Corporate Political Positioning and Sales: Evidence from a Natural Experiment

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Abstract

We study if and how corporate political positioning affects retail sales. We use data from a large U.S.-based specialty retail brand and a similar control brand before and after an involuntary revelation of the focal brand's political position. We find that the total sales dollar amount and quantity of the focal brand increase by 17.1% and 12.7% after the event relative to the control brand. Further, sales increase more in places where the local political preference aligns more with the focal brand's position. We also find that the change in the customer base rather than basket size drives the measured effect, suggesting an informative function of corporate political positioning by communicating the brand's political ideology to potential consumers who share a similar ideology. Furthermore, the results are driven by changes in the sales of conspicuous rather than inconspicuous products. This observation is consistent with consumers' use of consumption as a signaling device to communicate their political ideologies to others.

Keywords: corporate political positioning (CPP), political consumption, political ideology, marketing communication, natural experiment, signaling

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1. Introduction

On June 12, 2020, amid the push for local law enforcement agencies to be defunded during the nationwide “Black Life Matters” movement (Andrew 2020), Egard Watches, a luxury watch brand, launched a pro-police advertisement on YouTube, which soon generated millions of views (Kaplan 2020). This is not the first time companies have been publicly associated with one side of a divisive political or social issue, defined as corporate political positioning (CPP)[1] (Wettstein and Baur 2016). Other notable examples of CPP include Chick-fil-A’s donation to anti-gay groups (Edwards 2012), Nike signing Colin Kaepernick (a controversial former quarterback in the National Football League known for his protests during the national anthem) as a spokesperson for its brand (Gregory 2018), and Nordstrom dropping the Ivanka Trump clothing line (Abrams 2017).

The view on firms taking a political stand has been controversial. Traditionally, many believe that having an association with politically charged issues is too risky for their brands (CMO Survey 2020). Michael Jordan had been infamously linked to the quote, “Republicans buy shoes too” (Smith 1995). The recent trend of companies having a political perspective represents a departure from the traditional view that companies should avoid political debates and focus on maximizing profits (Friedman 1970). A company’s political perspective can become public knowledge through many avenues, such as political donations, advocacy, and revelation by third parties. For some companies, CPP may positively impact their reputation or even brand value, such as a surge in stock price for Nike after announcing its plan to feature Kaepernick in its campaign (Gibson 2018). For others, they may suffer from media backlash and protests, as in the case of Chick-fil-A (Edwards 2012). Thus, it is unclear how a company’s sales are affected when its position on political issues is revealed to the public. What may drive consumers to respond to

[1] Corporate political positioning is sometimes also termed as corporate political advocacy in the literature.

CPP, which seemingly has no direct effects on the economic benefit and cost of purchasing from a brand?

The main goal of this study is to assess the effect of CPP on sales in a quasi-experimental setting. We focus on a large U.S.-based specialty retail brand that revealed its political stance involuntarily. In May 2011, this brand's donation to a conservative Republican politician became public knowledge through a tweet by one of the most followed celebrities. Once revealed, the news generated much attention on social media (e.g., Twitter, Facebook) and was reported by national newspapers. In this natural experiment, we define the focal brand as the treated brand and use another similar brand (that the event had little impact on) of the same parent company as the control.

Using data before and after the event, we examine what happens to sales after the revelation of the treated brand's political stance. We find that the change in total sales dollar amount (quantity) after the event is 17.1% (12.7%) higher for the treated brand than the control brand, suggesting a positive main effect of CPP on sales. Furthermore, after allowing the effect of CPP to vary by local political preference, we find that consumers' reactions to CPP are dependent on the degree to which the local political preference aligns with the brand's political stance. Total sales increase more in locations where the local political preference aligns with the brand's position and can decrease in locations where the local political preference misaligns with the brand's position. We measure local political preference by calculating the difference in local votes (in percentages) that went to Republican candidate Mitt Romney and Democratic candidate Barak Obama in the 2012 U.S. presidential election. The effect size of CPP varies substantially across counties: the change in sales ranges from –12.7% to 91.5% with a mean of 28.4% for sales dollar amount, and ranges from –11.3% to 68.6% with a mean of 21.5% for sales quantity.

We then explore the underlying mechanisms through which the effect of CPP on sales operates. First, we demonstrate that changes in total sales are driven by shifts in the customer base rather than shifts in the basket size: new customers acquisition follows the same pattern as what we find in sales, but the average spending per customer does not change much after CPP. This finding suggests that CPP plays a specific marketing communication role: it informs consumers of the political ideologies of a brand so that they can more easily identify the brand that shares their political views.

Second, we provide suggestive evidence that CPP allows consumers to use purchases as a signaling device to show their political views to others. By examining the attributes of products purchased, we find that only sales of conspicuous products increase in places where consumers' political preference aligns with the brand's position. This observation is more consistent with the identity-signaling motivation than the motivation that consumers increase purchases of the treated brand purely because of their needs for identity consistency, as the latter predicts no strong difference in treatment effects between conspicuous and inconspicuous products.

Research that directly links CPP to consumer purchases (also referred to as political consumption; see Shah et al. 2007) is largely absent in the literature. Only a handful of studies have empirically examined the effect of CPP on either consumer or investor responses. Using surveys and lab experiments, Korschun et al. (2019) find that consumers tend to purchase more from a market-driven company that analyzes and adapts to the market but less from a value-driven company that stays true to its values when the company abstains from taking a political stand. Also using lab experiments, Hydock et al. (2020) find that a brand's market share moderates the effect of CPP on consumer choices. Bhagwat et al. (2020) focus on firm value and show that, in general, investors react negatively to CPP. Copeland and Boulianne (2022) conduct a meta-analysis to provide theories explaining why consumers engage in political consumption. Independent of this research, Liaukonyte et al. (2022) also examine the consequence of a brand taking a political stance. However, they focus on the effect of social media boycotts and do not explore the possible behavioral mechanisms.

Our research contributes to the political consumption literature in several ways. First, to our knowledge, this study is one of the first to empirically relate CPP to brand-level sales. Compared with previous studies, we use consumers' actual purchases and spending rather than self-reported data to quantify the effect of CPP. Second, we identify the effect of CPP utilizing an exogenous shock related to the revelation of a brand's political position, which alleviates the endogeneity concern that brands might strategically take a political stand in expectation of favorable reactions from some consumers. As such, our estimated effects of CPP are more likely to be causal than correlational. Third, we suggest a possible behavioral mechanism that drives the shift in sales after CPP.

2. Setting and Data

To assess the effect of CPP on sales, we use data provided by a large U.S.-based specialty retailer (which prefers to be anonymous). This data set is suitable for the study of CPP for two reasons. First, the data period covers time both before and after the exogenous event related to the company's political stance. On May 26, 2011, an influential celebrity with a large number of Twitter followers tweeted that one major brand of this company had made political donations to a conservative Republican politician, who later ran for the Republican presidential nomination in

2012.[2] This tweet quickly spread on social media and was featured in several mainstream news outlets such as the *Washington Post* and *Business Insider*, which generated nationwide attention. From the consumers' perspective, the political donation effectively established consumers' perception of the brand's political positioning. This event provides a relatively clean empirical setting to examine the effect of CPP because the event was less likely driven by the expected consumer responses, which can give rise to endogeneity problems.

