

# CEO Activism, Consumer Polarization, and Firm Performance

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## Abstract

CEOs are increasingly engaging in activism on controversial social and political issues that do not directly affect their businesses. Simultaneously, the general public is increasingly polarized. We examine how CEO support for gun control after two mass shootings affects firm performance and polarizes consumers. Using mobile phone location data to measure store-level visits, we study (a) the net effect of activism on store performance, (b) the potential for polarization to create asymmetry in the activism's effects on consumers, and (c) the persistence of those performance effects. We find that activism has small net effects on sales, polarizes consumers, has asymmetric effects across liberals and conservatives, and quickly dissipates. Our results highlight the strategic implications for executives pressured to take stances on controversial issues.

**Keywords:** CEO activism, guns, polarization, non-market strategy

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## 1. INTRODUCTION

*“It doesn’t matter how many people hate your brand as long as enough people love it.”*

— Phil Knight, co-founder, Nike

During one August weekend in 2019, two mass shootings in the United States killed 31 people and reignited a perennial, contentious debate over gun policy. Several weeks later, at least 146 CEOs of prominent companies joined the discussion by urging Congress to pass new gun control measures. Their activism is just one of several examples in recent years of CEOs speaking out about controversial issues that do not directly affect their business. Other examples include the CEO of Nike supporting Black Lives Matter, the CEO of Chick-fil-A opposing same-sex marriage, and more than 75 CEOs supporting access to abortion.

CEOs considering whether to take public positions on controversial issues that are not directly related to their business may anticipate several potential tradeoffs. Emerging research suggests that activism may align a firm with its employees’ values (Bermiss & McDonald, 2018; Burbano, 2020) and create positive brand associations for consumers who agree with the CEO’s position (Chatterji & Toffel, 2019; Panagopoulos *et al.*, 2020). Although CEO activism can differentiate a firm, it may also be controversial with many consumers. Therefore, its net effect on performance is unclear; the relative gains and costs associated with consumers who agree and disagree with the CEO are unknown (Mikeska & Harvey, 2015).

Unfortunately for wary executives, CEO activism cannot be covert (Werner, 2017) and staying silent may also be costly: survey evidence suggests that 65 percent of consumers want CEOs to speak on major social issues (Larcker *et al.*, 2018). Although many CEOs speak out on controversial topics (Chatterji & Toffel, 2018), the evidence on the effect of such activism remains insufficient to inform strategy. This paper adds observational evidence to the growing literature on

how CEO activism affects firm performance and emphasizes two novel observations about activism: (a) polarization can create asymmetry in the effects on liberals and conservatives and (b) persistence in CEO activism's effects over time has implications for its strategic use.

We use a large sample of mobile phone location data to measure how store-level foot traffic (a proxy for sales) is affected by CEO support for gun control. Our research using observational data complements recent research on consumer reactions to CEO activism, which has so far relied on surveys and experiments to measure purchase intentions (Mikeska & Harvey, 2015; Chatterji & Toffel, 2019; Panagopoulos *et al.*, 2020) or emphasized corporate governance issues and the prevalence of activism rather than its effects on consumers (Mayer, 2017; Larcker *et al.*, 2018). Furthermore, our data's granularity allows us to examine both activism's net effects and its effects in geographic areas more or less likely to support stricter gun control policies.

We find that CEO activism supporting gun control has a small, negative net effect on sales of about three percent but polarizes consumer spending. The impacts on likely supporters and opponents of gun control are asymmetric. Store visits do not change in the most politically liberal counties, but drop by about five percent in the most politically conservative counties. These effects, however, dissipate within a few weeks.

The strategy literature has mainly focused on differentiation in the market setting through product attributes, but the potential of CEO activism to polarize consumers suggests that executives could strategically use this, too, to differentiate their companies' products. Scholars have already shown that firm actions in the *non-market* environment can affect competitive dynamics in the market. Flammer (2015) finds that U.S. firms increased their corporate social responsibility (CSR) activities as a result of increased foreign competition, while Hull and Rothenberg (2008) find that firms can use corporate social performance to improve financial

performance. Adding to this research stream, we examine how non-market activities can affect firm performance. However, unlike most CSR activities, CEO activism often addresses issues that are highly controversial among consumers, making its effect on performance theoretically less clear. For example, on the issue of gun control, fewer than 50 percent of consumers think it is appropriate for companies to take positions compared to 75–85 percent who support activism related to issues such as pay equality or the environment (Chatterji & Toffel, 2018).

Our findings contribute to nascent research on CEO activism and to the broader literature on non-market strategy. First, we offer evidence on the effects of CEO activism using granular, store-level data. Thus, our results contribute to a body of evidence that can inform managerial decisions about whether to engage in activism. Second, we contribute to the non-market strategy literature by explicitly measuring the market consequences of non-market actions (Baron, 1995; Baron, 2001; de Figueiredo, 2009; Shotts, 2015; Oberholzer-Gee & Yao, 2018). Taking advantage of a triggering event (back-to-back mass shootings), we reduce the number of confounding factors that can affect observed outcomes. These advantages help us to more concretely connect non-market actions with market outcomes and alleviate identification concerns noted in previous research (Bonardi, Holburn & Vanden Bergh, 2006).

The remainder of the paper is organized as follows: Section 2 describes the activism event we study: CEO support for gun control in September 2019 following two mass shootings. Section 3 discusses relevant theories of how CEO activism may affect consumer behavior. Sections 4 and 5 explain the data and methodology, respectively. Section 6 presents our results and Section 7 concludes.

## 2. CEO ACTIVISM ON GUN CONTROL

On August 3 and 4 of 2019, two mass shootings—the first in a Walmart in El Paso, TX, and the second in a popular downtown area of Dayton, OH—killed 31 people and reignited debates over gun policy in the United States. Within a day of the shootings, Ohio Senator Sherrod Brown and Senate Minority Leader Chuck Schumer pressed for an emergency session in the Senate to vote on the Bipartisan Background Checks Act of 2019, a bill introduced in January 2019 that had earlier passed the House of Representatives. The bill, which was never passed, would have expanded background checks for gun purchases.

In the month after the shootings, 145 CEOs from various industries collectively voiced their support for stricter gun control. In a September 12 letter sent to U.S. senators, they referred to gun violence as “a public health crisis that demands urgent action.” They called on lawmakers to support expanded background checks and “red flag” laws that would enable courts to temporarily limit firearm possession by individuals at risk of hurting themselves or others (Chesky *et al.*, 2019). In a similar letter sent to lawmakers on September 3, Doug McMillon, the CEO of Walmart, explained that the company would stop selling certain firearms and ammunition, encouraged lawmakers to support expanded background checks (that is, a “red flag” law), and called for a debate on reauthorization of the federal assault weapons ban that had expired in 2004 (Johnson, 2019).

News of these CEOs’ support for gun control quickly spread through major news and social media channels such as *The Wall Street Journal*, *The New York Times*, *USA Today*, ABC’s evening television news broadcast, and Twitter (Bomey, 2019; Kapner, 2019; Muir, Angeles & Karl, 2019; Nassauer & Lucey, 2019; Sorkin, 2019). Additionally, the National Rifle Association weighed in on the day of Walmart’s letter and again the following week, characterizing McMillon’s stance on

gun policy as “a bridge too far” and suggesting that the move would “risk alienating whatever remaining pro-gun shoppers [Walmart had] left” (NRA, 2019).

### 3. THEORY AND HYPOTHESIS

CEO activism on controversial issues such as gun control, abortion, and LGBTQ rights can differentiate a firm from its competitors. For example, by supporting gun control, McMillon established Walmart as a company that cares about gun safety. As retailers’ positions on gun control diverge, they become attributes consumers can consider when choosing where to shop. As with other non-quality product attributes, consumers likely differ in their taste for patronizing stores whose CEOs take different positions on controversial issues. Support for gun control may please one group of customers and antagonize another, potentially polarizing a firm’s consumers. As a result, the net effect of CEO activism on firm performance is theoretically unclear.

To understand how CEO activism interacts with consumers’ values to affect performance, we draw from the political science literature on affective polarization and lifestyle politics. We argue that CEO activism on polarizing issues is likely to asymmetrically affect consumers who agree and disagree with the CEO’s position. Additionally, we consider reasons why activism’s effects may be short-lived and discuss the strategic implications of this.

#### 3.1. Polarization and asymmetric CEO activism effects on consumers

Given that CEO activism issues are often politically charged—especially the issue we study, gun control—we begin by examining polarization through the lens of political science theory. In this literature, there are two dominant theories of why groups become polarized: ideological polarization and affective polarization (Iyengar *et al.*, 2019). Theories of ideological polarization argue that the public is increasingly divided on issues, citing increasing differences in Democrats’

and Republicans' views on defense spending, health insurance, and abortion, among other issues (Abramowitz & Saunders, 2008). Affective polarization argues that polarization is driven by how people see members of the opposite political party (that is, Democrats versus Republicans) (Fiorina, Abrams & Pope, 2005).

Both types of polarization could influence the effect of CEO activism on store performance. In terms of ideological polarization, consumers who disagree with a CEO's activism may want to avoid financially supporting a business that advocates against their interests. For example, hunters might avoid patronizing Walmart because they believe such spending will strengthen the company's advocacy for stronger gun control laws. Consumers who support stricter gun laws may do the opposite.

In terms of affective polarization, consumers may change their behavior based on whether they perceive a CEO's actions as aligning with their preferred political party, regardless of their ideological views about gun policy (Fiorina, Abrams & Pope, 2008). Literature in sociology suggests that group affiliation is essential to our sense of self and that people instinctively think of themselves as representing broad categories rather than as distinctive packages of traits (Tajfel *et al.*, 1979; Brewer, 1991). People categorize those with the same beliefs as the ingroup, which triggers positive evaluations, and those with opposite beliefs as the outgroup, which triggers negative evaluations (Billig & Tajfel, 1973). In the case of CEO activism, consumers who affiliate with a political party may characterize a firm as either the ingroup or the outgroup based on its CEO's political stance. Consumers may then wish to purchase products from perceived co-partisans. The phenomenon of consumers finding social and political meaning in purchases is termed "lifestyle politics," a powerful force that can affect spending behavior, recreational experiences, and fashion decisions (Bennett, 1998; Shah *et al.*, 2016).

