

# The Effects of CEO Activism: Partisan Consumer Behavior and Its Duration

Young Hou<sup>1</sup> and Christopher W. Poliquin<sup>2</sup>

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## Abstract

CEOs increasingly engage in activism on controversial social and political issues that do not directly affect their core businesses. Simultaneously, the general public is increasingly politically polarized. We examine how CEO support for gun control after two mass shootings differentially affected the behavior of liberal and conservative consumers and the persistence of these effects. Using mobile phone location data to measure store-level visits, we find (a) consumer visits to stores decreased by three percent; (b) this decrease was asymmetric: visits in the most conservative counties decreased by about five percent but did not change in the most liberal counties; and (c) these effects dissipated within 10 weeks after activism. Our results highlight the strategic implications for executives pressured to take stances on controversial issues.

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

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<sup>1</sup> University of Virginia Darden School of Business, Charlottesville, VA 22903. [hoy@arden.virginia.edu](mailto:hoy@arden.virginia.edu)

<sup>2</sup> UCLA Anderson School of Management. 110 Westwood Plaza, Los Angeles CA 90095.  
[chris.poliquin@anderson.ucla.edu](mailto:chris.poliquin@anderson.ucla.edu)

## 1. INTRODUCTION

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

— Phil Knight, cofounder, Nike

During one August weekend in 2019, two mass shootings in the United States killed 32 people and reignited a perennial debate over gun policy. Several weeks later, at least 146 CEOs of prominent companies urged Congress to pass new gun-control measures. Their activism is just one of several recent instances of CEOs speaking out about controversial issues that do not directly affect their core business. Other examples include the CEO of Nike supporting Black Lives Matter, the CEO of Chick-fil-A opposing same-sex marriage, and hundreds of CEOs protesting a Georgia voting law passed following the 2020 presidential election.

Chatterji and Toffel (2019a) first defined CEO activism as “corporate leaders speaking out on social and environmental policy issues not directly related to their company’s core business.” While CEO activism is related to nonmarket strategy, it focuses on issues largely unrelated to the core business of these CEOs’ companies. Nonmarket strategy, in contrast, mostly focuses on how companies shape the business environment to promote their core business (Baron, 1995; Bonardi, Holburn, & Vanden Bergh, 2006). Additionally, CEO activism is related to, yet distinct from, nonmarket strategy because (a) it emphasizes the action of the individual CEO rather than the firm, (b) it is inherently public, and (c) “the audience for CEO activism is not only regulators and politicians... but also employees, consumers, and the public at large (Chatterji & Toffel, 2019a).

A CEO considering taking public positions on controversial issues not directly related to their company’s core business may anticipate several potential effects. Emerging research suggests that activism may align a firm with its employees’ values (Bermiss & McDonald, 2018; Burbano, 2021) and create positive brand associations for consumers who agree with the CEO’s position

(Chatterji & Toffel, 2019a; Panagopoulos, Green, Krasno, Schwam-Baird, & Endres, 2020). Although CEO activism can positively differentiate a firm, it may also alienate some stakeholders. For managers who wish to avoid speaking out on controversial issues, staying silent may also be costly. Surveys show that 65 percent of consumers want CEOs to speak on major social issues (Larcker, Miles, Tayan, & Kim, 2018).

This paper uses mobile phone location data to estimate the effect on consumers of CEO activism supporting gun control. We focus on how the effects differ for liberals and conservatives, and on the effects' duration. Studies on this topic are mostly experimental (Chatterji & Toffel, 2019a) and do not examine the persistence of CEO activism's effects. We offer complementary, observational evidence from a large sample of stores and explore how quickly CEO activism's effects on consumers dissipate. Persistence is important to study because whether changes in consumer behavior are short- or long-lived has implications for CEOs and firms that want to use activism strategically as part of a permanent differentiation strategy (Lenox & Chatterji, 2018).

Our analysis shows that CEO activism supporting gun control has a small, negative net effect on consumer visits to a company's stores of about three percent. Consumers' shopping behavior, however, is influenced by their political views, and the effects of activism on likely supporters and opponents of gun control are asymmetric. Store visits do not change in the most politically liberal counties but decrease about five percent in the most politically conservative counties. These results are comparable to those of Chatterji and Toffel (2019a), who also find a small effect that depends on consumers' political beliefs. Additionally, our finding that the effect for conservatives is greater in magnitude than the effect for liberals aligns with the results of Burbano (2021), who studies employees and first identified the potential for such asymmetric responses. Like us, she reports that CEO activism leads to greater effects among those who

disagree with a CEO's position. Extending past studies to examine persistence, we find these effects dissipate quickly; conservative consumers initially avoid stores whose CEOs support gun control (engage in activism), but return to them after about four weeks.

Our results contribute to our understanding of CEO activism and the broader literature in nonmarket strategy in three ways. First, research on CEO activism has so far relied on surveys and experiments to measure purchase intentions (Mikeska & Harvey, 2015; Chatterji & Toffel, 2019a; 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). Using a large sample of mobile phone location data, we measure how store-level foot traffic (a proxy for sales) is affected by CEO support for gun control. Our data's granularity enables us to examine both activism's net effects and its effects in geographic areas more or less likely to support stricter gun-control policies. Because issues such as gun control are highly partisan, a consumer's actions responding to CEO activism on this issue will likely depend on the consumer's political beliefs (Oreskes & Conway, 2010; Parker, Horowitz, Igielnik, Oliphant, & Brown, 2017; Kapner, 2019; Benton, Cobb, & Werner, 2021). Leveraging this, we study the differential effects of CEO support for gun control on liberal versus conservative consumers; responses from liberals (likely supporters) are muted, while those from conservatives (opponents) are more extreme.

Second, our results shed light on the persistence of CEO activism's effects, a crucial issue for strategic uses of CEO activism. We find consumer visits decrease by about three percent immediately following activism, but they quickly rebound and completely dissipate 10 weeks after activism. This is the case both at an aggregate level and for conservative consumers where the initial, negative response is greatest. These results indicate that the effects of activism on consumer behavior are short-lived and unlikely to have long-term consequences for purchasing decisions.

One implication of this finding is that CEOs may need to repeatedly engage in activism to use it as a differentiation strategy.

Third, our empirical examination of CEO activism contributes to the nonmarket strategy literature by better connecting nonmarket actions with market outcomes (Baron, 1995, 2001; de Figueiredo, 2009; Shotts, 2015; Oberholzer-Gee & Yao, 2018). Here, Bonardi, Holburn, and Vanden Bergh (2006) note that identification concerns make it difficult to evaluate the performance implications of nonmarket actions. By using a longitudinal study design and taking advantage of a triggering event (back-to-back mass shootings), we reduce the number of confounding factors that can affect observed outcomes to more precisely evaluate the performance implications of CEO activism.

The remainder of the paper is organized as follows: Section 2 discusses relevant theories linking CEO activism with consumer behavior. Section 3 describes the activism event we study: CEO support for gun control in September 2019 following two mass shootings. Sections 4 and 5 explain the data and methodology, respectively. Sections 6 and 7 present our results and robustness checks, and Section 8 concludes.

## **2. THEORY**

CEO activism on controversial issues such as gun control, voting laws, and LGBTQ rights may affect consumer behavior and differentiate a firm from its competitors. Consumers often link CEOs' positions on controversial issues to their firms and products (Mikeska & Harvey, 2015), and research on political consumerism shows that consumers frequently consider companies' positions on controversial issues when shopping (Stolle & Micheletti, 2013; Copeland & Boulianne, 2020). In large surveys, between 40 and 50 percent of registered U.S. voters report basing at least one purchasing decision in the previous 12 months on a company's social or political

values (Endres & Panagopoulos, 2017). Thus, when a CEO supports gun control, that support can inform consumption decisions. As with other non-quality product attributes, consumers likely differ in their taste for stores whose CEOs support or oppose controversial policies. Hence, CEO activism may please one group of customers and antagonize another. As a result, the net effect of CEO activism on sales and the duration of that effect are theoretically unclear.

Despite CEO activism's increasing importance as a common form of CEO communication, few studies exist on its effects on consumers and its potential strategic uses (Chatterji & Toffel, 2019b). Unlike traditional forms of CEO communication that have been examined (e.g., conference calls), CEO activism on controversial issues is more recent and appears to elicit stronger emotional reactions from consumers. These, in turn, provide activist CEOs with a large audience since the media widely report CEO statements on controversial topics (Westphal & Deephouse, 2011).

In this section, we draw from the political science literature on polarization and lifestyle politics (Bennett, 1998) to argue that CEO activism on controversial issues asymmetrically affects consumers who agree and disagree with a CEO's position. We then explore the persistence of activism's effects and discuss implications for firms.

## **2.1. Asymmetric effects of CEO activism on consumers**

To assess how consumers may respond to CEO statements on controversial issues (e.g., same-sex marriage, Black Lives Matter, gun control), we first consider the political science literature on affective polarization and examine why CEO activism on social and political issues may affect consumer behavior. This literature finds that polarization affects how people see members of the opposite political party (that is, Democrats versus Republicans) (Fiorina, Abrams, & Pope, 2005; Benton, Cobb, & Werner, 2021). In the case of CEO activism, consumers might see activist CEOs

as aligning with or opposing their preferred political party (Fiorina, Abrams, & Pope, 2008). Consumers who affiliate with one political party may characterize a firm as either the in-group or the out-group based on its CEO's activism, and they may then wish to purchase products from perceived co-partisans (McConnell, Margalit, Malhotra, & Levendusky, 2017). This phenomenon of consumers finding social and political meaning in purchases is termed "lifestyle politics" and has been shown to be a powerful force that can affect spending behavior, recreational experiences, and fashion decisions (Bennett, 1998; Shah *et al.*, 2016).

The potential for lifestyle politics to affect consumer behavior is evident in ongoing debates over gun control. Over the past three decades, gun ownership has become more partisan and is now a reliable predictor of voting Republican (Joslyn, Haider-Markel, Baggs, & Bilbo, 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). 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 policy issues related to firearms, favoring fewer restrictions on their ownership and use. Pew Research Center opinion polls during the 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 gun rights than to control gun ownership (Parker *et al.*, 2017). Given the close relationship between political affiliation and positions on gun control, we conjecture that political party affiliation is a good indicator of consumers' agreement with CEO activism favoring stricter gun control, and that conservative and liberal consumers will react along party lines.