Second, the company owns multiple brands that operate in the same industry. As the company purposefully makes the connections between the brands discrete, consumers are likely unaware of the relationship between different brands under this company. We also surveyed 81 undergraduate students at a large public university (part of the company's primary target audience) by asking questions related to brand awareness. We found that 65 out of the 81 undergraduate students were aware of both the focal and control brands. Only 2 out of 65 (3.1%) knew that the two brands were under the same parent company. The survey confirmed that the connection between the two brands is largely unknown, so this event likely only affected sales of the focal brand because the celebrity mentioned only this particular brand in the tweet.[3] Thus, our data on sales from multiple brands allow us to use another brand as a control in this research. The selected control brand sells in the same product categories as the treated brand. While both brands primarily target the market of young adults, the two brands differ in aesthetics and the lifestyle they represent. According to the parent company, they regularly monitored the customers' perceptions of the two

[2] Although we were unable to retrieve the exact number of followers at the time of the event, this celebrity had 45 million followers and was among the top 30 most followed Twitter accounts in 2020.

[3] To better illustrate our empirical setting, we can consider an analogy in the luxury fashion industry. LVMH hosts a house of brands, such as Louis Vuitton, Dior, and Fendi. Although under the same parent company, consumers are likely unaware of the associations of these brands. Thus, if Louis Vuitton makes a political statement, it is unlikely to affect customers of Dior and Fendi.

brands and adjusted the branding strategies to mitigate the potential cannibalization. This provides some assurance that consumers do not necessarily view the two brands as direct competitors.

We also investigated the treated brand's official blogs to examine whether the corporate team had made any public relations effort to control the "damage" of the seemingly negative incidence of CPP. By reading through each of the 78 blogs released by the treated brand from May 2011 to August 2011 on its archive website, we failed to find any information related to the celebrity's tweet or the political donation.[4]

Both the treated and control brands are vertically integrated in that each brand designs and produces its own in-house branded products and then sells these products in its own retail stores. The retail stores carry a variety of products, including clothes, shoes, accessories, and housewares. Some of these products are conspicuous, as their brand logos are publicly visible to others at the time of consumption (e.g., clothes). In contrast, other products are inconspicuous, as they are often consumed in private spaces (e.g., housewares). We used three external coders to code each product category into conspicuous and inconspicuous products. The difference in sales between conspicuous and inconspicuous products can help test the underlying motivations of consumer actions, as we show later in Section 4.

The main data set we use contains weekly sales of the two brands from 1,459 counties between the second week of March 2011 and the second week of August 2011, which covers the eleven weeks before and after the event. We choose the data window that ends in the middle of August to alleviate concerns about two potential confounders, back-to-school sales and new product launches for the fall/winter season, both of which are not directly observed in our data. The data provided by the company is compiled from purchases made by a subset of randomly

[4] We are unable to report the url of the blog archive for the sake of brand anonymity. Most blogs were related to fashion trends, product promotion, and health (e.g., workout guide) catering to the young audience.

sampled U.S. customers. Hence the sales figures in the data are representative of the unsampled sales, though the actual magnitudes differ. We have information on the number of items purchased and sales amount in U.S. dollars. We also know the number of customers who made a purchase each week, as well as the number of new customers who made their first recorded purchases with the brand in a given week.

We use the votes for the two candidates in the 2012 U.S. presidential election (MIT Election Data and Science Lab 2018) to create a proxy for the local political preference in each county during our data period. In particular, we measure the local political preference using the difference between the percentage of voters who voted for Mitt Romney and the percentage of voters who voted for Barack Obama in a location. The larger the vote share gap, the more votes went to Mitt Romney in the presidential election, suggesting that the location is leaning more toward "red." Conversely, a smaller vote share gap indicates that the location is leaning more toward "blue."[5] The gap in vote share ranges from –0.84 to 0.88, with a mean of 0.04 across counties.

We use a variety of outcomes as dependent variables to assess the effect of CPP. For each brand–county–week, we use total sales (in both quantity and revenue), average sales per customer, and the number of new customers. We also include multiple controls in our analyses. First, we control for direct marketing communications sent by the firm to customers in the sample. We measure direct marketing effort by counting the total number of both online and offline direct mailings sent by each brand to customers in a county. Second, we control for local physical store presence. For each county, we create the variable "# of retail stores" using the total number of offline stores within the county. Third, we control for the competition effect. We collected store

[5] In U.S. politics, "red" represents the Republican Party and "blue" represents the Democratic Party.

opening and closing data on six major competitor brands, not including the company's own brands. The firm identified the six major competitor brands as "brands that share a common customer base." Although we did not observe the exact number of competitors' stores, we collected the store opening data from the competitor websites and press releases mentioning store openings during the data period. We then create variables of the number of competitor stores opened or closed within each county each week as controls. Finally, our analysis also includes time and brand–county fixed effects. Table 1 provides descriptive statistics.

< Insert Table 1 about here >

3. Empirical Strategy and Results

3.1. Overview of Empirical Strategy

We explore how CPP affects total sales using a quasi-experimental design. We treat the Twitter incidence as the intervention, the focal brand as the treated brand, and use another brand owned by the same parent company as the control brand. We define the eleven weeks before the event (i.e., March 7, 2011, to May 22, 2011) as the pretreatment period and the eleven weeks after the event (i.e., May 30, 2011, to August 14, 2011) as the posttreatment period. As the event did not occur at the beginning of a week (May 26, 2011, Thursday), we define the week of the event (May 23, 2011, to May 29, 2011) as the pretreatment period.[6]

The unit of observation in the analysis is at the brand–county–week level. We compare the change in sales of the treated brand before and after the treatment with that of the control brand using fixed-effects Poisson regression (Hausman et al. 1984).[7] We choose this model specification

[6] We define a calendar week as a week beginning with Monday and ending with Sunday. We have also conducted analyses when treating the week of the event as the posttreatment period and found results almost unchanged.
[7] We show the robustness of the main findings to alternative model specifications in the Online Appendix, where we also present the support for parallel trends assumptions, additional robustness checks, and falsification tests.

for a number of reasons, including that it is particularly useful for nonnegative but skewed data (Azoulay et al. 2010; Wang and Goldfarb 2017).