Given that most CEO activism issues—such as gun control, abortion, and LGBTQ rights—are politically charged, it is difficult to separate ideological versus affective polarization. The two are not mutually exclusive and theoretically their effects on firm performance have the same sign. We therefore focus not on the specific forces but on their combined effects on firms.

Although the political science literature discusses the potential effects of polarization, (Fiorina, Abrams & Pope, 2008; Snyder, 2019; Panagopoulos *et al.*, 2020), less is known about the potential for asymmetric effects of polarization on different parties. Specifically, it is not known whether proponents and opponents will show a similar level of response toward a firm after its CEO takes an activism stance. One reason to expect asymmetry in responses is that the importance or salience of an issue for proponents and opponents of a CEO's position may differ. For example, Democrats and Republicans frequently disagree about the importance of issues such as climate change, racism, gay rights, abortion, and immigration (Brenan, 2020). Furthermore, certain issues are more likely to have personal stakes for proponents and opponents. With respect to gun control, Republicans are more likely to own guns, oppose gun control, and say that being a gun owner is important to their identity (Parker *et al.*, 2017). Gun owners, especially Republican gun owners, therefore have personal stakes in the resolution of policy issues related to firearms. As a result, gun owners tend to be more politically engaged on the issue of gun control (Goss, 2006); they are nearly twice as likely to contact public officials about guns and nearly three times more likely to donate money to organizations with positions on guns (Parker *et al.*, 2017).

Another, psychological, reason to expect asymmetric responses from proponents and opponents of a CEO's position is that consumers may respond to what for them is positive versus negative information about a firm differently. Asymmetric responses to positive and negative information have been studied most explicitly in the psychology literature. Taylor (1991) presented



evidence that negative events elicit more physiological, affective, cognitive, and behavioral activity, leading to more cognitive analysis than neutral or positive events do. Research also indicates that subjects tend to experience stronger physiological arousal when presented with opinions that contradict rather than support their own (Burdick & Burnes, 1958; Steiner, 1966) and that negative events are stronger determinants of mood than positive events (Vinokur & Selzer, 1975; Taylor, 1991).

Studies also show that positive and negative events and information do not seem to have the same effect on cognitive processing (Kanouse & Hanson, 1972). Peeters and Czapinski (1990) find that negative stimuli lead to more cognitive work and produce more complex cognitive representations than positive stimuli do. This translates to individuals assigning more importance to negative information—that is, social information such as learning that a CEO has taken a stance that opposes one’s own belief—than to positive information (Peeters & Czapinski, 1990; Kahneman & Tversky, 2013). Negative information outweighs positive information in impression formation, person perception, and morality judgments (Kanouse & Hanson, 1972).

It is conceivable, then, that consumers who disagree with a CEO will react more strongly than those who agree. In our setting, we expect consumers who oppose stricter gun control policies to exhibit greater response than those who support stricter gun control.

### **3.2. Duration of CEO activism effects**

Consumer choice is driven by several factors, such as price, convenience, and quality. The weight consumers put on CEO activism versus other factors is unknown. When Walmart’s CEO took a stance on stricter gun control policies, the company’s strategy of offering low prices on a wide selection of goods did not change. This suggests that consumers who chose to avoid or patronize

Walmart due to CEO activism likely incurred costs along other dimensions (e.g., prices, convenience, quality).

In addition, whereas other attributes will remain salient, consumers are likely to forget about any CEO activism absent continuous reminders. Research shows that consumers have limited attention in dealing with frequent activities such as household finances (DellaVigna, 2009; Stango & Zinman, 2014) and are often overloaded with competing information from advertisers (Anderson & de Palma, 2013). Even the effects of persuasive advertising are not permanent; increases in goodwill generated by advertising decay over the following weeks and firms must engage in intermittent advertising, or “pulsing,” to sustain the benefits of their marketing efforts (Dube, Hitsch & Manchanda, 2005; Lopez, Liu & Zhu, 2015).

Economic research on transient, visceral emotions suggests that visceral factors often drive people to behave in ways that they view as contrary to their own self-interest (Loewenstein, 2000). This stream of research suggests that at times—for example, when feeling road rage—people are biologically prone to make certain decisions with low cognitive mediation (LeDoux, 1996). When immediate visceral factors overpower cognitive deliberation, people take actions based on how they feel rather than on the expected consequences. In the case of CEO activism, upon learning of a CEO’s stance, consumers may act in ways that are not in their best economic interest, be that avoiding or supporting a particular company. However, given that visceral emotions are temporary (road rage, for example, fades quickly), consumer behavior towards a specific company should also quickly return to its original state. Additionally, consumers—aware of a past or current visceral emotion’s negative influence—may resist the behavioral impact of future visceral factors (Loewenstein, 2000). Taking such effects together with consumers’ limited attention span, we anticipate that the effects of CEO activism will be short-lived.

## 4. DATA

We combine data from three sources to examine how CEO support for stricter gun control affects firm performance. First, to measure store-level performance, we rely on mobile phone location data from SafeGraph. Second, we identify CEOs who supported gun control legislation in September 2019 by searching several news databases. Third, we match store locations to data on recent presidential elections from the MIT Election Lab to examine whether consumers' responses to CEO activism depend on their political affiliations. We discuss each of our primary data sources in greater detail below and in the Appendix.

### 4.1. Store-level performance

We measure weekly visits to individual stores from 2017 through 2019 using data from SafeGraph, a company that tracks foot traffic to millions of U.S. stores using mobile phone location data.<sup>1</sup> The SafeGraph sample is generally representative of the U.S. population, including on demographic variables such as race, education, and income (Squire, 2019).<sup>2</sup> We discuss the SafeGraph data in detail, including how well it represents the U.S. population and how we calculate store visits, in the Appendix.

CEOs from 146 companies supported gun control in early September 2019, but because our study focuses on consumers and relies on physical store visits to measure performance, we subset our data to include only firms with physical store locations. Thus, our sample consists of four companies whose CEOs supported stricter gun control—Walmart, Dick's Sporting Goods, Levi Strauss, and The Gap—collectively accounting for 5,766 stores.

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<sup>1</sup> Technically, SafeGraph tracks visits to places, which may or may not be stores; a daycare center, for example, is not a store. We refer to places as “stores,” however, because we restrict our analysis to those places that *are* stores.

<sup>2</sup> SafeGraph data exclude people under 13 years of age. This is unlikely to affect our results, given that stores in our sample do not target this specific consumer category and children have limited direct purchasing power.

## 4.2. Corporate activism and the control group

We examine consumer responses to CEO support for stricter gun control policies (see Section 2 for details) by comparing store visits for companies that did and did not engage in activism. To construct a control group using the SafeGraph data, we begin by selecting potential control firms from the universe of branded stores in the same counties and NAICS industries as our focal firms; we call these our “same-industry controls.” Then, as an alternative control group for robustness checks, we use a list of brands that SafeGraph identifies as related to our focal firms in terms of foot traffic; that is, a consumer who patronizes the focal firm is also likely to patronize the related firm. We refer to this control group as our “related-brand controls” and provide more details about its construction in the Appendix. As we explain in Section 6.1.1, using these two control groups to construct counterfactual outcomes for the activist stores<sup>3</sup> is helpful because the likely biases in the estimates from each have opposite signs.

## 4.3. Political affiliation

Republicans generally favor fewer restrictions on the ownership and use of firearms. Furthermore, gun ownership has become more partisan over the past three decades and is now a reliable predictor of voting Republican (Joslyn *et al.*, 2017). In fact, gun ownership in recent years has emerged as a better predictor of party affiliation than gender, sexual orientation, ethnicity, and several other demographic variables (Silver, 2012). Opinion polls by the Pew Research Center that coincide with the time period of this study suggest that gun control is among the most polarizing issues; 76 percent of Republicans—but only 22 percent of Democrats—say it is more important to protect

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<sup>3</sup> It is CEOs, not stores, who engage in activism, but, in this model, the effects of the activism are reflected in store-level performance. For simplicity, we refer to stores that are part of a company whose CEO has engaged in activism as “activist stores.”

gun rights than to control gun ownership (Parker *et al.*, 2017). Therefore, political party affiliation is likely to be a good proxy for consumers' agreement with CEO activism favoring stricter gun control.

We measure political leaning using county-level data from the MIT Election Lab (MIT Election Data and Science Lab, 2018). To measure how conservative a given county's consumers are, we calculate the average of the shares of votes cast for the Republican presidential candidate in each county during the 2008, 2012, and 2016 general elections. As election data are not available for Alaska or U.S. territories,<sup>4</sup> we exclude these locations from all analyses.

#### 4.4. Summary statistics

Table 1 shows summary statistics for our sample and Table 2 reports means and standard deviations separately for the treatment and control stores. The stores in our sample receive an average of 1,746 visits per week and are balanced between Democrat- and Republican-leaning counties. A map of stores included in the sample (Figure 1) shows they are spread across nearly all counties of the United States.