In addition to predicting how consumers will react, we believe the level of reaction among consumers who oppose versus support a CEO's activism stance will differ. When a consumer learns that the CEO of a store she frequents is taking a stance opposite to her own belief, we argue she is more likely to act than if she agrees with the CEO. Burbano (2021) first documents an asymmetric response to CEO activism in the context of an employee–employer relationship. She finds that when a worker disagrees with an employer's social-political stance, there is a demotivating effect, but when a worker agrees, there is no motivating effect. Burbano (2021) theorizes that a major driver of this asymmetric behavior is false consensus, where an employee falsely perceives that her employer holds a similar belief, only to be disappointed and demotivated later. Consumers may also suffer from false consensus: upon learning that the CEO of a firm they patronize holds different views on a controversial issue, they may be disappointed and avoid visiting the firm.

Additionally, consumers may also react to CEO activism with anger. Consistent with the literature on lifestyle politics and social movements, consumers who disagree with a CEO are more likely to boycott (King & Soule, 2007; McDonnell & King, 2013). Thanks to the efforts of various organizations and grassroots movements, today, the public has been primed to act. In the case of gun control, organizations such as the National Rifle Association and Everytown for Gun Safety—the largest gun-violence prevention organization in America—as well as demonstrations like March for Our Lives—a student-led demonstration in support of gun-control legislation—have encouraged the public to act. Because false consensus and anger are powerful forces, it is conceivable that consumers who disagree with a CEO's activism are more likely to react.



## 2.2. Persistence of CEO activism's effects

Where consumers shop is driven by a complex set of market and nonmarket forces. Market forces such as price, convenience, and quality have been well studied in a wide array of disciplines (Porter, 1979; Farquhar, 1989; Nevo, 2001). However, research on the relative importance of nonmarket forces, such as CEO activism, is nascent, and the persistence of their effects is unknown. When Walmart's CEO, Doug McMillon, supported stricter gun-control policies, consumers who chose to avoid or patronize Walmart due to his position likely incurred costs along other dimensions (e.g., price, convenience, quality). Consumers' willingness to continue making these trade-offs over the days, weeks, and months following activism is *a priori* unclear.

There are several reasons to think consumers' responses to CEO activism are short-lived. First, switching stores entails costs due to differences in product assortment, prices, and convenience, all vital factors consumers consider when choosing where to shop (Huff, 1964; Bell, Ho, & Tang, 1998; Briesch, Chintagunta, & Fox, 2009). For example, a consumer upset with Walmart following CEO activism may need to travel farther, shop at stores with a smaller product assortment, or pay higher prices in order to avoid patronizing Walmart. Given these costs, it is conceivable that although consumer behavior may change because of CEO activism, the change may be short-lived. Industry reports show that consumers are habitual in their frequent shopping; grocery and general merchandise stores are consistently ranked as top brands in consumer loyalty, and a majority of consumers do their regular grocery shopping at only two stores (PLMA, 2014; Smarty, 2021). These factors suggest that consumers are unlikely to permanently change their shopping behavior and that CEO activism will not have long-lasting effects on consumers.

Second, at the individual level, research shows that when doing frequent activities like shopping, consumers often do not pay attention (DellaVigna, 2009; Stango & Zinman, 2014), are

overloaded with competing information (Anderson & de Palma, 2013), and can forget information they acquired or considered during recent transactions (Grubb, 2014). In our context, when making a new decision about which store to visit, a consumer may have forgotten about the CEO activism that factored into previous shopping decisions.

Even the effects of persuasive advertising are not permanent; increases in goodwill generated by advertising decay rapidly, 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). Comparatively, news coverage of CEO activism centers on the dates of activism, so consumers are not reminded later of a CEO’s stance. This suggests that consumers may forget about a CEO’s activism and quickly revert to their original shopping behavior.

Third, with regard to boycotting, anger is likely ephemeral at both an individual and a collective level. At an individual level, economic research on transient, visceral emotions suggests that visceral factors can drive people to behave in ways they view as contrary to their self-interest (Loewenstein, 2000). However, these emotions are temporary (e.g., road rage), meaning consumers’ anger at a specific company is likely to quickly fade. At a collective level, Woliver (1996) highlights multiple barriers to mobilizing and sustaining grassroots movements, noting the difficulty of sustaining long-term public interest in a topic. As a result of forces such as switching costs, consumers’ limited attention spans, the transitory effects of visceral emotions, and the difficulty of sustaining collective action, we predict that consumers who initially avoided a particular store in response to CEO activism will quickly revert to their previous shopping habits.

### **3. CEO ACTIVISM ON GUN CONTROL**

CEO activism has garnered much attention in recent years as more CEOs are taking public positions on controversial topics (Chatterji & Toffel, 2019b; Segal, 2020). Examples include Dan

Cathy (Chick-fil-A) denouncing gay marriage (McGregor, 2012), Tim Cook (Apple) criticizing Indiana's Religious Freedom Restoration Act (Wakabayashi, 2015), dozens of CEOs supporting Black Lives Matter (Chintagunta, Kansal, & Pachigolla, 2020), and Robert "Bob" Unanue (Goya Foods) supporting Donald Trump in the 2020 election. This paper focuses on gun-control advocacy in 2019 by several CEOs, including some who previously engaged in activism. In 2015, Doug McMillon (Walmart) voiced opposition to Arkansas's "religious freedom" law, and in 2018, both Edward Stack (Dick's Sporting Goods) and Chip Bergh (Levi's) advocated for stricter gun-control policies (Kapner & Feinberg, 2015; Noguchi, 2018).

Here, we focus on letters supporting gun control sent to the U.S. Senate by 146 CEOs in September 2019 following two mass shootings. On August 3 and 4, 2019, two mass shootings—the first in a Walmart in El Paso, Texas, and the second in a popular downtown area of Dayton, Ohio—killed 32 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, 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, 146 CEOs from various industries voiced their support for stricter gun control. In a September 12 letter sent to U.S. senators, 145 CEOs called gun violence "a public health crisis that demands urgent action." They urged 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, Mestrich, Connaughton, & Pagliuca, 2019). In a similar letter sent to lawmakers on September 3, Doug McMillon explained that Walmart would stop selling certain firearms and ammunition,

encouraged lawmakers to support expanded background checks, and called for a debate on reauthorizing the federal assault-weapons ban that had expired in 2004 (McMillon, 2019).

News of these CEOs' support for gun control quickly spread through major news and social-media channels such as the *Wall Street Journal*, *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).

## 4. DATA

We combine data from three main 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 various 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. Mobile phone location data

Our dataset measuring store performance comes from SafeGraph, a company that tracks foot traffic to millions of U.S. stores using mobile phone location data.<sup>1</sup> SafeGraph collects data through application programming interfaces (APIs) and software development kits (SDKs) that

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<sup>1</sup>SafeGraph tracks visits to places that 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.

are directly embedded in mobile apps (SafeGraph, 2021a). Specifically, the dataset consists of individuals who have opted in to location tracking by allowing an application with an embedded SafeGraph SDK (such as a weather app) to use their location.<sup>2</sup> According to SafeGraph, data are sourced from thousands of applications and cover 10 percent of the U.S. population (Veraset, 2021). The resulting sample is generally representative of the U.S. population, including on demographic variables such as race, education, and income (Squire, 2019).<sup>3</sup> We discuss the SafeGraph dataset in detail, including how well it represents the U.S. population, how we identify stores, and how we calculate store visits, in the Appendix.

## **4.2. Political affiliation**

We measure political affiliation of a store's consumers using county-level data from the MIT Election Lab (MIT Election Data and Science Lab, 2018). To measure the political leaning of a county's consumers, 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. Because county-level election data are not available for Alaska or U.S. territories,<sup>4</sup> we exclude these locations from all analyses. We merge the county-level election data with the SafeGraph location data to identify stores located in Democratic- versus Republican-leaning counties.

## **4.3. Constructing treatment and control groups**

To construct our treatment group, we identify all firms whose CEOs signed a letter supporting stricter gun control in early September 2019 that also have physical store locations. We limit our

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<sup>2</sup> SafeGraph does not reveal which applications it partners with; we use weather as an example of an application people frequently allow to access their location.

<sup>3</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 consumer category and children have limited direct purchasing power.

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

selection of treatment companies to this subset because our study focuses on consumers and relies on physical store visits to measure performance. Of the 146 companies that supported gun control, four have physical store locations—Walmart, Dick’s Sporting Goods, Levi Strauss, and the Gap—collectively accounting for 5,724 stores.

To construct a control group, we use two different approaches that bias our estimates in opposite directions. First, we select potential control firms from the universe of branded stores in the same counties and six-digit NAICS industries as our focal firms; we call these our “same-industry-county controls.” As an example, a potential same-industry-county control store for Walmart in Johnson County, Kansas, is Big Lots in the same county. This approach has the benefit of comparing treated stores to control stores exposed to similar industry and geographic shocks. However, this approach suffers from potential substitution effects that bias our estimates upward in magnitude. Consumers who shop at treatment stores (e.g., Walmart) may switch to control stores (e.g., Big Lots), which would inflate our estimated difference between the two groups.

Second, as an alternative way to construct a control group, we use a list of brands that SafeGraph identifies as “related” to our focal firms in terms of store visits. That is, a consumer who patronizes the focal firm is also likely to patronize a “related” firm. For example, a potential related-brand control for a Walmart store is a Shell gas station or Starbucks coffee shop nearby. These are identified as related-brand controls by SafeGraph because consumers tend to visit them when visiting the focal store (e.g., Walmart). We refer to this control group as our “related-brand controls” and provide more details about its construction in the Appendix.

Fortunately, the expected bias when using the related-brand controls has a sign opposite to that of the same-industry-county 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>5</sup> If those consumers avoid Walmart, visits to the Shell station will decline. In this case, our estimates 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 likely to bias the estimates in opposite directions allows us to examine whether these biases meaningfully affect our inferences.

To ensure none of the stores in our control groups also supported gun control in 2019, we searched the ProQuest Newspapers database for each brand in the control group and the words “gun” or “firearm.” A research assistant then reviewed the search results to confirm that none of the control brands publicly supported gun control during our sample period.<sup>6</sup>

#### 4.4. Summary statistics

Table 1 shows summary statistics for our sample; Table 2 shows correlation coefficients; and Table 3 reports means and standard deviations separately for the treatment and control stores. The stores in our sample receive an average of 1,684 visits per week and are balanced between Democrat- and Republican-leaning counties.<sup>7</sup> Activist stores are, on average, larger than non-activist stores in terms of both visits and area; this is not surprising due to the inclusion of Walmart—the largest U.S. retailer—in the activist group.<sup>8</sup> Store area figures in the tables, however, should be interpreted

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<sup>5</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.