3.2. Main Effects

To identify the main effects of the brand's political positioning on sales, we compare the change in sales of the treated brand after the event with that of the control brand. Specifically, we estimate the following fixed-effects Poisson model to assess the average treatment effect of CPP on sales:

$$y_{blt} \sim \text{Poisson}(\mu_{bl} \exp(\beta Treated_b \times Post_t + X_{blt}\theta + \tau_t)), \quad (1)$$

where $Post_t$ is a dummy variable for the posttreatment period, which takes the value of 1 if $1 \leq t \leq 11$ and 0 if $-11 \leq t \leq 0$, μ_{bl} represents the brand-county fixed effects, and τ_t represents the weekly fixed effects that capture seasonality. We define the week of the event as $t = 0$. Because we include both the brand–county and the weekly fixed effects, the coefficients of $Treated_b$ and $Post_t$ are unidentifiable. We therefore exclude these two covariates from Equation (1). Our main coefficient of interest is β, which captures the average treatment effect across all locations.

Columns 1 and 2 in Table 2 report the estimation results of Equation (1) when using sales dollar amount and sales quantity as the dependent variable, respectively. The estimated coefficient β is significantly positive in both regressions, suggesting a positive average treatment effect of CPP on sales after controlling for potential confounders. The Poisson model specification suggests that the sales dollar amount of the treated brand increases by 17.1% ($\exp(0.158) - 1 = 0.171$), and the sales quantity of the treated brand increases by 12.7% ($\exp(0.120) - 1 = 0.127$) relative to the control brand after the event.

< Insert Table 2 about here >

3.3. Heterogeneous Effects Moderated by Local Political Preference

Next we investigate whether and how political preference moderates the effect of CPP on local consumer purchases. We investigate this moderating effect by including the vote share gap between Romney and Obama in the 2012 presidential election and its interactions with $Treated_b$, $Post_t$, and $Treated_b \times Post_t$ in the fixed-effects Poisson model as follows:

$$y_{blt} \sim \text{Poisson}(\mu_{bl} \exp(\beta Treated_b \times Post_t + \gamma Post_t \times Votegap_l + \delta Treated_b \times Post_t \times Votegap_l + X_{blt}\theta + \tau_t)), \qquad (2)$$

where $Votegap_l$ represents the vote share gap in county l. Because of the inclusion of brand–county fixed effects, the coefficients of $Votegap_l$ and $Treated_b \times Votegap_l$ are not identifiable, and therefore the associated covariates do not appear in Equation (2).

Columns 3 and 4 in Table 2 report the estimated results of Equation (2), where y_{blt} is sales dollar amount and sales quantity, respectively. In line with the findings in the first two columns in which we treat all locations homogeneously, we find that consumers indeed react to the brand's political stance by changing their purchase behavior. Furthermore, the direction and magnitude of the change depend on the degree of alignment of local political preference with the political party supported by the brand. As the treated brand was revealed to support the Republican Party, the positive and statistically significant coefficients of the three-way interactions (δ) among $Treated_b$, $Post_t$, and $Votegap_l$ suggest that the higher the support rate to the Republican candidate, the larger is the increase in sales of the treated brand than that of the control brand after the event. For the control brand, the estimated coefficients of $Post_t \times Votegap_l$ (γ) indicate that there is no significant difference in the change of sales across locations with different political preferences after the event, supporting the assumption that the control brand was not significantly affected by the event.

To gauge the effect size, we use the estimated parameters from the fixed–effects Poisson model specification to calculate changes in sales for each county where voting data are available. For a location l, a back-of-the-envelope calculation suggests a change of ($\exp(0.248 + 0.459 \times Votegap_l) - 1$) in expected sales dollar amount and a change of ($\exp(0.194 + 0.375 \times Votegap_l) - 1$) in expected sales quantity. In Figure 1, we illustrate the distribution of $Votegap$ across locations (bars) and the full range of effect sizes against $Votegap$ (lines). The solid and dotted lines represent the change in sales dollar amount and sales quantity, respectively. We find substantial heterogeneity in the effect of CPP across counties: the change in sales after the event ranges from –12.7% to 91.5% with a mean of 28.4% for sales dollar amount, and ranges from –11.3% to 68.6% with a mean of 21.5% for sales quantity. These results suggest that CPP leads to increased purchases (i.e., buycotts) by consumers who share the same political ideology with the brand and possibly reduced purchases (i.e., boycotts) by those who share the opposing political ideology. In addition, as the effect is significantly positive in a neutral location where $Votegap = 0$ (as suggested by the estimated coefficients of $Treated_b \times Post_t$ in columns 3 and 4 in Table 3), the positive effect on sales due to supporters' buycotts outweighs the negative effect due to opposers' boycotts.

< Insert Figure 1 about here >

3.4. Effects over Time

Figure 2 visualizes the weekly difference in sales between the treated and control brands over the entire data period. The estimated coefficients show that total sales generally increase in the first five weeks after the treatment and revert to the pretreatment level from week 6 onwards. As such, the effect of CPP on total sales mainly occurred in the first month after the event.

< Insert Figure 2 about here >

We also present the estimation results for red and blue counties separately. We use slightly coarser time intervals to summarize the time-varying treatment effects parsimoniously. Consistent with the pattern in Figure 2, we consider the first five weeks after the treatment as the short-term effect, and weeks 6 onwards as the long-term effect. In Table 3, on average, both red and blue counties experience an increase in sales in the short term despite a more substantial increase in red than blue counties, suggesting that the effect of CPP is the most prominent in the month after the event. In addition, the long-term effect of CPP only exists in red counties, as the treatment effects after week 5 are still positive and marginally significant (at the 0.10 level) in red counties but not in blue counties.

It is worth noting that our estimation results suggest relatively small negative effects (in relation to the positive effects) linking a brand's political stance to its sales. For instance, the estimated coefficients suggest that in a hypothetical county where $Votegap = 0$, the effect on sales is positive. Although we are not able to directly test why this happens, we offer some possible explanations. To understand where the negative effects on sales come from, we first need to understand the source of the effects. While the positive effect of CPP can come from both existing customers (who may purchase more) and newly acquired customers (who bring in additional sales), the negative effects can only come from the existing customers who decide to spend less with the brand or stop buying completely. The estimated negative effects hence depend on both the political composition of the existing customer base and their reactions to the brand's CPP event. If most existing customers are leaning towards the Republican Party, the proportion of customers who may react negatively towards the brand's CPP event would be much smaller than the case in which existing customers were mainly Democratic-leaning. Thus, the dominance of Republican-leaning existing customers might explain the dominance of the positive effect of CPP. Another plausible explanation is that the positive and negative reactions from consumers are asymmetric: the positive effect on sales of supporters may outweigh the potentially negative effect on sales of opposers. There is some preliminary evidence supporting this asymmetric effect. A recent study showed that consumers who expressed their engagement in boycotting the brand (because of the brand's political stance) did not actually purchase less than those who did not support the boycott (Eyler-Driscoll 2019), suggesting that the negative effect of CPP through boycotts could be small.

< Insert Table 3 about here >

4. Mechanisms

We explore the underlying mechanisms through which the effect of CPP on sales operates. We first show that changes in sales are driven by shifts in the customer base rather than shifts in basket size. We then provide suggestive evidence that CPP enables consumers to use consumption as a signaling device to communicate their political identity to others. We do so by investigating the effect of CPP on sales between conspicuous and inconspicuous products.