'Insert Table 1 here,'

'Insert Table 2 here,'

'Insert Figure 1 here,'

## 5. METHODOLOGY

We measure the effects of corporate activism on performance using difference-in-differences methods that leverage recent methodological advances to account for potential violations of the

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<sup>4</sup> These territories are American Samoa, Guam, Northern Mariana Islands, Puerto Rico, and the U.S. Virgin Islands.

parallel trends assumption (Bilinski & Hatfield, 2018). Our targets of inference are (a) the average treatment effect for the treated (the effect of activism on the performance of brands whose CEOs take public positions on gun control) immediately following the treatment (CEO activism) and (b) average treatment effects conditional on political affiliation. The starting point for our analyses is the familiar two-way fixed-effects model with additional controls for seasonality:

$$\ln Y_{it} = \beta D_{b(i)t} + \alpha_i + \lambda_{k(i)n(i)t} + \delta_{i_w(t)} + \theta_{i_y(t)} + \epsilon_{it}, \quad (1)$$

where  $Y_{it}$  is the number of visits to store  $i$  in week  $t$  and  $D_{b(i)t}$  is an indicator for whether store  $i$ 's parent brand  $b$  engaged in corporate activism related to gun control in or before week  $t$ .<sup>5</sup> The parameter  $\alpha_i$  is a store fixed effect that captures unobserved store-level attributes (such as square footage and location) that do not vary across years. The second fixed effect,  $\lambda_{k(i)n(i)t}$ , is a county-industry-time-specific parameter that absorbs shocks to foot traffic in period  $t$  for store  $i$ 's county ( $k(i)$ ) and industry ( $n(i)$ ), the latter identified by six-digit NAICS code.<sup>6</sup> This effect accounts for unobserved factors at time  $t$  that equally affect all stores in a given county and industry. The terms  $\delta_{i_w(t)}$  and  $\theta_{i_y(t)}$  are store-level, seasonal effects for each week of the year and for the year, respectively.<sup>7</sup> These fixed effects adjust for the fact that some stores may regularly have higher sales at certain times of the year. For example, we might expect a Walmart to have higher sales than other stores each year around the time students return to school. The store-level fixed effect ( $\alpha_i$ ) captures the fact that Walmart stores have persistently higher foot traffic than other general merchandise stores but does not adjust for regularly occurring *seasonal* differences in the number

<sup>5</sup> The function  $b(i)$  maps store  $i$  to brand  $b$ . Because the CEO activism events occurred midweek, our indicator in the initial activism period equals the fraction of the week that occurred post-activism.

<sup>6</sup> The function  $k(i)$  maps store  $i$  to its county location;  $n(i)$  is defined analogously for industry.

<sup>7</sup> Note that  $\theta_{i_y(t)}$  and  $\alpha_i$  are not separately identified; including the former results in a model that nests the case of store-level effects that are constant across years ( $\alpha_i$ ). We show both here for exposition purposes. For estimation, we present both models that exclude seasonal effects (i.e.,  $\delta = \theta = 0 \forall i$ ) as well as models that allow for seasonality.

of store visits. Our model adjusts both for Walmart’s generally higher foot traffic and for seasonal performance patterns.<sup>8</sup>

Because we are interested in how the effect of activism depends on consumers’ agreement with CEO support for stricter gun control policies, we also estimate versions of Equation (1)—and our other models—that interact CEO activism with continuous or categorical variables representing the average of the shares of voters who voted Republican in the 2008, 2012, and 2016 presidential elections; that is, we replace  $\beta D_{b(i) t}$  in Equation (1) with  $(\beta_1 + \beta_2 R_{k(i)}) D_{b(i) t}$ , where  $R_{k(i)}$  is average Republican vote share in store  $i$ ’s county.

In addition to Equation (1), we estimate “event-study”-style models with dynamic treatment effects that include indicators for each pre- and post-activism period:

$$\ln Y_{it} = \sum_{j=1}^T \beta_j \mathbb{1}(t = j \cap D_{b(i)} = 1) + \alpha_i + \lambda_{k(i)n(i)t} + \delta_{i w(t)} + \theta_{i y(t)} + \epsilon_{it}, \quad (2)$$

where we omit the term for the period immediately preceding activism; that is, we let  $T_0$ , where  $1 < T_0 < T$ , be the period in which CEOs support gun control, so that  $\beta_{T_0-1} = 0$ . The average treatment effect for the treated is then the average of the treatment effects in the individual post-treatment periods:

$$\beta = \frac{1}{T-T_0+1} \sum_{j=T_0}^T \beta_j, \quad (3)$$

which is identified using the performance of stores associated with brands that did not engage in activism as a counterfactual for those that did. In our analysis, we focus on the 10-week period

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<sup>8</sup> As we show in the results section, this seasonal adjustment is crucial in our setting; failing to adjust for seasonality would lead us to conclude that CEO activism has large negative effects on store visits. Taking into account seasonal patterns in performance, however, reveals that store visits regularly fall in early September and that activism on gun control had only modest net effects on store performance.

around the CEO activism and emphasize estimates of the individual  $\beta_j$  coefficients in the immediate post-activism period.

CEO activism is likely to have interesting dynamic effects. Specifically, it may have transitory effects on firm performance, with consumers responding immediately after the event, then reverting to normal behavior. Additionally, we believe our ability to attribute changes in store visits several weeks following the activism to CEO positions on gun control is limited. Stores exposed to negative effects of activism may adjust in unobserved ways—for example, by cutting prices, increasing marketing, or changing product offerings—which could confound our estimates.

### 5.1. Parallel trends assumption

Causal inference in our study design relies on the usual parallel trends assumption. In our setting, this means changes in log store visits for the control group reflect how visits would have changed in the absence of activism for stores whose CEOs took positions on gun control.

Figure 2 shows store visits over time for activist stores and our same-industry control stores, and Figure 3 shows the same trends by political affiliation of stores' counties. The first week of CEO activism (in September 2019) is marked with a black vertical line. In the weeks immediately preceding activism, store visits in both the treatment and control groups typically increase and decrease concurrently, although the activist stores show a slightly steeper upward trend in the previous summer. Following the activism, there is a conspicuous drop in store visits for the activist stores relative to the control group, and the drop is larger in more conservative counties. However, in both figures, a similar pattern is also visible in other years despite no activism occurring during this period.<sup>9</sup> Below, we show in our results (Table 3) that estimates of

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<sup>9</sup> This, too, illustrates the importance of modeling seasonality in our setting. Even controlling for counties and six-digit NAICS industries, store visits exhibit recurring seasonal patterns.



Equations (1) and (2) that omit the seasonal terms attribute the steep decline in store visits in September 2019 to CEO activism even though the decrease is a regular feature of the data.

‘Insert Figure 2 here,’

‘Insert Figure 3 here,’

Despite the similarity in pre-activism trends for the activist and non-activist stores, recent research on difference-in-differences methods cautions against using statistical tests to assess the plausibility of the parallel trends assumption (Bilinski & Hatfield, 2018; Roth, 2020). This work explains that traditional tests of pre-trends are often insufficiently powered to rule out meaningful violations of the parallel trends assumption. And even when they are sufficiently powered, the deviations from parallel pre-trends indicated by the tests may not meaningfully affect inferences about treatment effects of interest (Bilinski & Hatfield, 2018).

We therefore combine visual inspection of the data and estimates of pre-activism  $\beta_j$ 's from Equation (2) with recent formal methods that account for potential violations of the parallel trends assumption. Following Bilinski and Hatfield (2018), we examine how differences in trends between activist and non-activist stores affect our estimates by augmenting Equation (2) with either a linear or cubic spline time trend for activist stores:

$$\text{Ln } Y_{it} = \sum_{j=T_0}^T \beta_j \mathbb{1}(t = j \cap D_{b(i)} = 1) + f(tD_{b(i)}; \phi) + u_{it}, \quad (4)$$

where  $u_{it}$  encompasses the fixed effects and seasonal terms (see above) and  $f(tD_{b(i)}; \phi)$  is a trend difference for activist stores parameterized by  $\phi$ . For the model with a linear trend difference,  $f(tD_{b(i)}; \phi) = \phi t D_{b(i)}$ . For models with a nonlinear trend difference, we use a natural cubic spline with two degrees of freedom and a knot at the midpoint of the pre-activism period.<sup>10</sup>

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<sup>10</sup> Analyses were conducted in R (R Core Team, 2020).

## 6. RESULTS

Table 3 shows results from a series of difference-in-differences models—see Equation (1)—estimating the net effect of CEO activism as well as effects in politically liberal versus conservative counties. The estimates in Column 1 are not adjusted for seasonal patterns in store visits (that is, they omit the  $\delta$  and  $\theta$  coefficients in Equation (1)), while those in Columns 2–6 (our preferred estimates) do control for seasonality. The difference in results illustrates the importance of correcting for seasonal trends in these data. The coefficient on *Post-activism* in Column 1 indicates that CEO support for gun control results in a 9- to 10-percent reduction in store visits over the four weeks following the event, while the estimate in Column 2 (adjusted for seasonality) indicates a more modest three percent decrease, which is equivalent to a reduction of 185 visits per week for the average activist store.

‘Insert Table 3 here,’

Columns 3–4 of Table 3 show that the effect of CEO activism depends on the political affiliation of a store’s consumers. Column 3 interacts the indicator for activism with the average of the shares of votes cast for the Republican presidential candidate in the store’s county during the 2008, 2012, and 2016 presidential elections. As an alternative specification, Column 4 replaces this continuous measure with indicators for four categories of Republican support: *Very liberal* areas are those in which Republicans receive 30 percent or less of the vote, *Liberal* areas 31–50 percent, *Conservative* areas 51–70 percent, and *Very conservative* areas 71–100 percent. Both specifications indicate that consumers in more conservative counties respond more negatively to CEO support for gun control. The point estimates in Column 3 imply that stores in counties where Republicans typically win 25 percent of the vote see a 1.9-percent decrease in visits following CEO support for gun control, but stores in counties where Republicans typically win 75 percent

of the vote experience a 4.6-percent decrease in visits. Similarly, in Column 4, the point estimate for *Very liberal* counties implies that visits do not change while estimates for *Very conservative* counties indicate that visits decrease by five percent during the four weeks following CEO activism. Together, these results suggest that CEO activism has an asymmetric polarization effect on consumers: consumers who disagree with the CEOs' stance on gun control react more strongly to it than other consumers.