<sup>6</sup> Our subscription to ProQuest contains 1,514 publications, including the *New York Times*, *Washington Post*, *Los Angeles Times*, *Chicago Tribune*, and *Wall Street Journal*.

<sup>7</sup> Walmart operates hundreds of stores in both very liberal and very conservative counties. This is less true of Levi’s, Dick’s Sporting Goods, and Gap, which collectively operate far fewer stores than Walmart in counties that vote more than 70 percent Republican. Thus, most of our variation across the most liberal and most conservative counties comes from observing Walmart stores.

<sup>8</sup> Given the obvious differences between activist and non-activist stores, we rely on difference-in-differences methods (see Section 5) for estimating treatment effects that do not depend on having treatment and control units that are similar

cautiously, because some area measurements include parking lots or the space enclosing a focal location (e.g., the area measure for a retailer may include its parent shopping mall).<sup>9</sup> A map of stores included in the sample (Figure 1) shows they are spread across nearly all counties of the United States.

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## 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 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:

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

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in levels of the dependent variable. Our empirical model includes store-specific parameters that account for the differences in size evident in Table 3. As we explain in Section 5.2, what matters for our study is whether changes in log visits within stores would have evolved similarly in the absence of CEO activism so that control units can be used to estimate a reliable counterfactual for the treatment units in the absence of treatment.

<sup>9</sup> SafeGraph does not provide comprehensive data on which store areas include parking lots. It is therefore not possible to filter locations to create more comparable figures. The average Walmart in our sample is reportedly 223,992 ft<sup>2</sup>. The true average according to Walmart’s SEC filings, however, is between 105,000 and 178,000 ft<sup>2</sup>.



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>10</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 is identified by the six-digit NAICS code.<sup>11</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>12</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 each year around the time students return to school. The store-level fixed effect ( $\alpha_i$ ) captures Walmart stores' persistently higher foot traffic than other general merchandise stores, but does not adjust for regularly occurring *seasonal* differences in the number of store visits. Our model adjusts for both Walmart's generally higher foot traffic and seasonal performance patterns.<sup>13</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 average Republican vote share in the 2008, 2012, and 2016 presidential elections;

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<sup>10</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>11</sup> The function  $k(i)$  maps store  $i$  to its county location;  $n(i)$  is defined analogously for industry.

<sup>12</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.,  $\delta = \theta = 0 \forall i$ ) and models that allow for seasonality.

<sup>13</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, however, reveals that store visits regularly fall in early September and that activism on gun control had only modest net effects on store performance.

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.

When using the related-brand controls, which 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:

$$\ln Y_{it} = \beta \mathbb{1}(t > T_0 \cap D_{b(i)} = 1) + \alpha_i + \lambda_{g(i)t} + \delta_{i w(t)} + \theta_{i y(t)} + \epsilon_{it}, \quad (2)$$

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 Shell in the post-activism period.

### 5.1. Event study

In addition to Equations (1) and (2), we estimate “event-study”-style models with dynamic treatment effects that include indicators for each pre- and post-activism period:

$$\ln Y_{it} = \sum_{j=1}^T \beta_j \mathbb{1}(t = j \cap D_{b(i)} = 1) + \alpha_i + \lambda_{k(i)n(i)t} + \delta_{i w(t)} + \theta_{i y(t)} + \epsilon_{it}, \quad (3)$$

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:

$$\beta = \frac{1}{T-T_0+1} \sum_{j=T_0}^T \beta_j, \quad (4)$$

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

around 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, as consumers respond immediately after the event then revert to normal behavior. Additionally, we believe our ability to attribute changes in store visits in the several weeks following the activism event to CEO positions on gun control is limited. Stores exposed to negative effects of activism may adjust in unobserved ways (e.g., cutting prices, increasing marketing, or changing product offerings) that could confound our estimates.

## 5.2. Parallel trends assumption

Causal inference in our study design relies on the usual parallel trends assumption. In our setting, this means that changes in log store visits for the control group reflect how, without activism, visits would have changed for stores whose CEOs took positions on gun control.

Figure 2 shows store visits over time for activist stores and our same-industry-county control stores, and Figure 3 shows the same trends for both control groups by political affiliation of stores' counties. In areas classified as *Very Liberal*, Republicans receive 30 percent or less of the vote; in *Liberal* areas Republicans receive 31–50 percent; in *Conservative* areas 51–70 percent; and in *Very Conservative* areas 71–100 percent.<sup>14</sup> 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.

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<sup>14</sup> Our results are robust to using other cut points, such as quartiles of the Republican vote share or four even bins of 25 percentage points each. The cut points we use provide slightly larger sample sizes for the *Very Liberal* and *Very Conservative* categories so that our analysis for these categories can be more precise and, unlike using quartiles, center the categories around 50 percent Republican vote share.

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 an absence of activism.<sup>15</sup> We show in our results below (Table 4) that estimates of Equations (1) and (3) without the seasonal terms ascribe the steep decline in store visits in September 2019 entirely to CEO activism, even though some decrease in visits is a regular feature of the data.

‘Insert Figure 2 here,’

‘Insert Figure 3 here,’

Despite the apparent 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 (3) 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 (3) with either a linear or cubic spline time trend for activist stores:

$$\ln Y_{it} = \sum_{j=T_0}^T \beta_j \mathbb{1}(t = j \cap D_{b(i)} = 1) + f(tD_{b(i)}; \phi) + u_{it}, \quad (5)$$

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<sup>15</sup> This illustrates the importance of modeling seasonality in our setting. Even controlling for counties and six-digit NAICS industries, store visits exhibit recurring seasonal patterns.

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>16</sup> As with Equation (1), we also estimate versions of this model that interact all terms with the political affiliation of stores' counties.

## 6. RESULTS

Table 4 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 through 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 200 visits per week for the average activist store.

‘Insert Table 4 here,’

Columns 3 and 4 of Table 4 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 vote shares for the Republican presidential candidate in the store's county during the 2008,

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<sup>16</sup> We conducted the analyses in R (R Core Team, 2020).

2012, and 2016 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.<sup>17</sup> 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 1.5-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 5.2-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 to six percent during the four weeks after CEO activism. Together, these results suggest that CEO activism has an asymmetric effect on consumers: those who disagree with the CEOs’ gun control stance react more strongly than others.

To examine the duration of the effects of activism beyond four weeks, we extend the sample in Columns 5 and 6 of Table 4 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.

‘Insert Table 5 here’

Table 5 shows that results using the related-brand control group are similar to those using the same-industry-county controls (Table 4). Specifically, the estimates in Column 1

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<sup>17</sup> Most variation in our data across *Very Liberal* and *Very Conservative* counties comes from Walmart. There are only 35 *Very Conservative* counties with non-Walmart activist stores in our data, but more than 300 *Very Conservative* counties with Walmart stores. An advantage of studying Walmart is that it operates hundreds of stores in both *Very Liberal* and *Very Conservative* counties.

(corresponding to Column 2 of Table 4) suggest that weekly visits to activist stores decrease by four percent over the month following activism. Likewise, Columns 2 and 3 of Table 5 show that the effect of CEO activism on a store depends on the political affiliation of a store's customers. The more conservative a county's voters are, the greater the decrease in visits to stores whose CEOs support gun control; stores in the most conservative counties experience a four-to-five percent decrease in visits, while stores in the most liberal counties see decreases of about two percent. As before, extending the analysis to include 10 weeks following activism (Columns 4–6 of Table 5) suggests there is no persistent net effect of CEO activism, and in the most conservative counties, the effect even turns slightly positive.

To assess the magnitude of the effects over the four weeks following activism, we calculate the change in sales associated with the percentage changes in store visits (see the Appendix for details). Based on the estimates in Column 3 of Table 4, total sales losses across all firms are likely between \$800 million and \$1.1 billion over the four weeks following activism. Our estimated losses for Walmart represent about 0.2 percent of annual revenue. For comparison, Walmart's total revenue increased about 2 percent in 2019.

Next, we explore the pattern of dynamic effects week by week, but before doing so, we note how the estimates in Tables 4 and 5 compare with the effects reported in related experimental studies of consumers. Chatterji and Toffel (2019a) examine how statements by Apple CEO Tim Cook supporting same-sex marriage 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 (2019a) find that the effect of activism depends on the audience and is asymmetric; purchase intention increases for supporters of same-sex marriage, but they “find no

evidence that Cook’s statements altered the purchase intent of same-sex marriage opponents.” 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. Thus, the experimental evidence aligns in several ways with the main results of our observational study.

### 6.1. Dynamic effects of CEO activism

The estimates in Tables 4 and 5 reflect the average effect of CEO activism in the month following the event. In order to investigate the dynamic effects of activism and their persistence, we estimate the model in Equation (3), which includes indicators for each week pre- and post-activism (omitting the period immediately prior to activism), using the same-industry-county control group. 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 4). We first focus on interpretation of the post-activism coefficients, then on the pre-period coefficients and parallel trends assumption.

‘Insert Figure 4 here,’

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 4, 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 afterward. *Very Conservative* counties—those in which more than 70 percent of voters typically support the



Republican candidate for president—see weekly store visits fall about 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 (as shown in Table 4, the aggregate, net effect of activism in both liberal and conservative counties after 10 weeks is close to zero). The results, however, should be interpreted cautiously because changes in store visits—especially several weeks following activism—may reflect unobservable actions by store managers rather than CEO activism directly. 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 8 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 5.2 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.

## 6.2. Accounting for differences in pre-activism trends in store visits

Recent work on difference-in-differences 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 results’ sensitivity to plausible violations. To do so, we implement the method of Bilinski and Hatfield (2018)—see Equation (5)—to examine our estimates’ sensitivity 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 a linear (dark blue) or cubic (green) trend are generally larger than those from a model assuming parallel trends (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. 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, suggesting that the trend difference between the two groups is linear. All models indicate that CEO activism results in asymmetric consumer behavior immediately following the event. The differences in activism effects between very liberal and very conservative counties in the first and second week post-activism are between five and eight 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 (4). Again, all models suggest that after CEO activism, stores in liberal counties experience no change or slightly positive effects, while those in conservative counties experience decreases in store visits.<sup>18</sup>

## 7. ROBUSTNESS CHECKS

In this section, we explain how online sales might affect our conclusions and examine the robustness and sensitivity of our results to the measure of political affiliation, whether activist stores sell guns, the exclusion of individual states, and variations in the control group.