As the size of the customer base and average spending per customer jointly determine total sales, we begin with an investigation of the key drivers of the change in total sales by estimating the effects of CPP on these two factors separately. To assess the impact of CPP on the change in customer base, we estimate Equation (2) using the number of new customers per week as the dependent variable, identifying new customers of each brand using a "first purchase" flag in the data set provided by the company. We also estimate the same model using sales outcome per customer as the dependent variable to understand whether customers tend to increase their basket size or spend more following a CPP event.

Table 4 shows the results from the analyses on new customer acquisition and average spending per customer. Column 1 shows positive and significant coefficients for $Treated_b \times$

$Post_t$ (0.153, $p < 0.01$) and $Treated_b \times Post_t \times Votegap_l$ (0.261, $p < 0.05$). These two coefficients combined indicate that for the treated brand, the positive effect of CPP on new customer acquisition is greater in locations where more people support the Republican candidate than the Democratic candidate. In columns 2 and 3, we show that neither the average number of purchased items nor spending per customer appears to change after CPP, regardless of the local political preference. Together, the results in Table 4 suggest that the change in the size of the customer base, not the change in spending patterns, drives the effect of CPP on sales. This finding suggests that CPP plays a specific marketing communication role by informing people whose political view matches the brand's position.

< Insert Table 4 about here >

What drives consumers to purchase after learning the brand's political position? There are two possible motivations behind consumers' purchase reactions to CPP (i.e., political consumption). First, CPP offers consumers a way to more easily find a company with which they identify (Bhattacharya and Sen 2003), and this alignment of political ideologies with a brand satisfies their intrinsic need to support their ideology (Shah et al. 2007). Conversely, a misalignment of political views may drive consumers away from the brand because of the poor fit. Second, purchases provide a way for consumers to signal their political ideologies to others (Kleine et al. 1993; Hydock et al. 2019). As the credibility of signaling is higher for a more costly signal (Spence 1973), the signaling value through purchases is arguably greater than less costly activities such as social media posting. Notably, both types of motivations are grounded in social identity theory (e.g., Brewer 1991; Tajfel and Turner 1985), which predicts that the ordinary products consumers use in day-to-day life enable them to enact and express their various social identities (Kleine et al. 1993), including political identities.

We investigate these two possible motivations by examining product attributes to determine how the effect of CPP differs between conspicuous products (whose consumption typically occurs in public places and therefore is visible to others) and inconspicuous products (whose consumption typically occurs privately and therefore is invisible to others). If the identity-signaling function of CPP is at play, we expect a stronger effect of CPP for the sales of conspicuous products than that from inconspicuous products sales in areas where the need for identity-signaling is strong. However, if the intrinsic need to support political ideologies is the primary motivation, we expect the effects of CPP to be similar between the two types of products.

Table 5 examines the role of product attributes. Results show that sales of conspicuous products increase when the brand's position resonates with the local political preference, as suggested by the positive and significant coefficients of $Treated_b \times Post_t \times Votegap_l$. Nevertheless, for inconspicuous products, we only find a positive average treatment effect of CPP on sales, which can be explained by the increased brand awareness caused by the media coverage of the event. We do not find a significant association between the sales of inconspicuous products and local political preference. This observation is more consistent with the prediction from identity-signaling motivation than from intrinsic-need-satisfaction motivation. Remarkably, the distinction in the three-way interaction effects between conspicuous and inconspicuous products is robust to the measure of sales (dollar amount and quantity).

< Insert Table 5 about here >

5. Conclusion and Discussions

As companies increasingly take stands on divisive political or social issues, there is a pressing need to understand whether and how CPP affects consumer purchases. In this research, we use data before and after an exogenous event that reveals a brand's political position to examine the effect of CPP on sales. On average, we find that CPP leads to a significantly positive change in sales for the focal brand relative to the control brand, suggesting a positive main effect. However, the sales impact is mainly prominent in the first month after the event. Furthermore, we find that sales increase in areas where local consumers' political preference aligns with the brand's position and can decrease in areas where local consumers' preference misaligns with the brand's position. This result suggests a moderating effect of the degree to which political preference aligns with the brand's position on the effect of CPP.

Our findings suggest a possible mechanism through which CPP affects sales. CPP informs consumers of the political ideology of a brand so that they can more easily find brands that share their political views. CPP also enables consumers to use the subsequent consumption from this brand to signal their political ideologies to others.

This paper provides several important implications for marketing managers. Taking a political stance does not seem to prompt customers to spend more. It is, however, instrumental in increasing the exposure of the brand and helping acquire the "right customers" – those who have the same political belief as the brand. Interestingly, while it is true that taking a side on divisive political issues may repulse some customers, especially in locations where most consumers prefer the opposite of the company's political position (i.e., blue counties in our context), our analyses provide suggestive evidence that the positive effect is more pronounced than the negative effect on product sales. Finally, while the long-term effect of CPP on sales is not significant within our data range, managers should still be mindful of the potential change in brand equity through the change in customer base.

Although we use actual sales data to examine CPP, these data are from one company only, and therefore the estimated effect size is dependent on our specific data set. Despite the limitation, we take the first step toward identifying the effect of CPP on sales in this research. We hope this study invokes further interest in exploring this increasingly important topic at the intersection of business and politics.