To examine the duration of the effects of activism stretching beyond four weeks, we extend the sample in Columns 5 and 6 of Table 3 to cover the 10-week period after CEO support for gun control. The estimates in these columns indicate that activism has essentially no net effect on store visits over this 10-week period, suggesting that any decline in sales immediately following CEO support for gun control was later reversed. Next, we explore the pattern of dynamic effects week by week, but before doing so, note how the magnitude of our estimates in Table 3 compares with the effects reported in related experimental studies. Chatterji and Toffel (2019) examine how statements supportive of same-sex marriage by Tim Cook, the CEO of Apple, affect consumers' intention to purchase Apple products. Mean purchase intent in their study was five percent higher for people exposed to Tim Cook's pro-LGBTQ-rights message versus a generic message regarding his business philosophy. Like us, Chatterji and Toffel find that the effect of activism depends on the audience. In experiments examining how consumers respond to information about corporate political contributions, Panagopoulos *et al.* (2020) find that consumers become "more (less) likely to patronize chains that support (oppose) their [political] party." Pooling the results of several experiments, they report that the share of consumers who plan to "never patronize a [particular] chain store" moves four percentage points in either direction depending on the alignment of that company's political contributions with consumers' own political views.

‘Insert Figure 4 here,’

The estimates in Table 3 reflect the average effect of CEO activism in the month following the event. In order to investigate the dynamic effects of activism, we estimate the model in Equation (2), which includes indicators for each week pre- and post-activism (omitting the period immediately prior to activism). Figure 4 plots the individual coefficients for the net effect of CEO activism as well as effects by political affiliation (using the same categories as Table 3). We first focus on interpretation of the post-activism coefficients, then on the pre-period coefficients and parallel trends assumption.

Figure 4(a) shows that store visits decrease about two percent in the weeks immediately following CEO support for gun control, but quickly recover. Figure 4(b) shows effects by political party and, like Table 3, suggests that store visits decrease more in counties with more Republican voters. There is a sharp change in the pattern of store visits across liberal and conservative counties following activism: they perform similarly pre-activism, but diverge immediately afterwards. Very conservative counties—those in which more than 70 percent of voters typically support the Republican candidate for president—see weekly store visits fall five percent following CEO activism. The estimates in Figure 4(b), however, also indicate that the number of store visits was *greater* in more conservative counties five weeks following activism. One possible interpretation is that consumers who disagree with a CEO postpone store visits immediately following activism, but soon return to make up for missed trips. The results, however, are consistent with several explanations and should be interpreted cautiously because changes in store visits—especially several weeks following activism—may reflect unobservable actions taken by store managers rather than a direct, dynamic effect of CEO activism. For example, declining store visits may lead stores to lower their prices, increase marketing, or take other actions that increase sales. We further

discuss the implications of these patterns in Section 7 and note that research on CEO activism has not adequately measured its longer-term and dynamic effects on consumption, which is crucial for analyzing its full potential for product-market differentiation.

The coefficient estimates in the pre-activism period (Figure 4) indicate possible violations of the parallel trends assumption (see Section 5.1 for discussion of this assumption), which might affect inferences about the effects of activism. The pre-period coefficients in Figure 4(a) are close to zero, but show a downward trend in the weeks immediately preceding activism. The coefficients in Figure 4(b) likewise suggest that visits to activist stores were higher, but declining, relative to other stores prior to activism.

Recent work on difference-in-differences, however, cautions *against* using pre-period coefficients like those in Figure 4 to assess the plausibility of the parallel trends assumption (Bilinski & Hatfield, 2018; Roth, 2020). First, estimates of single-period effects may be insufficiently powered to detect important violations of the parallel trends assumption (Bilinski & Hatfield, 2018; Roth, 2020). Second, even when traditional hypothesis tests reject that the pre-period effects are zero, the deviations may have little practical significance for inferences about the effects of interest (Bilinski & Hatfield, 2018). Furthermore, Roth (2020) shows that conditioning publication of and inferences about treatment effects on “passing” a test of pre-trends can *exacerbate* bias. Instead of testing pre-trends or examining the statistical significance of pre-period coefficients, researchers can quantify how violations of the parallel trends assumption affect inference and the sensitivity of the results to plausible violations. To do so, we implement the method of Bilinski and Hatfield (2018)—see Equation (4)—to examine the sensitivity of our estimates to potential linear and nonlinear violations of the parallel trends assumption.

‘Insert Figure 5 here,’

Figure 5 shows how the post-activism estimates in Figure 4(b) change when the model includes a differential (linear or cubic spline) trend between activist and non-activist stores. Coefficient estimates including either a linear (shown in dark blue) or cubic (shown in green) trend are generally larger than those from a model that assumes parallel trends (shown in orange). Estimates including these differential trends, however, are overall similar in magnitude to those of the model without a differential trend and likewise show larger effects of activism in very conservative counties than in very liberal ones. Note that estimates from the model including a cubic spline trend difference are essentially indistinguishable from estimates assuming a linear trend difference between activist and non-activist stores, which suggests that the trend difference between the two groups is linear. There is evidence under all models that CEO activism polarizes consumers immediately following the event. The differences in activism effects between very liberal and very conservative counties in both the first and second week post-activism are between 4.5 and 5.2 percentage points and statistically different from zero in all models. Figure 6 shows estimates of the average treatment effect conditional on political affiliation over the four weeks following activism—see Equation (3). Again, all models suggest that stores in liberal counties experience either no change or slightly positive effects from activism while stores in conservative counties experience decreases in store visits.

## **6.1. Robustness checks**

We examine the robustness and sensitivity of our results to the composition of our control group, the measure of political affiliation, and the exclusion of individual states.

### **6.1.1. Alternative control group**

Our estimates in Table 3 and Figures Figure 4–Figure 6 rely on a sample of same-industry control stores and may therefore be biased due to substitution effects. For example, using Target stores to

construct counterfactual outcomes for Walmart stores will bias our estimates (they will be too large in magnitude) if consumers respond to CEO activism by switching from Walmart to Target or vice versa. In other words, the same-industry controls may themselves be affected by a competitor's CEO activism.

Therefore, as an alternative control group, we use a list of brands that SafeGraph identifies as related to our focal firms—in terms of foot traffic—by examining the tendency of consumers to patronize both locations. For example, if people who shop at a downtown Walmart also visit a nearby Shell gas station more frequently than do other shoppers, SafeGraph will label the Shell brand as related to that specific Walmart. We provide more specifics about this procedure, including the precise formulas used, in the Appendix. We refer to this control group as our “related-brand controls.”

Fortunately, the expected bias when using the related-brand controls has a sign opposite to that of the same-industry controls. The related-brand controls avoid the substitute problem, but potentially suffer from a complements problem due to consumers purchasing their products jointly with those of the activist firm. For example, a Shell gas station may rely on consumers from a nearby Walmart.<sup>11</sup> If those consumers stay away from Walmart, the Shell station will lose revenue. In this case, our estimates will be biased in the other direction—they will be too small in magnitude—because a decrease (increase) in visits to the activist stores will be matched by a decrease (increase) in visits to the related-brand control stores. Having two control groups that are likely to bias the estimates in opposite directions allows us to examine whether the above biases meaningfully effect our estimates.

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<sup>11</sup> “Anchor stores” in malls are another example of this phenomenon (Konishi & Sandfort, 2003). Large retailers attract consumers to a shopping center, which benefits smaller, proximate retailers. Any harm to the anchor has negative spillover effects on smaller businesses nearby.

Because related brands are typically not in the same industry as the activist stores, we adapt the industry-county-time fixed effects in Equation (1) to control for groupings of stores and their related brands:

$$\ln Y_{it} = \beta \mathbb{1}(t > T_0 \cap D_{b(i)} = 1) + \alpha_i + \lambda_{g(i)t} + \delta_{i w(t)} + \theta_{i y(t)} + \epsilon_{it}, \quad (5)$$

where  $\lambda_{g(i)t}$  is now a fixed effect for a store and its related-brand stores in each period. For example, a Walmart and its related Shell station would be grouped—that is, would have identical values of  $g(i)$ —to control for common shocks in each period. The effect of activism ( $\beta$ ) is then identified from deviations in visits to Walmart versus the Shell in the post-activism period.

Figure 7 shows trends in store visits for the activist and related-brand controls by political affiliation of the store’s county. Like Figure 3 (see Section 5.1), it supports the parallel trends assumption, suggesting that both the same-industry and the related-brand control groups provide plausible counterfactuals for stores whose CEOs supported gun control.

‘Insert Figure 6 here,’

‘Insert Table 4 here,’

Table 4 shows that results using the related-brand control group are similar to those using the same-industry controls (Table 3). The estimates in Column 1 (corresponding to Column 2 of Table 3) suggest that weekly visits to activist stores decrease three to four percent over the month following activism. Likewise, Columns 2–3 of Table 4 show that the effect of CEO activism on a store depends on the political affiliation of its customers. The more conservative a county’s voters, the greater the decrease in visits to stores whose CEOs support gun control; stores in the most conservative counties experience about a four percent decrease in visits while stores in the most liberal counties see decreases of about two percent.



### 6.1.2. Alternative political ideology measures

Our main results demonstrating consumer polarization rely on the average of the Republican vote shares in the 2008, 2012, and 2016 general presidential elections. As two alternative measures, we use Republican vote share in only the 2016 election—the most recent election preceding the activism—and county-level estimates of policy preferences from the American Ideology Project (Tausanovitch & Warshaw, 2013).<sup>12</sup> The latter measure pools data from several national surveys of policy preferences to create a continuous measure of ideology along the “left-right” political spectrum for each county’s mean citizen.

‘Insert Table 5 here,’

Table 5 re-creates the estimates from Columns 3–4 of Table 3 using the two alternative measures in place of the average Republican vote share variables. Like the main estimates, the estimates using the alternative measures suggest that CEO activism supporting gun control had a small, negative net effect on sales, with the largest effects in more politically conservative counties. Estimates based on Republican vote share in the 2016 presidential election (Columns 1–2) are nearly identical to those in Table 3 using the average of Republican vote shares across three elections. Estimates using the ideology measure (Columns 3–4) are generally smaller in magnitude but the most liberal areas continue to show no effect of activism while conservative areas see decreases in store visits of four percent (versus five percent using measures based on Republican vote share). Regardless of the measure we use, there is a statistically significant difference between the effects in the most liberal and most conservative areas.

