0.018^{*}	0.019^{*}	0.004	0.005
	(0.045)	(0.046)	(0.008)	(0.008)	(0.010)	(0.010)	(0.008)	(0.008)
≥ 12	0.132^{***}	0.115^{**}	0.020^{***}	0.023^{***}	0.050^{***}	0.035^{***}	0.021^{***}	0.020^{**}
	(0.037)	(0.049)	(0.006)	(0.009)	(0.009)	(0.011)	(0.006)	(0.009)

Notes: Coefficients plotted in Figure 4. TWFE columns are from an event-study regression with ZCTA and month fixed effects and indicators for 12 leads and 12 lags of entry, with the month before entry omitted; the first and last rows of the TWFE columns are binned endpoints (twelve or more months before or after entry). CS columns are Callaway and Sant'Anna (2021) event-time aggregates with never-entrant controls and the month before entry as the base period; their first and last rows are the effects at exactly -12 and $+12$ months. Sample and outcomes as in Tables 1 and 2. Standard errors clustered by ZCTA in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7. Implied Treatment Effects by Gender and Ownership

	Gun owners		Non-owners	
	Women (1)	Men (2)	Women (3)	Men (4)
Republican thermometer	4.34	5.48*	−0.30	10.18***
	(3.57)	(2.84)	(2.55)	(3.44)
Net partisanship (Rep. − Dem.)	3.99	10.05**	−10.00**	12.11**
	(5.67)	(4.72)	(4.46)	(5.88)
Support for less strict gun laws	−0.10	0.08	−0.14*	0.18
	(0.11)	(0.10)	(0.08)	(0.11)
Gun-attitude index	−0.02	−0.08	−0.02	0.30*
	(0.18)	(0.17)	(0.13)	(0.17)
Respondents	386	614	643	357

Notes: Effects of the gun video implied by the controlled specifications in columns (2) and (4) of Tables 4 and 5. Columns (1) and (3) report the coefficient on Treated (β_1); columns (2) and (4) report the sum of the coefficients on Treated and Treated × Male ($\beta_1 + \beta_3$). HC2 robust standard errors in parentheses. The respondent counts are for the full stratum; the controlled specifications drop three to four respondents with missing census region. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Experimental Materials

Survey flow. The sequence of the instrument was identical for all respondents apart from the assigned video and the partisan cue:

1. Consent, noting that the survey includes short videos and questions about firearms and politics.
2. Screener: whether anyone in the household owns a firearm and, if so, whether the respondent does. This assigns the ownership stratum.
3. Pre-treatment political items: seven-point party identification and seven-point ideology.
4. Pre-treatment filler and quality items: hours of sleep the previous night and an open-ended question on a favorite ice-cream flavor, used to screen automated or inattentive responses before assignment.
5. Random assignment to control, gun video, or gun video plus partisan cue, within the ownership stratum (40/30/30).
6. Assigned video, played to completion, with viewing time recorded.
7. Factual comprehension questions about the video, with a small bonus for correct answers.
8. Gun-policy items: preferred strictness of laws covering the sale of firearms (five categories); support for six gun-policy proposals (five categories each); importance of gun policy in vote choice; perceived threat to the right to own guns (four categories); political-action intentions; future gun purchase intention.
9. Partisan cue (cue arm only), with a minimum reading time. The median time on the cue page was 23 seconds.
10. Feeling thermometers for the Democratic and Republican parties (0–100).
11. Demographics: gender, age, education, Hispanic origin, race, family income, religion and ZIP code; census region and metropolitan status are derived from the ZIP code.
12. Debriefing explaining the purpose of the study and the random assignment of videos.

Partisan cue. The cue consisted of two paragraphs of roughly equal length. The Democratic paragraph described support for stronger regulation while preserving the right of law-abiding citizens to own firearms, listing universal background checks, closing loopholes in existing laws, red-flag laws, restrictions on certain semiautomatic rifles and high-capacity magazines, and federal investment in gun-violence prevention. The Republican paragraph described an emphasis on Second Amendment rights and opposition to new federal regulation, listing support for concealed carry, opposition to assault-weapons bans and most purchase restrictions, reducing regulatory burdens on gun ownership, and

a focus on enforcing existing laws, mental health, school security and prosecuting violent offenders.

Outcome construction. The Republican thermometer is the 0–100 rating of the Republican Party. Net partisanship is the Republican minus the Democratic thermometer. Support for less strict gun laws is the response to whether laws covering the sale of firearms should be made more strict, kept as they are, or made less strict, coded from 1 (much more strict) to 5 (much less strict). Perceived threat, a component of the index, is coded from 1 (not at all threatened) to 4 (very threatened). The gun-attitude index is the first principal component of the six gun-policy proposal items and perceived threat, scored on the raw survey scales. Its sign was verified rather than assumed: it loads positively on opposing each of the four restrictions (loadings of 0.36 to 0.43) and on perceived threat (0.32), and negatively on opposing the two expansions (-0.36 and -0.35), which in the raw coding means favoring them. The index has mean zero over the full sample and a standard deviation of about 2.0; mean values are $+0.75$ among owners and -0.75 among non-owners.

Estimation details. Standard errors are HC2 heteroskedasticity-robust; randomization is at the individual level with no clustering. Controlled specifications include fixed effects for census region, Hispanic ethnicity, race and education, estimated as indicator variables. Census region is missing for seven respondents, so the controlled specifications have 996 or 997 observations per stratum. The male indicator equals one for respondents who report their gender as male; all other respondents, including the few who report another gender, form the comparison group in the gender interactions. The joint balance tests use conventional F-tests because the covariate vector contains sparse indicators that create observations with leverage one, which makes robust Wald tests unreliable.