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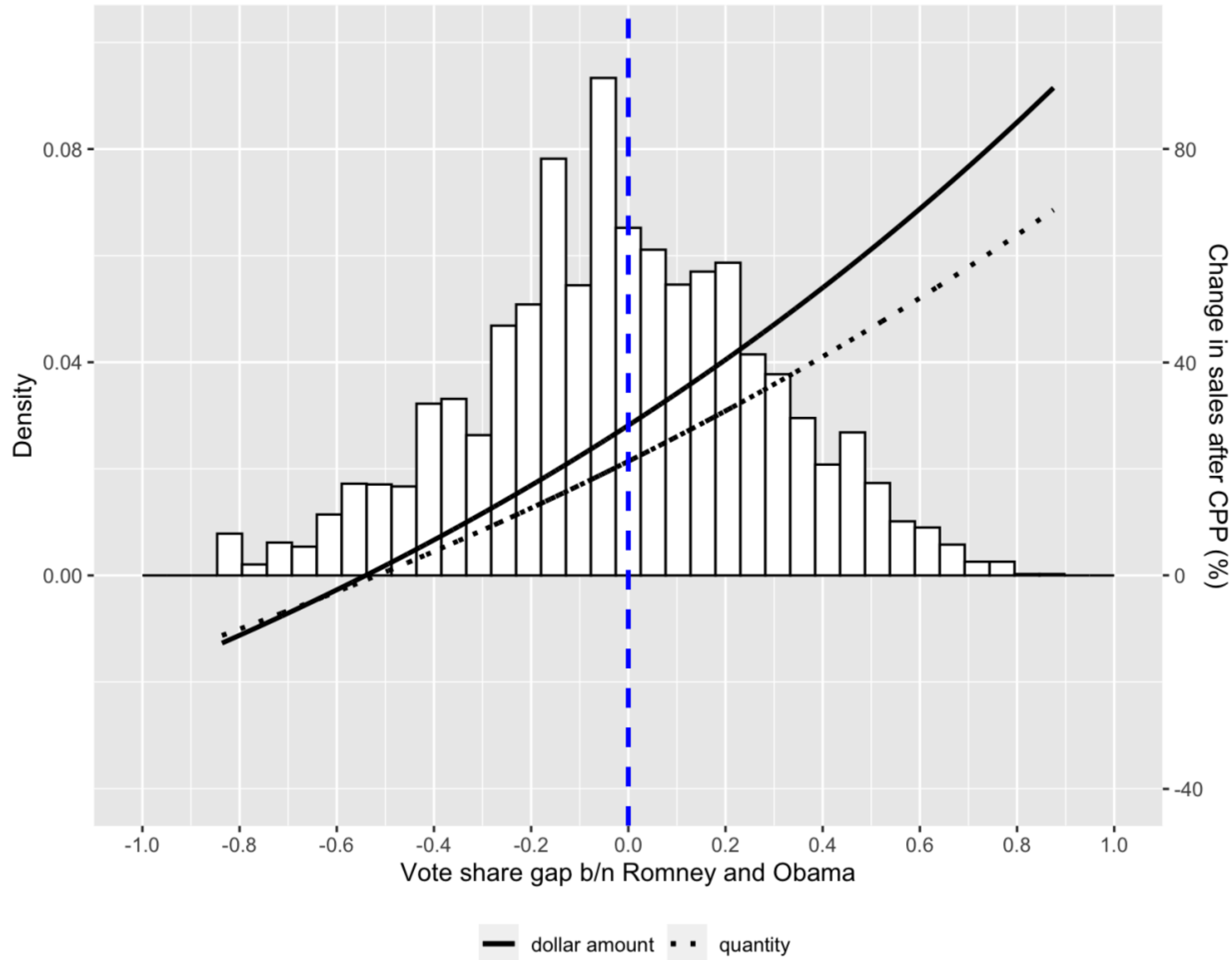
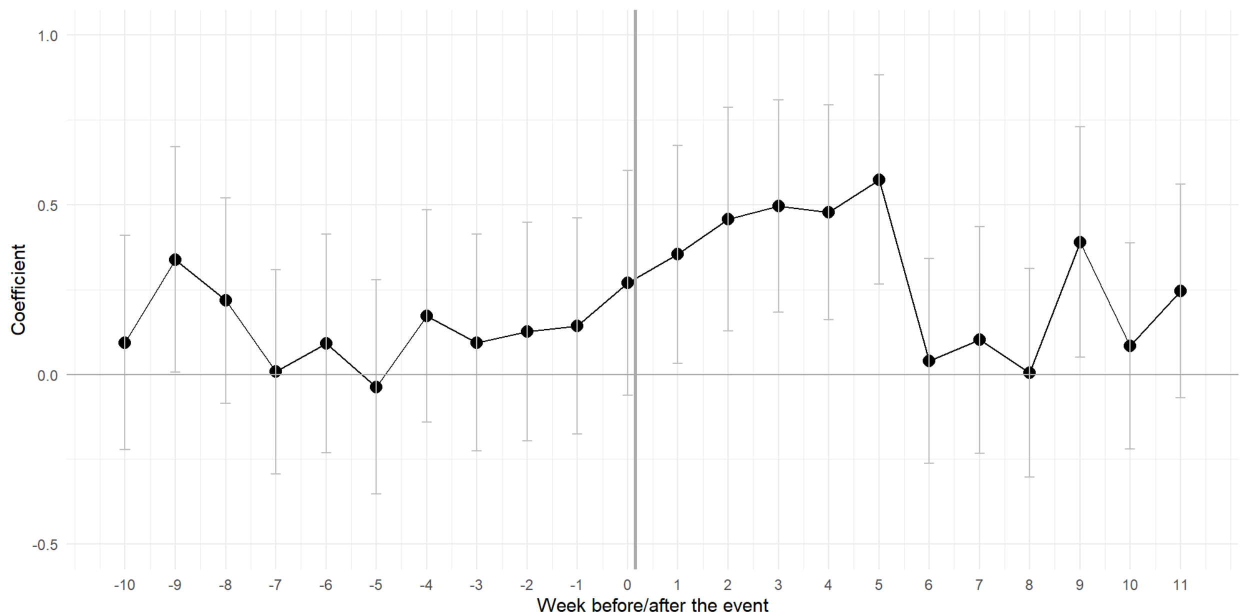
Figure 1. Histogram of Local Political Preference and Its Relationship with Effect Sizes



Notes. The bar plot shows a histogram of the vote share gap between Romney and Obama across counties. The solid and dotted lines show the change in sales dollar amount and sales quantity, respectively, after CPP. The dashed vertical line represents counties where the vote share gap is 0. The left Y-axis represents the probability density of the histogram and the right Y-axis represents effect sizes of the CPP event.

Figure 2. Change in Sales Dollar Amount before and after the Event



Notes. The solid lines show the coefficient estimates that capture the average difference in sales between the treated and control brands over time relative to the baseline of 11 weeks prior to the event. The error bars represent the 95% confidence intervals of the estimates. Standard errors are clustered at the brand–county level. The vertical line represents the week of the event.

Table 1. Summary Statistics by Brand–County–Week

	Mean	Std. Dev.	Min	Max
Total sales ($)	108.76	428.60	0	11,082
Total sales (qty)	3.10	12.92	0	401
# of new customers	0.20	0.60	0	10.33
Total sales per customer ($)	106.52	121.60	0	5,895
Total sales per customer (qty)	2.83	2.30	0	54
Conspicuous sales ($)	56.87	256.43	0	7,108
Conspicuous sales (qty)	1.43	7.30	0	241
Inconspicuous sales ($)	51.89	188.45	0	4,410
Inconspicuous sales (qty)	1.67	6.03	0	160
Vote share gap b/n Romney and Obama	0.04	0.30	-0.84	0.88
# of direct marketing communications	157.28	509.99	0	14,097
# of retail stores	12.42	26.80	0	84
# of competitor store open	0.07	0.63	0	6
# of competitor store close	0.01	0.13	0	2

Notes. Unit of observation is brand–county–week. The statistics for total sales per customer and online sales per customer exclude incidences of both zeroes in sales and the number of customers.