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<sup>12</sup> We use the 2016 release of the county-level estimates.

### 6.1.3. Omitting individual states

We confirm that our results do not depend on any individual state by removing stores located in each state and re-estimating the models presented in Columns 2 and 4 of Table 3. The results are presented in Figure 7, which shows that no single state drives estimates of the net effects. Similarly, estimates for the effects by county political affiliation are mostly stable across subsamples omitting each state and similar to estimates relying on the full sample of stores with the exception of models that omit California, which produce smaller coefficient estimates for the effects of activism in very liberal counties. California alone accounts for 30 percent of our observations in very liberal counties.

‘Insert Figure 7 here,’

## 7. DISCUSSION AND CONCLUSION

We evaluate the effect of CEO activism on store-level performance and consumer polarization using the decision of several CEOs to call for stricter gun control following two mass shootings in 2019. Those CEOs’ responses, which were widely covered in the popular press, provide an opportunity to examine the effect of CEO activism on store-level performance in a large, observational dataset. We examine three aspects of the relationship between CEO activism and performance: (a) the net effect of activism on sales, (b) potential asymmetry in the effects on consumers who agree versus disagree with the CEO, and (c) the persistence of the effects over time.

We find that CEO activism supporting stronger gun control resulted in a temporary and modest net decrease in store visits. The effects on supporters versus opponents of the CEOs’ stance differ and are asymmetric; in aggregate, the behavior of consumers who agree with the CEOs does

not change while consumers who disagree with the CEOs reduce visits to that company's stores. Specifically, stores serving conservative consumers—who are likely to disagree with the CEOs' position—experience a four to five percent decrease in visits.

These performance effects of CEO activism dissipate quickly. Net store visits decline three percent over the four weeks following activism, but quickly recover. We find no evidence of a persistent net effect or polarization effect on store visits over the 10-week post-activism period.

Our results are among the first, non-experimental measures of how CEO activism affects firm performance and of the relationship between consumer ideology and the response to activism. Affective polarization has greatly increased in the United States over the past several decades (Iyengar *et al.*, 2019; Boxell, Gentzkow & Shapiro, 2020) and consumers expect CEOs to speak out on controversial issues (Larcker *et al.*, 2018). Our study can inform managers' judgments about the likely costs and benefits of activism on controversial issues unrelated to their business.

Our study does, however, have limitations. One limitation of our study design, as of most difference-in-differences studies, is the difficulty of reliably estimating longer-term dynamic treatment effects. The persistence of consumer responses to activism is a key issue for strategy. CEO activism that has long-lived effects could be used to intentionally polarize a firm's consumers and thus differentiate its products ideologically in ways that would be hard for competitors to imitate. Our results suggest that one-off activism polarizes consumers only temporarily, with the caveat that our estimates of treatment effects several weeks removed from the activism may be biased by other events. Future research is therefore needed to establish how CEO activism affects consumers over the long term and whether would-be CEO activists must continuously engage in activism if it is to affect firm performance. Another possible limitation of our study is the assumption that foot traffic is a good proxy for sales. Although market research (Perdikaki,

Kesavan & Swaminathan, 2012) supports this assumption, our estimates would be biased if CEO activism resulted in fewer net visits, but much greater spending per visit. Mobile phone location data are often used by investors such as hedge funds to measure performance and by companies themselves for attributing sales to marketing efforts, which speaks to the reliability of store visits as a proxy for sales. We believe that the mobile phone location data used in this study are especially promising for research on CEO activism and other phenomena likely to have heterogenous treatment effects across a company's locations.

When discussing differentiation and low-cost strategies, the strategy literature has mainly focused on the market setting. More recently, there is interest in how firms can differentiate in non-market settings (Flammer, 2015), partly driven by the increasing difficulty of building and sustaining a unique market position (Oberholzer-Gee & Yao, 2018). Our study, by showing both net effects and polarization effects of CEO activism, highlights a potential pathway for firms to differentiate beyond the market. Such differentiation can uniquely position a firm among competitors by signaling its social values to stakeholders, who may then become more willing to purchase the firm's products or supply it with inputs. For such a strategy to succeed, this willingness must be persistent and the firm's non-market position must be difficult to imitate. As noted above, our results suggest that CEOs may need to engage in more than one-off activism to permanently change consumer behavior. One possibility in our context, however, is that the temporary, negative effect among consumers who oppose gun control was a "price" the CEOs paid for activism intended to benefit their employees or investors. Our data only allow us to examine the effect of activism on consumers, but future research should consider ways to measure several stakeholders' responses to activism events.

Future research should also address the feasibility of competitors imitating activist CEOs. Superficially, it seems trivial for any CEO to speak out on a controversial issue. We note several (non-exhaustive) reasons, however, that this may not be the case. First, the expected costs and benefits of speaking out may depend on complementary assets, such as public relations capabilities or reputational resources that are difficult to imitate. Second, CEOs may find that taking a stance on an issue is inconsistent with other elements of the firm's strategy. For example, in the summer of 2020, several organizations ostensibly supporting Black Lives Matter were criticized for not taking concrete actions to oppose racism (Chintagunta, Kansal & Pachigolla, 2020; Jan *et al.*, 2020). Third, there may be first-mover advantages to activism. The media, for example, widely acknowledged Merck CEO Ken Frazier as the first CEO to resign from Donald Trump's business advisory council following the President's remarks regarding a protest by white nationalists in Charlottesville, VA. But while speaking out first may take particular courage, whether CEOs and companies are rewarded for this is an open question.

CEOs increasingly face the difficult task of navigating contentious social issues such as gun control, abortion, LGBTQ rights, and police use-of-force regardless of whether the issue has anything to do with the company's products or services. The positions executives take can affect nearly all stakeholders, including consumers, employees, and investors. Our results indicate that CEO support for gun control following two mass shootings had small net effects on sales, but polarized consumers. Store visits declined in conservative counties, where consumers are least likely to agree with the CEOs' position, but were largely unchanged among consumers likely to support gun control. Even these effects, moreover, were temporary, suggesting that CEO activism on gun control may have had little long-term effect on firm performance even in politically conservative markets.

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## APPENDIX

### SAFEGRAPH MOBILITY DATA

This appendix discusses details of the SafeGraph data that we use to measure foot traffic in stores. We describe the origin and construction of the data, how well it represents the U.S. population, and how we normalize it to account for changes in the sample over time.

#### 1. Overview

SafeGraph measures foot traffic by tracking visits to more than 3 million commercial locations, using a sample of more than 45 million mobile devices in the United States (Squire, 2019).

#### 2. Sampling bias

SafeGraph assesses sampling bias in its data by comparing the residential location of the mobile devices in its data with information from the U.S. Census (Squire, 2019). Overall, the sampling bias is small.

As of October 2019, the devices tracked by SafeGraph are geographically representative of the U.S. population at the county level. The correlation between the number of mobile devices residing within a county in the SafeGraph dataset and that county's population according to the U.S. Census is 0.97. The sample is less representative at the level of Census block group; the correlation coefficient is 0.18 (Squire, 2019). The sample is slightly over-indexed on Black and educated consumers and on both rich and poor individuals and is under-representative of middle-income individuals (Squire, 2019).

### 3. Normalization

The sample of mobile devices tracked by SafeGraph has grown over time and longitudinal analyses must account for this. The basic problem is that the number of visits recorded for each store reflects both changes in actual foot traffic and changes in the sample of mobile devices tracked by SafeGraph. For example, measured store visits tend to increase over time in part because the number of mobile devices in the sample is increasing. To further complicate matters, the sample size does not change at the same rate in all locations.

To account for this, we follow SafeGraph’s recommended practices for normalizing the data (Squire, 2020). To approximate actual store visits, we use SafeGraph’s breakdown of visits and sample size by Census block group of customers’ homes to identify each store’s trade area—that is, the geographic area from which the store draws customers. We define each store’s trade area as the set of Census block groups from which at least five unique customers visited the store in a given month during 2019.<sup>13</sup> For each trade area, we count the number of devices in the SafeGraph sample and in the U.S. population—the latter measured by the Federal Communication Commission (2020).<sup>14</sup> We then calculate our estimate of true store visits from observed visits as:

$$\text{True Visits}_{it} = \frac{\text{Sample Visits}_{it}}{\text{Sample Visitors}_{it}} \times \text{Sample Visitors}_{it} \times \frac{\text{Population}_{iy(t)}}{\text{Devices}_{it}},$$

where  $\text{Sample Visits}_{it}$  and  $\text{Sample Visitors}_{it}$  are the count of visits and visitors, respectively, to store  $i$  in week  $t$  observed in SafeGraph’s sample of mobile devices;  $\text{Devices}_{it}$  is the number of mobile devices tracked by SafeGraph in store  $i$ ’s trade area; and  $\text{Population}_{iy(t)}$  is the

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<sup>13</sup> We use the five-visitor threshold because SafeGraph censors visitor counts less than five for privacy reasons. Furthermore, block groups that never produce five visitors in a single month seem unlikely to be an important part of the store’s customer base.

<sup>14</sup> We use population estimates from the FCC because the agency provides annual, block-level population numbers for our entire sample period (2017–2019) that are easily aggregated to the block-group level. Other approaches, such as relying on the 2016 American Community Survey estimates, produce similar results.

population of store  $i$ 's trade area in year  $y(t)$ . The term  $Sample\ Visitor_{it} \times \frac{Population_{iy(t)}}{Devices_{it}}$  scales the number of observed visitors to reflect the share of the population covered by the SafeGraph sample. For example, SafeGraph may observe 500 devices in a geographic area with a population of 10,000. In that case, each of these devices represents 20 people.

#### 4. Related brands

In several analyses, we rely on a control group of stores constructed from a list of related brands provided by SafeGraph. Here we explain how SafeGraph defines related brands and our process for assembling a sample of stores related to stores that engaged in activism, which we refer to as the “related-brand controls” and use for robustness checks in Section 6.1.1.