### 7.1. e-Commerce

One limitation of our study is the inability to observe online shopping behavior. SafeGraph data do not include consumers' online purchases. Since we cannot measure how much consumers switch between off-line and online sales channels in response to activism, the coefficients in Tables 4 and 5 could over- or underestimate the magnitude of CEO activism's effects on total sales. Online shoppers may be more likely to switch retailers in response to activism when competition is only a click away. In that case, our estimates and the difference between liberals and conservatives may be too small relative to the true effect on sales—that is, the result should be more positive for liberals and more negative for conservatives. Alternatively, online shoppers may be more loyal to specific brands (Danaher, Wilson, & Davis, 2003), which would have opposite effects.

Regardless of e-commerce, our estimates remain valid for the effect of CEO activism on store visits and likely proxy for changes to in-store sales. The effects of e-commerce outlined

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<sup>18</sup> Though still indistinguishable from zero, when incorporating a linear time trend, the effect on liberals over the four weeks following activism looks positive and is similar in magnitude to the negative effect for conservatives.

earlier do not bias the estimates when interpreted more narrowly as effects on in-store consumer behavior. However, e-commerce may affect the extent to which our estimates of in-store behavior reflect treatment effects of CEO activism on total sales. On this point, it is informative to consider e-commerce's share of total sales.

To gauge the potential importance of e-commerce, we examine aggregate data from Walmart's SEC filings. These data show that for the three months ended October 31, 2019, e-commerce accounted for only six percent of net sales (Walmart, 2019).<sup>19</sup> This figure is less than that for the broader sector: e-commerce sales represented roughly 11 percent of total retail sales in 2019 (U.S. Census Bureau, 2021). This relatively small percentage of consumers shopping online implies that our dependent variable—store visits—reflects a large percentage of total sales. Moreover, it suggests the effects of CEO activism on e-commerce behavior would have to be very large to offset the negative effects we estimate; to compensate for a 5.5-percent reduction to in-store activity in very conservative counties (Column 4 of Table 4), online purchases would need to increase by 86 percent in response to CEO activism.

## 7.2. 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>20</sup> The latter measure pools data from several national surveys

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<sup>19</sup> Total Walmart sales for the three months ended October 31, 2019, were \$83.19 billion, \$5 billion of which were related to e-commerce. This overstates the importance of online sales, however, because it includes omni-channel purchases (i.e., a consumer placing an order online and picking up in store), which are captured in SafeGraph data.

<sup>20</sup> We use the 2016 release of the county-level estimates.

of policy preferences to create a continuous measure of ideology along the left–right political spectrum for each county’s mean citizen.

‘Insert Table 6 here,’

Table 6 recreates the estimates from Columns 3 and 4 of Table 4 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 and 2) are nearly identical to those in Table 4 using the average of Republican vote shares across three elections. Estimates using the ideology measure (Columns 3 and 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 to five percent (versus five to six percent using measures based on Republican vote share). Regardless of the measure, there is a statistically significant difference between the effects in the most liberal and most conservative areas.

In addition to using alternative measures of partisanship and ideology, we explore the robustness of the results to alternative ways of defining the political categories in Column 4 of Table 4. The results, which are presented in the Appendix, show that our estimates are not sensitive to a particular way of defining these political categories.

### **7.3. Gun sales do not explain the decrease in store visits**

Some stores owned by Walmart and Dick’s Sporting Goods in our sample sell firearms. When Walmart’s CEO announced his support for gun control, he also indicated that Walmart would stop selling handgun ammunition. To examine whether gun sales can explain the decrease in store visits, we use data from the U.S. Bureau of Alcohol, Tobacco, Firearms and Explosives on the

name and address of all retailers licensed to sell firearms. We match these data to the SafeGraph sample and re-estimate our main difference-in-differences model, including an interaction between the post-activism period, political affiliation, and whether a store sells firearms.

‘Insert Table 7 here,’

Column 1 of Table 7 shows no difference in activism’s net effect on stores that do and do not sell guns—the coefficient on the interaction of activism and gun sales is close to zero and not statistically significant. Column 2 shows results by political affiliation (classified using Republican vote share as in Table 4) and suggests that stores selling guns saw a two-percentage-point smaller decline in store visits than non-gun-selling stores in very conservative areas. These point estimates also suggest that stores selling guns in *Very Liberal* counties saw a nearly six percent decrease in store visits, but the coefficient estimate is imprecise and not statistically significant. Not surprisingly, whether a store sells guns is strongly correlated with the political affiliation of a store’s consumers; as a result, few stores in very liberal areas can be used to identify this coefficient. The results in Column 3, which use the political-affiliation measure from the American Ideology Project (see Section 7.2 and Table 6), show that the estimates in Column 2—unlike our main results—are not robust and are specific to using Republican vote share to measure county political affiliation. The effect for gun sellers in very liberal counties continues to be statistically insignificant, and the difference between gun sellers and non-gun sellers in very conservative counties becomes indistinguishable from zero.

Together, these results suggest that whether a store sells guns does not greatly alter the effect of CEO support for gun control on consumers, and that our main estimates cannot be attributed to gun sales themselves driving the observed change in consumer behavior.

#### 7.4. 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 4. The results, presented in Figure 7, indicate 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 accounts for 30 percent of our observations in very liberal counties.

‘Insert Figure 7 here,’

#### 7.5. Variations in the control group

Our results do not change if we vary our control group to match activist stores with same-industry-county controls that are close in store size or geographic distance. In these tests, we use the single closest control store in terms of store size or distance. Although no store is identical to Walmart in size (it is the largest retailer in the United States), this matching exercise potentially identifies control stores that are more comparable to activist stores. Store size is an observable characteristic potentially correlated with attributes such as product assortment, pricing, advertising, and customer service. Alternatively, matching activists with the geographically closest control store means the treatment and control units are more likely to serve the same population of customers and hence be subject to similar shocks in demand.

In yet another test, we identify control units that are at least five miles from an activist store and *not* its closest competitor. This sample should reduce concerns about potential biases from consumers substituting between activist stores and nearby stores selling similar goods (see Section 4.3).

We present the results of this exercise in the Appendix; they show that our estimates are little affected by these variations in the control group that match stores based on size and distance.

## **7.6. Potential outliers**

Given the differences in store size between treatment and control stores (see Section 4.4 and Table 3), we confirm our results are not sensitive to extreme values of our dependent variable by identifying and dropping outliers, then re-estimating our main model from Column 4 of Table 4. The results of this exercise are presented in the Appendix and show that the coefficient estimates in Table 4 are robust to excluding potential outliers using several methods.

## **8. DISCUSSION AND CONCLUSION**

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 is related to the company's products or services. The positions executives take on controversial issues can affect nearly all stakeholders, including consumers, employees, and investors. We evaluate the effect of CEO activism on store-level performance using the decision of several CEOs to call for stricter gun control following two mass shootings in 2019. We examine three aspects of the relationship between CEO activism and consumer behavior: (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 results in a temporary and modest net decrease in store visits. We estimate that total sales losses across all firms during the four weeks following activism are likely in the \$800 million to \$1.1 billion range. For Walmart, the estimated losses represent only about 0.2 percent of annual revenue. The effects on supporters



versus opponents of the CEOs' stance differ and are asymmetric; in aggregate, consumers likely to agree with the CEOs (liberals) do not change their behavior, but those likely to disagree with the CEOs (conservatives) reduce visits to the companies' stores immediately following activism. Stores in the most conservative counties experience a four-to-six percent decrease in visits relative to a counterfactual constructed from stores in the same industry and county. These results, considered alongside other studies (Chatterji & Toffel, 2019b), point to a small but potentially significant impact of activism on consumer behavior.

The effects of CEO activism on consumer behavior dissipate quickly. Net store visits decline three percent over the four weeks following activism, but quickly recover. We find no persistent net or asymmetric effect on store visits over the 10-week post-activism period, suggesting no total sales losses over this period. Our results are among the first non-experimental measures of how CEO activism affects firm performance in the short and longer term, and can inform managers' judgments about the likely costs and benefits of activism on issues unrelated to their core business, providing them with better guidance as CEO activism increases in popularity (Chatterji & Toffel, 2019b).

Our study does, however, have limitations. In linking CEO activism to firm performance, we assume 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 for attributing sales to marketing efforts, which speaks to the reliability of store visits as a proxy for sales (Dezember, 2018). We believe 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.

Another 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 differentiate a firm in the eyes of partisan consumers. Our findings suggest that activism results in a temporary, asymmetric response from consumers, with the caveat that our estimates of treatment effects several weeks removed from the activism may be confounded 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.

Finally, as noted in Section 7.1, we do not capture consumers' online activities. Given that online sales represent a small percentage of retail sales (six percent for Walmart in 2019), we believe they minimally affect our results. Nevertheless, the inability to observe consumers switching between off-line and online channels is a limitation of our study. Future research should examine whether consumers substitute between off-line and online sales channels following CEO activism as a way to conceal their consumption from others.

Another fertile subject for future research is how the effects of activism differ across issues, CEOs, firms, and competitive environments. For example, CEO and firm histories could matter both for motivating a CEO to act and for shaping how consumers view the CEO's actions, although it is unclear how. For example, Ed Stack's history of contributing to Republicans might give him cover with conservatives to support gun control or make him a traitor in their eyes.<sup>21</sup> Similarly, the

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<sup>21</sup> Ed Stack has also given generously to Democrats; he donated \$300,000 to the House Majority PAC in 2016.

fact that Levi's stores are typically in liberal areas could dampen negative reactions or make its CEO's support for gun control less credible (Melloni, Pataconi, & Vikander, 2019). Additionally, future research could address the feasibility of competitors imitating activist CEOs. Superficially, it seems trivial for any CEO to speak out on a controversial issue, but the expected costs and benefits may depend on complementary assets such as public-relations capabilities, and CEOs may find that controversial stances are inconsistent with other elements of the firm's strategy.

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 nonmarket settings (Flammer, 2015), partly driven by the increasing difficulty of building and sustaining a unique market position (Oberholzer-Gee & Yao, 2018). 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. By showing both the net and partisan effects of CEO activism, our study 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 nonmarket 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 focus is the effect of activism on consumers, but future research should consider ways to measure several stakeholders' reactions to activism events, as well as the

dynamic interplay between stakeholders, who may even hold divergent views on controversial issues. Other stakeholder groups that have been studied in relation to CEO activism include employees (Burbano, 2021) and investors (Bhagwat, Warren, Beck, & Watson, 2020; Durney, Johnson, Sinha, & Young, 2020; Mkrtchyan, Sandvik, & Zhu, 2021).