Table 2. CPP, Local Political Preference, and Sales

	Main effect		With local political preference	
Dependent variable	**Sales ($)**	**Sales (qty)**	**Sales ($)**	**Sales (qty)**
Treated brand × Posttreatment ($\boldsymbol{\beta}$)	0.158*** (0.050)	0.120*** (0.044)	0.248*** (0.065)	0.194*** (0.055)
Posttreatment × Vote share gap b/n Romney and Obama ($\boldsymbol{\gamma}$)			-0.234 (0.182)	-0.193 (0.156)
Treated brand × Posttreatment × Vote share gap b/n Romney and Obama ($\boldsymbol{\delta}$)			0.459** (0.152)	0.375** (0.168)
# of direct marketing communications	0.00002 (0.00002)	0.00001 (0.00002)	0.00005** (0.00002)	0.00004 (0.00003)
# of retail stores	0.014*** (0.003)	0.013*** (0.003)	0.014*** (0.003)	0.013*** (0.003)
# of competitor store open	-0.001 (0.009)	0.007 (0.008)	-0.003 (0.009)	0.004 (0.008)
# of competitor store close	0.021 (0.048)	0.032 (0.050)	0.028 (0.047)	0.041 (0.050)
Brand–county fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
# of observations	32,338	32,338	32,338	32,338
Log-pseudolikelihood	-1,501,651	-50,045	-1,499,820	-50,013

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 3. Effects of CPP over Time

	Red counties		Blue counties	
Dependent variable	**Sales ($)**	**Sales (qty)**	**Sales ($)**	**Sales (qty)**
Treated brand × Post treatment (week 1)	0.648*** (0.243)	0.633*** (0.227)	0.141 (0.117)	0.060 (0.106)
Treated brand × Post treatment (weeks 2-5)	0.501*** (0.136)	0.341*** (0.129)	0.335*** (0.062)	0.237*** (0.060)
Treated brand × Post treatment (weeks 6-11)	0.221* (0.132)	0.204* (0.111)	-0.036 (0.070)	-0.027 (0.063)
# of direct marketing communications	0.0001 (0.0001)	0.0001 (0.0001)	0.00003 (0.00002)	0.00002 (0.00002)
# of retail stores	0.007 (0.020)	0.021** (0.010)	0.013*** (0.002)	0.011*** (0.003)
# of competitor store open	-0.004 (0.014)	0.011 (0.014)	-0.001 (0.008)	0.004 (0.008)
# of competitor store close	0.244* (0.130)	0.280* (0.162)	0.003 (0.049)	0.013 (0.051)
Brand–county fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
# of observations	17,825	17,825	14,513	14,513
Log-pseudolikelihood	-646,455	-20,475	-845,262	-29,380

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 4. Change in Newly Acquired Customers and Sales per Customer

Dependent variable	**# of new customers**	**Sales per customer ($)**	**Sales per customer (qty)**
Treated brand × Posttreatment	0.153***	0.084	0.023
	(0.045)	(0.072)	(0.051)
Posttreatment × Vote share gap b/n Romney and Obama	-0.107	-0.123	-0.061
	(0.100)	(0.192)	(0.137)
Treated brand × Posttreatment × Vote share gap b/n Romney and Obama	0.261**	0.134	0.095
	(0.129)	(0.208)	(0.154)
# of direct marketing communications	0.00003	4.06e-6	0.00005
	(0.00004)	(0.00005)	(0.00004)
# of retail stores	0.016***	0.003	0.002
	(0.003)	(0.005)	(0.003)
# of competitor store open	-0.002	-0.004	0.002
	(0.016)	(0.009)	(0.009)
# of competitor store close	0.010	-0.004	0.021
	(0.110)	(0.062)	(0.043)
Brand–county fixed effects	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes
# of observations	29,118	9,473	9,473
Log-pseudolikelihood	-12,892	-234,386	-15,087

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table 5. Change in Sales of Conspicuous and Inconspicuous Products

	Conspicuous Product		Inconspicuous Product	
Dependent variable	**Sales ($)**	**Sales (qty)**	**Sales ($)**	**Sales (qty)**
Treated brand × Posttreatment	0.222***	0.246***	0.279***	0.168**
	(0.085)	(0.085)	(0.088)	(0.065)
Posttreatment × Vote gap b/n Romney and Obama	-0.137	-0.095	-0.325	-0.256
	(0.194)	(0.187)	(0.224)	(0.172)
Treated brand × Posttreatment × Vote gap b/n Romney and Obama	0.513**	0.438**	0.348	0.285
	(0.227)	(0.223)	(0.250)	(0.194)
# of direct marketing communications	0.00008**	0.00005	0.00003	0.00002
	(0.00004)	(0.00004)	(0.00003)	(0.00002)
# of retail stores	0.014***	0.015***	0.013***	0.012***
	(0.004)	(0.004)	(0.005)	(0.004)
# of competitor store open	0.001	0.015	-0.007	-0.005
	(0.012)	(0.010)	(0.010)	(0.009)
# of competitor store close	-0.004	0.024	0.064	0.054
	(0.063)	(0.065)	(0.050)	(0.051)
Brand–county fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
# of observations	23,552	23,552	30,314	30,314
Log-pseudolikelihood	-814,733	-26,463	-920,129	-35,148

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Online Appendix

A.1. Support for the Parallel Trends Assumption

A critical identifying assumption for the DID method is that control and treatment groups will have parallel trends in average outcomes in the absence of intervention. If this assumption fails, the control brand will not be a good counterfactual for the treated brand, and therefore the DID estimates will be biased. Although the parallel trends assumption is not directly testable, researchers usually have more confidence in its validity when they find that the average outcomes of the treated and control units follow a similar path in pretreatment periods. To do so, we directly compare the trends in pretreatment sales between the treated and control brands as follows:

$$y_{blt} \sim Poisson(\mu_{bl}\, exp(\beta t + \gamma t \times Treated_b + X_{blt}\theta)), \tag{A1}$$

where b denotes the brand, l denotes the county, t denotes the week, and y_{blt} refers to sales. Here, $t = -11, \ldots, 0$, which covers the pretreatment period (i.e., March 7, 2011, to May 29, 2011). The dummy variable $Treated_b$ takes the value of 1 for the treated brand and 0 for the control brand. Finally, X_{blt} is a vector of controls, including direct marketing communications and the presence of own brand's and competitors' local stores, and μ_{bl} captures brand–county fixed effects.

We conduct an extensive set of tests for the parallel trends, estimating Equation (A1) using both the dollar amount and sales quantity for multiple sales measures from the pretreatment period. Tables A1 and A2 present the estimation results, in which column 1 compares the pretreatment trends for total sales. Columns 2 and 3 report results for conspicuous and inconspicuous product sales. Finally, in columns 4 and 5, we test the parallel trends assumption for sales in red and blue counties separately, where we define a county as a red (blue) county if the vote share of Romney is higher (lower) than that of Obama in the 2012 presidential election.

As Table A1 and Table A2 show, none of the estimated coefficients of the interaction between trend and the treated brand (γ) are statistically significant. Thus, we fail to reject the null of parallel trends in these sales variables before the event.