For each store in its sample, SafeGraph identifies other brands that consumers of that store frequently patronize. To identify these brands, SafeGraph calculates the share of focal store  $i$ 's consumers in a given month that patronize each other brand  $b$  and subtracts the overall tendency of people in the sample to patronize brand  $b$ :

$$S_{ibt} = \frac{|V_{it} \cap V_{bt}|}{|V_{it}|} - \frac{|V_{bt}|}{N_t},$$

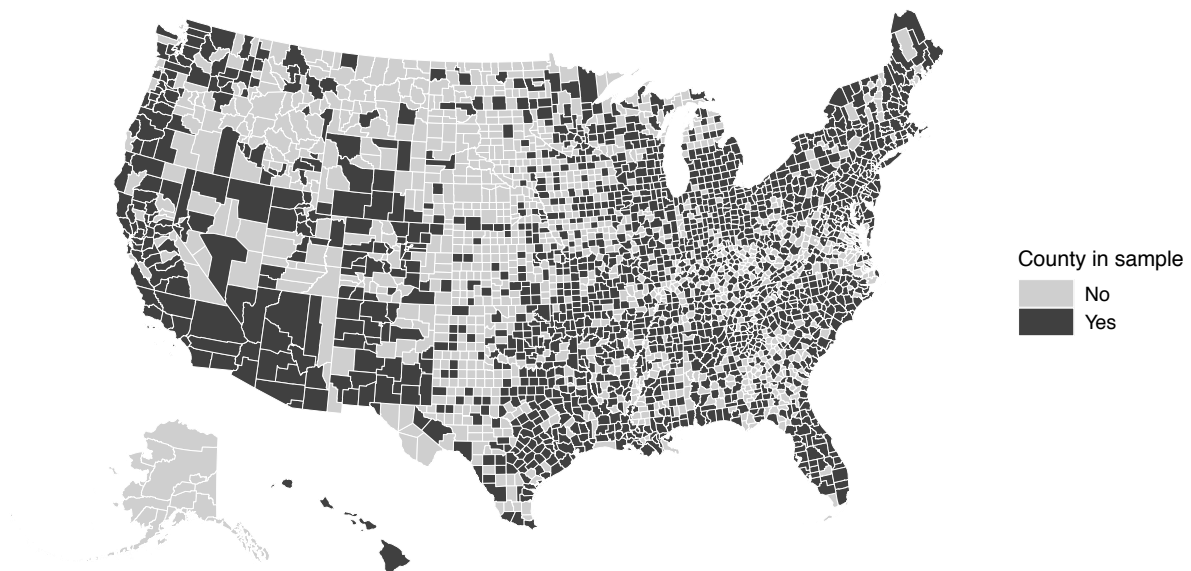
where  $S_{ibt}$  is the similarity between store  $i$  and brand  $b$  in month  $t$ ;  $V_{it}$  is the set of consumers who visit  $i$  in month  $t$ ; and  $N_t$  is the number of consumers in the sample. SafeGraph classifies a brand as related to store  $i$  if  $S_{ibt} > 5$ .

Note that this measure varies across periods and creates a correspondence between individual stores and related *brands*, which may themselves have multiple stores. For example, McDonald's could be a related brand for one specific Walmart, but not another (the set of brands related to any specific store will depend on other stores in its proximity). To determine which

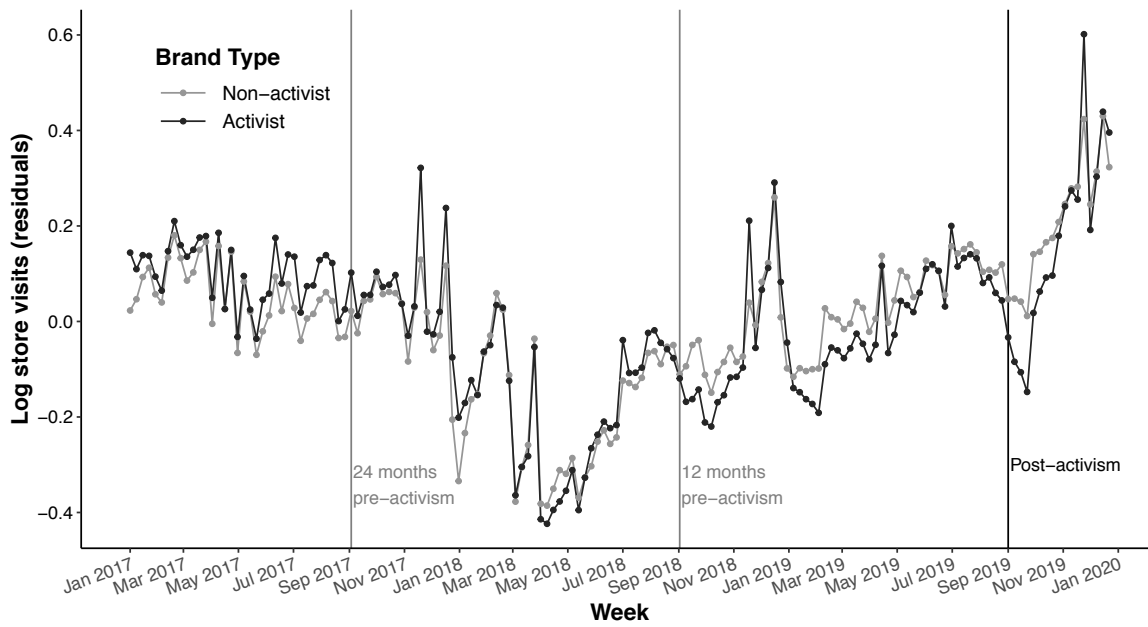
specific McDonald's locations are related to a given Walmart, we take all stores associated with those brands that SafeGraph determines are related to activist stores for at least 12 months of our sample period. We then identify the store belonging to these related brands that is geographically closest to the activist store and select as controls all stores whose distance to the activist is within one mile of the closest distance.

Figure 8 illustrates this process for a single Walmart (marked in red). This Walmart has several related brands and they have dozens of stores (marked in blue and green). When selecting the related-brands control group for this Walmart, we calculate the distance to the closest related store and select any stores less than that distance plus one mile from the Walmart (those marked in blue). Note that several other stores also belonging to related brands are not selected as controls (marked in green) because they are too far from the focal Walmart store.

FIGURES

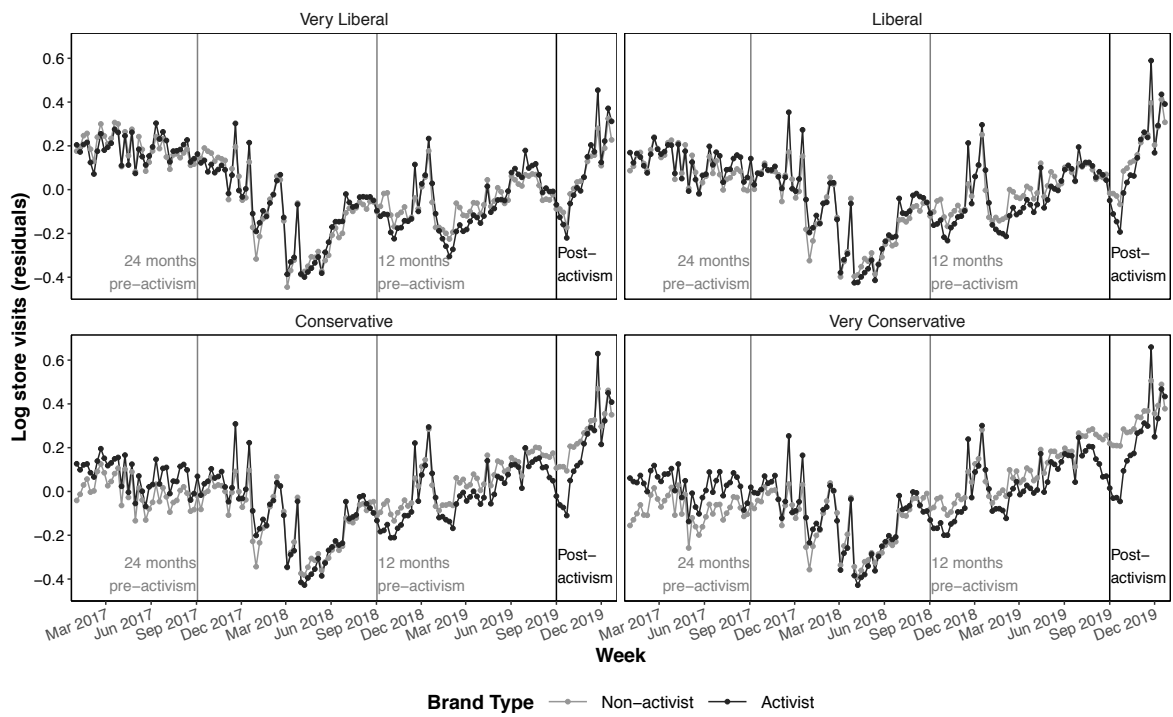


**Figure 1.** Map of counties with both activist and same-industry control stores



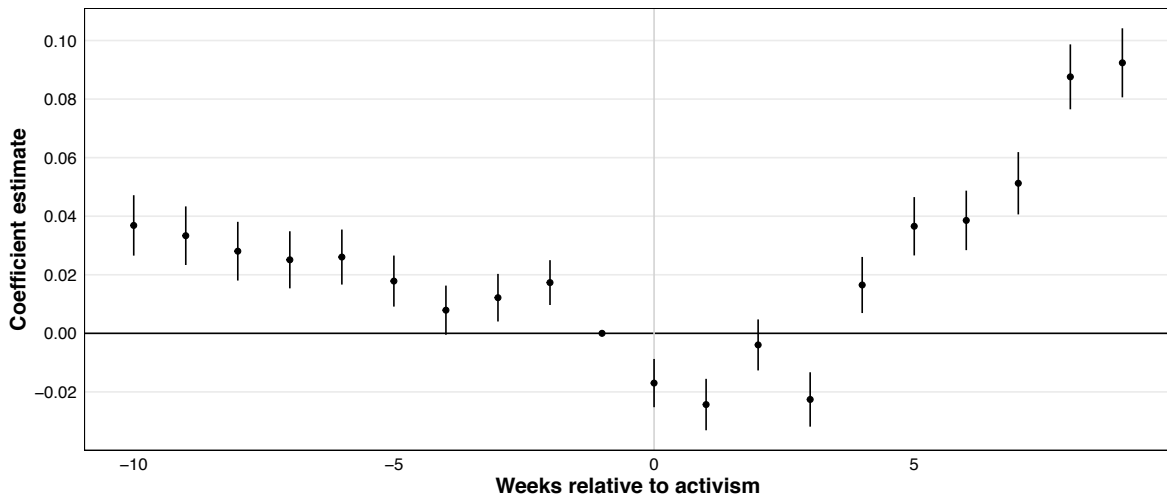
Note: Vertical lines are relative to CEO activism at Walmart, which occurred one week before the activism of the other sample stores. The plotted series represent mean log visits after residualizing with respect to store fixed effects.