CEO activism is an increasingly salient phenomenon, but research has only recently begun to examine its antecedents and consequences on different stakeholders. We complement the largely theoretical and experimental research to date by examining consumer reactions to CEO activism in a large, observational dataset. Our findings—that CEO activism results in a small, negative impact on sales and that the responses from likely supporters and opponents of the CEO are asymmetric—are consistent with experimental results (Burbano, 2021; Chatterji & Toffel, 2019a). Additionally, we extend previous findings by showing that the effect of CEO activism on consumers is transient; following activism, sales initially decline but quickly recover.

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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, how SafeGraph measures visits, and how we normalize visit counts 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). These data are collected through application programming interfaces (APIs) and software-development kits (SDKs) that are directly embedded in thousands of mobile applications (SafeGraph, 2021a). The resulting sample covers 10 percent of the U.S. population (Veraset, 2021).

#### 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; it is underrepresentative of middle-income individuals (Squire, 2019).

### **3. Visit attribution**

The raw data gathered by SafeGraph consist of high-frequency GPS data for a mobile device. SafeGraph filters the pings and clusters them into groups representing locations where a device was present for an extended period. It then uses polygon data representing the footprints of millions of points of interest (e.g., a Walmart store) with unique store and brand identifiers and assigns clusters of pings to specific places, thus measuring visits.

One difficulty with this process is that while many places (such as a Walmart store) stand independently and have well-known borders, others (such as a Gap store inside a shopping mall) may not. To handle places inside other places, SafeGraph identifies parent–child relationships among the polygons representing stores, locations that share polygons with another location, and locations for which it’s unsure of the shape. While SafeGraph can often still assign visits to specific stores in such cases, the company sometimes chooses to only report visits for a place’s parent location. For example, if a Gap store is entirely enclosed by a shopping mall, the company will report visits to the shopping mall, but not the individual Gap store (SafeGraph, 2021b).

To remove potential noise and misattributed visits from the data, we restrict our sample of stores to places for which SafeGraph has a well-defined polygon representative of the store and exclude locations that share a polygon with a parent location.<sup>22</sup> Our conclusions are not sensitive to this sample restriction.

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<sup>22</sup> This amounts to dropping places with a parent SafeGraph place identifier and “polygon class” of SHARED\_POLYGON, then keeping places with either a polygon class of OWNED\_POLYGON or a polygon they share with a child location. See the SafeGraph (2021b) geometry data documentation for more details.

#### 4. 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>23</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>24</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>23</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>24</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  $Sample\ Visitors_{it} \times \frac{Population_{iy(t)}}{Devices_{it}}$  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.

## 5. Related brands

In some 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.”

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 who patronize another 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 store  $i$  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 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 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.

## LARGEST CONTROL BRANDS

The following table shows the five largest control brands (in terms of number of stores) from the same-industry-county and related-brand control groups.

Same-Industry-County Controls		Related-Brand Controls	
Brand Name	Store Count	Brand Name	Store Count
Dollar General	7,125	McDonald's	6,348
Family Dollar Stores	6,102	Subway	4,725
Dollar Tree	5,620	Shell Oil	4,578
Big Lots Stores	1,129	Starbucks	4,260
Marshalls	585	Dollar Tree	3,377

## ALTERNATIVE DEFINITIONS OF POLITICAL CATEGORIES

Our primary analyses define *Very Liberal*, *Liberal*, *Conservative*, and *Very Conservative* counties using the following bins of Republican vote share: [0, 30], (30, 50], (50, 70], (70, 100]. In Figure 9 we show our results are not sensitive to this choice by re-estimating our main model from column

4 of Table 4 using a finer partitioning of Republican vote share into 8 bins covering the ranges [0, 20], (20, 30], (30, 40], (40, 50], (50, 60], (60, 70], (70, 80], (80, 100].<sup>25</sup> Figure 9 plots the coefficient estimate for each bin along with a 95 percent confidence interval. The estimates in the figure are less precise than those in the main text because each category contains much less data. The negative coefficients in the more conservative counties, however, remain statistically significant, and there is a clear, linear pattern between negative responses to CEO activism and the extent of support for Republicans in stores' counties.

## ESTIMATING REVENUE LOSS FOLLOWING ACTIVISM

This appendix presents back-of-the-envelope estimates for the magnitude of sales losses over the four weeks following activism implied by our estimates in Column 3 of Table 4.

To estimate the change in revenue, we first estimate each activist firm's average U.S. revenue over four weeks by scaling the annual revenue figures presented in its 10-K annual reports covering the 2019 activism period. For example, Levi's reports revenue in three segments—the Americas, Europe, and Asia—and says that licensing income is about two percent of reported revenue. Therefore, to approximate Levi's U.S. sales, we use 98 percent of total annual revenue reported for the Americas segment ( $\$3,057\text{m} \times 0.98 = \$2,996\text{m}$ ), divide by 52 to estimate weekly revenue, and multiply by four to estimate Levi's total net sales at U.S. stores in the four weeks following activism ( $\$2,996\text{m} \times 4/52 = \$230\text{m}$ ).

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<sup>25</sup> We widen the two extreme bins—[0, 20] and (80, 100]—because there are very few stores with such extreme levels of Republican support and no stores in counties where Republican vote share exceeds 90 percent; a (90, 100] category would therefore be empty. In addition to these bins, our results are similar if we use four even categories—[0, 25], (25, 50], (50, 75], (75, 100]—or quartiles weighted by the number of voters (which has the effect of putting the same number of voters in each category).

Using our estimates of four-week U.S. revenue, we then calculate the share of that revenue that likely comes from *Very Liberal*, *Liberal*, *Conservative*, and *Very Conservative* counties using two methods. First, we assume that revenue from each county type is proportional to population in those counties. Second, we assume that revenue is proportional to the total square footage of the company's stores as measured by SafeGraph.

Finally, we scale our approximation of sales in each county type using the coefficients in Column 3 of Table 4 to estimate the change in sales due to activism and aggregate across all county types to calculate the total change in sales for each firm.

Table 8 shows the estimated change in net sales over the four weeks following activism for each firm using either county population or store area to allocate revenue to counties (explained above). Walmart's revenue for its U.S. stores (including Sam's Club) for fiscal year 2020—which ended January 31, 2020—was nearly \$400 billion; a loss of \$800 million to \$1 billion would therefore represent a 0.2 to 0.25 percent decrease in annual revenue. For comparison, Walmart's change in total revenue for fiscal year 2020 was +1.9 percent and the increase for fiscal year 2019 was 2.8 percent. For another comparison, Walmart in 2019 had about \$44 billion in inventory and 11,501 retail units (worldwide) for a per-store inventory of roughly \$3.8 million. This implies the reduction in sales is equivalent to the value of the merchandise in about 250 Walmart stores at any given time.

## MATCHED CONTROLS

As explained in Section 7.5, we examine the sensitivity of the results to using variations in the control group that match stores based on size and distance, or exclude competitors closest to the activist stores to alleviate concerns about substitution effects that could bias the results (as described in Section 4.3). Table 9 reports results and shows that the conclusions of Table 4 are

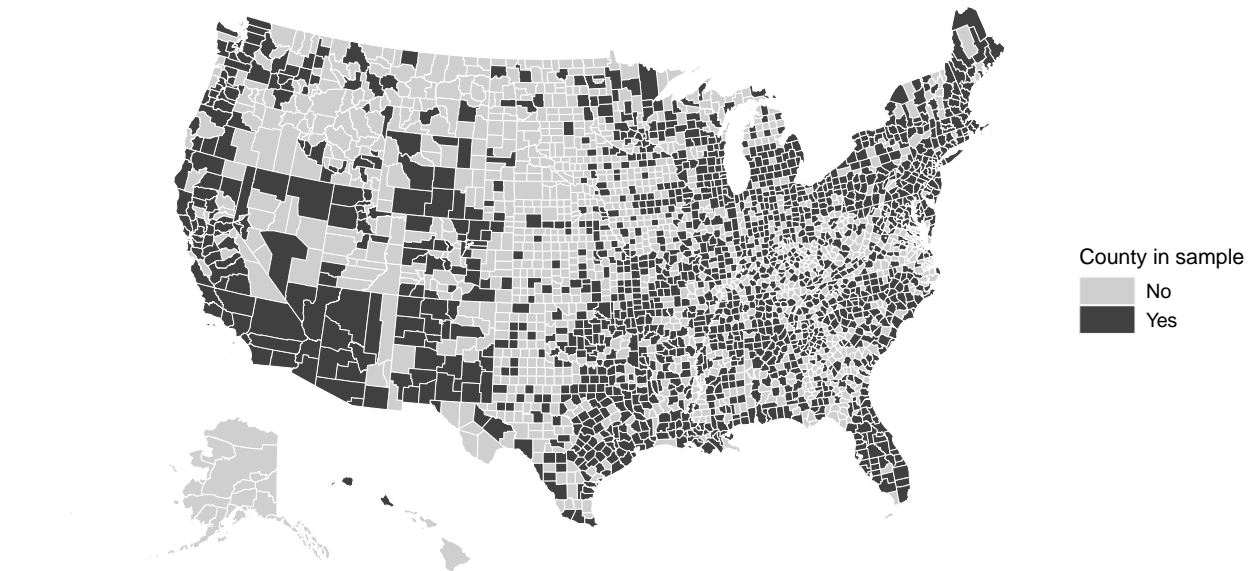


little changed if we use (a) the closest control store in terms of store area, (b) the geographically closest control, or (c) controls that are *not* the geographically closest store and at least five miles away from all activists.