Another set of tests that support the parallel trends in sales uses a series of weekly dummy variables instead of the time trend variable t in Equation (1). We plot the estimated coefficients associated with each week (β_t) to show how the effect of CPP on sales dollar amount changes over time in Figure 2 presented in the main text. None of the 11 pretreatment gaps in sales except one ($t = -9$) is statistically different from zero at the 0.05 level, providing additional support to the parallel trend assumptions in our context. Furthermore, a Wald test of the null hypothesis that all $\{\gamma_t\}_{t=-10}^{0}$ are jointly equal to zero cannot be rejected at the 5% level.

A.2. An Alternative Model Specification

The primary reason we used a fixed-effects Poisson model in the main analysis is its advantage in handling skewed data (Azoulay et al. 2010; Wang and Goldfarb 2017). We examine the extent to which our findings are driven by this model specification by considering an alternative specification of the DID model. Specifically, we re-estimate Equation (2) using log-linear models and report the results in Table A3. Consistent with Table 2, the coefficients of $Treated_b \times Post_t$ are all significantly positive, confirming a positive effect of CPP on sales in locations where consumers are indifferent between the two presidential candidates. The coefficients of $Treated_b \times Post_t \times Votegap_l$ are also positive and statistically significant, as we found in the fixed-effects Poisson regressions. These results suggest that the estimated treatment effect of CPP is not sensitive to the model specification of the DID analyses.

A.3. Improved Geographical Matching between the Control and Treated Brands

We include all counties in our data to avoid potential sample selection problems. However, the presence of physical stores is not always comparable between the treated and control brands across locations. Out of the 1,459 counties in our data set, 1,100 counties have no physical stores from either brand, and 159 counties have stores from both brands. The remaining 200 counties have stores from either the treated brand (195 counties) or the control brand (5 counties). To check whether our findings are sensitive to the geographical distributions of stores, we replicate Table 2 in Table A4 using a subsample of 1,259 counties in which the store availability is balanced between the two brands. Both the positive average treatment effects and the positive interaction effects with local political preferences remain, suggesting the robustness of our findings to the geographical distributions of stores.

A.4. Additional Analyses of Red vs. Blue Counties

Comparing to what we present in the main text, an alternative way to interpret the magnitudes of the effects across locations is to consider two alternative model specifications in which we focus on the differential treatment effects between red and blue counties. We first re-estimate Equation (2) by replacing $Votegap_l$ with a dummy variable for red counties denoted by Red_l, which equals 1 if $Votegap_l > 0$ and 0 otherwise. The coefficient of the three-way interaction ($Treated_b \times Post_t \times Red_l$) therefore measures the difference in changes in sales before and after the event between red and blue counties. Instead of using the pooled data, we also estimate Equation (1) using observations from red and blue counties separately. As such, the coefficients of $Treated_b \times Post_t$ measure the average treatment effect for red and blue counties, respectively.

Columns 1 and 2 in Table A5 show the estimation results when pooling all counties together. The positive and significant coefficients of the three-way interaction ($Treated_b \times$

$Post_t \times Red_l$) suggest that for the treated brand, the increase in sales in red counties is greater than that in blue counties after the event. But this is not true for the control brand: the nonsignificant coefficients of $Post_t \times Red_l$ suggest not much difference in the changes in sales between the red and blue counties. Columns 3 to 6 in Table A5 present the results when we separately estimate the treatment effects for red counties (columns 3 and 4) and blue counties (columns 5 and 6). The average treatment effects on both sales dollar amount and sales quantity are positive and significant for red counties. They are also larger than the effects for blue counties. These findings again confirm the moderating role of the match between consumers' political preferences and the brand's political stance.

A.5. Falsification Tests Using Other Moderators

We conduct two falsification tests to examine whether our identified moderator – local political preference – indeed drives the differences in treatment effects across counties. In particular, we consider two other possible moderators: the degree of urbanization of a location and the number of physical stores in a location. The first hypothesis relates to the conventional belief that Republican-leaning counties contain more rural areas and Democratic-leaning counties contain more urban areas. Thus, the differences in estimated treatment effects across locations are caused by the degree of urbanization rather than the local political preference. The second hypothesis is that red and blue counties systematically differ in the access to physical stores, and the stronger treatment effect found in red counties is driven by a larger number of physical stores in red than blue counties.

We first check the premises of these two hypotheses through correlation analyses. We measure the degree of urbanization of a county by first classifying each census tract as either urban or rural depending on whether its population meets the criteria of urban clusters defined by the U.S. Census Bureau in 2010,[8] and then calculate the percentage of rural tracts in a county (mean = 0.683). The correlation between the vote share gap and the degree of urbanization is positive but small (r = 0.108), providing some support to the premise of the first hypothesis. However, the negative correlation between the vote share gap and the number of physical stores (r = –0.193) suggests that the second hypothesis is unlikely to hold. We then conduct the falsification tests by re-estimating Equation (2) using each of the two potential moderators. As Table A6 shows, the coefficients associated with $Post_t \times X$ and $Treated_b \times Post_t \times X$ (where X represents the alternative moderator) are all statistically insignificant, suggesting that changes in sales we observe after CPP do not appear to be moderated by the degree of urbanization or the access to physical stores.

[8] https://www.census.gov/programs-surveys/geography/guidance/geo-areas/urban-rural/2010-urban-rural.html, accessed on March 31, 2022.

Table A1. Testing for the Parallel Trends Assumption for Sales Dollar amount

Dependent variable (all in $)	Total Sales (1)	Conspicuous Sales (2)	Inconspicuous sales (3)	Sales in Red Counties (4)	Sales in Blue Counties (5)
t	-0.011	-0.012	-0.010	-0.040	-0.007
	(0.010)	(0.012)	(0.011)	(0.038)	(0.007)
t × Treated brand (γ)	0.010	0.007	0.014	0.040	0.006
	(0.010)	(0.013)	(0.012)	(0.037)	(0.008)
# of direct marketing communications	0.0004	0.0004^{**}	0.0004	0.004^{**}	0.0003
	(0.0003)	(0.0002)	(0.0003)	(0.002)	(0.0002)
# of retail stores	-0.004	0.010	-0.014	0.002	-0.020
	(0.025)	(0.038)	(0.025)	(0.032)	(0.031)
# of competitor store open	-0.001	-0.010	0.009	0.005	-0.001
	(0.012)	(0.018)	(0.014)	(0.019)	(0.014)
# of competitor store close	-0.039	-0.197	0.0116^{*}	0.127	-0.112
	(0.084)	(0.165)	(0.067)	(0.153)	(0.105)
Brand–county fixed effects	Yes	Yes	Yes	Yes	Yes
# of observations	13,884	9,864	12,732	7,200	6,756
Log-pseudolikelihood	-719,049	-388,762	-423,538	-311,568	-408,869

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A2. Testing for the Parallel Trends Assumption for Sales Quantity