**Figure 2.** Trends in store visits for activist and same-industry control stores

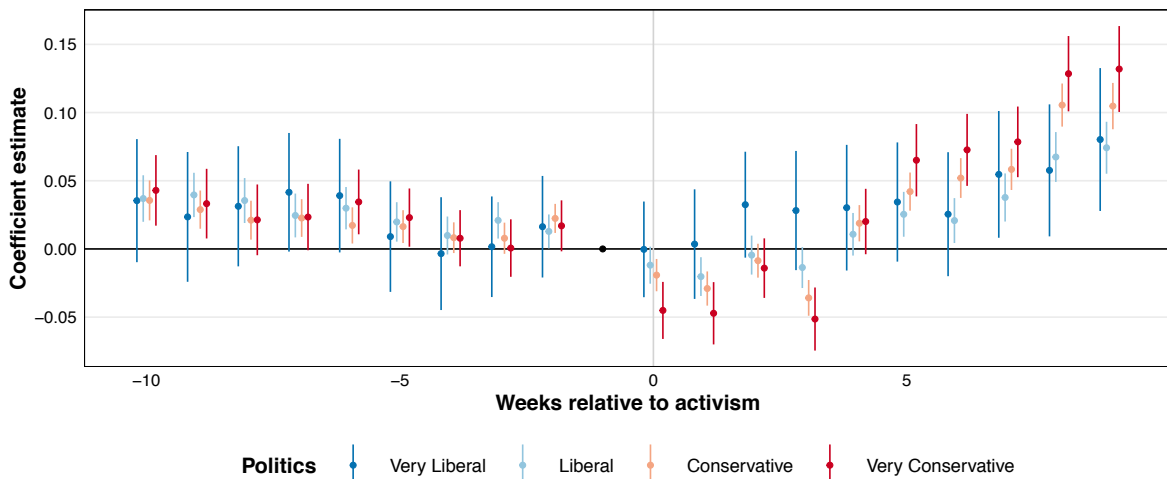


Note: See notes to Figure 2. Political categories are defined as in Table 3.

**Figure 3.** Trends in store visits by political affiliation of store location



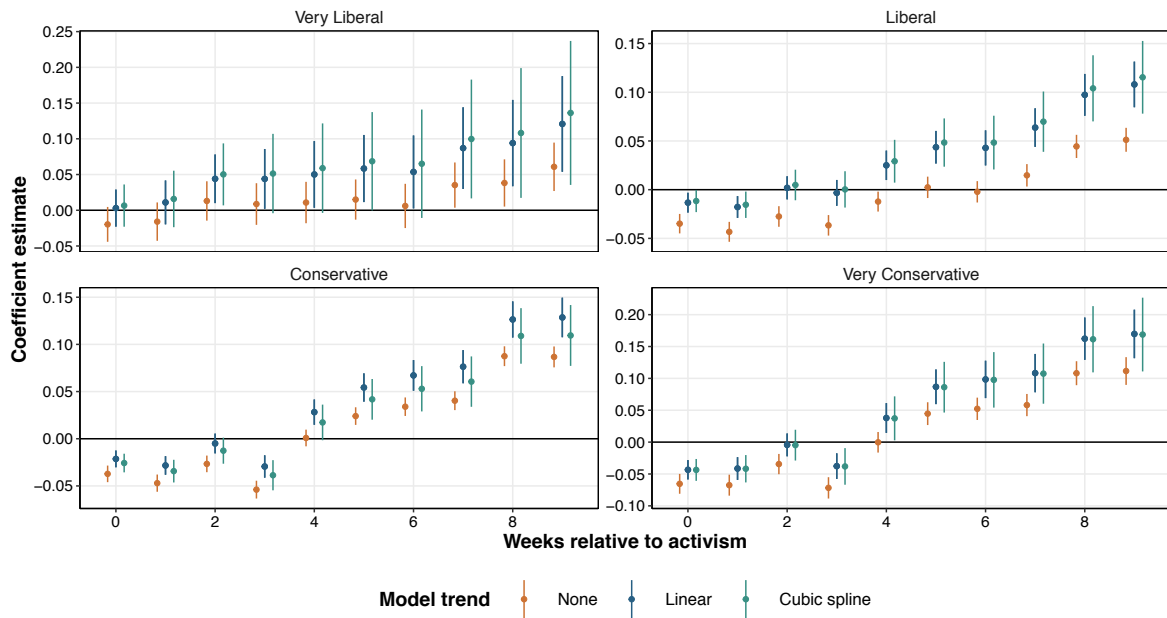
(a) Pooled analysis



(b) Effects by county political affiliation

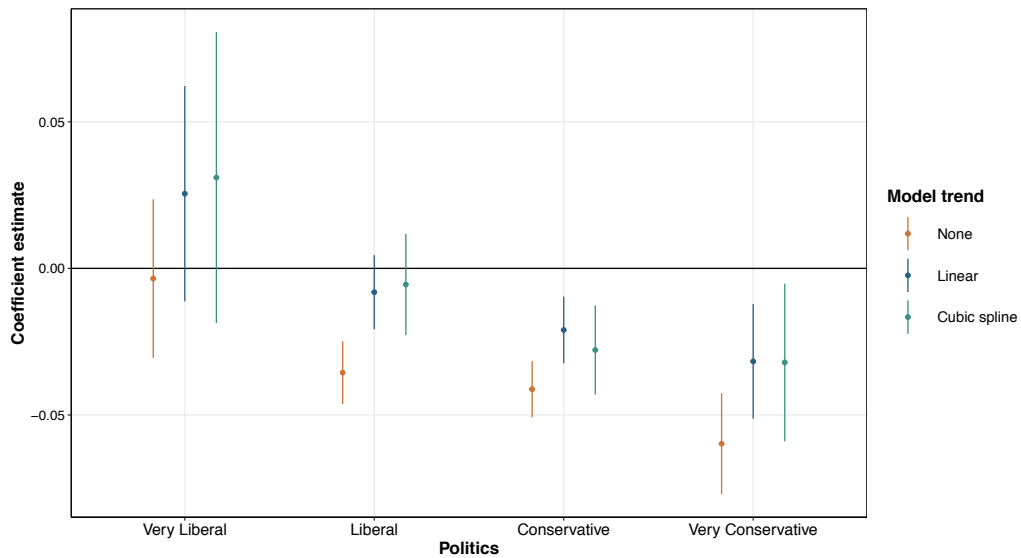
Figure 4. Effects of CEO activism by week





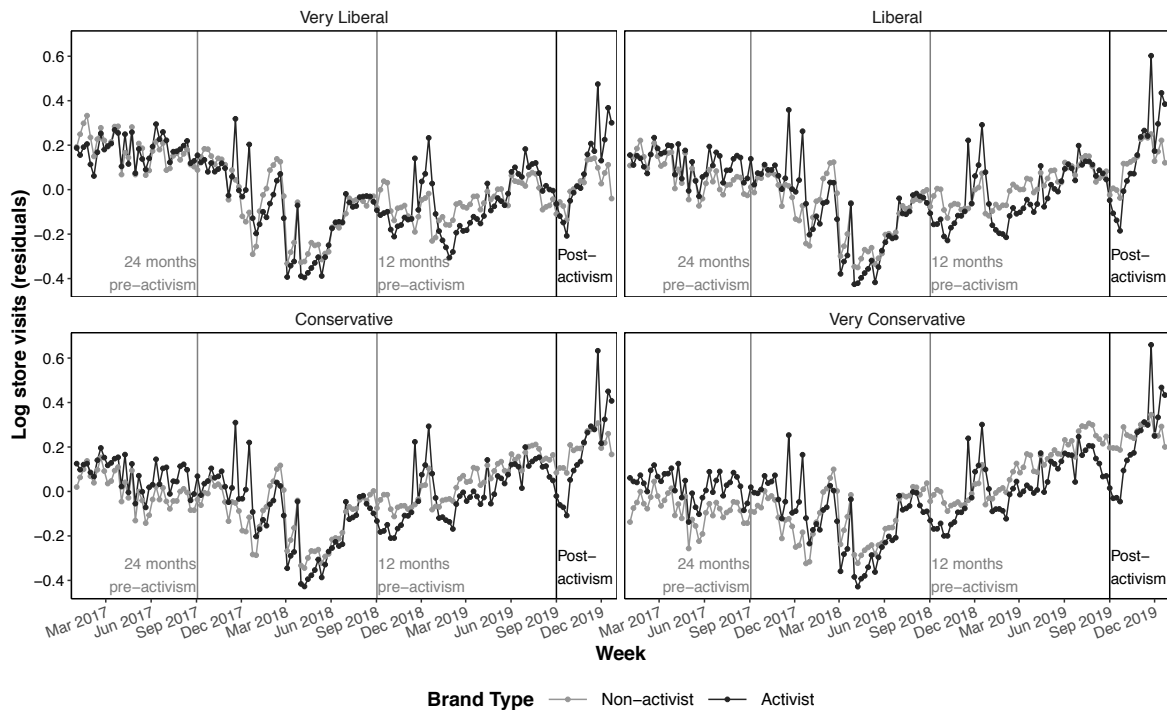
Note: Political categories are defined as in Table 3.