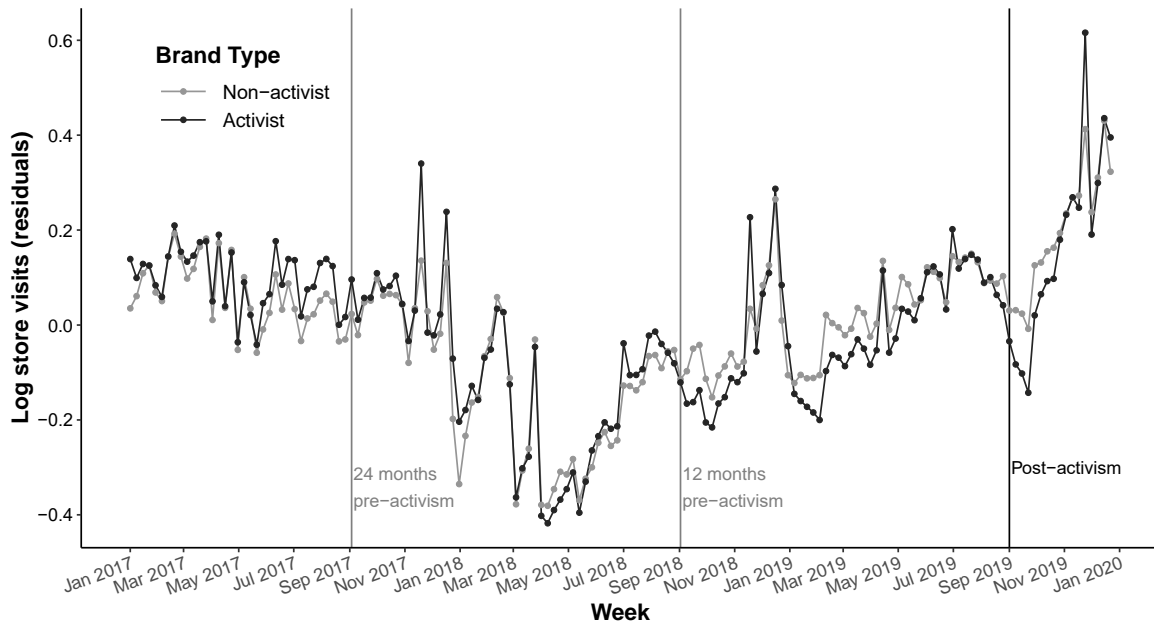
## POTENTIAL OUTLIERS

Given the differences in store size between treatment and control stores (see Section 4.4 and Table 3), we confirm our results are not sensitive to extreme values of our dependent variable by identifying and dropping outliers, then re-estimating our main model from Column 4 of Table 4. The results of this exercise are presented in Table 10. To identify outliers, we use two approaches. First, at the recommendation of a reviewer, we remove stores where the average value of the dependent variable exceeds the 90<sup>th</sup> percentile for the store's industry (Column 1). Second, we group the data by industry and use Tukey's (1977) method—which is suggested by SafeGraph (Squire, 2019) in its documentation on outliers—to remove observations outside the range  $[Q_1 - k * IQR, Q_4 + k * IQR]$ , where  $Q_j$  is the  $j^{\text{th}}$  quartile of average store visits over our sample period,  $IQR$  is the interquartile range, and  $k$  is a parameter that controls the acceptability of extreme values (conventionally set equal to 1.5). Columns 2 and 3 of Table 10 show results for  $k = 1.5$  and  $k = 2$ , respectively. The results are similar and consistent with the main results reported in Column 4 of Table 4 across all methods of identifying and removing outliers.

## FIGURES

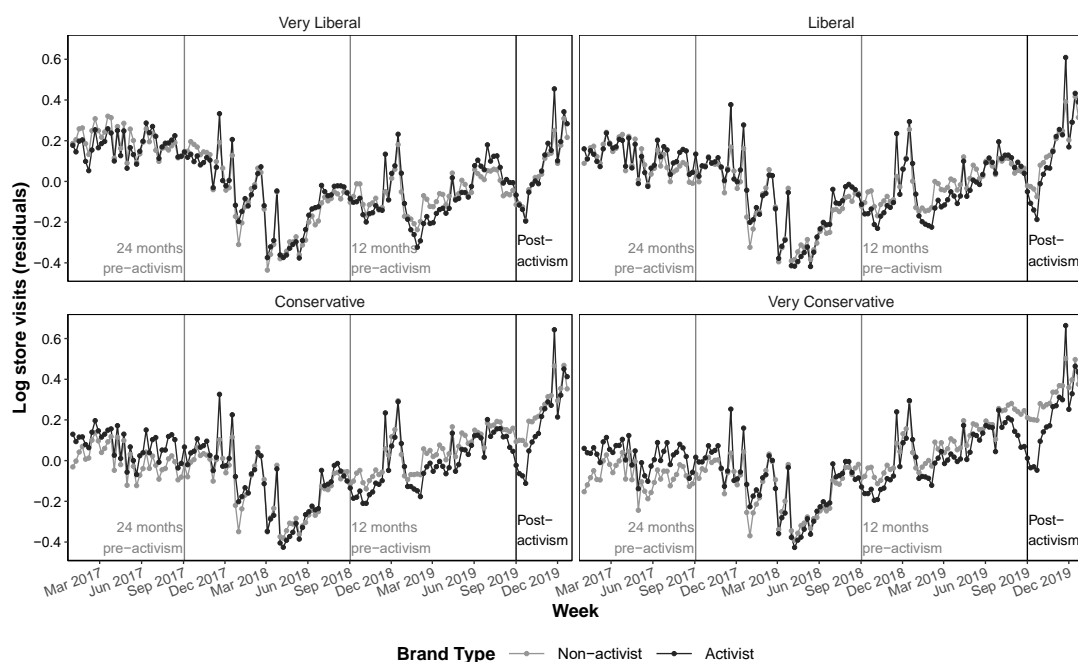


**Figure 1.** Map of counties with both activist and same-industry-county control stores

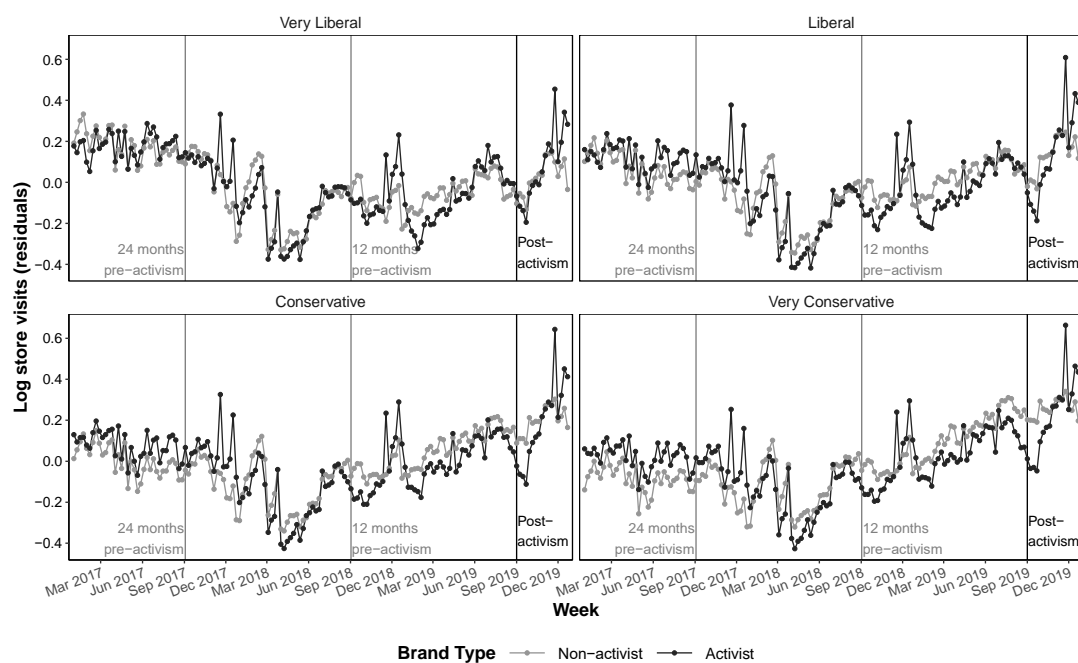


Note: Vertical lines are relative to CEO activism at Walmart, which occurred one week before the activism of the other sample stores. The plotted series represent mean log visits after residualizing with respect to store fixed effects.

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



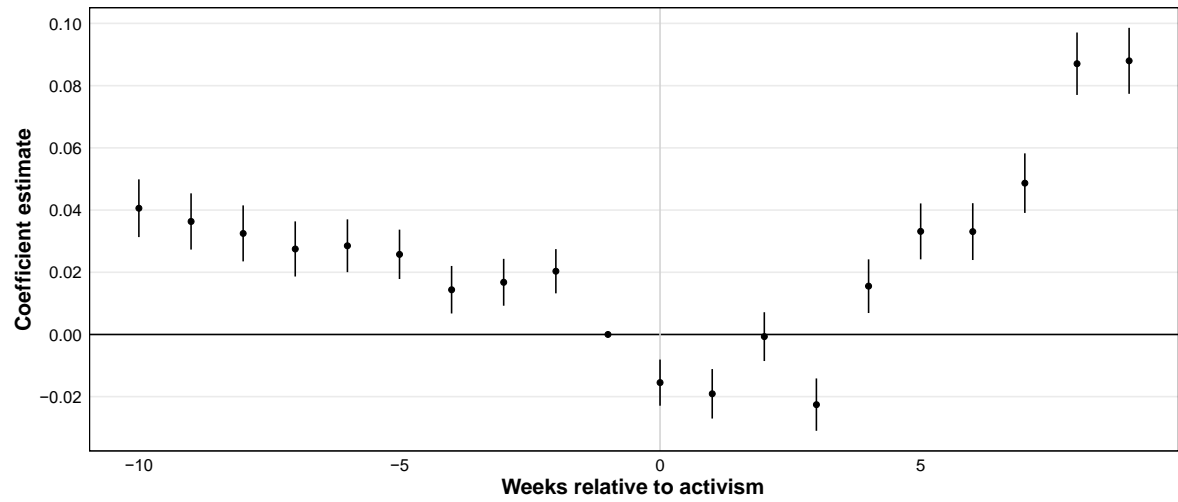
(a) Same-industry-county controls



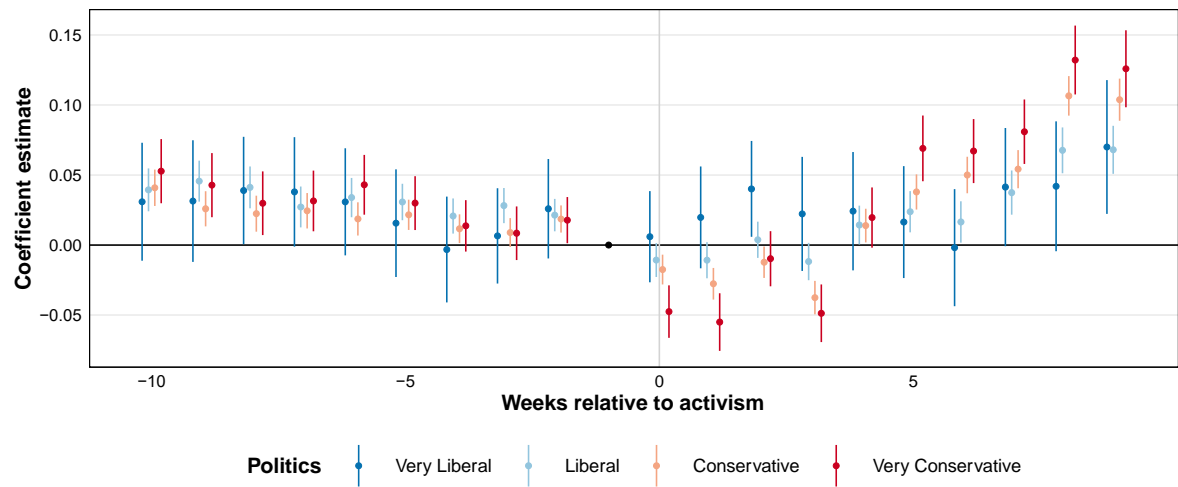
(b) Related-brand controls

Note: See notes to Figure 2.