Dependent variable (all in qty)	Total Sales (1)	Conspicuous Sales (4)	Inconspicuous sales (5)	Sales in Red Counties (6)	Sales in Blue Counties (7)
t	-0.003 (0.007)	-0.006 (0.011)	-0.002 (0.007)	-0.027 (0.028)	-0.001 (0.006)
t **× Treated brand (γ)**	-0.006 (0.008)	0.001 (0.012)	-0.011 (0.008)	0.013 (0.027)	-0.007 (0.007)
# of direct marketing communications	0.0003 (0.0002)	0.0001 (0.0002)	0.0005 (0.0003)	0.004^{**} (0.001)	0.0002 (0.0002)
# of retail stores	-0.005 (0.020)	0.045 (0.036)	-0.020 (0.024)	0.017 (0.023)	-0.027 (0.030)
# of competitor store open	0.002 (0.012)	0.004 (0.018)	0.0001 (0.012)	0.001 (0.020)	0.002 (0.014)
# of competitor store close	0.022 (0.103)	-0.105 (0.194)	0.103 (0.086)	0.184 (0.170)	-0.052 (0.135)
Brand–county fixed effects	Yes	Yes	Yes	Yes	Yes
# of observations	13,884	9,864	12,732	7,200	6,756
Log-pseudolikelihood	-23,106	-12,199	-15,827	-9,324	-13,817

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A3. Results from Log-Linear Regressions

	Main effect		With local political preference	
Dependent variable	**log(Sales+1) ($)**	**log(Sales+1) (qty)**	**log(Sales+1) ($)**	**log(Sales+1) (qty)**
Treated brand × Posttreatment (β)	0.138*** (0.042)	0.044*** (0.014)	0.126*** (0.038)	0.040*** (0.013)
Posttreatment × Vote share gap b/n Romney and Obama (γ)			-0.102 (0.097)	-0.028 (0.033)
Treated brand × Posttreatment × Vote share gap b/n Romney and Obama (δ)			0.235** (0.114)	0.073** (0.033)
# of direct marketing communications	0.0004*** (0.0001)	0.0003*** (0.0001)	0.0005** (0.0002)	0.0003*** (0.0001)
# of retail stores	0.025*** (0.006)	0.008*** (0.002)	0.025*** (0.004)	0.008*** (0.001)
# of competitor store open	-0.002 (0.017)	-0.001 (0.007)	-0.002 (0.016)	-0.001 (0.005)
# of competitor store close	0.133 (0.090)	0.053* (0.032)	0.133* (0.075)	0.053** (0.026)
Brand–county fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
# of observations	32,338	32,338	32,338	32,338
R^2	0.448	0.570	0.447	0.567

Notes. Unit of observation is brand–county–week. Fixed-effects log-linear regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A4. Results Using Counties with Balanced Store Availability

	Main effect		With local political preference	
Dependent variable	**Sales ($)**	**Sales (qty)**	**Sales ($)**	**Sales (qty)**
Treated brand × Posttreatment ($\boldsymbol{\beta}$)	0.143*** (0.053)	0.102** (0.047)	0.251*** (0.069)	0.187*** (0.059)
Posttreatment × Vote share gap b/n Romney and Obama ($\boldsymbol{\gamma}$)			-0.273 (0.187)	-0.223 (0.164)
Treated brand × Posttreatment × Vote share gap b/n Romney and Obama ($\boldsymbol{\delta}$)			0.509** (0.197)	0.409** (0.176)
# of direct marketing communications	0.00002 (0.00002)	0.00001 (0.00002)	0.00005** (0.00002)	0.00004 (0.00003)
# of retail stores	0.014*** (0.003)	0.013*** (0.003)	0.013*** (0.003)	0.013*** (0.003)
# of competitor store open	-0.0004 (0.009)	0.008 (0.008)	-0.003 (0.009)	0.005 (0.008)
# of competitor store close	0.077 (0.060)	0.012 (0.061)	0.016 (0.059)	0.025 (0.063)
Brand–county fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
# of observations	26,174	26,174	26,174	26,174
Log-pseudolikelihood	-1,172,317	-38,739	-1,170,396	-38,707

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A5. Change in Sales for Red and Blue Counties

	All counties		Red counties		Blue counties	
Dependent variable	**Sales ($)**	**Sales (qty)**	**Sales ($)**	**Sales (qty)**	**Sales ($)**	**Sales (qty)**
Treated brand × Posttreatment	0.102*	0.075	0.344***	0.283***	0.098	0.070
	(0.054)	(0.049)	(0.121)	(0.104)	(0.054)	(0.049)
Posttreatment × Red county	-0.149	-0.118				
	(0.122)	(0.101)				
Treated brand × Posttreatment × Red county	0.252*	0.206*				
	(0.130)	(0.111)				
# of direct marketing communications	0.00003*	0.00002	0.0001	0.0001	0.0004**	0.00003
	(0.00002)	(0.00002)	(0.0001)	(0.0001)	(0.0002)	(0.00002)
# of retail stores	0.014***	0.013***	0.008	0.022**	0.014***	0.012***
	(0.003)	(0.003)	(0.020)	(0.010)	(0.002)	(0.003)
# of competitor store open	-0.002	0.006	-0.002	0.011	-0.004	0.003
	(0.009)	(0.008)	(0.015)	(0.014)	(0.011)	(0.009)
# of competitor store close	0.022	0.035	0.248*	0.283*	0.013	0.017
	(0.048)	(0.050)	(0.129)	(0.161)	(0.050)	(0.050)
Brand–county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
# of observations	32,338	32,338	17,825	17,825	14,513	14,513
Log-pseudolikelihood	-1,500,682	-50,028	-647,262	-20,483	-849,310	-29,420

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Table A6. Results from Falsification Tests Using Other Moderators

Moderator (X)	**Degree of urbanization**		**# of retail stores**	
Dependent variable	**Sales ($)**	**Sales (qty)**	**Sales ($)**	**Sales (qty)**
Treated brand × Posttreatment	0.472*	0.367*	0.260**	0.215**
	(0.254)	(0.221)	(0.103)	(0.085)
Posttreatment × X	0.500	0.389	0.001	0.001
	(0.344)	(0.293)	(0.001)	(0.001)
Treated brand × Posttreatment × X	-0.456	-0.357	-0.001	-0.001
	(0.360)	(0.312)	(0.001)	(0.001))
# of direct marketing communications	0.00002	8.89e-6	0.00003	0.00002
	(0.00002)	(0.00002)	(0.00002)	(0.00002)
# of retail stores	0.014***	0.013***	0.013***	0.012***
	(0.003)	(0.003)	(0.003)	(0.003)
# of competitor store open	-0.001	0.007	-0.001	0.006
	(0.009)	(0.008)	(0.009)	(0.008)
# of competitor store close	0.015	0.030	0.022	0.034
	(0.048)	(0.050)	(0.048)	(0.050)
Brand–county fixed effects	Yes	Yes	Yes	Yes
Week fixed effects	Yes	Yes	Yes	Yes
# of observations	32,338	32,338	32,338	32,338
Log-pseudolikelihood	-1,500,980	-50,039	-1,501,328	-50,039

Notes. Unit of observation is brand–county–week. Fixed-effects Poisson regressions are shown here. Standard errors clustered at the brand–county level in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.