**Figure 5.** Effect of deviations from parallel trends on estimates



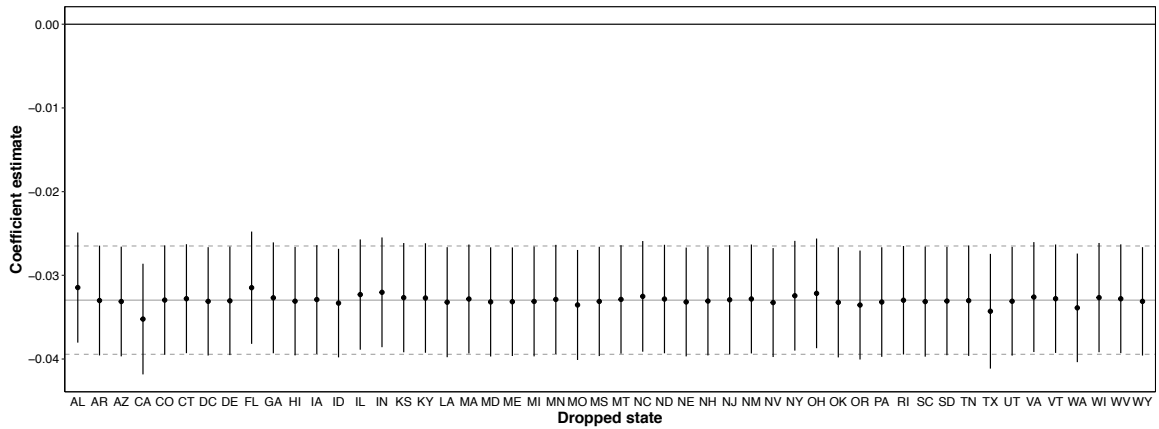
Note: Political categories are defined as in Table 3. Average treatment effects are estimated over the four weeks following activism.

**Figure 6.** Average treatment effects conditional on political affiliation

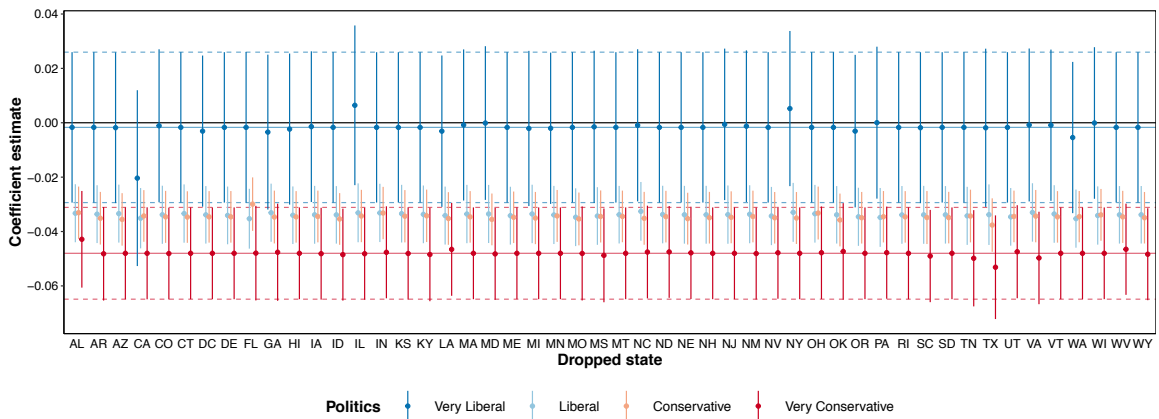


Note: See notes to Figure 2. Political categories are defined as in Table 3.

**Figure 7.** Trends in store visits for related-brand controls by political affiliation of store location



(a) Net effect of activism, dropping individual states



(b) Effects of activism by political affiliation, dropping individual states

Note: Horizontal lines show coefficient estimates using the full sample—those from Table 3—and dashed lines represent 95-percent confidence intervals. Lines for the *Liberal* and *Conservative* categories omitted from subfigure (b) to minimize clutter. Political categories are defined as in Table 3.

**Figure 7.** Trends in store visits for related-brand controls by political affiliation of store location



Note: Points represent a Walmart and several stores belonging to related brands in Falmouth, ME. Related stores in blue are those within 1 mile of the closest related store, while those in green are more than 1 mile away. In this case, our related-brand controls are the stores in blue. Illustration uses background map tiles from Stamen Design (<https://stamen.com>).

**Figure 8.** Approach for identifying related-store controls

## TABLES

**Table 1.** Full-sample summary statistics

Variable	Mean	Std. dev.	Percentile				
			10th	25th	50th	75th	90th
Activist	0.16	0.37	0	0	0	0	1
Store visits	1,746	2,960	199	372	664	1,311	5,372
Republican vote	50%	15	30	40	49	61	70

Note: Observations are store-weeks. The variable *Activist* is an indicator for stores whose CEOs took public positions on gun control. *Store visits* is weekly scaled store visits as defined in Appendix A. *Republican vote* is the average of the percentage of total votes cast for the Republican presidential candidate in the 2008, 2012, and 2016 general elections within the store's county.

**Table 2.** Summary statistics for activist and non-activist (control) stores

Variable	Activist stores (N = 422,760)		Same-industry controls (N = 2,166,900)		Related-brand controls (N = 6,434,760)	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Store visits	6,160	4,531	886	1,389	1,463	5,316
Republican vote	50%	14	50	15	48	16

Note: Observations are store-weeks. See notes to Table 1 for variable definitions.

**Table 3.** Difference-in-differences estimates

	Weeks [-10, 4]				Weeks [-10, 10]	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-activism	-0.097 (0.002)	-0.033 (0.003)	-0.006 (0.013)		0.005 (0.003)	-0.017 (0.013)
Post-activism × Republican vote			-0.001 (0.000)			0.000 (0.000)
Very Liberal				-0.002 (0.014)		
Liberal				-0.034 (0.005)		
Conservative				-0.035 (0.005)		
Very Conservative				-0.048 (0.009)		
Seasonality controls		●	●	●	●	●
Observations	647,265	1,941,795	1,941,795	1,941,795	1,941,795	1,941,795
Adjusted-R <sup>2</sup>	0.97	0.96	0.96	0.96	0.96	0.96

Note: All models include store and week-industry-county fixed effects. Models 1–4 cover the 10 weeks before and 4 weeks after the activism and Models 5–6 cover the 10 weeks before and 10 weeks after the activism. Model 1 uses data from 2019, while Models 2–6 additionally use data from 2017 and 2018 to control for week-of-year seasonality. *Post-activism* is an indicator for store-weeks after the CEO supported gun control. *Republican vote* is the average of the percentages of total votes cast for the Republican presidential candidate in the 2008, 2012, and 2016 general elections within the store’s county. *Very Liberal*, *Liberal*, *Conservative*, and *Very Conservative* are categorical variables based on Republican vote; the cutoffs for each category are [0, 30], (30, 50], (50, 70], and (70, 100], respectively. Standard errors in parentheses are clustered by store.

**Table 4.** Difference-in-differences estimates using related-brand control group

	Weeks [-10, 4]			Weeks [-10, 10]	
	(1)	(2)	(3)	(4)	(5)
Post-activism	-0.038 (0.003)	-0.029 (0.010)		0.006 (0.003)	-0.023 (0.010)
Post-activism × Republican vote		-0.000 (0.000)			0.001 (0.000)
Very Liberal			-0.021 (0.012)		
Liberal			-0.040 (0.005)		
Conservative			-0.038 (0.004)		
Very Conservative			-0.041 (0.007)		
Seasonality controls	●	●	●	●	●
Observations	5,136,030	5,136,030	5,136,030	6,848,040	6,848,040
Adjusted-R <sup>2</sup>	0.96	0.96	0.96	0.96	0.96

Note: All models include store and week-store-pair fixed effects. Models 1–3 cover the 10 weeks before and 4 weeks after the activism and Models 4–5 cover the 10 weeks before and 10 weeks after the activism. See note to Table 3 for variable definitions. Standard errors in parentheses are clustered by store.

**Table 5.** Alternative measures of political ideology

	2016 presidential election		American Ideology Project	
	(1)	(2)	(3)	(4)
Post-activism	-0.004 (0.012)		-0.032 (0.004)	
Post-activism × Republican vote	-0.001 (0.000)			
Conservative ideology			-0.020 (0.012)	
Very Liberal		-0.010 (0.013)		-0.008 (0.013)
Liberal		-0.032 (0.005)		-0.032 (0.006)
Conservative		-0.033 (0.005)		-0.037 (0.005)
Very Conservative		-0.054 (0.007)		-0.039 (0.006)
Seasonality controls	●	●	●	●
Observations	1,941,795	1,941,795	1,939,140	1,939,140
Adjusted-R <sup>2</sup>	0.96	0.96	0.96	0.96

Note: All models include store and week-industry-county fixed effects. *Republican vote* is the percentage of total votes cast for the Republican presidential candidate in the 2016 general presidential election within the store's county. *Conservative ideology* is the "left-right" measure of county-level political ideology from the American Ideology Project; higher values correspond to a more conservative (i.e., politically "right") ideology. *Very Liberal*, *Liberal*, *Conservative*, and *Very Conservative* are categorical variables based on *Republican vote* for Models 1–2 and based on *Conservative ideology* for Models 3–4. For *Republican vote*, the cutoffs for each category are [0, 30], (30, 50], (50, 70], and (70, 100], respectively. For *Conservative ideology*, the cutoffs for each category are  $(-\infty, -0.3]$ ,  $(-0.3, 0]$ ,  $(0, 0.3]$ , and  $(0.3, \infty)$ , respectively. Observations differ between Models 1–2 and 3–4 due to missing data. Standard errors in parentheses are clustered by store.