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

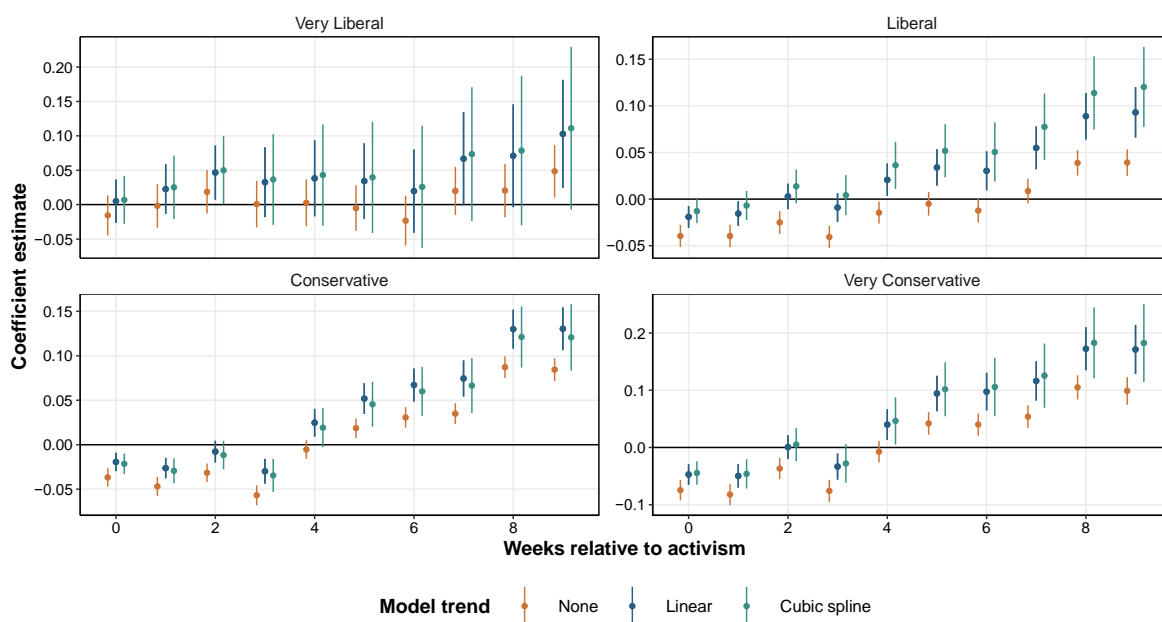


(a) Pooled analysis



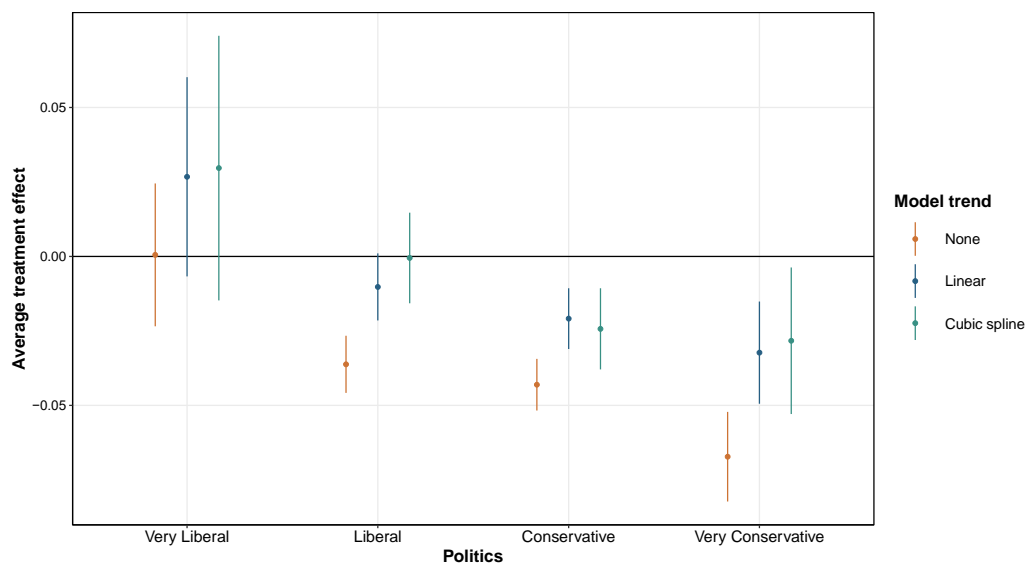
(b) Effects by county political affiliation

Figure 4. Effects of CEO activism by week



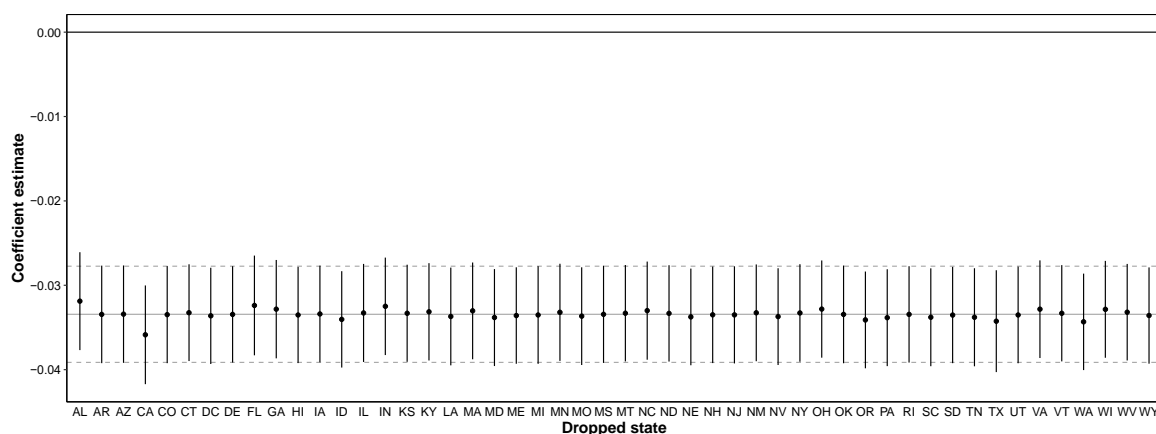
Note: Political categories are defined as in Table 4.

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

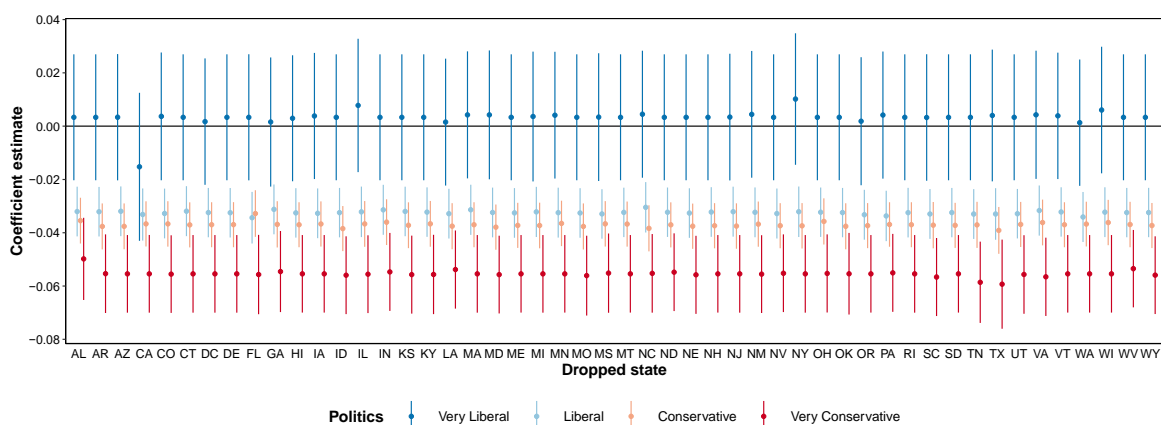


Note: Political categories are defined as in Table 4. Average treatment effects are estimated over the four weeks following activism.

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



(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 4), and dashed lines represent 95% confidence intervals. Lines for *Liberal* and *Conservative* categories are omitted from subfigure (b) to minimize clutter. Analysis uses the same-industry-county control group and political categories as defined in Table 4.

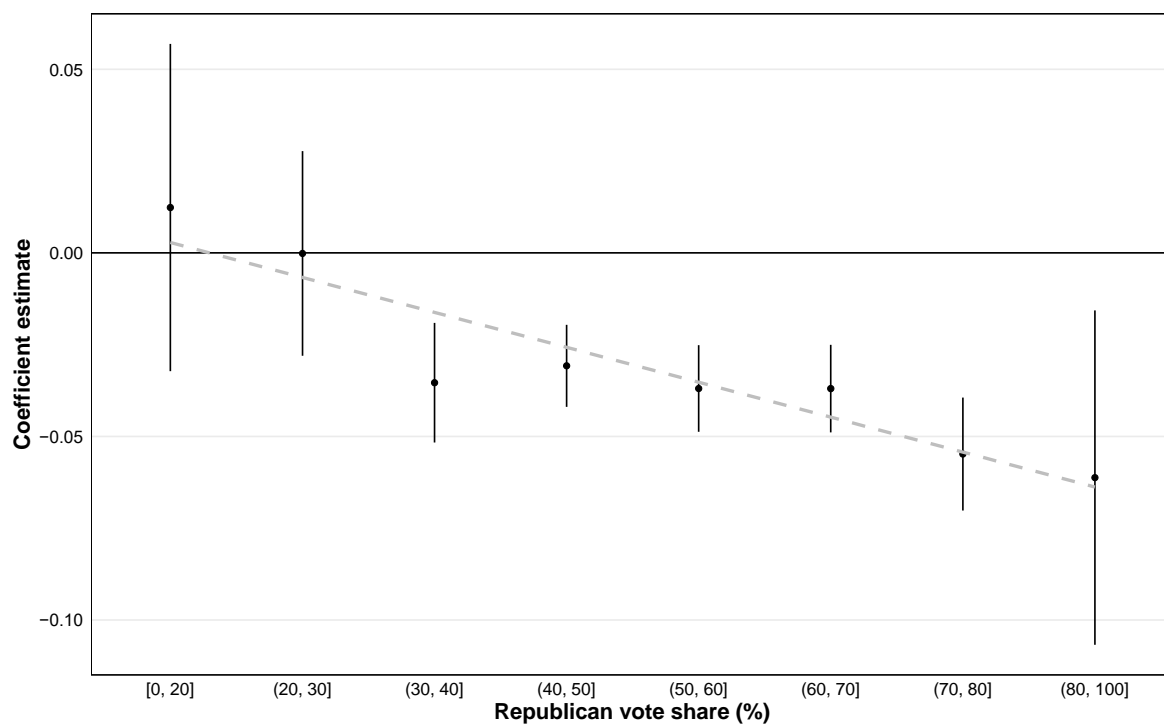
**Figure 7.** Difference-in-difference estimates omitting individual states



Note: Points represent a Walmart and several stores belonging to related brands in Falmouth, ME. Related stores in blue are those within one mile of the closest related store, while those in green are more than one 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





Note: Points represent coefficients analogous to those in Column 4 of Table 4 using finer categories of Republican vote share. Lines represent 95% confidence intervals.

**Figure 9.** Estimates using finer categories of Republican vote share

## TABLES

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

## A. Activists and same-industry-county control stores

Variable	Mean	Std. dev.	Percentile				
			10th	25th	50th	75th	90th
Activist	0.16	0.37	0	0	0	0	1
Store visits	1,684	2,885	189	362	661	1,290	4,832
Store area (sq. ft.)	40,845	89,490	4,775	8,141	10,540	25,312	125,928
Republican vote	49%	15	29	39	48	60	69

## B. Activists and related-brand control stores

Variable	Mean	Std. dev.	Percentile				
			10th	25th	50th	75th	90th
Activist	0.05	0.22	0	0	0	0	0
Store visits	1,693	5,409	289	541	989	1,856	3,592
Store area (sq. ft.)	37,794	155,495	2,023	3,405	9,790	32,423	88,037
Republican vote	49	15	27	39	48	60	69

Note: Observations are store-weeks. There are 2,085,180 same-industry-county control observations, except for store size, which has 2,084,697 observations. For the related-brand controls, there are 6,542,820 observations, except for store size, which has 6,537,418 observations. 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. *Store area* is SafeGraph's estimate of each store's square footage. *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.** Correlation coefficients

## A. Activists and same-industry-county control stores

Variable	Activist	Store visits	Store area
Store visits	0.67		
Store area	0.62	0.75	
Republican vote	0.05	0.09	0.09

## B. Activists and related-brand control stores

Variable	Activist	Store visits	Store area
Store visits	0.19		
Store area	0.19	0.69	
Republican vote	0.03	0.02	0.03

Note: Observations are store-weeks. See note to Table 1.

**Table 3.** Summary statistics for activist and non-activist (control) stores

A. Activists and same-industry-county control stores

Variable	Non-activist (N = 29,029)	Activist (N = 5,724)	Difference	t-stat
Store visits	891 (1,161)	6,454 (4,782)	-5,563	-87.5
Store area (sq. ft.)	16,278 (28,161)	165,403 (161,312)	-149,125	-69.7
Republican vote (%)	48 (15)	50 (15)	-1.98	-9.4

B. Activists and related-brand control stores

Variable	Non-activist (N = 103,323)	Activist (N = 5,724)	Difference	t-stat
Store visits	1,600 (5,437)	6,454 (4,782)	-4,854	-74.2
Store area (sq. ft.)	30,718 (152,066)	165,403 (161,312)	-134,685	-61.7
Republican vote (%)	49 (15)	50 (15)	-1.77	-8.9

Note: Observations are store averages for the 2019 pre-activism period. Numbers report means across stores with standard deviations in parentheses. See notes to Table 1 for variable definitions.

**Table 4.** Difference-in-differences estimates using the same-industry-county control group

	Weeks [-10, 4]				Weeks [-10, 10]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post-activism	-0.096 (0.003)	-0.033 (0.003)	0.004 (0.011)		0.002 (0.003)	-0.017 (0.011)	
Post-activism × Republican vote			-0.001 (0.000)			0.000 (0.000)	
Very Liberal				0.003 (0.012)			0.008 (0.012)
Liberal				-0.032 (0.005)			-0.008 (0.005)
Conservative				-0.037 (0.004)			0.009 (0.004)
Very Conservative				-0.055 (0.007)			0.009 (0.007)
Seasonality controls		●	●	●	●	●	●
Observations	521,295	1,563,885	1,563,885	1,563,885	2,085,180	2,085,180	2,085,180
Adjusted-R <sup>2</sup>	0.97	0.96	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; Models 5–7 cover the 10 weeks before and 10 weeks after the activism. Model 1 uses data from 2019, while Models 2–7 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 5.** Difference-in-differences estimates using related-brand control group

	Weeks [-10, 4]			Weeks [-10, 10]		
	(1)	(2)	(3)	(4)	(5)	(6)
Post-activism	-0.040 (0.002)	-0.029 (0.009)		0.002 (0.002)	-0.036 (0.009)	
Post-activism × Republican vote		-0.000 (0.000)			0.001 (0.000)	
Very Liberal			-0.022 (0.010)			-0.007 (0.010)
Liberal			-0.041 (0.004)			-0.007 (0.004)
Conservative			-0.042 (0.003)			0.008 (0.003)
Very Conservative			-0.044 (0.006)			0.022 (0.006)
Seasonality controls	●	●	●	●	●	●
Observations	4,907,115	4,907,115	4,907,115	6,542,820	6,542,820	6,542,820
Adjusted-R <sup>2</sup>	0.96	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 4–6 cover the 10 weeks before and 10 weeks after the activism. See note to Table 4 for variable definitions. Standard errors in parentheses are clustered by store.

**Table 6.** Alternative measures of political ideology using same-industry-county controls

	2016 presidential election		American Ideology Project	
	(1)	(2)	(3)	(4)
Post-activism	0.004 (0.010)		-0.032 (0.003)	
Post-activism × Republican vote	-0.001 (0.000)			
Conservative ideology			-0.029 (0.010)	
Very Liberal		-0.006 (0.011)		-0.003 (0.011)
Liberal		-0.031 (0.005)		-0.033 (0.006)
Conservative		-0.035 (0.005)		-0.037 (0.005)
Very Conservative		-0.059 (0.006)		-0.044 (0.005)
Seasonality controls	●	●	●	●
Observations	1,563,885	1,563,885	1,561,230	1,561,230
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 and 2 and based on *Conservative ideology* for Models 3 and 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 and 2 and models 3 and 4 due to missing data. Standard errors in parentheses are clustered by store.

**Table 7.** Gun sales and consumers' responses to CEO activism

	Net Effect (1)	Republican Vote (2)	Ideology Project (3)
Post-activism	-0.039 (0.003)		
Post-activism × Gun sales	-0.004 (0.004)		
Very Liberal		-0.006 (0.012)	-0.015 (0.011)
Liberal		-0.038 (0.005)	-0.041 (0.006)
Conservative		-0.042 (0.006)	-0.040 (0.006)
Very Conservative		-0.072 (0.011)	-0.049 (0.007)
Post-activism × Gun sales × Very Liberal		-0.058 (0.031)	-0.047 (0.032)
Liberal		-0.003 (0.007)	-0.002 (0.009)
Conservative		-0.001 (0.006)	0.000 (0.007)
Very Conservative		0.020 (0.010)	0.002 (0.007)
Seasonality controls	●	●	●
Observations	1,318,365	1,318,365	1,318,365
Adjusted-R <sup>2</sup>	0.96	0.96	0.96

Note: All models include store and week-industry-county fixed effects. *Gun sales* is a store-level indicator for whether a store is licensed to sell guns. In Column 2, *Very Liberal*, *Liberal*, *Conservative*, and *Very Conservative* are defined as in Table 4. In Column 3, we use the ideology-based measure from Table 6. Sample is restricted to industries and states in which at least one activist store sells firearms. Standard errors in parentheses are clustered by store.

**Table 8.** Estimates of four-week change in net sales (in millions of U.S. dollars)

	County Population	Store Area
Walmart	-\$826	-\$1,059
Dick's Sporting Goods	-\$18	-\$22
Gap	-\$20	-\$28
Levi Strauss	-\$5	-\$6

**Table 9.** Estimates using matched same-industry-county control stores

	Store Area (1)	Distance (2)	Exclude Nearby (3)
Post-activism ×			
Very Liberal	0.011 (0.024)	-0.003 (0.018)	0.001 (0.013)
Liberal	-0.030 (0.009)	-0.027 (0.007)	-0.033 (0.005)
Conservative	-0.040 (0.008)	-0.035 (0.006)	-0.037 (0.005)
Very Conservative	-0.061 (0.011)	-0.050 (0.010)	-0.051 (0.009)
Seasonality controls	●	●	●
Observations	391,905	480,690	1,219,680
Adjusted-R <sup>2</sup>	0.980	0.979	0.965

Note: Activists are matched to control stores in the same industry and county. *Store Area* (Column 1) uses the closest control to the activist in terms of store area. *Distance* (Column 2) uses the closest control in terms of geographic distance. *Exclude Nearby* (Column 3) uses control stores that are *not* the geographically closest store to the activist and at least five miles from all activists. Estimates are based on Equation (1) and use the four weeks following activism. Standard errors in parentheses are clustered by store.



**Table 10.** Estimates excluding potential outliers

	90 <sup>th</sup> Percentile (1)	$k = 1.5$ (2)	$k = 2$ (3)
Post-activism ×	0.020	0.002	0.003
Very Liberal	(0.017)	(0.012)	(0.012)
	-0.033	-0.033	-0.033
Liberal	(0.007)	(0.005)	(0.005)
	-0.037	-0.038	-0.037
Conservative	(0.007)	(0.004)	(0.004)
	-0.050	-0.057	-0.055
Very Conservative	(0.014)	(0.008)	(0.007)
	0.020	0.002	0.003
Seasonality controls	●	●	●
Observations	1,407,465	1,553,580	1,562,940
Adjusted-R <sup>2</sup>	0.934	0.961	0.962

Note: Models mirror those in Column 4 of Table 4 using the same-industry control group but exclude outliers on the dependent variable. Column 1 defines outliers using the 90<sup>th</sup> percentile of the dependent variable. Columns 2–3 use Tukey’s (1977) method with the parameter  $k$  controls the permissibility of extreme values (see Appendix text). Standard errors in parentheses are clustered by store.